The Impossible Evaluation of Risk[1]

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Summary

The current financial crisis stems from a massive under-estimation of mortgage risks, particularly of the subprime kind. This essay seeks to understand the origins of such an error. Economists most often advance the perverse incentive structure as the cause. This is a valid point, but it only provides a partial explanation. This text explores another hypothesis: the difficulty inherent in predicting the future when agents face uncertainty of a Knightian or Keynesian type. It seeks to show that economic uncertainty is of this type. Probability calculus cannot be applied to it. For that reason, economic uncertainty evades the only available method of prediction: statistical inference. Consequently, in a Knightian world, there is no such thing as an objective evaluation of risk. This point is illustrated by examining the case of the US presidential elections of 2000.

> These changes point towards greater use of judgment and less reliance on numbers in future. (*The Economist*, 2010, p. 7)

Introduction

The probabilistic model used in neo-classical finance fails to grasp the real difficulties that economic agents – including those that work in capital markets – encounter when trying to predict the future. It does not take into account the way that investors deliberate and make decisions. It does not provide a satisfactory basis for the analysis of financial reasoning. To penetrate this reasoning, I will focus on one particular variable that played a central role in triggering the financial crisis, namely the "subprime risk" – the default risk for subprime borrowers. One of the causes of the crisis was precisely the inability of the capital markets to evaluate this risk accurately. Thus, at the root of the crisis lay the massive inefficiency of these markets. To examine this issue, I shall bring into play the framework developed by John Maynard Keynes and apply it to the behaviour of economic agents over the period 2004–2007. Is the concept of Keynesian "uncertainty" more relevant than the neoclassical probabilistic hypothesis? The case of the 2000 presidential elections in the United States will be examined to help illustrate the answers to that question.

Uncertainty and subprime risk

The theoretical approach pioneered by Keynes takes the concept of uncertainty as a foundation for analysing the relation of capitalist economies to time. In a famous article published in 1937 in the *Quarterly Journal of Economics*, Keynes argues forcefully that the introduction of the long term into economic analysis radically undermines the "classical economic theory" that is focused on short-term changes, in other words an economy "in which the amount of the factors employed was given and the other relevant facts were known more or less for certain":

> The [classical economic theory] might work very well in a world in which economic goods were necessarily consumed within a short interval of time of their being produced. But it requires considerable amendment if it is to be applied to a world in which accumulation of wealth for an indefinitely postponed future is an important factor (p. 213).

The source of this "considerable amendment" is essentially of a cognitive nature, namely the fact that the knowledge that agents use to envisage the more distant future is of a different nature to the knowledge they use when their horizon is short term. It is much more indistinct and fluctuating: "The outstanding fact is the extreme precariousness of the basis of knowledge on which our estimates of prospective yield have to be made" (Keynes, 1936, p. 149). It is to define this particular dimension that Keynes introduces the concept of "uncertainty". Uncertainty is specific to our relation to the distant future, that is, a situation in which the fraction of objective knowledge shrinks to almost nothing: "If we speak frankly, we have to admit that our basis of knowledge for estimating the yield ten years hence [...] amounts to little and sometimes to nothing" (*ibid.*, pp. 149–50). Many of the problems encountered by neo-classical finance derive precisely from the fact that it models the relation of financial agents to the future in a strictly probabilistic form, thus rejecting the idea of Keynesian uncertainty.

Analysts generally use three essential parameters to define credit risk: (1) the probability of default by the borrower; (2) the expected severity of the loss if the borrower does indeed default; and (3) the "expected loss", which is the product of (1) multiplied by (2). Credit rating agencies are entrusted with the task of evaluating these risks, and they are supposed to do it as objectively and carefully as possible. Their methods are, thus, of particular interest to us.[3] Careful examination of their methods will allow us to understand how agents really carry out their predictions, what hypotheses they make, what methodology they follow. But first, let us look at the forecasts they made. How accurate were they?

[3] Note that the credit rating agencies differ from each other. Standard & Poor's and Fitch focus more on the probability of default (*PD*), while Moody's is more interested in the expected loss (*EL*). "The principles used to determine Fitch's collateralized debt obligation (CDO) ratings are very similar to those used [...] by Standard & Poor's in that they also target the tranche default probability. However, the process for determining Moody's ratings is somewhat more complex because they target expected loss and their targets are somewhat more stringent than those of Standard & Poor's and Fitch. Also, the expected loss-basis more accurately measures the risk associated with mezzanine tranches, which tend to have very high loss severities" (IMF, 2008, pp. 63–4). Standard & Poor's describe their own methodology as follows: "[w]e base our ratings framework on the likelihood of default rather than expected loss or loss given default. In other words, our ratings at the rated instrument level don't incorporate any analysis or opinion on post-default recovery prospects" (S&P, 2007, p. 3).

In 2004, the credit rating agencies evaluated the *EL* for subprime loans at about 4.5%. The figure grew over the next few years to reach 6% at the beginning of 2007. But this last estimate still fell far short of the actual losses incurred.[4] So the credit rating agencies seriously underestimated the subprime risk. The essential role that this underestimation played in the crisis cannot be stressed too strongly: it justified the high ratings that the agencies assigned to subprime mortgage-backed securities (MBS). Without these high ratings, subprime MBS would never have become so very widespread:

> The low expected loss assumptions were fundamental to the growth of subprime MBS in the four years leading up to the crisis. A low assumed expected loss is crucial for explaining how subprime mortgages were able to finance themselves more than 80% in the form of AAA debts, and more than 95% in the form of A, AA, or AAA debts, issued by subprime MBS conduits (Calomiris, 2008, p. 21).

Low expected loss assumptions were a decisive factor in subprime mortgages being financed more than 80% in the form of AAA debts, in other words evaluated as being of equal credit-worthiness as the sovereign debt of the United States! In a recent study, Donald MacKenzie (2009) compared the three-year default probability assumptions made by Standard and Poor's *CDO Evaluator* (in June 2006) with the realized default rate (observed in July 2009) for subprime MBS issued between 2005 and 2007, for given ratings. He obtained the table on the following page.

Mackenzie concludes: "In most rating categories, the recent incidence of subprime ABS [asset-backed securities] defaults has been over a hundred times the historical experience of ABSs as captured in *CDO Evaluator*'s assumptions" (pp. 57–8). Such a huge discrepancy calls for explanation. What on earth happened? How do credit rating agencies actually calculate the subprime risk?

[4] In October 2008, according to the Bank of England (2008, p. 7), the market evaluated this risk at 38%.

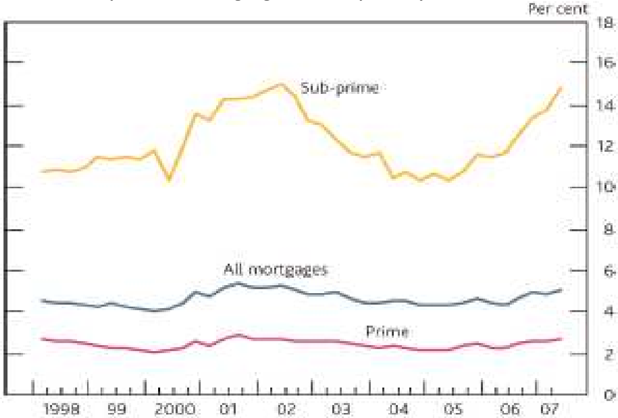
Table 1: *CDO Evaluator* 3-year default probability assumptions versus realized default rate of US subprime MBS issued from 2005 to 2007

	CDO Evaluator three-year default probability assumptions, as of June 2006 (percent)	Realized incidence of default, as of July 2009 (percent)
AAA	0.01	0.1
AA+	0.01	1.68
AA	0.04	8.16
AA-	0.05	12.03
A+	0.06	20.96
A	0.09	29.21
A-	0.12	36.65
BBB+	0.34	48.73
BBB	0.49	56.10
BBB-	0.88	66.67

Source: MacKenzie, 2009

They use the only method accepted in this field – statistical inference – which consists in extrapolating future default rates from the rates observed in the past. There are nevertheless several difficulties with this method. The first difficulty is purely technical, arising from the newness of subprime mortgages: there is very little historical data on which to base future estimates. They first emerged in the early1990s and only became significant at the very end of that decade. This means that estimates obtained through statistical analysis of the data are imprecise. Nevertheless, even in this short period, there was a subprime crisis during the recession of 2001 and 2002, with a significant increase in the default rate. Consequently, the credit rating agencies could consider that despite the lack of historical depth in the data, it was possible for them to make an accurate judgement about the level of subprime risk during periods of stress.

Figure 1: US subprime mortgage delinquency rate



Source: Buiter (2007)

Note: The y-axis plots the rate of default defined as at least one month's arrears in payment.

The second difficulty is more fundamental, because it is of a conceptual nature. It concerns the very validity of inductive methods. Statistical inference can only be used if the world is stationary, that is, if what happens tomorrow conforms to the regularities observed in the past. On this point, however, there is no certainty. The economic world and the social world are constantly changing: new techniques are invented, new behaviours appear, and new ideas radically alter our ways of doing things. All these processes are capable of permanently affecting social and economic relations, so that what has been observed in the past is no longer relevant for predicting the future. As regards our subject of interest, how can we be sure that the future subprime risk will be the same as the one that is present in the historical data? In this particular case, two phenomena have had a powerful impact on the nature of the subprime risk: credit issuing standards and housing prices. As the financial euphoria grew, lending institutions became ever more laxist in their subprime lending policies. The result was an endogenous deterioration of the subprime risk.[5] Moreover, the default rate is closely linked to the prevailing conditions on the

[5] In its *Financial Stability Report* of October 2007, the Bank of England noted a wide variation in the arrears rates of mortgage originators. This dispersion was held to be due to differences in underwriting standards and regional housing market conditions (chart 1.5, p. 17).

housing market, particularly price trends. And on this point, there is a vast difference between the recession of 2001–2002 and the one we are currently experiencing. In the former, house prices continued to rise, whereas they have fallen sharply since 2007. Consequently, there is a substantial difference between the subprime risk of 2001–2002 and that of 2007–2009. The subprime default rate, which had remained around 15%, has now reached 40%! Obviously, when prices fall, the probability of default rises because borrowers are reluctant to continue making repayments for properties that are worth less than the value of the outstanding loan (negative equity). Similarly, the loss incurred by lenders increases because the value of the assets they recover has fallen.

The credit rating agencies could, of course, incorporate these variables (in the form of N_t, the credit norm, and HPA_t, the home price appreciation rate at time t), into their analysis to obtain a model of the following form, where EL represents the expected loss:

(1) $$EL_t = f(N_t, HPA_t)$$

This presupposes that the relation between these variables has not changed, which brings us back to the hypothesis of stationarity. Furthermore, to predict the expected loss, equation (1) requires us to be able to evaluate the two exogenous variables. If equation (1) shows better understanding of the structure of the economy, from the forecasting point of view, the drawback is that it substitutes two variables (*N* and *HPA*) for one (*EL*). With hindsight, the credit rating agencies proved to be singularly incapable of predicting these variables accurately: they failed to form an accurate judgement on either the quality of the loans or the downturn in house prices. The result was a dramatic underestimation of the subprime risk. Only belatedly did they revise upwards their expected loss estimates. For instance, as recently as May 2007, the managing director of Moody's Investors Service declared before a Subcommittee of the US House of Representatives:[6] "Moody's is currently projecting that cumulative losses for loans backing 2006 subprime securitizations will generally range between 6% and 8% versus our original estimate of approximately 5.5% to 6%".

[6] Testimony before the Subcommittee on Financial Institutions and Consumer Credit, US House of Representatives, 8 May 2007, p. 16.

At that time, the default rate for the 2006 cohort of subprime loans still appeared to be in line with the 2001–2002 model, as shows a comparison of the graphs of the two cohorts 2006 and 2001. But the divergence then appeared very fast, and completely unforeseen. How can we account for such short-sightedness?[7]

The most widely proposed hypothesis to explain this inefficiency focuses on the structure of incentives that led credit rating agencies not to be too scrupulous, for fear of losing the substantial revenue they derived from the issuing of ABS and collateralized debt obligations (CDOs).[8] As far as it goes, I believe that to be perfectly correct. The deterioration in the quality of borrowers was not properly taken into account, despite the evidence of numerous indicators pointing to a dangerous decline. But this hypothesis alone cannot account for everything.[9] The huge discrepancy observed in Table 1 requires a much more powerful explanation.[10] I believe the answer lies in the shortcomings of the inferential method itself, and specifically the inability to imagine, in such a context, a drastic fall in housing prices. As many analysts have remarked: "The core assumption on which subprime lending had been based was the permanent appreciation of home prices" (Calomiris, 2008, p. 23). This error, however, was by no means specific to the credit rating agencies. It was widely shared. There was a *collective* belief underlying the conventions of evaluation during the period of euphoria. The inability to anticipate a fall of 30% in housing prices is a perfect illustration of the nature of economic uncertainty. Before 2007, nobody could have imagined such a development. It was absent from the historical data. It was this collective belief that led the credit rating agencies to seriously underestimate the housing bubble, and when they did modify their analyses, it was too late: "Broadly speaking, whereas in January 2007 (credit rating agencies) expected zero nationwide HPA during the housing market downturn, by July they had revised their expectation

[7] "In July 2007, as problems in subprime started to appear, loss assumptions increased substantially to roughly 8-11% […]. By the end of 2007, loss estimates had grown much more; in some subprime portfolios, estimated pool losses could exceed 50%" (Calomiris, note 15, p. 25).
[8] See Calomiris (2008).
[9] In particular, it does not explain why investors believed them so blindly, which raises serious questions about their rationality. On this point, see Orléan (2009).
[10] MacKenzie upholds a similar analysis: "Any accommodation by rating agencies to rating shopping is most unlikely to have been sufficient magnitude to explain the catastrophic performance of recent ABS CDOs and the dramatic change in the default behaviour of mortgage ABSs" (MacKenzie, 2009, p. 57).

to price in declines of about 10%, and by January 2008 to falls of 20%" (BIS, 2008, p. 5). Moreover, the systematic use of the term "Home Price Appreciation" (HPA) in itself shows the extent to which the idea of a fall in price was far from people's minds. Let us analyse this point by examining what economic agents thought about housing market trends in 2007. Did they believe there was a bubble? In a front-page article in June 2005, *The Economist* argued strongly in favour of this idea. But what about the markets?

The housing bubble

Let us start by recalling that a bubble should not be confused with an increase in prices.[11] All the analysts observed the strong growth in housing prices, but they did not interpret this situation as being the consequence of a bubble. The definition proposed by Joseph Stiglitz is often used to judge whether or not there is a bubble: "If the reason the price is high today is *only* because investors believe that the selling price will be high tomorrow – when 'fundamental' factors do not seem to justify such a price – then a bubble exists" (1990, p. 13).

At least until recently, most analysts considered that rising prices were explained by fundamental data. Consequently, they rejected the hypothesis of a bubble: the observed price rises were not due to speculation, but to an increase in the value of real estate. Jonathan McCarthy and Richard Peach, for example, wrote in an article in December 2005: "We argued that once the decline of nominal interest rates, other demand factors (including demographics), and improving quality of homes were taken into account, the rise of home prices through the middle of 2003 could be explained by fundamental factors" (p. 1).

These authors thus argue against the existence of a bubble. In particular, they stress the fact that price rises reflect an increase in the quality of the homes purchased.[12] A similar analysis is proposed by Charles Himmelberg *et al.* (2005):

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[11] As Case and Shiller forcefully put it: "the mere fact of rapid price increases is not in itself conclusive evidence of a bubble" (2003, p. 300).

[12] Note that this analysis led them to criticize certain indices that claim to be based on "constant quality" because they measure price changes for the same homes: for example the OFHEO (Office of Federal Housing Enterprise Oversight) "repeat sales index". McCarthy and Peach argue that this fails to take into account the fact that properties can be improved.

> As of the end of 2004, our analysis reveals little evidence of a housing bubble. In high appreciation markets like San Francisco, Boston and New York, current house prices are not cheap, but our calculations do not reveal large price increases in excess of fundamentals. […] Rather price growth is supported by basic economic factors such as low real long-term interest rates, high income growth and housing price levels that had fallen to unusually low levels during the mid-1990s (p. 68).

Both of these articles affirm that there is no such thing as a national housing market in the United States, but only regional markets disconnected from each other. McCarthy and Peach refer to "the disjointed nature of the US housing market" (p. 17), and according to Himmelberg *et al.* (p. 90): "house price dynamics are a local phenomenon, and national-level data obscure important differences among cities".

Because these studies date from 2005, I must reserve judgement as to the inadequacy of the statistical methods. Perhaps two years later the same tests would have detected a bubble. Moreover, these researchers are always very cautious in their conclusions. Nevertheless, these works show how controversial the question of a bubble can be, even in the face of unprecedented price increases. That is because the fundamental value is an ambiguous notion, dependent on the subjective interpretation of different authors. Even Karl Case and Robert Shiller, in 2003, observe that the fundamental analysis refutes the hypothesis of a housing bubble: "Clearly, one can construct an argument that home price increases nationally since 1995 have been driven by fundamentals" (p. 340). And when they do conclude that a bubble exists in three cities (Boston, Los Angeles, San Francisco), they do so on the basis of a study of the psychology of buyers, not on the fundamentals.

The view that there was no housing bubble in the United States found its most fervent champion in the person of Alan Greenspan, chairman of the Federal Reserve. This is particularly important because of the extraordinary aura surrounding Greenspan at that time. His judgement certainly influenced both the market players and the credit rating agencies. His point of view, expressed officially on 9 June 2005 before the Joint Economic Committee (JEC) of the US Congress, was that there was no housing bubble, but what he called "froth". This semantic invention was essential. It enabled him to acknowledge the steep climb in housing prices in various cities (who could deny it?), while rejecting the existence of a bubble. There is a huge difference between the two, because when there is a bubble, one must fear what necessarily follows: a crash. Froth, on the other hand, does not lead to a crash, and is therefore no danger to the US economy. He declared: "Although a 'bubble' in home prices for the nation as a whole does not appear likely, there do appear to be, at a minimum, signs of froth in some local markets where home prices seem to have risen to unsustainable levels".

To support his point of view, Greenspan stressed the fact that the US housing market is fundamentally heterogeneous, which has a strong limiting effect on speculation:

> The housing market in the United States is quite heterogeneous, and it does not have the capacity to move excesses easily from one area to another. Instead, we have a collection of only loosely connected local markets. Thus, while investors can arbitrage the price of a commodity such as aluminum between Portland, Maine, and Portland, Oregon, they cannot do that with home prices because they cannot move the houses. As a consequence, unlike the behaviour of commodity prices, which varies little from place to place, the behaviour of home prices varies widely across the nation.

In addition, to explain why housing prices had risen faster than the general price level, Greenspan invoked an argument based on "fundamentals": given that productivity gains in the housing sector had lagged behind those of the general economy, it led to the existence of a trend in its relative price:

> Because of the degree of customization of homes, it is difficult to achieve significant productivity gains in residential building despite the ongoing technological advances in other areas of our economy. As a result, productivity gains in residential construction have lagged behind the average productivity increases in the United States for many decades. This shortfall has been one of the reasons that house prices have consistently outpaced the general price level for many decades.

What followed was a globally optimistic doctrine, in the sense that Greenspan excluded the possibility of a nationwide fall in housing prices. There might be local price decreases, but these would have little in the way of repercussions, because securitization had made the financial system more robust by spreading the risk over the whole country:

> The U.S. economy has weathered such episodes before without experiencing significant declines in the national average level of home prices. […] Although we certainly cannot rule out home price declines, especially in some local markets, these declines, were they to occur, likely would not have substantial macroeconomic implications. Nationwide banking and widespread securitization of mortgages make it less likely that financial intermediation would be impaired than was the case in prior episodes of regional house price corrections.

Even in early 2007, Greenspan was still defending this theory in his book *The Age of Turbulence*, except that he now wrote in the past tense, because prices had stopped rising in 2006: "I would tell audiences that we were facing not a bubble but a froth—lots of small, local bubbles that never grew to a scale that could threaten the health of the overall economy" (2007, p. 256).

Then, to the question: "Are we heading towards a painful real estate crash?", his reply was a firm "no". He believed that prices were going to hold at their current levels, and he backed this up by drawing on the experience of Australia and Great Britain, where the real estate cycle was a year or two ahead of the United States: "Importantly, as the boom ended in those countries, prices levelled out or declined slightly but at this writing have not crashed" (*ibid.*, p. 257).

Greenspan's analysis seems perfectly representative of the knowledge and state of mind of those working in finance at the time: the possibility of a sharp fall in housing prices in the United States was considered a highly unlikely event.[13] Furthermore, statistical inference lent strong support to this conclusion, because since the 1930s, average house prices in the United States had never fallen! That is quite impressive. *The Economist* even used the term "mantra" to describe this deep-rooted

[13] And when pre-crisis reports did envisage a fall in house prices, it was never greater than 5%. Such moderation is quite revealing about the prevailing state of mind during the period of euphoria and deserves more systematic analysis.

belief: "…another mantra of housing bulls in America is that national average house prices have never fallen for a full year since modern statistics began".[14]

This clearly shows the limits of statistical inference. The possibility of a fall in prices was quite simply not in the data available in 2006! There is confirmation of this in the surveys conducted by Case and Shiller (2003) into the way US households envisage the housing market:

> One additional finding […] lends support to an important stylized fact about the U.S. housing market that has not been well documented in the literature, namely, that home prices are sticky downward. That is, when excess supply occurs, prices do not immediately fall to clear the market. Rather, sellers have reservation prices below which they tend not to sell. This tendency not to accept price declines is connected with a belief that prices never do decline, and with some of the parameters of thinking that underlie a housing bubble (p. 314).

So there was widespread belief in the idea that housing prices could not fall (they are "sticky downward"), a belief that played a large role in the perception of real estate as the "best investment" (Case and Shiller, page 340), because there is "only very little risk" (*ibid.*, p. 321): "the perception of risk of price decline is small: one may say that homebuyers did not perceive themselves to be in a bubble" (p. 322). As Case and Shiller put it: "the popular impression has been that real estate is an investment that cannot lose money" (p. 340). These examples show the extent to which long-lasting historical trends can lead to erroneous judgements. The fact that there had never been a nationwide decline in house prices, and little even in the way of local decreases,[15] generated a very optimistic conception of housing investment. On the basis of such past experience, how could anyone imagine a fall of more than 20%?

Greenspan was not alone in holding this view. It was expressed in all the reports published during this period, from the Fed, the Bank for International

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[14] *The Economist* (2005), p. 64. In fact, according to the index built by Case and Shiller, there was a slight fall of 0.9% in 1991.

[15] "The upward trend in home prices that is implied by the growth rate of income per capita, along with the tendency for home price decreases to be slow and sluggish, has meant that relatively few citywide home price declines have been observed in history" (Case and Shiller, p. 340).

Settlements, the International Monetary Fund and the European Central Bank. Take, for example, the testimony of Ben Bernanke – Alan Greenspan's successor at the Federal Reserve – on this same question of house prices, before the JEC on 27 April 2006. The most that he could acknowledge was that price rises might slow down: "House prices, which have increased rapidly during the past several years, appear to be in the process of decelerating, which will imply slower additions to household wealth and, thereby, less impetus to consumer spending."

At the time, inflation was the major preoccupation of the Federal Reserve. This declaration, suggesting a cooling-down of the overheating economy, was therefore greeted as good news. When it came to making a more direct forecast, Bernanke declared: "At this point, the available data on the housing market, together with ongoing support for housing demand from factors such as strong job creation and still-low mortgage rates, suggest that this sector will most likely experience a gradual cooling rather than a sharp slowdown." So that is what the best placed authorities to make macroeconomic projections were saying, as late as April 2006. They were still far from the idea that prices might fall. One year later, on 28 March 2007, Bernanke showed more awareness of the correction beginning to emerge on the housing market, but without any alarm: "Because of the decline in housing demand, the pace of house-price appreciation has slowed markedly, with some markets experiencing outright price declines".

This brief survey of beliefs about the housing market deserves more detailed study. It is a pity that economists – apart from some very rare exceptions – show so little interest in the representations formed by economic agents. These play an important role, but they remain largely ignored. This survey appears to supports two hypotheses: (1) the world is not stationary, and the long-lasting nature of past regularities offers no basis for an accurate prediction of the future; (2) to understand how agents form their expectations about the future, it is necessary to introduce the existence of conventions that shape the way investors relate to the future. Let us set this second hypothesis aside for the moment, and focus on the first:[16] the concept of Keynesian uncertainty. In other words, economic time is a discontinuous time, during which new events can occur. How can we model this situation? How can we predict risk in such a world?

[16] For the second hypothesis, see Orléan (1999).

Keynesian uncertainty

Neo-classical theory models the future in a very particular way. The basic hypothesis is that the future can be represented in the form of an exhaustive list of possible events (or states of the world). That is the canonical model. It appears in the context of the Arrow–Debreu general equilibrium model and is one of the foundations of neo-classical finance. In his book *Neoclassical Finance*, for example, Stephen A. Ross writes:

> To capture uncertainty, we will assume that there is a state space, Ω, and to keep the mathematics at a minimum, we will assume that there are only a finite number of possible states of nature:
>
> $$\Omega = \{\theta_1, \ldots, \theta_m\}$$
>
> The state space, Ω, lists the mutually exclusive states of the world that can occur, *m* (2004, p.4).

A probability π_i is attributed to each possible state. In this theoretical corpus, limited to one period for demonstrative purposes, the value of a security is obtained from the principle of no arbitrage and the hypothesis of market completeness. It is written:

(2) $$V(z) = \frac{1}{1+r} \sum \pi_i^* z_i$$

where *V(z)* is the value of the security *z*, *r* the risk-free interest rate, z_i the income obtained from *z* when the economy is at state θ_i, and π_i^* the risk-neutral probability.

This manner of viewing our relationship to the future is most singular, and this point needs to be emphasized. *The uncertainty that is taken into account is entirely objective*. It is the result of an effective variability governed by the fact that the economic factors depend on the state of the world θ. Depending on which state of the world arises, the economic variable will take a particular value. Consequently, probabilities are in no way a measure of the limits of our knowledge about the future. Quite the contrary, *the future is perfectly and entirely known*. It is just that it is of a probabilistic nature. This view of uncertainty is built along the lines as models of climatic risk: depending on what the weather is like tomorrow, agricultural productivity will vary in one particular way or another. Probabilities are the consequence of the intrinsic variability of the economic world, not of the difficulties we have in understanding that world. As an example, let us take a share. A given amount of dividend paid out is associated with each state of the world θ. Thus, a share is described by the payments it generates at each state: $d(\theta)$ for θ belonging to Ω, the set of states of the world. This hypothesis is clearly expounded by Robert Kast and André Lapied (1992, p. 23):

> (We can describe) uncertainty as follows: all economic situations relevant to the agents are listed in a set Ω. Each element θ of this set Ω characterizes a complete description of a possible state of the economy…. For any given θ, the dividend on each action is known. By this method, uncertainty is transferred from the dividends to the θ, called states of nature, states of the world, or simply states. In the end, a share is described by the payments it generates at each state: $d(\theta)$ for θ belonging to Ω (*our translation*).

The space Ω is then endowed with a measure that determines the probability of each state θ arising: "In a general asset market model, the hypothesis is made that the variations in payments for all the assets depend on a finite set of random variables that are assumed to be observable and of which the laws are known or can be estimated" (*ibid.*, p. 91). To summarize, neo-classical financial theory adopts the hypothesis that the future is objectively given in a probabilistic form. In such a context, uncertainty is entirely objective.

This representation of the future raises a number of problems that we shall now examine. First, can we really consider that the future exists objectively, even in a probabilistic form, at time t? Should we consider that the variability of dividends is given *ex ante*? How can this hypothesis be reconciled with the canonical representation of the past as the product of human action? My position is that the objectivity of the future at time t should be treated with caution, because the future results from the decisions taken by agents. It is the product of individual actions and only takes form once those actions have been decided. So, without wishing to enter too deeply into a long metaphysical debate, I believe it is wrong to consider future dividends to be determined before the actions have even been decided. Rather, we should consider that future dividends, like future profits, are not yet fixed, but depend on the actions that individuals choose to take.[17]

What significance could the idea of capital market efficiency have if profits were determined *ex ante*, even before the markets opened? The probabilistic representation adopted by neo-classical finance assumes just such an objective existence, which is why it must be rejected. It is true that individuals imagine what the future will be when deciding what action to take. This is an absolute necessity for them. So let us admit that they form an Arrow–Debreu type representation, in other words, a list of the events that could, in their opinion, occur. This is a defensible hypothesis, but under these circumstances, the representation is clearly subjective, not objective. Even assuming that this representation is shared by all the agents is already an extremely strong hypothesis that requires justification. If, on the other hand, the future is an objective fact like any other, this naturally leads us to suppose that rational, well-informed agents will necessarily end up adopting this same representation of the future. Just as rational and well-informed agents cannot fail to see the table in front of their eyes, so they will end up knowing the future, which presents itself in an equally inescapable fashion. Consequently, we will observe the necessary convergence of personal representations towards the "correct" representation, as long as the agents possess all the information and process it rationally. This is the essential theoretical point that characterizes the hypothesis of probabilistic objectivity of the future. Here, the fact that this objectivity is probabilistic is only secondary. This aspect was highlighted by Keynes, who understood perfectly that between certainty on the one hand, and uncertainty for which probabilities can be calculated on the other, the difference is negligible: "The calculus of probability was supposed to be capable of reducing uncertainty to the same calculable status as that of certainty itself" (Keynes, 1937, pp. 212–13).

The very existence of a future that is objectively given, even in a probabilistic form, constitutes a reference that prevents the subjective drift of estimations by rooting them in a foundation that rational activity cannot fail to recognize. As a consequence of this very strong hypothesis, it is possible to define an optimal expectation at time t, in other words, one that makes the best possible use of

[17] In the Arrow–Debreu general equilibrium model, all that is of no importance, as the economy stops at time 0.

all the relevant information available at that time. This expectation is thus independent of the idiosyncratic opinions of the agents, and can be said to be "rational". It can be expressed mathematically by means of the conditional expectation operator. As a result, it is possible, for any financial asset (that is, a right to uncertain future income) to define a best estimate, which becomes the reference estimate in the manner of equation (2) or the "fair value".

Both empirical studies and theoretical analysis lead me to criticize this approach. The future is not given objectively, and there is no objective value. The future is the result of the gambles made by economic agents. Our model must therefore take two loops into account: one going from the future to the present, in the form of subjective expectations, and the other producing the future as the result of actions taken today based on expectations about the future. This historical temporality is most certainly of a Keynesian nature. It is marked by the fragility of our knowledge about what will be tomorrow, as the subprime crisis has so forcefully illustrated. Consequently, there is no such thing as a best estimate or an objective value of securities. The capital market is not the place where true values are revealed, as the theory of financial efficiency would have us believe, but the place where the financial community wagers on what the future will be. Because they are wagers, they can be accurate or not. In fact, their accuracy cannot be judged *ex ante*; it is highly dependent on the dynamics that they set in motion. We must stick with this historical view of time: the future is opaque because it results from the strategic interaction between individual beliefs.

To my mind, the essential characteristic of the Keynesian view of the economy is the irreducible diversity of subjective estimates. It is in the nature of things that agents form diverse expectations. This hypothesis is the complete opposite to the one put forward by neo-classical theory, according to which there exists a "best expectation", in keeping with the objective probabilistic structure and the available information. In the Keynesian framework, equally rational and well-informed agents can hold different views about the future. This is because the world is not stationary: something new can appear to disrupt the regularities of the past. In a stationary world, on the contrary, it is possible to envisage a consensus between rational, well-informed agents. The observation of frequencies provides each individual with the same common basis for judging the probability of future events. Consequently, everyone shares the same analysis. The problem, however, is precisely that the economy is not stationary. Innovations emerge that permit agents to deviate from past frequencies, moving in directions which, by definition, cannot be understood in terms of past experience. It is a matter of judgement. Each individual can estimate the impact of these innovations as he or she sees fit. As Keynes wrote: "About these matters there is no scientific basis on which to form any calculable probability whatever. We simply do not know" (Keynes, 1937, p. 214).

Let us take the example of the dot.com bubble. With the computer revolution, many people thought that the economy was entering a "new age",[18] radically different from what had gone before. Certainly, many factors supported this point of view. But if that was the case – that is, if the lessons drawn from the past had become at least partially obsolete – then how was one to estimate values? Individuals were now free to form their own judgements, since in the absence of any historical perspective, nobody really knew what was going to happen. We then witnessed a proliferation of absolutely delirious estimates, based on extravagant fundamental scenarios. To those who objected that the hypotheses contained in these scenarios entailed growth rates or levels of productivity that had never been observed in the past,[19] it was replied that they were singularly lacking in imagination, and that just because something had never been observed in the past did not mean that it could not occur in the future. An irrefutable argument, and one that proved to be only too true! Nevertheless, from the moment that one rejects the lessons of the past on the (perfectly correct) grounds that the world is not stationary and new things are continually appearing, it is possible to counter every objection. There follows an irreducible subjectivity of fundamental valuation that seems to describe very accurately the situation in real economies. This result leads me to assert that the fundamental estimate must be considered as a pure opinion. The radical diversity of opinions characterizes the reality of the financial world, and this is what makes the capital markets so necessary. It is up to them to generate that which does not exist in the natural world: a benchmark reference.

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[18] This phenomenon is described by Robert Shiller (2001).

[19] We can only reject those scenarios that fail to respect the basic economic constraints, such as those that postulate a profit growth rate structurally higher than the rate of economic growth. But once that has been done, there still remains a very wide diversity of estimates.

All the important episodes of bubbles have been periods of major innovations, giving credibility to the hypothesis of the emergence of a new age[20] in which the regularities of the past are no longer valid. In doing so, they undermine important safeguards which, in normal times, impose limits on the deviation of individual expectations. The subprime crisis is of this type. People believed that the economy was more stable and more efficient. This gave rise to the term "the Great Moderation", expressing the idea that the developed economies had become less variable, more predictable. It was said that we had entered an era of low risk and high yields, a view confirmed by the historic records in terms of spread and volatility. The large wave of financial innovations that were developed during this period also lent support to this view. Securitization had made capitalism more structurally stable, first by spreading real estate risk over a large number of investors, instead of keeping it concentrated in banks, and second, by allowing the risk to be assumed by those best placed to do so: "These new participants, with different risk management and investment objectives [...], help to mitigate and absorb shocks to the financial system, which in the past affected primarily a few systemically important financial intermediaries" (IMF, 2006, p. 51). If agents believe in the emergence of a new age, that justifies the formation of different estimates to those observed in the past, because their expectations will differ according to how they perceive the impact of this new age. Consequently, diverse expectations can emerge, without there being any reason to believe that this diversity is the result of poor information or irrationality. This places strong limitations on the evaluation of risk. The view can no longer be maintained that there exists an objective evaluation that the agents must necessarily discover, as long as they make rational use of all the available information. To conclude, we shall now look at some of the different ways this radical uncertainty has been characterized, starting with Frank Knight, the first theorist of uncertainty.

The Knightian perspective

Knight distinguishes between three situations. The first is what he calls "*a priori* probabilities", corresponding to lottery-type situations in which, by construction,

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[20] Kindleberger (1978) acknowledged the importance of innovations through what he calls "displacements" (pp. 41–45), "some outside event that changes horizons, expectations, profit opportunities, behaviour" (p.41). This is precisely the view I am proposing here.

we are dealing with identical, equiprobable events. In this first situation, probability calculus is entirely applicable. This is the case for the game of roulette, for example. The second situation, which he calls “statistical probabilities”, corresponds to a configuration where one can group events together into classes that are sufficiently homogeneous for the observed frequencies to provide an acceptable approximation of probabilities. In the third situation, which he calls “judgements” or “estimates”, statistical inference can no longer be used because each event is too unique:

> We […] find three different types of probability situations:
> 1. *A priori probability*. Absolutely homogeneous classification of instances completely identical except for really indeterminate factors.
> 2. *Statistical probability*. Empirical evaluation of the frequency of association between predicates, not analyzable into varying combinations of equally probable alternatives.
> 3. *Estimates*. The distinction here is that there is no valid basis of any kind for classifying instances (Knight, 1921, pp. 224–5).

On this basis, the probabilistic approach cannot be applied to situations of the third type, corresponding to events without precedent, which the economy has never before experienced. Such is the nature of radical uncertainty, for both Knight and Keynes. It brings us back to the idea of non-stationarity, since it is based on the occurrence of new events, hitherto unknown. In conclusion, Knight draws a distinction between risk and uncertainty, depending on whether or not one can apply probability calculus. Risk corresponds to the first two situations (*a priori* probabilities and statistical probabilities), while uncertainty corresponds to the third (estimates):

> The practical differences between the two categories, risk and uncertainty, is that in the former the distribution of the outcome in a group of instances is known (either through calculation *a priori* or from statistics of past experience); while in the case of uncertainty this is not true, the reason being in general that it is impossible to form a group of instances, because the situation dealt with is in a high degree unique (*ibid.*, p. 233).

In situations of uncertainty, individuals must rely on their own judgement, because they do not possess an adequate basis for using statistical inference:

> The essential and outstanding fact is that the "instance" in question is so entirely unique that there are no others or not a sufficient number to make it possible to tabulate enough like it to form a basis for any inference of value about any real probability in the case we are interested in (*ibid.*, p. 226).

Saying that the uncertain is a matter of judgement means that there is a subjective element to it, that it is a question of *opinion*. For Knight, this element plays an essential role, because it makes all the difference between a good entrepreneur and a bad one. A good entrepreneur is one whose judgement turns out, with hindsight, to have been accurate. Because the economy is fundamentally uncertain, it requires the existence of entrepreneurs with good judgement. In the neo-classical probabilistic context, ultimately, everything can be calculated with a computer; the individual is not necessary. For Knight, on the contrary, the entrepreneur matters because judgement is important, and this is because the economy is fundamentally uncertain. The conclusion to be drawn from this argument is that in a situation of uncertainty, estimates about the future possess an irreducibly subjective dimension; they are *opinions*.

Keynes uses the same terminology. What he calls "uncertain" is the same as what Knight calls uncertain, as can be seen in this well-known passage:

> By "uncertain" knowledge, I do not mean merely to distinguish what is known for certain from what is only probable. The game of roulette is not subject, in this sense, to uncertainty.... Even the weather is only moderately uncertain. The sense in which I am using the term is that in which the prospect of a European war is uncertain ... or the position of private wealth-owners in the social system in 1970 (Keynes, 1937, pp. 231–4).

Once again, it appears that the uncertain is linked to non-stationarity, to radical novelty. In the face of the uncertain, everything is a matter of judgement. To appreciate the full critical weight of these propositions, one only has to recall that neo-classical theory takes the game of roulette (or the lottery) as a model to describe uncertainty.

In a much earlier paper (Orléan, 1989), I criticized the above quotation from Keynes, because it mixes events of very different natures, and as a result, the nature of what the uncertain is remains vague. In line with a reflection by Knight, I believe that the difference between risk and uncertainty is more of a quantitative than of a qualitative nature: "The principal subject for investigation is thus the *degree* of assimilability or the amount of homogeneity of classes securable or, stated inversely, the *degree of uniqueness* of various kinds of business contingencies" (Knight, 1921, p. 247). Such an approach does little to favour a clear definition of the uncertain. Moreover, the idea of novelty is in itself ambiguous. Many events that are new, in the sense that they have never been observed before, are perfectly predictable. Take, for example, the first satellite launched into space. In fact, novelty needs to be defined in relation to a state of knowledge: it is that which our knowledge is not capable of conceiving. This is the approach I proposed in 1989: "Novelty qualifies events for which there does not exist, *within the existing body of knowledge*, any basis on which to evaluate their plausibility" (Orléan, 1989, p. 245, *our translation*); in other words, it escapes from acquired knowledge. Now Karl Popper, in his reflection on the question of indeterminism and the laws of history, set out a powerful argument, according to which if there is one thing that fundamentally escapes from our current knowledge, it is *our future knowledge*: "There are certain things about ourselves which we cannot ourselves predict by scientific methods; more especially, *we cannot predict, scientifically, results which we shall obtain in the course of the growth of our own knowledge*" (Popper, 1982, p. 53).

I suggested that this uncertainty should be qualified as "epistemic", to underline its relation to knowledge. This epistemic uncertainty plays an important role in the economy, because in its functioning, the economy is highly dependent on knowledge – if only through production technologies. As we cannot predict scientifically what our knowledge will be tomorrow, it is impossible for us to know precisely what the state of the economy will be. It is a source of radical uncertainty.

More precisely, it is possible to consider a specific form of epistemic uncertainty, namely the observation of events that invalidate past knowledge[21] and require a profound reorganization of our knowledge. If Ω denotes the body of knowledge we possess today, obtained in particular by generalizing various past

[21] In Orléan (1989), I distinguished between two forms of epistemic uncertainty: the first, denoted I_1, corresponds to new knowledge that results in a "deepening or extension of the existing theory Ω" (p. 246, *our translation*) and the second, denoted I_2, corresponds to new knowledge that leads to "invalidation of the existing theory Ω" (*ibid.*).

observations into a law, then the set of these events can be identified with the Popperian concept of the class of potential falsifiers of the theory Ω. I shall denote this set *w*. Observing *w* leads to what Popper calls the falsification of Ω. By construction, the probability of *w* within the context of the theory Ω, denoted *Prob(w/ Ω)*, is equal to 0, since these are events that the theory Ω considers to be impossible.

Popperian epistemology, however, tells us that such falsification may perfectly well occur, even if our current knowledge cannot predict it. Moreover, a good deal of scientific work aims at such falsification. Consequently, it is perfectly rational, from the Popperian point of view, to consider that *w* may occur. Each individual, each researcher evaluates the probability of this event. In doing so, they express the level of their distrust of existing knowledge. This probability is of a purely subjective nature, since the only objective evaluation – that is, compatible with the body of knowledge Ω, – is 0. The interest of this presentation is that it provides an example in which the meaning of "subjective probability" is clear. It is a probability that diverges from the body of existing knowledge Ω. In these circumstances, the observed frequency, in this particular case 0 because the event *w* has never been observed, does not make it possible to calculate the probability of the event in question, namely the falsification of the theory.

In this example, we can recognize Popper's criticism of inductive inferences: there is no logical justification for moving from singular statements to universal statements. Just because the only swans observed up to now have been white does not give us the right to deduce that all swans are white. If one believes in the theory Ω, based solely on the observation of white swans, then one will write that the probability of observing a black swan is nil. And yet it is *possible* that a black swan will appear. It is just that the estimation of its probability is purely subjective.

This presentation of Knightian uncertainty shows that the possibility of drawing up an exhaustive list of all the events that might occur is far from obvious. Once we have listed all the events compatible with our state of knowledge at time t, which we have denoted Ω, there still remains a state of the world composed of events incompatible with Ω. By definition, this state of the world *w* is difficult or even impossible to describe, because it is contrary to current knowledge! I can illustrate this difficulty in drawing up, *ex ante*, an exhaustive list of possible states of the world with an example drawn from the US presidential election of 2000, between George W.

Bush and Al Gore. *A priori*, the situation appeared to be quite simple. For each voter, there were two possible states of the world, depending on whether they voted for Bush or for Gore. The reality was more complex, however. A first complexity arose because votes passed through the filter of voting machines. In Florida, two different types of machine were used: punch-card voting machines and optical scanners. One might imagine that this diversity would have little effect on the vote itself. The punch-card machines turned out to be less reliable, however, than the optical-scanner machines; on average, they produced three times as many errors. As Denis Lacorne wrote: "Everything depended on the county in which one voted. In the rich suburban counties, mainly Republican, the voters had the benefit of optical scanners; in the "inner-city" counties, most often poor and Democrat, voters had to make do with less efficient punch-card machines" (2001, p. 116). One might argue that this asymmetry could have been foreseen. But there are further levels of complication to come.

Let us look at the punch-card machines in more detail. The situation appears fairly simple to categorize. Surely it is simply a matter of seeing which name has been punched, is it not? Well actually… no! It turned out *ex post* that these machines were defective. They punched badly, with insufficient force, so that even after use, the chad that should have been detached from the ballot remained attached to it. It was therefore possible, for example, for an individual voting for Gore to produce a ballot paper on which the chad corresponding to Gore had not been totally detached. With a view to respecting voters' intentions as faithfully as possible, it was therefore necessary to consider the attachment of the chad to the ballot paper in greater detail. It then became apparent that more than two possible states of the world (detached or not) would have to be taken into account, contrary to what any *ex ante* analysis would have affirmed. For example, it was necessary to consider partially detached chads that remained attached by one, two or three corners. And then what were called "pregnant chads" also had to be taken into consideration. In this case, the chads had been punched, but they remained attached by all four corners. The voters' intentions were manifested solely by the fact that these chads had a distinct bulge, hence their name. The result is a classification that is already relatively complex, distinguishing between six possible cases:

1. Completely detached chad;
2. Hanging chad, attached to the ballot at only one corner;

3. Swinging chad, attached to the ballot at two corners;
4. Tri-chad, attached to the ballot at three corners;
5. Pregnant or dimpled chad, attached to the ballot at all four corners, but bearing an indentation indicating the voter may have intended to mark the ballot (Sometimes *pregnant* is used to indicate a greater mark than *dimpled*.);
6. No mark seen.

Clearly, this is just the start. New parameters can be included, because the reality is infinite. In the Annex, there is an example of the encoding really used by some of the groups that were responsible for the manual recounts. It can be seen that the reality is far stranger than the fiction. Let us set aside the fact that the classification distinguishes between the three types of voting machines used (*Datavote Ballots*, *Optical Scan Ballots*, *Votomatic Ballots*), which considerably increases the complexity. More incredible is the fact that, for the *Votomatic* machines that operated with punch cards, the typology comprises no less than 9 items, even more complicated than that described above, which already contained 6. The reason for this is that the category "dimpled chad" is divided, astonishingly, into four sub-categories! It is hardly believable. Already, the idea of considering the existence of "dimpled" or "pregnant" chads seemed to be almost unimaginable, verging on the absurd. So what can be said about the further distinction between four types of dimpled chads, depending on whether or not sunlight can be seen[22] and whether the dimple is "off chad, within borders" or "off chad, on border above"? This sophistication is closely linked to the importance of what was at stake and to the intensity of the contest between Bush and Gore. That is what led to the escalation of detail. As a result, the total typology contained 19 states of the world, far from the two possible states that the *ex ante* analysis had produced. Clearly, such a level of complexity could not have been predicted. The classification used was seen as a necessity *ex post*, but could not be predicted *ex ante*, because it was the endogenous result of the contest between the candidates, leading them suddenly to attach importance to details that were *a priori* of no interest. Likewise, on the field of battle, it is not possible to determine in advance which geographical feature will turn out to be crucial. It is only when the war

[22] Which supposes that those in charge of the recounts had light-boxes at their disposal.

comes that the enemies, from necessity, invest in one or another detail taken from the infinity of the physical world and make it a central element of their strategy. In other words, that detail did not pre-exist the conflict, but is the result of it. One cannot discover it *ex ante*. One can only observe it *ex post*. This totally refutes the hypothesis of an exhaustive *a priori* description of states that are "economically relevant to agents", as described by Kast and Lapied (1992, p. 23).

Conclusion

This essay has endeavoured to show the intrinsic limitations of financial estimation, and more especially of the estimation of risk. These limitations derive from the nature of economic time, which is opaque and radically uncertain. This has one fundamental consequence: there is no such thing as an *objective* evaluation of financial assets. Observation of the past does not provide a sufficient basis for predicting the future, either in the economic domain or in any other human activity. In the example of housing prices, the sophistication of tools and analyses did not allow agents to conceive the possibility of a sudden downturn in the market. This was a massive mistake. In this situation, the opaqueness of the future played its role to the full. Statistical inference proved to be totally incapable of informing investors. Nevertheless, it should not be thought that this fact alone can explain the euphoria. On the contrary, establishing that there is no objective basis allowing for a scientific estimation of risk is only the first step in an approach which, by showing the limits of objective analysis, highlights the role played by strategic interactions.[23] It is these interactions that must be analysed to explain the underestimation of risk. The present text, however, has been limited to proving that there is no such thing as an objective analysis of risk credible enough to constrain the interests of agents. This has an important consequence: one can never rationally convince investors that they are involved in a bubble. Even in the case of the Tulip-mania that swept 17th century Holland, economists are still debating whether or not it was a bubble!

23 For a complete analysis of the euphoria, see Orléan (2009).

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ANNEX: Coding Process[24]

A group of major US newspapers, including *The New York Times*, the *Wall Street Journal* and the *Washington Post*, commissioned an independent research institute – the National Opinion Research Center (NORC) – at the University of Chicago, to carry out an in-depth study of the voting ballots that were rejected in Florida during the 2000 presidential election. NORC thus devised a precise coding system, making it possible to classify all the situations observed. This very tedious analysis concerned 180,000 rejected Florida ballots.

NORC coders were provided with special coding forms for the three major types of ballot used in Florida: Votomatic, Datavote, and optical scan technologies. Each Florida county was represented by a FIPS code (Federal Information Processing Standards). In addition, each type of ballot technology was assigned its own set of codes. When working in counties that use Votomatic technology, coders were provided with light-boxes to help them decide whether "sunlight" was visible through a dimple or not.

Datavote Ballots

These are cards on which voters make selections by pressing a lever that punches out the chads mechanically. These punchcards have the candidate names and party information on them. The chads on these cards are not numbered. The codes for absentee datavote ballots are the same as the codes for votomatic ballots (see above). For non-absentee ballots, the codes are as follows:

Label	**Code**
No mark seen	0
Punch between the lines	4

(For detail see Datavote Coding Form and Datavote Coding Form Codes.)

[24] See: https://www2.norc.org/fl/method3.asp

Optical Scan Ballots

There are two types of optical scan ballots. The first contains an oval for each candidate. Voters fill in the oval that corresponds to their vote choice. The second scan ballot features a set of broken arrows. The voter completes the centre section of the arrow that points to their candidate's name. On both types of optical scan ballot, a machine reads the filled selections electronically. Codes for optical scan ballots are as follows:

Label	**Code**
No mark seen	00
Circled party name	11
Other mark on or near party name	12
Circled candidate name	21
Other mark on or near candidate name	22
Arrow/oval marked other than fill (circled, X, /, checked, scribbled)	31
Other mark near arrow/oval	32
Arrow/oval filled	44
Arrow/oval filled or marked other than fill, then erased or partially erased	88
Negated mark (scribble-through, cross-out, "no," and similar)	99

(For detail, see Optical Scan Coding Form and Optical Scan Coding Form Codes.)

Votomatic Ballots

These are cards on which the chad for each vote selection is punched out manually. These punchcard ballots have no candidate names or identification on them. Chads have only numbers. The codes for a votomatic ballot are listed below, ordered from the least detached chad state to the most detached chad state:

Label	Code
No mark seen	0
Dimpled chad (no sunlight seen)	5
Dimpled chad (sunlight)	6
Dimple (with or without sunlight), off chad, within borders	7
Dimple (with or without sunlight), off chad, on border above	8
1 detached corner	1
2 detached corners	2
3 detached corners	3
4 detached corners	4

(For detail on the coding forms, see Votomatic Coding Form.)