

Do Belief Systems Exhibit Dynamic Constraint?

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Abstract

As described in Converse (1964), belief systems are dynamically constrained if a change in one ideaelement causes a concomitant change in a related idea-element. While an enormous literature is dedicated to the study of static constraint (the extent to which individuals hold political views that “go together”), dynamic constraint is rarely studied, especially using experimental research designs. We offer a new formalization of the theoretical argument, evidence from survey experiments conducted with online convenience samples, a nationally representative sample, and a convenience sample of political elites, coupled with an analysis of aggregate time-series of opinion change across issue domains. Our results indicate that even among subjects whose belief systems are highly constrained in the static sense, a change in one attitude does not necessitate changes in related attitudes. These experimental results affirm and extend Converse’s thesis about the limited extent of dynamically constrained ideological thinking in the mass public.

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A half-century after its publication, Philip Converse’s (1964) essay “The Nature of Belief Systems in Mass Publics” remains at the forefront of public opinion scholarship. Its provocative thesis is that unlike “elites” (elected officials and party activists), members of the “mass public” (ordinary voters) typically hold political opinions that are a loosely structured *mélange* of liberal and conservative ideas. Members of Congress, for example, hold opinions that predictably reflect the worldview of either the left or right (Bafumi and Herron 2010). Conservatives in Congress resist social entitlement programs, support law-and-order policies, and seek to promote patriotism and traditional social values. Congressional liberals, by contrast, seek to expand the social safety net, promote the rights and well-being of disadvantaged groups, express concern about excessive police or military force, and challenge traditional values and the hierarchies that they engender. Among voters, these liberal or conservative ingredients are often jumbled together. On the whole, the public appears relatively moderate when their policy views are averaged across different issue domains, but as Ahler and Broockman (2017) point out, citizens who are centrist on average tend to support many policies that are not moderate.

Converse coined the term “constraint” to describe the interconnectedness of different political opinions. In a widely-quoted passage (p.2), he defines a “belief system” as “a configuration of ideas and attitudes in which the elements are bound together by some form of constraint or functional interdependence.” The coherence of a belief system may be assessed at a given point in time or by tracing its elements over time. In reference to the former, Converse (p.3) uses the term “static” constraint “to mean the success we would have in predicting, given initial knowledge that an individual holds a specified attitude, that he holds certain further ideas and attitudes.” Converse’s empirical assessment of static constraint demonstrates the “fragmentation” and “narrowness” of mass belief systems (p.54) by showing the weak correlations between survey responses to questions about assorted policy topics (p.31). In the decades since the publication of Converse’s essay, static constraint has remained the focus of the vast empirical literature on belief systems.¹ Scholars have pointed out that static constraint in the mass public may be underestimated when gauged by simple correlations (Achen 1975; Ansolabehere, Rodden and Snyder 2008), that constraint appears to operate more strongly now than in Converse’s day (see Freeze and Montgomery (2016) and Abramowitz and Saunders (2008) on mass constraint), and that mass constraint may be higher outside the United States (Converse and Pierce 1986; Malka,

¹In addition to static constraint, another of Converse’s methods for assessing ideological awareness is use of liberal and conservative parlance, which has also generated a large literature on the meaning and measurement of liberal-conservative self-identification (Jacoby 1991; Bauer et al. 2017)

Lelkes and Soto 2017). Nevertheless, it remains the case that in both absolute terms and in comparison to elites, ordinary voters hold opinions that are weakly correlated across different policy domains. In a recent analysis, Freeder, Lenz and Turney (2017) estimate that only approximately 20 to 40% of the U.S. mass public hold opinions that are constrained in the static sense.

Much less attention has been paid to what Converse calls “dynamic” constraint or “interdependence.” This characteristic of belief systems concerns “the probability that a change in the perceived status (truth, desirability, and so forth) of one idea-element would psychologically require, from the point of view of the actor, some compensating change(s) in the status of idea-elements elsewhere in the configuration.” (p.3) Converse contends that dynamic constraint operates weakly if at all among ordinary voters, who tend to acquire new policy opinions via “social” transmission (p.8) rather than through a principled reconciliation of new and old opinions. As a consequence, mass belief systems tend to span a narrow range of policy domains (p.5), too narrow to imbue election outcomes with meaning as ideological mandates (pp.58-64).

The data requirements for assessing *dynamic* constraint are more demanding because the measurement process requires an initial change in an “idea-element” whose ramifications can be traced within and across policy domains. For studies of this sort to generate statistically meaningful conclusions, the initial change must be genuine (not a sampling fluke), sizable (so that its ramifications can be detected with reasonable precision), and brought about by domain-specific causes (so that concomitant changes in policy views across domains cannot be attributed to environmental forces that *directly* affect opinions in multiple domains). Converse (1964, 1970) did not offer direct evidence of this kind. In those rare instances where other scholars have assessed the degree of dynamic constraint in mass opinion, they have leveraged seismic political events, such as the abrupt change in U.S.-Soviet relations under Perestroika (Peffley and Hurwitz 1992), the 9/11 terrorist attacks (Kinder and Kam 2010), or the tax revolt of the late 1970s and early 1980s (Sears and Citrin 1982). Of these studies, only Peffley and Hurwitz (1992) both addressed the topic of dynamic constraint directly and used panel data to track individual attitudes over time. To our knowledge, our study is the first to assess dynamic constraint experimentally.

We aim to revive this neglected strand of Converse’s seminal work on the nature of belief systems. We assess the degree of dynamic constraint in a novel way, by experimentally inducing opinion change in some domains and tracing the concomitant changes in other domains that may occur. We complement these experiments by examining the dynamics of aggregate opinion, again assessing whether policy preferences change in parallel within or across policy domains.

Consistent with Converse’s argument about the narrowness of mass belief systems, our experiments show that change in one idea-element tends to precipitate little or no change in other cognate issue domains. When dynamic adjustment occurs, it operates narrowly (within the same policy domain) and dissipates quickly. This finding led us to wonder whether respondents are indifferent to pressures to express views that are consistent with what they have said in previous questions. In a further series of experiments, we find that when respondents’ inconsistent views are called to their attention during the course of an interview, respondents subsequently become more consistent in the political positions they express; however, this effect dissipates within a few minutes. Taken together, our experiments underscore Converse’s contention that apparent static constraint may actually exaggerate the functional interdependence of a belief system. Even when respondents display a high degree of consistency in their policy opinions across domains, opinion change occurs in a localized and fragmented fashion. We conclude by considering some of the macro-level implications of weak dynamic constraint, focusing in particular on the disjointed way in which changes in “policy mood” (Stimson 1991) unfold, and by proposing a mechanism to explain why elites’ static constraint exceeds their dynamic constraint.

Formalizing the Empirical Implications of Dynamic Constraint

Unlike constructs such as conservatism, trust in government, or political knowledge, dynamic constraint is not a psychological trait that lends itself to measurement via standard psychometric tools. We cannot simply quiz people repeatedly about their opinions or beliefs in order to zero in on the latent trait of interest. Instead, dynamic constraint must be detected by tracing the ripples of some exogenous source of opinion change. For example, if a person becomes convinced that government spending is rife with corruption and waste, does she become more supportive of tax cuts designed to trim the fat in government (Sears and Citrin 1982)? The range of the belief system is suggested by the distance between the initial locus of change and responses to questions about less directly related topics. For example, does this respondent also become more supportive of a constitutional amendment to require a balanced budget?

Although evidence of such ripple effects would seem to suggest a dynamic process by which people maintain consistency among the various components of a far-reaching belief system, this interpretation hinges on additional assumptions about the underlying process that leads to opinion change across different domains or across different

facets of the same domain. The purpose of this section is to make these assumptions explicit.

In order to formalize the observable implications of dynamic constraint, we posit a set of four variables, only some of which are directly observed. These are:

I (unobserved). Ideology, or the set of functional interdependencies that may influence political attitudes.

Y_A (measured). Attitude A, as measured by the response to a survey question on issue A.

Y_B (measured). Attitude B, as measured by the response to a survey question on issue B.

Z_A (manipulated or set by nature). Information A, which is a causal factor that is directly relevant to issue A but not directly relevant to issue B. Z_A is the crucial factor that may set in motion the change in Y_A that may or may not be accompanied by a change in Y_B .

Notably absent from this list are the set of other unobserved factors that may influence ideology, attitude A, and attitude B, such as background beliefs, demographics, and culture. The implications of the model do not hinge on whether these unobserved initial conditions result in ideology or the intercorrelation of attitudes. Indeed, we expect them to be correlated for some individuals (such as members of the political elite) and less correlated for others (such as members of the mass public).

To this set of four variables, we apply a set of assumptions that are equivalent to a family of exclusion restrictions.

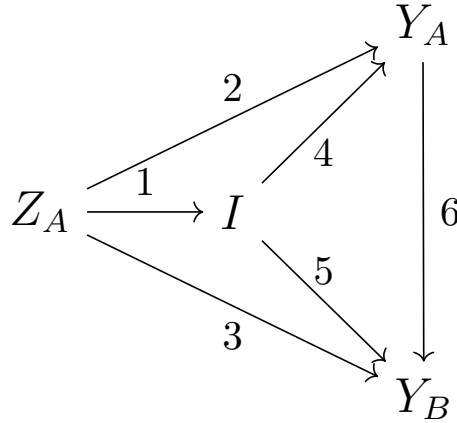
Assumption 1 (Exogeneity). We assume that Information A (Z_A) has no ancestors, i.e., it is randomly assigned or is otherwise exogenous. No paths lead from I , Y_A , or Y_B to Z_A .

Assumption 2 (Acyclicity) We assume a particular temporal ordering: Ideology (I) may affect attitudes (Y_A and Y_B) but attitudes do not affect ideology. Y_A may affect Y_B but not vice-versa. This choice amounts to requiring that Z_A affect one attitude “first,” and this ordering is reflected in the “A” subscript on the Z_A variable.

Assumption 3 (Functional Interdependence). Ideology (I) may or may not influence attitudes, but if it affects one attitude, it affects both. Suppose that, contrary to assumption 3, we allowed ideology to affect one attitude but not the other. This “ideology” would not deserve the name; instead it would be one of

the many unobserved factors that independently set the level of one attitude or another.

Figure 1: Model of Attitude Change with All Paths Consistent with Assumptions 1-3



These assumptions can be used to zero in on the observable implications of dynamic constraint. We operationalize Converse’s “functional interdependence” by defining dynamic constraint as an effect of Z_A that is mediated by I . That is, if the ideology is the *mechanism* by which Z_A affects attitudes, we would conclude that attitudes are dynamically constrained. If Z_A affects outcomes through pathways that do not include ideology (i.e., through direct paths only), we would not conclude that attitudes are dynamically constrained because there is no sense in which the idea-elements are bound together.

Figure 1 represents a causal graph in which our four variables are interrelated by six direct paths. No other paths could be added to this figure without violating some or all of assumptions 1-3. The figure represents a model of attitude change in which dynamic constraint plays a role in shaping attitudes: paths lead from Z_A to I and from I to Y_A and Y_B . Figure 1 is one of 32 possible configurations of paths that are consistent with assumptions 1-3. Each configuration of paths coincides with a distinct theory of how exogenous changes in the information environment and ideology interact to induce changes in opinions.²

²The other 31 graphs have the same nodes but a different combination of paths connecting them. We arrive at 32 possible graphs because there 6 total paths, but by assumption 2, paths 4 and 5 are either both present or both absent: $2^5 = 32$. Some of these graphs include paths from Z_A to Y_A and Y_B that pass through I , some do not. Table 1 enumerates the 32 possible graphs according to the presence or absence of the paths shown in Figure 1. For example, model 1 in the first row of the table has paths from Z_A to I and Y_A but excludes all other direct relationships. Model 10 has all possible paths and is the model represented in Figure 1.

In the empirical sections below, we will report on a series of studies in which some exogenous event Z_A occurs and we measure subjects' attitudes Y_A and Y_B . As noted above, I is unmeasured. Our studies can turn out one of four ways: We could observe that Z_A affects Y_A , Z_A affects Y_B , both, or neither. Which theories of attitude change (i.e., which of the 32 path diagrams) could have generated each pattern of evidence?

Table 1 is organized by the four empirical patterns that our experiments could reveal. If we find that Z_A affects Y_A but not Y_B , we know that (under assumptions 1-3), one of the first three models must have generated the data. If we find that Z_A affects Y_B but not Y_A , the data must have been generated by one of models 4 through 9. If Z_A affects both Y_A and Y_B , then any of the 17 models marked as model 10 through 26 could be responsible; if Z_A affects neither, any of the last six models could be the correct one.

What should we conclude under each of the four possible outcomes? A belief system is dynamically constrained if ideology mediates effects of Z_A on Y_A and Y_B , and not otherwise. If we observe that Z_A affects Y_A only, we can draw a clean inference because no models that generates such a pattern of evidence features paths from Z_A to Y_A and Y_B that pass through I . If Z_A affects neither Y_A nor Y_B , we could conclude that the effects are not mediated by ideology, but that claim is undermined by the lack of effects to mediate in the first place. If we find that Z_A affects Y_B only, we do indeed have evidence against dynamic constraint, but we might be concerned that the labels are simply misapplied: if Z_A affects Y_B only, it seems likely that Y_B should by rights be labeled Y_A in most applications. If we obtain evidence that Z_A affects both Y_A and Y_B , then dynamic constraint is possible, however, only 8 of the 17 models consistent with such a pattern feature dynamic constraint. Put another way, evidence that treatment affects both target and nontarget attitudes is a necessary but not sufficient condition for demonstrating dynamic constraint.

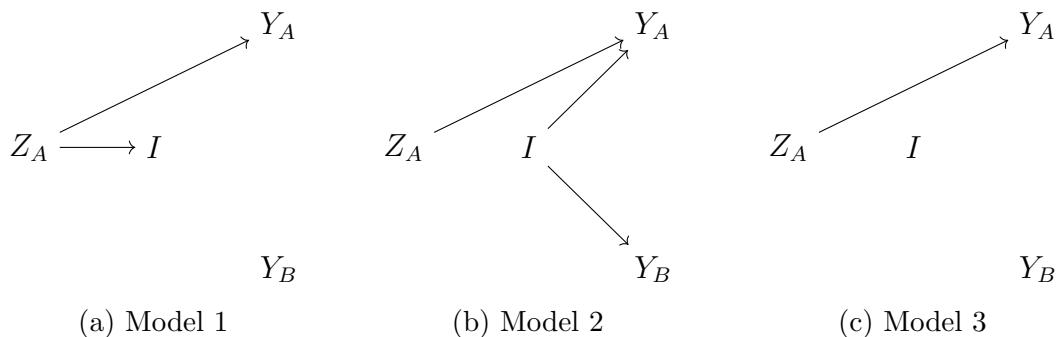
As we will show in the empirical sections below, our results come closest to the scenario in which Z_A affects Y_A only. For this reason, we explore the models consistent with that pattern of results in more detail in Figure 2. All three models are small variations on a theme: Z_A affects Y_A *directly*, i.e., without being mediated by ideology. If it is the case that the effects of Z_A on Y_A are not mediated by ideology, we can conclude that subjects' belief systems are not dynamically constrained. If we do find a pattern of evidence that shows Z_A affects Y_A only, we will not be able to adjudicate between these three models.³

³Furthermore, it might be that different models apply to different individuals. In model 2, ideology still plays a role in constraining attitudes, but *not* in a dynamic fashion. In models 1 and 3, ideology plays no such role, and the only difference is whether or not Z influences this ineffectual "ideology." An intriguing

Table 1: 32 Models of Dynamic Constraint, Organized by the Evidence they Generate

	$Z \rightarrow I$ Path 1	$Z \rightarrow Y_A$ Path 2	$Z \rightarrow Y_B$ Path 3	$I \rightarrow \{Y_A, Y_B\}$ Paths 4 and 5	$Y_A \rightarrow Y_B$ Path 6
Models generating evidence that Z_A affects Y_A only					
1	✓	✓			
2		✓		✓	
3		✓			
Models generating evidence that Z_A affects Y_B only					
4	✓		✓		✓
5	✓		✓		
6			✓	✓	✓
7			✓	✓	
8			✓		✓
9			✓		
Models generating evidence that Z_A affects both Y_A and Y_B					
10	✓	✓	✓	✓	✓
11	✓	✓	✓	✓	
12	✓	✓	✓		✓
13	✓	✓	✓		
14	✓	✓		✓	✓
15	✓	✓		✓	
16	✓	✓			✓
17	✓		✓	✓	✓
18	✓		✓	✓	
19	✓			✓	✓
20	✓			✓	
21		✓	✓	✓	✓
22		✓	✓	✓	
23		✓	✓		✓
24		✓	✓		
25		✓		✓	✓
26		✓			✓
Models generating evidence that Z_A affects neither Y_A nor Y_B					
27	✓				✓
28	✓				
29				✓	✓
30				✓	
31					✓
32					

Figure 2: Models Consistent with Data Showing Effects of Information on Attitude A Only



The experiments to follow will generate data on Z_A , Y_A , and Y_B but not I . This theoretical exercise has shown how the data that we obtain from these experiments will map on to the substantive quantities of interest. If we observe effects of our treatments on multiple attitudes, we cannot come to firm conclusions about the extent to which attitudes are dynamically constrained. If, on the other hand, we observe effects on target issues only, we may infer that ideology does not mediate these effects and hence, attitudes are not dynamically constrained.

possibility is that different models may hold for different individuals. For example, some individuals exhibit a high level of *static* constraint. Model 2 might do the best job of explaining their attitudes, while models 1 and 3 might best explain the attitudes of those exhibiting low levels of static constraint.

Study 1: Four Tests of Dynamic Constraint with Mass and Elite Subject Pools

The empirical strategy of this paper requires strong and statistically robust “first-stage” effects in which interventions directly affect attitudes in a given domain. For this reason, we make use of existing studies that convincingly demonstrate direct effects. Our aim is not merely to replicate their findings but rather to examine the downstream effects of the experimental intervention on opinions in other domains.

We begin with a survey experimental investigation of the effects newspaper opinion pieces. The experiment was conducted twice: first with a sample of 3,001 Mechanical Turk (MTurk) respondents and again with a sample of 2,181 “elite” respondents. The elite sample was constructed from mailing lists of Capitol Hill staffers, journalists, political professionals and financial industry workers. Subjects were randomly assigned to read one of four op-eds or a control condition, before answering a series of policy attitude questions related to each of the four op-eds.⁴

This design is ideal for assessing the presence or absence of dynamic constraint.⁵ The treatment op-eds were all real-world opinion pieces that were published in national outlets (the New York Times, the Wall Street Journal, USA Today and Newsweek) and advocated libertarian policy positions on transportation infrastructure, the flat tax, veterans’ healthcare, and the financial industry.⁶ Accordingly, the outcome variables (see box below for question wordings) are all scored so that higher values indicate higher agreement with the libertarian position. Each treatment op-ed is associated with a “target” attitude that is measured with four outcome questions each. For a visualization of the design of Study 1, see Figure 3. It describes how respondents in each sample were randomly assigned to the five experimental conditions before answering four attitudinal questions in each issue area, for a total of 16 outcomes. Within each treatment-control comparison, we can assess the effects of treatment on target and nontarget issues.

The demographic composition of each experimental sample was measured according to their age, gender, race/ethnicity, education, partisanship, and ideology.⁷ Compared with the MTurk sample, the elites are much older, more male, and whiter. The

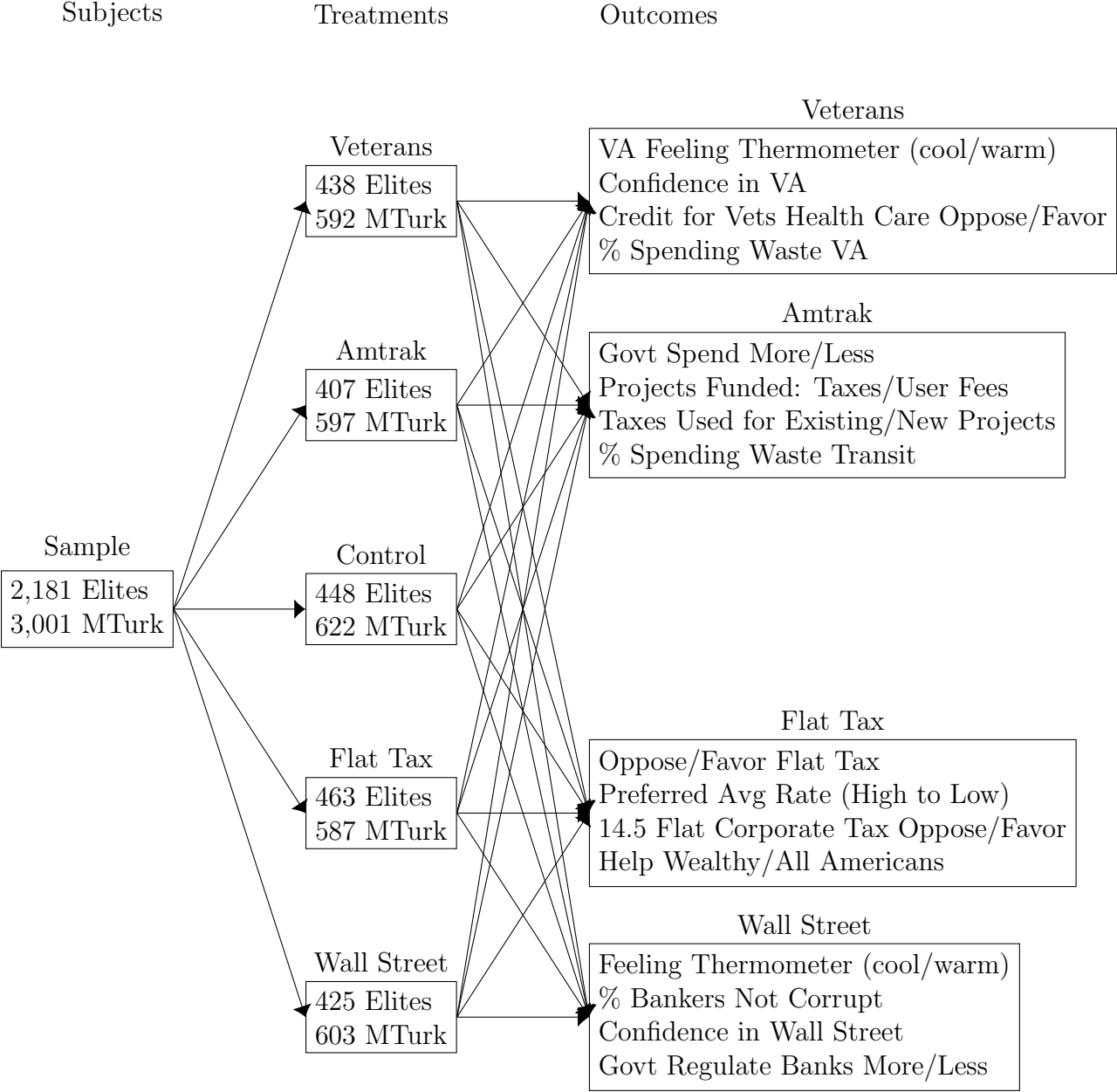
⁴In the MTurk version of the experiment, subjects could also be assigned to a fifth op-ed on climate change. We omit this treatment arm for clarity of presentation, but the substantive results we find for the other treatments are strongly paralleled in the climate change arm.

⁵The design of Study 1 is inspired by Hopkins and Mummolo (2017). Study 2 is a direct replication of that original study on two new samples.

⁶See the appendix for the full text of the op-eds.

⁷See Appendix Table A.1.

Figure 3: Study 1 Experimental Design



Study 1: Outcome Variables

Amtrak Outcomes:

1. Do you think the government should spend more, less, or about what it does now on transportation and infrastructure? [1: A lot more, A lot less]
2. Would you prefer government pay for building and maintaining roads and infrastructure through raising taxes for transportation spending, or through charging user-fees, like paying tolls when you drive on the highways? [1: Fund entirely through tax increases, 4: Both Equally, 7: Fund entirely through user fees]
3. If the government raised taxes to pay for more transportation spending, do you expect that money would primarily go toward building new infrastructure projects or maintaining and improving existing infrastructure? [1: Entirely toward NEW infrastructure projects, 4: Both Equally, 7: Entirely toward maintaining EXISTING infrastructure]
4. For every dollar the government spends on transportation and infrastructure projects, about how many cents do you think are spent inefficiently? [Slider 0 - 100, How Many Cents Spent Inefficiently?]

Flat Tax Outcomes:

1. Would you favor or oppose changing the federal tax system to a flat tax, where everyone making more than \$50,000 a year pays the same percentage of his or her income in taxes? [1: Strongly Favor, 7: Strongly Oppose]
2. What percentage of income, from 0 to 100, do you think Americans should pay in federal taxes on average? [Slider 0 - 100, Average Tax Rate]
3. Do you favor or oppose reducing the business and corporate tax rate to 14.5% percent? [1: Strongly Favor, 7: Strongly Oppose]
4. Do you think a flat tax on incomes over \$50,000 without tax deductions or credits will do more to help all Americans or do more to help wealthy Americans? [1: Do more to help ALL Americans, 7: Do more to help WEALTHY Americans]

Veterans Outcomes:

1. How would you rate your feelings toward the Department of Veterans Affairs (the VA) on a scale of 0 to 100, where a rating of 100 means you feel as warm and positive as possible and 0 means you feel as cold and negative as possible? How do you feel toward... [Department of Veterans Affairs]
2. How much confidence do you have in the Department of Veterans Affairs' ability to care for veterans? [1: A Great Deal, 7: None At All]
3. Would you favor or oppose changing the healthcare system for Veterans to a system where the government provides additional money sufficient for Veterans to purchase a government-approved health insurance plan from private health insurance companies? [1: Strongly Favor, 7: Strongly Oppose]
4. For every dollar the government spends on Veterans Benefits, about how many cents do you think are spent inefficiently? [Slider 0 - 100, How Many Cents Spent Inefficiently?]

Wall Street Outcomes:

1. How would you rate your feelings toward the following on a scale of 0 to 100, where a rating of 100 means you feel as warm and positive as possible and 0 means you feel as cold and negative as possible. How do you feel toward... [CEOs; Wall Street Bankers; Government Regulators]
2. What percentage of Wall Street bankers, from zero to one hundred, do you think are corrupt? [Slider 0 - 100: % Wall Street Bankers Corrupt]
3. How much confidence do you have in Wall Street bankers and brokers to do the right thing... [1: A Great Deal, 7: None at all]
4. Compared to what it's doing now, do you think the federal government needs to regulate banks and financial institutions [1: A lot more, A lot less]

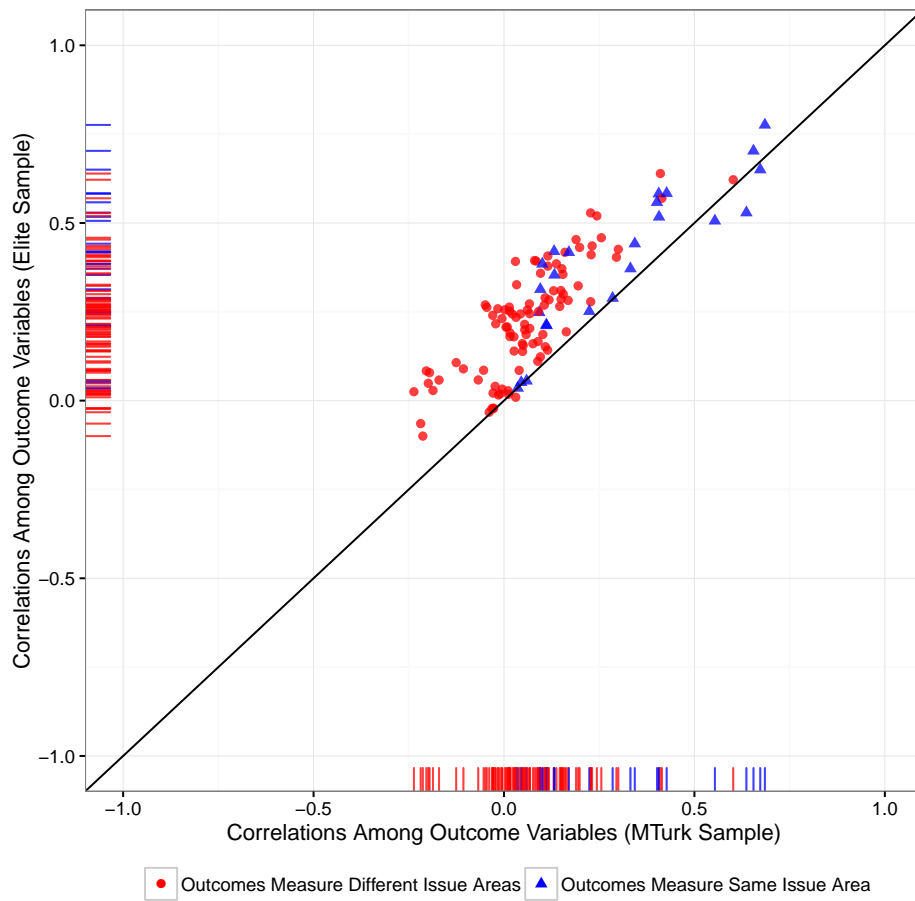
elites have received far more formal education, with nearly three-quarters having attended graduate school. Interestingly, the elites are more partisan than the MTurk respondents, but a higher proportion nevertheless report themselves to be moderate or libertarian. These demographic differences are all highly statistically significant and moreover, are relevant for the question at hand because elites are widely viewed to be more ideological in the static sense of knowing what goes with what.

We can confirm the notion that elites exhibit higher levels of static constraint than our MTurk respondents by estimating the extent to which subjects’ responses to these outcome questions are intercorrelated. Figure 4 plots the Pearson correlations for all pairwise combinations of the 16 outcome questions (16 choose 2 = 120 pairs). The correlations in the MTurk sample are plotted on the horizontal axis, while the correlations in the elite sample are plotted on the vertical axis. Pairs of outcomes that measure the same attitude are plotted as blue triangles, while pairs of outcomes that measure different attitudes are plotted as red circles. For both the MTurk and elite samples, the inter-item correlations are stronger for the pairs that measure the same attitude than pairs that measure different attitudes. Consistent with a large empirical literature on the differing levels of static constraint among elites and ordinary voters, elites’ attitudes are more strongly correlated than MTurk respondents’ attitudes. The plot shows that nearly all 120 measured correlations lie above the 45 degree line. On MTurk, the average correlation between attitudes in different domains was 0.06; the average correlation between attitudes in the same domain was higher, but still modest at 0.29. In the Elite sample, the average correlation between attitudes within domain was 0.39 while the average across domains was 0.22.⁸

This familiar pattern of static constraint (high levels among elites but low levels among “ordinary” voters) is strongly contrasted by the uniformly low levels of dynamic constraint shown in Figure 5. The figure has 16 panels, each of which corresponds to the effects of an op-ed on a set of outcome measures. The rows of panels correspond to the four attitudes, each of which are measured with four questions each. The columns are separate treatments. The points and bars within each panel represent treatment effect estimates and 95% confidence intervals; estimates from the elite study are plotted in red circles and estimates from the MTurk study are plotted in blue triangles. The shaded panels on the diagonal indicate treatment effect estimates for each op-ed on *target* issues (the estimated effects of Z_A on Y_A , to use the terminology from above) while the nonshaded panels on the off-diagonals show the estimated effects of treatment

⁸The reported correlations make no correction for measurement error in survey responses. Such corrections tend to make these correlations substantially higher (Ansolabehere, Rodden and Snyder 2008; Freeze and Montgomery 2016).

Figure 4: Correlations among Outcome Variables by Study Sample



on the *nontarget* issues (the effects of Z_A on a series of Y_B 's.).

All effects are standardized, so that an estimate of 0.25 indicates that the treatment group mean was 0.25 standard deviations higher than the control group mean. Proceeding diagonally down from the top left panel to the bottom right panel, we observe strong effects of information on target issues. Among both elites and MTurk respondents, the effects of the Amtrak treatment on the Amtrak outcomes hover around one-third of a standard deviation. Treatment effects on the third question in the Amtrak set of outcomes (regarding how subjects expect new taxes to be spent) were especially large, at 0.52 SDs among elites and 0.73 SDs on MTurk. Similarly strong effects are present in the MTurk sample for the effects of the Flat Tax and Veterans treatments on their target issues, while the effects of these treatments are more muted in the elite sample. The Wall Street treatment had the largest effects on its target issue of any of the treatments, averaging nearly 0.5 standard deviations in both samples. Out of the 32 estimated treatment effects on target issues, 27 are statistically significant at $p < 0.05$ or better. If we apply the Benjamini-Hochberg (1995) multiple comparisons correction to the associated p -values, 26 estimated effects remain significant.

The panels on the off-diagonals display the effects of op-eds on nontarget issues. In nearly every case, the estimates are very close to zero. Out of 96 estimated effects on nontarget issues, only 8 of the effects are significant, but when we apply the Benjamini-Hochberg correction, none of these estimates is deemed significant.

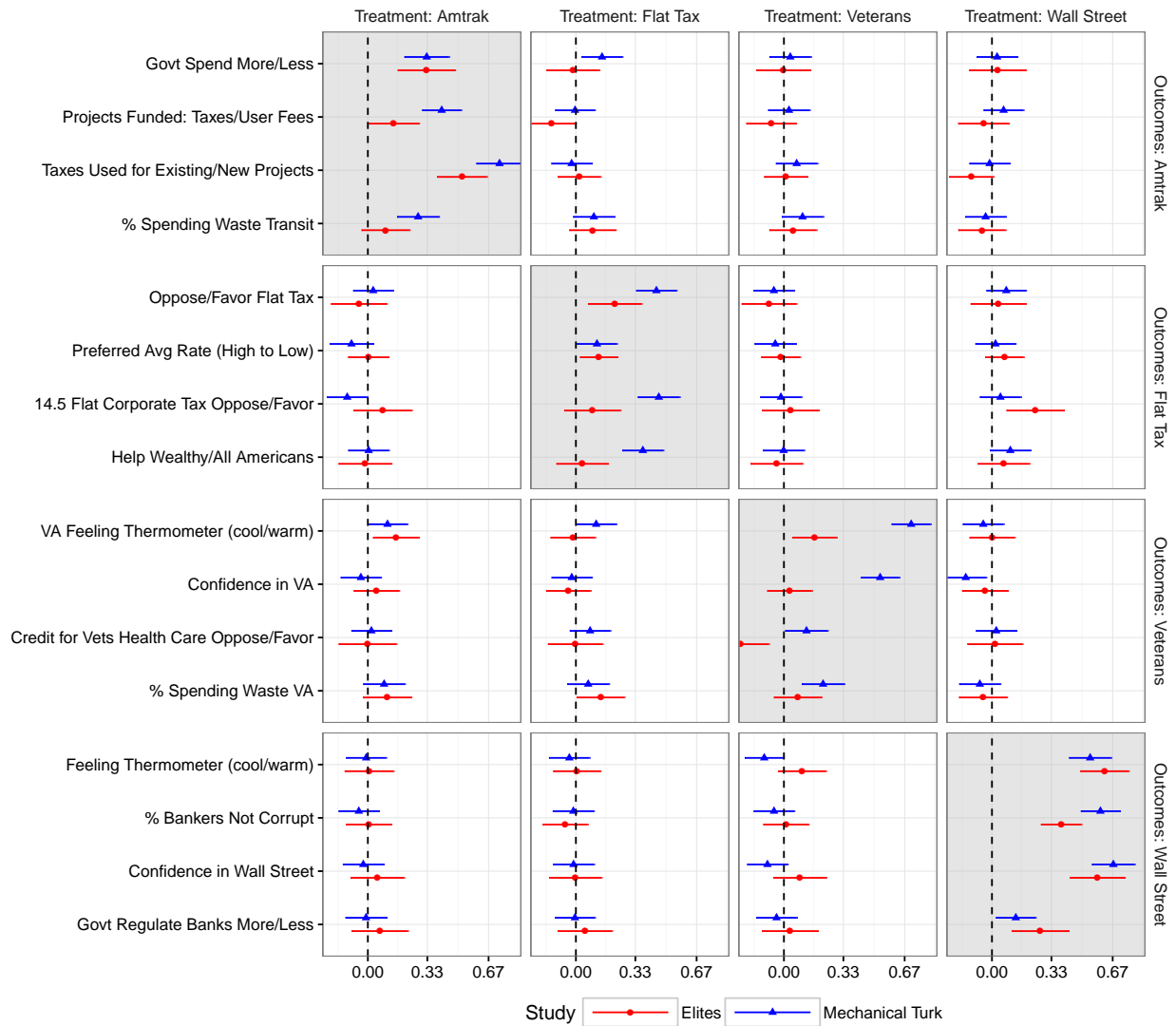
Figure 5 provides clear evidence against dynamic constraint. The precision weighted average of all treatment effects on target issues is 0.42 on MTurk and 0.22 in the elite sample. By contrast, the precision weighted average of effects on nontarget issues is tiny in both samples: 0.004 standard deviations on MTurk and 0.01 in the elite sample. This constitutes evidence that these treatments do indeed affect their target issues (Z_A affects Y_A) but do not affect nontarget issues (Z_A does not affect Y_B). Recall that none of the models in Table 1 that are consistent with this pattern of evidence include a role for ideology as a mediator of exogenously-induced changes in opinions.

We can express the size of the of the average effect on nontarget issues as a proportion of the average effect on target issues. On MTurk, our best guess is that the average indirect effect is 0.8% of the average direct effect, with a very tight confidence interval⁹ from -8.1% to 8.7%. On the elite sample, we estimate that indirect effects are 6.4% the size of the direct effects on average, with a wider confidence interval extending from -25.7% to 26.2%. If we pool the two studies together, our estimate is 1.4% (95% CI: -6.6%, 9.3%). In summary, this study was well-powered to detect even very small

⁹Because the treatment effect estimates are nonindependent, we constructed a confidence interval around this ratio by bootstrapping at the respondent level.

indirect effects on the scale of 10 to 20 percent the size of direct effects, but we found no indication at all that these treatments induced any opinion change in nontarget domains.

Figure 5: Effects of Newspaper Op-eds on Target and Nontarget Issues



Study 2: Frame Breadth

Our second study leverages the treatment effects described in Hopkins and Mummolo (2017). That paper aims to assess the “breadth” of framing effects. We use the identical treatment stimuli and outcome measures. In order to assess the robustness of the effects, we conducted one study using Mechanical Turk ($N = 2,972$), and the other was conducted among a nationally-representative sample assembled by GfK ($N = 3,140$).

The design of Study 2 is analogous to Study 1 (shown in Figure 3). Subjects were randomly assigned a control condition or to read one of four short statements that put a pro-conservative frame on a salient policy debate.¹⁰ In contrast to the long-form arguments that were employed in Study 1, these treatments are all short paragraphs that stake out a position but do not provide detailed arguments. Each statement was preceded by “The argument below was recently made by a U.S. Senator. Please take a moment to read the argument carefully and then tell us what you think.” The four treatment frames to which subjects could be randomly assigned were:

Crime: America is very vulnerable to violent crime, with forty-two Americans murdered every single day on average. Innocent people can be killed in their front yards. Across the country, we have to do everything we can to reduce the threat of violent crime. We have to stop violent criminals before they act. This means cracking down on the smaller offenses that all too often lead to violent crime, and making sure that convicted criminals always serve out their full sentences.

Health Care: Health care is one of the most complicated issues we face. It involves 1 of every 6 dollars spent here in the United States. The health care system includes millions of doctors and nurses and thousands of hospitals and clinics. Together, they regularly make decisions that can mean life or death. The government in Washington can’t even balance its own budget. How can we trust it to run something as complicated as the health care system?

Stimulus: With a recession as deep as this one, there are more than 10 million unemployed Americans, and it’s going to take years for our economy to recover. In February 2009, the government in Washington made things worse by passing an \$800 billion stimulus package, which is more than \$2,500 for every person living in this country. Now, it looks like a lot of that money didn’t help the economy. Unemployment is still very high. The money went to pork-barrel projects and federal bureaucrats rather than creating jobs for unemployed Americans. The government in Washington can’t even balance its own budget. How can we trust it to spend so much taxpayer money?

¹⁰Our design differs slightly from the design in Hopkins and Mummolo (2017). In the original study, subjects who were not in control were first randomly assigned to see one statement, then answer two policy questions, then to see a second statement before answering the remaining questions. In our study, subjects are randomly assigned to see one argument (or nothing) before answering all four policy questions. These small differences in design have negligible implications for the substantive interpretation of results.

Terror: The September 11th attacks and the news that al-Qaeda was planning new attacks on U.S. soil show how vulnerable America still is to terrorists. Innocent people can be killed while traveling to visit family or going to work. Across the country, we have to do everything we can to reduce the threat of terrorism. We have to stop terrorists before they act. This means conducting more frequent searches of suspicious people boarding planes, trains, subways, and buses.

Spending preferences were measured on a 1-7 scale (1: Decreased a lot, 7: Increased a great deal) in all four issue areas. The questions read “Should federal spending [on dealing with crime/on health care/to stimulate the economy/on the war on terrorism] be increased, decreased, or kept the same?” These questions plausibly measure both subjects’ views in each policy area as well as their federal spending preferences in general. Outcomes were assessed with a single question in each issue area, for a total of four outcome questions.¹¹

Figure 6 shows our results in a format that is analogous to Figure 5: the columns refer to the four different treatments, the rows to the four different outcome measures, the ATE estimates are plotted separately for GfK (red circles) and MTurk (blue triangles). Our findings largely match those reported in Hopkins and Mummolo (2017, Figure 3): mild effects for the target issues and mostly null effects on the nontarget issues.

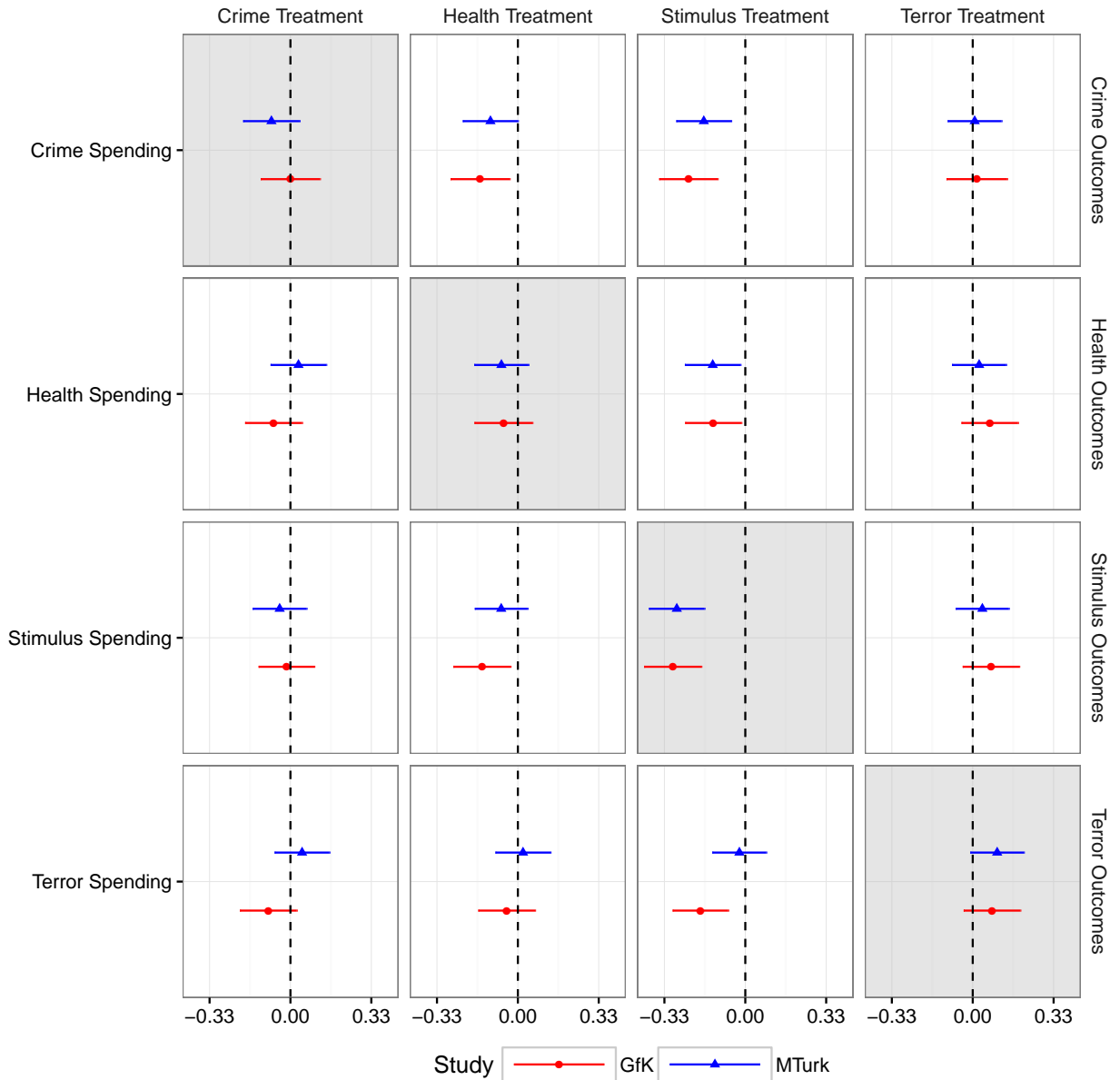
Our results differ from those reported in Hopkins and Mummolo (2017) in one case only: the *Stimulus* treatment appears to have affected all four outcomes. On its face, this result appears to lend some support for the conclusion that attitudes are dynamically constrained, because here we have an example of a treatment that affects both target and nontarget issues. However, the *Stimulus* treatment explicitly advocated that the federal government spend less money across the board. The outcome questions all assessed attitudes in various policy domains by asking subjects whether the federal government should increase or decrease spending in each of those domains. In our view, the fact that this treatment affected *spending* preferences across domains does not constitute dynamic constraint because in some sense, the “target issue” of this treatment is all spending preferences. In the theoretical language developed above, we are assessing the effects of Z_A on Y_{A1}, Y_{A2}, \dots , rather than the effects of Z_A on Y_A, Y_B, \dots .

One of the key objectives of this study was to assess whether any dynamic effects endure over time. We therefore measured the effects of all four treatments again after 10 days. The effects of the stimulus frame on the nontarget issues were no longer

¹¹For a comparison of the MTurk and GfK samples in terms of their demographic characteristics, see the appendix. The GfK sample is older, less well educated, more Republican, less independent, and less Democratic. With the exception of gender, all demographic differences are statistically significant.

distinguishable from zero after this relatively brief interval, supporting the idea that the treatment did not induce an ideological change in thinking about spending which then affected spending attitudes across a wide variety of domains. Instead, these results are consistent with the interpretation that the framing effect of the *Stimulus* treatment induced a momentary change in all spending preference questions.

Figure 6: Effects of Arguments on Target and Nontarget Spending Preferences



Study 3: Can Consistency be Induced?

Thus far, we have considered whether treatments designed to move one attitude also move related attitudes via a process of dynamic constraint; we have found that typically, they do not. We now consider whether subjects can be induced to express more consistent survey responses. Rather than intervene in ways that change a given attitude, we manipulate question wording to encourage specific responses to see whether this random inducement to answer in a particular way alters subsequent answers, as it does in experiments that demonstrate consistency bias outside the domain of politics (Falk and Zimmermann 2013). We also investigate whether explicitly calling attention to subjects’ apparent inconsistency causes their subsequent responses to become more consistent, as would be the case if respondents were intrinsically motivated to express ideologically consistent positions.

The motivation for studying “externally enforced” constraint is our desire to explain the apparent gap between static and dynamic constraint among elites and politically attuned members of the mass public. The experiments below suggest the extent to which consistency can be induced by light touch interventions, such as asking about cognate issues back to back, or heavy-handed interventions, such as chastising respondents for their apparent ideological inconsistency.

We present a series of three survey experiments, all conducted on the same sample of 1,087 responses gathered via Lucid, an online exchange for survey responses. Like most online samples, Lucid provides a convenience sample of subjects that can be considered representative of the general population only under strong assumptions. As shown in the Appendix, our Lucid sample is more female and better educated than the national population.

Our first experiment primes religious freedom with regard to hiring gays and lesbians; while this treatment has a large effect on subjects’ stated views on the acceptability of employment discrimination, this prime does not spill over into other attitudes about same sex marriage or school prayer. Similarly, our second experiment makes use of the classic General Social Survey welfare/aid to the poor experiment (Williamson 1974). Priming subjects to think about redistribution in terms of welfare instead of aid to the poor markedly changes their support for this kind of spending but does little to change subsequent answers about about government’s role in reducing inequality or other social programs such as Medicaid. In our third and final experiment, we restrict our sample to subjects who express inconsistent spending preferences; we randomize whether these subjects are informed of their inconsistency. We find that this heavy-handed correction does induce subsequent consistency, but only temporarily.

The overall picture of consistency we gain from these three experiments is that, indeed, consistency can be induced when subjects are asked, point blank, to please be consistent. But while responses across domains may be correlated, changes in response due to question order or wording do not appear to spill over into related outcomes.

Consistency Bias Experiment 1: Employment Discrimination

The following two experiments ask the further question of whether priming some feature of an attitude can influence the expression of related attitudes.

We perform a simple question wording experiment in which subjects were shown one version of the following question at random: “Do you agree or disagree with the following statement: Employers [who have deeply held religious convictions] should have the right to refuse to hire gay men and lesbians.” [1: Strongly disagree, 7: Strongly agree] We expected the manipulation to increase our subjects’ view that discrimination against gays and lesbians is acceptable, which is what we found. We wanted to know if raising the religious objection in subjects’ minds would change their responses to the following outcome questions:

Local Officials: Sometimes local officials who issue marriage licenses have religious objections to same-sex marriage. Should these local officials be required to issue marriage licenses to same-sex couples, or should local officials be excused from issuing licenses that violate their religious convictions? [Binary: excused = 1]

SSM Amend: Would you favor or oppose an amendment to the United States Constitution that would allow marriage only between a man and a woman, and outlaw marriages between people of the same sex? [1: Strongly oppose, 7: Strongly favor]

Prayer: Do you agree or disagree with the following statement: “Football coaches at public high schools should be allowed to lead their players in Christian prayer during games.” [1: Strongly disagree, 7: Strongly agree]

Prayer Amend: Would you favor or oppose an amendment to the United States Constitution that would require a daily prayer to be recited in public school classrooms? [1: Strongly oppose, 7: Strongly favor]

Table 2 shows that the religious exception treatment increased subjects’ agreement that employers should have the right to discriminate by 0.44 scale points; this estimate is highly statistically significant ($p < 0.001$) and represents an enormous first-stage effect. However, the four outcome measures did not change significantly in the wake

Table 2: Employment Discrimination Experimental Results

	Initial Outcome	Subsequent Outcomes			
	Employers	Local Officials	SSM Amend	Prayer	Prayer Amend
Religious Exemption	0.444*** (0.133)	-0.003 (0.029)	0.217 (0.143)	-0.075 (0.115)	0.087 (0.131)
Constant (Control Mean)	3.016 (0.090)	1.381 (0.021)	3.679 (0.099)	5.115 (0.081)	4.164 (0.093)
N	1,087	1,086	1,087	1,087	1,085

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

of this marked change in expressed opinion, as shown in columns 2 through 5. Two of the estimated effects are positive, and two are negative. When induced to choose a less tolerant survey response with respect to employers, subjects do not express less tolerant (or even more religious) survey responses.

Consistency Bias Experiment 2: Welfare

Our second experiment follows a very similar design to Experiment 1. Subjects were first randomly assigned to see one version of the classic GSS redistribution question: “We are faced with many problems in this country, none of which can be solved easily or inexpensively. We are going to name one of these problems, and we’d like you to tell us whether you think we’re spending too much money on it, too little money, or about the right amount: [Welfare / Aid to the poor].”

This experiment has been conducted dozens of times over more than 50 years and always shows that subjects prefer spending money on aid to the poor than on welfare. We find this same pattern in our sample. Subjects who saw the welfare version of the question supported redistribution 0.59 scale points less than subjects who saw the aid to the poor version. This estimate is highly statistically significant and again provides evidence of a robust first-stage effect. We further assess the effects of this manipulation on two outcome questions:

- Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing income differences between the rich and the poor. On a scale from 1 to 7, with a score of 1 meaning that the government ought to reduce the income differences between rich and poor, and a

score of 7 meaning that the government should not concern itself with reducing income differences, what comes closest to the way you feel? [Reverse coded]

- Some people say Medicaid is a good thing because our society should provide health care to poor people, while others say Medicaid is a bad thing because escalating costs are unfair to the average taxpayer. Which comes closer to your view?

Table 3 presents estimated treatment effects on cognate issues. The effect of the prime on subsequent support for government reducing income gaps is slightly negative and is only significant at the 10% level using a two-tailed test. The effect on support for Medicaid very close to zero and is not statistically significant. We interpret these results to indicate that the inducement to respond in a way that is more supportive of social spending has little to no downstream effect on expressed views about reducing inequality or providing health care to poor people. In other words, respondents do not spontaneously offer answers that are consistent across questions, either because ideological consistency is a weak motivation or because they are unaware that they are being inconsistent. B

Table 3: Welfare Experimental Results

	Initial Outcome	Subsequent Outcomes	
	Too Little on Welfare	Gov't Should Reduce Ineq.	Medicaid a Good Thing
Welfare Wording	-0.593*** (0.046)	-0.241* (0.130)	-0.002 (0.024)
Constant (Aid to the Poor)	2.383 (0.033)	4.678 (0.093)	0.813 (0.017)
N	1,087	1,086	1,084

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

Consistency Experiment 3: Spending Preferences and Direct Inducement to be Consistent

To what extent do subjects who give inconsistent responses become more consistent when encouraged to do so? In this final experiment, we gave subjects an opportunity to contradict themselves when expressing their opinion about spending preferences. Among people who did contradict themselves, we randomized whether we pointed out

their inconsistency or not. We then measured their subsequent spending preferences to see if these subjects become more consistent with their “general” spending view.

The general spending view question asked: “Generally speaking, would you say you favor smaller government with fewer services, or larger government with more services?” The next question asks the following: “Thinking about the federal budget, do you want to see the President and Congress increase spending on education, decrease spending, or keep it about the same?”

Inconsistency between preferences for smaller government and increased social spending is well documented (Sears and Citrin 1982; Ahler and Broockman 2017). Of our 1,087 subjects, 325 expressed inconsistent views. Of the people who wanted smaller government, 299 people said they wanted to increase spending on education. Of the people who wanted larger government, 26 said they wanted to decrease spending on education. We randomly assigned approximately half of these 325 subjects with to receive a query: “We noticed that you said that generally speaking, you preferred a smaller government with [fewer/more] services but that you also want to [increase/decrease] federal spending on education. Just to check, we’d like to ask you about education spending again.”¹²

The experimental outcome is not their response to the education spending question, but subsequent spending questions, asked directly after: “Still thinking about the federal budget, do you want to see the President and Congress increase spending on each of the following, decrease spending, or keep it about the same?” [Five spending categories, Social Security, Medicare, Health Care for Low-income Families, National Defense, Foreign Aid]. We average all five responses so that -1 on the resulting scale indicates that a respondent preferred decreasing spending in all five categories and +1 indicates a preference for increases in all categories.

When induced to become more consistent, people who prefer smaller government prefer less spending (-0.172 scale points, equivalent to incrementing a single response out of the five down by one response option). We see almost the identical coefficient in the opposite direction among those who prefer larger government, though that estimate is not statistically significant. If we reverse code the outcome scale for those those prefer smaller government so that higher values indicate a greater degree of consistency with subjects’ initially stated position, we obtain an average estimate of 0.172 scale points (robust SE: 0.046, $p < 0.001$). These results indicate a clear shift toward greater consistency among those who receive an encouragement, suggesting that respondents are capable of maintaining consistency when that becomes a salient concern. Interest-

¹²Among these treatment group subjects, 27% took the opportunity to change their answer to the education question.

ingly, the encouragement occurred early in the questionnaire, and the aforementioned questions about increasing spending on welfare or aid to the poor were asked toward the end of the interview. Although only a few minutes elapsed between the encouragement and this outcome measure, we find no detectable effect on support for spending in this domain ($p = 0.72$). Evidently, inducements to maintain consistency produce effects of moderate size that dissipate rapidly.

Conclusion

The collection of experiments presented above have three broad implications. First, by demonstrating the limited extent to which opinion change in one domain precipitates opinion change in other domains, the evidence underscores Converse’s contention that mass belief systems tend to be narrow and disjointed. For example, moving respondents’ attitudes on tax policy has no detectable effect on their opinions on the topic of veterans’ healthcare let alone an issue like railroad infrastructure that is conceptually more distant from the locus of opinion change. In Study 1, information induced significant opinion change on target issues in 27 of 32 opportunities, providing a quite powerful first-stage platform for the exploration of dynamic effects. But in only 8 of 96 opportunities did it induce significant changes on nontarget issues, and the average spillover effect is just 1.4% of the first-stage direct effect. This pattern of narrow opinion change has occasionally been noted by other studies (e.g., Finseraas and Kotsadam 2015¹³), but ours is the first systematic attempt to assess dynamic constraint experimentally across a range of substantive domains.

Second, in those instances in which interventions do induce opinion change on an array of issues simultaneously, as in the two experiments on spending preferences, these effects dissipate quite quickly. After two weeks, the *Stimulus* prime, which had initially moved spending preferences in multiple domains, no longer had detectable effects on any spending preference. One interesting exception to this pattern of narrow and ephemeral change is the finding by Broockman and Kalla (2016) that doorstep conversations between canvassers and voters on the subject of transgender rights affected not only policy opinions on that issue but also on the issue of gay rights. This instance of dynamic constraint is reminiscent of Converse’s argument (pp.38-42) that attitudes in the domain of race and civil rights are unusual in terms of their persistence and coherence because they allow people to reason based on *group* evaluations rather than

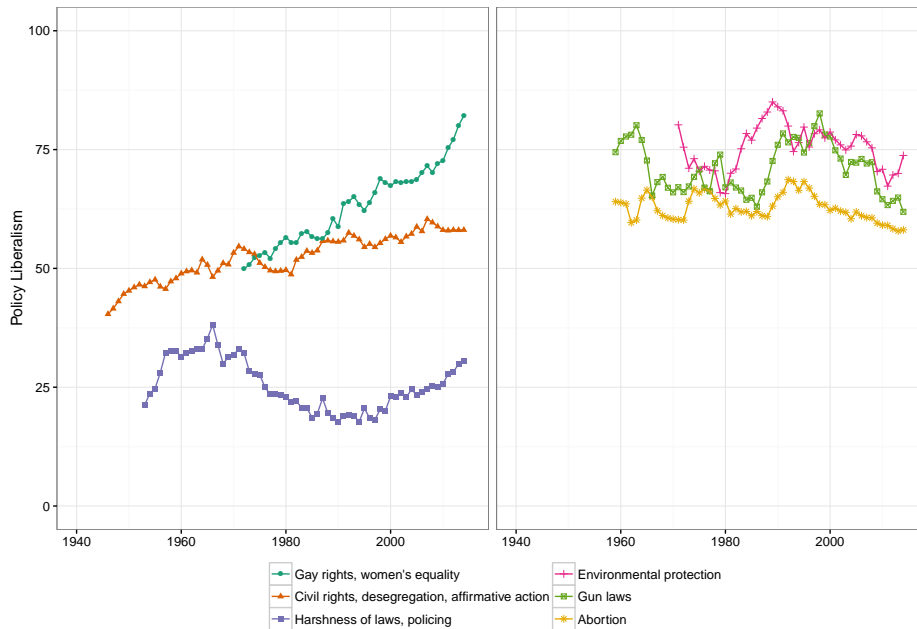
¹³This study finds that randomly induced exposure to immigrants reduces negative stereotypes about them but does not change policy preferences regarding immigration or the social welfare benefits to which they are entitled.

ideological ones.

Third, weak dynamic constraint is evident even among subgroups that display relatively high levels of static constraint. Although their opinions across issues were initially more correlated, the elite subject pools in our studies responded to information in a manner similar to the mass subject pools. Elites changed their opinions significantly on the targeted issue but not on related topics – despite the fact that their opinions on these topics were correlated initially. Although elites have a clearer sense of how idea-elements in an ideological belief system fit together, when one idea-element changes, elites do not spontaneously adjust these elements so as to make them consistent.

What are the macro implications of low levels of dynamic constraint? Perhaps the most direct implication is the prediction that trends in policy preferences will tend to be disjointed across issues. This proposition is readily assessed thanks to the work of the Comparative Agendas project, which builds on the pioneering “policy mood” analysis by Stimson (1991). Assembling a vast array of publicly available survey data, the project tracks trends in 56 opinion domains ranging from civil rights to foreign aid, using factor analysis to construct scales using data from different survey organizations and different question wording.

Figure 7: Six Policy Moods



One of the most interesting findings to emerge from their dataset is that trends in public opinion do not track in a consistently liberal or conservative fashion within or

across issue domains. Consider, for example, the left panel of Figure 7, which depicts trends in three related issue domains: civil rights (desegregation, affirmative action), gender equality (gay rights, women's rights), and due process rights (harshness of laws, police tactics). The gender equality series shows a steep liberal trajectory since the 1970s, while the due process series moves in a conservative direction from the 1970s through the 1990s, rebounding from 2000 on. The civil rights series moves in a liberal direction with occasional conservative downturns that do not consistently coincide with trends in either of the other two series. When we look across issue domains to environmental protection, abortion rights, and gun control, we see public opinion moving in a conservative direction on the latter two issues since the late 1990s. Trends in environmental protection are more muted, and the turning points in this series do not seem related to the other two. In sum, the six series move over time but not necessarily in tandem with one another. Just as Converse (pp.58-64) warned against taking the ideological pulse of the public from election results, caution is warranted when ascribing a change in ideological outlook to shifts in multi-issue indexes that gauge policy mood. Obviously, this conclusion remains provisional pending confirmation from studies conducted outside the United States, but for the moment, experimental and aggregate data from the American context both attest to the lack of dynamic constraint in public opinion.

With the benefit of several decades of aggregate opinion data and the advent of large-scale experimental studies of public opinion, contemporary scholarship is now able to fill in the gaps in Converse's empirical case against dynamic constraint. Evidence in favor of Converse's position is even stronger than he suspected. Changes in opinion do not ramify across domains, at least not for long or without strong encouragement. In the end, it may be the strength and frequency of encouragement that distinguishes elites from masses. Akin to Consistency Experiment 3, the latter are continually admonished by party leaders, interest groups, and commentators to maintain their fealty to a body of ideological ideas. For public officials and party elites, the encouragement that we administered experimentally is meted out daily and often with great vehemence.

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A Demographics

Table A.1: Study 1: Demographics by Experimental Sample

	Elites	MTurk
18 - 29	7.2	45.1
30 - 39	17.6	30.7
40 - 49	19.2	12.2
50 - 59	25.0	8.8
60+	31.1	3.2
Male	67.9	52.1
Female	32.1	47.9
Black	3.1	6.5
Hispanic	4.1	5.8
White	83.0	77.5
Other	9.7	10.2
Less than High School	0.0	0.5
High School	0.5	9.8
Some College	3.1	38.3
College	24.9	39.0
Graduate School	71.6	12.4
Strong Democrat	31.5	19.8
Not very strong Democrat	14.3	25.4
Lean Democrat	10.1	15.5
Independent	11.9	15.4
Lean Republican	7.0	7.1
Not very strong Republican	12.3	11.0
Strong Republican	12.8	5.7
Liberal	28.2	44.5
Moderate	36.7	26.5
Libertarian	11.7	6.2
Conservative	19.3	18.2
Other	4.2	4.6
N	2181	3001

All entries are sample percentages.
 Within demographic category, all cross-sample differences are statistically significant ($p < 0.001$ in all cases).

Table A.2: Study 2: Demographics by Experimental Sample

	GfK	MTurk
18 - 24	10.6	21.2
25 - 34	20.0	41.7
35 - 44	16.1	18.5
45 - 54	15.9	10.5
55+	37.4	8.1
Male	48.7	48.3
Female	51.3	51.7
Less than High School	12.2	0.4
High School	29.7	9.6
Some College	28.7	41.6
College	16.5	36.0
Graduate School	13.0	12.4
Strong Democrat	18.4	19.8
Not very strong Democrat	15.5	24.5
Lean Democrat	19.1	15.3
Independent	3.9	14.4
Lean Republican	18.6	7.1
Not very strong Republican	10.5	12.2
Strong Republican	14.2	6.8
N	3140	2972

All entries are sample percentages. GfK entries incorporate sampling weights.

Cross-sample differences by age, education, and partisanship are all statistically significant ($p < 0.001$).

Table A.3: Study 3: Demographics

	Lucid
18 - 29	15.8
30 - 39	17.7
40 - 49	16.2
50 - 59	20.0
60+	30.3
Male	37.7
Female	62.3
Less than High School	3.3
High School	20.3
Some College	39.8
College	23.8
Graduate School	12.7
Strong Democrat	20.5
Not very strong Democrat	16.7
Lean Democrat	5.4
Independent	17.7
Lean Republican	5.2
Not very strong Republican	13.5
Strong Republican	21.0
Liberal	22.2
Moderate	33.2
Conservative	31.4
Other / Prefer not to say	13.2
N	1087

All entries are unweighted sample percentages.

Due to a data error, we do not have the ethnicity information required to distinguish Hispanic/Latino subjects from others.

B Study 1: Experimental Stimuli

This section includes reproductions of four op-eds:

Treatment	Title	Author	Publication
Amtrak	The Amtrak Crash: Is More Spending the Answer?	Randal O'Toole	Newsweek
Flat Tax	Blow Up the Tax Code and Start Over	Rand Paul	The Wall Street Journal
Veterans	The Other Veterans Scandal	Michael F. Cannon and Christopher Preble	The New York Times
Wall Street	Wall Street Offers Very Real Benefits	Thaya Knight	USA Today

Newsweek

The Amtrak Crash: Is More Spending the Answer?

BY RANDAL O'TOOLE 5/13/15 AT 4:46 PM

It is too soon to tell what caused the Amtrak train crash that killed seven people on May 12. But advocates of increased government spending are already beginning to use the crash to promote more spending on infrastructure and are criticizing Republicans who voted to reduce Amtrak's budget the day after the crash.

Yet there is a flaw in the assumption that spending more money would result in better infrastructure. In fact, in some cases, the problem is that too much money is being spent infrastructure, but in the wrong places.

The reason for this is that politicians prefer to spend money building new infrastructure over maintaining the old. The result is that existing infrastructure that depends on tax dollars steadily declines while any new funds raised for infrastructure tend to go to new projects.

We can see this in the Boston, Washington, and other rail transit systems. Boston's system is \$9 billion in debt, has a \$3 billion maintenance backlog, and needs to spend nearly \$700 million a year just to keep the backlog from growing. Yet has only budgeted \$100 million for maintenance this year, and instead of repairing the existing system, Boston is spending \$2 billion extending one of its light-rail lines.

Similarly, Washington's Metro rail system has a \$10 billion maintenance backlog, and poor maintenance was the cause of the 2009 wreck that killed nine people. Yet, rather than rehabilitate their portions of the system, Northern Virginia is spending \$6.8 billion building a new rail line to Dulles Airport; D.C. wants to spend \$1 billion on new streetcar lines; and Maryland is considering building a \$2.5 billion light-rail line in D.C. suburbs.

On the other hand, infrastructure that is funded out of user fees is generally in good shape. Despite tales of crumbling bridges, the 2007 Minnesota bridge collapse was due to a construction flaw and the 2013 Washington state bridge collapse was due to an oversized truck; lack of maintenance had nothing to do with either failure.

Department of Transportation numbers show that the number of bridges considered structurally deficient has fallen by more than 50 percent since 1990, while the average roughness of highway pavement has decreased. State highways and bridges, which are almost entirely funded out of user fees, tend to be in the best condition while local highways and bridges, which depend more on tax dollars, tend to be the ones with the most serious problems.

Before 1970, almost all of our transportation infrastructure was funded out of user fees and the United States had the best transportation system in the world. Since then, funding decisions have increasingly been made by politicians who are more interested in getting their pictures taken cutting ribbons than in making sure our transportation systems run safely and smoothly.

Proponents of higher gas taxes and other increased funding on infrastructure may talk about crumbling bridges, but what they really want is to spend more money on new projects that are often of little value. For example, they want high-speed trains that cost more but go less than half the speed of flying and light-rail trains that cost more but can move fewer people than buses.

This country doesn't need more infrastructure that it can't afford to maintain. Instead, it needs a more reliable system of transport funding, and that means one based on user fees and not tax subsidies.

Randal O'Toole is a senior fellow with the Cato Institute and author of Gridlock: Why We're Stuck in Traffic and What to Do About It.

THE WALL STREET JOURNAL.

Blow Up the Tax Code and Start Over

Apply a 14.5% flat tax to personal income and to businesses. Cut deductions. Watch the economy roar.

By RAND PAUL

June 17, 2015 7:09 p.m. ET

Some of my fellow Republican candidates for the presidency have proposed plans to fix the tax system. These proposals are a step in the right direction, but the tax code has grown so corrupt, complicated, intrusive and antigrowth that I've concluded the system isn't fixable.

So on Thursday I am announcing an over \$2 trillion tax cut that would repeal the entire IRS tax code—more than 70,000 pages—and replace it with a low, broad-based tax of 14.5% on individuals and businesses. I would eliminate nearly every special-interest loophole. The plan also eliminates the payroll tax on workers and several federal taxes outright, including gift and estate taxes, telephone taxes, and all duties and tariffs. I call this “The Fair and Flat Tax.”

President Obama talks about “middle-class economics,” but his redistribution policies have led to rising income inequality and negative income gains for families. Here's what I propose for the middle class: The Fair and Flat Tax eliminates payroll taxes, which are seized by the IRS from a worker's paychecks before a family ever sees the money. This will boost the incentive for employers to hire more workers, and raise after-tax income by at least 15% over 10 years.

Here's why we have to start over with the tax code. From 2001 until 2010, there were at least 4,430 changes to tax laws—an average of one “fix” a day—always promising more fairness, more simplicity or more growth stimulants. And every year the Internal Revenue Code grows absurdly more incomprehensible, as if it were designed as a jobs program for accountants, IRS agents and tax attorneys. Polls show that “fairness” is a top goal for Americans in our tax system. I envision a traditionally All-American solution: Everyone plays by the same rules. This means no one of privilege, wealth or with an arsenal of lobbyists can game the system to pay a lower rate than working Americans.

Most important, a smart tax system must turbocharge the economy and pull America out of the slow-growth rut of the past decade. We are already at least \$2 trillion behind where we should be with a normal recovery; the growth gap widens every month. Even Mr. Obama's economic advisers tell him that the U.S. corporate tax code, which has the highest rates in the world (35%), is an economic drag. When an iconic American company like Burger King wants to renounce its citizenship for Canada because that country's tax rates are so much lower, there's a fundamental problem.

Another increasingly obvious danger of our current tax code is the empowerment of a rogue agency, the IRS, to examine the most private financial and lifestyle information of every American citizen. We now know that the IRS, through political hacks like former IRS official Lois Lerner, routinely abused its auditing power to build an enemies list and harass anyone who might be adversarial to President Obama's policies. A convoluted tax code enables these corrupt tactics.

My tax plan would blow up the tax code and start over. In consultation with some of the top tax experts in the country, including the Heritage Foundation's [Stephen Moore](#), former presidential candidate Steve Forbes and Reagan economist Arthur Laffer, I devised a 21st-century tax code that would establish a 14.5% flat-rate tax applied equally to all personal income, including wages, salaries, dividends, capital gains, rents and interest. All deductions except for a mortgage and charities would be eliminated. The first \$50,000 of income for a family of four would not be taxed. For low-income working families, the plan would retain the earned-income tax credit.

I would also apply this uniform 14.5% business-activity tax on all companies—down from as high as nearly 40% for small businesses and 35% for corporations. This tax would be levied on revenues minus allowable expenses, such as the purchase of parts, computers and office equipment. All capital purchases would be immediately expensed, ending complicated depreciation schedules.

The immediate question everyone asks is: Won't this 14.5% tax plan blow a massive hole in the budget deficit? As a senator, I have proposed balanced budgets and I pledge to balance the budget as president.

Here's why this plan would balance the budget: We asked the experts at the nonpartisan Tax Foundation to estimate what this plan would mean for jobs, and whether we are raising enough money to fund the government. The analysis is positive news: The plan is an economic steroid injection. Because the Fair and Flat

Tax rewards work, saving, investment and small business creation, the Tax Foundation estimates that in 10 years it will increase gross domestic product by about 10%, and create at least 1.4 million new jobs.

And because the best way to balance the budget and pay down government debt is to put Americans back to work, my plan would actually reduce the national debt by trillions of dollars over time when combined with my package of spending cuts.

The left will argue that the plan is a tax cut for the wealthy. But most of the loopholes in the tax code were designed by the rich and politically connected. Though the rich will pay a lower rate along with everyone else, they won't have special provisions to avoid paying lower than 14.5%.

The challenge to this plan will be to overcome special-interest groups in Washington who will muster all of their political muscle to save corporate welfare. That's what happened to my friend Steve Forbes when he ran for president in 1996 on the idea of the flat tax. Though the flat tax was surprisingly popular with voters for its simplicity and its capacity to boost the economy, crony capitalists and lobbyists exploded his noble crusade.

Today, the American people see the rot in the system that is degrading our economy day after day and want it to end. That is exactly what the Fair and Flat Tax will do through a plan that's the boldest restoration of fairness to American taxpayers in over a century.

Sen. Paul, a Republican from Kentucky, is running for his party's presidential nomination.

The New York Times

The Other Veterans Scandal

By MICHAEL F. CANNON and CHRISTOPHER A. PREBLE JUNE 15, 2014

WASHINGTON — THE Department of Veterans Affairs is mired in scandal. More than 57,000 veterans have been waiting at least three months for a doctor's appointment. Another 64,000 never even made it onto a waiting list. There are allegations that waits for care either caused or contributed to veterans' deaths.

But another, even larger problem with the Department of Veterans Affairs is being overlooked: Even when the department works exactly as intended, it helps inflict great harm on veterans, active-duty military personnel and civilians.

Here's how. Veterans' health and disability benefits are some of the largest costs involved in any military conflict, but they are delayed costs, typically reaching their peak 40 or 50 years after the conflict ends. Congress funds these commitments — through the Department of Veterans Affairs — only once they come due.

As a result, when Congress debates whether to authorize and fund military action, it can act as if those costs don't exist. But concealing those costs makes military conflicts appear less burdensome and therefore increases their likelihood. It's as if Congress deliberately structured veterans' benefits to make it easier to start wars.

The Department of Veterans Affairs is supposed to help wounded veterans, but its current design makes soldiers more likely to get killed or injured in the first place. The scandal isn't at the Department of Veterans Affairs. The scandal is the Department of Veterans Affairs.

Is there a better way? We propose a system of veterans' benefits that would be funded by Congress in advance. It would allow veterans to purchase life,

disability and health insurance from private insurers. Those policies would cover losses related to their term of service, and would pay benefits when they left active duty through the remainder of their lives.

To cover the cost, military personnel would receive additional pay sufficient to purchase a statutorily defined package of benefits at actuarially fair rates. The precise amount would be determined with reference to premiums quoted by competing insurers, and would vary with the risks posed by particular military jobs.

Insurers and providers would be more responsive because veterans could fire them — something they cannot do to the Department of Veterans Affairs. Veterans' insurance premiums would also reveal, and enable recruits and active-duty personnel to compare, the risks posed by various military jobs and career paths.

Most important, under this system, when a military conflict increases the risk to life and limb, insurers would adjust veterans' insurance premiums upward, and Congress would have to increase military pay immediately to enable military personnel to cover those added costs.

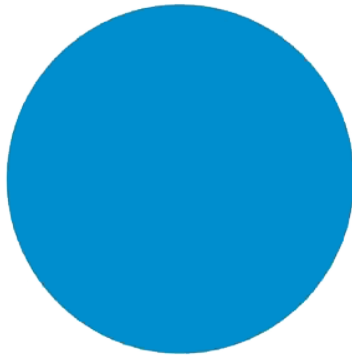
Consider how this system might have prevented Congress's misbegotten decision to authorize President George W. Bush to invade Iraq. In 2002, the Bush administration played down estimates that the war would cost as much as \$200 billion, insisting the cost would be less than \$50 billion. To give you a sense of how mistaken this was: The economists Linda J. Bilmes and Joseph E. Stiglitz put the cost of veterans' benefits alone, from the wars in Iraq and Afghanistan, at roughly \$1 trillion.

Like others before her, Hillary Rodham Clinton has admitted that voting to authorize the Iraq invasion was a "mistake," though she "made the best decision I could with the information I had." How many members of Congress would have voted differently if confronted with the long-term health and disability needs of the troops they had already sent into Afghanistan and those they were sending into Iraq? How many would have pressed harder to end the wars sooner if they had to confront the mounting cost of veterans' benefits, in addition to the wars' other growing costs, every year the wars dragged on?

The alternative system we propose combines the universal goal of improving

veterans' benefits with conservative Republicans' preference for market incentives and antiwar Democrats' desire to make it harder to wage war. Pre-funding veterans' benefits could prevent unnecessary wars, or at least end them sooner. We can think of no greater tribute to the men and women serving in our armed forces.

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Wall Street offers very real benefits: Opposing view

But headlines focus on the bad behavior.

Thaya Knight 7:16 pm May 26, 2015

Not every person on Wall Street is a morally corrupt Gordon Gekko. Do Wall Street traders want to make money? Yes. Are they generally people who thrive in a fast-paced, competitive environment? You bet. And that is a good thing.

At its core, here's what Wall Street does: It makes sure that companies doing useful things get the money they need to keep doing those things. Do you like your smartphone? Does it make your life easier? The company that made that phone got the money to develop the product and get it into the store where you bought it with the help of Wall Street.

When a company wants to expand, or make a new product, or improve its old products, it needs money, and it often gets that money by selling stock or bonds. That helps those companies, the broader economy and consumers generally.

When we have flashing headlines about Wall Street traders acting badly, as we had last week with news of five major banks pleading guilty to criminal charges, it is very easy to hate Wall Street. But we only hear headlines about the worst behavior.

No one writes news stories about traders who go about their business every day, carefully complying with the many (and there are many) rules and regulations that govern their work. Also, the financial sector, which is usually what people mean when they say "Wall Street," isn't only or even mostly the big banks.

There are small firms, banks, funds and advisers that make up a large portion of our financial industry. While the news about corruption, corporate welfare and lawbreaking is very bad, it doesn't mean the entire industry is rotten. Or, more important, that we don't need it.

Wall Street could be better. We could eliminate regulations that crowd out competition for the big banks. We could reform the system to do away with "too big to fail," making it harder for bad traders to get away with bad behavior. Either way, we shouldn't lose sight of the very real economic and social benefits Wall Street provides.

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