

Party Brands and Partisanship: Theory with Evidence from a Survey Experiment in Argentina

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Scholars disagree about the nature of party attachments, viewing partisanship as either a social identity or a rational maximization of expected utility. Empirically, much of this debate centers on the degree of partisan stability: findings of partisan fluctuations are taken as evidence against the social-identity perspective. But drawing such conclusions assumes that the objects of identity—parties—are fixed. If we instead allow party brands to change over time, then partisan instability is consistent with a social-identity conception of partisanship. To demonstrate this, I develop a branding model of partisanship in which voters learn about party brands by observing party behavior over time and base their psychological attachment to a party on these brands. The model suggests that convergence by rival parties, making their brands less distinguishable, should weaken party attachments. I test this implication using a survey experiment in Argentina and find evidence consistent with the model.

The concept of partisanship has occupied scholars of political behavior for several decades.¹ Its early proponents conceived it as a voter's enduring psychological attachment with a party, inherited like a religious affiliation and tending to persist over the life of an individual (Campbell et al. 1960; Miller 1976, 1991; Miller and Shanks 1996). A key insight of this conceptualization was the notion that partisanship is a type of social identity (Green, Palmquist, and Schickler 2005). Later authors offered a more rationalistic conceptualization in which voters evaluate parties over time to form a "running tally" and choose the party most likely to benefit them (Achen 1992; Fiorina 1981; Franklin and Jackson 1983; Jackson 1975; Jennings and Markus 1984; Page and Jones 1979). From this perspective, partisanship is not an identity but rather a product of voters maximizing their expected utilities.

Empirically, much of the debate between these contrasting perspectives—especially among comparativists—has focused largely on the question of partisan stability over time (Bartle and Bellucci 2009; Budge, Crewe, and Farlie 1976). Evidence that partisanship is stable over time is taken to support the social-identity perspective (e.g., Green, Palmquist, and Schickler 2005) that partisanship is an "unmoved mover." Conversely, evidence of partisan volatility is considered inconsistent with such theories (e.g., Thomassen 1976). The underlying logic is that while voters' evaluations of party performance fluctuate from year to year, social identities form in childhood or adolescence—whether by socialization or learning—and stabilize thereafter.

Yet, the implication that partisanship must be stable if it is a social identity assumes that the objects of identity (i.e., parties) are themselves stable. The possibility that

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¹I use the terms *partisanship*, *party attachments*, and *party identification* interchangeably to refer to an individual's self-identification with a political party.

parties are themselves moving parts is rarely even noted, much less theorized or empirically tested.² This gap is no doubt partly the result of the overwhelming empirical focus of partisanship research on advanced democracies, the United States in particular. In these contexts, the same parties tend to persist, and their reputations are slow to change (e.g., Baumer and Gold 1995).³ But in developing democracies, political parties are often new and may undergo dramatic transformations. In these contexts, the implications of existing theories of partisanship are not immediately apparent. Should we take partisan instability in these contexts as evidence that party attachments there are not social identities?

This article argues that a social-identity conception of partisanship need not imply partisan stability. Indeed, elaborating such theories further demonstrates that there are actually particular conditions under which we would expect partisanship to change.⁴ I develop a branding model of partisanship that builds on recent theories of partisanship but incorporates an important element of social-identity theory, the notion of *comparative fit*. That concept suggests that individuals self-categorize into identity groups not only when they think they resemble, or fit into, that group, but also when they think their group differs from other groups. My model suggests that certain party behaviors should strengthen party attachments while others erode them. In particular, when parties converge, diluting their brands by becoming

²Green, Palmquist, and Schickler recognize that “voters’ perceptions of which social groups affiliate with each party could change, as could the esteem in which they hold those groups” (2005, 138). Yet these authors do not derive the circumstances under which such perceptions might change. To my knowledge, only two studies of U.S. partisanship explicitly study the interaction between partisanship and party change (Green, Palmquist, and Schickler 2005; Grynaviski 2010). Jackson and Kollman (2011) include in their model a “state of the world” term that can encompass a party shift, but this black box does not allow us to derive what kinds of party shifts condition partisanship and how.

³Even within research on partisanship in advanced democracies, scholars debate whether the concept of partisanship applies at all beyond the borders of the United States. Some scholars of Western Europe are particularly skeptical that partisanship can be meaningfully distinguished from vote choice in parliamentary systems (e.g., Bartle and Bellucci 2009; Budge, Crewe, and Farlie 1976; Johnston 2006; Thomassen 1976). This is of little concern here because I focus on Argentina, whose electoral system resembles that of the United States in many ways. Moreover, I show elsewhere that patterns of partisanship across Latin America generally conform to the expectations of theories developed for the United States (Lupu 2011).

⁴A handful of scholars have examined how institutions or party characteristics condition partisanship cross-nationally (Huber, Kernell, and Leoni 2005; Richardson 1991), but here I am interested in the dynamic question of how partisanship responds to shifting party brands.

difficult for voters to distinguish, partisan attachments should weaken.

This article thus enriches social-identity theories of partisanship by both incorporating the notion of comparative fit and explicitly modeling the identification process. More generally, my branding model highlights the overlooked role of parties themselves as variables in theories of partisanship. Such theories should tell us not only about the origins and development of partisanship, but also how these are conditioned by party change.

Doing so also allows us to move beyond the limiting sample of advanced democracies in empirical studies of partisanship. Unless partisan attachments are a feature of only the most developed democracies—and there seems to be little reason to think they are (Dalton and Weldon 2007; Lupu and Stokes 2010)—our theories should travel to developing contexts.⁵ Despite some recent studies of partisanship in developing countries (e.g., Barnes, McDonough, and López Pina 1985; Brader and Tucker 2001, 2008a, 2008b; Medina Vidal et al. 2010; Miller and Klobucar 2000; Pérez-Liñán 2002; Samuels 2006), we still know very little about party attachments in these contexts. This article presents an empirical study of partisanship in the context of one developing democracy, Argentina. Given the paucity of studies of partisanship in such contexts, and particularly in Latin America, this study thus makes an important empirical contribution.

A final contribution of this article is methodological. Previous scholars of partisanship have arrived at diverging conclusions from the same observational data often by changing the assumptions underlying their empirical models. Instead, I employ an experimental test that improves our ability to identify causal effects.⁶ In order to test the effect of party convergence on partisanship, I embedded an experiment in a 2009 survey of voters in Argentina. The experiment manipulated voters’ information about the political parties in Argentina in ways that suggested either divergence or convergence among the parties.

Argentina in 2009 is a useful case in which to investigate the dynamics of partisanship. On the one hand, some Argentine parties are fairly old, predating the beginning of the current period of democracy in 1983. On the other hand, a series of alliances and policy switches severely diluted the brands of these parties over the course

⁵Or else we ought to delineate how differences between developing and advanced democracies affect the implications of our theories (see Brader, Tucker, and Duell forthcoming).

⁶To my knowledge, the only previous experimental tests of partisan change are Brader and Tucker (2008b) and Cowden and McDermott (2000).

of the 1990s and early 2000s (Lupu 2011). And new parties emerged with fairly ambiguous positions (Calvo and Escobar 2005). As a result, Argentine voters have some prior beliefs about some party brands, but these priors are fairly diffuse and, therefore, potentially pliable.⁷

Using this survey experiment, I find that convergence among parties indeed weakens partisanship in the ways my model predicts. As expected by the Bayesian framework, I find some evidence that the treatment effects are more pronounced among respondents likely to have weaker priors—less-informed and younger individuals. I also present some suggestive evidence regarding the mechanisms underlying the observed treatment effects. These findings highlight the benefits of considering the circumstances under which party brands affect individual partisanship. Thinking of party brands as variable in this way also suggests avenues for further research on the effects of party polarization or coalitions on partisanship, as well as implications for understanding party strategy.

Branding Model of Partisanship

Social psychologists have long noted that group identities are based on individuals' stereotypes about a social category—that is, a category prototype—and how well they perceive themselves to resemble (or *fit*) that prototype (Turner et al. 1987). As Huddy explains, “A prototype can either be the most typical group member—an actual person—or a fictional member who embodies the most common or most frequent attributes shared among group members” (2001, 133–34). Individuals thus self-categorize into social identities by constantly shifting back and forth between their individuality and that identity. The more that group membership maximizes similarities between an individual and other group members as well as differences with outsiders (a concept known as *comparative fit*), the more likely an individual is to identify with the group (Hogg et al. 2004; Turner 1999).

Like the theory proposed by Green, Palmquist, and Schickler (2005), the branding model of partisanship proceeds from this self-categorization notion of political-identity formation. Over the course of their lives, voters develop perceptions of partisan identities through their own observations of the party and its behavior (see also Baumer and Gold 1995; Rahn 1993; Sanders 1988). They learn what to associate with the prototypical Democrat

or the prototypical Republican, and they use these prototypes to inform their identity. These prototypes compose what I refer to as a *party brand*. This brand plays a crucial role in the development of individuals' political identities. In particular, individuals identify with a party to the extent that they consider themselves similar to the party brand, considering themselves to be a part of what consumer researchers call a *brand community* (Muniz and O'Guinn 2001).

The branding model of partisanship is thus based in a learning process that can be approximated as Bayesian updating,⁸ as in some previous models of partisanship (Achen 1992, 2002; Gerber and Green 1998; Grynaviski 2010). A formal statement of this updating process is provided in the online supporting information, but the intuition can be stated informally. Individuals place each party on some salient continuum, say an ideological left-right or a class dimension. Where individuals perceive each party to lie on the continuum constitutes the party brand. An individual who views the prototypical Democrat as poor or working class would perceive the Democratic party to be on the pro-poor side of the continuum.

Voters' perceptions of party brands are determined by observations of the party's behavior over time. This means that the updated party brand (the posterior) combines prior and new information. The new perceived party position is a weighted average of the prior position and the average party position signaled by the new observations of the party's behavior. In other words, voters have some conception of a party's brand based on past experience, but they continually average in new observations of the party.⁹

⁸Bayes' Rule need not accurately model psychological processes; indeed, psychologists have documented some violations of Bayesian updating, such as “cognitive conservatism,” the overweighting of prior beliefs (Tetlock 2005). Still, as Bullock (2009) notes, the Bayesian ideal need only serve as a benchmark against which to judge empirical processes. And even if individuals do not actually use Bayes' Rule in their everyday lives, it may be a useful and systematic way in which to model individual behavior and capture an interesting intuition (see Gerber and Green 1999).

⁹There is an implicit assumption here that all individuals interpret a given observation in more or less the same way. There is much debate among scholars about the existence of partisan bias, the effect of partisanship on individuals' interpretations of political events (e.g., Bartels 2002; Carsey and Layman 2006; Gerber and Green 1999). Indeed, the classic conception of partisanship suggested that partisan identities create a “perceptual screen” through which information is interpreted (Campbell et al. 1960). Still, the assumption of unbiased learning here seems reasonable. Modeling bias as the discounting of discordant observations would simply slow the updating process, not change the comparative statics. As Franklin and Jackson (1983) note, “Although previous partisan attachment acts to restrain change, it is like a sea anchor, which retards drift rather than arrests it entirely. If the tides of policy

⁷In my survey, while 34% of respondents (across treatment conditions) said they identified with a party, 68% and 59% said that their father and mother, respectively, identified with a political party when the respondent was growing up.

Party brands also have some variance around them, which is also updated as individuals observe party behavior. We can think of this variance as the strength of the party brand, the precision with which a party signals its position. As the variance decreases, voters become more certain about the party's position, develop a clearer image of its prototypical partisan, and the brand becomes stronger. As voter uncertainty about the party's position increases, the party appears to be more heterogeneous, perhaps containing multiple prototypes, and the brand becomes weaker or diluted.

These learned party brands form the basis of voters' attachments. A voter will feel the greatest affinity with the party whose prototypical partisan she thinks she most resembles, relative to all other parties. As with other social identities, partisan identity is determined on the one hand by the resemblance, or fit, between the voter's self-image and her image of the party prototype (Turner et al. 1987). Party attachments are therefore increasing in the voter's perceived fit with the party. Moreover, the more ambiguous the party brand, the weaker that attachment since a voter will perceive less fit with the party prototype (Hogg et al. 2004).

Finally, the degree of identification also depends crucially on comparative fit, the degree to which a voter thinks she resembles the prototype of one group and differs from that of another group (Turner 1999). Thus, a voter will feel most attached to a party when its prototype most resembles her and the prototypes of other parties seem very different.

The branding model suggests, then, that partisanship responds to the actions of party elites observed by voters. Parties whose brands become more ambiguous are likely to see their partisan ranks erode.¹⁰ Similarly, as major parties converge, voters become unable to distinguish one party brand from another, weakening partisan identities. In other words, when rival (that is, previously divergent) parties signal convergence in salient ways—say, forming an alliance—the differences between their brands shrink, making it less likely that voters will see a comparative fit with one over the other. Party conver-

evaluation are strong enough, conversions can and will take place" (1983, 969).

¹⁰Such ambiguity would result if a voter's new observations are inconsistent within the same period, as in times of intraparty conflict, or are inconsistent with prior beliefs about the party. This comparative static is explored in part by Grynaviski (2010), who studies periods of intraparty conflict in the United States, but I leave a fuller empirical test of this implication for future work.

gence is thus expected to weaken partisanship, whereas divergence strengthens it.¹¹

As with other models of Bayesian processes, the branding model offers some comparative statics with regard to the interaction of individual characteristics and partisanship. By the simple logic of the weighted-average calculation, individuals with weaker prior beliefs should update their beliefs more quickly than individuals with stronger prior beliefs. Individuals with stronger prior beliefs weigh new information less heavily than prior information and will therefore need a larger number of new observations to change their perception of the party brand. In other words, individuals with greater political experience will update more slowly than those with little or no political experience. At least two kinds of individuals typically have less political experience—the young and the less politically informed.¹² As an empirical matter, then, we would expect these groups to update their beliefs about parties more quickly when presented with new information (see, e.g., Achen 1992).

Evidence from a Survey Experiment

In order to test the individual-level implications of the branding model of partisanship, I turn to a survey experiment. By randomly assigning respondents to treatment and control, experiments make treatments exogenous to observed outcomes. They thus allow us to identify causal relationships in situations fraught with endogeneity and to do so without making untestable modeling assumptions (Gerber and Green 2012; Morton and Williams 2010; Mutz 2011).

I fielded a face-to-face survey of voters in 2009 in the provinces of Córdoba and Santa Fe in Argentina (see the supporting information for details of the survey

¹¹This hypothesis may seem to fly in the face of assumptions in U.S. politics about voter preferences for bipartisanship (Ramirez 2009; but see Harbridge and Malhotra 2011). However, it would not be inconsistent for voters to both have a conscious preference for bipartisanship or consensual politics and for such behavior to dilute party brands and weaken attachments through the more subconscious process described here.

¹²The comparative static that less politically informed individuals will update more quickly assumes that all individuals receive the same set of new observations. In the natural world, of course, less-informed individuals may also be paying less attention to politics and therefore receive fewer new observations (see, e.g., Bartels 1993). Since my empirical test will control the information provided to all respondents, however, we can expect less-informed individuals to update more quickly within the experimental context.

methodology),¹³ in which I embedded an experiment. The experiment uses a three-treatment, between-subjects design,¹⁴ meaning that four versions of the questionnaire were used. Respondents were randomly assigned to questionnaires, and standard randomization checks find no evidence that random assignment was abrogated.¹⁵

The survey experiment tests an important microlevel implication of the branding model: convergence among parties weakens partisan attachments. I test this implication by providing respondents with different types of information about the Argentine political parties—information that distinguishes parties from one another and information that blurs the lines between them—and examining its effect on their party attachments. The information was gleaned from party manifestos, official statements made during the 2009 by-election campaign that preceded the survey (Lupu 2010), and secondary case studies, and subsequently verified by country experts.

Respondents in all four conditions were provided with a page with references to the eight competitive parties in Argentina at the time of the survey. They were then told the following:

This page contains some information about some political parties in today's Argentina. Please

look over the information and I will then ask you some questions about these political parties. Please take your time and let me know when you have finished reading the information.

Those in the control condition were provided with a mostly blank page that listed only the party symbols along with the names of the party leaders, but no additional information (no information).¹⁶

Like the experiment conducted by Brader and Tucker (2008b) in a number of postcommunist countries, a first treatment provided respondents with information about each party's platform on economic, security, and federalism issues (platform information). The expectation from my model is that the platform information will enhance a respondent's certainty about party brands and, in turn, her ability to identify with a party. This is because greater clarity about party platforms and a greater ability to distinguish between parties is expected to make voters more certain of party brands. The more certain they are of party brands, and the more readily they can distinguish the parties, the more likely they are to choose one party with which they identify.¹⁷

A second treatment sought to provide respondents with the opposite kind of information, information that would suggest that parties are actually less distinguishable. For the sake of symmetry, policy information might have again been used, with issues selected that did not distinguish parties. The problem with this is that the issues on which parties agree also tend not to be salient to most voters.

Instead, I provided these respondents with information about interparty alliances and party switching (alliance/switching information). Alliances and party switching indirectly suggest that parties agree on policies, even if those policies are not specified directly. Indeed, Fortunato and Stevenson (forthcoming) find that when parties in Europe form governing coalitions, voters subsequently perceive them as more similar ideologically. In the Argentine context, interparty alliances and party switching have recently been common. The fragmented party space means that presidents often form legislative coalitions on an ad hoc basis. The economic crises of the

¹³These particular provinces were selected for three reasons. First, both provinces maintain good networks of roads and public transportation for relatively easy access to both urban residents of the provincial capital cities of Córdoba and Rosario and rural voters in surrounding towns and villages. Second, unlike in the more populous province and city of Buenos Aires, there are no major threats to personal security in Córdoba and Santa Fe, providing a more hospitable environment for interviewers. Third, Córdoba and Rosario are the country's second and third largest cities, and the provinces' distributions on demographic characteristics like gender, age, and education are similar to the national distributions (see Table A1 in the supporting information).

¹⁴A within-subject design might better approximate the theory that respondents use information to update their partisanship. This would have required asking respondents about their partisan attachments both before and after the information treatment. Within the context of a single survey, though, it is likely that the desire for consistency would lead respondents to report no change in their partisanship.

¹⁵Table A2 in the supporting information reports the average age, gender, income level, and education of respondents in each condition. The similarity of these averages across conditions suggests that treatments were balanced in terms of observable characteristics and that assignment was random. Indeed, difference-of-means tests suggest no significant differences (at 95% confidence) between conditions on these characteristics. Further evidence is provided by using a multinomial logit to regress assignment into each condition on these same characteristics. A likelihood ratio test is statistically insignificant (Wald $\chi^2_{(12)} = 13.22, p < 0.35$), reaffirming the balance among treatment conditions. Finally, an omnibus F-test following Hansen and Bowers (2008) found no imbalance ($\chi^2_{(7)} = 5.66, p < 0.58$).

¹⁶By providing respondents in the no-information condition a page physically identical to the one received by respondents in the treatment conditions, I am able to rule out any potential effects of actually holding a page with party names and to isolate the information effects.

¹⁷Brader and Tucker (2008b) hypothesize a similar effect (see also Franklin and Jackson 1983), although for them the underlying mechanism is that voters need to reach a certain threshold of information to be confident in choosing a partisan allegiance.

1980s and 1990s also provoked prominent party switches and surprising party alliances (Lupu 2011). I relied on these actual instances of alliances and switching to signal convergence among the parties.¹⁸ My expectation is that this alliance/switching information will make parties seem less distinct, which should weaken partisanship.

A final treatment condition showed respondents a page containing both types of information (all information). Since the two types of information are relatively balanced and expected to have countervailing effects, I expect to see no overall effect on partisanship of receiving both types of information together.

Figure 1 shows a sample of the information page translated into English. After reviewing the page, respondents were asked a standard question about their partisan attachments: “Independent of which party you usually vote for, is there a political party with which you identify?”¹⁹ Respondents who answered positively were then asked with which party they identified and the strength of that identification on a 0–10 scale.

The expectation from my model is that respondents in the platform-information condition will be more likely than those in the control condition to identify with a party and report having stronger partisan attachments.²⁰ Conversely, respondents presented with the alliance/switching information should exhibit opposite tendencies, with fewer respondents than in the control condition identifying with a party and weaker attachments among those who do identify.

¹⁸Convergence by all the parties in a political system is of course uncommon, but my treatment seeks to provide information of potential relevance to all respondents, at some expense of mimicking the natural world. I expect that each respondent finds information about some party or subset of parties more relevant than the information about the others. But I provided distinguishing or convergence information about all of the parties so that the treatment is not administered only to those respondents who care about a particular subset of parties.

¹⁹A closely related question was asked roughly 15 items later in the survey: “I am going to name various groups within Argentine society. For each one, please tell me which of these parties most represents that group, or if you think no party represents that group.” Respondents were presented with several groups, the last of which was “people like you.” This question thus measures whether respondents feel represented by a particular political party, a concept very close to party identification. Mutz (2011, 100–101) advocates using multiple items to measure outcomes in survey experiments. In this case, the standard usage of the party-identification item probably mitigates such necessity. Still, my results are robust to using an index composed of both items.

²⁰As Brader and Tucker (2008b) note, it is possible that the platform information might make respondents perceive the parties as more similar than they previously thought. Such a situation is likely to be rare since distinguishing information was specifically selected, but at worst this biases the experiment against finding the hypothesized effect.

The mechanism underlying this causal effect is theorized to be the perceived indistinguishability of the parties. To check for this mechanism, I included post-treatment items that asked respondents to place all eight parties on a 0–10 ideological scale. Using these items, I construct a measure of the degree of polarization perceived by the respondent to exist in the party system. If the mechanism underlying the treatment effects is consistent with the theoretical model, then the treatment effects should also be apparent in posttreatment perceptions of polarization.²¹

The branding model also implies that individuals with weaker priors about party brands—such as younger individuals and those with less political information—should update party brands more quickly than those with stronger priors. Given their weaker priors, these individuals should give greater weight to the new information. This experiment provides an opportunity to test these implications as heterogeneous treatment effects, with age and political information conditioning the average effects of the two information treatments. Unfortunately, the available measure of political information is blunt, relying on only two survey items.²² Sample size and measurement error thus present challenges, but my results can at least be taken as suggestive on this score.

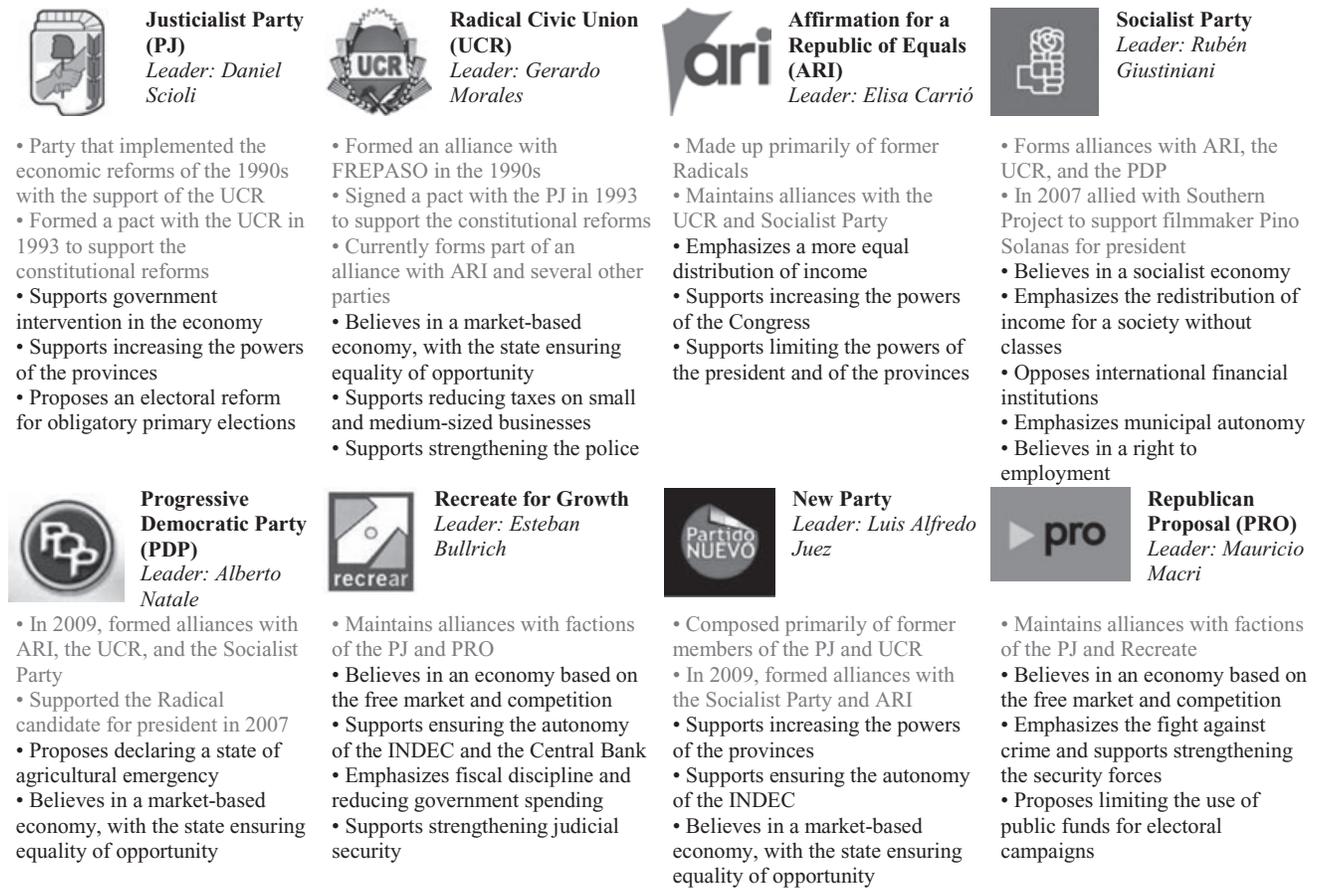
My analysis of the experimental results follows the convention of using difference-of-means and difference-of-proportions tests to measure average treatment effects.²³ I also corroborate my results using regression analyses in which the experimental treatment is included as an independent variable with and without covariates, namely gender, age, education, political information, and province (see Tables A4 and A5 in the supporting information).

²¹Note that while I treat these survey items as constituting causal mechanisms, or mediating variables, they were asked in the survey after the item on party identification, the main outcome variable. This was done to avoid priming, which would make any mediation result a possible artifact of question order.

²²Specifically, political information measures the number of factual political questions the respondent answered correctly. Respondents were told the following: “I am going to name a couple of political leaders. Please tell me the position they hold in the current government.” They were asked about two political figures, Aníbal Fernández, the chief of staff, and Julio Cobos, the vice president; 23.9% of respondents correctly identified Fernández’s position and 72.5% correctly identified Cobos’s.

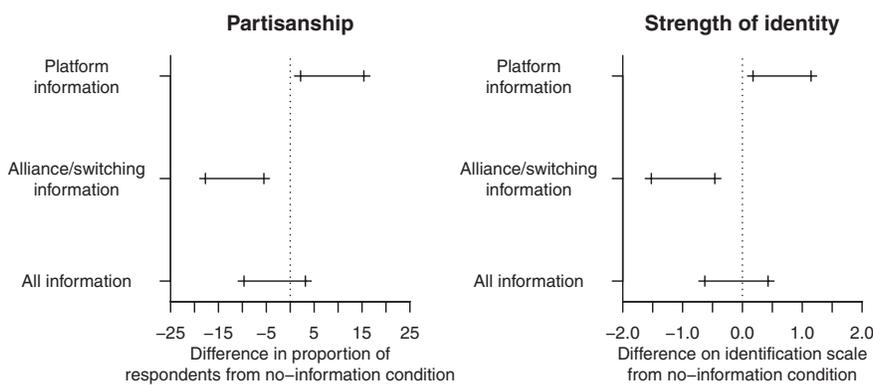
²³The difference-of-means tests reported here are two-tailed *t*-tests using Welch’s approximation to account for unequal variances between treatment groups. An alternative preferred by Keele, McCaughy, and White (2012) is the Wilcoxon rank-order test. My results are all robust to this alternative test, as well as to the Fligner-Policello rank-order test, which accounts for unequal variances between treatment groups (see Table A3 in the supporting information).

FIGURE 1 Complete Party Information Card



Note: Gray identifies the text included in the alliance/switching-information treatment.

FIGURE 2 Average Treatment Effects



Notes: Average treatment effects calculated as the difference between each treatment condition and the control (no information) condition. Values in the left panel refer to whether or not respondents said they identified with a political party in each condition. Values in the right panel refer to the average strength of partisanship's identification on a 0–10 scale. Horizontal lines represent 95% confidence intervals, while vertical bars indicate 90% confidence intervals.

Main Results

Figure 2 shows the main results of the information experiment, the average treatment effects (ATEs). The left-hand panel refers to the proportion of respondents who said they identified with a party, while the right-hand panel focuses on the strength of those identities among partisans.²⁴ The ATEs are measured simply as the difference on these metrics between each treatment condition and the no-information (control) condition.

Provided with no information, 35.8% of respondents in the control condition said they identified with a political party, and the strength of that attachment averaged 7.1 on the 0–10 scale. Among respondents who received the platform information, 44.6% said they identified with one of the parties, and the average strength of their attachment was 7.8. The differences between these conditions were both statistically significant ($p < 0.029$, $p < 0.026$), suggesting that the platform information had a positive average effect on partisanship. Consistent with expectations, respondents provided with information that distinguished parties from one another were more likely to identify with a party.

Respondents who received the alliance/switching information, on the other hand, were on average less likely than those in the control condition to assert a partisan attachment, and those party attachments they did have were weaker. In this condition, only 24.2% of respondents said they identified with a political party, and the strength of this attachment averaged 6.1, values that were statistically different from those for the control condition ($p < 0.002$, $p < 0.003$). Again, these results are consistent with the empirical implications of the theory. The information about convergence among the parties weakened partisan attachments.

When both types of information were provided simultaneously, the results were indistinguishable from control. In the all-information condition, 32.5% of respondents said they had a partisan attachment and the strength of those attachments was on average 7.0. These results were not statistically different from the control condition ($p < 0.405$, $p < 0.756$), suggesting that the countervailing effects of the platform and alliance/switching-information treatments canceled each other out.²⁵

²⁴Since the strength of attachment is measured only among identifiers, the control and treatment samples are not quite identical, so the results on this score should be considered suggestive.

²⁵There is a potential in this experiment for noncompliance among respondents assigned to treatment. This would occur if respondents handed pages with a particular kind of information simply failed to read it. One way to account for noncompliance is to calculate the

The magnitudes of the effects of the platform and alliance/switching information were substantial. The platform information increased the proportion of respondents saying they identified with a party by nearly 9 percentage points. And the alliance/switching information decreased that proportion by over 11 percentage points. These are sizable proportions of the population, particularly for a competitive political context with fairly small margins of victory. Moreover, the manipulation used here—that is, the information provided—is minimal relative to the political information with which voters are bombarded during the course of political events (and even more so during electoral campaigns). In that regard, the effects of convergence information in the natural world of political campaigns are likely even larger than the sizable differences observed in this experiment.²⁶ At the same time, my survey experiment measured the effects of different types of information *conditional* on receiving that information. In the natural world there is, of course, heterogeneity in the degree to which individuals receive political information, which could weaken the overall population effect.

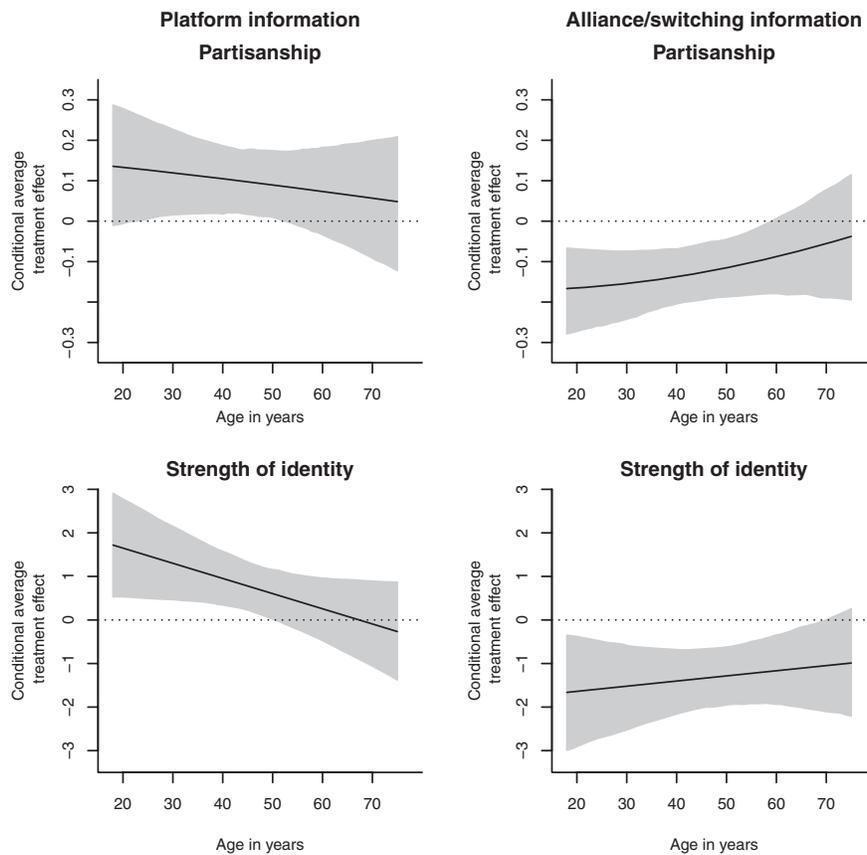
Heterogeneous Treatment Effects

The branding model suggests that individuals with weaker priors about party brands should most quickly update their perceptions and partisan attachments in response to new information. I expect that younger and less-informed individuals are the ones most likely to have weak priors about the parties. To test this proposition, I estimate conditional average treatment effects (CATEs) using age and political information as conditioning variables.

A straightforward approach to estimating CATEs is to estimate causal effects separately for different subgroups of respondents and compare them using analysis of

complier average causal effect (CACE) rather than the ATE. This means that we look at the effect of the treatment only on those who actually complied with the treatment (Gerber and Green 2012). In the information experiment, interviewers were instructed to record the amount of time a respondent spent reading the information page. One way to measure compliance in this experiment is thus to focus on those respondents who spent more than one minute reading the information page, since those who spent less than that likely did not receive the treatment. The results of such an analysis, reported in Table A6 in the supporting information, are substantively the same as the ATEs reported here.

²⁶Scholars have justifiably criticized survey experiments of this kind for their inattention to the duration of treatment effects (Gaines, Kuklinski, and Quirk 2007). Although the limitations of this survey did not allow me to measure effect duration, I would argue that the sizable effect of such a minimal manipulation at least suggests a durable effect in the natural world.

FIGURE 3 Average Treatment Effects, Conditioned by Age

Notes: Values represent the difference between the treatment and control (no information) conditions by age. In the top panels, values refer to the proportion of respondents who said they identified with a political party. In the bottom panels, values refer to the average strength of respondents' partisan attachments on a 0–10 scale. The left panels present the effect of the platform-information treatment; the right panels the effect of the alliance/switching treatment. Shaded regions indicate 95% confidence intervals.

variance. This avoids making assumptions of linearity, as in regression interaction models. In the case of age, I compare the average treatment effects for younger (under 35) respondents to those of older (over 55) respondents. In the case of political information, I compare respondents who answered neither question correctly (low information) with those who correctly answered both questions (high information). Sample sizes and measurement error mean that these results are primarily suggestive.

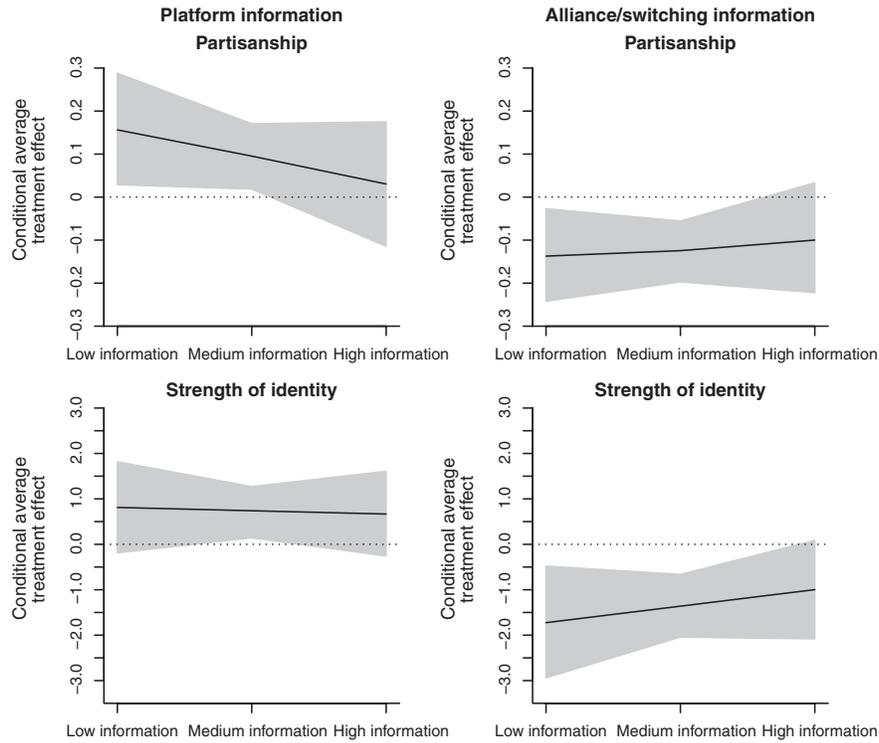
For expository purposes, I graph the linear interaction effects.²⁷ Figure 3 displays the average treatment effects of the platform and alliance/switching informa-

tion on partisanship and the strength of attachment conditioned on age. The theoretical model predicted that updating would slow down over the life course, suggesting that the positive effects of the platform information and negative effects of the alliance/switching information should both become attenuated and approach zero.

Indeed, the panels in Figure 3 suggest that age conditions the average treatment effects in the predicted direction. In the left-hand panels for the platform-information treatment, the positive treatment effect on both partisanship and the strength of attachment declined progressively with respondent age. The difference in effects was statistically significant when comparing the young and the old with regard to partisanship ($F_{(583)} = 6.21$, $p < 0.013$), but not with regard to the strength of partisanship ($F_{(222)} = 1.80$, $p < 0.181$). In the right-hand

²⁷These linear interaction effects were simulated using the *Zelig* package for R (Imai, King, and Lau 2007) and are based on a female Córdoba resident with the remaining variables at their sample means. The linear effects are statistically significant with regard to two of the age interactions and one of the information interactions.

FIGURE 4 Average Treatment Effects, Conditioned by Political Information



Notes: Values represent the difference between the treatment and control (no information) conditions by level of political information. In the top panels, values refer to the proportion of respondents who said they identified with a political party. In the bottom panels, values refer to the average strength of respondents’ partisan attachments on a 0–10 scale. The left panels present the effect of the platform-information treatment; the right panels the effect of the alliance/switching treatment. Shaded regions indicate 95% confidence intervals.

panels for the alliance/switching-information treatment, the average effect on both partisanship and the strength of attachment was significantly negative but increasing with respondent age. In both cases, the difference in effects between the young and old was also statistically significant ($F_{(592)} = 23.74, p < 0.000$; $F_{(172)} = 21.97, p < 0.000$).

The same general patterns can be seen with regard to political information. The theoretical expectation, as with age, was that political information would attenuate the information treatment effects. The measure of political information employed here is blunt, such that there are only three respondent categories to compare: those with low, medium, or high levels of political information. The result is that the sample is not evenly distributed among these three groups. Still, the panels in Figure 4 are broadly consistent with expectations. The figure presents the same average treatment effects as before, now conditioned by the level of political information of the respondent. In the left-hand panels for the platform-

information treatment, the average treatment effects on both partisanship and the strength of attachments appear to decrease with political information. But neither effect is statistically significant in a comparison of the low- and high-information respondents ($F_{(587)} = 0.87, p < 0.351$; $F_{(222)} = 0.06, p < 0.813$). In the right-hand panels for the alliance/switching-information treatments, the average treatment effects on partisanship and strength of attachment appear to be increasing with political information. Comparing low- and high-information respondents, the effect with respect to partisanship is statistically significant ($F_{(595)} = 9.23, p < 0.003$), while that for the strength of partisanship is not ($F_{(173)} = 1.90, p < 0.170$).

In short, I find some evidence that age and political information attenuate the ATEs. While limited samples and measurement error mean that not all of these predicted effects reach statistical significance, the direction of the CATEs is consistent with the expectations of the branding model, whereby individuals with weak priors

about party brands update more quickly than those with strong priors.

Causal Mechanisms

These results demonstrate that, on average, distinguishing (convergence) information increases (decreases) partisan attachments and strengthens (weakens) them. But we might still wonder about the causal mechanisms underlying these effects. Do respondents who received the platform information perceive more differences between the parties, as suggested by the branding model? Do those who received the alliance/switching information perceive fewer differences? There is some debate among experimentalists about the appropriate means for uncovering the mechanisms behind experimental treatments, what in other contexts is thought of as identifying mediating variables (see Bullock, Green, and Ha 2010; Green, Ha, and Bullock 2010; Imai et al. 2011). The present survey experiment is an instance of what Imai, Tingley, and Yamamoto (forthcoming) refer to as the single-experiment approach, wherein both the outcome and mediator are measured within the same experimental treatment.²⁸

In order to examine whether the mechanism behind the observed treatment effects resulted from a perception of party convergence/divergence, respondents were asked (posttreatment) to place the eight major Argentine political parties on a 0–10 left-right scale.²⁹ We can approximate the degree to which respondents perceive differences between the parties as the distance on this scale between the two parties the respondent perceives to be furthest apart. In other words, we can measure the degree of polarization perceived by the respondent to exist in the party system.³⁰ Many respondents failed to answer the left-right ideology questions (62% of respondents placed

²⁸The major limitation of this approach is that it requires the untestable assumption that pretreatment characteristics of the respondent are uncorrelated with the individual's value on the mediating variable (conditional on treatment status). Green, Ha, and Bullock (2010) and Imai, Tingley, and Yamamoto (forthcoming) suggest alternative designs that rely on the ability of the researcher to directly manipulate the mediating variable. Yet it seems difficult to directly manipulate respondents' perceptions about political parties. As Green, Ha, and Bullock suggest, an exploratory analysis may instead proceed by "introducing survey measures to check whether these inducements produce an intervening psychological effect consistent with the posited mediator" (2010, 208).

²⁹The wording was, "In politics, people sometimes talk about left and right. Imagine a scale from 0 to 10 where 0 represents left and 10 represents right. Using this scale, where would you position the following political parties?" The parties were then listed one by one.

³⁰This measure is similar to the one used by Berglund et al. (2006) in a related analysis (see also Schmitt and Holmberg 1995). Such a measure might be problematic in systems with ideologically ex-

at least two parties), so this measure is quite noisy. Still, a finding that the platform information increases both partisanship and perceived polarization—along with the reverse result for the alliance/switching information—would offer suggestive evidence that perceived differences between the parties underly the partisanship effects.

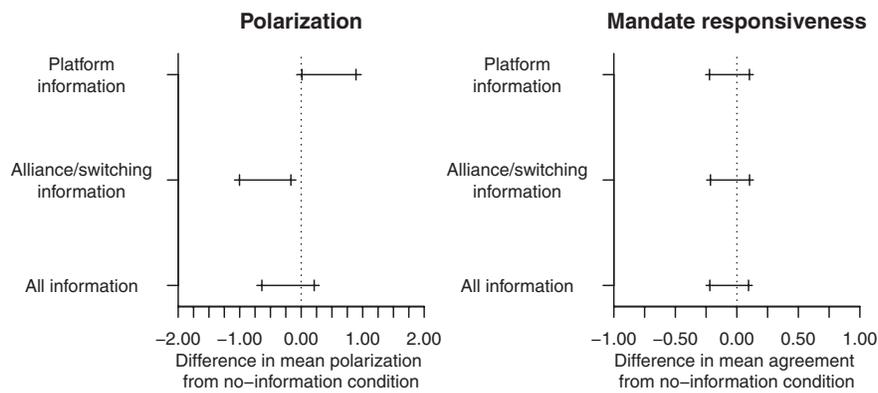
One alternative mechanism of the information treatments should also be considered. I expect the alliance/switching information to erode partisan attachments by reducing, in respondents' minds, the differences between the parties. However, it is plausible that this information could instead prime respondents to think about whether parties are responsive.³¹ Thinking about mandate responsiveness might generate a negative reaction if respondents have normative preferences for parties that fulfill their electoral platforms. That is, respondents may interpret the alliance/switching information as an indication that party elites do not fulfill their policy mandates.

The survey included a posttreatment item on mandate responsiveness. The precise wording of the question was, "Some people say that political parties in Argentina generally implement the policies they promise during the election campaigns. Do you disagree strongly, disagree somewhat, neither disagree nor agree, agree somewhat, or strongly agree with this?" In order to investigate whether norms of mandate responsiveness are driving the results, I examine whether the information treatments affected responses to this question. If the alliance/switching-information treatment affects partisanship but not responses to the mandate-responsiveness question, that would seem to provide compelling evidence that a rejection of parties for unresponsiveness is not driving the results.

Figure 5 shows the effects of the information treatments on these questions. Looking at the left panel on polarization, respondents in the platform-information condition appear significantly more likely to perceive party polarization than those in the control condition ($p < 0.092$). Conversely, those in the alliance/switching-information condition were significantly less likely to think that parties were distinct ($p < 0.022$). This is at least suggestive evidence that the mechanism underlying the average treatment effects is indeed the one implied by the theoretical model, that alliance/switching information

trems parties, but all eight Argentine parties included here are fairly moderate: on average, respondents placed them between 3.6 and 6.3 on the 0–10 scale.

³¹It is not clear how this mechanism would explain the positive effect of the platform information on partisanship, but it nevertheless seems important to address this potential alternative at least with regard to the effect of the alliance/switching information.

FIGURE 5 Average Treatment Effects—Causal Mechanisms

Notes: Average treatment effects calculated as the difference between each treatment condition and the control (no information) condition. Values in the left panel refer to the overall degree of polarization between respondents' placement of parties on a 0–10 ideological scale. Values in the right panel refer to the degree of agreement with the notion that parties fulfill their campaign promises once elected. Responses range from 0 (“strongly disagree”) to 4 (“strongly agree”). Horizontal lines represent 95% confidence intervals, while vertical bars indicate 90% confidence intervals.

makes respondents less likely to be partisan by making them less able to differentiate among party brands.

The results in the right panel of Figure 5, on the other hand, offer no reason to think that the mandate-responsiveness mechanism is driving the association between the convergence information and partisan attachments. Respondents in the alliance/switching-information condition were not significantly more likely than those in the control condition to agree with the statement that parties are unresponsive ($p < 0.564$).³² These results suggest that the mechanism underlying the information effects was not general disillusion with parties or the political elite, but rather the degree to which respondents could distinguish between party brands. Although further experiments are needed to better identify this mechanism, this constitutes preliminary evidence that information about interparty alliances and party switching made respondents less able to distinguish parties and, in turn, less likely to identify with one.

Party Brands and Partisanship

Much of the debate about the nature of partisanship—whether it should be thought of as a social identity or

the result of rationally maximizing expected utility—has centered on the degree of partisan stability. Findings of partisan fluctuations are taken as evidence against the social-identity perspective under the assumption that identities are supposed to be stable. But this article has highlighted that drawing such a conclusion also assumes that party brands are fixed. If instead party brands change over time, then partisan instability is perfectly consistent with a social-identity conception of partisanship. In other words, partisanship as a social identity is only an “unmoved mover” if parties are also unmoved. Once we relax the assumption of fixed party brands, we can derive the circumstances under which partisanship should weaken without abandoning the notion of social identity.

My branding model of partisanship does just that. Building on previous updating theories of partisanship, the model incorporates elements of social-identity theory, including the notion of comparative fit. It suggests that party attachments are based not only on learning about party brands, but also on the extent to which voters see parties as distinct from one another. Social identities, after all, are built not only on perceived ingroup affinity but also on perceived differences with outgroups. The model suggests that convergence by rival parties, making their brands less distinguishable, should weaken party attachments.

To test this hypothesis, I conducted a survey experiment in Argentina. By manipulating respondents' exposure to different types of actual information about Argentine parties, the experiment provided a clear test of the

³²An alternative test for these causal mechanisms is the mediation analysis proposed by Imai et al. (2011). Using this method, the estimated average causal mediation effects are consistent with the reported findings (see Table A7 in the supporting information).

hypothesis that party convergence dilutes partisanship. Providing respondents with distinguishing information about the parties increased partisanship and the strength of attachments. Conversely, providing respondents with information about alliances and switching among the parties decreased partisanship and weakened partisan attachments. These treatment effects also seemed to be attenuated by age and political information, as predicted by the Bayesian framework of the branding model. I offered suggestive evidence that the mechanism underlying these observed treatment effects is consistent with the theory. The survey experiment thus offers convincing evidence for the causal effect of party convergence on partisanship.

Like all survey experiments, these treatments are abstracted from actual politics and respondents receive information about the parties through artificial means. In the natural world, however, voters may be exposed to both distinguishing and convergence information. While the survey experiment allowed me to identify a causal relationship, doing so required clearing away some of the complexity of the natural world by presenting respondents with only one type of information. To study this process observationally, we would need to pay attention to whether the balance of voters' observations of the parties in a given time period leans substantially toward distinguishing parties from one another or instead toward convergence. Looking to observational evidence that voters indeed respond to party convergence in this way in the natural world would also increase our confidence in these experimental findings. It would also be useful to test other implications of the branding model in different settings in order to bolster the external validity of these findings. These are tasks I take up elsewhere (Lupu 2011).³³

In proposing and testing a branding model of partisanship, my aim has not been to adjudicate between social-identity and rationalistic conceptions of partisanship. Although I present my model as one of social-identity formation, it could be rewritten into an expected-utility framework (as in Grynaviski 2010).³⁴ A broad interpretation of the rationalistic perspective could justify including party platforms as signals to voters of the benefits they might expect to receive from a party. My experimental results would be consistent with such a model, and they have certainly not ruled out the possibility that performance evaluations also play a role in partisanship.

³³Note also that my findings regarding the platform-information treatment are consistent with those of Brader and Tucker (2008b) in postcommunist settings.

³⁴Presumably, such a model would need to also include performance evaluations, as previous models in this tradition have done (Achen 1992; Fiorina 1981; Gerber and Green 1998), since past performance is an obvious indicator of expected utility.

Nevertheless, proposing the branding model in terms of social identity showed that partisan instability is, under certain circumstances, entirely consistent with a social-identity perspective. Merely finding evidence of partisan instability does not, on its own, imply that partisanship is not a social identity. Such a conclusion would also require us to know something about the dynamics of party brands.

Since the vast majority of partisanship studies focus on advanced democracies, they may have been on solid theoretical ground in assuming fixed party brands. But we cannot overlook the interaction between party brands and partisanship in contexts where party brands are emerging or changing, as they are in many developing democracies. Our theories of partisanship will indeed be enriched as researchers expand its study to these contexts. Moreover, developing democracies like Argentina offer unique opportunities for testing partisanship theories in ways made unfeasible by the stable party brands and strongly held attachments in the United States and other advanced democracies.

Thinking of party brands as variable suggests avenues for further research not only in developing democracies but also in advanced ones. Even in these contexts, where party brands are relatively stable, parties do undergo incremental changes. The polarization of the U.S. parties is one instance in which my model would predict increasing partisan attachments.³⁵ As the brands of the two parties become clearer and more distinct, voters' attachments may become stronger. The convergence of many Western European parties in the realm of economic policy may have conversely eroded partisan attachments. Parties in parliamentary democracies also converge in instances of grand coalitions or unity governments. The effects of these party behaviors on partisanship is a topic scholars should take up.

The notion that convergence erodes partisanship also suggests a new way of thinking about party strategy. Spatial models of party competition typically focus on Euclidean distances to determine parties' electoral incentives. But the branding model of partisanship suggests that parties, if they wish to maximize their partisan ranks, may face conflicting electoral and branding incentives. Although convergence among parties is often an equilibrium result in spatial models (e.g., Enelow and Hinich

³⁵Various scholars have found that the polarization of U.S. political elites in recent decades is correlated with increased partisanship among voters (Brewer 2005; Hetherington 2001; Levendusky 2009). Similarly, Schmitt and his coauthors find cross-national evidence of an association between party-system polarization and aggregate partisanship (Berglund et al. 2006; Holmberg 1994; Schmitt 2009; Schmitt and Holmberg 1995).

1984), branding incentives may lead parties to prefer divergence in order to distinguish their brand from those of competing parties. Spatial models that incorporate parties' interests in fostering and maintaining partisan attachments might generate new expectations regarding party strategy and competition.

Taking party brands into account may also help us understand periods in which partisanship erodes, whether in advanced democracies or developing democracies with long party histories, as in Latin America. In Argentina, for instance, 64% of voters identified with one of the country's two oldest parties in 1985, but that number dwindled to 15% by 2002. In Venezuela, a 1973 survey found that over 70% of respondents identified with one of its established parties, but less than 10% still did so in 1997 (Lupu 2011). Scholars have offered a number of prominent explanations for partisan erosion (e.g., Dalton 1984; Wattenberg 1990), but these remain contested (Albright 2009; Pérez-Liñán 2002). My branding model suggests an alternative explanation: the dilution of party brands.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Formal Branding Model of Partisanship
Survey Methodology

Table A1: Survey sample representativeness

Table A2: Comparison of treatment conditions

Table A3: Average treatment effects

Table A4: Regression analyses of average treatment effects, without covariates

Table A5: Regression analyses of average treatment effects, with covariates

Table A6: Complier average causal effects

Table A7: Average causal mediation effects

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