Economists as Experts: Overconfidence in theory and practice

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Abstract Drawing on research in the psychology of judgment and decision making, I argue that individual economists acting as experts in matters of public policy are likely to be victims of significant overconfidence. The case is based on the pervasiveness of the phenomenon, the nature of the task facing economists-as-experts, and the character of the institutional constraints under which they operate. Moreover, I argue that economist overconfidence can have dramatic consequences. Finally, I explore how the negative consequences of overconfidence can be mitigated, and how the phenomenon can be reduced or eliminated. As a case study, I discuss the involvement of Western experts in post-communist Russian economic reforms.

Keywords: overconfidence, calibration, economists, experts, public policy, Russian reforms.

> ignorance more frequently begets confidence than does knowledge
> – Charles Darwin

> Unfortunately for the good sense of mankind, the fact of their fallibility is far from carrying the weight in their practical judgment which is always allowed to it in theory; for while everyone well knows himself to be fallible, few think it necessary to take any precautions against their own fallibility, or admit the supposition that any opinion, of which they feel very certain may be one of the examples of the error to which they acknowledge themselves to be liable. – John Stuart Mill

> Economists have nothing to lose by understating rather than overstating the extent of their certainty. Indeed, it is only when this is done that the overwhelming power to convince of what remains can be expected to have free play. – Lord Robbins

1 INTRODUCTION

Do economists display too much confidence in their judgments? F. A. Hayek, for one, often emphasized the discrepancy between what economists (and others) think they know and what they in fact do know, as well as the unanticipated and potentially disastrous results that may follow from acting on such beliefs (cf. Hayek 1973: 14 *idem*). The aim of this paper is to explore some evidence to the effect that individual economists acting as experts in matters of public policy might fall prey to overconfidence. My main thesis is that economists-as-experts are likely to be victims of significant overconfidence, and that the consequences can be dramatic. The case is based on research in the psychology of

judgment and decision making, which suggests that that overconfidence is endemic under a range of conditions. I argue that those conditions in fact obtain for individual economists-as-experts, given the nature of the task and the institutional constraints under which they operate.

As an illustration, I discuss the work of respected Swedish economist Anders Åslund, who acted as an advisor to the Russian government between 1991 and 1994. Åslund was one of the main proponents of 'shock therapy,' a policy of rapid deregulation and privatization presented as a means to quickly transforming Russia into a modern, market-based democracy. Though Åslund is typical in many ways, what makes his case interesting are the ways in which he is atypical. First, as a public figure and prolific writer, he is often admirably forthcoming. Thus, we have more information about his thinking processes, and about how they changed over time, than of most other economists in similarly important positions. Second, Åslund had a certain amount of influence over Russian economic policy during his tenure as an advisor in Moscow. Hence, we can actually say something about the consequences of heeding his advice. Though there is too little data to prove conclusively that Åslund was overly confident, his writings do appear to exhibit the marks of overconfidence.[1]

On a more positive note, I also propose a number of interventions that may serve to reduce the incidence of overconfidence among economists, or at least to weaken its negative consequences. In particular, drawing on the psychological literature, I explore how the institutional constraints under which economists operate might be modified so as to increase accountability and reduce overconfidence. Needless to say, overconfidence can affect all economists, including the present author. Fortunately, I will argue, the proposed interventions may also serve to diagnose the extent of overconfidence among those who

believe that economists-as-experts tend to be overconfident. Either way, I believe, a better understanding of overconfidence may help us make better decisions in matters of public policy, identify issues of value in public decision making, and preserve or restore trust in social science as a tool in rational decision making.

2 BASICS OF OVERCONFIDENCE RESEARCH

In this section I briefly review the psychological research on overconfidence. Empirical results on overconfidence appeared in the context of research on calibration, that is, the degree of correspondence between stated probabilities and observed frequencies. According to a standard definition: 'Formally, a judge is calibrated if, over the long run, for all propositions assigned a given probability, the proportion that is true equals the probability assigned' (Lichtenstein et al. 1982: 307). Thus, when a calibrated stock analyst assigns a probability of 90 percent that some stock will go up, nine times out of ten the stock in question goes up. It is worth noticing that it is possible to be perfectly calibrated even when outcomes cannot be precisely predicted, e.g. when the data-generating process is stochastic.

In general, researchers explore issues of calibration and overconfidence by asking subjects to make judgments and to include some measure of confidence. Thus, subjects may be presented with simple multiple-choice questions and prompted (for each question) to say how confident they are that the answer is correct. Alternatively, subjects may be asked to provide an estimate of some quantity, and to give a margin of error such as a 95 percent confidence interval. Then, judgments are grouped according to the degree of confidence and compared with actual hit rates, that is, the proportion of those judgments that were in fact correct. If the measure of confidence is higher than the hit rate, given

some range of confidence judgments, the subjects are said to be overconfident in that range.

Calibration has attracted the attention of researchers in psychology and related fields for decades. One of the most persistent findings in the literature is that people tend to attach more confidence to their judgments than they should in order to be calibrated. This conclusion was first established using undergraduate subjects answering general knowledge questions. Over time, however, manifestations of systematic overconfidence have been found also among physicists, doctors, psychologists, CIA analysts, and others making expert judgments.[2]

As miscalibration and overconfidence have been demonstrated in a large number of studies, I restrict attention to a few representative and well-known ones.[3] In an early and well-cited study, Fischhoff et al. (1977) explored the appropriateness of extreme confidence, that is, confidence levels approaching 100 percent. The researchers asked undergraduate subjects questions of the type 'Absinthe is (a) a liqueur or (b) a precious stone?,' and invited them to judge how confident they were that their answer was right. The degree of overconfidence was remarkable. Subjects who indicated that they were 100 percent certain that their answers were right were on the average correct 70 to 80 percent of the time (p. 554). Other studies explored confidence over the full range of confidence judgments (see Lichtenstein et al. 1982: 315). Typically, the studies conclude that overconfidence increases with confidence, and therefore is most extreme when confidence is high. Overconfidence is usually eliminated when confidence ratings are low, and when very low, subjects may even be underconfident.

Another conclusion is that overconfidence increases with the difficulty of the judgment task. Lichtenstein and Fischhoff (1977) found that overconfidence for difficult

questions turned into underconfidence for easy ones, when difficulty was defined by the number of correct answers (p. 180). Lichtenstein et al. (1982) report results from studies where difficulty was defined independently of subjects' responses. For instance, they would ask which of two cities was farther in distance from some third city, and define difficulty in terms of the ratio of actual distances. They, too, found that subjects were overconfident for hard questions and underconfident for easy ones (p. 316).

Overconfidence does not in general seem to decrease when judges receive more information, even when that information is relevant to answering the question. Some studies do suggest that more knowledgeable subjects – defined as those who make fewer errors – exhibit less overconfidence (see Lichtenstein and Fischhoff 1977: 168; and references). However, Oskamp (1982) found that all his groups of subjects became increasingly overconfident as they received more information. Oskamp asked his subjects questions about the behaviors, attitudes, and interest of a real patient. As the subjects received more and more information about the patient's life, they assigned more and more confidence to their answers. Meanwhile, their accuracy barely increased at all (1982: 291). Incidentally, the clinical psychologists in his sample – a majority of whom had Ph.D.'s – were no more accurate and no less confident than psychology graduate students and advanced undergraduates.

Yet another conclusion from these studies is that overconfidence is not ordinarily reduced when subjects are more motivated to give honest answers. To test whether increased motivation would decrease the degree of overconfidence, Fischhoff et al. (1977) asked subjects to express their confidence in terms of odds, and offered subjects to play a gamble based on those odds. The degree of overconfidence decreased somewhat, but remained high. When subjects said the odds that they were right were 100:1, in order to be

well calibrated they should have said 4:1; when they said the odds were 100,000:1, they should have said 9:1 (1977: 558). Since a majority of subjects agreed to play the gamble, the experimenters could have made a fair amount of money if they had actually collected their winnings. Again, Sieber (1974) gave two groups of students the same set of course-related multiple-choice questions. The test group was told that they were taking their midterm examination, while the control group was told that they were merely practicing. The test group, presumably more motivated to do well, were no more accurate than the control group, but significantly more confident (1974: 691). In this case, then, it appears that overconfidence actually increased with motivation.

Other studies indicate that overconfidence may be widespread outside of the laboratory as well. For example, Christensen-Szalanski and Bushyhead (1981) studied physicians diagnosing possible pneumonia patients. The degree of calibration was low indeed, and the doctors exhibited a large degree of overconfidence. When the doctors claimed to be more than 80 percent certain that the patient had pneumonia, X-rays confirmed the existence of the disease in only about 20 percent of the cases (see also Baumann et al. 1991). Henrion and Fischhoff (1986) examined physicists' estimates of physical constants, like the speed of light, and found remarkable degrees of overconfidence. The authors looked at 98 percent confidence intervals to assess degrees of confidence. When a well-calibrated judge gives a 98 percent confidence interval, the surprise index – the proportion of times the actual value lies outside of the interval – equals 2 percent. For measurements of the speed of light between 1875 and 1958, however, the surprise index was between 8 and 14 percent (1986: 794). These results indicate not only that overconfidence appears outside of the laboratory, but also that it appears when

knowledgeable judges make assertions within their field of specialization, and when they are motivated to provide accurate assessments.

There are exceptions, however, and some specialists are extremely well calibrated. Murphy and Winkler (1984) reviewed studies on meteorologists' forecasts of precipitation, and found exceptional calibration with minimal overconfidence. Keren (1987) also found that professional bridge players were well calibrated when judging the likelihoods that contracts will be fulfilled. Lichtenstein et al. (1982) attribute the successes of meteorologists to two factors. First, meteorologists make highly repetitive judgments. Second, they receive regular, prompt and unambiguous feedback. Lichtenstein et al. suggest that these conditions, which hold also for the professional bridge players but not for the physicians, work by making it easier for the judge to learn from experience.

Laboratory studies have confirmed that overconfidence can be reduced when judges receive feedback that is frequent, prompt, and unambiguous (Lichtenstein and Fischhoff 1980). Moreover, it appears that the tendency for overconfidence can be attenuated by requiring subjects to consider reasons that they may be wrong (Koriat et al. 1980). Simply telling subjects about the prevalence of overconfidence, and instructing them to be careful, only seems to make a minor difference (Fischhoff 1982: 437).

Incidentally, the overconfidence phenomenon receives indirect support from research on competence.[4] For example, many studies suggest that people overestimate their competence in various practical tasks. The vast majority of drivers – sometimes more than 90 percent – say that they are more skilful than the median driver (Svenson 1981). And undergraduate subjects whose test scores in grammar and logic put them in the bottom 25 percent of a group of peers, on the mean estimate that they are well above average (Kruger and Dunning 1999). Even more surprising, perhaps, when these subjects received more

information about their relative performance in the tests (by being asked to grade those of other subjects), the top-quartile subjects became better calibrated, but bottom-quartile subjects did not. Kruger and Dunning write: 'If anything, bottom-quartile participants tended to raise their already inflated self-estimates, although not to a significant degree' (p. 1127). On the basis of these results, the authors suggest that the least competent are at a double disadvantage, in that their incompetence 'not only causes poor performance but also the inability to recognize that one's performance is poor' (p. 1130).

Research on overconfidence has also been subjected to criticism, the most famous of which is perhaps associated with Gerd Gigerenzer.[5] In articles like 'How to Make Cognitive Illusions Disappear: Beyond "heuristics and biases"' (1991), Gigerenzer argues against the robustness of the overconfidence phenomenon. The criticism is based on the notion that overconfidence should be measured differently. Specifically, he writes: 'Ask people for their estimated relative frequencies of correct answers and compare them with the true relative frequencies of correct answers, instead of comparing the latter frequencies with confidences' (p. 89). Gigerenzer and co-authors started out by following the standard format, and presented subjects with multiple-choice questions and (in each case) prompted them to state the probability that they were correct. Afterwards, the experimenters asked subjects what proportion of the previous questions they thought that they had answered correctly. While subjects were overconfident according to the traditional measure, they were remarkably calibrated according to the new one. Gigerenzer concludes: 'Comparing their estimated frequencies with actual frequencies of correct answers made "overconfidence" *disappear*.... The "cognitive illusion" was gone' (p. 89, italics in original).

There is something odd about this argument, however. There appear to me to be at least two different concepts involved in this argument. What we can call *overconfidence*$_1$ is

overconfidence as it is usually understood in the literature, as referring to the discrepancy between subjective judgments of confidence and actual frequencies. What we can call *overconfidence*$_2$ is overconfidence as Gigerenzer thinks we should understand it, as referring to the discrepancy between judged frequencies and actual frequencies. While Gigerenzer happily acknowledges that subjects exhibit overconfidence$_1$, his main finding is that overconfidence$_2$ is virtually zero. This is an empirical result that would not have surprised Mill (cf. the epigraph), and that may be of some significance. But far from showing that overconfidence as it is typically understood (as overconfidence$_1$) disappears, Gigerenzer's research in fact confirms its presence under a range of conditions. Notice, by the way, that because this paper is concerned primarily with economists' confidence in their judgment at the time when they make that judgment, what matters here is overconfidence$_1$. As a result, Gigerenzer's empirical findings, while interesting, should not undermine my argument.

3 OVERCONFIDENCE AMONG ECONOMISTS-AS-EXPERTS

In August 1991, what was then the Soviet Union experienced a failed coup d'état. The event proved to be a major turning point of modern East European history. In retrospect, it marked the end of the Soviet Union and the reemergence of Russia as an independent state; a shift of power from Mikhail Gorbachev to Boris Yeltsin; and the beginning of the Russian transition to a democracy and market economy (Åslund 1995: 53; 2002: 58) The reform program that was adopted under Yeltsin was developed in cooperation with a team of Western economists who argued for 'shock therapy,' or a 'big bang,' as a means to transforming Russia into a modern market-based democracy. The core of this proposal was 'radical market-oriented economic reform,' i.e. a policy of rapid deregulation and privatization (Åslund 1995: 10).

In this section I argue that economist-experts like those involved in the Russian reforms are likely to be the victims of significant overconfidence. I will outline the various considerations, both theoretical and empirical, that support this contention. As a case study, I will focus on the activities of Anders Åslund, who acted as an advisor to the Russian government from November 1991 to January 1994, and was 'closely involved in the reform process' (Åslund 1995: 2). Åslund was one of the strongest proponents of shock therapy, which he called 'the only cure' (1992a). He predicted that the Russian Gross Domestic Product (GDP) might drop by 'at least 20 percent' as a result of the therapy before taking off again (1992b),[6] but he promised that the therapy would work fast (1993a), and have positive social consequences (1995: 10). Though the example concerns an economy in transition, the general argument applies also to mature market-based economies.

The main argument in support of my thesis is based on the mere prevalence of overconfidence. Economists-as-experts are overconfident, I would argue, not because they are different from everyone else, but because they are just like everyone else. As we saw in the previous section, overconfidence has been demonstrated among experts and lay people, among the more and the less educated, among the well-informed and to the not-so-well-informed, and in a wide range of knowledge domains. Exceptions appear, but under rather circumscribed conditions (which, I will argue below, fail to obtain for economists-as-experts). Since overconfidence is so prevalent, it would be surprising if economists-as-experts were not the victims of significant overconfidence. Overconfidence implies that the (possibly implicit) margin of error is significantly larger than intended. Thus, we would have had good reason to believe that the Russian reality would turn out to be quite different from Åslund's predictions.

Nevertheless, economists acting as experts often express themselves with great confidence. The studies reviewed in the previous section suggest that high confidence is a mark of overconfidence. Åslund, expressing his faith in shock therapy, wrote: 'The simple truth is that shock therapy works very well, and it works fast' (1993a). And: 'The more radical the liberalization, the sooner the markets will be cleansed from distortions and corruption. A swift transition will also have positive social effects' (1995: 10). His writings bore titles like 'The Triumph of Capitalism' (1993a), 'Russia's Success Story' (1994), and 'Russian Shock Therapy Succeeded' (1996). Many others have commented upon the remarkable confidence exhibited by certain economist experts. In his scathing criticism of the International Monetary Fund (IMF), which also was involved in the Russian reforms, Joseph Stiglitz wrote:

> IMF experts believe they are brighter, more educated, and less politically motivated than the economists in the countries they visit. In fact, the economic leaders from those countries are pretty good – in many cases brighter or better-educated than the IMF staff, which frequently consists of third-rank students from first-rate universities. (2000: 57)

As a former Chief Economist and Senior Vice President of the World Bank, Stiglitz has experience with both IMF experts and foreign economic leaders.

Of course, economists who act as experts in matters of public policy may have incentives to exaggerate their confidence. The famous remark about the one-handed economist, for example, suggests that policy-makers expect their economic experts to express themselves confidently. Assuming that economists (like everybody else) respond to incentives, this provides another reason for thinking that economists will exhibit

overconfidence. The possibility suggests that we should draw a distinction between the probability privately assigned to a given judgment and that publicly assigned to it. Similarly, we should perhaps draw a distinction between private and public calibration. If so, some economists may be publicly, but not privately, overconfident. The likelihood that public overconfidence in many cases may exceed private overconfidence, however, does not mean that private overconfidence is non-existent. The empirical evidence on which my main argument is based suggests that overconfidence affects judges even when they have strong incentives to be well calibrated, as in the study by Fischhoff et al. (1977).[7]

Another indication that economists-as-experts are overconfident is the fact that the judgment tasks they face are challenging. As we have seen, overconfidence tends to increase with difficulty. The difficulty in predicting economic variables is confirmed by the low degree of accuracy reached by experts in the field. In some studies, professionals actually do worse than chance. In a classic study, only three of 72 investment bankers, stock market experts, statisticians, and business administration instructors predicting stock prices did better than chance, i.e. better than they would have done if they had assigned equal probabilities to all the alternatives (Staël von Holstein 1972; cf. De Bondt 1991; Yates et al. 1991; Blix et al. 2001). Indeed, according to a popular view it is impossible to predict the value of a stock with success better than chance, so long as you do not have access to better information than the market does. The claim is referred to as the efficient market hypothesis, and asserts that all publicly available information is already incorporated into the price of a stock (Yates et al. 1991: 62). Donald McCloskey has argued that the conclusion carries over to 'all predictions of trend, in journalism, sociology, political science, commercial art, and elsewhere' (1992: 104). Whether the efficient market

hypothesis is strictly true or not, it is clear that the judgment tasks facing economist-experts are difficult indeed.

Given the difficulties involved in predicting economic variables, then, it is perhaps no coincidence that Åslund's predictions were well off the mark. According to official World Bank statistics, Russia's GDP in constant 1995 US dollars dropped by almost 40 percent between 1991 and 1998 (see figure).[8] This is twice Åslund's estimate. Though GDP increased between 1998 and 2003, the last date of available World Bank statistics at the time of writing, GDP remains more than 16% below the 1991 level. Similarly, Åslund's predictions about the positive effects of shock therapy do not appear borne out by subsequent events. For example, between 1991 and 1994 life expectancy in Russia dropped by as much as 4.7 years (6.2 years for men). Though the numbers have since picked up, the last available figure is still a full three years below that of 1991. In sum, the confidence Åslund expressed in the truth of his judgments may well have exceeded their accuracy.

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Though Åslund takes shock therapy to have been a success, not all knowledgeable observers concur. Indeed, some take exception in unusually strong terms. The Wall Street Journal calls the rapid privatization advocated by Åslund 'a dumb idea' (2000). Jagdish Bhagwati, Columbia professor of economics and outspoken proponent of free trade, talks about 'the supreme folly of "shock therapy"' (1999), and refers to the Russian reforms as a 'huge mistake' and 'spectacular failure' (Khanna 1998). In the past, Åslund has dismissed his critics as 'socialists' (1992a; 1992b). Yet it is perfectly possible to believe in a market-based democracy without taking shock therapy to be the best means to that end, as the

examples of The Wall Street Journal and Jagdish Bhagwati show. Obviously, these quotes do not by themselves prove that shock therapy failed. They do show, I think, that the evidence is not as clear-cut as Åslund suggests. There seems to be a discrepancy between the strength of the evidence, which appears at best ambiguous, and Åslund's unwavering confidence in his judgment.

Would not overconfident economists realize that they overestimate their abilities, and adjust accordingly? The answer seems to be 'not necessarily.' For starters, it is in the nature of their role as experts that the economists may not receive adequate outcome feedback. For example, an economist in the position of a government advisor may find that politicians in fact act on the advice, thereby subverting the possibility of ever knowing what would have happened in the absence of their interventions. In the case of the former Soviet Union, a lack of reliable statistics makes it particularly difficult to assess economic changes properly (Åslund 2002: 15-18). Moreover, many expert judgments are counterfactual in character, such as 'it would have been lethal to hesitate or move slowly' (Åslund 1995: 11). In the case of counterfactuals, of course, proper outcome feedback is impossible to come by even in principle.

Furthermore, learning from experience is more difficult than one might think, even in the presence of outcome feedback. One reason why we have trouble learning from experience, research suggests, is that people tend to overweight evidence that supports their position and underweight evidence that undermines it. This tendency is referred to as the confirmation bias. As Matthew Rabin writes: 'People tend to misread evidence as additional support for initial hypotheses' (1998: 26, italics in original). Indeed, people with diametrically opposed initial beliefs are capable of interpreting the same piece of ambiguous information as support of their view (1998: 27). If we tend to ignore

disconfirming evidence, we may not realize that we were wrong in the past, and we may see no need be more cautious in the future. The fact that the Russian GDP dropped more than expected under shock therapy, Åslund argued, was not due to flaws in the therapy, but a result of 'extraordinary rent-seeking' (1999a: 1). However, the fact that GDP seems to be increasing again is indeed an effect of comprehensive 'structural reforms' (Åslund 2000a; 2000b). Åslund's use of the positive evidence, and his dismissal of the negative one, is consistent with confirmation bias.

Another reason why it is hard to learn from experience, according to the psychological research, is that we exaggerate the predictability of past events. This tendency is referred to as the hindsight bias. Thus, we may exaggerate in hindsight what we could have predicted in foresight, and we misremember what we in fact did predict so as to exaggerate in hindsight what we knew in foresight (Fischhoff 1975). Victims of the hindsight bias may never learn that past predictions were no good, because they misremember what they in fact predicted. Thus, they will see no need to be less confident in their future predictions. In a 1999 article in Foreign Affairs, entitled 'Russia's Collapse,' Åslund suggested that wide-spread rent-seeking was entirely predictable, and claimed that 'Russia's current tragic situation ... was not inevitable, but its probability was always great' (1999b: 70). This statement could very well be an expression of hindsight bias. If rent-seeking and the subsequent collapse were entirely predictable, it is unclear why he made such optimistic statements about the effects of shock therapy in the first place.

Evidence shows that both the confirmation bias and the hindsight bias are stronger when predictions are vague and outcome feedback is ambiguous (Rabin 1998: 28; Fischhoff 2001: 547). Insofar as people have a tendency to misremember predictions and reinterpret outcomes so as to render them more compatible, their task is made that much

easier by ambiguity and vagueness. In fact, economic forecasts are often both vague and ambiguous. They are not seldom cast in terms whose meaning is ambiguous, and very often the time frame is insufficiently specified. Moreover, it does not help that many forecasts are expressed in terms of metaphor. A prediction like 'The economy is headed south' embodies all of these problems. Even standard economic terms are multiply ambiguous. 'GDP' is a good example; the World Bank reports several different varieties of the measure. Indeed, Åslund's predictions remain vague, and this makes it challenging to assess their accuracy after the fact. When predicting that Russian GDP may drop by 'at least 20 percent,' for example, Åslund did not specify what measure of GDP he had in mind, what the time frame was supposed to be, under what conditions the predictions were supposed to hold, or what the qualifier 'at least' should be taken to mean. Thus, almost no matter what happens, he can claim to have gotten it right. Anyway, if GDP dropped by less than 20 percent, few would complain.[9]

Several of the problems mentioned so far may in part stem from the character of the institutional constraints under which they operate. Economists acting as experts do not appear to face effective social sanctions that encourage them to minimize the ambiguity and vagueness of their predictions. Less vague and ambiguous predictions would make it easier to learn from experience, and should to some extent mitigate the effects of the confirmation and hindsight biases. Similarly, economists-as-experts do not appear to suffer noticeable penalties for expressing extreme confidence in their judgment. It is quite possible that social or institutional constraints that decrease confidence would reduce overconfidence. The lack of effective institutional constraints therefore supports the contention that economists are likely to exhibit overconfidence (both private and public); they work under social and institutional constraints that invite it.

Moreover, overconfidence among social scientists has in fact been demonstrated in empirical studies. Tetlock (1999) asked 'relevant area-studies specialists' to judge the likelihood of a number of political, economic, and military events occurring within some specified time frame (usually five years). An average of 27 experts answered each question. Although the actual hit rate was only slightly better than one would expect from chance alone, the experts were quite confident that they would be right. Thus, overconfidence was endemic: 'Across all seven predictions, experts who assigned confidence estimates of 80 percent or higher were correct only 45 percent of the time' (1999: 351). Moreover, experts whose predictions failed were unwilling to revise their understanding of the fundamental forces at play. While experts would explain correct predictions in terms of their deep understanding, they would only rarely explain incorrect predictions in terms of a lack of such understanding. Instead, their most common response to failed predictions was an assurance that the prediction in fact was 'almost right' (p. 351), something which in Tetlock's view supports the importance of the hindsight and confirmation biases (p. 363), and prevents experts from learning from their mistakes (p. 357).[10] Åslund, for one, does not see the need to reevaluate his fundamental assumptions. He affirms that 'the Russian experience has reinforced my convictions' (1995: 12), and concludes that Russia needs more shock therapy, not less: 'The post-Soviet government is distinctly evil…. The current situation requires even more radical liberal reforms than most people advocated earlier' (1997a; cf. 1999a and 2001a). Undeterred, he went on to advise the governments of Ukraine and Kyrgyzstan (see 2002: xiv-xv), and to argue that Sweden too needs shock therapy of an East European variety (1997b).

Tetlock's research illustrates how easy it is for economists-experts to explain away uncomfortable outcomes. To explain the disastrous drop in GDP, which at least

superficially is inconsistent with his predictions, Åslund develops a new measure of GDP, according to which Russian GDP dropped by no more than 6 percent before increasing again (2001b: 15). Again, this illustrates the importance of clear predictions; since Åslund did not specify what measure of GDP he had in mind in the first place, it is hard to determine whether this later move is a post hoc rationalization or not. In the process of deflecting criticism, Åslund has also argued that shock therapy never happened: 'It is a myth that Russia has been subjected to a big bang or "shock therapy"' (1993b: 19). This claim is of course difficult to reconcile with certain other assertions, such as 'Russian Shock Therapy Succeeded' (1996). Presumably, to have succeeded in Russia, a therapy must at some point have been implemented there. Again, this illustrates the importance of clearly specifying the meaning of the terms, and the conditions under which the prediction is intended to hold.

Finding post hoc rationalizations for seemingly misguided predictions is facilitated by the nature of economic predictions, generalizations, and 'laws.' Most or all predictions in economic matters should be seen as incorporating implicit provisos or ceteris paribus clauses, such as '... unless in the case of force majeure: war, natural disasters, unusual political and economic events, and so on.' If the actual outcome does not seem to conform to the predictions, therefore, it is possible for the judge to claim that some implicit condition was not in fact fulfilled. Again, this illustrates the importance of clear predictions. Note, however, that though implicit provisos can be invoked to explain why one particular prediction failed, they cannot explain miscalibration. If the economists really do know what they are talking about, they should have a sense for how often the provisos need to be invoked, and adjust their original predictions accordingly.

The point about implicit provisos can be clarified by looking at Åslund's prediction that shock therapy would have positive social effects. It is unclear what exact effects he had in mind, but the prediction seems disconfirmed by the fact that life expectancy in Russia has dropped dramatically. This fact is particularly paradoxical if we accept his own figures, according to which GDP has decreased by no more than 6 percent after the implementation of shock therapy (2001b: 15). In Åslund's view, the 'dominant explanation is probably psychological' rather than economic: 'The collapse of the Soviet Union appears to have brought about an existential shock to Baltic and East Slavic men' (2001b: 16). The presence of 'existential shock' is presumably one of the things covered by implicit provisos.[11] It is similarly strange that polled Russians consistently report that they are worse off after the reforms. However, Åslund argues that such reports should 'be taken with a great deal of skepticism,' since, among other things, people do not think in terms of Pareto optimality like economists do (2001b: 17).

Another reason to believe that economist-experts are overconfident has to do with selection bias. It is plausible to assume that confident economists tend to be overrepresented among those who act as experts in matters of public policy. The more confident individuals can be expected to be more likely to offer guidance, and to make themselves available when their services are in demand. Also, the confident are more likely to appear in the media, and therefore be well known outside of narrow professional and academic circles. Moreover, confidence may often be mistaken for competence (see Baumann et al. 1991: 167; Johnson et al. 1988). For these reasons, we should expect more confident economists to be overrepresented among those who act as experts. If we can assume that economist-experts are selected from some pool of roughly speaking equally

competent individuals, it follows directly that those selected will on the whole be more overconfident than the average.

In Åslund's case there are clear signs of self-selection. Two weeks after the failed coup in 1991 he apparently traveled to Moscow with the explicit intention to 'find out who would become the leading reformer,' and to advice him or her on 'radical and comprehensive market-oriented reform' (1995: 16). Moreover, Janine Wedel (1998) has argued that Åslund and his American colleagues used the added leverage given to them by the fact that they could promise access to Western money (p. 123 ff.). Presumably a more timid colleague of Åslund's would have been much less likely to end up as an advisor to the Russian government.

To summarize, there are many reasons to believe that economists-as-experts should fall prey to overconfidence. The main reason is that economists are human, and therefore subject to the same biases as the rest of us. Indeed, we should be surprised if they did not exhibit systematic overconfidence. The case is strengthened, however, by the fact that many economist-experts express themselves with great confidence, and by the fact that they work under conditions that invite overconfidence. The natural propensity for overconfidence is exacerbated by the nature of the judgment tasks – difficult problems with little or no outcome feedback – and by the lack of institutional constraints that moderate expressions of confidence and require minimally ambiguous predictions.

It should be mentioned that while overconfidence is sometimes referred to as a bias, nothing in my argument presupposes that it is irrational to exhibit overconfidence. An overconfident agent can still be internally consistent, and therefore rational in the Bayesian sense (Gigerenzer 1991: 88). Moreover, there may be real benefits associated with being overconfident (see e.g. Kyle and Wang 1997). Indeed, if overconfidence is e.g. biologically

adaptive, my argument that economists-as-experts are overconfident would only be strengthened.

4 CONSEQUENCES OF ECONOMIST OVERCONFIDENCE

What are the consequences of overconfidence among economist-experts? In this section I discuss three different kinds of adverse consequence, and argue that they can be dramatic. Moreover, I consider a counterargument to my claim that economist overconfidence is likely to have negative effects. The argument suggests that economists' overconfidence is inert, in a certain sense, since decision makers expect them to be overconfident and discount expressions of confidence to the appropriate degree.

Most obviously, overconfidence among economist-experts may lead to misguided policy decisions. Expert judgments often serve as the basis for economic policy, which affects people's lives in important ways. A policy based on overconfident estimates of the probability of success, for instance, is likely to be worse than policies based on realistic estimates. We should keep in mind that experts when good can be very helpful, when bad can be dangerous. Between 1991 and 2003, the population of Russia shrunk by 5.2 million people. Even if only a small fraction of the lives lost is attributable to misery caused by misguided economic policy, the cost in human lives would still be tremendous.

Moreover, overconfidence among economists obscures important issues about values in public decision making. Many decision problems are best seen as a matter of choice under uncertainty rather than choice under risk. Isaac Levi (1980) has made the case that in many cases the relevant probabilities are sufficiently ambiguous that we had better not assign a determinate number to them. Now, decision making under uncertainty requires a different set of principles of rational choice than does decision making under

risk.[12] There are several such principles, and the choice of principles can legitimately be seen as reflecting a value commitment on the part of the agent (Levi 1980: 156-63). Yet, experts according to whom most or all decisions involve risk rather than uncertainty will obscure the relevance of such principles. The issue about what values to rely on, and what principles to use, will be hidden from view.

Finally, overconfidence undermines public trust in economics as a tool in rational decision making. When expert forecasts fail, repeatedly and dramatically, it is understandable if economist-expert advice ultimately tends to be discounted or ignored. In 1981, Business Week discussed experts' predictions of macroeconomic variables, and concluded: 'By overselling their ability, they have virtually built an automatic credibility gap' (quoted in Ahlers and Lakonishok 1983: 1113). In short, overconfident economists give economics a bad name. In contrast, by realizing the limitations of expert knowledge, and by making more well calibrated judgments, that trust can be preserved or restored. If we believe that we are better off relying on good economics – properly gathered data and well-confirmed generalizations – in economic affairs, trying to defeat overconfidence is worth our while.

It may be objected, however, that real-world policy makers are aware of the fact that economists-as-experts express themselves with exaggerated confidence, and that expressions of confidence are discounted accordingly.[13] This line of argument can be supported by some simple game theoretic considerations. It is possible to imagine a game in which the economist chooses how confidently to express herself and the decision maker chooses by how much to discount expressions of confidence, and an equilibrium in which the policy maker discounts the confidence just as much as the economist exaggerated it. In

such an equilibrium, it can be argued, expressions of excessive confidence are inert, in the sense that their ultimate impact on the decision is zero.

There is little doubt that the interaction between economist and decision maker can be modeled in such a manner (see e.g. Crawford and Sobel 1982 for an analysis along these lines). The question is whether economists and decision makers are, in fact, in such an equilibrium. To the best of my knowledge, the most direct evidence relevant to this issue appears in a paper by Daylian M. Cain, George Loewenstein, and Don A. Moore (2005). The authors consider a situation in which one agent (the "estimator") attempts to estimate an uncertain quantity on the basis of the advice given to them by another agent (the "advisor") who has more information but an incentive to give biased advice (Cain et al. 2005, 8). This situation is relevant to the question at hand because we can take the uncertain quantity in question to be the actual degree of confidence of the economist advisor.

Cain et al. (2005) predict that estimators will be unable to properly discount advice from biased advisors. For example, they write:

> Research on the "curse of knowledge" ... shows that people's judgments are influenced even by information they know they should ignore. And research on what has been called the "failure of evidentiary discreditation" shows that when the evidence on which beliefs were revised is totally discredited, those beliefs do not revert to their original states but show a persistent effect of the discredited evidence (Cain et al. 2005, 6).

In their experiment, in effect, estimators failed to discount biased advice as much as they should have in order to maximize their payoffs (estimators earned money to the degree that their estimate of the uncertain quantity was correct) (Cain et al. 2005, 17). Incidentally,

Cain et al. also find that repetition of the task, and feedback about the actual value of the unknown quantity in previous tasks, did not improve estimators' ability to discount biased advice (Cain et al. 2005, 18).

The evidence from Cain et al. (2005) thus appears to undermine the proposition that economists and decision makers find themselves in an equilibrium in which expressions of excessive confidence are inert. This is not to say, of course, that decision makers do not discount expressions of overconfidence at all, only that they do not do it enough. Incidentally, insofar as people are unable to properly discount economists' expressions of confidence, it should make no difference whether the overconfidence in question is public and private, or merely public. As far as I can see, anyway, there is little reason to think that the effects would differ.

5 SOLUTIONS AND REMEDIES

Though I have argued that economists acting as experts in matters of public policy are likely to be overconfident, nothing in the argument implies that we should dispense with economics or economic experts when making public policy decisions. To the contrary, I take it for granted that we are on the whole better off relying on serious economic analysis in public decision making. Sometimes it may be possible to obviate the need for expert advice by forming our own opinion on the basis of available evidence, but doing so is often impractical or impossible. The question, then, is how we should handle overconfidence among economists-as-experts. Assuming overconfidence comes at a cost, should we learn to live with it or should we try to eliminate it?

In the short term, undoubtedly, we will need to treat overconfidence as a fact, and learn how to mitigate its negative effects. The most straightforward manner to handle

experts' overconfidence is to learn to take their judgments with an appropriate grain of salt. Though we may still seek expert advice, and rely on it when making policy, we should keep in mind that they may be overconfident. Research on the calibration of probabilities can help us here, in that it gives us a rough idea of the magnitude of the effect. For instance, we can estimate that a confidence of say 95 percent should be interpreted as X percent, and that confidence intervals should be multiplied by a factor of Y. Gordon and Kammen (1996) have begun to explore such questions. They analyze predictions of the Standard and Poor's 500 Stock Index, and conclude that 'standard confidence intervals must be increased by a factor of 3.4 ... to produce 95% confidence intervals' (1996: 193). Though it is likely that the degree of overconfidence varies across people and judgment tasks, empirical research gives us a better idea about how to plug expert judgments into our calculations. However, adjusting expert judgments according to some pre-determined factor can only serve as a temporary solution. If we cannot keep this procedure a secret from the experts themselves, we are running the risk that they modify their original predictions in such a way that nothing is gained (cf. Lichtenstein et al. 1982: 331-2).

In the longer term, therefore, a better strategy would be to try to eliminate overconfidence at the source. There are, in principle, two ways to do so: to improve hit rates, or to decrease confidence ratings. I take it for granted that economists-as-experts do not need not be told to improve their hit rates; presumably, they are already trying. Thus, potential interventions will focus on decreasing experts' confidence ratings. As we saw in section two, there is evidence that overconfidence can be reduced and even extinguished given the right sort of intervention. So far, two methods have been found reasonably effective in reducing or extinguishing overconfidence in judgment. The first involves requiring judges to provide arguments against their view, reasons why they may be wrong.

The second involves providing feedback that is frequent, prompt, and unambiguous. Though Arkes et al. (1987) manage to reduce overconfidence by giving subjects apparently simple, but deceptive, practice problems, and then providing feedback, I take their method to be impractical in the present context. And as we saw above, simply telling the experts to be more careful is unlikely to help much.

First, I suggest, we should take every opportunity to ask social scientists acting as experts for reasons that they may be wrong. What would it take for their predictions not to come true? How many ways are there that things can go wrong? Just how likely are they? Judging by casual observations, economists during academic conferences, on talk shows, in news reports, and so on are almost exclusively asked to provide reasons for their views. To reduce overconfidence, they should also be asked to provide reasons against their views. Referees should require papers to include a discussion of such reasons, talk show hosts and journalists should elicit them, and so on. Anticipating the questions, the experts will have an incentive to consider such reasons before advancing their theses in the first place. Second, we should make sure to provide useful feedback. Experts who volunteer their opinion in matters of public policy should have access to information about the judgment they and their colleagues have made in the past, as well as scores for accuracy, calibration, and so on. The presence of proper outcome feedback should reduce the impact of confirmation and hindsight biases, and help experts learn from experience.

To assess degrees of calibration, and to allow proper outcome feedback, we should discourage vagueness and ambiguity in expert judgment. When forecasts are unclear, they cannot be effectively assessed. Experts, therefore, need to specify what meaning they attach to the relevant terms, what the time frame is, and what measures they use. They need to clearly specify a margin of error or a confidence interval, and a probability that the actual

outcome will fall in that range. Moreover, they should try to specify under what conditions the prediction is intended to hold, i.e. make the (usually implicit) provisos and ceteris paribus clauses explicit. Of course, the predictions should be on the public record. Clear predictions, like clear outcome feedback, help others to assess past judgments and experts to learn from experience.

Moreover, whenever possible, we should try to keep official scores. This can be done in a variety of ways, but would involve keeping track of at least a representative sample of predictions made by the relevant experts, and checking, in due time, whether the predictions came true. Thus, insofar as there are interpersonal differences, we would know which experts have a better track record, and therefore may be more reliable. Also, we would know by how much a particular expert has been wrong in the past, so that we can adjust current predictions accordingly. Moreover, the scores can serve as feedback to the experts, which properly used can help them calibrate their judgments. If you are a baseball player, you have to live with your batting average until the day that you die. If you are an economist, you can advice the government of Russia for years without being forced to critically examine your hit rate. In spite of the many practical problems associated with the proposal, there is no in principle reason why economics should be different from baseball in this regard. Official scores would provide incentives for economists to offer candid expressions of their confidence, and would have the added benefit of helping determine whether people like myself – who believe economists-as-experts tend to be overconfident – are wrong or overconfident ourselves.

6 CONCLUSIONS

In the above, I have argued that economists-as-experts are likely to fall prey to significant overconfidence. This is so, primarily, because they are subject to the same biases as every one else. However, economist-experts also work under conditions that invite overconfidence, viz. where judgment tasks are difficult, outcome feedback hard to come by, and effective institutional constraints absent. There are, of course, a number of reasons why the argument presented here may be wrong. The psychological research may turn out to be flawed, the conditions under which economists operate may in fact be very different from what I have imagined, and so on. Nevertheless, I hope to have offered enough evidence to the effect that the proposed interventions may be worth implementing. In my view, the interventions, encouraging clear forecasts and unambiguous feedback, may help us assess the exact degree of overconfidence among economists-as-experts, offer a humbling experience to overly confident experts, help us make better decisions in matters of public policy, identify issues concerning values in public decision making, and preserve or restore trust in social scientists as experts.

In drawing these conclusions, I make no assertions about appropriate goals of public policy. In particular, I am not arguing against market-based reform in Russia or elsewhere. My point is that given a set of goals, such as implementing a functioning democracy and free markets in Russia, overconfidence may inhibit our ability to choose the best means to that end, occasionally to great detriment. Similarly, I do not aspire to present an argument against shock therapy. What I do want to claim is that economists like Åslund may fall prey to overconfidence, i.e. that the accuracy of his judgments about the effects of shock therapy may fail to match his confidence in them. Finally, the argument

does not entail that we should avoid relying on economists or economics in public decision making. To the contrary, I have assumed throughout that we are on the whole better off relying on serious economic analysis in public decision making. My point is that to make the best of the situation, we need to be aware of the limitations of expert advice, and try to anticipate diverse negative consequences.

7 NOTES

[1] Although I use the case of Åslund as an illustration, my case does not hinge on his being overconfident. The main argument in favor of my thesis is based on evidence from the psychology of judgment and decision making, along with some general facts e.g. about the nature of the judgment task facing the economist-as-expert.

[2] In some contexts, the term 'expert' is defined so as to imply that the person in question is calibrated. Under this definition, my title should read 'Economists as Purported Experts.' However, I will use 'P is an expert in domain D' to mean 'P professes to be exceptionally knowledgeable in domain D'. Thus, I allow for the possibility that an expert may not be very knowledgeable at all.

[3] For useful overviews, see Lichtenstein et al. (1982), Yates (1990: ch. 4), Griffin and Tversky (1992), and Plous (1993: ch. 19).

[4] In many of these studies, unlike the ones discussed above, confidence is assessed by asking subjects how their performance compares to that of other people. As Don A. Moore and Daylian M. Cain (manuscript) have shown, the relative nature of judgments in such tasks lead to additional complications that sometimes lead subjects to exhibit underconfidence. Note that this research does not undermine my thesis, since the judgments in which I am interested do not typically have this relative nature.

[5] See Gigerenzer et al. (1991), Dawes and Mulford (1996), Griffin and Tversky (1992), and Brenner et al. (1996) for a fuller discussion of the criticism of overconfidence research.

[6] All translations are my own.

[7] Hence, my argument does not presuppose that economists deliberately exaggerate their degree of confidence. While they may do so, the research reported in the previous section indicates that overconfidence stems in large part from cognitive factors, which affect also the most honest, forthright, and well intentioned.

[8] Unless otherwise noted, all statistics come from the World Development Indicators Online (World Bank 2004).

[9] Åslund et al. (1996) do attempt to define more clearly what they mean by 'radical economic reform.' In order to attenuate the effects of the confirmation and hindsight biases, however, the definition needs to be provided at the time of the prediction.

[10] There is no reason to think, as far as I can tell, to think that these experts deliberately exaggerated their confidence.

[11] It is unclear on what basis Åslund postulates 'existential shock' as the specific cause of Russians' early deaths.

[12] See chapter 13 of Luce and Raiffa's *Games and Decisions* (1957) for a classic discussion of principles of decision making under uncertainty.

[13] I thank the editor and referees of this *Journal* for pushing this point.

8 REFERENCES

Ahlers, David and Lakonishok, Josef (1983) 'A Study of Economists' Forecasts,' Management Science 29: 1113-25.

Arkes, Hal. R., Christensen, Caryn, Lai, Cheryl, and Blumer, Catherine (1987) 'Two Methods of Reducing Overconfidence,' Organizational Behavior and Human Decision Processes 39: 133-44.

Åslund, Anders (1992a) 'Eastern Europe: No alternative to shock therapy,' Financial Times, 6 March, p. 19.

Åslund, Anders (1992b) 'Systemskiftet måste gå snabbare!,' Dagens Nyheter, 30 May, p. A4.

Åslund, Anders (1993a) 'Kapitalismens triumf,' Dagens Nyheter, 22 January, p. A4.

Åslund, Anders (1993b) 'The Gradual Nature of Economic Change in Russia,' in Anders Åslund and Richard Layard (eds) Changing the Economic System in Russia, New York: St. Martin's Press, pp. 19-38.

Åslund, Anders (1994) 'Russia's Success Story,' Foreign Affairs 73: 58-71.

Åslund, Anders (1995) How Russia Became a Market Economy (Washington: Brookings Institution).

Åslund, Anders (1996) 'Rysk chockterapi lyckades,' Dagens Nyheter, 28 July, p. A4.

Åslund, Anders (1997a) 'Svensk modell fara för Ryssland,' Dagens Nyheter, 11 March, p. A4.

Åslund, Anders (1997b) 'Skär bort halva offentliga sektorn,' Dagens Nyheter, 28 October, p. A4.

Åslund, Anders (1999a) 'Why Has Russia's Economic Transformation Been So Arduous?,' Annual Bank Conference on Development Economics, 28-30 April, Washington D.C.: World Bank.

Åslund, Anders (1999b) 'Russia's Collapse,' Foreign Affairs 78: 64-77.

Åslund, Anders (2000a) 'The Bear Turns Bullish,' World Link, July/August, p. 49-54.

Åslund, Anders (2000b) 'Go Long On Russia,' The International Economy 14: 38-9.

Åslund, Anders (2001a) 'We Did Too Little,' Moscow Times, 5 February, p. 10.

Åslund, Anders (2001b) 'The Myth of Output Collapse after Communism,' Carnegie Working Paper, Carnegie Endowment for International Peace.

Åslund, Anders (2002) Building Capitalism: The transformation of the former Soviet bloc, Cambridge: Cambridge University Press.

Åslund, Anders, Boone, Peter, and Johnson, Simon (1996) 'How To Stabilize: Lessons from post-communist countries,' Brookings Papers on Economic Activity No. 1: 217-91.

Bhagwati, Jagdish (1999) 'US Known as Nation that Believes in Law of the Jungle,' Financial Times, 9 March, p. 16.

Baumann, Andrea O., Deber, Raisa B., and Thompson, Gail G. (1991) 'Overconfidence among physicians and nurses: The "micro-certainty, macro-uncertainty" phenomenon,' Social Science and Medicine 32: 167-74.

Blix, Mårten, Wadefjord, Joachim, Wienecke, Ulrika, and Ådahl, Martin (2001) 'How Good is the Forecasting Performance of Major Institutions,' Sveriges Riksbank Economic Review No. 3: 38-68.

Bohlen, Celestine (1992) 'At Last, Russia Wobbles Into the World of Real Money,' New York Times, 5 January, section 4, p. 3.

Brenner, Lyle A., Koehler, Derek J., Liberman, Varda, and Tversky, Amos (1996) 'Overconfidence in Probability and Frequency Judgments: A critical examination,' Organizational Behavior and Decision Processes 65: 212-19.

Cain, Daylian M., Loewenstein, George and Moore, Don A. (2005) 'The Dirt on Coming Clean: Perverse Effects of Disclosing Conflicts of Interest,' Journal of Legal Studies 34: 1-25.

Christensen-Szalanski, Jay J. J. and Bushyhead, James B. (1981) 'Physicians' Use of Probabilistic Information in a Real Clinical Setting,' Journal of Experimental Psychology: Human perception and performance 7: 928-35.

Crawford, Vincent P. and Sobel, Joel (1982) 'Strategic Information Transmission,' Econometrica 50: 1431-1451.

Dawes, Robyn M. and Mulford, Matthew (1996) 'The False Consensus Effect and Overconfidence: Flaws in Judgment or Flaws in How We Study Judgment,' Organizational Behavior and Decision Processes 65: 201-11.

De Bondt, Werner F. M. (1991) 'What Do Economists Know About the Stock Market?,' Journal of Portfolio Management 17: 84-91.

Levi, Isaac (1980) The Enterprise of Knowledge: An essay on knowledge, credal probability, and chance, Cambridge: MIT Press.

Fischhoff, Baruch (1975) 'Hindsight ≠ Foresight: The effect of outcome knowledge on judgment under uncertainty,' Journal of Experimental Psychology: Human perception and performance 1: 288-99.

Fischhoff, Baruch (1980) 'For Those Condemned to Study the Past: Reflections on historical judgment,' in Richard Shweder (ed.) New Directions for Methodology of Social and Behavioral Sciences: Fallible judgment in behavioral research, Vol. 4, San Francisco: Jossey-Bass, pp. 79-93.

Fischhoff, Baruch (1982) 'Debiasing,' in Kahneman et al. (1982), pp. 422-444.

Fischhoff, Baruch (2001) 'Learning from Experience: Coping with hindsight bias and ambiguity,' in J. Scott Armstrong (ed.) Principles of Forecasting: A handbook for researchers and practitioners, Boston: Kluwer, pp. 543-54.

Fischhoff, Baruch and Beyth, Ruth (1975) '"I Knew It Would Happen": Remembered probabilities of once-future things,' Organizational Behavior and Human Decision Processes 13: 1-16.

Fischhoff, Baruch, Slovic, Paul, and Lichtenstein, Sarah (1977) 'Knowing with Certainty: The appropriateness of extreme confidence,' Journal of Experimental Psychology: Human perception and performance 3: 552-64.

Gigerenzer, Gerd (1991) 'How To Make Cognitive Illusions Disappear: Beyond "heuristics and biases,"' in Wolfgang Stroebe and Miles Hewstone (eds) European Review of Social Psychology, Vol. 2, New York: Wiley and Sons, pp. 83-115.

Gigerenzer, Gerd, Hoffrage, Ulrich, and Kleinbölting, Heinz (1991) 'Probabilistic Mental Models: A Brunswikian Theory of Confidence,' Psychological Review 98: 506-28.

Gordon, Danielle A. and Kammen, Daniel M. (1996) 'Uncertainty and Overconfidence in Time Series Forecasts: Application to the Standard and Poor's 500 Stock Index,' Applied Financial Economics 6: 189-98.

Griffin, Dale and Tversky, Amos (1992) 'The Weighing of Evidence and the Determinants of Confidence,' Cognitive Psychology 24: 411-35.

Hausman, Daniel and McPherson, Michael (1997) 'Beware of Economists Bearing Advice,' Policy Options 18: 16-19.

Hayek, Friedrich A. (1973) Law Legislation and Liberty, Vol. I, Chicago: University of Chicago Press.

Henrion, Max and Fischhoff, Baruch (1986) 'Assessing Uncertainty in Physical Constants,' American Journal of Physics 54: 791-7.

Johnson, Clifford G., Levenkron, Jeffrey C., Suchman, Anthony L., and Manchester, Ralph (1988) 'Does Physician Uncertainty Affect Patient Satisfaction?,' Journal of General Internal Medicine 3: 144-9.

Kahneman, Daniel, Slovic, Paul and Tversky Amos (eds) (1982) Judgment under Uncertainty: Heuristics and biases, Cambridge: Cambridge University Press.

Keren, Gideon (1987) 'Facing Uncertainty in the Game of Bridge: A calibration study,' Organizational Behavior and Human Decision Processes 39: 98-114.

Khanna, Vikram (1998) 'Premature Capital Openness Broke Asia,' Business Times, 31 August, p. 10.

Koriat, Asher, Lichtenstein, Sarah, and Fischhoff, Baruch (1980) 'Reasons for Confidence,' Journal of Experimental Psychology: Human learning and memory 6: 107-18.

Kruger, Justin and Dunning, David (1999) 'Unskilled and Unaware of It: Difficulties in recognizing one's own incompetence lead to inflated self-assessments,' Journal of Personality and Social Psychology 77: 1121-34.

Kyle, Albert S. and Wang, Albert (1997) 'Speculation Duopoly with Agreement to Disagree: Can overconfidence survive the market test,' Journal of Finance 52: 2073-90.

Lichtenstein, Sarah and Fischhoff, Baruch (1977) 'Do Those Who Know More Also Know More about How Much They Know?,' Organizational Behavior and Human Performance 20: 159-83.

Lichtenstein, Sarah and Fischhoff, Baruch (1980) 'Training for Calibration,' Organizational Behavior and Human Performance 26: 149-71.

Lichtenstein, Sarah, Fischhoff, Baruch, and Phillips, Lawrence D. (1982) 'Calibration of probabilities: The state of the art to 1980,' in Kahneman et al. (1982), pp. 306-34.

Luce, R. Duncan and Howard Raiffa (1957) *Games and Decisions: Introduction and critical survey* (New York: Wiley).

McCloskey, Donald (1992) 'If You're So Smart…,' in J. Lee Auspitz, Wojciech W. Gasparski, Marek K. Mlicki, and Klemens Szaniawski (eds) Praxiologies and The Philosophy of Economics, London: Transaction.

Moore, Don. A. and Cain, Daylian M. (manuscript) 'Overconfidence and Underconfidence,' Carnegie Mellon Tepper Working Paper No. 2003-E76 (May 2005).

Murphy, Allan H. and Winkler, Robert L. (1984) 'Probability Forecasting in Meteorology,' Journal of the American Statistical Association 79: 489-500.

Oskamp, Stuart (1982) 'Overconfidence in Case-Study Judgments,' in Kahneman et al. (1982), pp. 287-93.

Plous, Scott (1993) The Psychology of Judgment and Decision Making, Philadelphia: Temple University Press.

Rabin, Matthew (1998) 'Psychology and Economics,' Journal of Economic Literature 36: 11-46.

Sieber, Joan E. (1974) 'Effects of Decision Importance on Ability to Generate Warranted Subjective Uncertainty,' Journal of Personality and Social Psychology 30: 688-94.

Svenson, Ola (1981) 'Are We All Less Risky and More Skillful than Our Fellow Drivers?,' Acta Psychologica 47: 143-8.

Stiglitz, Joseph (2000) 'The Insider: What I learned at the world economic crisis,' New Republic, 17 & 24 April, pp. 56-60.

Staël von Holstein, Carl-Axel S. (1972) 'Probabilistic Forecasting: An experiment related to the stock market,' Organizational Behavior and Human Decision Processes 8: 139-58.

Tetlock, Philip E. (1999) 'Theory-Driven Reasoning about Plausible Pasts and Probable Futures in World Politics: Are we prisoners of our preconceptions?,' American Journal of Political Science 43: 335-66.

Wall Street Journal (2000) 'Stiglitz Misses Elephant,' 12 April, p. A26.

Wedel, Janine R. (1998) Collision and Collusion: The strange case of Western aid to Eastern Europe 1989-1998 (New York: St. Martin's Press).

World Bank (2004) World Development Indicators Online (Washington D.C.).

Yates, J. Frank (1990) Judgment and Decision Making (Englewood Cliffs: Prentice Hall).

Yates, J. Frank, McDaniel, Linda S., and Brown, Eric S. (1991) 'Probabilistic Forecasts of Stock Prizes and Earnings: The hazards of nascent expertise,' Organizational Behavior and Human Decision Processes 49: 60-79.

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*** FIGURE ***

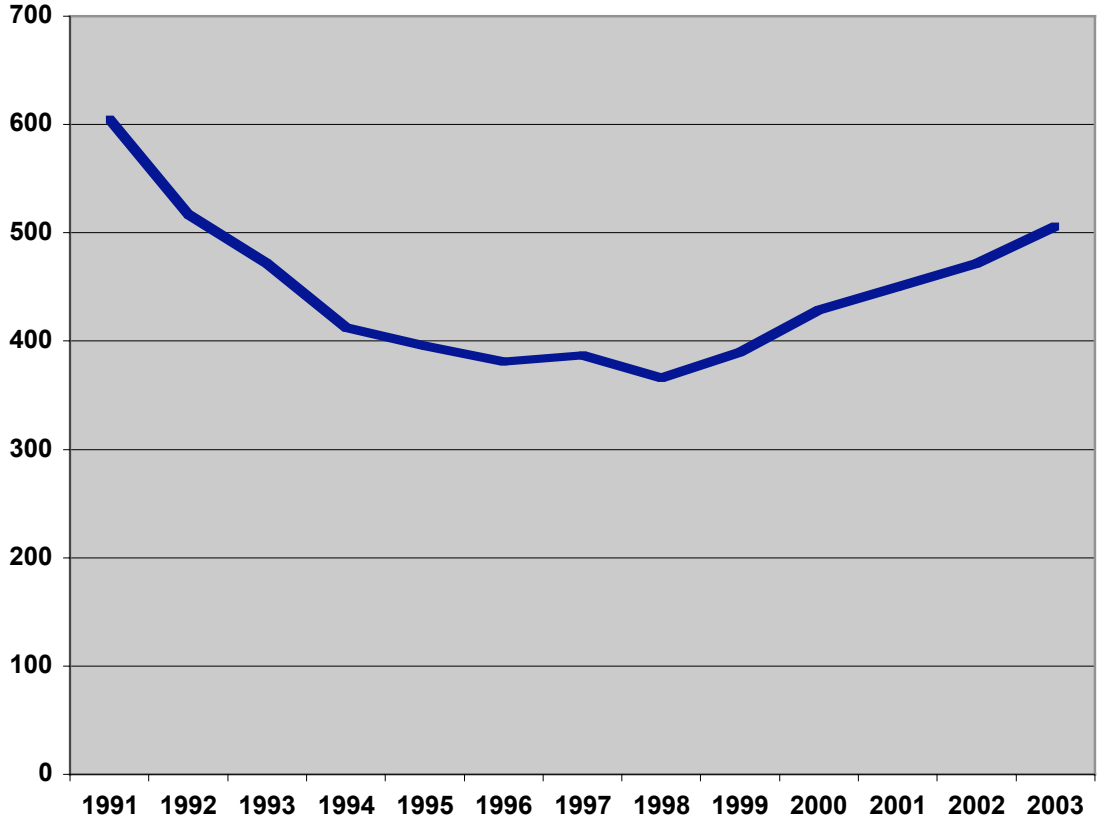

Figure. Russian GDP in constant 1995 US billion dollars, 1991-2003.

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