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URBAN POVERTY AND THE ONSET OF THE CORONAVIRUS PANDEMIC: EVIDENCE FROM AMERICAN CITIES

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This article investigates empirically whether urban poverty in American cities has affected the spread of COVID-19 at the early onset of the pandemic and whether such an effect was mitigated or amplified by mobility restriction policies. Using ACS data combined with data on mobility and confirmed cases, and after addressing bias arising from measurement error and unobserved confounders, we find that an increase in urban poverty is associated with a rise in COVID-19 cases. stay-at-home orders are found to be ineffective and instead reinforce the speed of contagion in cities where poverty is less evenly distributed across neighborhoods.

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1. INTRODUCTION

The worldwide outbreak of the Coronavirus pandemic has produced dramatic humanitarian and economic consequences, particularly impacting urban areas. Numerous studies have investigated the potential drivers of the pandemic onset worldwide. Concerning the United States, Desmet & Wacziarg (2021) identify

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demographics, transportation, and the presence of nursing home residents as important drivers of the pandemic. Using zip-code data, Benitez et al. (2020) find a positive correlation between the share of black and Hispanic residents and the number of confirmed cases. The geographic distribution of individuals with heterogeneous characteristics in other dimensions, such as income or poverty, is associated with the incidence of the pandemic.

In this article, we empirically investigate the extent to which the early onset of the Coronavirus pandemic in American cities is driven by the degree of incidence and distribution of poverty across a city's neighborhoods, which we measure by mean of *urban poverty* indices. The literature has brought about evidence about the correlation between poverty incidence and the Coronavirus pandemic, highlighting the underlying mechanisms. First, income influences residential choices and is associated with higher concentrations of poverty in specific areas of the city. As shown by Wu & McGoogan (2020), housing characteristics, population density, and the extent of urbanization are relevant for explaining the likelihood of exposure to the disease. This happens because lower-income households tend to be larger in size and encompass multiple generations. These families frequently reside in dwellings with limited space and rely more heavily on public transportation. This combination of factors increases the frequency of interpersonal interactions and reduces opportunities for self-isolation. Second, income correlates with the intensity and quality of interactions. Brown & Ravallion (2020) show that the cost of reducing social distancing is higher in poorer and more unequal counties. Lower compliance with distancing is also found in more densely populated counties (Jung et al., 2021) and with higher poverty rates (Chiou & Tucker, 2020), which also displayed higher rates of mobility during the COVID-19 early outbreak (Yilmazkuday, 2023). A possible explanation is that poor urban residents are more likely to commute more hours, use public transport, be employed in low-skill jobs, spend more time away from home, and live in smaller, overcrowded houses (Eichenbaum et al., 2022; Jay et al., 2020; Lou et al., 2020; Ruiz-Euler et al., 2020).

Overall, empirical evidence suggests that poor individuals were more vulnerable to the Coronavirus pandemic, not only as a consequence of their individual capabilities of limiting social contacts, but also by the effect of the extent to which poverty was concentrated (and hence experienced) in their place of residence. We contribute to the literature by providing evidence about this specific channel.

We adopt an econometric strategy to identify and estimate the effect of an exogenous change in urban poverty on the spread of the Coronavirus pandemic. The primary dependent variables measure the daily incidence of confirmed COVID-19 cases in American urban counties from February 2020 to April 29, 2020, that is, three weeks after the implementation of all stay-at-home orders across the United States. We select this time window to analyze the evolution of the Coronavirus pandemic before, during, and immediately after the introduction of travel bans and mobility restrictions. Focusing on the incept of the pandemic's first wave has clear identification advantages. First, due to the unexpected nature of the first pandemic wave, we observed no significant effects on the mobility choices of urban residents before the implementation of stay-at-home orders. Hence, our estimates are, to a large extent, unaffected by behavioral responses (e.g., reduced participation in public and social events) that might have consequences for spreading the pandemic. Second, by focusing on the early incidence period, it is reasonable to use available data on the distribution of poverty in cities and the county-specific covariates recorded in the pre-pandemic period to describe the drivers of the pandemic, assuming that the urban social composition observed in February–April 2020 is not dissimilar to what observed in the pre-pandemic period. Lastly, once mobility restrictions have been put in place, their effect becomes indistinguishable from the impact of other drivers of the pandemic.

Our treatment variable corresponds to urban poverty measures axiomatically characterized in Andreoli et al. (2021). A measure of urban poverty is an index assigning a number, understood as the level of urban poverty displayed by a city, to the distribution of poor and nonpoor residents in the neighborhoods of that city. Our preferred measure of urban poverty, the *UP* index, captures three aspects of the spatial distribution of poverty: the incidence, the distribution of poverty across neighborhoods where poverty is more concentrated, and the extent of segregation of poor and nonpoor individuals across high- and low-poverty neighborhoods.

For identification, we adopt an instrumental variable approach to address measurement errors in the number of COVID-19 cases and potential endogeneity issues due to unobserved factors related to the residential location of the poor, which may be correlated with the pandemic outbreak across American counties. Our estimates suggest that increasing urban poverty produces a rise in both the virus incidence and the speed of diffusion, which fades out when opportunities for mobility drastically reduce in response to lockdown measures. This evidence highlights a new health gradient attributable to the incidence of poverty in the neighborhood of residence.

We further analyze the interaction between urban poverty and lockdown policies in a dynamic fixed-effect model framework. In the absence of a vaccine, quarantine enforcement and social distancing policies have been regarded as unavoidable policy measures to mitigate the spread of the virus. Evidence about past pandemic events supports this view (Bootsma & Ferguson, 2007). About half of the American counties have introduced stay-at-home orders starting on March 25, 2020. There is mixed evidence on the effects of such measures. Some results suggest that these restrictions have contributed to mitigating the virus spreading by reducing movements and social contacts (Anderson et al., 2020; Chernozhukov et al., 2021; Courtemanche et al., 2020; Hsiang et al., 2020; Pei et al., 2020), as well as improving health outcomes during the pandemic (Dave et al., 2020, 2021; Sears et al., 2023). However, Berry et al. (2021) and Agrawal et al. (2021) suggest that the effects of lockdown policies on mobility have been short-lived, particularly among the poor. The effectiveness of mobility restrictions thus depends on the socioeconomic context where they are implemented. Recent evidence has shown that low-income individuals face greater constraints in complying with stay-at-home orders (Coven & Gupta, 2020; Garnier et al., 2021; Jay et al., 2020; Miller et al., 2020). Moreover, a high incidence of poverty tends to reduce compliance with stay-at-home orders in US cities (Jung et al., 2021; Lou et al., 2020; Wright et al., 2020) and in developing countries (Bargain & Aminjonov, 2020).

We analyze the relationship between COVID-19 cases, lockdown policies, and urban poverty across American cities. In our regression analysis, we control for county-specific time-invariant drivers of the intensity and speed of the virus spreading related to urban poverty. We find that stay-at-home orders (occurring between 7 and 14 days before the assessment of the virus incidence) do not significantly contribute to reducing the incidence of COVID-19 at the county level. Instead, introducing lockdown policies intensifies the effect of urban poverty on COVID-19 spreading.

The rest of the article is organized as follows. Section 2 introduces the urban poverty measures used in this article. Section 3 describes the data sources and the geographic matching we perform using a sample of 1064 American urban counties. The empirical strategy is set out in Section 4, whereas results are discussed in Section 5. Section 6 concludes.

2. MEASURING URBAN POVERTY

The main treatment variables that we study are *urban poverty* measures, which refer to the uneven distribution of poverty across the city's neighborhoods. A *urban poverty* index maps information about the degree of concentration of poor individuals in areas of the city where poverty is relatively over-represented into a number reflecting the level of urban poverty displayed by that city. Urban poverty measures capture the fact that, in the presence of urban poverty, the poor are more likely to share the same neighborhood and thus interact locally with other poor residents than the nonpoor population, thereby suffering a *double burden* of poverty: not only in areas where poor residents are highly concentrated there is more poverty, but also those living in these places suffer the detrimental consequences of being exposed to urban poverty in terms of limited opportunities for human and social development.

In this setting, every movement of a poor individual from a high-poverty neighborhood (i.e., where the share of poor residents is above a given acceptance threshold) toward a neighborhood where poverty is less concentrated is always bound to reduce urban poverty. Owing to this principle, alongside technical axioms, Andreoli et al. (2021) characterize a family of urban poverty measures combining information on (i) the incidence of poverty in the city, (ii) the distribution of poverty across high-poverty neighborhoods, and (iii) the extent of segregation of poor and nonpoor populations across high-poverty and low-poverty neighborhoods.

Our analysis is focused on the United States, and we use the notion of Metropolitan Statistical Areas (MSAs) to identify cities. Each MSA consists of n nonoverlapping census tracts, defining neighborhoods. Let $N_i \in \mathbb{R}_+$ denote the population living in census tract $i \in \{1, \dots, n\}$, whereas P_i is the poor population living in i. Then, $N = \sum_{i=1}^{n} N_i$ and $P = \sum_{i=1}^{n} P_i$ are, respectively, the overall population and the total number of poor in the MSA. In our model, we represent an MSA by the corresponding *urban poverty configuration* $\mathcal{A} = \{P_i^{\mathcal{A}}, N_i^{\mathcal{A}}\}_{i=1}^{n}$. Let $\zeta \in [0, 1]$ be the urban poverty line used to identify tracts where poverty is more concentrated, that is, where the ratio $\frac{P_i}{N_i} \geq \zeta$. Hence, for a given urban poverty line ζ, there are $z \geq 1$ tracts where poverty is highly concentrated. Assuming that tracts are ordered by decreasing magnitude of poverty incidence, so that $\frac{P_i}{N_i} \geq \frac{P_{i+1}}{N_{i+1}}$, then $\overline{P}_z = \sum_{i=1}^{z} P_i$ and $\overline{N}_z = \sum_{i=1}^{z} N_i$ denote the number of poor individuals and the total population residing in census tracts where poverty is highly concentrated, respectively. Following the U.S. Census Bureau definition, we set $\zeta = 0.2$ to identify high-poverty census tracts as places where the share of poor individuals is larger than 20% of the resident populations.

The *urban poverty UP* index, introduced and derived axiomatically in Andreoli et al. (2021), is a function that maps a configuration $\mathcal{A}$ into a number measuring the level of urban poverty in the MSA, and it is defined as follows:

$$(1) \quad UP(\mathcal{A},\zeta) := \beta\frac{\overline{P}_z - \zeta\overline{N}_z}{P} + \gamma\left(\frac{\overline{N}_z}{N}\right)\frac{\overline{P}_z}{P}G(\mathcal{A},\zeta) + \gamma\left(\frac{N-\overline{N}_z}{N}\right)\frac{\overline{P}_z - \zeta\overline{N}_z}{P},$$

where $\beta,\gamma \geq 0$ and $z \geq 1$, and $G(\mathcal{A},\zeta)$ measures inequality in the distribution of poverty proportions $\frac{P_1}{N_1}, \dots, \frac{P_z}{N_z}$ among neighborhoods $1, \dots, z$ by mean of the *Gini coefficient*, defined as:

$$(2) \qquad G(.;\zeta) \quad := \frac{1}{2\sum_{i=1}^{z} P_i / \sum_{i=1}^{z} N_i}\sum_{i=1}^{z}\sum_{j=1}^{z}\frac{N_i\, N_j}{\left(\sum_{i=1}^{z} N_i\right)^2}\left|\frac{P_i}{N_i} - \frac{P_j}{N_j}\right|.$$

The urban poverty index is such that $UP(\mathcal{A},\zeta) = 0$ if $z = 0$, which occurs when either there are no poor individuals in the city or when the poor are not over-concentrated in any of the census tracts.

The parameter γ represents the weight of the distributional component of urban poverty, which combines information about the distribution of poverty across high-poverty neighborhoods $i = 1, \dots, z$, as measured by the Gini index $G(\mathcal{A},\zeta)$, with information about the composition of poverty in the population. In particular, the third component of *UP* is a measure of inequality in the distribution of the poor population between high poverty (with weight $\frac{\overline{N}_z}{N}$) and low poverty (with weight $1 - \frac{\overline{N}_z}{N}$) neighborhoods, and it reflects the extent of segregation of the poor across high- and low-poverty neighborhoods. The parameter β is, instead, the weight assigned to poverty incidence as measured by the *urban poverty gap* (borrowing and adapting the terminology from Sen, 1976), that is, the difference between the count of poor in the z neighborhoods where poverty is highly concentrated, and the number of poor one would expect to see if poverty was just at the threshold ζ. Different parametric specifications allow focusing on different dimensions of urban poverty. For example, by setting $\gamma = 0$ and $\beta = 1$, Equation (1) reduces to the *adjusted concentrated poverty* index:

$$(3) \qquad CP^{*}(\mathcal{A},\zeta) := \frac{\overline{P}_z - \zeta\overline{N}_z}{P} = CP(\mathcal{A},\zeta) - \zeta\left(\frac{\overline{N}_z}{P}\right),$$

which represents a correction of the well-known concentrated poverty index $CP(\mathcal{A},\zeta) := \overline{P}_z/P$ (Iceland & Hernandez, 2017; Jargowsky & Bane, 1991; Wilson, 1987) that measures the proportion of poor people living in high-poverty census tracts as identified by the urban poverty line ζ. A related index is the *poverty incidence* at the city level, denoted as $H(\mathcal{A}) := P/N$.

By setting $\gamma = 1$ and $\beta = 0$, with $\zeta = 0$ to highlight that distributional concerns about poverty involve all city neighborhoods, the relevant urban poverty index in

Equation (1) equals the Gini index of the distribution of poverty incidence at the tract level:

$$UP(\mathcal{A},0) = G(\mathcal{A}) := \frac{1}{2P/N}\sum_{i=1}^{n}\sum_{j=1}^{n}\frac{N_i\,N_j}{N^2}\left|\frac{P_i}{N_i}-\frac{P_j}{N_j}\right|. \tag{4}$$

One interesting feature of this index is that it can be additively decomposed along the spatial dimensions into a *neighborhood* G_N and a *non-neighborhood* G_{nN} component, to keep track of the spatial clustering dimension of urban poverty.

3. DATA

We use a database of 1064 urban counties located in 332 MSAs in the United States. For each county, we have matched information about the spread of Coronavirus and its potential drivers issued from the U.S. Census Bureau database. Counties are the finest available geographic aggregates at which information on early incidence of cases testing positive for Coronavirus is reported. We have then matched counties to MSAs and attributed to each county the latter estimate of urban poverty measures available for the MSA where the county is located.

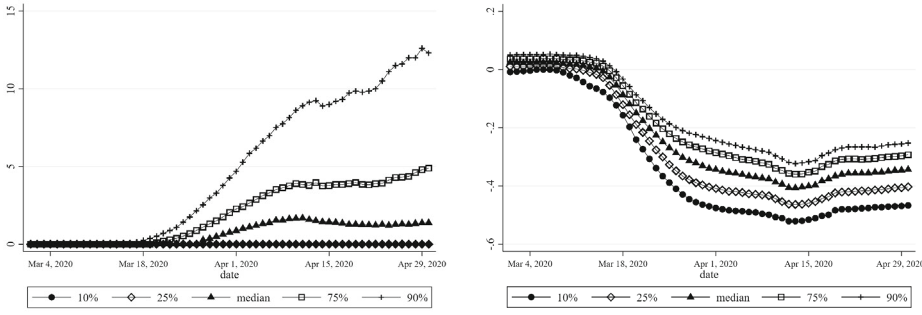
3.1. *Urban poverty*

We produce estimates of urban poverty for the largest American MSAs, using the information on the distribution of urban poverty obtained from the 5-year estimates of poverty collected in the 2015–2019 wave of the American Community Survey (ACS). Estimates from ACS are organized into tables, reporting information on poverty incidence for every census tract in the United States.[1] The population of interest for calculating poverty in ACS comprises all individuals living in census tracts but excludes prison inmates, members of the Armed Forces living in barracks, or college students living in dormitories, as well as unrelated individuals under the age of 15. We consider “poor” individuals who live in households whose income is lower than the federal income poverty line determined by the U.S. Census Bureau. This line varies according to family size, number of children, and the age of the family householder or unrelated individuals. For instance, the federal poverty line in 2019 for a household of four with two children was $25,926. All four members are considered poor if a family’s disposable income is below this threshold.[2]

We group census tracts into cities following the 2016 definition of MSA provided by the Federal Office Management Budget (see Andreoli & Peluso, 2018). For each census tract, we compute poverty incidence as the number of poor in a

[1] The data are described in Andreoli et al. (2022).

[2] Poverty thresholds by family size and number of related children can be found on the U.S. Census Bureau website: https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-thresholds.html. The measure of poverty that we use (known as Official Poverty Measure) has some drawbacks: for instance, it identifies the poor based on before-tax cash income excluding cash and noncash benefits. Alternative poverty definitions, such as the Supplemental Poverty Measure, would improve the identification of the poor. However, improved estimates of poverty based on alternative methods would not be available at the census tract level.



(a) (b)

Figure 1. COVID-19 spread and changes in mobility patterns, (a) Daily cases per 100 k residents, (b) Daily variation in mobility to workplace.

Note: Subfigures (a) and (b) represent the trends by different percentiles in daily COVID-19 cases per 100 k residents and mobility, respectively, between March and April 2020. Data in panel (a) are from the New York Times COVID-19 repository. Data in panel (b) are from the Economic Tracker, which refers to Google' COVID-19 Community Mobility reports.

given neighborhood normalized by the total number of residents therein. Then, we obtain urban poverty measures for high-poverty neighborhoods, that is, neighborhoods with a poverty incidence above 20% of the resident population.[3] Table A.1 in the Appendix provides distributional statistics about urban poverty measures across American MSAs in the pre-pandemic period.

3.2. *COVID-19 cases*

The data on COVID-19 cases are collected from the *Economic Tracker*, which reports the daily number of cases and deaths at the county, state, and national level, using information from the New York Times COVID-19 repository (for additional details of the dataset, see Chetty et al., 2024). We use data on the county's daily number of reported cases, normalized by 100 k residents and expressed as a seven-day moving average to smooth out daily fluctuations in the number of tests or report delays. Panel (a) of Figure 1 shows trends in the incidence of new cases between March and April 2020. We measure the speed of pandemics in the county as the change in this number in the one-week and two-week windows.[4]

3.3. *Mobility and stay-at-home orders*

We gather mobility data from the Economic Tracker, which reports data on daily time spent at various locations (e.g., parks, retail, grocery stores, transit locations, and workplaces) as a percentage of change from a baseline period of January

[3]The 20% threshold was introduced by Wilson (1987) to identify high-poverty neighborhoods (for additional references, see Andreoli et al., 2021) and later adopted also by the U.S. Census Bureau (see for instance Bishaw et al., 2020).

[4]The Centers for Disease Control and Prevention has produced a restricted-use dataset on county-level health outcomes related to COVID-19 (cases, hospitalization, death), disaggregated by characteristics of the patients. These data cover a limited amount of urban counties, thus reducing the reliability of our identification strategy. For these reasons, our analysis focuses on official aggregate statistics granting universal coverage of urban counties.

3 to February 5, 2020. The data are estimated from cellphone location information obtained from Google users who have enabled the Location History setting (for additional details, see Chetty et al., 2024).

We focus on county-level data on mobility to workplaces, for which urban counties have nearly universal coverage. From panel (b) in Figure 1, we note that mobility has dropped since mid-March 2020. Figure 2 illustrates the pattern of daily changes in mobility by different levels of urban poverty indicators. More specifically, we group counties into two categories based on the urban poverty level of the MSA to which they belong, that is, *low* below the median and *high* when above the median. Changes in mobility compared with the reference period are shown on the vertical axis, while the horizontal axis reports dates. The vertical dashed line is on March 29, 2020, that is, the first period considered in our analysis.

The figure is helpful in disentangling heterogeneous patterns of mobility across places that display high or low levels of urban poverty. Regardless of the dimension of urban poverty investigated, mobility patterns are similar across high- and low-poverty places. Similarly, mobility declines everywhere at the same rate as soon as COVID-19 causes of infection become known. Heterogeneity in mobility patterns across high- and low-poverty places materializes only after the introduction of stay-at-home orders, with sharp differences across indices. Looking at urban poverty as measured by the $UP(.,0.2)$ index, we note that the trend is almost the same for the different levels of poverty considered (panel (a)). In contrast, panel (b) shows that the mobility decline was faster in counties where the poor are more unevenly distributed. Interestingly, this difference has become evident since March 19, 2020, when the first stay-at-home order was issued. Furthermore, most of the differences due to the G index are attributable to the non-neighborhood (G_{nN}) component (panels (c) and (d)).

3.4. *Data on covariates*

From the 2015–2019 wave of the ACS, we gather additional information at the county level, reported in Table A.1 in the Appendix. These variables cover the following domains of heterogeneity: demographics, housing, education and employment, health insurance coverage, and ethnic segregation. Demographic data in Group A refer to the size of the county's population (in log) and its composition by racial/ethnic and age groups (as shares of the total population). Controls in Group B describe the aggregate quality of the housing market (such as the share of old houses aged more than 20 years) and the housing opportunities for low-income families (such as the share of owner-occupied homes). Covariates in Group C gather information on the human capital composition of the county population. The description of the county from the education perspective includes two indicator variables indicating whether the county belongs to a student town (i.e., a top 20 MSA in terms of students enrolled in any college) or a college town (i.e., an MSA hosting selective colleges with tier level equal to 1 or 2). Group C also includes the share of commuters less than a half-hour away from work and variables related to the county's income distribution, such as the county mean of the average and median household income by census tract, alongside measures of income dispersion. These distributional variables, along with information on

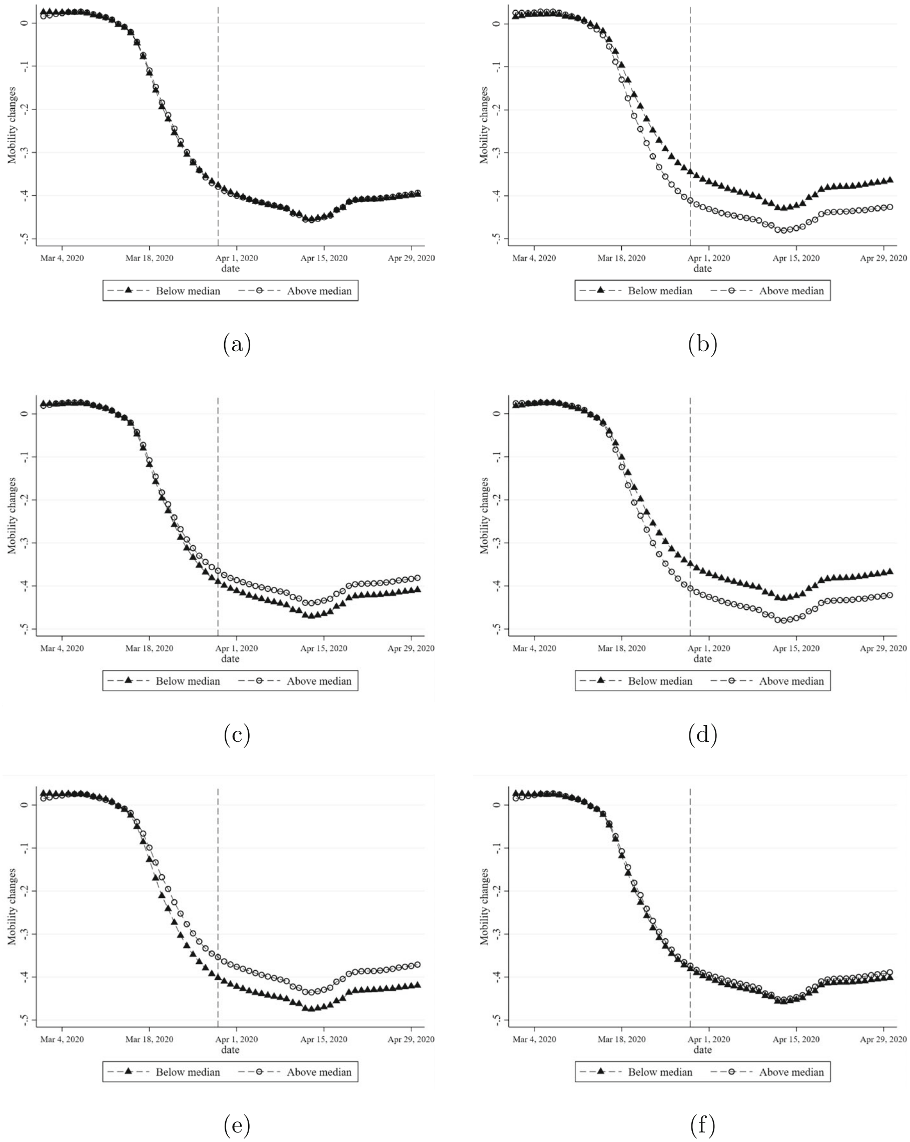


Figure 2. Mobility to Workplaces and Urban Poverty, (a) UP(.,0.2), (b) Gini$-G$, (c) G_N, (d) G_{nN}, (e) H, (f) CP(.,0.2).

Note: Data on mobility are from the Economic Tracker which refers to Google COVID-19 Community Mobility reports. Data on poverty indicators are from authors' elaboration of data from ACS.

rents and housing variables, provide good proxies for the county's affordability. Group D examines the health-related dimension of the county, while to describe the ethnic dimension of segregation across the MSA neighborhoods (E), we use the dissimilarity index for Blacks, Hispanics, and Asians relative to the overall population.

Lastly, we consider additional variables related to the Low Income Housing Tax Credit (LIHTC) program, which we use as instruments for urban poverty. Specifically, we consider the average number of LIHTC-eligible projects within a county. For a comprehensive discussion of the specific aspects and implementation features of the program, see Khadduri et al. (2012).

4. EMPIRICAL STRATEGY

4.1. *Baseline*

We endorse different identification strategies to estimate the impact of urban poverty on COVID-19 cases and its interaction with mobility restriction policies. First, we rely on variations in the incidence and distribution of urban poverty across MSAs, controlling for observable characteristics of the cities, for state fixed effects, and reporting estimates for different periods to disentangle the implications of mobility restrictions on actual levels of mobility.

We distinguish three subperiods. The earliest period covers the onset of COVID-19 outbreaks as of March 29, 2020 (10 days after the first stay-at-home order enacted in California). At this date, COVID-19 incidence data are likely unaffected by the early lockdown policies and reflect the initial patterns of the pandemic's evolution in response to local characteristics.

The second period ends on April 13, 2020, approximately 10 days after the nationwide implementation of lockdown restrictions. We look at the number of new cases as of April 13 and their speed of variation over one- and two-week windows to test the persistence of urban poverty effects on COVID-19 incidence following the implementation of lockdown policies.

The third period looks at Coronavirus incidence as of April 29, 2020, after the introduction of mobility restriction orders. We produce separate estimates of our baseline models across these different periods to reduce bias generated by simultaneity in lockdown measures implementation and incidence of COVID-19 at the county level.

The following regression model estimates the effect of interest separately for each period t in which COVID-19 cases are observed:

$$y_{cms}(t) = \alpha_0 + \beta_1 I_m + \beta_2 M_m + \beta_3 M_m * I_m + \boldsymbol{\beta}_4 \mathbf{X}_c + \beta_s + \varepsilon_{cms}, \tag{5}$$

where c, m, and s denote respectively county, MSA, and state. Notice that the spatial organization of the data reflects that MSAs are composed of more than one county, and more MSAs belong to the same state.

The dependent variable $y_{cms}(t)$ measures the incidence of new COVID-19 cases in American MSAs as a weekly moving average at date t (March 29, April 13, and April 29, 2020). We also consider models that focus on the speed of growth of new COVID-19 cases at the county level, measured as the difference in weekly averages of new cases in date t, with the new cases registered one week ($t - 7$) or two weeks ($t - 14$) ahead. Our baseline estimates are carried out separately for each date.

The variable I_m denotes an indicator for the level of urban poverty in the MSA m, chosen among those described in Section 2. The level of urban poverty is always normalized by the cross-MSA variability, implying that the effect of interest β_1 always measures the implications of a one standard deviation increase in urban poverty on COVID-19 incidence.

The variable M_m captures the average variation in mobility (using a weekly moving average) at the MSA level on a given date relative to January 2020. The coefficient β_3 captures the effect of an exogenous change of urban poverty as a function of changes in mobility due to the introduction of stay-at-home orders issued by state authorities. To address selection on observables, we control for county-specific characteristics $\mathbf{X}_c$, including distributive statistics, and add state fixed-effects β_s to account for state-specific features of lockdown policies as well as the local performances of the healthcare system in terms of COVID-19 spreading testing.

4.2. *Addressing endogeneity*

The coefficients of interest in Model (5) may be biased because of measurement errors in the dependent variable and because of unobservable drivers of the pandemic confounding the effect of interest. This may happen if, for instance, the distribution of urban poverty is correlated with the degree of accessibility to some essential services, such as transportation (Glaeser et al., 2008) and healthcare facilities (Eichenbaum et al., 2022; Mercado et al., 2007) which act, in turn, as drivers of the pandemic (Tirachini & Cats, 2020). This problem is likely less relevant in cities with higher median incomes, which tend to exhibit a more heterogeneous income mix at the neighborhood level with less inequality across census tracts (Andreoli et al., 2022) and provide greater access to urban amenities and localized public goods (Eichenbaum et al., 2022). Andreoli et al. (2022) also find that urban poverty correlates with individuals who commute to their jobs, are less educated on average, and are less likely to be managers. Finally, weaker social ties between high- and low-income residents, as captured by the segregation component of the urban poverty index, may lead to a more difficult diffusion of social practices and norms that prevent the spread of the virus.

To address endogeneity concerns, we suggest using an instrumental variable approach. An instrument for urban poverty should be correlated with the distribution of poverty in the city but should not be related to the potential sources of endogeneity. We propose using the Low Income Housing Tax Credit (LIHTC) Scheme as a source of identifying information. Established as part of the Tax Reform Act of 1986 to promote the development of affordable rental housing for low-income families, LIHTC has become, over time, the largest and most generous federal housing program (Ellen et al., 2009, 2016). Each year, the federal government allocates an LIHTC budget to states based on their demographic size. States assign tax credits to developers who submit projects to build or refurbish low-income rental housing. Projects eligible for the ten-year stream of tax credit must satisfy one of the following two criteria for at least 30 years: (i) at least 20% of households that will occupy the units must have income below 50% of the area median income (AMI), or (ii) at least 40% of units tenants must have income below 60% of the AMI. Projects in qualified census tracts (QCT) are eligible for a tax credit of at least 30% of the construction cost.[5]

The allocation mechanism of tax credits generates a quasi-random assignment of tax credits (and hence the supply of affordable housing and renovated housing) across census tracts as a function of their eligibility status. Eligibility is randomized according to administrative thresholds based on some relevant neighborhoods' characteristics, inducing a form of spatial discontinuity in the assignment of the LIHTC to projects across otherwise comparable neighborhoods (Baum-Snow & Marion, 2009).

Given its specific characteristics, LIHTC may serve as an exogenous source of relocation of high- and low-income households that smooth differences in the presence of such groups in low-income census tracts, thus reducing urban poverty. Overall, although QCTs receive more projects than other tracts (Baum-Snow & Marion, 2009), existing evidence shows that LIHTC tends to de-concentrate poverty (Ellen et al., 2009; Freedman & McGavock, 2015), albeit with a small effect overall (Ellen et al., 2016). Moreover, in the long run, there is an improvement in the neighborhood's conditions, which also becomes more attractive to affluent families (Baum-Snow & Marion, 2009; Ellen et al., 2016). We exploit quasi-randomization of LIHTC allocation across census tracts to identify exogenous marginal changes in urban poverty. We make use of the number (EZ_m) of housing projects implemented in the last 20 years across MSA m normalized by the total number of houses in m as an instrument for urban poverty indicators I_m, also measured at the MSA level.

Our instrumental variable estimates are obtained in two steps. First, we consider the endogenous variables in the baseline Model (5), I_m (constant across all counties c comprised in MSA m), and we regress it on available characteristics at the MSA level and the instrument according to the following regression model:

$$I_m = \gamma_0 + \gamma_1 EZ_m + \boldsymbol{\gamma}_3 \mathbf{X}_c + \gamma_s + v_{mcs}. \tag{6}$$

The model, specified at the MSA level, is estimated on county-level data. Identification relies on the variability of LIHTC incidence across counties located in different MSAs. Second, we use estimated coefficients from the first stage to predict values $\widehat{I}_m$ at the MSA level, which we include among the second-stage regressors, specified instead at the county level for which we observe the relevant measures of COVID-19 cases:

$$y_{cms}(t) = \alpha_0^{IV} + \beta_1^{IV} \widehat{I}_m + \boldsymbol{\beta}_2^{IV} \mathbf{X}_c + \beta_s^{IV} + \varepsilon_{cms}^{IV}. \tag{7}$$

The coefficient of interest is β_1^{IV}, which reports the effect of one standard deviation increase in urban poverty induced by the LIHTC scheme on the incidence of cases testing positive for COVID-19 in a reference period t. Our exclusion restriction rests on the fact that the LIHTC affects the distribution of poverty incidence across a city's census tracts without consequences on other dimensions, which are relevant for assessing pandemic incidence but are not captured by the urban poverty

[5]QCTs are tracts where at least 50% of households earn an income below 60% of AMI, or the poverty rate is above 25%.

indicator. Models (5) and (7) break down the effects by period (i.e., before, during, and after the introduction of the lockdown policies).

4.3. *Urban poverty and effectiveness of mobility restrictions*

Another aspect of COVID-19 onset that we consider is the interrelation between restriction mobility policies and the extent of urban poverty registered in those places where restrictions are implemented. We evaluate whether the effect of mobility restrictions on the pandemic spreading varies along the lines of urban poverty experienced by people subject to those restrictions. We assess such effect, exploiting variation across time and space (counties) of COVID-19 *daily* occurrences from early March 2020 to April 30, 2020. Our preferred specification involves a fixed-effects model, which allows us to investigate the magnitude of stay-at-home orders on COVID-19 spread by differentiating effects based on the incidence of urban poverty in the MSA m to which county c belongs:

$$
\begin{aligned}
(8) \qquad y_{\text{cmsd}} = \delta_0 + \delta_1 \mathbf{1}\{\text{stay-at-home}_c \geq d - 10\} + \delta_2 \mathbf{1}\{\text{stay-at-home}_c \geq d - 10\} \\
* I_m + \delta_d + \delta_c + \delta_m + \delta_s + \eta_{\text{cmsd}},
\end{aligned}
$$

where y_{cmsd} denotes the *daily* new positive testing cases occurring in county c on day d or the speed of contagion on the same date. The indicator $\mathbf{1}\{\text{stay-at-home}_c \geq d - 10\}$ takes value one starting 10 days after the date of introduction of a stay-at-home order in county c (stay-at-home$_c$) onward, zero otherwise. The 10-day delay in the assignment of lockdown policies allows us to measure changes in pandemic patterns attributable to policy effects. Including time, county, and MSA fixed-effects captures time-invariant attributes (such as urban poverty) across counties belonging to different MSAs, whereas state fixed-effects account for differences in healthcare supply across states. The coefficients of interest are δ_1 and δ_2, the latter providing the effect of rising urban poverty on the incidence of COVID-19 conditional on the introduction of lockdown policies. Small values of δ_2 suggest that the impact of urban poverty on COVID-19 spread depends on attributes of MSA unrelated to policies limiting movements.

We use linear regression methods (OLS and IV) to estimate Models (5) and (7), respectively, using the level or growth of new cases normalized by 100 k residents in the county. The propagation speed of COVID-19 is registered as a difference in new cases observed in different data over the period considered in this study. We also use Poisson count models to consider counts of new cases testing positive for COVID-19. We use longitudinal data at a county-day level on the COVID-19 cases and fit fixed-effect models to estimate Model (8) coefficients. Standard errors are always robust and clustered at the MSA level, corresponding to the geographic layer of aggregation of the treatments of interest. In the instrumental variable plug-in estimator, we follow the methodology in Murphy and Topel (2002) to correct second-stage standard errors for the uncertainty introduced by first-stage predictions.

5. Results

Table 1 reports the effect of increasing the index $UP(.;0.2)$ by one standard deviation on the incidence and speed of COVID-19 at the county level. We discuss different specifications of Equation (5). Models 1–3 are estimated using OLS on the number of new cases of COVID-19 normalized by 100 k residents, whereas Models 4–6 are not normalized and are estimated using a nonlinear count model (Poisson regression). Models 7–12 provide estimates of the speed of COVID-19 spreading at different dates ($t-7$ and $t-14$). Models differ in the explanatory variables. Model 1 includes only the urban poverty index as the main treatment, whereas Model 3 corresponds to the full specification of Equation (5). All models control for the explanatory variables A–E described above, as well as for state fixed-effects. Estimates are broken down into three periods.

OLS regressions do not provide evidence of significant effects of urban poverty on COVID-19 cumulative number of cases. We find a significant (at 10% confidence level) positive effect of urban poverty on the speed of propagation of the Coronavirus pandemic over a week time. The effects' magnitude ranges from 0.20 to 0.21 new cases per 100 k residents.

During the period when stay-at-home orders were heterogeneously introduced across the American States, we do not detect significant effects of rising urban poverty on new cases of COVID-19 and the speed of COVID-19 propagation. The negative and large coefficient associated with the variation in mobility reflects the late onset of stay-at-home orders in counties with a low incidence of COVID-19 cases (which, in turn, reflects only small reductions in the extent of mobility to work).

Results suggest that urban poverty increases the speed of new COVID-19 cases at the onset of the pandemic. Before April 29, 2020, a rise in urban poverty was associated with the rising speed of COVID-19 cases; the effect is often amplified by the degree of mobility within one or two weeks before the reference date. For instance, a rise in one standard deviation of urban poverty raised the speed of new COVID-19 cases by 2.86 positive cases over 100 k residents on April 13, the effect increasing substantially (7.27 cases per 100 k residents) in places displaying larger mobility at the end of March 2020. The two dimensions are likely correlated: cities displaying a more unequal distribution of poverty across their neighborhoods also display larger patterns of mobility, representing a mechanism reinforcing the effects of urban poverty on the rise of COVID-19 cases in American MSAs.

When restrictions to mobility are widely implemented through the issue of stay-at-home orders (as of April 29), mobility is reduced drastically across all MSAs. The effect of urban poverty on COVID-19 new cases and their speed of growth on this date becomes negative and often nonsignificant. There are several explanations for this pattern. First, the reduction in mobility all over the country has reduced the likelihood of contagion globally. The effect is stronger in places where mobility is reduced the most. Figure 2 highlights that mobility was reduced strongly in high-urban poverty areas compared with other places, although the levels of mobility were not significantly different from other areas before the introduction of stay-at-home orders. The effects registered after April 29 are, hence, confounding the true effect of urban poverty on COVID-19 cases with that induced by the reductions to mobility that high-urban poverty MSAs have experienced. A second explanation concerns the cycle of COVID-19 infections, which manifest in waves. As COVID-19 cases correlate positively with urban poverty at the very onset of the pandemic, new cases have likely grown faster in places manifesting a lower total number of cases at the very onset of the pandemic, which are low-urban poverty MSAs. Such a pattern could have been fueled by the nonrandom introduction of mobility restrictions across high- and low-urban poverty MSAs, as the mobility bans were decided in early April 2020 based on data about incidence and speed of the pandemic before that date (which was higher in high-urban poverty cities).

The patterns of the effects of rising poverty incidence in the city, reported in Table A.2 in the Appendix, mirror those in Table 1. Patterns related to concentrated poverty indices, reported in Table A.3, are also aligned. In both cases, OLS estimates are significant. Table A.4 shows that the Gini G urban poverty measure is seldom significantly associated with COVID-19 onset. Table A.5 reveals some similarity between the pattern of marginal effects of rising the neighborhood component of the Gini urban poverty index (G_N) on COVID-19 outbreak and coefficients registered in Table 1. Effects related to the non-neighborhood component of urban poverty (G_{nN}), reported in Table A.6, have opposite signs compared with those related to G_N but similar patterns of significance.

The effects described so far may be estimated with bias, which we address following an instrumental variable strategy. In Table A.7 in the Appendix, we report first-stage estimates from Equation (6). We interpret the estimated coefficients as the effects of a one percentage point increase in the LIHTC coverage citywide in terms of standard deviation units of urban poverty. As expected, an increase in the incidence of LIHTC units has a significant negative impact on several urban poverty measures, the reduction being fostered by the increased homogeneity in poor versus nonpoor individuals in the neighborhoods. We find that rising by one percentage point, the share of LIHTC census tracts in an MSA reduces $UP(.,0.2)$ by 0.145 standard deviations, reduces G by 0.079 standard deviations, and reduces $CP(.,0.2)$ and H by 0.118 standard deviations, whereas the same change does not significantly impact dimensions of urban poverty captured by the components of the G index. The sign and magnitude of the effects are compatible with those in Ellen et al. (2016), where it is shown that LIHTC-supported programs reduce the concentration of poverty by leveling the proportion of poor and nonpoor residents in high-poverty neighborhoods.

Second-stage effects based on the LIHTC instrumental variable approach are reported in Table 2 for each urban poverty index (by rows) and by period (by column) separately. Considering the patterns of significance at the first stage, we analyze the second-stage coefficients only for the urban poverty indices with significant coefficients, thereby omitting G_N and G_{nN}. Table 2 features effects on the early incidence of COVID-19 (Models 1–4), during the period where stay-at-home orders have been issued heterogeneously across states (Models 5–8), and during the post-lockdown settlement (Models 9–12). Each set of estimates features both new COVID-19 cases on a given date and measures the speed of progression of

Table 1
UP(., 0.2) and COVID-19

Dependent variable	COVID-19 cases						COVID-19 speed new cases					
Measure	#positives/100 k			#positives			#positives/100 k					
Time frame	–	–	–	–	–	–	7 days	7 days	7 days	15 days	15 days	15 days
	OLS			Poisson			OLS					
Method	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence–29 March												
UP(.,0.2)–SD units	0.26	0.25	0.17	10.68	12.29	7.75	0.21*	0.20*	0.20*	0.28	0.26	0.35*
	(0.18)	(0.19)	(0.22)	(7.24)	(7.83)	(9.16)	(0.11)	(0.12)	(0.12)	(0.17)	(0.18)	(0.19)
Δ Mobility workplaces (15 Mar)		0.56	8.06		−523.05*	−328.03						
		(4.18)	(8.69)		(302.65)	(243.97)						
UP(.,0.2)×Δ mobility (15 Mar)			−2.97			−98.68						
			(3.25)			(100.41)						
Δ Mobility workplaces (8–15 Mar)								−1.23	5.04			
								(5.00)	(6.44)			
UP(.,0.2)×Δ (avg.) mobility (8–15 Mar)									−2.45			
									(2.25)			
Δ Mobility workplaces (29 Feb–15 Mar)											1.90	18.48
											(6.46)	(13.80)
UP(.,0.2)×Δ (avg.) mobility (29 Feb–15 Mar)												−6.70
												(5.50)
N	1064	1010	1010	1064	1010	1010	1064	1014	1014	1064	1015	1015
R^2	0.532	0.543	0.543				0.434	0.444	0.445	0.521	0.530	0.531
Stay-at-home period–13 April												
UP(.,0.2)–SD units	0.57	0.35	−0.65	7.17	4.23	16.08	0.31	0.35	2.86**	0.32	0.22	1.05
	(0.49)	(0.43)	(1.81)	(27.48)	(27.50)	(99.02)	(0.22)	(0.23)	(1.31)	(0.35)	(0.30)	(1.25)

TABLE 1
Continued

Dependent variable	COVID-19 cases						COVID-19 speed new cases					
Measure	#positives/100 k			#positives			#positives/100 k					
Time frame	–	–	–	–	–	–	7 days	7 days	7 days	15 days	15 days	15 days
		OLS			Poisson				OLS			
Method	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δ Mobility workplace (30 Mar)		−9.96	−3.95		−1992.84**	−2049.63**						
		(10.10)	(14.92)		(417.91)	(654.57)						
UP(.,0.2)×Δ mobility (30 Mar)			−2.58			25.99						
			(3.95)			(199.03)						
Δ Mobility workplaces (23–30 Mar)								17.32**	0.65			
								(8.60)	(5.83)			
UP(.,0.2)×Δ (avg.) mobility (23–30 Mar)									7.27**			
									(3.32)			
Δ Mobility workplaces (16–30 Mar)											−0.21	−7.83
											(6.03)	(10.70)
UP(.,0.2)×Δ (avg.) mobility (16–30 Mar)												3.24
												(4.00)
N	1064	1021	1021	1064	1021	1021	1064	1022	1022	1064	1022	1022
R^2	0.563	0.577	0.578				0.327	0.355	0.365	0.414	0.429	0.430
Stay-at-home period–29 April												
UP(.,0.2)–SD units	−0.37	−0.38	−1.28	−5.10	−10.01	172.02	−0.70**	−0.59**	−3.82*	−1.06**	−0.79**	−1.59
	(0.36)	(0.32)	(2.27)	(41.30)	(43.51)	(187.56)	(0.30)	(0.29)	(2.09)	(0.40)	(0.34)	(2.44)
Δ Mobility workplace (15 Apr)		6.52	11.44		−3624.66**	−4435.88**						
		(24.66)	(33.56)		(779.56)	(1204.38)						
UP(.,0.2)×Δ mobility (15 Apr)			−2.02			363.08						

Table 1
Continued

Dependent variable	COVID-19 cases						COVID-19 speed new cases					
Measure	#positives/100 k			#positives			#positives/100 k					
Time frame	–	–	–	–	–	–	7 days	7 days	7 days	15 days	15 days	15 days
	OLS			Poisson			OLS					
Method	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
			(4.73)			(358.82)						
Δ Mobility workplaces (8–15 Apr)								22.52	40.29			
								(18.27)	(25.16)			
UP(.,0.2)×Δ (avg.) mobility (8–15 Apr)									−7.36*			
									(4.37)			
Δ Mobility workplaces (31 Mar–15 Apr)											43.40**	47.94
											(21.60)	(31.12)
UP(.,0.2)×Δ (avg.) mobility (31 Mar–15 Apr)												−1.90
												(5.30)
N	1064	987	987	1064	987	987	1064	1022	1022	1064	1023	1023
R^2	0.390	0.489	0.489				0.128	0.142	0.145	0.221	0.252	0.253
Controls (A) (B) (C) (D) (E)	y	y	y	y	y	y	y	y	y	y	y	y
State FE	y	y	y	y	y	y	y	y	y	y	y	y

Note: Based on the author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to the New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for (A), (B), (C), (D), (E), and state fixed-effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100 k residents (seven-day moving average) in the county (Models (1)–(3)) or the speed of new cases, that is, the difference between the daily cases in a one or two weeks window (Models (7)–(12)). Models (4)–(6) report marginal effects at the average. SD stands for standard deviation units.

Significance levels: *10% and **5%.

Table 2
2nd Stage–Urban poverty measures and COVID-19

Dependent variable	COVID-19 early incidence				COVID-19 stay-home period							
Measure	#Positives/100 k			#positives	#Positives/100 k			#positives	#Positives/100 k			#positives
Time frame	29 Mar	22–29 Mar	15–29 Mar	29 Mar	13 Apr	6–13 Apr	30 Mar–13 Apr	13 Apr	29 Apr	22–29 Apr	15–29 Apr	29 Apr
	OLS			Poisson	OLS			Poisson	OLS			Poisson
Method	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UP(.,0.2) – SD units	0.65**	0.50**	0.66**	26.49**	1.72*	0.14	1.03	76.24*	−0.32	−1.30**	−1.84**	101.33*
	(0.28)	(0.19)	(0.27)	(11.23)	(0.95)	(0.40)	(0.71)	(42.70)	(0.65)	(0.50)	(0.78)	(58.32)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.534	0.437	0.523		0.568	0.325	0.419		0.389	0.130	0.224	
G – SD units	−0.05	0.06	−0.02	46.64**	0.25	0.33	0.31	88.19**	−0.90	−0.89	−1.42	79.42
	(0.26)	(0.17)	(0.25)	(11.16)	(0.46)	(0.29)	(0.29)	(36.03)	(0.83)	(0.65)	(0.87)	(58.19)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.531	0.433	0.520		0.562	0.326	0.413		0.390	0.127	0.218	
G_{nN} – SD units	−0.22	0.09	−0.12	76.34**	1.97	2.94**	2.12**	210.02**	1.48	−0.03	−0.93	289.23**
	(1.00)	(0.84)	(1.00)	(20.60)	(1.71)	(1.30)	(0.96)	(76.40)	(1.53)	(1.13)	(1.31)	(114.74)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.531	0.433	0.520		0.562	0.333	0.415		0.390	0.125	0.214	
G_N – SD units	0.00	0.07	0.02	2.61	−0.46	−0.75	−0.40	−45.05	−2.27*	−1.57*	−2.09	−112.52
	(0.55)	(0.41)	(0.54)	(15.75)	(0.95)	(0.68)	(0.53)	(56.64)	(1.32)	(0.91)	(1.31)	(91.66)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.531	0.433	0.520		0.562	0.326	0.413		0.392	0.127	0.218	
CP(.,0.2) – SD units	0.75**	0.54**	0.75**	16.37	1.77*	−0.04	0.96	51.93	−0.28	−1.30**	−1.73**	65.93
	(0.30)	(0.20)	(0.29)	(10.88)	(0.97)	(0.43)	(0.72)	(39.98)	(0.63)	(0.48)	(0.73)	(55.79)

TABLE 2
Continued

Dependent variable	COVID-19 early incidence				COVID-19 stay-home period							
Measure	#Positives/100 k			#positives	#Positives/100 k			#positives	#Positives/100 k			#positives
Time frame	29 Mar	22–29 Mar	15–29 Mar	29 Mar	13 Apr	6–13 Apr	30 Mar–13 Apr	13 Apr	29 Apr	22–29 Apr	15–29 Apr	29 Apr
	OLS			Poisson	OLS			Poisson	OLS			Poisson
Method	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.535	0.438	0.524		0.569	0.325	0.418		0.389	0.131	0.223	
H – SD units	0.90**	0.64**	0.89**	10.91	2.03*	−0.10	1.06	45.45	0.13	−1.05**	−1.52**	67.65
	(0.35)	(0.23)	(0.34)	(11.68)	(1.06)	(0.44)	(0.77)	(43.01)	(0.57)	(0.45)	(0.69)	(58.92)
N	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064	1064
R^2	0.536	0.439	0.525		0.570	0.325	0.419		0.389	0.128	0.220	
Controls (A) (B) (C) (D) (E)	y	y	y	y	y	y	y	y	y	y	y	y
State FE	y	y	y	y	y	y	y	y	y	y	y	y

Note: Based on authors' elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for state fixed-effects. Estimates are based on urban counties. First-stage estimates are obtained with MSA-level variables. The instrumental variable is the share of LIHTC units to the housing stock in the MSA. Second-stage estimates are obtained with county-level variables. Robust standard errors are clustered at MSA level. Standard errors are adjusted using Topel and Murphy's SE adjustment in all models, except (4)–(8)–(12). Estimated effects have to be interpreted as variations in the county number of cases per 100 k residents expressed as a seven-day moving average (Models (1), (5), and (9)) or the speed of new cases, that is, the difference between the daily cases in a one- or two-week window (Models (2)–(3), (6)–(7), and (10)–(11)). Models (4)–(8)–(12) report marginal effects at the average. SD stands for standard deviation units.

Significance levels: *10% and **5%.

COVID-19 infections at 7 and 14 days lag, all normalized by 100 k residents and in absolute numbers.

We find that an increase of $UP(.,0.2)$ by one standard deviation is significantly associated with a rise of 0.65 cases per 100 k residents on March 29, 2020. The effect is relatively high, considering the weekly average new cases in the median US county (counties ranked by the proportion of cases testing positive for COVID-19) was about 2.5 per 100 k residents before April 2020. This effect is attributable to a large change in urban poverty corresponding to one standard deviation increase of $UP(.,0.2)$, which roughly corresponds to the gap between the bottom and the top quartile municipalities ranked by urban poverty display. Similar effects are also found for H (0.90 cases per 100 k residents) and $CP(.,0.2)$ (0.75 cases per 100 k residents), considering the number of poor people and their proportion living in high-poverty census tracts, respectively.

The effect on new cases grows in size when stay-at-home orders are issued (as of April 13, 2020). Conversely, effects on the post-lockdown period (post-April 13) are generally negative and aligned in magnitude with the effects described in Table 1. Overall, results for $UP(.,0.2)$ in Table A.8 represent our preferred estimates, addressing potential bias arising from measurement error and endogeneity.

The validity of the estimated effects relies on the exclusion restriction, which could be violated in the presence of unobservable confounders that correlate both with the presence of LIHTC projects and with the recorded number of COVID-19 cases but that are not absorbed by state fixed-effects and county-specific characteristics. Access to healthcare services is a potential (unobservable) confounder for the effects estimated in Table 2, because early cases of COVID-19 were exclusively detected where testing structures were made available. It is reasonable to assume that access to healthcare services positively correlates with the number of LIHTC projects because highly-served locations are more likely to attract constructors and demand from middle- and high-income families. Our exclusion restriction, which exploits the fact that increments in the supply of LIHTC projects reduce poverty concentration (and hence COVID-19 incidence), could fail if such increment is more frequent in locations with broader access to healthcare services, which is, instead, positively correlated with COVID-19 cases. Failing the exclusion restriction because of unobservable heterogeneity hence leads to an underestimation of the true impact of urban poverty on COVID-19, implying that the estimates in Table 2 should be considered lower bounds of the actual effect of urban poverty on the incidence of the pandemic.

Lastly, we analyze the interaction between mobility restriction policies issued through stay-at-home orders at the county level and urban poverty. Table 3 reports estimates of the relevant coefficients from Model (8). The data cover daily new COVID-19 cases over the entire period from February 2020 to April 29, 2020. During this period, mobility restriction policies were gradually introduced in American countries. Models 1–4 look at new cases (1, 2) and the speed of growth of new cases (3, 4), relating variability in COVID-19 incidence to urban poverty and to the introduction of stay-at-home orders 10 days ahead of April 29. Models 5–12 report robustness checks when data on COVID-19 cases are merged with information on stay-at-home orders implemented 7 days (models 5-8) or 14 days (models 9-12) ahead. Models 1–4 provide further evidence of the role of urban poverty in

TABLE 3
URBAN POVERTY, COVID-19 AND MOBILITY RESTRICTION POLICIES

Dependent variable	Spread of COVID-19											
Measure	#positives/100 k		Speed−2 weeks		#positives/100 k		Speed−2 weeks		#positives/100 k		Speed−2 weeks	
	Daily cases		Change in new cases		Daily cases		Change in new cases		Daily cases		Change in new cases	
	10 days				7 days				14 days			
Time frame	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Stayhome × UP(.,0.2)	11.16*	13.37**	2.10	−5.28	10.28*	15.02**	3.25	−2.02	9.81	9.45*	−2.20	−10.90
	(6.60)	(5.83)	(5.98)	(6.48)	(5.60)	(6.06)	(5.54)	(4.99)	(7.03)	(5.61)	(5.18)	(7.66)
Stayhome		−0.59		1.99		−1.29		1.43		0.09		2.30
		(1.22)		(1.57)		(1.08)		(1.36)		(1.36)		(1.60)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.441	0.441	0.136	0.138	0.440	0.441	0.136	0.137	0.440	0.440	0.136	0.138
Stayhome × G	6.00	24.74*	3.32	6.67**	5.01	24.37*	3.39	8.14**	6.13	25.59**	1.79	5.08
	(4.12)	(12.63)	(2.94)	(2.97)	(3.54)	(12.90)	(2.95)	(2.59)	(4.38)	(11.72)	(2.21)	(5.22)
Stayhome		−8.18**		−1.46		−8.45*		−2.07		−8.47**		−1.43
		(4.15)		(2.06)		(4.40)		(1.52)		(3.61)		(2.77)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.441	0.446	0.138	0.138	0.440	0.445	0.138	0.138	0.442	0.446	0.136	0.136
Stayhome × G_N	−6.07	−15.36	0.57	−3.98*	−7.02	−14.71	−0.23	−4.86**	−6.32*	−16.67*	−0.33	−3.01
	(4.90)	(9.41)	(3.63)	(2.03)	(5.14)	(9.40)	(2.96)	(2.12)	(3.80)	(8.91)	(4.26)	(3.79)
Stayhome		3.30		1.62		2.78		1.67		3.55		0.92
		(2.21)		(1.39)		(1.94)		(1.41)		(2.36)		(1.08)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.438	0.443	0.136	0.138	0.439	0.442	0.136	0.138	0.438	0.444	0.136	0.136
Stayhome × G_{nN}	10.16*	17.36*	4.34	4.59**	9.08	16.94*	4.67	5.64**	10.55*	18.27**	2.59	3.46
	(6.02)	(9.27)	(2.90)	(1.72)	(5.55)	(9.42)	(3.09)	(1.70)	(6.18)	(8.54)	(2.24)	(3.62)
Stayhome		−3.28		−0.11		−3.60*		−0.44		−3.46**		−0.39
		(1.99)		(1.45)		(2.11)		(1.16)		(1.59)		(1.71)

TABLE 3
Continued

Dependent variable	Spread of COVID-19											
Measure	#positives/100 k		Speed−2 weeks		#positives/100 k		Speed−2 weeks		#positives/100 k		Speed−2 weeks	
	Daily cases		Change in new cases		Daily cases		Change in new cases		Daily cases		Change in new cases	
	10 days				7 days				14 days			
Time frame	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.446	0.449	0.138	0.138	0.444	0.448	0.139	0.139	0.446	0.449	0.137	0.137
Stayhome × CP(.,0.2)	4.45	7.31*	1.22	−2.26	3.95	7.91**	1.58	−0.66	4.02	5.43	−0.50	−5.35
	(2.99)	(3.85)	(2.36)	(2.90)	(2.55)	(3.93)	(2.28)	(2.28)	(3.13)	(3.56)	(1.92)	(3.57)
Stayhome		−1.79		2.18		−2.50*		1.41		−0.87		2.99
		(1.31)		(1.77)		(1.38)		(1.43)		(1.29)		(2.03)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.441	0.442	0.136	0.137	0.440	0.442	0.136	0.137	0.440	0.440	0.136	0.138
Stayhome × H	8.40	−2.32	3.01	−18.02	6.54	1.14	3.68	−13.22	7.28	−10.82	−2.07	−27.49
	(7.99)	(12.34)	(8.90)	(13.99)	(6.41)	(13.12)	(8.12)	(10.50)	(8.60)	(11.03)	(7.74)	(16.98)
Stayhome		1.84		3.62*		0.93		2.92*		3.07		4.32*
		(2.37)		(2.09)		(2.29)		(1.76)		(2.44)		(2.38)
N	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460	65,530	65,530	65,460	65,460
R^2	0.438	0.439	0.136	0.139	0.438	0.438	0.136	0.138	0.438	0.439	0.136	0.139
County FE	y	y	y	y	y	y	y	y	y	y	y	y
MSA FE	y	y	y	y	y	y	y	y	y	y	y	y
State FE	y	y	y	y	y	y	y	y	y	y	y	y
Day FE	y	y	y	y	y	y	y	y	y	y	y	y

Note: Based on authors' elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). Estimates are based on urban counties. Robust standard errors are clustered at MSA level.
Significance levels: *10% and **5%.

rising COVID-19 incidence, which hampers the scope for stay-at-home orders to reduce the incidence of new COVID-19 cases onset.

All models' estimates reveal that the introduction of stay-at-home orders has implications for COVID-19 cases that vary linearly with the extent of urban poverty in the city. Columns 2, 6, and 10 reveal that the lockdown policies contributed to the rise in COVID-19 incidence in urban counties that are located in cities characterized by higher levels of urban poverty: for instance, higher urban poverty as measured by $UP(., 0.2)$ rises the number of cases testing positive to COVID-19 by 13.37 cases per 100 k residents in places that have introduced a stay-at-home order compared with places that do not, the effect being robust when urban poverty is assessed through the Gini index and its non-neighborhood component or the concentrated poverty index. Such a gradient has two potential explanations. First, the gradient can represent the consequences of the selection issue of lockdown policies along the lines of the degree of poverty in the county. Such an effect should be considered by the model specification insofar as MSA fixed effects are always controlled for. A second potential explanation concerns how lockdown policies complement the drivers of urban poverty. It has been shown in Andreoli et al. (2022) that urban poverty is larger in poorer cities, with lower median rents, with higher nonowner occupancy rates and where houses are smaller and shared by more occupants. Under these circumstances, policies that foster mobility restrictions can yield larger effects on low-income households (which are, on average, larger, multigenerational families living in smaller houses) in places where those families are more concentrated. These families have likely higher chances of spreading the contagion and are less vulnerable vis-à-vis the pandemic effects due, for instance, to reduced house dimensions and co-residency, or there is less widespread knowledge and access to prevention techniques in the place where they reside (a problem also related to the extent of poverty concentration in the neighborhood).

6. Concluding Remarks

This article investigates the empirical relationship between urban poverty, encompassing aspects of incidence, distribution, and segregation of the poor population in cities, and the onset of the Coronavirus pandemic across American urban counties. We combine different identification strategies. First, we exploit variability in Coronavirus spreading across American MSAs, controlling for state-specific levels. Our estimates show that a one standard deviation increase in urban poverty leads to an increment in the number of daily new COVID-19-positive cases and the rate of speed of infection by about 0.65 cases per 100 k residents. This magnitude is roughly equivalent to a 10% increase in the average county-level incidence of new COVID-19 cases in high-incidence counties. Furthermore, estimates of county fixed effects reveal that the MSAs' level of urban poverty curtails the effectiveness of mobility restrictions.

The results suggest that the distribution of poverty across the city neighborhood correlates with the insurgence of the pandemic, revealing a new dimension contributing to the double burden of concentrated poverty on people exposed to it. The most relevant drivers of urban poverty, such as low rates of home ownership and house overcrowding registered in places where poverty is more concentrated, have had a role in determining the speed at which the pandemic has evolved. From a policy perspective, this evidence suggests that policies addressing the drivers of urban poverty could lead to unintended spill-over effects in terms of the health outcomes of counties during a pandemic.

By contrast, mobility restriction policies have been primarily employed to contrast the growth of new cases of COVID-19 at the onset of the pandemic. We exploit the staggered nature of the introduction of stay-at-home orders across American counties from March to April 2020 to assess the role of such policies in the spread of COVID-19. We do not detect evidence of significant effects of mobility restriction policies on COVID-19 cases. We find, nonetheless, a positive gradient of such policies on the effect of urban poverty on COVID-19 new cases registered in the data and on the speed of their growth. These findings are consistent with the possibility that urban poverty contributes to the development of COVID-19 cases through the dynamic of interaction within households fostered by lockdown policies.

Lastly, this article provides evidence of the empirical relevance of the urban poverty index characterized by Andreoli et al. (2021). As mentioned, the index offers an improvement (from a measurement perspective) of alternative and popular measures of geographic dispersion of poverty, such as the poverty count and the concentrated poverty index, which do not comply with the reasonable and parsimonious axiomatic setting that we use to characterize the urban poverty index. Besides this, the urban poverty index combines different features of the incidence and distribution of poverty across urban neighborhoods. Our instrumental variable results are reassuringly consistent among alternative measures of poverty concentration and show that incidence and inequality in the distribution of the poor among high-poverty neighborhoods (captured by $UP(., \zeta)$), rather than across the city as a whole (captured by G and by its components), appears to drive the onset of the COVID-19 pandemic in US urban areas.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's web site:

Data S1. Online Appendix.
Data S2. Replication files.