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REPLY TO MY CRITICS

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REPLY TO MY CRITICS

ABSTRACT: *This symposium's objections to my book fall into two main categories: philosophical and empirical. The philosophical objections are largely sophistical. If we took them seriously, they would invalidate far more than my book: We would also have to give up social science and common sense. The empirical objections, in contrast, are often thoughtful and important. The most notable: Kiewiet and Mattozzi's vigorous defense of the American public's free-trade credentials, and Wittman's doubts about the magnitude of the belief gap between economists and the public. But Kiewiet and Mattozzi put too much weight on a single survey question; and the belief gap between economists and the public is much larger than the belief gap between the American public's far left and far right.*

I. REPLY TO BENNETT AND FRIEDMAN

1. *Philosophic Sophisms*

Bennett and Friedman (2008; henceforth BF) present a wide variety of arguments against most of my main theses. But—as their title suggests—their main disagreement is philosophical. They believe that economists' earlier concept of “rational ignorance” is self-contradictory, and that my concept of “rational irrationality” simply goes further down a deeply mistaken path.

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Are they right? Let us begin with BF's objections to the concept of rational ignorance. Although my book vigorously critiques economists' overuse of this concept, I never doubted its empirical relevance. BF, in contrast, argue that rational ignorance explains nothing about the world because it is literally contradictory.¹ As they succinctly explain:

Information economics reduces ignorance to the *known* cost of searching for *knowable* "information"—as if the searchers knew what they were looking for, where it was to be found, *and its value*. None of that would be possible, however, unless the searchers already knew the very things of which they are supposed to be "ignorant" *ex ante*. (BF 2008, 204)

Unfortunately, this argument is a simple sophism.² We don't need to know *everything* about something to estimate the value of learning more about it. In many cases, we barely need to know anything at all. For example, almost everyone deletes spam email unread. If we accept BF's argument, this practice is a deep philosophical error. After all, how do we know that a given piece of spam isn't worth our time unless we actually read it? Indeed, unless we send him the money, how do we know that a random Nigerian's offer to split an inheritance *isn't* the path to untold riches?

Consider another example. Suppose a student is weighing whether he should study philosophy. Contrary to BF, he doesn't need to actually study the subject to know a lot about its value to him. If he cares about money, he can look at salaries for philosophy majors. If he cares about personal enjoyment, he can consider whether he enjoys talking about abstract questions. It is absurd to claim that he has to actually earn a Ph.D. in philosophy before he can know whether it's worth it.

Admittedly, if by "know" you mean "know with 100 percent certainty," then you could embrace my *reductio*. But in ordinary language, we can "know" things that are merely probable; and in ordinary life, probable knowledge is all we need. When I receive an unsolicited offer to make millions of dollars, I instantly delete it even though I know that there is a tiny chance that the offer is genuine. Don't you?³

You might reply that my example only shows that rational ignorance explains our response to "known categories," such as spam email. Even if this reply were telling, it still overturns BF's blanket dismissal of information economics. But in any case, the "known categories" limitation is practically no limitation at all, because we know so many categories. Most people don't fall for the first spam email they ever receive. Even if

they've never heard of "spam," they still know the category of "fraud." Even if they don't know about "fraud," they know about "tricks."

Admittedly, this doesn't show that any particular error is the result of rational ignorance. It doesn't prove that our beliefs about spam are accurate, or even roughly accurate. (If you dispute such things, I would rather bet you than try to convince you). But it does show that, contrary to BF, the notion of rational ignorance is not contradictory, or a reflection of economists' philosophical illiteracy.

BF find it difficult to see why economists largely abandoned explanations based on "simple, inadvertent" ignorance in favor of rational ignorance. So it is worth highlighting some of the additional insight that rational ignorance offers. Unlike simple ignorance, rational ignorance can explain why people know *more* about some subjects than others. Namely: people are more likely to know things with a high marginal benefit relative to marginal cost than with a low marginal benefit relative to marginal cost. This is one important reason why, for example, modern Americans know a lot more about driving cars than riding horses. Rational ignorance can also explain why knowledge levels *change*. Namely: when the marginal benefit of knowing something goes up, or the marginal cost goes down, knowledge levels tend to rise; when the opposite happens, knowledge levels tend to fall. That is one important reason why, over the course of the twentieth century, car-driving knowledge increased, and horse-riding knowledge decreased. If all ignorance were "simple" and "inadvertent," it would be hard to understand these patterns, or countless others like them.

BF are correct to claim that my concept of rational irrationality builds on the pre-existing concept of rational ignorance. So it is necessary for me to defend rational ignorance against their objections. But it is hardly sufficient. For BF, rational irrationality suffers from its own unique set of contradictions:

We find this thesis incoherent, simply because we do not know what it would mean for someone to hold an economic opinion (or any other kind of opinion) if she did not think the opinion were correct. (BF 2008, 211)

Once again, BF's philosophical veto is sophistical. First, it is not contradictory to say that a person has contradictory beliefs.⁴ If I say "I believe X and not-X," my view is incoherent. But if I say "That voter believes X and not-X," my view could easily be true; it is contradictory *states of affairs*, not contradictory beliefs, that are logically impossible. Second,

even if it were impossible for a person to simultaneously have the conscious belief that X and the conscious belief that not-X, most of our beliefs are not conscious at any given time. A person can consciously believe that dying in battle against the infidel is a direct path to paradise—without rejecting his unconscious belief that dying in battle against the infidel is a direct path to dying. Third, it is not contradictory to say that when a person has contradictory beliefs, the side of the contradiction that he consciously believes and acts upon can respond to incentives. In the mosque, it is cheap to consciously believe in divine rewards for a glorious death; on the battlefield, however, the cost sharply rises, and beliefs respond accordingly.

BF's other main theoretical objection to rational irrationality hinges on the contrast between conscious decisions to be irrational, and inadvertent error. If my position claims the former, they dismiss it as contradictory; if my position claims the latter, I am merely reaffirming BF's simple ignorance story:

Maybe he [Caplan] really means to say that erring voters are, by virtue of their errors, *inadvertently* acting *as if* they were consciously trading social injury against personal ideology—but that in fact, they do *not* know that the policies they favor are socially injurious, so they are not literally “thinking” that they might as well “indulge” themselves in voting for what they *know* is harmful to others. But this reading is inconsistent with the theory of rational irrationality, because, we repeat, this reading gives us an account of voter ignorance, not an account of voter irrationality. (BF 2008, 209)

Contrary to BF, it is logically possible for a person to make a conscious decision to be irrational. Consider: Have you ever made a conscious decision to stop thinking about something? (For example, “I’m going to stop thinking about my ex.”) A sophist might respond, “It’s impossible to consciously decide to stop thinking about X, because you have to think about X in order to make that decision.” What this argument overlooks, however, is that once you make your conscious decision to stop thinking about X, you may not have to keep thinking about your decision in order to act in accordance with it. In the same way, once you consciously decide to be irrational, you may not have to keep thinking about your decision to act in accordance with it.

In any case, even if a fully conscious decision to be irrational were impossible, “inadvertent error” would not be the only remaining

possibility. In the real world, there is a *continuum* of intent, with completely deliberate choice at one end, and total accident at the other. Suppose a person crashes his car because he was driving drunk. It would be odd to call the crash a deliberate choice, but it is just as odd to call the crash a total accident. The driver deliberately drank alcohol; he knew that drinking greatly increased his chance of crashing; and he drove anyway. It is fair to say, then, that his crash was partly, or to some degree, or “in some sense” a conscious choice. More importantly, once you notice the conscious element in the choice, it is easy to see how car crashes could respond to incentives. People rarely get in drunken accidents “on purpose,” but harsher punishments for drunken accidents can still persuade people to abstain, drink less, designate a driver, or drink at home.

The same goes for absurd beliefs. At one end of the continuum we have people who explicitly think, “I’m going to be irrational in order to believe some comforting absurdities.” At the other end, we have people who, to use BF’s phrase, “stumble into mistaken ideas” (BF 2008, 211). In between these poles, we have a full spectrum of psychological stances: people who default to emotionally appealing beliefs, people who refuse to listen to criticism because it upsets them, people who jump to hasty conclusions on complex subjects, and so on. To equate this whole intermediate spectrum with “stumbling” throws away a great deal of information. More importantly, this equation leads to the sophisticated conclusion that beliefs cannot respond to incentives because “Mistake is not a *preference*. It is involuntary. People do not ever deliberately choose it” (*ibid.*, 230).

BF judge the average voter with shocking leniency. In the absence of absolute proof of “willful perversity,” they pronounce the voter innocent—even when there is more than enough evidence to convict on the lesser charge of intellectual *negligence*. This passage is particularly revealing:

In order to “refuse” to “fairly weigh all claims,” the voter would have to *know* about them—and, indeed, not only to know that the claims exist, but to know what the arguments behind the claims are. Only after knowing these things would it be *possible* to “choose” to weigh the claims in a “nepotistic” manner, because of the low incentives to do otherwise—as opposed to favoring one’s own opinion simply because one doesn’t know the counterarguments, or even that there *are* counterarguments. (BF 2008, 210)

If one takes BF literally, it seems like the only things a voter needs to do to keep his rationality above reproach are (a) avoid information about the

existence of other views, and (b) if he learns about the existence of other views, don't listen to any of their arguments. But what are these actions, if not glaring examples of intellectual nepotism? Furthermore, if the average voter can't fairly weigh competing theories without knowing about their existence (and arguments!), what business does he have being anything other than agnostic?

2. *The Evidence: What, If Anything, Counts?*

In addition to criticizing the conceptual coherence of rational irrationality, BF are also unimpressed by my empirical evidence. Or to be more precise, they deny that I offer any evidence:

We will maintain that Caplan has provided no case for his theory of "rational irrationality": no evidence of public error; no evidence of rational calculation or deliberate decision; no evidence of public "emotionalism" or "ideology." (BF 2008, 198)

Notice: BF simultaneously claim that (a) rational irrationality is impossible by definition, and (b) that I fail to offer any evidence on its behalf. If they are right about first claim, however, the second claim is redundant. If a theory is impossible by definition, any "evidence" in its favor *must* be false or irrelevant. Under the influence of this prejudice, BF fail to give some serious evidence a serious hearing.

To take the most obvious example, BF (2008, 198) completely dismiss the evidence that economists and the public systematically disagree:

He [Caplan] does not even provide evidence of public error. He merely shows that in 1996, in a one-shot poll about economic issues, a random sample of the public tended to answer the survey questions in a manner that Caplan ... considers to be literally "crazy"—because these answers differed from the free-market answers that Caplan's fellow economists tended to give to the same questions.

I agree that this is not irrefutable proof that the public errs.⁵ But if massive disagreement between laymen and experts is *no evidence* of public error, what is?

BF's response, seemingly, is that I should have explained the reasoning behind the economic consensus. As they ask, "Why couldn't Caplan

simply have *demonstrated* that the public's economic opinions are wrong, using whatever logically valid arguments are available?" (BF 2008, 202) However, Caplan 2007, 23–49, repeats most of economists' classic arguments—and provides many references for curious readers. Apparently none of this counts as evidence either.

What *does* count as evidence for BF? Frankly, they set their standards so high that virtually nothing qualifies. Consider their blanket dismissal of both economics and political science:

Economists are, by virtue of their Ph.D.s, “experts” only in the literature of contemporary economics, just as political scientists are, by virtue of their Ph.D.s, experts only in the literature of contemporary political science. (BF 2008, 202)

Could this be why, for BF, none of my citations to dozens of other surveys about economic beliefs count as evidence? It is worth adding, moreover, that they have little reason to treat natural sciences differently. After all, these do not “necessarily” “confer expertise about reality” either (*ibid.*, 202). Indeed, virtually everything that an individual scientist thinks he knows comes from reading the *literature* of contemporary science—not first-hand experimentation.

Do I exaggerate BF's Pyrrhonism? No. Consider how they respond when I test specific doubts about economists' expertise:

Caplan rebuts two weak counterarguments: that economists are biased because they are rich and because they are politically conservative. He does not address the real problem, which is that economists are human beings. (BF 2008, 245)

I freely admit that we should moderate our confidence to compensate for human error. But strange as it sounds, BF seem to draw the stronger inference that expert judgment has zero evidentiary value because experts are human.

3. *Will the Real Fanatic Please Stand Up?*

Before concluding, let me examine what I see as BF's genuinely illuminating criticisms.

BF argue that many of my criticisms of the average voter are off-target. Despite my accusations of fanaticism, elites, not masses, are the real fanatics:

According to Caplan's theory, however, ideological constraint (dogmatism) should *positively* correlate with political ignorance. The typical voter is supposed to be dogmatic about his economic biases because his political impotence, as a lone member of the mass electorate, diminishes the utility of being objective about politics. In truth, however, the people who are the *least* ideological are also the ones who are the *most* ignorant. (BF 2008, 236)

BF's equation of ideological constraint with dogmatism is questionable; one could just as easily take ideological constraint as a sign of *consistency*. Furthermore, since better educated people think more like economists, I naturally see them as unusually reasonable, not fanatical or dogmatic. But BF (2008, 237) go on to make a more thought-provoking point:

Ordinary citizens, relative to well-informed elites, are inattentive to politics, don't understand it, and therefore don't know how to organize the political impressions they gather. . . . By contrast, a small minority of people may come across some grand belief system—a political ideology—that allows them to understand, integrate, and remember relatively large amounts of political information. . . . They would end up being extremely well informed, relative to most people, and—because of the amazing ability of their perceptions of the world to confirm their ideology's picture of the world—extremely dogmatic.

I readily concede that the most extreme fanatics are usually elite.⁶ You are a lot more likely to find doctrinaire Leninists at UC Berkeley than at your local mall. But this fits perfectly with my politics-as-religion analogy. The average person knows little about religion. The most fanatical believers are usually theologians and religious activists. But the reason is *not* that education leads to fanaticism. In fact, religious views become more reasonable as education rises. Belief in the literal interpretation of the Bible, for example, sharply declines as education rises.⁷ The reason why the most fanatical people are in some sense well informed is, instead, selection: To become a fundamentalist theologian, you need to believe in Biblical literalism *despite* your education, intelligence, and other elite attributes. While my model does not specifically predict this pattern, it certainly doesn't rule it out.

4. *Is Economics Counterintuitive? Does It Matter?*

BF (2008, 226) fault me for my “theory-driven refrain that economics is nothing but ‘common sense.’” They are of course right that *many* economic arguments—such as Say’s Law and Ricardo’s Law of Comparative Advantage—are non-obvious. But I stand by my claim that basic economic truths are obvious to any adult of normal intelligence who calmly considers them. It is obvious that trade is mutually beneficial; it is obvious that trade remains mutually beneficial even if the traders come from different countries; it is obvious that protecting unproductive jobs makes society poorer; and it is obvious that, over the long run, living standards have drastically improved.

BF (2008, 225) point out that when “Caplan allows that economic theory is ‘counterintuitive,’ we believe that he has made a slip.” I agree. I misspoke. Adam Smith’s observation that the market gives businesses an incentive to please consumers is yet another truism that is obvious to any person of normal intelligence who calmly considers it. Instead of “counterintuitive,” I should simply have said “unpopular.”

In any case, my main arguments work even if economics isn’t intuitive. If people who never studied economics would simply admit their ignorance and become agnostic, the Miracle of Aggregation would lead democracies to adopt sound economic policies. The mechanism is simple: If the economically illiterate ignored candidates’ economic proposals, demagoguery would no longer win votes. Candidates would accordingly tailor their economic platforms to appeal to the economically literate—and democracies would choose much better policies.

5. *Sociotropic Contradiction?*

Unlike most economists, I recognize that material self-interest is a poor predictor of people’s political views. It is easy to see, then, why BF (2008, 208) accuse me of contradicting myself:

While elsewhere in the book, Caplan (2007a, 148–53) endorses political scientists’ empirical finding that voters tend to vote “sociotropically” instead of selfishly (Kinder and Kiewiet 1981), in the final passage just quoted, and elsewhere, he forgets this, and assumes that voters would value economic truth only if it were in their self-interest to do so.

However, the contradiction is only apparent. As I explain in footnote 20 of chapter 6:

To be more precise, they [voters] are not selfish in the conventional sense of trying to maximize their wealth or income. My analysis *does* assume that people choose their political beliefs based on psychological benefits to themselves, ignoring the costs to society. Thus, my thesis is that voters are selfish in an unusual but non-tautologous sense of the word. (Caplan 2007a, 229)

BF (2008, 236) similarly misunderstand my view when they claim that my theory predicts that leaders will be rational:

While the relative *absence* of ideology among the general public is bad enough for Caplan's theory, so might be the other side of the coin: its presence among the fraction of the public that consists of politically well-informed people.

Recall Caplan's Pakistani judges who ban interest payments: such people *do* make public policy, by definition, unlike any lone modern voter. So even if these victims of economic fallacy were, in Caplan's view, "irrational," they cannot be "rationally" irrational—preferring (for some reason) to believe in a fallacy because they know their beliefs don't matter. Their beliefs *do* matter, very much. Caplan (2007a, 33, *emph. original*) further undermines his theory by citing with approval "a fascinating survey of 63 environmentalists, congressional staffers, and industry lobbyists," which found that "*not one* could explain economists' standard rationale for tradable [pollution] permits." Environmental activists, congressional staffers, and industry lobbyists are much more politically powerful than individual voters. According to logic of rational-ignorance theory, then, such people *should* want to know the truth—so if they are ignorant of economics, it must be *despite* the incentives.

I specifically address this point in chapter 7. Elites tend to be rational about subjects that affect their career success. However, disagreeing with the public hurts one's career, so rational policy analysis often doesn't pay:

Politicians, unlike average voters, make some political choices where their cost of systematic error is high. In these cases, we should expect leaders to be shrewd and clear-eyed... .

However, there is one important area where matters are less clear: *Beliefs about policy effectiveness*. Does it pay politicians to correctly diagnose how

well policies work? If all that voters care about is adherence to their policy preferences, the answer is no. For the vote-maximizing politician, the majority is always right. (Caplan 2007a, 167–68)

Again, to say “Politicians, like voters, are sociotropic, so they will carefully examine their policy views even if it hurts their careers” misses my whole point about sociotropic voting. As Caplan 2007a, 150–51 explains, we should expect people to act more altruistically in politics when the *personal* cost of doing so is low. When a citizen votes to “help the poor” the expected cost is a fraction of a penny. When a politician votes for unpopular policies, however, the expected cost could easily be his career. The same goes for an environmental activist who takes economic analysis too seriously. Who wants to become a pariah in exchange for a modest chance of improving policy?

6. Toward a Pluralism of Error

At times, BF (2008, 239) accuse me of ruling alternatives to my view out of court: “Caplan’s theory, by attributing error to incentives, *precludes* inadvertent error at the start.” As I explain in chapter 4, though, my claim is not that *any* error is proof of irrationality. Instead, I point to a number of symptoms that increase the probability that an error stems from irrationality: bias, emotionalism, dogmatism, and so on.

It is true that I treat *rational* ignorance, not BF’s “inadvertent ignorance,” as the main alternative hypothesis. But the reason isn’t that I think that inadvertent ignorance never matters. It occasionally does. Still, you have to judge the public very leniently to conclude that its major errors are inadvertent. When BF (2008, 234) say:

People who do not deliberately, rationally *choose* to be ignorant will *not* form random opinions, unless the genetic and cultural influences on them are also random—which flies in the face of everything we know about genes and culture.

My response is to ask: Are these people supposed to be unaware that culture affects beliefs? That’s absurd—every adult of normal intelligence knows, for example, that children tend to adopt whatever religion their parents teach them. Once you know this fact, you can compensate for it:

Either you study the views of other cultures, or you embrace agnosticism. If you fail to do either, your ignorance is not truly inadvertent.

II. REPLY TO COLANDER

1. *The “Works-Better” Justification*

Much of David Colander’s reply to *The Myth of the Rational Voter* builds on Churchill’s position that “democracy is the worst form of government, except all those other forms that have been tried from time to time” (Eigen and Siegel 1993, 10). Colander (2008, 263) calls it the “works-better” justification: “Most people who have studied the issue have concluded that the democracy we have works better than the alternative forms of government that have been tried.”

The problem with Colander’s argument, as with Churchill’s, is that it shows far less than it claims to. Partitioning all possible forms of government into “democracy” or “non-democracy,” and observing that “democracy works better” than the other category, obscures a key point: Democracy is a range on continuum, not a single point. We call the United States a democracy despite the existence of the Supreme Court, the First Amendment, the electoral college, and the disproportionate influence of small states in the Senate. We called Great Britain a democracy before 1949, even though it had plural voting for college graduates. To oppose additional limitations on majority rule because “we do not know that any other system will work better” (Colander 2008, 262) is open to an immediate response: I’m trying to *improve* democracy, not offer another system.

Sometimes, of course, delegation “removes a set of questions from direct democratic decision, but at the same time maintains the right to overrule that regulatory body” (Colander 2008, 264). But this is not always true; governments that we call democratic often deny the majority an ultimate veto. In the United States, for example, you need a super-majority to change certain policies. What stops us from going further in this direction?

Thus, Colander’s claim that “any democracy retains overall control of the market, and if people choose, they can remove the market as the coordinating mechanism” (Colander 2008, 264) is an overstatement. There are *some* democracies where a simple majority decision is sufficient to overrule the market. There are other democracies where the decision rule is more involved. There have even been democracies—like the

United States before the New Deal—that frequently overruled the “people’s choice” to overrule the market. I suspect that Colander and I would still disagree about whether the latter system would work better than what we have today; but I don’t see how he can deny its possibility.

2. *What Are Economists Experts About?*

I broadly agree with Colander’s complaints about graduate education in economics.⁸ Fortunately, as he observes, “many economists nonetheless become sophisticated policy analysts through informal training and reading outside the classroom” (Colander 2008, 268). In defense of my survey results, then, it is worth pointing out that respondents to the Survey of Americans and Economists were not graduate students or general-equilibrium theorists. Instead, the survey deliberately limited itself to economists specializing in domestic policy (Blendon et al 1997, 106).

Colander (2008, 267) also seems to say that even policy-oriented economists have no expertise on normative matters:

In a democracy, normative questions are settled by the people at large. It is not for economists to tell people what they should believe about normative issues, but that is precisely what they would be doing if they were to draw policy conclusions from their “positive” or scientific theories.

Colander’s point might be valid if we were talking about *purely* normative judgments, judgments that are entirely independent of how the world works. But most policy debates blend positive and normative analysis. Consider the minimum wage. A well-designed 1996 Gallup poll found that support for increasing the minimum wage was very sensitive to beliefs about its employment effects.⁹ As I explain in an earlier exchange with Donald Wittman:

This survey split a sample of about a thousand people into two groups. The first was asked: “Do you favor or oppose raising the minimum wage from four dollars and 25 cents an hour to five dollars and 15 cents an hour?” Responses were typical: 81 percent in favor, 17 percent opposed. The second group was asked: “Would you favor or oppose raising the minimum wage if it resulted in fewer jobs available to low paid workers in this country?” Mentioning this moderate drawback drastically reduced support for a higher minimum wage; only 40 percent favored it, with 57 percent

opposed. Even if one doubts the unemployment effect of the minimum wage, the point is that its popularity depends heavily on beliefs about its effects. (Caplan 2005, 175)

From this perspective, economists have as much right to “tell people what they should believe about normative issues” as doctors do. In both cases, experts combine widely shared premises about ultimate ends (prosperity, health) with their specialized knowledge to yield practical advice. In fact, economists’ emphasis on trade-offs makes their normative analysis especially credible. Doctors, for example, often speak as if health is the *only* thing that matters, and ignore competing values like pleasure, convenience, and money. Economists, in contrast, realize that material prosperity is one good among many—and often phrase their advice accordingly.

Colander (2008, 268) has one other major objection to my claims about economists’ expertise: Whatever economists think they know depends on how they “frame” their questions:

Any policy expert will necessarily frame highly complex issues in a certain way. The problem is that just about any policy problem could be framed in many ways, not just in the way that the expert frames it. A different frame might lead to a different conclusion. What this means for me is that society cannot rely on experts in any subfield for a “correct” answer—only for a correct answer given their frame.

Maybe Colander is merely making the common-sense point that economists’ frame *might* be misleading, and that it is prudent to double-check by considering other frames. If so, I agree. But if we take Colander (and his scare quotes around the word *correct*) literally, his position amounts to radical skepticism: there is no truth about the world, just truth about frames. I have to wonder: Why does Colander say that he would “generally turn to an economist if I were looking for economic policy advice,” unless he thinks that economists’ frame is more likely to be correct? And if no frames are correct, why are we arguing?

III. REPLY TO ELSTER AND LANDEMORE

1. *Am I Throwing Stones from a Glass House?*

Elster and Landemore’s (henceforth EL) strongest criticism of my book is that I am a good example of my own theory:

Caplan seems to be so wedded to his idea that voters are wedded to their beliefs that he is willing use any kind of rhetorical device, ranging from analogy to snide remarks, to support his claims. If we may allow ourselves for a moment to adopt his own insinuating style, it would probably cost *him* a great deal to give them up. (EL 2008, 280)

I take this point seriously. My self-image is closely linked to my ideas. On my own view, then, I need to monitor myself carefully if I really want to figure out the truth.¹⁰ If EL say they can show that my self-monitoring failed, they have my attention. So what specific evidence of my irrationality do they present?

a. They object to the “one-sidedness” if my approach: “The failure to play devil’s advocate by thinking about possible weaknesses in one’s own position is often a sign of ideological commitment” (EL 2008, 281).

Indeed it is. But did I really fail to play devil’s advocate? Some major counter-examples: (i) I raised the possibility that economists themselves might be biased (Caplan 2007a, 52–6). (ii) I discussed experimental economists’ skepticism about the responsiveness of rationality to incentives (Caplan 2008, 135–7). (iii) I admitted that democracies adopt better policies than raw public opinion would lead you to expect (Caplan 2008, 156–62). In fact, EL treat this as a significant concession on my part. (iv) I emphasized that many of the institutional reforms I favor suffer from a Catch-22 (Caplan 2008, 199). Admittedly, none of these admissions led me to actually reject my own view. But that is trivial; if I changed my own mind in the process of playing devil’s advocate, I would simply have changed the book to reflect my revised view.

b. EL (2008, 280) allege that I “neglect the fact that economists, like the rest of us, often exhibit herding behavior.”¹¹ But my book specifically admits that the economic consensus is fallible:

Frankly, the strongest reason to accept its reliability is to flip through a basic economics text, then read the SAEE questions for yourself. You may not be fully convinced of economists’ wisdom. I, too, doubt it on occasion. (Caplan 2008, 83)

Stranger still, when I share my specific doubts, EL (ibid., 281–82) rush to defend the mainstream view. Yes, there is a common pattern to my reservations: “Where the economists tend to *agree* with the general population, Caplan argues that they are wrong.” But EL’s complaint now seems to be that I *am* accusing economists of herding not just with each other, but with the broader public as well.

c. EL (2008, 280) object to my “departures from the norms of scholarship,” including my readiness to use “any kind of rhetorical device, ranging from analogy to snide remarks.” They make a fair point; but to be candid, this is a feature of my work, not a bug. “The norms of scholarship” provide some valuable quality control; but they also shunt important debates into the ghetto of verbal argument.

In economics, to take the field I know best, if you don’t have a mathematical model or econometrics, you probably don’t have a publishable paper. When economists meet face-to-face, however, their conversations are richer. For example, some rational-choice theorists deny that “preferences over beliefs” are possible. I find that the analogy between politics and religion is an excellent way to make people rethink their position. “Analogy” is not *just* a rhetorical device; it opens people’s minds to possibilities they have been too quick to dismiss.

Ridicule, or what EL call “snide remarks,” is another rhetorical device that helps people see the flaws in assumptions that they mistakenly take for granted. I realize that norms of scholarship discourage the publication of ridicule, but this norm is counter-productive. When a false position is internally consistent, calling attention to its sheer implausibility is often the only path back to truth. As Thomas Reid (1940, 368) explains in *Essays on the Intellectual Powers of Man*:

To discountenance absurdity, nature has given us a particular emotion—to wit, that of ridicule—which is intended for this very purpose of putting out of countenance what is absurd, either in opinion or practice.

This weapon, when properly applied, cuts with as keen an edge as argument. Nature hath furnished us with the first to expose absurdity, as with the last to refute error. Both are well fitted to their several offices and are equally friendly to truth when properly used.

Both may be abused in the cause of error, but the same degree of judgment which serves to detect the abuse of argument in false reasoning serves to detect the abuse of ridicule when it is wrong directed.

EL (2008, 280) also accuse me of resorting to ad hominem arguments, specifically against Fernandez and Rodrik. But it is important to distinguish mere name-calling, which academic norms sensibly discourage, from analysis of arguments’ intellectual underpinnings. If someone drives from Paris to Vienna by way of Berlin and Prague, there is nothing wrong with pointing out that his route is “convoluted,” or observing that “in the absence

of scruples against entering Bavaria, he would have taken a shorter route.” Why is it any more objectionable for me to point out that the Fernandez-Rodrik model is “convoluted,” or remark that “in the absence of professional scruples against voter irrationality, Fernandez and Rodrik would not have bothered with their model”¹² (Caplan 2007a, 208)?

2. *The Political Economy of Altruism*

At times, EL (2008, 287) echo BF’s philosophical objections to my views:

If I buy a beer, I can tell myself that I do so to slake my thirst. If I buy altruism, I cannot—*logically* cannot—tell myself that I do so to enhance my self-image.

...

To the extent that economic models rely on rationality, there cannot be an economic model of the purchase of altruism, any more than there can be a rational-choice model of the purchase of irrational beliefs. This is a conceptual objection.

Unlike BF, however, EL (*ibid.*, 286) seem to realize that there is a continuum from purely accidental behavior to fully conscious choice. They specifically mention “self-deception” as an intermediate possibility:

These theories require that agents *deceive themselves* about their own motivation: It is conceptually incoherent to assume that the conscious motivation behind voting or giving could be the desire to enhance one’s self-image, since that effect will come about only if one believes one is acting for the good of others.

Like BF, EL overstate the logical impossibility of consciously being irrational. To repeat, it is not contradictory to say that another person has contradictory beliefs. In any case, though, little hinges on this point. As long as self-deception responds to incentives, my arguments about the political prevalence of irrationality and altruism work.

Similarly, when EL (2008, 286, *emph. added*) say that “even self-deception *may* be reality-constrained, in the sense that some genuine sacrifice *may* be needed for self-credibility,” they are literally correct. I never denied that reality has *some* connection to our beliefs. My point is that

there are important classes of behavior—like voting—where reality constraints are awfully weak. Many people convince themselves that voting against their material interests is a “major sacrifice,” even though it’s not.

Finally, EL (2008, 287) overinterpret my simple model of a demand for altruism. I never denied the existence of “genuine altruism.” In fact, my argument *requires* a small amount of genuine altruism (Caplan 2007a, 229n34). Purely selfish agents would buy zero altruism regardless of the price, so their political behavior would be no less selfish than their private behavior. Furthermore, when I say that “altruism and morality generally are consumption goods like any other,” I merely mean that people are less likely to do the right thing as the cost of doing the right thing rises. I did not mean—nor do I believe—that no one will pay a high price to do the right thing, or that morality is a fiction.

3. *Miscellany*

a. According to EL (2008, 273), “there is no doubt” that in my view, the less democratic a regime is, the better. Their claim contradicts what I wrote.

When cataloging the failures of democracy, one must keep things in perspective. Hundreds of millions of people under democratic rule enjoy standards of living that are, by historical standards, amazingly good. The shortcomings of the worst democracies pale in comparison with those of totalitarian regimes. At least democracies do not murder millions of their own citizens. (Caplan 2007a, 3)

b. I acknowledge that democracies give us *better* policies than one would expect given public opinion. But this hardly justifies EL’s claim that my book should have been subtitled “Why Democracies Should Theoretically Choose Bad Policies Even If They Don’t” (EL 2008, 274). Every democracy, for example, has protectionist policies. These policies are almost always popular, and any politician who actively campaigned for their abolition would do great damage to his career. Yes, trade policies are not quite as protectionist as the public wants; but protectionist policies are still bad, and public opinion is largely to blame for their existence.

c. Even if my analysis is correct, are the inefficient policies that democracies adopt actually “bad”? As EL (2008, 277) remark, “it is far from clear

that an inefficient outcome more in line with popular views about distributive justice is inferior to an efficient outcome at odds with those views.”

My response: To keep the project manageable, I follow the “enlightened-preference” approach. I take voters’ fundamental normative values as given, and question only their factual beliefs. Empirically, voters are quite sociotropic, so if their beliefs about what is “good for the economy” are seriously biased, we can infer that most people would favor sharply different policies if their biases went away. This doesn’t imply that the maximally efficient policy would enjoy unanimous support from an economically literate electorate. As EL observe, there is an equality/efficiency tradeoff, and compensation for losers is imperfect. All I am saying is that, holding values constant, correcting the mistaken belief that policy A is “better for the economy” than policy B will tend to increase support for policy B.

The main problem with the enlightened-preference approach, admittedly, is that people’s values could be wrong. In fact, I suspect that popular views about distributive justice are as wrong as popular views about economics.¹³ If so, *The Myth of the Rational Voter* actually understates the difference between policy as it is and policy as it ought to be.

IV. REPLY TO GAUS

1. *The Value of the Obvious*

Gerald Gaus doubts that economists have much useful policy knowledge. As he puts it, “Economists don’t possess significant predictive knowledge, and general economic laws are of little help in predicting the magnitude of the effects of a specific policy in a multi-variable, complex world” (Gaus 2008, 291). In fact, he doubts that *any* experts have much useful policy knowledge. Predicting “overall consequences” is virtually impossible due to the complexity of the world. Predicting “a few variables in the short-run” is much easier, but still not easy enough: Gaus favorably discusses Philip E. Tetlock’s (2005) work on expert political judgment, which shows that experts do worse than random guessing.

Gaus realizes that he and I interpret Tetlock’s findings differently (Caplan 2007b). In particular, I emphasize that Tetlock heavily handicapped the experts by deliberately excluding “obvious” questions.¹⁴ Gaus

(2008, 302) is unimpressed by this response: “Experts, if they earn their bread at all, earn their bread by giving us advice on the answers to non-obvious questions.”

The problem with Gaus’s objection is that the public often rejects the answers that the experts consider “obvious.” Indeed, this is one of the main messages of *The Myth of the Rational Voter*. The general public systematically disagrees with *basic* economics. Even when the questions are easy, the public gravitates toward the opposite of the correct conclusion. The upshot: Even if economists knew nothing but obvious truths, these are a vast improvement over public opinion. In the land of the blind, the one-eyed man is king.

2. *How Much Precision Do Advisors Need?*

Gaus has high standards for useful advice. First, useful advice requires detailed empirical study: “Unless policy makers have a good way to predict the magnitude of the costs and benefits, they cannot have a sound basis for advocating or opposing the policy” (Gaus 2008, 301). Second, useful advice requires detailed empirical study of *many* things: Advisors must “factor in all the relevant, real-world variables” (ibid., 298) as well as “the complex interaction of different variables” (ibid., 299)

By Gaus’s standards, economists (and experts generally) do not measure up. Even on relatively simple questions, “the experts’ poor absolute levels of prediction ... is really striking” (Gaus 2008, 303). If you take the problems of omitted variables and interaction effects seriously, good advice is effectively impossible: “We seldom if ever know all the relevant variables” (ibid., 298), and even if we did know all the relevant variables, “it is usually impossible to predict how they will interact” (ibid., 299).¹⁵

Gaus’s arguments prove too much. They don’t just rule out useful *policy* advice; they rule out useful advice of any kind. For example, suppose a student asks me if he should go to grad school. By Gaus’s standards, to “have a sound basis” for my advice, I would need fairly precise estimates of the magnitude of the costs and benefits of *all* the relevant, real-world variables and their interactions. I don’t have that. No one does.

I take this to be a *reductio ad absurdum*: If your standards imply that useful advice is impossible, something is wrong with your standards.

Where does Gaus go wrong? First, he exaggerates the importance of precise information about magnitudes. In the real world, advice based on *rough estimates* is often very useful. Second, he exaggerates the practical importance of complexity. We can ignore most variables—and most interactions—and still give useful advice, even in a complex world.

How can advice based on mere “rough estimates” be of any value? At minimum, rough estimates allow us to drastically narrow down our choices. If a student with a D average asks me about grad school, I don’t need detailed empirics to tell him where to apply. I just politely tell him that he’s not cut out for grad school. In many cases, rough estimates take us straight to the best answer: If one choice is much better than another, we don’t need to crunch the numbers to know what to do. If an econ major with an A average, high GRE scores, and an academic personality asks me what he should do with his life, I tell him to apply to top economics programs, because his other options aren’t even close. Most policy advice works the same way, and there’s no need to apologize for it. When a politician says we should cap the price of gas at \$2.00 per gallon, economists, unlike the public, realize that the result would be large shortages and long lines. Economists don’t need to figure out the precise size of the shortages and the exact length of the lines before they can reasonably advise against price controls. They can make do with ballpark estimates based on past experience.

Gaus may object that economists need to study all the relevant variables and their interactions before reaching a conclusion. But in the real world, this is overkill: Unless you can plausibly point to benefits of price controls comparable to the costs, refining the calculation is probably more trouble than it’s worth.

Of course, if you demand absolute certainty, my approach will horrify you. But if you’re looking for useful personal or policy advice, my approach is good enough. Furthermore, if laymen ignore costs and benefits that are both large and obvious, the experts’ rough estimates will normally be a big improvement.

If this sounds too easy, consider Gaus’s approving reference to Hayek: “This point [the knowledge problem] is fundamental to Hayek’s analysis of the market, his case against planning, and his argument against the misuse of economic models” (Gaus 2008, 298). But how does Hayek know that the magnitude of the knowledge problem is big enough to outweigh all the purported benefits of central planning? How can Hayek escape the charge that he ignores countless variables and interactions

relevant to the choice between markets and central planning? In practice, Hayek did the same thing that other economists do: He looked at the horrors of socialism, and figured, “The purported benefits of socialism are too small to tip the scales in its favor.”

I do not mean to be flippant. Policy advice varies in quality. Precise, nuanced estimates are generally of higher quality than rough-and-ready estimates. But when the public counts cheap imports as a *cost* of international trade, economists don’t need a spreadsheet to do better.

3. *Consequentialism*

Gaus (2008, 306) complains that economists “typically view all evaluation as a form of consequentialism,” adding that “so deep is this bias that Caplan (2007a, 14ff.) sees those who have non-instrumental beliefs as *irrational*.” The passage to which Gaus refers actually has nothing to do with consequentialism; I am making the descriptive claim that people directly value their beliefs, not the prescriptive claim that only consequences matter. But Gaus’s broader point is correct: Normative analysis by economists focuses on consequences, not processes, and *The Myth of the Rational Voter* is no exception.

If I were a pure consequentialist, I could just say, “So what?” and move on. But I am not a pure consequentialist. Like Gaus, I think that the process by which a situation arose affects its moral status. Why, then, do I ignore non-consequentialist issues? First: As long as the consequences of public policies have *some* moral importance, my findings are worth heeding. Second: I think that the policies economists favor on consequentialist grounds are usually better on non-consequentialist grounds as well, so my omissions do not overstate my case. Third: The book is complex and controversial enough already. I discuss its normative implications in greater detail elsewhere (Caplan 2009).

V. REPLY TO KIEWIET AND MATTOZZI

At first glance, Kiewiet and Mattozzi (henceforth KM) seem to totally reject my main theses: “Caplan is wrong—wrong about voters, wrong about their policy preferences, and, most importantly, wrong about the connection between public opinion and public policy” (KM 2008, 314).

On closer examination, though, we are not so far apart. My book explicitly acknowledges many of KM's points, and the evidence that they fault me for ignoring is easy to incorporate into my framework. In fact, their evidence is more compatible with my framework than it is with the standard rational-choice approach.

1. *Is Ignorant Dogmatism Possible?*

Like Bennett and Friedman, KM (2008, 316) wonder how I can simultaneously describe voters as both ignorant and dogmatic:

These hypotheses about voter error do not logically fit together. How can people be intensely committed to ideas and causes that they know nothing about? How can they fall sway to political demagoguery when they have no interest in politics?

The co-existence of ignorance and dogmatism may sound paradoxical, but examples are all around us. Belief in Biblical literalism, to take a classic example of a dogmatic position, declines sharply with education. Belief in creationism follows the same pattern. Even more remarkably, as education declines, *certainty* about the existence of God and creationism rises.¹⁶ Of course, it takes a little knowledge even to hold a position on such questions. At minimum, you need to know that the Bible exists, and that it says that human beings descend from Adam and Eve. But once people know these basic facts, they are quite capable of building dogmatic positions around them—and the less they know, the more dogmatic their positions typically are.

KM (2008, 316) add that “what Caplan should have said is that *some* voters are ignorant, *some* are dogmatic, and others, who may also be members of the first two groups, are susceptible to being swept away by a crowd mentality.” I agree, as long as KM admit that ignorance and dogmatism, far from being mutually exclusive categories, are often complements.

2. *How Protectionist Is the U.S. Public?*

KM argue that my own data contradict my claims about the protectionism of the American voter. In particular, on a 0–2 scale, (0=bad, 1= “doesn’t

make much difference,” 2=good), non-economists in the SAEE lean toward a favorable view of the economic effects of trade agreements, with an average response of 1.33. KM (2008, 318) conclude that “the harmful ‘biases’ that ordinary citizens exhibit are not biases in an absolute sense, but rather biases relative to the positions espoused by Ph.D. economists.”

Before letting the public off the protectionist hook, it is worth looking at more than a single survey question. The SAEE also finds that 68 percent of respondents see outsourcing as a major economic problem; 54 percent think trade agreements have cost the United States jobs; and only 43 percent think that “new technology, competition from foreign countries, and downsizing” will be economically beneficial even in the long run. Other well-established surveys confirm the public’s protectionism. For example, since the Worldviews survey began in 1976, it has found that protectionists have *always* sharply outnumbered free-traders.¹⁷ The General Social Survey similarly finds that about two-thirds of Americans agree that “America should limit the import of foreign products in order to protect its national economy.”¹⁸ Taking all the evidence into account, the U.S. public is indeed protectionist in absolute terms—not just relative to economists.

KM’s other attempts to defend voters’ free-trade credentials are unimpressive. First, they insist that “every president since World War II has been a staunch advocate of free trade” (KM 2008, 320). I agree that presidents—like other elites—are *less* protectionist than the general public. But they are hardly “staunch” advocates of free trade. U.S. presidents often back high-profile protectionism for certain industries, like automobiles and steel, and their general support for free trade is half-hearted. Who has ever seriously tried to eliminate protectionism during his presidency? Who has even paid lip service to this goal?

Second, KM (2008, 320) point to Clinton’s campaign for NAFTA, and conclude that “as voters become more informed, more engaged, and presumably more enlightened, they become more favorably disposed to free trade.” I see this more as a triumph of Clinton’s charisma than of public reason; if Clinton had campaigned against NAFTA, would he have been any less persuasive (Caplan 2003, 235–6)? Furthermore, if it takes a major campaign to sell a treaty like NAFTA, there must be plenty of other worthy trade agreements that die on the vine because leaders can’t spare the time and political capital to push them through. In any case, isn’t the fact that a president had to struggle

mightily to sell the public on a trade agreement evidence that the public *doesn't* support free trade?

Third, KM affirm that democracies are less protectionist than dictatorships. I agree; but this hardly shows that the public isn't protectionist.

In any case, even if KM were right about the average voter's sympathy for free trade, my main claims still hold. My book defines antiforeign bias as "a tendency to *underestimate* the economic benefits of interacting with foreigners" (Caplan 2007a, 36), not as an absolute denial of these benefits. When I analyze the effect of voter bias, my conclusion is not that all democracies will be autarchic. My conclusion is merely that since voters underestimate the benefits of free trade, the level of protection will be inefficiently high.¹⁹

KM's final attempt to exonerate the public for the existence of trade barriers sidesteps the debate about public opinion. Public opinion can't be to blame, they argue, because protectionist policies are extremely detailed:

We do not know whether members of the general public, if presented the case involving carbazole violet pigment 23, would favor or oppose imposing duties on Chinese or Indian producers of this substance. But we are certain that this decision, and virtually all others that are made by the International Trade Commission, will never appear on the public's radar screen. So why does this product, as well as paper clips, paintbrushes, and brake rotors, receive trade protection, while hundreds of thousands of other imported products do not? (KM 2008, 323)

Strangely, though, my book explicitly acknowledges KM's point. As I explain, public opinion determines only the broad outlines of policy. It delegates the details to political insiders:

Politicians' wiggle room creates opportunities for special interest groups—private and public, lobbyists and bureaucrats—to get their way. On my account, though, interest groups are unlikely to directly "subvert" the democratic process. Politicians rarely stick their necks out for unpopular policies because an interest group begs them—or pays them—to do so. Their careers are on the line; it is not worth the risk. Instead, *interest groups push along the margins of public indifference*. If the public has no strong feelings about how to reduce dependence on foreign oil, ethanol producers might finagle a tax credit for themselves. No matter how hard they lobbied, though, they would fail to ban gasoline. (Caplan 2007, 20; footnotes omitted)

Although KM and I agree about these basic facts, we disagree about the underlying mechanism. Why do we have protectionism? KM reaffirm the standard “concentrated benefits/diffuse costs” approach. But as Wittman (1995) and others explain, a democracy of rational voters could easily cope with this problem. For example, voters could pass a Free Trade Amendment to the Constitution. Why is there no public support for such solutions? KM don’t have an answer. In fact, they don’t seem to realize that anyone has proposed such solutions.

My explanation, of course, is that a Free Trade Amendment would never get off the ground because *most people want protection*. Voters don’t have to know the tariff rate on paper clips to affirm that the American government should protect American jobs from unfair foreign competition. What’s “unfair”? The public is happy to leave boring details like that to its representatives.

VI. REPLY TO KLAMER

After fairly (and even sympathetically) summarizing my book, Arjo Klammer challenges my methodology. If economic consensus is the “benchmark of truth,” how can I account for disagreement between American and (for example) Dutch economists? How can I account for disagreement among American economists? How can I account for my own disagreements with the economic consensus? As Klammer (2008, 330) puts it, “Were Caplan to take his benchmark seriously, he would never disagree with the consensus views of his colleagues. But in fact, we know of at least one instance in which he does disagree with the consensus view of his colleagues: in regard to the irrationality of voters.”

If I claimed that the consensus of economics were an infallible oracle, Klammer’s objection would be absolutely correct. But I never claimed this. In fact, I repeatedly state that the accuracy of the economic consensus is merely a *presumption* (Caplan 2007, 52–56, 81–83). People who dissent from the consensus might be right. That goes for Dutch economists, heterodox U.S. economists, the general public, and me. But the burden of proof reasonably lies on the dissenters. That is why I wrote a book arguing that economists are wrong about voter rationality. I’m not contradicting myself; I’m trying to meet the burden of proof that I impose upon all dissenters, myself included.

VII. REPLY TO QUIRK

I only have a few responses to Paul Quirk's fair-minded critique of my book.

1. *Influence Inflation*

Quirk (2008, 338) attributes much of the lay-expert belief gap to “the fact that “ordinary citizens see almost every factor as more important than economists do.” He calls this “influence inflation,” and adds that “there are no obvious practical implications of believing that everything matters, or that all problems are serious” (ibid., 353).²⁰

Quirk may be on to something, but he overstates his case. When conditions get worse, voters typically demand decisive action to deal with them. So why shouldn't we conclude that voters will demand *excessive* action if they think that conditions are worse than they really are? Granted, there is probably a tendency for exaggerated fears to crowd each other out. But why should we believe that this tendency is strong enough to make influence inflation harmless?

2. *Taking Tetlock Seriously*

Quirk, like other critics, cites Tetlock's (2005) evidence that experts are overrated. But as I explain in my review essay on Tetlock's remarkable book (Caplan 2007b), Tetlock's approach exaggerates experts' weaknesses and downplays their strengths. Experts *are* overconfident on issues that are controversial among experts themselves. But Tetlock deliberately excludes easier questions where experts shine relative to both laymen and random guessing.

Populists often use Tetlock to argue that we should simply ignore experts. Quirk (2008, 342) has a wiser reaction: “The practical lesson of Tetlock's studies is not that citizens should ignore experts. It is that citizens and policymakers should discount all predictions and give more weight to the sheer uncertainty of human affairs.” Properly interpreted, though, the practical lesson of Tetlock's studies is somewhat different: On relatively easy questions where experts agree, citizens and policymakers should defer to experts. When an issue is controversial among experts

themselves, however, the wise will indeed discount all predictions and give more weight to the sheer uncertainty of human affairs.

3. *Is the Public Blameworthy?*

Quirk (2008, 354) suggests that the public should not “be blamed in some way for not holding” the same views as economists, and adds that “Caplan would not disagree.” Actually, I do disagree. Yes, “indulgence is rational under the circumstances” because the cost is low. But there is a big difference between *explaining* bad behavior and *excusing* it. Polluting the air with toxins is often in one’s rational self-interest; that’s why people do it. But that’s an explanation, not an excuse: We can still blame people for poisoning their fellow men. In a similar vein, polluting the polity with irrational beliefs is often in one’s rational self-interest, but we can still blame people for voting for bad policies.

4. *The Value of Low-Hanging Fruit*

Quirk (2008, 347) remarks that “Caplan has, in effect, picked the low-hanging fruit of usable expert opinion.” This is an extremely perceptive sentence; he describes my strategy perfectly. My only quibble is that I see this as point in favor of my approach, not a defect of it. Since many researchers practically deny the existence of fruit, the existence of *low-hanging* fruit is remarkable. Social scientists have been studying voter knowledge and political failure for decades. The fact that there is low-hanging fruit left for me to pick suggests that earlier harvesting techniques were flawed.

Moreover, while Quirk is right to say that it will be *harder* to find more examples of “usable expert opinion,” it may still be easy in absolute terms. If my approach has worked well so far, why not keep trying and see where it takes us?

VIII. REPLY TO WITTMAN

Donald Wittman’s reply is encouraging. After years of debate, he and I have reached agreement on a long list of issues—and in almost every case,

Wittman has moved in my direction. In this response, I hope to change Wittman's mind about a few of our remaining disagreements.

1. *When Does Bias Matter?*

My empirical work highlights the fact that the public and economists systematically disagree. These disagreements are almost always *statistically* significant—in other words, they are extremely unlikely to arise by chance alone. Wittman (2008, 362) basically accepts this point: “Caplan is probably correct when he claims that there is a disconnect between the average voter’s beliefs about the economy and the average economist’s beliefs.” But he denies that the public’s biases are *economically* significant—in other words, that they are big enough to deserve serious attention.

In Wittman’s view, we should worry about voter bias only if “voters go one way on an issue while economists go the other way.” In other words, “one ultimately wants to know whether there is more disagreement than agreement between economists and the public.” He goes on to argue that it doesn’t matter if the public and economists disagree about the *size* of a problem: “The key issue is whether economists and the public agree that there is a problem, not whether they disagree about its magnitude” (Wittman 2008, 364).

Wittman should rethink his position. After all, he is a staunch defender of the Median Voter Model (Wittman 2005). According to this model, anything that shifts the *median* policy preference shifts policy to an equal degree. If getting a Ph.D. in economics would change the median policy preference, then the public’s lack of economic understanding changes policy. Furthermore, from the standpoint of the Median Voter Model, disagreement about the magnitude of a problem is a “key issue.” After all, people who think that a problem is “major” are almost certainly going to want more drastic action than people who think that a problem is merely “minor.” In terms of his own favorite model, then, there is no reason for Wittman to limit his attention to topics where there is “more disagreement than agreement between economists and the public.” At minimum, he should be concerned about any question where the *median* answers of economists and the public differ.²¹

On further reflection, though, even that would set the bar of relevance too high. Like almost all surveys, the SAEE forces respondents to take discrete positions. In a continuous world, this masks a great deal of

disagreement. Suppose, for example, that policy options range from 0 to 100, and that economists' preferences are uniformly distributed from 30 to 80, while the public's preferences are uniformly distributed from 40 to 100. Suppose further that a survey asks respondents whether they prefer 0 or 100, and respondents affirm the discrete response closest to their continuous preference. What happens? Three-fifths of economists will say "100"; so will five-sixths of the public. The median *survey response* of the two groups is the same. But the median *preference* of the two groups—the variable that really matters in the Median Voter Model—is very different. The median economist's preference is 55, versus 70 for the public.

An interesting implication: When surveys shoehorn continuous preferences into discrete responses, the *mean* survey response often tells us more about the underlying median preference than the *median* survey response. In my example, economists' average answer is 60, while the public's is 83. The 23-unit gap in mean response is moderately larger than the 15-unit gap in median preference. But the 15-unit gap in median preference is much larger than the 0-unit gap in median response.

Admittedly, Wittman could grant all these points, but respond, "I'm still not convinced that the disagreement between economists and the public is big." To get some perspective, I calculated belief gaps between laymen of the far left (very liberal Democrats) and laymen of the far right (very conservative Republicans). For the SAEF's 37 questions, the average absolute value of this belief gap is .30 on a 0–2 scale.²² I also calculated belief gaps between Ph.D. economists and the general public.²³ All else equal, the average absolute value of this belief gap is .52. In other words, the belief gap between economists and the public is more than 70 percent larger than the belief gap between America's far left and far right. If that isn't big, what is?

Every politically aware reader has some idea of how much economic policy would change if the median voter embraced the far left or far right. If the median voter thought like an economist, the change in economic policy would be even more radical.

2. *What Does the Act of Voting Show?*

Much of my argument hinges on the assumption that individuals *realize* that they are very unlikely to influence the outcome of an election. Wittman (2008, 373) questions this assumption:

The evidence points in the opposite direction: voters behave as if their votes were important. First, they vote, which is costly; if they thought their vote did not count, then they probably would not vote. Second, if they did not think their votes could make a difference, they would probably cast many more votes for minor-party and write-in candidates than they do.

There are several flaws in this argument. First, the mere act of voting is quite cheap: To vote in presidential elections you have to spend about an hour every four years. In contrast, *well-informed* voting is extremely expensive—you could easily spend all your waking hours studying policy and politics. It is hardly surprising, then, that voting is common, while informed voting is rare.²⁴

Second, influencing policy is far from the only reason to vote. People may vote for entertainment, out of a sense of duty, or simply to avoid the reproach of family and friends. Admittedly, these are also motives to inform oneself about politics. But to repeat, the cost of voting is small compared to the cost of being well informed. Motives strong enough to make people vote are rarely strong enough to make people vote well.

Third, there is a simpler reason why third-party and write-in votes are rare: conformism. People like to fit in; they don't want to take actions that brand them as misfits and oddballs. Furthermore, even if people had no intrinsic desire to conform, it's simply *easier* to do so. As Wittman (1995) emphasizes, information about front-runners is basically free; the media and politicians give you a lot of facts whether you want them or not. To vote unconventionally, in contrast, you have to do your own homework. If your vote can't change the outcome anyway, why bother?

3. *Miscellany*²⁵

a. *Do voters have less biased beliefs?* Wittman (2008, 365) muses:

We know from numerous studies that those who are the least informed are the least likely to vote. So perhaps voters, as opposed to survey respondents, do not have biased beliefs; or, more likely, they have less biased beliefs.

Caplan 2002b (429) tests this hypothesis. As long as we include my standard list of control variables, the economic beliefs of registered voters are

statistically indistinguishable from those of nonvoters. Admittedly, more-educated people (a) think more like economists and, (b) are more likely to vote. This is probably an important reason why democracy works as well as it does (Caplan 2007a, 156–58). But after we control for education and other confounding factors, voters are as biased as non-voters.

b. Antigovernment bias? Wittman is willing to admit that the public has an antimarket bias. But he suspects that the public has an antigovernment bias as well:

Caplan argues that the public has an anti-market bias and therefore chooses government rather than the market to resolve problems in the economy. I agree that his data show that the public has an anti-market bias. But I also see an anti-government bias in his data, and it is not clear which bias is greater. (Wittman 2008, 367)

Wittman points to two SAEF questions to support his case. According to my results, the public overestimates the economic damage of both high taxes and regulation. Don't these findings show that the public has an unreasonable aversion to government?

Taken in isolation, these questions about taxes and regulation are admittedly *consistent* with an "antigovernment bias." But I don't think this is the best way to interpret the evidence. The public overstates the negative effect of taxes (as well as the benefits of tax cuts), but these anti-tax views do not indicate a desire for smaller government. Consider: In 1996, the same year the SAEF was administered, the General Social Survey asked respondents:

If the government had a choice between reducing taxes or spending more on social programs like health care, social security, and unemployment benefits, which do you think it should do? (We mean all taxes together, including social security, income tax, sales tax, and all the rest.)²⁶

Only 40 percent wanted lower taxes; 60 percent wanted more spending. This is hardly what you would expect from people with an antigovernment bias.

So what is the public thinking? The most plausible story is that the public greatly overestimates the amount of *wasteful* government spending. Most people are convinced that "taxes could be significantly reduced without cutting back on popular government functions" (Caplan 2007, 57). In other words, people's bias is toward pessimism, not hostility to

government. The policy effects of this are unclear. Pessimism leads people to expect bad results, but it doesn't tell them to rely less on government and more on something else.

What about regulation? Quirk (2008, 339) suggests that some of the SAE's belief gaps reflect the public's "influence inflation." The regulation question is an especially convincing example of Quirk's concerns. In absolute terms, the public says the problem is more severe than economists do. In relative terms, though, the pattern sharply reverses. Out of 18 economic problems, economists say overregulation is the fifth most severe; the public, in contrast, puts it at sixteenth place. The upshot: It is hard to tell if the public is really more worried about regulation than economists are. We need more research to sort this problem out.

c. *Error or antipathy?* One of my main premises is that if people got rid of their biased economic beliefs, they would change their policy preferences, too. Wittman (2008, 367), in contrast, argues that biased beliefs are a mask for malevolent values:

Caplan does not show that the biases he has identified are fundamentally *economic* biases instead of more deeply held prejudices that are masked by false economic arguments.

I believe that dislike of "the other" drives many of the "economic" opinions Caplan investigates. . . .

These voters may be wrong about the economic effects of immigration, but their answers *correctly reveal their preferences*.

Take immigration. Maybe voters oppose immigration because they underestimate its economic benefits, as I claim. Another possibility, though, is that voters' economic misconceptions about immigration are a fig leaf for gut-level antipathy toward immigrants.

I can believe that Wittman's mechanism works for a small fraction of the population. But as a general explanation, it is psychologically implausible. How often can people sustain hatred without a foundation of descriptive claims? As far as I can tell, very rarely; normal people need ugly stories to keep their negative feelings alive.

In any case, Wittman's hypothesis is testable. We can regress policy preferences on measures of both values and beliefs. If Wittman is right, we would expect the effect of values to be larger than the effect of beliefs. At the very least, values should have a noticeable effect controlling for

beliefs. After all, even if Wittman's hypothesis were correct, beliefs would only be an *imperfect* proxy for values.

What happens if we take Wittman's mechanism to the data? Since he relies heavily on the example of immigration, this is a natural topic to put to the test. The General Social Survey asks respondents about their immigration policy preferences, their left-right ideology, and their beliefs about the effect of immigration on economic growth.²⁷ If you simply regress immigration preferences on ideology, ideology has substantial predictive power.²⁸ But if you add beliefs about the effect of immigration on growth to the set of independent variables, beliefs about growth turn out to be overwhelmingly important, and ideology's coefficient is no longer statistically significant.²⁹ At least for immigration, Wittman's story doesn't check out.

Of course, this is only a preliminary result on a single issue. Wittman's hypothesis may work better elsewhere. If he pursues this line of research, I will be curious to see what he finds.

For the time being, though, we should stick with the simple story that economic policy would drastically change if the public's biases disappeared.³⁰

NOTES

1. Strangely, though, BF (2008, 213) also tell us that "the theory of rational ignorance is logically airtight, like most theories of rational choice."
2. For further criticism of Kirznerian notions of "radical uncertainty," see Caplan 1999.
3. As Jeffrey Friedman pointed out at Critical Review's Conference on Political Ignorance and Dogmatism, the continued existence of spam suggests that some people fail to infer that they will probably lose their money. He is quite right; but this only shows that rational ignorance fails to describe the tiny minority of people who take spam seriously.
4. It better not be, because BF think that I have contradictory beliefs!
5. What I cannot understand, though, is why BF say that all of my claims depend on a single 1996 survey. I cite dozens of other surveys of economic beliefs that reach similar results (Caplan 2007a, 50–52).
6. In fact, *The Myth of the Rational Voter* specifically describes the median voter as a "relatively moderate" fanatic (Caplan 2007, 147).
7. In the General Social Survey, every additional year of education reduces the frequency of Biblical literalism by 4.4 percentage-points. (GSS variable identifier BIBLE.)
8. I also fully accept Colander's complaints about economists' communication skills; see Caplan 2007, 199–204.

9. May Wave 1 Questionnaire, 1996, Questions 24 and 24a. Available with registration at <http://brain.gallup.com>.
10. In fact, an earlier draft of the book contained a section on this self-referential problem. See <http://www.gmu.edu/departments/economics/bcaplan/selfref.doc>.
11. EL's main example is poorly chosen. While they say that "the Great Depression owed some of its depth and persistence to the dogmatic adherence of policy makers to the dominant economic doctrines of the day," a solid consensus of economists favored reflation and opposed higher tariffs. Many, if not most, economists emphasized the need to reduce real wages (Dorfinan 1959, 658–722). There is ample reason to think that these simple remedies would have worked if tried (Bernanke 1995).
12. EL (2008, 280) also ask "How does he [Caplan] know" that methodological strictures motivated Fernandez and Rodrik's approach? The most telling evidence comes from Rodrik 1996, 38, which repeatedly criticizes other researchers for "their habit of attributing myopia or irrationality to political actors—whether explicitly or, more often, implicitly ... " on purely methodological grounds.
13. Is it really morally permissible, for example, for Americans to ignore the massive benefits of immigration *for immigrants*?
14. My review essay (Caplan 2007b) on Tetlock points out several other objections to the anti-expert interpretation of his results. These include: (1) Many of Tetlock's questions are controversial among experts, which implies that that *average* expert cannot do well; (2) Tetlock crafts the response options to make "random" guessing easy; (3) When predictive accuracy is important, many experts (especially economists) rely on the winner of Tetlock's prediction horserace: formal statistical models.
15. The last statement specifically deals with minimum wage laws, but from the context it is clear that Gaus would say the same about almost any other economic policy issue.
16. GSS variable identifiers BIBLE, SCITEST4, and GOD.
17. For details, see Caplan 2007a, 51. The wording of the Worldviews question is exceptionally balanced:

It has been argued that if all countries would eliminate their tariffs and restrictions on imported goods, the costs of goods would go down for everyone. Others have said that such tariffs and restrictions are necessary to protect certain manufacturing jobs in certain industries from the competition of less expensive imports. Generally, would you say you sympathize more with those who want to eliminate tariffs or those who think such tariffs are necessary?

18. GSS variable identifier IMPORTS.
19. Caplan 2007, 142–53. Admittedly, we can construct theoretical examples where underestimation is harmless. Suppose, for example, that all trade agreements are good, but the median voter merely believes that the *average* trade agreement is good. If politicians have a binary choice to either (a) support all trade agreements, or (b) oppose all trade agreements, then political competition leads politicians to

- embrace free trade across the board. In the real world, however, politicians are not limited to binary choices; they can support the agreements the median voter favors, and oppose the rest.
20. Another possibility is that the public just likes hyperbolic language, leading to “semantic inflation.” The correction for semantic inflation proposed in Caplan 2002a, 443, could also be used to handle Quirk’s influence inflation.
 21. Strictly speaking, it would be better to compare the median answers of the Enlightened Public and the actual public.
 22. Three questions in the SAEF are not on a 0–2 scale; I rescaled them to preserve comparability.
 23. This is equivalent to the gap between the public and what my book calls the Enlightened Public.
 24. There is an interesting parallel in religion. Church attendance is common. Extensive knowledge of comparative religion—or your own religion—is much rarer (Wachlin 2004).
 25. I respond to Wittman’s arguments about irrationality in the market in Caplan 2005.
 26. GSS variable identifier TAXSPEND.
 27. GSS variable identifiers LETIN, POLVIEWS, and IMMECON. LETIN is on a 1–5 scale; POLVIEWS is on a 1–7 scale; IMMECON is on a 1–4 scale. Lower numbers for LETIN and IMMECON indicate more favorable views on immigration. Lower numbers for POLVIEWS indicate more liberal ideology.
 28. The estimated equation (t-stats in parentheses) is:

$$\text{LETIN} = 3.456 + .066 \text{ POLVIEWS}$$

$$(54.857)(4.57)$$
 29. The estimated equation (t-stats in parentheses) is:

$$\text{LETIN} = 2.414 + .043 \text{ POLVIEWS} + .429 \text{ IMMECON.}$$

$$(30.557)(3.160) (19.699)$$
 30. For additional evidence that biased beliefs about economics have a large effect on policy preferences, see my earlier reply to Wittman (Caplan 2005).

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