Is the Public Incompetent? Compared to Whom? About What?

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1. Popular Ignorance v. Elite Competency?

John Stuart Mill had few illusions about public opinion — it was the opinion of a mass, "that is to say, collective mediocrity" (1977a [1859]: 268). Understandably, for Mill the great danger inherent in representative government is that it may be controlled by a "low grade of intelligence" (1977b [1861]: 448). A century and a half of inquiry has apparently confirmed Mill's worry. Philip E. Converse's landmark study of mass publics (2007a [1964]) found that much of the public did not have meaningful political beliefs, and the beliefs they did hold tend to change capriciously. Converse's research contrasted the coherent, unified, ideological, beliefs systems of the political elite with amorphous and changing views of the mass public. Subsequent work has confirmed what we might call the mass ignorance hypothesis. According to public choice theory such ignorance is rational. Roughly when a politically rational agent calculates the expected instrumental gains from casting her ballot for her favored party she must compare the expected value of her vote (the extra utility she will receive if her favored party wins times the probability that she will cast a decisive vote) to the cost of voting. It is only rational to vote if the expected gains exceed the costs. But because the probability of casting the decisive vote is so low (often approaching zero), the expected instrumental gains are miniscule, and are usually outweighed by the small costs involved in voting (Gaus, 2008: 184-91). Maybe it can make sense to cast a ballot, but given the very low expected utility of voting, it will hardly ever make sense to incur significant information costs involved in finding out about politics in general, or the specific candidates in particular (see e.g., Somin, 2007; Pincione and Tesón, 2006). So a rational voter should be content to rely on whatever background knowledge she can pick up for free: it is

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rational for her to be ignorant of politics. Recently Bryan Caplan (2007a) has redoubled this line of criticism, arguing that democratic publics are not simply rationally ignorant, but (at least on economic issues) out-and-out irrational: they have plenty of beliefs, but they are systematically erroneous ones. He advances a *mass irrationality* hypothesis to replace the mass ignorance hypothesis. In contrast to Converse, Caplan thinks that it is the devotion to ideology (over truth) that is at the heart of the public's irrationality: (among other things) emotional devotion to false beliefs lead them to endorse bad policies.

Defenders of democracy typically advance three responses. *First*, following Mill's lead — for Mill was not simply a critic, but also a defender of democracy — they may argue that there are features of democracy that tend, over the long run, to improve the public's political competency — its store of politically correct information. There may be some such tendency, but it is hard to have as much faith in 2008 as Mill had in 1861 (see Converse, 2007b: 311-14). *Second* — and in recent years most importantly — democrats have argued that the aggregation of judgments can produce reliable collective choices even when most voters are ill-informed. "[E]ven if individual opinions … are ill-informed, shallow, and fluctuating, collective opinion can be real, highly stable, and …. even wise" (Page and Shapiro, 1992: 17). If the majority's opinions on an issue are ill-informed, but randomly so, they will tend to cancel each other out, leaving the informed section of the public to have decisive influence over the final electoral outcome. In a similar vein, according to the Condorect jury theorem, if the judgment of the average voter has even a tiny bit better than random chance of being correct, the majority opinion is almost certainly correct (see Gaus, 2003: 158ff; List and Goodin, 2001). *Lastly*, again following Mill, democrats may admit that mass democracy does suffer from radical ignorance (or worse) on the part of voters, but argue that the overall workings of representative institutions temper mass ignorance with elite expertise. Famously, Mill advocated a system of plural votes according to which those who are more educated, and presumably have a better grasp of public affairs, receive more votes (1977b

[1861]: ch. 8; 1977c [1859]).[1] Rather optimistically, Mill believed that most people would acknowledge that the political judgment of some is superior to their own, and so would agree that those with superior judgment should have more say in the management of joint affairs. "No one but a fool, and only a fool of a peculiar description, feels offended by the acknowledgment that there are others whose opinion…is entitled to greater consideration than his" (1977b: 474).

This brings us to our question: are the opinions of those elite "others" really entitled to greater consideration? In the long history of thinking about this problem, it has been generally accepted in that elites have genuine expertise of the sort that makes them more likely to propose and endorse good policies. From Mill onwards the problem has been framed in terms of whether the public measures up to the acknowledged competency of the experts or the political elite. The attention is always on the poor public — whether it is as ignorant as much research shows, and if it is, whether democracy can somehow still yield good outputs. If only the collective mediocrity could become as wise as elites. But all this assumes that (i) we know what sort of knowledge is relevant for "good outputs" and (ii) the experts or the elite have a lot more of it. We cannot even begin to provide an answer to (ii) unless we know (i). We need to know what it is important to have before we determine whether elites are to be deferred to because they have more of it. Say, for example, that we accept that members of the public do not know the names or parties of their representatives (I must admit, I don't know mine). Suppose, though, a good policy is one that tends to conform to the judgments of justice of the voters. We would need to know that failure to recall names or the party of one's representative means that voters are unable to render competent electoral judgments about, say, the justice of a war. Perhaps one's vote cannot render a competent judgment unless one knows names and parties, but perhaps it can. We thus require an analysis that ties some conception of a "good electoral outcome" to a type of important political knowledge. Once we know what this important knowledge is, we then need

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[1] At one point Mill provides the following scale of voting power: Unskilled laborers – 1 vote; Skilled laborers – 2 votes; Foremen – 3 votes; Farmers, manufacturers and traders – 3 or 4 Votes; Professionals – 5 or 6 votes; University graduates – at least 5 or 6 votes. (Mill, 1977c [1859]: 324–5).

to show that the public has a lot less of it than some elite. Only then have we provided the foundations for a critique of the ignorant democratic public.

2. Economic Policy and Economic Experts

Let us begin with a candidate for important political knowledge that would seem especially hospitable to the thesis that, compared to elites, the public is grossly incompetent, viz. that intelligent economic policy must be based on sound economic knowledge. This is the ground on which Caplan bases his case for voter irrationality. “Economic policy is the primary activity of the modern state, making voter beliefs about economics among the most — if not *the* most — politically relevant beliefs” (Caplan, 2007a: 10, emphasis in original). Voters, he argues, have systematically wrong-headed views about economics that depart from the knowledge of economic science shared by economists. Because economists agree on the truth of economics — while the ignorant public dissents — economists are in a vastly better position to chart successful economic policy. I am skeptical. I shall argue (1) the evidence suggests that Caplan significantly exaggerates the extent to which economics endorses a large set of stable truths and (2) it is uncertain that whatever truths economists possess give them a superior insight into successful policy, because there are deep doubts whether economics is a predictive science in the sense required for effective policy.

Economic Consensus? On the Truth?

Caplan’s main empirical chapter is devoted to showing that “the public suffers from systematically biased beliefs about economics” (2007a: 50). He writes:

> There are numerous surveys of the economic beliefs of both economists and the general public. … Take the case of free trade versus protection. A long-running survey initiated by J.R. Kearl and coauthors has repeatedly asked economists whether they agree that “tariffs and import quotas usually reduce the general welfare of society.” In 2000, 72.5% mainly

> agreed, and an additional 20.1% agreed with provisos; only 6% mainly disagreed. The breakdowns for 1990 and the late 1970s are even more lopsided in favor of free-trade" (Caplan, 2007a: 50-51).[2]

Caplan then compares this consensus among economists to the views of the public, which are much more skeptical of free trade. Thus, we are to conclude that the public is "biased." Drawing extensively on the Survey of Americans and Economists on the Economy, Caplan shows that the average economist and the average member of the public disagree on a host of economic issues.

Now as Jeffrey Friedman well notes, showing that someone disagrees with an economist about free trade is not quite the same thing as showing that she is wrong or biased (Friedman, 2008). That rather begs the question: to most of us, that we disagree with the economics profession and that we are wrong are rather different issues (if I didn't think that I'd never disagree with economists). Throughout Caplan's analysis, however, the main proof that economists have access to the truth seems to be that they disagree with public. To be sure, Caplan insists that he is not claiming that "the average belief of the economics profession is an infallible oracle" (Caplan, 2007a: 56); and indeed even he sometimes disagrees with the average belief of his colleagues (2007a: 220, n. 34). Which is just as well, since evidence suggests that public choice economists are in some ways outliers in the economics profession. Indeed, members of the American Economics Association (who are not public choice economists) are more likely to accept than reject the proposition that "simple majority rule is the preferred way to make group decisions," while public choice economists are against it by a ratio of 3 to 1 (Whaples and Heckelman, 2005: 69).

Why do lay and expert opinion in economics diverge? Caplan effectively dismisses some plausible rival hypotheses: he shows that the "lay-expert" gap is not the result of self-servingness by, or ideological blinkers of, economists. However, this does not go very far towards establishing his favored hypothesis: that economists possess the truth and the public are

[2] Note that this implies that economists are disagreeing more about free trade.

irrational and wrong. A discipline can go wrong in other ways than through such out-and-out bias. Economists, for example, are generally committed to rational choice analysis (Friedman, 2008); indeed, within public choice theory itself, economists and political scientists significantly disagree on the status of the rationality assumption and the role of self-interested behavior, and this seems to account for the much greater wariness of government by economists than political scientists (Whaples and Heckelman, 2005: 77). Economists also have exceptionally thin value theories (they often are at a loss when people talk of morality, justice or fairness, and not surprisingly none of these terms make it into Caplan's index — in a book on democratic choices in economic policy).[3] Economists typically espouse (as almost self-evidentially correct) consequentialist and aggregationist views of social welfare that set them apart from much of the population (see section 3). Indeed, Caplan's own research shows that 'thinking like a man' is related to thinking like an economist (154-56).

Even leaving aside this rather inferential large gap between group consensus and truth, group consensus is a more hit and miss affair among economists than Caplan indicates. For the most part he reports differences of group central tendencies between economists and "lay" people, but this tells us little about the degree of consensus within economics. To be sure, the Kearl et. al. studies that Caplan cited above do investigate the degree of consensus within economics, but their results are rather more ambiguous than Caplan suggests. In their 1992 study, Richard M. Alston, Kearl, and Michael B. Vaughan found that the decade in which an economist was trained affects his or her views, suggesting that socialization during graduate school may have an important influence on economists' views throughout their career.[4] At the same time, they found that there had been "significant change in economists' views on 10 of 21 common propositions surveyed in 1976 and 1990" (1992: 209). In a 1996 study (Whaples, 1996) concerning consensus among labor economists, we again find a mixed picture. Labor economist

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[3] Caplan does have a short discussion of "altruism" on pp. 151ff.

[4] This effect was not observed in economists working at the top ten research universities.

largely agree, for instance, that minimum wage laws decrease employment (more on that anon); that a worker's pay usually approximates his marginal product; that increasing wages of most adult males will not influence their labor supply while the supply of female labor will respond to higher wages; that leisure is a normal good for most workers; that the overall gains to American society from immigration exceed the losses; and that legislation to ensure equal pay for equal worth will not promote efficiency. On the other hand, labor economists disagree on a host of issues, including the economic impact of unions, the amount of discrimination in the labor market, whether reducing the length of the work week will reduce unemployment, whether, other things equal, those in unpleasant jobs receive higher wages, the role of higher education on the labor market, the trend of average real wages, whether there is a natural rate of unemployment.

More disturbing for those who claim that, as a matter of fact, the economics profession displays great consensus on basic law-like propositions is the phenomena of emerging contrary results. Robert S. Goldfarb's study of the empirical literature in economics reveals a pattern according to which "first, evidence accumulates to support an empirical result. As time passes, however, contrary results emerge that challenge that result," leading to a regular overturning of apparently established empirical findings" (1997: 220). Although better data and more advanced mathematical techniques are a factor in over half the changes, Goldfarb concludes that the instability of empirical economic findings represents "a serious problem for the conscientious economist trying to make warranted inferences from empirical literature in economics. If seemingly established results often provoke the emergence of contradictory findings, the dependability of inferences based on existing literature is weakened" (Goldfarb, 1997: 237). Goldfarb concludes that while some theoretical propositions are "done in" by the data, for a significant group of economists, "if the data do not fit the theory, too bad for the data" (1997: 238). (On the lack on empirical testing of standard economic models, see Backhouse, 1997: ch. 14.)

Specific Prediction and Policy

It is widely agreed that "[e]conomists need to be able to predict . . . because predictions are necessary if they are to fulfill the role of providing policy advice" (Backhouse, 1997: 113). Now it is important to distinguish two types of predictive laws. Consider first:

> *Ia*. *Ceteris paribus*, raising the price of labor by X will decrease employment by quantity Q;
>
> *Ib*. Typically, raising the price of labor by X will decrease employment by Q;
>
> *Ic*. In an ideally competitive market, where utility functions satisfy standard conditions, raising the price of labor by X will decease employment by Q.

Ia-Ic are different ways of articulating an economic law relating the cost of labor to the demand for labor (all follow from the more general idea of downward sloping demand curves). There are interesting and important issues concerning which is the best way to conceive of economic laws, or whether some other way is superior.[5] For our purposes, what is important is that none of the above provides the basis for a high level of confidence in a prediction along the lines of:

> II. Under the actual conditions that obtain in place A at time t, policy P, raising the price of labor by X, will decrease employment by Q.

Claim II predicts the effects of a specific policy, and so must factor in all the relevant variables. There are two grave barriers to successful predictions of type II. First, we seldom if ever know all the relevant variables. This point, of course, is fundamental to Hayek's analysis of the market, his case against planning, and his argument against the misuse of economic models. No central intelligence — and that includes the economist qua advisor — has access to all the local and personal knowledge that is relevant to the specific proposed policy (Hayek, 1945). Think about *Ia*: other things equal, an increase in the minimum wage will decrease employment. In specific circumstances other things are not apt to be equal. Indeed, although we have seen that labor economists (and economists in general) widely agree that increases in the minimum wage will

[5] Formulation 1c can, I think, be cashed out in terms of "covering law analysis"; the others cannot.

reduce employment, the empirical data on the effects of specific laws in specific circumstances is murky indeed. Famously, David Card and Alan B. Kruger (1994) found no evidence that a rise in minimum wage reduced unemployment in the fast food industry. In their book *Myth and Measurement* (1995), Card and Kruger argued — in my view quite correctly — that economists have relied too much on general economic theory and have failed to do enough empirical work. Some recent investigators have also failed to find evidence of employment decrease (Lemos, 2007, Dube, Naidu, and Reich, 2007) or decrease in incidence of holding second jobs (Robinson and Wadsworth, 2007). Under some conditions, the data suggests that minimum wage laws may actually help "resorb excess labor supply" (Dessing, 2004). On the other hand, a number of studies have found negative effects on employment (Partridge and Partridge, 1999; Campolieti, Gunderson, and Riddell 2006; Singell, and Terborg, 2007; Gindlinga and Terrell 2007; Wessels, 2007). However, some data suggests that protectionist policies can offset decrease in employment (Lal Agarwal, et al. 2007). Others have found rather modest negative effects on employment (Burkhauser, Couch and Wittenburg, 2000; O'Neill, Nolan and Williams, 2006). There is also evidence that the effects differ by groups (Yuen 2003) with negative effects on teen-age unemployment often found (Neumark, and Wascher, 2004; Wessels, 2005), though some studies find no evidence of adverse effects on youth employment immediately following legislation, and weak evidence of longer-term employment loss (Hyslop and Stillman 2007). However, an *increase* in hours worked by teenagers after minimum wage legislation was also found (Hyslop and Stillman 2007). In any event, the nature of the effects on different groups is quite complicated (Zavodny, 2000). As several economists have concluded, we simply do not know a great deal about the employment effects of raising the minimum wage by a certain amount (Keenan, 1995; Watson, 2004).[6] But if that is so economic expert advice is apt to be of little help in specific policy contexts.

The second barrier concerns the complex interaction of different variables. Even if we should

[6] See the references in these two essays for further conflicting studies of minimum wage legislation.

somehow know all the relevant factors affecting the relation between the proposed minimum wage law and levels of employment, it is usually impossible to predict how they will interact, and what other, unknown consequences, a policy may have. This is the problem of complexity in economics, which I have explored elsewhere (Gaus, 2007). Hayek was one of the first to appreciate that the interaction of economic variables forms complex systems whose behavior can only be predicted within broad ranges (Gaus, 2006). The complexity of economic systems is, I think, an insuperable barrier to high-confidence predictions of Type II that seek to encompass the specific global or systematic impact of a proposed policy. Donald Saari, a mathematician, argues that the level of complexity of economic systems dwarfs that of the systems studied by most natural scientists:

> [W]hat we do know indicates that even the simple models from introductory courses in economics can exhibit dynamical behavior far more complex than anything found in classical physics or biology. In fact, all kinds of complicated dynamics (e.g., involving topological entropy, strange attractors, and even conditions yet to be found) already arise in elementary models that only describe how people exchange goods (a pure exchange model).
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> Instead of being an anomaly, the mathematical source of this complexity is so common to the social sciences that I suspect it highlights a general problem plaguing these areas. If true, this assertion explains why it is so difficult to achieve progress in the social sciences while underscoring the need for new mathematical tools (Sarri, 1995: 222).

Sarri shows that the hidden complexity of social science derives from aggregation out of the unlimited variety of preferences, "preferences that define a sufficiently large dimensional domain that, when aggregated, can generate all imaginable forms of pathological behavior" (Sarri, 1995: 229).

But We Do it, Don't We?

In response to this line of analysis it is often argued that it simply cannot be correct since we *do* have good economic models, which *do* allow us to predict the overall effects of a specific policy. In fact, I do not think we do. Few models seek to predict overall systemic effects of our policies; attempts to do so fail because they run smack into Hayek's knowledge and complexity problems. Samuel Brittan (2007) has recently argued that overall macro models are of little use and should be jettisoned (though he admits that people find it very hard to accept this conclusion). There is certainly good evidence that financial models have no real predictive value (Taleb, 2007). A recent study of predictions by the *Wall Street Journal's* panel of economists found that, in predicting the Treasury bill rate their performance was indistinguishable from a random walk; in predicting Treasury bond rates and exchange rates they were significantly *worse* than random (Mitchell and Pearce, 2007). (Hayek is reported to have quipped that he knew of few people who made money by acting on economic forecasts, but many who had made money by selling them; Brittan, 2007). One of the constants of policy analysis is that "the real benefits usually are not the ones we expected, and the real perils are not the ones we feared" (Tanner, 1996: 272).

Our predictive ability increases if we do not concern ourselves with overall consequences, but aim only at predicting a few variables in the short-run. As we have seen, though some empirical studies of the effects of minimum wages laws have shown no negative effect on employment, most agree that in actual conditions there is some negative impact on employment. However — and this is an important point — although the direction of effect is usually as we would predict from the general law of downward sloping demand curves, the strength of the observed effect varies a great deal, and is very often far less than policy makers expected. We find this general phenomenon of inability to predict magnitudes of effect with matters as simple as predicting lives saved from seat belts laws (Gaus, 1997: 19ff). In the case of a complicated policy such as the North American Free Agreement, the complex models that policy makers employed to predict the effects agreed in the general direction but generated marked differences in magnitude of effects (U.S. International Trade Commission, 1992). Successful policy, however, requires that policy makers have a good handle on the costs and benefits of proposals. Every policy has opportunity costs, transaction costs, and negative impacts on some citizens. Unless policy makers have a good way to predict the magnitude of the costs and benefits, they cannot have a sound basis for thinking the policy is desirable. Sometimes they can predict with a high level of confidence direction of effect of a few variables; seldom can they predict any more.

Yet we think we can predict well. The work of Amos Tversky and Daniel Kahneman indicate that people consistently ascribe high levels of probability to very faulty predictions. Indeed, they report that "subjects are most confident in predictions that are most likely to be off the mark." "[P]eople are prone to experience much confidence in highly fallible judgments, a phenomenon that might be called the illusion of validity" (Kahneman and Tversky, 1982: 66). Philip E. Tetlock (2005) studied the predictions of experts across a range of political and economic issues. For over a decade Tetlock studied the ability of political and economic experts to predict, among other things, economic performance (growth rates in GDP, inflation, unemployment rates) as well as political developments. Tetlock asked experts in history, political science, and economics to predict future events and the movement of key variables. Would key variables go up (in both the short term and the long term), go down, or stay the same? The good news is political and economic experts do better than quickly briefed undergraduates at predicting future events in their field of expertise. Unfortunately, that is about all the good news. Experts do not do significantly better that what Tetlock calls "dilettantes" — people who regularly read the *Economist* or the *New York Times* (hopefully they do not pay too much attention to the *Wall Street Journal's'* panel of economists). Tetlock distinguished two criteria of a good prediction: discrimination and calibration. Someone who always makes predictions that track the base rate (for example, "a 33% chance of a downward movement in an index") would score high on calibration but low on discrimination; someone gets the highest discrimination score when she assigns a probability of 1 to events that occur and 0 to those that do not (Tetlock, 2005: 47). On the discrimination measure the experts and dilettantes would beat a chimpanzee that made predictions by throwing a dart at a board on which the dart can land on “variable will go up,” “variable will go down,” or “variable will stay the same.” Unfortunately the chimpanzee beats the dilettantes and experts on the calibration index.

All this is about comparative performance. What about absolute performance? Experts are better, I have said, on the discrimination dimension — they make more precise, if less accurate, predictions than does the chimpanzee. How good are they? The better half of the expert group predicts a meager 18 percent of the variance, the less good group about 14 percent. An average of about 16 percent of the variance is accounted for by expert prediction. On the basis of these findings, Tetlock is forced to concede the crux of the skeptical hypothesis (which he relates to complexity theory): expert prediction and guesswork are essentially the same (Tetlock, 2005: 76ff).

Caplan — whom, it will be recalled argues for mass irrationality — has a rather different take on Tetlock’s results. In his review of Tetlock’s book in this journal, Caplan (2007b) stresses two points. *First*, Tetlock made the chimps look better and the experts worse by excluding “no brainer” questions, such as whether there will be a United States presidential election in 2008. Since all the experts would say there would be, and the chimp would guess randomly, the chimp would have done much worse. It does though, seem plausible to accept Tetlock’s suggestion that experts, if they earn their bread at all, earn their bread by giving us advice on non-obvious questions. *Second*, Caplan points out that the experts and the chimps do better than quickly briefed undergraduates (though, it should be pointed out, if Tetlock included no-brainer questions, it would have made the chimps look worse but the undergraduates better). So, Caplan insists, Tetlock’s study still shows that experts do better than the public. Indeed, Caplan’s main criticism is that, in his view, Tetlock presents his findings in such a way that “demagogues” (2007b: 83) can exploit them to discredit experts. But it is not merely a matter of presentation: Tetlock is right to concede (and it is a concession, since his aim is to figure out what makes for a better expert, not to debunk experts) that it is reasonable to be skeptical that experts are good predictors in their own fields. Caplan focuses on the experts' relative superiority to undergraduates, but it is experts' poor absolute levels of prediction that is really striking. And it is striking that dilettantes do just about as well as experts, and that having a PhD or years of experience in one's field is not a significant predictor of the level of one's predictive capacity (Tetlock, 2005: 69).

3. Where the Emotional Public Comes into Its Own

But We Must Do Something! And We Can

But surely, it is said, we (i.e., government) cannot stand by with our collective hands in our collective pockets in the face of serious problems: we must *do something*. Indeed, it can be pointed out, not to do something is still to do something, since it is to affirm the *status quo*. Thus, as has been said, "decisions must be based on incomplete empirical data, even conjecture" (Kaplow and Shavell, 2002: 458). We *have* to do something, even if on the basis of an "intelligent" guess. Can policy making by intelligent guesswork be justified?

Surprisingly, it can. Consider a simple case. Suppose we are deciding between two policies, P1 and P2. Suppose, as seems correct, that the elite (experts and dilettantes) predict somewhat better than the public at large (including undergraduates), so we put the matter in their hands. Say that they can predict along the lines of Table 1.

P1		P2	
*C*1	*C*2	*C*3	*C*4
$p = .1$	$p = .05$	$p = .1$	$p = .15$
$\mu = 100$	$\mu = -125$	$\mu = 140$	$\mu = -75$
$E\mu = 10$	$E\mu = -6.25$	$E\mu = 14$	$E\mu = -11.25$
3.75		2.75	

Table 1

In this case the elite knows that there is a .1 probability that P1 will have consequences *C*1, which would have +100 units of utility (so the expected utility of *C*1 = 10); there is a .05 P1 that it will have consequence $C2$, which would have a negative utility –125 (so we can say that the expected utility of $C1 = –6.25$). P2 has a .1 probability of $C3$ (which would have a utility of +140) and a .15 chance of $C2$, which would have a negative utility of 75. Note that we *cannot* say here that the expected utility of P1 is +3.75 and P2 is +2.75, since with P1 we have only accounted for 15% of the outcomes and for P2 25%. We only know a small fraction of the total costs and benefits of our choice, but these other utilities are entirely unpredictable. Call the latter the *large unknown residue*. So our elite's calculations include some small, predictable costs and benefits and the unknown, large residue. Now we can develop principles of rational choice in the face of such uncertainty. Suppose we say that the expected utility of the large unknown residue of P1 might be either (i) greater than P2 or (ii) less than P2. So here we might appeal to the principle of insufficient reason that explicitly directs us to treat (i) and (ii) as equally probable; they are mutually exclusive events, and we have no reason to assign different probabilities to them.[7] But if we treat (i) and (ii) as equally probable, then the expected utility of the unknown, large residue of P1 and P2 is a wash: it provides no grounds for deciding between them. But then it looks as if the only grounds for deciding between them are the known, even if fairly insignificant, local effects: P1 beats P2 on this score (3.75 to 2.75), so we finally have a ground for a rational choice. But note that it is very likely that one's choice will not maximize utility.

What is the Goal?

So long as our only reasons to choose are reasons of expediency — aiming to bring about an acknowledged good result — there is a plausible case for choosing one policy or the other even though we are well aware our chosen option will be only a small improvement on a random

[7] Note that we are applying the principle of insufficient reason over expected utilities. There are worries about this. It might be pointed out that since we know more of the variance in P2 (25% of the probability rather than 15%): we have more reason to think that the unknown residue of policy P1 will eventuate than the unknown residue of policy P2. It might be objected to the principle discussed in the text that it does not take this into account. We can develop alternative principles that would accommodate this concern: my aim here is just to show that we can devise principles for rational choice in these situations.

choice. This is important: if our only reasons to choose are reasons that aim at producing a certain result, then even if P1 has only a miniscule advantage in expediency, we have reason to choose it, though we have firm grounds to doubt that choosing it is likely to turn out better than opting for P2. So it would be rational for the people to be guided by the elite, even though it is pretty likely that the elite is wrong. This, I think, is the strongest case that can be made for the elite having a guiding role in the formation of public policy. I trust it is clear that it is not an overwhelming case.

But all this supposes that all our uncertainty is about the way to get to our common goal; about this the elite has some — not great — advantage. What we have entirely bracketed is the normative criteria for evaluating the policy. We have supposed that there is a "utility" that can be ascribed to each outcome. But a great amount of democratic decision-making is about the criteria that should be used to evaluate policy: fairness, efficiency, civic virtue, wealth maximization, individual responsibility, security (and security is itself a complex idea: the idea of "risk" turns out to have several components and different people weigh them differently; Slovic, 1991). Many economists seek to dismiss notions of fairness, or seek to depict a person's aim to secure fairness as a sort of psychic benefit or a way to feel good (Kaplow and Shavell, 2002: 17, 431). It is often assumed that once we have considered such benefits they can be aggregated via a social welfare function (Kaplow and Shavell, 2002: 27ff) or simply collapsed into something called "social welfare" (Caplan, 2007a: 152). But there is certainly no uncontroversial (or, I think, even plausible) way to aggregate these standards into a function to be maximized.

Now in contrast to predicting economic outcomes, citizens have a fairly good handle on their evaluative or normative standards. Factors that Caplan labels as "emotional" and "ideological loyalty" which "corrupts" our thinking (Caplan: 2007a: 15) can, I think, less tendentiously understood as moral and other normative commitments that inform our view of what constitutes an acceptable policy. Although we did not evolve to make complex probabilistic statements about the long-term effects of consequences in large societies (there is overwhelming evidence that people have a very hard time reasoning probabilistically), we did evolve to make judgments of fairness, rightness and wrongness (Joyce, 2007; Nichols, 2004). Of course much more empirical work needs to be done on the ways we apply moral rules and norms: we need to know what level of questions we are best at answering (about all of society? Interpersonal dealings?), and the extent to which we agree and disagree in the way we apply moral rules and norms. All we can say now, I think, is that current research supports the claim that, to a significant extent, we converge in our reasoning about what our social norms are and what they require (Bicchieri, 2006). There is also evidence that people — indeed even young children — are good at applying moral rules and distinguishing them from conventional rules (Cummins, 1996, 2002; Fiddick, 2006). Of course we disagree too; but much of democratic politics can be understood as ways to resolve this disagreement (Gaus, 1996: Part III).

So the apparent advantage of the experts over the *demos* begins to evaporate when we realize that, though the experts have some slight advantage in means-ends predictive reasoning, what they think the end should be is simply one view among many — and, as I have stressed, generally an outlier view based on a thin theory of value and morality. Economists often think that wealth-maximization is *the* obvious social goal; Caplan tells us that the "full price of ideological loyalty" "*is the material wealth you forego….*" (2007a: 17, emphasis in original). Of course utility functions are much more complex than that: wealth is one factor, but there are many things that people believe are worthy of pursuit, and there are many costs to ideological loyalty (again, the thinness of most economists' value theory becomes apparent). Only once we know the normative basis for can the policy expert earn his bread by telling us how to get there, but he has no special insight into what those normative standards should be.

Expediency v. Rules: Where the People Really Come Into Their Own

Many of our moral judgments do not depend on what experts do best (but not well): calculating the consequences of our policies. As I remarked above, one of the biases of economics is that it supposes that all evaluation is means-end related, and so all evaluation is a form of consequentialism. Indeed, so deep is this bias that Caplan sees those who have non-instrumental beliefs as irrational (Caplan, 2007a: 14ff).[8] But this is surely not so. Our beliefs that in cooperative endeavors fairness is important and that moral rules and social norms should not be violated are not held as instrumental to achieving some goal. Indeed, following rules and norms of fairness in this way is probably fundamental to social life and cannot be reduced to having a taste or preference for fair outcomes (Bicchieri, 2006: ch. 3). People care about the process through which outcomes come about as well as the outcome: indeed because outcomes are so difficult to predict, process considerations are often uppermost.

This brings us back to Hayek. A recurring theme in his work is the contrast between principles/rules and expediency, and the insistence that a free society requires that governments follow principles or rules rather than pursue apparently expedient social and economic policies that appear to make us better off (Hayek, 1973: 55). We have seen that outcome-oriented policy gives some slight weight to the elite's reasoning based on their superior predictive skills. But we now must add social norm moral rule-based reasoning. We might say that the total set of reasons (R_t) to opt for P1 depend on both the strength of one's reasons of expediency (R_e) and the strength of one's rule-based reasons (R_r). So, we can say $R_t = (w_i)R_e + (w_{1-i})R_r$, where w_i is a weight between 0 and 1 attached to reasons of expediency.[9] Now we can see that if rule-based reasons do not enter into the choice, (the weight for $R_e = 1$), then it is plausible that even the weakest reasons of expediency still determine R_t. As bad as they are, we have reason to follow the experts and their predictive skills. However, if, as we have seen, our reasons of expediency are mightily weak, and if there are relevant rule-based reasons or principles of fairness, then

[8] The claim that the basis for all justified beliefs is their instrumental value is, I think, ultimately incoherent. See Gaus, 2008: 7-12.

[9] For the idea of such complex decision value schemes (though not focused on rule-based reasoning) see Nozick (1993).

even rules that are weighted modestly will almost certainly determine R_t.

The public now comes back into the picture. As we have seen, the evidence suggests that normal reasoners are competent at applying moral rules and social norms. Moreover, there is not an identifiable set of moral experts, so on these matters there is no socially recognized claim to expertise, thus supporting political equality (Gaus, 1996: 184-186, 251ff). So we can conclude, at least tentatively: (1) the people are pretty good at what can be done well (apply rules and principles to processes and policies); (2) there is no socially acknowledged expert group who can do this better and (3) although economic and other experts are better than the people at predicting the effects of policies, they are still so bad at it that we must discount these reasons when making rational decisions about what to do. It is important to stress that we need not really show that rule-based reasons are terribly strong (i.e., that they always have a high weight attached to them), since our analysis of the predictive ability of experts indicates that, at least when it comes to government policy, the value of R_e will be so low that even a high weight will not greatly affect R_t.

The advocate of the elite may dispute claim (2): the people, it may be charged are not as good even at applying moral and social rules because, while they can apply rules, they do not know enough political facts to understand the context. If you don't know what is going on, you can't know whether what is going on violates the rules. So, it might be thought, we are back to the special competency of the experts. Two replies are in order. *First*, we should not forget the benefits of aggregation (section 1), and the way that a great deal of noise can be factored out in elections, leaving decisive influence with those who have a better grasp of what is going on. *Secondly*, though, the advantage of the experts in knowing the contexts in which rules and principles might be applied is often cancelled out by the experts' idiosyncratic value theories. Those influenced by economics, for example, tend to be highly dismissive of commonsense ruled-based morality. Consider, for example, Russell Hardin's take on moral rules. As "Mill argued from his somewhat elitist stance," Hardin maintains, moral rules are fine for teaching children and the “intellectually impaired”; however, they miss “almost everything of significance” in “our adult world” (Hardin, 2003: 101). With such an attitude towards social and moral rules, it seems advisable for the people — who are not all intellectually impaired — to avoid relying on Mill’s “elitist” stance. Which, it will be recalled, is where we began.

4. Conclusion

It sometimes seems that democratic political theory is divided between those who think that the voice of the people is the voice of God, and those influenced by economics, who think the voice of the people is the voice of a collective moron. I have suggested here that the evaluation of the competency of the people depends on whom the people are being compared to, and on what questions. If we compare them to economists on predicting the economic effects of economic policies they certainly come out worse — but the economists come out bad too. The experts have an advantage, but it is not nearly as impressive as most think. If we are concerned with the competency of the people in applying basic norms of fairness and rules that evaluate processes, they seem competent and there are no moral experts who can plausibly show they do better. When we compare the weighted importance of these two competencies, I have argued, the people’s competency comes out on top. That is, they come out on top if, as Hayek argued a free society must, we base our policies on social and moral rules and principles and abjure the hopeless attempt to construct policies that efficiently promote desired ends. There is impressive evidence that pursuing such ends is an impossible task, yet it is one that, for deep psychological reasons, we are convinced that we can do. If we persist in trying we should listen to the experts — but we are almost certain to be disappointed.

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