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Longer trips to court cause evictions

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Studying ~200,000 evictions filed against ~300,000 Philadelphians from 2005 to 2021, we focus on the role of transit to court in preventing tenants from asserting their rights. In this period, nearly 40% of tenants facing eviction were ordered to leave their residences because they did not show up to contest cases against them and received a default judgment. Controlling for a variety of potential confounds at the tenant and landlord level, we find that residents of private tenancies with longer transit travel time to the courthouse were more likely to default. A 1-h increase in estimated travel time increases the probability of default by between 3.8% and 8.6% points across different model specifications. The effect holds after adjusting for direct distance to the court, unobserved landlord characteristics, and even baseline weekend travel time. However, it is absent in public housing evictions, where timing rules are significantly laxer, and during the COVID-19 pandemic, when tenants had the opportunity to be present virtually. We estimate that had all tenants been equally able to get to the court in 10 min, there would have been 4,000 to 9,000 fewer default evictions over the sample period. We replicate this commuting effect in another dataset of over 800,000 evictions from Harris County, Texas. These results open up a new way to study the physical determinants of access to justice, illustrating that the location and accessibility of a courthouse can affect individual case outcomes. We suggest that increased use of video technology in court may reduce barriers to justice.

eviction | access to justice | housing | transit | mapping

Significance

Recent disruptions have brought a surge of interest in the determinants of access to justice as the downstream effects of legal outcomes like eviction and criminal adjudication become more apparent. We demonstrate the connection between physical barriers to justice and individual case outcomes in a 15-y sample of Philadelphia eviction outcomes. Tenants who must travel longer to get to court are more likely to fail to show up and contest their evictions—they take default judgments. By contrast, during the COVID-19 pandemic, when tenants had an option to Zoom, and in public housing evictions, where tenants have more robust procedural protections, this transit effect disappears. We recommend that courts permit more flexible and remote scheduling to achieve justice.

As the first paragraph of Philadelphia's official tenant guidance document warns, those subject to eviction must appear at the Landlord Tenant Court downtown by 8:45 AM. sharp on their hearing day. Or else:

> If you are late or fail to appear, a default judgment will be entered against you. The court will send you a notice that a default judgment has been entered against you. You may file a petition to open the default judgment at 1339 Chestnut Street, Room 1000. You must have a good reason for missing or being late for the trial, must file the petition promptly after learning of the default judgment, and must have a valid, meritorious claim or defense.*

The default judgments that result from failing to show up—or just as often, appearing late—to eviction court in Philadelphia and elsewhere should matter to policymakers. Scholars increasingly have come to conclude that evictions not only reflect but in fact cause social pathologies, making understanding one of their major drivers an urgent concern. Defaults are notoriously difficult to reopen, even if the tenant simply missed her bus and arrived late. And, though policymakers have poured resources into eviction diversion programs, tenants who do not show up cannot take advantage of the newly robust "Civil Gideon" protections offered in major urban areas.

It is thus surprising that although policymakers describe defaults as a part of the eviction crisis (2), we lack information about their incidence across jurisdictions. Many localities do not gather default data or categorize it in inconsistent ways. Reported default rates vary enormously between cities, with some places (Chicago) having few defaults and others (as in Texas) reporting defaults in about half of all filed cases.† Defaults also vary over time. In Maricopa County, Arizona, authorities reported that default rates (normally between 20% and 30%) have recently declined to just 12%. As the Clerk of Court wrote us: "During the pandemic we're holding almost all cases remotely

*See ref. 1. As we describe in the text below, almost all show-up times historically were at 8:45, but a distinct minority were at 12:45 PM. During the Covid-19 eviction moratorium, these timing rules changed.

†Despite contacting court clerks and examining public dockets, we could only imprecisely estimate default rates at select large urban jurisdictions: New York (~25%), Chicago (~8 to 24%), Houston (~40%), and San Antonio (~55%). For others, like Los Angeles, Dallas, and San Diego, no good data exist. Defaults in smaller communities can be significantly lower—well under 10% (3).

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and scheduling only 25 cases per hour. That allows people to simply call in rather than take a whole day off work and have to wait for their case to be called. It's a simple matter now to call in and have the case be done in less than an hour."

But these anecdotes shed dim light. Basic questions remain, including the following: How many defaults are there, really? Are the tenants who fail to show up worse off? Why do tenants fail to show up? And, given the shock of Zoom justice wrought by COVID-19 in eviction court, did making justice remotely accessible matter to outcomes?

Studying 223,862 eviction proceedings from 2005 to 2021 from Philadelphia's landlord tenant court, we provide a large-scale account of default rates in eviction court across time in a large urban center. We find that defaults are common though in decline, from almost half of unsubsidized housing cases in 2005 to a bit over 30% today. Missed opportunities to contest evictions are sticky as petitions to reopen defaults are rarely filed and infrequently granted.

Controlling for census tract characteristics and residence type, we find that excess public transit commuting time increases default rates. This effect holds when comparing properties owned by the same landlord, when controlling for direct distance to the courthouse, and even when controlling for commuting time as measured on a weekend. As a placebo test, when tenants are offered Zoom hearings during the COVID-19 pandemic, the commuting time effect disappears. The result is also absent for tenants in public housing, whose eviction processes and substantive rules are more tenant-protective. Finally, we replicate the transit effect in a large dataset of cases from Harris County, Texas, where the presence of multiple courthouses allows us to directly compare default rates among tenants of the same building that happen to be assigned to different court locations.

We estimate that for every 10 min in additional transit commuting time, tenants are between .65% and 1.4% points more likely to default depending on model specification. This small percentage has a cumulative impact. In our sample, had tenants been afforded an equally short trip of a maximum of 10 min to the courthouse, Philadelphians would have suffered between 4,125 and 9,246 fewer default evictions. In our supplementary analysis of data from Harris County, Texas (discussed in *SI Appendix*, section 9), the effect of a 10-min increase in driving commute time is estimated to be about three times as large.

These results contribute to four related literatures.

First, many scholars have focused on the structural legal barriers that prevent disadvantaged people from flourishing. Redlining, undersupply of polling places, pretextual traffic stops, excessive bail, and extractive fines are but a few examples of many that mark the legal system's subordination of the poor, particularly members of minority groups (4). To date, that work has not empirically demonstrated that structural factors—like where the courtroom is located in a city and the efficiency of the transit network—can directly impact individual legal case outcomes.[‡] Our contribution here is to identify a way through which transit affects legal outcomes: by causing citizens to be evicted. That said, there is work on distributive effects of transit time on residents outside of the legal context. Previous research has shown adverse effects of transportation challenges on a variety of outcomes related to access to health care (6–8), employment (9), and voter turnout (10).

Second, scholars have asked how justice operates in lawyerless courts. A particular concern is citizens who do not show up to the court (11, 12). For some types of judgments, like debt collection, more than 90% of defendants do not show up to contest claims, even though many may have had meritorious defenses (13). We are aware of only one large-N study of defaults in eviction court, a paper from 2006 studying around 700 cases, finding (primarily) that defaults were more likely in cases alleging a failure to pay rent (14). Other papers have noted the absence of large scale evidence on the determinants of default (15).

Third, we contribute to the literature on the effects of housing instability on various social ills, including crime (16), preterm birth (17), and other measures of health (18), lack of civic engagement (19), as well as the spread of COVID-19 (20).

Many existing studies in this literature, unfortunately, are difficult to interpret as causal since the determinants of eviction (e.g., poverty, personal instability, and loss of a job) are socially pathologic. That is, crime may cause evictions (21), even as evictions cause crime. Or, having children may cause evictions but might also affect the downstream outcome of interest either directly or through a mediating variable like a reduction in net income (22). Recent work has attempted to address endogeneity problems through more credible identification strategies, such as leveraging the random assignment of judges with varying levels of "severity" in eviction courts as an instrument for eviction (23). In Philadelphia, unfortunately, judge assignment cannot be reliably observed from docket records.[§] Additionally, considering how few cases in eviction court involve actual adjudication, the amount of variation in severity across judges may be minimal for many jurisdictions and only weakly predict eviction outcomes.

We show how eviction can potentially be untangled from its confounds by identifying a mechanism that influences outcomes in eviction court but is only weakly related to many of these common socioeconomic factors: commuting time. We thus illustrate a possible instrument for researchers to use in estimating the downstream effects of eviction. Transit could also be a hidden cause of outcomes in other legal proceedings where showing up at a particular time determines the exercise of substantive rights. This would include criminal and immigration proceedings as well as a wide variety of low-stakes civil claims.

Finally, we add to a nascent line of research on the effect of remote hearings on legal outcomes. Prior to the COVID-19 pandemic, scholars had written widely on the advantages and disadvantages of limited experiments in remote access to justice (24–26). That scholarship has suggested that remote access might make it easier for less-resourced defendants to access court proceedings but that decision-makers tend to be less sympathetic to remote participants (27–30). The recent large-scale adoption of remote hearings heightened the stakes of this trade-off but suggested that the introduction of online access could significantly reduce both default rates and disparities based on race, gender, and income (31–34). If remote justice reduces barriers, it would fit alongside another interventions—like court-prompting text messages, calls, and postcards (35–37)—in the repertoire of policymakers.

Institutional Background

There is a small but growing literature about state courts adjudicating low-value claims, where most defendants lack lawyers (3). Mass adjudication is how most citizens experience civil lawsuits, though it bears little to no relationship to the highly

[‡]A recent related field experiment failed to find that transit subsidies decreased the rate at which defendants failed to appear for low-level civil and criminal offenses (5).

[§]The docket records a judge's name for each eviction. However, we found that these names do not reliably indicate the actual judge sitting in the courtroom on the day that the case was heard.

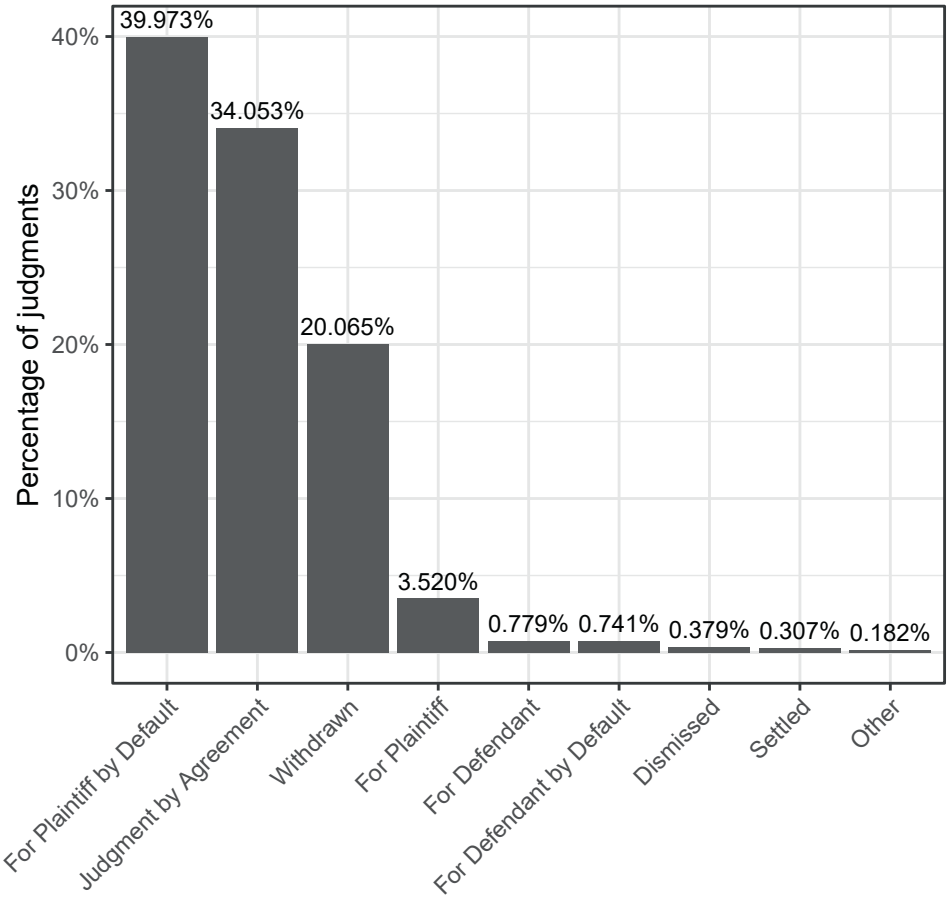

Fig. 1. Distribution of outcomes in Philadelphia Landlord-Tenant court: 2005 to 2021. 223,840 eviction proceedings, 283,812 defendants.

formalized and legalized courtroom culture depicted on TV. One emphasis in that research is the importance of local legal cultures, rules, and procedures in meaningfully evaluating outcomes. As Mona Lynch explained:

> [H]ow criminal and penal law as practiced is significantly shaped by the local (and locale) such that, although law on the books might lead us to expect some homogenization of outcomes within state and federal jurisdictions, law in action indicates much more microlevel variation shaped by local norms and culture related to how the business of criminal justice happens in any given place (38).

We focus on Philadelphia's eviction system.[¶] In previous work, we extracted approximately 170,000 leases from eviction proceedings and studied their terms (40). Here, we return to the underlying eviction dockets. Philadelphia's formal eviction process may begin with a "Notice to Quit"—a letter from the landlord to the tenant directing the latter to vacate. That notice is required unless waived by contract. We previously found that approximately 70% of all unsubsidized leases in Philadelphia waived the notice to quit.

Next, the landlord will file a complaint in municipal court, a lower-level trial court, and receive a date and time for the proposed eviction hearing. Landlords must serve the tenant with notice of the eviction in writing (and the court will also mail the tenant a copy of the eviction complaint). The complaint and its attachments are public documents.

After filing the complaint, the court sets a day and time for the hearing. During the period we studied, there were two primary times during weekdays at which the court calendared each eviction: 8:45 AM. and 12:45 PM. The morning times were far more common. Of the 210,074 pre-COVID nonpublic housing eviction proceedings in our data, 144,849 were scheduled for 8:45 AM, and 60,769 were scheduled for 12:45 PM. The tiny remainders are typically scheduled for 30 min after either of these two times. The assignment time is not random: We find a positive relationship between the total number of cases in which a landlord attorney appears in the data and the share of those cases scheduled for 8:45 AM. For example, although about 69% of cases are scheduled for 8:45 AM on average, the most common landlord attorney in our data has 84% of their cases scheduled for that time. Frequent filers cluster in particular times, perhaps because the court fills up time slots in bins and seeks to maximize the efficiency of the process (so that one attorney can present multiple cases in a row).

If tenants do not arrive by their assigned time, the court staff will mark them as having defaulted. An eviction judgment will then issue unless the tenant successfully files a notice to reopen the default. Notices to reopen require a showing of good cause—both a meritorious defense and a good excuse. The prevailing wisdom among advocates is that default judgments are very hard to reopen (41), and we find that this is indeed the case from the eviction docket data (*SI Appendix*, section 3). After default, if a tenant does not voluntarily vacate the property, the

[¶] For previous, though now dated, qualitative research, see ref. 39.

landlord must undertake two additional legal steps to secure a court order which it can us to enlist enforcement officers to lock the tenant out of the property, resulting in a delay of at least 21 d.

Materials and Methods

Data. From Philadelphia Legal Assistance (PLA), we obtained 339,172 eviction dockets involving residential properties from January 2005 to July 2021. Each docket entry contains a set of covariates related to the property and the eviction proceedings such as tenant and landlord names and the address of the property. Addresses entered into the court docket website were imperfect, containing typographical errors or ambiguous locations. PLA attempted to geocode as many of these locations as possible given the address data. We filtered this dataset to only those properties where we could unambiguously parse an address number and street name from the text of the listed address and obtain, from the Google Maps API, a correctly matching address with a latitude and longitude. We provide additional detail for this process and discuss the representativeness of our remaining sample in *SI Appendix*, section 1.

Our primary confounders of concern are the socioeconomic characteristics of different neighborhoods in Philadelphia. If lower-income or predominantly minority segments of the city have longer commute times by virtue of their geographic location and also have higher default rates, we may see a spurious relationship between commuting time and default if we do not account for these factors. We also are concerned about potential unobserved confounding driven by variation in landlords across the city.

For the demographic covariates, we identified the census tract and block for each of our properties. Median income and median contract rent were obtained from the 2015 American Community Survey. Because racial demographic data are available at a finer-grained level, we measure the percentage of Black, White, and Hispanic residents using the 2010 census' block-level data. To obtain data on landlords, we rely on a database of Philadelphia's landlords that we obtained via an agreement with the Pew Charitable Trust. Because the docket often lists only the filing LLC as the plaintiff, and since individual landlords may own multiple properties through different LLCs, we would be unable to identify common landlords across eviction proceedings without additional data. Pew Charitable Trust used Philadelphia administrative data to match roughly 55,000 landlords to 136,000 rental properties (42).

To generate our primary independent variable of interest—commuting time to the Municipal Courthouse—we queried the Google Maps Distance Matrix API to determine the estimated distance and travel time between each building in our dataset and the Philadelphia Municipal Court.[#] We measured the distance and time it would take to reach the courthouse via public transit on a weekday—when hearings are scheduled—and on the weekend.[‖] Because historical transit data are not available on the Google Maps API, our measure only varies across space and captures the general ease or difficulty of commuting downtown. We queried transit times for a fixed day (Wednesday, May 11 for weekday and Sunday, May 15 for weekend) and time (1:00 PM).

For each case, we extracted the outcome of the proceedings from the docket. Because eviction proceedings are often filed against multiple individuals and result in different outcomes—as is the case where one tenant defaults while the other does not—we generated the case outcome data at the individual level. After all of our preprocessing, our primary dataset consists of a total of 223,840 eviction proceedings across 61,014 unique buildings and 283,812 unique named defendants. A total of 280,143 of our defendants' filings precede the COVID-19 pandemic, and about 40% of these defendants defaulted. We focus our primary analysis on these pre-COVID cases as eviction proceedings during the pandemic changed significantly, as we describe below.

The plurality of our judgments favor the landlord (plaintiff) and nearly all are defaults. The second most common outcome is a negotiated judgment by agreement—a settlement. Recent research, analyzing a sample of these settlements in Massachusetts, found that 1/3 led to the tenant leaving the property (often after a grace period), and 2/3 led to a set of conditions for repayment of rent due, enforceable by a later summary eviction action (47). The third most common outcome is a withdrawal of the case by the landlord. Fewer than 2% of judgments are tenant wins. Most evictions are filed by a small fraction of landlords. Among private evicting landlords, the median number of filings is just 2, but the most frequent filer brought 3,412 cases. Of 25,855 landlords, the top 10 alone account for 10% of all eviction filings.

Fig. 2 displays the trends in default judgments, plotting separate trends for cases involving public housing and nonpublic housing cases. Default rates vary over time and space, and generally decline between January 1, 2005, and February 28, 2020. It shows that during the early period of our data (around 2005 to 2010), default judgments comprised over 40% of all case outcomes for nonpublic housing cases. That number has dropped steadily over time. This drop coincided with the imposition of new landlord regulations, which increased the costs of filing frivolous evictions.[**] Notably, only 20 to 25% of public housing evictions are defaults, and there is no similar pattern of steady decline. Because those evictions are so distinctive, for our primary analysis, we focus on non-PHA cases.

Empirical Approach. Our quantity of interest is the average effect of a unit increase in weekday transit commuting time to the Philadelphia Municipal Courthouse on the probability that a tenant defaults. We use a selection-on-observables strategy with an extensive set of controls to identify this effect. We leverage the fact that Philadelphia's public transit system developed in a haphazard, politically charged, and contingent manner, resulting in a system with only two primary subway lines running east–west and north–south (bisecting near the courthouse at City Hall) operating alongside a variety of bus routes that service the other parts of the city (48). As a consequence, properties located in neighborhoods with similar socioeconomic characteristics may nevertheless have wildly different commuting times due to the peculiar layout of the transit network.

Our first and simplest design assumes that commuting time is as good as randomly assigned conditional on a set of covariates

[#] See ref. 43 for a discussion of the advantages of the Maps API for estimating travel times and refs. 44–46 for other work that uses this methodology.

[‖] We also obtained estimates of travel time by driving. Driving and transit commute times correlate quite highly ($r = .749$) but diverge significantly for certain regions.

[**] In 2011, a law (originally passed in 2006 but subject to court challenges) required that eviction filing be accompanied by certain documents, including a "Certificate of Rental Suitability," to be issued if the landlord has a property clear of code and tax violations, had secured a valid housing inspection license, and averred that the property is fit for habitation. The court was not supposed to issue a default without a CRS certificate, but it was underenforced. Beginning in 2012, landlords who owned properties built before 1978 and who knew that they had tenants with children under the age of 6 y on the property were to obtain certificates certifying that they had made efforts to make the property safe from lead poisoning. Again, enforcement of that rule was weak. Starting in February, 2017, the city required lead safe certification as a condition of obtaining a CRS and thus a default judgment. Compliance until 2020 involved self-reporting.

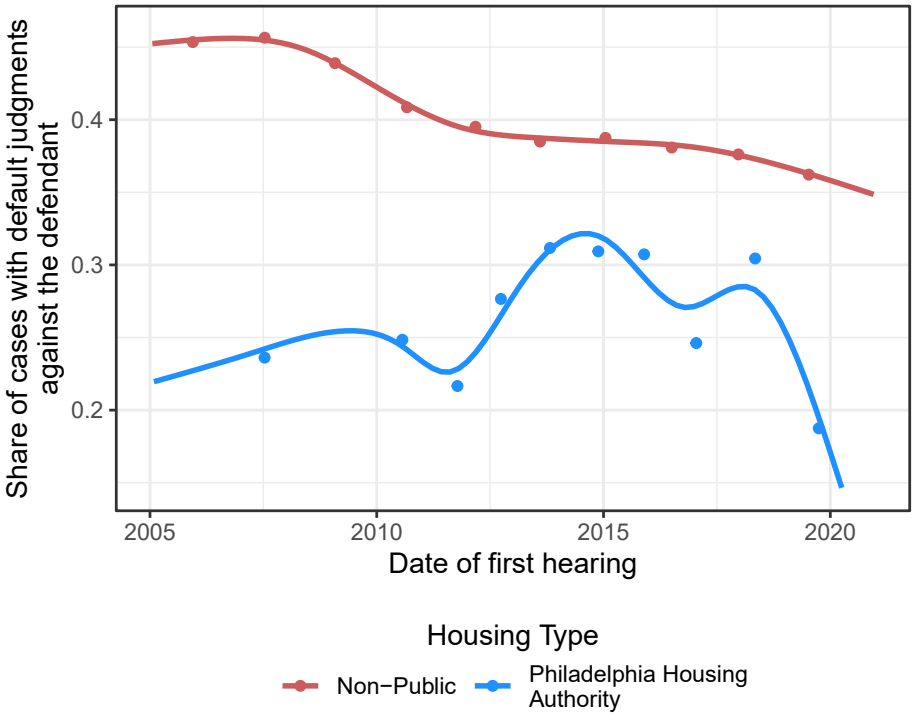

Fig. 2. Default rates over time. Eviction cases filed prior to the COVID-19 pandemic. N = 280,143 individuals; 220,964 eviction cases (210,052 nonpublic housing); plots are smooth regression splines. Points denote binned averages.

and the month and year of the hearing. The covariates include variables that are likely to be correlated with general proximity to downtown Philadelphia and that would also be predictive of default. Three highly salient covariates—income, racial demographics, and rent—are discussed in greater detail in *SI Appendix*, section 4.

To estimate the effect of a change in commuting time, we fit the following linear probability model:

$$E[Y_{ijt}] = \tau D_j + X_j'\beta + \gamma_t,$$

where Y_{ijt} is whether we observe a default against defendant i in building j at month-year t, γ_t is a fixed effect for each unique month-year combination, and X_j is a vector of covariates associated with the building (tract-level, block-level, or building-level characteristics depending on how fine-grained our measure is). Specifically, we control for logged census tract median income (2015 ACS), logged census tract median contract rent (2015 ACS), a quadratic polynomial for the share of White residents in the census block (consistent with the curvilinear form we see in *SI Appendix*, Fig. S6), a quadratic polynomial for the share of Hispanic residents in the census block, and whether the building is an apartment (provided by the Pew dataset). To address possible within-tract variations in rents, we also adjust for our best estimate of the monthly rent owed by the tenant. Although we cannot observe this directly from each lease, we do have access to a very close proxy—the amount of monthly "ongoing rent" demanded by the landlord in the filing. This should typically be equivalent to the monthly rent in the lease but is not always present in each filing. Also, due to data entry or other errors, we observe in the data a small number of extreme and likely incorrect values in both directions. We recoded as "missing" ongoing rent entries above 10,000 dollars per month. In our main analysis, about 7% of cases have missing or potentially erroneous "ongoing rent" amounts. We therefore use the missing covariate indicator method to adjust for this confounder without further dropping additional observations and reducing statistical power. We recode the missing rents to an arbitrary value (0) and control for two additional quantities: the observed rent interacted with an indicator for nonmissingness and an indicator for whether the rent data are missing.[††] We cluster all standard errors at the level of the building as this is the level at which the "treatment" of interest is assigned (two tenants in the same building will, by construction, receive the same value of the treatment) (51).

We augment our simple identification strategy with three additional approaches that expand the conditioning set and weaken the as-good-as-random assumption even further. The first leverages the fact that the distance from the center of the city and the commuting time are not perfectly correlated. Two buildings that may be the same "as-the-crow-flies" distance from the city center can nevertheless have dramatically different commuting times due to the structure of Philadelphia public transit. For example, even when we restrict the sample to just those properties that are between 5 and 10 km from the courthouse, the variation in commuting times is still substantial—from 15 to 65 min. Properties that are not located on the two main north–south or east–west subway lines and which rely on inconsistent bus transport have much higher commuting times even when holding constant the distance from the center of the city. We therefore weaken our conditional ignorability assumption by including the direct distance between the property and the Philadelphia

[††] As we are not interested in the coefficient on rent itself as it is a nuisance parameter in our analysis and clearly not casual, we are not as concerned about bias in the estimation of that particular parameter as we are in addressing sources of confounding of commuting time and default. Recent work has shown that with small levels of missingness, the missing covariate indicator adjustment method provides for generally valid inferences (49). Intuitively, under correct model specification and the absence of confounding between the "missing" covariates and the treatment/outcome, the method can provide unbiased estimates of the treatment effect (50). In general, the inclusion or exclusion of this covariate does not alter the effect estimates in any appreciable manner.

Municipal Courthouse. To avoid imposing strong functional form assumptions on the relationship between distance and the outcome in the regression model, we do this adjustment via a set of "ring fixed effects": dummy indicators that take on a value of 1 if the property is located within a particular 2-km-wide ring around the courthouse.[‡‡] This approach allows us to adjust for all unobserved confounders that are correlated with commuting time only by way of their relationship with the direct distance between the property and the Philadelphia city center. In other words, any remaining unobserved confounder that might threaten identification under this strategy would have to be associated specifically with transit commuting time and not merely the distance from downtown Philadelphia. We also are interested in potential heterogeneity in the treatment effect based on distance. It may be the case that there is very little effect for the areas of the city that are furthest from the center (such as the northeast) because most residents there already primarily commute by car.

We next estimate a model that includes landlord fixed effects. Using the Pew data, we match each property to its ultimate landlord. While some landlords have only a single property, a not-insignificant number have properties in multiple locations across the city. In some cases, the differences in commuting time among common properties can be significant. The landlord with the most unique buildings in our data—517—has properties with commuting times that range from 13.5 to 43 min. The next largest landlord has properties with commuting times that range from 18.5 to 58.7 min. Examining variation in defaults among properties with common owners allows us to rule out unobserved confounders associated with landlord characteristics, such as their overall propensity to evict or the characteristics of their leases (40), that may affect the ability of the tenant to appear in court.

Finally, we consider an identification strategy that uses the idiosyncratic differences in the quality of the transit network between the weekday and the weekend. Since all hearings are held on weekdays, we estimate the effect of transit time as measured on a weekday. However, because of changes in traffic patterns and variations in the bus and subway service schedules, the estimated commuting times for some properties can differ substantially depending on whether they are measured on a weekday or on the weekend, even when arrival time is held constant. *SI Appendix*, section 5 illustrates the differences between these two measures in our dataset. Even after adjusting for "crow flies" distance or our other covariates, we may be concerned that bad transit is an amenity of housing, so that the value of staying put is lowered in those locations. It is also possible that transit is correlated with other amenities (ability to access check cashing services, access to government support) that we do not easily observe. However, if such an unobserved confounder did exist, it is very unlikely that it would affect weekend commutes differently from weekday commutes. Therefore, our final identification strategy further controls for weekend commuting time in addition to all of our other covariates—essentially adjusting for a "baseline" measure of public transit quality. Under this design, the variation used to identify the treatment effect comes entirely from the gap between weekday and weekend commuting times, which is much more plausibly assigned "as-if-randomly."

To further evaluate the plausibility of our identification strategies, we estimate our regression models on two "placebo" samples—eviction proceedings for public housing and eviction proceedings conducted after the start of the COVID-19 pandemic—where we anticipate null effects due to the absence of our primary causal mechanism.

Before filing an eviction, Philadelphia public housing tenants are entitled to a protracted grievance and hearing process: almost no public housing tenants are evicted without knowing about its likelihood. A Resident Advisory Board is often in touch with those subject to eviction, providing them with advice and support. In addition, public housing evictions occur in a distinct courtroom and a later time (9:15 AM. instead of 8:45 AM.) They do not initially see a judge in that court but rather a PHA manager or paralegal who attempts to resolve their eviction without recourse to the court. Finally, according to lawyers for tenants, the show-up time is not usually strictly enforced, unlike in private tenant courtrooms. One wrote to us:

> "In public housing court, they will work with you when you show up even if you are late . . . If they can't resolve the dispute on that day, a new court date will be set for the public housing tenant to go before a judge . . . The legal representatives of PHA are less bent on obtaining judgments at any cost, unlike the way that the handful of repeat landlord attorneys build defaults into their economic model and depend on them to get through many eviction listings in a court morning."

We thus anticipate that the effect of commuting time on defaults in public housing evictions should be zero if our selection-on-observables assumptions hold true.

Likewise, changes in both Philadelphia law and the operation of the court during the COVID-19 pandemic made distance less salient for this time period. First, the eviction moratorium altered the pool of cases. At the same time, tenants were given the option to apply for a remote—phone or Zoom—hearing. In our data, only 1,820 evictions were filed from July 2020 (when post-COVID filings resume in our data) through December 2020. This is a substantial drop from the 7,773 filed in the comparable period from July 2019 to December 2019. Of all the post-COVID, nonpublic housing filings in our dataset (through July 2021) about 7% were marked as having a remote request. But the possibility that tenants could file for a remote hearing means that those which were not requested are a selected group. We find that despite the moratorium, about 47% of hearings resulted in defaults during the COVID-19 emergency period.

Results

For our analysis of the relationship between commuting distance and (default) eviction, we focus on cases where the date of the first outcome was the same as the first hearing, removing any cases where the first hearing was continued, deferred, or rescheduled. This leaves us with, in the pre-COVID period, a dataset comprised of 232,709 defendants in 181,958 cases across 53,578 unique buildings (53). Fig. 3 plots the spatial distribution of commuting times and default rates across these evictions.

In the commuting time map, we can clearly see the contours of the main public transit arteries—notably the Broad and Market Street subway lines. This transit effect means that properties located at similar distances from the center of the city may have drastically different commuting times. Among eviction cases in our sample, the typical defendant can expect a commuting time between 25 and 50 min, with a handful of defendants facing commuting times of over an hour. The right panel shows the spatial distribution of default judgments. While much

[‡‡]The longest distance for any property in our dataset is around 24 km, resulting in 12 such bins. For a different use of "circles" as neighborhood controls, see ref. 52.

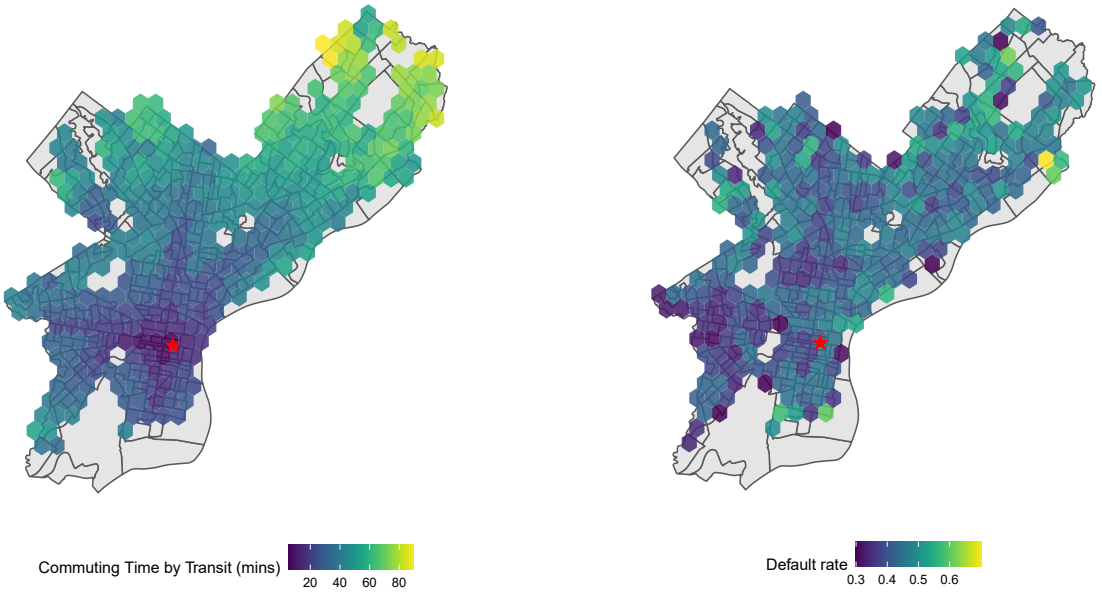

Fig. 3. Distribution of public transit commuting times to Philadelphia Municipal Courthouse and default eviction rates. January 2005 to February 2020. 53,578 unique buildings, 181,958 eviction proceedings, and 232,709 defendants. Transit times were estimated using Google Maps Distance Matrix API. Hexes with fewer than 25 defendants were treated as blank.

noisier than the transit plot, we can nevertheless see similar contours with lower default rates along the two major subway lines.

Fig. 4 plots the unadjusted bivariate relationship between commuting time (in minutes) and the probability of a default judgment. Notably, the relationship appears to be roughly linear with no obvious deviations from the regression fit when we overlay a binned scatterplot. The first plot also gives a sense of the range of the causal variable: most of the observations lie roughly between 25 and 75 min, suggesting that even a 1-h increase in commuting time is within the support of the treatment variable. The second plot shows the bivariate regressions of default on commuting time within each 5-km-wide ring around the Philadelphia Municipal Courthouse. We see that the slopes vary substantially across these rings, with the largest positive relationship between commuting time and default rate appearing among properties between 5 and 10 km from the courthouse. This suggests that both potential confounding correlated with distance and also likely heterogeneity in the treatment effect, which we explore in the subsequent analysis.

Fig. 5 presents the estimated average treatment effects of a 1-h increase in commuting time on the probability of default. We report the corresponding regression tables in *SI Appendix*, section 8. Adjusting for covariates and including month-year fixed effects, we estimate that increasing commuting time by 1 h raises the probability of default by about 8.5% points. In other words, for every 10 additional minutes that we predict a defendant will need to commute to court by public transit, the probability that the tenant fails to appear and the landlord receives a favorable judgment by default increases by about 1.4% points. Incorporating either the 2-km-wide distance ring fixed effects or landlord fixed effects reduces the estimated treatment effect by about half, but the effect remains positive and statistically significant. Moreover, even after adjusting for weekend commuting time, we find a strong positive and statistically significant relationship between weekday commuting time and default across all specifications. While the confidence intervals for these estimates are much larger, the estimated effects are all of a comparable magnitude to what we find in our primary regressions.

Taking the most conservative estimate of the treatment effect of a 1-h increase in commuting time from the landlord fixed effects analysis—3.8% points—we evaluate the counterfactual

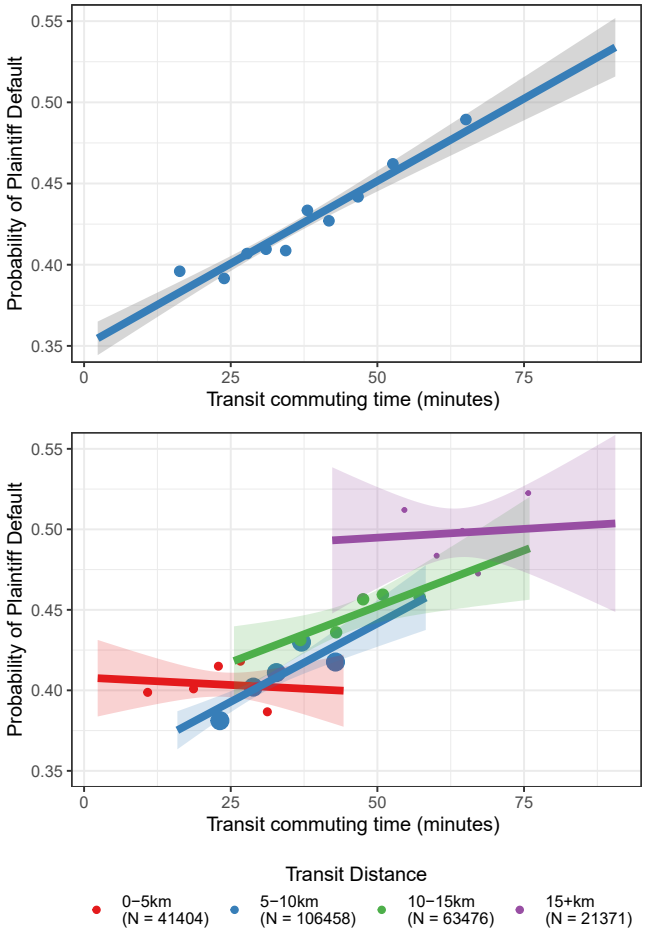

Fig. 4. Bivariate regression of default probability on commuting time. 53,578 unique buildings, 181,958 eviction proceedings, and 232,709 defendants. Points denote binned averages of the outcome variable. Robust standard errors are clustered on building.

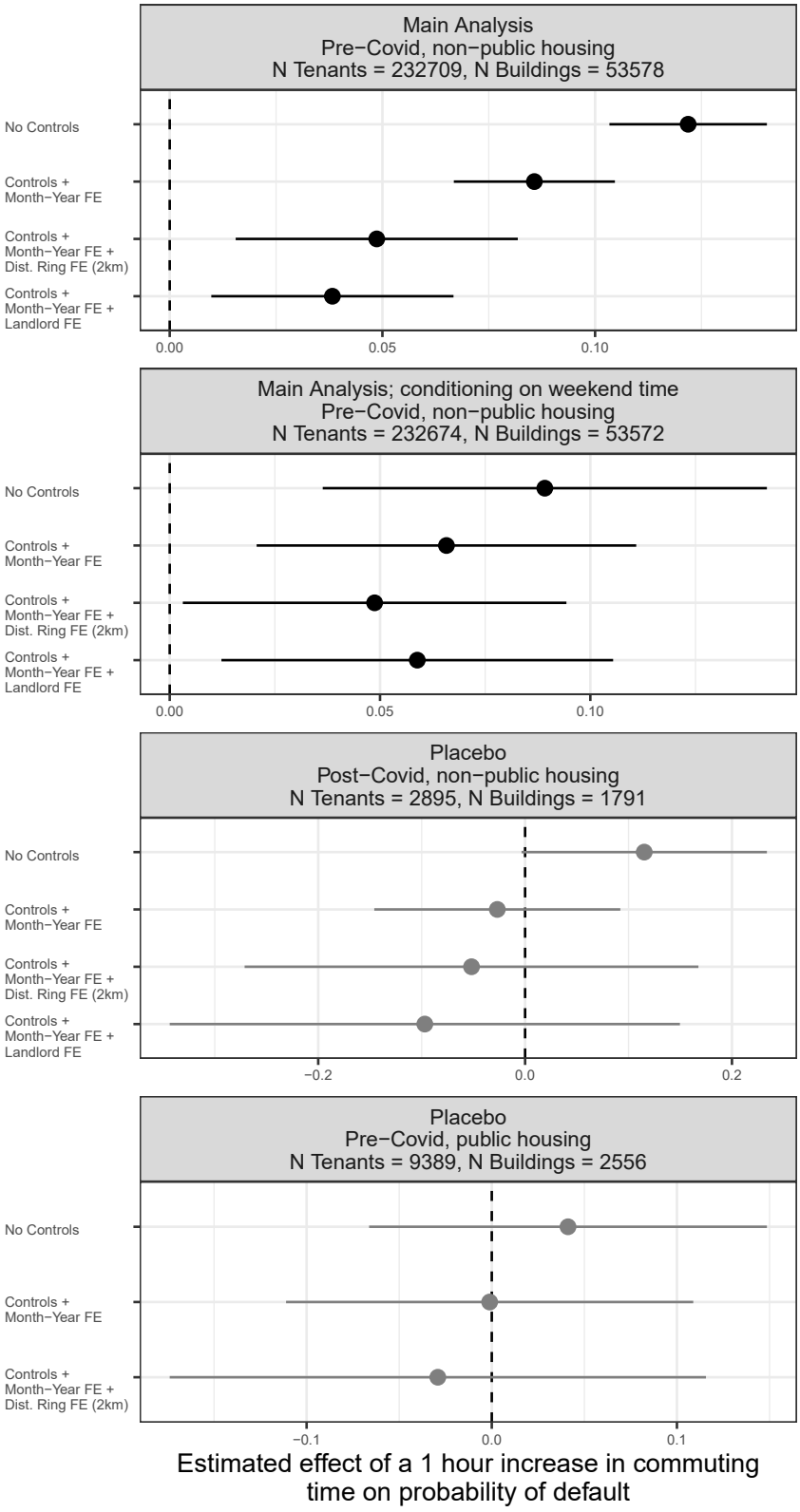

Fig. 5. Estimated average effects of a 1-h increase in commuting time on probability of tenant default. Estimates from a linear probability model estimated by ordinary least squares. Covariates include census tract median income (logged), census tract median contract rent (logged), a quadratic polynomial of census block % White, a quadratic polynomial of census block % Hispanic, estimated monthly rent from eviction complaint and whether the building is classified as an apartment. Lines denote cluster-robust 95% confidence intervals.

scenario where no tenant in our sample has more than a 10-min commuting time. In the dataset used for analysis, we observe 99,283 default judgments. We predict that the default probability for the average tenant will be reduced by about 1.8% points. Across the entire sample, such a change would have resulted in about 4,125 fewer default judgments against tenants. For our

largest estimate with covariate adjustment—8.6% points—we predict that the average tenant's default probability under the counterfactual would be reduced by about 4% points, resulting in 9,246 fewer defaults.

Our placebo analyses also suggest that there does not appear to be a significant source of unobserved confounding driving our results. As shown in the lower two panels of Fig. 5, we see no statistically significant relationship between commuting time and default rates among either public housing or post-March 2020 cases, particularly when we include relevant covariates. While it is the case that our standard errors are much larger in these analyses due to a much smaller sample of both post-COVID and public housing evictions, all of the point estimates in the placebo analyses where we include covariates are either negative or essentially zero.

We also examine whether the treatment effect varies by distance. Fig. 6 plots the estimated average treatment effects adjusting for covariates and month-year fixed effects within four 5- km-wide strata. The effect of commuting time is positive and statistically significant within all of the strata except for the subset of properties that are further than 15 km from the courthouse. Notably, the presence of a strong positive effect in the 0 to 5 km ring despite the absence of a relationship prior to adjusting for the confounders illustrates how some of the confounding between commuting time and default may be biasing us against discovering a treatment effect. While there does appear to be some effect heterogeneity, with commuting time mattering less for properties that are furthest out from the city center, for the vast majority of our sample, we detect a strong positive relationship between transit time and default judgments. In *SI Appendix*, section 7 we also investigated whether seasonality moderates the effect of treatment. We find no statistically significant heterogeneity in the treatment effect between the spring, summer, fall, and winter seasons. The treatment effects we identify appear to persist throughout the entire year.

Last, we attempted a replication of the travel distance effect in Harris County, Texas, using data accessed through the Eviction Lab (54). We relay the results of that replication in *SI Appendix*, section 9. Studying over 800,000 evictions from 2000 to 2018, we find results that are extremely similar to Philadelphia, despite the radically different jurisdiction. There, we find that a 10-min increase in driving time—since mass transit is largely unavailable in Harris County—raises the probability of default by about 3% points. Notably, the presence of multiple courthouses allows us to leverage differences in the assigned courthouse at the block and even building level in a way that we could not in Philadelphia. Even when we compare evictions taking place in the same building, in the same month assigned to two different courthouses, defaults are more likely at the courthouse that is further away.

A potential concern is that our observed default effect is offset by a positive effect on reopening petitions. But we find no significant association between commuting time and whether a tenant either files a reopening petition or successfully reopens a default judgment. While a small number of defaults are mitigated via reopening, most judgments are final. We discuss these results more in *SI Appendix*, section 3.

A second potential concern is that tenants who default may have received only slightly less bad outcomes in court had they shown up. As we explore in *SI Appendix*, section 6, there is a complicated relationship between defaults, "settlements" at the landlord tenant court and landlord withdrawing their petitions. To the extent that defaults substitute for landlord withdrawal, they are obviously bad outcomes for tenants. But in those cases in which landlords show up too, tenants in our data, like those previously studied, were locked out at a rate only slightly lower, and slightly later, than those who took defaults (55). That is so because such in-court settlements were historically the result of significant differences in bargaining power and sophistication between repeat-player landlords and tenants, at least in many cases. Unobserved differences among landlords, and between tenants, make it difficult to disentangle exactly when withdrawal or settlement is the most likely counterfactual for all of our defaults. For example, landlords who file weaker cases at the outset may prefer to avoid court, but those with stronger ones may prefer to show up, meaning that we would expect in-court settlements to be unusually tilted against tenants.

Nevertheless, there remain good reasons to believe that defaulting in court is a bad outcome, which should be prevented. Tenants who default cannot achieve the good outcome of a landlord withdrawing the case, and they will be deemed to have admitted the complaints' allegations including the amount due. Those who show up preserve their options for better outcomes. And at least in Philadelphia, they will be increasingly likely to achieve them, considering recent changes in the experience of unrepresented tenants in court. In 2018, the city initiated

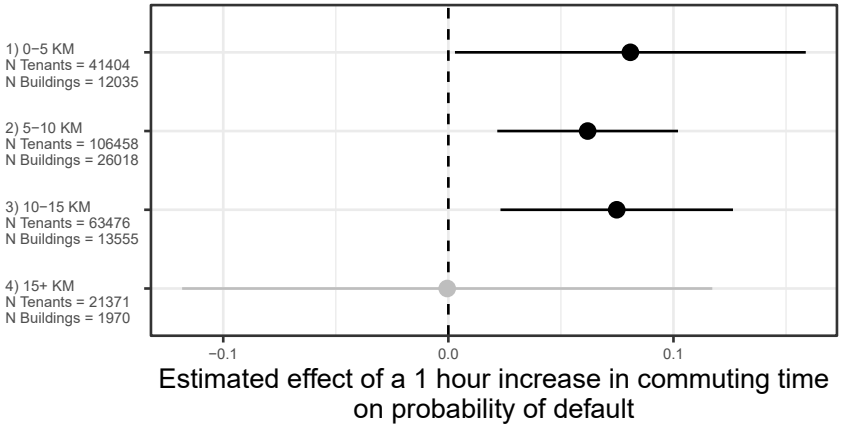

Fig. 6. Estimated average effects of a 1-h increase in commuting time on the probability of tenant default-heterogeneity by distance. Estimates from a linear probability model estimated by ordinary least squares. Covariates include census tract median income (logged), census tract median contract rent (logged), a quadratic polynomial of census block % White, a quadratic polynomial of census block % Hispanic, estimated monthly rent from eviction complaint and whether the building is classified as an apartment. Month-year fixed effects are included in all regressions. Lines denote cluster-robust 95% confidence intervals

the Philadelphia Eviction Prevention Project, which included a variety of measures aimed at improving tenants' ability to navigate the legal system, including the provision of a "courtroom navigator" to provide generalized advice and counseling to tenants facing eviction. The subsequent year, the City Council passed "Right to Counsel" legislation to allow low-income tenants to access free legal representation (56). Despite setbacks due to the pandemic, the program has been expanded and funded such that in 2022, tenants in two Philadelphia zip codes with high eviction rates are now eligible for representation through the program (57).

These changes in the aggregate have resulted in significant differences for those who show up today, versus the past: tenants are now told about their rights explicitly on entry to the court, and all provided with lay, and some legal, assistance in negotiations with landlords. This means that the welfare effects of missing court are now even more significant than they were before since the inequality that generated relatively poor settlement outcomes will be ameliorated. Thus, we have strong reasons to believe that there would be advantages to virtual court proceedings which would reduce the incidence of defaults since only tenants who show up, virtually or in person, can receive assistance with negotiations, and get better outcomes as a result.

Conclusion

Our results indicate that policymakers should consider the distributive effects of rules which forfeit legal rights conditional on showing up to the courthouse by a particular time. Alternatives—from remote hearings, to easy rescheduling, to no-excuse reopening—are available and would reduce the incidence of this pathologic practice. And scholars should inquire as to whether other legal proceedings are similarly affected by transit. Essentially, we highlight the role of physical place in producing access to justice. And our results may offer a better-identified tool to study the downstream effects of evictions.

Philadelphia centralizes its eviction system in one court. And its mass transit system may have unique characteristics: because it (relatively) serves West Philadelphia well, but Northeast Philadelphia poorly, it provides relatively better transit to City Hall for (distant) Black than White citizens. Since non-White citizens face distinct barriers in accessing justice, the role of race in failing to appear to court may be dampened in our results. Regardless, as default evictions are a national phenomenon (as are no-shows in many court proceedings), our results illustrate the crucial importance of transit access in the disposition of legal rights. The fact that the transit effect replicates in Harris County, which operates under different procedures and a different transit network, gives us some confidence that it is a phenomenon that might show up in other kinds of courts as well.

Eviction is a social pathology that the legal system channels into an adversarial process. The purpose of that process is to ensure that the state does not sponsor wrongful deprivations of shelter. And yet eviction courts—like other parts of the legal system that primarily burden the poor (33)—offer one-sided fights. Over the last 20 years, a plurality of all cases in landlord tenant court have resulted in defaults and most evictions have followed from default judgments. In this paper, we have asked about the role of structure, process, and procedure in producing that lamentable, and preventable, status quo.

Data, Materials, and Software Availability. The data and code necessary to replicate the analysis are available via the Harvard Dataverse at https://doi.org/10.7910/DVN/WUXLF2 (53).

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