Do Authoritarian Elections Help the Poor? Evidence from Russian Cities

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Abstract

Do local elections under autocracy help the poor? We argue that local appointees in electoral authoritarian regimes have political incentives that undermine public service provision; regime leaders' preoccupation with national electoral control encourages them to overlook local governance problems if subnational officials can still deliver requisite votes in national elections. Using geographic and temporal variation across Russian cities (2002-2012) in the elimination of mayoral elections, we investigate how mayoral appointments affect the maintenance of aging housing infrastructure. We find that, compared to elected mayors, appointed mayors allow more of their Soviet-era housing stock to become dilapidated and unsafe. Moreover, bad housing increases more in cities where appointees deliver high vote shares to the ruling party in national elections. Thus, while local elections under authoritarianism can improve local governance, the holding of semi-competitive national elections can actually undermine incentives for local *appointees* to provide public services.

Keywords: authoritarian elections, decentralization, public services, political incentives, Russia

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Electoral incentives are central to many theories of government responsiveness to the poor. Much of this research is at the regime level, with scholars debating vigorously about whether democracies do a better job than autocracies at providing public goods to the poor (Sen 1999, Bueno de Mesquita et al. 2003, Keefer & Khemani 2005, Ross 2006). However, many of the things most needed to improve the lives of poor people – clean water, transportation, safe neighborhoods, affordable housing, basic education and health care – are provided by local governments. Consequently, the provision of public services that affect the poor may depend as much on local leaders' political incentives as on national regime type.

In this paper, we examine how the career incentives of local officials in electoral authoritarian regimes affect pro-poor policies. In contrast to previous literature, we theorize the incentives of both *appointed* and *elected* officials. Existing research on the topic, which has been studied predominantly in democratic contexts, has focused intensively on whether *elected* local officials have incentives to respond to poor voters. At the same time, the literature tends to gloss over *appointed* leaders, as if the absence of elections implies a lack of political incentives that affect policy choices. Yet, we know local appointees have reappointment and promotion goals that incentivize them to pursue policies that will please those who appoint them (i.e. central leaders). Are poor citizens under electoral autocracy better served by elected local officials or by appointed ones?

We argue that local appointees in an electoral authoritarian regime have political incentives that undermine public service provision. In electoral autocracies, regime leaders place great emphasis on winning national elections and winning them handily. This leads them to value subnational officials who can deliver votes for the ruling party in national elections. Since national election campaigns focus on host of issues aside from local public goods – e.g. evaluations of the national leader, foreign affairs, the national economy, etc. – local appointees whose primary task is mobilizing votes in national elections will be less responsive to local conditions than locally-elected officials who are well-incentivized to respond to local concerns. Moreover, as local appointees display more success in mobilizing votes for the regime in national elections, central leaders are even more likely to overlook poor local governance. Thus, our two central claims are: 1) *local* elections

under autocracy improve local governance, and 2) in electoral autocracies, the need to deliver votes in national elections undermines local *appointees'* incentives to promote good governance.

To test these claims, we investigate how replacing mayoral elections with mayoral appointments in Russian cities has affected the condition of low-quality housing that is maintained by local governments. Compared to government spending measures, which may not translate into tangible development outcomes, or mortality and educational indicators, which are co-determined by many factors aside from government policy, housing maintenance in Russian cities represents a specific pro-poor outcome that is directly and almost exclusively linked to local government policy efforts. Russian municipal governments are responsible for ensuring that pre-existing apartment buildings meet certain minimum standards of quality and safety. When buildings fall below this standard, municipal governments are required to resettle residents into acceptable alternative housing. However, in practice, cities vary in the extent to which they achieve this. Importantly for our purposes, dilapidated apartment blocks in Russia are mostly occupied by low-income households.

Using geographic and temporal variation in the way Russian cities select their local executives (2002-2012), we find that the amount of bad housing remains lower in cities that retain elected mayors compared to cities that switch to an appointment system. Consistent with our argument, we also find that appointees' ability to perform political services for the regime conditions their responsiveness to local needs: dilapidated housing increases more in cities where appointees can deliver high vote shares in national elections to the ruling party, United Russia. This suggests that central leaders have been more willing to overlook bad housing outcomes in cities where appointees have successfully fulfilled regime leaders' core political goals.

Our study has several implications for the literature. First, it helps establish the conditions that need to be in place in order for local elections to improve public goods provision. The conventional wisdom treats local elections an as unalloyed good because electoral accountability creates incentives for public goods provision. We are sympathetic to this view, but our study highlights the fact that no comparison between elections and appointments is possible without consideration of appointees' career incentives. If their reappointment hinges on providing good governance, then

elections may not outperform appointments. But if reappointment hinges on political service to the regime, then elections are likely to do a better job of ensuring the provision of local public goods.

Our study has a similar message for those who are skeptical that elections improve local governance. Some scholars argue that state capture, imperfect information, and resource imbalances can undermine local electoral accountability (e.g. Blanchard and Shleifer 2001, Bardhan and Mookherjee 2006). Our findings suggest that, compared to appointments, elections will only be worse for public service provision if appointees have sufficient incentives to pursue good governance.

The paper also has important implications for the study of autocracy. Consistent with Miller (2015), we show that electoral accountability pressures help lead to better development outcomes under autocracy, despite the fact that electoral manipulation tends to blunts these pressures. Local elections under autocracy lead elected Russian mayors to do better at maintaining their cities' aging housing stock. But our study also highlights some unappreciated side effects of electoral competition under autocracy. When appointed subnational officials are primarily incentivized to help the ruling party dominate major elections, they can focus myopically on that goal. If these officials can deliver vote totals in national elections for the ruling party without making costly investments in local public goods, then this is what they will do. Scholars of authoritarian politics have identified different appointment criteria operating in different settings (Li & Zhou 2005, Reuter & Robertson 2012, Beazer 2015, Wang 2015, Wallace 2016, Hassan 2017, Reisinger & Moraski 2017). Our study shows how these appointment criteria can have meaningful consequences for everyday citizens. For example, studies find that the Chinese economy has benefited from the regime's practice of evaluating subnational officials on the basis of economic performance (Maskin, Qian, and Xu 2000, Xu 2015). This is consistent with the perspective we have offered here. As a non-electoral regime, the Chinese Communist Party does not need to manage national elections to stay in power. This frees China's leaders to use governance as a primary criteria for evaluating local officials. By contrast, electoral autocracies such as Russia apply politicized appointment criteria because they need sub-national officials to help them win semi-competitive elections. Our findings indicate that emphasizing this political service comes at a cost to local governance.

Local Elections and Pro-Poor Policies

In this paper, we examine how the elimination of local elections under autocracy affects public service provision for the poor. In trying to understand how elections affect the poor, social scientists have primarily focused on the national level. Prominent arguments assert that electoral incentives make the poor in democracies better off because democratic leaders must satisfy a wide range of supporters, not just a powerful economic elite (Meltzer & Richard 1981, Bueno de Mesquita et al. 2003, Sen 1999). Despite strong theoretical expectations, the empirical evidence is mixed. Some studies find that democracies spend more on education and healthcare (Brown & Hunter 2004, Stasavage 2005) and perform better on indicators of human development (Lake & Baum 2001, Brown & Mobarak 2009), but others find no such relationships (Ross 2006, McGuire 2006).

As an alternative to studying regime effects, we study changes in how local-level leaders are selected. This allows us to hold country-level factors constant and concentrate on the presence or absence of elections – the key feature of democracy that is supposed to help the poor. It has long been argued that local elections inform leaders about local conditions and that reelection concerns make locally-elected governments more responsive to citizens, including the less affluent (de Tocqueville 1835, Treisman 2011). Others, however, are more skeptical about local elections' supposed benefits for social welfare.[1] In particular, the decentralization literature warns against the danger of state capture by local elites. According to these concerns, rent-seeking elites and interest groups can obtain outsized influence over local elected officials, leading them to divert resources away from public goods in return for reelection support. The resulting corruption and inefficiency threatens the quality of local public services (Rose-Ackerman 1999, Bardhan & Mookherjee 2006).

Despite the topic's clear importance, relatively few studies provide empirical insights into political decentralization's effects on pro-poor policies.[2] Foster and Rosenzweig (2001) find that the

[1]Critics have identified several factors that inhibit the poor in democracies: information asymmetries (Keefer & Khemani 2005), disparities in resources (Ross 2006, Bartels 2010), institutionalized inequality (Bonica et al. 2013), and unequal participation (McCarty, Poole & Rosenthal 2016). Such concerns likely apply to local elections as they do to national elections.

[2]Here we define political decentralization as giving local actors, particularly citizens, the ability

presence of village elections in India increased the quality of roads, irrigation, and schools. Research in Indonesia, meanwhile, finds that introducing direct elections for district heads had no effect on investment in public infrastructure (Sjahrir, Kis-Katos & Schulze 2014). Using survey data from 60 Chinese villages, Zhang et al (2004) find that introducing direct elections for village governments shifted the tax burden from individuals toward enterprises. Others have used larger surveys to demonstrate positive associations between direct elections and investment in public goods (Luo et al. 2007). In a similar manner, we exploit institutional variation across both years and cities in Russia to test our argument about local officials' differing incentives to provide public services, but we do so in an electoral authoritarian context and focus more narrowly on policies that disproportionately affect the low income strata.

Electoral Autocracy and Local Leaders' Incentives

Existing arguments about public service provision are lopsided, concentrating almost exclusively on elected leaders' incentives while glossing over appointees' incentives. In fact, most political economy models go no further than to note that appointees lack direct electoral incentives to respond to voters. What are the incentives facing non-elected officials? This is not an idle question, particularly in authoritarian regimes. To take one example, those who argue that locally-elected officials are vulnerable to capture must also explain why unelected regime appointees would be more motivated to provide public services than elected-but-captured local officials. For the state capture argument to stand under autocracy, scholars also need to theorize about why non-democratic central governments would incentivize their agents to promote local pro-poor policies.

To understand appointees' incentives, one must consider the criteria that central leaders use to appoint them. In electoral autocracies, winning elections by large margins is a core regime priority; even narrow electoral victories can embolden regime opponents and encourage elite defections. While elections may offer significant long-run benefits to autocrats, research shows that individual elections can be moments of severe vulnerability (Knutsen, Nygard, and Wig, 2017, Robertson and

to select their own local leaders. This is separate from the voluminous literature on the effects of administrative and fiscal decentralization, which we do not consider here.

Pop-Eleches 2015, Tucker 2007). Special care must be taken to address this systemic vulnerability. To maintain their electoral dominance, autocrats sometimes take drastic actions, such as repression and ballot-box fraud. However, regime leaders primarily use subtler means of disadvantaging the opposition: cooptation, bribery, media control, vote buying, voter intimidation, clientelism, and patronage spending (Levitsky & Way 2010, Magaloni 2008). Implementing these tasks effectively requires sustained coordination and support by state officials, even outside of election years.

In this regard, subnational officials are key to the regime's vote mobilizing efforts. Subnational officials wield authority and influence over local elites and voters. They understand local conditions, and their connections are indispensable to maintaining local patron-client networks. In Russia, for example, subnational officials are the regime's primary vote brokers (Reuter 2013, Golosov 2011). These officials use a myriad of democratic and undemocratic tactics to win elections. Via their elite networks, they may facilitate clientelist mobilization and, sometimes, repression. They can put pressure on firms to mobilize their employees (Frye, Reuter and Szakonyi 2012). They may also use control over local media outlets to privilege regime candidates. Or, they may lend their authority and influence to the ruling party via endorsements and public appearances.[3] As a result, when regime leaders have the ability to appoint local leaders, they make delivering votes to the ruling party a fundamental duty. Research on electoral autocracies shows that appointed sub-national officials who do well at mobilizing votes for the ruling party are more likely to be reappointed or promoted (Reuter & Robertson 2012, Reisinger & Moraski 2017, Blaydes 2010).

Given the regime's desire to enlist subnational officials' help in maintaining electoral dominance, the method of selecting local leaders has consequences for public service provision. Because appointed leaders have incentives to prioritize their principals' goals, political centralization encourages local leaders to concentrate their efforts on helping the regime win votes in *national* elections. In the process, local public services can become neglected.

[3]Numerous scholars have written about the operation of regional and local political machines in Russia. For a discussion of these machines at the municipal level see Bychkova & Gel'man (2010), Gel'man & Ryzhenkov (2011), and Gilev et al. (2017).

To see this, one can contrast the political incentives of locally-elected mayors with those of appointees who are evaluated on their ability to mobilize votes in national elections. Recent literature shows that, despite electoral manipulation, there is some electoral accountability in competitive authoritarian elections (Magaloni 2008, Miller 2015). Thus, to a degree, elected regime officials have some career incentives to placate voters. This means that, in competitive authoritarian regimes, locally-elected executives must exert some effort to please voters in local elections, and voters in local elections are primarily concerned with the performance of local governments.

By contrast, appointees do not need to please voters in local elections. To the extent that local appointees must please voters to stay in office, it is in *national* elections, where they are charged with mobilizing votes for the ruling party.[4] This is a key distinction. Individual voters in national elections care not just about local public goods, but also other national issues (e.g., national economy, national security, foreign affairs, federal policy, president's conduct, etc.). Some voters in national elections may care exclusively about national issues, while others may care about both national and local issues. The share of the electorate voting solely on the basis of local public goods, however, is much smaller in national elections than in local elections. It should come as no surprise, then, that national campaigns in federal countries focus more on national issues, while local campaigns focus on local issues (e.g. Atkeson and Partin (2001)).

The predominance of national issues in national election campaigns undermines local appointees' incentives to provide public goods. In *local* elections, the marginal electoral return on effective public goods spending is high because voters in those elections care primarily about local governance, and elected mayors are well-incentivized to invest in them. In *national* elections, however, the marginal electoral return on local public goods spending is much lower because voters do not vote on the basis of local governance or do so only in part.[5] Thus, for appointed local leaders, whose focus is delivering votes to the regime in national elections, the marginal electoral return on

[4]This is their first priority. Regime leaders also want subnational officials to mobilize votes at lower levels (e.g. for city council, etc.), but the first priority is securing votes in national elections.

[5]Indeed, as we show below, elected mayors are punished for poor housing outcomes in their own elections. By contrast, additional analyses in the appendix reveal that United Russia's vote

local public goods is lower, making them more likely than elected mayors to divert their time and resources to other uses, such as rent-seeking.[6] Compared to appointees, locally-elected mayors are deterred from such rent-seeking, in part, because they receive a high electoral return on providing public goods. Consequently, we expect that, under electoral authoritarianism, local leaders who are elected will provide more public services than local leaders who are appointed.

Naturally, electoral autocrats may see value in local public service provision. Some regime goals, such as political stability, are easier to achieve when local leaders attend to citizens' needs, and bolstering pro-poor services might help the regime win more votes. But even if regimes want both vote mobilization and good local governance, not all agents have enough resources or expertise to be equally effective at both. Thus, rulers may often face a choice between appointing officials based on their political skills or their capacity for good governance. Given its importance in electoral authoritarian regimes, the capacity to mobilize votes will often win out. Myerson (2015) notes that local appointees may not be fired for poor governance if they offer other services that the ruler values even more. Likewise, we argue that electoral autocrats will be willing to overlook poor local governance when agents do a good job of achieving their first priority – delivering votes in major elections. And, if subnational officials can mobilize votes in national elections without devoting extensive resources to public services, then they will likely choose that easier path.

This reasoning suggests that the effects of political centralization on local pro-poor policies can depend on appointees' political service to the regime. As local appointees deliver more votes to satisfy the regime's electoral needs, they have even fewer career incentives to provide public services to citizens in need. Thus, we posit that appointees will be less likely to pursue pro-poor

share in Duma elections is unaffected by the state of local housing.

[6]Appointee incentives may also depend on who is appointing. Where *national* officials *directly* appoint local leaders, voters may be more likely to punish the national government for local policy failures. By contrast, in Russia, where nationally-appointed *regional* officials in turn appoint local officials, voters may be less likely to connect local issues to national elections. Instead, as Beazer and Reuter (2019) show, they may focus their ire on the regional authorities.

policies when they are performing well at mobilizing votes. Conversely, if appointees are failing to impress superiors on the most important reappointment criteria, they may need to exert extra effort to succeed in other areas. Thus, when appointees mobilize fewer votes, they must pay more attention to public service provision because they cannot afford to underperform on local governance criteria as well. We test these implications in the empirical analyses that follow.

Selection of Local Executives in Russia

Russia offers a unique opportunity to study the consequences of local elections and appointments. Due to a wave of reforms we discuss below, the use of appointments varies across Russian cities: some mayors are directly elected while other mayors are appointed by regime officials. By analyzing subnational variation, we can study the effects of local elections on pro-poor policies while holding constant political and cultural factors that would be difficult to account for in a cross-national study of electoral authoritarian regimes. Furthermore, the number of cities holding mayoral elections has changed over time and within regions. We exploit this temporal and cross-sectional variation to investigate the impact of local elections on public service provision.[7]

By Russian law, city councils can determine how their municipality's chief executive is selected. Before the 2000s, roughly 90% of Russia's large cities elected their mayors directly. Starting in the mid-2000s, some cities began replacing direct mayoral elections with a system of indirect elections that were de facto appointments. Under the appointment model, candidates were chosen by a commission of officials from the city legislature and the regional administration.[8] The city legislature then selected from that list of candidates. This trend towards mayoral appointments continued steadily until, by 2012, roughly half of Russia's large cities had appointed mayors.

Despite city councils' formal role in their municipal governance model, all observers interpret

[7]During the period of study, neither local governments' policy responsibilities nor levels of fiscal centralization changed significantly. Subnational governments did lose significant tax autonomy during this period, but that happened on a national scale, not city-by-city.

[8]Initially, commissions were 2/3 local legislative delegates and 1/3 regional delegates. Around 2014, this composition shifted to 50% from each.

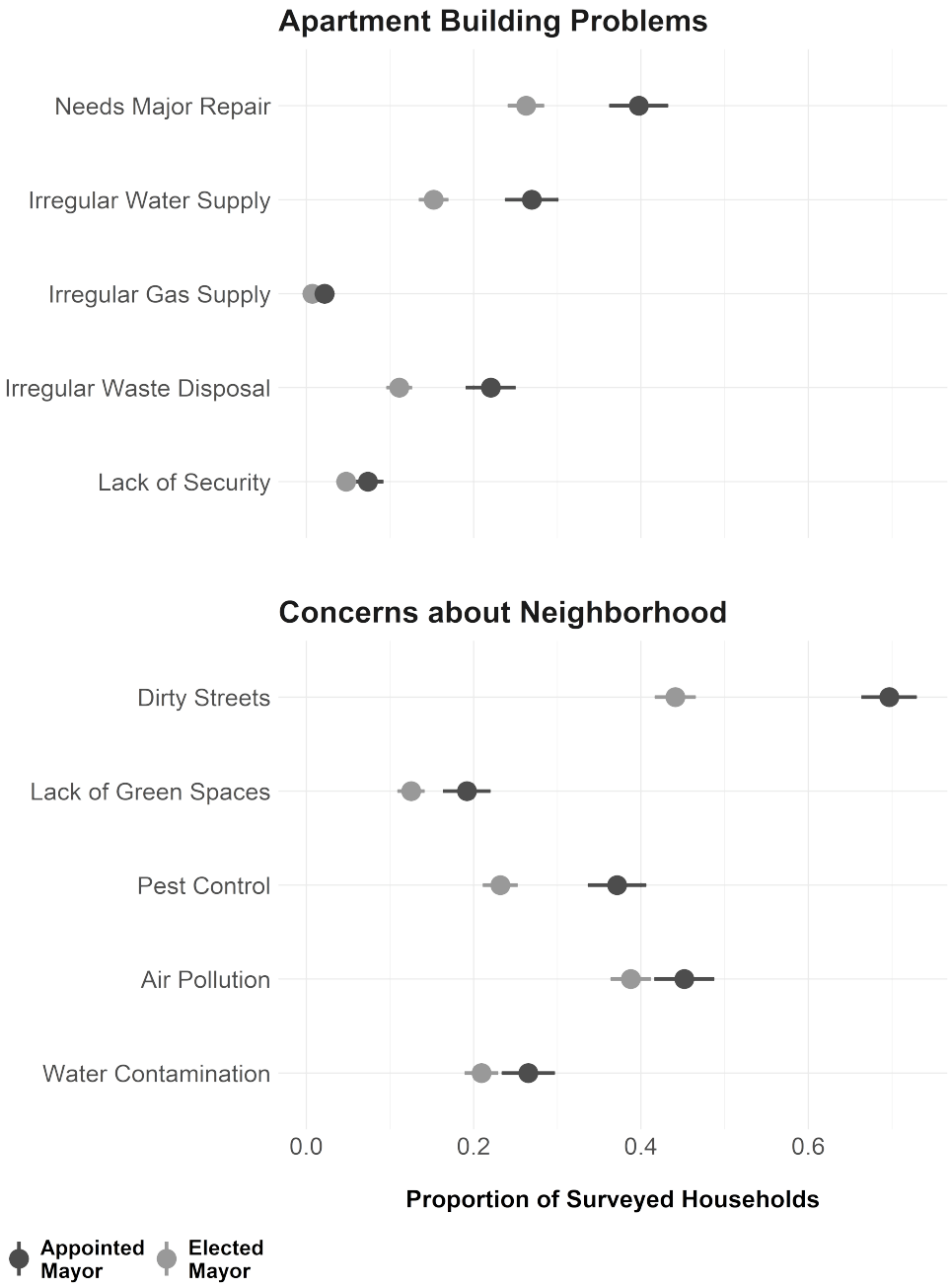
the shift to appointed mayors as part of Vladimir Putin's efforts to recentralize political authority. The cancellation of local elections closely tracks other Kremlin efforts to increase the federal center's power. Throughout the 2000s, high oil prices and Putin's popularity tipped the balance of resources decidedly in the center's favor, allowing the Kremlin to further weaken regional elites' power through de jure institutional reforms. For example, between 2004 and 2012, Russia's regional governors were appointed by the president (subject to confirmation by the regional legislature). Accordingly, governors became the Kremlin's agents with the primary task of managing regional politics. Studies find that their reappointment hinged on their ability to mobilize votes for United Russia (Reuter and Robertson 2012, Reissinger and Moraski 2013)

During this time period, United Russia came to dominate national and subnational politics. By 2012, 86% of city councils in large cities had UR majorities (Reuter et al. 2016). Within this context, governors began working through UR factions in city councils, pressing deputies into canceling mayoral elections.[9] Indeed, most qualitative and press accounts suggest that governors and/or regional UR branches typically initiated the cancellation process and that when regional authorities tried to remove local elections, they usually succeeded. Governors then pressured municipal deputies – either informally or via the regional UR branch – to ensure loyal candidates were selected (Panov 2018, Avdonin 2015). Thus, even though local legislatures formally select city managers, both scholars and observers treat these city mangers as effectively appointed by the regional administration (see Kynev 2010, Gel'man and Lankina 2008, and Golosov et al. 2016).

We note that leaders' discretion over when and where to cancel elections requires researchers to exercise extra caution. In our investigations, we adopt a variety of empirical approaches to deal with this specific issue. We find no evidence that the decision to abandon mayoral elections depended on local governance conditions. We also demonstrate that our results hold when control-

[9]See, for example, Gel'man and Lankina (2008), Makarkin (2007), and Ross (2008). See also Petrov, Nikolai. "Freely Elected Mayors a Dying Breed?" *Moscow Times.* 1 June 2010 and Kynev, Alexander "Otmenyaya pryamyie vybory merov, gubernatory ne usilivayut, a oslablyayut vlast kak takavuyu" Gazeta.ru 9 September 2010.

Figure 1: **Russian Household Survey: Respondents with Appointed Mayors are More Dissatisfied with Public Services**



Note: Data from the 2009 wave of the *Russian Longitudinal Monitoring Survey* (RLMS). Respondents grouped based upon the elected/appointed status of their local executive: $n_{elected} = 1558$; $n_{appointed} = 735$. Bands represent 95% confidence intervals. Within every category, group means are statistically different at $p \leqslant 0.01$.

ling for remaining plausible explanations for local elections' removal. These extra analyses appear later in the paper's penultimate section, which deals exclusively with concerns about potential endogeneity and the selection of cities into the appointment model.

How plausible is the notion that political centralization in Russia may have changed local public service provision? Evidence from a 2009 sociological survey of Russian households provides some suggestive support for this idea. Respondents were asked to identify concerns about their housing conditions and their neighborhood that relate directly to services under the local administration's purview. Figure 1 reveals a telling schism: dissatisfaction with public services is significantly higher among Russian households in cities with appointed mayors than in cities with elected mayors. Respondents with appointed mayors are more likely to complain about a variety of conditions, from building maintenance, utilities delivery, and security to dirty streets, poor pest control, and issues with air and water quality. Based on these descriptive data, it appears that appointed mayors in Putin's Russia are devoting less attention to basic services for their community. In the following empirical section, we test this more rigorously using changes in mayors' elected/appointed status alongside time-series cross-sectional data on municipalities' troubled and dilapidated housing.

Data Description & Empirical Methodology

To test our hypotheses, we collect a number of economic and political indicators for 200 of Russia's largest cities for the time period 2002-2012. Economic data come from various Russian statistical publications as gathered by *Multistat*. Data on mayoral appointments was originally collected by the International Center for the Study of Institutions and Development (ICSID) at Moscow's Higher School of Economics and extended by the authors. The unit of analysis is the city-year.

Dependent Variable: Maintenance of Aging Soviet Apartment Blocks

Our dependent variable, which proxies for public service provision to the poor, is the amount of BAD HOUSING in Russian cities. In order to alleviate severe postwar housing shortages under Khrushchev and later Brezhnev, Soviet planners adopted a mass-scale approach to housing construction that used prefabricated concrete panels and uniform floor plans to supply modest-sized apartments quickly and inexpensively. Notoriously, many of these so-called *khrushchyovki*

buildings were built as low-quality, temporary stopgaps with an expected 25-year lifespan. Almost 60 years later, however, cities continue to have many neighborhoods that are predominately *khrushchyovki*. A smaller, but significant share of at-risk housing stock is located in Stalinist-era concrete buildings and pre-revolutionary brick constructions. After the USSR's collapse, municipalities became the *de facto* caretaker for many of these pre-existing buildings, despite often lacking sufficient resources to provide consistent upkeep or meet residents' expectations (Shomina & Heywood 2013). Unless maintained properly over time, this outdated housing stock tends to deteriorate rapidly, creating a host of problems that range from inconvenience to health and safety hazards to neighborhood blight. Because affluent individuals have more means and opportunity to move away from ill-maintained apartment buildings to newly-constructed buildings, scholars have identified troubled and dilapidated housing conditions as a persistent welfare issue that afflicts poorer individuals and threatens to create slums, which heretofore have not been a part of Russia's urban landscape (Alexandrova, Hamilton & Kuznetsova 2004).

Housing is the ideal policy area for testing our hypotheses. Russian voters consistently rank housing and utilities as among the most important issues affecting their daily lives, making the issue politically salient.[10] Moreover, policy governing dilapidated housing is primarily determined by local governments, so voters can and should assign blame to municipal authorities for not dealing with bad housing (Frolov and Agafanov 2011). Indeed, housing is often one of, if not the, primary issue in Russian municipal campaigns. Next, poor-quality housing is an outcome that mostly affects low-income households. Finally, and not unimportantly, we are able to locate city-level data on housing *outcomes* in Russian cities. Some studies of pro-poor policies use government expenditures as the dependent variable, but public spending may not always reach poor voters. Other studies use broad development outcomes, such as mortality and educational attainment, but because these outcomes are affected by many factors aside from government policy, it is harder to

[10]Survey data indicates that housing concerns rank alongside other major issues, such as inflation, unemployment, and low salaries/pensions. Figures A3-A5 in the appendix show relative rankings from surveys across multiple years.

establish causal influence. Moreover, there is little data on the quality of other politically-salient, locally-provided public goods for the poor in Russian cities. Bad housing is unique in this regard.

Classifying structures as troubled or dilapidated involves many actors (Frolov and Agafonov 2011). The process begins when the federal housing inspection service conducts an inspection, which can be planned, unplanned, or in response to residents' complaints; local government is not involved in this step. Outstanding matters are referred to an intergovernmental commission, whose members include representatives of federal, regional and local bureaucracies, as well as local utility companies.[11] After a building is declared dilapidated, the municipal government has three options: 1) purchase an existing building that meets standards and resettle residents, 2) purchase housing in a building already under construction, or 3) build new housing. Although financing comes from a mix of federal, regional, and local budgets, local governments allocate and administer the funds.

Thus, the stock of bad housing is determined by both the city's ability to maintain the quality of existing housing stock and their speed in resettling residents into new or existing housing. Municipal authorities can quickly and substantially affect the stock of bad housing by making repairs to troubled housing that is in danger of becoming unlivable and by efficiently resettling residents to better quality buildings, or by speeding existing construction projects. They may also initiate new construction, but this entails a longer process.

The dependent variable in our analysis is BAD HOUSING. The Russian state statistical agency (*Rosstat*) provides data on housing in the municipality that has been categorized by the government as either troubled (*vetkhiy*) or dilapidated (*avariyniy*).[12] For our main analyses, we measure BAD HOUSING in 1000 m^2 or normalized as m^2 per capita. We also investigate results' robustness to

[11]Members often include representatives of the federal Emergency Situations ministry (MChS), the federal consumer protection bureau (*Ropotrebnadzor*), the city architecture bureau, the city council, the federal real estate registry (BTI), the city planning department, the regional branch of the Safety and Standards department(*Otdel Nadzornoi Deyatelnosti*), and local utility companies. The mayor does not serve on the commission.

[12]By the classification system, troubled (*vetkhiy*) housing evaluates as having 30% to 65% wear and requires major repairs; dilapidated (*averiyniy*) housing evaluates as above 65% wear. Both are

alternative codings such as a logarithmic scale or as a ratio of total housing stock.

Explanatory Variables

Our main independent variable of interest is POLITICAL CENTRALIZATION. This is a dummy variable that takes a value of 0 for city-years with a popularly elected mayor as of January 1, and 1 for years where the mayor is appointed per the procedure described above. In 2002, about 12% of the observations in our analyses had appointed executives; ten years later, that proportion is over 40%. Our argument predicts that, on average, replacing direct mayoral elections with subnational appointments should be detrimental to public service provision for the poor. Accordingly, we anticipate that POLITICAL CENTRALIZATION leads to an increase in bad housing stock.

We also want to guard against potential bias that could arise from omitting political, economic, and social factors relating to both local leaders' maintenance of aging housing stock and municipalities' level of political centralization. Therefore, alongside our main variable of interest, we include measures for AVERAGE INCOME, WORKING AGE POPULATION as a percent of total city population, and BIRTH RATE to account for the different housing demands and political conditions that might arise in poorer cities, locales with fewer workers, or cities with lower population growth. Likewise, we control for city population as a proportion of total regional population to control for the influence that REGIONAL PROMINENCE might have on cities' housing stock and the Kremlin's decision to maintain/remove mayoral elections. To account for subnational variation in the degree to which regions provide a political environment that might invite or prevent scrutiny of public service provision and affect the regime's attitude towards local elections, we include an ordinal measure of PRESS FREEDOM that ranges from "not free" to "somewhat free" and a measure for REGIONAL POLITICAL CLIMATE, an index of regional democracy developed by Nikolai Petrov and Alexei Titkov. In all models, we also include fixed effects for city and year in order to net out time-invariant, unobserved heterogeneity across cities and control for common shocks within a given year. Additionally, we use clustered standard errors to account for within-municipality correlations, including serial autocorrelation (Angrist & Pischke 2009), and estimate the models

considered to have adverse effects on the health and safety of building occupants.

using ordinary least squares (OLS).

To analyze the relationship between public service provision and political centralization, we use the following base model specification:

$$\text{Bad Housing}_{it} = \alpha + \beta_1 C_{it-1} + \gamma X_{it} + \theta_i + \eta_t + \varepsilon_{it}$$

where i indexes each municipality and t indexes the year; BAD HOUSING$_{it}$ is the measure of the stock of old and unsafe housing within a given city-year; C is the centralization measure indicating whether or not cities have an appointed mayor; $\mathbf{X}$ is a vector of relevant control variables; α, β, and γ are parameters to be estimated; θ and η are fixed effects parameters for municipality and year, respectively; and ε is the error term. This fixed effects specification represents a generalized difference-in-differences design where, in a given year, cities with an elected mayor serve as control group against which to compare changes in bad housing stock that may occur in cities switching from elected to appointed mayors.[13] Under the identifying assumptions of that framework, these estimates measure the effect of abandoning direct mayoral elections on the maintenance of aging housing stock. Table 1 reports the statistical results for these analyses.

Results

Do appointments under authoritarianism make local leaders less attentive to the living conditions of their poorer citizens? Table 1 supports this claim. Across all models, the coefficient estimates on POLITICAL CENTRALIZATION are positive and statistically significant, indicating that switching from elected mayors to appointed mayors is associated with an increase in bad housing. The dependent variable's coding in the first two columns makes the results straightforward to interpret. In column 1, the estimated coefficient for POLITICAL CENTRALIZATION is 20.136 ($s.e. = 8.754$), indicating that replacing mayoral elections with appointments is associated with an average increase

[13]This framework's key assumption is that observed trends in housing maintenance within cities that keep mayoral elections are the same as we would have observed in cities with appointed mayors *had they not abandoned mayoral elections.* We address this parallel trends assumption in a later section using placebo tests and entropy balancing.

Table 1: **Bad Housing Increases under Appointed Mayors**

DV: BAD HOUSING *old & unsafe housing*	1000s m^2	per capita	logged per capita	as % of total
	(1)	(2)	(3)	(4)
Political Centralization	20.136	0.068	0.038	0.489
	(8.754)	(0.025)	(0.014)	(0.184)
	.022	.006	.006	.009
Press Freedom	-13.992	-0.078	-0.041	-0.650
	(6.892)	(0.027)	(0.016)	(0.190)
	.044	.004	.011	.001
Working Age Population	0.351	0.005	0.002	0.013
	(0.327)	(0.003)	(0.001)	(0.010)
	.283	.075	.035	.218
Average Income	-0.449	-0.001	-0.001	-0.003
	(0.263)	(0.001)	(0.001)	(0.009)
	.089	.444	.089	.750
Regional Political Climate	0.094	-0.004	-0.003	-0.021
	(1.310)	(0.004)	(0.002)	(0.023)
	.943	.391	.270	.347
Birth Rate	4.058	0.003	0.000	-0.083
	(2.825)	(0.010)	(0.005)	(0.134)
	.152	.780	.990	.537
Regional Prominence	266.601	-1.463	-0.866	11.757
	(386.693)	(0.818)	(0.452)	(9.130)
	.491	.075	.057	.199
Population	0.307	-0.001	-0.001	-0.014
	(0.599)	(0.001)	(0.000)	(0.008)
	.609	.165	.155	.081
Total Housing	-0.020			
	(0.011)			
	.074			
Number of Observations	2027	2027	2027	2027
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Data on Russian mayoral appointments collected by ICSID; all economic data from *Multi-Stat*. Parameter estimates for year fixed effects and model constants not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

of just over 20,100 m^2 of unsafe housing. Given that a standard two-bedroom *khrushchyovka* apartment was designed to be 45 m^2 (484 ft^2), this estimated increase works out to an additional 447 apartments' worth of bad housing in cities that cancel mayoral elections. Similarly, column 2 reports a statistically-significant increase in the square meters of bad housing per capita after Russian cities begin appointing their mayors ($\beta = 0.068$, $s.e. = 0.025$). For the median city population of 182,000, this approximates to 275 additional unsafe apartments.

Turning briefly to the models' control variables, we see that PRESS FREEDOM and AVERAGE INCOME have negative and significant coefficient estimates, suggesting that neglected apartment buildings are less of a problem in wealthier cities and in cities where media can more easily publicize unresolved social problems. In some models, the negative coefficients for WORKING AGE POPULATION and REGIONAL PROMINENCE are also statistically different from zero, but this is inconsistent across models. The models provide no evidence of a statistical relationship between the remaining control variables and bad housing stock.

As a robustness check, columns 3 and 4 demonstrate that using logged quantities or analyzing bad housing as a share of total housing stock also produces coefficients for POLITICAL CENTRALIZATION that have the predicted positive sign and are statistically significant. In their study of these transitions to mayoral appointments, Reuter et al. (2016) argue that the Kremlin was more likely to allow mayors to retain elections if they had strong political machines to use on the regime's behalf; keeping elections was a cost-effective way for the Kremlin to gain access to local political networks without needing to build them. Controlling for this potential confounder here using Reuter et al.'s measure for these local political machines (margin of victory in the last mayoral election) produces no noteworthy changes to our findings. In the penultimate section, we devote more time to addressing concerns about factors like local political machines that might potentially drive the selection of cities for political centralization. Our results in Table 1 are also robust to dropping observations from Russia's Caucasus regions, which are often viewed as outlying regions on a number of dimensions. Finally, we also explore alternate dependent variables related to housing policy, such as the number of families resettled from bad housing and household utility costs. As

with the stock of bad housing, these other indicators reveal a discernible decline in public service provision under appointed mayors. Results for these robustness analyses appear in the appendix.

Are elected leaders punished for bad housing?

Given that voters' ability to sanction errant local leaders is an important mechanism behind our theory, these findings raise the natural follow-up question of whether Russia's elected mayors are actually punished by voters for housing infrastructure problems, as the theory implies. To test this causal mechanism, we analyze the results of mayoral elections. We model incumbent mayors' vote share in mayoral elections as the dependent variable and regress it on our bad housing variable. BAD HOUSING enters the model with a one-year lag to reduce concerns about temporal ordering.

Table 2: **Elected Mayors Lose Votes when Bad Housing is Higher**

DV: INCUMBENT VOTE SHARE *in % of total votes in mayoral election*	(1)	(2)
Bad Housing$_{t-1}$	-0.071	
	(0.030)	
	.020	
Bad Housing Per Capita$_{t-1}$		-19.714
		(9.405)
		.038
Total Housing$_{t-1}$	0.001	
	(0.004)	
	.830	
Number of Observations	284	283
Includes All Controls	✓	✓
City Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Data on Russian mayors collected by ICSID; all economic data from *MultiStat*. Model specification also includes control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, change in unemployment, city and year fixed effects, and model constant. Parameter estimates for year fixed effects and model constants not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table 2 confirms that elected officials do indeed have political incentives to deal with the problem of aging apartment buildings. Controlling for our available covariates as well as fixed effects for city and year of election, we observe negative and statistically-significant coefficients on BAD HOUSING. As expected, incumbent mayors receive fewer votes in municipalities with more bad housing. Depending on the variable's functional form, an increase of bad housing stock by one standard deviation predicts that the incumbent mayor's vote share would decline by somewhere between 14 percentage points (model 1) and 13 percentage points (model 2). These results help to bolster the notion of local elections as a sanctioning device, even in an authoritarian regime.

Additional analyses underscore that housing conditions do not appear to hold the same sway in national elections that they do in local elections. We have argued that appointed mayors need not devote as much attention to local public services because their survival in office is tied more closely to delivering votes in national elections, where concerns about other issues carry more weight than local issues. Indeed, Table A8 in the appendix demonstrates that city-level vote returns for UR in national legislative elections are uncorrelated with the stock of bad housing. In contrast to bad housing's influence on local elections, increases in bad housing do not appear to harm United Russia's vote share in national elections. This finding helps explain why the regime would be willing to tolerate appointed local officials' laxness towards housing or similar local matters: it is unlikely to lose the ruling party votes in the contests that matter most.

Together, Tables 1 and 2 and the additional analyses in the appendix provide empirical support for claims that political centralization reduces the provision of pro-poor services. First, we find that bad housing increased significantly in Russian cities that replaced mayoral elections with appointed mayor systems. Moreover, additional analyses clarify why elected mayors might care about renovating or replacing Soviet-era apartment buildings – voters hold elected mayors accountable for poorly-maintained housing during local elections. When local executives need voters to be reelected, they must remain mindful of local public service provision or else face the electoral consequences. Conversely, when local leaders are instead appointed from above, we observe that local public services receive less attention. We have argued that this shift occurs as appointed leaders

are incentivized to prioritize the goals of their political principals at higher levels of government over the demands of local citizens. In the next section, we extend the investigation to examine sub-national appointees' political incentives within an electoral authoritarian regime and how fulfilling these political imperatives affects their responsiveness to local citizens' needs.

Political Incentives under Centralization

In our theoretical discussion, we have argued that centralization diminishes responsiveness to local needs because it directs local officials' attention away from local governance and towards concerns that predominate at higher levels of government. For electoral authoritarian leaders, the paramount concern is national electoral dominance. Accordingly, we have argued that political centralization reduces local leaders' incentives to provide public services because higher-level officials will tend to overlook shortcomings in local governance as long as those local appointees can still deliver the required votes for the regime in national elections. This line of logic yields additional testable implications: 1.) the ability to deliver UR votes to the regime in national elections should prolong appointed mayors' tenure in office, and 2.) political centralization should most sharply reduce responsiveness to public service needs in places where leaders successfully demonstrate an ability to help the regime maintain national electoral control. We test both claims here. We begin by investigating whether an appointed mayor's ability to deliver votes for UR affects their reappointment chances. We then turn toward our more central concern, which is to show that satisfying the regime's political imperatives attenuates the responsiveness of appointees to local needs.

Do appointed local leaders need to provide votes to the ruling party?

If regime leaders do evaluate local appointees on the basis of their political performance – i.e., delivering votes to the ruling party in major elections – then we should observe a correlation between local UR vote share in national elections and appointed mayors' tenure in office. We test this in Table 3 by analyzing a binary indicator of APPOINTED MAYOR REPLACEMENT as a function of various measures of United Russia vote share in the most recent national Duma election, city and region covariates, and fixed effects for mayor and year. The main variable of interest in column 1 is UR VOTE DELIVERY, which is the share of United Russia's vote in the most recent national

parliamentary election from a given municipality with an appointed mayor. Columns 2 and 3 break this continuous variable into a pair of dummy variables that can compare high and low vote shares against a medium reference category: HIGH VOTE DELIVERY, which takes a value of 1 for the top quartile (62% or above) and a value of 0 otherwise, and LOW VOTE DELIVERY, which codes 1 for the bottom quartile (37% or lower) and 0 otherwise.

Table 3: **Delivering United Russia Votes Helps Keep Appointed Mayors in Office**

DV: APPOINTED MAYOR REPLACEMENT *dummy;* 1 = *replaced*	(1)	(2)
UR Vote Delivery$_{t-1}$	-0.016	
	(0.007)	
	.019	
High Vote Delivery$_{t-1}$		-0.445
		(0.188)
		.021
Low Vote Delivery$_{t-1}$		0.336
		(0.185)
		.074
Bad Housing$_{t-1}$	-0.001	-0.001
	(0.001)	(0.001)
	.217	.234
Number of Observations	234	234
Includes All Controls	✓	✓
Mayor Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Linear probability models of turnover in appointed mayors. Data on Russian mayors and UR vote share collected by ICSID; all economic data from *MultiStat*. Model specification also includes control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, change in unemployment, mayor and year fixed effects, and model constant. City-clustered standard errors in parentheses; p-values appear below standard errors.

The analyses in Table 3 indicate that appointed mayors' probability of being replaced is significantly related to delivering high vote shares for UR in national parliamentary elections. Using the continuous measure of UR vote delivery, column 1 reports the expected negative coefficient

($\beta = -0.016$, $p = .019$). Substantively, this model predicts that a one standard deviation increase in UR vote delivery (17.8) over the last parliamentary election is associated with a 28 percentage point *decrease* in the probability that an appointed mayor will be replaced. When vote delivery is binned into broad categories in model 2, we observe a similar relationship between vote delivery and appointee retention. In appointment cities that deliver high vote shares to UR, mayors have a predicted probability of being replaced that is on average 45 percentage points *lower* than appointees who deliver middling vote shares ($p = .021$). The positive coefficient estimate on LOW VOTE DELIVERY is estimated less precisely ($p = .074$), but it is consistent with expectations – mayors in the bottom quartile of UR vote delivery have a *higher* predicted probability (34 percentage points) of being replaced that mayors supplying "average" vote shares. These findings suggest that delivering high vote shares in national elections helps subnational leaders in this electoral authoritarian regime to keep their office. In contrast, these appointed leaders' tenure in office appears unrelated to public service provision.[14] Across all models, we observe no relationship between the level of bad housing stock and the probability of appointed mayors being replaced.[15]

How does fulfilling regime goals affect appointed mayors' provision of public services?

Having established appointed local leaders' incentives to perform political service to the regime, we now return to the primary question about outcomes. To examine the effects of appointed mayors' political incentives on public service delivery, we investigate how the relationship between political centralization and bad housing stock changes depending on local leaders' ability to produce votes for United Russia. Specifically, we reestimate the baseline analyses from our previous section while interacting POLITICAL CENTRALIZATION in turns with the measures of regime ser-

[14] Additional analyses in the appendix (Table A7) also provide no indication of an interaction between vote delivery and bad housing.

[15] We also investigate whether there are perverse fiscal incentives for local leaders to *want* more bad housing, e.g. to attract extra federal transfers. In the appendix, Table A9 shows no evidence of this: municipal budgets do not attract more federal transfers following increases in bad housing. Relatedly, Table A11 shows that our results are robust to controlling for the size of city budgets.

vice used in the previous analysis, UR VOTE DELIVERY and HIGH VOTE DELIVERY. By our argument's logic, appointed mayors' incentives to respond to local concerns are decreasing in their ability to supply UR with strong electoral results in nationwide elections. Thus, we expect a positive coefficient on the interaction term (i.e., bad housing stock increases more under appointed mayors who can deliver more/many votes to the regime in national elections).

Table 4 reports the results from these analyses. In the first two columns, the fixed effects specification effectively compares changes in UR vote share in the most recent election to UR vote share in the prior election within a given city; in these models, mayors' performance in delivering UR votes is *city-oriented*, relating current outcomes to past UR vote share in the same city. The remaining two columns use an alternative coding, only comparing changes in UR vote share if the same mayor is in office for both current and previous elections. Thus, these models strictly interpret performance to be *mayor-oriented* because each mayor's vote delivery is analyzed independent of their predecessors' (in)ability to deliver. The findings from both approaches support our claims that local appointees' ability to meet the regime's political goals in national elections reduces the pressure on them to attend to local concerns. In all models, the interaction terms are positive and statistically significant, indicating that political centralization is associated with larger increases in bad housing stock where local officials can also deliver higher UR vote returns in national elections. Figure 2 plots the marginal effects of centralization, conditional on UR vote delivery.

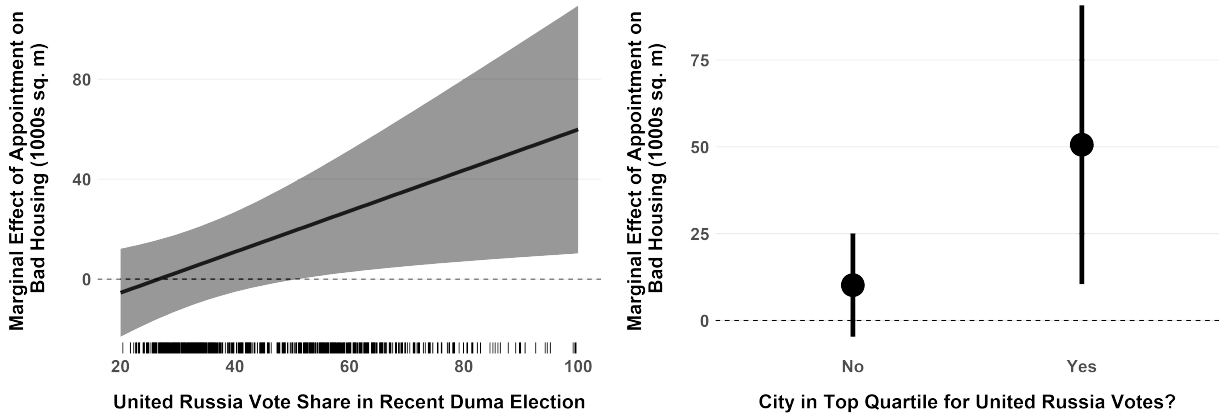
Figure 2's left panel shows results for the continuous measure of VOTE DELIVERY. At the sample median (50% UR vote share), we estimate that switching from elected to appointed mayors is associated with an average increase of 19,000 m^2 of old and unsafe housing. This effect grows, however, with larger UR vote shares. At 67% UR vote share (+1 SD), the increase in bad housing is an estimated 33,000 m^2 – approximately 730 two-bedroom apartments worth of ill-maintained housing stock. In contrast, at lower levels of UR vote share, there is no statistically significant difference in bad housing stock between cities with elected versus appointed mayors. The right panel in Figure 2 tells a similar story if we instead use the dichotomous measure to compare centralization's effects across places where local leaders have HIGH VOTE DELIVERY (i.e., in the top

Table 4: **Bad Housing Increases More under Appointed Mayors Who Can Deliver Strong Electoral Results for United Russia**

DV: BAD HOUSING *old & unsafe housing (1000s m^2)*	reference for mayors' vote delivery			
	by city		*by mayor*	
	(1)	(2)	(3)	(4)
Political Centralization	-21.783	10.174	-22.848	16.913
	(14.129)	(7.616)	(20.449)	(10.076)
	.125	.183	.265	.095
UR Vote Delivery$_{t-1}$	-0.339		-0.317	
	(0.336)		(0.440)	
	.315		.472	
Centralization	0.817		1.045	
$\times$ Vote Delivery$_{t-1}$	(0.360)		(0.590)	
	.024		.078	
High Vote Delivery$_{t-1}$		-10.769		-15.130
		(8.482)		(9.938)
		.206		.129
Centralization		40.428		50.857
$\times$ High Vote Delivery$_{t-1}$		(16.274)		(26.631)
		.014		.058
Total Housing	-0.025	-0.025	-0.032	-0.033
	(0.018)	(0.018)	(0.022)	(0.022)
	.179	.171	.150	.138
Number of Observations	1652	1652	1181	1181
Includes All Controls	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of cities' stock of old and unsafe housing. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. All models also include control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, city and year fixed effects, and model constant; parameter estimates presented in the online appendix to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Figure 2: **Marginal Effects of Mayoral Appointments on Bad Housing Stock, Conditional on Delivery of United Russia Votes**



Note: Left panel based on model 1 in Table 4 (continuous measure of UR vote delivery). Right panel based on model 2 (dichotomous measure of UR vote delivery). Bands represent 95% confidence intervals.

quartile) versus where vote delivery is low or medium. With high vote delivery, political centralization is associated with a large, positive increase in bad housing stock (50,600 m^2). Without high vote delivery, the marginal effect is still positive, but more modest (10,200 m^2) and with confidence intervals that overlap zero. These estimated effects are statistically different ($p = 0.014$).

Taken together, these results provide empirical support for our theoretical argument. In doing so, they help clarify why replacing local elections in an electoral authoritarian regime with appointments can undermine public service provision. Whatever local governance's salience to regime longevity, it seems that national electoral performance remains regime leaders' overriding political priority, and local appointees who can convincingly help achieve that goal can prolong their time in office regardless of their performance on other dimensions. Thus, under an appointment system, as local leaders become more secure in their ability to satisfy the regime's national electoral needs, they have fewer career incentives to provide public services to local citizens. The analyses also provide suggestive evidence that this moral hazard problem diminishes in the absence of convincing electoral support. Where parliamentary elections yield less impressive results for UR, appointed officials have less capital to spend with their political superiors and thus cannot

as easily afford to ignore key local issues such as housing policy.

Addressing Threats to Inference

Unfortunately, the processes that lead some cities to abandon local elections are unlikely to have been random. Therefore, we must investigate the possibility that our findings reflect some unmodeled differences that prompted the cancellation of local elections in the first place. To bias our conditional findings, these omitted confounders would need to affect the maintenance of housing stock differentially, depending on UR's vote share in Duma elections. For example, perhaps regional officials wanted reforms in some cities specifically because their elected mayors were beginning to let local infrastructure deteriorate. If this happened primarily in UR electoral strongholds, then we might observe similar patterns to those presented here. In this section, we take a multi-pronged approach to investigate whether such selection into treatment drives our findings.

We begin by comparing appointment cities' pre-reform years with those cities that retain elected mayors by the sample's end. Statistical tests show that, before reforms take place, both sets of cities look remarkably similar on key variables.[16] We see no evidence that pre-appointment cities had on average more bad housing, measured either in percent of total housing ($p = 0.79$) or in per capita terms ($p = 0.39$). Neither does it appear that pre-appointment cities differed greatly in their ability to deliver votes to United Russia in the 2003, 2007, or 2011 national parliamentary elections. Finally, both groups have similar records of economic performance, as measured by unemployment in levels ($p = 0.48$) or changes ($p = 0.82$). Although far from exhaustive, these tests help to rule out the most likely pathways through which selection might bias our results.

Secondly, to guard against nonrandom selection operating through less likely pathways, we employ entropy balancing to further reduce any potentially meaningful differences across the two types of cities. Entropy balancing is a technique that uses iterative algorithms to specify weights so that treatment group (cities that get appointments) and control group (cities that never get appointments) have identical moments on observable characteristics in the pretreatment period (Hainmueller 2012). Thus, to the extent that regime leaders during the pre-reform period select

[16]The full table of difference of means tests is available in Table A12 of the online appendix.

cities for centralization based on political and economic differences that we can observe in the data, entropy balancing allows us to reweight control observations such that these differences become ignorable. In other words, if the underlying concern is that our results may have biased by systematic differences between cities that get appointments and cities that keep elections, then imposing parity via entropy balancing can remove those differences and provide accurate estimates of the relationship between local leaders' incentives and public service provision. Accordingly, we use this method to find the set of weights for each control city such that the control and treatment groups have identical means for pretreatment levels of bad housing and also a broad range of time-varying and time-invariant covariates: press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, city population, unemployment rates, change in unemployment, the strength of mayors' political machines, regions' status as republic, whether located in the Caucasus region, ethnic Russians' share of region's population, and historical strength of civil society.[17] The first two columns of Table 5 show that our main results are robust to this procedure. Adjusting for observable pre-reform differences via entropy balancing yields estimates that are substantively similar to our previous findings.

Third, in addition to controlling for selection on observables, we conduct an additional placebo test looking for evidence that our findings are driven by *unobserved* confounders that determine both cities' elected/appointed status as well as housing stock quality. For our placebo, we replace our CENTRALIZATION measure with PRE-CENTRALIZATION, a time-invariant dummy indicator equaling 1 for cities that have appointed mayors by the end of the dataset, then restrict the sample to city-years with elected mayors (i.e., the indicator equals 1 in the *pre-reform* years of cities that eventually switch to appointments).[18] Without the interaction term, this specification inves-

[17]Because entropy balancing forces balance across observed pretreatment traits, it also helps to ensure the parallel trends assumption for our difference-in-differences setup (Truex 2014).

[18]Since PRE-CENTRALIZATION is time-invariant, we drop city fixed effects and instead include additional time-invariant control variables: regions' status as a republic, ethnic Russians as percent of regional population, historical strength of civil society, and a dummy of Caucasus region.

Table 5: **Additional Tests Provide No Support for Plausible Rival Explanations**

DV: BAD HOUSING *old & unsafe housing in 1000s* m^2	entropy balancing		placebo test	
	(1)	(2)	(3)	(4)
Political Centralization	19.508	-31.316		
	(9.757)	(16.800)		
	.047	.064		
UR Vote Delivery$_{t-1}$		-0.653		1.253
		(0.552)		(1.569)
		.239		.426
Centralization		0.985		
× Vote Delivery$_{t-1}$		(0.392)		
		.013		
Pre-Centralization			-22.515	-8.629
			(28.201)	(57.064)
			.426	.880
Pre-Centralization				-0.043
× Vote Delivery$_{t-1}$				(0.930)
				.963
Number of Observations	1985	1617	1421	1100
Includes All Controls	✓	✓	✓	✓
City Fixed Effects	✓	✓		
Time-Invariant Controls			✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of cities' stock of old and unsafe housing. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. Estimates in columns (1) and (2) use entropy balancing to reweight control observations (cities that retain mayoral elections) to match covariate distributions of appointment cities during the pretreatment period. Columns (3) and (4) test for differences across cities that retain mayoral elections and the pre-reform period of cities that eventually move to an appointment system. All models also include control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, city and year fixed effects, and model constant. In addition, columns (3) and (4) include time-invariant controls for regions' status as republic, whether the city is in the Caucasus region, ethnic Russians' share of region's population, and historical strength of civil society. Parameter estimates not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

tigates whether housing outcomes in cities that never cancel elections are different from housing outcomes in the appointment cities *during the years preceding reform*. With the interaction term, it tests for pre-reform differences in the relationship between UR vote share and bad housing stock among cities in the placebo versus control group. Since no observations have actually introduced an appointment system, statistically-significant coefficients would indicate that some heretofore unidentified heterogeneity across cities – and not actual appointment incentives – produces our main findings. Table 5's final two columns displays the results of these tests. As anticipated by the argument, we find no significant relationship between bad housing stock and pre-reform status.[19]

Finally, sensitivity analyses provide an alternative tool for assessing our results' robustness to potentially omitted factors that might determine both cities' institutional reform and the condition of aging housing stock. Using the sensitivity analysis suggested by VanderWeele (2011), we find that in order to reduce the estimated effects from Table 1 or Table 4 to zero, the unmodeled confounders would have to be both: 1.) very highly correlated with bad housing levels (i.e., with a magnitude comparable to roughly twice the estimated effects of press freedom decreasing from max to min), and 2.) overwhelmingly more prevalent in cities that end up with appointed mayors compared to those that retain elected mayors (i.e., 90% vs. 40%). The improbability that such large and distinct differences across cities would go unobserved strengthens our confidence in these results. Results for this analyses appear in the appendix's Table A14.

Conclusion

Social scientists have long taken interest in how democratic institutions affect policy outcomes that affect society's least affluent. This paper demonstrates that, even under autocracy, local-level

[19]These results also bolster confidence in the parallel trends assumption of our difference-in-differences framework. The counterfactual in our analysis assumes that the observed relationship between UR vote share and public service provision in cities with mayoral elections is the same we would have observed in cities with appointed mayors *had they retained mayoral elections.* Thus, we should *not* see differences in bad housing stock's relationship with UR vote delivery between cities that keep mayoral elections and appointment cities *before they remove mayoral elections*.

elections can improve local governments' responsiveness to the poor. Examining the incremental elimination of mayoral elections across Russia's medium and large cities from 2002-2012, we find that switching from elected to appointed mayors led to an increase in dilapidated housing stock. We also find empirical evidence of this electoral mechanism at work: in cities with mayoral elections, incumbent mayors lose votes at reelection time if they allow bad housing to rise.

Yet, elected officials' incentives are only half the story of how political centralization affects local public service provision. A key theoretical contribution of this research is the insight that local appointees under *electoral* autocracy have little incentive to focus on providing local public goods; the regime's desire to maintain national electoral dominance shifts local appointees' incentives away from good governance to providing political services in the form of votes for the ruling party in national elections. The data provide compelling support for these claims. We demonstrate that appointed mayors are more likely to stay in office when they can deliver high vote shares for United Russia. Crucially, we also find evidence that appointees' ability to perform this vital regime service conditions their responsiveness to local needs: appointees who can deliver high UR vote shares in national elections appear to do a much worse job of maintaining their cities' aging housing stock.

By highlighting appointees' political incentives, this paper helps explain why existing research on local elections can yield contradictory findings. Just as the varying quality of elections should affect leaders' attentiveness to the plight of their voters (Beazer 2015), appointed leaders' career incentives can either motivate or undercut good governance. Our study shows that incentivizing subnational appointees to mobilize votes for the ruling party in national elections can give rise to a perverse myopia. It seems clear that political centralization in Russia was undertaken with an eye towards increasing the Kremlin's influence over local politics. Yet, this study suggests that this centralization has come at the cost of eroding living conditions for the poor.

One remaining question is how our findings travel. On one hand, some particulars of the Russian case – elected local officials serving simultaneously alongside appointed local officials – are rare.[20] And yet, our findings speak directly to governance in all autocracies where local officials

[20]Although other examples exist. Prominently, Chinese village elections were phased in grad-

make policy. We have argued that appointed subnational officials in electoral autocracies are likely to focus on vote mobilization at the expense of good governance. This argument is relevant to any electoral autocracy where appointed local officials are tapped to mobilize votes for the regime.[21] Regimes without multi-party elections – such as China – do not suffer from this particular myopia. Of course, in non-electoral regimes, leaders may privilege other types of political services such that good local governance does not become an important performance criteria in those settings either. One prominent threat to all autocrats – electoral and nonelectoral alike – is the threat of coups and schisms. This threat is best confronted by the security services, not local officials, so new research might focus there. Likewise, subnational officials are often tasked to help control protest and protest. Future research could examine how quelling mass protest and ensuring political stability helps regional leaders avoid sanction for poor governance.

There is little cross-national research on these questions, but the perspective we offer is consistent with the findings on economic performance in the appointments literature in China and Russia. In single-party China, where subnational officials do not need to mobilize votes for the regime, most studies find that economic performance indicators are used in the evaluation of local officials (e.g. Li and Zhou 2005, Landry et al. 2018). In Russia, where local officials must mobilize votes, there is very little evidence that economic performance plays an important role in appointments (e.g. Rochlitz et al. 2015). Future research could profit by conducting more cross-national research and identifying how other types of political imperatives might displace performance-based appointments.

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ually over the course of the 1980s. In Malaysia, local council elections were cancelled in some municipalities the late 1960s, but retained for a short time in other municipalities (Tennan 1973)

[21]Leon and Wantchekon (2019) and Mares and Young (2016) discuss how local officials are natural vote brokers in decentralized systems. Aside from Russia, this phenomenon has been studied by scholars in several electoral autocracies, including Venezuela (Albertus 2015), Egypt (Blaydes 2011), the Philippines (1997) and Ukraine (Matsuzato 2001)

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Appendix for "Do Authoritarian Elections Help the Poor? Evidence from Russian Cities"

This appendix contains additional analyses that are not reported fully in the main text.

Appendix Tables

Appendix Figures

Appendix Discussion

Table A1: **Summary Statistics: Russian Municipalities Dataset (2002-2012)**

Variable	N	Mean	Std. Dev.	Min, Max
Bad Housing	2,168	135.67	205.18	[0.04, 2515]
Bad Housing Per Capita	2,148	0.56	0.65	[0.0002, 3.60]
Political Centralization	2,163	0.25	0.44	[0, 1]
United Russia Vote Share	2,017	48.04	17.08	[20.43, 99.61]
Press Freedom	2,176	2.02	0.69	[1, 3.5]
Working Pop.	2,147	63.90	3.75	[0.06, 89.74]
Average Income	2,229	8.46	5.18	[1.72, 120.92]
Regional Political Climate	2,271	30.46	6.17	[14, 46]
Birth Rate	2,130	11.25	2.56	[3.6, 31.6]
Regional Prominence	2,192	0.17	0.15	[0.001, 0.66]
Population	2,253	280.65	275.20	[0.3, 1523.80]

Note: Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*.

Table A2: **Robustness Check: Dropping Outliers and Controlling for Electoral Margin in Past Mayoral Elections**

DV: Bad Housing	1000s m^2		per capita	
old & unsafe housing	(1)	(2)	(3)	(4)
Political Centralization	18.251	16.782	0.067	0.077
	(9.713)	(6.087)	(0.030)	(0.024)
	.062	.006	.029	.002
Press Freedom	-10.878	-6.878	-0.056	-0.040
	(7.571)	(5.557)	(0.029)	(0.030)
	.153	.217	.057	.182
Working Age Population	0.077	0.521	0.011	0.005
	(1.159)	(0.357)	(0.008)	(0.003)
	.947	.146	.195	.061
Average Income	-0.441	-0.609	-0.001	-0.002
	(0.320)	(0.222)	(0.001)	(0.001)
	.171	.007	.310	.123
Regional Political Climate	-0.336	0.011	-0.007	-0.002
	(1.443)	(1.338)	(0.004)	(0.004)
	.816	.993	.122	.650
Birth Rate	4.546	-0.619	0.015	-0.008
	(2.772)	(2.670)	(0.010)	(0.012)
	.103	.817	.134	.512
Regional Prominence	130.224	-197.378	-0.468	-2.967
	(494.823)	(374.687)	(1.327)	(0.738)
	.793	.599	.725	.000
Population	-0.279	0.352	-0.001	0.000
	(0.464)	(0.471)	(0.001)	(0.001)
	.549	.456	.135	.727
Margin of Victory	-63.674		-0.111	
	(33.368)		(0.064)	
	.058		.086	
Number of Observations	1678	1699	1678	1699
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Excluding Caucasus Cities		✓		✓

Note: Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. Columns 1 and 3 demonstrate robustness to controlling for the strength of local political machines by controlling for the margin of victory in the city's last mayoral election. Columns 2 and 4 demonstrate robustness to dropping cities in the Caucasus region. Parameter estimates for year fixed effects and model constants not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A3: **Alternative Dependent Variable: Resettling Families Waitlisted for New Housing**

DV: Families Resettled	*% of wait list*	*# of resettled per capita*
	(1)	(2)
Political Centralization	-0.792	-0.397
	(0.403)	(0.203)
	.051	.052
Press Freedom	0.635	0.079
	(0.398)	(0.178)
	.112	.659
Working Age Population	-0.021	-0.135
	(0.023)	(0.120)
	.366	.261
Average Income	-0.050	-0.336
	(0.054)	(0.148)
	.356	.024
Regional Political Climate	0.119	0.014
	(0.088)	(0.066)
	.178	.828
Birth Rate	0.231	-0.195
	(0.170)	(0.124)
	.177	.117
Regional Prominence	11.091	-58.834
	(17.032)	(20.081)
	.516	.004
Population	-0.048	0.026
	(0.019)	(0.008)
	.013	.002
Number of Observations	2024	890
City Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. Parameter estimates for these additional model components not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A4: **Additional Support from Household Survey Panel: Change to Appointed Mayors Followed by Increased Utility Costs**

DV: Respondents' Utility Bill *3 month average (in rubles)*	utility costs	unpaid utility bills
	(1)	(2)
Political Centralization	297.871	218.056
	(80.346)	(61.214)
	0.0002	0.0004
Press Freedom	-58.221	-60.720
	(92.569)	(62.608)
	0.529	0.332
Working Age Population	-7.970	0.140
	(15.941)	(12.800)
	0.617	0.991
Average Income	128.939	107.483
	(50.204)	(42.785)
	0.010	0.012
Regional Political Climate	1.683	2.502
	(9.654)	(7.131)
	0.862	0.726
Birth Rate	-94.534	-73.249
	(30.296)	(25.200)
	0.002	0.004
Regional Prominence	-11400	-11600
	(6534.728)	(4617.012)
	0.080	0.012
Population	6.915	3.523
	(3.237)	(2.546)
	0.033	0.166
Number of Observations	19443	20596
Includes Household Controls	✓	✓
City Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Analyses using panel data on household utility costs (in rubles) from the Russian Longitudinal Monitoring Survey (RLMS) during the years 2003-2012. All economic data from *MultiStat*. Models also include household-level controls: number of family members, respondent gender, age, whether respondent own or rent their housing. Parameter estimates for these additional model components not presented in table to save space. Household-clustered standard errors in parentheses; p-values appear below standard errors.

Table A5: **Full Results from Table 2: Elected Mayors Lose Votes when Bad Housing is Higher**

DV: Incumbent Vote Share *in % of total votes in mayoral election*	(1)	(2)
Bad Housing$_{t-1}$	-0.071	
	(0.030)	
	.020	
Bad Housing Per Capita$_{t-1}$		-19.714
		(9.405)
		.038
Press Freedom	-6.835	-7.715
	(4.121)	(4.215)
	.099	.069
Working Age Population	-0.039	-0.094
	(2.104)	(2.113)
	.985	.965
Average Income	1.105	1.022
	(1.368)	(1.389)
	.420	.463
Regional Political Climate	1.381	1.299
	(0.828)	(0.843)
	.098	.126
Birth Rate	0.987	1.077
	(3.541)	(3.463)
	.781	.756
Regional Prominence	207.514	201.783
	(324.497)	(327.471)
	.523	.539
Population	-0.028	0.003
	(0.244)	(0.196)
	.909	.989
Total Housing$_{t-1}$	0.001	
	(0.004)	
	.830	
Number of Observations	284	283
City Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Data on Russian mayors collected by ICSID; all economic data from *MultiStat*. Parameter estimates for city and year fixed effects and model constant are not presented to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A6: **Full Results from Table 3: Delivering United Russia Votes Helps Keep Appointed Mayors in Office**

DV: Appointed Mayor Replacement *dummy; 1 = replaced*	(1)	(2)
UR Vote Delivery$_{t-1}$	-0.016	
	(0.007)	
	.019	
High Vote Delivery$_{t-1}$		-0.445
		(0.188)
		.021
Low Vote Delivery$_{t-1}$		0.336
		(0.185)
		.074
Bad Housing$_{t-1}$	-0.001	-0.001
	(0.001)	(0.001)
	.217	.234
Press Freedom	0.082	0.067
	(0.112)	(0.117)
	.464	.569
Working Age Population	0.058	0.056
	(0.051)	(0.049)
	.265	.264
Average Income	0.038	0.041
	(0.073)	(0.074)
	.599	.582
Regional Political Climate	0.045	0.049
	(0.048)	(0.049)
	.346	.322
Birth Rate	0.131	0.133
	(0.060)	(0.060)
	.031	.031
Regional Prominence	13.627	12.994
	(9.814)	(10.247)
	.170	.209
Population	-0.008	-0.009
	(0.009)	(0.009)
	.402	.338
Total Housing$_{t-1}$	0.0000	0.0001
	(0.0002)	(0.0002)
	.816	.539
Number of Observations	234	234
Mayor Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Linear probability models of turnover in appointed mayors. Data on Russian mayors and UR vote share collected by ICSID; all economic data from *MultiStat*. Parameter estimates for city and year fixed effects and model constant are not presented to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A7: **Additional Analysis: Examining Interaction between Vote Delivery and Bad Housing for Appointed Mayors**

DV: Appointed Mayor Replacement *dummy; 1 = replaced*	(1)	(2)
UR Vote Delivery$_{t-1}$	-0.017	
	(0.008)	
	0.037	
Bad Housing$_{t-1}$	-0.001	-0.0004
	(0.001)	(0.001)
	0.340	0.614
UR Vote Delivery$_{t-1}$	0.000	
× Bad Housing$_{t-1}$	(0.000)	
	0.792	
High Vote Delivery$_{t-1}$		-0.354
		(0.239)
		0.143
High Vote Delivery$_{t-1}$		-0.001
× Bad Housing$_{t-1}$		(0.001)
		0.477
Low Vote Delivery$_{t-1}$		0.400
		(0.194)
		0.043
Low Vote Delivery$_{t-1}$		-0.001
× Bad Housing$_{t-1}$		(0.001)
		0.437
Number of Observations	234	234
Includes All Controls	✓	✓
Mayor Fixed Effects	✓	✓
Year Fixed Effects	✓	✓

Note: Linear probability models of turnover in appointed mayors. Data on Russian mayors and UR vote share collected by ICSID; all economic data from *MultiStat*. Model specification also includes control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, change in unemployment, mayor and year fixed effects, and model constant. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A8: **Local Increases in Bad Housing Do Not Harm United Russia in National Elections**

DV: United Russia Vote Share *% votes from city; Duma elections 2003, 2007, 2011*	(1)	(2)	(3)	(4)
Bad Housing$_{t-1}$	0.001	0.003		
	(0.004)	(0.004)		
	.738	.514		
Bad Housing$_{t-1}$		-0.010		
× Centralization$_{t-1}$		(0.011)		
		.334		
Bad Housing Per Capita$_{t-1}$			-1.299	-1.190
			(1.782)	(1.915)
			.467	.535
Bad Housing Per Capita$_{t-1}$				-0.036
× Centralization$_{t-1}$				(3.461)
				.992
Political Centralization$_{t-1}$		1.582		0.446
		(1.642)		(1.776)
		.337		.802
Number of Observations	559	549	556	547
Includes All Controls	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of vote share from a municipality for United Russia in State Duma elections. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. All models also include control variables for press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, city and year fixed effects, and model constant; parameter estimates not presented to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A9: **Municipal Budgets Do Not Increase from Having More Bad Housing**

DV: Federal Transfers to City Budget *in constant rubles*	logged (1)	logged (2)	per 1000 residents (3)	per 1000 residents (4)
Bad Housing (1000 m^2)	0.00003	0.116		
	(0.0003)	(0.122)		
	.916	.342		
Bad Housing m^2 Per Capita			0.042	35.238
			(0.092)	(34.100)
			.651	.303
Total Housing	0.00006	0.035		
	(0.00004)	(0.035)		
	.118	.317		
Press Freedom	0.028	-9.327	0.030	-8.019
	(0.032)	(10.007)	(0.032)	(8.761)
	.386	.352	.349	.361
Working Age Population	-0.003	-12.424	-0.003	-12.461
	(0.013)	(12.487)	(0.013)	(12.543)
	.814	.321	.821	.322
Average Income	0.004	1.715	0.004	1.700
	(0.002)	(2.163)	(0.002)	(2.137)
	.011	.429	.009	.427
Regional Political Climate	0.022	2.718	0.022	2.579
	(0.009)	(3.038)	(0.009)	(2.818)
	.022	.372	.022	.361
Birth Rate	-0.013	-7.859	-0.009	-5.136
	(0.017)	(8.327)	(0.017)	(5.637)
	.464	.347	.613	.363
Regional Prominence	2.494	488.040	2.081	275.306
	(2.563)	(610.872)	(2.522)	(446.344)
	.332	.425	.410	.538
Population	-0.001	-0.735	0.001	0.515
	(0.002)	(0.732)	(0.001)	(0.561)
	.473	.317	.543	.360
Number of Observations	1367	1367	1367	1367
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of vote share from a municipality for United Russia in State Duma elections. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. All models also include city and year fixed effects and a model constant; parameter estimates not presented to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A10: **Full Results from Table 4: Bad Housing Increases More under Appointed Mayors Who Can Deliver Strong Electoral Results for United Russia**

DV: Bad Housing *old & unsafe housing (1000s m^2)*	reference for mayors' vote delivery			
	by city		*by mayor*	
	(1)	(2)	(3)	(4)
Political Centralization	-21.783	10.174	-22.848	16.913
	(14.129)	(7.616)	(20.449)	(10.076)
	.125	.183	.265	.095
UR Vote Delivery$_{t-1}$	-0.339		-0.317	
	(0.336)		(0.440)	
	.315		.472	
Centralization	0.817		1.045	
× Vote Delivery$_{t-1}$	(0.360)		(0.590)	
	.024		.078	
High Vote Delivery$_{t-1}$		-10.769		-15.130
		(8.482)		(9.938)
		.206		.129
Centralization		40.428		50.857
× High Vote Delivery$_{t-1}$		(16.274)		(26.631)
		.014		.058
Total Housing	-0.025	-0.025	-0.032	-0.033
	(0.018)	(0.018)	(0.022)	(0.022)
	.179	.171	.150	.138
Press Freedom	-11.283	-11.242	-10.316	-10.299
	(6.212)	(6.177)	(6.439)	(6.332)
	.071	.070	.111	.105
Working Age Population	0.206	0.078	-0.310	-0.554
	(0.804)	(0.820)	(0.956)	(1.019)
	.798	.924	.746	.587
Average Income	-0.628	-0.634	1.996	1.747
	(0.218)	(0.216)	(1.926)	(1.870)
	.004	.004	.301	.351
Regional Political Climate	-0.896	-1.018	0.545	0.202
	(1.969)	(1.941)	(2.626)	(2.654)
	.650	.600	.836	.940
Birth Rate	2.861	2.964	2.698	2.833
	(2.389)	(2.258)	(2.835)	(2.826)
	.232	.191	.342	.317
Regional Prominence	46.495	51.968	-12.394	-4.976
	(462.530)	(459.465)	(948.643)	(925.818)
	.920	.910	.990	.996
Population	0.653	0.662	0.694	0.724
	(0.940)	(0.930)	(1.216)	(1.192)
	.488	.478	.569	.544
Number of Observations	1652	1652	1181	1181
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of cities' stock of old and unsafe housing. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. Parameter estimates for city and year fixed effects and model constant not presented to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A11: **Robustness Check: Controlling for Municipal Revenues & Social Unrest**

DV: Bad Housing *old & unsafe housing (1000s* m^2*)*	(1)	(2)	(3)	(4)
Political Centralization	-25.531	0.606	-23.908	0.017
	(9.829)	(5.006)	(8.377)	(3.970)
	.010	.904	.005	.997
UR Vote Delivery$_{t-1}$	-0.077		-0.257	
	(0.218)		(0.190)	
	.726		.178	
Centralization	0.621		0.564	
× Vote Delivery$_{t-1}$	(0.180)		(0.155)	
	.001		.0001	
High Vote Delivery$_{t-1}$		-3.192		-5.235
		(4.372)		(4.181)
		.466		.212
Centralization		22.723		20.785
× High Vote Delivery$_{t-1}$		(6.584)		(5.267)
		.001		.0001
Municipal Revenue$_{t-1}$ *millions of rubles, logged*	0.021	0.112		
	(2.620)	(2.555)		
	.994	.965		
Regional Social Unrest *number of protest events*			0.206	0.203
			(0.149)	(0.149)
			.170	.174
Number of Observations	1004	1004	1123	1123
Includes All Controls	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Selected coefficient estimates from linear regression models of cities' stock of old and unsafe housing. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. All models also include control variables for total housing stock, press freedom, working age population, average income, regional political climate, birthrate, cities' share of regional population, city and year fixed effects, and model constant; parameter estimates presented in the online appendix to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A12: **Additional Analysis: Comparing Pre-Reform Differences in Russian Municipalities**

	Group Means			
	Non-appointment	Pre-appointment	n_1, n_2	p-value
Bad Housing / Total Housing *percent*	2.93 (0.11)	2.88 (0.18)	(1031, 425)	0.79
Bad Housing Per Capita *in* m^2 *per person*	0.59 (0.02)	0.56 (0.03)	(1031, 423)	0.39
UR Vote 2003 *UR's vote share in Duma election years*	34.38 (0.92)	34.13 (1.22)	(93, 65)	0.87
UR Vote 2007 *UR's vote share in Duma election years*	60.46 (0.85)	58.71 (1.17)	(111, 43)	0.26
UR Vote 2011 *UR's vote share in Duma election years*	41.87 (1.50)	36.25 (2.56)	(112, 11)	0.25
Unemployment *unemployment rate (%)*	1.38 (0.03)	1.42 (0.04)	(1001, 430)	0.48
ΔUnemployment *difference from previous year*	-0.004 (0.02)	-0.005 (0.02)	(988, 424)	0.82

Note: Tests compare group means between cities that retain mayoral elections (non-appointment) and the pre-reform period of cities that eventually move to an appointment system (pre-appointment). Standard errors in parentheses below group means; p-values are two-tailed.

Table A13: **Full Results from Table 5: Additional Tests Provide No Support for Plausible Rival Explanations**

DV: Bad Housing *old & unsafe housing in 1000s* m^2	entropy balancing		placebo test	
	(1)	(2)	(3)	(4)
Political Centralization	19.508	-31.316		
	(9.757)	(16.800)		
	.047	.064		
UR Vote Delivery$_{t-1}$		-0.653		1.253
		(0.552)		(1.569)
		.239		.426
Centralization		0.985		
× Vote Delivery$_{t-1}$		(0.392)		
		.013		
Pre-Centralization			-22.515	-8.629
			(28.201)	(57.064)
			.426	.880
Pre-Centralization				-0.043
× Vote Delivery$_{t-1}$				(0.930)
				.963
Total Housing	-0.032	-0.037	-0.021	-0.021
	(0.019)	(0.025)	(0.010)	(0.011)
	.094	.140	.033	.056
Press Freedom	-4.870	-0.973	-49.333	-46.248
	(5.807)	(6.193)	(18.723)	(19.774)
	.403	.875	.009	.020
Working Age Population	0.048	-2.818	-5.848	-6.019
	(0.299)	(2.913)	(4.647)	(4.953)
	.872	.335	.210	.226
Average Income	-0.475	-0.662	-1.174	-1.408
	(0.303)	(0.261)	(1.417)	(1.368)
	.118	.012	.409	.305
Regional Political Climate	0.078	-1.416	1.667	2.440
	(1.988)	(2.827)	(2.543)	(2.200)
	.969	.617	.513	.269
Birth Rate	9.035	8.282	17.987	15.160
	(6.316)	(7.200)	(8.579)	(8.894)
	.154	.251	.037	.090
Regional Prominence	1269.761	652.109	217.536	258.831
	(1058.596)	(1553.709)	(178.181)	(177.126)
	.232	.675	.224	.146
Population	0.475	0.958	0.714	0.721
	(0.695)	(1.370)	(0.220)	(0.255)
	.495	.486	.001	.005
Number of Observations	1985	1617	1421	1100
City Fixed Effects	✓	✓		
Time-Invariant Controls			✓	✓
Year Fixed Effects	✓	✓	✓	✓

Note: Linear regression models of cities' stock of old and unsafe housing. Data on Russian mayoral appointments collected by ICSID; all economic data from *MultiStat*. Columns (3) and (4) include time-invariant controls for regions' status as republic, whether the city is located in the Caucasus region, ethnic Russians' share of region's population, and historical strength of civil society. Parameter estimates not presented in table to save space. City-clustered standard errors in parentheses; p-values appear below standard errors.

Table A14: **Sensitivity Analysis: Estimates of Appointments on Bad Housing, Depending on Assumptions about Unobserved Confounders**

	γ					
δ	0.5	1	5	10	30	50
0.1	20.05	20.0	19.6	19.1	17.1	15.1
0.3	19.95	19.8	18.6	17.1	11.1	5.1
0.5	19.85	19.6	17.6	15.1	5.1	-4.9

γ = the effect size of some latent factor(s) U on bad housing
δ = the difference in prevalence of U in appointment vs. election cities.

Discussion

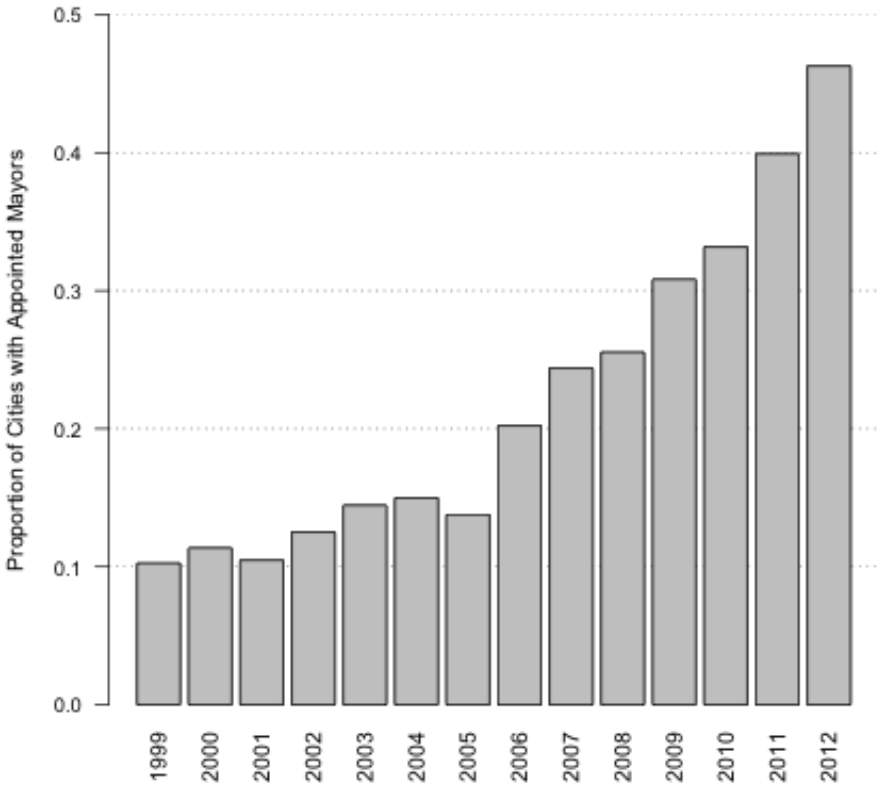
In addition to the robustness checks and placebo tests described in this main text, sensitivity analyses represent a complementary tool for the robustness of established results to potential bias arising from unobserved factors. As opposed to ruling out inferential threats, sensitivity analyses help to clarify and quantify how pervasive and powerful latent differences would need to be in order to overturn a given result.

We follow the method described in VanderWeele (2011) and investigate our results' sensitivity to unobserved confounders with varying combinations of two traits. First, we vary assumptions about the size of the latent factors' effect on bad housing in a given city (γ). Independently, we also vary assumptions about the difference in the prevalence of these latent factors within election versus appointment cities (δ).

This analysis assesses the sensitivity of the estimated relationship between increases in bad housing stock and political centralization. Based on the estimates from the first column of Table 1, the paper reports that replacing mayoral elections with mayoral appointments is associated with an average increase of roughly 20,136 m^2 of old and unsafe housing (β= 20.136).

In order to reduce these estimated effects to zero, the results above indicate that unmodeled confounders would need to be both highly correlated with bad housing ($\gamma \geqslant 50$ – a magnitude comparable to roughly twice the estimated effects of press freedom decreasing from max to min) and overwhelmingly more prevalent in cities that end up with appointed mayors compared to those that retain elected mayors (i.e., $\delta = 0.5$ is consistent with the factor being present in 90% of appointed cities vs. 40% of elected cities). It is improbable that such important and distinct differences across cities would go unnoticed. This strengthens our confidence in the results.

Figure A1: **Adoption of Mayoral Appointments Over Time**



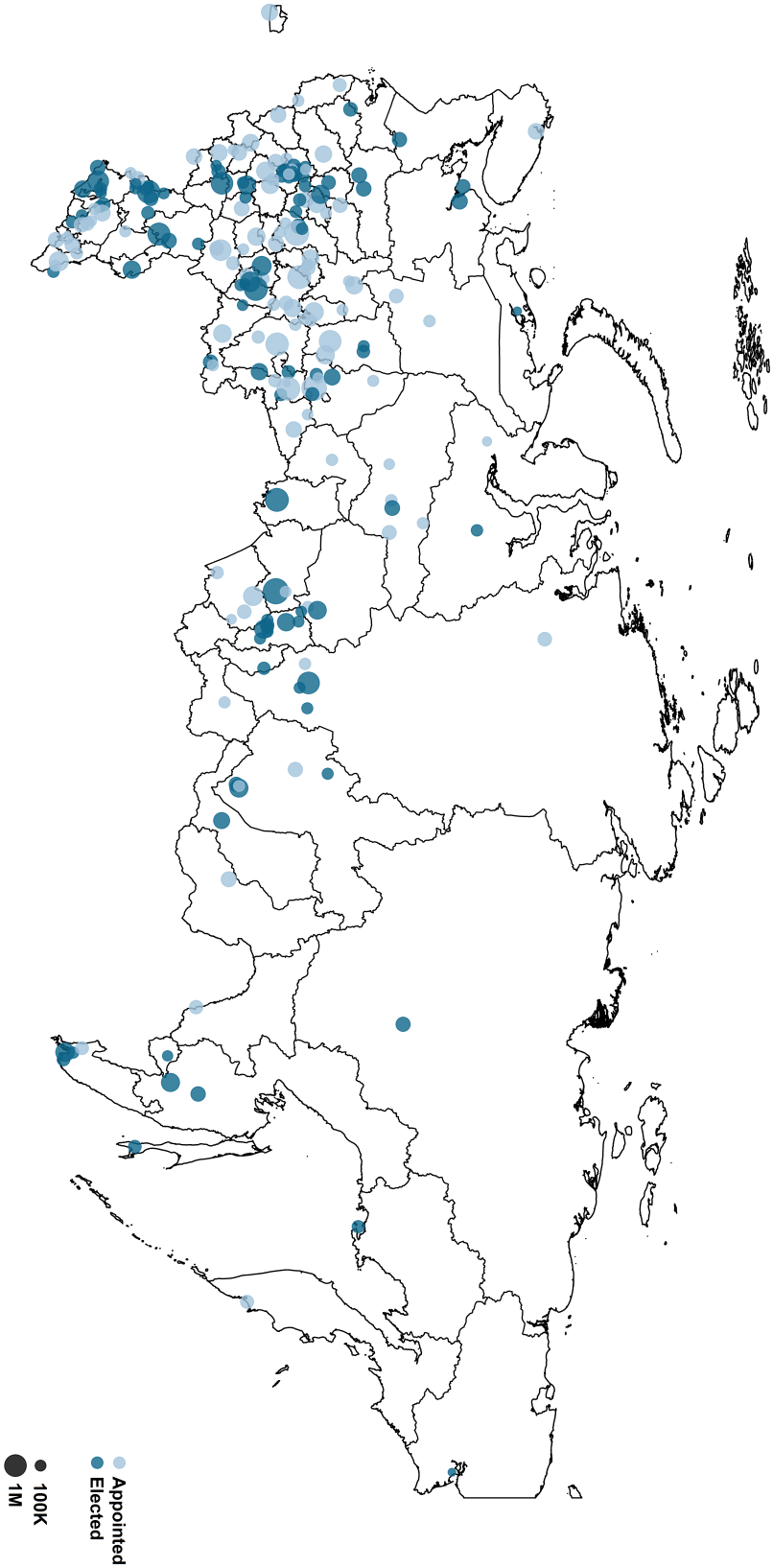
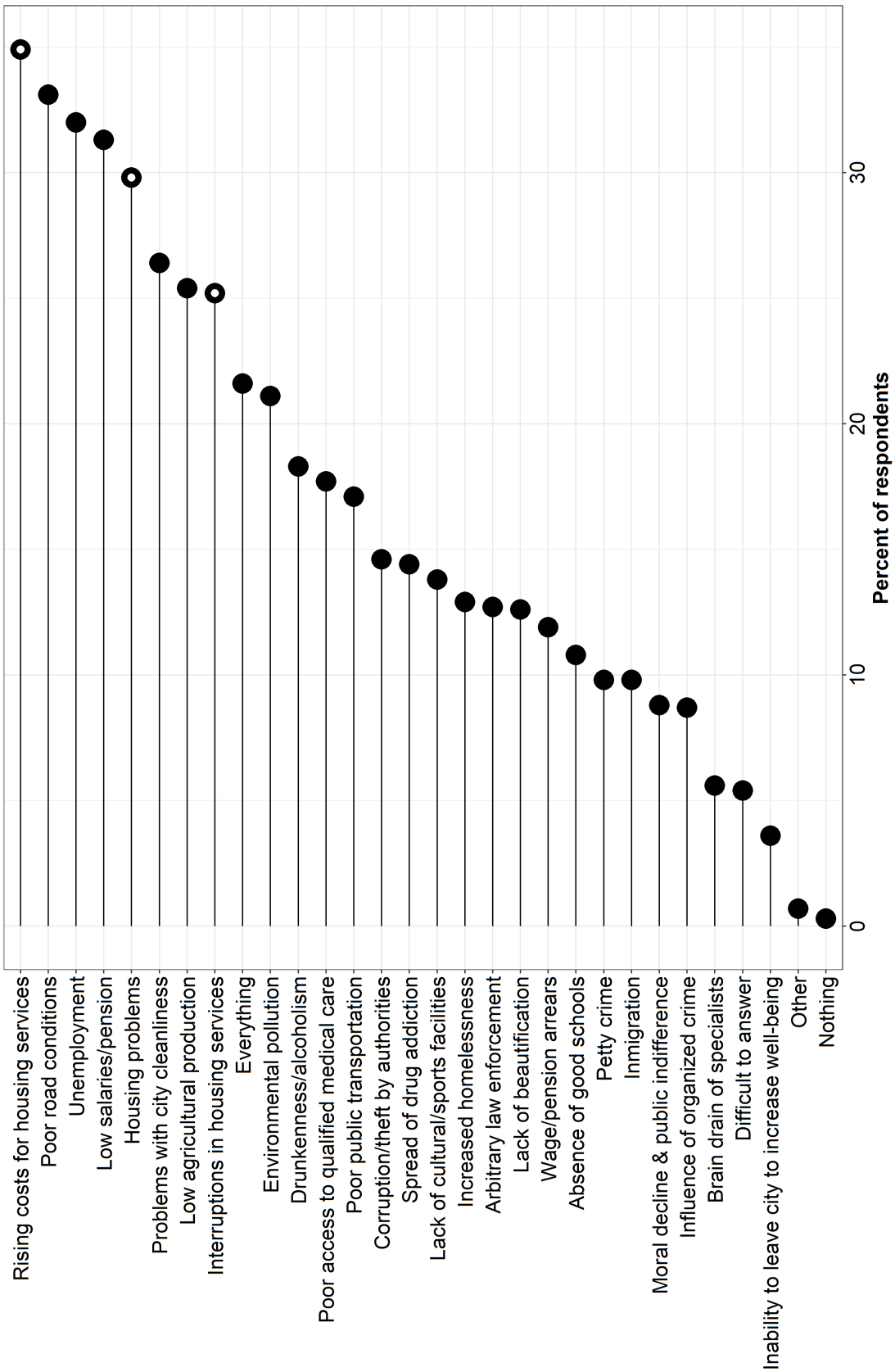


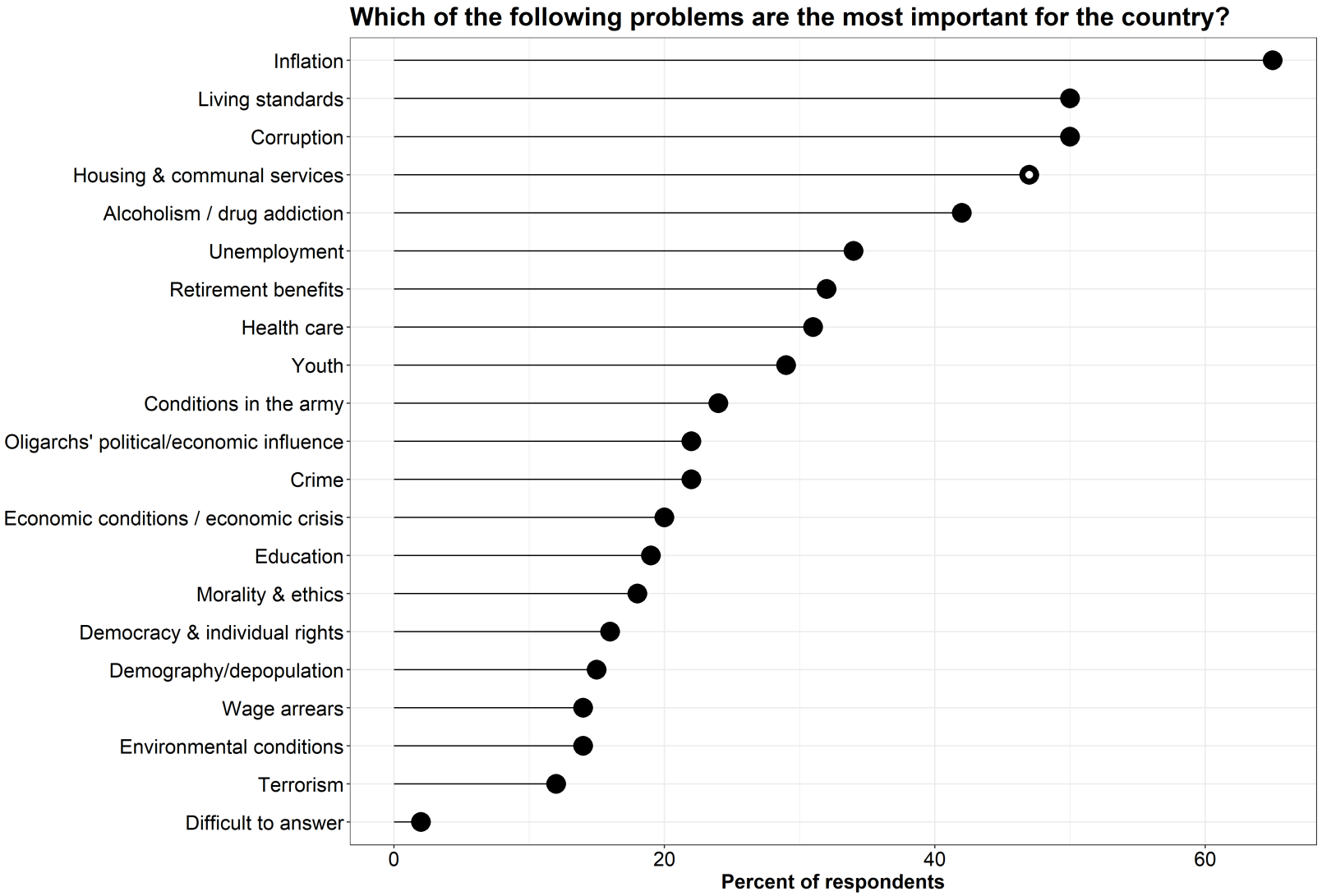
Figure A2: Geographic Variation in the Distribution of Appointed Mayors, 2002-2012

Figure A3: **Survey Evidence: Russians See Housing as a Pressing Problem for Local Governments (2008)**



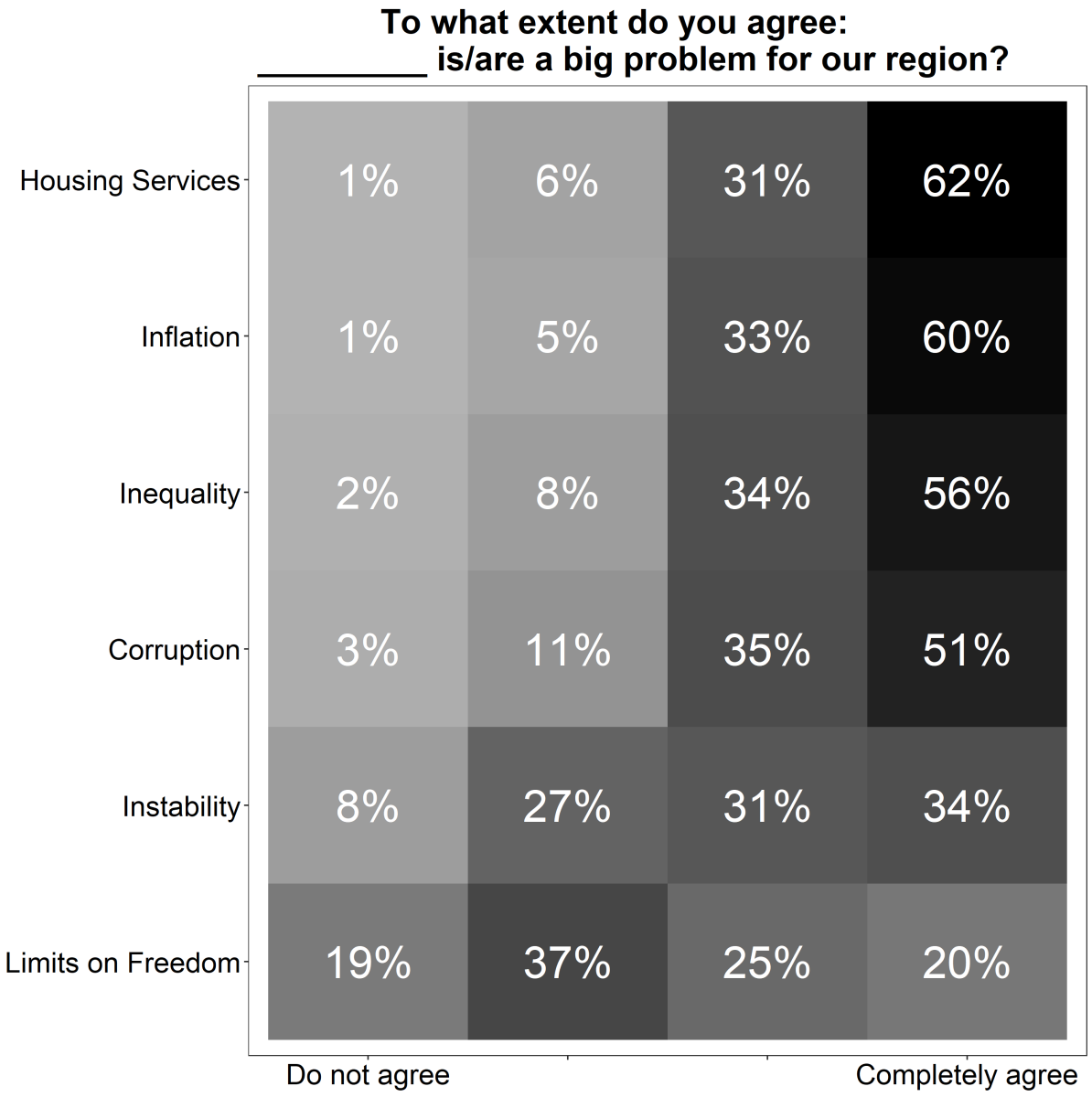
Note: Data from Center for Research on Civil Society and the Nonprofit Sector (Higher School of Economics) 2008; 1600 respondents, nationally-representative sample.

Figure A4: **Survey Evidence: Housing Issues Perceived as a Major Problem in the Country (2011)**



Note: Data from VTsIOM, 2011; nationally-representative sample.

Figure A5: **Survey Evidence: Housing Issues Perceived as a Major Problem in Russia's Regions (2014)**



Note: Data from TMF, 2014. 4273 respondents in 20 regions, not nationally representative. Oversample of employed and employed in heavy industry.

Description of Aggregating District-Level Electoral Results to City-Level Data

One of our key independent variables is United Russias vote share in cities during federal parliamentary elections. Unfortunately, the raw data released by the Central Election Commission is not (usually) aggregated at the level of the city. The highest level of aggregation available from the CEC is the Territorial Election Commission (Territorialnaya Elektoralnaya Kommissia or TIK, hereafter). Each TIK contains between 10 and 362 precincts (the median was 44 in the 2011 Duma elections). TIKs usually, but not always, correspond roughly to municipal districts (*munitsipal'niye okruga*) and city districts (*gorodskiye okruga*). For smaller cities – i.e. those with populations less than 150,000there is only one TIK per city. The task of calculating United Russias vote share at the city level is straightforward for these cities. For larger cities, however, there are multiple TIKs per city.

In order to calculate the the share of the vote for United Russia in cities with multiple TIKs, we (and our research assistants) identified the TIKs that were located in each large cities. We then summed the number of votes for UR across theses TIKS and divided it by the number of valid ballots. For most large cities, it is straightforward to identify the TIKs that are located in each city because they are assigned names that identify them as part of the city. For example, the seven TIKs located in Chelyabinsk for the 2007 State Duma elections were called: Cheljabinsk, Central'naja, Cheljabinsk, Kalininskaja, Cheljabinsk, Kurchatovskaja, Cheljabinsk, Leninskaja, Cheljabinsk, Metallurgicheskaja, Cheljabinsk, Sovetskaja, and Cheljabinsk, Traktorozavodskaja.

For a handful of cities, however, the names of TIKs are not so easily associated with a particular city. For example in the regional elections in Volgogradskaya Oblast in 2003 the TIKs associated with Volgograd city were named: Traktorzavodkaya, Krasnoktyabrskii, Dzerzhinski, Voroshilovskaya, Tsentralnaya, Kirovskaya, Sovetskaya, and Krasnoarmeiskaya. In those cities where the names of TIKs are not obviously associated with a given city, we consulted the archived websites of regional election commissions (using web.archive.org) and regional media sources.

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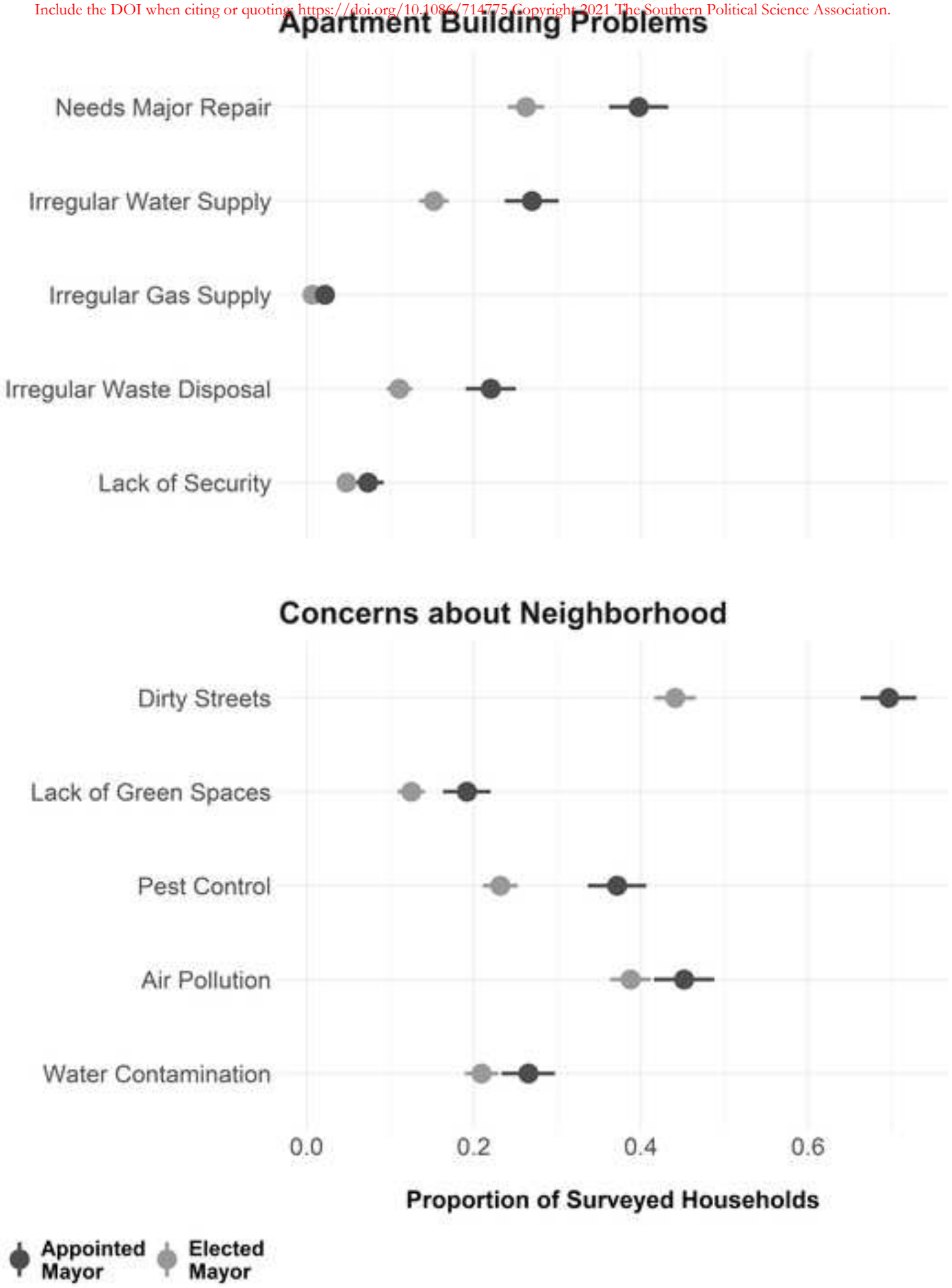

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