Supplementary Information to "Loyal Leaders, Affluent Agencies: The Budgetary Implications of Political Appointments in the Executive Branch"

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The Swedish Case

In this section, we provide some additional notes on the Swedish case. Because more general overviews of the Swedish political system can be found elsewhere (e.g. Ahlbäck Öberg and Wockelberg 2016; Bergman 2003; Pierre 1995), we focus our discussion on the core characteristics of the appropriations and appointment processes, while providing some substantive examples along the way.

The Appropriations Process

Our observation period covers two qualitatively distinct budgeting regimes (Hallerberg 2004, 157-168; Molander 2000). Until the mid-1990s, the yearly appropriations process started in September with each individual agency head presenting their ministry with a budget bid for the coming fiscal year. The ministries then processed the requests and, based on the bids from the agencies, approached the Ministry of Finance for funding. Negotiations between the Ministry of Finance and the other ministries usually lasted from September to December, with the cabinet ministers presenting the final proposal to the parliament in January. Once in parliament, the bill would then typically be referred to the various standing committees for comments, before reaching the final passage vote (Mattson 2000; Vinde 1971). The old appropriations process was thus typically bottom-up and initiated by the agencies: no formal budget frames were set in advance; the ministries based their decisions largely on proposals made by the agencies; and there was limited monitoring of agency spending.

The current appropriations process took shape over roughly a ten-year period, starting in the 1980s and culminating with a new comprehensive budget law in 1996. The Ministry of Finance, then led by Kjell-Olof Feldt of the Social Democratic Party, acted as a prime mover for reform, but a parallel initiative with similar intentions started around the same time in the parliament. In the context of a severe economic downturn in the early 1990s, the old regime

faced heavy criticism for being too de-centralized and opaque (Ministry Publication Series 1992; Swedish Government Official Reports 1993). The dour economic climate increased the pressure for reform, and in 1996 the parliament enacted the Central Government Budget Act (SFS 1996:1059). Unlike in the old regime, an expenditure ceiling was now to be defined at the outset of the process, which empowered the Ministry of Finance and forced the other ministries to fund their agencies within a centralized budget that was already set. The parliament changed its decision-making processes to mirror the same procedure: first, decide on the frames for the budget; and then, start the policy discussions in the standing committees. The new parliamentary procedure heavily favored the governing parties, who could now place hard constraints on the committees' policy discussions by first deciding how much money the committees had to work with (Mattson 2000). Above all, the new law clarified the rights and obligations of the cabinet ministers, who were now supposed to set the budgetary agenda, assure bureaucratic performance, and inform the parliament about objectives and results.[1]

[1] A potential concern in this context is whether the economic downturn or reformed appropriations process might somehow bias our analyses. As can be expected, the dour economic climate in the early 1990s had large negative effects on overall public spending. Quite surprisingly, however, the bureaucracy appears to have made it through the downturn largely unscathed (cf. Carpenter and Lewis 2004). In fact, according to our data, total government spending on the bureaucracy steadily increased throughout the 1990s. Similarly, a casual inspection of the data reveals no transformative shifts in spending following the introduction of the new appropriations process (other than the technically induced budget expansion in 1996). The only point during our study period that can be characterized as a proper break coincides with the investiture of the Reinfeldt cabinet in late 2006, who cut public

The reorganization of the appropriations process marked an important shift in Swedish fiscal policy, yet its implications for agency budgets should not be exaggerated. Even before the reform, the ministers could shape the budgetary agenda by giving informal instructions to the agencies prior to the bidding process (Jacobson 1984), and perhaps more importantly, by earmarking budget posts for special purposes and appropriating funds with special spending rules (e.g. forbidding agencies to save part of their resources for the next fiscal year). Moreover, even after the reform, the inputs from the agency heads remain central. The heads are responsible for assuring both that auditors, citizens and politicians can learn what they need to know about an agency's operations, and that subordinate positions act in accordance with the law of the land. Without reliable information and enforcement, the responsible ministries cannot calculate the costs and benefits of different policy proposals and will thus be at risk of misallocating the resources that they have their disposal. The ministries were not impotent in the old regime, and the agencies are not irrelevant in the new regime.

Finally, in light of our arguments, we also note that the governing parties did not try to frame the new appropriations process as a simple technical adjustment, but instead motivated the reform with concerns over distributive outcomes. Though much of Swedish fiscal policy over the past few decades has been aimed at containing national debts and deficits, spurring inflation, and getting the economy back on track from the crash in the early 1990s, the development of the new appropriations process started well before it stood clear that a crisis was looming on the horizon. In fact, when Finance Minister Feldt began promoting a more comprehensive take on fiscal oversight in the 1980s—covering not only detailed reviews of agency budgets and spending estimates, but also of their internal organizations and regulatory

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spending across the board. Finally, note that all of our models include year fixed-effects, which adjust for developments like these by allowing each fiscal year to have a distinct intercept.

activities—he emphasized that the purpose of the new monitoring regime was not primarily to help the government save on scarce resources, but rather to provide more opportunities for strategic targeting and redistribution.[2] Although we focus on statistical analyses in the article, we thus emphasize that our assumptions regarding the ministers' allocative decisions can also be substantiated with historical evidence.

The Appointment Process

The Swedish government treats the decision to appoint an agency head similarly to any other executive decision. Legally speaking, the "appointment process" exists largely as a set of general constitutional principles that demand meritocratic recruitment, on the one hand, and collective decision-making among the cabinet ministers, on the other.[3] Each individual minister is *de facto* responsible for keeping track of potential vacancies within their portfolio. They also handle all the preparations for new nominations. The recruitment procedure can include an open call for applications, but can also proceed entirely behind closed doors. Once a suitable

[2] Government Bill 1987/88:150, Appendix 1, p. 69. Strictly speaking, the centralization of oversight responsibilities in the Ministry of Finance had been on-going process at least since the 1960s, but it was not until the 1980s that the regime became truly comprehensive in the sense of covering the entire bureaucracy.

[3] The Swedish Instrument of Government explicitly stipulates that all government decisions are taken collectively at formal cabinet meetings (ch. 7, art. 3) and that all bureaucrats are appointed on meritocratic principles (ch. 12, art. 5). Additionally, all agencies are also formally independent in the sense that no one but the agency itself is allowed to dictate how the agency should rule in specific cases or implement the law (ch. 12, art. 2). See Ahlbäck-Öberg and Wockelberg (2016) for a discussion.

candidate has been identified, however, he or she must also be confirmed by the other ministers at a formal cabinet meeting. In general, the parliament plays no part at either the nomination or the confirmation stage of the process.[4] Because of the sparse procedural regulations, the cabinet ministers typically enjoy a good deal of latitude in deciding both who to hire and how they should go about in hiring them.

Once the ministers decide to appoint an agency head, however, the situation changes quite dramatically. Formally, the ministers make use of three types of employment contracts for the agency heads, all of which include "for-cause" protections against early dismissals (Swedish Government Official Reports 1985, 2011). The first and by far most common type of contract covers a fixed term and requires a mutual agreement between the employer and employee to be broken. In other words, once an agency head enters office, neither the ministers nor the agency head have the legal authority to terminate the contract unilaterally. The second type of contract also covers a fixed term but, unlike the first type, permits the agency head to terminate the contract on their own. In an overview from 2011, however, only one agency head served under such conditions (Swedish Government Official Reports 2011, 70). The third type of contract is not strictly speaking a fixed term, but rather covers the period up until retirement, which is currently set at age 67. Like the second type of contract, the third type can also be terminated unilaterally by the agency head. Historically, however, the ministers have reserved the retirement-type contracts for agencies with quasi-judicial duties. Only two agencies in our sample are affected: the Chancellor of Justice; and the Swedish Prosecution Authority. Notably, save for these latter retirement-type contracts, the term-lengths are not subject to any kind of formal regulations. Instead, the ministers have developed an informal practice wherein the heads initially serve on a fixed six-year term, but with a possibility for three-year

[4] The exception concerns a few agencies that serve under the parliament, such as the Riksbank.

extensions. The practice is also reflected in our data, where the mean and median appointment durations are both approximately six years long. While the cabinet ministers enjoy a good deal of latitude in deciding who to hire, then, their opportunities to renege on prior appointments are heavily restricted by more general contract and labor laws.

The basic organization of the appointment process has been more stable than the appropriations process, but still seen some notable adjustments over time. Around the same time as the parties started debating the merits of the appropriations process, they also started debating the merits of the appointment process. One salient issue revolved around the partisan affiliation of the agency heads. The liberal-conservative coalition that entered office in late 1976 after forty years of social democratic rule claimed that their initiatives were hampered by a social democratic bureaucracy (Pierre 1995). When the social democrats returned to power in late 1982, they expressed similar apprehension against the bourgeoisie ideology now allegedly imprinted in agencies central to the social democratic policy agenda (Rothstein 1996). The governing parties of course usually deny that their appointment decisions are guided by anything but relevant merits and competence, but the opposition parties often disagree (e.g. the Committee on the Constitution 1997). Another salient issue concerned the sheer volume of bureaucratic activities that had emerged over the past few decades. During their respective terms in the early 1980s, both blocs independently commissioned evaluations of the government's steering capacity (Swedish Government Official Reports 1983, 1985). The commissions concluded that, although the size and complexity of the bureaucracy had indeed made it more difficult to govern, the governing parties faced no inherent constitutional or practical problems to clarify hierarchical relations, revoke powers that had previously been delegated, or increase their influence over powers that remained delegated—if they so desired.

The changes that followed were spearheaded by the social democrats and would eventually lead to the same kind of New Public Management doctrines embraced by many

other countries of the era (Hood 1991). In the context of our study, one particularly important part of the package was Government bill 1986/87:99, which introduced a number of new escape clauses that the ministers could invoke to transfer agency heads to other positions, if deemed necessary for "organizational reasons" or "the greater good of the agency" (ch. 7, art. 10). Since the implementation of the transfer reform, the parties in power have made occasional use of temporary positions in the Government Offices to buy-out particularly resistant agency heads. The consequences of this change have by no means been dramatic, however. The ministers still require for-cause justifications to transfer an agency head from their original post, and the parliament's Committee on the Constitution is generally attentive to any attempts at circumventing the employment regulations, along with the media. While the politicization of the agency heads is widely acknowledged, it is important to keep in mind that, overall, the Swedish bureaucracy still ranks as one of the most professional and least corrupt bureaucracies in the world (Dahlström and Lapuente 2017).

Finally, in light of our arguments, we also note that the ministers did not try to frame the new escape clauses as a simple technical adjustment, but instead motivated the reform with concerns over the bureaucracy's policy priorities. While expertise has naturally played a central role in the public debate over agency personnel management throughout the observed period, the ministers did not present the professional competence of agency leaders as either a goal or an obstacle at the time of reform. Instead, in Government bill 1986/87:99, Minister of Civil Affairs Bengt Holmberg explicitly stated that the purpose of increasing control over executive appointments was to assure that agency operations were aligned with political agendas and contributing to the production of satisfactory policy outcomes (p. 11). As in the case of the appropriations process, the core components of our arguments are thus not taken out of thin air, but have figured prominently among central Swedish policy-makers and been used to justify organizational reforms of the Swedish bureaucracy.

Data

In this section, we provide some additional notes on our data and coding procedures. Table A1 reports descriptive statistics for our variables, while Table A2 reports the covariate balance. Note in particular that: our growth measure includes a number of extreme outliers; we have around 10-14 percent missing values on our agency head covariates; and most of the covariates are approximately balanced across our ideological conflict variable. Because we have relied on different sources when assembling the appointments and appropriations data, we discuss the contents and limitations of each dataset separately.

The Appropriations Data

The appropriations data cover all administrative agencies with their own lines in the national budget. Our general methodology for charting the individual allocations has been as follows: first, we have identified all agencies with their own formal authorization in the Swedish Code of Statutes during the observed period; and then, we have examined official records of government budgets to determine the amount of funds allocated to each agency during each fiscal year (*Specifikation av utgifter på statens budget*, 2000-2014; *Statsbudgetens utfall*, 1980/81-1999; *Budgetredovisning för budgetåret*, 1970/71-1979/80). The data covers the period between fiscal years 2014, which marks the liberal-conservative Reinfeldt cabinet's final year in office, and 1970/71, which marks the beginning of the second term for the social democratic Palme cabinet.[5]

Substantively, what we refer to as an "agency budget" should generally be understood as a summary measure of an agency's operating costs (insurance, marketing, offices, personnel

[5] Fiscal year 1970/1971 also saw the introduction of a new reporting praxis for the government budget, making it difficult to compare with prior years.

Table A1. Descriptive Statistics.

Covariates	*Obs.*	*Mean*	*Std. Dev.*	*Min.*	*Max.*	*Missing*
Budget growth	6555	45427	1834794	-100	1.29e+08	-
Ideological conflict	6555	0.32	0.47	0	1	-
Academic sector	5621	0.35	0.48	0	1	14.3 %
Political sector	5863	0.34	0.47	0	1	10.6 %
Private sector	5803	0.09	0.29	0	1	11.5 %
Public sector	5830	0.60	0.49	0	1	11.1 %
Advisory council	6555	0.26	0.44	0	1	-
Management Board	6555	0.60	0.49	0	1	-
Conflicting creator	6555	0.40	0.49	0	1	-
(log)Budget level$_L$	6555	17.80	2.58	7.11	24.19	-

Table A2. Covariate Balance (Estimation Sample).

Covariates	*Ideological conflict*		*Balance*	
	1	*0*	*Standardized difference*	*Variance Ratio*
Academic sector	0.35	0.35	0.00	1.00
Political sector	0.32	0.33	-0.02	0.98
Private sector	0.08	0.09	-0.03	0.91
Public sector	0.62	0.61	0.02	0.99
Advisory council	0.26	0.27	-0.01	0.99
Management Board	0.62	0.62	-0.00	1.00
Conflicting creator	0.59	0.32	0.57	1.12
(log)Budget level$_L$	17.92	17-96	-0.01	1.09

Notes. Ideological conflict shows the mean values of the covariates under each level of ideological conflict. Balance shows the standardized difference and variance ratio of the covariates, where perfect values would correspond to 0 and 1, respectively.

training, travel, etc.). The measure includes: (1) the amount of funds initially allocated to an agency at the start of a given fiscal year; (2) any potential amendments made to an agency's funds throughout the remainder of the fiscal year; and (3) any potential unspent funds from an agency's previous fiscal years. This way of measuring agency budgets corresponds to how the Ministry of Finance calculates the agencies' annual grants, and the standing committees in the parliament rely on the resulting values when assessing bureaucratic expenditures. Because of how the ministers have decided to organize the appropriations process and resulting budget records, however, the data also comes with some caveats:

1. Some agencies have their budgets reported collectively, which means that we are unable to match individual appointments with individual appropriations for those agencies. These agencies can be divided into four sub-categories: county administrative boards; museums; police authorities; and universities prior to 1991. Of the agencies with joint funding, 26 are county administrative boards, 13 museums, 23 police authorities, and 26 universities. We have coded all of these agencies as missing in our data.
2. Self-financed agencies, such as government-owned businesses and agencies funded by fees, can sometimes operate without financial support from the government. Our sample includes 8 government-owned businesses and 10 agencies funded entirely by fees, which we have coded as missing.
3. Some agencies receive their funding through other agencies. This category is mainly constituted by small committee-like organizations, often without any permanent staff and that assemble only to make certain specific decisions on behalf of the parent agency. In total, 184 agencies are funded in this way, making it our by far largest category of missing agencies.
4. We have also identified 36 agencies that do not appear to share any notable characteristics, other than that they are missing from the government budget. As far as we understand, these agencies do not actually have any permanent funding, but instead receive their funds conditional on some program appropriation. We discuss the distinction between agency and program appropriations in more detail below.

It is difficult to say how the missing agencies might affect our results, but some of them are likely to be relevant for our analyses. For example, the county administrative boards are typically led by former politicians, while the universities are typically led by career bureaucrats (Dahlström and Niklasson 2013). We thus miss out on two relatively large sets of agency heads

that tend to be highly politicized and professionalized, respectively. On the other hand, the agencies that we have identified under point #3 are not exactly missing, but simply merged with other agencies. However, because the sub-agencies can have different appointees than the parent agencies, we might still miss out on some theoretically relevant relationships in this category.

The most notable issue in our view, however, concerns the budget's distinction between agency and program appropriations. In addition to the operating costs that we measure, some agencies can also have access to other funds through more targeted policy initiatives (e.g. cash transfers to specific groups) and redistributions that occur further downstream in the policy process (e.g. some agencies may have it as part of their mission to redistribute funds to other agencies). Let us take the Swedish Environmental Protection Agency as an example. The government budget defines a unique appropriation for the SEPA, which we use as our budget measure. In reality, however, the budget draws this number from a lower and much more detailed level of financial accounts. In these lower-level accounts, the SEPA can sometimes be assigned both a certain set of "administrative funds", which is what the government budget reports as the SEPA's annual appropriation, and a certain set of "program funds", which the government budget reports as a separate program. In 2014, for example, the budget includes one line for "the Swedish Environmental Protection Agency" and one line for "International Environmental and Nuclear Security Collaboration with Russia". If one were to examine the contents of the latter program in the lower-level of accounts, however, one would find that it includes funding for the Swedish Environmental Protection Agency, the Swedish Radiation Safety Authority, and the Legal, Financial and Administrative Services Agency. According to our interviews with budgeting officials, the reasoning behind combing these three sub-categories into one program appropriation (as opposed to including them in the appropriations for each agency) is that it allows readers of the budget to get an idea of how much the

government spends on, in this case, environmental and nuclear security collaborations with Russia. The downside is that it makes it more difficult to get a clear sense of exactly how much money an agency is administering in a given year. The interviewees were unable to explain why the government chooses this prioritization for some issues but not others.

The fact that our budget measure focuses on the agency appropriations has several implications for how to read our study. A major benefit with the agency appropriations is that they are relatively straightforward to trace, substantively important for an agency's operations, and comparable within as well as between agencies. However, because we eschew the program appropriations, our study also carries a more natural affinity with the literature on "bureaucratic politics" than with the literature on "distributive politics", with our budget measure being more akin to an indicator of the amount of authority the winning legislative coalition is willing to delegate than an indicator of pure policy benefits. Although our arguments build on both of these literatures, we thus emphasize that our research design tilts more towards the organizational aspects of delegation than real distributive outcomes, reflecting what we believe to be a sensible tradeoff between substance and feasibility.

The Appointments Data

The appointments data cover the highest-ranking officials in all agencies with their own formal authorization in the Swedish Code of Statutes, which we refer to as "agency heads" for convenience. We have used the same general methodology for mapping agency heads as when mapping the agency budgets: first, we have identified all agencies with their own formal authorization in the Swedish Code of Statutes during the observed period; and then, we have examined official records of public personnel to determine the highest-ranking official in each agency during each fiscal year. We have mainly relied on two public sources when assembling the data: *Sveriges statskalender*, which is a government-published yearbook with information

on operative public organizations and employees; and *Vem är det?*, which is a Swedish biographical dictionary (similar to the British "Who's Who?"). We have collected information on the heads for the period 1960-2014 but, as previously discussed, here we focus on the period 1971-2014 in order to align the appointments data with the appropriations data.

While we have managed to identify the heads of all agencies with their own budget lines, the data also comes with two caveats:

1. The government yearbooks do not specify exact appointment dates, but instead simply present a cross-section of leading agency personnel in March/April. While the vast majority of observations should be unaffected by this limitation, because we do not know exactly when one head exits and another enters, it does cause some precision issues during replacement years.
2. The biographical dictionaries do not always contain comparable information, which can sometimes make it difficult to determine whether a covariate should be coded as missing or zero. In cases where we have been unable to verify a head's professional background, we have coded the covariate as missing.

Regarding the first issue, because our "ideological conflict" variable is based on partisan shifts in government, we have spent considerable efforts on validating the data against alternative sources during turnover years in order to ensure that each appointee is correctly attributed to a given executive cabinet. This includes examining more detailed but less systematic government publications, such as announcements of new hires. Regarding the second issue, we have supplemented the information from the dictionaries with CVs acquired through personal communications with agency staff, which has enabled us to fill in some (but not all) of the missing values.

The timing of the appointments data relative to the appropriations data is also important to keep in mind for our combining of the two datasets. As we described above, in the old

appropriations process, the government typically proposed a budget to the parliament in January, which aligns fairly well with our snapshot of agency heads. In the current appropriations process, the ministers typically propose a budget in September, which creates a more notable gap between the cabinet's budget planning and our appointments data. However, because the ministers have always had the right to update the budget in various ways throughout the fiscal year, in either case we are left to assume that the heads we measure are also the heads that the winning legislative coalition had in mind when allocating resources. While this assumption should be fairly innocent for the bulk of our data, because the ministers may sometimes adjust the budget bill either in rational anticipation of a future replacement or in strategic response to a prior replacement, we note that our matching procedure may suffer from some imprecision during replacement years.

Alternative Model Specifications

In this section, we provide outputs from a set of alternative model specifications. As we noted in the article, while our models are fairly flexible, we also suffer from some missing values on the covariates, extreme outliers on the outcome, and imbalances between the treated and untreated units. To assure that our results do not hinge on arbitrary modeling choices, we have also estimated models with multiple imputation of the covariates, different range restrictions on the outcome, inverse probability weighting on the treatment, and lagged instrumentation. We briefly present results from some alternative model specifications below.

In Table A3, we first present the full regression outputs from the article models. Although we included a variety of covariates in our model specifications, most of them have no real impact on our results. This should come as no surprise. As we mentioned in the manuscript (and showed in Table A2), most of the covariates are approximately balanced across our ideological conflict variable. In practical terms, this means that we are comparing

cases that are mostly like-for-like, at least with regards to the background factors that we have access to. The specific exception is a covariate we refer to as "Conflicting Creator", which denotes whether an agency was created by an ideological opponent of the winning legislative coalition.[6] To illustrate the covariates' limited relevance, we also present one model that includes the year- and agency- effects only ("Empty Model"), one model that includes the year- and agency-effects along with the creator covariate ("Creator Model"), and one model that includes the year- and agency-effects along with the board covariate ("Board Model"). Notice that the coefficient on Ideological Conflict is quite similar across both the fully specified Model 1 and the more limited specifications. We of course cannot guarantee that we have measured all relevant background factors. But given that perfect balance typically requires proper randomization, we believe the lack of correlations between the covariates and our conflict measure speaks quite strongly in favor of our research design.

In Table A4, we replicate the three models presented in the article, except we also use multiple imputation by chained equations to fill in the missing covariate values. Because the agency heads can differ on a variety of dimensions other than their ideological relations to the governing parties, we made sure to include a variety of measures of the heads' professional backgrounds. As we showed in Table A1, however, a problem with these covariates is that they all suffer from a non-trivial amount of missing values. Because we do not know exactly why the missing values are missing, furthermore, it is also difficult to justify any particular

[6] Because the parties that create an agency will also typically appoint the agency, we also run into some unavoidable correlation issues here, particularly during the first few years of an agency's existence. For example, if we observe an agency in its third year and see that it was created by an ideological opponent of the winning legislative coalition, then the same will almost certainly hold true for the appointment of the agency head.

Table A3. Article Regression Results.

Variables	*Model 1*	*Model 2*	*Model 3*	*Empty Model*	*Creator Model*	*Board Model*
Ideological conflict	-1.79	-1.96	-1.69	-1.90	-1.90	-1.94
	(0.58)	(0.53)	(0.79)	(0.53)	(0.54)	(0.54)
Academic sector	-1.22	1.38	3.93			
	(0.94)	(0.60)	(1.47)			
Political sector	-0.71	-0.20	-0.77			
	(0.69)	(0.53)	(1.13)			
Private sector	0.66	0.38	2.16			
	(0.94)	(0.86)	(1.48)			
Public sector	0.83	-0.34	1.98			
	(0.65)	(0.53)	(1.19)			
Advisory council	0.38	-0.64	-0.00			
	(0.90)	(0.50)	(1.72)			
Management board	-2.05	-1.01	-1.30			-1.69
	(0.92)	(0.57)	(2.22)			(0.73)
Conflicting creator	-0.12	0.31	1.39		0.05	
	(0.50)	(0.51)	(0.76)		(0.52)	
Budget $level_L$	-0.79	0.22	1.72			
	(0.61)	(0.18)	(0.60)			
Budget $growth_L$			-0.01			
			(0.02)			
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed-effects	Yes		Yes	Yes	Yes	Yes
Agency random-intercepts		2.76				
Agency random-coefficients		0.51				
Instruments			88			
Agencies	371	371	353	402	402	402
Observations	5316	5316	4806	6294	6294	6294
Estimation	OLS	GLS	GMM	OLS	OLS	OLS

Notes. Outcome variable is the annual agency budget growth rate. Ideological conflict indicates a conflict of interest between the appointing and appropriating parties. Covariates are time-varying variables within each agency. Fixed-effects are binary indicators of fiscal year and agency. Random components are estimated standard deviations. Instruments list the number of lagged instruments. Bootstrapped standard errors clustered by agency in parenthesis (1000 replications; Windmeijer-corrected in Model 3).

corrective measure. For the models we presented in the article, we relied on simple listwise deletion to handle the missing data, which requires that the heads are missing completely at random (in the sense of Little and Rubin 2014) in order to not bias the results. For the models in Table A3, by contrast, we use logistic regression to estimate twenty multiply imputed datasets, with the treatment, outcome, and all covariates included in the imputation model (we

Table A4. Replications with Multiple Imputation.

	Model 1	*Model 2*	*Model 3*
Ideological conflict	-1.91	-1.80	-1.92
	(0.54)	(0.52)	(0.73)
Covariates	Yes	Yes	Yes
Lagged outcome			Yes
Fixed year effects	Yes	Yes	Yes
Fixed agency effects	Yes		Yes
Random agency effects		Yes	
Agencies	402	402	384
Observations	6294	6294	5696
Estimation	OLS	GLS	GMM

Notes. Replications of all models in the article using multiple imputation by chained equations to impute missing covariate values. Outcome variable is the annual agency budget growth rate. Ideological conflict indicates a conflict of interest between the appointing and appropriating governments. Covariates indicate the full set of time-varying covariates. Fixed effects are binary indicators of fiscal year and agency. Random components are estimated standard deviations. Robust standard errors clustered by agency in parenthesis. Based on twenty multiply imputed datasets.

fit separate imputation models for the static and dynamic models). As we can see from the outputs, the results do not differ substantially from the models with listwise deletion, with all of our point estimates remaining well within one standard error of those presented in the article. Also note that the imputed Model 1 in Table A4 yields approximately the same coefficient as the Empty Model in Table A3.

In Table A5, we replicate Model 1, except with a set of alternative range restrictions on the growth outcome. In the original sample, the range of observed growth rates stretch from a suspiciously low minimum of -100 percent to an absurdly large maximum of +129 300 000 percent, with a standard deviation of 1 830 611 percent (cf. Table A1). While some of the outlying observations do represent real organizational changes, many of them also stem from an older type of appropriation colloquially referred to as a "thousand crown grant", which meant that the ministers would temporarily set an agency's budget to about a hundred dollars, but with an implicit and unspecified credit line to cover any additional expenditures. For the models we presented in the article, we excluded 261 outliers using the generic procedures suggested by Hadi (1994). Without those 261 observations, the growth rates range from a

Table A5. Replications with Alternative Outcome Restrictions.

	-100 to +100	*-80 to +80*	*-60 to +60*	*-40 to +40*	*-20 to +20*
Ideological conflict	-2.48	-1.87	-1.92	-1.10	-0.73
	(0.70)	(0.59)	(0.56)	(0.44)	(0.32)
Covariates	Yes	Yes	Yes	Yes	Yes
Fixed year effects	Yes	Yes	Yes	Yes	Yes
Fixed agency effects	Yes	Yes	Yes	Yes	Yes
Agencies	371	371	371	370	366
Observations	5408	5317	5227	4996	4452
R^2	0.20	0.26	0.27	0.20	0.11

Notes. Replications of Model 1 using alternative range restrictions on the outcome. Outcome variable is the annual agency budget growth rate, with the range restriction in the title. Ideological conflict indicates a conflict of interest between the appointing and appropriating parties. Covariates indicate the full set of time-varying covariates. Fixed effects are binary indicators of fiscal year and agency. Robust standard errors clustered by agency in parenthesis.

minimum of -76.8 percent to a maximum of +82.2 percent, with a mean of 2.8 and standard deviation of 17.8. The results we present in Table A4, by contrast, uses arbitrary restrictions ranging from -100 to +100, -80 to +80, -60 to +60, -40 to +40, and -20 to +20. The results support the results in the article but, as can be expected, with the coefficient shrinking along with the tighter range restrictions.

In Table A6, we replicate the dynamic model from the article, except with five alternative lag specifications. The models build on the two-step system generalized method of moments estimator outlined in Blundell and Bond (1998), which incorporates lags in both differences and levels to instrument the lagged outcome variable. Because GMM regressions can be exquisitely sensitive to the choice of lag structure, however, here we present a few different model specifications for reference. We difference the outcome using the orthogonal deviations transformation and treat the year and conflicting creator indicators as strictly exogenous. All other variables are allowed to be predetermined. To avoid overfitting the endogenous components, we use a collapsed instrument matrix throughout. The GMM models utilize lag distances ranging from one to five, but we also include a simple two-way fixed-effects model without instrumentation for reference ("No Lags"). The model with four lags is identical to Model 3 in the article. In addition to the coefficients and standard errors for the ideological conflict and lagged outcome variables, we report both the Arellano-Bond statistics

Table A6. Replications with Lagged Instrumentation.

	Five Lags	*Four Lags*	*Three Lags*	*Two Lags*	*One Lag*	*No Lags*
Ideological conflict	-1.62	-1.69	-1.64	-1.74	-1.64	-1.63
	(0.82)	(0.79)	(0.79)	(0.76)	(0.80)	(0.59)
Budget growth$_L$	-0.01	-0.01	-0.01	-0.01	-0.01	-0.07
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Arellano-Bond AR(1) test	0.00	0.00	0.00	0.00	0.00	
Arellano-Bond AR(2) test	0.71	0.69	0.68	0.69	0.74	
Hansen J-test	0.09	0.11	0.15	0.25	0.28	
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed agency effects	Yes	Yes	Yes	Yes	Yes	Yes
Instruments	97	88	79	70	61	
Agencies	353	353	353	353	353	353
Observations	4806	4806	4806	4806	4806	4806
Estimation	GMM	GMM	GMM	GMM	GMM	OLS

Notes. Replications of Model 3 with varying lag structures. Outcome variable is the annual agency budget growth rate. Ideological conflict indicates a conflict of interest between the appointing and appropriating parties. Covariates indicate the full set of time-varying covariates. Fixed effects are binary indicators of fiscal year and agency. Robust standard errors clustered by agency with Windmeijer's finite-sample correction in parenthesis. Arellano-Bond lines report tests for serial correlation. Hansen line reports tests for exogeneity. Model titles report the instrument matrix.

for serial correlation and the Hansen J-test statistic. As we can see from the outputs, the estimated coefficients are all quite similar to the estimate that we presented in the article.

Finally, in Table A7 we also report results from a set of models with inverse probability of treatment weighting (IPTW). As we mentioned above, although most of our covariates are approximately balanced, the conflicting creator covariate displays a quite strong correlation with our ideological conflict variable. For the models we presented in the article, we relied on regression standardization to ensure that the treated and untreated units both have similar covariate distributions. The models in Table A6, by contrast, accomplish the same goal using propensity score weights (e.g. Hernán and Robins 2020). Because the IPTW estimator requires strictly positive assignment probabilities in each covariate stratum, however, our data setup for these models is a bit different. We first drop all agencies and fiscal years without variance on our ideological conflict variable. Next, we also replace the agency-effects with ministry-effects. This latter change means that we effectively shift our point of comparison from different budgets within the same agency to different budgets within the same ministry. We fit

Table A7. Replications with Inverse Probability of Treatment Weighting.

	IPTW 1	*IPTW 2*	*FE 1*	*FE 2*
Ideological conflict	-1.71	-1.66	-1.87	-1.71
	(0.48)	(0.48)	(0.58)	(0.58)
Imai-Ratkovic test	0.96	0.95		
Covariates	Yes	Yes	Yes	Yes
Lagged outcome		Yes		Yes
Fixed year effects	Yes	Yes	Yes	Yes
Fixed agency effects			Yes	Yes
Fixed ministry effects	Yes	Yes		
Agencies	299	294	299	294
Observations	4,608	4,270	4,608	4,270

Notes. Replications of Model 1 using inverse probability of treatment weighting. Outcome variable is the annual agency budget growth rate. Ideological conflict indicates a conflict of interest between the appointing and appropriating parties. Covariates indicate the full set of time-varying covariates. Lagged outcome indicates the lagged outcome. Fixed effects are binary indicators of fiscal year, agency, and ministry. Robust standard errors clustered by agency in parenthesis.

one model that includes the year-effects, ministry-effects, and all covariates (IPTW 1), and one model that includes the year-effects, ministry-effects, all covariates, and the lagged outcome variable (IPTW 2). For reference, we also present two analogous models that utilize the same two-way fixed-effects estimator as in the article (FE 1 and FE 2). The tables report the coefficient on ideological conflict, along with the Imai-Ratkovic balance test (Imai and Ratkovic 2014). As we can see from the results, the IPTW models are somewhat more conservative than the unweighted models, but still produce estimates in line with those we presented in the article.

Potential Extensions and Limitations

Our research design is tailored to identify the budgetary effects of political appointments, but could also potentially be extended into many other areas of bureaucratic politics. In particular, while we have placed great emphasis on the relationship between appointments and appropriations, we have also left the role of regulations mostly implicit. In practice, however, politicians may not only shape the bureaucracy's organization through personnel management and resource allocation. In addition, they may also wield a variety of regulatory instruments that can impose further constraints on both their political competitors

as well as the bureaucrats (e.g. McCubbins, Noll and Weingast 1987, 1989). If we take the policy impact of these regulatory instruments seriously, furthermore, then it also opens for the possibility that the agencies' ideological orientations may not only depend on their politically appointed leaders, but also on numerous other organizational factors as well. To conclude the appendix, we therefore briefly discuss how the agencies' broader legal environments and ideological orientations fit into our research design, before finally considering how our conflict measure relates to more conventional conflict measures.

The Law

As we mentioned in the article, one potential limitation of our study is that we do not measure the various legislative statutes and executive orders that define the agencies' legal environments explicitly. This omission could potentially create some inferential problems for us if the rules and procedures that surround the agencies' operations also affect the politicians' budget allocations and appointment strategies. For example, one could imagine that certain agency-specific regulations might affect the agencies' political stakes and, by extension, the ministers' appointment and budgetary incentives. While our research design is well suited to adjust for some types of regulatory constraints, we also depend on some assumptions about the Swedish case in order to circumvent the potential impact of others.

To begin with, notice that our fixed-effects should pick up a good deal of the most important regulatory variance. The broader legal frameworks that affect all of the government's agency relationships equally should generally fall under the year-effects, while stable differences between the agencies should generally fall under the agency-effects. For example, reforms of general laws like the Administrative Procedures Act or the Budget Act could potentially create some temporal breaks, which we depend on our year dummies to capture. Likewise, agencies like the Swedish Pensions Agency and the Swedish Armed Forces could

potentially operate under fundamentally different regulatory conditions simply because of their vastly different missions, which we depend on our agency dummies to capture. The two-way fixed-effects estimator is of course no panacea (see e.g. Imai and Song 2020), but under conventional statistical assumptions it should adjust for both agency-specific (but year-invariant) and year-specific (but agency-invariant) legal factors in a flexible manner.

The more complicated issue concerns the potential impact of rules and procedures that may vary within each agency, from one budget to the next. In particular, note that when the Swedish ministers prepare the implementation of the budget bill, they do not just give the agencies a bag of money to use as they please. As in most other modern democracies, they also typically submit a formal spending instruction along with each grant. Anecdotally, we also know that the specificity of these instructions can vary, ranging from saying very little to spelling out in detail where, when, and how the agency should use its resources. Because the substantive contents of these instructions essentially constitute a time-varying variable within each agency, they could potentially fall outside both the agency-effects and the year-effects. And if the parties in power were then also to adjust both their appointment strategies and budget allocations in response to the instructions' contents, then the relationship between the appointments and the budgets could turn out to be spurious.

Our positive argument for why it makes sense to exclude these spending instructions in our particular case is simple. The instructions are under the complete control of the very same ministers that also define the contents of the budget bill; and hence, much more likely to be affected by our appointments variable than the other way around. To see how this works, note in particular that the spending instructions are not set out in laws or any other legislative documents, but in executive orders. Consequently, once the parliament passes the budget bill, the ministers are free to write whatever instructions they want, without any further parliamentary involvement. Meanwhile, the situation with the agency heads is the exact

opposite. Because of the fixed-term appointment system, the ministers are not free to replace the heads arbitrarily. So, while the ministers could easily adjust the spending instructions in response to potential policy conflicts with the agency heads (in the spirit of e.g. Huber and Shipan 2002), it would be much more difficult for them to adjust the agency heads in response to the instructions. This has important methodological implications since, if our appointments variable can affect both the budgets *and* the instructions, then including the instructions in our models would really just leave us with a massive endogeneity problem, rather than help clarify our budgetary estimates (see e.g. Rosenbaum 1984 for a general discussion of this problem).

The potential downside of this approach is that we have no way of determining whether or how much the instructions might mediate the relationship between the appointments and the budgets. For example, consider what could happen if the ministers were to face an agency head who disagrees with their policy objectives. Given that the ministers author both the budget bill and the spending instructions, they could then: (a) write a more detailed instruction than they would have done in the absence of a policy conflict; and (b) grant more money than they would have done in the absence of a more detailed instruction. Put differently, if the ministers are as strategic as we suggest, then the spending instructions could potentially work as a sort of equalizer between allied and opposing agency heads, and thereby *reduce* some of the budgetary differences that we observe between them. Note, though, that even in this scenario, our budget estimates would still not be biased. Instead, the mediating effect of the instructions would simply become part of the total effect of our appointments variable. If we had access to an exogenous source of the instructions, we could perhaps have constructed an empirical model where we held them constant. But given that the budgets and the instructions are both endogenously determined by the ministers in our case, we believe the most defensible methodological stance would be to simply exclude them and focus on the "average treatment effect" of the appointments instead, as we do in the article.

Finally, while we mainly treat the agencies' broader legal environments as an empirical problem for the purposes of this particular study, this is not to say that we believe them to be unimportant. For example, in theory, one could imagine a scenario where the bureaucracy's personnel, procedures, and resources are all controlled by distinct political principals. A casual extension of our argument would then suggest that, contingent on their ideological relations, each principal should have incentive to adjust their interventions in response to the other principals' interventions. As this scenario would effectively create a three-way feedback loop, it would also make it very difficult to sort out the relationships between them empirically. We circumvent this problem by taking the appointments as a fixed constraint, but other institutional settings may present other research design opportunities. In the end, the broader point of our study is not that appointments and appropriations are the only factors that matter in bureaucratic politics. Rather, the broader point centers on how institutional divisions of public authority can distort government decision-making, and a more comprehensive theory of political-administrative relations would naturally want to include the law of the land in a more explicit manner than we have done here.

Agency Ideology

A closely related issue concerns the agencies' ideological orientations. Our study focuses on the ideological relations between the politicians responsible for appointing an agency's leadership and the politicians responsible for funding the agency. In practice, however, the agency's leadership is but one out of many factors that could influence the ideological orientation of the agency as a whole. For example, the agencies' formal missions could also in and of themselves be more or less well-aligned with the policy goals of the parties in power. Like the regulatory issue discussed above, this could potentially create some inferential problems for us if an agency's broader ideological alignment has an independent

effect on both the politicians' budget allocations and appointment strategies. While there are no simple ways to capture an agency's ideology empirically (see e.g. Clinton et al 2012), we do have access to two imperfect proxies.

First, notice that all our models include a dummy variable for whether an agency was created by an ideological opponent of the winning legislative coalition (*conflicting creator*). So, in each fiscal year, some agencies may have been created by the governing parties' political allies (0), while other agencies may have been created by their political opponents (1). We spent almost no space on explaining the logic behind this variable in the article, but our main motivation for including it was precisely to gain some leverage over the agencies' ideological orientations (and the possibility that the agencies may also themselves vary in their ideological alignment with the governing parties). For this variable to do us any good in this context, we must of course assume that the agencies reflect the ideological orientations of their creators. While this is no doubt a strong assumption, note that there is also a good deal of empirical evidence available to support it (e.g. Lewis 2002; Holmgren 2018)

Second, we also have access to another variable that targets the agencies' formal missions in a more direct manner. When a government decides to create an agency, they must always authorize the agency's operations via an executive order. The order typically defines (in very broad terms) the agency's formal mission, leadership structure, reporting requirements, and related organizational features. Importantly for our purposes, the governing parties can also sometimes decide to issue a completely new order, for instance, if they want to redirect the agency towards some other policy objective than the original one. Because we happen to observe all of these orders, we can also code the orders' ideological alignments in the same way as we code the ideological alignments of the agencies themselves. So, in each fiscal year, some agencies may carry orders authorized by the winning coalition's political allies (0), while other agencies may carry orders authorized by their political opponents (1). When combined

with the conflicting creator covariate, we can thus trace both the potentially lingering ideological imprint from an agency's creators, as well as any potential ideological updates from succeeding political leaders via the orders.

However, despite having access to the executive orders, we ultimately decided not to include them in our final model specification. Our reason for excluding them is essentially the same as our reason for excluding the spending instructions: we deem it far more likely that the ministers would change an agency's order in response to the agency head than change the agency head in response to the order. The orders, after all, are under the complete control of the ministers, while the agency heads are legally locked in place. Accordingly, if the ministers were ever to find themselves with an agency order that they do not support, they could simply issue a new order and be done with it (see e.g. Thrower 2017). For reference, adding the order covariate to Model 1 in the article would reduce the point estimate from 1.79 to 1.65. In other words, a notable difference given the huge sums of money involved, but not exactly a fatal one on theoretical grounds.

Finally, much as in the regulatory case discussed above, while we mainly treat the agencies' broader ideological orientations as an empirical problem for the purposes of this particular study, this is not to say that we believe them to be unimportant. For example, recent research has started to look beyond the politics of political appointments to also consider the potential politicization of lower-level career bureaucrats (e.g. Doherty, Lewis and Limbocker 2019). By contrast, we have essentially treated the underlying composition of public personnel as a constant. As we mentioned previously, the Swedish bureaucracy usually ranks as one of the most professional and least corrupt bureaucracies in the world (Dahlström and Lapuente 2017), which gives some justification to this assumption. In many other contexts, however, the ideological characteristics of the career bureaucrats may be much more salient—and hence, constitute an equally important part of the politicians' budgetary incentives. From a broader

perspective, political appointments simply represent one potential source of policy conflict out of many, and future research would accordingly do well to decompose the agencies' ideological orientations even further in order to achieve a better understanding of the incentives at work.

Divided Government

A final issue concerns the logic behind our ideological conflict measure. What are the main differences and similarities compared to more conventional conflict measures? In the article, we connected our study with a large literature that focuses on how divided government can shape delegation outcomes (e.g. Epstein and O'Halloran 1999; Huber and Shipan 2002). In practice, however, most scholars would probably associate "divided government" exclusively with having different parties in control of the legislature and executive. In our study, by contrast, we draw all our variance from partisan shifts in government, and not from any particular legislative-executive configuration. In fact, if we were to add a measure of the government's legislative-executive status to our models—say a dummy variable for majority government—all we would really accomplish would be to create a perfect correlation between that measure and our year dummies. Because the temporal dimension can make it difficult to see how our conflict measure relates to more conventional conflict measures, here we shall briefly make the connection a bit more explicit.

To see the point of contact, recall how scholars usually measure the operative level of policy conflict between the winning legislative coalition and the bureaucracy. Epstein and O'Halloran's (1999) seminal treatment of the American system paved the way for what has now become the standard solution. They argued that, because an ideologically hostile president is more likely to steer the federal bureaucracy away from Congress' policy goals than an ideologically allied president, Congress should also be less inclined to delegate under divided government than under unified government. Huber and Shipan (2002) built their study around

essentially the same causal logic, but also highlighted a number of other potential divisions in order to accommodate a more comparative approach. In particular, they noted that single-party majority government in parliamentary systems can be thought of as broadly analogous to unified government in presidential systems, while coalition and minority government can serve the role of divided government. They justified this move by arguing that delegation in parliamentary systems occurs sequentially from legislature to executive to bureaucracy—much like in presidential system—and with a group of potentially opportunistic cabinet ministers serving in the middle. In both the presidential and parliamentary case, the underlying assumption is that the chief executive will use their personnel management powers to align the bureaucracy with their own policy goals, which we can then exploit to study how the legislature may respond to varying levels of policy conflict.

Our main concern with the conventional approach sketched above does not center on the theoretical logic, but rather on the limited empirical applicability. The problem is that many political systems around the world feature the legislative-executive relationships that hardly ever vary. Notice that the only way to really use the conventional divided/unified dichotomy as a treatment variable is by comparing either: (a) the same political system at different points in time; (b) different political systems at the same point in time; or (c) some combination of the two. So, for example, you could take two electoral terms in a given country—one divided and one unified—and then compare the legislature's delegation strategies between them. As a matter of general research design, this systemic approach has served comparative politics quite well over the years and we do not mean to suggest that it is useless. But focusing exclusively on systemic features like legislative-executive relations does carry some inherent limitations that can place hard constraints on the sort of inferences that we can reasonably expect to make.

Let us take the United Kingdom as a stylized example. Suppose that we want to examine the effects of policy conflict on the British parliament's delegation strategies. Can we

apply the Epstein and O'Halloran treatment to the UK? Obviously, since the UK has no president, a literal application would be impossible. To make some empirical progress here, we must first somehow account for the fact that the British parliament chooses their own executive. Can we perhaps overcome the regime-type difference by using minority and majority governments as surrogates for divided and unified governments, as Huber and Shipan suggest? Unfortunately, because the British general elections rarely produce anything other than single-party majority governments, this would leave us with little to no variance for most of the UK's post-war history. Given that the British government has historically been a very unified one, how exactly are we supposed to capture the operative level of policy conflict in a theoretically relevant way? If we were to casually follow the conventional approach, then we would simply end up with decades' worth of data where no credible inferences can be made.

Our policy conflict measure extends the conventional approach in two important respects. First, we define our policy conflicts at the agency level, not at the system level. This move immediately provides us with much more variance to work with, since we are no longer restricted to comparing entire electoral terms against each other. Instead, we can compare individual agencies both within the same electoral term and between different electoral terms. Second, in order to determine a given agency's conflict status, we focus on the legislature's relationship with the appointing executive, not on the legislature's relationship with the current executive. To see why this move matters, notice that the legislature may not only face agencies appointed by the current executive. In addition, they may also face a wide array of holdover agencies appointed by previous executives. By tracing precisely which parties were responsible for appointing which agencies, we can classify each individual agency as more or less closely aligned with the winning legislative coalition, before finally relating them to our delegation outcome of interest. We call this an "extension" of the conventional approach because the core assumption is the same, i.e. we assume that whoever controls the executive will fill the

bureaucracy with ideological allies and that whoever controls the legislature will then adapt accordingly. The crucial difference is just that we permit the policy conflicts between the branches to play out over multiple time-periods, instead of restricting ourselves to a single time-period only.[7]

Re-construing the standard principal-agent relationship as a process that unfolds over time has real implications for the general applicability of many of the arguments that have been advanced in the bureaucratic politics literature. For example, our conflict measure immediately resolves the British conundrum referenced above, since it makes no difference whether we happen to encounter several decades of unified governments, several decades of divided governments, or several decades of a more even mix between the two. With our approach, the key question is not whether any given government can be classified as unified or divided. Instead, the key question is whether we can observe any ideological divisions between a government's central decision-makers *over time*; all our variance arises as a natural consequence of government turnover and does not require any specific legislative-executive configuration to carry force. But it can also serve as a useful alternative to the conventional approach in many other situations. For example, in the Swedish case, all single-party majority

[7] Notice that the conventional approach also emerges as a special case of our approach. If the current executive is responsible for appointing all operative agencies, then our measure will be identical to the standard divided/unified dichotomy. In practice, however, each new executive will typically inherit the bulk of their bureaucracies from previous executives, which immediately opens for at least some holdover bureaucrats to remain in service across elections. The Swedish fixed-term appointment system could of course turn this inheritance into an unusually debilitating affair, but see e.g. Lewis (2008) for a detailed review of the same turnover dynamic in the US case.

governments over the past 100 years or so also happen to be social democratic governments, which makes it next to impossible to distinguish the effects of "unified government" from the effects of "social democratic government". By turning our attention from legislative-executive confrontations to partisan shifts in government, we can take the basic logic of modern delegation theory to institutional settings that would otherwise be inaccessible.

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