The Real Consequences of Symbolic Politics: Breaking the Soviet Past in Ukraine

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Conflicts over symbolic issues are prominent in public affairs, but do they have wider political consequences, and if so, why? We study the electoral effects of *Leninopad* ("Lenin's free fall"), a sudden wave of demolitions of Soviet monuments in Ukraine. Difference-in-differences estimates show that the removals of the Soviet symbols mobilized supporters for parties with a relatively sympathetic view of Ukraine's Soviet past. We attribute this backlash effect to a signaling mechanism: the removals indicated the weakening power status of the Soviet legacy parties, which motivated their supporters to turn out in elections. This backlash dissipated once the Soviet symbols ceased being a contentious partisan issue due to the escalating war in eastern Ukraine. Symbolic politics has real, nonsymbolic consequences, but only when it maps onto partisan cleavages.

Some of the most contested, passionate, and often violent conflicts concern issues that have no apparent tangible value to the disagreeing parties. Relocation of a World War II monument in Estonia sparked the largest riots in the country's modern history. An attempt to remove a Civil War memorial in Charlottesville prompted violent clashes on the streets. India's Prime Minister Narendra Modi spent precious record-breaking billions on statues to Hindu figures at the risk (or perhaps with the goal) of sectarian tensions with the Muslims. The US Congress and President Donald Trump found the time—in the midst of a historic pandemic—to battle over whether military bases can be named after Confederate generals (Baker and Cooper 2020).

Political actors often engage in symbolic politics by fighting over and exploiting symbolic issues like linguistic conventions, topographic names, iconography, or historical narratives (Forest and Johnson 2011; Lupu 2003; Wedeen 1999).[1] The preponderance of conflict over symbolic issues seems at odds with the conventional view that sees politics as a conflict over resources and power. A great deal of political capital is spent on deciding how to name streets, which monuments to erect or remove, or how to interpret history. Does symbolic politics have tangible repercussions, and if so, why?

This article proposes that conflicts over symbols matter because they are opportunities for competing groups to test and signal their power. A party that successfully challenges the status quo on an issue will be perceived as de facto more powerful by virtue of being able to prevail, even if the issue at hand is only symbolic and does not have a direct impact on the distribution of resources and power. Because of the status-threat logic (Bustikova 2019; McClendon 2018; Mutz 2018), the "losers" of a conflict over symbols will be mobilized to counteract the shifting distribution of de facto power. When they map onto partisan cleavages, conflicts over symbols affect competition over votes by mobilizing turnout within the "losing" group.

We assess this theoretical intuition on the case of Ukraine. As a former Soviet state, Ukraine has had thousands of public monuments to Soviet figures. The sudden outbreak of Euromaidan protests in the fall of 2013 unleashed a mass wave of

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1. In the behavioral tradition, the term "symbolic politics" refers to emotional as opposed to rational (self-interested) motivation in political decision-making (Sears and Funk 1990). We use the term in the comparative politics tradition (e.g., Forest and Johnson 2011) in which it refers to how political actors engage in symbolic acts or employ symbolic objects. Relatedly, our use of the term "symbolic conflict" comes from anthropological literature, where it refers to a "dimension of political conflict which focuses on the manipulation of symbols" (Harrison 1995, 255).

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demolitions of these monuments, locally known as *Leninopad* ("Lenin's free fall"), resulting in a swift transformation of Ukraine's public spaces. We assembled a comprehensive data set on the locations and removals of these monuments. The granular temporal and spatial variation in the removals of the monuments provides a rare opportunity to systematically assess the wider electoral impact of symbolic politics.

Difference-in-differences (DID) estimates show that *Leninopad* mobilized supporters of the Soviet legacy parties—those with a relatively favorable interpretation of Ukraine's Soviet past. Historically, these parties had managed to thwart attempts to remove the Soviet monuments. The failure to protect the Soviet memorials during *Leninopad* served as a public signal of the diminishing influence of the Soviet legacy parties, which motivated higher turnout among their sympathizers. Using data from social media, we rule out the possibility that the effects of *Leninopad* were driven by protests that often accompanied the removals of the monuments.

Importantly, we find that the removals of the Soviet monuments mattered only when their status was openly contested by the competing parties. Through text analysis of mass media narratives, we show that *Leninopad* stopped provoking opposition precisely when a crosscutting cleavage—national sovereignty—emerged because of the proxy war with Russia in the Donbas region of Ukraine. Instead of representing the entrenchment of the Soviet legacy parties, Lenin's monuments began to symbolize an assault on Ukraine's sovereignty—a nonpartisan issue. As the Soviet symbols lost their polarizing partisan charge, their removals ceased having an impact on elections.

This article makes two contributions. First, it offers a theoretical framework to think about the political role of symbols. We suggest a simple logic to explain why and when conflicts over symbols are politicized and how they affect competition over real political power. Second, we provide empirical evidence that changing the symbolic status quo, under specified scope conditions, may produce an electoral backlash consistent with the proposed power-signaling logic.

With the exception of the study by Forest and Johnson (2011) on the political role of historical monuments in postcommunist states, the quantitative literature on symbolic politics has emerged only very recently. Johnson, Tipler, and Camarillo (2019) use an online survey experiment to estimate the effects of deliberation on the support for the removal of the Confederate memorials in the United States. Rahnama (2020) studies how the actual removals of the Confederate memorials affected racial prejudice. Our focus is not on the impact of symbolic politics on norms, which is an independently important question, but on the distribution of power through elections. Dinas, Martínez, and Valentim (2020) analyze how the Spanish flag ceased being a stigmatized symbol. While their scope is close to our secondary analysis on the changing meaning of symbols, our primary concern is the impact of symbolic politics on electoral competition.

Politically contentious symbols usually memorialize a controversial historical event, personality, or institution: they represent past oppression and violence to some and past glory to others. In that regard, this article speaks to the literature on the historical legacies of exploitation (Acharya, Blackwell, and Sen 2016), state violence (Lupu and Peisakhin 2017; Rozenas and Zhukov 2019), and authoritarian political organizations (Grzymala-Busse 2006). One critique of this literature is that the mechanisms reproducing the historical legacies are rarely specified (Simpser, Slater, and Wittenberg 2018). By showing how the symbolic artifacts of the past shape current electoral competition, this study suggests one such mechanism.

SYMBOLIC CONFLICT AS POWER SIGNALING

Why do symbolic issues—those that do not directly concern resources or power—end up being politically important? We propose that this happens because of the "signaling effects" that conflicts over symbols can generate. By imposing its will on an issue—even if that issue is only symbolic—a group will appear more powerful. In and of itself, there may be no difference whether the national flag is red or green, but a group that manages to impose the green flag will appear more powerful. The desire to signal power may incentivize people to fight over the color of a flag even though intrinsically they may not care about it. In the same way as acquiring formal education allows one to signal intellectual ability (Spence 1973), prevailing on a symbolic matter produces a valuable signal of political prowess.

The signaling effects of symbolic politics show up in many forms. Syrians routinely engaged in nauseating praises to president Hafez al-Assad without believing their own words. The regime incentivized these symbolic rituals not because it enjoyed hearing hypocritical praise but because these rituals allowed the regime to project its awesome ability to submit people to its will (Wedeen 1999). Or consider Vaclav Havel's famous description of a greengrocer who diligently displays slogans of devotions to the communist regime in Czechoslovakia: he does so not out of conviction but to signal "his preparedness to conform"; if many others engage in this symbolic show of conformity, it "reinforces the perception that society is solidly behind the Party" (Kuran 1991, 27).

A centuries-long struggle over the national flag in Haiti is another case in point. Haiti's founding flag of 1804 showed red and black bands. In 1820, it was replaced by a flag featuring red and blue colors, which was taken to convey the dominant role of the mulattos over blacks. Seeing themselves as the only true Haitians (the *authentiques*), the black leaders

sought to reinstate the original flag. This was accomplished by president Francois Duvalier who, elected in 1957, took seven years to prevail on this question: "only in 1964 Duvalier felt strong enough to impose this flag on the country, and thus symbolically to consummate the victory of the *authentiques*" (Nicholls 1996, 235). This was a "new equilibrium" representing "a major shift in power from the established, predominantly mulatto, elite to a new black middle class" (212). In our interpretation, replacing the flag allowed Duvalier to solidify his authority by making this new equilibrium common knowledge.

Soon after ascending to power in Russia, Lenin initiated the Plan for Monumental Propaganda consisting of two parts—the removal of monuments erected in honor of the tsars and their servants and the production of projects for monuments to the Russian Socialist Revolution. Lenin may have simply disliked the aesthetics of the tsarist monuments, but a more realistic possibility is that he conducted a public test of his power: swift removals of the memorials of the previous regime would show that the new government commands obedience from local administrators and citizens. After the communist regimes broke down, new democratic governments engaged in similar symbolic politics by replacing the communist monuments to assert their own power status (Forest and Johnson 2011).

If symbolic change signals a shift in the de facto distribution of power, what sort of reaction should it generate? It is conceivable that the winners of the symbolic conflict could capitalize on their symbolic victory and demand changes on tangible issues. But the losers could also be mobilized to countermand the shift in the balance of power. The existing literature suggests that the latter effect should dominate, because the loss of power status tends to mobilize groups: European right-wing parties performed better as the economic status of immigrants improved (Bustikova 2019); Polish Jews faced more pogroms in places where their political organization posed a challenge to the titular group (Kopstein and Wittenberg 2018); many white Americans voted for Donald Trump because of the perceived threat to their status (Mutz 2018).

For symbolic conflicts to have a backlash effect, certain scope conditions must hold. It is necessary that the demarcation lines on the symbolic issue align with the partisan divisions. Some groups may intrinsically support the status quo on a symbolic issue and some may oppose it, but if those disagreements do not map onto the partisan cleavages, then the change in the symbolic status quo will not signal the shifting distribution of the de facto political power. Changing the flag in Haiti served as a signal of the declining power of mulatto elites only because the conflict over the flag broke down precisely along racial lines. Had the disagreement over the flag cut across the racial cleavage, changing the flag would not have meant a growing influence of one racial group at the expense of another.

Consider the following exotic yet instructive example of the War of Comedians in mid-eighteenth-century France. After an Italian opera company arrived to Paris in 1752, Parisian society was embattled over the value of French versus Italian music until the king ordered the Italians out in 1754. According to Harrison (1995, 257), "this initially aesthetic dispute escalated into an affair of state [because] . . . it became entangled with underlying political conflicts." The progressives favored the Italian style, whereas the conservatives preferred the French one. What on the surface looked like an aesthetic dispute "became a code through which opposed political interests sought implicitly to express themselves and challenge each other" (257). The conflict over aesthetics was political because it reflected the underlying partisan conflict.

These scope conditions for the backlash effect are particularly important in the context of competitive elections. If the competing parties are not publicly divided on a symbolic issue, then resolving the issue in one direction or another will say little about the de facto power of the parties, and the losing side will have little to react to. For example, were the Republicans in the United States to take a public stand in favor of removing the Confederate memorials, then the removals of these memorials would not signal the weakening local power of the party since now the removals would be taking place with the consent of the party. This way, arguably, this particular symbolic issue would be deactivated.

The principal prediction of this theoretical discussion is that changing the symbolic status quo, under the specified scope conditions, can mobilize a backlash. This intuition is supported by anecdotal evidence from a diverse set of cases: Russian speakers rioted against the displacement of the Soviet memorial in Estonia; white supremacists paraded against the removals of Civil War monuments in the United States; racists in Bristol (United Kingdom) responded to the toppling of a statue of a slave trader by vandalizing a statue of the black poet Alfred Fagon. But drawing conclusions from anecdotes alone is risky, since we may notice only those cases of symbolic politics that provoke a backlash. We now discuss the case in which these theoretical claims can be evaluated more systematically.

THE CASE AND THE HYPOTHESES

After Ukraine gained independence in 1991, it started the process of de-Sovietization by renaming streets, rewriting history books, and removing Soviet symbols from the public spaces (Budko and Horobets 2015). These efforts were largely

confined to the traditionally more nationalist western regions. In late 2013, President Viktor Yanukovich decided to reverse Ukraine's geopolitical course away from Europe toward Russia. This sparked a nationwide wave of antigovernment protests, known as Euromaidan. In early December, protesters in the capital Kyiv tore down a Lenin monument, which ignited a chain reaction, and soon Lenins started to fall all across the country.

To give a sense of the scale of the *Leninopad*, figure 1 shows the cumulative count of Lenin's monuments varied in time. During the last election before *Leninopad*, in October 2012, there were 1,438 monuments to Lenin, but a third of them were gone by the next election in May 2014. When President Yanukovich fled the country at the end of February 2014, 340 monuments were demolished in five days. After this first explosive wave, *Leninopad* continued at a slower rate.

The status of the Soviet symbols was a contentious partisan issue, on which the competing factions disagreed publicly. The Soviet legacy parties who had espoused views sympathetic to Ukraine's Soviet past, like the Communist Party of Ukraine or the Party of Regions, denounced *Leninopad* as an illegal, "barbarian" assault on Ukraine's history (LB.ua 2013). The forces behind Euromaidan lauded the demolitions as an indication of Ukraine's long overdue "farewell to the Soviet era" (iPress 2013). *Leninopad* was also openly supported by Petro Poroshenko, the winner of the 2014 presidential election (Riafan.ru 2014).

Historically, the local elites aligned with the Soviet legacy parties had often managed to thwart the removals of the Soviet symbols. The standing Soviet monuments served as a reminder that Ukraine's break from the Soviet past was incomplete because of the entrenchment of the Soviet legacy parties. The unscrupulous demolitions of the Soviet monuments during *Leninopad* indicated that the Soviet legacy parties were losing de facto power, both nationally and locally, to Euromaidan forces (Gayday and Liubarec 2016).

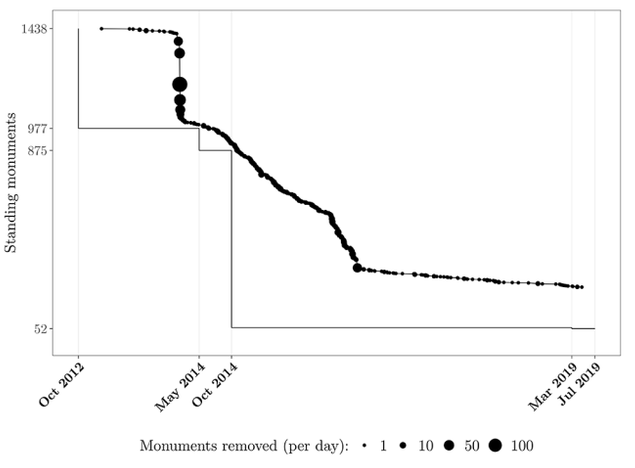

Figure 1. Monument removals in time. Dotted lines trace the number of standing monuments (for which precise dates of demolitions could be found). The solid line traces the number of standing monuments before each round of elections: since some removals could not be identified at daily precision, the step function is generally below the dotted line. Data exclude Luhansk, Donetsk, and Crimea.

Since the conflict over the Soviet symbols, at least in the early stages of *Leninopad*, mapped clearly onto the partisan cleavages, this case satisfies the scope conditions under which the change in symbolic status quo should, in theory, produce a backlash. All else equal, we would expect to see higher pro-Soviet electoral mobilization in places where Lenin's monuments were removed compared to where they remained standing. The prediction here is not that *Leninopad* changed voting preferences but that it mobilized those who already supported the Soviet legacy parties. Thus, the signaling mechanism is consistent with facts only if these two hypotheses hold concurrently:

H1. Monument removals increase the overall turnout (votes cast for all parties relative to eligible voters).

H2. Monument removals increase the pro-Soviet turnout (votes cast for the Soviet legacy parties relative to eligible voters).

Our empirical test is "hard" because it demands us to reject the signaling mechanism in a multitude of cases. For example, the evidence would be inconsistent with the signaling mechanism if we found that the removals reduced the overall turnout but increased the pro-Soviet turnout (only hypothesis 2 confirmed). This would suggest that some centrist voters switched to support pro-Soviet parties in response to symbolic politics being used as a diversion from real bread-and-butter issues (Solt 2011). It could also be that the overall turnout increased while the pro-Soviet turnout decreased because of *Leninopad* (only hypothesis 1 confirmed), which would be more consistent with the retrospective voting model ("pro-Western" voters rewarded their parties for implementing their preferred policy). A null result on either of the two hypotheses would also compel us to reject the signaling mechanism.

DATA

Our initial source of data on the Soviet monuments was a crowd-sourced platform Leninstatues.ru containing (often incomplete and imprecise) records on the locations and the demolitions of 2,410 monuments to Lenin in Ukraine.[2] We

2. Lenin is the only major Soviet political figure whose monuments survived into Ukraine's independence. Monuments to Stalin and other

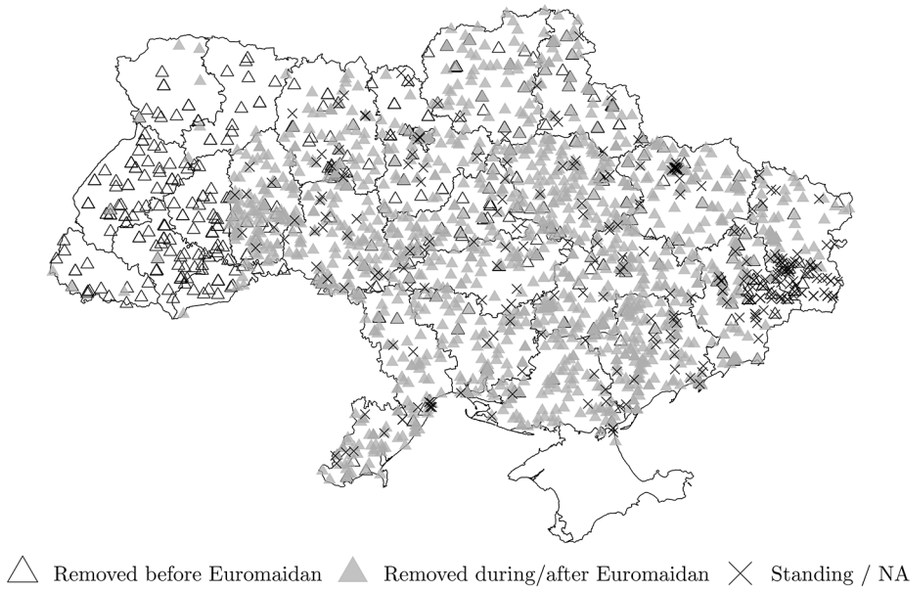

Figure 2. Locations of Soviet monuments. Data for Crimea were not collected.

then cross-validated these data, collected additional information, and georeferenced them.

We first compared our starting list against the official registry of Ukraine's objects of cultural heritage (Ministry of Culture of Ukraine 2016). The two lists could be compared because, fortunately, they use the same nomenclature to reference the monuments. All monuments listed in the official registry appeared on our initial list, which is reassuring. We also used data from the official registry to identify more precise locations of some of the monuments and the dates of their demolitions.

We cross-validated these data against news reports on media websites and search engine queries on demolitions of Lenin's monuments. We found reports on the demolitions of eight monuments that were not mentioned on Leninstatues.ru, which we then added to our database. The fact that we were able to find only eight additional monuments indicated that the coverage of our data was fairly complete.

From the media reports, we also obtained more precise dates of statue demolitions and their locations. One issue we encountered was that multiple demolitions took place in close proximity to one another that could not be easily discriminated (e.g., they occurred in the same town). In those cases, we compared the images of demolitions presented in mass media to the images posted on Leninstatues.ru. Lenin's monuments come in a variety of forms (seated, standing, wearing a hat, etc.), so differentiating them and improving the precision of data proved laborious but straightforward.

We then geocoded the locations of the monuments using Google Maps and Yandex Maps services. When complete addresses were available, we used automated geocoding. If the precise location could not be determined from a specified address, we used the satellite mode of Google Maps to identify the monument or an empty pedestal (in cases when the satellite picture was taken after a removal). Using a combination of these procedures, we geocoded over 99% of the monuments.

Figure 2 shows the locations of Lenin's monuments. A vast majority of monuments in western Ukraine were removed in the early 1990s. Most of the standing monuments are located in the conflict regions of Luhansk and Donetsk, which we excluded from the analyses. Since our analyses only include monuments that were standing when *Leninopad* began, most of our observations are from central regions of Ukraine.

The temporal variation in the monument removals was shown earlier in figure 1. Precise demolition dates could not be obtained for some monuments, but in all except 93 cases, we were able to identify the interelection period in which the monument was removed. This kind of time-stamping is sufficient for the type of analysis we do.

We analyze the electoral effects of the *Leninopad* on four elections: the presidential elections in May 2014, the parliamentary elections in October 2014, the first round of the presidential election in March 2019, and the parliamentary elections in July 2019. We exclude the runoff presidential election in 2019 because neither of the two candidates in that election—the incumbent Petro Poroshenko and his challenger

major Soviet figures were largely removed before the breakup of the Soviet Union.

Volodymyr Zelensky—represented the Soviet legacy position (see app. A). We obtained results of these elections at the level of the electoral precinct from the Central Election Commission of Ukraine. To study the pretreatment trends in voting behavior, we collected data on all national-level elections going back to 2004.[3]

The two outcomes of interest are the percentage of eligible voters who turned out (overall turnout) and the percentage of pro-Soviet votes relative to the vote-eligible population (pro-Soviet turnout). We code parties as "pro-Soviet" if they espouse sympathetic views toward Ukraine's Soviet past, organize celebrations of Soviet holidays or otherwise promote Soviet nostalgia, or call for Ukraine's closer integration with Russia. Some of the Soviet legacy parties, like the Communist Party of Ukraine, are direct successors of the Soviet regime. Others, like the Party of Regions, are not direct successors of the Soviet regime party, but they do share ideological affinities with it. We coded presidential candidates as "pro-Soviet" if they were members of pro-Soviet parties or served in Yanukovich's government. Appendix A lists pro-Soviet parties and candidates and presents a validation study of our coding scheme.

EMPIRICAL STRATEGY

Our empirical strategy is based on variants of the following DID regression:

$$\mathbb{E}(y_{it}) = \alpha_i + \gamma_t + \beta \times D_{it} + \sum_{j=1}^{23} t \times \text{Oblast}_{j[i]} + \sum_{k=1}^{K} \eta_k \times t \times x_{ik},$$

where y_{it} is either overall or pro-Soviet turnout in precinct i on election t. The precinct fixed effect α_i captures unobserved time-invariant characteristics of the precincts, and election fixed effects γ_t capture election-specific shocks. The variable D_{it} is the treatment status of precinct i at time t. The temporal trends are allowed to vary by oblast ($\text{Oblast}_{j[i]}$) and pretreatment characteristics of the precincts (x_{ik}).

We estimate two versions of the above regression. First is the generalized DID regression (or multiperiod DID), which includes all 10 elections ($t = 1, \ldots, 10$) from December 2004 to July 2019 and in which the treatment variable $D_{it} = 0, 1, 2, \ldots$ measures the number of monuments removed in a precinct i up to election t. The coefficient of interest β captures the effect of one removed monument. This specification includes six elections that took place before *Leninopad* started ($D_{it} = 0$ for all i and $t \leq 6$). Assuming that the trends are linear, we can use the pretreatment elections to estimate the pretreatment oblast- and covariate-specific trends that are then extrapolated into the posttreatment time frame (see Angrist and Pischke 2008, 178).[4] In all analyses, we exclude nontreatable precincts—those that had no monuments before *Leninopad*.

We also estimate the above regression in a standard two-period DID setting with a binary treatment. We arrange the five elections starting with October 2012 into four consecutive pairs so that, within each pair, $t = 0$ and $t = 1$ refer to pre- and posttreatment periods, respectively. The precinct is considered as treated if it had at least one monument removed between the two elections ($D_{i0} = 0$ for all i, and $D_{i1} = 1$ if at least one monument was removed in precinct i).[5] Recent literature suggests that, in some settings, the two-period DID is more credible than the multiperiod specification (Imai and Kim 2021). Reassuringly, our results are consistent across both versions.

With each subsequent round of elections, the number of treatable precincts (those with any monuments at the baseline) shrinks, as no new monuments were erected. For this reason, we cannot compare the March 2019 and July 2019 elections because by March 2019, only 49 precincts had standing monuments. Thus, the two-period DID regressions compare the following pairs: (1) October 2012 versus May 2014, (2) May 2014 versus October 2014, (3) October 2014 versus March 2019, and (4) October 2014 versus July 2019.

Since the outcome variables are fractional, they are measured more accurately in precincts with more voters. We account for this variable measurement error by weighting precincts by vote-eligible population (the denominator of the dependent variables).[6] Standard errors are two-way clustered by the precinct, per standard DID practice, and by oblast, to account for spatial autocorrelation.

Whether a monument was removed was sometimes dictated by factors orthogonal to politics. In Kharkiv, for example, activists failed to remove a monument because it was too tall and made of bronze and granite (Abramovich 2014). In Odessa,

3. We did not have geographic boundaries for precincts before 2012. To merge the pre- and post-2012 data, we geocoded the addresses of the polling stations in 2004–10 using a combination of Yandex and Google mapping services, and then, for each precinct in 2012–14, we found a precinct with the nearest polling station in each election from 2004 to 2010.

4. A more flexible version of this regression with oblasts and covariates interacted with time as factor yields very similar results (see app. sec. B.1).

5. In over 90% of cases, only one monument was removed. Alternative definitions of the treatment lead to identical results (see app. sec. B.3).

6. The estimates are very similar if we do not use weights (see app. sec. B.4).

a diamond saw blade could not be found to cut through a monument made of solid granite (Gordonua 2016). We do not claim that the removals were exogenous across the board, however. Our empirical strategy relies on a weaker assumption that election outcomes in the treated precincts would have trended in the same way had they not been treated.

We see two key threats to this assumption of common trends. It could be that political actors were more eager to remove monuments in precincts where they expected pro-Soviet electoral mobilization to rise. In Ukraine's fluid party system, it is very unlikely that anyone could anticipate changes in election returns at such a small geographic scale as a precinct. But given the importance of regional politics (Katchanovski 2006), it is possible that these trends could be anticipated on a larger, regional scale. Oblast-level time trends are included to partial out such anticipatory effects.

The literature also suggests that factors like urbanization and development (Birch 2000) or geographic proximity to Russia (Peisakhin and Rozenas 2018) are predictive of voting patterns in Ukraine. Since it is possible that monument removals were more likely to occur in urbanized places or places further from Russia, we include covariate-specific time trends. The covariates include a precinct size category (according to the official classification of precincts as small, intermediate, or large), the density of roads in the precinct, the longitude and latitude of a precinct's centroid, and their product. We also allow time trends to vary by proximity to Kyiv, since this is where *Leninopad* started. We further corroborate the common trends assumption through specification with precinct-specific trends, synthetic control analysis, and falsification tests.

RESULTS

Table 1 shows the output from different multiperiod DID regressions. Column 1 uses the most basic specification with only precinct and election fixed effects. In column 2, we add oblast-level time trends, and in column 3, we add covariate-specific trends. In column 4, we allow each precinct to have its own time trends (which subsumes oblast- and covariate-specific trends). The estimates are similar across the specifications. A removal of one monument increased the overall turnout by 3.3–4.3 percentage points, and it also increased the turnout among pro-Soviet voters from 1.5 to 2.4 percentage points, depending on the specification.

Table 2 reports estimates from two-period DID regressions. Here we cannot include precinct-level trends, but we do include oblast-specific and covariate-specific trends—the specification that yielded the most conservative estimates in the multiperiod setting (col. 3 in table 1). We see that the results in multiperiod regressions were driven entirely by the elections of May 2014, during which a monument removal increased the overall and the pro-Soviet turnout by 1.6 and 1.7 percentage points, respectively. Monument removals had no statistically detectable effects in any subsequent elections. These conclusions are robust to alternative specifications, definitions of the treatment, and regression weights. Also, they are not driven by any particular oblast (see app. secs. B.1–B.5).

Table 3 decomposes two-period DID estimates for this set of elections. In October 2012, the overall turnout in treated, and control precincts was nearly identical. By May 2014, the overall turnout decreased by 1.1 percentage points in the control precincts, but it increased by 0.5 percentage

Table 1. Estimates from Multiperiod DID Regressions

	(1)	(2)	(3)	(4)
Effect on overall turnout	3.6***	3.4***	3.3***	4.3***
	(.6)	(.6)	(.6)	(.8)
Effect on pro-Soviet turnout	2.4***	1.6***	1.5***	1.8***
	(.5)	(.3)	(.3)	(.4)
Precincts FE	✓	✓	✓	✓
Election FE	✓	✓	✓	✓
Oblast FE × time		✓	✓	
Covariates × time			✓	
Precincts FE × time				✓

Note. Coefficients represent the effect of one removal on the respective outcome (in percentage points). Ten rounds of elections are included (N = 11,860). Standard errors (in parentheses) are clustered by precinct and oblast. FE = fixed effects.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Table 2. Two-Period DID Regressions

	Overall Turnout	Pro-Soviet Turnout	Precincts
October 2012–May 2014	1.6 (.5)**	1.7 (.4)***	1,296
May 2014–October 2014	−.5 (.4)	−.1 (.2)	887
October 2014–March 2019	−1.3 (.7)	−.6 (.9)	792
October 2014–July 2019	−.2 (.7)	−.8 (.8)	792

Note. Coefficients represent the effect of at least one removal. Only precincts with standing monuments at the baselines are included. Standard errors (in parentheses) are clustered by precinct and oblast.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

points in the treated ones. Under common trends, without *Leninopad*, the average turnout in May 2014 would have been 1.6 percentage points lower than observed.

Euromaidan and annexation of Crimea by Russia drastically depleted support for the Soviet legacy candidates. In control precincts, pro-Soviet turnout dropped by 21.2 percentage points, but in the treated precincts, turnout dropped by 19.5 percentage points—1.7 percentage points less. Given that the average pro-Soviet turnout in May 2014 was around 8%, the difference of 1.7% seems substantial.[7]

Can the effect of removals on pro-Soviet turnout be attributed to mobilization or to party switching? The party-switching hypothesis would be plausible if removals had increased pro-Soviet turnout without changing the overall turnout (voters switched parties). Instead, we see that the overall turnout increased by a nearly identical magnitude as did pro-Soviet turnout, suggesting that we should favor the mobilization hypothesis.

Two additional pieces of evidence reinforce this interpretation. In general, higher turnout favored pro-Soviet parties (see app. sec. C.1). Also, if it happened at all, party switching toward pro-Soviet parties must have come from the "centrist" voters rather than nationalist voters with polar opposite preferences. If so, we should see a negative effect of removals on the centrist turnout. We found no support for this prediction in the data: the estimated DID effect of removals on the centrist turnout is insignificant and positive (see app. sec. C.2).

The mobilization hypothesis suggests an important unobservable confounder: Lenin's monuments could have been removed in places where the local tensions mobilized the population to both remove the monuments and also take part in elections rendering our results spurious. Adjusting for this type of confounding directly beyond what we already do is difficult, but we can tease out and test one implication of this argument: if higher mobilization accounts for both removals and later turnout, then the removals should be associated with higher turnout for nationalist parties whose supporters often were behind the removals. Instead, we find that slightly fewer nationalist voters were mobilized by the removals (see app. sec. C.2), which is the opposite to what this type of confounding implies.

THE COMMON TRENDS

Our inferences assume that the analyzed outcomes trended independently of the monument removals. We have accounted for a number of channels through which this assumption could have been violated, including a specification with precinct-level linear trends. But this solution is only partial if the underlying trends are nonlinear. Also, as table 3 shows, pro-Soviet turnout is imbalanced at the baseline (fortunately, the overall turnout is balanced). Even though DID design does not require outcome variables to be balanced at the baseline, stark imbalances are concerning.

To draw inferences on the basis of more balanced comparisons, we use the generalized synthetic control method (Xu 2017), which constructs a counterfactual control group that matches the treatment group in its pretreatment trajectories (which are allowed to be nonlinear) as well as levels using the matrix completion method. Figure 3 shows the average values of the variables in the treated precincts that saw statues removed (*black solid line*), in the control precincts where statues existed but were not removed (*black dashed line*), and the synthetic control group constructed from precincts that match

7. Note that pro-Soviet turnout in the treatment group was hitting the lower bound in May 2014, which raises a concern that the coefficient for pro-Soviet turnout was driven by the floor effects. We show in app. sec. B.6 that this is unlikely because the results are very similar if we limit our analyses to precincts where the floor effects were not binding.

Table 3. Decomposition of the DID Effects

	Control			Treatment			
Turnout	October 2012	May 2014	Δ_C	October 2012	May 2014	Δ_T	$\Delta_T - \Delta_C$
Overall	57.2	56.1	−1.1	57.5	58.0	.5	1.6
Pro-Soviet	31.0	9.9	−21.2	21.1	1.7	−19.5	1.7

the treatment group before May 2014 (*gray dashed line*).[8] The figure also shows the average treatment effects on the treated (the average differences between the treated group and the synthetic control group), which represents the estimated counterfactual to the treated group.[9]

As far as the overall turnout is concerned, the observed control group is already a fairly accurate counterfactual to the treated group: they match each other closely in both levels and trajectories, in the pretreatment period. In the case of pro-Soviet turnout, we see again (as we did in table 3) that the observed control group had consistently higher pro-Soviet turnout, but, importantly, the trajectories of the two groups are very similar. In both cases, the matrix completion method seems to have fixed the imbalances in levels and trajectories extremely well: the treatment group and the synthetic control are virtually identical in the pretreatment period.

Comparing the treated group with the synthetic control group, we see that there are essentially no differences between the two in the pretreatment period, but they diverge in the posttreatment only, which is reassuring. The effects of removals estimated on the basis of the synthetic control group are consistent with our earlier results: a removed monument produced about a 7% larger overall turnout and about a 5% larger pro-Soviet turnout in the May 2014 elections. These larger magnitudes suggest that the potential violations of the common trends assumption are likely to attenuate our baseline results. At the same time, no ex post adjustment can completely make up for the lack of balance in the raw data, and so the pretreatment imbalance in the levels of pro-Soviet turnout remains a caveat in our analysis.

Figure 3 also suggests that the effects of removals persisted. The overall turnout stayed higher for two rounds of elections, and the pro-Soviet turnout remained higher in all elections following the removals. Unpacking the causes of this persistence is outside the scope of this article, but given that removals that took place after May 2014 did not have an effect on elections, it is unlikely that the persistence was due to voters recalling the early removals in the later elections. A more plausible explanation is habitual voting (Gerber, Green, and Shachar 2003): the early wave of *Leninopad* prompted pro-Soviet citizens to vote, and they continued doing so out of habit.

As a further check, we conducted a falsification test that aims to uncover short-term differential trends. We estimated two-period regressions for each successive pair of elections from December 2004 to July 2019 with the treatment variable $D_{it} = 1$ if precinct i had a monument removed between October 2012 and May 2014 and if $t = 1$ (within each pair). Under common trends, we should only see effects of removals on the May 2014 elections. Figure 4 shows that this was indeed the case: in all cases, except those for May 2014, the null hypotheses cannot be rejected.

THE ROLE OF PROTESTS

Leninopad unfolded in the context of mass protests. The compounding of antigovernment protests and the removals of the Soviet monuments poses an inferential challenge: Can we attribute the estimated effects to *Leninopad*, or should they be attributed to protests that often accompanied those removals? Additional data we collected (see below) indicate that over 90% of removals before May 2014 were conducted by activists, usually during protests, which underscores the problem of compound treatment.

To address this issue, we collected several types of data on Euromaidan protests from the start of Euromaidan through February 2014, the period during which most of the pre–May 2014 demolitions occurred.[10] Our first measure uses protests

8. Turnout in presidential elections is, on average, 8% higher than in the parliamentary ones. When arranged in time, the turnout figures show a "chain saw" pattern causing complications in the estimation of pretreatment trends. We "evened out" the turnout figures by subtracting 4% from all presidential elections and adding 4% to all parliamentary ones. This normalization does not bias our estimates, as the same constant is added to the control and treatment units.

9. For consistency with the DID regressions, we weigh each observation by the number of voters.

10. Including protests that occurred after the removals would result in posttreatment bias.

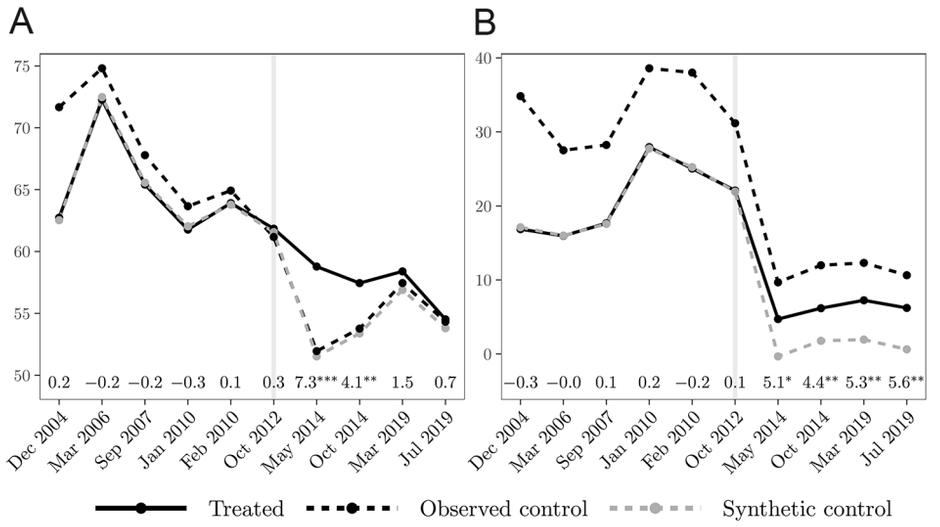

Figure 3. Generalized synthetic control analysis: *A*, overall turnout; *B*, pro-Soviet turnout. Vertical bars separate pre- and posttreatment trajectories. Numbers at the bottom are average treatment effects on the treated. Asterisks represent significance levels (* $p < .05$, ** $p < .01$, *** $p < .001$). *P*-values are bootstrapped to account for clustering by precinct and oblast and Bonferroni-corrected for multiple comparisons.

reported in Ukraine's mass media (see app. sec. D.1 for details on data collection). Since media reports often lack information to identify the location of protests to the level of a precinct, we georeferenced protest locations to the level of a council (*rada*), Ukraine's third-tier administrative unit.

To rule out the selective biases in the protest reporting in the media, we constructed two additional proxy measures of protests using data from Twitter, which played a crucial role in Euromaidan (Bohdanova 2014). Our social media data consist of 2,420,807 georeferenced tweets posted in Ukraine from December 2013 through February 2014 collected by Wilson (2017).[11] These data constitute the entire population of tweets by users with activated geotracking. Using the subset of these data confined to the capital city of Kyiv, Wilson (2017) found that protests could be predicted quite accurately from the volume of tweets by well-connected users. Motivated by this result, we constructed two social media-based measures of protests across Ukraine.

First, we calculated the total number of tweets per council that contained the word or hashtag "Euromaidan" weighted by the number of followers of the tweeting account. This measure captures the online coordination on the protest event by well-connected social media users, which arguably preceded offline mobilization.

Second, we used machine learning to predict protest activities from the daily usage patterns of Twitter. For each council-day, we calculated the total number of tweets, the number of tweets on "Euromaidan," and both of these totals weighted by the number of followers of the respective tweets. We then used the random forest algorithm (Muchlinski et al. 2016) to predict daily protests recorded in the media from these four features of Twitter usage in the sample of councils where at least one protest was recorded by the media. The algorithm was able to predict recorded protests with 86% accuracy.[12] We then calculated the number of predicted protests per council (including out-of-sample councils with no recorded protests) as a measure of local protest activity.[13]

With these three measures of protest, we reestimated the two-period DID regressions by adding an interaction between a protest and the posttreatment indicator.[14] The DID coefficient for removals should attenuate toward zero if removals were epiphenomenal to protests. In addition, we ran DID regressions with protest as the only treatment variable in the set of councils where no monuments existed (and so none could be removed). If protests, not the removals, produced the backlash, then we should observe consistent backlash effects of protests in places without the monuments.

11. Because of Twitter's terms of use, we were not able to obtain the actual texts of the tweets, but we did have an identifier of whether a tweet mentions Euromaidan (in the text or the hashtag).

12. We do not want this accuracy to be too high, because the data on which the algorithm is trained likely underreport actual protests. Of the 9,119 councils with no protests recorded in the media, the algorithm predicted protests in 3,458 of them.

13. Appendix sec. D.2 provides technical details.

14. Because of skewness, all protest variables are transformed using the $\ln(1 + x)$ function.

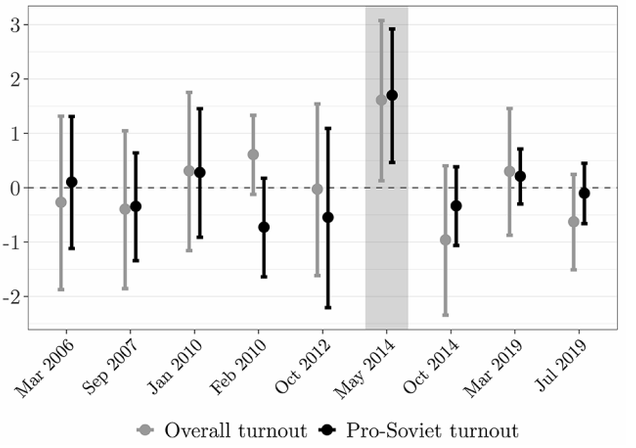

Figure 4. Falsification test. DID results for successive pairs of elections where the precinct is considered as treated if it had a removal between October 2012 and May 2014. The 95% confidence intervals are Bonferroni-corrected for multiple comparisons (since we have nine point estimates for each outcome, the nominal confidence of the displayed intervals is $100 \times (1 - .05/9) \approx 99.4\%$).

The results of these analyses are shown in table 4. These two-period DID regressions are estimated at the level of a council (with council-level fixed effects) and include oblast-specific and covariate-specific trends. The first two columns show that, irrespective of the measure of protest, the coefficients for removals are robust and consistent with the baseline estimates. There is also no consistent relationship between protests and elections in places without monuments, as shown in the last two columns of the table.[15] This suggests that the effects of the early *Leninopad* cannot be attributed to the protests that often surrounded monument removals.

WHEN DOES SYMBOLIC POLITICS MATTER?

We now investigate why only the first wave of monument demolitions had detectable electoral consequences. There were many differences between each round of elections, and the dynamics of *Leninopad* also varied. Any of these—or other—differences could potentially underlie the effect heterogeneity.

Differences in voting options

Voting options varied significantly across the four elections. The Communist Party of Ukraine—the most ideological Soviet legacy party—was outlawed soon after the October 2014 elections. The Party of Regions—the most successful Soviet legacy party—faced mass defections and rebranded itself into the Opposition Block. What if *Leninopad* had no electoral impact after May 2014 simply because pro-Soviet voters had fewer viable choices?

This conjecture is questionable because the Soviet legacy parties were already ostensibly weak during the presidential election of May 2014. Many candidates from the Soviet legacy camp ran in the May 2014 elections (thereby fragmenting the pro-Soviet votes), but none of them carried the weight of its ousted leader Yanukovich. The best performing Soviet legacy candidate in May 2014—Serhiy Tihipko, a vice prime minister under Yanukovich—received only 5.2% of the votes. If anything, with the two focal Soviet legacy parties—the Communist Party and the Opposition Block—on the ballot, pro-Soviet voters had more viable choices in October 2014 than in May 2014.

Differences in how monuments were removed

As figure 1 shows, over 90% of all removals occurred within four months of the May 2014 elections, whereas the later removals were more spread out. Could the temporal proximity of removals to elections belie the effect heterogeneity? To answer this question, we redefine a precinct as treated if it had a monument removed within four months of an election and then reestimate the two-period DID regressions. Figure 5*A* shows that even when we consider only the removals that took place close to the election, only removals before May 2014 had an impact on elections. The temporal distribution of removals cannot explain the effect heterogeneity.

Another source of effect heterogeneity could be the involvement of different actors behind the removals at different stages of *Leninopad*. Using mass media reports and publicly available videos and photos of Lenin statue demolitions, we were able to identify who was behind 87% of recorded demolitions. In the early stages, the vast majority of demolitions were done by political activists, usually during a protest: 94% before May 2014 and 73% before October 2014. But after October 2014, about 80% of removals were conducted by government authorities. Could it be that only early removals produced a backlash simply because they were conducted by protesters?

Two pieces of evidence suggest that this is not a convincing explanation. First, we did not detect electoral effects of removals in October 2014, even though protesters were behind 73% of them. Second, in figure 5 we show the estimated effects of removals by authorities versus protesters. The removals post May 2014 had no electoral effects irrespective of whether they were conducted by protesters or government; only the removals by protesters before 2014 had a discernible effect. The involvement of protesters seems to be a necessary but not sufficient condition for the backlash effect.

15. Similar results are borne out if we use *rayon* as the unit of analysis (see app. sec. D.3).

Table 4. Protest as an Alternative Mechanism

	Councils with Monuments		Councils without Monuments	
	Overall	Pro-Soviet	Overall	Pro-Soviet
Protests reported in the media:				
Protests	.4***	.4*	−.0	−.0
	(.1)	(.2)	(.2)	(.3)
Removals	2.6***	2.4***		
	(.5)	(.6)		
Tweets on "Euromaidan":				
Protests	.3***	.2**	.1	.0
	(.0)	(.1)	(.1)	(.1)
Removals	2.4**	2.5**		
	(.7)	(.8)		
Protests predicted from social media:				
Protests	.2	.2	.6	.8**
	(.2)	(.2)	(.3)	(.3)
Removals	3.3***	3.1***		
	(.6)	(.7)		
N	2,034		16,444	

Note. DID regression coefficients for different measures of protest and monument removals. All specifications control for oblast- and covariate-specific time trends. The unit of analysis is council (*rada*). Standard errors (in parentheses) are clustered by council and oblast.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Political deactivation of symbols

According to our theoretical discussion, symbolic politics is likely to affect elections only when the conflicts over symbols map onto the partisan cleavages. Observers of Ukrainian politics had noted that the dominant interpretation of Soviet symbols and their removals had shifted profoundly between the start of Euromaidan and the summer of 2014. Early on, supporting *Leninopad* meant opposition toward the Communist Party and the Party of Regions and endorsement of Euromaidan, whereas speaking against *Leninopad* was a way to criticize changes wrought by Euromaidan.

The proxy war with Russia in Donbass, which escalated in the summer of 2014, perturbed this interpretation of the Soviet symbols and their removals. "The Soviet symbols and symbols

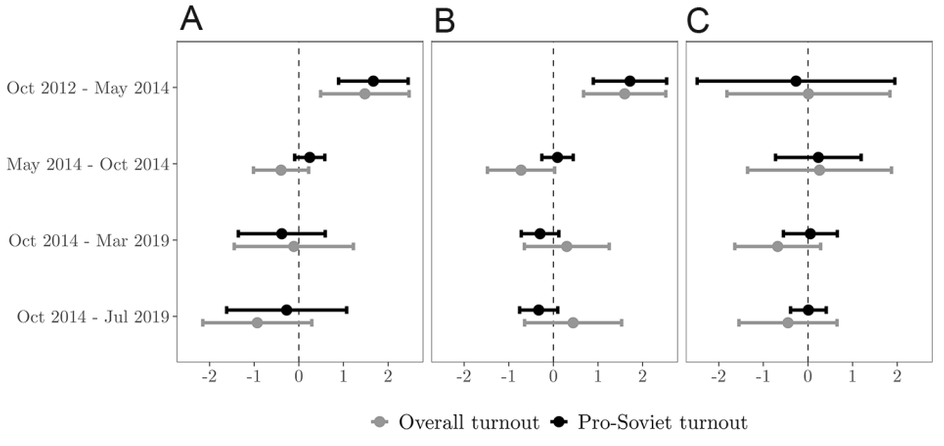

Figure 5. Estimates for different types of removals: *A*, removed within four months before election; *B*, removed by protestors; *C*, removed by government. Two-period DID point estimates with 95% confidence intervals for differently defined treatments (oblast- and covariate-specific trends are included).

of anti-Maidan as well as symbols of the regime of Yanukovich now became symbols of separatists" (Gayday and Liubarec 2016, 37). Lenin and his monuments began to represent not just the persistent legacy of the Soviet past but an assault on Ukraine's national sovereignty, an issue on which Ukrainians were united across partisan lines (Jekaterynczuk 2015). The new crosscutting cleavage deactivated the controversies surrounding *Leninopad*, which could explain why its electoral impacts began to dwindle following the summer of 2014.

A key empirical implication of the above argument is that the meaning of the Soviet symbols shifted after the first post-Maidan elections. To assess this claim, we assembled a corpus of news reports that mentioned the word "Lenin" from Ukraine's daily newspapers, news websites, and one national TV station, totaling 771 articles from December 2013 until March 2019.[16] We then used the structural topic model (Roberts, Stewart, and Tingley 2019) to estimate how the topics covered in these articles have changed throughout the three inter-election periods. In particular, did the narratives surrounding Lenin shift from being a contentious partisan topic to a consensual topic?

After preprocessing the text, we estimate a model with three topics.[17] We then labeled the topics by inspecting the most frequent words associated with them. We labeled the first topic "Protest" as it is associated with contentious events like protests ("monument," "city," "people"). The second topic, which we labeled "Culture," is about representation of Soviet history in culture ("films" and "books"). The third topic, which we labeled "Sovereignty," is distinguished by the use of terms that became central in the discussion of Ukraine's sovereignty in the context of war: "Russia," "country," "war," "power," and "state."

Figure 6 shows how the predicted proportions of these three topics changed over time. Before May 2014, roughly 60% of news coverage related to Lenin concerned contentious protest events. However, by October 2014, the distribution of topics shifted substantially: the articles that mentioned Lenin did so less in the context of protests and more in the context of national sovereignty. The prevalence of Protest decreased by about 13 percentage points ($p < .05$),

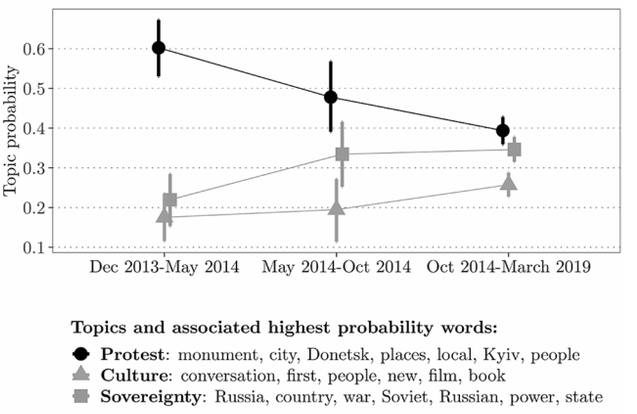

Figure 6. Meaning of Lenin. Predicted probabilities of topics with 95% credible intervals, estimated by the structural topic model.

whereas the prevalence of Sovereignty increased by about 12 percentage points ($p < .05$). In the period after October 2014, the prevalence of Protest decreased by an additional 7 percentage points ($p < .05$). Instead, Lenin was increasingly discussed as a cultural topic: the prevalence of Culture increased by 8 percentage points ($p < .05$).

During the protests of winter 2013 and spring 2014, *Leninopad* represented the contentious issue of decommunization (Gayday 2018). Even though most Ukrainians probably did not care whether Lenin's monuments existed, their demolitions activated dormant pro-Soviet sentiments, mobilizing those who explicitly opposed changes to the status quo (Kasyanov 2019). *Leninopad* fitted neatly into the cleavage between pro-Russian and pro-Western forces, which largely defined Ukrainian politics since the Orange Revolution of 2004. Lenin's fall represented de facto empowerment of the pro-Western parties at the expense of the pro-Russian ones.

The escalating war in the summer of 2014 shifted attention toward territorial integrity, an internal displacement crisis, and mass casualties. Proseparatist organizations like Oplot or Ukrainian Front, which actively opposed *Leninopad*, employed Soviet symbols in their public relations campaigns (Barkov 2018). These groups failed to gain meaningful support, but their use of pro-Soviet rhetoric and symbols was widely noted. The norms surrounding the Soviet symbols changed: supporting these symbols now meant approval of separatism, whereas removing these symbols now meant support for Ukraine's sovereignty (Kasyanov 2019). Subsequently, the removals of these symbols faced little opposition from either the elites or the public (Gayday and Liubarec 2016).

The above interpretation has one important caveat: it could be that *Leninopad* would have ceased being a contentious issue naturally, even without the new crosscutting cleavage. This is a possibility that we cannot rule out directly, but

16. The sources include pro-Ukrainian (pravda.com.ua, zn.ua), pro-Russian (fakty.ua), and neutral mass media (unian.ua, day.kyiv.ua, 24tv.ua, glavred.info, gazeta.ua).

17. We lemmatized the text, removed stop words and the words "Lenin" and "Ukraine," as they were featured in all extracted topics. We fit a low-dimensional model with three topics because, first, we do not want to risk overfitting given that our corpus of texts is quite small and, second, our experimentation with more complex models did not reveal new, substantively distinct topics. The loss of topical coherence indicates redundancy of additional topics (Roberts et al. 2019).

we think that at least two facts speak against it. First, a natural decline of political salience of an issue should be gradual, but we see that *Leninopad* stopped affecting elections quite abruptly, within five months after May 2014. Second, the status of the Soviet symbols continued to be discussed publicly, especially with the introduction of the Decommunization Law in May 2015, but the issue did not have the same partisan charge as it did in the spring of 2014.

CONCLUSION

Given that symbols and symbolic actions do not have immediate implications on the distribution of power or resources, it is remarkable how much political energy is spent on them. Why is it such a big deal if American football players kneel during the national anthem, if the national soccer team in France sings the "La Marseillaise." or if the constitution of the European Union mentions God? Why is it so important, in political terms, which historical personality is memorialized in a monument as a hero or dememorialized as a villain?

We argued that conflicts over symbols matter because they allow competing groups to publicly test their strength. Lenin's monuments stood in Ukraine for almost a century. They might have easily gone unnoticed during the tumultuous events of 2014 as had happened during the Orange Revolution in 2004. When these memorials of Ukraine's Soviet past started being openly challenged, they became focal points of partisan contention. By removing these memorials, the Euromaidan forces were able to demonstrate the growth of their power, but that in turn agitated opponents of Euromaidan to claim back their lost status at the ballot box. *Leninopad* accrued interim benefits for Ukraine's "pro-Western" forces by invigorating the protest movement, but it also generated downstream electoral costs.

The scope conditions under which symbolic politics matter are limited in a way that makes theoretical sense. *Leninopad* sparked an electoral reaction only in its initial stage, and we argued that this was because the proxy war with Russia encumbered those who would have otherwise risen against the diminishing power status of the Soviet legacy parties. The issue of sovereignty cut across the partisan lines along which the conflict over the Soviet symbols was fought, and *Leninopad* morphed into a routine bureaucratic operation without mobilizing charge. These scope conditions resonate well with the finding that the removals of the Confederate memorials provoke opposition only when conducted without consensus-building deliberation (Johnson et al. 2019) as well as with the literature on how crosscutting cleavages abate conflicts (Siroky and Hechter 2016).

Symbols that become politically contentious often have something to do with past violence, oppression, and domination. Even long after violence and oppression end, the shadow of the past continues to mold politics. Not only are political norms shaped by history, but politics at large is often a competition between polarizing interpretations of the past (Pop-Eleches and Tucker 2017). When such polarizing cleavages dominate politics, challenges to the artifacts that memorialize this contentious history can be perceived as a threat to the status quo distribution of political status.

We have found this to be the case even when the issue concerns something as immaterial, from the point of view of individual utility, as a monument. When the vestiges of the past concern more tangible issues, like transitional justice initiatives that threaten perpetrators of past repression (Nalepa 2010) or economic reforms that threaten the wealth basis of past elites (Radnitz 2010), the backlash could be more pronounced than in the case analyzed here. Without a crosscutting cleavage that deactivates contentious historical legacies, such reforms may carry electoral costs.

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