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Abstract

Objectives: Criminological scholars have long been interested in how macro-level characteristics of cities, counties, or metropolitan areas are related to levels of crime. The standard analytic approach in this literature aggregates constructs of interest, including crime rates, to the macro geographic units and estimates regression models, but this strategy ignores possible sub-city-level processes that occur simultaneously.

Methods: One solution uses multilevel data of crime in meso-level units within a large number of cities; however, such data is very difficult and time intensive to collect. We propose an alternative approach which utilizes insights from existing literature on meso-level processes along with meso-level socio-demographic measures in cities to impute crime data from the city to the smaller geographic units. This strategy allows researchers to estimate full multilevel models that estimate the effects of macro-level processes while controlling for sub-city level factors.

Results: We demonstrate that the strategy works as expected on a sample of 91 cities with meso-level data, and also works well when estimating the multilevel model on a sample of cities different from the imputation model, or even in a different time period.

Conclusions: The results demonstrate that existing studies aggregated to macro units can yield considerably different (and therefore potentially problematic) results when failing to account for meso-level processes.

Keywords: neighborhoods, cities, macro criminology, imputation

Bio

John R. Hipp is a Professor in the departments of Criminology, Law and Society, and Sociology, at the University of California Irvine. He is the director of the Metropolitan Futures Initiative (MFI). His research interests focus on how neighborhoods change over time, how that change both affects and is affected by neighborhood crime, and the role networks and institutions play in that change. He approaches these questions using quantitative methods as well as social network analysis. He has published substantive work in such journals as *American Sociological Review*, *Criminology*, *Social Forces*, *Social Problems*, *Mobilization*, *City & Community*, *Urban Studies* and *Journal of Urban Affairs*. He has published methodological work in such journals as *Sociological Methodology*, *Psychological Methods*, and *Structural Equation Modeling*.

Seth A. Williams is a Ph.D. candidate in the Department of Criminology, Law and Society at the University of California, Irvine. His research interests center on urban inequality, the role of housing and mobility in the production of neighborhood change, social and ecological predictors of neighborhood perceptions, and how these factors relate to neighborhood rates of crime over time.

Accounting for Meso- or Micro-Level Effects When Estimating Models using City-level Crime Data: Introducing a Novel Imputation Technique

Stemming from multiple theoretical traditions, place and space have increasingly become central concerns within the field of criminology. How researchers have theorized, conceptualized, and measured place as it relates to rates of crime has varied across time according to theoretical development, new sources of data, and new modes of analysis. Shifts in these domains contributed to a shift in theoretical and empirical focus from macro-level units such as cities and counties, to the meso-level neighborhood studies which rose to prominence beginning in the 1990s, and then the turn to even smaller geographic units in the micro-level analyses that have risen to prominence over the last 10 years[1]. While these developments have been incredibly productive in terms of theoretical refinement and empirical findings, we argue that the field of community criminology can benefit from an increased emphasis on the macro context. However, given the empirical findings generated by the explosion of meso- and micro-level research, we argue that it is unreasonable to examine city-level relationships without accounting for processes at the sub-city scale. While technological advancements have allowed for the proliferation of more spatially precise crime and social data, an enduring challenge to multi-level studies of place and crime is the availability of sub-city-level crime data across multiple city contexts.

Our focus here is in appropriately assessing how the city context impacts the level of crime across the neighborhoods of a city. We highlight that this is different from questions focused on whether meso-level processes operate similarly over different city contexts or asking whether meso-level processes are moderated in a systematic way by particular city-level contexts

[1] For an in-depth review of the history of geographical criminology, see Bruinsma (2017).

(i.e. cross-level interaction effects). While these questions are certainly of theoretical and substantive interest as suggested by the handful of studies that have explored them (e.g. Baumer, Wolff, and Arnio 2012; Lyons, Velez, and Santoro 2013), they require intensive data collection at the sub-city-level across multiple cities. Instead, we are interested in how city-level contexts impact the level of crime in cities, a question that was central to many macro-level studies, particularly in the 1980s, and popular at that time given the availability of city-level crime data for nearly all cities since 1960. Here we demonstrate that the common approach of fitting regression models which predict city-level crime with city-level constructs runs the risk of obtaining biased results by ignoring sub-city-level effects.

In this paper, we propose a novel imputation technique which enables researchers to conduct city-level studies which account for sub-city-level effects even in lieu of neighborhood-level crime data. Throughout this manuscript we will refer to sub-city effects as "meso-level effects". This is only because in the example we present here we use meso-level data for building our imputation model. We emphasize that our strategy will generalize to virtually any sub-city units (micro-, meso-, or otherwise) for which the researcher has crime data from a reasonable sample of cities for building the imputation model. We will return to this point in the discussion. In the section to follow, we make a case on both theoretical and methodological grounds for accounting for meso-level relationships in studies of crime across city contexts. We argue that such an approach will help to isolate the true city-level relationship, potentially opening up the possibility of new theoretical development and refinement in models of place and crime.

Background: Meso or Macro Ecological Units

Within the field of community criminology, as well as in the broader social ecology and neighborhood effects literature, there has been consistent debate as to what sort of unit best captures the notion of 'neighborhood.' More recently, researchers are increasingly interested in determining the proper spatial scale at which various social processes operate, be it cities, neighborhoods, or micro-level units such as street segments. The current consensus of the field is that what constitutes the proper ecological unit is dependent on the process being examined and the question at hand (see Taylor 2015; Hipp 2007). This implies that different processes posited by varying theoretical perspectives can operate at different scales. Further, the same construct can operate simultaneously at different scales, though the interpretation of the effect (i.e. the implied mechanisms) should differ across scales and can take on different meanings (Boessen and Hipp 2015). Even so, the choice of geographic unit is often one of convenience rather than theoretical grounding.

Macro-level studies of the 20th century often focused on cities, counties, or metropolitan areas, usually due to data availability rather than explicit reasoning about their choice of spatial scale. Early work was particularly interested in the notion that urbanism itself was a criminogenic force, drawing on the work of Wirth (1938) to assess the association between city or metropolitan population characteristics and rates of crime (Schuessler 1961; Schuessler and Slatin 1964; Gibbs and Erickson 1976; Skogan 1977; Danzinger 1976). Later work would focus in on the particular effects of poverty, unemployment, and inequality within cities or metro areas, drawing on conflict, strain, and anomie perspectives (Chiricos 1987; Eberts and Schwirian 1967; Bailey 1984; Blau and Blau 1982; Messner 1982). Several key studies addressed the relationship between macro-level racial composition and crime levels (Liska and Bellair 1995; Shihadeh and Flynn 1996; Messner and Golden 1992; Messner 1983) drawing on various theories including subculture of violence, conflict perspectives, and theories of racial inequality. Other work would examine how characteristics of local police agencies influenced crime in a deterrence framework (Tittle and Rowe 1974; Greenberg, Kessler, and Logan 1979; Decker and Kohfeld 1985; Marvell and Moody 1996). While some of these studies made conscious arguments as to why these processes should unfold at the macro-level, many seemed to pose questions at the city-level out of convenience. Researchers would go on to situate these problems at the neighborhood level as tract-level data became more accessible.

Neighborhoods are salient units of analysis for a number of reasons, but in particular, neighborhood-level constructs are meant to capture place-based group processes, rather than just the aggregate characteristics of individuals (Bursik 1988). With renewed interest in social disorganization theory (Kornhauser 1978; Stark 1987; Bursik 1988; Sampson and Groves 1989; Bursik Jr and Grasmick 1993) theoretical refinement of the systemic model (Bursik Jr and Grasmick 1993; Kasarda and Janowitz 1974; Bellair 1997; Warner and Rountree 1997), collective efficacy theory (e.g. Morenoff, Sampson, and Raudenbush 2001; Sampson, Raudenbush, and Earls 1997), and the emergence of large-scale community based surveys such as the Project on Human Development in Chicago Neighborhoods (PHDCN) (Earls et al. 2002), there was an explosion of research focused on the relationship between neighborhood structure and crime, and intervening mechanisms. More recent studies have extended this further in arguing that micro-level units are primarily of interest as their characteristics are thought to shape criminal opportunity (Brantingham and Brantingham 1984; Brantingham and Brantingham 2008), though some also argue that various community processes operate at the block face level as well (Taylor 1997; Grannis 2009).

Cities as ecological units

Without diminishing the importance of micro- or meso-level processes, we argue that the macro context is also an important focus of research, and that cities specifically are important contexts for several key reasons. First is the simple empirical observation that cities differ markedly in their rates of crime. It is not likely that such variation is reducible to drastic differences in the properties of micro or meso places. Consequently, it is important to understand why, holding all else constant, cities differ in their rates of crime. Studies using multilevel data of neighborhoods nested in cities have directly demonstrated this possibility. For example, multilevel studies have found that police behavior can shape the relationship between neighborhood disadvantage and violent crime (Martin, Wright, and Steiner 2016). Work on immigration finds that the negative relationship between neighborhood immigrant concentration and violence is stronger in cities with a greater representation of minority groups on the police force (Chenane and Wright 2018) and that location in established immigrant destinations is associated with lower violence in neighborhoods with moderate immigrant concentration (Ramey 2013). Other work finds that city-level inequality impacts neighborhood crime rates net of neighborhood characteristics (Chamberlain and Hipp 2015), and the level of racial segregation in cities is associated with higher levels of crime in neighborhoods, even controlling for key neighborhood-level measures in multilevel models (Peterson and Krivo 2010). Research finds that the positive relationship between neighborhood racial diversity and crime is weaker in cities with greater levels of racial diversity (Wenger 2018). This body of research provides clear evidence that characteristics which vary across cities shape neighborhood rates of crime.

While there is some debate as to the appropriate macro-level unit for certain constructs (Hipp and Kane 2017; Kim and Hipp 2018), cities are salient as they are political entities. Cities enact a range of policies directly aimed at reducing crime, they shape policing and criminal justice practices, and craft policies which mitigate or exacerbate criminogenic social or structural features such as residential instability or poverty. Past macro-level research has shown that dimensions of the structure of city politics are consequential for crime and related outcomes – specifically, local governments which are more directly representative of local residents (both in demographic and geographical representation, as well as in form of government) have less crime (Stucky 2003, 2005, 2012). Jacobs and Wood (1999) find that interracial homicides are affected by the presence of a Black mayor, where Black victimization by white offenders occur more often and white victimization of Black offenders occurs less often. As further evidence of the importance of the macro political environment, Rosenfeld, Baumer, and Messner (2001) proxied civic engagement with the proportion of the voting age population that voted, arguing that "high levels of civic engagement should strengthen social organization, and promote informal social control, thereby yielding lower levels of crime and violence" (pg. 286). Multilevel studies have found that the negative relationship between immigrant concentration and neighborhood violent crime is stronger in cities with more favorable immigrant political opportunities (Lyons, Vélez, and Santoro 2013), and that the positive relationship between percent Black and violence in neighborhoods is effectively eliminated in cities with favorable political contexts for Black residents (Velez, Lyons, and Santoro 2015).

Several studies have examined the effect of city policies such as three-strikes policies (Kovandzic, Sloan III, and Vieraitis 2004), concealed-carry gun laws (Kovandzic, Marvell, and Vieraitis 2005), sanctuary city policies (O'Brien, Collingwood, and El-Khatib 2019; Martínez-Schuldt and Martínez 2017; Lyons, Vélez, and Santoro 2013), Community Oriented Policing Services (COPS) funding (Zhao, Scheider, and Thurman 2002), and other policing interventions (Rosenfeld, Fornango, and Baumer 2005) on city crime rates. Still others consider substantive questions which emerge as important considerations in particular historical periods for impacting crime rates such as crack use (Baumer 1994; Baumer et al. 1998), rates of immigration (Ousey and Kubrin 2009; Stowell et al. 2009); historical effects of lynching in the South (Messner, Baller, and Zevenbergen 2005) and the so-called “Ferguson effect” (Pyrooz et al. 2016). Although researchers within community criminology seem to increasingly take a ‘smaller is better’ approach to the selection of the proper unit of analysis (Tita and Radil 2010), this existing macro-level body of work indicates that some questions are properly situated at the city level of analysis and are important considerations in the study of place and crime.

Given the wealth of insights stemming from this macro-level research, and the basic motivations for undertaking city-level research outlined above, we argue that the city is an important context which warrants explicit attention by criminologists interested in questions of place and crime, as well as those interested in the effect of social policies on crime. However, we also know from the robust literature on neighborhood-level processes related to crime that the meso-level is a meaningful unit that cannot be neglected.

City-level studies that neglect meso-level (or micro-level) relationships risk misspecification. This necessitates the ability to account for sub-city-level relationships to truly isolate, assess, and perhaps more cogently theorize city-level processes. This poses a challenge as neighborhood-level crime data has been historically difficult to obtain for multiple cities. Existing city-level studies that do not account for meso-level processes must either assume that their exclusion does not bias the results, or assume that the meso-level processes act as mechanisms (a point we will return to in the discussion). Thus, the imputation approach proposed in the present study provides a means to account for neighborhood-level processes in studies of city contexts, allowing us to disentangle the level at which certain characteristics of places produce more or less crime.

Methodological strategy: Imputation

As we described earlier, although researchers would prefer to have meso-level data for a large number of cities, this is typically difficult to come by. Although models accounting for meso-level processes would be ideal, collecting such data is a particularly onerous project as it requires a large enough sample of cities to have reasonable statistical power at the city-level. Furthermore, collecting longitudinal data would be even more difficult to accomplish. Instead, what is available are crime data aggregated to the city level, collected over a long period of time, which provide the opportunity for researchers to explore longitudinal questions.

The typical strategy when working with city-level crime data is to collect city-level variables of interest and examine their association with levels of crime in cities. This approach implicitly ignores processes occurring at the sub-city-level. In contrast, our approach proposes imputing city-level crime down to sub-city units, allowing us to account for these smaller geographic units when assessing research questions located at the macro-scale. A naïve imputation strategy would be to simply impute the crime rate uniformly to all smaller units in the city. This is certainly wrong based on how neighborhood crime concentrates. A better approach is to build a more informed imputation model which includes meso-level socio-demographic characteristics known to be important predictors of crime from past research. This approach is feasible given the accessibility of Census data on socio-demographics and city-level crime data from the Uniform Crime Reports.

Our approach only requires that we have a sample of cities with crime measured in sub-city geographic units. The larger or the more representative the sample of cities, the better our imputation model will arguably be. We will build this imputation model on the sample of cities, and use the parameter estimates to impute crime to the smaller geographic units within a number of cities that are not part of the sample. By imputing values multiple times, we appropriately account for the uncertainty of the imputation by leveraging insights of the multiple imputation literature, and computing proper standard errors (Rubin 1976).

All models require assumptions, and it is worth highlighting the assumptions present in our strategy compared to other strategies. Our model relies on the assumption that the meso-level model does a reasonable job predicting crime levels. The better the meso-level model, the better our macro-level parameter estimates will be. But it is worth emphasizing that we are only facing a *prediction problem*. That is, it is not a question of whether the coefficients of the imputation model are unbiased, but rather how good a job we do of predicting the level of crime across the meso units. We leave it as an open question to assess how much error our approach can handle and still return reasonable results; this question is outside the scope of this initial introduction of the method.

Conversely, the traditional approach of estimating city-level models requires much stronger assumptions. Such models effectively assume that there are *no* meso-level processes occurring, other than those directly caused by the macro-level measures. Given the voluminous literature exploring how crime is spatially distributed within macro units, and the systematic relationship between certain measures and this spatial distribution, this is an untenable assumption. One goal of the current study is to provide a demonstration of how problematic this assumption is. The traditional approach is also not necessarily capturing the total combined effect of the city and neighborhood level, as we demonstrate later in the manuscript. We emphasize that our approach is useful for obtaining proper macro-level parameter estimates while simultaneously accounting for meso-level characteristics. However, our approach is not able to obtain unique neighborhood-level parameters, nor is it able to test for cross-level interactions. Such research questions require intensive data collection for multilevel data of crime nested in macro units.

A second goal of the current study is to provide a brief example of how this approach might work in the context of longitudinal models. This addresses the question of how well our meso-level imputation model would work for imputing data in other decades given the availability of city crime data back to 1960. We assess this by including a non-random sample of cities from ten years later and showing the consequences of using these parameter estimates from a later time point for the imputation model in the current year.

Data and Methods

To demonstrate our novel approach for imputing neighborhood-level crime data, we use a neighborhood-level dataset collected across 89 cities (Peterson and Krivo 2010). An advantage to using this dataset is that we are able to demonstrate our technique on data in which we can estimate the "true" model, given that we actually have data for tracts nested in cities. This is a representative sample of neighborhoods in large cities (greater than 100,000 population), which provides a useful example of our technique. Although a downside to this dataset is that the smallest geographic units are tracts, which precludes assessing more micro-level effects, we highlight that our imputation approach generalizes in a straightforward manner to data with even smaller geographic units, such as blocks or street segments. This would require crime data for a national sample of micro-units in cities, which is something not currently available. Our approach also generalizes to other aggregations of "neighborhood", such as egohoods (Hipp and Boessen 2013).

Imputing tract-level crime

Our general analytic strategy will be to demonstrate: 1) how our approach works when imputing the crime data to tracts for the 89 cities and then estimating the model on those same 89 cities; 2) the robustness of the approach when basing the imputation on coefficients from data ten years later; 3) the robustness of the approach on “out of sample” cases by randomly splitting the sample of cities and estimating the imputation model on 39 of the cities, and then imputing the crime data and estimating the model on the remaining cities (and performing this random split 500 times to assess the bias and mean absolute error of the approach); 4) how the approach works on a sample of 517 cities with at least 50,000 population (cases in which we do *not* have tract-level crime data, and do not know the true model). The advantage of the first three demonstrations is that we know the “true” model, allowing us to directly assess the robustness of the proposed technique. The first set of analyses simply demonstrate that the approach works as expected, as it is a trivial task to impute the data to the same sample of cities. The second and third sets of analyses are important as they demonstrate the technique when the imputation model is built on cities at a different time point, or a set of cities that is separate from the set of cities that are used to estimate the model of interest. The fourth set of analyses allows us to demonstrate how the strategy would work in a “typical” analysis using city-level crime data.

Data

This study uses tract-level crime data from the National Neighborhood Crime Study (NNCS), collected by Peterson and Krivo (2010). For most cities, the crime data comes from three years (1999-2001) and the average is computed over the three years. We use data from the U.S. Census for 2000 to create measures capturing the socio-demographic characteristics of the tracts and cities. We used address-level business data obtained from Reference USA (Infogroup 2015). For the final set of analyses, we obtained crime data from 1999-2001 for cities in the U.S.

with at least 50,000 population from the Uniform Crime Reporting (UCR) program (United States Department of Justice. Federal Bureau of Investigation 2005). We also constructed a non-random set of 80 cities with crime data in 2010 and estimated the imputation model on these cities to assess the consequences of imputing data in one decade based on a model from a later decade; this provides insight into how this approach might work for longitudinal models going back multiple decades.[2]

Dependent Variables

The dependent variables for these analyses were violent and property crime rates (per 100,000 population), log transformed. These logged crime rate variables exhibited a normal distribution, allowing treating them as continuous measures. Our approach generalizes to count outcomes, but here we demonstrate the approach using a multilevel linear model. The violent crime measure is a summation of the number of aggravated assaults, robberies, and homicides. The property crime measure is a summation of the number of burglaries, motor vehicle thefts, and larcenies. Given missing neighborhood crime data in certain cities, we had 78 cities with violent crime data, and 89 cities with property crime data. We constructed similar measures of logged violent and property crime rates using the city-level UCR data.

Tract-level independent variables

We constructed several socio-demographic variables that have been shown in prior studies to be important covariates of crime levels in neighborhoods. These measures were constructed at the census tract level. Given that our primary goal at the meso level is predicting

[2] The cities are: Akron Alexandria Asheville Atlanta Aurora Austin Bakersfield Baltimore Baton Rouge Bellevue Boston Boulder Buffalo Cambridge Carrollton Cary Chapel Hill Chattanooga Chicago Cincinnati Cleveland Corpus Christi Dallas Dayton Denton Denver Detroit Durham Fayetteville Flint Fort Collins Fort Worth Fresno Frisco Gilbert Glendale Grand Rapids Hartford Honolulu Houston Indianapolis Irving Kansas City Kent Lexington Los Angeles Louisville Milwaukee Minneapolis New Orleans New York Oakland Orlando Philadelphia Pittsburgh Portland Raleigh Reading Richmond Rochester Rockford Sacramento Salt Lake City San Antonio Sandy Springs San Francisco San Jose Savannah Scottsdale Seattle Sioux Falls Spokane St Louis Stockton St Paul Toledo Tucson Tulsa Urbana Washington

the level of crime in these units, it is better to be more inclusive in the variables included in the model rather than parsimonious. There is not a concern of multicollinearity in the imputation model given that we are focused on obtained predicted values. We include measures based on several different meso-level theoretical perspectives. Social disorganization theory posits that the socio-economic status of the neighborhood will impact the level of crime, and we capture this with a measure of *logged average household income*. We measured the level of *residential stability* by computing the mean of the standardized values of percent homeowners and percent new residents in the last 5 years. We account for the racial/ethnic composition with measures of *percent Black* and *percent Latino*. We capture *racial/ethnic heterogeneity* with a Herfindahl Index of five racial/ethnic groups (percent Asian, Black, Latino, White, and other race).

Given that vacant units can attract crime, we created a measure of *percent occupied units*. Scholars have argued that inequality at various spatial scales can impact crime (Hipp 2007), so we created a measure of *income inequality* based on the Gini coefficient.[3] We included a measure of *percent aged 16 to 29*, as this captures the age group that is more active in criminal offending. Recent work has suggested that the age of housing can operate as a proxy for physical disorder (Hipp, Kim, and Kane 2019), which may be related to levels of crime, so we created a measure of *average age of housing*. Given that long commutes imply that residents are away from the home for longer periods of time, which may reduce guardianship (Cohen and Felson 1979), we constructed a measure of *average commute time;* this is based on the binned data of time of commute, and we assigned the midpoint of each bin for computing the average.

[3] To account for the binning of the income data, we utilize the prln04.exe program provided by Francois Nielsen at the following website: http://www.unc.edu/~nielsen/data/data.htm. The program uses the Pareto-linear procedure (Aigner and Goldberger 1970; Kakwani and Podder 1976), which was adapted by Nielsen and Alderson (1997) from the U.S. Census Bureau strategy.

Imputing tract-level crime

There is a long line of literature positing that *population density* can impact levels of crime (Boessen and Hipp 2015; Hipp and Roussell 2013; Raleigh and Galster 2015), so we constructed this measure by dividing the population of the tract by the land area in square miles. There is also a voluminous literature focusing on the ecological effect of immigrants in communities (Ousey and Kubrin 2018), so we constructed a measure of *percent immigrants*. Given that parents are less likely to be involved in crime as offenders, and are often more engaged in the neighborhood and as such more likely to provide informal social control, we capture this with a measure of *percent households with children*. We constructed a measure of the *unemployment rate*, as this may impact crime by providing more offenders. Finally, business districts can impact the location of crime, so we captured this with measures of *total employees* and *retail employees*.

City-level independent variables

We constructed five variables at the city-level that are of particular theoretical interest. These include measures of *racial/ethnic heterogeneity* and *inequality* at the city-level, which are conceptually distinct from the tract-level measures given that the meaning of distributional measures changes depending on the scale at which they are measured. The size of the city may have important consequences for the crime rate, so we constructed a measure of *population*. Note that we are assessing whether there is a nonlinear relationship between population and crime, given that the crime rate construction assumes a linear relationship, and this population coefficient assesses if there is an additional impact of population. Given evidence that recent city growth can impact crime levels (Hipp and Kane 2017), we included a measure of the *percentage change in population from 1990 to 2000*. To capture the importance of the socio-economic level of the city, we constructed a measure of *average household income* (logged) in the city. In addition to these five city-level variables of theoretical interest, for certain model specifications we constructed variables at the city-level analogous to those constructed at the tract level described earlier.

The summary statistics for the variables used in the analyses are presented in Table 1. We highlight that although some may be concerned that the correlation between tract-level and city level variants of the same measure may be very high, this is not the case. For example, the correlation between tract- and city-level racial/ethnic heterogeneity is just .28, highlighting that these are capturing conceptually distinct measures. Likewise, the correlation between tract- and city-level inequality is .18, and the correlations between the average income measures is .40.

<<<Table 1 about here>>>

Methods

Imputation strategy

The main focus of our analysis is the introduction of our novel imputation strategy of crime from the city-level to smaller geographic units. The first step is to build a meso-level model. The strategy requires crime data at these smaller geographic units for some set of cities, to allow estimating the imputation model for these smaller units, and then using the results to impute from the city-level crime data to these smaller units. In our example here, we build a meso-level model given that we have crime data aggregated to census tracts across 89 cities for property crime (and 78 cities for violent crime). To assess how crime is distributed within these cities, we estimate a city-level fixed effects model:

(1) $$y_{ij} = \mathrm{X}_{ij}\mathrm{B} + \mathrm{C}\Phi + \mu_{ij}$$

where y is the level of crime in tract i in city j, X_{ij} is the vector of tract-level measures that we identified earlier as covariates of the level of crime, B is a vector of the estimated parameters showing the relationship between these measures and the level of crime in the tract, C is a vector of dummy variables for cities with Φ effects on the level of crime, and μ_{ij} is an error term for tract *i* in city *j*.

After estimating this model, we use the B vector of estimated coefficients to impute the level of crime in tracts. This first step is straightforward, as it is simply computing the predicted value of crime in each smaller unit (the "y-hat"). To accomplish the multiple imputations, for each imputation we pull a random number from a normal distribution with a mean of zero and a standard deviation equal to the root mean squared error from the initial estimated imputation model (equation 1). This random number is added to the linear prediction (y-hat) of crime in the tract to create an estimate of crime in the tract for this particular imputation.

Our estimate of tract level crime requires that we also adjust for the overall level of crime in the city. We have this information, so next we adjust the imputed level of crime in tracts such that they will sum to the level of crime in the city. Given that we are estimating a model with logged crime rates as the outcome, this adjustment requires a couple of extra steps from what would be needed if crime counts were the outcome: 1) we first exponentiate this predicted level of logged crime, to convert it to a crime rate, 2) we then multiply it by the tract population to convert it to a crime count; 3) we sum these crime counts for all tracts in the city; 4) we divide this city-level crime count by the city population to create an estimate of the crime rate; 5) we divide the actual city-level crime rate by this estimated crime rate based on the imputation model (this creates a ratio of the true crime rate to the estimated crime rate); 6) we multiply this ratio by the estimated crime count from step 2; 7) we compute a new logged crime rate for the tract by dividing the value from step 6 by the population of the tract, and log transforming this value. The result of this approach is that we now have a measure of the crime count in each of the tracts in the city that sum up to the proper level of crime in the city, but have levels of crime proportionate to the initial imputation model estimated in equation 1. We then perform these steps multiple times for each of the initial y-hats (10 in this example) for the multiply imputed crime values in tracts.[4]

Analytic models

In the first set of analyses, we estimate several competing models to compare the estimates from our imputation approach against plausible alternative strategies. The "true" model that we will estimate is a multilevel model of crime in neighborhoods nested in cities:

$$y_{ij} = \eta_j + \Gamma X_{ij} + \varepsilon_{ij} \qquad (2)$$

$$\eta_j = B_C Z_j + \varepsilon_j \qquad (3)$$

where y_{ij} is the logged crime rate in the *i*-th tract of *I* tracts in the *j*-th city, η_j is the random city-level component of the logged crime rate in the city, X_{ij} is a matrix of the tract-level variables for each tract *i* in city *j*, Γ shows the effect of these predictors on the logged crime rate, and ε_{ij} is a disturbance term. In equation 3, η_j represents the random estimate of logged violent crime rate in city *j*, Z_j represents a matrix of city-level variables in city *j*, B_C is a vector of their parameters, and ε_j is a disturbance for city *j*. In Z_j we include the five variables described earlier.[5] There was no evidence of collinearity problems in our models, with no variance inflation factors above 4.

We estimate the following models: 1) the "true" model that is a multilevel model of tracts nested in cities (shown in equations 2 and 3); 2) a city-level model (the dominant strategy in the macro criminology research); 3) a city-level model that is weighted by the number of census tracts in the city (to more heavily weight larger cities), with standard errors corrected for

[4] We provide a Stata ado file to create these estimates at the following web location: SUPPRESSED.
[5] The model includes population density at the tract level, which is conceptually distinct from city population size, or the change in population (Hipp and Roussell 2013).

clustering at the city level (this is somewhat analogous to city-level studies that use a weighted least squares estimator (Velez, Krivo, and Peterson 2003); 4) a regression model based on a uniform imputation strategy in which the city-level crime rate is uniformly imputed to all tracts in the city (standard errors are adjusted for city-level clustering)(this is a particularly naïve imputation strategy)[6]; 5) a multilevel model using our model-based imputation approach, but only using a single imputation; 6) a multilevel model using our preferred model-based imputation approach with multiple imputation; 7) a multilevel model using our model-based multiple imputation approach based on coefficients from 2010 data. In these analyses, we are comparing these various competing strategies to the "true" model.

In the second set of analyses, we assessed the out of sample properties of our approach. For this, we first randomly selected 39 of the cities (roughly half), estimated the imputation model of crime in tracts, and then with the remaining cities imputed crime to tracts (based on the estimated imputation model parameters) and then estimated the models with these remaining cities. Thus, the "true" model, our imputed model (both single imputation and multiple imputation), and the uniform imputation strategy (#4 from the first set of analyses) are all estimated on the sample of these remaining cities. We repeated this procedure 500 times to give us 500 random samples. In each sample, we computed the average distance between a particular coefficient in a particular imputation strategy and the coefficient in the "true" model as a measure of average bias, and we computed the average absolute value of this difference across samples as a measure of variability. We then computed the ratio of these differences to the "true" coefficient, to give an estimate of bias or variability in terms proportionate to the actual coefficient. It is worth noting that these ratios are not as meaningful for true coefficients that are

[6] Note that a multilevel model cannot be estimated with this imputation strategy, given that the crime rate does not vary across tracts within a particular city. This is simply a necessary consequence of this imputation strategy, and we therefore estimate it as a linear regression model.

close to zero, as a consequence of the small denominator there can be artificially large ratio values. Given this, we recommend focusing on the ratios for variables with significant parameter estimates in the "true" model.

In the third set of analyses, we used UCR data for 517 cities with a population of at least 50,000 for which we had Census tract data. Here we used the imputation model based on the full sample of 89 cities from the NNCS, and then multiply imputed crime data to the census tracts of these 517 cities. We then estimated a multilevel model using our imputed tract-level data similar to the one in the prior analyses. We compare our results to the more standard approach of simply using the aggregated city-level data. We compared two different city-level models: 1) one that includes just the city-level variables from the multilevel model using our imputed data (equation 3); 2) one that also includes city-level versions of the tract-level variables in the multilevel model. In these analyses we do not have a gold standard "true" model, so we are simply demonstrating the unique insights provided by our approach that accounts for tract-level processes when estimating city-level effects. Furthermore, we argue that our approach contains less stringent assumptions than does the city-level modeling approach.

Results

Comparing in-sample imputation strategies: violent crime

We begin with the models using all available cities from the NNCS. In Table 2, column 1 presents the results from the "true" model, which is a multilevel model in which the outcome variable is the actual logged violent crime rate in the tract, and we include both our tract-level variables as well as the five city-level variables. The tract-level measures generally exhibit the expected relationships with violent crime based on theory and prior research: tracts with higher levels of racial/ethnic heterogeneity, percent Black, percent Latino, income inequality, older housing, average commute time, unemployment, and total and retail employees have higher levels of violent crime. Alternatively, tracts with higher levels of residential stability, occupied units, average household income, percent aged 16 to 29, population density, percent immigrants, and percent households with children all have lower violent crime rates. At the city level, we see that cities with larger populations have more violence, and cities that have experienced a larger increase in population over the prior decade have somewhat more violent crime ($p < .10$).

<<<Table 2 about here>>>

We are particularly interested in comparing these city-level coefficients to the other strategies, and we highlight that in the more common strategy of simply estimating a city-level model (model 2) the coefficient estimates are quite different. The city-level average household income is estimated as a negative relationship in column 2, and appears to be picking up the tract-level negative effect in the "true" model. The coefficient estimate for population size is similar to the "true" model, but the estimate for population change is opposite in sign from the "true" model (though not significant). The estimates for inequality and racial/ethnic heterogeneity are both positive in this model, which may be picking up the neighborhood-level effects in the "true" model.

In model 3 the results are presented for the city-level model weighted by the number of tracts in each city (thus all covariates are measured at the city-level). The effects for average household income and inequality remain similar to model 2. Although the coefficient for population size is positive, it is now only 70% as large as the true model. Racial/ethnic heterogeneity has now become insignificant, which mirrors the true model but indicates that it is not capturing the tract-level effect. And the coefficient for population change is now significantly negative, which is opposite the true model. These results demonstrate that the estimates derived from city-level models (with or without weighting by number of tracts) are substantially different from the true model.

In model 4, we used a rather naïve imputation strategy of simply assigning the city-level violent crime rate to each tract in the city and then estimating a model that mimics the true model using the true tract-level exogenous variables. This approach does not do a very good job estimating the city-level coefficients: average household income has a large negative coefficient (when it was nonsignificant in the true model), change in population has an opposite sign to the true model (but is not significant), and inequality has a significant positive coefficient (when it was nonsignificant in the true model). The model also does a quite poor job of estimating the tract-level coefficients: many of the coefficients are nonsignificant in this model (when they were significant in the true model), and average household income and population density have the incorrect sign. Thus, the approach of assigning the city-level violent crime rate to each tract in the city is quite a poor strategy.

Models 5 and 6 use our preferred imputation strategy of crime into tracts, for single and multiple imputation, respectively. As expected, the coefficient estimates between models 5 and 6 are quite similar. The most notable difference, as expected, is that the standard errors for the tract-level variables are smaller in the single imputation and so they have larger t-values. The t-values in model 5 are closer to those of the true model, but they do not appropriately account for the uncertainty of the imputation. Model 6 uses a multiple imputation strategy and as a consequence, the tract-level coefficients have larger standard errors than the true model (and smaller t-values), more accurately representing the uncertainty of the coefficient estimates (Schafer 1997). The standard errors for the city-level variables are quite similar whether single or multiple imputation is used. Most importantly, the coefficient estimates for model 6 and model 1 (the true model) are generally quite similar, implying that this imputation strategy works as expected.

Comparing in-sample imputation strategies: property crime

Table 3 presents an analogous set of models for the property crime results. In column 1 the “true” model results are presented, and we see that the tract-level measures generally have similar effects as in the violent crime model. In addition, there are more significant city-level effects here compared to the violent crime model. Not only do tracts with higher average income have lower property crime rates, but there is also a contextual effect in which tracts in cities with higher average income have lower property crime rates. Although cities with larger population size do not have more property crime, tracts in cities that have experienced a larger population increase in the last decade do have higher property crime rates. For inequality, there is a reinforcing effect in which tracts with more inequality have more property crime, but also a contextual effect in which tracts in cities with more inequality have more property crime. In contrast, the positive effect of racial/ethnic heterogeneity appears to operate at the tract level, whereas cities with more racial/ethnic heterogeneity have lower levels of property crime, after controlling for these tract- and city-level measures in the model.

<<<Table 3 about here>>>

Column 2 presents the city-level analyses, and we again see different results compared to the true model. Cities with higher average income appear to have considerably less property crime, but this appears to be conflating both the tract- and city-level effects in the true model. The coefficient estimate for population change is the correct sign, but it is less than half the size of the true model and only significant at $p < .10$. Both the city-level inequality and racial/ethnic heterogeneity coefficients appear to conflate the tract- and city-level measures from the true model: for racial/ethnic heterogeneity the positive tract-level coefficient and the negative city-level coefficient from the true model appear to cancel out to an effectively zero coefficient in model 2.

Things are not much better in columns 3 and 4 where cities are weighted by the number of tracts (again, all covariates are measured at the city-level). Population size is now estimated to have a significant negative effect when there was no effect in the true model. The other coefficients in model 3 are similar to the unweighted results in column 2.In model 4 in which we use a naïve imputation strategy of apportioning city-level crime uniformly to all tracts in the city (and the covariates are measured appropriately using tract-level data), the results are quite poor. The tract-level measures are nearly all insignificant, and inequality has the wrong sign. The city-level coefficients exhibit a similar pattern to columns 2 and 3.

In models 5 and 6 we again see that our preferred imputation strategy performs well. Once again, the standard errors for the tract-level measures are smaller in model 5 with the single imputation compared to model 6 with the multiple imputations. The multiple imputation strategy is appropriately accounting for the uncertainty of the imputation. The coefficient estimates for models 5 and 6 are quite similar. We again see the very important result that the coefficient estimates for model 6 and model 1 (the true model) are generally quite similar. This demonstrates that our approach operates as expected, which is encouraging, but not terribly surprising given that we have imputed the crime data from the same sample of cities. We next address how our approach performs using an imputation model based on a different year.

Imputing based on data from a different decade

One possible use of our technique is for estimating longitudinal models based on city-level data back in time. A question is how well our imputation approach might work if the estimated coefficients are based on a different year. To assess this, we estimated the imputation model on a non-random sample of cities in 2010, and assessed the quality of the results compared to the gold standard models in Tables 2 and 3. First, we highlight that the model estimates are indeed different from 2010 compared to our 2000 models, as shown in Appendix Table A1. In 2010, the coefficient for residential stability is 80% larger for violent crime than in 2000; likewise, the coefficients in 2010 for violent crime are larger for percentage aged 16 to 29, and smaller for racial/ethnic heterogeneity, income inequality, population density, percent immigrants, and total employees, and the coefficient for average commute time has an opposite sign. Several coefficients are similarly different in the property crime model. However, what is crucial is the correlation in the *predicted values* for tracts based on models for 2000 vs. 2010 data, and we find that the correlation is .93 for property crime and .98 for violent crime. This implies that using imputations based on data in a different decade may still be useful, and we next compare the coefficient estimates in column 7 of Tables 2 and 3 to the true model.

Of most importance are the city-level coefficients, and we see in Table 2 for violent crime that the estimates in column 7 using the imputation model based on 2010 data are very similar to those in column 6 based on 2000 data. City population size is the only significant coefficient, and the .203 estimate based on a 2010 imputation model is actually slightly closer to the true model than the .195 estimate based on a 2000 imputation model. And the other variables remain insignificant with relatively similar magnitudes. The story is similar for the property crime models in Table 3, as the same three variables are significant in columns 6 and 7 with similar magnitudes. These results for violent and property crime reveal that imputing the data based on a model from a later year is not very problematic in this instance. We next turn to the out of sample imputation in the same year to further assess the robustness of the strategy.

Imputing tract-level crime

Out of sample imputation results

We next ask how our approach does in the more realistic case in which we are imputing crime in tracts for cities outside the sample used to estimate the imputation model, and compare it to the naïve uniform imputation strategy.[7] We pulled 500 samples in which we randomly split the NNCS cities into: a) the 39 imputation model cities, and b) the remaining model estimation cities. In Table 4, we present the averaged results over the samples. For example, for the residential stability tract-level measure, in the uniform imputation strategy model (in which we impute city-level crime uniformly to all tracts in the city) the (absolute) difference between the estimated coefficient and the coefficient in the true model is 73% of the size of the coefficient in the true model, indicating that the inaccuracy is approaching the size of the true coefficient, which is considerably inaccurate. Our approach using either single or multiple imputation does better as the absolute value of the difference is about 45% as large as the actual coefficient from the true model. For many of the other measures the results for our strategy are considerably better: the average absolute value of the difference is 32% of the coefficient for racial/ethnic heterogeneity, about 22% for occupied units and percent Latino, and about 18% for percent Black, average income, and inequality. Of particular importance, the estimates in our approach for the city-level variables that were actually statistically significant in the true model are quite good: for population size the average absolute difference was just 11% of the true coefficient, and for the change in population it was 29% (recall that this measure was only marginally significant). In contrast, the coefficients from the uniform imputation model are further from the true coefficient for population size, and extremely bad for the change in population coefficient.

<<<Table 4 about here>>>

[7] We also estimated the standard city-level model. These results were quite similar to the naïve imputation strategy results.

Imputing tract-level crime

Turning to the property crime results, we again see that the city level coefficients are estimated reasonably well based on our imputation strategy. The average absolute value difference is 21% of the true coefficient for inequality, 20% for average income, and just 18% for the change in population. And the tract-level measures are typically within a similar range of the true model values as they were in the violent crime model. It should be pointed out that for coefficients that were relatively close to zero (particularly when they were not statistically significant in the model), the average absolute value of the difference can be quite high, but this is artificially inflated since the denominator is quite small, and is not of much interest or concern. Once again, in the uniform imputation strategy model the coefficients for all of these variables—including the city-level variables—are considerably far from the true model, implying that it is a poor strategy.

The bottom part of Table 4 shows the average bias for these coefficient estimates. In general, our imputation strategy does not exhibit any notable patterns of bias for various coefficients. In the violent crime model, most of the tract-level variables are within 5% of the true value when aggregated across the 500 samples. The coefficients in the property crime models using our preferred imputation strategy are similarly in line with those of the true model. In contrast, the uniform imputation model yields estimated coefficients that are very biased compared to the true model in both the violent and property crime models.

Example using UCR city-level data

For our final set of analyses, we demonstrate our approach using a sample of 517 cities with UCR crime data and at least 50,000 population. In Table 5 we present the violent crime and property crime multilevel models based on our imputed tract-level crime data (columns 1 and 4) and compare the results with two different city-level models. In column 1, the tract-level measures show the expected effects, which is not surprising given that we have imputed the violent crime data based on the coefficients estimated in the NNCS data. Of particular interest are the city-level variables, which capture these city-level effects when controlling for the neighborhood-level processes. In column 1 we see that there is no relationship between the average income of a city and the violent crime rate once controlling for the tract-level measures (including the negative coefficient for the tract-level average income measure). Cities with more population and greater increase in population ($p < .10$) have higher violent crime rates. The city-level inequality effect is non-significant, although there is a positive relationship between city-level racial/ethnic heterogeneity and violent crime rates.

<<<Table 5 about here>>>

In column 2, we estimated a city-level model that only included the same five city-level measures as in column 1—this would be the typical way to estimate a city-level model with these measures—and there are some notable differences in the estimated coefficients between the two models. In the city-level model, it appears that higher levels of average income are associated with lower violent crime rates, which appears to conflate the tract-level effect detected in the multilevel model in column 1. The estimate for population size looks quite accurate in column 2, but the coefficient estimate for the change in population is significant in the wrong direction in this model, resulting in misleading conclusions. The coefficients for inequality and racial/ethnic heterogeneity are both positive in column 2, and thus appear to conflate the tract- and city-level effects in model 1.

In column 3, we estimated a city-level model in which we also included city-level versions of the tract-level measures in the multilevel model in column 1: although this would not typically be the strategy taken by researchers estimating city-level models, we assessed this model to see if it “corrects” for excluding these meso-level measures. The city-level average household income remains significantly negative, and is conflating the tract-level effect of model 1 with the nonsignificant city-level effect. The population size coefficient is positive, but weaker than column 1 and only significant at $p < .10$. The coefficient for change in population is the opposite sign compared to column 1 (but not significant). Inequality and racial/ethnic heterogeneity again have positive coefficients that conflate the tract- and city-level effects from model 1. Furthermore, residential instability, percent Black, percent Latino, and average age of housing appear to have positive relationships with violent crime at the city level, but in the multilevel model these relationships appear to in fact be at the tract level.

Turning to the property crime models, column 4 presents the results of the multilevel model using our preferred imputation strategy. We see a reinforcing effect in which tracts with higher average household income have less property crime, but there is an additional effect in which cities with higher average income have lower levels of property crime. There is a similar reinforcing positive effect for tract- and city-level inequality. There is no evidence that cities with larger population size have more property crime, but cities with a larger population increase over the prior decade have more property crime. And although tracts with more racial/ethnic heterogeneity have higher levels of property crime, there is no evidence of a city-level effect once accounting for these tract- and city-level measures.

In column 5 we estimated the city-level model that only includes the five city-level variables from column 4. Similar to the violent crime results, we see that the negative coefficient estimate for city-level average income is much larger in the city-level model compared to the multilevel model, implying that it conflates the tract-level effects. The effects for the two population coefficients are quite different from the multilevel model: population size appears to have a somewhat negative relationship with property crime (in contrast to the nonsignificant coefficient estimated in the multilevel model), whereas the change in population is estimated as a nonsignificant relationship (when it was significantly positive in the multilevel model in column 4). Similar to the violent crime results, the estimates for city-level inequality and racial/ethnic heterogeneity in the city-level model appear to conflate the tract- and city-level estimates from the multilevel model.

In column 6 we estimate the city-level model, but also include city-level versions of the tract-level measures in model 4. The results for the main city-level variables are quite similar to those from column 5, and so this specification does not appear to correct the bias. Furthermore, the remaining city-level variables do a relatively poor job of serving as proxies for the tract-level measures in column 4. Several of the tract-level variables that were statistically significant in the multilevel model in column 4 are not significant in column 6 when included as city-level measures. This is not terribly surprising, since we would not necessarily expect there to be a straightforward linear aggregation to larger geographic units from smaller ones, but it does highlight that making any such linear aggregation assumptions is not appropriate.

Do city-level measures simply capture combined meso- and city-level effects?

As one final consideration, we ask whether one can interpret the coefficients from the city-level model as capturing the total effects of the tract- and city-level measures in the multilevel model. We find that this is not the case. To assess this, we computed the marginal estimated change in violent crime for a one standard deviation change in the city-level variable for the model 3 results, and compared that to the marginal estimated change in violent crime for a one standard deviation change in the tract-level variable and a one standard deviation change in the analogous city-level variable for the model 1 results and compared them by computing their ratio (analogous computations were done for the property crime models in columns 6 and 4). The ratios for the violent crime model are shown in column 7, and those for the property crime model are shown in column 8. While some yielded somewhat similar results (as the ratios were close to 1), others were quite different. For example, the total effects of residential stability in the city-level model are 85.8% of the multilevel model for violent crime, which implies a relatively reasonable capturing of the total effects. However, the total effects for percent occupied units are slightly positive, though they are negative in the multilevel model, implying a completely incorrect conclusion. The total effect of percent Black in the city-level model is about half the size of the effect in the multilevel model, population density and percent households with children are about 40% of the size in the multilevel model, and retail employees are essentially zero in the city-level model but show a very strong positive relationship in the multilevel model.

In the property crime model, the effects in the city-level model for several variables are only about half as strong as the multilevel model, including residential stability, percent Black, percent Latino, average age of housing, and retail employees. The effects for population density and the unemployment rate are close to zero in the city-level model but are relatively strong effects in the multilevel model. There is clearly no reasonable way to translate the results from a city-level model to the information that can be obtained from the multilevel model, indicating that there is unique information available from our imputation approach.

Conclusion

We have introduced a new technique that allows researchers to assess the relationship between macro-level constructs and crime while taking into account sub-city-level processes. We have demonstrated that prior macro-level work failing to account for these lower-level geographic processes obtains results that, at best, appear to conflate macro and meso level processes, and, at worst, yield biased results that are incorrect. Our approach adopts insights from micro- or meso-level studies to impute crime data at these smaller geographic units and then estimates multilevel models to appropriately capture macro-level effects. Of course, a preferable strategy is for researchers to obtain micro- or meso-level data for a large number of cities and directly estimate such models. Although these data may well become more available from many city police departments over time, such data take considerable effort to clean and also do not allow exploring more long-term macro research questions.

We demonstrated that our approach appears to work well. It works as expected on a sample of cities with meso-level crime data. We also demonstrated how the approach works in what would be a typical strategy using the approach by estimating the multilevel model on one sample of cities, and then estimating the models of interest on a different sample of cities. Our results showed that the approach worked well, as we obtained coefficient estimates at the macro-level that were reasonably in line with the true model. Importantly, our approach yielded particularly accurate estimates of coefficients for city-level measures even when building the imputation model on a sample of cities separate from those on which the model is estimated. This mimics the typical situation in which researchers would use our approach, providing confidence in the strategy. Notably, the results highlighted that the more common strategy of estimating city-level models while ignoring processes at smaller geographic scales (meso-level processes in this study) yield very different results compared to our approach.

As we mentioned earlier, our strategy generalizes in a straightforward manner to building models based on other sub-city units, such as street blocks. The only limitation is obtaining such data for enough cities, but that is rapidly becoming more feasible with the increasing number of agencies making such data publicly available. Our approach would be used in exactly the same way as demonstrated here. Similarly, such data could also be used to estimate models that combined both micro- and meso-level measures, and then used those estimates in the imputation procedure we describe here. Note that a question is whether including micro-level information in the imputation model would yield notably different city-level coefficients compared to a meso-level imputation model. That is, even though there is evidence that crime clusters in such micro locations, the important question is whether accounting for these micro locations in the imputation model would change the city-level coefficient estimates. If the characteristics of micro-locations simply *shift* the location of crime, this will have little impact on the type of results we showed here using meso-level analyses; it is only to the extent that micro-locations actually *change* the amount of crime that the results would be consequential (for a discussion of this general issue, see Hipp 2011: 634-35). This is an empirical question that needs to be addressed in future work.

An important question then is what are the consequences of the results presented here for the large body of existing studies measured entirely at the macro level? From one point of view, such existing analyses have quite possibly yielded biased results that are untrustworthy. This strong conclusion would be the case if one believes the multilevel models we have estimated are correct. In this view, prior macro-level studies failed to account for meso-level processes, producing untrustworthy coefficients. As we have demonstrated, the bias can be quite extreme: we showed examples in which conclusions about variables are completely reversed when estimating a multilevel model rather than ignoring such meso-level effects. For example, in the property crime model we found that the size of the effect for the change in city-level population over the prior decade was over twice as strong in the multilevel model compared to the city-level model, and for violent crime the city-level model returned a coefficient with the wrong sign. Furthermore, for variables whose mechanisms might operate at either the meso- or macro-level, the city-level model can yield coefficients that do not distinguish between these two possibilities. These results are indeed disturbing for interpreting the results of existing studies.

There is, however, another interpretation less critical of existing macro-level studies. If meso-level processes can be viewed as *mechanisms* of macro-level measures, then accounting for them in the model is not appropriate. For example, if one could argue that the growth in population over a decade impacted crime levels because it systematically changed certain neighborhood measures—such as residential instability—and in doing so caused more crime in these specific neighborhoods, then one would not want to control for these specific sub-city-level measures in the model. These competing possibilities raise fundamental theoretical questions and cannot be adjudicated through statistical tests. These distinctions do, however, highlight the need for scholars to carefully consider the geographic levels at which processes operate, how they operate, and further, if they operate at multiple geographic levels simultaneously. These are certainly challenging issues, but ones that scholars need to more carefully consider (Hipp and Williams 2020).

While we have argued that our approach is useful for scholars given the number of important macro-level research questions that need to be addressed, another important advantage of our approach is that it enables scholars to address long-term longitudinal questions that are feasible given the city-level crime data stretching back to at least 1960. This long-term data is underutilized, and we argue that our strategy is important for scholars to utilize when exploring longitudinal questions with such macro-level data. While it would still be quite useful for studies to collect sub-city-level crime data over a large number of cities, typically such data is only available for a shorter number of years, limiting the possibility of longitudinal studies. At best, studies can typically only focus on changes in crime over a decade or so. The availability of city-level data over six decades provides the opportunity for research questions that consider how macro processes may have changed over time (Baumer, Velez, and Rosenfeld 2018). Our results highlight that scholars wishing to explore such research questions would be advised to not ignore meso-level processes. A necessary assumption for such longitudinal models is that the imputation model at the micro- or meso-level at one point in time would operate satisfactorily at another point in time. It is certainly possible that over time there could be large changes in the coefficients at the micro- or meso-level. Nonetheless, the key question for our approach is how well the model *predicts* over time, not the stability of the coefficients. Furthermore, it was encouraging in our demonstration that when we used an imputation model from ten years later that our approach still performed quite satisfactorily. Notably, the imputation model from the wrong time point also greatly outperformed the traditional approaches to estimating macro-level models.

We acknowledge some limitations of this study. First, our approach relies on the proper sub-city-level imputation model. We highlight that our concern with misspecification only extends to the quality of the predicted crime rate within neighborhoods, and not the more common concern about biased coefficients. Still, further research determining how sensitive the strategy is to this assumption will be useful. Second, the approach has limited ability to test cross-level interactions given that an imputation model would need to include these interactions, which would build these imputations into the final estimated models. However, although we have not done so here, in principle one could build such cross-level interactions into the imputation model and use this information when imputing the crime data. We leave such an extension to future work. Third, although we demonstrated that our approach worked well even using data from a different decade for the imputation model, whether the assumption that an imputation model could work reasonably well even further back in time to explore long-term longitudinal research questions is an open question.

We have highlighted the importance for scholars to continue focusing on macro-level research questions. In addition to the reasons for this that we highlighted earlier, we also point out that the routine policy implications from neighborhood-level research are often too broad and vague to be enacted. For example, a clear implication of social disorganization research is that in general, neighborhood levels of poverty and residential instability are consequential for crime. While tackling poverty and instability are laudable goals, these represent "wicked" problems (Brown, Harris, and Russell 2010) that are not easily amenable to local intervention. Some argue that the proper level of intervention is the block, where policymakers and community advocates can adopt a "small wins" approach (Taylor 2015). But given what we know about spatial effects, such interventions are likely to be stymied by not only persistent structural disadvantage in the focal neighborhood, but criminogenic conditions in nearby neighborhoods as well. Thus, building a collective knowledge as to the *unique* effects of city characteristics on crime, distinct from those at the neighborhood-level, should provide insights to aid in the development of city policy.

Although macro-level research questions are important to criminology scholars, such questions should not be explored at the expense of ignoring sub-city-level processes that occur simultaneously. We have introduced a strategy that imputes city-level crime data to the sub-city-units within the city, allowing for more appropriate multilevel analyses. Our analyses demonstrated that the traditional approach to estimating city-level models that ignores sub-city effects can yield biased parameter estimates. We believe that our approach will be useful to researchers as they turn back to asking, and potentially answering, macro-level questions about the distribution of crime. Furthermore, the ability to address such macro questions over very long time periods should also be able to provide key insights for the field.

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Tables and Figures

Table 1. Summary statistics of variables used in analyses

	NNCS sample cities				UCR Cities			
	Tract level measures		City level measures		Tract level measures		City level measures	
Dependent variables	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Violent crime rate (logged)	2.13	0.92			6.14	1.21	6.07	0.78
Property crime rate (logged)	3.90	0.73			8.22	0.84	8.34	0.46
Residential stability	0.00	1.00	-0.16	1.04	-0.17	1.04	-0.29	0.91
Racial/ethnic heterogeneity	38.35	19.83	52.14	13.96	39.65	19.61	46.04	16.74
Percent occupied units	92.83	6.07	93.26	3.08	93.40	6.24	94.15	3.24
Percent Black	25.83	32.80	18.96	16.64	21.31	29.93	12.96	16.18
Percent Latino	20.51	25.62	19.36	18.86	19.11	23.47	17.47	18.42
Inequality (income or home values)	41.97	6.81	32.14	6.51	41.50	6.99	28.85	6.48
Average household income (logged)	10.74	0.45	10.86	0.23	10.78	0.44	10.90	0.26
Percent aged 16 to 29	23.20	8.33	23.66	3.88	23.18	9.43	23.22	5.67
Average age of housing	38.66	13.62	34.48	10.63	38.01	13.70	33.06	10.31
Average commute time	25.59	6.44	23.53	3.89	25.60	8.14	22.91	5.15
Population density	8.76	8.95	4.41	2.75	11.57	18.38	4.20	4.01
Percent immigrants	15.87	16.07	15.40	12.25	16.68	16.13	14.58	12.02
Percent households with children	50.16	12.19	51.18	4.67	50.08	12.02	51.41	5.63
Unemployment rate	8.59	7.02	6.89	2.79	8.28	7.05	6.52	2.74
Total employees	2.01	4.42	2.16	0.66	2.02	4.32	2.10	0.86
Retail employees	0.41	0.78	0.45	0.17	0.42	0.79	0.48	0.22
Population (per million)			0.43	0.56			0.17	0.44
Change in population in prior decade			0.14	0.21			1.18	0.31
N	9502		89		22,445		517	

Table 2. Violent crime models, comparing different imputation techniques and modeling techniques

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	True model		City-level model		City-level model (pop-weighted)		Tract-level model (city crime with uniform distribution)		Tract-level model (single imputation)		Tract-level model (multiple imputation)		Tract-level model (multiple imputation) (2010 coefs)	
City-level variables														
Average household income (logged)	-0.061		-1.279	**	-1.080	**	-1.224	**	0.004		-0.044		-0.054	
	-(0.38)		-(6.47)		-(4.28)		-(5.75)		(0.03)		-(0.27)		-(0.33)	
Population (per million)	0.212	**	0.241	**	0.146	**	0.125	**	0.193	**	0.195	**	0.203	**
	(3.40)		(3.08)		(2.84)		(4.00)		(3.20)		(3.17)		(3.40)	
Change in population in prior decade	0.310	†	-0.360		-0.644	*	-0.245		0.244		0.282		0.230	
	(1.71)		-(1.63)		-(2.29)		-(0.93)		(1.39)		(1.57)		(1.27)	
Home value inequality (Gini)	0.006		0.020	**	0.029	**	0.028	**	0.007		0.006		0.006	
	(1.05)		(2.75)		(4.00)		(4.58)		(1.22)		(1.05)		(1.01)	
Racial/ethnic heterogeneity	-0.003		0.008	*	0.003		0.001		-0.002		-0.003		-0.002	
	-(1.27)		(2.50)		(0.55)		(0.17)		-(1.01)		-(1.23)		-(0.70)	
Tract-level variables														
Residential stability	-0.100	**					-0.030	†	-0.082	**	-0.092	**	-0.191	**
	-(11.58)						-(1.75)		-(9.50)		-(7.08)		-(11.43)	
Racial/ethnic heterogeneity	0.005	**					0.001		0.004	**	0.005	**	0.002	**
	(15.98)						(0.80)		(14.03)		(12.14)		(4.87)	
Percent occupied units	-0.016	**					0.001		-0.018	**	-0.015	**	-0.018	**
	-(15.53)						(0.24)		-(17.08)		-(9.52)		-(8.92)	
Percent Black	0.011	**					0.003	**	0.011	**	0.011	**	0.013	**
	(40.47)						(2.81)		(39.83)		(29.09)		(27.08)	
Percent Latino	0.009	**					0.000		0.009	**	0.009	**	0.009	**
	(21.56)						-(0.10)		(21.46)		(14.50)		(10.99)	

Income inequality (Gini)	0.014	**					0.000		0.014	**	0.014	**	0.005	**
	(14.26)						-(0.12)		(15.05)		(10.72)		(2.87)	
Average household income (logged)	-0.499	**					0.136	*	-0.530	**	-0.510	**	-0.441	**
	-(26.73)						(2.56)		-(28.31)		-(20.52)		-(13.63)	
Percent aged 16 to 29	-0.005	**					-0.001		-0.006	**	-0.005	**	-0.008	**
	-(5.92)						-(0.53)		-(7.01)		-(4.48)		-(5.59)	
Average age of housing	0.018	**					0.004	*	0.015	**	0.017	**	0.017	**
	(30.45)						(2.14)		(26.46)		(19.69)		(15.30)	
Average commute time	0.004	**					0.011	**	0.003	*	0.004	**	-0.010	**
	(3.66)						(3.75)		(2.22)		(2.62)		-(5.93)	
Population density	-0.008	**					0.003	*	-0.006	**	-0.008	**	-0.003	†
	-(10.38)						(2.18)		-(7.98)		-(5.13)		-(1.74)	
Percent immigrants	-0.003	**					0.000		-0.004	**	-0.003	*	-0.001	
	-(4.47)						(0.13)		-(5.10)		-(2.43)		-(0.41)	
Percent households with children	-0.003	**					-0.002	†	-0.003	**	-0.003	**	-0.005	**
	-(5.84)						-(1.77)		-(6.08)		-(4.50)		-(5.14)	
Unemployment rate	0.010	**					0.000		0.008	**	0.010	**	0.007	**
	(9.55)						(0.28)		(7.23)		(5.89)		(3.17)	
Total employees	0.015	**					0.002		0.018	**	0.016	**	0.007	*
	(9.82)						(1.31)		(11.76)		(5.61)		(2.16)	
Retail employees	0.165	**					0.002		0.136	**	0.157	**	0.168	**
	(19.77)						(0.38)		(16.27)		(10.02)		(8.51)	
Intercept	7.512	**	14.778		12.733		12.449	**	7.376	**	7.413	**	7.799	**
	(4.27)		(6.87)		(4.66)		(6.18)		(4.33)		(4.23)		(4.48)	
R-square			0.674		0.608		0.665							
N of tracts	8257				8267		8257		8257		8267		8267	
N of cities	78		78		78		78		78		78		78	

*Note: ** p < .01; * p < .05; † p < .10. T-values in parentheses.*

Table 3. Property crime models, comparing different imputation techniques and modeling techniques

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	True model		City-level model		City-level model (pop-weighted)		Tract-level model (city crime with uniform distribution)		Tract-level model (single imputation)		Tract-level model (multiple imputation)		Tract-level model (multiple imputation) (2010 coefs)	
City-level variables														
Average household income (logged)	-0.539	**	-1.119	**	-1.021	**	-0.918	**	-0.467	**	-0.521	**	-0.439	**
	-(3.95)		-(7.04)		-(5.30)		-(4.82)		-(3.58)		-(3.55)		-(2.69)	
Population (per million)	0.036		-0.053		-0.104	*	-0.074	†	0.001		0.009		0.031	
	(0.70)		-(0.85)		-(2.18)		-(1.74)		(0.03)		(0.17)		(0.57)	
Change in population in prior decade	0.707	**	0.324	†	0.528	**	0.511	**	0.677	**	0.717	**	0.836	**
	(4.58)		(1.82)		(2.94)		(2.60)		(4.59)		(4.43)		(4.68)	
Home value inequality (Gini)	0.014	**	0.025	**	0.023	**	0.021	**	0.014	**	0.012	*	0.011	*
	(3.05)		(4.46)		(4.14)		(4.11)		(3.02)		(2.36)		(2.20)	
Racial/ethnic heterogeneity	-0.004	*	-0.001		-0.001		0.000		-0.004	†	-0.004	†	-0.003	
	-(2.13)		-(0.53)		-(0.46)		-(0.07)		-(1.82)		-(1.93)		-(1.23)	
Tract-level variables														
Residential stability	-0.160	**					-0.042	*	-0.144	**	-0.153	**	-0.217	**
	-(18.72)						-(2.15)		-(16.70)		-(12.41)		-(11.86)	
Racial/ethnic heterogeneity	0.004	**					0.000		0.004	**	0.004	**	0.003	**
	(14.70)						-(0.82)		(13.03)		(11.27)		(4.38)	
Percent occupied units	-0.008	**					0.002		-0.010	**	-0.008	**	-0.012	**
	-(7.69)						(0.88)		-(9.25)		-(4.81)		-(4.93)	
Percent Black	0.004	**					0.001		0.004	**	0.004	**	0.007	**
	(13.94)						(1.17)		(13.54)		(10.72)		(12.12)	
Percent Latino	0.004	**					0.001		0.004	**	0.004	**	0.004	**
	(9.19)						(1.11)		(9.07)		(6.60)		(4.28)	

Income inequality (Gini)	0.009	**					-0.002	*	0.010	**	0.009	**	0.001	
	(9.40)						-(2.00)		(9.84)		(7.46)		(0.34)	
Average household income (logged)	-0.338	**					-0.020		-0.371	**	-0.344	**	-0.212	**
	-(18.11)						-(0.76)		-(19.73)		-(13.12)		-(5.53)	
Percent aged 16 to 29	-0.002	*					0.001		-0.003	**	-0.002		-0.007	**
	-(2.02)						(0.70)		-(3.31)		-(1.52)		-(4.36)	
Average age of housing	0.014	**					0.001		0.012	**	0.014	**	0.017	**
	(24.88)						(1.08)		(21.09)		(16.16)		(13.48)	
Average commute time	-0.003	**					-0.002		-0.006	**	-0.005	**	-0.029	**
	-(2.78)						-(0.44)		-(4.87)		-(2.89)		-(12.42)	
Population density	-0.017	**					-0.003		-0.016	**	-0.017	**	-0.008	**
	-(22.91)						-(1.16)		-(20.50)		-(12.21)		-(3.81)	
Percent immigrants	-0.005	**					-0.005	**	-0.006	**	-0.005	**	-0.003	
	-(7.49)						-(3.42)		-(8.37)		-(4.21)		-(1.61)	
Percent households with children	-0.009	**					-0.001		-0.009	**	-0.009	**	-0.008	**
	-(17.45)						-(1.49)		-(17.04)		-(13.45)		-(7.72)	
Unemployment rate	0.001						-0.001		-0.001		0.001		0.003	
	(0.64)						-(1.19)		-(1.26)		(0.35)		(1.16)	
Total employees	0.014	**					0.000		0.018	**	0.015	**	0.002	
	(9.41)						-(0.19)		(11.69)		(5.63)		(0.55)	
Retail employees	0.280	**					0.003		0.253	**	0.274	**	0.286	**
	(33.00)						(0.49)		(29.55)		(18.87)		(13.29)	
Intercept	13.124	**	15.342		14.391		13.494	**	12.979	**	13.039	**	11.721	**
	(8.83)		(8.83)		(7.03)		(6.70)		(9.15)		(8.30)		(6.83)	
R-square			0.516		0.443		0.487							
N of tracts	9502				9512		9502		9502		9512		9512	
N of cities	89		89		89		89		89		89		89	

*Note: ** p < .01; * p < .05; † p < .10. T-values in parentheses.*

Table 4. Assessing bias and absolute value of difference between true coefficient and estimated coefficient based on imputed data in out of sample cities, violent and property crime models

	Violent crime model				*Property crime model*		
	Uniform imputation strategy	Tract model: single imputation	Tract model: multiple imputation		Uniform imputation strategy	Tract model: single imputation	Tract model: multiple imputation
Average absolute value of difference							
City-level variables							
Logged average household income	20.298	1.859	1.680		0.751	0.220	0.203
Population	0.341	0.131	0.113		1.719	0.847	0.888
Change in population 1990-2000	1.802	0.292	0.290		0.358	0.194	0.182
Home value inequality	3.905	0.501	0.485		0.529	0.246	0.206
Racial/ethnic heterogeneity	0.872	0.251	0.232		0.270	0.094	0.078
Tract-level variables							
Residential stability	0.727	0.452	0.459		0.803	0.296	0.296
Racial/ethnic heterogeneity	0.899	0.331	0.316		1.036	0.232	0.221
Percent occupied units	1.009	0.220	0.231		1.113	0.412	0.399
Percent Black	0.799	0.183	0.174		0.855	0.365	0.366
Percent Latino	1.059	0.244	0.215		0.840	0.626	0.619
Income inequality	0.984	0.187	0.151		1.167	0.329	0.314
Average household income (logged)	1.222	0.189	0.180		0.904	0.281	0.276
Percent aged 16-29	0.801	0.592	0.607		1.403	1.629	1.541
Average age of housing	0.785	0.092	0.088		0.940	0.134	0.128
Average commute time	1.559	0.873	0.817		0.941	1.049	0.982
Population density	1.328	0.445	0.446		0.820	0.245	0.244
Percent immigrants	1.364	1.234	1.101		0.467	0.565	0.558
Percent with children	0.542	0.605	0.600		0.922	0.264	0.256
Unemployment rate	0.956	0.290	0.220		3.825	4.382	3.917
Total employees	0.892	0.387	0.393		1.044	0.498	0.485
Retail employees	0.993	0.173	0.161		0.993	0.158	0.155

Average difference (bias)							
City-level variables							
Logged average household income	20.298	-0.305	-0.113		0.751	0.009	0.024
Population	0.328	0.110	0.084		1.719	0.689	0.763
Change in population 1990-2000	1.801	0.095	0.139		0.340	0.002	-0.026
Home value inequality	-3.905	-0.022	-0.038		-0.520	0.212	0.172
Racial/ethnic heterogeneity	-0.841	-0.003	0.030		-0.270	0.019	-0.002
Tract-level variables							
Residential stability	-0.723	-0.118	-0.088		-0.803	-0.040	-0.035
Racial/ethnic heterogeneity	0.899	0.122	0.047		1.036	-0.002	0.003
Percent occupied units	-1.009	-0.002	-0.016		-1.113	0.038	0.039
Percent Black	0.799	0.042	0.032		0.855	0.034	0.038
Percent Latino	1.059	0.092	0.034		0.833	0.053	0.050
Income inequality	0.984	-0.127	-0.011		1.167	-0.015	-0.014
Average household income (logged)	-1.222	0.082	0.028		-0.904	0.018	0.019
Percent aged 16-29	-0.790	-0.046	0.026		-1.274	-0.060	0.006
Average age of housing	0.785	0.030	0.018		0.940	0.003	0.002
Average commute time	-1.479	-0.127	-0.039		0.058	-0.037	-0.019
Population density	-1.328	0.039	-0.006		-0.820	0.004	-0.001
Percent immigrants	-1.280	-0.473	-0.185		-0.372	-0.068	-0.059
Percent with children	-0.392	-0.269	-0.108		-0.922	-0.034	-0.035
Unemployment rate	0.956	0.217	0.066		3.461	0.106	-0.006
Total employees	0.892	-0.175	-0.075		1.044	-0.018	-0.022
Retail employees	0.993	0.043	0.021		0.993	-0.018	-0.016

Table 5. Violent and property crime models comparing cities based on imputed tract-level crime data, smaller level 2 model specification

	Violent crime models			Property crime models			Ratio of total effects	
City-level variables	Multi-level model	City-level model	City-level model	Multi-level model	City-level model	City-level model	Violent crime	Property crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average household income (logged)	-0.2048 *	-1.5112 **	-1.1316 **	-0.3908 **	-0.9068 **	-0.8012 **	68.3%	74.0%
	-(2.20)	-16.07	-7.22	-(5.65)	-15.16	-7.7		
Population (per million)	0.1251 **	0.1320 *	0.1075 *	0.0459	-0.0566 †	0.0208		
	(2.66)	(2.47)	(2.11)	(1.29)	-(1.67)	(0.62)		
Change in population in prior decade	0.0659	-0.2715 **	-0.1129	0.2797 **	0.0884 †	0.0698		
	(0.86)	-(3.51)	-(1.19)	(4.98)	(1.80)	(1.11)		
Home value inequality	-0.0027	0.0168 **	0.0117 *	0.0077 **	0.0307 **	0.0152 **	63.9%	78.1%
	-(0.76)	(4.41)	(2.45)	(2.84)	(12.72)	(4.80)		
Racial/ethnic heterogeneity	0.0061 **	0.0205 **	0.0097 **	-0.0012	0.0021 *	0.0039 **	94.5%	74.9%
	(4.13)	(14.66)	(5.49)	-(1.02)	(2.40)	(3.33)		
Tract-level variables								
Residential stability	-0.1179 **		-0.1795 **	-0.1659 **		-0.0911 **	85.8%	48.9%
	-(14.10)		-(3.92)	-(27.63)		-(3.00)		
Racial/ethnic heterogeneity	0.0073 **			0.0046 **				
	(13.32)			(10.58)				
Percent occupied units	-0.0162 **		0.0000	-0.0078 **		-0.0064	-40.0%	65.1%
	-(14.46)		(0.00)	-(8.71)		-(0.91)		
Percent Black	0.0135 **		0.0161 **	0.0039 **		0.0048 **	52.7%	47.2%
	(34.75)		(7.25)	(13.35)		(3.29)		
Percent Latino	0.0117 **		0.0086 **	0.0039 **		0.0016	67.2%	28.9%
	(20.20)		(4.09)	(8.83)		(1.15)		
Income inequality (Gini)	0.0186 **			0.0096 **				
	(17.79)			(11.88)				
Average household income (logged)	-0.7313 **			-0.3500 **				
	-(28.70)			-(18.24)				

Percent aged 16 to 29	-0.0037	**			-0.0261	**	-0.0016	*			-0.0132	**
	-(3.95)				-(4.84)		-(2.46)				-(3.67)	
Average age of housing	0.0238	**			0.0241	**	0.0151	**			0.0076	**
	(29.10)				(5.48)		(25.82)				(2.61)	
Average commute time	0.0061	**			0.0119	†	-0.0034	**			-0.0078	†
	(4.39)				(1.82)		-(3.23)				-(1.81)	
Population density	-0.0112	**			-0.0168	*	-0.0193	**			-0.0071	
	-(23.36)				-(2.03)		-(46.20)				-(1.29)	
Percent immigrants	-0.0034	**			0.0020		-0.0052	**			-0.0058	*
	-(5.02)				(0.55)		-(9.75)				-(2.40)	
Percent households with children	-0.0059	**			-0.0067		-0.0095	**			-0.0039	
	-(7.53)				-(1.20)		-(18.26)				-(1.05)	
Unemployment rate	0.0067	**			0.0152		0.0005				0.0019	
	(5.48)				(1.08)		(0.48)				(0.20)	
Total employees	0.0126	**			0.0368		0.0138	**			0.0191	
	(6.78)				(0.85)		(8.55)				(0.67)	
Retail employees	0.2200	**			0.0862		0.2831	**			0.3318	**
	(20.04)				(0.48)		(31.50)				(2.76)	
Intercept	15.0671	**	21.4118	**	17.0444	**	15.7773	**	17.1370	**	17.1847	**
	(15.18)		(20.89)		(9.05)		(21.46)		(26.29)		(13.76)	
N of tracts	22,445						22,445					
N of cities	517		517		517		517		517		517	

*Note: ** p < .01; * p < .05; † p < .10. T-values in parentheses.*

Appendix

Table A1. Comparing imputation model results from 2000 and 2010 (fixed effects models)

	(1)		(2)		(3)		(4)	
	Violent crime 2000		Violent crime 2010		Property crime 2000		Property crime 2010	
Tract-level variables								
Residential stability	-0.100	**	-0.180	**	-0.160	**	-0.224	**
	-(11.54)		-(24.71)		-(18.60)		-(22.96)	
Racial/ethnic heterogeneity	0.005	**	0.002	**	0.004	**	0.003	**
	(15.95)		(7.28)		(14.69)		(6.46)	
Percent occupied units	-0.016	**	-0.018	**	-0.008	**	-0.012	**
	-(15.52)		-(26.01)		-(7.64)		-(12.60)	
Percent Black	0.011	**	0.012	**	0.004	**	0.006	**
	(40.27)		(46.70)		(13.84)		(18.20)	
Percent Latino	0.009	**	0.008	**	0.004	**	0.003	**
	(21.54)		(24.66)		(9.03)		(8.00)	
Income inequality (Gini)	0.014	**	0.004	**	0.009	**	0.000	
	(14.26)		(6.47)		(9.50)		(0.35)	
Average household income (logged)	-0.497	**	-0.374	**	-0.335	**	-0.193	**
	-(26.50)		-(22.02)		-(17.83)		-(8.50)	
Percent aged 16 to 29	-0.005	**	-0.008	**	-0.002	*	-0.007	**
	-(5.94)		-(12.36)		-(2.03)		-(8.54)	
Average age of housing	0.018	**	0.016	**	0.015	**	0.018	**
	(30.27)		(35.53)		(24.80)		(30.22)	

Average commute time	0.004	**	-0.009	**	-0.003	**	-0.027	**
	(3.60)		-(8.26)		-(2.58)		-(18.29)	
Population density	-0.008	**	-0.003	**	-0.017	**	-0.008	**
	-(10.36)		-(11.69)		-(22.79)		-(20.77)	
Percent immigrants	-0.003	**	-0.001		-0.005	**	-0.003	**
	-(4.40)		-(1.50)		-(7.18)		-(3.89)	
Percent households with children	-0.003	**	-0.004	**	-0.009	**	-0.008	**
	-(5.85)		-(9.20)		-(17.32)		-(13.45)	
Unemployment rate	0.010	**	0.007	**	0.001		0.003	*
	(9.61)		(8.29)		(0.69)		(2.48)	
Total employees	0.015	**	0.006	**	0.014	**	0.001	
	(9.80)		(5.45)		(9.41)		(0.56)	
Retail employees	0.165	**	0.158	**	0.280	**	0.291	**
	(19.75)		(21.15)		(32.99)		(29.23)	
Intercept	7.138	**	6.499	**	7.608	**	6.685	**
	(28.34)		(28.88)		(30.09)		(22.18)	
R-square	0.713		0.627		0.513		0.370	
N of tracts	8257		11715		9502		12161	

*Note: ** p < .01; * p < .05; † p < .10. T-values in parentheses.*