Institutional Weakness and Societal Vulnerability: Evidence from the Sichuan Earthquake

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Abstract

Institutions that are built on corrupt or patronage-based foundations may continue to operate without fully exposing their flaws because they are never put under stress. However, they can silently undermine a society's ability to withstand negative shocks and amplify the damage when a shock occurs. In this paper, I use the 2008 Sichuan Earthquake as a unique opportunity to reveal the latent vulnerability that institutional weakness creates. Using an original dataset that covers 1,065 buildings in the earthquake-affected area, I show that buildings constructed when county officials had patronage connections to their supervisors (based on hometown ties) are 83 percent more likely to collapse compared to the no-connection benchmark. I provide supplementary evidence that poorer enforcement of building codes is most likely responsible for this difference. The findings highlight the critical interplay between governance institutions and adverse shocks.

Keywords: Institution; Vulnerability; Patronage; Disaster; China

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1 Introduction

Effective governance institutions are essential for socioeconomic development (North, 1991; Acemoglu et al., 2001). Yet institutional weaknesses often go unnoticed until they are exposed by a severe shock. For example, the U.S. government's inadequate preparedness for emergency response was revealed in the wake of Hurricane Katrina's excess damage. The risks associated with unregulated financial intermediaries became evident only after the financial crisis hit. Many institutions that are built on corrupt or patronage foundations may persist without fully revealing their flaws or shortcomings because they have never been stress tested.

In this paper, I show that unobserved institutional weaknesses can silently undermine a society's capabilities to withstand negative shocks and amplify the damage when a shock arrives.[1] That is, institutional weakness creates a societal vulnerability to more severe consequences from adverse shocks. Although this intuition has been articulated widely in many contexts and has provoked sustained interest in the scientific community, there is limited empirical evidence on its existence or the underlying mechanisms at work.[2]

I do so in the context of the 2008 Sichuan Earthquake, which killed 87,587 people, making it the third deadliest quake of the 21st century. Most of the deaths resulted from building collapses. In the earthquake's aftermath, there were many anecdotal accounts of substandard construction, which possibly raised the death rate. One salient example is the observably unequal levels of damage buildings in near-identical locations. A photo published in *New York Times* shows a completely destroyed primary school whereas two adjacent buildings survived fairly well (Yardley, 2008). Post-earthquake reconnaissance surveys also reveal that most of the buildings that collapsed featured a lack of reinforcing materials in their columns and had very little seismic resistance, ductility, or redundancy (Miyamoto and Gilani, 2008; He et al., 2011a,b). The seismic inadequacy is often attributed either explicitly or implicitly to

[1] I adopt Brinks et al. (2019)'s term "institutional weakness" to refer to any failure of an institution to reach its desired outcomes. This can be caused by various factors, such as poor design, inadequate adherence to established rules, or inconsistency.

[2] Natural scientists who study natural disasters have highlighted the need to consider the interplay between naturally-occurring hazards and the vulnerability induced by institutional factors (O'Keefe et al., 1976; Adger et al., 2005; Mcnutt, 2015; Eakin et al., 2017); however, empirical evidence is currently limited to cross-country correlations between democratic or corruption indices and disaster-related fatalities (Kahn, 2005; Escaleras et al., 2007; Ambraseys and Bilham, 2011).

the poor institutional enforcement of the relevant building codes. [3] Yet, there is no systematic evidence to substantiate this link.

The unequal damage to buildings in the earthquake offers a unique opportunity to dig into the institutional sources of such vulnerabilities. I focus specifically on the variations generated by patron-client relations, an informal institution that is globally widespread and one that has long been embedded in China's political system.[4] In China's local politics, patron-client connections play a crucial role in shaping political selection, resource allocation and governmental accountability (Shih, 2012; Jia et al., 2015; Fisman et al., 2020; Jiang and Zhang, 2020; Chu et al., 2020). It is worth emphasizing that the societal consequences of patronage connections are theoretically ambiguous, hinging to a large extent on whether they are used to facilitate soft information transfer or favor exchange.[5] Thus, whether these connections led to more or less damage in the earthquake (and more generally work to society's benefit or detriment) remains largely an empirical question.

To investigate this question, I construct an original dataset at the building level, with which I am able to associate vulnerability (revealed by damage levels) with past patronage networks based on a building's year of construction. The sample consists of 1,065 buildings in the quake-affected area, constructed between 1978 and 2007. The types of buildings include schools, hospitals, government headquarters, state-owned factories, and other public organizations. For each building, I first observe its damage level in the 2008 earthquake from official seismic surveys on a 5-point scale (1=intact, 5=fully collapsed). I then identify, from archival sources, the incumbent county officials during the year in which the building was constructed. [6] I measure the presence of patron-client connections at the county-year level (or "patronage leadership") by whether these officials had the same hometown with their prefectural

[3]There has been much media coverage of *doufu zha gongcheng* (shoddily constructed buildings, literally "tofu dregs engineering") following the earthquake, with an emphasis on school buildings. The reports uncovered a pattern of corner-cutting and laxity about quality control in the construction process. This was even admitted by a government official, who stated, "Its structure is not completely sound or its materials are not very strong ... We've built school buildings relatively fast, so some construction problems might exist." See Wong (2008) for details.

[4]I follow Hicken (2011) to defined patron-client relations broadly as personal connections between individuals of unequal political status.

[5]In the former case, they can help to align incentives and consolidate monitoring effectiveness (Dewan and Squintani, 2016; Jiang, 2018; Toral, 2019; Voth and Xu, 2020), whereas in the latter, they may reduce accountability and undermine law enforcement (e.g., Stokes, 2005).

[6]I consider the two leading officials in each county: the party secretary and the governor.

leaders, or "hometown ties," a traditional and prevalent means of favor exchange in China (see, for example, Fisman et al. (2017)).

My identification strategy is similar to a generalized difference-in-differences framework. Specifically, I look at buildings constructed under the authority of county officials with prefectural connections and compare these buildings to others built in the absence of such connections. The design exploits two sources of variation. First, buildings located within the same county (and, therefore, that experienced similar seismic intensity) differ in their years of construction by which connections are defined. Second, for buildings constructed during the same year (so that they are in the same cohort), there is spatial variation in patronage ties. The identification assumption states that in the absence of connection, the difference between buildings constructed in connected and unconnected county administrations should be constant over time. In particular, the strategy does not require connections to be assigned at random as long as the preceding assumption holds.

The estimated results indicate that patronage connections have played a significant role in creating vulnerability: Buildings constructed when the county officials had connections are expected to incur more severe damage during the earthquake. In particular, the probability of partial or full collapse increases by 13 percentage points (or 83 %) for buildings constructed under the authority of connected officials relative to the unconnected benchmark. I evaluate my identifying assumption using event-study type analyses on the effect of gaining and losing connections. In so doing, I find no differential effects for buildings constructed before the county gained its connection, and the difference diminishes after the connection terminates. That is, while the allocation of connected officials was not necessarily random, the unobservables associated with this allocation do not seem to directly affect building damage in the absence of such connections, an observation that allows for a causal interpretation. I also consider other prominent mechanisms that might bias my estimates — selection in damage reporting, preference for building features, economic resources, and any unobservable factors associated with a specific hometown — and find little support for any of these possibilities.

I present evidence that the association between patronage leadership and building damage likely resulted from poor enforcement of the relevant building codes, which is a specific type of institutional weakness, in administrations led by connected officials. First, patronage connections matter primarily for buildings located in moderately affected regions where seismic intensity is equivalent to the resistance requirements. These buildings should have survived the quake, but suffered greater damage than would have been expected, which is often a pattern indicative of corner-cutting and code violations. Second, the detrimental consequences are observed mostly for officials who are in direct charge of public administration and legal enforcement (i.e., the governors) rather than those who maintain more political authority to set agendas (i.e., the party secretaries); this finding suggests that poor enforcement by the government administration might have contributed to the lack of compliance. Third, the effects are driven primarily by schools and hospitals, whereas government headquarters in which officials themselves stay appear immune from the effects; this result suggests that connected officials were able to implement the code effectively when they internalized the cost and benefit of building safety into their decision making. Finally, I find that private enforcement, proxied by the involvement of private investment or donation, helps to mitigate or even offset the negative consequences associated with having connected officials. Although none of these pieces of evidence is conclusive on its own, they collectively present a chain of evidence that patron-client connections may have created weaker institutional enforcement, possibly as a result of the shirking or corruption behaviors of connected officials.

I explore two reasons for which patron-client connections may have created such a weaker institutional environment: negative political selection and moral hazard incentives. I disentangle these two channels by exploiting variations in connectedness status both at the time of official appointment and at the time of building construction. The results show that both "connected appointment" and "connected construction" lead to worsened building damage, indicating that both the selection and the incentive explanations may be at play. This further implies that senior officials in the patron-client relationship may have provided positions to connected junior officials who have lesser administrative ability or are less ethical and protected them from being accountable for their breach of duty.

To establish the broader implications of my study, I supplement the building-level findings with an analysis of county-level aggregates that allows me to examine a set of economically relevant outcomes. Using a dataset that covers all 181 counties in Sichuan Province, I first document a positive cross-county correlation between earthquake losses and the period of the connectedness of county officials: One additional year of having a connected official is associated with an 8 % increase in mortality and a 3 % increase in direct economic loss from the earthquake.[7] This pattern is observed across all sectors except for government agencies. These findings, though not necessarily causal, show that the conclusions drawn from my building-level analysis are likely relevant for aggregate outcomes.

My work sits at the intersection of two large literatures: research on the consequences of adverse shocks and work on the role of governmental institutions in determining societal outcomes. Although each has been studied separately, their interplay is less well-explored.

The paper provides empirical evidence that institutional weaknesses can create a societal vulnerability that amplifies the damage from adverse shocks. Such concerns may only increase in future years, as economies become ever more vulnerable to climate change and other systemic risks. There have been multiple calls from the scientific community to seriously consider the role of institutional factors in the impact of adverse shocks (O'Keefe et al., 1976; Adger et al., 2005; Mcnutt, 2015; Eakin et al., 2017). Empirical examinations of this hypothesis, however, are limited to cross-country correlations between institutional indexes (such as democracy and corruption perception) and disaster deaths (Kahn, 2005; Escaleras et al., 2007; Ambraseys and Bilham, 2011). I contribute to the literature by offering micro-level causal evidence that substantiates this idea. In particular, I highlight the long-term vulnerability that institutional failure can create even before hazardous events occur. Although the consequences may be hidden initially, they may accumulate over time and exacerbate the damage from a much later exogenous shock. This distinguishes my work from the literature on distributive politics in post-disaster reliefs (Tarquinio, 2021). The paper also differs from studies on the institutional causes of famines in its emphasis on the vulnerability created by weak accountability and poor law enforcement instead of food inequality that results from inappropriate procurement (Sen, 1981; Lin and Yang, 2000; Kung and Chen, 2011; Meng et al., 2015). Although I focus on the physical loss from a natural disaster as a particularly striking example, the notion that institutional weaknesses create a societal vulnerability to adverse shocks may be applicable to other settings, including the financial system and public health infrastructure.

There is consensus among social scientists that political institutions matter for development (see North (1991) and Acemoglu et al. (2001) for seminal contributions and Baland et al. (2020) for a recent review). The literature has generally focused

[7] In this paper, mortality includes missing persons as well as those whose death is verified.

on the impacts of institutions on aggregate developmental outcomes (mostly GDP per capita or growth), without considering the possibility that the impacts may vary across states of nature. This gap in the literature leaves many unanswered questions — for instance, why do some economies with weak institutions experience high growth and then suddenly collapse? I speak to these puzzles by highlighting that an institution's performance may be state-contingent: A fragile institution may appear to perform well under "normal" circumstances, but create vulnerabilities that emerge only when tested under the pressure of unanticipated negative shocks. Thus, the deficits of an institution, especially one that fosters high growth, may become clear only when stress tested.

Given my focus on patron-client connections, I also contribute to the growing literature on the role of patronage in the functioning of bureaucracies. Recent empirical work has demonstrated the prevalence of favoritism associated with patronage connections in various institutional and cultural contexts, particularly in the allocation of public offices and other resources (see, e.g., Colonnelli et al., 2020; Jia et al., 2015; Shih, 2012; Xu, 2018; Voth and Xu, 2020; Fisman et al., 2020; Jiang and Zhang, 2020). Although some of these papers also concern the costs and benefits that arise from such favoritism, they focus mainly on the tradeoff within the principal-agent framework, without a clear notion of its societal consequences, which may not necessarily align with the principals' payoffs.[8] One attempt is a study by Jia (2017), who finds that connected politicians seem to favor technologies that pollute but enhance economic growth. By focusing on earthquake damage plausibly due to corner-cutting, I emphasize the lack of accountability rather than promotion incentives for multi-task agents.

More broadly, this paper offers several new perspectives on the measurement, consequences, and alleviation of corruption. In terms of measurement, the literature relies mainly on surveys or experiments to identify evidence of corruption (Olken, 2006; Bertrand et al., 2007; Weaver, 2021); these approaches are either subjective or expensive (Bertrand and Mullainathan, 2001). I suggest a novel detection device, which exploits variations in disaster damage, that is objective, economical, and applicable to a broad range of settings. As to the consequences of corruption, the

[8]For example, in a very well-identified study on how patronage affects the promotion and incentives of governors within the Colonial Office of the British Empire, Xu (2018) provides convincing evidence that connected colonial governors generated less revenue for the British Empire; yet it is less clear whether lower revenue generation is good or bad for colonized people.

literature focuses mainly on the efficiency loss from misallocation and distortion (see, e.g., Schoenherr (2019), Lehne et al. (2018), Cingano and Pinotti (2013), and Olken and Pande (2012)), whereas my work highlights a more direct and salient social cost on a substantial scale. This aligns my work with that of Fisman and Wang (2015) and Jia and Nie (2017); they look at firm-government connections whereas I focus on corruption within bureaucratic hierarchies. Finally, the existing literature generally emphasizes the roles of electoral accountability and government auditing in fighting corruption (Olken, 2007; Bobonis et al., 2016; Avis et al., 2018). My results suggest, in contrast, that private participation also may help to alleviate corruption in certain public projects, especially in settings in which auditors themselves could collude with the agent (Duflo et al., 2013; Chu et al., 2020).

The remainder of this paper is organized as follows. Section 2 provides the geographical, institutional, and cultural background of the 2008 Sichuan earthquake, along with the roles of patronage ties in China's local politics. Section 3 presents the sources and processing of the data and their potential limitations. The empirical design and results that link patronage connections to building damage are presented in Section 4. Section 5 concerns the interpretation of the main findings, and Section 6 concludes.

2 Background

2.1 The 2008 Sichuan Earthquake

The 2008 Sichuan earthquake occurred on May 12, with a moment magnitude of $7.9 M_W$. The epicenter was Yingxiu Town, Wenchuan County, 80 kilometers northwest of Chengdu, the capital city of Sichuan Province. The earthquake killed 87,587 people, injured another 374,643, and incurred a direct economic loss of 845 billion RMB (80% of Sichuan's 2007 GDP), which makes it one of the most costly earthquakes in human history. [9] Most of the deaths resulted from the destruction of buildings; of these, public buildings were among the most vulnerable and deadly ones in the earthquake.

[9] The death toll of this earthquake was extraordinarily high compared with other earthquakes of similar magnitude. Figure C1 provides a plot of the fatalities of the 20 most notable earthquakes (in terms of magnitude) since 2000 against their magnitudes. The death toll in the 2008 Sichuan earthquake is much larger than that of the other earthquakes of similar magnitude and comparable only to the two strongest earthquakes of magnitudes 9 and above. The relationship holds even after we control for population density.

[10]

Although the official announcement attributed the collapse of buildings to the severity of the earthquake, it is widely believed that suspected shoddy construction also was responsible. A scandal emerged, with the salient observation that some buildings crumbled to dust — the structures were so inflexible that they collapsed in less than 10 seconds with no shaking at all — while others directly adjoining them remained mostly intact. The fragility of certain buildings sharply contrasted with the performance of a few sturdy buildings that were standing at the very heart of the disaster zone. [11] Investigative news reports have discovered the use of low-grade cement and inadequate steel reinforcements in some destroyed buildings; the reports also probe a few dubious construction practices that may be associated, either directly or indirectly, with the local government's neglect of building safety. Although there are widespread anecdotes and speculation, there is no formal evidence that can be used to examine quantitatively the potential link between possible corruption and its associated damage.

2.2 Patronage in China's Local Politics

Patronage networks are widely observed across the spectrum of regime types and are often considered a prominent example of informal institutions. The defining feature of this relationship is a favor exchange through informal personal connections between two or more actors (or groups of actors) of unequal political status (Hicken, 2011). Such networks have been long embedded in Chinese society since as early as the second century A.D. (Ebrey, 1983), and are pervasive in shaping the political and social lives of the Chinese people.[12] In China's political system, particularly at the county and prefectural levels, the senior officials (patrons) provide a range of benefits, including resources, information, opportunity, and protection in times of trouble, in

[10]In a survey of 484 buildings, 57% of the schools were no longer usable or had to be removed immediately, more than twice as much as the share of residential houses (Ye and Lu, 2008). It is also remarkable that 87% of the government headquarters in their sample remained safe, aside from some repair requirements.

[11]The two most prominent examples are Bailu Town Central Primary School and Liu Han Hope Elementary School, both located directly above the rupture surface. In the former case, a three-story school building was elevated three meters above the ground, but the main building stood firmly, and 1,046 students successfully evacuated the building. See Branigan (2008) and China Daily (2011) for details.

[12]In particular, patron-client connections constitute the basis of what Nathan (1973) famously refers to as "factions" in CCP elite politics.

return for loyalty, obedience, and political support from junior officials (clients).[13]

The favor exchange between senior and junior officials via patronage ties may undermine local institutional environments and law enforcement in two distinct ways. First, senior officials at the prefectural level may set a lower bar for their clients when appointing officials at the county level.[14] As a result, officials selected based on their patronage ties may be less qualified in terms of either ability (less competent) or ethics (more prone to corruption). Second, prefectural officials may provide protection to their clients, which prevents them from being held accountable for corrupt activity or dereliction of duty. Such lenience would cause county officials to be less scrupulous in maintaining economic and political integrity. In either case, patronage leadership would lead to a weaker institutional environment and would undermine the local governments' administrative capabilities to enforce the established rules, including the building codes.[15]

For this study, I measure patronage ties between county and prefectural officials using their hometown connections (*laoxiang guanxi*), which have been recognized as the most common and distinctive basis for the establishment of a patron-client relationship between local officials (Douw et al., 1999; Chen and Chen, 2004). Since as early as the 16th century, having a shared hometown has served as fertile ground for building social networks, creating emotional bonds, and trading reciprocal favors with people from various occupational and social backgrounds (Moll-murata, 2008). In the past few decades, social networks organized around the hometown also have played a crucial role in sustaining China's historically unprecedented rural-urban migration and the growth of private enterprise (Zhao, 2003; Hu, 2008; Dai et al., 2020). Recent studies also document the prevalence of favoritism via hometown ties in the business, political, and academic worlds (Shih, 2012; Jia et al., 2015; Fisman et al., 2017; Shen et al., 2019; Chu et al., 2020; Fisman et al., 2020). In particular, social networks based on hometowns appear to facilitate bureaucratic and business collusion. It is

[13] The political support often involves making decisions on the awarding of contracts, supporting or opposing particular policy initiatives, and voting for candidates who are being considered for promotion. See Hillman (2014) for further details.

[14] During the period studied, a prospective county official was first nominated by prefectural officials; the nomination had to be approved by provincial officials before taking effect (Yang and Peng, 2009).

[15] It is not uncommon for county officials to be directly involved in construction-related corruption activities — in terms of either direct misappropriation of public funds or bribe-taking from contractors (see, e.g., Yu et al., 2019; Zhou, 2011).

not uncommon for corrupt officials to cluster around native locations; being aware of this phenomenon, the Chinese government explicitly prohibited its officials from participating in any hometown-based associations in 2015. [16]

3 Data

I have combined information from multiple sources to construct a building-level dataset. The dataset contains 1,065 buildings from 37 counties in the heart of the earthquake zone; all of the sampled buildings were built between 1978 and 2007. [17] I measure patronage networks by the hometown connections (i.e., having the same hometown) between county officials and their prefectural-level superiors. I associate a building's damage to past patronage networks based on the year during which the building was constructed.

3.1 Building Damage

I construct the building-level dataset by combining two collections of local gazetteers. The first is the local *Earthquake Relief Reports* (*Kangzhen Jiuzai Zhi*), from which I obtain a list of buildings and the extent of damage to which they were subject.[18] The second collection is the general *County Gazetteers* (*Xian Zhi*), from which I obtain the construction history of a second list of buildings. I manually compare the two lists of buildings by their documented names and locations and have identified 1,065 buildings that were mentioned in both lists so that their damage and construction records are observed. There are five types of buildings in my linked sample: schools, hospitals, government headquarters, public organizations (e.g., libraries), and state-owned factories.

The damage levels of the sampled buildings are coded according to a 5-point scale, following the official guidelines.[19] The key features that determine a building's

[16]See, for example, Guo (2019) for a prominent example of collective corruption of high-ranking officials originating in Shanxi Province, and China Comment (2017) for a more localized case. For the government's ban on officials' participation in hometown associations, see Huang (2015).

[17]I restrict the sample to buildings constructed during this period for two reasons. First, local governance was substantially disrupted during the Cultural Revolution (1966–1976); second, there were no strict building codes in China until 1978.

[18]See Section B.1 for further details about the nature of these archival sources and the procedure to collect them.

[19]Appendix B.1 provides more details about the coding process.

damage level are the extent to which the load-bearing components were affected, and whether the building could be used with or without repairing, or had to be removed immediately. The detailed definitions and indexes of the damage levels are summarized as follows:

1. **Intact or slight damage:** Load-bearing components are intact or have minor (less than 5%) cracks; non-load-bearing components and attachments have various levels of damage; safe to use with no or minor repairs.

2. **Moderate damage:** Load-bearing components have some major cracks; non-load-bearing components and attachments have visible damage that must be repaired before use.

3. **Severe damage:** Load-bearing components have many severe cracks and minor areas of collapse; some non-load-bearing components and attachments have fallen and are no longer serviceable.

4. **Partial collapse:** Load-bearing components have deteriorated significantly and must be removed immediately.

5. **Full collapse:** The entire building has collapsed or fallen apart; nothing remains of the basic structure.

3.2 Patronage Connection

I define patronage ties between local officials and their prefectural superiors by whether they have the same city of origin (i.e., *hometown ties*).[20] I focus on the top two county officials, i.e., the county party secretary and governor, and the top two prefectural officials, i.e., the prefectural party secretary and mayor, in defining patronage ties. I construct a list of county- and prefecture-level officials and their cities of origin from various sources, including county gazetteers, *Information on the Organizational History of the CCP in Sichuan Province* (*Zhongguo Gongchandang Sichuan Sheng Zuzhishi Ziliao*), *Sichuan Year Book* (*Sichuan Nianjian*), Chen et al.

[20] The political connection literature has provided two other measures of connection in China's context: One is "college ties" (*xiaoyou guanxi*), and the other is "workplace ties" (*tongshi guanxi*). Both types of connections are relevant for the formation of patron-client networks. Information on county officials' education and working experience, however, was difficult to obtain before 2000, making it infeasible to examine the effects of these alternative ties in my context.

(2019), and the online biographies of these officials. I also collected their gender, year of birth, education, and ethnicity, whenever available.

For each county in a given year, I define the county as having a connected official if one or both of its top officials (i.e., the county secretary and the governor) share the same city of origin with at least one of their superiors (i.e., the prefecture secretary and the mayor). [21] I then associate the connectedness status of county officials to buildings in my linked sample based on the year in which the building was constructed.

3.3 Covariates

I construct additional variables to account for other factors that might determine the damage to a building from the earthquake. These covariates include: (i) building characteristics (e.g., size, height), geographical features (seismic motion intensity and terrain ruggedness), individual profiles of the officials (gender, ethnicity, age, education, and term), and county-wide socioeconomic conditions (per capita GDP and population). The definitions and construction of these variables are explained in more detail in Section B.3.

3.4 Descriptive statistics

I present the summary statistics of my building level dataset in Table C1.[22] There are 1,065 matched buildings in the sample. The damage is coded according to the 5-point scale, with a mean of 2.84. Buildings constructed under the authority of a connected county official represent 16% of the sample. Figure C2 plots the probability distribution of building damage by the connectedness of county officials. The probability of partial or full collapse (coded as 4 or 5) of connected buildings (i.e. those constructed under the auspices of connected county officials) is 2.5 times that of unconnected buildings; in particular, whereas, at the time of construction only about 16% of buildings are connected, about 50% of the fully collapsed buildings exhibit this connectedness.

[21] For transition years in which multiple county secretaries or governors have been in their positions, I considered the connections of the ones who were in their positions for the longest time within that year. Using alternative definitions does not alter my estimates.

[22] One issue highlighted in the table is the prominence of missing values for many control variables, especially those of building features. To utilize this information as much as possible in the analysis, I first encoded the missing values as 0 and then included a set of dummy indicators that denoted each of the missing variables.

3.5 Limitations

One fundamental challenge to my study is that there are no publicly available comprehensive and systematic statistics on building damage, and it is even more difficult to identify their years of construction and the economic and institutional circumstances in the past. [23] I overcome this difficulty by combining two collections of archival records — one of damage and the other of construction history — and identifying the jointly mentioned buildings. I find, however, that neither type of information is in a standardized statistical format, which introduces important caveats for the selectivity and representativeness of my sample.

One leading concern about my sampling process is that the selection of buildings is not random.[24] In particular, because the gazetteers are compiled and published by each individual county, the selection function may vary across counties, making samples from different counties incomparable as to buildings sampled. This concern can be significantly mitigated, however, by the inclusion of county fixed effects by which I compare only buildings in the same county if the selection is consistent within the county. In the possible situation that the selection function might be inconsistent even within a county, the identification relies on an additional assumption that the selection does not depend on the connectedness of county officials for the year in which the buildings were constructed.[25] In Section 4.3, I present a tentative test of this assumption and discuss the possible scenarios in which this assumption might be violated.

The second concern is that my sample may not be representative of the universe of buildings in the quake area. In particular, it takes into account only buildings that are recognizable in a county, and, for this reason, most of the sampled buildings are public projects. To address the external validity of this selective sample, I present an analysis in Section A.1 of county-level aggregates, which suggests the extent to which the conclusions drawn from my building-level sample may exhibit more general

[23]For example, one seemingly possible approach to obtain comprehensive building damage information is to identify collapsed buildings from satellite photos. Nevertheless, before-and-after comparison is difficult, because most of the available high-resolution satellite images of the area of interest were taken after 2008.

[24]For example, a county might record only buildings whose damage was salient, so that only extraordinarily good or extraordinarily bad buildings get observed.

[25]This assumption is generally reasonable, as past officials were no longer in the same positions in 2008 (some of them had even retired) and, therefore, should have a very limited impact on the compilation of the gazetteers after the earthquake.

implications.

4 Empirical Strategy and Results

This section presents the main analyses, using the building-level dataset. I describe the empirical design, discuss the identification assumptions, and formalize the model specifications. This is followed by the baseline estimation of the impact of county officials' hometown connections on damage to the buildings. I then address some of the most prominent concerns that might bias the estimates, including the selection in damage reporting, the economic conditions in the year of construction, and common shocks to officials from some specific hometown.

4.1 Research Design and Model Specification

The research design is similar to a generalized difference-in-differences framework, in which I compare buildings constructed under the authority of connected (via hometown ties) county officials relative to their unconnected counterparts. I exploit two sources of variation, which are illustrated in Figure 1 — the first for buildings located in the same county, for which the exposure to connected officials varies in the year in which they were constructed, and the second, for those constructed in the same cohort, for which the connection status of the incumbent bureaucrats varies across counties. The design is formalized by estimating the following equation:

$$Damage_{ict} = \beta HometownTie_{ct} + \delta_c + \sigma_t + \mathbf{X'_{ict}\Gamma} + \varepsilon_{ict} \tag{1}$$

where i indexes buildings, c indexes counties, and t indexes building cohorts (i.e., years of construction). The outcome of interest, denoted $Damage_{ict}$, is the damage level, on the 5-point scale, of building i, in county c, built in year t. $HometownTie_{ct}$ is an indicator variable that denotes that the county officials in county c, year t share a home of origin with their prefecture-level superiors. The equation also controls for county and year fixed effects, δ_c and σ_t, respectively; $\mathbf{X'_{ict}}$ denotes a vector of other building or county level covariates that also vary in time; and ε_{ict} denotes the error term. I compute standard errors that allow for clustering by counties on the rationale that the buildings have been sampled by individual counties. The coefficient of interest is β, which, if positive, would suggest buildings constructed under the authority of connected officials as being more vulnerable than their unconnected counterparts in the 2008 earthquake.

The estimation strategy inherits all the advantages and potential pitfalls of the classical difference-in-differences estimators. In the model, the county fixed effects control for time-invariant factors that differ between counties, including, for example, location and average earthquake intensity; they also capture the potential county-specific sampling functions of buildings and factors that may affect the allocation of connected officials across counties. The year fixed effects take into account any regular patterns of earthquake damage that affect all buildings in the same cohort, for example, building age or the construction technology. I also consider the following set of additional controls that may nevertheless vary within a county: first, the basic features of the building, such as type of use, size, number of stories and structure; second, within-county variations in the geographical characteristics of the building's location, including seismic motion (measured by peak ground acceleration) and terrain ruggedness at the building's site; and third, the profiles of the county officials, including gender, age, education, ethnicity, and term.

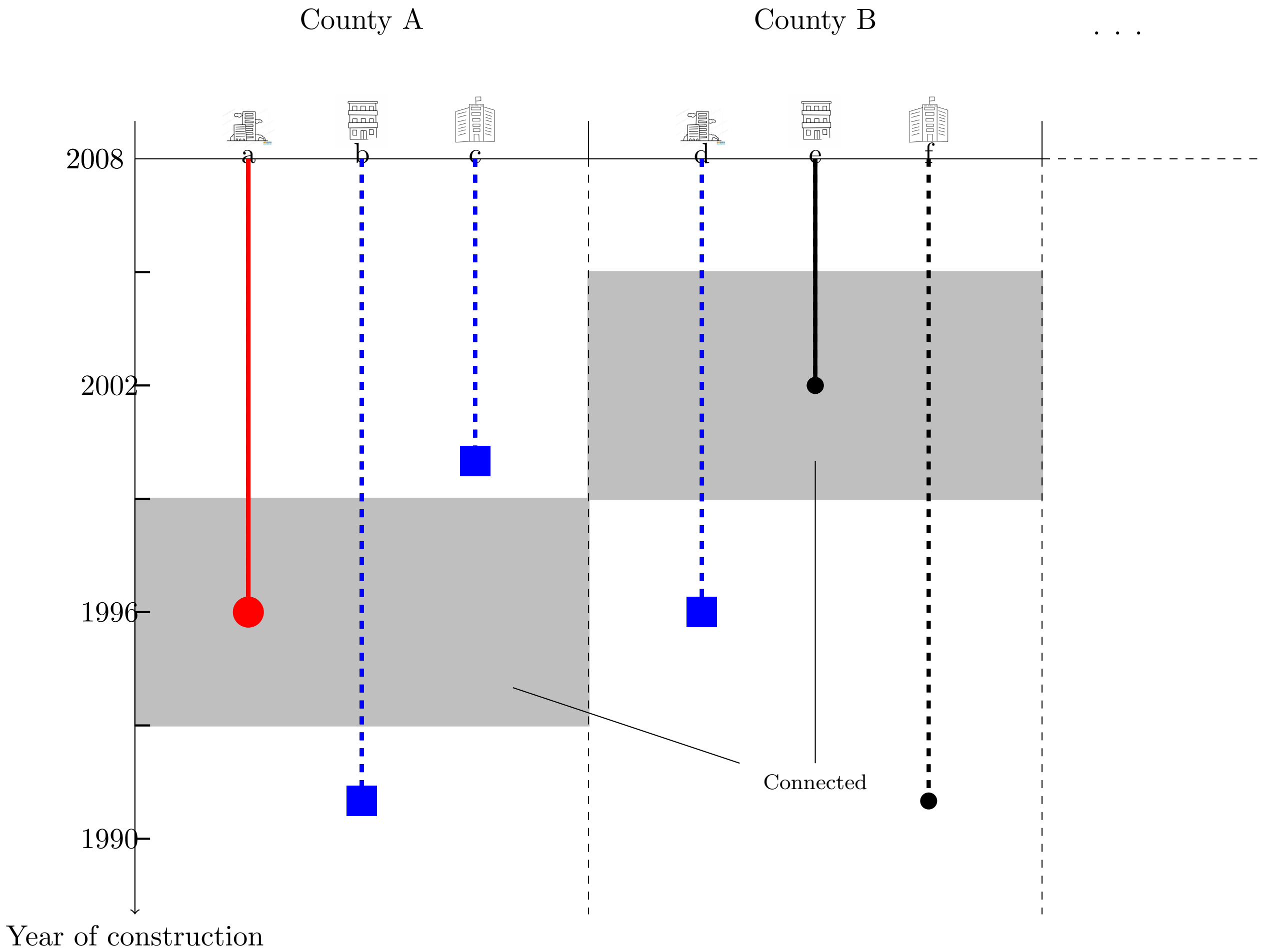
The identification requires that buildings constructed in a connected and unconnected regime should be otherwise identical in damage in the absence of the connections, conditional on the factors that have been controlled for. It does not, however, require the treatment (i.e., the presence of connected officials) to be assigned randomly as long as the proceeding assumption holds. In the following exercises, I will present various diagnostic tests of this assumption and consider some of the most prominent mechanisms through which this assumption could be violated.

4.2 Main Results

Baseline I start by estimating the effect of bureaucrats' hometown connections on the earthquake damage to buildings using both the linear and ordered-probit versions of Equation (1). The results are reported in Table 1. Column (1) shows the linear estimate of Equation (1), including only $HometownTie$ without any covariates. The estimated raw coefficient is 0.44, significant at the 1 % level. The magnitude is about 15 % of the mean damage index, or 54 % of its standard deviation.

In Column (2), I include the sets of county and year fixed effects to the equation. This specification reduces the estimated coefficient by 30 % to 0.31, significant

Figure 1: Graphic illustration of the identification design for the building-level analysis



at the 5 % level. The reduction in magnitude suggests that time-invariant county characteristics (e.g., location) and cohort effects (e.g., age) might explain a large portion of the effects. Yet, the association between hometown connections and building damage remain significant, both statistically and economically, for within-county and within-cohort comparisons.

In Column (3), I include building type × year fixed effects, which capture, for example, the evolution of technology and safety requirements that may vary across different types of buildings. In doing so, I rule out the comparison between different types of buildings and exploit only the variations among simultaneously constructed buildings identical in type. The coefficient on $HometownTie$ is almost unchanged, although the level of significance improves from 5% to 1%.

The results in Columns (4) and (5) take into consideration additional building-specific characteristics that might influence the earthquake damage. For the results in Column (4), I control for building features, including size, number of stories, number of rooms, and structure. Because these variables are available for only a very small subset of buildings in the sample, I also include a set of dummies that indicate those that are missing. The coefficient and level of significance on $HometownTie$ remain constant. The results in Column (5) take into consideration the geography of the building's location, including local peak ground acceleration (PGA) — the seismic ground motion parameter — and terrain ruggedness, both measured at the building's locality; the results remain mostly the same. Finally, in Column (6), I include the personal profiles of the county officials, i.e., their gender, ethnicity, age, education, and term, taking an average of the party secretary and the governor. Again, my estimates remain the same.

For the results in Column (7), I estimate, with the complete set of controls, the ordered-probit model of Equation (1) to accommodate the ordinal nature of the dependent variable. The estimated coefficient of $HometownTie$ on the latent outcome variable is 0.65, significant at the 1 % level. The overall marginal effect, calculated as the linear combination of the marginal effects for each outcome value, is 0.307, which is comparable to the estimated coefficients in linear models. Thus, our estimates are robust to the potential nonlinearity of ordinal damage measures.

To aid in the exposition of the estimated effect, I compute the predictive margins of $HometownTie$, i.e., the predicted probability of falling within each of the five categories by connectedness, and plot the results in Figure 2, along with the 95% confidence intervals. The figure shows a pattern that echoes my previous results: Buildings constructed under the authority of connected county officials stochastically dominate their unconnected counterparts in earthquake damage. In particular, the officials' $HometownTie$ increases the probability of partial or full collapse (coded 4 and 5) by 13 percentage points (or 83 %) from 15.7% to 28.7%.

Overall, the results in Table 1 indicate that patronage ties of local officials have a robust effect in making the buildings vulnerable in the earthquake. I also verify that my findings are robust to alternative treatment intensities such as the number of ties or the duration being connected and to using a different damage classification method[26].

[26]These results are available upon request.

Table 1: Patronage ties and building damages

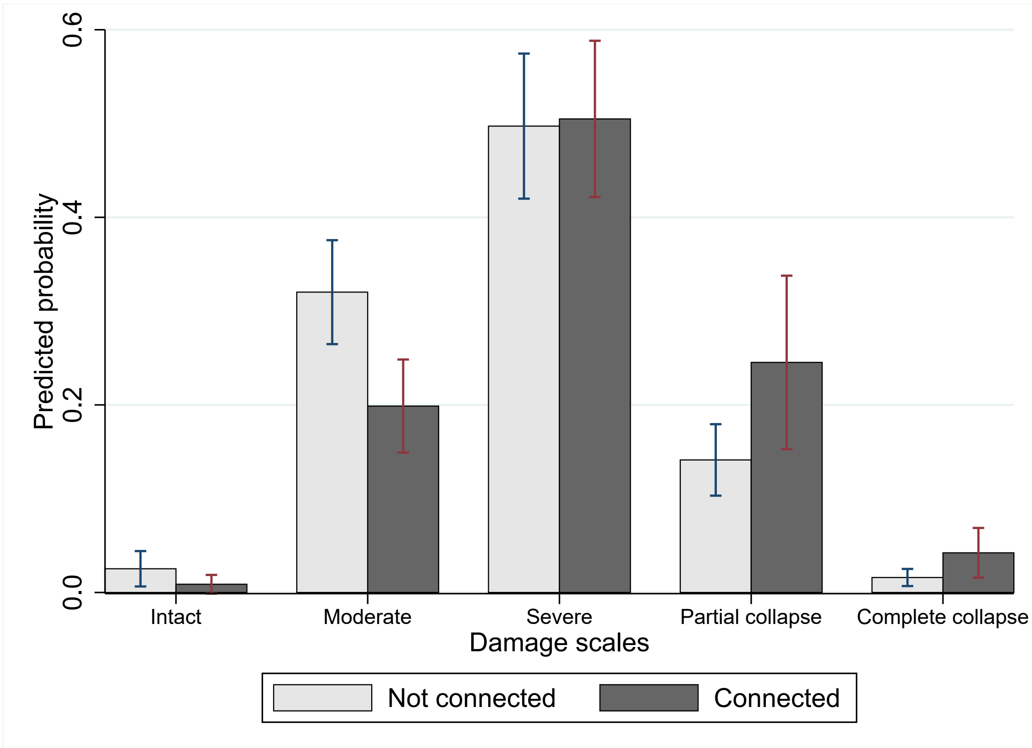
	Dependent Variable: Damage Scale (1–5)						
	OLS						Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HometownTie	0.440***	0.309**	0.310***	0.305***	0.300***	0.299***	0.634***
	(0.154)	(0.114)	(0.098)	(0.101)	(0.102)	(0.097)	(0.161)
Individual Controls						Y	Y
Geographic Controls					Y	Y	Y
Building Controls				Y	Y	Y	Y
BuildingType × Year FE			Y	Y	Y	Y	Y
County FE		Y	Y	Y	Y	Y	Y
Year FE		Y	Y	Y	Y	Y	Y
Marginal effect							0.307
Wild cluster p-value	0.015	0.008	0.018	0.028	0.023	0.036	
Mean(Dep.var)	2.856	2.857	2.861	2.861	2.861	2.861	2.861
# Counties	37	35	35	35	35	35	35
# Observations	1065	1062	1050	1050	1050	1050	1050
Adjusted R^2	0.042	0.332	0.385	0.388	0.389	0.390	
Pseudo R^2							0.286

Note. The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. $HometownTie$ is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. The marginal effect in column (7) is calculated as a linear combination of the marginal effects for each outcome value. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Event Studies The identification in my baseline specification relies on the assumption that buildings constructed under the authority of connected and unconnected officials should be otherwise identical in terms of earthquake damage in the absence of such connections. Because a direct test of this counterfactual assumption is not feasible, I employ some diagnostic approaches that allow me to examine the extent to which the assumption holds. One strategy is to look at the effects of entering and exiting a connected regime in an event-study framework. Specifically, I investigate the year-by-year differences in earthquake damage for buildings constructed right before and right after the county officials gain and lose their hometown ties using the following flexible specifications:

$$Damage_{ict} = \sum_{j=-3}^{3} \beta_j GainTie_{cjt} + \delta_c + \sigma_t + \mathbf{X}'_{\mathbf{ict}}\mathbf{\Gamma} + \varepsilon_{ict} \tag{2}$$

Figure 2: Predictive margins of hometown ties for each damage category



Note. The figure depicts the predictive margins of hometown ties derived from the ordinal-probit estimation in Column (6), Table 1. Each bar represents the predicted probability for each of the damage scales a building would have experienced with and without a connected official when constructed. The regression considers account county fixed effects, year fixed effects, building type by year fixed effects, building features, and geographic controls. Standard errors are clustered by county.

$$Damage_{ict} = \sum_{j=-3}^{3} \beta_j LoseTie_{cjt} + \delta_c + \sigma_t + \mathbf{X}'_{\mathbf{ict}}\mathbf{\Gamma} + \varepsilon_{ict} \quad (3)$$

where $GainTie_{cjt}$ ($LoseTie_{cjt}$) is a set of dummies that indicates the normalized year j relative to the moment that county c enters (exits) a connected regime. Buildings constructed beyond 3 years from a connected regime are included in the comparison group. If the identification assumption holds, we should expect a consistently positive effect for buildings constructed within a connected regime and no differences before the county gains or after it loses its connection.

I estimate these flexible equations with the full set of controls and present the results in Figure 3. Panel (a) shows the results when I examine the effect of gaining political connections using Equation (2) and provides a plot of the estimated coefficients along with their 95% confidence intervals. The horizontal axis is normalized to the year in which the county enters a connected regime. The comparison is relative to buildings constructed more than 3 years before the establishment of the political connections. The figure shows that buildings constructed ahead of a connected regime exhibit no tendency toward vulnerability, a pattern consistent with a generalized common trends assumption. I also observe a notable increase in earthquake damage if a building is constructed after the connection has been established.

Turning to the effect of losing a connection, I plotted, in Panel (b), the coefficients and confidence intervals estimated from Equation (3), in which the relative year is centered around the county's exiting a connected regime. Buildings constructed more than 3 years after the connection ends are included in the comparison group. A symmetric pattern emerges from the estimates that buildings tend to have greater strength — despite some apparent noise, which I will explain in Section 5.2 — if constructed after the county lost its connection.

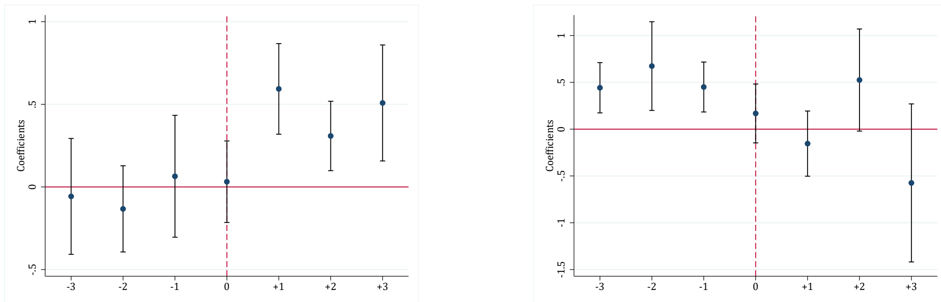
Taken together, the event studies show no anticipatory or carryover effects of hometown ties, which provides evidence that the counterfactual assumption is likely to hold. Although the allocation of connected officials might not necessarily be random, the null effects before gaining and losing connections suggest that, conditional on what I have controlled for, the unobservables associated with this allocation do not seem to directly affect building damage in the absence of such connections. This finding thus allows a causal interpretation of the main results.

In sum, the results I have presented in this section provide robust evidence that buildings constructed under the authority of politically connected officials tended to be more severely damaged in the 2008 earthquake. I also provide evidence that may be informative about the counterfactual in the context of the absence of connections, which facilitates causal interpretations of the results.

4.3 Identification Concerns

The results I have obtained reveal a clear association between the county officials' hometown ties and the buildings' earthquake damage. To make credible causal claims, however, I have to rule out the alternative mechanisms that might bias my estimates.

Figure 3: The effects of gaining and losing connections on building damages



(a) Gaining connection (b) Losing connection

Note. The figures depict the effects of gaining and losing a connected official on building damages. The markers and capped spikes represent the OLS estimators and 95% confidence intervals. Figure 3(a) normalizes the years of construction to the year when the county gains a connected official (year 0), with buildings constructed more than 3 years earlier as the comparison. Figure 3(b) normalizes the years of construction to the year when the county loses a connected official (year 0), with buildings constructed more than 3 years later as the comparison. The sample contains 788 buildings for which the year of construction was reported with precision. The dependent variables are the level of damages on the 1–5 scale. The regression considers county fixed effects, year fixed effects, building type by year fixed effects, building features, and geographic controls. Standard errors are clustered by county.

Sample selection One major concern of this study, as discussed in Section 3.5, is that the buildings observed in the sample may not be randomly selected. If the selection criteria are consistent within a county, this concern can be addressed directly by the inclusion of county fixed effects, which effectively eliminates between-county differences. If the selection is inconsistent within a county, the identification relies on an additional assumption that the selection does not depend on the connectedness of county officials for the year in which the buildings were constructed. Although this is a generally plausible assumption, there are situations in which it may be violated. If, for example, the connected ex-officials had persistent influence over the compilation of county gazetteers after the earthquake occurred, they may use that influence to interfere with the selection process — although it appears more likely a downward bias if they wanted to hide information.

To get a sense of the extent to which the selection of buildings might depend on the connection, I examine whether the hometown tie is predictive of a building's damage being observed in my sample. Specifically, I take the list of buildings for which I can observe the construction history from the *County Gazetteers*, and regress a building's being selected into the linked sample (i.e., the building's damage being observed in the *Earthquake Relief Reports*) on the hometown tie of the county officials during the building's construction. Although this list of buildings may well be unrepresentative of the population, it is at least suggestive of the nature of the selection process in regard to the role of hometown connections.

Table 2 presents the results for all specifications parallel to those in the baseline. The outcome is a dummy variable that denotes whether the building's damage is observed. Columns (1) – (5) provide the OLS estimates with different sets of controls, and Column (6) presents the estimation of a probit model to exploit the potential nonlinear effects. The estimated coefficients are negative and statistically insignificant across all specifications. They suggest that, despite the lack of randomization in the sampling process, it is unlikely that there is selection based on the hometown ties of ex-officials that could otherwise contaminate my main results.

Endogenous treatment A second concern is that the presence of connected officials in a county may not be exogenous. If, for example, officials with patron-client connections were appointed disproportionally to counties with certain characteristics, the estimates might be biased if any of those characteristics affected building damage directly in the absence of such connections. In the previous sections, I have addressed this issue partially in two ways. First, the baseline estimates control for county fixed effects, which rules out biases that come from endogenous allocation of connected officials according to time-invariant county characteristics (for example, connected officials appointed to more mountainous or historically more corrupted areas). Second, the lack of differential trends in the event studies suggests that factors that could have affected the allocation of connected officials do not seem to have an impact on building damage directly in the absence of such connections. Thus, although connected officials were not assigned to counties at random, it does not seem likely that the endogenous allocation could bias my estimates.

Here, I present two additional pieces of evidence that further alleviate this concern. In the first exercise, I include additional socioeconomic variables — per capita GDP and population — into the specification. The variables are chosen because they may guide the allocation of connected officials while constraining local resources to keep buildings compliant.[27] The results are presented in Table C4. First, I find that, higher

[27]Note that these variables might be the consequence of having a connected official, for which

Table 2: Patronage ties and selection of buildings

	Dependent Variable: $\mathbb{1}\{DamagesObserved\}$					
	OLS					Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	-0.0375	-0.0210	-0.0185	-0.0184	-0.0090	-0.0763
	(0.0278)	(0.0245)	(0.0242)	(0.0250)	(0.0218)	(0.0988)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	0.175	0.175	0.175	0.176	0.176	0.211
# Counties	63	63	63	62	62	36
# Observations	6128	6127	6127	6096	6096	4799
Adjusted R^2	0.222	0.314	0.319	0.321	0.325	
Pseudo R^2						0.298

Note. The sample includes all buildings for which the years of contruction are observed. The dependent variable is an indicator variable denoting that the building's damage scale is observed. $HometownTie$ is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Column (6) drops observations of which the outcome variable can be perfectly predicted by the set of fixed effects. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

per capita GDP, as expected, significantly mitigates building damage; the increase in population, in contrast, contributes to the vulnerability of the buildings. More importantly, the effect of $HometownTie$ on a building's damage is, across various specifications, robust to the inclusion of socioeconomic controls. The coefficients across all columns appear approximately 25% smaller than those in the baseline after partialing out the socioeconomic constraints, yet they remain significant at the 5% level or above. These results suggest that, although some of the effects of having a connected county official on building damage may come from the lack of financial resources during the construction, this mechanism alone cannot explain most of my

reason I did not include them in the main specification, as I wished to avoid bad control problems.

findings. The findings also reinstate the argument that the consequences of patronage ties extend far beyond the immediately visible economic outcomes.

In addition, I perform a placebo test, in which I consider officials with patron-client connections whose patrons were in a different but neighboring prefecture. If officials with and without patron-client connections are systematically different in attributes relevant to building quality (including but not limited to the type of places to which they were assigned), we would expect a similar effect for other officials with such connections though not to their supervisors. The results, obtained for all specifications parallel to the baseline, are presented in Table C5. I observe, across all columns, close-to-zero effects of this non-supervisor connection. This test suggests that having a patron, per se, does not imply poorer building quality unless the connection is associated with a direct supervisor. Thus, my findings are unlikely to be driven by the differential allocation of officials with patron-client connections. The exercise also helps to alleviate the more general concerns about systematic differences between officials with and without a patron at the prefectural level.

Preference for buildings A third source of bias is that connected and unconnected officials may construct buildings (or, more precisely, manage construction projects) differently. For example, they may have different preferences for where to construct a building and what specifications to use for it. Such choice of site and specifications, even in the absence of malicious intent or misconduct, may be correlated with the seismic intensity that a building experienced or its resistance. In my baseline estimation, I controlled for the building characteristics, including their locations and specifications, for all linked buildings in the sample. To further investigate the extent to which connected and unconnected officials might have constructed buildings differently, I examine whether hometown connections affected the choice of building sites and specifications using the larger sample that contains all building constructions that I collected from *County Gazetteers* (regardless of whether their damage was reported).[28]

I present the results in Table C3. The first two columns provide the results in regard to whether buildings constructed by connected officials tend to be located in seismically more dangerous regions. The outcome variables of interest are PGA

[28]The results are robust to restricting the sample to the one used in my baseline analysis, for which both the construction and the damage are observed.

and terrain ruggedness, both measured at the site of the buildings. The following four columns contain the results related to the effects of hometown connections on the structure/specifications of buildings. I consider four pieces of information that are occasionally documented in the gazetteers: height, size, number of rooms, and number of phases. For all specifications, I control for county fixed effects, year fixed effects, building type × year fixed effects, and the set of individual-level controls of those county officials. I do not find hometown connections to be associated with any of these decisions in regard to where and how to construct the buildings.

Other Robustness Checks I consider several additional robustness checks to address the remaining identification concerns. First, to capture the potential geographic differences not captured by the baseline controls, I include, in Table C6, a set of fixed effects for each 1×1 arcminute (approximately 1.6 kilometers) grid cell. Thus, I compare buildings not only within the same county but also located very close to one another. Second, the analysis presented in Table C7 controls for the set of hometown fixed effects so that the comparison is of officials with the same homes of origin; this specification rules out the possibility that officials who come from the same hometowns may have similar attributes associated with the specific place, regardless of whether they are connected. Third, Table C8 includes the set of prefecture × year dummies to capture any confounding factors at the prefectural level, which rules out, for example, weak prefectural institutions that allow prefectural officials to appoint more of his or her clients and cause construction-related corruption activities prefecture-wide. Finally, in view of the possible negative weighting problems in two-way fixed effects models (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021), Table C9 presents the results of an alternative two-stage difference-in-differences estimation procedure proposed by Gardner (2021). My findings are robust to all of these alternative specifications and methods.

Taken together, the results that I have presented in this section show a robust causal effect of patronage ties at the time of building construction on the damage of buildings during the 2008 Sichuan earthquake. The effect is significant both statistically and economically. There is no evidence that the findings suffer from sample selection, manipulation, or other bias associated with the most prominent omitted variables at the building, county, and individual levels.

5 Interpretation

The results I have presented provide clear evidence that the poor resistance of buildings may be attributable to patronage leadership in local government administrations. In this section, I delve deeper into why this effect occurred. Specifically, I investigate two questions: (i) Does this effect reflect institutional weaknesses or other unintended consequences that result from patronage connections? and (ii) Does patronage weaken the local institution by selecting poorer officials or by distorting their incentives? Both questions are difficult to answer, as we do not observe directly either the behaviors or the incentives of those officials in the building construction process. Yet, developing at least a conceptual understanding of these questions is critical to an accurate interpretation of the findings.

5.1 Institutional Weaknesses or Unintended Consequences?

One central question for the interpretation is whether the poorer earthquake resistance results from institutional weaknesses or merely nonintentional consequences associated with connected officials. According to Brinks et al. (2019), institutional weakness refers to any failure of an institution to reach its desired outcomes, which can be caused by poor design, non-compliance to established rules, or lack of consistency. The anecdotal evidence associated with shoddy buildings suggests that poorer building code enforcement by the government, which is a specific type of institutional weakness, might be a plausible (if not definitive) explanation. Because I do not have information about the downstream contractors (or their interactions with local officials) in my data, however, there might be alternative scenarios in which the consequence is unintended. Below, I present four pieces of evidence that help to rule out several prominent explanations and collectively support the mechanism of weak institutional enforcement of established building codes.

Violation of Building Code I start by documenting the extent to which the excess damage reflects a violation of the relevant building code in the construction process. The answer is not obvious from the baseline estimation, which, in its essence, represents the relative difference between connected and unconnected buildings in their seismic resistance. One possible scenario is that the connected buildings were by no means defective, and they appeared shoddy only because the unconnected buildings to which they were compared had exceptionally high resistance. Given the strong magnitude of the earthquake, it could be that only buildings of exceptional resistance survived.[29] The excellent resistance of those surviving buildings may not even be efficient if the (ex ante) seismic hazard was low (given that an earthquake is essentially a tail event).

To better understand the nature of the difference, I leverage additional information on the requirement for seismic resistance specified in the building codes, which serves as a reasonable benchmark for what is legally or ethically "acceptable."[30] As noted in Section 2, the building codes specify the range of ground motion under which a building should *not* collapse.[31] I compare this required resistance to the actual (perceived) ground motion that a building experienced during the earthquake at its locality and partition my sample into three groups: buildings for which the perceived motion is weaker than, equivalent to, or stronger than the resistance requirements (hereafter, "mildly," "moderately," and "severely" hit buildings). A building compliant with the building codes should not collapse when hit by seismic waves that are weaker than or equivalent to the level of resistance that it is required to have, and its collapse is perhaps understandable if the seismic waves are stronger than the required level of resistance. In contrast, the collapse of a mildly or moderately hit building is likely a signal of code noncompliance.

I estimate the effects of having a connected official for each of the three groups by multiplying the $HometownTie$ indicator with the set of dummies that denote whether a building was mildly, moderately, or severely hit by the earthquake (in terms relative to the level of resistance it was required to have).[32] The dependent

[29] As noted earlier, the government officially attributed the collapse of buildings and the high mortality rate to the "unusually severe extent" of the earthquake (Caixin, 2009).

[30] Note that being legally or ethically acceptable is neither sufficient nor necessary for being economically efficient, as the building codes might not be efficiently specified (e.g., too strict requirements when the hazards are low). Although the efficient level of resistance remains an open question, it is unlikely that the required resistance was higher than that, given the high earthquake hazards in the area (see Section 2). In fact, the government revised the building codes to increase the required level of resistance significantly after the 2008 earthquake (National Codes of P.R.C., 2015), which indicates that the previous requirement might have been too low.

[31] This resistance requirement is location specific, and was modified in 1990 and 2001, as was documented in National Codes of P.R.C. (2001) and China Earthquake Administration (1990, 1977). I extract the location- and period-specific requirement for each building from `http://www.gb18306.net/`.

[32] My findings are robust to using absolute measures of earthquake intensity without referring to the required resistance.

variable is an indicator of building collapse (either partial or full).[33] I run a probit regression and plot in Figure 4 the predictive margins of the probability of building collapse. First, by focusing on the unconnected buildings (marked by triangles), I find that the probability of collapse barely changes when the perceived ground motion is weaker or equivalent to the required resistance and increases substantially when the ground motion exceeds the required resistance levels, a pattern that makes perfect sense for code-compliant buildings. In regard to the connected buildings (marked by circles), I find, in contrast, a substantial increase in the probability of collapse when the perceived seismic intensity is just within the range of required resistance. Further, these buildings are at least as likely to collapse as they are when hit by stronger, beyond-resistance seismic waves. In addition, although connected buildings appear more likely to collapse than do their unconnected counterparts overall, a pattern consistent with the baseline, the gap is particularly stark for a ground motion equivalent to the required resistance. There is, however, no statistical difference between the two groups for stronger motions. Thus, my findings mainly reflect connected buildings' failure to achieve a certain standard rather than unconnected buildings' ability to exceed it.

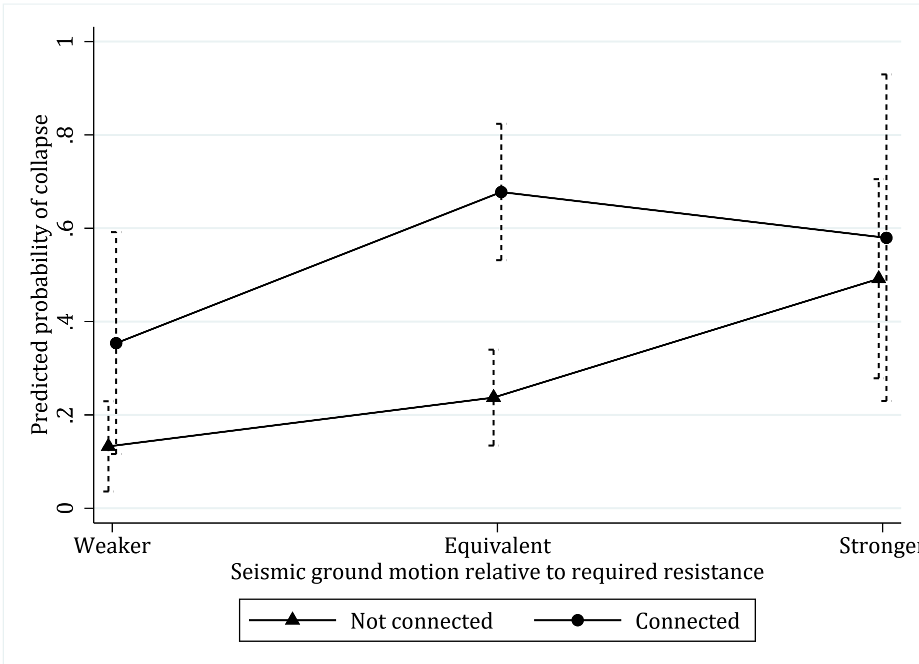
Taken together, the observed patterns suggest that corner cutting and code noncompliance cause the vulnerability of connected buildings.

Responsibility of Governmental Enforcement Next, I examine the extent to which enforcement by the local governments was responsible for the lack of building code compliance. An alternative scenario in regard to agenda setting may exist. That is, connected and unconnected officials may have different policy agendas and give different priorities to building safety. For example, connected officials may prioritize quantity over quality or devote more resources to other issues that the county needs to address (e.g., growth, pollution). As a result, the difference in building damage might reflect the different policy objectives that connected and unconnected officials choose to prioritize rather than the failure to meet their objectives.

To investigate this possibility, I explore the differential effects of hometown connections for party secretaries and for governors. Although both are the top officials in a county, they differ substantially in their ranges of responsibilities. The party sec-

[33] I use the building collapse indicator as the dependent variable for this exercise to facilitate the interpretation in terms of code violation. The estimates are consistent with the ones that use the same 5-point damage scale as I used in the baseline.

Figure 4: Resistance requirements, seismic intensities and earthquake damage



Note. The figure depicts the predictive margins of patronage ties, by seismic groups, derived from a probit estimation in which the outcome is an indicator of building collapse. The sample contains 1065 buildings in the linked sample. The scatters and connected lines represent the predicted probability of collapse for buildings suffering from a ground motion weaker than, equivalent to, and stronger than the seismic resistance requirements, respectively. The regression considers county fixed effects, year fixed effects, building type by year fixed effects, building features, and geographic controls. Standard errors are clustered by county.

retary, who retains the formal political authority in the county (and is more powerful politically), sets the general policy line and oversees the work of the government; the governor, being the head of the government agency, is responsible for making and implementing specific policies and administering social programs (Shirk, 1993). Consequently, party secretaries are more responsible for any consequences associated with agenda setting, whereas governors are in a position that is more susceptible to direct embezzlement or favor exchange in the implementation process. Motivated by this institutional structure, I attempt to distinguish between the hometown ties of the party secretary and that of the governor and separately estimate their impacts on building damage. The results, summarized in Table 3, reveal that the overly-damaged buildings were constructed mainly in the administrations of connected governors; the connected party secretaries, in contrast, exhibit much smaller effects, which, although still positive, are nonsignificant at conventional levels. Therefore, the evidence suggests that a differential in policy priorities is *not* a significant factor; if it were, we would expect most effects to come from connected party secretaries. The evidence also provides additional support for the weak enforcement interpretation by showing that the effects come mainly from officials who are in direct charge of project administration (i.e., governors) who have more opportunities for rent-seeking.

Table 3: Patronage ties and building damages by position

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie(secretary)	0.1672	0.1674	0.1483	0.1310	0.0734	0.1847
	(0.1267)	(0.1399)	(0.1481)	(0.1456)	(0.1225)	(0.2285)
HometownTie(governor)	0.2735**	0.2794**	0.2820**	0.2914**	0.3308**	0.6809***
	(0.1066)	(0.1172)	(0.1280)	(0.1169)	(0.1302)	(0.2306)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.839	2.843	2.843	2.843	2.843	2.843
# Counties	35	35	35	35	35	35
# Observations	981	969	969	969	969	969
Adjusted R^2	0.332	0.382	0.386	0.386	0.388	
Pseudo R^2						0.290

Notes: The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. $HometownTie(secretary)$ is an indicator denoting that the county has a connected party secretary via hometown ties when the building was constructed. $HometownTie(governor)$ is an indicator denoting that the county has a connected governor via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Feasibility of Effective Implementation Although the preceding pieces of evidence indicate that inadequate enforcement by the local government may be responsible for code noncompliance, it remains unclear whether this reflects the selfish motives of local leaders or a general lack of information/knowledge to implement quality/safety control. An alternative scenario could be that connected and unconnected officials have different information or skill sets that relate to (managing) construction projects, in which case effective enforcement may not be feasible even when the connected officials have goodwill. For example, connected officials may be ignorant of the seismic resistance measures to which they should pay attention, or they may lack the ability/experience to enforce them effectively.[34] Another possibility is that connected and unconnected officials may have different perceptions of earthquake hazards and/or the optimal level of resistance. In either case, the outcome should not hinge on the officials' private stakes associated with building safety.

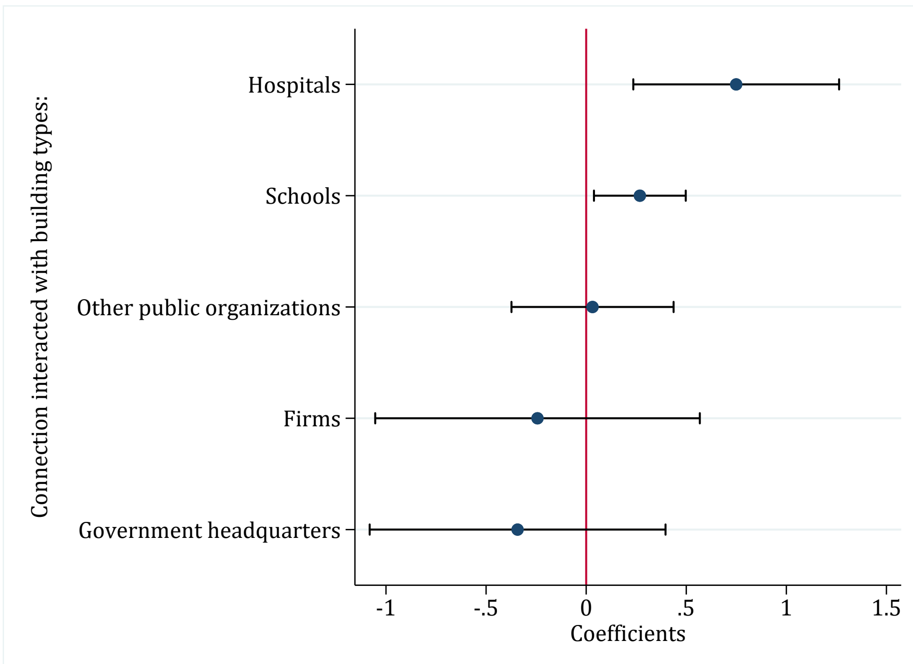
I evaluate this explanation by exploring the heterogeneity in treatment effects across building types, for which the officials' private stakes may vary. For example, the safety of government headquarters would be most directly associated with the officials' own welfare, as they work (and in many cases, live) there. Conversely, buildings accessed mainly by the public (e.g., schools, hospitals, libraries) may be less relevant and receive a smaller weight in the officials' private utility function. If my findings mainly reflect the gap in knowledge or information that affects feasibility, we should expect the effects to be homogeneous across all types of buildings. If, in contrast, the effects are pertinent to the officials' personal stakes in the buildings, the evidence would undermine the plausibility of a feasibility scenario and suggest that selfish motives might be at play.

Figure 5 presents the results visually; I obtain these by multiplying the $HometownTie$ indicator with the set of dummies, each denoting a specific type of building in my sample: hospitals, schools, public organizations, state-owned factories, and government headquarters. For the figure, I plot the estimated coefficients for each building type, along with their 95% confidence intervals. As seen in the figure, schools and hospitals appear to be particularly vulnerable to the authority of politically connected officials, whereas other types of buildings are less susceptible. More importantly, the effect of $HometownTie$ is even negative, despite its large standard error, for state-owned factories and government headquarters. I conduct

[34]Such difference in knowledge may arise from the potentially distinct career paths between connected and unconnected officials. For example, one possible scenario could be that connected officials have an advantage in political tactics whereas unconnected officials have more expertise knowledge in specific fields (although I do not have enough information in my data to test this hypothesis).

a post-estimation test that confirms statistically significant differences between hospitals and other types of buildings. Overall, the pattern suggests that connected officials seem able to implement the building code as effectively as their unconnected counterparts when they internalize the cost and benefit of building safety into their decision making. Therefore, the difference in earthquake damage does not reflect merely a potential knowledge/information gap between connected and unconnected officials.

Figure 5: Hometown ties and building damages by building types



Note. The figure depicts the effect of political connection on building damages across different types of buildings. The markers with capped spikes represent the OLS estimators and 95% confidence intervals of the interaction terms between political connection and each of the building types. The sample contains 1065 buildings in the linked sample. The dependent variable is the level of damages on the 1–5 scale. The regression considers county fixed effects, year fixed effects, building type by year fixed effects, building features, and geographic controls. Standard errors are clustered by county.

Substitution with Private Supervision The evidence I presented above rules out several alternative explanations of the empirical findings and consistently points toward the interpretation that connected officials failed to assume their duty in administering public projects and enforcing the building code, leading to a pattern of corner cutting and laxity with respect to quality/safety control in the construction process. One implication is that the difference between connected and unconnected buildings should be reduced if there are other parties involved in the construction that might be able to fulfill these duties. I test this hypothesis by looking at buildings that were privately financed, through fundraising, donations, and individual or corporate investment. These financial backers often have direct interest in ensuring the quality and safety of the buildings.[35] I multiply $HometownTie$ and an indicator that equals 1 if private funds have at least partially financed the project (the information is occasionally revealed in the county gazetteers) to estimate the heterogeneous treatment effects by funding source.

The results are reported in Table 4, with all specifications parallel to those in the baseline. A few patterns emerge from the table. First, the coefficients on $HometownTie$ across all specifications are larger than those in the baseline, and all are significant at the 1% level once the funding source has been accounted for. This set of coefficients estimates the average effect of $HometownTie$ on earthquake damage for buildings not associated with any form of private resources. Second, the coefficients on the interaction term, $HometownTie \times PrivateFund$, are negative and significant at least at the 10% level in the most saturated specifications. Moreover, the magnitude of the coefficients on the interaction term is as large as, if not larger than, those on $HometownTie$, suggesting that the involvement of private capital serves to mitigate or even offset the adverse effect of having a connected official. Thus, the evidence shows that other stakeholders may effectively fulfill the roles that an otherwise responsible administrator should have fulfilled.

One alternative interpretation of this mitigation effect is that having private funding might imply having more funding to spend, which would naturally increase the quality and resistance of buildings. If this explanation holds, we would expect an improvement in seismic resistance for unconnected buildings as well. The coefficient on $PrivateFund$, which captures the role of private funds for buildings constructed outside a connected regime, however, is close to zero and statistically nonsignificant. Thus, the involvement of private funds does not appear to improve building safety in general. Rather, there appears a substitution between institutional and private en-

[35] For example, they may name the buildings after their brands or give the buildings their own names, so the building quality matters for their reputation. To do so, they often participate in managing and overseeing the project to prevent corner cutting activities. See Branigan (2008) for a notable example.

forcement of the building code, and private monitoring serves only to fill the gap and prevent the detrimental effects that connected county officials would have otherwise caused.

Table 4: Patronage ties and building damages by funding source

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	0.3386***	0.3653***	0.3571***	0.3530***	0.3522***	0.7449***
	(0.1112)	(0.0877)	(0.0908)	(0.0935)	(0.0867)	(0.1457)
PrivateFund	0.0150	-0.0239	-0.0349	-0.0359	-0.0411	-0.0944
	(0.1298)	(0.1300)	(0.1302)	(0.1261)	(0.1319)	(0.2576)
HometownTie × PrivateFund	-0.2398	-0.4135**	-0.3922**	-0.3948**	-0.4073*	-0.8069*
	(0.1734)	(0.1860)	(0.1827)	(0.1916)	(0.2112)	(0.4305)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.857	2.861	2.861	2.861	2.861	2.861
# Counties	35	35	35	35	35	35
# Observations	1062	1050	1050	1050	1050	1050
Adjusted R^2	0.332	0.389	0.392	0.392	0.394	
Pseudo R^2						0.290

Notes: The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. *PrivateFund* is an indicator denoting that private capital has participated in the building's construction. Individual Controls include an indicator for female, an indicator for minority, average age, average education and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

In sum, in this section, I have presented a chain of evidence that collectively supports the institutional weakness interpretation and, more specifically, the poor regulation and law enforcement by local government administrations under patronage leadership: (a) the collapse of connected buildings reflects corner cutting and code noncompliance during the construction, (b) officials in direct charge of public administration and law enforcement play a major role, (c) effective implementation should have been achievable if officials had internalized the costs of low-resistance buildings, and (d) the involvement of private stakeholders can substitute for the missing role of institutional enforcement. In doing so, I also have ruled out several prominent alternative explanations. Although I do not observe directly the activities of these county leaders in the construction process and their interactions with downstream contractors, it appears that shirking responsibility or corruption is at least a plausible, if not definite, explanation for the weaker institutional environments that they create.

5.2 Selection or Incentive?

The previous section presents evidence that patronage leadership might have created poorer institutional environments in administrations led by connected officials (possibly as a result of shirking or corruption). As a result, the buildings administered by connected officials were not code compliant, had insufficient seismic resistance, and collapsed more easily during the earthquake.

In this section, I go one step further to investigate the channels through which patronage leadership might have played this role. There are two possible channels, each associated with a different type of benefit that the patron may offer to the client. The first channel is political selection (in which case the patron offers jobs): Connected individuals may have lesser administrative ability or be less ethical than unconnected ones if having connections gives them an additional advantage over other, perhaps more qualified candidates. Such negative selection has been extensively documented in the patronage literature (e.g., Xu, 2018). The second channel is moral hazard (in which case the patron offers shelter or protection): Connected officials may have fewer incentives to exert effort or maintain integrity once they know that they will not be held accountable under the authority of their patrons.

To investigate whether one or both of these channels play a role in explaining my findings, I leverage additional information on whether the county officials had patronage ties with their prefectural superiors when they were first appointed to the position. This information provides a possible indicator of those who have benefited from the selection channel. A Comparison of buildings constructed in the administration of "connected appointments" (officials connected when appointed) versus "unconnected appointments" (officials unconnected when appointed) would show the effect that comes from political selection. Correspondingly, a comparison the effects of "connected construction" (connected at the time of building construction) versus "unconnected construction" (unconnected at the time of building construction) holding the selection channel constant, would suggest the role of incentive changes induced by patronage ties.

Following this idea, I divide the buildings in my sample into four groups by the connectedness status of the governing officials at the time of appointment and at the time of building construction:

1. Connected at the time of appointment and at the time of construction
2. Connected at the time of appointment but not at the time of construction
3. Not connected at the time of appointment but connected at the time of construction
4. Not connected at the time of appointment nor at the time of construction

I estimate the differences in building damage across the four categories, for which Group 4 (buildings with "unconnected appointment and unconnected construction") is taken as the reference group for comparison. The estimation controls for all the covariates that have been included in the baseline estimation.

I plot the estimated coefficients, along with their 95% confidence intervals, in Figure C3. First, focusing on the comparison within the "unconnected appointment" group (the first two columns), I find a large and significant increase in building damage if the previously unconnected county officials became connected (as a result of personnel changes at the prefecture level) at the time of building construction. Because these officials did not have connections when they were appointed to office (hence, no selection), this coefficient estimates the effect that comes solely from changes in their incentives. I do not observe a corresponding effect among buildings constructed by "connected appointment" officials: The two estimated coefficients are similar and cannot be statistically distinguished from each other. Thus, officials do not seem to become more prudent after their patrons leave office.

To probe the effect of political selection, I compare the difference in building damage associated with the governing officials' connectedness status at the time of appointment while holding constant connections at the time of building construction. The difference between Columns (1) and (3) provides an estimate of the effect of a connected appointment in the absence of a connected construction (hence no incentive). The coefficient is positive, significant at the 10% level, which shows that negative political selection also may be at play. For buildings constructed in a connected year (connected construction), the coefficient on connected appointment is smaller than the one on unconnected appointment (although the difference is not significant at conventional levels), which may be indicative of the relative importance of the two channels.

Taken together, the estimates in Figure C3 suggests that both the selection and the incentive channels may be at play. This finding further indicates the range of benefits that a senior official may offer to a junior official through the patron-client relation. Consistent with the theoretical account in the literature, the patrons seem to be offering positions to connected individuals who have lesser administrative ability or are less ethical and protecting them from being accountable for illicit activities.

5.3 Additional Discussions

In online Appendix, I discuss additional issues related to the interpretation of the results. First, to address the concern that the sample is non-random and may not be representative of all buildings in the quake-affected area, I supplement my building-level findings with an analysis of county-level aggregates that allows me to examine more systematic and economically relevant outcomes. Using a dataset that covers all 181 counties in Sichuan Province, I first document a positive cross-county correlation between earthquake losses and the period of the connectedness of county officials: One additional year of having a connected official is associated with an 8 % increase in mortality and a 3 % increase in direct economic loss from the earthquake. This pattern is observed across all sectors except for government agencies. These findings, though not necessarily causal, show that the conclusions drawn from my building-level analysis are likely relevant for aggregate outcomes. I present the method, data, and empirical results in Appendix A.1.

Second, because an earthquake is by and large a tail event, I investigate the extent to which patronage connections could have been welfare-improving in the absence of an earthquake. Using a balanced county-year panel between 1978 and 2007, I do *not* find evidence of a quantity-quality tradeoff in which connected officials construct more buildings at the cost of their quality. Instead, I find that connected officials receive additional resources from the upper governments (but no evidence of better socioeconomic performance) and are more likely to be involved in corruption activities.

Although it is beyond the scope of this study to make any decisive welfare calculation, these findings indicate that the latent costs of patronage connections that I have uncovered are not unique and may represent only the tip of the iceberg. The results are presented and discussed in Appendix A.2

6 Concluding Remarks

In this paper, I have examined how institutional weakness creates societal vulnerability in the context of the 2008 Sichuan earthquake. I have constructed two original datasets, one at the building level and the other at the county level. Using the building-level dataset, I have established a plausibly causal relationship between local institutional conditions shaped by patron-client relationships in the year in which a building was constructed and the extent of damage to that building in the 2008 earthquake. The estimates across a variety of specifications robustly suggest that buildings constructed under the authority of a connected official are 83% more likely to collapse relative to their non-connected counterparts. I have offered some suggestive evidence that the detrimental effects are likely attributable to the lack of building code enforcement due to dereliction of duty or corruption by connected officials. To evaluate the external validity of these findings, I have analyzed a second county-level dataset that allows me to examine more systematic and economically relevant outcomes. The findings show that the cumulative number of years that a county has had a connected official, conditional on geographic and socio-economic conditions, is positively correlated with the aggregate statistics of earthquake damage, such as fatalities and direct economic loss. This result, while not necessarily causal, suggests that the patterns I have observed in my building-level dataset, a possibly selective sample, are likely representative of the role that patron-client connections play in worsening the effects of earthquakes.

The findings in this study offer insight into the performance of institutions and the impacts of natural shocks. In particular, the paper provides causal evidence that institutional failures may create a societal vulnerability that amplifies the damage from adverse shocks. In addition, the study highlights the notion that frail institutions may be obvious only ex-post when they break under stress and expose that frailty. Although I focus specifically on a natural disaster in China as a striking example, the implications of this study are likely applicable to other economic realms and to other countries.

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Online Appendix for The Social Costs of Patronage Ties: Lessons from the 2008 Sichuan Earthquake

Yiming Cao

A Additional Discussion

The building-level analyses provide plausibly causal evidence that the connected county officials may have been associated with violations and abuses in the construction industry that reduced the resistance of buildings to collapse. The internal validity of the causal inference has been established by a difference-in-differences style design that compares buildings constructed under the authority of connected county officials to their unconnected counterparts conditional on various geographic, building, and individual profiles as well as a few additional checks that rule out the most prominent alternative explanations. The external validity and welfare consequence of this causal relation, however, remain unclear. In this section, I offer a tentative discussion of these issues without the intention of drawing any definitive conclusion. Understanding the potential and limitations of this study with respect to external validity and welfare consequences is critical to assessing the broader implications of my findings.

A.1 External Validity: Cross County Evidence

As discussed in Section 3.5, one limitation of my study is that the sample is non-random and may not be representative of all buildings in the quake-affected area. Also unclear are the economic implications of the excess building damage. To address these issues, I supplement my building-level findings with an analysis of county-level aggregates that allows me to examine more systematic and economically relevant outcomes. Although this analysis admits only cross-sectional correlations with no causal implications, it is at least suggestive of the extent to which the causal relation I draw from my building-level analysis can be generalized.

Model Specification My county-level sample contains all 181 counties in Sichuan Province. For each county, I observed the aggregate statistics of fatality and direct economic loss decomposed by sectors. I aggregated a county's exposure to connected officials between 1978 and 2007 to construct two measures: an indicator that denotes whether the county once had a connected official and the cumulative number of years of having connected officials. The estimating equation takes the following form:

$$Y_i = \beta Tie_i + \mathbf{X_i'\Gamma} + \epsilon_i \tag{4}$$

where i indexes counties. Y_i denotes any of the aggregate outcomes that I study: the earthquake fatality and direct economic loss. Tie_i denotes any of the cumulative measures of exposure to connected officials: ever-connected and the cumulative number of years being connected. $\mathbf{X_i'}$ denotes a vector of county-level covariates: average seismic motion, average ruggedness, the logarithms of GDP in 2007, the population in 2007, and the connection status of the county officials in 2008. These controls take into consideration the geographic determinants of earthquake intensities, the socioeconomic conditions at the time of the earthquake, and the potential manipulation of the statistics of damage. The model does not, however, account for the potential factors that could possibly make vulnerable counties more favorable to the connected officials, for example, worse rule of law, which could bias the results. Therefore, I refrain from making any causal claims beyond noting cross-sectional correlations between the exposure to connected officials and the mortality and economic loss in the 2008 earthquake.

Average Effect I start by estimating the average effect of hometown connections on aggregate damage statistics according to Equation 4. The results are presented in Table C10. The results in the first three columns take into consideration the logarithm of fatalities (the total number of people who died or were missing in the earthquake). In Column (1), the results indicate a comparison of the earthquake fatalities between the ever-connected counties versus the never-connected ones, conditional on the geographic, socioeconomic, and during-earthquake connectedness controls. The coefficient on *OnceConnected* is 0.457, significant at the 5% level. This suggests that the total number of dead or missing is approximately 46% higher, on average, in counties with exposure to connected officials relative to that in never-connected counties. The results in Columns (2) and (3) take into consideration the marginal effects of having one additional year of exposure, accounting for the cumulative number of years that a county has had a connected official. The coefficient on $YearsConnected$ in Column (2) is 0.125, significant at the 1% level. This suggests that one additional year of having a connected official is associated with an approximate 12.5% increase in earthquake fatality. This coefficient is reduced by about one-third if I restrict the comparison to counties within the same prefecture, as shown in Column (3), in which the set of prefecture fixed effects is included; nevertheless, the effect remains significant at least at the 10% level.

The next three columns present the results when I examine the effects of cumulative hometown ties on the logarithm of direct economic loss. The results in Column (4) represent a comparison of the ever-connected versus the never-connected counties. The estimated coefficient on $OnceConnected$ suggests, on average, a 33% higher direct loss in economic value in counties that ever had a connected official, an effect significant at the 5% level. The results in Columns (5) and (6) are estimates, with and without the prefecture fixed effects, of the marginal effects of the cumulative number of years being connected. The results show that one additional year of having a connected official is associated with a 3–5% increase in total economic loss, significant at least at the 5% level.

Incremental Effect Next, I identify how hometown connections exacerbated the damage of the earthquake at different intensity levels. Specifically, I include the interaction terms between each of the two connection measures ($OnceConnected$ and $YearsConnected$) and the local ground motion parameter (PGA), which estimates the incremental mortality and economic loss associated with hometown connections as a function of seismic intensity. The results are reported in Table C11, the structure of which parallels that of Table C10. By focusing on the main effects, I first find that the coefficient on $ln(PGA)$, which estimates the linear effect of seismic intensity on damage in the absence of connections, is positive and statistically significant across all specifications. The connection measures, which estimate the influence of patronage ties in places untouched by the earthquake (zero ground motion), do not matter for mortality and economic loss from the earthquake. Both sets of results may serve as verification of the validity of the aggregate damage statistics.

What is even more interesting are the coefficients on the interaction terms

($OnceConnected \times \ln(PGA)$ and $YearsConnected \times \ln(PGA)$), which estimate how patronage ties affect the association between earthquake intensity and its damage. The estimates for mortality are all positive and statistically significant (Columns 1–3), showing that an increase in seismic intensity leads to more additional deaths in once (or more frequently) connected counties than in never (or less frequently) connected ones. The marginal increase is significant both statistically and economically: An increase in earthquake intensity in counties once governed by a connected official leads to a $0.58/1.52 = 38\%$ larger mortality increase (in logs) relative to an increase in earthquake intensity in unconnected counties (Column 1), and having been governed by connected officials for an additional year leads to a $0.09/0.87 = 10\%$ larger mortality increase (in logs). Thus, having been governed by connected officials for 10 years (out of the 30-year period) is equivalent to a doubling of the ground motion parameter ($\ln(PGA)$), which is in turn equivalent to increasing the seismic intensity scale from, for example, VI (the intensity of a magnitude $4.9M_w$ earthquake at the epicenter) to VIII (the intensity of a magnitude $6.1M_w$ earthquake at the epicenter), or to being 100km closer to the epicenter of this earthquake. I do not find the association between earthquake intensity and economic loss to depend on hometown connections.[36]

Sector-specific effects In additional, I explore the effects of cumulative hometown ties on direct economic loss in different sectors. The outcomes that I observe include losses in economic value to infrastructure, education facilities, health facilities, and government agencies, and physical losses in agriculture, manufacturing, and service sector operations. All estimations take into consideration the geographic and socioeconomic controls and the set of prefecture fixed effects. The results are summarized in Table C12. I first observe a consistent positive effect of cumulative hometown ties on direct losses in all sectors. The magnitudes range between 0.5 and 5.0%, depending on the specific sector. Most of the coefficients are significant at least at the 10% level, with the only exception's being that of government agencies, which is, nevertheless, still consistent with the pattern that I observe in my building-level results.

[36]This is unsurprising because mortality rate is more sensitive to the interplay of earthquake intensity and building resistance than is economic loss.

Broader Implications Although I focus on a single earthquake in China, the idea that institutional failure leads to greater damage upon negative shocks may be applicable to much broader settings. There are news reports from across the globe that describe the common phenomenon of suspicious building collapse as a result of substandard construction, which is often an indicator of corruption. The phenomenon is ubiquitous across a wide spectrum of countries regardless of the form of government. For example, rampant corner cutting was discovered after the collapse of Florida's Champlain Towers South on June 24, 2021, a catastrophe that killed a total of 98 people (Putzier et al., 2021). It was reported that the developers of the condominium building were accused of paying off officials to get through the permit system as the site was being built in 1981 (Swaine et al., 2021; Fitz-Gibbon, 2021). Other examples include similar reports from Turkey (Kinzer, 1999), Iran (Pejhan, 2003), Mexico (Lin, 2017) and Italy (Scaglia, 2010).

The institutional determinants of disaster deaths also have been documented in cross-country studies. Kahn (2005) shows that the annual deaths from natural disasters for 73 nations from 1980 to 2002 is negatively correlated with regulatory quality, voice and accountability, rule of law, and control of corruption. Ambraseys and Bilham (2011) show that the death toll from earthquakes is positively correlated with the corruption perception index of a country. Both exercises exploit country-level variations, suggesting that the association between institutional failure and disaster deaths may be a systematic phenomenon beyond occasional anecdotal accounts.

More broadly, the notion of institutional failure and social vulnerability may apply to other realms of the economy beyond natural disasters. One such example is shadow banking in the financial system. In China, at least four top-level regulators and nine senior banking executives have been under investigation for providing illicit financial services (Wu and Cheng, 2018). In the United States, collusion between banks and regulators has been blamed for the oversight failures that amplified the financial crisis (Kaufmann, 2009). Another example is public health: Several studies have provided tentative evidence that links corruption to adverse health outcomes (e.g., Delavallade, 2006; Azfar and Gurgur, 2008; Glatman-Freedman et al., 2010). During the ongoing COVID-19 pandemic, corruption and institutional failure were identified as important factors in inadequate government response and vaccination rollout delays (Noon, 2021).

Overall, the county-level results confirm the presence of a correlation between the authority of politically connected officials and the human and economic loss attributable to the earthquake. Although the association is not necessarily causal, it suggests that the patterns that I observe at the building level are likely representative of the general role that patronage ties may play in worsening the outcomes of the earthquake. This idea may be applicable to broader settings outside China and beyond natural disasters.

A.2 Welfare Implications

The analyses that I have presented demonstrate the detrimental social consequences of patronage ties. This study, however, focuses only on the cost side of these ties, and there may be other net benefits that come from these ties that I do not capture. In particular, because an earthquake is by and large a tail event, the expected welfare gain from patronage networks may outweigh the social costs that I have uncovered if the earthquake had not occurred. That is, ex ante, these ties may have improved overall social welfare by a tradeoff of the expected gains in normal times against a small probability of entering a bad state with significant loss. Although it is beyond the scope of my data and analysis to make any decisive welfare calculations, I offer a tentative discussion of two most notable sources of potential benefits in the absence of an earthquake. First, with a focus specifically on building construction, I evaluate the extent to which there may be a quantity-quality tradeoff of buildings. I then examine how these connections may be associated with broader political and socioeconomic outcomes.

Quality-Quantity Tradeoff One prominent framework that accommodates the possibility of a welfare gain is the quality-quantity tradeoff of buildings. Specifically, connected officials may have more resources and discretionary power to generate a higher volume of construction activity at the cost of quality. The increase in the number of public buildings, especially schools and hospitals, may greatly improve human capital (in terms of education and health) and provide significant benefits for growth and social welfare (Duflo, 2001). The costs of lowering the quality of these buildings may not have an immediate consequence or even no consequence at all in the absence of a destructive earthquake.

To evaluate the extent to which the findings may be embedded in a framework that features the quality-quantity tradeoff of buildings, I construct a balanced county-year panel that consists of 65 counties in the quake-affected area between 1978 and 2007. I examine whether a county tends to construct more buildings in a year in which its top officials are connected via patronage ties, and present the results in Table C13. The dependent variables are the number of buildings constructed in a county and year according to the records in county gazetteers.[37] The results in the first three columns provide an estimate of the effect of hometown connections on the construction of any type of building in the sample, with and without county fixed effects, year fixed effects, and individual-level controls. The results in Columns (4)–(8) concern the construction for each of the building types, separately. Across all columns, I do not find evidence that connected officials sacrifice building quality to be able to construct additional buildings.

Socioeconomic Outcomes The previous analysis suggests that the lower building quality associated with connected officials does not seem to have been compensated for by more buildings being constructed, which may indicate a partial welfare loss from substandard building construction. This finding, however, does not necessarily imply an overall welfare loss, as patron-client connections may serve to improve welfare in many other domains, and an overall welfare gain does not seem implausible. I leave an accurate evaluation of welfare to future studies; here, I confine myself to a brief exploration of how patronage ties in my sample may be associated with various political and socioeconomic outcomes beyond building construction. If these connections are associated with a broader set of benefits, it may still be worth acknowledging that patronage ties have some socioeconomic value.

I start by examining whether patronage ties have played a role in promoting local development by estimating their effects on GDP and population growth. The results are reported in the first two columns of Table C14, controlling for county fixed effects, year fixed effects, and individual profiles of county officials (age, gender, education, and term). The estimates are positive but statistically nonsignificant, showing no

[37] One caveat of this measure is that it essentially captures the construction of buildings documented in county gazetteers, which may be different from the actual number of buildings constructed. Although I do not have other sources by which to evaluate the selection issue here, I point out that to alter the interpretation of this exercise would require a scenario in which connected officials systematically under-report the buildings that they have constructed.

evidence of better economic development associated with these ties. I then examine how patron-client connections affect the public finance of a county by looking at the growth rate in transfer payments that the county receives from prefectural and provincial government sources (Column 3 of Table C14). I find a significant increase in transfer payments to counties where officials have hometown connections, which is consistent with the impact of patronage on resource allocation that Jiang and Zhang (2020) document. The next column (Column 4) shows how connected officials distribute these additional funds and finds a significant increase in administrative expenditure, which is often an indicator of higher-level corruption (Cai et al., 2011). I confirm this association by showing that connected officials in my sample also are more likely to be investigated for corruption as part of the recent anti-corruption campaign (Column 5).

The finding that connected officials also are more likely to be investigated for corruption poses the intriguing question of whether they have been held accountable for the substandard construction and building collapse. To investigate this question, I examine the interplay between having connections and building collapse on the probability of being investigated for corruption. Specifically, I regress whether an official has been investigated for corruption on four dummies that indicate whether he or she has been connected at least once and whether any building constructed in his or her administration has collapsed. I plot the estimated coefficients in Figure C4, in which connected officials without building collapse are considered the reference group. The first two bars show that connected officials are slightly less likely to be investigated if none of the buildings that were constructed in their administration has collapsed. In contrast, the last two bars suggest that among officials associated with at least one building collapse, connected ones are much more likely to be investigated for corruption afterwards. Building collapse does not seem to affect the probability of being investigated for corruption for unconnected officials (a comparison of the first and third bars). Taken together, these patterns can be reconciled with the explanation that connected officials are in general more corrupt than are unconnected officials, yet their illicit behavior has been largely sheltered by their patrons until brought to light by the earthquake.

To summarize, in this section, I explore the potential welfare implications of patronage ties by examining a broader set of political and socioeconomic outcomes beyond earthquake damage. I do not find evidence that could associate these connections with beneficial consequences, such as increased levels of construction or enhanced economic development. Rather, connected officials seem to have received additional resources from the higher levels of government, and are more likely to be involved in corrupt activity, which is often sheltered by their patrons under normal circumstances. I reiterate, however, that the evidence I have presented does not permit a thorough evaluation of the overall costs and benefits associated with patronage ties. In particular, my finding does not imply a higher social welfare in the absence of such patron-client networks in China's local politics (as these connections also may facilitate information, build trust, and improve state capacity, and many of these benefits may not be immediately observable). Notwithstanding the possibility of hidden benefits, my study highlights a massive social cost associated with patronage ties. Given the difficulty of observing such costs, it is, in fact, quite possible that the damage caused by the Sichuan earthquake, though devastating, is just the tip of the iceberg.

B Additional Data Description

B.1 Building Level Data Construction

As explained in the paper, the building level dataset is constructed by combining two lists of buildings from the archives. In this section, I provide additional information on the nature of the data source and the procedure of sample construction.

Data on Building Damage The data on building damage is obtained from the local *Earthquake Relief Reports* (*Kangzhen Jiuzai Zhi*), which are issued by each county through the local Gazetteer Office (*Difangzhi Bangongshi*). These reports are similarly formatted, although not entirely consistent in terms of the data they present. Counties in Sichuan Province issued these on an occasional basis, and I have used those that are publicly available. As of 2019, 31 counties and three prefectures had published their *Earthquake Relief Reports* — from which I extracted a list of buildings located in 37 counties. A prefectural *Earthquake Relief Report* covers materials from all of the counties it governs, which allows me to observe some additional counties that have yet to publish their own *Earthquake Relief Report*.

The books are generally comprised of three parts: the damage, the rescue efforts, and the reconstruction projects during and following the 2008 quake. The damage sections contain detailed descriptions and statistics of the damage caused by the earthquake; it is also common for the report to mention the damage to individual buildings. In most cases, the materials are compiled and presented by sectors and by towns. As a result, buildings recognizable within a town-sector's scope are most likely to be recorded. Representative types include schools, hospitals, government headquarters, some other public organizations (e.g., libraries, news outlets, postal offices, nursing homes), and a few prominent local factories (mainly state-owned). Residential or commercial buildings are rarely covered in the records.

The national standard categorizes building earthquake damage into five grades: "intact," "slight," "moderate," "severe," and "collapsed" (National Codes of P.R.C., 2002). Most of the buildings that I observe are referred to according to these grades. There are, however, buildings that have been described according to parallel standards (e.g., National Codes of P.R.C. (2008), which use four grades to rank building safety) or in words. The damage of these buildings was manually coded through a careful reading of the descriptions in accordance with the definitions of the standard grades. The work was conducted by a second person, who saw only the list of descriptions without knowing the details of the buildings that were being described (e.g., which county the building is located in, whether it had been linked to those in the other source).

It is worth mentioning that the indexes that I employ for the analysis vary slightly from the standard recommendations in National Codes of P.R.C. (2002). First, I group "intact" and "slight damage" into one single category because there are literally no "intact" buildings that entered the sample. Second, I split the standard "collapsed" into two categories to differentiate fully collapsed buildings (especially the notoriously shoddy ones such as those described in Section 2) whenever the data are specific enough to permit making a distinction. These modifications allow me to exploit better the types of variations in this specific context in which the seismic intensities are extraordinarily strong and the average buildings are "severely" affected. The results are robust to using an alternative index system that strictly follows the recommendation in the national standard (i.e., grouping all collapsed buildings into one single category).

Data on Building Construction Records The data on building construction records are obtained from the general *County Gazetteers* (*Xian Zhi*), which are published by each county's Gazetteer Office on an occasional basis, every few decades. Most counties in Sichuan Province have published two rounds of *County Gazetteers* since 1949. The first round was published generally between 1985 and 1989, covering materials that start from 1949 (and, in some cases, from 1911) until the publication year; the second round renewed the coverage until the 2003–2007 period. Because these gazetteers were published before the 2008 earthquake, it is unlikely for the observed construction projects to be selected by their future level of damage.

The *County Gazetteers* are book-length volumes of local history that document the county's major events. They are often regarded as a locality's "encyclopedia." The materials in these books are generally compiled and presented by town and by sector, and the prominent construction projects completed within the town-sector scope are often highlighted in the gazetteers. The building types likely to be recorded in these gazetteers are similar to those described in the *Earthquake Relief Reports*. This feature makes it feasible to identify a set of buildings that have been jointly mentioned in the two sources.

One potential issue with this data source is that, although some buildings report the date of their groundbreaking, others may report the date of their completion, and there is only a very small set of buildings for which both dates are reported. This inaccuracy could lead to serious measurement errors (which would bias the estimates toward zero) if the construction spans multiple years. Fortunately, China is famous for its speed in implementing public construction projects so that most of the building construction should be completed within one or two years[38] (which is verified with the small subset of buildings for which I observe both dates). In my analysis, I define a building's year of construction as the beginning of the construction project, which captures the period during which most planning, licensing, and inspection activities take place. For buildings that report only the date of completion, I take the previous year as their year of construction. My findings are robust to restricting the sample to buildings whose year of construction was reported with precision.

In addition to the year of construction, I also collect, whenever available, other

[38]This argument has been verified with the small subset of buildings for which I observe both dates, in which 50% of the buildings were completed within a year and another 30%, within two years.

building features, such as their size, number of stories, structure and material, as well as their funding source.

B.2 County Level Data Construction

To address the concern about the external validity of my building-level analysis, I construct a second dataset at the county level, which covers all 181 counties in Sichuan Province. This dataset contains aggregate damage statistics that are comprehensive, systematic, and economically relevant. In Section A.1, I use this dataset to evaluate the extent to which conclusions drawn from my building-level dataset are relevant for aggregate outcomes.

Earthquake Damage I obtained the county-level damage information from Zhang (2008). This statistical compendium provides systematic damage statistics for all 181 counties in Sichuan Province. It reports county-aggregates of physical and economic losses as well as sector breakdowns. I use optical character recognition techniques to extract the statistical tables from a digital version of the book and manually correct the recognition errors.

Hometown Ties The measures of hometown ties for the county-level sample are obtained from the same sources as those for the building-level sample. I aggregate the connectedness of the county officials over the period 1978–2007 to construct two measures. The first one is an indicator that denotes whether any of the county officials had connections during this period, and the second one counts the number of years in which a county official had been connected. In addition, I construct an indicator that denotes whether the county officials had hometown ties in 2008 to account for the impact of patronage ties during the quake and post-quake.

Description The summary statistics of my county-level dataset are presented in Table C2. The average death toll and direct economic loss are 479 lives and 3.6 billion RMB respectively. Of the counties, 60% have had a connected official, and the mean number of years of connectedness is 2.67.

B.3 Covariates

I construct some additional variables to account for other factors that might determine the damage to a building from the earthquake, including a set of building characteristics, geographical features, individual profiles of the officials, and county-wide socioeconomic conditions; these variables are explained in more detail below.

Building Features The first set of controls to consider are the characteristics of the buildings that may be relevant for their resistance. I collect these characteristics from the general *County Gazetteers* which also provide information about building construction history. The documents also mention, though inconsistently, some basic characteristics of the buildings, such as size, number of stories, structure, and materials used, as well as the funding source. For such buildings, I observe these characteristics and include them in my analyses; for cases of unreported information, I create a set of indicators that denote the specific missing variables.

Geographic Features Another factor that plays a central role in determining earthquake damage is geography, in particular, local seismic intensity and terrain ruggedness. For seismic intensity, I use PGA — a standard parameter in seismology that measures local ground motion. [39] The PGA is from *ShakeMap* (U.S. Geological Survey, 2017). The index for terrain ruggedness is constructed for each 30×30 arc-second grid cell using the elevation data from GTOPO30 (U.S. Geological Survey, 1996), following the procedure described in Nunn and Puga (2012). For the building sample, I geocode each building's location using Google Maps Geocoding API services to determine its local ground motion parameter and terrain ruggedness. For the county sample, I take the average of all lands within a county to calculate its overall intensity and ruggedness.

Individual Characteristics Whether county officials have patronage ties may be determined by information in their profiles that is relevant for local governance. Therefore, I also collect the individual profiles of these county officials from their online biographies, which indicate gender, year of birth, education, ethnicity, and the first year in their current positions. Because there are two county officials of interest,

[39] My empirical results are robust to using distance to the epicenter as an alternative proxy for seismic intensity.

I construct the following variables for a given county and year: an indicator that denotes gender, average age, average years of education, an indicator of belonging to an ethnic minority, and average number of years of tenure in their current positions. I also construct a set of indicators that denote missing values.

Economic and Demographic Conditions Finally, I include economic and demographic factors that might constrain the financial resources available and, thus, affect building resistance. I focus on per capita GDP and population measures. I obtain these data from the *China County Statistical Yearbook*. For the building-level analysis, I include the per capita GDP and population of the county in the year in which the building was constructed. [40] For the county-level analysis, I include these variables in 2007 to capture the local economic condition prior to the earthquake's occurrence.

C Supplementary Figures and Tables

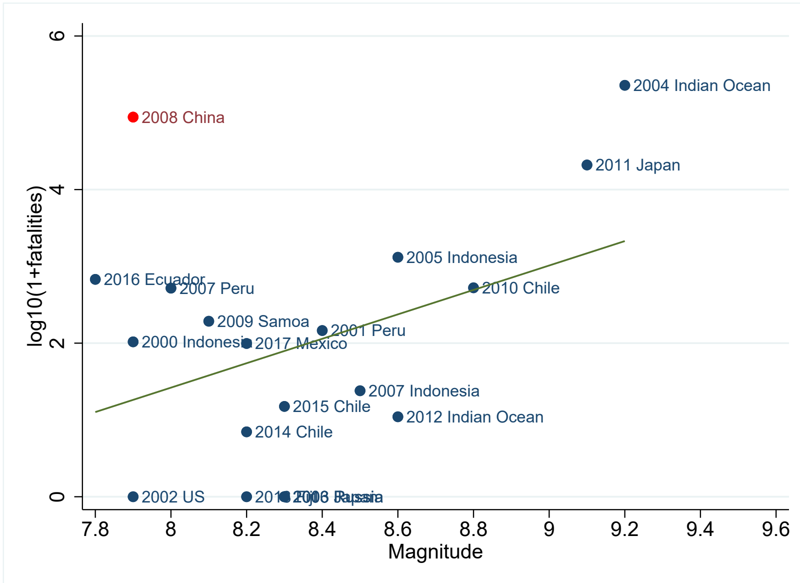
Table C1: Descriptive statistics of main variables in the building-level analysis

	Obs.	Mean	S.D	Max.	Min.
Outcome					
Damage Scale	1065	2.86	0.79	5.00	1.00
Treatment					
HometownTie	1065	0.16	0.37	1.00	0.00
Geographics					
Peak ground acceleration (% of g)	1065	28.72	23.05	104.00	4.00
Ruggedness	1065	265.96	302.27	1682.99	0.00
BuildingFeatures					
Stories #	55	4.65	2.44	13.00	2.00
Size (1,000 m^2)	611	4.88	14.83	220.00	0.00
Politicians					
AnyFemale	546	0.06	0.23	1.00	0.00
avg(Age)	639	44.05	4.58	56.00	32.00
avg(YrEdu)	792	15.13	2.47	18.00	9.00
avg(Term)	1065	2.97	1.57	8.00	1.00

Note. The unit of observation is a building in the quake-affected area.

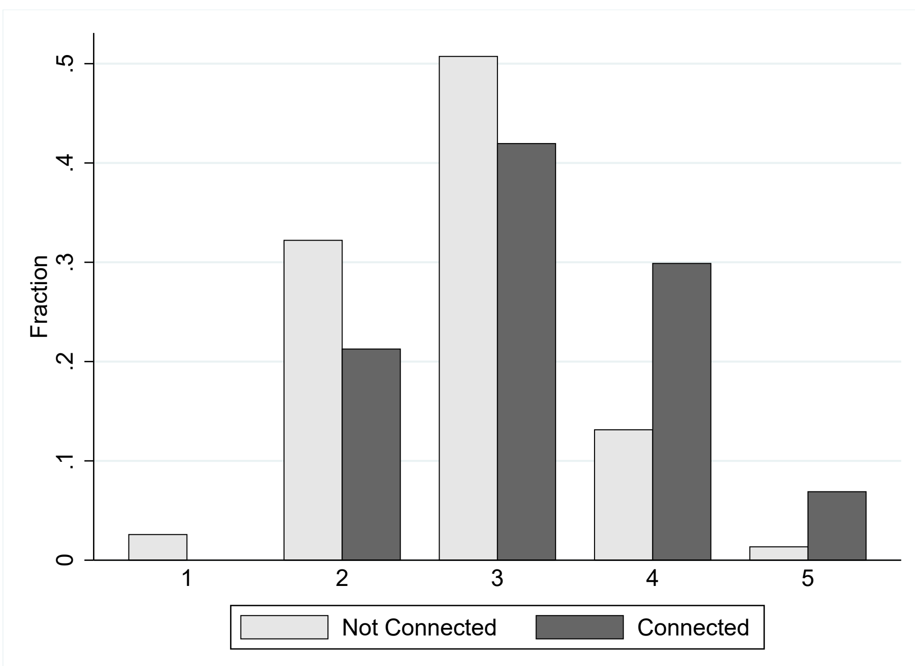
[40] Note that the economic and demographic constraints (which may affect building resistance) themselves might be an outcome of existing patronage ties, a matter often referred to as "bad controls" (Pearl, 2009). In view of this possibility, I do not include these conditions in my baseline specification in Section 4.2. Instead, I evaluate them as a robustness check in Section 4.3.

Figure C1: The 20 most notable earthquakes since 2000 in terms of magnitude



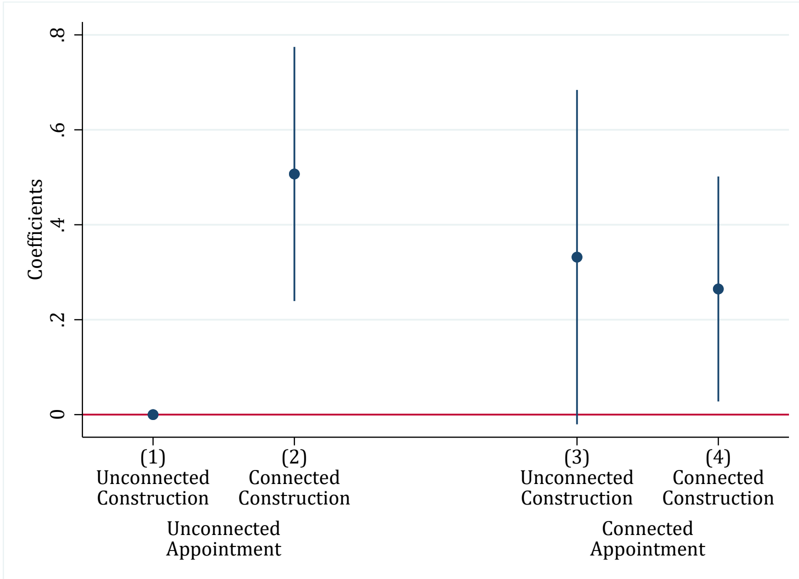
Source: USGS

Figure C2: Distribution of damage scales by connectedness



Note. The figure depicts the distribution of damage scales with and without hometown ties. Each bar represents the fraction of buildings that experienced each of the damage scales with and without hometown ties during their years of construction.

Figure C3: Patronage ties and building damages: selection vs. incentive



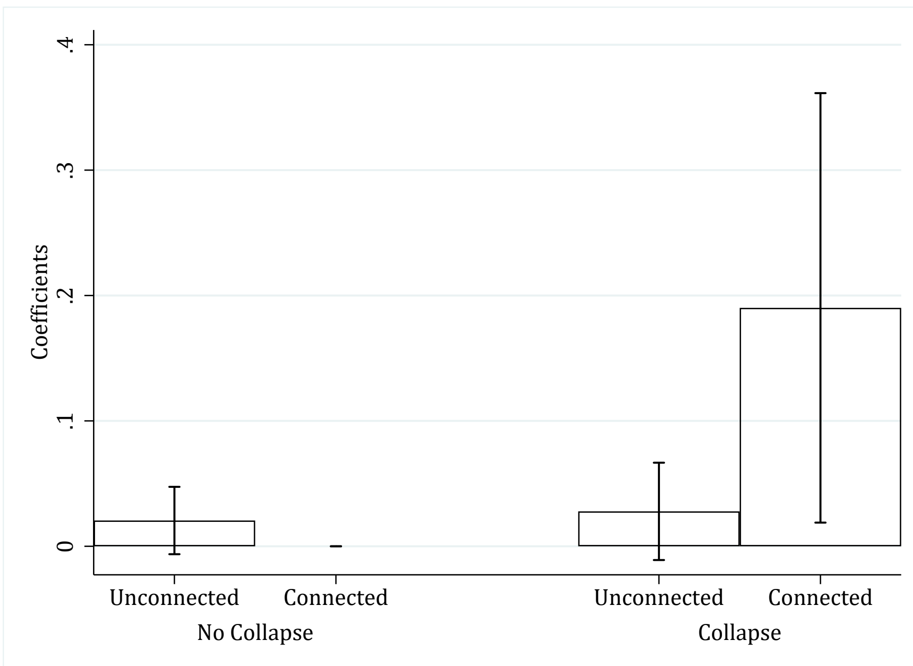
Notes: The figure depicts the estimated coefficients of building damage on the connectedness of the governing officials at the time of building construction and at the time of their appointment. The selection effect is estimated by the effect of connected appointment holding the connectedness status upon building construction constant. The incentive effect is estimated by the effect of connected construction holding the connectedness status upon official appointment constant. Buildings constructed by county officials who were unconnected in both time points are in the reference group. The sample contains 1065 buildings in the linked sample. The regression takes into account county fixed effects, year fixed effects, building type by year fixed effects, building features and geographic controls. Standard errors are clustered by county.

Table C2: Descriptive statistics of main variables in the county-level analysis

	Obs.	Mean	S.D	Max.	Min.
Dead_or_missing	181	479.46	2603.65	23787.00	0.00
Total_econ_loss (100M RMB)	181	36.16	86.50	596.76	0.00
EverConnected	181	0.59	0.49	1.00	0.00
YearsConnected	181	2.67	3.32	13.00	0.00
Peak ground acceleration (% of g)	136	11.01	15.07	70.83	1.00
Ruggedness	166	305.60	250.71	901.00	8.51
GDP (100M RMB)	137	43.91	43.31	282.19	1.83
Population (10K)	138	47.26	40.36	157.00	3.00
2008Connectedness	181	0.28	0.45	1.00	0.00

Note. The unit of observation is a county in Sichuan Province.

Figure C4: Patronage ties, building damages, and corruption investigation



Notes: The figure depicts the estimated coefficients of corruption investigation on patronage ties and building damage. The sample consists 261 county officials from the building sample, and is constructed by aggregating the damage of buildings (any or no collapse) constructed under the official's authority and the connectedness of the official (once or never connected). The dependent variable is an indicator that equals one if the county official was investigated for corruption. The regression controls for the average ground motion (PGA) of all buildings constructed under the official's authority. Standard errors are robust to heteroskedasticity.

Table C3: Patronage ties and building construction decisions

	Dependent Variables:					
	Location		Structure			
	ln(PGA)	Ruggedness	ln(Height)	ln(size)	ln(# Rooms)	ln(# Phases)
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	-0.0231	-5.1935	-0.3423	-0.1495	-0.0820	0.1272
	(0.0156)	(12.8770)	(0.3384)	(0.1299)	(0.0916)	(0.0935)
Individual Controls	Y	Y	Y	Y	Y	Y
BuildingType × Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	3.072	238.675	1.629	7.635	0.312	0.348
# Counties	62	62	17	51	26	27
# Observations	6096	6096	156	2445	268	352
Adjusted R^2	0.926	0.789	0.663	0.287	0.371	0.283

Note. The sample includes all buildings for which the years of contruction are observed. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C4: Patronage ties and building damages with social economic controls

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	0.2628**	0.2427**	0.2286**	0.2258**	0.2148**	0.4575**
	(0.1041)	(0.1002)	(0.1004)	(0.1032)	(0.1022)	(0.1817)
Per capita GDP (1,000 RMB)	-0.0140**	-0.0230**	-0.0229**	-0.0238**	-0.0220*	-0.0469**
	(0.0063)	(0.0104)	(0.0105)	(0.0109)	(0.0118)	(0.0227)
Population (1,000)	0.0001	0.0002**	0.0002**	0.0002**	0.0002**	0.0005**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.857	2.861	2.861	2.861	2.861	2.861
# Counties	35	35	35	35	35	35
# Observations	1062	1050	1050	1050	1050	1050
Adjusted R^2	0.334	0.389	0.392	0.393	0.394	
Pseudo R^2						0.291

Notes: The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C5: Non-supervisor connection and building damages

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie (w/ non-supervisor)	-0.0408	0.0669	0.0565	0.0625	0.0597	0.1012
	(0.1297)	(0.1119)	(0.1097)	(0.1082)	(0.1062)	(0.1980)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.857	2.861	2.861	2.861	2.861	2.861
# Counties	35	35	35	35	35	35
# Observations	1062	1050	1050	1050	1050	1050
Adjusted R^2	0.320	0.375	0.377	0.379	0.380	
Pseudo R^2						0.280

Notes: The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. $HometownTie(non-supervisor)$ is an indicator variable denoting that the county has an official connected with a prefectural-level official in an adjacent prefecture when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C6: Patronage ties and building damages with geographic coordinates fixed effects

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	0.0941	0.2429**	0.2427**	0.2258**	0.2684***	1.0126***
	(0.0827)	(0.0896)	(0.0890)	(0.0887)	(0.0970)	(0.2701)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Cell FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.858	2.866	2.866	2.866	2.866	2.866
# Counties	35	34	34	34	34	34
# Observations	910	894	894	894	894	894
Adjusted R^2	0.541	0.593	0.593	0.594	0.594	
Pseudo R^2						0.569

Note. The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county. *CellFE* is a set of fixed effects for each 1×1 arcminute (approximately 1.6 kilometers) cell based on the building's latitude and longitude.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C7: Patronage ties and building damages with hometown fixed effects

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	0.2456***	0.1901*	0.1836*	0.1688*	0.1551*	0.3559**
	(0.0801)	(0.0977)	(0.0963)	(0.0946)	(0.0895)	(0.1601)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
HomeCity FE	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	2.857	2.861	2.861	2.861	2.861	2.861
# Counties	35	35	35	35	35	35
# Observations	1062	1050	1050	1050	1050	1050
Adjusted R^2	0.349	0.402	0.406	0.407	0.406	
Pseudo R^2						0.315

Notes: The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county. *HomeCityFE* is a set of fixed effects for each specific city of origin.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C8: Patronage ties and building damages fixing prefecture-year effects

	Dependent Variable: Damage Scale (1–5)					
	OLS					Ordered Probit
	(1)	(2)	(3)	(4)	(5)	(6)
HometownTie	0.528***	0.587***	0.602***	0.604***	0.584***	1.358***
	(0.153)	(0.162)	(0.173)	(0.174)	(0.192)	(0.387)
Individual Controls					Y	Y
Geographic Controls				Y	Y	Y
Building Controls			Y	Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Prefecture × Year FE	Y	Y	Y	Y	Y	Y
Wild cluster p-value	0.058	0.009	0.010	0.005	0.017	
Mean(Dep.var)	2.859	2.862	2.862	2.862	2.862	2.862
# Counties	35	34	34	34	34	34
# Observations	1041	1030	1030	1030	1030	1030
Adjusted R^2	0.380	0.434	0.435	0.436	0.434	
Pseudo R^2						0.374

Note. The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C9: Patronage ties and building damage: two-stage diff-in-diff estimation

	Dependent Variable: Damage Scale (1–5)				
	(1)	(2)	(3)	(4)	(5)
HometownTie	0.318**	0.436***	0.431***	0.425***	0.440***
	(0.154)	(0.098)	(0.104)	(0.106)	(0.117)
Individual Controls					Y
Geographic Controls				Y	Y
Building Controls			Y	Y	Y
BuildingType × Year FE		Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Mean(Dep.var)	2.856	2.856	2.856	2.856	2.856
# Counties	37	37	37	37	37
# Observations	1065	1065	1065	1065	1065

Note. The dependent variable in all specifications is the level of damages on the 1(intact or slight damage)–5(full collapse) scale. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. The estimation follows the two-stage difference-in-differences approach described in Gardner (2021). Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Geographic Controls include the seismic ground motion parameter and terrain ruggedness of the building's location. Building Controls include the building's size, number of storeys, number of rooms, and a set of indicators denoting missing values. BuildingType includes a set of indicators denoting schools, hospitals, government headquarters, manufacture plants, and other public organizations. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C10: Cumulative connections and aggregate loss

	Dependent Variables (arcsinh): Dead or missing			Direct economic loss		
	(1)	(2)	(3)	(4)	(5)	(6)
OnceConnected	0.2846			0.3049**		
	(0.2484)			(0.1477)		
YearsConnected		0.1068***	0.0793*		0.0379*	0.0328*
		(0.0351)	(0.0415)		(0.0224)	(0.0193)
Controls	Y	Y	Y	Y	Y	Y
Prefecture FE			Y			Y
Mean(Dep.var)	1.803	1.803	1.803	2.418	2.418	2.418
# Observations	181	181	181	181	181	181
Adjusted R^2	0.675	0.689	0.795	0.793	0.791	0.924

Notes: The dependent variable in the first three columns is the inverse hyperbolic transformation of the number of deaths (including missings); the dependent variable in the last three columns is the inverse hyperbolic transformation of total economic loss. *OnceConnected* is an indicator variable denoting whether the county ever had a connected official since 1978. *YearsConnected* denotes the cumulative number of years that the county had a connected official since 1978. The control variable include the county's average seismic ground motion parameter (PGA), average terrain ruggedness, GDP in 2007, population in 2007, connection status in 2008, and a set of indicators denoting whether each of these variables is missing. Robust standard errors in parentheses.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C11: Cumulative connections, seismic intensity, and aggregate loss

	Dependent Variables (arcsinh): Dead or missing			Direct economic loss		
	(1)	(2)	(3)	(4)	(5)	(6)
OnceConnected	-0.5243			0.2678		
	(0.3257)			(0.2326)		
Once connected × ln(PGA)	0.6115***			0.0280		
	(0.2168)			(0.1202)		
YearsConnected		-0.0552	-0.0757		0.0224	0.0476
		(0.0509)	(0.0599)		(0.0393)	(0.0330)
Years connected × ln(PGA)		0.0957***	0.0887**		0.0092	-0.0085
		(0.0240)	(0.0341)		(0.0161)	(0.0169)
ln(PGA)	1.7214***	1.6685***	0.9168***	1.7853***	1.7569***	0.8104***
	(0.2023)	(0.1858)	(0.2518)	(0.1108)	(0.1000)	(0.1180)
Controls	Y	Y	Y	Y	Y	Y
Prefecture FE			Y			Y
Mean(Dep.var)	1.803	1.803	1.803	2.418	2.418	2.418
# Observations	181	181	181	181	181	181
Adjusted R^2	0.695	0.717	0.808	0.792	0.790	0.923

Notes: The dependent variable in the first three columns is the inverse hyperbolic sine transformation of the number of deaths (including missings); the dependent variable in the last three columns is the inverse hyperbolic sine transformation of total economic loss. *OnceConnected* is an indicator variable denoting whether the county ever had a connected official since 1978. $YearsConnected$ denotes the cumulative number of years that the county had a connected official since 1978. $ln(PGA)$ denotes the average peak ground acceleration in the county, measured as % of g. Other control variables include average terrain ruggedness, GDP in 2007, population in 2007, connection status in 2008, and a set of indicators denoting whether each of these variables is missing. Robust standard errors in parentheses.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C12: Cumulative connections and aggregate economic loss by sector

	Dependent Variables: Economic loss in ... (arcsinh) Infrastructure	Education	Health	Government	Agriculture	Manufacture	Service
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
YearsConnected	0.0354**	0.0089*	0.0076*	0.0099	0.0411***	0.0686***	0.0477**
	(0.0167)	(0.0046)	(0.0041)	(0.0127)	(0.0135)	(0.0233)	(0.0228)
Controls	Y	Y	Y	Y	Y	Y	Y
Prefecture FE	Y	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	0.954	0.166	0.081	0.192	0.413	0.581	0.486
# Observations	181	181	181	181	181	181	181
Adjusted R^2	0.846	0.641	0.528	0.620	0.798	0.758	0.702

Notes: The dependent variables are the inverse hyperbolic sine transformation of economic loss in each sector. $YearsConnected$ denotes the cumulative number of years that the county had a connected official since 1978. The control variable include the county's average seismic ground motion parameter (PGA), average terrain ruggedness, GDP in 2007, population in 2007, connection status in 2008, and a set of indicators denoting whether each of these variables is missing. Robust standard errors in parentheses.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C13: Patronage ties and building construction records

	Dependent Variable: Number of buildings (ln)							
	All types			Hospital	School	Public Org.	Factory	Gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HometownTie	-0.0879	0.0304	0.0328	0.0286	-0.0398	0.0423	0.0192	-0.0193
	(0.1422)	(0.0967)	(0.0884)	(0.0393)	(0.0839)	(0.0703)	(0.0739)	(0.0359)
Individual Controls			Y	Y	Y	Y	Y	Y
County FE		Y	Y	Y	Y	Y	Y	Y
Year FE		Y	Y	Y	Y	Y	Y	Y
Mean(Dep.var)	1.085	1.085	1.437	1.437	1.437	1.437	1.437	1.437
# Counties	65	65	63	63	63	63	63	63
# Observations	1400	1400	1057	1057	1057	1057	1057	1057
Adjusted R^2	0.000	0.538	0.492	0.204	0.260	0.263	0.588	0.260

Note. The sample is a balanced county-panel of all damaged counties between 1978–2007 (including those with zero construction records). The dependent variables are the number of buildings constructed, calculated as the natural logarithm of one plus the value. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C14: Patronage ties and other political and economic outcomes

	Dependent Variables:				
	Growth rate in:				Corruption
	GDP	Population	Transfer	Admin. Expense	Investigation
	(1)	(2)	(3)	(4)	(5)
HometownTie	1.8459	0.0036	0.1606***	0.2065**	0.1017**
	(2.9728)	(0.0145)	(0.0412)	(0.0814)	(0.0485)
Individual Controls	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Mean(Dep.var)	15.510	0.021	0.275	0.501	0.142
# Counties	64	64	64	64	65
# Observations	731	733	733	685	1387
Adjusted R^2	0.072	0.016	0.460	0.516	0.360

Note. The sample is a balanced county-panel of all damaged counties between 1978–2007 (including those with zero construction records). The dependent variables are the economic and political outcomes in the county. *HometownTie* is an indicator variable denoting that the county has a connected official (either the party secretary of the governor) via hometown ties when the building was constructed. Individual Controls include an indicator for female, an indicator for minority, average age, average education, and average term of the party secretary and the governor. Standard errors are clustered by county.
Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.