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# Inductive risk in macroeconomics: Natural Rate Theory, monetary policy, and the Great Canadian Slump

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## Abstract

This paper has two main goals. The first is to fill a gap in the literature on inductive risk by exploring the relevance of the notion of inductive risk to macroeconomics. The second is to draw some general lessons about inductive risk from the case discussed here. The most important of these lessons is that the notion of inductive risk is no less relevant to the relationship between the proximate and distal goals of policy than it is to the relationship between specific policies and their proximate goals.

**Keywords:** inductive risk; macroeconomics; monetary policy; Natural Rate Theory

## 1. Introduction

In the last couple of decades, philosophers have been devoting increasing attention to the relationship between science and public policy.<sup>1</sup> As part of this general trend, the field has witnessed a renewed interest in the notion of inductive risk, which, narrowly construed, refers to the non-epistemic risks associated with accepting false scientific hypotheses or rejecting true ones. So far, most of the literature on inductive risk has focused on the natural sciences and, in particular, on the environmental and biomedical sciences.<sup>2</sup> However, as of yet, little or no attention has been devoted to inductive risk in economics. Given the prominent and pervasive role played by economics in shaping public policy, this situation is somewhat surprising. This paper has two main goals. Its less ambitious goal is to fill this obvious gap in the literature by discussing a case of inductive risk in

<sup>1</sup>See e.g. Kitcher (2001, 2011), Douglas (2009), Mitchell (2009) and Shrader-Frechette (2014).

<sup>2</sup>For example, none of the 13 papers included in a recent (and otherwise excellent) collection on inductive risk (Elliott and Richards 2017) was on inductive risk in economics and only one of them was on inductive risk in the social sciences. This is especially surprising considering that the collection included a section entitled 'The Breadth of Inductive Risk' and that it even included a paper about inductive risk in theoretical physics.

macroeconomics. Its more ambitious goal is to draw some general lessons about inductive risk from the case discussed.

Section 2 introduces the notion of inductive risk. Section 3 briefly outlines the standard story often told by economists about the development of the macroeconomic understanding of the relation between inflation and unemployment in the second half of the 20th century. Section 4 discusses a different take on that same story, one which, among other things, highlights the relevance of inductive risk. Section 5 questions some of the central aspects of that version of the story and Section 6 presents a short case study. Section 7 draws some general lessons about inductive risk and public policy. The most important of these lessons, in my opinion, is that the notion of inductive risk is no less relevant to the relationship between the proximate goals and the ultimate goals of policy than it is to the relationship between specific policies and their goals, a phenomenon that I dub ‘the fog of policy’.

## 2. Inductive risk

In order to introduce the notion of inductive risk, it is helpful to consider a specific case. Suppose that policymakers are considering a ban on widely used glyphosate-based herbicides on the grounds that glyphosate might cause cancer in humans and suppose that they seek advice from the relevant experts. According to supporters of the Value-Free Ideal, in cases such as this, there is a clear division of policymaking labour between the experts and the policymakers – the experts are responsible for providing the policymakers with the relevant facts and should leave any (non-epistemic) value judgements to the policymakers.

Critics of the Value-Free Ideal, however, point out that this picture of the division of policymaking labour presupposes an unrealistic picture of science. According to proponents of the *Argument from Inductive Risk*, since no isolated scientific hypothesis can ever be conclusively verified or falsified, the decision to accept or reject a hypothesis carries a risk, as accepting a false hypothesis or rejecting a true one can have very serious moral, social or political consequences.<sup>3</sup> For example, while there is growing evidence that glyphosate is a human carcinogen, the evidence is not conclusive either way. The World Health Organization’s International Agency for Cancer Research, for instance, classifies glyphosate as ‘probably carcinogenic to humans’ (IARC 2007: 112). The uncertainty expressed by that ‘probably’ means that both courses of action carry some risk. If, on the one hand, glyphosate is a human carcinogen, then its continued use exposes the population to an increased risk of cancer, with all of the associated human and social costs. If, on the other hand, glyphosate is not a human carcinogen, then a ban on its use might have significant economic and social costs (especially in poorer countries), as herbicides contribute to higher crop yields and lower food prices. According to the proponents of the *Argument from Inductive Risk*, when

<sup>3</sup>The original version of this argument is due to Richard Rudner (1953). The current revival of Rudner’s argument has been primarily sparked by Heather Douglas (see, in particular, Douglas 2000, 2009). Douglas expands on Rudner’s original argument and emphasizes its relevance to contemporary democracies in which scientific experts and advisors play an increasingly crucial role.

researchers and advisors are faced with the choice of whether to accept or reject the hypothesis that glyphosate is a human carcinogen, they have a duty to take those possible non-epistemic consequences into account, which means that, contrary to what the supporters of the Value-Free Ideal maintain, both epistemic and non-epistemic values are relevant to their decision.

Supporters of the Value-Free Ideal often reply that the Argument from Inductive Risk does not undermine their view of the division of policymaking labour – it only highlights the importance of the experts' communicating the relevant uncertainties to policymakers as clearly as possible. However, it is still up to the policymakers to make the final decision about the relative importance of the risks associated with the various options.<sup>4</sup>

Proponents of the Argument from Inductive Risk, however, argue that this reply underestimates the extent of the problem. First, the sorts of uncertainties involved are not as easily quantifiable as this reply suggests, as there is no uncontroversial approach to objectively quantifying the probability that a certain hypothesis is false or communicate it clearly to policymakers and, therefore, even attaching probabilities or confidence intervals to an hypothesis requires making some value judgements (see Steele (2012) for a discussion of this and similar issues). Second, inductive risk cannot be neatly circumscribed to the final stage of the research process (when scientists decide whether to accept or reject a certain hypothesis on the basis of the evidence available to them) – it permeates all of the stages of that process. Suppose, for example, that the scientists who study the carcinogenicity of glyphosate in lab rats can choose between a more specific but less sensitive test and a more sensitive but less specific test to detect cancer. This choice requires weighing, among other things, the non-epistemic consequences of a higher rate of false positives against those of a higher rate of false negatives, which involves making value judgements about what sort of error is more tolerable given what is ultimately at stake (see Douglas (2000) for an in-depth discussion of this issue).

In light of these arguments, it has become increasingly difficult to deny that, at least, in practice, it is impossible for scientists not to make any value judgements in the course of the research and advice process and leave all value judgements to policymakers, as the Value-Free Ideal maintains.

### 3. The macroeconomics of unemployment and inflation: a first look

In this section, I briefly recount a crucial episode in the history of 20th century macroeconomics – the demise of the original Phillips Curve and the rise of Natural Rate Theory. While the version of the story I tell in this section is not entirely accurate, it is the story that is often told in economics textbooks and lectures. The relevance of inductive risk to this story will become clearer in the next section, where I discuss a different (and more accurate) take on the standard story told in this section.

<sup>4</sup>A similar reply was already offered by Richard Jeffrey in response to Rudner's original version of the Argument from Inductive Risk (see Jeffrey 1956). For a contemporary defence of this sort of reply, see Betz (2013).

In 1958, economist A.W. Phillips published a paper in which he carried out an econometric analysis of almost a century's worth of British data that seemed to show the existence of a negative relationship between inflation and unemployment, a relation that was encapsulated in graphic form by what later would become known as 'the Phillips Curve' (Phillips 1958). The idea was popularized in the USA by two prominent economists, Paul Samuelson and Robert Solow, who two years later published a paper that identified a Phillips Curve in data from the US economy (Samuelson and Solow 1960). As empirical data accumulated, economists came to believe in the existence of a trade-off between inflation and unemployment. One of the key policy implications of this body of work was that policymakers essentially faced a dilemma – they could try to lower either inflation or unemployment, but not both.

According to the standard story, this picture started falling apart in the late 1960s, when economists Edmund Phelps and Milton Friedman independently argued that the notion of a stable trade-off between inflation and unemployment stood on shaky theoretical grounds. In particular, Phelps and Friedman argued that the original Phillips Curve failed to take into account the inflationary expectations of economic agents. In his celebrated 1967 Presidential Address before the American Economic Association, Friedman offered roughly the following argument (Friedman 1968: 9–11). Suppose that the current rate of unemployment is its natural rate – i.e. the rate of unemployment that correspond to the equilibrium in the markets for labour and for goods under the prevailing structural market conditions – and that the rate of inflation is currently zero. Now, suppose that the monetary authority decides to engage in expansionary monetary policy in an attempt to bring the rate of unemployment below its 'natural' rate. The expansionary monetary policy will initially stimulate spending and, since prices tend to respond faster than wages to external shocks, at first, the prices of goods will increase, but nominal wages won't, which means that, even if the employees are still earning the same nominal wages, their wages will have decreased in real terms. In response to the increased demand for goods and to the lower real wages, employers will hire more labour, which, initially, will bring unemployment down. In the short run, the situation is, thus, consistent with what we would expect according to the Phillips Curve. However, in the longer run, two effects come into play. The first is that, eventually, employees will come to realize that their wages have decreased in real terms and will demand increases in their nominal wages. The second is that the increased demand for labour and the lower unemployment will put an upward pressure on real wages, which will tend to push both real wages and unemployment up towards their original equilibrium level. Therefore, in the long term, expansionary monetary policy will not result in lower unemployment, as, eventually, unemployment will be back to its natural rate, but both in higher inflation and in a change in the inflationary expectations of employers and employees, who will now come to expect inflation and factor it in when setting both wages and prices.

Friedman and Phelps concluded that the natural rate of unemployment is the only rate that will not lead to ever accelerating inflation (or deflation). In order to keep unemployment constantly below its natural rate, the monetary

authorities would have to engage in a permanently expansionary monetary policy, which means that inflation would turn out to be always higher than both employers and employees expected and which would lead both employers and employees to revise their inflationary expectations constantly upwards giving rise to an inflationary spiral. In an uncharacteristically dramatic tone, Phelps warned that 'perpetual maintenance of the unemployment ratio below that level (perpetual over-employment) would spell eventual hyper-inflation and ultimately barter' (Phelps 1967: 256).

Both Friedman and Phelps took their arguments to undermine the understanding of the relationship between inflation and unemployment encapsulated in the Phillips Curve. As Phelps put it in his 1967 paper:

The policy trade-off is not a timeless one between permanently high unemployment and permanently high inflation but a dynamic one: a more inflationary policy permits a transitory increase of the employment level in the present at the expense of a (permanently) higher inflation and higher interest rates in the future steady state. (Phelps 1967: 256)

Friedman also drew a more general conclusion, which is often referred to as *the neutrality of money*:

the monetary authority controls nominal quantities directly, the quantity of its own liabilities. In principle, it can use this control to peg a nominal quantity – an exchange rate, the price level, the nominal level of national income, the quantity of money by one or another definition – or to peg the rate of change in a nominal quantity – the rate of inflation or deflation, the rate of growth or decline in nominal national income, the rate of growth of the quantity of money. It cannot use its control over nominal quantities to peg a real quantity – the real rate of interest, the rate of unemployment, the level of real national income, the real quantity of money, the rate of growth of real national income, or the rate of growth of the real quantity of money. (Friedman 1968: 11)

According to the standard story, Phelps' and Friedman's theoretical insight was soon to be vindicated by the economic events of the 1970s, when many countries around the world experienced stagflation (i.e. a combination of high inflation and high unemployment), a phenomenon that seemed to be inconsistent with the original Phillips Curve.

As Olivier Blanchard and David Johnson put it in their popular undergraduate macroeconomics textbook:

Economists are usually not very good at predicting major changes before they happen, and most of their insights are derived after the fact. Here is an exception. In the late 1960s, while the original Phillips curve still gave a good description of the data, two economists, Milton Friedman and Edmund Phelps questioned the existence of such a trade-off between unemployment and inflation. They questioned it on logical grounds,

arguing that such a trade-off could exist only if wage setters systematically underpredicted inflation. And that they were unlikely to make the same mistake forever. Friedman and Phelps also argued that if the government attempted to sustain lower unemployment by accepting higher inflation, the trade-off would ultimately disappear . . . Events proved them right, and the trade-off between the unemployment rate and the inflation rate indeed disappeared. (Blanchard and Johnson 2013: 170)

Blanchard and Johnson's assessment is fairly representative in two respects. First, it characterizes Friedman's and Phelps' contribution as, primarily, a theoretical breakthrough. According to the standard story, before Friedman and Phelps made them see the error in their ways, economists had simply failed to take inflationary expectations into account. Second, it suggests that the economic events of the 1970s had vindicated Friedman's and Phelps' arguments by showing that high inflation and high unemployment could co-exist. This supposedly provided empirical confirmation of Phelps' and Friedman's insight and was the last nail in the coffin of the original Phillips Curve.

#### 4. The macroeconomics of unemployment and inflation: a second look

It is often said that history is written by the victors and this case does not seem to be an exception. While the story I told in the previous section is still popular among economists, its accuracy has often been challenged.<sup>5</sup> One of its crucial aspects that seems to be inaccurate is that Phelps' and Friedman's contribution is portrayed as a theoretical breakthrough that corrected a glaring mistake made by their opponents. Friedman himself seems to have encouraged this interpretation. In his 1976 Nobel Prize Lecture, Friedman went as far as suggesting that the work of his opponents was muddled by money illusion (i.e. the tendency to fail to distinguish between nominal and real economic quantities). In the conclusion of the lecture, Friedman stated:

One consequence of the Keynesian revolution of the 1930s was the acceptance of a rigid absolute wage level and a nearly rigid absolute price level, as a starting point for analyzing short-term economic change. It came to be taken for granted that these were essentially institutional data and were so regarded by economic agents, so that changes in aggregate nominal demand would be reflected almost entirely in output and hardly at all in prices. The age-old confusion between absolute and relative prices gained a new lease on life. In this intellectual atmosphere it was understandable that economists would analyze the relation between unemployment and nominal rather than real wages and would implicitly regard changes in anticipated nominal wages as equal to changes in anticipated real wages. (Friedman 1977: 468–469)

<sup>5</sup>The most forceful, comprehensive and detailed case against the standard story that I told in the previous section is arguably the one made by economist and historian of economics James Forder (see in particular Forder 2014). In this paper, I only focus on the inaccuracies that are most relevant to our topic.

This passage implies that money illusion affected not only the agents in the models of Keynesian economists but the economists themselves, a suggestion that is as bold as it is implausible. It seems at best uncharitable to suggest that economists of the calibre of Phillips, Samuelson, or Solow had failed to realize the importance of the distinction between real and nominal quantities. After all, the notion of money illusion, which had been popularized by Irving Fisher in the 1920s, was used by Keynes and his followers to explain recalcitrant phenomena, such as the downward stickiness of wages.

As for the suggestion that the work of Phillips and his followers ignored inflationary expectations, it is belied by the fact that, in his 1958 paper, Phillips himself explicitly mentioned cost-of-living adjustments as one of the factors affecting wages. In fact, Phillips alluded to money illusion as a possible reason why real wages might not increase as much as real prices (Phillips 1958: 283–284). Phillips argued that, while employees will ask for cost-of-living adjustments, employers are likely to take advantage of their employees' money illusion to give nominal wage increases that were only partially sufficient to cover for increases in real prices. What matters here is not so much the soundness of Phillips' argument as much as the fact that, contrary to what the standard story implies, the disagreement between the two camps turns out to be not so much a theoretical disagreement as much as an empirical disagreement about the extent to which real-world employers and employees are susceptible to money illusion.

This alternative interpretation of the standard story has been defended most clearly and forcefully by George Akerlof and Robert Shiller (see in particular Akerlof and Shiller 2010: Ch. 9; but see also Akerlof *et al.* 1996). According to Akerlof and Shiller, the Phelps–Friedman argument presupposes that ordinary people are completely immune from money illusion. However, there is a wealth of evidence (ranging from the anecdotal to the empirical evidence from both laboratory and field studies) that people often fall prey to money illusion (see e.g. Shafir *et al.* 1997). Akerlof and Shiller argue:

In the absence of any money illusion at all, natural rate theory will hold. But, just as we think it was naïve to believe that wage bargains and price setting take no account of inflationary expectations, we also think it is naïve to presume that there is no money illusion at all. It seems unlikely to us that there is not some money illusion, somewhere in the economy. And if there is, will this money illusion not induce some long-run trade-off between inflation and unemployment, even if it is a diminished one? (Akerlof and Shiller 2010: 108–109)

While, according to Akerlof and Shiller, the disagreement between natural rate theorists and their opponents is primarily an empirical disagreement concerning the extent to which ordinary people suffer from a money illusion (as opposed to a theoretical one), they seem to suggest that there is also a second important dimension of disagreement, which concerns the potential non-epistemic consequences of adopting Natural Rate Theory as a guide to economic policy. They write:

If correct, natural rate theory has major consequences for monetary policy. If it is correct, there is little loss from very low inflation targets. Long-term price

stability, with an inflation target of zero, can be achieved with no permanent ill consequences. On the average, over a long period of time unemployment will be unaffected by the choice of inflation target. If, on the other hand, natural rate theory is not true, so that there is a long-term trade-off between inflation and unemployment, a zero inflation target is poor economic policy. The calculated increase in the unemployment rate of 1.5% would make a significant difference. In human terms, for the United States such an increase would render jobless 2.3 million people – more than all the men, women, and children within the city boundaries of Boston, Detroit, and San Francisco combined. It would entail a loss of GDP of more than \$400 billion per year. (Akerlof and Shiller 2010: 110–111)

The concern expressed by Akerlof and Shiller in this passage should sound familiar, as it is clearly a concern about what philosophers would call inductive risk – it is a concern about the potential social and economic consequences of accepting Natural Rate Theory for policymaking purposes if that theory happens to be false. Moreover, Akerlof and Shiller's concern seems to have been shared by Samuelson. Akerlof recalls that, in a graduate seminar that Akerlof attended while he was a graduate student at MIT in 1964, Samuelson had offered similar reasons to be wary of what later would become known as Natural Rate Theory (Akerlof and Shiller 2010: 113). If Akerlof's recollection is accurate, the episode is remarkable for two reasons. First, it provides further evidence that the Friedman-Phelps argument was not as novel as the standard story suggests. Apparently, Samuelson was already aware of a similar argument a few years before either Friedman or Phelps had presented it publicly.<sup>6</sup> Second (and more importantly for our present purposes), the passage implies that the axiological disagreement between the supporters of Natural Rate Theory and their opponents (which focuses on the potential non-epistemic consequences of accepting Natural Rate Theory for policymaking purposes) might be almost as significant as the empirical disagreement (which, primarily, focuses on the prevalence of the money illusion among real-world economic agents).

Akerlof and Shiller conclude with some words of caution:

We should admit that it is hard to estimate the way Phillips curves depend on inflationary expectations. But economic policy must be made in an environment in which there are doubts. Our problem with natural rate theory is not with the theory itself. We think that it does offer a correct insight: that wage and price setting will both be affected by inflationary expectations. But we are highly skeptical that these inflationary expectations always, exactly, and invariably affect wage setting and price setting one for one. (Akerlof and Shiller 2010: 113)

Irrespectively of one's views about the merits of Natural Rate Theory or its role in monetary policy, it is hard to disagree with Akerlof and Shiller's call for caution. In

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<sup>6</sup>According to Akerlof, Samuelson attributed the argument to Raymond J. Sauliner, an economist who had served as Chairman of the President Eisenhower's Council of Economic Adviser.



fact, given the inherent uncertainty of much of our economic knowledge of the world as well as its influence on public policy, their advice seems highly generalizable, or so I argue in §7.1 below.

However, while Akerlof and Shiller's retelling of the standard story is more accurate than the original version and can teach us an important lesson about the relevance of inductive risk to economics, their version of the story is not entirely accurate either, or so I argue in the next section.

## 5. The macroeconomics of unemployment and inflation: a third look

Akerlof and Shiller's version of the standard story seems to suggest that, while the opponents of Natural Rate Theory were sensitive to the inductive risks associated with using Natural Rate Theory as a basis of economic policy, its supporters were not. However, this suggestion does not seem to be correct. Friedman, for one, warned against the use of Natural Rate Theory as a basis for monetary policy already in his 1967 Presidential Address (Friedman 1968: 10–11). In fact, one of Friedman's reasons against basing economic policy on Natural Rate Theory seems to appeal implicitly to considerations of inductive risk. Friedman stated:

What if the monetary authority chose the “natural” rate – either of interest or unemployment – as its target? One problem is that it cannot know what the “natural” rate is. Unfortunately, we have as yet devised no method to estimate accurately and readily the natural rate of either interest or unemployment. And the “natural” rate will itself change from time to time. (Friedman 1968: 10)

By emphasizing the uncertainty of all estimates of the natural rate of unemployment, Friedman is implicitly drawing attention to the negative consequences of targeting the wrong rate. If targeting the wrong rate of unemployment did not have any negative consequences, the inaccuracy of its estimates would not be a reason not to try to target it. After all, all economic policy decisions are based on somewhat inaccurate estimates. So, while Friedman's concerns are not expressed explicitly as those of Samuelson or of Akerlof and Shiller and the uncertainty that concerned Friedman was the uncertainty of estimates of the natural rate (as opposed to the truth of Natural Rate Theory *per se*), Friedman's argument seems to be implicitly an argument about inductive risk, too – it seems about the non-epistemic consequences of accepting an inaccurate hypothesis about the natural rate of unemployment.

Moreover, contrary to Akerlof and Shiller's suggestion, Friedman and Phelps seemed to take the policy implications of Natural Rate Theory to be primarily negative – i.e. they were about the limited effectiveness of monetary policy. As we have seen in §3, the first policy implication was that policymakers should not overestimate the effectiveness of monetary policy, which cannot have long-term effects on real quantities, such as the rate of unemployment. The second was that any effort by policymakers to lower the unemployment rate by accepting higher inflation is not only futile but harmful, as the effect of monetary policy on the rate of unemployment is, at best, temporary but its effects on inflation are long-lasting (see e.g. Phelps 1995: 15).

Neither Friedman nor Phelps, however, seemed to draw the stronger positive policy implications suggested by Akerlof and Shiller. In particular, in the passage that I quoted above, Akerlof and Shiller state: ‘if the monetary and fiscal authorities acted on [Natural Rate Theory], and it was not true, we would have permanently higher unemployment’ (Akerlof and Shiller 2010: 113). While it is not entirely obvious what they have in mind when they mention fiscal authorities, it should be clear that, since fiscal policy can affect real variables, and not only nominal ones, it has the power to change the ‘natural’ rate of unemployment according to Natural Rate Theory. Thus, insofar as Natural Rate Theory has any positive policy implications, they are very generic – it suggests that, in order to lower unemployment, policymakers have to make the sort of structural changes that affect the ‘natural’ rate.

Finally, even according to Natural Rate Theory, monetary policy does have short-term effects and, since most policymakers are mindful of the short-term consequences of their policies, Natural Rate Theory does not seem to support the conclusion that policymakers should not be concerned about the disemployment consequences of their policies, especially given that what counts as ‘short-term’ for economists does not necessarily count as ‘short-term’ for policymakers.<sup>7</sup>

However, while neither Friedman nor Phelps seemed to draw strong policy implications from Natural Rate Theory, one might suspect that policymakers read more into Natural Rate Theory than its proponents. Since such a general claim is difficult to assess, in the next section, I focus on the specific case that Akerlof and Shiller use to illustrate their more general point.

## 6. The Great Canadian Slump: a closer look

In this section, I focus on the example that Akerlof and Shiller use to illustrate their general point – i.e. the case of the Great Canadian Slump (Akerlof and Shiller 2010: 114). While some countries experienced a mild recession in the early 1990s, Canada entered its longest and deepest recession since the Great Depression. Unemployment peaked at above 12% in November 1992 and the country accumulated 15.7 point-years of excess unemployment in the first half of the decade (Fortin 1996). Akerlof and Shiller’s analysis of this case relies heavily on the work of Canadian economist Pierre Fortin, who, in his 1996 Presidential Address to the Canadian Economic Association, argued that the recession was primarily a product of the monetary policy pursued by the Bank of Canada under its then Governor, John Crow (Fortin 1996). Akerlof and Shiller argue that the Great Canadian Slump should serve as a cautionary tale about the dangers posed by the excessive reliance of central bankers on Natural Rate Theory.

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<sup>7</sup>Moreover, these days even supporters of Natural Rate Theory no longer believe that the effects of monetary policy are as short-lived as Natural Rate Theory would seem to suggest. One of the phenomena Natural Rate Theory struggles to explain is what economists call *hysteresis*, which occurs when the rate of unemployment remains high even after the effects of a negative shock have subsided.

For the sake of simplicity, here, I assume that Fortin's analysis of the causes of the Great Canadian Slump is largely correct.<sup>8</sup> However, while Fortin does lay the blame for the recession primarily at the feet of the Bank of Canada, it is not clear to what extent those policies can be blamed on Crow's excessive faith in Natural Rate Theory, as Akerlof and Shiller suggest.<sup>9</sup> The main piece of evidence in support of Akerlof and Shiller's interpretation seems to come from a technical report that provided the background for the Bank of Canada's anti-inflationary policy. The report stated that 'the benefits of price stability – or, conversely, the costs of inflation – are many and large whereas the costs of attaining and maintaining price stability are transitory and small by comparison' (Selody 1990: 10).<sup>10</sup> This and similar passages seem to suggest that, while Natural Rate Theory played a role in the considerations offered by the report, it seems to have played at most a supporting role – the leading role is played by the 'many and large' benefits of price stability, which, somewhat ironically, include its alleged capacity to 'reduce the amplitude of economic cycles' (Selody 1990: 10).

This interpretation seems to be confirmed by Crow's official statements. Shortly after his appointment as Governor of the Bank of Canada, Crow delivered the 1988 Hanson Memorial Lecture at the University of Alberta in Edmonton, which, due to its programmatic tone, has come to be known as 'the Edmonton Manifesto'. In that lecture, Crow stated:

To say that the goal of monetary policy should be price stability is not simply an arbitrary preference. Rather it is a recognition of the plain fact that because inflation creates distortions, output will be higher over time in conditions of price stability than in those of inflation. The argument for avoiding inflation therefore goes beyond the conclusion from the debate of the 1960s and 1970s, that there is no long-run employment advantage or trade-off to be had from tolerating some degree of inflation. Nor do the short-run employment gains associated with a pick-up in inflation provide a convincing argument for the pursuit of inflationary policies. Experience has shown that such policies cannot in the end deliver a healthy economy. The concern of monetary policy should be a healthy economy. (Crow 1988: 4–5)

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<sup>8</sup>Fortin's analysis is not entirely uncontroversial. For an alternative analysis, see e.g. Freedman and Macklem (1998), and for a reply, see Fortin (1999). While it is often difficult to attribute causal responsibility for large and complex economic events, it seems hard to deny that the contractionary monetary policy pursued by the Bank of Canada played a causal role in the Great Canadian Slump. However, since a detailed discussion of this case is beyond the scope of this paper, in this paper I assume that Fortin's analysis is, by and large, correct.

<sup>9</sup>For the sake of simplicity, I follow Akerlof and Shiller in focusing on Crow's decisions and beliefs. Given that, at the time, the Governor of the Bank of Canada was still ultimately responsible for making monetary policy decisions, this is not entirely inappropriate. However, I should note that, even in that sort of institutional context, decisions are never taken by isolated individuals and that the opinions of key members of the staff of the Bank of Canada (as well as those of the federal government) are likely to have played an important role in Crow's decisions.

<sup>10</sup>Portions of this passage from the report are quoted directly in Fortin (1996: 775).

This passage seems to confirm that, while Crow accepted the conclusion of the Friedman-Phelps' argument, his primary concern was about the negative effects of inflation on the economy and, in particular, on economic growth.

Reflecting on his experience as Governor a quarter of a century later, Crow offered some interesting remarks about the goal of price stability:

Everyone at this conference probably knows, and central bankers certainly do, that it is much easier to talk about price stability than to define it. And at no point did the [Bank of Canada] volunteer a numerical price stability target – although early on I did, in response to a media question, indicate that as regards a desirable rate of inflation, “three is better than four, two better than three, one better than two, and zero better than any of them.” In any case, for the earlier part of my term inflation was, notwithstanding anything the bank said or did, moving up as a result of general demand pressures – not a single inflationary supply shock in sight. So the bank could hardly be faulted that severely for raising interest rates, and then keeping them up. However, what was made clear even then was that as far as the bank was concerned, ‘price stability’ would be distinctly less than 4 percent inflation (where we had started) and that zero inflation was not being ruled out. It also became clear that the bank insisted on being judged on how it did regarding inflation and regarding progress toward price stability. (Crow 2013: 45)

Crow's casual remarks about the ‘desirable rate of inflation’ seem to presuppose that (i) the lower the inflation, the better and that (ii) the only consideration in choosing an inflation target is price stability. And yet Crow did not offer any argument for either of these controversial premises. One can have many reasons to think that price stability is good for the economy, but, in Crow's case, it is not entirely clear what these reasons are, especially given that, for example, most economists and central bankers today would be weary of a zero-inflation target (see e.g. Bernanke *et al.* 1999: 28–30).

It is even less clear what sorts of reasons one might have to think that price stability is of paramount importance, as Crow's remarks suggest. In fact, it is hard to believe that absolute price stability is so important that all other considerations have to be sacrificed in its pursuit. As British economist Roger Bootle once put it in a now-classic paper reviewing the negative effects of inflation,

Being against inflation is rather like being against sin – everyone agrees with you, but when you go on to define what you are talking about, and to propose action to deal with it, very real disagreement begins. What is novel and controversial about the economic policy of the current UK Government is not its opposition to inflation, but rather the extent to which it is prepared to subordinate other objectives to the aim of defeating inflation. (Bootle 1981: 42)

While Bootle was writing about the anti-inflationary policies of the Thatcher government in early 1980s Britain, he might as well have been writing about the

anti-inflationary policies pursued by the Bank of Canada in the early 1990s. One does not have to reject Natural Rate Theory to believe that the sort of aggressive contractionary monetary policy pursued by the Bank of Canada under Crow might succeed in lowering inflation at the cost of causing or exacerbating a recession, as, according to Fortin's analysis, it eventually did. If the main worry about inflation is its negative effects on economic growth or on the business cycle, then pursuing an anti-inflationary policy that results in (or contributes to) a recession seems to be self-defeating.

Moreover, while there is some empirical evidence supporting the hypothesis that inflation hampers economic growth (see e.g. Kormendi and Meguire 1985), it is still unclear whether there is a direct causal connection between the two phenomena, especially when it comes to moderate inflation of the sort Canada was experiencing at the time (in the 4–5% range). First of all, there is still no definitive theoretical model explaining the causal mechanisms that supposedly link inflation to decreased economic growth (see e.g. Gillman *et al.* 2004). Second, it is not clear whether the negative correlation between inflation and growth is causal or is the result of a common cause. In the absence of any clear theory of the causal mechanisms that allegedly connect higher inflation to decreased growth, it is possible that some third factor causes both higher inflation and lower economic growth and the sort of panel data that are usually used to establish the anti-correlation between growth and inflation cannot easily exclude such alternative explanations. Third, there is some evidence that the negative relation between inflation economic growth is substantial only above certain rates of inflation (see e.g. Khan and Senhadji 2001), which would not seem to justify Crow's zero-tolerance approach.

Moreover, while Crow's reasons for taking price stability to be of paramount importance are not entirely clear, his last remark in the passage above suggests that his stance on inflation was partly due to his belief that price stability should be the primary goal of the Bank of Canada and that the performance of the Bank (as well as his performance as a Governor) would be assessed on how effectively that goal was achieved. It is worth noting that, as Crow surely knew, this understanding of the goals of the Bank is much narrower than its official one as set out in the *Bank of Canada Act*, which states:

Whereas it is desirable to establish a central bank in Canada to regulate credit and currency in the best interests of the economic life of the nation, to control and protect the external value of the national monetary unit and to mitigate by its influence fluctuations in the general level of production, trade, prices and employment, so far as may be possible within the scope of monetary action, and generally to promote the economic and financial welfare of Canada.  
(Bank of Canada Act R.S.C., 1985, c, B-2)

However, while Crow's understanding of the goals of the Bank of Canada might diverge from its official goals, it was far from being idiosyncratic. Price stability has been increasingly understood as the main goal of monetary authorities (if not their only goal) and, while it is beyond the scope of this paper to trace the forces that shaped this understanding of the goals of central banks, the case of

the Great Canadian Slump clearly illustrates how an excessively narrow understanding of an institution's policy goals might be detrimental. I return to this point in §7.6 below.

To sum up, the evidence I reviewed in this section seems to support the conclusion that, while widespread acceptance of Natural Rate Theory might have played a role in shaping the Bank's anti-inflationary policies, that role seems to have been at most a supporting role. The leading roles seem to have been played by (i) the view that inflation harms the economy and (ii) the assumption that the primary goal of monetary authorities is price stability. However, even if this is correct, this does not mean that Akerlof and Shiller were wrong in emphasizing the inductive risks of accepting Natural Rate Theory, as the evidence indicates that the theory was part of the reason why the Bank underestimated the importance of disemployment side-effects of its contractionary monetary policy. Moreover, a different sort of inductive risk seems to have played a role. The inductive risk in question is the one of incorrectly accepting the hypotheses that inflation (even in its lower range) hampers economic growth or that it exacerbates the economic cycle. I believe that what is interesting about this case is that this latter sort of inductive risk is different from the ones that are usually discussed by philosophers – it concerns the relationship between proximate and ultimate goals of our policies instead of the one between those policies and their proximate goals, a point I discuss in more detail in §7.5 below.

## 7. Six lessons

I think we can draw a number of lessons from the case that I have discussed in this paper. In this final section, I focus on what I take to be the six most significant lessons.

### ***7.1. The notion of inductive risk as a conceptual tool for economists and policymakers***

The first lesson is that both economists and policymakers should be mindful of the relevance and pervasiveness of inductive risk in economics. Even the most ardent supporters of mainstream economics are likely to concede that, due to a variety of factors, our economic knowledge of the world is limited and that economic theories, models and hypotheses are shrouded in uncertainty. And yet, as critics of mainstream economics often point out, economists are not always very careful when drawing policy conclusions from their theories and models (see e.g. Angner 2006; Quiggin 2010). While the implicit appeal to considerations of inductive risk by two Nobel laureates might alert both economists and policymakers to the very real dangers associated with the inherent uncertainty of much of our economic knowledge of the world, it would be even better for both economists and policymakers to see Akerlof and Shiller's specific concern as an instance of a more general concern. An explicit concept of inductive risk would be a helpful addition to the conceptual toolkit of economists and policymakers, as it might help them explore and identify the inductive risks associated with using specific economic theories, models or hypotheses to guide policy in specific contexts.

In particular, an explicit notion of inductive risk might serve as a reminder for policymakers (and, especially, for policymakers who are not particularly familiar with economics and who are more likely to take expert advice from economists uncritically) of the uncertainties that are inherent to our economic knowledge of the world and of the potential ethical and social consequences of relying on false theories, unrealistic models or inaccurate estimates for policymaking purposes.

The general notion of inductive risk would also be helpful for economists, as it might help them explore and identify the potential risks associated with the uncertainties of their theories, models, etc. Economists should be mindful of the range of potential audiences their work might reach and try to make as clear as possible to the different audiences its limitations and the associated uncertainties. This is particularly true when they are acting as advisors to policymakers and even more so when the policymakers have little or no economics training.

It is also important for economists to consider the potential uses and abuses of their research. For example, since the early 1970s, economists have been exploring different approaches to estimating the natural rate of unemployment (see Crump *et al.* (2019) for a brief overview of the two main research programmes (and an interesting approach to unifying them)). Could Friedman's remarks about the limitations of estimates of the natural rate in his Presidential Address have had the seemingly unintended consequence of encouraging these research programmes? And could (allegedly) more accurate estimates of the natural rate of unemployment encourage policymakers to try to target the 'natural' rate? While I cannot answer these questions here, it is something that researchers should keep in mind when engaging in those research programmes. It is natural for economists (and especially academic economists) to think that their audience consists mostly of other economists and assume that the limitations of their work are understood.<sup>11</sup> However, as Keynes famously remarked,<sup>12</sup> the ideas of economists often have a far-reaching influence and economists should be mindful of that. This does not mean that economists should limit the range of topics they study, but it does mean that they should be mindful of the potential misuses of their work and that they have a responsibility to try to prevent them as far as possible. In this sense, economists are not that different from, say, virologists or nuclear scientists, who have a clear moral duty to take the risks of their own research into consideration. Just because economic research appears to be more removed from the practical consequences of the harmful policies it might inspire, it does not mean that economists should be complacent.

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<sup>11</sup>See John (2015) for an argument that the problem of inductive risk is primarily a problem about the communicative duties of experts and what this entails for what John calls 'public communication'. While I do not entirely agree with John, on this point, an extended discussion of these topic is beyond the scope of this paper.

<sup>12</sup>Keynes famously wrote: 'The ideas of economists and political philosophers, both when they are right and when they are wrong are more powerful than is commonly understood. Indeed, the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually slaves of some defunct economist' (Keynes 1936: 383). While this passage seems to overestimate the influence of the ideas of economists and political philosophers, it helpfully reminds us that we should not underestimate their reach, their power or their potential harmfulness.

The notion of inductive risk might also help make economists and policymakers more comfortable with abandoning the idea that economics can be pursued as a purely descriptive science, as maintained by the still-dominant tradition of 'positive' economics (see Friedman (1966) for an influential statement of the view). If, as proponents of the Argument from Inductive Risk claim, value judgements are to some extent inevitable in much policy-relevant science, then economists might feel more comfortable with acknowledging that there is no such thing as value-free economics. Instead of denying that they make any value judgements, economists should rather try to make their value judgements more explicit and transparent.

A similar lesson applies to technocrats, such as central bankers. Technocrats in general and central bankers in particular tend to portray their decisions as purely technical and devoid of any value judgement. However, as the case examined here suggests and as critics of the value-free conception of central banking have often argued, that technocratic decisions are entirely value-free appears to be a myth (see e.g. Dietsch *et al.* 2018). This myth is belied by the popular 'punch bowl' analogy popular among central bankers (see e.g. Conti-Brown 2017). If the role of the central bankers is analogous to that of the chaperone who removes the punch bowl when the party gets going, it is because central bankers are making a value judgement about the balance between the short-term interests of various actors (including politicians and consumers) and their long-term interests and they are in a position to make a better value judgement than those actors. While it might be convenient for technocrats to proceed as if their decisions do not require making any value judgements (or involve only uncontentious value judgements), it is rarely true that this is the case, as even apparently simple technical decisions such as that of targeting a certain rate of inflation involve a number of value judgements that are far from being uncontroversial, if only because of their distributional implications (see e.g. Hohberger *et al.* 2020).<sup>13</sup> In an era in which central banks strive to be more transparent and accountable, central bankers should become more comfortable with formulating explicitly their value judgements in order to increase the transparency of their policy decisions. As we have seen, the motivations and value judgements that guided the Bank of Canada's anti-inflationary policies under Crow were not entirely clear and transparent. It is possible that greater transparency about value judgements might lead to better value judgements or, at least, improved public understanding of monetary policy, better public discussion of it and greater accountability.<sup>14</sup>

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<sup>13</sup>In this respect, it seems that, on closer scrutiny, no economic policymakers ever have a completely 'simple' mandate to use Carlo Martini's terminology (Martini 2015) (although, to be fair, Martini would likely agree that his distinction between simple and complex mandates is a matter of degree).

<sup>14</sup>Some would argue that, despite the rhetoric of transparency, central bankers often try to disguise their influence on the economy in order to avoid responsibility for economic outcomes (see e.g. Krippner 2007). However, these contentions have lost some of their plausibility after the Great Recession, when many central banks have not hesitated to make open and large-scale use of unconventional tools such as quantitative easing, which were likely to attract (and did attract) a great deal of scrutiny and criticism.



### 7.2. The relevance of inductive risk to economics

The second lesson is about the relevance of the notion of inductive risk to the social sciences in general and economics in particular. So far, philosophers have mostly focused on inductive risk as it relates to the natural sciences, and, in particular, the environmental and biomedical sciences. However, given the crucial role played by economics in informing and shaping public policy and given the uncertainties inherent to our economic knowledge of the world, philosophers should pay more attention to inductive risk in economics. This is not merely a matter of completeness. Exploring inductive risk in economics is also likely to enrich our understanding of inductive risk in general. For example, as I argue below, I think we can draw some important general lessons about inductive risk from the case discussed in this paper.

### 7.3. The complexity of policy-relevant values

The third lesson is that the sort of value judgements that are relevant to economic policy are often much more complex than the ones that are often used in the examples discussed in the literature on inductive risk. These examples often seem to suggest that the value judgements required are relatively simple and clear-cut.<sup>15</sup> The moral dilemmas that face researchers in the prototypical examples discussed in the literature are often of the sort: ‘Are human lives more important than economic profits?’ I take it that almost every disinterested observer would agree that those sorts of questions have an obvious answer. However, the sorts of questions faced by experts and policymakers who deal with economic policy issues are usually much more complex than that and the answers are rarely simple and clear-cut.

On the one hand, the complexity of policy-relevant values in economics seems to strengthen the Argument from Inductive Risk. One of the recurring themes of economists’ education is that well-meaning policies often have consequences that are both unintended and undesirable. Much of this paper has focused on the debate over whether there is a stable trade-off between inflation and unemployment. However, people with no economics background often do not realize that unemployment and inflation might be related (if only in the ‘short’ run) or might underestimate the effects of high inflation on the economy. For example, people who have broadly egalitarian sympathies but have little familiarity with economics might think that, if indeed there is a trade-off between inflation and unemployment, the choice between lower inflation and lower unemployment would be simple and clear-cut, as unemployment is a greater social ill than inflation. However, on closer scrutiny, it is far from clear that this is actually the case even if one accepts broadly egalitarian values. For example, inflation has a disproportionately negative effect on low-income earners (in fact, economists sometimes refer to inflation as ‘the cruelest of all taxes’ in large part because of its effect on the poor (see e.g. Easterly and Fischer

<sup>15</sup>Of course, not all examples considered in the literature are equally simplistic from an axiological point of view. Anya Plutynski, for one, offers a very nuanced view of the complex value judgements required in the context of breast cancer screening (Plutynski 2017).

2001). In light of these considerations, it seems that, at least when it comes to the sorts of value judgements that are relevant to economic policy, the experts might be in a better position to appreciate the complexity of the value structure, which seems to be a further argument for entrusting the relevant value judgements to them.

On the other hand, due to their training, economists (and experts in general) tend to accept some version of the Value-Free Ideal and tend to be uncomfortable with making explicit value judgements and ill-equipped to make such judgements (see e.g. Earle *et al.* 2017). If the Argument from Inductive Risk is sound and researchers and advisors cannot avoid making value judgements along the process, it is likely that they make these judgements without much awareness or reflection. This illustrates one of the ways in which, as the proponents of the Argument from Inductive Risk sometimes claim, the Value-Free Ideal might not be merely unrealistic but harmful. We cannot trust the experts to make sound value judgements if they are uncomfortable with the idea that value judgements are pervasive in science and that they should be made reflectively and explicitly instead of unreflectively or implicitly. Philosophers of economics can play an important role in the transformation of economists' understanding of the relationship between science and values and in helping them acknowledge, map out, and navigate the relevant values.

#### 7.4. *The threat of technocracy*

The fourth lesson has to do with the threat of technocracy. The argument from inductive risk presents democratic societies with a dilemma. Ideally, we would like our policies to be informed by our best knowledge of the world, but we would also like the value judgements that underpin those policies to be made by policymakers who have the democratic mandate to make them on our behalf and who are accountable to us. However, if the arguments of the proponents of the Argument from Inductive Risk are sound, then we cannot have it both ways. Proponents of the Argument from Inductive Risk usually try to navigate this dilemma by entrusting researchers and advisors with making the necessary value judgements while specifying sets of normative principles to which researchers and advisors should adhere in making those value judgements. For example, critics of the Value-Free Ideal often distinguish between legitimate and illegitimate uses of non-epistemic values in the research process and usually insist that any value judgements should be as transparent and as possible (see e.g. Anderson 2004; Douglas 2009; Elliott 2017).

However, it is unclear to what extent this approach is likely to work. In particular, proponents of the Argument from Inductive Risk often seem to assume that researchers and advisors make (or, at least, ought to make) the relevant value judgements on the basis of values that are widely shared by society (and, more specifically, that they themselves share).<sup>16</sup> However, while this might be true when scientists who do not have any vested interests are considering largely

<sup>16</sup>This assumption, of course, is not shared by all philosophers. In fact, it is one of the main themes of the literature on the relationship between experts and liberal democracy (see e.g. Turner 2003, 2013; Brown 2009).

uncontroversial questions, such as the question of whether human lives are more important than profits, as I have argued in the previous subsection this is not always (or even usually) the case. Cases such as the Great Canadian Slump should make us less comfortable with entrusting individual experts with the relevant value judgements. Even a cursory look at the media coverage of Crow's tenure as Governor of the Bank of Canada clearly indicates that his policies were widely unpopular at the time among Canadians and yet Crow presumably held a genuine belief that inflation was a greater ill than the Canadian public realized.

The risk with leaving the value judgements entirely to the individual experts is that their value system might not be representative of the values of society at large. If the expert happens to hold idiosyncratic, or objectionable moral views (e.g. they hold racist, sexist or classist views), can society really trust them to make the right value judgements on its behalf? If, as the proponents of the Argument from Inductive Risk argue, it is practically impossible not to entrust experts with making value judgements on our behalf, then, at the very least, we should try to think of ways to make the relevant scientific community more demographically representative of the population at large, the process more transparent, and the experts more accountable, and more responsive to society's values, as a democracy that leaves too many important policy-relevant value judgements to unelected, unaccountable, and unrepresentative experts runs the risk of turning into a technocracy. Nowadays, monetary policy decisions are usually made by specially appointed committees, as opposed to individuals.<sup>17</sup> However, while this a step in the right direction, it is unlikely to be sufficient to guarantee transparency and representativeness and it might be advisable to explore more radical reforms, such as the ones suggested in (Earle *et al.* 2017), which include the formation of lay councils within central banks as well as more general proposals to diversify the economic profession and economic education.

### 7.5. The fog of policy

The fifth and, I believe, most important lesson is that our theories and hypotheses about how different proximate policy goals advance more distal policy goals and, eventually, the ultimate goals of society are as uncertain as (if not more uncertain than) our theories and hypotheses about how specific policies advance our proximate policy goals. No policy can directly advance the ultimate goals of society (whatever these might be). Usually, policies are meant to advance some proximate goals, which, in turn, are supposed to advance more distal goals and, eventually, the ultimate goals. However, even if we could all agree on the ultimate goals of society (e.g. maximizing total utility, promoting human flourishing, . . .), there would still be widespread disagreement concerning how to reach those goals, as it is far from clear which intermediate goals we should

<sup>17</sup>One of Fortin's two proposals in his CEA Presidential Address was to institute a committee to make monetary policy decisions. Monetary policy decisions at the Bank of Canada are now taken by its Governing Council, a committee that includes the Governor, the Senior Deputy Governor and four Deputy Governors and which was instituted under Crow's successor, Gordon Thiessen. For an illuminating empirical study on the effects of epistemic diversity on the decisions of the Bank of England's Monetary Policy Committee, see Hansen *et al.* (2014).

adopt to advance those ultimate goals and which policies we should adopt to achieve the intermediate goals. For example, as we have seen, one might think that inflation hampers economic growth and that economic growth contributes to maximizing total utility (or whatever the ultimate goals of society might be). In this sense, reducing inflation becomes an intermediate goal insofar as it promotes more distal goals, such as promoting economic growth and, eventually, society's ultimate goals. However, as I briefly discussed in §6, the hypothesis that inflation hampers economic growth is itself subject to significant uncertainty (and this is particularly true of inflation in the range that the Canadian economy was facing in the late 1980s and early 1990s).

So far, most of the literature on inductive risk has focused on the inductive risks associated with the hypotheses that connect specific policies to their proximate goals. However, the lesson I want to draw here is that the theories and hypotheses that connect our proximate policy goals to more distal goals and, eventually, to our ultimate goals tend to be much more general, less clear, and less amenable to empirical testing than the theories and hypotheses that connect specific policies to their intended goals. As such, theories of the former sort are even more subject to inductive risk than theories of the latter sort.

In his Hanson Lecture, Crow offered an interesting variation on the standard analogy between driving and monetary policy:

Accepting, as we must, this real world uncertainty means recognizing that the anticipated consequences of any given actions by the Bank of Canada for total spending or prices in the economy will never be precise. (This is not to deny the usefulness of the Bank's macroeconomic models, which are as large, or as small, as anybody's, and just as sophisticated. But we know their limitations as well as their virtues in organizing our thoughts about policy and the economy.) We must then conclude that even though we may know our destination and the general route by which we must get there, conducting monetary policy in such circumstances is akin to driving without full vision – perhaps like driving in a rainstorm with defective windshield wipers. It can be done, but only very carefully. (Crow 1988: 11)

If my analysis is correct, then Crow's analogy does not go nearly far enough – the uncertainty is not just limited to the weather, the road conditions or the effectiveness of the windshield wipers (i.e. the real-world context in which policies are implemented), but it also involves the accuracy of the map (i.e. the accuracy of the theories, hypothesis and models that guide those policies) and the way in which we have charted the route to the final destination (i.e. how our policies relate to our proximate goals and the proximate goals relate to the ultimate goals).

In fact, another familiar analogy might be more apt. Paraphrasing von Clausewitz's famous remark about war, we might say: 'policy is the realm of uncertainty and most of the factors on which policy decisions are based are wrapped in a fog of greater or lesser uncertainty'. The fog of policy does not only envelop the relation between the means (our policies) and their proximate ends, but also the one between the intermediate ends and the ultimate ends.

While it is beyond the scope of this paper to explore all of the implications of this important lesson, I briefly turn to the one that is most relevant to this case in the next subsection.

### 7.6. Institutional tunnel vision and the fetishism of proximate goals

One of the consequences of the phenomenon that I have dubbed ‘the fog of policy’ in the previous subsection is that it seems to contribute to the fetishization of intermediate goals. From within the fog of policy, it is easy to lose track of what the distal goals are and how they advance the ultimate goals and act as if the proximate goals are of absolute value instead of having only instrumental value. There might be a number of ways in which this can happen but one of them, as we have seen in §6, is a certain sort of institutional tunnel vision. The sixth and final lesson of this paper is that the way we conceive of institutions and their goals might contribute to reducing the visibility of the ultimate goals through the fog of policy and encourage the fetishization of intermediate goals. It is easy for experts and policymakers who work for an institution whose main task is the pursuit of certain intermediate goals to lose sight of the fact that, in the grand scheme of things, those goals are not ultimate goals of absolute value, but only intermediate goals of instrumental value and that, as such, they have to be balanced with other goals.

The increasingly narrow understanding of the goals of central banks is a very clear instance of this problem. For a variety of reasons, the responsibilities of central banks tend to be very limited and they often only include the safeguard of monetary stability. It is easy for someone who works for an institution whose principal goal is to promote monetary stability and whose performance is evaluated on the basis of its ability to achieve that goal to assume that that goal is of paramount importance.

## 8. Conclusion

This paper had two goals. Its less ambitious goal was to call attention to the relevance of inductive risk to economics. Its more ambitious goal was to draw some general lessons about inductive risk from the examination of a specific case of inductive risk in economics. In my opinion, the most important of these lessons is the one I dubbed ‘the fog of policy’ – i.e. the notion that the theories and hypotheses that connect our proximate policy goals to our distal policy goals are as uncertain as (if not more uncertain than) those that connect specific policies to their proximate goals. I have suggested that the fog of policy has a number of significant real-world consequences. For example, it seems to contribute to what I have called the fetishization of intermediate goals and to institutional tunnel vision. While I was only able to provide the briefest sketch of this phenomenon in this paper, I hope that future work can provide us with a clearer and fuller picture of it and of its consequences.

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