

# I'm a Survivor: Political Dynamics in Bureaucratic Elites' Partisan Identification

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**T**his article challenges the common assumption that the partisan identification of bureaucratic elites is fixed over time. Building on principal-agent and organization theory, we hypothesize that bureaucratic elites may respond to political turnover by adjusting their partisan identification toward that of their (new) elected principals. We test this prediction using data from the American State Administrators Project (ASAP) over the 1964–2008 period, which allows us to study the same US agency leaders ( $N=951$  individuals) before and after partisan shifts in their agency's elected principals. We find significant evidence that agency leaders remaining in office following a shift in the party in power on average reorient their partisan identity in response to such turnover events. These adjustments are stronger for agency leaders directly appointed by, or in more frequent contact with, their elected principals. Our results suggest a malleability of partisanship seldom attributed to bureaucratic elites in public and academic discourse.


## INTRODUCTION

**T**he (in)stability of individuals' partisan identification is among the most prominent and persistent debates within the study of American politics (Key 1966; Kollman and Jackson 2021; Lazarsfeld, Berelson, and Gaudet 1944; MacKuen, Erikson, and Stimson 1989). Going back to the pioneering work of Campbell et al. (1960), some scholars treat partisan identification as a near-constant social identity that “stabilizes partisan preferences over a lifetime” (Tucker, Montgomery, and Smith 2019, 310). Others challenge this presumption of (near-)stability and theorize that “claimed identities can change (...) over time, as people have new experiences and encounter new contexts” (Egan 2020, 701). To date, this debate concentrates on (un)stable partisanship within the mass public. Yet, understanding the (non-)adaptive nature of partisanship among bureaucratic elites also holds important implications for our understanding of politics and public policy (Clinton and Lewis 2008; Kappe and


Schuster 2022; Nixon 2004; 2023; Richardson, Clinton, and Lewis 2018). Public bureaucracies are, after all, deeply involved in policy formulation, development, and implementation (Aberbach, Putnam, and Rockman 1981; Key 1942; Murdoch, Connolly, and Kassim 2018; Waldo 1952). As we note below, most of the bureaucratic politics literature tacitly assumes that bureaucratic elites do not change their party identification over time. This article is, to the best of our knowledge, the first to challenge this assumption.

More specifically, we investigate whether and how bureaucratic elites respond to a political turnover event, which we define as a change in the party holding power in the bureaucrat's political environment. Previous work shows that political turnover can have substantial effects on staff turnover in public organizations (Aberbach, Putnam, and Rockman 1981; Bach and Veit 2018; Bolton, de Figueiredo, and Lewis 2021; Christensen, Klemmensen, and Ostrup 2014; Colonelli, Prem, and Teso 2020; Mayntz and Derlien 1989; Spenkuch, Teso, and Xu 2023). Although staff turnover is *not* the only possible response to political turnover, much less attention has been awarded to other coping strategies available to public employees (Bolton, de Figueiredo, and Lewis 2021; Golden 2000; Scholz 1986). This article addresses this persistent gap by investigating a previously unexplored coping strategy, which we refer to as a “malleability of partisanship”: that is, the ability to reorient one's partisan identification after a political turnover event.

We hypothesize that bureaucratic elites may respond to political turnover by modifying their partisanship toward that of their new political principals through a

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process of “adaptive adjustment” (Bendor and Moe 1985; March and Olsen 1975; Moe 1985; Yackee 2023). We theorize several mechanisms that can bring about this response and furthermore maintain that the strength of this adaptive process differs depending on how closely bureaucratic elites’ world is intertwined with that of politicians. Building on insights from organization theory and psychology, two moderating factors are included as a central part of our theorizing: the nature of bureaucratic elites’ appointment process (Bach and Veit 2018; Ban, Park, and You 2023; Mayntz and Derlien 1989) and contact patterns between bureaucratic elites and politicians (Bolton, de Figueiredo, and Lewis 2021; Burkhardt 1994; Christensen and Ostrup 2018; Egeberg and Trondal 2011; Kiecolt 1988).

Our empirical analysis relies on data from the American State Administrators Project (ASAP), which is a survey of all state agency leaders across all US states fielded ten times over the 1964–2008 period (Yackee and Yackee 2021). This dataset holds four benefits for our analysis. First, it provides data from the perspective of bureaucratic elites (Kertzer and Renshon 2022). Second, it contains information about US state agency leaders’ partisanship (our dependent variable), as well as their appointment process and contact patterns (our moderators). The ASAP also allows for the integration of other data based on the ASAP survey year and US state where a respondent is employed, including political turnover events (our main independent variable). Third, the 45-year timeframe offers substantial variation across time and space in the party in power at the US state level. Since political turnover events derive from state-wide elections and are exogenous to the bureaucratic elites under analysis, we can exploit them to identify our main relationship of interest (i.e., political dynamics in bureaucratic elites’ partisanship). Finally, although each ASAP wave is an independent cross-sectional study, we implement an innovative methodology to extract a panel dataset that allows following the *same* state agency leaders (951 individuals) before and after partisan shifts in their agency’s elected principals (i.e., either the governor or state legislature). This longitudinal dimension at the *individual level* enables the estimation of first-difference regression models that accommodate unobserved individual-level heterogeneity (Wooldridge 2010). Thus, we leverage a (quasi-)natural experimental design to derive stronger causal inferences than is feasible via cross-sectional analyses (Baekgaard, Herd, and Moynihan 2023; Foos and Bischof 2022).

Our findings indicate that US state agency leaders who remain in office after a change in the party in power on average display a tendency to reorient their partisan leaning toward their new political principals. Importantly, we do *not* find a similar response to partisan turnover in the US presidency or in the balance of partisan support among state-level voters. This strengthens our inference that US state agency leaders adjust their partisan leaning toward that of their direct political principals, rather than toward general ideological shifts in the country or state. Furthermore, in line with theoretical expectations, the observed adaptive

adjustment is more prominent among those appointed by, or in more frequent contact with, their elected principals. Overall, our findings show that bureaucratic elites are sensitive to changes in political power particularly when their position is closely intertwined with the world of politicians.

As mentioned, our results are conditional on retaining one’s position after a political turnover event (henceforth designated as “survivors”). Yet, such survivors are a pivotal group to study as they constitute the majority of those in state agency leader positions. For instance, 58% of ASAP respondents “survived” at least one partisan shift in the governor’s office. Thus, the modal state agency leader is a survivor. Given this, a critical insight from our analysis is that, on average, “surviving” state-level bureaucratic elites meaningfully adjust their partisanship in line with changing political dynamics in their state. Whether this is a good thing arguably depends on one’s normative standpoint. Some may desire bureaucratic elites to offer advice untainted by partisan pressures or inhibitions, while others may appreciate a degree of responsiveness or “serial loyalty” toward elected officials. Either way, our findings require that we revisit the persistent characterization of bureaucratic elites as notable mainly for their “disconnection and fossilization” (Lodge and Hood 2003, 135; Moynihan and Ingraham 2010; Rainey and Steinbauer 1999).

## THEORETICAL BACKGROUND AND HYPOTHESES

In this section, we first build on the adaptive model of bureaucratic politics developed by Bendor and Moe (1985) and Moe (1985) to argue that bureaucratic elites may—for several reasons—adjust their partisan identification following political turnover. Then, we turn to organization theory and organizational psychology to derive two scope conditions under which such adaptive adjustments are particularly (un)likely to arise.

Bendor and Moe (1985) set out a general framework to study the interdependent decisions of political and administrative actors. This framework takes from principal-agent models that the relationship between politicians and administrators is hierarchical and that both sets of actors choose among available alternatives based on a self-interested calculation of expected costs and benefits (Moe 1985). Yet, crucially, all participants in the system are assumed to be boundedly rational (Simon 1957). They act on incomplete information and are not aware of all alternatives and their consequences. The latter assumption allows for dynamic processes whereby actors “adapt in simple ways to their environments, moving in directions that appear to promise them greater utility” (Bendor and Moe 1985, 756). This becomes reflected in agents’ “adaptive adjustment” to signals arriving from the broader (political) environment (Bendor and Moe 1985; Moe 1985, 1094; Yackee 2023). In a similar spirit, March and Olsen (1975, 147) build on a “view of limited rationality” to argue that “individuals in organizations

modify their understanding in a way that is intendedly adaptive.”

Applying this general framework to our setting, we maintain that any adaptive adjustment of bureaucratic elites' partisan leaning following a change in the party in power can come about for (at least) three reasons. First, politicians are often argued to prefer bureaucrats who better match their own ideological stance and policy preferences (the *ally principle*; Bendor, Glazer, and Hammond 2001; Palus and Yacke 2016; 2022). The reason is that such ideological alignment helps mitigate problems and inefficiencies related to task delegation (Bendor, Glazer, and Hammond 2001; Fiva et al. 2021; Spenkuch, Teso, and Xu 2023; Toral 2024). When politicians surround themselves with allies via politically motivated hiring, firing, promotion, and remuneration decisions (Bach and Veit 2018; Colonnelli, Prem, and Teso 2020; Fiva et al. 2021; Mayntz and Derlien 1989), any perceived lack of alignment can prove costly for bureaucratic elites. This gives them an instrumental motivation to “move far in accommodating political executive demand” (Christensen, Klemmensen, and Ostrup 2014, 215). In our setting, we argue that this could include reorienting their partisan leaning following a change in the party in power.

Second, recent research in organizational psychology argues that leaders can produce both short- and long-term changes in subordinates by influencing—through their rhetoric, actions, and characteristics—the relative salience of different aspects within subordinates' claimed identities (Epitropaki, Kark, and Mainemelis 2017; Geys et al. 2020). The core idea is that the “refinement of one's identity is an ongoing and central aspect of organizational membership that depends, in part, on the relationship with one's supervisor” (Lord, Gatti, and Chui 2016, 125). As illustrated by Geys et al. (2020, 555), leadership successions can then “invoke observable shifts in individuals' attitudes” by affecting subordinates' sense-making within organizations (Weick 1995). In a similar vein, the arrival of new political principals with a different partisan orientation can be expected to trigger an adaptive adjustment in bureaucratic elites' partisan identification.

Finally, most humans share a basic need of belonging (Baumeister and Leary 1995; Van Ryzin 2021). This makes them keen to conform to the opinions of significant others—such as elected principals in the case of bureaucratic elites—“for the purpose of altering (shaping) positively the evaluations or attributions of relevant others” (Liden and Mitchell 1988, 572). A closely related psychological argument relates to human's need to limit feelings of “cognitive dissonance” (Festinger 1957) when, for instance, discord arises between the partisan leaning of bureaucratic elites and the arrival of new political principals. Since it is impossible for bureaucratic elites to adjust the partisan leaning of a new party in power, adjusting their own partisan identification is possible to mitigate any sense of dissonance (Acharya, Blackwell, and Sen 2018; Homola, Pereira, and Tavits 2020).

Common to all three lines of argument is that political turnover can prompt shifts in bureaucratic elites'

partisan identification toward that of their new elected principals. The ensuing “politicized identity shifting” (Egan 2020, 714) reflects an ideological pragmatism that constitutes a “fundamental attribute of an adaptive decision-maker” (Bendor and Moe 1985, 764; March and Olsen 1975; Scholz 1986). This leads to the first hypothesis:

**H1:** *Bureaucratic elites adjust their partisan identification in the same direction as the shift in partisanship of their elected political principals.*

Naturally, the response of bureaucratic elites to political leadership succession may differ depending on how closely their world is intertwined with that of politicians. Building on organization theory and organizational psychology, we assert that appointment processes and contact patterns constitute two key moderating factors.

First, the organization theory approach to public administration emphasizes that organizational features of public bureaucracies shape civil servants' role perceptions and opinions (Christensen, Lægheid, and Røvik 2021; Egeberg 1999; March and Olsen 1984). One of these features relates to staff members' appointment procedures. Appointments requiring explicit political consent or being in the purview of politicians—as in the so-called spoils system (Ferguson 2018)—are more likely to guide bureaucratic elites' role perceptions and opinions toward those of their political principals compared to recruitment via, for instance, a civil service or merit-based appointment (Bach and Veit 2018; Ban, Park, and You 2023; Mayntz and Derlien 1989). The reason is that the influence of politicians over appointment procedures strengthens the instrumental motivation of pragmatic bureaucratic elites to adapt to their elected principals. As a result, we hypothesize that bureaucratic elites appointed by their political principals show a higher level of adaptive adjustment to political turnover:

**H2:** *Adaptive adjustments in bureaucratic elites' partisan identification are stronger when their appointment requires explicit political approval.*

Second, it is well known from research in social and organizational psychology that individuals' attitudes are formed and developed through interactions with significant others (Burkhardt 1994; Kiecolt 1988). Organization theory scholarship likewise holds that direct and personal contacts enabled via organizational structures form individuals' attitudes and behavior by conveying information, expectations, and feedback (Christensen and Lægheid 2003; Egeberg and Trondal 2011). Building on this line of argument, Christensen and Ostrup (2018) highlight that close politician–bureaucratic interactions induce higher levels of political responsiveness among public employees. Bolton, de Figueiredo, and Lewis (2021, 462) make a similar argument by stating that adjustments by public employees toward (the preferences of) political principals are stronger among those “most likely to interact

with political appointees.” In our setting, this would imply that:

**H3:** *Adaptive adjustments in bureaucratic elites’ partisan identification are stronger when they have more frequent contact with their political principals.*

## INSTITUTIONAL SETTING AND DATA

### Institutional Setting

The 50 US state governments hold legislative, executive, and judicial authority within the state’s boundaries. Every state has a bicameral legislative system with elected lower and upper chambers (except the unicameral Nebraska), and the executive branch of government is headed by a directly elected governor. Elections for all these political bodies are partisan in nature (except the legislative elections in Nebraska) and are usually dominated by the Republican and Democratic parties. US states have wide-ranging authority in, among others, education, healthcare, economic development, infrastructure, emergency management, security, and environmental policy. In each of these areas, state-level public agencies play a key role in the determination and implementation of public policy.

Our empirical analysis focuses on the top administrative positions in US state-level public agencies. Considerable variation in appointment procedures for such positions exists across as well as within US states. There are five main mechanisms. First, some leaders—such as lieutenant governors, secretaries of state, and state treasurers—are popularly elected in some states but not in others (Folke, Hirano, and Snyder 2011). Second, the governor directly appoints numerous agency leaders: a power that is considered more significant when the legislature is *not* required to consent to the appointment (Ferguson 2018). This appointment style comes the closest to the so-called spoils system whereby the governor can use political appointments to the top positions across the bureaucracy to reward loyalty.

Third, some agency leaders are appointed by other department heads—such as a state’s Secretary of the Department of Transportation appointing a state’s Highway Administrator. In these cases, as Bowling (2018) states, the department head is likely to be a political appointee of the governor, which makes these subordinate agency leaders one step removed from the governor’s influence. Fourth, some states have a considerable number of independent boards or commissions whose leaders are sometimes chosen by the governor and sometimes not (Folke, Hirano, and Snyder 2011). Fifth, a number of agency leaders are appointed via a merit-based system (Bowling 2018). Table 1 provides an overview of the relative prevalence of these appointment methods in our data, which we exploit to operationalize the level of political influence over appointment processes when testing hypothesis H2.

**TABLE 1. Appointment Procedures for US State Agency Leaders**

	ASAP dataset (%)	Panel dataset (%)
Governor with legislative consent	23	18
Governor without legislative consent	16	13
Board/commission with governor’s consent	6	7
Board/commission without governor’s consent	14	19
Department head	26	25
Popular election	4	4
Other (mainly civil service merit system)	12	13

*Note:* The table shows the share of respondents in the dataset(s) who obtained their agency leadership position via a given appointment procedure. The ASAP dataset includes 11,592 responses from 10,399 unique respondents, while our panel dataset contains 2,081 responses from 951 unique respondents.

### Data

Our dataset combines two main data sources. The first is the ASAP dataset, which brings together surveys conducted among US state agency leaders in 1964, 1968, 1974, 1978, 1984, 1988, 1994, 1998, 2004, and 2008. Each wave includes respondents from all 50 American states and covers agencies across all functional areas of government (Brudney and Wright 2002; Yackee and Yackee 2021). Crucially, the ASAP surveys include detailed individual background characteristics, including respondents’ partisan self-identification and their contacts with political elites. Survey respondents were guaranteed full confidentiality. While this rules out any attempt to personally identify the respondents (see also below), it considerably strengthens the integrity and reliability of the dataset (Kertzer and Renshon 2022). Respondent confidentiality is indeed well known to mitigate incentives to obfuscate or mischaracterize attitudes and self-reported behavior—even in response to highly sensitive questions (Cohen and Cassell 2023; Ong and Weiss 2000; Robertson et al. 2018).

Our second data source yields information on the political party in power in US states over the period of 1964–2008. We collected information on the partisan affiliation of the state governor (Democrat, Republican, or other), as well as the seat distribution in the state’s lower and upper legislative chambers.<sup>1</sup> The latter allows

<sup>1</sup> As Nebraska has a unicameral legislature, we follow Anzia and Jackman (2013) in including it as an upper chamber. Nebraska’s formally non-partisan legislature is also coded as neither Democratic nor Republican majority. As mentioned below, dropping Nebraska—or any other state—does not affect our inferences.

us to define variables indicating whether there is a Democratic or Republican majority in the lower and upper chambers, as well as the partisanship of the state legislature as a whole (i.e., Democratic majority, Republican majority, or a split branch legislature). Both data sources are integrated based on the ASAP survey year and US state where a respondent is employed.

Each ASAP survey constitutes an independent cross-sectional study, and there are no individual identifiers to link respondents across waves (Yackee and Yackee 2021). To innovate with these repeated cross-sectional data, we employ an empirical strategy pioneered by Geys (2023) and Murdoch et al. (2019). It allows us to link ASAP respondents across two consecutive survey waves when they have the same sex, race, US state of birth, US state of employment, and highest education level, as well as reporting the same education and occupation for both parents. We additionally verify that these respondents' age and length of employment increase with the correct number of years between consecutive waves (i.e., four or six years) and that their religion, career history, training and work experience, and agency characteristics are consistent across both survey waves (whenever available). Although two ASAP respondents may share several of these background characteristics, the probability of sharing *all of them simultaneously* is vanishingly small.

We extensively validate our data to ensure that we are correctly retrieving panel respondents. We first do so by estimating the number of false positives (i.e., incorrectly designating two distinct respondents as the same "individual") and false negatives (i.e., failing to link the same individual across survey waves) likely to arise via a simulation exercise using a setting with realistic data characteristics. We thereby simulate 1,000 datasets that replicate the main characteristics of the ASAP dataset in terms of the number, detail, and intercorrelation of included variables (full procedural details in Supplementary Material B), and we apply Geys' (2023) methodology to these computer-generated datasets. Our simulations show that the high level of detail available in the ASAP surveys allows us to correctly identify more than 99% of repeat respondents (i.e., there are very few false negatives). We can also expect to match respondents inaccurately very rarely across surveys (i.e., fewer than 1% false positives) (see Supplementary Figure B.1). Even so, all potential matches uncovered across consecutive ASAP surveys were verified by two independent researchers and retained in the final sample only when both agreed. As a result, we can be confident that the ASAP respondents matched across two consecutive survey waves are the same individual.<sup>2</sup>

The procedure reveals 951 unique respondents that appear in more than one ASAP survey (9.1% of the

10,399 unique respondents in the ASAP database). Their 2,081 survey responses form the dataset for our analysis, and Supplementary Table A.1 presents their descriptive statistics.<sup>3</sup> The final column in this table presents the p-value of formal balancing tests evaluating how our sample relates to the set of respondents appearing only once in the ASAP dataset. The results indicate mostly small differences, even though many are statistically significant at conventional levels. This suggests that entry into our panel dataset is not a random event, limiting the potential to generalize any findings to the general population of state agency leaders. Nonetheless, high internal validity—which is critical for hypothesis-testing—can still be achieved when a (quasi-)natural experiment divides the respondent sample into treated and untreated groups in a random manner (Geys et al. 2020; Jilke, Van de Walle, and Kim 2016). In our setting, this requires that there should exist no significant differences between the state agency leaders who are "treated" and "untreated" with partisan turnover of their elected principals. Supplementary Table A.2 illustrates this is the case—with the sole exception of respondents' state of employment. Yet, this simply reflects that certain US states are more likely to witness political turnover (i.e., swing states) relative to others (i.e., safe states), and we return to it below.

## EMPIRICAL ANALYSIS

### Model Specification

To evaluate whether, when, and how bureaucratic elites adjust their partisan leaning following a partisan change in their elected principals, we estimate the following first-difference regression model (with subscript  $i$  for individuals):

$$\Delta PartyID_i = \alpha + \beta_1 \Delta GovParty_i + \varepsilon_i \quad (1)$$

Our *dependent variable*— $\Delta PartyID_i$ —captures the change in respondents' self-reported partisan leaning from one survey wave to the next. It is based on the question: "Generally speaking, do you consider yourself to be a Democrat, a Republican, or an Independent? If independent, which party are you closer to?"<sup>4</sup> We use this question to derive two closely related variables. *Shift Party ID* exploits only the three main

<sup>3</sup> Most matched respondents answer only two (85.5%) or three (11.2%) consecutive ASAP survey waves, while the maximum is six survey waves (one respondent).

<sup>4</sup> ASAP survey protocols prohibit publicly revealing information shared by respondents. This promise of confidentiality appears to have been highly credible, since very few respondents decline to state their partisan self-identification despite the potentially sensitive nature of this question (1.7% in our sample; 3.7% in the full ASAP dataset). This increases our confidence that we capture bureaucratic elites' sincere responses at the time of survey implementation. Note that the partisan leaning of Independents was asked starting with the 1974 survey wave.

<sup>2</sup> We follow all ethical guidelines set out in Geys (2023), which are compatible with the General Data Protection Regulation (GDPR) as well as APSA's Principles and Guidance for Human Subjects Research. While we identify specific background *profiles*, we do *not* personally identify individual respondents (which would be ethically and legally unacceptable).

answer categories (coded 1 for Democrat, 2 for Independent, and 3 for Republican) and therefore ranges from -2 (move to the left from Republican to Democrat) to 2 (move to the right from Democrat to Republican). *Shift Party ID (detailed)* considers the partisan leaning of Independents using a five-point scale with 1 for Democrat, 2 for Independent leaning Democrat, 3 for Independent, 4 for Independent leaning Republican, and 5 for Republican. It therefore ranges from -4 (move to the left from Republican to Democrat) to 4 (move to the right from Democrat to Republican). In both cases, respondents who do not change their partisan leaning receive value 0. Given the ordinal character of these variables, we estimate Equation 1 using ordered logistic regressions.

Supplementary Figure A.1 illustrates the magnitude of individual-level shifts in self-reported partisan leaning over time. In line with Nixon's (2023) recent work on the dynamics of bureaucratic preferences, we find that stability is the norm. Most respondents (83.6%) do *not* change between survey waves when looking only at the three main answer options. Yet, 14.5% change from either Democrat or Republican to Independent, or from Independent to Democrat or Republican. The remaining 1.9% shifts between the two major parties (distributed equally across the 45-year period). When including more fine-grained information about Independents, 69.6% claim the same partisan identification in both survey waves. In this case, a considerable number of shifts occur within the Independent category as well as in both directions between Democrat/Republican and Independent (28.5%).<sup>5</sup>

The main *independent variable*— $\Delta GovParty_i$ —captures a change in the party in power from one survey wave to the next. We operationalize this in two ways. First, we look at the partisan affiliation of the state governor. As we differentiate between Democratic (coded 1), Independent (coded 2), and Republican (coded 3) governors, this variable ranges from -2 to 2 in the same way as *Shift Party ID*.<sup>6</sup> Second, an alternative operationalization looks at changes in the distribution of power within the state legislature (i.e., lower and upper chambers). Across the 45-year period of our analysis, every US state changes the partisan affiliation of its governor and state legislature at least once. Since such changes derive from state-wide elections, they are exogenous to the bureaucratic elites under analysis. This allows us to identify how the *same* US agency leaders respond to partisan turnover of their elected principals.<sup>7</sup>

For our evaluation of H2, we exploit the diversity of appointment procedures for agency leaders within and across US states (see Table 1). We expect  $\beta_1$  to be larger when the governor has direct influence over the appointment of a state agency leader (i.e., top three rows of Table 1) compared to the situation where appointments do not require gubernatorial consent (i.e., bottom four rows of Table 1). For H3, we exploit information from the ASAP surveys about the (self-reported) contact pattern of state agency leaders. The question is: “*On average, how often do you personally have phone or face-to-face contacts with the following persons during the course of carrying out your official duties?*” and respondents answer on a five-point scale (1 daily; 2 weekly; 3 monthly; 4 less than monthly; 5 never). Following Geuijen et al. (2008) and Vantaggiato et al. (2024), we compare those reporting at least monthly contacts with the governor and governor's staff versus those reporting less than monthly contacts. We do the same for contacts with state legislators and their staff. We measure this within the second survey observation to ensure the contact pattern relates to the elected principal *after* any shift in power, and we expect  $\beta_1$  to be larger for those with more frequent contacts. While we split the sample using these moderating variables in our main analysis (thus estimating Equation 1 separately using the respective subsamples), we show that our findings are qualitatively similar when using encompassing models with fully specified interaction terms.

## Causal Identification

Using a (quasi-)experimental design based on exogenous election-induced political turnover allows us to take advantage of the longitudinal dimension of the ASAP data to explore dynamic relationships at the individual level. The first-difference regression models also mitigate concerns regarding omitted variable bias by controlling for all time-invariant aspects of respondents—such as innate personality characteristics, education level, or US states of birth and employment (Wooldridge 2010).

That said, causal interpretation of  $\beta_1$  requires that agency leaders exposed and not exposed to political turnover are not developing differently in terms of partisan identification *prior* to a turnover “treatment” (the parallel pre-trends assumption). We assess this in two ways. First, although we usually only observe the same individual for two consecutive ASAP waves (85.5% of our sample), a small subset can be followed for three or more waves ( $N = 138$  individuals). Hence, we can assess how these respondents' partisan identification develops *prior* to a shift in their agency's political principals. Specifically, we extend Equation 1 with a forward lag of  $\Delta GovParty_i$  and expect its coefficient estimate to be 0 (Schmidheiny and Siegloch 2023). Supplementary Table A.3 shows that this is the case.

<sup>5</sup> These numbers are similar to those observed in panel datasets of American voters (Green and Platzman 2024).

<sup>6</sup> Since there are very few Independent state governors (0.56% of the sample), we exclude them throughout the analysis. Nonetheless, including them does not affect any of our inferences.

<sup>7</sup> All models include a full set of survey wave fixed effects to accommodate potential temporal confounders. We also cluster standard errors at the individual level, which accommodates that our dataset includes multiple observations for the same individuals. Nonetheless,

all inferences are unaffected when we cluster standard errors at the US state level instead (where shifts in the party in power take place).

While there is a significant effect of *contemporaneous* changes in the governor's partisan affiliation (we return to this key finding below), the coefficient of changes four/six years *ahead in time* is close to zero and statistically insignificant. Second, since the test above is based on a small subsample, we also investigate historical developments in political turnover in Supplementary Figure A.2. This again returns evidence against pre-trend concerns.

## Main Findings

Table 2 contains our main findings. Panel I reports results using *Shift Party ID* as the dependent variable (which treats Independents as a unified group), whereas Panel II allows Independents to have a partisan leaning by using *Shift Party ID (detailed)* as the dependent variable. Columns (1) to (3) look at the effect of changes in the governor's partisan affiliation. Column (1) includes the full sample of panel respondents, while column (2) excludes respondents whose party identification moved from Democrat to Republican or vice versa between two survey waves ( $N = 21$  individuals). Column (3) excludes all respondents who self-report as Independent at the initial point of observation, which implies a focus on shifts from Democrat/Republican partisanship toward Independent status. Finally, column (4) turns to shifts in the balance of

power in the state legislature and again uses the full sample of panel respondents.

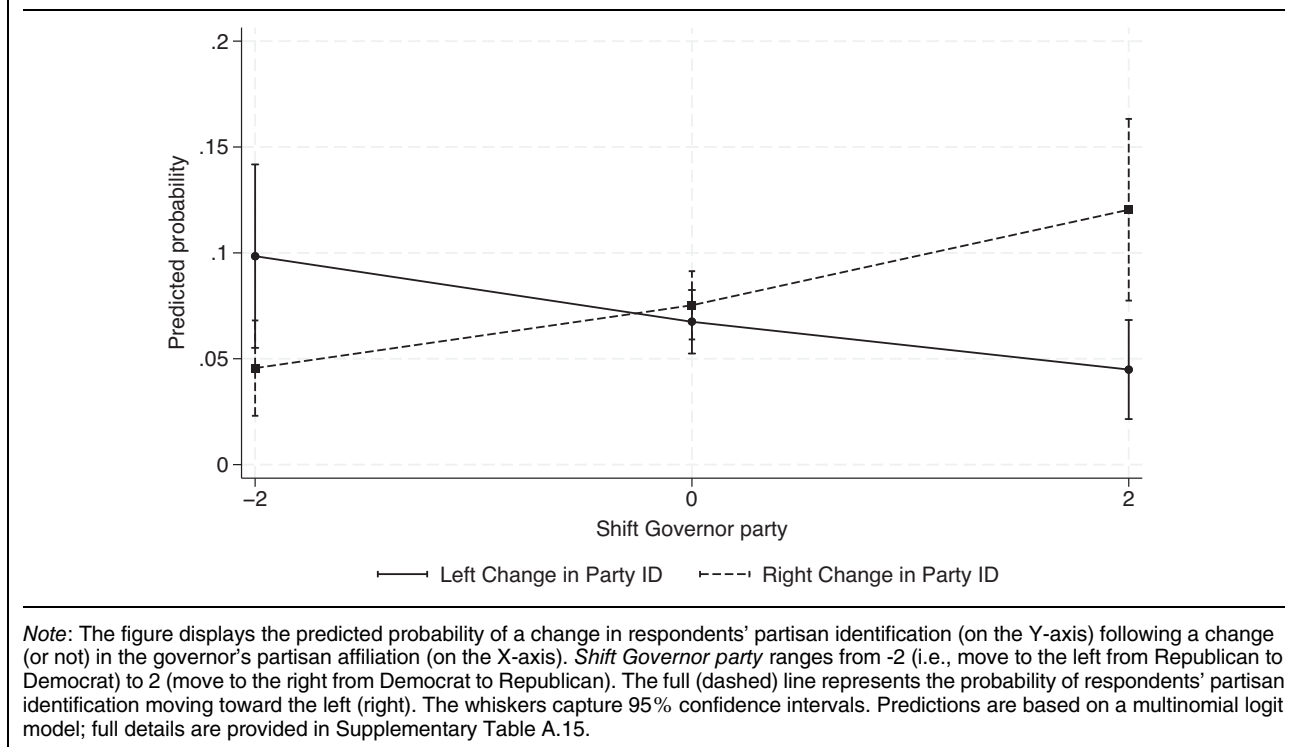
All point estimates in Table 2 are positive and statistically significant at the 90% confidence level or better. A positive point estimate implies that US state agency leaders who remain in office after a shift in the party in power on average adjust their self-reported partisan leaning in line with their new political principals—meaning toward the left (right) if power shifts away from a Republican (Democratic) governor/legislature. These findings are robust to excluding the few respondents whose party identification moved from Democrat to Republican or vice versa (column 2). Interestingly, our point estimates marginally *increase* when excluding respondents who self-identify as Independent at the initial point of observation (column 3). This suggests that our findings may be predominantly driven by individuals who shift from overt Democrat/Republican partisanship toward Independent status.

Since our data contain a subset of agency leaders who respond to more than two ASAP waves (see note 3), one might worry that their repeated responses are driving our results. This is *not* the case. Auxiliary results show that including only the first two waves of each respondent provides similar results (Supplementary Table A.4). We also verified that our results are robust when excluding observations with six-year gaps

**TABLE 2. Main Results on Shifts in (Self-Reported) Partisan Identification**

	All respondents (Governor shift) (1)	Excluding cross-party shifts (2)	Excluding Independents (3)	All respondents (Legislature shift) (4)
<i>Panel I: Dependent variable "Shift Party ID"</i>				
Shift Governor party	0.234*** (0.075)	0.245*** (0.083)	0.255*** (0.100)	–
Shift legislative party in power	–	–	–	0.257* (0.152)
N (observations)	1,068	1,048	705	934
N (individuals)	910	891	612	794
<i>Panel II: Dependent variable "Shift Party ID (detailed)"</i>				
Shift Governor party	0.208*** (0.069)	0.215*** (0.073)	0.241** (0.117)	–
Shift legislative party in power	–	–	–	0.235* (0.132)
N (observations)	826	811	538	835
N (individuals)	705	691	463	711

*Note:* The table reports coefficient estimates obtained from ordered logistic regression models. The dependent variable in Panel I is *Shift Party ID*, which represents the shift in respondents' partisan identification between two consecutive survey waves. It is derived from partisanship variables coded 1 for Democrat, 2 for Independent, and 3 for Republican and therefore ranges from -2 (i.e., move to the left from Republican to Democrat) to 2 (move to the right from Democrat to Republican). The dependent variable in Panel II is *Shift Party ID (detailed)*, which is derived from partisanship variables coded 1 for Democrat, 2 for Independent leaning Democrat, 3 for Independent, 4 for Independent leaning Republican, and 5 for Republican. It thus ranges from -4 (i.e., move to the left from Republican to Democrat) to 4 (move to the right from Democrat to Republican). The independent variable *Shift Governor party* has the same construction and range as *Shift Party ID*. Since there are only very few Independent state governors (0.56% of the sample), these are excluded throughout the analysis. The independent variable *Shift legislative party in power* refers to the party holding the majority in the state legislature (coded 1 for Democrat, 2 for a hung legislature, and 3 for Republican). All models include a full set of survey wave fixed effects. Standard errors clustered at the individual level are shown in parentheses. Variations in N are due to differences in the number of missing values across variables. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

**FIGURE 1. Predicted Probabilities from Multinomial Logit Models**

between survey waves (Supplementary Table A.5). This restriction avoids potential complications from multiple gubernatorial elections between subsequent survey waves, as well as the increased difficulty in identifying panel respondents when the time between survey waves increases (Geys 2023). Qualitatively similar results furthermore arise when looking at the upper and lower chambers of the state legislature separately—albeit with substantively and statistically stronger effects for partisan turnover in the lower chamber (column [1]–[2] in Supplementary Table A.6).

As the point estimates in Table 2 are hard to interpret, we re-ran the model in column (2) of Panel I using a multinomial logit model in order to calculate predicted probabilities for shifts in surviving agency leaders' partisan self-identification (full details in Supplementary Table A.15). Figure 1 displays these predicted probabilities on the y-axis (with 95% confidence intervals) depending on the change (or not) in the governor's partisan affiliation (on the x-axis). This offers a better sense of the estimated effect sizes. The full line represents the probability of respondents' partisan identification moving toward the left, while the dashed line gives the probability of a move toward the right. Figure 1 shows that in the absence of a shift in the governor's partisan affiliation (value 0 on the x-axis), agency leaders who remain in their position are equally likely to shift their partisan identification toward the left (6.7%) or right (7.5%). However, when the governor shifts toward the left, the probability that survivors also shift to the left increases to 9.8%, while the probability that they shift toward the right drops to

4.6% (difference statistically significant at  $p = 0.08$ ). Similarly, a shift in the governor's party toward the right increases the probability that surviving agency leaders shift to the right (12.0%) and decreases shifts toward the left (4.5%) (difference statistically significant at  $p = 0.01$ ).<sup>8</sup>

Overall, the results in Table 2 and Figure 1 indicate that surviving state agency leaders become notably more likely to alter their partisan identification when the party in power moves away from them between survey waves. The broadly symmetric nature of the observed shifts suggests that there is *no* partisan bias in these adaptive adjustments. This is confirmed in Supplementary Table A.7. The absolute value of the point estimates for leftward and rightward shifts are never statistically significantly distinct at conventional levels, which also validates our linear specification in Equation 1 and Table 2.

Taken together, our results thus far provide convincing support for H1 and strongly suggest a malleability of partisanship that is not often attributed to bureaucratic elites. Even so, one might wonder how deeply this malleability extends and whether it also becomes reflected in a broader attitudinal (or even behavioral) transformation. Thus, Supplementary Table A.8 extends our analysis using shifts in respondents' placement on a seven-point liberal-conservative scale as a

<sup>8</sup> Not shown in Figure 1, the probability that agency leaders do *not* change their partisan identification remains stable at 83–86% independent of any shift in the governor party.



dependent variable.<sup>9</sup> The results indicate that surviving agency leaders' liberal/conservative self-placement shifts to the right (left) when the governor shifts to the right (left), much like we observe for their partisan self-identification. Although these movements are weaker than those in partisanship—as would be expected given the smaller sample size and the fact that party identification and ideology are not identical constructs (Shor and McCarty 2011)—the pattern of these auxiliary results is consistent with our main findings. They also suggest that the partisan malleability observed in Table 2 and Figure 1 may extend to ideological change over time, although this would require further analysis in future research.

In an ideal world, we would also extend our analyses to behavioral change. Yet, we know of no dataset that provides that type of information at the individual level across all states and across time.<sup>10</sup> Still, we were able to verify that our main results on partisan self-identification are not picking up a general pattern in the data. To confirm this, as a placebo check, we analyzed more objective questions about respondents' weekly hours worked and yearly salary. The results in Supplementary Table A.9 confirm that responses to these variables are unaffected by shifts in the party in power, as would be expected.

We should reiterate that our results apply to respondents retaining their position after political turnover (otherwise they drop out of the ASAP surveys, and hence, our sample). This is important since less malleable individuals could be more likely to leave the organization (either voluntarily or by force). The ASAP data show that the share of state agency leader "survivors" lies around 58%.<sup>11</sup> Hence, our analysis is germane to most state agency leaders (even though we

caution against generalizing our findings to the remaining 42% of state agency leaders, who, as we suggest above, might hold different preferences and levels of malleability).

## Heterogeneous Effects

In Table 3, we investigate the hypothesized moderating roles of bureaucrats' appointment process (H2) and contact patterns (H3). Columns (1) and (2) differentiate between respondents whose appointment was with or without explicit governor consent (see Table 1). Columns (3) and (4) differentiate between respondents based on their contact frequency with the governor and/or their staff at the current point of measurement (i.e., at least monthly contact versus less than monthly), while columns (5) and (6) do the same for contacts with the state legislature and/or their staff. Panels I and II have *Shift Party ID* and *Shift Party ID (detailed)*, respectively, as dependent variable. In all cases, we use the full sample of respondents.

The results in Table 3 indicate that our point estimates are both statistically and substantively stronger when looking at the subset of respondents directly appointed by, or having more frequent contacts with, their elected principals.<sup>12</sup> Interestingly, looking separately at those individuals appointed using "other" procedures (N = 132 individuals)—which predominantly captures appointments under civil service (merit) procedures—we find a non-significant point estimate that is even slightly negative ( $\beta_1 = -0.032 / -0.097$ ;  $p > 0.10$ ). As would be expected, civil service appointments benefit the political independence of agency leaders, while increased political influence over appointments induces stronger adjustments to shifts in the party in power. The findings in Table 3 thus are consistent with H2 and H3. Any adaptive adjustments in surviving agency leaders' partisan allegiance following a change in the party in power are most prominent among those more closely intertwined with the world of politicians.

Beyond our hypothesized effects, one might also wonder whether the observed adaptive adjustments in self-reported partisanship are more common when there is more elite party polarization at the state level. Such polarization might raise the costs of an agency leader's failing to adjust to a political turnover event. We investigate this proposition with two distinct data sources. First, using the previously described ASAP question tapping respondent ideology on a 7-point liberal-conservative scale, we identify the mean placement by party, state, and year for all ASAP respondents for the available 1994–2008 period. We then calculate the "ideological distance" between respondents from both major parties by state and year as a

<sup>9</sup> The question wording changed slightly over time. In 1994, respondents were asked: "On a scale of 1 to 7, with 1 being very conservative and 7 being very liberal, do you generally consider yourself to be more conservative or more liberal?" In 1998–2008, the question was: "On a scale of 1 to 7, with 1 being very conservative and 7 being very liberal, please indicate how you rate or rank yourself on (a) social and morality issues, and (b) taxing and spending issues." We combined both questions into an overall measure of liberal-conservative placement to maximize the number of years available for analysis (1994–2008).

<sup>10</sup> One exception might have been the *Database on Ideology, Money in Politics, and Elections* (DIME) (Bonica 2014; 2019). Unfortunately, the credible identification of state agency leaders in the DIME dataset is difficult and error prone, which precluded its use.

<sup>11</sup> This is calculated based on agency leaders' years of service (as reported in the ASAP) and the number of years since the last partisan change in the state's governor. Hence, it reflects the share of ASAP respondents "surviving" at least one governor party change during their tenure. Naturally, this number is higher among those whose appointment was without (65%) rather than with (50%) explicit gubernatorial consent (cf. Table 1). Note also that a vibrant literature on the pull and push factors behind public sector employee turnover (intentions) highlights a complicated mix of political, personal, organizational, or market-driven factors (Hur and Abner 2024; Sowa 2021). While political factors—such as partisan turnover, (lack of) policy influence, or democratic backsliding—are often stressed as important determinants (e.g., Alan-Barkat et al. 2024; Bauer 2023; Dahlström and Holmgren 2019; Doherty, Lewis, and Limbocker 2019), our data do not indicate why state agency leaders (did not) leave their office.

<sup>12</sup> These two groups show considerable overlap (pairwise correlation  $r = 0.336$ ). Yet, estimating an encompassing model with *both* appointments and contacts highlights that both are important sources of heterogeneity in their own right (Supplementary Table A.10). Note also that separating frequent contacts between those with the governor/legislature or those with their staff does not affect our findings (Supplementary Table A.11).

**TABLE 3. Heterogeneity by Nature of Appointment and Contact Frequency**

	Appointment with governor consent (1)	Appointment without governor consent (2)	High contact frequency with governor office (3)	Low contact frequency with governor office (4)	High contact frequency with legislature office (5)	Low contact frequency with legislature office (6)
<i>Panel I: Dependent variable "Shift Party ID"</i>						
Shift Governor party	0.395*** (0.140)	0.172* (0.089)	0.341*** (0.136)	0.158* (0.093)	–	–
Shift legislative party in power	–	–	–	–	0.394** (0.178)	0.168 (0.258)
N (observations)	386	637	440	480	608	311
N (individuals)	348	538	399	415	530	278
<i>Panel II: Dependent variable "Shift Party ID (detailed)"</i>						
Shift Governor party	0.419*** (0.160)	0.153** (0.077)	0.304*** (0.115)	0.128 (0.086)	–	–
Shift legislative party in power	–	–	–	–	0.263 (0.167)	0.185 (0.222)
N (observations)	277	524	380	433	534	288
N (individuals)	250	445	345	374	465	256

*Note:* The table reports coefficient estimates obtained from ordered logistic regression models. For details on the dependent and independent variables, see note to Table 2. Columns (1) and (2) differentiate between respondents whose appointment was with or without governor consent. Columns (3)–(6) differentiate between respondents based on their contact frequency with the governor/legislature and/or their staff at the current point of measurement (i.e., at least monthly contact versus less than monthly). All models include a full set of survey wave fixed effects. Standard errors clustered at the individual level are shown in parentheses. Variations in N are due to differences in the number of missing values across variables. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ .

proxy for polarization. As we show in Supplementary Figure A.3, agency leaders' adaptive adjustments are indeed more common when there is greater polarization. We return the same conclusion when instead using a state's elected Members of Congress' NOMINATE scores (Lewis et al. 2024) to measure the ideological distance between a state's political elites. Those results are reported in Supplementary Figure A.4 (full details in Supplementary Table A.16).

Finally, we also considered whether bureaucratic elites' partisan malleability depends on their task portfolio or on the identifiability of their partisan leaning from participation in primaries or general elections. The former reflects the organization theory perspective that task portfolios shape bureaucratic elites' actions and political exposure (Ban, Park, and You 2023; Christensen and Lægveid 2013; Egeberg 1999). The latter might create differences in the perceived visibility and relevance of partisan identification. Supplementary Figure A.5 shows that respondents more heavily involved in policy development appear somewhat more likely to reorient their partisan leaning toward that of their elected principals after political turnover, although these findings are not very robust across estimations (full details in Supplementary Table A.16). Supplementary Table A.12 reveals that our findings are strongest in states where partisan orientation is identifiable from voters' electoral activity, consistent with the notion that this might make bureaucratic elites more sensitive to any partisan (non-)alignment with their political principals.

## Robustness and Validity Checks

In this section, we briefly discuss the results of several robustness and validity checks. For brevity, we relegate detailed results to the Supplementary Material.

Starting with the dependent variable, our central line of argument implies that agency leaders wish to mitigate political misalignment with their elected principals. In the ASAP surveys, this could be reflected in respondents failing to answer the question on partisan identification. Despite a non-response rate below 2%, we specified a variable equal to 1 whenever a respondent provided a partisan identification in one survey wave but *not* in the subsequent wave (0 otherwise). This captures individuals opting to self-censor a previously revealed party preference. We then assess whether such self-censoring varies depending on respondents' (mis)alignment with the state governor in the previous survey wave ("Aligned/Misaligned wave 1") and whether there was a change in the party of the governor ("Shift/No shift in governor party"). The results are reported in Supplementary Table A.13. While a joint test for differences across all four groups fails to reach statistical significance at conventional levels, the pattern of these findings corresponds to theoretical expectations. Specifically, individuals aligned during their first survey wave who witness a change in governor (and thus become misaligned) are most likely to stop reporting a party preference (2.78%). Individuals aligned during their first survey

wave who do not witness a change in governor (and thus remain aligned) are least likely to start censoring their party preference (0.56%).<sup>13</sup>

Turning to our independent variable, one might worry that agency leaders are responding to more general ideological shifts in the country or their state—rather than shifts in their direct political principal(s). To address this, we implement two auxiliary checks. The first defines shifts in the party in power using the partisan affiliation of the US *president* (coded in the same way as  $\Delta GovParty_i$ ). The second looks at the balance of *voter* support at the state level between the two main parties in the presidential election nearest to an ASAP survey wave (i.e., Republican minus Democratic vote share). Columns (3)–(5) of Supplementary Table A.6 show at best weak effects for these variables when used independently as explanatory variables, while only  $\Delta GovParty_i$  provides significant explanatory power in a “horserace” specification including all three types of partisan shifts. Taken together, this strengthens our inference that political turnover at the state level drives the effects observed in Table 2 and Figure 1, rather than general ideological shifts in the country or the state-level population.

Next, despite the extensive validity checks on our panel dataset, one might still be concerned about the influence of false positives on our results. Since false positives by construction ascribe changes in partisan identification to observations where no unique individual (and, hence, no change) exists, we can assess their influence via a counterfactual exercise using only the non-panel respondents in the ASAP dataset ( $N = 9,448$ ). That is, we can make all of these into “false positives” by randomly assigning them a (counterfactual) change in partisan identification that follows the distribution of such changes observed in our panel dataset (see the left-hand panel of Supplementary Figure A.1).<sup>14</sup> Repeating this exercise 1,000 times, and subsequently estimating the model in column 2 of Table 2, leads to the results provided in Supplementary Figure A.6. The left- and right-hand panels show the distribution of the main point estimate

( $\beta_1$ ) and its associated t-statistic, respectively. For comparison, the vertical lines represent the result in column 2 of Table 2. Crucially, the distribution of point estimates in these counterfactual exercises is centered around zero and far from our actual estimate. This highlights that our main findings would be extremely unlikely to arise as a statistical fluke in a dataset dominated by false positives (or measurement error in partisan self-identification across survey waves). It furthermore suggests that any bias due to false positives and measurement error would most likely bias  $\beta_1$  toward zero.

As mentioned, generalizing our findings beyond the set of “survivors” that make up the majority of state agency leaders should be treated with extreme caution. Even so, we can assess econometrically how our estimates would be affected by the inclusion of “non-survivors.” That is, we can estimate Equation 1 on a dataset extended with  $x\%$  non-panel respondents (with  $x = 10, 20, 30, 40$ ) for whom the dependent variable  $\Delta PartyID_i$  is set to 0. This imposes a “worst-case scenario” with *no* partisan adaptive adjustment among non-survivors. We replicate this exercise 1,000 times for each value of  $x$  and report the resulting distribution of our main parameter of interest (i.e.,  $\beta_1$ ) in Supplementary Figure A.7. As expected, our point estimates decline. Yet, crucially, our key results persist: the point estimate of  $\beta_1$  remains consistently positive and statistically significant at conventional levels, even when adding 40% “non-survivors.”

Finally, some US states are prone to more regular political turnover than others (i.e., swing states versus safe states). One might worry that this varying baseline probability for political turnover could bias our findings. That is, while (the timing of) each gubernatorial/legislative shift is exogenous to our respondents, the different probability of being exposed to them may influence whether and how they respond to such shifts. We deal with this in two ways. First, we replicate the analysis 50 times excluding each US state one by one. The results in the top panel of Supplementary Figure A.8 show that this leaves our results unaffected, such that no one state is driving our findings (the same is true for any individual survey wave; see the bottom panel of Supplementary Figure A.8). Second, we count the number of shifts in the partisan affiliation of the governor in each US state since the end of World War II. Higher numbers reflect a less stable political environment, which may affect responsiveness to specific instances of political turnover (e.g., in the expectation that these will soon be reversed). Supplementary Table A.14 indicates that including these additional controls leaves the point estimates and statistical significance of our main variable of interest unchanged.

## CONCLUSION

The partisanship of public bureaucracies and/or agency leaders is a major topic of recent scholarly inquiry and holds key implications for our understanding of the

<sup>13</sup> We also assess whether respondents adjust their partisan alignment again after witnessing a second, opposing shift in the party in power. Unfortunately, our dataset includes only 28 respondents witnessing such multiple partisan turnovers. Interestingly, however, we observe three cases (or 11%) where respondents who shift their partisan identification in line with a first change in the governor's party subsequently move back toward their original partisan self-identification after a second, opposing change in the governor's party. While consistent with our main findings and argument, we are apprehensive about putting too much weight on this finding given the very small sample size.

<sup>14</sup> Any measurement error in respondents' partisan self-identification across survey waves would likewise imply our data may document shifts where no “true” change exists (e.g., Green and Platzman 2024). Even though this type of measurement error appears less likely among bureaucratic elites than the general population (see footnote 4), this counterfactual exercise also addresses the potential impact of any such measurement error on our main findings. Note furthermore that false negatives are arguably less problematic in our setting, since they mainly reduce our sample size and, therefore, statistical power (Geys 2023).

relationship between politics and administration (Bertelli and Grose 2011; Clinton et al. 2012; Clinton and Lewis 2008; Nixon 2004; 2023; Richardson, Clinton, and Lewis 2018). Research also illustrates that an agency's partisanship may change over time as a result of turnover among its staff (Bolton, de Figueiredo, and Lewis 2021; Dahlström and Holmgren 2019; 2023; Doherty, Lewis, and Limbocker 2019). Nevertheless, no scholarship to our knowledge contemplates the fact that bureaucratic elites' partisan identification may itself be malleable and change due to a dynamic process of adaptive adjustment to shifts in their immediate political environment. In this article, we propose and test just this type of adaptation.

Methodologically, our article contributes to extant research by applying a novel tool to extract a panel dataset from repeated cross-sectional surveys (Geys 2023). This innovative approach allows us to take advantage of a quasi-experimental longitudinal research design that strengthens causal inferences. We can draw three main conclusions from our analysis, which are applicable to the large share of state agency leaders (i.e., approximately 60%) retaining their positions after a change in the party in power. First, we find that, on average, such "surviving" US state agency leaders significantly reorient their partisan identity following political leadership changes. Second, we demonstrate that the direction of this adaptive adjustment is *toward* the party of incoming political leaders. Third, we uncover evidence that the prevalence and strength of this partisan malleability is affected by agency leaders' appointment procedure and contact pattern with their political principals.

Taken together, our findings call for a re-evaluation of a tacit assumption present across much of the bureaucratic politics literature that the partisan identity of bureaucratic elites is stable across time (Bendor, Glazer, and Hammond 2001; Fiva et al. 2021; Spenkuch, Teso, and Xu 2023). Our results also contribute to a robust scholarly debate around the degree to which partisan identification may or may not be altered based on short-term political changes (Egan 2020; Fiorina 1981; Key 1966; Lazarsfeld, Berelson, and Gaudet 1944; MacKuen, Erikson, and Stimson 1989). That literature, which operates within the political behavior tradition, focuses on the partisanship of the mass public, whereas we shift focus to bureaucratic elites (Kertzer and Renshon 2022).

Clearly, however, future research is needed to link the malleability explored in this article to an agency leader's (potential) behavioral transformation, as well as to her/his agency's policy decisions. Existing research suggests an overlap between citizens' self-identified party identification and their preferences on numerous public policy topics—especially those that are more closely "branded" to one party or the other (Dias and Lelkes 2021; Orr, Fowler, and Huber 2023). Future work might thus exploit variation in such "issue ownership" (Geys 2012; Petrocik 1996) to investigate the connection between self-professed party identification and the actual policy decisions made by bureaucratic elites. This would also allow novel insights into

whether and when bureaucratic elites merely "act in accordance with [perceived] expectations" rather than truly internalizing their new principals' values (Beyers 2010; Checkel 2005, 804; Murdoch et al. 2019). From an organization theory perspective, distinguishing both possibilities is critical to understand when and how organizational design choices might be used to "guide" bureaucratic elites' adaptive adjustments (Murdoch 2015; Olsen 2010).

Our dataset concentrates exclusively on the US state agency leaders who stay in their position after a political turnover event, and our conclusions are circumscribed to those individuals. We acknowledge that this prevents us from generalizing our argument and findings to the entire population of US state agency leaders. Yet, crucially, it does *not* undermine our main insight, which is that state agency leader "survivors" on average display a meaningful tendency to reorient their own partisan identity toward that of their political principals. This implies that a malleability of partisanship may be an important "coping strategy" for public employees after political turnover (Bolton, de Figueiredo, and Lewis 2021; Golden 2000). Our findings also imply the need for future research to investigate malleability among federal political appointees and civil servants, who differ in important ways from their state leader counterparts. In the end, some may view our findings as a normative challenge to public governance because they imply that the advice and expertise of select agency leaders may be tainted by partisan pressures. Yet, others may see our results as normatively positive in that they suggest a bureaucratic responsiveness that has been previously underappreciated by scholars and observers alike. Given these divergent views, we see this topic as a fruitful avenue for future data collection, analysis, and inquiry in the US setting and beyond.

## SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <http://doi.org/10.1017/S0003055424000844>.

## DATA AVAILABILITY STATEMENT

Limitations on data availability and explanation of the requirements to access the data used in this article are provided in the Supplementary Materials. Codes to replicate the findings of this study are openly available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/OO6RB0>.

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## AUTHOR CONTRIBUTION

All authors contributed equally to this work.

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## CONFLICT OF INTERESTS

The authors declare no ethical issues or conflicts of interest in this research.

## ETHICAL STANDARDS

The authors declare that they were not directly involved in data collection. Therefore, the authors affirm this research did not involve human participants.

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