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How Political Dynasties Concentrate Advantage within Cities: Evidence from Crime and City Services in Chicago

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Classic models of urban inequality acknowledge the importance of politics for resource distribution and service provision. Yet, contemporary studies of spatial inequality rarely measure politics directly. In this paper, we introduce political dynasties as a way of integrating political economy approaches with ecological theory to better understand the political construction of urban spatial inequality. To do so, we examine the case of political dynasties within the Chicago city council. We show that, from 2011 to 2018, blocks in dynastic wards saw fewer homicides, assaults, robberies, and thefts relative to those in non-dynastic wards. We then leverage the 2015 ward redistricting to provide evidence that dynastic effects play some role in producing these outcomes: blocks annexed into dynastic wards experienced a decline in assaults and robberies and an increase in pothole coverings. While dynastic politicians improve outcomes for blocks they annex, they also withdraw power from those they displace; and displaced blocks had relatively higher levels of crime than annexed blocks in 2015. Taken together, our findings provide evidence that dynastic politicians are contributing to spatial inequalities within Chicago.

Introduction

Names like Bush, Rockefeller, and Kennedy are synonymous with political power in the United States, but they are just a few examples of the political dynasties that operate in democracies throughout the world (Dal Bó, Dal Bó, and Snyder 2009; Feinstein 2010). In fact, dynastic politicians, or elected officials who have a relative in a prior government position (Mendoza et al. 2016), have been part of US politics and government since the country's founding (Clubok, Wilensky, and Berghorn 1969; Kurtz 1989). But research on the social consequences of political dynasties has focused primarily on developing democracies. In the Philippines, for example, political dynasties have been shown to increase inequality between provinces (Mendoza et al. 2016).

Political dynasties are thus a potentially important, but understudied, form of durable political power that may affect the distribution of public resources in the United States. And because dynastic power may allow politicians to concentrate resources within a specific geography—the boundaries of an electoral district—they also have the potential to shape inequality between places, as is the case in the Philippines. Such spatial inequality has been a focus of urban

sociologists dating back to the earliest days of the Chicago School (Park and Burgess 1925; Sampson 2011), and yet political dynasties in US cities have yet to be considered as a cause of some portion of these inequalities. This is due to both empirical challenges in isolating the causal effects of politics over urban outcomes (Vélez and Lyons 2014) and to recent sociological scholarship on US urban politics, which has focused on a shift from an era of machine control over resource distribution (Trounstine 2008), to one in which partnerships among nonprofits, foundations, and professional city managers govern technocratically (Levine 2021; Marwell 2007; Pacewicz 2016). But the focus on what *has* changed in urban governance has obscured what has not: the fact that some power still operates through elected officials amidst these new governance arrangements (da Cruz, Rode, and McQuarrie 2019). This raises the question, to what extent do urban politicians continue to shape inequality within cities?

If there were any elected officials in US cities that might still matter for generating spatial inequalities, it would be dynastic politicians. These politicians benefit from the political capital that their family members have passed down and use this capital to gain access to key positions of power within city government that enable them to funnel scarce public resources to their districts. Given these advantages, our study investigates: how do political dynasties affect urban inequality?

We answer this question through a case study of the Chicago city council. First, we draw on longitudinal, block-level data on city service provision and crime from 2011 to 2018 to show that blocks within dynastic wards fare better on several crime metrics, relative to those within non-dynastic wards. Second, we assess whether there is evidence that dynastic power itself is contributing to this difference—i.e. whether dynastic politicians are affecting outcomes within their wards. Specifically, we exploit the 2015 Chicago ward redistricting, when 1839 blocks were annexed into dynastic wards, and use event study models to isolate within-block variation before and after annexation.

Regression results show that, from 2011 to 2018, Chicago's dynastic wards saw meaningfully better outcomes than other areas of the city when it comes to homicides, assaults, robberies, and thefts. Furthermore, our analyses of the 2015 redistricting provide evidence that dynastic power itself is shaping urban outcomes: in the years following the 2015 redistricting, blocks annexed into dynastic wards saw fewer robberies and assaults, as well as more potholes covered. Moreover, we find suggestive evidence pointing to the mechanisms by which dynastic politicians are generating these effects: by leveraging their receipt of extra public funds through powerful Committee Chair positions to improve city services, increase civic engagement, and ultimately lower crime within their wards (Vélez and Lyons 2014).

These findings advance efforts to identify the political-economic roots of urban inequality (Vélez, Lyons, and Santoro 2015) by showing how political power can still shape spatial inequality within cities. Although providing disadvantaged neighborhoods with links to powerful urban elites might be a pathway toward decreasing spatial inequality, this is not what we observe here: if anything, our findings suggest that dynastic politicians displace blocks with relatively high levels of crime, which are also poorer and contain more Black and Hispanic residents, than those they annex during redistricting. As such, dynastic politicians are withdrawing power from blocks in greater need of assistance and *concentrating advantage* on the relatively advantaged blocks within their territories. In the zero-sum game of neighborhood competition over limited urban resources, political inequality translates into spatial inequality (Levine and Gershenson 2014).

Crime, City Services, and Place Stratification

Despite longstanding recognition that political power affects urban inequality, research that models this relationship in the context of the new urban political economy is nascent. Instead, quantitative studies of urban spatial inequality remain grounded in ecological theory, which conceptualizes social phenomena in cities as spatially ordered and linked to durable features of a place that outlast individual inhabitants (Park and Burgess 1925; Sampson 2011). The

contemporary instantiation of this tradition, "neighborhood effects" research, follows from Shaw and McKay's (1942) social disorganization model, showing that neighborhoods suffering from a concentration of disadvantage—including poverty, racial segregation, and residential instability—struggle to maintain the kind of effective social ties that prevent crime (McNulty and Bellair 2003; Morenoff, Sampson, and Raudenbush 2001; Peterson and Krivo 1993). Conversely, communities that maintain high levels of interpersonal trust, civic engagement, and willingness to intervene on behalf of their neighbors—or collective efficacy—can lower their crime rates (Browning, Dietz, and Feinberg 2004; Sampson, Raudenbush, and Earls 1997).

Neighborhood effects scholars focus on mechanisms linking factors *internal* to residential contexts with crime and other negative outcomes (e.g. Sharkey and Faber 2014). But as urban sociologists have increasingly argued, this focus elides the second half of urban ecological theory, which argues that spatial units are interdependent with one another and fit into the larger city like pieces of a puzzle (Bursik 1988; Marwell, Marantz, and Baldassarri 2020; Sampson 2011).

In contrast, a longstanding critique from urban political economy scholars has claimed that spatial inequality within cities is politically constructed, as advantaged neighborhoods use their political power to maintain their advantages (Logan 1978; Logan and Molotch 1986; Molotch 1967). This means that neighborhood (dis)advantage results in part from their access to external resources—public and private capital that help neighborhoods improve local infrastructure, build collective efficacy, and limit crime (Lyons, Vélez, and Santoro et al. 2013; MacDonald et al. 2010; Marwell et al. 2020; Ramey and Shrider 2014; Vélez, Lyons, and Santoro 2015). Powerful and receptive politicians can serve as neighborhood brokers, leveraging access to limited resources to address the problems that residents raise (Carr 2003; Vélez and Lyons 2014; Vélez et al. 2015). In the context of the zero-sum game of urban politics, access to political power helps certain neighborhoods advantage themselves vis-à-vis other neighborhoods.

But even as scholars have generated important insights into the relationship between political power and spatial inequality, there are two challenges to advancing this literature: empirical challenges in measuring and causally identifying the effects of political power over urban outcomes (Vélez and Lyons 2014) and theoretical challenges in conceptualizing how political power continues to shape resource allocation within new urban governance contexts (da Cruz, Rode, and McQuarrie 2019). In this paper, we offer a way to address both challenges by identifying political dynasties as one piece of the explanation for the uneven geography of city services and crime in urban America.

Political Dynasties and Urban Political Power in the Twenty-First Century

Why are political dynasties an apt way of conceptualizing how political power shapes resource distribution in twenty-first-century cities? Urban political power was central to the process of place stratification throughout the twentieth century. Under machine rule, urban bosses controlled the "local state" and directed public resources to their core constituencies, creating inequalities across neighborhoods typically defined by ethnic and racial boundaries (Trounstine 2008).

In such a context, it is easy to see how select individuals might dictate the unequal distribution of resources within a city. But since the downfall of machine politics, sociologists have argued that a new technocratic form of urban governance has emerged as long-term "partnerships" between elected officials, bureaucrats, and nonprofits now determine how cities distribute resources (Marwell 2007; McQuarrie 2013; Pacewicz 2016). In these accounts, elected officials still shape resource distribution within cities, but they face a different context after federal efforts toward the privatization and devolution of social welfare (Levine and Gershenson 2014; Marwell 2004; Marwell and Gullickson 2013; Marwell et al. 2020; Vargas 2016).

In this context, as Marwell et al. (2020) have recently argued, urban political power should be understood *relationally*: situated at the interstices of elected officials, nonprofits, churches, and

foundations who now conjointly control access to public and private resources. In Chicago, as Sampson (2011) has shown, elected officials remain at the center of these resource distribution networks.

Our study builds on these insights, offering a way to conceptualize and measure how elected officials continue to affect spatial inequality within an urban governance framework that has expanded to include many non-state actors. If it is no longer the case that a single boss operates by fiat, then which politicians might still have the power to influence urban outcomes? If any local politicians might retain such power, we argue that it would be *dynastic politicians*, defined as elected officials who have a family member that has previously served in an elected office or government position (Mendoza et al. 2016).

Our conceptualization of political dynasties draws on Ibn Khaldûn's classical work (1378[2016]), which describes dynasties as multi-generational ruling families who maintained power in cities through relations of clientelist exchange. Crucially, in Khaldûn's account, dynasties are *durable and adaptable relations of power*. Much like Khaldûn, analysts today conceptualize political dynasties as grounded in family ties that confer on candidates both electoral advantage and access to clientelist networks (Chandra 2016; Cruz, Labonne, and Querubin 2017; Cruz, Labonne, and Querubín 2020; Dal Bó, Dal Bó, and Snyder 2009; Kurtz 1989; Querubin 2016). Although elected officials do not inherit their offices, as did dynastic successors under monarchy, elected dynastic politicians *do* inherit their family's political capital, including "brand name advantages" (Feinstein 2010) and ties to donors, other elected officials, and powerful bureaucrats.

Although dynasties have always been part of American politics (Dal Bó, Dal Bó, and Snyder 2009; Feinstein 2010; Kurtz 1989), scholars of US spatial inequality have yet to examine how they may influence the unequal distribution of resources across constituencies. Dynasties form when incumbents accrue political power by building ties to donors, other elected officials, and powerful bureaucrats, then incorporating their family members into those networks, thus perpetuating political power in ways that can transcend the election cycle. For this reason, dynastic power is an apt way of understanding how political power has become embedded *within* new structures of technocratic governance. Because technocratic reforms are shaped by pre-existing power relations (Baiocchi, Heller, and Silva 2011), new forms of urban governance have not erased the political processes by which cities operated in the past. While urban bosses have fallen, political dynasties have persisted.

Dynastic Power and Spatial Inequality

Dynastic constituencies may see better outcomes than non-dynastic constituencies through two (not mutually exclusive) processes. First, electoral boundaries are themselves politically constructed (Vargas et al. 2021). During redistricting processes—especially where redistricting takes place through legislative bodies rather than independent commissions (McDonald 2004)—politicians have some say in choosing their voters.[1] Dynastic politicians may have more say than the average elected official, and if they annex constituencies with lower levels of crime and better infrastructure than those they displace during redistricting, we would observe a positive correlation between dynastic wards and desirable outcomes even in the absence of a true effect of dynastic power.

Second, once their constituencies are determined, dynastic politicians may also be able to improve city services and crime on their blocks. As Vélez and Lyons (2014:226) argue, "ties between neighborhoods and city elites … can make or break neighborhoods" because powerful elites can funnel private and public resources into disadvantaged neighborhoods. We argue that dynastic politicians are precisely the kinds of powerful elites who have competitive advantages, vis-à-vis other council members, in securing scarce resources for their constituents. This is because of their inherited network of relationships with other elected officials, city department heads, and nonprofits, and because of their positions as chairs of important government committees. In the context of increasingly scarce public resources available to the average city councilor, this is the

crux of dynastic power: not only are these politicians *receptive* to the concerns of their constituents (Lyons et al. 2013; Vélez et al. 2015), they are also *able* to address those concerns.

Dynastic politicians' ability to funnel resources to neighborhoods may also reduce crime, in both the short and long run. In the short run, external resources used to improve local infrastructure can help lower neighborhood crime (Braga, Welsh, and Schnell 2015; Sampson 2011; Vargas 2016). Over the long run, these improvements can help stave off negative spirals of disinvestment and population decline (Vélez and Lyons 2014; Vélez, Lyons, and Boursaw 2012). According to social disorganization theory, these improvements may encourage residents' civic and political engagement, thus increasing collective efficacy and further reducing crime (Ramey and Shrider 2014; Sampson, Raudenbush, and Earls 1997; Shrider and Ramey 2018).

It is also possible that dynastic politicians may take direct action to address crime by working with city police, but it is unlikely that such efforts will succeed in lowering all kinds of crime, especially not over short time periods. Violent crime in particular tends to be responsive to sustained and targeted interventions by local nonprofits and community-based organizations (Grunwald and Papachristos 2017; Papachristos and Kirk 2015; Sharkey, Torrats-Espinosa, and Takyar 2017). Although dynastic politicians may be particularly well positioned to cultivate such organizations within their wards over the long run (Marwell et al. 2020; Vélez and Lyons 2014), these effects would take a long time to materialize.

In sum, we expect dynastic wards to have better outcomes in terms of city services and crime because of dynastic politicians' efforts to cultivate certain electoral boundaries (a selection process), and their ability to improve outcomes within their wards (a treatment effect). We have further argued that dynastic politicians generate this treatment effect in the short run through three related mechanisms: (1) they gain control over limited public resources and funnel them to their districts, which (2) improves local infrastructure and (3) lowers crime. Over the medium-to-long run, (1) and (2) may lead to (4) increased collective efficacy, which further lowers crime.

This has important implications for spatial inequality: following a political economy perspective that understands neighborhoods as competing with one another over limited public and private resources, anytime dynastic politicians use their power to advantage their own wards, they are necessarily taking resources away from other parts of the city (Logan and Molotch 1986). If dynastic politicians serve as a key link between *disadvantaged* neighborhoods and limited city resources, they would be leveling the playing field—without them, wealthy neighborhoods would be more likely to "win" (Vélez and Lyons 2014). But, if dynastic politicians are instead using redistricting processes to shed their relatively disadvantaged blocks, they are exacerbating inequalities between neighborhoods.

The Case of Dynastic Power and Spatial Inequality in Chicago

We illustrate these claims through the case of the Chicago city council, which has been composed of 50 wards since 1923, with each ward serving ~50,000 residents (Simpson 2001). After the demise of the city's classic political machine in the 1970s, the Mayor's office found other ways to manage neighborhoods through the strategic allocation of city resources, campaign funds, and ties to private philanthropy (Emanuel 2020; Fremon 1986). The political dynasties on Chicago's city council have assisted the mayor in these efforts, in some cases as far back as the 1950s. But importantly for our purposes, dynasties' power is linked not just to one mayoral administration, but to broader ties within Chicago's political elites. In this study, we analyze 11 dynastic politicians that were in office throughout the 2011–2018 period. Specifically: 6th, 13th, 14th, 21st, 26th, 33rd, 34th, 39th, 40th, 48th, and 50th Wards. Based on previous literature on political dynasties and our conceptualization of how dynastic power shapes urban inequality today, we considered a ward to be dynastic if the current alderman had a previous family member involved in Chicago or Illinois politics or government (Cruz et al. 2020; Dal Bó, Dal Bó, and Snyder 2009).[2] Figure 1 shows the ward boundaries of dynastic wards in 2005 in panel (a) and 2015 in panel (b).
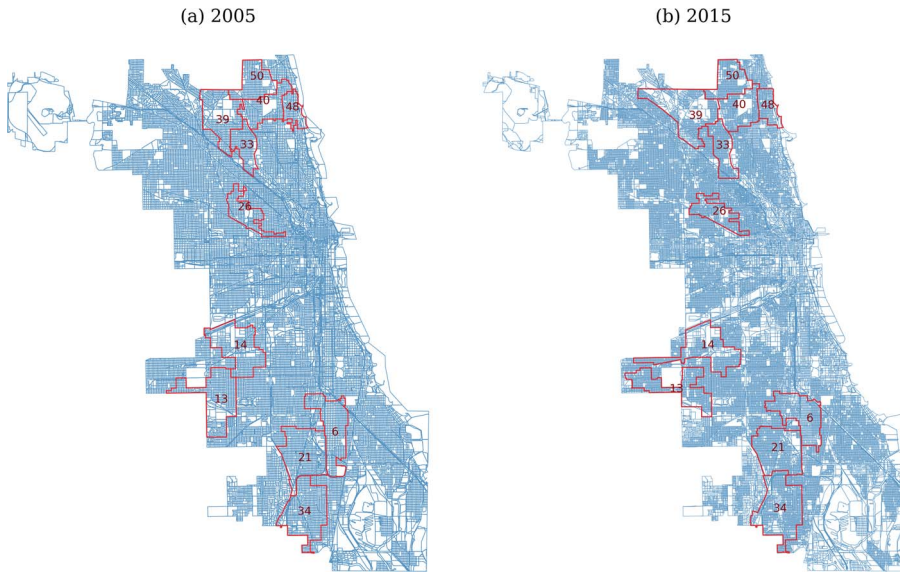
(a) 2005   (b) 2015

Figure 1. Dynastic ward boundaries, before and after 2015 redistricting. The maps depict the boundaries of the dynastic wards in 2005 (map (a)) and in 2015 (map (b)).

We have already argued that dynastic politicians have pre-existing family ties which embed them in relationships of power before entering office. Although many of these resources may be private (e.g. Marwell et al. 2020), in the case of Chicago, dynastic politicians often have access to more public resources than their peers. One reason for this is that they are more likely than average Alders to gain a chairmanship of one the city council's committees. These chairmanships are incredibly lucrative: between 2012 and 2018 the committees provided, on average, $4.96 million of extra money for their chairpersons to hire "personnel."[3] Although these funds are ostensibly to hire staff for committee work, dynastic politicians employ those staff primarily in conducting "ward service," often filling out service requests or making calls directly to city departments to address issues within their wards (Schutz 2019). As there were sixteen committees on the Chicago city council during this time, thirty-four councilors were excluded from the benefits of the extra staffing funds each year. Six of the eleven dynastic politicians served as committee chairs during this time, and those that did held some of the most powerful and lucrative positions: on average during this period, dynastic politicians received $2,922,595 extra dollars for personnel funds (59 percent of the total available). Figure 2 summarizes these funds by year, for each dynastic politician.

Thus, committee chair positions are both an outcome and a mechanism of dynastic power: dynastic politicians are disproportionately benefiting from these resources because they are embedded in relations of power, and they also use these resources to improve services within their wards. Even though new mayoral administrations can appoint new chairs, they are often constrained by existing relations of power: they need to keep powerful alders in chair positions so that they can gain allies to enact their agendas (see Dumke 2019 for an example).

Although committees are one way that dynastic politicians secure advantages for their wards, they are not the only way—politicians with strong social networks may also be more influential in passing zoning and budgetary decisions that are beneficial to their constituencies. Moreover, Chicago's service request system also has the potential to benefit dynastic politicians. When service requests from "Aldermanic accounts" reach city departments, city employees can see which Alderman's office has made the request. This means that city staff may choose to privilege
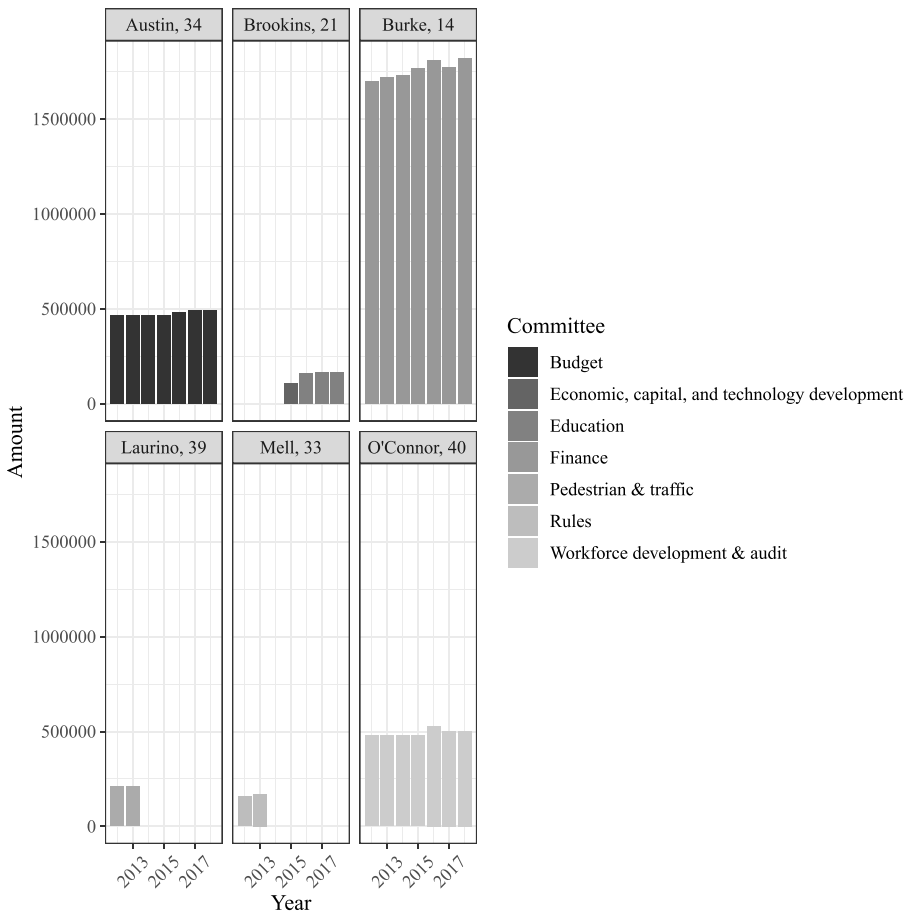
Figure 2. Dynastic politicians' committee positions and extra income. Data collected from annual Chicago City budgets, which are publicly available. The charts total the amount of funds budgeted for "personnel services" for each committee in each year. To identify the committee chairperson for each year, we consulted meeting minutes of the Chicago City Council for the years 2011–2018. Because the 2011 city budget was voted in by the slate of councilors in office in 2010 (prior to the 2011 election), we do not include 2011 in these calculations. Separately, Margaret Laurino of the 39th Ward also received hundreds of thousands of dollars for personnel annually in her position as head of the Legislative Reference Bureau.

responding to service requests from dynastic politicians, and they may be particularly likely to do so when that politician chairs a committee that supervises appointments or budgeting in that department.

Data and Methods
Data Sources and Variable Construction

We draw on block-level counts of homicides, assaults, robberies, thefts, and pothole coverings from the city of Chicago's Data Portal over the period 2011–2018.[4] Our outcome variables are constructed by aggregating point data to the block level using 2010 Census boundaries. Chicago underwent decennial redistricting processes in 2005 and in 2015, so the 2011–2018 period captures the ward boundaries as they were after the 2005 redistricting and allows us to isolate the effect of dynastic power as city blocks were annexed into dynastic wards in 2015.[5]

Analytic Strategy

Analysis 1: Comparing Dynastic and Non-Dynastic Wards. To establish differences between dynastic and non-dynastic wards, we first run a series of Ordinary Least Squares (OLS) regressions in which we predict block-level homicides, robberies, thefts, assaults, and potholes covered, based on whether the block is part of a dynastic ward. Specifically, for each outcome we estimate Model (A):

$$\text{Outcome}_{it} = \beta \text{DynasticWard}_i + \gamma_t + X_{it} + \varepsilon_{it}$$

Our coefficient of interest is DynasticWard_i, which takes on a value of 1 if a block i was part of a dynastic ward during the 2011–2018 period, and 0 otherwise. In these analyses, we exclude blocks that were annexed or displaced from dynastic wards. γ_t represents year fixed effects, which account for any shocks particular to each year; and e_{it} represents the idiosyncratic error term, which is clustered at the block level. Outcome_{it} represents each of the five outcomes in block i in year t. X_{it} represents a vector of time-varying demographic, socioeconomic, and political controls at the block-group level, which may be correlated with both dynastic ward membership and our outcomes of interest, based on American Community Survey's 5-year estimates. These include the total population, the percentage of residents who are Asian, Non-Hispanic White, Black, Hispanic, women, and 18 or over; the median household income (held constant in 2011 dollars); the percentage of residents 18 or over who are unemployed; the percentage of residents with at least a college degree; and the number of occupied buildings within a block group.[6] Table A1 of the appendix shows the mean value of these variables for dynastic and non-dynastic wards.

We also control for political changes: first, whether a block was redistricted in 2015 (many blocks were redistricted but were not moved into or out of a dynastic ward); and second, we created three binary indicators that varied over time to capture changes in racial representation. In the first, a block received a one in each year if most of its residents were Black (based on 2010 block-level Census data) and most of its ward's residents were also Black; and it received a zero otherwise. This is a time-varying indicator because after the 2015 redistricting, many blocks were redistricted into or out of a ward that matched their racial composition. We created the same indicator for Hispanic and White blocks. These variables control for important factors that may affect crime, as blocks may fare better on crime when their block's racial composition matched the ward's racial composition (Vargas 2016).

Analysis 2: Quasi-Experimental Research Design. In the second analytic step, we assess whether there is causal evidence of dynastic effects on our outcomes. To do so, we use an event study design that includes year and block fixed effects as well as pre-treatment lags and post-treatment leads to assess over-time variation in treatment effects. Specifically, for each outcome we estimate Model (B):

$$\text{Outcome}_{it} = \sum\nolimits_{t=2011}^{t=2013} \beta_t \text{Treatment}_{it} + \sum\nolimits_{t=2015}^{t=2018} \beta_t \text{Treatment}_{it} + \alpha_i + \gamma_t + X_{it} + \varepsilon_{it}$$

Following Model (A), Outcome_{it} indicates the five outcomes in block i in year t; γ_t represents year fixed effects; e_{it} represents the error term, which is again clustered at the block level; and X_{it} represents the time-varying controls that we believe may be correlated with both annexation and our outcomes of interest. Model (B) also includes α_i, which represents the block fixed effects, which control for observed and unobserved time-invariant differences across blocks (Angrist and Pischke 2009).

Finally, Treatment_{it} indicates the treatment for block i in year t. It takes the value of 1 if a block is treated (annexed into a dynastic ward) and 0 if it has not been treated. When creating the treatment variables, we merged the spatial boundaries of the 2010 Census blocks with the 2005 and 2015 ward boundaries and assigned blocks to wards if their centroid fell in that ward.

Figure 3. Quasi-experimental research design, 14th Ward example. The map shows the 14th Ward's boundaries in 2005, outlined in blue, and its new boundaries after the 2015 redistricting, outlined in red.

To illustrate this, figure 3 shows how we designated treatment groups for the 14th Ward as an example.

Our analyses include 1839 treatment blocks that were annexed in 2015 and 34,440 control blocks that were never part of a dynastic ward in the 2011–2018 period. In these analyses, we exclude any blocks that were displaced from dynastic wards and any blocks that remained within dynastic wards throughout the period.

We include the pre-treatment lags as $\sum_{t=2011}^{t=2013}\beta_t \text{Treatment}_{it}$, and the post-treatment leads as $\sum_{t=2015}^{t=2018}\beta_t \text{Treatment}_{it}$, using as baseline the year prior to treatment, 2014. We designate 2015 as the first year of treatment because Chicago's municipal elections took place on February 25, 2015, and the candidates were running for the first time within the new ward boundaries designated by the redistricting process. They were sworn into office on May 18, 2015. Because the very process of running for office, during which candidates went door-to-door and constituents learned that they were about to have a new, powerful Alderperson, could affect some of our outcomes, we consider the year 2015 as treated.

Blocks retain their treatment status from 2011 to 2018. β_t, where $t = \{2011, 2012, \ldots, 2018\}$, represents the coefficients on seven interaction terms between the treatment variable and each year of the analysis (excluding 2014). Our coefficients of interest are the interaction terms during the post-treatment period, from 2015 to 2018. We can interpret these as the effect of treatment in each year following redistricting.

To interpret $\sum_{t=2015}^{t=2018}\beta_t \text{Treatment}_{it}$ as the causal effect of annexation in each year t, we do not assume the random distribution of crime or city services prior to treatment (Levine and Gershenson 2014). Instead, our research design isolates variation within blocks and over time. The key assumption for our estimates to be unbiased is parallel trends; in other words, that treated blocks would have been on the same outcome trajectory as the control blocks, absent the treatment. Because block fixed effects control for all time-invariant factors specific to city blocks that generally lead them to receive more/less city services or experience more/less crime (Angrist and Pischke 2009), it is not a threat to our potholes estimate if certain blocks were in slightly worse condition than other blocks before redistricting.[7] But fixed effects do not control for changes over time that may be correlated with both annexation and our outcomes of interest. For example, a block's infrastructural needs are not time invariant. But for any changes in objective need for potholes to be spuriously driving our findings, those changes would have to affect treated and untreated blocks in systematically different ways, over time. This could happen if, for example, Alderpersons are able to accurately predict changes in blocks' future infrastructural needs (as opposed to their current state) and select blocks for annexation based on these predictions. We believe this is a relatively implausible scenario.

Assessing the Parallel Trends Assumption

Even so, because fixed effects cannot account for these kinds of time-varying differences across treated and untreated blocks, we take several steps to confirm the validity of our findings. First, we rely on the interaction terms during the pre-treatment period, from 2011 to 2013, to search for evidence of different outcome trends between treated and control blocks prior to treatment. If the redistricting process followed a quasi-experimental distribution, then we should observe $\beta_t = 0$ for $t = \{2011, 2012, \ldots, 2013\}$. In other words, if $\text{Outcome}_{it\{2011\}} = \text{Outcome}_{it\{2012\}} = \text{Outcome}_{it\{2013\}} = 0$, it provides evidence supporting the assumption of parallel trends.

We first evaluate evidence of pre-trends visually, by estimating the event study models using Callaway and Sant'Anna's DID package in R (Callaway and Sant'Anna 2021). Next, we formally test for the presence of pre-trends using a Wald estimator that tests a joint null hypothesis stating that all coefficients are equal to zero. If we cannot reject the null for a given outcome, it provides some evidence that the parallel trends assumption is valid for that outcome. Table A5 summarizes the Wald findings. For additional evidence, we also use a new pre-trends test developed by de Chaisemartin and D'Haultfœuille (2020), which compares the outcome evolution of the control and treatment groups from the future to the past, rather than from the past to the future, as in the Wald test. These results are shown in table A7 of the appendix. Neither analysis can prove the presence of parallel trends because that is an unobservable counterfactual; as such, our main analyses control for differing pre-trends between treatment and control groups, per Model (B) (Angrist and Pischke 2009).

Finally, we estimate the event study models in two ways: our main models shown in figure 4 are not conditional on covariates, and in the appendix we replicate those models conditioning on the time-varying demographic and political controls as described above and summarized in table A1 of the appendix. There is missingness in the median income data ($n = 6773$); as such, the analyses are not directly comparable. Even so, we find largely similar results (which also hold when we include all observations and a flag for missingness), providing further evidence that selection on observable differences between treatment and control blocks is not driving our findings.

Findings

Dynastic vs. Non-Dynastic Wards

Table 1 offers the first insight into differences between dynastic and non-dynastic wards. It reports regression results in which we regress the average number of potholes filled, homicides, assaults, robberies, and thefts on a binary variable, which takes the value of 1 if a block was part of a dynastic ward consistently over the 2011–2018 period, and 0 otherwise. The table reports only the coefficient of interest for each outcome, but per Model (A) we include year fixed effects and the full set of time-varying covariates, which control for the kinds of people who live on each block, year-by-year changes that might affect crime and city services, whether the block matched its ward's racial composition, and whether it was redistricted at all in 2015. Table A3 of the appendix shows the coefficients on all covariates.[8]

As we can see, the differences between dynastic and non-dynastic wards appear even after accounting for these other factors that likely affect crime and city service provision. Dynastic wards perform better than non-dynastic wards across all four crime outcomes: they see fewer homicides, assaults, robberies, and thefts. Dynastic wards do not, however, see more potholes covered. We consider this outcome in greater detail below.

For those outcomes where dynastic wards fare better than non-dynastic wards, the differences are substantively meaningful. Given that there are 8495 blocks that were consistently part of a dynastic ward during this period, the coefficients indicate that these blocks saw 42 fewer homicides relative to blocks in non-dynastic wards; 985 fewer assaults; 527 fewer robberies; and 3254 fewer thefts. To put these figures in perspective, during our study period the average annual crime in Chicago included 538 homicides; 18,758 assaults; 11,488 robberies; and

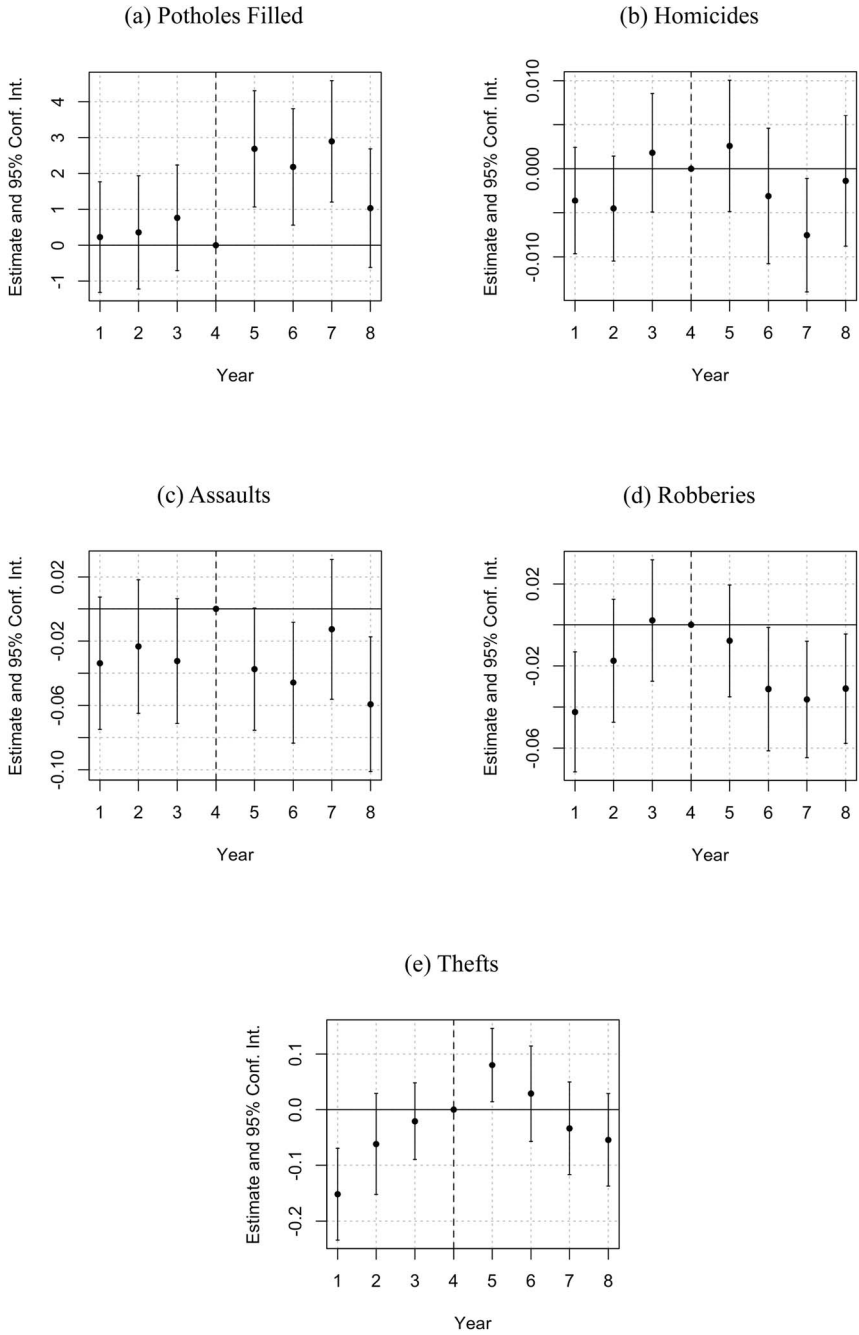**Figure 4.** Effects of annexation into dynastic wards. Each panel presents the coefficient estimates for the treatment × year interaction terms by outcome, based on Model (B). This includes year and block fixed effects. The vertical lines indicate a 95% confidence interval for those coefficient estimates, based on estimates of standard errors clustered at the block level.

Table 1. OLS Regression of Crime and City Services on Block Membership in Dynastic vs. Non-Dynastic Wards, 2011–2018

	Potholes covered (1)	Homicides (2)	Assaults (3)	Robberies (4)	Thefts (5)
Dynastic ward	0.105	−0.005***	−0.116***	−0.062***	−0.383***
	(−0.358, 0.568)	(−0.006, −0.004)	(−0.134, −0.098)	(−0.074, −0.050)	(−0.479, −0.287)
Year FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes
Observations	337,216	337,216	337,216	337,216	337,216
R^2	0.011	0.009	0.044	0.035	0.028

Note: Standard errors clustered at the block level. A block is coded as 1 if it is in a dynastic ward for the entire period from 2011 to 2018, 0 if it was not in a dynastic ward at all during the period. Blocks that were redistricted into and out of dynastic wards during the period are removed for these analyses and considered later. *$p < .05$. **$p < .01$. ***$p < .001$.

65,849 thefts. During that same period, the city received 531,472 service requests each year, on average.

In short, these analyses show that dynastic wards are a key piece of the geography of urban inequality: residents of Chicago who live on similar *kinds of blocks* that are in *different kinds of wards* experience different levels of crime.

Annexation into a Dynastic Ward

But these analyses do not yet tell us *why* this is the case. Dynastic wards may see better outcomes because of "dynastic effects," in which Alderpersons improve crime and city service provision on blocks once they are within their wards, or because Alderpersons have historically chosen to displace worse-off blocks during decennial redistricting processes (recall that these dynasties often retain office over several decades). To assess whether there is evidence of "dynastic effects," we turn to event study analyses that compare variation *within* blocks before and after annexation into dynastic wards, with blocks that remained outside of dynastic wards.[9]

Figure 4 reports the effects of a block being annexed into a dynastic ward over the five outcomes, based on estimates from the event study model, Model (B), which pre-trends as well as block and year fixed effects. Our main results do not include time-varying covariates, but we include covariates in figure A2 and reach similar conclusions. Each figure plots the coefficients on the *year × treatment* interaction terms from 2011 to 2018 for each outcome. The dotted vertical line in each panel indicates 2014, the last year of the pre-treatment period and the reference category here. The vertical lines surrounding the coefficient estimates represent 95 percent confidence intervals with standard errors clustered at the block level.[10]

Our coefficients of interest are those for the *year × treatment* term during the post-treatment period, from 2015 to 2018. Dynastic annexation has statistically significant effects across multiple years for three of our five outcomes: pothole coverings increased on annexed blocks (panel (a)), and assaults and robberies fell (panels (c) and (d)). Panel (b) shows that there is also a downward trend in homicides on annexed blocks, relative to non-annexed blocks, but the effect is only statistically significant in 2017 and disappears with the inclusion of time-varying covariates in figure A2; as such, we do not have confidence that dynastic power is producing statistically or substantively significant declines in homicides, at least in the short run.

There is also an increase in thefts (panel (e)); however, this is the only outcome for which our Wald test suggests evidence of different pre-trends between annexed and non-annexed blocks, regardless of whether we condition on covariates ($p < .01$). When we condition on covariates (as shown in table A6), we do find some evidence of pre-trends for robberies. As such, we turn to the pre-trends test developed by de Chaisemartin and D'Haultfœuille (2020) and summarized in table A7 for an additional robustness check. We again find clear evidence that pre-trends are driving theft results; in contrast, we cannot reject the null hypothesis of common trends for

homicides, assaults, robberies, and pothole coverings at $p <.05$. That said, there is some mixed evidence with respect to robberies: in support of the parallel trends assumption, table A7 shows that the coefficient for the placebo test in each of the pre-treatment years (2011–2014) is not statistically significant, and the joint placebo test across all pre-treatment years is not statistically significant at $p <.05$. But the joint test is statistically significant at $p <.10$. As such, we believe there is evidence for a negative dynastic effect on robberies, but we caution that it is also possible that pre-trends may be driving some portion of the robbery results shown in figure 4.

Taken together, these findings indicate that blocks annexed into dynastic wards receive clear material benefits. In 2015, 1839 blocks were annexed into a dynastic ward. If each of these experienced the treatment effect shown in figure 4 (coefficient estimates reported in table A5), this means that annexation into dynastic wards led to 14,274 additional potholes being covered between 2015 and 2017; 193 fewer assaults across the years 2016 and 2018; and 208 fewer robberies between 2016 and 2018. Based on the average annual incidence of crime and pothole coverings in Chicago during our period of study, 1839 blocks would typically experience 21,553 pothole coverings, 746 assaults, and 457 robberies in a year. In other words, these effects make for a meaningful difference for residents living on those blocks.

Potential Mechanisms for Dynastic Effects

In this section, we explore evidence regarding the mechanisms that produce dynastic effects. As noted above, the political economy literature indicates that powerful urban elites can lower neighborhood crime in the short run by (1) gaining control over limited public resources and funneling them to their districts, which (2) improves local infrastructure and (3) lowers crime. Over the medium to long run, (1) and (2) may lead to (4) increased collective efficacy, which further lowers crime. This renders three testable predictions about mechanisms. First, per mechanism (2), we have already shown that declines in block-level crime due to dynastic power coincide with improvements in block-level infrastructure. This suggests that infrastructural improvements are both an outcome of dynastic power (i.e. they are a public good in their own right), as well as a mechanism by which dynastic power improves crime.

Second, we evaluate mechanism (1) by testing whether Chicago's dynastic politicians are using committee chair positions to improve outcomes within their wards. To do so, we replicate our main analyses using a treatment that takes on a value of 1 in 2015–2018 if a block was part of a ward whose Alderperson held a committee chair position during those years and had not been a part of those wards in prior years, 0 if the block did not have an Alder in a committee chair position at all during the 2011–2018 period.[11] The results are shown in figure 5. Figure A3 replicates these results conditioning on time-varying covariates and shows nearly identical results.

Compared to the effect of dynastic annexation, we see that gaining an Alder with a committee chair has very similar effects on robberies (panel (d)), which decline for several years after annexation. But figure 5 shows no evidence that gaining a committee chair leads to an increase in pothole coverings or a decline in assaults. It does, however, appear to decrease thefts (particularly in 2018), which we did not observe for annexation into dynastic wards. These findings suggest that committee chair positions are not just an outcome of dynastic power (in that dynastic politicians disproportionately benefit from their resources), but a mechanism by which dynastic politicians funnel resources into their wards and improve crime. They are not, however, the only mechanism by which dynastic politicians generate the effects we observe in figure 4. Dynastic politicians have other relational advantages through which they can benefit their wards, as described above, which we are unable to measure directly.

Finally, mechanism (4) suggests that dynastic power might—particularly over the medium-to-long run—increase collective efficacy and civic engagement, further lowering crime. We attempt to measure this indirectly, using service request data from the Chicago City Data Portal from 2011 to 2018. Using service request data as a measure of civic or political engagement has several pitfalls: aggregate counts of service requests do not allow for straightforward interpretation as to why requests are rising or falling on a particular block. White and Trump (2018), for example,
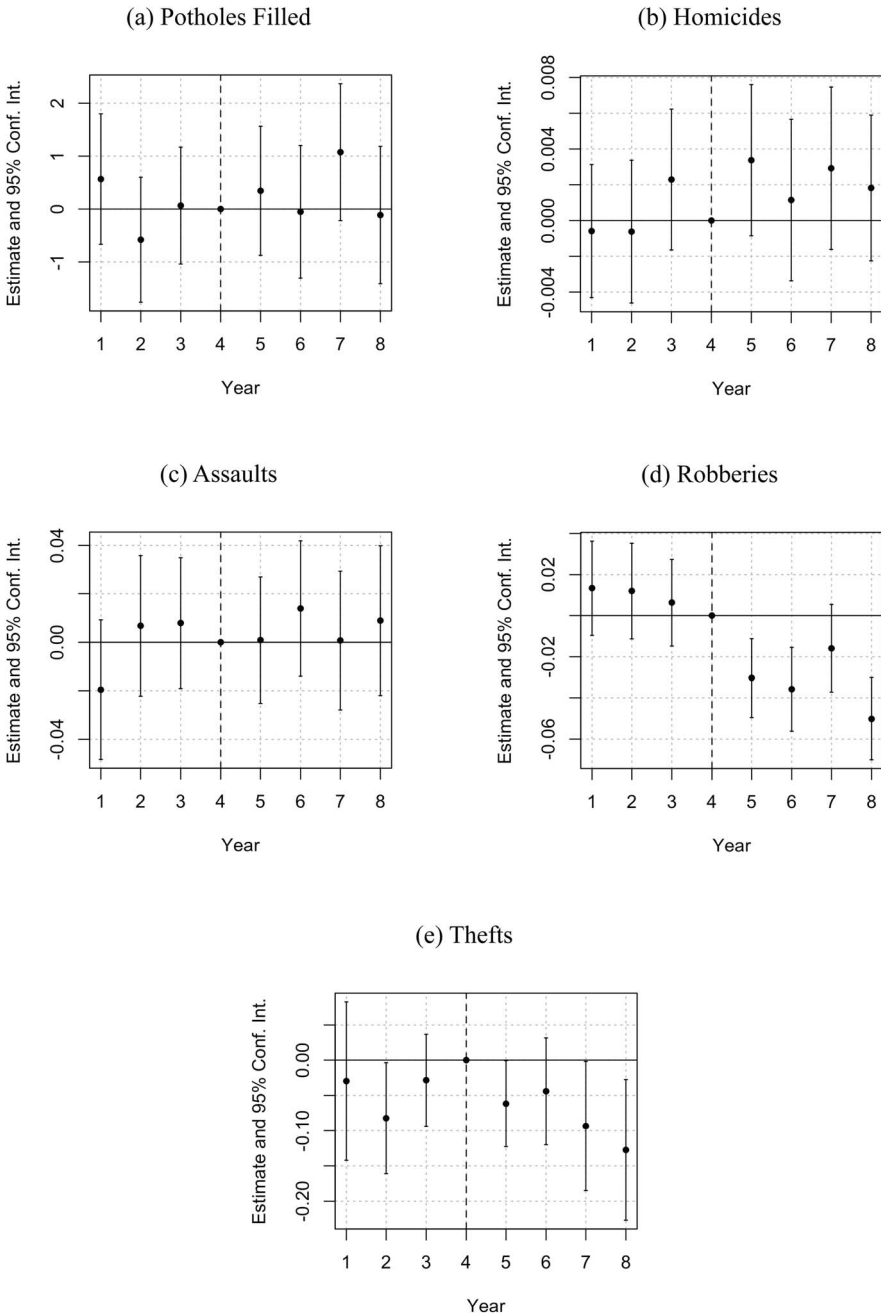
(a) Potholes Filled

(b) Homicides

(c) Assaults

(d) Robberies

(e) Thefts

Figure 5. Effects of gaining an Alderperson with a committee chair position. Each panel presents the coefficient estimates for the treatment × year interaction terms by outcome, based on Model (B). This includes year and block fixed effects. The vertical lines indicate a 95% confidence interval for those coefficient estimates, based on estimates of standard errors clustered at the block level.

Table 2. Mechanisms—Service Requests

	Service requests (1)	Service requests, no super-callers (2)	Service requests, duplicates (3)
Dynasty effect 2015	0.133 (−0.374, 0.640)	−0.092 (−0.374, 0.191)	−0.062 (−0.345, 0.222)
Dynasty effect 2016	0.816** (0.244, 1.387)	0.361* (0.066, 0.656)	0.122 (−0.196, 0.440)
Dynasty effect 2017	0.354 (−0.193, 0.901)	0.356* (0.041, 0.671)	−0.025 (−0.315, 0.265)
Dynasty effect 2018	0.383 (−0.162, 0.928)	0.287 (−0.019, 0.592)	−0.037 (−0.340, 0.265)
Year and block FEs	Yes	Yes	Yes
Time-varying covariates	Yes	Yes	Yes
Observations	284,933	284,933	284,933
R^2	0.706	0.747	0.409

Note: Standard errors clustered at the block level. $*p < .05.$ $**p < .01.$ $***p < .001.$

note that individual "super-users" may disproportionately be driving aggregate counts. To conduct the most robust analysis possible, we examined three different measures of service requests: all requests; all requests, excluding those made from the same address within the same year, regardless of the issue; all requests, including those labeled by the city of Chicago as "duplicates"—those that refer to the same issue in the same vicinity within a certain window of time. The second measure is the best way to exclude super-users given the limitations of Chicago's service request data. The third measure indicates whether, conditional on some resident noticing a problem on a block, other residents also report it.

We replicated our main analyses using all three service request measures as outcomes, including time-varying covariates. As we can see in table 2 below, the measure including all service requests rises in the year after dynastic annexation; the measure of service requests excluding super-callers rises in the 2 years following dynastic annexation; and annexation has no effect on service requests including duplicates.

We interpret these results with caution, not only because of the challenges inherent in using service request data as a measure of civic engagement, but also because Wald tests indicate pre-trends for the first two measures; as such, our findings are only conditional on the inclusion of pre-trends and time-varying covariates. For this reason, we believe these findings provide suggestive evidence in support of the idea that, even in the short run, dynastic annexation may make residents more willing to contact the city, in part because they may expect less discrimination and a higher quality of service once they learn they have gained a dynastic Alderperson (Lerman and Weaver 2014; Levine and Gershenson 2014).[12] As we discuss further below, it is possible that changes in civic and political engagement emerge over longer time horizons as well, thus further fostering collective efficacy and lowering crime.

From Political Inequality to Spatial Inequality

Thus far, the evidence of dynastic effects suggests that dynastic politicians can improve outcomes within their wards. But if they are achieving these outcomes for relatively disadvantaged blocks, dynastic power would serve to decrease spatial inequality within Chicago. In contrast, if they are concentrating advantages on blocks that are already relatively well-off, dynastic power would exacerbate inequalities. The lack of pre-trends for most outcomes shown in figure 4 and table A5, as well as the similarities between the event study results with and without time-varying covariates, all suggest that dynastic politicians did not annex blocks that were observably different from control blocks; this is what gives us confidence in our causal estimates of a dynastic effect. That said, dynastic politicians may still have systematically chosen to displace blocks that were observably faring worse on crime metrics than those they annexed—this would actually be easier for politicians who are already familiar with the challenges within their existing ward

Table 3. Difference in Means between Blocks Subject to Dynastic Annexation vs. Displacement

	Annexed	Displaced	Difference *(displaced—annexed)*
Avg no. homicides, 2011–2014	0.009	0.008	−0.001
Avg no. assaults, 2011–2014	0.32	0.35	0.03
Avg no. robberies, 2011–2014s	0.19	0.27	0.08***
Avg no. thefts, 2011–2014	0.91	1.28	0.37**
Avg no. pothole coverings, 2011–2014	11.0	13.0	2.0***
Average % Black residents, 2010	21	24	3.0**
Average % White residents, 2010	37	18	−19**
Average % Hispanic residents, 2010	27	42	15**
Average unemployment rate of pop, 2011–2014 > 18	8.9	9.0	0.1
Average % w/ college degree, 2011–2014	17.0	16.0	−1.0***
Median household income, 2011–2014	52,357	44,234	−8123***

Note: We assess whether the difference in group means is statistically significant using a Welch two-sample *t*-test. Data for crime and pothole coverings are block-level averages from 2011 to 2014; for unemployment, education, and income, they are block-group averages from 2011 to 2014; for race, they are block level from just the year 2010. $^*p < .05$. $^{**}p < .01$. $^{***}p < .001$.

boundaries, and this process would not be incorporated into our pre-trends analyses because we exclude blocks that were displaced from dynastic wards in our event study models.

Table 3 assesses this possibility, comparing the characteristics of blocks that experienced dynastic annexation and those that were displaced from dynastic wards across the same block-level outcomes we show in figure 4, as well as relevant demographic characteristics for the 2011–2014, pre-treatment period. Because we do not have block-level data after 2011, or for unemployment, college education, and income in any years, we use block-level racial characteristics from 2010, and block-group-level data for the socioeconomic indicators. We use block-level counts for all crimes and pothole coverings.

As table 3 shows, the blocks and block groups displaced from dynastic wards are, relative to those that are annexed, Blacker and more Hispanic, and have slightly lower rates of college education and substantially lower median household incomes. Although some of these differences are substantively small (as with education), they are all statistically significant at $p < .01$. Importantly, displaced blocks also experienced more assaults (although only at $p < .10$), robberies, and thefts than annexed blocks. Although they also had more pothole coverings, given the lack of controls here, this may be due to the fact that there were more potholes on displaced blocks.

Taken together, figure 4 and table 3 provide evidence that Chicago's dynastic politicians are exacerbating spatial inequalities by displacing blocks with relatively high levels of crime, and less racial and class privilege, than those they annex, thus depriving less-advantaged blocks of the benefits of dynastic power shown in figure 4.

Conclusion

The preceding analyses confirm that dynastic wards are a piece of the uneven geography of crime and service provision within cities. Where scholars such as Vélez and Lyons (2014) envision powerful elites connecting disadvantaged neighborhoods to resources—thereby reducing spatial inequalities within cities—we instead find evidence to support the opposite conclusion: dynastic politicians displace territories that have worse crime outcomes (and are also poorer and have more Black and Hispanic residents) than those they annex during redistricting (table 3). When they lower crime and improve city services on annexed blocks (figure 4), they are *concentrating advantage* within certain, relatively well-off areas and withholding those benefits from other areas of the city. In short, dynastic politicians are exacerbating spatial inequalities within Chicago.

But dynastic politicians are not all-powerful: we find little evidence of short-run effects on homicides. Alternatively, this might be due to the fact that homicide tends to be responsive to sustained forms of community intervention that go beyond what elected officials can directly control (Grunwald and Papachristos 2017). Similarly, we may be underestimating the effect of dynastic power on robberies and assaults because short-run infrastructural improvements may have long-run impacts on crime as collective efficacy grows. This suggests that some of the differences between dynastic and non-dynastic wards shown in table 1 are the result of longer-term consequences of dynastic power.

These findings make several contributions. First, our study builds on insights from the urban governance literature to advance research on the political construction of urban inequality. Despite critiques launched at the Chicago School of Urban Sociology for its inattention to politics, Chicago urbanists from Park and Burgess (1925) to Janowitz (1984) and Suttles (1972) wrote about the importance of politics for urban conditions, they just never empirically examined the political (Sampson 2011). Even as political economy approaches have shown that neighborhood ties to resource brokers play a key role in shaping urban spatial inequality, these studies have come up against two challenges: isolating the causal effect of politics over urban outcomes (Vélez and Lyons 2014) and developing an approach to studying urban political power in the context of rising technocratic forms of governance (da Cruz, Rode, and McQuarrie 2019).

This paper helps the field advance beyond these challenges, offering political dynasties as a way to conceptualize how certain forms of political power have persisted amidst shifting urban governance arrangements, and then empirically identifying a causal effect of dynastic power on ecological outcomes such as crime and city service provision.

This highlights our second contribution: illuminating a broader theoretical puzzle about how political power continues to affect urban outcomes amidst cities' turn toward impartial technocratic urban governance. As da Cruz, Rode, and McQuarrie (2019, p. 6) have recently argued, cities' turn toward nonprofit partnerships and technocratic governance "tell us very little about the power structures arising or evolving from these technological developments." Our findings begin to address this gap.

Third, to our knowledge this is the first study that introduces a way to measure the effects of political dynasties on social outcomes in the United States. While Chicago is not unique in having several dynastic politicians in its legislature (Dal Bó, Dal Bó, and Snyder 2009), Chicago politics is somewhat distinct among major US cities in the post-machine era. As such, we expect that political dynasties are most likely to affect spatial inequality in US cities and states where the legacy of single party rule and quid pro quo remain strong. In such cases, social scientists may find it fruitful to examine how dynastic power shapes social inequalities.

And finally, these findings offer a complicated picture about the intersection of racial politics and political power and their role in shaping urban spatial inequalities Many of the dynastic wards in Chicago are majority-Black with Black Alderpersons; and yet, per table 3, dynastic politicians on average annexed Whiter blocks than those they displaced during the 2015 redistricting. Moreover, tables A3 and A6 provide suggestive evidence that majority-White blocks in majority-White wards tended to fare better on certain crime outcomes and worse on pothole coverings, while the reverse is true for majority-Black blocks in majority-Black wards. Majority-Hispanic blocks in majority-Hispanic wards, according to table A3, struggle on nearly all dimensions, except thefts. And yet, dynastic wards (including the several majority-Black wards) do fare better across our outcomes than non-dynastic wards. In short, it is not the case that the benefits of dynastic power are neatly divided along racial lines, but there is evidence (although not causally identified) that only politically powerful racial minority politicians are able to improve outcomes within their wards.

These results suggest that ecological measures of race may be inadequate for fully capturing the relationship between race, dynastic power, and inequality. Fully unpacking the role of race in political dynasties requires a deeper dive into the policymaking and electoral processes. Studying these processes would also help identify additional structures (intersecting with race), which may

help explain the working of dynasties across cities and even countries. In Chicago, the intersection of race, class, and machine politics have historically been especially relevant. In other cities, it may be race and religion.

Our research design also has several limitations in identifying the effects of dynastic power on crime and city services. As noted above, some dynastic effects might take longer to emerge than we can observe in our 4-year post-treatment window. Moreover, our use of aggregate data—while essential for isolating causal effects—leaves us unable to assess the effects of political dynasties at the individual level. These limitations raise questions for future research about what happens on the ground in the months following redistricting: How do grassroots organizers respond to the new opportunities and challenges presented by being incorporated into, or kicked out of, a powerful ward? How do powerful politicians operate with and through nonprofits to shape crime and city services in their wards? While we cannot answer these questions with our data, we hope our findings motivate urban ethnographers to pay deeper attention to elected officials and intergenerational political processes to unpack the micro–macro links between political structures and urban inequality.

Endnotes

1. Even where redistricting takes place within the legislature, as in Chicago, politicians are constrained: electoral maps are subject to review by state and federal courts, and politicians might also fear public backlash against distorted maps (Cain 2012).
2. Four additional alders (of the 7th, 11th, 24th, 28th Wards) also had a prior relative in Chicago/Illinois politics, but they were in office for only part of the 2011–2018 period. In our main analyses, we include blocks that were part of those wards as control units; in figure A4 of the appendix, we exclude them entirely and find similar results.
3. See note for figure 2 on data sources.
4. See: https://www.chicago.gov/city/en/narr/foia/CityData.html.
5. A 2010 change in how the city collected and reported service request data led to an upsurge in recorded requests. We chose to begin the study in 2011 to limit the possibility of drawing incorrect inferences based on changes to the data management system. See https://www.chicago.gov/city/en/depts/311/supp_info/311hist.htm.
6. Because there are several hundred vacant block-groups, we code their demographic characteristics as 0% and also include a dummy variable to indicate that they are vacant.
7. As White and Trump (2018) note, city services data should not be used in the absence of "high-level controls."
8. Table A2 reports the raw descriptive statistics.
9. Figure A1 plots the outcome variables over time, by treatment status.
10. Table A4 of the appendix shows the two-way fixed effect estimates without conditioning on covariates.
11. This treatment is not assigned solely through the redistricting process, as the Mayor doles out assignments after each election, within the constraints described above. As such, several blocks gained an Alderperson with a committee chair in 2015 not just because of redistricting but because that Alderperson was granted new power. As with figure 4, we leave out blocks that lost an Alder with a committee chair and those that always had a committee chair during this period. We include the 39th Ward as having a committee chair position through 2014 and the 23rd Ward as having a committee chair position through 2018, although each of those years were partial years of service.
12. An alternative explanation is that, on annexed blocks, conditions are deteriorating more than conditions on control blocks, leading residents to call on the city more. Although this is possible, this seems relatively less likely.

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Supplementary Material

Supplementary material is available at *Social Forces* online.

Data availability

The data and replication code underlying this article will be made available in an online repository which will be accessible via a DOI link.

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