

Distributive Consequences of Democratic Backsliding: Evidence from the Turkish Mass Housing Administration (TOKİ)*

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Abstract

How does democratic backsliding reshape the distribution of public goods? We develop a model in which backsliding provides an authoritarian-leaning incumbent with political slack to arbitrarily distribute resources by making elections less competitive via extra-electoral means. Focusing on the Turkish Mass Housing Administration (TOKİ), a centralized provider of social housing and infrastructure, we assemble a dataset by scraping 3,193 public procurement tenders, covering the construction of 748,468 housing units across Türkiye's 973 districts between 2004-2020. Following local elections, we show the ruling Justice and Development Party (AKP) has systematically channeled TOKİ contracts toward districts governed by AKP mayors. Targeted allocation has intensified in parallel with the erosion of democratic institutions. Effect sizes are largest in districts won against the Republican People's Party (CHP), the main opposition. Accordingly, as elections have become *de facto* less competitive, we also find allocation has shifted from a swing district logic towards rewarding less competitive districts pre-post backsliding, consistent with model predictions. We confirm this using a close election regression discontinuity design.

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Democracy is like a tram. You ride it until you arrive at your destination, then you step off.

– Recep Tayyip Erdoğan, President of Türkiye¹

1 Introduction

One of the largest contemporary debates in comparative politics concerns the ongoing recession of democratic institutions across several corners of the planet. One strand of literature has paid close attention to the drivers of backsliding, from executive aggrandizement to strategic electoral manipulation by elites (Bermeo 2016; Carothers and Hartnett 2024). Another subset of work has been dedicated to measurement, studying the degree to which certain country contexts can truly be classified as “backsliders” (Little and Meng 2024a; Little and Meng 2024b). However, less attention has been paid, both theoretically and empirically, to understanding how democratic backsliding affects material outcomes, such as the allocation of public goods and services.

This paper argues that democratic backsliding reconfigures distributive politics by changing the logic of public investment. Under competitive democracy, electoral uncertainty and institutional constraints limit overtly partisan allocation, encouraging incumbents to distribute public resources according to broad electoral or needs-based considerations rather than exclusively rewarding regime loyalists (Besley and Persson 2011; Bueno de Mesquita et al. 2003). While some districts may receive favorable attention, this will remain limited to a handful of highly responsive (i.e. “swing”) constituencies with tight electoral margins (Lindbeck and Weibull 1987; Persson and Tabellini 2000). However, as elections become *de facto* less free, fair and hence competitive under backsliding, and as constraints on executive discretion weaken, this generates political slack for the incumbent to arbitrarily distribute resources. Public investment subsequently tilts in favor of regime supporters, not by excluding opposition areas altogether, but by systematically distorting allocation away from a competitive democratic benchmark (Levitsky and Way 2010; Magaloni 2006).

In this paper, we study the distributive consequences of backsliding in Türkiye, a widely regarded case of democratic erosion. Focusing on the Turkish Mass Housing Administration (TOKİ), a centralized public procurement vehicle attached to the prime minister’s office, we show that after the onset of backsliding, TOKİ has consistently awarded large-scale social infrastructure contracts to districts electing municipal mayors from the ruling Justice and Development Party (AKP). We further show the intensity of targeted allocation has broadly increased over time in tandem with the piecemeal erosion of institutions, with null effects during the height of Turkish democracy, before backsliding commenced. We also find largest effect sizes among districts that switch away from the Republican People’s Party (CHP) as the main opposition. As inferred by our theoretical expectations, strategies of distribution have also evolved vis-à-vis the institutional slide, with the

1. Interview with newspaper Milliyet, June 14th 1996 (Cerrahoğlu 1996).

AKP switching from a classic swing motivated logic (e.g. [Lindbeck and Weibull 1987](#)) under its early experience with democracy in the 2000s towards rewarding less competitive districts under backsliding.

Türkiye’s experience under the AKP and Recep Tayyip Erdoğan provides an apposite setting for studying how backsliding reshapes distributive politics. Widely regarded as a canonical backslider, Türkiye has combined democratic erosion with the politicization of public procurement ([Diamond 2015](#); [Esen and Gumuscu 2016](#)).² Although the landmark 2002 Public Procurement Law (Law No. 4734) sought to increase transparency and depoliticize spending, subsequent AKP amendments have re-centralized control, weakened competitive bidding, and expanded discretionary procurement ([Gürakar and Bircan 2019](#); [Adiguzel, Kaba, and Koyuncu 2025](#)). This reversal has ultimately increased the government’s ability to channel infrastructure and housing projects for political purposes, echoing similar patterns of institutional erosion and politicized public spending among other backsliding contexts, such as Hungary under Viktor Orbán and Serbia under Aleksandar Vučić ([Caruana Galizia et al. 2026](#); [Vladisavljev 2023](#)).

Building on canonical accounts of distributive politics (e.g. [Dixit and Londregan 1996](#); [Persson and Tabellini 2000](#)), we develop a reduced-form model linking multi-district allocation to institutional erosion. We posit backsliding operates through two channels: it compresses electoral uncertainty beyond policy proposals – such as via electoral manipulation and restricted media access, among other tactics ([Bermeo 2016](#); [Schedler 2002](#)) –, and sharpens the “targeting technology” of the state budget toward districts aligned with the incumbent.³ Under democracy, allocation largely tracks a per-capita benchmark: while a few highly responsive districts with tighter margins may receive more expenditure, the difference in *mean* spending between aligned and non-aligned districts is approximately zero. However, under backsliding, these forces tilt expenditures toward aligned districts, generating a widening allocation gap. Whereas some theories of autocratic distribution emphasize exclusionary allocation or even the withdrawal of resources ([Albertus, Fenner, and Slater 2018](#); [Burgess et al. 2015](#); [Magaloni 2006](#)), our model instead implies broad but *distorted* distribution: spending remains widespread but tilts away from the per-capita benchmark in favor of aligned districts. This reflects the logic of electoral/competitive authoritarianism, where incumbents must still appeal to a broad cross-section of society but do so unevenly ([Levitsky and Way 2010](#); [Schedler 2002](#)). Figure 1 illustrates this pattern: under democracy, TOKİ contract values track district populations with mild concavity regardless of partisan alignment; under backsliding, allocation tilts clearly toward AKP-governed districts, especially populous ones.

2. Appendix Figure A.1 conveys this expedited erosion of institutions using the first principal component of relevant democracy indicators from the Varieties of Democracy (V-Dem) dataset.

3. Uncertainty compression reflects [Schedler \(2002\)](#)’s observation: “by placing... elections under tight authoritarian controls [electoral authoritarian regimes] try to cement their continued hold on power. Their dream is to reap the fruits of electoral legitimacy without running the risks of democratic uncertainty” (p. 36-37).

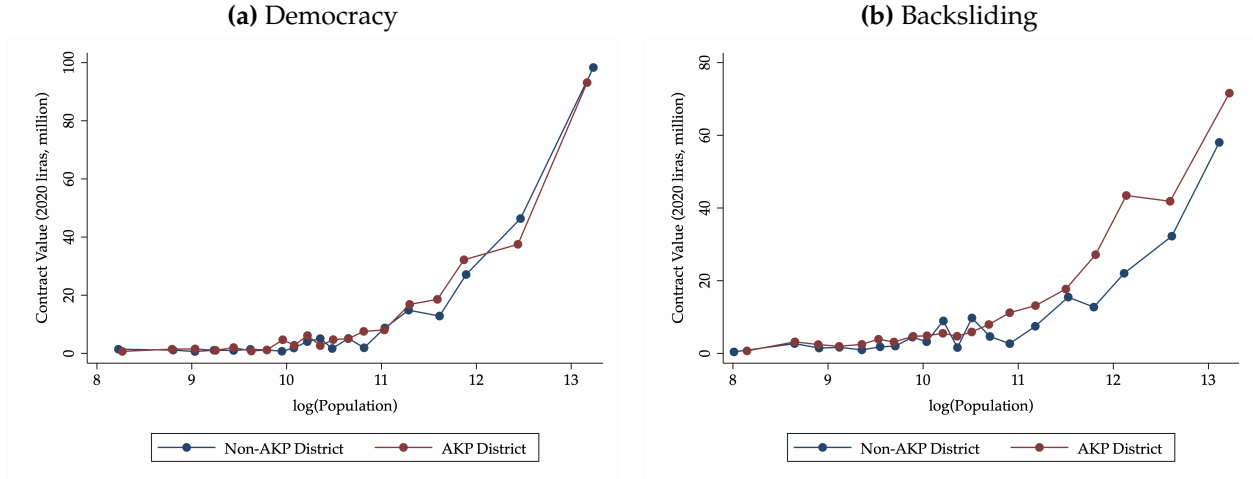


Figure 1: Contract Allocation and Population Distribution by Regime

Note: estimated using binscatter. Sample for democracy uses all contracts and district-level population distributions before 2011. Sample for backsliding uses all contracts and district-level population distributions after and including 2011. Year fixed effects are absorbed. Dots refer to vigintile bins. Maroon dots outline districts governed by an AKP mayor. Blue dots outline districts governed by a non-AKP mayor.

In taking the theory to the data, we assemble our own dataset on TOKİ public procurement expenditures by scraping 3,193 tenders crossing Türkiye’s 973 districts between 2004-2020. This provides information on project type, estimates of capital expenditure amounts from winning tender bids, in addition to the number of planned houses for construction among contracts dedicated to social housing, of which our data covers tender values for 748,468 housing units. Given TOKİ projects are a highly localized public good, frequently featuring on the websites of districts which are governed by mayors, we match this tender dataset to Türkiye’s local election results covering the elections in 2004, 2009, 2014 and 2019 (with the former being the first local election the AKP competed in). Our identification strategy exploits the fact that, despite overt violations under backsliding, local elections in Türkiye remain reasonably competitive. This provides variation in electoral outcomes to estimate differences in awarded social infrastructure contracts pre-post AKP victories, identified through opposition-to-AKP switches and AKP re-election.

As [Acemoğlu and Üçer \(2020\)](#) claim, the “institutional slide accelerated during AK Party’s third term that began in June 2011” (p. 31). Using a standard two-way fixed effects (TWFE) framework alongside Poisson quasi-maximum likelihood (QMLE) models, interacting AKP mayoral victories with the onset of democratic backsliding in 2011, we show that districts with newly incumbent AKP mayors under backsliding receive TOKİ contracts that are 46.4% more valuable per capita than districts with non-AKP incumbents. On the extensive margin, this also translates into a 4.2 percentage point increased probability of receiving contracts from TOKİ, approximately a quarter of the overall sample average, in addition to an 84.5% probability of receiving additional housing units within a contract. This effect is not only significant but also quantitatively meaningful.

When scaled to the average district population, and relative to the baseline mean contract value among opposition districts, totaling ₺11.3 million in 2020 prices (USD 1.6 million), our core estimate suggests districts with AKP mayors receive contracts that are greater in value by ₺5.2 million in 2020 prices (circa USD 740,000).

Strikingly, and as implied by our theoretical model, our interaction effects provide estimates that are essentially equal and opposite in sign to the impact of local AKP mayoral victories under backsliding. This suggests democratic institutions effectively quelled the selective allocation of TOKİ contracts to AKP-affiliated mayors, consistent with other evidence on the moderating impact of democracy on arbitrary distribution (Burgess et al. 2015). As an alternative approach, relying on Liu, Wang, and Xu (2024)’s counterfactual imputation estimator for dynamic treatment effects, we estimate average treatment effects on the treated as a (continuous) function of calendar year. We find evidence that, in expectation, favoritism has mostly grown smoothly over time as Türkiye has gradually eroded democratic institutions and altered public procurement laws, with significant effects appearing *only* after backsliding commences post-2010.

Additional to serving as a reduced-form framework, the theoretical model permits estimating *counterfactual* allocations, such that a democratic incumbent allocates TOKİ expenditures using a population-weighted rule (as suggested by panel (a) of Figure 1). Simulating counterfactual expenditures using this allocation rule, we show that the post-2010 rise in TOKİ spending among AKP-affiliated districts substantially exceeds what would largely be predicted by demographic need.⁴ The difference between observed and counterfactual expenditures suggests that by 2020, districts with AKP mayors receive *three times* more TOKİ expenditure per capita relative to a democratic allocation benchmark, on average, whereas non-AKP governed districts have largely hovered close to a population-weighted counterfactual. The political distortions from backsliding on infrastructure allocation are thus substantial.

To better understand the nature of the “targeting technology” from our theoretical model, we first explore heterogeneity regarding incumbent turnover. We unpack our TWFE and Poisson QMLE results by specific party switches to the AKP, including re-election and the AKP’s first victories that occurred mostly in their first local election of 2004. We show that most of the core result is driven by switches from the CHP, the main opposition, to the AKP, followed by seats where the AKP were re-elected. This pattern is consistent with a distributive logic in which newly captured opposition strongholds become especially valuable targets for partisan consolidation and reward under backsliding. Although, noticeably, these effects were again moderated during the period of democratic institutions, only materializing once the country commences backsliding post-2010.

Second, to probe potential shifts in electoral logics, we unpack heterogeneity vis-à-vis vote margins. As implied by our reduced-form model, although the difference in mean allocation between

4. This exercise is very similar in spirit to Burgess et al. (2015), albeit for housing and social infrastructure as opposed to roads.

aligned and non-aligned districts should be approximately zero under democracy, there may still remain a handful of competitive districts that are highly responsive to public expenditures on the margin. Unpacking AKP mayoral victories by margin size pre-post the onset of backsliding, the exercise reveals a distinct pattern: at the height of Turkish democracy at the turn of the 21st century, standard concerns regarding democratic electoral politics strongly predicted distribution, whereby swing districts received greater allocation. However, as the country has democratically eroded, with elections becoming less free, fair and hence competitive, targeting has shifted towards districts with stronger vote margins towards the AKP.

As a final source of identification, we thus rely on the fact that competitive vote margins only mattered during Türkiye's early period under democracy to run a "coin-flip" election regression discontinuity design (Lee 2008; Eggers et al. 2015). We find that highly competitive districts received greater TOKİ allocation on both the intensive and extensive margin, but only following the AKP's first local election contest in 2004. All other elections – 2009, 2014 and 2019 – that comprise a post-election window under the period of backsliding reveal consistent null effects. This provides further evidence, exploiting an alternative source of identification, that when Türkiye was a stronger democracy in the early 2000s, targeted distribution was still evident, albeit driven by classic tight-margin electoral considerations for a small number of highly competitive districts. However, in line with the institutional slide, the strategy of distribution has shifted, rewarding districts aligned with the AKP in less competitive areas (Magaloni 2006; Díaz-Cayeros, Estévez, and Magaloni 2016).

Our primary contribution is to the literature on democratic backsliding. While much of this work has focused on the drivers of institutional erosion (Bermeo 2016; Carothers and Hartnett 2024) and its measurement (Little and Meng 2024a), we turn to its distributive consequences. Theoretically, we show that backsliding regimes exhibit a distinctive distributive signature, different from both the per-capita benchmark of democratic allocation and patterns of distribution under more consolidated authoritarian or hegemonic-party regimes (Albertus, Fenner, and Slater 2018; Magaloni 2006). In this context, allocation remains broad, but tilts systematically toward aligned constituencies. This reflects the underlying logic of competitive authoritarianism, whereby incumbents must still appeal to a broad electorate but do so on an uneven playing field (Levitsky and Way 2010; Schedler 2002). Empirically, we document when targeted allocation materializes alongside institutional erosion, who is most affected, and how the underlying electoral logic governing distribution evolves as elections become *de facto* less competitive.

This paper further contributes to the well-established literature on distributive politics (Albertus 2024; Golden and Min 2013). In political economy, one strand of literature has studied how allocation of public goods and services varies across institutions within the same country context, such that democracy tilts provision of government coffers towards collective public goods as opposed to private transfers under autocracy (Besley and Persson 2011; Bueno de Mesquita et al.

2003; Burgess et al. 2015; Hodler and Raschky 2014).⁵ A separate branch of literature homes in on the electoral forces *within* democracies that predict allocation of public resources, such as whether parties should target “swing” versus “core” voters as vote-maximizing strategies (Cox and McCubbins 1986; Dixit and Londregan 1996; Lindbeck and Weibull 1987). We provide a theoretical model and evidence that connects these two strands of literature, outlining how strategies of distribution are *dynamic* and can shift as a result of institutional change.

Finally, we contribute to the literature on distributive politics in 21st century Türkiye (Adiguzel, Cansunar, and Corekcioglu 2023; Adiguzel, Kaba, and Koyuncu 2025; Akbulut-Yuksel, Okoye, and Turan 2024; Aytaç 2014; Bircan and Saka 2021; Luca and Rodríguez-Pose 2015). We are not the first to study the allocation of TOKİ projects and public procurement in Türkiye. Marschall, Aydoğan, and Bulut (2016) argue that TOKİ expenditure increases AKP support in local elections. Gürakar (2016) and Gürakar and Bircan (2019) also show that politically connected firms to the AKP are more likely to receive TOKİ contracts relative to firms linked to opposition parties. Given municipalities are the largest procurement entities in Türkiye, we build on previous efforts by addressing existing endogeneity concerns, exploiting incumbency turnover across a range of empirical strategies. This helps address the issue that TOKİ allocation and local alignment with the AKP are simply driven by correlated unobservables. Substantively, we also more directly show how the nature of targeting public goods and services has evolved in Türkiye vis-à-vis the regression towards authoritarianism, especially given prior work has mostly focused on allocation before the institutional slide occurred in 2011.

The rest of the paper is organized as follows. In section 2 we provide some institutional background behind Türkiye’s trend towards authoritarianism and the knock-on effects this has had on TOKİ and public procurement. Section 4 outlines our data collection process and core empirical strategies. Section 5 provides the core analysis, alongside various robustness checks. Section 6 unpacks heterogeneous treatment effects and probes plausible mechanisms. Section 7 presents the results from the coin-flip election regression discontinuity design. Section 8 concludes.

2 Background

2.1 The AKP’s Rise to Power and Growth

The AKP emerged onto the Turkish political scene in the 2002 general election. The first term of the AKP is often regarded as a success with increasing productivity, institutional improvements, and “high-quality growth” (Acemoğlu and Üçer 2020). This was motivated by IMF-supported pro-market reforms, including the Public Finance and Debt Management Law of 2002 and the Public Financial Management and Control Law of 2003, which substantially enhanced fiscal transparency

5. For example, contributions such as Burgess et al. (2015) show that democracy quells selective distribution of transport infrastructure investments to co-ethnics relative to periods of unconstrained authoritarianism. See Gerring, Knutsen, and Berge (2022) for review of democracy and outcomes.

and accountability (Acemoğlu and Üçer 2020). The Public Procurement Law (No. 4734) was also a landmark piece of legislation ratified in January 2002, “to make public spending more efficient and transparent and to depoliticize the procurement process” (Gürakar and Bircan 2019, p. 237). This was bolstered by the Public Procurement Authority to oversee procurement as an independent regulator.

The AKP’s increased hold on power subsequently led to the deterioration of the institutional reforms from the early 2000s, eventually ending the high-quality growth period of 2002-2006 (Acemoğlu and Üçer 2020). A key manifestation of this surrounded gradual amendments to the Public Procurement Law itself, which was amended almost two-hundred times since its inception, substantially altering the transparency of government-business transactions (Gürakar and Bircan 2019). Growth over the 2010s has thus largely been driven by the construction sector instead of improvements in technology and high employment.

2.2 TOKİ and Public Procurement

Founded in 1984, Türkiye’s Mass Housing Administration (TOKİ) aims to provide affordable housing to citizens and acts as a lever to accelerate urbanization. Following a 2003 presidential decree (No. 25348), TOKİ has been directly connected to the prime minister’s office, and is thus a heavily centralized, state-run vehicle of public procurement (Demirors 2023).⁶ TOKİ subsequently tenders projects to private contractors, often granting development rights on public land under revenue-sharing schemes, to deliver vast housing complexes and related urban infrastructure.

An amendment in 2003 (No. 4964) made the housing function of TOKİ exempt from several initial restrictions under the Public Procurement Law. In 2011, this exemption remit was expanded via another amendment (No. 6111) to include all construction projects undertaken by TOKİ.⁷ Thus, aside from housing projects, TOKİ also build shopping centers, public parks, government buildings, stadiums, among a variety of other conspicuous developments. Table 1 outlines the kinds of projects TOKİ plan and undertake.

As per the multitude of amendments made to the Public Procurement Law by the AKP, this ever-increasing set of exceptions have decreased the regulatory impediments on business engagements involving the central government. Informal networks between pro-AKP local businessmen and public officials have thus come to dominate procurement, in line with decreased transparency and accountability to an independent regulator. Gürakar and Bircan (2019) show that firms directly linked to the AKP receive 42% of all TOKİ contracts, while those firms linked to opposition parties receive only 6%. This highlights the electoral/competitive authoritarian tendencies of the AKP that have emerged vis-à-vis the country’s politico-economic institutional changes: “using its

6. TOKİ was later transferred to the Ministry of Environment and Urbanization under Law No. 703, after Turkey’s 2018 transition to a presidential system and the abolition of the prime ministership.

7. See Doğru (2016) for review of amendments.

access to state resources, the [AKP] has rewarded its supporters and punished or isolated its enemies” (Esen and Gumuscu 2016, p. 1590). Members of the main opposition party, the CHP, have also accused the government of misusing its authority over TOKİ and planning housing in areas where the AKP had relatively more votes (Lıcalı 2014).

Additional to politically connected firms (Faccio 2006), local elections remain one of the most important political determinants of TOKİ allocation (Marschall, Aydogan, and Bulut 2016). This is because local elections determine municipal mayors, who have seen increased autonomy in local government-business relations, such as the ability to take out loans, set municipal budgets and approve construction and building permits (Adiguzel, Kaba, and Koyuncu 2025; Bircan and Saka 2021; Gürakar and Bircan 2019). Mayors can also lobby Ankara for TOKİ projects, of which several developments frequently feature on the websites of municipalities (Haliliye Belediyesi 2021). Thus, while TOKİ is a centrally controlled vehicle, its allocation of social infrastructure is strongly shaped by local electoral dynamics, making municipal elections a critical determinant additional to firm-level political connections.

3 Model: Distributive Politics Under Backsliding

3.1 Setup

Consider a polity consisting of districts $d \in \mathcal{D}$, where each district contains n_d voters. Define $\sigma \in \mathbb{R}^+$, capturing political institutions, where $\sigma = 0$ denotes full democracy and $\sigma > 0$ denotes increasing autocratization (Besley and Persson 2011; Burgess et al. 2015). Unlike Dixit and Londregan (1996), which focuses on vote-maximization within a single-member district, we study competition in a multi-district setting, where the maximand is the probability of winning a given seat (Cox 2012). There are two parties: incumbent A and challenger C . Each party $k \in \{A, C\}$ proposes a vector of public goods expenditures $\{T_d^k\}_{d \in \mathcal{D}}$, subject to an aggregate budget constraint, $T = \sum_d T_d^k$. Voter i in district d votes for incumbent A if:

$$u_{id}^A + b_{id} > u_{id}^C,$$

where $b_{id} \stackrel{\text{iid}}{\sim} U[-\phi, \phi]$ is an idiosyncratic preference shock for the incumbent, with density $1/2\phi$. Utility from party A and C 's policy is, respectively:

$$u_{id}^A (g_d^A)^\alpha = \frac{1}{\alpha} (g_d^A)^\alpha - c_i^A(\sigma); \quad u_{id}^C (g_d^C)^\alpha = \frac{1}{\alpha} (g_d^C)^\alpha,$$

where $\alpha \in (0, 1)$ governs the concavity of preferences over proposed public goods, g_d^k . Given TOKİ projects are highly conspicuous, we assume full observability. We further suppose that voting for the incumbent bears a cost increasing and convex in the backsliding effort, with $c(0) = 0$, $c'(\sigma) > 0$, and $c''(\sigma) > 0$ (Nelson and Witko 2022; Svolik 2023).

3.2 Electoral Probabilities

Define $\Delta_{id} \equiv u_{id}^C - u_{id}^A$ as the utility differential between the challenger party and incumbent party for voter i in district d . The probability that voter i votes for the incumbent is thus:

$$\Pr(b_{id} > \Delta_{id}) = \frac{1}{2} \left\{ 1 - \frac{\Delta_{id}}{\phi} \right\}$$

Summing over all i voters, the expected vote share in district d for incumbent A and (by symmetry) for challenger C is

$$V_d^A = n_d \left[\frac{1}{2} \left\{ 1 - \frac{\Delta_{id}}{\phi} \right\} \right], \quad V_d^C = n_d \left[\frac{1}{2} \left\{ 1 + \frac{\Delta_{id}}{\phi} \right\} \right].$$

Define the district vote margin as $m_d = V_d^A - V_d^C + \varepsilon_d$, where ε_d captures residual non-policy uncertainty common to both parties, with $\varepsilon_d \stackrel{\text{iid}}{\sim} [-\mu_d(\sigma), \mu_d(\sigma)]$ and $\mu_d(\sigma) \geq n_d |\Delta_{id}| / \phi$. A smaller μ_d implies a narrower distribution, with more mass concentrated around zero; tighter margins thus suggest districts are more responsive to policy. The incumbent's objective is to maximize the number of districts won, $\Pr(m_d > 0)$:

$$\max_{\{T_d, \sigma\}} \sum_d \left[\frac{1}{2} - \frac{n_d \Delta_{id}}{2\mu_d(\sigma)\phi} \right] \quad \text{s.t.} \quad \sum_d T_d^k \leq T, \quad \sigma \geq 0. \quad (3.1)$$

3.3 Regime Type and Targeting Technology

Let $\mathcal{I}$ denote the set of districts *aligned* with the incumbent party, where we assume $n_d \perp \mathbf{1}\{d \in \mathcal{I}\}$. We define public goods expenditure received by voters in district d from party k as

$$g_d^k = \begin{cases} (1 + \lambda(\sigma))T_d^k & \text{if } \sigma > 0, d \in \mathcal{I} \\ T_d^k & \text{otherwise} \end{cases}$$

where $\lambda(\sigma) \geq 0$ captures the degree to which weakened democratic institutions amplify the electoral returns to spending in aligned districts.⁸ We impose the following assumptions on $\lambda(\sigma)$:

$$\lambda(0) = 0, \quad \frac{\partial \lambda(\sigma)}{\partial \sigma} \begin{cases} > 0 & \text{if } d \in \mathcal{I} \\ = 0 & \text{if } d \notin \mathcal{I} \end{cases}$$

Thus, as institutions erode, the incumbent gains the ability to favor aligned districts with public goods, such as through state capture of procurement and fiscal discretion (Gürakar 2016). Non-aligned districts do not benefit from this technology advantage. This mirrors the backsliding context in Turkey, whereby the “AKP government [has] politicised public resources to skew the playing field against the opposition. In the electoral arena this created an enormous advantage for the ruling party” (Esen and Gumuscu 2016, p. 1589).

8. This interpretation parallels the treatment of distributional efficiency parameters in Dixit and Londregan (1996) whereby targeting focuses on “core” voter groups.

We further assume that backsliding compresses the residual uncertainty across districts. Specifically, $\mu_d(\sigma) \in (0, \bar{\mu}]$ is decreasing in σ , such that weakening democratic institutions makes electoral outcomes in given districts more predictable and policy spending more electorally effective at the margin. As Bermeo (2016) notes, this can occur through a variety of channels, such as curtailing media access, impeding voter registration, and manipulating electoral rules to favor incumbents, alongside a range of other extra-electoral strategies.

Assumption 1 (Institutional Environment) *For an incumbent, the institutional variable, σ , affects (i) the targeting technology $\lambda(\sigma)$; and (ii) the dispersion of non-policy shocks $\mu_d(\sigma)$. Denote a regime as a tuple $(\lambda(\sigma), \mu(\sigma))$. Then:*

1. (Democracy) For $\forall d$, $\lambda(0) = 0$, $\mu_d(0) \approx \bar{\mu}$.
2. (Backsliding) $\lambda(\sigma) > 0$ for $d \in \mathcal{I}$ and $\mu_d(\sigma) < \mu_d(0)$ for $\forall d$.

Under democracy, all districts are thus approximately symmetric from the perspective of a democratic incumbent. The assumption over uncertainty, $\mu_d(0) \approx \bar{\mu}$, suggests that almost all districts, d , draw non-policy shocks from a common distribution, $\varepsilon_d \stackrel{\text{iid}}{\sim} [-\bar{\mu}, \bar{\mu}]$. On the other hand, the key distinction with a backsliding regime concerns the shift in the targeting technology and the decline in uncertainty over non-policy shocks within given districts.

3.4 Equilibrium Public Goods Expenditure

We solve for equilibrium public goods expenditure across parties, district type, and political institutions. The timing of the game is thus as follows:

1. The incumbent sets the level of institutions, $\sigma \geq 0$.
2. Incumbent party A and challenger party C simultaneously announce public goods expenditure platforms, $\{T_d^A\}_d$ and $\{T_d^C\}_d$, respectively, subject to budget constraint, $\sum_d T_d^k \leq T$.
3. Voter i in district d observes party platforms, realizes preference shocks, ϕ , and votes for their most preferred party.
4. Votes are aggregated within districts, the party with the most votes wins, and implements their allocation platform, $\{T_d^k\}_d$, with full commitment.

3.4.1 Democratic Equilibrium

Let us define the equilibrium expenditure in district d as a function of institutions, $\tilde{T}_d^A(\sigma)$. Under democracy, $\sigma = 0$, the equilibrium public goods expenditure to district d by the incumbent satisfies:

$$\tilde{T}_d^A(0) = T \frac{\left(\frac{n_d}{\mu_d(0)}\right)^{\frac{1}{1-\alpha}}}{\sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(0)}\right)^{\frac{1}{1-\alpha}}}, \quad (3.2)$$

which is symmetric across both parties $\tilde{T}_d^A(0) = \tilde{T}_d^C(0)$. See Appendix B.1 for derivation.

Proposition 1 (Democratic Allocation Gap) Denote $\mathbb{E}_d[\tilde{T}_d^A(\sigma)] := \frac{1}{|\mathcal{D}|} \sum_{d \in \mathcal{D}} \tilde{T}_d^A(\sigma)$ as the district average public goods expenditure. The difference in mean allocation of expenditures across aligned and non-aligned districts is:

$$\mathbb{E}_d[\tilde{T}_d^A(0) \mid d \in \mathcal{I}] - \mathbb{E}_d[\tilde{T}_d^A(0) \mid d \notin \mathcal{I}] \approx 0.$$

Unpacking Proposition 1, heterogeneity in $\mu_d(0)$ may lead a small number of highly responsive districts (those with relatively low $\mu_d(0) < \bar{\mu}$) to receive slightly higher expenditures at the margin. However, such deviations do not generate systematic differences in mean allocation across aligned and non-aligned districts. This reflects the raw patterns in the data as shown in panel (a) of Figure 1, such that public goods expenditure across all districts largely trended with population subject to concavity and minor deviations based on AKP-alignment.

3.4.2 Backsliding Equilibrium

Under backsliding, $\sigma > 0$, the equilibrium public goods expenditure satisfies:

$$\tilde{T}_d^A(\sigma) = \begin{cases} T \left(\frac{1}{\mathcal{Z}(\sigma)} \right) \left[(1 + \lambda(\sigma))^\alpha \left(\frac{n_d}{\mu_d(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} & \text{if } d \in \mathcal{I} \\ T \left(\frac{1}{\mathcal{Z}(\sigma)} \right) \left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}} & \text{if } d \notin \mathcal{I} \end{cases} \quad (3.3)$$

where $\mathcal{Z}(\sigma) = \sum_{d' \in \mathcal{I}} \left[(1 + \lambda_{d'}(\sigma))^\alpha \left(\frac{n_{d'}}{\mu_{d'}(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} + \sum_{d' \notin \mathcal{I}} \left(\frac{n_{d'}}{\mu_{d'}(\sigma)} \right)^{\frac{1}{1-\alpha}}$ is a common normalizing constant. The challenger, lacking the incumbent's institutional advantages, proposes expenditures more approximate to the democratic allocation, where $\mu(\sigma)$ is taken as given:

$$\tilde{T}_d^C(\sigma) = T \frac{\left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}}}{\sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(\sigma)} \right)^{\frac{1}{1-\alpha}}} \text{ for } \forall d.$$

This breaks the symmetry of the democratic equilibrium and generates policy divergence under backsliding: the incumbent over-provides to aligned districts $d \in \mathcal{I}$ relative to the challenger, while implicitly providing a smaller share of overall expenditures to non-aligned districts $d \notin \mathcal{I}$. See Appendix B.1 for derivation.

Proposition 2 (Backsliding Allocation Gap) Under backsliding, $\sigma > 0$, let us denote the difference in mean public goods allocation across aligned and non-aligned districts as:

$$\tilde{\beta}(\sigma) := \mathbb{E}_d[\tilde{T}_d^A(\sigma) \mid d \in \mathcal{I}] - \mathbb{E}_d[\tilde{T}_d^A(\sigma) \mid d \notin \mathcal{I}].$$

Then $\tilde{\beta}(\sigma) > 0$.

3.5 Distributive Incentives for Backsliding

The previous subsections characterize allocation conditional on an institutional environment. Because the incumbent first chooses σ , we now outline when the democratic benchmark is locally stable. Consider a marginal deviation from democracy, $\sigma = 0$. Taking the derivative of the incumbent's value function in equation (3.1) with respect to σ at the democratic boundary yields:⁹

$$\sum_{d \in \mathcal{I}} \frac{n_d}{\mu_d(0)} \left[(T_d^A)^\alpha \frac{\partial \lambda(0)}{\partial \sigma} \right] > \sum_d \frac{n_d}{\mu_d(0)} \left(\frac{\partial c_d^A(0)}{\partial \sigma} \right) \quad (3.4)$$

Condition (3.4) compares the marginal benefit of backsliding with its marginal electoral cost. The left-hand side captures the increase in the incumbent's expected district-level support generated by a stronger targeting technology, $\lambda(\sigma)$, in aligned districts. The right-hand side captures aggregate backlash from institutional erosion, $c_d^A(\sigma)$, which lowers the utility voters receive from supporting the incumbent. Backsliding is locally attractive when gains among aligned districts exceed losses from voters who punish democratic erosion (Svolik 2023).¹⁰

This condition should not be interpreted as a fully fledged theory of democratic backsliding. Rather, it closes the first stage of the game and clarifies how institutional choice and policy allocation interact. Because public expenditures exhibit diminishing marginal electoral returns through $\alpha \in (0, 1)$, while the costs of institutional erosion are increasing and convex, the incumbent may face incentives for partial and incremental erosion rather than complete authoritarian consolidation. This is consistent with the piecemeal nature of contemporary backsliding, where incumbents weaken democratic institutions gradually rather than through abrupt breakdown (Levitsky and Ziblatt 2018).

A final corollary of condition (3.4) is that marginal benefits to backsliding will be greater among aligned districts with larger populations, n_d . This is consistent with evidence of an "urban bias" underpinning authoritarian policy (Wallace 2013). Intuitively, institutional manipulation becomes more valuable where the incumbent can convert targeted public expenditures into support among a larger number of voters. Backsliding therefore amplifies incentives to favor populous aligned districts, as conveyed in panel (b) of Figure 1.

3.6 Allocation Pre-Post Backsliding

Suppose now there are two periods, $s \in \{1, 2\}$, where nature sets $\sigma_1 = 0$. Under full commitment, allocations in each period are given by the stage-game optimum conditional on σ_s . Define $\tilde{\theta}$ as the mean difference in allocation between aligned and non-aligned districts under democracy and

9. See Appendix B.2 for derivation.

10. This logic is consistent with the political conflict surrounding urban transformation in Turkey, including the 2013 Gezi Park protests, which began in response to redevelopment plans in Istanbul and quickly broadened into a larger confrontation over authoritarian governance.

backsliding:

$$\tilde{\theta} = \underbrace{\mathbb{E}_d[\tilde{T}_d^A(0) \mid d \in \mathcal{I}] - \mathbb{E}_d[\tilde{T}_d^A(0) \mid d \notin \mathcal{I}]}_{=\tilde{\beta}(\sigma_1)} - \underbrace{\mathbb{E}_d[\tilde{T}_d^A(\sigma_2) \mid d \in \mathcal{I}] - \mathbb{E}_d[\tilde{T}_d^A(\sigma_2) \mid d \notin \mathcal{I}]}_{=\tilde{\beta}(\sigma_2)} \quad (3.5)$$

Assuming electoral uncertainty is sufficiently homogeneous across districts under democracy, then for a given district $d \in \mathcal{I}$, there is an increase in public goods expenditure under backsliding if

$$\frac{1}{\mathcal{Z}(\sigma_2)} \left[(1 + \lambda(\sigma_2))^\alpha \left(\frac{1}{\mu_d(\sigma_2)} \right) \right]^{\frac{1}{1-\alpha}} > \frac{1}{\sum_{d'} (n_{d'})^{\frac{1}{1-\alpha}}}, \quad (3.6)$$

which clearly holds when $\lambda(\sigma_2) > 0$ and $\mu_d(\sigma_2) < \bar{\mu}$. The right-hand side corresponds to a uniform per-capita allocation benchmark, where districts receive expenditures proportional to population size. The left-hand side captures the effective political weight of district d under backsliding, combining both the targeting advantage $\lambda(\sigma_2)$ and reduced electoral uncertainty $1/\mu_d(\sigma_2)$. The condition thus states that, under backsliding, aligned districts receive more than their population share whenever their marginal political return to spending exceeds the democratic per-capita benchmark.

Proposition 3 (Pre-Post Backsliding Allocation) *If condition (3.6) holds, then for a given aligned district $d \in \mathcal{I}$, the difference in mean public goods expenditure pre-post backsliding is:*

$$\hat{\theta} \approx \mathbb{E}_d[\tilde{T}_d^A(0) - \tilde{T}_d^A(\sigma_2) \mid d \in \mathcal{I}] < 0, \quad (3.7)$$

which implies that $|\hat{\theta}| \approx \hat{\beta}(\sigma)$.

Proposition 3 thus confirm that, in expectation, aligned districts receive systematically more expenditure under backsliding than under democracy. Moreover, democracy effectively quells targeting across aligned and non-aligned districts in sufficient magnitude to offset any gains in equilibrium expenditures under backsliding as an aligned district.

4 Data and Empirical Strategy

4.1 Data

Districts and Local Elections The Turkish political geography comprises 81 provinces (*il*) which themselves contain several districts (*ilçe*). Before 2008, there were a total of 923 districts. After 2008, new districts emerged, with further splits of “central districts” (*merkez ilçe*) occurring in 2013 across the country’s thirty metropolitan provinces.¹¹ The number of districts after 2013 then reached a total of 973, which has remained unchanged since.

11. Metropolitan municipalities are essentially provinces, but not all provinces are metropolitan municipalities. Before 2012, there were 16 metropolitan municipalities, including Istanbul, Ankara, and Izmir. A major reform in 2012 (Law No. 6360) expanded this number to 30.

This paper focuses on local electoral outcomes at the district-level. General elections determine the seats in the Grand National Assembly of Türkiye, whereas local elections determine the mayor of a given municipality and its council members. Using a first-past-the-post electoral system, these elections have been consistently held every five years, of which we focus on results from 2004, 2009, 2014 and 2019.¹² We collect data on local election vote shares from Türkiye’s Supreme Election Council website. We also gather data on turnout rates within districts to account for the possibility that more politically active districts demand greater TOKİ projects, and also collect information on the proportion of “invalid votes” to proxy for any electoral malpractice. See Appendix A.2 for summary statistics of all variables used in the paper.

TOKİ Projects TOKİ publishes project names and auction documents on its website. Each project belongs to one of eighteen categories as listed in Table 1. We scraped this data and matched each project with a district and year based on the project name and auction date. We take the winning bid as the approximate cost (i.e. the contracted capital expenditure) of the project.¹³ For 204 projects whose cost variable was missing, we take the average cost of the given category as a measure of central tendency to maintain these observations. The overall number of distinct tenders we scraped data on totaled 3,193 between 2004 to 2020.

To convert our TOKİ cost variable to real Turkish Liras, we further obtained the consumer price index (CPI) from the Central Bank of the Republic (CBRT) of Türkiye.¹⁴ To account for district “size”, we also use the Turkish Statistical Institute’s (TURKSTAT) regional variables database for district-level population measures covering the years 2007-2020. There is a lack of annual data in earlier years as Türkiye transitioned from an original nationwide census to an address-based population counting system in 2007. We thus estimated the previous years using 2000 census data and assumed a linear trend between 2000 and 2007.

To convey the spatial distribution of TOKİ expenditures and local election outcomes, Figure 2 plots average TOKİ contract values and AKP vote shares between 2004-2020 across districts.¹⁵ As shown, for both total TOKİ expenditures and vote shares, there appears to be little spatial autocorrelation in the raw data.

General Election Covariates We also collect data on vote shares, turnout rate and the proportion of invalid votes in general elections from the Supreme Election Council. Using these as covariates helps address the potential channel that members of parliament could wield some influence over TOKİ distribution and supporting local mayors.¹⁶

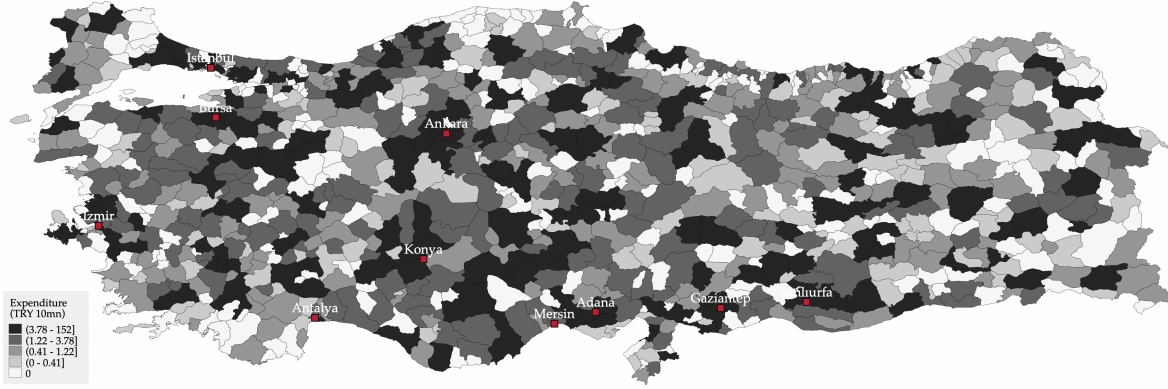
12. General elections, in contrast, are run using a version of proportional representation (the D’Hondt method).

13. While the actual realized cost may diverge from the winning bid over the project’s life cycle, due to factors such as construction delays, renegotiations, or firm-specific conditions, the bid amount remains a reliable proxy for the level of investment committed by the AKP to a given district.

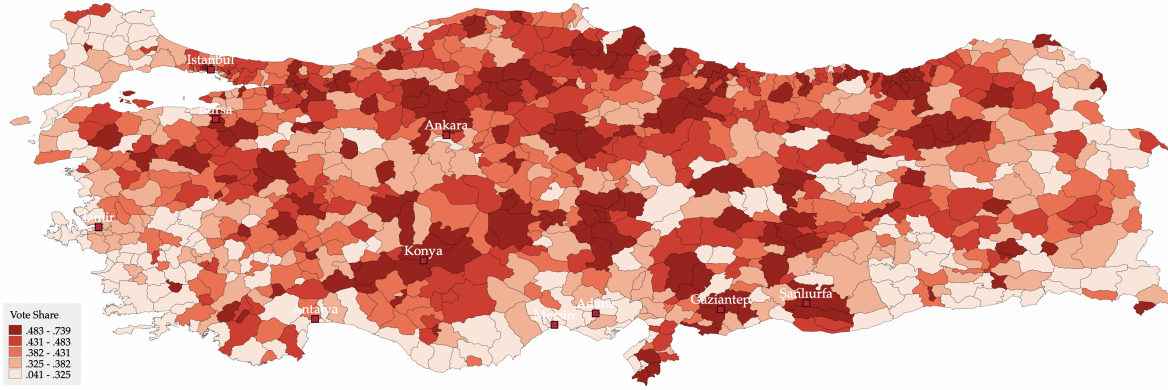
14. The base year provided by the CBRT is for 2003.

15. See Appendix A.4 for total contract values per year across Türkiye’s subregions.

16. We note with some caution that including these covariates could lead to post-treatment bias (e.g. local election results in 2009 may impact general election results in the 2011 general election), but results are robust to their inclusion or exclusion.



(a) Distribution of TOKİ Expenditure



(b) Distribution of AKP Vote Shares

Figure 2: Distribution of TOKİ Expenditure and AKP Vote Shares (2004-2020)

Note: panel (a) plots total TOKİ capital expenditures and panel (b) plots average AKP vote shares in local elections. Both variables are pooled over 2004-2020. The legend refers to quintiles of the overall distribution. The top 10 most populous cities in Türkiye are also plotted for reference.

Other District Characteristics To also control for district-level characteristics, we use satellite data from the European Space Agency’s (ESA) Climate Change Initiative (CCI) Land Cover project to measure annual urban settlements. More densely populated areas may require greater housing needs, and the stock of buildings may also serve as some constraint on development. Finally, to capture district-specific shocks from natural disasters, we use data from the EM-DAT database. Focusing on earthquakes and floods, we take the geolocations provided and use additional information on impact magnitude from EM-DAT to generate buffers gauging the total area of impact. We then create a dummy variable for whether a district was coincident with this area.

Table 1: Summary Statistics of
TOKİ Contracts by Project Type, 2004-2020

	(1)	(2)	(3)	(4)	(5)	(6)
	Contracts	Houses	Mean	Std. Dev.	Min.	Max.
Agricultural Village	47	6,429	5.86	5.33	1.21	32.68
Consultancy Services	2	–	1.79	–	1.79	1.79
Disaster Relief	93	27,412	12.68	8.55	0.71	46.37
Government Housing	670	266,636	14.15	12.08	0.36	72.01
Government Services	140	683	9.65	23.69	0.24	253.10
Infrastructure and Social Facilities	901	12,194	8.63	14.92	0.06	267.57
Low Income Housing	101	42,667	12.23	9.57	0.85	42.84
Maintenance and Repair	15	144	2.90	3.37	0.30	8.84
Ministry of Education	30	–	6.76	6.46	2.09	34.13
Other	31	1,098	19.71	27.45	0.03	97.05
Public Gardens and Parks	74	–	15.46	79.41	1.02	678.95
Replenishment	89	13,786	10.15	13.48	0.08	74.54
Requested/Demand-Based	216	50,514	9.43	6.38	0.93	33.15
Restoration	15	–	2.46	5.86	0.18	23.15
Shared Income	34	19,403	55.17	56.81	3.64	227.73
Social Housing	477	191,559	14.97	13.59	0.02	148.88
Stadiums	20	–	45.30	24.61	3.84	109.77
Urban Transformation/Renewal	238	115,943	19.94	12.74	0.42	59.84

Note: mean values refer to pooled 2004–2020 time averages, with corresponding standard deviations. Contracts refers to the total number of contracted projects that have been procured from TOKİ per category. Where relevant, houses refers to the number of housing units awarded in contracts per category. Mean refers to average contract expenditure per project category. All expenditure for mean, standard deviation, min and max is denoted in real (2020) million Turkish Liras. The following categories consist exclusively of projects that do not include any housing developments: Consultancy Services, Ministry of Education, Public Gardens and Parks, Restoration and Stadium. See Appendix A.3 for description of project types.

4.2 Empirical Strategy

Our core identification strategy exploits variation in the entry and exit of AKP mayors following local election outcomes. Given the timing of local elections is fixed, the timing of “treatment” itself is pre-determined (i.e. snap elections are not called during more expedient periods that correlate with greater AKP success and the distribution of TOKİ contracts).

4.2.1 Two-Way Fixed Effects (TWFE)

Our baseline approach uses a standard two-way fixed effects (TWFE) specification with an interaction term to capture heterogeneity pre-post Türkiye’s institutional shift:

$$y_{dprt} = \alpha_d + \delta_{rt} + \beta m_{dprt}^{\text{AKP}} + \theta \left(m_{dprt}^{\text{AKP}} \times \mathbf{1}\{t < 2011\} \right) + \rho \mathbf{X}'_{dprt} + u_{dprt}, \quad (4.1)$$

$$\text{where } y_{dprt} = \log \left(\frac{\sum_{c \in C_{dprt}} \text{Contract Value}_{dprt}}{\text{Population}_{dprt}} + \epsilon \right)$$

Here, our core outcome variable, y_{dprt} , represents the (natural) logarithm of the winning contract value on TOKİ infrastructure projects per capita, at the time of award, in district d of province p in subregion r (Türkiye’s 26 NUTS-2 level subregions) of year t .¹⁷ While this may differ from actual disbursements as projects are constructed, this serves as the best proxy for dedicated public capital expenditures. If a district received multiple contracts, c , where $|C_{dpt}|$ represents the total number of contracts for a given district-year, we sum the contract values and then divide through by the respective population.

In equation (4.1), we include district fixed effects, α_d , and subregion $\times$ year fixed effects, δ_{rt} , to absorb time-invariant unobservables at the district-level in addition to any time-varying shocks common to districts across Türkiye’s subregions.¹⁸ Our main regressor of interest is whether the incumbent mayor belongs to the AKP, m_{dprt}^{AKP} . Variation is thus driven by transitions to the AKP within a district, which can occur from any incumbent opposition party, given Türkiye is a multi-party democracy, or re-election of an AKP incumbent. The control group thus remains districts that never switch to the AKP (i.e. “never-treated” units), in addition to districts where the AKP lose an election.

To capture heterogeneity pre-post backsliding, we introduce an interaction term, $m_{dprt}^{\text{AKP}} \times \mathbf{1}\{t < 2011\}$, which outlines the years Türkiye was largely considered a democracy. As per Appendix Figure A.1, we see the institutional shift in Türkiye beginning around the early 2010s, and this is corroborated by [Acemoğlu and Üçer \(2020\)](#) who claim backsliding accelerated after 2011. The coefficient θ thus allows us to explore whether targeted allocation of TOKİ contracts to local mayors was dampened during Türkiye’s early experience with democracy, as arguably manifest with more competitive, transparent and fairer procurement processes. The estimate for β hence captures selective allocation during the period that Türkiye begins to backslide. These estimands are clearly empirical analogs for the parameters $\hat{\theta}$ and $\hat{\beta}(\sigma)$ from our theoretical model, thus allowing us to directly test propositions 1-3.¹⁹

$\mathbf{X}'_{dprt}$ captures other district-level observable characteristics that could affect both TOKİ distribution and party vote shares. We include controls for local municipal elections (turnout rate, pro-

17. We add an $\epsilon (= 0.01)$ to maintain observations that were awarded no contracts.

18. Results are essentially identical when using standard year fixed effects.

19. This is a similar identification strategy to [Burgess et al. \(2015\)](#), who study ethnic favoritism in road development by exploiting Kenya’s multiple transitions into and out of multiparty democracy.

portion of invalid votes, and whether the central mayor also belongs to the AKP), general election controls (turnout rate, proportion of invalid votes and AKP vote share), in addition to the number of urban settlements and presence of any natural disasters.

To further address issues of zero-inflation, given contract allocations are generally episodic spending shocks versus smooth annual expenditure flows, we create a dummy variable as to whether a district received any contract from TOKİ in a given year, $y_{dprt} = \mathbf{1} \left(\sum_{c \in C_{dprt}} \text{Contract Value}_{dprt} > 0 \right)$. This consequently allows us to examine the effect of AKP incumbency vis-à-vis TOKİ allocation on the extensive margin via a basic linear probability model, using the specification in equation (4.1).

4.2.2 Poisson Quasi-Maximum Likelihood (QMLE)

Another way to probe intensive margin variation, at least for projects involving social housing construction, concerns the number of houses awarded for development within a given contract. To estimate the impact of local AKP mayoral incumbency on these (count) outcomes, we use the Poisson quasi-maximum likelihood (QMLE) estimator:

$$h_{dprt} = \exp \left(\alpha_d + \delta_{rt} + \beta m_{dprt}^{\text{AKP}} + \theta (m_{dprt}^{\text{AKP}} \times \mathbf{1}\{t < 2011\}) + \rho \mathbf{X}'_{dprt} \right) \cdot u_{dprt}, \quad (4.2)$$

Following equation (4.2), h_{dprt} now refers to the number of housing units awarded within a contract, which we again sum over all housing-relevant contracts awarded in a given district-year. We continue to include district and subregion $\times$ year fixed effects, alongside relevant district-level covariates.²⁰ Again, the interaction term allows us to separately estimate the impacts of AKP incumbency both before and after democratic backsliding commences.

4.2.3 Fixed Effects Counterfactual Imputation (FECT)

The growing literature on the drawbacks of static TWFE models suggests there may be heterogeneous treatment effects or observations that receive negative weights which could bias the estimates from these specifications (Goodman-Bacon 2021). Given our treatment variable of interest does not follow a “staggered adoption” set-up, as AKP-affiliated mayors can enter and exit office based on electoral results, treatment status further permits “reversals”.

A second issue concerning our core specifications using TWFE and QMLE is that the interaction term suggests a sharp discontinuity exists between Türkiye’s the period of democratic institutions and the transition to electoral authoritarianism. Although this set-up is transparent, and easy to interpret heterogeneous treatment effects on the extensive margin, theoretical accounts of democratic backsliding suggest institutional erosion is largely a piecemeal phenomenon, as also suggested by the backsliding condition in equation (3.4) (Levitsky and Ziblatt 2018).

20. QMLE also helps to address the use of log-like transformations for variables that are weakly positive but can equal zero (Chen and Roth 2023).

To address the aforementioned issues, we rely on [Liu, Wang, and Xu \(2024\)](#)’s fixed effects counterfactual estimator (FECT) to impute the counterfactual for treated units. The method relies on using all data under control status to impute the counterfactual outcome for units that enter treatment, following a four-step procedure. First, estimate $y_{dprt}(0) = f(\mathbf{X}_{dprt}) + h(\mathbf{U}_{dprt}) + u_{dprt}$, where $f(\cdot)$ and $h(\cdot)$ refer to flexible functions for observables and unobservables, respectively, using all untreated observations and pre-treatment data.²¹ Second, use estimates of $\hat{f}(\mathbf{X}'_{dprt})$ and $\hat{h}(\mathbf{U}'_{dprt})$ to impute counterfactual outcomes, $\hat{y}_{dprt}(0)$, for all treated observations. Third, take the difference in imputed outcomes versus observed outcomes for treated units to calculate individual level treatment effects, $\hat{\beta}_{dprt} = y_{dprt} - \hat{y}_{dprt}(0)$. Finally, take averages of $\hat{\beta}_{dprt}$ by calendar year to calculate the average treatment effect on the treated (ATT) as a function of time. Given the linearity assumption embedded in the FECT method, we pursue this estimation strategy only for the log of contract values per capita in addition to the contract award dummy as outcome variables, as per equation (4.1).

5 Core Results

5.1 Static Results based on TWFE and Poisson QMLE

Table 2 outlines the core results, showing clear heterogeneity in the targeted allocation of TOKİ contracts following local AKP mayoral victories pre-post democratic backsliding. Using TWFE, column 1 suggests that, under backsliding, districts with either newly incumbent AKP mayors or where the AKP were re-elected received contracts that were 46.4% greater in value per capita. On the extensive margin, in column 3 we also find that AKP incumbency following local elections when Türkiye becomes an electoral autocracy results in a 4.2 percentage point increased probability of being awarded a contract from TOKİ (approximately half of the baseline sample mean for districts with non-AKP mayors). In column 7, when focusing on the total number of housing units using Poisson QMLE, despite some loss of sample size, AKP mayors under backsliding receive contracts with 84.5% more houses relative to districts that do not switch to the AKP.

Strikingly, the estimated interaction effects under Türkiye’s period of democracy across columns 1, 4 and 7 are essentially equal and opposite in sign to the effects under backsliding, thus confirming supporting 3 from the theoretical model. Such evidence suggests that democratic institutions, as also manifest in more competitive procurement practices before the AKP dismantled several procurement laws, strongly quelled the selective targeting of TOKİ contracts towards districts aligned with the regime. F-tests in columns 1 and 4 and a Chi-squared test in column 7 provide a sharper test of this result, where we fail to reject the null hypothesis that the total effect of AKP electoral victories under democracy on TOKİ allocation is equal to 0, thus consistent with

21. We use the default TWFE approach to model unobservables, such that $h(\mathbf{U}_{dprt}) = \alpha_d + \delta_t$. When we try to more flexibly model observables and unobservables, both the factor-augmented models of [Bai \(2009\)](#)’s interactive fixed effects (IFE) and [Athey et al. \(2021\)](#)’s matrix completion (MC) give identical results to just using TWFE. Using cross-validation, the number of factors IFE provides as optimal is 0, and the nuclear norm estimated via MC is 1.

proposition 1 from the model.

These effects are not only statistically significant but are also quantitatively meaningful. The core estimate in column 1 suggests that, relative to the pooled baseline mean of districts with a non-AKP affiliated incumbent, districts with AKP mayors receive ₺213.27 more per person in 2020 prices. When aggregated over the mean district population of circa 80,000 individuals, this translates into approximately ₺5.2 million more overall capital expenditure on social infrastructure projects (approximately USD 740,000) relative to the mean contract value of opposition districts totaling ₺11.3 million (USD 1.6 million). The economic consequences of democratic backsliding for voters are thus clearly non-negligible.

While the interaction approach is one method to probe heterogeneity, running the specifications in equations (4.1) and (4.2), the estimates for β and θ pool across switches from all elections pre-post the institutional shift. As an additional approach, we subset the data by homing in on the 2009 and 2014 elections as specific treatment “cohorts” to more cleanly probe heterogeneity via a sample split. The former cohort comprises AKP wins just before backsliding precipitated, thus allowing us to also include the democracy interaction term to see if effects are truly being picked up after 2011. On the other hand, the 2014 cohort involves switches to the AKP after backsliding occurred, where we should expect larger effect sizes if the AKP has intensified targeted allocation.

In columns 2, 5 and 8 of Table 2, we home in on the 2009 election. We find very similar results to those in columns 1, 4 and 7, such that AKP incumbency robustly predicts the allocation of TOKİ contracts after backsliding commences, but appears to be symmetrically moderated under democracy. Again, we fail to reject the null hypothesis for the equality of the base and interaction effects using F-tests and Chi-squared tests. Focusing on the 2009 election itself provides more compelling evidence on the timing of backsliding; even districts where the AKP won in 2009, just before the institutional slide commenced, but where mayors were still incumbent under backsliding after 2011 and just before the 2014 election, show a sharp increase in politically targeted allocation. This pattern suggests that favoritism in TOKİ distribution intensified precisely when institutional constraints were sufficiently weakened.

Finally, with the exception of column 9, we find marginally larger effect sizes for targeted TOKİ allocation in columns 3 and 6, focusing only on AKP electoral victories following the 2014 election after backsliding had commenced. Comparing columns 2 and 3 for contract values, newly incumbent AKP mayors in the 2014 local election received contracts that were essentially 6% more valuable per capita than districts from the 2009 election under backsliding. This provides further support to the notion that the weakening of democratic institutions has enabled greater selective allocation of TOKİ contracts on both the intensive and extensive margins.

Table 2: Heterogeneous Effects Pre-Post Democratic Backsliding

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Contract Value per Capita			Contract Award			Housing Units		
AKP Mayor	0.381*** (0.087)	0.425* (0.224)	0.488** (0.204)	0.042*** (0.009)	0.038* (0.022)	0.051** (0.022)	0.613*** (0.142)	1.256*** (0.415)	0.835* (0.505)
AKP Mayor $\times 1\{t < 2011\}$	-0.230* (0.118)	-0.931** (0.413)		-0.028** (0.012)	-0.102** (0.046)		-0.383* (0.204)	-2.146*** (0.791)	
$H_0 : \beta + \theta = 0$ [p-value]	2.832 [0.093]	1.663 [0.198]		2.247 [0.134]	2.198 [0.139]		2.058 [0.151]	1.316 [0.251]	
Observations	15,976	4,898	3,914	15,976	4,898	3,914	8,917	1,410	1,059
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subregion $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
General Election Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Cohort	All	2009	2014	All	2009	2014	All	2009	2014

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. Columns 1-6 use TWFE, and column 7-9 use Poisson QMLE. Columns 4-9 control for the natural logarithm of population as a covariate. Columns 2, 5 and 8 only use switchers to the AKP from the 2009 election (i.e. treatment cohort), and columns 3, 6 and 9 only use switchers to the AKP from the 2014 election, with the control group being districts that never switch to the AKP. Columns 1-2 and 4-5 use an F-test, whereas columns 7-8 use the Chi-squared test. See Appendix E for estimates of all control variables.

5.2 Dynamic Results based on FECT

Additional to Table 2, we now use Liu, Wang, and Xu (2024)'s imputation method to estimate dynamic treatment effects as a function of calendar time. Figure 3 plots the ATTs calculated by calendar year, visualizing clear treatment effect heterogeneity in TOKİ allocation between 2004 to 2020. We find consistent null effects in the pre-2011 democratic environment but then significant effects post-2010 at the 5% level. Using Liu, Wang, and Xu (2024)'s method, a locally estimated scatterplot smoothing (LOESS) function is applied across these effects to provide smooth trendlines with corresponding 95% confidence intervals. As opposed to our extensive margin results in Table 2 using an interaction term, this allows us to evaluate dynamic treatment heterogeneity more continuously.

The LOESS trendline further confirms heterogeneous effects over time, as estimated effects of AKP incumbency on TOKİ allocation only become significant after 2011 (indicated by the vertical dashed line), and hence confidence bands no longer cross zero. The ATTs for 2011, 2012, 2015 and 2017 are the positively estimated and significant point estimates that push the LOESS trendline upwards. While this trend has been qualitatively identified by Acemoğlu and Üçer (2020), Figure

3 provides direct support that TOKİ distribution, as determined by new AKP mayoral incumbency at the local level, intensified upon Türkiye’s institutional decline in 2011.

Several caveats to Figure 3 are worth addressing. Although individual point estimates are slightly volatile, even in the backsliding period, this likely reflects the underlying election cycle rather than model instability. Appendix C.1 provides event study plots for the 2009 and 2014 treatment cohorts, averaging $\hat{\beta}_{dprt}$ across specific pre- and post-treatment periods rather than averaging all cohort-level effects by calendar year. Positive and significant estimates emerge only after electoral turnover to the AKP, with little evidence of pre-trends. Interestingly, for the 2009 cohort, positive effects only appear after the onset of backsliding post-2010. And as a type of placebo test, for the 2014 cohort, we find no positive effects in the three pre-treatment years before a district switches to the AKP, despite backsliding having already commenced.

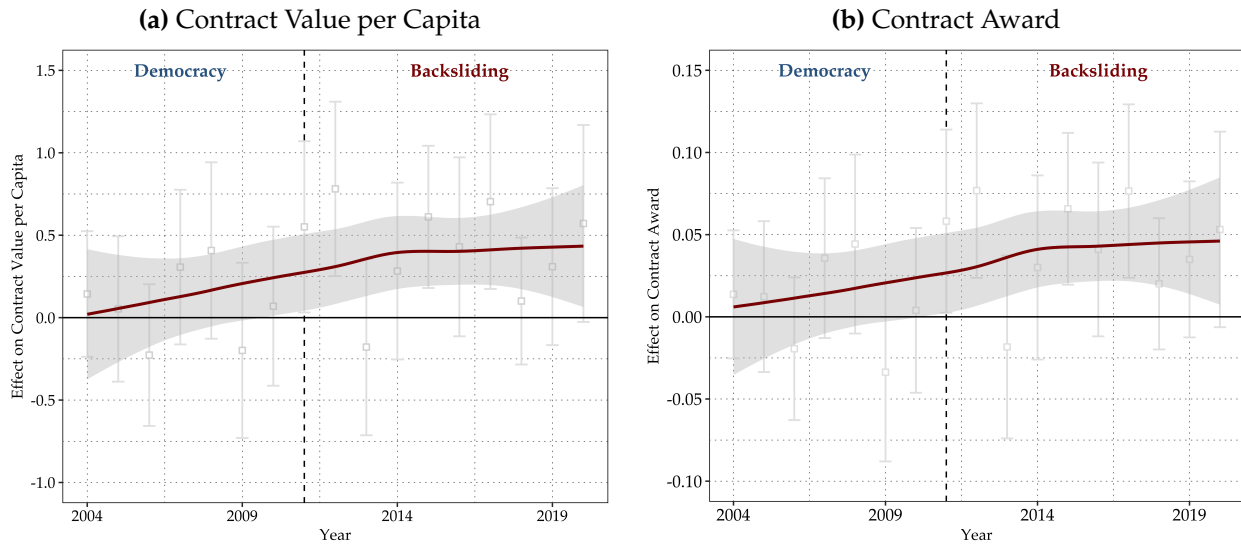


Figure 3: Heterogeneous Treatment Effects by Year

Note: light grey squares and 95% confidence intervals plot calendar year ATTs. LOESS trendline is in red with 95% confidence intervals. LOESS is weighted by the number of observations under treatment status per year. Dashed vertical line at 2011 indicates the general start of democratic backsliding in Türkiye. We include municipality election controls, general election controls, and district-level controls (see Table 2 notes). See Appendix E for estimates of all control variables.

On balance, these patterns suggest that TOKİ distribution is broadly a post-election phenomenon: contracts tend to be awarded following AKP mayoral victories rather than before. This is loosely consistent with our model’s assumption of full commitment to distribution – while TOKİ projects may be verbally campaigned around prior to elections, their *realized* disbursement occurs after electoral turnover. Accordingly, even during the post-2010 backsliding period, ATTs are generally insignificant in the intervals immediately preceding local elections (2013, 2018) and in election years themselves (2014, 2019). Figure 3 thus offers limited support for a political business cycle interpretation of TOKİ distribution (Nordhaus 1975).

5.3 Robustness Checks

The core identifying assumption for estimating β and θ in equation (4.1) is *strict exogeneity*, requiring that u_{dprt} be uncorrelated with past, current, and future regressors. This rules out feedback effects, anticipation effects, and time-varying confounders, and implies the standard parallel trends assumption that untreated potential outcomes evolve similarly across districts. We address these concerns through a series of robustness and validation exercises below.

Tests for Pre-Trends and Carryover Effects In Appendix C.2 we more formally test for pre-trends using Liu, Wang, and Xu (2024)’s bespoke tests. Another identification assumption is no *carryover effects*, such that, once an AKP mayor is defeated in an election, the treatment effect of contract allocation should “turn off”. For both pre-trends and carryover effects, we largely reject the null hypothesis of their presence.

Province $\times$ Year Fixed Effects Although we absorb time-varying unobservables at the NUTS-2 level across Türkiye in Table 2 with subregion $\times$ year fixed effects, in Appendix C.3 we also include province $\times$ year fixed effects, δ_{pt} , across Türkiye’s 81 provinces. Albeit a more saturated model, our core pooled results in Table 2 remain highly stable and similar in magnitude. This also helps to absorb any slow-moving but time-varying unobservables that coincide with backsliding, such as province-specific trends in economic development, fiscal capacity, and baseline demand for public goods, as well as differential exposure to nationwide shocks (e.g. the Turkish economic crisis in 2018).

Dropping Units Despite controlling for urban settlements using satellite data from the ESA, there may still be urban centers which drive AKP incumbency and the distribution of TOKİ projects. Certain electoral districts may also be highly important, such as the major cities of Ankara, Istanbul and Izmir. We thus sequentially drop all districts that fall within each of Türkiye’s 81 provinces to ensure the overall result is not driven by potential outliers. Separately, to address any zero inflation issues, we also repeat the specifications in Table 2 but drop districts that never receive any TOKİ contracts between 2004-2020. See Appendix C.4 and C.5.

Randomization Inference For each election, we randomly reassign AKP incumbency across districts and re-estimate our main specifications 10,000 times under the sharp null of no treatment effect (for both the base effect and interaction term). The observed estimates from Table 2 lie in the respective upper tails of the simulated distributions, providing strong evidence against the sharp null and reinforcing a non-artificial link between AKP incumbency and greater TOKİ contract allocation pre-post backsliding. See Appendix C.6.

Non-AKP Switches If TOKİ allocation increases only following AKP electoral victories, it suggests there is partisanship-specific favoritism, and allocation is hence not driven by general electoral turnover effects. In Appendix C.7, we use Liu, Wang, and Xu (2024)’s procedures to estimate (and explicitly visualize) switches to the CHP from all other opposition parties, and vice versa. We find no meaningful, systematic evidence across pre- and post-treatment periods to suggest

that TOKİ allocation has been responsive to these kinds of electoral switches. This provides confidence in the results from Table 2 and Figure 3, such that the relationship between switches to the AKP in local elections and TOKİ allocation pre-post backsliding is non-artificial.

The Role of House Prices A potential concern is that TOKİ allocates contracts in response to real estate market conditions rather than electoral considerations, such that observed patterns reflect housing fundamentals rather than political targeting. We address this through two complementary strategies. First, using Residential Property Price Index (RPPI) data at the NUTS-2 level from the Central Bank of Türkiye, we control for house price dynamics from 2010 onward (see Appendix C.8). Although geographic variation is not as granular than our district-level analysis, our main estimates are robust to this control, though the democracy interaction becomes noisier given the truncated sample.

Second, a related concern is that TOKİ contracts, by shifting housing supply, could depress prices for existing homeowners and generate a political backlash that co-determines both allocation and political preferences. Using district-level housing sales data from TURKSTAT since 2015, we show via Poisson QMLE that contract awards do not predict either first-hand or second-hand (count) house sales (Appendix C.8). While we do not claim TOKİ is inconsequential for Turkish residential markets broadly, the timing of selective contract allocation within district-years does not appear to systematically co-move with real estate conditions, providing confidence that our estimates capture political targeting rather than market fundamentals.

Alternative Backsliding Measures Our core analysis exploits the institutional threshold motivated by Acemoğlu and Üçer (2020) and as corroborated by the evolution of V-Dem indicators shown in Appendix Figure A.1. However, objective and perception-based measures of democratic backsliding do not always align, making it useful to examine alternative indicators (Little and Meng 2024a). Because our argument concerns the erosion of both political institutions and economic-legal constraints, we exploit the passage of Law No. 5812 at the end of 2008 as an alternative threshold. This amendment constituted one of the largest revisions to Turkey’s public procurement framework, expanding the scope for restricted tendering procedures and increasing discretionary authority in the allocation of large public works contracts (Gürakar 2016). If democratic backsliding operates partly through the weakening of competitive and transparent procurement institutions, we should observe similar patterns around this reform. Albeit marginally weaker, results using this alternative threshold are substantively unchanged to those in Table 2. Moreover, results using a simple linear time-trend interaction, which also captures the gradual and piecemeal nature of democratic backsliding, albeit more agnostically, are likewise quantitatively similar. See Appendix C.9.

5.4 Counterfactual Scenarios

5.4.1 Estimating Counterfactual Allocations

Additional to serving as a framework for studying the forces that drive distributive politics under backsliding, the model in section 3 also provides a benchmark for simulating *counterfactual* expenditures in the absence of backsliding. Following a similar approach to Burgess et al. (2015), we use these counterfactuals to further test whether backsliding generates systematic deviations in targeted allocation relative to a democratic planner’s benchmark, and hence quantify the magnitude of the resulting political distortions.²²

We estimate counterfactual TOKI expenditures across districts using the equilibrium benchmark derived in Proposition 1 of the theoretical model. This allocation rule suggests that a democratic planner would largely distribute TOKI expenditures according to a population-weighted benchmark (as suggested by panel (a) in Figure 1), normalized by the residual electoral uncertainty spread. Taking logs of equation (3.2), and normalizing allocations in per-capita terms, the empirical analog of the equilibrium democratic allocation rule can be written as

$$\log \left(\frac{T_{dprt}}{n_{dprt}} \right) = \delta_{rt} + \gamma \log n_{dprt} + \beta \log \mu_{dpr} + \rho \mathbf{X}'_{dprt} + u_{dprt}, \quad (5.1)$$

where n_{dprt} represents district-level population, and μ_{dpr} reflects the residual uncertainty spread around AKP vote margins (this is time-invariant given how we estimate μ_{dpr} , see below).^{23,24} Moreover, T_{dprt}/n_{dprt} represents TOKI contract values per capita, as similarly defined in equation (4.1), and we also include covariates $\mathbf{X}'_{dprt}$ to increase precision. We can thus estimate equation (5.1) directly using solely the democratic sample of the data (i.e. all data up until 2010), and based on the sign of the parameters from equation (3.2), we should thus expect $\gamma > 0$ and $\beta < 0$.

From the theoretical model, given we assume the residual of vote margins is distributed $\varepsilon_d \sim [-\mu_d, \mu_d]$, we estimate μ_d by regressing realized AKP vote margins from the democratic sample of the data on fixed effects and covariates, predicting the residual, $\hat{\varepsilon}_{dprt}$, and from the variance of a uniform distribution deriving $\hat{\mu}_{dprt} = \sqrt{3} \hat{\sigma}_{dpr}$, where $\hat{\sigma}_{dpr}$ represents the estimated standard deviation of $\hat{\varepsilon}_{dprt}$ (see Appendix C.10 for further discussion). Plugging $\hat{\mu}_{dpr}$ back into equation (5.1), we can thus use OLS to directly estimate β and γ , thereby allowing us to back out the estimate for the elasticity of public expenditures to votes via parameter inversion (see Appendix C.10 for full derivation and point estimates):

$$\hat{\alpha} = \frac{\hat{\gamma}}{1 + \hat{\gamma}},$$

22. Other studies such as Szeidl and Szucs (2021) have also used counterfactual analysis in backsliding contexts (Hungary) to quantify mechanisms driving distribution, although their paper focuses on media capture.

23. Due to the slow-moving nature of district-level populations, we must omit district fixed effects, α_d , to leave enough identifying variation to pin down the parameter γ . This also enables us to estimate β given μ_{dpr} is time-invariant.

24. Note from equation (3.2) that our subregion $\times$ year fixed effects, δ_{rt} , absorb the logarithms of T and $\sum_{d'} (n_{d'})^{\frac{1}{1-\alpha}}$, given they are not district-specific. See Appendix C.10.

Throughout the core empirical analysis we have relied on subregion $\times$ year fixed effects, δ_{rt} , such that district-level dynamics are always relative to within subregion-year baselines. For consistency, we thus construct counterfactual allocations by redistributing each subregion-year TOKİ budget (i.e. total realized TOKİ expenditures) across districts within the same subregion-year cell. Using the potential outcomes framework, denote $T_{dprt}(0)$ as the counterfactual allocation of TOKİ expenditures for a given district d in year t . Plugging in for all relevant parameters and variables, we thus derive counterfactuals via the following allocation rule:

$$T_{dprt}(0) = T_{rt} \frac{(n_{dprt}/\hat{\mu}_{dpr})^{\frac{1}{1-\hat{\alpha}}}}{\sum_{d' \in r} (n_{d'prt}/\hat{\mu}_{d'pr})^{\frac{1}{1-\hat{\alpha}}}},$$

where $T_{rt} = \sum_{d \in r} T_{dprt}$, and $\hat{\mu}_d$ is our time-average prediction of residual electoral uncertainty under democracy for a given district d .²⁵ Because the counterfactual only reallocates the observed subregion-year “budget” (i.e. realized total TOKİ expenditures), the allocation rule mechanically preserves total expenditures within each subregion-year: $T_{rt} = \sum_{d \in r} T(0)_{dprt}$. Finally, dividing by observed district-level populations, n_{dprt} , provides counterfactuals in per-capita units.

5.4.2 Results

Figure 4 plots the average realized versus counterfactual per-capita allocations for both AKP and non-AKP affiliated districts over time. As per panel (a), we see a striking pattern: during Türkiye’s experience with democracy in the 2000s, observed spending in expectation closely tracks counterfactual allocations, subject to a minor level difference. However, after the onset of backsliding in 2011, there is clear divergence, with observed allocations increasing relative to the counterfactual democratic benchmark.

Panel (b) homes in on non-AKP affiliated districts. Unlike the observed AKP series where we see divergence from counterfactual estimates pre-post backsliding, realized and counterfactual allocations for non-AKP districts clearly trend together over time, subject to some discernible albeit non-overwhelming level difference. Noticeably, the AKP and non-AKP counterfactual series across panels (a) and (b) broadly hover around similar averages of ₺120 ($\approx$ USD 17 in 2020 prices). This clearly suggests that backsliding induced political distortions specifically among AKP-affiliated districts away from a democratic planner’s benchmark. By 2020, AKP-affiliated districts receive almost a threefold increase in expenditures relative to what a democratic planner would distribute.

25. The province, p , subscript vanishes in the summation given districts are nested within unique provinces.

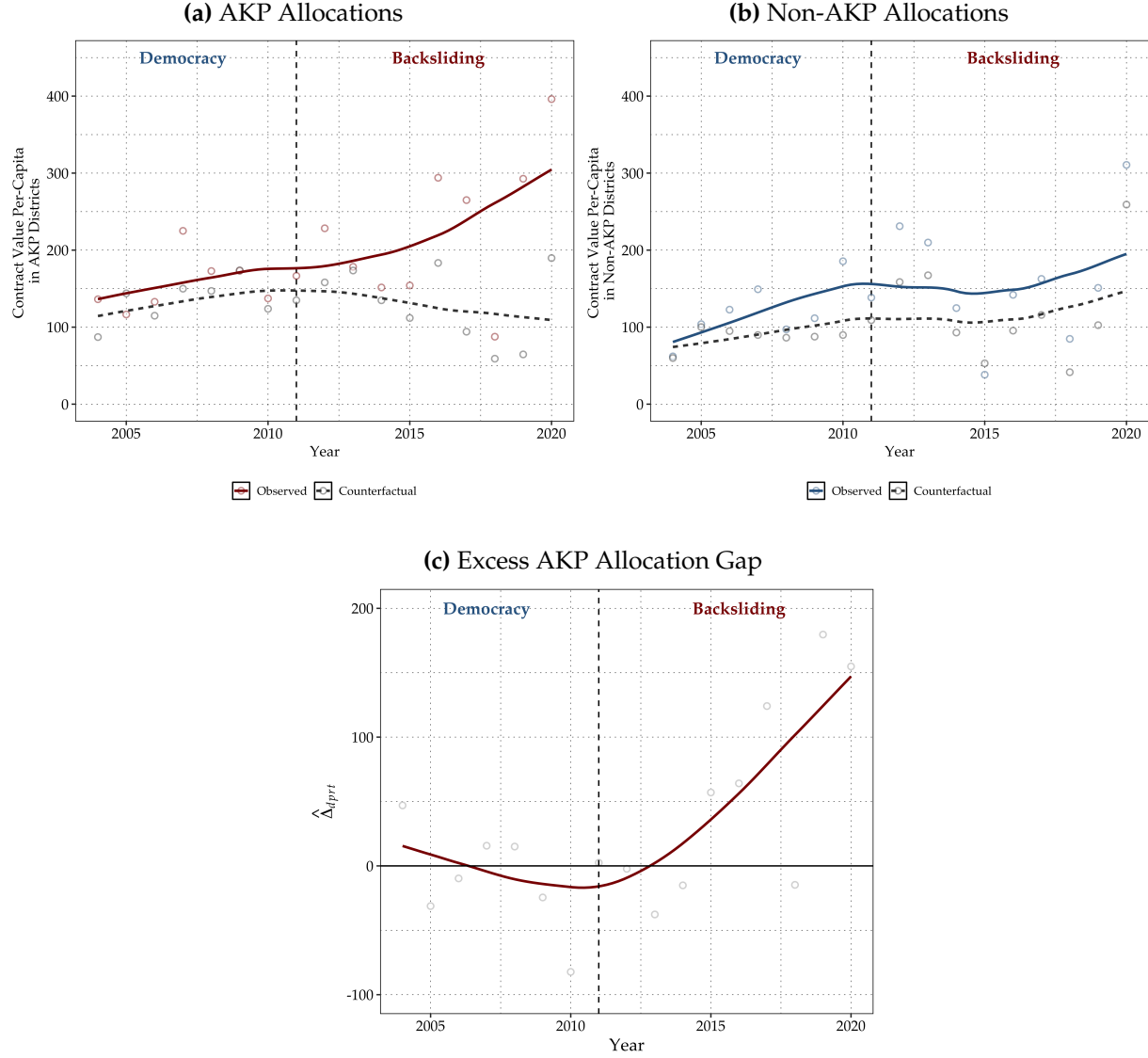


Figure 4: Observed versus Counterfactual Allocations

Note: panel (a) plots the average observed (red line) and counterfactual (black dashed line) allocations for AKP-affiliated districts. Panel (b) plots the average observed (blue line) and counterfactual (black dashed line) allocations for non-AKP-affiliated districts. Panel (c) plots the average estimates of $\hat{\Delta}_t$ from equation (5.2). The vertical dashed line conveys the onset of democratic backsliding. Trend-lines for both panels are LOWESS estimates, and faded dots represent the underlying data. All values on the y-axis refer to 2020 real Turkish liras. Only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Observed total TOKİ expenditures are redistributed within subregion $\times$ year cells.

To further interpret the magnitude of the political distortion from backsliding, we can define $\tau_{dpri} := \frac{T_{dpri} - T^{(0)}_{dpri}}{n_{dpri}}$, the district-level wedge, as the difference between observed and counterfactual TOKİ allocations. Taking the difference in mean τ_{dpri} 's between AKP and non-AKP affiliated districts thus provides a measure of how much excess spending AKP districts receive relative to

non-AKP districts, net of what would be predicted by the counterfactual allocation rule:

$$\hat{\Delta}_t = \mathbb{E}_d[\tau_{dprt} \mid d \in \text{AKP}] - \mathbb{E}_d[\tau_{dprt} \mid d \in \text{Non-AKP}] \quad (5.2)$$

If $\hat{\Delta}_t \approx 0$, this suggests deviations from the counterfactual benchmark are similar across AKP- and non-AKP-affiliated districts, implying little evidence of an AKP-specific excess allocation premium. Alternatively, if $\hat{\Delta}_t \gg 0$, this indicates that AKP-affiliated districts receive a larger observed spending advantage than would be expected under the counterfactual benchmark.

Panel (c) of Figure 4 plots LOWESS trend-lines through estimates of average $\hat{\Delta}_t$'s. We clearly see a striking pattern emerge: before backsliding, there was minimal difference in excess TOKI spending between AKP- and non-AKP-affiliated districts. However, after the onset of backsliding in 2011, the excess AKP spending gap increases over time, with the raw estimate for 2020 suggesting that AKP-affiliated districts receive approximately ₺150 (circa USD 21 in 2020 prices) more TOKI expenditures per capita in expectation than non-AKP-affiliated districts, relative to the counterfactual benchmark.

In Appendix C.11, consistent with Figure 3, we provide uncertainty intervals around the LOWESS estimates. Appendix C.12 reports a leave-one-subregion-out validation exercise. Prediction errors are similar for AKP and non-AKP districts during the democratic period, but increase sharply for AKP districts after 2011, consistent with post-backsliding allocations in aligned districts departing from the democratic allocation rule. In Appendix C.13, we also plot 3-year moving averages to show the visual interpretation in Figure 4 is not solely a product of using scatterplot smoothing. We also show the trends are robust to using province $\times$ year cells to redistribute observed total expenditures (see Appendix C.14). Finally, motivated by the limiting case in Appendix C.15, where homogeneous residual electoral uncertainty implies a population-based democratic allocation benchmark, we estimate a fully apolitical per-capita counterfactual and find that post-backsliding distortions become sharper relative to this stricter benchmark. Finally, similar to Burgess et al. (2015), as an additional placebo test we use both democratic and per-capita counterfactual allocations as an outcome variable, and re-estimate the core pooled specification employed in Table 2 (see Appendix C.10). We consistently estimate null effects, such that AKP incumbency pre-post backsliding has no explanatory power on counterfactual allocations.

6 Heterogeneous Effects

The theoretical model's core prediction is an average alignment premium of expenditure under backsliding. It does not, however, fully specify where within the set of aligned districts this premium should be largest. We therefore use heterogeneity analyses to probe the political content of the targeting technology, $\lambda(\sigma)$. First, we examine whether effects differ by the type of electoral transition that produces alignment, distinguishing switches from the main opposition, switches from other parties, first AKP victories, and AKP re-election. Second, we examine whether effects

vary by realized vote margins, which provide an observable proxy for the responsiveness channel captured by μ_d in the model.

6.1 Specific Switches

Although the model treats alignment as binary, following the literature on electoral/competitive authoritarianism, we might expect districts that experience switches from key opposition parties to experience more targeted distribution of public goods and services (Levitsky and Way 2010). In the context of Türkiye, evidence suggests districts affiliated with the AKP’s main rivals, the CHP, have systematically received fewer public resources (Cammatt and Luca 2018; Lıcalı 2014).

Given Türkiye is a multi-party environment, we can decompose the core treatment variable of switches to the AKP in equation (4.1) in four ways: i) switches from the CHP to the AKP, ii) switches from all other opposition parties to the AKP, iii) the first election the AKP won a given seat (mainly comprising switches in 2004), and iv) re-election (only applicable to the 2009, 2014 and 2019 elections). Although it is possible to unpack the result across all relevant opposition parties, we focus on the CHP as the main opposition party and then aggregate all other opposition parties into a single category. The core treatment variable in equation (4.1), m_{dprt}^{AKP} , can thus be dummied out into these four separable variables:

$$m_{dprt}^{AKP} = m_{dprt}^{CHP \rightarrow AKP} + m_{dprt}^{Non-CHP \rightarrow AKP} + m_{dprt}^{First\ Win} + m_{dprt}^{Re-election} \quad (6.1)$$

Define the set $\mathcal{K} = \{CHP \rightarrow AKP, Non-CHP \rightarrow AKP, First\ Win, Re-election\}$, such that $m_{dprt}^{AKP} = \sum_{k \in \mathcal{K}} m_{dprt}^k$. Plugging the above regressors into equations (4.1) and (4.2), we can thus run the following specifications:

$$y_{dprt} = \alpha_d + \delta_{rt} + \sum_{k \in \mathcal{K}} \beta_k m_{dprt}^k + \sum_{k \in \mathcal{K}} \theta_k \left(m_{dprt}^k \times \mathbf{1}\{t < 2011\} \right) + \rho \mathbf{X}'_{dprt} + u_{dprt} \quad (6.2)$$

$$h_{dprt} = \exp \left(\alpha_d + \delta_{rt} + \sum_{k \in \mathcal{K}} \beta_k m_{dprt}^k + \sum_{k \in \mathcal{K}} \theta_k \left(m_{dprt}^k \times \mathbf{1}\{t < 2011\} \right) + \rho \mathbf{X}'_{dprt} \right) \cdot u_{dprt} \quad (6.3)$$

Here, k indexes the type of switch, where β_k now represents the effect for a given type of switch k under backsliding, whereas θ_k shows the effect under democracy. An F-test or Chi-squared test for the null hypothesis, $H_0 : \hat{\beta}_k + \hat{\theta}_k = 0$, again provides a sharper test for the moderating effect of democratic institutions on targeted TOKİ allocation, but now specifically for a given switch type k . Running these specifications thus allows us to transparently estimate the effect of AKP incumbency in local elections heterogeneously with respect to i) specific opposition parties, ii) the AKP’s first victory in a district, and iii) re-election(s). The control group remains districts that never switch to the AKP (i.e. “never-treated” units), in addition to districts where the AKP lose a given election to an opposition party.

Figure 5 outlines the individual estimates for β_k and θ_k . Almost all panels continue to provide evidence for heterogeneous effects pre-post democratic backsliding. Strikingly, for the CHP, all

panels show a negative and statistically significant effect for CHP-AKP switches under democracy. These coefficients are roughly equal in magnitude and opposite in sign to the estimates under backsliding.

This suggests the more democratic institutional environment not only prevented general targeting across all AKP mayors as found in Table 2 on average, but sharply moderated targeting specifically towards districts where the AKP won against the main opposition group. The F-test and Chi-squared test statistics at the bottom of the panels provide a sharper test of this argument, where for CHP-AKP switchers we fail to reject the null hypothesis $H_0 : \hat{\beta}_k + \hat{\theta}_k = 0$.

Additional to the CHP, we also find that re-election is significant in the backsliding period, although there is less evidence of moderation for these switchers when Türkiye was a more genuine democracy (which would only be applicable to the 2009 election). This is a similar pattern for districts where the AKP win for the first time, with these estimates across all panels being marginally significant at the 5% level under backsliding.

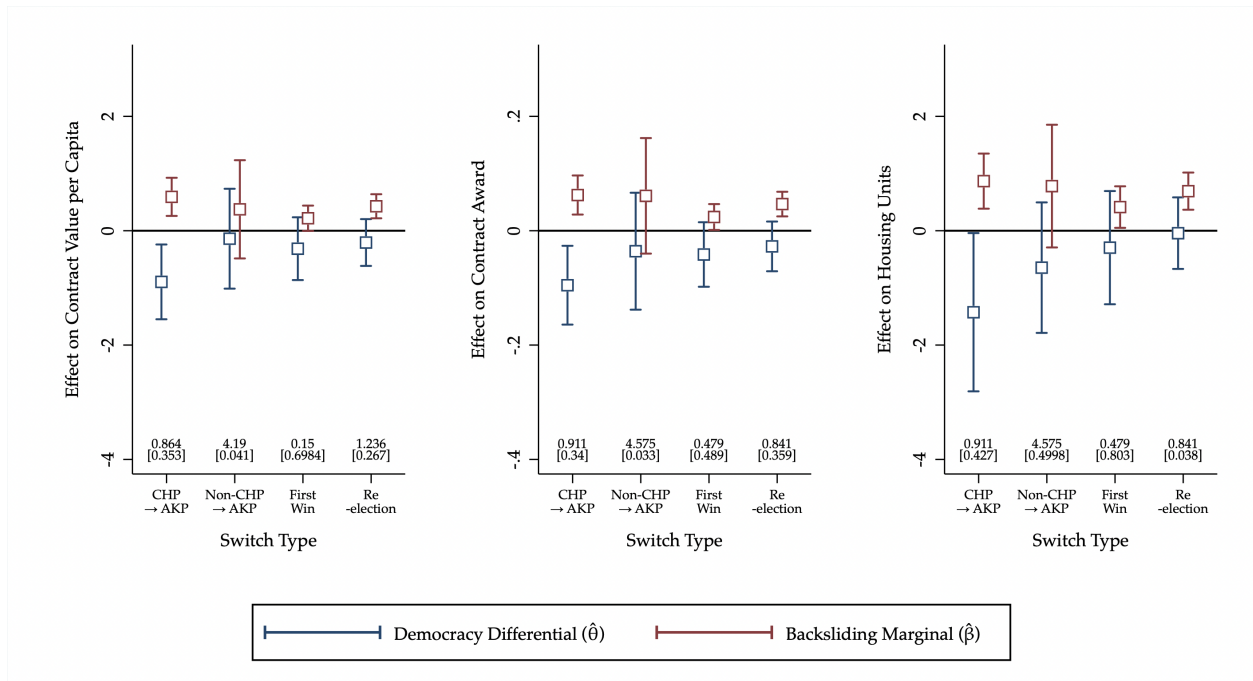


Figure 5: Party Switches Pre-Post Democratic Backsliding

Note: 95% confidence intervals plotted using robust standard errors clustered by district. Estimates for all panels come from the same specification. All panels include district and subregion $\times$ year fixed effects, in addition to municipality election controls, general election controls and district controls. The left and middle panels use TWFE, and the right panel uses Poisson QMLE. The numbers at the bottom outline F-test statistics for the left and middle panel, and the Chi-squared test statistic for the right panel, with corresponding p -values in square parentheses. See Appendix E for estimates of all control variables.

6.2 Vote Margins

As a final source of heterogeneity, we now explore the potential moderating impact of vote margins. In the Turkish political context, evidence suggests vote margins have played some role in distorting the allocation of public resources, such as bank credit (Bircan and Saka 2021). The theoretical model implies that under democracy, allocation should partly follow a swing-district logic: a handful of districts with lower residual electoral uncertainty, μ_d , have higher marginal returns to public investment and may therefore receive greater expenditures. Realized vote margins provide an observable, though imperfect, proxy for this latent responsiveness. Under backsliding, however, the targeting technology, $\lambda(\sigma)$, introduces an additional alignment-based return to spending. Heterogeneity by margin size thus tests whether under democracy allocation is concentrated in electorally competitive districts, and whether there is a departure from this logic once backsliding strengthens alignment-based targeting.

We can now decompose the core treatment variable of m_{dprt}^{AKP} in equation (4.1) with respect to vote margin size.²⁶ We dummy out the main treatment variable of AKP incumbency in equations (4.1) and (4.2) into four sub-treatment variables: razor-thin switches (vote margin of $(0\% - 1\%]$), competitive switches (vote margin of $(1\% - 5\%]$), moderate switches (vote margin of $(5\% - 10\%]$) and safe switches (vote margin of $> 10\%$). Hence, we have that:

$$m_{dprt}^{AKP} = m_{dprt}^{(0\%-1\%]} + m_{dprt}^{(1\%-5\%]} + m_{dprt}^{(5\%-10\%]} + m_{dprt}^{>10\%} \quad (6.4)$$

Redefining the set $\mathcal{K} = \{(0\% - 1\%], (1\% - 5\%], (5\% - 10\%], > 10\%\}$, we can thus re-run the specifications in equations (6.2) and (6.3). This subsequently allows us to study whether pre-post a given type of switch to the AKP based on size of the vote margin, both under backsliding and democracy, how are TOKİ contracts allocated to these mayors. This is again relative to the control group of districts that never switch to the AKP or where the AKP lose to an opposition party.

Figure 6 outlines the results. The left-hand panels focus on the estimates including the interaction with the democracy dummy (i.e. the total effect), and the right-hand panels show coefficients solely for the “base” effects (i.e. effects under post-2010 backsliding). To obtain the coefficients under Türkiye’s democratic period, we take a linear combination of the base and interacted versions for each variable and then calculate respective standard errors using the delta method.²⁷

26. Although one could decompose treatment effects jointly by party switch and vote margin (e.g., competitive CHP-to-AKP switches, safe non-CHP-to-AKP switches, or AKP re-election under moderate margins), doing so would require estimating sixteen treatment indicators in the same specification. Many cells contain few observations, thus leaving limited power to identify meaningful heterogeneous effects.

27. For example, for $\beta_1 m_{dprt}^{(0\%-1\%]} + \theta_1 \left(m_{dprt}^{(0\%-1\%]} \times \mathbf{1}\{t < 2011\} \right)$, we take the linear combination of $\hat{\beta}_1 + \hat{\theta}_1$, and then calculate $\sqrt{\widehat{\text{Var}}(\hat{\beta}_1 + \hat{\theta}_1)}$.

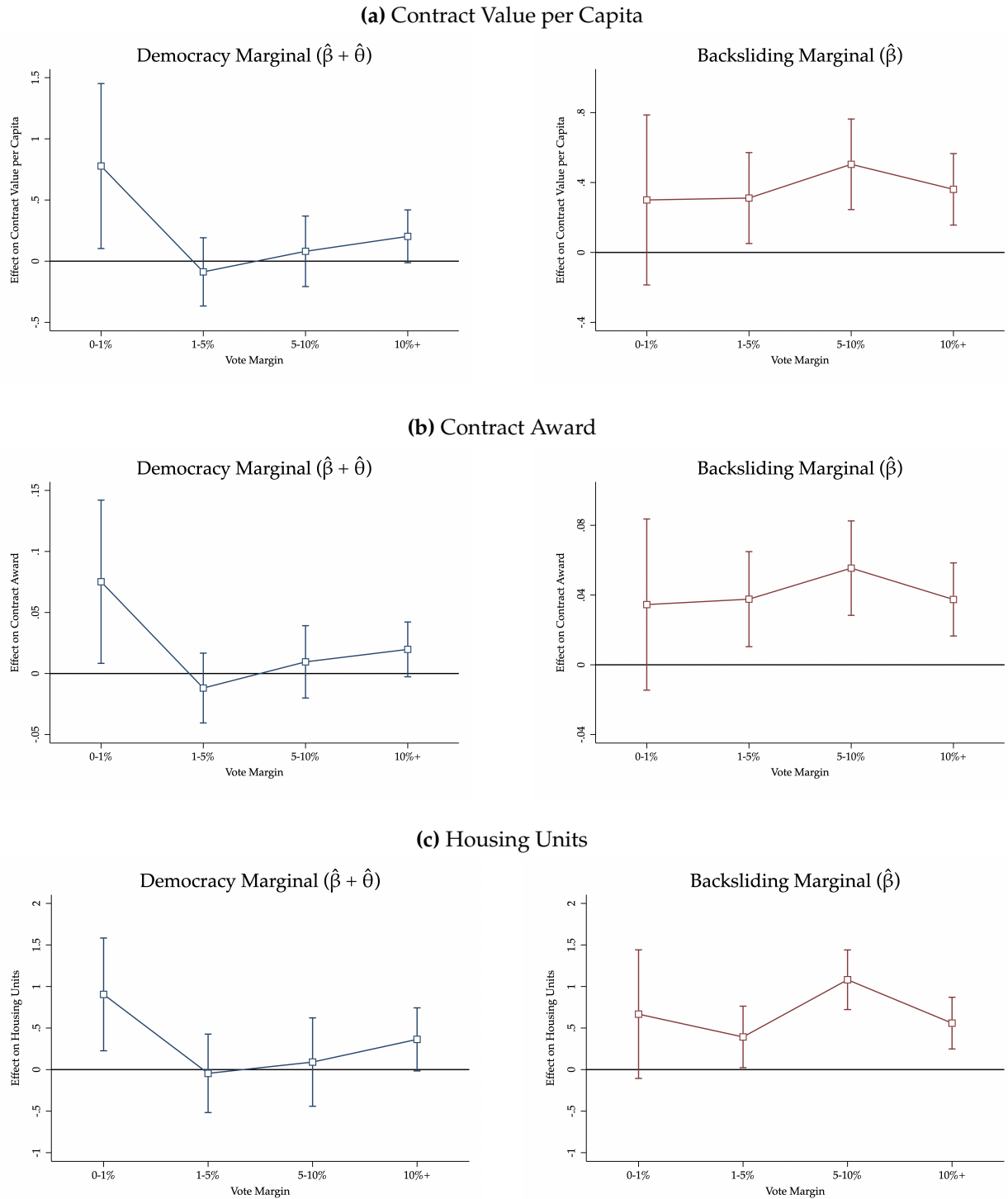


Figure 6: Heterogeneity in AKP Mayoral Incumbency by Vote Margin

Note: panel A uses the TWFE specification in column 2 from Table 2. Panel B uses the TWFE specification in column 4 from Table 2. Panel C uses the Poisson QMLE specification in column 6 from Table 2. Estimates for both right-hand and left-hand panels come from the same specification. The estimates under democracy are calculated using the delta method by taking a linear combination of the base and interacted effects. See Appendix E for estimates of all control variables.

Across all three measures, using TWFE for contract value per capita and the contract dummy alongside Poisson QMLE for total housing units awarded, we see a striking pattern: only razor-thin districts under democracy receive any TOKİ allocation following local election outcomes, but not under backsliding. On the other hand, during the period of Türkiye’s institutional decline, targeting shifts towards less competitive districts, with largest effect sizes concentrated among seats with vote margins between 5-10%, followed by very safe districts with margins greater than 10%.

7 Regression Discontinuity Design

The previous subsection showed that the impact of AKP incumbency on the allocation of TOKİ projects has increased over time, yet the role of vote margins as a mediating variable has evolved. At the height of Turkish democracy in the 2000s, “classic” tactics of distributive politics is present in the data vis-à-vis TOKİ allocation, such that the incumbent party targets swing districts (Lindbeck and Weibull 1987). However, as Türkiye has eroded democratically, the AKP has begun allocating TOKİ contracts to those districts that are seemingly less competitive in local elections.

The coefficients for the 0-1% margin districts in Figure 6 provided initial evidence of targeting among razor-thin districts, although this relied on within-district variation. In this section, as a final source of identification, we thus run a “coin flip” election regression discontinuity (RDD) design to further test this pattern. The RDD for studies of distributive politics is not only useful for more credible identification, such that winning a given constituency is quasi-random, but serves as a direct test of the extent to which swing districts receive more public services after an election.

Let $y_{dpre}(1)$ denote the potential outcome of TOKİ allocation when the AKP wins a district d in election $e \in \{2004, 2009, 2014, 2019\}$, and $y_{dpre}(0)$ as the potential outcome when the AKP loses. Denote the AKP’s vote margin against the second largest party within a given district-election as v_{dpre}^{AKP} , which we use as the running variable with cut-off at 0. We now estimate the following (local average) treatment effect, exploiting cross-sectional variation in districts based on vote margin size, and do this for each election:

$$\beta_e = \lim_{v_{dpre}^{AKP} \downarrow 0} \mathbb{E}[y_{dpre}(1)|v_{dpre}^{AKP} = 0] - \lim_{v_{dpre}^{AKP} \uparrow 0} \mathbb{E}[y_{dpre}(0)|v_{dpre}^{AKP} = 0] \quad (7.1)$$

To estimate β_e per election, we collapse our annual dataset into election-year bins, summing all expenditures between election cycles, inclusive of the election year itself.²⁸ We thus focus on TOKİ allocation across all years both including and after a local election has occurred. For contract value per capita, we sum total values across years within an election cycle, and then use the population of the election year to normalize values in per capita terms.

28. We sum all expenditures within a district between 2004-2008 for the 2004 election, 2009-2013 for the 2009 election, 2014-2018 for the 2014 election, and 2019-2020 for the 2019 election.

We use [Calonico et al. \(2017\)](#)’s package to estimate equation (7.1) via kernel-weighted least squares. We also apply the method of [Imbens and Wager \(2019\)](#), which uses a finite min–max procedure to choose the bandwidth that minimizes the worst-case mean squared error. Given both methods rely on a smoothness assumption of the conditional expectation function via a local linear framework, we focus on contract values per capita and the contract award dummy as our core outcome variables.

Table 3 shows the results when running the RDD per election. For the 2004 election in panel (a), we clearly see very strong results. Across all specifications, districts where an AKP mayoral candidate marginally won in 2004 saw a large increase in TOKİ contracts over the post-election period. Using preferred specification of column 3, we find that districts where an AKP candidate marginally won receives TOKİ contracts that are approximately fivefold more valuable per capita, relative to districts where the AKP marginally lost, over the post-election period. On the extensive margin, when looking at column 6, highly competitive districts saw a large 16.2 percentage point increase in the probability of being allocated a contract by TOKİ in the post-election period.

For all other elections in panels (b), (c) and (d), we see no strong evidence to suggest there was targeting in razor-thin districts. All estimates are insignificant and imprecisely estimated. Given the insignificance of the results in panel (b) for the 2009 election, despite being held just before Türkiye’s institutional slide, the results for the 0-1% margin districts in Figure 6 thus appear to be driven exclusively by switches to the AKP in the 2004 election.

Figure 7 visualizes the discontinuity for each election using local polynomials. We focus on contract value per capita but show the same figures for the contract award dummy in Appendix D.1 for completeness. As shown in the top-left panel for the 2004 election, there is a discontinuous jump at the 0% vote margin threshold. For all other elections, where the post-election period overlaps with Türkiye’s backsliding period, the discontinuity is far less evident, with confidence intervals overlapping substantially.

This reaffirms the conclusions raised in the previous subsection. At the height of Turkish democracy, vote margins appeared to play a stronger role in the post-election distribution of TOKİ projects. On the other hand, tight electoral margins have no impact on allocation in post-election periods besides the 2004 election, once Türkiye enters a more non-democratic institutional environment. In expectation, although the AKP have in some capacity always rewarded incumbents at the local level with TOKİ projects, the nature of targeted distribution has thus shifted as the country has democratically declined.

We again run a series of robustness checks. In Appendix D.2, running [McCrary \(2008\)](#)’s density test for each election shows no sorting near the vote margin threshold. Using the log of population, prior general election vote shares, and urban settlements as alternative outcome variables, we find consistent null effects, thus suggesting the discontinuities in Figure 7 are not coincident with other district-level characteristics (see Appendix D.3). Arbitrarily changing the cut-off in Appendix D.4 to ± 0.05 also reveals general null effects, providing confidence that tight elections where margins

are close to 0 provides a non-artificial discontinuity, especially for the 2004 election where we find significant estimates. The results for the 2004 election are also insensitive to varying the treatment bandwidth, as shown in Appendix D.5.

Table 3: Close Election RDD Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Contract Value per Capita			Contract Award		
<i>Panel A - 2004</i>						
AKP Mayor	3.193*** (1.094)	3.958*** (1.390)	1.623*** (0.499)	0.320*** (0.105)	0.367*** (0.129)	0.162*** (0.049)
Observations	921	921	921	923	923	923
<i>Panel B - 2009</i>						
AKP Mayor	-0.916 (0.999)	-1.487 (1.291)	0.653 (0.493)	-0.044 (0.097)	-0.087 (0.128)	0.069 (0.050)
Observations	952	952	952	952	952	952
<i>Panel C - 2014</i>						
AKP Mayor	-0.853 (0.906)	-1.005 (1.280)	0.508 (0.476)	-0.084 (0.089)	-0.084 (0.125)	0.030 (0.049)
Observations	970	970	969	970	970	969
<i>Panel D - 2019</i>						
AKP Mayor	1.090 (0.912)	1.045 (1.295)	0.405 (0.452)	0.143 (0.091)	0.139 (0.127)	0.050 (0.046)
Observations	973	973	973	973	973	973
Polynomial Method	Linear CCT	Cubic CCT	– IW	Linear CCT	Cubic CCT	– IW

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors in parentheses. Columns 1-2 and 4-5 use [Calonico et al. \(2017\)](#)'s (CCT) method. Columns 3 and 6 use [Imbens and Wager \(2019\)](#)'s (IW) method. Columns 1 and 4 use a linear polynomial, whereas columns 2 and 5 use a cubic polynomial.

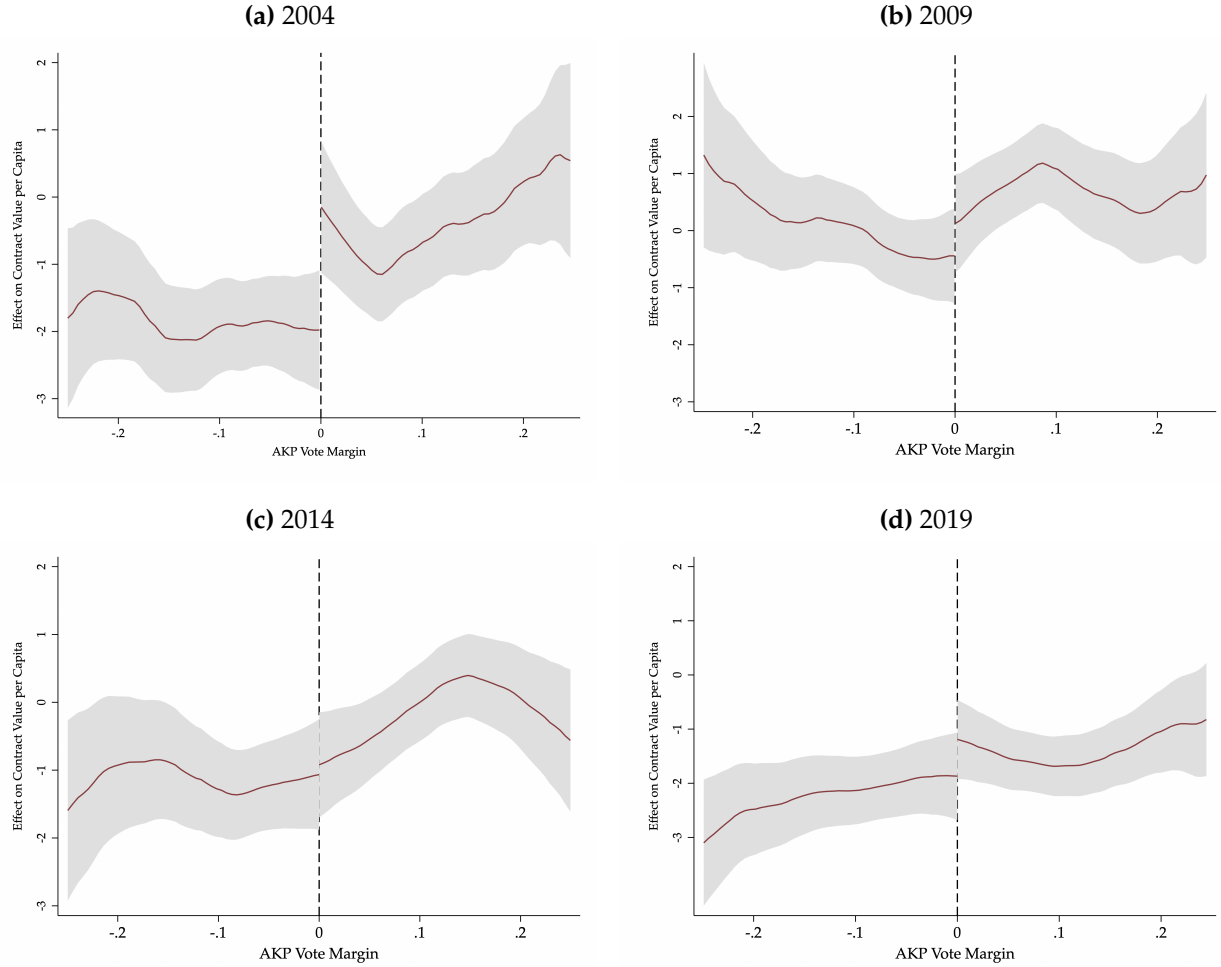


Figure 7: RDDs per Local Election for Contract Value per Capita

Note: each panel plots local polynomial fits per election above and below the vote margin threshold with shaded 95% confidence intervals. See Appendix D.1 for similar figures using the contract award dummy variable as the outcome variable.

8 Conclusion

This paper has documented how the regression from democracy towards electoral authoritarianism in Türkiye has increased the targeted distribution of TOKİ projects towards the incumbent dominant AKP. As Türkiye has shifted away from democracy since the 2010s, we find robust evidence across a variety of empirical strategies that incumbent AKP mayors following local elections receive greater TOKİ allocation on both the intensive and extensive margin. Effect sizes are strongest for districts that win against a previous incumbent affiliated with the main opposition party, the CHP, although democracy played a strong moderating role in overall targeting. As competitive democracy has eroded, strategies of distribution have also departed from a swing district logic.

Our main contribution is to highlight the material consequences that can arise from democratic backsliding. Although there are several debates as to whether indices of democracy accurately convey a global trend toward greater authoritarianism, a fruitful avenue, which has been the focus of this paper, is to explore the material consequences of institutional erosion on public investment (Little and Meng 2024b). How democratic backsliding has shaped allocation incentives among other autocratizing economies embarking upon major infrastructure pipelines, such as India's 'National Infrastructure Pipeline' under Narendra Modi's Bharatiya Janata Party (BJP) or Serbia's public investment drive under Aleksandar Vučić's Serbian Progressive Party (SNS), is open to further investigation.

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Online Appendix

**Distributive Consequences of Democratic Backsliding:
Evidence from the Turkish Mass Housing Administration
(TOKİ)**

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Appendix A Data Background

Appendix A.1 Democratic Backsliding in Türkiye

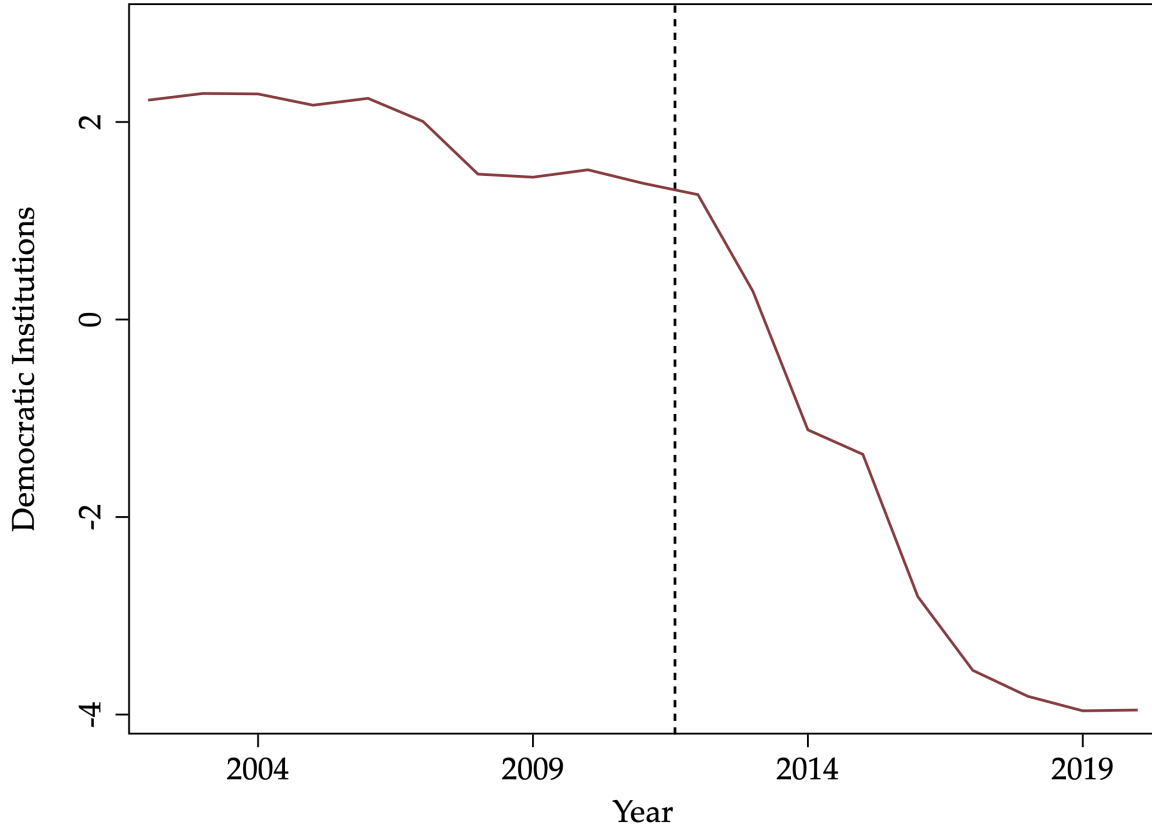


Figure A.1: Democratic Institutions in Türkiye, 2004-2020

Note: democratic institutions are measured using the first principal component of seven indices from V-Dem: electoral democracy (*v2x_polyarchy*), freedom of expression and alternative sources of information (*v2x_freexp_altinf*), free and fair national elections (*v2xel_frefair*), judicial constraints (*v2x_jucon*) and legislative constraints on executive power (*v2xlg_legcon*), free and fair subnational elections (*v2elffelr*) and state ownership of the economy (*v2clstown*). Vertical dashed line outlines the onset of democratic backsliding as of June 2011 (Acemoğlu and Üçer 2020).

Appendix A.2 Summary Statistics

Table A.1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
	Obs.	Mean	Std. Dev.	Min.	Max.
Contract Value per Capita	16,162	36.93	213.3	0	13,169
Contract Award	16,196	0.144	0.351	0	1
Housing Units	16,196	33.23	191.4	0	7,588
AKP Mayor	16,171	0.557	0.497	0	1
AKP Vote Margin	16,171	0.0137	0.203	-0.840	0.745
CHP to AKP Switch	16,126	0.0386	0.193	0	1
Non-CHP to AKP Switch	16,126	0.172	0.378	0	1
First Win	16,126	0.0937	0.291	0	1
Re-election	16,126	0.252	0.434	0	1
Very Close Election ($m_{dpt}^{(0\%-1\%)}$)	16,171	0.0265	0.161	0	1
Close Election ($m_{dpt}^{(1\%-5\%)}$)	16,171	0.0970	0.296	0	1
Moderately Competitive Election ($m_{dpt}^{(5\%-10\%)}$)	16,171	0.111	0.314	0	1
Safe Election ($m_{dpt}^{>10\%}$)	16,171	0.322	0.467	0	1
Population	16,162	79,703	132,565	1,473	1.018e+06
Turnout Rate (local election)	16,171	0.847	0.0765	0.000719	1
Prop. of Invalid Votes (local election)	16,126	0.0407	0.0129	0.00458	0.209
AKP Municipal Mayor	16,196	0.631	0.483	0	1
Turnout Rate (general election)	16,071	0.841	0.0707	0.356	1.017
Prop. of Invalid Votes (general election)	16,071	0.0273	0.0139	0	0.233
AKP Vote Share (general election)	16,071	0.455	0.175	0.00917	0.959
log(Urban Settlements)	16,181	0.690	2.084	-4.605	5.550
Disaster	16,196	0.0197	0.139	0	1
First-Hand House Sale	3,353	1,034	1,589	1	23,580
Second-Hand House Sale	3,353	1,397	2,253	0	26,338

Note: Mean values refer to pooled 2004-2020 time averages, with corresponding standard deviations, minimums and maximums. AKP incumbent refers to our core independent variable of interest, whether a district has an incumbent AKP mayor. AKP central mayor refers to either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities.

Appendix A.3 Description of TOKİ Project Types

- Agricultural Village (*tarımköy*) projects develop rural settlements with agricultural infrastructure.
- Consultancy Services (*danışmanlık hizmetleri*) involve architectural, engineering, and project management contracts.
- Disaster Relief (*afet konut*) projects are constructions of new housing units following earthquakes, floods, and wildfires.
- Government Housing (*idare konut*) consists of mass housing projects.
- Government Services (*kamu hizmet*) comprise construction of government buildings.
- Infrastructure and Social Facilities (*altyapı ve/veya sosyal donatı*) consists of rehabilitation centers, hospitals, health centers, gyms, nuclear research centers, and universities.
- Low Income Housing (*alt gelir*) comprises housing built for lower-income households.
- Maintenance and Repair (*bakım onarım*) consists of refurbishing and repairing existing buildings and facilities.
- Ministry of Education (*protokol meb*) projects include schools and education centers.
- Other (*diğer*) is a catch-all residual category for projects not fitting into the above groups.
- Public Gardens and Parks (*millet bahçesi*) refers to large public park developments.
- Replenishment (*ikmal ihalesi*) projects include state hospitals, police stations, dorms, and job training centers.
- Requested/Demand-Based (*talep organizasyon*) consists of mass housing and landscaping projects initiated at the request of local authorities or communities.
- Restoration (*restorasyon*) projects focus on the preservation and renovation of historical or culturally significant buildings.
- Shared Income (*gelir paylaşımı*) projects include apartments, entertainment centers, schools, and mosques, built under revenue-sharing agreements.
- Social Housing (*konut sosyal donatı*) involve amenities and community facilities associated with housing complexes (e.g., parks, playgrounds, cultural centers).
- Stadiums (*stadyum*) include the construction or renovation of sports stadiums and arenas.
- Urban Transformation/Renewal (*kentsel dönüşüm*) includes housing projects, landscaping, and shopping centers as part of urban renewal schemes.

Appendix A.4 Subregional Total Contract Values (2004-2020)

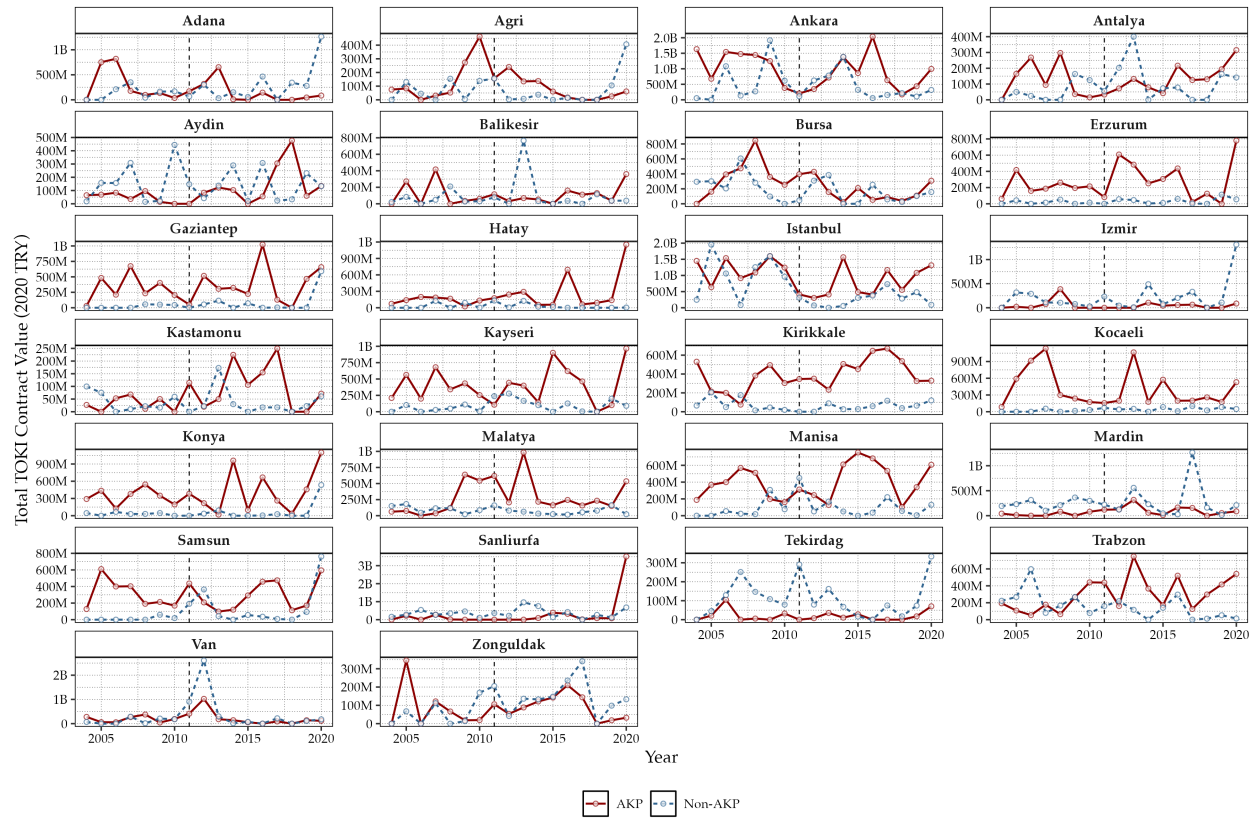


Figure A.2: Total Contract Values by Subregion (2004-2020)

Note: data plots total contract values per year across subregions between 2004-2020. Red line refers to total contract values for AKP-affiliated districts. Blue line refers to total contract values for non-AKP-affiliated districts. All values refer to 2020 Turkish liras. Vertical dashed line indicates the onset of backsliding circa 2011. Subregions refer to NUTS-2 units.

Appendix B Model Derivations

Appendix B.1 Equilibrium Allocations

The incumbent's objective function is:

$$\max_{\{T_d, \sigma\}} \sum_d \left[\frac{1}{2} - \frac{n_d \Delta_d}{2\mu_d(\sigma)\phi} \right] \quad \text{s.t.} \quad \sum_d T_d^k \leq T, \quad \sigma \geq 0. \quad (\text{B.1})$$

Recall σ is set in the first stage of the game, and so the only constraint is the budget constraint, T .

Democratic Allocation Under democracy ($\sigma = 0$), we know that $g_d^k = T_d^k$ (i.e. there is no “targeting technology”) for both parties k . Focusing on the incumbent party A , and only focusing on relevant terms, we can set up the Lagrangian:

$$\mathcal{L} = \sum_d \frac{n_d}{2\mu_d(0)\phi} (T_d^A)^\alpha + \nu \left(T - \sum_d T_d^A \right)$$

For a single district d , we thus have:

$$\frac{\partial \mathcal{L}}{\partial T_d^A} = \frac{\alpha n_d}{2\mu_d(0)\phi} (T_d^A)^{\alpha-1} - \nu$$

Setting the partial derivative equal to 0 to find the first order condition, we thus have:

$$T_d^A = K \left(\frac{n_d}{\mu_d(0)} \right)^{\frac{1}{1-\alpha}}, \quad \text{where } K = \left(\frac{\alpha}{2\phi\nu} \right)^{\frac{1}{1-\alpha}}$$

Plugging T_d^A back into the budget constraint:

$$K \sum_d \left(\frac{n_d}{\mu_d(0)} \right)^{\frac{1}{1-\alpha}} = T$$

Solving for K and finally plugging back into T_d^A , we thus have that:

$$\tilde{T}_d^A(0) = T \frac{\left(\frac{n_d}{\mu_d(0)} \right)^{\frac{1}{1-\alpha}}}{\sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(0)} \right)^{\frac{1}{1-\alpha}}}$$

And given the problem is symmetric across parties, then $\tilde{T}_d^A(0) = \tilde{T}_d^C(0)$.

Backsliding Allocation Under backsliding ($\sigma > 0$), the utility differential can now vary based on the alignment of a given district:

$$\Delta_d = \begin{cases} (T_d^C)^\alpha - [(1 + \lambda(\sigma))T_d^A]^\alpha & \text{if } d \in \mathcal{I} \\ (T_d^C)^\alpha - (T_d^A)^\alpha & \text{if } d \notin \mathcal{I} \end{cases}$$

The objective function of the incumbent thus varies across districts:

$$\max_{\{T_d, \sigma\}} \sum_d \left\{ \frac{1}{2} - \frac{n_d}{2\mu_d(\sigma)\phi} \left[(T_d^C)^\alpha - [(1 + \lambda(\sigma))T_d^A]^\alpha \right] \right\} + \quad (\text{B.2})$$

$$\sum_d \left\{ \frac{1}{2} - \frac{n_d}{2\mu_d(\sigma)\phi} \left[(T_d^C)^\alpha - (T_d^A)^\alpha \right] \right\} \quad \text{s.t.} \quad \sum_d T_d^k \leq T, \quad \sigma \geq 0. \quad (\text{B.3})$$

To simplify notation, we can define:

$$f_d(\sigma) = \begin{cases} \frac{n_d}{2\mu_d(\sigma)\phi} (1 + \lambda(\sigma))^\alpha & \text{if } d \in \mathcal{I} \\ \frac{n_d}{2\mu_d(\sigma)\phi} & \text{if } d \notin \mathcal{I} \end{cases} \quad (\text{B.4})$$

First focusing on the incumbent's allocations, we can set up the Lagrangian:

$$\mathcal{L} = \sum_d f_d(\sigma) (T_d^A)^\alpha + \nu \left(T - \sum_d T_d^A \right) \quad (\text{B.5})$$

Taking the first order condition and solving for $(T_d^A)^\alpha$:

$$T_d^A = \left[\frac{\alpha}{\nu} f_d(\sigma) \right]^{\frac{1}{1-\alpha}} \quad (\text{B.6})$$

Now plugging in respective values of $f_d(\sigma)$:

$$T_d^A = \begin{cases} K \left[(1 + \lambda(\sigma))^\alpha \left(\frac{n_d}{\mu_d(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} & \text{if } d \in \mathcal{I} \\ K \left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}} & \text{if } d \notin \mathcal{I}, \end{cases} \quad (\text{B.7})$$

where constant $K = \left(\frac{\alpha}{2\phi\nu} \right)^{\frac{1}{1-\alpha}}$. Plugging T_d^A back into the budget constraint:

$$T = \sum_{d \in \mathcal{I}} T_d^A + \sum_{d \notin \mathcal{I}} T_d^A \quad (\text{B.8})$$

$$= \sum_{d \in \mathcal{I}} K \left[(1 + \lambda(\sigma))^\alpha \left(\frac{n_d}{\mu_d(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} + \sum_{d \notin \mathcal{I}} K \left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}} \quad (\text{B.9})$$

Solving for K :

$$K = T \left(\frac{1}{\mathcal{Z}(\sigma)} \right), \quad (\text{B.10})$$

where $\mathcal{Z}(\sigma) = \sum_{d \in \mathcal{I}} \left[(1 + \lambda(\sigma))^\alpha \left(\frac{n_d}{\mu_d(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} + \sum_{d \notin \mathcal{I}} \left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}}$. Finally, plugging K back into T_d^A provides the equilibrium allocations:

$$\tilde{T}_d^A(\sigma) = \begin{cases} T \left(\frac{1}{\mathcal{Z}(\sigma)} \right) \left[(1 + \lambda(\sigma))^\alpha \left(\frac{n_d}{\mu_d(\sigma)} \right) \right]^{\frac{1}{1-\alpha}} & \text{if } d \in \mathcal{I} \\ T \left(\frac{1}{\mathcal{Z}(\sigma)} \right) \left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}} & \text{if } d \notin \mathcal{I} \end{cases} \quad (\text{B.11})$$

For party C , given $\Delta_d = (T_d^C)^\alpha - (T_d^A)^\alpha$ for $\forall d$, then:

$$\tilde{T}_d^C(\sigma) = T \frac{\left(\frac{n_d}{\mu_d(\sigma)} \right)^{\frac{1}{1-\alpha}}}{\sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(\sigma)} \right)^{\frac{1}{1-\alpha}}} \text{ for } \forall d.$$

Appendix B.2 Optimal Backsliding

We can explicitly make institutions an argument of the utility differential. After the second-stage allocation game is solved, define the equilibrium utility differential as

$$\Delta_d^* \equiv \frac{1}{\alpha} \left(\tilde{T}_d^C(\sigma) \right)^\alpha - \frac{1}{\alpha} \left[(1 + \lambda(\sigma)) \tilde{T}_d^A(\sigma) \right]^\alpha + c_d^A(\sigma), \quad (\text{B.12})$$

where we can plug in for equilibrium allocations, $(\tilde{T}_d^A(\sigma), \tilde{T}_d^C(\sigma))$, given this is determined later in the game and so solved “earlier” by backward induction. The incumbent’s first-stage value function is therefore

$$\mathcal{V}(\sigma) = \sum_d \left[\frac{1}{2} - \frac{n_d}{2\phi\mu_d(\sigma)} \Delta_d^*(\sigma) \right]. \quad (\text{B.13})$$

To test whether democracy is locally optimal, evaluate the derivative of $\mathcal{V}(\sigma)$ at the boundary $\sigma = 0$. Using the quotient rule,

$$\frac{\partial \mathcal{V}(\sigma)}{\partial \sigma} = - \sum_d \frac{n_d}{2\phi\mu_d(\sigma)} \left(\frac{\partial \Delta_d^*(\sigma)}{\partial \sigma} \right) + \sum_d \frac{n_d \Delta_d^*(\sigma)}{2\phi\mu_d(\sigma)^2} \left(\frac{\partial \mu_d(\sigma)}{\partial \sigma} \right) \quad (\text{B.14})$$

At democracy ($\sigma = 0$), we know there is no targeting technology, $\lambda(0) = 0$, there is no cost to backsliding by definition, $c_d^A(0) = 0$, and equilibrium allocations across parties are symmetric, $\tilde{T}_d^A(0) = \tilde{T}_d^C(0)$, hence $\Delta_d^*(0) = 0$. The second summation from the derivative thus drops out, leaving us with:

$$\frac{\partial \mathcal{V}(0)}{\partial \sigma} = - \sum_d \frac{n_d}{2\phi\mu_d(0)} \left(\frac{\partial \Delta_d^*(0)}{\partial \sigma} \right) \quad (\text{B.15})$$

Since allocations are equilibrium second-stage choices, the marginal effect of σ can be evaluated holding the democratic equilibrium allocation fixed at $\tilde{T}_d^A(0)$ and $\tilde{T}_d^C(0)$:

$$\frac{\partial \Delta_{id}^*(0)}{\partial \sigma} = \begin{cases} - (T_d^A)^\alpha \frac{\partial \lambda(0)}{\partial \sigma} + \frac{\partial c_d^A(0)}{\partial \sigma} & \text{if } d \in \mathcal{I} \\ \frac{\partial c_d^A(0)}{\partial \sigma} & \text{if } d \notin \mathcal{I} \end{cases} \quad (\text{B.16})$$

Substituting these expressions into the derivative of the first-stage value function gives

$$\frac{\partial \mathcal{V}(0)}{\partial \sigma} = \sum_{d \in \mathcal{I}} \frac{n_d}{2\phi\mu_d(0)} \left[(T_d^A)^\alpha \frac{\partial \lambda(0)}{\partial \sigma} - \frac{\partial c_d^A(0)}{\partial \sigma} \right] - \sum_{d \notin \mathcal{I}} \frac{n_d}{2\phi\mu_d(0)} \left(\frac{\partial c_d^A(0)}{\partial \sigma} \right) \quad (\text{B.17})$$

$$= \sum_{d \in \mathcal{I}} \frac{n_d}{2\phi\mu_d(0)} \left[(T_d^A)^\alpha \frac{\partial \lambda(0)}{\partial \sigma} \right] - \sum_d \frac{n_d}{2\phi\mu_d(0)} \left(\frac{\partial c_d^A(0)}{\partial \sigma} \right) \quad (\text{B.18})$$

Since the incumbent chooses $\sigma \geq 0$, the incumbent has a local incentive to initiate backsliding if and only if

$$\frac{\partial \mathcal{V}(0)}{\partial \sigma} > 0. \quad (\text{B.19})$$

Hence, the backsliding condition is

$$\sum_{d \in \mathcal{I}} \frac{n_d}{\mu_d(0)} \left[(T_d^A)^\alpha \frac{\partial \lambda(0)}{\partial \sigma} \right] > \sum_d \frac{n_d}{\mu_d(0)} \left(\frac{\partial c_d^A(0)}{\partial \sigma} \right), \quad (\text{B.20})$$

where the left-hand side captures the marginal benefit from backsliding among aligned districts and the right-hand side reflects the aggregate marginal cost across all districts.

Appendix C Core Results Robustness Checks

Appendix C.1 FECT Event Study for 2009 and 2014 Elections and Pooled Sample

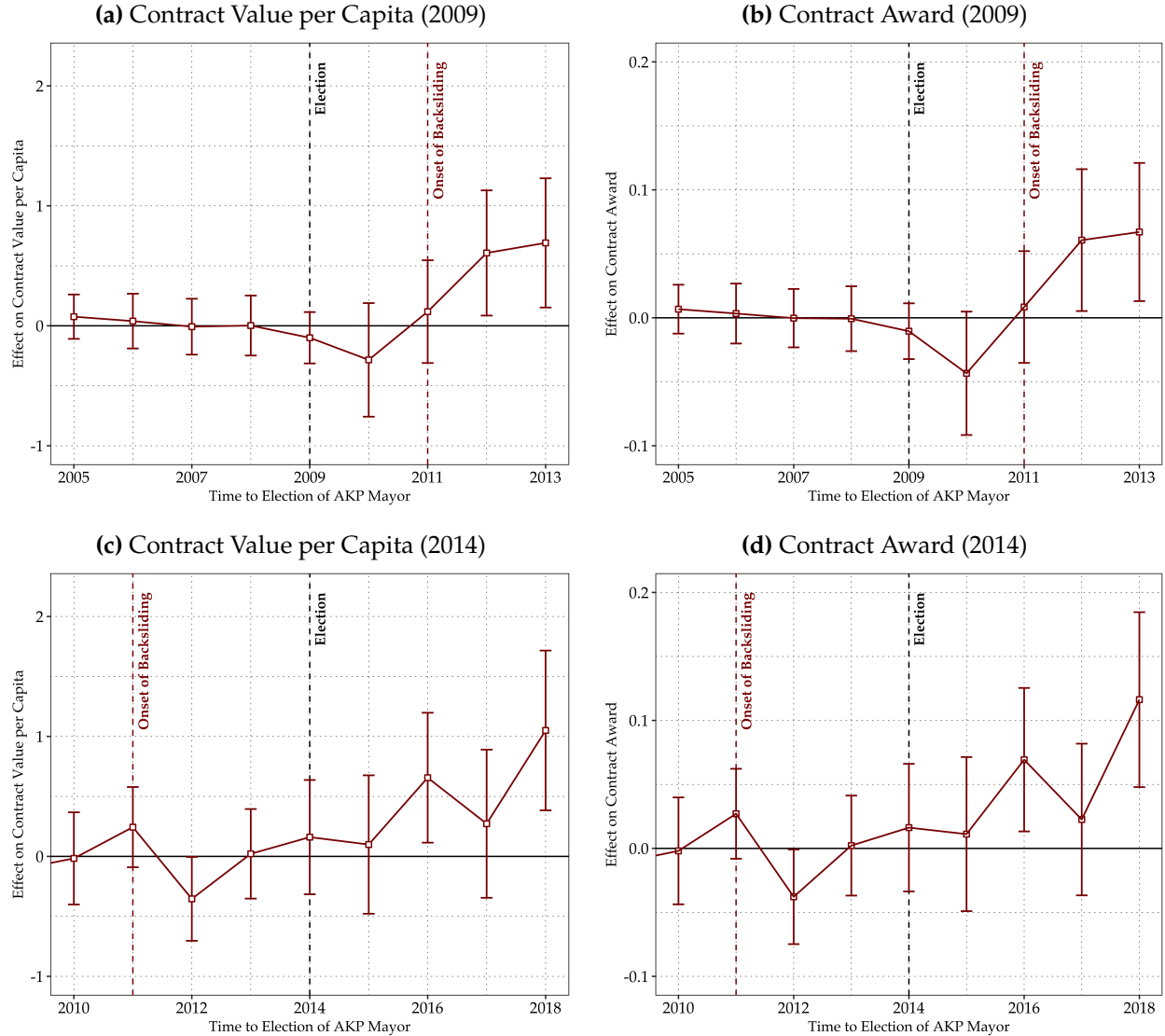


Figure C.1: Switches to the AKP by Election

Note: point estimates and 95% confidence intervals plotted. We use [Liu, Wang, and Xu \(2024\)](#)'s estimator. All panels include district and year fixed effects, in addition to municipality election controls, general election controls, and district controls. Panels (b) and (d) also control for the natural logarithm of population. Red dashed vertical line refers to the onset of democratic backsliding in 2011. See Appendix E for estimates of all control variables.

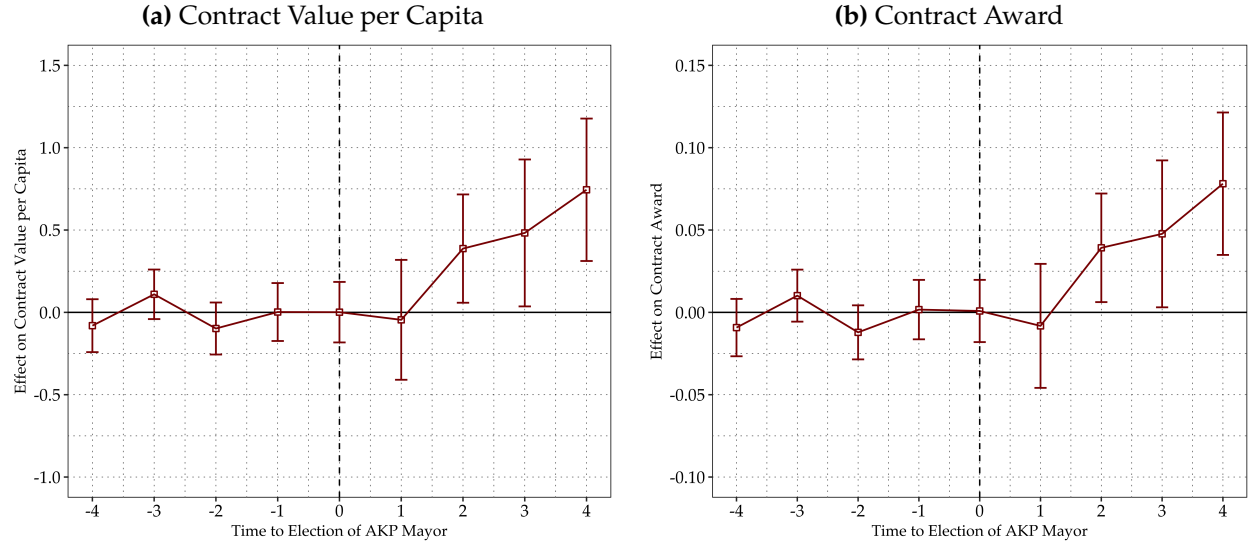


Figure C.2: Switches to the AKP (Pooled Sample)

Note: point estimates and 95% confidence intervals plotted. We use [Liu, Wang, and Xu \(2024\)](#)'s estimator. All panels include district and year fixed effects, in addition to municipality election controls, general election controls, and district controls. Panel (b) also controls for the natural logarithm of population. Sample pools across all treatment cohorts. See Appendix E for estimates of all control variables.

Appendix C.2 Pre-Trends and Carryover Tests

Following Liu, Wang, and Xu (2024), we report placebo, pre-trend, and carryover diagnostic tests for our imputation-based estimates. For the cohort-specific diagnostics, the placebo tests hold out periods $t = -1$ to $t = 0$ and the carryover tests hold out period $t = 1$. Under our event-time convention, these correspond to 2007-2008 and 2009 for the 2009 cohort, and 2012-2013 and 2014 for the 2014 cohort, respectively. For the pooled diagnostics, the placebo tests hold out periods $t = -4$ to $t = -1$, while the carryover tests hold out periods $t = 1$ to $t = 4$. The pre-trend tests assess whether estimated pre-treatment effects are jointly distinguishable from zero. We report both standard and two one-sided equivalence-test (TOST) p -values. High standard p -values and low TOST p -values support the identifying assumptions.

Table C.1: Diagnostic Tests

	Contract Value per Capita			Contract Award		
	2009	2014	Pooled	2009	2014	Pooled
Placebo	(0.419) [0.000]	(0.619) [0.001]	(0.468) [0.000]	(0.379) [0.000]	(0.574) [0.001]	(0.355) [0.000]
Pre-Trends	(0.949) [0.000]	(0.683) [0.002]	(0.390) [0.000]	(0.940) [0.000]	(0.664) [0.002]	(0.318) [0.000]
Carryover	(0.335) [0.013]	(0.260) [0.141]	(0.795) [0.000]	(0.401) [0.008]	(0.225) [0.129]	(0.728) [0.000]

Note: p -values of test statistics are provided in parentheses. p -values of equivalence test statistics are provided in square brackets. The placebo and carryover tests use the t -test. The pre-trend test uses an F -test.

Appendix C.3 Core Results with Province $\times$ Year Fixed Effects

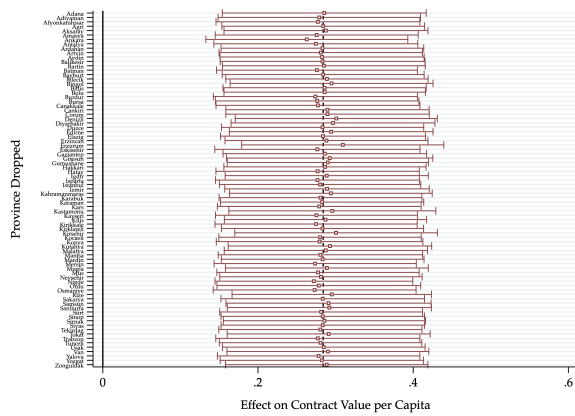
Table C.2: Table 2 Core Results
Including Province $\times$ Year Fixed Effects

	(1)	(2)	(3)
	Contract Value per Capita	Contract Award	Housing Units
AKP Mayor	0.290*** (0.068)	0.030*** (0.007)	0.518*** (0.114)
Observations	15,976	15,976	6,456
District Fixed Effects	Yes	Yes	Yes
Province $\times$ Year Fixed Effects	Yes	Yes	Yes
General Election Controls	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes
District Controls	Yes	Yes	Yes

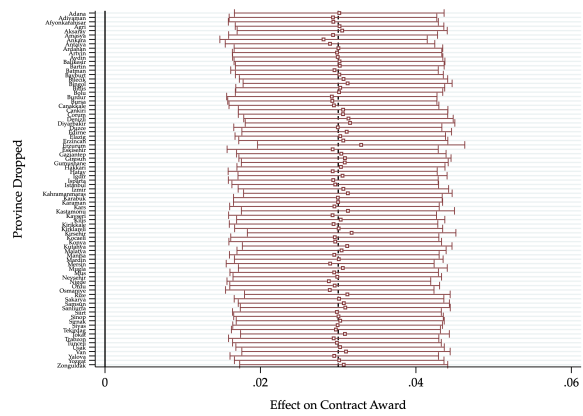
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Columns 1 and 2 use TWFE, whereas column 3 uses Poisson QMLE. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. See Appendix E for estimates of all control variables.

Appendix C.4 Dropping Provinces

(a) Contract Value per Capita



(b) Contract Award



(c) Housing Units

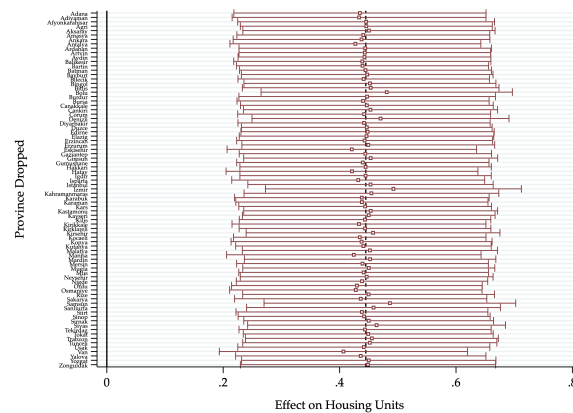


Figure C.3: Core Results by Iteratively Dropping Provinces

Note: point estimates and 95% confidence intervals plotted. The y -axis refers to the districts of the specified province dropped. Panel (a) uses the specification in column 2 of Table 2. Panel (b) uses the specification in column 4 of Table 2. Panel (c) uses the specification in column 6 of Table 2. See Appendix E for estimates of all control variables.

Appendix C.5 Dropping Districts with Zero Contracts

Table C.3: Dropping Districts with Zero TOKİ Contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	Contract Value per Capita		Contract Award		Housing Units	
AKP Mayor	0.407*** (0.092)	0.398*** (0.092)	0.043*** (0.009)	0.043*** (0.009)	0.406*** (0.111)	0.445*** (0.110)
Observations	12,008	12,008	12,008	12,008	9,874	8,917
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No	Yes	No
Subregion $\times$ Year Fixed Effects	No	Yes	No	Yes	No	Yes
Municipality Election Controls	Yes	Yes	Yes	Yes	Yes	Yes
General Election Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Sample drops all districts that never receive any TOKİ contracts. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. Columns 1-4 use TWFE, and column 5-6 use Poisson QMLE. Columns 3-6 control for the natural logarithm of population as a separate covariate. See Appendix E for estimates of all control variables.

Appendix C.6 Randomization Inference

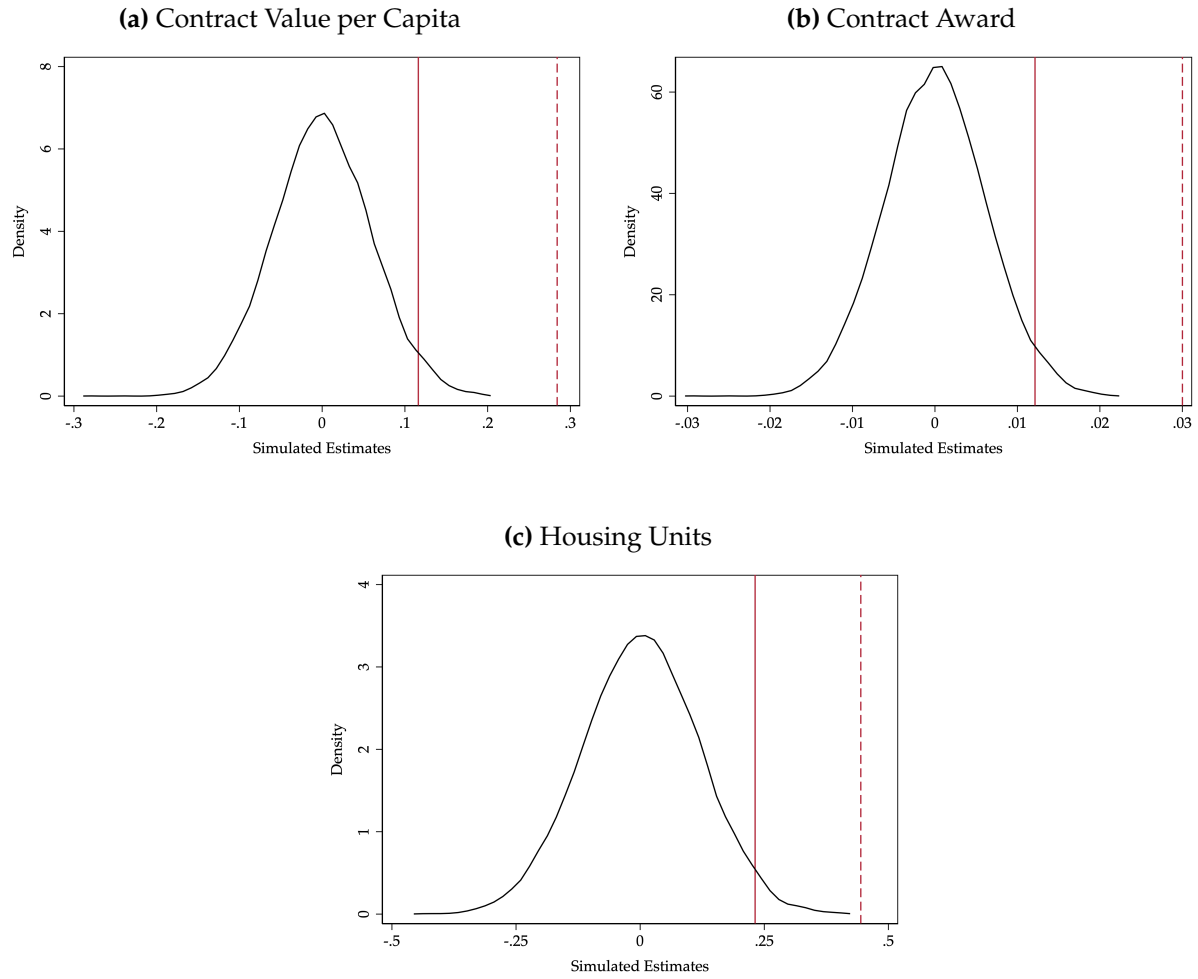


Figure C.4: Randomization Inference

Note: panel (a) uses the specification in column 2 of Table 2. Panel (b) uses the specification in column 4 of Table 2. Panel (c) uses the specification in column 6 of Table 2. The vertical solid red line outlines 5% significance threshold from the distribution of simulated estimates, and vertical dashed red line outlines observed estimate in Table 2 (column 2 for panel (a), column 4 for panel (b), column 6 for panel (c)).

Appendix C.7 Placebo Test: Non-AKP Switches

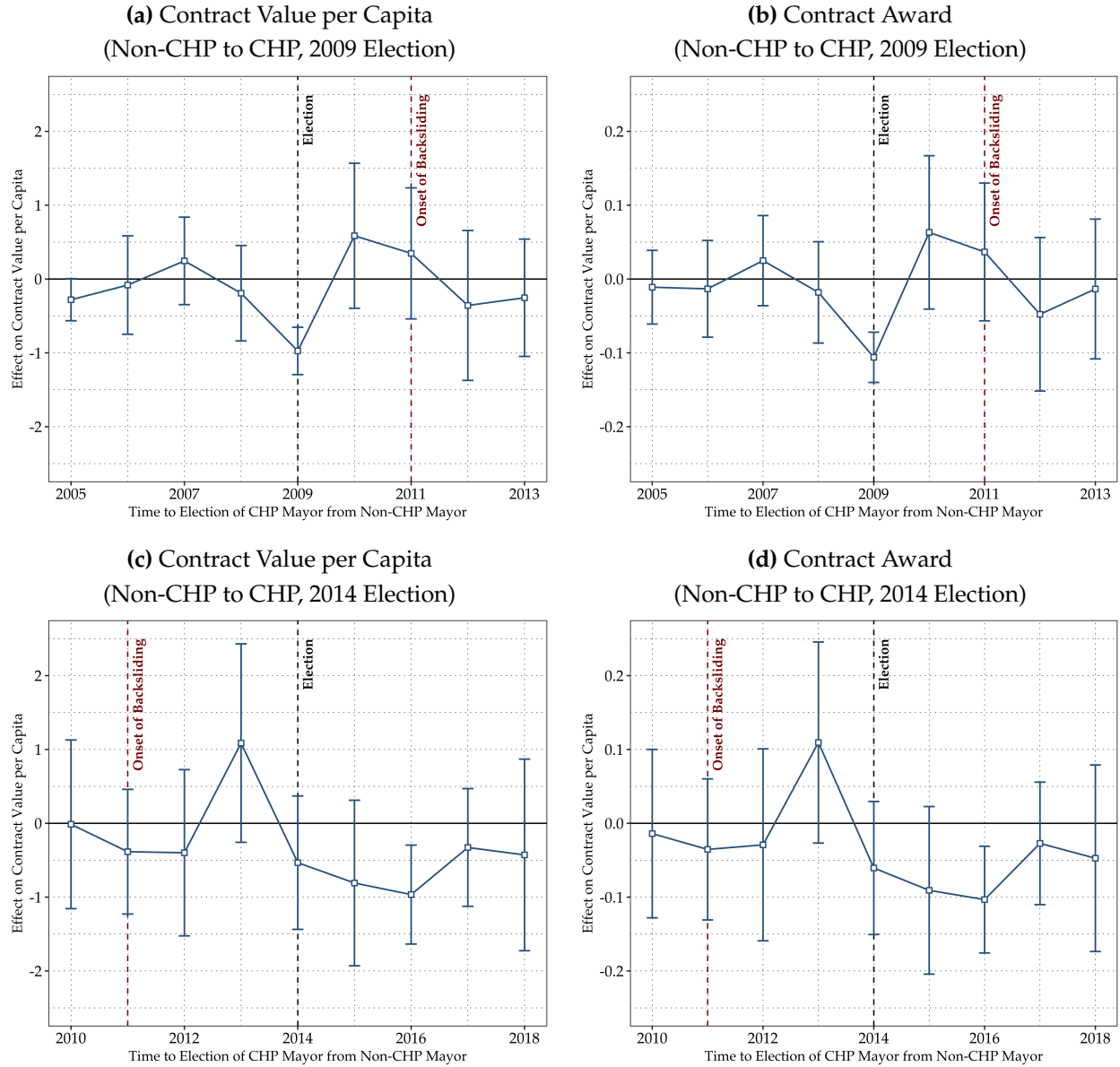


Figure C.5: Non-CHP to CHP Switches, Excluding the AKP

Note: point estimates and 95% confidence intervals plotted. We use [Liu, Wang, and Xu \(2024\)](#)'s estimator. All panels include district and year fixed effects, in addition to municipality election controls, general election controls, and district controls. 'Non-CHP' refers to all other non-AKP opposition parties that are not the CHP (e.g. Nationalist Movement Party, Peoples' Democratic Party, etc). For panels (a) and (b) we take the 'Non-CHP $\rightarrow$ CHP' and 'First Win' dummies as the treatment variable, and use a control group of districts that never switch to CHP or where the CHP lost. Panels (b) and (d) also control for the natural logarithm of population. See Appendix E for estimates of all control variables.

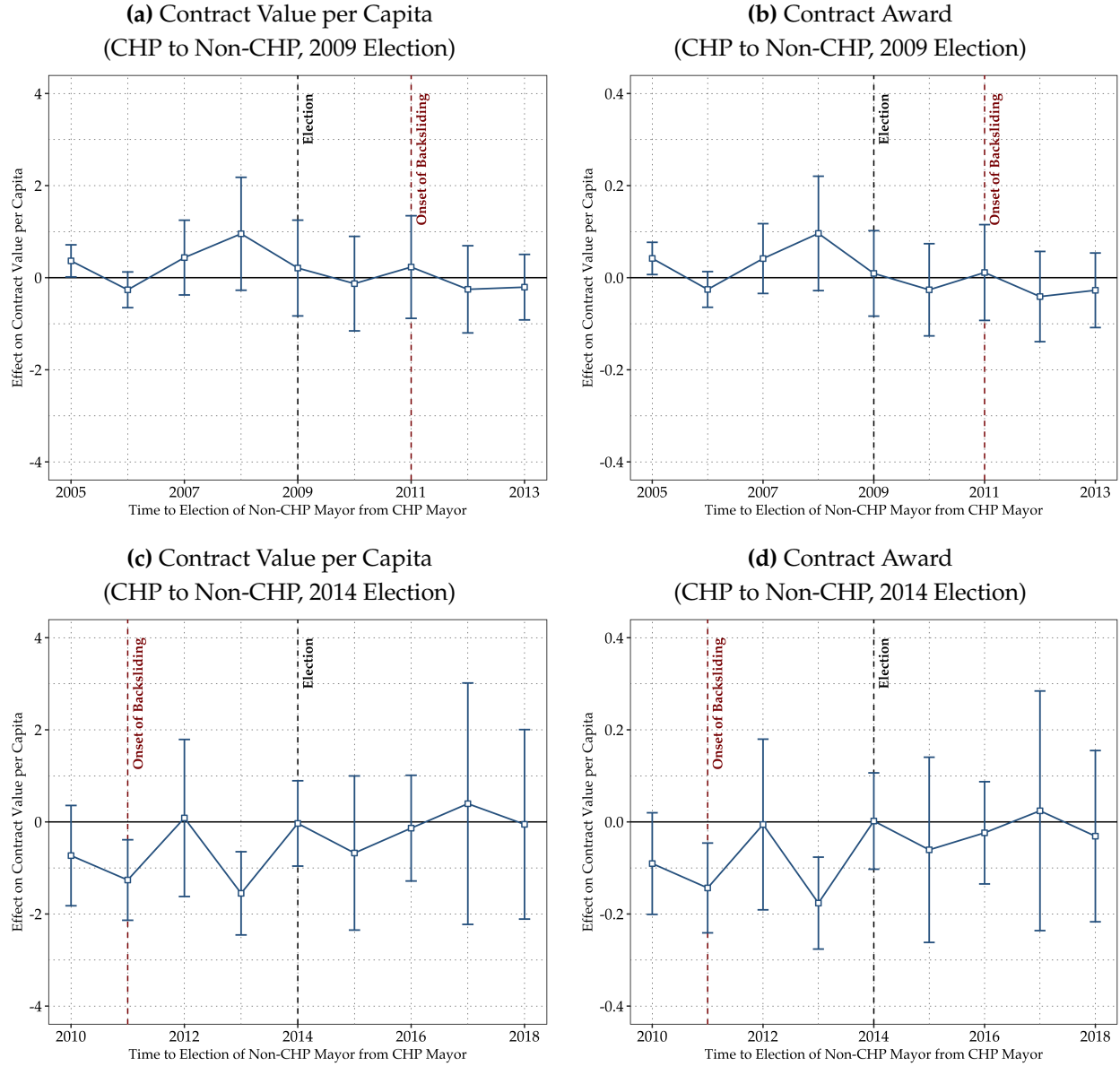


Figure C.6: CHP to Non-CHP Switches, Excluding the AKP

Note: point estimates and 95% confidence intervals plotted. We use [Liu, Wang, and Xu \(2024\)](#)'s estimator. All panels include district and year fixed effects, in addition to municipality election controls, general election controls, and district controls. 'Non-CHP' refers to all other non-AKP opposition parties that are not the CHP (e.g. Nationalist Movement Party, Peoples' Democratic Party, etc). For panels (a) and (b) we take the 'CHP → Non-CHP' and 'First Win' dummies as the treatment variable, and use a control group of districts that never switch to a Non-CHP party or where a Non-CHP party lost. Panels (b) and (d) also control for the natural logarithm of population. See Appendix E for estimates of all control variables.

Appendix C.8 House Prices and Sales

Table C.4: Controlling for Residential Property Price Index (RPPI)

	(1)	(2)	(3)
	Contract Value per Capita	Contract Award	Housing Units
AKP Mayor	0.305*** (0.092)	0.034*** (0.009)	0.327** (0.154)
Residential Property Price Index	0.021*** (0.007)	0.002*** (0.001)	0.012 (0.010)
Observations	10,524	10,524	5,128
District Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Subregion $\times$ Year Fixed Effects	No	No	No
General Election Controls	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes
District Controls	Yes	Yes	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. The ‘residential property price index’ varies at the subregion (NUTS-2) level, so we omit subregion $\times$ year fixed effects. The RPPI is only available since 2010. See Appendix E for estimates of all control variables.

Table C.5: TOKİ Housing Contract Awards and House Sales

	(1)	(2)	(3)	(4)
	First Hand Sales		Second Hand Sales	
Housing Contract Award	-0.014 (0.018)	-0.023 (0.015)	-0.019 (0.013)	-0.011 (0.011)
Observations	3,286	3,286	3,286	3,286
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Subregion $\times$ Year Fixed Effects	No	Yes	No	Yes
General Election Controls	Yes	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. All specifications use Poisson QMLE. ‘Housing Contract Award’ is a dummy for whether a TOKİ contract for social housing was awarded in a given district-year. ‘First Hand Sales’ refer to the number of new homes sold for the first time within a given district-year. ‘Second Hand Sales’ refer to the number of previously owned homes resold within the same period and district. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. House sales data is only available at the district level since 2015. See Appendix E for estimates of all control variables.

Appendix C.9 Alternative Measures of Backsliding

Table C.6: Core Results with Alternative Measures of Backsliding

	(1)	(2)	(3)	(4)	(5)	(6)
	Contract Value per Capita		Contract Award		Housing Units	
AKP Mayor	0.113 (0.115)	-42.987* (24.493)	0.011 (0.012)	-4.738* (2.562)	0.136 (0.203)	-70.353* (42.638)
AKP Mayor $\times$ 1{Law No. 5812}	0.002* (0.001)		0.000* (0.000)		0.004* (0.002)	
AKP Mayor $\times$ Year		0.022* (0.012)		0.002* (0.001)		0.035* (0.021)
Observations	15,976	15,976	15,986	15,986	8,917	8,917
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Subregion $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
General Election Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. Columns 1-4 use TWFE, and column 5-6 use Poisson QMLE. Columns 3-6 control for the natural logarithm of population as a covariate. Columns 1, 3, and 5 interact AKP mayor with an indicator for the post-2008 period following the implementation of Law No. 5812. Columns 2, 4 and 6 interact AKP Mayor with a linear trend. See Appendix E for estimates of all control variables.

Appendix C.10 Counterfactual Estimation Discussion

Derivation of Democratic Equilibrium Specification Recall the democratic equilibrium allocation from the theoretical model as:

$$\tilde{T}_d^A(0) = T \frac{\left(\frac{n_d}{\mu_d(0)}\right)^{\frac{1}{1-\alpha}}}{\sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(0)}\right)^{\frac{1}{1-\alpha}}},$$

Given this is symmetric across both parties $\tilde{T}_d^A(0) = \tilde{T}_d^C(0)$, and using just the democratic sample of the data, we can simplify notation to $\tilde{T}_d$ and suppress the (0) condition. Taking logs of the equilibrium allocation:

$$\log \tilde{T}_d = \delta_{rt} + \frac{1}{1-\alpha} \log n_d - \frac{1}{1-\alpha} \log \mu_d,$$

where $\delta_{rt} = \log T - \frac{1}{1-\alpha} \log \sum_{d'} \left(\frac{n_{d'}}{\mu_{d'}(0)}\right)$. Define $\beta = \frac{1}{1-\alpha}$. Normalizing $\tilde{T}_d$ by population, we thus have:

$$\begin{aligned} \log \left(\frac{\tilde{T}_d}{n_d}\right) &= \delta_{rt} + \beta \log \left(\frac{n_d}{\mu_d}\right) - \log n_d \\ &= \delta_{rt} + (\beta - 1) \log n_d - \beta \log \mu_d \\ &= \delta_{rt} + \gamma \log n_d - \beta \log \mu_d, \end{aligned}$$

where $\beta = 1 + \gamma$, and therefore $\alpha = \frac{\gamma}{1+\gamma}$. The empirical analog for a given district d in province p of subregion r in year t , including covariates, $\mathbf{X}'_{dp\tau}$, is thus:

$$\log \left(\frac{T_{dp\tau}}{n_{dp\tau}}\right) = \delta_{rt} + \gamma \log n_{dp\tau} + \beta \log \mu_{dp\tau} + \rho \mathbf{X}'_{dp\tau} + u_{dp\tau}, \quad (\text{C.1})$$

where $\beta < 0$ and $\gamma > 0$.

Estimating $\hat{\mu}_d$ To estimate $\mu_{dp\tau}$, we run the following specification, again solely on the democratic sample of the data:

$$m_{dp\tau} = \alpha_d + \delta_{rt} + \rho \mathbf{X}'_{dp\tau} + \varepsilon_{dp\tau},$$

where $m_{dp\tau} \in [-1, 1]$ is the AKP vote margin. We then obtain predicted residuals, $\hat{\varepsilon}_{dp\tau}$, and given the theoretical model assumes $\varepsilon_{dp\tau} \sim [-\mu_{dp\tau}, \mu_{dp\tau}]$, the variance of $\hat{\varepsilon}_{dp\tau}$ is thus:

$$\begin{aligned} \text{Var}(\hat{\varepsilon}_{dp\tau}) &= \frac{(\hat{\mu}_{dp\tau} - (-\hat{\mu}_{dp\tau}))^2}{12} \\ &= \frac{\hat{\mu}_{dp\tau}^2}{3} \end{aligned}$$

This implies that district-level electoral uncertainty can be recovered as:

$$\hat{\mu}_{dp\tau} = \sqrt{3} \hat{\sigma}_{dp\tau},$$

where $\hat{\sigma}_{dp\tau}$ is the estimated standard deviation of $\hat{\varepsilon}_{dp\tau}$. This can subsequently be plugged back into equation (C.1) to estimate β . We show these results on the next page.

Table C.7: Results for Estimating and Using Counterfactual Allocations

	(1)	(2)	(3)	(4)	(5)
	Contract Value per Capita (Observed)			Contract Value per Capita (Counterfactual)	
$\log(\text{Population})$	0.657*** (0.068)	0.652*** (0.068)	0.548*** (0.053)		
$\log(\text{Population}) \times \mathbf{1}\{t < 2011\}$			0.136** (0.054)		
$\log(\hat{\mu}_{dpr})$		-0.044 (0.046)	0.003 (0.040)		
$\log(\hat{\mu}_{dpr}) \times \mathbf{1}\{t < 2011\}$			-0.044 (0.047)		
AKP Mayor				-0.011 (0.013)	-0.005 (0.007)
AKP Mayor $\times \mathbf{1}\{t < 2011\}$				-0.016 (0.017)	-0.007 (0.011)
Observations	6,360	6,360	15,381	15,380	15,380
District Fixed Effects	No	No	No	Yes	Yes
Subregion $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
General Election Controls	Yes	Yes	Yes	Yes	Yes
Municipality Election Controls	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes
Sample	Democracy	Democracy	Pooled	Pooled	Pooled
Counterfactual Benchmark	–	–	–	Democracy	Per-Capita

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by district in parentheses. Municipality election controls refer to turnout rate, proportion of votes that were invalid and whether the central provincial mayor belongs to the AKP. We define the central mayor to be either the mayor of metropolitan municipalities or the mayor of the *merkez* in non-metropolitan municipalities. General election controls refer to the AKP vote share for general elections, alongside turnout rate and proportion of votes that were invalid. District controls refers to a dummy for the presence of any natural disaster (earthquake and/or flood) in addition to the log of urban settlements. Across columns 1-3, only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Columns 1-2 only use the democratic sample of the data. Columns 3-5 use all years in the dataset. The dependent variable for column 4 is counterfactual contract value per capita using the democratic benchmark, and column 5 is counterfactual contract value per capita using the per-capita benchmark. See Appendix E for estimates of all control variables.

Column 3 provides suggestive evidence consistent with the paper’s core mechanisms. The population gradient remains positive but weakens under backsliding, falling from 0.684 during democracy to 0.548 after 2011, suggesting allocations become less population-based as political distortions enter the allocation rule. The estimates for $\hat{\mu}_{dpr}$ point in the expected direction, though the coefficient is small and statistically insignificant, so we interpret the uncertainty channel cautiously given likely attenuation from measurement error. Columns 4 and 5 show no evidence of AKP incumbency effects pre-backsliding, consistent with less targeted contract allocations.

Appendix C.11 Figure 4 with Uncertainty Intervals

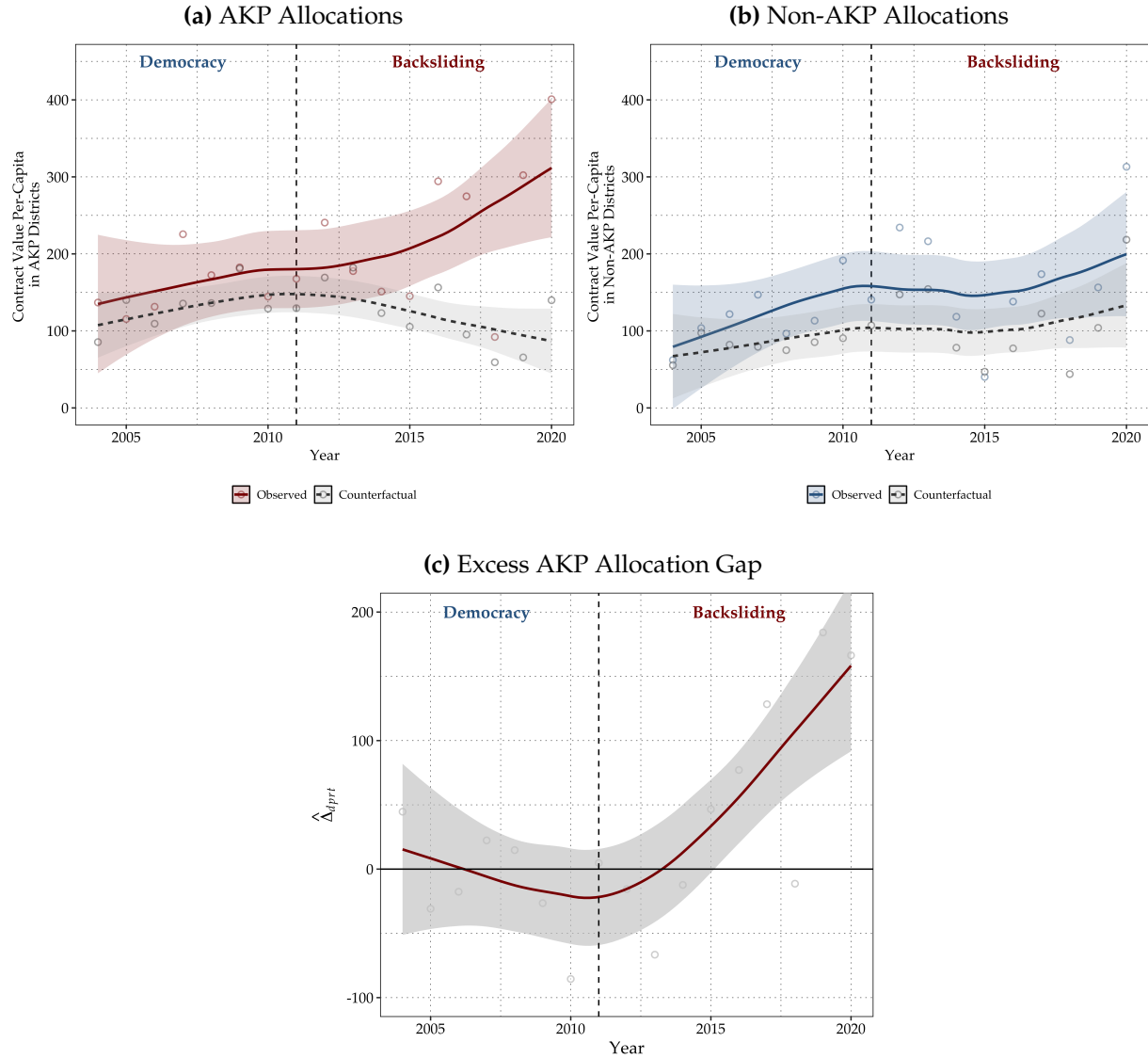


Figure C.7: Observed versus Counterfactual Allocations with Uncertainty Intervals

Note: panel (a) plots the average observed (red line) and counterfactual (black dashed line) allocations for AKP-affiliated districts. Panel (b) plots the average observed (blue line) and counterfactual (black dashed line) allocations for non AKP-affiliated districts. Panel (c) plots the the average estimates of $\hat{\Delta}_t$ from equation (??). The vertical dashed line conveys the onset of democratic backsliding. Trend-lines for both panels are LOWESS estimates with 95% confidence intervals, and faded dots represent the underlying data. All values on the y -axis refer to 2020 real Turkish liras. Only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Observed total TOKİ expenditures are redistributed within subregion $\times$ year cells.

Appendix C.12 Leave-One-Out Cross-Validation

To assess out-of-sample fit, we sequentially leave out each NUTS-2 subregion, estimate the democratic allocation rule using all remaining subregions, and predict allocations for districts in the held-out subregion. Table C.8 reports the resulting prediction errors. During the democratic period, errors are similar for AKP and non-AKP districts, suggesting that the democratic allocation rule fits both groups comparably well out of sample. After the onset of backsliding, however, prediction errors increase sharply for AKP districts, while remaining much closer to their democratic-period levels for non-AKP districts. This pattern is consistent with the main counterfactual results: post-2011 allocations in AKP-aligned districts depart substantially from what would be predicted by the democratic allocation benchmark.

Table C.8: Leave-One-Out Cross-Validation Results

Regime	District	MAE	RMSE
Democracy	AKP	34.1	43.5
Democracy	Non-AKP	38.4	50.0
Backsliding	AKP	102.6	135.7
Backsliding	Non-AKP	53.1	58.3

Note: ‘Democracy’ refers to years 2004–2010, and ‘Backsliding’ refers to years 2011–2020. We sequentially leave out each subregion, estimate the democratic allocation rule using all remaining subregions, and predict counterfactual allocations in the held-out subregion. MAE and RMSE are calculated from annual group-average prediction errors in 2020 TRY per capita.

Table C.9: Leave-One-Out Subregions and Parameters

Held Out Region	No. of Training Districts	No. of Held Out Districts	$\hat{\gamma}$	$\hat{\beta}$	$\hat{\alpha}$
Adana	894	22	0.642	-0.042	0.391
Agri	890	26	0.645	-0.046	0.392
Ankara	892	24	0.619	-0.051	0.382
Antalya	878	38	0.680	-0.058	0.405
Aydin	868	48	0.688	-0.053	0.407
Balikesir	885	31	0.668	-0.045	0.400
Bursa	879	37	0.655	-0.050	0.396
Erzurum	886	30	0.669	-0.034	0.401
Gaziantep	894	22	0.633	-0.033	0.388
Hatay	887	29	0.648	-0.032	0.393
Istanbul	885	31	0.655	-0.040	0.396
Izmir	888	28	0.656	-0.038	0.396
Kastamonu	875	41	0.641	-0.046	0.390
Kayseri	869	47	0.634	-0.043	0.388
Kirikkale	879	37	0.676	-0.021	0.403
Kocaeli	873	43	0.645	-0.038	0.392
Konya	879	37	0.640	-0.052	0.390
Malatya	875	41	0.667	-0.042	0.400
Manisa	863	53	0.661	-0.041	0.398
Mardin	886	30	0.644	-0.042	0.392
Samsun	869	47	0.661	-0.032	0.398
Sanliurfa	892	24	0.651	-0.048	0.394
Tekirdag	890	26	0.658	-0.058	0.397
Trabzon	837	79	0.595	-0.068	0.373
Van	887	29	0.669	-0.049	0.401
Zonguldak	900	16	0.647	-0.047	0.393

Note: each row reports one leave-one-subregion-out fold. The held-out region is excluded from the democratic allocation regression, which is estimated using districts in all remaining subregions. Training districts gives the number of districts used to estimate the fold-specific model, while held-out districts gives the number of districts for which counterfactual allocations are predicted. $\hat{\gamma}$ is the estimated coefficient on log population, $\hat{\beta}$ is the coefficient on predicted residual uncertainty, $\hat{\mu}_{dpr}$, and $\hat{\alpha} = \hat{\gamma}/(1 + \hat{\gamma})$ is the implied concavity parameter.

Appendix C.13 Counterfactual Estimates with 3-Year Moving Averages

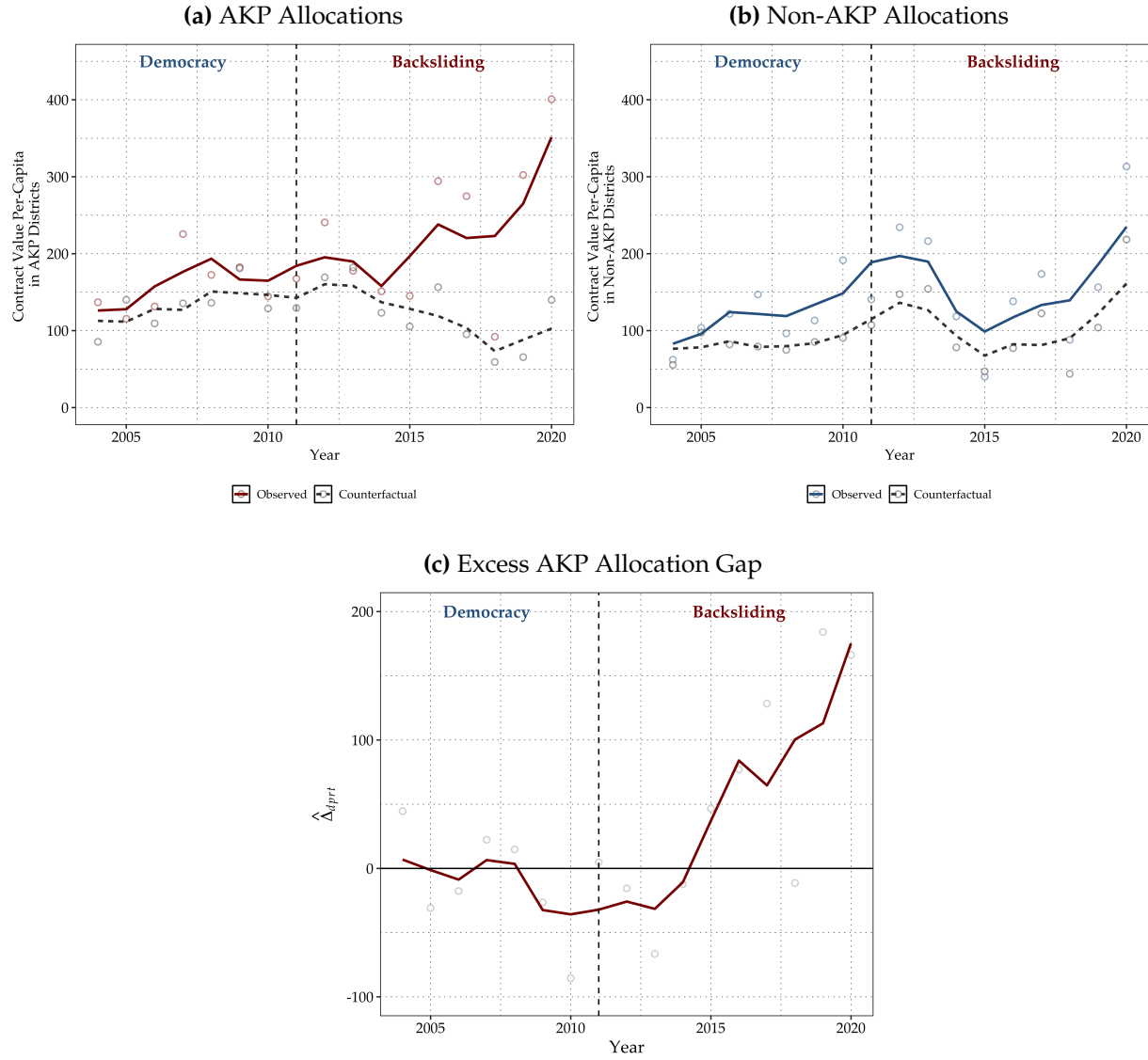


Figure C.8: Observed versus Counterfactual Allocations (3-Year Moving Averages)

Note: panel (a) plots the average observed (red line) and counterfactual (black dashed line) allocations for AKP-affiliated districts. Panel (b) plots the average observed (blue line) and counterfactual (black dashed line) allocations for non AKP-affiliated districts. Panel (c) plots the the average estimates of $\hat{\Delta}_t$ from equation (??). The vertical dashed line conveys the onset of democratic backsliding. Trend-lines for both panels are 3-year moving averages, and faded dots represent the underlying data. For the endpoints (2004 and 2020), we use a 2-year average to maintain a complete series across all years. All values on the y -axis refer to 2020 real Turkish liras. Only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Observed total TOKİ expenditures are redistributed within subregion $\times$ year cells.

Appendix C.14 Counterfactual Estimates with Province $\times$ Year Cells

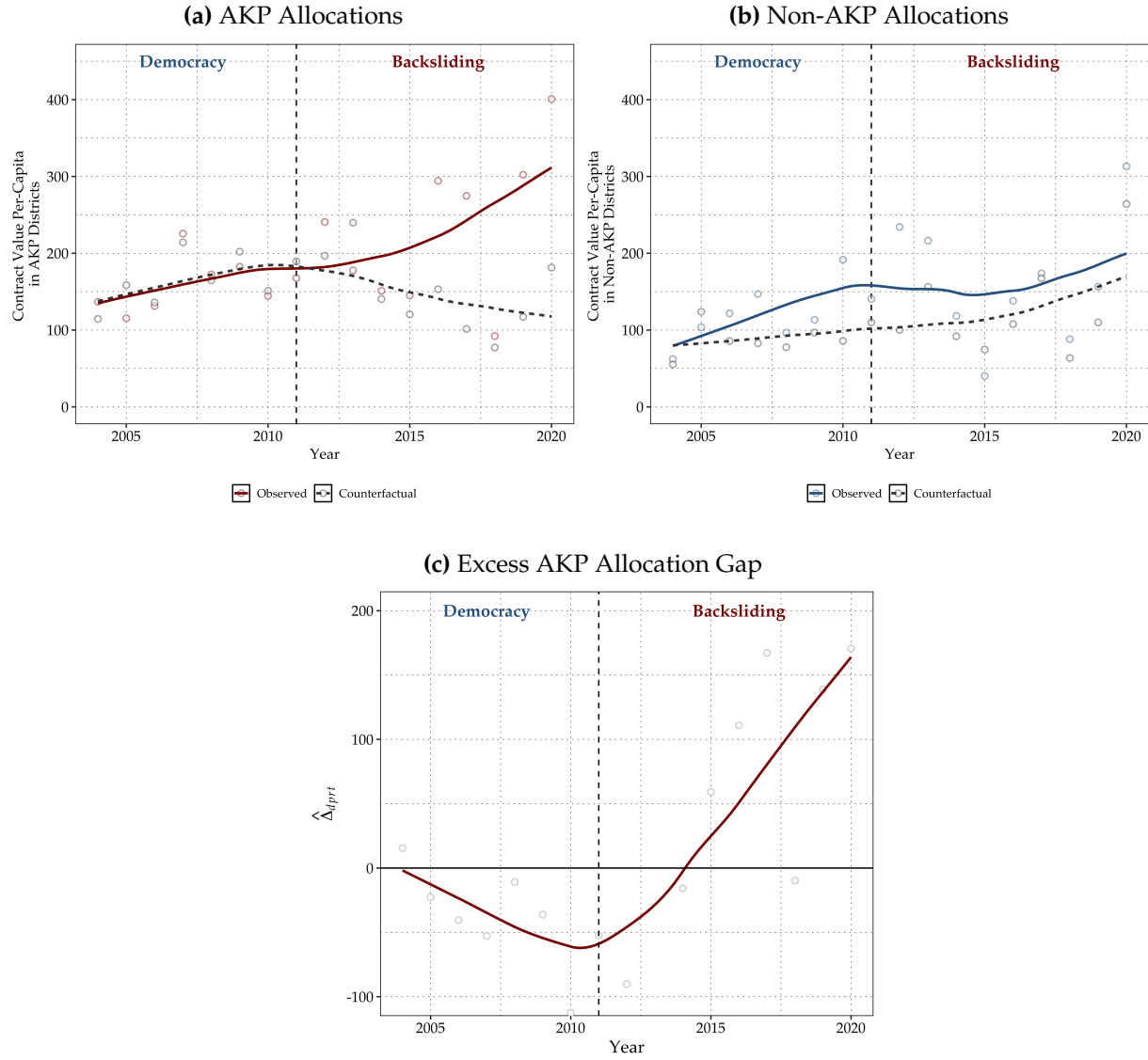


Figure C.9: Observed versus Counterfactual Allocations (Province $\times$ Year Cells)

Note: panel (a) plots the average observed (red line) and counterfactual (black dashed line) allocations for AKP-affiliated districts. Panel (b) plots the average observed (blue line) and counterfactual (black dashed line) allocations for non AKP-affiliated districts. Panel (c) plots the the average estimates of $\hat{\Delta}_t$ from equation (??). The vertical dashed line conveys the onset of democratic backsliding. Trend-lines for all panels are LOWESS estimates, and faded dots represent the underlying data. All values on the y -axis refer to 2020 real Turkish liras. Only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Observed total TOKİ expenditures are redistributed within province $\times$ year cells.

Appendix C.15 Counterfactual Estimates (Per-Capita Benchmark)

From the model, a corollary arises from equilibrium democratic allocations:

Corollary 1 (Population Proportionality) *If electoral uncertainty is sufficiently homogeneous across districts, so that $\mu_d(0) \approx \bar{\mu}$ for most d , then the equilibrium allocation rule in equation (3.2) is well-approximated by*

$$\tilde{T}_d^A(0) \approx T \left\{ \frac{(n_d)^{\frac{1}{1-\alpha}}}{\sum_{d'} (n_{d'})^{\frac{1}{1-\alpha}}} \right\}. \quad (\text{C.2})$$

Corollary 1 implies, in the limiting case where every district is equally competitive and hence electoral uncertainty is equal across districts, expenditures are primarily driven by population considerations and the total absence of systematic political targeting. This would reflect a pure, apolitical planner's allocation rule, where no distinct political weight is given to any district, and only a weighted per-capita allocation formula is employed.

Taking logs of equation (C.2), we can thus estimate equilibrium allocations following a fully apolitical, per-capita benchmark. This is shown in Figure C.10 below. Noticeably, the AKP and non-AKP allocations are almost the same in levels, and fall marginally below the democratic benchmark. This suggests even under democracy, political distortions can still arise due to electoral competition over and above pure population considerations.

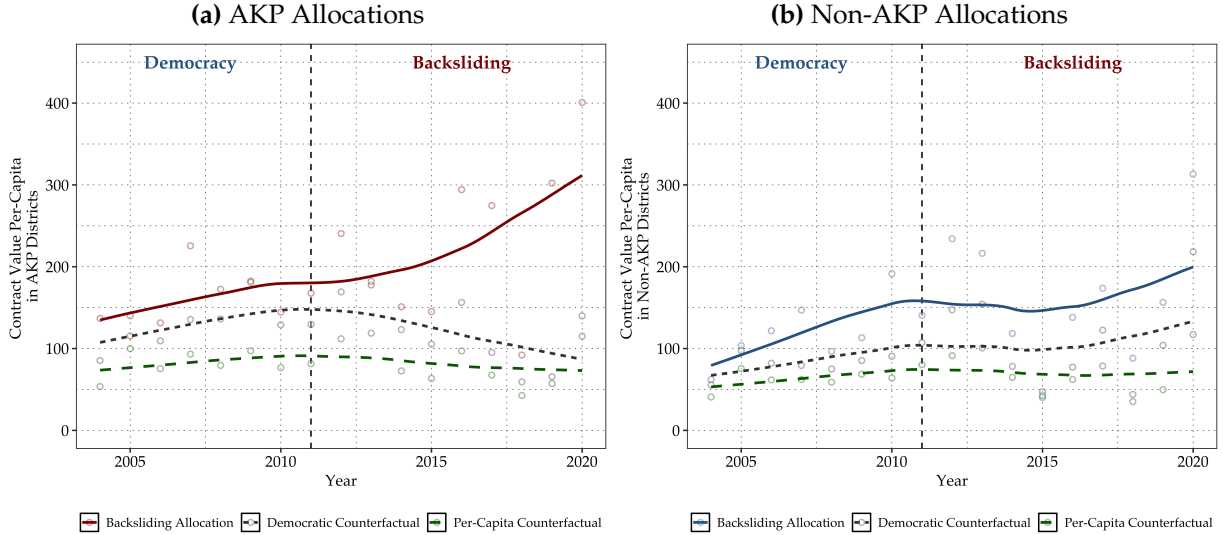


Figure C.10: Observed versus Counterfactual Allocations (Per-Capita Benchmark)

Note: panel (a) plots the average observed (red line), democratic counterfactual (black dashed line) and per-capita counterfactual (green dashed line) allocations for AKP-affiliated districts. Panel (b) plots the average observed (red line), democratic counterfactual (black dashed line) and per-capita counterfactual (green dashed line) allocations for non-AKP-affiliated districts. Trend-lines for both panels are LOWESS estimates, and faded dots represent the underlying data. All values on the y -axis refer to 2020 real Turkish liras. Only districts with some experience of democracy (i.e. non-missing AKP margin vote share data from the 2004 or 2009 election) are used. Observed total TOKİ expenditures are redistributed within subregion $\times$ year cells.

Appendix D RDD Robustness Checks

Appendix D.1 Discontinuity for Contract Award (Figure 6)

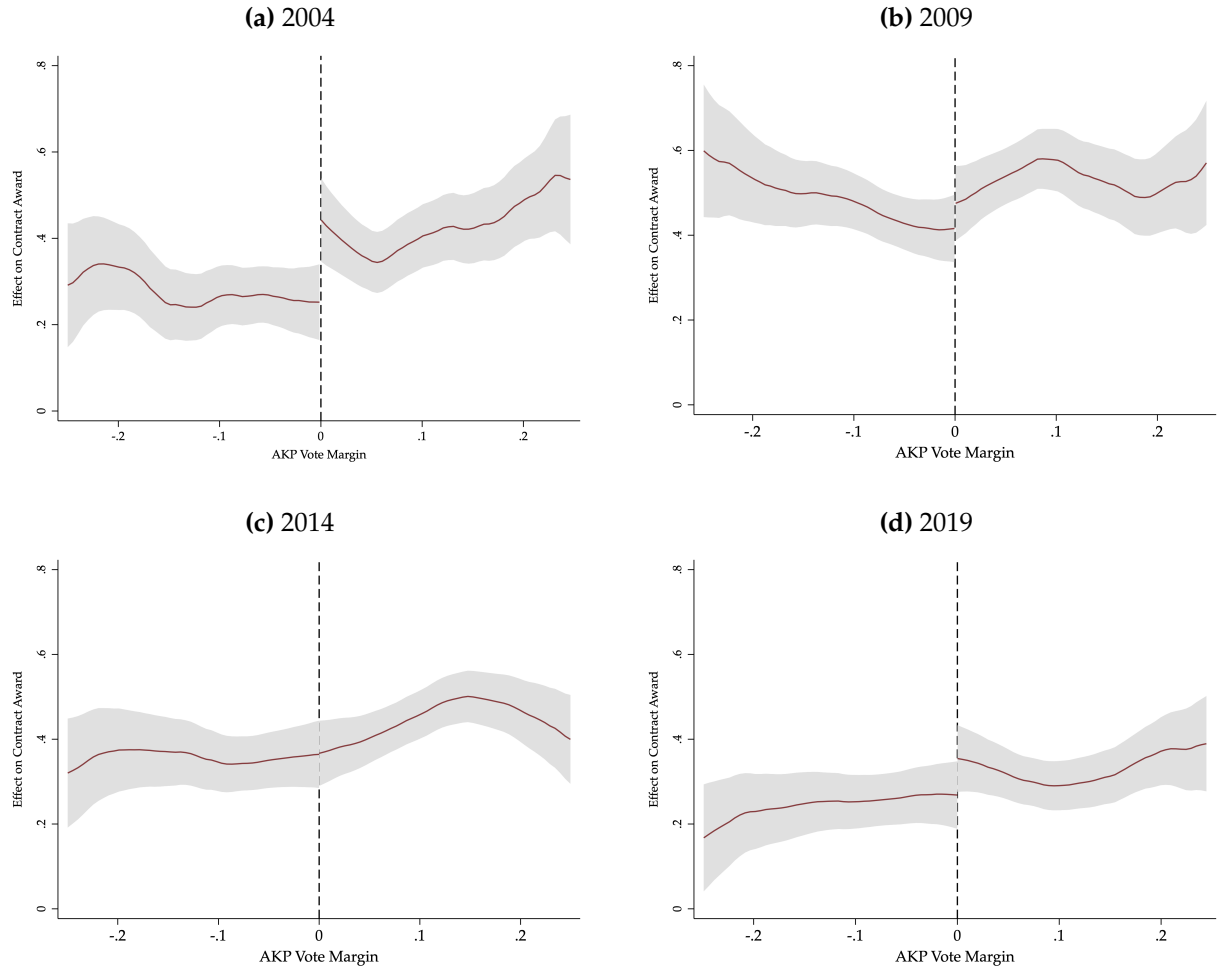


Figure D.1: RDDs per Local Election for Contract Awards

Note: each panel plots local polynomial fits per election above and below the vote margin threshold with shaded 95% confidence intervals.

Appendix D.2 McCrary (2008) Density Tests

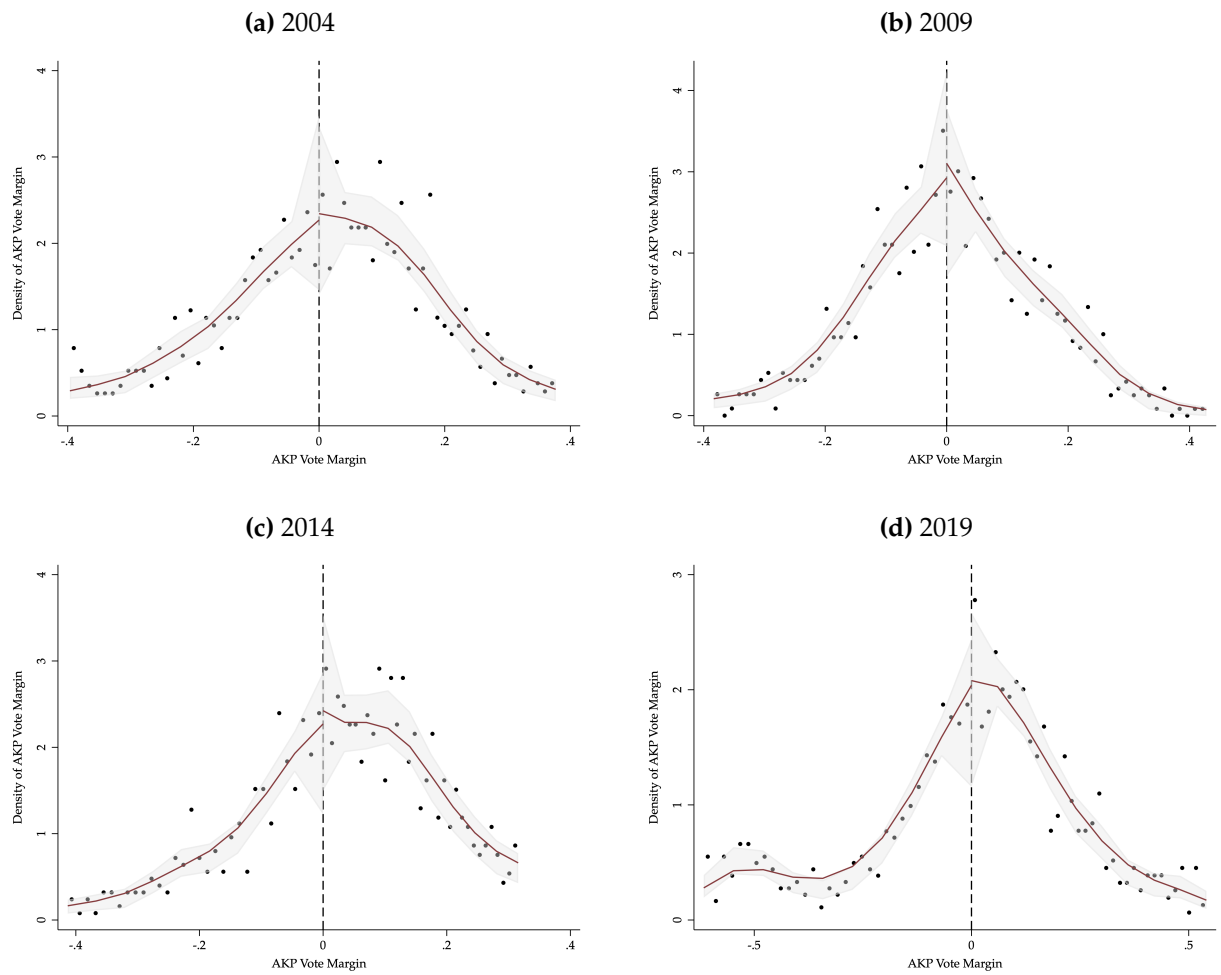


Figure D.2: McCrary (2008) Density Tests per Local Election

Note: each panel plots the density test by McCrary (2008). Dots refer to equally sized bins of the AKP vote margin running variable.

Appendix D.3 Placebo Outcomes

Table D.1: Placebo Outcomes for RDD (Table 5)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Population			Urban Settlements			General Election Vote Share		
<i>Panel A - 2004</i>									
AKP Mayor	0.175 (0.202)	0.175 (0.258)	0.143 (0.114)	-0.098 (0.419)	-0.104 (0.619)	-0.007 (0.228)	0.018 (0.023)	0.001 (0.039)	-0.002 (0.016)
Observations	921	921	921	921	921	921	922	922	922
<i>Panel B - 2009</i>									
AKP Mayor	-0.103 (0.195)	0.148 (0.314)	0.104 (0.121)	-0.025 (0.362)	-0.160 (0.525)	-0.131 (0.205)	-0.018 (0.027)	-0.031 (0.035)	0.026* (0.015)
Observations	952	952	951	951	951	951	908	908	908
<i>Panel C - 2014</i>									
AKP Mayor	-0.008 (0.199)	-0.096 (0.264)	-0.123 (0.124)	-0.079 (0.301)	-0.163 (0.444)	-0.194 (0.203)	0.026 (0.019)	0.012 (0.031)	0.020 (0.015)
Observations	970	970	969	970	970	969	944	944	943
<i>Panel D - 2019</i>									
AKP Mayor	0.321 (0.206)	0.559* (0.314)	-0.055 (0.130)	0.359 (0.357)	0.544 (0.496)	-0.304 (0.198)	0.007 (0.021)	0.026 (0.033)	0.016 (0.015)
Observations	973	973	973	973	973	973	970	970	970
Polynomial Method	Linear CCT	Cubic CCT	- IW	Linear CCT	Cubic CCT	- IW	Linear CCT	Cubic CCT	- IW

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors in parentheses. Columns 1-2, 4-5 and 7-8 use [Calonico et al. \(2017\)](#)'s (CCT) method. Columns 3, 6 and 9 use [Imbens and Wager \(2019\)](#)'s (IW) method. Columns 1, 4 and 7 use a linear polynomial, whereas columns 2, 5 and 8 use a cubic polynomial.

Appendix D.4 Alternative Threshold Cut-Off

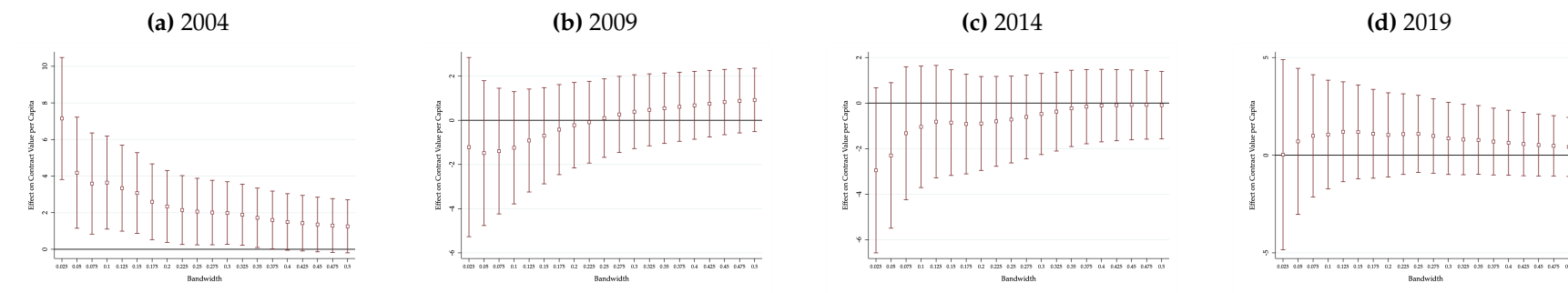
Table D.2: Close Election RDD Placebo with Alternative Cut-Offs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	c = -0.05						c = 0.05					
	Contract Value per Capita			Contract Award			Contract Value per Capita			Contract Award		
Panel A - 2004												
Placebo Treatment	1.047 (1.553)	2.857 (2.244)	-0.229 (1.039)	0.112 (0.154)	0.304 (0.226)	-0.018 (0.103)	-0.132 (1.896)	0.251 (2.487)	1.271 (0.998)	-0.055 (0.170)	-0.039 (0.225)	0.141 (0.097)
Observations	406	406	406	406	406	406	515	515	515	517	517	517
Panel B - 2009												
Placebo Treatment	-2.090 (1.971)	-0.270 (3.164)	-1.837** (0.919)	-0.246 (0.199)	-0.088 (0.330)	-0.176* (0.092)	0.061 (1.722)	3.232 (2.648)	-0.140 (1.011)	-0.023 (0.167)	0.297 (0.266)	0.013 (0.099)
Observations	475	475	475	475	475	475	477	477	477	477	477	477
Panel C - 2014												
Placebo Treatment	-1.611 (1.733)	0.692 (3.158)	-1.565 (1.018)	-0.170 (0.184)	0.019 (0.337)	-0.127 (0.107)	-0.822 (1.745)	-0.763 (2.489)	0.916 (0.939)	-0.055 (0.175)	-0.017 (0.260)	0.080 (0.092)
Observations	376	376	376	376	376	376	593	593	593	593	593	593
Panel D - 2019												
Placebo Treatment	1.585 (1.253)	2.597 (2.148)	0.162 (1.068)	0.154 (0.131)	0.274 (0.233)	0.008 (0.107)	1.297 (1.421)	3.658 (2.273)	0.783 (0.958)	0.140 (0.145)	0.378 (0.239)	0.072 (0.098)
Observations	437	437	437	437	437	437	536	536	536	536	536	536
Polynomial Method	Linear CCT	Cubic CCT	- IW	Linear CCT	Cubic CCT	- IW	Linear CCT	Cubic CCT	- IW	Linear CCT	Cubic CCT	- IW

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors in parentheses. Columns 1-2, 4-5, 7-8 and 10-11 use [Calonico et al. \(2017\)](#)'s (CCT) method. Columns 3, 6, 9 and 12 use [Imbens and Wager \(2019\)](#)'s (IW) method. Columns 1, 4, 7 and 10 use a linear polynomial, whereas columns 2, 5, 8 and 11 use a cubic polynomial. Columns 1-6 use a threshold cut-off of -0.05, using only observations below the original threshold of 0. Columns 7-12 use a threshold cut-off of 0.05, using only observations above the original threshold of 0. These alternative cut-offs determine the value of 'Placebo Treatment'.

Appendix D.5 Varying RDD Bandwidth

Panel A: Contract Value per Capita



Panel B: Contract Award

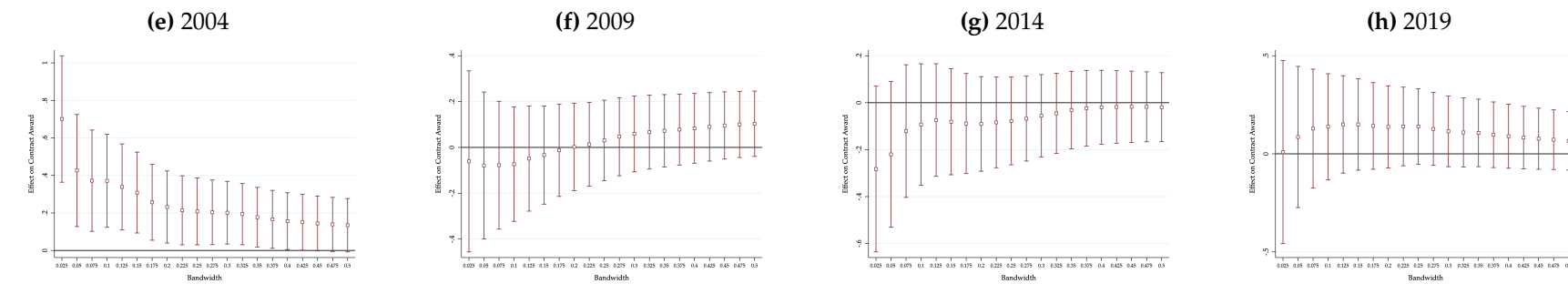


Figure D.3: Varying Bandwidth for Contract Value per Capita and Contract Award

Note: Panels A and B use the specifications from columns 1 and 4 of Table 5, respectively. We vary the bandwidth for AKP vote margin between 0.025 to 0.5 in increments of 0.025.

Appendix E Control Variables

Appendix E.1 Municipality Election Controls

Table E.1: Turnout Rate (Local Election)

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	1.273	(1.484)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.133	(0.158)
Figure 5	Panel (a)	Contract Value per Capita	-0.458	(0.992)
Figure 5	Panel (b)	Contract Award	-0.057	(0.102)
Figure 5	Panel (c)	Housing Units	1.125	(1.747)
Figure 6	Panel (a)	Contract Value per Capita	-0.42	(0.984)
Figure 6	Panel (b)	Contract Award	-0.053	(0.102)
Figure 6	Panel (c)	Housing Units	0.981	(1.728)
Figure C.5	Panel (a)	Contract Value per Capita	0.286	(1.002)
Figure C.5	Panel (b)	Contract Award	0.011	(0.104)
Figure C.5	Panel (c)	Contract Value per Capita	0.286	(1.002)
Figure C.5	Panel (d)	Contract Award	0.011	(0.104)
Figure C.6	Panel (a)	Contract Value per Capita	0.076	(1.092)
Figure C.6	Panel (b)	Contract Award	-0.017	(0.113)
Figure C.6	Panel (c)	Contract Value per Capita	0.076	(1.092)
Figure C.6	Panel (d)	Contract Award	-0.017	(0.113)
Table 2	Column 1	Contract Value per Capita	-0.492	(0.99)
Table 2	Column 2	Contract Value per Capita	-0.401	(1.766)
Table 2	Column 3	Contract Value per Capita	-1.423	(1.827)
Table 2	Column 4	Contract Award	-0.053	(0.102)
Table 2	Column 5	Contract Award	-0.109	(0.18)
Table 2	Column 6	Contract Award	-0.152	(0.195)
Table 2	Column 7	Housing Units	1.024	(1.747)
Table 2	Column 8	Housing Units	5.786	(3.208)
Table 2	Column 9	Housing Units	-0.201	(3.838)
Table C.2	Column 1	Contract Value per Capita	-0.229	(1.105)
Table C.2	Column 2	Contract Award	-0.031	(0.113)
Table C.2	Column 3	Housing Units	1.037	(2.185)
Table C.3	Column 1	Contract Value per Capita	0.514	(1.234)
Table C.3	Column 2	Contract Value per Capita	-0.248	(1.381)
Table C.3	Column 3	Contract Award	0.041	(0.127)
Table C.3	Column 4	Contract Award	-0.029	(0.141)
Table C.3	Column 5	Housing Units	2.525	(1.497)
Table C.3	Column 6	Housing Units	1.043	(1.755)
Table C.4	Column 1	Contract Value per Capita	-1.699	(1.72)
Table C.4	Column 2	Contract Award	-0.314	(0.174)
Table C.4	Column 3	Housing Units	1.751	(4.301)
Table C.5	Column 1	First Hand Sales	1.705	(0.86)
Table C.5	Column 2	First Hand Sales	2.537	(0.788)
Table C.5	Column 3	Second Hand Sales	1.401	(0.714)
Table C.5	Column 4	Second Hand Sales	1.442	(0.643)
Table C.6	Column 1	Contract Value per Capita	-0.489	(0.99)
Table C.6	Column 2	Contract Value per Capita	-0.493	(0.99)
Table C.6	Column 3	Contract Award	-0.052	(0.102)
Table C.6	Column 4	Contract Award	-0.052	(0.102)
Table C.6	Column 5	Housing Units	1.004	(1.748)
Table C.6	Column 6	Housing Units	1.007	(1.748)
Table C.7	Column 1	Observed Contract Value per Capita	-1.855	(1.029)
Table C.7	Column 2	Observed Contract Value per Capita	-1.868	(1.03)
Table C.7	Column 3	Observed Contract Value per Capita	-2.153	(0.835)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.266	(0.141)
Table C.7	Column 5	Counterfactual Contract Value per Capita	0.138	(0.093)

Note: 'Table/Figure' refers to the exact item in the article or Appendix from a which a covariate was included. 'Model' refers to the specific panel for figures or column from tables. 'Outcome' lists the associated dependent variable. 'Estimate' shows the point estimate, with corresponding standard error in the next column. All standard errors are clustered at the district-level.

Table E.2: Proportion of Invalid Votes (Local Election)

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	-4.783	(3.57)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	-0.605	(0.359)
Figure 5	Panel (a)	Contract Value per Capita	1.14	(2.729)
Figure 5	Panel (b)	Contract Award	0.043	(0.277)
Figure 5	Panel (c)	Housing Units	5.091	(7.561)
Figure 6	Panel (a)	Contract Value per Capita	1.221	(2.792)
Figure 6	Panel (b)	Contract Award	0.062	(0.284)
Figure 6	Panel (c)	Housing Units	5.978	(7.626)
Figure C.5	Panel (a)	Contract Value per Capita	2.335	(2.829)
Figure C.5	Panel (b)	Contract Award	0.124	(0.286)
Figure C.5	Panel (c)	Contract Value per Capita	2.335	(2.829)
Figure C.5	Panel (d)	Contract Award	0.124	(0.286)
Figure C.6	Panel (a)	Contract Value per Capita	0.525	(3.454)
Figure C.6	Panel (b)	Contract Award	-0.063	(0.358)
Figure C.6	Panel (c)	Contract Value per Capita	0.525	(3.454)
Figure C.6	Panel (d)	Contract Award	-0.063	(0.358)
Table 2	Column 1	Contract Value per Capita	1.347	(2.738)
Table 2	Column 2	Contract Value per Capita	-7.355	(4.361)
Table 2	Column 3	Contract Value per Capita	-2.538	(4.975)
Table 2	Column 4	Contract Award	0.04	(0.277)
Table 2	Column 5	Contract Award	-0.854	(0.453)
Table 2	Column 6	Contract Award	-0.392	(0.509)
Table 2	Column 7	Housing Units	6.156	(7.795)
Table 2	Column 8	Housing Units	-15.91	(13.972)
Table 2	Column 9	Housing Units	-3.533	(11.159)
Table C.2	Column 1	Contract Value per Capita	1.11	(3.103)
Table C.2	Column 2	Contract Award	0.006	(0.316)
Table C.2	Column 3	Housing Units	9.355	(9.215)
Table C.3	Column 1	Contract Value per Capita	2.092	(3.593)
Table C.3	Column 2	Contract Value per Capita	1.663	(3.925)
Table C.3	Column 3	Contract Award	0.048	(0.365)
Table C.3	Column 4	Contract Award	0.001	(0.393)
Table C.3	Column 5	Housing Units	2.679	(7.467)
Table C.3	Column 6	Housing Units	6.086	(7.797)
Table C.4	Column 1	Contract Value per Capita	3.432	(3.656)
Table C.4	Column 2	Contract Award	0.213	(0.368)
Table C.4	Column 3	Housing Units	-2.791	(7.109)
Table C.5	Column 1	First Hand Sales	3.157	(2.008)
Table C.5	Column 2	First Hand Sales	4.507	(2.334)
Table C.5	Column 3	Second Hand Sales	2.83	(1.079)
Table C.5	Column 4	Second Hand Sales	1.154	(1.292)
Table C.6	Column 1	Contract Value per Capita	1.336	(2.742)
Table C.6	Column 2	Contract Value per Capita	1.328	(2.743)
Table C.6	Column 3	Contract Award	0.04	(0.277)
Table C.6	Column 4	Contract Award	0.039	(0.277)
Table C.6	Column 5	Housing Units	5.988	(7.752)
Table C.6	Column 6	Housing Units	5.99	(7.753)
Table C.7	Column 1	Observed Contract Value per Capita	0.791	(4.207)
Table C.7	Column 2	Observed Contract Value per Capita	0.755	(4.21)
Table C.7	Column 3	Observed Contract Value per Capita	0.733	(2.834)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.184	(0.439)
Table C.7	Column 5	Counterfactual Contract Value per Capita	-0.246	(0.234)

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Table E.3: AKP Municipal Mayor

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	-0.104	(0.14)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	-0.014	(0.015)
Figure 5	Panel (a)	Contract Value per Capita	0.002	(0.096)
Figure 5	Panel (b)	Contract Award	0	(0.01)
Figure 5	Panel (c)	Housing Units	-0.157	(0.17)
Figure 6	Panel (a)	Contract Value per Capita	0.026	(0.095)
Figure 6	Panel (b)	Contract Award	0.003	(0.01)
Figure 6	Panel (c)	Housing Units	-0.156	(0.169)
Table 2	Column 1	Contract Value per Capita	0.012	(0.096)
Table 2	Column 2	Contract Value per Capita	-0.244	(0.2)
Table 2	Column 3	Contract Value per Capita	0.042	(0.211)
Table 2	Column 4	Contract Award	0.001	(0.01)
Table 2	Column 5	Contract Award	-0.023	(0.021)
Table 2	Column 6	Contract Award	0.001	(0.023)
Table 2	Column 7	Housing Units	-0.153	(0.17)
Table 2	Column 8	Housing Units	-0.986	(0.317)
Table 2	Column 9	Housing Units	0.523	(0.376)
Table C.2	Column 1	Contract Value per Capita	0	(0)
Table C.2	Column 2	Contract Award	0	(0)
Table C.2	Column 3	Housing Units	0	(0)
Table C.3	Column 1	Contract Value per Capita	-0.09	(0.112)
Table C.3	Column 2	Contract Value per Capita	-0.006	(0.135)
Table C.3	Column 3	Contract Award	-0.012	(0.012)
Table C.3	Column 4	Contract Award	-0.001	(0.014)
Table C.3	Column 5	Housing Units	-0.029	(0.158)
Table C.3	Column 6	Housing Units	-0.165	(0.172)
Table C.4	Column 1	Contract Value per Capita	-0.174	(0.119)
Table C.4	Column 2	Contract Award	-0.02	(0.012)
Table C.4	Column 3	Housing Units	-0.09	(0.212)
Table C.5	Column 1	First Hand Sales	0.019	(0.034)
Table C.5	Column 2	First Hand Sales	0.12	(0.05)
Table C.5	Column 3	Second Hand Sales	0.018	(0.021)
Table C.5	Column 4	Second Hand Sales	0.04	(0.045)
Table C.6	Column 1	Contract Value per Capita	0.01	(0.096)
Table C.6	Column 2	Contract Value per Capita	0.01	(0.096)
Table C.6	Column 3	Contract Award	0.001	(0.01)
Table C.6	Column 4	Contract Award	0.001	(0.01)
Table C.6	Column 5	Housing Units	-0.153	(0.171)
Table C.6	Column 6	Housing Units	-0.154	(0.171)
Table C.7	Column 1	Observed Contract Value per Capita	0.109	(0.128)
Table C.7	Column 2	Observed Contract Value per Capita	0.104	(0.128)
Table C.7	Column 3	Observed Contract Value per Capita	0.103	(0.087)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.002	(0.015)
Table C.7	Column 5	Counterfactual Contract Value per Capita	-0.007	(0.007)

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Appendix E.2 General Election Controls

Table E.4: Turnout Rate (General Election)

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	0.537	(1.135)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.03	(0.117)
Figure 5	Panel (a)	Contract Value per Capita	1.447	(0.846)
Figure 5	Panel (b)	Contract Award	0.136	(0.088)
Figure 5	Panel (c)	Housing Units	0.354	(1.732)
Figure 6	Panel (a)	Contract Value per Capita	1.328	(0.846)
Figure 6	Panel (b)	Contract Award	0.124	(0.088)
Figure 6	Panel (c)	Housing Units	0.15	(1.726)
Figure C.5	Panel (a)	Contract Value per Capita	-0.071	(0.795)
Figure C.5	Panel (b)	Contract Award	-0.02	(0.082)
Figure C.5	Panel (c)	Contract Value per Capita	-0.071	(0.795)
Figure C.5	Panel (d)	Contract Award	-0.02	(0.082)
Figure C.6	Panel (a)	Contract Value per Capita	-0.56	(0.94)
Figure C.6	Panel (b)	Contract Award	-0.064	(0.098)
Figure C.6	Panel (c)	Contract Value per Capita	-0.56	(0.94)
Figure C.6	Panel (d)	Contract Award	-0.064	(0.098)
Table 2	Column 1	Contract Value per Capita	1.44	(0.847)
Table 2	Column 2	Contract Value per Capita	1.658	(1.548)
Table 2	Column 3	Contract Value per Capita	1.012	(1.769)
Table 2	Column 4	Contract Award	0.128	(0.087)
Table 2	Column 5	Contract Award	0.135	(0.166)
Table 2	Column 6	Contract Award	0.083	(0.195)
Table 2	Column 7	Housing Units	0.25	(1.765)
Table 2	Column 8	Housing Units	-3.676	(4.687)
Table 2	Column 9	Housing Units	0.952	(4.968)
Table C.2	Column 1	Contract Value per Capita	0.897	(0.899)
Table C.2	Column 2	Contract Award	0.056	(0.093)
Table C.2	Column 3	Housing Units	-0.973	(2.105)
Table C.3	Column 1	Contract Value per Capita	0.612	(1.01)
Table C.3	Column 2	Contract Value per Capita	1.886	(1.213)
Table C.3	Column 3	Contract Award	0.026	(0.103)
Table C.3	Column 4	Contract Award	0.163	(0.124)
Table C.3	Column 5	Housing Units	0.839	(1.812)
Table C.3	Column 6	Housing Units	0.279	(1.772)
Table C.4	Column 1	Contract Value per Capita	-0.215	(1.265)
Table C.4	Column 2	Contract Award	-0.03	(0.132)
Table C.4	Column 3	Housing Units	-0.147	(3.482)
Table C.5	Column 1	First Hand Sales	1.447	(0.937)
Table C.5	Column 2	First Hand Sales	1.125	(1.091)
Table C.5	Column 3	Second Hand Sales	-2.832	(0.618)
Table C.5	Column 4	Second Hand Sales	-2.965	(0.736)
Table C.6	Column 1	Contract Value per Capita	1.425	(0.846)
Table C.6	Column 2	Contract Value per Capita	1.429	(0.846)
Table C.6	Column 3	Contract Award	0.126	(0.087)
Table C.6	Column 4	Contract Award	0.126	(0.087)
Table C.6	Column 5	Housing Units	0.361	(1.769)
Table C.6	Column 6	Housing Units	0.361	(1.771)
Table C.7	Column 1	Observed Contract Value per Capita	1.542	(0.928)
Table C.7	Column 2	Observed Contract Value per Capita	1.536	(0.928)
Table C.7	Column 3	Observed Contract Value per Capita	3.149	(0.837)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.113	(0.139)
Table C.7	Column 5	Counterfactual Contract Value per Capita	-0.164	(0.098)

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Table E.5: Proportion of Invalid Votes (General Election)

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	-5.896	(4.318)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	-0.607	(0.45)
Figure 5	Panel (a)	Contract Value per Capita	-0.232	(2.893)
Figure 5	Panel (b)	Contract Award	-0	(0.288)
Figure 5	Panel (c)	Housing Units	-1.946	(6.554)
Figure 6	Panel (a)	Contract Value per Capita	0.008	(2.912)
Figure 6	Panel (b)	Contract Award	0.034	(0.291)
Figure 6	Panel (c)	Housing Units	-1.104	(6.367)
Figure C.5	Panel (a)	Contract Value per Capita	1.458	(2.612)
Figure C.5	Panel (b)	Contract Award	0.202	(0.267)
Figure C.5	Panel (c)	Contract Value per Capita	1.458	(2.612)
Figure C.5	Panel (d)	Contract Award	0.202	(0.267)
Figure C.6	Panel (a)	Contract Value per Capita	2.646	(3.655)
Figure C.6	Panel (b)	Contract Award	0.317	(0.388)
Figure C.6	Panel (c)	Contract Value per Capita	2.646	(3.655)
Figure C.6	Panel (d)	Contract Award	0.317	(0.388)
Table 2	Column 1	Contract Value per Capita	-0.108	(2.903)
Table 2	Column 2	Contract Value per Capita	6.968	(6.588)
Table 2	Column 3	Contract Value per Capita	-0.867	(4.378)
Table 2	Column 4	Contract Award	-0.004	(0.288)
Table 2	Column 5	Contract Award	0.77	(0.637)
Table 2	Column 6	Contract Award	0.024	(0.466)
Table 2	Column 7	Housing Units	-2.395	(6.508)
Table 2	Column 8	Housing Units	5.888	(14.079)
Table 2	Column 9	Housing Units	-22.3	(18.858)
Table C.2	Column 1	Contract Value per Capita	-1.631	(3.452)
Table C.2	Column 2	Contract Award	-0.131	(0.355)
Table C.2	Column 3	Housing Units	0.971	(9.377)
Table C.3	Column 1	Contract Value per Capita	-0.607	(3.643)
Table C.3	Column 2	Contract Value per Capita	-0.098	(4.193)
Table C.3	Column 3	Contract Award	-0.05	(0.367)
Table C.3	Column 4	Contract Award	0.015	(0.417)
Table C.3	Column 5	Housing Units	6.751	(9.804)
Table C.3	Column 6	Housing Units	-3.126	(6.539)
Table C.4	Column 1	Contract Value per Capita	-4.268	(5.157)
Table C.4	Column 2	Contract Award	-0.614	(0.561)
Table C.4	Column 3	Housing Units	-3.634	(16.376)
Table C.5	Column 1	First Hand Sales	3.218	(0.998)
Table C.5	Column 2	First Hand Sales	9.063	(2.953)
Table C.5	Column 3	Second Hand Sales	0.866	(0.6)
Table C.5	Column 4	Second Hand Sales	2.469	(1.697)
Table C.6	Column 1	Contract Value per Capita	-0.111	(2.909)
Table C.6	Column 2	Contract Value per Capita	-0.108	(2.909)
Table C.6	Column 3	Contract Award	-0.006	(0.288)
Table C.6	Column 4	Contract Award	-0.006	(0.288)
Table C.6	Column 5	Housing Units	-2.461	(6.529)
Table C.6	Column 6	Housing Units	-2.484	(6.531)
Table C.7	Column 1	Observed Contract Value per Capita	-4.143	(2.963)
Table C.7	Column 2	Observed Contract Value per Capita	-4.299	(2.944)
Table C.7	Column 3	Observed Contract Value per Capita	0.195	(3.014)
Table C.7	Column 4	Counterfactual Contract Value per Capita	0.898	(0.598)
Table C.7	Column 5	Counterfactual Contract Value per Capita	-0.356	(0.502)

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Table E.6: AKP General Election Vote Share

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	1.357	(0.558)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.134	(0.058)
Figure 5	Panel (a)	Contract Value per Capita	0.796	(0.417)
Figure 5	Panel (b)	Contract Award	0.066	(0.043)
Figure 5	Panel (c)	Housing Units	2.35	(0.778)
Figure 6	Panel (a)	Contract Value per Capita	0.81	(0.417)
Figure 6	Panel (b)	Contract Award	0.067	(0.043)
Figure 6	Panel (c)	Housing Units	2.174	(0.765)
Table 2	Column 1	Contract Value per Capita	0.796	(0.417)
Table 2	Column 2	Contract Value per Capita	0.696	(0.841)
Table 2	Column 3	Contract Value per Capita	0.972	(0.824)
Table 2	Column 4	Contract Award	0.056	(0.042)
Table 2	Column 5	Contract Award	0.044	(0.087)
Table 2	Column 6	Contract Award	0.086	(0.089)
Table 2	Column 7	Housing Units	2.311	(0.786)
Table 2	Column 8	Housing Units	4.387	(1.917)
Table 2	Column 9	Housing Units	3.836	(2.332)
Table C.2	Column 1	Contract Value per Capita	0.518	(0.481)
Table C.2	Column 2	Contract Award	0.024	(0.049)
Table C.2	Column 3	Housing Units	1.402	(0.975)
Table C.3	Column 1	Contract Value per Capita	1.817	(0.505)
Table C.3	Column 2	Contract Value per Capita	1.336	(0.584)
Table C.3	Column 3	Contract Award	0.148	(0.051)
Table C.3	Column 4	Contract Award	0.093	(0.059)
Table C.3	Column 5	Housing Units	2.075	(0.817)
Table C.3	Column 6	Housing Units	2.139	(0.795)
Table C.4	Column 1	Contract Value per Capita	1.732	(0.636)
Table C.4	Column 2	Contract Award	0.144	(0.067)
Table C.4	Column 3	Housing Units	2.487	(1.61)
Table C.5	Column 1	First Hand Sales	-0.307	(0.31)
Table C.5	Column 2	First Hand Sales	-0.056	(0.397)
Table C.5	Column 3	Second Hand Sales	0.415	(0.214)
Table C.5	Column 4	Second Hand Sales	0.141	(0.276)
Table C.6	Column 1	Contract Value per Capita	0.781	(0.417)
Table C.6	Column 2	Contract Value per Capita	0.781	(0.417)
Table C.6	Column 3	Contract Award	0.054	(0.042)
Table C.6	Column 4	Contract Award	0.054	(0.042)
Table C.6	Column 5	Housing Units	2.296	(0.782)
Table C.6	Column 6	Housing Units	2.289	(0.783)
Table C.7	Column 1	Observed Contract Value per Capita	0.548	(0.355)
Table C.7	Column 2	Observed Contract Value per Capita	0.552	(0.355)
Table C.7	Column 3	Observed Contract Value per Capita	0.667	(0.284)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.114	(0.074)
Table C.7	Column 5	Counterfactual Contract Value per Capita	-0.194	(0.048)

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Appendix E.3 District Controls

Table E.7: Log Urban Settlements

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	0.11	(0.069)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.013	(0.007)
Figure 5	Panel (a)	Contract Value per Capita	0.032	(0.047)
Figure 5	Panel (b)	Contract Award	0.006	(0.005)
Figure 5	Panel (c)	Housing Units	0.052	(0.084)
Figure 6	Panel (a)	Contract Value per Capita	0.033	(0.047)
Figure 6	Panel (b)	Contract Award	0.006	(0.005)
Figure 6	Panel (c)	Housing Units	0.058	(0.082)
Figure C.5	Panel (a)	Contract Value per Capita	0.035	(0.048)
Figure C.5	Panel (b)	Contract Award	0.007	(0.005)
Figure C.5	Panel (c)	Contract Value per Capita	0.035	(0.048)
Figure C.5	Panel (d)	Contract Award	0.007	(0.005)
Figure C.6	Panel (a)	Contract Value per Capita	0.085	(0.067)
Figure C.6	Panel (b)	Contract Award	0.012	(0.007)
Figure C.6	Panel (c)	Contract Value per Capita	0.085	(0.067)
Figure C.6	Panel (d)	Contract Award	0.012	(0.007)
Table 2	Column 1	Contract Value per Capita	0.034	(0.047)
Table 2	Column 2	Contract Value per Capita	-0.07	(0.127)
Table 2	Column 3	Contract Value per Capita	0.127	(0.097)
Table 2	Column 4	Contract Award	0.007	(0.005)
Table 2	Column 5	Contract Award	-0.002	(0.012)
Table 2	Column 6	Contract Award	0.015	(0.01)
Table 2	Column 7	Housing Units	0.061	(0.088)
Table 2	Column 8	Housing Units	0.068	(0.169)
Table 2	Column 9	Housing Units	0.198	(0.312)
Table C.2	Column 1	Contract Value per Capita	0.026	(0.049)
Table C.2	Column 2	Contract Award	0.006	(0.005)
Table C.2	Column 3	Housing Units	0.106	(0.101)
Table C.3	Column 1	Contract Value per Capita	0.071	(0.084)
Table C.3	Column 2	Contract Value per Capita	0.07	(0.082)
Table C.3	Column 3	Contract Award	0.012	(0.008)
Table C.3	Column 4	Contract Award	0.013	(0.008)
Table C.3	Column 5	Housing Units	0.061	(0.103)
Table C.3	Column 6	Housing Units	0.069	(0.088)
Table C.4	Column 1	Contract Value per Capita	0.049	(0.092)
Table C.4	Column 2	Contract Award	0.007	(0.009)
Table C.4	Column 3	Housing Units	-0.117	(0.187)
Table C.5	Column 1	First Hand Sales	-0.06	(0.056)
Table C.5	Column 2	First Hand Sales	0.005	(0.056)
Table C.5	Column 3	Second Hand Sales	0.052	(0.038)
Table C.5	Column 4	Second Hand Sales	0.069	(0.042)
Table C.6	Column 1	Contract Value per Capita	0.034	(0.048)
Table C.6	Column 2	Contract Value per Capita	0.034	(0.048)
Table C.6	Column 3	Contract Award	0.007	(0.005)
Table C.6	Column 4	Contract Award	0.007	(0.005)
Table C.6	Column 5	Housing Units	0.059	(0.088)
Table C.6	Column 6	Housing Units	0.06	(0.088)
Table C.7	Column 1	Observed Contract Value per Capita	0.088	(0.032)
Table C.7	Column 2	Observed Contract Value per Capita	0.088	(0.032)
Table C.7	Column 3	Observed Contract Value per Capita	0.052	(0.026)
Table C.7	Column 4	Counterfactual Contract Value per Capita	0.018	(0.014)
Table C.7	Column 5	Counterfactual Contract Value per Capita	0.023	(0.007)

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Table E.8: Disaster

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (a)	Contract Value per Capita	0.326	(0.353)
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.028	(0.035)
Figure 5	Panel (a)	Contract Value per Capita	0.232	(0.304)
Figure 5	Panel (b)	Contract Award	0.009	(0.029)
Figure 5	Panel (c)	Housing Units	0.853	(0.466)
Figure 6	Panel (a)	Contract Value per Capita	0.235	(0.303)
Figure 6	Panel (b)	Contract Award	0.01	(0.029)
Figure 6	Panel (c)	Housing Units	0.802	(0.448)
Figure C.5	Panel (a)	Contract Value per Capita	0.176	(0.232)
Figure C.5	Panel (b)	Contract Award	0.01	(0.023)
Figure C.5	Panel (c)	Contract Value per Capita	0.176	(0.232)
Figure C.5	Panel (d)	Contract Award	0.01	(0.023)
Figure C.6	Panel (a)	Contract Value per Capita	0.011	(0.326)
Figure C.6	Panel (b)	Contract Award	-0.009	(0.032)
Figure C.6	Panel (c)	Contract Value per Capita	0.011	(0.326)
Figure C.6	Panel (d)	Contract Award	-0.009	(0.032)
Table 2	Column 1	Contract Value per Capita	0.228	(0.303)
Table 2	Column 2	Contract Value per Capita	0.367	(0.653)
Table 2	Column 3	Contract Value per Capita	0.662	(0.725)
Table 2	Column 4	Contract Award	0.009	(0.029)
Table 2	Column 5	Contract Award	0.016	(0.063)
Table 2	Column 6	Contract Award	0.052	(0.07)
Table 2	Column 7	Housing Units	0.812	(0.455)
Table 2	Column 8	Housing Units	2.002	(0.89)
Table 2	Column 9	Housing Units	1.201	(0.906)
Table C.2	Column 1	Contract Value per Capita	0.305	(0.367)
Table C.2	Column 2	Contract Award	0.013	(0.036)
Table C.2	Column 3	Housing Units	1.218	(0.51)
Table C.3	Column 1	Contract Value per Capita	0.126	(0.265)
Table C.3	Column 2	Contract Value per Capita	0.344	(0.393)
Table C.3	Column 3	Contract Award	0.004	(0.026)
Table C.3	Column 4	Contract Award	0.015	(0.038)
Table C.3	Column 5	Housing Units	0.279	(0.36)
Table C.3	Column 6	Housing Units	0.798	(0.464)
Table C.4	Column 1	Contract Value per Capita	1.259	(0.574)
Table C.4	Column 2	Contract Award	0.106	(0.053)
Table C.4	Column 3	Housing Units	1.607	(0.48)
Table C.5	Column 1	First Hand Sales	0.035	(0.064)
Table C.5	Column 2	First Hand Sales	-0.074	(0.087)
Table C.5	Column 3	Second Hand Sales	-0.001	(0.035)
Table C.5	Column 4	Second Hand Sales	-0.02	(0.039)
Table C.6	Column 1	Contract Value per Capita	0.227	(0.303)
Table C.6	Column 2	Contract Value per Capita	0.227	(0.303)
Table C.6	Column 3	Contract Award	0.009	(0.029)
Table C.6	Column 4	Contract Award	0.009	(0.029)
Table C.6	Column 5	Housing Units	0.809	(0.46)
Table C.6	Column 6	Housing Units	0.809	(0.461)
Table C.7	Column 1	Observed Contract Value per Capita	-0.017	(0.332)
Table C.7	Column 2	Observed Contract Value per Capita	-0.014	(0.332)
Table C.7	Column 3	Observed Contract Value per Capita	0.259	(0.307)
Table C.7	Column 4	Counterfactual Contract Value per Capita	-0.002	(0.013)
Table C.7	Column 5	Counterfactual Contract Value per Capita	0.013	(0.008)

Note: 'Table/Figure' refers to the exact item in the article or Appendix from a which a covariate was included. 'Model' refers to the specific panel for figures or column from tables. 'Outcome' lists the associated dependent variable. 'Estimate' shows the point estimate, with corresponding standard error in the next column. All standard errors are clustered at the district-level.

Table E.9: Log Population

Table/Figure	Model	Outcome	Standard	
			Estimate	Error
Figure 3 / Figure C.1 / Figure C.2	Panel (b)	Contract Award	0.008	(0.033)
Figure C.5	Panel (b)	Contract Award	-0.029	(0.019)
Figure C.5	Panel (d)	Contract Award	-0.029	(0.019)
Figure C.6	Panel (b)	Contract Award	-0.019	(0.021)
Figure C.6	Panel (d)	Contract Award	-0.019	(0.021)
Table 2	Column 4	Contract Award	-0.025	(0.016)
Table 2	Column 7	Housing Units	0.031	(0.172)
Table C.2	Column 2	Contract Award	-0.024	(0.017)
Table C.2	Column 3	Housing Units	-0.056	(0.205)
Table C.3	Column 3	Contract Award	-0.031	(0.018)
Table C.3	Column 4	Contract Award	-0.036	(0.02)
Table C.3	Column 5	Housing Units	0.047	(0.127)
Table C.3	Column 6	Housing Units	0.038	(0.169)
Table C.4	Column 2	Contract Award	-0.088	(0.024)
Table C.4	Column 3	Housing Units	-0.199	(0.231)
Table C.5	Column 1	First Hand Sales	0.351	(0.271)
Table C.5	Column 2	First Hand Sales	0.358	(0.328)
Table C.5	Column 3	Second Hand Sales	0.74	(0.114)
Table C.5	Column 4	Second Hand Sales	0.81	(0.14)
Table C.6	Column 3	Contract Award	-0.025	(0.016)
Table C.6	Column 4	Contract Award	-0.025	(0.016)
Table C.6	Column 5	Housing Units	0.029	(0.173)
Table C.6	Column 6	Housing Units	0.03	(0.173)
Table C.7	Column 1	Observed Contract Value per Capita	0.657	(0.068)
Table C.7	Column 2	Observed Contract Value per Capita	0.652	(0.068)
Table C.7	Column 3	Observed Contract Value per Capita	0.548	(0.053)

Note: ‘Table/Figure’ refers to the exact item in the article or Appendix from a which a covariate was included. ‘Model’ refers to the specific panel for figures or column from tables. ‘Outcome’ lists the associated dependent variable. ‘Estimate’ shows the point estimate, with corresponding standard error in the next column. All standard errors are clustered at the district-level.

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