

Ideology and Performance in Public Organizations*

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Abstract

We combine personnel records of the United States federal bureaucracy from 1997-2019 with administrative voter registration data to study how ideological alignment between politicians and bureaucrats affects turnover and performance. We document significant partisan cycles and turnover among political appointees. By contrast, we find no political cycles in the civil service. At any point in time a sizable share of bureaucrats is ideologically misaligned with their political leaders. We study the performance implications of this misalignment for the case of procurement officers. Exploiting presidential transitions as a source of “within-bureaucrat” variation in political alignment, we find that procurement contracts overseen by misaligned officers exhibit greater cost overruns and delays. We provide evidence consistent with a general “morale effect,” whereby misaligned bureaucrats are less motivated to pursue the organizational mission. Our results thus help to shed some of the first light on the costs of ideological misalignment within public organizations.

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1 Introduction

Mission-oriented organizations pursue objectives beyond profit maximization. Instead of providing employees with high-powered financial incentives, these organizations tend to attract workers whose own values and preferences are closely aligned with the greater mission (Besley and Ghatak, 2005). In fact, explicit pecuniary incentives may backfire when agents derive intrinsic benefits from furthering an organization’s goal (Bénabou and Tirole, 2003). While mission can act as a powerful intrinsic motivator, it may also create frictions when the preferences of leaders and their subordinates become misaligned.

Frictions of this kind may be particularly relevant in bureaucracies, whose mission can change from one day to the next due to political turnover. When politicians face a large share of subordinates who no longer agree with the new priorities of the organization and whose compensation is not directly tied to performance, their real authority as the principal can be severely limited (Aghion and Tirole, 1997). Anecdotal evidence of ideological mismatch between bureaucrats and politicians abound. For instance, the Trump administration’s decision to roll back environmental regulations was reportedly met with fierce resistance from within the Environmental Protection Agency (EPA), with bureaucrats refusing to comply, undermining directives by leaking confidential information to the press, or deciding to leave the agency (Plumer and Davenport, 2019). Similarly, throughout much of 2020, scientists from the Centers for Disease Control and Prevention (CDC) disagreed sharply with members of the Trump administration over public messaging related to the ongoing COVID-19 pandemic. At the height of these disagreements, Michael Caputo, a political appointee and top spokesperson for the Department of Health and Human Services (HHS), publicly accused the CDC of harboring a “resistance unit” and engaging in “sedition.” Examples like these can be found across the world and in both non-profit and for-profit organizations.

In this paper, we turn to the U.S. federal government to investigate the role of mission alignment within organizations. We examine how the personnel policies and performance of the organization are affected by ideological (mis)alignment between bureaucrats and their political leaders (i.e., agents and their principals). The U.S. federal bureaucracy provides for an almost ideal setting to study these questions. As the executive arm of the federal government, its goal—or mission—is tightly linked to the policy agenda of the White House. At the same time, the vast majority of bureaucrats serve in civil service positions that are, in principle, protected from political interference. Many civil servants have their own preferences and ideological leanings, which may conflict with those of the president. Moreover, the party in power changes repeatedly, generating sharp shifts in the priorities of the organization. As a consequence, to implement an administration’s agenda, politicians and department heads

often need to work with bureaucrats whose personal values are not aligned with the present mission of their department.

Our study draws on a large, novel data set that contains information on the partisan leanings of U.S. bureaucrats. We link personnel records for the near-universe of federal employees between 1997–2019 with administrative data on all registered voters in the United States. By combining both sources of information, we are the first to measure ideology—and thus political alignment—for almost two million individuals throughout nearly the entire federal bureaucracy.

In the first part of the paper, we provide a descriptive analysis of how the careers of federal bureaucrats depend on their ideological alignment with the party in power. We show that the president can and does leverage his limited power over personnel in order to achieve greater ideological alignment with high-ranking bureaucrats. Specifically, we document a great amount of turnover and significant partisan cycles among political appointees. Under a Democratic president, political appointees are 49 p.p. more likely to be fellow Democrats than under a Republican one. For Republican appointees we observe a 46 p.p. increase relative to years in which the president is a Democrat. In sharp contrast, we observe virtually no political cycles in the career civil service. In our data, the share of Democrats remains nearly constant over the entire time period. The share of Republicans exhibits a slight monotonic downward trend, which is offset by a corresponding rise in the fraction of independents. These trends are not affected by the party of the sitting president. Moreover, we find no evidence that political alignment has an impact on civil servants’ career progression. Our descriptive findings, therefore, suggest that the core of the U.S. federal government resembles a Weberian bureaucracy, which is largely protected from political interference. At any given point in time, however, a significant number of rank-and-file bureaucrats are ideologically misaligned with their political superiors.

In the second part of the paper, we provide the first evidence on the performance implications of this kind of misalignment. In light of the insulated nature of the career civil service, it stands to reason that many bureaucrats experience shocks to mission alignment whenever a new president and new political appointees take over from a previous regime. However, constructing performance measures for everyone in the federal bureaucracy with its wide range of occupations is exceedingly difficult. To make progress, we focus on a subset of important bureaucrats who complete comparable tasks with measurable outcomes: procurement officers. Procurement officers play a crucial role both in the selection of buyers and in the monitoring of contracts. Moreover, procurement contracts account for a significant share of the federal budget. In 2019, federal procurement expenditures amounted to about 10% of U.S. gross domestic product ([Schwarzenberg, 2022](#)).

We link data on procurement contracts from the Federal Procurement Data System (FPDS) to our matched personnel and voter registration data. To examine mission-alignment, we exploit the fact that the raw procurement data contain information on the identity of the officers processing particular contracts. This hitherto underutilized feature allows us to assign contracts to more than 15,000 individual procurement officers across nearly all departments of the federal government. We can thus investigate the performance implications of misalignment at the level of the officer that oversees the respective contract. Following the procurement literature, we use cost overruns and delays as contract-level measures of performance (Bajari and Tadelis, 2001; Decarolis et al., 2020b; Kang and Miller, 2020). Our analysis focuses on services and works contracts, which require significant monitoring and exhibit substantial variation in cost overruns and delays.

Relying on “within-officer” variation to compare contract outcomes in years in which the officer is and is not aligned with her political superiors, we find that misalignment increases cost overruns by approximately 1% of initial contract value—about 6% relative to the mean overrun. This result holds even when comparing procurement officers working in the same department and year. We show that the alignment effect is concentrated in high-value contracts with a large expected value and high uncertainty, consistent with a greater importance of officers’ effort in determining the outcome of contracts that are more complex. In addition, our findings are significant around both the Bush-Obama (2001–2017) and the Obama-Trump transition (2009–2019), suggesting that they are not specific to a particular president. Finally, we find that complex contracts overseen by a misaligned officer are also more likely to exhibit delays in their execution.

In the last part of the paper, we attempt to shed light on the mechanisms behind these previously unknown alignment effects. We first evaluate whether the lower performance of misaligned officers may result from differential assignment of officers to tasks. We show that this is unlikely to be the case. Officers’ workload and contract characteristics, such as size, projected duration, or contract complexity, do not significantly covary with political alignment. We also find no evidence that alignment and performance interact in determining officers’ career progression, which suggests that the lower performance of misaligned officers is unlikely to be driven by differential pecuniary incentives. Instead, using data from a large, repeated survey of U.S. federal bureaucrats, we provide evidence that hints at a general “morale effect” of mission-alignment. We examine a host of measures of employees’ perceptions and attitudes towards their workplace, and we show that bureaucrats report exerting more effort and identifying to a greater extent with the mission of their organization when they are politically aligned with the party controlling the White House.

Related Literature. Our findings contribute to three broad literatures. First, our results

are related to a growing literature on bureaucratic turnover, selection, and performance. Prior work has documented different real-world costs due to turnover of bureaucrats (Iyer and Mani, 2011; Akhtari et al., 2020). There is also evidence on how political turnover affects employment outcomes within and selection into the bureaucracy (Colonnelli et al., 2020; Barbosa and Ferreira, 2019; Brassiolo et al., 2020; Fiva et al., 2021; Xu, 2018). It is important to note, however, that extant work focuses on developing countries, where the bureaucracy may be more susceptible to political interference, even if it is nominally insulated. In the context of the U.S., we document the existence *and* absence of political cycles. While politicians can and do use their discretion in hiring to increase ideological alignment at the highest levels of the federal bureaucracy, we find little evidence of political interference in the selection of rank-and-file bureaucrats. While a politically insulated civil service is typically considered a hallmark of good government, we present empirical evidence that it does come with a cost.

More closely related to our work is a small, recent literature in political science and public administration that empirically studies partisanship in the U.S. bureaucracy. One strand of this literature attempts to estimate the degree of ideological proximity between different departments, political appointees, and the president (see, e.g., Nixon, 2004; Bonica et al., 2015; Bertelli and Grose, 2011). Another strand focuses on bureaucratic turnover. Bertelli and Lewis (2020) use data from a survey of federal executives to show that human capital and perceptions of policy influence correlate with bureaucrats’ intentions to leave. Bolton et al. (2020) study turnover in the aftermath of presidential transitions. They present evidence of an increase in turnover among the most senior civil servants in the first year of a new administration, especially in departments whose employees are estimated to have, on average, divergent views from the president. Our study builds and improves upon extant work in two ways. By linking personnel records to administrative voter registration data, we are able to measure ideological alignment and trace its consequences at the *individual* level throughout nearly the entire federal bureaucracy. In other words, our analysis does not have to contend with the limitations of small-scale surveys and estimated agency-level ideological scores. Unlike previous work, we can thus not only ask whether politically aligned individuals are more likely to be hired, promoted, or to exit relative to their misaligned counterparts in other agencies but also *within* the same department. In addition, our individual-level data allow us to ask entirely new questions. By linking individual procurement officers to contract outcomes, we can investigate whether bureaucrats’ ideological alignment is associated with tangible differences in performance.

Second, our results speak to the literature on incentives and mission in public organizations (see, e.g., Dewatripont et al., 1999; Besley and Ghatak, 2005; Ashraf and Bandiera,

2018). A growing body of work provides evidence on the role of pecuniary incentives in motivating bureaucrats (Khan et al., 2016; Bertrand et al., 2019; Khan et al., 2018; Leaver et al., 2020), and on the role of public service motivation in the selection of frontline providers in developing-country settings (Ashraf et al., 2014; Deserranno, 2019; Ashraf et al., 2020; Khan, 2021). We add to this literature by exploiting the sharp shifts in the organizational mission of the U.S. bureaucracy to document how mission-alignment shapes performance. In the polarized American two-party system, differences in partisanship are indicative of diverging attitudes towards policies and the overarching mission of the state. Our findings provide empirical evidence that “mission matters,” even in the context of a textbook bureaucracy in a high-income country.

Third, our results contribute to an important body of work on public procurement. Prior research has examined the role of individual procurement officers in explaining contract performance (Bandiera et al., 2009; Best et al., 2016; Decarolis et al., 2020b), the role of discretion in contracting (Szucs, 2020; Decarolis et al., 2020a; Baltrunaite et al., 2020; Bandiera et al., 2020; Baltrunaite, 2019), as well as the role of competition (Kang and Miller, 2020; Carril et al., 2021). Some of the work in the procurement literature focuses on (political) connections between procurement officers, the ruling party, and sellers. It typically exploits variation *across* organizations to identify potential distortions. By contrast, our focus lies on ideological alignment *within* the same organization. We show that the performance of the same procurement officers changes over time depending on their alignment with the organizational mission. To the best of our knowledge, we are the first to exploit the individual identifiers in the U.S. raw procurement data to relate procurement performance to the identity and characteristics of procurement officers.

2 Data and context

Our analysis combines data on employees of the U.S. federal bureaucracy, information on the partisan affiliation of registered voters, and data on U.S. federal procurement contracts. In this section we describe the sources of these data and how we link them. Additional details are provided in the Online Appendix.

2.1 Federal employment records

Information on employees of the U.S. federal government for the 1973–2019 period come from the Office of Personnel Management (OPM), an independent government agency that manages the civilian workforce. For the period up to 2017, we use data that were made

publicly available by BuzzFeed News, which, in turn, obtained the respective files via a series of Freedom of Information Act (FOIA) requests. We made an additional FOIA request to the OPM in May 2022, extending the coverage of our data to the end of 2019. Since we are constrained in our ability to measure partisanship over time (cf. Section 2.2), we restrict our analysis to 1997–2019. This period is sufficient to study outcomes under four different presidents—two Democrats and two Republicans—and across three presidential transitions.

The OPM data constitute a panel at the employee-by-quarter level, which contains rich information on federal employees and their positions in the government. For instance, we observe the department and bureau associated with a particular position, the location of employment, the employee’s occupation and pay, as well as the full name, education level, and age (expressed in five years intervals).¹

The data come with two caveats. First, they do not include information on the identity of law enforcement officers and employees in certain sensitive departments, such as Defense.² Second, starting in the third quarter of 2014, the data cease to contain unique employee identifiers. To nonetheless be able to track employees over time, we rely on their full name and educational attainment to create identifiers for the last five years of the panel.

The OPM data also include information on the type of appointment to each position. We use this information to divide positions into two broad categories.³ Specifically, we broadly differentiate between positions that are filled by a political appointee, and those in which appointments and removals are formally insulated from political influence. Political appointments are made by the president, or by a department head. These positions belong to one of three categories: Presidential appointments in top executive positions (with or without Senate confirmation), politically appointed members of the Senior Executive Service (SES), and Schedule C appointees. The first category includes the highest level officers in the U.S. federal bureaucracy, such as cabinet secretaries and their immediate subordinates, as well as heads of government departments and employees in the Executive Office of the President (Davis and Greene, 2017). The second category—politically appointed member of the SES—includes executive positions just below the top presidential appointees. While most SES employees are selected by departments through meritocratic procedures, up to 10% of them can be politically appointed government-wide (Shimabukuro and Staman, 2019).⁴ The third category—Schedule C appointees—comprises positions with a confidential

¹Throughout the paper, we refer to both federal departments and independent agencies as *departments*, while we refer to sub-units of departments or independent agencies as *bureaus*.

²In some cases the departments are not included in the data, while in other cases the names of the employees are redacted. See Online Appendix C for the list of departments for which no information is reported.

³For the full list of OPM type-of-appointment codes, see Online Appendix C.

⁴In addition, a small number of politically appointed SES fall in the limited term appointment category,

or policy-determining nature. Schedule C appointees must have a presidential appointee, a SES appointee, or a Schedule C appointee as direct supervisor ([The Plum Book, 2020](#)). Regardless of the specific category, political appointees do not enjoy job protection, and can be removed at any time. They represent a small minority of all employees of the federal government—about 0.24% of positions throughout the 1997–2019 period.

All remaining positions are “non-political” in nature. To differentiate them from political appointments, we refer to these positions as “civil service positions,” and to employees in these positions as “civil servants.” Civil service positions can be divided into three categories: employees in the competitive service, Career SES, and the excepted service. Employees in the competitive service represent the clear majority of the civilian workforce. They are hired based on a competitive selection process with objective standards. Career SES positions include senior executives that are selected through a merit-based hiring process.⁵ Finally, employees in the excepted service are hired without being subjected to a competitive examination. These “unclassified” positions are used by departments when competitive examination is not practicable and recruitment is better achieved through alternative selection procedures. Examples include attorneys, policy analysts, or STEM occupations. Employees in any one of our three civil service categories generally enjoy significant protection from removal, sometimes after a probationary period.

[Figure 1](#) provides a high-level summary of our categorization scheme. It also reports, for each category, the average number of bureaucrats employed during our sample period. Our final dataset includes 2,940,914 employees with non-missing information on name, for a total of 76,813,841 employee-quarter observations.⁶

2.2 Voter registration data

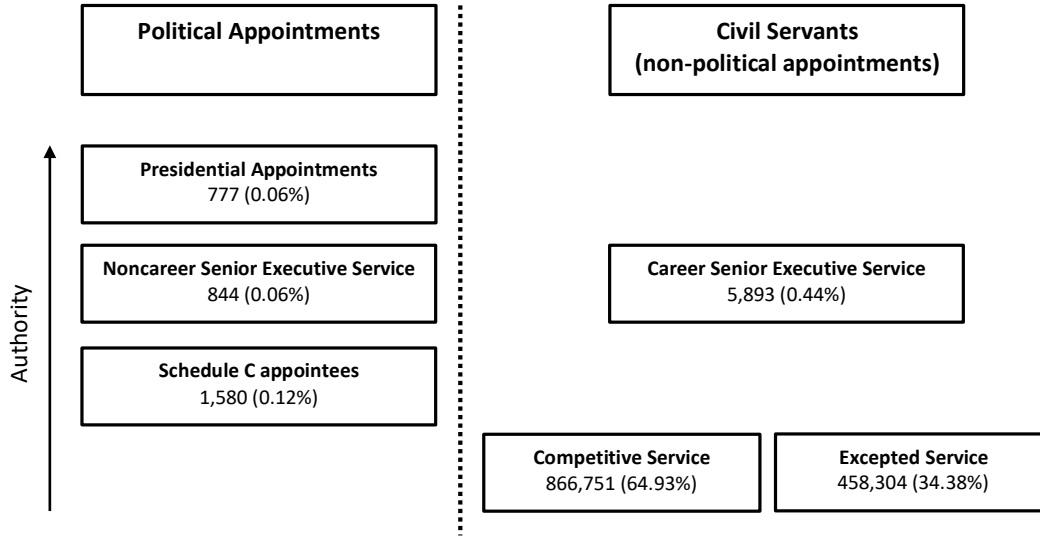
In order to be able to measure the political leanings of federal employees, we have acquired information on the universe of registered voters in the U.S. These data come from L2, Inc., a non-partisan for-profit data vendor that maintains high-quality databases of registered voters, political donors, and consumers. L2 collects, integrates, and standardizes information from different administrative and commercial sources, such as local election boards and Secretaries of State, the Federal Election Commission (FEC), mortgage and real estate records,

which can be used to fill positions that are either temporary (e.g., to lead a special project), or meet an unanticipated, urgent need.

⁵SES positions are designated as “career reserved” or as “general.” To ensure impartiality and insulation from political influence, the former positions can only be filled by career appointees. The latter can be filled by either career or political appointees. Noncareer appointments, however, cannot exceed 10% of SES positions government-wide, nor can they surpass 25% of a particular department’s SES positions.

⁶Online Appendix Figure [A1](#) shows how the number of employees in our data varies over the 1997–2019 period.

Figure 1: Categorization of positions in the U.S. federal bureaucracy



Notes: Breakdown of positions in the U.S. federal bureaucracy by the type of appointment (political appointments vs. non-political civil service appointments). The numbers reported are the average yearly number of employees employed in each position type between 1997-2019. The shares shown in parentheses are the average yearly shares of employees employed in each position type between 1997-2019.

Experian, and marketing mailing lists. It sells these data to political candidates and action committees (PACs), advocacy groups, and interested academics, among others. We have four waves of data, for 2014, 2016, 2018, and 2020.

In all but fifteen states does the partisanship of individuals in the L2 data coincide with the party affiliation in the respective states' voter registration lists. The remaining fifteen states do not collect information on voters' partisan leanings. For voters in these states, L2 uses predictive modeling to impute a "likely" party affiliation.⁷ Per the company, their proprietary machine-learning algorithms use an array of public and private data sources, including participation in primaries, demographics available through states' voter files, exit polling from presidential elections, commercial lifestyle indicators, census data, self-reported party preferences from private polling, and more. L2 does not guarantee that any single voter will self-identify as being associated with the assigned "likely" party, but it claims an accuracy level of 85% or better. Another limitation of the L2 data is that we only observe individuals who are registered to vote. According to Census Bureau estimates, registered voters make up about 70% of voting-aged citizens ([File, 2018](#)).

We construct a time-invariant measure of political ideology by classifying individuals in

⁷Specifically, L2 models party affiliation in the following states: HI, IL, WA, MT, ND, MN, WI, MI, VT, SC, MO, AL, TX, VA, and GA. In the OPM data, the share of bureaucrats from these states is 27%. In [Online Appendix E](#), we show that our main results are substantively unchanged if we drop from our sample bureaucrats who are matched to individuals in L2 who reside in one of these states.

the L2 data as Republican (Democrat) if we observe them more often registered as Republican (Democrat) than Democrat (Republican) across the four L2 waves. We classify as independent all individuals which are classified as such across all the L2 waves, or which we observe as Republican and Democrats for an equal number of waves. We interpret our measure as capturing a latent, time-invariant trait that proxies for the set of ideas and principles—in short, the political ideology—of each person.⁸

2.3 Matching of OPM and voter registration records

To recover information on the partisanship of government employees, we link individuals in the OPM and L2 voter registration data using a combination of name, location of residence, (i.e., state and county) and age. Overall, we are able to successfully match 1,985,726 out of the 2,940,914 bureaucrats in our sample, i.e., about 67.5%.⁹ The fact that about 32.5% of federal bureaucrats remain unmatched could be due to one of three issues. First, our matching procedure is conservative. In particular, we do not allow for even minor discrepancies in the spelling of first and last names across both data sources. Second, we consider as unmatched all instances in which a bureaucrat is matched to multiple voter registration records, unless all the voter records have the same partisan affiliation. Third, a significant fraction of bureaucrats is not registered to vote. Based on our analysis of data from the 2010–18 Voting and Registration Supplements to the Current Population Survey, only about 86% of civilian federal government employees are registered voters.¹⁰

Table 1 shows how matched and unmatched bureaucrats differ in terms of age, education, experience, annual pay, and location of employment. Columns 1 and 3 report the mean of each characteristic among matched and unmatched bureaucrats, respectively, and columns

⁸Changes in partisan registration across waves are relatively rare. Of the bureaucrats that we match to L2, only 6% change party across different waves, and only 2.6% are registered for the same number of waves as Democrat and Republican. In Online [Appendix E](#), we show that our main results are substantively unchanged if we drop the 6% of bureaucrats who change party across different waves.

⁹We first match employees to the L2 waves that are closest in time to the years in which we observe them in the data, and then match the remaining ones to the other L2 waves. Among the successfully matched individuals, 76.5% are matched by name, year of birth, and location. Since we lack information on age for a small share of federal employees, and since employees may be registered in a state that is different than the state of employment, we also allow for less stringent matching requirements. 12.7% of the matched individuals are linked by name and year of birth, while 10.8% are matched by name and location. We additionally assign partisan affiliation to individuals with multiple matches to L2, as long as all the matches share the same partisan affiliation. In Online [Appendix E](#), we show that our main results are substantively unchanged if we drop from our sample the 6.9% of bureaucrats with multiple matches. The matching rate increases slightly over the 1997-2019 period. Online [Appendix Figure A2](#) reports success rates for each year over the sample period. For additional details on how we combine the OPM data with voter registration records, see Online [Appendix C](#).

¹⁰Given the likely direction of survey bias in this setting, it stands to reason that 86% may be an upper bound on the true share of registered voters among federal bureaucrats.

Table 1: Differences in observables between matched and unmatched bureaucrats

	(1)	(2)	(3)	(4)	(5)	(6)
	Matched		Unmatched		Matched – Unmatched	
	Mean	Standard Deviation	Mean	Standard Deviation	Standardized Difference	<i>p</i> -value Equivalence test
Age less than 30	0.423	0.494	0.454	0.498	-0.061	0.000
Age 30-40	0.259	0.438	0.250	0.433	0.020	0.000
Age 40-50	0.173	0.378	0.157	0.364	0.041	0.000
Age 50-60	0.110	0.313	0.099	0.299	0.035	0.000
Age more than 60	0.035	0.185	0.040	0.196	-0.025	0.000
Highest education: college	0.232	0.422	0.218	0.413	0.035	0.000
Highest education: more than college	0.266	0.442	0.253	0.434	0.030	0.000
Quarters in federal bureaucracy	43.285	44.104	27.731	38.311	0.362	1.000
Annual pay	40074.176	34186.187	43173.673	36916.879	-0.088	0.000
Employed in D.C.	0.124	0.330	0.128	0.334	-0.012	0.000
Observations	1,985,726		955,188		2,940,914	

Notes: Descriptive statistics (mean and standard deviation) of individuals for which party affiliation is available (matched, columns 1-2) and for those for which party affiliation is unavailable (unmatched, columns 3-4). Column 5 reports the difference in means, divided by the standard deviation in the pooled sample of matched and unmatched. Column 6 reports the *p*-value from a tests for the equivalence of means using a two one-sided t tests approach; the reported *p*-value is the largest of the two *p*-values from two one-sided t-tests, under the null hypothesis that the difference is larger than 0.1 standard deviation, or smaller than -0.1 standard deviation, respectively. Sample includes all civil servants with non-redacted names serving between 1997-2019.

2 and 4 report the standard deviations in both samples. Column 5 reports the standardized difference in means between matched and unmatched bureaucrats.¹¹ Given the large sample size, all differences in Table 1 are statistically significant. Most of the magnitudes, however, are small.

To formalize this assertion we conduct equivalence tests. That is, we test the null hypothesis that the observed difference in some characteristic is economically large. We say that a difference in means is meaningfully large if it exceeds 10% of a standard deviation, and in column 6 of Table 1 we report *p*-values for the associated hypothesis test. In our view, equivalence testing is preferable to assessing the null hypothesis of no difference, especially when working with big data. In large data sets, even economically inconsequential differences may well register as statistically significant. Importantly for our purposes, we reject the null hypothesis of economically large differences for nine out of ten characteristics.

Only in terms of experience do we see an economically meaningful difference between matched and unmatched bureaucrats. Matched bureaucrats are present in the data for 15.5 additional quarters on average (corresponding to 0.362 standard deviations).¹²

¹¹We measure age, education, and pay at entry, namely as of the first quarter in which we observe the employee in the data in the 1973-2019 period. We measure experience as the total number of quarters in which we observe an employee in the data in the 1973-2019 period.

¹²A nontrivial part of the difference in experience between matched and unmatched bureaucrats is due

Matched bureaucrats are also on average older and more educated, but these differences are economically small. Relative to unmatched employees, matched bureaucrats are 3.1 p.p. less likely to be younger than 30. They are 1.4 p.p. more likely to have a four-year college degree, and 1.3 p.p. more likely to have some form of post-graduate education. The observed differences mirror those between registered and unregistered Americans in the general population. In the 2018 Voting and Registration Supplement to the Current Population Survey, about 24% of registered voters have a four-year college degree, and about 15% of registered voters have some form of post-graduate education. The corresponding shares among unregistered individuals are 11% and 4%. We see similar differences in terms of age, with an average age of 50.7 among registered voters and of 43.3 among unregistered individuals.

Finally, to directly address potential concerns over any observed differences between matched and unmatched bureaucrats, in Online [Appendix F](#) we show that our main results are qualitatively robust when we correct for selection into our sample by using inverse probability weighting (see [Wooldridge 2007, 2002](#)).

2.4 Procurement data

To relate political misalignment to tangible outcomes, we rely on U.S. federal procurement data covering 2004–2019. This allows us to study two presidential transitions (Bush–Obama, Obama–Trump).¹³ These data are collected through the Federal Procurement Data System (FPDS), and are made available through the FPDS-Next Generation database. For each procurement contract, the data list the initial procurement award and subsequent modifications (if any). We use this information to construct cost overrun and delay measures by comparing the initially projected costs and completion dates to realized costs and actual completion dates. Throughout our analysis, we focus on service and works contracts, since these are the types of contracts for which cost overruns and delivery delays are empirically most important. Given that our OPM data do not contain de-identified information for the Department of Defense, we drop all defense contracts.¹⁴ We further impose a range of standard sample restrictions from the related procurement literature ([Bajari and Tadelis,](#)

to the fact that our matching procedure relies on bureaucrats’ location of employment/residency. This is necessary in order to disambiguate between common names, given that we are matching bureaucrats to the universe of Americans who are registered to vote. Bureaucrats with longer employment spells have a more stable location of employment and residence, leading to a higher matching rate.

¹³Data limitations prevent us from extending the analysis further back in time, which should be kept in mind when assessing the external validity of our results.

¹⁴In addition to the OPM data being redacted for the Department of Defense, only 8.7% of DoD contracts provide information on the individual procurement officer in charge (compared to 60% for the non-DOD sample). Defense contracts account for about 60% of all procurement contracts in our sample period. The share of defense contracts among service and works contracts (excluding R&D) is 35%. We advise readers to keep these numbers in mind when thinking about the external validity of our results.

2001; Kang and Miller, 2020). In particular, we disregard indefinite delivery vehicle (IDV) contracts as well as lease and rental contracts, and we limit the sample to contracts that were performed within the U.S.¹⁵ Finally, we probe the robustness of our results by restricting attention to contracts of at least USD 25,000, which results in the exact same sample restrictions as in Decarolis et al. (2020b). We winsorize at the 5th and 95th percentiles to remove outliers.¹⁶

Critical for our purposes, the raw procurement data also list the email address of the officer in charge of the contract. We exploit this feature of the data to identify individual officers and subsequently match them to our OPM data. Specifically, we first construct the universe of unique email addresses in the FPDS database, from which we remove those that do not contain a name (e.g., admin@dept.gov). We then extract individuals’ names as well as the department and bureau for which they work. Before matching procurement officers to the OPM data based on name and bureau, we further enrich the data by linking email addresses to name directories in govtribe.com.¹⁷ This last step is useful because email addresses of federal employees do not always contain their owner’s middle or full first name. Our final dataset covers 1,079,923 procurement contracts created by 15,187 procurement officers across 168 departments and bureaus. Appendix Table B3 provides a step-by-step documentation of the sample selection process, and Table B4 reports summary statistics.¹⁸

3 Political alignment in the U.S. bureaucracy

In this section, we use our matched data to document the presence of large partisan cycles among political appointees, and the absence of partisan cycles among civil servants.

3.1 Partisan cycles among political appointees

We begin by documenting how the partisan leanings of political appointees covary with the party of the president. Political appointments are the prerogative of the president, vice president, or department heads. Since staffing decisions constitute one of the few direct tools to align the bureaucracy with the goals of the White House (Pfiffner, 2001; Clinton

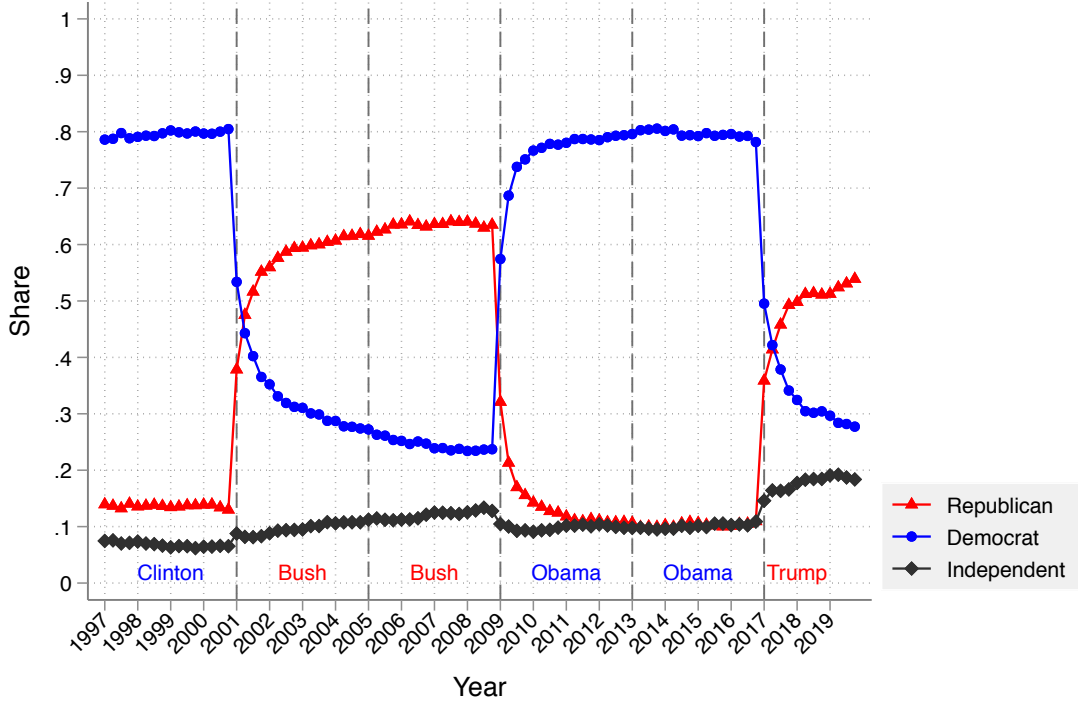
¹⁵Indefinite delivery vehicle contracts reflect long-running contractual arrangements that do not exactly specify quantities ex ante. Contracts that are performed and delivered outside the U.S. have very different cost structures and are thus typically omitted (Kang and Miller, 2020).

¹⁶We show in Online Appendix Table B6 that our results are robust to alternative cutoffs for winsorizing.

¹⁷Govtribe is a private data provider that specializes in providing information on federal contracting and grant-making.

¹⁸For additional details on our selection and matching criteria, see Online Appendix D.

Figure 2: Partisan affiliation of political appointees



Notes: Share of political appointees (presidential appointments, non-career senior executive service, schedule C appointees) by party over time. Dashed vertical lines mark presidential terms.

et al., 2012), we expect significant cycles in the ideology of political appointees.¹⁹

Figure 2 shows the raw share of political appointees that are affiliated with the Democratic party, with the Republican Party, and those who are independent.²⁰ Consistent with the use of the spoils system to increase ideological alignment between politicians and top bureaucrats, we observe large partisan swings coinciding with presidential transitions. The share of Democratic appointees falls from about 80% under Presidents Clinton and Obama to about 30% under Presidents Bush and Trump. The share of Republican political appointees increases from around 10% under Democratic presidents to more than 60% during the Bush administration, and to about 50% during the Trump administration. The cycles among independent appointees are more limited, with the share of independents raising from about 10% to about 20% under the Trump administration.

¹⁹Previous work documents the ideological proximity between the president and his political appointees, drawing on a variety of data sources, including the voting records of appointees who have previously served in Congress (Nixon, 2004), campaign donations (Bonica et al., 2015), or policy positions that cabinet members express during congressional testimony (Bertelli and Grose, 2011).

²⁰In this figure, we pool all political appointments, i.e., presidential appointments, non-career SES, and schedule C appointees.

Table 2, Panel A reports regression estimates that more precisely quantify the magnitude of the observed shifts. In columns 1 and 5, we regress an indicator for whether a political appointee is a Democrat or Republican on an indicator for the party of the president and a linear time trend. We also add bureau fixed effects in order to assess the extent to which political cycles are driven by parties’ tendencies to increase their representation in specific bureaus.²¹ Under a Democratic president, political appointees are 49.4 p.p. more likely to be a fellow Democrat—a 171% change relative to years in which the president is a Republican. Political cycles are even larger for Republican appointees. Relative to years with a Democratic president, we observe an increase of 45.8 p.p., or 371%, when a Republican rises to power.

Columns 2–4 and columns 6–8 of **Table 2**, Panel A report estimates of partisan cycles for each category of political appointment. Interestingly, we see larger effects for Noncareer SES and Schedule C appointees than for presidential appointments to top executive positions. This observation is consistent with the fact that the latter commonly require confirmation from the Senate, which may induce the president to either nominate more independents or a more-balanced mix of partisans. In a similar vein, Online Appendix **Figure A3** shows that the partisan composition of Noncareer SES and Schedule C appointees changes discontinuously in the year of a presidential transition, whereas changes in the partisan composition of presidential appointees occur much more gradually—presumably due to delays in the process of their confirmation.

²¹The estimates are very similar if we drop independents from the analysis, or if we do not include bureau fixed effects.

Table 2: Political cycles among political appointees and civil servants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employee is Democrat				Employee is Republican			
<i>Panel A: Political Appointees</i>								
Sample:	All Pol. Appointees	Presidential Appointees	Noncareer SES	Schedule C Appointees	All Pol. Appointees	Presidential Appointees	Noncareer SES	Schedule C Appointees
President Democrat	0.494*** (0.008)	0.251*** (0.016)	0.591*** (0.015)	0.546*** (0.011)				
President Republican					0.458*** (0.008)	0.237*** (0.016)	0.550*** (0.015)	0.505*** (0.011)
Observations	139,114	32,245	36,734	70,164	139,114	32,245	36,734	70,164
Effect size	+171%	+62%	+235%	+213%	+371%	+109%	+571%	+537%
<i>Panel B: Civil Servants</i>								
Sample:	All Civil Servants	Competitive Service	Career SES	Excepted Service	All Civil Servants	Competitive Service	Career SES	Excepted Service
President Democrat	-0.002*** (0.000)	-0.003*** (0.000)	0.002 (0.004)	0.001* (0.000)				
President Republican					0.000** (0.000)	-0.000 (0.000)	0.002 (0.003)	0.003*** (0.000)
Observations	58,882,915	38,041,926	291,662	20,665,132	58,882,915	38,041,926	291,662	20,665,132
Effect size	-0.4%	-0.5%	+0.4%	+0.2%	+0.1%	-0.1%	+0.7%	+0.8%
Bureau FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Regression estimates of the party alignment effect. The unit of observation is the individual-quarter. In columns 1-4, the dependent variable is a dummy that is 1 if the civil servant is a Democrat, and 0 otherwise. In columns 5-8, the dependent variable is a dummy that is 1 if the civil servant is a Republican, and 0 otherwise. *President Democrat* is a dummy that is 1 if the president is a Democrat, and 0 otherwise. *President Republican* is a dummy that is 1 if the president is a Republican, and 0 otherwise. The sample covers all matched individuals between 1997-2019. In Panel A, columns 1 and 5 restrict the sample to all political appointees, columns 2 and 6 restrict the sample to presidential appointments, columns 3 and 7 restrict the sample to non-career senior executive service officers, columns 4 and 8 restrict the sample to Schedule C appointees. In Panel B, columns 1 and 5 restrict the sample to all civil servants, columns 2 and 6 restrict the sample to the competitive career service, columns 3 and 7 restrict the sample to career senior executive service officers, columns 4 and 8 restrict the sample to employees in the non-political excepted service. All regressions include a linear time trend, and bureau fixed effects. The effect size is defined as the estimated coefficient divided by the mean of the dependent variable when the president is Republican (columns 1-4) or Democrat(columns 5-8). Standard errors in parentheses, clustered at the individual-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.2 Political insulation of civil servants

Next, we ask how civil servants’ appointments and career progression depend on ideological alignment with the current administration. Although formally insulated from political interference, there exist at least two potential mechanisms that could lead to the emergence of political cycles among civil servants.

First, presidents and political appointees may attempt to exert control over civil service positions by manipulating extant personnel policies. Such strategies are known to have been used by the Nixon administration, which summarized them in the *White House Personnel Manual*. This “manual” was distributed to political appointees as a guide on how to fill positions with ideologically aligned individuals. In one prominent example, political appointees were instructed that, in order to induce a career executive to leave, “you simply call an individual in and tell him he is no longer wanted. [...] There should be no witnesses in the room at the time” (Subcommittee on Manpower and Civil Service, 1976, p. 163). Political appointees could also use transfers to remove unwanted employees from key positions, with the expectation that they would hire or promote individuals who were recommended by the White House (Cole and Caputo, 1979). More recently, the Trump administration has been accused of using reassignments in order to push out unwanted employees (Halper, 2017).

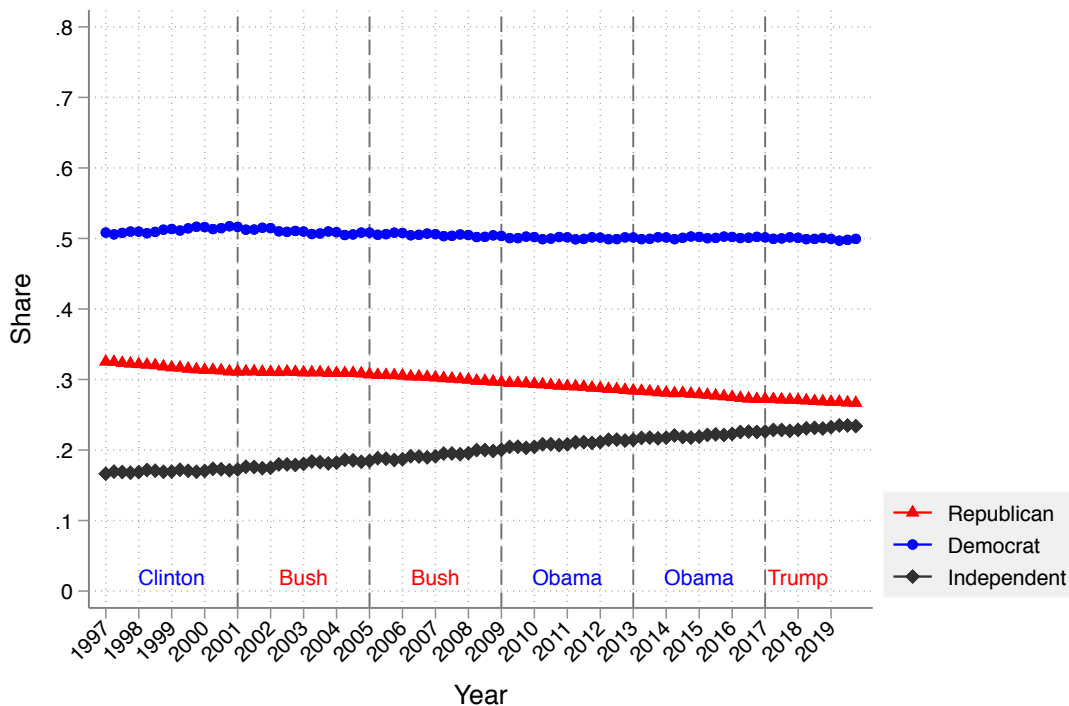
Second, civil servants may leave on their own accord if their ideological preferences are no longer aligned with the administration. For example, Trump’s targeting of the Environmental Protection Agency’s mission was reportedly responsible for the departures of several career employees (Plumer and Davenport, 2019). Additionally, prospective civil servants may not even apply for a position if they disagree with the overall direction of the organization.

We quantify the aggregate relevance of these channels in Figure 3 and Table 2, Panel B. The former depicts trends in the party affiliation of civil servants over time, while the latter presents regression estimates. In marked contrast to political appointees, there are no visually apparent partisan cycles among career civil servants. The share of Democrats remains nearly constant over the entire sample period, while that of Republicans exhibits a slight monotonic downward trend, which is offset by a corresponding increase in the fraction of civil servants that are independents. None of these trends appear to be affected by which party controls the government. This impression is confirmed by the coefficients in Table 2, Panel B. Although our estimates are very precise—due to the size of our panel—they are economically small, both when we consider all civil servants (columns 1 and 5) and when we separately analyze each type of civil service position (columns 2–4 and columns 6–8).²²

In Online Appendix Table B1, we focus on the hiring margin. For each civil servant,

²²Online Appendix Figure A4 shows trends in partisan affiliation for each type of civil servant position.

Figure 3: Partisan affiliation of civil servants



Notes: Share of (non-political) civil servants (competitive service, career senior executive service, excepted service) by party over time. Dashed vertical lines mark presidential terms.

we keep the first observation in an employment spell and re-estimate the same econometric models as in [Table 2](#). At the hiring margin, we do find evidence of political cycles in the excepted service and, especially, in the senior executive service. The magnitude of the relevant estimates, however, is relatively small when compared to those of political appointees. In quarters with a Democratic (Republican) president, new senior executive hires are 5.7% (10.1%) more likely to be fellow Democrats (Republicans). This finding is consistent with political appointees trying to exert control over civil servants in managerial positions. Another explanation might be that candidates for senior executive positions have lucrative opportunities outside of the federal government, which makes them less likely to apply for a civil position if they disagree with the overall direction of the organization (see, e.g., [Bolton et al. 2020](#)).

We also explore whether political alignment is associated with changes in earnings. To this end, we regress civil servants' log annual earnings on an indicator equal to one if they are aligned with the party of the president, individual fixed effects, and quarter (or quarter \times bureau) fixed effects. In light of the rigid pay structure in the U.S. civil service, increases in a bureaucrat's compensation are best interpreted as progressions along the career

Table 3: Political alignment and career progression of civil servants

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total pay		Transferred away from D.C.			
Politically aligned	0.0002** (0.0001)	-0.0008*** (0.0001)	0.0002** (0.0001)	0.0001 (0.0001)	0.0000 (0.0005)	-0.0002 (0.0005)
Observations	57,445,624	57,445,176	6,169,574	6,168,189	146,434	142,893
Sample	All	All	Non-SES	Non-SES	SES	SES
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FEs	Yes		Yes		Yes	
Year-Quarter-Bureau FEs		Yes		Yes		Yes

Notes: Regression estimates of the party alignment effect on pay and transfers away from D.C. The unit of observation is the individual-quarter. The sample covers all matched (non-political) civil servants between 1997-2019. In columns 3-4, the sample is restricted to civil servants who work in D.C. and are not members of the Senior Executive Service. In columns 5-6, the sample is restricted to Senior Executive Service civil servants who work in D.C. In columns 1-2, the dependent variable is the log annual total pay. In columns 3-6, the dependent variable is a dummy that is 1 if the individual’s work location changed from D.C. to outside D.C. *Politically aligned* is a dummy that is 1 if the civil servant and president are from the same party. Standard errors in parentheses, clustered at the individual-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ladder. The results from our regressions are shown in columns 1 and 2 of [Table 3](#). They are very precise but provide no evidence of economically significant alignment effects on the compensation of civil servants.²³

We additionally investigate whether employees who are misaligned with the president are more likely to be transferred—a strategy that department heads may use to create vacancies in key positions. In particular, we focus on transfers away from D.C., which may be interpreted as assignments to less prestigious jobs. The results are shown in columns 3–6 of [Table 3](#). Since the former may be more likely to be targeted by the administration, we separately consider members of the SES and non-SES civil servants. Once again, we do not observe economically significant alignment effects.

In sum, we find only limited evidence that political cycles affect civil servants’ careers. The insulation of most civil service positions from political interference makes it difficult for the administration to facilitate the hiring, termination, or promotion of ideologically aligned bureaucrats.²⁴ First, for most bureaucrats, the benefits of a long-term career in the federal

²³Online Appendix [Table B2](#) shows that we do not find alignment effects on compensation in the sample of political appointees. This is in line with the selection margin (hiring and terminations), and not the influence on career progression, being the primary tool with which Presidents put aligned individuals in key managerial positions in the government.

²⁴We also analyzed the exit margin. Again, we find no meaningful increase in overall exit rates around Presidential transitions. If we focus only on the career senior executive service, however, then we do observe a significant increase in exits during the last quarter of the Obama administration. This is consistent with previous results by [Bolton et al. \(2020\)](#), who document an increase in turnover among the most senior civil servants when a new administration takes office.

government may outweigh the intrinsic costs of temporarily serving an objectionable administration. Second, misaligned bureaucrats may decide to remain in the federal government as a way to influence the direction of the organization from within the system.²⁵

4 Ideological alignment and procurement performance

Our descriptive analysis shows that, at any one point in time, a sizeable share of federal bureaucrats is ideologically misaligned with the administration they serve. This raises the question of whether (mis)alignment has any bearing on their performance.

Since our analysis covers the vast majority of federal workers, obtaining a comparable measure of performance among such a varied set of employees is difficult. To make progress, we focus on a subset of bureaucrats that specialize in fulfilling a comparable and important function across all arms of the federal government: procurement officers. Procurement officers are in charge of purchasing a wide range of goods and services on behalf of the government. They play a crucial role in both the ex ante selection of buyers and the ex post monitoring of contract execution. Moreover, procurement contracts make up a sizeable share of the federal budget. In 2019, the combined value of federal procurement expenditures amounted to 10% of U.S. gross domestic product (Schwarzenberg, 2022).

Online Appendix [Figure A7](#) shows the share of procurement officers by party over time. The patterns therein mirror the results in the previous section. In other words, there is no evidence of partisan cycles among procurement officers.²⁶

4.1 Measurement and empirical strategy

To study the implications of ideological misalignment among procurement officers, we construct two measures of procurement performance: in-scope cost overruns and delays. Cost overruns and delays constitute ex post deviations from the initial contract and are standard measures of contract performance in the procurement literature (see, e.g., [Bajari and Tadelis, 2001](#); [Decarolis et al., 2020b](#); [Kang and Miller, 2020](#)). Our analysis focuses on cost overruns as the main performance measure as it allows us to directly quantify the monetary costs of (mis)alignment. The measure is defined as the difference between the realized and the (ex

²⁵This rationale is explicitly mentioned in a 2017 *Washington Post* opinion column by a senior U.S. diplomat. Despite leaving his post following the Trump administration’s decision to withdraw from the Paris Agreement, he invited his colleagues to remain in their positions “so that they can continue to work within the system to make things a little bit better, a little bit at a time.”

²⁶A regression of an indicator for whether a procurement officer is a Democrat on an indicator for whether the president in power is Democrat, and a similar regression for Republicans, confirm the absence of political cycles (p -values are 0.705 and 0.470, respectively).

ante) expected outcome, relative to the initial expectation:

$$\text{cost overrun}_j = \frac{(\text{actual cost}_j - \text{initial cost}_j)}{\text{initial cost}_j}, \quad (1)$$

where actual cost_j is the ex post realized cost, and initial cost_j denotes the expected cost of contract j . We construct our measure of delay in the same way.²⁷

With these performance measures in hand, we estimate the following contract-level regression model:

$$y_j = \beta \cdot \textit{Politically aligned}_{I(j)T(j)} + \theta_{I(j)} + \tau_{T(j)} + \varepsilon_j \quad (2)$$

where y_j is the procurement outcome of contract j (e.g. its cost overrun), which was created in year \times month $t = T(j)$. $i = I(j)$ denotes the procurement officer who created it, and $\textit{Politically aligned}_{I(j)T(j)}$ is an indicator equal to one if and only if the officer is affiliated with the same party as the president when the contract was created. $\theta_{I(j)}$ and $\tau_{T(j)}$ are procurement officer and year \times month fixed effects, respectively.²⁸ To account for the fact that officers handle multiple contracts, we cluster standard errors at the officer-level.

To see how β is identified, note that turnover in the White House creates shocks to the political alignment of individual procurement officers. Since we control for time fixed effects, β is identified by comparing over-time changes in the performance among officers who experience shocks, i.e., officers who switch from being aligned with the apex of government to being misaligned and vice versa.²⁹

Our measure of political alignment captures ideological congruence between procurement officers and the White House at the time of contract award. The execution of larger and longer term contracts, however, can span multiple presidencies.³⁰ In addition to alignment at the time of the award, there is thus intensive margin variation in how long contracts were managed by an aligned officer. We exploit this fact to refine our measure of alignment by computing the variable $\textit{Share politically aligned}_{I(j)T(j)}$. This variable corresponds to the fraction of a given contract’s expected life-cycle in which the assigned procurement officer was

²⁷The definition above follows Carril et al. (2021). Our results are robust to using alternative measures, such as those in Decarolis et al. (2020b).

²⁸The year \times month fixed effects absorb common temporal shocks. For example, they alleviate the concern that the timing of contract initiation might vary systematically with political alignment.

²⁹In Equation 2, Independents are never aligned and experience no changes in alignment. Although Independents contribute no identifying variation, we include them in our regressions in order to more precisely estimate the coefficients on various control variables.

³⁰In our sample, 7% of contracts span two presidencies. These are also contracts that tend to require more monitoring and for which ex post modifications are more frequent. Our results also hold when restricting the sample to only contracts executed and completed under the same political alignment throughout.

ideologically aligned with the current administration. *Share politically aligned* thus varies continuously between zero and one.³¹ Below, we show results from specifications that use this alternative alignment measure in lieu of the *Politically aligned* $_{I(j)T(j)}$ indicator.

4.2 Political alignment reduces cost overrun

Table 4 reports the results from estimating Equation 2. Panel A shows the results using the relative cost overrun measure defined in Equation 1. Panel B focuses on the extensive margin, using as the dependent variable an indicator equal to one if the contract exhibits any cost overrun. All results include procurement officer fixed effects as well as fixed effects for year \times month of contract creation.

Column 1 shows that contracts overseen by procurement officers under political alignment with the president are completed with lower average cost overruns. This is true along both the extensive and the intensive margin. The estimated effect size is economically significant, amounting to 1% of the initial contract value or about 6% of the average overrun. On the extensive margin, contracts overseen by aligned officers are 1 percentage points (p.p.) less likely to exhibit overruns.

Column 2 adds controls for the officer’s years of service and a wide range of contract characteristics, such as the initial contract size, expected duration, award type fixed effects, fixed effects for the type of contract pricing, industry fixed effects, as well as product and service type fixed effects.³² We also control for the total number of contracts a given officer has created in the same year and month. The coefficients of interest, however, remain virtually unchanged. This suggests that the alignment effects are unlikely to be driven by differences in contract characteristics—a mechanism that we revisit in section 5.

Column 3 includes even more granular fixed effects, comparing only procurement officers in the same department and year. If the observed alignment effects were driven by departments with more aligned procurement officers receiving lighter workloads or easier procurement tasks, then we would expect the point estimates to noticeably decrease after controlling for Department \times Year fixed effects. This is not the case.

Columns 4–6 replicate our baseline results using our refined, continuous measure of alignment, which exploits variation in the share of contract duration that was executed under an aligned president. The results are, if anything, somewhat larger and more precise, suggesting that exposure to alignment during the contract’s execution period is important in explaining

³¹Since the contract duration is potentially endogenous, we compute the share of alignment using the pre-determined expected duration at time of contract award.

³²These granular product and service type fixed effects ($\approx 1,400$ categories) allow us to account for differences in the composition of products and services procured which might vary with political alignment if, for instance, aligned officers are more likely to contract products and services that are mission-critical.

Table 4: Political alignment reduces cost overrun

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Relative cost overrun</i>						
Mean of dep. var	0.186	0.186	0.186	0.186	0.186	0.186
Politically aligned	-0.010** (0.004)	-0.013*** (0.004)	-0.011*** (0.004)			
Share politically aligned				-0.014*** (0.004)	-0.015*** (0.004)	-0.013*** (0.004)
<i>Panel B: Any cost overrun</i>						
Mean of dep. var	0.235	0.235	0.235	0.235	0.235	0.235
Politically aligned	-0.010*** (0.003)	-0.012*** (0.003)	-0.010*** (0.003)			
Share politically aligned				-0.010*** (0.003)	-0.012*** (0.003)	-0.011*** (0.003)
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes		Yes	Yes
Department \times Year FEs			Yes			Yes
Observations	1,079,923	1,079,923	1,079,923	1,079,923	1,079,923	1,079,923

Notes: The unit of observation is the contract. The dependent variable in Panel A is *Relative cost overrun*: the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). The dependent variable in Panel B is *Any cost overrun*: a dummy that is 1 if the contract exhibits any cost overrun. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party when the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract’s expected duration in which the procurement officer and the president were from the same party. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the effects that we uncover. In other words, contracts that were handled by an officer who was aligned for a longer period of time exhibit significantly lower cost overruns. In fact, as shown in Online Appendix Figure A8, the partial relationship between our continuous measure of alignment and cost overruns appears to be approximately linear.

In the Online Appendix, we provide a series of additional robustness checks to corroborate our main finding. In Online Appendix Table B5, we show that the results are robust to alternative definitions of cost overrun. Online Appendix Table B6 shows that the results are also robust to using a variety of alternative thresholds for dropping outliers. Online Appendix Figure A9 demonstrates that our results are not driven by any particular department. Online Appendix subsection F.2 shows that the results also hold up when reweighting the matched sample to be representative of the full sample in terms of initial contract size, duration, and procurement officer experience. Finally, Online Appendix Table E16 shows that the results are robust to dropping procurement officers who (i) are matched to multiple voter registration

records, (ii) change party affiliation across different L2 waves, and (iii) are matched to voter registration records in states where L2 models party affiliation.

4.3 Heterogeneity and event study

In [Table 5](#), we examine heterogeneity in effect size. In Columns 1 and 2, we split the full sample into procurement contracts above and below the \$25,000 threshold. Contracts above \$25,000 tend to be contracts that are more complex and for which discretion – and hence an individual officer’s effort – is likely to be more important ([Decarolis et al., 2020b](#)). Consistent with this view, we find that alignment effects are largely concentrated in contracts over \$25,000. Although we do observe a reduction in overrun for small contracts, the point estimate is considerably smaller and statistically insignificant.³³

In Columns 3 and 4, we divide our sample according to a different measure of complexity. We classify each good and service according to how predictable cost overruns are by computing the standard deviation in the residual after conditioning on ex-ante observable contract characteristics. Contracts for goods and services with higher residual variance are less likely to be standardized, which leads us to expect that procurement officers’ effort and discretion would have greater impact. Furthermore, these more uncertain contracts are also likely to be those where moral hazard issues are more prevalent ([Carril et al., 2021](#)). Although we do observe non-trivial alignment effects for contracts below median uncertainty in overruns, our estimates are even larger for contracts with less predictable outcomes.

In Columns 5 and 6, we ask whether the observed alignment effect varies across presidential transitions. We split our sample into the Bush-Obama transition (2001-2017) and the Obama-Trump transition (2009-2019). This allows us to test whether alignment effects are driven by a particular transition and whether they depend on whether Democratic or Republican officers become aligned. As both columns show, the estimated alignment effects are approximately symmetric: both Democrats *and* Republicans see lower cost overruns of comparable magnitude under an aligned president.³⁴ This is an important finding because it suggests that the alignment effects the we uncover are more likely to reflect a general phenomenon, rather than a party- or president-specific effect.

We can combine both transitions to provide visual evidence for the effect of political alignment on cost overruns. We focus on contracts over \$25,000 and estimate an event study around the time of officers’ switch in alignment status. We focus on two windows around

³³In Online Appendix [Table B7](#) we also show the breakdown by contract-size quartiles.

³⁴Given the multitude of differences between the Bush-Obama and Obama-Trump transition, the apparent symmetry in effect size might be surprising. While we cannot reject that the alignment effects are symmetric across transitions, we do note that the point estimates are slightly larger for the Obama-Trump transition.

Table 5: Political alignment and cost overrun, by contract complexity and transition

	(1)	(2)	(3)	(4)	(5)	(6)
			Relative cost overrun			
Mean of dep. var	0.116	0.282	0.0797	0.290	0.181	0.197
<i>Panel A: Political alignment at time of award</i>						
Politically aligned	-0.002 (0.004)	-0.019*** (0.005)	-0.007* (0.003)	-0.013** (0.006)	-0.010** (0.004)	-0.012** (0.005)
<i>Panel B: Share of contract duration politically aligned</i>						
Share politically aligned	-0.003 (0.004)	-0.022*** (0.005)	-0.008** (0.004)	-0.016** (0.007)	-0.014*** (0.004)	-0.016*** (0.005)
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sample	\$25k-	\$25k+	SD(Residual overrun)		Bush-Obama	Obama-Trump
			Low	High		
Observations	626,651	450,664	536,986	540,183	926,085	873,841

Notes: The unit of observation is the contract. *Relative cost overrun* is the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party when the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract’s expected duration in which the procurement officer and the president were from the same party. Columns 1 and 2 restrict the sample to contracts with an expected contract size of below \$25,000, or at least \$25,000, respectively. Columns 3 and 4 restrict the sample to contracts with below, or above median uncertainty in overruns (measured as the standard deviation in the residual overrun after conditioning on ex-ante observable contract characteristics), respectively. Columns 5 and 6 restrict the sample to contracts created between 2001-2017 and 2009-2019, respectively. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the two presidential transitions in our data (2004-2011 for the Bush-Obama transition, and 2012-2019 for the Obama-Trump transition) and estimate the following regression model:

$$y_j = \sum_{s=-3}^{+4} \beta_s \cdot \text{Become aligned}_{I(j)p(j)} \cdot \mathbf{1}[k(j) = s] + \theta_{I(j)p(j)} + \tau_{k(j)p(j)} + X_j' \gamma + \varepsilon_j \quad (3)$$

where $p(j)$ indexes the presidential transition event window in which contract j was created, and $k(j)$ indexes the time when the contract was created (i.e., the year relative to the year of the transition). $\theta_{I(j)p(j)}$ are procurement officer \times presidential transition fixed effects, and $\tau_{k(j)p(j)}$ are time \times presidential transition fixed effects. $\text{Become aligned}_{I(j)p(j)}$ is an indicator equal to one if the officer creating the contract is “treated” during a given transition p ,

namely if the officer is misaligned with the party of the president before the transition and becomes aligned after the transition. The coefficients of interest β_s measure the change in cost overruns of treated officers s years before/after a transition, compared to the change in outcomes of the officers who were aligned before the transition and are misaligned after it. The omitted reference year is the year before the transition.³⁵ We use the same set of contract-level controls and fixed effects (X'_j) as in column 3 of [Table 4](#).

[Figure 4](#), panel (a) shows the result. We observe a persistent reduction in cost overruns starting from the year of the transition. Importantly, we do not observe significant pre-trends: prior to the switch in alignment status, there is no differential trend in cost overruns between those who would eventually become aligned vs. those who would eventually become misaligned.

4.4 Political alignment and other procurement outcomes

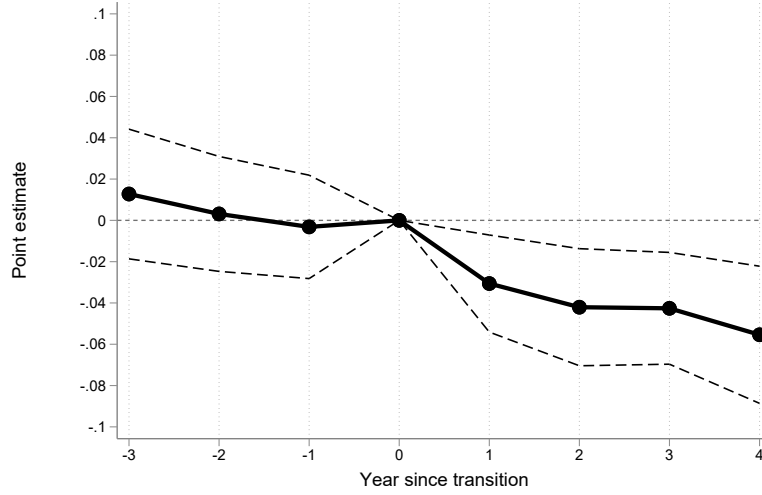
In [Table 6](#), we consider a range of additional procurement outcomes that could perhaps offset any cost saving effects of political alignment. We first investigate whether political alignment affects delays. Our measure of delay is constructed in the same fashion as the cost overrun measure in [Equation 1](#). As shown in column 1, political alignment also decreases delays, although the estimate is not statistically significant. In column 2, we focus on complex, “high duration” contracts, as measured by having an above median expected duration (≥ 148 days). Resembling the heterogeneity we observed with respect to cost overruns, we find significant alignment effects concentrated among more complex contracts, for which discretion and individual officer’s effort are likely more relevant. Among these contracts, those assigned to an officer who is politically aligned exhibit 1.8% shorter contract durations, corresponding to 4.5% of the average delay.³⁶ As before, we can visualize the delay effects using an event study. In [Figure 4](#), panel (b), we re-estimate our event-study specification using relative delays as the dependent variable. Consistent with the results above, we find that procurement officers who become aligned see their contracts completed faster relative to those who become misaligned. Reassuringly, we do not observe significant pre-trends.

We also ask whether political alignment affects the number of ex post modifications that are made to the procurement contract. We focus on in-scope modifications which reflect

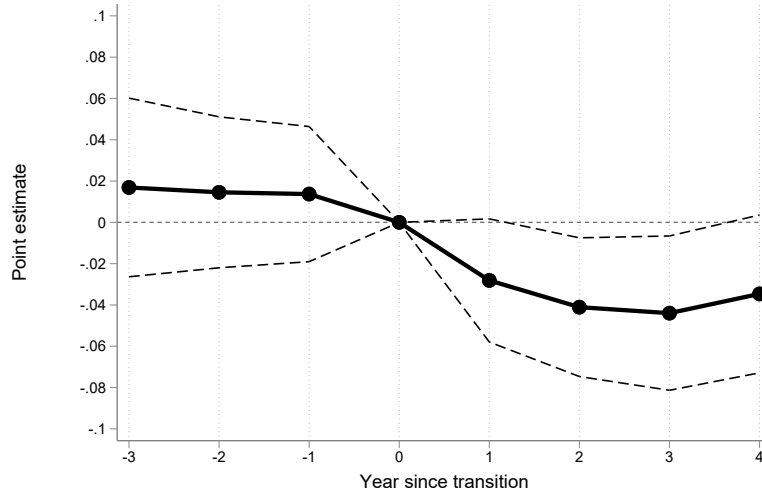
³⁵The majority of contracts created in 2008 and 2016 carry over into the following years (73% and 81%, respectively), and most of them are performed for a greater number of days under the new administration than under the old one (62% and 66%, respectively). In light of this fact, we center the event window in [Figure 4](#) around 2007 and 2015, which means that 2008 and 2016 are classified as the first post-transition years. For this reason, our event-study results are more directly comparable with the specifications relying on the variable “Share politically aligned,” which directly accounts for the fact that contracts signed in 2008 and 2016 are mostly performed under a new administration.

³⁶In Online Appendix [Table B8](#) we also show the breakdown by quartiles of the initial contract duration.

Figure 4: Event study – Cost overrun, delays and shock in alignment



(a) Relative cost overrun



(b) Relative delays

Notes: Estimated β_s coefficients from Equation 3, with 95 percent confidence intervals based on standard errors clustered at the procurement officer-level. The estimating equation includes the same set of contract-level controls and fixed effects as in column 3 of Table 4. In Panel (a), the dependent variable is relative cost overrun and the sample is restricted to contracts with an expected size of at least \$25,000. In Panel (b), the dependent variable is relative delays and the sample is restricted to contracts with an expected above median duration (≥ 148 days).

Table 6: Political alignment and other procurement outcomes

	(1)	(2)	(3)	(4)	(5)
	Relative delays		Modifications		Contract
			Any	IHS #	terminated
Mean of dep. var	0.424	0.397	0.486	0.714	0.413
<i>Panel A: Political alignment at time of award</i>					
Politically aligned	-0.003 (0.006)	-0.018*** (0.006)	-0.009** (0.004)	-0.018*** (0.007)	-0.002 (0.033)
<i>Panel B: Share of contract duration politically aligned</i>					
Share politically aligned	-0.005 (0.007)	-0.022*** (0.007)	-0.009** (0.004)	-0.019*** (0.007)	0.005 (0.033)
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Sample	Full	High duration	Full	Full	Full
Department \times Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	1,074,675	536,661	1,074,675	1,074,675	1,074,675

Notes: The unit of observation is the contract. *Relative delays* is the difference between the actual contract duration and the expected duration, normalized by the expected duration (see Equation 1). *Any modifications* is a dummy that is 1 if any modification to the contract was made after contract award. *IHS# modification* denotes the inverse hyperbolic sine transformation of the number of ex post modifications. *Terminated* is a dummy that is 1 if the contract was terminated, rescaled by 100 for legibility. Columns 1 and columns 3-5 show results in the full sample of contracts, while column 2 (High duration) restricts the sample to contract with an above median expected contract duration at time of award (148 days). Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

amendments made within contracts' original scope. The results in columns 3 and 4 of Table 6 imply that contracts overseen by procurement officers who are politically aligned exhibit a lower likelihood and number of ex post modifications. This is consistent with cost overruns and delays being partly due to ex post adjustments.

Finally, in column 5 of Table 6 we examine whether the contract was prematurely terminated. Terminations are rare events in which the contract is either terminated due to the failure of the seller to meet contractual obligations (terminate for default), or because the procurement good or service was no longer needed (terminate for convenience). Given the rare nature of the outcome, we scale the dependent variable by 100 for legibility. Political alignment is associated with a lower likelihood of termination in only one of our specifications, with a small and statistically insignificant point estimate.

Taken together, the evidence in this section suggests that ideological misalignment of individual officers has a nontrivial impact on cost overruns and number of ex post modifications. In addition, complex contracts overseen by a misaligned officer exhibit also greater delays. We conclude that political misalignment is detrimental to contract performance.

5 Discussion and mechanisms

In this section, we investigate the possible mechanisms behind the alignment effect that we document above. We consider three possibilities. First, political alignment may be associated with differential task assignment. Ideologically aligned procurement officers might enjoy a lighter workload or be assigned simpler contracts, resulting in higher contract performance.

A second potential explanation is that performance is rewarded less when civil servants are misaligned with the apex of the government. If procurement performance and political alignment are complements for career progression, the incentives that civil servants face might induce them to exert greater effort when they are aligned.

A third explanation may be a general “morale effect,” whereby misaligned civil servants are less motivated to exert effort. This channel is succinctly described by [Besley and Ghatak \(2005\)](#), who argue that “the productivity of the bureaucracy will change endogenously if there is a change in the mission due to the principal being replaced [...]. This provides a possible underpinning for the difficulty in reorganizing public sector bureaucracies and a decline in morale during the process of transition” (p. 629).

5.1 Differential task assignment

We investigate whether task assignment varies with political alignment in [Table 7](#). We report results from estimating the specification in [Equation 2](#) using officers’ workload or the characteristics of the contracts that they oversee as dependent variables.

In column 1, we aggregate our contract-level panel to the procurement officer-quarter level, and test whether the number of contracts assigned to a procurement officer varies with political alignment. We do not find that officers enjoy lighter workloads in quarters in which they are politically aligned. This null effect is precisely estimated and suggests that differential workloads are unlikely to explain the higher procurement performance we observe.

To ask whether aligned procurement officers work on different contracts, the analysis in the remaining columns is conducted at the contract-level. In column 2, the dependent variable is the (log) initial contract size. As the results show, aligned officers do not work on

contracts with lower expected cost at the time of award. Similarly, the estimate in column 3 shows that the expected duration of the contract on which an officer works does not vary with alignment. These estimates are inconsistent with aligned officers working on contracts that are ex ante less complex.

In addition, we can utilize the full set of contract-level observables to measure contract complexity. In column 4, the dependent variable is the predicted cost overrun of the contract. We predict cost overrun by regressing our measure of relative cost overrun in [Equation 1](#) on the full set of ex ante observable characteristics, i.e., expected contract size and duration, fixed effects for industry, product and service type, type of award, and type of contract pricing, as well as the overall number of contracts assigned to the officer in the quarter. For instance, this measure captures the fact that contracts dealing with specific products, or in specific industries, might be more likely to result in overruns. The estimate in column 4 shows that aligned procurement officers do not work on contracts that, based on our rich set of ex ante observables, are on average more likely to result in overruns.³⁷ In column 5, we repeat the analysis with predicted delays as the dependent variable. Once again, we do not find that procurement officers are assigned to contracts that, based on ex ante observable contract characteristics, are more likely to result in delays.³⁸

Finally, we note that the coefficients on *Share politically aligned* in columns 4-6 of [Table 4](#) would remain virtually unchanged if we conditioned on officer alignment at the time of contract initiation. This simple observation suggests that differential task assignment between aligned and misaligned procurement officers is unlikely to drive the political alignment effects we uncover. Taken together, our results suggest that differential task assignment is unlikely to be a major driver of alignment effects on contract performance.

5.2 Promotion incentives

We have also investigated whether promotion incentives change with alignment. Career progression of procurement officers is primarily based on a combination of seniority, quali-

³⁷In Online Appendix [Table B10](#), we show that alignment does not predict bidding type (competed or not), the number of bidders, or whether the supplier is politically aligned with the procurement officer, considering a supplier as likely democrat if it is a minority-owned or disadvantaged women-owned business. We also show that the results remain quantitatively unchanged when including supplier fixed effects, though the sample size declines due to the omission of singleton firms. In sum, we find no evidence to suggest that differential supplier selection drives ex post cost overruns. Note, our results comparing political alignment at the individual-level within departments, are complementary to [Dahlström et al. \(2021\)](#), who show that more politicized agencies use more non-competitive procedures.

³⁸Since our analysis in the previous section yields evidence of stronger alignment effects for high-value and long-duration contracts, we have replicated the analysis in columns 2-5 of [Table 7](#) restricting attention to such contracts only. The results provide no evidence to suggest that task assignment varies with political alignment (cf. Online Appendix [Table B9](#)).

Table 7: Task assignment does not vary by political alignment

	(1)	(2)	(3)	(4)	(5)
	Number	Expected		Predicted	
	contracts	Contract size	Duration	Overrun	Delay
Mean of dep. var	3.784	9.833	4.707	0.186	0.438
Politically aligned	-0.065	0.008	0.002	-0.000	-0.000
	(0.150)	(0.020)	(0.016)	(0.001)	(0.002)
Year \times Quarter FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Observations	284,528	1,079,923	1,079,923	1,079,923	1,079,923

Notes: Unit of observation in column 1 is the balanced individual-quarter level. Unit of observation in columns 2-5 is the contract-level. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party. *Number contracts* is the number of contracts a procurement officer created in a given quarter. *Expected contract size* is the (log) expected size (in USD) of the contract at time of award. *Expected duration* is the (log) expected contract length (in days) at time of award. *Predicted overrun* (*Predicted delay*) is the cost-overrun (delay) predicted by regressing our measure of cost-overrun (delay) on the full set of contract characteristics: *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, Industry FEs, award type FEs, contract pricing FEs, and product service code FEs. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

fications, and on-the-job training, although performance that “contributes to achieving the cost goals, schedule goals, and performance goals” may be taken into consideration for promotion decisions (see United States Code 1703(f)(2)). Given the rigid institutional context, it is a priori unlikely that the link between promotion and performance changes systematically with alignment. Nonetheless, we can directly test for this possibility by combining our personnel and procurement data.

To implement this test, we aggregate our contract-level panel to the procurement officer-year level. This allows us to relate career progression events to cost overruns and delay as well as their interactions with political alignment. We focus on three measures of career progression: promotions (defined as an increase in the officer’s paygrade), demotions (a decrease in paygrade), and exit from the civil service. The results are reported in Online Appendix [Table B11](#). Our two performance measures in these regressions are the average relative cost overrun and the average delay of projects that were completed in the same year, both of which are standardized to have a mean of zero and a standard deviation of one.

We find no evidence that career progression patterns change markedly with alignment. In particular, officers with greater cost overruns or delays do not appear to be less likely to be promoted, and, importantly, we do not observe that the link between our measures of performance and promotion changes systematically with political alignment. The same is true with respect to demotions and exits from the civil service. Taken together, our results suggest that differential promotion incentives are unlikely to be a major driver of the

observed alignment effects.

5.3 Morale effects

We now provide evidence suggesting that a morale effect is an important mechanism behind the effect of political alignment on performance. In our context, morale can be shaped by both intrinsic and extrinsic factors. Working for an aligned administration might motivate bureaucrats to perform if bureaucrats have preferences directly over the policies pursued by the government. In addition, better and more frequent interactions between bureaucrats and their aligned superiors might increase their willingness to exert effort.

To provide evidence consistent with the presence of a morale effect, we make use of the Federal Employee Viewpoint Survey (FEVS). Collected by the Office of Personnel Management (OPM), this survey measures employees' perceptions and attitudes towards their workplace. FEVS is designed to be representative of non-political, non-seasonal federal workers, and repeated cross-sections are regularly drawn in proportion to office size. This electronic survey is administered to both full-time and part-time employees of departments and large independent agencies. The mean response rate is 47%.³⁹ We use data for 2006–2020, for a total of 5,565,930 responses. There are two important limitations to the FEVS. *(i)* It does not elicit partisanship, and *(ii)* survey takers remain anonymous. Together these limitations make it impossible for us to precisely measure ideology for any given respondent. To address this issue, we exploit the fact that there is significant variation in the share of Democrats and Republicans across departments. Similarly, sex and minority status are strongly predictive of partisanship.⁴⁰ We proceed by calculating the share of Democrats in each sex \times minority-status \times department cell in the OPM data. We then use this number as the probability that a FEVS respondent in the same cell identifies as Democrat. We focus on Democrats because they comprise the plurality of civil servants in 90% of cells. We unfortunately do not have sufficient variation in cell composition to separately disentangle effects for Republicans and independents.

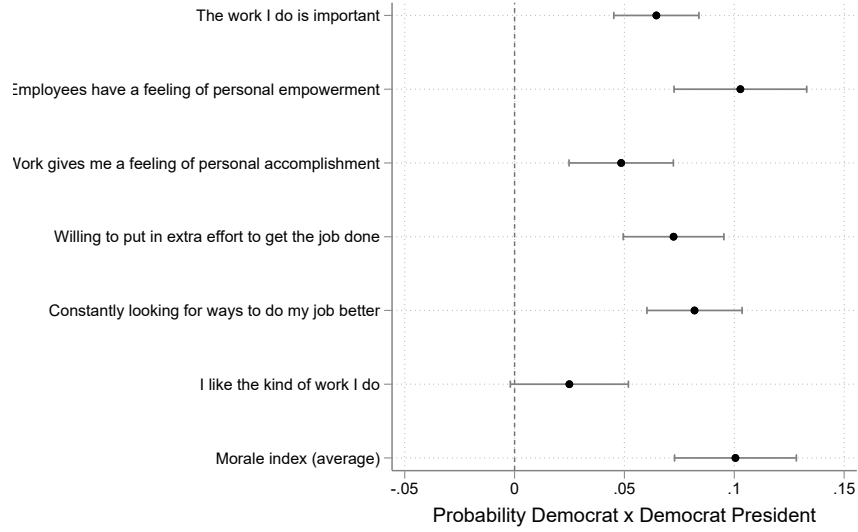
To test whether the political alignment of individual i in year $t = T(i)$ affects her morale and attitude towards her department's mission, we estimate the following regression model:

$$y_i = \beta \cdot \text{Prob. Democrat}_i \cdot \text{Democrat President}_{T(i)} + \tau_{T(i)} + \mu_K(i) + \gamma' x_{iT(i)} + \varepsilon_i \quad (4)$$

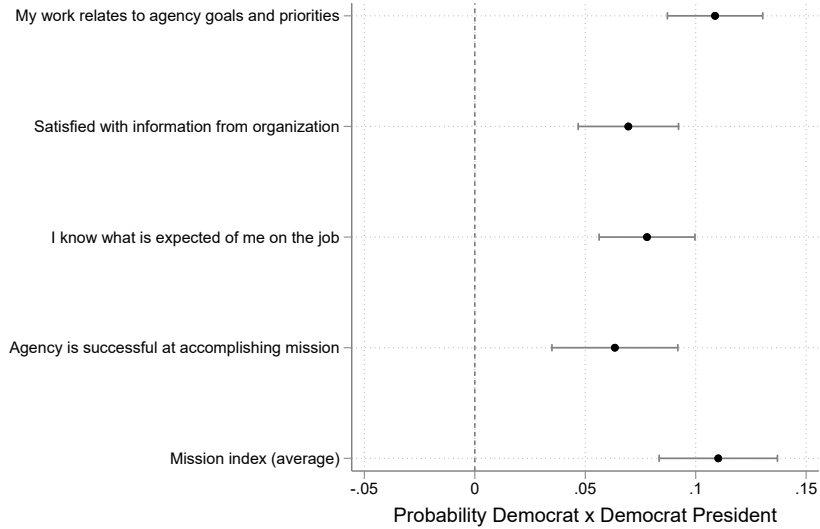
³⁹This number is from the published *Technical Reports* available online for 2008–2019.

⁴⁰In our data, the share of Democrats is 56% among female civil servants but only 43% among men. Similarly, the share of Democrats is 73% among minorities and 40% among non-minority federal employees. The share of Democrats ranges from more than 60% in the Department of Education and in the Department of Housing and Urban Development to 41% in the Department of Agriculture and 38% in the Department of Transportation.

Figure 5: Morale and mission increase with political alignment



(a) Measures of general morale



(b) Measures of identification with mission

Notes: Each row reports the regression coefficient of *Probability Democrat* \times *Democrat President* from Equation 4 for different dependent variables together with 95% confidence intervals, based on standard errors clustered at the Sex \times Minority \times Department-level. All dependent variables are on the Likert scale (1: Strongly disagree, 5: Strongly agree) and standardized to have mean 0 and standard deviation 1. *Morale index* and *Mission index* are averages of all measures in their respective panel. *Probability Democrat* is the share of OPM civil servants who are registered Democrat in a given sex \times minority status \times department cell. The regression table is reported in Online Appendix Table B12.

where y_i captures agreement with different statements on the survey (e.g., “The work I do is important”). These responses are measured on a Likert scale (ranging from 1=Strongly disagree to 5=Strongly agree), which we standardize to have a mean of zero and a standard deviation of one. $Prob. Democrat_i$ is the probability that i is a Democrat (as opposed to a Republican/Independent), and $Democrat President_{T(i)}$ denotes an indicator equals to one if the president in year $t = T(i)$ is a Democrat (as opposed to a Republican). $\tau_{T(i)}$ are year fixed effects, $\mu_K(i)$ are sex \times minority \times department fixed effects, and $x_{iT(i)}$ are department \times year fixed effects.

In [Figure 5](#), we report how civil servants’ self-assessed morale (Panel A) and identification with the mission of their department (Panel B) varies with political alignment. Each row reports the estimated coefficient on the interaction term in the model above (i.e., $\hat{\beta}$) for a different survey outcome.⁴¹ This measures how the attitudes of respondents who are more likely to be Democrat change when they become politically aligned.

Our estimates imply that respondents report higher morale when they are politically aligned with the president (Panel A). For example, they report finding their work more important and being willing to exert more effort. We see similar results across the different measures, and when we use a “morale index”, constructed as the average across individual outcomes. In Panel B, we study the impact of alignment on attitudes towards the organization’s mission. The results suggest that civil servants who have a higher probability to be Democrat are more likely to identify with the mission of their organization when serving under Democratic presidents. The estimates are similar across all measures of identification with the mission. The evidence in [Figure 5](#) thus provides suggestive evidence of a “morale effect” due to political alignment.

To shed further light on the mechanism, Online Appendix [Table B13](#) asks whether the effects on self-reported morale and identification with the organizational mission vary with bureaucrats’ seniority. Interestingly, we find larger morale effects for supervisors, though the difference between supervisors and ordinary bureaucrats is statistically insignificant. Since supervisors tend to occupy higher positions in the organizational hierarchy, one might suspect that they would be more directly affected by political turnover. Finally, morale might be shaped by both intrinsic and extrinsic factors, and our survey questions are likely to capture both. While it is difficult to fully disentangle the relative importance of intrinsic and extrinsic drivers of morale, we can leverage additional questions in the FEVS to provide suggestive evidence that extrinsic factors might also play a role. In Online Appendix [Figure A10](#), we show that alignment correlates with reports of more frequent and constructive feedback from supervisors, and with higher trust. It is important to point out, however, that we do

⁴¹The corresponding regression tables can be found in Online Appendix [Table B12](#).

not know whether the observed effects are due to changes in worker or supervisor behavior (i.e., aligned workers being more likely to seek out advice versus being provided unsolicited feedback). We further note that the magnitude of the estimates in Appendix [Figure A10](#) is considerably smaller than the reduced-form morale and mission effects in [Figure 5](#).

6 Implications for Theories of the Bureaucracy

We now discuss how our findings relate to extant theoretical work on bureaucracies. Previous research has long recognized the importance of non-pecuniary incentives for motivating agents, especially in public sector organizations (see, e.g., [Bénabou and Tirole, 2003](#); [Besley and Ghatak, 2005](#); [Francois, 2000](#); [Prendergast, 2007](#); [Delfgaauw and Dur, 2010](#); [Dal Bó et al., 2013](#); [Forand et al., 2022](#)). Although theoretically well appreciated, there remain subtle but important questions related to the exact nature of the non-pecuniary motives of bureaucrats. [Delfgaauw and Dur \(2010\)](#) and [Dal Bó et al. \(2013\)](#) model “public sector motivation” as a fixed utility from working for the government. By contrast, in [Francois \(2000\)](#), [Besley and Ghatak \(2005\)](#), and [Prendergast \(2007\)](#) agents derive intrinsic utility from the public sector output they produce, while [Forand et al. \(2022\)](#) allow bureaucrats to have policy preferences. Our results on the performance implications of ideological mismatch are not easily rationalized through the lens of models in which bureaucrats and their supervisors only care about working for the government. Our findings are consistent, however, with theories in which agents are, at least in part, motivated by *what* the government produces. This could be because bureaucrats have preferences directly over policies, or because working under an ideologically misaligned administration reduces the marginal benefit from public sector output.

Our empirical work also contains lessons for theories of the internal organization of the bureaucracy. A central conclusion of this literature is that politicians have an incentive to make delegation to and selection of bureaucrats dependent on their type, i.e., their expertise and preferences (see, e.g., [Epstein and O’Halloran, 1994](#); [Bendor et al., 2001](#); [Bendor and Meirowitz, 2004](#); [Prendergast, 2003](#); [Maskin and Tirole, 2004](#); [Alesina and Tabellini, 2007](#); [Gailmard and Patty, 2007](#); [Wiseman, 2009](#)). While ideology-based selection seems to be a good first-order approximation in the upper echelons of the federal bureaucracy, the vast majority of civil servants in the U.S. do not appear to be selected based on ideological alignment with the current administration. This is true for both incumbents as well as new hires. One potential explanation for this finding is that current civil service rules provide little scope for political interference. Another, complementary explanation is that applicants for civil service positions anticipate serving under multiple administrations with

different missions (as in [Forand et al. 2022](#)). If civil servants anticipate having to work for misaligned administrations, then self-selection may mitigate the cost of mission misalignment in bureaucracies relative to the private sector. Yet, in private-sector firms misaligned workers might be more easily re-assigned to different tasks, or they might leave the organization entirely. Our empirical results suggest that both margins of adjustment are very limited in the context of the U.S. civil service. Thus, whether mission (mis)alignment is more important in private- or public-sector organizations remains an open empirical question.⁴²

In sum, our findings support the key idea behind theories with intrinsically motivated bureaucrats. Our results imply caution, however, when it comes to assuming that bureaucrats can be neatly selected based on mission alignment. It is precisely politicians’ limited ability to achieve alignment with low-level bureaucrats that creates the cost of ideological mismatch that we document in this paper.⁴³

7 Conclusion

A central question in the governance of any organization is how to align the objectives of leaders with those of their subordinates. In this paper, we turn to the U.S. federal bureaucracy to study the role of mission alignment in organizations.

To this end, we combine administrative data on the near universe of federal government workers with data on all registered voters in the U.S. The resulting dataset allows us to shed some of the first light on the ideological leanings of a large number of individual civil servants, and thereby peek into the black box of “bureaucratic politics.”

We show that presidents do use the limited power they have over personnel policies in order to achieve greater ideological alignment between themselves and the upper echelon of the bureaucracy. Partisan cycles among political appointees are consistent with the use of the spoils system to better align the highest layers of the bureaucracy with the policy of the White House. By contrast, we find a remarkable absence of partisan cycles among career civil servants. Our findings, therefore, suggest that the bulk of the federal government resembles a Weberian bureaucracy, which is largely protected from political interference.

The existence of an impartial and politically insulated career civil service is often seen as the hallmark of good governance and a Weberian state. While the insulation of the career

⁴²Limited control over misaligned bureaucrats also implies that politicians have an incentive to either circumvent the bureaucracy or find other, indirect ways to achieve their goals (see, e.g., [Iyer and Mani, 2011](#); [Ujhelyi, 2014b,a](#); [Moreira and Pérez, 2021](#)).

⁴³In independent work, [Forand et al. \(2022\)](#) develop an equilibrium model with this feature, and use it to study the emergence of partisanship in modern bureaucracies. The predictions of their theory are remarkably consistent with the results above.

civil service prevents political interference, civil servants may have their own preferences and ideological leanings, which can conflict with those of the president. As a consequence, politicians and department heads often need to work with bureaucrats whose personal values are not aligned with the present mission of the organization.

In order to assess the consequences of such misalignments, we focus on a subset of civil servants who work across all departments of the government and for whom we can measure performance: procurement officers. Linking procurement contracts to the matched personnel and voter registration data allows us to examine the effect of mission-alignment on the performance of procurement officers across nearly all departments of the federal bureaucracy. Strikingly, we find that political misalignment decreases officers' performance, leading to greater cost overruns, ex post modifications, and delays. To the best of our knowledge, these are the first results on the performance implications of ideological misalignment in a textbook bureaucracy.

More speculatively, we provide evidence to suggest that a general morale effect may be an important driver behind our main finding. Taking federal employees' self-reports at face value, bureaucrats who are ideologically misaligned with the White House appear to have lower motivation. Even more speculatively, as more and more organizations embrace a mission-driven focus, our findings may have implications beyond the public sector.

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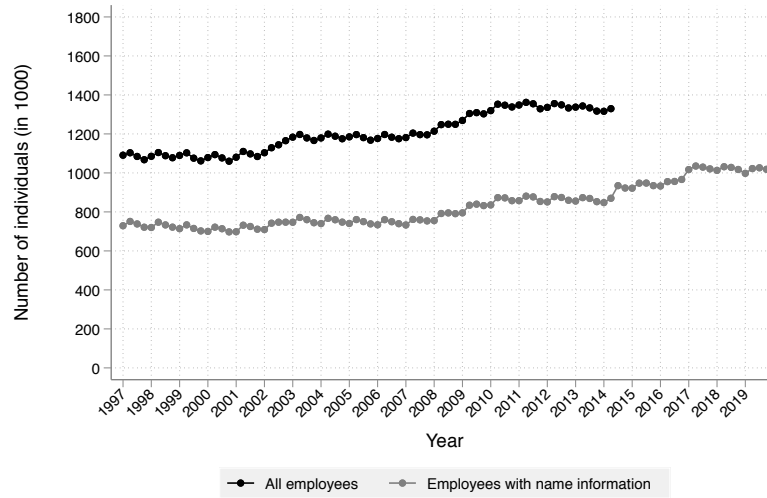
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ONLINE APPENDIX

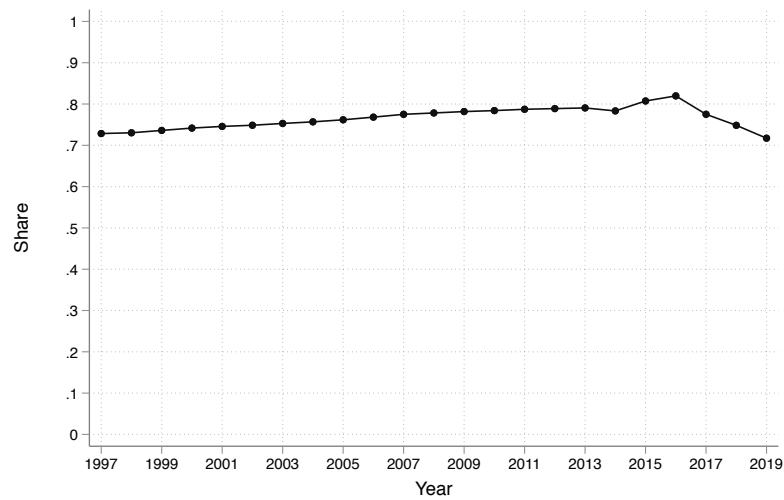
A Appendix Figures:

Figure A1: Number of employees in the OPM over time



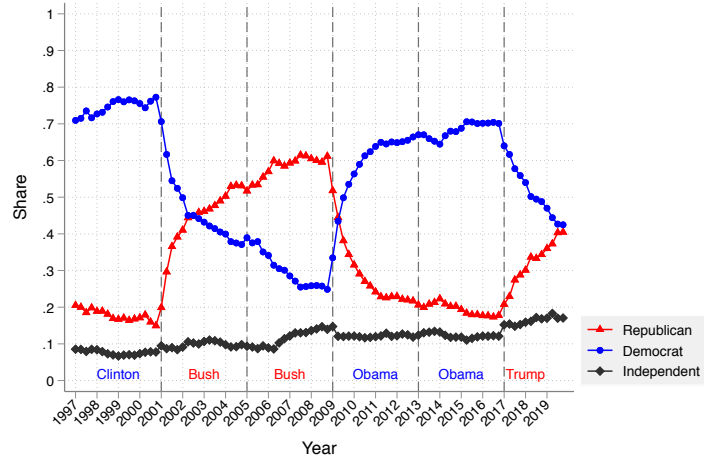
Notes: Showing the number of OPM individuals over time (in 1,000). Black line denotes all employees and the gray line denotes employees for whom names were not redacted. Note that since the OPM does not provide unique identifiers after 2014, we cannot compute the number of unique employees among those with redacted names.

Figure A2: Share of Federal Employees Matched to Partisan Affiliation Data

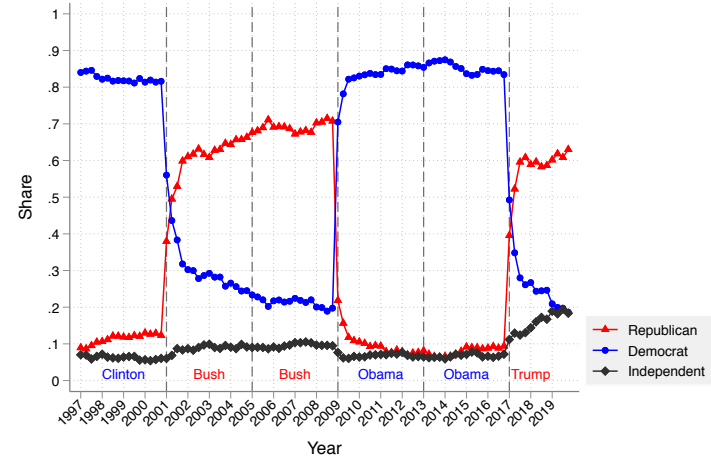


Notes: Share of OPM individuals with non-redacted names who could be matched to the L2 voter registration data over time.

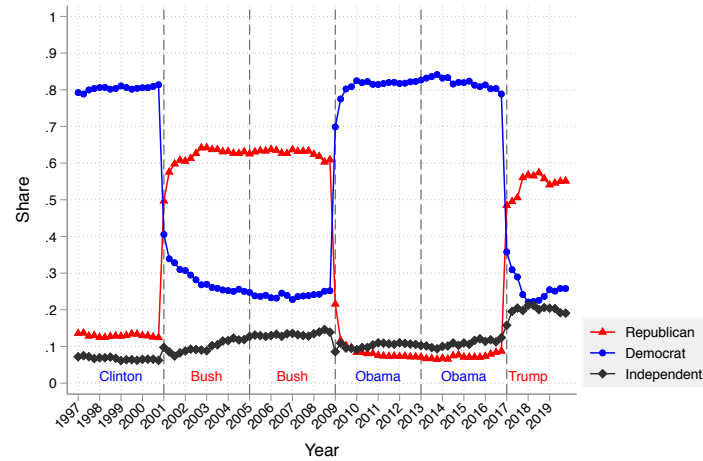
Figure A3: Partisan Affiliation of Political Appointees – By Type



(a) Presidential Appointments



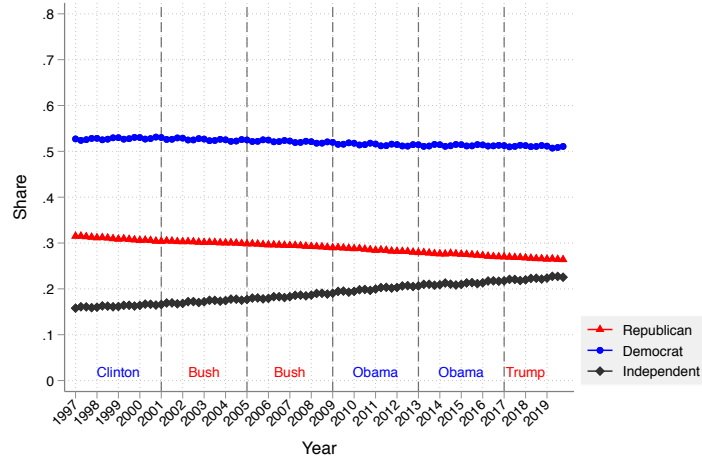
(b) Senior Executive Service - Noncareer



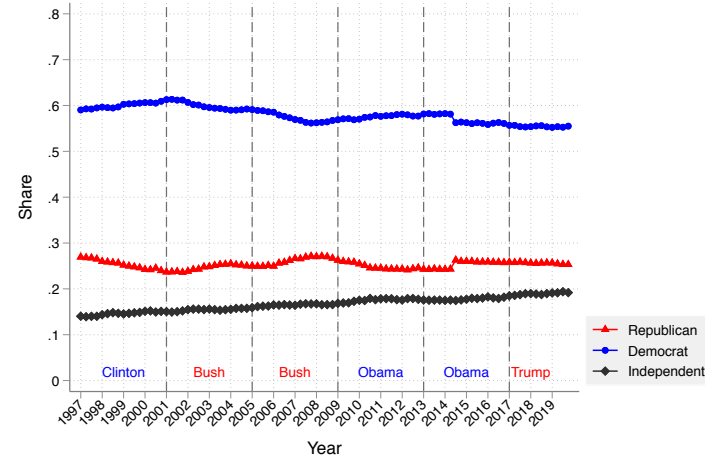
(c) Schedule C

Notes: Party shares for different types of political appointments over time. Panel A shows presidential appointments. Panel B shows non-career senior executive service. Panel C shows Schedule C appointments. Dashed vertical lines mark presidential terms.

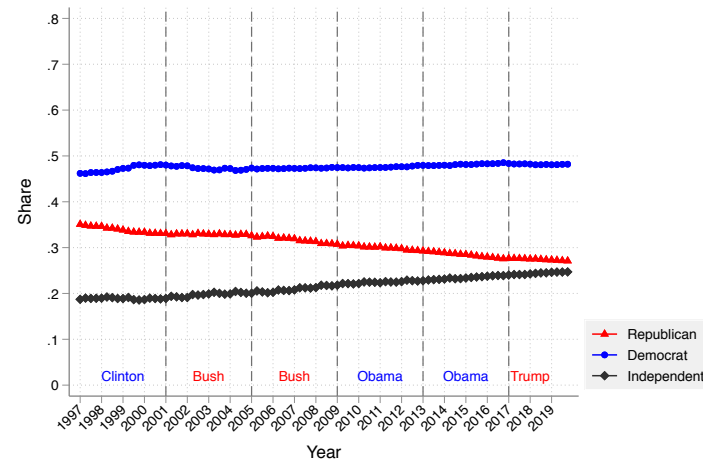
Figure A4: Partisan Affiliation of Civil Servants – By Type



(a) Competitive Career Service



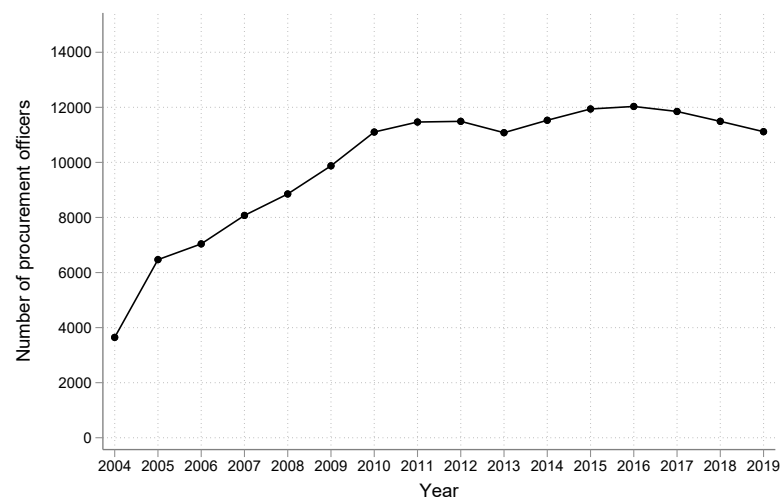
(b) Senior Executive Service - Career



(c) Excepted Service - Nonpolitical

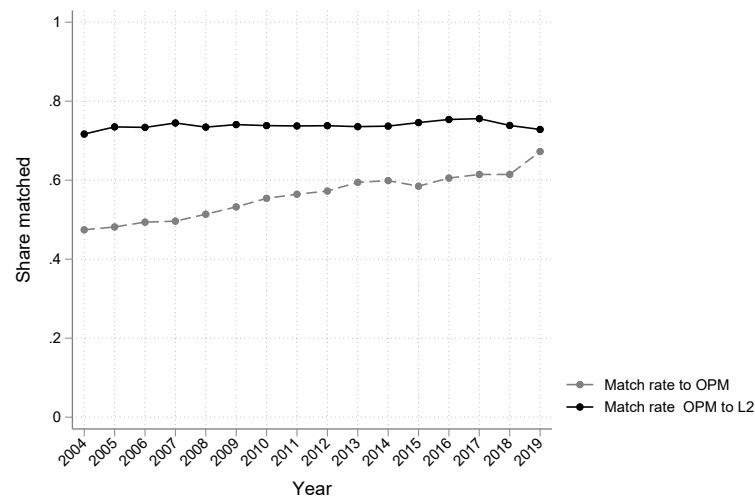
Notes: Party shares for different types of (non-political) civil servants over time. Panel A shows the competitive career service. Panel B shows the career senior executive service. Panel C shows the non-political excepted service. Dashed vertical lines mark presidential terms.

Figure A5: Number of identifiable procurement officers over time



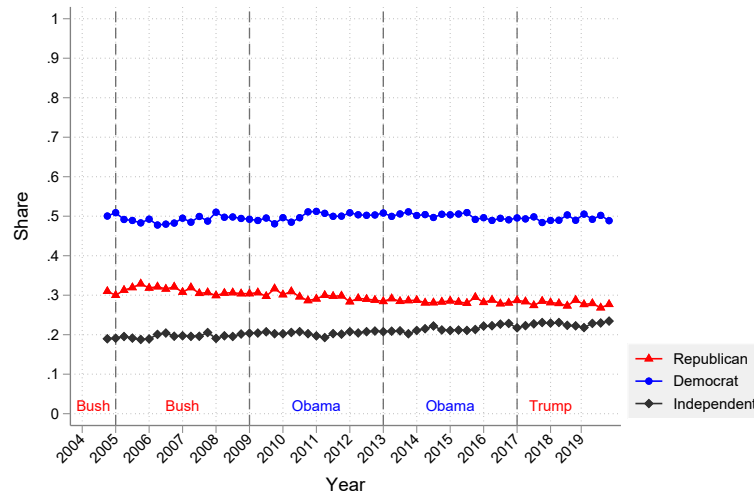
Notes: Number of individually identifiable procurement officers for contracts created in a given year. Contracts to services and works contracts in our analysis sample (see [Table B3](#)).

Figure A6: Share of Procurement Officers matched to Partisan Affiliation Data



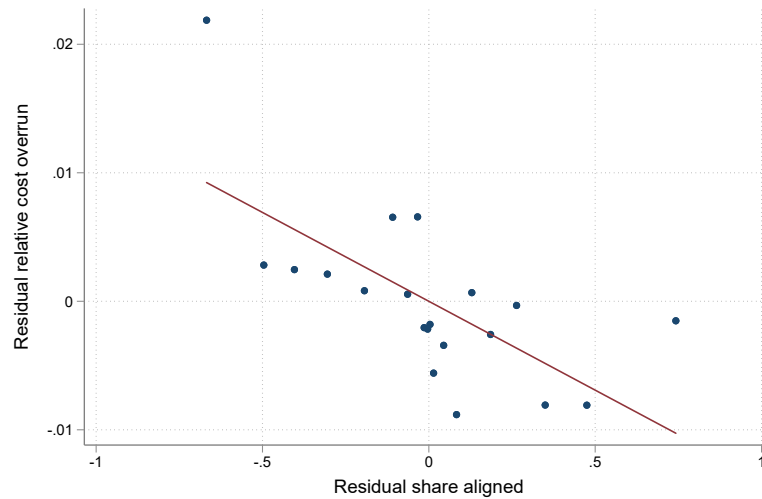
Notes: Black solid line shows share of active procurement officers who could be matched to the OPM. Dashed gray line shows match rate to the L2 voter registration data, conditional on being matched to the personnel (OPM) data.

Figure A7: Share of procurement officers by party affiliation



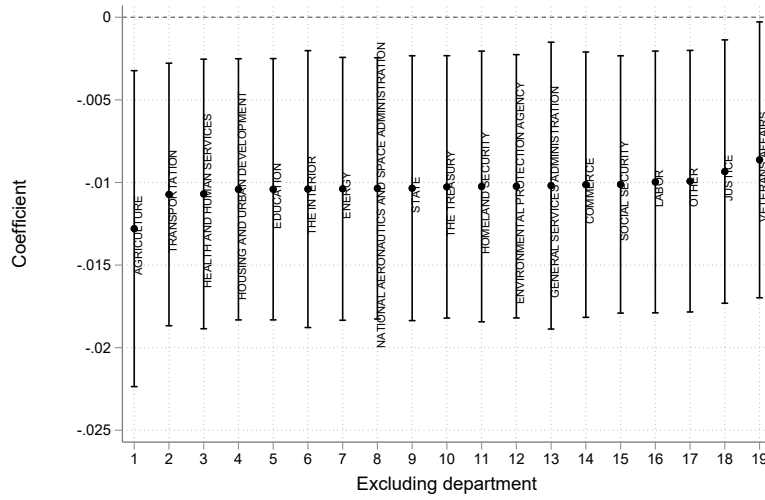
Notes: Share of active procurement officer by party affiliation over time. The party shares for procurement officers closely track the shares for the entire bureaucracy (see Figure 3).

Figure A8: Greater political alignment decreases cost overrun



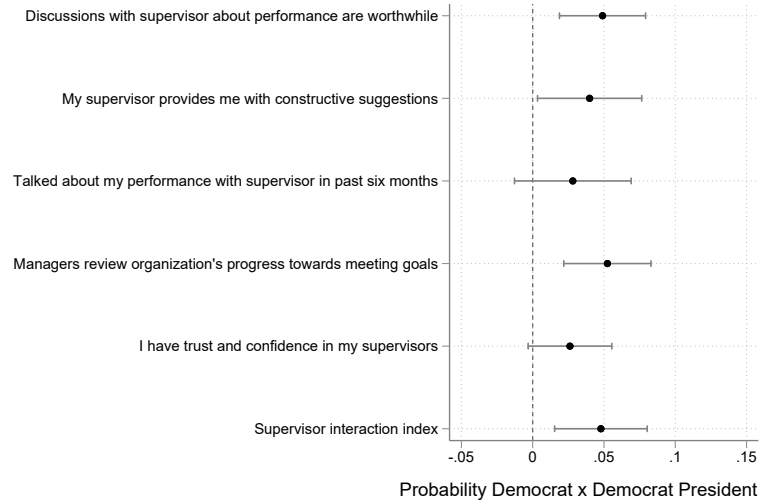
Notes: The figure shows the partial correlation between *Share politically aligned* and *Relative cost overrun* in a bin scatter plot. The relationship shown is after partialing out individual fixed effects and year \times quarter fixed effects (see Table 4, column 4).

Figure A9: Cost overrun and political alignment – dropping one department at a time



Notes: Figure reports point estimates of the political alignment effect (specification from Table 4, column 1 of the paper) dropping one department at a time. Reporting 95% confidence intervals.

Figure A10: Supervisor interaction and political alignment



Notes: Each row reports the regression coefficient of *Probability Democrat* \times *Democrat President* from equation 4 of the paper for different dependent variables together with 95% confidence intervals, based on standard errors clustered at the Sex \times Minority \times Department-level. All dependent variables are on the Likert scale (1: Strongly disagree, 5: Strongly agree) and standardized to have mean 0 and standard deviation 1. *Supervisor interaction index* computes the average of all measures in their respective panel. *Probability Democrat* is the share of OPM civil servants who are registered Democrat in a given sex \times minority status \times department cell.

B Appendix Tables:

Table B1: Political cycles among civil servants – Hiring margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employee is Democrat				Employee is Republican			
Sample:	All Civil Servants	Competitive Service	Career SES	Excepted Service	All Civil Servants	Competitive Service	Career SES	Excepted Service
President Democrat	0.002*** (0.001)	0.002 (0.001)	0.030*** (0.010)	0.007*** (0.001)				
President Republican					0.000 (0.001)	-0.000 (0.001)	0.026*** (0.009)	0.004*** (0.001)
Observations	1,979,703	1,077,837	9,242	1,181,448	1,979,703	1,077,837	9,242	1,181,448
Effect size	+0.5%	+0.4%	+5.7%	+1.5%	+0.2%	-0.1%	+10.1%	+1.5%
Bureau FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Regression estimates of the party alignment effect. The unit of observation is the individual-quarter, restricting the sample to individuals who were hired in the specific category in a given quarter. In columns 1-4, the dependent variable is a dummy that is 1 if the civil servant is a Democrat, and 0 otherwise. In columns 5-8, the dependent variable is a dummy that is 1 if the civil servant is a Republican, and 0 otherwise. *President Democrat* is a dummy that is 1 if the president is a Democrat, and 0 otherwise. *President Republican* is a dummy that is 1 if the president is a Republican, and 0 otherwise. The sample covers all matched individuals between 1997-2019. Columns 1 and 5 restrict the sample to all civil servants, columns 2 and 6 restrict the sample to the competitive career service, columns 3 and 7 restrict the sample to career senior executive service officers, columns 4 and 8 restrict the sample to employees in the non-political excepted service. All regressions include a linear time trend, and bureau fixed effects. The effect size is defined as the estimated coefficient divided by the mean of the dependent variable when the president is Republican (columns 1-4) or Democrat (columns 5-8). Standard errors in parentheses, clustered at the individual-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B2: Political alignment and career progression of political appointees

	(1)	(2)
	Log total pay	
Politically aligned	0.0032 (0.0131)	-0.0044 (0.0085)
Observations	134,351	129,508
Individual FEs	Yes	Yes
Year-Quarter FEs	Yes	
Year-Quarter-Bureau FEs		Yes

Notes: Regression estimates of the party alignment effect on pay. The unit of observation is the individual-quarter. The sample covers all matched political appointees between 1997-2019. The dependent variable is the log annual total pay. *Politically aligned* is a dummy that is 1 if the civil servant and president are from the same party. Standard errors in parentheses, clustered at the individual-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B3: Sample restrictions for procurement contracts

Sample	Mean characteristics			Contracts left in sample
	Size	Duration	Year	
<u>Sample restrictions</u>				
All service & works contracts (excluding R&D) 2004-2019	9.638	4.622	2010.78	7,936,258
Drop Department of Defense	9.277	4.616	2010.78	5,130,057
Drop Indefinite Vehicle Contracts (IDV) [3]	-	-	2010.74	4,853,069
Drop lease and rental contracts [1]	9.266	4.469	2011.26	4,030,893
Drop contracts performed outside the US [1] [2]	9.276	4.513	2011.34	3,791,416
Drop already initialized contracts [3]	9.257	4.495	2011.24	3,646,877
Drop those with missing email addresses	9.236	4.485	2011.45	3,533,846
<u>Matching</u>				
Drop contracts with anonymous creator (e.g., admin@dept.gov)	9.658	4.650	2012.10	2,848,375
Drop those unmatched to OPM (personnel data)	9.713	4.708	2012.41	1,661,268
Drop those unmatched to L2 (voter registration data)	9.729	4.713	2012.44	1,217,148
Drop missing/inconsistent data [1][2][3]	9.833	4.706	2012.62	1,079,923

Notes: Table documents the sample restrictions moving from the full sample to the final analysis sample, reporting the mean characteristics and the number of remaining contracts after each stage. *Size* is the (log) expected contract size, *Duration* is the (log) expected contract duration, and *Year* is the year the contract was initiated. Sample restrictions follow the standard procurement literature. [1] denotes restrictions from [Decarolis et al. \(2020b\)](#), [2] are restrictions from [Kang and Miller \(2020\)](#), and [3] are restrictions from [Carril et al. \(2021\)](#). We do not report mean characteristics for Indefinite Vehicle Contracts as – by definition – they do not have a fixed size and duration.

Table B4: Descriptive statistics – procurement outcomes

	(1)	(2)	(3)	(4)
	Mean	Median	IQR	Obs.
Politically aligned	0.417	0	1	1,079,923
Share aligned	0.414	0	1	1,079,923
Expected obligation (in \$)	90,213.38	16,910.4	65,664	1,079,923
Actual obligation (in \$)	118,211.4	17,544	76,193.6	1,079,923
Expected contract duration (days)	214.59	148	327	1,079,923
Actual contract duration (days)	311.15	199	321	1,079,923
Modifications	1.429	0	2	1,079,923
Terminated ($\times 100$)	0.400	0	0	1,079,923
Competed	0.244	0	0	1,079,923
Number of offers received	3.811	1	2	1,079,923

Notes: Reporting descriptive statistics (mean, median, interquartile range, and total observations) for procurement outcomes and the key explanatory variables. The unit of observation is the contract. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party when the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. *Expected obligation* is the expected contract size at time of initiation, and *Actual obligation* is the actual contract size at time of completion. *Expected contract duration* is the number of expected days of contract duration at time of initiation, and *Actual contract duration* is the number of actual days between initiation and completion date. *Modification* denotes the number of ex post modifications. *Terminated* is a dummy that is 1 if the contract was terminated, rescaled by 100 for legibility. *Competed* is a dummy that is 1 if the contract was awarded by full and open competition. *Number of offers* is the number of offers received by bidders.

Table B5: Alternative measure of procurement performance

	(1)	(2)	(3)	(4)	(5)	(6)
		Cost performance (Decarolis et al. 2020)				
Mean of dep. var	0.907	0.907	0.907	0.907	0.907	0.907
Politically aligned	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)			
Share politically aligned				0.005*** (0.002)	0.006*** (0.001)	0.005*** (0.001)
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes		Yes	Yes
Department \times Year FEs			Yes			Yes
Observations	1,079,773	1,079,773	1,079,773	1,079,773	1,079,773	1,079,773

Notes: The unit of observation is the contract. The dependent variable is the cost performance measure used by Decarolis et al. (2020). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B6: Alternative thresholds for Winsorizing

	(1)	(2)	(3)	(4)	(5)
	Relative cost overrun				
Mean of dep. var	0.365	0.327	0.274	0.186	0.168
<i>Panel A: Political alignment at time of award</i>					
Politically aligned	-0.017*	-0.018**	-0.017***	-0.011***	-0.009***
	(0.010)	(0.008)	(0.006)	(0.004)	(0.003)
<i>Panel B: Share of contract duration politically aligned</i>					
Share politically aligned	-0.028***	-0.026***	-0.022***	-0.013***	-0.011***
	(0.010)	(0.008)	(0.007)	(0.004)	(0.003)
Winsorizing fraction in each tail	0.005	0.01	0.025	0.05	0.075
Year x Month FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Department x Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	1,079,923	1,079,923	1,079,923	1,079,923	1,079,923

Notes: The unit of observation is the contract. *Relative cost overrun* is the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS)* fixed effects, *award type* FEs, *contract pricing* FEs, *product service code* FEs. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B7: Cost-overrun by initial contract size quartile

	(1)	(2)	(3)	(4)	(5)
	Relative cost over-run				
Mean of dep. var	0.186	0.0898	0.127	0.186	0.333
<i>Panel A: Politically aligned</i>					
Politically aligned	-0.011***	-0.002	-0.004	-0.015***	-0.015***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
<i>Panel B: Share aligned</i>					
Share politically aligned	-0.013***	-0.002	-0.004	-0.018***	-0.022***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	1,079,923	267,587	267,951	267,709	268,451
Year × Month FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Department × Year FEs	Yes	Yes	Yes	Yes	Yes
Sample	Full	Quartile in initial contract size			
		1st	2nd	3rd	4th
Cut-off (\$)		[0,5k)	[5k,17k)	[17k,71k)	[71k,∞)

Notes: The unit of observation is the contract. Column 1 includes all contracts, while columns 2-5 include contracts in the first, second, third, and fourth quartile of initial contract size, respectively. *Relative cost overrun* is the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS)* fixed effects, *award type* FEs, *contract pricing* FEs, *product service code* FEs. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B8: Delays by expected duration quartile

	(1)	(2)	(3)	(4)	(5)
	Relative delays				
Mean of dep. var	0.424	0.387	0.506	0.420	0.379
<i>Panel A: Politically aligned</i>					
Politically aligned	-0.003 (0.006)	0.022 (0.017)	-0.004 (0.011)	-0.017* (0.009)	-0.021*** (0.007)
<i>Panel B: Share aligned</i>					
Share politically aligned	-0.005 (0.007)	0.019 (0.017)	-0.005 (0.011)	-0.022** (0.009)	-0.019** (0.008)
Observations	1,074,675	264,771	267,511	208,386	325,104
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes	Yes	Yes
Sample	Full	Quartile in initial duration			
		1st	2nd	3rd	4th
Cut-off (days)		[0,37)	[37,148)	[148,364)	[364, ∞)

Notes: The unit of observation is the contract. Column 1 includes all contracts, while columns 2-5 include contracts in the first, second, third, and fourth quartile of expected duration, respectively. *Relative delays* is the difference between the actual contract duration and the expected duration, normalized by the expected duration (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS)* fixed effects, *award type* FEs, *contract pricing* FEs, *product service code* FEs. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B9: Task assignment and political alignment, complex contracts

	(1)	(2)	(3)	(4)
	Expected		Predicted	
	Contract size	Duration	Overrun	Delay
<i>Panel A: Expected cost \geq \$25,000</i>				
Mean of dep. var	11.78	5.274	0.234	0.522
Politically aligned	0.017 (0.011)	0.001 (0.013)	0.000 (0.001)	0.001 (0.001)
Observations	450,664	450,664	450,664	450,664
<i>Panel B: Expected duration \geq 148 days</i>				
Mean of dep. var	12.01	5.918	0.269	0.462
Politically aligned	0.020 (0.013)	-0.001 (0.007)	-0.000 (0.001)	0.001 (0.001)
Observations	299,877	299,877	299,877	299,877
Year \times Month FEs	Y	Y	Y	Y
Individual FEs	Y	Y	Y	Y

Notes: Unit of observation is the contract-level. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party. *Expected contract size* is the (log) expected size (in USD) of the contract at time of award. *Expected duration* is the (log) expected contract length (in days) at time of award. *Predicted overrun* (*Predicted delay*) is the cost-overrun (delay) predicted by regressing our measure of cost-overrun (delay) on the full set of contract characteristics: *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *Industry* FEs, *award type* FEs, *contract pricing* FEs, and *product service code* FEs. Panel A restricts the sample to only procurement contracts with an expected contract size of at least \$25,000. Panel B restricts the sample to only procurement contracts with above median duration (corresponding to contracts with a projected duration of at least 153 days). Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B10: Procurement performance and supplier selection

	(1)	(2)	(3)	(4)	(5)
Mean of dep. var	Competed	IHS # offers	Provider aligned	Relative cost overrun	
	0.252	1.252	0.0921	0.185	0.185
<i>Panel A: Political alignment</i>					
Politically aligned	-0.004 (0.004)	-0.007 (0.017)	0.002 (0.002)	-0.011*** (0.004)	-0.012*** (0.004)
<i>Panel B: Share aligned</i>					
Share politically aligned	-0.007* (0.004)	-0.002 (0.017)	0.000 (0.002)	-0.013*** (0.004)	-0.014*** (0.004)
Year \times Month FEs	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes	Yes	Yes
Supplier firm FEs					Yes
Observations	1,013,069	1,013,069	1,013,069	1,013,069	1,013,069

Notes: The unit of observation is the contract. *Competed* is a dummy that is 1 if the contract was awarded by full and open competition. *IHS # offers* is the inverse hyperbolic sine transformation of the number of offers received by bidders. *Provider aligned* is a dummy that is 1 if the supplier firm is owned by a minority or disadvantaged women and the procurement officer a Democrat. *Relative cost overrun* is the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party when the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. *Supplier firm FEs* are based on the recipient unique identifiers (DUNS) from the procurement data. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS)* fixed effects, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B11: Promotion incentives of procurement officers do not vary by political alignment

	(1)	(2)	(3)
Mean of dep. var	Promoted	Demoted	Exit
	2.566	0.375	4.629
Current political alignment	0.013 (0.209)	0.134 (0.085)	0.176 (0.342)
Average relative overruns	-0.103 (0.334)	-0.157* (0.092)	-0.097 (0.413)
Average relative delays	-0.299 (0.325)	0.009 (0.148)	0.404 (0.388)
Current political alignment \times Avg. relative overruns	-0.150 (0.350)	-0.097 (0.114)	-0.292 (0.441)
Current political alignment \times Avg. relative delays	0.156 (0.344)	-0.066 (0.132)	0.460 (0.460)
Year \times Month FEs	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes
Party \times Avg. cost overrun & delay	Yes	Yes	Yes
Observations	34,691	34,691	34,691

Notes: The unit of observation is the individual \times year. *Promoted* is a dummy that is 1 if the officer experienced an increase in the pay grade. *Demoted* is a dummy that is 1 if the officer experienced a decrease in the pay grade. *Exit* is a dummy that is 1 if the officer left the civil service in the given year. *Promoted*, *Demoted* and *Exit* are scaled by 100 to ease the legibility of the resulting coefficient estimates. *Current political alignment* is a dummy that is 1 if the procurement officer and president are from the same party in the current year. *Average relative overruns (delays)* are the average relative cost overruns (delays) for contracts that were completed in the given year. Both average contract performance measures are standardized to have a mean 0 and SD 1. *Party \times Avg. cost overrun & delay* comprise the average relative overrun and delay measures interacted with the Democrat and Republican dummies. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B12: Morale and mission increase with political alignment

	(1)	(2)	(3)
	Pr(Dem) \times Dem pres		
	Coeff.	Std. err.	Obs.
<i>Panel A: General morale</i>			
The work I do is important	0.065***	(0.010)	4,075,397
Employees have a feeling of personal empowerment	0.103**	(0.015)	4,025,301
Work gives feeling of personal accomplishment	0.049***	(0.012)	4,619,183
Willing to put in the extra effort to get a job done	0.072***	(0.012)	3,959,941
Constantly looking for ways to do my job better	0.082***	(0.011)	3,964,771
I like the kind of work I do	0.025*	(0.014)	4,088,489
Morale index	0.101***	(0.014)	3,749,545
<i>Panel B: Identification with mission</i>			
My work relates to the agency's goals and priorities	0.109***	(0.011)	4,604,602
Satisfied with information from organization	0.069***	(0.012)	4,626,062
I know what is expected of me on the job	0.078***	(0.011)	4,462,187
Agency is successful at accomplishing its mission	0.063***	(0.014)	4,410,053
Mission index	0.110***	(0.014)	4,325,660
Year FEs	Yes		
Department \times Year FEs	Yes		
Sex \times Minority \times Department FEs	Yes		

Notes: Each row reports the regression coefficient of *Prob. Democrat* \times *Democrat President* from Equation 4 for different dependent variables, where *Prob. Democrat* is the share of Democrat civil servants in the OPM 1997-2019 in a given Sex \times Minority \times Department cell. All dependent variables are on the Likert scale (1: Strongly disagree, 5: Strongly agree) and standardized to have a mean 0 and SD 1. Column 1 reports the estimated interaction effect of *Pr(Dem)* \times *Democrat President*. Column 2 reports the associated standard error and Column 3 reports the total number of observations corresponding to the regression. *Morale index* and *Mission index* are averages of all measures in their respective panel. Standard errors are clustered at the Sex \times Minority \times Department-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B13: Morale, mission, and political alignment by supervisory status

	(1)	(2)	(3)	(4)
	Morale index		Mission index	
Pr(Democrat) \times Dem. president	0.114**	0.071*	0.141***	0.090**
	(0.044)	(0.039)	(0.050)	(0.043)
Sample	Supervisory	Non-supervisory	Supervisory	Non-supervisory
Observations	803,417	2,928,863	931,752	3,375,560
Department \times Year FEs	Yes	Yes	Yes	Yes
Race \times Sex FEs	Yes	Yes	Yes	Yes
Department FEs	All	All	All	All
Test of equality (p-value)		0.318		0.219
Mean of dep. var	0.259	-0.0632	0.214	-0.0516

Notes: Unit of observation is an individual-year. Relating morale and mission-related attitudes from the FEVS survey to political alignment by supervisory status. *Morale index* and *Mission index* are standardized averages of all morale (mission)-related outcomes (see Figure 5 in the paper). *Pr(Democrat)* is the share of OPM civil servants who are registered Democrat in a given Sex \times Minority \times Department cell. Dem. president is a dummy that is 1 if the president in office is a Democrat, and 0 otherwise. Standard errors are clustered at the Sex \times Minority \times Department-level.

C Appendix Documentation: OPM

In this section, we provide additional details on the OPM data, and on the process of matching the data to the L2 party registration data. Specifically, we describe *(i)* two limitations of the OPM data, and how we deal with them, *(ii)* the mapping between “type of appointment” codes in the OPM and our categorization of employees into “political appointees” and “civil servants,” and *(iii)* the matching between the OPM and L2.

C.1 Data limitations in the OPM

The OPM data come with two caveats. The first caveat is that the data do not include information on employees in a number of departments and bureaus. These are: employees in defense and security (Air Force, Army, Navy, Defense, Defense Consolidated Metropolitan Technical Personnel Center, Defense Career Management and Support Agency, FBI, Secret Service, DEA, ATF, CIA, Defense Intelligence Agency, National Geospatial-Intelligence Agency, National Security Agency, Office of the Director of National Intelligence), the U.S. Mint, Foreign Service personnel of the State Department, IRS, U.S. Postal Service, Postal Regulatory Commission, White House Office, Office of the Vice President, Office of Policy Development, Board of Governors of the Federal Reserve, Tennessee Valley Authority, Panama Canal Commission, a number of legislative branch bureaus (Members or employees of Congress, Architect of the Capitol, Botanic Garden, Library of Congress, General Accountability Office, Congressional Budget Office, Stennis Center for Public Service, Office of Compliance), Commission on Security and Cooperation in Europe, Foreign Nationals Overseas, Public Health Service’s Commissioned Officer Corps, and Non-appropriated fund employees. Furthermore, employees in a few occupations (mostly law enforcement officers and nuclear engineers) are excluded, independently of the department where they are employed.

The second caveat of the OPM data is that, starting in the third quarter of 2014, the data do not include employee identifiers, which allow to easily track over time employees with similar names. For this reason, we created employee identifiers for employees appearing after the third quarter of 2014 . We do so on the basis of information on the employee’s full name and education level, which are the two demographics which are included in the data for the full sample period (since we do not have information on age after 2016). Specifically, for each year, we assign the same employee identifier to all observations with the same employee’s full name and education. We can use data for the 1997-2014q2 (which contain identifiers provided by the OPM) period to validate our approach to the creation of identifiers: reassuringly, in the 1997-2014q2 period, around 99% of observations with the same employee

name and education level in a year are assigned the same identifier; similarly, around 99% of identifiers in a year have no variation in employee name and education level (which can theoretically be possible, if an employee changes name or obtains additional training). We then match employees in the 2014q3-2019 period (for which we created personal identifiers) with those in the 1997-2014q2 period (for which we have OPM identifiers) based on full name and education. Specifically, we start by matching employees in the 2014 (for quarters 3 and 4) to 2014 (for quarters 1 and 2); for those employees not found, we match them to employees in 2013; for those employees not found, we match them to employees in 2012; we continue with this procedure up until 1997. We then repeat the same procedure for employees in 2015, 2016, 2017, 2018, and 2019 (namely, employees in each of these years are matched to employees in the previous years).

C.2 Type of appointment codes

Throughout the paper, we differentiate employees between those who are in a position filled by a political appointee, and those in which appointments and removals are formally insulated from political influence. We do so on the basis of the OPM variable “type of appointment”. The mapping between “type of appointment” codes and our categories is as following:

- Presidential appointments in top executive position: code 36 (Executive - Excepted Service Permanent), and code 46 (Executive - Excepted Service Nonpermanent)
- Politically appointed members of the Senior Executive Service (SES): code 55 (Non-career SES permanent), code 60 (Limited Term SES - Nonpermanent), and code 65 (Limited Emergency SES - Nonpermanent).
- Schedule C appointees: code 44 (Schedule C - Excepted Service Nonpermanent).
- Competitive service: code 10 (Career - Competitive Service Permanent), code 15 (Career-Conditional - Competitive Service Permanent), and code 20 (Competitive Service Nonpermanent).
- Career members of the Senior Executive Service (SES): code 50 (Career SES permanent).
- Excepted service: code 30 (Schedule A - Excepted Service Permanent), code 32 (Schedule B - Excepted Service Permanent), code 35 (Schedule D - Excepted Service Permanent), code 38 (Other - Excepted Service Permanent), code 40 (Schedule A - Excepted Service Nonpermanent), code 42 (Schedule B - Excepted Service Nonpermanent), code

45 (Schedule D - Excepted Service Nonpermanent), and code 48 (Other - Excepted Service Nonpermanent).

C.3 Matching

We match federal government employees to the L2 voter registration data using a combination of name, state and county of residence, and age (as of the last quarter in which the employee is observed in the data).⁴⁴ We consider the state and county of employment as an employee’s state and county of residence. We allow for multiple states/counties of residence for the small minority of employees employed in multiple locations. We assign Virginia and Maryland, in addition to D.C., as possible states of residence for individuals employed in D.C.⁴⁵ We perform the matching using only the initials of first and/or midname for the minority of federal employees with only the initials of first and/or midname reported in the data. The OPM reports information on employees’ age using a 5 years age window (starting from 15-19 to 70-74). For employees over 74 (or 64, for some years), the OPM only reports the age window as “75 or more” (or “65 or more”). Therefore, we implement our matching by age by specifying that the year of birth of the individual in the L2 data must be in the 5 years window implied by the employee’s age range window (while for employees older than 65 or older than 75, we only specified an upper bound to the year of birth of the individual in the OPM data).

We implement several steps of matching. First, we match employees to the L2 wave that is closest in time to the year in which we observe the employee in the OPM data, using 8 different combinations of first name, midname, last name, state, county, and age range.⁴⁶ We then repeat each of the steps of matching, allowing employees to be matched to the three L2 waves other than the one that is closest in time to the year in which we observe them in the OPM data. This gives us a total of 16 steps of matching. Importantly, at each step of the matching, we consider as unmatched cases in which a federal employee is matched to multiple records in the L2 voter registration data, or cases in which an individual in the L2 voter registration data is matched to multiple employees.

⁴⁴While in the paper we focus on the period 1997-2019, we also match federal employees using OPM data from 2020 and 2021, which was available at the time in which we performed the matching (July 2022). All the numbers reported in this section pertain only to individuals employed up to 2019.

⁴⁵In our matching procedure, successful matches on state/county are those in which the state/county of residence in the L2 voter registration data is among the employee’s possible states/counties of residence inferred from the OPM data.

⁴⁶Specifically, we match employees appearing in the period 1997-2014 in the OPM to the 2014 L2 wave, employees appearing in the period 2015-2016 in the OPM to the 2016 L2 wave, employees appearing in the period 2017-2018 in the OPM to the 2018 L2 wave, and employees appearing in 2019-2021 in the OPM to the 2020 L2 wave. If an employee appears for multiple periods in the OPM, we match her to each of the closest L2 waves for each period.

Finally, for federal bureaucrats who are still unmatched, we allow for multiple matches with the L2 data: within each step of matching, we can obtain information on partisan affiliation of bureaucrats who are matched to multiple individuals in L2, if all candidate matches share the same party affiliation. For instance, if John Doe, who lives in California and is born in 1958 is matched to multiple individuals in L2 with the same name, state and year of birth, but sharing the same affiliation as democrat, we can confidently assign a democratic affiliation to this federal bureaucrat.

Overall, we are able to successfully match 1,985,726 out of the 2,940,914 bureaucrats in our sample, for a 67.5% matching rate. The table below summarizes our matching steps, and the number and share of employees matched in each step.

Table C14: Matching steps

Variables used	L2 Wave	Number matched	Share of matched
1. first name + midname + last name + state + county + age range	closest wave	525,142	26.45
2. first name + last name + state + county + age range	closest wave	186,896	9.41
3. first name + midname + last name + state + age range	closest wave	402,866	20.29
4. first name + last name + state + age range	closest wave	264,095	13.30
5. first name + midname + last name + age range	closest wave	129,809	6.54
6. first name + last name + age range	closest wave	79,263	3.99
7. first name + midname + last name + state	closest wave	130,084	6.55
8. first name + last name + state	closest wave	52,267	2.63
9. first name + midname + last name + state + county + age range	other waves	9,856	0.50
10. first name + last name + state + county + age range	other waves	3,598	0.18
11. first name + midname + last name + state + age range	other waves	10,115	0.51
12. first name + last name + state + age range	other waves	11,602	0.58
13. first name + midname + last name + age range	other waves	13,791	0.69
14. first name + last name + age range	other waves	12,208	0.61
15. first name + midname + last name + state	other waves	10,264	0.52
16. first name + last name + state	other waves	6,461	0.33
17. Multiple matches sharing same partisan affiliation	closest and other waves	137,409	6.92
Total		1,985,726	100

D Appendix Documentation: Procurement

D.1 Sample selection

Appendix [Table B3](#) summarizes the steps we take to get from the raw data to the final analysis sample. We start with the set of procurement contracts classified as service and works. In contrast to products, these are contract types where the vendor’s effort can influence the outcome post-award, allowing us to construct cost overrun and delay measures ([Decarolis et al., 2020b](#)). These contracts can be identified using product service codes. We follow [Carril et al. \(2021\)](#) and also exclude R&D contracts since they are subject to a unique set of acquisition rules (FAR Part 35). This yields a total number of initial procurement contracts of 7,936,258.

Unfortunately, the OPM data does not provide the names of Department of Defense (DoD) employees. We therefore exclude from the analysis all DoD contracts. This reduces the sample of contracts to 5,130,057. In the next step, we drop indefinite vehicle contracts (IDV). These are contracts where the quantity of the supplies and services is not explicitly defined ex ante, making it difficult to compute reliable measures of overrun and delays. This reduces the number of contracts to 4,853,069.

Following [Decarolis et al. \(2020b\)](#), we exclude lease and rental contracts from the analysis. These are contracts where ex post effort and thus cost-overrun and delays are limited. This reduces our sample to 4,030,893 contracts. We then drop all contracts performed outside of the U.S., leaving us with a sample of 3,791,416. This is another standard assumption that is followed in the literature ([Decarolis et al., 2020b](#); [Kang and Miller, 2020](#)) as the cost structure and contracting rules for non-U.S. contracts differ significantly. Finally, we drop the small number of contracts that were already in process (and for which we thus cannot measure the initial contract size and expected duration). This reduces the sample to 3,646,877 contracts. Finally, we drop those contracts where we have missing e-mail addresses, resulting in a sample of 3,533,846 contracts.

D.2 Matching

To link the 3,533,846 contracts to the personnel data and party affiliation, we use individual identifiers of procurement officers based on their email addresses. Each contract in the federal procurement database contains the email identifier for the individual who created the procurement contract (e.g. JOHN.SMITH@dept.gov), as well as the email addresses of

those who subsequently modified the contract.⁴⁷ We can thus match the officer based on the email address and the corresponding bureau to the personnel data. To increase the match rate, we assign a contract to the first procurement officer for whom we have party affiliation data. In 98% of the cases, this coincides with the officer who initiated the contract.

A limitation in this setting is that not all procurement contracts contain email addresses indicating the names of the assigned procurement officers. Instead, email addresses might only list a code or generic function (e.g. terminal1@dept.gov, admin@dept.gov). Since these contracts cannot be linked to individuals, we omit them from our analysis, reducing our sample to 2,848,375 (see Appendix Table B3). As Appendix Table B3 shows, these contracts with anonymous email addresses tend to be smaller contracts in terms of initial contract size and duration. They also happen to be created earlier in our sample period of 2004-2019. Appendix Figure A5 shows the total number of procurement officers over time. Since contracts are less likely to have anonymous email addresses in the later years, we see a gradual increase in the number of identifiable procurement officers over time. After 2010, the total number of procurement officers is at around 11,000.

While all email addresses list a full surname, we often only have the initial of the first name (e.g. JSMITH@dept.gov). Furthermore, middle names are often omitted, making it difficult to uniquely identify individuals with common last names and first name initials. We therefore use information from the Govtribe.com database, which includes the full names of officers corresponding to a given email address.

We match in multiple steps. In the first step, we match individuals uniquely to those in the personnel dataset based on their exact full name and bureau. As with the matching of the OPM and L2 data, we proceed by using different combinations of the first name, middle name and last name:

- Step 1: first name + midname + last name + bureau
- Step 2: first name + midname initial + last name + bureau
- Step 3: first name + last name + bureau
- Step 4: last name + first name + bureau
- Step 5: last name + first name + midname + bureau
- Step 6: last name + first name + midname initial + bureau
- Step 7: last name + first name initial + bureau

⁴⁷Most of the contracts (79%) are overseen by a single officer, as measured by the number of distinct email identifiers. Almost all contracts (95%) are overseen by less than three procurement officers.

- Step 8: last name + first name initial + midname initial + bureau
- Step 9: first name initial + last name + bureau
- Step 10: first name initial + mid name initial + last name + bureau

In the second step, for those with multiple matches, we disambiguate when possible by matching to the individual whose occupation is explicitly classified as a procurement officer.⁴⁸ Overall, we are able to match 54% of the procurement officers (or 58% of all contracts) to the OPM. As Appendix Table B3 shows, the contracts that could not be matched to the personnel records tend to be smaller (both in contract size and duration) and created earlier. Appendix Figure A6 (gray line) shows the match rate from the procurement identifiers to the OPM data over time.

Finally, we restrict the sample to the 88% of OPM-matched procurement officers who have party affiliation from the L2 dataset.⁴⁹ This reduces the sample of contracts to 1,217,148. In the last step, we drop observations for which data is missing or inconsistent, resulting in a final analysis sample of 1,079,923 contracts. Appendix Figure A7 shows the share of procurement officers broken down by party over time. The pattern closely resemble the results using the full sample of civil servants (see Figure 3). The share of Democrat procurement officers remains around 50% throughout the sample period. At the same time, there is a gradual monotonic decline in the share of Republican officers, which is offset by an increase in independents.

E Appendix: Robustness of results to different sample restrictions

In this appendix, we show that our main results are substantively unchanged if we drop from the sample bureaucrats who (i) are matched to multiple voter registration records, (ii) change party affiliation across different L2 waves, and (iii) are matched to voter registration records in states where L2 models party affiliation. Appendix Table E15 presents estimates from columns 1 and 5 of Table 2 for these different sample restrictions. Appendix Table E16 presents estimates from column 3 of Table 4, applying the same sample restrictions: the

⁴⁸Although the OPM explicitly provides procurement-specific occupation codes, there are also a series of generic clerical occupation codes under which procurement officers are classified. We use the explicit occupation codes of 1102 (Contracting series), 1105 (Purchasing series), 1106 (Procurement clerical and technician series).

⁴⁹Appendix Figure A6 (black line) shows the match rate to the L2 conditional on procurement officers being linked to the OPM over time.

extent of political cycles for political appointees, the political insulation of civil servants, and the effect of alignment on cost overruns are similar in these different samples.

Table E15: Political Cycles Among Political Appointees and Civil Servants - Robustness to different sample restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employee is Democrat				Employee is Republican			
<i>Panel A: Political Appointees</i>								
Sample:	All	Uniquely matched	Constant affiliation	No party imputation	All	Uniquely matched	Constant affiliation	No party imputation
President Democrat	0.494*** (0.008)	0.519*** (0.009)	0.528*** (0.009)	0.514*** (0.012)				
President Republican					0.458*** (0.008)	0.478*** (0.009)	0.499*** (0.009)	0.474*** (0.012)
Observations	139,114	125,662	115,572	75,738	139,114	125,662	115,572	75,738
Effect size	171%	197%	189%	165%	371%	398%	458%	558%
<i>Panel B: Civil Servants</i>								
Sample:	All	Uniquely matched	Constant affiliation	No party imputation	All	Uniquely matched	Constant affiliation	No party imputation
President Democrat	-0.002*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)				
President Republican					0.000** (0.000)	0.001*** (0.000)	0.000** (0.000)	0.000 (0.000)
Observations	58,882,915	55,795,418	51,933,276	37,395,177	58,882,915	55,795,418	51,933,276	37,395,177
Effect size	-0.4%	-0.4%	-0.4%	-0.4%	0.1%	0.2%	0.2%	0.1%
Bureau FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Regression estimates of the party alignment effect for different sample restrictions. Columns 1 and 5 present the main estimates on the full sample (as in columns 1 and 5 of Table 2); columns 2 and 6 drop from the sample bureaucrats who are matched to multiple voter registration records; columns 3 and 7 drop from the sample bureaucrats who change party affiliation across different L2 waves, and columns 4 and 8 drop from the sample bureaucrats who are matched to voter registration records in states where L2 models party affiliation. Panel A restricts the sample to political appointees, and Panel B restricts the sample to civil servants. See the notes to Table 2 for additional details on the estimation. Standard errors in parentheses, clustered at the individual-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E16: Political alignment reduces cost overrun – Robustness to different sample restrictions

	(1)	(2)	(3)	(4)
		Relative cost overrun		
Mean of dep. var	0.186	0.165	0.164	0.159
<i>Panel A: Politically aligned</i>				
Politically aligned	-0.011*** (0.004)	-0.012*** (0.004)	-0.012*** (0.004)	-0.010* (0.005)
<i>Panel B: Share aligned</i>				
Share politically aligned	-0.013*** (0.004)	-0.014*** (0.004)	-0.013*** (0.005)	-0.011** (0.005)
Year \times Month FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes	Yes
Sample	All	Uniquely matched	Constant affiliation	No party imputation
Observations	1,079,923	973,079	913,596	644,549

Notes: The unit of observation is the contract. *Relative cost overrun* is the difference between the actual costs and the expected costs, normalized by the expected costs (see Equation 1). *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party in the year the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Columns 1 is the baseline specification, corresponding to Table 4, Panel A. In column 2, the sample is restricted to individuals to who could be uniquely matched. In column 3, the sample is restricted to individuals who did not change party affiliation over time. In column 4, the sample excludes states in which L2 imputes the party affiliation. Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

F Appendix: Robustness of results to IPW

Despite a match rate of 67.5%, concerns over selection into our data may remain. To address these concerns as much as possible we resort to inverse probability weighting (IPW) (see, e.g., [Horvitz and Thompson 1952](#); [Wooldridge 2007, 2002](#); [Hirano et al. 2003](#)). IPW is a non-parametric procedure by which individual observations are re-weighted according to the estimated probability that they are part of the sample. As [Wooldridge \(2002\)](#) explains, IPW purges estimates of selection bias provided that selection is well captured by observable characteristics.

F.1 OPM

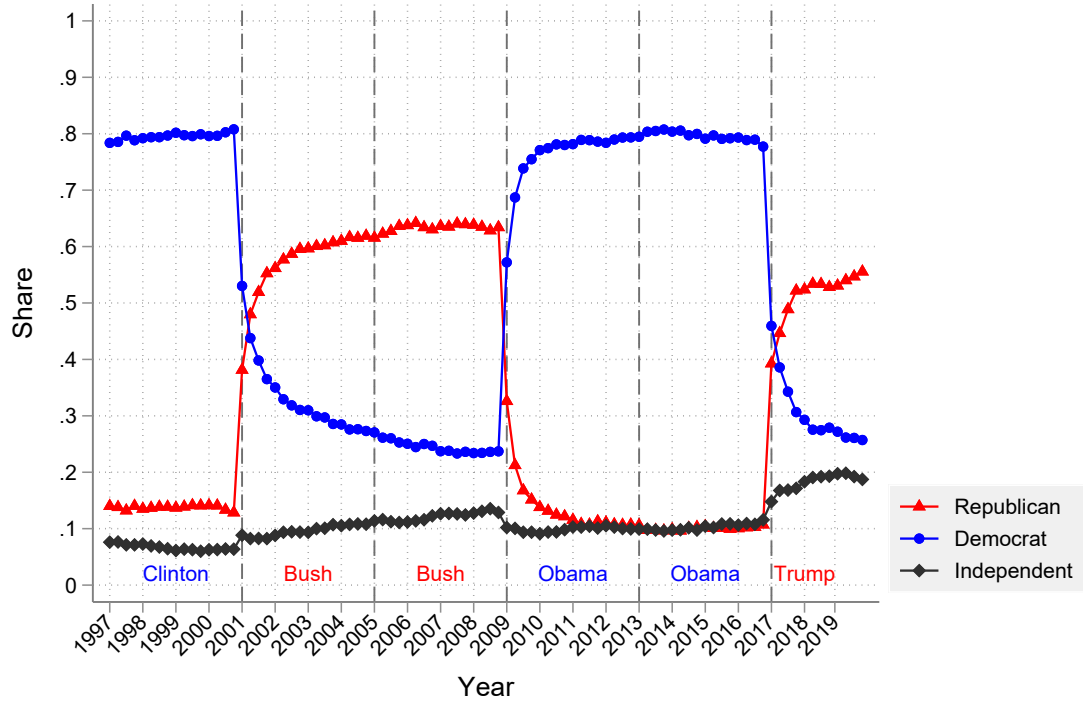
To operationalize this approach, we empirically predict whether a bureaucrat can be matched to our voter registration data based on age (using five bins – less than 30, 30-40, 40-50, 50-60, more than 60), educational achievement (college, more than college), his/her numbers of quarters in the federal bureaucracy, pay (using \$10,000 bins), and an indicator for being employed in D.C. As [Table F17](#) shows, mean differences between the matched and unmatched samples are – by virtue of inverse probability weighting – negligible. More importantly, as [Figure F11](#) and [Figure F12](#) show, relying on IPW to account for selection yields results that are qualitatively equivalent to those in [Figure 2](#) and [Figure 3](#).

Table F17: Average differences in observables between matched and unmatched bureaucrats (OPM) – IPW

	(1)	(2)	(3)	(4)
	Matched		Unmatched	
	Mean	Standard Deviation	Mean	Standard Deviation
Age less than 30	0.433	0.496	0.434	0.496
Age 30-40	0.253	0.435	0.259	0.438
Age 40-50	0.168	0.374	0.168	0.374
Age 50-60	0.108	0.310	0.105	0.306
Age more than 60	0.038	0.190	0.034	0.182
Highest education: college	0.227	0.419	0.227	0.419
Highest education: more than college	0.259	0.438	0.261	0.439
Quarters in federal bureaucracy	38.272	42.904	37.931	42.902
Annual pay	39934.67	33823.35	41235.35	34831.09
Employed in D.C.	0.123	0.329	0.123	0.328
Observations	1,985,726		955,188	

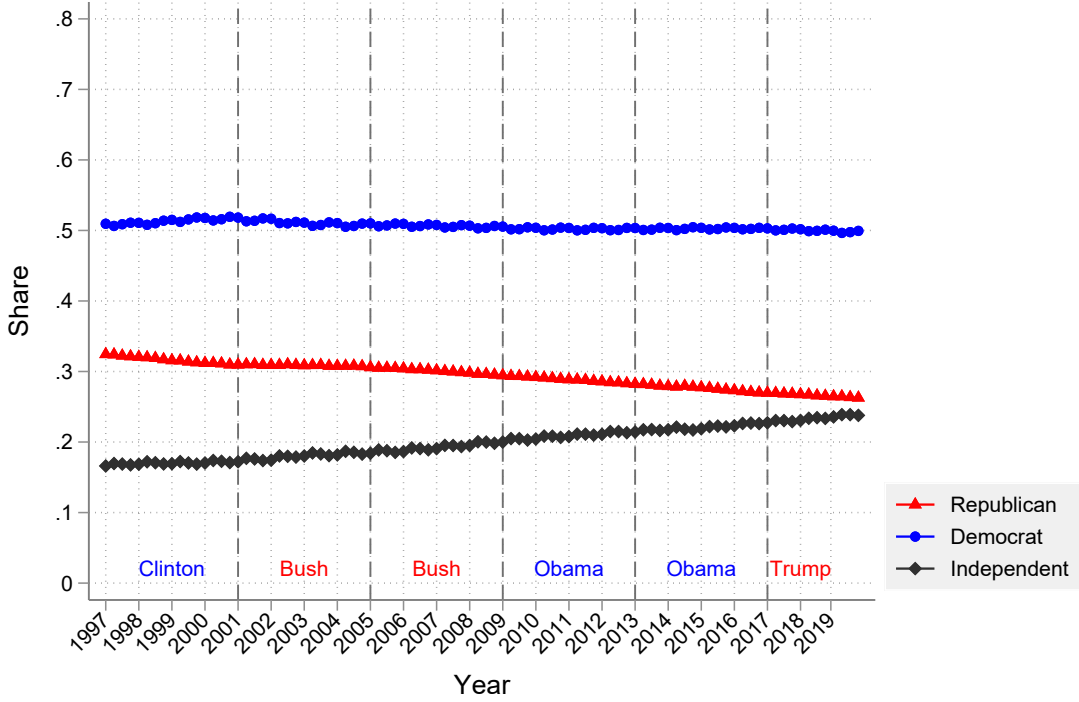
Notes: Descriptive statistics (mean and standard deviation) of individuals for which party affiliation is available (matched, columns 1-2) and for those for which party affiliation is unavailable (unmatched, columns 3-4). Matched sample is reweighted to match the full sample based on the age (five age bins as shown in table), education (college, more than college), quarters in the federal bureaucracy (exact quarters), pay (bins of \$10,000), and being employed in DC. Sample includes all civil servants with non-redacted names serving between 1997-2019.

Figure F11: Partisan affiliation of political appointees – IPW



Notes: Share of political appointees (presidential appointments, non-career senior executive service, schedule C appointees) by party over time. Dashed vertical lines mark presidential terms. Matched sample is reweighted to match the full sample based on the age (five age bins as shown in table), education (college, more than college), quarters in the federal bureaucracy (exact quarters), pay (bins of \$10,000), and being employed in DC. Sample includes all civil servants with non-redacted names serving between 1997-2019.

Figure F12: Partisan affiliation of civil servants – IPW



Notes: Share of (non-political) civil servants (competitive service, career senior executive service, excepted service) by party over time. Dashed vertical lines mark presidential terms. Matched sample is reweighted to match the full sample based on the age (five age bins as shown in table), education (college, more than college), quarters in the federal bureaucracy (exact quarters), pay (bins of \$10,000), and being employed in DC. Sample includes all civil servants with non-redacted names serving between 1997-2019.

F.2 Procurement results

Since we do not have procurement officer covariates for those contracts overseen by officers that are unmatched to the OPM, we reweight the matched sample to be representative of all contracts based on the initial contract size (bins of \$2,500), duration (5 bins), and procurement officer experience (as measured by the years we observe an officer in the procurement data, 5 bins). We coarsen the continuous variables in order to obtain cells with sufficient sample size for reweighting. The results, however, are not sensitive to the particular choice of binning. As [Table F18](#) shows, reweighting the sample effectively eliminates the differences we observe in terms of the covariates on which we match. As [Table F19](#) shows, relying on IPW to account for selection yields results that are qualitatively equivalent to those reported in the main text.

Table F18: Average differences in observables between matched and unmatched contracts (procurement) – IPW

	(1)	(2)	(3)	(4)
	Matched		Unmatched	
	Standard		Standard	
	Mean	Deviation	Mean	Deviation
Log(Initial contract size)	9.655	2.105	9.656	2.072
Log(Expected duration)	4.637	1.515	4.654	1.510
Experience (Years in federal bureaucracy)	3.518	3.070	3.504	3.066
Observations	1,296,564		1,513,768	

Notes: Descriptive statistics (mean and standard deviation) of contracts overseen by procurement officers for which party affiliation is available (matched, columns 1-2) and for those for which party affiliation is unavailable (unmatched, columns 3-4). Matched sample is reweighted to match the full sample based on the initial contract size (bins of \$2,500), expected duration (5 bins), years of experience (5 bins).

Table F19: Cost overrun and political alignment – IPW

	(1)	(2)	(3)	(4)
	Relative cost overrun			
Mean of dep. var	0.186	0.186	0.186	0.186
Politically aligned	-0.01039*** (0.00395)	-0.01022*** (0.00389)		
Share politically aligned			-0.01255*** (0.00409)	-0.01245*** (0.00400)
Year \times Month FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes
Experience (years) FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Department \times Year FEs	Yes	Yes	Yes	Yes
Weighting	None	IPW	None	IPW
Observations	1,079,923	1,079,923	1,079,923	1,079,923

Notes: The unit of observation is the contract. The dependent variable is *Relative cost overrun*: the difference between the actual costs and the expected costs, normalized by the expected costs. *Politically aligned* is a dummy that is 1 if the procurement officer and president are from the same party when the contract was created, and 0 otherwise. *Share politically aligned* is the share of a given contract's expected duration in which the procurement officer and the president were from the same party. Columns 1 and 3 report the unweighted estimates, while columns 2 and 4 report the estimates using inverse probability weighting (IPW). We reweight based on initial contract size (bins of \$2,500), duration (5 bins), and the procurement officer's experience (5 bins). Controls comprise: *Years of experience fixed effects*, *Log(Contract size in USD)*, *Log(expected duration in days)*, *Log(total contracts created in a given year and quarter)*, *industry (NAICS) fixed effects*, *award type FEs*, *contract pricing FEs*, *product service code FEs*. Standard errors in parentheses, clustered at the procurement officer-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.