Legislative Networks and Ministerial Turnover: Analyzing the Impact of Network Embeddedness on Ministerial Dismissals

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Abstract

What influences the durability of cabinet ministers? We argue that the extent to which ministers are embedded in the network of social relations with other politicians is an important factor of their survival because network embeddedness affects how much they face severe challenges and criticisms from other politicians. To test this argument, we identify networks among politicians in Japan based on their co-directorship in legislative committees between 1947 and 2017. We show that cabinet ministers who are more embedded in the legislative network—as measured by closeness centrality—are less likely to be dismissed than those who have fewer connections. Our results are robust to an instrumental variable approach, where we exploit arguably exogenous changes in network structures due to the close elections of network neighbors.

Keywords: Ministerial Turnover, Legislative Networks, Committees, Japan

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In parliamentary democracies, cabinet ministers play important roles in formulating and implementing policies. They are selected mostly from among MPs by the head of government (i.e., Prime Minister), and once they are selected, they serve as a minister and manage government ministries until they get fired or the government terminates. Although minister selection is often analyzed in tandem with the process of government and cabinet formation (Ono 2012; Strøm, Müller and Bergman 2003), a considerable number of minister selection actually happens during the cabinet term as well, in which some ministers are dismissed and replaced by others (Martínez-Gallardo 2014). This means that some ministers are screened out and allowed to serve only briefly, while others are kept in the cabinet for a long time. To the extent that the patterns of ministerial replacement are poorly explained by the theories of cabinet formation (Fischer, Dowding and Dumont 2012; Huber and Martinez-Gallardo 2008), it is important to ask what determines ministerial dismissals during the cabinet term.

Empirical studies on ministerial turnover suggest that ministers' attributes (Fischer, Dowding and Dumont 2012), institutional settings (Bäck et al. 2012; Bucur 2017; Huber and Martinez-Gallardo 2008), and external factors (Camerlo and Pérez-Liñán 2015*a*; Martínez-Gallardo 2014) influence ministerial dismissals. However, these studies do not pay explicit attention to the fact that ministers are embedded in the network of social interactions with other politicians. By ignoring ministers' social relationships with their peers, prior work precludes the possibility that there is a social and relational component in the patterns of ministerial dismissals. This omission may be significant because the important roles of social networks among politicians have been fully addressed in other fields of legislative politics (Fong Forthcoming; Kirkland 2011; Tam Cho and Fowler 2010; Zelizer 2019).

In this study, we draw on social network theories to examine the relationship between networks among politicians and ministerial turnover (Coleman 1988; Granovetter 1985). We argue that the extent to which ministers are embedded in the network with other politicians can affect the probability of their dismissals. On the one hand, network embeddedness conditions how much ministers are trusted by their peers, including those of oppositions, and

able to receive their affective social support. Hence, network embeddedness can potentially function as a buffer against severe challenges and criticisms that ministers face. On the other hand, network embeddedness also means that ministers' actions are easily monitored and sanctioned by other politicians. As a result, strong embeddedness may induce ministers to comply with the norms of appropriate legislative conducts, reducing their incentives to engage in opportunistic behavior. We hypothesize that these two factors make government heads less willing to replace ministers with greater network embeddedness during the cabinet term.

To test this argument, we focus on the patterns of ministerial dismissals in Japan. One important challenge we face is the lack of credible relational data among Japanese politicians. To address this issue, we approximate social connections among them by using the information on co-directorship in legislative committees. We expect that if two politicians assume leadership positions (as a chair or director) in the same committee, it is a great opportunity for them to cultivate an affective tie through day-to-day operations of the committee, even beyond their partisan differences. Therefore, networks built on co-directorship ties should offer a meaningful way to capture informal social relationships among Japanese politicians.

By analyzing ministerial turnover between 1947 and 2017, we demonstrate that ministers who exhibit greater network embeddedness—as measured by closeness centrality in the network of committee co-directorship—are indeed less likely to be dismissed than those with lower embeddedness. Moreover, our findings are robust to the use of an instrumental variable approach, in which we exploit arguably exogenous changes in network structures due to the close elections of network neighbors.

Determinants of Ministerial Turnover

The issue of ministerial turnover has been analyzed under the principal-agent framework, which concerns the delegation of power from the head of government to individual min-

isters (Huber and Martinez-Gallardo 2008; Indridason and Kam 2008; Strøm, Müller and Bergman 2003). According to this approach, ministerial replacement is seen as a strategic action of government heads to overcome two types of delegation problems. First is adverse selection, or uncertainty about the incentives and abilities of individual ministers (Huber and Martinez-Gallardo 2008). By replacing ministers, the heads of government can weed out "bad" ministers and select "good" ones. The second problem is moral hazard, which suggests that ministers have incentives to use their power in a way that runs against the interests of the cabinet (Indridason and Kam 2008). Frequent ministerial replacement limits the opportunities of sharking by ministers, enabling the heads of government to reduce agency loss.

Building on the principal-agent framework, prior studies have identified several factors that influence minister turnover.[1] These factors operate at three different levels, either individual ministers, political institutions, or external contexts. First, at the level of individual ministers, their competence and loyalty are supposed to influence their survival (Camerlo and Pérez-Liñán 2015*b*). Hence, ministers' performance to accomplish the goals of the cabinet is one of the most important determinants of their durability (Berlinski, Dewan and Dowding 2010; Søyland 2017). Prior experiences as a minister also play a critical role in affecting their turnover (Bovens, Brandsma and Thesingh 2015). Further, some of their attributes, such as age, gender, and education, can predict their survivals (Escobar-Lemmon and Taylor-Robinson 2015; Fischer, Dowding and Dumont 2012).

Second, institutional factors also play an important role in ministerial turnover because they shape the abilities and constraints of government heads to use the power to reshuffle their cabinets. For example, the prestige of portfolios conditions the probability of firing (Bright, Döring and Little 2015; Hansen et al. 2013). Coalition governments may show less frequent use of ministerial replacement than single-majority governments because the former

[1]Minister termination and survival are only weakly connected to the patterns of government termination and survival, meaning that the theories of ministerial turnover should go beyond the theories of cabinet turnover (Fischer, Dowding and Dumont 2012; Huber and Martinez-Gallardo 2008).

needs a larger number of actors to agree on a firing decision than the latter does (Huber and Martinez-Gallardo 2008). For a similar reason, divided governments may show lower rates of ministerial dismissal than unified governments (Bucur 2017). Moreover, the institutional power and autonomy of government heads influence how easily they can dismiss ministers (Bäck et al. 2012; Martínez-Gallardo 2014).

Third, external factors are equally critical for determining ministerial turnover because government heads often use ministerial replacement to respond to changing political environments. For instance, economic and financial crises may increase the necessity of minister reshuffling (Martínez-Gallardo 2014). Shifts in cabinet popularity can trigger ministerial replacement (Camerlo and Pérez-Liñán 2015*b*). Some evidence further suggests that protest and scandals can affect ministerial turnover contingent on the timing of elections (Camerlo and Pérez-Liñán 2015*a*).[2]

Although prior studies have improved our understanding of ministerial dismissals, what is missing is an insight into how the social networks of individual ministers influence their survival. This omission may be problematic because the importance of social connections has been emphasized in other domains of legislative politics. For example, some studies show that legislators make use of their informal networks for cue-taking and voting decisions (Fong Forthcoming; Ringe, Victor and Gross 2013; Wojcik and Mullenax 2017).[3] Indeed, even seemingly minor interactions in office and seat proximities on the floor can have a considerable impact on their roll-call behavior (Liu and Srivastava 2015; Masket 2008; Zelizer 2019).[4] Other studies also demonstrate that the structures of legislative networks determine the collective outcomes of the policy-making process and legislative productivity (Kirkland 2011; Tam Cho and Fowler 2010).

[2]Some empirical evidence suggests that ministerial replacement can improve the popularity of cabinets (Dewan and Dowding 2005; Miwa 2018).

[3]Montgomery and Nyhan (2017) and Nyhan and Montgomery (2015) also suggest that legislators who are connected by the same third parties, such as campaign firms and legislative staff, tend to show similar behavior.

[4]However, Rogowski and Sinclair (2012) caution that the effect of social connections on roll-call behavior may suffer from endogenous selection because like-minded politicians who are likely to vote together are more likely to form a social tie.

Given the importance of social networks in the legislative process, it is critical to conceive that ministers are embedded in the network of social relationships with other politicians. By so doing, we can evaluate the social and relational aspect of municipal turnover. In the next section, we advance our argument on how networks among politicians can influence ministers' survival. Specifically, we suggest that network embeddedness prevents ministers from being dismissed from their positions.

Network Embeddedness and Ministerial Turnover

In theorizing the relationship between social networks and ministerial turnover, we pay attention to the following two functions of network embeddedness. The first is that network embeddedness enhances the extent to which ministers receive affective social support from other politicians. The second is that network embeddedness constrains the opportunistic behavior of ministers by increasing the monitoring capacity of other politicians.

First, social network theories suggest that repeated social interactions play a critical role in cultivating trust and reciprocity among individuals (Coleman 1988; Granovetter 1985). On the one hand, this proposition means that the network of social interactions helps establish a basis of social cooperation, and those who are tightly embedded in the network can receive various forms of affective support—such as intimacy, respect, emotional closeness, and social approval—from other actors. On the other hand, the above proposition also implies that repeated interactions reduce uncertainty about actors' behavior.[5] By so doing, network embeddedness makes it easier for actors to establish their legitimacy and avoid unnecessarily conflicts with other actors.

Trust and reciprocity generated by social networks can be important sources of one's job performance. In fact, organizational studies show that network embeddedness creates a workplace environment in which well-connected individuals are treated generously (Gulati

[5]Another important resource that network embeddedness provides is information because actors who are tightly incorporated in the network can have fast access to information that other actors have. We return to this point in our empirical analysis.

1995). As a result, those with stronger embeddedness tend to face more favorable work climate and less work stress than those with weaker embeddedness do (Hayton, Carnabuci and Eisenberger 2012). Furthermore, due to the network support they receive, the former exhibit better job performance than does the latter (Brüderl and Preisendörfer 1998; Uzzi 1996; Van Emmerik and Sanders 2004).

Second, social network theories also suggest that network embeddedness constrains the behavior of individual actors (Coleman 1988; Granovetter 1985). Being tightly incorporated in the network means that the behavior of an actor is easily monitored by other actors in the network. As a result, one's violation of social norms is easily detected and punished by other actors. The fear of collective sanctions can eventually incentivize those with strong embeddedness to behave in a trustworthy manner and to refrain from opportunistic behavior (Gulati 1995). Therefore, network embeddedness imposes some sorts of social obligation, reinforcing actors' tendency to behave in a way that upholds the appropriate norms of the organization.

The above argument that network embeddedness (1) cultivates trust and (2) restricts opportunistic behavior has important implications for the patterns of ministerial turnover. First, we expect that legislators who are tightly embedded in the legislative network are more trusted by peers than those without such connections. Then, once they are appointed to ministerial positions, the network-based social support is likely to provide them with a buffer against hazards they face during their tenure. For example, opposition politicians may have lower incentives to challenge ministers with tight embeddedness than those with fewer connections for fear of jeopardizing established social relations. For the same reason, oppositions may withhold a harsh criticism against well-connected ministers even when they make inappropriate remarks or cause scandals.

Second, at the same time, strong network embeddedness also encourages ministers to behave in a trustworthy manner. After all, they may expect that their misconducts may be easily detected and lead to costly sanctions that exceed potential benefits that opportunistic

behavior may provide. To avoid reputation losses, they may want to follow the norms of appropriate legislative conducts and refrain from agency shirking. By so doing, they can further strengthen their reputation within the legislative arena.

From the perspective of government heads, the above two features of well-connected ministers look appealing in maintaining the government. On the one hand, if ministers who are more tightly embedded in the legislative network face smaller obstacles than those with fewer connections, government heads can expect that the former type of minister is less likely to hinder the effective operation of the cabinet. In this way, network embeddedness reduces uncertainty about ministers' performance and competence, alleviating the problem of adverse selection. On the other hand, if ministers who are tightly embedded in the legislative network are more likely to avoid opportunistic behavior than others, government heads can also become less concerned about moral hazard. Both these factors make the heads of government want to retain well-connected ministers in the cabinet. Therefore, we hypothesize that greater network embeddedness should be negatively associated with the probability of dismissal once being appointed to a minister.

Data and Methods

To test our argument, we analyze the patterns of ministerial turnover in Diet—the national parliament of Japan—between 1947 and 2017. It offers a great case to test our argument because cabinets were reshuffled regularly and frequently without changing the composition of governing parties, while at the same time institutional features of cabinet system (such as the size of a cabinet) were kept stable for a long time (Ono 2012). Below, we first explain how to measure legislative networks in Japan, which makes a unique innovation under the limited availability of relational data among politicians. Then, we elaborate on our empirical strategies.

Identifying Legislative Networks in Japan

Prior studies have constructed networks among politicians using cosponsor bills (Kirkland 2011; Tam Cho and Fowler 2010), third parties such as congressional staff and consultant firms (Montgomery and Nyhan 2017; Nyhan and Montgomery 2015), or surveys (Ringe, Victor and Gross 2013; Wojcik and Mullenax 2017). However, some challenges exist in those measures. Cross-partisan cosponsorship of bills rarely occurs in many parliamentary democracies, especially in so-called arena legislatures (Polsby 1975). This makes it difficult to detect non-partisan networks based on bill proposals. Credible information about politicians' connections with external actors is not usually available to the public, because they have strong incentives to hide those informal and private connections. Surveys might be useful to highlight unrevealed social ties among politicians, but one-time surveys do not allow us to detect changes and stability in the relationship for a long period of time. To overcome these challenges, we create politicians' networks based on their co-leadership in legislative committees.

We believe that politicians' shared experiences as committee leaders are a meaningful construction of their social connections in the parliament. In Japan, in particular, committee chairs and directors jointly take a leading role in committee management, such as setting agendas and determining a schedule for deliberation (Morimoto 2017). In this process, they represent their party and communicate closely with each other behind the closed door. Hence, these are very rare opportunities for MPs to work together across aisle within the parliament and cultivate strong relationships beyond the partisan line. Reflecting mutual relationship they build, committee directors often draft cross-partisan bills together.

We extract data on politicians' assignments to committee chairs and directors in the House of Representatives (HOR) from *Kokkai Giin Hakusho*.[6] Each committee has one chair and, on average, eight directors.[7] They are composed of politicians from different

[6]It is a website that records the parliamentary activities of individual politicians. See https://kokkai.sugawarataku.net/.

[7]In section A of the appendix, we analyze who is more likely to be a committee chair and director.

parties. It is common for politicians to serve as directors in more than one committees in each session of the parliament.[8] Further, there are always some changes in directors between sessions as well as elections.

To construct a network in parliament t, we check whether a pair of politicians previously served as committee leaders (chair or director) at the same time up until parliament $t-1$. Hence, if politicians i and j assumed the leadership roles in the same committee in any session of the previous parliaments, we assume that they have a social connection in the current parliament.[9] Because of this coding rule, our legislative networks are unweighted. In other words, ties among politicians do not take into account the intensity of their connections (e.g., how many times they served in the same committees or how long they have known each other). To the extent that we dichotomize every tie, we are likely to generate conservative estimates of network embeddedness in the analysis below. We visualize legislative networks between 1947 and 2017 (the 24th and 47th parliaments) in section B of the appendix.

These networks are, of course, not without limitations, and we note two points. First, we do not have data on committee assignments before 1947. Due to this censoring, some of the ties that politicians formed before the 23rd parliament may be missing. However, due to the institutional discontinuity caused by World War II, this problem may not be critical. Second, since our networks are based on legislative committees in the HOR, we do not have networks in the House of Councillors (upper house). For this reason, our analysis below focuses only on ministers who have a seat in the HOR.

Using the networks of committee co-directorship, we measure the extent to which individual politicians are embedded in the legislative network. Specifically, we use the measure of closeness centrality, which quantifies to what extent a politician is close to all other politicians in the network. Formally, it is calculated as the inverse of the sum of the shortest

[8] Each parliament of the HOR consists of three types of sessions: regular (annual), extraordinary, and special sessions.

[9] Those who have never served as a committee chair or director are isolated in the network.

paths between the node i and all other nodes:

$$C_i = \frac{N-1}{\sum_j d_{ij}} \tag{1}$$

where d_{ij} is the shortest path between nodes i and j. By normalizing the inverse of the sum of shortest paths by the size of the network minus one $(N-1)$, we can compare closeness centrality across networks with varying sizes. A greater value in the closeness centrality indicates that the politician can reach others in the network with fewer steps, hence greater embeddedness.[10]

Closeness centrality is a global measure of network positions because it takes account of every tie that is present in the network. In this respect, it is distinct from degree centrality, which is simply the number of direct ties that politicians have. In this study, using closeness centrality is more appropriate than degree centrality because our theory suggests that what matters to ministers' survival is not necessarily with whom they are directly connected. Rather, our argument is that the extent to which they are embedded in the network of social relations within the legislature affects their durability. This point requires us to focus on ministers' social ties beyond their immediate connections.[11]

The Baseline Specification

Our unit of analysis is the minister (politician i in cabinet c), and the outcome variable is a dummy indicator of his/her dismissal. It takes the value of 1 if the minister is dismissed in the middle of a cabinet term, and 0 otherwise. According to this definition, ministerial changes that occur during cabinet reshuffles (i.e., the formation of a new cabinet) are not regarded as dismissals. For example, during the Third Abe Cabinet between 2014 and 2017, three cabinet reshuffles occurred in October 2015, August 2016, and August 2017, respectively. If

[10] In section C of the appendix, we explore how closeness centrality affects cabinet appointment.

[11] The importance of indirect ties is illustrated by the fact that people tend to be favorably disposed not only to their friends but also to their friends' friends.

ministers were replaced at the time of these reshuffles, we do not consider that they were dismissed.[12] Moreover, if ministers change their positions from one post to another in the middle of a cabinet term, the outcome variable takes the value of 0. In our data, ministerial dismissals occur in 6.3% of the cases.[13]

In the analysis below, we control for several factors that may confound the relationship between network embeddedness and dismissal. First, we include *Age* because it is presumably an important determinant of resignation decisions (Fischer, Dowding and Dumont 2012). Second, we also control for several personal attributes of politicians that are shown to shape their behavior in Japan and elsewhere (e.g., Baumann, Debus and Müller 2015; Ono 2015; Smith 2018; Tavits 2009). Specifically, *Female* takes the value of 1 if the minister is a woman. *Dynasty* is a dummy indicator of ministers whose family relatives previously served in parliament. *Local* is a dummy variable that captures the previous local political experiences of politicians (as a governor, mayor, or local assembly member). Then, *Bureaucrat* is a dummy for ex-bureaucrats. Finally, since closeness centrality is strongly affected by the term length of politicians, we also include *Tenure* and its squared term.[14]

To assess the effect of network embeddedness on ministerial dismissals, we fit a logistic regression with random effects by cabinet.[15] Since the same politicians are repeatedly appointed to a minister in different cabinets, politician i enters into the data multiple times. To account for the possibility that error terms are not independent across the same individuals,

[12] We use different identifiers for reshuffled cabinets. Hence, the Third Abe Cabinet consists of four different cabinets.

[13] Data on ministers and their dismissals come from the Cabinet Office (see `https://www.cao.go.jp/index-e.html`). Note that we do not distinguish between the reasons for dismissals because our theory is not about how ministers resign. In some cases, dismissals are due to ministers' suicides or death. We cannot rule out the possibility that social connections within the legislature influence the chance of suicides and death in office as network embeddedness can reduce work-related stress (Hayton, Carnabuci and Eisenberger 2012).

[14] All these variables are based on the Reed-Smith Japanese House of Representatives Elections Data Set (Reed and Smith 2017). Descriptive statistics are in section D of the appendix.

[15] Since the characteristics of Prime Ministers can also affect ministerial turnover (Fischer, Dowding and Dumont 2012), it is ideal to rely on a strict within-cabinet comparison, meaning the use of cabinet fixed effects. However, due to the incidental parameter problem, it is not appropriate to include fixed effects in logistic regressions. In fact, if we use cabinet fixed effects, the negative effect of closeness centrality tends to be overestimated.

we estimate standard errors using a clustered bootstrap approach, where clusters are defined by each politician.

An Instrumental Variable Approach

An obvious challenge to the above approach is that committee assignments are not random (Cirone and Van Coppenolle 2018; Fujimura 2012). Ministers with greater closeness centrality may be systematically different from those with lower centrality in their characteristics. In fact, if we regress closeness centrality on ministers' characteristics, we see that these factors are statistically significant predictors of network positions (model 1 in Table 1). The presence of systematic differences in the observable characteristics implies that ministers with higher and lower centrality scores may be also different in unobservable ways. This point raises concerns about omitted variable bias in the above approach. For instance, an omitted variable could be some unmeasurable "quality" of the politician, which affects both more embeddedness and better skill at doing his/her ministerial jobs (e.g., avoiding controversy).

To partly mitigate this concern, we use an alternative strategy that relies on close elections of network neighbors, which is so-called an instrumental variable approach similar to those used by Hyytinen et al. (2018) and Waldinger (2011). We first create a new variable based on as-good-as-random variation in the maintenance and disappearance of ties between politician i and his/her network neighbors due to the close elections of the latter. Then, we use this variable as an instrument for politician i's closeness centrality. Intuitively, this approach exploits arguably exogenous changes in network structures due to the results of close elections to explain the actual network positions of politicians.

We construct our instrument as follows. First, we set a very narrow bandwidth of the margin of victory $\pm\varepsilon$, within which we can arguably assume that the winners and losers of elections are as-if randomly determined. Hence, if the network neighbors of politician i have the margins of victory between $-\varepsilon$ and 0, we may believe that they lost the elections by chance. Conversely, if they have the margins of victory between 0 and $+\varepsilon$, they won

Table 1: Cavariate Balance

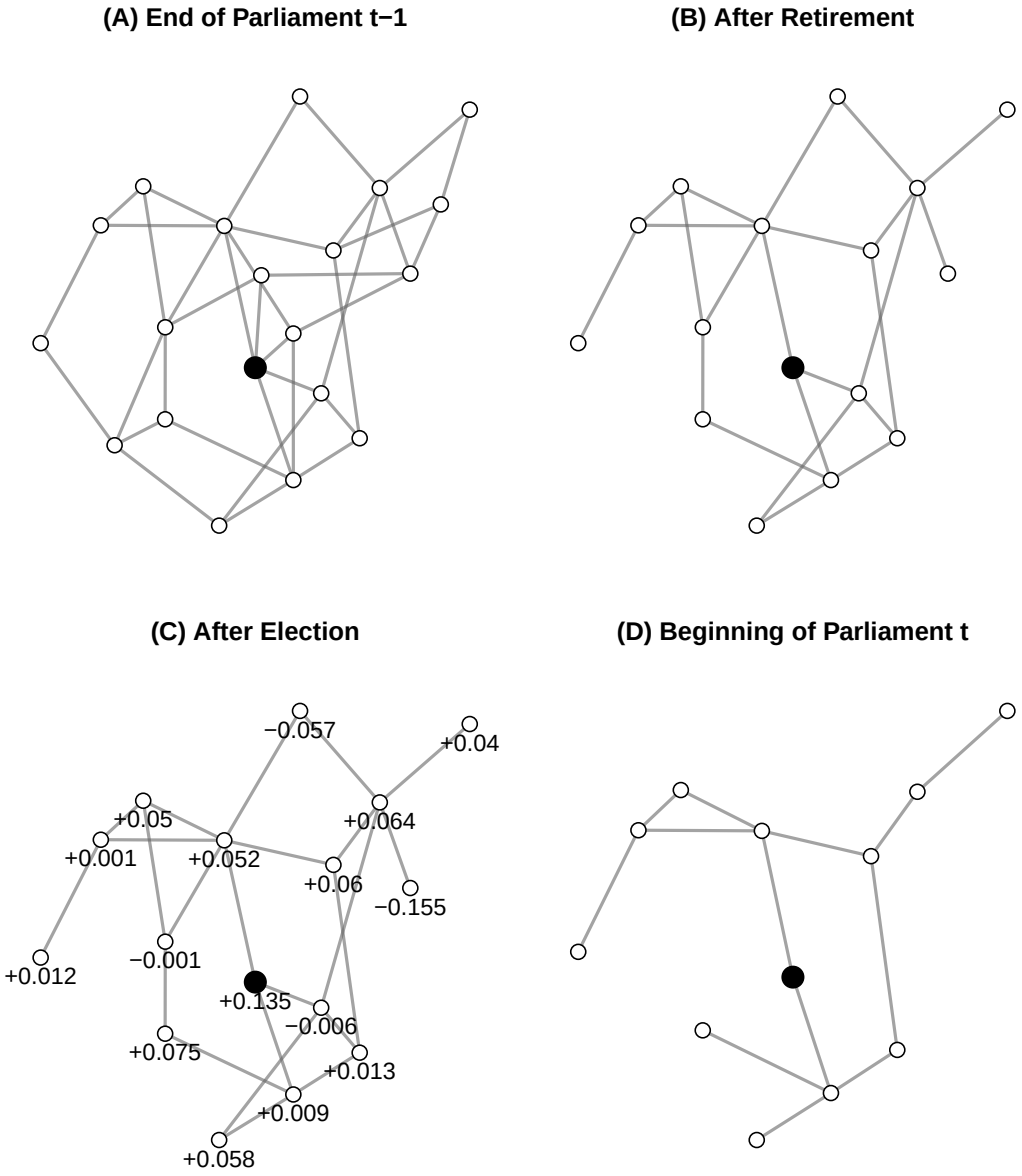
	(1) Closeness Centrality	(2) Instrument BW = ±0.002	(3) Instrument BW = ±0.003	(4) Instrument BW = ±0.004
Age	−0.039*	−0.003*	−0.003*	−0.003*
	(0.004)	(0.001)	(0.001)	(0.001)
Female	0.117	−0.005	−0.003	−0.005
	(0.183)	(0.039)	(0.043)	(0.044)
Dynasty	−0.178*	−0.024	−0.026	−0.027
	(0.058)	(0.012)	(0.014)	(0.014)
Local	0.200*	0.010	−0.001	0.001
	(0.068)	(0.015)	(0.016)	(0.016)
Bureaucrat	−0.217*	0.010	0.011	0.002
	(0.063)	(0.013)	(0.015)	(0.015)
Tenure	1.217*	0.010	0.018	0.020
	(0.048)	(0.010)	(0.011)	(0.011)
Tenure^2	−0.075*	0.0001	−0.0003	−0.0004
	(0.003)	(0.001)	(0.001)	(0.001)
σ_c	0.289	0.010	0.009	0.010
N	1,483	1,483	1,483	1,483
N of Cabinets	94	94	94	94

Note: *p<0.05. Models are estimated with a linear model with random effects by cabinet. σ_c is a variance component of cabinet random effects. Standard errors in parenthesis.

the elections by chance. Second, we count the number of politician i's network neighbors whose margins of victory fall within the ranges of $(-\varepsilon, 0]$ (i.e., random losers) and $(0, +\varepsilon)$ (i.e., random winners). Finally, we divide the number of random winners by the sum of the numbers of random winners and losers, which becomes our instrumental variable.

To further illustrate these steps, Figure 1 plots hypothetical networks among politicians. First, panel A shows a legislative network at the end of parliament $t-1$, where there are 20 politicians. Second, when we move from panels A to B, we lose 4 politicians because they decide not to run for reelection. Retirement leads to the initial change in network structures. Then, the election for parliament t happens in panel C, and we observe the margins of victory of politicians in the network. Here, positive values indicate that they win the elections whereas negative values show that they lose the elections (we assume that 4 politicians lose). Finally, panel D shows the network in parliament t, based on which we measure the closeness centrality of politicians. Between panels C and D, the election induces the second change in network structures, leaving 12 politicians in the network.

Figure 1: Hypothetical Changes in Network Structures between Parliaments $t-1$ and t



Note: The figure shows structural changes in hypothetical networks among 20 politicians between Parliaments $t-1$ and t. As we move from panels A to D, the number of politicians decreases due to retirement (A $\rightarrow$ B) and election (C $\rightarrow$ D). The numbers in panel C indicate the margins of victory (negative values mean losing the election).

Now we focus on the black node in Figure 1. It had 5 network neighbors at the end of parliament $t-1$ (panel A). However, two of them retired, and only three of the network neighbors ran for office (panel B). Then, two of these neighbors won the election, but one lost (panel C). Consequently, in parliament t, the black node has only two immediate connections (panel D). According to panel C, the network neighbors of the black node had the margins

of victory of +0.052, +0.009 and −0.006, respectively. If we set $\varepsilon = 0.01$, we assume that the election results for two of them were as-if randomly determined, with one random winner and one random loser. As a result, the black node maintained one connection by chance whereas lost one connection by chance. The instrument for the black node is calculated as $1/(1+1) = 0.5$.[16]

In the analysis below, we use $\varepsilon = 0.002$, 0.003, and 0.004 to select our bandwidths. When we use these values, our instrument does not seem to be weak and simultaneously maintains apparent covariate balance.[17] For example, in Models 2 to 4 of Table 1, we regress our instrument on some characteristics of ministers. These results show that when we use sufficiently narrow bandwidths between ±0.002 and ±0.004, ministers' attributes do not show a statistically significant effect on the instrument, with *Age* being the only exception. Hence, our instrument seems to reduce covariate imbalance effectively. Our hope is that this is also the case for unobservable factors.

Does our instrument satisfy the requirements of the instrumental variable approach? First, as we show below, no evidence suggests that our instrument is particularly weak. Second, an increase in the number of direct ties never leads to a decrease in closeness centrality, meaning that our instrument is consistent with the assumption of monotonicity. Third, exclusion restriction in this context means that the outcomes of network neighbors' close elections should not influence the dismissal of a minister other than through changing his/her network position. This assumption would be violated if, for example, the number of as-if-random winners and losers within immediate connections systematically changed the behavior and motivations of the minister.[18]

[16] If none of the network neighbors had a close election, the instrument takes the value of 0. When creating the instrument, we exclude those who lost in the single member district tier but obtained a seat in the proportional representation tier.

[17] We find that if we set $\varepsilon = 0.001$, our instrument is weak. By contrast, if we set $\varepsilon > 0.005$, apparent covariate imbalance increases. Note that our bandwidths are considerably smaller than optimal bandwidths used by Ariga (2015) and Ariga et al. (2016) perhaps because we focus on the close elections of network neighbors.

[18] A related concern is the violation of the Stable Unit Treatment Value Assumption (SUTVA) because we rely on the close elections of network neighbors. However, this problem biases our results only if the characteristics of politician i or the characteristics of i's neighbors are correlated with the number of close

It is also important to note that there is an extensive debate on whether the outcomes of close elections are truly random in the U.S. context (e.g., Caughey and Sekhon 2011; Eggers et al. 2015). In the case of Japan, close elections are used to evaluate incumbent advantages (Ariga 2015; Ariga et al. 2016). However, we are not aware of any debate on the validity of the exogenous assumption of close elections in Japan. We show that our instrument reduces covariate imbalance (Table 1), and a more systematic assessment of the exogenous assumption is beyond the scope of this study. For this reason, our instrumental variable approach may be seen as a robustness test rather than a clean causal identification.

In terms of model specification, we rely on a two-stage residual inclusion (2SRI) estimator (Terza, Basu and Rathouz 2008). Since both our instrument and endogenous variables are continuous variables and our outcome is a dichotomous variable, 2SRI produces less biased estimates than the two-stage least squares (2SLS) estimator (Burgess and Thompson 2012; Terza, Basu and Rathouz 2008). In the first-stage regression, we estimate closeness centrality on our instrument using a multi-level linear model:

$$C_{ic} = \alpha + \beta S_{ic} + \nu_c + \epsilon_{ic} \tag{2}$$

where C_{ic} represents the closeness centrality of politician i in cabinet c. S_{ic} is our instrument (based on three bandwidths: $\varepsilon = 0.002$, 0.003, and 0.004). ν_c denotes random effects by cabinet, and ϵ_{ic} is an idiosyncratic error term. Then, in the second-stage regression, we estimate a multi-level logistic regression, including the actual value of closeness centrality and estimated residuals $\widehat{\epsilon}_{ic}$ from the first stage regression:

$$Pr(R_{ic} = 1) = \text{logit}(\gamma + \delta C_{ic} + \eta\widehat{\epsilon}_{ic} + \kappa_c + \zeta_{ic}) \tag{3}$$

where the outcome is dismissal, κ_c represents cabinet random effects, and ζ_{ic} is a second-stage error term. Our main interest is in the value of δ. Intuitively, including the first-stage resid-

elections among network neighbors (see, e.g., Forastiere, Airoldi and Mealli 2016).

uals in the second stage regression allows us to adjust for unmeasured confounding factors because they are by definition unbiased estimates of confounders (Burgess and Thompson 2012). In the analysis below, we also include *Age* in both equations because we found that our instruments do not eliminate the imbalance in age (models 2 to 4 in Table 1). We report clustered bootstrap standard errors by each politician to account for the fact that the same politicians become a minister multiple times.

Results

Model 1 of Table 2 shows the results of our baseline specification. It is based on a multilevel logistic regression with random effects by cabinet. We find that closeness centrality is negatively associated with ministerial dismissal with $p = 0.01$. This means that ministers with greater network embeddedness, who can reach other politicians in the network with fewer steps, are less likely to be dismissed from their posts in the middle of the cabinet term than less connected ministers. This finding is consistent with our expectation.

The effect of network embeddedness is substantive. In our data, the mean value of closeness centrality is 4.33 and its average standard deviation within the cabinet is 0.98. Since the coefficient estimate of closeness centrality is -0.27, one standard deviation increase in closeness centrality from its mean value leads to a 7% decrease in the probability of dismissal, holding other things constant. Our results are also unchanged both statistically and substantively when we restrict our observations to LDP cabinets (section E in the appendix).

Next, we turn to the results of 2SRI regressions. In model 2, we create our instrument setting the bandwidth to ± 0.002. The results of the first-stage regression are summarized at the bottom of Table 2. First, we find that the first-stage F-statistic is sufficiently greater than 10, a commonly used cutoff for the weak instrument. Second, we see that our instrument is positively associated with closeness centrality at a statistically significant level. Moving

Table 2: The Effect of Legislative Networks on Ministerial Dismissal

	(1) Logit	(2) 2SRI BW = ±0.002	(3) 2SRI BW = ±0.003	(4) 2SRI BW = ±0.004
SECOND STAGE:				
Closeness Centrality	−0.270*	−0.194	−0.199*	−0.200*
	(0.105)	(0.099)	(0.098)	(0.098)
Age	0.020	0.034	0.034	0.034
	(0.020)	(0.017)	(0.017)	(0.017)
Female	1.183			
	(0.803)			
Dynasty	−0.203			
	(0.277)			
Local	0.352			
	(0.312)			
Bureaucrat	0.010			
	(0.298)			
Tenure	0.185			
	(0.235)			
Tenure^2	−0.005			
	(0.016)			
First-Stage Residual		0.048	0.057	0.058
		(0.144)	(0.143)	(0.143)
σ_{c2}	0.426	0.390	0.390	0.390
FIRST STAGE:				
Instrument		0.722*	0.739*	0.718*
		(0.105)	(0.179)	(0.161)
Age		−0.038	−0.038*	−0.038*
		(0.020)	(0.006)	(0.006)
σ_{c1}		1.503	1.507	1.505
N	1,483	1,483	1,483	1,483
N of Cabinets	94	94	94	94
First-Stage F-test		25.65	32.88	32.40

Note: *p<0.05. Model 1 is estimated with a logistic regression with random effects by cabinet. Models 2 to 4 are estimated using a two-stage residual inclusion (2SRI) estimator with cabinet random effects. σ_{c1} and σ_{c2} are variance components of cabinet random effects in the first- and second-stage regressions, respectively. Clustered bootstrap standard errors in parenthesis (clusters defined by politician).

to the results of the second-stage regression at the top of Table 2, we find that closeness centrality is negatively associated with dismissal. The coefficient estimate fails to reach the 95% level of statistical significance only slightly ($p = 0.051$).

In models 3 and 4 of Table 2, we construct our instruments using the bandwidths of ±0.003 and ±0.004. In both models, the first-stage regressions suggest that there is no strong evidence for weak instrument and our instruments are strong predictors of closeness

centrality. In the second-stage regressions, the two models show that the coefficient on closeness centrality is -0.199 and -0.200, respectively. Both estimates are statistically significant at the 95% level ($p = 0.043$ and 0.042). In general, 2SRI generates more conservative estimates of closeness centrality than a multi-level logistic model. However, we confirm that the effect of network embeddedness remains negative and statistically significant. Hence, the two different approaches consistently suggest that ministers with greater embeddedness are less likely to be dismissed than those with lower embeddedness, which corroborates our expectation.

Mechanism

Although we found a negative association between network embeddedness and ministerial dismissals, the underlying mechanism remains unexplored. In fact, while we claim that ministers with greater network embeddedness tend to face less severe challenges and criticisms from other politicians than ministers with fewer connections, there are other potential explanations. For example, network embeddedness may provide an information advantage within the parliament, which enables ministers with greater closeness centrality to perform better than ministers with lower centrality. To validate our argument, we plan to analyze Diet debates and check the raw count or relative share of the number of times that opposition MPs mentioned a given minister's name or posed questions to the minister, which may be a rough measure of scrutiny and criticism by the opposition. We also want to test the heterogeneous effect of networks on dismissals by cabinet popularity (e.g., Ono 2012).

Conclusion

In this study, we explore how social networks influence ministerial dismissals by analyzing the patterns of ministerial turnover in Japan for seventy years between 1947 and 2017. Although our empirical analysis is still incomplete because we have not validated our mechanism, we

provide some evidence that the network embeddedness of ministers is negatively associated with the likelihood of their replacement.

The contribution of this study is twofold. First, it offers one way to effectively draw social networks among MPs in arena legislatures using their co-directorship in legislative committees, where MPs representing their parties negotiate and bargain behind the scenes. Second, this study further deepens our understanding of the determinants of ministerial turnover and the role of legislative networks in the policy-making process. While the existing literature has paid attention to the personal attributes of ministers, political institutions, and external political contexts to understand ministerial turnover, no study has yet to examine the effect of legislative networks that politicians develop extensively in the parliament. Future research might be to analyze the text data of parliamentary speech to explore whether the network embeddedness of ministers truly reduces scrutiny and criticisms toward them from oppositions in the legislature.

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Online Appendix for: "Legislative Networks and Ministerial Turnover: Analyzing the Impact of Network Embeddedness on Ministerial Dismissals"

Contents

A Who Becomes a Committee Chair/Director?

Table A.1: The Determinants of Committee Leadership

	(1) All Committees	(2) Pork-Related Committees
Age	−0.019*	−0.007*
	(0.002)	(0.003)
Female	−0.595*	−1.130*
	(0.104)	(0.169)
Dynasty	0.050	0.052
	(0.045)	(0.051)
Local	0.195*	0.289*
	(0.046)	(0.052)
Bureaucrat	−0.018	−0.115
	(0.055)	(0.066)
Tenure	0.681*	0.871*
	(0.031)	(0.050)
Tenure^2	−0.071*	−0.114*
	(0.003)	(0.006)
σ_t	0.250	0.166
N	12,231	12,231
N of Parliaments	25	25

Note: *p<0.05. Models are estimated with a logistic regression with random effects by parliament. σ_t is a variance component of parliament random effects. Standard errors in parenthesis.

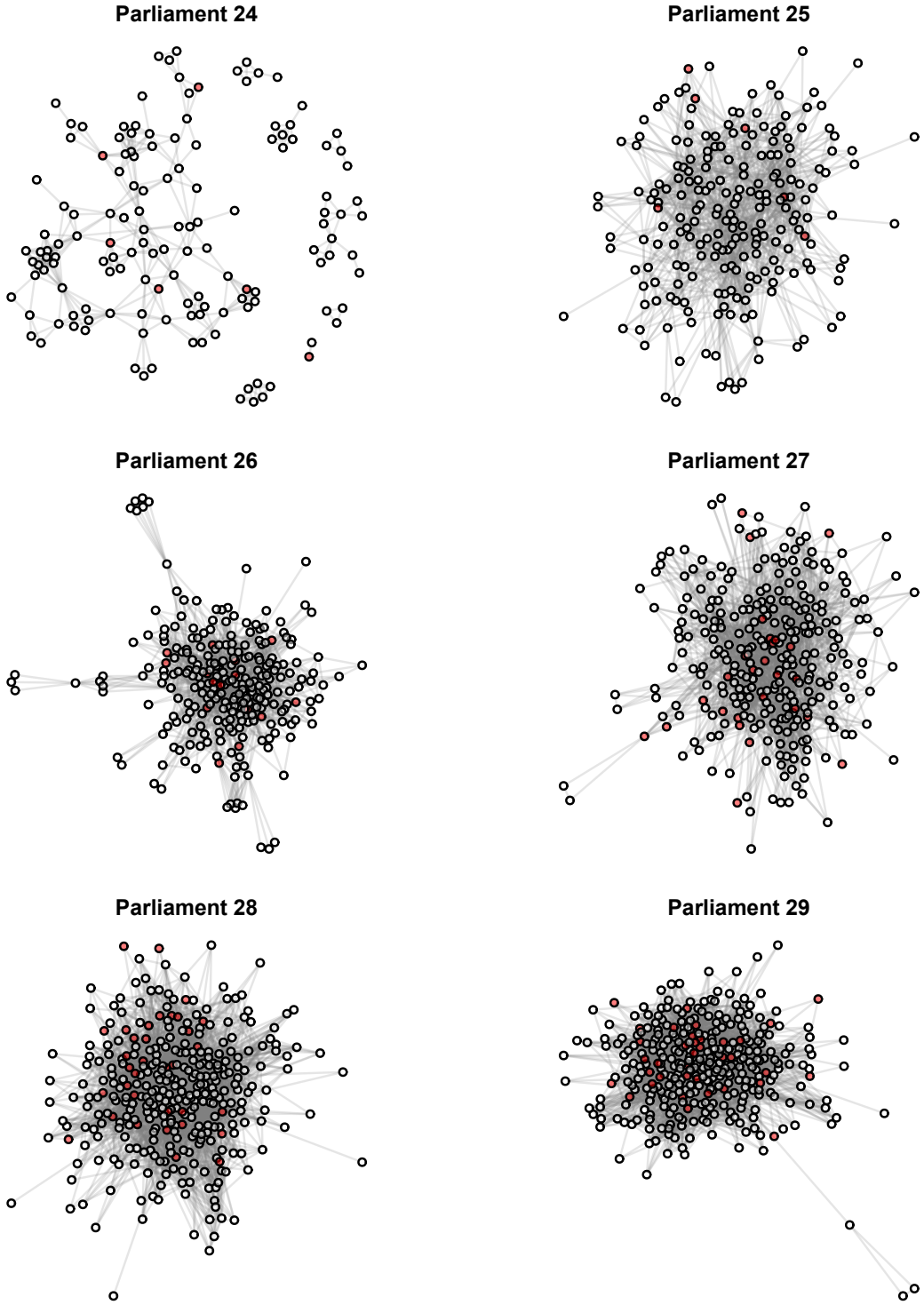
In Table A.1, we explore who is more likely to be a committee director. The observations include all politicians who had a seat in the HOR at some point between the 23rd and 47th parliaments (1947-2017). The outcome variable takes the value of 1 if politician i is appointed to a committee chair or direction in any session of parliament t. The models are based on a logistic regression with random effects by parliament. In model 1, we focus on appointments in all committees. By contrast, in model 2, we focus exclusively on appointments in committees that deal with pork-related issues (e.g., agriculture, infrastructures, mining, etc.).

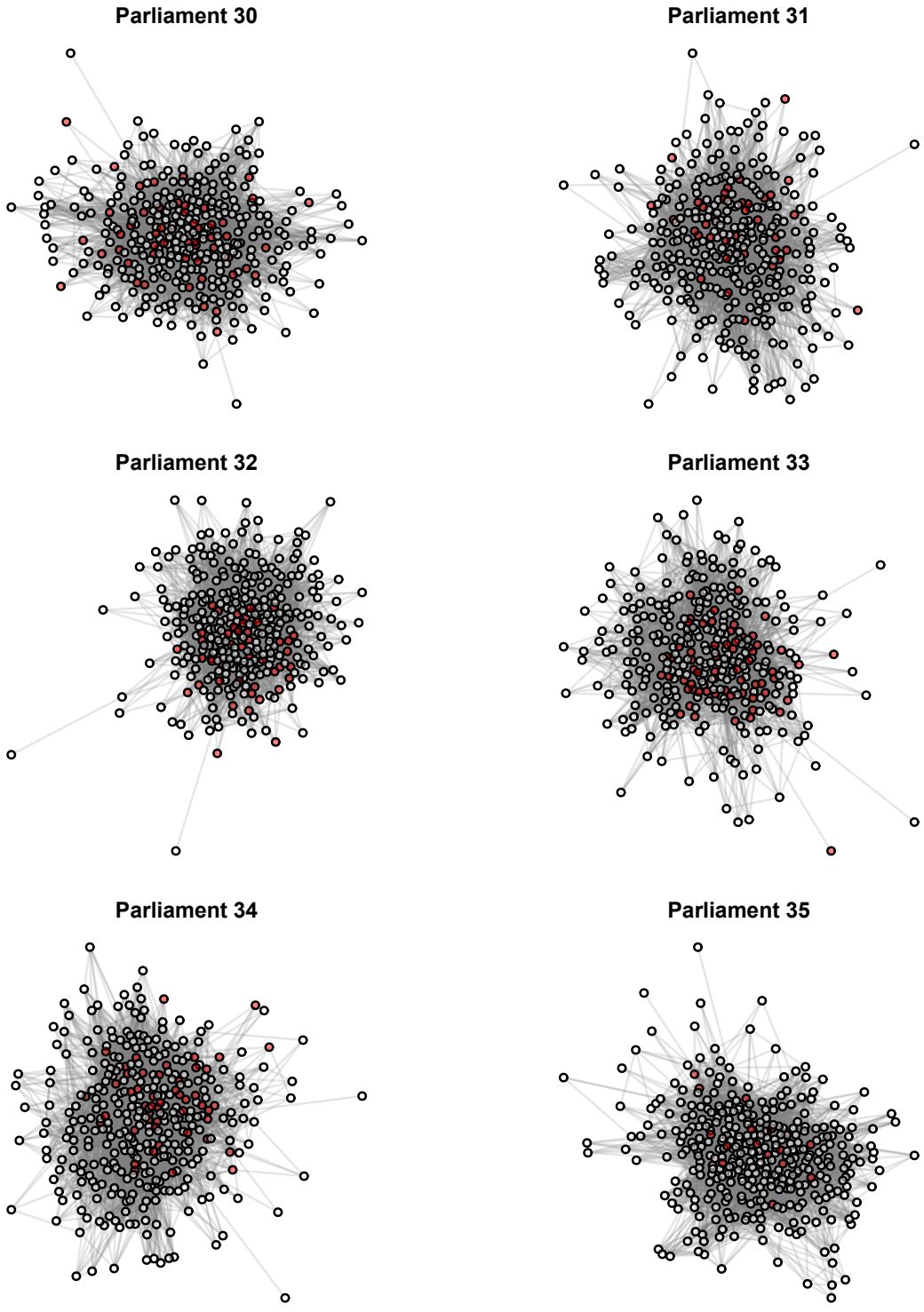
We find that age is negatively correlated with the appointment to the committee leadership. Women are less likely to be appointed to a committee leader than men and even less so in pork-related committees. Local political experiences have a positive effect on the probability of the appointment to committee leadership, especially in pork-related committees.

Finally, we also see a strong inversed U-shape relationship between tenure and committee leadership appointment. Note that we also see a similar inversed U-shaped relationship between tenure and closeness centrality.

B Legislative Networks by Parliament

Figure B.1: Legislative Networks (Red Nodes = Ministers)

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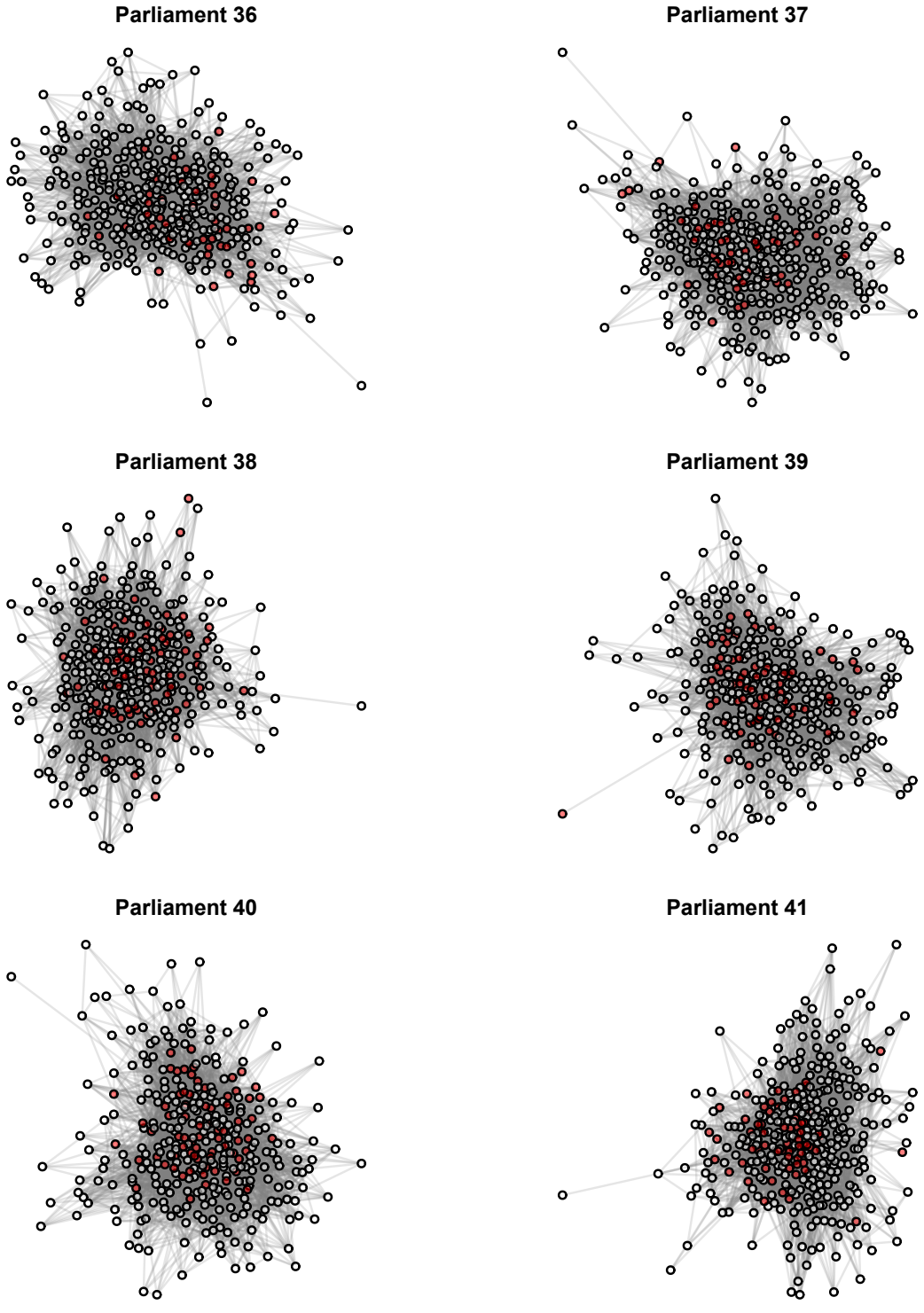
Parliament 31
Parliament 32
Parliament 33
Parliament 34
Parliament 35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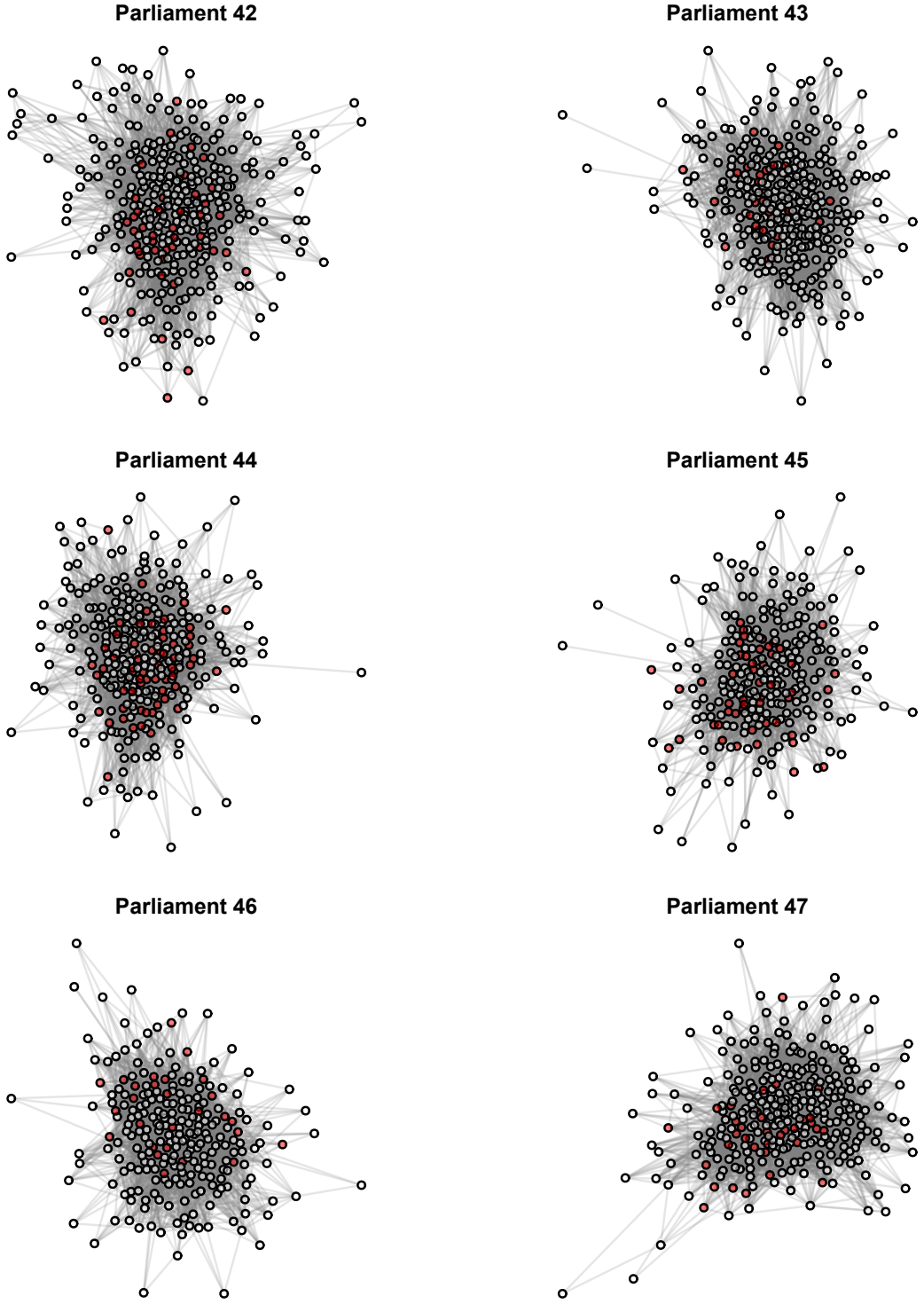
Parliament 37
Parliament 38
Parliament 39
Parliament 40
Parliament 41


Parliament 42
Parliament 43
Parliament 44
Parliament 45
Parliament 46
Parliament 47

C Does Closeness Centrality Predict Cabinet Appointment?

Table C.1: The Effect of Legislative Networks on Cabinet Appointment

	All			LDP		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Big 3	Non-Big 3	All	Big 3	Non-Big 3
Closeness Centrality	−0.026	−0.332**	0.064	−0.031	−0.396**	0.090*
	(0.036)	(0.063)	(0.041)	(0.041)	(0.069)	(0.047)
Age	−0.005	−0.033**	0.001	−0.016**	−0.045**	−0.008
	(0.005)	(0.012)	(0.005)	(0.006)	(0.013)	(0.006)
Female	0.719**	0.065	0.799**	1.087**	0.527	1.063**
	(0.280)	(0.744)	(0.290)	(0.312)	(0.755)	(0.322)
Dynasty	0.248**	0.521**	0.143*	0.190**	0.665**	0.081
	(0.083)	(0.196)	(0.086)	(0.089)	(0.224)	(0.093)
Local	−0.228**	−0.264	−0.216**	−0.194*	−0.051	−0.207*
	(0.096)	(0.242)	(0.100)	(0.105)	(0.262)	(0.109)
Bureaucrat	0.313**	0.564**	0.187*	0.287**	0.527**	0.174*
	(0.092)	(0.196)	(0.097)	(0.100)	(0.219)	(0.105)
Tenure	1.472**	1.460**	1.374**	1.529**	1.623**	1.424**
	(0.086)	(0.168)	(0.095)	(0.094)	(0.187)	(0.104)
Tenure^2	−0.091**	−0.075**	−0.090**	−0.092**	−0.081**	−0.091**
	(0.006)	(0.011)	(0.007)	(0.007)	(0.012)	(0.007)
σ_t	0.306	0.173	0.262	0.200	0.000	0.178
N	6,627	6,627	6,627	5,214	5,214	5,214
N of Parliaments	24	24	24	19	19	19

Note: *p<0.05. The models are estimated with a logistic regression with random effects by parliament. σ_t is a variance component of parliament random effects. Standard errors in parenthesis.

In Table C.1, we predict cabinet appointment on closeness centrality. In models 1 to 3, observations include politicians in the governing parties in the 24th to 47th parliaments. In models 4 to 6, observations are LDP politicians in the 27th to 47th parliaments except for the 40th parliament.

In models 1 and 4, the outcome takes the value of 1 if a politician i is appointed to a minister in any cabinet in parliament t.[1] In models 2 and 5, the outcome takes the value of 1 if politician i is appointed to one of the three most prestigious ministerial posts (Minister of Finance, Minister of Foreign Affairs, and Minister of Trade and Industry/Minister of

[1]Note that there are normally more than one cabinets in parliament t.

Economy, Trade and Industry; henceforth Big 3) during parliament t. Finally, in models 3 and 6, the outcome takes the value of 1 if politician i in parliament t is appointed to a post other than the three most prestigious ones (Non-Big 3). The models are estimated with a multi-level logistic regression with random effects by parliament.

In models 1 and 4 of Table C.1, which focus on the appointment to any ministerial post, we fail to find that social connections have a significant effect on cabinet appointment. However, once we differentiate ministerial posts by their importance, we see some interesting patterns. First, in models 2 and 5, we find that closeness centrality is negatively correlated with the appointment to Big 3. One reason for this is that in order to gain Big 3 positions, politicians must serve for relatively long terms in parliament. Because those with longer tenure tend to have lower closeness centrality than those with shorter tenure, politicians who are promoted to one of Big 3 may still have lower closeness centrality than others even after we control for tenure and its squared term. Second, although the effect of closeness centrality is not significant in model 3, it shows a marginally significant positive effect on the appointment to Non-Big 3 in model 6 ($p = 0.055$).

One important takeaway of this exercise is that the effect of closeness centrality on promotion may vary by the type of ministerial position. In this respect, which post a politician can get may be seen as a post-treatment variable of closeness centrality.

D The Descriptive Statistics of Minister Data

Table D.1: Descriptive Statistics

Variable	N	Mean	SD	Min	Max
Ministerial Dismissal	1,483	0.063	0.243	0	1
Closeness Centrality	1,483	4.327	1.600	0	6
Age	1,483	59.184	8.049	31	81
Tenure	1,483	6.289	2.505	1	14
Female	1,483	0.023	0.150	0	1
Dynasty	1,483	0.593	0.491	0	1
Local	1,483	0.220	0.414	0	1
Bureaucrat	1,483	0.305	0.461	0	1
Instrument (BW = $\pm$0.002)	1,483	0.061	0.236	0	1
Instrument (BW = $\pm$0.003)	1,483	0.073	0.255	0	1
Instrument (BW = $\pm$0.004)	1,483	0.077	0.260	0	1

E LDP Cabinets Only

Table E.1: The Effect of Legislative Networks on Ministerial Dismissal among LDP Cabinets

	(1)
Closeness Centrality	−0.237*
	(0.107)
Age	0.009
	(0.021)
Female	1.568*
	(0.645)
Dynasty	−0.525
	(0.291)
Local	0.113
	(0.330)
Bureaucrat	−0.129
	(0.331)
Tenure	0.429
	(0.269)
Tenure^2	−0.016
	(0.017)
σ_c	0.334
N	1,217
N of Cabinets	75

Note: *p<0.05. The Model is estimated with a logistic regression with random effects by cabinet. σ_c is a variance component of cabinet random effects. Standard errors in parenthesis.

In model 1 of Table E.1, we reestimate model 1 of Table 2 in the main text using only LDP cabinets. The coefficient on closeness centrality keeps showing a negative and statistically significant sign. Hence, legislative connections have a negative impact on their dismissal among LDP cabinet members. Although we do not report here, we do not find the significant effect of closeness centrality on resignation when we implement 2SRI to the LDP subsets.