

Do Public Camping Ordinances Reduce Crime?

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ECO 395K: Causal Inference

1 Introduction

It is common in America for cities to issue various citations to unsheltered people in order to deter them from establishing residence in certain areas. In Austin, Texas, this has come in the form of city ordinances banning public camping, aggressive solicitation, and sitting or lying on public sidewalks (I generally refer to all three of these ordinances as “the camping ban”). These ordinances have been a de facto norm in the city since the 1990s, and an analysis published by the *Texas Observer* found that 20,000 citations had been issued between January 2000 and May 2020. Of those cited, 75% failed to appear and were issued arrest warrants for a Class C Misdemeanor [5].

Homelessness advocates point out that these city ordinances force people to camp in more isolated areas of the city, further from access to supportive services. Moreover, they argue that a camping ban effectively criminalizes and perpetuates homelessness, with arrest warrants preventing the unhoused from obtaining a job or apartment.

Proponents of these city ordinances generally argue that the homeless are either unseemly, bad for business, or commit crimes. This paper is an attempt to produce reduced-form evidence of this last claim. I exploit a sudden reversal in policy by the Austin City Council in 2018 to estimate the treatment effect of the camping ban on crime using a differences-in-differences approach. I discuss the limitations of this approach and conclude with a suggestion of a more convincing model.

2 Background and Data

2.1 History of the Camping Ban

In November 2017, the Office of City Auditor released a highly critical report on the effectiveness of Austin’s homelessness policy [9]. It disparaged the city ordinances for the barriers created by the excess of arrest warrants issued. It also noted that citations were not effectively connecting unsheltered persons to case management services, as had been promised. The report ultimately recommended either repealing the city ordinances or amending them with less punitive language.

Over the next eight months, the City Council appeared to consider revising the camping ban multiple times, according to a fact sheet produced by councilmember Greg Casar [6]. In their June 28, 2018 meeting, an agenda item was introduced proposing an amendment to the solicitation ordinance [1]. The item was withdrawn and not discussed during the meeting, but local news outlets began to report that the City Council was on the brink of repealing the anti-homeless city ordinances [12]. Enforcement fell sharply (see Section 2.2 for discussion), and the City Council eventually followed through on what many saw as a foregone conclusion, repealing the solicitation ordinance and amending the sit/lie and camping ordinances to drastically reduce their scope during their June 20, 2019 meeting.

It is important to develop this timeline here before elaborating a model, since it underscores the difficulty in defining a “high enforcement period” and “low enforcement period” over time. The City Auditor’s report noted that the Austin Police Department had already revised their internal guidelines on the sit/lie ordinance in 2017, giving offenders a grace period of 30 minutes before issuing the citation and hence precipitating a decline in the number of citations written. Furthermore, there is an intervening period of one year between when the Council effort to repeal the ordinances became public knowledge and when it was enacted. And, even though APD has stopped issuing citations, the Texas Department of Transportation, at the behest of the Governor, still periodically conducted “sweeps” up until the COVID-19 pandemic that attempted to clear out encampments under state highway overpasses.

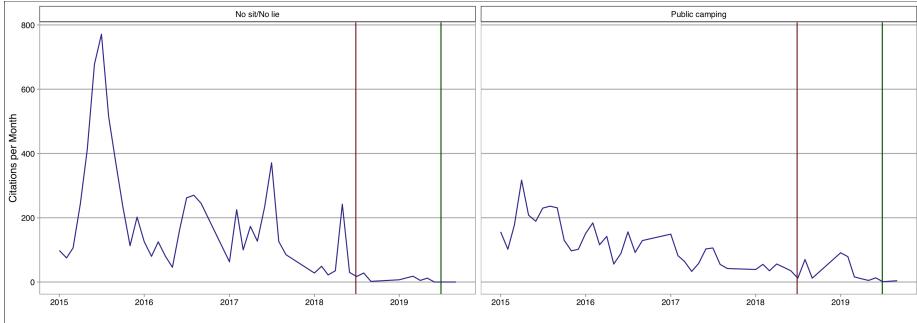


Figure 1: Monthly citations for public camping and sit/lie ordinances, 2015–2019. Red line is date of proposed policy change in June 2018, green line is date of implementation in June 2019.

2.2 Data on Ordinance Enforcement

There are two courts in Austin to which citations are referred: the Downtown Austin Community Court (DACC) for citations issued within the downtown area, and the Austin Municipal Court (AMC) for citations issued elsewhere within the city. I obtained all court records for DACC and AMC from January 2015 to December 2019 through the city of Austin’s Open Data Portal [8]. I then cross-referenced the offense codes listed with data from the *Texas Observer* and filtered out only public camping and no sit/no lie citations.

Finally, I geocoded the street addresses to latitude, longitude, and census tract. There was a marginal error rate in geocoding—I discarded samples that were geocoded because of an ambiguous address to a location outside of Austin. This constituted much less than 1% of the sample.

Figure 1 gives a plot of the citations per month issued from 2015 to 2019. The vertical line in red indicates the date of the City Council meeting of June 28, 2018. The vertical line in green indicates the date of the Council meeting on June 20, 2019. Though citations do not drop to exactly zero during the intervening year between June 2018 and June 2019, there is apparently a large “intent to treat” that changed officers’ behavior in issuing citations.

2.3 Crime Report Data

I obtained a database of crime reports from the city’s Open Data Portal [8]. The crime data are subdivided into broad categories of offenses, and I chose to focus on offenses from the “Theft” and “Robbery” categories. It is reasonable to focus on this category since it constitutes a wide array of property crimes, which fit the stereotypical notion of what a person without means may do to subsist. I geocoded all relevant crime reports from 2015 to 2019, again discarding the

small proportion of the sample that was mistakenly geocoded outside of Austin.

2.4 Note on Timeline

As a postscript on the background section, I will talk about the changes in camping ban enforcement and crime during the pandemic. After initially stating that they would not permit campers on the hiking trails that surround Lady Bird Lake in downtown, city officials did not attempt to move them after the coronavirus pandemic began and tensions flamed. A Political Action Committee called Save Austin Now garnered enough petition signatures to put the camping ban to a public vote as Proposition B in the May 1, 2021 city election. It passed by a 15% margin, and the camping ban is reinstated as of May 11, 2021. For more background, see [4][7].

I choose not to include data from 2020 in my post-treatment period because of the effect of the pandemic. Besides anomalous changes in camping ban enforcement, there are news reports that crime may have increased during the pandemic [11]. Whatever the practical implication of COVID-19, I abstain from trying to measure and control for this utterly unheard-of phenomenon.

3 Theoretical Framework

The main hurdle to overcome in answering causal questions about the unsheltered homeless is that there is little data on their whereabouts. The Ending Community Homelessness Coalition of Austin (ECHO) conducts a yearly point-in-time (PIT) count of all the unsheltered people living in the city [3]. The irregularity of the sample makes it difficult to conduct meaningful inference, and the survey is subject to methodological issues (including, for instance, that the survey is conducted by volunteers and reliability is hence dependent on volunteer turnout).

Instead, I will try to conduct inference based on the anti-homeless citation data that I have. This means I will have to develop some stylized facts that describe the relationship between the issuance of citations and the location decisions of unsheltered people. An ideal model would allow for the number of citations issued in area i in time t , cit_{it} , to depend on the number of unsheltered people n_{it} and a policy variable p_{it} :

$$cit_{it} = f(n_{it}, p_{it}, \varepsilon_{it}; \theta)$$

This captures the interplay between the policing policy that the City Council and Police Department set and the naturally tendency for certain parts of the city to be more attractive to unsheltered people. We would naturally expect

$$\frac{\partial cit_{it}}{\partial n_{it}}, \frac{\partial cit_{it}}{\partial p_{it}} > 0$$

as long as increasing p_{it} denotes a stricter policing policy. As reduced-form evidence, consider Figure 2. This shows the number of citations written monthly, by census tract. The PIT count notes that nearly 40% of the entire unsheltered population resides within the Downtown area of Austin. This corresponds to the census tract 001100. Figure 2 highlights this census tract, and it is at least anecdotally obvious that the number of citations is higher in this area of Austin where the unsheltered population tends to concentrate. Likewise, Figure 1 provides some anecdotal evidence that as the City Council relaxed their stance on the anti-homeless ordinances, the number of citations fell.

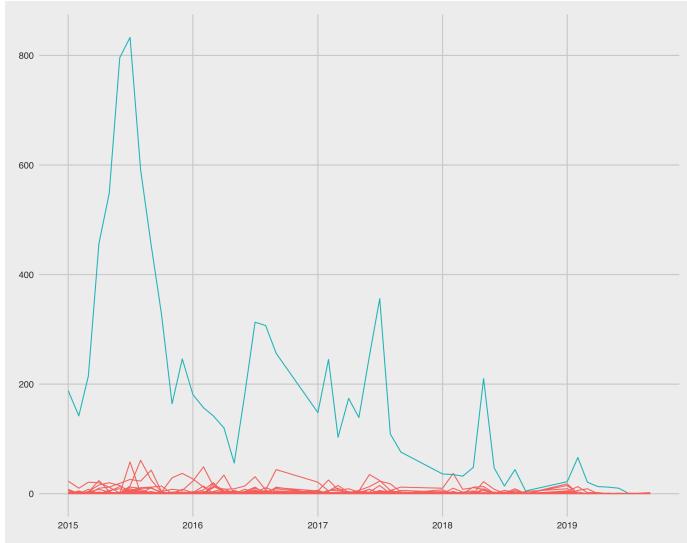


Figure 2: Monthly Ordinance Citations by Census Tract. Downtown (Census Tract 001100) Highlighted

More difficult to establish is the true deterrent effect that issuing citations has on unsheltered people. We may like to specify some intertemporal model of deterrence:

$$n_{it} = g(n_{i,t-1}, cit_{i,t-1}, \xi_{it}; \theta)$$

where there is state dependence, and citations written in a previous period will tend to deter people from residing in the same area in the next period:

$$\frac{\partial n_{it}}{\partial n_{i,t-1}} > 0, \quad \frac{\partial n_{it}}{\partial cit_{i,t-1}} < 0$$

In practice, anecdotal evidence from the City Auditor's report may cast doubt on the strength of this deterrent effect. They report a high number of serial ordinance offenders who are repeatedly cited and refuse case management services. Some 65 individuals accounted for around 10% of the 18,000 citations the

auditor reviewed between 2014 and 2016, and the most egregious offender was cited more than 120 times in one year alone [9].

I will generally assume then that a citation has a transitory deterrent impact. The PIT always finds that the same parts of Austin have the highest unsheltered counts, namely areas around downtown (see Figure 3). If enforcement policy had any permanent effect then this would not be the case. Rather, it seems that this is a strongly local effect, with the camping ban doing little to force relocation of people experiencing homelessness. Indeed, accounts from unsheltered people depict a life lived just out of sight in the numerous creeks and wild areas that permeate urban Austin [5][10]. The geographic dispersion changes little.

Table 3. Unsheltered Persons by City Council District counted during the Point-in-Time Counts in 2016 &

District	2016	2017	Change from Previous Year
1	79	54	-25
2	31	53	+22
3	116	101	-15
4	47	79	+32
5	41	40	-1
6	24	8	-16
7	53	49	-4
8	15	45	+30
9	356	397	+41
10	7	6	-1

Figure 1. Unsheltered Persons by City Council District, Counted on January 27, 2017

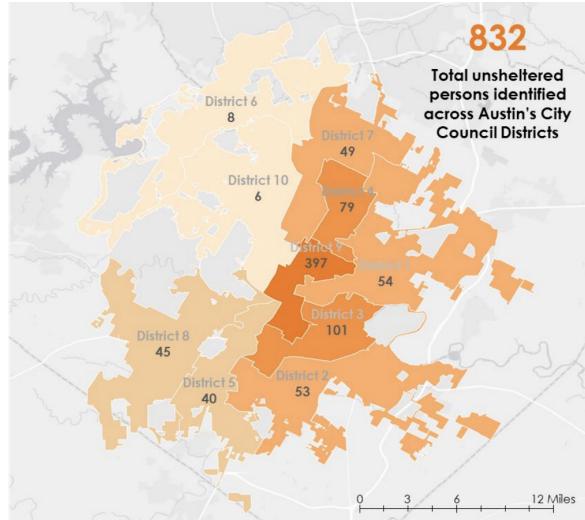


Figure 3: Select Figure from PIT Count 2017 [2]. City Council District 9, which represents Downtown Austin, accounts for majority of unsheltered count even amidst policy change.

It is reasonable to question, then, what treatment effect we hope to isolate by comparing high-citation to low-citation areas of the city. If it is not a physical

displacement, does it change unsheltered people’s behavior? When trying to isolate the effect of the camping ban on crime, it is important to keep in mind this ambiguous treatment effect. In a sense, the causal mechanism is for proponents of the camping ban to define.

Ambiguity aside, we turn to a basic model of crime. Lacking time and data, I do not try to build on the rich literature on determinants of crime. Instead, I note that the demographic determinants that are often pointed to as integral are relatively immutable. So once again, I think of this as a primarily localized phenomenon. Figure 4 shows monthly crime reports by census tract, with the downtown census tract 001100 again highlighted. In a reduced-form comparison of crime with and without the camping ban, I will assume that the within-tract variation in crimes committed captures the variation due to the (ambiguous) treatment induced by the City Council’s camping ban repeal. Under this assumption, a fixed-effect model can yield a causal interpretation.

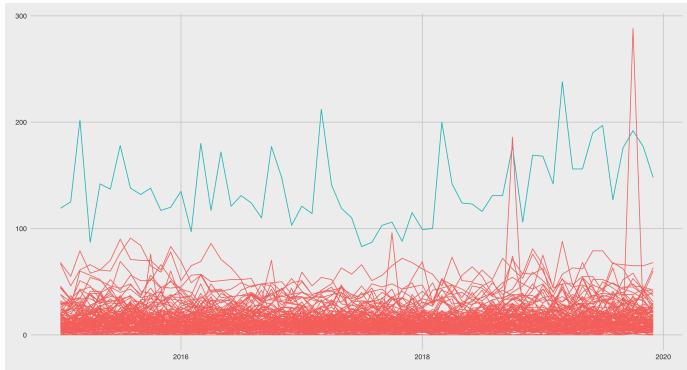


Figure 4: Monthly Reports of Robbery and Theft, by census tract. Downtown Austin (001100) highlighted for distinction.

4 Model and Results

In this section I develop a simple differences-in-differences estimate of the effect of the City Council’s revision of the camping ban on crime. I aggregate both crime data and citation data at the census tract level on a monthly basis to create a balanced panel of $T = 60$ months and $N = 113$ census tracts. I define a treatment cutoff q_D as the 90th percentile of the citations pre-treatment:

$$q_D = G_{(.9)}; G = \left\{ \frac{1}{t^*} \sum_{t=1}^{t^*} x_{it} \mid i = 1, \dots, N \right\}$$

Here, x_{it} is the number of camping ordinance citations in census tract i in month t . I let t^* represent the treatment date—I will present results using both June 28, 2018 and June 20, 2019 as treatment dates, which represent the

Cutoff Date	Estimate	Std Error
June 28, 2018	3.1285164	2.584715
June 20, 2019	3.5410402	2.8821456

Table 1: Point estimates and bootstrap standard errors of OLS DID estimate

date the Council announced ordinance revisions and when they went into effect, respectively.

Next, I assign a treatment to each census tract i , D_i , based on the treatment cutoff q_D :

$$D_i = 1 \left(\frac{1}{t^*} \sum_{t=1}^{t^*} x_{it} > q_D \right)$$

Given my assumed correspondence between the number of citations given, the number of unsheltered people, and the transience of the deterrent effect, treated census tracts represent the areas of the city with the largest homeless populations. A map of Austin with treated areas shaded is in Figure 5.

Letting $S_t = 1(t > t^*)$, I construct the model

$$y_{it} = \theta_0 + \theta_1 S_t + \theta_2 D_i + \delta(D \times S)_{it} + \alpha_i + \varepsilon_{it}$$

The outcome y_{it} is the number of crime reports in census tract i in month t . I let α_i control for unobserved time-invariant heterogeneity in tract i , with ε_{it} mean-independent error. The coefficient of interest is δ , which measures the mean change in monthly crime reports in affected areas of the city post ordinance reform. Table 1 presents the coefficient estimates for each possible cutoff date t^* , with bootstrapped standard errors (clustering at the census tract level).

4.1 Identifying Assumptions

In order to identify our model we must make both the typical strict exogeneity assumption of a fixed-effect model and the parallel trends assumption of a difference-in-difference model. Conditional on the tract-level fixed effect, we are assuming that absent the ordinance reform, high-homelessness census tracts would have experienced the same change in crime as the low-homelessness tracts. The strict exogeneity assumption implies that unobservables are uncorrelated across time, or that $E(\varepsilon_{it}\varepsilon_{is}) = 0$, $\forall i, t \neq s$. In other words, we are assuming that there were no other unobservable changes, outside of the treatment effect, which shifted the number of crimes committed in the city. Inclusion of data from 2020 would cast doubt on my identifying assumptions, since the onset of COVID-19 appears to have had confounding implications on both encampment decisions of the homeless and the crime rate.

While I make no claim to be an expert in empirical crime studies, I think that

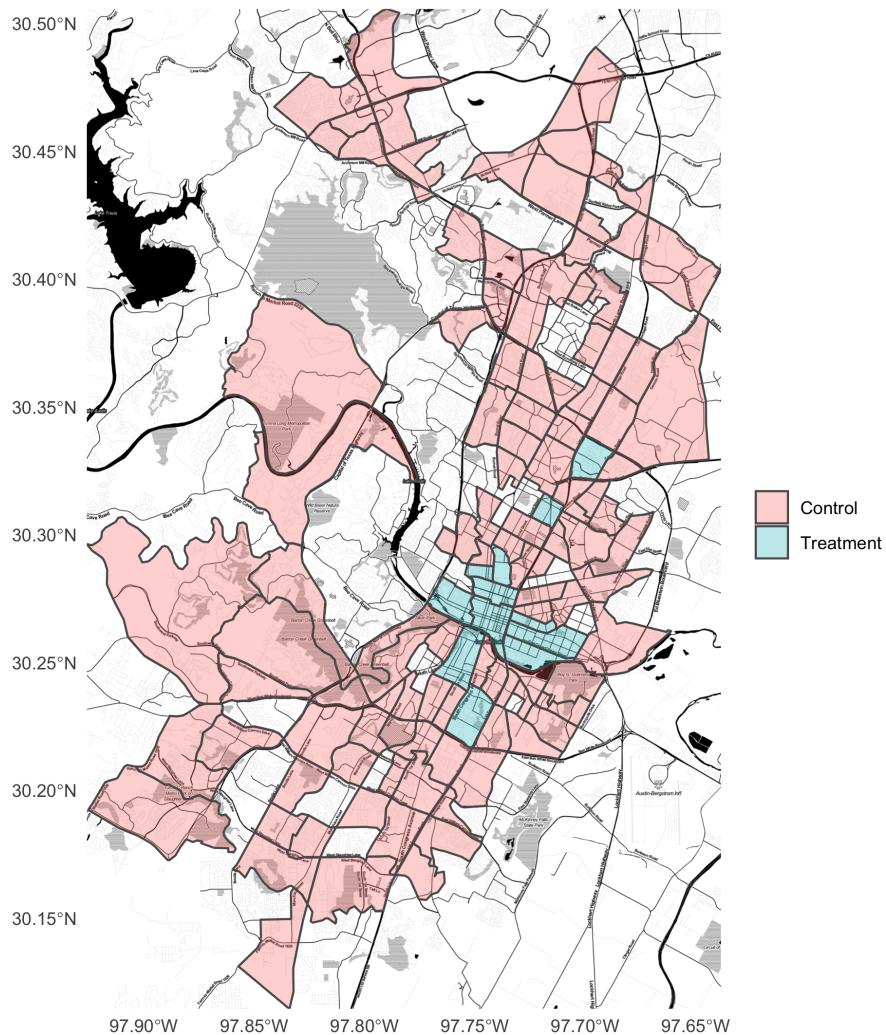


Figure 5: Map of treated and control areas of Austin according to pre-treatment citation levels. Note the main treated areas are near Downtown Austin and the University of Texas.

Panel A: Using $t^* = \text{June 28, 2018}$			
	Treated	Control	Difference
Before	-1.3916667	-0.4531117	-0.9385549
After	3.2472222	1.0572607	2.1899615
Difference	4.6388889	1.5103725	3.1285164

Panel B: Using $t^* = \text{June 20, 2019}$			
	Treated	Control	Difference
Before	-0.5243827	-0.1702787	-0.354104
After	4.7194444	1.5325083	3.1869362
Difference	5.2438272	1.7027869	3.5410402

Table 2: Difference-in-difference breakdown of demeaned monthly crime reports

both assumptions are more credible if conditioned on a larger set of covariates. Following a rational actor model of crime, we would expect opportunistic criminals to target higher-income and more densely populated areas. For Austin in particular, revelrous crowds of inebriated young adults are prime candidates for a pinch of thievery. In short, criminals and the unsheltered are drawn to the same parts of the city, albeit for different reasons. The introduction of a fixed effect is a crude way to control for this differential appeal. With more time I would draw tract-level characteristics from census data to make a more credible identification effort.

To illustrate identification of a difference-in-difference estimate in a fixed-effect framework, I present a classic breakdown in Table 2, conducted on deviations from group means. This illustrates that inference is conducted in the model with respect to deviations from a time-invariant tract-level effect.

4.2 Event Study Robustness Check

In order to provide a robustness check, I will use an event study design with a set of quarter-year time dummies spanning the 60 months in my sample. I estimate the model

$$y_{it} = \theta_0 + \theta_1 D_i + \sum_q 1(Q(t) = q)[\theta_{2q} + \delta_q D_i] + \alpha_i + \varepsilon_{it}$$

where $Q(t)$ represents the quarter of month t . I decide not to use month-year dummies because of the large parameter space this creates. I argue this does not significantly hinder interpretation: If there were some persistent treatment effect then we should see it even aggregating per quarter. I assign the treatment

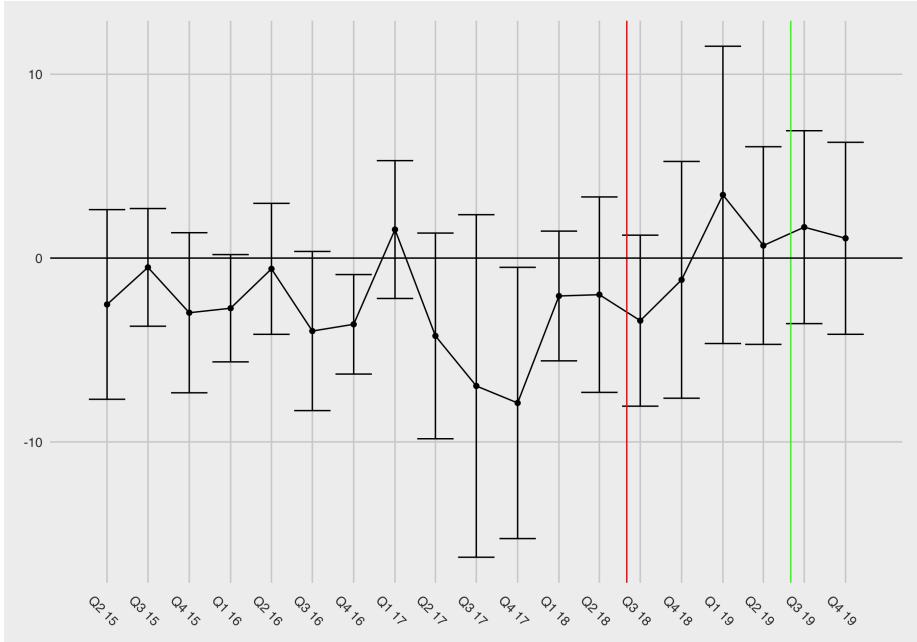


Figure 6: Treatment effect estimates using an event study design. Error bars denote a 95% confidence interval using block bootstrapped standard errors. Red and green lines are June 28, 2018, and June 20, 2019, respectively.

D_i based on the number of citations prior to June 28, 2018. Otherwise, results are not dependent on the choice of t^* . Assigning the treatment areas using $t^* = \text{June 20, 2019}$ did not significantly alter my findings.

Figure 6 plots the 95% confidence interval for each δ_q estimate, using clustered bootstrap for standard error estimation. I drop the first quarter because of colinearity. All but two quarters in the pre-treatment period fail to differ significantly from zero. Point estimates do appear to move from negative to positive in the first quarter of 2019 and stay positive afterwards, but standard errors are large.

5 Conclusion

If one is inclined to believe the identifying assumptions I've presented, then they could reasonably conclude that these results constitute evidence of absence of an effect. That is, the sudden and unprecedented policy reversal by Austin City Council did not create any knock-on effects for crime, and in an indirect sense they might conclude that homelessness does not portend crime. I do not endorse this inference with such enthusiasm, and my inclination is more

towards concluding there is an absence of evidence. Conditional on a richer set of covariates, this analysis may be more convincing.

Ultimately, we must accept that this vein of analysis has limitations. For one, it is difficult to infer conclusions on the movements and encampment decisions of the unsheltered homeless based solely on citation data. These data do not include information on sweeps conducted by state law enforcement, and moreover, the idea of a “treatment effect” is fairly abstract in this case, as I previously discussed.

Second, it is hard to pin down what time-varying heterogeneity may be affecting the level of crime in the city. In general, inclusion of a fixed effect is sufficient to capture some of the classical determinants of crime, such as income and demographic composition, which are relatively static within a census tract. Then again, Austin is a rapidly changing city. There are demographic pressures from the influx of high-profile companies like Google, Facebook, Samsung, and Amazon. Austin is one of the fastest-growing cities in the United States, and new arrivals tend to be upper-income, white-collar workers. For any resident of Austin, it is obvious that this rapid gentrification may introduce time-varying heterogeneity in both income and demographics.

Moreover, I would argue that a structural model that makes a more genuine effort to enumerate the rational motivations behind both the encampment decisions of the unsheltered homeless and the larcenous decisions of a criminal actor would produce more convincing results. My assignment of treatment based on the upper decile of mean citations is admittedly naïve and subject to the Lucas critique in the sense that it makes a rigid assumption about the reaction of the unsheltered homeless to the City Council’s policy change. A more thorough approach would be to identify structural parameters that are invariant to policy changes, although developing and estimating a structural model of homelessness would certainly be a sizable and novel task.

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