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ON POLICY FEEDBACK: INSIGHTS FROM SURVEY EXPERIMENTS

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ABSTRACT

In comparative social science, policy feedback has become a widely popular device with which to understand policy persistence and the impacts of state-making and political entrepreneurship on mass opinion. Although the existence of such effects is frequently taken for granted, recent work has challenged prevailing assumptions about the unproblematic nature of feedback from policy change to mass opinion. This is an opportune time to put policy feedback to further test. We do so by bringing to bear the two main theoretical perspectives that underlie established and recent scholarship, and applying for the first time survey experiments to evaluate key expectations. Focusing on the relatively novel domain of counter-terrorism policy, we analyze data drawn from a national survey conducted in 2009. Results from embedded experiments suggest new evidence for policy feedback effects. Analysis of mechanisms suggests limits in interest-centered explanations, and the relevance of some under-studied, cognitive factors. We discuss implications and limits of our study for policy feedback scholarship, and with further reference to the case of U.S. attitudes toward the war on terror.

On Policy Feedback: Insights from Survey Experiments

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On Policy Feedback: Insights from Survey Experiments

The concept of policy feedback has become a popular, even defining, tool of comparative social science. When new laws are enacted or existing policies extended, such changes are said to redirect preferences and expectations on the part of citizens, interest groups, and parties. That is policy feedback. It is encapsulated by the expression “policies produce politics” (Pierson 1993).

Policy feedback has furnished analysts with a perspective from which to understand self-reproducing dynamics and the historical “stickiness” of certain public policies and institutions. For instance, the design of welfare policies shapes how citizens view program beneficiaries (Gilens 1999; Steensland 2006; Bruch Ferree, and Soss 2010), in turn molding public attitudes toward government programs. In social democracies, a universalistic distribution of entitlements (Korpi and Palme 1998; Rothstein 1998; Svallfors 2006) buttresses welfare state legitimacy and persistence. More generally, a nation’s institutional arrangements are frequently viewed as becoming as self-maintaining as the QWERTY keyboard (Lijphart 1994; Powell 2000; Alexander 2001).

But alongside successful applications of the policy feedback thesis has been a limitation. Here, a tendency is to focus on “positive” cases, where policy adoption ultimately leads to acceptance on the part of national populations or interest groups. But what happens when new pieces of legislation fail to secure legitimacy, or simply leave mass attitudes unmoved?

Until quite recently, questions of this sort were not a priority in policy feedback scholarship. But their growing importance is powerfully demonstrated by new work challenging views of a straightforward process of feedback from policy change to mass opinion-formation. Analyzing survey data before and after the 1996 reform of Aid to Families with Dependent Children, Soss and Schram (2007) find no evidence of change in public attitudes towards the poor and towards specific welfare programs. Also focusing on the case of U.S. welfare reform, Dyck and Hussey (2008) find that AFDC retrenchment failed to dislodge the strong impacts of racial attitudes on policy opinions.

These impacts persisted even though changes in means-tested benefits were also accompanied by declining stereotypes of welfare recipients in the media (Dyck and Hussey 2008, p. 596).

How can we square these results with firmly-entrenched expectations from the policy feedback literature? There is a deep-seated yet largely unarticulated clash of theoretical perspectives at hand. In established applications, feedback from new legislation to mass attitude-formation, particularly as involving state-making or programmatic initiatives (Pierson 1993; Huber and Stephens 2001; Mettler and Soss 2004), is often seen as routine and unproblematic. This is because citizens view their interests as tied to the new regime, and policy-makers often design programs with an eye to enhancing perceptions of legitimacy (May 1991; Kumlin 2002; Svallfors 2010).

In contrast, an alternative perspective, one implicated in a major strain of cognitive-psychological scholarship (Gilovich, Griffin, and Kahneman 2002), is that prior beliefs and biases filter how individuals reason about, and respond to, policy change. This makes the influence of policy much more contingent. Of further significance is the phenomenon of “motivated” reasoning (Ditto and Lopez 1992; Jost Federico, and Napier 2009), where entrenched beliefs may lead to negative views of policy modification to the status quo. Here, individuals tend to disregard environmental changes or stimuli that are inconsistent with their preferences.

The clash between the preceding views of policy feedback is important. At stake are sharply divergent expectations about what causal factors matter, and just when feedback is likely to occur. When coupled with a suitable research design, we believe a closer engagement with these competing perspectives offers new insights for policy feedback scholarship.

This is where the current study comes in. We start by laying out key assumptions and expectations underlying previous studies, distinguishing global versus contingency perspectives. *Global* perspectives see feedback as common: policy change shifts citizens’ perception of interests, or instead elicit compliance through cognitive dissonance reduction. *Contingency*-oriented alternatives

point to a much stronger role for *prior* beliefs and preferences. Now citizens' pre-existing biases filter (and render less predictable) their responses to policy change, making policy feedback less common, and dependent upon which biases are activated in specific contexts.

We seek to advance scholarly understanding of these two perspectives with data from a new national survey, *The 2009 Survey of American Policy Attitudes*. *SAPA* fields experiments into the dynamics of opinion formation, enabling us to systematically gauge how policies influence opinions as “treatment effects” (Vergne and Durand 2010; see also Chong and Druckman 2007). Survey experiments combine probability samples' generalizability with a way of separating information about policy change from other stimuli with which they are typically correlated. Ours is the first study (of which we are aware) to bring experimental methods to the study of policy feedback.

We focus on the case of counter-terrorism policy and the dynamics of opinion formation in the United States (Davis 2007; Merolla and Zechmeister 2009). Investigating a relatively novel policy domain, we offer results that complement and extend those based on social policy-making domains. We discuss how findings shed new light on theoretical tensions between global versus contingency perspectives on policy feedback dynamics. Of course, there are limitations in our study pertaining to methods and substantive focus, and we discuss these as well in conclusion.

TWO PERSPECTIVES ON POLICY FEEDBACK

State-Centered Theories and Global Feedback

The presumption of much established scholarship is that policy feedback is common and relatively unproblematic. This view owes much to the pioneering work of North (1990), who demonstrated how even inefficient economic institutions tend to persist because their rules continually shape organizations and players operating within their fields. A rupture in this path dependent process requires the appearance of a new actor, one who would benefit from radical

modifications to existing rules, while at the same time having the capacity to successfully pursue implementation. Severe contradictions between economic and political institutions (North and Shirley 2008), or the intervention of an exogenous organization (Boettke et al. 2008) could facilitate this type of change. But for the most part this is seen as an infrequent phenomenon.¹

North did not initially consider policy feedbacks and mass opinion as such, but comparative social scientists soon became interested in precisely such applications. A pioneer in this theory transfer was Pierson (1993; 1996; see also Skocpol 1992), who argued that social welfare programs had reshaped the structure of interests in developed capitalist societies. So dependent are most citizens on some form of government provision that alternatives to the status quo, especially in the form of spending cuts, are vigorously resisted. Policy feedbacks should be seen as “ratchets,” establishing “the new point of reference for discussions on further welfare state development” (Huber and Stephens 2001, p. 3).

Key to these policy feedback applications is that politicians and political institutions are fundamental influences on the lives and orientations of citizens. Indeed, this has been a defining assumption of the “state-centered” turn in comparative social science (Evans, Rueschemeyer, and Skocpol 1985). Together, this leads to what we term the *global* feedback thesis, where new laws or a redirection of policy are expected to be of major consequence for mass opinion-formation.

How and why do laws and policy tend to exert influence over mass attitudes? Scholars within the state-centered tradition have frequently pointed to interests as the central mechanism. The design of institutions and policy arrangements are seen as shaping citizens’ incentives to support specific policies (Pierson 1993, 2004; Huber and Stephens 2001; Iversen 2005). When governments

¹ “Wars, revolutions, conquest, and natural disasters are sources of discontinuous institutional change... But the single most important point about institutional change, which must be grasped if we are to begin to get a handle on the subject, is that institutional change is overwhelmingly incremental” (North, 1990, p. 89).

offer job-specific training and protections, members of high-skill occupations come to depend upon, and strongly support, such arrangements (Iversen and Soskice 2001).² In social-democratic nations, a universalistic distribution of benefits and services gives working- and middle-class citizens alike a vested interest in maintaining access to government provision (Korpi and Palme 1998).

But is there ever more to policy effects than economic incentives? What if national publics' dependence on existing institutions and their attitudinal responses to policy change have an intrinsically cognitive dimension? It is here we can readily discern a second and less explicitly articulated type of mechanism.³ We identify this mechanism as *cognitive dissonance* reduction, drawing from a longstanding tradition of psychological research on the subject.

Cognitive dissonance involves individuals adapting to behavioral or environmental change. They do so by adjusting attitudes to reduce psychic tensions. In Festinger's classic studies (1957; Festinger and Carlsmith 1959), individuals receiving minimal payment for completing a repetitive task changed their attitudes *more* (and in a more *positive* direction) than those given larger payments for the same task. Here, individuals seek to (unconsciously) avoid stress caused by behavioral compliance with environmental demands. There is a tendency to bring attitudes in line with new realities, making policy feedback a common phenomenon. But whereas interest-centered explanations see policy change as shaping mass attitudes by giving individuals new benefits, cognitive dissonance views policy change as shaping mass opinion by instead providing *little-to-no*

² In this context, there is a parallel between welfare state scholarship and the "dynamic representation" model of Erikson, MacKeun, and Stimson (2002). This model focuses on aggregated measures of policy legislation, yet its emphasis on rational expectations is analytically comparable to the interest-centered view of policy feedback. We do not seek in this study to unpack the dynamic representation model, but findings applicable to the global feedback approach may have relevance in future scholarship on the topic.

³ North's later work (2006) issues a call for greater consideration of cognitive processes that link individuals to institutions and institutional change, offering a sharp distinction between "interests" and (non-rational) processes of belief-formation. Similarly, Svallfors (1997, p. 172) hypothesizes that "Institutions provide 'normalcy,' that is, they suggest to people what is 'the normal state of affairs'..."

benefits. Both lines of thinking thus lead to global expectations of policy feedbacks as common, but they differ with respect to the mechanism responsible for such effects.

Festinger's results have been extended in a rich array of laboratory studies (Aronson, Wilson, and Akert 2006; Cooper 2007; Egan, Santos, and Bloom 2007), where, for instance, individuals who read material with which they disagree are more likely to *upwardly* revise their opinions when receiving no tangible rewards. Cognitive dissonance captures the broad class of cases where forced compliance may lead to attitude adjustment, even in the absence of clear reward or perceived improvement to welfare. If policy change leaves citizens with little alternative to compliance, cognitive dissonance is a candidate mechanism behind feedback effects on opinion-formation.

Voter Heuristics, Contingent Policy Feedback

Expectations of global policy feedback have been constructively challenged by the studies of U.S. welfare reform discussed in the introduction. Underlying these studies is a very different view of mechanisms behind opinion-formation, especially as regards processing of information about changes in the environment. This alternative view is best understood as emphasizing the *mediating* role of heuristics and biases. This leads to the expectation that policy feedback is a less common and more *contingent* phenomenon. Whether policy shifts reorient citizens' preferences depends upon the pre-existing biases that dispose some individuals to welcome (or instead reject) the changes at hand.

In Dyck and Hussey's study (2008), U.S. voters retained negative attitudes toward welfare programs and beneficiaries in the face of a transformation in the federal government's AFDC program. It is these enduring attitudes that suggest *heuristic* reasoning and the influence of prior biases. Like partisanship or ideological identification (Sniderman, Brody, and Tetlock 1991; Bartels 2002; Jost et al. 2009), for instance, racial attitudes can be a powerful filter of the information that

individuals are willing to accept.⁴ In the case of U.S. welfare reform, negative views of the assumed intersection of welfare dependency and race operated as a lens through which many voters saw the issues at hand. As such, they were not easily reshaped by simply downsizing the AFDC program.

In the heuristics and biases approach, then, individuals rarely encounter policy and other environmental change without recourse to prior beliefs (Gilovich et al. 2002). Of course, which particular set of beliefs are accessible is probabilistic (Iyengar 1990; Zaller 1992), varying across context as well as individual. But diverging from the expectation of state-centered theorizing, policy change and other shifts in the macro-environment do not always enjoy special causal status.

When it comes to how policy change shapes mass opinion, the heuristics and biases approach thus predicts that this influence will be mediated by which predispositions are activated in specific contexts. The impact of policy change interacts with prior beliefs. That leads to heterogeneous effects across the population, and, again, a more contingent view of policy feedback.

Consider, for instance, this study's empirical focus on counter-terrorism policies. It is conceivable that self-identified liberals and conservatives respond differently to information about policy change. If liberals oppose policies viewed as restricting rights, they may be unmoved by the specter of legislative change, in line with the "motivated reasoning" scenario.⁵ Policy change would thus interact with ideology, and this interaction makes policy feedback a contingent phenomenon.

Theoretical Expectations and the Case of Counter-Terrorism Policies

⁴ Of note in this context is how racial attitudes and biases, once acquired in childhood (Sears and Funk 1999) show considerable stability over the life course, paralleling the "aging/stability" found for partisanship and other ideological orientations (Alwin, Cohen, and Newcomb 1991; see also Krosnick and Berent 1993).

⁵ Motivated reasoning occurs when individuals' beliefs dispose them to reject news and other information perceived to be inconsistent with prior expectations (Ditto and Lopez 1992; Jost et al. 2009). In one classic study, individuals receiving negative medical news reacted by intensifying beliefs in their positive health status.

To summarize and recap, what should we expect if the global expectations associated with state-centered theorizing and established scholarship are met? Then, policy influence on opinion-formation should be common and consequential. It should operate by redefining citizens' interests or through cognitive dissonance.

But if citizens instead view policy change through the lens of prior beliefs, feedback process will be more contingent. Policy change will not always reorient opinion, and impacts will be mediated by which heuristics and biases are activated. Liberals and conservatives may, for instance, respond quite differently when confronted with the scenario of new counter-terrorism policies.

How can we use these competing perspectives to advance our understanding of policy feedback? The domain of counter-terrorism policy has much to offer scholars. Since the 9/11 terrorist attacks, counter-terrorism laws and policies have experienced a watershed series of changes in the United States.⁶ That provides a rich source of material for hypothesis-testing. Welfare state policy-making, while representing by far the dominant focus in policy feedback scholarship, shows a good deal more continuity. By the same token, the 1996 retrenchment of the AFDC program reveals the analytic benefits of research taking advantage of a policy domain experiencing extensive change.

A focus on counter-terrorism policies has a second analytic benefit. As discussed further below, it enables us to begin disentangling mechanisms underlying feedback effects involving interest/incentive-related factors. In particular, the expected utility of counter-terrorism policies involves calculations about the risk of future attacks. By directly measuring these calculations in the analyses, we can better gauge the impact of interest versus alternative factors behind policy feedback.

⁶ These include the 2001 Patriot Act, the 2006 Military Commissions Act, the 2008 Foreign Intelligence Surveillance Amendment Act (providing retroactive authorization for the National Security Agency's "warrantless wiretapping" program), and the establishment of military detention centers alongside the (now-banned) use of torture (Lichtblau 2008; Mayer 2008; Bergen and Tiedemann 2010).

When conjoined with the survey experiments discussed below, this empirical focus yields new findings about mechanisms underlying policy feedback effects on opinion.

EXPERIMENTS, DATA, AND MEASURES

Survey Experiments

In this study, we use experiments to test hypotheses and probe mechanisms behind policy feedback. Why experiments? A general benefit is to guard against selection bias (and methodological skepticism) more systematically than is possible in conventional, non-experimental research. Ours is the first study (of which we are aware) to employ experiments to analyze policy impacts on opinion-formation. This provides policy feedback scholarship with some new tools for analysis.

A second feature is that our experiments are embedded in a nationally-representative survey. This design harnesses the strengths of experiments to the generalizing power of the social survey. The use of embedded survey experiments has been central to advances in political psychology (Sniderman et al. 1991; Chong and Druckman 2007; see also Schuman and Presser 1977).

In the experiments we analyze, survey respondents are randomly assigned to policy change (*treatment*) and baseline (*control group*) conditions. In the control group, respondents answer a baseline question probing attitudes toward a counter-terrorism law or measure. In the experimental condition, policy change is a treatment-effect, and the baseline question is conjoined with reference to a specific instance of policy change. By virtue of randomization, difference in measured attitudes across control and treatment conditions can be attributed to the reference to policy change.⁷

⁷ We emphasize that we are exposing respondents to information about policy change they may already possess. By virtue of randomization, any pre-existing differences in information level about policy change are, of course, independent of exposure to the treatment at hand. As a result, estimation of policy feedback effects is methodologically conservative. This lends some confidence to results, and we return to this point in conclusion.

A New National Survey

Our data are from *The Survey of American Policy Attitudes (SAPA)*. Conducted in 2009, *SAPA* has a total sample size of 1,542, although estimation samples are smaller due to the design of experiments. Data collection was carried out by the Center for Survey Research at Indiana University. The instrument was pre-tested in April of 2009, and data collection took place in May and June. Additional details are discussed in Appendix I.

SAPA offers two experiments into policy feedback. In each, the baseline (control group) item probes respondents' opinions on a specific counter-terrorism policy or practice. Differences across control versus treatment groups are evaluated using analysis of variance and regression.⁸

Dependent Variables

Our first experiment concerns torture. As summarized in Table 1, there is a pair of treatment-effect conditions relating to policy change. The first prefaces the baseline item with reference to President Obama issuing an order stopping the “use of coercive interrogation methods.” The second treatment-effect prefaces the item with reference to past “use of coercive interrogation methods when interrogating suspects in places such as Abu Ghraib...” This is, by design, a demanding test for feedback because individuals would have to instantaneously bring their policy attitudes in line with the specter of legislative change.

⁸ We follow Krosnick and Berent (1993) by using a two-part format, where respondents first answer an initial agree/disagree question, with a second question probing opinion strength. This branching design yields Likert-type response categories ranging from strongly agree to strongly disagree, and as established by Krosnick and Berent's experiments, the branching format tends to improve survey measurement.

[TABLE 1 ABOUT HERE]

The second counter-terrorism policy item probes attitudes toward electronic surveillance by the National Security Agency. Now, the treatment effect refers to the 2008 Foreign Intelligence Surveillance Amendment Act. This piece of legislation provided (retroactive) authorization to earlier, “warrantless” surveillance involving domestic communications (Lichtblau 2008).

Independent Variables

In addition to the feedback experiments, we use regression to analyze mechanisms behind policy feedback. The pay-off here is to test for interactions between the policy feedback conditions and independent variables measuring candidate mechanisms. The two main mechanisms we consider are *interests* relating to counter-terrorist policies, and also *ideology*.

Recall that established scholarship and state-centered theorizing assume policy feedback generally occurs because individuals see themselves as benefiting from, and coming to have a vested interest in, the policy at hand. In the case of counter-terrorism policies, we measure interests using an item probing respondents’ fears of a terrorist attack.⁹ If interests/incentives underlie the operation of policy feedback, we expect an interaction between the policy-feedback condition and the covariate for terrorism fears. That is because it is individuals with higher levels of fear who are expected to be more favorably disposed to support a given counter-terrorism policy *when exposed to information about policy change*. It is, in principle, possible that there are other interest-related processes at work, but we follow recent scholarship in focusing on fear/anxiety as a potent (and easily mobilized) factor behind counter-terrorism policy support (Merolla and Zechmeister 2009).

⁹ *All in all, how concerned are you that the United States might suffer another terrorist attack in the next 12 months?* <1> not concerned at all; <2> not very concerned; <3> somewhat concerned; <4> very concerned.

The second independent variable is for the key heuristic of ideology. We follow Sniderman et al. (1991; see also Jost et al. 2009), measuring ideology using a seven-point scale in which respondents' classify themselves using scores from 1 (“strong liberal”) through 7 (“strong conservative”). The relevant hypothesis is that experimental conditions interact with ideology. But in contrast to interests, the feedback-by-ideology interaction relates to the heuristics and biases approach. If mechanisms underlying feedback involve prior beliefs of this sort, liberals and conservatives should respond in different ways to information about policy change.

In addition to terrorism fears and ideology, we consider the following independent variables in the regression analysis. Nationalism is a scale constructed from responses to three items.¹⁰ Education and age are continuous covariates (measured in years), indexing values or cognitive styles associated with schooling and life course/generational processes. The remaining independent variables are for gender, race, class, and church attendance. Gender (female = 1) and race (black = 1) are dichotomies. A seven-category measure of class follows the EGP scheme (Erikson and Goldthorpe 1992), and we treat this as a series of indicator variables. The church attendance covariate is continuous, with seven categories ranging from daily attendance to never.

RESULTS

Does Policy Change Matter?

Using the 2009 *SAPA* data, we consider the evidence that policy change may influence the process of opinion-formation on issues of counter-terrorism. In Figure 1 below, there are two

¹⁰ These items ask respondents their level of agreement/disagreement with the following propositions: *America should follow its own interests, even if this leads to conflicts with other nations? Generally speaking, America is a better country than most other countries? People should support their country even if their country is in the wrong?* Following the lead of scholarship using these items in the *General Social Surveys* (Smith and Jarkko 1998), we transform the items into z-scores, summing them into a scale.

treatment-effect conditions. In the first, priming individuals with information about a presidential executive order leads to *higher* support for the use of torture. In this case, there is evidence for a significant policy feedback effect, albeit in an unexpected direction. Looking at the second torture experiment, priming individuals with information about past uses of torture by the government has essentially the same effect, raising by .09 support for this contentious activity.

[FIGURE 1 ABOUT HERE]

Moving to Figure 2's results, the next experiment is for attitudes toward domestic surveillance practices employed by the National Security Agency. The baseline item elicits a low level of initial support (.19). But when reference is made to the earlier passage of the 2008 Foreign Intelligence Security Amendments Act, measured support is notably higher (.31).

[FIGURE 2 ABOUT HERE]

What Mechanisms Underlie Feedback?

So far, *SAPA's* experiments suggest intriguing evidence for the impact of policy change on counter-terrorism policy opinions. This evidence now calls out for clarification and additional consideration of mechanisms. To put the matter as a question, why does providing information about policy change often reshape policy attitudes?

We address this question using results presented in Table 2 below. These are tests for interactions between experimental exposure to policy feedback and two key covariates measuring mechanisms underlying the global versus contingency perspectives on policy feedback: interests and ideology. The interaction effects are from regression models that now treat the policy-attitude items

as dependent variables. There are also main effects for respondents' assignment to control and treatment-effect conditions (alongside the remaining independent variables). These are not presented in Table 2 to avoid clutter.

[TABLE 2 ABOUT HERE]

Looking at the torture results, there is no evidence of interactions involving policy-feedback conditions and the key measures of interests or ideology.¹¹ In other words, whether or not respondents are given information about policy change, they modify their opinions without first consulting their initial ideological inclinations. The same is true with respect to their underlying stakes in reducing anxieties concerning the prospect of terrorism.

For NSA surveillance, there is evidence for interactions involving both interests and ideology, at least when the latter are estimated in separate statistical models. But as displayed in the last two rows of Table 2, when both interactions are estimated in the same model, but there are little grounds for retaining the ideology \times policy-feedback condition. The results thus point to the single, interest \times policy-feedback interaction, and with a coefficient of $-.10$ (s.e. = $.04$), the policy feedback effects for NSA surveillance are stronger among fearful respondents. We discuss the magnitude of this interaction in the paper's next section.

So what should we make of the case of torture in which policy feedback effects are statistically independent of ideology and interest-related factors? This tells us that interest-related factors, as measured by terrorism fears, do not explain the feedback process in the torture

¹¹ Disaggregating tests for interactions in the torture experiment's multiple policy feedback conditions yields identical results. For the record, we also find no evidence of interactions involving the nationalism covariate.

experiments. The same is true of ideological orientations. This is not to say that terrorism fears and ideology have no significant main effects, but that these effects do little to account for the key feedback process at hand.

This leaves us with cognitive dissonance as a candidate. For attitudes towards toward torture, cognitive dissonance may underlie the policy feedback effect. If so, when information about relevant policy activities is made available, individuals respond by adjusting their opinions, avoiding potential discord caused by the new environment at hand.

The Magnitude of Feedback Effects

Just how large are the feedback effects on opinion-formation that we have unearthed? Coefficients from our regression models give perspective. Because the independent variables of interest are a mix of categorical (feedback condition assignment) and continuous covariates (terrorism fears and ideology), we employ a range-standardization to compare coefficients.¹²

We start with Table 3's results for torture. By way of example, the .18 estimate indicates that moving from lowest to highest response options on the terrorism fears item increases support for the use of torture. These standardizations suggest that at .09, the magnitude of the feedback effect is half that of terrorism fears (.18), and overshadowed by ideological predispositions (.48).¹³

[TABLE 3 ABOUT HERE]

¹² We present in Appendix II unstandardized coefficients and standard errors for models.

¹³ Complementing the feedback effect estimate presented in Table 3, the parallel estimate for the second feedback condition ("President Obama issued an order") is also .09.

In Table 4 we consider regression results for NSA surveillance. The interaction means that the feedback effect is much smaller at lower levels of terrorism fear (-.04) than at higher levels of fear (.26). Similarly, the impact of terrorism fears is smaller in the baseline condition (-.03) than in the feedback condition (.27). For its part, ideology has substantial direct effects (.24) on policy attitudes. But as discussed further below, the absence of any interaction between ideology and feedback condition is telling when it comes to the clash of global versus contingency perspectives.

[TABLE 4 ABOUT HERE]

DISCUSSION

When and why does policy feedback occur? Since the pioneering work of North (1990) was vigorously applied to comparative welfare state development (Pierson 1993; see also Skocpol 1992), a consensus has been that policy change routinely re-makes mass opinion by shaping citizens' interests and their incentive to comply with new institutional arrangements. But looking at the 1996 reform of the U.S. AFDC program, recent work has constructively challenged this consensus.

This study offers a novel application of survey experiments and a fresh focus on counter-terrorism policy. Together, our experiments suggest evidence that policy change in this domain can shape the process of opinion-formation. Counter-terrorism policies are an instructive context in which to extend policy feedback scholarship, for they represent issues that can be viewed as “distant” from most citizens' immediate experience, and thus potentially poor candidates for feedback effect (Soss and Schram 2007, p. 121).

The evidence for policy feedback effects on opinion may thus be surprising to some, and indeed our survey experiments present a deliberately demanding test. In particular, because the experimental treatments make available information respondents may already possess, this design, if

anything, may understate the impact of policy change on mass attitudes. As a result, the experiments provide some degree of confidence, though their initial character should again be emphasized.

What of the clash between universal and contingency perspectives underlying established versus more recent scholarship on policy feedback? Initially, we might expect counter-terrorism policy feedbacks to be best explicated by the literature on heuristics and biases. But recall our thematic finding that the mere mention of an instance of policy change nudges mass attitudes in new directions. This diverges from the heuristics and biases expectation that the filtering of information will make policy feedback a more contingent phenomenon. Prior beliefs such as ideological identification do appear to shape counter-terrorism attitudes (that is consistent with estimates in our regression models). But they appear to have limited explanatory bearing on the feedback processes at hand, for there is no evidence for any *selective* mobilization of pre-existing biases.

This brings us to the global perspective, where our evidence for policy feedbacks in counter-terrorism opinions should be comforting to many comparative social scientists. Yet the detailed results may be surprising to analysts who assume policy feedback operates primarily through incentives and interests. The expected utility of counter-terrorism policies involves calculations about the risk of future attacks, where individuals experiencing greater fear should be *more* receptive to information about policy change. But we find evidence of this sort in only one of our two cases.

For the NSA surveillance issue, results are consistent with the operation of cognitive dissonance, where individuals seek to avoid anxieties caused by compliance with environmental demands, bringing attitudes in line with new realities. Our evidence for cognitive dissonance is of course indirect and in need of further study. In general, we would anticipate that cognitive dissonance has greatest merit as a mechanism behind policy feedback when opinion change unfolds rapidly, and when there is also an absence (or irrelevance) of incentives. If so, our results may caution against assuming interests alone to be the sole micro-foundations of policy feedback effects.

Together, then, our results signal the fruitfulness of further work. Experiments have promise in providing some new foundations for linking macro-conditions to individual-level processes of opinion-formation. We would emphasize the positive-sum character of these tools with respect to comparative/historical and over-time survey analyses (e.g., Soss and Schram 2007; Bruch et al. 2010; Svallfors 2010). As the development of U.S. counter-terrorism policies continues to unfold, there is merit in applying multiple methods of inquiry, here and with respect to other policy domains as well.

Conclusion

What about counter-terrorism as a focus in its own right? There is much at stake, as changes in this policy domain since September 11, 2001 have been extensive. What are mechanisms behind opinion-formation on counter-terrorism policies and practices? We know from past research that Americans continue to offer general endorsements of rights and liberties (Best et al. 2006; Davis 2007). Simultaneously, however, the capacities of political elites to mobilize fears of terrorism (Mueller 2006; Merolla and Zechmeister 2009; see also Willer and Adams 2008) buttress support for many counter-terrorism policies.

Our addition of policy feedback experiments adds a new consideration to the mix. It is again of note that in both the experiments we conducted, the mere mention of a change in law or government action is enough to nudge attitudes in new directions. Our experiments appear to be picking up an important dynamic in how Americans respond readily (and rapidly) to the pervasive influence of counter-terrorism policies and practices themselves.

In this context, it is notable that the torture experiment into effects of an executive order banning “coercive interrogation techniques” raised support for this contentious activity. Does this mean that policy and legal changes reining-in counter-terrorism measures will tend to follow a similar pattern? That seems premature, and the current results lack the reach to engage this broad

question. New applications of survey experiments may provide one line of fruitful research, and we believe the case of counter-terrorism attitudes is an important one in its own right, and also for policy feedback scholarship.

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Appendix I: Survey of American Policy Attitudes, 2009 (SAPA)

The Survey of American Policy Attitudes was developed in cooperation with the Center for Survey Research (CSR) at Indiana University. Data were collected using computer assisted telephone interviewing methods (CATI), and numbers were randomly generated using the Genesys list-assisted method. This method allows for unpublished numbers and new listings to be sampled. After selecting a random sample of telephone numbers, numbers were matched to a database of business and non-working numbers, and all matches were subsequently purged from the sample. The sample was nationwide, and at each residential number a respondent from all household members age 18 or older was selected.

The data collection staff included 11 supervisors and 54 interviewers. All interviewers received at least 20 hours of training in interviewing techniques before production interviewing. Interviewers were instructed to read questions and response categories at a pace slower than conversation, and to use neutral probes and feedback phrases. Audio and visual monitoring was regularly conducted by the telephone survey supervisors using the CSR facilities. Monitoring was conducted randomly, with each interviewer being monitored at least once during each 3-hour shift.

All cases with confirmed valid telephone numbers were called up to 15 times, unless the respondent refused or there was insufficient time before the end of the study. Cases with unknown validity (persistent no answers or answering devices) were called a minimum of 8 times, with calls made during the morning, afternoon, evening, and weekend. Interviewers attempted to convert each "refusal" at least twice. When possible, a conversion attempt was made at the first instance of refusal and a second attempt is usually made after a few days.

Final Disposition Summary

The following tables classify every case according to its final disposition. These dispositions are based on the guidelines for Final Disposition Codes for RDD Surveys established by the American Association for Public Opinion Research (AAPOR) Standard Definitions for Final Dispositions of Case Codes, 2004. Using AAPOR's RR3 (Response Rate 3 formula), we calculated the response rate as 0.1527 (or 15.3%).

This response rate is in line with ongoing declines in participation found in U.S. telephone surveys. Research on the impact of these trends on data quality suggests that declining response rates are not by themselves tantamount to response bias, or that probability sampling should necessarily be abandoned in favor of such alternatives as quota sampling (Groves 2006). A relevant result of experiments into effects on data quality (Keeter et al. 2006) is that low rates are likely a source of bias only insofar as the probability of survey participation is correlated with one or more measured variables of interest. In SAPA, the majority of non-responses (unknown eligibility or not-interviewed) was a product of conditions such as no answers on the line and an initial refusal to participate *before* the start of the survey itself (or any item) were described.

Interview	RDD	Panel	Total
Completed interviews	1325	217	1542
Partial completions	7	10	17
Total Interviews	1332	227	1559
Eligible, Non-Interview			
Refusal	4324	112	4436
Break-off (Refused after starting interview)	143	3	146
Respondent never available	896	73	969
Telephone answering device (message confirms housing unit)	124	17	141
Respondent away duration of the survey	18	2	20
Deceased	1	5	6
Physically or mentally unable/incompetent	38	0	38
Language	169	0	169
Total Eligible, Non-Interviews:	5713	212	5925
Unknown Eligibility, Non-Interview			
Always busy	112	9	121
No answer	1786	34	1820
Telephone answering device (unknown if housing unit)	1652	136	1788
Call Barrier	2	0	2
Technical phone problems (line/circuit problems)	837	18	855
Respondent not found	0	111	111
Total Unknown Eligibility, Non-Interviews:	4389	308	4697
Not Eligible			
Fax/data line	1014	0	1014
Non-working/disconnected number	3495	0	3495
Temporary non-working/disconnected number	54	0	54
Number change	70	0	70
Cell phone	13	0	13
Call forwarded	22	0	22
Business, government office, other organization	615	0	615
Institution	27	0	27
Group quarters	7	0	7
Seasonal home	31	0	31
Not eligible – no adult household members	11	0	11
Quota met	0	62	62
Total Not Eligible:	5359	62	5421
Total Sample:	16793	809	17602

Appendix II: Regression estimates – torture

	coefficient (s.e.)
“Obama stopped use” (0 = baseline)	.08* (.03)
“Govt. used methods” (0 = baseline)	.09* (.03)
fear of terrorism	.06* (.02)
education	< -.01 (<.01)
ideology	.08* (.01)
nationalism	.11* (.02)
age	< -.01 (<.01)
female (0 = male)	-.03 (.03)
black (0 = non-black)	.02 (.04)
manager (0 = non-working)	-.03 (.07)
professional (0 = non-working)	.02 (.04)
routine white collar (0 = non-working)	.08* (.04)
self-employed (0 = non-working)	.11 (.06)
skilled worker (0 = non-working)	.06 (.05)
unskilled worker (0 = non-working)	.09 (.05)
church attendance	-.02* (.01)
constant	.03 (.11)
R^2 / N	.26 / 947

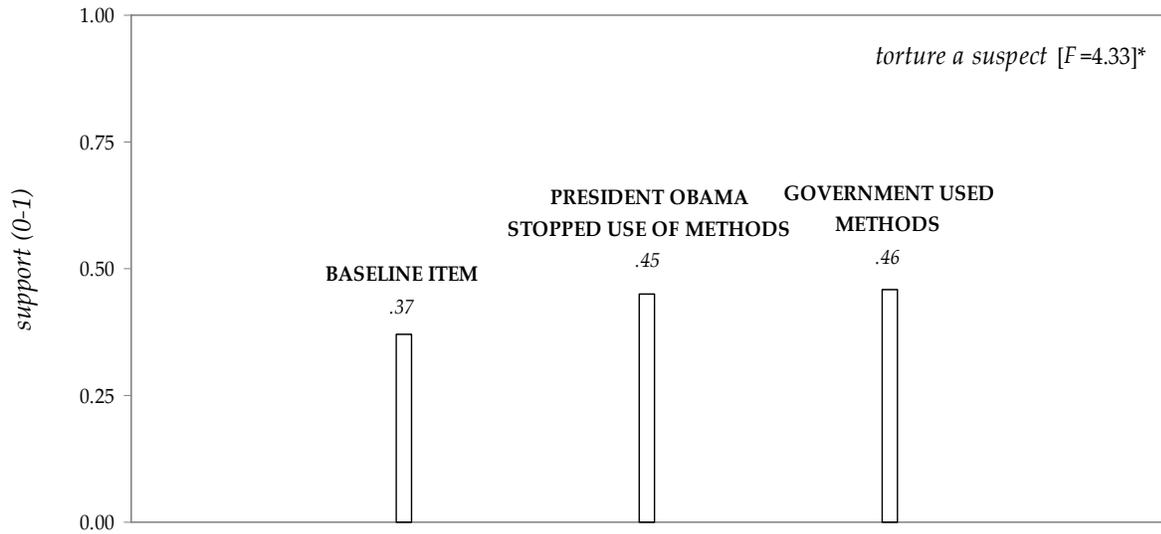
* indicates $p < .05$ (t -test).

Appendix II (continued): Regression estimates – NSA surveillance

	coefficient (s.e.)
“2008 FISA Act” (0 = baseline)	-.14 (.11)
fear of terrorism	-.01 (.03)
fear of terrorism × feedback condition	-.10* (.04)
education	-.01 (.01)
ideology	.04* (.01)
nationalism	.03 (.03)
age	< -.01 (<.01)
female (0 = male)	-.02 (.03)
black (0 = non-black)	-.03 (.05)
manager (0 = non-working)	-.03 (.09)
professional (0 = non-working)	-.01 (.06)
routine white collar (0 = non-working)	.06 (.05)
self-employed (0 = non-working)	.06 (.07)
skilled worker (0 = non-working)	-.06 (.05)
unskilled worker (0 = non-working)	-.04 (.06)
church attendance	.01 (.01)
constant	.22 (.14)
<i>R</i> ² / <i>N</i>	.12 / 653

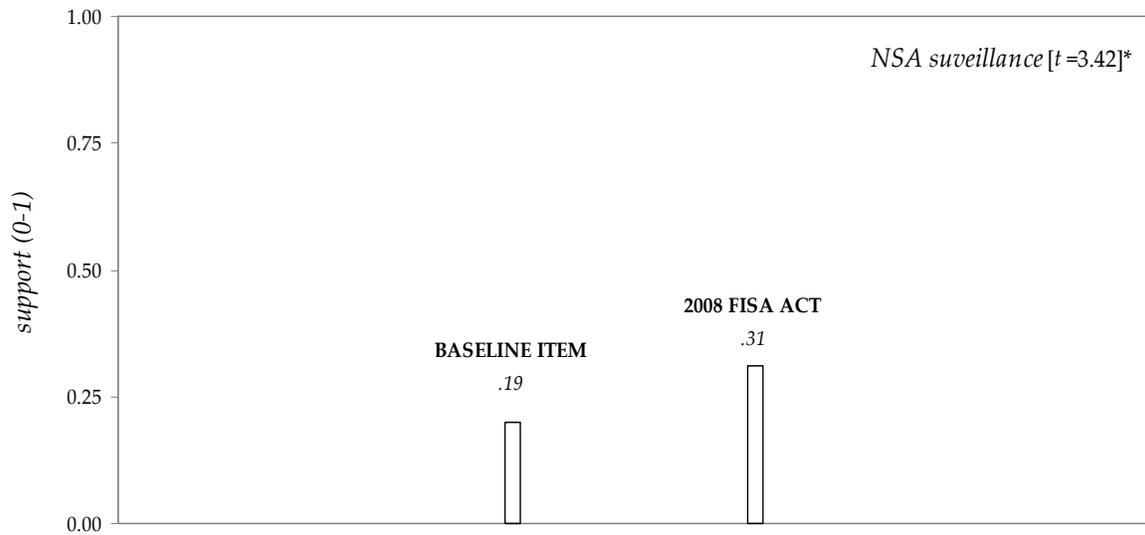
* indicates $p < .05$ (*t*-test).

Figure 1: Feedback experiments – torture



* indicates $p < .05$.

Figure 2: Feedback experiments – NSA surveillance



* indicates $p < .05$.

Table 1: Policy feedback experiments and dependent variables

Torture

baseline (control group) item:

Do you agree or disagree that government authorities should have the right to torture a suspect if they think it will help prevent a terrorist attack from taking place in the United States?

experimental (policy feedback) item:

As you may know, in 2009, President Obama issued an order stopping the use of coercive interrogation methods. Do you agree or disagree that government authorities should have the right to torture a suspect if they think it will help prevent a terrorist attack from taking place in the United States?

experimental (policy feedback) item:

As you may know, after the terrorist attacks of September 11, 2001, the U.S. government sometimes used coercive interrogation methods when interrogating suspects in places such as Abu Ghraib and Guantanamo Bay. Do you agree or disagree that government authorities should have the right to torture a suspect if they think it will help prevent a terrorist attack from taking place in the United States?

NSA surveillance

baseline (control group) item:

Do you think that the federal government should monitor telephone conversations, banking transactions, and email between American citizens in the United States?

experimental (policy feedback) item:

As you may know, the 2008 Foreign Intelligence Surveillance Amendments Act gives government agencies new powers to engage in domestic surveillance of both citizens and foreign nationals without getting a court order allowing them to do so.] Do you think that the federal government should monitor telephone conversations, banking transactions, and email between American citizens in the United States?

Table 2: Tests for interactions

	<i>test results^a</i>
<i>torture:</i>	
× interests [terrorism fears]	F(2, 928) = .10; $p > F = .91$
× ideology [liberal/conservative identification]	F(2, 928) = .95; $p > F = .39$

<i>NSA surveillance:</i>	
× interests [terrorism fears]	F(1, 636) = 7.34; $p > F < .01$
× ideology [liberal/conservative identification]	F(1, 636) = 4.45; $p > F < .04$
× interests [terrorism fears] ^b	F(1, 635) = 4.93; $p > F < .03$
× ideology [liberal/conservative identification] ^b	F(1, 635) = 2.06; $p > F = .15$

a. Shaded entry indicates significance at the .05 level.

b. Interactions between feedback condition and both interests and ideology estimated in the same model.

Table 3: Magnitude of policy feedback effects – torture

<i>torture</i>	
<hr/>	
feedback (<i>government used methods</i>)	.09
terrorism fears (Δ <i>not at all</i> \rightarrow <i>very</i>)	.18
ideology (Δ <i>strong lib.</i> \rightarrow <i>strong con.</i>)	.48

Table 4: Magnitude of policy feedback effects – NSA surveillance

<i>NSA surveillance</i>	
feedback (2008 FISA Act) @ terrorism fears = not at all	-.04
feedback (2008 FISA Act) @ terrorism fears = very	.26
terrorism fears (Δ very \rightarrow not at all) @ baseline condition	-.03
terrorism fears (Δ very \rightarrow not at all) @ feedback condition	-.27
ideology (Δ strong lib. \rightarrow strong con.)	.24

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2. **BROOKS, C. & CALZADA, I.** On policy feedback: insights from survey experiments.