

UNIVERSITÉ PARIS 1 PANTHÉON-SORBONNE

PSE-ÉCOLE D'ÉCONOMIE DE PARIS

École doctorale 465 Économie Panthéon-Sorbonne (ED 465)

Laboratoire Paris-Jourdan Sciences Économiques (UMR 8545 PjSE)

THÈSE

*Pour l'obtention du grade de Docteur en Sciences Economiques
présentée et soutenue publiquement le 17 juin 2026 par*

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**VIOLENCES FAITES AUX FEMMES : ESSAIS SUR LES RÉPONSES
INSTITUTIONNELLES DANS LE SYSTÈME JUDICIAIRE ET
L'ÉCONOMIE DU CARE**

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Laboratoire Paris-Jourdan Sciences Économiques (UMR 8545 PjSE)

Ph.D. Thesis

Submitted for the Degree of Doctor of Philosophy in Economics

Date of defense: June 17, 2026

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VIOLENCE AGAINST WOMEN: ESSAYS ON INSTITUTIONAL
RESPONSES IN THE JUSTICE SYSTEM AND THE CARE
ECONOMY

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Remerciements

Cette thèse n'aurait pas vu le jour sans nombre d'entre vous : collègues de PSE, ami-es, famille, et toutes les incroyables personnes que j'ai pu rencontrer au cours de ces années et m'ont chacune aidée à aller de l'avant. Ces remerciements sont donc un peu longs.

Avant toute chose, je voudrais exprimer ma plus sincère gratitude envers mes directeur-rices de thèse, Thomas Breda et Camille Hémet, qui se sont parfaitement complétés tout au long de cette préparation de thèse. Merci, Thomas, pour ta confiance lorsque tu as décidé de me superviser à la suite de mon mémoire de master sur un sujet pourtant peu commun, ainsi que pour ton soutien et tes conseils avisés, toujours donnés en toute franchise et avec le sourire. Ton accompagnement aura été très formateur. Merci, Camille, pour ton engagement, ta patience, ta bienveillance et tous tes conseils si riches et pertinents. J'admire ta détermination et ton investissement dans tes projets, malgré tous les obstacles. J'espère suivre vos pas dans le monde de la recherche.

Je tiens aussi à remercier infiniment Sylvie Lambert, qui a accepté de m'accompagner tout au long de cette thèse en prenant part à mon comité. Tes apports scientifiques, toujours si éclairants, ainsi que tes conseils et nos discussions plus générales pour prendre du recul sur mon travail ou lorsque je rencontrais des difficultés, m'ont été d'une aide inestimable.

I am also deeply thankful to Emily Nix, Johanna Rickne, and Héléne Périvier for enthusiastically agreeing to serve on my dissertation committee. I have long admired your work on gender inequalities and violence against women, important subjects economics has increasingly engaged with but that still deserve far more attention as some of the most pressing issues our discipline must address. You have each been a role model and a great source of inspiration for the research I aspire to conduct and the impact I hope to have, and having you on my committee has been a true privilege. The valuable and encouraging feedback from Emily, Héléne, and Sylvie during my pre-defense was also tremendously helpful.

I am particularly grateful to Johanna Rickne, who agreed to welcome me at SOFI at Stockholm University. This visit, as well as her positive and enthusiastic feedback, was extremely helpful at a rather difficult time in this journey. I also owe a great deal to all the wonderful people I met in Stockholm, who helped me make huge progress on my work. Likewise, I would like to extend my deepest thanks to Núria Rodríguez-Planas for welcoming me at IEB at the University of Barcelona, and to the incredible professors, PhD students, and colleagues I met there, whom I unfortunately cannot all cite. Their warm encouragement and collective

support meant a great deal to me.

While switching to English, let me also take the opportunity to thank outstanding researchers I have had the chance to meet during this journey, who provided invaluable support and feedback and helped me move forward in difficult times: Roberto Galbiati, Kristiina Hutunen, Andreas Kotsadam, Olivier Marie, Benjamin Monnery, Aurélie Ouss, Sarah Schneider-Strawczynski, and Jérôme Valette. Many thanks as well to all the people I have had the chance to meet during visits, workshops, and conferences, who allowed me to enjoy the more festive side of academic life while sharing invaluable advice and feedback along the way.

Je suis ravie d'avoir pu réaliser cette thèse à l'École d'Économie de Paris. J'exprime ma sincère gratitude à l'ensemble des professeur-es de PSE, auprès desquel-les j'ai tant appris depuis mon arrivée en master PPD, et que je ne peux malheureusement pas tou-tes citer ici. Merci en particulier à Gabrielle Fack, pour m'avoir donné le goût de la recherche dès mes années de licence au CPES ; David Margolis, pour ses retours toujours si utiles à la suite de mes séminaires ; Mathieu Parenti, pour avoir accepté de faire partie de mon comité de suivi de thèse ; Elena Stancanelli, pour tous ses conseils sur la recherche en économie du genre ; et Nicolas Jacquemet pour son temps et ses retours précieux sur mon papier. Enfin, merci infiniment à Carmel Connell pour son empathie pendant la préparation du job market, et à Angelo Secchi pour son soutien et sa disponibilité sans faille durant cette année difficile.

Je souhaite également remercier les équipes administratives et logistiques de PSE et de Paris 1, qui nous permettent de travailler dans d'excellentes conditions et toujours dans la bonne humeur, en particulier Radja Aroquiaradja, Roxana Ban, Stéphane Brice, Mathieu Colin, Sarah Dafer, Elisabeth Dedieu, Vincent Dupont, Christelle Gauvrit, Evgeniya Kovalenko, Audrey Le Jeune, Nathalie Louni, Aïcha Payet, Patrick Petit, Marie Philipon, Lindsay Polienor et Alice Ruguet. Un grand merci à Jean-Marc Tallon pour avoir validé toutes ces missions à l'étranger pendant ma thèse. Merci aussi à Dominique Meurs et Eric Maurin pour m'avoir généreusement prêté un bureau de la Chaire Travail durant ces derniers mois. Je tiens par ailleurs à saluer l'engagement de Sylvie Lambert et Catherine Bobtcheff en tant que co-directrices du programme doctoral de PSE, dont le soutien a été précieux pour l'ensemble de la communauté des doctorant-es.

Mes remerciements vont aussi à l'Institut des politiques publiques, là où cette thèse a finalement débuté, ainsi qu'à toutes les personnes incroyables que j'ai pu y rencontrer ces dernières années. En particulier, un immense merci à mes co-auteur-rices de projets IPP : Camille Ciriez, Manon Garrouste, Nina Guyon, Laura Khoury, Audrey Rain, Delphine Roy et Léa Toulemon. Je suis très fière de participer à des projets de recherche sur des sujets si importants pour le débat public à vos côtés. Un big up à la team mineurs 100 % féminine, que j'admire beaucoup et avec qui mon travail de recherche s'étend au-delà de cette thèse. Merci également à Joyce Sultan et Maxime Tô pour ma première découverte de l'IPP, ainsi qu'à Paul Dutronc-Postel pour sa curiosité intellectuelle. Merci enfin à Antoine Bozio, Étienne Fize et Thibaut Dernoncourt pour m'avoir aidée à sortir cette note IPP dont je ne m'attendais pas à

ce qu'elle fasse autant de bruit, et pour leur soutien et leur patience dans ce qui a suivi.

Je suis également très reconnaissante à l'Institut national de l'audiovisuel d'avoir cru en mon projet (parfois plus que moi) dès le tout début de ma thèse, et de m'avoir accompagnée et soutenue tout au long de celle-ci. Je remercie en particulier Arthur Lezer, Claude Mussou et Géraldine Poels pour leur disponibilité, leur enthousiasme et leur bienveillance.

Je tiens aussi à remercier toutes les personnes de terrain ou issues du monde associatif, admirables, engagées et profondément humaines, que j'ai rencontrées lors de diverses tables rondes dédiées à la lutte contre les violences faites aux femmes en France. Merci en particulier à Violaine de Filippis-Abate pour avoir si souvent cité et relayé mes travaux, et pour avoir contribué à faire le pont entre mes recherches et la réalité du terrain. Sentir que ma recherche pouvait avoir un réel impact au-delà du monde académique a été une véritable source de motivation dans les moments difficiles.

J'adresse aussi mes remerciements aux étudiant·es à qui j'ai eu la chance d'enseigner au cours de ma thèse à Paris 1, de la macroéconomie à la microéconomie, ainsi qu'aux professeur·es auprès desquel·les j'ai eu le plaisir d'être chargée de TD : Stéphane Gauthier, Claire Pignol, Goulven Rubin et Jean-Philippe Tropeano. Les questions, les remarques et parfois les incompréhensions des étudiant·es m'ont obligée à clarifier ma pensée et ont profondément enrichi ma manière d'enseigner comme de comprendre l'économie.

J'ai ensuite tant d'ami·es de PSE à remercier, ces années ayant été si riches de rencontres et de migrations de bureaux. Merci tout d'abord à mes co-bureaux de R3-01, avec qui j'ai partagé tant de rires et de larmes : Éléonore, toujours positive et avec tellement de pêche ; Léa, si passionnée dans ton travail comme dans la vie, un modèle de résilience que j'ai toujours admiré ; Nica, la touche de folie dont ce bureau avait tant besoin ; Rafael, le seul mec, mais un réel allié ; Romaine, toujours à l'écoute et de si bon conseil sur la thèse ou la vie en général.

Ma gratitude va également à tou·tes les autres doctorant·es avec qui j'ai pu me lier d'amitié au fil de ces années. Artur mérite une place particulière : merci infiniment pour ta présence et ton soutien sans faille depuis le master. Des discussions autour de problèmes économétriques à celles sur la vie en général, des moments plus légers hors thèse jusqu'aux coups de stress de la fin, ton écoute, ta curiosité et ta bienveillance m'ont toujours fait un immense bien. Julia mérite aussi quelques lignes à part : si brillante, drôle et généreuse, tu es une chercheuse et une personne incroyable. Au-delà de nos pauses café à rallonge à parler de tout et de rien, de la thèse comme des choses de la vie, notre amitié m'est infiniment précieuse.

À mes copain·es de la Chaire Travail que j'ai annexé·es, en particulier Florentine, pour ta générosité et ton écoute ; Baptiste et Katharina, mes co-bureaux de fin de thèse si compréhensif·ves ; Inès, pour nos galères communes avec les données de crime ; et Julia et Nathan : merci infiniment pour votre accueil chaleureux. I would also like to thank all those I met at PSE who brightened lunch, coffee breaks, and daily life there and beyond: the development team, whom I always felt so close to in spirit. Andréa and Manasi, my successive co-authors on a paper that I still hope will one day come to completion; Anna, Balasai, Eric, Laura, and Oscar, for

all the fun moments shared along the way; Hector and Marcelo, who make such a great pair, and who know better than most how frank (and unmistakably French) I can be; Paola, for your unique character and sarcastic humor; Sebastián, the great sage since the master's; and Vrinda, for all the little gossip that made this PhD lighter.

I am also grateful to students from other groups, whom I met from the master to the CASD room and across social events, and who made life at PSE nicer: Bertille, Cécile, Clément, Chloé, Damien, Dessie, Donia, Fabian, Gísli, Julius, Laïla, Louis, Pascale, Philipp, Pietro, Rémi, Thomas, Vitalia, Vivien, Xavier, and Yannic. Enfin, merci à Kenza et Margaux pour ce soutien permanent en cette dure année de job market et pour avoir fait ma pub sur tous les réseaux.

Cette thèse n'aurait pas été possible sans mes ami-es de toujours, qui, au-delà de PSE, m'ont été d'un soutien immense. Merci à Camille, Claire et Chloé, qui, depuis mon départ à Paris, n'ont pas toujours bien compris ce que je faisais. Nos moments partagés ont été une vraie coupure. À mes ami-es pour la vie du CPES, dont l'ordre alphabétique ne préjuge en aucun cas du soutien différencié : Agathe, Apolline G., Apolline M., Astrid, Célie, Emma, Héloïse, Juliette, Léonie, Louise, Lucie, Ninon, Suzanne, Tymek et Valentine. Vous avez été mon refuge et mon pilier dans les moments difficiles : votre joie contagieuse, votre confiance infaillible en moi, et toutes ces sorties et activités diverses et variées, de Paris à Montpellier en passant par Strasbourg et Barcelone, m'ont aidée à garder un équilibre précieux. Je suis si reconnaissante de vous avoir dans ma vie. À mes amitiés plus récentes mais déjà si solides : Ana, Billy, Julian, Luc, Margarita, María, María José, Maria Livia, Mario, Paul, Rodrigo et Vicky. Merci à cette team latino pour tous ces moments festifs et si précieux.

À Davi, pour qui aucun remerciement ne sera jamais à la hauteur du soutien apporté pendant cette thèse, et dont la liste des raisons d'être remercié aurait presque pu constituer un chapitre à part entière. Ta patience, ta présence et ton soutien indéfectible ont été un ancrage précieux tout au long de ces années. Merci infiniment d'avoir pris soin de moi, de m'avoir si souvent redonné confiance en moi, et de m'avoir permis de traverser cette aventure plus sereinement. J'ai hâte de découvrir ce que ce nouveau chapitre et la vie nous réservent.

Je voudrais enfin exprimer toute ma gratitude à ma famille. À ma sœur, qui sera finalement devenue docteure avant moi et avec qui j'ai partagé l'aventure de la thèse, bien que notre famille n'ait pas toujours très bien compris ce que nous faisons toutes ces années. À mes parents, qui m'ont malgré tout toujours encouragée à suivre ma voie : votre amour et votre soutien inconditionnels m'ont permis d'aller au bout de ce projet, tout en m'offrant de précieuses bouffées d'air et de repos en Bretagne. À tout le reste de ma famille, pour tous ces moments heureux et ces pauses bien méritées qui me remettaient aussi les pieds sur terre. J'ai enfin une pensée particulièrement émue pour mon grand-père, qui nous a quittés trop tôt et n'aura pas pu voir l'aboutissement de cette thèse, dont je sais qu'il aurait été si fier. Je vous aime infiniment.

Paris, le 15 mai 2026

Résumé

Les violences faites aux femmes constituent un problème social majeur qui touche plusieurs sphères de la société. Cette thèse étudie la manière dont les institutions répondent aux violences et à la vulnérabilité, en se concentrant sur les violences faites aux femmes dans le système de justice pénale et l'économie du *care*. Elle se compose de trois essais qui examinent la manière dont les institutions publiques répondent aux violences de genre, comment ces réponses sont façonnées par des contextes sociaux et informationnels, et comment améliorer les conditions de travail dans un secteur du soin à forte prédominance féminine.

Le **premier chapitre** analyse les déterminants du signalement des violences sexuelles en France à partir de données d'enquêtes de victimation représentatives au niveau national. Il montre que le comportement de signalement varie selon les profils des victimes et dépend en particulier du statut socioéconomique et de la présence de blessures physiques. En s'appuyant sur des informations inédites concernant les raisons déclarées de non-signalement, le chapitre met en lumière les mécanismes sous-jacents à ces différences. Les résultats suggèrent que les victimes de statut socioéconomique plus élevé sont moins susceptibles de signaler les faits parce qu'elles sont davantage conscientes de la faible probabilité d'une issue judiciaire favorable, tandis que les blessures physiques semblent favoriser le signalement en l'alignant davantage sur les représentations stéréotypées persistantes du viol.

Le **deuxième chapitre** étudie si la couverture médiatique des violences faites aux femmes affecte le traitement judiciaire des affaires de violences sexuelles et conjugales. En combinant de nouvelles micro-données administratives sur les affaires pénales avec des données à haute fréquence sur le contenu quotidien des journaux télévisés en France, il montre que la médiatisation de ces violences augmente les taux de poursuite à court terme, sans affecter les décisions de condamnation ou de peine, tout en entraînant une hausse modérée du signalement. Ces résultats s'inscrivent dans un contexte où les procureurs jouent un rôle central dans l'issue des affaires. L'augmentation des poursuites est particulièrement marquée après #MeToo, en cohérence avec une réponse stratégique à une attention publique accrue. Les affaires poursuivies à la suite de cette médiatisation apparaissent tout aussi susceptibles d'aboutir à une condamnation, ce qui suggère qu'une plus grande visibilité permet à davantage de dossiers viables d'accéder au procès sans influencer les décisions des juges.

Le **troisième chapitre** s'intéresse à l'économie du *care* et examine si une innovation organisationnelle peut améliorer les conditions de travail dans l'aide à domicile en France.

Ce secteur, majoritairement féminin, est marqué par des conditions de travail difficiles, des horaires fragmentés, un isolement professionnel, de faibles salaires et un turnover élevé. À travers une expérimentation randomisée de terrain, le chapitre évalue les effets de l'organisation en équipes autonomes, qui accorde aux aides à domicile une plus grande autonomie dans l'organisation des plannings, des remplacements et des soins. Les résultats montrent que cette nouvelle organisation améliore les conditions de travail, la satisfaction professionnelle et la santé mentale, tout en réduisant le turnover, et sans générer d'effets négatifs pour les bénéficiaires. Ils suggèrent que l'organisation en équipes autonomes peut améliorer le bien-être et la rétention des travailleurs dans le secteur du soin et de l'accompagnement, avec des implications importantes dans un contexte de vieillissement de la population.

Pris ensemble, ces chapitres montrent comment les représentations sociales, l'attention publique et l'organisation du travail façonnent les réponses institutionnelles aux violences et à la vulnérabilité dans différentes sphères.

***Mots-clés**—Économie du genre, Économie de la criminalité, Économie du travail, Violences faites aux femmes, Justice pénale, Organisation du travail*

Abstract

Violence against women is a widespread social problem that affects multiple spheres of society. This dissertation studies how institutions respond to violence and vulnerability, focusing on violence against women in the criminal justice system and the care economy. It consists of three essays that study how public institutions respond to gender-based violence, how these responses are shaped by social and informational contexts, and how working conditions can be improved in a female-dominated care sector.

The **first chapter** analyzes the determinants of reporting sexual violence to the police in France using nationally representative victimization survey data. It shows that reporting behavior varies systematically across victim profiles and depends in particular on socioeconomic status and the presence of physical injuries. Drawing on unique information about victims' stated reasons for not reporting, the chapter sheds light on the mechanisms underlying these patterns. The findings suggest that higher-status victims may be less likely to report because they are more aware of the low probability of a successful judicial outcome, while physical injuries seem to encourage reporting by aligning it more closely with persistent stereotypical representations of rape.

The **second chapter** studies whether media coverage of violence against women affects judicial decision-making in sexual and intimate partner violence cases. By combining novel administrative microdata on criminal cases with high-frequency data on daily television news content in France, it shows that media coverage of such violence increases prosecution rates in the short run, without affecting conviction or sentencing decisions, while also moderately increasing reporting. These findings emerge in a context where prosecutors play a central role in case outcomes. The shift in prosecutions is particularly strong in the post-#MeToo era, consistent with strategic responses to heightened public scrutiny and accountability. Cases prosecuted following media coverage appear just as likely to lead to conviction, suggesting that greater visibility helps additional viable cases reach trial without influencing judges' rulings.

The **third chapter** turns to the care economy and examines whether organizational innovation can improve working conditions in the French home care sector. This predominantly female sector is marked by difficult working conditions, fragmented work schedules, professional isolation, low wages, and high turnover. Using a randomized controlled trial, the chapter evaluates the effects of introducing self-managed teams, which grant home care workers

greater autonomy over scheduling, replacements, and care planning. The results show that self-organization improves working conditions, job satisfaction, and mental health, while reducing turnover and generating no detrimental effects for care recipients. These findings provide rare causal evidence that organizational design can improve worker well-being and retention in long-term care, with important implications in a context of population aging.

Together, the chapters show how social representations, public attention, and work organization shape institutional responses to violence and vulnerability across different spheres.

Keywords—*Gender economics, Crime economics, Labor economics, Gender-based violence, Criminal justice, Work organization*

Contents

Remerciements / Acknowledgements	1
Résumé	5
Abstract	7
Contents	9
Introduction générale	13
General introduction	25
1 Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019	40
1.1 Introduction	42
1.2 Literature and theoretical framework	44
1.2.1 What is known about SV victimization reporting?	44
1.2.2 Hypotheses	46
1.3 Data: French victimization survey	47
1.3.1 SV prevalence, sample representativeness, and measurement bias	47
1.3.2 Sample characteristics and selection	49
1.3.3 Changes in the legal and social contexts over the analysis period	52
1.4 Empirical methodology	54
1.4.1 Reporting determinants	54
1.4.2 Reasons for not reporting	55
1.5 Results	57
1.5.1 Role of physical injury and the victim’s SES	57
1.5.2 Low expectations regarding police and judicial responses and better alignment with the “classic rape” stereotype	61
1.6 Conclusion	63
References	64
Appendix to Chapter 1	70
1.A Data and descriptive statistics	70
1.B Self-reported male victims of sexual violence	74
1.C Justification for the socioeconomic status index	77
1.D Complaint filing versus global reporting decision	79
1.E Linear probability model results	81

1.F	Heterogeneity analyses and interaction between socioeconomic status, physical injuries, and relationship status	84
1.G	Additional explanatory hypotheses	88
1.H	Expectations of police and judicial response	92
	References	95
2	Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women	96
2.1	Introduction	98
2.2	Institutional Background	104
2.3	Data and Descriptive Statistics	107
2.3.1	Judicial data	107
2.3.2	Descriptive statistics on judicial data	109
2.3.3	TV news data	114
2.3.4	Descriptive statistics on TV news data and sample restriction	115
2.4	Empirical Framework	118
2.4.1	Theoretical basis	119
2.4.2	Estimation strategy	120
2.4.3	Identification strategy	122
2.5	Results	124
2.5.1	Main results	124
2.5.2	Heterogeneous effects on prosecutorial decisions	130
2.5.3	Changes in victim reporting or police recording behavior	132
2.5.4	Long-term effect of news coverage on conviction	135
2.5.5	Robustness and sensitivity checks	137
2.6	Discussion	139
2.6.1	Magnitudes	139
2.6.2	Specificity to violence against women	141
2.6.3	Potential mechanisms	142
2.7	Conclusion	145
	References	146
	Appendix to Chapter 2	153
2.A	Additional elements on the French criminal justice system	153
2.A.1	Judicial actors and jurisdictions	153
2.A.2	Legal concepts and procedures	154
2.A.3	Legal qualification of offenses	155
2.B	Additional elements on judicial data	156
2.B.1	Further information on the data source and construction	156
2.B.2	Further descriptive results	157

2.C	Additional elements on TV news data	162
2.C.1	Further information on the data source and construction	162
2.C.2	News consumption according to ELIPSS data	166
2.C.3	Further descriptive results	169
2.D	Empirical strategy	174
2.D.1	Illustrations of the empirical strategy	174
2.D.2	Seasonality and balance tests	175
2.E	Additional results on reporting	178
2.E.1	Additional results on the number of cases reported	178
2.E.2	Reporting or incidence?	184
2.E.3	Placebos	187
2.F	Additional results on prosecution	188
2.F.1	Additional results on the decision to dismiss a case	188
2.F.2	Placebos	190
2.F.3	Heterogeneity analysis	193
2.F.4	Robustness checks	201
2.F.5	Extrapolation to other crimes	210
2.F.6	Additional results on the decision to prosecute and case processing time	215
2.G	Additional results on conviction	221
2.G.1	Additional results on the decision to convict and the sentence length	221
2.G.2	Additional results on long-term effects	227
2.H	#MeToo analysis	230
	References	236

3	The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment	237
3.1	Introduction	239
3.2	Institutional Background	243
3.2.1	Long-term Care in France	243
3.2.2	The Buurtzorg-Inspired Model	244
3.3	Intervention and Experimental Design	246
3.3.1	Intervention: The Self-Managed Teams Organization	246
3.3.2	Experimental Design	247
3.4	Data Collection and Descriptive Statistics	250
3.4.1	Data Sources	250
3.4.2	Survey Response Rates	252
3.4.3	Baseline Descriptive Statistics and Sample Representativity	253
3.4.4	Baseline Balance	256
3.5	Empirical Strategy	259

3.5.1	First Stage: Effective Implementation of Local Self-managed Teams . . .	259
3.5.2	Non-compliance	260
3.5.3	Estimation Models	263
3.6	Results	264
3.6.1	Main results on care workers	264
3.6.2	Main results on care recipients	269
3.6.3	Robustness	271
3.7	Conclusion	275
	References	275
	Appendix to Chapter 3	280
3.A	Design of the experiment	280
3.A.1	Balance tests	286
3.A.2	Attrition	287
3.B	Descriptive statistics	295
3.C	Treatment compliance	300
3.C.1	Potential Spillover Effects	306
3.D	Estimation results	309
3.D.1	Results on care workers	309
3.D.2	Results on care recipients	325
3.D.3	Robustness	333
3.E	Other material	336
	Appendix: Implementation Framework	337
	Appendix: Implementation Charter	339
	Index	343
	List of Figures	343
	List of Tables	348

Introduction générale

*« A cada minuto, de cada semana
Nos roban amigas, nos matan hermanas
Destrozan sus cuerpos, los desaparecen
No olvide sus nombres, por favor, señor presidente »*

Canción Sin Miedo

Vivir Quintana, 2020

*« Perpétuer les choses assure une certaine quiétude
et provoque toujours moins de heurts que vouloir les changer. »*

Une farouche liberté

Gisèle Halimi, 2020

Violence, vulnérabilité et réponses institutionnelles

Les inégalités de genre se manifestent dans de nombreuses sphères, qu'il s'agisse de l'éducation, du marché du travail, de la représentation politique, des rapports au sein du foyer ou de l'exposition différenciée à la violence. Parmi leurs manifestations les plus graves, les violences faites aux femmes constituent à la fois une violation des droits humains et un problème social majeur. Les Nations unies définissent les violences faites aux femmes comme « tout acte de violence fondée sur le genre entraînant, ou susceptible d'entraîner, des atteintes ou souffrances physiques, sexuelles ou psychologiques pour les femmes » ([UN General Assembly, 1993](#)). Malgré une prise de conscience croissante et le renforcement des politiques de lutte contre ces violences, elles restent très répandues : les estimations mondiales récentes indiquent qu'environ une femme sur trois âgée de 15 ans ou plus a subi des violences physiques ou sexuelles au cours de sa vie ([WHO, 2021](#)).

Les réponses institutionnelles jouent un rôle central dans la manière dont les sociétés font face à ces violences. Les institutions judiciaires déterminent si les violences sont sig-

nalées, enregistrées, instruites, poursuivies et sanctionnées, mais la progression à travers ces différentes étapes reste souvent inégale et incertaine. Les violences sexuelles et conjugales figurent parmi les infractions les moins déclarées (Palermo et al., 2014), contribuant ainsi au « chiffre noir » de la criminalité, c'est-à-dire à la part des infractions absentes des statistiques officielles parce qu'elles ne sont ni signalées ni enregistrées par les autorités. Parmi les affaires qui entrent finalement dans le système judiciaire, beaucoup sont ensuite classées sans suite ou n'aboutissent pas au cours de la procédure, un phénomène généralement désigné sous le terme d'attrition judiciaire (Daly and Bouhours, 2010). Au-delà des définitions juridiques et des exigences en matière de preuve, le parcours des affaires peut aussi dépendre du contexte social et informationnel dans lequel les victimes, les policiers, les procureurs et les juges prennent leurs décisions.

Cette thèse étudie ces réponses institutionnelles dans le contexte français. Elle s'intéresse d'abord au système de justice pénale, où la question centrale est de savoir dans quelle mesure les violences faites aux femmes sont transformées en affaires judiciaires et comment ces affaires sont ensuite traitées par les institutions judiciaires. Elle se tourne ensuite vers un autre domaine où les institutions contribuent à façonner des formes de vulnérabilité qui touchent principalement les femmes : l'économie du *care*, entendue ici comme l'ensemble des activités d'aide, d'accompagnement et de soin auprès des personnes dépendantes. Ce secteur, très majoritairement féminin, rassemble des emplois exigeants, souvent marqués par des conditions de travail difficiles. Il constitue ainsi un autre espace social où les règles organisationnelles influencent l'autonomie des travailleuses, leur santé mentale, leur satisfaction professionnelle et leur maintien dans l'emploi. Bien que le système judiciaire et le secteur du *care* renvoient à des réalités et à des contextes distincts, ils soulèvent une question commune : comment les institutions répondent-elles à des formes de violence et de vulnérabilité qui affectent de manière disproportionnée les femmes, et comment ces réponses sont-elles façonnées par le contexte social, l'attention publique et l'organisation du travail ?

Pourquoi une approche économique ?

L'étude des violences faites aux femmes et des formes de vulnérabilité qui touchent plus particulièrement les femmes est par nature interdisciplinaire. La sociologie, la psychologie, la criminologie, le droit et la santé publique offrent des cadres essentiels pour comprendre les traumatismes, les rapports de pouvoir, la stigmatisation et le traitement institutionnel de ces violences. Ces disciplines s'appuient pour cela sur des travaux qualitatifs et quantitatifs qui

documentent les expériences des victimes, les facteurs sociaux associés aux violences, ainsi que leurs conséquences et leur prise en charge par les institutions. L'économie complète ces approches en s'intéressant aux coûts, aux contraintes, aux incitations, aux croyances et aux processus de décision. Elle mobilise pour cela des sources de données variées ainsi que des méthodes statistiques et économétriques permettant d'appuyer l'identification causale. Au-delà de cette dimension méthodologique, cette thèse mobilise l'approche économique de trois manières principales.

Une première perspective économique porte sur les coûts associés à ces violences. Leurs conséquences dépassent largement les dommages immédiats subis par les victimes. Une vaste littérature interdisciplinaire documente des effets graves et persistants sur la santé, le bien-être et les trajectoires familiales (Aizer, 2011; Scodellaro, 2020; Brown et al., eds, 2021; Brunton and Dryer, 2022). Un nombre croissant de travaux en économie montre également que les violences faites aux femmes perturbent les trajectoires scolaires, professionnelles et salariales (Borker, 2021; Folke and Rickne, 2022; Adams-Prassl et al., 2024; Coutolleau et al., 2024; Batut et al., 2026; Adams et al., 2026). Ces coûts sont supportés non seulement par les victimes, mais aussi par les familles, les employeurs, les services publics et la société dans son ensemble (Day et al., 2005). Les violences faites aux femmes ne constituent donc pas seulement un préjudice individuel ou un enjeu de santé publique. Elles représentent également un enjeu économique, car elles affectent l'accumulation de capital humain, la participation au marché du travail, les dépenses publiques et l'accès aux opportunités scolaires, professionnelles et économiques.

Une deuxième perspective économique s'appuie sur la théorie du choix rationnel en économie du crime, en analysant les décisions de signalement et les décisions judiciaires comme des décisions prises dans un contexte d'incertitude et de contraintes (Becker, 1968; Ehrlich, 1973). Pour les victimes, signaler des violences à la police peut être compris comme un choix institutionnel coûteux, fondé sur l'anticipation de bénéfices et de coûts incertains (Bowles et al., 2009; Chaussebourg et al., 2011). Le dépôt de plainte peut offrir une protection, une reconnaissance ou une réparation, mais il implique aussi des coûts psychologiques, sociaux, financiers et juridiques, notamment la stigmatisation, la peur des représailles, l'incertitude liée à la procédure judiciaire et de faibles attentes vis-à-vis du soutien institutionnel. Dans ce contexte, les informations disponibles et les représentations sociales des violences faites aux femmes peuvent façonner les attentes des victimes quant aux bénéfices possibles d'une action judiciaire, et ainsi influencer leur décision de signaler ou non les violences.

Cette même perspective économique peut également être appliquée aux acteurs judici-

aires. Une fois qu'une affaire entre dans le système de justice pénale, les procureurs et les juges prennent des décisions importantes sous contraintes informationnelles et matérielles (Landes, 1971; Easterbrook, 1983). Ces contraintes impliquent des arbitrages autour du niveau de preuve requis et du risque d'erreur judiciaire, dans un contexte où les acteurs judiciaires disposent d'un temps, de moyens et d'une attention limités à consacrer à chaque affaire. Elles sont particulièrement fortes pour les infractions qui se déroulent souvent dans la sphère privée, avec peu de témoins et peu de preuves matérielles (Daly and Bouhours, 2010), ce qui les rend particulièrement difficiles à instruire et à poursuivre. Dans le même temps, ces contraintes laissent souvent une marge importante de discrétion dans le traitement des affaires, ouvrant potentiellement la voie à l'influence de facteurs externes, notamment les normes sociales, les croyances et les attitudes à l'égard des violences faites aux femmes. Dans ce cadre, cette thèse étudie la manière dont les acteurs judiciaires réagissent aux évolutions de leur environnement social et informationnel, en se concentrant en particulier sur la couverture médiatique des violences faites aux femmes comme indicateur de la visibilité croissante de cette question dans le débat public. Ce faisant, elle s'appuie également sur la littérature théorique et empirique en économie politique des médias, qui montre que les médias peuvent exercer une influence importante sur la société à travers différents mécanismes comportementaux et incitatifs (DellaVigna and La Ferrara, 2015).

Enfin, cette thèse inscrit l'étude des vulnérabilités qui touchent particulièrement les femmes dans une réflexion plus large sur le travail, les organisations et la soutenabilité des services d'aide et de soin de longue durée. Cette perspective est particulièrement importante dans le secteur de l'aide à domicile, où le vieillissement démographique accroît rapidement la demande dans les pays développés (OECD, 2025). Répondre à cette demande ne constitue pas seulement un défi social et sanitaire, mais aussi un enjeu économique central : il faut attirer et retenir des travailleuses et travailleurs dans un secteur déjà sous tension, pourtant essentiel au bien-être des personnes dépendantes. En mobilisant les théories de l'économie des organisations et les travaux sur l'autonomie, la coordination, le soutien entre pairs et la gouvernance, cette thèse examine comment l'organisation interne du travail façonne la qualité du travail, le maintien dans l'emploi et la qualité des services fournis dans ce secteur (Bandiera et al., 2005; Mas and Moretti, 2009; Batt and Colvin, 2011).

Le contexte français : violences, justice et *care*

La France constitue un terrain particulièrement pertinent pour étudier les réponses institutionnelles aux formes de vulnérabilité qui touchent plus particulièrement les femmes. Les violences faites aux femmes y sont devenues un enjeu public majeur, notamment depuis le mouvement #MeToo et la visibilité croissante des violences sexuelles et des féminicides dans le débat public. Cette évolution s'est accompagnée d'une couverture médiatique accrue, de mobilisations collectives et d'initiatives politiques autour de cette question (Cavalin et al., 2022). Pourtant, l'ampleur du phénomène demeure considérable. Près de 15 % des femmes âgées de 20 à 69 ans déclarent avoir subi au moins une fois au cours de leur vie un viol, une tentative de viol ou une autre forme d'agression sexuelle (Hamel et al., 2016). En 2021, les autorités ont enregistré 247 000 victimes de violences sexuelles, dont 88 % étaient des femmes, ainsi que 422 000 victimes de violences conjugales, dont 76 % étaient des femmes (SSMSI, 2023). Les conséquences sur la santé sont également très lourdes : en France, 33 % des femmes adultes ayant subi un viol ou une tentative de viol déclarent avoir fait une tentative de suicide, contre 6 % dans l'ensemble de la population féminine (Scodellaro, 2022).

L'écart entre l'ampleur des violences et la réponse institutionnelle reste important. Les enquêtes de victimation montrent que seule une minorité de victimes signalent ces violences aux autorités : seule une victime de violences sexuelles sur dix dépose plainte, et moins d'une victime sur cinq dans le cas des violences conjugales (Guedj, 2017; SSMSI, 2023; Stricot, 2025). Ce faible taux de signalement limite l'accès à la protection juridique et contribue à la persistance d'un important chiffre noir de la criminalité. Même lorsque les affaires entrent dans le système pénal, l'attrition judiciaire reste élevée. Les données administratives montrent qu'une grande partie des affaires de violences sexuelles et conjugales traitées par les parquets sont classées sans suite (Stricot, 2024), souvent parce que l'infraction est considérée comme insuffisamment caractérisée. Les exigences en matière de preuve et la marge de discrétion des procureurs jouent ainsi un rôle central dans le traitement judiciaire des violences faites aux femmes dans le contexte français.

Le secteur du *care* offre un autre exemple de la manière dont les règles institutionnelles et organisationnelles peuvent structurer les conditions de vie et de travail des femmes en France. L'aide à domicile occupe une place centrale dans les politiques publiques visant à favoriser le maintien à domicile des personnes âgées. Pourtant, comme dans de nombreux autres pays développés, ce secteur fait face à d'importantes difficultés de recrutement et de rétention. Sa main-d'œuvre, très majoritairement féminine, est confrontée à de bas salaires, des horaires fragmentés, un isolement professionnel, des perspectives de carrière limitées, un

travail physiquement et émotionnellement exigeant, ainsi qu'un fort turnover (Bailly et al., 2013; El Khomri, 2019). Ces difficultés affectent non seulement les travailleuses, mais aussi la qualité et la continuité de l'accompagnement reçu par les personnes âgées et dépendantes. Elles ont également entraîné des transformations organisationnelles dans les services d'aide à domicile en France, offrant une occasion unique d'évaluer les effets de nouvelles formes d'organisation du travail dans ce secteur.

État de la littérature, méthodologies et contributions

Cette thèse contribue à plusieurs champs de la littérature en économie et en sciences sociales portant sur les violences faites aux femmes, la justice pénale, les médias et l'organisation du travail. Ces objets sont difficiles à étudier empiriquement, car les décisions pertinentes interviennent souvent dans des contextes privés, sensibles ou administrativement complexes. Les décisions de signalement des victimes ne sont observées qu'en partie, les trajectoires judiciaires sont difficiles à reconstruire, la visibilité médiatique est rarement mesurée à haute fréquence, et les réformes organisationnelles dans le secteur du soin font rarement l'objet d'évaluations expérimentales. Une contribution transversale de cette thèse consiste donc à mobiliser des sources de données rares et originales, difficiles d'accès ou de collecte, ainsi que des méthodes descriptives et d'inférence causale. Elle s'appuie ainsi sur des données d'enquêtes de victimation représentatives au niveau national, des données administratives détaillées sur le traitement judiciaire, des données textuelles issues d'archives médiatiques et des données d'enquête de première main collectées dans le cadre d'une expérimentation randomisée de terrain. Ces données et méthodes permettent de répondre à quatre lacunes dans la littérature existante.

Premièrement, si les recherches existantes ont étudié les facteurs associés à la prévalence et au signalement des violences faites aux femmes, les mécanismes qui conduisent les victimes à signaler ou non les violences sexuelles qu'elles ont subies restent moins bien compris. Une grande partie des connaissances disponibles repose sur de petits échantillons, qui ne sont pas représentatifs de la population générale. Le premier chapitre répond à cette limite à partir des enquêtes françaises de victimation *Cadre de Vie et Sécurité*, représentatives au niveau national. Ces données restent rarement mobilisées en économie, alors même qu'elles fournissent des informations particulièrement riches sur la victimation, les comportements de signalement et les raisons de non-signalement, à partir d'un échantillon représentatif de la France métropolitaine sur une longue période, de 2007 à 2019. Contrairement aux données

policières, ces enquêtes permettent de distinguer la prévalence des violences de leur signalement, et donc d'analyser plus précisément le recours aux institutions. Elles permettent ainsi d'étudier à la fois l'évolution du signalement des violences sexuelles au cours du temps et les barrières individuelles qui empêchent certaines victimes de se tourner vers la justice.

Deuxièmement, si la littérature en économie du crime et de la justice a largement étudié le traitement policier, les décisions de condamnation et de détermination des peines, la discrétion du parquet et les facteurs qui influencent les décisions des procureurs restent moins bien compris. Cette lacune est d'autant plus importante que les procureurs jouent un rôle central dans la sélection des affaires qui parviennent jusqu'au procès dans de nombreux systèmes judiciaires. Comprendre les décisions du parquet est particulièrement crucial dans le cas des violences sexuelles et conjugales, où les preuves sont souvent difficiles à collecter et les enquêtes difficiles à mener. Le deuxième chapitre aborde cette question en mobilisant de nouvelles micro-données administratives portant sur la quasi-totalité des infractions traitées par le système pénal français entre 2012 et 2019, issues du système de gestion des affaires *Cas-siopée* du ministère de la Justice. Cette thèse constitue le premier projet de recherche à utiliser ces micro-données judiciaires exceptionnellement riches dans le cadre d'une analyse quantitative. Elles offrent une vision particulièrement détaillée du contenu des affaires pénales, de leur traitement et de leurs trajectoires, depuis leur enregistrement jusqu'aux décisions du parquet et aux décisions de jugement.

Troisièmement, les recherches existantes montrent que la couverture médiatique peut influencer les attitudes, les normes et les comportements institutionnels, mais on sait encore peu de choses sur l'effet des variations de court terme de l'attention publique sur les décisions ordinaires de justice pénale, en particulier avant que les affaires n'arrivent au procès. Le deuxième chapitre répond à cette question en reliant ces micro-données administratives judiciaires à des données exhaustives et très fiables sur le contenu quotidien des principaux journaux télévisés nationaux du 20 heures en France, issues des archives de l'Institut national de l'audiovisuel pour la période 2000–2021. Ce rapprochement permet d'associer le calendrier des décisions judiciaires aux variations quotidiennes de la visibilité médiatique des violences faites aux femmes. En exploitant les variations quasi aléatoires des sujets diffusés à la télévision, le chapitre examine si la couverture médiatique ordinaire des violences faites aux femmes affecte causalement le traitement judiciaire de ces infractions. Il propose ainsi une manière originale d'étudier la façon dont les institutions judiciaires réagissent aux changements de court terme de l'attention publique.

Enfin, cette thèse contribue à la littérature sur le genre, le travail et les organisations, plus

spécifiquement sur l'organisation du travail dans les métiers du *care*, un secteur faiblement rémunéré et très majoritairement féminin. Les recherches existantes soulignent le rôle de la ségrégation professionnelle, de la répartition inégale des tâches domestiques et des responsabilités de soin au sein de la famille, ainsi que des contraintes au travail dans la persistance des inégalités de genre. Cependant, les preuves causales restent limitées quant à la capacité d'une transformation de l'organisation interne du travail à améliorer les conditions d'emploi dans ces métiers. Le troisième chapitre répond à cette limite à travers l'évaluation par tirage aléatoire d'une nouvelle organisation du travail dans les services d'aide à domicile, la première expérimentation rigoureuse de ce type mise en œuvre, tous pays et secteurs confondus. En combinant des méthodes expérimentales, des outils économétriques d'inférence causale et des données de terrain originales collectées auprès des aides à domicile, des bénéficiaires et des aidants informels, il fournit une évaluation causale inédite d'une innovation organisationnelle visant à améliorer les conditions de travail dans le secteur du *care*.

Présentation des chapitres

Chapitre 1 – Comprendre le signalement des violences sexuelles : analyse empirique en France (2007-2019)

Le premier chapitre étudie les déterminants du signalement à la police des violences sexuelles récentes en France. À partir des données des enquêtes de victimation *Cadre de Vie et Sécurité*, conduites chaque année de 2007 à 2019, il analyse un échantillon nationalement représentatif de 1 523 femmes ayant déclaré avoir subi des violences sexuelles au cours des deux années précédant l'enquête, en dehors ou au sein de leur ménage actuel. Le chapitre estime des modèles de régressions logistiques afin d'étudier, de manière descriptive, comment les comportements de signalement varient selon le profil sociodémographique des victimes et les circonstances des agressions. Il mobilise ensuite les informations inédites contenues dans l'enquête sur les motifs de non-signalement déclarés par les victimes, afin d'examiner les mécanismes pouvant expliquer ces différences de signalement. L'analyse se concentre en particulier sur le rôle des stéréotypes du viol, de la crainte de la stigmatisation sociale et des attentes vis-à-vis de la réponse judiciaire.

Les résultats montrent que les victimes de statut socioéconomique plus faible et celles ayant subi des blessures physiques sont significativement plus susceptibles de signaler les faits aux autorités. L'analyse des raisons déclarées de non-signalement permet de réexaminer les explications généralement avancées pour comprendre ces résultats. Le faible

taux de signalement observé chez les victimes au statut socioéconomique élevé s'explique davantage par leur conscience des faibles chances de succès judiciaire que par la crainte de la stigmatisation sociale. À l'inverse, l'impact significatif des blessures physiques sur le signalement tient plus à une correspondance plus étroite avec le stéréotype du « viol classique » qu'aux chances perçues de gagner un procès grâce à des preuves matérielles. Ces résultats montrent comment les normes sociales et les représentations stéréotypées des violences peuvent fonctionner comme des barrières informelles à la justice, renforçant ainsi le sous-signalement des violences sexuelles.

Chapitre 2 — « Breaking news » : comment la couverture médiatique façonne le traitement judiciaire des violences faites aux femmes

Le deuxième chapitre analyse si la couverture médiatique des violences faites aux femmes affecte le traitement judiciaire des affaires de violences sexuelles et conjugales. Il combine de nouvelles micro-données administratives du ministère de la Justice, couvrant près de 800 000 affaires de violences sexuelles et conjugales traitées par les tribunaux français entre 2012 et 2019, avec des données à haute fréquence sur le contenu quotidien des principaux journaux télévisés nationaux du 20 heures, à savoir ceux de TF1 et de France 2. En exploitant le caractère quasi aléatoire du calendrier des sujets d'actualité consacrés aux violences faites aux femmes, distincts des affaires en cours de traitement, le chapitre estime l'effet causal de court terme de cette visibilité médiatique accrue sur les décisions des procureurs et des juges.

Les résultats montrent que la médiatisation des violences faites aux femmes influence significativement les décisions des procureurs, avec une augmentation de 2,3 % du taux de poursuite dans la semaine suivant la diffusion de ces sujets à la télévision, ainsi qu'une hausse modérée du signalement. En revanche, l'exposition médiatique n'a pas d'effet direct sur les décisions de condamnation ou de détermination des peines prises par les juges. Ce résultat s'inscrit dans un contexte où les procureurs déterminent largement le devenir des affaires : ils classent sans suite près de 80 % des dossiers, tandis que la plupart des affaires poursuivies aboutissent à une condamnation. Des éléments suggestifs indiquent que cette hausse des poursuites est principalement portée par des réponses stratégiques à une surveillance publique et à une responsabilisation accrues. L'effet n'apparaît que dans la période post-#MeToo, où son ampleur est plus de deux fois supérieure. Cela suggère que la médiatisation ordinaire des violences faites aux femmes est devenue plus conséquente dans un contexte où ces violences constituaient déjà un enjeu saillant dans l'espace public. Un résultat central est que les affaires supplémentaires poursuivies à la suite de la couverture

médiatique semblent avoir une probabilité de condamnation comparable à celle des autres affaires poursuivies. Ces résultats suggèrent que, dans des contextes de ressources limitées, une visibilité accrue des violences faites aux femmes peut permettre à davantage de dossiers viables d'accéder au procès, sans compromettre la qualité des dossiers ni influencer directement les décisions des juges, renforçant ainsi la réponse judiciaire globale à ces violences.

Chapitre 3 – Effets de l'introduction de l'organisation en équipes autonomes dans le secteur de l'aide à domicile : une évaluation par tirage aléatoire

Le troisième chapitre¹ porte sur l'économie du *care* et évalue les effets d'une nouvelle forme d'organisation du travail dans le secteur de l'aide à domicile. Ce secteur en forte expansion, très majoritairement féminin, fait face à des pénuries chroniques de main-d'œuvre, liées à des conditions de travail difficiles, des horaires fragmentés, un isolement professionnel, de faibles salaires et un turnover élevé. Face à ces difficultés, de nombreuses structures se sont tournées vers l'organisation en équipes autonomes. Ce nouveau modèle donne aux aides à domicile davantage de contrôle sur l'organisation des plannings, des remplacements et des soins, mais peut également accroître les coûts de coordination et les charges administratives. À partir d'un essai contrôlé randomisé mené dans plusieurs structures d'aide à domicile en France, le chapitre estime les effets causaux de l'introduction de l'organisation en équipes autonomes. Au sein de chaque structure, les équipes ont été assignées aléatoirement soit à adopter le nouveau modèle, soit à conserver l'organisation hiérarchique classique, permettant de limiter les biais de sélection. L'analyse combine des données originales d'enquêtes de terrain, collectées auprès des aides à domicile, des bénéficiaires et de leurs aidants informels avant l'intervention puis 12 à 18 mois plus tard, avec des données administratives détaillées issues des structures participantes.

Les résultats montrent de façon robuste que l'organisation en équipes autonomes améliore significativement le bien-être et le maintien dans l'emploi des aides à domicile. L'assignation à cette nouvelle organisation du travail augmente la satisfaction au travail de 0,24 écart-type et améliore les conditions de travail globales de 0,41 écart-type, avec des progrès notables dans l'organisation du travail et la gestion des plannings. Les travailleuses des équipes traitées déclarent également une moindre tension au travail et une meilleure santé mentale, se disant plus heureuses et moins stressées au travail. Ces améliorations dans la qualité de l'emploi se traduisent aussi par une réduction de 7,8 points de pourcentage de la probabilité de quitter

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l'organisation. Ces effets positifs pour les travailleuses ne se font pas au détriment de la qualité du service : les bénéficiaires déclarent une meilleure adéquation du temps d'accompagnement, sans effet négatif détecté sur leur santé ou leur satisfaction globale. Ces résultats montrent ainsi que l'organisation du travail peut constituer un levier important pour améliorer la qualité de l'emploi et la rétention des travailleurs dans le secteur du soin et de l'accompagnement, avec des implications importantes pour les politiques publiques dans un contexte de hausse de la demande liée au vieillissement démographique.

Enseignements et implications

Pris ensemble, les chapitres montrent que les réponses institutionnelles aux formes de vulnérabilité qui touchent particulièrement les femmes sont façonnées par les croyances, la visibilité et l'organisation du travail. Les décisions de signalement des victimes dépendent à la fois de la manière dont elles interprètent les violences subies et des attentes qu'elles formulent à l'égard de la réponse institutionnelle. Les décisions des procureurs peuvent réagir aux variations de l'attention publique lorsque les contraintes en matière de preuve et de ressources laissent une place au jugement discrétionnaire. Enfin, le bien-être et le maintien dans l'emploi des travailleuses du *care* s'améliorent lorsque les règles organisationnelles leur accordent davantage d'autonomie et de contrôle collectif sur leur travail. Ces résultats conduisent à deux principaux enseignements : améliorer l'accès à la justice pour les victimes de violences faites aux femmes et repenser l'organisation du travail dans l'économie du *care*.

Renforcer la réponse judiciaire. Les deux premiers chapitres montrent que la réponse judiciaire aux violences faites aux femmes se heurte à des contraintes à deux moments clés : avant l'entrée dans le système judiciaire, lorsque les victimes décident de signaler ou non les violences subies, puis une fois les affaires enregistrées, lorsque les procureurs décident si elles doivent donner lieu à des poursuites. Améliorer l'accès à la justice suppose donc d'agir sur ces deux dimensions. Les politiques visant à accroître le signalement doivent s'attaquer aux représentations sociales qui façonnent les attentes des victimes, notamment aux représentations stéréotypées des violences sexuelles et aux croyances relatives à la crédibilité des victimes. Elles doivent également réduire les coûts perçus et réels du signalement, en rendant les réponses institutionnelles plus transparentes, plus accessibles et plus susceptibles d'apporter protection, reconnaissance ou réparation.

Les résultats suggèrent également que l'attention publique peut renforcer la respons-

abilisation institutionnelle au stade du parquet. La visibilité médiatique semble permettre à des affaires viables supplémentaires d'accéder au procès, sans affecter les décisions de condamnation ou de peine prises par les juges. Toutefois, cette visibilité ne peut se substituer aux capacités structurelles du système judiciaire. Si des affaires viables sont plus souvent poursuivies lorsque les violences faites aux femmes sont particulièrement visibles dans l'espace public, cela souligne le rôle des ressources, de la capacité administrative et des routines institutionnelles dans les décisions du parquet. Renforcer la réponse judiciaire suppose donc à la fois de transformer les perceptions sociales et d'investir dans les capacités du système judiciaire.

Transformer l'économie du *care*. Le troisième chapitre met en évidence l'organisation du travail comme un levier encore trop peu pris en compte dans les débats sur le secteur du soin et de l'accompagnement de long terme. Dans un contexte marqué par des pénuries de main-d'œuvre, une demande croissante liée au vieillissement démographique et des budgets contraints, voire en baisse, repenser l'organisation du travail peut constituer un moyen à la fois réaliste et prometteur de renforcer l'attractivité et la soutenabilité du secteur. Les résultats éclairent également un enjeu central des politiques du *care* : l'amélioration des conditions de travail dans ces métiers est parfois perçue comme coûteuse ou susceptible de se faire au détriment des bénéficiaires. L'expérimentation étudiée dans cette thèse suggère au contraire que, du moins dans ce contexte, de meilleurs emplois ne nuisent pas aux bénéficiaires et peuvent préserver la continuité et la qualité de l'accompagnement.

Plus largement, cette thèse souligne le rôle central de l'organisation institutionnelle dans la réduction des formes de vulnérabilité qui touchent particulièrement les femmes. Dans le système pénal, les institutions façonnent la possibilité que les violences soient reconnues et poursuivies. Dans le secteur du *care*, les organisations déterminent les conditions dans lesquelles un travail essentiel est réalisé, et si ces conditions soutiennent ou fragilisent le bien-être des travailleuses. Dans l'ensemble, les réponses institutionnelles jouent donc, dans différents domaines, un rôle décisif dans la reproduction ou la réduction des violences et des vulnérabilités liées au genre.

General Introduction

*« A cada minuto, de cada semana
Nos roban amigas, nos matan hermanas
Destrozan sus cuerpos, los desaparecen
No olvide sus nombres, por favor, señor presidente »*

Canción Sin Miedo
Vivir Quintana, 2020

*« Keeping things as they are brings a certain tranquility
and always creates less friction than seeking to change them. »*

Une farouche liberté
Gisèle Halimi, 2020

Violence, Vulnerability, and Institutional Responses

Gender inequalities are reflected across many spheres, including education, the labor market, political representation, household relations, and unequal exposure to violence. Among its most severe manifestations, violence against women is both a violation of human rights and a major social problem. The United Nations defines violence against women as “any act of gender-based violence that results in, or is likely to result in, physical, sexual, or psychological harm or suffering to women” (UN General Assembly, 1993). Despite growing public awareness and policy efforts, it remains highly prevalent: recent global estimates indicate that around one in three women aged 15 or older has experienced physical or sexual violence during her lifetime (WHO, 2021).

Institutional responses are central to how societies address this violence. Legal institutions determine whether violence is reported, recorded, investigated, prosecuted, and sanctioned, but progression through these stages is often uneven and uncertain. Sexual and intimate partner violence are among the most underreported crimes (Palermo et al., 2014), contributing to

the “dark figure of crime”, that is, the share of offenses that remain absent from official statistics because they are not reported to or recorded by authorities. Among cases that eventually enter the criminal justice system, many are then dropped or fail to progress through the legal process, a phenomenon commonly referred to as case attrition (Daly and Bouhours, 2010). Beyond legal definitions and evidentiary standards, progression through these stages may also depend on the social and informational contexts in which victims, police, prosecutors, and judges make decisions.

This dissertation studies these institutional responses in the French context. It first focuses on the criminal justice system, where the central issue is whether violence against women is transformed into a legal case and how these cases are subsequently handled by legal institutions. It then turns to another domain in which institutions contribute to shaping forms of vulnerability that primarily affect women: the care economy, understood here as encompassing activities related to assistance, support, and care for dependent individuals. This predominantly female sector brings together demanding jobs often characterized by difficult working conditions. It therefore constitutes another social sphere in which organizational rules shape female workers’ autonomy, mental health, job satisfaction, and retention. Although the criminal justice system and the care sector refer to distinct realities and contexts, they raise a common question: how do institutions respond to forms of violence and vulnerability that disproportionately affect women, and how are these responses shaped by social context, public attention, and work organization?

Why an Economic Approach?

The study of violence against women and gendered vulnerability is inherently interdisciplinary. Sociology, psychology, criminology, law, and public health provide essential frameworks for understanding trauma, power relations, stigma, and the institutional treatment of violence. These disciplines rely on qualitative and quantitative research that documents victims’ experiences, the social factors associated with violence, its consequences, and the ways in which institutions respond to it. Economics complements these approaches by focusing on costs, constraints, incentives, beliefs, and decision-making processes. It does so by mobilizing diverse data sources as well as statistical and econometric methods that support causal identification. Beyond this methodological dimension, this dissertation builds on the economic approach in three main ways.

A first economic lens concerns the costs of violence. The consequences of violence against

women extend far beyond the immediate harm experienced by victims. A large interdisciplinary literature documents severe and persistent effects on health, well-being, and family trajectories (Aizer, 2011; Scodellaro, 2020; Brown et al., eds, 2021; Brunton and Dryer, 2022). A growing body of economic research further shows that violence against women disrupts education, employment, and earnings (Borker, 2021; Folke and Rickne, 2022; Adams-Prassl et al., 2024; Coutolleau et al., 2024; Batut et al., 2026; Adams et al., 2026). These costs are borne not only by victims, but also by families, employers, public services, and society as a whole (Day et al., 2005). Violence against women is therefore not only a private harm or a public health issue. It is also an economic issue, because it affects human capital accumulation, labor market participation, public spending, and access to educational, professional, and economic opportunities.

A second economic lens builds on rational choice theory in the economics of crime by analyzing reporting and criminal justice decisions as forms of decision-making under uncertainty and constraints (Becker, 1968; Ehrlich, 1973). For victims, reporting violence to the police can be understood as a costly institutional choice, shaped by expected benefits and costs under uncertainty (Bowles et al., 2009; Chaussebourg et al., 2011). Reporting may offer protection, recognition, or redress, but it also entails psychological, social, financial, and legal costs, including stigma, fear of retaliation, uncertainty about the judicial process, and low expectations of institutional support. In this context, available information and social representations of violence against women may shape victims' expectations regarding the potential benefits of legal action, thereby influencing their decision whether to report the violence.

The same economic perspective also applies to legal actors. Once a case enters the criminal justice system, prosecutors and judges make key decisions under informational and resource constraints (Landes, 1971; Easterbrook, 1983). These constraints involve trade-offs over evidentiary thresholds and the risk of erroneous decisions, in a context where legal actors have limited time, resources, and attention to devote to each case. They are especially salient for crimes that often occur in private settings, with few witnesses and limited material evidence (Daly and Bouhours, 2010), making them particularly difficult to investigate and prosecute. At the same time, such constraints often leave considerable room for discretion in how cases are processed, potentially opening the door to the influence of external factors, including social norms, beliefs, and attitudes surrounding violence against women. Within this framework, the dissertation studies how legal actors respond to changes in their social and informational environment, focusing in particular on media coverage of violence against women as an indicator of the growing visibility of this issue in public debate. In doing so, it also draws on the

theoretical and empirical literature in the political economy of the media, which shows that media can exert a powerful influence on society through several behavioral and incentive-based mechanisms (DellaVigna and La Ferrara, 2015).

Finally, the dissertation places the study of vulnerabilities that particularly affect women within a broader reflection on work, organizations, and the sustainability of long-term care services. This perspective is particularly important in the home care sector, where demographic aging is rapidly increasing demand in developed countries (OECD, 2025). Meeting this demand is not only a social and public health challenge, but also a central economic issue: it requires attracting and retaining workers in a sector already under strain, yet essential to the well-being of dependent individuals. Drawing on organizational economics theory and research on autonomy, coordination, peer support, and governance, this dissertation examines how the internal design of work shapes job quality, worker retention, and the quality of services provided in this sector (Bandiera et al., 2005; Mas and Moretti, 2009; Batt and Colvin, 2011).

The French Context: Violence, Justice, and Care

France provides a particularly relevant setting for studying institutional responses to violence and gendered vulnerability. Violence against women has become a major public issue, especially since the #MeToo movement and the increased visibility of sexual violence and femicides in public debate. This shift has been accompanied by greater media coverage, collective mobilization, and policy initiatives around this issue (Cavalin et al., 2022). Yet the scale of the phenomenon remains substantial. Almost 15% of women aged 20–69 report having experienced rape, attempted rape, or another form of sexual assault at least once in their lifetime (Hamel et al., 2016). In 2021, authorities recorded 247,000 victims of sexual violence, 88% of whom were women, and 422,000 victims of intimate partner violence, 76% of whom were women (SSMSI, 2023). The health consequences are also severe: in France, 33% of adult women exposed to rape or attempted rape report having attempted suicide, compared with 6% in the general female population (Scodellaro, 2022).

The gap between the scale of violence and the formal institutional response remains large. Victimization surveys show that a minority of victims report such violence to the authorities: only one in ten victims of sexual violence files a complaint, and fewer than one in five victims of domestic violence do so (Guedj, 2017; SSMSI, 2023; Stricot, 2025). This low reporting rate limits access to legal protection and contributes to the persistence of a large dark fig-

ure of crime in France. Even when cases enter the criminal justice system, attrition remains substantial. Administrative evidence shows that a large share of sexual and intimate partner violence cases handled by prosecutors are dismissed (Stricot, 2024), often because the offense is considered insufficiently characterized. Evidentiary constraints and prosecutorial discretion therefore play a central role in shaping the legal treatment of violence against women in the French context.

The care sector represents a second key site where institutional design shapes gendered vulnerability in France. Home care is central to the French policy objective of supporting aging at home. Yet, as in many other developed countries, the sector struggles with severe recruitment and retention challenges. Its predominantly female workforce faces low wages, fragmented schedules, professional isolation, limited career prospects, physically and emotionally demanding work, and high turnover (Bailly et al., 2013; El Khomri, 2019). These difficulties affect not only workers, but also the quality and continuity of support received by elderly and dependent individuals. They have also triggered organizational transformations within French home care services, providing a unique opportunity to evaluate the effects of new forms of work organization in this sector.

Literature Gaps, Methodologies, and Contributions

This dissertation contributes to several strands of literature in economics and the social sciences on violence against women, criminal justice, media, and work organization. These topics are difficult to study empirically because the relevant decisions often occur in private, sensitive, or administratively complex settings. Victims' reporting decisions are only partially observed, judicial trajectories are difficult to reconstruct, media visibility is rarely measured at high frequency, and organizational reforms in care work are seldom evaluated experimentally. A common contribution of the dissertation is therefore to use rare and original data sources, which are difficult to access or collect, together with descriptive and causal inference methods: nationally representative victimization survey data, detailed administrative data on judicial case processing, textual data from media archives, and original survey data collected as part of a randomized field experiment. These data and methods allow the dissertation to address four gaps in the existing literature.

First, while existing research has studied the factors associated with the prevalence and reporting of violence against women, less is known about the mechanisms that lead victims to report or not report the sexual violence they experienced. Much of the available evidence

comes from small samples that are not representative of the broader population. The first chapter addresses this gap with nationally representative data from the French *Cadre de Vie et Sécurité* victimization surveys. These data remain rarely used in economics, despite providing unusually rich information on victimization, reporting behavior, and reasons for non-reporting in a sample representative of metropolitan France over a long period, from 2007 to 2019. Unlike police records, these survey data distinguish prevalence from reporting, allowing for a more accurate analysis of engagement with institutions. They therefore make it possible to study both the evolution of sexual violence reporting over time and the individual barriers that prevent victims from engaging with the justice system.

Second, while the economics of crime and justice literature has extensively studied policing, conviction, and sentencing decisions, prosecutorial discretion and the factors that influence prosecutors' decisions remain less well understood. This gap is especially important because prosecutors play a central role in selecting which cases reach trial in many judicial systems worldwide. Understanding prosecutorial decisions is particularly crucial for sexual and intimate partner violence, where evidentiary constraints are substantial and investigations are often difficult to conduct. The second chapter addresses this issue using novel administrative microdata on nearly all offenses processed by the French criminal justice system between 2012 and 2019, drawn from the *Cassiopée* case management system of the Ministry of Justice. This dissertation constitutes the first research project to use these exceptionally rich judicial microdata for quantitative research. They provide an unusually detailed view of criminal case content, processing, and trajectories, from registration to prosecutorial and sentencing decisions.

Third, existing research shows that media coverage can affect attitudes, norms, and institutional behavior, but less is known about whether short-run changes in public attention affect routine criminal justice decisions, especially before cases reach trial. The second chapter speaks to this question by linking these administrative judicial microdata to exhaustive and highly reliable data on the daily content of France's main 8 p.m. national TV newscasts, obtained from the archives of the French National Audiovisual Institute for the 2000–2021 period. This linkage makes it possible to match the timing of judicial decisions with daily variation in the media visibility of violence against women. Leveraging as-good-as-random variation in TV news coverage, the chapter examines whether routine, everyday media coverage of violence against women causally affects the judicial handling of these offenses. It thereby provides an original way to study how legal institutions respond to short-term changes in public attention.

Finally, this dissertation contributes to the literature on gender, work, and organizations, and more specifically to research on work organization in care occupations, a low-paid and predominantly female sector. Existing research highlights the role of occupational segregation, the unequal division of domestic and care responsibilities within families, and workplace constraints in sustaining gender inequality. However, causal evidence remains limited on whether changes in the internal organization of work can improve job quality and employment conditions in these occupations. The third chapter addresses this gap through a randomized evaluation of a new work organization in home care services, the first rigorous experiment of its kind to be implemented in any country or sector. Combining experimental methods, econometric causal inference tools, and original field data collected from care workers, care recipients, and informal caregivers, it provides a unique causal assessment of an organizational innovation aimed at improving working conditions in this sector.

Overview of the Chapters

Chapter 1 – Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019

The first chapter examines the determinants of reporting recent sexual violence to the police in France. Using data from the *Cadre de Vie et Sécurité* victimization surveys conducted annually from 2007 to 2019, it analyzes a nationally representative sample of 1,523 female victims who reported having experienced sexual violence in the two years preceding the survey, either outside or within their current household. The chapter estimates logit regression models to descriptively study how reporting behavior varies across victims' sociodemographic profiles and assault circumstances. It then leverages unique survey information on victims' stated reasons for not reporting to investigate the mechanisms behind these patterns, focusing on the role of rape stereotypes, fear of social stigma, and expectations regarding the judicial response.

The results show that the propensity to report sexual violence is significantly higher among victims with lower socioeconomic status and among those who sustained physical injuries. The analysis of stated reasons for non-reporting allows the chapter to reassess common explanations for these patterns. The lower reporting rate among victims of higher socioeconomic status is better explained by their greater awareness of the low likelihood of a successful trial than by a fear of social stigma. Conversely, the significant impact of physical injuries on reporting stems more from closer alignment with the "classic rape" stereotype

than from perceived prospects for winning a case based on concrete evidence. These findings highlight how social norms and stereotypical representations of violence can operate as informal barriers to justice, reinforcing the underreporting of sexual violence.

Chapter 2 – Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women

The second chapter analyzes whether media coverage of violence against women affects the criminal justice response to sexual and intimate partner violence. It combines novel administrative microdata from the French Ministry of Justice, covering nearly 800,000 cases of sexual and intimate partner violence processed by French courts between 2012 and 2019, with high-frequency data on the daily content of France’s main 8 p.m. national TV newscasts, namely the TF1 and France 2 prime-time news programs. Exploiting the quasi-random timing of news stories related to violence against women, but unrelated to the cases currently being processed, the chapter estimates the short-run causal effect of media visibility of violence against women on prosecutors’ and judges’ decisions.

The results show that media coverage of violence against women significantly influences prosecutors’ decisions, with a 2.3% increase in the prosecution rate in the week following the broadcast of such stories on television, along with a moderate increase in reporting. By contrast, media exposure has no direct effect on judges’ conviction or sentencing decisions. This occurs in a context where prosecutors largely determine case outcomes: they dismiss nearly 80% of cases, while most prosecuted cases ultimately result in conviction. Suggestive evidence indicates that this shift in prosecutions is primarily driven by strategic responses to heightened public scrutiny and accountability. The effect emerges only in the post-#MeToo period, when the estimate more than doubles, suggesting that routine, everyday media coverage of violence against women became more consequential once this violence was already a salient public issue. Crucially, the additional cases prosecuted following news coverage appear just as likely to result in conviction as other prosecuted cases. These findings suggest that, in resource-constrained environments, increased visibility of violence against women can allow additional viable cases to reach trial without compromising case quality or influencing judicial rulings, thereby strengthening the overall judicial response to such violence.

Chapter 3 – The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment

The third chapter² turns to the care economy by evaluating a new form of work organization in the French home-care sector. This rapidly expanding, predominantly female sector faces chronic labor shortages, driven by difficult working conditions, fragmented schedules, professional isolation, low wages, and high turnover. In response, many organizations have turned to the self-managed teams model. This new organization grants care workers greater autonomy over scheduling, replacements, and care planning, while potentially increasing coordination costs and administrative burdens. Using a randomized controlled trial conducted across multiple home-care organizations in France, the chapter estimates the causal effects of introducing self-organization. Within each participating service, teams were randomly assigned either to adopt the new model or to remain under the standard hierarchical organization, thereby addressing selection bias. The analysis combines original field survey data collected from home care workers, care recipients, and their informal caregivers before the intervention and again 12 to 18 months later, with detailed administrative monitoring data from the participating organizations.

The results provide robust causal evidence that self-organization significantly improves worker well-being and retention. Assignment to self-managed teams increased job satisfaction by 0.24 standard deviations and improved overall working conditions by 0.41 standard deviations, with notable improvements in work organization and scheduling. Treated workers experienced lower job strain and better mental health, reporting feeling happier and less stressed at work. These improvements in job quality also translated into a 7.8 percentage point reduction in the probability of leaving the organization. These positive outcomes for workers did not come at the expense of service quality: care recipients reported better adequacy of care time, with no detected detrimental effects on their health or overall satisfaction. These findings show that work organization can be an important lever for improving job quality and retention in long-term care, with important policy implications in a context of rising demand due to population aging.

²Co-authored with Thomas Breda (Senior Researcher, CNRS, IPP), Camille Ciriez (Junior Economist, IPP), Audrey Rain (Senior Economist, IPP), Delphine Roy (Senior Economist, IPP), and Léa Toulemon (Economic and Social Consultant, Syndex).

Lessons and Implications

Together, the chapters show that institutional responses to forms of vulnerability that particularly affect women are shaped by beliefs, visibility, and work organization. Victims' reporting decisions depend both on how they interpret the violence they experienced and on their expectations regarding the institutional response. Prosecutors' decisions can respond to shifts in public attention when evidentiary and resource constraints leave room for discretionary judgment. Finally, care workers' well-being and retention improve when organizational rules grant them greater autonomy and collective control over their work. These findings point to two main lessons: improving access to justice for victims of violence against women, and rethinking work organization in the care economy.

Strengthening the judicial response. The first two chapters show that the criminal justice response to violence against women is constrained at two key moments: before cases enter the justice system, when victims decide whether to report the violence they experienced, and after cases are recorded, when prosecutors decide whether they should proceed. Improving access to justice therefore requires action on both margins. Policies aimed at increasing reporting must address the social representations that shape victims' expectations, including stereotypical representations of sexual violence and beliefs about victims' credibility. They must also reduce the perceived and actual costs of reporting by making institutional responses more transparent, more accessible, and more likely to provide protection, recognition, or redress.

The findings also suggest that public attention can strengthen institutional accountability at the prosecutorial stage. Media visibility appears to help additional viable cases reach trial without affecting judges' conviction or sentencing decisions. Yet visibility cannot substitute for structural judicial capacity. If viable cases are more likely to proceed when violence against women is particularly visible in the public sphere, this points to the role of resources, administrative capacity, and institutional routines in shaping prosecutorial decisions. Strengthening the judicial response therefore requires both changes in social perceptions and investments in the capacity of the justice system.

Transforming the care economy. The third chapter points to workplace organization as an often overlooked policy margin in debates on long-term care. In a context marked by labor shortages, growing demand driven by population aging, and constrained or even declining budgets, rethinking work organization may provide both a realistic and promising

way to strengthen the sector's attractiveness and long-term sustainability. The findings also speak to a central concern in care policy: improving working conditions in these occupations may be seen as costly or potentially detrimental to care recipients. The experiment studied in this dissertation instead suggests that, at least in this setting, better jobs do not come at the expense of care recipients and can preserve the continuity and quality of care.

More broadly, the dissertation highlights the central role of institutional design in reducing forms of vulnerability that particularly affect women. In the criminal justice system, institutions shape whether violence is recognized and pursued. In the care sector, organizations shape whether essential work is performed under conditions that sustain or undermine workers' well-being. Overall, institutional responses in different settings therefore play a decisive role in determining whether gender-based violence and vulnerability are reproduced or reduced.

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Chapter 1

Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019

This chapter was published in *Population* in 2025 (Stricot, 2025).

Acknowledgements

This work has benefited from the support of the Agence Nationale de la Recherche through the program Investissements d’Avenir ANR-17-EURE-0001. I also acknowledges financial support from the research unit PjSE (UMR 8545) and the University Paris 1 Panthéon-Sorbonne. Access to data was granted by Prodego-Adisp. This chapter greatly benefited from discussions and helpful comments from Davi Bhering, Thomas Breda, Camille Hémet, Andreas Kotsadam, Sylvie Lambert, Emily Nix, Léa Toulemon, Hélène Périvier, Selma Walther, the editorial board of *Population*, and four anonymous reviewers. I would also like to thank the participants of the 2023 AFSE Annual Congress, the 2023 Luxembourg University Workshop on Gender and Economics, and the 2023 Paris Nanterre University Workshop on Gender Issues and Development for their valuable feedback.

Abstract

Sexual violence and its underreporting are major social problems. This study examines why victims choose—or choose not—to report recent sexual violence to the police. It uses data from the French victimization survey conducted annually from 2007 to 2019 on a large, nationally representative sample. The analysis shows that the propensity to report sexual violence is significantly higher among victims with low socioeconomic status and those who sustained physical injuries. Leveraging unique data on the reasons for not reporting, the study discusses the mechanisms through which these two structural factors operate on the reporting process. The lower reporting rate among victims of higher socioeconomic status is better explained by their greater awareness of the low likelihood of a successful trial than by a fear of social stigma. Conversely, the significant impact of physical injuries on reporting stems more from closer alignment with the “classic rape” stereotype than from perceived prospects for winning a case based on concrete evidence.

JEL classification: D91, J16, K42, Z13

Keywords: Sexual violence, Victimization survey, Crime reporting, Criminal justice, Social norms, France

1.1 Introduction

Sexual violence (SV) is a global social problem worldwide with long-term negative consequences for victims, their families, and society (WHO, 2021; Day et al., 2005). Beyond causing significant harm to victims' health and well-being (Aizer, 2011; Scodellaro, 2020; Brunton and Dryer, 2022), SV has serious economic and social costs when it comes to victims' education and employment trajectories (Borker, 2021; Brown et al., eds, 2021; Folke and Rickne, 2022; Adams-Prassl et al., 2024; Batut et al., 2026). In France, almost 15% of women and 4% of men aged 20–69 report having experienced rape, attempted rape, or another sexual assault at least once in their lifetime (Hamel et al., 2016). In 2022, this represented 247,000 victims, 88% of whom were women (SSMSI, 2023). The impact on victims' health is particularly striking: for instance, in 2015, 33% of adult women in France exposed to rape or attempted rape have attempted suicide compared with 6% in the general female population (Scodellaro, 2022).

Reporting the assault to the authorities is necessary for any police response and for obtaining legal justice and compensation for the harm suffered. Yet, sexual crimes are among the most underreported crimes (Bachman, 1998; Palermo et al., 2014). In France, victimization surveys estimate that only 1 in 10 rape victims file a complaint (Guedj, 2017). Despite significant social and legal changes surrounding SV since the #MeToo movement, reporting rates remain strikingly low in recent victimization surveys (Guedj and Zilloniz, 2022; SSMSI, 2023). This underreporting weakens incapacitation and deterrence by reducing crime detection rates, which is critical since deterrence is a key function of the criminal justice system in reducing crime (Becker, 1968; Ehrlich, 1973; Chalfin and McCrary, 2017). It may also lead to ineffective crime policy and a suboptimal allocation of resources to prevent SV.

If the harm suffered is so severe, why do so few victims seek legal redress? To answer this question, this study first reviews the literature on reporting SV to the police and theorizes a set of testable hypotheses (Section I). A large body of cross-disciplinary literature has examined various factors influencing the decision to report SV, mostly in core English-speaking countries. Yet, evidence based on large representative samples, particularly in the French context,¹ is lacking, as earlier research often used small samples (typically between 200 and 400 individuals) and targeted specific groups not representative of the entire female population, such as students or women who contacted health or victim assistance centers.

¹Vanier and Langlade (2018), who provided initial insights into rape victims' complaint behavior in France, is a notable exception. Chaussebourg et al. (2011) studied complaint behavior for personal injuries other than sexual or domestic violence in the French context.

This study's first contribution is a descriptive analysis of the characteristics associated with reporting SV to the police, thereby advancing understanding in an area that has received little empirical attention. The analysis draws on data from the French victimization survey *Cadre de Vie et Sécurité*, conducted annually from 2007 to 2019 among approximately 16,000 nationally representative households (Section II). Unlike annual administrative data on police records, survey data effectively distinguish between prevalence and reporting, allowing for a more accurate measurement and understanding of SV reporting behavior. Given the much larger proportion of women compared to men among victims of SV across all walks of life and at all ages (Hamel et al., 2016), and to avoid mixing reporting dynamics that may differ substantially by gender, the analysis is restricted to female victims. The final sample, pooling all waves, covers 1,523 female respondents who reported having experienced SV in the preceding 2 years, either outside or inside their current household. Although focusing on recent SV potentially overlooks earlier cases (e.g., during childhood) and may underestimate the phenomenon due to the practice of late reporting (Brown et al., eds, 2021), examining short-term reporting behavior remains crucial given the policy importance of quick and formal reporting. Policymakers have indeed encouraged victims to report promptly to combat SV, as cases reported quickly are more likely to lead to prosecutions (Frohmann, 1991; Pérona, 2023).

The empirical analysis uses logit regression models to investigate the relationship between victims' sociodemographics, assault circumstances, and the reporting decision (Section III). In combining characteristics of victims and assaults with unique information on reasons for not reporting, this study advances knowledge of the mechanisms that may drive these relationships, an underexamined area in the literature. Specifically, this study examines how victims' socioeconomic status and the occurrence of physical injuries shape the psychological and social mechanisms underlying the reporting process. The analysis focuses on three key pathways through which these two structural factors operate: (a) rape stereotypes; (b) fear of social stigma; and (c) expectations regarding police and judicial responses. This methodology highlights various mechanisms underlying the reporting decision, which are essential for understanding barriers to reporting and for designing policy tools to address them (Section IV).

1.2 Literature and theoretical framework

1.2.1 What is known about SV victimization reporting?

The decision to report SV to the police can be viewed as a cost-benefit calculation under uncertainty, where individuals determine a rational reporting strategy (Bowles et al., 2009; Chaussebourg et al., 2011). Formal reporting may be one way to satisfy victims' needs (Morton et al., 2023), offering several psychological and material benefits. At the start of the process, victims may seek help and protection from perpetrators. Reporting increases the likelihood of both the perpetrator to experience apprehension and the victim to obtain legal and financial reparations for the harm suffered. In some cases, going to the police is essential for gaining access to associations and social services, obtaining their support, or securing custody in divorce proceedings. Alternatively, reporting may give victims a sense of fulfilling a moral or civic duty by preventing further attacks. It can also help them be recognized as victims by society, often a vital first step in recovery after an assault.

Victims will weigh these potential benefits against the costs of reporting. Initially, reporting may incur direct or indirect monetary costs. Victims may need to take time off work to go to the police and attend subsequent proceedings, leading to potential income loss. They may also face travel costs to reach legal or medical services and incur legal fees. If they have children, they might need to arrange childcare during these appointments.

Moreover, research in social psychology has long highlighted the existence of rape myths, defined by Burt (1980) as "prejudicial, stereotyped, or false beliefs about rape, rape victims, and rapists" (p. 217). These myths serve to minimize or justify sexual aggression and can have direct psychosocial costs for victims by exposing them to blame, skepticism, and stigma (Amir, 1971; Lonsway and Fitzgerald, 1994). A corollary to these myths is the "classic rape" stereotype, involving a sudden violent attack by a stranger in a deserted public space, with physical resistance from the victim, leaving her injured (Estrich, 1987). These stereotypes remain deeply rooted in society and are embraced by victims, police, prosecutors, and judges (Stewart et al., 1996; Parratt and Pina, 2017; IPSOS, 2019).

Such stereotypes may influence police and judicial bodies to pursue cases that most closely align with rape myths (Amir, 1971; Sleath and Bull, 2017; Pérona, 2017, 2023; Le Goaziou, 2019). In addition, these stereotypes can deter victims from reporting if they do not see themselves as "real" victims. The literature has shown that these costs may decrease with aggravated cir-

cumstances of the crime that could support the victim's credibility. There are higher reporting rates for assaults involving physical violence, injuries, or weapon use (Feldman-Summers and Norris, 1984; Williams, 1984; Bachman, 1993; Du Mont et al., 2003; Allen, 2007; Wolitzky-Taylor et al., 2011; Vanier and Langlade, 2018). Depending on their socioeconomic position, the prevalence of rape myths may also lead victims to fear social recrimination and loss of social status if they report to the police (Deitz et al., 2015), due to fear of social shame or family pressure to keep the matter quiet (Le Goaziou, 2013). In this regard, the literature indicates that the likelihood of reporting rape *decreases* with education and standard of living (Lizotte, 1985; Allen, 2007; Vanier and Langlade, 2018). Lower socioeconomic status is also highly correlated with stronger ties to social services, which may act as intermediaries or incentives to report to the police (Le Goaziou, 2013; Jouanneau, 2024).

Future potential reporting costs should also be considered depending on the perpetrator's relationship to the victim. Victims who know their perpetrator may fear retaliation (Spencer et al., 2017). Domestically abused women may need to secure alternative housing and protect their children. Disclosing the assault may also have negative psychological consequences for other family members, as suggested by research on information avoidance (Golman et al., 2017). Accordingly, the literature suggests that when a victim knows her perpetrator, she is less likely to report them. The victim may want to protect the perpetrator or to keep the case private out of fear of retaliation (Gartner and Macmillan, 1995; Fisher et al., 2003; Vanier and Langlade, 2018).

Furthermore, given the uncertainty of the perpetrator's apprehension, victims' expected gains from reporting may be lower than the expected costs. Sexual crimes often remain unpunished (Le Goaziou, 2019): 83% of SV complaints examined by prosecutors between 2012 and 2019 did not lead to prosecution in French correctional and juvenile courts (Stricot, 2024), typically due to lack of material evidence, victims' altered states of consciousness, or consent-related issues (Juillard and Timbart, 2018). This uncertain payoff may be more acutely perceived by victims of higher socioeconomic status, who may have a better understanding of the criminal justice system and greater reluctance to use it, especially if the perpetrator is also from an affluent background (Le Goaziou, 2013). However, uncertainty about the likelihood of prosecution may be reduced by certain factors, such as physical injuries, which make the violence more visible and may serve as tangible evidence of the assault in the eyes of the victims (Allen, 2007).

1.2.2 Hypotheses

This study examines how two structural factors shape the reporting process: victims' socioeconomic status (SES) and the occurrence of physical injuries. Given the results of previous research, the likelihood of reporting is expected to be *lower* among victims of higher SES. The following testable hypotheses are proposed to explain this relationship, contributing to the long-standing debate on the psychological and social mechanisms behind this effect (Lizotte 1985; Vanier and Langlade 2018):

H1. Victims with higher SES report less because:

H1a. They anticipate stigma and social recrimination from their peers if they report (*high costs*).

H1b. They are more aware that the process will be highly costly for low chances of success (*low gains*).

Furthermore, based on the literature, the likelihood of reporting is expected to be *higher* among victims who suffered an assault involving physical injuries. Two potential, nonexclusive mechanisms are considered to empirically assess what drives this relationship, as debated in the literature (Williams 1984; Allen 2007):

H2. Victims who sustained physical injuries report more because:

H2a. Physical injuries align with the “classic rape” stereotype and decrease the reputational costs associated with reporting (*low costs*).

H2b. Physical injuries provide concrete evidence that may increase their chances of success in pressing charges (*high gains*).

Additional potential mechanisms are discussed in Appendix 1.G, concerning whether victims of lower SES report more due to closer connections with social services and whether physical injuries increase reporting through victims' contact with emergency medical services.

Box 1. The legal qualification and recording of sexual violence in France

Sexual violence refers to any sexual act committed without consent and can take various forms. Under French law, rape is a crime tried in criminal court, where the maximum

sentence exceeds 10 years of incarceration. It is defined as “any act of sexual penetration, of any kind, or any oral-genital act committed by a perpetrator on another person or on the perpetrator through violence, coercion, threat, or surprise” (Article 222-23 of the Penal Code). Sexual assaults other than rape constitute misdemeanors tried in correctional court, where the maximum sentence is less than 10 years. They are defined as “any sexual violation committed through violence, coercion, threat, or surprise” (Articles 222-22 and 222-27), such as touching or forcing to touch the victim’s or perpetrator’s genitals. Sexual harassment, exhibitionism, and genital mutilation are not covered in this study.

When a person files a complaint, the police must notify the prosecutor, who oversees the case and any investigation. As of January 1, 2025, the law provides that rape victims have 20 years to file a complaint if they were over 18 at the time of the offense (Law No. 2017-242), and 30 years upon reaching adulthood if they were under 18 (Law No. 2018-703). For sexual assaults, the filing period is 6 years if the victim was over 18, and 10 or 20 years if they were under 18 or under 15, respectively.

When victims report to the police, the recording of sexual violence can be affected at various points in the criminal justice process. First, instead of recording a formal complaint, the police may record victims’ statements as *mains courantes*, which document the incident’s nature and date without prosecuting the perpetrator and can be useful in a future trial. These *mains courantes* are not forwarded to the prosecutor, allowing police officers to screen out such cases (Pérona, 2018). Second, rapes can be reclassified from a crime to a misdemeanor by the police or justice system, either if rape has not been established (“requalification”) or even if it has (“dequalification”) (Cromer et al., 2017).

1.3 Data: French victimization survey

1.3.1 SV prevalence, sample representativeness, and measurement bias

This study uses data from the French victimization survey *Cadre de Vie et Sécurité* (CVS), a repeated cross-section conducted annually by the French statistical office (INSEE) from 2007 to 2019 (SSMSI, 2019). This survey covers various crimes and offenses experienced by household members in the preceding 2 years, including SV suffered by a randomly selected household

member aged 18–74. Enumerators conducted on-site interviews using a list of addresses representative of the metropolitan French population living in ordinary households, once sampling weights had been applied. The sample is therefore only representative of people living outside of institutions (such as residential care facilities, hospitals, jails, etc.), which account for a small proportion of the population but likely constitute a group highly exposed to SV (Brown et al., eds, 2021).

SV was surveyed in two stages, depending on whether it was committed by someone currently living with the respondent. The question identifying self-reported victims focused on rape or sexual assault, asking “*Has anyone forced unwanted sexual touching or intercourse on you, or attempted to do so, using violence, threats, coercion, or surprise in the past 2 years?*”² If the answer was yes, it then asked whether the violence had been rape, attempted rape, sexual touching, or another sexual assault. No additional help or information was provided to clarify what these acts entailed, leaving them open to interpretation and variation among respondents. Moreover, the survey categories did not exactly align with the legal classifications outlined in Box 1. This may have resulted in underreporting or misclassification of SV acts that respondents may not have recognized as such.

Focusing on SV only within the preceding 2 years means examining a subset of all SV cases and the reporting decision in the short to medium term. It is well established that rape and sexual assault are often reported long after the incident (Le Goaziou, 2011). Acknowledging the violence experienced and moving toward filing a complaint can take years in a victim’s resilience process (Morton et al., 2023). Additionally, according to the Virage survey on lifetime SV, most SV occurs before age 18 and thus cannot be captured by CVS due to the survey’s age restrictions (Brown et al., eds, 2021). Because recent and lifetime violence generally do not refer to the same situations, the analysis focused on a subsample that may not be representative of all victims. Despite this limitation, the data still provide insights into short-term reporting behavior for recent incidents involving adult victims. Given policymakers’ efforts to encourage prompt reporting and the higher likelihood of prosecution for swiftly reported SV cases (Frohmann, 1991; Pérona, 2023), analyzing and understanding short-term reporting behavior remains crucial. Furthermore, public policies aimed at encouraging quick formal reporting are likely more feasible for cases involving adults than for children due to the distinct nature and context of the violence experienced (CIIVISE, 2023).

²The wording was slightly different before 2017. The exact phrasing for each SV type is available in Appendix Table 1.A.1.

Regarding the data collection protocol, CVS questions about SV were administered through audio computer-assisted self-interviewing (ACASI) rather than face-to-face questionnaires. Respondents entered their answers directly into the computer without interacting with the enumerator. Self-interviewing ensures confidentiality, likely reducing social desirability and underreporting biases that are often strong with such sensitive issues (Cullen, 2020; von Russdorf et al., 2024). However, this type of interview may still have an emotional cost associated with identifying as a victim, especially among upper-class victims. Additionally, using yes/no questions that list behaviors, rather than directly asking respondents about their SV experiences, may further improve victimization reporting by preventing respondents from having to articulate the events themselves, which can be difficult and painful to recount (Brown et al., eds, 2021; Ilies et al., 2003). Finally, since the French statistical office is a well-known, trusted organization, the perceived risk of data leakage is low.

Follow-up questions asked self-reported victims about the frequency of SV and, for the most recent event, the assault's location, the nature of the attack, the perpetrator's identity, the use of physical violence or threats, the respondent's behavior, the consequences of the assault, and the actions taken by the victims. Specifically, it asked whether they had reported the assault to the police and, if not, the reasons for not doing so. The survey also collected detailed sociodemographic information about individuals and households.

1.3.2 Sample characteristics and selection

Each wave of the CVS contains approximately 16,000 observations (one per household). This study pooled data from the 13 survey waves (2007–2019), yielding an initial sample of 157,972 individuals who answered the SV section. As this study aims to analyze police reporting behavior, the sample was restricted to self-reported victims of SV. Hereafter, *victims* refer to individuals who reported having experienced violence in the 2 years preceding the survey. Likewise, *assaults* refer to self-reported assaults in the survey.

Although the survey methodology was designed to minimize bias in reporting SV victimization due to stigma or privacy concerns, identifying certain acts as SV and reporting them in a survey may still vary according to sociodemographic characteristics. For instance, young women or students may experience violence more frequently than older individuals, but their overrepresentation in the victim population could also stem from greater awareness and recognition of violence, particularly given the evolving social norms surrounding

SV among younger generations (Brown et al., eds, 2021). Likewise, the costs associated with acknowledging violence may partially explain the variations observed in SV victimization across different social statuses. Consequently, the sample of self reported victims may not be perfectly representative of the entire population of victims. It likely reflects a selection bias toward individuals who are more inclined to identify SV and recognize having experienced it.

The weighted data reveal that 1.3% of the population reported having experienced SV in the preceding 2 years (2% of women and 0.6% of men). The final sample comprises 1,523 observations, restricted to women in the context of this study: 1,201 assaulted at least once in the 2 years preceding the survey by someone not currently living with them (*outside current household*), 408 by someone currently living with them (*inside current household*), and 86 in both contexts.³

Table 1.1 defines the sociodemographic variables, and Table 1.2 reports the weighted distributions of the sample characteristics. Among women who reported experiencing SV in the preceding 2 years, most were born in France (82.9%). However, they were less often French-born than women in the general population (87.1%). They most often had a medium level of education (42.3%), which is comparable to the overall distribution in the population. They were also younger, less likely to be in a relationship, and more likely to live in densely populated areas than the total female population. The student population was overrepresented among self-reported victims of SV, with 14.4% being students compared with 6.8% in the general female population. They were also overrepresented among the unemployed and low-income households, and underrepresented among the upper class and high-income households.

Most reported assaults were perpetrated outside the current household (75.8%) and by previously known perpetrators (82.2%), with two-thirds being rapes or attempted rapes. Almost half (47.8%) were repeat assaults, and 95.1% involved male perpetrators. The perpetrator was the victim's partner or ex-partner in 32.0% of cases. In 37%–41% of cases, the perpetrator was under the influence of drugs or alcohol, or engaged in violent physical acts such as hitting, strangling, or threat with a weapon. Among self-reported victims inside their current household, the perpetrator was most often the partner (58.9%), followed by another family member (14.6%), another cohabiting person (13.4%), or the partner's parent or child (13.1%), according to the weighted data (see Appendix Table 1.A.3 for details on the context in which

³With only 331 self-reported male victims of SV, this study lacks the statistical power to draw conclusions on men's reporting behavior. Appendix 1.B nevertheless provides a descriptive overview of the male sample, highlighting several notable differences from female victims.

Table 1.1: Construction of sociodemographic variables

Category	Variable	Definition
Marital status	In a couple	In a relationship (living together or not)
	Single	Not in a relationship
Educational attainment	High education	Bachelor's or higher
	Medium education	High school diploma or equivalent
	Low education	Middle school or lower; No diploma
Activity status	Employed	Employed; Paid apprentices/interns
	Unemployed	Unemployed
	Inactive	Student, unpaid intern; Retired; Disabled; Other
Socio-occupational category	Upper class	Large business owners; Executives; Liberal professions; Intellectual and artistic professions
	Middle class	Craftspeople; Retailers; Intermediate professions; Technicians; Foremen
	Lower class	Employees; Skilled/unskilled workers; Small farmers
	Other	Never worked; Conscripts; Inactive (excluding retirees)
Household income	High	> 150% of median income
	Middle	70-150% of median income
	Low	< 70% of median income
Living area	Priority district	<i>Zone urbaine sensible (ZUS)</i> or <i>quartier prioritaire (QP)</i> , gathering the poorest urban areas in need of public intervention and urban renewal

Note: A more detailed table is provided in Appendix Table 1.A.2.

violence occurred). Outside the current household, victims were primarily assaulted by someone they knew personally (53.5%) or by sight (21.7%), rather than by an unknown perpetrator (25%). Among those known personally, the most common perpetrators were the ex-partner (35.5%), a close friend or acquaintance outside the family (30.0%), or a work colleague (9.8%). Among those known by sight, perpetrators were most often a neighbor (28.1%) or a work colleague (10.6%). Assault outside the current household mostly occurred in the victim's home (32.5%) or someone else's home (26.5%), while 13.7% took place in a public space and 6.8% at the workplace. Perpetrators are therefore overwhelmingly known to the victim, and intimate partnerships represented the highest-risk relational context. While violence was embedded in everyday social networks (e.g., home, workplace, neighborhood), private spaces dominated assault locations outside the household, consistent with the perpetrator-familiarity pattern.

The reporting rate for this recent SV is low. In the sample, only 15.6% of female victims went to the police (Table 1.2). Of those, 69.5% filed a formal complaint, 24.8% filed a *main courante* (see Box 1) and 6.7% decided against following a statement (see Appendix Table 1.A.4). The remainder of the paper analyzes the reasons for non-reporting, a behavior which includes the 6.7% of victims who went to the police but ultimately decided not to file a statement. In this analysis, reporting is measured using a dummy variable. It takes a value equal to 1 if a self-reported victim went to the police and filed a complaint or *main courante*, and 0 if

they did not go to the police or went but did not file a formal report.⁴ Overall, most victims did not report to the police, with only 13.4% doing so (14.2% for SV outside the current household and 11.8% for SV inside the current household). Nevertheless, because the focus here is on short-term reporting behavior, the analysis may miss some victims who report after survey participation, as trauma and the recognition of violence can delay reporting (Morton et al., 2023).

1.3.3 Changes in the legal and social contexts over the analysis period

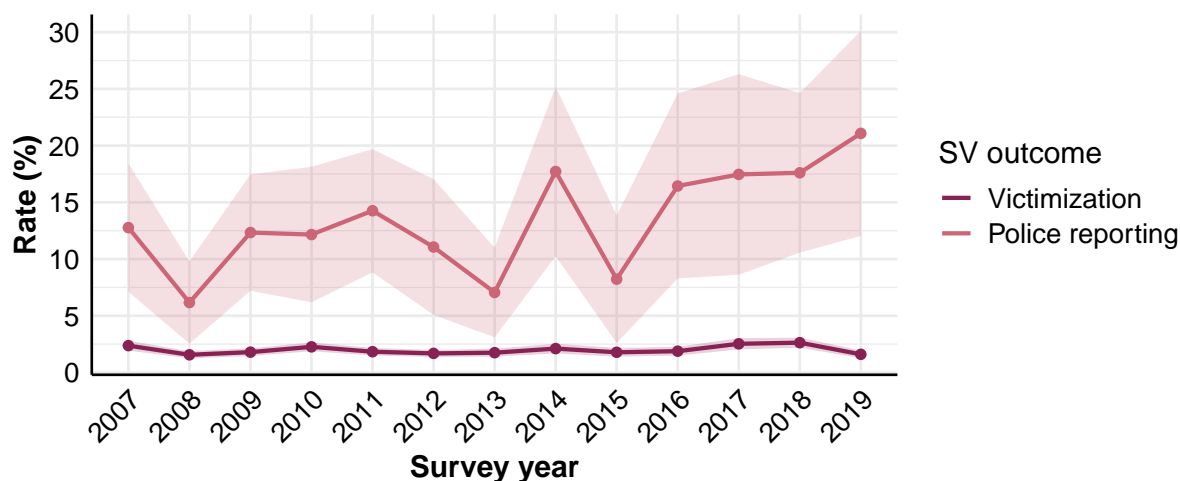
The social and legal contexts surrounding SV have undergone multiple changes since 2007. In October 2017, a series of public disclosures of sexual harassment and assault perpetrated by the film producer Harvey Weinstein gave rise to the global #MeToo movement. Using police records and leveraging #MeToo as a shock to awareness and acceptance of SV in society, studies have shown that this movement increased the reporting of sexual crimes in France and the United States (Gauthier, 2022; Levy and Mattsson, 2023; SSMSI, 2024). Alongside shifts in social awareness, police behavior has also evolved following training aimed at improving the reception and support of victims. Additionally, several legislative changes have been introduced to better protect victims, such as the *protocole-cadre* of January 2014, which redefined the rules for recording *main courante* entries in intimate partner violence cases (Pérona, 2018),⁵ as well as legislative reforms adopted in 2017 and 2018 that extended the reporting period for rape and sexual assault.

Accordingly, Figure 1.1 shows that the police reporting rate for SV tended to increase over time, particularly since 2017 and the Weinstein affair that led to #MeToo. In contrast, the victimization rate remained fairly stable (around 2%)—despite changes in societal context and norms surrounding SV—suggesting that reporting SV in the survey may be relatively unbiased. Although police reporting increased after #MeToo, the underreporting gap remains significant, with more than 80% of victims still not reporting their assault to the police in 2018-2019. Therefore, it is unlikely that these changes have substantially altered the drivers of reporting related to victims' sociodemographics and assault characteristics. To mitigate

⁴Complaints and *mains courantes* were aggregated in an attempt to understand victims' decision to engage in a formal reporting process, regardless of how the report was ultimately recorded. Victims may end up filing a *main courante* due to police influence, even if they initially intended to file a formal complaint (Pérona, 2018), so restricting the analysis to complaints only might bias our results. Appendix 1.D presents the results for victims who filed a formal complaint only, and not a *main courante*.

⁵In 2021, the French government terminated *mains courantes* in cases of intimate partner violence, but they remain in place for SV.

Figure 1.1: Change in sexual violence victimization and police reporting rates



Note: Share of women who reported experiencing sexual violence (SV) outside or inside their current household in the previous 2 years, computed from the full sample of women who answered the SV questions; and share of female victims who formally reported their assault to the police, computed from the subsample of self-reported SV victims (weighted statistics). Shaded areas represent 90% confidence intervals. Source: CVS survey data, 2007-2019.

this potential bias, time fixed effects were incorporated into the empirical strategy to account for the evolving social context and shifting attitudes toward SV over time.

More recent victimization surveys have been conducted in France since 2021, using different data collection protocols that allow for larger sample sizes. The *Genre et sécurité* (GENESE) survey, conducted in 2021, focused on gender-based and sexual violence (Guedj and Zilloniz, 2022), while the *Vécu et Ressenti en matière de Sécurité* (VRS) survey, stemming from the redesign of the CVS survey, was first conducted in 2022 (SSMSI, 2023). Some differences arise between the VRS and CVS surveys in the characteristics of the population reporting having experienced SV in the preceding year. For instance, VRS highlights a significantly higher representation of young women aged 18–24 among self-reported victims, as well as smaller disparities in victimization rates based on income level (SSMSI, 2023). While this could reflect changes in self-identification as a victim after #MeToo, the results of both surveys are not directly comparable due to methodological differences, notably in data collection methods and the questions used to measure SV victimization. However, these surveys document a similar picture of victim and perpetrator profiles, as well as the persistently low (if not lower) rate of adult female victims formally reporting recent SV to the police, even after #MeToo (Guedj and Zilloniz, 2022; SSMSI, 2023).

1.4 Empirical methodology

1.4.1 Reporting determinants

The empirical strategy applied the following specification to the full sample of self-reported female victims:

$$Y_{ijtr} = \alpha + \beta S_i + \delta X_i + \gamma Z_j + \mu_t + \phi_r + \epsilon_{ij} \quad (1)$$

The dependent variable Y_{ijtr} is a dummy variable equal to 1 if victim i living in region r at survey time t reported the assault j to the police, and 0 otherwise. Since education, employment status, socio-occupational category, and household income are highly correlated (see Appendix 1.C), a new variable was introduced that aggregates them into a socioeconomic status (SES) index S_i . The index was constructed by standardizing the individual scales and using principal component analysis to account for correlations between the variables and to allow them to have different weights. The resulting index was normalized to a mean of 0 and a standard deviation of 1, and was constructed to increase with the individual's SES. X_i is a vector of other sociodemographic characteristics, including age (log); relationship status; whether the victim was born in France; number of household members; size of the urban unit (rural areas, less than 100,000 inhabitants, more than 100,000 inhabitants, Paris); and whether the victim lived in a priority district.

Z_j is a vector of assault characteristics including type of SV (inside or outside the current household, or both); type of assault (rape, attempted rape, other sexual assault, or not specified); number of occurrences (once or more than once); number of perpetrators (one or more); victim–perpetrator relationship (partner/ex-partner, other known perpetrator, or unknown); and whether the assault caused physical injuries (beyond those associated with SV). When victims were sexually assaulted both inside and outside their current household, the more serious assault in terms of criminal liability—rape, followed by attempted rape, then other sexual assault—was selected to avoid double-counting victims in the analysis. The analysis also controlled for survey-year fixed effects μ_t to eliminate unobserved time-varying shocks that affect all individuals simultaneously, such as changes in laws or attitudes regarding SV over time. Similarly, ϕ_r represents region fixed effects that absorb any unobserved time-invariant variation in the outcome variable caused by factors that vary across regions, such as different

norms or access to police and justice services.⁶

This methodology highlights correlations but not causalities, since much of the reporting behavior is likely explained by unobserved factors correlated with some explanatory variables, such as psychological reasons or personality traits.⁷ The main specification is a logit model, which is more suitable for modeling probabilities. To interpret the results, the estimated coefficients are presented as average marginal effects of the explanatory variables on the probability of reporting.⁸

1.4.2 Reasons for not reporting

The survey asked respondents who had experienced SV but either did not go to the police or, having gone, did not file a report, to state why, offering six possible reasons. Since only 13.4% of victims reported to the police on average, many were asked this question ($N = 1,228$).⁹ Because the six reasons were not mutually exclusive (i.e., respondents could choose multiple reasons), multiple correspondence analysis was used to transform the correlated response options into new, less correlated variables while minimizing information loss. Figure 1.2 illustrates the results of this analysis. Based on the variables' position along the two dimensions, the analysis identified three main clusters of reasons for not reporting recent SV to the police. Although these clusters are not mutually exclusive either, grouping the reasons in this way helps reduce overlap between them and reveals distinct patterns. These patterns align with three potential psychological and social mechanisms underlying the reporting decision:

1. *Not serious assault* (equals 1 if the victim mentioned that it was not serious, and 0 otherwise), which likely reflects the influence of rape stereotypes (mentioned by 44.4% of non-reporting victims);
2. *Not worth reporting* (equals 1 if the victim said she preferred to find another solution or thought reporting would have been useless, and 0 otherwise), which aligns with expectations regarding police or judicial responses (89.7%);

⁶These region fixed effects divide metropolitan France into eight large zones and are the smallest geographical level available in the public data version for confidentiality reasons.

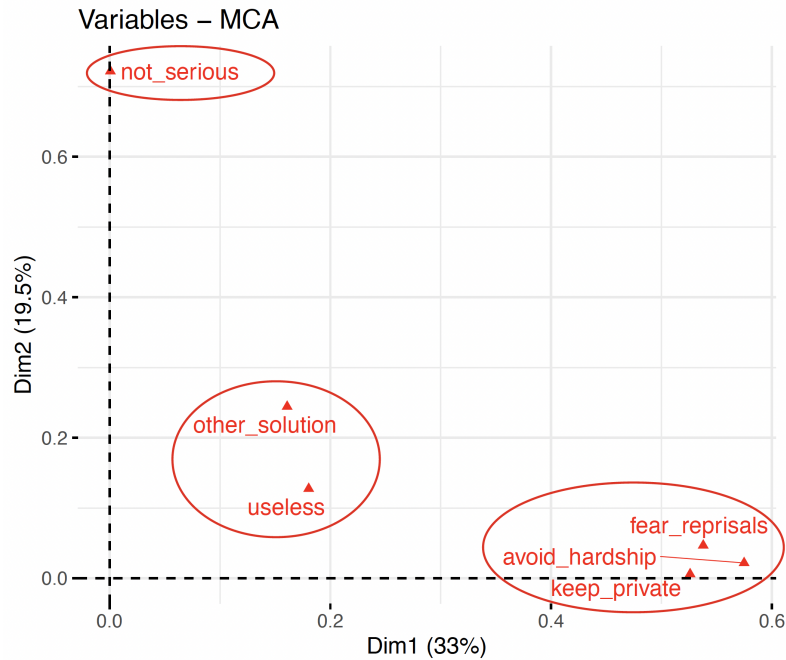
⁷For instance, victims may be deterred from reporting SV to the police due to shame, psychological trauma, or manipulation by perpetrators, as documented in psychology.

⁸Appendix 1.E reports the main results using a linear probability model.

⁹Respondents could answer yes or no, or not answer, with 3%–5% missing values due to non-response, depending on the question.

3. *Fear of reporting consequences* (equals 1 if the victim said she wanted to keep the matter private, was afraid of retaliation or revenge from the perpetrator, or wished to avoid further hardship, and 0 otherwise), which partly reflects fear of social stigma (71.9%).

Figure 1.2: Multiple correspondence analysis



Note: Female victims of sexual violence outside or inside their current household in the preceding 2 years, who did not report it to the police. The variables are “To prevent this from becoming known” (*keep_private*), “Preferred to find another solution” (*other_solution*), “For fear of reprisals” (*fear_reprisals*), “Would have been useless” (*useless*), “It was not serious” (*not_serious*), “To avoid further hardship” (*avoid_hardship*).

Source: CVS survey data, 2007-2019.

To highlight the mechanisms underlying the reporting decision, the same logit specification as in Equation 2.1 was estimated, using the clusters of reasons for not reporting as new binary dependent variables in the subsample of self-reported victims who answered these questions. A potential concern with this analysis is sample selection: reasons for not reporting are only observed among victims who chose not to report, which is likely correlated with various sociodemographic and assault characteristics. These observable characteristics were included in the empirical specification to mitigate this bias. Moreover, since only 13.4% of victims filed a report, this selection bias is likely small.

1.5 Results

1.5.1 Role of physical injury and the victim's SES

Table 1.3 presents the estimation results of Equation 2.1 using logit regressions on the reporting decision. Column 1 includes only sociodemographic variables, Column 2 only the type of SV suffered, Column 3 the other assault variables, and Column 4 includes all variables.

In line with H1, the victim's SES had a significant negative effect on short-term SV reporting. Column 1 shows that a one-standard-deviation increase in the SES index was associated, on average, with a 4.4 percentage-point decrease in the reporting propensity, holding other factors constant. This correlation is statistically significant at the 1% confidence level. The coefficient decreased slightly to 3.2 percentage points when controlling for assault characteristics in Column 4 but remains precisely estimated. With an average reporting rate of 13.4%, this represents a notable 24% drop in the reporting propensity. This gradient may be partly driven by compositional differences in the context of violence across socioeconomic groups. According to Appendix Table 1.A.5, higher-SES victims are more likely to be assaulted at the workplace (8.8% vs. 4.9%) and by a work colleague (14.8% vs. 3.9% among perpetrators known personally), which may create specific reputational costs that discourage reporting. By contrast, lower-SES victims are more often assaulted by an ex-partner (42.8% vs. 31.2%) or a family member (11.6% vs. 6.0%), contexts associated with distinct barriers to reporting such as economic dependence or fear of reprisals. These patterns should nonetheless be interpreted with caution, as small cell sizes for some subcategories limit the precision of these estimates. Furthermore, Appendix Table 1.F.4 shows that the negative effect of SES on reporting is more pronounced among victims who are in a couple, suggesting that the interaction between socioeconomic status and relationship status may partially account for this gradient.

Other factors stand out, such as the victim's age, urban unit size, and the number of household members, all of which are positively associated with the likelihood of reporting. Consistent with the literature, victims were also significantly less likely to report recent SV to the police when assaulted by a partner/ex-partner or another known perpetrator compared to an unknown perpetrator. This result appears specific to SV, as knowing the perpetrator usually increases the likelihood of reporting other types of individual victimization (Chausebourg et al., 2011).

Regarding assault characteristics, sustaining physical injuries strongly correlated with the

decision to report SV in the short run, consistent with H2. Column 4 shows that physical injuries were associated, on average, with a 14.5 percentage-point increase in the reporting propensity, all else being equal, significant at the 1% level. This represents a sizable 108% increase in reporting compared to the small baseline. This factor appears to have a greater influence than the severity of the crime itself: the average reporting rate for rape or attempted rape without physical injuries was 9.2% compared with 16.6% for sexual assault other than rape/attempted rape with physical injuries. Attempted rape, which is more likely to involve physical injuries, was also associated with a higher reporting propensity than rape (Table 1.3). Interestingly, this contrasts with the reporting determinants for other types of individual victimization, where the primary motivation for reporting is the severity of the crime (Chaussebourg et al., 2011). However, the effect of physical injuries on reporting did not meaningfully vary across socioeconomic groups because the interaction with SES was not statistically significant (see Appendix E).

Furthermore, compared to Column 2, Column 3 suggests that when considering equivalent crimes—that is, keeping all other assault characteristics constant—victims were significantly more likely to report to the police if they were assaulted by someone not currently living with them. One possible reason is the greater stigma associated with marital rape, which could lead victims of domestic violence to report only physical violence to the police, even if they have also experienced SV. Higher reporting costs due to financial dependence among domestically abused women may also play a role. Moreover, the initial negative effect being in a couple had on reporting largely disappeared after controlling for physical injuries and, to a lesser extent, whether the assault occurred inside or outside the current household. This suggests that partnered victims report less often due to differences in the composition of assault types and the likelihood that the assault involved physical injuries. However, there was no further heterogeneity between victims assaulted outside or inside their current household. Apart from the frequency of assault, most observable characteristics correlated with the decision to formally report SV committed outside or inside the current household in the same way (see Appendix 1.F).

Table 1.2: Sample characteristics (2007-2019)

	Sexual violence victims			All female respondents		
	<i>N</i> = 1,523	Missing values (%)	Frequency (%)	<i>N</i> = 87,651	Missing values (%)	Frequency (%)
Sociodemographics						
Aged 18–29	1523	0.0	36.0	87651	0.0	20.3
Aged 30–54	1523	0.0	46.9	87651	0.0	48.5
Aged 55–76	1523	0.0	17.1	87651	0.0	31.2
Born in France	1523	0.0	82.9	87651	0.0	87.1
In a couple	1523	0.0	49.2	87651	0.0	67.8
<i>Educational attainment</i>						
High education	1521	0.1	25.6	87465	0.2	29.4
Medium education	1521	0.1	42.3	87465	0.2	41.4
Low education	1521	0.1	32.0	87465	0.2	29.2
<i>Employment status</i>						
Employed	1523	0.0	49.1	87651	0.0	56.0
Unemployed	1523	0.0	12.5	87651	0.0	6.3
Student	1523	0.0	14.4	87651	0.0	6.8
Other inactive	1523	0.0	24.0	87651	0.0	31.0
<i>Socio-professional category</i>						
Upper class	1521	0.1	8.7	87493	0.2	15.4
Middle class	1521	0.1	17.4	87493	0.2	20.3
Lower class	1521	0.1	49.2	87493	0.2	50.4
Other category	1521	0.1	24.7	87493	0.2	13.9
<i>Household income</i>						
High (> 150%*median)	1469	3.5	20.6	82933	5.4	34.0
Middle (between 70–150%*median)	1469	3.5	43.8	82933	5.4	46.3
Low (< 70%*median)	1469	3.5	35.6	82933	5.4	19.7
Household size > 2	1523	0.0	47.5	87651	0.0	49.4
Living in a priority district	1523	0.0	10.1	87651	0.0	7.1
<i>Size of the urban unit</i>						
Countryside	1522	0.1	16.0	87645	0.0	23.1
< 100,000 inhabitants	1522	0.1	26.8	87645	0.0	30.2
> 100,000 inhabitants	1522	0.1	37.4	87645	0.0	29.7
Paris urban unit	1522	0.1	19.8	87645	0.0	17.0
Assault characteristics						
Outside current household	1523	0.0	75.8			
Inside current household	1523	0.0	29.6			
Rape	1434	5.8	36.2			
Attempted rape	1431	6.0	29.6			
Other sexual assault	1430	6.1	47.2			
Occurred twice or more	1452	4.7	52.2			
Several perpetrators	1416	7.0	6.6			
Only male perpetrators	1243	18.4	95.1			
Unknown perpetrator	1454	4.5	17.8			
Victim's partner or ex-partner	1401	8.0	32.0			
Other known perpetrator	1454	4.5	51.3			
Perpetrator drugged or drunk	1352	11.2	37.3			
Physical violence or weapon threat	1391	8.7	41.3			
Physical injury	1462	4.0	37.3			
Legal actions taken						
Went to the police	1523	4.4	15.6			
Made a report	1523	5.1	13.4			
Reasons for not reporting						
To keep it private	1523	21.6	49.4			
Preferred to find another solution	1523	21.7	71.0			
For fear of reprisals	1523	21.9	38.6			
Would have been useless	1523	21.3	73.7			
It was not serious	1523	22.5	44.6			
To avoid further hardship	1523	22.0	62.5			

Interpretation: Thirty-six percent (36%) of sexual violence victims were aged 18–29 compared with 20.3% of the total number of female respondents.

Note: Characteristics of the sample of women who reported experiencing sexual violence outside or inside their current household in the past 2 years, compared with the total population of women. The column headings indicate the total unweighted number of observations: *N* = 1, 523 victims of SV, *N* = total number of female respondents. The columns “Number of observations” provide the number of observations used to calculate the weighted frequency (in %) within each group. The columns “Missing values (%)” provide the percentage of missing values in relation to the total number of observations. Values are missing as a result of 1) lack of response to certain questions linked to sociodemographic or assault characteristics, or 2) not all respondents being asked each question (for example, the questions on legal actions taken or reasons for not reporting).

Source: CVS survey data, 2007-2019.

Table 1.3: Determinants of the decision to report sexual violence to the police

	<i>Dependent variable: Made a report (0/1)</i>			
	(1)	(2)	(3)	(4)
Age (log)	0.079*** (0.026)			0.073*** (0.025)
Born abroad	0.002 (0.031)			0.016 (0.031)
In a couple	-0.056*** (0.025)			-0.019 (0.025)
Socioeconomic status index	-0.044*** (0.013)			-0.032*** (0.013)
Size of the urban unit (<i>Ref: Rural areas</i>)				
<i>Less than 100,000</i>	0.053*** (0.025)			0.042* (0.027)
<i>More than 100,000</i>	0.054*** (0.025)			0.047** (0.027)
<i>Paris urban unit</i>	0.341*** (0.206)			0.260* (0.179)
Priority district	0.014 (0.028)			-0.002 (0.029)
Number of household members	0.019*** (0.009)			0.022*** (0.008)
Type of SV (<i>Ref: Inside current household</i>)				
<i>Outside current household</i>		0.044* (0.028)	0.067*** (0.034)	0.070*** (0.032)
<i>Both contexts</i>		0.062 (0.053)	0.037 (0.046)	0.032 (0.045)
Assault type (<i>Ref: Rape</i>)				
<i>Attempted rape</i>			0.064* (0.039)	0.058* (0.036)
<i>Other sexual assault</i>			-0.052* (0.035)	-0.055** (0.031)
<i>Not specified</i>			0.002 (0.054)	0.016 (0.057)
Several perpetrators			-0.028 (0.045)	-0.046 (0.046)
Victim-perpetrator relationship (<i>Ref: Unknown perpetrator</i>)				
<i>Victim's partner or ex-partner</i>			-0.064* (0.048)	-0.070* (0.048)
<i>Other known perpetrator</i>			-0.078*** (0.040)	-0.078** (0.041)
Occurred twice or more			-0.011 (0.023)	-0.025 (0.022)
Physical injuries			0.159*** (0.024)	0.145*** (0.021)
Year and region fixed effects	Yes	Yes	Yes	Yes
Observations	1,396	1,445	1,373	1,329
Pseudo R-squared	0.08	0.03	0.13	0.17

Note: Logit-estimated coefficients (average marginal effects) from Equation 2.1 for the reporting propensity. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years.

Source: CVS survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.5.2 Low expectations regarding police and judicial responses and better alignment with the “classic rape” stereotype

The effect of the victim’s SES and physical injuries on reporting behavior raises questions about underlying mechanisms. Table 1.4 addresses these by presenting logit estimates from Equation 2.1, analyzing factors associated with the clusters of reasons for not reporting recent SV.

Among victims who did not report their assault to the police, a one-standard-deviation increase in the SES index was positively associated with the propensity to consider reporting not worth it (a 2.1% increase over baseline) and negatively associated with the propensity to fear reporting consequences (a 5.4% decrease). The first relationship suggests that victims with higher SES perceive lower expected benefits from reporting, as they are more likely to be aware of the low likelihood of prosecution following police and judicial responses. They may have alternative resources and know that the process will be highly costly—both materially and psychologically—with low chances of success, aligning with H1b. However, if higher-SES victims were avoiding reporting due to social recrimination costs (H1a), we would expect a positive relationship with fear of reporting consequences, which likely reflects concerns about reputational damage. Instead, the correlation was negative, suggesting that the lower reporting propensity among higher-SES victims stems from a lack of perceived benefits rather than high social recrimination costs.

Additionally, Table 1.4 highlights a negative relationship between the occurrence of physical injuries and the propensity to consider the assault as not serious (a 43.6% decrease over baseline), and a positive relationship with the fear of reporting consequences (a 34.2% increase). While one might expect tangible evidence to improve a victim’s chances of success when reporting (H2b), there was no negative correlation between physical injury and a victim’s doubts about the benefits of reporting (*not worth reporting*). In fact, physical injury was even *positively* associated with the perception that reporting was not worth it (although the relationship was not statistically significant), contradicting H2b. This suggests that victims’ reporting decisions are not primarily driven by the belief that strong evidence will strengthen their case. Instead, the lower likelihood of considering the assault as not serious when physical injuries are present suggests that physical injury may enhance victims’ credibility and reduce the reputational costs of reporting, assuming that perceived seriousness bolsters credibility. This finding further supports H2a and the internalization of dominant

narratives about what constitutes a “real” sexual assault, since assaults involving physical injuries are more likely to align with the “classic rape” stereotype (Estrich, 1987). Therefore, victims who sustained physical injuries appear to report more due to lower perceived social discrimination costs rather than higher expected benefits.

Table 1.4: Impact of SES and physical injuries on the reasons for not reporting sexual violence

	<i>Dependent variables: Cluster of reasons (0/1)</i>		
	Not serious assault	Not worth reporting	Fear of reporting consequences
	(1)	(2)	(3)
Socioeconomic status index	0.011 (0.019)	0.019** (0.011)	−0.039*** (0.016)
Physical injuries	−0.192*** (0.037)	0.021 (0.022)	0.246*** (0.040)
Other controls	Yes	Yes	Yes
Year and region fixed effects	Yes	Yes	Yes
Observations	1,097	1,097	1,097
Pseudo R-squared	0.09	0.17	0.18

Note: Logit-estimated coefficients (average marginal effects) from Equation 2.1 for the clusters of reasons for not reporting. The three clusters are not mutually exclusive: victims could fall into more than one category, but grouping the correlated response options into these clusters helps reduce overlap and highlight distinct patterns. Other controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim–perpetrator relationship, presence of physical injuries. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence (SV) outside or inside their current household in the preceding 2 years who did not report SV to the police.

Source: CVS survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Additional mechanisms that could explain how these two factors affect the reporting decision include the likelihood of victims’ contact with social services or emergency medical services. The data indicate that lower-SES victims were more likely to discuss their situation with social services, while sustaining physical injuries increased the likelihood of seeking emergency care, and both actions were positively associated with reporting to the police. However, as detailed in Appendix 1.G, incorporating variables for these other actions taken by

the victim in the main specification on police reporting is challenging due to potential reverse causality. Reporting to the police may influence whether a victim contacts social services or emergency medical services. Since the survey did not determine the sequence of events, this complicates drawing firm conclusions about these additional channels. Importantly, controlling for these other actions in the model did not qualitatively alter the effect of victims' SES and physical injuries on the reporting decision.

Overall, the low perceived benefits and lack of incentive to report—even when solid evidence exists—suggest that expectations about police and judicial responses strongly influence the reporting process. This is supported by the finding that nearly 90% of non-reporting victims believed reporting would not have been worthwhile, especially in contexts where SV perpetrators are rarely prosecuted (Le Goaziou, 2019; Stricot, 2024) and victims' treatment by police is frequently criticized (Gauthier et al., 2025). Appendix 1.H further examines how perceptions of the police and justice system affect the decision to formally report recent SV by analyzing questions on satisfaction with these institutions. It shows that satisfaction with the judicial system significantly reduces the propensity to think that reporting would not have been worthwhile. However, given the limitations of this analysis—including the fact that satisfaction measures capture only general attitudes rather than SV-specific expectations and may be shaped by prior interactions with the police or justice system—these findings should be considered suggestive rather than strong evidence of how perceptions of the justice system affect the reporting decision.

1.6 Conclusion

SV and its underreporting to formal authorities are major public policy issues. While formal reporting is not the only way to address victims' needs (Morton et al., 2023), it is a crucial first step in understanding the extent of the problem and designing effective policies to prevent such violence. Using data from the French victimization survey, this article complements previous studies on the factors influencing the decision to report SV to the police, focusing on the short-term reporting behavior of adult female self-reported victims. In a context where only 13.4% of victims formally report sexual assaults experienced within the preceding 2 years, our findings highlight two key factors shaping the reporting process through distinct psychological and social mechanisms.

First, the reporting propensity significantly decreases with the victim's SES. To understand the main mechanism behind this effect, this study examined the role of fear of social stigma and expectations regarding the police and judicial responses. The findings suggest that higher-SES victims report less frequently primarily because they are more aware of the limited benefits of pressing charges, rather than because of fear of social recrimination. Second, sustaining physical injury is the strongest predictor of reporting SV to the police. The analysis investigated whether this relationship is driven by rape stereotypes or expectations regarding the police and judicial responses. The results indicate that victims who sustained physical injuries report more frequently because their experiences align more closely with the "classic rape" stereotype, reducing reputational costs, rather than because of increased expectations of successful reporting based on tangible evidence.

These findings suggest that policymakers should address harmful social norms surrounding SV, for instance, through awareness campaigns that challenge stereotypical representations of the "classic rape". Equally important is addressing victims' low expectations of police and judicial responses, which could be achieved through reforms aimed at increasing transparency and responsiveness to victims' needs.

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Appendix to Chapter 1

Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019

1.A Data and descriptive statistics

In the CVS survey, SV is assessed in two stages, depending on whether it was committed by someone currently living with the respondent. The most recent survey question identifying self-reported victims focuses on rape or sexual assault, asking: “*Has anyone forced unwanted sexual touching or intercourse on you, or attempted to do so, using violence, threats, coercion, or surprise in the preceding 2 years?*”. However, as highlighted in Table 1.A.1, the wording differed slightly before 2017 across the two modules.

Table 1.A.1: Formulation of the survey questions regarding sexual violence committed outside or inside the current household

	Before 2017	From 2017
SV outside current household	Apart from the people who currently live with you, has anyone ever forced you to endure sexual touching or intercourse against your will, or attempted to do so?	Apart from the people who currently live with you, has anyone ever forced unwanted sexual touching or intercourse on you, or attempted to do so, using violence, threats, coercion, or surprise?
SV inside current household	Apart from these episodes of violence, has anyone currently living with you ever forced unwanted sexual touching or intercourse on you using violence, threats, coercion, or surprise?	Apart from these episodes of violence, has anyone currently living with you ever forced unwanted sexual touching or intercourse on you, or attempted to do so, using violence, threats, coercion, or surprise?

Note: Before 2017, the questions related to sexual violence in the extra-household and intra-household SV module were not phrased in the same way.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.A.2: Construction of sociodemographic variables

Category	Variable	Definition
Marital status	In a couple	In a relationship, living in the same dwelling or not
	Single	Not in a relationship
Educational attainment	High education	Doctorate, Master's Degree, Bachelor's Degree or equivalent; University Technical Diploma (DUT), Higher Technician's Certificate (BTS) or equivalent; Paramedical and social diploma equivalent to bac+2; DEUG
	Medium education	General/Technological/Vocational Baccalaureate or equivalent; Certificate of Professional Competence (CAP), Vocational Training Certificate (BEP) and equivalent
	Low education	Middle School Diploma (DNB,BEPC); Primary School Certificate; Other recognized diploma; No diploma
Activity status	Employed	Employed; Paid apprentices and interns
	Unemployed	Unemployed
	Inactive	Student, unpaid intern; Retired; Housewife/househusband; Inactive due to disability; Other inactive status
Socio-occupational category	Upper class	Business owners with ≥ 10 employees; Liberal professions; Public sector executives; Intellectual and artistic professions; Corporate executives; Former executives and intermediate professions; Large-scale farmers
	Middle class	Craftspeople; Retailers; Intermediate professions in education, healthcare, public service, and similar fields; Intermediate administrative and commercial professions in businesses; Technicians; Foremen supervisors; Medium-scale farmers; Former farmers, craftsmen, retailers or business owners
	Lower class	Public sector employees; Administrative employees in businesses; Commercial employees; Personnel in direct services to individuals; Skilled workers; Unskilled workers; Agricultural workers; Small-scale farmers; Former employees and workers
	Other	Unemployed individuals who never worked; Conscripted military personnel; Various inactive individuals (excluding retirees)
Household income	High	Above 150% of the median income (computed for each survey wave)
	Middle	Between 70% and 150% of the median income
	Low	Below 70% of the median income
Living area	Priority district	<i>Zone urbaine sensible</i> (ZUS) or <i>quartier prioritaire</i> (QP), gathering the poorest urban areas in need of public intervention and urban renewal

Note: Author's elaboration on *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.A.3: Incident locations and perpetrator familiarity by type of sexual violence

	Outside current household			Inside current household		
	#	Prop.	(SE)	#	Prop.	(SE)
Occurred at home	443	0.325	(0.017)			
Occurred at someone else's home	263	0.265	(0.017)			
Occurred at workplace	72	0.068	(0.010)			
Occurred in a public space	137	0.137	(0.013)			
Occurred in another place	212	0.205	(0.015)			
Unknown perpetrator	275	0.250	(0.016)			
Known perpetrator	849	0.750	(0.016)			
Perpetrator known by sight	240	0.217	(0.015)			
<i>Known by sight:</i> Neighbor	74	0.281	(0.034)			
<i>Known by sight:</i> Work colleague	27	0.106	(0.021)			
<i>Known by sight:</i> Other	141	0.612	(0.037)			
Perpetrator known personally	607	0.535	(0.018)			
<i>Known personally:</i> Ex-partner	238	0.355	(0.024)			
<i>Known personally:</i> Family member	45	0.083	(0.014)			
<i>Known personally:</i> Close person	162	0.300	(0.024)			
<i>Known personally:</i> Work colleague	52	0.098	(0.015)			
<i>Known personally:</i> Other	104	0.164	(0.018)			
Spouse/partner				191	0.589	(0.035)
Partner's parent				22	0.092	(0.023)
Partner's child				24	0.039	(0.010)
Other family member				61	0.146	(0.023)
Household cohabitant				54	0.134	(0.025)

Note: #: number of victims in each category. Prop.: weighted proportion of victims in each category and standard errors in parentheses.
Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.A.4: Legal remedies undertaken by SV victims by type of sexual violence

	Outside current household		Inside current household		Total (in either context)	
	#	Prop. (SE)	#	Prop. (SE)	#	Prop. (SE)
Went to the police	191	0.160 (0.014)	72	0.147 (0.024)	254	0.156 (0.012)
Filed a formal complaint	131	0.725 (0.036)	44	0.619 (0.053)	169	0.695 (0.034)
Filed a <i>main courante</i>	38	0.223 (0.033)	14	0.473 (0.041)	51	0.248 (0.034)
Renounced filing a statement	12	0.052 (0.018)	4	0.296 (0.038)	16	0.067 (0.019)
Went to the police and filed a report	169	0.142 (0.013)	58	0.118 (0.022)	218	0.134 (0.011)

Note: #: number of victims in each category. Prop.: weighted proportion of victims who undertook a given legal action and standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.A.5: Incident locations and perpetrator familiarity by type of sexual violence and socioeconomic status

	Outside current household						Inside current household					
	Below median SES			Above median SES			Below median SES			Above median SES		
	#	Prop.	(SE)	#	Prop.	(SE)	#	Prop.	(SE)	#	Prop.	(SE)
Occurred at home	230	0.344	(0.025)	206	0.323	(0.023)						
Occurred at someone else's home	143	0.310	(0.025)	112	0.222	(0.024)						
Occurred at workplace	22	0.049	(0.014)	47	0.088	(0.014)						
Occurred in a public space	59	0.117	(0.018)	72	0.151	(0.019)						
Occurred in another place	85	0.180	(0.022)	117	0.217	(0.020)						
Unknown perpetrator	126	0.227	(0.021)	141	0.275	(0.023)						
Known perpetrator	407	0.773	(0.021)	416	0.725	(0.023)						
Perpetrator known by sight	119	0.231	(0.023)	113	0.206	(0.021)						
<i>Known by sight:</i> Neighbour	48	0.362	(0.052)	23	0.173	(0.035)						
<i>Known by sight:</i> Work colleague	7	0.078	(0.029)	18	0.136	(0.033)						
<i>Known by sight:</i> Other	65	0.559	(0.055)	72	0.691	(0.045)						
Perpetrator known personally	286	0.542	(0.026)	303	0.523	(0.025)						
<i>Known personally:</i> Ex-partner	123	0.428	(0.035)	111	0.312	(0.031)						
<i>Known personally:</i> Family member	30	0.116	(0.023)	15	0.060	(0.017)						
<i>Known personally:</i> Other close person	69	0.266	(0.033)	86	0.315	(0.034)						
<i>Known personally:</i> Work colleague	12	0.039	(0.013)	38	0.148	(0.024)						
<i>Known personally:</i> Other	49	0.150	(0.025)	50	0.165	(0.024)						
Spouse/partner							81	0.552	(0.048)	99	0.628	(0.041)
Partner's parent							12	0.121	(0.030)	9	0.049	(0.016)
Partner's child							16	0.057	(0.018)	8	0.030	(0.013)
Other family member							26	0.157	(0.038)	35	0.165	(0.032)
Household cohabitant							26	0.113	(0.025)	27	0.128	(0.030)

Note: #: number of victims in each category. Prop.: weighted proportion of victims in each category and standard errors in parentheses. Statistics are reported separately for victims below and above the median socioeconomic status (SES) index.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

1.B Self-reported male victims of sexual violence

Given the low number of self-reported male victims and to avoid pooling reporting dynamics that may differ substantially by gender, the main analysis focuses on female victims only. Nevertheless, this appendix provides a descriptive overview of the male victim sample.

Sociodemographic profile. Table 1.B.1 shows that among men who reported experiencing SV in the preceding 2 years ($N = 331$), most were born in France (68.5%), though this share is markedly lower than in the general male population (86.5%). Male victims were more likely to have low educational attainment (41.7% vs. 26.4% in the general male population), to belong to lower socio-professional categories (52.8% vs. 44.8%), and to be overrepresented among low-income households (28.0% vs. 16.0%), making them markedly more socioeconomically disadvantaged than female victims.

Assault characteristics. Most reported assaults occurred outside the current household (81.4%), a higher share than among female victims (75.8%). Unlike female victims, rape and attempted rape were less prevalent relative to other sexual assault, the perpetrator was far less often the victim's partner or ex-partner (12.2% vs. 32.0%), and male victims were more likely to face multiple perpetrators (19.2% vs. 6.6%) or an unknown perpetrator (28.6% vs. 17.8%). Turning to perpetrator familiarity and assault location (Table 1.B.2), outside the current household, male victims were more likely than female victims to face an unknown perpetrator (37.5% vs. 25.0%) and less likely to be assaulted by someone known personally (39.6% vs. 53.5%), with close friends or acquaintances being the most common personally known perpetrator (40.3% vs. 30.0%) rather than the ex-partner (11.9% vs. 35.5%). Within the household, the partner remained the most common perpetrator (45.1% vs. 58.9%), though the partner's parent accounted for a notably larger share (23.0% vs. 9.2%). Assaults were less concentrated in private homes and more likely to occur in another place (28.8% vs. 20.5%), consistent with the higher prevalence of unknown perpetrators.

Legal actions taken. The reporting rate among male victims is similarly low: only 14.3% went to the police and 11.8% filed a formal report (Table 1.B.3). Among those who did report, male victims were somewhat less likely than their female counterparts to file a formal complaint (53.8% vs. 69.5%) and more likely to file a *main courante* (40.7% vs. 24.8%), suggesting their interactions with the police may result in a less formal outcome. The reasons for not reporting were broadly similar to those cited by female victims, though male victims more often considered the assault not serious enough to report (58.6% vs. 44.6%).

Table 1.B.1: Sample characteristics (2007-2019)

	Sexual violence male victims			All male respondents		
	<i>N</i> = 331	Missing values (%)	Frequency (%)	<i>N</i> = 70,321	Missing values (%)	Frequency (%)
Sociodemographics						
Aged 18-29	331	0.0	26.8	70321	0.0	21.1
Aged 30-54	331	0.0	41.3	70321	0.0	49.3
Aged 55-76	331	0.0	32.0	70321	0.0	29.6
Born in France	331	0.0	68.5	70321	0.0	86.5
In a couple	331	0.0	61.8	70321	0.0	70.6
<i>Educational attainment</i>						
High education	331	0.0	15.1	70180	0.2	26.5
Medium education	331	0.0	43.2	70180	0.2	47.1
Low education	331	0.0	41.7	70180	0.2	26.4
<i>Employment status</i>						
Employed	331	0.0	49.5	70321	0.0	63.6
Unemployed	331	0.0	9.9	70321	0.0	6.7
Student	331	0.0	9.8	70321	0.0	6.2
Other inactive	331	0.0	30.8	70321	0.0	23.6
<i>Socio-professional category</i>						
Upper class	331	0.0	8.7	70182	0.2	22.7
Middle class	331	0.0	20.3	70182	0.2	24.5
Lower class	331	0.0	52.8	70182	0.2	44.8
Other category	331	0.0	18.1	70182	0.2	7.9
<i>Household income</i>						
High (> 150%*median)	314	5.1	18.3	66730	5.1	38.2
Middle (between 70-150%*median)	314	5.1	53.7	66730	5.1	45.8
Low (< 70%*median)	314	5.1	28.0	66730	5.1	16.0
Household size > 2	331	0.0	51.2	70321	0.0	51.4
Living in a priority district	331	0.0	15.5	70321	0.0	6.9
<i>Size of the urban unit</i>						
Country side	330	0.3	17.0	70313	0.0	24.5
< 100,000 inhabitants	330	0.3	31.5	70313	0.0	29.7
> 100,000 inhabitants	330	0.3	34.5	70313	0.0	29.2
Paris urban unit	330	0.3	17.0	70313	0.0	16.6
Assault characteristics						
Outside current household	331	0.0	81.4			
Inside current household	331	0.0	22.2			
Rape	275	16.9	21.8			
Attempted rape	275	16.9	21.2			
Other sexual assault	275	16.9	58.4			
Occurred twice or more	287	13.3	64.4			
Several perpetrators	256	22.7	19.2			
Only male perpetrators	225	32.0	50.7			
Unknown perpetrator	278	16.0	28.6			
Victim's partner or ex-partner	255	23.0	12.2			
Other known perpetrator	278	16.0	60.2			
Perpetrator drugged or drunk	254	23.3	38.6			
Physical violence or weapon threat	271	18.1	28.1			
Physical injury	287	13.3	29.9			
Legal actions taken						
Went to the police	284	14.2	14.3			
Made a report	280	15.4	11.8			
Reasons for not reporting						
To keep it private	227	31.4	33.3			
Preferred to find another solution	228	31.1	61.9			
For fear of reprisals	228	31.1	23.7			
Would have been useless	230	30.5	62.2			
It was not serious	229	30.8	58.6			
To avoid further hardship	226	31.7	41.8			

Interpretation: Twenty-seven percent (26.8%) of male victims of sexual violence were aged 18–29 compared with 21.1% of the total number of male respondents.

Note: Characteristics of the sample of men who reported experiencing sexual violence outside or inside their current household in the past 2 years, compared with the total population of men. The column headings indicate the total unweighted number of observations: *N* = 331 male victims of SV, *N* = total number of male respondents. The columns “Number of observations” provide the number of observations used to calculate the weighted frequency (in %) within each group. The columns “Missing values (%)” provide the percentage of missing values in relation to the total number of observations. Values are missing as a result of 1) lack of response to certain questions linked to sociodemographic or assault characteristics, or 2) not all respondents being asked each question (for example, the questions on legal actions taken or reasons for not reporting).

Source: CVS survey data, 2007-2019.

Table 1.B.2: Incident locations and perpetrator familiarity by type of sexual violence

	Outside current household			Inside current household		
	#	Prop.	(SE)	#	Prop.	(SE)
Occurred at home	70	0.298	(0.035)			
Occurred at someone else's home	44	0.202	(0.030)			
Occurred at workplace	25	0.091	(0.020)			
Occurred in a public space	30	0.122	(0.025)			
Occurred in another place	54	0.288	(0.037)			
Unknown perpetrator	89	0.375	(0.036)			
Known perpetrator	132	0.625	(0.036)			
Perpetrator known by sight	44	0.228	(0.035)			
<i>Known by sight:</i> Neighbour	19	0.364	(0.060)			
<i>Known by sight:</i> Work colleague	4	0.099	(0.043)			
<i>Known by sight:</i> Other	22	0.537	(0.051)			
Perpetrator known personally	86	0.396	(0.036)			
<i>Known personally:</i> Ex-partner	14	0.119	(0.033)			
<i>Known personally:</i> Family member	6	0.077	(0.030)			
<i>Known personally:</i> Close person	29	0.403	(0.052)			
<i>Known personally:</i> Work colleague	14	0.141	(0.035)			
<i>Known personally:</i> Other	22	0.260	(0.046)			
Spouse/partner				19	0.451	(0.047)
Partner's parent				10	0.230	(0.054)
Partner's child				1	0.083	(0.000)
Other family member				4	0.112	(0.035)
Household cohabitant				8	0.124	(0.027)

Note: #: number of male victims in each category. Prop.: weighted proportion of victims in each category and standard errors in parentheses. Coverage: Male victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.B.3: Legal remedies undertaken by male victims by type of sexual violence

	Outside current household		Inside current household		Total (in either context)	
	#	Prop. (SE)	#	Prop. (SE)	#	Prop. (SE)
Went to the police	35	0.132 (0.024)	8	0.139 (0.037)	41	0.143 (0.025)
Filed a formal complaint	17	0.575 (0.043)	5	0.758 (0.015)	20	0.538 (0.051)
Filed a <i>main courante</i>	10	0.359 (0.037)	2	0.25 (0)	12	0.407 (0.043)
Renounced filing a statement	4	0.066 (0.03)	1	0.333 (0)	5	0.073 (0.031)
Went to the police and filed a report	27	0.107 (0.022)	7	0.128 (0.039)	32	0.118 (0.024)

Note: #: number of male victims in each category. Prop.: weighted proportion of victims who undertook a given legal action and standard errors in parentheses.

Coverage: Male victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

1.C Justification for the socioeconomic status index

Education, employment status, socio-occupational category, and household income are highly correlated in the data (Table 1.C.1). As such, Table 1.C.2 shows that, when considered individually, each of these variables is strongly associated with the reporting decision (Columns 1 to 4 and Columns 5 to 9). However, when included simultaneously as regressors, their effects almost disappear due to a lack of statistical power to estimate the marginal effect of each variable conditional on the others (Columns 5 and 10).

Therefore, to evaluate the impact of the victim’s socioeconomic status with sufficient statistical power, I create a new variable that aggregates these factors into a socioeconomic status index S_i . I construct the index by standardizing the individual scales and applying principal component analysis (PCA) to account for the correlation between the variables and allow them to have different weights. The resulting index is normalized such that it has a mean of zero and a standard deviation of one, and rises with the individual’s socioeconomic status.

Table 1.C.1: Correlation matrix of socioeconomic variables

	Educational attainment	Employment status	Socio-occupational category	Household income
Educational attainment	1.000	0.262	0.425	0.161
Employment status	0.262	1.000	0.265	0.284
Socio-occupational category	0.425	0.265	1.000	0.279
Household income	0.161	0.284	0.279	1.000

Note: Pearson correlation coefficients.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

Table 1.C.2: Influence of socioeconomic factors on the decision to report sexual violence to the police

	<i>Dependent variable: Filed a report (0/1)</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Education (Ref = High education)</i>										
Medium education	0.038*				0.011	0.026				0.002
	(0.029)				(0.035)	(0.028)				(0.034)
Low education	0.086***				0.045	0.061**				0.034
	(0.032)				(0.038)	(0.032)				(0.038)
<i>Employment status (Ref = Employed)</i>										
Unemployed		0.098***			0.081***		0.065***			0.058**
		(0.031)			(0.032)		(0.032)			(0.033)
Inactive		0.048**			0.037		0.033			0.022
		(0.025)			(0.037)		(0.027)			(0.036)
<i>Socio-occupational category (Ref = Upper class)</i>										
Middle			0.073*		0.053			0.084*		0.070
			(0.050)		(0.052)			(0.055)		(0.057)
Low			0.097***		0.054			0.093***		0.059
			(0.046)		(0.050)			(0.051)		(0.057)
Other			0.110***		0.041			0.106**		0.057
			(0.051)		(0.062)			(0.056)		(0.067)
<i>Household income (Ref = High income)</i>										
Middle income				0.063*	0.049				0.048	0.035
				(0.041)	(0.044)				(0.040)	(0.042)
Low income				0.096***	0.058				0.071*	0.040
				(0.044)	(0.046)				(0.045)	(0.046)
Other sociodemographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other assault controls	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Year and region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,442	1,444	1,442	1,400	1,396	1,370	1,372	1,370	1,333	1,329
Pseudo R ²	0.06	0.06	0.06	0.07	0.08	0.15	0.16	0.15	0.17	0.18

Note: Logit-estimated coefficients (average marginal effects) obtained when regressing the reporting propensity on each variable composing the socioeconomic index. Other controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.D Complaint filing versus global reporting decision

The focus of this paper is to understand the factors influencing victims' reporting decisions. We consider both complaints and *mains courantes* together in the analysis because both indicate that the victim engaged in a formal reporting process. Additionally, victims may be advised by the police to file a *main courante*, even if their initial intention was to file a formal complaint (Pérona, 2018). In such cases, the filing of *mains courantes* is influenced by police behavior, and excluding them could bias the results regarding the determinants of reporting.

Table 1.D.1 presents the logit-estimated results of Equation 1 (average marginal effects) on the probability of filing a complaint, excluding *mains courantes* from the reporting outcome. These results can be compared with those in Table 3 of the main paper. Overall, the determinants of the decision to report to the police remain similar, particularly the main coefficients related to the victim's socioeconomic status and the occurrence of physical injuries. There is a change in the magnitude or significance of a few variables: the coefficient associated with physical injuries slightly decreases, while the coefficients for the number of household members and the type of violence become insignificant.

Therefore, the overall results do not qualitatively change when focusing only on victims who formally filed a complaint, but they are more likely to be biased due to the police's influence on the victims' decision to file a *main courante* rather than a complaint.

Table 1.D.1: Determinants of the decision to file a complaint for sexual violence

	<i>Dependent variable: Filed a complaint (0/1)</i>			
	(1)	(2)	(3)	(4)
Age (log)	0.064*** (0.022)			0.056*** (0.022)
Born abroad	-0.001 (0.028)			0.006 (0.029)
In a couple	-0.034* (0.023)			-0.010 (0.022)
Socioeconomic status index	-0.043*** (0.012)			-0.033*** (0.012)
Size of the urban unit (<i>Ref = Rural areas</i>)				
<i>Less than 100,000</i>	0.040** (0.021)			0.034* (0.024)
<i>More than 100,000</i>	0.052*** (0.023)			0.048** (0.025)
<i>Paris urban unit</i>	0.226 (0.182)			0.192 (0.159)
Priority district	0.019 (0.024)			0.005 (0.025)
Number of household members	0.006 (0.008)			0.008 (0.008)
Type of SV (<i>Ref = Inside current household</i>)				
<i>Outside current household</i>		0.027 (0.027)	0.034 (0.032)	0.025 (0.031)
<i>Both contexts</i>		0.026 (0.045)	-0.004 (0.037)	-0.014 (0.038)
Assault type (<i>Ref = Rape</i>)				
<i>Attempted rape</i>			0.041 (0.037)	0.035 (0.033)
<i>Other sexual assault</i>			-0.055* (0.034)	-0.055** (0.028)
<i>Not specified</i>			-0.043 (0.039)	-0.038 (0.042)
Several perpetrators			-0.038 (0.041)	-0.054 (0.043)
Victim-perpetrator relationship (<i>Ref = Unknown perpetrator</i>)				
<i>Victim's partner or ex-partner</i>			-0.086** (0.044)	-0.086** (0.045)
<i>Other known perpetrator</i>			-0.082*** (0.040)	-0.079** (0.041)
Occurred twice or more			0.003 (0.020)	-0.007 (0.019)
Physical injuries			0.126*** (0.022)	0.114*** (0.020)
Year and region fixed effects	Yes	Yes	Yes	Yes
Observations	1,395	1,444	1,372	1,328
Pseudo R ²	0.10	0.04	0.14	0.18

Note: Logit-estimated coefficients (average marginal effects) from Equation 1 for the propensity to file a complaint. Design-robust standard errors in parentheses. Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74. Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.E Linear probability model results

This appendix presents the results of the main estimations (Tables 3 and 4 of the main paper) using a linear probability model (LPM), rather than a logit model. LPM can predict probabilities outside the [0-1] range, which is why logit or probit models are often preferred for modeling probabilities. On the other hand, LPM is more easily interpretable than logit, for which the estimated coefficients are not directly interpretable, and their marginal effects depend on the values of the data (Angrist and Pischke, 2009). As shown below, the results of the LPM model are qualitatively and quantitatively very similar to those obtained with the logit model (average marginal effects) in the main paper. In particular, the results regarding the effect of the victim's socioeconomic status (SES) and the occurrence of physical injuries on the reporting process are relatively similar in terms of magnitude and significance.

Table 1.E.1: Determinants of the decision to report sexual violence to the police

	<i>Dependent variable: Filed a report (0/1)</i>			
	(1)	(2)	(3)	(4)
Age (log)	0.084*** (0.030)			0.082*** (0.031)
Born abroad	0.011 (0.040)			0.034 (0.043)
In a couple	-0.059** (0.026)			-0.024 (0.027)
Socioeconomic status index	-0.041*** (0.012)			-0.029** (0.013)
Size of the urban unit (<i>Ref = Rural areas</i>)				
<i>Less than 100,000</i>	0.060* (0.032)			0.051 (0.034)
<i>More than 100,000</i>	0.062* (0.033)			0.050 (0.034)
<i>Paris urban unit</i>	0.152*** (0.056)			0.148** (0.063)
Priority district	0.025 (0.038)			0.013 (0.039)
Number of household members	0.021* (0.012)			0.025** (0.012)
Type of SV (<i>Ref = Inside current household</i>)				
<i>Outside current household</i>		0.045 (0.031)	0.076* (0.040)	0.085** (0.041)
<i>Both contexts</i>		0.062 (0.054)	0.040 (0.058)	0.038 (0.060)
Assault type (<i>Ref = Rape</i>)				
<i>Attempted rape</i>			0.076* (0.043)	0.074* (0.042)
<i>Other sexual assault</i>			-0.047 (0.040)	-0.048 (0.038)
<i>Not specified</i>			0.015 (0.040)	0.017 (0.045)
Several perpetrators			-0.024 (0.058)	-0.042 (0.058)
Victim-perpetrator relationship (<i>Ref = Unknown perpetrator</i>)				
<i>Victim's partner or ex-partner</i>			-0.054 (0.048)	-0.059 (0.049)
<i>Other known perpetrator</i>			-0.075* (0.039)	-0.078* (0.040)
Occurred twice or more			-0.014 (0.024)	-0.024 (0.024)
Physical injuries			0.177*** (0.030)	0.163*** (0.029)
Year and region fixed effects	Yes	Yes	Yes	Yes
Observations	1,396	1,445	1,373	1,329
Adjusted R ²	0.041	0.008	0.085	0.112

Note: OLS-estimated coefficients from Equation 1 for the reporting propensity, using a linear probability model. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.E.2: Impact of SES and physical injuries on the reasons for not reporting sexual violence

	<i>Dependent variables: Reason group (0/1)</i>		
	Not serious assault	Not worth reporting	Fear of reporting consequences
	(1)	(2)	(3)
Socioeconomic status index	0.011 (0.020)	0.022* (0.011)	−0.032* (0.018)
Physical injuries	−0.194*** (0.040)	0.021 (0.025)	0.193*** (0.032)
Other controls	Yes	Yes	Yes
Year and region fixed effects	Yes	Yes	Yes
Observations	1,098	1,116	1,116
Adjusted R ²	0.091	0.082	0.146

Note: OLS-estimated coefficients from Equation 1 for the reasons for not reporting, using a linear probability model. Other controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74, and who did not report it to the police.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.F Heterogeneity analyses and interaction between socioeconomic status, physical injuries, and relationship status

This appendix explores whether the impact of victims' sociodemographics and assault characteristics varies based on whether the sexual assault occurred inside or outside the current household. We investigate the heterogeneity effect by interacting each observable characteristic with a dummy variable indicating whether the sexual violence (SV) occurred outside rather than inside the current household. As shown in Tables 1.F.1 and 1.F.2, there is little heterogeneity between victims assaulted inside or outside their current household. Apart from the frequency of assault (Table 1.F.2), most observable characteristics correlate with the decision to formally report SV in the same way, regardless of whether the assault occurred inside or outside the current household, as the interaction terms are not statistically significant.

Moreover, we investigate whether the effect of physical injuries on the decision to report SV to the police varies by victims' socioeconomic status (SES) by including an interaction term between the victims' SES and the occurrence of physical injuries in the main specification. The aim is to test whether the impact of physical injuries is more pronounced among lower-SES victims, as they are more likely to come into contact with social services. Table 1.F.3 shows that while the interaction term is negative, suggesting that the effect of physical injuries on reporting may be weaker for higher-SES victims, it is not statistically significant. Therefore, while physical injuries strongly increase reporting rates, this effect does not vary meaningfully across socioeconomic groups and the impact of physical injuries is not particularly more pronounced among lower-SES victims. Finally, we investigate whether the effect of socioeconomic status on reporting varies by relationship status. Table 1.F.4 shows that the interaction term between SES and being in a couple is negative and statistically significant, suggesting that the negative effect of SES on reporting is more pronounced among victims who are in a couple. This result is consistent with the hypothesis that higher-SES partnered victims face compounding reputational costs that may discourage reporting.

As noted in several papers in the literature revising probability estimation models (Ai and Norton, 2003; Norton et al., 2004; Breen et al., 2018; McCabe et al., 2022), the coefficients and marginal effects of interaction terms are not directly interpretable with logit. Since marginal effects for interaction terms are particularly difficult to derive in logit, and their standard errors must be calculated in a much more complex way, we retain the linear probability model (LPM) for the specifications investigating heterogeneity effects and using interaction terms.

Table 1.F.1: Heterogeneous impact of sociodemographics by sexual violence type

	<i>Dependent variable: Filed a report (0/1)</i>
	(1)
Outside current household SV	-0.379 (0.526)
Age (log)	0.013 (0.112)
Outside current household SV × Age	0.092 (0.121)
Born abroad	0.095 (0.126)
Outside current household SV × Born abroad	-0.089 (0.135)
In a couple	-0.057 (0.058)
Outside current household SV × In a couple	0.041 (0.065)
Socioeconomic status index	0.003 (0.030)
Outside current household SV × Socioeconomic status index	-0.045 (0.033)
Size of the urban unit (<i>Ref = Rural areas</i>)	
Less than 100,000	0.029 (0.050)
More than 100,000	0.054 (0.065)
Paris urban unit	0.102 (0.087)
Outside current household SV × Less than 100,000	0.024 (0.066)
Outside current household SV × More than 100,000	-0.007 (0.076)
Outside current household SV × Paris urban unit	0.050 (0.085)
Priority district	0.036 (0.097)
Outside current household SV × Priority district	-0.038 (0.105)
Number of household members	-0.004 (0.035)
Outside current household SV × Number of household members	0.035 (0.037)
Assault controls	Yes
Year and region fixed effects	Yes
Observations	1,329
Adjusted R ²	0.114

Note: OLS-estimated coefficients obtained when regressing the reporting propensity on the type of SV suffered, victims' sociodemographics and their interaction. Since marginal effects for interaction terms are particularly difficult to derive in logit and their standard errors must be calculated in a much more complex way (Breen et al., 2018; Ai and Norton, 2003), we chose to retain the linear probability model (LPM) for this specification. Assault controls include type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.F. HETEROGENEITY ANALYSES AND INTERACTION BETWEEN SOCIOECONOMIC STATUS, PHYSICAL INJURIES, AND RELATIONSHIP STATUS

Table 1.F.2: Heterogeneous impact of assault characteristics by sexual violence type

	<i>Dependent variable: Filed a report (0/1)</i>
	(1)
Outside current household SV	0.177** (0.090)
Assault type (<i>Ref = Rape</i>)	
Attempted rape	0.002 (0.070)
Other sexual assault	0.149 (0.138)
Not specified	0.008 (0.047)
Outside current household SV × Attempted rape	0.082 (0.085)
Outside current household SV × Other sexual assault	−0.219 (0.143)
Outside current household SV × Not specified	−0.121 (0.075)
Several perpetrators	0.072 (0.166)
Outside current household SV × Several perpetrators	−0.110 (0.177)
Victim's partner or ex-partner	−0.012 (0.057)
Outside current household SV × Victim's partner or ex-partner	0.016 (0.069)
Occurred twice or more	0.062 (0.050)
Outside current household SV × Occurred twice or more	−0.129** (0.059)
Physical injuries	0.144** (0.062)
Outside current household SV × Physical injuries	0.023 (0.074)
Sociodemographic controls	Yes
Year and region fixed effects	Yes
Observations	1,304
Adjusted R ²	0.123

Note: OLS-estimated coefficients obtained when regressing the reporting propensity on the type of SV suffered, assault characteristics and their interaction. Since marginal effects for interaction terms are particularly difficult to derive in logit and their standard errors must be calculated in a much more complex way (Breen et al., 2018; Ai and Norton, 2003), we chose to retain the linear probability model (LPM) for this specification. Sociodemographic controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district. We cannot control for the relationship between the victim and the perpetrator in the same way as in the main specification due to collinearity between having been assaulted inside the current household and knowing the perpetrator, which prevents us from estimating the interaction term. Instead, in this specification, we control for whether the perpetrator was the victim's partner or ex-partner. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.F.3: Determinants of the decision to report sexual violence to the police, interacting socioeconomic status and physical injuries

	<i>Dependent variable: Filed a report (0/1)</i>
	(1)
Socioeconomic status index	−0.018 (0.013)
Physical injuries	0.162*** (0.028)
Socioeconomic status index × Physical injuries	−0.033 (0.029)
Other controls	Yes
Year and region fixed effects	Yes
Observations	1,329
Adjusted R ²	0.113

Note: OLS-estimated coefficients from Equation 1 for the reporting propensity, adding an interaction between victim’s socioeconomic status and the occurrence of physical injuries. Since marginal effects for interaction terms are particularly difficult to derive in logit and their standard errors must be calculated in a much more complex way (Breen et al., 2018; Ai and Norton, 2003), we chose to retain the linear probability model (LPM) for this specification. We interacted these two factors to examine whether the effect of physical injuries on reporting varies by socioeconomic status. The interaction term is small and not statistically significant, suggesting no meaningful variation in the effect of physical injuries on the reporting decision across socioeconomic groups. Other controls include age (log), being born abroad, being in a relationship, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.F.4: Determinants of the decision to report sexual violence to the police, interacting socioeconomic status and being in a couple

	<i>Dependent variable: Filed a report (0/1)</i>
	(1)
Socioeconomic status index	−0.003 (0.016)
In a couple	−0.017 (0.028)
Socioeconomic status index × In a couple	−0.052** (0.025)
Other controls	Yes
Year and region fixed effects	Yes
Observations	1,329
Adjusted R ²	0.117

Note: OLS-estimated coefficients from Equation 1 for the reporting propensity, adding an interaction between victim’s socioeconomic status and being in a couple. Since marginal effects for interaction terms are particularly difficult to derive in logit and their standard errors must be calculated in a much more complex way (Breen et al., 2018; Ai and Norton, 2003), we chose to retain the linear probability model (LPM) for this specification. We interacted these two factors to examine whether the effect of socioeconomic status on reporting varies by relationship status. The interaction term is negative and statistically significant, suggesting that the negative effect of socioeconomic status on reporting is more pronounced among victims who are in a couple. Other controls include age (log), being born abroad, being in a relationship, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship. Robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

1.G Additional explanatory hypotheses

Victim's socioeconomic status

Another mechanism that could explain the higher likelihood of reporting SV to the police among victims of lower socioeconomic status (SES) is their stronger connection to social services, which may act as intermediaries or provide incentives to report SV to the police. Indeed, research in sociology has shown that women in economically precarious situations are more likely to turn to the judicial system in cases of sexual or domestic abuse, potentially due to the closer monitoring of families dependent on social services (Le Goaziou, 2013; Jouanneau, 2024). For instance, lower-SES women may be encouraged by social services to report to the police in order to receive assistance from associations or to secure custody in divorce proceedings.

To investigate this hypothesis, we use a CVS question that asks self-reported victims of SV whether they discussed their situation with social services at least once following the assault (yes/no/did not answer), without specifying the sequence of events. A sub-question, introduced in 2017, aims to determine whether this meeting occurred at the police station where the victim reported the incident. Among the 329 self-reported Female victims of sexual violence in the 2017 to 2019 surveys, 32 reported having discussed it with social services, and of these, 9 had this meeting at the police station.

As expected, lower-SES victims are more frequently in contact with social services, which could be one additional channel explaining why they report more to the police. Indeed, Table 1.G.1 shows that the likelihood of discussing the incident with social services significantly decreases with the victim's SES. We then add this covariate in the estimation of our main Equation 1. Table 1.G.2 shows that discussing the incident with social services is significantly correlated with reporting to the police, although it is not possible to determine the direction of this correlation with certainty. However, the victim's SES still has an effect of similar magnitude and significance as in Table 3. The effect is only marginally reduced when controlling for assault characteristics in Column 6, and the coefficient remains negative and statistically significant. This suggests that while the stronger connection of lower-SES victims with social services may partially explain the effect of the victim's SES on the reporting decision, it does not fully account for it.

Table 1.G.1: Effect of victims’ socioeconomic status on contact with social services

	<i>Dependent variable: Spoke with social services (0/1)</i>		
	(1)	(2)	(3)
Socioeconomic status index	−0.051*** (0.012)	−0.050*** (0.012)	−0.032*** (0.012)
Other sociodemographics	No	Yes	Yes
Assault controls	No	No	Yes
Year and region fixed effects	Yes	Yes	Yes
Observations	1,374	1,373	1,325
Pseudo R ²	0.05	0.10	0.19

Note: Logit-estimated coefficients (average marginal effects) obtained when regressing a dummy for having ever discussed one’s situation with social services on the victim’s socioeconomic status index. Other sociodemographics include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district. Assault controls are: type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.G.2: Effect of contact with social services on the decision to report sexual violence to the police

	<i>Dependent variable: Filed a report (0/1)</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Spoke with social services	0.202*** (0.023)	0.194*** (0.024)	0.169*** (0.023)	0.189*** (0.024)	0.178*** (0.025)	0.160*** (0.024)
Socioeconomic status index				−0.038*** (0.012)	−0.034*** (0.012)	−0.025** (0.013)
Physical injuries			0.122*** (0.022)			0.126*** (0.020)
Other sociodemographics	No	Yes	Yes	No	Yes	Yes
Assault controls	No	No	Yes	No	No	Yes
Year and region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,415	1,414	1,366	1,371	1,370	1,324
Pseudo R ²	0.10	0.12	0.20	0.12	0.14	0.22

Note: Logit-estimated coefficients (average marginal effects) obtained when regressing the reporting propensity on whether the victim spoke with social services. Other sociodemographics include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district. Assault controls are: type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Physical injuries occurrence

Another mechanism that could explain the higher reporting propensity among victims who sustained physical injuries is their increased likelihood of coming into contact with emergency medical services, which in turn may increase their likelihood of reporting to the police. To test this hypothesis, we use CVS questions that ask SV victims whether they saw a doctor or spent one or more nights in the hospital (yes/no/did not answer). As expected, sustaining physical injuries significantly increases the likelihood of seeking emergency medical services, as shown in Table 1.G.3. When adding this covariate to the estimation of our main Equation 1, Table 1.G.4 shows that having seen a doctor or spending at least one night in the hospital also significantly increases the likelihood of reporting SV to the police.

Table 1.G.3: Effect of physical injuries on contact with emergency medical services

	<i>Dependent variable: Saw medical emergency (0/1)</i>		
	(1)	(2)	(3)
Physical injuries	0.256*** (0.021)	0.217*** (0.021)	0.191*** (0.021)
Other assault controls	No	Yes	Yes
Sociodemographic controls	No	No	Yes
Year and region fixed effects	Yes	Yes	Yes
Observations	1,454	1,374	1,330
Pseudo R ²	0.16	0.19	0.22

Note: Logit-estimated coefficients (average marginal effects) obtained when regressing a dummy for having been in contact with emergency medical services on the occurrence of physical injuries. Other assault controls include type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Sociodemographic controls are: age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

^{*} $p < 0.1$; ^{**} $p < 0.05$; ^{***} $p < 0.01$.

However, the occurrence of physical injuries and contact with emergency medical services are highly correlated. Among self-reported Female victims of sexual violence who sustained physical injuries ($N = 569$), 36.5% saw a doctor or spent at least one night in the hospital, compared to 8.3% of victims who did not sustain physical injuries ($N = 892$). Therefore, it is difficult to determine whether their decision to report the assault to the police was driven by seeking medical care or by the physical injuries themselves. Furthermore, the estimated coefficient associated with physical injuries remains high and significant even after adding a covariate for contact with emergency medical services to the regression (Columns 4 to 6 of Table 1.G.4). Although the magnitude is reduced by almost half compared to our main results in Table 3, physical injuries still have a significant positive effect on the decision to report SV to the police.

Table 1.G.4: Effect of contact with emergency medical services on the decision to report sexual violence to the police

	<i>Dependent variable: Filed a report (0/1)</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Contact with medical emergency	0.241*** (0.015)	0.254*** (0.018)	0.249*** (0.017)	0.213*** (0.018)	0.228*** (0.017)	0.227*** (0.017)
Physical injuries				0.065*** (0.025)	0.067*** (0.026)	0.063*** (0.022)
Socioeconomic status index			−0.020** (0.011)			−0.017* (0.011)
Other assault controls	No	Yes	Yes	No	Yes	Yes
Sociodemographic controls	No	No	Yes	No	No	Yes
Year and region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,444	1,375	1,331	1,441	1,373	1,329
Pseudo R ²	0.24	0.28	0.32	0.25	0.29	0.33

Note: Logit-estimated coefficients (average marginal effects) obtained when regressing the reporting propensity on whether the victim saw emergency medical services. Other assault controls include type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries. Sociodemographic controls are: age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Interestingly, when controlling for the victim’s contact with social services in the specification, Table 1.G.2 shows that the effect of physical injuries on the decision to report SV to the police is slightly reduced compared to the result from the main estimation (see Table 3 in the paper). This suggests that the effect of physical injuries on the reporting decision may be partially explained by the victim’s interaction with social services. Likewise, when controlling for contact with emergency medical services in the regression in Table 1.G.4, the effect of the victim’s SES is also attenuated compared to the result from the main estimation (see Table 3 in the paper). This suggests that the effect of SES on the decision to report SV may be partially explained by the victim’s contact with medical emergency.

1.H Expectations of police and judicial response

The large share (90%) of non-reporting victims who believe reporting would have been useless or preferred to find another solution suggests that expectations of police and judicial responses may play a decisive role, especially in a context where SV perpetrators are rarely prosecuted (Le Goaziou, 2019; Stricot, 2024) and police reception of SV victims is often questioned (Gauthier et al., 2025). This hypothesis is further supported by our main results, which highlight the low perceived gains of the procedure and the lack of incentive to report, even with solid evidence. To investigate the role of perceptions of the police and justice system on the reporting decision, we use questions on satisfaction with the overall performance of these institutions in French society today (yes/no/did not answer). While these questions may not fully capture respondents' beliefs about the attitude of the police and justice system in the specific context of SV, they provide the best available proxy within the CVS survey. These questions were asked to all respondents but were not introduced simultaneously in the survey waves. Satisfaction with the local police was introduced in 2007, satisfaction with the police in general in 2010, and satisfaction with the justice system and courts in 2013.

Table 1.H.1: Effect of satisfaction with the police/justice system on the reporting decision

	<i>Dependent variable: Filed a report (0/1)</i>		
	(1)	(2)	(3)
Satisfied with police in general	0.053** (0.027)		
Satisfied with police in local neighborhood		0.014 (0.022)	
Satisfied with justice in general			0.017 (0.036)
Other controls	Yes	Yes	Yes
Year and region fixed effects	Yes	Yes	Yes
Observations	1,000	1,329	661
Pseudo R ²	0.19	0.19	0.20

Note: Logit-estimated coefficients (average marginal effects) from Equation 1 for the reporting propensity. Other controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries, along with recent victimization at the household (burglary, theft, vandalism) and individual (robbery, assault) levels. The varying number of observations is due to the gradual introduction of the satisfaction questions in the survey. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74.

Source: Cadre de vie et sécurité survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.H.1 presents the logit estimation results for the regression of reporting likelihood on satisfaction with the police and the justice system, controlling for sociodemographic and assault characteristics from Equation 1, as well as other recent household (burglary, theft, vandalism) and individual (theft, assault) victimizations. The weighted data reveals that 43.6% of women who reported experiencing SV in the preceding 2 years are satisfied with the police in general, 38.8% with the police in their local neighborhood, and 23.3% with the justice system. Victims of SV who are satisfied with the police in general are significantly more likely to report their assault than those who are dissatisfied or have no opinion. However, satisfaction

with the local police or the justice system is not associated with a higher reporting propensity. It is important to note that correlations between satisfaction with these institutions and reporting behavior may be biased by victims' past interactions with law enforcement. While controlling for other victimizations helps mitigate this bias, responses may still be influenced by previous experiences with the police or the justice system.

Table 1.H.2 performs the same exercise but regressing the reasons for not reporting on satisfaction with the police and justice system. Column (6) shows that the main reason for not reporting – thinking it would not have been worth it – is significantly associated with satisfaction with the judicial system, with a significant 9.7 percentage points (p.p.) decrease in the likelihood of considering reporting worthless among non-reporting victims satisfied with the justice system in general (11% decrease over baseline). In contrast, the coefficients for satisfaction with the police, whether in general (Column 4) or in the local neighborhood (Column 5), are relatively small and insignificant. These findings suggest that perceptions of the criminal justice system's responses constitute a key pathway influencing the decision not to report. While satisfaction with the police also plays a role, its influence appears less significant compared to expectations of the post-complaint judicial response.

It is worth noting that perceptions of police and justice handling of SV may differ between recent and long-standing cases. Older cases are potentially less aligned with “classic rape” stereotypes (Pérona, 2023), which may partly explain the low significance of police satisfaction in reporting recent SV. This may contrast with findings related to lifetime SV. Likewise, focusing solely on recent cases may underestimate the impact of satisfaction with the justice system on the reporting decision.

Table 1.H.2: Effect of satisfaction with the police/justice system on the reasons for not reporting sexual violence

	<i>Dependent variable: Reason group (0/1)</i>								
	Not serious assault			Not worth reporting			Fear of reporting consequences		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Satisfied with police in general	0.009 (0.040)			-0.014 (0.023)			0.035 (0.034)		
Satisfied with police in local neighborhood		0.003 (0.036)			-0.022 (0.021)			-0.040* (0.030)	
Satisfied with justice in general			0.083* (0.060)			-0.097*** (0.027)			0.024 (0.053)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year and region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	821	1,097	540	821	1,097	540	821	1,097	540
Pseudo R ²	0.12	0.10	0.10	0.15	0.17	0.21	0.20	0.19	0.22

Note: Logit-estimated coefficients (average marginal effects) from Equation 1 for the reasons for not reporting. Other controls include age (log), being born abroad, being in a relationship, socioeconomic status index, number of household members, size of the urban unit, priority district, type of SV, type of assault, number of occurrences, number of perpetrators, victim-perpetrator relationship, presence of physical injuries, along with recent victimization at the household (burglary, theft, vandalism) and individual (robbery, assault) levels. The varying number of observations is due to the gradual introduction of the satisfaction questions in the survey. Design-robust standard errors in parentheses.

Coverage: Female victims of sexual violence outside or inside their current household in the preceding 2 years, living in metropolitan France and aged 18-74, and who did not report it to the police.

Source: *Cadre de vie et sécurité* survey data, 2007-2019.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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Chapter 2

Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women

Acknowledgements

This work has benefited from the support of the Agence Nationale de la Recherche through the programs Investissements d’Avenir (ANR-17-EURE-0001) and SOCOCITY (ANR-18-CE22-0013). I also acknowledge financial support from the research unit PjSE (UMR 8545), the University Paris 1 Panthéon-Sorbonne, and the National Audiovisual Institute (INA). Access to some confidential data has been made possible within a secure environment offered by CASD (Centre d’accès sécurisé aux données). I am particularly grateful to the INA and the statistical service of the Ministry of Justice for their assistance in accessing and using the data. This chapter greatly benefited from discussions and helpful comments from Cristina Bellés-Obrero, Davi Bhering, Thomas Breda, Caroline Coly, Mathile Emeriaux, Gabrielle Fack, Lidia Farré, Rosa Ferrer, Camille Hémet, Nicolas Jacquemet, Johann Koehler, Sylvie Lambert, David Margolis, Olivier Marie, Benjamin Monnery, Emily Nix, Aurélie Ouss, Hélène Périvier, Thomas Renault, Johanna Rickne, Núria Rodríguez-Planas, Sulin Sardoschau, and David Strömberg. I also thank participants at seminars at the Paris School of Economics, Namur University, Paris Nanterre University, Paris Cité University, the Stockholm School of Economics, Stockholm University, the University of Barcelona, and INED, as well as participants at conferences and workshops including Doctorissimes 2024, AFEPOP 2024, JMA 2024, EALE 2024, ESPE 2025, EALawE 2025, TWEC 2025, AFED 2025, the 2024 IEB Workshop on Public Policies, the 2025 IEB Workshop on Violence Against Women, and the LSE Mannheim PhD Symposium 2026, for their valuable feedback.

Abstract

This paper examines whether media visibility of violence against women (VAW) affects the criminal justice response to sexual and intimate partner violence. It combines novel administrative microdata on criminal cases with high-frequency data on daily TV news broadcasts in France. Leveraging the quasi-random timing of TV news stories about VAW relative to case processing, I find that news coverage of unrelated VAW cases leads to a 2.3% increase in the prosecution rate, with no change in conviction outcomes in the week following coverage. Suggestive evidence indicates that this shift in prosecutions is primarily driven by strategic responses to heightened public scrutiny and accountability, particularly in the post-#MeToo era. This occurs in a context where prosecutors largely determine case outcomes: they dismiss nearly 80% of cases, while most prosecuted cases ultimately result in conviction. Consistent with this pattern, I find that cases prosecuted following VAW news coverage appear just as likely to result in conviction as other cases. These results suggest that in resource-constrained environments, increased salience of VAW enables additional viable cases to reach trial without influencing judges' rulings, thereby strengthening the overall judicial response.

JEL classification: D91, I18, J12, J16, K4, K14, L82

Keywords: Gender-based violence, Media, Judicial decision, Crime, France

2.1 Introduction

Violence against women (VAW) is a pervasive social and public health issue, with one in three women worldwide reporting having experienced physical or sexual violence in her lifetime (WHO, 2021). Beyond the immediate physical and mental harm, such violence can have long-lasting economic consequences for victims' educational and employment trajectories (Borker, 2021; Folke and Rickne, 2022; Adams-Prassl et al., 2024; Batut et al., 2026; Adams et al., 2026). Despite these substantial costs to women and society, criminal justice systems worldwide continue to face challenges in responding effectively, with available evidence documenting widespread underreporting and low prosecution rates across many countries (Daly and Bouhours, 2010).¹ These patterns contribute to high levels of case attrition, whereby cases are dropped or fail to progress through the criminal justice process. In addition to limiting the justice system's ability to hold perpetrators accountable and deter such crimes (Chalfin and McCrary, 2017), this attrition may further discourage victims from coming forward and reinforce a cycle of impunity (Wieberneit et al., 2024).

These crimes typically occur in private settings, with few witnesses and little material evidence, making them especially difficult to investigate and prosecute. At the same time, such constraints often leave considerable room for discretion in how cases are processed, potentially opening the door to harmful social norms, beliefs, and attitudes that tend to downplay or justify this violence² and can influence how police, prosecutors, and judges handle these crimes (Sleath and Bull, 2017; Bielen et al., 2022). Yet norms and institutional behavior are not fixed. For example, the #MeToo movement has shown that increased visibility of sexual violence can shift beliefs and attitudes toward such violence (Szekeres et al., 2020; Acquaviva et al., 2021; Levy and Mattsson, 2023). But does greater salience and public attention to VAW lead to changes in judicial responses?

This paper seeks to answer this question by examining the impact of media coverage of VAW on criminal justice decisions. The media, by reflecting and shaping public opinion re-

¹For example, in France, victimization surveys estimate that only 10–20% of victims of sexual or intimate partner violence report their assault to the authorities (SSMSI, 2019). Administrative judicial data further indicate that, among these reported cases, 70–80% are dismissed by prosecutors in correctional and juvenile courts, and 15–30% result in conviction in those courts (Stricot, 2024).

²For instance, research in social psychology has long highlighted the existence of rape myths, formally defined by Burt (1980) as “prejudicial, stereotyped, or false beliefs about rape, rape victims, and rapists” (p. 217), such as the belief that women lie about rape or were responsible for it (Lonsway and Fitzgerald, 1994). A corollary to these myths is the “classic rape” stereotype, involving a sudden violent attack by a stranger in a deserted public space, with physical resistance from the victim (Estrich, 1987).

garding how such violence should be handled, provide a valuable means of studying these dynamics. Assessing whether this impact is causal is empirically challenging without granular data and an exogenous setting to address reverse causality. Moreover, it is not clear ex ante whether media influence should be viewed as desirable: while prevailing social norms may lead to the minimization of VAW in the justice system, heightened public expectations could also risk pushing decision-makers toward over-punishment. I address this question by combining high-frequency data on the daily content of France's main 8pm national TV newscasts, which reach over 10 million viewers each day, with rich, novel administrative microdata on nearly all cases processed by the French criminal justice system between 2012 and 2019. These individual case-level data contain detailed information on 792,802 cases of sexual or intimate partner violence handled in correctional and juvenile courts, tracking each case from the initial incident through every stage of the process, including reporting, prosecution, and sentencing. Leveraging quasi-random variation in the timing of TV news stories, this setting enables causal identification of the impact of VAW coverage on prosecutors and judges' decisions.

I begin by examining the internal dynamics of the French criminal justice system to better understand where media exposure is most likely to affect judicial decisions. I document the extent of prosecutors' power and discretion in the handling of sexual and intimate partner violence, showing that they dismiss more than three-quarters of cases at the outset, primarily on the grounds of insufficient evidence or indeterminate facts. These legal grounds reflect evidentiary challenges and typically involve a high degree of prosecutorial discretion. Strikingly, however, once prosecuted, the vast majority of cases (94%) result in conviction, suggesting that the primary margin for adjustment lies at the prosecution stage. This pattern is consistent with a model of decision-making under constraints: in judicial systems with limited resources, pursuing cases with weak evidence is costly when conviction is unlikely (Landes, 1971; Easterbrook, 1983). As a result, prosecutors may adopt implicit evidentiary thresholds to decide which cases proceed to trial, which could be affected by extraneous factors such as the media.

To explore how media coverage of VAW may relate to judicial responses, I first investigate their respective trends over time. Using keyword analysis, I identify news stories covering this violence in the French 8pm national TV newscasts. Between 2000 and 2021, such stories accounted for less than 1% of all topics, remaining fairly stable until 2017 and increasing no-

tably thereafter, in the wake of the #MeToo movement. This rise has also been accompanied by a shift in media framing, reflected in the growing use of the term “femicide”. In parallel, I document a steady increase in the number of cases handled by the criminal justice system since 2012, with a marked acceleration beginning in 2017 and a declining dismissal rate for intimate partner violence since 2016. These global trends suggest an alignment between growing media attention to VAW and its judicial handling, especially after #MeToo.

To assess whether this increased visibility of VAW shapes judicial responses, I estimate the causal effects of TV news coverage of VAW on each stage of the criminal justice process. The empirical strategy leverages the quasi-random timing of TV news stories about VAW relative to case processing in the short run, comparing prosecution, conviction, and sentencing decisions made shortly after isolated days of news coverage, versus during periods without such coverage, using a similar design to that employed by [Philippe and Ouss \(2018\)](#). Identification relies on the assumption that, absent news coverage, outcomes in the days following TV news stories about VAW would have evolved similarly to those in the days following no stories, after controlling for case characteristics and seasonality. Empirically, I show that cases are similar regardless of whether decisions are handed down just after news stories. I rule out reverse causality by focusing on news stories about crimes against women that do not mention trials or legislation and are unlikely to relate to the cases under examination. To ensure that results are not driven by changes in the composition of cases entering the system, I also examine impacts on reporting.

I find that TV news coverage of VAW significantly influences prosecutorial decisions in the short run. On average, each additional news story on crimes against women decreases the propensity to dismiss a case in the following seven days by 0.54 percentage points. Relative to a baseline prosecution rate of 23%, this corresponds to a 2.3% increase in the likelihood of prosecuting sexual or intimate partner violence, translating to approximately 260 additional cases prosecuted per year. The effect is concentrated in relatively straightforward cases characterized by greater prosecutorial discretion, such as those involving multiple offenses, shorter investigations, or previous dismissals for factual reasons, and emerges only after the #MeToo movement, at which point the estimate more than doubles. News stories also trigger a modest but immediate reporting response, with around 5 additional cases per news story registered over a one-week period but no change in reporting lag. However, I find no direct impact of media exposure on judges’ decisions regarding conviction or sentence length at trial. Overall,

the results suggest that media attention primarily affects the prosecution stage by bringing more cases to trial.

A key concern is whether this increased likelihood of prosecution leads to weaker cases entering the system. If prosecutors were lowering their standards in response to increased public attention, conviction rates would be expected to drop. However, examining long-term outcomes, I find that cases prosecuted following news coverage seem just as likely to result in conviction as other cases. The absence of any direct effect on judges' decisions, combined with the typically long interval between prosecution and conviction, makes it unlikely that judges are also adjusting their behavior. This suggests that prosecutors are not compromising case quality; instead, cases that might have been dismissed without media attention were legally viable and met the evidentiary threshold for conviction.

I conduct several falsification exercises and robustness checks to ensure that the results are not driven by other underlying trends. In particular, I test whether news coverage of crimes against women affects the propensity to dismiss unrelated offenses, and whether news unrelated to VAW affects the propensity to dismiss VAW cases. Reassuringly, I find no effect in either placebo test. A falsification test using 500 randomly assigned fake treatment dates similarly yields no significant effect. Moreover, controlling for periods of heightened visibility of women (e.g., International Women's Day) or VAW-related legislative changes does not alter the results, helping to rule out potential confounding factors. Finally, the findings remain robust to alternative measures of news coverage and to variations in sample restrictions and model specifications, including limiting the sample to cases registered before the news coverage, controlling for holidays, removing weekends, and excluding each news day, month, or county in leave-one-out analyses.

I investigate three plausible underlying mechanisms for my findings: priming, reputational concerns, and increased effort. The empirical patterns suggest that media coverage alters prosecutorial behavior primarily through strategic, short-run responses to reputational risks rather than through purely unconscious psychological reactions or increased institutional effort. Crucially, the effect does not affect independent judges and emerges only after the #MeToo movement heightened public scrutiny and institutional accountability. Furthermore, I find no such effect for other serious offenses—such as drug trafficking, organized crime, or serious interpersonal violence—suggesting this dynamic is specifically tied to the discretionary margins and normative stakes of VAW cases. The absence of systemic delays

or crowding-out of other violent crimes further confirms that this shift does not stem from a change in total resource allocation.

Overall, these findings underscore the pivotal role of prosecutors in the criminal justice response to VAW. While they dismiss nearly 80% of cases at baseline, this initial decision proves malleable: media coverage increases prosecution rates in the short run, allowing additional viable cases to reach trial without affecting conviction decisions. This suggests that judicial outcomes are not determined solely by evidentiary strength but also by severe capacity constraints, and that they respond to contextual factors such as the public salience of VAW. This reveals that in resource-constrained environments, broad prosecutorial discretion can lead to sub-optimal attrition of merit-worthy cases. From a policy perspective, these results suggest that while increasing the visibility of such violence could significantly improve judicial handling, there is also an urgent need for greater court resources to safeguard the equity and efficiency of the criminal justice system.

Related literature and contributions This paper contributes to three strands of the literature. First, I build on the literature examining how extraneous factors influence judicial decisions. Prior research has mostly focused on judges and jury decision-making, using highly selected samples of tried cases to examine sentencing variations driven by contextual factors (Danziger et al., 2011; Eren and Mocan, 2018; Philippe and Ouss, 2018; Heyes and Saberian, 2019; McConnell and Rasul, 2021; Shumway and Wilson, 2022; Bharti and Roy, 2023; Chen and Philippe, 2023), or by intrinsic characteristics of judges, jurors and defendants (Shayo and Zussman, 2011; Anwar et al., 2012, 2014, 2019; Abrams et al., 2012; Alesina and La Ferrara, 2014; Ash and Marangon, 2024). In contrast, despite the central role of prosecutors in case attrition globally (Daly and Bouhours, 2010; Hohl and Stanko, 2015), prosecutorial decision-making remains comparatively underexplored in the economics literature (Garoupa, 2012; Tonry, 2012; Voigt and Wulf, 2019). This is especially true outside the United States (U.S.), where prosecutors are typically appointed rather than elected, although a few recent studies have examined the effect of salience shocks on prosecutorial decisions in the Netherlands (Bielen and Grajzl, 2021; Hanemaaijer et al., 2026). I contribute to this literature by shifting the focus from sentencing alone to earlier stages of the judicial process, including prosecution, in a modern European setting where judicial actors are appointed civil servants. My results highlight the role of prosecutorial discretion in the handling of VAW cases and the factors that influence it.

Second, I add to a growing literature on the effect of media on policy-relevant behaviors, particularly crime and judicial decisions. Prior work has shown that media exposure is a powerful driver of attitudes and behavior and can challenge entrenched gender norms, for instance by reducing the acceptability of intimate partner violence and increasing the willingness to report it (Jensen and Oster, 2009; DellaVigna and La Ferrara, 2015; Banerjee et al., 2019; Green et al., 2020; Colagrossi et al., 2023). It can also affect judicial behavior by increasing accountability, especially in settings where judges are elected (Lim et al., 2015; Lim, 2015; Ash and Poyker, 2024). Yet, despite the growing visibility of VAW and the challenges of investigating such crimes, little is known about how media coverage shapes judicial responses. Existing evidence is scarce and limited to a few settings. Notably, Vasishth (2022) finds that local news coverage of sexual violence increases conviction rates in India, and Philippe and Ouss (2018) show that general crime coverage affects sentencing decisions made by civilian jurors for severe crimes in France, although their focus is not specifically on VAW. Using new data from France that allow me to examine earlier stages of the judicial process, I contribute to this literature by providing the first causal evidence that media coverage of crimes against women shifts prosecutorial behavior in the handling of sexual and intimate partner violence cases.

Third, this paper complements the literature on the impact of gender norms and policy interventions on gender-based violence. Prior research has examined how institutional reforms (Aizer and Dal Bo, 2009; Iyengar, 2009; Chin and Cunningham, 2019; Ferraz and Schiavon, 2022; Amaral et al., 2023, 2025; Costa et al., 2024; García-Hombrados et al., 2024; Sviatschi and Trako, 2024; Arteaga et al., 2025) and increased female representation (Iyer et al., 2012; Miller and Segal, 2019; Amaral et al., 2021) can reduce prevalence or increase reporting, and how gender norms shape behavior and institutions in the context of VAW (Tur-Prats, 2019; González and Rodríguez-Planas, 2020; Alesina et al., 2021; Guarnieri and Tur-Prats, 2023). Much less is known about how changes in public attention, such as through media coverage, affect institutional responses to this violence. I contribute to this literature by showing that even temporary increases in the visibility of VAW can enhance criminal justice responses. While previous studies have linked social movements like #MeToo to increased reporting of gender-based violence (Gauthier, 2022; Levy and Mattsson, 2023; Battisti et al., 2024), I show that it also marked a turning point in how routine media coverage influences judicial actors. This complements findings by Cai et al. (2024) and Marchenko and Pakzad-Hurson (2025)

regarding the impact of #MeToo on judicial decisions and case processing. More broadly, I demonstrate that shifts in public awareness and visibility can help reshape institutional responses, with concrete implications for how such violence is addressed.

The rest of the paper is organized as follows. Section 2.2 provides a brief overview of the institutional background. Section 2.3 presents the data and some descriptive statistics. Section 2.4 outlines the theoretical basis and empirical strategy. Section 2.5 presents the main results, discussed in Section 2.6. Finally, Section 2.7 concludes.

2.2 Institutional Background

The French criminal justice system The French criminal justice system consists of three main stages shown in Figure 2.1, during which police officers, prosecutors, and judges each make key decisions. All of these actors are civil servants affiliated with the Ministry of the Interior or the Ministry of Justice, with specific provisions for judges, such as guarantees of independence and security of tenure. Unlike in the U.S., where these officials are elected, prosecutors and judges in France are appointed by presidential decree on the recommendation of the Ministry of Justice, after passing competitive entrance exams and completing a formal training program.

Criminal proceedings typically begin when an offense is reported to the police, either through a complaint filed by the victim or a report submitted by a third party, such as a witness. Before launching an investigation, the police must immediately forward information about the reported offense to the public prosecutor. The law stipulates that this transmission must occur as quickly as possible, within a matter of hours or days.³ The public prosecutor may also become aware of an offense through the direct receipt of complaints or denunciations, or through any civil servant who are legally obligated to report if they learn of an offense.

Prosecutors and judges In France, the public prosecutor is the magistrate who advocates for the enforcement of the law and leads criminal proceedings on behalf of society's interest.⁴ Their authority primarily applies to criminal matters, from the initiation of the police investigation to the trial of the defendant. Prosecutors are responsible for qualifying the facts and

³Article 40 of the French Code of Criminal Procedure.

⁴Articles 53 to 80 of the French Code of Criminal Procedure.

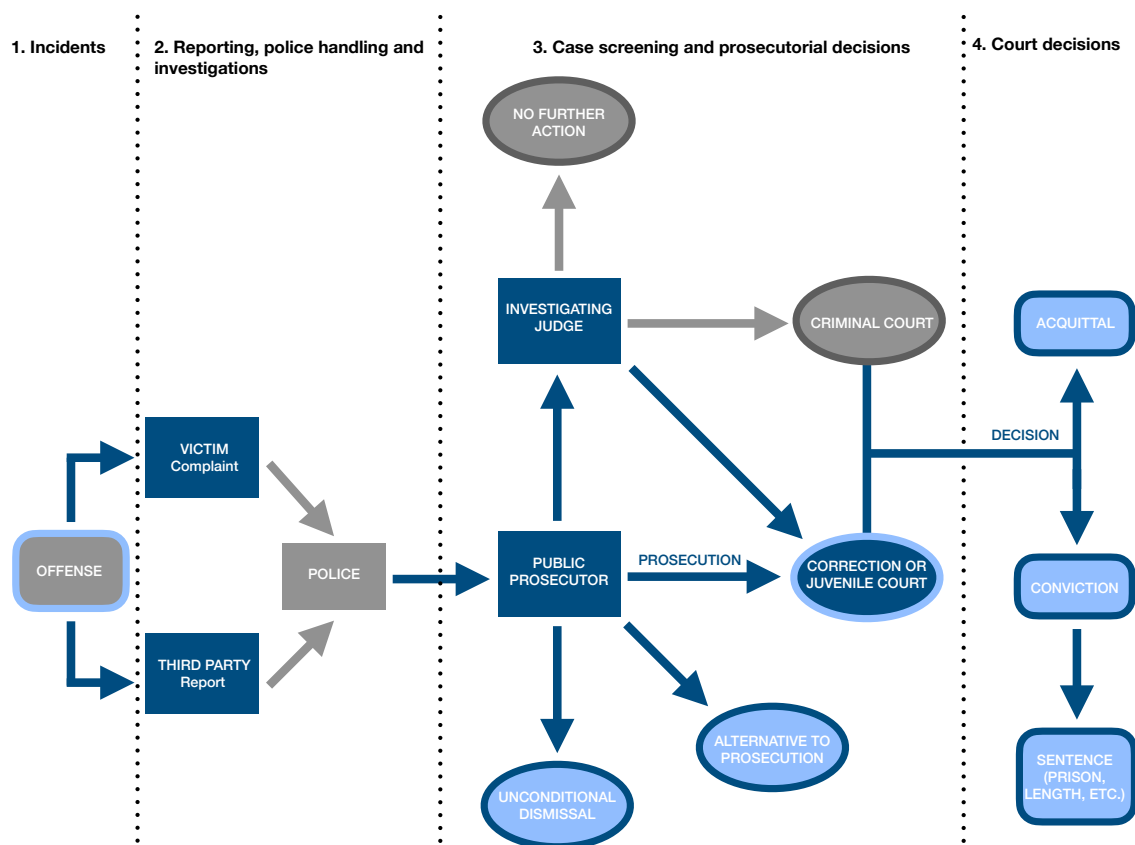


Figure 2.1: Chronology of the French criminal procedure

Notes: Main steps of the criminal justice system procedure in France, from offense reporting to the perpetrator’s final trial. The data contain information only about cases registered by the authorities, without details on the police handling of cases. Among those registered cases, it provides information on the universe of completed cases handled by the criminal justice system, excluding those dropped by the investigating judge or sent to the criminal court (steps shown in gray). In 2019, 84% of cases prosecuted by the public prosecutor’s office were brought before correction courts, 8% before juvenile courts, 5% before police courts and 3% were referred to investigating judges (SDSE, 2019).

are the sole authority in deciding the direction of a case. For example, they supervise police investigations and custody measures, and decide whether to prosecute within a judicial court. Each court has its own public prosecutor’s office, composed of a Chief Prosecutor and several deputies or substitutes, who function as a single, unified body. Members of the public prosecutor’s office are considered indivisible and interchangeable, meaning that any of them may represent the public prosecutor’s office at any stage of the proceedings.⁵ Prosecutors are subject to a hierarchical structure and are subordinate to the Ministry of Justice.⁶ They receive directives and circulars aimed at implementing a comprehensive and uniform criminal policy

⁵In 2023, there were approximately 2,200 full-time equivalent prosecutors across all French jurisdictions (3.2 per 100,000 inhabitants). By contrast, there were about 7,900 professional judges in judicial and administrative jurisdictions (11.5 per 100,000 inhabitants) (SDSE, 2024).

⁶Prosecutors have no fixed term of office; they are appointed to specific posts for a set period (often 3 to 7 years), but may be transferred or promoted over the course of their careers and reassigned without their consent. Unlike in some countries, French public prosecutors and judges do not systematically change with each new government.

across the country, such as guidelines for handling cases and prioritizing certain offenses. In addition, Chief Prosecutors may establish local guidelines within their own judicial court.

By contrast, judges are responsible for applying the law and delivering decisions with full independence and impartiality. In criminal matters, they determine guilt and impose sentences on convicted offenders. Their role is to examine case files, conduct hearings, and render judgments in the name of the people. Their status guarantees independence and security of tenure, meaning that they cannot be reassigned without their consent. This independence is further protected by the fact that the government cannot interfere in their career advancement.

Prosecutors' handling of a case After learning of an offense, prosecutors are responsible for overseeing the police investigation and have several options for directing the case based on the information received (see Figure 2.1). If they decide not to prosecute, they can either pronounce a pure dismissal, for instance, if the perpetrator is unknown or if prosecution seems inappropriate. There is no further investigation, no trial, and the case is closed.⁷ The decision to dismiss a case is subject to a notice from the public prosecutor's office sent to the complainant, indicating the reason for dismissal. Prosecutors can also order alternatives to prosecution (e.g., reparation of damage, compensation for the victim, therapeutic injunction, reminder of the law, etc.), which also legally result in a dismissal and do not appear on the defendant's criminal record.⁸

Alternatively, if prosecutors deem that there are sufficient grounds, they can decide to prosecute the defendant. Prosecuted cases are referred to a trial court or investigating judge, depending on the seriousness and complexity of the facts. If further investigation is needed, prosecutors can refer the case to an investigating judge, which is mandatory for felonies. The investigating judge can then decide to drop the case or send it to court for trial. Prosecutors can choose among several procedural routes to bring a case before a court for trial (e.g., immediate appearance, summons, plea bargaining procedure, etc.). Each has

⁷Dismissing a case is not a final decision. Prosecutors can revisit their decision at any time and choose to initiate legal proceedings, unless the statute of limitations has expired or the perpetrator has died. The complainant can also contest the dismissal and has legal means to ensure that action is taken, either by filing a complaint with an investigating judge (complaint with civil-party application) or by directly referring the case to court through a direct summons (although this is rare in practice), or by filing a civil claim for damages in compensation for the harm suffered—all at their own expense.

⁸Except for the penal composition (*composition pénale*), which is a reinforced alternative to prosecution that is recorded on the criminal record.

distinct implications, for instance regarding the time to judgment, the possibility of pretrial detention, or the severity of the sentence.

Criminal judgment and types of courts A judgment is then handed down by the court, where judges determine whether the offender is guilty or innocent, resulting in either a conviction or an acquittal. There are three main types of trial courts regarding criminal matters in France: (i) correctional courts (*tribunal correctionnel*), (ii) criminal courts (*cour d'assises*) and (iii) juvenile courts (*tribunal pour enfants*). The most serious offenses (felonies, e.g., murder or rape), which carry a maximum prison sentence of more than 10 years, are tried in criminal courts composed of professional judges and civilian jurors. Other offenses (misdemeanors, e.g., sexual assault) which carry a maximum sentence of less than 10 years, are tried in correctional courts composed solely of professional judges.⁹ Offenses involving defendants younger than 18 (resp. 16) at the time of the misdemeanors (resp. felonies) are tried in juvenile courts.¹⁰ This paper focuses on cases tried in correctional and juvenile courts, as the data do not cover criminal courts. Guilt and sentencing are both determined on the day of the verdict in these courts, and the decision can be appealed by the defendant or the prosecutor. Further details on the organization and functioning of the French criminal justice system are provided in Appendix 2.A.

2.3 Data and Descriptive Statistics

2.3.1 Judicial data

Data source The main data source on judicial decisions comes from the management software *Cassiopée*, provided by the French Ministry of Justice. This software is used by court clerks within French judicial courts to process all offenses related to minor offenses, mis-

⁹In principle, rape is classified as a felony under French law and should therefore be tried in criminal court. However, a relatively large proportion of rapes are downgraded to misdemeanors (*correctionnalisation*) to be tried in correctional courts, either upon referral to the public prosecutor's office or following an investigation. Law n°2004-204 of March 9, 2004, legalized this judicial reclassification process to relieve congestion in criminal courts. Several studies based on court files estimate that almost half of all sexual assaults prosecuted in correctional courts are, in fact, reclassified rapes (Cromer et al., 2017). In 2018, 80% of rape convictions were handed down by criminal courts and 20% by other jurisdictions (SDSE, 2019). My results are robust to excluding rapes from the analysis.

¹⁰At first instance, correctional courts are made up of three professional judges or a single judge for less serious offenses. Juvenile courts are made up of professional and civilian jurors in the case of felonies and only professional judges when it comes to misdemeanors.

demeanors, and felonies, committed by individuals (adults and minors) or legal entities. It enables court clerks and magistrates to keep track of all ongoing cases for administrative purposes.¹¹ The French Ministry of Justice also exploits these data for statistical purposes.

The micro-data extracted from this software provide unique and rich information on all completed cases received by prosecutors and judges, classified as dismissed or prosecuted in correctional or juvenile courts from 2012 to 2023.¹² The software was gradually rolled out to all public prosecutors' offices between 2008 and 2013, with data completeness ensured from 2012 onwards. However, since data are available only for completed cases and not for those still being processed, the Ministry of Justice's statistics department considers them reliable only up to two years before the latest release. For these reasons, and because judicial activity was substantially disrupted by the COVID-19 pandemic in 2020, I limit the analysis to the 2012–2019 period. With one observation per case (author \times offense level), the initial dataset contains over 22 million charge-level observations for this period.

This source collects information on the incident (e.g., nature of the offense, number of offenses, number of victims), the perpetrator (e.g., gender, age, nationality, recidivism), the jurisdiction that handled the case (type and location), the decision to dismiss or prosecute the case made by the prosecutor and the associated motives, the decision to convict or acquit the defendant made by the judge and the associated sentence, as well as the dates of each stage in the process. Information pertains only to the judicial process, with no data on police handling, preventing me from accessing direct information on police practices related to the recording, forwarding, or investigation of cases. Because I do not observe the exact date of the complaint, I approximate the reporting date using the date on which the case is first registered within the criminal justice system, as transmission from the police to prosecutors is legally required to occur almost immediately (see Section 2.2). In addition, the data include no personal identifiers for prosecutors or judges, including gender and age, as their collection and analysis are prohibited under French law.¹³

Case identification The nature of cases is identified by an official nomenclature, which assigns a main case type (NATAFF) upon arrival at the public prosecutor's office, enabling an

¹¹For example, court clerks use this software to create the date of the summons for the defendant once the hearing date has been set, or to record the date of the decision that will prevail in a contested case.

¹²The data are anonymous and only accessible through a highly secured platform that maintains data confidentiality, the Secure Data Access Center (CASD).

¹³Article 33 of Law No. 2019-222 of 23 March 2019 on the Programming and Justice Reform.

initial classification of the case.¹⁴ Present in all affairs, it is used to classify cases into broad categories throughout the referral process.¹⁵ Using the NATAFF code, I restrict the sample to judicial cases of sexual violence (defined as rape, sexual assault, or sexual harassment) or intimate partner violence (defined as violence by a spouse, partner, or ex-partner), which amounts to 792,802 charge-level observations involving 703,695 identified authors and corresponding to 732,644 affairs (3.6% of all cases handled by the criminal justice system). Marital rapes will generally enter the category of intimate partner violence. When there is more than one incident of sexual violence, the classification refers to the most serious. It is important to note that the NATAFF classification may not capture all cases of intimate partner violence, as some may fall under other categories of physical violence (e.g., assault resulting in a temporary inability to work). These cases can be retrospectively identified as intimate partner violence using their NATINF code, which is assigned at the prosecution stage. However, this correction can only be applied to prosecuted cases for which a NATINF code exists, which may introduce selection bias. That said, this limitation appears to affect only a very small proportion of cases. Additional details on case identification are provided in Appendix 2.B.

2.3.2 Descriptive statistics on judicial data

Table 2.1 presents descriptive statistics on sexual and intimate partner violence cases received by the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. On average, 305 new cases reach the justice system each day, 64% of which are cases of intimate partner violence, 23% sexual assault, 12% rape, and 1% sexual harassment. The vast majority of cases were referred to the justice system by the police (87%), while 7% originated from reports made by other administrations and 5% from individuals (e.g., victims, witnesses) who approached the prosecutor directly. Defendants are predominantly male (90%) and born in France (76%), with an average age of 36.2 years. Fourteen percent of prosecuted defendants were repeat offenders (5% in sexual violence cases and 16% in intimate partner violence cases).

¹⁴The nature of the case (NATAFF) is described using a 350-item nomenclature (subject to minor variations over time). A single case can be assigned up to three different NATAFF codes. I identify cases as relevant if at least one NATAFF code of interest is present among the three, regardless of their order.

¹⁵Beyond this NATAFF classification that marks the beginning of the case, as soon as a prosecution decision is taken, at least one type of offense (NATINF code) is systematically assigned to the case. This code describes the nature of the offense in great detail (with over 18,000 different items), but does not exist for all cases. While magistrates systematically assign a precise legal classification to prosecuted individuals, and therefore a NATINF code, this is done less consistently for dismissed cases.

Table 2.1: Summary statistics on judicial cases of sexual and intimate partner violence

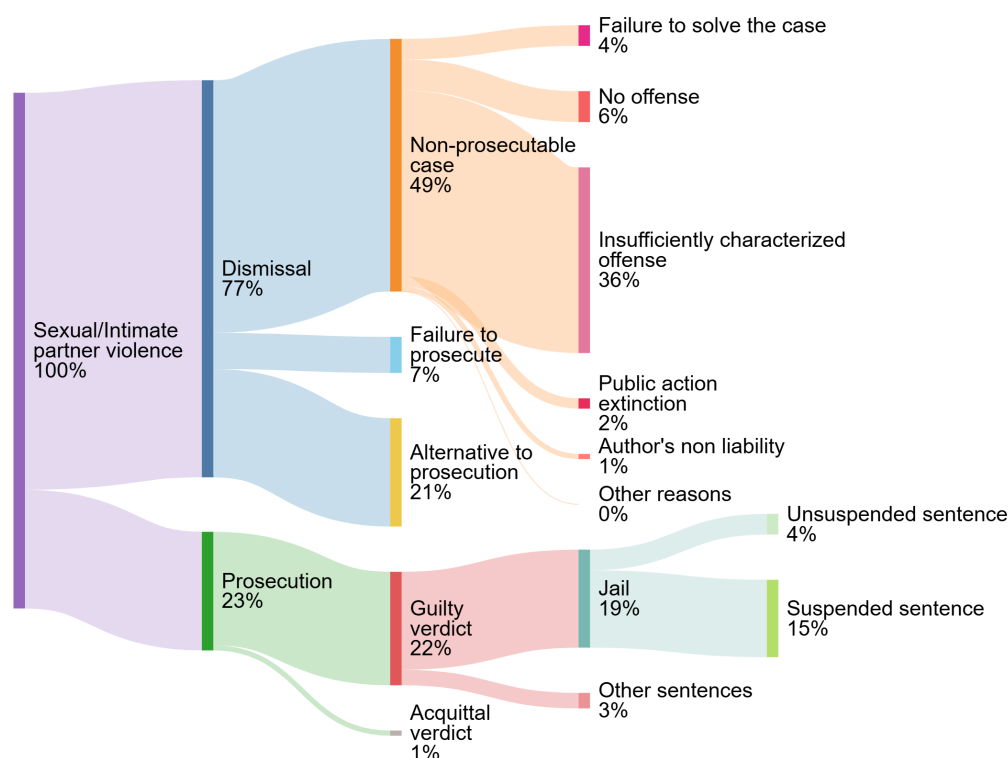
	Mean	Sd	Median	Min	Max
A. Sexual and intimate partner violence cases ($N = 792, 802$)					
No. of new cases per day	305.20	118.75	333.00	77.00	2044.00
<i>Offense:</i>					
Rape	0.12	0.33	0.00	0.00	1.00
Sexual assault	0.23	0.42	0.00	0.00	1.00
Sexual harassment	0.01	0.11	0.00	0.00	1.00
Intimate partner violence	0.64	0.48	1.00	0.00	1.00
<i>Origin:</i>					
Police	0.87	0.34	1.00	0.00	1.00
Persons	0.05	0.22	0.00	0.00	1.00
Justice	0.01	0.11	0.00	0.00	1.00
Other administrations	0.07	0.25	0.00	0.00	1.00
Prosecution rate	0.24	0.43	0.00	0.00	1.00
Conviction rate (if prosecuted)	0.94	0.24	1.00	0.00	1.00
Prison sentence (days) (if convicted)	274.80	330.25	180.00	6.00	6480.00
Investigation length (days) until decision	204.71	215.25	127.00	0.00	1076.00
Investigation length (days) until dismissal	224.80	281.08	120.00	0.00	1814.00
<i>Author:</i>					
Fraction male	0.90	0.30	1.00	0.00	1.00
Age	36.22	13.54	35.00	10.00	96.00
Fraction French-born	0.76	0.43	1.00	0.00	1.00
Fraction recidivist (if prosecuted)	0.14	0.34	0.00	0.00	1.00
B. Sexual violence cases ($N = 279, 511$)					
No. of new cases per day	111.33	54.90	124.00	9.00	663.00
<i>Origin:</i>					
Police	0.71	0.46	1.00	0.00	1.00
Persons	0.10	0.29	0.00	0.00	1.00
Justice	0.03	0.16	0.00	0.00	1.00
Other administrations	0.17	0.37	0.00	0.00	1.00
Prosecution rate	0.17	0.37	0.00	0.00	1.00
Conviction rate (if prosecuted)	0.90	0.30	1.00	0.00	1.00
Prison sentence (days) (if convicted)	557.94	533.32	360.00	8.00	5400.00
Investigation length (days) until decision	383.41	278.49	357.00	0.00	1075.00
Investigation length (days) until dismissal	274.13	324.41	149.00	0.00	1789.00
<i>Author:</i>					
Fraction male	0.94	0.23	1.00	0.00	1.00
Age	32.08	16.76	30.00	10.00	96.00
Fraction French-born	0.81	0.39	1.00	0.00	1.00
Fraction recidivist (if prosecuted)	0.05	0.22	0.00	0.00	1.00
C. Intimate partner violence cases ($N = 519, 183$)					
No. of new cases per day	196.37	68.75	208.00	52.00	1407.00
<i>Origin:</i>					
Police	0.96	0.19	1.00	0.00	1.00
Persons	0.02	0.15	0.00	0.00	1.00
Justice	0.00	0.07	0.00	0.00	1.00
Other administrations	0.01	0.10	0.00	0.00	1.00
Prosecution rate	0.29	0.45	0.00	0.00	1.00
Conviction rate (if prosecuted)	0.95	0.21	1.00	0.00	1.00
Prison sentence (days) (if convicted)	204.62	201.37	150.00	6.00	6480.00
Investigation length (days) until decision	159.56	168.27	108.00	0.00	1076.00
Investigation length (days) until dismissal	196.43	248.61	105.00	0.00	1814.00
<i>Author:</i>					
Fraction male	0.88	0.32	1.00	0.00	1.00
Age	37.83	11.64	36.00	10.00	96.00
Fraction French-born	0.73	0.44	1.00	0.00	1.00
Fraction recidivist (if prosecuted)	0.16	0.37	0.00	0.00	1.00

Notes: Summary statistics on all cases of sexual and intimate partner violence received across all French jurisdictions and either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019.

Reading: Between 2012 and 2019, there was an average of 305 new sexual and intimate partner violence cases registered daily within the French criminal justice system, with 64% of these cases pertaining to intimate partner violence. Among these cases, 24% were prosecuted, and 94% of the prosecuted perpetrators were convicted.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Figure 2.1: Judicial handling of sexual and intimate partner violence cases (2012-2019)



Notes: Share of cases at each stage of the criminal justice system process, among all cases of sexual and intimate partner violence received across all French jurisdictions and either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019.

Reading: 77% of sexual and intimate partner violence cases completed between 2012 and 2019 were dismissed, with 49% of all cases dismissed because they were deemed not prosecutable. The perpetrator was prosecuted in 23% of cases and found guilty in 21%, with a prison sentence imposed in 19% of all sexual and intimate partner violence cases.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

The prosecution rate is low: only 24% of cases are prosecuted on average (17% for sexual violence and 29% for intimate partner violence), with some variation across courts and counties (Appendix Figures 2.B.1 and 2.B.2). As a result, this initial stage of decision-making is a key driver of case attrition. Figure 2.1 shows that 21% of all cases are dismissed with an alternative to prosecution (mostly a simple reminder of the law or mediation), 7% are dismissed following a failure to prosecute (notably because the victim does not comply with requests for information or because the perpetrator cannot be identified), and about half are dismissed because they are deemed non-prosecutable. Within this last category, the most common reason is that the offense is insufficiently characterized (36% of all cases). This does not imply that the offense did not occur, but rather that there is not enough evidence or the facts are not clearly established. Because this ground for dismissal hinges on evidentiary assessments, it entails substantial prosecutorial discretion and is particularly salient in sexual and intimate partner violence cases.

Indeed, Appendix Figure 2.B.3 shows that the dismissal rate for any other type of crime—which are mainly thefts or burglaries (28.62%), driving violations such as alcohol or drugs use (8.76%), physical violence (8.40%) or property damage (8.34%)—is also very high (80%). However, these affairs are mainly dismissed due to failure to solve the case, notably because the perpetrator is only identified in three out of five cases. In contrast, perpetrators of sexual and intimate partner violence are identified in 90% of the cases.

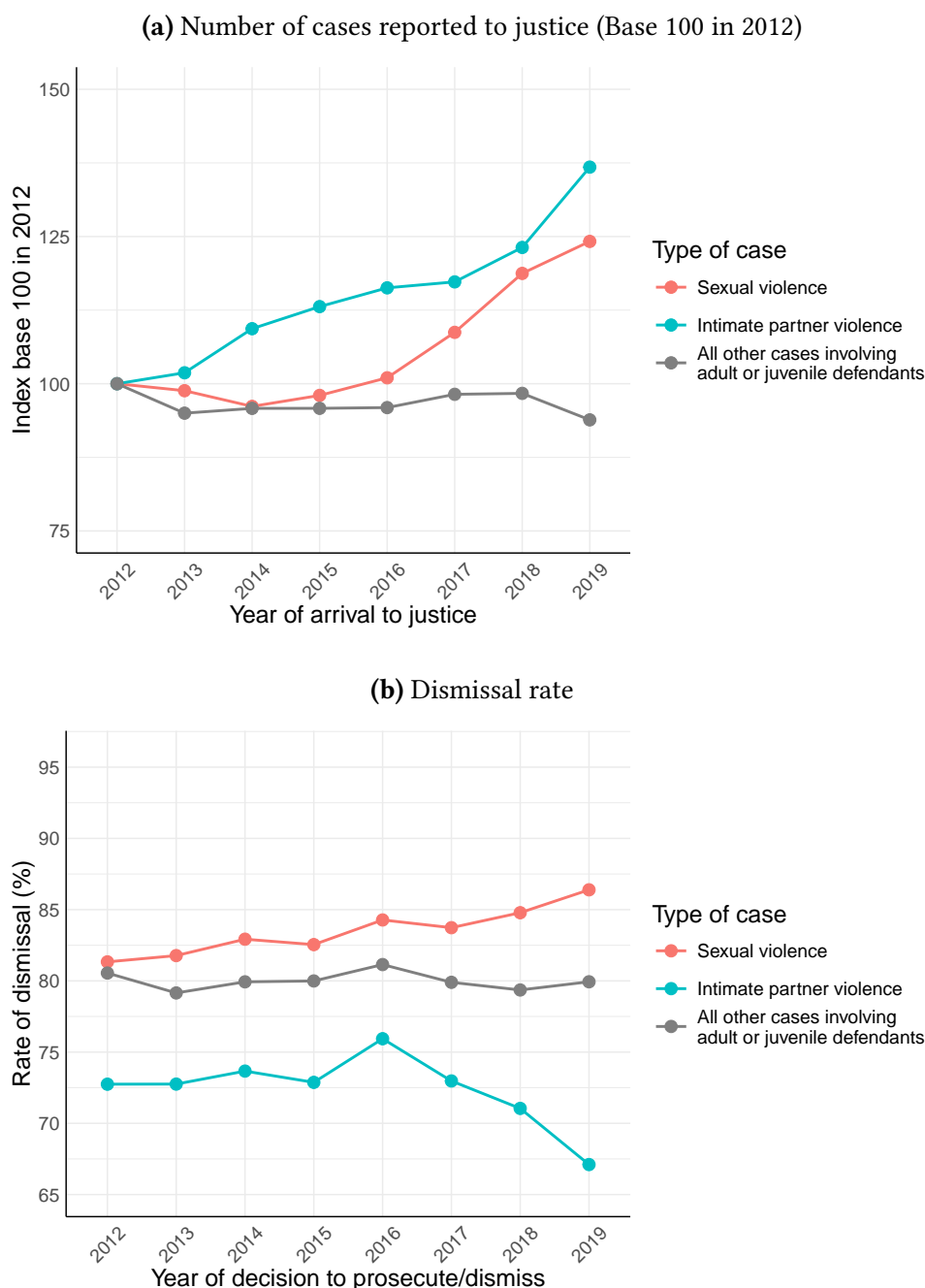
Strikingly, once prosecuted, the vast majority of defendants (94%) are found guilty at trial (90% for sexual violence and 95% for intimate partner violence). Appendix Figure 2.B.4 displays the share of cases prosecuted by mode of prosecution. Combining the prosecution and trial decisions, Figure 2.1 shows that one fifth of all cases ultimately led to a conviction and 19% to a jail sentence, but only 4% to an unsuspended sentence, in which the offender is actually incarcerated. These stylized facts highlight the considerable power and discretion exercised by prosecutors in France, who determine the outcome (most often, the dismissal) of most cases. By contrast, there appears to be little room to influence judicial decision-making at the conviction stage since most prosecuted cases result in a conviction.

Regarding global trends, Figure 2.2a shows that the number of sexual and intimate partner violence cases registered within the criminal justice system has been steadily increasing since 2012, while it has remained stable (or even slightly decreased) for other types of offense. The evolution in absolute numbers is shown in Appendix Figure 2.B.5. The rise accelerated in 2017, the year when the #MeToo movement¹⁶ emerged and the French government announced a series of new measures to combat domestic violence, which began to be implemented in 2018. This increase is more likely to be explained by a change in the victims' behavior, who report more, than by a change in the police behavior, taking more complaints. Indeed, Appendix Figure 2.B.6 shows that the average number of days between the start of the incident and its arrival to justice has also increased over time, suggesting that victims tend to report older incidents. For reference, Appendix Figure 2.B.7 shows the evolution of victimization and reporting rates for this type of violence over time, computed from the French victimization survey.

Finally, Figure 2.2b shows a drop in the share of intimate partner violence cases dismissed from 2016 onward, contrasting with a slight increase in the dismissal rate for sexual violence.

¹⁶In October 2017, a series of public disclosures of sexual harassment and assault in work-related contexts by film producer Harvey Weinstein gave rise to the global #MeToo movement, sparking a flood of statements about experiences of sexual violence. Although the case originated in the United States, it also had a significant media impact in France, leading to the creation of the #BalanceTonPorc movement.

Figure 2.2: Evolution of reported cases and dismissal rates (2012–2019)



Notes: Time evolution of cases of sexual violence, intimate partner violence (IPV), and other crimes reported to the French criminal justice system between 2012 and 2019. Panel A shows the number of cases relative to 2012 levels. Panel B shows the share of cases dismissed by public prosecutors.

Reading: Compared to 2012, the number of sexual violence cases increased by 25% in 2019 (Panel A), while the dismissal rate for IPV cases decreased from 73% in 2012 to 67% in 2019 (Panel B).

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

The rise in dismissals was especially pronounced for rape and sexual assault, while the share of sexual harassment cases dismissed fell slightly (see Appendix Figure 2.B.8). Many reasons could explain these global trends. For instance, the lower dismissal rate for intimate partner

violence could be partly explained by changes in the criminal justice system policy following the various measures implemented by the French government to fight this violence. Besides, the increase in sexual violence complaints recorded by the police since #MeToo came along with a rise in reporting of old incidents, which are more difficult to prove and therefore to prosecute (Baux et al., 2020). The aim of this paper is to examine whether the media can explain some part of these global trends, focusing on the effect of news coverage of VAW in the short run.

2.3.3 TV news data

Data source To measure media coverage of VAW, I use archives of all 8pm news broadcasts (i.e., the prime-time news program) of the main French national TV channels¹⁷ collected by the National Audiovisual Institute (INA) from 2000 to 2021 (Lezer, 2022). An example of the data structure appears in Appendix Table 2.C.1. For each news story, these data provide information on the channel, date and time of broadcast, the duration of the story and program type, a title (about 7 words), a short summary (about 48 words), and a list of keywords describing its content (about 12 words). The INA staff manually document information for each show broadcast on TV and assign keywords using a precise standardized list (Philippe and Ouss, 2018). I also have access to the audience ratings for each news broadcast, as calculated by Médiamétrie, the official organization responsible for measuring TV and media audiences in France. I focus on TV news as television remains one of the main sources of information in France on that period.¹⁸

Identification of stories Using these data, I identify stories on VAW through a keyword search in all text-based variables. I code a news broadcast as featuring a news story about VAW if the title, summary, or keywords variables include words pertaining to sexual violence

¹⁷TF1, France 2, France 3, Canal+, France 5, M6, Arte.

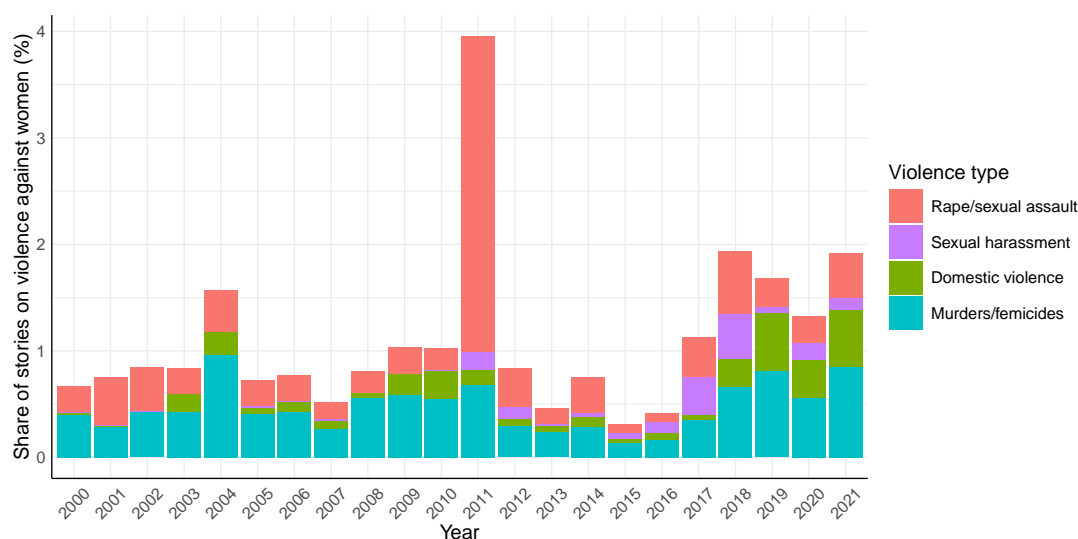
¹⁸As highlighted by Kennedy and Prat (2019), the main French media groups are centered around television, which remains citizens' main source of information. This pattern is confirmed by the 2021 Reuters Institute Digital News Report, indicating that television was still the primary source of news in France from 2013 to 2021, though it declined slightly in favor of online media. Relying on TV news offers significant methodological advantages: the INA provides exhaustive and highly reliable data, avoiding the limitations of retroactive social media scraping, and TV coverage is strongly correlated with other mainstream media like radio (see Appendix Figure 2.C.16). Appendix 2.C.2 uses ELIPSS survey data to provide further evidence on television news consumption and viewer profiles. While TV news content is more heavily curated and its audience tends to be slightly older than social media users, its occupational profile closely mirrors the general population.

(rape, sexual assault, sexual harassment) or intimate partner violence and femicides.¹⁹ I make some restrictions to exclude news stories related to violence against children and pedophilia in the church to focus on women and adolescents only. I also exclude cases taking place abroad (except those involving French nationals and #MeToo) to focus on the domestic context of victims, prosecutors, and judges. Appendix Figure 2.C.2 displays the most common keywords that appear in the selected news stories.

I then group these stories on VAW into three exclusive categories based on their content: (i) current crimes (e.g., rape, murder, or disappearance cases, excluding trials), (ii) trials, and (iii) law and society (e.g., awareness campaigns, legislation/policies to fight VAW, societal issues, etc.). Appendix 2.C.1 presents the full list of keywords used to perform this classification. I manually reviewed all news stories of the final sample to discard errors. This methodology gives me 100% precision (zero false positives or type I errors) but does not prevent missing news stories (some false negatives or type II errors).

2.3.4 Descriptive statistics on TV news data and sample restriction

Figure 2.3: Share of stories on VAW in national 8pm TV news, by type of VAW



Notes: Time evolution of the share of TV news stories (in number) about violence against women (VAW) in the 8pm news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte), by type of violence.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

¹⁹The precise list of keywords is the following: rape, sexual violence, sexual assault, sexual harassment, street harassment, sexual abuse, molestation, rapist, sexist violence, sexual crime, forced sexual intercourse, intimate partner violence, conjugal violence, femicide, marital drama, crime of passion, gender violence, woman+murder, woman+assault, woman+violence, girl+murder, girl+assault, girl+violence, spouse+murder, spouse+violence, murder+NAME.

Figure 2.3 shows the evolution of the share of news stories on VAW among all stories covered in the 8pm news broadcasts on the seven national channels listed above. On average, VAW stories represent 0.9% of all news stories broadcast on this program during the 2000-2021 period, and a similar share of airtime (see Appendix Figure 2.C.10). The surge in the share of stories on rape and sexual assault in 2011 corresponds to the criminal case involving Dominique Strauss-Kahn, a French political figure, which is excluded from my main analysis.²⁰ In comparison, this figure stands at 4.7% if we focus on news stories pertaining to crime in general (Appendix Figure 2.C.11). The share of VAW stories has risen over time, particularly since 2017 concerning rape and sexual harassment. This evolution aligns with the series of public disclosures of sexual harassment and assault in work-related contexts by the film producer Harvey Weinstein, which sparked the #MeToo movement in October 2017. Furthermore, there has been a notable increase in coverage of intimate partner violence and femicides since 2018, following the high-profile case of Alexia Daval’s murder by her husband in Haute-Saône and the French government’s announcement of various measures to combat domestic violence.

Besides the amount of VAW cases covered, the way in which these stories are covered on TV newscasts also seems to have evolved. Most VAW stories relate to crime news (48%), while 31% cover trials and 21% address legal and societal aspects related to VAW. The share of trial stories has decreased over time, while coverage of the legal and societal aspects of VAW has increased. In contrast, coverage of crimes against women remained broadly unchanged over the period (see Appendix Figure 2.C.12). Moreover, the increase in news coverage of VAW has been accompanied by a reframing of how such violence is portrayed in the media. Appendix Figure 2.C.13 shows that, among news stories dealing with VAW, the word “femicide” began to be used more frequently starting in 2019, while the share of stories employing terms that tend to minimize the violence – such as “family drama” or “crime of passion” (Houel et al., 2014; Sapio, 2019; Bazin, 2022) – has declined. This shift, which notably began in 2019, suggests a changing narrative around the nature and seriousness of such violence in the media.

Sample and time restriction For the empirical analysis, I restrict the sample to France’s

²⁰In May 2011, the economist and politician Dominique Strauss-Kahn was accused of sexual assault and attempted rape by Nafissatou Diallo, a hotel maid at the Sofitel in New York. At the time, DSK led voting intentions for the French 2012 presidential election, explaining why this affair has received a lot of media attention in France. However, this case was more an exception than a turning point in news coverage of sexual violence. It is not included in my main analysis sample, starting in 2012.

two main free national TV channels (TF1 and France 2), which are the best documented by INA staff and attract the largest audiences (see Appendix 2.C.2), over the same analysis period (2012–2019) as the judicial data.²¹ These two channels had average audiences of 10.4 million viewers per day for the 8pm news program on this period (5.8 million for TF1 and 4.6 million for France 2, from a French population of 66 million inhabitants), drawing about 47% of French television viewers.²² Daily prime time starts at 8pm, lasts about 35 minutes, and covers an average of 26 stories on each of these channels.

Appendix Figures 2.C.14 and 2.C.15 show the evolution of news coverage of VAW over time on TF1 and France 2. Such coverage provides a reliable proxy for broader media attention to VAW, as Appendix Figure 2.C.16 shows a strong correlation between VAW coverage on these two channels and coverage in other media outlets. Therefore, although older individuals are overrepresented among TF1 and France 2 viewers (Appendix 2.C.2), these TV channels remain a major source of news and closely track coverage in other media, particularly radio, which is the medium most likely to be consumed by magistrates.

Following the design of Philippe and Ouss (2018), I construct three main measures of TV news coverage of VAW, for each news category, grouping stories from TF1 and France 2 together: the number of stories, their duration in minutes, and dummies for whether these topics were covered at all. Table 2.2 shows that between 2012 and 2019, there was at least one news story on VAW in 11% of the days, mostly covering current crimes against women (5% of the days). Conditional on a topic being covered, the average number of stories on VAW rises to 1.87 per day, for a total duration of nearly 3 minutes, with 70% aired during the news opening. The main analysis focuses on crime-related news stories due to their high editorial prominence, whereas other types of coverage (such as trials or legislation) are more likely to raise endogeneity concerns (see Section 2.4).

Examples of such news stories are provided in Appendix Table 2.C.3. Most of these crime-related news stories concern extreme cases of VAW, as Appendix Figure 2.C.4 shows that the majority involve serious felonies (rape or murder).²³ Finally, Figure 2.4 displays the weekly

²¹Although I focus on TF1 and France 2 for the main estimation sample to ensure better data quality, I present descriptive statistics over a longer period and across all available channels to fully exploit the richness of this unique dataset and provide a broader overview of media coverage of VAW in France.

²²Author's calculations using the INA dataset.

²³Although my justice data are intended to cover only a small share of such cases – many of which are tried in criminal courts – there is still a clear link between the two: they relate to VAW of varying severity, and many rape cases are reclassified so that they are tried in correctional courts, and are therefore also handled by the magistrates I analyze.

Table 2.2: Summary statistics on 8pm TV news content (TF1 & France 2, 2012-2019)

	Mean	Sd	Median	Max
<i>Probability of at least one story per day:</i>				
Violence against women	0.11	0.31	0.00	1.00
Crime	0.05	0.23	0.00	1.00
Trial	0.03	0.17	0.00	1.00
Law & society	0.04	0.19	0.00	1.00
<i>Number of stories (conditional on coverage):</i>				
Violence against women	1.87	1.33	1.00	8.00
Crime	1.59	1.07	1.00	8.00
Trial	1.46	1.00	1.00	8.00
Law & society	1.69	1.08	1.00	7.00
<i>Time in minutes (conditional on coverage):</i>				
Violence against women	2.80	2.37	2.00	13.77
Crime	2.10	1.86	1.66	12.23
Trial	1.85	1.41	1.54	8.30
Law & society	3.23	2.26	2.67	12.07
<i>Share of stories in news opening (conditional):</i>				
Violence against women	0.70	0.29	0.67	1.00
Crime	0.82	0.25	1.00	1.00
Trial	0.85	0.25	1.00	1.00
Law & society	0.70	0.30	0.67	1.00

Notes: Summary statistics on the coverage of violence against women (VAW) in the 8pm TV news broadcasts of TF1 and France 2 between 2012 and 2019. While the first block (probability) is calculated over the full sample, statistics for the number of stories, duration, and position are conditional on there being at least one news story on the topic on a given day. The "news opening" statistic represents the share of stories broadcast within the first quartile (top 25%) of the total number of subjects in a newscast.

Reading: On average between 2012 and 2019, there was at least one news story on VAW broadcast on TF1 and France 2 prime-time news on 11% of the days, and a news story on crimes against women on 5% of the days. On those specific days, the average number of stories on VAW was 1.87, and 70% of them were aired during the news opening.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

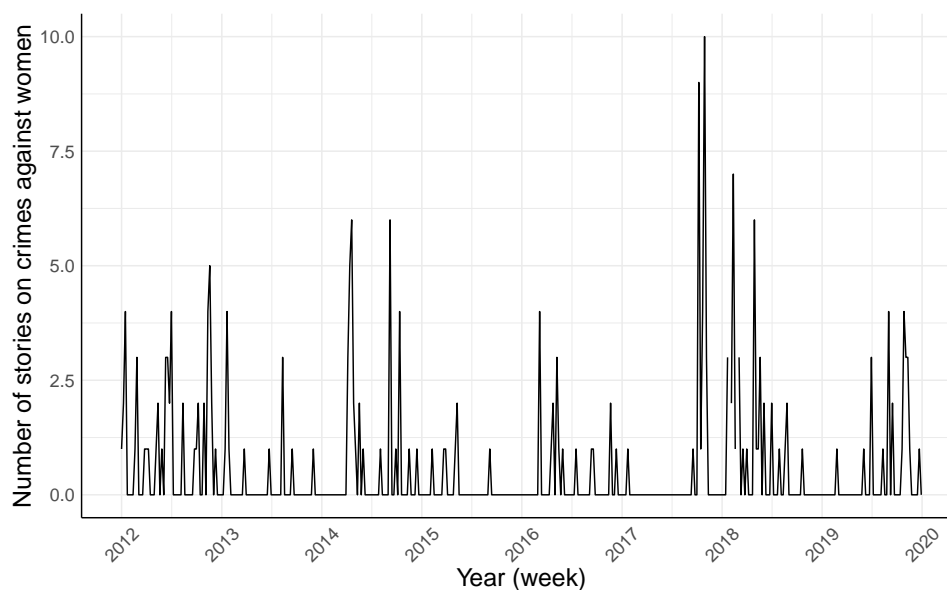
variation in news coverage of crimes against women in the 8pm prime-time TV news used for the analysis. The data reveal significant fluctuations in coverage, with extended periods containing no news on this topic at all.²⁴

2.4 Empirical Framework

The previous descriptive results suggest an alignment between growing media attention to VAW and judicial responses to such violence over time. Whether this apparent alignment reflects a causal relationship is the key question I address next. Before turning to how I address this question empirically, I briefly outline the theoretical basis for understanding whether and how media coverage of VAW could affect criminal justice responses.

²⁴Reassuringly, Appendix Figure 2.C.17 shows that news coverage of VAW is essentially uncorrelated with coverage of unrelated topics (e.g., politics, sports, travel, cooking, gardening, leisure, DIY) and other types of crime, suggesting that this measure does not capture broader or unrelated media trends.

Figure 2.4: Weekly evolution of news coverage of crimes against women in TF1 & France 2 8pm TV news



Notes: Time evolution of the number of stories on crimes against women in the 8pm TV news broadcast on TF1 and France 2, per week, from 2012 to 2019.

Reading: The weekly number of news stories on crimes against women broadcast on TF1 and France 2 prime time news ranged from zero to ten between 2012 and 2019.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

2.4.1 Theoretical basis

In most country settings, judicial decision-makers – such as prosecutors and judges – can be modeled as agents seeking to minimize expected social harm, subject to informational and resource constraints (Landes, 1971). In this context, social harm includes both the direct harm caused by the crime and the expected harm from future offenses, conditional on the current decision. Future harm is minimized by obtaining maximum deterrence given available resources (Easterbrook, 1983). Regarding informational constraints, agents face a fundamental trade-off when deciding whether to prosecute or seek a conviction: balancing Type I errors (wrongfully prosecuting or convicting an innocent person) against Type II errors (failing to prosecute or convict a guilty person) (Kaplow, 1994). The perceived social cost of each type of error—and the relative weight assigned to it—may vary depending on the nature of the crime. For example, this trade-off is highly salient in VAW cases, where the cost of under-prosecution can be especially severe due to repeat victimization and the risk of escalation (Jelveh and Motta, 2024).

Agents also face resource constraints, including limits on time, effort, and attention they can devote to each case, a challenge that is especially salient for prosecutors. Given

the limited resources of the criminal justice system, pursuing cases with weak evidence is costly, particularly if they are unlikely to result in a conviction (Reinganum, 1988; Baker and Mezzetti, 2001; Garoupa, 2009; Bandyopadhyay and McCannon, 2015). Consequently, prosecutors may adopt implicit evidentiary thresholds based on the probability of making a correct decision; cases falling below these thresholds are dismissed rather than prosecuted. Previous research in criminology suggests that in VAW cases, prosecutors tend to prioritize minimizing the risk of pursuing cases unlikely to yield a conviction over avoiding the dismissal of cases that likely would (Frohmann, 1991).

Channels of media influence. External factors such as media coverage can influence judicial decision-making through three primary channels. First, news coverage may shift attention and change the perceived risk of making a wrong decision by altering the weights assigned to Type I versus Type II errors. By increasing the perceived social harm or the salience of recidivism risk, media coverage could effectively raise the perceived cost of a Type II error. This shift could move implicit evidentiary thresholds, potentially leading prosecutors to behave sub-optimally due to myopia or recency bias. Second, media attention may alter these evidentiary thresholds through external pressures, such as heightened public scrutiny, reputational risk, or fairness concerns. Third, it may prompt the allocation or substitution of more resources, time, and effort toward VAW cases, resulting, for instance, in more thorough investigations or faster judgments.

2.4.2 Estimation strategy

To analyze whether increased public attention to VAW causally affect judicial decision-making regarding such crimes, I exploit variation in the exact timing of news coverage and prosecutors' and judges' decisions. I compare decisions made *shortly after* more TV news coverage of VAW with those made following relatively less or no coverage. Since my treatment consists of several news stories on VAW that occur repeatedly over a short period of time and at varying intervals, I focus on the short-term (daily) effect of TV news coverage of VAW. This strategy follows the approach introduced by Philippe and Ouss (2018) and has since been adopted in several other studies using high-frequency media news data in different contexts (Durante and Zhuravskaya, 2018; Emeriau, 2024). Specifically, I estimate regressions of the form:

$$Y_{i,t,j} = \alpha + \sum_{k=-6}^7 \beta_k TV_{t-k} + \sigma X_t + \gamma W_j + \eta Z_i + \epsilon_{i,t,j} \quad (2.1)$$

where $Y_{i,t,j}$ represents the different judicial decisions observed at the case level: (i) a dummy variable for whether the case was dismissed, (ii) (if prosecuted) a dummy variable for whether the defendant was convicted and (iii) (if convicted) the associated sentence length (in log days),²⁵ for a case i whose decision happened on date t in county j . For each decision, the analysis is conducted on a distinct sample of cases that reached this stage, with different event times t based on the decision date. TV_{t-k} captures daily measures of TV news coverage of VAW k days before and after the decision date in t . X_t controls for seasonality (day of the week, calendar month, and year fixed effects) and W_j for the county of jurisdiction that handled the case. Depending on the outcome of interest, Z_i controls for certain offender and case characteristics (age, born in France, number of offenses, number of victims, nature of the offense, repeat offender status, attempted crime, co-offending, mode of prosecution, and mode of judgment; I exclude cases involving female offenders). The main specification is a linear probability model using ordinary least-squares (OLS) regressions, with heteroskedasticity-robust standard errors.²⁶

The coefficient of interest is $\hat{\beta}_k$, which captures the difference in the probability of prosecution, conviction, or average sentence length between (1) cases decided in the days following TV news coverage of VAW and (2) those on other days (i.e., when there is relatively less or no coverage), controlling for observable case characteristics and seasonality.²⁷ Intuitively, I compare otherwise similar cases that differ only in whether they were processed shortly after relatively more coverage of VAW (see illustration in Appendix Figure 2.D.1). Including lead coefficients allows me to test for pre-trends by assessing whether cases decided shortly before news coverage resemble those decided when there is no coverage (Appendix Figure 2.D.2). In my main specification, TV news coverage of VAW is measured by using the number of news stories on crimes against women, but the results are robust to alternative measures.

²⁵I also examine the time taken to reach each of these decisions as additional outcomes.

²⁶The results are robust to clustering standard errors at the county level or day level, among others, which partly addresses potential dependence across cases exposed to the same news coverage.

²⁷Appendix Figure 2.D.3 documents strong seasonal patterns in judicial activity: the number of decisions varies markedly over the week, with substantially fewer decisions issued on weekends (though not zero, consistent with on-call procedures for the most urgent cases). I account for these patterns in the main specification, and I further assess robustness by excluding weekends.

Validity of the empirical design This strategy relies on certain parametric assumptions regarding the treatment, notably the independence of news events and the additivity of their effects, which apply to the continuous specifications using the number or duration of news stories. While including both leads and lags of news coverage in the regression helps account for potential serial correlation in news stories over time, these assumptions may still be violated when a major high-profile case receives coverage over several consecutive days, which also complicates the interpretation of the results. To address this concern, I restrict the main analysis sample to periods with either a single day of TV news coverage of crimes against women or no coverage at all in the seven days preceding the decision. Although this restriction yields a slightly smaller sample than the one used for the descriptive statistics, it allows for a clearer interpretation of the results.²⁸

Moreover, judicial outcomes are conditional on a decision being made. Consequently, the estimated effect may capture both the likelihood that a decision is made (e.g., whether the case is examined by prosecutors or brought before a judge) and the nature of that decision (e.g., whether the defendant is prosecuted or convicted). While this is less of a concern for judges' decisions, as trial dates are usually set months in advance, the timing of prosecutorial decisions may be more easily manipulated. To address this concern, I also examine the cumulative number of dismissal and prosecution decisions over a two-week period to assess whether prosecutors are simply postponing their decisions.

Finally, the composition of cases brought before judicial decision-makers may change following news coverage—for instance, if the news prompts more victims to report or leads police to register additional complaints. To test this possibility, I examine the effect of TV news coverage of crimes against women on the number of cases reported, using as a supplementary outcome the natural logarithm of the number of VAW cases registered with the justice system on date t in county j .²⁹

2.4.3 Identification strategy

Identifying assumption The coefficient $\hat{\beta}_k$ has a causal interpretation under the assumption that, on average, outcomes on days following relatively more TV news coverage

²⁸The results are robust to including periods with multiple days of news coverage.

²⁹The logarithm is used to capture relative effects, given the time trend in the number of reports over the analysis period. There are no days without newly registered sexual and intimate partner violence cases during this period.

of VAW would have been similar to those on days following relatively less or no coverage, had the coverage not occurred, after controlling for case characteristics and seasonality. This assumption holds if the timing of TV news coverage is as good as random in the short run. Although media coverage is unlikely to be exogenous to long-term trends in judicial decisions, the identification strategy only requires it to be exogenous to short-run changes around a news episode. This assumption is likely to hold given that the timing and intensity of daily news stories on extreme crimes against women fluctuate at high frequency in ways that are difficult to anticipate or manipulate by the criminal justice system, at least in the short run. For this reason, I restrict the analysis to short-term effects, capturing the impact of news coverage over a one-week horizon.

Threats to identification There are three main threats to this identification strategy. First, one may worry about reverse causality. Yet it seems unlikely that TV channels adjust their coverage of VAW immediately after changes in judicial decisions. Moreover, most crimes and judicial decisions do not make national news (Philippe and Ouss, 2018): over the analysis period, there are on average only 0.20 news stories per day (Table 2.2), mostly concerning extreme crimes such as murders or rapes, compared with 305 new cases registered per day (Table 2.1). To nevertheless alleviate concerns that the news could anticipate or directly cover upcoming judicial decisions, the main analysis focuses on TV news stories about current crimes against women, excluding those on VAW-related trials or legislation. Estimates including trial coverage are reported as robustness checks. Furthermore, given the time elapsed between offenses and judicial decisions (on average, 567 days between the initial incident and the final decision),³⁰ the crimes covered in the news are not the same as those being processed by the criminal justice system at that time. Finally, to rule out any possible overlap between the judicial cases under examination and the news stories, I present as a robustness check a specification that excludes cases coinciding with coverage of crimes occurring in the same county as the jurisdiction handling them. Appendix Table 2.C.1 shows that only 2.3% of crime stories took place in the same county and 6.7% in an adjacent county.

Second, there may be measurement errors. For instance, news covered in the 8pm TV news broadcast may have already appeared on the same day (or the day before) on radio morning shows or press articles. The effect of news that occurs on the same day as the judicial outcome

³⁰On average, 930 days for sexual violence and 358 days for intimate partner violence; 492 days until conviction and 588 days until dismissal.

is thus ambiguous. Moreover, while I manually reviewed all news stories to ensure no false positives, some stories may still be missing. Overall, the bias that these measurement issues may introduce would likely reduce the estimated effect and hence make the results a lower bound.

Third, there could be unobserved omitted variables affecting both judicial decision-making and the likelihood of covering VAW in TV news broadcasts in the short run (such as the #MeToo movement). These factors are likely to be mostly absorbed by the time fixed effects. I also perform several falsification exercises to ensure that the results are not driven by other time-varying unobservables. Finally, I show that the findings are unlikely to be driven by selection, as TV news coverage is not systematically correlated with most observable characteristics of the judicial cases under examination (see Tables 2.D.1 and 2.D.2). The main notable imbalance concerns the type of violence committed, but the point estimates are small and marginally significant, and this variable is explicitly controlled for in the regression models. Crucially for the identification strategy, the last column of Table 2.D.1 shows that the time elapsed between the registration of a case at the prosecutor's office and the final prosecutorial decision is not associated with media content in the days preceding the decision. This rules out the possibility of strategic manipulation based on news coverage and supports the validity of the design: the exact timing of prosecutorial decisions is orthogonal to media coverage of crimes against women, and cases examined before and after coverage are similar with respect to observable characteristics.³¹

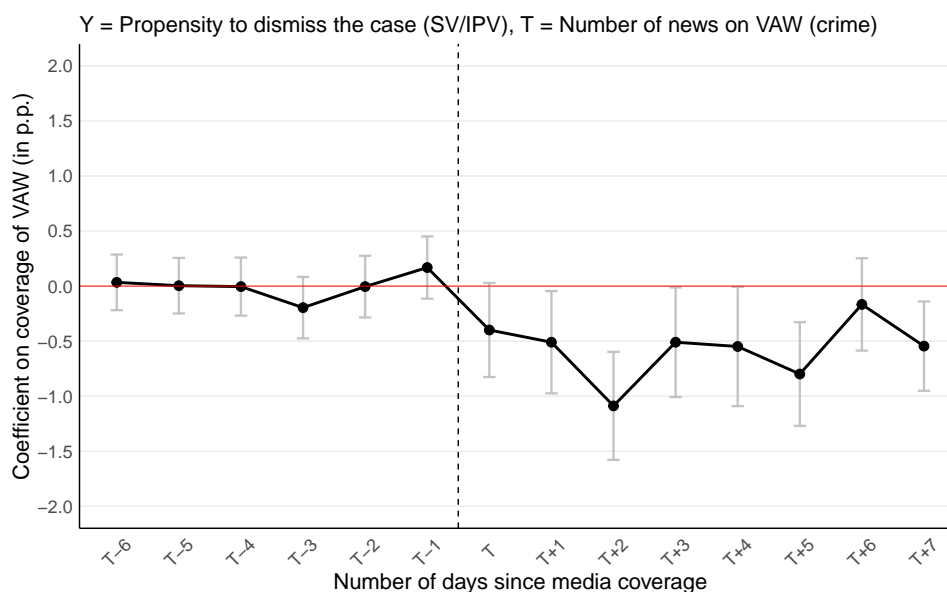
2.5 Results

2.5.1 Main results

Prosecution I start by analyzing the effect of news coverage on prosecutors' decisions. The estimation results from Equation 2.1 are displayed in Figure 2.1, which shows the effect of TV news coverage of crimes against women on the propensity to dismiss cases of sexual or intimate partner violence, for each lead and lag around the news coverage, which occurred

³¹I do not observe the exact dates of trial scheduling, which prevents me from running an analogous test for judicial decisions. However, concerns about manipulation of such decisions are less relevant, as trial dates (and their duration) are typically set months in advance according to court calendars and are therefore much harder to adjust strategically in response to media coverage.

Figure 2.1: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Reading: Two days after the TV news coverage, the propensity to dismiss sexual and intimate partner violence cases decreases by 1 percentage point (significant at the 5% level).

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

at time T .³² I find no evidence of pre-trends, consistent with cases decided just around news coverage being comparable to those decided on days without coverage. The figure highlights a significant drop in the propensity to dismiss a case following news coverage, starting on the day the coverage airs and lasting up to six days afterward. A particularly pronounced decrease is observed at $T+2$, a pattern primarily driven by intimate partner violence cases (see Appendix Table 2.F.1 for precise lead and lag estimates).³³ On average, each additional news story on crimes against women decreases the propensity to dismiss a case in the following seven days by 0.54 percentage points (Table 2.1). Given the baseline prosecution rate of 23% for such violence, this effect corresponds to a 2.3% increase in the likelihood of prosecution. The impact is short-lived: while the dismissal rate decreases in the week after the coverage, it returns to its prior level thereafter (Appendix Figure 2.F.1). This indicates that prosecutors

³²For interpretability, the event time is redefined relative to the date of news coverage (T), such that negative values correspond to days before coverage and positive values to days after.

³³This lag in the effect for IPV, compared to the more immediate response for sexual violence, may reflect differences in case processing or police transmission speeds. The sociology literature suggests that IPV cases often face procedural hurdles or a tendency among legal actors to discredit such claims (Pérona, 2017, 2023), potentially leading to longer administrative delays before prosecution. However, the available data do not allow for a formal test of this institutional mechanism.

Table 2.1: OLS results for the effect of news coverage of crimes against women on each stage of the criminal justice process

<i>Dependent Variables:</i>	Reporting	Prosecution		Sentencing	
	Log. Number of new cases reported (1)	Dismissal (0/1) (2)	Log. Number of decisions made by prosecutors (3)	Conviction (0/1) (4)	Log. Sentence length (days) (5)
Number of news on VAW crimes in the 7 days before the decision	0.0171** (0.0080)	-0.0054*** (0.0009)	0.0081 (0.0189)	-0.0014 (0.0012)	0.0010 (0.0037)
Number of news on VAW crimes in the 6 days after the decision	-0.0002 (0.0021)	-7.02×10^{-5} (0.0003)	0.0015 (0.0032)	0.0003 (0.0004)	-0.0023* (0.0013)
Year, month, day-of-the-week FE	Yes	Yes	Yes	Yes	Yes
County of jurisdiction FE	Yes	Yes	Yes	Yes	Yes
Case-level controls	No	Yes	No	Yes	Yes
<i>Fit statistics</i>					
Observations	2,647	687,198	2,647	150,703	123,686
R ²	0.80745	0.18649	0.86295	0.03079	0.48931
Within R ²	0.00206	0.12849	0.00016	0.02544	0.47545

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

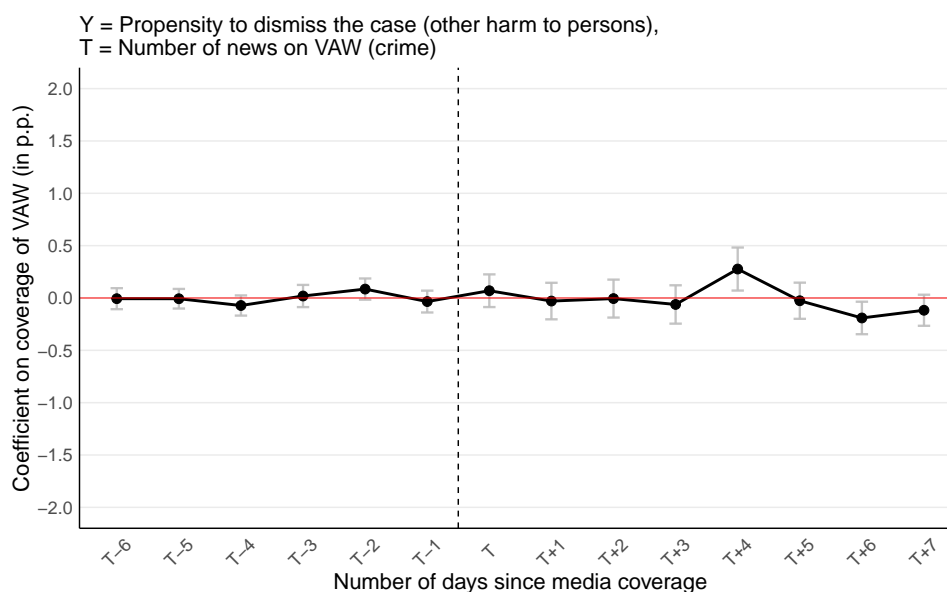
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on reporting, prosecution and sentencing decisions, pooling the leads and lags together into two event-time dummies. The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. In Column (1), the dependent variable is the natural logarithm of the number of new cases registered within the criminal justice system. In Column (2), the dependent variable is a dummy equal to one if the case was dismissed by prosecutors. In Column (3), the dependent variable is the total number of decisions (log) made by prosecutors regarding such cases. In Column (4), the dependent variable is a dummy equal to one if the case led to a conviction. In Column (5), it is the natural logarithm of the sentence length (in days) for convicted cases. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. Case-level controls include: French-born author, author's age, number of victims, type of assault (Columns 2 to 5), dummies for attempt, complicity, recidivism, mode of prosecution, nature of the judgment, prosecution time (Columns 4 to 5). *Source:* Author's calculations based on administrative data extracted from the management software *Cassiopee* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

respond to the context in which they make decisions in the short run: otherwise similar cases are less likely to be dismissed when examined shortly after news stories on crimes against women.

To assess whether prosecutors are not merely postponing dismissal decisions, I examine the effect of news coverage on the number of prosecutorial decisions. Column (3) of Table 2.1 shows no significant change in the total number of decisions made in the seven days following news coverage. Appendix Figure 2.F.2 displays cumulative results over a longer period. Coefficients are normalized by the average value of the lead coefficients, and standard errors are corrected to account for the cumulation. The figure shows a general increase in prosecutions over the two weeks following coverage, alongside a decrease or no change in dismissals. This suggests that the reduction in dismissal rates is associated with an effective rise in prosecutions, rather than a postponement of dismissals over time.

Placebos To verify that the results are not due to an artifact, I perform several placebo

Figure 2.2: Effect of news coverage of crimes against women on the propensity to dismiss other offenses against the person



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss other offenses against the person, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of other offenses against the person unrelated to VAW that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Reading: Up to seven days after the TV news coverage, the propensity to dismiss other harm to persons remains unaffected.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

tests. First, I estimate the same regression but using the propensity to dismiss other offenses against the person as the new outcome variable, being mostly physical violence (32%), threats or blackmail (13%), injuries following traffic accidents (6%) and privacy invasion (5%). Reassuringly, Figure 2.2 shows no effect of news coverage of crimes against women on the propensity to dismiss such violence. Likewise, I find no significant decrease when restricting the sample to murders or other intentional bodily harm, which constitute the most comparable groups in terms of gravity and legal penalties (Appendix Figure 2.F.3).

Second, I estimate the impact of unrelated news stories on the propensity to dismiss VAW cases.³⁴ Appendix Figure 2.F.4 shows that news coverage related to gardening, crafts, leisure, or travel has a precisely estimated null effect on prosecutorial decisions. Interestingly, coverage of other crimes—excluding VAW—does not affect the propensity to dismiss VAW cases either. This null result holds when restricting the analysis to specific crime categories, such as news about drug trafficking, criminal conspiracy, theft, or other serious interpersonal vi-

³⁴Since such news stories are far more frequent than those about crimes against women, I cannot restrict the analysis to single-day coverage without ending up with too few observations. Hence, periods with multiple days of news coverage are also included in the analysis for these placebo tests.

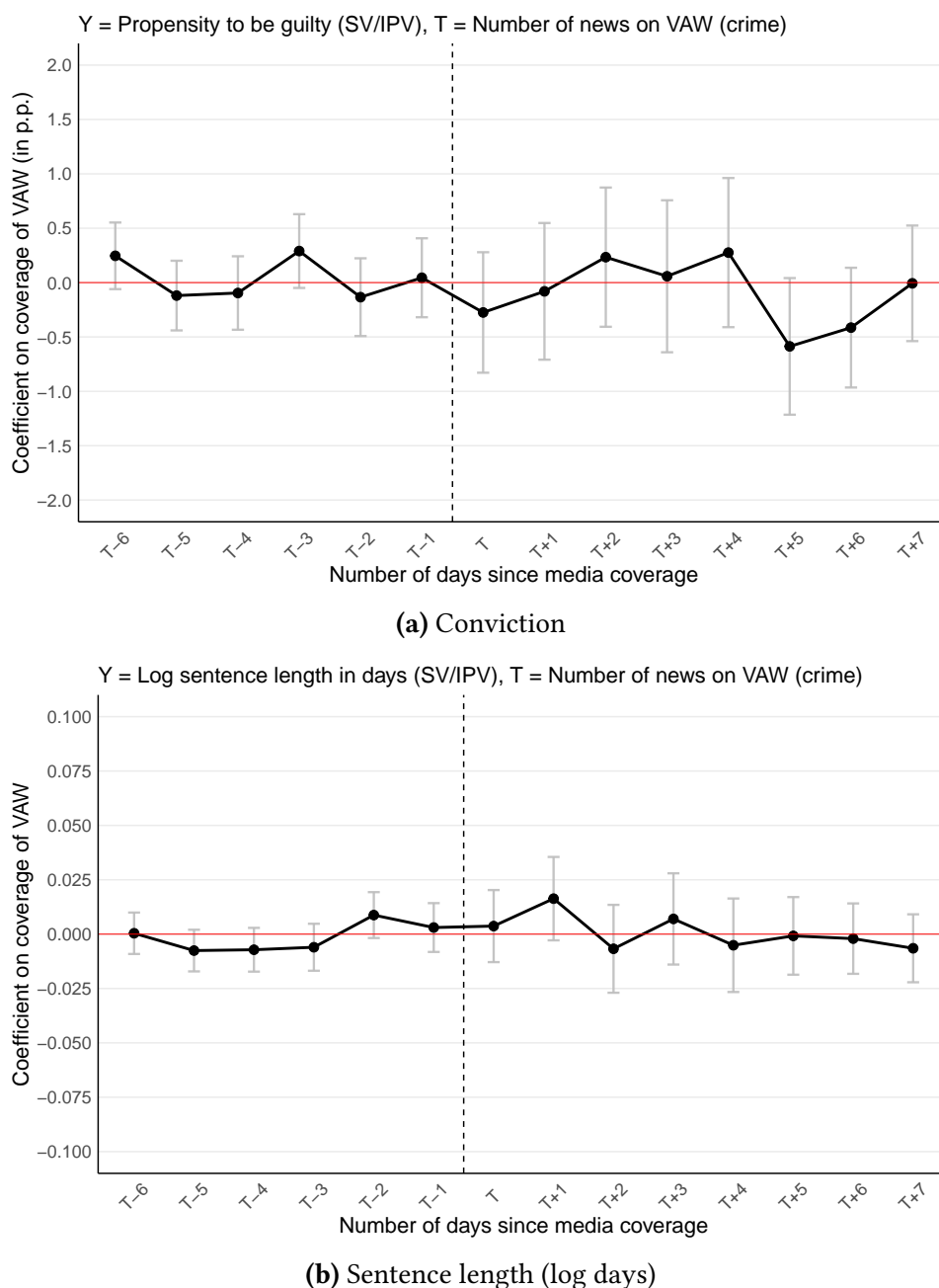
olence (see Appendix Table 2.F.6). In Section 2.6.2, I explore whether these dynamics extend to other offenses and if news coverage of such crimes similarly influences their own judicial outcomes.

Finally, I conduct a placebo falsification test using 500 randomly assigned fake treatment dates. Appendix Figure 2.F.5 displays the distribution of the estimated pooled coefficient for seven days following these placebo treatments, which is reassuringly centered around zero. In contrast, the actual estimated coefficient lies at the extreme end of this distribution. These various tests suggest that the main effect is unlikely to be driven by seasonal patterns or random fluctuations in the data, and the results are further robust to a battery of checks described in Section 2.5.5.

Conviction and sentencing Next, I examine the effect of TV news coverage of crimes against women on conviction and sentencing decisions for cases prosecuted in correctional or juvenile courts and tried by professional judges. The main estimation results are presented in Table 2.1. Figure 2.3 shows that news coverage of crimes against women has no impact on conviction. For offenders sentenced to jail, there is no significant effect on the sentence length decided by the judge either (whether measured in log days or in levels; see Appendix Figure 2.G.1). These results hold across various categories of case and news characteristics, including sexual and intimate partner violence, high and low audience levels, and the periods before and after the #MeToo movement, among other dimensions (Appendix Figures 2.G.2 and 2.G.3).

Selection concerns arise because the conviction and sentencing analyses condition on cases that are prosecuted, and I find that news coverage may affect the decision to prosecute a case. However, given the long average interval between prosecution and final judgment (199 days on average; 420 for sexual violence and 130 for intimate partner violence), such selection bias is likely to be minimal. Moreover, the results remain unchanged when restricting the sample to cases in which prosecution occurred before the coverage episode that anchors the sentencing event window (Appendix Figure 2.G.4), or when broadening the exposure measure to include trial-related news reports (Appendix Figure 2.G.5). Furthermore, Appendix Figure 2.G.6 indicates that news coverage of crimes against women does not affect the time elapsed between prosecution and conviction decision, suggesting that it does not lead to faster judgments either.

Figure 2.3: Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to convict such violence and the sentence length, with corresponding 95% confidence intervals. The sample consists of cases of sexual or intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. In the upper panel, the dependent variable is a dummy variable equal to one if the perpetrator was convicted at trial, with the estimated coefficient expressed in percentage points. In the lower panel, the dependent variable is the number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Reading: Up to seven days after the TV news coverage, the propensity to convict sexual and intimate partner violence cases remains unaffected. In contrast, the sentence length decreases slightly two days after the TV news coverage, with the effect being marginally significant. Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Rationalizing the differential response These findings can be rationalized in two main

ways. As discussed above, professional judges in France operate with full independence and are not subject to direct accountability mechanisms or public approval. This structural insulation makes them less likely to respond to external signals such as media coverage. By contrast, prosecutors operate within a hierarchical framework and are more sensitive to institutional expectations and reputational risks among their peers and superiors, making them structurally more responsive to shifts in public scrutiny. Second, the potential for further judicial adjustment is inherently limited by a ceiling effect. Given that 94% of prosecuted cases already result in a conviction (Table 2.1), there is very little scope for media coverage to further increase conviction rates. Conversely, the high dismissal rate at the prosecutorial stage provides a much wider margin for adjustment.

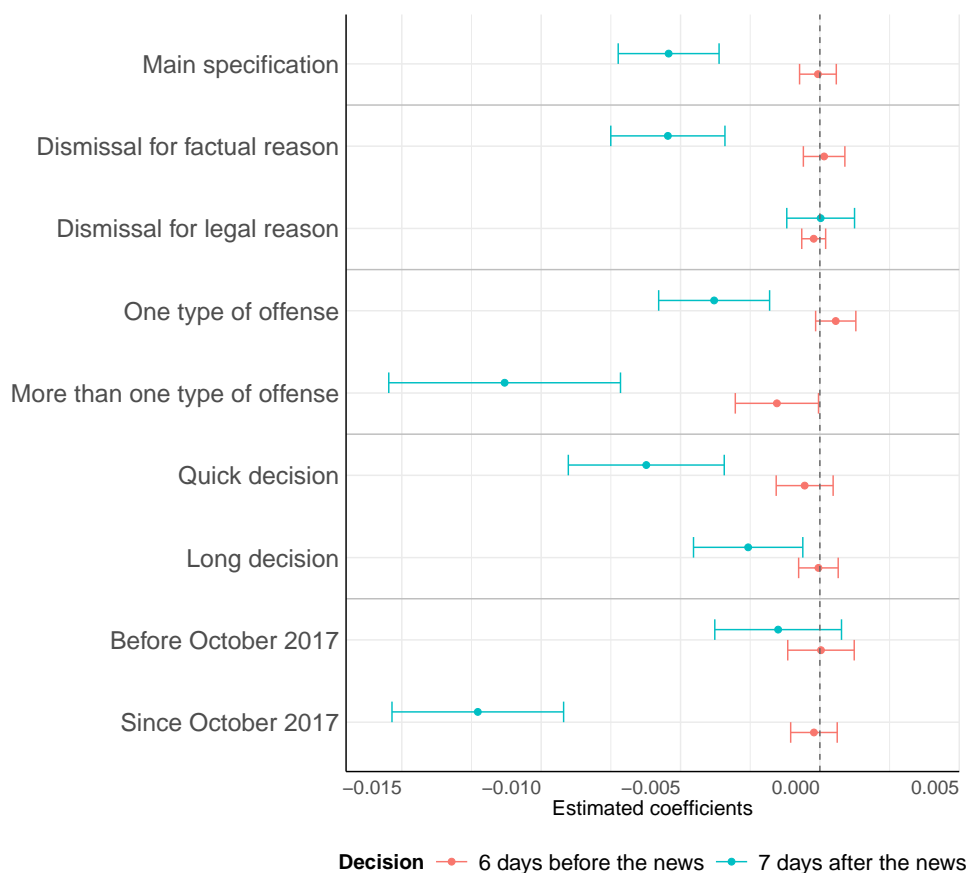
2.5.2 Heterogeneous effects on prosecutorial decisions

To better understand the nature of the cases driving this lower dismissal propensity following news coverage, I examine heterogeneity effects. Figure 2.4 shows that the effect is stronger in settings where prosecutorial discretion is greatest. First, the reduction in dismissals is concentrated among cases dismissed based on factual rather than legal grounds. Factual dismissals (69% of dismissals) include situations in which the offense is deemed insufficiently characterized or the case remains unsolved, whereas legal dismissals (8%) reflect mechanical constraints, such as statutes of limitations, procedural irregularities or mental incapacity of the perpetrator. Because factual grounds rely more heavily on evidentiary assessments, they leave greater room for prosecutorial discretion.

Second, the effect is stronger for cases involving multiple offense categories (23% of cases), as identified by the NATAFF codes, than for those with a single offense recorded. Such cases are typically more serious and provide more tangible elements on which to build a prosecution, making them easier to pursue. Likewise, the reduction in dismissal propensity is more pronounced for faster decisions, where the time between the case's arrival in court and the prosecutor's decision is shorter than the median (96 days \approx 3 months). This suggests that the effect pertains mainly to cases that are relatively clear or straightforward to prosecute, which are also more likely to be influenced at the margin by external factors such as media coverage.

Importantly, the impact is primarily driven by the post-#MeToo period. As shown in Figure 2.4, the reduction in dismissal rates materializes only after October 2017. The point estimate more than doubles during this period, with each news story decreasing dismissal propensity

Figure 2.4: Heterogeneous effects of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, by case characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence by audience, with corresponding 95% confidence intervals. The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.
Reading: In the baseline specification, each additional news story on crimes against women decreases the propensity to dismiss a case in the following seven days by 0.54 percentage point on average.
Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

by 1.23 percentage points. This timing suggests that prosecutorial responsiveness to routine, everyday media coverage of VAW is conditional on a prior, broader shift in social norms. As documented by previous studies, the #MeToo movement profoundly altered the perception of sexual and domestic violence, significantly increasing reporting rates and public sensitivity to these issues (Gauthier, 2022; Levy and Mattsson, 2023; SSMSI, 2024). By reframing VAW as a systemic social issue rather than a series of isolated incidents, the movement likely heightened public scrutiny regarding how the justice system handles these cases. Consequently, prosecutors appear more likely to adjust their behavior in response to the short-term shocks in salience generated by daily news coverage after #MeToo.

When examining heterogeneity by news characteristics (Appendix Figure 2.F.6 and Ta-

ble 2.F.2), the effect is stronger when news stories reach larger audiences (above median for each channel) and during the winter rather than the summer months. This pattern is consistent with an exposure mechanism: the impact of media coverage is larger when people are more likely to watch TV and prosecutors are thus more likely to have been exposed to the news.³⁵ However, headline stories that open the 8pm news broadcasts do not yield a stronger effect, suggesting that the most prominent news events do not necessarily generate stronger behavioral responses. That said, because headline stories may differ substantially in tone or content from standard reports, their distinctive features could still elicit different reactions from prosecutors.

Finally, I explore additional sources of heterogeneity, including the type of violence, the recency of the offense, the victim's age (adult vs. minor), and the offender's country of birth (Appendix Figure 2.F.7). The effect appears to be slightly larger for intimate partner violence than for sexual violence, although the difference is not statistically significant. Consistent with this pattern, the reduction in dismissals is stronger after news stories about intimate partner violence or femicides than after stories about sexual violence (Appendix Figure 2.F.6). While other differences in magnitude emerge, these estimates are imprecise and do not allow for strong conclusions.

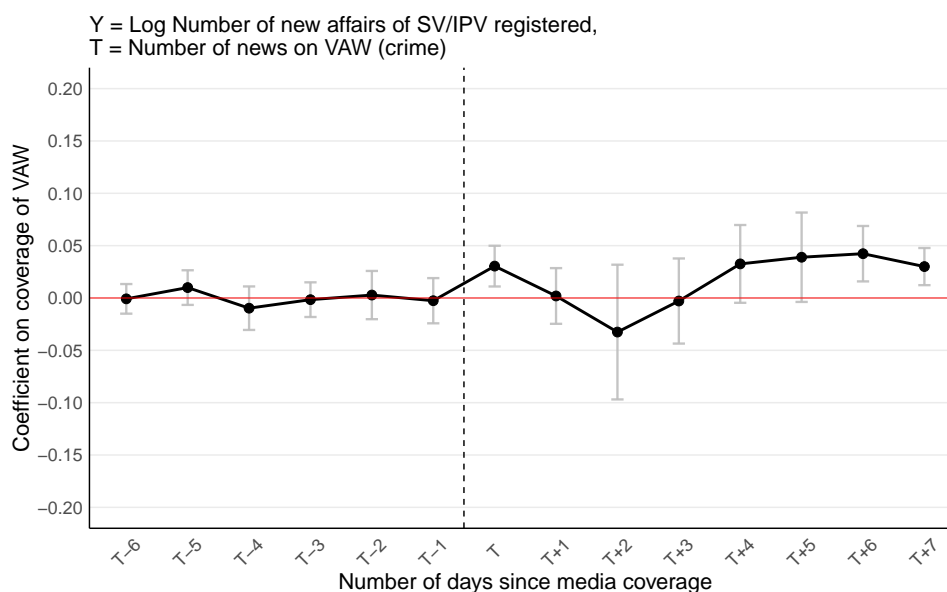
2.5.3 Changes in victim reporting or police recording behavior

A possible concern is that the composition of cases reaching the justice system could have changed following media coverage, potentially biasing the observed effects on prosecutorial decisions. This could occur through behavioral responses at the reporting stage, either from victims becoming more likely to report incidents or from police being more likely to record and transmit complaints. To examine this possibility, I analyze the impact of news coverage on the (log) number of cases registered with the justice system.

Figure 2.5 shows an increase in the number of cases reported to justice at time T , followed by an almost symmetrical (though statistically insignificant) decrease at $T + 2$, before increasing again. On average, each additional news story on crimes against women is associated with a 1.7% increase in newly registered cases over the following seven days (Table 2.1),

³⁵Nevertheless, audience size may also correlate with the type of stories covered on TV, as higher viewership is typically associated with more sensational news, which itself may prime judicial decision-makers more strongly. Therefore, I cannot rule out the possibility that the nature of crimes featured in the news differs between high- and low-audience days, or between summer and winter periods.

Figure 2.5: Effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases reported to justice



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual or intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019. The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Reading: On the day of the TV news coverage, the number of new sexual and intimate partner violence cases registered within the criminal justice system increases by approximately 2% (significant at the 5% level).

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

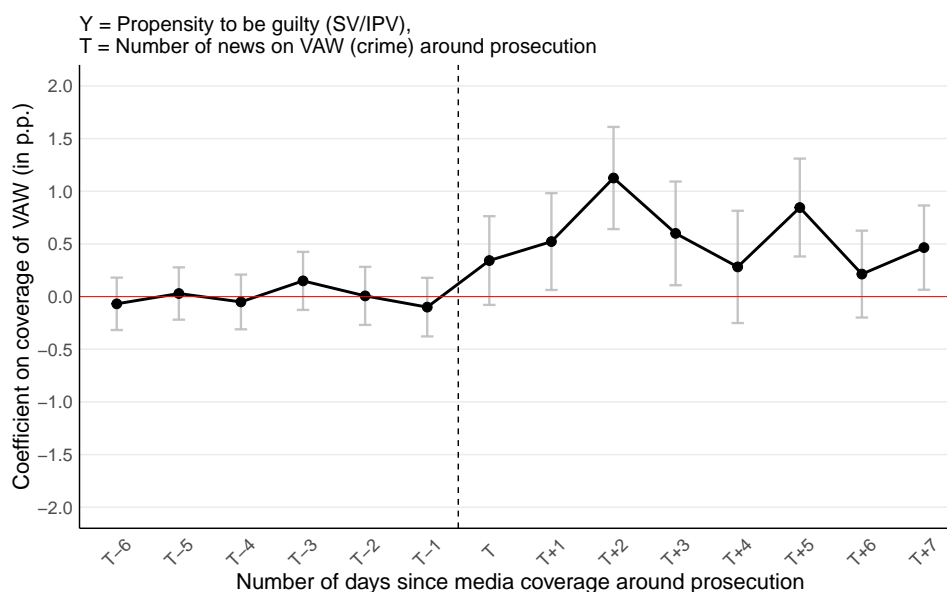
statistically significant at the 5% level. Relative to a daily baseline of 305 new cases registered, this represents approximately 5 additional cases per news story over a one-week period. Further analyses presented in Appendix 2.E, leveraging data on the date of incidence versus the date of recording, suggest that this effect stems from a greater willingness to report or record complaints rather than an actual rise in underlying incidence. Crucially, placebo tests show that the immediate jump at time T is specific to VAW and is not observed for other offenses against the person (Appendix Figure 2.E.10). While the point estimates for other offenses become comparable in later days, the robustness of the effect at T suggests that news coverage triggers a modest but immediate reporting response, without leading to a sustained increase in volume beyond the day of the broadcast.

Despite this short-lived increase in volume, the overall composition of cases appears largely unaffected. First, I find no significant heterogeneity in this reporting effect across case characteristics or over time. Notably, the reporting response to daily news appears relatively stable before and after the #MeToo movement (Appendix Figure 2.E.3), especially for intimate

partner violence, while it is only significant in the pre-#MeToo period for sexual violence. Second, if this increase were driven by lower-quality reports—as explored by Marchenko and Pakzad-Hurson (2025) in the context of #MeToo—one might expect a shift in the nature of the cases arriving at the prosecutor’s desk. However, I find no major differences in the profiles of cases registered following news coverage, except for minor variations in their administrative origin (see Appendix Figures 2.E.4 and 2.E.5). Finally, I find no impact on the time elapsed between the incident and its arrival at the public prosecutor’s office (Appendix Figure 2.E.6). This stability in reporting delays (averaging 678 days for sexual violence and 194 days for intimate partner violence) suggests that the flow of cases reaching the justice system remains qualitatively consistent despite the immediate impact on volume. Even if TV news coverage slightly increases reporting on the day it airs, selection is unlikely to bias the results on judicial decisions, given the long delay between reporting and case outcomes. On average, cases take 210 days to reach a dismissal or prosecution decision, and 255 days to reach a final sentencing decision. To rule out composition effects, I conduct robustness checks in Section 2.5.5 to ensure that this short-term increase in reporting or police recording does not drive the main prosecutorial results.

Drivers of the reporting response This modest effect is likely explained by several factors. Unlike judicial actors, whose daily work involves making such decisions, victims face a fundamentally different process when deciding whether to report their assault to the police. Because reporting is likely a high-stakes and costly decision, media coverage may exert less influence on it compared with other factors, such as economic and material conditions or the direct consequences of the assault (Stricot, 2025). Moreover, it is not clear that media exposure would always encourage victims to report. Hearing about crimes against women could increase fear and discourage victims from coming forward, or they might see examples of a dysfunctional justice system, which could offset any potential incentive to report. Finally, even though the transmission of complaints from the police to the justice system is expected to occur almost immediately, there could still be some noise around the event time associated with the reporting decision.

Figure 2.6: Long-term effect of news coverage of crimes against women on conviction for sexual and intimate partner violence cases



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around the time of prosecution on the long-term propensity to convict such violence, with corresponding 95% confidence intervals. The sample consists of all cases of sexual or intimate partner violence that were completed between 2012 and 2019, either dismissed or prosecuted in correctional or juvenile courts. The dependent variable is a dummy variable equal to one if the perpetrator was convicted at trial, with the estimated coefficient expressed in percentage points. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 around the prosecutor’s decision, with coverage occurring at time T .

Reading: sexual and intimate partner violence cases prosecuted two days after the TV news coverage see a 0.5 percentage point increase in the propensity to result in a conviction in the long run (significant at the 5% level).

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.5.4 Long-term effect of news coverage on conviction

I find that TV news coverage of crimes against women significantly increases prosecution rates of sexual and intimate partner violence in the short run, without affecting conviction and sentencing decisions at trial. But this raises a key question: what happens to the cases that are prosecuted following media attention in terms of conviction? If media pressure pushes prosecutors to lower their threshold for prosecution, one might expect the newly prosecuted cases to be of weaker evidentiary quality, potentially leading to lower conviction rates at trial. To assess this long run effect, I examine the impact of news coverage around the prosecution decision on the final conviction outcome. The analysis includes all cases, assigning the conviction outcome to zero for those dismissed to account for composition effects, since news coverage affects whether a case is prosecuted.

Figure 2.6 displays the results of this analysis. It shows that the unconditional propensity to convict a defendant increases significantly for cases whose prosecution decision is made following TV news coverage of crimes against women. The estimated effect on the propensity

to convict (0.53 p.p., see Appendix Table 2.G.1) is nearly identical to the one previously estimated for the propensity to prosecute (0.54 p.p.). As shown in Figure 2.1, when defendants are prosecuted, they are almost systematically convicted. This implies that the additional cases prosecuted following news stories likely result in convictions at the same rate as other cases.³⁶ Moreover, these cases lead to slightly longer jail sentences, though the effect is marginal in magnitude and significance (Appendix Figure 2.G.7). Taken together, these findings suggest that the pool of cases prosecuted following news coverage does not differ significantly in quality from the rest of the prosecuted caseload.

Alternatively, the stable conviction rates could theoretically imply that judges *also* adjust their behavior by lowering their standards to convict these potentially "weaker" cases. However, this interpretation is unlikely. The significant time lag between prosecution and sentencing (130 days for intimate partner violence and 420 days for sexual violence, on average) makes it implausible that judges are responding to the specific news coverage that aired months before the trial. Instead, the most plausible explanation for this stability lies in the nature of these additional prosecutions. As documented in the heterogeneity analyses (Section 2.5.2), these "marginal" cases do not appear particularly complex or legally weak. When media pressure prompts prosecutors to prioritize these files, they prove strong enough to meet the evidentiary threshold for conviction without requiring judges to alter their standards.

This provides strong evidence that media coverage partially corrects an under-prosecution issue stemming from resource scarcity, rather than compromising case quality. Cases that might have been dismissed in the absence of salience prove to be legally viable, revealing how current court resource constraints can lead to sub-optimal case attrition. These findings have important policy implications regarding the role of prosecutorial discretion and its potential to affect both the equity and efficiency of the criminal justice system. Specifically, they demonstrate that under systemic limitations, the broad discretionary power of prosecutors may lead to the dismissal of merit-worthy cases. This underscores an urgent need for greater court resources to ensure that the pursuit of justice is driven by the strength of the evidence rather than institutional capacity constraints.

³⁶I confirm this by analyzing the long-term effect of news coverage around the initial prosecution date on subsequent sentencing, conditional on the cases being prosecuted. Although results are noisier due to the smaller sample size, Appendix Figure 2.G.8 shows no significant decrease in the propensity to convict.

2.5.5 Robustness and sensitivity checks

The results on prosecutorial decision-making are robust across a wide range of other alternative specifications, samples, and definitions of media coverage, reported below and in Appendix Table 2.F.3.

Alternative modeling strategies and fixed effects Column (1) of Appendix Table 2.F.3 displays the main result from the estimation of Equation 2.1. Column (2) replicates the analysis using a Probit model. In Column (3), I modify my base regression to a two-step procedure: first, I residualize the data to eliminate the overall time trend and the day-of-the-week pattern, using the following specification:

$$Y_t = dow_t + month_t + year_t + \epsilon_t \quad (2.1)$$

where dow_t , $month_t$, and $year_t$ are day of the week, calendar month, and year fixed effects. I then compute the residuals from this regression \bar{Y}_t and use the residualized outcome in Equation 2.1. In all cases, the estimated effect remains negative and significant.

Columns (4) to (7) sequentially introduce increasingly flexible time fixed effects by interacting year with month, quarter, weekday, and day. Column (8) adds court fixed effects, and Column (9) includes a linear time trend. In all cases, the effect of news coverage on the propensity to dismiss remains robust and comparable in magnitude.

Controlling for time-varying confounders To address potential seasonality or symbolic calendar effects, Columns (10) and (11) control for school vacations and official holidays. Column (12) accounts for periods of heightened female visibility (e.g., International Women's Day, #MeToo), and Column (13) for the adoption of new VAW-related legislation enacted during the study period (e.g., measures to facilitate the filing of complaints, changes to the statute of limitations, Istanbul Convention, etc.). Column (14) excludes weekend decisions, and Column (15) aggregates news on weekends to test whether unusual patterns on weekends might bias the results. The results consistently hold across these variations.

Inference and clustering Columns (16) to (20) examine the robustness of inference to different clustering levels: by day, county, year-month, individual case (affair), and court. The effect remains significant under all clustering strategies.

Alternative samples and outcomes Columns (21) to (24) test the sensitivity of results to sample restrictions: limiting to known authors (21), removing duplicated authors involved in multiple charge-level cases (22), excluding alternatives to prosecution from the outcome definition (23), and excluding rape cases (24). The results remain robust in all situations. Importantly, Appendix Figure 2.F.10 displays the dynamic effects when restricting the sample to cases registered prior to the news coverage, to ensure that no case is affected twice by the news coverage and given the observed increase in reporting (Column (25)). The fact that the reduction in dismissal rates persists for cases already in the system ensures that the results are driven by changes in prosecutorial behavior rather than by shifts in the composition or "quality" of incoming complaints.

Alternative measures of media exposure Columns (26) and (27) replace the main measure of media coverage with (i) a dummy indicating the occurrence of any news, and (ii) the total duration of news stories in minutes. These specifications yield qualitatively similar effects, as visualized in Appendix Figure 2.F.11. Column (28) includes news on VAW-related trials. Although we see a similar downward trend in dismissal is observed when news stories about trials are included in the specification, the effect is somewhat weaker (Appendix Figure 2.F.12). Column (29) excludes stories related to the high-profile Dominique Strauss-Kahn case that might disproportionately affect results. Columns (30) and (31) examine each TV channel separately (TF1 and France 2, respectively), with similar coefficients across channels that are not significantly different from each other. To ensure that there is no possible overlap between the judicial cases and the news stories, Columns (32) to (34) present the effects of news on crimes against women that occurred in the same county as the jurisdiction, in adjacent counties, or in other counties (the latter accounting for 91% of the news). The results are significant and of similar magnitude across the three estimates, indicating that the effect is not driven solely by events occurring in the same or neighboring counties.

Addressing exposure overlap and news absence Finally, Column (35) expands the sample to include periods with multiple days of news coverage during the seven days preceding a decision, while Column (36) excludes periods without any news coverage. Appendix Figure 2.F.13 confirms that the main dynamic pattern also appears under the

broader exposure criterion.

Additional tests Appendix Figure 2.F.14 further shows no abrupt break in the time trends of news coverage or dismissal rates that could artificially generate the main result. To further verify that no specific time period or location drives the findings, I perform leave-one-out exercises by removing each day, month, or county individually from the dataset. The main effect persists even after excluding each of the 76 isolated days with news coverage, each of the 96 months of the 2012–2019 analysis period, and each of the 96 French metropolitan counties (see Appendix Figure 2.F.15). Moreover, correcting for multiple hypothesis testing using several approaches still yields a statistically significant effect (Appendix Figure 2.F.16).

2.6 Discussion

Having established the causal impact of media coverage on prosecutorial outcomes, this section evaluates the scale of these effects, their specificity to VAW, and the behavioral and institutional channels through which media salience influences judicial decision-making.

2.6.1 Magnitudes

What do these findings imply in terms of magnitude? Overall, the impact of media coverage across the judicial chain is nuanced. On the reporting side, the effect is modest: the increase observed on the day of coverage is equivalent to approximately 5 additional cases registered over a one-week period relative to a daily baseline of 305 cases. Similarly, media coverage does not appear to affect the sentencing or conviction decisions made by professional judges. The most substantial shifts occur at the prosecutorial stage. Using a dummy variable for the occurrence of any news coverage of crimes against women, I find that cases decided by prosecutors after such coverage (which accounts for 20% of the sample) are, on average, 0.64 percentage points less likely to be dismissed than those decided when no stories are aired during the 2012–2019 period (Appendix Table 2.F.3).

During that period, prosecutors examine on average 1,995 cases of sexual violence or intimate partner violence over a seven-day period (approximately 1,526 dismissed and 469 prosecuted). Given that news coverage does not affect the overall volume of decisions (Table 2.1), this estimate implies that approximately 13 additional cases ($1,995 \times 0.0064$) are prosecuted

over a seven-day period following news exposure. I then extrapolate this estimate by considering the number of days with news on crimes against women over the sample period. With 162 unique news days between 2012 and 2019 (either on TF1 or France 2), this corresponds to roughly 20 unique news days per year on average ($162 \div 8$). This implies around 260 additional cases (13×20) prosecuted per year on average, most of which are then likely to result in conviction.^{37 38}

However, these figures represent a conservative lower bound when focusing on the post-#MeToo era. Between October 2017 and December 2019, the average weekly volume of cases increased to 2,201 (1,661 dismissed and 540 prosecuted). During this period, the estimated effect also rises significantly: cases decided after such coverage are 1.55 percentage points less likely to be dismissed. This implies that approximately 34 additional cases ($2,201 \times 0.0155$) are prosecuted over a seven-day period in the post-2017 context. Extrapolating this to the higher frequency of media coverage observed recently, with 71 unique news days over the 27 months of the post-#MeToo subsample (approximately 31 days per year), this would imply around 1,054 additional cases (34×31) prosecuted annually. This fourfold increase compared to the full-period average highlights how the judicial response to media salience has intensified in recent years, driven by both a higher baseline volume of cases and a stronger sensitivity of prosecutors to news coverage.

Comparison with the literature These average effects are consistent with the literature on how media influences reporting and judicial decisions. For instance, [Philippe and Ouss \(2018\)](#) find that news stories about unrelated crimes lead to a 2.4% increase in sentence length for convictions delivered by civilian juries in French criminal courts, but observe no effect for sentences handed down by professional judges in correctional courts. Likewise, [Ash and Poyker \(2024\)](#) show that exposure to conservative news in the U.S. does not affect the sentencing harshness of appointed judges. Regarding reporting, [Colagrossi et al. \(2023\)](#) find that calls to the Italian helpline against gender-based violence increase by 11% in the week following

³⁷Using the more conservative measure of unique news days, which excludes stretches of multiple consecutive days, there are 76 isolated days of coverage over the 2012-2019 period (either on TF1 or France 2). This corresponds to roughly 9.5 unique news days per year on average ($76 \div 8$) and implies around 123 additional cases (13×9.5) prosecuted per year on average.

³⁸This behavioral response is consistent with the media habits of legal professionals. Survey data suggest that “Senior civil servants” (the category encompassing magistrates) are highly informed, with over 95% actively following the news (see Appendix Figure 2.C.7). They rely heavily on traditional media, such as television and radio, suggesting that this news coverage is likely to reach prosecutors, either through direct exposure or through professional and social interactions.

news of a femicide within the affected province, which is likely to involve lower barriers than reporting to the police. As for the post-#MeToo period, [Levy and Mattsson \(2023\)](#) observe a 10% increase in sex crime reports during the first six months of the #MeToo movement. By contrast, my findings suggest that daily news coverage of crimes against women is not as impactful as large-scale social movements like #MeToo in encouraging victims to report. Consistently, in [Appendix 2.H](#), I show that the #MeToo movement was associated with a larger increase in reporting for both sexual and intimate partner violence and a steeper decline in dismissal rates for intimate partner violence. While this highlights the difference in magnitude between the effects of high-frequency media coverage and those of broader salience shocks, the two mechanisms may interact. In particular, I find that #MeToo increased prosecutors' sensitivity to everyday news about VAW, making routine media coverage more consequential after the movement. Part of the institutional effect of #MeToo may therefore operate by changing how recurrent media signals are received and acted upon.

2.6.2 Specificity to violence against women

A key question is whether the observed prosecutorial response is a general reaction to crime news or is specifically tied to the nature of VAW. This distinction is important because VAW cases differ from many other offenses in the legal reasons for dismissal. As documented in [Section 2.3.2](#), sexual and intimate partner violence cases are more often dismissed because the offense is considered insufficiently characterized, reflecting evidentiary difficulties and prosecutorial discretion over whether the case is strong enough to proceed. By contrast, other offenses are more frequently dismissed because the perpetrator is unidentified. Media salience may therefore have greater scope to affect prosecutorial decisions in VAW cases than in other serious crime categories.

To investigate this, I examine whether news coverage of other high-profile offenses triggers similar shifts in prosecutorial outcomes for the corresponding offense category. For each category, I identify TV news stories related to that type of crime and estimate their effect on justice outcomes for cases in the same category. The categories considered are drug trafficking, organized crime, theft and receiving stolen goods, serious interpersonal violence and homicides, and incest and pedophilia involving young children. The keywords used to identify category-specific news stories in the TV news data, as well as the NATAF codes used to identify the corresponding cases in the justice data, are provided in [Appendix 2.F.5](#).

To ensure comparability, I apply the same methodology as in my main specification to these alternative crime categories, following the same logic of identifying short-run shocks. I restrict the selection of news stories to "miscellaneous news items" (*faits divers*), specifically excluding trial coverage or long-term judicial procedures, and exclude all news stories that also mention VAW to avoid any overlap with the main treatment. However, a methodological challenge arises. Because some offenses, such as homicides or serious assaults, receive much more frequent media coverage than VAW, restricting the analysis to isolated news days (i.e., days with only one news event in the preceding seven days) leads to a substantial reduction in both the number of observations and the number of news events analyzed (see Appendix Table 2.F.5). To address this, I present results using both the restricted sample of isolated news days and the full sample, which includes periods of repeated or overlapping coverage. Standard errors are also checked for robustness by clustering at the daily level, allowing for correlation across prosecutorial decisions made on the same day.

Overall, I find no consistent evidence that media coverage of other crimes influences their corresponding prosecutorial outcomes (see Appendix Tables 2.F.7 and 2.F.8). When using the full sample, a marginally significant reduction in the probability of dismissal appears for drug trafficking cases, though the effect vanishes once standard errors are clustered by day. Similarly, while the restricted sample of isolated news days initially yields significant estimates for organized crime, severe physical assaults, and sexual violence against minors, these effects lose statistical significance under more robust clustering.

These null results suggest that the impact of media salience documented in this paper is not a universal phenomenon across all crime types. Instead, the responsiveness appears to be specific to VAW cases, indicating that the institutional or behavioral drivers of the effect may be uniquely tied to how the justice system handles this specific category of violence. This lack of generalizability to other serious offenses provides a basis for a deeper investigation into the specific mechanisms at play in the following section.

2.6.3 Potential mechanisms

Having documented the magnitude and specificity of the effect, I conclude by exploring the potential channels through which media coverage of VAW may shape judicial decision-making. While these mechanisms are difficult to disentangle fully, and the data do not allow for a direct elicitation of prosecutors' internal beliefs, analyzing these patterns yields

important insights into the behavioral and institutional dynamics at work. I focus on three potential drivers: psychological priming, reputational concerns, and changes in institutional effort.

Priming A first potential mechanism is psychological priming, whereby news coverage temporarily increases the salience of VAW in decision-makers' minds, eliciting emotional or cognitive reactions. However, a purely unconscious priming effect appears conceptually unlikely. First, because processing VAW cases is a core component of their daily workload, prosecutors' professional decisions are less likely to be altered by the temporary salience of a broadcast than those of occasional jurors. Second, I find no effect on professional judges, who should theoretically be just as susceptible to such cognitive biases if the mechanism were strictly psychological. That said, using basic keyword-based text analysis on a subsample of TV transcripts, I find that the impact on prosecutorial decisions is more pronounced following news stories that feature victims' testimonies ($N = 24$ isolated news days) than after crime news involving sensational cases ($N = 25$ isolated news days) or political and media figures ($N = 19$ isolated news days) (Appendix Figure 2.F.8). While the small number of news days warrants a cautious interpretation, this pattern suggests that stories evoking stronger empathy or emotions might be more effective at influencing decision-makers than other types of news reporting.

Reputational concerns Another plausible explanation for these findings lies in reputational concerns, where decision-makers strategically adjust their behavior in response to temporary increases in public scrutiny and institutional accountability. Driven by the need to manage their professional image, prosecutors may behave more cautiously when VAW is under the media spotlight due to potential pressure from society or hierarchical superiors. This mechanism appears more consistent with the observed empirical patterns. First, the divergence between judicial actors aligns with their varying degrees of institutional accountability: independent judges remain unaffected, whereas prosecutors, who operate within a more hierarchical and accountable structure, adjust their decisions. Second, the fact that the effect materializes only after the #MeToo movement supports the hypothesis of a response driven by external pressure. By reframing VAW as a systemic social issue, the movement likely heightened the reputational risks associated with dismissing such cases,

making prosecutorial responsiveness conditional on this broader shift in social norms. This interpretation is further supported by court-level heterogeneity. Although no significant difference emerges based on court size or baseline dismissal rates (Appendix Figure 2.F.9), the reduction in dismissals is concentrated in courts processing an above-median volume of VAW cases in the prior two weeks (an average of 47.5 cases compared to a median of 32). This suggests that responsiveness is higher where prosecutors are more frequently exposed to these cases and may therefore be more attentive to the stakes of public scrutiny. Finally, the prosecutorial response is slightly more pronounced following stories highlighting institutional responsibility, such as cases involving prior reporting by victims or a perpetrator's previous involvement with the justice system, rather than types of coverage less likely to trigger reputational stakes (although the difference is not statistically significant). While based on a limited number of news days, this suggestive evidence reinforces the accountability hypothesis.

Increased effort or motivation Alternatively, the observed shifts in prosecutions could reflect increased prosecutorial effort and motivation or a reallocation of resources toward VAW cases. If media shocks prompted a more time-consuming and careful examination of files, one would expect to see longer processing delays or a decrease in the total volume of decisions. However, I find no significant effect on either the number of decisions regarding sexual and intimate partner violence cases or the time elapsed between case registration and the prosecutor's decision (Appendix Tables 2.F.9 and 2.F.10). This holds despite a substantial baseline mean processing time of 210 days (242 days for dismissals and 98 days for prosecutions). Interestingly, I observe a 10% relative increase in the use of immediate trials (*comparution immédiate*)³⁹, a fast-track procedure for serious offenses (Appendix Figure 2.F.18). While this could suggest a targeted effort to secure swift convictions in specific cases, it does not appear to reflect a broader systemic shift. Furthermore, I find no evidence of a substitution effect or the crowding out of other violent crimes, as outcomes and processing times for other crimes against persons remain unchanged (Appendix Table 2.F.11). The absence of clear operational trade-offs suggests that the observed prosecutorial response is more likely driven by a prioritization of existing viable cases than by a substantial increase

³⁹This fast-track procedure allows prosecutors to bring a defendant to court immediately following police custody. It is reserved for relatively straightforward but serious offenses requiring a swift penal response, accounting for only about 3% of prosecution modes in VAW cases at baseline.

in institutional effort or total resource allocation.

Overall, the evidence suggests that the reduction in dismissal rates is not a purely mechanical or unconscious reaction to media salience. Instead, the patterns are consistent with a strategic response involving institutional accountability. While other mechanisms may co-exist (as discussed in Appendix 2.F.6), the concentration of the effect in the post-#MeToo era, in high-caseload courts, and only among prosecutors is suggestive of media coverage acting as a corrective signal. This may prompt prosecutors to re-evaluate their discretionary decisions in a normative environment where the social cost of under-prosecuting VAW has significantly increased.

2.7 Conclusion

Although VAW has become more visible in recent years, criminal justice systems worldwide continue to face challenges in effectively addressing it. This paper asks whether greater visibility of VAW in public discourse can shift judicial responses in the short run. To answer this question, I combine novel administrative data on nearly all cases of sexual and intimate partner violence processed in French courts with high-frequency data from France's 8pm TV news broadcasts. Exploiting as-good-as-random variation in the timing of unrelated TV news stories on crimes against women, I estimate the causal effects of media coverage of VAW on each stage of the criminal justice process.

I find that news coverage of VAW leads to a significant short-run increase in prosecution rates, particularly in the post-#MeToo period, while having a modest immediate impact on reporting and no effect on sentencing decisions. Suggestive evidence indicates that this shift in prosecutions is likely driven by strategic adjustments to increased public scrutiny and accountability. The results underscore the central role of prosecutors, who dismiss nearly 80% of all cases while most prosecuted cases ultimately result in conviction, making prosecution the key decision stage. Media coverage influences precisely this margin: by lowering dismissal rates, it allows more cases to proceed to trial. Importantly, once cases reach trial, judges' behavior remains unchanged, addressing concerns that media visibility might bias judicial rulings. Moreover, the additional cases brought forward appear just as likely to result in conviction as those prosecuted otherwise, suggesting that many viable cases would have

been prematurely dismissed absent this salience shock. While I find no evidence that this heightened focus crowds out the processing of other violent crimes, a potential reallocation of resources away from minor offenses cannot be ruled out.

From a policy perspective, these findings indicate that prosecutorial decisions are malleable, and that increasing the salience of VAW can strengthen the criminal justice response by enhancing prosecutor's accountability without influencing judges' decisions. The fact that these additionally prosecuted cases are legally viable suggests that their prior dismissal was not due to a lack of merit, but rather to severe capacity constraints and backlogs within the justice system. This reveals a critical tension: in an environment of limited administrative bandwidth, the broad discretionary power of prosecutors can lead to a sub-optimal attrition of merit-worthy cases. Consequently, these results underscore an urgent need for greater resources and increased capacity within the court system. Ultimately, while the overall welfare implications of increased salience appear positive, the net effect will likely depend on downstream outcomes, such as whether this increased accountability deters future violence and encourages future victim reporting, which are critical questions that lie beyond the scope of this paper.

These results come with certain limitations. First, because identification targets short-run, local responses to isolated news coverage, generalizing to longer horizons requires caution. Second, the media measure focuses on national evening TV news, whereas effects may differ for regional TV, print, or social media. Third, the lack of direct information on police behavior may introduce bias if changes in police practices also affect which cases are recorded and forwarded to the justice system or how they are investigated. Finally, external validity is bounded by institutional context: France's civil-law system with appointed prosecutors and professional judges may respond differently than systems with elected officials or jury trials.

Nevertheless, the framework and results established in this paper can be extended in several fruitful directions. First, I aim to further elucidate the underlying mechanisms by collecting more granular data on prosecutors and courts. Second, a promising extension would involve conducting heterogeneity analyses based on the specific narratives used to portray victims or offenders, using transcription data from TV news stories. Finally, comparing these effects with those obtained from other media formats, such as talk shows, dramas, or awareness campaigns, would provide further insight into how different types of content shape judicial responses.

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Appendix to Chapter 2

Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women

2.A Additional elements on the French criminal justice system

2.A.1 Judicial actors and jurisdictions

Judicial actors. There are two main categories of magistrates tasked with applying and upholding the law in France: judicial magistrates (*magistrats du siège*), also known as judges, who apply the law by ruling on criminal cases and issuing impartial decisions in accordance with legal standards; and prosecutorial magistrates (*magistrats du parquet*), or prosecutors, who advocate for the enforcement of the law and lead criminal proceedings on behalf of society's interest.

Judicial magistrates enjoy guarantees of independence and security of tenure, meaning they cannot be reassigned without their consent. Their independence is further protected by the fact that the government cannot intervene in their career paths. **Prosecutorial magistrates**, on the other hand, do not issue rulings. They are appointed by the President of the Republic, based on proposals from the Ministry of Justice and following the opinion of the *High Council for the Judiciary* (*Conseil supérieur de la magistrature*).

Prosecutors operate under the authority of the Minister of Justice and are subject to a principle of hierarchical subordination. While they may receive general policy instructions from the Minister of Justice, they cannot be directed in specific cases. Their freedom to speak during court hearings is fully protected. **Unlike judges, prosecutors do not benefit from security of tenure** and may be reassigned without their consent.

Each judicial court (*tribunal judiciaire*) has a **public prosecutor's office** led by a **prosecutor of the Republic**, supported by deputy prosecutors, vice-prosecutors, and substitutes.

They represent the state before the correctionnal court, juvenile court, investigating judges, and civil chambers. All members of the same prosecutor's office form an **indivisible and interchangeable body**, meaning any member can represent the prosecution at any stage of the legal proceedings.

Jurisdictions. In France, different types of courts handle criminal cases, judging both individuals and legal entities suspected of having committed an offense. The appropriate court depends on the severity of the alleged crime.

The **correctional court** (*tribunal correctionnel*) is the lower criminal court responsible for trying adults accused of misdemeanors (*délits*) such as theft or serious assault. It may impose prison sentences of up to 10 years—or up to 20 years in the case of repeat offenders—as well as fines and alternative or complementary sanctions. Proceedings are handled either by a single judge or a panel of three professional judges.

The **criminal court** (*cour d'assises*) is the higher criminal court responsible for trying felonies (*crimes*) punishable by more than 15 years of imprisonment (e.g., murder, rape, armed robbery, etc.), committed by adults or minors over the age of 16 at the time of the offense. It is composed of three professional judges and a jury of six randomly selected citizens. Cases are referred to this court following a judicial investigation.

The **departmental criminal court** (*cour criminelle départementale*), generalized across France in 2023, tries adults accused of felonies punishable by between 15 and 20 years of imprisonment, such as rape or armed robbery, excluding repeat offenders. It is composed solely of five professional judges and handles cases referred by an investigating judge.

Juvenile criminal courts (*tribunal pour enfants*) (including the juvenile judge, the juvenile court, and the juvenile correctional court) rule on the guilt of minors charged with criminal offenses and, if applicable, impose appropriate educational or punitive measures.

The present study includes only first-instance rulings delivered by the correctional courts or juvenile criminal courts. Judgments rendered by the *cour d'assises* or *cour criminelle* are not included in the dataset and are therefore excluded from the analysis. However, such cases represent a small share of criminal proceedings overall. For example, in 2019, the vast majority of criminal cases prosecuted by public prosecutors were referred to correctional courts (84% of prosecuted cases) or juvenile courts (8%) (SDSE, 2019).

2.A.2 Legal concepts and procedures

Case orientation refers to the decision taken by the public prosecutor regarding how to proceed with a case, generally leading either to prosecution or to case dismissal. **Prosecution** corresponds to the initiation of criminal proceedings aimed at bringing the alleged offender before a criminal court. **Dismissal** (*classement sans suite*) is the prosecutor's decision not to prosecute the alleged offender. This may occur in different situations: non-prosecutable cases, failure to prosecute, or after a successful alternative to prosecution. **Non-prosecutable cases** are dismissed because prosecution is impossible, either due to factual reasons (e.g., unknown offender, insufficiently established offense) or legal reasons (e.g., statute of limitations). **Failure to prosecute** refers to cases that could be prosecuted but are dismissed in consideration of the public interest, legal principles, justice, or fairness. An **alternative to prosecution** is a measure imposed by the prosecutor aimed at compensating the harm done to the victim,

addressing the disturbance caused by the offense, or promoting the rehabilitation of the offender, without initiating formal prosecution. If the measure is successfully completed, the case is dismissed. Alternatives to prosecution are not recorded on the criminal record—except in the case of a **penal composition**, which is considered a more formalized type of alternative.

Unlike prosecutorial dismissals, a **judicial dismissal** (*ordonnance de non-lieu*) is issued by an investigating judge when the facts are not established, do not constitute an offense, when the prosecution is no longer possible, or when the perpetrator could not be identified. Judicial dismissals, which typically concern serious crimes requiring referral to an investigating judge, are not included in the dataset and are therefore excluded from the analysis. However, they represent only a small share of prosecuted cases. For instance, in 2019, judicial dismissals accounted for only 3% of all prosecuted cases referred to investigating judges (SDSE, 2019).

2.A.3 Legal qualification of offenses

Sexual violence refers to situations in which a person imposes sexual behavior or comments on another person. It covers different forms of violence. **Rape** is defined as any act of sexual penetration, of any kind, or any oral-genital act committed on another person or on the perpetrator by means of violence, coercion, threat, or surprise. **Sexual assault** is a sexual act committed with violence, coercion, threat, or surprise. It also requires physical contact but excludes penetration, which characterizes rape. **Sexual harassment** consists of repeatedly imposing sexual remarks or behaviors on a person that either violate their dignity due to their degrading or humiliating nature, or create an intimidating, hostile, or offensive environment. Rape is classified as a **felony** (*crime*), the most serious category of offense, punishable by more than 15 years of imprisonment. Sexual assault and sexual harassment are classified as **misdemeanors** (*délits*), i.e., acts prohibited by law and punishable by a fine and/or imprisonment of less than 10 years (or up to 20 years in case of repeat offenses).

Intimate partner violence refers to all forms of violence (physical, sexual, psychological, and economic) committed within couples, whether married, in civil unions, cohabiting, or separated. Intimate partner violence is generally classified as a misdemeanor, except in cases of voluntary homicide or rape, which are considered crimes.

Downgrading of charges (*correctionnalisation*) refers to the process by which an offense initially classified as a felony is requalified as a misdemeanor (for instance, a rape reclassified as a sexual assault). This can be done by the public prosecutor at the end of the preliminary investigation or by the investigating judge. Such reclassification may reflect a justified **legal reassessment** of the facts when the elements constituting rape are not sufficiently established by the investigation and is inherent to the functioning of the judicial system. In contrast, **undercharging** or **declassification** refers to cases where the offense is recognized as rape but still sent to the correctional court (*tribunal correctionnel*) instead of the higher criminal court (*cour d'assises*), for example to reduce court backlog or when it is believed that the victim is unlikely to succeed before a jury. Several studies based on judicial file reviews estimate that nearly half of the sexual assaults prosecuted before the correctional court are in fact rapes that were downgraded, although no reliable official statistics are available on this matter (Le Goaziou, 2019).

2.B Additional elements on judicial data

2.B.1 Further information on the data source and construction

The data used in this study come from the CASSIOPÉE management software. This system is used by French courts to process all criminal offenses, including fifth-class contraventions, misdemeanors (*délits*), and felonies (*crimes*), involving either individuals (adults and minors) or legal entities. The information is primarily entered by court clerks. The dataset used from this software was extracted by the French Ministry of Justice. It includes information on all criminal cases received by public prosecutors, dismissed or prosecuted in first instance by correctional courts or juvenile courts between 2012 and 2023.

While this database is primarily used by the Ministry of Justice for official statistical publications, it has only recently been made accessible to researchers via the secure data access center (CASD). This paper is among the first to exploit these data. However, since the data only cover closed cases and not those still under investigation, time-series trends for the most recent years should be interpreted with caution, which is why I stop the analysis before 2020. In addition, the dataset does not include cases dismissed following an investigative phase or those tried in criminal courts (*cours d'assises* or *cours criminelles*)—although such cases represent a small share of the total criminal caseload.

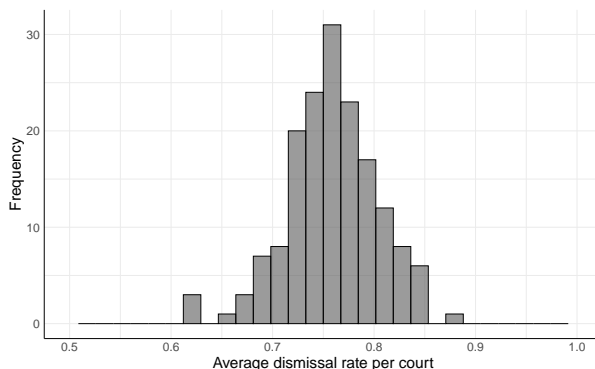
Each case is classified using an official coding system (NATAFF), which assigns one to three main offense types to each case based on the transmitted file. This provides a broad categorization of the case (among 350 different codes at the most detailed level). The NATAFF code is case-level and shared across all suspects involved. It is less detailed than the offense-level NATINF code, which is assigned to individual suspects. However, unlike NATINF—which is often missing, especially for cases that are dismissed (43% missing in this dataset)—NATAFF is systematically recorded for all cases.

Given that legal qualifications can evolve throughout the judicial process (as new elements are uncovered or depending on who is in charge of qualifying the offense), I rely primarily on the NATAFF code as recorded when the case enters the prosecutor's office. This allows a consistent application of case classification across the entire sample, regardless of whether the case was dismissed or prosecuted. Judicial outcome statistics (e.g., convictions and sentencing) are based on the NATAFF recorded at the time of the decision—that is, the final qualification assigned by the prosecutor. I use this classification (NATAFF at entry or at decision) to restrict the sample to cases involving sexual violence (identified as rape, sexual assault, or sexual harassment) and intimate partner violence (intimate partner violence; identified as violence committed by a spouse, partner, or ex-partner). These two categories may overlap, for instance, in cases of marital rape.

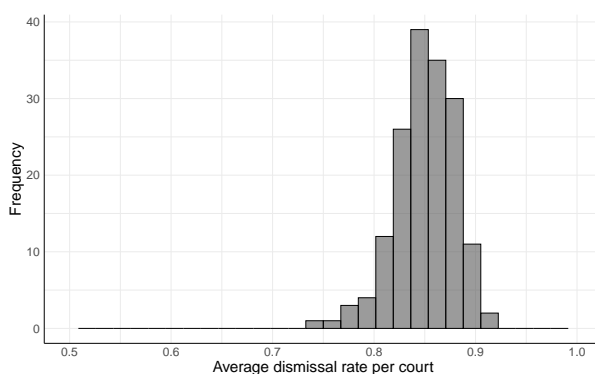
Rape is legally classified as a felony in France and should in principle be adjudicated in criminal courts (*cours d'assises*). However, a significant share of rape cases are reclassified either at the prosecutor's office or after an investigation. As such, the study focuses on cases recorded as rape upon entry into the judicial system but which were either dismissed or downgraded to a lesser charge (see Section 2.A.3). These downgraded rape cases are either requalified as misdemeanors (e.g., sexual assault) because the legal definition of rape is not met or are deliberately prosecuted as lesser offenses despite fulfilling the definition of rape.

2.B.2 Further descriptive results

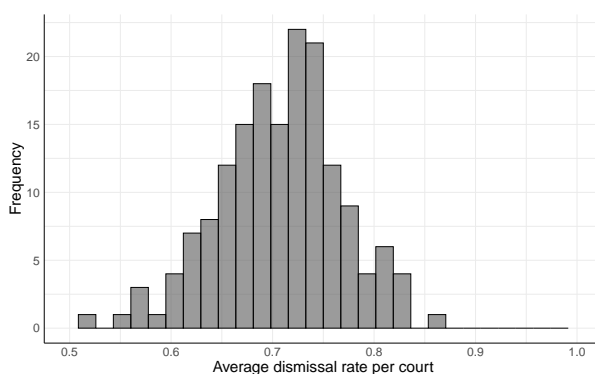
Figure 2.B.1: Distribution of the average propensity to dismiss sexual and intimate partner violence cases per court



(a) Sexual and intimate partner violence



(b) Sexual violence

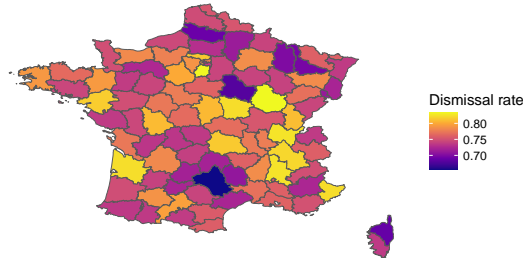


(c) Intimate partner violence

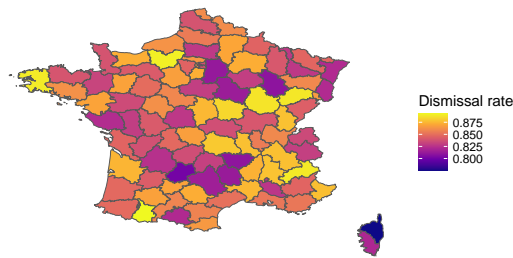
Notes: Distribution of the average propensity to dismiss sexual and intimate partner violence cases per court over 2012-2019. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The mean is 0.76, with a standard deviation of 0.05, a min of 0.61 and a max of 0.88. The first quartile is 0.73 and the third quartile 0.79.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

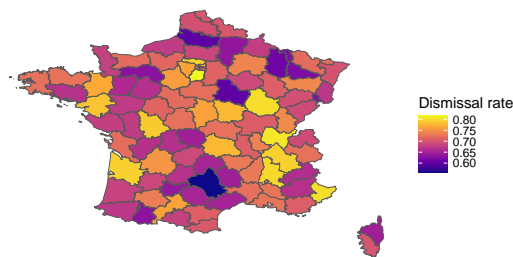
Figure 2.B.2: Distribution of the average propensity to dismiss sexual and intimate partner violence cases per county



(a) Sexual and intimate partner violence



(b) Sexual violence

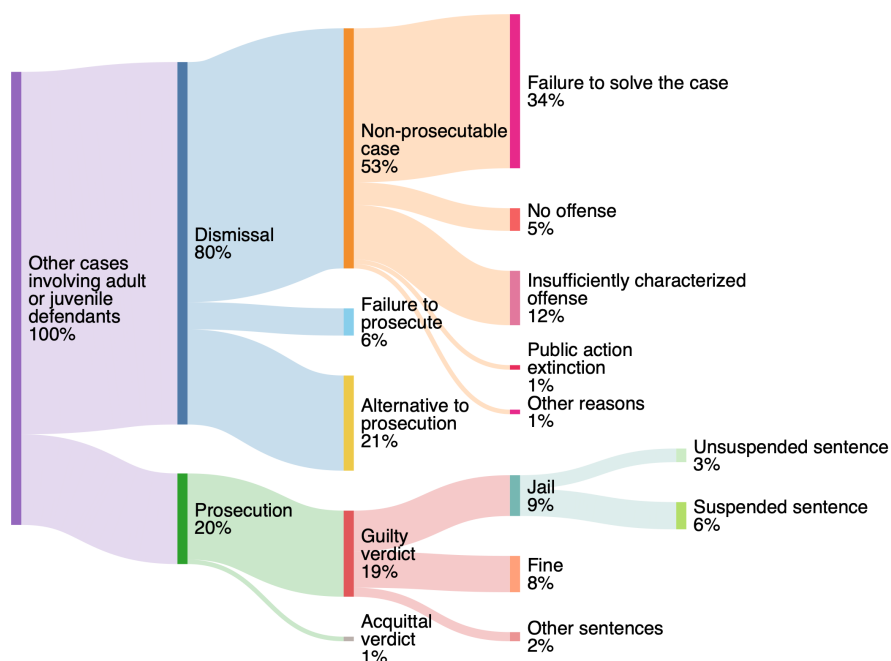


(c) Intimate partner violence

Notes: Distribution of the average propensity to dismiss sexual and intimate partner violence cases per county over 2012-2019. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.B.3: Judicial handling of other crimes involving persons (2012-2019)

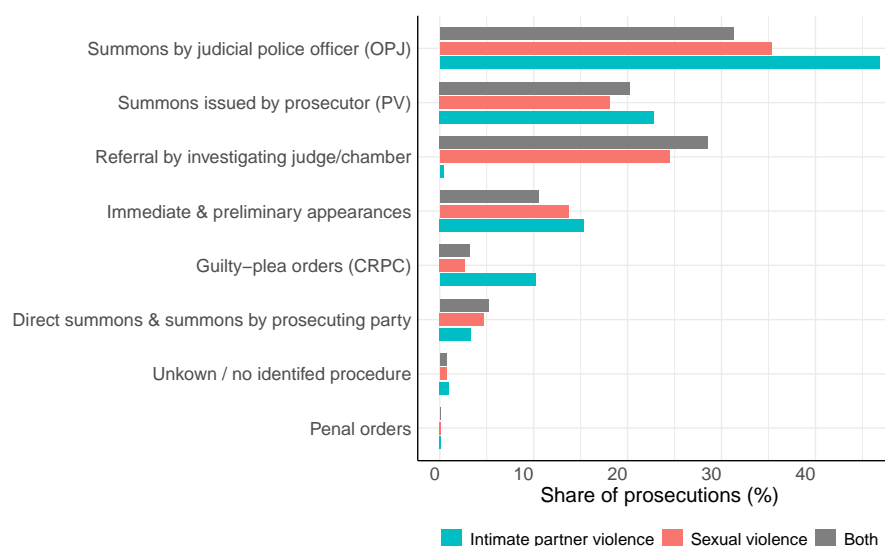


Notes: Share of cases at each stage of the criminal justice system process, among all cases other than sexual or intimate partner violence involving adult or juvenile defendants received across all French jurisdictions and either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. These are mainly cases of thefts or burglaries (28.62%), driving violations such as driving under alcohol or drugs (8.76%), physical violence (8.40%) or destruction (8.34%).

Reading: 80% of all cases other than sexual and intimate partner violence completed between 2012 and 2019 were dismissed, with 54% of all cases dismissed because they were deemed not prosecutable. The perpetrator was prosecuted in 20% of cases and found guilty in 19%, with a prison sentence imposed in 9% of all cases.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Figure 2.B.4: Prosecution rate by mode of prosecution

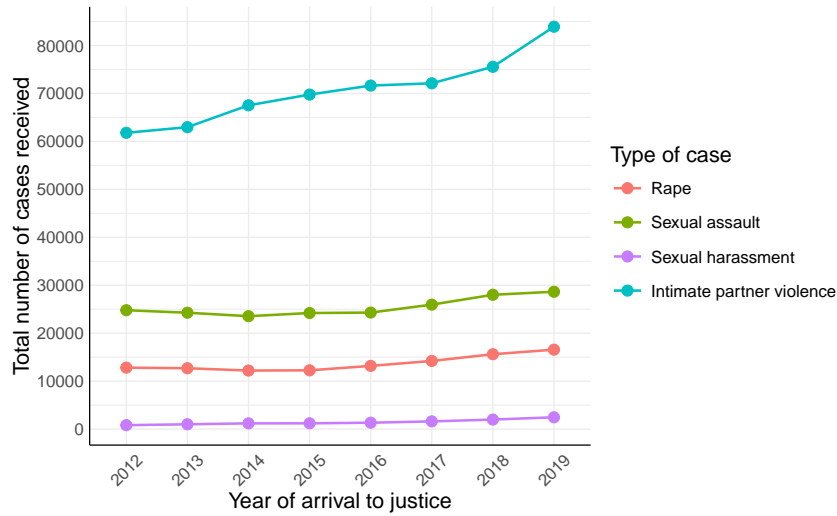


Notes: Share of cases of sexual violence and intimate partner violence processed by mode of prosecution across all French jurisdictions between 2012 and 2019.

Reading: Over 2012-2019, 46% of intimate partner violence cases were prosecuted through summons by judicial police officers, while 15% were prosecuted through immediate or preliminary appearances to court.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Figure 2.B.5: Number of cases of sexual and intimate partner violence registered within the criminal justice system, by type of case

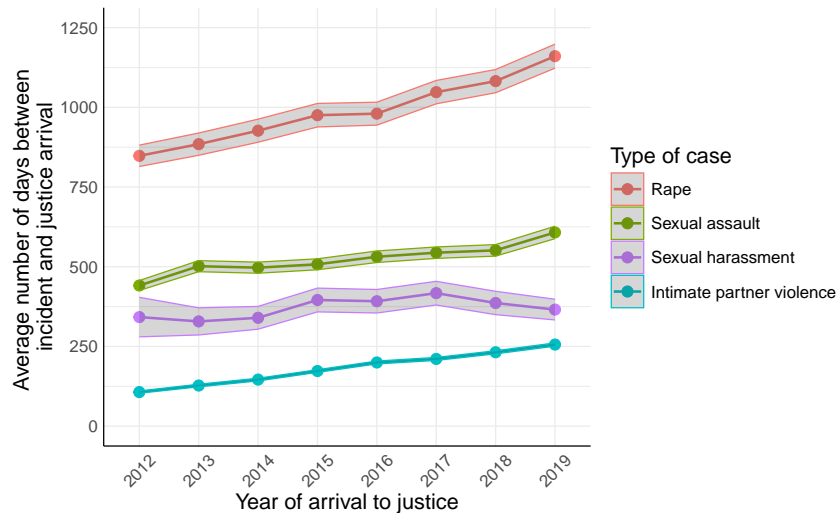


Notes: Time evolution of the number of cases of sexual violence, intimate partner violence, and other crimes reported to the French criminal justice system between 2012 and 2019 (and later dismissed or prosecuted in correctional or juvenile courts), in absolute numbers.

Reading: The total number of new intimate partner violence cases registered within the criminal justice system grew from around 61,000 in 2012 to almost 85,000 in 2019.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Figure 2.B.6: Time elapsed between date of incident and justice arrival, by type of case

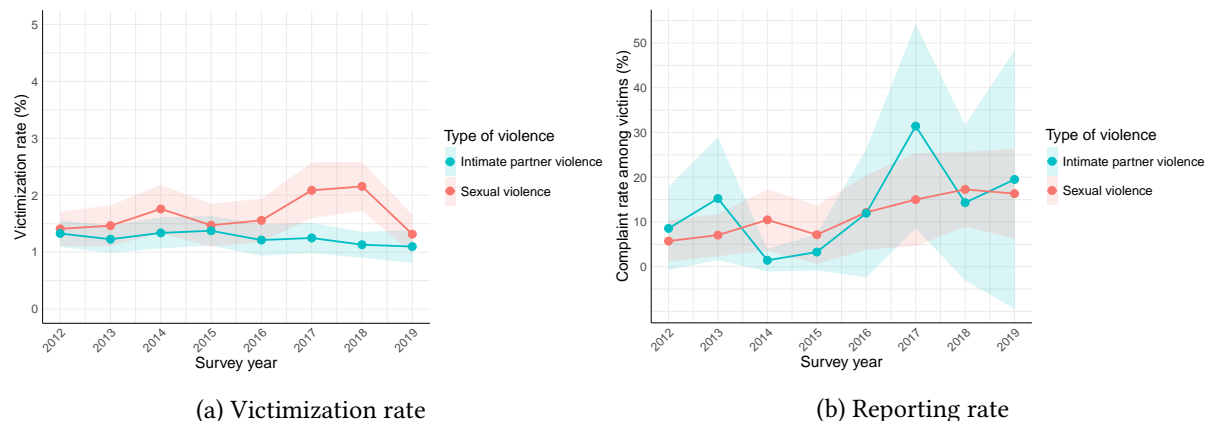


Notes: Time evolution of the average number of days elapsed between the start of the incident and its arrival to justice, for cases of sexual and intimate partner violence registered within the French criminal justice system between 2012 and 2019 (and later dismissed or prosecuted in correctional or juvenile courts).

Reading: The rape reporting delay increased from around 2 years in 2012 to 3 years in 2019.

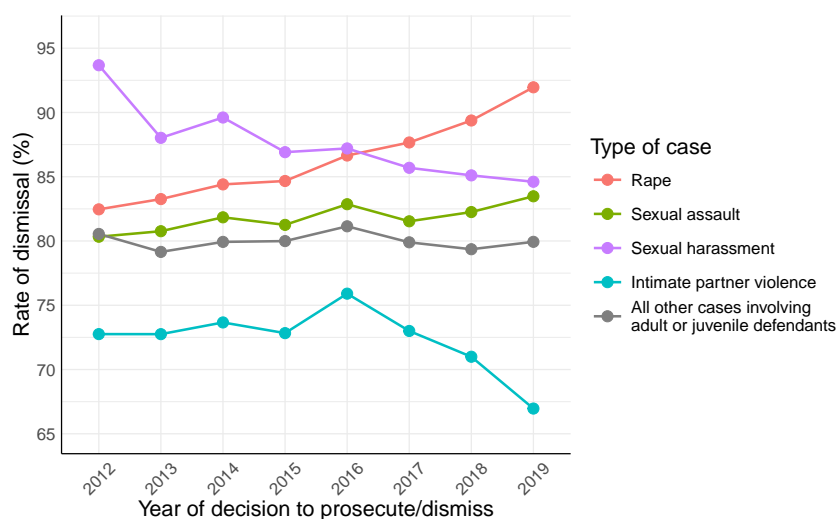
Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Figure 2.B.7: Time evolution of victimization and reporting rates for sexual and intimate partner violence



Notes: Time evolution of the share of self-reported female victims of sexual and intimate partner violence in the past two years (panel a) and the share of those who filed a complaint with the police to report this recent violence (panel b).
Reading: The victimization rate remained fairly stable over time, ranging from 1% to 2% of women on average. By contrast, the reporting rate increased over time, especially after 2016, rising from around 10% to nearly 20-30%.
Source: French Victimization Survey CVS data (INSEE), 2012-2019.

Figure 2.B.8: Dismissal rate by type of case – Zoom on sexual violence type



Notes: Time evolution of the share of cases of rape, sexual assault, sexual harassment, intimate partner violence, and other crimes dismissed by public prosecutors across all French jurisdictions between 2012 and 2019.
Reading: The dismissal rate for intimate partner violence cases decreased from an average of 73% in 2012 to 67% in 2019.
Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

2.C Additional elements on TV news data

2.C.1 Further information on the data source and construction

Figure 2.C.1: Example of INA data structure

Title	Channel	Date	Hour	Length (sec)	Descriptors	Summary
[Serial Rapist in Paris: Police Launch Witness Appeal]	TF1	2012-01-13	20:15:20	78	Parisian region, news story, rape, witness appeal, judicial police	Report in Paris where a serial rapist has been active since December. The Paris Judicial Police launch a witness appeal to find him. Interview with a witness. Excerpts from statements by Christian FLAESCH, Director of the Judicial Police of the Prefecture of Police in Paris, alternating with factual images.
[Analysis Segment: Plan Against Violence Against Women]	France 2	2013-11-22	20:12:02	82	society, women's condition, women's rights, woman, violence (domestic violence), couple, plan, Najat Vallaud-Belkacem	Analysis on set by Perrine Bonnet of the plan against violence against women recently launched by Najat Vallaud-Belkacem. She highlights the statistics on violence and the measures outlined in this plan. Her statements are accompanied by factual images and infographics.
[Brief News Segment: Alain Penin Sentenced to Life Imprisonment]	France 2	2014-01-23	20:30:03	15	trial, verdict, murder, young girl, conviction, Alain Penin (life imprisonment)	Brief. The Northern Assize Court has sentenced Alain PENIN to life imprisonment, with a minimum sentence of 22 years, for the murder of Natacha MOUGEL. Commentary on factual images.

Notes: Example of the TV news data structure.
Source: TV news data collected from the National Audiovisual Institute.

Figure 2.C.2: Words cloud



Notes: Words cloud of the most common words appearing in the list of keywords describing the content of the TV news stories on violence against women during the 8pm news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte).

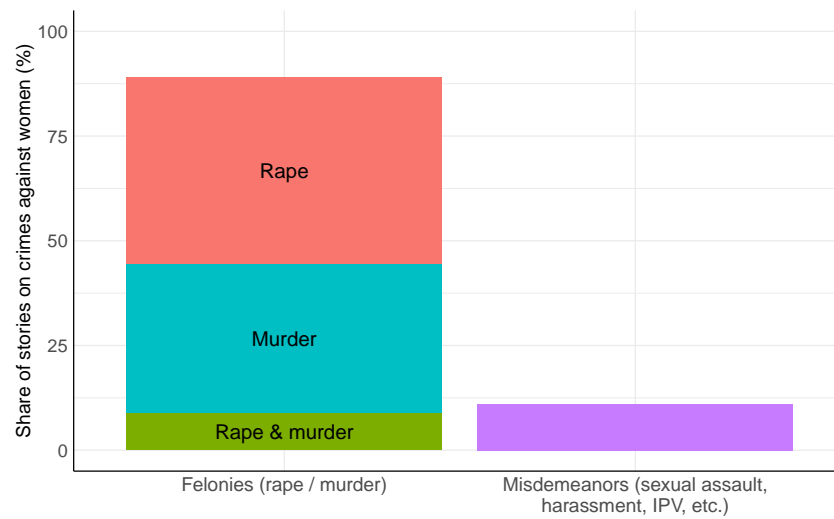
Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

Figure 2.C.3: Examples of news on crimes against women in 8pm TV news (TF1 & France 2)

Title	Date	Summary
[Serial Rapist in Paris]	2012-01-13	In Paris, the police have issued a search warrant for a dangerous man suspected of being a serial rapist after the rape of three women: two women on December 23rd, one of whom was assaulted with a knife.
[Investigation: Marie Jeanne Meyer, Jogger Found Dead]	2012-01-21	Report in Tournon-sur-Rhône dedicated to the ongoing investigation into the murder of Marie Jeanne Meyer, a jogger found dead in Ardèche last June. Update on the lead involving a suspected outsider.
[Discovery of Marie Bouchard's Body in Dijon]	2012-03-01	Incident in Dijon. The body of a 21-year-old woman, Marion Bouchard, was found. Her partner confessed to strangling and dismembering her after a violent dispute.
[Chloe's Kidnapping: Opening of a Judicial Investigation for Rape]	2012-11-19	Chloe's kidnapping: announcement of the opening of a judicial investigation for rape.
[A Woman Found Dead in Nîmes]	2013-01-25	A 33-year-old woman was killed while jogging on a path in Nîmes.
[Double Assault in Colombes]	2013-08-13	A man has been arrested for two extremely violent sexual assaults committed in Colombes. The suspect was captured on surveillance cameras. He is a repeat offender. Convicted in 2009 for rape and in 2012 for several offenses, he was on parole. The mother of a victim, currently in a coma, is filing a complaint against the justice system for endangering lives. The suspect faces 20 years in prison.
[DNA Tests on High School Students]	2014-04-12	At a private school in La Rochelle, DNA samples will be collected from male students and staff members to identify the person who raped a female student at the school last September.
[Rape Case of the Police Force]	2014-04-25	Extended custody of the three police officers indicted for the rape of a Canadian tourist in the premises of the judicial police in Paris.
[Highly Disturbing Disappearance in Haute-Saône]	2017-10-29	Haute-Saône, news story: the worrying disappearance of a 29-year-old jogger, Alexia Daval, near the town of Gray.
[North: Arrest of a Rapist]	2018-02-28	Reportage on the arrest of a serial rapist in the North.
[Femicide: Woman's Body Found in a Parking Lot]	2019-09-01	The body of a 21-year-old woman was found in the parking lot of a dump in Cagnes-sur-Mer, and her partner was apprehended. She is the hundredth woman killed by her partner since the beginning of the year.

Notes: Examples of news on crimes against women in the national 8pm TV news broadcast on TF1 and France 2.

Source: TV news data collected from the National Audiovisual Institute.

Figure 2.C.4: Breakdown of news stories on crimes against women (TF1 & France 2)

Notes: Felonies (rape/murder) vs. misdemeanors (sexual assault, harassment, IPV, etc.).

Source: TV news data collected from the National Audiovisual Institute.

Keywords used for VAW stories classification. Below is the list of words used to define the aggregate measures of media coverage of VAW for each content category. The categories have been created in the following order. News stories in later categories were selected only if they did not already fit into a former category, with the exception of certain word combinations.

1. **Trial:** trial, lawsuit, court, appeal, assizes, criminal, Cassation, courthouse, judge, justice, recourse, procedure, requisition, court transcript, hearing, judicial hearing, verdict, conviction, indictment, convicted, guilty, criminal confinement, dismissal, without prosecution, acquitted, acquittal, release, released, freedom, defamation, appeal, appealed, judicial control, compensation, civil procedure, grace measure, imprisonment, pardoned, commutation of sentence, parole, rejection of parole.
2. **Law and society** – Subdivided into:
 - **Preventative campaigns, protective measures, associations:** campaign, advertising campaign, toll-free number, listening number, helpline, 119, 3919, information campaign, communication campaign, advertising, information, security device, association, support group, SOS Solidarité Femmes, collective, training, prevention, care, reception center, medical treatment, phone call, reception, internship, self-defense, advertising message, screening, platform, emergency police, medical check-up, medical aid, human relations, legal aid, dog, BLAST, surveillance system, support group.
 - **Laws, government, criminal policy:** criminal procedure, criminal justice, criminal law, reform, law, jurisprudence, experimentation, experience, political program, Grenelle, plan, budget, device, bracelet, emergency phone, mobile phone, grand danger device, virtual reality, restorative justice, report, council of ministers, Matignon, minister, brigade, domestic policy, public policy, measure, Raffarin, circular, National Education, State Secretariat, constitution, constitutional, Macron, President of the Republic.

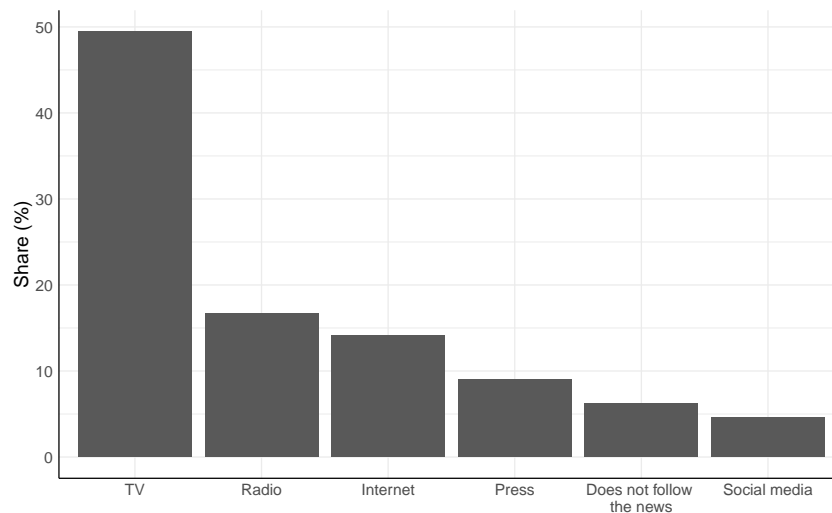
- **Society, women's rights, events, arts:** women's rights, international day, thematic day, World Day, tribute, march, observatory, society, opinion poll, demonstration, protest, tribune, white march, gathering, social network, social phenomenon, film, documentary, music video, book, play, exhibition, TV series, novel, short film, sexism, testimony, feminine condition, condition of women, figure, insecurity, numerical data, journalism, media.
3. **Crime:** disappearance, abduction, kidnapping, body, blood, dead, miscellaneous facts, news item, serial killer, police investigation, arrest, custody, indicted, police sketch, false testimony, public prosecutor's office, judicial inquiry, accusation, accused, complaint, rape, rapist, serial rapist, murder, death, suspected, assault, harassment, judicial procedure, immolation, genetic footprint, DNA, confession, actively sought, lifted.

2.C.2 News consumption according to ELIPSS data

ELIPSS survey data. To document television consumption as a source of news, I use data from ELIPSS (*Étude Longitudinale par Internet pour les Sciences Sociales*), a probability-based longitudinal panel of adults in metropolitan France (excluding Corsica), living in private households and aged 18–79 (Tiberj and Gougou, 2020). ELIPSS interviews are self-administered online on a tablet provided to respondents (with an internet connection), and fielded on a regular basis. The analyses draw on the DYNAMOB module (wave 13; Sept.–Oct. 2016; $N = 2,687$ respondents). The survey includes questions on respondents’ main sources of news information (e.g., television vs. other media) and the TV channel most often watched for news. It also provides survey weights and detailed sociodemographic characteristics, which allow for the construction of audience profiles.

Figure 2.C.5 shows that television is the main source of news information for nearly 50% of respondents in 2016. Figure 2.C.6 further indicates that TF1 and France 2 are among the most commonly watched TV channels for news. Figure 2.C.7 shows that magistrates—who are more likely to belong to the CSP categories “Senior civil servants” or “Intermediate professions”—primarily rely on television and radio as their main sources of news. Relative to the overall French population, TF1/France 2 news viewers have broadly similar observable characteristics, but are overrepresented among individuals with low education and those aged 50 and above (including retirees), and slightly underrepresented among employed individuals (Figure 2.C.8). Finally, the occupational-class (CSP) composition of TF1/France 2 viewers is overall close to that of the French population (Figure 2.C.9).

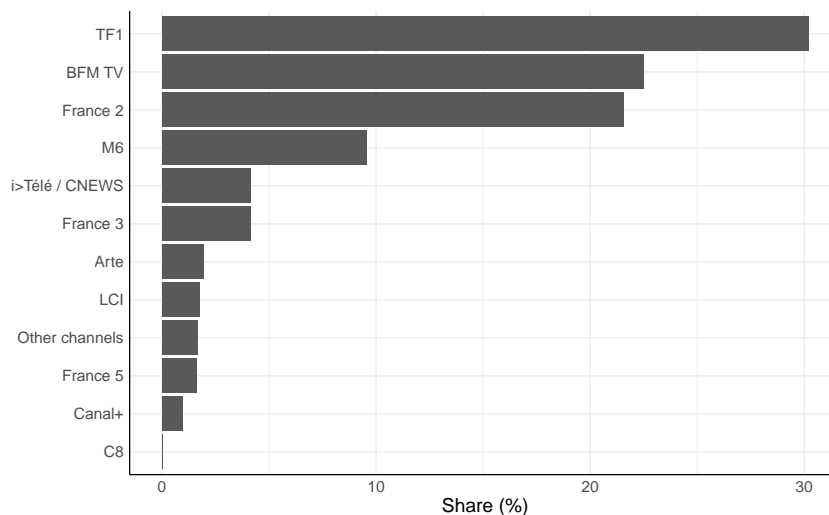
Figure 2.C.5: Main source of news information



Notes: Main source of news information based on weighted data from the 2016 ELIPSS survey.

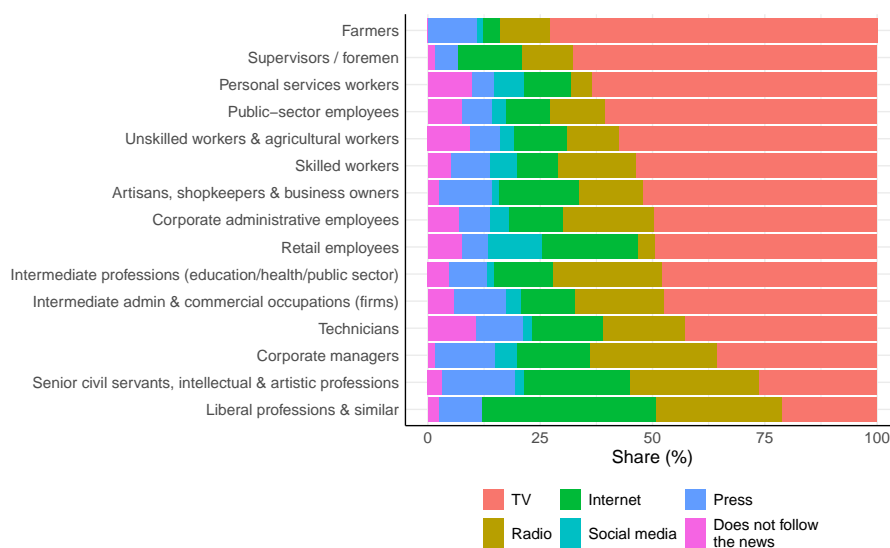
Source: ELIPSS panel (2016)

Figure 2.C.6: Main TV channel used for news information



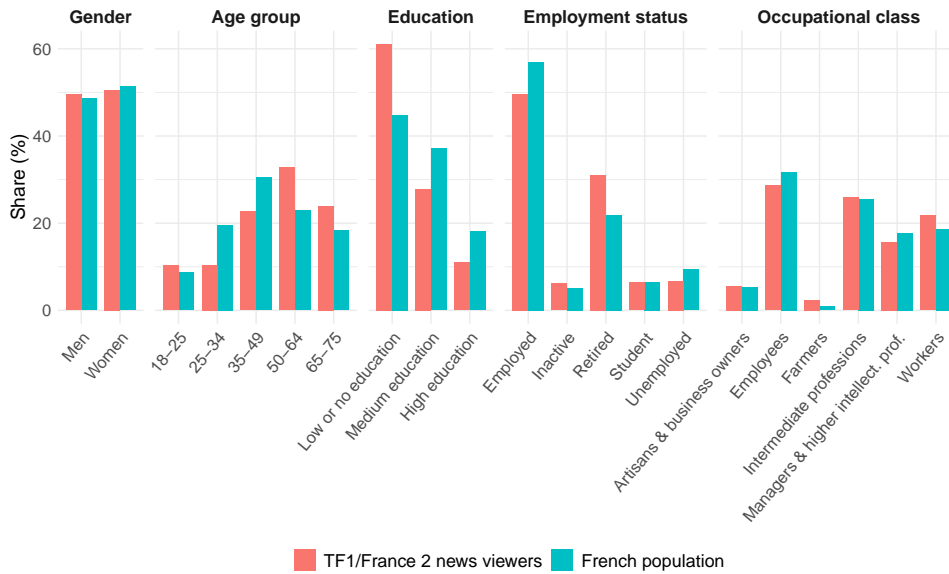
Notes: Main TV channel used for news information based on weighted data from the 2016 ELIPSS survey.
Source: ELIPSS panel (2016)

Figure 2.C.7: Main source of news by occupational class



Notes: Main source of news information across occupational classes (CSP18), based on weighted data from the 2016 ELIPSS survey.
Source: ELIPSS panel (2016)

Figure 2.C.8: Audience profile of TF1/France 2 news viewers compared to population



Notes: Audience profile of TF1/France 2 news viewers compared to the rest of the French population, based on weighted data from the 2016 ELIPSS survey.
 Source: ELIPSS panel (2016)

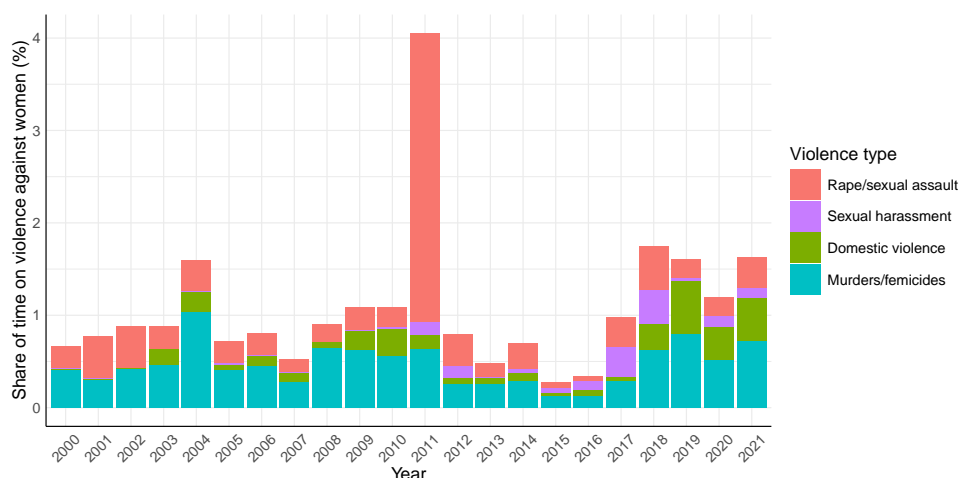
Figure 2.C.9: Detailed CSP profile of TF1/France 2 news viewers compared to population



Notes: Detailed CSP profile of TF1/France 2 news viewers compared to the rest of the French population, based on weighted data from the 2016 ELIPSS survey.
 Source: ELIPSS panel (2016)

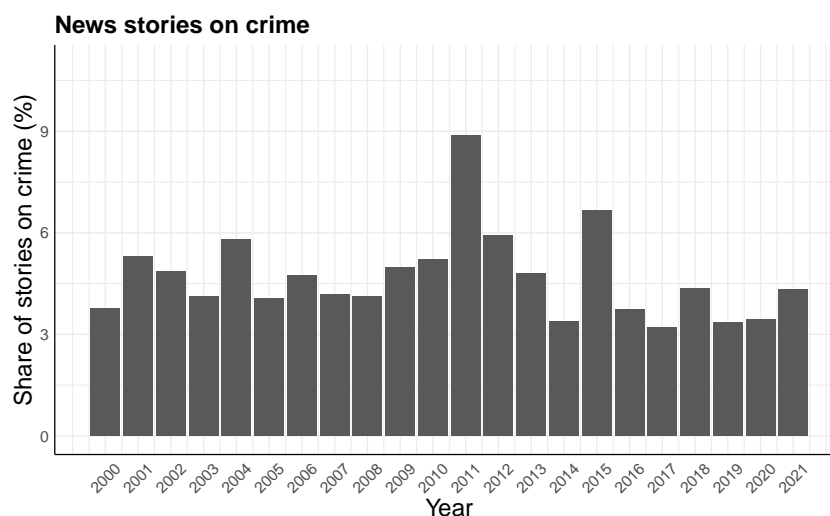
2.C.3 Further descriptive results

Figure 2.C.10: Share of stories on VAW (time) in national 8pm TV news, by type of VAW



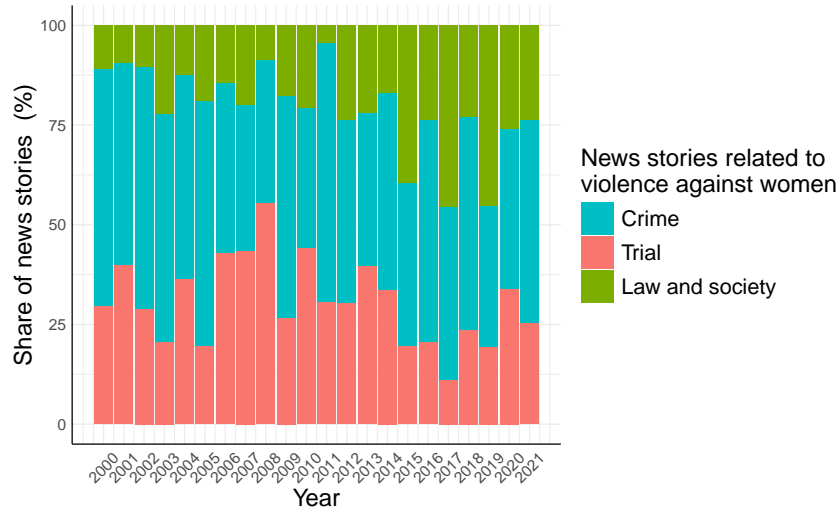
Notes: Time evolution of the share of TV news stories (in time) about violence against women (VAW) in the 8pm news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte), by type of violence.
Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

Figure 2.C.11: Share of stories on any crime in national 8pm TV news



Notes: Time evolution of the share of TV news stories about crime in general (including crimes against women) in the 8pm news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte), by type of violence.
Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

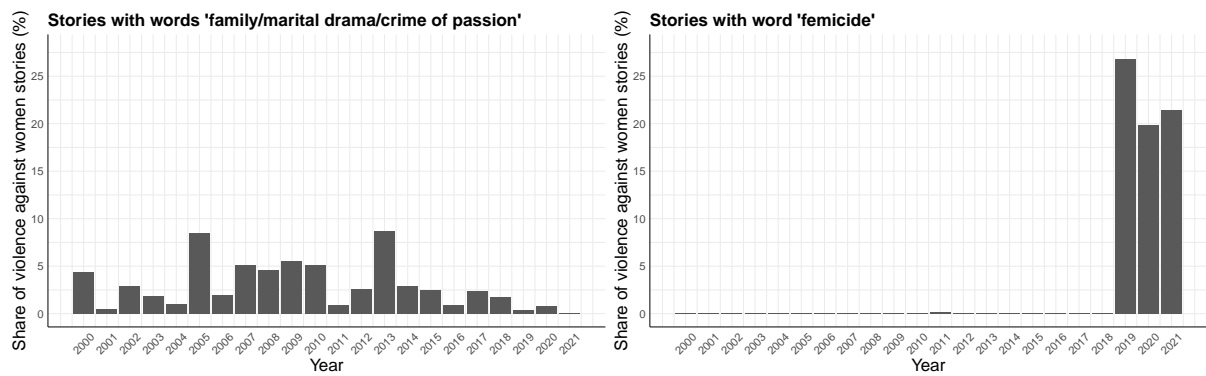
Figure 2.C.12: Content of VAW stories in national 8pm TV news, by news category



Notes: Time evolution of the share of stories on violence against women (VAW) in national 8pm TV news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte) within each exclusive content category: (i) crime, (ii) trial or (iii) law and societal issues related to VAW.

Source: Author’s calculations based on TV news data collected from the National Audiovisual Institute.

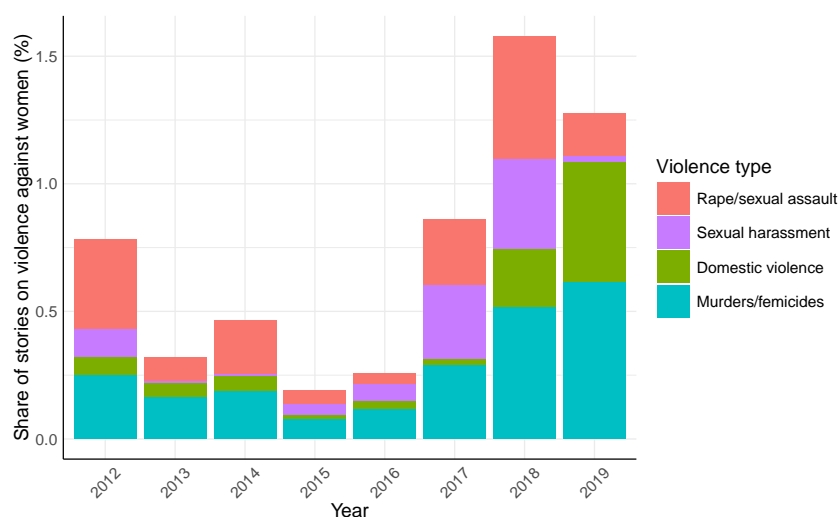
Figure 2.C.13: Coverage of intimate partner violence in national 8pm TV news



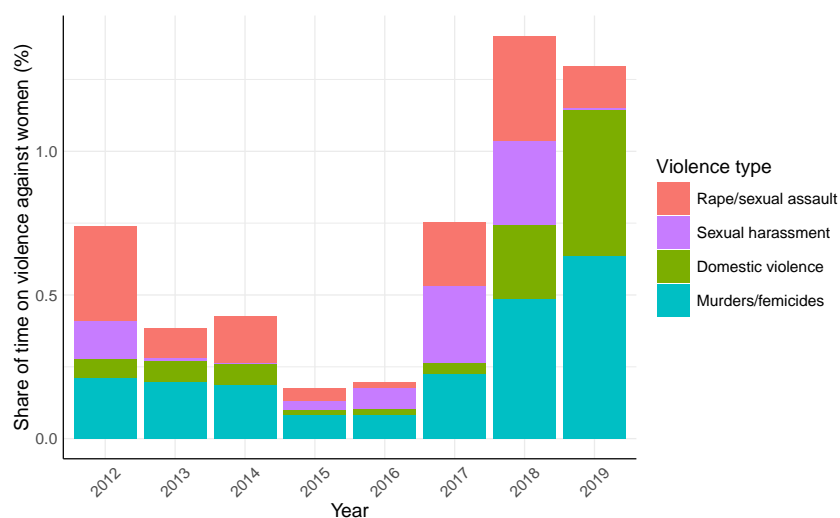
Notes: Time evolution of the share of TV news stories on violence against women using specific narratives in the 8pm news broadcast of the main French national TV channels (TF1, France 2, France 3, Canal+, France 5, M6, Arte).

Source: Author’s calculations based on TV news data collected from the National Audiovisual Institute.

Figure 2.C.14: Evolution of VAW coverage in TF1 & France 2 8pm TV news, by violence type



(a) Share of news stories (number)



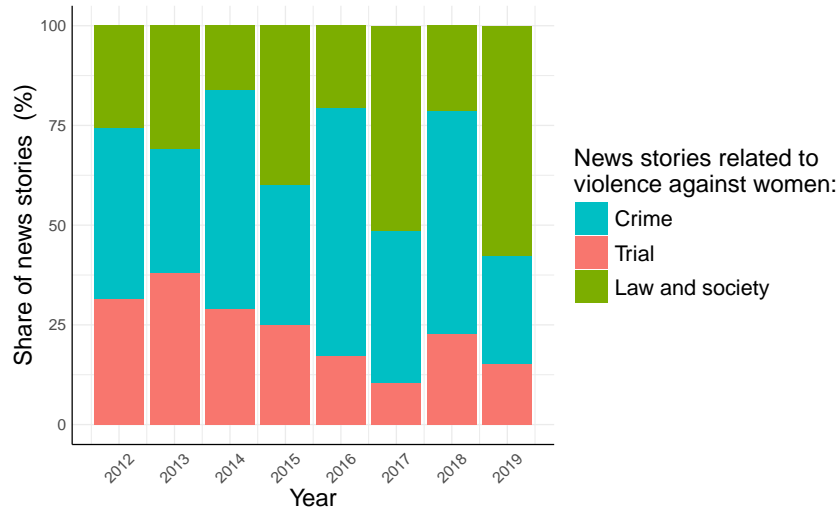
(b) Share of news stories (time)

Notes: Time evolution of the share of TV news stories about violence against women (VAW) in the 8pm TV news broadcast on TF1 and France 2, by type of violence.

Reading: The topic of VAW represented on average 0.75% of all news stories broadcast on TF1 and France 2 prime time news in 2012.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

Figure 2.C.15: Content of VAW stories in TF1 & France 2 8pm TV news, by news category

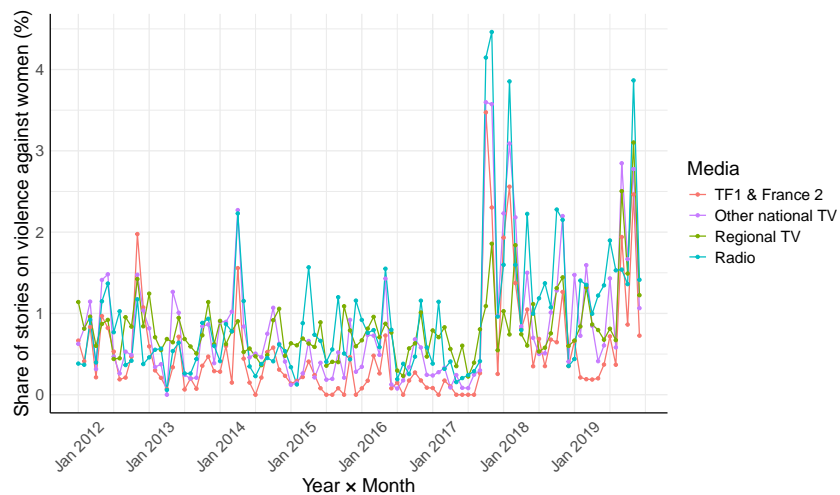


Notes: Time evolution of the share of news stories on violence against women (VAW) in the 8pm TV news broadcast on TF1 and France 2, within each exclusive content category: (i) crime, (ii) trial or (iii) law and societal issues related to VAW.

Reading: In 2012, news stories on VAW in TF1 and France 2 prime-time news were divided as follows: 45% focused on crimes, 30% on trials, and 25% on law and societal issues.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

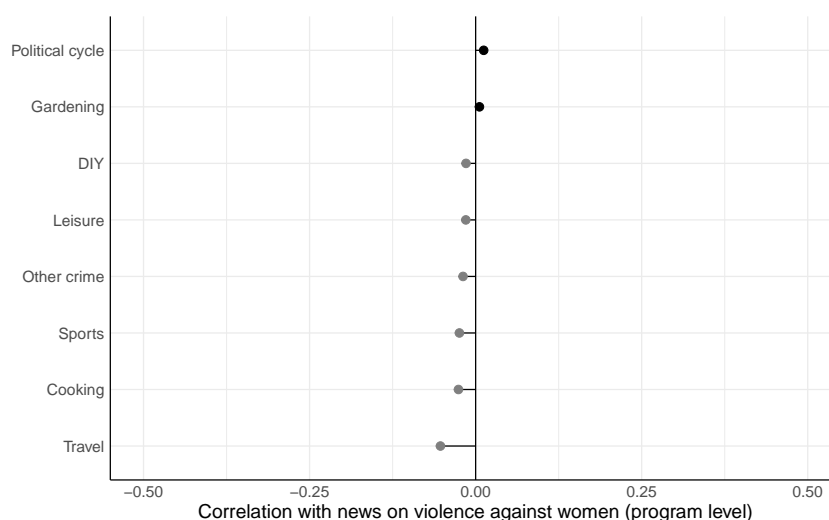
Figure 2.C.16: Correlation between news coverage on 8pm TV news and other media



Notes: Correlation of the share of news stories on violence against women across different media outlets: national TV, regional TV and radio.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

Figure 2.C.17: Correlation of violence against women topics with other news topics



Notes: Correlation of the share of news stories on violence against women with other news topics.

Source: Author's calculations based on TV news data collected from the National Audiovisual Institute.

Table 2.C.1: Breakdown of news stories of crimes against women, by distance to the court

News story that occurred...	Proportion of news stories on crimes against women
...in the same county	2.3%
...in adjacent county	6.7%
...in other counties	91%

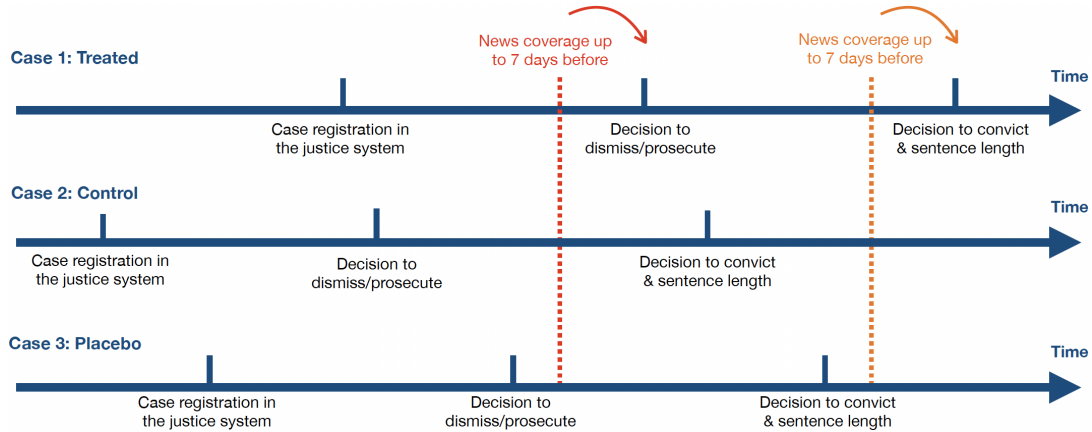
Reading: 2.3% of news stories on crimes against women covered in the 8pm TV news broadcasts of TF1 and France 2 occurred in the same county as the jurisdiction, and only 6.7% occurred in an adjacent county.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute (a subsample of TF1 and France 2 news story transcriptions on crimes against women).

2.D Empirical strategy

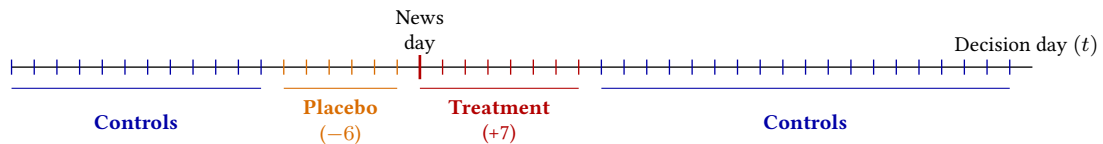
2.D.1 Illustrations of the empirical strategy

Figure 2.D.1: Illustration of the empirical design



Notes: I start from my judicial decisions database and include all cases in the analysis. For some cases, the decision is issued a few days after a news story about violence against women (VAW) (with coverage measured over the previous 7 days), which defines the treatment group. For other cases, the decision is issued when there is no such news coverage, which defines the control group. The idea is to compare these otherwise similar cases (taking into account case characteristics and seasonality) that differ only in whether they were processed shortly after relatively more coverage of VAW. To assess whether cases decided just before a media coverage episode are comparable to those decided far from any coverage, I also implement a placebo test: I compare decisions issued up to 7 days before a VAW news story to decisions issued away from any media coverage.

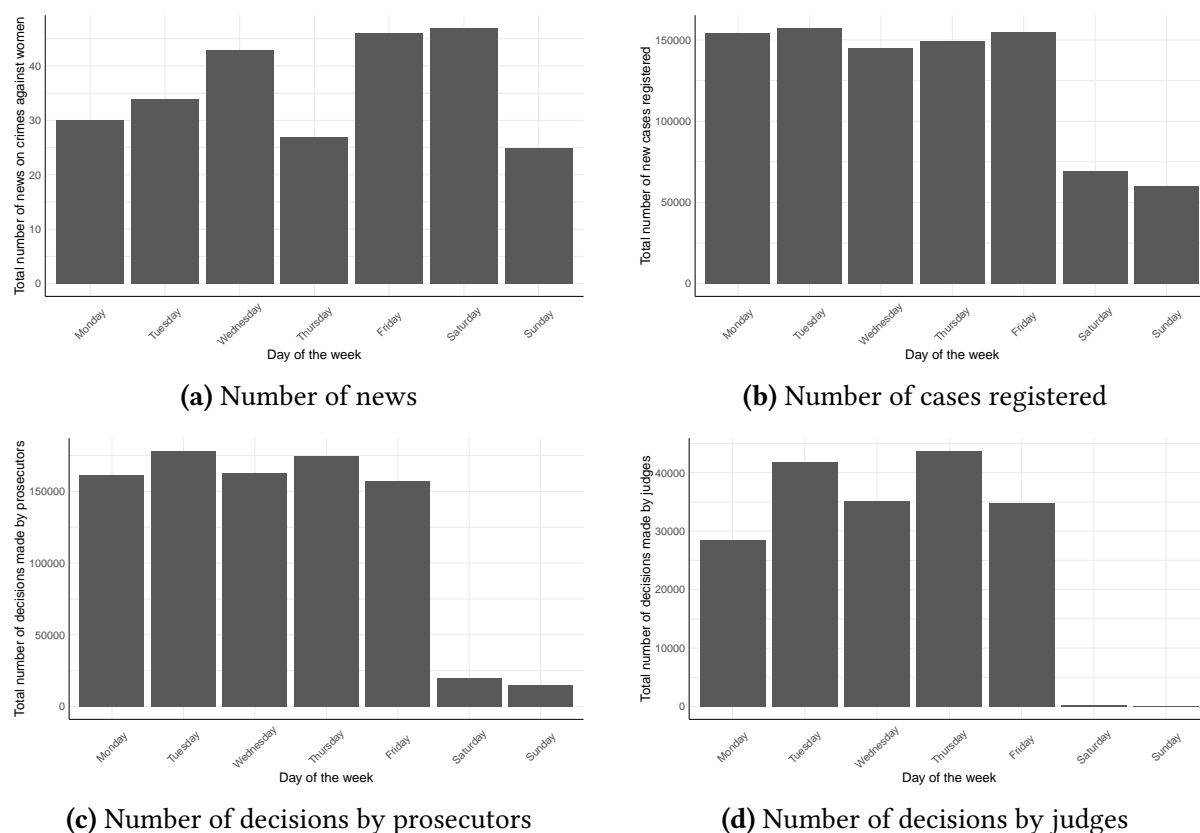
Figure 2.D.2: Defining treated and control decision days



Notes: This figure illustrates the event-time design around a day of TV news coverage of violence against women (VAW). Each observation corresponds to a judicial decision (outcome Y) issued on day t . The treatment period includes decisions made in the seven days following a news episode ($t = 0$ to $t = +7$), while decisions made in the six days preceding the news ($t = -6$ to $t = -1$) serve as placebo leads to test for pre-trends. All other decision days constitute the control group. TV news coverage may occur on multiple days over the 2012–2019 period. In the preferred specification, I exclude periods in which more than one news episode occurred within the previous seven days, although results are robust to including all periods.

2.D.2 Seasonality and balance tests

Figure 2.D.3: Seasonality of the number of news and judicial decisions by day of the week



Notes: Total number of decisions regarding cases of sexual and intimate partner violence taken by prosecutors by day of the week, for cases that reached the criminal justice system and were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. Source: Author's calculations based on administrative data extracted from the management software *Cassiopée*, provided by the French Ministry of Justice.

Table 2.D.1: Covariate balance: Characteristics of cases examined by prosecutors, by media coverage on the days before the prosecutorial decision

Dependent Variables:	Socio-demographics			Affair's origin			Type of offense				Other case's characteristics			
	Male	French	Age	Police	Justice	Other	Rape	Sexual assault	Sexual harassment	IPV	# Victims	Incident length	Reporting length	Orientation length
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
<i>News on VAW crimes</i>														
# of news 7 days before	0.0014*	-0.0004	-0.0050	-0.0003	4.73×10^{-5}	0.0002	0.0002	3.77×10^{-5}	-0.0002	0.0023**	-0.0021	-3.483	-1.993	-0.0762
	(0.0007)	(0.0011)	(0.0340)	(0.0008)	(0.0003)	(0.0007)	(0.0018)	(0.0018)	(0.0007)	(0.0011)	(0.0019)	(3.477)	(2.564)	(0.7669)
<i>Fit statistics</i>														
Observations	672,085	579,460	605,586	753,759	753,759	753,759	274,370	274,370	274,370	753,759	753,759	283,119	749,888	752,008
R ²	0.00221	0.09597	0.00456	0.01893	0.00448	0.01837	0.00576	0.00624	0.00473	0.01592	0.00631	0.00354	0.00723	0.02196
<i>News on VAW crimes & trials</i>														
# of news 7 days before	0.0005	-0.0010	0.0235	-0.0006	-0.0001	0.0007	-0.0015	0.0016	-6.77×10^{-5}	0.0017*	0.0004	-0.3759	-3.063	0.4921
	(0.0006)	(0.0009)	(0.0282)	(0.0006)	(0.0002)	(0.0006)	(0.0014)	(0.0015)	(0.0006)	(0.0009)	(0.0017)	(2.877)	(2.113)	(0.6245)
<i>Fit statistics</i>														
Observations	626,363	540,164	564,491	702,384	702,384	702,384	255,312	255,312	255,312	702,384	702,384	264,001	698,887	700,770
R ²	0.00220	0.09606	0.00457	0.01895	0.00431	0.01841	0.00582	0.00616	0.00475	0.01593	0.00645	0.00349	0.00726	0.02197
<i>Fixed-effects</i>														
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.90	0.75	36.35	0.87	0.02	0.11	0.33	0.63	0.03	0.64	1.28	359.55	345.25	210.31
<i>Heteroskedasticity-robust standard-errors in parentheses</i>														
<i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>														

Notes: OLS estimates of regressions on the effect of TV news coverage of violence against women on various characteristics of judicial cases investigated by prosecutors. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variables are the case characteristics of the cases examined by prosecutors, listed in the first row of the table. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. The first panel uses news on VAW crimes, while the second panel also includes news on VAW-related trials.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.D.2: Covariate balance: Characteristics of cases investigated by judges, by media coverage on the days before the judge decision

Dependent Variables:	Socio-demographics			Affair's origin			Type of offense				Other case's characteristics				
	Male	French	Age	Police	Justice	Other	Rape	Sexual assault	Sexual harassment	IPV	# Victims	Incident length	Reporting length	Prosecution length	Total case duration
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	
<i>News on VAW crimes</i>															
# of news 7 days before	0.0002 (0.0009)	-7.17×10^{-5} (0.0020)	0.0303 (0.0636)	-0.0024** (0.0012)	0.0020*** (0.0006)	-3.31×10^{-5} (0.0010)	0.0003 (0.0020)	0.0002 (0.0024)	-0.0006 (0.0014)	-0.0019 (0.0021)	0.0104* (0.0059)	-0.9425 (6.313)	2.104 (4.083)	-0.1812 (1.093)	7.508 (5.056)
<i>Fit statistics</i>															
Observations	158,099	157,921	157,374	158,099	158,099	158,099	39,669	39,669	39,669	158,099	158,099	58,832	156,561	155,461	157,675
R ²	0.00463	0.10363	0.00730	0.01412	0.00677	0.01427	0.01341	0.01248	0.01310	0.02054	0.01430	0.00895	0.00867	0.01825	0.01521
<i>News on VAW crimes & trials</i>															
# of news 7 days before	0.0008 (0.0007)	0.0014 (0.0016)	0.0379 (0.0519)	-0.0003 (0.0010)	0.0004 (0.0004)	-0.0003 (0.0008)	-0.0018 (0.0016)	0.0022 (0.0019)	-0.0004 (0.0012)	-0.0038** (0.0017)	0.0001 (0.0045)	-3.869 (4.912)	1.084 (3.316)	-0.8859 (0.8994)	4.585 (4.125)
<i>Fit statistics</i>															
Observations	146,424	146,261	145,782	146,424	146,424	146,424	36,610	36,610	36,610	146,424	146,424	54,583	145,034	144,004	146,053
R ²	0.00485	0.10391	0.00741	0.01418	0.00680	0.01445	0.01401	0.01254	0.01339	0.02076	0.01457	0.00873	0.00888	0.01825	0.01553
<i>Fixed-effects</i>															
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.97	0.75	35.83	0.94	0.01	0.05	0.04	0.94	0.02	0.75	1.49	319.08	245.72	85.81	584.37

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of regressions on the effect of TV news coverage of violence against women on various characteristics of judicial cases examined by judges. The sample consists of cases of sexual and intimate partner violence that were prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variables are the case characteristics of the cases examined by judges, listed in the first row of the table. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. The first panel uses news on VAW crimes, while the second panel also includes news on VAW-related trials.

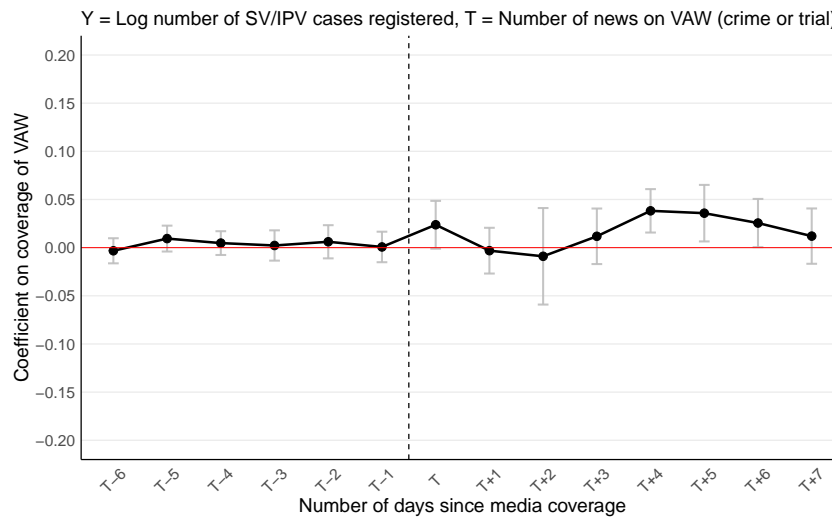
Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.E Additional results on reporting

2.E.1 Additional results on the number of cases reported

This appendix provides additional results on the effect of TV news coverage of crimes against women on reporting to the justice system. Figure 2.E.1 shows that the increase in the number of cases reported to justice still holds when news on trials related to VAW cases is included, while Figure 2.E.9 shows that it also holds when restricting the analysis to cases that occurred before the coverage episode surrounding the registration of the case. No clear additional effects emerge when examining a longer time period (Figure 2.E.2). Figures 2.E.4 and 2.E.5 indicate that the result is similar for both sexual and intimate partner violence cases and reveal no other significant heterogeneity.

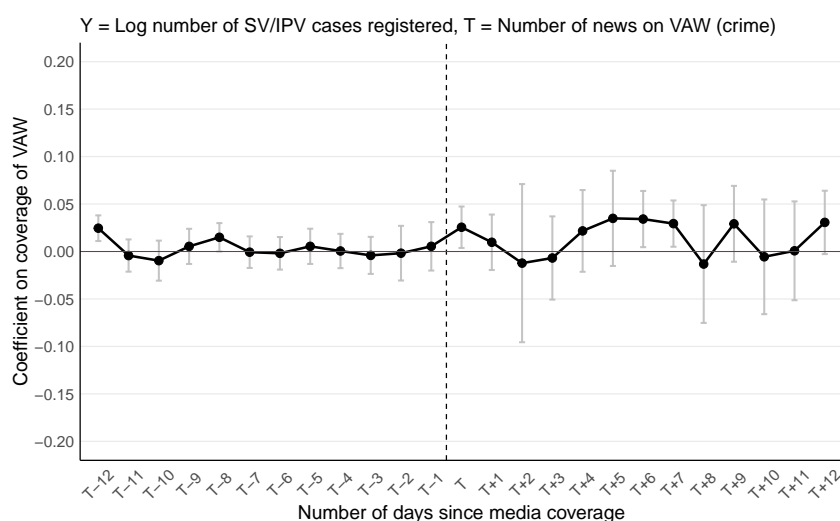
Figure 2.E.1: Effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases reported to justice, including news on trials



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women or related trials on the reporting of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the number of news stories on crimes against women or related trials broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

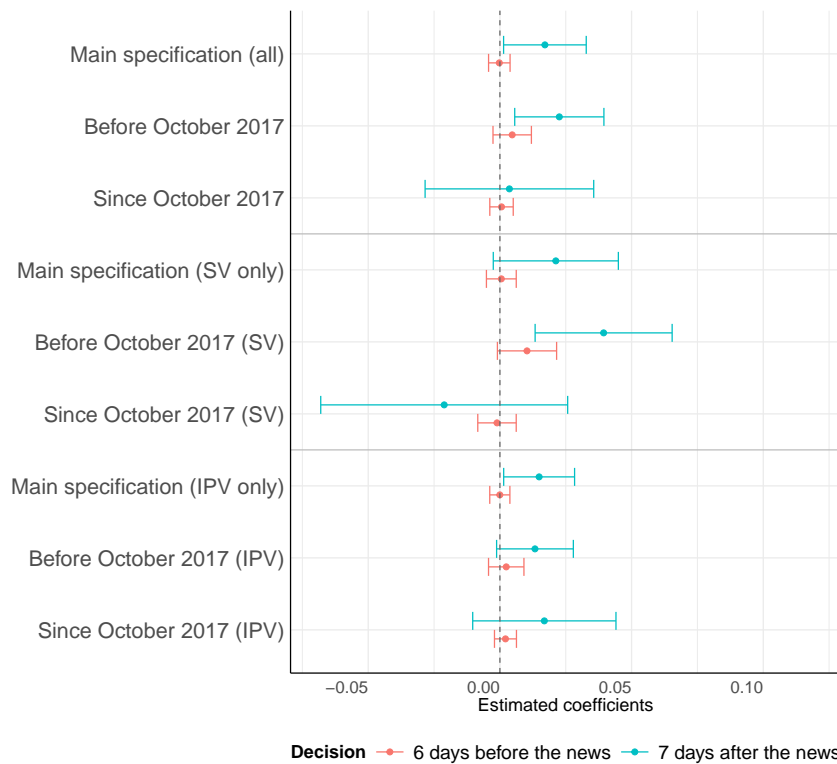
Figure 2.E.2: Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice – Using more periods



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

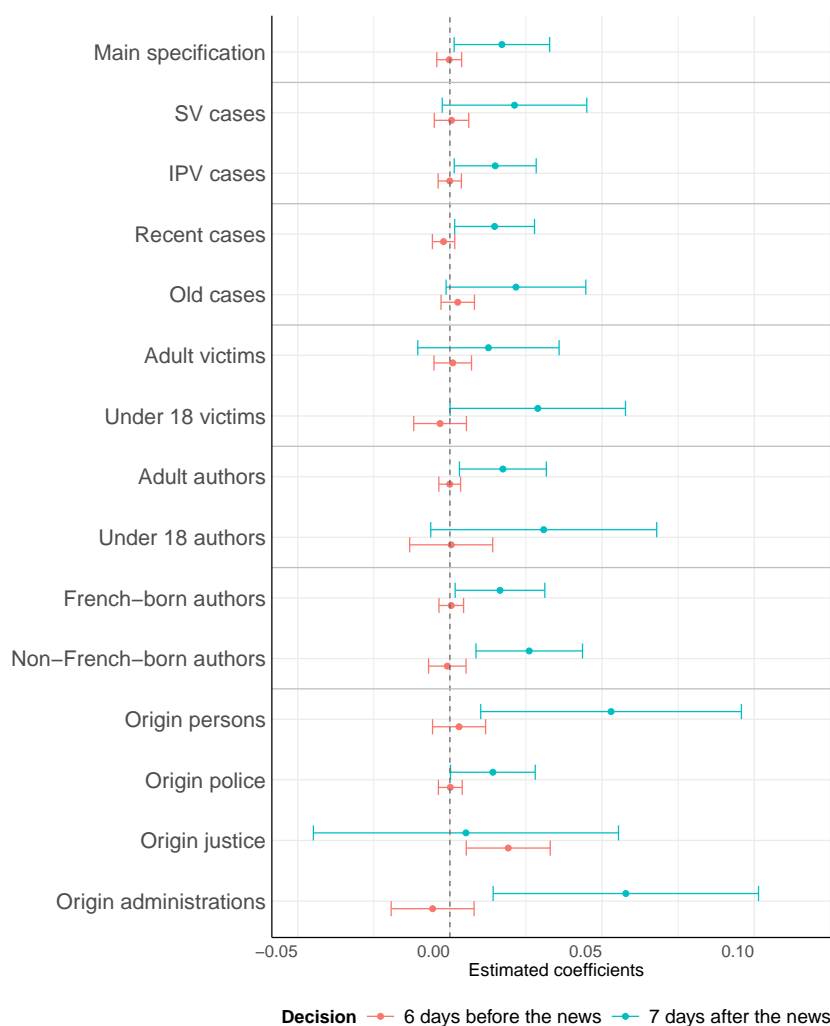
Figure 2.E.3: Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by period



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence by period, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the case registration.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

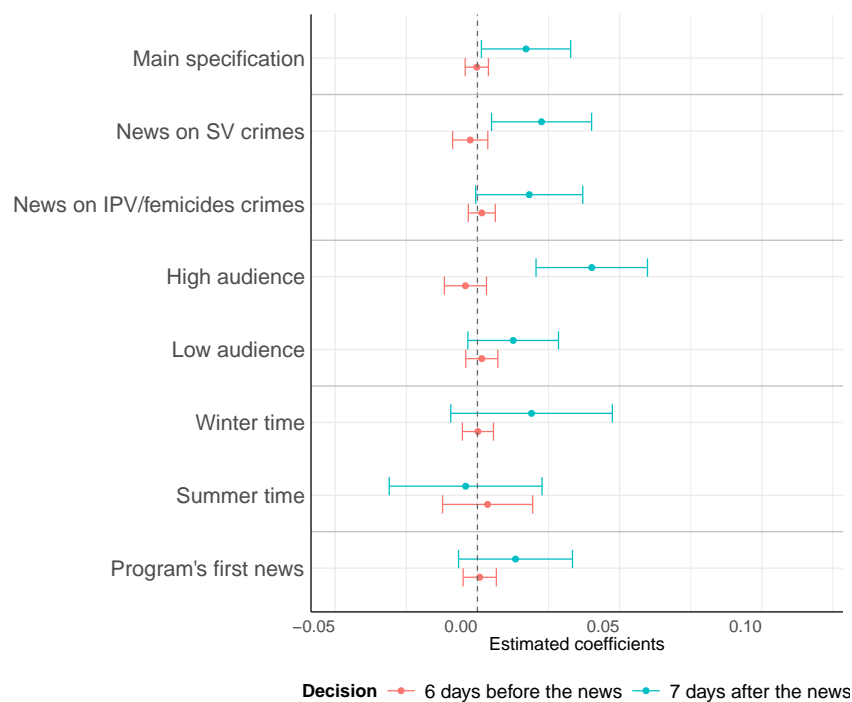
Figure 2.E.4: Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by case characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence by subgroups of case characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the case registration.

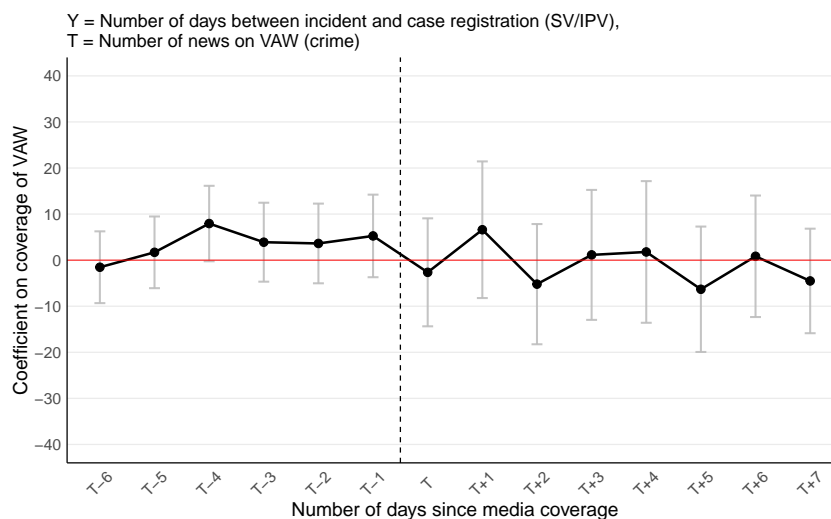
Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.E.5: Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by news characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence by subgroups of news characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the case registration. Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.E.6: Effect of news coverage of crimes against women on the number of days elapsed between incident and justice arrival for sexual and intimate partner violence cases



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the time elapsed between the incident and justice arrival, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$), restricting to cases that occurred before the news coverage. The dependent variable is the number of days elapsed between the start of the incident and its arrival to justice for cases reported to justice. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.E.2 Reporting or incidence?

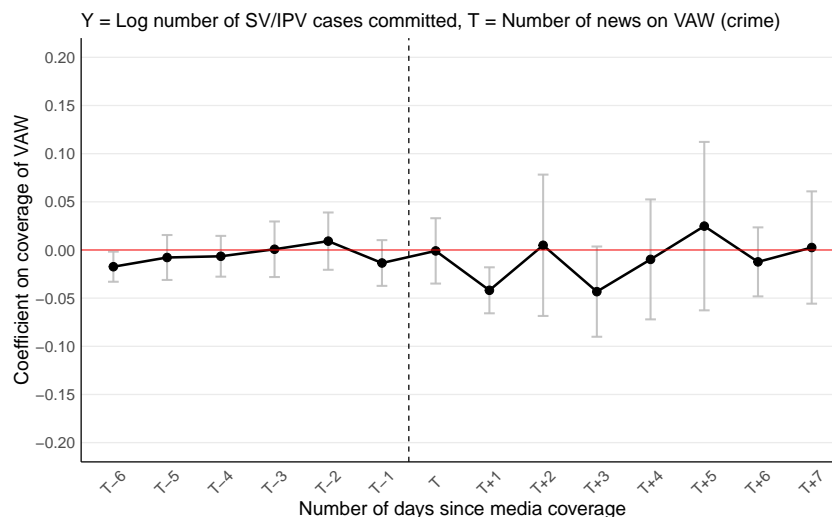
A key question in the literature investigating the determinants of reporting in VAW cases is whether observed effects are driven by increased reporting per se or by changes in incidence. Although the increase in reporting of sexual and intimate partner violence cases following TV news coverage of crimes against women is not entirely robust (see Section 2.5.3), I nonetheless attempt to assess whether this effect could reflect an actual rise in violence using several strategies.

First, I perform a similar analysis as in Section 2.5.3 but focusing on the effect of news coverage around the starting date of an incident. Although this does not allow for measuring the true prevalence of VAW, as I only observe cases that were reported to justice, Figure 2.E.7 suggests no increase in violence around the time of news coverage.

Second, I focus on the reporting of female homicides, which are arguably less subject to under-reporting. Figure 2.E.8 shows no increase in the reporting of female homicides following news coverage of crimes against women, whether in general or specifically related to femicides.

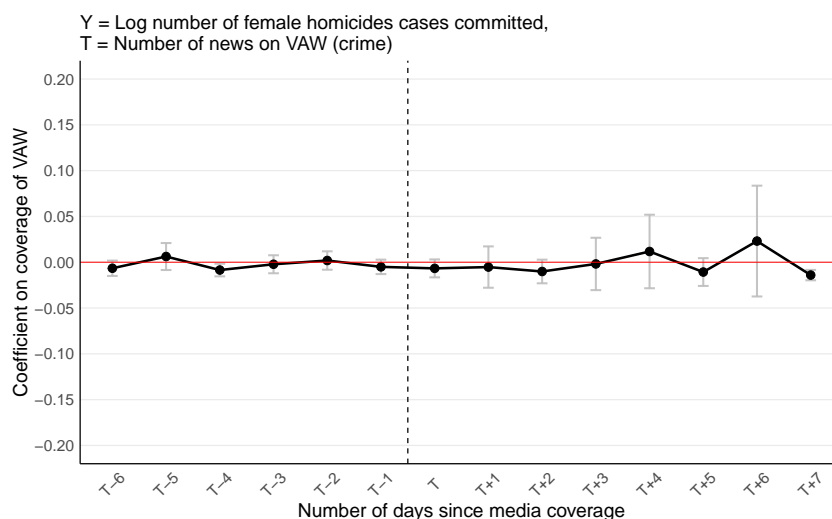
Third, using the incident start date, I restrict the analysis to cases in which the offense occurred before the coverage episode that anchors the case's registration event window, so their occurrence could not have been triggered by the news coverage. Figure 2.E.9 shows results that are indistinguishable from those in the full sample, indicating that the effect is driven by increased reporting rather than changes in incidence.

Figure 2.E.7: Effect of news coverage of crimes against women on the incidence of sexual and intimate partner violence cases

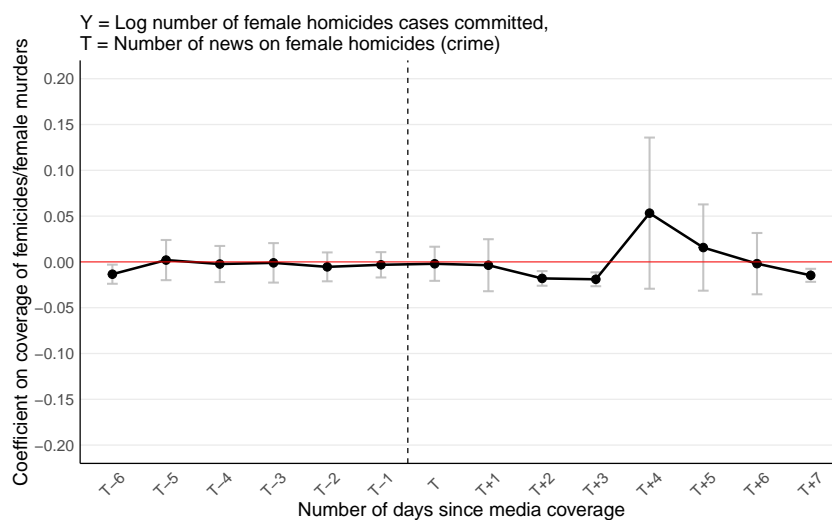


Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the incidence of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases occurring. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T . *Source:* Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.E.8: Effect of news coverage of crimes against women on the incidence of female homicides



(a) News on crimes against women

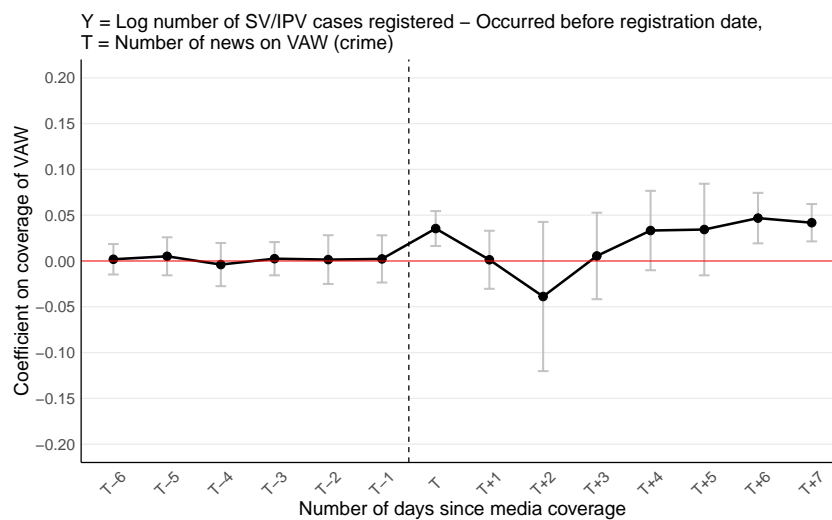


(b) News on female homicides

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the incidence of female homicides, with corresponding 95% confidence intervals. The sample consists of cases of female homicides registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$), defined as "Homicide volontaire" or "Coups mortels ou atteintes volontaires à la personne ayant entraîné la mort" where there is at least one female victim and no male victim. The dependent variable is the natural logarithm of the number of new cases occurring. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T . In the upper panel, the sample is restricted to news about VAW crimes in general. In the lower panel, the sample is restricted to femicide news.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.E.9: Effect of news coverage of crimes against women on the number of cases reported to justice, for cases that occurred before the news coverage (82% of cases)

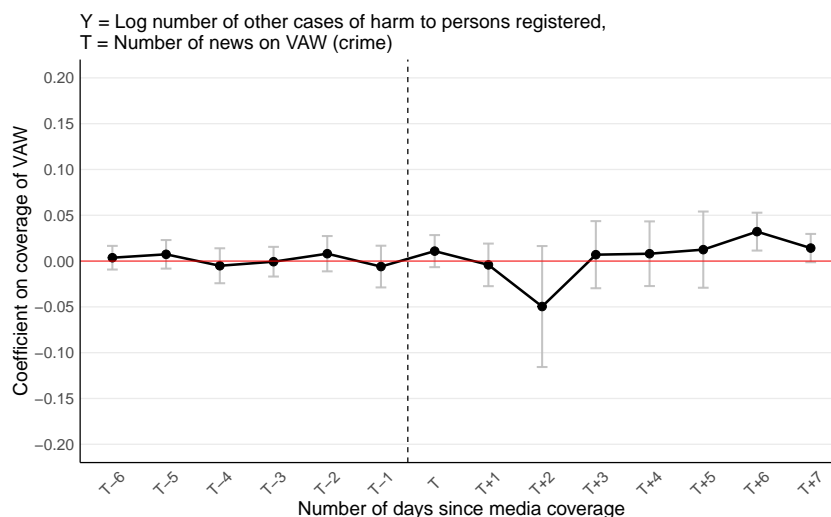


Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$), restricting to cases that occurred before the news coverage. The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

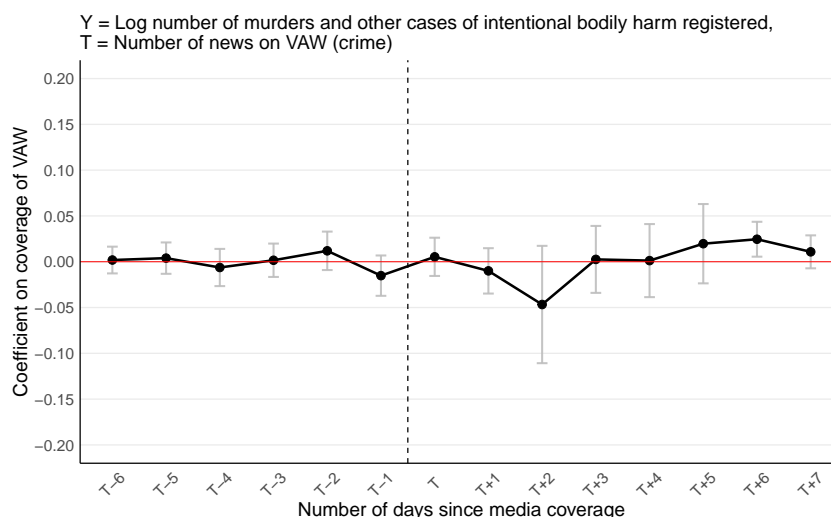
Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.E.3 Placebos

Figure 2.E.10: Effect of news coverage of crimes against women on the number of other offenses against the person reported to justice



(a) Other offenses against the person



(b) Murders and other intentional bodily harm

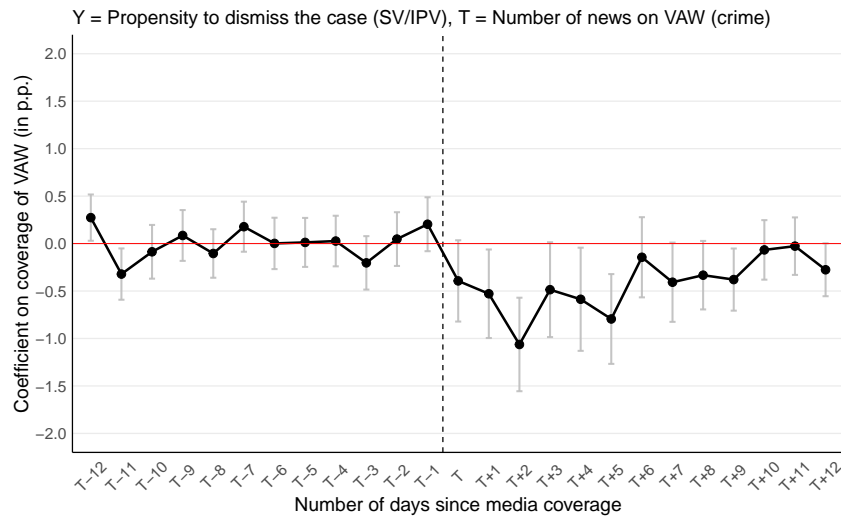
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the reporting of other offenses against the person, with corresponding 95% confidence intervals. In the upper panel, the sample consists of cases of other offenses against the person unrelated to VAW that were registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). In the bottom panel, the sample consists of a subsample of murders or intentional bodily harm excluding VAW that were registered daily within the French criminal justice system and dismissed or prosecuted in correctional or juvenile courts from 2012 to 2019 ($N = 2,921$). The dependent variable is the natural logarithm of the number of new cases reported to justice. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.F Additional results on prosecution

2.F.1 Additional results on the decision to dismiss a case

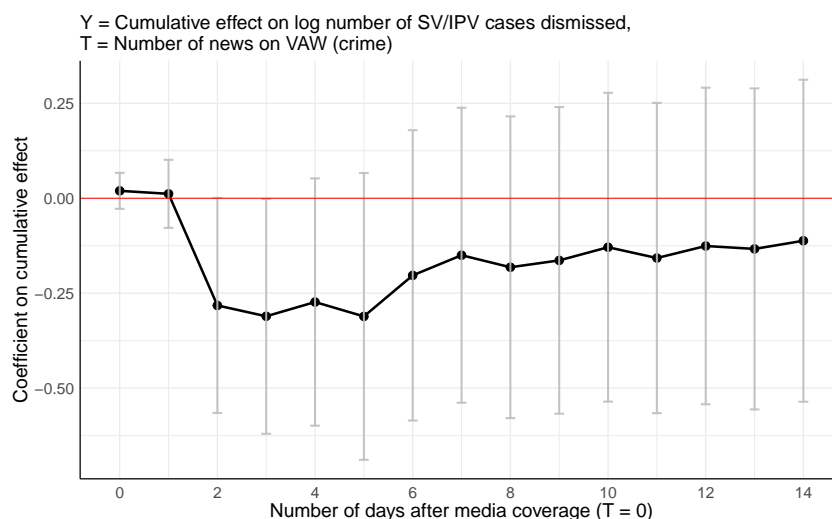
Figure 2.F.1: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Using more periods



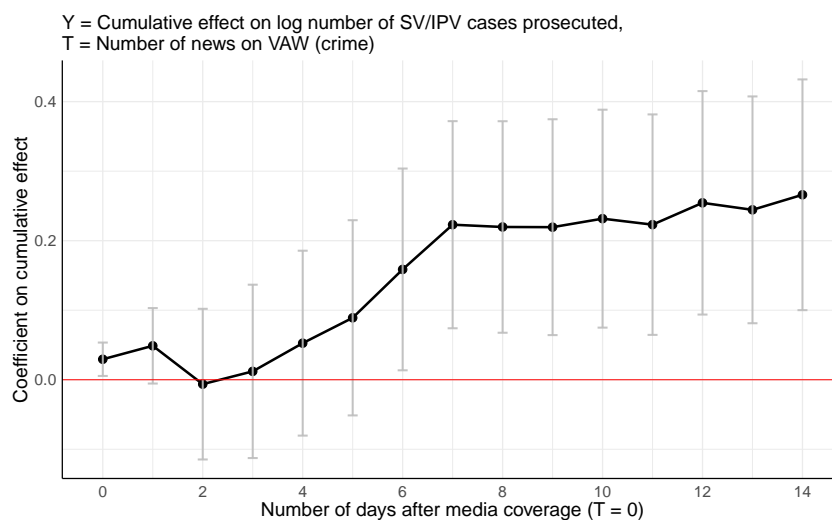
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.2: Cumulative effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases dismissed or prosecuted over a 2-weeks horizon



(a) Log number of affairs dismissed



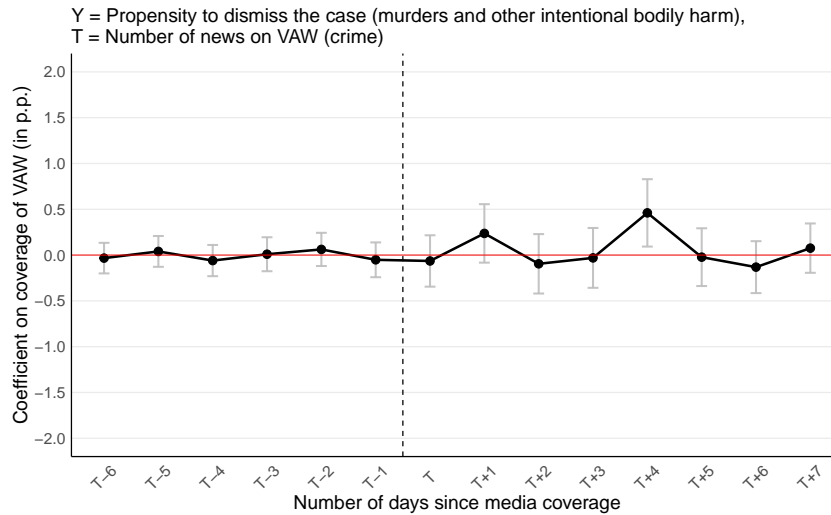
(b) Log number of affairs prosecuted

Notes: OLS estimates of cumulative lag coefficients from Equation 2.1, showing the effect of TV news coverage of crimes against women on the number of cases dismissed or prosecuted in the two weeks following the coverage, with corresponding 95% confidence intervals. Coefficients are normalized by the average value of the lead coefficients, and standard errors are corrected to account for cumulation. The sample includes cases of sexual and intimate partner violence that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. In the first panel, the dependent variable is the natural logarithm of the number of cases dismissed by prosecutors. In the second panel, the dependent variable is the natural logarithm of the number of cases prosecuted by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time $T = 0$.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.F.2 Placebos

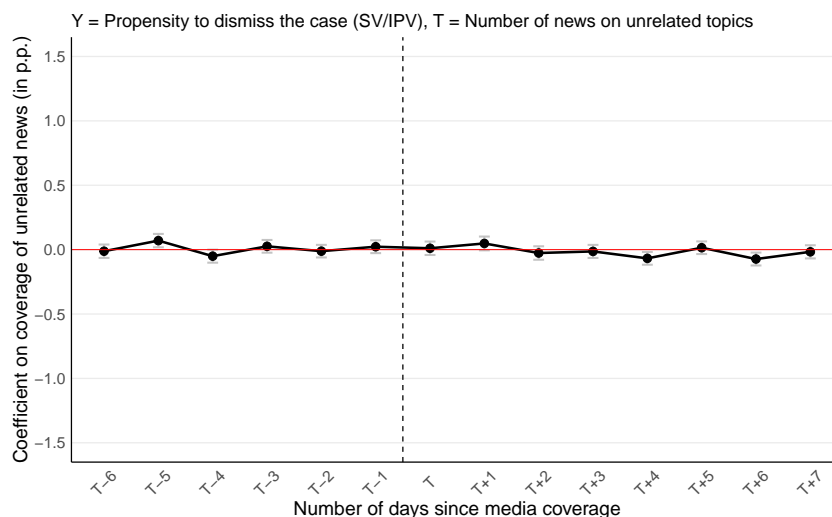
Figure 2.F.3: Effect of news coverage of crimes against women on the propensity to dismiss murders and other intentional bodily harm



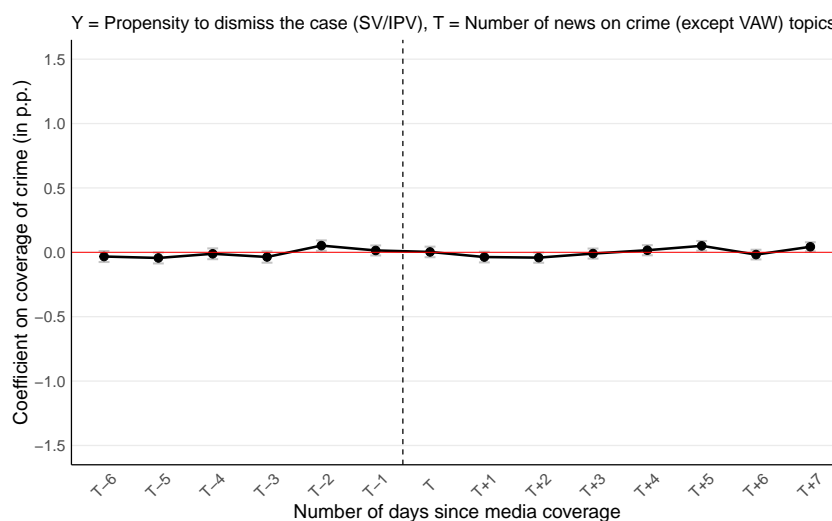
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss murders and other intentional bodily harm, with corresponding 95% confidence intervals (in percentage points). The sample consists of a cases of murders or intentional bodily harm excluding VAW that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopee* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.4: Effect of unrelated news coverage on the propensity to dismiss sexual and intimate partner violence cases



(a) News on unrelated topics



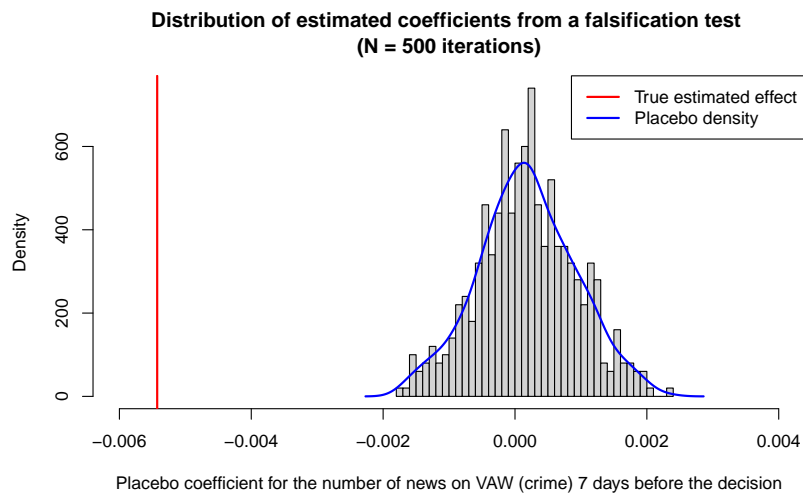
(b) News on crimes except VAW

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crime news not related to violence against women (VAW) on the propensity to dismiss VAW, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. The upper panel shows the effect of other news unrelated to VAW (gardening, crafts, leisure, and travel). The bottom panel shows the effect of TV news on crimes except VAW. News coverage is measured using the number of news stories on each topic broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Reading: Up to seven days after the TV news coverage of crime news unrelated to VAW, the propensity to dismiss sexual and intimate partner violence cases remains unaffected.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.5: Falsification test for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases

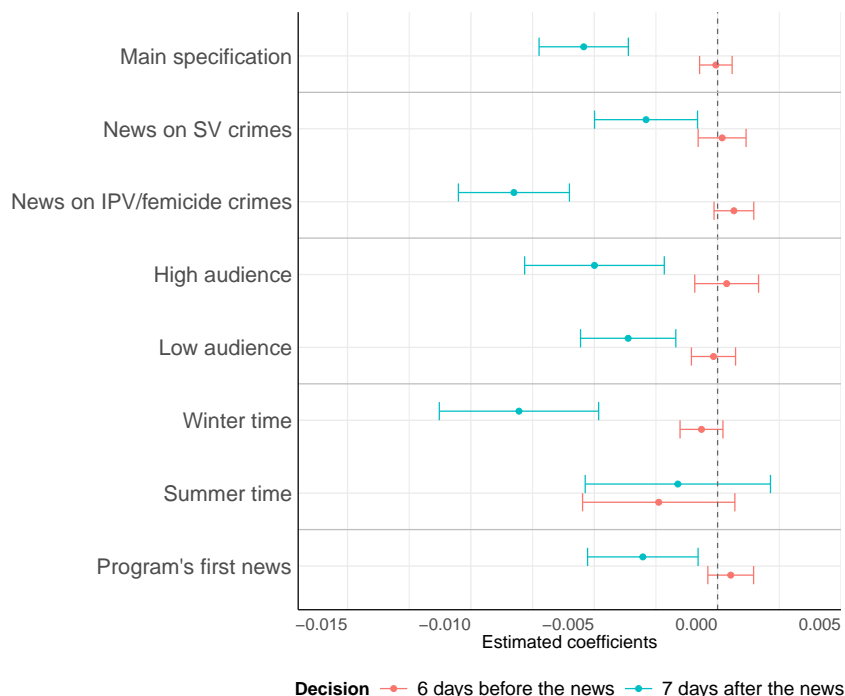


Notes: Falsification test of the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, using 500 placebo dates. The results show the distribution of the 500 estimated coefficients at $T + 2$, compared to the actual estimated effect.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

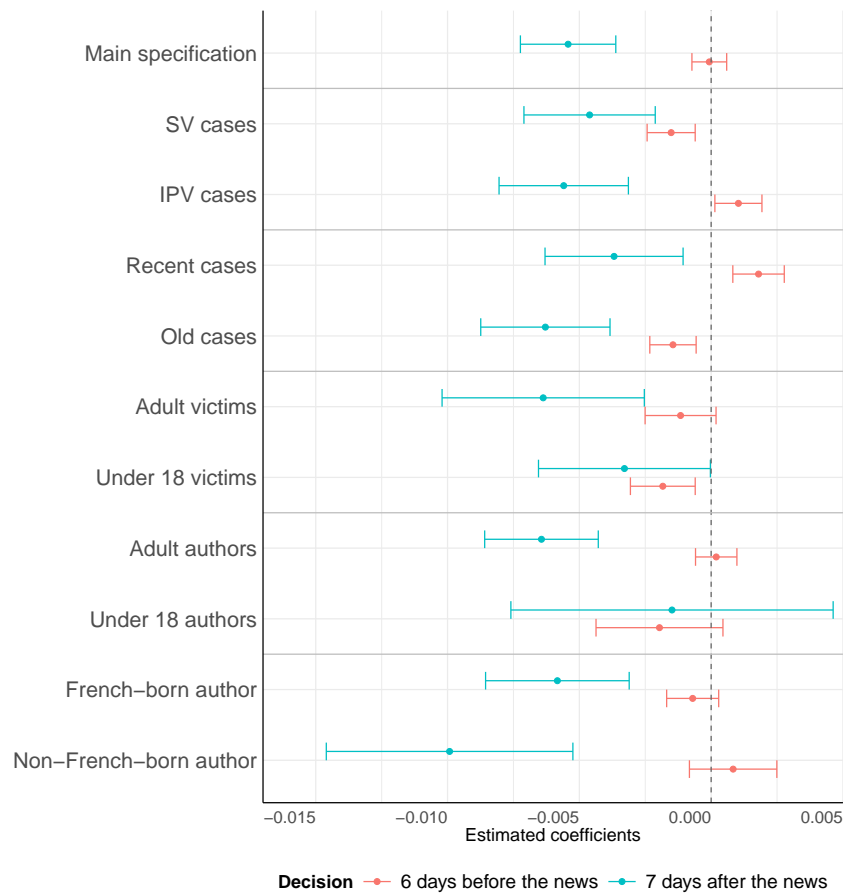
2.F.3 Heterogeneity analysis

Figure 2.F.6: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by news characteristics



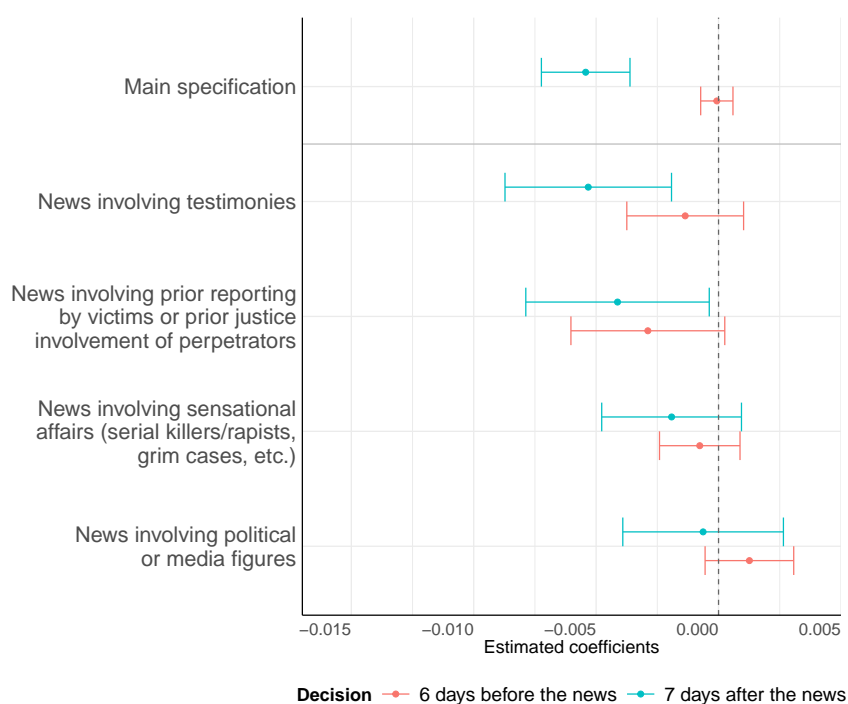
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence by subgroups of news characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.7: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by other case characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence by subgroups of case characteristics with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. *Source:* Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

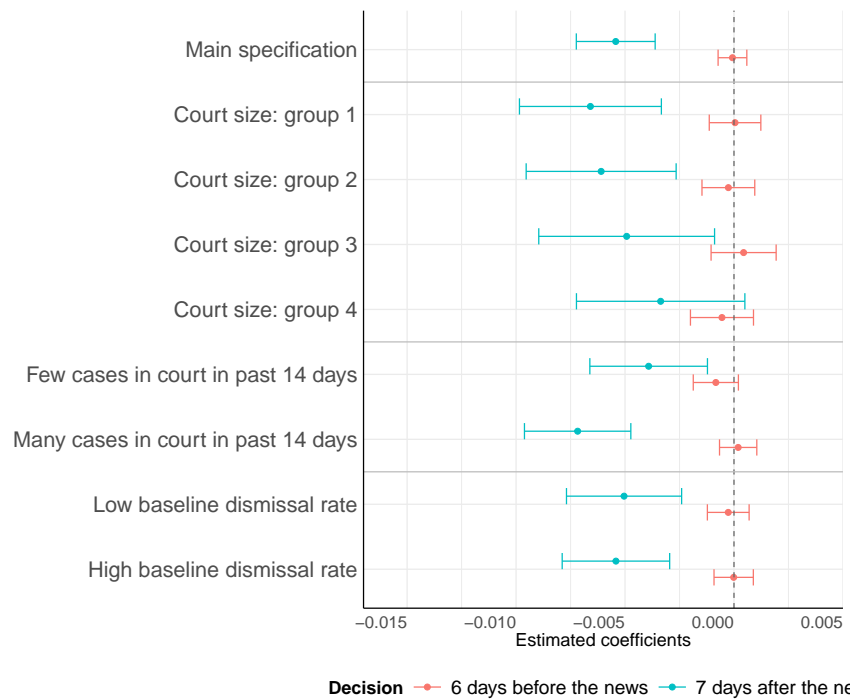
Figure 2.F.8: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by news content



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence by news content, with corresponding 95% confidence intervals. The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.9: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by court characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence by court characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.1: OLS results for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases

Model:	<i>Dependent Variable: Dismissal (0/1)</i>		
	SV/IPV (1)	IPV only (2)	SV only (3)
7 days after the news	-0.0055*** (0.0021)	-0.0052* (0.0028)	-0.0042 (0.0028)
6 days after the news	-0.0017 (0.0021)	-0.0032 (0.0029)	0.0004 (0.0029)
5 days after the news	-0.0080*** (0.0024)	-0.0091*** (0.0033)	-0.0057* (0.0032)
4 days after the news	-0.0055** (0.0028)	-0.0056 (0.0037)	-0.0053 (0.0038)
3 days after the news	-0.0051** (0.0025)	-0.0046 (0.0034)	-0.0043 (0.0035)
2 days after the news	-0.0109*** (0.0025)	-0.0147*** (0.0034)	-0.0038 (0.0034)
1 day after the news	-0.0051** (0.0024)	-0.0033 (0.0032)	-0.0075** (0.0033)
Same day as the news	-0.0040* (0.0022)	-0.0021 (0.0029)	-0.0070** (0.0031)
1 day before the news	0.0017 (0.0014)	0.0041** (0.0019)	-0.0021 (0.0020)
2 days before the news	-4.55×10^{-5} (0.0014)	0.0010 (0.0020)	-0.0004 (0.0020)
3 days before the news	-0.0020 (0.0014)	-0.0016 (0.0019)	-0.0038* (0.0020)
4 days before the news	-4.55×10^{-5} (0.0013)	0.0009 (0.0018)	0.0005 (0.0018)
5 days before the news	3.7×10^{-5} (0.0013)	0.0006 (0.0018)	-0.0010 (0.0018)
6 days before the news	0.0003 (0.0013)	0.0021 (0.0017)	-0.0028 (0.0018)
Year, month, day-of-the-week fixed effects	Yes	Yes	Yes
County of jurisdiction fixed effects	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes
Observations	687,198	430,096	262,684
R ²	0.18650	0.16281	0.21285
Within R ²	0.12851	0.08817	0.17953

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T . Case-level controls include: French-born author, author's age, number of victims, type of assault.

Reading: Two days after the TV news coverage, the propensity to dismiss sexual and intimate partner violence cases decreases by 0.51 percentage point (significant at the 5% level).

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.2: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Heterogeneity by case characteristics, period, news content, and court characteristics

Model:	<i>Dependent Variable: Dismissal (0/1)</i>										
	Main specification (1)	SV cases (2)	IPV cases (3)	Factual reason (4)	Legal reason (5)	One type offense (6)	Multiple offenses (7)	Quick decision (8)	Long decision (9)	Recent cases (10)	Old cases (11)
<i>Variables</i>											
# of news 7 days after decision	-0.0054*** (0.0009)	-0.0046*** (0.0013)	-0.0056*** (0.0013)	-0.0055*** (0.0010)	2.78×10^{-5} (0.0006)	-0.0038*** (0.0010)	-0.0113*** (0.0021)	-0.0062*** (0.0014)	-0.0026** (0.0010)	-0.0037*** (0.0013)	-0.0063*** (0.0013)
# of news 6 days before decision	-7.02×10^{-5} (0.0003)	-0.0015*** (0.0005)	0.0010** (0.0005)	0.0002 (0.0004)	-0.0002 (0.0002)	0.0006 (0.0004)	-0.0015** (0.0008)	-0.0005 (0.0005)	-5.26×10^{-5} (0.0004)	0.0018*** (0.0005)	-0.0014*** (0.0004)
<i>Fit statistics</i>											
Observations	687,198	262,684	430,096	687,198	687,198	537,656	149,542	343,507	341,994	341,060	342,437
R ²	0.18649	0.21282	0.16278	0.10978	0.03224	0.17422	0.21453	0.23116	0.09815	0.22508	0.14287
Within R ²	0.12849	0.17950	0.08814	0.06691	0.02403	0.11884	0.13985	0.15692	0.07876	0.14732	0.10973
<i>Fixed-effects</i>											
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroskedasticity-robust standard-errors in parentheses. Case-level controls: offender's age, nationality, number of victims, type of violence.

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

(Table continued on next page)

(Table Heterogeneity: continued)

Model:	<i>Dependent Variable: Dismissal (0/1)</i>										
	Adult victims (12)	Under 18 victims (13)	Adult authors (14)	Under 18 authors (15)	French-born author (16)	Before Oct 2017 (17)	Since Oct 2017 (18)	High audience (19)	Low audience (20)	Winter time (21)	Summer time (22)
<i>Variables</i>											
# of news 7 days after	-0.0064*** (0.0020)	-0.0033** (0.0017)	-0.0064*** (0.0011)	-0.0015 (0.0031)	-0.0058*** (0.0014)	-0.0015 (0.0012)	-0.0123*** (0.0016)	-0.0050*** (0.0014)	-0.0036*** (0.0010)	-0.0081*** (0.0016)	-0.0016 (0.0019)
# of news 6 days before	-0.0012* (0.0007)	-0.0018*** (0.0006)	0.0002 (0.0004)	-0.0020 (0.0012)	-0.0007 (0.0005)	3.95×10^{-5} (0.0006)	-0.0002 (0.0004)	0.0004 (0.0007)	-0.0002 (0.0005)	-0.0007 (0.0004)	-0.0024 (0.0016)
<i>Fit statistics</i>											
Observations	113,440	149,244	551,256	54,268	391,010	486,555	200,643	740,954	697,233	291,435	168,198
R ²	0.20414	0.23537	0.15616	0.16138	0.10560	0.18087	0.21095	0.18768	0.18698	0.17977	0.19235
Within R ²	0.15495	0.21003	0.09128	0.12617	0.04473	0.12406	0.14710	0.12898	0.12877	0.12271	0.13161
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroskedasticity-robust standard-errors in parentheses. Case-level controls: offender's age, nationality, number of victims, type of violence.

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

(Table continued on next page)

(Table Heterogeneity: continued)

Model:	Dependent Variable: Dismissal (0/1)										
	Program's 1st news (23)	News w/ testimonies (24)	News w/ prior reporting (25)	Sensational affairs (26)	Political figures (27)	Few cases (14d) (28)	Many cases (14d) (29)	Q1 court dismissal rate (30)	Q2 court dismissal rate (31)	Q3 court dismissal rate (32)	Q4 court dismissal rate (33)
<i>Variables</i>											
# of news 7 days after	-0.0030*** (0.0011)	-0.0053*** (0.0017)	-0.0041** (0.0019)	-0.0019 (0.0015)	-0.0006 (0.0017)	-0.0039*** (0.0014)	-0.0072*** (0.0012)	-0.0079*** (0.0026)	-0.0026 (0.0021)	-0.0012 (0.0017)	-0.0089*** (0.0015)
# of news 6 days before	0.0005 (0.0005)	-0.0014 (0.0012)	-0.0029* (0.0016)	-0.0008 (0.0008)	0.0013 (0.0009)	-0.0008 (0.0005)	0.0002 (0.0004)	-0.0004 (0.0010)	-6.97×10^{-5} (0.0007)	0.0005 (0.0006)	-0.0005 (0.0006)
<i>Fit statistics</i>											
Observations	732,145	742,177	750,636	738,948	739,202	327,959	359,239	97,235	141,280	197,835	250,848
R ²	0.18753	0.18761	0.18722	0.18743	0.18765	0.18420	0.19423	0.22345	0.20365	0.19679	0.15125
Within R ²	0.12924	0.12902	0.12892	0.12874	0.12919	0.13582	0.12384	0.17237	0.15249	0.13721	0.09991
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroskedasticity-robust standard-errors in parentheses. Case-level controls: offender's age, nationality, number of victims, type of violence.

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

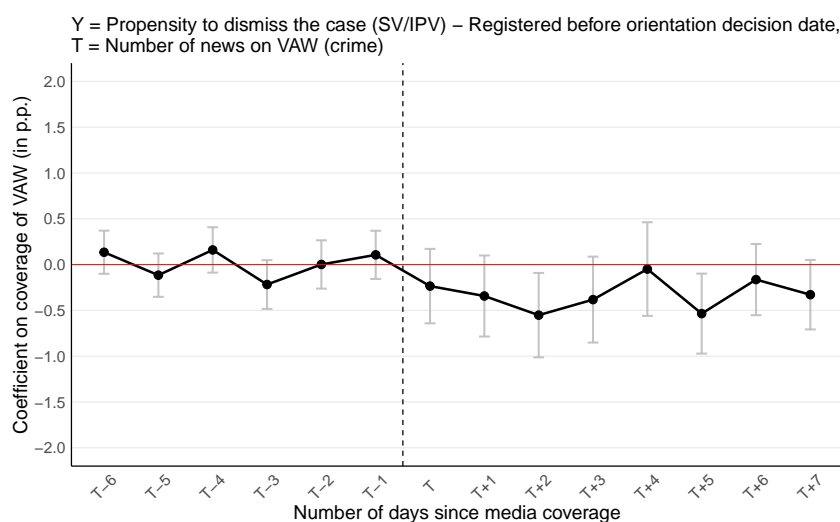
Notes: This table presents heterogeneity analyses of the effect of TV news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases. The sample consists of cases processed in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured as the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2.

Reading: Each column corresponds to a different sub-sample or specific outcome margin: (1) Main specification; (2)–(3) Cases of sexual violence (2) and intimate partner violence (3); (4)–(5) Dismissals motivated by factual (4) or legal (5) reasons; (6)–(7) Cases involving a single type of offense (6) or multiple offenses (7); (8)–(9) Decisions made quickly (below median) (8) or after a longer delay (9); (10)–(11) Decisions regarding recent (10) or older (11) cases; (12)–(13) Cases with adult victims (12) or victims under 18 (13); (14)–(15) Cases with adult authors (14) or authors under 18 (15); (16) Cases with French-born authors; (17)–(18) Decisions before (17) or after (18) the October 2017 #MeToo normative shift; (19)–(20) Periods of high (19) or low (20) TV news audience; (21)–(22) Decisions during winter (21) or summer (22); (23) News stories appearing as the program's first news; (24)–(27) News content featuring victim testimonies (24), prior judicial involvement (25), sensational affairs (26), or political/media figures (27); (28)–(29) Courts with few (28) or many (29) cases in the past 14 days; (30)–(33) Courts grouped by their average baseline dismissal rate, from the lowest (Q1) to the highest (Q4) quartile.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.F.4 Robustness checks

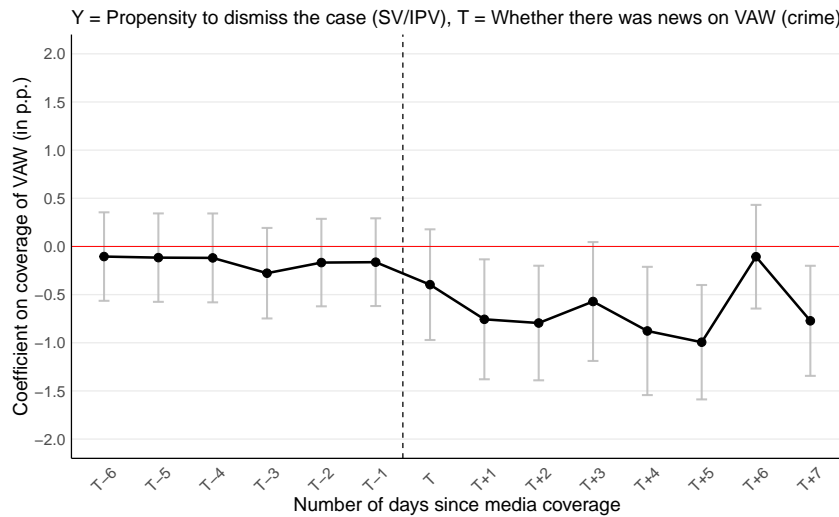
Figure 2.F.10: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, for cases registered before the prosecutors' decision (87% of cases)



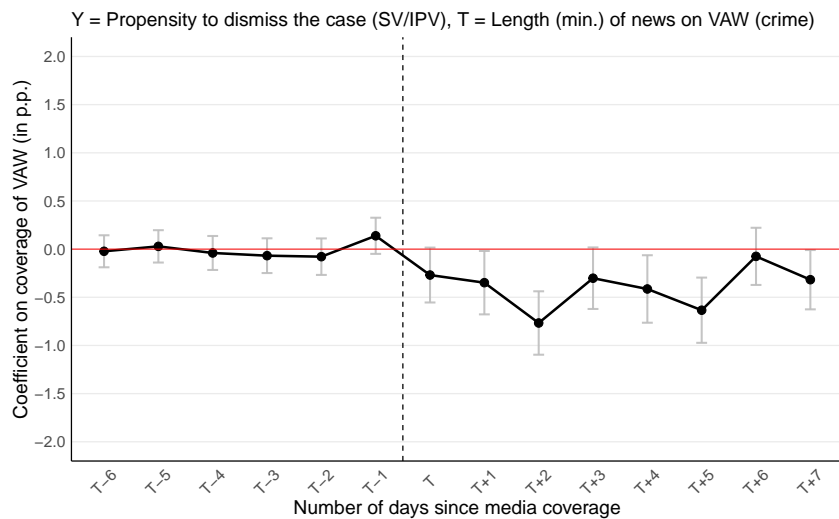
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019, restricting to cases registered before the news coverage. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.11: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Using other measures of news coverage



(a) Using occurrence of news stories (0/1)

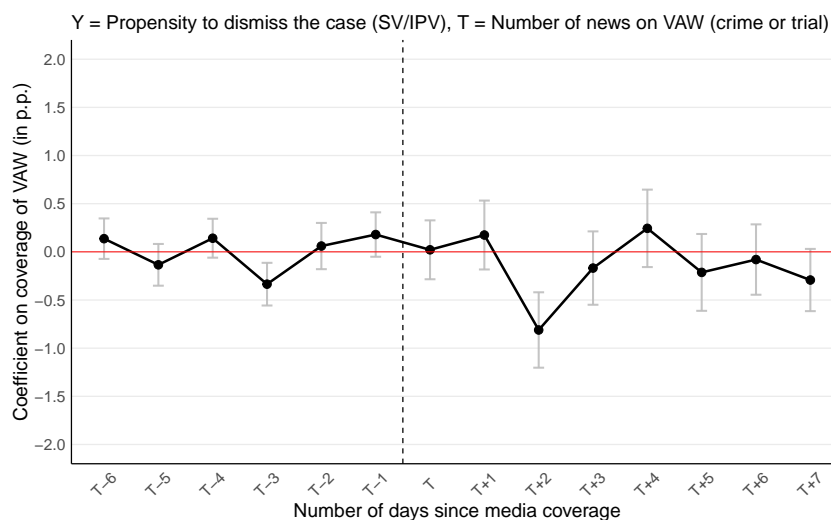


(b) Using duration of news stories (minutes)

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence using alternative measures of news coverage, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. In the upper panel, news coverage is measured with a dummy indicating whether there was any news on crimes against women broadcast during the 8pm national TV news on TF1 and France 2. In the lower panel, news coverage is measured with the length of stories (in minutes) on crimes against women broadcast during the 8pm national TV news on TF1 and France 2. The TV news coverage occurs at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

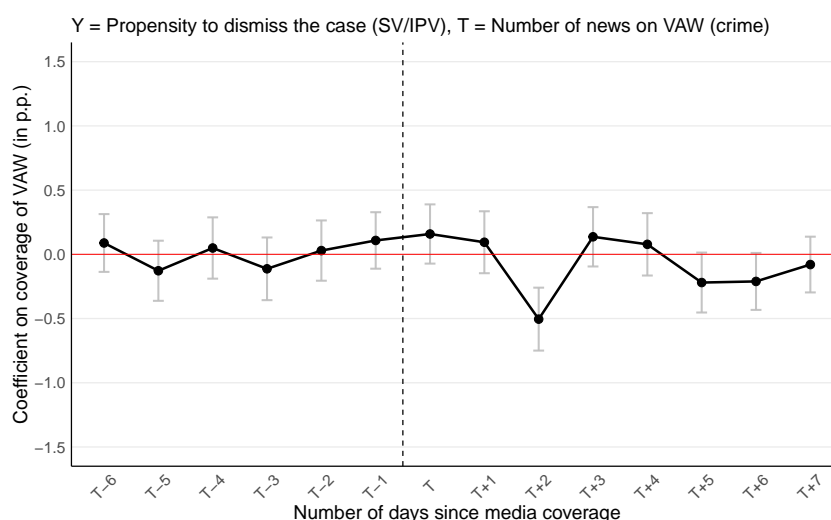
Figure 2.F.12: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, including news on trials



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women or related trials on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women or related trials broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

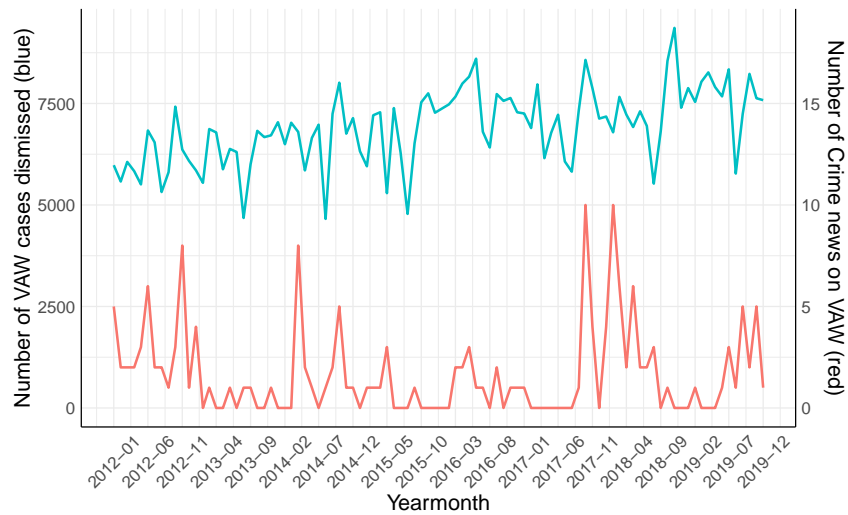
Figure 2.F.13: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, including periods with multiple days of VAW news coverage



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, restricting to periods with one unique news day in the seven days before the decision, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

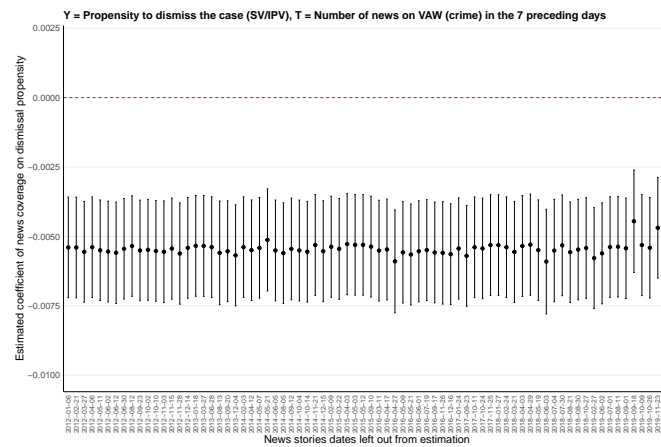
Figure 2.F.14: Time evolution of the number of news on crimes against women and the number of dismissals for sexual and intimate partner violence cases



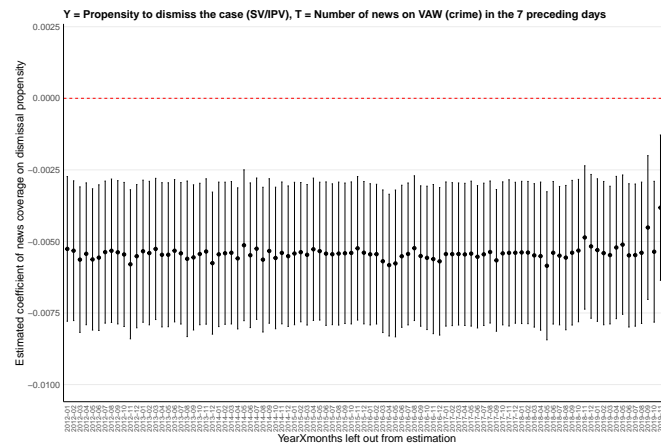
Notes: Time evolution of the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, as well as the absolute number of dismissal decisions regarding cases of sexual violence (SV) and intimate partner violence (IPV) by month of the year.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

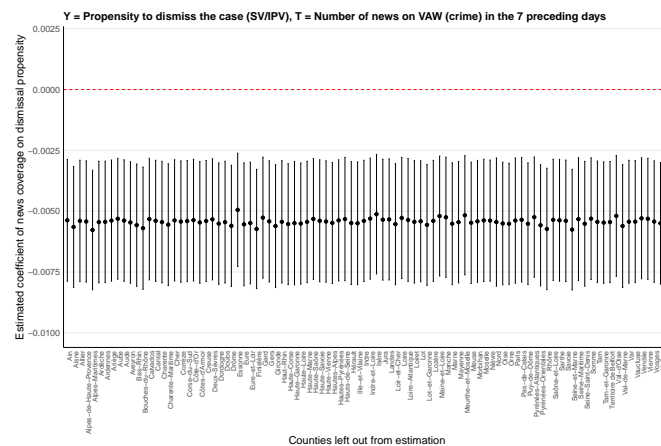
Figure 2.F.15: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, leaving out each news event day, month, and county



(a) Leaving out each news event day



(b) Leaving out each month

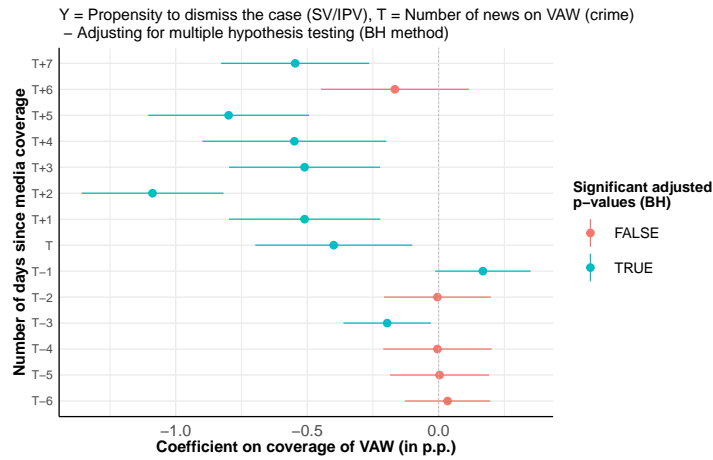


(c) Leaving out each county

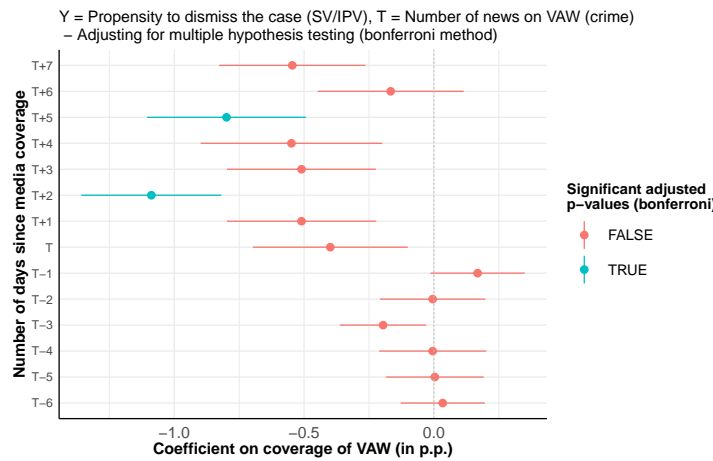
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points), performing leave-one-out analyses. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. The upper panel replicates the main analysis while sequentially removing each of the 76 isolated days with news coverage. The middle panel sequentially removes each of the 96 months from the 2012–2019 analysis period, and the bottom panel sequentially removes each of the 96 French metropolitan counties.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

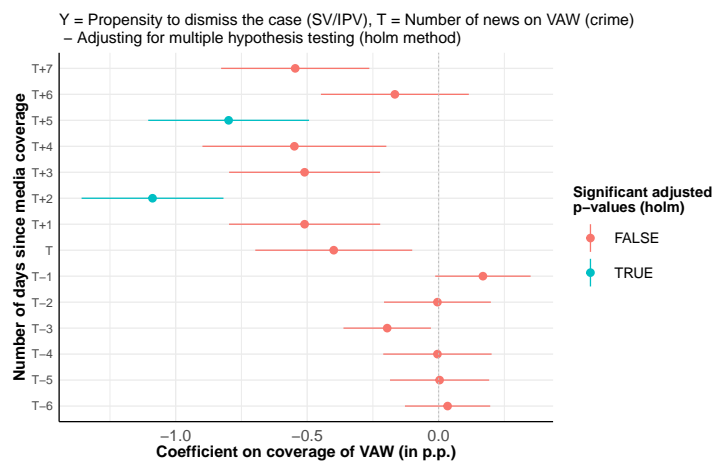
Figure 2.F.16: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, adjusting for multiple hypothesis testing



(a) BH method



(b) Bonferroni method



(c) Holm method

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to dismiss such violence, with corresponding 95% confidence intervals (in percentage points), correcting for multiple hypothesis testing with three different methods. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.3: Robustness checks for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases

Model:	Dependent Variable: Dismissal (0/1)											
	Main specification	Probit model	Residualization	Interacting Year*Month	Interacting Year*Quarter	Interacting Year*Weekday	Interacting Year*Day	Court fixed effects	Linear time trend	Controlling for vacations	Controlling for bank holidays	Controlling for periods when women more visible
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Variables</i>												
# of news 7 days before	-0.0054*** (0.0009)	-0.022 (0.004)	-0.0058*** (0.0009)	-0.0013 (0.0011)	-0.0026*** (0.0010)	-0.0057*** (0.0010)	-0.0085*** (0.0010)	-0.0054*** (0.0009)	-0.0055*** (0.0009)	-0.0053*** (0.0009)	-0.0057*** (0.0009)	-0.0054*** (0.0009)
# of news 6 days after	-7.02×10^{-5} (0.0003)	0.000 (0.001)	-0.0002 (0.0003)	0.0013*** (0.0004)	0.0009** (0.0004)	-0.0005 (0.0003)	-0.0007* (0.0004)	-4.29×10^{-5} (0.0003)	-0.0001 (0.0003)	-7.69×10^{-5} (0.0003)	-7.62×10^{-5} (0.0003)	-6.44×10^{-5} (0.0003)
<i>Fit statistics</i>												
Observations	687,198	687,198	687,198	687,198	687,198	687,198	687,198	687,198	687,198	687,198	687,198	687,198
R ²	0.18649		0.13301	0.18734	0.18696	0.18665	0.14701	0.18851	0.18650	0.18650	0.18906	0.18649
Within R ²	0.12849		0.12707	0.12837	0.12838	0.12850	0.13672	0.12779	0.12850	0.12850	0.13125	0.12849
Log.Lik.		-298797.508										
RMSE		0.38										
<i>Fixed-effects</i>												
Year, month, dow fixed effects	✓	✓	✓					✓	✓	✓	✓	✓
I(year * month), dow fixed effects				✓								
I(year * quarter), dow fixed effects					✓							
I(year * weekday), dow fixed effects						✓						
I(year * day), dow fixed effects							✓					
Year, week, dow fixed effects					✓							
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓
Court fixed effects								✓				
Time trend <i>t</i>									✓			

Heteroskedasticity-robust standard-errors in parentheses (unless otherwise specified). Case-level controls: offender's age, nationality, number of victims, type of violence.
 Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

(Table D2: To be continued on next page)

(Table D2: continued)

Model:	Dependent Variable: Dismissal (0/1)											
	Controlling for VAW-related laws	Removing weekends	Grouping weekends	Errors clustered per day	Errors clustered per county	Errors clustered per Year*Month	Errors clustered per affair	Errors clustered per court	Restricting to known authors	Removing duplicated authors	Excluding alternatives to prosecution	Excluding rape cases
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
<i>Variables</i>												
# of news 7 days before	-0.0054*** (0.0009)	-0.0050*** (0.0009)	-0.0036*** (0.0007)	-0.0054*** (0.0014)	-0.0054*** (0.0013)	-0.0054*** (0.0020)	-0.0054*** (0.0010)	-0.0054*** (0.0013)	-0.0061*** (0.0010)	-0.0054*** (0.0009)	-0.0065*** (0.0010)	-0.0052*** (0.0010)
# of news 6 days after	-6.45×10^{-5} (0.0003)	-9.13×10^{-5} (0.0003)	0.0002 (0.0003)	-7.02×10^{-5} (0.0005)	-7.02×10^{-5} (0.0005)	-7.02×10^{-5} (0.0006)	-7.02×10^{-5} (0.0004)	-7.02×10^{-5} (0.0005)	-5.57×10^{-5} (0.0004)	-5.5×10^{-5} (0.0003)	-0.0014*** (0.0004)	0.0002 (0.0004)
<i>Fit statistics</i>												
Observations	687,198	659,022	636,291	687,198	687,198	687,198	687,198	687,198	605,524	686,414	687,198	598,930
R ²	0.18649	0.13523	0.13494	0.18649	0.18649	0.18649	0.18649	0.18649	0.15522	0.18631	0.25635	0.17938
Within R ²	0.12849	0.12735	0.12709	0.12849	0.12849	0.12849	0.12849	0.12849	0.09470	0.12811	0.23039	0.11787
<i>Fixed-effects</i>												
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
<i>Clusters</i>												
Errors clustered by day				✓								
Errors clustered by county					✓							
Errors clustered by year * month						✓						
Errors clustered by single affair							✓					
Errors clustered by court								✓				

Heteroskedasticity-robust standard-errors in parentheses (unless otherwise specified). Case-level controls: offender's age, nationality, number of victims, type of violence.

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

(Table D2: To be continued on next page)

(Table D2: continued)

Model:	Dependent Variable: Dismissal (0/1)											
	Restricting to cases registered before the decision (25)	Using occurrence of news stories (26)	Using length (min.) of news stories (27)	Including news on trials (28)	Removing news on DSK (29)	Using only the TF1 news stories (30)	Using only the France 2 news stories (31)	Using news in the same county (32)	Using news in an adjacent county (33)	Using news in neither same nor adjacent county (34)	Including periods with multiple VAW news days (35)	Removing periods with zero news (36)
<i>Variables</i>												
# of news 7 days before	-0.0032*** (0.0009)	-0.0064*** (0.0012)	-0.0037*** (0.0006)	-0.0013* (0.0008)	-0.0057*** (0.0010)	-0.0042*** (0.0014)	-0.0065*** (0.0012)	-0.0041*** (0.0011)	-0.0042*** (0.0011)	-0.0037*** (0.0010)	-0.0007*** (0.0002)	-0.0026** (0.0012)
# of news 6 days after	7.64×10^{-5} (0.0003)	-0.0016** (0.0007)	-0.0001 (0.0006)	-5.94×10^{-5} (0.0003)	-0.0003 (0.0004)	-0.0005 (0.0007)	0.0005 (0.0005)	0.0001 (0.0004)	9.07×10^{-5} (0.0004)	1.04×10^{-5} (0.0004)	0.0002 (0.0003)	0.0008** (0.0004)
<i>Fit statistics</i>												
Observations	592,378	687,198	687,198	640,220	558,827	710,650	713,391	681,513	682,143	694,049	758,305	236,566
R ²	0.13839	0.18649	0.18649	0.18595	0.18622	0.18688	0.18680	0.18655	0.18659	0.18688	0.18767	0.19311
Within R ²	0.09276	0.12849	0.12849	0.12773	0.12738	0.12880	0.12882	0.12832	0.12836	0.12852	0.12922	0.13431
<i>Fixed-effects</i>												
Year, month, dow fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County of jurisdiction fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Heteroskedasticity-robust standard-errors in parentheses (unless otherwise specified). Case-level controls: offender's age, nationality, number of victims, type of violence.

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Robustness checks of the effect of TV news coverage of crimes against women on the propensity to dismiss such violence. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.

Reading: Each column corresponds to a different robustness check or model specification: (1) Main specification; (2) Probit model; (3) Residualization of the outcome variable to remove time and weekday trends; (4)–(7) Interactions of fixed effects with year and time indicators: year*month (4), year*quarter (5), year*weekday (6), year*day (7); (8) Adds court fixed effects; (9) Includes a linear time trend; (10)–(11) Control for school vacations (10) and bank holidays (11); (12) Controls for periods of increased female visibility (e.g., Women's Day, #MeToo); (13) Controls for legislative changes on VAW; (14) Excludes weekend decisions; (15) Aggregates weekend decisions; (16)–(20) Vary the level at which standard errors are clustered: by day (16), county (17), year*month (18), case/affair (19), and court (20); (21) Restricts to cases involving known authors; (22) Removes duplicated authors within the same case; (23) Excludes alternatives to prosecution from the dismissal outcome; (24) Excludes rape cases; (25) Restricts to cases registered before the decision; (26) Measures treatment as the occurrence (rather than number) of news stories; (27) Measures treatment using the duration (minutes) of news stories; (28) Includes news on VAW-related trials; (29) Excludes coverage of the DSK case; (30) Restricts the treatment to TF1 news only; (31) Restricts the treatment to France 2 news only; (32) Restricts treatment to news in the same county; (33) Restricts treatment to news in adjacent counties; (34) Restricts treatment to news in neither the same nor adjacent counties; (35) Includes periods with multiple days of news coverage in the 7-day window; (36) Excludes periods with zero news coverage.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.F.5 Extrapolation to other crimes

Keywords used for other crime types classification. Below is the list of words used to define the measures of media coverage for other crime categories used in the extrapolation analysis (Section 2.6.2). To ensure these stories are distinct from the main treatment, news stories mentioning VAW keywords were systematically excluded. Furthermore, the selection was restricted to "miscellaneous news items" (*faits divers*), excluding stories focused on trials or judicial procedures.

1. **Drug Trafficking:** drug trafficking, trafficker, dismantling of a network, drug network, dealer, deal point, drug sales point, go-fast, narcotics trafficking, clandestine laboratory, drug pipeline, cocaine seizure, cannabis seizure, heroin seizure.
2. **Organized Crime:** criminal conspiracy, organized gang, criminal organization, major organized crime, crime syndicate, mafia, underworld, gang, cartel.
3. **Theft and Receiving Stolen Goods:**
 - **Organized Robbery:** organized theft, armed robbery, heist, robber, hold-up, jewelry store heist, bank heist, armored truck attack, homejacking.
 - **Aggravated Receiving:** receiving stolen goods, aggravated receiving, fence, harboring criminals, stolen merchandise, stolen jewelry.
4. **Serious Interpersonal Violence and Homicides:**
 - **Homicide:** murder, voluntary homicide, assassination, killer, criminal, killed, stabbed, knife wound, gunshot, shot dead, executed, found body/corpse, killing, fatal brawl/fight, fatal assault, fatal violence, neighborhood drama, dispute resulting in death.
 - **Fatal Assault:** fatal blows, beaten to death, lynching, fatal drubbing, fatal assault and battery.
 - **Serious Physical Violence:** seriously injured, critical condition, total incapacity for work (ITT), hospitalized following assault, vital prognosis, severely beaten, assault with a knife/white weapon, violence and battery.
 - **Violent Extortion:** racket, extortion, threat for money, threat of violence, threat with a weapon, intimidation, under threat, constraint through violence.
5. **Incest and Pedophilia (Young Children):** incest, pedophilia, pedocriminal, rape of a minor, sexual assault of a minor, sexual abuse of a child, sexual assault of a young girl/boy, rape of a young girl.

Note: All keywords were originally used in French with their respective morphological variations (e.g., plurals, feminine forms, and specific conjugations). The list above provides the closest English equivalents used for identifying the news content in the INA database.

Table 2.F.4: Legal identification and sample shares of other crime types in the justice data

Crime Category	Legal Definition (NATAFF labels)	Count	Share of total cases (%)
Drug Trafficking	Drug trafficking (excluding possession, transport, or simple supply)	122,561	0.55
Organized Crime	Criminal conspiracy and harboring of criminals	14,346	0.06
Criminal Robbery and Heists	Armed robbery and organized group theft	61,914	0.28
Receiving Stolen Goods	Theft-related receiving, non-theft receiving, and aggravated receiving	350,612	1.58
Homicides	Voluntary homicide and fatal intentional violence resulting in death	9,827	0.04
Serious Physical Violence	Intentional violence resulting in total incapacity for work (ITT) exceeding 8 days	132,684	0.6
Sexual Violence (Minors)	Rape of a minor, sexual assault of a minor, corruption of a minor, and child pornography	197,356	0.89

Notes: Legal definitions (NATAFF codes) used and relative weight of each category within the total universe of cases processed by the French justice system during the sample period (2012-2019). The shares represent the proportion of each category relative to the total number of criminal cases registered in the national justice database during the study period ($N = 22,232,441$).

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice.

Table 2.F.5: Attrition Analysis: Comparison between Full and Restricted Samples

Crime Category	Observations		News Events (Days)	
	Dropped	Kept	Dropped	Kept
Drug Trafficking	13%	87%	51%	49%
Organized Crime	15%	85%	58%	42%
Theft & Receiving Stolen Goods	21%	79%	59%	41%
Serious Violence & Homicides	69%	31%	91%	9%
Sexual Abuse on Children	4%	96%	33%	67%

Notes: This table shows the percentage of observations and news days lost when restricting the sample to isolated news days (i.e., days with only one news event in the preceding seven days). High-frequency categories, such as serious violence and homicides, experience the most significant attrition.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.6: Effect of news coverage of other crimes on propensity to dismiss sexual and intimate partner violence

<i>Dependent Variable:</i> Sub-sample	Dismissal (0/1)			
	Drug Trafficking	Organized Crime	Theft and Receiving Stolen Goods	Serious Interpersonal Violence and Homicides
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Number of news on crimes against women in the 7 days before the decision	0.0005 (0.0008)	-0.0004 (0.0006)	-0.0005 (0.0006)	-0.0011 (0.0011)
Number of news on crimes against women in the 6 days after the decision	0.0001 (0.0005)	-0.0003 (0.0005)	0.0007*** (0.0003)	-0.0014*** (0.0004)
<i>Fixed-effects</i>				
Year, month, day-of-week FE	Yes	Yes	Yes	Yes
County of jurisdiction FE	Yes	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	650,030	605,278	602,790	238,651
R ²	0.19265	0.19169	0.19251	0.18119
Within R ²	0.13418	0.13210	0.13345	0.12422

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of other crimes on prosecution decision for sexual and intimate partner violence cases, pooling the leads and lags together into two event-time dummies. The sample consists of cases of sexual or intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the total number of news stories on categories of crimes unrelated to violence against women, namely (1) drug trafficking, (2) organized crime, (3) theft and receiving stolen goods, (4) serious interpersonal violence and homicides broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision. Case-level controls include: French-born author, author's age, number of victims, type of assault.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.7: Effect of media coverage of other crimes on propensity to dismiss such other crimes: restricted sample

<i>Dependent Variable:</i>	Dismissal (0/1)									
	Drug Trafficking		Organized Crime		Theft & Receiving		Violence & Homicides		Sexual Abuse on Children	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of news (Lags) <i>(7 days before decision)</i>	-0.0020 (0.0020)	-0.0020 (0.0034)	0.0154*** (0.0047)	0.0154* (0.0090)	-0.0008 (0.0009)	-0.0008 (0.0142)	-0.0056** (0.0026)	-0.0056 (0.0035)	-0.0033* (0.0020)	-0.0033 (0.0024)
Number of news (Leads) <i>(7 days after decision)</i>	-0.0003 (0.0012)	-0.0003 (0.0017)	-0.0011 (0.0036)	-0.0011 (0.0068)	0.0150*** (0.0004)	0.0150** (0.0006)	-0.0015 (0.0009)	-0.0015 (0.0012)	0.0011 (0.0012)	0.0011 (0.0014)
<i>Fixed-effects & Controls</i>										
Year, month, day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County of jurisdiction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Clustering</i>										
Standard error:	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.
<i>Fit statistics</i>										
Observations	105,879	105,879	15,414	15,414	313,564	313,564	42,656	42,656	193,039	193,039
R ²	0.34843	0.34843	0.36076	0.36076	0.22622	0.22622	0.32344	0.32344	0.22875	0.22875
Within R ²	0.28854	0.28854	0.24811	0.24811	0.17561	0.17561	0.28548	0.28548	0.20573	0.20573

Standard errors in parentheses. "Rob." indicates robust SE; "Clust." indicates SE clustered at the date level

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Notes: OLS estimates of leads and lags from Equation 2.1. The coefficients show the effect of TV news coverage of specific crime categories on the propensity to dismiss cases of the same nature, pooling leads and lags into two event-time dummies. The sample includes cases tried in correctional or juvenile courts between 2012 and 2019 across five categories: (1) drug trafficking, (2) organized crime, (3) theft and receiving stolen goods, (4) serious interpersonal violence and homicides, and (5) sexual abuse on children. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured as the total number of news stories related to the corresponding crime category broadcast during the 8pm national news (TF1 and France 2). To ensure comparability with the main analysis, results are presented for the "restricted sample", which isolates short-run shocks by selecting periods with a single news day in the preceding week. For each specification, standard errors are reported using two methods: heteroskedasticity-robust standard errors (Robust) and standard errors clustered at the daily level (Clustered) to account for potential correlation across prosecutorial decisions made on the same day. Case-level controls include: author's French-born status, author's age, number of victims, and the specific nature of the offense.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.8: Effect of media coverage of other crimes on propensity to dismiss such other crimes: full sample

<i>Dependent Variable:</i>	Dismissal (0/1)									
	Drug Trafficking		Organized Crime		Theft & Receiving		Violence & Homicides		Sexual Abuse on Children	
Sub-sample:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of news (Lags) <i>(7 days before decision)</i>	-0.0019** (0.0008)	-0.0019 (0.0015)	0.0013 (0.0026)	0.0013 (0.0050)	-0.0004 (0.0003)	-0.0004 (0.0004)	-0.0004 (0.0003)	-0.0004 (0.0004)	0.0011 (0.0010)	0.0011 (0.0012)
Number of news (Leads) <i>(7 days after decision)</i>	-0.0006 (0.0010)	-0.0006 (0.0014)	-0.0003 (0.0030)	-0.0003 (0.0058)	0.0005 (0.0003)	0.0005 (0.0005)	0.0001 (0.0003)	0.0001 (0.0004)	0.0005 (0.0011)	0.0005 (0.0014)
<i>Fixed-effects & Controls</i>										
Year, month, day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County of jurisdiction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Clustering</i>										
Standard error:	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.	Rob.	Clust.
<i>Fit statistics</i>										
Observations	121,045	121,045	18,078	18,078	395,832	395,832	137,195	137,195	201,718	201,718
R ²	0.34676	0.34676	0.35876	0.35876	0.23035	0.23035	0.32875	0.32875	0.22845	0.22845
Within R ²	0.28780	0.28780	0.25230	0.25230	0.18001	0.18001	0.29196	0.29196	0.20528	0.20528

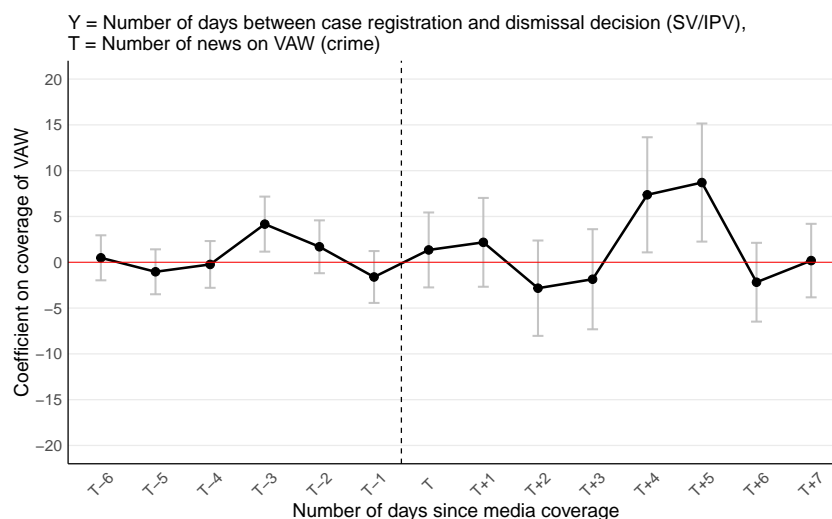
Standard errors in parentheses. "Rob." indicates robust SE; "Clust." indicates SE clustered at the date level
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1. The coefficients show the effect of TV news coverage of specific crime categories on the propensity to dismiss cases of the same nature, pooling leads and lags into two event-time dummies. The sample includes cases tried in correctional or juvenile courts between 2012 and 2019 across five categories: (1) drug trafficking, (2) organized crime, (3) theft and receiving stolen goods, (4) serious interpersonal violence and homicides, and (5) sexual abuse on children. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured as the total number of news stories related to the corresponding crime category broadcast during the 8pm national news (TF1 and France 2). Due to the high frequency of news for certain offenses, restricting the analysis to isolated news days significantly reduces the sample size. Consequently, results for the full sample are also provided in this table to ensure comprehensiveness. For each specification, standard errors are reported using two methods: heteroskedasticity-robust standard errors (Robust) and standard errors clustered at the daily level (Clustered) to account for potential correlation across prosecutorial decisions made on the same day. Case-level controls include: author's French-born status, author's age, number of victims, and the specific nature of the offense.

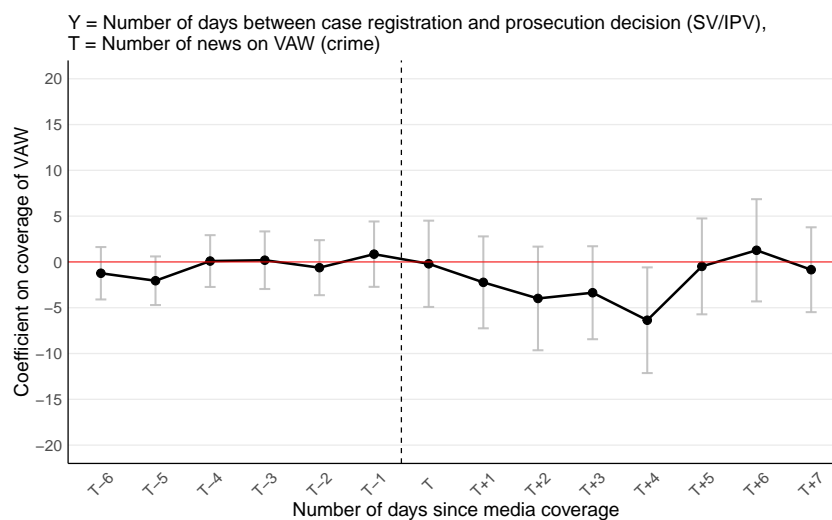
Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.F.6 Additional results on the decision to prosecute and case processing time

Figure 2.F.17: Effect of news coverage of crimes against women on the number of days elapsed between justice arrival and prosecutor’s decision – Dismissal vs. Prosecution



(a) Time until dismissal

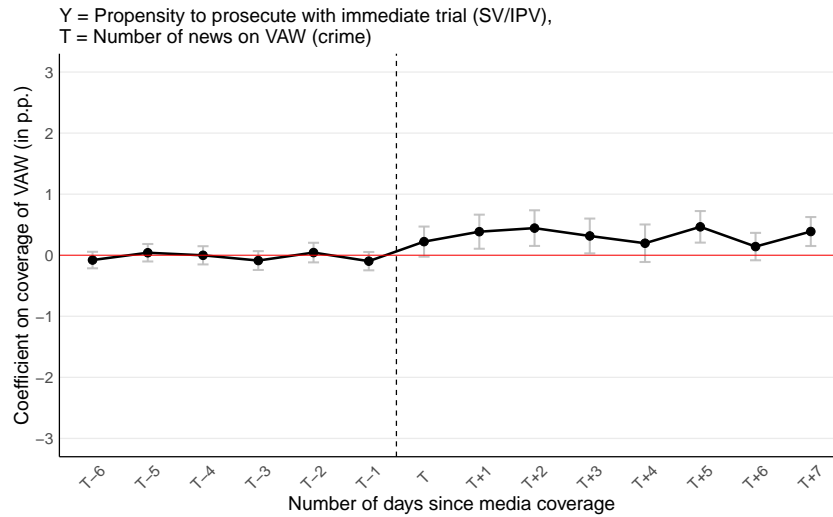


(b) Time until prosecution

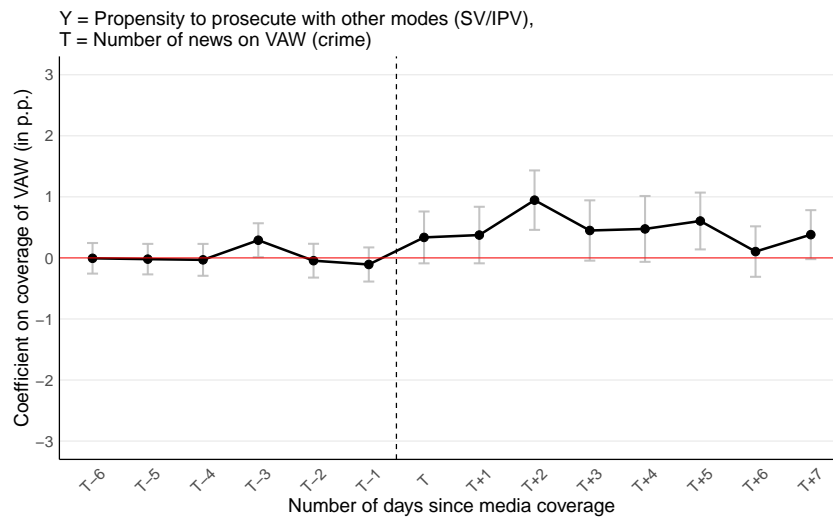
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the time elapsed between the justice arrival and prosecution decision, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is the number of days elapsed between the date of the case arrival to justice and the decision to dismiss or prosecute the case. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T . In the upper panel (time to dismiss), the sample is restricted to cases which were dismissed. In the lower panel (time to prosecute), the sample is restricted to cases which were prosecuted.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.F.18: Effect of news coverage of crimes against women on the propensity to prosecute sexual and intimate partner violence cases, by prosecution mode



(a) Prosecution with immediate trial



(b) Prosecution with other modes

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to prosecute such violence by mode of prosecution, with corresponding 95% confidence intervals (in percentage points). The sample consists of cases of sexual and intimate partner violence that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. The dependent variable is a dummy equal to one if the case was dismissed by prosecutors. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T . In the upper panel (immediate trial), the dependent variable is a dummy variable equal to one if the case was prosecuted with immediate trial and zero otherwise. In the lower panel (other modes), the dependent variable is a dummy variable equal to one if the case was prosecuted with any other mode (penal orders, summons, investigation) and zero otherwise.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.9: Effect of news coverage of crimes against women on the number of decisions taken by prosecutors

<i>Dependent Variables:</i>	Number of decisions made (logs)		
	Any decision	Dismissal	Prosecution
Model:	(1)	(2)	(3)
Number of news on crimes against women in the 7 days before the decision	-0.0081 (0.0189)	-0.0183 (0.0311)	0.0277** (0.0110)
Number of news on crimes against women in the 6 days after the decision	0.0015 (0.0032)	0.0003 (0.0069)	0.0015 (0.0031)
<i>Fixed-effects</i>			
Year, month, dow fixed effects	✓	✓	✓
<i>Fit statistics</i>			
Observations	2,647	2,644	2,647
R ²	0.86295	0.85240	0.78962

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around prosecution on the number of decisions taken by prosecutors, pooling the leads and lags together into two event-time dummies. The sample includes all cases of sexual and intimate partner violence that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. In Column (1), the dependent variable corresponds to the log number of any decision taken. In Column (2), it corresponds to the log number of dismissal decisions, and in Column (3) to prosecution decisions. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the prosecution decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.10: Effect of news coverage of crimes against women on prosecutor's case processing time

<i>Dependent Variables:</i>	Time between registration and decision		
	Any decision (1)	Dismissal (2)	Prosecution (3)
Model:			
Number of news on crimes against women in the 7 days before the decision	-0.0348 (0.7961)	1.239 (0.9873)	-1.618 (1.065)
Number of news on crimes against women in the 6 days after the decision	0.3755 (0.2761)	0.5886* (0.3407)	-0.5520 (0.3600)
<i>Fixed-effects</i>			
Year, month, dow fixed effects	✓	✓	✓
<i>Fit statistics</i>			
Observations	685,501	517,517	167,984
R ²	0.06765	0.05274	0.08855

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around prosecution on the time elapsed between the case arrival to justice and the prosecutorial decision, pooling the leads and lags together into two event-time dummies. The sample includes all cases of sexual and intimate partner violence that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. In Column (1), the dependent variable corresponds to the time until any decision taken by prosecutors. In Column (2), it corresponds to the time until dismissal, and in Column (3) to the time until prosecution. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the prosecution decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.11: Effect of news coverage of crimes against women on decisions taken by prosecutors for other crimes

<i>Dependent variable:</i>	Number of decisions made (logs)		Dismissal decision (0/1)		Time to decide	
	Other harm to persons	Other intentional bodily harm	Other harm to persons	Other intentional bodily harm	Other harm to persons	Other intentional bodily harm
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Number of news on crimes against women in the 7 days before the decision	0.0026 (0.0172)	0.0069 (0.0195)	-0.0003 (0.0003)	0.0003 (0.0006)	-0.3379 (0.3247)	-0.4183 (1.170)
Number of news on crimes against women in the 6 days after the decision	0.0029 (0.0029)	0.0016 (0.0032)	-4.92×10^{-5} (0.0001)	-4.88×10^{-5} (0.0002)	0.1814 (0.1151)	0.6880 (0.4196)
<i>Fixed effects</i>						
Year, month, day-of-the-week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County of jurisdiction fixed effects	No	No	Yes	Yes	Yes	Yes
Case-level controls	No	No	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	2,647	2,647	3,476,088	1,382,374	3,470,543	1,378,896
R^2	0.85826	0.86799	0.24654	0.27406	0.06729	0.06399
Within R^2	0.00017	0.00013	0.20521	0.21840	0.03233	0.02586

Heteroskedasticity-robust standard errors in parentheses.

Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around prosecution on the log number of decisions, dismissal rate, and time to decide for other harms to persons, pooling the leads and lags together into two event-time dummies. The sample includes all cases of other harms to persons or other intentional bodily harm that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the prosecution decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.F.12: Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases by number of news stories broadcast

<i>Dependent Variable:</i>	Dismissal (0/1)			
	Only one (1)	Two (2)	Three (3)	Four (4)
Max. Number of news in past 7 days:				
<i>Variables</i>				
Any news on crimes against women in the 7 days before the decision	-0.0054*** (0.0013)	-0.0062*** (0.0012)	-0.0064*** (0.0012)	-0.0064*** (0.0012)
Any news on crimes against women in the 6 days after the decision	-0.0016** (0.0007)	-0.0014** (0.0007)	-0.0016** (0.0007)	-0.0016** (0.0007)
Year, month, day-of-the-week fixed effects	Yes	Yes	Yes	Yes
County of jurisdiction fixed effects	Yes	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	668,792	683,452	686,869	687,198
R ²	0.18584	0.18612	0.18650	0.18649
Within R ²	0.12813	0.12838	0.12848	0.12849

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around prosecution on the log number of decisions, dismissal rate, and time to decide for other harms to persons, pooling the leads and lags together into two event-time dummies. The sample includes all cases of other harms to persons or other intentional bodily harm that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the prosecution decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopee* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Other potential mechanisms. Beyond the primary channels discussed in the main text, the empirical evidence also allows me to rule out alternative explanations such as slow-moving social trends or rational learning.

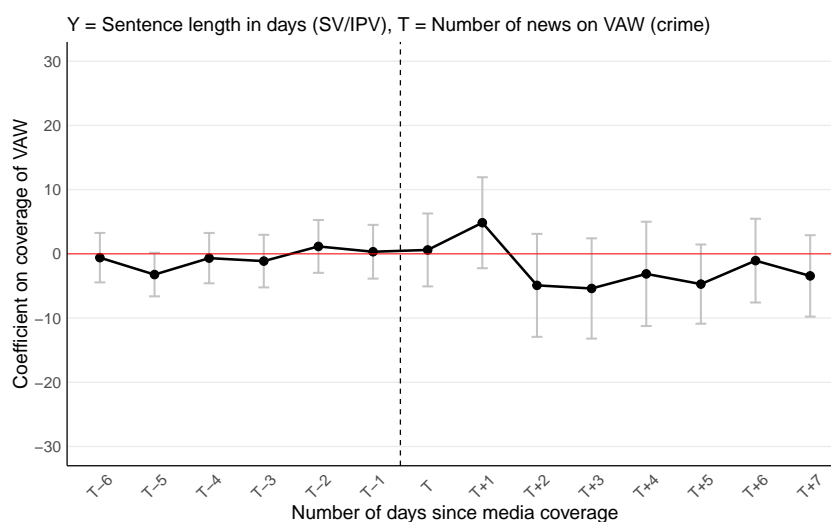
First, the observed effects are highly transient, fading out quickly within a few days (Figure 2.F.1). While broader shifts in gender norms, such as the post-#MeToo structural break, clearly condition the justice system's overall responsiveness, the ephemeral nature of these media-induced shocks is fundamentally incompatible with the gradual, persistent evolution of societal attitudes.

Second, the findings do not support an information-gathering or rational learning mechanism. Given their professional expertise and daily exposure to VAW, prosecutors are unlikely to rely on general television news for factual information. Empirically, this is corroborated by the fact that the behavioral response is driven entirely by the mere presence of coverage (the extensive margin), while additional news stories do not amplify the effect (Table 2.F.12). This lack of an intensive margin response directly contradicts a Bayesian learning process, where the accumulation of new information should theoretically lead to further updating of beliefs and stronger behavioral adjustments.

2.G Additional results on conviction

2.G.1 Additional results on the decision to convict and the sentence length

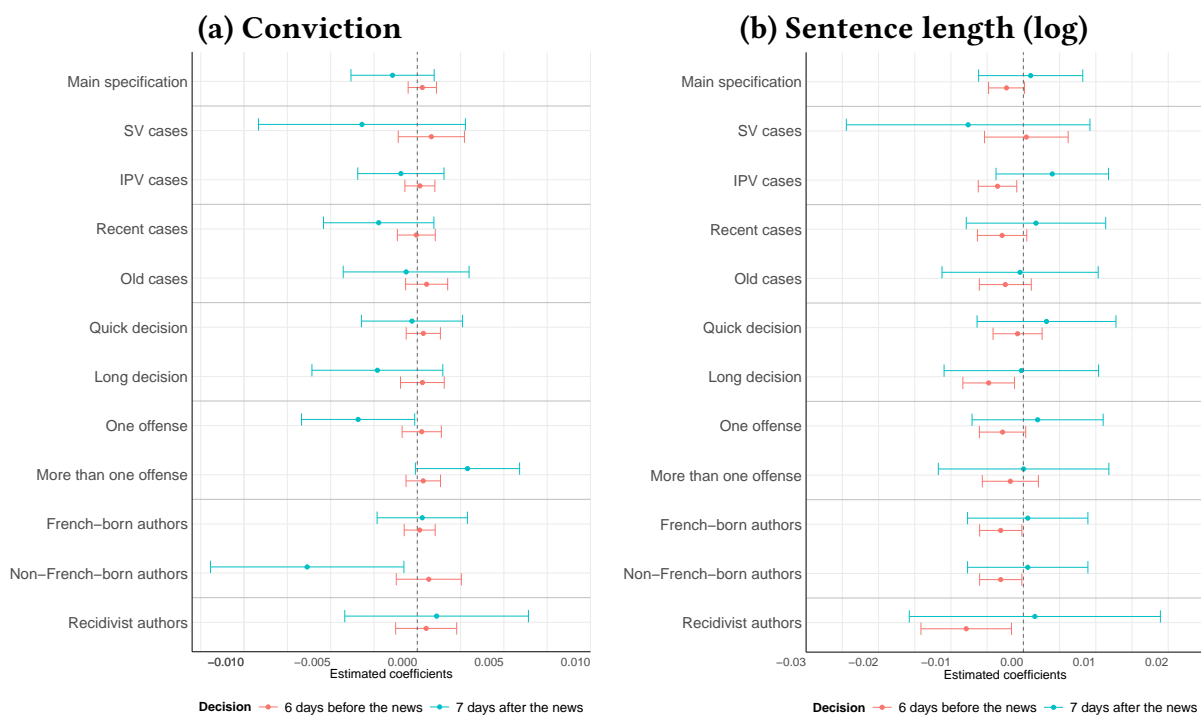
Figure 2.G.1: Effect of news coverage of crimes against women on sentence length for sexual and intimate partner violence



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the sentence length for convicted perpetrators of such violence, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. The dependent variable is the number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

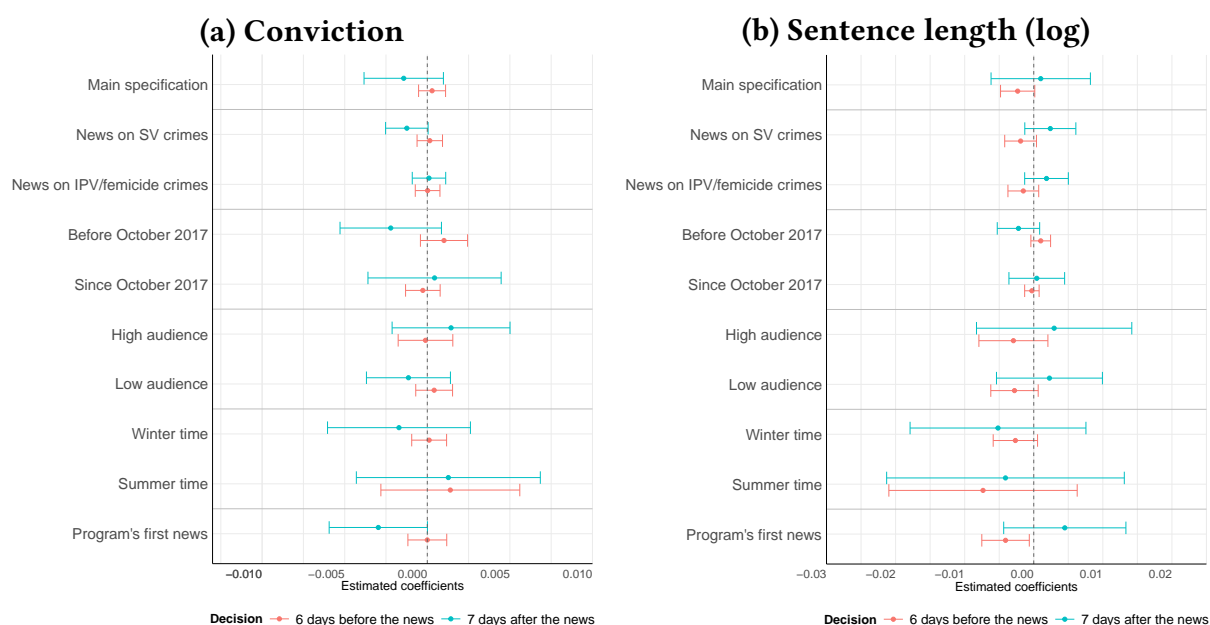
Figure 2.G.2: Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence: heterogeneity by case characteristics



Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to convict (panel a) and the sentence length (panel b) by subgroups of case characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

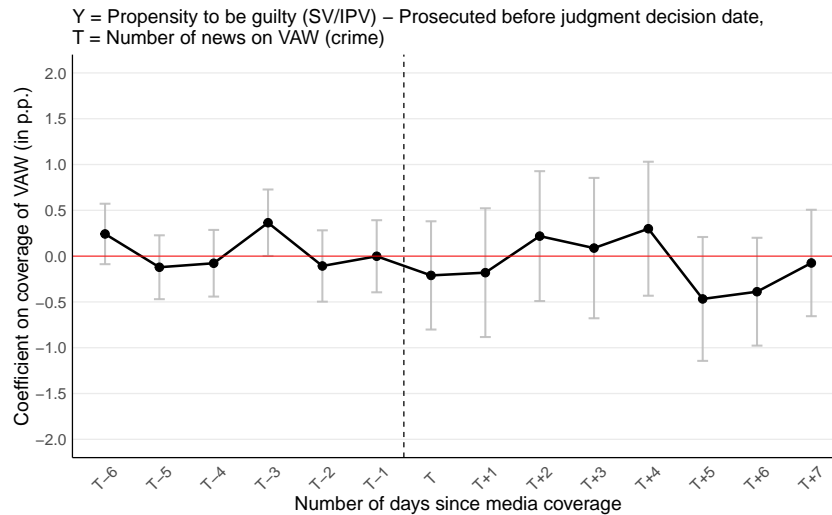
Figure 2.G.3: Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence: heterogeneity by news characteristics



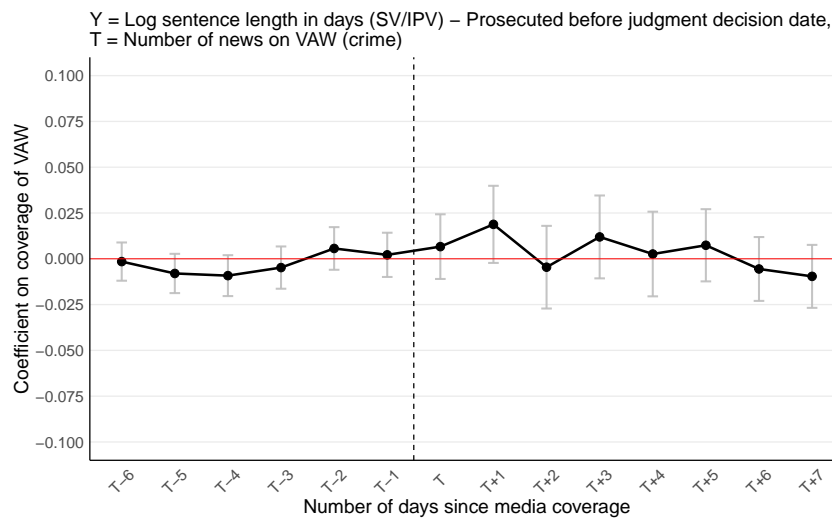
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to convict (panel a) and the sentence length (panel b) by subgroups of news characteristics, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the decision.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.G.4: Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence, for cases prosecuted before the judge's decision (91% of cases)



(a) Conviction

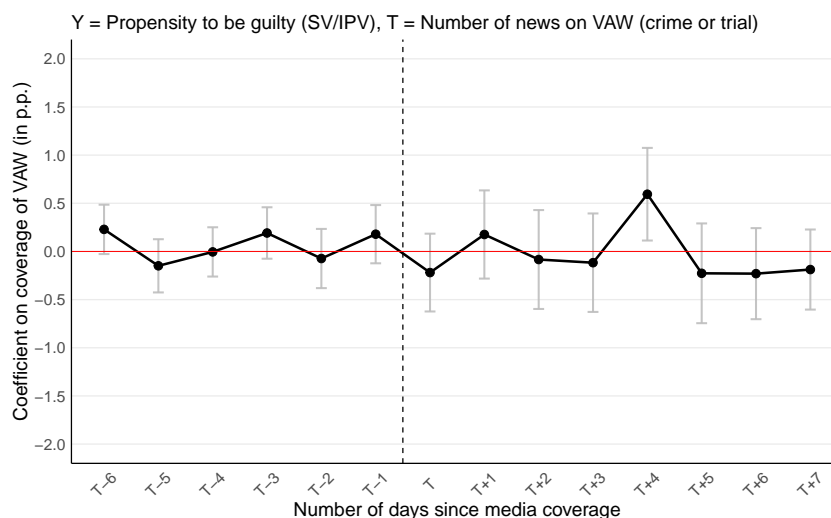


(b) Sentence length (log days)

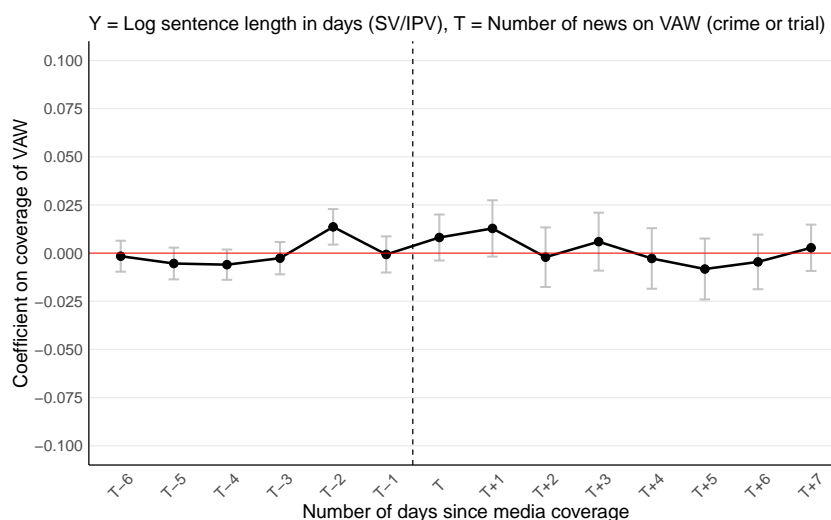
Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the propensity to convict such violence and the sentence length, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019, restricting to cases prosecuted before the news coverage. In the upper panel, the dependent variable is a dummy variable equal to one if the perpetrator was convicted at trial, with the estimated coefficient in percentage points. In the lower panel, the dependent variable is the number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.G.5: Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases, including news on trials



(a) Conviction

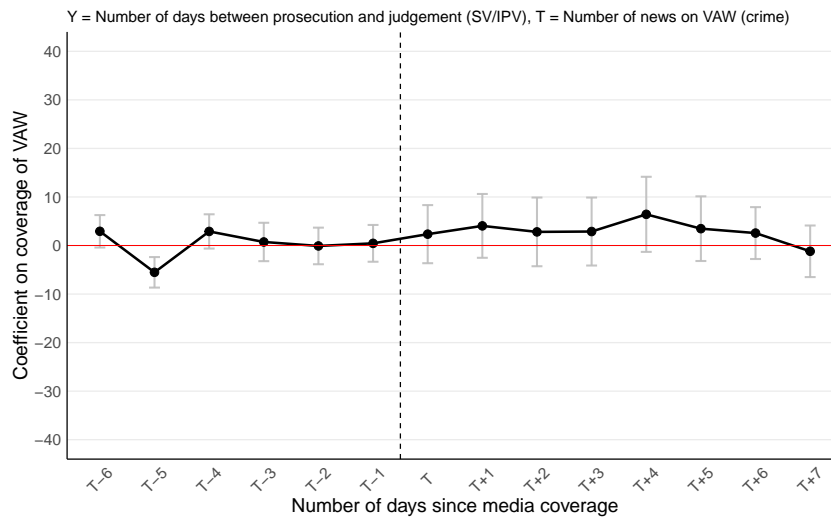


(b) Sentence length (log days)

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women or related trials on the propensity to convict such violence and the sentence length, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. In the upper panel, the dependent variable is a dummy variable equal to one if the perpetrator was convicted at trial, with the estimated coefficient expressed in percentage points. In the lower panel, the dependent variable is the number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women or related trials broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.G.6: Effect of news coverage of crimes against women on the number of days elapsed between prosecution and final court decision

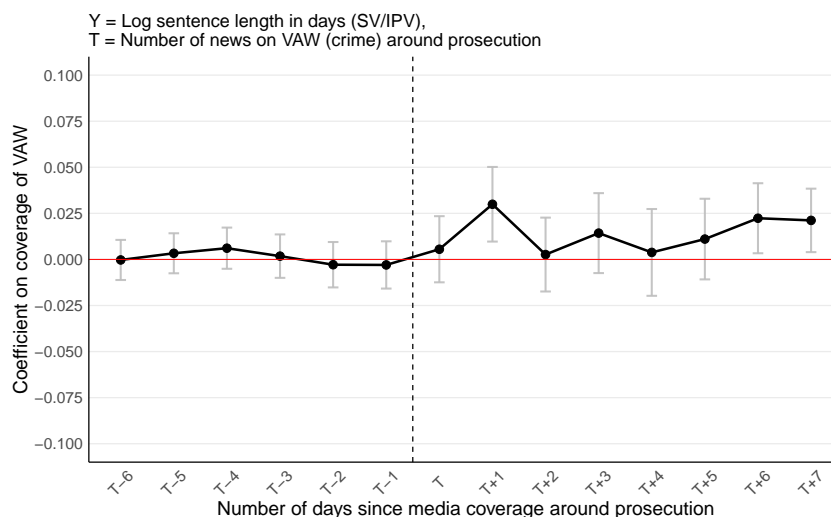


Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women on the time between prosecution and final judgment, with corresponding 95% confidence intervals. The sample consists of cases of sexual and intimate partner violence that underwent trial in correctional or juvenile courts between 2012 and 2019. The dependent variable is the number of days elapsed between prosecution and the final trial decision. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.G.2 Additional results on long-term effects

Figure 2.G.7: Long-term effect of news coverage of crimes against women on sentence length for sexual and intimate partner violence cases

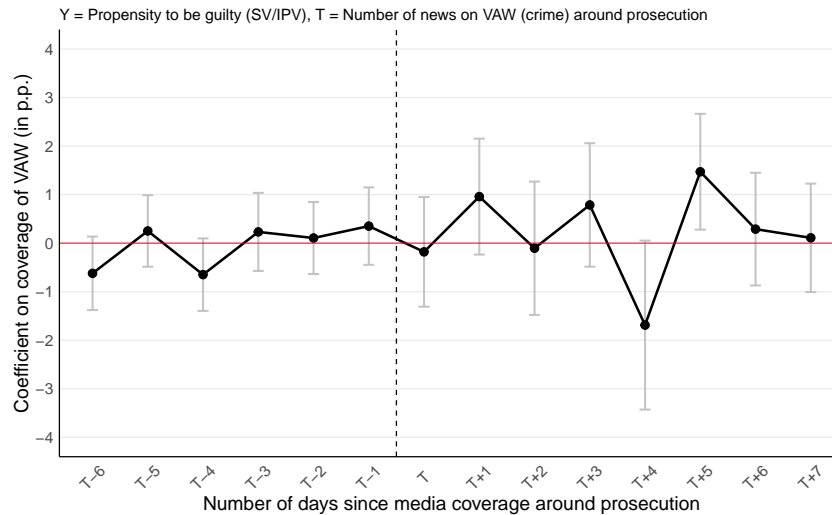


Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around the time of prosecution on the long-term sentence length, with corresponding 95% confidence intervals. The sample consists of all cases of sexual and intimate partner violence that were completed between 2012 and 2019, either dismissed or prosecuted in correctional or juvenile courts. The dependent variable is the log number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 around the prosecutor’s decision, with coverage occurring at time T .

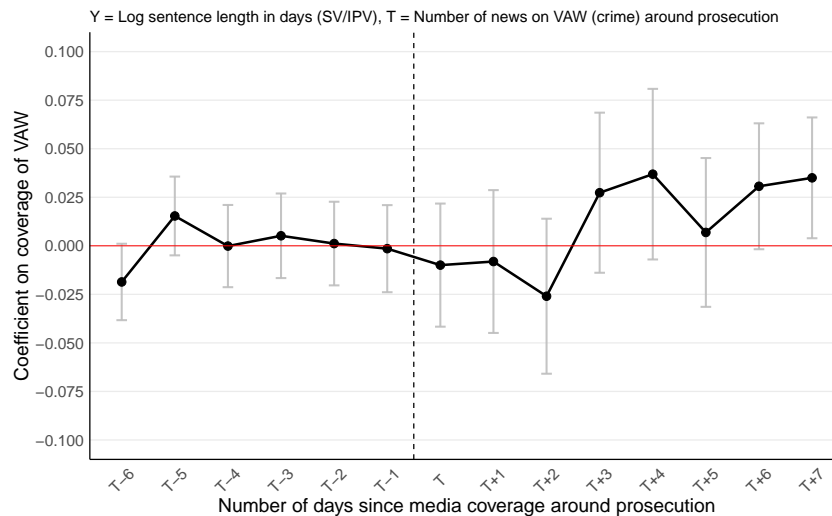
Reading: sexual and intimate partner violence cases prosecuted two days after the TV news coverage see a marginally significant increase in the sentence length for convicted cases.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Figure 2.G.8: Long-term effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases, among prosecuted cases only



(a) Conviction



(b) Sentence length (log days)

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around the time of prosecution on the long-term propensity to convict such violence and the sentence length, with corresponding 95% confidence intervals. The sample consists of all cases of sexual and intimate partner violence that were prosecuted in correctional or juvenile courts between 2012 and 2019 (excluding dismissed cases). In the upper panel, the dependent variable is a dummy variable equal to one if the perpetrator was convicted at trial, with the estimated coefficient expressed in percentage points. In the lower panel, the dependent variable is the log number of days of the prison sentence handed down to the convicted perpetrator. News coverage is measured using the number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 around the prosecution decision, with coverage occurring at time T .

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

Table 2.G.1: OLS results for the long-term effect of news coverage of crimes against women on the propensity to convict and the sentence length for sexual and intimate partner violence cases

<i>Dependent Variables:</i>	Conviction (0/1) (1)	Sentence length (days) (2)	Log. Sentence length (days) (3)
Number of news on VAW crimes in the 7 days before the decision	0.0053*** (0.0009)	3.289** (1.426)	0.0141*** (0.0040)
Number of news on VAW crimes in the 6 days after the decision	-3.38×10^{-5} (0.0003)	0.3796 (0.5132)	0.0015 (0.0014)
Year, month, day-of-the-week fixed effects	Yes	Yes	Yes
County of jurisdiction fixed effects	Yes	Yes	Yes
Case-level controls	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	687,198	159,182	142,693
R ²	0.17653	0.31962	0.27610
Within R ²	0.12066	0.31089	0.26112

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates of leads and lags from Equation 2.1, showing the effect of TV news coverage of crimes against women around prosecution on the propensity to convict such violence and the sentence length, pooling the leads and lags together into two event-time dummies. The sample includes all cases of sexual and intimate partner violence that were either dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019. In Column (1), the dependent variable is a dummy equal to one if the case led to a conviction. In Column (2), it is the sentence length (in days) for convicted cases. In Column (3), it is the natural logarithm of the sentence length (in days) for convicted cases. News coverage is measured using the total number of news stories on crimes against women broadcast during the 8pm national TV news on TF1 and France 2 in the 7 days before the prosecution decision. Case-level controls include: French-born author, author's age, number of victims, type of assault.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice and TV news data collected from the National Audiovisual Institute.

2.H #MeToo analysis

To benchmark the day-to-day effects of TV news coverage documented above, I also examine a larger, one-off salience shock. In particular, I study the #MeToo movement as a macro-level comparison to the high-frequency media variation on violence against women (VAW), allowing me to contrast short-lived coverage effects with a sustained shift in public attention and discourse. In October 2017, a series of public disclosures of sexual harassment and assault in work-related contexts by film producer Harvey Weinstein gave rise to the global #MeToo movement, sparking a flood of statements about experiences of sexual violence. Although the case originated in the United States, it also had a significant media impact in France, leading to the creation of the #BalanceTonPorc movement.

I conduct a difference-in-differences analysis to estimate the effect of the #MeToo movement on judicial decisions in cases of sexual and intimate partner violence, compared to other harm to persons. The model is specified as follows:

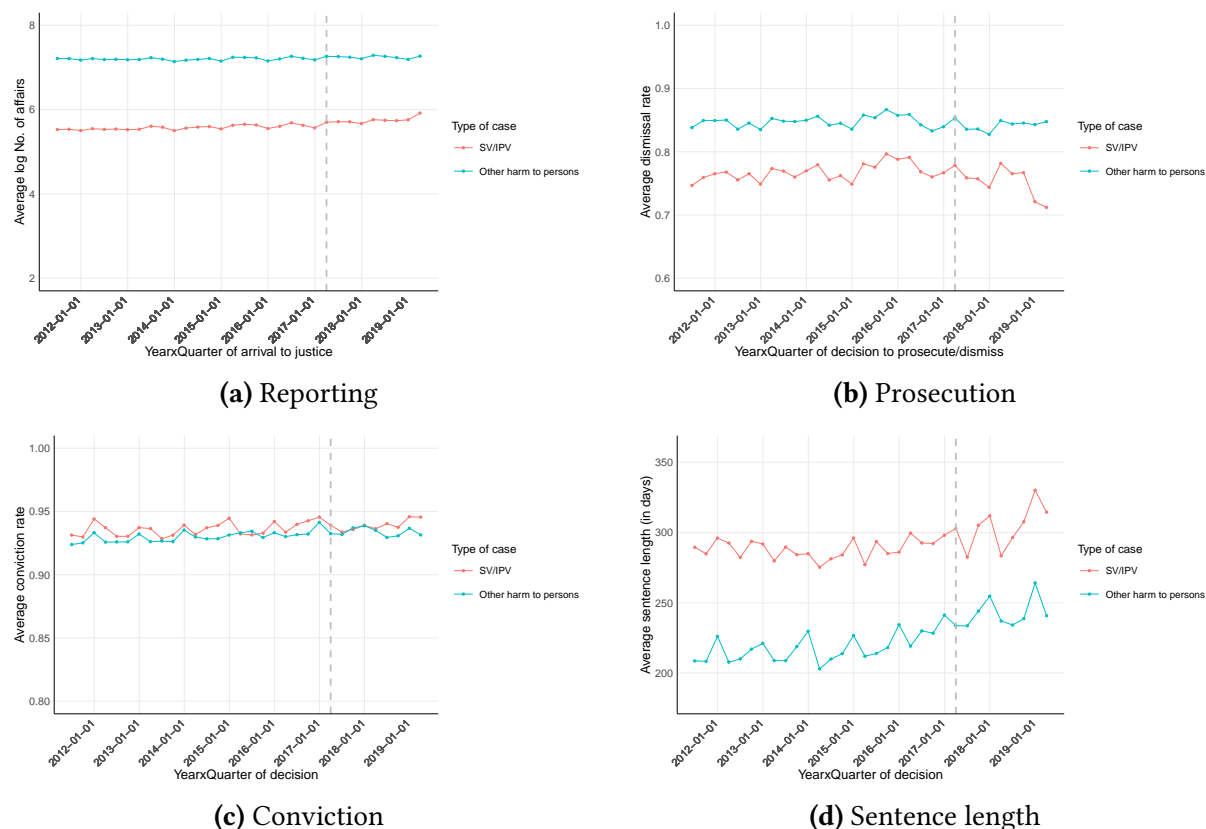
$$Y_{ict} = \beta_0 + \beta_1(SV/IPV_i \times Post\#MeToo_t) + X'_{ict}\gamma + \mu_c + \lambda_t + \varepsilon_{ict}, \quad (2.H.1)$$

where i indexes cases, c refers to counties of jurisdiction, and t denotes decision dates. The outcome Y_{ict} captures four stages of judicial processing: (i) the log number of cases reported, (ii) the propensity to dismiss, (iii) the propensity to convict, and (iv) the length of prison sentences (in days). $Post\#MeToo_t$ is a dummy equal to one after October 15th 2017, when the movement gained visibility in France. Case-level controls (X_{ict}) include offender and case characteristics such as age, French nationality, number of offenses, number of victims, nature of the offense, as well as dummies for repeat offender status, attempted crime, co-offending, mode of prosecution, and mode of judgment. County fixed effects (μ_c) and decision-date fixed effects (λ_t) account for unobserved heterogeneity across jurisdictions and time. The main specification is estimated by ordinary least-squares (OLS) regressions with heteroskedasticity-robust standard errors.

Identification. The difference-in-differences design relies on the parallel-trends assumption—that, absent the #MeToo shock, outcomes for sexual and intimate partner violence and other harm-to-persons cases would have evolved similarly. Figure 2.H.1 supports this assumption: the event-study leads from Eq. 2.H.1 show no discernible pre-treatment dynamics. The parallelism appears particularly strong for reporting and prosecution outcomes, while for conviction and sentence length the pre-trends are somewhat less flat, though still broadly comparable across groups. Combined with county and decision-date fixed effects and rich case-level controls, and given the plausibly exogenous timing of the #MeToo salience shock, these pre-trend patterns lend credibility to the identifying assumption that post-#MeToo deviations capture the causal effect of the movement rather than differential pre-existing dynamics.

Results. Table 2.H.1 presents the estimates of Equation 2.H.1. Across all outcomes, we find large baseline differences between sexual and intimate partner violence and other harm-to-persons cases: violence against women cases are significantly less likely to be reported, less likely to be dismissed, slightly more likely to lead to conviction, and, when convicted, associated with substantially longer sentences. Turning to the interaction with the

Figure 2.H.1: Pre-trends of judicial outcomes (raw data)



Notes: Time series of judicial outcomes for cases of violence against women and cases of other harm to persons before and after #MeToo (October 15th 2017). The sample consists of cases of sexual violence (sexual violence), intimate partner violence (IPV) or other harm to persons that were dismissed or prosecuted in correctional or juvenile courts between 2012 and 2019.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice.

#MeToo period, the results indicate moderate but consistent changes. For reporting, sexual and intimate partner violence cases are more likely to be registered after the movement. For dismissal, the interaction terms are negative and significant across specifications, suggesting a reduction in the propensity to dismiss such cases. For conviction, the coefficients are small and only marginally significant, pointing to limited changes in conviction rates. Finally, sentencing outcomes show a positive and significant effect: sexual and intimate partner violence cases received somewhat longer sentences in the post-#MeToo period, with effects that remain robust when including controls and fixed effects. Overall, the results suggest that the #MeToo movement was associated with modest but meaningful shifts in judicial responsiveness to violence against women in France, primarily through fewer dismissals and longer sentences.

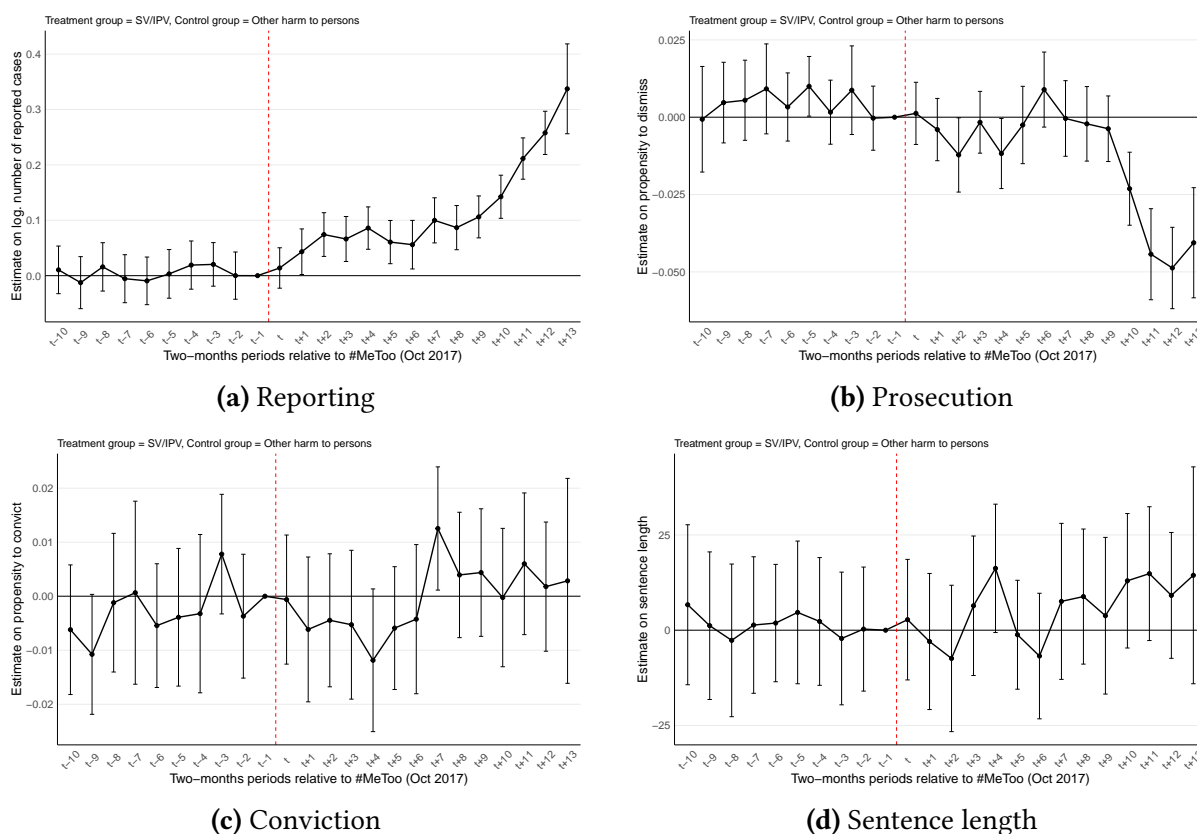
Dynamic effects. To explore the dynamics of the #MeToo movement, I estimate an event-study specification by interacting the sexual and intimate partner violence dummy with time dummies defined over two-month periods relative to October 2017. The coefficients are displayed in Figure 2.H.2. In all four outcomes, the pre-treatment coefficients fluctuate closely around zero and are statistically insignificant, which provides additional support for the validity of the parallel-trends assumption in the DiD framework. After the onset

of #MeToo, distinct patterns emerge. For reporting, the estimates rise steadily and remain significantly positive, suggesting a sustained increase in the number of sexual and intimate partner violence cases brought to court in the post-#MeToo period. For dismissal, the interaction coefficients fall below zero and remain significantly negative, indicating a lower propensity to dismiss these cases after #MeToo, mainly toward the end of the period. By contrast, the results for conviction and sentence length are much less precise: the estimated coefficients are small and surrounded by wide confidence intervals, which makes it difficult to conclude on systematic changes in these outcomes. Overall, the event-study analysis confirms that the #MeToo movement was followed by sustained increases in reporting and fewer dismissals of sexual and intimate partner violence cases, while evidence for conviction and sentencing outcomes remains weak.

Heterogeneous effects. Table 2.H.2 shows that effects differ markedly between sexual violence (SV) and intimate partner violence (IPV). Reporting rises for both categories, but somewhat more for SV. Prosecutorial decisions diverge: for IPV, dismissal propensities fall after #MeToo, whereas SV shows small positive coefficients on dismissals, implying no reduction—and possibly a slight increase—relative to other harm-to-persons cases. Conviction effects remain close to zero and largely insignificant for both SV and IPV. Sentencing becomes harsher for IPV, while SV sentencing effects are mixed—negative in parsimonious models and small positive in saturated ones. Overall, #MeToo is associated with stronger and more consistent prosecutorial and sentencing shifts for IPV than for SV, despite similar gains in reporting.

Comparison with media coverage. The magnitude of the #MeToo effect on prosecution is larger than the one estimated for day-to-day news coverage. While each additional news story on crimes against women decreases the propensity to dismiss sexual and intimate partner violence cases by about 0.54 percentage point (a 2.3% increase in the likelihood of prosecution given the baseline), the #MeToo movement is associated with a reduction in dismissal propensity of roughly 1 percentage point, sustained over time. This suggests that although prosecutors respond to short-term fluctuations in media salience, the broader visibility shock created by #MeToo had a more persistent and quantitatively stronger impact on prosecutorial decision-making. This pattern is in line with the view that day-to-day media coverage induces lighter or more transient shifts in prosecutorial behavior, whereas a large-scale social movement such as #MeToo generates a more profound normative shock.

Figure 2.H.2: Event-study analyses for the effect of #MeToo on judicial decisions



Notes: Event-study estimates of judicial outcomes for cases of sexual and intimate partner violence compared to other harm to persons, before and after the onset of the #MeToo movement (October 15, 2017). The sample includes cases dismissed or prosecuted in correctional and juvenile courts in metropolitan France between 2012 and 2019.

Source: Author's calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice.

Table 2.H.1: #MeToo movement and judicial decisions for cases of violence against women versus other harm to persons

	Ln(Number of cases reported)		Propensity to dismiss			Propensity to convict			Sentence length (days)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Variables</i>											
SV/IPV case × Post #MeToo	0.1308*** (0.0265)	0.1308*** (0.0045)	-0.0106*** (0.0011)	-0.0099*** (0.0011)	-0.0074*** (0.0010)	0.0008 (0.0013)	0.0022 (0.0013)	0.0022* (0.0013)	-8.105*** (2.135)	4.075** (1.691)	3.813** (1.407)
SV/IPV case	-1.637*** (0.0125)	-1.637*** (0.0022)	-0.0765*** (0.0006)	-0.0339*** (0.0010)	-0.0330*** (0.0009)	0.0039*** (0.0008)	0.0108*** (0.0008)	0.0099*** (0.0008)	70.16*** (1.200)	57.87*** (0.9562)	57.76*** (0.9503)
Post #MeToo	0.0212 (0.0181)		-0.0051*** (0.0004)	0.0067*** (0.0004)		0.0039*** (0.0007)	-0.0014** (0.0007)		24.84*** (0.9391)	15.34*** (0.7709)	
<i>Controls and Fixed-effects</i>											
Case controls	No	No	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Decision date fixed effects	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
County of jurisdiction fixed effects	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
<i>Fit statistics</i>											
Observations	5,844	5,844	4,757,143	4,757,143	4,757,143	874,307	871,405	871,405	625,540	624,271	624,271
R ²	0.78169	0.99679	0.00607	0.15670	0.20222	0.00011	0.01871	0.02479	0.01057	0.35618	0.36858

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates from Equation 2.H.1, showing the effect of the #MeToo movement on different stages of judicial decisions for cases of sexual violence (SV) and intimate partner violence (IPV), compared to other harm to persons. The outcomes are, respectively: the log number of cases reported to the courts (Columns 1–2), the propensity to dismiss the case (Columns 3–5), the propensity to convict (Columns 6–8), and the sentence length in days (Columns 9–11). Each block reports results from progressively saturated specifications including case-level controls (e.g., author’s nationality, age, number of victims, type of assault), as well as fixed effects for decision date and county of jurisdiction. The sample covers cases prosecuted in correctional and juvenile courts in metropolitan France between 2012 and 2019.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice.

Table 2.H.2: #MeToo movement and judicial decisions for cases of violence against women versus other harm to persons – Heterogeneity

	Ln(Number of cases reported)		Propensity to dismiss			Propensity to convict			Sentence length (days)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Sexual violence</i>											
SV case × Post #MeToo	0.1498*** (0.0325)	0.1498*** (0.0104)	0.0331*** (0.0015)	0.0297*** (0.0014)	0.0280*** (0.0014)	-0.0029 (0.0030)	0.0012 (0.0030)	0.0015 (0.0030)	-14.29** (6.156)	7.683 (5.220)	8.556* (5.178)
SV case	-2.681*** (0.0158)	-2.681*** (0.0057)	-0.0094*** (0.0009)	-0.0185*** (0.0008)	-0.0201*** (0.0008)	-0.0353*** (0.0016)	-0.0260*** (0.0017)	-0.0278*** (0.0017)	360.8*** (3.271)	215.7*** (2.830)	216.5*** (2.806)
Post #MeToo	0.0212 (0.0181)		-0.0051*** (0.0004)	0.0064*** (0.0004)		0.0039*** (0.0007)	-0.0014** (0.0007)		24.84*** (0.9391)	14.69*** (0.7731)	
<i>Fit statistics</i>											
Observations	5,844	5,844	4,267,097	4,267,097	4,267,097	732,341	729,807	729,807	505,066	504,042	504,042
R ²	0.86128	0.99172	0.00012	0.16022	0.20243	0.00142	0.01991	0.02684	0.08720	0.37781	0.39106
<i>Intimate partner violence</i>											
IPV case × Post #MeToo	0.1260*** (0.0242)	0.1260*** (0.0062)	-0.0390*** (0.0015)	-0.0345*** (0.0014)	-0.0294*** (0.0014)	-0.0003 (0.0013)	0.0006 (0.0013)	0.0004 (0.0013)	11.21*** (1.589)	11.07*** (1.304)	10.29*** (1.301)
IPV case	-2.079*** (0.0113)	-2.079*** (0.0029)	-0.1172*** (0.0008)	-0.0439*** (0.0014)	-0.0414*** (0.0013)	0.0191*** (0.0008)	0.0244*** (0.0008)	0.0238*** (0.0008)	-27.90*** (0.8362)	8.100*** (0.6938)	8.013*** (0.6955)
Post #MeToo	0.0212 (0.0181)		-0.0051*** (0.0004)	0.0066*** (0.0004)		0.0039*** (0.0007)	-0.0012* (0.0007)		24.84*** (0.9391)	15.23*** (0.7711)	
<i>Fit statistics</i>											
Observations	5,844	5,844	4,460,983	4,460,983	4,460,983	823,166	820,738	820,738	587,114	586,048	586,048
R ²	0.87580	0.99596	0.01142	0.15637	0.20291	0.00096	0.02001	0.02636	0.00347	0.33387	0.34766
<i>Controls and Fixed-effects</i>											
Case controls	No	No	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Decision date fixed effects	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
County of jurisdiction fixed effects	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: OLS estimates from Equation 2.H.1, showing the effect of the #MeToo movement on different stages of judicial decisions for cases of sexual violence (SV) and intimate partner violence (IPV), compared to other harm to persons. The outcomes are, respectively: the log number of cases reported to the courts (Columns 1–2), the propensity to dismiss the case (Columns 3–5), the propensity to convict (Columns 6–8), and the sentence length in days (Columns 9–11). Each block reports results from progressively saturated specifications including case-level controls (e.g., author’s nationality, age, number of victims, type of assault), as well as fixed effects for decision date and county of jurisdiction. The sample covers cases prosecuted in correctional and juvenile courts in metropolitan France between 2012 and 2019.

Source: Author’s calculations based on administrative data extracted from the management software *Cassiopée* provided by the French Ministry of Justice.

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Chapter 3

The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment

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Acknowledgements

This work has benefited from the support of the Agence Nationale de la Recherche through the programs Investissements d'Avenir (ANR-17-EURE-0001) and received funding from the *Institut des politiques publiques*, the *départements de l'Oise et de l'Eure*, the Ministry of Social Affairs' *Direction Générale de la Cohésion Sociale*, and grants from IRESP (project "EvalOrgaSad") and ANSES (project "BetterSupport"). We thank Juliette Ducoulombier, Emma Faure, Stéphane Frachot, and Titouan Lino for their research assistance, and the *département de l'Eure* for its initial collaboration. This chapter greatly benefited from discussions and helpful comments from Davi Bhering, Sylvie Lambert, Emily Nix, Hélène Périvier, Thomas Piketty, Simon Rabaté, and Maria Zumbuehl. I would also like to thank the participants of the PSE Applied Economics Seminar and the 2026 AFEPOP Conference for their valuable feedback.

Abstract

The home-care sector is expanding rapidly worldwide but faces chronic labor shortages driven by difficult working conditions, fragmented work schedules, professional isolation, low wages, and high turnover among its predominantly female workforce. Using a unique randomized controlled trial conducted in multiple home-care organizations across France, we estimate the causal effects of introducing self-organization in the home-care sector. In theory, self-organization could improve working conditions by granting care workers greater autonomy over scheduling, replacements, and care planning. Yet it may also increase administrative burdens and require constant availability, making its overall impact uncertain. We find that self-organization improves working conditions, job satisfaction, and mental health, and reduces worker turnover, without detrimental effects on care recipients. These results provide rare causal evidence that greater autonomy can improve workers' well-being and strengthen retention in long-term care, with important policy implications for France and beyond amid rising demand due to population aging.

JEL classification: I11, J24, J28, J81, M54

Keywords: Self-organization, Home care, Working conditions, Job satisfaction, Randomized controlled trial, France

3.1 Introduction

As developed countries worldwide face the demographic shift of aging populations, the demand for long-term care services is rapidly increasing. Across OECD countries, the share of individuals aged 65 and over is projected to increase from 18.5% in 2023 to 26.4% by 2050, while the share aged 80 and over—most likely to require long-term care—is expected to double from 4.9% to 9.6% over the same period (OECD, 2025). To address this growing need, public policy in many countries has embraced a "domiciliary shift", prioritizing aging at home over institutionalization. In France, over 21 million residents will be aged 60 or older by 2030, with an estimated 3 million people expected to experience loss of autonomy and requiring daily assistance – up from 2.5 million in 2015.

Home care services are central to this strategy, but the sector faces severe structural challenges in many countries. In France, the workforce comprises around 570,000 workers – 96% of whom are women – with an average age of 49 years, older than the national average. Despite labor forecasts anticipating a need for 305,000 additional workers by 2030 (El Khomri, 2019), the sector remains chronically understaffed and suffers from high turnover and absenteeism. This paradox stems from longstanding issues: part-time and fragmented contracts, low wages, physically and emotionally demanding work, poor working conditions, and limited career prospects. Many workers report professional isolation, unpredictable work schedules, and limited communication with managers or colleagues (Bailly et al., 2013). These challenges have fueled a growing interest in alternative work organization models that could improve job quality, increase worker engagement, and ultimately enhance care delivery.

This paper examines the effects of a new work organization in the home-care sector, the self-managed team organization, by designing and implementing a randomized controlled trial (RCT) in France. By shifting decision-making from supervisors to frontline care workers, this model aims to empower care workers while improving operational efficiency, productivity, and job quality. Under this framework, small teams of care workers operate with limited hierarchical oversight and take collective responsibility for organizing work and coordinating care. Supported by lean administrative structures, workers exercise greater autonomy over scheduling, replacements, and care planning.

Initially developed in the Netherlands in 2006 in the nursing sector under the *Buurtzorg* name, the model aimed to attract professionals and is now being tested in home care services in several countries, including France. Self-managed teams are viewed as a promising ap-

proach to address structural weaknesses of the sector, such as recruitment difficulties, high turnover, and loss of meaning at work, by giving frontline workers more autonomy and control over their professional practice. The model is expected to increase intrinsic motivation, improve team functioning, and ultimately enhance care quality. However, it also entails significant changes in work practices and increased investment from staff. Workers must develop new skills in planning, coordination, and decision-making, while adapting to a form of work that requires greater individual and collective responsibility, heightened coordination demands and rapid responsiveness. Whether this organizational model improves working conditions, and whether it translates into measurable benefits for care recipients, therefore remains an open empirical question.

To date, evidence on the effects of this model outside the Dutch nursing care context remains largely qualitative. Initial experiments have been conducted in the United Kingdom, notably in Scotland (Leask et al., 2020) and in London (Drennan et al., 2018), and have primarily been studied through semi-structured interviews. In their literature review of the implementation of Buurtzorg-derived models outside the Netherlands, Hegedüs et al. (2022) mention that several studies report higher staff satisfaction, linked to opportunities for upskilling, participation in decision-making, greater involvement in patient care, increased self-confidence, and positive feedback received from patients. Overall, patients themselves also tend to express high levels of satisfaction. In England, for example, Drennan et al. (2018) find that neighborhood nursing teams can make the work environment more attractive for nurses. At the same time, the literature highlights implementation challenges, including a heavier administrative burden and difficulties maintaining work–life balance due to expectations of constant availability and frequent team meetings. While mutual monitoring within teams can raise effort and productivity, it may also generate stress and worsen working conditions (Stratelys, 2021).

Given the uncertainty surrounding the effects of the self-managed teams model and the scarcity of rigorous quantitative evidence, assessing the causal impact of this work organization is essential. This paper seeks to fill this gap by designing and implementing a randomized controlled trial (RCT) across ten home care services in four French districts (*départements*). Within each service, teams (or individual workers) were randomly assigned to adopt the self-managed team model or to remain under the traditional hierarchical organization. Randomization was stratified by service to ensure balance and conducted at the team or individual (worker) level. The intervention lasted approximately 12 months, during which treated teams

received training, adopted new coordination practices, and gained autonomy over scheduling and work organization. Relying on intention-to-treat (ITT) effects, this design allows us to estimate the causal impact of self-managed teams on care workers' outcomes, including working conditions, job satisfaction, psychosocial risks, and absenteeism, as well as on care recipients' outcomes.

Our findings reveal substantial improvements in worker outcomes. Assignment to the self-managed team model significantly increased job satisfaction by 0.24 standard deviations and improved overall working conditions by 0.41 standard deviations. Treated workers experienced a significant reduction in job strain (-0.25 SD), driven primarily by lower psychological demands and increased social support from colleagues and supervisors. Mental health improved by 0.24 standard deviations, with workers reporting feeling happier, less stressed, and less discouraged at work. We also observe a reduction in absenteeism, with treated workers spending fewer days on leave (-0.28 SD). These improvements appear to stem from better work organization: treated workers gained greater flexibility in adapting schedules to personal constraints, experienced fewer last-minute changes, and were less likely to rush through tasks.

Crucially, these improvements in job quality translated into significantly higher worker retention, with attrition 7.8 percentage points lower in the treatment group. This reduction, primarily due to fewer team changes and retirements, directly reflects improved job quality: across groups, leavers reported significantly lower satisfaction than stayers. From an estimation standpoint, this selective attrition implies that our ITT estimates represent a lower bound of the true impact: since the control group lost its least satisfied members at a higher rate, our results likely underestimate the model's full effect.

From the care recipients' perspective, the model significantly improved the perceived adequacy of care time, suggesting better service customization. While these gains have not translated into broader shifts in recipients' health or overall satisfaction, they align with workers' own reports of greater professional fulfillment and an increased ability to meet beneficiaries' needs. Although the analysis of recipients is constrained by lower statistical power, the evidence indicates that self-management can substantially bolster worker well-being without compromising the quality of care. Overall, our results provide robust causal evidence that the self-managed team model can meaningfully improve job quality in the home care sector, addressing the critical challenges of retention and well-being while maintaining high standards

of care delivery.

This paper contributes to two main strands of the literature. First, we build on the literature in labor and personnel economics that examines the role of incentives, peer effects, and teamwork in shaping productivity (Bandiera et al., 2005; Mas and Moretti, 2009). The productivity effects of incentives to work in teams have been documented in various contexts, including healthcare (Bartel et al., 2014; Chen, 2021) and other sectors (Hamilton et al., 2003, 2012; Friebel et al., 2017; Hoffman and Stanton, 2024)). The teamwork literature emphasizes that peer monitoring and peer pressure can shape worker behavior (Hoffman and Stanton, 2024). Hamilton et al. (2012) notably highlights that teamwork can influence productivity through multiple channels: more productive workers can teach their colleagues to perform tasks more efficiently; teams can allocate tasks according to each member's comparative advantage; and teams may be better at discovering new methods or engaging in process innovations by combining the unique knowledge of individual team members. However, the literature also points to potential downsides: team production can exacerbate free-riding problems, making effort harder to enforce when individual contributions are imperfectly observable (Kandel and Lazear, 1992; Mas and Moretti, 2009). We contribute to this literature by providing one of the first quantitative causal evidence from a randomized field experiment on the effects of introducing self-managed teams in a real-world care setting, thereby complementing earlier work based on qualitative approaches. Our design allows us to measure the causal impacts of this work organization on both workers and care recipients, highlighting clear improvements for workers overall, while effects on care recipients are more limited.

Second, demographic change worldwide has prompted research in the economics of aging and public health to expand into the topic of long-term care for dependent elderly (Gramain et al., 2005; Roquebert et al., 2019; Carrère and Jusot, 2020; Carrère, 2021; Bensaid et al., 2022; Roquebert and Tenand, 2023), which is an area expected to become increasingly central in the coming years (OECD, 2020). Rapidly growing care demand implies that existing long-term care systems must adapt to avoid excessive costs or deteriorating service quality, including through the development of new work organizations such as teamwork in the healthcare sector (Bartel et al., 2014; Chen, 2021). In this context, we contribute to the growing literature on organizational innovation in care work by documenting how organizations that expand frontline workers' autonomy can help address key constraints in long-term care. We provide empirical evidence on attracting and retaining workers while improving cost efficiency, of-

fering actionable policy insights for a vital public health sector facing chronic labor shortages and rising demand due to population ageing (Nadash et al., 2012; Kotschy and Bloom, 2022).

The remainder of the paper is organized as follows. Section 3.2.2 describes the institutional background, including long-term care in France and the self-managed team model. Section 3.3.2 presents the experimental design. Section 3.5.1 provides descriptive evidence from the data. Section 3.5.3 outlines the empirical strategy. Section 3.6 presents and discusses the main results. Finally, Section 3.7 concludes.

3.2 Institutional Background

3.2.1 Long-term Care in France

As in many other developed countries, the French home care sector faces growing structural pressure due to rapid population aging. Excluding care workers directly employed by individual employers, in 2022 the sector employed 475,000 workers – 95% of whom were women – and three-quarters were employed part-time (DREES, 2025). Labor forecasts anticipate a sharp increase in demand. For instance, Borey et al. (2025) estimate that in addition to replacing retiring workers, a 50% increase in the number of workers, or a 42% increase in full-time equivalent employment, will be necessary over the next 25 years to meet the needs of an aging population, for a total of 300,000 workers to be hired between 2025 and 2030 (Jolly et al., 2023).

Yet the sector is currently chronically understaffed and struggles to attract workers. Care jobs for the elderly are heavily affected by demanding working conditions. The rates of work-related accidents and occupational diseases (AT/MP), as well as absenteeism, are among the highest of all sectors. In nursing homes and home care, accident rates are three times the national average and one-third higher than in construction work (El Khomri, 2019). Employees in home care professions have highly fragmented and atypical work schedules, which makes it difficult to balance professional and personal life. Working in direct contact with the people they assist, they report few tensions or aggressions, but face emotional demands and ethical conflicts. They feel isolated from colleagues and management, even though they generally feel recognized for their work (Memmi et al., 2021). These features can result in a depersonalized care experience, high levels of frustration among both users and professionals, and increasing recruitment difficulties.

High turnover rates are likely to have negative consequences for the quality of care and the health of care recipients, as documented in institutional settings (Antwi and Bowlis, 2018). Moreover, with an average age of 48, home care workers are on average older than the general working population (DREES, 2025). Hiring difficulties are widespread in the sector, which has led the *Conseil économique, social et environnemental* to classify it as one of the “occupations under strain” (*métiers en tension*). In this context, public authorities and care providers alike have called for new organizational models that could improve working conditions, support worker retention, and help make the sector both more attractive and sustainable.

3.2.2 The Buurtzorg-Inspired Model

Origins and Principles of the Buurtzorg Model

One organizational model has attracted particular attention in the care sector: the self-managed teams, inspired by the *Buurtzorg* initiative developed in the Netherlands. Launched in 2006 by nurse Jos de Blok, *Buurtzorg* (meaning “neighborhood care” in Dutch) emerged as a reaction to the increasing bureaucratization and fragmentation of the home nursing system. Frustrated by his inability to change the system from within, de Blok created a new organization built on radically different principles (Centre for Public Impact, 2025).

Buurtzorg promotes a decentralized and horizontal form of governance – often referred to as holacracy or a “liberated company” model – in which frontline care teams are granted a high degree of autonomy in managing their work.¹ The primary unit of organization is a team of around ten professionals who collectively handle scheduling, holidays, logistics, service delivery, training, and sometimes recruitment decisions. Middle management is eliminated or significantly reduced, and day-to-day decisions are made by consensus or collective deliberation within the team, supported by lean administrative structures. The model aims to empower care workers, foster autonomy and professional engagement, improve motivation and job satisfaction, and ultimately enhance the quality of care delivered to users.

In addition to changes in internal governance, the *Buurtzorg* approach emphasizes a more

¹Beyond home care, the broader idea of the “liberated company” and self-managing organizations has gained substantial influence in the management literature (Getz, 2009; Lee and Edmondson, 2017). Often framed as a response to rigid hierarchies and worker disengagement, this organizational philosophy is promoted as a pathway to greater employee fulfillment and performance by replacing traditional supervisory control with decentralized, peer-based decision making (Bernstein et al., 2016). Its appeal lies in the promise of rehumanizing work by fostering autonomy, responsibility, and intrinsic motivation, ultimately yielding benefits not only for workers but also for service users and the broader economy.

integrated way of organizing care. Teams are responsible for a stable set of care recipients within a defined area, and care is conceived holistically rather than as a sequence of fragmented tasks. Care recipients can contact the team directly during designated times, which is intended to improve responsiveness and continuity of care. Teams are also encouraged to identify and mobilize informal caregivers (family, friends, neighbors) and to coordinate with other formal health and social care providers involved in the beneficiary's support.

Starting with a single team, Buurtzorg grew rapidly and now employs over 11,000 people. Its success has inspired numerous care organizations across Europe and beyond, including in the French home care sector (Gray et al., 2015; Hegedüs et al., 2022).

Implementation in French Home-Care Services

In France, several local governments and care providers have recently begun experimenting with self-managed teams, particularly in the non-medical home care sector. This interest stems not only from a desire to improve working conditions but also from a more pragmatic objective: reducing absenteeism and turnover under tight fiscal constraints, often at constant or even decreasing budgets.

Implementing the Buurtzorg-inspired model in a different institutional context nevertheless raises significant challenges (Leask et al., 2020). The French home-care sector shares several similarities with the Dutch nursing care system prior to Buurtzorg's reforms: rigid administrative care plans with fixed time allocations, fragmented scheduling involving multiple short visits by different staff members, limited consideration of users' social needs, and weak integration of informal caregivers. Yet important differences limit the direct transferability of the Dutch model. In particular, the French home-care workforce is on average less qualified than Dutch nursing staff and is often accustomed to highly prescriptive and hierarchical modes of organization. Transitioning to self-managed teams requires new skills in planning, coordination, and collective decision-making, as well as greater investment in work. This shift may be particularly demanding for a workforce that is not only underqualified in many cases, but also sometimes affected by fatigue or burnout. Training and support are therefore critical, yet their scope, duration, and quality vary considerably across services.

Several experiments of self-managed teams in the French home-care sector have been subject to both qualitative and quantitative evaluations (CNSA, 2025). These evaluations suggest that regular meetings promote the sharing of best practices, while flexible scheduling and

adapted timetables enhance work–life balance, reduce stress, and improve employee satisfaction. Furthermore, standardizing practices and management tools may support overall well-being. Workday durations are reduced while the duration of paid work increases, bringing overall work intensity more in line with other sectors. However, to the best of our knowledge, no existing evaluation accounts for selection bias regarding participation in such experiments.

Moreover, most existing French implementations differ significantly from the original Buurtzorg model. Many adopt semi-self-managed formats in which teams retain some hierarchical supervision and do not have full control over recruitment, scheduling, or training. These adaptations reflect both institutional caution and the practical constraints imposed by existing regulatory and funding frameworks. In addition, the limited available experiences have often taken place in favorable settings and rely heavily on testimonials from directly involved stakeholders. As such, they provide little evidence on the feasibility and effects of large-scale or top-down implementation.

The few qualitative studies available on the Buurtzorg-inspired model also suggest that the outcomes of such organizational change depend critically on implementation details: whether workers receive pay increases alongside new responsibilities, whether time is formally allocated for coordination tasks, and whether collective time is offered to reduce isolation. For instance, [Olivares Bøgeskov and Segoli \(2024\)](#) argues that challenges often stem from unclear roles and resistance to cultural change, and that successful self-managed team establishment requires both cultural and structural changes to ensure sustainability. There is also ongoing debate about whether the observed improvements stem from greater autonomy per se, or rather from other correlated factors, such as the introduction of team meetings, changes in scheduling practices, or access to training. In this context, providing rigorous, evidence-based causal estimates of the effects of self-managed teams is essential to determine whether the model can deliver on its promises and be generalized beyond a few favorable pilot settings.

3.3 Intervention and Experimental Design

3.3.1 Intervention: The Self-Managed Teams Organization

We evaluate the impact of introducing self-managed teams in the French home-care sector. The intervention rests on two key pillars: (i) team structure and governance, and (ii) the care delivery model. In our setting, these teams function as relatively autonomous units of 8

to 14 workers with minimal oversight from central management. Teams assume collective responsibility for scheduling, task distribution, absence management, and decision-making, relying on regular team meetings for coordination and peer support. Furthermore, the sector manager's role shifts from supervisor to coach, while teams are equipped with enhanced communication tools to facilitate information sharing and care coordination. Consistent with the Buurtzorg-inspired approach, each team is responsible for a stable cohort of care recipients within a defined geographical area. Teams are expected to enhance care continuity and coordination through direct contact with users during designated times and, when relevant, coordination and collaboration with informal caregivers and other providers. Detailed features of the self-managed teams model are described in Appendix 3.E.²

Recruitment. Participating home-care services were recruited through existing partnerships with French districts (starting with Eure and later expanding to Haut-Rhin, Pas-de-Calais, and Oise) and national care provider networks. Ten services joined the experiment. Participating services were required to commit to the implementation framework for self-managed teams and facilitate survey administration and follow-up by signing a commitment charter (see Appendix 3.E).

3.3.2 Experimental Design

Randomized Controlled Trial. To rigorously assess the impact of the self-managed teams implementation, we designed a randomized controlled trial (RCT). The aim of the trial is to estimate the causal effects of the organizational reform on both workers (e.g., absenteeism, job satisfaction, turnover) and care recipients (e.g., self-declared health, perceived quality of care, coordination).³ The proposed analyses, including both primary and secondary outcomes, were described in a pre-analysis plan (PAP) registered with the AEA RCT Registry (AEARCTR-0008487).

The study uses random assignment to ensure that treated and control units are comparable. Specifically, within each participating service, teams or individual workers (depending

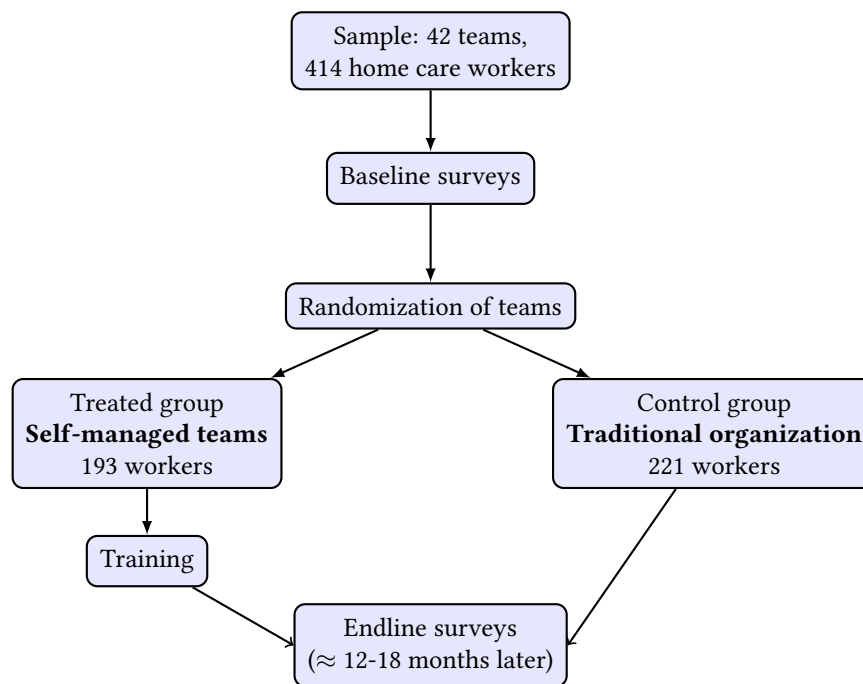
²To make the model operational and comparable across sites, we collaborated with the National Agency for the Improvement of Working Conditions (ANACT), one departmental council (Eure), and participating providers to define a shared set of minimum requirements for self-managed teams. These specifications serve both as an implementation guide and as an evaluation reference.

³The protocol was reviewed and approved by the Paris School of Economist institutional review board (IRB), application 2021-013.

on operational constraints) were randomly assigned to either adopt the self-managed teams model or remain under the standard organizational structure. This procedure addresses a key identification concern: in the absence of randomization, teams or workers who volunteer for organizational change might systematically differ from others. As a result, any observed improvement could not be confidently attributed to the organizational change itself, and comparisons would be vulnerable to selection bias.

To allow for a gradual implementation of the new work organization and minimize disruptions, the randomization protocol was designed to determine the *order of transition* to self-managed teams within each service. This approach aligned with the operational preferences of many providers, who were already inclined to begin the organizational change with a few pilot teams before expanding the model more broadly.

Figure 3.1: Experimental design



Implementation process. Figure 3.1 illustrates our experimental design. After enrolling in the experiment, each home-care service collaborated with its local management to identify potential teams or individuals within specific sectors and determine the appropriate unit of randomization (e.g., half of teams, or one in two per sector). Service management then submitted lists of workers to the research team, resulting in 414 participants eligible for random assignment across the ten services.⁴ Local management organized these eligible workers into

⁴Prior to randomization, 413 workers were invited to complete the baseline survey. One worker withdrew

42 teams, consisting of both pre-existing units and newly formed teams for the study (see Table 3.A.1 for the distribution of workers and teams). Of these, 27 teams – comprising 193 workers – were assigned to the treatment group. We conducted the randomization primarily at the team level, adapting the unit to account for service-level constraints. For one service, we utilized individual-level randomization within a predefined team, while for others, we employed stratification by geographical area (to account for service zones) or workforce characteristics (distinguishing between home care aides and nursing assistants) to ensure balance between the treatment and control groups (Table 3.A.1 also specifies the randomization unit for each service).⁵

Managers subsequently informed workers of their treatment assignment and organized training sessions to prepare the treatment group for the transition to the self-managed team model. Although these sessions were intended to facilitate an immediate launch, several services faced significant delays due to budgetary constraints or scheduling issues with external training providers. These providers varied across structures, though they were typically shared within the same department. Training modalities also differed by organization size: in smaller services, all workers generally received direct training, whereas larger organizations sometimes adopted a "train-the-trainer" model, where sector managers (coaches) were trained first and subsequently cascaded the knowledge to their respective teams. Following these staggered starts, the research team conducted bimonthly follow-ups to monitor progress, assess implementation quality, and track workforce dynamics, including staff turnover and reasons for departure in both treatment and control groups.

Given the complexity of organizational change and the variation in its rollout across home-care services, we closely monitored the actual implementation of the model at each site. To this end, we established a set of minimum criteria based on the shared self-managed teams framework to verify, in coordination with management staff, whether teams were effectively operating autonomously. This enabled us to assess each team's adherence to the model (e.g., the degree of autonomy, collective work practices, and care coordination) to evaluate whether

before treatment assignment but later rejoined the experiment and subsequently completed the survey, bringing the initial sample to 414 care workers.

⁵Following Duflo et al. (2007), we initially considered three randomization levels: service, team, and individual. Service-level randomization was rejected as power calculations indicated a prohibitively large number of required services. Most providers deemed individual-level randomization unfeasible to avoid reallocating workers already assigned to specific geographical sectors. In one case involving eleven workers in a confined area, individual-level randomization was used. In three other services, one team per sector was randomized into the treatment group, allowing managers to pilot the approach. These "paired" designs may introduce spillover effects due to shared geography or management.

implementation was sufficiently advanced to conclude the experiment and conduct the end-line surveys. We further discuss treatment implementation and compliance in Section 3.5.2.

3.4 Data Collection and Descriptive Statistics

3.4.1 Data Sources

The core of our evaluation relies on an original survey targeting home care workers. The survey was administered twice: prior to randomization (baseline) and approximately 12 to 18 months after implementation of the self-managed teams organization (endline), in order to measure changes over time. We also conducted a complementary survey of care recipients (or their informal caregivers) at the same two points in time. Finally, we draw on surveys of home-care services and managers to track the implementation process of the new organization over time.

Worker survey. The worker questionnaire is designed to assess the effects of the self-managed teams model on care workers' working conditions and well-being. It also collects sociodemographics and information on the work environment, allowing for heterogeneity analysis across subgroups. The main sections of the questionnaire include:

- Sociodemographic characteristics (age, gender, tenure, employment status);
- Job satisfaction and professional engagement;
- Working conditions and psychosocial risk factors, based on the Karasek Job Content Questionnaire;
- Team functioning and collaboration with informal caregivers;
- Help provided to service recipients;
- Work hours, time pressure, and schedule constraints;
- Autonomy and decision-making latitude;
- Work climate, team dynamics, and perceived support;
- Absenteeism and self-reported health;

- Changes in the structure.

Surveys were administered in person using tablets during team meetings whenever possible, with support from field staff.⁶ Respondents who were unable to attend were invited to complete the survey online or by phone (see the response rate by survey mode in Appendix Figure 3.A.3). In all cases, survey participation was considered paid working time for employees.

We also administered a shorter follow-up exit survey to workers who left the organization before the endline wave and whom we were able to reach. This questionnaire collected a retrospective assessment of working conditions and satisfaction with the self-managed teams model, as well as self-reported reasons for leaving, in particular whether the organizational change contributed to their departure.

User survey. The user questionnaire, administered to care recipients or their informal caregivers,⁷ focuses on perceived changes in service quality and user well-being. It includes the following components:

- Identification of the respondent (care recipient or informal caregiver);
- Sociodemographic characteristics of the care recipient;
- Satisfaction with the service (organization, time allocation, quality of interactions, etc.);
- Perceived health, well-being, life satisfaction and mental health.
- General life satisfaction.

Surveys were administered either by phone or online, using a link shared with respondents, with the support of home care services, which informed care recipients and their families in advance.

Home care service and management surveys. Three additional questionnaires were developed to document organizational characteristics and track implementation processes over

⁶The questionnaire was anonymous and administered without the presence of hierarchical staff, in order to maximize both response rates and the quality of responses.

⁷Although the questionnaire was initially designed to be administered directly to care recipients, it was adapted to allow completion by informal caregivers when necessary, particularly in cases where cognitive limitations made it difficult for care recipients to respond themselves.

time. The *initial service questionnaire* collects baseline information on home-care services, including size, legal status, workforce composition, human resources practices, and initial plans for implementing the self-managed teams model. The *follow-up service questionnaire*, administered at regular intervals (approximately every three months), monitors the rollout of self-managed teams, adjustments made during implementation, support mechanisms, training initiatives, and perceived benefits or challenges. This questionnaire was administered alongside a worker-tracking file with each participating service, which allowed us to monitor workforce turnover, including departures and new arrivals in each participating team, as well as the corresponding reasons for exits and entries as reported by administrative staff (rather than by workers themselves).

A separate *coach questionnaire* targets the team coordinators or sector managers responsible for supporting self-managed teams. It collects information on their role, training they received, the perceived feasibility of task delegation, observed effects of the new organization, and the hierarchical support provided to them. Together, these instruments provide complementary insights into how the self-managed teams model is operationalized on the ground and how it reshapes management practices.

3.4.2 Survey Response Rates

Among the 414 workers initially included in the experiment, 370 completed the baseline survey, yielding a high overall response rate of 89%. This baseline response rate was perfectly balanced between the experimental arms (89% in the treatment group vs. 90% in the control group, see Appendix Tables 3.A.2 and 3.A.9). At endline, the response rate among eligible workers who were still in their service ($N = 361$) remained high at 84%. Again, there was no differential non-response bias, with rates strictly balanced between the treatment (83%) and control (84%) groups. While response rates were symmetric, there was a slight difference in the survey administration mode at endline: treated workers were more likely to be surveyed in person rather than online or by phone (75% vs. 59%, $p = 0.003$), as the surveys were often conveniently integrated into their self-managed team meetings (see Appendix Figures 3.A.3 and 3.A.4).

It is important to clearly distinguish this survey non-response from actual sample attrition (i.e., individuals who permanently left their job or moved to another team). Between baseline and endline, 33% of enrolled workers (137 out of 414) exited the experiment, as documented

in Appendix 3.A.2. Consequently, our primary analysis sample consists of a balanced panel of 277 workers, of which 232 completed both survey waves.⁸ Attrition rates ranged from 20% to 47% across the ten home care services (see Appendix Table 3.A.1 and Figure 3.A.6). Regarding their profile, baseline characteristics of attriters and non-respondents show minimal selection bias, although those who left were slightly younger and more likely to hold fixed-term contracts than stayers (see Appendix Table 3.A.10). Because worker retention is a critical issue in the home care sector, we treat this exit behavior not merely as a sampling artifact, but as a primary behavioral outcome of the intervention. We therefore analyze these differential attrition patterns in detail, along with their implications for our estimates, when presenting the main results in Section 3.6.1.

Regarding the care recipients, 479 out of the 1,935 beneficiaries initially included in the experiment completed the baseline survey, leading to a response rate of 25% at baseline. This was balanced between the treatment and control groups (see Appendix Table 3.A.3). At endline, the response rate slightly decreased to 21% (for a total of $N = 1,641$ care recipients reached), with a marginally significant imbalance between the treatment and control groups. Directly surveying this elderly and highly dependent population is inherently challenging, as severe physical or cognitive limitations often preclude the use of standard phone or online questionnaires. Consequently, surveys were predominantly completed by their informal caregivers, whose limited availability naturally constrains participation. These figures are entirely consistent with typical response rates for internal quality assessments within the home care sector. Likewise, due to both survey non-response and the institutional placement or death of some care recipients during the study, we observe attrition in this population. Specifically, 31% ($N = 619$) of beneficiaries left the organizations between baseline and endline, resulting in a primary sample of $N = 1,316$ beneficiaries present for the full duration of the experiment. Of these, only 138 completed both the baseline and endline surveys.

3.4.3 Baseline Descriptive Statistics and Sample Representativity

Home care services characteristics. The sample reflects the diversity of organizational types within the French home care sector. According to Appendix Table 3.B.1, eight of the participating services (80%) are SAADs (*services d'aide et d'accompagnement à domicile*),

⁸In total, 186 workers exited the services during the experiment – 137 from the baseline cohort and 49 who both entered and left during the experimental period. To capture outcomes for workers who left the organization during the study, we administered a short follow-up exit survey, achieving a 9% response rate ($N = 13$).

with one Integrated Home Care and Support Services and one Home Nursing Care Service (respectively, *Services Polyvalents d'Aide et de Soins à Domicile*, SPASAD, and *Services de Soins Infirmiers à Domicile*, SSLAD) also included. In terms of legal status, the sample comprises 5 nonprofit associations (50%), 3 municipal or intermunicipal public centers (30%), and 2 for-profit private companies (20%). No mutualist organizations or nonprofit private enterprises are represented. On average, participating home care services employ 122 full-time equivalent staff members, including approximately 35 home-care workers. This heterogeneity in size and structure reinforces the need for a flexible self-managed teams implementation model and supports an analysis of heterogeneous treatment effects across different organizational contexts.

Worker profile and baseline working conditions. Baseline characteristics of worker profile are reported in Appendix Table 3.B.2. The worker sample is predominantly female (98%), with a high prevalence of part-time contracts and long tenure in the sector (average tenure: 8 years). Most respondents are aged around 46 and a majority have dependent children (55%) and hold a diploma relevant to the profession (80%). While only 12% are on fixed-term contracts (CDD), just 27% hold a full-time position, with an average workload of 30.6 hours per week.

Consistent with documented challenges in the French home-care sector, baseline data reveal limited autonomy and high exposure to psychosocial risks. Using the Karasek Job Content Questionnaire, we assessed psychological demand, decision latitude, and social support.⁹ Average scores indicate high psychological demand (21.9), limited decision latitude (67.1), and moderate social support (23.3). Consequently, 24.1% of the sample is classified as experiencing job strain (defined as high demand combined with low latitude). We also assess psychological well-being using a mental health index adapted from the five-item Mental Health Inventory (MH-5).¹⁰ The average score is 62.0 (SD 18.1), reflecting moderate mental health levels. More-

⁹The questionnaire comprises 26 items aggregated into composite scores: psychological demand (9 items), decision latitude (9 items), and social support (8 items). Responses are coded on a four-point scale and aggregated into composite scores. Following standard methodology (Karasek Jr, 1979; Niedhammer et al., 2007), we distinguish four work profiles based on median splits: "relaxed" (low demand, high latitude), "active" (high demand, high latitude), "passive" (low demand, low latitude), and "strained" (high demand, low latitude). A worker is classified as experiencing job strain when psychological demand exceeds the median and decision latitude falls below the median, representing a high-risk combination.

¹⁰Following standard MH-5 methodology, our index aggregates responses to five emotional states experienced at work: feeling tense, happy, stressed, sad, and discouraged. Negative items (tense, stressed, sad, discouraged) are reverse-coded so that higher scores reflect better well-being. Missing responses to individual items are imputed using the individual's mean across other items when at least one item is answered. The final score is

over, while general satisfaction levels were moderate to high – 7.0/10 for life and 7.1/10 for work – satisfaction with pay was low (28%).

Time-related constraints were also frequent: 40% of workers had irregular or alternating schedules, 72% worked weekends, and 25% experienced breaks of 3 hours or more in a single day. A majority reported working under time pressure, frequently rushing tasks (55%) and facing last-minute schedule changes (68%), and approximately 30% regularly skipped breaks or worked beyond their normal hours. Only 47% of workers felt able to adapt their schedule to personal needs, while 70% reported that their families felt they were not sufficiently available due to work. Over the past 12 months, the average number of days absent from work was 22. Despite these challenges, most workers expressed strong attachment to their organization: 97% intended to stay in the next year, and 79% declared willingness to remain over the next five years.

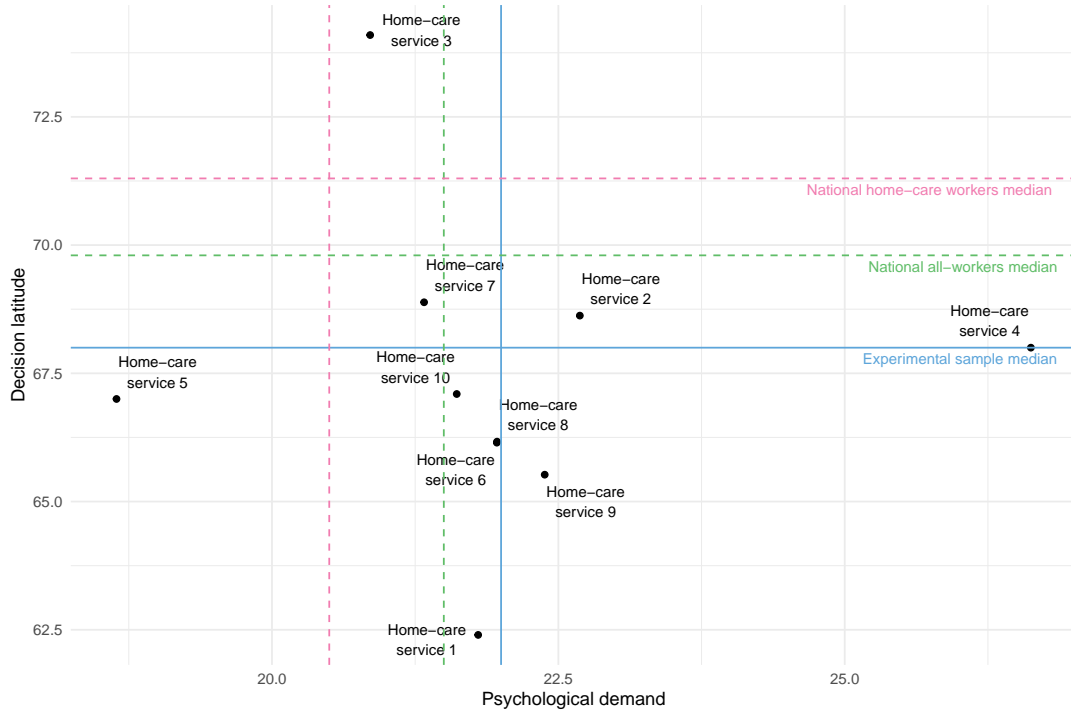
Representativeness of our sample. To assess the external validity of our findings, we compare our baseline data to national figures from the 2016–2017 SUMER survey, focusing on female home-care workers (PCS 563b). The comparisons are displayed in Appendix Table 3.B.3. Overall, our sample is broadly similar in sociodemographic characteristics: the median age is 48 (vs. 49 in SUMER), 87% of workers hold a permanent contract (vs. 86%), and only 7% are foreign nationals (vs. 8%). However, our sample is not fully representative of the sector’s institutional composition, as it overrepresents the public and non-profit sectors (which account for nearly 80% of our sample vs. 43% nationally) while underrepresenting the private for-profit sector (20% vs. 56%). Moreover, some notable differences emerge in terms of work organization and exposure to psychosocial risks.

Our sample reports higher levels of psychological demand (median score 22 vs. 20.5) and slightly lower decision latitude (68 vs. 71.3), placing a greater share of workers in the “job strain” quadrant in Figure 3.1. Exposure to work intensity also appears higher: for example, 73% of our sample say their work requires intense effort (vs. 57.5% in SUMER), and 65% report not having enough time to do their job properly (vs. 32.1%). By contrast, indicators of autonomy and social support are generally more favorable. For instance, 33% of workers report having the ability to influence how their work is organized (vs. 21.3% in SUMER), and only 14% declare lacking opportunities for cooperation (vs. 37%).

transformed to a 0–100 scale using the formula: $(\text{score} - 1) / 4 \times 100$, where 100 represents maximal well-being.

These differences suggest that our sample may not be fully representative of the home care workforce in France with respect to working conditions. This is consistent with the hypothesis that organizations joining the self-managed teams experiment may have done so to actively address mounting work-related pressures.

Figure 3.1: Baseline psychological demand and decision latitude by home-care service



Notes: This figure compares the experimental sample with data from the SUMER survey (2017), using median values. Results are shown for home-care workers and for all other employees in the national workforce.

3.4.4 Baseline Balance

Baseline balance. To assess the effectiveness of the random assignment, we test for baseline differences in socio-demographic characteristics and outcomes of interest between treatment and control groups, using the following specification estimated on the 370 completed baseline surveys:¹¹

$$Y_{ijs} = \alpha + \beta D_{ijs} + \delta_s + \varepsilon_{ijs} \tag{3.1}$$

where Y_{ijs} corresponds to the variable of interest measured at baseline for the individual i , in team j , within service s ; D_{ijs} is a binary indicator for assignment to the treatment group

¹¹Survey responses are the only source of worker-level information available to us.

(i.e., being randomly selected to join a self-managed team); and δ_s captures service fixed effects (or service-by-territory fixed effects when randomization was stratified by territory within a service). Standard errors are clustered at the level of the unit of randomization, i.e. the team (or the individuals for one service).

Table 3.1: Treatment–Control Balance of Socio-Demographic Characteristics at Baseline

Outcomes	Control group (1)	Treatment group (2)	Difference (T-C) (3)	P-value (4)	N (5)
Woman	0.990 (0.007)	0.971 (0.013)	-0.019 (0.013)	0.137	368
Age	47.006 (0.840)	46.085 (0.928)	-1.069 (0.966)	0.274	370
Number of dependent children	1.036 (0.092)	1.121 (0.090)	0.08 (0.108)	0.462	370
State-certified care professional	0.799 (0.030)	0.812 (0.031)	0.022 (0.034)	0.516	349
Seniority in the organization	8.515 (0.639)	7.578 (0.627)	-1.204* (0.637)	0.065	369
On a fixed-term contract	0.092 (0.021)	0.144 (0.027)	0.039* (0.023)	0.09	370
Has a different previous occupation	0.602 (0.035)	0.580 (0.038)	-0.024 (0.039)	0.539	370
Unemployed > 3 months	0.214 (0.029)	0.287 (0.034)	0.065** (0.030)	0.035	370
Born abroad	0.056 (0.016)	0.086 (0.021)	0.025 (0.021)	0.248	370
Parents born abroad	0.117 (0.023)	0.126 (0.025)	0.003 (0.026)	0.896	370
Test of joint significance F-stat: 1.440 (p-value: 0.193)					

Notes.

OLS estimates of baseline differences between workers in the treatment and control groups. Outcome variables are listed on the left, with the worker as the unit of observation for all variables. For each variable, we report the treatment assignment effect, with standard errors shown in parentheses. Column (1) reports the mean and standard deviation of the control group, while Column (2) reports the mean and standard deviation of the treatment group. Column (3) provides the estimated treatment assignment effect and Column (4) the associated P-value. All specifications include fixed effects for the stratification unit, and standard errors are clustered at the randomization unit, namely the team (or the individual for one structure). The last row reports the joint significance test that none of the covariates predict treatment assignment. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Regarding the socio-demographic characteristics of workers, Table 3.1 shows that the randomization procedure achieved balance on most covariates in the full baseline sample ($N = 370$). Mean differences are small and statistically insignificant for the majority of individual variables, including gender, age, and education. However, a few variables display marginal imbalances: the treatment group exhibits slightly lower seniority in the organization, a higher share of fixed-term contracts, and a higher probability of recent unemployment. Rather than isolated issues, these differences likely reflect a single, correlated "junior" profile drawn by chance. We also conduct an omnibus test of joint orthogonality by regressing the treatment indicator on the baseline covariates and testing whether all coefficients are jointly zero. The null hypothesis that none of the covariates predict treatment assignment cannot be rejected (F-statistic = 1.440; p-value = 0.193). When restricting the sample to respondents observed at both baseline and endline ($N = 232$), which is our main analysis sample to estimate treatment effects, these patterns persist, alongside an additional marginal imbalance in gender (see Appendix Table 3.A.5. To ensure our estimates are not driven by this specific

profile, we explicitly account for these pre-existing differences in our empirical analysis.

Table 3.2: Treatment–Control Balance of Main outcomes at Baseline (balanced panel)

Outcomes	Control group (1)	Treatment group (2)	Difference (T-C) (3)	N
Job satisfaction (0–10)	7.112 (0.154)	7.149 (0.154)	0.137 (0.155)	370
Working conditions	2.827 (0.047)	2.833 (0.052)	0.034 (0.055)	370
Number of work leave days	23.469 (3.721)	17.511 (3.090)	-5.835* (3.136)	370
Job strain	0.209 (0.029)	0.276 (0.034)	0.051* (0.030)	370
Mental health score	61.665 (1.270)	62.406 (1.406)	1.653 (1.198)	369

Notes.

OLS estimates from regressions of each outcome on a treatment-assignment dummy using baseline data. The sample consists of all workers who completed the baseline questionnaire ($N = 370$). Outcomes are listed in the leftmost column. Columns (1) and (2) report mean values for the control and treatment groups, respectively, with standard deviations in parentheses. Column (3) reports the estimated treatment assignment effect, and the final column shows the number of observations. All specifications include stratification-unit fixed effects, and standard errors are clustered at the randomization unit. All outcomes are expressed in their original units (natural scale).

Regarding baseline differences in the outcomes of interest, we systematically test for imbalances and find that some pre-existing differences remain between the treatment and control groups. These differences are documented in Table 3.2 for the full baseline sample ($N = 370$) and in Appendix Table 3.A.6 for the main analysis sample of respondents observed at both baseline and endline ($N = 232$). These baseline outcome differences are consistent with the marginal socio-demographic imbalances discussed previously, as workers with shorter tenure and more precarious contracts are likely to have accumulated fewer work leave days over the past year and exhibit slightly higher initial job strain. We take these baseline differences fully into account in our empirical strategy, as described in Section 3.5.3, to ensure they do not confound our treatment effect estimates.

3.5 Empirical Strategy

3.5.1 First Stage: Effective Implementation of Local Self-managed Teams

To assess whether treatment assignment resulted in effective implementation, we estimate the specification in Equation 3.1 using various dimensions of the self-managed team model as dependent variables. This analysis relies on endline responses from individuals enrolled at baseline, irrespective of whether they responded to the initial survey ($N = 243$). Table 3.1 shows a strong correspondence between random assignment and the actual adoption of the self-managed teams model. At endline, 97% of baseline workers in the treatment group reported being part of a self-managed team, compared with 35% in the control group, a difference that is large and statistically significant ($p < 0.001$).

Treated workers also reported significantly higher exposure to the key components of the self-managed teams model. Specifically, 66% indicated that their team managed its own schedules (versus 29% in the control group, $p < 0.001$), 43% were involved in managing logistics (versus 22%, $p < 0.001$), and 53% participated in self-managed teams-related training (versus 30%, $p < 0.001$). Autonomy over care planning was also more frequent in the treated group (31% versus 19%, $p < 0.001$). However, some components, particularly the use of digital tools and the involvement of the care recipient's informal caregivers, appear to have been only weakly implemented or not implemented at all, as evidenced by the similar response patterns observed across both groups. Overall, these results confirm that treatment assignment led to substantial differences in exposure to the self-managed teams model between the treatment and control groups, validating the first stage of the intervention.¹²

Yet these differences were not perfectly aligned with the initial random assignment. As is common in field experiments, partial non-compliance occurred: some workers assigned to the control group were exposed to the self-managed model, while some in the treatment group were not. Indeed, Table 3.1 reveals that about one-third of workers in the control group (35%) reported belonging to a self-managed team at endline. In contrast, nearly 3% of workers assigned to the treatment group did not report being in a self-managed team at endline. We provide a detailed documentation and analysis of these cases of non-compliance in the next

¹²Results including new entrants (i.e. individuals who joined after baseline) are reported in Appendix Table 3.C.3 ($N = 303$). New entrants were integrated into pre-formed teams, with home-care services indicating the team to which each entrant was assigned.

Table 3.1: First-stage (without new entrants)

Outcomes	Control group (1)	Treatment group (2)	Treatment effect (3)	N
Is in an autonomous team	0.352 (0.047)	0.974 (0.015)	0.616*** (0.078)	220
Works in a team	0.472 (0.045)	0.858 (0.032)	0.371*** (0.053)	243
Planning: not by hierarchy	0.017 (0.012)	0.059 (0.022)	0.044* (0.025)	237
Planning: not by hierarchy or almost always modifiable	0.336 (0.043)	0.542 (0.046)	0.206** (0.081)	237
Works in a team managing its schedules	0.287 (0.044)	0.661 (0.045)	0.367*** (0.060)	220
Works in a team deciding its care plans	0.190 (0.039)	0.315 (0.045)	0.153*** (0.048)	213
Works in a team managing logistics	0.238 (0.042)	0.431 (0.048)	0.208*** (0.053)	214
Use of digital tools	0.226 (0.076)	0.378 (0.057)	0.126 (0.097)	105
Solicitation of the user's entourage	0.114 (0.029)	0.083 (0.025)	-0.034 (0.029)	243
Has participated in autonomous team training	0.301 (0.043)	0.538 (0.049)	0.232*** (0.071)	217

Notes.

OLS estimates from regressions of each outcome on a treatment-assignment dummy using baseline data. Outcomes are listed in the leftmost column. Columns (1) and (2) report mean values for the control and treatment groups, respectively, with standard deviations in parentheses. Column (3) reports the estimated treatment effect, and the final column shows the number of observations. All specifications include stratification-unit fixed effects, and standard errors are clustered at the randomization unit.

subsection.

3.5.2 Non-compliance

To investigate the nature of non-compliance, we cross-referenced workers' survey responses with the organizations' staff monitoring files. These files tracked every entry, exit, and team transfer with exact dates and motives, allowing us to objectively measure non-compliance with treatment assignment. It is crucial to distinguish between non-compliance based on the endline self-reported status (where 17% of the sample reported a status differing from their original assignment), which may reflect subjective misperceptions, and the actual organizational switching of workers across teams.

Field constraints and early switching. Overall, most teams complied with their assigned treatment status. Monitoring data (summarized in Appendix Table 3.C.1) reveal that partial non-compliance was primarily driven by two objective factors. First, unavoidable field constraints, such as employee relocations, successive resignations leading to understaffing, internal promotions, or interpersonal conflicts, forced organizations to occasionally adjust team compositions. These isolated adjustments account for a small share of non-compliance, involving 16 workers across three home-care services according to the turnover files completed by the organizations.

Second, the primary source of non-compliance was “early switching”, where several control teams transitioned to the self-managed model ahead of schedule. In several instances, organizations eager to implement the new model did not promptly report these transitions to the research team. Consequently, some control-assigned workers—specifically most control teams in Service 8 (49 workers) and the entire control group in Service 9 (11 workers)—completed the endline questionnaire one to four months after the organizational change had already begun. This discrepancy was further exacerbated by a subset of questionnaires being completed late by phone or online, sometimes after the new implementation had started (see Appendix Figure 3.C.1). Importantly, however, these control teams that eventually switched status still remained compliant with their original assignment for the vast majority of the experimental period, ensuring a significant difference in cumulative exposure between the groups.

To quantify this, we computed the duration of exposure to the “wrong” treatment status using the precise dates of organizational changes reported by the management staff. As summarized in Appendix Table 3.C.2, even for these “early switchers” the effective duration of non-compliance was relatively short, averaging about 80 days before the final data collection. Overall, when considering the entire experimental volume (total person-days), the global non-compliance ratio accounts for only 3.31% of total person-days from randomization to endline (and 4.82% from the start of training to endline). This confirms that the vast majority of the study period was spent in full compliance with the initial assignment.

Operational constraints and terminology. For the 3% of treated workers who reported not being in a self-managed team, the discrepancy likely stems from operational constraints preventing full implementation, the isolated individual changes described above, or a misunderstanding of the definition. Indeed, several respondents who answered “no” to the question “*Are you part of a self-managed team?*” nevertheless reported key features of the model, such as self-managed scheduling, frequent team meetings, or autonomy over care plans. This suggests potential confusion regarding terminology rather than a lack of actual exposure to the organizational change.¹³

¹³This interpretation is reinforced by the monitoring files, which showed fewer official status change for these treated workers, as seen by comparing Appendix Table 3.C.1 and Figure 3.C.1. The discrepancy appears driven by subjective perception rather than actual assignment shifts.

Characteristics of non-compliers. To better understand non-compliance, we compare the characteristics of “compliers” (workers whose self-reported status at endline matched their assignment) and “non-compliers” (those whose self-report diverged). As shown in Appendix Tables 3.C.4 and 3.C.5, non-compliers tend to be slightly older, more frequently employed on fixed-term contracts, and less likely to have long tenure in the organization. Multivariate regressions confirm that these factors are modest predictors of non-compliance, and the overall explanatory power of observable characteristics remains limited (Appendix Table 3.C.6).

Importantly, compliers and non-compliers differ in their actual exposure to self-managed practices (Appendix Table 3.C.7). As expected, treated compliers report high levels of autonomy in scheduling (67%), care planning (34%), and logistics (44%). In contrast, control non-compliers (those who report being in a self-managed team despite their control assignment) also report substantial autonomy (e.g., 56% for scheduling), though somewhat less consistently. This confirms their exposure to the model through deviations. Conversely, treated non-compliers (those assigned to treatment but not exposed) report very low levels of self-managed teams features, consistent with operational delays or terminological confusion about the team’s status.

Furthermore, non-compliance is more common in larger services but is not clearly associated with structure type (public vs. private) or baseline workforce composition (Appendix Table 3.C.8). This supports the interpretation that deviations from treatment assignment were driven by managerial decisions or operational constraints rather than systematic baseline differences between treated and control units. We perform robustness checks in Section 3.6.3 by excluding teams individually to ensure our results are not driven by non-compliant units.

Spillover effects. Finally, since randomization occurred at the team level within services, cross-arm interactions could theoretically generate spillover effects, potentially explaining some of the observed non-compliance. However, survey evidence indicates limited contamination: only 20% of control-group workers reported attempting to adopt elements of the self-managed model independently. This suggests that spillovers are unlikely to be significantly biasing our estimates (see Appendix 3.C.1).

3.5.3 Estimation Models

Given partial non-compliance with treatment assignment, we estimate *intention-to-treat* (ITT) effects, which capture the average causal impact of being *assigned* to the self-managed teams model, regardless of whether it was actually implemented. This approach preserves the internal validity of the experiment, since treatment assignment was randomized and thus exogenous by design. As shown in Section 3.5.1, assignment to the treatment group strongly predicts actual exposure to self-managed teams.

Our main identification strategy relies on an Analysis of Covariance (ANCOVA) estimation that conditions on the baseline level of the outcome when available, following McKenzie (2012):

$$Y_{ijs} = \alpha + \beta D_{ijs} + \rho Y_{is0} + X'_{ijs} \gamma + \delta_s + \varepsilon_{ijs} \quad (3.1)$$

where Y_{ijs} denotes the outcome of interest for individual i , in team j , within service s ; D_{ijs} is a binary indicator equal to one if the individual (or its team) was randomly assigned to the treatment group (i.e., selected to join a self-managed team); Y_{is0} denotes the baseline value of the outcome variable; and X'_i is a vector of baseline sociodemographic characteristics included to control for chance imbalances in sample composition (gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and self or parents born abroad for care workers; age and living alone for care recipients). The term δ_s captures home-care service fixed effects (or service-by-territory fixed effects when randomization was stratified at that level).

As shown in Section 3.4.4, workers' baseline outcomes differ across groups by chance. The ANCOVA specification directly addresses these pre-treatment differences by conditioning on baseline outcomes, while providing significantly more precise estimates than the more common difference-in-differences (DID) specification (McKenzie, 2012). This efficiency gain is particularly relevant when the autocorrelation of outcomes (i.e., the correlation between baseline and follow-up measurements) is low.¹⁴ By using ANCOVA, we maximize our ability to detect meaningful treatment effects despite the relatively small sample sizes. As a robustness check, we also estimate a DID specification that uses baseline responses as a reference point and accounts for pre-treatment differences by focusing on changes over time, using

¹⁴While DID estimation subtracts the baseline value from the follow-up, essentially "over-correcting" for differences that may not have strong predictive power, ANCOVA adjusts the degree of correction based on the actual correlation (ρ) observed in the data. For economic and well-being outcomes, which often exhibit low autocorrelations, ANCOVA offers a substantial boost in statistical power.

either service \times time fixed effects or individual fixed effects.

All specifications are estimated on a balanced panel of workers (or care recipients) who completed both baseline and endline questionnaires. To facilitate the interpretation of results and comparison across outcomes measured on different scales, we present treatment effects both in original units and as standardized effect sizes in standard deviation (SD) units. For the latter, we standardize all outcome variables to have a mean of zero and a standard deviation of one.¹⁵

Standard errors are clustered at the unit of randomization – typically the team level (or the individual level for one structure that opted for individual-level assignment) – to account for potential correlation in outcomes among members of the same team. All treatment effects are reported with conventional p -values. Beyond our primary pre-registered outcomes, our questionnaires provide a range of secondary indicators. To account for the risk of false positives inherent in multiple hypothesis testing, we adjust p -values by controlling the False Discovery Rate (FDR). Specifically, we report sharpened two-stage q -values following the step-up procedure established by [Benjamini and Hochberg \(1995\)](#) and refined by [Benjamini et al. \(2006\)](#) and [Anderson \(2008\)](#). This correction is applied separately within each outcome family (e.g., satisfaction measures, Karasek subscales, etc.), where the correction factor n is defined as the number of outcomes displayed in the corresponding table.

3.6 Results

3.6.1 Main results on care workers

Table 3.1 presents intention-to-treat (ITT) estimates from the ANCOVA specification for our five pre-registered primary outcomes for care workers: job satisfaction, working conditions, absenteeism, job strain, and mental health. Columns (1) and (2) report results in original units, while Columns (3) and (4) present estimates for outcomes standardized to have mean zero and standard deviation one, allowing for comparability of effect sizes. Overall, the introduction of the self-managed teams model significantly improved key dimensions of job quality among home care workers.

¹⁵For the ANCOVA specification, we standardize using the endline mean and standard deviation only. For the difference-in-differences specifications, we standardize using the pooled mean and standard deviation across both baseline and endline periods.

Table 3.1: Main outcomes for care workers

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Overall job satisfaction	7.27 (2.06)	0.490* (0.249)	0.592** (0.246)	0.238* (0.121)	0.287** (0.119)
Satisfaction with working conditions	2.90 (0.62)	0.257*** (0.084)	0.299*** (0.090)	0.414*** (0.134)	0.480*** (0.145)
Number of days on leave	21.59 (42.60)	-11.826** (4.765)	-11.419** (4.554)	-0.278** (0.112)	-0.268** (0.107)
Job strain	0.23 (0.43)	-0.108** (0.040)	-0.139*** (0.048)	-0.252** (0.094)	-0.326*** (0.111)
Mental health score	61.67 (18.61)	4.417** (1.765)	4.590** (1.831)	0.237** (0.095)	0.247** (0.098)
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Job satisfaction and working conditions. Assignment to the self-managed teams model significantly improved workers' overall job satisfaction. The estimate ranges from an increase of 0.24 standard deviations ($p < 0.10$) without controls to 0.29 standard deviations ($p < 0.05$) when including baseline sociodemographics (Table 3.1). Similarly, treated workers reported substantially better working conditions, with estimates of 0.41 to 0.48 standard deviations ($p < 0.01$).

The improvement in job satisfaction appears to be driven primarily by better working conditions rather than other dimensions such as pay or workplace atmosphere (Appendix Table 3.D.1). Decomposing this aggregate measure reveals that treated workers were significantly more satisfied with their working hours and reported greater pride in working for their organization (Appendix Table 3.D.2). They were also less likely to work with the fear of losing their job. These improvements in subjective well-being represent meaningful gains for a sector characterized by low job quality and high turnover.

Absenteeism. The self-managed teams model also reduced the number of days workers spent on leave. Table 3.1 shows a decline of 0.27 to 0.28 standard deviations ($p < 0.05$). Disaggregated results suggest this effect is driven primarily by fewer days off due to work accidents, while sick leave remains unchanged (Appendix Table 3.D.3). Treated workers were also less likely to report frequent absences due to personal emergencies (Appendix Table 3.D.4). Given that high absenteeism was one of the sector's key challenges motivating the intervention, this reduction represents an important improvement.

Job strain. Treated workers experienced significant reductions in job strain following assignment to the self-managed teams model. The estimates in Table 3.1 indicate a decrease ranging from 0.25 ($p < 0.05$) to 0.33 standard deviations ($p < 0.01$) depending on the inclusion of baseline sociodemographic controls. As described in Section 3.4.3, job strain is measured using the Karasek Job Content Questionnaire and defined as the combination of high psychological demand and low decision latitude, representing a high-risk work situation. Breaking down the three Karasek dimensions reveals that the reduction in job strain was driven primarily by lower psychological demands and increased social support, while decision latitude remained surprisingly largely unchanged (Appendix Table 3.D.5).

The decline in psychological demand reflects multiple improvements in work organization (Appendix Table 3.D.6). Treated workers were less likely to report being asked to do an excessive amount of work, more likely to feel they had enough time to complete tasks properly, and reported that their work was less hectic. They also experienced fewer task interruptions, contributing to a more manageable workload. Improved social support manifested in stronger relationships with both supervisors and colleagues (Appendix Table 3.D.7). Treated workers reported that their colleagues showed greater interest in them and were more helpful in completing tasks. They also perceived their supervisors as better at fostering cooperation and more supportive in helping them complete their work. While decision latitude as a composite measure did not change significantly, one specific component showed a large effect: treated workers were substantially more likely to report that their work required creativity (Appendix Table 3.D.8). This suggests that the self-managed teams model may have enhanced certain dimensions of autonomy even if overall decision-making authority remained constrained.

Mental health. Mental health scores also improved significantly in the treatment group. Table 3.1 shows improvements of approximately 0.24 to 0.25 standard deviations ($p < 0.05$). This composite index is based on the Mental Health-5 (MH-5) subscale, which measures psychological distress through five items asking workers how often they felt tense, happy, stressed, sad, and discouraged at work. Responses are coded on a five-point scale (never to almost all the time) and combined into a 0–100 score, with higher values indicating better mental health. Treated workers notably reported feeling significantly happier, less stressed, less sad, and less discouraged at work (Appendix Table 3.D.9). These effects indicate that the organizational changes introduced by the self-managed teams model reduced work-related stress and improved psychological well-being, addressing core dimensions of job quality in a high-strain occupation.

Work organization and scheduling. Beyond our main pre-registered outcomes, several important secondary outcomes also emerge from the estimation. We first observe that treated workers were significantly less likely to report having long breaks (split shifts) of three hours or more during their workdays (Appendix Table 3.D.10). We also constructed a more objective proxy for workday structure based on the reported number of hours worked, time spent in transport, and total daily amplitude. Although these precise temporal measures were challenging for workers to estimate with accuracy due to high daily schedule variation and the difficulty of precisely quantifying effective travel and work time, the reconstructed daily "Dead Time" (the gaps between interventions and travel times) aligns with the reported reduction in split shifts. While not statistically significant, this idle time fell by 27 minutes per day on average for the treatment group (Appendix Table 3.D.10). This optimization of the workday is further reflected in an "efficiency ratio" increase, suggesting a more compact workday where daily amplitude decreased by 13.5 minutes despite a slight increase in estimated total daily work time (+23.5 minutes) (Appendix Table 3.D.11).

Treated workers also experienced greater flexibility in adapting their work hours to personal constraints, a critical dimension in a sector where irregular schedules are major sources of dissatisfaction (Appendix Table 3.D.12). Consistently, they reported a better work pace and reduced time pressure (Appendix Table 3.D.13). They were less likely to have tasks interrupted before completion, and more likely to have at least two consecutive days off

per week. Finally, treated workers were significantly less likely to rush through tasks that required more time and care due to workload pressure (Appendix Table 3.D.14).

Quality of care. Importantly, improvements in worker well-being did not come at the expense of care quality. On the contrary, treated workers reported being more able to give beneficiaries enough time, better able to meet their expectations, and expressed greater satisfaction with the care they provided (Appendix Table 3.D.15). From the workers' perspective, this suggests that the self-managed teams model succeeded in aligning the interests of staff and beneficiaries, improving both job quality and perceived care quality simultaneously. However, these assessments should be put into perspective with the results reported by care recipients in the following section, which appear more nuanced.

Worker Retention and Differential Attrition. In a sector characterized by chronic turnover and recruitment difficulties, the decision to remain in one's position is perhaps the most consequential behavioral measure of job quality and satisfaction. As a primary objective outcome of this experiment, we find that the intervention significantly improved worker retention. Overall, workers in the treatment group were 7.8 percentage points less likely to leave their team or service than those in the control group (29% vs. 37%, $p < 0.10$, see Appendix Table 3.A.9). This reduction was driven primarily by fewer voluntary departures (such as retirements) and fewer administrative transfers to non-experimental teams, rather than by differences in involuntary separations or sick leave. This suggests that the intervention effectively increased the "threshold" for exiting the organization: by improving day-to-day autonomy and working conditions, the model retained workers who might have otherwise left due to organizational friction.

This selective attrition has direct implications for our econometric estimates. Endline comparisons confirm that the decision to exit is strongly linked to well-being. Regardless of treatment status, leavers systematically reported significantly lower job satisfaction and organizational trust than stayers at baseline and endline (see Appendix Tables 3.A.11 and 3.A.12). From an estimation standpoint, this differential attrition therefore introduces a downward bias that actually reinforces the strength of our findings. Because the control group experienced higher attrition, it disproportionately lost its least satisfied members. This "survival" of the most satisfied workers mechanically inflates the control group's average well-being at

endline. Consequently, our Intention-to-Treat (ITT) estimates are artificially compressed and represent a conservative *lower bound* of the true positive impact of the self-managed teams model.

3.6.2 Main results on care recipients

Table 3.2 presents ITT estimates from ANCOVA for the four primary outcomes for care recipients: physical health, mental health, overall satisfaction, and care time adequacy, as reported by the care recipients themselves or their informal caregivers. Overall, the implementation of the self-managed teams model slightly improved the perceived adequacy of care time, while broader well-being measures remained unchanged.

Table 3.2: Main outcomes for care recipients

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Current physical health	2.77 (0.78)	0.136 (0.117)	0.120 (0.118)	0.175 (0.150)	0.155 (0.151)
Mental health index (MH5)	41.14 (22.08)	-2.099 (2.375)	-2.154 (2.533)	-0.095 (0.108)	-0.098 (0.115)
Overall satisfaction	3.02 (0.71)	0.002 (0.086)	0.012 (0.092)	0.003 (0.121)	0.017 (0.130)
Care time adapted to needs	3.14 (0.76)	0.271** (0.128)	0.246** (0.121)	0.356** (0.169)	0.323** (0.159)
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		120	120	120	120

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Health and professional evaluation. The impact of assignment to the self-managed teams model on care recipients' general health appears mixed. While the composite mental

health index and physical health showed no significant changes (Table 3.2), disaggregated results reveal a marginally significant positive effect on happiness, with an increase of 0.24 to 0.25 standard deviations (Appendix Table 3.D.19).¹⁶ Evaluations of professionals—including relationship quality, availability, and listening capacity—as well as maltreatment indicators, remained stable and positive across both groups (Appendix Tables 3.D.20 and 3.D.21).

Care adequacy, organization and satisfaction. Regarding the perceived quality of service, the intervention significantly improved care recipients' perception of the adequacy of care time. The estimate shows an increase of 0.355 standard deviations ($p < 0.05$), which remains robust at 0.323 SD ($p < 0.05$) when controlling for age and living arrangements (Table 3.2). However, this improvement has not translated into a significant increase in overall satisfaction with the service (Appendix Table 3.D.22). From the recipients' perspective, the transition did not detectably alter continuity of care or schedule stability either (Appendix Table 3.D.23).

Convergence with workers' perception. Importantly, these findings are consistent with the perceptions reported by the workers themselves. Treated workers reported being significantly more able to give beneficiaries enough time, better able to meet their expectations, and expressed greater satisfaction with the care they provided (Appendix Table 3.D.15). This convergence suggests that the self-managed model succeeded in aligning the interests of staff and beneficiaries regarding daily care delivery. However, while workers felt they had drastically improved overall care quality, this perception is more nuanced when put into perspective with the recipients' reports, which show stability rather than structural transformation.

Interpretation of Null Effects. The general absence of significant effects on care recipients' broader health and satisfaction must be interpreted with caution. First, statistical power is severely limited by the small size of the balanced panel ($N = 138$). This sample size reflects cumulative attrition resulting from low response rates, high item non-response when surveys were completed by informal caregivers rather than the recipients themselves, and

¹⁶Heterogeneity analysis suggests that this lack of impact on mental health may mask divergent patterns based on care intensity. While no effect is found for high-frequency recipients, those with fewer than five visits per week report lower mental health scores (see Appendix Table 3.D.26), which may reflect the destabilizing effect of organizational transitions on recipients with the fewest points of contact. However, given the very small size of this subgroup ($N = 39$), these results are merely suggestive.

the natural turnover due to death or institutionalization within this population. Second, the 12-to-18-month timeframe of the experiment may simply be too short to observe structural improvements in the physical or mental health trajectories of highly vulnerable individuals, or broader improvements in care delivery.

At the same time, these precise null effects on care quality provide a crucial and reassuring finding. In organizational economics, decentralizing task allocation and reducing managerial oversight carries the theoretical risk of deteriorating service quality, especially if workers require strict guidance. Our results confidently reject this hypothesis. They demonstrate that self-organization in the home care sector is not detrimental to care recipients. On the contrary, the stability of service quality, coupled with the significant improvement in the perceived adequacy of care time, suggests a "win-win" scenario or, at the very least, a strict Pareto improvement where workers' well-being increases at no cost to the beneficiaries.

3.6.3 Robustness

We conduct several robustness checks to assess the sensitivity of our main findings regarding care workers to alternative econometric specifications and sample compositions.

Alternative specifications. As a first robustness check, we estimate a pseudo difference-in-differences (DID) specification that uses baseline responses as a reference point and accounts for pre-treatment differences by focusing on changes over time:

$$Y_{ijst} = \alpha + \beta(D_{ijs} \times Post_t) + X_i' \gamma + \delta_{st} + \varepsilon_{ijst} \quad (3.1)$$

where $Post_t$ is a post-intervention time indicator and δ_{st} captures service \times time fixed effects (or service-by-territory \times time fixed effects when randomization was stratified at that level). We estimate this specification with and without baseline sociodemographic controls (Columns (1) and (2) of Table 3.3). We also estimate a DID specification replacing service \times time fixed effects δ_{st} and individual controls X_i with worker fixed effects α_i , which controls for all time-invariant unobserved heterogeneity and more closely resembles a pure difference-in-differences design (Column (3)). Results across these three specifications confirm the stability of our main estimates: the coefficients for working conditions, job strain, and mental health remain positive and significant at the 5% or 1% level, with magnitudes closely following those

of our main ANCOVA model.

Second, given the small number of clusters, we assess the robustness of our inference by re-estimating our preferred ANCOVA specification using randomization inference (RI). Rather than relying on asymptotic approximations, this approach generates placebo treatment assignments and compares the observed treatment effect to the distribution of effects obtained under random permutations. This approach yields a randomization p -value: the probability, under the null hypothesis of no treatment effect, of observing a statistic as extreme as (or more extreme than) the one actually observed, which is robust to small-cluster concerns (Column (4)). The RI p -values (reported in brackets) remain below conventional significance thresholds for four out of five main outcomes. Notably, the effect on satisfaction with working conditions ($p = 0.029$), number of days on leave ($p = 0.049$), and job strain ($p = 0.038$) are highly robust to this more conservative inference method.

Sample restrictions. We examine the sensitivity of our findings to specific sample composition choices by excluding late endline responses. We define these as surveys completed more than 30 days after the on-site data collection wave to mitigate concerns that control workers might have already transitioned to the self-managed model, which could attenuate treatment effects (Columns (5) and (6)). Excluding these responses actually tends to increase the magnitude of our estimates, particularly for mental health (from 0.237 in the full sample to 0.359 in Column 6) and satisfaction with working conditions (reaching 0.502). This suggests that our main results might be slightly conservative due to potential treatment diffusion over time in the control group.

Leave-one-out analysis. To verify that our results are not driven by any single service, we perform a leave-one-out analysis by re-estimating our preferred specification while successively excluding each of the ten services from the sample (see Appendix Table 3.D.27). The estimates remain stable across all ten sub-samples, with the main effects on overall satisfaction, working conditions, and the number of days absent consistently significant and of comparable magnitude. We observe slightly less significant results when home care service 8 (the largest structure in our sample) is removed, although the magnitude and sign of the effects remain consistent. Extending this analysis to the team level, we exclude each of the individual teams one by one (see Appendix Figure 3.D.1). The point estimates remain

remarkably stable both in terms of magnitude and significance. This confirms that our findings reflect a broad organizational shift rather than the idiosyncratic influence of specific local teams.

Attrition. To assess whether sample attrition affects our conclusions (documented in Appendix Section 3.A.2), we augment the balanced panel with responses from workers who exited the services during the experiment but nevertheless completed our short exit survey, thereby providing both baseline and endline outcomes. Among the 28 workers who completed the leaver questionnaire, only 13 provided non-missing responses for both baseline and endline outcomes, limiting the scope of this analysis. We nonetheless include these 13 leavers in an augmented sample ($N = 245$) and re-estimate our preferred specification. As shown in Appendix Table 3.D.28, results are virtually unchanged, indicating that selective attrition does not meaningfully bias our estimates.

In conclusion, across all specifications and sample restrictions, the main findings remain substantively unchanged and statistically significant. The effects on satisfaction with working conditions and job strain are particularly robust, remaining significant even in the most conservative RI inference and the most restrictive sample filters. This consistency confirms that the observed improvements in job quality are a robust consequence of the transition to self-managed teams rather than an artifact of modeling or sample selection.

Table 3.3: Main Outcomes: Robustness Specifications

Model	Pseudo DID		DID	ANCOVA RI Inference	ANCOVA wo/ late responses	
	(1)	(2)	(3)	(4)	(5)	(6)
Overall job satisfaction	0.277** (0.126)	0.358*** (0.115)	0.334** (0.158)	0.238 (0.121) [0.155]	0.225 (0.153)	0.323** (0.155)
Satisfaction with working conditions	0.547*** (0.130)	0.585*** (0.136)	0.552*** (0.173)	0.414** (0.134) [0.029]	0.414** (0.188)	0.502** (0.203)
Number of days on leave	-0.091 (0.159)	-0.125 (0.181)	-0.059 (0.212)	-0.278** (0.112) [0.049]	-0.203* (0.118)	-0.247** (0.116)
Job strain	-0.295** (0.122)	-0.342** (0.132)	-0.262* (0.155)	-0.252** (0.094) [0.038]	-0.159* (0.094)	-0.240* (0.122)
Mental health score	0.202** (0.095)	0.236** (0.093)	0.246* (0.139)	0.237* (0.095) [0.080]	0.339** (0.144)	0.359** (0.158)
Fixed Effects	Service × Time		Individual	Service	Service	Service
Controls	No	Yes	No	No	No	Yes
Observations	464	414	464	232	170	149

Notes: Intention-to-treat (ITT) estimates with outcomes in standardized units (SD). Columns (1) and (2) report a pseudo difference-in-differences (DID) specification with service *times* time fixed effects. Column (3) reports a DID specification with worker fixed effects. Columns (4) to (8) report ANCOVA specifications (endline outcome regressed on treatment assignment and the baseline value of the outcome). Column (4) reports our preferred ANCOVA specification where p-values (in brackets) and significance stars are calculated using randomization inference (RI) to account for the small number of clusters. Columns (5) and (6) exclude late responders, defined as workers who completed the endline survey more than 30 days after the on-site data collection. Columns (2) and (6) additionally include the following baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers. Standard errors in parentheses are clustered at the team level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

3.7 Conclusion

This paper provides some of the first quantitative evidence on the causal effects of self-managed teams in the French home care sector. Using a randomized controlled trial across ten services, we find that the model significantly improves worker outcomes, particularly regarding working conditions, job strain, and mental health. For care recipients, the intervention notably increased the perceived adequacy of care time, suggesting better service customization, though these gains have not translated into broader improvements in health or satisfaction. Our findings offer important insights for policy in a sector facing critical labor shortages, high turnover, and rising demand due to an aging population. The self-managed team model demonstrates that organizational innovation can meaningfully address structural challenges in home care, enhancing worker well-being and retention while maintaining service quality.

Several key analyses to complement these findings remain to be completed in the near term. First and foremost, we plan to reinforce the robustness of our findings with additional tests. Second, we plan to examine heterogeneity in treatment effects to identify where and for whom the model works best, analyzing variation by baseline organizational constraints (such as initial job strain levels), worker characteristics (tenure, contract type, team composition), and structural features (service size, management style, implementation timeline). Relatedly, we aim to investigate the mechanisms underlying these effects using detailed data on actual implementation practices, collective engagement and team dynamics, and managerial support. Preliminary qualitative evidence suggests that implementation success may differ substantially between urban and rural contexts.

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Appendix to Chapter 3

The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment

3.A Design of the experiment

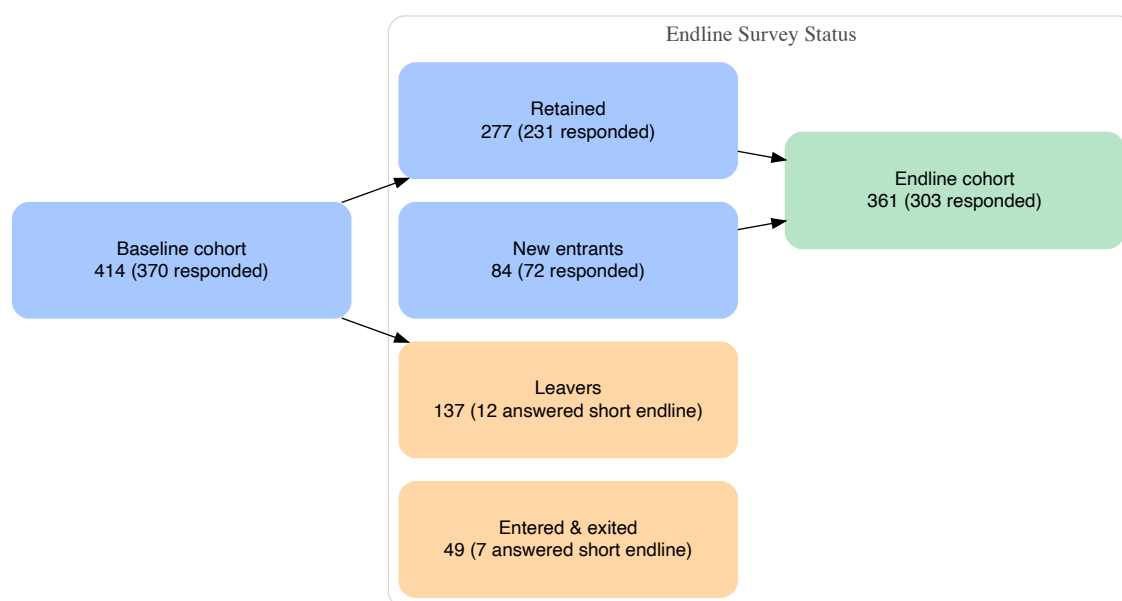
Table 3.A.1: Experimental design characteristics

Department	Home-care Service	Randomization unit	# teams	# workers at baseline	# baseline workers observed at endline	Attrition rate (%)	# workers at endline	# treated units	Stratified randomization	# teams per stratum	# treated teams per stratum	# treated workers at baseline	# treated workers at endline
Eure	Service 1	Team	2	21	13	38.1	14	1				11	9
	Service 2	Team	2	16	11	31.2	12	1				8	5
Haut-Rhin	Service 3	Team	2	22	15	31.8	17	1				9	9
Oise	Service 4	Team	2	18	13	27.8	21	1				9	10
	Service 5	Team	2	16	8	50	12	1				8	5
Pas-de-Calais	Service 6	Team	4	27	18	33.3	27	2	Area	2	1	13	15
	Service 7	Team	6	76	40	47.4	69	3	Area	2	1	35	41
	Service 8	Team	16	154	109	29.2	127	8	Area	2	1	71	65
	Service 9	Team	2	15	13	13.3	18	1				7	7
		Worker	2	10	8	20	9	7				7	7
	Service 10	Area	2	38	29	23.7	35	1				14	14
Total			42	413	277	31 (mean)	361	27		6	3	192	187

Notes: This table summarizes the experimental design and sample characteristics across participating home-care services. Service names have been anonymized. Counts are based on the randomization (draw) files and therefore exclude one worker who withdrew prior to treatment assignment but later rejoined the experiment, explaining why the total number of workers at baseline is 413 rather than 414.

Figure 3.A.1 illustrates the flow of individuals through the experiment and the corresponding response rates to the baseline and endline surveys. At baseline, 414 workers were surveyed, of whom 370 completed the questionnaire, yielding a response rate of 89%. By the time

Figure 3.A.1: Surveyed workers and response rates



NOTES: Yellow boxes indicate participants targeted by a distinct survey instrument, even though some answered to the main endline survey.

of the endline survey, 361 workers were still part of the experiment. This group included 277 from the original cohort – implying an attrition rate of 33% – along with 84 new entrants who had joined during the study period. Of the 361 workers present at endline, 303 responded to the questionnaire (response rate: 84%). In practice, however, the number of responses collected was slightly higher, as 19 individuals who were initially thought to have exited the experiment still participated, bringing the total to 322 completed endline questionnaires. This occurred either because they were mistakenly contacted while on medical leave or while in the process of leaving the service. Figure 3.A.2 shows the endline survey response rate for each of the ten home care services included in the experiment, and indicates that survey errors were concentrated in six of the ten structures.

For the main analysis, we restrict the sample to the balanced panel of workers present in both waves (277), of whom 223 completed both the baseline and endline questionnaires, since we use DiD regressions that rely on responses from both time points. In addition, we include the responses from the 8 workers classified as exiters who nonetheless completed both surveys. According to management, these workers had either been transferred outside the experiment, were on parental leave (2 cases), had resigned (1 case), left due to issues related to the autonomous team (1 case), or were on long-term sick leave (3 cases). We choose to retain their responses, yielding a balanced panel of 231 individuals with observations in both waves, which constitutes the core sample for estimating treatment effects. The main analysis therefore focuses on this balanced panel and excludes the 84 entrants.

Table 3.A.2: Workers' response rates by treatment assignment at baseline and endline

Panel A: Baseline				
	Overall N = 414	Control, N = 221 (53%)	Treatment, N = 193 (47%)	p-value
Response rate	370 (89%)	196 (89%)	174 (90%)	0.6
In-person response	171 (46%)	93 (47%)	78 (45%)	0.6
Panel B: Endline				
	Overall N = 361	Control, N = 174 (48%)	Treatment, N = 187 (52%)	p-value
Response rate	303 (84%)	145 (83%)	158 (84%)	0.8
In-person response	205 (68%)	86 (59%)	119 (75%)	0.003
<i>Including 19 returnees surveyed later</i>				
Response rate	322 (85%)	155 (84%)	167 (85%)	0.8
In-person response	212 (66%)	88 (57%)	124 (74%)	<0.001

¹ n (%)² Pearson's Chi-squared test

Notes: Response rates computed among the sample of workers present in each wave. The two additional endline rows report response rates when expanding the endline-eligible sample to include 19 workers initially recorded as having exited the experiment but later surveyed at endline.

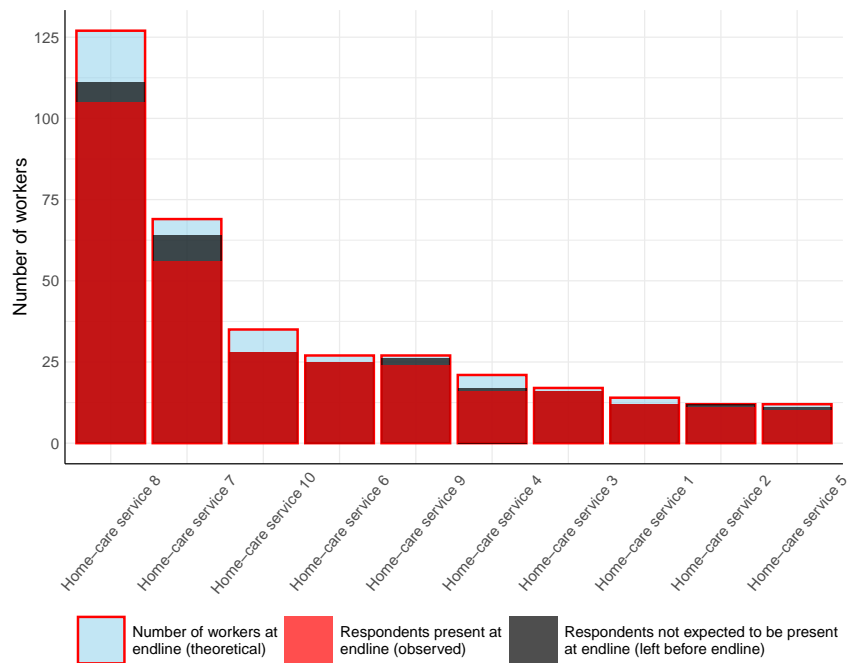
Table 3.A.3: Care recipients' response rates by treatment assignment at baseline and endline

Panel A: Baseline				
	Overall N = 1935	Control, N = 1116 (58%)	Treatment, N = 819 (42%)	p-value
Response rate	479 (25%)	242 (22%)	237 (29%)	<0.001
Panel B: Endline				
	Overall N = 1641	Control, N = 910 (55%)	Treatment, N = 731 (45%)	p-value
Response rate	342 (21%)	167 (18%)	175 (24%)	0.006

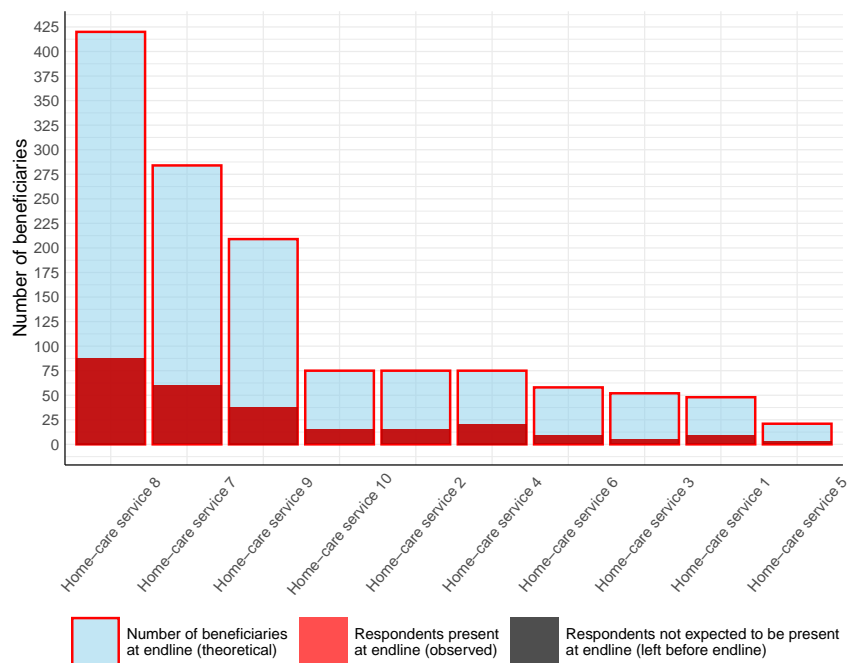
¹ n (%)² Pearson's Chi-squared test

Notes: Response rates computed among the sample of care recipients present in each wave.

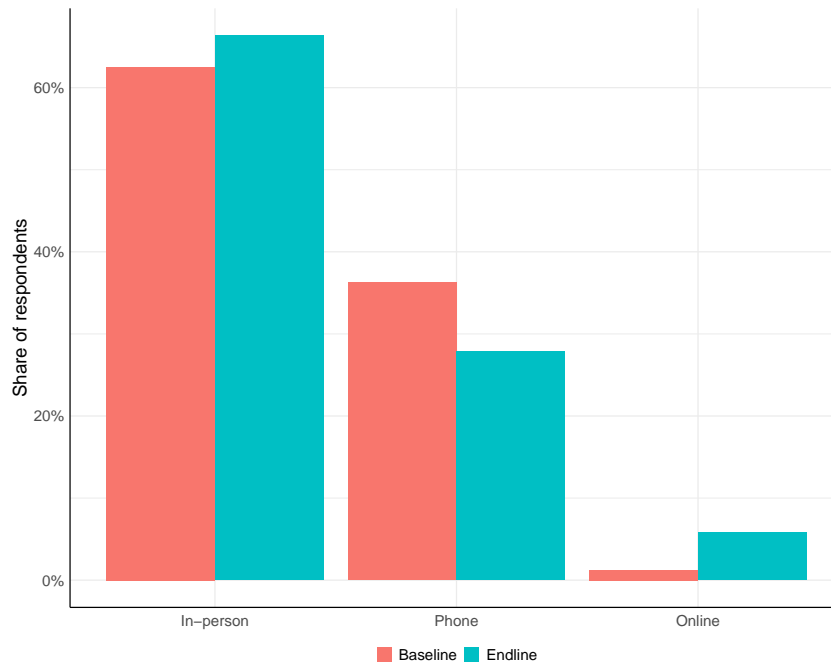
Figure 3.A.2: Number of respondent workers and care recipients at endline



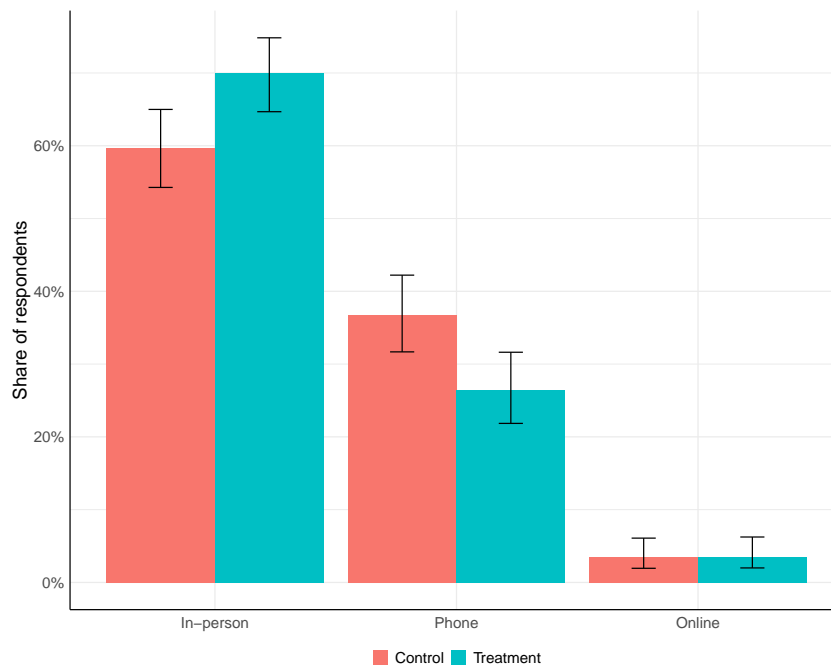
(a) Workers



(b) Care recipients

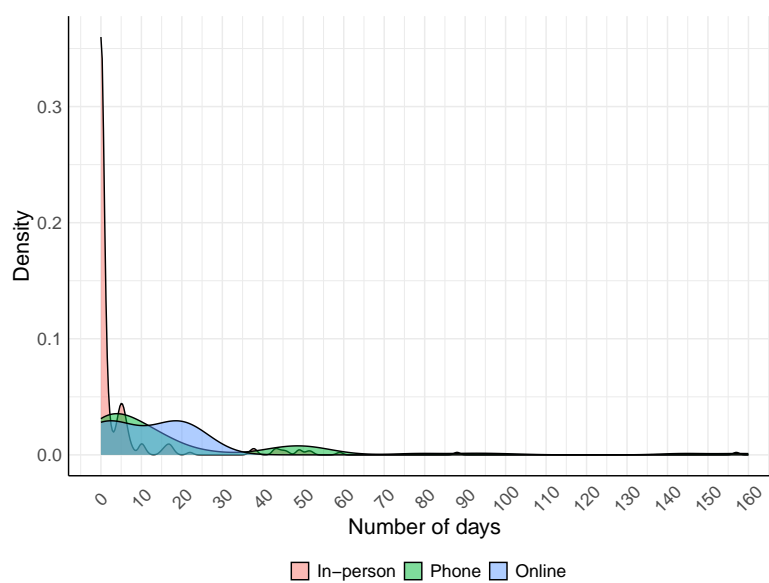
Figure 3.A.3: Share of worker respondents by survey mode

Notes: This figure shows the distribution (share) of responses to the worker questionnaire by survey mode (in-person, phone, online), separately for baseline and endline, based on the collected survey data.

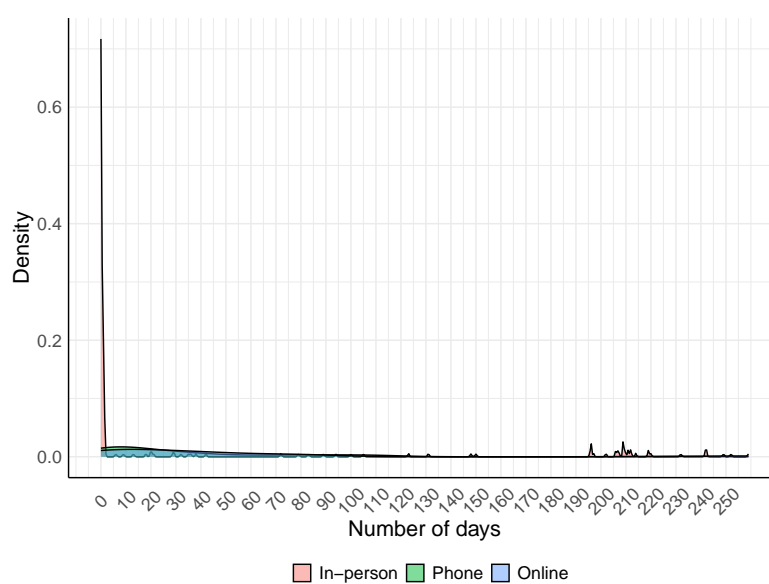
Figure 3.A.4: Share of worker respondents by survey mode and assignment status

Notes: This figure displays the distribution of worker survey mode (in-person, phone, online) by treatment assignment (control vs treatment), pooling baseline and endline. Shares are computed among workers present in each wave.

Figure 3.A.5: Distribution of the number of days since the first worker survey, by survey mode



(a) Baseline



(b) Endline

Table 3.A.4: Days elapsed since the first survey administration

Statistic	Baseline	Endline
Median	1.00	1.00
Mean	10.32	35.15
Std. Dev.	23.70	67.67
Max.	160.00	259.00

3.A.1 Balance tests

Table 3.A.5: Treatment–Control Balance of Socio-Demographic Characteristics at Baseline (balanced panel)

Outcomes	Control group (1)	Treatment group (2)	Difference (T-C) (3)	N
Woman	1.000 (0.000)	0.974 (0.015)	-0.031** (0.012)	230
Age	47.203 (1.052)	47.327 (1.063)	0.402 (1.164)	232
Number of dependent children	1.017 (0.106)	1.214 (0.112)	0.095 (0.107)	232
State-certified care professional	0.817 (0.037)	0.826 (0.037)	0.029 (0.041)	218
Seniority in the organization	8.983 (0.837)	8.274 (0.814)	-0.767 (0.890)	232
On a fixed-term contract	0.061 (0.022)	0.137 (0.032)	0.06*** (0.018)	232
Has a different previous occupation	0.617 (0.046)	0.615 (0.045)	-0.018 (0.055)	232
Unemployed > 3 months	0.165 (0.035)	0.291 (0.042)	0.129*** (0.041)	232
Born abroad	0.052 (0.021)	0.077 (0.025)	0.019 (0.030)	232
Parents born abroad	0.096 (0.028)	0.111 (0.029)	0.012 (0.035)	232

Notes.

OLS estimates from regressions of each outcome on a treatment-assignment dummy using baseline data. The sample consists of all workers observed in both waves, who completed both the baseline and endline questionnaires ($N = 232$). Outcomes are listed in the leftmost column. Columns (1) and (2) report mean values for the control and treatment groups, respectively, with standard deviations in parentheses. Column (3) reports the estimated treatment assignment effect, and the final column shows the number of observations. All specifications include stratification-unit fixed effects, and standard errors are clustered at the randomization unit.

Table 3.A.6: Treatment–Control Balance of Main outcomes at Baseline (balanced panel)

Outcomes	Control group (1)	Treatment group (2)	Difference (T-C) (3)	N
Job satisfaction (0–10)	7.270 (0.197)	6.949 (0.200)	-0.146 (0.216)	232
Working conditions	2.896 (0.053)	2.744 (0.061)	-0.121* (0.064)	232
Number of work leave days	21.591 (5.134)	13.564 (2.424)	-8.787** (3.815)	232
Job strain	0.226 (0.039)	0.256 (0.041)	0.024 (0.046)	232
Mental health score	61.674 (1.627)	61.880 (1.762)	1.691 (1.488)	232

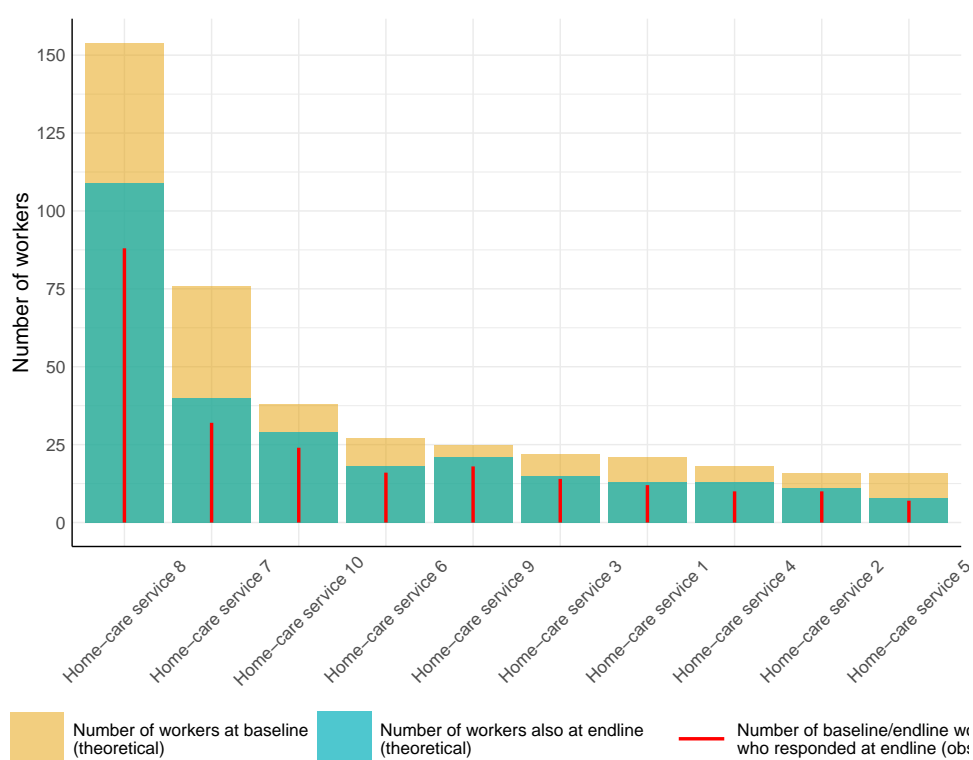
Notes.

OLS estimates from regressions of each outcome on a treatment-assignment dummy using baseline data. The sample consists of the balanced panel of workers observed in both waves ($N = 232$). Outcomes are listed in the leftmost column. Columns (1) and (2) report mean values for the control and treatment groups, respectively, with standard deviations in parentheses. Column (3) reports the estimated treatment assignment effect, and the final column shows the number of observations. All specifications include stratification-unit fixed effects, and standard errors are clustered at the randomization unit. All outcomes are expressed in their original units (natural scale).

3.A.2 Attrition

Attrition merits attention for two reasons. First, it constitutes an outcome of interest per se: we examine whether the self-managed teams model improves worker retention, a core policy objective given the sector’s high turnover and absenteeism. Second, differential attrition between treatment and control groups could bias estimated impacts on the main outcomes, making it essential to assess both the magnitude and composition of sample exits. Between the baseline and endline survey waves, 33% of enrolled workers (137 out of 414) exited the experiment, leaving 277 workers who could be followed across both waves. As shown in Table 3.A.1 and Figure 3.A.6, attrition rates ranged from 20% to 47% across the ten home care services.

Figure 3.A.6: Number of care workers in the experiment at baseline and endline



Information on why workers exited the study comes from the bimonthly reporting file that we sent to each home care service to be completed and returned. These descriptions were not standardized: services differed in how much detail they provided and in the terminology used. We therefore cleaned and reclassified the data to harmonize entries into coherent categories. A small share of exit reasons remains missing or only partially reported.

Table 3.A.7 presents the distribution of attrition and exit reasons as recorded in the administrative tracking files. As previously said, 33% of the workers present at baseline had left the experiment by the endline. Among the reasons cited by home-care services, voluntary departure and temporary departure (primarily sick leave) were the most frequent, accounting for 13% and 12% of the total baseline sample, respectively (representing approximately 38% and 36% of all leavers). Involuntary departures initiated by the employer concerned 4.1% of

Table 3.A.7: Attrition and Reason for leaving by treatment status

	Overall N = 414	Control N = 221	Treated N = 193	p-value
Left the experiment by endline	137 (33%)	81 (37%)	56 (29%)	0.12
<i>Reason for leaving:</i>				
Involuntary departure	30 (7.2%)	17 (7.7%)	13 (6.7%)	0.8
Employer-led termination	17 (4.1%)	8 (3.6%)	9 (4.7%)	0.6
Change of team/sector	13 (3.1%)	9 (4.1%)	4 (2.1%)	0.3
Voluntary departure	52 (13%)	33 (15%)	19 (9.8%)	0.14
Employee-led termination	39 (9.4%)	24 (11%)	15 (7.8%)	0.3
Retirement	13 (3.1%)	9 (4.1%)	4 (2.1%)	0.3
Temporary departure	50 (12%)	28 (13%)	22 (11%)	0.8
Sick leave	41 (9.9%)	22 (10.0%)	19 (9.8%)	>0.9
Maternity leave	9 (2.2%)	6 (2.7%)	3 (1.6%)	0.5
Not specified	5 (1.2%)	3 (1.4%)	2 (1.0%)	>0.9

¹ n (%)² Fisher's exact test

the workers, while smaller shares exited due to retirement (3.1%), maternity leave (2.2%), or a change of team (3.1%).

The category “change of team” encompasses a heterogeneous set of situations reported by managers. These include routine mobility, such as sector changes following a residential move or transfers requested by the employee. However, it also covers reassignments motivated by organizational constraints or cases where managers withdrew a worker from a self-managed team because limited availability or scheduling constraints made it difficult to ensure continuity of care. Consequently, “change of team” captures both routine mobility unrelated to the experiment and potentially problematic exits that could reflect challenges in adapting to the self-managed model.

Another way to document attrition is to exploit the short endline survey administered to workers who left the service before the second wave, in order to capture the workers' perspective rather than that of the home-care services. Among the leavers we were able to reach, 28 responded to this survey, 13 of whom were already present at baseline (representing a response rate of 9% out of the 137 workers who left between the baseline and endline). The most common reason for exit reported was voluntary departure (82%), primarily driven by resignation, retirement, or a return to studies (see Table 3.A.8).

Differential attrition. From an identification standpoint, attrition that is directly related to the implementation of self-managed teams poses a potential threat. If such exits occur at different rates in treated and control groups – because they are induced by the treatment – and are correlated with unobserved determinants of the outcomes, the resulting estimates may

Table 3.A.8: Treatment versus control: reasons for exit

Reasons for exit:	Overall N = 28	Control, N = 17	Treatment, N = 11	p-value
Involuntary departure	4 (14%)	1 (5.9%)	3 (27%)	0.3
Dismissed	1 (3.6%)	1 (5.9%)	0 (0%)	>0.9
End of probation period	2 (7.1%)	0 (0%)	2 (18%)	0.15
Non-renewal of contract	1 (3.6%)	0 (0%)	1 (9.1%)	0.4
Voluntary departure	23 (82%)	16 (94%)	7 (64%)	0.062
Resigned	17 (61%)	11 (65%)	6 (55%)	0.7
Retired	2 (7.1%)	2 (12%)	0 (0%)	0.5
Further studies	2 (7.1%)	1 (5.9%)	1 (9.1%)	>0.9
Relocation	1 (3.6%)	1 (5.9%)	0 (0%)	>0.9
Change of team	1 (3.6%)	1 (5.9%)	0 (0%)	>0.9
Not specified	1 (3.6%)	0 (0%)	1 (9.1%)	0.4

¹ n (%)

² Fisher's exact test

Notes:

Reasons for exit are based on self-reported responses from exiting workers who completed the short endline questionnaire for leavers that we were able to reach, rather than reasons recorded by the home-care services/administration in the tracking files. Among the 28 leavers who completed this questionnaire, 15 joined the study after the baseline, meaning only 13 were present at both baseline and endline.

be biased. Table 3.A.7 suggests that attrition was higher in the control group (37%) than in the treatment group (29%). Using leavers' responses, Table 3.A.8 also suggests that voluntary departure was more frequent in the control group than in the treatment group, although the small number of respondents warrants caution when interpreting these proportions. To formally assess whether this difference is statistically significant, we estimate Equation 3.1 on the full baseline sample of 414 workers (including non-respondents), regressing the probability of exit on treatment assignment. Table 3.A.9 presents the resulting estimates for overall attrition as well as for each mutually exclusive exit category, as reported by the home care services.

The first row shows that workers assigned to the treatment group were 7.8 percentage points less likely to exit the study between baseline and endline, a result significant at the 10% level. When decomposing this effect, the reduction in attrition appears to be spread across several categories rather than concentrated in a single one. The effect is most pronounced for voluntary departures (-4.7 p.p., significant at 10%), driven primarily by a reduction in retirements (-2.5 p.p., significant at 10%). The treatment also led to a significant decline in changes of team or sector (-2.3 p.p., significant at 5%), which falls under the broader category of involuntary departures. Other categories, such as sick leave, maternity leave, or employee-led resignations, show small and statistically insignificant coefficients.

The lower attrition rate among treated workers does not seem to stem from exit reasons that the intervention could most directly influence in the short term, such as sick leave or resignations, but rather from a reduction in retirements. This pattern suggests that treated

Table 3.A.9: Treatment Assignment Effects on Attrition and Response Rates

	Treatment Assignment (1)	N
Attrition	-0.078* (0.043)	414
<i>Reason for leaving:</i>		
Involuntary departure	-0.013 (0.020)	414
Employer-led termination	0.010 (0.014)	414
Change of team/sector	-0.023** (0.010)	414
Voluntary departure	-0.047* (0.024)	414
Employee-led termination	-0.022 (0.020)	414
Retirement	-0.025* (0.014)	414
Temporary departure	-0.017 (0.021)	414
Sick leave	-0.006 (0.022)	414
Maternity leave	-0.012 (0.009)	414
Not specified	-0.000 (0.007)	414
Response Rate		
Response (balanced)	0.010 (0.033)	277
Response (entrants incl.)	0.018 (0.026)	361

Notes: Each row represents a separate OLS regression of the outcome variable on the treatment assignment indicator. Robust standard errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. The categories **Involuntary**, **Voluntary**, and **Temporary departure** are aggregated groupings of the subsequent indented reasons. The detailed reasons from 'Employer-led termination' to 'Not specified' are mutually exclusive categories of attrition. Response outcomes are conditional on workers not having attrited by endline (balanced) or including new entrants.

workers remained in the sample longer partly because they were, on average, younger. While this could raise concerns about the integrity of randomization – for instance, if older workers resistant to change were more likely to exit or switch teams – it may also reflect baseline age differences between groups.

The decline in "change of team/sector" exits is particularly notable. As previously discussed, this category encompasses both routine mobility (e.g., sector changes following relocation) and exits potentially related to organizational tensions or constraints within self-managed teams. The negative and significant coefficient (–2.3 p.p.) indicates that the self-managed model did not increase such exits and may have even reduced them, suggesting potential improvements in team cohesion or reduced organizational friction.

Differential survey response rates. Beyond attrition, a further concern is differential survey response between treated and control among non-attriters. In particular, we worry that control workers were systematically less likely to respond, leading to under-collection of their answers. This concern arises because in-person surveys were conducted during regular team meetings, a feature of the self-managed model. As a result, treated units were more likely to be surveyed in person (75% of responses) than control workers, who were more often contacted by mail or phone (59% responded in-person, $p = 0.003$), as shown in Figure 3.A.4 and Table 3.A.2. This difference may have contributed to the lower response rates observed among control workers at endline. Differences in participation rate between treated and control could bias the estimated treatment effect by reducing the comparability of the groups.

To assess this, we estimate Equation 3.1, regressing the probability of endline response on treatment assignment while restricting the sample to individuals observed in both waves, thereby conditioning on non-attrition (277 observations, of which 231 responded as shown in Figure 3.A.1). The bottom of Table 3.A.9 shows that treatment assignment does not significantly increase response probability. This remains true both for the balanced panel and when the 84 new entrants are included in the sample (361 endline respondents). This lack of a significant effect suggests that the intervention did not induce a response bias among the workers who remained in the home-care services, further supporting the validity of the subsequent outcome analysis.

Overall participation is thus certainly balanced, but it remains the case that responses were significantly more likely to be collected in person in the treatment group than in the control group. This raises an additional concern about differential timing at endline, as control responses tended to occur later. Non-in-person surveys (online or by phone) were typically administered after in-person sessions and often required additional time to motivate workers to respond, as shown in Figure 3.A.5 and Table 3.A.4, which report the number of days elapsed since the first on-site interview within each service. On average, remote responses were collected more than 10 days later at baseline and over 35 days later at endline, with some occurring several months after the initial interviews because of various logistical constraints within the home-care services.

To examine whether there was selection into attrition and response, we regress baseline characteristics on two indicators: one capturing pure attrition at endline among the 370 baseline respondents, and another capturing non-response at endline among baseline respondents who did not attrit. The results show that those who left the experiment were

younger and more likely to hold fixed-term contracts (Column 1 of Table 3.A.10). Other characteristics – including gender, parental responsibilities, educational attainment, and employment history – do not differ significantly across groups, suggesting that attrition was not strongly selective along most observable dimensions. Column 2 reports differences in characteristics between respondents and non-respondents, conditional on remaining in the experiment. The two groups differ only in the share of individuals with a different previous occupation (not as home care workers), which appears to be lower among non-respondents.

Differential job satisfaction. Finally, to further investigate whether leavers significantly differ from those who remained in the experiment, we compare their endline outcomes using the common subset of questions administered in both the main endline survey and the short exit survey to measure job satisfaction. Table 3.A.11 shows that leavers report significantly lower job satisfaction (5.0 vs. 7.2, $p=0.030$) and organizational commitment than stayers. For instance, while 94% of stayers reported being proud to work for their organization, this share drops to 42% among leavers ($p<0.001$). Similarly, leavers were significantly more likely to report having to perform tasks they disapprove of (67% vs. 23%, $p=0.002$).

Overall, these descriptive results suggest that attrition is non-random and concentrated among workers experiencing lower well-being and a poorer fit with their organization's values. While this pattern confirms that leavers constitute a specific subgroup, the fact that survey response rates remain balanced between treatment and control (as shown in Table 3.A.9) mitigates the risk that this selection process systematically biases the comparison between groups.

Table 3.A.10: Attrition and Non-response: Balance of Socio-Demographic Characteristics at Baseline

Outcomes	Effect attriters (1)	N	Effect non-response (2)	N
Woman	0.003 (0.011)	368	-0.03 (0.037)	260
Age	-3.136* (1.743)	370	0.481 (2.063)	262
Number of dependent children	-0.021 (0.130)	370	-0.266 (0.171)	262
Has a diploma	-0.06 (0.041)	349	-0.024 (0.068)	247
Seniority in the organization	-1.075 (0.930)	369	0.652 (1.576)	261
On a fixed-term contract	0.067** (0.031)	370	0.035 (0.040)	262
Has a different previous occupation	0.016 (0.052)	370	-0.164* (0.089)	262
Unemployed > 3 months	0.042 (0.050)	370	0.01 (0.082)	262
Born abroad	-0.012 (0.031)	370	0.033 (0.048)	262
Parents born abroad	0.038 (0.039)	370	0.067 (0.058)	262

Notes: OLS estimates of baseline differences between attriters and non-attriters and respondents and non-respondents. Explained variables are on the left. Columns (1) and (2) report estimated effects from regressions using, respectively, the attriters dummy and the non-respondents dummy. All specifications include fixed effects for the stratification unit, and standard errors are clustered at the randomization unit. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.A.11: Comparison of Outcomes between Stayers and Leavers at Endline

Outcome	Stayer N = 232	Leaver N = 21	p-value
Overall job satisfaction (0-10)	7.21 (1.98)	5.00 (3.21)	0.030
Satisfaction with working conditions	187 (81%)	4 (31%)	<0.001
Satisfaction with pay	71 (31%)	2 (15%)	0.4
Satisfaction with atmosphere	199 (86%)	7 (54%)	0.008
Satisfaction with working hours	151 (65%)	4 (31%)	0.017
Has to do things she disapproves of	50 (23%)	8 (67%)	0.002
Works with the fear of losing job	18 (8.0%)	2 (17%)	0.3
Can balance work and personal life	174 (75%)	5 (42%)	0.017
Feels work is recognized at fair value	67 (29%)	2 (17%)	0.5
Is proud to work for the organization	216 (94%)	5 (42%)	<0.001
Shares the organization's values	208 (91%)	4 (33%)	<0.001
Trusts the organization	202 (88%)	4 (33%)	<0.001
Gets along well with colleagues	156 (70%)	5 (45%)	0.10

Notes:

Results are presented as Mean (SD) for continuous variables and as N (%) for binary indicators. p-values are calculated using One-way ANOVA and Fisher's exact test.

Table 3.A.12: Comparison of Outcomes between Stayers and Leavers at Baseline

Outcome	Stayer N = 232	Leaver N = 138	p-value
Overall job satisfaction (0-10)	7.11 (2.14)	7.17 (2.05)	0.8
Satisfaction with working conditions	177 (76%)	100 (72%)	0.5
Satisfaction with pay	57 (25%)	46 (33%)	0.073
Satisfaction with atmosphere	208 (90%)	119 (86%)	0.3
Satisfaction with working hours	147 (63%)	81 (59%)	0.4
Has to do things she disapproves of	57 (26%)	38 (29%)	0.5
Works with the fear of losing job	19 (8.5%)	11 (8.3%)	>0.9
Can balance work and personal life	178 (77%)	100 (73%)	0.4
Feels work is recognized at fair value	63 (28%)	44 (32%)	0.3
Is proud to work for the organization	212 (93%)	120 (90%)	0.4
Shares the organization's values	199 (88%)	113 (88%)	>0.9
Trusts the organization	196 (85%)	110 (84%)	0.8
Gets along well with colleagues	139 (65%)	76 (60%)	0.4

Notes:

This table tests if workers who eventually left the organization (Leavers) were already different at baseline from those who stayed (Stayers).

Table 3.A.13: Baseline Characteristics: Stayers vs. All Leavers by Treatment Status

Outcome	Stayer - Control N = 115	Stayer - Treated N = 117	Leaver - Control N = 81	Leaver - Treated N = 57	p-value
Overall job satisfaction (0-10)	7.27 (2.11)	6.95 (2.16)	6.89 (2.23)	7.56 (1.69)	0.13
Satisfaction with working conditions	96 (83%)	81 (69%)	54 (67%)	46 (81%)	0.014
Satisfaction with pay	34 (30%)	23 (20%)	26 (32%)	20 (35%)	0.093
Satisfaction with atmosphere	101 (88%)	107 (91%)	70 (86%)	49 (86%)	0.6
Satisfaction with working hours	74 (64%)	73 (62%)	45 (56%)	36 (63%)	0.7
Has to do things she disapproves of	25 (23%)	32 (28%)	22 (29%)	16 (30%)	0.7
Works with the fear of losing job	9 (8.2%)	10 (8.8%)	7 (8.8%)	4 (7.5%)	>0.9
Can balance work and personal life	85 (75%)	93 (79%)	54 (67%)	46 (82%)	0.13
Feels work is recognized at fair value	33 (29%)	30 (26%)	24 (30%)	20 (36%)	0.6
Is proud to work for the organization	107 (93%)	105 (92%)	70 (89%)	50 (93%)	0.7
Shares the organization's values	96 (86%)	103 (89%)	67 (88%)	46 (87%)	>0.9
Trusts the organization	101 (88%)	95 (83%)	63 (83%)	47 (85%)	0.7
Gets along well with colleagues	74 (69%)	65 (61%)	49 (66%)	27 (51%)	0.13

Notes:

Results compare baseline outcomes for workers who stayed in the experiment (N=232) vs. all workers who left before endline (N=138).

3.B Descriptive statistics

Table 3.B.1: Type of home-care service and organizational structure

Type of service	Number (%)
SAAD	8 (80%)
SPASAD	1 (10%)
SSIAD	1 (10%)
Type of organization	
Non-profit organization	5 (50%)
For-profit private company	2 (20%)
Non-profit private company	0 (0%)
Municipal (CCAS) or inter-municipal (CIAS) social service center	3 (30%)
Mutual (non-profit) service provider	0 (0%)
Average number of full-time equivalent employees	122.3
Average number of home care workers	34.6

Table 3.B.2: Baseline descriptive statistics (workers)

Variable	Mean	SD	N
Panel A. Sociodemographics			
Female (0/1)	0.981	0.137	368
Age (years)	46.573	11.980	370
Has dependent children (0/1)	0.554	0.498	370
Number of dependent children	1.076	1.241	370
Has a diploma or degree (0/1)	0.805	0.397	349
Tenure in the organization (years)	8.076	8.626	369
Fixed-term contract (0/1)	0.116	0.321	370
Previously worked in another occupation (0/1)	0.592	0.492	370
Unemployed for more than 3 months in the past 5 years (0/1)	0.249	0.433	370
Born abroad (0/1)	0.070	0.256	370
Parents born abroad (0/1)	0.122	0.327	370
Panel B. First-stage outcomes			
Works in a team (0/1)	0.405	0.492	370
Schedule not set by hierarchy (0/1)	0.064	0.245	312
Schedule not set by hierarchy or almost always adjustable (0/1)	0.327	0.470	312
Works in a team managing its own schedules (0/1)	0.339	0.474	271
Works in a team deciding care plans (0/1)	0.314	0.465	255
Works in a team managing logistics (0/1)	0.346	0.477	266
Uses digital tools (0/1)	0.396	0.492	96
Involves the care recipient's relatives (0/1)	0.105	0.307	370
Panel C. Job satisfaction and psychosocial outcomes			
Life satisfaction (0–10)	6.986	1.954	370
Job satisfaction (0–10)	7.130	2.102	370
Satisfied with working conditions (0/1)	0.749	0.434	370
Satisfied with pay (0/1)	0.278	0.449	370
Satisfied with workplace atmosphere (0/1)	0.884	0.321	370
Satisfied with working hours (0/1)	0.616	0.487	370
Has to do things she disapproves of at work (0/1)	0.271	0.445	351
Works while fearing job loss (0/1)	0.084	0.278	357

Table 3.B.2: Baseline descriptive statistics (workers) (*continued*)

Variable	Mean	SD	N
Able to balance work and personal life (0/1)	0.755	0.430	368
Feels that her work is recognized at its fair value (0/1)	0.293	0.456	365
Proud to work for the organization (0/1)	0.917	0.276	362
Shares the organization's values (0/1)	0.876	0.330	356
Trusts the organization (0/1)	0.848	0.360	361
Feels tense at work (0/1)	0.216	0.412	366
Feels happy at work (0/1)	0.657	0.475	362
Feels stressed at work (0/1)	0.312	0.464	368
Feels sad at work (0/1)	0.044	0.205	366
Feels discouraged at work (0/1)	0.157	0.364	364
Mental health index	62.012	18.101	369
Intends to stay in the organization next year (0/1)	0.968	0.176	313
Intends to stay in the organization over the next five years (0/1)	0.792	0.407	255
Psychological demand score (Karasek)	21.889	5.212	370
Decision latitude score (Karasek)	67.119	9.463	370
Social support score (Karasek)	23.338	4.470	370
Job strain index	0.241	0.428	370
Iso-strain index	0.149	0.356	370
Panel D. Relationship with care recipients			
Available for care recipients (0/1)	0.981	0.137	366
Devotes enough time to care recipients (0/1)	0.750	0.434	364
Listens to care recipients' problems or difficulties (0/1)	0.986	0.116	370
Able to meet care recipients' expectations (0/1)	0.930	0.255	359
Satisfaction with the care provided (0–10)	8.368	1.333	370
Panel E. Working hours and time constraints			
Full-time contract (0/1)	0.273	0.446	370
Number of hours worked	30.601	9.953	366
Feels she works unpaid overtime (0/1)	0.382	0.487	280
Would like to work more hours with corresponding pay (0/1)	0.527	0.500	317
Has breaks of 3 hours or more during the day (0/1)	0.254	0.436	370
Has staggered working hours (early mornings or late evenings) (0/1)	0.416	0.494	370
Has irregular or rotating working hours (0/1)	0.405	0.492	370
Works on weekends (0/1)	0.719	0.450	370
Exceeds normal working hours (0/1)	0.324	0.469	370
Skips or shortens meals or does not take breaks (0/1)	0.300	0.459	370
Performs tasks too quickly when more care would be needed (0/1)	0.186	0.390	370
Works during rest time, leave, or sick leave (0/1)	0.046	0.210	370
Has to rush while working (0/1)	0.549	0.498	370
Frequently has to interrupt a task to handle an unplanned one (0/1)	0.305	0.461	370
Has at least two consecutive days off per week (0/1)	0.557	0.497	352
Relatives say she is not available enough because of work (0/1)	0.697	0.460	370
Knows tomorrow's work schedule (0/1)	0.943	0.232	370
Knows next week's work schedule (0/1)	0.786	0.410	370
Knows next month's work schedule (0/1)	0.422	0.494	370
Often experiences last-minute schedule changes (0/1)	0.678	0.468	370
Can adjust working hours to personal constraints (0/1)	0.470	0.500	370
Can change the order of her visits (0/1)	0.157	0.364	370
Panel F. Coworker relations and workplace atmosphere			
Gets along well with coworkers (0/1)	0.630	0.483	341
Can cooperate with coworkers in her work (0/1)	0.859	0.348	369
Trusts coworkers to help in case of problems (0/1)	0.924	0.266	340
Trusts management to help in case of problems (0/1)	0.900	0.300	351
Receives adequate support in difficult situations (0/1)	0.764	0.425	288

Table 3.B.2: Baseline descriptive statistics (workers) (*continued*)

Variable	Mean	SD	N
Feels more isolated than two years ago (0/1)	0.875	0.331	280
Receives the respect she deserves from supervisors (0/1)	0.838	0.369	290
Receives the respect she deserves from coworkers (0/1)	0.833	0.374	275
Given all my efforts, I receive the respect and esteem I deserve at work (0/1)	0.671	0.471	289
Is treated unfairly at work (0/1)	0.253	0.436	233
Trade union or staff representation in the organization (0/1)	0.831	0.375	278
Is a staff representative or union delegate (0/1)	0.090	0.286	234
Good relations between employees and management (0/1)	0.384	0.487	328
Panel G. Absences and health			
At least one sick leave spell (0/1)	0.633	0.483	354
Number of days on sick leave	21.978	46.808	224
At least one work accident leave spell (0/1)	0.175	0.380	360
Number of days on work accident leave	4.714	22.076	360
At least one commuting accident leave spell (0/1)	0.091	0.289	339
Number of days on commuting accident leave	3.029	17.472	339
Number of days of absence	20.668	47.137	370
Number of days of absence excluding commuting accidents	17.892	43.250	370
Feels that work harms her health (0/1)	0.256	0.437	351
Went to work while ill (0/1)	0.170	0.377	311
Never absent outside sick leave/work accident/leave (0/1)	0.654	0.476	361
Sometimes absent due to personal emergency (0/1)	0.219	0.414	361
Often absent due to personal emergency (0/1)	0.014	0.117	361
Sometimes absent due to fatigue (0/1)	0.014	0.117	361
Often absent due to fatigue (0/1)	0.006	0.074	361
Sometimes absent for other reasons (0/1)	0.113	0.317	362
Often absent for other reasons (0/1)	0.006	0.074	361

Table 3.B.3: Comparison with SUMER 2017 (baseline sample)

Indicator	Our sample (baseline)	Home-care workers (SUMER 2017)	Other employees (SUMER 2017)
Panel A. Worker characteristics			
Median age (years)	48	49	43
Non-EU foreign nationality	7	8	2
Permanent contract (open-ended)	87.3	86	69
Tenure below 3 years	40.1	37	23
Employer type	-	-	-
Private sector	20.3	56	63
Mutual / non-profit / cooperative organization	64.3	36	8
Public sector	15.4	7	27
Panel B. Working time			
Part-time	70.8	75.6	19.7
Choice over working time	-	61.5	85.2
Work on Sundays or public holidays (even occasionally)	-	57.4	35.3
Work on Saturdays (even occasionally)	-	67.58	50.26
Work on weekends	71.9	-	-
Night work (midnight–5am)	0	5.51	8.41
Evening work (8pm–midnight)	-	13.77	27.58
Irregular schedules (not the same hours every day)	40.5	51	24.23
Two or more work periods per day	-	39.51	10.52
Breaks of 3 hours or more within a day	25.4	-	-
Shifted hours (early morning / late evening)	41.6	-	-
Relatives say I am not available enough because of my job	69.7	50.4	35.59
Do not know next week's schedule	21.4	15.02	5.09
No 48 consecutive hours of rest	44.3	29.03	20.44
Panel C. Work intensity			
My job involves repetitive tasks	89.5	88.7	84.61
My job requires working very fast	70.5	51.4	65.95
My job requires working intensely	72.7	57.5	70.46
I am asked to do an excessive amount of work	54.1	31.8	38.15
My tasks are often interrupted before completion, requiring me to resume them later	25.4	34.4	58.14
I do not have enough time to do my job properly	64.9	32.1	32.71
I receive contradictory instructions from others	28.1	22.5	26.12
My job requires long periods of intense concentration	43.2	28.7	56.58
My work is very rushed	51.9	31.1	42.19
Panel D. Autonomy at work			
My job requires being creative	29.5	27.36	36.29
My job requires a high level of skill	30	37.08	34.34
My job often allows me to make decisions myself	18.1	12.3	18.17
I have very little freedom to decide how I do my work	32.7	28.7	24.66
My work involves varied activities	23	13.4	15.17
I can influence how my work is carried out	32.7	21.3	23.15
I have the opportunity to develop my professional skills	18.1	13.8	19.99
Panel E. Social relations at work			
Cannot discuss with colleagues when facing difficulties doing the job properly	-	24.05	4.4
Cannot discuss with supervisors when facing difficulties doing the job properly	-	14.45	6.93
Do NOT trust colleagues to help if needed	7.6	-	-
Do NOT trust supervisors to help if needed	10	-	-
Do NOT have the possibility to cooperate (information sharing, mutual help)	14.1	37.39	8.28

Table 3.B.3: Comparison with SUMER 2017 (baseline sample) (*continued*)

Indicator	Our sample (baseline)	Home-care workers (SUMER 2017)	Other employees (SUMER 2017)
My supervisor cares about subordinates' well-being	28.4	23.8	26.47
My supervisor pays attention to what I say	26.2	19.7	18.94
My supervisor helps me get the job done	25.9	20.4	23.75
My supervisor can easily foster collaboration among subordinates	30.5	21.5	23.58
The colleagues I work with are professionally competent	27.8	15.4	8.74
The colleagues I work with show interest in me	25.7	20.8	10.71
The colleagues I work with are friendly	22.2	11.3	9.25
The colleagues I work with help me get the job done	22.7	15.5	13.22
I receive the respect I deserve from my supervisors	83.8	77.55	72.07
I receive the respect I deserve from my colleagues	83.3	77.72	82.2
At work, I receive satisfactory support in difficult situations	76.4	73.54	68.92
I am treated unfairly at work	25.3	75.17	82.71
Given all my efforts, I receive the respect and recognition I deserve at work	67.1	71.05	66.09

Note:

Source: SUMER 2017.

3.C Treatment compliance

Table 3.C.1: Non-compliance with treatment based on organizational monitoring files

Home-care service ID and number of workers concerned by the switch	Non-compliance type	Switching period	Training period	Date deadline
<i>Home-care service 5 (2 teams)</i>				
Team 1 (1 worker)	Control to treatment [†]	December 2023	Nov-Dec. 2023	June 2025
<i>Home-care service 7 (6 teams)</i>				
Team 1 (3 workers)	Control to treatment [†]	February 2023	Feb-March 2023	March 2024
Team 2 (7 workers)	Treatment to control ^{†,†}	February 2023	Feb-March 2023	March 2024
<i>Home-care service 8 (16 teams)</i>				
Team 1 (1 worker)	Treatment to control [†]	August 2022	March 2023	March 2024
Team 2 (1 worker)	Treatment to control	April 2023	March 2023	March 2024
Team 3 (1 worker)	Treatment to control	June 2023	March 2023	March 2024
Team 4 (1 worker)	Control to treatment	January 2024	March 2023	March 2024
Team 5 (1 worker)	Control to treatment [†]	May 2023	March 2023	March 2024
Team 5 (full remaining team, 9 workers)	Control to treatment [†]	January 2024	March 2023	March 2024
Team 6 (full team, 11 workers)	Control to treatment ^{†,†,†}	January 2024	March 2023	March 2024
Team 7 (full team, 8 workers)	Control to treatment ^{†,†,†}	January 2024	March 2023	March 2024
Team 8 (full team, 10 workers)	Control to treatment ^{†,†,†}	January 2024	March 2023	March 2024
Team 9 (full team, 11 workers)	Control to treatment ^{†,†,†,†}	January 2024	March 2023	March 2024
<i>Home-care service 9 (3 teams)</i>				
Team 1 (full team, 8 workers)	Control to treatment [†]	December 2023	March-Sept. 2023	April 2024
Individual workers (3 workers)	Control to treatment [†]	April 2024	March-Sept. 2023	April 2024

Note: Data are based on monitoring files provided by the home-care services, which provide direct information on staff changes as well as the exact reasons and dates of these changes, allowing us to objectively measure non-compliance with treatment assignment. Worker counts only include employees who were present at the baseline (new entrants during the experiment are excluded). Each dagger symbol (†) indicates one worker from the baseline count who left the organization before the endline survey. For example, in Service 7 (Team 2), two daggers indicate that out of the 7 baseline workers who switched status, 2 left before the endline and are therefore excluded from our final balanced panel.

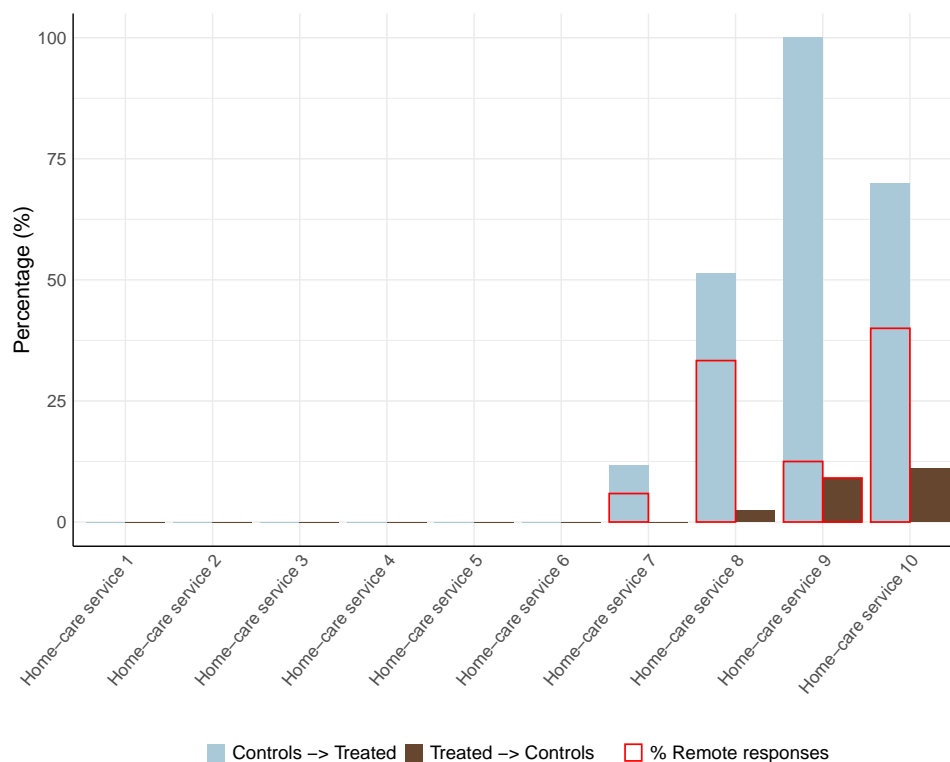
Context: Discrepancies may arise from internal team reorganizations due to field constraints, such as employee relocations, successive resignations leading to understaffing, internal promotions, interpersonal conflicts, or early transitions to autonomous management models before the end of the experiment (e.g., home-care service 9).

Table 3.C.2: Administrative Non-Compliance: Durations and Exposure Ratios

Type of Switch	N	Mean Days (Administrative)	Mean Days (Effective Exposure)
C to T (Individual workers)	3	290.3	272.3
C to T (Teams/Early Switch)	41	79.7	79.7
T to C (Individual workers)	7	381.3	362.7
<i>TOTAL NON-COMPLIERS (Admin)</i>	<i>51</i>	<i>133.5</i>	<i>129.8</i>

Note: This table presents the number of workers involved in administrative team reassignments compared to their baseline assignment group and the average duration of these deviations. Mean Administrative Days measures the total time elapsed between the physical reassignment and the endline, while Mean Effective Exposure only counts the days occurring after the start of official training sessions in each structure. The count is strictly restricted to the theoretical balanced panel (workers present at both baseline and endline according to administrative records), regardless of whether they completed the questionnaires. Based on these durations, we calculate a temporal non-compliance ratio by dividing the total sum of non-compliance person-days by the experiment's total volume (total workers times total days of study). The ratio since randomization (Baseline reference) is equal to 3.31%, measuring the global deviation from the protocol, while the ratio of effective contamination (Training reference) is equal to 4.82%, focusing on the treatment phase only.

Figure 3.C.1: Mismatch between observed and assigned treatment, by mode of data collection



NOTE. The sample excludes new-entrants (as for the first-stage regression).

LECTURE. In Structure 7, 100% of employees assigned to the control group report being in an autonomous team, and almost 10% of those assigned to the treatment group report being in the control group. Notably, all of these treatment-to-control switchers completed the survey online, after the in-person round.

Table 3.C.3: First-stage (with new entrants)

Outcomes	Control group (1)	Treatment group (2)	Treatment effect (3)	N
Is in an autonomous team	0.344 (0.042)	0.967 (0.015)	0.633*** (0.068)	280
Works in a team	0.455 (0.041)	0.785 (0.033)	0.335*** (0.051)	303
Planning: not by hierarchy	0.014 (0.010)	0.060 (0.019)	0.051** (0.023)	291
Planning: not by hierarchy or almost always modifiable	0.350 (0.040)	0.570 (0.040)	0.244*** (0.066)	291
Works in a team managing its schedules	0.283 (0.040)	0.641 (0.040)	0.373*** (0.055)	272
Works in a team deciding its care plans	0.200 (0.037)	0.343 (0.041)	0.156*** (0.038)	257
Works in a team managing logistics	0.214 (0.038)	0.444 (0.043)	0.251*** (0.044)	252
Use of digital tools	0.250 (0.073)	0.387 (0.051)	0.112 (0.085)	129
Solicitation of the user's entourage	0.117 (0.027)	0.114 (0.025)	-0.002 (0.026)	303
Has participated in autonomous team training	0.254 (0.038)	0.447 (0.042)	0.191*** (0.069)	275

Notes.

OLS estimates from regressions of each outcome on a treatment-assignment dummy using baseline data. Outcomes are listed in the leftmost column. Columns (1) and (2) report mean values for the control and treatment groups, respectively, with standard deviations in parentheses. Column (3) reports the estimated treatment effect, and the final column shows the number of observations. All specifications include stratification-unit fixed effects, and standard errors are clustered at the randomization unit.

Table 3.C.4: Compliers versus non-compliers: sociodemographic characteristics

Sociodemographic characteristics	Overall N = 294	Non-compliers, N = 51 (17%)	Compliers, N = 243 (83%)	p-value
Female (0/1)	287 (98%)	51 (100%)	236 (97%)	0.6
Age (years)	46 (34, 56)	51 (39, 58)	45 (34, 55)	0.048
Has dependent children (0/1)	163 (55%)	28 (55%)	135 (56%)	>0.9
Number of dependent children				0.3
0	132 (45%)	23 (45%)	109 (45%)	
1	59 (20%)	11 (22%)	48 (20%)	
2	68 (23%)	14 (27%)	54 (22%)	
3	24 (8.2%)	1 (2.0%)	23 (9.5%)	
4	9 (3.1%)	1 (2.0%)	8 (3.3%)	
5	2 (0.7%)	1 (2.0%)	1 (0.4%)	
Has a diploma or degree (0/1)	233 (79%)	41 (80%)	192 (79%)	0.8
Tenure in the organization (years)	5 (2, 12)	7 (3, 18)	5 (2, 11)	0.13
Fixed-term contract (0/1)	18 (6.1%)	8 (16%)	10 (4.1%)	0.005
Previously worked in another occupation (0/1)	176 (60%)	27 (53%)	149 (61%)	0.3
Unemployed for more than 3 months in the past 5 years (0/1)	60 (20%)	8 (16%)	52 (21%)	0.4
Born abroad (0/1)	12 (4.1%)	1 (2.0%)	11 (4.5%)	0.7
Parents born abroad (0/1)	27 (9.2%)	4 (7.8%)	23 (9.5%)	>0.9

¹ n (%); Median (Q1, Q3)² Fisher's exact test; Wilcoxon rank sum test; Pearson's Chi-squared test

Table 3.C.5: Compliers versus non-compliers (treated versus control): sociodemographic characteristics

Sociodemographic characteristics	Overall N = 294	Compliers		Non-compliers		p-value
		Treated, N = 155 (53%)	Control, N = 88 (30%)	Treated, N = 5 (1.7%)	Control, N = 46 (16%)	
Female (0/1)	287 (98%)	149 (96%)	87 (99%)	5 (100%)	46 (100%)	0.4
Age (years)	46 (34, 56)	46 (34, 56)	40 (33, 54)	49 (42, 50)	51 (39, 58)	0.13
Has dependent children (0/1)	163 (55%)	89 (57%)	46 (52%)	4 (80%)	24 (52%)	0.6
Number of dependent children						
0	132 (45%)	67 (43%)	42 (48%)	1 (20%)	22 (48%)	
1	59 (20%)	33 (21%)	15 (17%)	1 (20%)	10 (22%)	
2	68 (23%)	30 (19%)	24 (27%)	3 (60%)	11 (24%)	
3	24 (8.2%)	18 (12%)	5 (5.7%)	0 (0%)	1 (2.2%)	
4	9 (3.1%)	6 (3.9%)	2 (2.3%)	0 (0%)	1 (2.2%)	
5	2 (0.7%)	1 (0.6%)	0 (0%)	0 (0%)	1 (2.2%)	
Has a diploma or degree (0/1)	233 (79%)	118 (76%)	74 (84%)	3 (60%)	38 (83%)	0.3
Tenure in the organization (years)	5 (2, 12)	5 (2, 11)	5 (2, 12)	5 (2, 11)	7 (3, 18)	0.4
Fixed-term contract (0/1)	18 (6.1%)	9 (5.8%)	1 (1.1%)	0 (0%)	8 (17%)	0.006
Previously worked in another occupation (0/1)	176 (60%)	98 (63%)	51 (58%)	4 (80%)	23 (50%)	0.3
Unemployed for more than 3 months in the past 5 years (0/1)	60 (20%)	32 (21%)	20 (23%)	2 (40%)	6 (13%)	0.3
Born abroad (0/1)	12 (4.1%)	9 (5.8%)	2 (2.3%)	0 (0%)	1 (2.2%)	0.5
Parents born abroad (0/1)	27 (9.2%)	16 (10%)	7 (8.0%)	0 (0%)	4 (8.7%)	>0.9

¹ n (%); Median (Q1, Q3)

² Fisher's exact test; Kruskal-Wallis rank sum test; NA

Table 3.C.6: Non-compliance with treatment on sociodemographic characteristics

	Non-compliance with assigned treatment										
Female	0.177*										0.161+
	(0.072)										(0.086)
Age	0.002*										0.002
	(0.001)										(0.001)
Has dependent children	0.017										0.013
	(0.029)										(0.035)
Has a diploma or degree	0.011										0.030
	(0.053)										(0.053)
Tenure in the organization	0.002										0.000
	(0.003)										(0.004)
Fixed-term contract	-0.055										-0.089
	(0.069)										(0.080)
Previously worked in another occupation	-0.055										-0.067
	(0.041)										(0.046)
Unemployed for more than 3 months in the past 5 years	-0.025										0.014
	(0.049)										(0.057)
Born abroad	0.013										0.044
	(0.089)										(0.126)
Parents born abroad	-0.019										-0.037
	(0.063)										(0.079)
Num.Obs.	294	292	294	294	289	293	294	294	294	294	286
R2	0.246	0.245	0.242	0.242	0.236	0.241	0.246	0.242	0.241	0.242	0.249

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

F-test: F(10, 275) = 1.61, p = 0.103

Wald test with fixed effects: F(10, 265) = 1.06, p = 0.393

Table 3.C.7: Compliers versus non-compliers: first stage

Dimensions of a self-managed team:	Compliers		Non-compliers		p-value
	Treated, N = 155 (53%)	Control, N = 88 (30%)	Treated, N = 5 (1.7%)	Control, N = 46 (16%)	
Work schedule is not set by management	8 (5.4%)	2 (2.4%)	0 (0%)	0 (0%)	0.3
Work schedule is not set by management, or is often/very often adjustable	86 (58%)	20 (24%)	2 (50%)	28 (61%)	<0.001
Works in a team that manages its own schedules	96 (67%)	9 (12%)	1 (20%)	23 (56%)	<0.001
Works in a team that decides its own care plans	46 (34%)	8 (11%)	0 (0%)	15 (38%)	<0.001
Works in a team that manages intervention logistics	59 (44%)	10 (14%)	1 (25%)	11 (31%)	<0.001
Uses digital tools for scheduling and organizing interventions	36 (38%)	2 (22%)	1 (100%)	6 (26%)	0.3
Involves the user's relatives	19 (12%)	12 (14%)	0 (0%)	5 (11%)	>0.9
Has participated in training on self-managed teams	60 (43%)	12 (15%)	3 (60%)	18 (39%)	<0.001

¹ n (%)

² Fisher's exact test

Table 3.C.8: Home-care Service Characteristics by Assignment Compliance

Home-care services characteristics:	Full compliance, N = 6 (60%)	Some non-compliance, N = 4 (40%)
Mean number of workers	12	43
Gender (share female)	1.00	0.98
Mean age	47.27	46.99
Share of workers with at least one dependent child	0.62	0.55
Number of children	1.25	1.03
Has a degree	0.77	0.84
Tenure in the organization	9.6	9.8
Share on fixed-term contracts (CDD)	0.03	0.11
Share who previously worked in another occupation	0.61	0.60
Unemployed > 3 months over the last 5 years	0.10	0.16
Born abroad	0.16	0.03
Parents born abroad	0.25	0.09
Type of organization		
For-profit private company	1 (17%)	1 (25%)
Local public social action center (CCAS/CIAS)	2 (33%)	1 (25%)
Non-profit association	3 (50%)	2 (50%)
Type of service		
SAAD	5 (83%)	3 (75%)
SPASAD	1 (17%)	0 (0%)
SSIAD	0 (0%)	1 (25%)

¹ Mean; n (%)

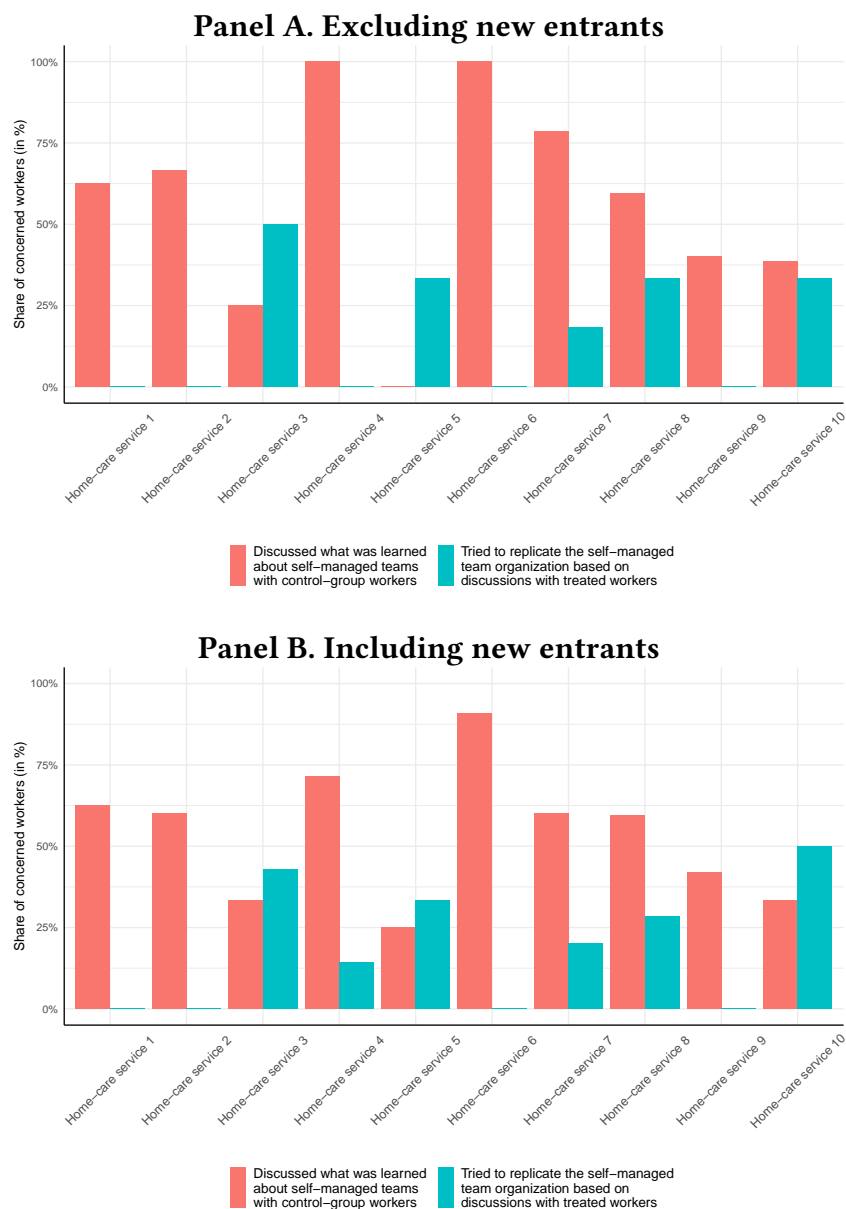
Notes. The sample excludes new entrants (as for the first-stage regression).

3.C.1 Potential Spillover Effects

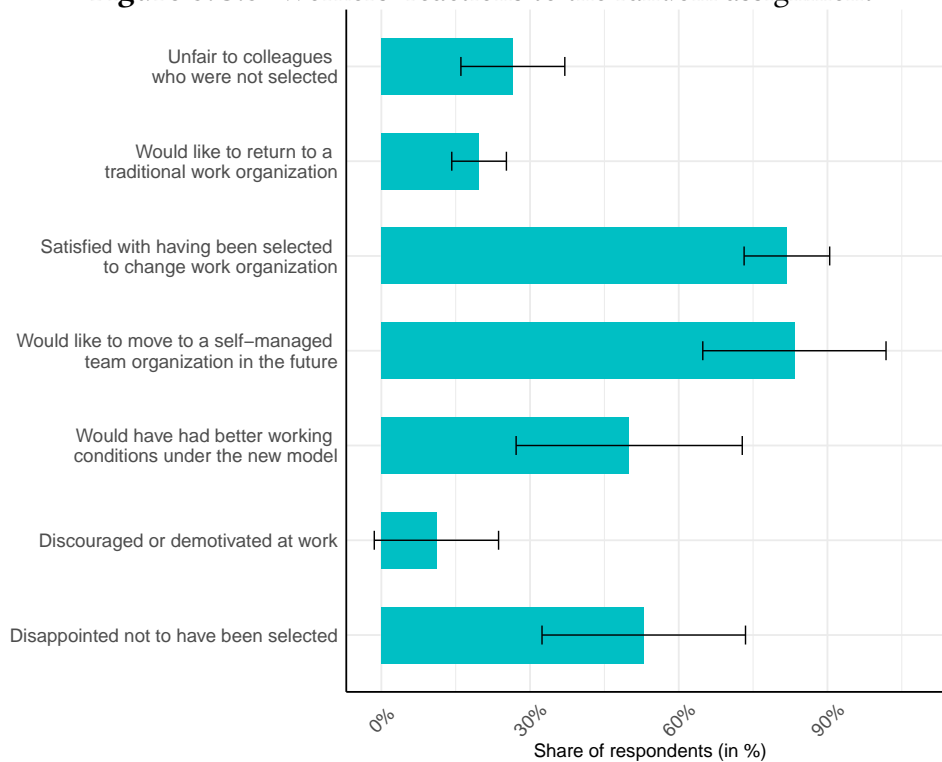
While randomization was conducted at the team level, potential spillover effects across treatment arms cannot be ruled out. Among workers assigned to the treatment group, a majority (56%) reported having discussed the self-managed team model with colleagues in the control group. However, only 20% of control-assigned workers indicated that they had attempted to adopt elements of the self-managed team organization based on these exchanges. These figures suggest limited diffusion of the treatment across groups, but some contamination cannot be excluded, especially within services where treated and control teams coexisted. Figure 3.C.2 shows the proportion of such behaviors by structure, confirming that these dynamics were concentrated in a small number of services.

Figure 3.C.3 further illustrates the subjective perceptions generated by the experimental assignment. Over 80% of treated workers declared being satisfied with having been selected to participate in the self-managed team experiment, while about a quarter expressed that the process had been unfair to their control-assigned colleagues. On the other hand, more than half of control group members reported disappointment at not being selected, or believed that they would have benefited from better working conditions under the new model. These reactions reinforce the credibility of the intervention and the perceived benefits of self-managed teams implementation, but also suggest the possibility of motivational or behavioral spillovers, especially in settings where interaction between groups was frequent.

Figure 3.C.2: Potential treatment transmission or replication across groups



Notes: Endline data only. Bars show the share of treated workers reporting discussions with control-group colleagues and the share of control-assigned workers reporting attempts to replicate elements of the self-managed team model. Service names are anonymized for confidentiality.

Figure 3.C.3: Workers' reactions to the random assignment

Notes: Endline data only. Bars report the share of respondents agreeing with each statement; whiskers show 90% confidence intervals.

3.D Estimation results

3.D.1 Results on care workers

Table 3.D.1: Satisfaction

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Overall life satisfaction	7.17 (2.04)	0.245 (0.256) [0.413]	0.379 (0.281) [0.220]	0.120 (0.126) [0.459]	0.186 (0.138) [0.245]
Overall job satisfaction	7.27 (2.06)	0.490* (0.249) [0.110]	0.592**†† (0.246) [0.035]	0.238* (0.121) [0.146]	0.287**†† (0.119) [0.041]
Satisfaction with working conditions	2.90 (0.62)	0.257***†† (0.084) [0.021]	0.299***†† (0.090) [0.011]	0.414***†† (0.134) [0.029]	0.480***†† (0.145) [0.015]
Satisfaction with pay	2.10 (0.78)	0.047 (0.087) [0.651]	0.061 (0.098) [0.587]	0.060 (0.112) [0.682]	0.078 (0.125) [0.615]
Satisfaction with atmosphere	3.04 (0.63)	-0.002 (0.066) [0.971]	0.018 (0.067) [0.789]	-0.004 (0.104) [0.971]	0.029 (0.106) [0.789]
Satisfaction with working hours	2.63 (0.74)	0.170* (0.094) [0.131]	0.227**†† (0.091) [0.035]	0.229* (0.126) [0.153]	0.306**†† (0.122) [0.041]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.2: Perceptions of work

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Has to do things she disapproves of	1.94 (0.90)	-0.137 (0.095) [0.263]	-0.198* (0.109) [0.163]	-0.153 (0.106) [0.263]	-0.220* (0.121) [0.163]
Works with the fear of losing their job	1.30 (0.71)	-0.136** (0.066) [0.165]	-0.133** (0.063) [0.163]	-0.191** (0.093) [0.165]	-0.187** (0.088) [0.163]
Can balance work and personal life	2.89 (0.84)	0.055 (0.103) [0.627]	0.123 (0.105) [0.287]	0.066 (0.122) [0.627]	0.146 (0.124) [0.287]
Feels their work is recognized at its fair value	1.91 (0.94)	-0.165 (0.113) [0.263]	-0.168 (0.138) [0.287]	-0.177 (0.121) [0.263]	-0.179 (0.147) [0.287]
Is proud to work for the organization	3.35 (0.67)	0.132** (0.063) [0.165]	0.169* (0.087) [0.163]	0.198** (0.094) [0.165]	0.253* (0.130) [0.163]
Shares the organization's values	3.10 (0.69)	0.101 (0.076) [0.263]	0.159* (0.092) [0.163]	0.146 (0.109) [0.263]	0.229* (0.133) [0.163]
Trusts the organization to treat her well	3.15 (0.73)	0.035 (0.072) [0.627]	0.070 (0.089) [0.432]	0.048 (0.098) [0.627]	0.096 (0.121) [0.432]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		211	187	211	187

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.3: Absences and health stoppages

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
At least one sick leave	0.57 (0.49)	-0.017 (0.055) [0.841]	-0.043 (0.051) [0.502]	-0.035 (0.112) [0.865]	-0.087 (0.103) [0.525]
Number of days on sick leave	17.48 (40.07)	-10.632**† (4.981) [0.096]	-9.351* (4.950) [0.145]	-0.265** (0.124) [0.102]	-0.233* (0.124) [0.174]
At least one work stoppage (work accident)	0.12 (0.37)	-0.001 (0.039) [0.972]	-0.019 (0.041) [0.653]	-0.004 (0.108) [0.972]	-0.051 (0.112) [0.653]
Number of days on work stoppage (work accident)	2.89 (10.24)	-2.348**† (1.090) [0.096]	-2.807**† (1.047) [0.092]	-0.229** (0.107) [0.102]	-0.274**† (0.102) [0.083]
At least one stoppage due to a commuting accident	0.08 (0.31)	0.027 (0.032) [0.496]	0.025 (0.034) [0.511]	0.088 (0.103) [0.529]	0.081 (0.109) [0.525]
Number of days on leave due to a commuting accident	2.20 (10.46)	-1.172 (0.742) [0.243]	-1.013 (0.617) [0.155]	-0.112 (0.071) [0.243]	-0.097 (0.059) [0.174]
Feels my job harms my health	2.14 (0.92)	-0.235**† (0.103) [0.096]	-0.273**† (0.120) [0.092]	-0.256** (0.112) [0.102]	-0.298** (0.130) [0.110]
Went to work while sick	1.90 (0.94)	0.173 (0.139) [0.315]	0.224* (0.127) [0.145]	0.184 (0.148) [0.353]	0.239* (0.136) [0.174]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		201	180	201	180

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.4: Absenteeism

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Never absent outside sick leave/work accident/paid leave	0.68 (0.48)	0.018 (0.038) [0.642]	0.047 (0.051) [0.498]	0.038 (0.080) [0.642]	0.099 (0.106) [0.498]
Sometimes absent due to a personal emergency	0.23 (0.42)	0.015 (0.029) [0.642]	-0.022 (0.037) [0.608]	0.036 (0.068) [0.642]	-0.052 (0.088) [0.608]
Often absent due to a personal emergency	0.00 (0.10)	-0.024** (0.010) [0.123]	-0.034** (0.015) [0.215]	-0.229** (0.093) [0.123]	-0.322** (0.144) [0.215]
Sometimes absent due to fatigue	0.01 (0.15)	0.008 (0.013) [0.642]	0.020 (0.018) [0.489]	0.055 (0.087) [0.642]	0.133 (0.122) [0.489]
Often absent due to fatigue	0.00 (0.07)	-0.007 (0.006) [0.522]	-0.010 (0.008) [0.489]	-0.112 (0.086) [0.522]	-0.158 (0.115) [0.489]
Sometimes absent for another reason	0.10 (0.29)	0.009 (0.017) [0.642]	0.014 (0.027) [0.608]	0.032 (0.058) [0.642]	0.049 (0.095) [0.608]
Often absent for another reason	0.01 (0.08)	0.005 (0.004) [0.522]	0.007 (0.006) [0.489]	0.066 (0.054) [0.522]	0.085 (0.075) [0.489]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		223	198	223	198

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.5: Karasek scores

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Psychological demand score	21.84 (5.20)	-1.108**† (0.534) [0.058]	-1.253**†† (0.496) [0.023]	-0.213**† (0.103) [0.060]	-0.241**†† (0.095) [0.024]
Decision latitude score	67.30 (9.41)	0.392 (0.737) [0.597]	0.032 (1.026) [0.976]	0.042 (0.078) [0.597]	0.003 (0.109) [0.976]
Social support score	23.40 (4.51)	1.344**†† (0.552) [0.045]	1.824***†† (0.582) [0.013]	0.298**† (0.123) [0.052]	0.405***†† (0.129) [0.012]
Job strain	0.23 (0.43)	-0.108**†† (0.040) [0.045]	-0.139***†† (0.048) [0.013]	-0.252**† (0.094) [0.052]	-0.326***†† (0.111) [0.015]
Iso-strain	0.16 (0.36)	-0.073**† (0.034) [0.058]	-0.080*† (0.042) [0.087]	-0.202**† (0.092) [0.060]	-0.219*† (0.116) [0.090]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.6: Results on psychological demand indicators

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Psychological demand score	21.84 (5.20)	-1.108**† (0.534) [0.075]	-1.253**†† (0.496) [0.039]	-0.213**† (0.103) [0.075]	-0.241**†† (0.095) [0.039]
My work requires me to work very fast	2.89 (0.94)	-0.078 (0.069) [0.355]	-0.046 (0.067) [0.602]	-0.083 (0.073) [0.355]	-0.048 (0.071) [0.602]
My work requires me to work intensely	2.93 (0.90)	-0.102 (0.083) [0.341]	-0.104 (0.079) [0.259]	-0.114 (0.093) [0.341]	-0.116 (0.088) [0.259]
I am asked to do an excessive amount of work	2.56 (0.93)	-0.273***††† (0.098) [0.041]	-0.299**††† (0.120) [0.039]	-0.294***††† (0.105) [0.041]	-0.322**††† (0.129) [0.039]
I have enough time to do my work properly	2.15 (0.87)	0.246***††† (0.085) [0.041]	0.258**††† (0.100) [0.039]	0.284***††† (0.098) [0.041]	0.297**††† (0.115) [0.039]
I receive contradictory instructions from other people	1.95 (0.91)	-0.005 (0.097) [0.962]	-0.004 (0.111) [0.971]	-0.005 (0.107) [0.962]	-0.004 (0.123) [0.971]
My work requires long periods of intense concentration	2.23 (0.90)	-0.097 (0.123) [0.524]	-0.178 (0.128) [0.258]	-0.107 (0.137) [0.524]	-0.198 (0.142) [0.258]
My tasks are often interrupted before they are completed	1.97 (0.88)	-0.169**† (0.074) [0.075]	-0.155*† (0.080) [0.099]	-0.192**† (0.085) [0.075]	-0.177*† (0.091) [0.099]
My work is very hectic	2.33 (0.96)	-0.264**††† (0.099) [0.041]	-0.245**††† (0.101) [0.039]	-0.276**††† (0.103) [0.041]	-0.256**††† (0.105) [0.039]
Having to wait for colleagues' or other departments' work	2.14 (0.98)	0.013 (0.109) [0.962]	-0.041 (0.107) [0.764]	0.013 (0.112) [0.962]	-0.042 (0.109) [0.764]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.7: Results on social support indicators

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Social support score	23.40 (4.51)	1.344***††† (0.552) [0.042]	1.824***††† (0.582) [0.006]	0.298**††† (0.123) [0.042]	0.405***††† (0.129) [0.006]
My supervisor cares about employees' well-being	2.96 (0.83)	0.194* (0.107) [0.103]	0.331***††† (0.093) [0.004]	0.234* (0.129) [0.103]	0.399***††† (0.112) [0.004]
My supervisor pays attention to what I say	2.98 (0.86)	0.156 (0.098) [0.137]	0.264***††† (0.094) [0.012]	0.181 (0.114) [0.137]	0.306***††† (0.109) [0.012]
My supervisor helps me complete my tasks	2.90 (0.82)	0.150*†† (0.077) [0.088]	0.258***††† (0.073) [0.004]	0.184*†† (0.094) [0.088]	0.316***††† (0.089) [0.004]
My supervisor is good at making employees cooperate	2.88 (0.76)	0.202**††† (0.087) [0.046]	0.313***††† (0.090) [0.004]	0.267**††† (0.115) [0.046]	0.413***††† (0.118) [0.004]
The colleagues I work with are professionally competent	2.97 (0.78)	0.135 (0.086) [0.137]	0.130 (0.107) [0.252]	0.173 (0.111) [0.137]	0.168 (0.137) [0.252]
The colleagues I work with show interest in me	2.85 (0.79)	0.270***††† (0.087) [0.037]	0.194* (0.112) [0.113]	0.342***††† (0.110) [0.037]	0.245* (0.142) [0.113]
The colleagues I work with are friendly	2.92 (0.81)	0.041 (0.086) [0.635]	0.021 (0.103) [0.839]	0.051 (0.107) [0.635]	0.026 (0.128) [0.839]
The colleagues I work with help me get the job done	2.93 (0.81)	0.228***††† (0.081) [0.040]	0.265***††† (0.099) [0.015]	0.280***††† (0.100) [0.040]	0.325***††† (0.121) [0.015]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.8: Results on decision latitude indicators

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Decision latitude score	67.30 (9.41)	0.392 (0.737) [0.798]	0.032 (1.026) [0.976]	0.042 (0.078) [0.798]	0.003 (0.109) [0.976]
In my work, I have to learn new things	3.00 (0.85)	0.013 (0.076) [0.901]	-0.029 (0.087) [0.976]	0.016 (0.089) [0.901]	-0.034 (0.103) [0.976]
My work involves repetitive tasks	3.37 (0.76)	0.092 (0.068) [0.798]	0.062 (0.079) [0.976]	0.121 (0.089) [0.798]	0.082 (0.104) [0.976]
My work requires me to be creative	2.82 (0.80)	0.264***††† (0.063) [0.001]	0.260***††† (0.081) [0.032]	0.328***††† (0.078) [0.001]	0.323***††† (0.101) [0.032]
My work often allows me to make decisions myself	3.04 (0.82)	0.076 (0.075) [0.798]	0.028 (0.097) [0.976]	0.093 (0.092) [0.798]	0.034 (0.118) [0.976]
My work requires a high level of skill	2.77 (0.85)	0.065 (0.082) [0.798]	0.031 (0.099) [0.976]	0.077 (0.097) [0.798]	0.037 (0.117) [0.976]
In my job, I have very little freedom to decide how I do my work	2.05 (0.87)	0.116 (0.101) [0.798]	0.096 (0.122) [0.976]	0.133 (0.116) [0.798]	0.110 (0.140) [0.976]
In my work, I have varied activities	2.97 (0.76)	-0.039 (0.080) [0.798]	-0.048 (0.095) [0.976]	-0.051 (0.105) [0.798]	-0.063 (0.125) [0.976]
I have the possibility to influence how my work is carried out	2.77 (0.82)	0.008 (0.064) [0.901]	-0.029 (0.086) [0.976]	0.010 (0.079) [0.901]	-0.036 (0.105) [0.976]
I have the opportunity to develop my professional skills	2.96 (0.76)	0.043 (0.099) [0.798]	0.043 (0.120) [0.976]	0.057 (0.131) [0.798]	0.057 (0.159) [0.976]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.9: Work emotions

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Tense	2.87 (1.00)	-0.120 (0.113) [0.291]	-0.112 (0.132) [0.401]	-0.120 (0.113) [0.291]	-0.113 (0.133) [0.401]
Happy	3.83 (0.89)	0.160*† (0.083) [0.074]	0.254***†† (0.092) [0.032]	0.180*† (0.093) [0.074]	0.285***†† (0.103) [0.032]
Stressed	2.90 (1.06)	-0.249**† (0.097) [0.052]	-0.246**†† (0.101) [0.032]	-0.235**† (0.092) [0.052]	-0.232**†† (0.095) [0.032]
Sad	2.32 (0.96)	-0.211**† (0.102) [0.074]	-0.196 (0.138) [0.205]	-0.221**† (0.107) [0.074]	-0.205 (0.145) [0.205]
Discouraged	2.40 (1.08)	-0.287**† (0.120) [0.052]	-0.304**†† (0.124) [0.032]	-0.267**† (0.111) [0.052]	-0.283**†† (0.115) [0.032]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		228	204	228	204

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.10: Daily time decomposition and dead time

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Daily commuting time (min)	101.83 (73.19)	1.954 (9.692) [0.841]	-3.781 (10.482) [0.720]	0.027 (0.132) [0.841]	-0.052 (0.143) [0.720]
Estimated daily productive time (Care/Admin) (min)	391.15 (152.98)	7.831 (18.343) [0.783]	11.435 (20.512) [0.677]	0.051 (0.120) [0.783]	0.075 (0.134) [0.677]
Daily 'Dead Time' (waiting time and gaps) (min)	245.95 (145.53)	-16.781 (18.224) [0.634]	-27.029 (20.265) [0.332]	-0.115 (0.125) [0.634]	-0.186 (0.139) [0.332]
Efficiency ratio (Total Work / Amplitude)	0.66 (0.19)	0.014 (0.023) [0.762]	0.027 (0.025) [0.407]	0.077 (0.126) [0.762]	0.143 (0.134) [0.407]
Has breaks of 3 hours or more during a workday	2.48 (1.14)	-0.172* (0.096) [0.184]	-0.241** (0.117) [0.107]	-0.152* (0.084) [0.184]	-0.212** (0.103) [0.107]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		197	176	197	176

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.11: Contract, hours and daily amplitude

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Full-time contract	0.26 (0.44)	-0.029 (0.039) [0.495]	-0.052 (0.049) [0.639]	-0.067 (0.089) [0.495]	-0.117 (0.110) [0.639]
Number of weekly hours worked (declared, incl. travel)	30.88 (9.73)	0.669 (0.974) [0.495]	0.864 (1.099) [0.639]	0.069 (0.100) [0.495]	0.089 (0.113) [0.639]
Daily work amplitude (Departure to Return) (min)	714.56 (118.10)	-8.539 (11.027) [0.495]	-13.531 (13.193) [0.639]	-0.072 (0.093) [0.495]	-0.115 (0.112) [0.639]
Estimated total daily work time (incl. travel) (min)	482.38 (149.41)	16.754 (16.531) [0.495]	23.503 (17.003) [0.639]	0.112 (0.111) [0.495]	0.157 (0.114) [0.639]
Weekend work	3.60 (1.08)	-0.061 (0.073) [0.495]	0.002 (0.090) [0.985]	-0.056 (0.067) [0.495]	0.002 (0.083) [0.985]
Unpaid overtime	2.24 (1.14)	-0.111 (0.134) [0.495]	-0.088 (0.166) [0.699]	-0.097 (0.118) [0.495]	-0.077 (0.146) [0.699]
Wants to work more	0.50 (0.50)	-0.068 (0.051) [0.495]	-0.050 (0.066) [0.639]	-0.136 (0.101) [0.495]	-0.100 (0.133) [0.639]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.12: Scheduling

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Knows tomorrow's schedule	0.97 (0.19)	0.032* (0.017) [0.313]	0.033 (0.024) [0.513]	0.167* (0.088) [0.313]	0.171 (0.123) [0.513]
Knows next week's schedule	0.83 (0.39)	0.014 (0.025) [0.792]	0.012 (0.032) [0.813]	0.036 (0.065) [0.792]	0.032 (0.084) [0.813]
Knows next month's schedule	0.43 (0.50)	-0.006 (0.047) [0.899]	-0.004 (0.043) [0.920]	-0.012 (0.094) [0.899]	-0.009 (0.086) [0.920]
Has atypical hours (early mornings, late evenings)	3.05 (1.20)	-0.039 (0.104) [0.817]	-0.087 (0.114) [0.616]	-0.033 (0.087) [0.817]	-0.073 (0.096) [0.616]
Has irregular or rotating schedules	3.09 (1.10)	-0.095 (0.113) [0.677]	-0.119 (0.120) [0.616]	-0.086 (0.103) [0.677]	-0.108 (0.110) [0.616]
Often has last-minute schedule changes	2.93 (0.88)	-0.142* (0.080) [0.313]	-0.088 (0.105) [0.616]	-0.162* (0.091) [0.313]	-0.101 (0.120) [0.616]
Can adapt work hours to personal constraints	2.57 (0.79)	0.316***†††† (0.064) [0.000]	0.356***†††† (0.079) [0.001]	0.401***†††† (0.082) [0.000]	0.452***†††† (0.101) [0.001]
Can change the order of visits/tasks	1.95 (0.55)	0.037 (0.042) [0.677]	0.071 (0.047) [0.513]	0.067 (0.076) [0.677]	0.130 (0.086) [0.513]
More constrained schedules over the past two years	2.63 (1.03)	-0.188 (0.156) [0.677]	-0.204 (0.178) [0.616]	-0.183 (0.152) [0.677]	-0.198 (0.173) [0.616]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.13: Work pace

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Is forced to hurry at work	2.74 (0.94)	-0.171**†† (0.078) [0.049]	-0.169 (0.101) [0.155]	-0.182**†† (0.083) [0.049]	-0.179 (0.108) [0.155]
Often has to stop a task to deal with an unplanned one	0.26 (0.45)	-0.125***††† (0.039) [0.012]	-0.115**† (0.052) [0.075]	-0.280***††† (0.088) [0.012]	-0.258**† (0.116) [0.075]
Has at least 2 consecutive days off per week	0.63 (0.50)	0.108**†† (0.042) [0.027]	0.115**† (0.052) [0.075]	0.217**†† (0.084) [0.027]	0.233**† (0.106) [0.075]
Relatives say they are not available enough because of work	2.88 (0.98)	-0.200**†† (0.079) [0.027]	-0.204**† (0.098) [0.079]	-0.204**†† (0.080) [0.027]	-0.208**† (0.100) [0.079]
Work pace has intensified over the past two years	3.96 (1.13)	0.178 (0.144) [0.261]	0.213 (0.165) [0.261]	0.157 (0.127) [0.261]	0.188 (0.146) [0.261]
More tasks to perform than two years ago	2.66 (0.93)	0.124 (0.109) [0.261]	0.069 (0.108) [0.524]	0.133 (0.117) [0.261]	0.074 (0.116) [0.524]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		207	185	207	185

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.14: Workload impacts

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Works beyond normal hours	3.21 (0.94)	0.049 (0.131) [0.712]	0.167 (0.135) [0.445]	0.052 (0.139) [0.712]	0.177 (0.143) [0.445]
Skips/shortens meals or does not take breaks	2.82 (1.16)	-0.037 (0.097) [0.712]	0.069 (0.112) [0.723]	-0.032 (0.083) [0.712]	0.059 (0.096) [0.723]
Does a task too quickly that would require more time/care	2.71 (1.06)	-0.305***††† (0.063) [0.000]	-0.263***††† (0.087) [0.016]	-0.288***††† (0.060) [0.000]	-0.249***††† (0.082) [0.016]
Works during rest time, leave, or sick leave	1.53 (0.91)	0.034 (0.085) [0.712]	-0.002 (0.088) [0.980]	0.037 (0.093) [0.712]	-0.002 (0.096) [0.980]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.15: Relationship with beneficiaries

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Available for clients/beneficiaries	3.49 (0.58)	0.001 (0.051) [0.990]	0.011 (0.056) [0.850]	0.001 (0.089) [0.990]	0.019 (0.097) [0.850]
Gives them enough time	2.98 (0.81)	0.143**†† (0.056) [0.032]	0.158**†† (0.071) [0.047]	0.176**†† (0.068) [0.032]	0.194**†† (0.087) [0.047]
Listens to their problems or difficulties	3.63 (0.51)	0.035 (0.050) [0.583]	0.080 (0.057) [0.196]	0.069 (0.098) [0.583]	0.156 (0.110) [0.196]
Can meet their expectations	3.16 (0.59)	0.093*† (0.048) [0.086]	0.134**†† (0.059) [0.047]	0.157*† (0.081) [0.086]	0.227**†† (0.100) [0.047]
Satisfaction with the care provided	8.43 (1.29)	0.302**†† (0.120) [0.032]	0.316**†† (0.132) [0.047]	0.234**†† (0.093) [0.032]	0.245**†† (0.103) [0.047]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.16: Work relationships

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Gets along well with colleagues	3.87 (0.95)	0.044 (0.147) [0.766]	0.080 (0.159) [0.773]	0.046 (0.154) [0.766]	0.084 (0.167) [0.773]
Can cooperate with colleagues to do the job properly	2.97 (0.82)	0.184* (0.100) [0.359]	0.221* (0.111) [0.134]	0.224* (0.121) [0.359]	0.269* (0.135) [0.134]
Trusts colleagues to help in case of problems	3.24 (0.77)	0.044 (0.086) [0.766]	0.028 (0.105) [0.794]	0.057 (0.111) [0.766]	0.036 (0.137) [0.794]
Trusts management to help in case of problems	3.29 (0.72)	0.134 (0.116) [0.424]	0.226** (0.107) [0.134]	0.185 (0.160) [0.424]	0.311** (0.148) [0.134]
Receives satisfactory support in difficult situations	4.44 (0.98)	0.178 (0.122) [0.379]	0.177 (0.160) [0.456]	0.182 (0.125) [0.379]	0.181 (0.163) [0.456]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		206	185	206	185

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.17: Intentions to stay

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Wants to stay next year	2.72 (0.54)	0.089 (0.057) [0.251]	0.167**†† (0.071) [0.048]	0.165 (0.106) [0.251]	0.309**†† (0.132) [0.048]
Wants to stay next 5 years	2.48 (0.78)	-0.058 (0.100) [0.565]	-0.086 (0.094) [0.366]	-0.074 (0.128) [0.565]	-0.109 (0.120) [0.366]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		181	160	181	160

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.18: Reasons to leave

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Wants to change employer while keeping the same job	0.00 (0.13)	0.008 (0.011) [0.518]	0.021 (0.016) [0.193]	0.059 (0.082) [0.518]	0.158 (0.119) [0.193]
Wants to change occupation	0.01 (0.25)	-0.059* (0.030) [0.105]	-0.068**†† (0.028) [0.042]	-0.239* (0.120) [0.105]	-0.277**†† (0.115) [0.042]
Would like to, or will, stop working	0.03 (0.14)	0.004 (0.006) [0.518]	0.015 (0.011) [0.193]	0.028 (0.042) [0.518]	0.110 (0.077) [0.193]
Other reason	0.02 (0.20)	-0.059**† (0.024) [0.078]	-0.080***††† (0.024) [0.006]	-0.288**† (0.119) [0.078]	-0.395***††† (0.117) [0.006]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		232	207	232	207

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin (self or parents born abroad). Estimates are based on a balanced panel of 232 workers who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the the unit of randomization (typically the team) level. Daggers indicate sharpened two-stage q-values (FDR) to account for multiple testing. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

3.D.2 Results on care recipients

Table 3.D.19: Mental and physical health outcomes

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Mental health index (MH5)	41.14 (22.79)	-2.168 (2.374) [0.573]	-2.154 (2.533) [0.628]	-0.095 (0.108) [0.573]	-0.098 (0.115) [0.628]
Felt happy	3.34 (0.92)	0.219** (0.096) [0.166]	0.235** (0.101) [0.156]	0.237** (0.104) [0.165]	0.254** (0.110) [0.156]
Felt relaxed	3.20 (1.04)	0.182 (0.136) [0.570]	0.167 (0.155) [0.628]	0.174 (0.130) [0.570]	0.160 (0.148) [0.628]
Felt sad	2.67 (1.13)	-0.059 (0.166) [0.737]	-0.081 (0.152) [0.718]	-0.052 (0.154) [0.737]	-0.075 (0.141) [0.718]
Felt discouraged	2.47 (1.14)	0.063 (0.162) [0.737]	0.043 (0.168) [0.798]	0.055 (0.143) [0.737]	0.038 (0.147) [0.798]
Felt nervous	2.60 (1.21)	-0.181 (0.180) [0.573]	-0.157 (0.192) [0.628]	-0.150 (0.153) [0.573]	-0.134 (0.164) [0.628]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		118	120	120	120

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.20: Evaluation of professionals

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Relationship quality	3.44 (0.54)	0.034 (0.111) [0.759]	0.052 (0.104) [0.819]	0.063 (0.204) [0.759]	0.095 (0.190) [0.819]
Availability	3.38 (0.58)	0.080 (0.097) [0.759]	0.096 (0.092) [0.819]	0.140 (0.168) [0.759]	0.167 (0.160) [0.819]
Listening capacity	3.43 (0.63)	-0.056 (0.135) [0.759]	-0.035 (0.117) [0.819]	-0.089 (0.214) [0.759]	-0.055 (0.186) [0.819]
Sense of initiative	3.29 (0.75)	-0.072 (0.157) [0.759]	-0.045 (0.146) [0.819]	-0.096 (0.209) [0.759]	-0.059 (0.195) [0.819]
Coord. with professionals	3.32 (0.74)	-0.123 (0.268) [0.759]	0.057 (0.246) [0.819]	-0.167 (0.364) [0.759]	0.077 (0.334) [0.819]
Coord. with caregivers	3.24 (0.67)	-0.184 (0.246) [0.759]	-0.308 (0.272) [0.819]	-0.277 (0.370) [0.759]	-0.462 (0.408) [0.819]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		129	123	129	123

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.21: Feelings of maltreatment

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Harsh words	1.14 (0.52)	-0.027 (0.122) [0.825]	-0.027 (0.115) [0.896]	-0.053 (0.237) [0.825]	-0.052 (0.222) [0.896]
Ignored	1.04 (0.51)	-0.056 (0.160) [0.825]	-0.068 (0.147) [0.896]	-0.110 (0.313) [0.825]	-0.133 (0.288) [0.896]
Neglected	1.19 (0.65)	-0.055 (0.168) [0.825]	-0.022 (0.167) [0.896]	-0.084 (0.257) [0.825]	-0.034 (0.256) [0.896]
Lack of privacy/rhythm respect	1.07 (0.50)	-0.171 (0.140) [0.825]	-0.160 (0.136) [0.896]	-0.342 (0.279) [0.825]	-0.320 (0.272) [0.896]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		112	109	112	109

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.22: Satisfaction with service

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Overall satisfaction	3.02 (0.71)	0.002 (0.086) [0.978]	0.012 (0.092) [0.894]	0.003 (0.121) [0.978]	0.017 (0.130) [0.894]
Schedule satisfaction	3.06 (0.72)	0.038 (0.124) [0.978]	0.081 (0.110) [0.768]	0.052 (0.173) [0.978]	0.113 (0.154) [0.768]
Respect of hours	3.18 (0.75)	-0.088 (0.165) [0.978]	-0.087 (0.154) [0.768]	-0.118 (0.221) [0.978]	-0.116 (0.206) [0.768]
Time spent satisfaction	3.12 (0.66)	0.114 (0.114) [0.978]	0.139 (0.097) [0.633]	0.173 (0.174) [0.978]	0.211 (0.147) [0.633]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		129	121	129	121

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.23: Organization and Turnover

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Schedule changes	2.97 (0.98)	-0.028 (0.117) [0.965]	-0.039 (0.133) [0.989]	-0.029 (0.120) [0.965]	-0.040 (0.136) [0.989]
Bothered by schedule changes	0.68 (0.50)	-0.004 (0.088) [0.965]	-0.001 (0.090) [0.989]	-0.008 (0.178) [0.965]	-0.003 (0.181) [0.989]
Number of professionals/week	4.74 (2.29)	0.758 (0.613) [0.965]	1.058* (0.609) [0.551]	0.330 (0.267) [0.965]	0.461* (0.266) [0.551]
Professionals organize well	2.95 (0.77)	-0.041 (0.165) [0.965]	-0.063 (0.180) [0.989]	-0.054 (0.216) [0.965]	-0.083 (0.235) [0.989]
Bothered by change of professional	0.67 (0.49)	0.038 (0.097) [0.965]	-0.002 (0.102) [0.989]	0.077 (0.199) [0.965]	-0.003 (0.208) [0.989]
Knows who is coming	0.77 (0.41)	-0.011 (0.070) [0.965]	-0.003 (0.081) [0.989]	-0.026 (0.169) [0.965]	-0.006 (0.195) [0.989]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		134	126	134	126

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.24: Service quality

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Quality: Housekeeping	3.11 (0.68)	0.018 (0.090) [0.847]	0.044 (0.091) [0.631]	0.026 (0.132) [0.847]	0.064 (0.133) [0.631]
Quality: Healthcare	3.37 (0.67)	0.167 (0.101) [0.215]	0.109 (0.116) [0.537]	0.250 (0.151) [0.215]	0.164 (0.174) [0.537]
Quality: Meals	2.96 (0.63)	0.220 (0.163) [0.254]	0.172 (0.201) [0.537]	0.346 (0.257) [0.254]	0.271 (0.318) [0.537]
Quality: Groceries	3.43 (0.65)	1.000***††† (0.000) [0.000]	4.010***††† (0.000) [0.000]	1.530***††† (0.000) [0.000]	6.135***††† (0.000) [0.000]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		108	100	108	100

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.D.25: Satisfaction with the organization

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Ease of contact	3.13 (0.69)	0.051 (0.084) [0.551]	0.098 (0.080) [0.307]	0.074 (0.122) [0.551]	0.142 (0.116) [0.307]
Admin simplicity	3.21 (0.65)	0.093 (0.082) [0.519]	0.131 (0.088) [0.291]	0.144 (0.126) [0.519]	0.202 (0.136) [0.291]
Listening to needs	3.19 (0.69)	0.100 (0.154) [0.551]	0.102 (0.140) [0.471]	0.144 (0.221) [0.551]	0.147 (0.202) [0.471]
Flexibility	3.09 (0.70)	0.203 (0.158) [0.519]	0.202 (0.134) [0.291]	0.289 (0.225) [0.519]	0.287 (0.191) [0.291]
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		119	115	119	115

Notes:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Estimates are based on an initial balanced panel of 138 users who completed both baseline and endline questionnaires. Standard errors in parentheses are clustered at the team level. Daggers indicate BH-FDR adjusted q-values. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Heterogeneity effects by frequency of care visits

Table 3.D.26: Heterogeneity of Treatment Effects by Care Frequency

Outcome	Control Mean (SD)	Original Units		Standard Deviations	
		(1)	(2)	(3)	(4)
Panel A: High Frequency Care (≥ 5 days/week)					
Physical health	2.76 (2.76)	0.137 (0.175)	0.151 (0.175)	0.186 (0.237)	0.204 (0.238)
Mental health index (MH5)	46.81 (46.81)	1.132 (3.546)	0.437 (3.646)	0.054 (0.168)	0.021 (0.173)
Overall satisfaction	2.94 (2.94)	0.061 (0.108)	0.046 (0.131)	0.083 (0.147)	0.063 (0.179)
Care time adapted to needs	3.12 (3.12)	0.274 (0.189)	0.221 (0.201)	0.362 (0.249)	0.292 (0.265)
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		76	76	76	76
Panel B: Lower Frequency Care (< 5 days/week)					
Physical health	2.70 (2.70)	0.080 (0.224)	0.097 (0.258)	0.094 (0.266)	0.114 (0.306)
Mental health index (MH5)	36.35 (36.35)	-12.520** (5.567)	-13.954** (6.588)	-0.550** (0.244)	-0.613** (0.289)
Overall satisfaction	3.04 (3.04)	0.146 (0.193)	0.132 (0.212)	0.222 (0.294)	0.201 (0.324)
Care time adapted to needs	3.10 (3.10)	0.175 (0.161)	0.201 (0.209)	0.227 (0.210)	0.261 (0.272)
Baseline Controls		No	Yes	No	Yes
Fixed Effects		Service	Service	Service	Service
Observations		39	39	39	39

Note:

Intention-to-treat (ITT) estimates from ANCOVA specifications. Control Mean & SD represents the baseline average and standard deviation (in parentheses) for the control group. Columns (1) and (2) are expressed in original units. Columns (3) and (4) are expressed in standard deviations (SD). Columns (2) and (4) include baseline sociodemographic controls (age and living alone). Panel A presents results for patients with high care frequency (5 days or more per week), while Panel B focuses on patients with lower care frequency (less than 5 days per week). Standard errors in parentheses are clustered at the team level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

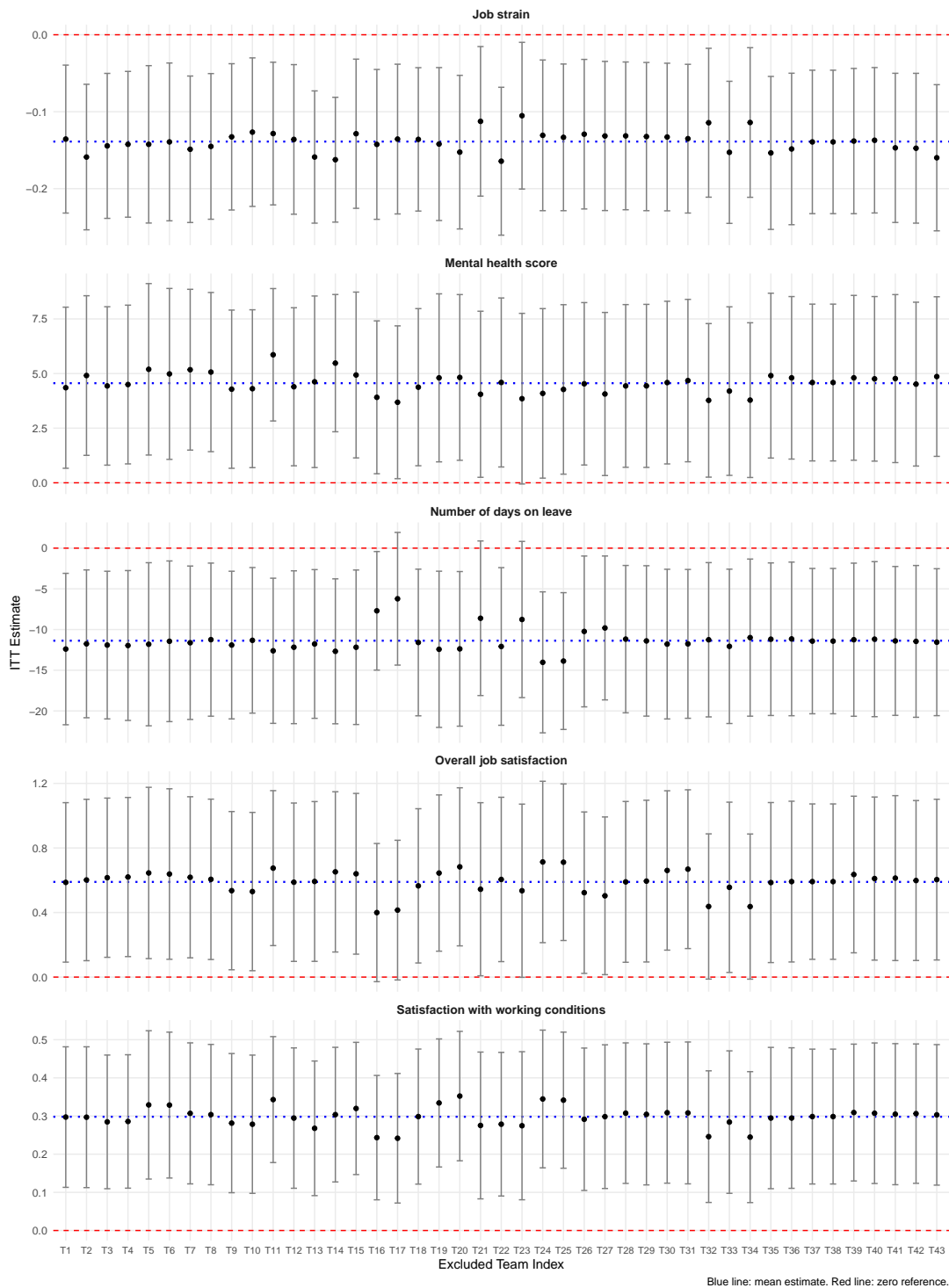
3.D.3 Robustness

Table 3.D.27: Robustness: Leave-One-Structure-Out Analysis (ANCOVA with Controls)

Excluding each Home-care Service	Service 1	Service 2	Service 3	Service 4	Service 5	Service 6	Service 7	Service 8	Service 9	Service 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Overall job satisfaction	0.288** (0.130)	0.287** (0.121)	0.284** (0.118)	0.314** (0.124)	0.300** (0.122)	0.306** (0.127)	0.293** (0.136)	0.246* (0.138)	0.177 (0.106)	0.355*** (0.125)
Satisfaction with working conditions	0.458*** (0.155)	0.480*** (0.152)	0.470*** (0.142)	0.508*** (0.152)	0.456*** (0.143)	0.492*** (0.155)	0.515*** (0.169)	0.509** (0.188)	0.396*** (0.135)	0.521*** (0.145)
Number of days on leave	-0.281** (0.113)	-0.282** (0.110)	-0.267** (0.106)	-0.258** (0.112)	-0.277** (0.108)	-0.264** (0.110)	-0.290** (0.129)	-0.190 (0.129)	-0.199* (0.100)	-0.346*** (0.115)
Job strain	-0.359*** (0.111)	-0.316*** (0.114)	-0.320*** (0.109)	-0.320*** (0.113)	-0.333*** (0.112)	-0.400*** (0.117)	-0.353*** (0.128)	-0.081 (0.160)	-0.314** (0.118)	-0.365*** (0.105)
Mental health score	0.227** (0.105)	0.235** (0.099)	0.242** (0.097)	0.256** (0.104)	0.241** (0.099)	0.261** (0.104)	0.297** (0.112)	0.073 (0.137)	0.189* (0.096)	0.357*** (0.083)
Fixed Effects	Service	Service	Service	Service	Service	Service	Service	Service	Service	Service
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	196	198	207	198	200	192	173	121	191	187

Notes: Intention-to-treat (ITT) estimates with outcomes in standardized units (SD). Each column reports the results of our preferred ANCOVA specification (including baseline value of the outcome and sociodemographic controls) while excluding one specific service from the sample to assess the stability of the results. Services are anonymized from 1 to 10. The baseline sociodemographic controls include: gender, age, couple status, presence of children, education level, tenure, fixed-term contract status, other occupation, past unemployment, and foreign origin. Estimates are based on a balanced panel of 232 workers. Standard errors in parentheses are clustered at the team level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Figure 3.D.1: Leave-One-Out-Team Analysis (ANCOVA with controls)



Notes: This figure shows a leave-one-team-out sensitivity analysis for ITT coefficients. Each point represents the estimate when excluding each team (T1–T43) from the sample, with outcomes expressed in original units. Vertical bars indicate 95% confidence intervals using team-clustered standard errors. The blue dotted line represents the mean estimate across all iterations, while the red dashed line marks zero. Models are estimated via ANCOVA with controls and baseline scores. The consistency of signs and stability of estimates confirm that no single team drives the results.

Table 3.D.28: Robustness: Attrition Analysis (Balanced Panel vs. Including Leavers)

	Standard Panel		Including Leavers	
	(1)	(2)	(3)	(4)
Overall job satisfaction	0.238*	0.288**	0.227*	0.275**
	(0.121)	(0.126)	(0.113)	(0.115)
Satisfaction with working conditions	0.414***	0.480***	0.420***	0.490***
	(0.134)	(0.145)	(0.135)	(0.151)
Satisfaction with pay	0.060	0.084	0.093	0.139
	(0.112)	(0.128)	(0.105)	(0.117)
Satisfaction with atmosphere	-0.004	0.025	-0.006	0.054
	(0.104)	(0.101)	(0.113)	(0.109)
Satisfaction with working hours	0.229*	0.306**	0.239*	0.347***
	(0.126)	(0.122)	(0.119)	(0.117)
Has to do things she disapproves of	-0.153	-0.220*	-0.170	-0.256**
	(0.106)	(0.120)	(0.113)	(0.123)
Works with the fear of losing job	-0.191**	-0.189**	-0.155*	-0.178*
	(0.093)	(0.090)	(0.084)	(0.090)
Can balance work and personal life	0.066	0.151	0.115	0.211
	(0.122)	(0.140)	(0.117)	(0.138)
Feels work is recognized at fair value	-0.177	-0.179	-0.177	-0.172
	(0.121)	(0.147)	(0.113)	(0.136)
Is proud to work for the organization	0.198**	0.254*	0.148	0.214*
	(0.094)	(0.131)	(0.093)	(0.127)
Shares the organization's values	0.146	0.234*	0.122	0.237*
	(0.109)	(0.132)	(0.110)	(0.135)
Trusts the organization	0.048	0.096	0.043	0.142
	(0.098)	(0.121)	(0.095)	(0.116)
Gets along well with colleagues	0.046	0.083	0.018	0.076
	(0.154)	(0.171)	(0.139)	(0.159)
Fixed Effects	Service	Service	Service	Service
Controls	No	Yes	No	Yes
Observations	232	207	245	220

Notes: Intention-to-treat (ITT) estimates with outcomes in standardized units (SD). Columns (1) and (2) use the standard balanced panel (N = 232). Columns (3) and (4) include leavers (N = 245), i.e., workers who were present at baseline but left the organization before endline, who provided both baseline and endline outcomes (N = 13). Columns (2) and (4) include baseline sociodemographic controls (gender, age, couple status, children, education, contract, occupation, past unemployment, and origin). Standard errors in parentheses are clustered at the team level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

3.E Other material

The following documents include (i) the specifications shared with participating home-care services, which outline the core requirements of the intervention, and (ii) a separate charter through which services formally commit to implement the model as defined and to comply with the experimentation rules, including its duration (generally 24 months).

Overall, key features of the self-managed team model include:

- Geographically concentrated teams of 5–10 workers sharing a stable group of users;
- Joint responsibility for planning and scheduling, with no middle management layer;
- Team decision-making by consent on key activities (recruitment, training, plan adjustments);
- Redefinition of the sector manager’s role from supervisor to coach;
- Dedicated team meetings and collective time for coordination and peer support;
- Enhanced communication tools to share information and coordinate care;
- Greater user and informal caregiver involvement in care planning.

Despite these commitments, treatment assignment was not always respected: in a few instances, control-group teams adopted the “self-managed team” model before the end of the experiment. These protocol deviations were reported to us by the participating organizations.

Modèle « équipe autonome »

Cahier des charges

I. Règles relatives à l'organisation des équipes

Les équipes doivent avoir une taille limitée, mais suffisante pour permettre des remplacements quand un salarié est absent. Les équipes devront se composer de 8 à 10 personnes, réparties sur une zone géographique réduite afin de limiter les temps de trajet, et avec un nombre maximal de bénéficiaires (à définir).

Objectif: cette équipe doit se composer de 8 à 10 salariés.

Les équipes doivent être autonomes avec peu de prescriptions de la structure centrale, ces dernières étant clairement formalisées dans un document et décidé en accord entre les équipes et la structure centrale. On peut envisager de fixer un nombre maximal de règles.

Prérequis: les règles sont explicitement fournies aux équipes, par écrit. Elles doivent autant que possible être communes aux structures participantes.

Objectif: les règles sont claires, peuvent évoluer et sont adaptées à la réalité.

Les équipes prennent leurs décisions au consentement, un modèle de décision qui repose sur le consensus, privilégie le dialogue et prend en compte les besoins de chacun. Une décision n'est pas adoptée si un des membres de l'équipe s'y oppose fortement.

Prérequis : les salariés doivent être formés à ce type de fonctionnement.

Objectif: chaque salarié a le sentiment d'être entendu lors de la prise de décision.

Les équipes doivent disposer d'un outil informatique commun permettant de faciliter la planification et la vérification du décompte des heures.

Prérequis: un smartphone par salarié équipé des logiciels adaptés et une tablette ou ordinateur portable par équipe.

Objectif: utilisation efficace des outils afin d'être à l'aise sur le logiciel de planning et la Télégestion. Les membres d'une équipe doivent avoir un outil pour échanger entre eux et avec d'autres équipes autonomes

Une forte polyvalence est nécessaire dans les équipes, qui réalisent aussi bien des missions d'intervention que les analyses préalables des besoins des bénéficiaires ou encore le décompte d'heures. Les équipes doivent être constituées sans se focaliser sur l'existence d'affinités.

Objectif: les équipes réalisent des tâches variées.

Les équipes définissent elles-mêmes les plannings afin d'organiser les déplacements de façon cohérente.

Objectif intermédiaire: le planning est réalisé par le coach puis modifié et validé par les équipes en réunion

Objectif: les équipes définissent elles-mêmes leur planning. L'un des membres de l'équipe assure le rôle de référent planification (à tour de rôle).

Le coach est un facilitateur, qui aide l'équipe à améliorer ses compétences internes, afin de les guider vers l'autonomie permettant de gérer les situations qui se présentent à elle. Le coach doit être efficace et bienveillant.

Objectif: le coach peut efficacement accompagner les équipes autonomes

II. Les règles relatives à la bonne prise en charge des bénéficiaires

Les équipes s'organisent comme elles le souhaitent pour être joignables par les bénéficiaires au minimum 5 jours sur 7, sur une plage horaire de 7 heures par jour. Une procédure de gestion des messages téléphoniques est mise en place afin de ne pas déranger les équipes pendant les interventions.

Objectif intermédiaire: un bénéficiaire peut à minima joindre le coach à tout moment de la journée. Ce dernier relaye le besoin aux équipes qui doivent rappeler le bénéficiaire si l'appel concerne leur champ de compétence.

Objectif: un bénéficiaire peut joindre directement l'équipe à tout moment de la journée aux horaires d'ouverture habituels.

Les intervenants sont stables, pour établir une relation individualisée entre l'aidant et le bénéficiaire.

Objectif intermédiaire: le bénéficiaire a un intervenant référent.

Objectif: le bénéficiaire a un intervenant habituel et maximum quatre remplaçants.

Une orientation vers des missions de prévention, par exemple une sortie collective ou une activité physique qui répondra obligatoirement aux impératifs du programme coordonné et de la procédure de financement de la conférence des financeurs

Prérequis: les Directeurs de SAAD doivent s'approprier l'outil de la Conférence des financeurs.

Objectif: les équipes ont réalisé au moins une action de prévention annuelle.

Les intervenants organisent un écosystème de service/soin autour des bénéficiaires, qui inclut par exemple un contact avec les aidants, les voisins ou encore les personnes en charge du suivi santé des bénéficiaires.

Objectif intermédiaire: le coach a un outil de référence qu'il remplit à la 1^{ère} visite et ces informations doivent être rendues accessibles aux salariés.

Objectif: les salariés sont en charge de cette mission.

Les équipes organisent la prise en charge du bénéficiaire en tenant compte de ses besoins et ses spécificités.

Objectif intermédiaire: le Département ne préconise plus de plan d'aide détaillé. Seule l'enveloppe d'heures, déterminée par l'intervenante médico-sociale en fonction du GIR et de l'évaluation globale, sera communiqué aux SAAD et aux bénéficiaires. Le coach organise lui-même le plan d'aide

Objectif: l'équipe doit être en mesure d'organiser elle-même les plans d'aide.

La communication auprès des familles sur l'organisation en place doit être simplifiée au maximum et ne pas engendrer de stress.

Prérequis : Elles doivent être outillées pour porter le message.

Objectif: les équipes doivent assurer la communication. L'équipe doit être en mesure d'expliquer elle-même son fonctionnement de façon positive.

Expérimentation des équipes autonomes : Charte d'engagement des structures participantes

Le passage au modèle en **équipes autonomes** est une **expérimentation** mise en place dans le secteur de l'aide à domicile, dans différents départements. Une évaluation scientifique, mise en œuvre par l'Institut des politiques publiques (voir encadré) est actuellement en cours. Cette évaluation vise à mesurer l'effet de ce changement d'organisation sur les conditions de travail des salariés, ainsi que sur la santé des personnes accompagnées. Si le modèle d'équipes autonomes a un effet positif sur les salariés et les bénéficiaires, l'évaluation constitue l'opportunité de le prouver de façon rigoureuse, afin de promouvoir ce modèle auprès d'autres structures et d'en faire le mode d'organisation principal dans le secteur de l'aide à domicile.

L'Institut des politiques publiques (www.ipp.eu)

L'Institut des politiques publiques a été créé dans le cadre d'un partenariat scientifique conclu par PSE-École d'Économie de Paris et le Centre de Recherche en Économie et Statistique (CREST). L'IPP vise à promouvoir l'analyse et l'évaluation quantitatives des politiques publiques en s'appuyant sur les méthodes les plus récentes de la recherche en économie.

Les travaux des chercheurs affiliés à l'IPP se rapportent à des thématiques qui couvrent un large éventail de politiques publiques : la fiscalité, les politiques sociales, les politiques d'emploi, l'éducation, la santé, les retraites, le logement, l'aménagement du territoire et les politiques sectorielles. Ils visent à la fois à développer la recherche scientifique dans le domaine des politiques publiques et à favoriser l'appropriation par les citoyens des termes du débat public. Pour ce faire, l'IPP publie ses travaux et résultats non seulement dans un format universitaire classique, mais également dans des formats mieux adaptés à un plus large public (notes et rapports).

L'IPP défend une stricte indépendance scientifique, tant vis-à-vis des financeurs publics que privés. À ce titre, tous les résultats des travaux menés par l'IPP sont rendus publics.

Une structure participant à l'expérimentation doit s'engager à respecter l'ensemble des règles décrites ci-dessous.

Engagement 1 : faire respecter le résultat du tirage au sort

Les structures doivent d'abord fournir l'identité des salariés présélectionnés et volontaires pour participer à l'expérimentation. Ces salariés peuvent être regroupés en équipe et les structures peuvent demander que les équipes déjà constituées soient maintenues. Un tirage au sort est ensuite effectué parmi les salariés ou équipes de salariés des structures d'aide à domicile participant à l'évaluation. Les salariés ou équipes tirés au sort dans le groupe « de traitement » adoptent le modèle en équipes autonomes. Les salariés ou équipes tirés au sort dans le groupe « de comparaison » ne changent pas de modèle et conservent leur organisation du travail habituelle.

Le tirage au sort est la condition sine qua none de participation à l'expérimentation et il doit impérativement être respecté. Si certains salariés ou équipes ne peuvent pas participer à l'expérimentation, cela doit être clairement indiqué avant de procéder au tirage au sort.

Respecter le tirage au sort implique ensuite les obligations suivantes :

- Pour les salariés tirés au sort (groupe traitement) : mettre en place le nouveau modèle organisationnel tel que défini par le cahier des charges (en Annexe) dans les 6 mois suivant le lancement de l'expérimentation, et le maintenir en place pour une durée minimale de 12 mois à l'issue des 6 premiers mois, sauf en cas de force majeure qui devra alors être discuté avec le comité de pilotage de l'expérimentation.
- Pour les salariés non tirés au sort (groupe de comparaison) : maintenir l'organisation en place pour une durée de 18 mois. En particulier, et sauf cas de force majeure qui devra impérativement être discuté avec le comité de pilotage, ne pas implémenter de changement organisationnel substantiel, qu'il s'agisse de l'organisation par équipes autonomes ou de toute autre innovation organisationnelle.

Aucun changement de salariés (que ce soit un ajout, une interversion ou un retrait) ne peut être fait parmi les équipes traitées et témoins au cours de l'expérimentation. Seul le départ d'un salarié participant à l'expérimentation peut justifier son remplacement via une nouvelle embauche ou un salarié de la structure n'ayant pas initialement participé à l'expérimentation. Ainsi, les salariés nouvellement embauchés ne peuvent rejoindre l'expérimentation qu'en cas de départ d'un salarié participant et afin de remplacer ce dernier. Par exemple, si un salarié remplace un salarié qui était dans le groupe de traitement, le salarié remplaçant sera aussi dans le groupe de traitement.

Engagement 2 : assurer le respect du cahier des charges pour le groupe de traitement

Le département de l'Eure, les premières structures participant à l'expérimentation et l'équipe d'évaluation ont défini un « cahier des charges » détaillant l'ensemble des changements organisationnels qui doivent obligatoirement être mis en place lors du passage au modèle par équipes autonomes. Les structures participantes s'engagent à mettre en place ces changements pour les salariés ou équipes de salariés tirés au sort pour être traités. Certaines adaptations additionnelles sont possibles pour s'adapter au mieux aux contraintes de chaque structure mais les changements précisés dans le cahier des charges demeurent non négociables et doivent être mis en place.

Engagement 3 : collaborer avec l'IPP pour faire passer des questionnaires auprès des salariés (groupe traitement et groupe témoin) et des bénéficiaires ou de leurs aidants

Les structures doivent aider l'IPP à collecter des données par questionnaire concernant les salariés participant à l'expérimentation (groupe de traitement et groupe de comparaison). Un premier questionnaire leur sera adressé avant la mise en place du nouveau modèle, et un second questionnaire entre 12 et 18 mois après sa mise en place. Ces questionnaires vont permettre une comparaison de l'évolution des indicateurs dans les deux groupes (traitement et comparaison).

Ces questionnaires seront administrés via des tablettes par l'équipe d'évaluateurs. Pour cela, un ou plusieurs membres de l'équipe d'évaluation devront pouvoir se rendre dans les locaux de la structure pendant une demi-journée ou une journée et rencontrer l'ensemble des salariés participant à l'expérimentation. Les structures participantes doivent donc s'engager à organiser au mieux la passation des questionnaires avec l'équipe d'évaluateurs. Il s'agit en particulier de trouver une plage horaire ou quelques plages horaires limitées permettant à l'équipe d'évaluateurs de rencontrer l'ensemble des salariés participant à l'expérimentation, de mettre à disposition une salle de taille suffisante pendant ces plages horaires, et de rémunérer une heure de travail correspondant à la passation du questionnaire pour chaque salarié participant.

Un questionnaire sera également adressé aux bénéficiaires ou à leurs aidants. Dans la mesure du possible la passation de ces questionnaires sera prise en charge par les CLIC (Centres Locaux d'Information et de Coordination) et l'aide des structures ne sera pas nécessaire. Cependant, en cas de difficultés, les structures doivent s'engager à faciliter autant que possible la passation de ces questionnaires additionnels auprès des personnes aidées.

NB : Les questionnaires permettront de recueillir le consentement des salariés et des personnes accompagnées ou de leurs aidants pour l'utilisation de leurs données personnelles à des fins de recherche, ainsi que des informations plus qualitatives sur la satisfaction au travail des salariés et sur la santé des bénéficiaires. La participation aux questionnaires n'est pas obligatoire. L'équipe d'évaluateurs s'engage à respecter l'ensemble des règles en vigueur en matière de protection des données à caractère personnel et se tiendra à disposition de chaque structure pour apporter les éclaircissements nécessaires concernant les modalités de protection des données et le respect des règles de confidentialité.

Engagement 4 : répondre au questionnaire mensuel de l'équipe d'évaluateurs par téléphone

Pour assurer le suivi de l'expérimentation, l'équipe d'évaluateurs doit pouvoir interroger chaque structure participante une fois par mois puis une fois tous les deux mois. Les structures s'engagent donc à pouvoir répondre à un bref questionnaire dispensé par téléphone au plus une fois par mois. Les questions porteront sur l'avancement de la mise en place du modèle par équipes autonomes, le respect du cahier des charges, le respect du tirage au sort, le départ ou l'arrivée de nouveaux salariés, le nombre de jours d'absence des salariés, dont les jours d'arrêts maladie, le départ ou l'arrivée de nouveaux bénéficiaires, ou encore des changements d'aide à domicile pour certains bénéficiaires concernés par l'expérimentation. En plus du questionnaire de suivi, les structures devront communiquer directement et dans les plus brefs délais à l'équipe d'évaluateurs tout changement majeur susceptible de mettre en péril le succès de l'évaluation. En particulier, tout événement susceptible de menacer le respect du tirage au sort devra être indiqué immédiatement.

Je soussigné(e), _____

1. M'engage à faire respecter le résultat du tirage au sort	
2. M'engage à assurer le respect du cahier des charges parmi les équipes du groupe de traitement	
3. M'engage à collaborer avec l'IPP pour faire passer des questionnaires auprès des salariés (groupe traitement et groupe témoin) et des bénéficiaires ou de leurs aidants	
4. M'engage à répondre au questionnaire mensuel de l'IPP par téléphone	

Date, signature

List of Figures

Chapter 1: Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019

- 1.1 Change in sexual violence victimization and police reporting rates 53
- 1.2 Multiple correspondence analysis 56

Chapter 2: Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women

- 2.1 Chronology of the French criminal procedure 105
- 2.1 Judicial handling of sexual and intimate partner violence cases (2012-2019) 111
- 2.2 Evolution of reported cases and dismissal rates (2012–2019) 113
- 2.3 Share of stories on VAW in national 8pm TV news, by type of VAW 115
- 2.4 Weekly evolution of news coverage of crimes against women in TF1 & France 2 8pm TV news 119
- 2.1 Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases 125
- 2.2 Effect of news coverage of crimes against women on the propensity to dismiss other offenses against the person 127
- 2.3 Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases 129
- 2.4 Heterogeneous effects of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, by case characteristics 131
- 2.5 Effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases reported to justice 133
- 2.6 Long-term effect of news coverage of crimes against women on conviction for sexual and intimate partner violence cases 135
- 2.B.1 Distribution of the average propensity to dismiss sexual and intimate partner violence cases per court 157
- 2.B.2 Distribution of the average propensity to dismiss sexual and intimate partner violence cases per county 158
- 2.B.3 Judicial handling of other crimes involving persons (2012-2019) 159
- 2.B.4 Prosecution rate by mode of prosecution 159
- 2.B.5 Number of cases of sexual and intimate partner violence registered within the criminal justice system, by type of case 160
- 2.B.6 Time elapsed between date of incident and justice arrival, by type of case . 160

2.B.7	Time evolution of victimization and reporting rates for sexual and intimate partner violence	161
2.B.8	Dismissal rate by type of case – Zoom on sexual violence type	161
2.C.1	Example of INA data structure	162
2.C.2	Words cloud	163
2.C.3	Examples of news on crimes against women in 8pm TV news (TF1 & France 2)	163
2.C.4	Breakdown of news stories on crimes against women (TF1 & France 2) . . .	164
2.C.5	Main source of news information	166
2.C.6	Main TV channel used for news information	167
2.C.7	Main source of news by occupational class	167
2.C.8	Audience profile of TF1/France 2 news viewers compared to population . .	168
2.C.9	Detailed CSP profile of TF1/France 2 news viewers compared to population	168
2.C.10	Share of stories on VAW (time) in national 8pm TV news, by type of VAW .	169
2.C.11	Share of stories on any crime in national 8pm TV news	169
2.C.12	Content of VAW stories in national 8pm TV news, by news category	170
2.C.13	Coverage of intimate partner violence in national 8pm TV news	170
2.C.14	Evolution of VAW coverage in TF1 & France 2 8pm TV news, by violence type	171
2.C.15	Content of VAW stories in TF1 & France 2 8pm TV news, by news category	172
2.C.16	Correlation between news coverage on 8pm TV news and other media . . .	172
2.C.17	Correlation of violence against women topics with other news topics	173
2.D.1	Illustration of the empirical design	174
2.D.2	Defining treated and control decision days	174
2.D.3	Seasonality of the number of news and judicial decisions by day of the week	175
2.E.1	Effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases reported to justice, including news on trials	178
2.E.2	Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice – Using more periods	179
2.E.3	Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by period	180
2.E.4	Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by case characteristics	181
2.E.5	Effect of news coverage of crimes against women on the number of cases of sexual and intimate partner violence reported to justice: heterogeneity by news characteristics	182
2.E.6	Effect of news coverage of crimes against women on the number of days elapsed between incident and justice arrival for sexual and intimate partner violence cases	183
2.E.7	Effect of news coverage of crimes against women on the incidence of sexual and intimate partner violence cases	184

2.E.8	Effect of news coverage of crimes against women on the incidence of female homicides	185
2.E.9	Effect of news coverage of crimes against women on the number of cases reported to justice, for cases that occurred before the news coverage (82% of cases)	186
2.E.10	Effect of news coverage of crimes against women on the number of other offenses against the person reported to justice	187
2.F.1	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Using more periods . . .	188
2.F.2	Cumulative effect of news coverage of crimes against women on the number of sexual and intimate partner violence cases dismissed or prosecuted over a 2-weeks horizon	189
2.F.3	Effect of news coverage of crimes against women on the propensity to dismiss murders and other intentional bodily harm	190
2.F.4	Effect of unrelated news coverage on the propensity to dismiss sexual and intimate partner violence cases	191
2.F.5	Falsification test for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases . .	192
2.F.6	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by news characteristics	193
2.F.7	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by other case characteristics	194
2.F.8	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by news content	195
2.F.9	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases: heterogeneity by court characteristics	196
2.F.10	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, for cases registered before the prosecutors’ decision (87% of cases)	201
2.F.11	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Using other measures of news coverage	202
2.F.12	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, including news on trials .	203
2.F.13	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, including periods with multiple days of VAW news coverage	203
2.F.14	Time evolution of the number of news on crimes against women and the number of dismissals for sexual and intimate partner violence cases	204

2.F.15	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, leaving out each news event day, month, and county	205
2.F.16	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases, adjusting for multiple hypothesis testing	206
2.F.17	Effect of news coverage of crimes against women on the number of days elapsed between justice arrival and prosecutor’s decision – Dismissal vs. Prosecution	215
2.F.18	Effect of news coverage of crimes against women on the propensity to prosecute sexual and intimate partner violence cases, by prosecution mode . . .	216
2.G.1	Effect of news coverage of crimes against women on sentence length for sexual and intimate partner violence	221
2.G.2	Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence: heterogeneity by case characteristics	222
2.G.3	Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence: heterogeneity by news characteristics	223
2.G.4	Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence, for cases prosecuted before the judge’s decision (91% of cases)	224
2.G.5	Effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases, including news on trials	225
2.G.6	Effect of news coverage of crimes against women on the number of days elapsed between prosecution and final court decision	226
2.G.7	Long-term effect of news coverage of crimes against women on sentence length for sexual and intimate partner violence cases	227
2.G.8	Long-term effect of news coverage of crimes against women on conviction and sentence length for sexual and intimate partner violence cases, among prosecuted cases only	228
2.H.1	Pre-trends of judicial outcomes (raw data)	231
2.H.2	Event-study analyses for the effect of #MeToo on judicial decisions	233

Chapter 3: The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment

3.1	Experimental design	248
3.1	Baseline psychological demand and decision latitude by home-care service	256
3.A.1	Surveyed workers and response rates	281
3.A.2	Number of respondent workers and care recipients at endline	283
3.A.3	Share of worker respondents by survey mode	284
3.A.4	Share of worker respondents by survey mode and assignment status	284

3.A.5	Distribution of the number of days since the first worker survey, by survey mode	285
3.A.6	Number of care workers in the experiment at baseline and endline	287
3.C.1	Mismatch between observed and assigned treatment, by mode of data collection	301
3.C.2	Potential treatment transmission or replication across groups	307
3.C.3	Workers' reactions to the random assignment	308
3.D.1	Leave-One-Out-Team Analysis (ANCOVA with controls)	334

List of Tables

Chapter 1: Understanding Sexual Violence Reporting Behavior: Evidence from France, 2007–2019

1.1	Construction of sociodemographic variables	51
1.2	Sample characteristics (2007-2019)	59
1.3	Determinants of the decision to report sexual violence to the police	60
1.4	Impact of SES and physical injuries on the reasons for not reporting sexual violence	62
1.A.1	Formulation of the survey questions regarding sexual violence committed outside or inside the current household	70
1.A.2	Construction of sociodemographic variables	71
1.A.3	Incident locations and perpetrator familiarity by type of sexual violence	72
1.A.4	Legal remedies undertaken by SV victims by type of sexual violence	72
1.A.5	Incident locations and perpetrator familiarity by type of sexual violence and socioeconomic status	73
1.B.1	Sample characteristics (2007-2019)	75
1.B.2	Incident locations and perpetrator familiarity by type of sexual violence	76
1.B.3	Legal remedies undertaken by male victims by type of sexual violence	76
1.C.1	Correlation matrix of socioeconomic variables	77
1.C.2	Influence of socioeconomic factors on the decision to report sexual violence to the police	78
1.D.1	Determinants of the decision to file a complaint for sexual violence	80
1.E.1	Determinants of the decision to report sexual violence to the police	82
1.E.2	Impact of SES and physical injuries on the reasons for not reporting sexual violence	83
1.F.1	Heterogeneous impact of sociodemographics by sexual violence type	85
1.F.2	Heterogeneous impact of assault characteristics by sexual violence type	86
1.F.3	Determinants of the decision to report sexual violence to the police, interacting socioeconomic status and physical injuries	87
1.F.4	Determinants of the decision to report sexual violence to the police, interacting socioeconomic status and being in a couple	87
1.G.1	Effect of victims' socioeconomic status on contact with social services	89
1.G.2	Effect of contact with social services on the decision to report sexual violence to the police	89
1.G.3	Effect of physical injuries on contact with emergency medical services	90
1.G.4	Effect of contact with emergency medical services on the decision to report sexual violence to the police	91

1.H.1	Effect of satisfaction with the police/justice system on the reporting decision	92
1.H.2	Effect of satisfaction with the police/justice system on the reasons for not reporting sexual violence	94

Chapter 2: Breaking News: How Media Coverage Shapes Judicial Responses to Violence Against Women

2.1	Summary statistics on judicial cases of sexual and intimate partner violence	110
2.2	Summary statistics on 8pm TV news content (TF1 & France 2, 2012-2019) .	118
2.1	OLS results for the effect of news coverage of crimes against women on each stage of the criminal justice process	126
2.C.1	Breakdown of news stories of crimes against women, by distance to the court	173
2.D.1	Covariate balance: Characteristics of cases examined by prosecutors, by media coverage on the days before the prosecutorial decision	176
2.D.2	Covariate balance: Characteristics of cases investigated by judges, by media coverage on the days before the judge decision	177
2.F.1	OLS results for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases	197
2.F.2	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases – Heterogeneity by case characteristics, period, news content, and court characteristics	198
2.F.3	Robustness checks for the effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases . . .	207
2.F.4	Legal identification and sample shares of other crime types in the justice data	211
2.F.5	Attrition Analysis: Comparison between Full and Restricted Samples	211
2.F.6	Effect of news coverage of other crimes on propensity to dismiss sexual and intimate partner violence	212
2.F.7	Effect of media coverage of other crimes on propensity to dismiss such other crimes: restricted sample	213
2.F.8	Effect of media coverage of other crimes on propensity to dismiss such other crimes: full sample	214
2.F.9	Effect of news coverage of crimes against women on the number of decisions taken by prosecutors	217
2.F.10	Effect of news coverage of crimes against women on prosecutor’s case processing time	218
2.F.11	Effect of news coverage of crimes against women on decisions taken by prosecutors for other crimes	219
2.F.12	Effect of news coverage of crimes against women on the propensity to dismiss sexual and intimate partner violence cases by number of news stories broadcast	220
2.G.1	OLS results for the long-term effect of news coverage of crimes against women on the propensity to convict and the sentence length for sexual and intimate partner violence cases	229
2.H.1	#MeToo movement and judicial decisions for cases of violence against women versus other harm to persons	234

2.H.2	#MeToo movement and judicial decisions for cases of violence against women versus other harm to persons – Heterogeneity	235
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Chapter 3: The Effects of Introducing Self-organization in Home Care Services: Evidence from a Randomized Experiment

3.1	Treatment–Control Balance of Socio-Demographic Characteristics at Baseline	257
3.2	Treatment–Control Balance of Main outcomes at Baseline (balanced panel)	258
3.1	First-stage (without new entrants)	260
3.1	Main outcomes for care workers	265
3.2	Main outcomes for care recipients	269
3.3	Main Outcomes: Robustness Specifications	274
3.A.1	Experimental design characteristics	280
3.A.2	Workers’ response rates by treatment assignment at baseline and endline .	282
3.A.3	Care recipients’ response rates by treatment assignment at baseline and endline	282
3.A.4	Days elapsed since the first survey administration	285
3.A.5	Treatment–Control Balance of Socio-Demographic Characteristics at Baseline (balanced panel)	286
3.A.6	Treatment–Control Balance of Main outcomes at Baseline (balanced panel)	286
3.A.7	Attrition and Reason for leaving by treatment status	288
3.A.8	Treatment versus control: reasons for exit	289
3.A.9	Treatment Assignment Effects on Attrition and Response Rates	290
3.A.10	Attrition and Non-response: Balance of Socio-Demographic Characteristics at Baseline	293
3.A.11	Comparison of Outcomes between Stayers and Leavers at Endline	293
3.A.12	Comparison of Outcomes between Stayers and Leavers at Baseline	294
3.A.13	Baseline Characteristics: Stayers vs. All Leavers by Treatment Status	294
3.B.1	Type of home-care service and organizational structure	295
3.B.2	Baseline descriptive statistics (workers)	295
3.B.3	Comparison with SUMER 2017 (baseline sample)	298
3.C.1	Non-compliance with treatment based on organizational monitoring files .	300
3.C.2	Administrative Non-Compliance: Durations and Exposure Ratios	300
3.C.3	First-stage (with new entrants)	302
3.C.4	Compliers versus non-compliers: sociodemographic characteristics	302
3.C.5	Compliers versus non-compliers (treated versus control): sociodemographic characteristics	303
3.C.6	Non-compliance with treatment on sociodemographic characteristics . . .	304
3.C.7	Compliers versus non-compliers: first stage	305
3.C.8	Home-care Service Characteristics by Assignment Compliance	306
3.D.1	Satisfaction	309
3.D.2	Perceptions of work	310
3.D.3	Absences and health stoppages	311
3.D.4	Absenteeism	312

3.D.5	Karasek scores	313
3.D.6	Results on psychological demand indicators	314
3.D.7	Results on social support indicators	315
3.D.8	Results on decision latitude indicators	316
3.D.9	Work emotions	317
3.D.10	Daily time decomposition and dead time	318
3.D.11	Contract, hours and daily amplitude	319
3.D.12	Scheduling	320
3.D.13	Work pace	321
3.D.14	Workload impacts	321
3.D.15	Relationship with beneficiaries	322
3.D.16	Work relationships	323
3.D.17	Intentions to stay	324
3.D.18	Reasons to leave	324
3.D.19	Mental and physical health outcomes	325
3.D.20	Evaluation of professionals	326
3.D.21	Feelings of maltreatment	327
3.D.22	Satisfaction with service	328
3.D.23	Organization and Turnover	329
3.D.24	Service quality	330
3.D.25	Satisfaction with the organization	331
3.D.26	Heterogeneity of Treatment Effects by Care Frequency	332
3.D.27	Robustness: Leave-One-Structure-Out Analysis (ANCOVA with Controls)	333
3.D.28	Robustness: Attrition Analysis (Balanced Panel vs. Including Leavers)	335

