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Rejoinder to Wittman: True Myths

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CONTINUATION OF THE EXCHANGE BETWEEN BRYAN CAPLAN AND DONALD WITTMAN FROM THE APRIL 2005 ISSUE OF *EJW*.

[Caplan Comment on Wittman \(April 2005\)](#)

[Wittman Reply \(April 2005\)](#)

WITTMAN HAS WRITTEN A CHARACTERISTICALLY ENGAGING response to my critique. While he emphasizes his continued disagreement with me, I am struck by the important concessions he makes. In particular, he has virtually abandoned the rational expectations assumption that drives his trademark results. This retreat has a high price, because his new watered-down standards of rationality are consistent with vast democratic inefficiency. Apparently, democratic failure is not a myth after all.

Wittman's main theoretical effort to salvage his position is to shift a great deal of weight to arguments about discrete choice. While there is a kernel of truth to his approach, it is fairly easy to show that false beliefs about discrete choices are about as dangerous as false beliefs about continuous choices. Furthermore, existing empirical evidence contradicts Wittman's conjecture.

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Wittman correctly observes that I do not present any formal statistical evidence that people are more rational as consumers than they are as voters. I applaud his proposals for new empirical tests to resolve the matter. However, Wittman overlooks the direct approach: Design and administer tests of consumer knowledge comparable to existing tests of voter knowledge.

WITTMAN'S SURPRISING CONCESSIONS

Rational Expectations

The assumption that voters have rational expectations is central to Wittman's defense of democracy. Critics of democracy have argued, for example, that voter ignorance leads to wasteful pork barrel spending. But Wittman has repeatedly pointed out that if ignorant voters have rational expectations, this does not follow.

[A]ll their arguments rely heavily on some asymmetry in voter behavior. For example, Fiorina and Noll (1978) assume that the voters are aware of the benefits, but not the costs, of incumbent facilitation of constituents' needs. Shepsle and Weingast (1981) assume that the voters recognize the job gains from pork barrel in their district but underestimate the job loss from the sum total of pork barrel in all other districts. Clearly, such asymmetry is the driving force for too much pork barrel. But I have argued in the previous sections that the assumption of voter asymmetry is unwarranted; that is, imperfect information does not imply biased estimates. (Wittman 1989: 1410)

As far as I can tell, Wittman (2005) quietly repudiates his assumption that voters have rational expectations. He used to take it for granted; now Wittman (2005) states, "when we deal with levels instead of comparative statics, determining what behavior is irrational is extremely difficult, and there is likely to be little consensus on what is irrational" (29). Wittman (1995 and 1989) ably used rational expectations as a battering ram against

traditional public choice; Wittman (2005) is almost agnostic about whether it was true in the first place.

Wittman now distances himself from the standard predictions of rational expectations. If voters have rational expectations about the share of the budget devoted to foreign aid, for example, the public's average belief is supposed to approximately equal the true value.

If the average voter is informed, or has rational expectations, then the average voter will predict that foreign aid is 1% of the federal budget. If the average voter is uninformed and/or does not have rational expectations, then the prediction will be not 1%. (Wittman 2005, 28)

Wittman no longer expects this. In fact, he asserts that the prediction “makes no sense.”

One is comparing a point estimate to the whole space minus the one point. Obviously, it makes no sense to compare the two (as they are not the same thing) and stated this way, all that irrational expectations predicts is that the average estimate will not be 1%, which is not a prediction at all. (Wittman 2005: 28)

This is the most puzzling statement in Wittman's reply. To me, it obviously *does* make sense to compare the two. Economists test point estimates all the time—for example, that the effect of expected inflation on output equals zero. Admittedly, Wittman is correct if he means that you should not reject rational expectations just because the public's average belief is not *exactly* one percent; the difference has to be statistically significant. But what statistically literate person has ever argued that you should reject hypotheses for failing to work *perfectly*?

A valid concern that Wittman raises about tests of rational expectations is that some issues have a lower or upper bound. For example, foreign aid cannot be less than zero percent of the budget. Evidence of bias might, therefore, simply be a “statistical artifact” (Wittman 2005, 23). But Wittman misses the fact that bounded responses can mask biases as well as exaggerate them. For example, when the Survey of Americans and Economists on the Economy asks respondents to classify foreign aid as a major, minor, or non-reason why the economy is not doing as well as it otherwise would, economists cluster at “not a reason,” while the public clusters at “major

reason” (Caplan 2002a). If members of the public had more extreme options, some would probably endorse them, revealing the apparent lay-expert gap to be *understated*.

After dismissing standard tests of rational expectations, Wittman proposes an idiosyncratic alternative.

One solution is to have uniform priors on the set of possible irrationalities . . . so that irrational expectations predicts 50%, but then the actual average voter perception of 10% is much closer to 1% than 50% so we should reject the irrational expectations model in favor of the rational expectations model. And even if the set of irrational beliefs were confined to being less than 50%, uniform priors would suggest an expected irrationality of 25%, which is still further away from 10% than 1%.¹ (Wittman 2005, 28)

If I understand Wittman’s proposed test, then, we should “reject the irrational expectations model in favor of the rational expectations model” even if voters overestimate the budget share of foreign aid *by a factor of ten*. This dilutes the rational expectations assumption to the point of meaninglessness. Suppose I believe that I, personally, earn *twenty-four percent of world income*—about \$13 trillion dollars per year. This overestimates my true income by more than a factor of a hundred million. Wittman’s test would count my belief as evidence in favor of the rational expectations model, because twenty-four percent is closer to zero percent than it is to fifty percent.

In any case, if Wittman insists on this approach, he faces a serious problem. His new version of “rational expectations” implies nothing about democratic efficiency. The authors that Wittman (1995 and 1989) criticized now have a simple response. Fiorina and Noll (1978), to take only one case, could now object: “Of course we assume rational expectations. In our

¹ Wittman seems to mistake my hypothetical ten percent figure for an actual data point. The original text (Caplan 2005, 10) reads: “If the average response of a representative sample is 10 percent, there is strong evidence that the public systematically overestimates government spending on foreign aid. Empirical work along these lines finds large systematic errors on important questions. For example, the National Survey of Public Knowledge of Welfare Reform and the Federal Budget (1995) reports that the public heavily overestimates the share of the federal budget devoted to welfare spending and especially foreign aid, and underestimates the share going to Social Security.” Since Wittman showed me a draft of his manuscript, I share responsibility for his misinterpretation.

model, voters think that pork barrel spending is free, and that is much closer to the truth than the assumption that pork barrel spending is 50 percent of GDP.” The same goes for all the other writers Wittman faults for equating ignorance and bias.

Elsewhere in his reply, Wittman seems to drop rational expectations altogether in favor of a much weaker benchmark of rationality.

The main way that economists have tested consumer rationality is to see whether demand goes down when price goes up. If demand curves were upward sloping, that would be evidence for consumer irrationality. Because they don't, we are pretty confident that consumers are rational. The same type of test should be employed to see whether voters are rational. Do they have (weakly) downward sloping demand curves? (Wittman 2005, 26)

I agree that this is *one way* that economists have tested for consumer rationality, though since the rational expectations revolution of the 1970s it has not been the “main way.” In any case, the problem with the “demand-slopes-down” standard of rationality is that it is consistent with massive democratic inefficiency.² Take Fiorina and Noll's (1978) assumption that voters treat pork barrel spending as free. If the cost of pork doubles, voters' desired quantity stays the same, because they still think it is free. These voters, therefore, have a (weakly) downward-sloping demand curve. But the political equilibrium is inefficient, because voters eagerly support candidates who deliver goods that cost them far more than they are worth.

The bottom line is that Wittman can no longer derive his conclusion that democracy is efficient. If he is agnostic about rational expectations, he has to be agnostic about democratic efficiency. If he adopts another benchmark of rationality, rationality no longer implies democratic efficiency. Either way, Wittman cannot stand by his original democratic optimism.

Admittedly, just because democracy falls short of the happy picture that Wittman (1995 and 1989) painted, it does not follow that markets are better. If we can test the rationality of voters and find them wanting, it is also possible to discredit the rationality of consumers. Although Wittman

² It is worth mentioning that demand is supposed to have a negative slope with respect to *private* cost—the price that the consumer actually pays. But many of Wittman's arguments seem to assume that voter demand has a negative slope with respect to *social* cost—the price that society as a whole pays.

does not provide or cite any statistical evidence to this effect, I find his example of medical quackery fairly convincing.³

Suppose I undertook a parallel test of consumer rationality. One might ask consumers whether homeopathic medicine works, Ginko Biloba improves memory, Echinacia prevents colds, and colloidal silver helps the immune system. I suspect that the answers provided would differ greatly from experts at the major medical schools and the National Institute of Health and that consumers' understanding of many medical matters was greatly off the mark. (Wittman 2005, 26)

Wittman could appeal to a great deal of research in behavioral economics for additional support (Rabin 1998; Thaler 1992). However, he is too hasty to put me in a "quandary."

Either he [Caplan] agrees that this data implies that consumers are irrational, thereby agreeing with Lenin that neither economic markets nor democracy works because the actors are irrational, or he believes that this data does not prove that consumers are irrational, thereby undermining his parallel evidence that voters are irrational (unless he can find a very clever way of distinguishing between the two irrationalities). (Wittman 2005, 26)

Wittman strangely ignores the fact that irrationality is a question of degree.⁴ You do not have to become a Leninist just because you find that both consumers and voters have some beliefs that are irrational to some

³ It is possible, however, that people who *underestimate* the benefits of alternative medicine—most obviously, by mistaking useless treatments for harmful ones—are as common as people who overestimate the benefits.

⁴ Wittman raises this issue later in his reply: "[T]here is often a counting problem. We can point to instances of irrationality, but we can also point to instances of rationality. If we are forced to assume either that people are always rational, or always irrational, because we have no good way to predict when one is operative, we will have to choose the hypothesis that works best over all cases. This means considering all cases, not just providing examples that fit with our notions (rational voters for Wittman; irrational voters for Caplan). This is a hard thing to do and people make little effort in this direction. This problem has plagued the debate." But economists (along with other scientists) overcome such "counting problems" all the time. There is no perfect solution, but this does not stop authors in, e.g., the *Journal of Economic Literature* from summarizing the "overall results" in a field.

degree. For example, my empirical research on beliefs about economics did not find that the public has a few mild biases. It found that the public has large biases on a long list of policy-relevant topics. As a rule, voters heavily underestimate the benefits of markets, especially international and labor markets.⁵ Wittman has not shown—nor, to the best of my knowledge, has anyone else—that consumers suffer from comparable defects. The hundreds of millions of dollars Wittman says consumers waste on “worthless cures” are a small fraction of GDP.

Incidentally, which side of the preceding “quandary” would Wittman choose for himself? Does he dispute the medical consensus against homeopathy? Or does he, like me, side with the experts, and conclude that consumers are wasting their money? If he is willing to trust experts in other fields, why not his own? When economists and the public disagree about the benefits of free trade, for example, why is Wittman so reluctant to say “We’re right, they’re wrong”?

His answer, as best as I can tell, is that economists correctly maintain that free trade is good for the economy *as a whole*, but non-economists who oppose free trade correctly maintain that free trade is bad for them *personally*.

[S]ome differences are to be expected. A very large percentage of economists are in favor of free trade. But it would be irrational for all voters to be in favor of free trade as a great number of voters are hurt by it. (Wittman 2005, 25)

But this story does not fit the facts. Even if you specifically ask about the effects of free trade on the economy as a whole, non-economists are much more negative than economists (Blendon et al. 1997). One could argue that their belief gap stems from self-serving bias: Those who benefit from free trade convince themselves that it is good for society, and those who lose convince themselves of the opposite. But this story does not fit the facts either, because large lay-expert belief differences persist controlling for income, income growth, job security, and other measures of self-

⁵ Wittman (2005, 23) says that “It would be interesting to discover whether the survey response of those who regularly vote differs systematically from those who vote rarely, if at all.” Caplan (2002b, 429) finds that controlling for other characteristics, the beliefs of registered voters differ from those of non-registered voters less often than would be expected by chance.

interest⁶ (Caplan 2002a). Wittman may not want to take a side when economists and non-economists disagree, but he does not have much choice.

Rational Irrationality

Wittman is surprisingly willing to accept my premise that consumption of irrationality is sensitive to its price.

There is little cost to being misinformed when *your choice* would be the same if you were informed... It would make little sense for strong vegetarians to stay abreast of the latest research on meat. . . . As a result, strong vegetarians might be misinformed, possibly holding irrational views, about meat. (Wittman 2005, 23)

But he refuses to accept my deduction: “this is to be distinguished from one of Caplan’s arguments, with which I disagree, that voters make irrational choices because their choice will not affect the outcome” (2005, 23).

I want to know *why* Wittman disagrees. He accepts the premise that rationality depends on incentives. Does he believe that individual voters *do* have a substantial effect on political outcomes? If not, Wittman has at least as much reason to accept my claim than his own. If a voter does not bother being rational when it would not change his *conclusion*, one would also expect the voter to not bother being rational when it would not change *policy*.

Wittman’s disagreement is all the more puzzling because my rational irrationality model is consistent with one of his main challenges to critics of the rationality assumption.

⁶ Caplan (2002a) also suggests a simple explanation for Wittman's evidence about the political orientation of Harvard, Yale, and Princeton faculty: ideological bias. The belief gap between economists and the general public persists controlling for party identification and ideology. The gap between Harvard/Yale/Princeton faculty and the general public would be far less robust to these controls, because in a random sample, there is little correlation between education and party identification (Caplan 2001).

If most people's cognitive abilities are so poor that they cannot achieve what they want, why have they been able to survive in the presence of superior beings who occupy a similar ecological niche? Hunters (human or otherwise) who incorrectly estimate the likelihood of finding food in various locations will have a lower chance of survival and ultimately lower reproductive success than those who make correct estimates. (Wittman 1995, 60)

My response is that false beliefs about policy, unlike false beliefs about how to find food, have essentially no effect on an individual's chance of survival or reproductive success. Why does Wittman find this answer unsatisfactory?

WITTMAN'S DISCRETE RETREAT

Wittman (1995 and 1989) relied heavily on the assumption that voters have rational expectations. Wittman (2005) argues that he may never have needed this assumption, because severely biased voters hold the same positions as they would have if they did have rational expectations.

[P]eople who greatly overestimate are against foreign aid and would still be against foreign aid even if they were informed of the true value. If this is the case, there is little cost to their being uninformed since they would take the same position (reduce foreign aid) even if correctly informed. (Wittman 2005, 24)

Wittman gives a striking example from the 2004 election.

According to an October 21, 2004 Harris Poll, 52 percent of those who preferred Bush thought that Saddam had helped plan and support the hijackers who attacked the U.S. on September 11 (it was 23 percent for those who preferred Kerry) and 58% of those who preferred Bush thought that Iraq had weapons of mass destruction when

the U.S. invaded (it was 16% for those who preferred Kerry). Neither of these assertions is true. . . . Now some might say this would demonstrate how irrational Bush supporters are, but I think it is entirely rational. So let us try a little thought experiment. If you were strongly in favor of one of the candidates, and then you found out that you were wrong about several facts regarding the candidate, would you be in favor of the other candidate? If the answer is no, then why bother checking your facts in the first place, as it is unlikely to alter your vote. (Wittman 2005, 24)

Part of Wittman's story is correct. When people face discrete choices, their behavior *often* remains the same when conditions change. But Wittman ignores an equally important feature of discrete choices: Some people's behavior changes *drastically* when conditions change *slightly*. If a car manufacturer raises the price by \$100, most people who were going to buy the car still do. So why not raise the price? Because that \$100 price hike leads some people to buy zero cars instead of one. When you net the two effects, there is no reason to think that demand for discrete products is less price elastic than demand for continuous products.

Similarly, Wittman is arguably correct that most Bush supporters would have continued to support him even if they abandoned their foreign policy misconceptions. But you do not need *most* supporters to change their favorite candidate in order to change the result of the election. In fact, if merely 2.5 percent of Bush supporters would have changed their vote upon learning the truth about Iraq, Kerry would have won the popular vote. Of course, neither Wittman nor I would expect Bush to take this lying down. If voters lost their biases about Iraq, costing Bush 2.5 percent of his votes *holding his policies constant*, his natural response would be to change his policies—for example, by not invading Iraq.

Incidentally, this suggests an answer to an objection that Wittman did not explicitly raise: Since the median economist is politically independent and ideologically moderate, policy would remain roughly the same even if all voters thought like economists.⁷ The problem with this argument is that economists do want large *policy* changes; they just happen to be policy

⁷ Similarly, he could have argued that since education makes people think more like economists, but correlates weakly with party identification, policy would remain roughly the same even if all voters were well-educated.

changes that both parties currently oppose. If the median voter turned into an economist, politicians of all parties would revamp their platforms to curry his favor.

Wittman's reply is supposed to be exploratory, so it would be churlish to ask for empirical evidence in support of his claim that biased beliefs have little or no effect on policy preferences. However, it is fair to point out that my original article surveyed evidence that contradicts Wittman's speculation. My own empirical research admittedly takes the link between biased beliefs and policy preferences for granted. But the "enlightened preference" literature does not. It demonstrates precisely what Wittman denies—individuals' policy preferences systematically change when their knowledge increases (Althaus 2003; Caplan 2005, 10-11).

Admittedly, people's support for some policies *seems* extremely stubborn. The minimum wage is a prime example. In my experience, it is hard to raise doubts even in a captive audience of freshmen. Is it possible that Wittman's claim is at least correct for policies like the minimum wage that enjoy deep-rooted support? A well-designed 1996 Gallup poll suggests that the answer is no.⁸ 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 disemployment effect of the minimum wage, the point is that its popularity depends heavily on beliefs about its effects. Policy preferences are stubborn in large part because beliefs about what works are stubborn, and not—as Wittman argues—the other way around.

⁸ May Wave 1 Questionnaire, 1996, Questions 24 and 24a. Available with registration at <http://brain.gallup.com>.

WITTMAN'S EMPIRICAL CHALLENGES

The Best of Wittman

Wittman's best point is that "voter rationality and consumer rationality should be tested in the same way and compared" (2005, 25). I agree. I have focused on voters' beliefs because that is where my experience led me to suspect that irrationality would be easiest to find. Running parallel tests on consumers' beliefs is the next logical step for research in this area to take.

The challenge is to credibly create questions of comparable difficulty. A determined critic of consumer rationality could probably devise questions hard enough to generate systematic errors similar in size to those I report (Caplan 2002a). He might even make the circular argument that if the errors are not similar in size, the consumers' questions must be too easy. A defender of consumer rationality would face the opposite temptation.

For example, I would like to compare consumers' beliefs about their own budgets to voters' beliefs about the governments' budget. That seems like a reasonable comparison to me, but I doubt that Wittman will be impressed if consumers, unlike voters, know where their money goes. Voters frequently think they spend more on foreign aid than national defense. Will Wittman give consumers credit if they realize they spend more on housing than charity?

I can think of two solutions. One is to use a survey designed for orthogonal purposes. For example, according to the authors of the Survey of Americans and Economists on the Economy (Blendon et al. 1997), their *aim* was not to find the areas where economists and the public disagree the most. They selected topics primarily because they had long been in the news and popular discussion. A survey of consumers' beliefs with a similar motivation might let us sidestep controversy about question selection.

Another approach is for scholars who disagree about consumer rationality to join forces to write a mutually acceptable survey. Perhaps it is naive to hope for a meeting of the minds, but it is easier to reach a consensus *ex ante* than *ex post*. A test of consumer rationality jointly written by Donald Wittman and Bryan Caplan would have more credibility than two studies of this question that we ran independently.

Readers may be wondering: Hasn't behavioral economics (Rabin 1998; Thaler 1992) already marshaled a great deal of evidence against consumer rationality? Unless he has changed his position, however,

Wittman is *less* impressed by this literature than I am (Wittman 1995, 38-61). My main reservation about behavioral economics is that experimental conditions are usually too different from real-world conditions to confidently move from one to the other. In contrast, Wittman calls it a “hodgepodge of contradictory results rather than an intellectual foundation” (1995, 38). Since his response to my research on voter irrationality is markedly less negative, perhaps he should tentatively adopt my view that consumers are more rational than voters, pending the arrival of better data.

Wittman’s Eight Hypotheses

Wittman is a constructive critic. He does not merely argue that I am mistaken; he advances eight testable hypotheses to help resolve our dispute. Since he has gone to this trouble, I now consider their merits one by one.

Hypothesis #1: Those people who overestimate the cost of a program (say foreign aid) are more likely to be against the program than those people who underestimate it, both before and after they are given the true facts of the situation.

The “before” part is trivial. Obviously, the worse a person thinks a program is, the less he supports it. The “after” part is not trivial, but I have some concerns. First, one of my main claims is that people irrationally fail to change their beliefs when you give them the “true facts.” Indeed, unless you have a solid gold reputation, perfectly rational agents could easily doubt that you really are the bearer of true facts. So Wittman’s hypothesis should be revised to say “after they *accept* the true facts of the situation.”

My other concern is that hypothesis #1 is weaker than the surrounding text, where Wittman suggests that debiasing will have *no effect* on policy preferences. Existing evidence contradicts this claim; all else equal, voters who know more want systematically different policies. (Althaus 2003) But as written, hypothesis #1 requires only that debiasing fails to make the policy preferences of the initially biased *match* those of the initially unbiased. I do not strongly disagree with this weaker claim, nor does my theory commit me to it. Maybe support for protection is 50 percent due to irrational beliefs, and 50 percent due to expressive voting (Brennan and Lomasky 1993).

In sum, hypothesis #1 is *not* inconsistent with existing evidence, but it is not inconsistent with my position either. If Wittman revises hypothesis

#1 to state, “people will not change their policy preferences after debiasing,” I disagree. But existing evidence already shows that I am right.

Hypothesis #2: Republicans are more likely to overestimate the cost of welfare than Democrats.

This hypothesis is trivial. Almost every model predicts it. For example, if voters have rational expectations about the cost of welfare, some will overestimate it and others will underestimate it. The over-estimators will be more likely to support the party that wants less welfare; the under-estimators will be more likely to support the party that wants more welfare.

A non-trivial substitute for Hypothesis #2 is that, “Independent voters will have unbiased beliefs about the cost of welfare.” I predict the opposite. But to be fair, existing data already confirm that I am correct.

Hypothesis #3: Voters who are strongly in favor of one candidate are likely to have biased beliefs favoring that candidate, but when such voters are informed of the truth, they are unlikely to prefer the other candidate.

This hypothesis suffers from several ambiguities. Again, instead of “informed of the truth,” Wittman should say “accept the truth.” Furthermore, does “the truth” mean the truth on *one specific issue*, or *overall*? If supporters of a candidate abandon their false beliefs about one minor issue, for example, I would not expect many to switch teams either. On the other hand, I definitely predict that correcting many small biases, or a few important biases, would have an effect on voter choice. Bartels (1996) already uses the enlightened preference method to confirm that voters’ false beliefs have a substantial effect on vote shares. And as stated earlier, this underestimates the effect of voters’ biases because politicians would adjust their platforms to appeal to a more rational electorate.

One related test that Wittman does not consider would be to study people who actually change their minds about factual issues, and see whether they subsequently change their vote. Take, for example, Republicans who used to think that Iraq had WMDs, but changed their mind before the election. Were they more likely to vote against Bush than other Republicans, controlling for other variables (including strength of partisanship)? This issue is sufficiently important that I predict they would. Does Wittman disagree?

Hypothesis #4: When the cost of a policy increases, voters on average will be less likely to vote for the policy.

This hypothesis suffers from an ambiguity between objective and subjective cost. I agree that when the *perceived* cost of a policy increases, voters will be less likely to support it. For Wittman to distinguish our positions, he would have to predict that when the *actual* cost of a policy increases, voters will be less likely to support it. Even then, however, this is not a good test of my approach.

First, I am not aware of any data suggesting that voters have biased beliefs about *changes* in costs. It would not surprise me if they did, but this should be confirmed before we test hypothesis #4.

Second, even if voters have highly biased beliefs—for example if everyone believes that cost changes are twice as large as they really are—their behavior passes Wittman’s test. To fail his test, changes in perceived and actual costs would have to be negatively correlated! But milder biases still imply inefficient choices. If the price of gas rises by 25 cents, but you believe it has risen by fifty cents, you reduce your consumption by an inefficiently large amount—without violating the law of demand. Similarly, if the price of prescription drugs rises by one percent, but voters believe it has risen by two percent, they reduce their support by an inefficiently large amount. Wittman’s fourth hypothesis sweeps these inefficiencies under the rug.

Hypothesis #5: Voters do not have significantly more money illusion than consumers and workers.

Fair enough. I predict the opposite, and agree to become marginally less confident in my overall position if proven wrong. Of course, since this is only one topic, it would not be reasonable for either Wittman or me to surrender to the other on the basis of this test.

Hypothesis #6: Scan the brain and see whether voters use more primitive centers of the brain when voting than when making purchases.

Despite the rising profile of neuroeconomics (Camerer et al. 2005), this test raises more questions than it answers. Wittman of all people should presume that the division of cognitive labor in the brain is functional. If we use our “primitive centers” to form political beliefs, why not conclude that these are the optimal centers to use for this purpose? It is at best premature

to equate the output of the “primitive centers of the brain” with irrationality. In fact, since lower animals do not hold political ideologies, my guess is that ideological thinking uses centers of the brain unique to man.

In proposing this test, Wittman makes a fascinating admission. “One would have to control, however, for the possibility that people get more excited about politics than about what clothes to wear (at least this is true for the people that I know).” It is true for the people that I know, too, but we should not control for it. Maybe the *reason* why people hold irrational political beliefs is that they let their excitement cloud their judgment.

Wittman’s last two hypotheses refer to the following proposed experiments.

In experiment 1, the subject gets the payoff from A, B, C or D if he chooses A, B, C or D. Further, he gets the highest monetary payoff if he chooses D, but somehow the experiment is designed so that it takes complicated logic for the person to understand that the choice should be D. In experiment 2, the subject gets the payoff from A, B, C, or D if a majority chooses A, B, C or D. (Wittman 2005: 30)

Hypothesis #7: The majority decision will, on average, be more accurate than the individual decision.

Hypothesis #8: The larger the number of potential voters, the more accurate the decision is likely to be. If Caplan’s argument is correct, then individuals will be more irrational because they are less likely to have an effect on the outcome.

This is a valuable approach, but I have a major reservation. People are far less likely to have strong emotions about choices labeled A, B, C, or D than they are about real political issues such as tariffs and the minimum wage. In fact, even if you used familiar emotionally-charged labels, they would not pack their usual punch in an experimental setting. I abhor murder in real life, but have slaughtered millions in computer games.

If experimental subjects lack strong emotions about their choices, I find Wittman’s prediction plausible. The average guess of the weight of an ox is notoriously accurate, and large groups are more accurate than small groups (Surowiecki 2004, xii-xiii). Wittman’s proposed experiment makes beliefs about policy no more emotionally engaging than beliefs about the

weight of an ox. It would not be surprising if democracy performed well in this case.

Still, there is a variant of Wittman's experiment that I would find probative. Pick topics where we *already know* that beliefs are biased, even *under experimental conditions*. Monty Haul's Three Doors problem is a good example (Friedman 1998). Give subjects the opportunity to either have fun or research the question—for example, by giving them a half hour of Internet access. Do not pressure subjects to solve the problem; for example, tell them “When you are satisfied that you have the correct answer, feel free to browse the web until your time is up.” Then collect their answers. I predict that the experiment with individual rewards will reveal substantially less biased beliefs than the experiment with collective rewards. I also predict that democracy will do worse as the number of voters rises.

One last worry about Wittman's experimental design is that it seems to *simultaneously* test (a) the relative merits of markets versus democracy *and* (b) the relative merits of plurality rule versus proportional representation.⁹ Suppose that consumers are substantially more accurate than voters. When people choose for themselves, 40 percent choose D, and A, B, and C get 20 percent each; when they vote, 28 percent choose D, and A, B, and C get 24 percent each. Under proportional representation (voters get 28 percent of the D payoff, plus 24 percent of the A, B, and C payoffs), markets have higher average payoffs than democracy. Under *plurality rule*, however, democracy has a perfectly efficient outcome, because everyone gets 100 percent of the D payoff.

Wittman might reply: “That's my whole point.” But the putative aim of his experiment is to test whether consumers are more rational than voters, not whether democracy out-performs markets holding rationality constant.¹⁰ More importantly, plurality rule yields bimodal outcomes; it is easier to get 100 percent efficiency, but also easier to get 0 percent efficiency. If voters were systematically biased against D, then plurality rule

⁹ Since there are four options in Wittman's proposed experiment, he should have said *plurality* rule, not majority rule.

¹⁰ Wittman defends the latter position elsewhere: “False political advertising may fool a minority, yet it will have no harmful effect since votes for the minority will not be translated into political power. In contrast, a business does not have to persuade a majority of consumers, only a few, to have any sales. So a majority may want to protect a minority in the commercial arena” (1995, 16-17). Wittman's conclusion would reverse, however, if the majority were wrong. Then markets at least let the rational minority take the optimal action, but democracy forces a bad decision on everyone.

would *never* select it, leading democracy to perform worse than markets even if consumers are equally biased. The simplest solution is to use proportional representation, not plurality rule, to rate democracy's performance.

Some of Wittman's hypotheses are trivial or irrelevant, some are already known to be false, some would be good after reformulation, and one is fine as written. By no means is that a bad track record; creating good testable hypotheses is not easy. But before pursuing any of Wittman's hypotheses, we should heed his simpler exhortation—test voter rationality and consumer rationality in the same way. Existing research confirms the reality of large systematic biases about policy-relevant questions. For purposes of comparison, the next step is to write a good test of consumer knowledge, and see if large systematic biases frequently emerge there too.

CONCLUSION

Wittman (2005) gives a great deal of ground, but it is not obvious whether he has really changed his mind, or is playing devil's advocate. The answer is probably a mix of both, but he makes important revisions that appear genuine. In particular, Wittman retreats from the rational expectations assumption that drives so many of his results. When Wittman (1995 and 1989) challenged the political failure literature for assuming "voter irrationality," his evidence was that voters need biased beliefs to generate the standard conclusion. But now he says,

The major method of testing rationality of consumers is via comparative statics and in particular the test of (weakly) downward sloping demand. This should be the prime method of testing rationality of voters, as well. (Wittman 2005, 27)

Wittman used to set the bar of rationality extremely high; now he sets it extremely low. If downward-sloping demand is all that "voter rationality" means, then Wittman's original challenge to the political failure literature was off the mark. Though many models of political failure require what Wittman calls "extreme voter stupidity," I am aware of none that assume that voters want more when the perceived cost of a policy rises.

Wittman's main defense of his original position is that biased beliefs do not change people's policy preferences. While there is a kernel of truth here—a discrete choice is more likely to stay the same when conditions change—it is basically wrong. Discrete choices are also more likely to change sharply when conditions change. The net effect is ambiguous. Furthermore, existing empirical evidence shows that biased beliefs have systematic effects on policy preferences.

Wittman is on firmer ground when he criticizes my views instead of defending his own. Econometric evidence reveals that voters have deeply biased beliefs about the economy. But are voters more biased than consumers? I *believe* that they are. When I listen to non-economists discuss their consumption behavior, I find them remarkably insightful; when I listen to non-economists discuss policy, I find them disturbingly obtuse. And in my experience, learning economics often drastically changes people's political and economic outlook, but has only marginal effects on their personal behavior. That was how it worked for me.

Still, I can hardly expect my casual empiricism to convince Wittman, and to the best of my knowledge, there is no econometric evidence on consumers' beliefs comparable to my research on voters' beliefs. One response to this lacuna would be to pursue Wittman's eight hypotheses, but designing and administering tests of consumer rationality to parallel existing tests of voter rationality has a higher rate of return. If Wittman and I can reach a consensus on what these tests should contain, the answer to one of the biggest of the Big Questions—the relative merits of democracy versus the market—is within our reach.

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