

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 opinion causes a concomitant change in a related opinion. 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 that suggests an identification strategy for detecting dynamic constraint. We present evidence from survey experiments conducted with convenience samples of both the mass public and of political elites. Our results indicate that even among respondents whose belief systems are highly constrained in the static sense, a change in one attitude need not precipitate 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. The lack of dynamic constraint among our elite sample raises the question of how they come to hold political opinions that are constrained in a static sense. We present an experiment that suggests a potential explanation: elites may be more likely to be chided for expressing inconsistent positions.

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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 (2018) 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

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)

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), but see Jewitt and Goren (2016), who find that increases occurred only among the most politically interested and active), and that mass constraint may be higher outside the United States (Converse and Pierce 1986; Malka, 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 (2019) 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. In order 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.

Two important gaps persist in the literature on dynamic constraint. The first is a lack of formalization. Converse devotes just a few sentences to the topic of dynamic constraint and does not lay out an identification strategy that would allow researchers to detect it empirically. One contribution of the current paper is to present an array of psychological models that exhaust all of the ways that an exogenous shift in a given idea-element might affect other idea-elements. This exercise provides a mapping between theory and data, allowing us to identify patterns of empirical results that would be consistent with psychological processes that give rise to dynamic constraint. The second is a dearth of experiments designed to introduce these exogenous shifts in a given idea-element. Our empirical contribution is to offer novel experimental results for both mass and elite samples. 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. Taken together, our experiments underscore Converse’s contention that apparent static constraint may actually exaggerate the functional interdependence of a belief system. Even among elite respondents, who display a higher degree of consistency in their policy opinions across domains, opinion change occurs in a localized and fragmented fashion. These results suggest a hitherto unappreciated puzzle: If elites tend not to display dynamic constraint, how did they come to hold correlated attitudes that imply static constraint? We speculate, based on experimental results suggesting that respondents can be chided into expressing more consistent views, that the answer may lie in the environmental forces that monitor and reward elites’ consistency.

Formalizing the Empirical Implications of Dynamic Constraint

Unlike constructs such as liberalism-conservatism 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 formalize these assumptions so as to clarify the connection between the underlying theoretical process and observable patterns that could emerge from a randomized experiment. Doing so not only tells us what kinds of experiments might be informative; it also alerts us to the limits of what can be learned from such experiments.

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, or 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.

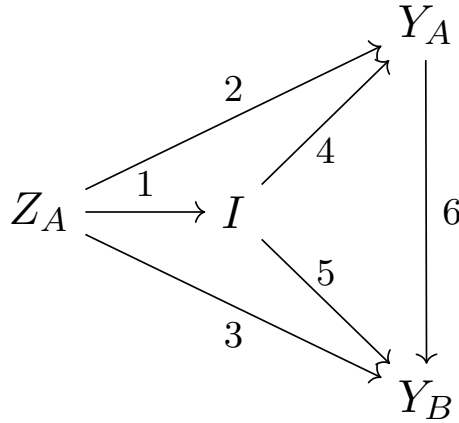
To this set of four variables, we apply a set of assumptions that are equivalent to a family of exclusion restrictions.

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 merit 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 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.²

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 forces and ideology interact to induce changes in opinions. The 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 practice, two empirical strategies are used to draw inferences from putatively exogenous inputs such as information, argumentation, frames, and the like. The first is to track non-experimental changes in these inputs over time (e.g., due to a major political event) or across individuals (e.g., across segments of the public that are or are not aware of such an event). The problem with over time comparisons is that sudden events are often “compound treatments” in the sense that they bear on many policy domains simultaneously and could well influence both Y_A and Y_B directly. Comparisons across people with different levels of exposure to a given input pose problems of omitted variables bias, since we cannot tell whether different levels of exposure influence opinions

²Although some combinations of pathways might be characterized as imposing “vertical” as opposed to “horizontal” constraint insofar as they induce change by invoking core values (Pollock, Lilie and Vittes 1993), we remain agnostic about this distinction. Converse does not use these terms, and either process could contribute to dynamic constraint.

or merely reflect unobserved attributes that are correlated with different opinions. The inferential challenges of omitted variables bias can be sidestepped by randomly assigning exposure to the exogenous intervention. Experiments do not resolve the question of *how* a manipulation that targets Y_A might affect Y_B , which is why our identification strategy is agnostic on this question, allowing for many possible causal scenarios.

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?

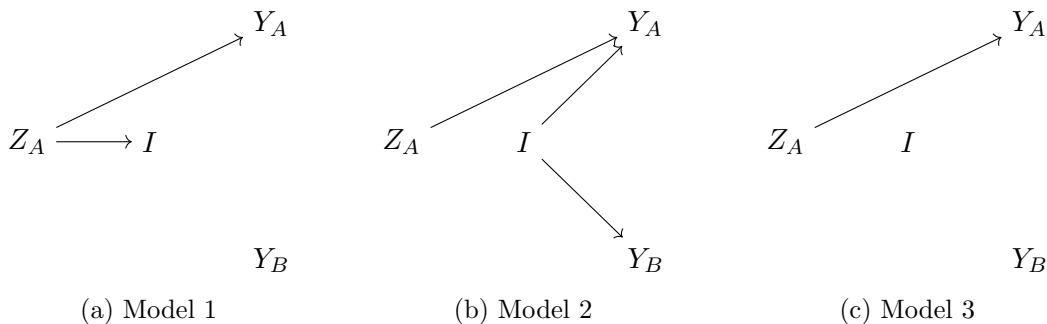
Table 1 is organized by the four empirical patterns that our experiments could reveal. If we find that Z_A affects Y_A but not Y_B , we know that (under assumptions 1-3), one of the first three models must have generated the data. If we find that Z_A affects Y_B but not Y_A , the data must have been 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 model 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 somewhat hollow because of course nothing mediates a truly null effect. 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 non-target

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



attitudes is a necessary but not sufficient condition for demonstrating dynamic constraint.³

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 among these three models.⁴

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.

This identification approach departs markedly from the existing literature. Quite a bit of research on mass opinion is concerned with *scaling* subjects on one or possibly more ideological di-

³In Appendix C, we provide a slight elaboration of our framework in which attitudes could affect ideology, which then in turn influences other attitudes. The conclusions we draw from our data analysis do not depend on which version of the theory we employ, but we present both for completeness.

⁴Furthermore, 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 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.

mensions. Some approaches include asking subjects to place themselves on scales that range from extremely liberal to extremely conservative (see Kinder and Kalmoe 2017, Appendix A). Other approaches estimate subjects’ latent ideological ideal points by positing an underlying factor structure, then fitting that model to subject responses to policy questions (e.g., Table 5 in Jewitt and Goren 2016). The I in our causal diagram is far more agnostic with respect to how changes in I affect attitudes; we deliberately avoid imposing structure on I , which is unobserved in our studies. Second, in contrast to studies of static constraint, our estimand is not the correlation between policy attitudes. Instead, we seek to estimate the extent to which interventions that affect a target attitude also affect non-target attitudes.

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 the original findings but rather to repurpose the data in order to examine the downstream effects of the experimental intervention on opinions in other domains.

We begin with a survey experimental investigation of the effects of newspaper opinion pieces.⁵ 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. While we do not have direct measures of how much more ideologically sophisticated the elite sample is than the mass public sample, we do have information on their education and strength of partisanship. A full 96% of the elite sample holds a college degree, compared with 51% of the MTurk sample; 71% of the elites holds a graduate degree, compared with 13% for the MTurk

⁵The results of this experiment on target attitudes were published in [citation withheld]. Here we focus on the effects of the manipulations on non-target attitudes, which (as described in the previous section) provides the crucial test of dynamic constraint.

sample. Nearly half – 44% – of the elite sample describes themselves as “strong” partisans, but only a quarter of the MTurk sample chooses these most extreme options. The most distinctive attribute of the elite sample is its extremely high level of engagement with political news: 97% say they read the news at least daily, and 64% say they talk about politics with their family, friends, or coworkers at least once per day.⁶

Both sets of respondents 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.⁷ The treatment op-eds were all actual 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. Accordingly, the outcome variables are all scored so that higher values indicate higher agreement with the libertarian position.⁸ Each treatment op-ed is associated with a “target” attitude that is measured with four outcome questions. 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 non-target issues.

The op-eds themselves took up a diverse set of topics that at first glance seem somewhat unrelated. The common thread across all four op-eds, however, is criticism of wasteful government spending and bureaucratic inefficiency. The Amtrak op-ed concludes “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.” In his op-ed, Senator Rand Paul summarizes his views on taxes as, “The tax code has grown so corrupt, complicated, intrusive and antigrowth that I’ve concluded the system isn’t fixable.” The veterans piece opens with a description of how poorly government delivers healthcare: “More than 57,000 veterans

⁶See the supplemental materials for additional descriptive information about the two convenience samples.

⁷In 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.

⁸See the appendix for the full text of the op-eds and all outcome questions.

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.” The op-ed defending Wallstreet bankers closes with a call for further financial deregulation: “We could eliminate regulations that crowd out competition for the big banks.” If indeed these op-eds were to affect attitudes on nontarget attitudes, we suspect that they would do so by eroding confidence in government’s ability to deliver public goods and services efficiently and to create the conditions for the free market to deliver private goods and services. In this setting “functional interdependence” would mean applying the criticism of government waste in one domain to attitudes in a different domain.

We can confirm the hypothesis 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 ($16 \text{ choose } 2 = 120 \text{ pairs}$). 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. Reassuringly, the inter-item correlations are stronger for the pairs that measure the same attitude than pairs that measure different attitudes in both samples. 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. In the Elite sample, the average correlation between attitudes within domain was 0.39, while the average across domains was 0.22. On MTurk, the average correlation between attitudes in the same domain was modest at 0.29, and the average correlation between attitudes in different domains was very small at 0.06.⁹

This familiar pattern of static constraint (high levels among elites but low levels among the general public) is strongly contrasted by the uniformly low levels of dynamic constraint shown in

⁹The 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 3: Study 1 Experimental Design

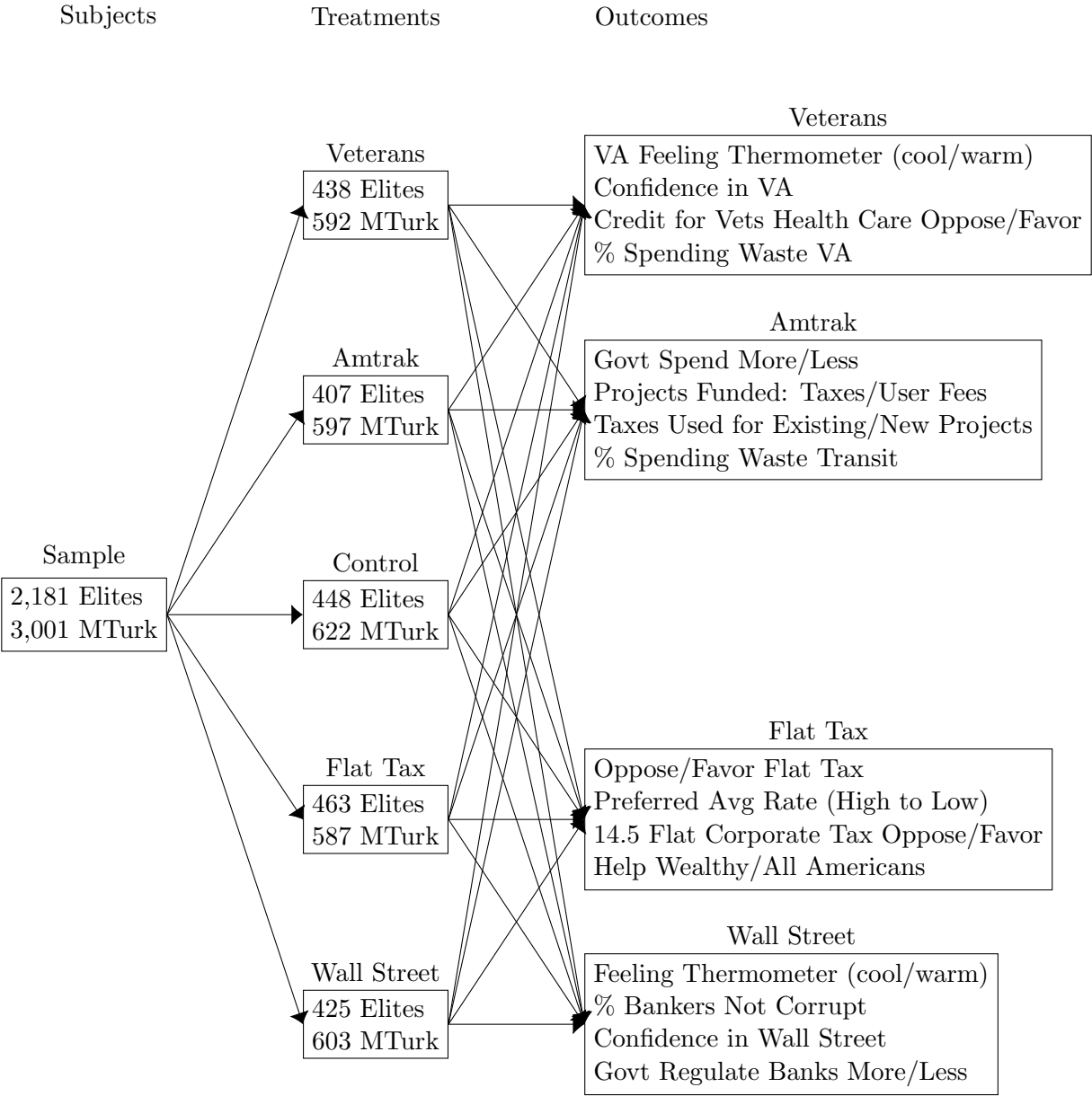


Figure 4: Correlations among Outcome Variables by Study Sample

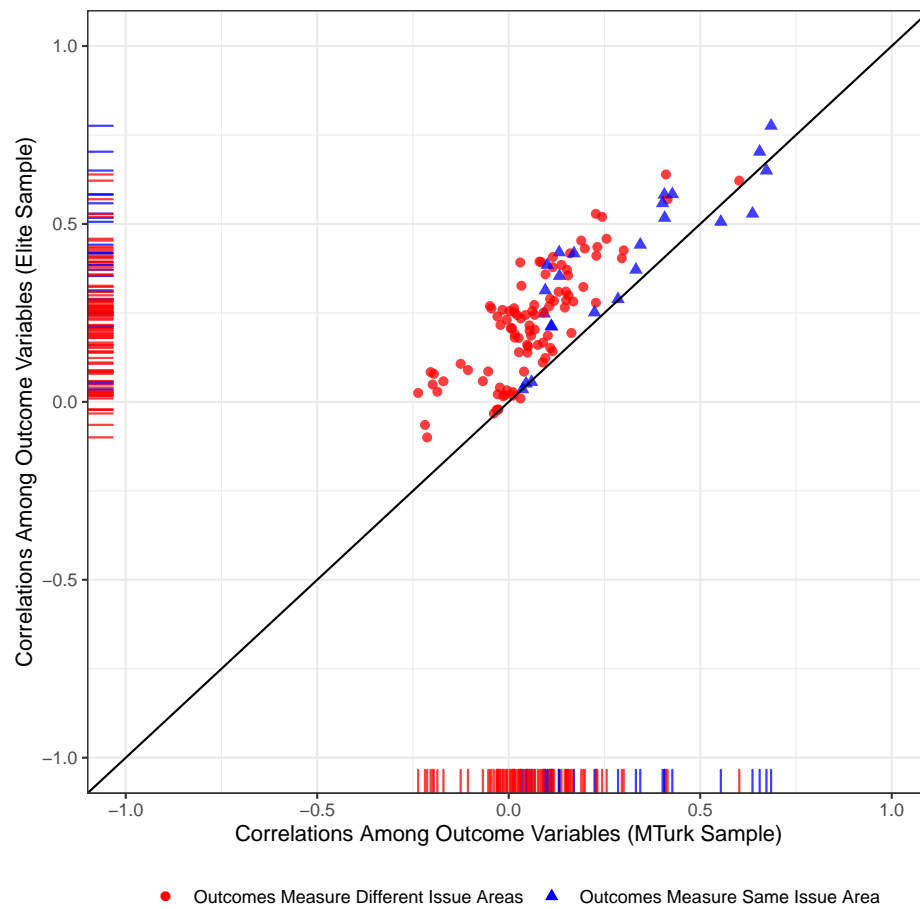


Figure 5. The figure has 16 panels, each of which corresponds to the effects of an op-ed on a set of outcome measures. The rows of panels correspond to the four attitudes, each of which are measured with four questions. The columns are separate treatments. The points and bars within each panel represent 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 on the *non-target* issues (the effects of Z_A on a series of Y_B 's.).

All of the reported 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. 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. 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. 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. Overall, 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 non-target issues. For example, in the second row of the first column, we see how the flat tax treatment affected opinions concerning Wall Street; in the second column of the first row, we see how the Wall Street treatment affected attitudes about the flat tax. In nearly every case, the estimates are very close to zero. Out of 96 estimated effects on non-target issues, only 8 of the effects are significant, and when we apply the

Benjamini-Hochberg correction, none of these estimates is deemed significant.

In summary, 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 non-target 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 non-target 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 non-target 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 interval¹⁰ 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 indirect effects on the order 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 non-target domains.

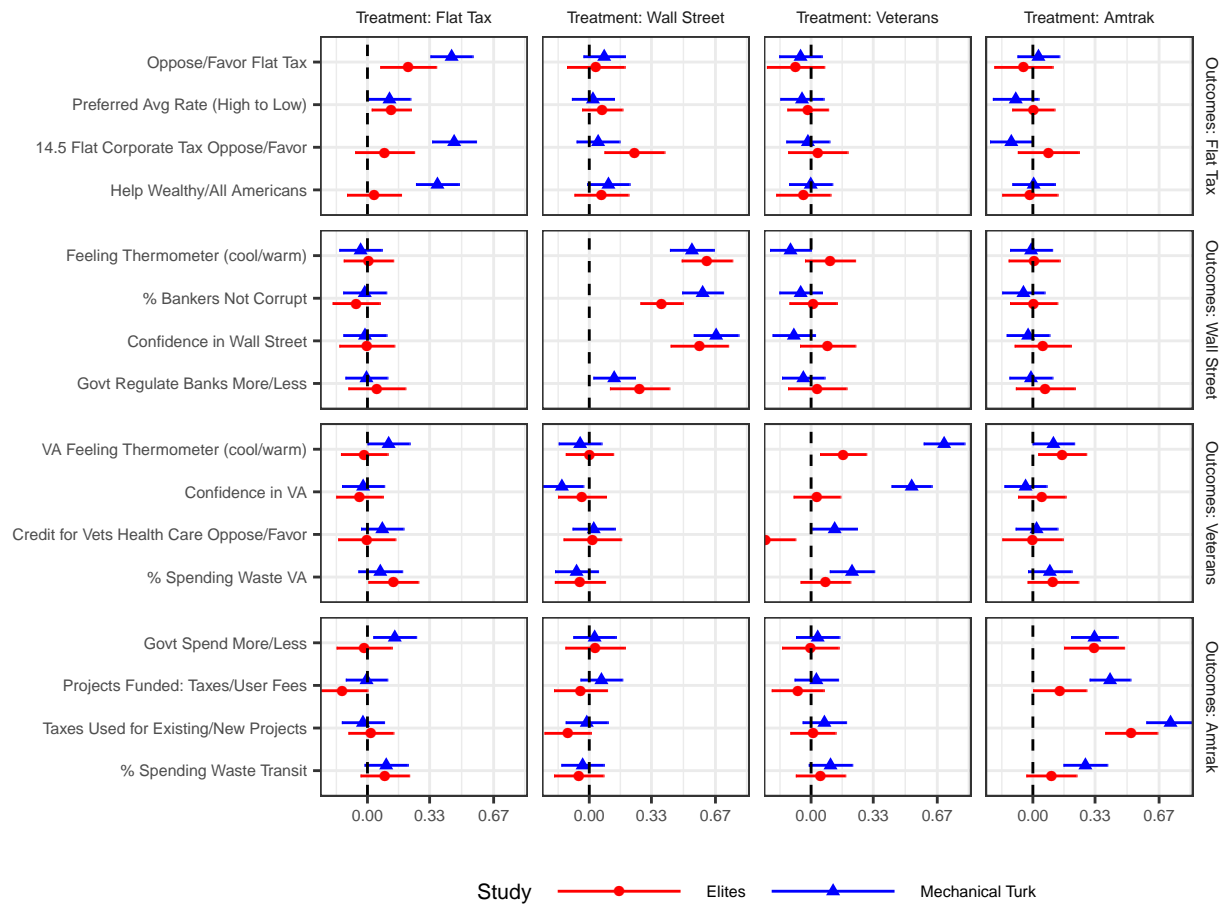
Study 2: Priming to Induce Constraint within a Policy Domain

Study 2 asks the further question of whether priming some feature of an attitude can influence the expression of related attitudes. The 1,087 subjects for this experiment were gathered via Lucid, a provider of online convenience samples that have been quota sampled to match U.S. Census margins (Coppock and McClellan 2019). As shown in the Appendix, the demographic targeting is not perfect. Our Lucid sample is more female and somewhat better educated than the national population.

Subjects were shown one version of the following question at random, with the experimental

¹⁰Because the treatment effect estimates are nonindependent, we constructed a confidence interval around this ratio by bootstrapping at the respondent level.

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



variation in wording indicated in brackets: “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 religious convictions manipulation to increase our subjects’ view that discrimination against gay men 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 putatively related 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]

Same-sex Marriage Amendment: 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 Amendment: 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 additional outcome measures did not change significantly in the wake 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, but all are close to zero and statistically insignificant. When primed to consider a value – affirmation of others’ religious convictions – people become substantially more accepting of

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 < .05

Robust standard errors are in parentheses.

job discrimination against gay men and lesbians, but this predisposition evidently has no bearing on the way they adjudicate the tension between religious convictions and processing marriage licenses for same-sex couples, nor does it change the weight they accord religious conviction on other church-state issues. What Singer (1966, p.69) called the “drive for consistency” seems to operate very weakly in the domain of political attitudes, even when respondents are asked to address closely connected issues.¹¹

Discussion

Since the publication of Converse’s famous essay, the empirical and theoretical literature on ideological constraint has focused almost exclusively on static constraint, often measured by the correlation among respondents’ political attitudes across issues. With the exception of Peffley and Hurwitz (1992), which focused on foreign policy views, Converse’s notion of dynamic constraint has largely escaped academic scrutiny.

One reason why dynamic constraint may have been understudied to date is that the research design challenges are somewhat daunting. First, we need some exogenous shock that changes a target attitude. Second, we have to be able to credibly assume that the shock does not itself directly affect non-target attitudes, but instead affects non-target attitudes only through an intermediate variable, ideology. Finally, the object of study is a causal mediation process (always

¹¹ Relatedly, Hopkins and Mummolo (2017) assesses the “breadth” of framing effects by randomly exposing subjects to short arguments made by U.S. senators, then measuring subjects spending preferences in both target and non-target domains. For the most part, arguments against wasteful spending in one domain affected target spending preferences and did not influence non-target preferences.

difficult to study, even experimentally (Imai, Tingley and Yamamoto 2013)), but here we have the added challenge that the mediator is unobserved. We addressed the first two challenges by using a randomized design in which we were in control of both the allocation and the content of the treatments. We grappled with the third challenge by enumerating the full set of theoretical models that met our foundational assumptions, mapping those into observable empirical patterns, then using our experimental estimates to rule out some models in favor of others.

What did we find? In Study 1, the intervention 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 non-target issues, and the average spillover effect is just 1.4% of the first-stage direct effect. Study 1 covered an assortment of substantive issues, and our results are consistent across all of them. Inducing attitude change in one domain does not bring about change in related domains. The only theoretical models that are consistent with this empirical pattern rule out ideology as a mediator. On this basis, we conclude that our subjects' attitudes were not dynamically constrained.

A possible criticism of Study 1 is that some of the issues are too technical and opinion change on one of them is just difficult to apply to the others. Study 2 remedies this by studying the “easy” issue (Carmines and Stimson 1980) of employer discrimination against gay men and lesbians, which is neither technically complex nor primarily about means versus ends.¹² Reminding subjects that some employers may have religious objections clearly increases subjects' support for discrimination, but this change does not spill over into closely related issues like school prayer or same-sex marriage. In other words, those randomly induced to condone discrimination against gay men and lesbians are no more likely than their control group counterparts to condone discrimination on the closely related topic of same-sex marriage. Study 2, like Study 1, offers little evidence of dynamic constraint.

¹²Although the experimental literature on persuasion has not addressed dynamic constraint explicitly, three studies are pertinent to the easy issues in which social group attitudes figure prominently. Finseraas and Kotsadam (2017) find 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. Trump and White (2018) use a clever manipulation to change respondents' beliefs about the trajectory of income inequality in the U.S. This manipulation substantially changed beliefs about inequality but did not cause corresponding shifts in “system justification.” 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.

Another reason that dynamic constraint has received less scholarly attention than it deserves is that observers may have assumed that because levels of *static* constraint have been low, the scope for politically meaningful *dynamic* constraint is relatively limited. This intuition seems flawed – there is no necessary connection between levels of dynamic and static constraint. To see this, consider a set of subjects among whom Y_A and Y_B are very weakly correlated, which is to say static constraint is low. Now imagine a treatment Z_A that successfully increases target attitude Y_A for all subjects and, via dynamic adjustment, also increases non-target attitude Y_B for all subjects by the same amount. The two post-treatment attitudes Y_A and Y_B remain just as uncorrelated as before but have moved together via a dynamically constrained process. While we do not find this pattern in our experiments, it was of course a possibility *ex ante*.

The lack of a necessary connection between static and dynamic constraint is underscored by a comparison of the elite and mass public samples in Study 1. These two sets of respondents displayed the same pattern that Converse documented decades ago: low static constraint in the mass public, but comparatively higher static constraint among the elites. Although we were able to generate strong treatment effects on target attitudes for both mass and elite, our treatments had essentially zero effect on non-target attitudes for both groups. 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.

This disjuncture raises a theoretical puzzle. Why do we observe relatively high levels of static constraint among elites if they did not arrive at them dynamically? That is, how do elites construct their ideologies, if not by applying lessons from one domain to others? Our Study 2 included a small additional experiment that suggests one possibility. We gave respondents an opportunity to contradict themselves when expressing their opinion about spending preferences, using the well-documented tension between opposition to spending in general and support for spending in specific domains (Sears and Citrin 1982). 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 full results of this experiment may be found in the supplementary materials, but the bottom

line is clear: when subjects who expressed inconsistent views are chided about it, the opinions they express subsequently tend to be much more consistent.

By extension, when party elites and members of Congress express ideologically inconsistent views, they immediately encounter exactly the sorts of criticism that induce consistency. We therefore hypothesize that it may be the strength and frequency of encouragement that distinguishes elites from masses, as the former are continually admonished by party leaders, interest groups, and commentators to maintain their fealty to a body of ideological ideas. For those on the front lines of politics, the encouragement that we administered experimentally is meted out daily and often with great vehemence.

With 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. Our studies only consider a few of the issue domains across which dynamic constraint could manifest itself, and more work is needed to assess whether our results hold for other opinion domains and other types of interventions that could precipitate opinion change. That said, judging from the evidence at hand, it appears that support for Converse's position is perhaps even stronger than he suspected. Changes in opinion do not ramify across domains, at least not without strong encouragement, which for most people rarely occurs.

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