Electoral Impacts of Memory Imposition*

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Abstract

Governments commemorate events and people to justify their authority, discredit their opponents, or mobilize supporters. We call the use of unavoidable state institutions to advance a contested historical narrative that legitimizes the incumbent memory imposition. Whether memory imposition actually benefits incumbents at the ballot box remains an open question. We study a case of memory imposition in which Turkey's long-governing AKP renamed schools to celebrate resistance to the failed 2016 coup attempt. Combining school registries with a neighborhood-level panel of four elections and exploiting the staggered timing of renamings with counterfactual estimators, we find that rather than consolidating support, memory imposition backfired: renamings reduced the vote shares of the AKP by 1.6 points and its ultra-nationalist ally, the MHP, by 3.5 points, while the center-left CHP and pro-Kurdish HDP each gained about 1.2 points. Commemoration experienced as a contested display of power can cost incumbents votes.

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1. Introduction

Governments use commemoration to promote an account of the recent or distant past that justifies their authority, discredits the opposition, and mobilizes their supporters. We call this *memory imposition*: commemoration that valorizes the incumbent government's actions, delivered through state institutions that citizens cannot easily avoid, and targeting a population in which the government's historical narrative is not widely accepted. Tactics include designating national holidays; renaming streets, schools, and other public spaces; and rewriting public school curricula. These commemorations celebrate selected events and people — and omit others — crafting a historical narrative that presents the government's past actions and continued rule as warranted, if not inevitable.

Memory imposition is common across regime types, and governments invest considerable resources in it. In Russia, President Putin has turned the annual Victory Day celebrations into a stage for his own leadership, transforming a shared national memory into a source of personal legitimacy (Wood 2011). In the United States, the Trump administration moved to reinstate Confederate monuments and removed exhibits on slavery from national parks and historic sites under a 2025 executive order, "Restoring Truth and Sanity to American History," which called for a more celebratory account of the nation's past (Jackson 2025). In Egypt, after security forces killed hundreds of protesters at Cairo's Rabaa al-Adawiya Square in 2013, the Sisi government renamed the square after the prosecutor general who had authorized the dispersal, in line with its official narrative that the crackdown had saved the nation from terrorism (Al Jazeera 2015). Incumbents may pursue such efforts in the belief that favorable historical narratives bolster their legitimacy and public support, yet whether memory imposition actually benefits them at the ballot box remains an open question. On the one hand, memory imposition operates through institutions that citizens encounter frequently and cannot opt out of, ensuring repeated exposure to the official narrative that may persuade citizens or mobilize the incumbent's supporters (Festinger 1957). On the other hand, unavoidable exposure to a contested narrative may provoke resentment and backlash (Villamil and Balcells 2021).

The growing empirical literature on memory politics has focused on state efforts to confront past atrocities, whether by expressing contrition for the state's role (Balcells et al. 2022; De Juan et al. 2025; Villamil and Balcells 2021) or by teaching about past atrocities (Balcells and Voytas 2025, 2023; Balcells et al. 2026) and on commemorative initiatives led by non-governmental actors (Ankori-Karlinsky 2025; Ditlmann et al. 2025; Zhukov 2025; Rahnama 2025; Rozenas and Vlasenko 2022). Far less is known about the political effects of memory imposition, despite its prevalence.

To address this gap, we study the electoral impacts of a large-scale campaign of memory imposition in Turkey. The incumbent Justice and Development Party (*Adalet ve Kalkınma Partisi*, AKP), led by President Erdoğan, has presented the failed coup attempt of July 15, 2016, as a second war of independence and the AKP as the savior of Turkish democracy (Solomonovich 2021; Hammond 2020; Taş 2018). To advance this narrative, the government adopted a range of commemorative practices, from a national holiday to a museum honoring those who resisted the coup. Among the most far-reaching was the top-down renaming of public schools in over 650 neighborhoods, carried out by centrally appointed provincial governors rather than local communities. As a point of comparison, the most inclusive counts identify 330 schools in the US that carry the names of individuals associated with the Confederacy (Riser-Kositsky and Mitchell 2020), the

product of more than a century of commemoration in a country with roughly four times Turkey's population. New school names include those of individuals who died resisting the coup and were granted official *martyr* status, as well as commemorative names such as "15 July Democracy School."

We construct a neighborhood-level panel comprising 50,628 neighborhoods over four general elections (June 2015, November 2015, 2018, and 2023), using school registries to code whether a neighborhood contains a school renamed to commemorate resistance to the coup. Because neighborhoods are small geographic units, averaging 1,128 residents aged 18 and older, and schools must display signage legible from 50 meters, a renamed school is likely to be a familiar local landmark (Burdick–Will 2018). More importantly, schools double as polling stations, so renamed schools serve as everyday memorials that voters encounter even as they cast their ballots (Berger et al. 2008; Bloom et al. 2023).

We address two questions. First, which neighborhoods were more likely to have a school renamed? We know little about who memory imposition is meant to influence: incumbents may target their own strongholds to mobilize supporters or more competitive areas to persuade or demobilize opponents. Although we cannot observe how appointed officials chose which schools to rename, we test whether neighborhoods' pre-coup political composition and electoral trends predict renaming. Neighborhoods with renamed schools gave somewhat more support to the Nationalist Movement Party (*Milliyetçi Hareket Partisi*, MHP), the AKP's ally since the coup attempt, and somewhat less to the AKP and the pro-Kurdish HDP. These differences are modest in absolute terms: the predicted probability of renaming is 1.7% in a neighborhood where just under a quarter of voters supported the MHP (the 90th percentile), compared with 0.9% where MHP support was negligible. We find that electoral trends have virtually no predictive power for school renaming. This no difference in trends is reassuring for the next analysis.

Second, we estimate the causal effect of school renaming on vote shares, exploiting the staggered timing of renamings across neighborhoods. We find that renaming schools to commemorate resistance to the coup reduced vote shares for both the MHP and AKP: the MHP's vote share falls by about 3.5 points; the AKP's share falls by about 1.6 points. By contrast, the main opposition parties each gained about 1.2 points. That these gains went to the very parties that had contested the AKP's account of the coup is consistent with a backlash effect: both the center-left Republican People's Party (*Cumhuriyet Halk Partisi*, CHP) and the pro-Kurdish Peoples' Democratic Party (*Halkların Demokratik Partisi*, HDP), succeeded in 2023 by the Peoples' Equality and Democracy Party (*Halkların Eşitlik ve Demokrasi Partisi*, DEM) had campaigned against Erdoğan's use of the coup attempt to consolidate his power.[1]

Additional analyses highlight the robustness of our findings. Pre-treatment estimates show no differential trends between treated and untreated neighborhoods, which supports the parallel trends assumption. The treatment is not confounded by curriculum changes because the centralized education system applies them nationwide. Renaming is also uncorrelated with teacher dismissals, which were widespread after the coup attempt. These results are robust to excluding neighborhoods that may have been subject to other crackdowns, namely curfews were imposed or schools associated with the Gülen Movement were located. Our results also hold when we include province-by-election or district-by-election fixed effects, which compare

1. While preserving school names was not a major rallying cry for these parties, both campaigned against further democratic backsliding following the coup attempt.

treated neighborhoods only with untreated neighborhoods in the same province or district, absorbing local shocks and any governor-specific influences.

Our study contributes to the literature by introducing the concept of memory imposition and offering empirical evidence on both its targeting and its electoral impacts. Renamings were somewhat more likely in pro-government strongholds, but rather than consolidating support for the governing parties, memory imposition backfired, reducing their vote shares and increasing support for the main opposition parties. Our findings suggest that incumbent-led commemoration can be self-defeating when the narrative it advances is seen as a justification for political repression rather than as an expression of shared identity. Even if the intention is to unify, we show that the result can be increased polarization.

2. Conceptual Framework

2.1 Defining Memory Imposition

The growing literature on memory politics studies the origins and effects of efforts to shape what people remember and how their understanding of the past affects present-day political attitudes and behaviors. While the literature's constituent studies offer rich descriptions of specific cases, the field does not yet have an accepted taxonomy that encodes the features that distinguish different forms of commemoration. Yet, we expect that content, ubiquity, and controversy jointly affect the direction and magnitude of any effects. We do not attempt such a taxonomy here. Instead, we focus on one form of commemoration that is common but understudied, which we call *memory imposition*: commemoration that (1) valorizes the incumbent government's actions, (2) is delivered through state institutions that citizens cannot easily avoid, and (3) advances an interpretation of recent or distant past events or figures that is not broadly shared across the population. Memory imposition is thus celebratory, pervasive, and contentious.

What earns the label of memory imposition? In Russia, President Putin has used the nation-wide "Victory Day" celebrations along with "Memory lessons" held in schools, to highlight Russia's heroic role in World War II and, more controversially, to cast himself as the natural heir to the Soviet victory and to proclaim Russia's restored status as a great power (Wood 2011). In Poland, the Law and Justice (PiS) government's 2016 "de-communization" law required municipalities to rename streets commemorating the communist era and empowered centrally appointed provincial governors to impose new names where local councils refused, in some cases naming streets after the party's late co-founder, President Lech Kaczyński (Ochman 2024). Unlike the bottom-up, locally initiated renamings of the early 1990s, this campaign was imposed from above, resisted by opposition-run city councils in the courts, and sharply divided public opinion along partisan lines (Ochman 2024). In our own case, Turkey's incumbent AKP has chosen to commemorate the civilian resistance to the July 15 coup attempt with a narrative that casts Erdoğan and his supporters as the nation's saviors, promoted through various practices including a national holiday, the renaming of public places, and changes to school curricula (Taş 2018). Opposition parties, which also condemned the coup attempt, have contested this narrative, arguing that it served to justify the erosion of democracy and Erdoğan's concentration of power (Aydın-Düzgit and Balta 2018).

The Trump Administration's 2025 executive order, "Restoring Truth and Sanity to American History," may be a less obvious case. It was celebratory in the sense that it promoted an account of American great-

ness while removing content said to disparage Americans (Executive Office of the President 2025). It was also controversial, drawing widespread criticism from historians and civil society groups (Hellmann 2026; Tucker 2025). While changes within any single institution, such as the Smithsonian, one of the order's primary targets, could easily be avoided, the order's reach extended across the 433 units of the National Park System, which recorded more than 330 million visits in 2024, as well as monuments in public squares, making its narrative difficult to avoid in aggregate. Thus, the widespread reach of the order's effects satisfies our definition.[2]

Many existing studies evaluate interventions in memory politics that do not meet this definition. Turkoglu et al. (2023) study the effect of *Stolpersteine* in Germany, small monuments embedded in streets and sidewalks that acknowledge individuals persecuted by the Nazis. These privately funded stones acknowledge the German state's role in past atrocities, which is not a contentious act of contrition in a country where it is illegal to deny or trivialize the Holocaust. While more controversial, the *Last Address* memorials to victims of Stalin's Great Terror are, like *Stolpersteine*, privately funded and does not valorize the incumbent government (Zhukov 2025). Finally, Balcells et al. (2022), De Juan et al. (2025), and Balcells and Voytas (2025) study the effects of exhibitions related to past conflicts in Chile, Germany, and Northern Ireland, respectively. Although these exhibitions receive varying degrees of state funding, none is celebratory: in Chile, the museum documents human rights violations under Pinochet's dictatorship; in Germany, the exhibition describes the military's role in the Holocaust; and in Northern Ireland, the exhibition is intentionally neutral. Without denying the importance of museums in memory politics, they are not hard to avoid.

Applying our definition thus reveals that extant empirical work has rarely studied memory imposition. Instead, it has focused on an important but different set of interventions: commemoration that acknowledges rather than celebrates the state's role in past atrocities, that initiated by non-governmental actors, or that citizens can easily avoid. Whether and how memory imposition, a different form of intervention, shapes political behavior remains an open question. In the next section, we draw on existing theory to develop expectations about how it might do so.

2.2 Electoral Effects of Memory Imposition

The effect of memory imposition on the incumbent's electoral support is prima facie ambiguous. Existing research offers expectations that point in opposite directions, and plausible models of voter behavior support each. On the one hand, memory imposition could strengthen the incumbent. Collective memory is socially constructed (Halbwachs 1992), and states have long used invented traditions, rituals, and national symbols to legitimize their authority (Hobsbawm and Ranger 1983; Nora 1989). By making their account of the past unavoidable across many domains of life, governments may lead some voters to accept (parts of) it and, as a consequence, to view the incumbent more favorably (Zaller 1992). Research on state-sponsored messaging shows that such exposure can shift political attitudes and behavior in the direction of the message (Adena et al. 2015; Aytaç 2021; Blouin and Mukand 2019), and features of the physical environment, even those

2. The US case is instructive, highlighting that the features we use to define memory imposition are continuous and, as with most efforts to code political phenomena, labeling necessitates judgment calls. Nonetheless, we find it useful to identify three dimensions that allow us to organize different forms of commemoration, even if it remains difficult to precisely identify threshold values along each dimension.

encountered only in passing, can do the same by making particular considerations more salient and accessible (Burbank 1995; Butz et al. 2007; Bruter and Harrison 2021). Such cues need not be encountered daily to matter: where people vote can affect how they vote (Berger et al. 2008; Bloom et al. 2023). Even without changing anyone's views, memory imposition can rally voters already sympathetic to the government's account of the past, strengthening their turnout and loyalty (Lazarsfeld et al. 1944).[3] In the United States, for example, a 2025 YouGov poll found that Republicans were 21 percentage points more likely than Democrats to say that American history museums put too much emphasis on slavery, and 12 points more likely to say that museums should emphasize American achievements (Orth and Montgomery 2025). Trump's executive order could mobilize such voters without altering their understanding of American history. By promoting its narrative across public space, the government may also signal its strength, its capacity to reshape the everyday environment, and its hostility to alternative viewpoints. Such signals could deter dissent and demobilize opposition voters, or lead some voters to side with an incumbent that appears dominant and durable (Huang 2015).

On the other hand, commemoration is rarely a consensual project and is often the object of political struggle over whose past is remembered and how (Gillis 1994), and therefore, the incumbent's narrative may alienate those who do not share it and produce backlash effects (Zhukov 2025; Villamil and Balcells 2021; Rozenas and Vlasenko 2022). Voters may resent the politicization of the recent or distant past, or object to the specific account the government seeks to impose (Edelman 1985). We defined memory imposition as a campaign that targets a population where the government's interpretation of past events or figures is not a consensus view. Contested commemoration can alienate those who do not share the celebrated narrative. For example, in the US South, Black and Latino residents report feeling less safe and welcome at courthouses that display Confederate monuments (Wager and Britt 2025). Rather than being persuaded, those who reject the narrative may hold their views more firmly when repeatedly confronted with it (Festinger 1957; Taber and Lodge 2006), and state propaganda can backfire among audiences not predisposed to accept it (Adena et al. 2015). Such resistance can turn into collective action. In Hong Kong, parents and students staged mass protests against a compulsory patriotic education curriculum, which the government eventually withdrew (Morris and Vickers 2015); in Poland, many people incriminated themselves in protest against the 2018 "Holocaust Law" (Cadier and Szulecki 2020). And while displays of power may deter dissent, they can also mobilize those who reject the government's account (Huang 2015). Where organized political actors such as opposition parties contest the narrative, voters may bristle at what they see as attempted indoctrination and throw their support to those parties.

These mechanisms are not mutually exclusive: they could operate simultaneously on different voters, and over different time horizons. We therefore have no strong directional expectation: the net effect of memory imposition on the incumbent's vote share could be negative, null, or positive, depending on which mechanisms dominate over the period we study. Our aim is to estimate this net effect; identifying the mechanisms that produce it is a task for future research.

3. Citizens need not support the governing party to be sympathetic to the historical narrative it adopts.

3. Case

3.1 15 July Coup Attempt and its Aftermath

Since transitioning to a multiparty system in 1950, Turkey has experienced several successful and failed military interventions, including the 2016 attempt. What distinguished the 2016 attempt was the role of popular mobilization, spurred by President Erdoğan, who called on citizens to take to the streets and resist (Esen and Gumuscu 2017). Thousands responded by pouring into streets, and in the ensuing violence 252 people were killed, most of them civilians, and more than two thousand were wounded (Esen and Gumuscu 2017).

The failed coup attempt is widely attributed to the followers of Fethullah Gülen, a US-based Turkish Islamist cleric (Esen and Gumuscu 2017; Taş 2018). Over decades, the movement built its ranks within the Turkish bureaucracy, judiciary, police, and military, effectively constituting a state within a state. When Erdoğan's Islamic conservative AKP came to power in 2002, the AKP and the Gülenist movement entered a "marriage of convenience," united by a shared opposition to the secular establishment. The AKP's electoral dominance and the Gülenists' expansive network across state institutions complemented each other (Taş 2018 p. 5). However, as competition over control of state institutions replaced cooperation over the years, the former allies became bitter rivals, and the coup attempt represented the disintegration of an alliance that had shaped Turkish politics for more than a decade (Esen and Gumuscu 2017).

The coup attempt came amid democratic backsliding that had been underway since at least 2013, marked by growing restrictions on press freedom, judicial independence, and civil society (Freedom House 2018), and it accelerated this trajectory dramatically. Five days after the attempt, Erdoğan's government declared a state of emergency, which allowed the executive to rule by decree with minimal parliamentary oversight. This state of emergency was renewed seven times and lasted until July 2018.

Erdoğan used these emergency powers to consolidate power and silence dissent. The crackdown that ensued extended well beyond those implicated in the coup attempt, targeting Kurdish political actors, leftists, secular and liberal critics, academics, journalists, and media organizations (Taş 2018; Esen and Gumuscu 2017), as well as tens of thousands of public employees, including teachers and education workers, dismissed by emergency decree for alleged links to the Gülen Movement (Human Rights Watch 2016). This concentration of executive power also enabled the government to impose its commemorative framing of July 15 while suppressing dissenting interpretations (Hammond 2020).

3.2 Memory Imposition in the Post-Coup Period

Erdoğan has long sought to reorient official memory away from the secular republic founded by Atatürk in 1923 and toward the religious, Ottoman past (Hammond 2020). The failed coup attempt offered a further opportunity to pursue this aim: the popular resistance to the coup was cast as a second war of independence, comparable to the American or French Revolution (Taş 2018; Hammond 2020). In this account, the Turkish people had risen up to "liberate" the state from Gülenist infiltrators, who were not former political allies of the AKP, but "pawns of hostile Western powers" operating in secret (Taş 2018 p. 8). The commemoration of July 15 focused attention on this narrative. Civilians killed in the resistance were given official martyr status, which "elevates [their] act of resistance to the sacred" and casts them "...not as mere casualties or victims

but as willing sacrifices for the nation" (Hammond 2020 p. 543). July 15 was declared a national holiday: "Democracy and National Unity Day", a national commemoration not anchored in the secular tradition but in the AKP's own conservative and religious ideology (Solomonovich 2021). State institutions carried out further commemorative activities. The Ministry of National Education mandated national-level uniform commemorative practices in all schools, which revised curricula, required every class to create a panel board including pictures, news, or stories regarding July 15, and obligated each school to establish dedicated "memorial corners" (Aydemir and Decker 2025). These rituals were designed to instill the government's account of July 15 through public education. As we discuss below, hundreds of schools were also renamed to commemorate the coup.

The commemorative practices soon took on an explicitly partisan character as the official narrative valorized Erdoğan and the AKP, and presented the crackdown that followed as a noble effort to honor the martyrs' sacrifice and save Turkey's democracy. The center-left CHP and pro-Kurdish HDP rejected the government's framing of July 15 and opposed extensions of emergency rule (Taş 2018; Esen and Gumuscu 2017), while Erdoğan increasingly equated criticism of the crackdown with sympathy for the putschists (Taş 2018). The rift also extended to commemoration, and by July 2017, opposition parties were not even invited to the official parliamentary ceremony marking the first anniversary of the coup attempt (Cumhuriyet 2017a).

Two fundamentally irreconcilable accounts of what July 15 meant, and of what kind of political order it was being used to construct, emerged. The opposition advanced a counter-narrative focused on democratic erosion and Erdoğan's concentration of power. The CHP drew a sharp line between July 15, which, in senior CHP parliamentarian Özgür Özel's formulation, 'belongs to all of us,' and July 20, the date the state of emergency was declared, which it characterized as the beginning of the deliberate political exploitation of the coup attempt (Cumhuriyet 2017b). Interviews with opposition parliamentarians and civil society figures conducted in early 2017 suggest that opposition political elites overwhelmingly viewed July 15 not as a unifying moment but as one that deepened political polarization (Aydın-Düzgit and Balta 2018). The opposition's dissent also manifested in concrete political resistance, as both the CHP and HDP voted against extensions of emergency powers (Esen and Gumuscu 2017).

3.3 Renaming Public Schools

While activities around the anniversary of the coup attempt, officially designated "Democracy and National Unity Day," generate a temporary nationwide burst of attention, renaming public schools is a more enduring, if geographically uneven, form of commemoration. By 2023, the government had renamed schools in 670 neighborhoods (1.3% of all neighborhoods) to include "15 July" or the name of a martyr killed resisting the coup, embedding the commemoration permanently in local landscapes and polling places. Residents may encounter these names in passing, on mandatory signage legible from 50 meters away; as parents, when sending their children to school; and as voters, when casting their ballots.

School naming in Turkey is highly regulated and centralized. The Ministry of National Education's "Regulation on the Opening, Closing, and Naming of Institutions" specifies which names schools may receive: natural or local features; officially recognized martyrs (*şehit*) and veterans (*gazi*); distinguished

figures in science, culture, and the arts; Atatürk, the founder of the Turkish Republic; and places, dates, and events of historical importance. The regulation prohibits renaming institutions that have acquired a historical identity and are known locally by their name (Millî Eğitim Bakanlığı 2017), which in principle limits renamings to schools with generic names.

The process is bureaucratic and top-down. A provincial education commission of centrally appointed officials proposes names to the governor, who submits them to the Ministry for approval. Governors (*vali*) are thus the key actors. They are appointed by the central government (since 2018, directly by the President), rotated every two to four years, and rarely serve in their home provinces, so they have few social or political ties to the provinces they administer. Political loyalty has also long been an important, if unofficial, criterion for their appointment (Yıldız 2017). The renaming process therefore does not formally include, and is unlikely to reflect, the preferences of local residents.

Governors are not required to justify publicly which schools they rename. Given their career incentives, we expect them to choose targets that benefit the President politically. They may target AKP or MHP strongholds, where the government's narrative is likely to resonate. Alternatively, much as Confederate monuments were disproportionately built in Southern counties where political control remained contested (Barceló et al. 2026), they may target opposition areas. There, commemorating July 15 or specific martyrs might persuade voters to support the government and its use of emergency powers, or renaming a prominent public institution might signal the government's dominance and demobilize the opposition.

Organized resistance emerged in only a handful of the hundreds of renamings, typically led by alumni. In Düzce, the alumni association of Düzce Anadolu Lisesi formally objected to the proposed renaming, and the governor initially reversed the decision, only for the Minister of National Education to intervene and reinstate the new name, Düzce 15 Temmuz Şehitler Anadolu Lisesi (Kamudan Net Haber 2023). In İzmir, alumni of Karşıyaka Anadolu Lisesi failed to prevent the school's renaming despite attracting significant public attention. In Trabzon, criticism from local politicians and the press did not halt the renaming of Hasan Ali Yücel Primary School (Evrensel 2016).

The rarity of local opposition to the renamings suggests that governors may have targeted sympathetic localities, although the risks of opposing the government in the post-coup climate may also have deterred residents from objecting. More importantly, even where community resistance materialized and sometimes initially succeeded, the central government prevailed, further evidence that this form of commemoration was imposed by the center.

4. Data

4.1 Vote Shares

The primary outcomes of interest are parties' vote shares in general elections at the neighborhood-level.[4] We focus on the four most recent elections for which detailed data are available: June 2015, November 2015, June 2018, and May 2023. We focus on the vote shares of four major parties: from left to right, the

4. During our period of analysis, Turkey comprised 81 provinces subdivided into 970 districts, which were further divided into 50,628 neighborhoods — small administrative units averaging 1,128 eligible voters.

pro-Kurdish HDP/DEM[5], the secular, center-left CHP, the governing AKP, and the ultra-nationalist MHP, which has been allied with the AKP since the failed coup attempt and joined it in a formal electoral alliance in 2018 and 2023.[6] All electoral data are sourced from the Supreme Election Council.

4.2 School Names

We use the administrative school registry maintained by the Ministry of National Education to locate schools within neighborhoods and to determine whether and when each was renamed. We code a binary indicator for whether a neighborhood contains one or more schools named in reference to July 15, 2016 or a martyr killed resisting the coup. Of 670 ever-treated neighborhoods, 598 are treated by the 2018 election (that took place during the state of emergency) and another 72 are treated between the 2018 and 2023 elections.

More than a quarter of treated schools refer to the date (*15 Temmuz*) such as 15 July High School, 15 July Martyrs Primary School, and 15 July National Will Secondary School. Around 10% are named after Ömer Halisdemir, an officer who became the most celebrated symbol of the resistance. The remaining schools are named after other martyrs: of the 252 martyrs, 237 have at least one school named after them. All schools named after individuals carry the title *Şehit* (Martyr) before the person's name. Some also include the date (e.g., 15 July Martyr Erkan Yiğit Secondary School), but most refer only to the individual (e.g., Martyr Mustafa Serin Secondary School).

4.3 Teacher Dismissals

Because school renamings coincided with a broad post-coup crackdown, as discussed above, our estimates could capture the effects of repression rather than commemoration if the two targeted the same neighborhoods. We therefore compile local indicators of coup-related state repression over the same period that could confound them.

In the aftermath of the coup attempt, the government dismissed hundreds of thousands of public servants, including Ministry of National Education personnel, through executive decrees. The full list of 34,193 individuals dismissed from the Ministry of National Education was published in the Official Gazette and includes personal information such as the district of employment. We digitize this data and compute totals at the district-level, as neighborhood-level information is not available.

4.4 Gülen Movement Schools

The government attributed the coup attempt to the Gülen Movement, and individuals and institutions linked to the movement bore the brunt of the subsequent crackdown. Especially relevant for our study, schools deemed affiliated with the movement were closed by emergency decree and their property confiscated. Many of these buildings were later reopened as public institutions, including public schools. We identify these schools using Emergency Decree No. 667, published in the Official Gazette a week after the coup attempt, which ordered their confiscation and listed each school with its address. Matching these addresses to neighborhoods, we find that the decree confiscated 997 schools in 468 neighborhoods.

5. They ran as the Green Left Party, YSP, in 2023

6. In 2023, the CHP partnered with the İYİ Party; where the CHP did not run, we impute the İYİ Party's vote share.

4.5 Curfews

School renamings may also coincide with other repressive measures, especially if they target opposition strongholds. Most such measures are implemented at the national, provincial, or district level, which is too coarse for our neighborhood-level design. An important exception is the curfews that were imposed on specific neighborhoods, predominantly in the Kurdish southeast, during security operations against the PKK beginning in August 2015 (Canavan and Turkoglu 2025). We use data from the Human Rights Foundation of Turkey (Türkiye İnsan Hakları Vakfı), an NGO that documents human rights violations, including where and when curfews were imposed. Because these records are available only at the district level, we identify the affected neighborhoods using government websites and local and national news sources. In total, 627 neighborhoods experienced curfews between 2015 an 2018.

5. Where Did the Government Rename Schools?

As discussed above, the placement of memorials can be strategic (Barceló et al. 2026): the government could target opposition areas, to persuade opponents or signal its dominance, or its own strongholds, to mobilize potential supporters. We first examine where the government renamed schools, testing whether pre-coup election outcomes predict whether a neighborhood had a school renamed. This analysis also helps us understand the selection process and thus strengthens the empirical strategy we use to estimate the effects of renaming on electoral outcomes.

5.1 Modest Targeting of Nationalist Strongholds

We begin by comparing the electoral profiles of ever-treated and never-treated neighborhoods in the two elections prior to the coup attempt. For each of these groups, we compute the average vote share for each party in June 2015 and November 2015, as well as how much each party's share changed between the two elections. Two patterns stand out in Table 1. First, ever-treated neighborhoods exhibit higher MHP support and less HDP support: the MHP's vote share is 3.7 points higher in June 2015 and 3.4 points higher in November 2015. By contrast, the pro-Kurdish HDP's vote share is around 2.8 points lower in both elections in ever-treated neighborhoods. Second, the differences in AKP and CHP support, while statistically significant, are small relative to control levels.

Next, we use a cross-sectional logit model to predict whether a neighborhood will have a school renamed based on parties' vote shares in November 2015. This model has an in-sample AUC of 0.65: for a randomly drawn pair of ever-treated and never-treated neighborhoods, the model assigns the treated neighborhood a higher predicted probability of having a school renamed 65% of the time, compared with 50% for random guessing (see Appendix Figure A.1). We use this model to compute the predicted probability of treatment in different parties' strongholds in Table 2. For example, the 90th percentile of MHP support is a neighborhood where the party receives 23% of the vote. Across these MHP strongholds (those between the 89th to 91st percentiles), the AKP receives 51% of the vote on average, the CHP 20%, and the HDP receives just over 1%. The predicted probability of treatment in a neighborhood with this political composition is 1.7%. By contrast, the predicted probability of treatment in an HDP stronghold is just 0.9%.

Table 1: Balance on Political Trends

	Never-treated		Ever-Treated			
	Mean	SD	Mean	SD	Diff. in Means	Std. Error
AKP						
Share (Jun 2015)	46.31	(25.57)	44.18	(17.57)	-2.13*	(0.69)
Share (Nov 2015)	54.96	(27.55)	53.04	(19.33)	-1.92*	(0.76)
Share Change (Nov - Jun)	8.66	(9.14)	8.86	(5.14)	0.21	(0.20)
CHP						
Share (Jun 2015)	19.34	(21.43)	20.51	(15.87)	1.17	(0.63)
Share (Nov 2015)	19.19	(22.02)	20.92	(16.62)	1.73*	(0.66)
Share Change (Nov - Jun)	-0.15	(5.73)	0.41	(2.55)	0.55*	(0.10)
HDP						
Share (Jun 2015)	14.16	(28.23)	11.32	(19.52)	-2.83*	(0.77)
Share (Nov 2015)	11.96	(25.97)	9.20	(17.86)	-2.76*	(0.71)
Share Change (Nov - Jun)	-2.20	(6.09)	-2.12	(3.28)	0.07	(0.13)
MHP						
Share (Jun 2015)	15.31	(12.52)	19.00	(10.94)	3.69*	(0.43)
Share (Nov 2015)	10.57	(9.86)	14.01	(8.85)	3.44*	(0.35)
Share Change (Nov - Jun)	-4.73	(6.69)	-4.98	(4.65)	-0.25	(0.18)
OTHER						
Curfew Imposed	0.01	(0.11)	0.01	(0.12)	0.00	(0.00)
Gülen School Present	0.01	(0.08)	0.28	(0.45)	0.28*	(0.02)

Table 1: Neighborhood-level means and standard deviations of party vote shares (%) in the two pre-coup elections. Ever-treated neighborhoods (658) had a school renamed to commemorate 15 July by 2023; never-treated neighborhoods (49,341) did not. Share change is the November minus the June 2015 share. The last two rows are indicators for neighborhoods placed under curfew and for those containing a Gülen-affiliated school closed after the coup. The final columns report the difference in means (ever- minus never-treated) and its standard error. * $p < 0.05$.

MHP strongholds are almost twice as likely to be targeted as HDP strongholds, yet the absolute differences in predicted probabilities across parties' strongholds are small. This suggests that school renamings were only weakly shaped by neighborhoods' prior political composition.

More importantly for our identification strategy, we also examine whether changes in parties' vote shares before the coup attempt predict treatment. We find that the trends in parties' support are statistically indistinguishable between ever-treated and never-treated neighborhoods for the MHP, AKP, and HDP. The CHP is an exception: ever-treated neighborhoods saw a slightly larger average change in CHP support in the pre-treatment period (0.55 pp). We note that in both the ever- and never-treated groups, CHP vote share is quite stable between June and November 2015, especially relative to the other parties: the average change is less than a half percent in both groups. We return to this concern below and use trajectory balancing, which constructs synthetic counterfactuals that generate balance on pre-treatment trajectories.

Table 2: Predicted Probability across Parties' Strongholds

Stronghold	MHP	AKP	CHP	HDP	Pred. Prob.	95% CI
MHP	**23**	51	20	1.2	0.017	[0.016, 0.019]
AKP	4.7	**89**	2.6	1.1	0.010	[0.009, 0.012]
CHP	12	30	**51**	4.5	0.014	[0.013, 0.016]
HDP	1.3	31	7.3	**57**	0.009	[0.008, 0.011]

Table 2: A party's strongholds are neighborhoods between the 89th and 91st percentiles of its November 2015 vote share. The first four columns give average vote shares (%) in each set of strongholds; bold marks the defining party. Pred. Prob. is the probability of ever having a school renamed, predicted at that average profile from a logit of ever-treated on November 2015 MHP, AKP, CHP, and HDP shares across 49,999 neighborhoods (in-sample AUC = 0.65). The unconditional rate is 0.013.

Similar to our analysis of levels, we attempt to predict which neighborhoods get treated using changes in parties' vote shares, and find that our cross-sectional logit model is now little better than a coin flip, with an in-sample AUC of 0.59 (see Appendix Figure A.1). Trends in parties' pre-coup support thus do not appear to be connected to where schools were renamed.

5.2 Limited Overlap between Renamings and Repression

We next examine whether the renaming of schools coincided with other forms of repression and efforts to consolidate authority described above. Doing so serves two purposes: it reveals whether the government layered these tactics, and importantly, it identifies potential threats to our strategy for estimating the effects of renaming schools on electoral outcomes.

Teacher Dismissals. We begin with dismissals of teachers and school administrators by emergency decree, which we measure at the district level. Dismissals were unevenly distributed across districts, ranging from none in some to 11 in the median district and more than 100 at the 90th percentile. Figure 1 plots, for each district, teacher dismissals per 1,000 students against renamed schools per 1,000 students. We normalize both by the number of students because districts with more students have more teachers and schools that could be targeted. At the district-level, we find no relationship between teacher dismissals and school renamings: a linear fit is effectively flat and statistically null, and the R^2 is just 0.0024; more flexible, non-linear models do not uncover a meaningful relationship.

The purge of teachers and civil servants across Turkey's education system was thus not concentrated in the same neighborhoods where the government renamed schools to commemorate resistance to the coup.

Gülen Schools. Renamings overlapped substantially with one form of repression: schools were renamed in 40% of the 468 neighborhoods where the government took over or closed Gülen-affiliated schools, compared with 1.3% of neighborhoods overall.

This generates the level difference we report in Table 1 and motivates the sub-group analysis we perform in the next section. Importantly, our analysis show that the electoral effects of renaming schools are robust to excluding these neighborhoods.

Curfews. Renamings did not overlap with the curfews imposed during counterinsurgency operations in the southeast: the share of neighborhoods placed under curfew was nearly identical among ever-treated and never-treated neighborhoods (see Table 1).

Figure 1: Teacher Dismissals and Treatment Saturation at District-Level

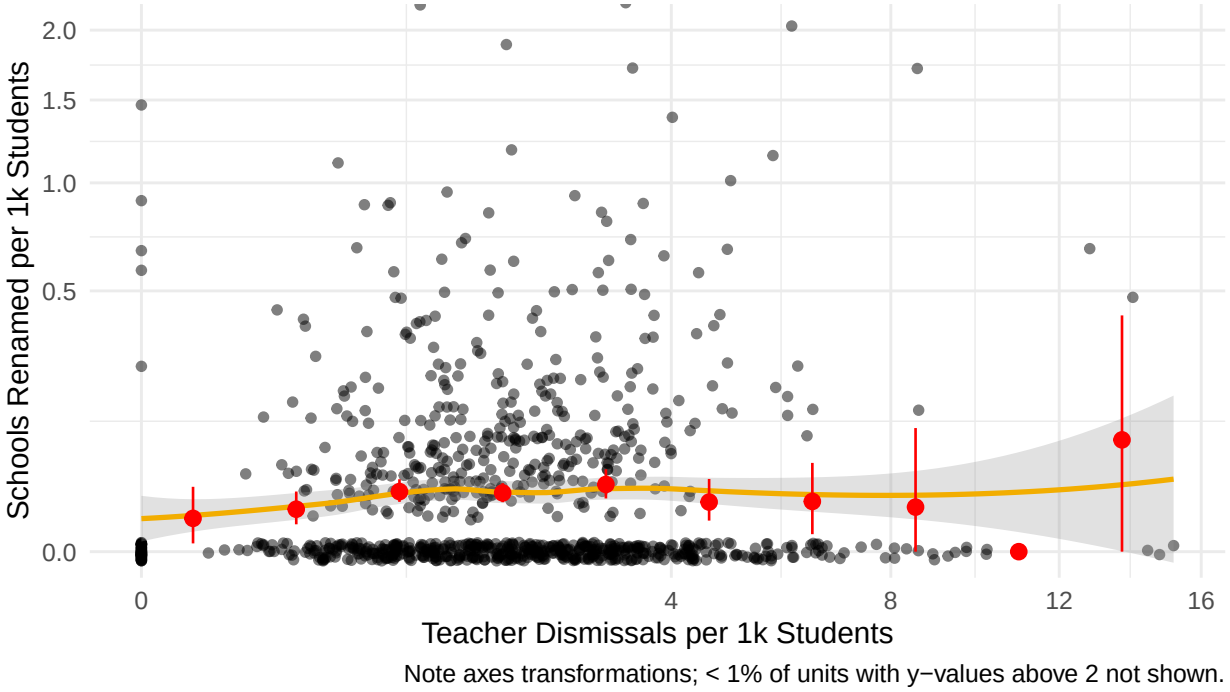

Figure 1: Each point is a district (N = 973). The x-axis gives Ministry of National Education personnel dismissed by emergency decree after the coup; the y-axis gives schools renamed to commemorate 15 July. Both are per 1,000 school-age residents and plotted on square-root scales, and points are jittered vertically. The yellow line is a loess fit with its 95% confidence band. Red points are means within ten bins of the x-axis, with bootstrapped 95% confidence intervals.

Changes in curriculum. Curricular changes, including the addition of the coup attempt and the resistance to it, and other commemorative practices such as memorial corners were mandated nationwide by the Ministry of National Education. Schools named after July 15 therefore do not differ from other schools in what they teach and how they commemorate the event. Because our design compares neighborhoods within the same election, such national changes are absorbed by election fixed effects and cannot bias our estimates of the effects of renaming.

Taken together, these suggest that, with the exception of the confiscation of Gülen-affiliated schools, which we address in our subgroup analysis, renaming did not coincide with other forms of repression that could independently shift voting behavior. These other repressive measures are thus unlikely to invalidate the parallel-trends assumption on which our estimates of the effects of renaming rely.

6. Did Renaming Schools Affect Election Outcomes?

We exploit the staggered timing of school renamings, and the fact that most neighborhoods never had a school renamed, to estimate the effect of renaming on electoral outcomes at the neighborhood level. We use the fixed effects counterfactual estimator (FECT) from Liu et al. (2024), which uses untreated observations to estimate neighborhood and election fixed effects. It then computes the average difference between the estimated counterfactual and the realized outcome for treated units to recover the average treatment effect on the treated (ATT), with bootstrapped standard errors clustered on neighborhood, which is our unit of

assignment. This estimator avoids pathologies that can arise when standard two-way fixed effects models (TWFE) are used in settings where the timing of treatment is staggered (Goodman-Bacon 2021). In our case, the share of never-treated units is very large, and most ever-treated units transition as part of the first treatment cohort, before the 2018 election. As such, simpler TWFE models estimated via OLS yield nearly identical estimates.

Table 3: Electoral Effects of Renaming Schools

	MHP	AKP	CHP	HDP
Panel A: Full Sample				
Obs. equally weighted	-3.536*	-1.590*	1.152*	1.197*
	(0.313)	(0.272)	(0.186)	(0.178)
Placebo period = t-1	-0.025	-0.239	0.603*	0.198
	(0.203)	(0.228)	(0.109)	(0.125)
Predicted Counterfactual	15.964	41.544	20.475	7.298
Ever-Treated	658	658	658	658
Never-treated	49958	49958	49958	49958
Elections	4	4	4	4
Observations	199,212	199,212	199,212	199,212
Panel B: Excluding curfew and Gülen Movement Schools				
Obs. equally weighted	-2.886*	-1.761*	1.199*	1.202*
	(0.381)	(0.334)	(0.235)	(0.204)
Predicted Counterfactual	16.07	43.956	18.751	6.302
Ever-Treated	466	466	466	466
Never-treated	49060	49060	49060	49060
Elections	4	4	4	4
Observations	195,117	195,117	195,117	195,117

Table 3: Average treatment effects on the treated (ATT) of having a school renamed to commemorate 15 July, in percentage points of vote share. Estimates use the fixed-effects counterfactual estimator (FECT) with neighborhood and election fixed effects on a neighborhood panel of four general elections, June 2015 to May 2023. The placebo row holds out the last pre-treatment election and estimates an "effect" there; values near zero support parallel trends. Predicted counterfactual is the average estimated untreated vote share of treated observations. Panel B drops neighborhoods placed under curfew or containing a closed Gülen-affiliated school. Neighborhood-clustered bootstrap standard errors in parentheses. * $p < 0.05$.

Table 3 (panel A) shows that renaming schools reduced electoral support for parties in the governing coalition: the AKP and its ultranationalist coalition partner, the MHP, see their vote shares decline by 1.6 and 3.5 percentage points, respectively. Relative to the vote shares these parties would have received without renaming (i.e. the counterfactual), as predicted by the neighborhood and election fixed effects, these effects amount to proportional declines of 3.8% for AKP and 22.1% for MHP. The large effects on MHP may reflect the 2017 split that produced the İYİ Party, whose founders opposed the MHP's alignment with Erdoğan. By making the government's partisan account of the coup more visible, renaming may have highlighted this alignment and given nationalist voters a reason to defect.

We interpret these results as evidence of backlash: rather than rallying pro-government voters or persuading centrists, this memory imposition eroded support for the governing parties, particularly the MHP. Opposition and smaller parties gained correspondingly: The CHP, the main secular opposition party, saw its vote share increase by 1.15 points, and the pro-Kurdish HDP/DEM gained 1.20 points — increases of 5.6% and 16.4% relative to their predicted counterfactuals. These combined gains fall short of the governing coalition's combined losses: the residual share of votes is won by smaller parties (see Appendix Section B.1).[7]

Table 3 (panel A) also reports placebo tests, which assign treatment timing up by one election and then estimate placebo effects using pre-treatment outcome data. These tests assess whether pre-treatment trends in to-be-treated neighborhoods diverge from untreated areas even prior to schools being renamed. We do not observe any significant trends for AKP, MHP, and HDP. The placebo test is, however, positive and statistically significant for CHP, indicating that the party's support was already rising in to-be-treated neighborhoods. This divergence is visible in the bottom left panel of Figure 2 for the FECT estimator (See the yellow line in the figure). To address it, we use trajectory balancing (Hazlett and Xu 2018), which reweights the control sample to match the pre-treatment trends of treated neighborhoods. The pre-treatment divergence disappears after trajectory balancing (blue line). Both estimators find a positive and statistically significant effect of renaming on the CHP's vote share. More generally, our conclusions do not change qualitatively when we use trajectory balancing, although the estimated backlash against the MHP is less severe (top left panel in the figure).

We perform various robustness checks. First, to confirm that our estimates are not driven by coinciding forms of repression Panel B of Table 3 reports the results when excluding the neighborhoods with a confiscated Gülen School or a curfew. The estimates are substantively unchanged.

Second, we replace the election fixed effects with province-by-election (Appendix Table A.1) or district-by-election (Appendix Table A.2) fixed effects. In these models, treated neighborhoods are compared only with untreated neighborhoods in the same province or district. These specifications absorb any time-varying confounds at the province- or district-level such as changes in provincial government. Because governors make renaming decisions at the province level, province-by-election fixed effects also absorb any effects specific to a given governor. While some point estimates attenuate under these more demanding specifications, our substantive conclusions are unchanged: in treated neighborhoods, the governing coalition loses support and the opposition gains.

Third, because schools were modestly more likely to be renamed in MHP strongholds, our estimates could reflect regression to the mean: neighborhoods with unusually high MHP support might have seen it decline even without treatment. To address this concern, in Appendix Table A.3, we split neighborhoods into quintiles based on pre-treatment MHP support and estimate the effect of renaming a school on the MHP's vote share within each quintile. The estimate is negative and significant in four of the five quintiles, and null only in the quintile with the highest MHP support, where regression to the mean should be strongest. The decline in MHP support is thus not driven by its strongholds.

7. In 2018, Saadet was the smallest "other" party, and it also ran in opposition to the governing coalition.

Figure 2: ATTs Estimated using FECT and Trajectory Balancing

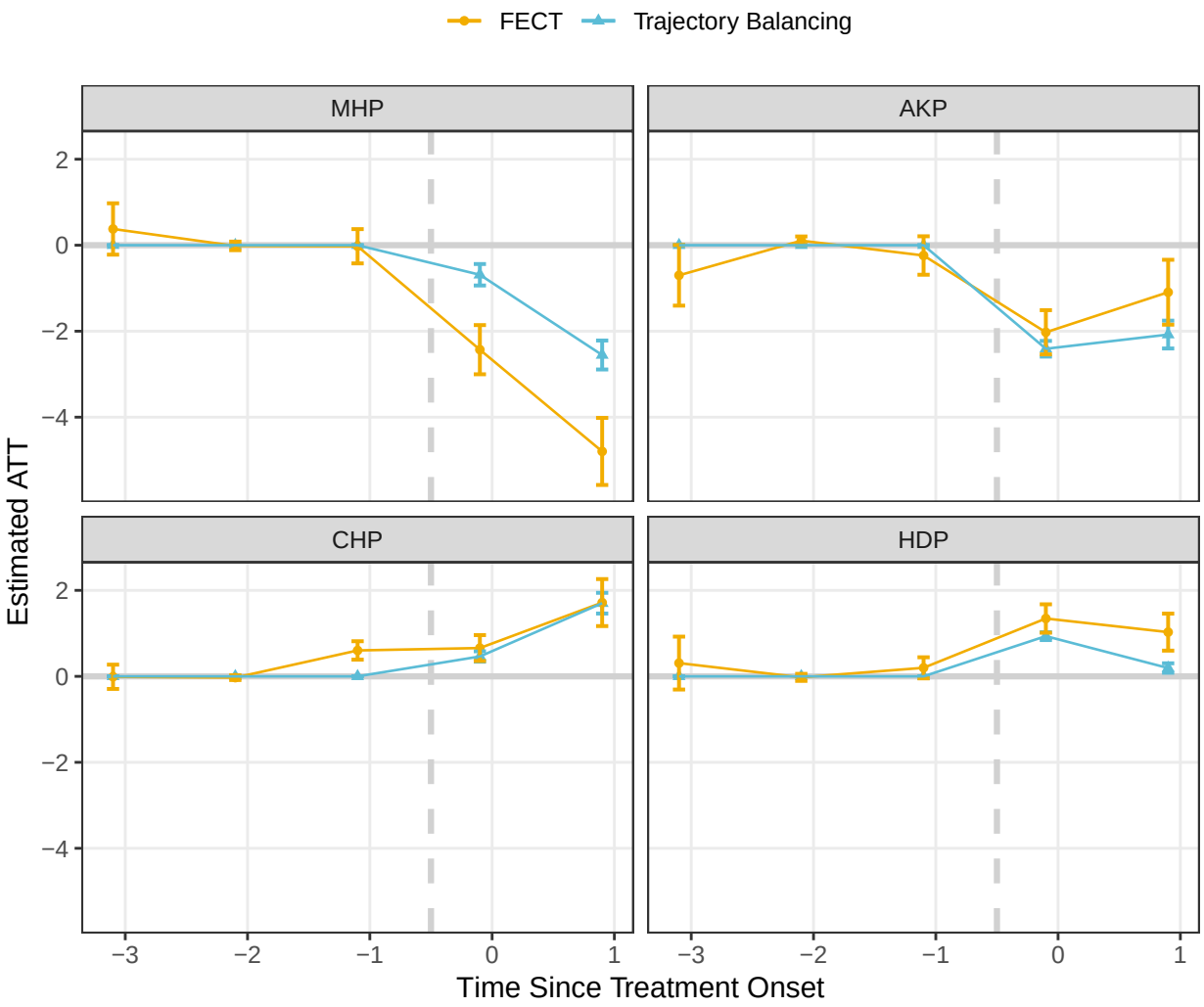

Figure 2: Estimated effects of a renaming on each party's vote share (percentage points), by election relative to the first election after a school is renamed (0); the dashed line separates pre- and post-treatment periods. Period −3 comes only from neighborhoods first treated in 2023, and period 1 only from those treated by 2018. FECT (yellow) imputes counterfactuals from neighborhood and election fixed effects; bars are 95% bootstrap confidence intervals. Trajectory balancing (blue) reweights never-treated neighborhoods to match treated neighborhoods' pre-treatment vote shares, so its pre-treatment estimates are zero by construction. It uses neighborhoods observed in all four elections; bars are 95% jackknife confidence intervals.

If renaming schools moved voters, we would expect the effects to be concentrated where partisan attachments were weakest. To test this, we measure each neighborhood's pre-treatment electoral volatility using the Pedersen index (Pedersen 1979) across the June and November 2015 elections, split neighborhoods at the median, and re-estimate our models separately for volatile and stable neighborhoods (Appendix Table A.5).[8] In volatile neighborhoods, the AKP's vote share falls by 3.7 points, whereas in stable neighborhoods the estimate is small and positive (0.5 points); the CHP and HDP likewise gain more in volatile

8. This index is half the sum of the absolute changes in the vote shares of the AKP, CHP, MHP, HDP, and all other parties.

neighborhoods (2.2 and 1.5 points) than in stable ones (0.4 and 0.8 points).[9] This analysis further corroborates our main findings.

Finally, in Table A.4, we explore whether the effect of renaming a school is different when the commemorative name is generic (e.g., 15 July Martyrs Primary School) versus when it carries the name of a person killed resisting the coup. Names commemorating a specific individual may evoke sympathy for the victim rather than the government's broader narrative, which may potentially soften backlash effects. Our core results do not depend on the type of name: the backlash is similar whether or not a school's new name recalls a specific martyr.

6.1 Generalizability

Several conditions likely shape how far our findings travel. First, although we study school names, other forms of memory imposition, such as street names or public buildings, may have similar effects. Second, the backlash we observe arose where both the narrative and the display of power behind it were openly contested: although the renamings themselves were not a campaign issue, opposition parties publicly rejected both the government's account of July 15 and the concentration of power it was used to justify. Third, despite democratic backsliding and repression, Turkish elections remained competitive (Esen and Yardimci-Geyikçi 2020), giving dissatisfied voters viable alternatives. With civil society severely restricted under the state of emergency, the ballot box was one of the few channels through which citizens could express discontent. Where elections are less competitive, or where civil society can contest official narratives directly, or where no organized group or political party contests the official narrative, memory imposition may fare differently.

7. Conclusion

Commemoration takes many forms, from plaques or memorials honoring victims of violence and museums documenting past atrocities to monuments to national founders, holidays marking independence, and streets named after heroes of liberation struggles. Most of what we know about its political effects comes from commemoration that acknowledges wrongdoing, is led by civil society, is broadly consensual, or is easy to avoid. Rather than offering a full taxonomy of commemoration, we have conceptualized and examined one common form that combines the opposite features: *memory imposition*. It celebrates the incumbent government or leaders who sponsor it, is delivered through institutions citizens cannot avoid, and advances a contested narrative. Because it operates through different channels and pursues different ends, its political consequences need not resemble those of the commemoration studied so far. In particular, whether memory imposition benefits the incumbents who sponsor it has remained an open question.

We study the renaming of public schools to commemorate resistance to Turkey's 2016 coup attempt, part of a broader campaign that offers a clear case of memory imposition. Because such campaigns typically operate nationally, through holidays, curricula, or nationwide celebrations, which reach all citizens at once,

9. The MHP is the exception: its losses are larger in stable neighborhoods (4.8 points) than in volatile ones (2.3 points), although the gap is no longer statistically distinguishable once we include province- or district-by-election fixed effects. The MHP lost a substantial share of its support between the June and November 2015 elections, falling nationally from 16.3% to 11.9%, and much of the volatility we measure reflects this loss: in volatile neighborhoods, the MHP's share fell by 7.1 points between the two elections, compared with 2.4 points in stable neighborhoods. Places with a more volatile MHP vote may therefore be those where the party's support had already bottomed out before any schools were renamed, and it may simply be hard to lose more in such places.

their effects are difficult to isolate. The school renamings in Turkey, by contrast, took a local form, varying across neighborhoods and over time, which allows us to estimate their electoral consequences. Drawing on neighborhood-level returns from four general elections, we find that the renamings produced a substantial electoral backlash: The governing coalition lost about five percentage points of combined vote share in treated neighborhoods, while the opposition parties that rejected the government's narrative gained.

Several limitations point to productive directions for future work. First, our analysis rests on a single case, and the direction and magnitude of memory imposition's effects may vary across political contexts. Comparative work is needed to identify the conditions under which memory imposition consolidates rather than erodes incumbent support. Second, our analysis covers the elections that followed renaming; longer-run dynamics, such as the gradual internalization of official narratives or a compounding backlash through opposition mobilization, remain to be examined. Third, while our design identifies the electoral consequences of memory imposition, it cannot directly observe the mechanisms behind them. That the largest losses fell on the MHP, whose voters might be expected to sympathize with a nationalist narrative, raises the possibility that the backlash reflects resistance to the imposition itself, not only to its content. However, the MHP's realignment with the AKP over the same period complicates this interpretation. Survey and qualitative work could clarify whether the backlash reflects opposition to the politicization of memory itself, to the broader policies with which it was associated, or both.

Finally, our analysis included adult voters who lived near renamed schools in the elections that followed, yet the most sustained exposure falls on the students who attend them. The longer-run consequences of memory imposition may depend less on how current voters respond than on how it shapes the political socialization of those who grow up with it (Paglayan 2024; Fouka 2020). Whether memory imposition in schools ultimately cultivates loyal citizens or fosters resentment among future voters is a question that tracking students over time could answer.

More broadly, our findings caution against reading the proliferation of imposed commemorative practices as evidence of the consolidation of incumbent power. A symbolic landscape remade in the incumbent's image can look like a sign of strength. Our results suggest, however, that where its narrative is contested, it can also be a liability. Nor is memory imposition confined to regimes undergoing democratic backsliding: incumbents in established democracies, too, have turned state institutions toward advancing contested accounts of the near and distant past, as recent efforts in the United States illustrate. As incumbents in diverse political settings turn to memory as an instrument of power, understanding when such efforts persuade and when they backfire deserves far more attention than it has received.

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Supporting Information

Electoral Impacts of Memory Imposition

For online publication

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A. Targeting

A.1 Predictive Power of Levels vs. Trends in Voting

Figure A.1: Propensity Scores as a Function of Levels vs. Trends in Electoral Support

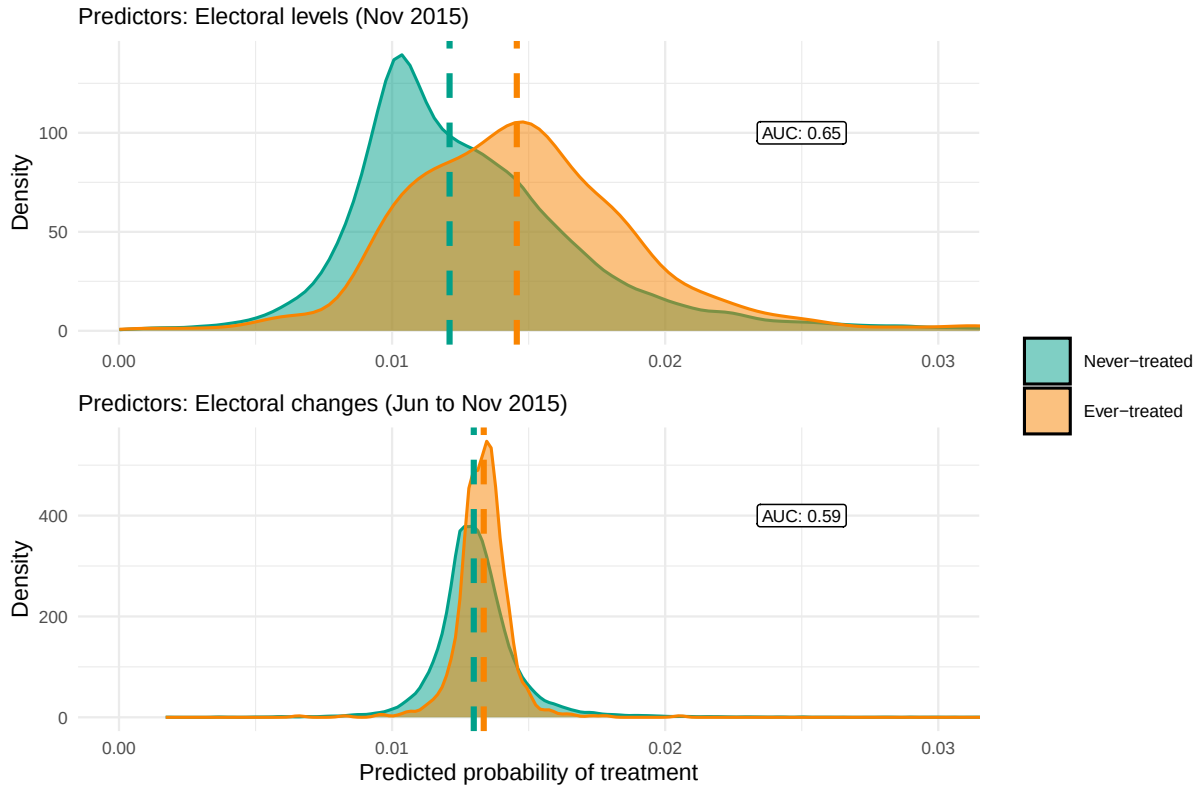

Figure A.1: Densities of neighborhoods' predicted probability of ever having a school renamed, by eventual treatment status. Top panel: logit of treatment on MHP, AKP, CHP, and HDP vote shares in November 2015. Bottom panel: logit on the change in those shares from June to November 2015. Dashed lines mark group medians. AUC is the probability that a randomly drawn ever-treated neighborhood has a higher predicted probability than a randomly drawn never-treated one; 0.5 means no predictive power. The x-axis is truncated at 0.03.

Suppose you randomly selected a treated and a control neighborhood and compared their propensity scores based on MHP, AKP, CHP, and HDP vote shares in November 2015. If these levels perfectly predicted treatment, then the treated neighborhood's propensity score would always be larger (AUC = 1). If, on the other hand, the levels had no predictive power, then the treated neighborhood's propensity score would be larger 50% of the time (AUC = .5). The propensity score would be no better than a coin flip in predicting which of the two units is treated. We find an AUC of 0.65 when we use levels of electoral support from Nov. 2015 in a logistic regression. The AUC dips to 0.59 — only marginally better than a coin flip — when we use changes in electoral support from June to November 2015 to predict which neighborhoods will be treated.

B. Electoral Impact

B.1 Accounting for the Full Ballot

The main text notes that the opposition's combined gains (about 2.4 percentage points) fall short of the governing coalition's combined losses (about 5.1 points). We define the residual share as valid votes cast for any party other than the AKP, CHP, MHP and HDP, divided by valid votes, and estimate its ATT using FECT alongside the four party shares. All five outcomes are estimated on an identical sample as in the main analysis. Because the five shares are an algebraic partition of valid votes and FECT is linear in the outcome, the five ATTs must sum to zero, and they do: their sum is 0, and the predicted counterfactual levels ($41.544 + 20.475 + 15.964 + 7.298 + 14.719$) sum to 100%. This accounting is also reflected in Appendix Table A.4, which uses a simpler TWFE model to estimate treatment effects and reports the effect on "Other" parties.

B.2 Province-by-Election Fixed Effects

Table A.1: Electoral Effects of Renaming Schools (province-by-election FEs)

	MHP	AKP	CHP	HDP
Obs. equally weighted	-3.060*	-0.564*	1.265*	0.372*
	(0.288)	(0.220)	(0.155)	(0.140)
Placebo period = t-1	-0.247	0.171	0.343*	-0.111
	(0.162)	(0.197)	(0.098)	(0.100)
Predicted Counterfactual	15.488	40.517	20.362	8.124
Ever-Treated	658	658	658	658
Never-treated	49958	49958	49958	49958
Elections	4	4	4	4
Observations	199,212	199,212	199,212	199,212

Table A.1: As in Table 3, Panel A, but adding province-by-election fixed effects, so that renamings are compared only with untreated neighborhoods in the same province and election. This absorbs province-wide shocks, including those tied to governors, who decide renamings. Neighborhood-clustered bootstrap standard errors in parentheses. * $p < 0.05$.

B.3 District-by-Election Fixed Effects

Table A.2: Electoral Effects of Renaming Schools (district-by-election FEs)

	MHP	AKP	CHP	HDP
Obs. equally weighted	-2.881*	-0.461*	1.102*	0.415*
	(0.272)	(0.202)	(0.160)	(0.144)
Placebo period = t-1	-0.505*	0.129	0.329*	0.010
	(0.164)	(0.178)	(0.113)	(0.095)
Predicted Counterfactual	15.309	40.415	20.525	8.081
Ever-Treated	658	658	658	658
Never-treated	49958	49958	49958	49958
Elections	4	4	4	4
Observations	199,212	199,212	199,212	199,212

Table A.2: As in Table 3, Panel A, but adding district-by-election fixed effects, so that renamings are compared only with untreated neighborhoods in the same district and election. Neighborhood-clustered bootstrap standard errors in parentheses. * $p < 0.05$.

B.4 Effect by Baseline MHP support

Table A.3 sorts neighborhoods into quintiles of MHP vote share averaged over the June and November 2015 elections, both pre-treatment. The two estimate columns answer different questions. The decomposition interacts treatment with quintile under a single common election effect; its coefficients weight up to the pooled ATT (-3.46 against -3.41) and so apportion the headline estimate across subgroups, but each coefficient is measured against the pooled time path rather than against controls in its own quintile. The within-quintile column adds quintile-by-election fixed effects, giving each quintile its own time path. Those coefficients are numerically identical to separate TWFE estimates fit inside each quintile and, for that reason, do not aggregate to the ATT.

Table A.3: Treatment Effects and Selection by Baseline MHP Support

	N	BL MHP (%)	ATT Decomposition	W/in-quintile Design	Pr(max$\{D_{it}\}$ = 1) [95% range]
Q1	38,830	0.0–2.3	1.98 (0.25)	-1.59 (0.24)	0.010 [0.006, 0.014]
Q2	39,795	2.3–8.7	2.99 (0.57)	-2.81 (0.57)	0.011 [0.006, 0.015]
Q3	39,924	8.7–14.4	-1.87 (0.39)	-3.66 (0.39)	0.012 [0.006, 0.016]
Q4	39,894	14.4–21.4	-3.72 (0.52)	-1.35 (0.53)	0.014 [0.007, 0.019]
Q5	39,908	21.4–91.2	-8.69 (0.57)	0.26 (0.57)	0.019 [0.008, 0.038]

Table A.3: Rows are quintiles of neighborhoods' baseline MHP vote share, averaged over the June and November 2015 elections; N counts neighborhood-elections. The ATT decomposition interacts treatment with quintile indicators in a model with neighborhood and election fixed effects, so its coefficients approximately recover the pooled ATT. The within-quintile design adds quintile-by-election fixed effects, which is equivalent to a separate difference-in-differences within each quintile. Standard errors clustered by neighborhood in parentheses. The last column gives the mean predicted probability of ever being treated, from a logit on November 2015 party shares (AUC = 0.65), and in brackets the 2.5th to 97.5th percentiles across the quintile's neighborhoods.

In the decomposition, the effect grows steadily with prior MHP strength: renaming costs the MHP nothing where it had little support (1.98 in Q1) but 8.69 points where it was strongest. If this pattern only reflected different trends across quintiles, it should disappear once each quintile gets its own time path. But it does not: within quintiles, the effects in Q1–Q4 range from 1.35 to 3.66, all negative and three clearly different from zero.[10]

Selection on baseline levels is a separate matter, and a modest one: the estimated probability of treatment rises from 0.010 in Q1 to 0.019 in Q5, but the ranges overlap heavily across quintiles and the selection model discriminates weakly overall (AUC 0.65).

10. A -1.59 point effect in a quintile whose baseline tops out at 2.3% is not arithmetically impossible, because the relevant ceiling is the counterfactual in the treated periods rather than the 2015 level: Q1 control neighborhoods recover to 3.11% MHP support by 2018 and 4.36% by 2023, while treated Q1 neighborhoods reach 2.42% and 3.13%.

B.5 Effects of Specific Namesakes vs. Generic References

Some schools take a generic commemorative name, such as 15 July Martyrs Primary School; others carry the name of a specific individual killed resisting the coup. If the electoral response is to commemoration of the coup as an event, both kinds of renaming should move votes. If instead it is driven by something particular to honoring a local martyr — a family in the neighborhood, a locally salient death — the two should diverge. Splitting the treatment tests whether the pooled estimate averages over two distinct treatments.

Table A.4 reports the split. Panel A pools all renamings; Panel B divides treated neighborhoods on whether any school in them took a generic 15 July name. The contrast is therefore between any generic name present and martyr names only. Both panels are estimated by two-way fixed effects rather than FECT, with neighborhood and election fixed effects, standard errors clustered on neighborhood. We note that the pooled coefficients in Panel A are close to the FECT estimates in the main text (for the MHP, -3.41 against -3.54).

Table A.4: Electoral Effects of Renaming Schools by Specificity of Name

	MHP	AKP	CHP	HDP	Other
Panel A: Pooled treatment					
Renamed school	-3.412*	-1.549*	0.886*	1.061*	3.013*
	(0.298)	(0.275)	(0.194)	(0.145)	(0.238)
Observations	198,859	198,859	198,859	198,859	198,859
Panel B: Disaggregated by name type					
Specific Martyr's Name	-3.161*	-1.738*	0.489*	1.369*	3.041*
	(0.355)	(0.322)	(0.231)	(0.135)	(0.271)
15 July	-3.985*	-1.118*	1.791*	0.360	2.951*
	(0.538)	(0.512)	(0.344)	(0.353)	(0.470)
p-value: Equiv. test	0.199	0.302	0.002	0.007	0.868
Observations	198,859	198,859	198,859	198,859	198,859

Table A.4: Two-way fixed effects estimates (neighborhood and election fixed effects) of the effect of a renamed school on vote shares, in percentage points; Other is the combined share of all remaining parties. Panel A pools all renamings. Panel B splits treated neighborhoods by name type: 15 July neighborhoods (208) have at least one school with a generic 15 July name, and 27 of these also have a school named for a martyr; Specific Martyr's Name neighborhoods (462) have only schools named for individual martyrs. The *p*-value tests equality of the two coefficients. Standard errors clustered by neighborhood in parentheses. * $p < 0.05$.

The core result does not depend on which kind of name a school takes. The MHP's vote share falls by 3.16 points where only martyr names were assigned and by 3.99 where a generic name was assigned; the AKP's falls by 1.74 and 1.12. Each arm is independently signed and significant, so the agreement between them is not a low-power null: equality cannot be rejected for either party (p = 0.199 for the MHP, 0.302 for the AKP), and the two arms land this close despite the generic arm being less than half the size, with standard errors roughly 50% wider. The HDP gains 1.37 points under martyr renamings but an insignificant 0.36 under generic ones (equality rejected, p = 0.007), while the CHP runs the other way, gaining 0.49

against 1.79 ($p = 0.002$). We read this as evidence that the displacement of votes is a response to the renaming itself, while the destination of those votes among opposition parties depends on the kind of name imposed.

B.6 Electoral Effects of Renaming Schools by Pre-Treatment Volatility

If renaming schools moved voters, we would expect the effects to be concentrated where partisan attachments were weakest. To test this, we measure each neighborhood's pre-treatment electoral volatility using the Pedersen index (Pedersen 1979) across the June and November 2015 elections, split neighborhoods at the median, and re-estimate our models separately for volatile and stable neighborhoods in Table A.5. This index is half the sum of the absolute changes in the vote shares of the AKP, CHP, MHP, HDP, and all other parties.

In volatile neighborhoods, the AKP's vote share falls by 3.7 points, whereas in stable neighborhoods the estimate is small and positive (0.5 points); the CHP and HDP likewise gain more in volatile neighborhoods (2.2 and 1.5 points) than in stable ones (0.4 and 0.8 points). The MHP is the exception: its losses are larger in stable neighborhoods (4.8 points) than in volatile ones (2.3 points), although the gap is no longer statistically distinguishable once we include province- or district-by-election fixed effects. The MHP lost a substantial share of its support between the June and November 2015 elections, falling nationally from 16.3% to 11.9%, and much of the volatility we measure reflects this loss: in volatile neighborhoods, the MHP's share fell by 7.1 points between the two elections, compared with 2.4 points in stable neighborhoods. Places with a more volatile MHP vote may therefore be those where the party's support had already bottomed out before any schools were renamed, and it may simply be hard to lose more in such places. This analysis further corroborates our main findings.

Table A.5: Electoral Effects of Renaming Schools by Pre-Treatment Volatility

	MHP	AKP	CHP	HDP
Panel A: Volatile neighborhoods				
Obs. equally weighted	-2.275*	-3.725*	2.200*	1.485*
	(0.554)	(0.455)	(0.288)	(0.301)
Predicted Counterfactual	17.099	48.097	13.565	6.666
Ever-Treated	298	298	298	298
Never-treated	24697	24697	24697	24697
Elections	4	4	4	4
Observations	99,188	99,188	99,188	99,188
Panel B: Not volatile neighborhoods				
Obs. equally weighted	-4.747*	0.543	0.426	0.762*
	(0.341)	(0.312)	(0.249)	(0.214)
Predicted Counterfactual	15.232	35.827	25.955	8.014
Ever-Treated	360	360	360	360
Never-treated	24635	24635	24635	24635
Elections	4	4	4	4
Observations	99,134	99,134	99,134	99,134

Table A.5: Average treatment effects on the treated (ATT) of having a school renamed to commemorate 15 July, in percentage points of vote share. Panel A covers neighborhoods with above-median electoral volatility between the June and November 2015 elections, measured by the Pedersen index. Panel B covers neighborhoods at or below the median. Estimates use the fixed-effects counterfactual estimator (FECT) with neighborhood and election fixed effects on a neighborhood panel of four general elections, June 2015 to May 2023. Predicted counterfactual is the average estimated untreated vote share of treated observations. Neighborhood-clustered bootstrap standard errors in parentheses. * $p < 0.05$.

B.7 Results of Figure 2 in a table

Table A.6: Event-Time Effects of Renaming Schools, FECT and Trajectory Balancing

	MHP	AKP	CHP	HDP
Panel A: FECT				
t = -3	0.378	-0.701	-0.010	0.308
	(0.305)	(0.360)	(0.144)	(0.314)
t = -2	-0.016	0.105*	-0.030	-0.024
	(0.051)	(0.050)	(0.029)	(0.042)
t = -1	-0.025	-0.239	0.603*	0.198
	(0.203)	(0.228)	(0.109)	(0.125)
t = 0	-2.430*	-2.025*	0.658*	1.345*
	(0.292)	(0.262)	(0.154)	(0.169)
t = 1	-4.797*	-1.094*	1.716*	1.029*
	(0.398)	(0.385)	(0.279)	(0.220)
Treated neighborhoods (t = 0)	658	658	658	658
Panel B: Trajectory Balancing				
t = -3	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
t = -2	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
t = -1	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
t = 0	-0.687*	-2.409*	0.462*	0.935*
	(0.127)	(0.093)	(0.062)	(0.046)
t = 1	-2.554*	-2.079*	1.702*	0.193*
	(0.172)	(0.165)	(0.123)	(0.056)
Treated neighborhoods (t = 0)	658	658	658	658

Table A.6: Results of Figrue 2 are reported in a Table. Panel A reports the results for FECT (Liu et al. 2024) and Panel B reports the results for trajectory balancing (Hazlett and Xu 2018).

Appendix References

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