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Urban Revitalization:

Assessment Methodologies and Expected Impacts

Housing and Urban Development Division

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Urban areas in Latin America and the Caribbean are feeling the effects of urban decay in their city centers through the loss of critical urban functions and the decline of economic and social vitality. Revitalization efforts aim to enhance the functionality of public spaces and mobility, as well as to attract new businesses and residents. Nevertheless, revitalization initiatives are rarely subjected to a credible evaluation strategy, and as such, it is difficult to assess the extent of the benefits they generate. Further, existing evaluations tend to focus on the positive impacts in socio-economic and urban livability terms, but ignore any potential drawbacks, such as the displacement of current residents due to increased rents. With this in mind, this report: (1) provides a critical review of the literature on urban revitalization experiences and displacement of long-term residents; (2) presents impact evaluation methods that come closer to identifying a causal effect of urban interventions in local and individual outcomes; (3) discusses a method to approximate and characterize the extent of displacement of residents; and (4) offers suggestions that are relevant for the implementation of these methods in the Latin American and Caribbean context.

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Contents

1. Introduction

This report is an IDB knowledge product which formulates a suitable and implementable methodology for measuring the socio-economic impacts of urban revitalization projects in Latin America and the Caribbean (LAC), through a critical review of the methodologies and data requirements of existing evaluations, as documented in the literature. The primary audience of this review is comprised of administrators and researchers in Latin America and the Caribbean, especially those involved in implementing and evaluating urban revitalization projects, as well as technical and research staff and consultants of international development agencies who support these projects and their evaluations.

As in many developed countries, urban areas in LAC are feeling the effects of urban decay in their city centers, with loss of critical urban functions and decline of economic and social vitality. Revitalization efforts undertaken in most city centers aim to enhance the functionality of the areas from the perspectives of public space and mobility, as well as to attract businesses and residents capable of rejuvenating the local economy.

Revitalization interventions are costly and thus policy makers need to know whether they are effective at achieving these intended goals. Unfortunately, most revitalization initiatives are rarely subjected to a credible evaluation strategy, and so, it is hard to assess the extent of the benefits that they may generate. Specifically, evaluations tend to compare outcomes in the targeted area before and after the intervention, neglecting the possibility that any estimated effects could have also occurred in absence of the intervention. Furthermore, evaluations tend to focus on the positive impacts in socio-economic and urban livability terms, but ignore any potential drawbacks that may come with the intervention. For instance, one issue that has received much attention by the media and local authorities is the increased likelihood of current residents' displacement due to urban revitalization interventions that make city centers more desirable, and thus more expensive.

With this in mind, this report: (1) provides a critical review of the literature on urban revitalization experiences and displacement of long-term residents; (2) presents impact evaluation methods that come closer to identifying a causal effect of urban interventions in local and individual outcomes; (3) discusses a method to approximate and characterize the extent of displacement of residents; and (4) offers suggestions that are relevant for the implementation of these methods in the Latin American and Caribbean context.

The report focuses on interventions in core or central neighborhoods of urban areas. These neighborhoods are typically deprived, or underperforming, and have experienced a decline in income relative to the metropolitan area over a span of several decades. At the same time, these central neighborhoods have better accessibility, amenities and cultural heritage than other newer neighborhoods with similar socio-economic characteristics. The analysis is not focused on slums of metropolitan areas. From an urban perspective slums are quite different from central city neighborhoods. Slums tend to be on the outskirts of metropolitan areas and face other problems, such as the lack of durable housing that protects them against extreme climate conditions or precarious access to safe water or adequate sanitation. Urban interventions in slums are best studied separately.

The report contains three sections. The current section is section one. Section two contains a literature review of studies that assess the impact of urban revitalization interventions on neighborhoods, drawing largely on the European and North American experiences as well as on some project evaluations in Latin American cities. A literature review on the smaller group of studies that examine the link between gentrification experiences and displacement of current residents is also included. The main objective of this revision is to emphasize the need for solid empirical methods when assessing effects of urban revitalization interventions and to present the data requirements in those studies. Section three presents two separate methodologies; one that estimates the impact of a revitalization intervention and one that proxies the amount of people or business relocations are undertaken after such intervention. Both methodologies present substantial advantages compared to 'naive' methods such as pre- and post-intervention comparisons that tend to be plagued by omitted factor biases or sample selection concerns.

The conclusion covers the limitations and offer suggestions for the implementation of revitalization interventions in Latin American cities. These recommendations consider the Latin American and Caribbean context where informality, insecurity and inequality are all relatively higher than in other regions and where data unavailability and institutional constraints are more pronounced. We thus advise on a restricted set of outcomes or indicators feasible for impact evaluation analysis and discuss why other metrics, while legitimate, are either too loose or ambitious for the Latin American scenario.

2. Literature Review

The vast majority of urban revitalization programs lie within a broader type of urban policy known as place-based policy. A place-based policy is an intervention aimed at improving the economic performance and quality of life in a deprived area. The intervention may take the form of employment subsidies, block grants, working tax credits or investment in urban infrastructure. Place-based policies can be broadly classified in two groups: 'place-based people strategies' or those that target disadvantaged residents in underperforming areas (e.g. enterprise zones that aim to create jobs for poor residents in those areas), and 'pure place-based strategies' or those that target deprived areas regardless of the socio-economic status of residents (e.g. revitalization of downtowns).[1] The criteria to select the areas for pure place-based interventions might depend on the socio-economic status of residents, yet, the intervention specifically targets the infrastructure and amenities in the area. [2] This categorization of urban revitalization policies within a larger set of place-based policies is relevant, since as it will become clear later in the report, most of the advances in empirical evaluation methods for spatially targeted policies come from this broader literature.

The goal of urban revitalization policies is to regenerate areas with deteriorated urban infrastructure or inadequate urban amenities and services. The specifics of these programs vary substantially from comprehensive redevelopment programs sometimes encompassing significant public-private partnerships to much more discrete projects revamping the streetscape and local amenities in one or two main commercial avenues. Revitalization programs are usually designed to make deprived neighborhoods more attractive for high or middle-high income people to prevent neighborhoods from having high concentrations of poverty or from becoming densely populated by a disadvantaged minority (Tatian, Kingsley, Parilla and Pendall, 2012). To this extent, urban revitalization policies are essentially income-mixing policies that act to restore the stability of a neighborhood when it experiences substantial changes in income or demographic composition.

[1] In contrast to place-based policies, 'people-based' policies are aimed at improving the welfare of individuals regardless of their place of residence. Often, these interventions intend to relocate individuals from distressed areas to high opportunity areas. The largest people-based intervention in the United States was the Moving to Opportunity for Fair Housing (MTO) program. This was a randomized social experiment sponsored by the Department of Housing and Urban Development (HUD) in the 1990s among 4,600 low-income families with children living in public housing projects in high poverty neighborhoods. See Ludwig, Sanbonmatsu, Gennetian, Adam, Duncan, Katz, Kessler, Kling, Lindau, Whitaker, and McDade (2011) and Chetty, Hendren, and Katz (2016) for recent empirical assessments of short and long-term outcomes of MTO on individuals. We do not review the literature on people-based policies here given that urban revitalization programs are place-based and not people-based interventions.

[2] Some urban revitalization programs may provide direct subsidies to local residents (e.g. for home improvement), yet, urban revitalization interventions are generally not individual-based but neighborhood-based (e.g. upgrading of several buildings or blocks in an area) and, hence, affect many local residents regardless of their specific socio-economic status.

This report reviews two main strands of literature. First, we focus on studies that assess the impacts of urban revitalization policies on individual and local outcomes in developed countries. As already stated, one crucial limitation of most urban revitalization interventions is that they lack credible impact evaluation analyses. Therefore, we pay special attention to empirical studies that use methodologies that help identify treatment and control areas and that exploit quasi-experimental variation to quantify the causal effects of urban revitalization interventions on household and neighborhood outcome.[3] These methods provide the basis for the first methodology presented in section 3. In addition, we discuss the limited number of empirical studies that estimate the impacts of urban revitalization interventions in Latin America. Second, we review studies that examine potential displacement effects of urban revitalization policies. One concern of any urban revitalization policy is that area improvements may capitalize in land values and so affect over time disadvantaged residents who are less likely to afford higher rents. Unfortunately, few studies examine this type of potential displacement. We thus extend our review to recent studies that analyze the incidence of displacement in gentrifying neighborhoods, which may have become more successful due to several factors and not necessarily as a result of an urban revitalization intervention.

One important remark is that our literature review covers urban revitalization interventions in core or central neighborhoods of urban areas. Although these neighborhoods are often deprived and underperforming, they tend to have relatively good accessibility to more affluent neighborhoods. Moreover, in several cases, they have experienced an income decline from above to below metropolitan area median income levels.[4] Therefore, we note that the characteristics and findings of the studies reviewed here will be relevant to better understand and anticipate potential effects of urban revitalization interventions in central though underperforming neighborhoods of Latin American metropolitan areas.

[3] See Zuk, Bierbaum, Chapple, Gorska, Loukaitou-Sideris, and Ong (2015) for a more extensive review on the body of literature that relates several types of public investments in infrastructure (e.g. rail and bus-rapid transit, highways, parks, etc.) and cultural or retail strategies to local property values and neighborhood demographic change. We focus our attention only on the reduced set of studies that attempt to estimate a causal effect of urban revitalization initiatives.

[4] There is also a small but growing literature that evaluates the effects of slum upgrading, mainly in peripheral squatters of metropolitan areas in developing countries. We do not review these studies here given that urban revitalization initiatives are generally focused in core neighborhoods of urban areas.

Evidence of Local Effects of Urban Revitalization Interventions

Urban Revitalization Interventions in Developed Countries

We review studies that use credible evaluation methods to assess the impacts of urban revitalization interventions. Table 1 summarizes the main characteristics, methods, and findings of these studies.

Rossi-Hansberg, Sarte, and Owens (2010) examine whether housing externalities exist in a revitalization intervention in Richmond, Virginia. Renewing the stock of housing in a designated neighborhood should lead to increases in housing prices, however, it should also affect housing prices of nearby properties outside the neighborhood through a positive spillover effect or externality. If these housing externalities exist and are large, then they should be considered by policy makers when implementing a revitalization intervention.

The main problem faced by researchers is that housing externalities are hard to detect since any observed change in nearby land or housing prices cannot be necessarily attributed to the revitalization policy. To address this concern, Rossi-Hansberg, Sarte and Owens (2010) exploit the unique implementation of the revitalization program in Richmond to identify an exogenous source of variation and estimate the magnitude of revitalization externalities. The Neighborhoods-in-Bloom (NIB) program was implemented in the city of Richmond between 1999 and 2004 and consisted of new building construction, rehabilitation and renovation of the existence stock in four deprived neighborhoods (Church Hill Central, Blackwell, Highland Park Southern Tip-South Barton Heights and Jackson Ward-Carver). A fifth neighborhood, Bellemeade, facing the same level of deprivation, was not selected because per the program guidelines it did not have an active Community Development Corporation. The authors clearly show that, aside from this caveat, all neighborhoods had similar demographic and economic characteristics.[5,6]

In the empirical application, they estimate how housing prices vary with distance as we move away from both the intervened areas (the four neighborhoods) and

[5] Of course, one potential concern is that Community Development Corporations increased the amount of community services (e.g. job creation) at the same time the NIB was implemented leading to an overestimation of the magnitude of housing externalities. The authors argue that "discussions with city officials directly involved with NIB suggest that other programs were neither enacted nor expanded during the period studied" (Rossi-Hansberg et al., 2010, p. 521).

[6] The NIB program was funded by The Department of Housing and Urban Development (HUD), the Community Development Block Grant (CDBG) and the Home Investment Partnership (HOME) programs. Most of these resources came from the federal government and did not result in a reduction of community expenditures or local investment. This remark is important since a reduction of community spending should decrease local economic activity and mitigate any positive housing externality.

the control area (Bellemeade). Using a semi-parametric hedonic price equation, they first assess how housing prices relate to several housing attributes and location. Next, they relate this estimated location effect to the distance from the area of intervention. The authors find that housing prices in the four treated neighborhoods increased by an extra two to five percent per year than in the non-treated neighborhood. Furthermore, the housing externality (i.e., the value of land as we depart from the area of intervention) decreased by around a half for every 1,000 feet. They conclude that these findings reveal that housing externalities should be taken into account when measuring the effectiveness of urban revitalization policies.

In a more recent study for the United States, Autor, Palmer, and Pathak (2016) also analyze the housing externality hypothesis using as case study the unexpected removal of a rent control law in Cambridge, Massachusetts in 1995. One could expect that whenever a rent control law is binding, i.e., a price ceiling is set below the market equilibrium price, landlords have few incentives to invest in improving their rented homes. Therefore, an elimination of this constraint would create incentives to renew their properties and obtain higher rents. As a side effect or externality, nearby properties would also benefit from this neighborhood increase in the number of home renewals.[7]

Using administrative data on assessed residential value, housing prices and characteristics of all residential transactions from 1988 to 2005, Autor, Palmer, and Pathak (2016) test whether eliminating a rent control law leads to higher prices in houses that were never subject to this law but happen to be located nearby decontrolled houses. They construct a rent control exposure measure for each residential unit that calculates the fraction of other residential units within a 0.20-mile radius that were subject to rent control as of 1994. They find that residential properties at the 75th percentile of rent control exposure gained around 13 percent more in assessed value following decontrol than did properties at the 25th percentile of exposure. This finding validates the housing externality hypothesis. Further, they show that residential investments account for only a quarter of the total increase in Cambridge' market value and that most of the effect can be attributed to housing externalities. Therefore, these findings are in line with a story of high-income tenants moving into never-controlled properties attracted by amenities of the renewed housing stock and rising neighborhood income levels. In turn, landlords respond by improving both decontrolled and never-controlled units as more affluent neighbors move in.

[7] Note that this study is not testing the local effects of a pure urban revitalization policy, however, it provides a convenient setting to isolate externalities on local housing values from actions of neighborhood residents. Generally, this is quite hard to do, since the types of residents and local amenities as well as the quantity and quality of houses are all determined simultaneously.

Turning to the European experience, Ahlfeldt, Maennig, and Richter (2016) study the urban revitalization program that followed the reunification period in Berlin, Germany. Given that after reunification the housing stock in many areas of East Berlin was severely deteriorated, the government implemented a revitalization program in 22 areas out of 39 potential areas between 1993 and 1995. According to official reports, 1.94 billion of euros were spent on renovating the private housing stock among those areas until 2012.[8] The selection criteria to designate these 22 areas were not specified by the Berlin Senate and were not based in any known index of deprivation.

The authors test whether this urban revitalization policy influenced housing prices over the period 1990–2012. To do so, they use a difference-in-difference setting that compares housing prices before and after the policy in the 22 designated areas (the treated group) and the remaining areas of the originally 39 areas that were not treated (the control group). In addition, they consider alternative control groups by looking at areas with similar socioeconomic features or areas in the surroundings of treated areas.

Results show that the urban revitalization policy improved the quality of housing stock in the targeted zones: the policy increased the propensity of buildings being in good condition within targeted areas by 0.8–2.6 percent per year. Correspondingly, housing prices increased by a 0.4 to 2 percent per year relative to the non-treated zones. In contrast to the previous findings, they do not find evidence in favor of the housing externality hypothesis as housing prices in the surroundings of the intervention did not exhibit a relative increase in prices. The authors conclude that "while the policy seemed to have sped up the renovation of significant fractions of the urban fabric and, as such, helped to eliminate the visible traces of the division period, it has also primarily been a cash transfer to those landlords participating in the program" (Ahlfeldt, Maennig, and Richter, 2016, p. 24). We will comment on these results in our discussion below.

Koster and van Ommeren (2016) examine the effects of an urban revitalization program implemented in deprived neighborhoods in the Netherlands on housing prices and sales times. This revitalization project started in 2007 and was funded by the national government with 2.75 billion euros to be invested among the targeted areas. The aim of the program was to improve the quality of life in deteriorated areas and reduce social inequality. Funds were mainly invested in renewing the public housing stock while a smaller share was spent in improving green spaces and expanding social programs.

[8] Private investments were supported through tax abatements, loans, cash advances and co-financing. In 2002 the focus of the revitalization program shifted to improvements in social infrastructure.

The selection criterion was based on a deprivation index constructed as a weighted average of 18 socio-economic and urban indicators organized in four categories: social deprivation (income levels, education and unemployment), physical deprivation (quality of housing stock), social problems (vandalism and crime) and physical problems (noise and air pollution, satisfaction with living environment). The national authorities computed this deprivation index for all the neighborhoods in the country and 83 neighborhoods with a score above a certain threshold were selected. This selection criterion based on a numerical rule is the most suitable for applying a Regression Discontinuity Design (RDD). The RDD method compares outcomes of interest in neighborhoods just above the cut-off point with outcomes in neighborhoods just below. A researcher can argue that neighborhoods with scores around the threshold are similar in terms of observable and unobservable characteristics.[9]

Using this RDD methodology, the authors find that the revitalization program increased housing prices in treated neighborhoods both in the short run (2.2 percent after one year) and in the long run (3.8 percent after five years). One finding of special interest in this study is that the time that houses in treated neighborhoods spent trading in the market decreased in the short run by around 20 percent compared to neighborhoods below the cut-off point. This effect vanished after five years. The authors claim that given the negative correlation between housing prices and sales time, the latter is a key outcome of interest to validate the effectiveness of any place-based intervention: if we observe an increase in housing prices but no change in sales time, the increase might be the result of an unobserved factor and not due to the revitalization program.

González-Pampillón, Jofre-Monseny, and Viladecans-Marsal (2016) study the effects on population dynamics of urban revitalization policies in neighborhoods that concentrate large and growing immigrant populations in Catalonia, Spain.[10] Since distressed neighborhoods tend to experience quite dramatic increases in their shares of foreign born (and in the odds of evolving towards a full-immigrant enclave), the Catalan regional government passed a law in 2003 (Llei de Barris) to intervene in areas deserving 'special attention' by means of improving its amenities (e.g. renewal of plazas and community centers) through urban revitalization initiatives.

[9] In practice, most neighborhoods that were treated exhibited high levels of deprivation, with an index z-score about two standard deviations above the average. However, a handful of neighborhoods with low z-scores were also incorporated in the treatment group with no clear justification. Since this addition generates some noise in the probability of being treated, the authors use a 'fuzzy' regression discontinuity design.

[10] In the region of Catalonia, the share of immigrants increased from 4% in 2000 to 17% in 2009 and some neighborhoods experienced an even more dramatic increase in the share of foreign born. For instance, in La Ribera (Barcelona) and Salt (Girona), the share of foreign-born population increased by more than 40 percentage points.

Table 1: Studies on urban revitalization interventions in develop countries.

Authors	Country	Period	Intervention	Data sources	Outcomes	Method	Findings
Rossi-Hansberg, Sarte, and Owens (2010)	Richmond, VA, United States	1999–2004	Neighborhood-in-Bloom program ($14 million) Acquisition, demolition, rehabilitation, and new construction of housing	Geocoded data of intervened areas Geocoded data of all properties sold between 1993–2004	Land prices	Semi-parametric hedonic price equation: four treated neighborhoods vs. one very similar non-treated neighborhood	Land prices increased by 2–5% per year in the four treated neighborhoods Effect decreases by one half every 1,000 feet
Autor, Palmer, and Pathak (2016)	Cambridge, MA, United States	1995–2005	(Unexpected) removal of a rent control law in Cambridge, MA	Administrative data on assessed residential value, house prices on residential transactions between 1988–2005, with exact location	Property prices	Difference-in-difference: treated properties more exposed to nearby de-controlled properties vs. properties less exposed to decontrolled-units	Prices of never-controlled properties increased more when surrounded by more de-controlled properties Evidence on housing externalities
Ahlfeldt, Maennig, and Richter (2016)	Berlin, Germany	1993–2012	Renovation of private housing stock in many areas of East Berlin; €1.94 billions spent since reunification until 2012	Geocoded transactions of built-up land between Jan 1990 and Aug 2012 GIS information about local amenities	Property prices Buildings condition	Difference-in-difference: 22 treated areas vs. 39 control areas, intended to be treated, but left untreated	Odds of buildings in good condition increased 0.8–2.6% per year and house prices increased 0.4–2% per year No evidence on housing externalities
Koster and van Ommeren (2016)	The Netherlands	2007–2012	Renovation of public housing stock, plus some upgrading of green spaces and expansion of social programs; €2.75 billions invested	Geocoded data on house transactions between 2000 to 2014 Local deprivation index score used for program eligibility	Property prices Sales time	Fuzzy regression-discontinuity design: treated vs. non-treated neighborhoods around the cut-off score that determines elegibility	Sales time decreased only in the short run Property prices increased in the short & long run by 3.5% Moderate evidence on housing externalities
González-Pampillón, Jofre-Monseny, and Viladecans-Marsal (2016)	Catalonia, Spain	2004–2010	Recovery of public spaces (80% of funds) and renovation of housing stock (20%); €693 millions	Neighborhood-level socio-economic characteristics Elegibility criteria and maps of treated and rejected areas	University education Foreign-born EU15 and outside EU15	Parametric re-weighting difference-in-difference approach: 39 treated vs. 103 rejected or accepted but non-funded areas	Effects limited to historic districts in Barcelona Slowdown in the growth of foreign born Some gentrification proxied by level of education
Montolio (2016)	Catalonia, Spain	2008–2009	Renovation of public spaces (e.g. facilities, cultural infrastructure, schools, sports arenas; 177 euros per resident	Municipal employment and unemployment data by month Geocoded crime incidents	Local emp & unemp rates Local violent and property crime rates	Difference-in-difference and an instrumental variable methodology	Unemployment and crime rates decreased only in the short-run Car thefts, minor thefts and probability of repeated offenders dropped

Source: prepared by the authors.

The criteria to select targeted neighborhoods followed a two-round process that the authors use to identify the causal effects of the intervention. In the first round, the regional government determined targeted areas by setting a numerical threshold for an index score that combined urban and socio-economic indicators. In the second round, all areas with a score above this threshold had to submit a revitalization project to be eligible for funding. A committee based its final decision by mainly combining the first-round score and an assessment of the quality of the project. The program had ten calls from 2004 to 2010, but interestingly, only projects that were accepted prior to 2008 were fully executed given that at that time Catalonia suffered the effects from the Great Recession. The authors construct a treatment group using accepted projects between 2004 and 2007 and a control group from the pool of projects that were either rejected in the second round or accepted after 2007 but did not receive funding.[11]

González-Pampillón, Jofre-Monseny, and Viladecans-Marsal (2016) find that the policy has not been effective in reverting the ethnic dynamics of distressed neighborhoods despite huge investments. Nevertheless, when focusing on historic districts in the city of Barcelona, they find evidence of reversal in the tipping dynamic (e.g. a decrease in the share of immigrants from outside the European Union) and of gentrification as proxied by residents' college attainment. These results suggest that the effectiveness of urban revitalization policies could come at the cost of expelling groups of vulnerable residents.

Finally, Montolio (2016) studies the effect of local infrastructure investment policies on unemployment rates and neighborhood crime. To mitigate the severe effects of the Great Recession in Spain in the late 2000s, the central government announced a nationwide public fund of eight billion euros to finance investments in local infrastructure, the so-called Plan E. These funds were mostly spent on revitalization projects that improved local public spaces, facilities, schools, cultural infrastructure and sports arenas. One of the key objectives of the program was to boost the economic activity of municipalities focusing mainly on the creation of jobs in the construction sector, which was severely affected by the economic crisis.[12]

The author exploits the fact that projects were approved and executed at different random dates, which enables him to compare municipalities that

[11] González-Pampillón, Jofre-Monseny, and Viladecans-Marsal (2016) show that treated areas exhibit worse socioeconomic indicators than control areas. Therefore, as in Busso, Gregory, and Kline (2013), they apply the parametric re-weighting method developed by Kline (2011) to balance both treatment and control groups in all neighborhood characteristics that determine selection into the program (or pre-intervention characteristics). Reassuringly, treatment and control groups exhibit similar pre-treatment levels and trends in outcomes not directly used to determine selection. This method will be described in section 3.

[12] Municipalities had to submit an electronic proposal to secure funding. The great majority of municipalities (more than 8,000 in total) submitted a proposal and 99.6 percent of submissions were approved. The allocated funds averaged 177 euros per inhabitant up to a maximum of 5 million euros per municipality.

initiated local investments first with others that started later. Further, since this policy was launched in November 2008, after municipalities had submitted their annual budgets, the funds received were an unanticipated positive shock in the finances of local governments. Using a difference-in-difference approach and an instrumental variable estimation, Montolio (2016) finds that the program reduced unemployment rates mainly for the unemployed males who previously worked in the construction sector and, correspondingly, crime also declined significantly. He shows that some types of crimes like car thefts and other minor thefts decreased and that the probability of repeated offenses also dropped. However, he acknowledges that these effects occurred only in the short run.

Urban Revitalization Interventions in LAC

Many neighborhood revitalization initiatives have taken place in Latin America and the Caribbean, however, only a handful of studies have carefully assessed their impact. We review here those studies that carry out solid evaluations, keeping in mind that unlike the cases presented for the United States and the European Union, the types of interventions in Latin America are not pure urban revitalization initiatives. In addition, most of the interventions take place in peripheral areas that tend to exhibit lower quality of neighborhood services and public infrastructure. Table 2 summarizes the main characteristics, methods and findings of these studies.

González-Navarro and Quintana-Domeque (2016) analyze the impact on housing prices of a local urban revitalization program that provided street pavement to a set of unpaved city blocks in the town of Acayucan, Mexico. The authors, in coordination with the local authorities, randomly assigned the intervention among properties that lacked paved streets which allows them to clearly identify the causal effect of the policy. In addition, they conducted household surveys collecting data on housing and tenant characteristics in the baseline period (prior to the intervention in 2006) and three years after in 2009.

Using a difference-in-difference approach that compares outcomes before and after the implementation of the program, the authors investigate whether the project effectively improved streets in treated blocks and, more important, they examine whether these improvements were capitalized into higher housing prices. They show that the revitalization program increased self-reported housing prices and rents in treated zones by 28% and 36%, respectively. Moreover, they estimate that street pavement led to a doubling in the average number of home improvements a household engaged in over the previous six months: from 0.4 to 0.8 reforms. The rise in housing prices also boosted household durable-goods consumption through a wealth effect mechanism. As housing prices increased, they allowed households with prior access to credit to

get bigger financial loans and purchase more expensive durable goods; for instance, vehicle ownership increased by 43 percent, from approximately one in every four households to one in every three. Households with no prior access to credit increased consumption in home appliances.

Although they do not focus on a pure urban revitalization intervention, the article by Galiani and Schargrodsky (2010) studies the effect of allocating property rights in former urban squatters on overall economic development.[13] Policy makers are interested on the causal effect of granting property rights on housing values and other socio-economic indicators. However, measuring this effect is complicated since tenured dwellings may differ markedly from non-tenured dwellings (e.g. they tend to be in more traditional, richer neighborhoods), and so any observed difference in outcomes between them is not very informative. To address this concern, Galiani and Schargrodsky (2010) take advantage of a natural experiment in the allocation of land titles in unregulated settlements of Buenos Aires. In the early 1980s, squatters had occupied land plots in several suburbs of the city and the government decided to 'purchase' these plots from the owners and grant free property rights to the occupiers. Some owners accepted the deal while others did not and, thus, these uneven (spatial) decisions generated some random allocation process.

The authors find that land rights in unregulated settlements led to an increase in property values. They show that households that received a property right increased housing investment and education of their children. Interestingly, getting a property right did not increase access to credit. The authors conclude that this policy seems to be an effective tool to break intergenerational poverty.[14]

The Inter-American Development Bank (IDB) has funded a series of urban housing and development projects in Latin American and Caribbean countries of which 20 of them have been subjected to some form of impact evaluation. Ten of these projects have been implemented in Brazil, four in Ecuador, and single projects have been executed in Argentina, Bolivia, Colombia, Ecuador, El Salvador, Honduras, Mexico and Trinidad and Tobago. In half of these projects the targeted geographical area is a single municipality, whereas in the others, several localities are targeted as part of a national program. Most projects are assessed using a quasi-experimental methodology except for the case of Campo Grande, Brazil where a randomized trial was implemented. Four projects

[13] See also Field (2007) for evidence on the effects of granting property rights on female labor supply in Peru.

[14] Another revitalization program in Latin America that is worth mentioning is 'Favela Bairro II' in Rio de Janeiro, aimed at improving the quality of life in the slums. However, the impact evaluation strategy compares treated with non-treated slums using only post-intervention data, given that baseline information was not collected. Results show positive effects on access to basic services like sanitation and an increase in property values and income.

have finished and only one has been assessed. We provide here a brief description of their impact evaluation results.

Program for integrated urban development of the municipality of Campo Grande V, Brazil: An intervention of about $35 million invested in the revitalization of the city center (Orla Ferroviaria), the restoration of urban spaces (mainly in Orla Morena) and the improvement of transportation infrastructure (Via Morena and Avenida Julio Castilho). The project goal is to revitalize and improve the quality of life in neighborhoods that have experienced a continuous process of economic and urban deterioration, especially in downtown areas of the municipality. The project evaluation uses a difference-in-difference strategy to assess the impact of the program on housing prices in the treated area relative to the control area (defined as the surrounding neighborhoods of treated areas). The main finding shows there is no overall statistically significant impact of the interventions on property prices. The only meaningful increase in property prices (6.7 percent) takes place in the surroundings of Avenida Julio Castilho, which suggests a key role for improvement of transportation infrastructure.

Neighborhood improvement program—Third individual operation, Argentina: A slum-upgrading program (not a pure urban revitalization program) aimed at improving the living conditions of households located in shantytowns and unregulated settlements across the country. The main interventions consist of legalization of land tenancy ($2.5 million), improvement of urban infrastructure ($196 million in sanitation and social equipment) and urban development ($15.5 million), local management training ($4.5 million) and enhancement of local administration services ($3.5 million). This project has not yet been assessed but the IDB is currently working on the design of the evaluation strategy. Possible control groups include future treated areas (that are accepted but in later waves), areas with accepted but unexecuted projects, and areas with similar observable characteristics in other regions that were not included in the program. In addition, a boundary discontinuity design has also been suggested.

Table 2: Studies on urban revitalization interventions in Latin America and Caribbean countries.

Authors	Country	Period	Intervention	Data sources	Outcomes	Method	Findings
González-Navarro and Quintana-Domeque (2016)	Acayucan, Mexico	2006–2009	Local urban revitalization program that randomly provided street pavement to unpaved city blocks	Household surveys conducted by authors with data on housing and tenant characteristics	Property prices Land values Consumption of durable goods	Difference-in-difference: randomly paved-streets vs. non-paved-streets	Property prices and land values increased among newly-paved streets Consumption of durable goods increased
Galiani and Schargrodsky (2010)	Buenos Aires, Argentina	2003–2007	Allocation of property rights by the congress of the Province of Buenos Aires in former urban squatters	Two household surveys carried out in 2003 and 2007 with data on socio-economic indicators	Property prices Housing investments Children education	Natural experiment: property rights randomly allocated between occupiers	Property prices and housing investments increased Children education increased No improvements in access to credit
IDB Urban development and housing Project: BR-L1104	Campo Grande, Brazil	2009–2013	Revitalization of city center, restoration of urban spaces and improvement of transport infrastructure (Avenida Julio Castilho)	Geocoded data on property transactions from Nov 2008 to Feb 2013 Historic information on property characteristics from land registry	Property prices	Difference-in-difference: control areas defined as surrounding neighborhoods of treated areas	Property prices increased only in the short run and possibly due to improvement in transportation
IDB Urban development and housing Project: AR-L1179	Urban squatters, Argentina	2013–2016	Legalization of land tenancy, improvement of sanitation and social equipment, local management training	Data on property prices Detailed data on urban revitalization interventions Census 2001 and 2010	Property prices	Proposed evaluation strategy: difference-in-difference	Under evaluation

Source: prepared by the authors.

Brief Review of Place-Based Policy Interventions

We mentioned in the introduction that urban revitalization interventions can be embedded within a broader set of place-based urban policies that are aimed at improving the economic performance and quality of life in a deprived neighborhood. These broader policies do not necessarily concentrate exclusively on places as urban revitalization initiatives, but also target disadvantaged residents in those underperforming neighborhoods. The extant literature that evaluates the impact of place-based interventions is large and has advanced the empirical methods used in the evaluation of spatially targeted urban policies. We do not provide in this report a detailed review of the main studies on place-based policies, we only summarize them in tables 3 and 4. The main goal is to illustrate how the methods and data sources share many features with studies on urban revitalization. Further, one of the methodologies that will be presented in the next section elaborates on the methods applied in studies of urban place-based policies.

Most studies that assess the impact of place-based policies focus on the experiences in the United States and France. Both countries have designed tax incentive programs aimed at increasing local employment. Enterprise zones are the main example of place-based policies in the United States given their large geographic coverage and amount of funding. These are federal level initiatives that started in 1993 and provided tax grants and hiring credits through two programs: Empowerment Zone (EZ) and Enterprise Communities (EC). The EZ program subsidized economically distressed areas within a city mostly through tax incentives, whereas the EC program provided much smaller incentives packages.

The first round (EZ round 1) of the federal EZ program began in 1994. The Department of Housing and Urban Development (HUD) oversaw designating the empowerment zones in urban areas based on two criteria: high level of poverty rate (at least 20 percent) and high unemployment rate (greater than 6.3 percent). Applications were submitted by state and local governments and only six urban communities from Atlanta, Baltimore, Chicago, Detroit, Philadelphia/Camden and New York were awarded an EZ program out of 78 applicants. Several of the rejected proposals (referred to as 'runner-up' areas) were awarded a less generous EC program. Table 3 summarizes the main characteristics, methods and findings of studies that assess the impact on individuals, businesses and neighborhoods of EZ round 1 designation.

France also launched an ambitious enterprise zone program in 1997, specifically targeted to municipalities with high levels of unemployment and low levels of education. Like the us enterprise zone program, the French program was also designed to boost local economies in deprived areas by exempting new and

existing business from paying business and corporate taxes as well as social security contributions for a minimum of five years.[15] More specifically, firms that hired residents (at least 20 percent of their employees in payroll) received a tax exemption that represented around a third of total labor costs. Firms were required to have fewer than 50 employees and total sales below a lax threshold. In 1997, the French government awarded the 'Zones Franches Urbaines' (ZFU) program to 44 territories identified as the most disadvantaged areas, and the 'Zones de Redynamisation Urbanie' (ZRU) to other 416 territories. The ZFU zones received a more generous incentives package. In 2004, 41 zones from the pool of 416 ZRU territories were granted ZFU status while 15 new territories were incorporated to the ZFU pool in 2006. Table 4 summarizes the main characteristics, methods and findings of studies that assess the impact on individuals, businesses and neighborhoods of ZFU designation.

The evidence on the local effects of enterprise zone designations is very mixed and as suggested by Neumark and Simpson (2015) more research is needed to disentangle which are the program features that lead to successful outcomes. The studies for the United States suggest that there is little effect on employment levels and employment growth in intervened neighborhoods, although there is much heterogeneity around the results (e.g. more jobs are created only in areas with lower manufacturing jobs and where local managers engage in outreach activities). The study by Busso, Gregory, and Kline (2013) uses more comprehensive data and a solid methodology that makes treatment and control areas comparable and finds there are notable increases in employment and earnings per worker and no evidence of an increase in the cost of living. Meanwhile, the empirical evidence for France shows that enterprise zone designation increased employment growth and establishment births and decreased duration of unemployment spells, especially in the short run. However, the increase in the number of establishments in targeted areas appears to be driven from relocations of nearby establishments, indicating a displacement effect of the policy. This evidence on negative spillover effects is also found in the United States (Hanson and Rohlin, 2013).

Summary and Discussion

We now summarize and discuss key takeaways from the extant evidence on urban revitalization programs for developed countries and Latin American countries.

[15] The program also facilitated the relocation of some firms away from targeted areas through property, corporate income and wage tax reliefs.

- First and foremost, causal identification on the effects of urban revitalization interventions is crucial. Many studies and evaluations compare outcomes (e.g. housing prices or crime rates) before and after an urban revitalization intervention in a particular neighborhood or sets of neighborhoods. These studies are by construction plagued by omitted variable biases and sample selection concerns. We will elaborate on these issues when we present the methodology to evaluate the impacts of revitalization policies in the next section. What is important to stress is that all the studies discussed try to identify causal effects of the revitalization policies by using methods that compare pre- and post-outcomes in treated neighborhoods with changes in outcomes in similar control neighborhoods or counterfactuals.

- It is fundamental to know the geographic scope and implementation details of any urban revitalization intervention. In most of the studies reviewed, the authors know the precise boundaries of the policy and can identify the blocks that are directly affected by the intervention. The spatial units (e.g. parcels or blocks) that benefit directly from urban revitalization do not always correspond one to one to administrative or political units. This is a source of concern when data are available at larger spatial scales (e.g. a municipality) that may contain areas that were treated and others that were not. Moreover, the intervention may overlap several areas of two or more municipalities. These concerns can be addressed by simultaneously getting access to the precise boundaries of the revitalization intervention and by narrowing down the spatial level at which data are collected (e.g. block-level or household-level data like in a census).

- Identifying proper counterfactuals is an essential stage in the evaluation process. We will provide details on this matter in sections 3 and 4. Further, it is recommended to select more than one counterfactual or control area. A single control area, as in the article by Rossi-Hansberg, Sarte, and Owens (2010), may experience a change or shock after the implementation of the revitalization policy that is not observed by the researcher. To overcome this concern, the researcher can choose in practice more than one control area so that all potential unobserved changes average out across control groups (Ahlfeldt, Maennig, and Richter, 2016). We should keep in mind that the methods used in the assessment of revitalization interventions compare differences in means between treated and non-treated neighborhoods and thus, a priori, the larger the number of units in both groups the less likely these averages will reflect noise.

- The great majority of studies use as main outcome of interest either land or property prices. The underlying idea is that any effect of a revitalization intervention should be capitalized in the value of land. In fact, economists

measure welfare gains of local policies through changes in earnings and land prices. Of course, there are some assumptions behind and, as stated by Koster and van Ommeren (2016), this approach makes more sense when assuming absentee landowners and no housing search costs in markets. In reality, land and housing prices may not respond immediately after revitalization occurs due to the fact that it takes time to sell a house and this adjustment often takes longer in neighborhoods with higher homeownership rates. In addition to differences in housing prices, Koster and van Ommeren (2016) propose to examine differences in sales time of properties in treated and control neighborhoods as a short-run outcome to proxy success of the revitalization intervention.

- Although the value of land is theoretically the main outcome of interest, we rarely observe many land transactions in neighborhoods that are already developed, and those land plots that are traded may not be representative of typical land plots. One alternative is to examine housing prices. Housing transactions are much more common and sample selection is (relatively) less of a concern. One limitation is that the researcher generally does not observe differences in quality across houses neither renovations that may have taken place after the implementation of the revitalization policy. Higher quality or recent renovations lead to higher housing prices and may confound the treatment effect of the urban revitalization intervention. Therefore, the researcher should include controls that can partially account for observed differences in housing prices and must be aware about the (upward) bias that these unobserved differences (e.g. quality or renovations) may cause on the estimated effect of the policy.

- One could think that other outcomes of interest could be potential dependent variables in the analysis. For instance, an urban revitalization intervention could decrease the level of crime in a neighborhood, improve the quality of existing businesses or attract new businesses that hire residents and, hence, reduce average commuting times. Indeed, the researcher could consider these alternative outcomes and many others, however, changes in any of these outcomes should also get, partially or fully, reflected in higher land values. Then, looking at changes in land or housing values that are due to the intervention is a natural way to estimate the effects of the policy, at least those that get reflected in private returns. Of course, the researcher might also be concerned about other indicators, especially those that may better capture social returns (e.g. education spillovers or changes in social networks).

- Another advantage of examining housing prices is that data is generally available, whereas collecting data on alternative outcomes is sometimes quite difficult or, in many cases, it is hard to even conceptualize an outcome.

While local crime rates are gradually becoming available due to GIS technology and better coordination among police stations, commute times are rarely available unless we rely on time surveys that interview few individuals at a high cost. Moreover, business turnover is high in central areas of cities so documenting the opening of new business is not straightforward, unless we rely on annual business registers that are not common in many Latin American cities. Further, measuring business quality is an abstract concept that requires many assumptions and may become quite data intensive. Recent studies are making notable progress by relying on Google Maps and machine learning techniques that examine changes in the aspects of local stores (e.g. coffee shops replacing pawn shops or old diners). Although this is a promising avenue of research, we are still at an early stage to rely on these sources of data to construct manageable outcomes, especially in the Latin American context.

Other more complex outcomes could also be considered. For example, some individuals may experience a decline in social stigma after a revitalization intervention as they may feel there is no longer a problem to self-identify as residents of the treated neighborhood. This would be a highly valuable outcome of the intervention, yet, it is even more complicated to outline than others. How do we construct a measure of social stigma? A priori we can design a survey and ask respondents to rate their levels of stigma from one to ten. Sociologists have studied this concept and reinforced their analyses with qualitative methods, but when we try to assess quantifiable effects of urban revitalization interventions these numbers or indices are quite hard to interpret. Moreover, several studies have documented a pro-project bias when residents are asked about the benefits of the revitalization intervention. The easiness to interpret results is one of the main advantages of using land or housing prices compared to other outcomes or indices. Besides some of the caveats already mentioned, one of the key advantages of land or housing prices is that they are expressed in monetary units and allows to approximate measures of welfare.

- Most of the studies reviewed on the effects of urban revitalization interventions show that land and housing prices increased annually by three percent because of the policy, with estimates in the range from one to five percent.[16] The evidence is more mixed for studies on the effects of enterprise zones. In addition, findings reveal that housing investments and the prevalence of buildings in good condition increased in intervened neighborhoods in tandem with property prices. Most of these studies rely on administrative data on assessed residential value and property transactions with precise location of properties. The use of survey data with self-reported

[16] It is worth noting that larger effects are found in the United States for low-density neighborhoods with higher homeownership rates, while the estimates in denser European neighborhoods are generally lower. The latter seem to be more appropriate for central neighborhoods in Latin American cities.

housing values appears to be a lower-quality substitute when administrative data are not available.

- In addition, most of the studies reviewed find mild to moderate evidence of housing externalities, that is, spillovers of the intervention on housing values of nearby properties outside of the targeted areas. Nevertheless, these externalities when present are extremely local and decay rapidly—by one half every 1,000 feet in Rossi-Hansberg, Sarte, and Owens (2010) and disappear after 1.5 km in Koster and van Ommeren (2016), though presumably they decay at a faster rate given that their geocoded data are less precise. These findings suggest that externalities, though present, do not appear to be quantitatively very important and give support to the use of neighboring areas as potential valid controls or counterfactuals. Other studies also show that housing externalities increase with exposure to and with the size of the revitalization intervention (Autor, Palmer, and Pathak, 2016). In fact, a few studies that do not estimate causal effects but only associations indicate that increases in property prices are larger when properties are exposed to new construction as opposed to rehabilitation and are also larger when revitalization interventions are more spatially concentrated than dispersed throughout the neighborhood (Ding, Simons, and Baku, 2000).

- The two studies on the effects of revitalization interventions in Latin America that we reviewed show large effects in property prices, housing investments and other indicators like consumption of durables. González-Navarro and Quintana-Domeque (2016) find that property prices in paved streets increased by 17 percent according to appraisers, by 28 percent when using homeowner valuations and by 134 percent when looking at properties that were traded (while rents also increased notably by 36 percent). Galiani and Schargrodsky (2010) also show that an index of housing quality increased by 37 percent in properties that received a property right. These effects are large given that interventions took place on peripheral areas and so we should expect much smaller effects on housing values and other outcomes for revitalization interventions in central neighborhoods.

- Studies that evaluate the impacts of urban revitalization interventions do not examine labor market outcomes. This is not surprising since the focus of these policies is not to target disadvantaged residents, but to enhance the functionality of the areas and attract commercial activity and new residents. The choice of labor market outcomes makes more sense in the evaluation of place-based policies and, generally, studies in the Unites States and France have found mild to moderate increases in employment growth, in mean incomes and reductions in unemployment rates (though only in the short run) in designated areas. It is hard to anticipate any effects of revitalization

interventions on employment outcomes and informal sector jobs, an outcome that is quite relevant for the Latin American context. Even if there are any meaningful effects, the mechanisms are not apparent beforehand. Therefore, unless an urban revitalization program specifically addresses the extent of informality in the intervened area (e.g. by prohibiting local street vendors), we should not expect any effect of the policy on these outcomes. We will extend this discussion and comment on the data requirements to test these outcomes in the Latin American context in section 4.

- In addition, studies that evaluate urban revitalization interventions have also not paid attention to establishment outcomes such as business turnover or new openings. In contrast, studies that assess place-based policies have found that new establishments open in treated neighborhoods as a result of the zone designation, yet there is plenty of evidence for a displacement effect where nearby establishments relocate across boundaries to benefit from subsidies. Again, we will extend this discussion and comment on the data requirements to test these outcomes in the Latin American context in section 4.

- On a final note, a burgeoning literature is studying the effects of historic districts' designation on land and property prices. Preservation advocates argue that a market-driven process is not able to protect a neighborhood's heritage, as developers cannot internalize the full benefit to society of historic preservation. They argue that preservation can generate positive spillovers by increasing tourism and thriving the art scene in the neighborhood. Conversely, critics of preservation argue that historic designation is another type of lobby by incumbents that are against change and in favor of restricting housing supply. The effect of historic designation on land and property prices is not, a priori, clear. On the one hand, developers are not allowed to build higher in low-density high-demand areas, which negatively affects land and property values. On the other hand, designation may increase the amenity value in the neighborhood by preserving historic beauty and may reflect in higher property values. In a recent paper, Been, Ellen, Gedal, Glaeser, and McCabe (2016) find that historic designation in New York City increases property values but mainly in lower-valued neighborhoods outside Manhattan. In addition, the blocks surrounding the historic district experience an increase in value after designation, which suggests that houses located right outside a historic district benefit from the amenity but are unaffected by strict regulations on the use of land and property improvements.

Table 3: Studies on enterprise zone (EZ) designations in the United States

Authors	Program	Data sources	Outcomes	Method	Findings
Hanson (2009)	EZ Round 1	Census-tract level data in 1990 and 2000	Local employment rates Poverty rates Median house values	Difference-in-difference and instrumental variables: treatment areas are those that received tax incentives while control areas are rejected or runner-up areas	No effect on employment levels No effect on poverty rate growth Median house values increased
Kolko and Neumark (2010)	EZ Round 1 State-level version for California	Panel of establishments with information on sales and employment Detailed geographic information on the coverage of the policy	Employment rate Job creation	Difference-in-difference: treated areas are those that received an EZ in California while control areas are those that received EZ incentives but later in time	No effect on employment growth Positive effect on job creation in areas with lower share of manufacturing and with local managers active in marketing and outreach activities
Hanson and Rohlin (2013)	EZ Round 1	Census-tract level data in 1990 and 2000	Number of establishments	Difference-in-difference: neighboring areas of a treated region compared with neighboring areas of a rejected region before and after the policy intervention	Number of establishments in areas close to boundaries of EZ designations drops Negative spillover effect hypothesis confirmed
Busso, Gregory, and Kline (2013)	EZ Round 1	Confidential individual-level data from decennial censuses 1980–2000 Longitudinal Business Database with data on establishments	Number of jobs Mean earnings per worker Local rents and house values	Parametric re-weighting difference-in-difference approach: control areas are rejected and future applicants	Local employment and mean earnings per worker increased No effect on the cost of living Based on both findings, residents' welfare in targeted zones increased

Notes: The first round of the Empowerment Zone program (EZ Round 1) consisted of subsidies to firms located in distressed areas of a city mostly through tax incentives (e.g. tax credits for hiring workers who live and work in these areas) and block grants to be used in business assistance, infrastructure investment, training programs, etc. The main period of analysis in all studies spans from early 1990s to early 2000s.

Source: prepared by the authors.

Table 4: Studies on enterprise zone (Zone Franches Urbaines, ZFU) designations in France

Authors	Round	Data sources	Outcomes	Method	Findings
Gobillon, Magnac, and Selod (2012)	ZFU Round 1 Paris region	Panel data on unemployment spells and geocoded locations of individuals in unemployment (1993–2003)	Unemployment duration in days	Difference-in-difference: municipalities that received exemptions from social security contributions compared with similar municipalities that did not receive them	Duration of unemployment spells for individuals in treated areas decreased, but only in the short (less than three years)
Givord, Rathelot, and Sillard (2013)	ZFU Round 2	Geocoded administrative data on the universe of establishments from 2002 to 2007	Employment rate Number of new firms	Difference-in-difference: territories treated in ZFU round 1 compared to two control groups: territories treated in ZFU round 2 and ZRU territories that never got ZFU status	Rise in employment growth Number of new firms increased Evidence on negative spillover effects: firm birth rates declined in areas nearby ZFU territories
Briant, Lafourcade, and Schmutz (2015)	ZFU Round 2	Panel data on establishments Topographical maps to proxy spatial isolation of areas (e.g. centrality and transport accessibility)	Employment rate Number of establishments	Difference-in-difference: compare outcomes in treated and control areas (defined as ZRU territories) before and after the policy	Employment growth and firm birth rates increased but only in the least isolated ZFU territories
Mayer, Mayneris, and Py (2015)	ZFU Round 2	Census-block panel data with composition of establishments (2000–2007) Maps with boundaries of targeted zones	Location of establishments	Difference-in-difference: compares outcomes in the treatment group (ZFU round 2) and the control group (ZRU at the time of round 2, 2004) before and after the policy	Number of establishments in targeted areas increased Evidence of displacement effect: new establishments mainly relocate from non-ZFU territories to ZFU territories

Notes: Zones Franches Urbaines ZFU Round 1 and 2 exempted new and existing business in distressed areas from paying business and corporate taxes as well as social security contributions for a minimum of five years. ZFU Round 1 began in 1997 while ZFU Round 2 followed in 2004. See main text for additional details.

Source: prepared by the authors.

Evidence on Neighborhood Gentrification and Displacement

The literature that relates urban revitalization interventions to the neighborhood dynamics of population flows is scarce and not quantitatively solid. The relatively few studies available have mainly focused on registering the in-migration of new residents and business openings that follow the revitalization intervention. However, as already discussed, revitalization efforts capitalize in land values and potentially affect over time disadvantaged residents who are less likely to afford higher rents. Unfortunately, there are no studies that evaluate the causal effect of revitalization policies on potential displacement of residents. We instead focus on a growing literature that explores whether neighborhoods that experience increases in mean incomes and rents (referred to as gentrifying or ascending) also face higher levels of displacement.

It is important to distinguish the difference between any type of displacement associated to revitalization interventions and that associated to gentrifying neighborhoods. This distinction is highlighted in Zuk, Bierbaum, Chapple, Gorska, Loukaitou-Sideris, and Ong (2015) as "the flows of capital versus flows of people to neighborhoods." On one hand, any revitalization policy brings a huge inflow of public and private capital that suddenly affects the neighborhood landscape. This is a large perturbation and so we might expect in response swift changes in neighborhood composition. On the other hand, the inflows of *gentrifiers* in central cities are a consumer driven response by people with cultural and aesthetic preferences that are also in search of lower rents. This demand side response is more gradual over time and thus any observed displacement linked to gentrification should be harder to detect. We might thus expect any displacement due to revitalization interventions to be stronger than the type of displacement we will review below.

This section is organized as follows. First, we explain how scholars measure and document neighborhood gentrification with a focus on the United States. Second, we review recent studies that analyze the incidence of displacement in gentrifying neighborhoods, which may have become more prosperous due to several factors and not necessarily because of an urban revitalization intervention. Third, we present evidence on the potential consequences of gentrification; although this is not causal evidence by any means, it could shed light on how to better guide future research on this topic. Finally, we discuss the main takeaways of this section.

Stylized Facts and Drivers of Neighborhood Gentrification

Gentrification has gradually become a matter of concern for central and local governments due to its potential harmful effects on the most vulnerable sectors

of the population. In recent years, it has received much more attention in the media as it quickly propagated to more cities and became an apparent phenomenon. Hwang and Lin (2016) review the literature on gentrification in the United States to shed light on its causes and consequences during the 21th century. They broadly define gentrification as a sharp change in socio-economic status experienced by neighborhoods within a period (usually ten years). While us cities tend to be characterized by the poor living in the city center and the rich in the suburbs, since the seventies and especially since 2000, downtown areas of many cities have experienced a profound change in their economic configuration and have embarked on a process of gentrification.

Hwang and Lin (2016) document a series of stylized facts regarding the gentrification process across us cities. First, using an index of socio-economic status that combines neighborhood college attainment and average income, they observe that neighborhoods located in downtown areas experienced a large increase in this index particularly after 2000.[17] Second, this gentrification process seems to be more prevalent in larger cities, although city centers of medium-sized cities have also started to gentrify recently. This latter fact might explain why there is an increase in the level of awareness by public authorities and other institutions related to gentrification and its consequences. Third, neighborhoods located in central business districts (CBDs) are now experiencing changes in the composition of households without overall growth in population, an indication that this process could potentially be displacing people. Fourth, the number of jobs demanding low skills is gradually declining in neighborhoods in the city center. Fifth, improvements in socioeconomic status are taking place only in a handful of CBD neighborhoods while neighborhoods around the CBD keep exhibiting lower socio-economic status. Finally, there is a lot of heterogeneity across cities when examining these changes in socio-economic status of neighborhoods.

From a theoretical perspective, several factors such as the quality of housing, the level of local public goods, the presence of amenities and the quality and efficiency of the transportation infrastructure may lead neighborhoods to change. Recent studies attempt to disentangle the extent to which these factors are driving neighborhood change. Edlund, Machado, and Sviatchi (2015) and Baum-Snow and Hartley (2016) observe that the time value of commuting has increased with income over the last decades, especially for individuals with a college degree. This rise in the value of time has come together with a national increase in the rates of assortative mating (i.e., individuals are over time more likely to have a partner with their same level of education), which has intensified the rise in the opportunity cost of time for more educated couples. Both studies

[17] This index is constructed by combining the within metropolitan area rank of a census tract in terms of the share of adults 25 years and older with college degree and average household income.

show that as a result of these national trends, more jobs demanding high skills have been created in central areas of cities in response to households' desire to minimize joint commuting times. Further, this expansion in the number of college-educated residents in the city center may also attract other types of services and goods to the area and, thus, boost the attractiveness of these gentrifying neighborhoods. In sum, better transportation infrastructure and job accessibility seem to partly explain changes in neighborhood composition, mainly in areas close to the city center.

Local amenities are another potential driver of neighborhood change. Baum-Snow and Hartley (2016) note that nowadays individuals of high socio-economic status have a higher valuation of amenities in downtown neighborhoods. Couture and Handbury (2016) find that taste for amenities (like restaurants, bars or personal services) plays an important role in explaining the residential location decision of the young and college-educated that live close to downtowns. They argue that this rise in taste for amenities was particularly salient between 2000 and 2010. Despite this evidence, the extant literature falls short of identifying those types of amenities that stimulate redevelopment and eventually lead to gentrification. For instance, Schuetz (2014) finds that art galleries, a usual suspect of initial neighborhood revival, do not cause gentrification but instead galleries tend to locate in neighborhoods that are more likely to attract high-income residents and commercial activity, even in the absence of bohemian intervention.

Other factors like public infrastructure could also fuel the gentrification process. However, the empirical literature that relates local public interventions to the dynamics of population flows is scarce. The evidence on the neighborhood effects of new affordable housing projects in developed countries reveals that property prices increase after these interventions. Diamond and McQuade (2016) estimate housing spillovers of properties financed by the Low-Income Housing Tax Credit (LIHTC) in the United States and find that this type of development revitalizes low-income neighborhoods and increases housing prices by 6.5 percent. Neighborhoods that experience new LIHTC development experience a decrease in crime rates and inflows of racially and income diverse populations, however, the effects of development change sign (i.e., housing prices fall) when these developments are placed in more affluent neighborhoods (with a median income above $54,000 and a minority population below 50 percent). Moreover, re-building or demolition of run-down public housing projects or urban initiatives such as the HOPE VI program may intensify the gentrification process. Aliprantis and Hartley (2015) estimate a large decrease in crime after the closure and demolition of roughly 20,000 units of high-rise public housing in Chicago, which could spur a gentrification process. Although these studies do not mention this explicitly, the provision of affordable housing

can be viewed as a type of urban revitalization that generates large increases in property prices especially in low-income communities.

Other public interventions that enhance the value of amenities such as historic district designation, business improvement districts, zoning and land use changes, tax increment financing practices, homelessness removal policies and beautification of public spaces could all potentially accelerate a gentrification process. As already mentioned, the urban revitalization policy implemented by the government of Catalonia seems to be intensifying the gentrification process in historic districts of Barcelona (González-Pampillón, Jofre-Monseny, and Viladecans-Marsal, 2016). Similarly, using decennial census-tract data between 1970 and 2010, McCabe and Ellen (2016) estimate that neighborhood mean household income and the share of college-educated residents increased after historic district designations in New York City. For instance, mean income in designated neighborhoods increased by 6 percent after a decade and between 4 to 6 percent in future decades relative to surrounding neighborhoods, whereas the increase in the share of residents with college was also large at between 5 to 10 percent.

Does Gentrification Lead to Displacement?

Relatively few studies have come close to answer this question. The main reason behind is the lack of appropriate data that can track individuals that move across neighborhoods. We summarize two main articles that rely on rich, nationally-consistent data for the United States and then briefly comment on other articles that present more local evidence. The studies by McKinnish, Walsh, and White (2010) and McKinnish and White (2011) use individual-level census data for 1990 and 2000 that allow the authors to identify the characteristics of those residents who moved in and stayed in neighborhoods that experienced rapid changes in income (a proxy for ascending or gentrifying neighborhoods). The quality of these data is a substantial advantage in relation to previous studies that rely on aggregate data at the neighborhood or census-tract level.

McKinnish, Walsh, and White (2010) study residential mobility patterns in gentrifying low-income urban neighborhoods during the nineties. The main novelty in their study is that they have access to confidential census data, the Long Form survey, which is administered to a 1-in-6 sample of all households in the United States and contains a set of socio-economic characteristics as well as the household's location in 1990 and 2000. This micro-level data enables the authors to construct socio-economic characteristics for each census tract (a neighborhood proxy of 4,000 residents on average). In addition, these data allow them to better identify who moves in, who moves out and who stays in

these neighborhoods over a decade while also distinguish the socio-economic characteristics of in-movers and stayers.

McKinnish, Walsh, and White (2010) establish some necessary categorizations: low-income tracts are those with a mean family income in the bottom quintile of the 1990 distribution, whereas gentrifying tracts or neighborhoods are those low-income tracts which experienced an increase in mean family income of at least $10,000 between 1990 and 2000. Based on this categorization most of the gentrifying tracts are in central cities. The main empirical strategy compares socio-economic characteristics of in-migrants to gentrifying tracts with those of in-migrants to low-income but non-gentrifying tracts. Additionally, they compare characteristics of in-migrants to low-income gentrifying tracts with those of in-migrants to middle-class neighborhoods defined using neighborhood income in 2000. This latter group is more broad and richer on average as it includes neighborhoods that were low-income in 1990 (and gentrified throughout the nineties) and stable middle-class neighborhoods. For the case of out-migration, one limitation of the Long Form census data is that it is not possible to identify those individuals who exit gentrifying neighborhoods. To partially address this concern the authors, compare cohorts of individuals in gentrifying and non-gentrifying tracts. That is, for every tract in 1990, they create groups based on age, race and education categories and examine whether each group decreases in size by 2000. Any larger reduction in the size of a group in gentrifying tracts compared to non-gentrifying tracts could indicate there was excessive out-migration for that specific group or cohort. This method, known as synthetic cohort analysis, will be presented in section 3.

The authors find evidence that gentrifiers, i.e., those who are more likely to move into neighborhoods that experienced a notable increase in mean income over the nineties, are typically white, childless, under 40 years of age and with college. Regarding out-migration, results show that black or Hispanic householders do not disproportionately exit gentrifying neighborhoods. In fact, they observe that a high share of in-migrants into gentrifying tracts is accounted by black high-school graduates, particularly in predominantly black gentrifying neighborhoods. Although the authors estimate a slightly higher exit of households with low education and marginally higher retention of households with high education in gentrifying neighborhoods, in general, results suggest that gentrification of predominantly black neighborhoods makes these neighborhoods more attractive to middle-class black households.

One of the most salient findings is that mean incomes for the group of black householders with high school degrees who remain in gentrifying neighborhoods increase at least 20 percent more than for the same group in non-gentrifying neighborhoods. Although this rise in income is not necessarily a consequence of gentrification, several findings in this study cast doubts on the

highly negative view associated to gentrification processes, which tends to relate gentrification to displacement of minorities and to the absence of positive neighborhood spillovers.

In a related study, McKinnish and White (2011) analyze residential mobility in mixed-income neighborhoods to shed light on income sorting and segregation patterns. They study whether mixed-income neighborhoods attract heterogeneous or homogeneous in-migrants in terms of income. This is an important research question since mixed-income neighborhoods can be viewed as neighborhoods that have experienced recent gentrification. The types of in-migrants that move into mixed-income neighborhoods are key to predict whether these neighborhoods will remain income diverse or tip into either poor or high income. Again, to carry out this analysis they use confidential census data from 1990 and 2000 (the Long Form survey data) that allow them to identify moves of individuals at the tract or neighborhood level. The main empirical specification regresses the coefficient of variation on income in 2000 (i.e., the ratio between the standard deviation of income and its mean) calculated for different in-migrant cohorts on the 1990 coefficient of variation for the same neighborhood.[18] A positive relationship between these two variables would imply that mixed-income neighborhoods tend to attract pools of in-migrants that are heterogeneous or diverse in terms of income.

McKinnish and White (2011) find that in-migrants from the lower and upper tail of the income distribution are more likely to move into mixed-income neighborhoods. This result reveals that mixed-income neighborhoods were stable during the nineties and reinforces the findings in McKinnish, Walsh, and White (2010) where gentrifying neighborhoods face an influx of higher-income residents and do not experience displacement of minorities. However, the authors also show that the correlation of income dispersion for past and more recent in-migrant cohorts is less than one which suggests that mixed-income neighborhoods are becoming less heterogeneous and less likely to remain stable over time. Finally, they also show that neighborhoods with high minority shares attract a less economically diverse set of in-migrants.

Several other articles are narrower in scope and study the link between gentrification and displacement in a specific location. Vigdor (2002) uses American Housing Survey (AHS) data from 1974 to 1993 to analyze gentrification in the Boston metropolitan area. He finds no evidence that low-income households are more likely to exit their housing unit in gentrifying neighborhoods. Similarly, Freeman and Braconi (2004) use data on New York City's rent regulation policy in the nineties to test whether mobility increased in

[18] They consider additional measures of income dispersion such as the ratio of the mean to the median and the interquartile range standardized by the median.

seven gentrifying neighborhoods in Manhattan and Brooklyn. They estimate that mobility of low-income households was lower in gentrifying than in non-gentrifying neighborhoods. More recently, Freeman, Cassola, and Cai (2015) study the relationship between gentrification and displacement for England and Wales. They rely on annual data from the British Household Panel Survey (BHPS) between 1991 to 2009 which contains socioeconomic and demographic characteristics as well as the precise geographic identifiers. Like in the studies for the United States, Freeman, Cassola, and Cai (2015) do not have information about the reasons for moving and, thus, propose an approach that compares mobility rates between gentrifying and non-gentrifying neighborhoods after controlling for life-cycle and housing context factors. The authors do not find evidence of higher mobility rates in gentrifying neighborhoods and note there is only weak evidence of elevated mobility rates for low-income households in gentrifying neighborhoods in London.

Consequences of Gentrification on Individual Outcomes

Most of the literature on gentrification focuses on identifying gentrifying areas and examining its principal drivers. However, very few studies pay attention to the consequences of gentrification. Gentrification may change the whole environment of the neighborhood especially when it induces displacement of long-time residents.

Ding and Hwang (2016) study the relationship between financial health, measured through individual credit scores, and gentrification in Philadelphia from 2002 to 2014. To that end, the authors combine two sources of information: data from the Federal Reserve Bank of New York (FRBNY) Consumer Credit Panel/Equifax, which contain detailed information on consumer financial health and credit use, together with addresses of adult individuals that enable the authors to track mobility patterns; and data from the 2000 census and the American Community Survey (ACS) from 2009 to 2013. Using the latter data sets, the authors define gentrifying neighborhoods as those census tracts that were initially low-income (per the 2000 census) and experienced a large boost in median gross rents or median home value (above the citywide median increase) and in the percentage of college graduates from 2000 to 2013. Once gentrifying neighborhoods are identified, the authors can monitor individuals' credit risk score and their location decisions as they decide to remain in the neighborhood or move to another area.
Ding and Hwang (2016) argue that residents in gentrifying neighborhoods could experience gains and losses from gentrification. On the positive side, financial firms may open branches in gentrifying neighborhoods and improve access of residents to better financial products and information. Moreover, these openings

may boost the neighborhood economy and potentially improve residents' labor market outcomes and, eventually, their financial health. On the negative side, housing prices and living cost may increase affecting the financial health of the most vulnerable residents. In sum, from a theoretical perspective gentrification could either improve or harm the financial health of different groups of residents.

To analyze the relationship between gentrification and financial health, the authors estimate a series of linear regression models. In particular, they regress for stayers the three-year period change in the credit risk score on an indicator variable that takes value one if the neighborhood is gentrifying plus a set of control variables such as the initial credit risk score and individual and financial characteristics (e.g. age groups, whether the individual has a mortgage or financial accounts in serious delinquency, etc.). In a separate regression analysis, the authors focus only on residents who live in gentrifying neighborhoods to test whether there are differences in financial health between recent movers and non-movers in the neighborhood. It is important to point out that all these analyses provide only a correlation and are far from identifying a causal relationship of gentrification on financial health because of endogeneity issues (e.g. individuals who expect high improvements in credit risk scores may sort into gentrifying neighborhoods and thus generate in the data a positive correlation between gentrification and financial outcomes).

Results show that non-movers in gentrifying neighborhoods experience an increase in their financial health as measured by Equifax credit risk scores. A more striking result shows that this positive association is even stronger among those non-movers living in neighborhoods in the more advanced stages of gentrification. Furthermore, those stayers that are highly vulnerable also experience an improvement in their financial health, though the magnitude of the effect attenuates. Overall, it appears that all residents in gentrifying neighborhoods (older or younger, short-term or long-term, with high or low credit scores) benefit from better financial health compared to similar residents in non-gentrifying neighborhoods. On the negative side, results suggest that vulnerable residents who leave gentrifying neighborhoods and move to poorer neighborhoods see their credit scores decrease relative to those residents who stay.

Another recent study that has examined the consequences of living in gentrifying neighborhoods compares the outcomes for public housing residents in New York City in three different neighborhood types: persistently low-income, persistently high-income, and increasing income (a proxy for gentrification or ascent). The study by Abt Associates and NYU's Furman Center (2015) reveals that public housing residents in more affluent neighborhoods exhibit better socio-economic outcomes despite having similar characteristics (e.g. same racial and age composition) as public housing residents in more disadvantaged

neighborhoods. For instance, annual household incomes were $4,500 and $3,000 higher in high-income and increasing-income neighborhoods, respectively, than in low-income neighborhoods. Moreover, as expected, violent crimes rates faced by residents were notably lower in more affluent neighborhoods. Finally, public housing residents in increasing-income neighborhoods go to schools with higher test scores, but even more important, the kids in these public housing projects score much higher grades in math and reading. Therefore, the scant empirical evidence on the consequences of gentrification shows positive associations between increasing-income neighborhoods and outcomes of vulnerable or disadvantaged residents.

Summary and Discussion

We now summarize and discuss key takeaways from the extant evidence on neighborhood gentrification and displacement as well as the drivers and consequences of the gentrification process.

Although the term gentrification is usually associated to several concepts, many of them with negative connotation such as displacement of long-term residents and undesired neighborhood change, there is little consensus on a definition. Most scholarly studies are reaching some agreement on the fact that gentrification takes place when relatively poor neighborhoods (below metro area median income) exhibit a large increase in median income over a period (typically ten years, the frequency at which census data are collected). For instance, McKinnish, Walsh, and White (2010) set the income growth threshold at a $10,000 increase in mean income for neighborhoods in the lowest quintile of the distribution of national neighborhood income. Similarly, Abt Associates and NYU Furman Center for Real Estate and Urban Policy (2015) identify gentrifying neighborhoods in New York City as those that experience increasing income over a decade relative to other neighborhoods.

Several studies, such as Ding and Hwang (2016), consider an increase in the share of college-educated residents in addition to the increase in neighborhood income. Guerrieri, Hartley, and Hurst (2013) look at house price growth instead of income growth, but again in relation to the metro area average house price growth. To identify gentrifying neighborhoods the researcher can adopt any or these definitions and verify how consistent they are: generally, income growth, rent growth and share of residents with college should move in tandem. However, any definition that identifies gentrifying neighborhoods based on race/ethnicity or immigrant status of residents (i.e., identifying a gentrifying neighborhood as one that tips from majority black/immigrant to majority white/native) is likely to miss the whole picture. By using this flawed definition, the researcher would ignore inflows of higher-income same-race or immigrants

in initially disadvantaged areas (i.e., it would not be possible to detect when middle-income blacks gentrify an already black neighborhood).

In relation to the previous point, some scholars and commentators tend to associate the birth of gentrification episodes with physical changes in neighborhoods and inflows of culturally-diverse individuals like artists. Actually, the arts-led regeneration advocates argue that changes in land use from lower-valued industrial space to adaptable lofts and commercial businesses (e.g. 'Arts District' designations) should regenerate blighted neighborhoods. It is not clear though whether these shifts in land use and rehabilitation of run-down buildings precede gentrification. Although we have little evidence on this regard, the study by Schuetz (2014) shows that art galleries are not precursors of gentrification but rather locate in neighborhoods that are more likely to attract high-income residents and commercial activity, even in the absence of bohemian intervention. In sum, alternative definitions of gentrification (e.g. based on land use shifts) though insightful, are not necessarily more robust than conventional and simple definitions based on growth in income (or housing prices) and increasing shares of college-educated residents in initially underperforming neighborhoods.

Despite that gentrification has increased over the past decades, as central city neighborhood experienced sharp declines in crime rates and socio-economic upgrading, local authorities and the media began to pay much more attention in the past decade or so (Hwang, 2016). One potential explanation is that city centers in medium-sized cities started to gentrify between 2000 and 2010, while prior to that time, this phenomenon was mainly observed in large cities. Thus, this widespread national expansion in gentrification experiences might explain the increase in the level of awareness by public authorities and the media.

Regarding the drivers or causes of gentrification, several recent studies suggest that one key factor that has changed is the taste for amenities, specifically for some groups of gentrifiers (typically childless, educated and young individuals) and especially during the last decade. Although a change in preferences is unlikely to be the most convincing explanation for any phenomenon, the studies by Couture and Handbury (2016) and Baum-Snow and Hartley (2016) coincide in this analysis. Hence, it appears that more recent cohorts have stronger preferences to live close to restaurants, bars and businesses with personalized services.

Another driver for gentrification that has received much attention is the link between the national rise in assortative mating and the opportunity cost of higher incomes (i.e., the value of time) over the last decades. As individuals have become more educated and more prone to 'marry their likes' (at least in terms of education), rising individual incomes have led to higher opportunity costs of

time for couples. They, in turn, have responded by shifting their place of residence to central neighborhoods to minimize commuting costs for both. Edlund, Machado, and Sviatchi (2015) and Baum-Snow and Hartley (2016) show that in response to these national trends more jobs demanding high skills have been created in central neighborhoods. Further, these large inflows of college-educated residents have attracted other types of services and amenities inducing additional flows of higher-income residents, reinforcing the initial effect.

Understanding whether gentrification causes or leads to displacement is a crucial question for policy makers. Unfortunately, few studies have been successful in answering this question mainly due to the lack of appropriate data. The ideal data set should record demographic and economic characteristics (e.g. age, marital status, level of education, income, wealth, etc.) for many residents in a city in a given time, in both gentrifying and non-gentrifying neighborhoods. The data set should also collect the same information for those individuals in a later period, that is, it should follow the same individuals over time and across space. In this later period, individuals may still reside in their previous neighborhood or may have moved to other neighborhoods, regardless of whether the neighborhood gentrified or not. A data set with this structure would help us answer the following questions. Are residents in gentrifying neighborhoods more likely to move than similar residents in non-gentrifying neighborhoods? Do gentrifying neighborhoods experience larger inflows of in-migrants or do they deter potentially new in-movers compared to non-gentrifying neighborhoods? What are the characteristics of those residents who move in and move out of gentrifying neighborhoods? Are these characteristics much different from those of in-movers and out-movers from non-gentrifying neighborhoods?

These data requirements are hard to meet and, thus, few studies have done significant progress in answering these questions for the nation as a whole. One notable exception is McKinnish, Walsh, and White (2010) who study residential mobility patterns in gentrifying low-income urban neighborhoods between 1990 and 2000, using confidential us census data that collects information on 1-in-6 households in the nation. These rich data allow them to better identify who moves in, who moves out and who stays in gentrifying neighborhoods. Results show that black or Hispanic householders do not disproportionately exit gentrifying neighborhoods (relative to blacks and Hispanics in non-gentrifying neighborhoods). In fact, the authors observe that a high share of in-migrants into gentrifying tracts is accounted by black high-school graduates, particularly in predominantly black gentrifying neighborhoods. Results seem to suggest that gentrification of predominantly minority neighborhoods makes them more attractive to middle-class minority households, a finding that is not in line with anecdotal evidence shown in the media.

Other studies focus instead on a single or a set of metropolitan areas. All these articles acknowledge the need to follow residents over time and space in order to examine the (causal) relationship between gentrification and displacement. None of these studies finds evidence on displacement: low-income households were not more likely to exit their housing unit in gentrifying neighborhoods in Boston between 1974 and 1993 (Vigdor, 2002); mobility of low-income households was actually lower in gentrifying than in non-gentrifying neighborhoods in New York City between 1991 and 1999 (Freeman and Braconi, 2004); no consistent relationship between residence in a gentrifying tract and residential mobility was found in a study using the Panel Study of Income Dynamics from 1972 to 2003 (Lee, 2014).

Overall, most studies that examine the demographic and economic composition of neighborhoods before and after gentrification episodes do not find evidence of displacement of more vulnerable residents. However, most of the evidence focuses on the nineties and only a handful of studies have examined these patterns for the last decade. Findings in these more recent studies suggest that some vulnerable residents who move out of gentrifying neighborhoods experience slight declines in outcomes, relative to those long-term residents who do not move (Ding and Hwang, 2016). Still, these effects appear to be smaller than what critics of gentrification suggest.

Future data sets, and especially administrative data sets, will facilitate the tracking of individuals over time and space. The demographic and economic characteristics collected will also be much richer as already seen in many available data sets for Scandinavian countries and other Southern European countries. For instance, several of these sources provide individual continuous measures of education, income, credit scores, financial debt and homeownership status that are crucial to better identify incidences of gentrification displacement.

One aspect to keep in mind is that renters face a higher risk of relocating after a gentrification episode, however, the number of renters in specific neighborhoods is sometimes too small to allow for meaningful statistical analysis even in large administrative data sets. The fraction of renters varies substantially across developed countries: very high in Switzerland (59%) and Germany (55%) and quite low in Spain (22%) and Italy (28%) (Carliner and Marya, 2016). It also varies considerably across cities within a country: among the 20 largest metropolitan areas in the United States, Los Angeles ranks first with 52% and St. Louis comes last with 30% (Joint Center for Housing Studies, 2013). This is one data limitation that is hard to overcome when countries exhibit small rental housing markets like many in Latin America and the Caribbean. For instance, in Nicaragua, Paraguay, Peru and Venezuela the fraction of renters is below 15% and it does not exceed 35% in countries with

the largest rental housing rates like Dominican Republic and Colombia (Blanco, Cibils, and Muñoz, 2014).

It is worth noting that even richer administrative data sets will not allow us to identify displacement events with certainty unless we ask residents about their reasons to move. Many homeowners may decide to take advantage of increasing property prices in gentrifying neighborhoods, sell their homes and relocate to less expensive and lower-quality neighborhoods. It would be hard to claim these residents experience a welfare loss as a result of gentrification. Even renters may decide to out-migrate from a gentrifying neighborhood because it might no longer reflect their preferred combination of amenities and services for given sets of prices. Therefore, administrative data would need to be complemented with survey data that asks residents on moving reasons to better identify cases of eviction or housing unaffordability.

The few studies that examine the consequences of gentrification for long-term residents find positive effects on several quality-of-life indicators. Long-term residents experienced an increase in their credit scores (a proxy for financial health) after gentrification in several neighborhoods in Philadelphia, while public housing residents in New York City benefit from higher incomes, better school performance and lower violent crime in increasing-income neighborhoods. Although the link between gentrification and displacement is weak, there is some indication it may become stronger, and hence, the evidence on positive effects of gentrification shows that existing affordable housing might act as a mechanism to prevent residents from displacement and allow these individuals to benefit from resources and opportunities that can come with gentrification.

Two remarks are appropriate here. First, it might be the case that to capture displacement effects that are due to gentrification one might need to examine longer time frames. Given the benefits that come with gentrification, long-term residents might decide to remain in their neighborhoods for many years as they perceive the upgrading of services and amenities. In fact, many studies find that mobility rates are unexpectedly lower in gentrifying neighborhoods. However, as rents keep increasing some residents will eventually leave. Another way in which they may feel displaced is by realizing that their offspring will no longer be able to afford the neighborhood they grew up in.

Another type of displacement that has received less attention is exclusionary displacement. This happens when gentrifying neighborhoods become segmented in terms of retail, amenities and some services via higher prices. This is a source of concern as long-term residents are less able to enjoy the benefits that come with gentrification. Unfortunately, such type of exclusion is hard to detect in the data or even to quantify and requires deep qualitative analysis. This is an open area of research for urban sociologists.

We have not reviewed the nascent literature on gentrification and displacement in Latin America. The vast majority of these studies elaborate narratives on gentrification processes, examine qualitative case studies or use comparative analysis across selected cities. Only a handful of them provide descriptive data. Future research projects should focus on collecting longitudinal neighborhood and individual data, identifying gentrification episodes and producing some statistical analysis.

The few studies available do not find evidence of displacement associated to gentrification. Delgadillo (2016) administered a random survey to 3,000 residents in neighborhoods at risk of gentrifying in Mexico City and found that only residents in four neighborhoods report considerable changes in their neighborhoods. Yet, it is not clear from the evidence presented whether residents perceive these changes as positive or negative. Martí-Costa, Durán, and Marulanda (2016) calculate a gentrification index for census tracts in Quito based on changes in levels of education, occupational status and land use between 2001 and 2010. They find no evidence of a downtown revival or displacement of long-term residents in expanding suburban neighborhoods. They argue that in Ecuador (and perhaps in other Latin American countries) we may have witnessed the transition of middle-low income neighborhoods into middle-income neighborhoods as a result of economic expansion in the last decade. Moreover, peripheral expansions of middle-high income neighborhoods do not seem to generate displacement events.

3. Empirical methods

Assessing the Local impact of Urban Revitalization Policies

A natural starting point to assess the impact of an urban revitalization intervention is to examine the change in a given outcome (e.g. crime rate) in the area or neighborhood before and after the intervention. If the intervention generated a favorable outcome (e.g. a decrease in the crime rate) in this treated neighborhood, then the researcher might be tempted to conclude that the policy was successful. However, the main caveat of this analysis is that one cannot be confident enough that the observed change in the outcome can be attributed to the intervention or to any other varying factor (e.g. a citywide decline in crime or an increase in the number of police units).

To address this concern, the researcher would like to know what would have happened in the treated neighborhood if it had not been treated. If we were able to conduct this experiment and found that, absent the intervention, there was no change in the outcome (e.g. the crime rate remained constant), then we would be more confident that the observed decline in the crime rate was a result of the policy intervention. Of course, this experiment is impossible to administer since the same area cannot be treated and untreated at the same time. For this reason, researchers build a counterfactual or control area to measure what would have happened to the treatment area in absence of treatment. With this experiment, it is possible to compare the change in the outcome in both the treated and control areas before and after the policy and then interpret any difference between these two changes as a causal effect of the policy. This exercise is commonly labeled as difference-in-difference (DiD).

As it has become clear throughout the literature review, the vast majority of empirical studies that assess the impact of urban revitalization interventions use a difference-in-difference strategy. This methodology encounters two major challenges. First, it is necessary to clearly delimit the area of influence of the intervention or treatment, i.e., which specific neighborhoods or even blocks will benefit from the policy; otherwise the researcher cannot identify the geographic scope of the intervention and may end up obtaining a biased estimate of the causal effect of the policy.[19] Second, and quite relevant, it is necessary to build an appropriate counterfactual by finding for treated neighborhoods some neighborhoods that were not included in the policy but that are similar to the treated ones in terms of initial or pre-treatment characteristics.

Several studies that assess the impact of urban revitalization policies define control areas based on proximity. Neighborhoods that surround the area of treatment are likely to be similar to the treated neighborhoods in their socio-economic characteristics and bundles of amenities. However, if the effects of the revitalization intervention spillover to neighboring areas (as suggested in several of the studies reviewed), then by using this set of surrounding areas it is not possible to identify the magnitude of the true effect of the policy. More recently, the literature on this topic has tackled this concern by using rejected areas or later applicants as control units, i.e., neighborhoods that will be eventually treated at later stages (Kolko and Neumark, 2010, Busso, Gregory, and Kline, 2013, Mayer, Mayneris, and Py, 2015, Neumark and Simpson, 2015). In

[19] In many cases the baseline data are provided at a larger spatial scale that contains the area of intervention (e.g. a municipality may include several neighborhoods of which only a handful received treatment) or the spatial scale does not match the boundaries of the intervention. These concerns can be addressed by further narrowing the spatial level at which data are collected (e.g. block-level or household-level data like in a census).

this latter case, the identification strategy exploits the timing in the exposure to the treatment.

An additional concern in some urban revitalization interventions could be the lack of proper control areas given that, in some circumstances, the treated neighborhoods may exhibit high levels of deprivation that are unique within a metropolitan area. As an alternative and complementary approach, Kline and Moretti (2014) try to find neighborhoods with similar pre-intervention characteristics that can be used as control units but that are in other metropolitan areas throughout the country. To that end, they use propensity score matching to build control units like the treatment ones in terms of observable characteristics.

Furthermore, only a single unit may be treated in some types of revitalization initiatives (e.g. the whole downtown of a country's largest metropolitan area) and it is not possible to find proper controls in other parts of the country. In this case the researcher might consider using a synthetic control method that constructs an artificial ('synthetic') counterfactual as a weighted average of comparable large metropolitan areas in other countries (see Abadie, Diamond, and Hainmueller, 2015 for a description and Gobillon and Magnac, 2016, for calculation of confidence intervals).

After defining the treatment and control groups, the simple DiD strategy compares the beforeand-after differences between the two groups and obtains the DiD estimator. More specifically, the causal effect of the program is estimated using the following regression setting:

$$\Delta Y_{nm} = \beta T_{nm} + X'_{nm}\gamma + C'_{m}\delta + \varepsilon_{nm} \quad (1)$$

where ΔY_{nm} represents the change in one outcome variable between period t and $t+1$ in neighborhood n of metropolitan area m (e.g. the change in the crime rate in neighborhood n between two periods); T_{nm} is a treatment status indicator in neighborhood n (that is, it takes value one if the neighborhood is ever treated and zero otherwise); X'_{nm} is a vector of neighborhood controls in period t (e.g. share of residents in poverty or with college education prior to the urban revitalization intervention); C'_{m} is a vector of controls at the metropolitan area level in period t (e.g. citywide unemployment rate prior to the intervention); β, γ and δ are parameters to be estimated and ε_{nm} is an error term.[20] The main

[20] Some controls like unemployment rate or labor force participation may not be available at the neighborhood level but only at larger spatial units like metropolitan areas. When the urban intervention is implemented in many neighborhoods across the country it is important to include metropolitan area controls to capture differences across urban areas that may be correlated with the outcome of interest. One alternative is to include metropolitan area fixed effects and thus restrict the analysis to treated and control neighborhoods within a metropolitan area.

parameter of interest is β which captures the average difference between the change in the outcome in the treated and control neighborhoods after controlling for several other neighborhood and metropolitan area covariates.

The main assumption in a DiD setting is the existence of parallel trends, which implies that in the absence of any intervention, treated areas would have evolved similarly to control areas. Hence, observing similar trends before the policy implementation would provide evidence that this assumption holds. Yet, whenever this assumption does not hold, the standard DiD setup provides biased estimates of the true causal effect. To circumvent this problem, the DiD strategy can be combined with matching techniques such as propensity score matching. The main goal of this approach is to create weights that make both trends in the treated and non-treated areas quite similar prior to the implementation of the policy. In sum, the approach consists of estimating a propensity score or probability of treatment for both treated and control areas, and then run a weighted did regression using the estimated probability of treatment as weights.

The Parametric Re-Weighting Estimator

Another way to address the problem of not having parallel trends prior to the revitalization intervention is to implement the 'parametric re-weighting' estimator proposed by Kline (2011) and applied in Busso, Gregory, and Kline (2013). Similar to other matching techniques like propensity score, the aim of this approach is to estimate the counterfactual mean of the treated units (that is, what would have happened to the treated neighborhoods in absence of the treatment), by means of balancing pre-treatment characteristics across treated and control neighborhoods.

To obtain the parametric re-weighting estimator we follow two steps. First, we estimate the following equation for non-treated neighborhoods by ordinary least squares:

$$\Delta Y_{nm} = X'_{nm}\,\rho + C'_{m}\,\theta + \xi_{nm}\,. \quad (2)$$

Next, we use $\hat{\rho}$ and $\hat{\theta}$, to compute the average treatment effect (ATT) as follows:

$$ATT = \hat{\boldsymbol{\mu}} - 1/N_T \sum_n T_n(X'_{nm}\,\hat{\rho} + C'_{m}\,\hat{\gamma})\,. \quad (3)$$

In equation (2) we estimate the change in the outcome variable between period t and $t+1$ on neighborhood and metropolitan area variables using only non-treated neighborhoods. Therefore, we predict, for instance, the change in the

crime rate before and after the urban revitalization policy in neighborhoods that were not intervened. In equation (3) the ATT has two terms. The first term ($\hat{\mu}$) is the unconditional mean of the change in the outcome of interest in the treated units, $E[\Delta Y_{n,m}/T_n = 1]$, whereas the second term is the counterfactual mean of the treated units using the estimated parameters obtained in equation (2) for non-treated units. These estimated parameters are also known as the Oaxaca-Blinder weights as described below.[21]

This methodology is in the spirit of the Oaxaca-Blinder decomposition that is widely used to study mean outcome differences between groups. One of the most well-known applications looks at gender discrimination in the labor market and attempts to answer the question 'what would be the earnings of males if the labor market returns or payoffs to their characteristics were the same as for women?' To that end, researchers estimate in the first stage an ordinary least squares regression of female earnings on individual characteristics. These estimated returns for female characteristics are then used to compute the counterfactual mean of earnings for males. Analogously, for our treatment evaluation of urban revitalizations, we estimate parameters in the first stage ($\hat{\rho}$ and $\hat{\theta}$) using only non-treated neighborhoods to then construct a counterfactual mean of the outcome for treated neighborhoods.

One of the advantages of the parametric re-weighting approach is that weights have the desirable property of summing up to one. Also, they can have negative values for control neighborhoods that have a very low probability of being treated. Furthermore, all the covariates used in the first-stage estimation become perfectly balanced by construction in the second stage.[22] For instance, if the share of neighborhood residents with university education is included in the first-stage estimation, then the difference in means for this variable between treated and (re-weighted) non-treated neighborhoods is zero.

One limitation of propensity score matching is the perfectly prediction problem. This happens whenever there are few treated units in a very unbalanced sample and some covariates can perfectly explain the probability of being treated in the context of a logit model (e.g. treatment is assigned to neighborhoods with unemployment rate above 30 percent and one of the covariates is such

[21] In a more technical note, we can define $Zn \equiv [X'_{nm}, C'_m]$ and $\varphi \equiv [\rho, \theta]$. Busso, Gregory, and Kline (2013) show that: $1/N_T \sum_n T_n Z'_n \hat{\varphi} = 1/N_T \sum_n T_n Z'_n (Z_n D Z'_n)^{-1} (Z'_n D \Delta Y_{nm}) = 1/N_T \omega D \Delta Y_{nm}$ where D is a $N \times N$ diagonal matrix with $1 - T_n$ in the diagonal; N_T is the number of treated units and ω refers to the Oaxaca-Blinder weights.

[22] This property leads to a perfect match in first moments for the variables used in constructing the weights. One way to represent this property is to express the counterfactual as follows: $1/N_T \sum_n T_n Z'_n \hat{\varphi} = 1/N_T \sum_n T_n H \Delta Y_{nm}$ where $H = Z'_n (Z_n D Z_n)^{-1} Z'_n D$ and $\varphi \equiv [\rho, \theta]$. Since H is an orthogonal projection, then $HZ_j = Z_j$ with $Z_j \in Z_n$. Therefore, $1/N_T \sum_n T_n H Z_j = 1/N_T \sum_n T_n Z_j$.

indicator variable).[23] This prediction problem is not a concern for the parametric re-weighting approach given that the first stage is estimated by ordinary least squares. In fact, this approach becomes even more appealing when the number of treated neighborhoods is low, the number of potential control neighborhoods is high, and the researcher has information on multiple neighborhood characteristics. Certainly, these conditions hold in many revitalization interventions.

Like other matching techniques, one remaining challenge in this approach (and topic of ongoing research) is how to select variables for the right-hand side of the first-stage estimation. In general, only pre-intervention variables that potentially affect the likelihood of treatment should be included. Thus, a priori, the researcher should include all variables and indicators that were used by the policymakers to designate the treatment areas. This requires profound knowledge of the eligibility rules of the policy and access to the information.

Assessing the impact of urban revitalization policies on displacement

The literature review found no systematic evidence on the displacement of residents because of an increase in neighborhood income (a process referred to as gentrification). Two main issues deserve further attention. First, observing high exit rates in gentrifying neighborhoods for vulnerable demographic groups is not necessarily evidence of displacement. For this to happen, the researcher would need to statistically test whether such exit rates are significantly higher than exit rates in other similar neighborhoods that have not experienced gentrification. Second, it is not clear to the researcher whether relocations away from neighborhoods that have experienced increases in income are voluntary or are forced by more stringent economic conditions. As we have already mentioned, many homeowners may decide to leave a neighborhood that has experienced an increase in housing values to profit from higher rents while living in less expensive neighborhoods. It is thus not possible to document with certainty the reasons that led the residents to move out of a neighborhood unless we survey the movers and follow them over time into their new location. These types of data collection are expensive and require high-quality longitudinal data since it is not easy to identify movers and follow them across space.

Despite these data limitations, it is possible to roughly approximate relocation patterns in or out of neighborhoods that experience large increases in mean or

[23] Note that the weights in the Oaxaca-Blinder approach are the same as the ones in propensity score matching whenever the error term in the assignment model follows a log-logistic distribution.

median income. To that end, we present a simple but informative approach known as synthetic cohort analysis.

Synthetic cohort analysis

This empirical method attempts to approximate the extent of in-migration or out-migration of a demographic group over a period. The idea is very simple: let's consider all residents in a neighborhood with a set of demographic traits at a point in time (e.g. residents between 20–29 years old, who are single and with less than high school education in 2000). Then, ten years later, we count the number of residents in the same neighborhood that are between 30–39 years old, who are single and with less than high school education. A priori, any difference between the sizes of these two groups over both periods would provide a relative approximation of the extent of in-migration or out-migration in the neighborhood. More important, it is the relative change in the size of a cohort in a neighborhood compared to the average change for all other neighborhoods that give an indication of the incidence of in-migration or out-migration.

Suppose we plan to assess the effect of an urban revitalization policy on displacement (proxied as neighborhood out-migration) using decennial census data. Census data often collect information on socio-economic characteristics at the census tract level (a proxy for neighborhoods). Let's define four age groups using the census in period *t*: 20–29, 30–39, 40–49 and 50–59. Ten years later, each member is ten years older. We are interested in individuals who ten years later (census in period $t + 1$) did not change residence (i.e. stay at least ten years in the house). Moreover, within each age group we include two additional dimensions: marital status (non-married vs. married) and four education categories (less than primary, primary, secondary and tertiary). This example, end up with 32 cohorts, but we may consider other categorical variables (e.g. renters vs. homeowners) depending on census data availability and sample sizes for each cohort.

Now suppose the urban revitalization policy was implemented during this ten-year interval (after period *t* and prior to period $t + 1$). We further assume that the intervened areas can be perfectly overlapped into groups of tracts. Hence, the intervened zone (denoted by *z*) consisted of a group of census tracts or neighborhoods (denoted by *n*). Finally, we consider rejected and later applicants as control units for this hypothetical evaluation exercise. To analyze whether the revitalization policy produced an outflow of members of one particular group, we estimate the following regression:

$$\%\Delta Pop_{cnz} = \alpha_0 + \sum_{c=1}^{31} \alpha_c C_{cnz} + \sum_{c=1}^{32} \beta_c (C_{cnz} \times T_z) + X'_n\gamma + \varepsilon_{cnz} \quad (4)$$

where cnz denotes a cohort c in tract n and zone z; $\%\Delta Pop_{cnz}$ is the percent change in cohort population between period t and $t+1$; $C_{1nz}...C_{32nz}$ are indicator variables that capture the average population change for each cohort in the period of analysis; T_z is a treatment indicator that takes value one in treated areas; and X_n is a set of tract-level controls. In this setting, our coefficients of interest are $\beta_1...\beta_{32}$. An estimated $\beta_1 < 0$, would suggest that the first cohort (e.g. single residents aged 20–29 and with less than primary education) experienced population loss relative to the same cohort in control areas (rejected and later applicants in this example).

4. Challenges and Recommendations in the Latin American Context

Implementing impact evaluation strategies for urban revitalizations in LAC

From a methodological point of view, the most credible approach to assess the impact of an urban policy is a randomized controlled trial (RCT). A RCT implies that treated areas are randomly assigned among a pool of applicants that initially fulfill certain criteria (e.g. being a deprived neighborhood according to a set of indicators such as poverty incidence or crime rate). This assignment allows the researcher to compare outcomes in treated neighborhoods relative to outcomes in control areas (i.e., those that did not receive treatment due to randomization). Any observed difference can be causally attributed to the urban policy given that randomization helps address concerns about any unobserved factor influencing the findings. This is usually considered the benchmark approach in most experimental scenarios.

However, from a political perspective it is often unfeasible to implement RCT interventions in urban settings. Local authorities are not willing to randomize the allocation of urban infrastructure within a city given that residents are likely to complain about what they perceive as unfairness in the spatial distribution of services or amenities. Given that both groups, randomized and control neighborhoods alike will pay the same amount of taxes, it is hard to justify to the voter the unevenness in the allocation of infrastructure. In response, residents may decide to penalize the local authority through any voting mechanism. For this reason, there are very few studies using a RCT approach to

evaluate urban interventions.[24] In sum, although RCTs provide the necessary setting to estimate the causal effect of urban policies, we do not believe this is a feasible evaluation strategy for revitalization initiatives in Latin American metropolitan areas.[25]

A natural alternative to RCTs is to implement a quasi-experimental approach. As we have stressed throughout the literature review, researchers often use a difference-in-difference methodology that compares outcomes in treated and control neighborhoods before and after the implementation of the policy. The main challenge that arises in this setting is how to pick appropriate control units, that is, neighborhoods that are ex-ante similar to the treated ones.

The first recommendation to select control areas is to rely on the eligibility rule criteria followed in the treatment designation. In several cases, policy makers designate specific areas to be revitalized based on socio-economic or urban decay indicators such as unemployment, poverty, share of low-educated residents, share of buildings in poor condition, accessibility to transportation infrastructure and others. If the policy follows a clear designation criteria which depend on a set of objective indicators that are hard to manipulate by neighborhoods, then control areas can be selected among the pool of neighborhoods that are not intervened but share similar pre-treatment indicators with the treated neighborhoods.[26] It is worth noting that to avoid selection biases, the designation should not be based on discretionary decisions. This contamination in the designation process (e.g. a neighborhood with a low poverty rate gets intervened despite that its rate is below the poverty threshold required for designation) is not insurmountable, but makes the designation experiment fuzzier. Therefore, to perform a credible assessment and choose appropriate control areas we need to precisely know the eligibility criteria adopted by policy makers and whether there were any discretionary decisions.

Nevertheless, it is possible that the worst performing areas end up being the treated ones. Then, even if we know in detail the eligibility rule, we could easily run out of control neighborhoods that exhibit similar pre-treatment characteristics as treated neighborhoods (e.g. if only extremely dangerous neighborhoods in the city are intervened we cannot observe neighborhoods with similar levels of crime that are not treated). To that end, some studies have

[24] One exception is González-Navarro and Quintana-Domeque (2016) who convinced the authorities of the municipality of Acayucan, Mexico, to randomly pave streets in some deprived neighborhoods.
[25] The randomization developed in Acayucan, Mexico, by González-Navarro and Quintana-Domeque (2016) is an exception rather than a rule. Moreover, Acayucan is a small city with less than 100,000 inhabitants in 2010. Most urban revitalization interventions happen in large metropolitan areas in which randomization is close to unfeasible.
[26] As long as the eligibility criteria are based on a combination of indicators, local neighborhoods or municipalities will find it hard to manipulate numbers to benefit from the urban intervention (e.g. it will be quite difficult to change both the share of low-educated residents and buildings in poor condition in the short run). Moreover, the eligibility criteria might rely on past data, which makes even more unlikely the chances for neighborhoods to modify past indicators.

exploited the timing of the program or any financial constraint in the implementation to choose control areas. While the latter is not a desirable feature of any urban policy implementation, the former is an impact evaluation strategy that helps to clearly establish similar treatment and control groups.

Thus, the second recommendation to select control areas is to persuade local authorities to implement the policy in stages. In most metropolitan areas, we can find more than one deprived neighborhood that could be subject to treatment intervention. The key idea is that instead of treating all the deprived neighborhoods at the same time, local authorities could implement the policy in several stages or at least in two stages. As a result, some neighborhoods among the pool of the most deprived get treated, while the others receive treatment but in a later stage, with the order of intervention being determined randomly. This strategy provides a clean impact evaluation setting whereby outcomes in now similar treated and control areas can be compared prior to and after the urban intervention.

Unfortunately, most of the urban interventions in Latin American cities lack clear eligibility rules that are based on objective indicators. Moreover, it is not always possible to persuade policy makers to implement revitalization interventions in stages since there is often local pressure of residents to increase expenditures. When neither of these two options is available, we can think of a third recommendation to select control areas based on statistical analysis. More specifically, control groups can be chosen by pooling together many neighborhood observable characteristics from the initial period (e.g. poverty rate before the intervention) as well as the outcome variable prior to the intervention. In line with the propensity score approach introduced above, the characteristics that will matter for the assignment to treatment are those that will be statistically significant in the analysis. These relevant variables will also determine the set of control groups. The main two caveats of this approach are that selection of control units is based on an ad-hoc set of socio-economic indicators chosen by the researcher and that we will likely fail to account for unobservable variables that may affect the eligibility criteria and that may be correlated with the outcome of interest. In consequence, this alternative way of defining control groups is less solid, however, it provides a solution when the two initial recommendations are not available.

Finally, a fourth method to select control areas is to rely on surrounding areas of treated neighborhoods. The main advantage of this method is that nearby areas are likely to share the same socio-economic characteristics (and perhaps also trends in variables prior to the intervention) as treated neighborhoods. Moreover, one could think that both areas are subject to the same economic shocks over time, which further increases the degree of similarity. At the same time, the main concern is that spillover effects from treated neighborhoods to

nearby non-treated areas may contaminate the analysis and bias results. To avoid this problem, several studies construct ring belts around the treated neighborhood and exclude from the analysis the ring that is closest to the treated region. This strategy to select control groups is not very demanding in terms of data (provided the researcher has access to the spatial implementation of the policy) and can be carried out when the urban intervention was not subjected to any of the conditions described above.

It is important to note that all these strategies to determine control areas are not mutually exclusive among them, and that in fact, it is expected from the researcher to obtain difference-in-difference estimations using several of these counterfactual definitions. The ranking in recommendations (from the eligibility rule, followed by the implementation in stages, the statistical analysis, and the vicinity area) is intended to provide a degree on the credibility of the findings given that the former strategies are more likely to generate causal and less-biased effects of the urban intervention.

Some urban revitalization interventions in Latin America are related to a particular neighborhood or comprise the revitalization of one big city attraction. For example, a policy could designate an area as a historic district restricting and supervising new development or the intervention may revitalize one specific site like 'El Mercado de Goes' in Montevideo, Uruguay. These interventions restrict the options to select control neighborhoods. For instance, we cannot exploit different stages of timing in the intervention since only one area is affected at one point in time. Likewise, the eligibility criteria do not apply here as only one neighborhood is intervened. We are thus left with the options of creating control groups using statistical analysis as suggested above—mainly the synthetic control method followed by either the parametric re-weighting estimator or propensity score matching. Another option is relying on boundaries or vicinities to estimate the effects of the urban intervention.

Selecting Outcomes for Evaluation

One critical issue in any impact evaluation strategy is to determine the outcome of interest. While policy makers may be confident that the urban revitalization intervention may affect a range of outcomes, this of course will depend on the design and breadth of the policy. The role of the researcher or the administration/team that leads the impact evaluation is to determine which outcomes are feasible to construct based on data availability, and more important, which are relevant to consider in most urban revitalization interventions. We present here a classification of outcomes into three broad

groups. We adapt this classification to the Latin American context and briefly mention whether the availability of data allows constructing such outcomes.

Group 1: Standard and Feasible Outcomes

First, we consider a set of outcomes that are driven by the theory of 'pure' place-based interventions as discussed in section 2. These outcomes are not only feasible to gather in terms of data availability, but more important, they are extensively used in most of the empirical studies that were discussed in our review of the literature.

- Property or land values: This is the main outcome of interest considered in most studies. From a theoretical point of view, urban revitalization interventions generate an exogenous change in the level of neighborhood amenities that affects the individuals' utility level. This increase in welfare gets capitalized in the value of land and thus affects location decisions of agents. It should not be surprising that all the studies reviewed in this report use land or property prices as their main outcomes of interest.

  Although the value of land is theoretically the main outcome of interest, we rarely observe many land transactions in neighborhoods that are already developed, and those land plots that are traded may not be representative of typical land plots. One alternative is to examine property prices. Property transactions are much more common and sample selection is (relatively) less of a concern. Moreover, the recent availability of micro-data on housing transactions is a big step forward as the researcher can better control for differences in observable characteristics across properties.

  Regarding data availability in Latin American countries, it is important to remark that it is generally difficult to access data on land and property values. Online data sources from websites that sell and rent houses could be a potential source of information. However, these sources contain posted prices that are usually measured with error and lead to bias in the estimation. The most reliable sources on housing prices are tax agencies. Most Latin American countries have specific sales taxes that apply whenever a house is traded. The usual tax-base for this sales tax is the transaction property value and, hence, it represents a highly reliable source of information, already available in Argentina, Brazil, Uruguay and Chile. The main concern regarding these data is confidentiality since there are laws that preserve the anonymity of individuals involved in the transaction and that complicate the release of these data to interested researchers.

- Crime levels and rates: Many revitalization initiatives target run-down buildings or vacant properties, places that could likely be used for criminal activity (e.g. drug dealing or prostitution). Once these buildings are restored

one expects crime to be displaced to nearby neighborhoods reducing local crime rates. Moreover, the revitalization process itself requires the hiring of workers, creating job opportunities in the construction sector and 'placing more eyes on the street,' all of which increase the opportunity cost of participating in criminal activities. An additional aspect to consider is whether the revitalization intervention affects street lighting, which has been found to be a deterrent of criminal activity.

Local crime rates are gradually becoming more available in Latin American countries due to GIS technology and better coordination among police stations. Researchers are increasingly using geocode locations for crime incidences with data on the precise time and type of crime or misdemeanor. This opens the possibility to consider crime rates as a relevant outcome affected by urban revitalization interventions, especially given the high levels of crime in Latin American cities.

- Tax collection: Although not a frequent outcome in most impact evaluation studies of urban revitalization, this becomes an outcome of interest for Latin American cities given their high levels of informality. Several revitalization programs restore old markets (e.g. 'Mercado de Goes' in Montevideo), put abandoned facilities (e.g. Puerto Madero in Buenos Aires) into use, and incentivize new businesses. The opening of businesses and potential growth in existing ones could boost local economies and increase formality. This increase in activity should get reflected in tax collection that is usually available through tax agencies in the region, though it is unlikely to be provided at the establishment level. Even though, figures that are aggregated at the level of the neighborhood or municipality are also valuable.

- Building sales and renovation permits: As a result of the increase in property prices, developers should become interested in building in these targeted neighborhoods. We expect an increase in the number of building sales (especially run-down buildings) and construction or renovation permits issued by municipalities. Some Latin American countries, for example Uruguay, have information on building and renovation permits which are available online and geocoded for each specific property. Again, figures that are aggregated at the level of the neighborhood or municipality are still valuable.

- Share of college-educated residents: This outcome might be used as an imperfect proxy of housing prices, though it is of interest by itself. If property prices increase in targeted neighborhoods, then it is relatively easier for highly educated individuals to find affordable housing opportunities as they are less likely to be financially constrained. Furthermore, highly educated individuals show on average higher willingness to pay for amenities, and

therefore, any urban revitalization intervention that increases the level of local amenities should especially attract this population group.
The number or share of residents with college is provided through censuses at a very fine level of geography (e.g. neighborhood), yet they tend to be spaced in time (e.g. usually every ten years). The researcher should be able to use a census wave that precedes the intervention as a baseline and the later census wave to estimate the effects of the urban intervention. Of course, in any case either the baseline will be less informative, or the estimated effect will only capture a long-term outcome. Several countries are increasingly relying on continuous population censuses (e.g. 'Padrón Continuo' in Spain) that are updated annually as residents move across municipalities. Hopefully, these richer data could become available soon in Latin American countries.

- Transit ridership: Revitalization interventions should make neighborhoods more attractive, particularly those with cultural heritage like downtowns or bohemian neighborhoods. It is possible to approximate this increased interest by examining daily, weekly or monthly data on transit ridership. These data are usually available for mass transit options such as metros, bus rapid transits and ordinary buses. In many instances the data are provided at fine levels of geography, so it is possible to obtain the number of passengers in specific transit stations and frequencies within a day. This information should be valuable to infer whether transit riders are commuters or visitors and in some cases some demographic information on them (e.g. students, retirees or large households). Unfortunately, only a subset of Latin American cities has developed efficient transportation systems that collect this information via electronic passes purchased in advance. In many cities, a large share of passengers relies on informal systems of buses or vans, use taxis or more recent technology (e.g. Uber, Easy Taxi) to escape congestion or protect from high levels of crime.

Group 2: Relevant but less feasible outcomes

Second, we consider several outcomes that although potentially relevant are hard to measure due to the lack of data. For some of these outcomes, even when data become available, it is quite hard to conceptualize a proper measure. These outcomes are rarely tested in empirical studies, yet, they should be of interest to policy makers.

- Sales time: Economists measure welfare gains of local policies through changes in earnings and land prices. Yet land and housing prices may not respond immediately because it takes time to sell a house. Therefore, some researchers propose to examine differences in sales time of properties in

treated and control neighborhoods as a short-run outcome to proxy success of the revitalization intervention. Unfortunately, information on sales time of properties is hard to obtain and rarely available. To measure this outcome, the researcher would need to design an expensive survey. We think this outcome is quite informative but hard to collect.

- Business openings and quality of existing establishments: Urban revitalizations may induce new businesses to locate in intervened neighborhoods or existing businesses to upgrade their quality. Although these outcomes are highly desirable, publicly available sources in Latin American countries do not include this type of information. Business registers demand a lot of data collection, are very expensive and are only available for a subset of neighborhoods in Latin American cities. The researcher should keep in mind that business turnover is high, especially in central neighborhoods, so recording precise opening of business is an intricate task. While more businesses publish their information online (e.g, Google Maps, Four Square or Yelp), these businesses might be a selected sample, especially since not all consumers in Latin American cities can access this information.

  Furthermore, measuring business quality is an abstract concept that requires many assumptions. Recent studies are making notable progress by relying on Google Maps and machine learning techniques to track changes in the facades of local stores (e.g. coffee shops replacing pawn shops or old diners). Although this is a promising avenue of research, we are still at an early stage to rely on these sources of data to construct manageable outcomes. Hence, we consider this to be a hard outcome to envision though it will certainly become more accessible in future years.

- Stigma or neighborhood belonging: A complex outcome that could also be considered from a broad perspective. Some individuals may experience a decline in social stigma after a revitalization intervention as they may feel there is no longer a problem to self-identify as residents of the treated neighborhood. This would be a highly valuable outcome of the intervention, yet, it is perhaps the most complicated one to conceptualize. How do we construct a measure of social stigma? Sociologists have studied this concept and reinforced their analyses with qualitative methods, but if we try to assess quantifiable measures of the effect of an urban revitalization intervention these numbers or indices are quite hard to interpret. Moreover, several studies have documented a pro-project bias when residents are asked about the benefits of the revitalization intervention. Therefore, we believe this outcome is highly valuable but quite unfeasible to evaluate an urban revitalization intervention.

- Indices: Combining several outcomes into one metric is feasible; however, we discourage their use. Indices are discrete or continuous measures typically ordered from less to more desirable values. If the result of a DiD estimation shows that the proposed index increased from 2 to 3 as a result of revitalization, it is not immediately clear to the researcher how to interpret this result. It might be the case that most of the increase got reflected in higher property prices, then the natural question would be, why not consider housing prices as a separate outcome? It is very informative to know that the intervention had large effects in one or two outcomes and negligible or insignificant effects in others. This analysis is not possible when these outcomes are pooled together into a single index. One additional advantage of relying on numerical outcomes, in particular those expressed in monetary units, is that they provide an effect that helps approximate measures of welfare. This is a simple way to convey any effects of urban revitalization policies.

Group 3: Potential outcomes with testable effects

Finally, we consider some outcomes that are not directly targeted by the revitalization intervention, yet they may end up being affected through an indirect mechanism. To be clear, these outcomes are not an explicit goal of revitalization policies but, given the quasi-experimental evaluation setting in mind, any observed change could be attributed to the intervention.

- (Formal) job creation and unemployment: As opposed to place-based policies that have a clear goal of increasing the number of jobs in a locality, urban revitalizations focus on improving living conditions among residents and attracting new residents and commercial activity. Businesses may open or move to the intervened neighborhood, however, there is no explicit requirement that they need to hire residents to operate. We might expect an increase in the number of jobs created in the neighborhood, especially formal jobs, but it is hard to anticipate any effect on local unemployment rate as residents' labor supply might not be affected by the policy.

  The effect on the number of jobs and local unemployment rates of revitalization policies is an empirical question that can be tested if data is available at small geographic scales. Municipal unemployment rates and counts of jobs can be obtained in Latin American censuses (with the caveat that they are spaced in time, usually every ten years, as already noted) or broadly approximated via labor force surveys that collect information on tens of thousands of individuals.

- Income and financial health: In line with the previous argument, labor income for long-term residents in intervened neighborhoods may increase,

but through indirect mechanisms (e.g. via lower expenditures on crime prevention or higher likelihood of finding a nearby job) and not as a direct effect of revitalization. Studies in developed countries show that public housing residents in increasing-income neighborhoods, a population that is less likely to sort into specific neighborhoods, experience also a rise in income. Of course, income will increase when measured at the aggregate level since new residents are more likely to be richer as they are attracted by the upgrading of the amenities that comes with revitalization. In sum, researchers may expect a slight to moderate increase in incomes for long-term residents, yet the mechanisms are not straightforward to identify.

Data on individual income are not generally available in Latin American countries at a disaggregated geographic scale. For those countries that have rich administrative data on income reported by tax agencies such as Brazil, the data cover mainly employees in the formal sector. Income data in labor force surveys are helpful but depend on the size of the survey, as they tend to be representative only at the national or provincial level. One promising data source for many Latin American countries are entities that collect data on individual credit risks. Most individuals, whether in the formal or informal sector, are now able to obtain credit through large retailers that have their own financial branches (e.g. this practice has become widespread in several countries like Argentina, Chile and Peru). As long as these institutions that monitor risk collect data on individuals' place of residence, they may become a valuable source of information to track them over time and across space and to proxy their financial health.

- Commuting time: This outcome is intrinsically related to job availability, and given the predictions stated above, it should not be directly affected by the revitalization policy. Only if residents can get jobs in the area because of the influx of businesses or expansion of existing establishments, then we might expect a drop in commuting times. Some interventions invest directly in transportation infrastructure, so in this context, we might expect an effect on commuting. One example is the revitalization intervention in the municipality of Campo Grande, Brazil, where part of the intervention improved transportation infrastructure and connectivity.

  Unfortunately, average commuting times are rarely available at the individual level in Latin American cities, unless we rely on time surveys that interview few individuals at a high cost. Small sample sizes for the intervened and control neighborhoods do not allow for statistical power in DiD analysis.

- Informality vendors: In line with the predictions on (formal) job creation and local unemployment rates, if the revitalization intervention does not consider an explicit mechanism to decrease informality, then informal vendors have no incentives at all to become formal. Again, formalization of residents is not an explicit goal of the policy neither formalization of existing street vendors.

The only reason why we might expect a decrease in the number of informal vendors or activities in treated neighborhoods is if police monitoring bans these activities. Data on informal activities can be collected through labor force surveys in many Latin American countries, though not at a fine geographic level.

5. Conclusion and Discussion

Urban revitalization initiatives aim to enhance the functionality of areas from the perspectives of public space and mobility as well as to attract dynamic commercial activity and resident populations. However, it is not clear whether most revitalization interventions fulfill these expectations. Revitalization initiatives are rarely subjected to a credible evaluation strategy, and so, it is hard to assess the extent of the benefits that they may generate. In addition, existing evaluations tend to focus on the positive impacts in socio-economic and urban livability terms, but ignore any potential drawbacks that may come with the intervention.

We have approached the analysis of urban revitalization initiatives with these two concerns in mind. First, we have narrowed our description of the empirical literature to those studies that assess the impact of revitalization interventions by using a treatment and control group design. Otherwise, it is very complicated to gauge the causal impact of an urban policy when a control group is not present. These studies also rely on some source of quasi-experimental variation in the design or implementation of the program, and mainly examine changes in property and land prices before and after the policy. In general, there is consensus that urban revitalization programs capitalize in higher land and property prices and that housing spillovers exist, though they decay at a rapid scale as we move away from the area of intervention. Other studies that look at complementary outcomes find a decrease in crime rates and a shift in neighborhood composition towards residents with higher levels of education.

Second, we have reviewed the literature that relates gentrification with displacement of long-term residents, an unfavorable outcome of any place-based policy that increases the attractiveness of a neighborhood. It is hard to establish such relationship not only because of the lack of appropriate data to measure any causal effect of gentrification, but also because it is not clear how to define both events. The extant evidence has established some consensus around stylized facts: gentrification (measured as neighborhoods that experienced a large increase in mean incomes, rents and shares of college-educated residents over a decade) is a phenomenon that increased over the

nineties and became more visible in the last decade; studies do not find evidence of displacement (proxied as higher rates of out-migration of vulnerable groups) in gentrifying neighborhoods in the nineties and only mild to null evidence of displacement in the last decade; long-term residents in neighborhoods that gentrify appear to benefit from greater financial health, lower violent crime rates, better test scores in local schools and higher incomes; and, existing affordable housing in gentrifying neighborhoods may help residents perceive the benefits of gentrification without experiencing risk of displacement.

Third, by considering the data sources and empirical strategies followed in the studies reviewed, we have summarized two separate methodologies that estimate the impact of a revitalization intervention and that proxy the amount of people or businesses' relocation. Both methodologies take us closer to identifying causal effects of urban interventions on local and individual outcomes and present substantial advantages compared to naive methods such as pre-and post-intervention comparisons that tend to be plagued by omitted factor biases or sample selection concerns. In absence of randomization, as long as researchers get access to the designation criteria for treatment areas, properly identify these treated areas and carefully select relevant control groups following one of the alternatives discussed, they can be confident that the results obtained using difference-in-difference estimators are causal. However, the synthetic cohort analysis presented to approximate the extent of displacement as a result of a revitalization intervention provides only suggestive results that should be taken as informative but not necessarily as causal. In this method, the researcher should acknowledge that it is harder to tease other confounding effects, mainly due to the lack of longitudinal data and information on drivers of mobility.

Fourth, we provide suggestions for the implementation of urban revitalization interventions in Latin American countries. Specifically, we are concerned about the choice of control groups by the researcher and recommend relying on the eligibility rule criteria that ranks neighborhoods based on observable characteristics and that allows the researcher to pick comparable neighborhoods that are not intervened. An alternative is to persuade local authorities to implement the revitalization intervention in stages and exploit the timing of the policy to compare outcomes in treated neighborhoods relative to outcomes in those neighborhoods that will be treated in future stages of the implementation. We understand that these options to select control units are not always feasible and, under that scenario, we recommend to rely on statistical methods described in the report or to use surrounding areas of intervened areas as control units.

Finally, as a last contribution, we provide a categorization of individual and local outcomes, based on their relationship to theory and the availability of data.

Some outcomes are relevant and feasible to construct (e.g. property and land prices, neighborhood crime rates, number of building sales and permits or share of college-educated residents in the neighborhood), while others are also relevant but harder to collect in the Latin American context (e.g. number of business openings or sales time for properties) or in any context (e.g. extent of neighborhood belonging). Other outcomes are not directly targeted by the revitalization intervention; however, they may end up being affected through an indirect mechanism (e.g. creation of formal jobs, local unemployment rates or commuting times of residents). We believe these outcomes should inform researchers and administrators involved in implementing and evaluating urban revitalization projects in Latin America and the Caribbean.

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