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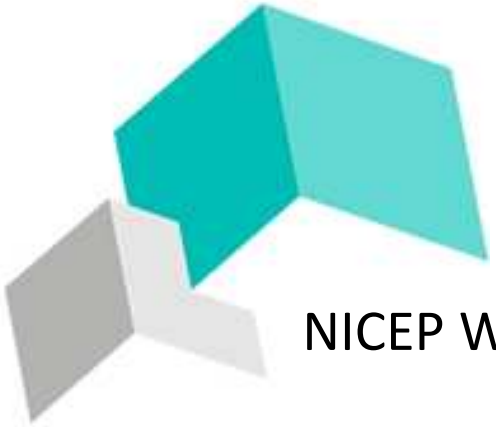

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Welcoming the tired and poor: Grassroots associations and immigrant assimilation during the age of mass migration

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WELCOMING THE TIRED AND POOR: GRASSROOTS ASSOCIATIONS AND IMMIGRANT ASSIMILATION DURING THE AGE OF MASS MIGRATION *

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Abstract

I examine the impact of the Progressive-era Settlement movement on immigrant assimilation in the United States between 1880 and 1940. Settlements provided services such as job training and childcare to immigrants. Using an individual-level triple difference strategy based on cross-cohort and over-time variation in settlement exposure, I find that settlements increased labor force participation and income for men but not for women. These responses persisted into the generation exposed to settlements during childhood. The gendered effects stem from increased fertility and in-group marriage that excluded women from labor markets, particularly among immigrants from countries with more conservative gender norms.

Keywords: Age of Mass Migration, Assimilation, Immigration, Social Movements.

JEL Classification: J15, N31, N91, O15.

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I INTRODUCTION

Social capital is a key component of cohesive, well-functioning democracies (Putnam, 2000). Low levels of social capital have been associated with the recent worldwide surge of populist movements and nativist reactions that emerged partly in response to rising immigration (Giuliano and Wacziarg, 2020; Guriev and Papaioannou, 2022). This paper examines whether, conversely, social capital can facilitate the economic and cultural integration of out-group members. Local communities frequently organize grassroots initiatives to assist immigrants.[1] Understanding how civil society engagement affects immigrants is thus central to assessing how social capital shapes the societal responses to the economic and political challenges associated with immigration.

I study the impact of natives' bottom-up social movements on immigrants' economic and cultural assimilation, drawing on the Settlement movement, a pivotal social capital innovation in US history (Putnam, 2000).[2] Settlement house volunteers provided critical support to immigrants during the Age of Mass Migration (1850–1920). I employ a triple difference estimator that leverages the location and timing of settlements, as well as across-cohort variation in exposure, to quantify the causal effects of settlements. I find that settlement houses improved immigrants' economic conditions, but exclusively for men. Increased economic participation fostered language integration; however, segregation along ethnic lines increased. The diverging labor-market trajectories between genders are plausibly explained by increased fertility in response to settlement activity. Throughout this period, exclusionary gender roles virtually mandated women's dropout from the labor force upon having children (Goldin, 1980). In line with this interpretation, I find that the settlements' exclusionary effects on women are driven by immigrants from countries with more conservative gender norms.

Settlement houses were key institutions that supported the wave of immigrants entering the United States, who primarily settled in urban areas, where living conditions were often precarious. The activities and services offered by settlement houses to urban immigrants were diverse (Berry, 1986). These included childcare assistance programs, such as free kindergartens and nurseries, and educational activities for older children. Settlement houses offered technical and professional education to adults. Additionally, Progressive reformers, typically natives from middle- and upper-class backgrounds, explicitly aimed to foster the cultural assimilation of immigrants into American society through language and citizenship classes.

[1] Mapping the scale of third-sector associations working to support immigrants is difficult because they are typically small and local. Mayblin and James (2019) estimate that, in the UK, approximately 150 associations supported refugees and asylum seekers as of 2017.

[2] Social capital is not necessarily linked to "better" political and economic outcomes. Satyanath, Voigtländer and Voth (2017), for example, document that the high density of associations in Weimar Germany was conducive to the rise of Nazism.

I use comprehensive, newly collected data on settlement houses to study how exposure to settlements shaped immigrants' economic and cultural outcomes. My analysis leverages *within-city*—enumeration district-level—variation in exposure to settlement houses in eleven major US cities.[3] I present two main sets of results. First, I explore what factors determined the rise of the settlement movement within cities. Second, I present causal evidence on the impact of settlements on immigrants. I conclude by investigating the potential underlying mechanisms.

To study the determinants of the rise of the Settlement movement, I construct consistent sub-city units that I follow over time between 1880 and 1940. Using the address of each settlement, I geo-code them to precise coordinates, allowing me to locate them within each city. I estimate a difference-in-differences model comparing areas with and without a settlement house over time.

The presence of immigrants is the most relevant predictor of the emergence of settlements. Settlement houses were more likely to be established in areas that, in 1880, had a higher share of immigrants. More specifically, settlements emerged in neighborhoods with more Southern European immigrants, who faced considerably more intense nativist backlash than earlier immigrants from Northwest Europe (Higham, 2002). Over time, districts with at least one settlement house received higher inflows of immigrants—particularly from Southern European countries—and the immigrant share, consequently, increased by approximately 10%. Possibly because of labor market competition, labor force participation among immigrants decreased by 4%, immigrants became more likely to work in blue-collar manufacturing occupations (by 8%) and less likely to take up white-collar jobs (by 20%). Consequently, the average labor income among migrants increased by 10%.

The results are consistent with existing qualitative historical evidence (Bremner, 1956). In industrializing cities, people experiencing poverty lived in precarious conditions, where pollution, congestion, and lack of sewage and clean water contributed to higher mortality rates. My evidence indicates that settlement houses emerged primarily in response to poverty among newly arrived immigrants.

In the second part of the paper, I leverage the granularity of the census data to perform an individual-level analysis to document the causal effects of settlements on immigrants. Using the intergenerational links provided by the Census Linking (Abramitzky, Boustan, Eriksson, Rashid and Pérez, 2022a,b) and the Census Tree (Price, Buckles, Van Leeuwen and Riley, 2021; Buckles, Haws, Price and Wilbert, 2023) Projects, I construct an exposure to settlement houses measure for immigrants observed in population censuses between 1880 and 1940 in terms of their residence in 1900, when the settlement

[3]The settlement house data cover the universe of settlements. Historical neighborhood-level `GIS` files are available for a subset of the largest cities, limiting the range of the analysis. However, the eleven cities in the sample hosted more than 50% of settlements in the entire US.

movement started to gain traction.[4]

To identify the effects of settlement houses, I leverage three sources of variation in a triple difference setting: across neighborhoods, over time, and across cohorts. Settlement houses primarily targeted relatively young individuals through professional training and childcare support. The triple difference estimator thus compares young and old individuals living in neighborhoods with and without a settlement house before and after the settlement was established. Formally, I estimate a stacked cross-sectional triple differences regression. By comparing individuals in the same neighborhood over time, the model provides a consistent estimate of the causal treatment effect even if settlement houses were not randomly allocated across neighborhoods. The underlying identification assumption requires that the difference in outcomes between young and old individuals in treated and control neighborhoods would not have diverged if settlement houses had not been established.

I perform two key exercises to gauge the empirical plausibility of this assumption. First, I compare young and old individuals across census waves before and after the settlement is established. This exercise supports the identification assumption, as I do not find evidence of statistically significant pre-settlement differences between young and old individuals. Second, I let the treatment effect of settlement vary across cohorts. The estimates indicate that relatively old individuals do not exhibit statistically significant responses to settlements. Hence, the estimated average effects are entirely driven by relatively younger individuals. This pattern suggests that my estimates likely reflect the effect of settlement activity because any correlated confounding factor would be conflated in the estimated treatment effects only if it differentially affected young as opposed to old individuals in treated neighborhoods.

I find that immigrants exposed to settlements at a relatively younger age display a 0.6% higher rate of labor force participation and a 2% increase in labor income. Additionally, they are 1% more likely to be employed in white-collar occupations. The average treatment effect, however, conceals substantial heterogeneity across genders. The positive effects of settlement houses, in fact, are entirely driven by men. Labor force participation among men increases by 1%, labor income grows by 4.2%, and the probability of white-collar employment raises by 1.5%. Conversely, women exposed to settlement houses exhibit no labor market response to exposure to settlement houses. The divergence between men and women is confirmed when I estimate the treatment effect over time and across cohorts. Young and old individuals did not display differences in labor market outcomes before settlement

[4]To address the concern that the Census Linking and Census Tree project may not be representative of the population, I construct an alternative sample using the nationally representative links developed by Althoff, Gray and Reichardt (2025), which nonetheless feature a considerably lower matching rate. I find that my baseline sample and the alternative one are broadly comparable along all outcomes I consider in my analysis.

houses were established, and the treatment effects are driven by relatively young individuals who were the primary target of settlement activity.

Cultural assimilation was an explicit purpose of many Progressive-era reformers.[5] My evidence indicates that immigrants exposed to settlement houses—regardless of their gender—were 1% more likely to speak English and 0.8% more likely to display both written and spoken command of the language. However, they were also 4% more likely to marry migrants from their same country and 3.6% less likely to marry natives. In both cases, the effect is larger for men than for women, but the gender differences are quantitatively minor. I find no significant effect of settlement houses on the probability of marrying immigrants from other countries. In addition, I follow the methodology to measure cultural assimilation proposed by Abramitzky, Boustan and Eriksson (2020) and find that immigrants, and, particularly, men, exposed to settlement houses give 0.4% more foreign-sounding names to their children. My findings thus provide mixed evidence on the effects of settlement houses on immigrant assimilation and plausibly indicate that increased labor market participation fostered the acquisition of language competence, but settlement houses ushered in increased immigrant segregation along ethnic lines.

What factors explain the divergent labor market effects of settlement houses across genders? Throughout this period, women with children faced considerable stigma against working. Goldin (1980, 1990) argues that women commonly dropped out of the labor force upon marrying. I thus explore whether settlement houses modified family and fertility decisions. My results indicate that immigrants exposed to settlements were more likely to have children (by 1.4%), had 3.4% more children, and were three months younger when they had their first child. In all cases, the effects are driven by women, while men display smaller fertility responses to settlement activity.

These findings suggest that conservative gender norms and the positive labor-market effects of settlement houses for men led to the exclusion of women from the labor market. To provide more evidence in this direction, I investigate the heterogeneous treatment effects of settlements in terms of the degree of conservatism of gender norms in the immigrants' countries of origin. I find that there is a robustly negative association between the effect of settlements on female labor force participation and conservative gender norms, measured either through the Male Dominance Index of Guarnieri and Tur-Prats (2023) or total fertility in 1900 (Coale and Treadway, 1986). Conversely, the association between the treatment effect of settlements on the number of children and traditional gender norms is robustly positive. These patterns indicate that gender norms shaped the response of immigrants to settlement

[5] I do not take a stance on whether immigrant assimilation is desirable. In fact, pluralism and diversity are likely conducive to countries' prosperity (e.g., Alesina and La Ferrara, 2005).

houses and, in particular, their implications for women's empowerment.[6]

Finally, I investigate the intergenerational effects on individuals exposed to settlement houses during childhood. Consistent with the previous evidence, childhood exposure to settlement activity increases labor force participation by 0.3% and labor income by 1.5% for men, but reduces them for women. The probability of having children increases (by 1.1%), the number of children increases by 2.2%, and the treatment effects on women are larger than on men. In terms of assimilation, I find that early exposure to settlements increases the probability of marrying immigrants from other countries by 1.1% and decreases marriages with natives by 0.8%, suggesting that settlement houses fostered inter-group contact among immigrants from different countries. These results echo a large literature on the intergenerational transmission of values and norms and indicate that the beneficial effects of settlements on men were transmitted to the younger generation but did not spill over to women (Bisin and Verdier, 2001; Fernández, Fogli and Olivetti, 2004).

This paper studies the economic and cultural effects of grassroots social movements on immigrants. On the one hand, my findings indicate that community-driven associations can provide beneficial support to immigrants. On the other hand, they highlight that a marginalized group's own cultural values and norms—in this case, gender roles—shape how its members react to such initiatives, thus influencing their ultimate effectiveness.

Contributions to the Literature This paper contributes to three strands of literature. First, I add to the literature studying immigrant assimilation (among others, see Borjas, 1985; Lubotsky, 2007). Recent studies document that, unlike previously hypothesized, upward economic mobility and cultural assimilation during the Age of Mass Migration were remarkably low (Abramitzky, Boustan and Eriksson, 2014; Abramitzky *et al.*, 2020). Evidence on the mechanisms that enabled—or hindered—assimilation, however, is more limited and mainly concentrates on top-down institutional and technological factors, such as schooling (Bandiera, Mohnen, Rasul and Viarengo, 2019), marriage (Adda, Pinotti and Tura, 2020), and language (Fouka, 2020) laws, and religious institutions (Gagliarducci and Tabellini, 2022; Abramitzky, Boustan and Giuntella, 2025). Jaschke, Sardoschau and Tabellini (2022) study how local anti-immigrant attitudes influence immigrants' economic and cultural assimilation. I inform this literature by providing the first exploration of a bottom-up social movement that aimed at providing material and moral assistance to urban immigrants as a potential driver of assimilation

[6] I cannot rule out that the effects I estimate reflect women's preferences. It is possible that settlements *allowed* women to reduce their labor supply. Since I do not have a hard measure of within-household women's agency and preferences, I do not directly disentangle this possibility.

into American society.[7]

Second, I contribute to the literature on the cultural constraints on women's labor market—and, more generally, economic—participation (Bertrand, 2011; Jayachandran, 2015, 2021). Existing studies indicate that within-household gender norms influence women's economic outcomes (among others, see Ashraf, Field and Lee, 2014; Bertrand, Kamenica and Pan, 2015; Bursztyn, González and Yanagizawa-Drott, 2020; Folke and Rickne, 2020; Tur-Prats, 2021). My results, in particular, echo recent experimental evidence by Abou Daher, Field, Swanson and Vyborny (2023), who document that conservative social norms hinder women's economic empowerment. This paper informs this literature by exploring how improved economic conditions for the husband impact the wife's economic participation and segregation, depending on the conservatism of within-household gender norms. In a context where married women faced considerable stigma against participation in the labor market, I find that a better economic standing of the husband may hamper economic participation for the wife. Moreover, the effect is larger for households with more male-dominated gender norms.

Finally, my results add to a growing literature on the economic history of the Progressive era. Progressive reformers advocated—in many cases, successfully—for multiple pieces of legislation, including child labor laws (Moehling, 1999; Manacorda, 2006; Feigenbaum and Russo, 2020), minimum wages (Fishback and Seltzer, 2021), charity nurseries (Ager and Malein, 2024), and public schooling (Margo and Finegan, 1996). Reformers actively engaged in welfare programs, such as the kindergarten movement (Ager and Cinnirella, 2020). To the best of my knowledge, this is the first paper studying the causes and consequences of the Settlement movement.

Outline of the Paper The rest of the paper is organized as follows. Section II provides a high-level overview of the historical background. In section III, I describe the data and explain how I construct the analysis samples. Section IV presents a quantitative exploration of the factors that originated the Settlement movement. I discuss the causal effects of settlement houses on immigrants in section V and investigate the underlying mechanism in section VI. Section VII concludes.

II HISTORICAL BACKGROUND

This section presents the central features of the historical background I examine. First, I provide a succinct overview of the history of the American Settlement movement within the broader Progressive era. Then, I present an essential history of the Age of Mass Migration, focusing on the urban segregation of immigrants at the turn of the Nineteenth century.

[7] The study of social movements is established in sociology (e.g., see Della Porta and Diani, 1999), but it has thus far received relatively little attention from economists (Boudreau, Macchiavello, Minni and Tanaka, 2024).

II.A The Settlement Movement in the United States

The Settlement movement in the United States originated in 1886 when Stanton Coit founded the University Settlement in New York's Lower East Side. Coit had spent three months in Toynbee Hall, a settlement house in the working-class parish of St. Jude's, London (Briggs and Macartney, 1984). In the United Kingdom, members of the economic élite had already started establishing settlement houses in deprived parts of industrial centers to alleviate the deteriorated living conditions of urban workers.

In the United States, the Settlement movement faced peculiar challenges (Bremner, 1956). American cities underwent tumultuous industrialization and growth between the end of the Nineteenth and the beginning of the Twentieth centuries. Their sprawl was partly fueled by large inflows of immigrants, mainly from Europe, who reached America in the tens of thousands every year during the so-called Age of Mass Migration (Eriksson and Ward, 2019). Immigrants were segregated along ethnic lines within cities, seldom spoke English, and constituted the bulk of the poor working class.

Coit, himself a graduate of Amherst College, the other founders of the University Settlement, and the vast majority of the reformers that participated in the Settlement movement were not members of the urban working class (Carson, 1990). Most settlement houses were established by relatively wealthy individuals who moved into impoverished areas of sprawling industrial cities. The middle- and upper-middle-class origins of the Settlement movement in the United States are reflected in the double purpose of settlement houses, explicitly declared by its participants, as centers of "learning" about the living conditions of the poor as much as providers of assistance. The "Settlement" name itself reflects that relatively wealthy individuals "settled" poor neighborhoods. They resided in the settlements, paid room and board, and volunteered their time in community service (Trolander, 1987). Non-resident volunteers, especially in larger houses, were also present. Residents did not have prior education in social work, but there is sporadic evidence that the leaders of the settlements sought to provide them with essential training.[8] An overwhelming majority of volunteers in settlement houses, as well as its most representative spokespersons, were educated women (Goldin, 2021).

The prevailing approach among social workers was to decide the services the settlement house would provide together with the neighborhood members (Berry, 1986). This practice implied that settlement houses offered a diverse range of activities depending on the specific needs of their communities. Free kindergartens, a major innovation in childcare that would shape the evolution of the American family, were popularized by settlement houses. Settlements would host camps and playgrounds for chil-

[8] For example, in 1903, Graham Taylor of Chicago Commons started offering training in conjunction with the University of Chicago. These classes evolved into what is today the Crown School of Social Work of the University of Chicago.

dren and serve educational purposes in support of formal schooling. They pioneered health services such as clinics, convalescence homes, and milk stations. As part of their effort to support the immigrants' assimilation into American societies, settlement houses offered language classes and courses designed to prepare them for naturalization. A typical settlement would also offer technical and professional classes to improve the labor market opportunities for immigrants, along with recreational community-building activities, ranging from dance to literary and arts and crafts clubs.

In their dual role of social workers and reformers, settlement house volunteers spearheaded the broader Progressive movement for social reform (Davis, 1984). Jane Addams, the founder of Hull House in Chicago, is among the most well-known representatives of the Settlement movement and the first woman to win a Nobel prize for peace. In her words, settlement workers not only needed "scientific patience in the accumulation of facts," but they also had to "arouse and interpret the public opinion of their neighborhoods, [...] furnish data for legislation, and use their influence to secure it" (Addams, 1920, p. 127). Participants of the Settlement movement championed—and, in many cases, obtained—reforms in disparate areas. These included improved sanitation and health services, access to social housing, increased coverage of public schooling, and the abolition of child labor. Progressive activists promoted unionization and democratic institutions and actively participated in the Civil Rights Movement after the Second World War.

II.B Immigration in American Cities During the Age of Mass Migration

Between 1850 and 1920, the "Age of Mass Migration," almost 30 million European immigrants settled in the United States (Abramitzky and Boustan, 2017).[9] Immigration was largely an urban phenomenon. In 1900, approximately 63% foreign-born lived in urban centers compared to 35% of natives, and by 1930, the share increased to 79% compared to 53% among natives (Eriksson and Ward, 2022). Cities offered high wage premia, which benefitted immigrants. Living conditions, however, were poor. Congestion, pollution, lack of sewage, and clean water resulted in high mortality rates and precarious sanitary environments (Troesken, 2004; Ager, Feigenbaum, Hansen and Tan, 2024).

Within cities, immigrants formed enclaves along ethnic lines (Eriksson and Ward, 2019). Immigrant segregation was strong for some first-wave immigrants in some cities—e.g., the Irish in Boston and the Germans in Cincinnati—but considerably increased for second-wave immigrants from countries such as Italy and Russia. Social networks within ethnic enclaves provided assistance to the immigrants.

[9] Throughout the period, the United States maintained an open-border policy approach. Country-specific immigration restrictions targeted the Chinese (Chinese Exclusion Act, 1882) and the Japanese ("Gentlemen's Agreement" between the US and the Japanese governments, 1907) but did not affect European migrants. The open-border approach was challenged by nativist movements since the early 1910s and was eventually abandoned with the 1921-1924 Quota Acts.

Recent evidence, however, indicates that ethnic-based networks and enclaves may have hindered cultural assimilation and possibly exerted a negative impact on the immigrants' economic welfare (Eriksson, 2020; Gagliarducci and Tabellini, 2022; Abramitzky, Boustan and Connor, 2024).

Cultural assimilation patterns also dramatically diverged between first- and second-wave immigrants. Second-wave immigrants, in particular, were perceived as more culturally distant from natives, were more likely to be male, younger, and less likely to settle in the United States permanently (Hatton and Williamson, 1998). Abramitzky *et al.* (2020) use American-sounding names given to children to document gradual, albeit incomplete, cultural assimilation that increased in the length of stay in the US and was stronger for immigrants from more culturally distant countries from the US.

III DATA

This section describes the data I use in the analysis and the procedures I follow to construct the final datasets. I first describe the newly digitized data on historical settlement houses. Then, I briefly comment on the variables constructed from the population censuses. Finally, I explain how I construct consistent within-city geographical units to study the causes of the emergence of settlement houses and the intergenerational individual-level samples employed to assess their consequences.[10]

III.A Settlement Houses

Data on settlement houses are digitized from the *Handbook of Settlements* (Woods and Kennedy, 1911). The *Handbook* was published in 1911 to continue the activity of the *Bibliography of Settlements*, edited by the College Settlement Association, which surveyed existing settlement houses but had been discontinued in 1905. The *Handbook* contains detailed information on *all* settlement houses, active and extinct, in 1911. Each settlement is described in a separate section, whose length ranges from half a page to several pages, depending on the variety of volunteering and scientific activities it conducts. I digitize the entire book, which covers 411 settlement houses.[11]

The information contained in the *Handbook* covers the name of the settlement, the date—day, month, and year—it was established and, possibly, terminated, the address—and changes thereof, along with the move-in dates—, a list of activities carried out by the volunteers and the residents, the number of residents and volunteers, typically split by gender, the group of users it targeted, the church affiliation, if any, and the name of the superintendent(s). Out of the 411 settlements in the volume, 15 do not list

[10] Appendix section A.I provides additional complementary information.

[11] This count excludes federation of settlement houses, which typically did not offer any service, nor did they have a physical venue, but served organizational purposes.

the year of establishment; hence, I discard them. Moreover, the address is missing in 24 other cases, which I also exclude from the sample. Ultimately, the sample thus comprises 372 settlement houses established between 1882 and 1911. In some cases, information on the target group, the religious affiliation, and the residents and volunteers' composition is missing; however, since I do not use these variables in the analysis, I retain the settlements with missing data in these categories in the sample.

Using the address listed in the digitized records, I geo-reference each settlement to precise coordinates using a commercial geo-coding software tool. This procedure allows me to locate each settlement within the city where they are located and assign them to historical enumeration districts.

Figure I provides a glance at the temporal evolution of the settlement movement and the spatial distribution of settlements in one sample city. Panel Ia plots the number of active settlements by year of establishment in the entire United States (black line) and the urban sample (dashed gray line). The number of settlements steadily increased throughout the period and peaked in 1920, when almost 500 settlements were active (Danilov, 2013). While the coverage of the *Handbook* ends in 1911, my sample thus comprises over 80% of all settlements within the Settlement movement. Panel Ib reports the settlements' location (red dots) in Boston.[12] The black line reports the borders of the enumeration districts in 1880, while the gray polygons display the hexagonal tessellation that generates consistent geographical within-city geographical units across censuses, as explained below. Settlements were scattered over the Boston urban area although, unsurprisingly, they clustered in the North and West End districts, which hosted the bulk of the Boston immigrant community.

III.B Census Data

I use data from the federal population censuses between 1880 and 1940 to construct several outcome and control variables at the neighborhood and individual levels (Ruggles, Alexander, Genadek, Goeken, Schroeder, Sobek *et al.*, 2024).[13] Broadly speaking, I construct outcome variables related to the labor market, family and fertility decisions, and assimilation dynamics.

To look at the labor market success of immigrants, I consider the rate of labor force participation, the probability of having a high-skill occupation, the probability of having blue-collar *vis-á-vis* white-collar manufacturing occupations, and the inverse hyperbolic sine (IHS) of an occupation-based measure of income.[14] To study family and fertility decisions, I consider the probability of marriage, an

[12] Appendix figure C.1 reports the location of the settlements in all other cities.

[13] The records of the 1890 census are no longer existent.

[14] The "high skill" occupations are those categorized as "Professional, Technical" and " Managers, Officials, and Proprietors" in the IPUMS taxonomy. The rationale is that these occupations require substantial investment in human capital. Since

indicator of whether an individual has at least one child, and the IHS of the number of children. Importantly, both fertility variables reflect *completed* fertility.[15] Lastly, to measure the cultural assimilation of the immigrants, I look at the probability of speaking English, being naturalized as a US citizen, marrying an immigrant from another country, and marrying a native US citizen. In addition, I construct the Foreign Name Index (FNI) along the lines of (Abramitzky *et al.*, 2020). According to this metric, immigrants who gave more foreign-sounding names to their offspring assimilated to a lower extent into the American society.

From the 1940 census, which I use to evaluate the intergenerational consequences of the settlements, I also extract information on educational attainment, which is not recorded in earlier waves.

The nature of the treatment implies that at usual levels of aggregation, such as counties or cities, I would be unable to detect the effects of settlement houses. Settlement houses operated on smaller scales—neighborhoods—given their size, and aggregating individuals over such large areas would artificially dilute their effects. To run the analysis at the sub-city level, however, I need to locate individuals within the city where they lived. To do so, I combine information about the enumeration district contained in the census with historical neighborhood `GIS` data constructed by Shertzer, Walsh and Logan (2016), as detailed in the next section.[16]

III.C Construction of the Samples

I construct two datasets to conduct the analysis: the first one is a panel of within-city consistent geographical units that I follow at a decade frequency between 1880 and 1940. The other is an individual-level cross-sectional dataset compiled by stacking census waves from 1880 to 1940. In this section, I explain how I construct these two datasets.

III.C.1 Hexagon-Level Panel

To study the determinants of the emergence of settlement houses, I would ideally need to observe the evolution of each neighborhood over time between 1880, before the Settlement movement, and

actual income is not recorded until 1940, occupation-based income is the standard proxy for earlier periods (e.g., Ager, Boustan and Eriksson, 2021).

[15] Appendix A.I.2 contains a detailed description of the algorithm I follow to construct the parent indicator and the number of children. The challenge is that the census returns information on children living in the household at the time of the census, but does not provide longitudinal information on those who left the household before. I leverage intergenerational linked data to circumvent this issue.

[16] The enumeration district `GIS` files are available for Baltimore, Boston, Brooklyn, Chicago, Cincinnati, Cleveland, Detroit, Manhattan, Philadelphia, Pittsburgh, and St. Louis. These cities contain approximately 50% of the settlements in the entire US territory.

1940. Within-city geography is available at the enumeration district level, and, using the information contained in the population census, I can map individuals to their enumeration district of that census wave. Enumeration district boundaries, however, change substantially across census waves and are thus unusable as a consistent unit of observation over time.

I follow the methodology proposed by Shertzer and Walsh (2019) to tackle this limitation. I overlay a hexagonal grid on each decade's enumeration district `GIS` files to construct consistent geographical units. Then, I compute crosswalk weights to impute data from the enumeration district to the hexagon level. The weights are proportional to the share of the area of each district that overlaps with the area of the hexagons. Because the hexagonal grid is time-invariant, this procedure allows me to observe a balanced panel of hexagons at the census-decade frequency between 1880 and 1940. Importantly, since city boundaries vary over time, I restrict them to the area occupied by each city in 1880. Appendix A.I provides additional technical details.

Panel A of Appendix Table B.2 provides key descriptive statistics for a set of variables for the hexagon sample. Columns (1–4) (resp. 5–8) refer to men (resp. women). Approximately 6.4% of the hexagons have a settlement. On average, hexagons have a population of 3,000, of which 30% are immigrants. The Table then compares labor-market indicators on the entire and the immigrant population. Immigrants are more likely, on average, to be in the labor force, earn more (occupation-based), and are substantially more likely to hold blue-collar manufacturing occupations.

III.C.2 Individual-Level Cross Section

To explore the effect of settlement houses on immigrants' welfare, I restrict the attention to immigrants already in the United States when the Settlement movement emerged in the early 1900s. I compile an individual-level cross-sectional dataset by stacking data from census waves between 1880 and 1940. I link individuals between each wave and the 1900 census using the intergenerational links produced by the Census Tree Project (Price *et al.*, 2021; Buckles *et al.*, 2023) to observe the enumeration district where they lived when settlement houses were being established, as well as other individual-level variables included as controls in the analysis.[17] My analysis thus excludes temporary migrants, who represented a substantial share of the immigrant inflow (Bandiera, Rasul and Viarengo, 2013), as is customary in the literature working with across-census linked samples (e.g., Abramitzky *et al.*, 2014). The sample ultimately comprises the entire foreign-born working-age population (aged between 15 and 65).

[17] A major advantage of the Census Tree Project, compared to previous intergenerational linking methods, such as those employed by the Census Linking Project (Abramitzky *et al.*, 2022a,b), is that it allows me to observe women. Reassuringly, however, all the results I document for the male subsample remain unchanged when using the links produced by the Census Linking Project.

One concern with the Census Tree Project is that linked individuals do not constitute a representative sample of the population. In Appendix Table B.1, I thus compare my estimation sample with an alternative sample obtained using the links constructed by Althoff *et al.* (2025). While the advantage of these links is that they are representative of the population, the linking rate is substantially lower. The sample obtained using the representative weights comprises only 10% of the observations in the estimation sample.[18] This large drop in sample size motivates why I use the Census Tree links in the baseline analysis. Despite the different methodologies employed by the two linking algorithms, however, I find that the differences between my estimation sample and the alternative sample obtained using the links of Althoff *et al.* (2025) are very small in magnitude (columns 2–3) relative to the mean (column 4). The two samples remain comparable even when looking separately at men (columns 5–7) and women (columns 8–10). These patterns suggest that the non-representativeness of the Census Tree links is unlikely to be a major concern for the analysis.

Using the intergenerational links, I can thus observe the enumeration district where each individual lived in 1900. I construct a measure of exposure of each district to settlement houses based on their proximity to the nearest settlement. Specifically, I consider an individual "treated" if at least one settlement house existed within 250 meters (0.15 mi) of the centroid of the enumeration district where that person lived in 1900. I assign the year when that settlement is established as the treatment date for that individual. In robustness exercises, I evaluate how the treatment effects vary when changing the exposure distance threshold.

Panel B of Appendix Table B.2 provides sample statistics for the individual-level dataset by gender. Approximately 12% of immigrants in 1900 lived in districts exposed to a settlement house.

IV UNDERSTANDING THE EMERGENCE OF SETTLEMENT HOUSES

In this section, I explore the proximate causes of the emergence of settlement houses.[19] First, I provide descriptive evidence on settlement houses and the activities they offered to the immigrants. Then, I show that the presence of immigrants in 1880 is the most predictive variable for the emergence of settlement houses. Finally, I look at population dynamics after settlement houses were established.

[18] This count excludes the 1900 census wave, which appears in its entirety because the other census waves are linked to the 1900 wave.

[19] The Settlement movement had broader cultural foundations, but tracing their historical significance is beyond the scope of this paper. My purpose is to understand why settlement houses were established where I observe them.

IV.A Descriptive Evidence on Settlement Activity

Appendix Table B.3 reports several descriptive statistics on the final dataset of settlement houses. In columns (1–3), I report the statistics for the entire sample contained in the *Handbook*; columns (4–6) restrict the attention to the settlements located in one of the cities in the analysis sample.

The number of residents—workers who lived in the house venue—and volunteers conveys the sense of the size of those establishments. Settlements had, on average, 28 volunteers and six residents. The settlements in the urban sample are slightly bigger, with eight residents and 37 volunteers. Settlements thus constituted important elements in their communities: by comparison, the average manufacturing establishment in 1880 had 14 employees (Hornbeck and Rotemberg, 2024). As evidenced by the historical literature, most involved personnel were female (75%). Most settlement houses (85%) were located in Northeastern and Midwestern states. Within the urban sample, which over-samples cities in those areas, almost 93% of settlements are located in those two areas.

Settlements offered a wide range of services. A primary purpose of settlements was professional development. More than 80% settlements offered professional classes covering various subjects, from sewing to metalworking. Childcare also featured prominently: 50% of houses had a nursery, and almost 60% offered kindergarten services. Further education activities, akin to primary schooling, were offered by 64% of the settlements. A non-negligible share of settlements (19% in the entire sample and 24% in the urban sample) offered citizenship, naturalization, and English language classes in English, which were explicitly devoted to the cultural assimilation of immigrants.

Immigrants were, in fact, the primary target of the settlement house movement. More than 50% of settlements declared immigrants to be their primary target. This share increases to 70% in the urban sample, as one would expect given that immigrants typically clustered in urban centers. Approximately 20% settlements list Italians, the largest ethnic group among the "new immigrants," as their primary target. Between 20% and 30% addressed the Jewish community, partly reflecting the fact that more than 10% of the settlement houses listed "Jewish" as their religious denomination.

IV.B What Factors Determined the Establishment of Settlement Houses?

I now provide a more formal assessment of the factors that influenced the establishment of social settlements in US cities. To do so, I employ the hexagon-level dataset but restrict the sample to the 1880 decade. The first settlement house was established in 1882, therefore, looking at hexagons before the Settlement movement had taken off permits to isolate the factors that contributed to its diffusion.

I run a set of regressions where the main explanatory variable of interest is the presence of a settlement

house in the later years:

$$y_h = \alpha + \beta \times Settlement_h + X_h'\Gamma + \varepsilon_h, \tag{1}$$

where h denotes a hexagon, $Settlement_h$ is equal to one if, throughout the sample period, a settlement is established in hexagon h, X_h collects hexagon-level controls, and ε_h is the idiosyncratic error term. Since hexagons are considerably heterogeneous in terms of their population, I weigh them by population to ensure that, population-wise, small areas do not drive the results. Standard errors are clustered at the hexagon level. The term X_h is either empty or includes city-fixed effects. For comparability, the dependent variables y_h are standardized.

Figure II reports the results. The dots report the estimated $\hat{\beta}$ coefficient from regression (1). The black dots refer to the specification without city fixed effects, whereas the gray dots include them. City fixed effects are important because the decision to establish a settlement house was undertaken at the local level by the urban élites. Therefore, by including city fixed effects, the estimates reflect within-city variation instead of less relevant, from the decision maker's perspective, between-city variation. Appendix Table B.4 displays analogous results in tabular form. In Appendix Figure C.2, I report the visual positive correlation between settlement presence and the immigrant share.

In Panel IIa, the dependent variable is constructed over the entire population. There is a positive correlation between the presence of a settlement and population. Quantitatively, districts with a settlement have half-a-standard-deviation larger populations, which is approximately equivalent to 600 individuals. Except for population, I do not find any systematic correlation between other demographics and the presence of settlements. Hexagons with a settlement have slightly higher labor force participation, but this quantitatively small correlation disappears when including city fixed effects. Similarly, they have slightly higher income per capita, but this pattern is driven by between-city variation.

In Panel IIb, I explore whether demographics related to the immigrant population are more relevant to explain the emergence of settlement houses. To this end, the dependent variables are computed on the immigrant population.[20] In hexagons with at least one settlement, the share of immigrants within the population is considerably higher: between .75 and .5 standard deviations, depending on whether city fixed effects are included. The historical literature suggests that the composition of the immigrant population should matter. Immigrants from countries that had entered the period of mass migration earlier, such as the UK, Germany, and the Nordic countries, had been assimilating for several decades and, by the end of the century, would not be part of the poor masses entering the United States (Abramitzky *et al.*, 2014). By contrast, immigration from Southern and Eastern Europe was on

[20] For example, the labor force participation rate, which in Panel IIa is simply the share of individuals who are working, in Panel IIb is the share of working *immigrants*.

the rise, and the migrants from those countries would flow into the large masses of urban poor. The evidence in Panel IIb confirms these conjectures. Immigration from North-Western European countries is not associated with the establishment of settlement houses. Interestingly, neither is Eastern European immigration, even though this may be due to the small number of immigrants from those countries in 1880. Immigration from South-Western European countries is, instead, strongly associated with the future presence of social settlements. Conversely, the composition of the immigrant population in terms of occupation, gender, and age is not associated with a differential likelihood of settlement presence.[21]

This quantitative exercise confirms that settlement houses were established primarily in response to increased immigration. The social reformers of the Progressive era reacted to the deprived living conditions of immigrants in cities by establishing one of the first forms of welfare state: the settlement houses. The preponderant centrality of immigration as the core driver of the expansion of settlement houses motivates the focus of the rest of the paper on immigrants.

IV.C Population Dynamics After the Establishment of Settlement Houses

I now explore how the inflow of immigrants across neighborhoods and their labor market performance evolved after settlement houses were established. I employ the hexagon decade-level panel described in section III.C.1. I compare hexagons before and after a settlement is established within their borders in a difference-in-differences setting to net out aggregate trends in immigration and other unobserved heterogeneity.

I estimate variations on the following specification:

$$y_{h,t} = \alpha_h + \alpha_{c(h)\times t} + \sum_{\substack{k=-20 \\ k\neq -10}}^{30} \beta_k \times I\left(t - \tau_h = k\right) + \varepsilon_{h,t}, \tag{2}$$

where h, $c(h)$, and t denote a hexagon, the city where it is located, and a census decade. The terms α_h and $\alpha_{c(h)\times t}$ denote, respectively, hexagon and city-by-decade fixed effects. The term τ_h denotes the first decade after which at least one settlement house is established in hexagon h, and the variables $I(\cdot)$ are event-time dummies. City-by-time fixed effects imply that I leverage within-city time variation at the neighborhood level, thus ensuring that my estimates do not conflate city-level, possibly correlated shocks. As in the previous analysis, hexagons are weighted by population to ensure that thinly populated units do not drive the results. Standard errors are clustered at the hexagon level.

[21] In Appendix figure C.3, I use LASSO to select the most relevant predictors of the establishment of settlement presence and compute their correlation with the presence of a settlement. The estimates confirm the patterns highlighted here.

For space, I also estimate a variant of specification (2) where the pre- and post-treatment periods are conflated into two categories.

Settlements are established at different times across hexagons. As noted by Goodman-Bacon (2021), the standard two-way fixed-effects estimator (2) fails to estimate a convex average of the treatment effects when those are not constant over time. I thus employ the estimator proposed by de Chaisemartin and d'Haultfœuille (2024).

The estimates thus obtained do not necessarily convey the *causal* effects of settlement houses. The identifying parallel trends assumption requires that districts with and without a settlement house would not have experienced diverging trajectories in the outcomes in the absence of the settlements. The ten-year frequency of the data does not allow me to produce a convincing evaluation of the plausibility of this assumption. While I generally estimate pre-treatment coefficients ($\hat{\beta}_{-20}$) that are not statistically different from zero, I cannot rule out that, within the treatment decade, settlement houses are established in response to changes in the outcome variable that happen before, but that I observe as contemporaneous to the settlement because of the ten-year window. Thus, this analysis should be interpreted as providing evidence of the evolution of population dynamics before and after the establishment of settlement houses rather than as the effect of settlement houses on those variables.[22]

Figure III presents the results of the flexible difference-in-differences model (2). Panel IIIa shows that hexagons with a settlement received a large inflow of immigrants. The inflow peaked ten years after the settlement was established and reverted to zero over the following decades. Immigration was quantitatively sizable, as the number of arrivals over two decades is approximately equal to the pre-treatment average hexagon immigrant population. Following this inflow, the immigrant share in "treated" hexagons increased, as shown in Panel IIIb by 10%, and the increase remains statistically significant until 20 years after the settlement is established. In line with the historical scholarship, my preferred interpretation of these patterns is that social reformers constituted settlement houses in response, at least partially, to the booming immigrant communities.

In Panel IIIc, the dependent variable is the share of working specification, also termed the labor force participation rate. The estimates indicate that the share of working immigrants decreased by 5% in hexagons with at least one settlement. It is plausible that the decrease in the rate of labor force participation is jointly explained by an inflow of younger immigrants, who would be more likely to have children, who would, in turn, not work, and by increased competition in the labor market following the inflow of the working-age immigrants themselves. In Panel IIId, I look at the occupation-based imputed income per migrant. The estimates reveal a drop in income per capita among migrants.

[22] Conversely, I cannot rule out that some of the dynamics I document are *not* caused by the settlement houses.

While this pattern partly reflects lower overall labor force participation rates, it also indicates that the immigrants took on increasingly less well-paid occupations.

Table I replicates the previous results in the pre-post setting. In Panels B and C, I report the results split by gender. The change in the male immigrant population (column 1) was 25% larger than the female, reflecting the skewed sex ratio of the overall immigrant population. The labor force participation rate and income per migrant decreased more among men than women. In columns (5) and (6), I show that the share of immigrants working in more skilled white-collar occupations increased. In contrast, the blue-collar employment share increased, in the entire population and separately by gender.

Appendix Table B.5 displays the shift in the mean share of immigrant mothers (column 1), the number of children per immigrant woman (column 2), the share of immigrant wives (column 3), and the number of foreign-born children at school and at work (columns 4 and 5) after the establishment of settlement houses. I estimate a 12% increase in the number of children per woman and an 8.4% increase in the share of foreign-born children attending school, which corresponds to approximately 10% of the mean. These patterns are consistent with a younger population of newly arrived immigrants.

The results are consistent with the historical scholarship on the Settlement movement. Settlement houses emerged in high-immigration neighborhoods, where the newly arrived immigrants were younger, less likely to work, poorer, and more likely to be employed in low-skill manufacturing jobs.[23]

V THE EFFECTS OF SETTLEMENT HOUSES ON IMMIGRANTS

This section presents the main causal results of the paper. I first discuss how exposure to settlement houses impacted the economic welfare of immigrants. Then, I explore the assimilation dynamics triggered by the presence of settlement houses, which indicated cultural assimilation of the immigrants into American society as one of their primary purposes.

V.A Research Design

To analyze the causal effects of settlement houses on immigrants, I employ the individual-level dataset described in section III.C.2. I employ a triple difference framework, which relies on three sources of variation: (i) across districts, as settlement houses were established in some districts and not in others; (ii) across cohorts, because settlement houses offered assistance that primarily targeted the young working-age population; and (iii) over time, namely, before and after settlements were established.

In terms of across-district variation (i), I consider a district as "exposed" to settlement houses if at least

[23] By contrast, in Appendix table B.6, I show that the non-migrant population shrunk in areas with settlements.

one settlement is established within 250 meters (0.15 miles) from its centroid.[24] Settlement houses were community-based associations that operated on a relatively small geographic scope. It is thus sensible to assume that only individuals who lived in their proximity could access the services they provided.

In terms of across-cohort variation (ii), I consider an individual as "exposed" to a settlement if they were below 40 when the settlement was established. The activities offered by settlement houses targeted relatively young immigrants. Kindergarten and nursery services benefitted young households. Professional classes were similarly targeted at immigrants entering the labor market rather than those who had already acquired the necessary skills. Finally, it is plausible that naturalization and assimilation activities would be more beneficial to relatively younger immigrants and their children compared to older cohorts. Additionally, in this part of the analysis, I focus on the effects of settlements on adults. Hence, the sample only includes individuals aged 15 or above who live in neighborhoods exposed to a settlement.[25]

In terms of across-time variation (iii), individuals living in a district with at least one settlement are considered "treated" only after the settlement is established. Since the dataset is a stacked cross-section, the time variation in exposure is measured at the (census) decade frequency. By leveraging variation across time, I can evaluate the empirical plausibility of the underlying identification assumption, as explained in more detail in the next section.

I estimate variations of the following empirical specification:

$$y_i = \alpha_{d(i)\times c(i)} + \alpha_{d(i)\times t(i)} + \alpha_{c(i)\times t(i)} + X_i'\Gamma + \delta \times \left(Settlement_{d(i)} \times Post_{t(i)} \times Young_{c(i)} \right) + \varepsilon_i, \quad (3)$$

where i denotes an individual in cohort $c(i)$, observed in census wave $t(i)$ who, in 1900, lived in enumeration district $d(i)$. Term X_i collects individual-level controls, namely, country of origin, race, gender (when applicable), and year of immigration. The term $Settlement_{d(i)}$ is an indicator equal to one if at least one settlement house is established within 250 meters of the centroid of district $d(i)$. The variable $Post_{t(i)} \equiv I\left(t(i) \geq t^*_{d(i)}\right)$ is an indicator equal to one for census waves that follow set-

[24] The results remain qualitatively unchanged when moving this threshold between 150 and 500 meters. I report the estimates obtained with alternative proximity thresholds in Appendix Figures C.8, C.9, C.10, and C.11. Additionally, in Appendix Figures C.4, C.5, C.6, and C.7, I show that the estimates remain stable when excluding one city at a time from the estimation sample. Appendix Tables B.12, B.13, B.14, and B.15 report the results obtained using alternative standard error estimators and confirm that the significance of the baseline results remains virtually unaltered.

[25] I choose the 15-year-old threshold as most states in 1900 prescribed compulsory schooling until 14 years old. The results remain qualitatively unchanged when the threshold is increased to 20 years old. In addition, I exclude those born before 1850 because they would have largely dropped out of the labor force by 1930.

tlement house established in year $t^*_{d(i)}$, if any, and zero otherwise. Finally, the term $Young_{c(i)}$ is equal to one for individuals who were 40 or younger when the settlement was established. The triple interaction between these three terms captures the estimated treatment effect of settlement houses. In the next section, I provide a detailed characterization of the associated estimand and the identification assumptions required for valid causal inference. Standard errors are clustered by district, which is the level at which the roll-out of settlement houses varies.

Regression (3) is a saturated triple difference model. The term $\alpha_{d(i)\times t(i)}$ denotes district-by-census fixed effects and controls for time-varying confounding factors that vary at the district level. Hence, this fixed effect term flexibly controls for the inflow of immigrants in district $d(i)$ over time. More generally, district-by-census fixed effects partial out any time-varying confounding factor that impacts individuals living in the same district, regardless of their age.[26] By including district-by-cohort fixed effects ($\alpha_{d(i)\times t(i)}$), I control for factors that affect individuals living in districts exposed to settlement houses irrespective of the time when settlements are exposed. Lastly, census wave-by-cohort fixed effects ($\alpha_{c(i)\times t(i)}$) control for age effects: since individuals born in the same year can be observed in different censuses, census-birth year pairs uniquely identify age categories. Hence, census-cohort fixed effects ensure that the identifying variation compares individuals of the same age, thereby eliminating life-cycle dynamics that could otherwise spuriously drive the results.

V.B Discussion of the Identification Assumption

To identify the causal effect of settlement houses, I do not require that settlements be randomly distributed across districts. The most natural challenge to such an argument is, in fact, that their location and timing were endogenous to time-varying correlated shocks. Consider, for the sake of the argument, a simpler double difference estimator that relies on variation in settlement presence ($Settlement_{d(i)}$) over time ($Post_{t(i)}$). In this case, identification would require that districts with and without a settlement would have followed similar trajectories if settlements had not been established. In Section IV, however, I showed that settlement houses were more likely to be established in areas with larger immigrant inflows. More generally, it is plausible that possibly unobserved time-varying confounding factors at the district level correlate with immigrant inflow and the establishment of settlement houses.

For the triple difference coefficient δ to identify the average treatment effect on the treated, instead,

[26] A non-exhaustive list of possibly correlated shocks that are purged out of the identifying variation through neighborhood-by-time fixed effects—assuming that the shocks do not differentially affect young and old individuals within the same neighborhood—includes ethnic religious institutions (Gagliarducci and Tabellini, 2022), labor unions (Farber, Herbst, Kuziemko and Naidu, 2021), and kindergartens (Ager and Cinnirella, 2020).

specification (3) requires that trends in the treated and the control groups would not have diverged if settlement houses had not been established (Olden and Møen, 2022). By comparing young and old individuals within the same districts through district-by-census fixed effects ($\alpha_{d(i)\times c(i)}$), specification (3) ensures that the identifying variation is purged of time-varying factors that do not have a differential impact depending on age.

I provide two tests to evaluate the empirical plausibility of this identification assumption. First, I perform a set of event-study designs that display the evolution of the differences between young and old individuals by the time since the settlement is established. Formally, I estimate variations on the following regression:

$$\begin{aligned} y_i = &\alpha_{d(i)\times c(i)} + \alpha_{d(i)\times t(i)} + \alpha_{c(i)\times t(i)} + X_i'\Gamma + \\ &+ \sum_{\substack{k=-20 \\ k\neq -10}}^{k=+20} \delta_k \times \left[Settlement_{d(i)} \times Young_{c(i)} \times I\left(t(i) - t^*_{d(i)} = k\right)\right] + \varepsilon_i, \end{aligned} \tag{4}$$

where the generic indicator $I\left(t(i) - t^*_{d(i)} = k\right)$ codes the number of periods since a settlement was established in district $d(i)$.[27] Coefficients $\{\delta_k\}_{k=-20}^{+20}$ quantify the difference between young and old individuals before and after the establishment of the settlement. Statistically insignificant pre-treatment coefficients $\{\hat{\delta}_k\}_{k<0}$ lend support to the identification assumption that, if settlement houses were not established, young and old individuals in treated and non-treated neighborhoods would not have experienced diverging outcomes.

Second, I let treatment effects vary across age bins at the settlement establishment date:

$$\begin{aligned} y_i = &\alpha_{d(i)\times c(i)} + \alpha_{d(i)\times t(i)} + \alpha_{c(i)\times t(i)} + X_i'\Gamma + \\ &+ \sum_{k=60}^{20} \delta_k \times \left[Settlement_{d(i)} \times Post_{t(i)} \times I\left(c(i) - t^*_{d(i)} \in (k, k-5]\right)\right] + \varepsilon_i, \end{aligned} \tag{5}$$

where the running variable k bins age at exposure into five-year windows and the set of dummy terms $I\left(c(i) - t^*_{d(i)} \in (k, k-5]\right)$ code the age when individual i is first exposed to a settlement house. The initial age bin $(65, 60]$ serves as the baseline category. Since settlement houses offered services that especially targeted young individuals, I expect the treatment effects $\{\hat{\delta}_k\}_{k\leq 40}$ to be statistically significant. By contrast, the coefficients associated with older cohorts provide "placebo" tests. Under

[27] Since censuses are run at the decade frequency but settlements can be established in any year, I round the year when the first settlement is established in district d to the closest subsequent decade. Hence, for example, if in a given district, the first settlement is established in 1907, I consider the 1910 census wave as the first treated period. With a slight abuse of notation, I thus denote with $t(i) - t^*_{d(i)}$ the number of decades between census $t(i)$ and the closest census year that follows the year when the settlement is established $t^*_{d(i)}$.

the stated identification assumption, older individuals should not respond to exposure to settlement houses. Hence, any statistically significant estimate in that age range would falsify the identification assumption.

To provide additional evidence on the causal nature of my estimates, I report the estimates when weighing individuals by their propensity score in the Online Appendix.[28] This procedure ensures that treatment and control individuals have similar probabilities of being treated and yields very similar results to the baseline estimates. Appendix section A.II describes the additional robustness exercises mentioned in passing in the main text.

V.C The Labor Market Effects of the Settlement Movement

I start by looking at the labor-market impact of settlement houses on the immigrant population. In Figure IV, I report the across-time and across-cohort effects of settlement houses on white-collar employment. In Panel IVa, each dot reports the estimate of one coefficient in regression (4). In Panel IVb, each dot reports the estimate of one cohort-specific coefficient in regression (5). The gray dots refer to the female subsample, while the black dots are obtained from the male subsample.

I uncover notably divergent trajectories across genders. Starting from Panel IVa, I find no statistically significant differences among either young and old men or women in treated cohorts and neighborhoods before settlement houses were established. This pattern provides evidence in support of the parallel trends assumption. After settlement houses are established, however, my estimates indicate that men and women experience substantially different trajectories. White-collar employment increases among men as the dynamic treatment effects are statistically significant up to twenty years after settlements are established. Quantitatively, the probability of working in a white-collar occupation increases by 2-4% over time after the opening of a settlement. By contrast, the probability of white-collar employment for women does not change after settlement houses are established.

In Panel IVb, I display the results of the complementary exercise, which lets the treatment effect vary by age. The estimates indicate that relatively younger men drive the increase in white-collar employment in response to settlement activity—between 35 and 15 years old—whereas older cohorts do not display a statistically significant treatment effect. This pattern is consistent with my argument that settlement houses would primarily target younger, working-age individuals as opposed to older cohorts. By contrast, women display stable and null responses to settlement activity regardless of their age.

[28] To compute the propensity scores, I include multiple variables that could influence the probability of being exposed to a settlement. These are the neighborhood, gender, race, country of origin, and year of immigration.

Table II reports the estimates of a variant of specification (3) where I conflate treated and untreated units into two categories for multiple labor-market outcomes. Panel A reports the estimates obtained over the entire sample, whereas in panel B (resp. C), I focus on the male (resp. female) subsample. The estimates refer to my preferred saturated specification, which flexibly controls for neighborhood-by-year, neighborhood-by-cohort, and cohort-by-year fixed effects, as well as individual-level time-invariant characteristics. Online Appendix table B.7 reports the estimates obtained from other specifications, including, in columns (4), (6), and (8), the propensity-score weighting scheme. The results remain qualitatively unchanged throughout.

The effect of settlements on labor force participation (column 1) and imputed income (column 2) is positive and statistically significant. Labor force participation increases in the overall sample, but this aggregate pattern conceals gender heterogeneity. The aggregate increase in LFP is driven by men (panel B). In contrast, women are less likely to work when they are exposed to settlement houses, even though their response is not statistically significant (panel C).

In columns (2) and (3), I split employment into white- and blue-collar occupations. White-collar occupations include liberal and managerial professions, clerical and sales workers, and the service sector. Blue-collar occupations, on the other hand, consist of low- and middle-skilled manufacturing jobs and farm laborers. The presence of settlement houses has a positive and statistically significant impact on the likelihood that immigrants will hold a white-collar occupation. Settlement houses, as discussed previously, offered educational services. It is thus plausible that these professional training and educational activities improved the immigrants' ability to land relatively skill-intensive occupations. The aggregate effect, which corresponds to a 1% increase, however, is entirely driven by men (panel B), while I estimate no statistically significant response among women (panel C). For men, the increase in white-collar employment is large, as it corresponds to approximately 10% of the mean.

The effect of settlement houses on blue-collar employment is small and hardly significant, especially when splitting the sample into men and women. Increased skilled employment and an overall higher labor force participation indicate that settlement houses plausibly improved the immigrants' labor market opportunities. The average imputed income commanded by individuals employed in White-collar occupations (40.9) is almost twice that of individuals employed in other occupations (26.01). The occupational income score, displayed in column (4), corroborates this interpretation, as it displays an average 2.1% increase. This gain is larger for men (4.2%) and lower for women (-1.1%, albeit statistically insignificant).

Settlement houses offered training and educational classes that could enhance immigrants' employment prospects and their ability to secure higher-skilled jobs. My estimates point, at least partly, in

this direction. Labor force participation and income increased, as did the probability of taking up white-collar, hence higher-skilled, jobs. These beneficial effects, however, are driven by men. Women exposed to settlement houses experienced no beneficial effects and, if anything, deteriorating labor market conditions in terms of labor force participation and income. In section VI, I explore some likely drivers of these gender differences.

V.D Settlement Houses and Immigrant Assimilation

Besides improving the living conditions of the urban poor, progressive activists explicitly promoted the cultural assimilation of the immigrants into American society. In this section, I thus evaluate the effects of settlement houses on several indicators of immigrant assimilation.

Figure IV reports the results on the probability of speaking English obtained from the flexible specification (4) (Panel IVc) and (5) (Panel IVd). I find no statistically significant difference between young and old cohorts of individuals on the probability of speaking English before settlements are established. By contrast, English command increases in the following years. Quantitatively, exposure to a settlement house raises immigrants' English spoken command by approximately 2.5 percentage points over the twenty years that follow its establishment. Unlike the labor market outcomes, I find no statistically significant difference in the treatment effects on men and women, which indicates that both genders benefited from exposure to settlement activity. In Panel IVd, I compare the response to settlement activity across cohorts. The estimates reveal that the bulk of the effect of settlements on English proficiency is driven by relatively young individuals aged between 35 and 15. By contrast, older individuals, i.e., those between 40 and 65, do not exhibit a statistically significant response to exposure to settlement houses. On the one hand, these figures thus provide reassuring evidence in favor of the identification assumption of the triple difference estimator. On the other hand, they indicate that settlement houses plausibly facilitated the integration of young immigrants into American society.

Table III reports the effects of settlement houses on a wider range of indicators of cultural assimilation. As before, panel A reports the estimates obtained on the entire population, whereas panels B and C focus on men and women. In columns (1–3), I leverage marriage data to construct measures of cultural assimilation. Column (1) reports the effect of settlements on the probability of marrying an immigrant from the same country, whereas in column (2) I focus on the probability of marrying an immigrant from a different country. I interpret both variables as indicating inter-group contact, but the probability of marrying a US native correlates more directly with cultural assimilation. A higher probability of marrying natives would indicate more frequent contact between the migrants and natives and would thus signal assimilation into American society. Between-country marriages, in turn, indi-

cate lower within-country immigrant segregation. Marrying a native citizen or an immigrant from another country was rare, as documented by Carlana and Tabellini (2024), partly because of the 1907 Expatriation Act, which deprived native women of citizenship if they married an immigrant. Citizenship would be restored if the husband would become naturalized. Overall, approximately 27% immigrant men (resp. 16% women) married a native US citizen, and 10% for both genders married an immigrant from another country. Column (3) focuses on the residual event of out-group marrying with native-born Americans. In column (4), I report the effect on the FNI, which by construction is only available for individuals with at least one child. Columns (5–6) display the effect of settlements on immigrants' language spoken (column 5) and both spoken and written (column 6) proficiency. Language is arguably a central indicator of cultural integration. Finally, in column (7), I look at the probability of obtaining naturalization status.

I find mixed messages on the effect of settlement houses on cultural assimilation. The estimates shown in columns (1–3) indicate that young individuals exposed to settlement houses were more likely to marry in-group with their co-nationals. In-group marriage comes at the expense of the likelihood of marrying native-born individuals. Quantitatively, exposure to settlement houses increased the probability of marrying immigrants from the same country by 3.9%, which approximately corresponds to 6.3% of the mean, and decreased the probability of marrying natives by 3.6%, which is equivalent to 22% of the mean. The two effects broadly cancel each other out, hence I find no statistically significant effect of settlement houses on the probability of marriage with immigrants from another country. The effects are generally larger for men (panel B) than for women (panel C), and while the difference between the two groups' triple difference coefficients is statistically significant, it is quite small in magnitude.

Column (4) reports the effect of settlement houses on the Foreign Name Index (FNI) of Abramitzky *et al.* (2020).[29] The FNI measures the excess diffusion of each name among non-natives compared to the native population. Increasing values of FNI indicate that the name is increasingly more diffused among non-natives than natives. I assign the FNI to all individuals in the sample with at least one child as the average FNI among their child(ren). I find that immigrants exposed to settlement houses give more foreign-sounding names to their offspring. Quantitatively, exposure to settlements results in a 0.5% increase in the Foreign Name Index. As in the case of in-group marriages, the effect is stronger for men (panel B) than women (panel C), although this difference may indicate men's higher

[29] Formally, I follow Abramitzky *et al.* (2020) and define the Foreign Name Index of name n (FNI_n) as the share of non-natives with name n, normalized by the sum of the share of non-natives with name n and the share of natives with name n. The FNI_n thus ranges between 0 and 1, where 1 indicates that n is only diffused among non-natives, and 0 indicates that no non-native carries name n. I apply the inverse hyperbolic sine transformation to the raw FNI to reduce the influence of outliers.

bargaining power in naming decisions rather than women's preference for less non-native-sounding names.

These results indicate that settlement houses did not foster assimilation but rather further segregation along ethnic lines. In columns (5–6), I look at how language proficiency responded to settlement house activity. As anticipated in the event-study graphs, I find that spoken and written command of English among immigrants increased as they were exposed to settlement houses. Quantitatively, my estimates indicate that the probability of speaking and writing English both approximately increased by 1%. The effect on spoken language ability is significant only among women, whereas I estimate a statistically significant increase in literacy only among men. This difference is consistent with the hypothesis that settlement volunteers provided men with more formal education to pursue more skilled employment opportunities. Finally, in column (7), I explore how the presence of settlements impacted the probability of naturalization. While the aggregate treatment effect is not significant (panel A), naturalization rates increased among women exposed to settlement houses (panel C) and did not significantly move among men (panel B).

Appendix Table B.8 replicates the results employing different layers of fixed effects, controls, and the propensity score weighting scheme. All results remain qualitatively unchanged. Appendix Section A.II provides a detailed discussion of the robustness analysis.

This analysis suggests mixed results of the Settlement movement on immigrants' assimilation and economic success. Settlement houses had notable positive effects on the economic standings of male immigrants but did not produce any improvement for women's participation in the labor market. Language proficiency of immigrants increased in response to settlement houses, but so did in-group marriage probabilities and, plausibly, overall segregation along ethnic lines.

VI MECHANISMS: FAMILY, FERTILITY, AND GENDER NORMS

In this section, I explore the potential mechanisms underlying the heterogeneous responses to settlement houses across genders. First, I study how settlement houses impacted the fertility and family decisions of the immigrants. Second,, I study how those responses vary in terms of the gender norms of the various immigrant groups. Third, I provide a complementary perspective of the functioning of settlement houses as a coordination device for public goods provision in ethnically diverse neighborhoods. I conclude by evaluating the effects of settlement houses on those who spent their childhood in their proximity.

VI.A The Family and Fertility Effects of Settlement Houses

The historical scholarship and my data alike indicate that childcare services—such as kindergartens and nurseries—were a central component of the welfare activities offered by settlement houses. In this section, I explore the marriage and fertility decisions of the immigrants in response to settlement houses. Kindergarten and nursery services offered by social settlements decreased childcare costs, and, as seen in the previous section, settlements increased the probability of men working. These two effects could jointly exert a positive impact on fertility decisions. Working mothers and wives faced substantial stigma, and most women would drop out of the labor force upon marriage (Goldin, 1990, 2006). From this perspective, higher fertility and earlier marriages could thus hamper the women's ability to seek employment and segregate them into housework.[30]

To estimate settlement houses' fertility and marriage impacts, I employ the baseline 1880-1940 individual-level cross-sectional dataset used in sections V.C-V.D. I estimate regression (3) using the probability of having children, the inverse hyperbolic sine of the number of children, the individual's age when they had the first child, and the marriage probability as the outcome variables. The sample is the same as in the previous analysis. I report the results in Table IV. Column (1) refers to the probability of having children: the dependent variable is equal to one if the individual ever had at least one child, zero otherwise. The estimates indicate a small and positive effect of settlement houses on the probability of having at least one child, even though the coefficients on the aggregated sample and on the men-only sub-sample are not significant (panels A and B), unlike in the woman sub-sample (panel C). Quantitatively, the probability of having at least one child increases by 1.7% for women.

In column (2), I report the results using the (IHS) total number of children as the dependent variable. My estimates indicate that immigrants exposed to settlement houses when young had a statistically significantly higher number of children. Quantitatively, exposure to a settlement house results in a 1.5% increase in the number of children (panel A). The effect is larger for women (3.4%, panel C) than for men (1.3%, panel B). In column (4), I report that I find no statistically significant response of marriage rates to settlement activity. This result is not entirely surprising given that the vast majority—over 80%—of both men and women in the sample eventually married.

Panel IVe in Figure IV reports the estimates obtained from equation (4), where I compute the estimated treatment effect of exposure to settlement houses for young *vis-à-vis* old individuals in the event time,

[30] On the one hand, in this period, childcare was almost exclusively the woman's burden. Thus, by providing childcare services, settlement houses could allow women to take up regular jobs. However, as seen in the previous section, women's labor supply *decreased* following the establishment of settlement houses. More generally, existing studies over a similar period do not find large changes in women's labor supply in response to childcare services (Ager and Cinnirella, 2020).

i.e., the periods since the settlement is established. The estimates indicate no statistically significant difference in the number of children between young and old individuals before settlements are established. This pattern provides evidence in support of the parallel trends assumption. By contrast, the number of children increases over the years that follow the establishment of settlement houses. The treatment effect is positive both for men and for women, even though the latter sample displays the largest and most precisely estimated response. In Panel IVf, I complement the event-study estimates by comparing the treatment effects of settlement houses across age bins. Consistent with the rationale of the triple difference estimator, I find that women in younger age bins (between 15 and 35) drive the average treatment effect, whereas older individuals do not display a statistically significant response to settlement presence. For men, I estimate statistically significant responses for slightly older age bins, between 25 and 45. This pattern is in line with the well-known fact that men tended to marry and have children at an older age compared to women.

My results indicate that the effect of settlement houses on fertility was predominantly at the extensive margin. The probability of having a child increased in response to exposure to settlement houses, but the magnitude of the effect is considerably smaller than the positive treatment effect on the number of children. This result is not entirely surprising, given that childless married couples were rare in this period.

Childbearing was a critical factor in shaping the labor supply decisions of women in this period. First, prevailing social norms heavily discouraged women with children from participating in formal labor markets (Goldin, 2006). Second, formal institutions—among others, marriage bars—were designed to either forbid the hiring of married women and women with children or mandate that women be fired upon marrying and having children (Goldin, 2021).[31]

The age of women when their first child was born can be interpreted as a measure of the women's agency within households (e.g., see Field and Ambrus, 2008; Buchmann, Field, Glennerster, Nazneen and Wang, 2023). In column (3), I thus explore how the age at first child reacts in response to the establishment of settlement houses.[32] In column (4), I estimate that the age at first child of immigrants exposed to settlement houses decreases by approximately 0.2 years. The effect for men (panel B) is comparable in magnitude to that of women (panel C).

Appendix Table B.9 reports the estimated effect of exposure to settlement houses on family and fertil-

[31] Marriage bars were common in many occupations and industries, especially teaching. In 1928, approximately 60% of the urban US population—the sample I study—lived in school districts where married women would not be hired, and 48% in districts where a woman would be fired upon marrying (see Figure 4.2 in Goldin, 2021).

[32] It is worth noting that, in this case, the sample excludes individuals with no children.

ity outcomes using several specifications. The treatment effect remains quantitatively stable regardless of the included fixed effects and applying the propensity score matching approach.

The evidence presented thus far conveys a consistent picture of the evolution of immigrant families in response to the establishment of settlement houses. Women exposed to settlement houses had more children and had their first child when they were younger. Throughout this period, marriage and motherhood constituted substantial obstacles to female labor force participation (Goldin, 2006). My evidence thus indicates that the positive effects of settlement houses on (men's) labor opportunities and the childcare services provided by their volunteers reinforced each other's push toward segregating women into household duties.

VI.B Gender Norms of the Immigrants and the Settlement Movement

The results indicate that, by providing male immigrants with better job opportunities and female immigrants with reduced childcare costs, social settlements impressed an upward fertility shift that hampered women's employment possibilities. Implicitly, this interpretation relies on the assumption that immigrant households preferred the former when faced with the decision between more children and increased female labor force participation rates. In this section, I leverage variation in gender norms across the immigrants' countries of origin to test this prediction and study how traditional gender roles shaped the immigrants' response to settlement houses.[33]

I use two measures of male-dominated gender roles in the immigrants' countries of origin. First, I employ the Male-Dominance Index (MDI) developed by Guarnieri and Tur-Prats (2023). The MDI is a synthetic index compiled from gender-equal traits—matrilineality, polygyny, dependence on shifting agriculture, dependence on nonherding animal husbandry, dependence on gathering, and dependence on fishing—and male-dominance traits—plough use, dependence on pastoralism, and nuclear families. I map the ethnicity-level MDI to countries by taking the MDI of the largest group within each country.[34] Second, I measure exclusionary gender roles as the total fertility rate in 1900. This ap-

[33] This analysis provides a natural test of my interpretation of the results, which posits that I should expect to find a negative relationship between exclusionary gender roles and the treatment effect on female employment and a positive one between exclusionary gender roles and fertility outcomes.

[34] In practice, European countries typically have a single ethnicity entry in the Murdock (1967) Atlas. When they have more than one, their MDI is the same. Hence, I could take an average of them, and the country-level results would be unchanged. The only exception with multiple heterogeneous ethnic groups is Russia. However, most groups are small and located in remote areas where transatlantic out-migration was plausibly very low. I thus use the MDI of the "Russians" ethnic group for the entire country. Additionally, I exclude Sweden and Germany from the MDI sample. For Sweden, the only available MDI refers to Lapps, who constitute a small minority of the population. For Germany, the MDI refers to East Prussians, but the bulk of US migrants from Germany originated from the Western regions of Prussia, where key components of culture,

proach follows extensive literature in family economics (Doepke, Hannusch, Kindermann and Tertilt, 2023). I interpret higher fertility rates as associated with more male-dominated gender roles. Historical fertility data have been compiled by the Princeton European fertility project (Coale and Treadway, 1986) and widely used by economists (for a discussion, see Spolaore and Wacziarg, 2022). The countries with available MDI and fertility rates do not completely overlap; hence, using both indices is valuable as it allows me to maximize the coverage rate over the immigrant population in the US.[35]

To explore how gender roles shaped the immigrants' responses to settlement houses, I employ the baseline stacked cross-sectional data. I estimate regression (3) and interact the baseline treatment with country-of-origin indicators:

$$\begin{aligned} y_i = \alpha_{d(i)\times c(i)} + \alpha_{d(i)\times t(i)} + \alpha_{c(i)\times t(i)} + \alpha_{\omega(i)} + X_i'\Gamma + \\ + \sum_o \delta^o \times \left(Settlement_{d(i)} \times Post_{t(i)} \times Young_{c(i)} \right) \times I\left(\omega(i) = o\right) + \varepsilon_i, \end{aligned} \tag{6}$$

where, as in the previous analysis, i denotes an individual observed in census $c(i)$ who, in 1900, lived in district $d(i)$, was born in year $t(i)$. The term X_i collects the same individual-level controls of (4)-(7): race, gender (when applicable), and year of immigration. Term $\alpha_{\omega(i)}$ denotes fixed effects for the immigrant's country of origin. These control for country-of-origin-level differences—due, among others, to different gender roles—in y_i that may otherwise confound the estimates.[36] The only difference relative to model (3) is that I interact the triple-difference term with country of origin dummies ($I\left(\omega(i) = o\right)$). These terms code a set of indicators equal to one for immigrants born in country o and zero otherwise. As in the rest of the paper, standard errors are clustered at the neighborhood level. Since this analysis focuses on women, I exclude men from the estimation sample.

Figure V reports the estimates of $\hat{\delta}^o$ on the y-axis. The x-axis reports, for each country, the MDI (panels Va and Vb) or the total fertility rate in 1900 (panels Vc and Vd). The red line overlays a linear fit for visualization purposes. I focus on two dependent variables: female labor force participation (FLFP, panels Va and Vc) and the (IHS) number of children (panels Vb and Vd).[37] The estimates indicate a robustly negative association between the treatment effect of settlement houses on female labor force participation and both the MDI and total fertility. In other words, female immigrants from relatively more male-dominated cultures were less likely to work when exposed to settlement houses. The aggregate estimate conceals substantial heterogeneity: relatively more gender-equal countries—such

such as religion, were substantially different.

[35] The countries for which either the MDI or the fertility rate is available cover over 95% of the immigrant population in 1930.

[36] Country-of-origin fixed effects are included in all specifications. In (3), they are part of X_i. In (6), however, I include them more explicitly to emphasize that I control for such time-invariant factors.

[37] Appendix table B.10 provides the associated tabular evidence.

as France and the UK—display positive treatment effects, whereas more unequal ones—especially in Eastern Europe—drive the overall negative impact of settlement houses on FLFP.

The association between the country of origin's gender inequality and the treatment effect on the number of children is, on the other hand, positive. Female immigrants from more male-dominated cultures responded to settlement houses by increasing marriage rates. As with FLFP, there is substantial cross-country heterogeneity. Immigrants from relatively more gender-equal countries, especially France, were *less* likely to marry in response to exposure to settlement houses. Immigrants from male-dominated countries, on the other hand, display significantly higher marriage rates in response to settlement houses. These correlations hold across the two measures of exclusionary gender roles.

These patterns provide consistent, if suggestive, evidence that gender norms shaped the response of immigrant households to settlement houses. The welfare services provided by settlement houses resulted in further segregation of women from highly male-dominated cultures, who constituted the majority of the immigrant stock during my study period. More generally, my results highlight that intra-household cultural factors profoundly shape the effects of welfare-providing institutions, thus echoing experimental evidence by Abou Daher *et al.* (2023).

VI.C Ethnic Diversity and Settlement Houses

Throughout this period, immigrants relied heavily on ethnic networks. Recent studies find large effects of ethnic networks on assimilation (Gagliarducci and Tabellini, 2022; Abramitzky *et al.*, 2024, 2025). I conclude this section by asking how settlement houses interacted with the ethnic social networks. Ethnic networks arguably provided more effective "safety nets" within homogeneous communities than fractionalized ones. I thus explore whether settlement houses helped solve a coordination problem by providing assistance to immigrants in more diverse communities.

To measure the degree of diversity of the immigrant communities, I calculate the Hirschman-Herfindahl index (HHI) based on the shares of immigrants by country of origin.[38] There is considerable variation in the degree of concentration of immigrant communities, with HHI varying between 0.12 and 0.84, with an average value of 0.34. Then, I divide neighborhoods into terciles of the HHI distribution and label those above median HHI as "highly diverse."[39] Then, I estimate the baseline regression (3) on the adult sample of immigrants separately on diverse and non-diverse neighborhoods as well

[38] Formally, let s_{od} denote the share of immigrants from country o in district d in 1900. The Hirschman-Herfindahl index is defined as $HHI_d \equiv \sum_o s_{od}^2$. By definition, HHI_d is bounded in $[0,1]$, and it increases in the concentration of the immigrant community, i.e., it is equal to 0 when all immigrants originate from the same country and to 1 when they all come from different countries.

[39] Results remain qualitatively similar if, instead of the top 50% of the HHI distribution, I consider the top 33% or 25%.

as on the joint sample, where I further include an additional interaction term between the baseline treatment and a binary variable equal to one for highly diverse neighborhoods and zero otherwise. The coefficient of this interaction term thus captures the differential effect of settlement houses by the diversity of their immigrant community.

Figure VI reports the results looking at white-collar employment, the number of children, and the probability of speaking English as the outcome variables.[40] The gray bars report the triple difference coefficient on the sub-sample of below-median diversity neighborhoods, whereas the black bars refer to the highly diverse neighborhoods sample. Above each pair of bars, I further report the difference between the two coefficients, measured through the regression on the joint sample, and the associated p-value. The results highlight a consistent pattern across the various dependent variables. Settlements did not significantly impact immigrants' labor market, fertility, and assimilation dynamics in ethnically homogeneous areas. The estimate of the baseline treatment effect is almost always statistically indistinguishable from zero, and the heterogeneous responses hold both in the entire sample and when focusing on men and women separately. Their effects were concentrated in more diverse communities, where sparser ethnic networks were a less efficient substitute for native-provided assistance through settlement houses. These results thus suggest that settlement houses solved a coordination problem that emerged in more diverse immigrant communities. In contrast, they were largely irrelevant in ethnically homogeneous areas, where immigrants plausibly preferred ethnic-based assistance over natives in settlement communities.

VI.D The Intergenerational Consequences of the Settlement Movement

Progressive-era reformers viewed childcare support as central in their efforts to enhance the living conditions of the urban poor and foster assimilation (Lazerson, 1971; Davis, 1984; Berg, 2004). Kindergarten and nursery services and primary schooling classes featured prominently among the activities performed by volunteers in settlement houses. I thus conclude by focusing on individuals who were exposed to settlement houses during childhood and study their later-in-life trajectories.

To answer this question, I employ the baseline stacked cross-sectional dataset, but focus on individuals who were exposed to a settlement house when they were younger than 15. This is thus the complementary sample of the previous analyses. Moreover, I focus on post-settlement census waves (1920–1940) because the vast majority of those aged 0–15 at the time when settlements were estab-

[40] Labor force participation rates were extremely high among men. Hence, white-collar employment provides a more adequate measure of the labor market benefits of settlement houses compared to the sheer employment rate.

lished were not born in preceding censuses. I estimate variations on the following specification:

$$y_i = \alpha_{m(i)\times c(i)} + \alpha_{m(i)\times t(i)} + \alpha_{c(i)\times t(i)} + X_i'\Gamma + \beta \times Settlement_{d(i)} + \varepsilon_i, \tag{7}$$

where i denotes an individual born in year $t(i)$, observed in census $c(i)$, and residing in city $m(i)$. The term X_i collects the same controls as the previous analysis: country of origin, race, gender (when applicable), and immigration year. The main variable of interest, $Settlement_{d(i)}$, is an indicator equal to one if the neighborhood $d(i)$ where i lived in 1900 had a settlement established within 0.250 Km of its centroid—the baseline treatment definition—the sample period and zero otherwise. Specification (7) mirrors (3) but substitutes districts with cities in the fixed effects because the treatment varies at the district level. As in the rest of the empirical analysis, standard errors are clustered at the city level.

In (7), I compare immigrants who grew up in neighborhoods close to at least one settlement house in 1900 with immigrants who did not have access to settlement houses. For $\hat{\beta}$ to have a causal interpretation, I thus need to assume that no omitted factor correlates with the presence of settlements and the outcomes. Since I cannot leverage cross-cohort comparisons to assess the plausibility of this assumption as in (3), I do not claim that my estimates convey a causal interpretation. However, all results remain remarkably stable upon including increasingly demanding city- and individual-level controls, hence substantially restricting the space of remaining potential confounding factors.

With this caveat in mind, Table V reports the results on labor force participation (column 1), occupational income score (column 2), fertility choices, namely, the probability of having children (column 3) and the (IHS) number of children (column 4), and marriage dynamics, namely, the probability of marriage (column 5), and the probability of marrying a foreign-born from the same country (column 6), a foreign born from another country (column 7), and a native-born (column 8). Appendix Table B.11 reports the estimates obtained using alternative regression specifications. I do not find strong evidence of differential labor force participation rates among individuals who grew up near settlement houses in the aggregate sample (column 1). However, when breaking down the effect by gender, I uncover substantial heterogeneity. Labor force participation among men increased, albeit modestly (0.3%), while it decreased among women (-1.6%). Labor income analogously increased by 1.5% among men (column 2, panel B) and decreased among women (column 2, panel C) by almost 5%. In columns (3–4), I explore the intergenerational effects of settlement exposure on completed fertility. The results align with the baseline effects on adults. Exposure to settlement houses increased the probability of having children (1.1%) and the number of children (2.2%). Both these effects are larger for women, as the number of children increases by 3.2% (panel C), than for men (1.5%).

In columns (5–8), I explore the association between exposure to settlement houses during childhood

and the likelihood of marrying foreign-born or native-born individuals. First, in column (5), I show that marriage rates among women increased by approximately 2%. This shift may reflect lower rates of labor force participation at a time when there was considerable stigma attached to married women working (.e.g. Goldin, 1980). Individuals exposed to settlement houses were not differentially more likely to marry immigrants from their same origin country (column 6), but were more likely to marry foreign-born individuals from other countries (column 7). The aggregate increase (1.1%) is primarily driven by women (2.3%). These patterns are consistent with the historically plausible hypothesis that settlement houses created immigrant communities that transcended ethnic boundaries. It would then be more likely for individuals who grew up in those diverse communities to marry immigrants from other countries rather than with people originating from their own country and with natives. From this perspective, settlement houses created more integrated communities of immigrants but hampered contact between those communities and the native population, thus plausibly delaying assimilation. Consistent with this hypothesis, in column (8), I find that the probability of marrying natives decreased by approximately 1%.

The intergenerational results indicate that the gender disparities generated by the presence of settlement houses among adults—seen in sections V.C-V.D—trickled down onto their children. The wedge between men and women in the labor market, measured through labor force participation rates and occupational income, widened as the positive effects of settlement houses accrued solely to men and differential fertility decisions persisted into the later generation. My results align with previous literature studying the intergenerational persistence of norms (e.g., Bisin and Verdier, 2001, 2023) and, especially, gender roles (e.g., Fernández *et al.*, 2004; Alesina, Giuliano and Nunn, 2013).

VII CONCLUSIONS

An extensive literature views social capital as conducive to a cohesive, well-functioning democracy (e.g., Putnam, 2000). Over the past decades, immigration has posed significant political and social challenges, fueling the rise of populist movements worldwide (Guriev and Papaioannou, 2022). Existing studies typically focus on the nativist backlash triggered by immigration. This paper complements this literature by examining how bottom-up social movements emerging in response to immigration influence the economic and cultural assimilation of immigrants.

I study the Settlement movement, a characteristic feature of the Progressive era. Using newly digitized data, I first document that settlement houses in urban centers emerged in response to high immigration, especially from Southern and Eastern European countries, and poverty.

I then explore the settlements' impact on immigrants through detailed individual-level data. Expo-

sure to settlement houses positively affected immigrants' labor force participation and income. These effects, however, are entirely driven by men. Women do not exhibit a significant response to settlement activity. In terms of cultural assimilation, settlement houses fostered basic human capital, such as spoken and written command of English, but increased in-group marriages among immigrants, thus plausibly exacerbating segregation along ethnic lines. These effects spilled over to the next generation. Those who grew up near settlement houses were more likely to work and earn more, but men entirely drive these effects. However, they also display higher fertility and are less likely to marry native citizens.

To rationalize these results, I explore the effects of settlement houses on family and fertility decisions. Immigrants exposed to settlement houses had more children and were more likely to marry at a younger age. In this period, women with children faced considerable stigma when participating in labor markets (Goldin, 2006). Hence, my results indicate that exclusionary gender roles translated increased male income into higher segregation of women into housework. Consistent with this interpretation, immigrants from countries with more male-dominated cultures drive the segregating effects of settlement houses on women.

From a policy perspective, this paper provides one finding and one suggestion. On the one hand, my results indicate that community-driven grassroots associations can substantially and positively impact disadvantaged groups. On the other hand, they highlight the interplay between social capital and out-group culture. The identity and cultural values of the immigrants shape how they react to external inputs, thus fundamentally affecting the effectiveness of bottom-up immigrant assistance programs.

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TABLES

Table I. Population Dynamics After the Establishment of Settlements

	Number of Immigrants	Share of Immigrants	D.V. Normalized by Number of Immigrants			
	(1)	(2)	(3) In Regular Employment	(4) Imputed Income	(5) White Collar Workers	(6) Blue Collar Workers
Panel A. All Immigrants						
Post Establishment of Settlement	3545.244***	0.131***	-0.098***	-2.349***	-0.112***	0.077***
	(951.321)	(0.032)	(0.022)	(0.408)	(0.022)	(0.018)
Mean Dep. Var.	0.074	0.074	0.074	0.074	0.074	0.074
Panel B. Men						
Post Establishment of Settlement	2049.745***	0.136***	-0.218***	-5.929***	-0.195***	0.085***
	(499.235)	(0.032)	(0.033)	(0.592)	(0.028)	(0.026)
Mean Dep. Var.	0.074	0.074	0.074	0.074	0.074	0.074
Panel C. Women						
Post Establishment of Settlement	1495.500***	0.121***	-0.021	-0.031	-0.054**	0.039**
	(456.105)	(0.033)	(0.025)	(0.400)	(0.022)	(0.017)
Mean Dep. Var.	0.074	0.074	0.074	0.074	0.074	0.074
Hexagon FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Hexagons	11,246	11,246	11,246	11,246	11,246	11,246
Number of Observations	67,476	67,476	67,476	67,476	67,476	67,476

Notes. This table reports the relationship between the establishment of settlement houses and a set of demographic and labor-market variables. The unit of observation is a hexagon at a (census) decade frequency between 1880 and 1940, except 1890. Panel A refers to the entire immigrant population; panels B and C refer to men and women immigrants, respectively. The treatment variable equals one after a settlement is established in the hexagon and zero otherwise. The dependent variable is the number of immigrants (column 1), the share of immigrants (column 2), the share of immigrants in the labor force (column 3), the occupation-based imputed income per migrant (column 4), and the share of immigrants in white (column 5) and blue (column 6) collar manufacturing occupations. Hexagons are weighted by population. All regressions include hexagon and city-by-decade fixed effects. Standard errors are reported in parentheses and are clustered at the hexagon level. Referenced on page(s) 18. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table II. The Labor Market Effects of Settlement Houses

	Labor Force Participation			Occupational Income
	(1) Overall	(2) White Collar	(3) Blue Collar	(4)
Panel A. Entire Population				
Post × Settlement × Young	0.006*	0.010***	-0.007*	0.021*
	(0.003)	(0.003)	(0.004)	(0.012)
Number of Individuals	5,280,290	5,280,290	5,280,290	5,280,290
Mean Dep. Var.	0.564	0.080	0.251	2.148
Panel B. Men				
Post × Settlement × Young	0.010***	0.015***	-0.009	0.042***
	(0.004)	(0.005)	(0.006)	(0.015)
Number of Individuals	2,737,015	2,737,015	2,737,015	2,737,015
Mean Dep. Var.	0.898	0.138	0.427	3.529
Panel C. Women				
Post × Settlement × Young	-0.002	0.001	-0.003	-0.011
	(0.005)	(0.002)	(0.004)	(0.017)
Number of Individuals	2,483,305	2,483,305	2,483,305	2,483,305
Mean Dep. Var.	0.196	0.017	0.057	0.626
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes

Notes. This table reports the effect of settlements on labor-market variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In panel A, the sample comprises all individuals between 15 and 65 years old; in panels B and C, the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual works (column 1), if they work in a white-collar occupation (column 2), or if they work in a blue-collar occupation (column 3), and the inverse hyperbolic sine of the occupation-based imputed income (column 4). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 22. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table III. The Assimilation Effects of Settlement Houses

	Married with...			Foreign Name Index	Language		Naturalization
	(1) Foreign-Born (Same Country)	(2) Foreign-Born (Other Country)	(3) Native-Born	(4)	(5) Spoken	(6) Written	(7)
Panel A. Entire Population							
Post × Settlement × Young	0.039***	-0.001	-0.036***	0.004***	0.010**	0.008**	-0.002
	(0.004)	(0.003)	(0.003)	(0.001)	(0.005)	(0.004)	(0.005)
Number of Individuals	5,280,290	4,351,170	4,351,170	4,193,535	4,739,460	5,263,039	4,861,678
Mean Dep. Var.	0.618	0.086	0.166	0.560	0.887	0.907	0.445
Panel B. Men							
Post × Settlement × Young	0.046***	0.000	-0.043***	0.005***	0.008	0.010**	-0.012
	(0.005)	(0.003)	(0.004)	(0.002)	(0.006)	(0.005)	(0.008)
Number of Individuals	2,737,015	2,349,289	2,349,289	2,146,431	2,451,144	2,726,653	2,526,989
Mean Dep. Var.	0.620	0.081	0.200	0.562	0.900	0.924	0.692
Panel C. Women							
Post × Settlement × Young	0.031***	-0.005	-0.026***	0.003*	0.015**	0.007	0.014***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.006)	(0.005)	(0.005)
Number of Individuals	2,483,305	1,936,827	1,936,827	1,981,222	2,233,480	2,476,269	2,279,371
Mean Dep. Var.	0.616	0.093	0.126	0.558	0.871	0.889	0.175
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effect of settlements on cultural assimilation variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In panel A, the sample comprises all individuals between 15 and 65 years old; in panels B and C, the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. In columns (1–3), the dependent variable is one if individuals are married with immigrants from the same country (column 1), immigrants from other countries (column 2), or natives (column 3). In column (4), the dependent variable is the foreign name index. In columns (5) and (6), the outcome is one for individuals who can speak (column 5) or both speak and write (column 6) in English. In column (7), the dependent variable is one if the individual is a citizen. All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 24. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table IV. The Family and Fertility Effects of Settlement Houses

	Fertility		Age When First Child	Married
	(1) Has Children	(2) Number of Children	(3)	(4)
Panel A. Entire Population				
Post × Settlement × Young	0.005	0.015**	-0.189***	-0.003
	(0.004)	(0.008)	(0.052)	(0.004)
Number of Individuals	5,280,290	5,280,290	3,990,768	5,280,290
Mean Dep. Var.	0.787	1.495	26.038	0.805
Panel B. Men				
Post × Settlement × Young	0.003	0.013	-0.177***	-0.010*
	(0.004)	(0.009)	(0.066)	(0.005)
Number of Individuals	2,737,015	2,737,015	2,090,249	2,737,015
Mean Dep. Var.	0.792	1.498	27.626	0.788
Panel C. Women				
Post × Settlement × Young	0.014***	0.034***	-0.192***	0.002
	(0.004)	(0.010)	(0.065)	(0.004)
Number of Individuals	2,483,305	2,483,305	1,849,658	2,483,305
Mean Dep. Var.	0.785	1.500	24.139	0.826
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes

Notes. This table reports the effect of settlements on family and fertility variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In panel A, the sample comprises all individuals between 15 and 65 years old; in panels B and C, the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual has at least one child (column 1), the inverse hyperbolic sin of the number of children (column 2), the age when they have their first child (column 3), and indicator equal to one if they are married (column 4). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 27. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table V. The Intergenerational Effects of Settlement Houses

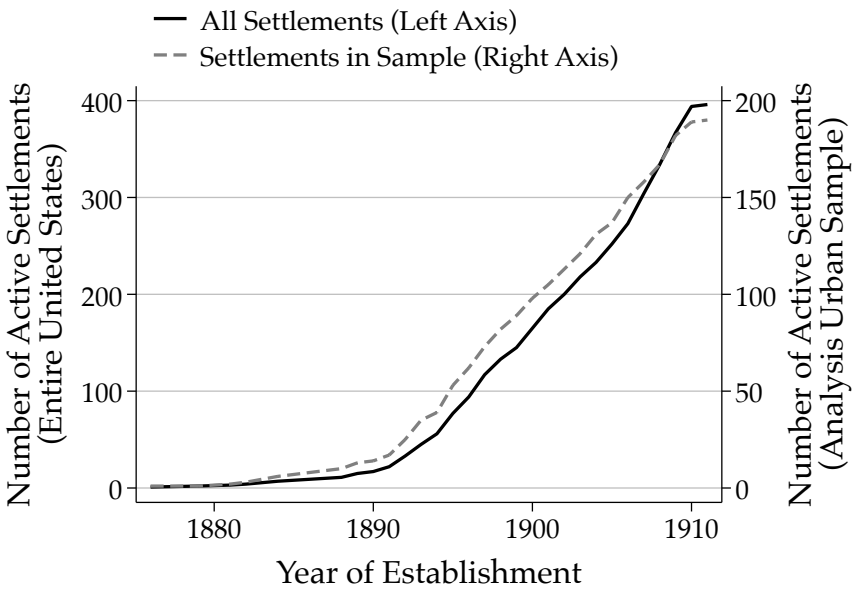
	Labor Force Participation	Occupational Income	Fertility		Married with...			
	(1)	(2)	(3) Has Children	(4) Number of Children	(5) Anybody	(6) Foreign-Born (Same Country)	(7) Foreign-Born (Other Country)	(8) Native-Born
Panel A. Entire Population								
Settlement	-0.003	-0.007	0.011***	0.022***	0.009***	0.002	0.011***	-0.008**
	(0.002)	(0.009)	(0.003)	(0.008)	(0.003)	(0.004)	(0.003)	(0.004)
Number of Individuals	274,174	274,174	274,174	274,174	274,174	274,174	225,878	225,878
Mean Dep. Var.	0.719	2.849	0.779	1.346	0.819	0.449	0.108	0.348
Panel B. Men								
Settlement	0.003**	0.015**	0.009**	0.015*	0.004	0.007	0.007*	-0.010**
	(0.002)	(0.008)	(0.004)	(0.009)	(0.003)	(0.005)	(0.004)	(0.005)
Number of Individuals	184,801	184,801	184,801	184,801	184,801	184,801	161,910	161,910
Mean Dep. Var.	0.931	3.743	0.800	1.363	0.846	0.469	0.105	0.360
Panel C. Women								
Settlement	-0.016**	-0.048**	0.012**	0.032**	0.019***	-0.014**	0.023***	-0.004
	(0.006)	(0.023)	(0.006)	(0.014)	(0.006)	(0.007)	(0.007)	(0.007)
Number of Individuals	89,364	89,364	89,364	89,364	89,364	89,364	63,958	63,958
Mean Dep. Var.	0.281	1.000	0.734	1.310	0.763	0.407	0.113	0.317
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effect of childhood exposure to settlements. The unit of observation is an individual immigrant observed in one census between 1920 (i.e., after the last settlement house is established) and 1940. In panel A, the sample comprises all individuals between 15 and 65 years old; in panels B and C, the samples exclude women and men, respectively. Regressions further exclude individuals above 15 years old when the first settlement house is established in their neighborhood. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. The dependent variable is: one if the individual works (column 1) and the inverse hyperbolic sine of the occupational income score (column 2), an indicator equal to one if they have children (column 3) and the inverse hyperbolic sine of the number of children (column 4), and an indicator if they are married (column 5), married with an immigrant from their same country (column 6), married with an immigrant from a different country (column 7), and married with a native (column 8). All regressions include city-by-census wave, city-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 33. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

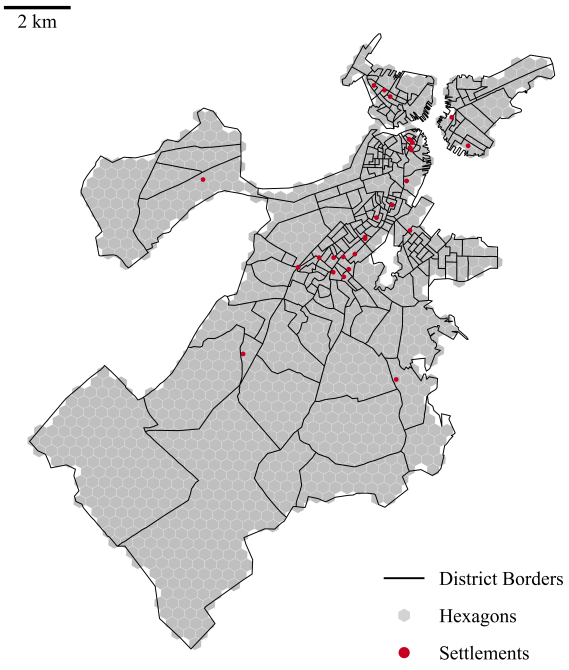
FIGURES

Figure I. Settlements over Time and Across Space

(a) Number of Settlements by Year of Establishment

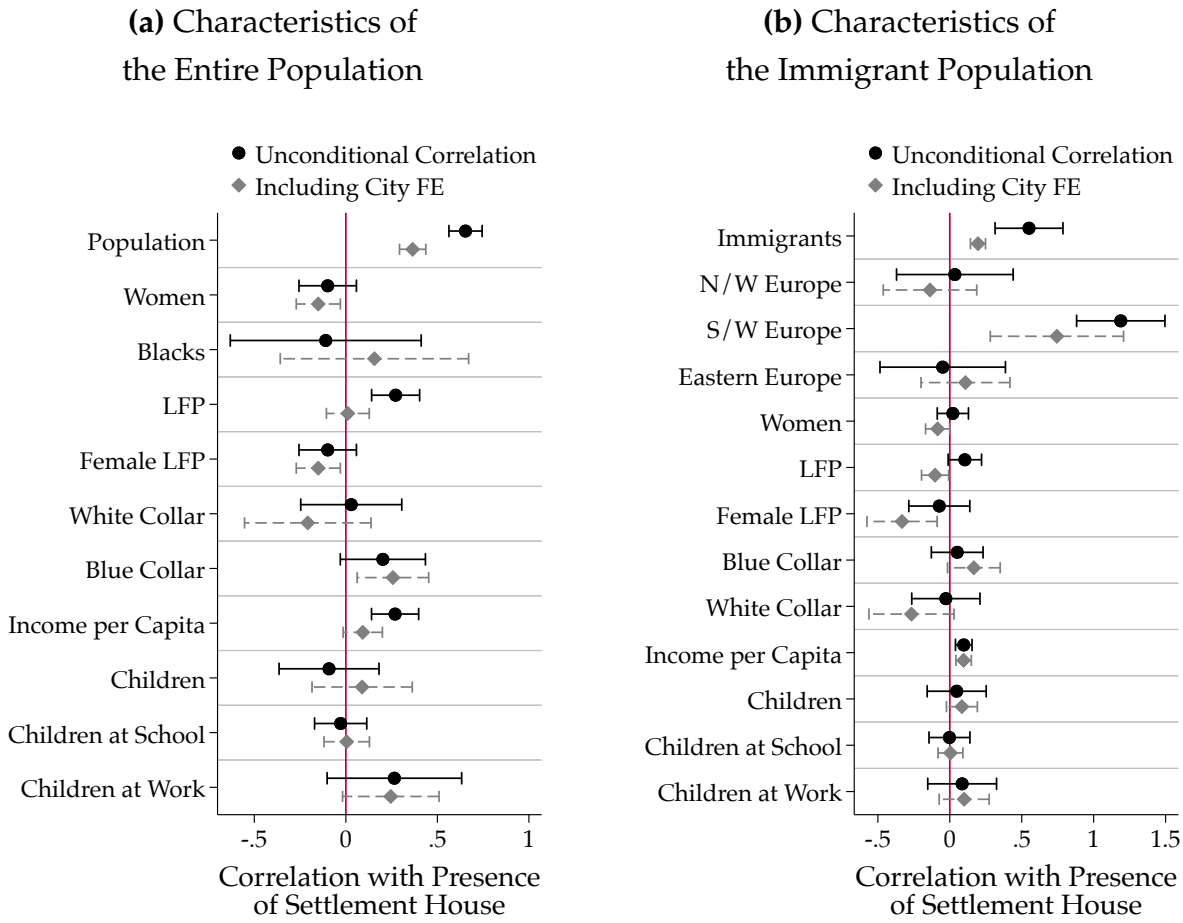


(b) Distribution of Settlements in Sample City (Boston)



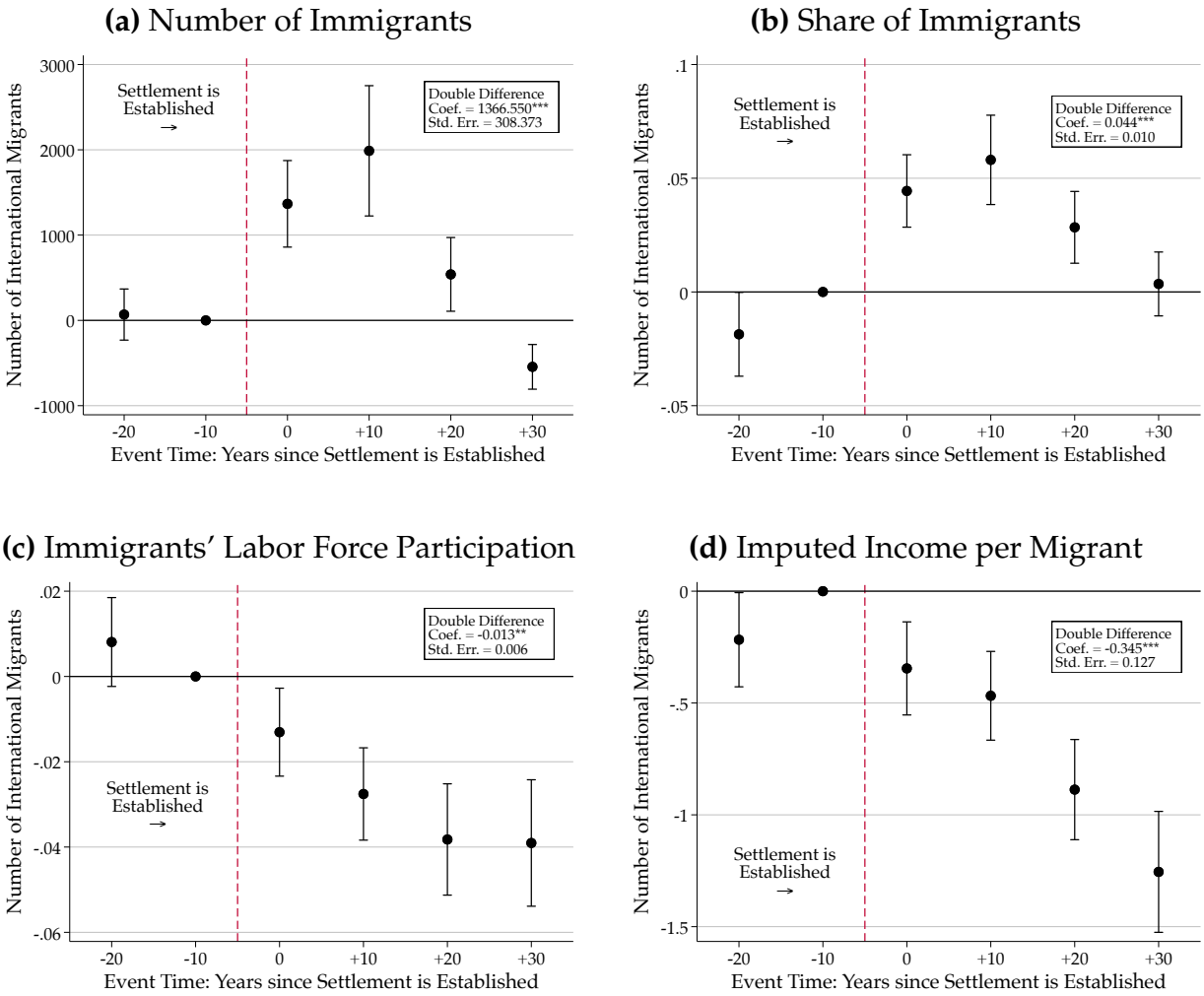
Notes. Panel Ia reports the total number of settlements between 1882 and 1911 in the United States (solid black line, left axis) and in the analysis sample (dashed grey line, right axis). Panel Ib plots the spatial distribution of settlements (red dots) in Boston. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 10.

Figure II. Presence of Settlements and Demographic Characteristics in 1880



Notes. This figure reports the correlation between the presence of a settlement between 1882 and 1911 and hexagon-level demographic characteristics in 1880. Each dot reports the correlation between one variable and a binary indicator, which equals one for hexagons with at least one active settlement. Black dots report unconditional correlations; gray dots report the correlation net of city fixed effects. Hexagons are weighted by population. In panel IIa, the variables are constructed over the entire population and expressed as population shares, except for the first row. In panel IIb, the variables are constructed over the immigrant population and are normalized by the number of immigrants, except for the first row. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 15, A5, C27, C27.

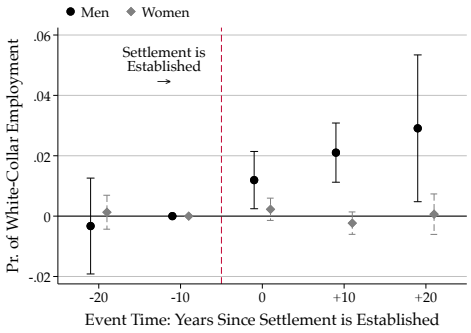
Figure III. Presence of Settlements and Hexagon-Level Population Dynamics



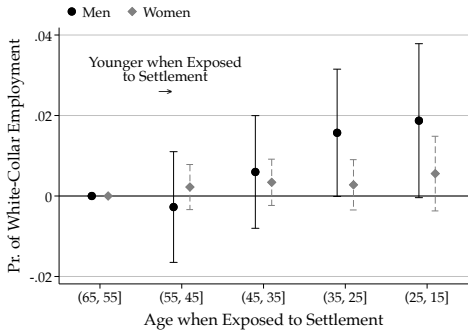
Notes. This figure reports the relationship between the establishment of social settlements and a set of demographic and labor-market variables. The unit of observation is a hexagon at a (census) decade frequency between 1880 and 1940, except 1890. The dots report the coefficients of a set of dummy variables coding the number of years since the first settlement was established in the hexagon, if any. The dependent variable is the number of immigrants (panel IIIa), the share of immigrants (panel IIIb), the share of immigrants in the labor force (panel IIIc), and the occupation-based imputed income per migrant (panel IIId). All regressions include district and city-by-year fixed effects. Hexagons are weighted by population. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 17.

Figure IV. Individual-Level Effects of Settlements

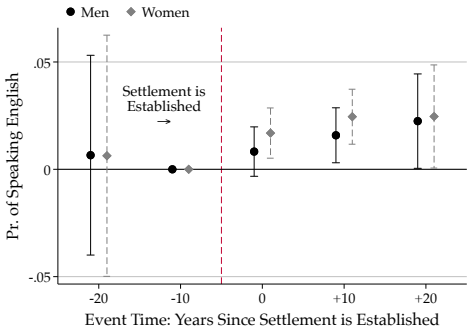
(a) Labor Force Participation, Cross-Census Comparison



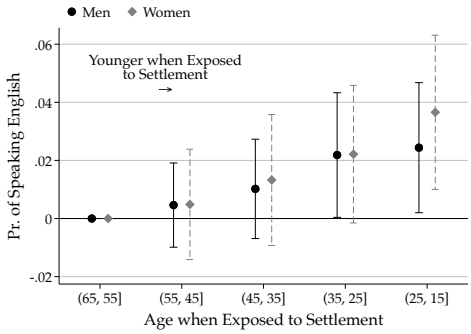
(b) Labor Force Participation, Cross-Cohort Comparison



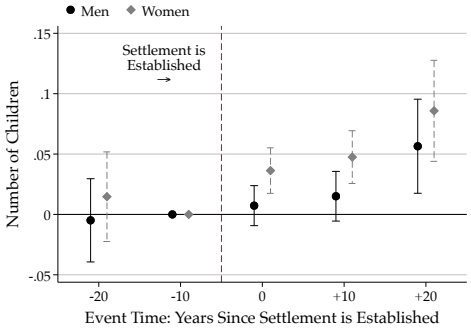
(c) Probability of Speaking English, Cross-Census Comparison



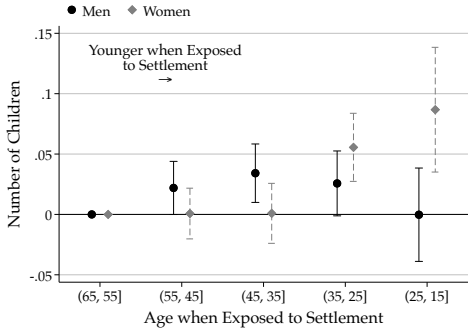
(d) Probability of Speaking English, Cross-Cohort Comparison



(e) (IHS) Number of Children, Cross-Census Comparison



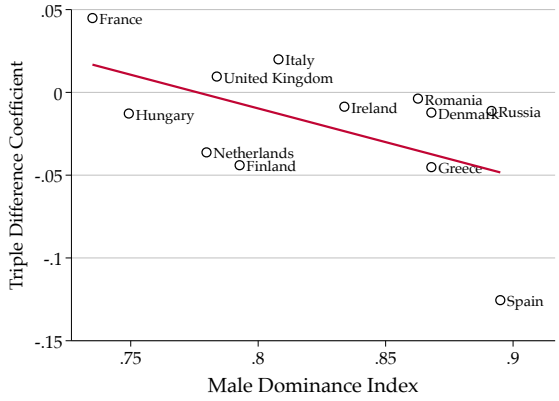
(f) (IHS) Number of Children, Cross-Cohort Comparison



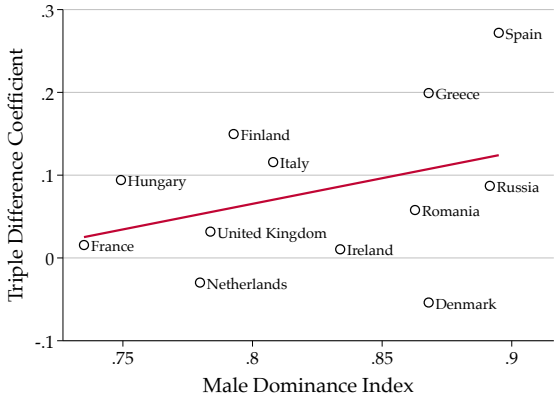
Notes. This figure reports the effect of settlements on labor force participation (IVa–IVb), the probability of speaking English (IVc–IVd), and the (IHS) number of children (IVe–IVf) and the probability of marriage (IVf). The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In panel A, the sample comprises all individuals between 15 and 65 years old; in panels B and C, the samples exclude women and men, respectively. In panels IVa, IVc, and IVe, the dots report the coefficients associated with regression (4); in panels IVb, IVd, and IVf, the dots report the coefficients associated with regression (5). Black dots refer to men; gray dots refer to women. All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bars report 90% confidence intervals. Referenced on page(s) 22, 24, 27.

Figure V. Heterogeneous Responses to Settlements by Origin Country's Gender Norms

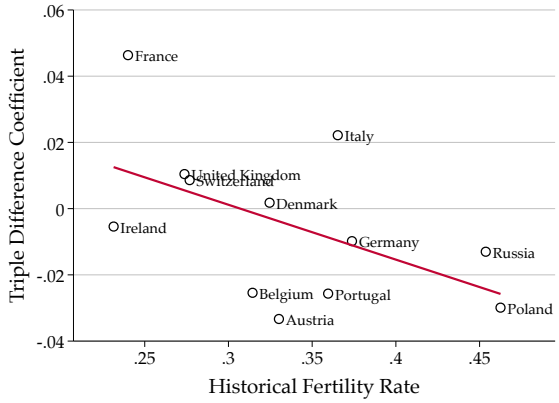
(a) Male Dominance Index and Labor Force Participation



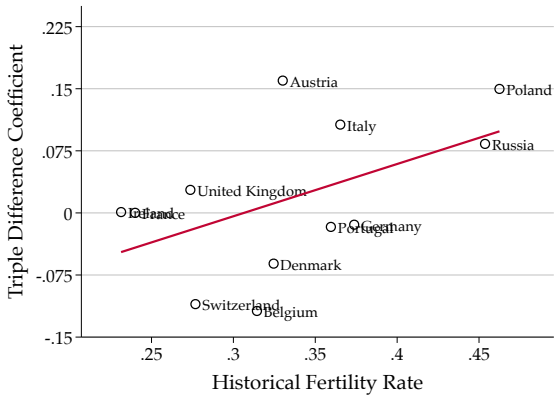
(b) Male Dominance Index and Number of Children



(c) Total Fertility Rate and Labor Force Participation

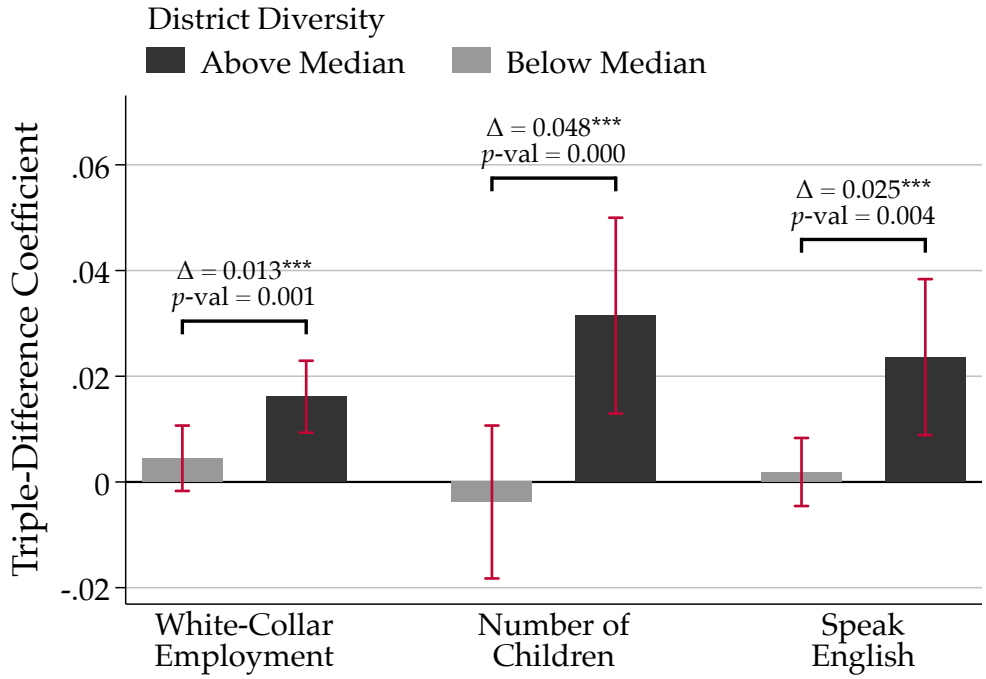


(d) Total Fertility Rate and Number of Children



Notes. This figure reports the response to the establishment of settlements by the country of origin of the immigrants. In all panels, the unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all women aged between 15 and 65. The dependent variable is labor force participation (panels Va and Vc) and the inverse hyperbolic sine of the number of children (panels Vb and Vd). On the y-axis, each dot reports the coefficient associated with an interaction term between the baseline triple-differences treatment (i.e., an interaction between (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established) and country-of-origin indicators. All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. The x-axis reports two indices of male-dominant gender norms of origin countries: the Male Dominance Index (panels Va–Vb) of Guarnieri and Tur-Prats (2023), and the total fertility rate in 1900 (panels Vc–Vd) of Coale and Treadway (1986). Referenced on page(s) 30.

Figure VI. Heterogeneous Responses to Settlements by Diversity of Neighborhood



Notes. This figure reports the heterogeneous effect of settlements in terms of the diversity of the immigrant community. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The gray bars report the estimated treatment effect on the sub-sample of neighborhoods below the median diversity of the immigrant community; the black bars report the estimated treatment effect on the sub-sample of neighborhoods above the median diversity of the immigrant community. The diversity of the immigrant community is measured in terms of the Hirschman-Herfindahl index of the population of immigrants by country of origin in each neighborhood in 1900. The dependent variables are white-collar employment, the inverse hyperbolic sine of the number of children, and the probability of speaking English. The figure annotates the difference between the estimated treatment effects in above- and below-median diversity neighborhoods and the p-value of a test for the statistical significance of the difference between the two. All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 32.

ONLINE APPENDIX

Welcoming the Tired and Poor: Grassroots Associations and Immigrant Assimilation During the Age of Mass Migration

Davide M. Coluccia

June, 2025

TABLE OF CONTENTS

A DATA APPENDIX AND ROBUSTNESS

This appendix provides details on the primary and secondary data used in the paper, the sample construction methodology, and the robustness analyses reported in Appendix sections B and C.

A.I Further Details on the Data

A.I.1 Handbook of Settlements

The *Handbook of Social Settlements* provides a comprehensive snapshot of settlement houses in 1911. It contains information on 411 active and extinct settlements. In the individual-level analysis, I consider individuals as treated if, in 1900, they lived in proximity to a settlement. Since all extinct settlements closed after 1900, I include them in the sample. The *Handbook* also contains information on federations of settlements, such as the Boston Social Union. Federations coordinated settlements but did not provide services to the immigrants and seldom had a physical address different from one of their constituents. I thus exclude them from the sample.

The *Handbook* contains detailed information on the name of each settlement, the date it was established, the address—up to the street name and number in large cities, such as all those in the estimation sample—the number of volunteers and residents, typically split by gender, the activities they carried out, the religious denomination, the group it targeted, and the superintendent(s). When settlements change address, I assign them to the first address where they are observed. I georeference the addresses using Google Maps. I can assign the year of establishment, longitude, and latitude to 372 settlements.

Activities are reported for all settlements in the *Handbook*. I code them into eight categories using the textual description in the source: nursery, kindergarten, young adult education, professional education, recreational classes, health services, assimilation classes, and financial relief services. A small number of activities performed by a single settlement does not fall within this taxonomy, and I exclude them from the analysis. Importantly, however, most settlements performed multiple activities, so it is not feasible to disentangle the effect of each activity on the immigrants. In table B.3, however, I provide a breakdown of settlements by their activities.

A.I.2 Census Data

I use federal census data provided by Ruggles *et al.* (2024) to construct hexagon-level and individual-level variables. In this section, I describe how I construct the variables used in the analysis.

I define an individual as employed if they report an occupational response in the census. I define

as a white-collar occuapation the job titles labeled by IPUMS as "Professional, Technical" (OCC1950 between 0 and 99) and "Managers, Officials, and Proprietors" (OCC1950 between 200 and 299). I classify blue-collar occupations the job titles listed as "Craftsmen" (OCC1950 between 500 and 590), "Operatives" (OCC1950 between 600 and 690), "Farm Laborers" (OCC1950 between 810 and 840), and "Laborers" (OCC1950 between 910 and 970).

Measuring parenthood from the census is challenging because the records report the number of children living in each household (NCHILD) *at the time of the census*. The individual-level analysis leverages a sample of individuals linked between the 1900 and the 1880–1940 censuses. Hence, it is straightforward to check whether the individuals had children residing in their household in each year. However, this approach would not identify those who had children between those years and whose children left the household before the census was taken.[1] To circumvent this issue, I exploit the linked data produced by Price *et al.* (2021). Specifically, I link each census in years 1880–1940 to each other, so that I observe whether each individual who appears in a given year has children in each other census year. This approach allows me to identify parents—namely, those with at least one child in any census year—and pair them with the number of children, which I define as the largest number of children living in their household in a given census year. Since I only need to be able to observe parenthood in a single year (rather than in all of them), the attrition rate introduced by the linking algorithm is a minor concern for this approach.[2]

Literacy is the ability to write and read in English (LIT equal to 4). In 1940, literacy was not recorded; hence, I consider any individual who attended at least grade 5 as literate. Conversely, the English-speaking variable pertains to the oral command of the language. The naturalization variable equals one for foreign-born individuals who are naturalized or have received the papers to complete naturalization (CITIZEN equal to 2 or 4). Spoken English proficiency was recorded between 1900 and 1930; naturalizations were recorded between 1900 and 1940.

I define children as all those aged 15 or less. To identify mothers and fathers, I leverage information on the household composition. Specifically, I construct an indicator for kids (RELATE equal to 3). Then, within each household, the father and the mother are the household head (RELATE equal to 1) and his spouse (RELATE equal to 2) or the head, where the father is absent.

To construct the foreign-name index (FNI), I use confidential data from the 1900, 1910, 1920, and 1930

[1] This challenge does not arise when measuring marriages, because the census reports whether a person has *ever* been married, regardless of whether they are still married at the time of the census.

[2] The results do not change if I only measure fertility using the contemporaneous census, but it is likely that this naïve approach heavily underestimates fertility.

federal censuses provided by IPUMS and follow the approach proposed by Abramitzky *et al.* (2020). These contain information on the first name of each individual. For each census, I keep individuals born over the preceding decade (for example, I extract the records of those born between 1900 and 1909 from the 1910 census). Then, I keep children born to native and foreign-born parents. For each name that appears at least 100 times, I compute the number of native-born and foreign-born children with that name. The FNI of name n is then defined as the share of foreign-born children with name n normalized by the sum of the share of native-born and foreign-born children with name n.

A.I.3 Construction of the Datasets

To construct the hexagon-level panel, I partition each city into equal-sized hexagons using the tessellation algorithm implemented in the `h3pandas` package in `Python`. I apply the algorithm to the enumeration district `GIS` files developed by Shertzer *et al.* (2016), which trace the evolution of enumeration districts in eleven US cities from 1880 to 1940. Then, I construct geographical crosswalks between the enumeration districts and the hexagons. Specifically, let w_{dh} be the share of hexagon h's area that overlaps with district d, and let $\mathcal{D}_h$ denote the set of districts with a non-empty intersection with h.[3] District-level variable x_d, measured from the population census, maps into hexagon h as $x_h = \sum_{d \in \mathcal{D}_h} w_{dh} x_d$. This approach allows me to construct geographically consistent units that I can follow from 1880 to 1940 in a balanced decade-level panel.

The individual-level cross-sectional dataset stacks census waves between 1880 and 1940. I linked the individuals between each census and 1900 to be able to observe where they resided when settlement houses were being established. In the baseline analysis, an individual is exposed to a settlement if they lived within 0.250 meters of a settlement in 1900. The estimation sample comprises all individuals of working age (i.e., between 15 and 65). In all exercises but the intergenerational analyses, individuals are considered exposed to a settlement if they were younger than 40 when they were first exposed to a settlement, and I exclude those younger than 15. In the intergenerational analyses, I focus on those younger than 15 when they were first exposed to a settlement.

A.II Summary of the Robustness Analyses

A.II.1 Hexagon-Level Analysis

Table B.4 provides a tabular display of the comparisons between hexagons with and without settlement houses. In each line, I report the correlation between a variable measured in 1880 and an

[3] The weights w_{dh} are obtained by overlaying the enumeration districts' GIS files with the hexagon tessellation and computing the share of overlapping areas.

indicator equal to one for hexagons with a settlement over the study period and zero otherwise. I include city-fixed effects in columns (3–4) and (7–8) to compare hexagons in the same city. In columns (5–8), I control for population because, as shown in the first row of panel A, there is a strong association between population and settlement presence. Panel A refers to the entire population, whereas in panel B, each variable is constructed by taking the share relative to the immigrant population.

Table B.5 reports evidence on the dynamics of family and fertility decisions of the immigrants in hexagons with and without a settlement. The dependent variable is the number of foreign-born mothers (column 1), married women (column 3), and children (column 2), divided by the number of foreign-born women. In columns (4) and (5), the dependent variable is the number of foreign-born children at school and work, respectively, relative to the total number of foreign-born children. The estimates are obtained from the baseline hexagon-level regression described in the main text using the estimator developed by de Chaisemartin and d'Haultfœuille (2024). The number of children per woman increased in hexagons with settlements, as did the share of children attending school. The other variables do not display statistically significant changes after the establishment of a settlement.

Table B.6 reports the association between settlement houses and the size and share of the native population. In the main text, my main focus is on immigrants. The estimates provided in the table indicate that the number of natives (column 1), native men (column 3), and native women (column 5) decreased after settlements were established. Because the number of immigrants increases, as shown in the main text, the share of natives within the population decreased by approximately 13 % (columns 2, 4, and 6).

In figure C.2, I report the correlation between the number of settlement houses established in the hexagons (y-axis) and the immigrant share, expressed in percentage share (x-axis). To compare hexagons in the same city, the graph partials out city fixed effects. The figure reports the regression coefficient and the associated standard error—clustered at the city level—and the R^2. The figure indicates a positive and statistically significant association between the number of settlement houses established over 1892–1911 and the immigrant share in 1880.

In figure C.3, I report the correlation between baseline neighborhood characteristics measured in 1880 and the presence of settlement houses. This figure thus mirrors II, except that I include all predictors of settlement presence into a single regression, and plot the resulting coefficients. I apply the LASSO penalized logit regression to select which variables to include in the regression. In panel C.3a, the regression does not control for city fixed effects; panel C.3b reports the estimates including city fixed effects. In both cases, population is a strong predictor of the presence of settlements. The LASSO method always selects the immigrant share and the share of immigrants from southern European

countries, and their coefficients are the second- and third-largest. This pattern confirms the baseline insight highlighted in the main text: population and immigration are the three most relevant predictors of the presence of settlements. By contrast, all other variables are dropped by the LASSO or have a small and insignificant association with settlement presence. These results confirm that immigration is the most relevant factor determining the emergence of settlement houses.

A.II.2 Individual-Level Analysis

Moving to the individual-level analysis of the effects of settlement houses on immigrants, Table B.10 reports how the labor-market effects of settlement houses vary in terms of the degree of gender conservatism of the immigrants' countries of origin. As in the main text, I use the Male Dominance Index (MDI) of Guarnieri and Tur-Prats (2023) and the total fertility rate in 1900 of Coale and Treadway (1986) as indicators of conservative gender norms across European countries. The MDI is non-missing for 85% of women in the sample, whereas countries with available total fertility rate estimates cover 90% of the female population.

In the table, I report the baseline treatment, the MDI (columns 1–2), the total fertility rate (columns 3–4), and an interaction term between the two.[4] Female immigrants from countries characterized by more traditional gender roles were less likely to work (column 3), although, interestingly, this pattern does not hold when using the MDI and have more children, as shown in columns (2) and (4). This association is consistent with the idea that conservative gender roles are historically associated with women's exclusion from the labor market. Settlement houses, however, exacerbate these differences. Immigrant women who were young when they were exposed to settlement houses were increasingly less likely to work and more likely to have more children. This pattern corroborates the evidence provided in the main text. It indicates that settlement houses excluded foreign-born women from labor markets, particularly among immigrants from countries with more conservative gender norms.

Tables B.7, B.8, B.11, and B.9 report a set of sensitivity analyses for the individual-level effects of settlement houses on labor market, assimilation, intergenerational, and family and fertility outcomes shown in the main text. The structure of the tables is the same. In columns (1–4), the sample comprises the entire population; columns (5–6) and (7–8) focus on the male and female populations. Column (1) includes neighborhood and cohort fixed effects.[5] In column (2), I substitute cohort with city-by-cohort fixed effects to compare immigrants in the same city but different neighborhoods. Columns

[4] I omit country-of-origin fixed effects because I am interested in the association between the two indicators of gender conservatism, which vary at the country-of-origin level, and the variables of interest.

[5] In table B.11, the treatment varies at the neighborhood level, so I substitute neighborhood with city fixed effects to compare individuals living in the same city.

(2), (5), and (7) report the results of the main text, which further include individual-level fixed effects for gender, country of origin, race, and immigration year. In column (1), I exclude the individual controls. In column (3), I interact each control with cohort-specific year dummies. In columns (4), (6), and (8), I report the results obtained using the propensity score matching approach. I first predict the propensity score by regressing the treatment status against individual-level characteristics—city, gender, race, country of origin, and year of immigration—measured in 1900. Then, I estimate the baseline regression, weighing individuals by their propensity score to ensure that the probability of being treated is similar across units.

The analysis sample comprises eleven major cities. A plausible concern is that a subset of them drives the results. In figures C.4, C.5, C.7, and C.6, I thus exclude one city at a time from the sample and report the estimated effect of settlement houses separately for men (black dots) and women (gray markers). The estimates remain remarkably stable irrespective of the city excluded from the sample. I can never reject the fact that the full-sample estimate is statistically equal to the leave-out estimates. Except for Manhattan, statistical significance is preserved irrespective of the excluded city. Unsurprisingly, the estimates are less precise when I exclude Manhattan, which accounts for approximately 27% of the sample, but their sign and magnitude remain unaltered.

Tables B.12, B.13, B.14, and B.15 report various estimates of the standard errors for the baseline treatment effects of settlement houses on immigrants. For each estimator, I report 90% confidence bands around the baseline treatment effect separately for men and women. I consider non-clustered standard errors in panel A and clustered standard errors at different layers in panel B. In practice, however, the estimated standard errors remain remarkably similar irrespective of the estimator, and statistical significance is consequently stable across the various specifications.

In the baseline analysis, individuals are exposed to a settlement house if they live within 250 meters of a settlement. In figures C.8, C.9, C.11, and C.10, I report the estimated treatment effects using alternative definitions of proximity between 100 and 1000 meters. Most of the estimates remain qualitatively unchanged for bandwidths between 100 and 400 meters and turn statistically insignificant thereafter. As displayed in Figure C.1, a 400-meter circle around a settlement in urban environments is large. Therefore, it is not surprising that settlement houses, which hosted an average of 45 volunteers, both residents and non-residents, did not impact immigrants beyond this area. The size of the treatment effects indicates that, reassuringly, the impact of settlements is largest within a relatively small area around them and is diluted once further immigrants are included in the treatment group.

B ADDITIONAL TABLES

Table B.1. Comparison between Althoff *et al.* (2025) and Census Tree Project Linked Datasets

	Full Population			Men			Women		
	Difference (1)	Std. Err. (2)	Mean (3)	Difference (4)	Std. Err. (5)	Mean (6)	Difference (7)	Std. Err. (8)	Mean (9)
Panel A. Labor Market Outcomes									
Labor Force Participation	0.019	(0.006)	0.57	-0.009	(0.004)	0.919	0.054	(0.006)	0.164
White-Collar Empployment	-0.006	(0.002)	0.101	-0.01	(0.002)	0.172	0.005	(0.001)	0.019
Blue-Collar Employment	0.001	(0.002)	0.258	-0.011	(0.002)	0.44	0.015	(0.002)	0.047
Occupational Income Score	0.044	(0.02)	2.213	-0.055	(0.015)	3.651	0.168	(0.015)	0.539
Panel B. Assimilation Outcomes									
Married with Immigrant (Same Country)	-0.004	(0.003)	0.588	0.005	(0.004)	0.591	-0.023	(0.003)	0.582
Married with Immigrant (Other Country)	0.001	(0.002)	0.1	0.002	(0.002)	0.094	0.0	(0.002)	0.108
Married with Native	-0.026	(0.005)	0.201	-0.031	(0.006)	0.239	-0.006	(0.002)	0.152
Foreign Name Index	0.002	(0.001)	0.553	0.004	(0.001)	0.553	0.0	(0.001)	0.552
Speak English	-0.011	(0.001)	0.929	-0.013	(0.003)	0.941	-0.013	(0.001)	0.914
English Literacy	-0.007	(0.001)	0.933	-0.008	(0.001)	0.95	-0.008	(0.001)	0.914
Naturalized	-0.03	(0.016)	0.575	-0.054	(0.005)	0.741	-0.028	(0.021)	0.387
Panel C. Family and Fertility Outcomes									
Has Children	-0.081	(0.007)	0.867	-0.077	(0.007)	0.865	-0.07	(0.005)	0.872
(IHS) Number of Children	-0.294	(0.026)	1.676	-0.256	(0.028)	1.652	-0.31	(0.016)	1.711
Age at First Marriage	0.337	(0.16)	25.643	0.187	(0.166)	27.264	0.623	(0.118)	23.602
Is Married	-0.05	(0.006)	0.873	-0.044	(0.004)	0.862	-0.045	(0.007)	0.887

Notes. This table compares the cross-sectional linked database used in the main analysis with an alternative dataset constructed using the links developed by Althoff *et al.* (2025). In each row, I report in columns (1), (4), and (7), the coefficient of a regression between the row variable and an indicator equal to one if the individual is in the Census Tree Project but not in the Althoff *et al.* (2025) dataset and zero otherwise. Hence, each coefficient captures the difference between individuals linked in the Census Tree Project but not in the representative linked sample and individuals linked in both datasets. Columns (2), (5), and (8) report the associated standard errors. For comparison, columns (3), (6), and (9) report the sample mean of each variable. Columns (1–3) report the differences in the entire population, whereas columns (4–6) and (7–9) respectively look at the male and female sub-samples. All regressions include neighborhood-by-census wave, neighborhood-by-cohort, cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the census-wave level. Referenced on page(s) 12.

Table B.2. Descriptive Statistics

	Men				Women			
	Mean (1)	Std. Dev. (2)	Median (3)	Obs. (4)	Mean (5)	Std. Dev. (6)	Median (7)	Obs. (8)
Panel A. Hexagon-Level Panel								
Has Settlement	0.064	0.244	0.000	67476	0.064	0.244	0.000	67476
Year of First Settlement	1904.870	5.960	1900.000	978	1904.870	5.960	1900.000	978
Population	1506.770	1617.055	1021.858	67476	1493.715	1524.409	1037.112	67476
Immigrants	570.303	998.064	240.794	67476	526.964	892.284	231.383	67476
Share in the Workforce	0.554	0.108	0.563	67476	0.238	0.092	0.219	67476
Share of Immigrants in the Workforce	0.718	0.125	0.747	67476	0.233	0.138	0.195	67476
Income per Person	17.148	2.899	16.810	67476	4.477	1.614	4.312	67476
Income per Immigrant	22.569	2.677	22.541	67476	3.879	1.732	3.609	67476
Share White Collar	0.271	0.132	0.242	67476	0.163	0.099	0.141	67476
Share White Collar among Immigrants	0.310	0.149	0.281	67476	0.160	0.138	0.112	67476
Share Blue Collar	0.364	0.107	0.379	67476	0.077	0.041	0.072	67476
Share Blue Collar among Immigrants	0.536	0.159	0.555	67476	0.076	0.048	0.067	67476
Panel B. Individual-Level Stacked Cross-Section								
Has Settlement	0.116	0.320	0.000	5439628	0.116	0.320	0.000	5439628
Young when Settlement Established	0.083	0.276	0.000	5439628	0.083	0.276	0.000	5439628
Employed	0.898	0.303	1.000	2848079	0.197	0.398	0.000	2591549
White-Collar Employment	0.139	0.346	0.000	2848079	0.017	0.129	0.000	2591549
Occupation-Based Income	3.529	1.230	3.872	2848079	0.630	1.306	0.000	2591549
Literacy	0.924	0.264	1.000	2848079	0.890	0.313	1.000	2591549
Has Children	0.790	0.407	1.000	2848079	0.782	0.413	1.000	2591549
N. of Children	1.494	0.920	1.818	2848079	1.491	0.935	1.818	2591549
Age at First Child	27.721	6.348	27.000	2848079	24.278	6.288	23.000	2591549
Married	0.787	0.409	1.000	2848079	0.825	0.380	1.000	2591549
Married with Foreign-Born (Same Country)	0.620	0.485	1.000	2848079	0.616	0.486	1.000	2591549
Married with Foreign-Born (Different Country)	0.081	0.273	0.000	2848079	0.092	0.289	0.000	2591549
Married with US-Born	0.200	0.400	0.000	2848079	0.125	0.331	0.000	2591549
Foreign Name Index	0.562	0.127	0.572	2848079	0.557	0.125	0.568	2591549
Speaks English	0.900	0.299	1.000	2848079	0.873	0.333	1.000	2591549
Naturalized Citizen	0.692	0.462	1.000	2848079	0.172	0.377	0.000	2591549

Notes. This table reports a set of descriptive statistics for the main treatment and outcome variables used in the paper. Columns (1) and (5), (2) and (6), (3) and (7), and (4) and (8) report, respectively, the mean, standard deviation, median, and number of observations. Columns (1–4) refer to the men population; columns (5–8) refer to the female population. Panel A refers to the panel at the hexagon-by-census decade level; the statistics in panel B are constructed from the stacked 1880–1940 individual-level cross-sectional dataset. The statistics of Panel B are constructed on the same immigrant sub-samples used in the main analysis. In panel A, hexagons are weighted by population. Referenced on page(s) 12, 13.

Table B.3. Descriptive Statistics on Settlement Houses

	All Settlements			Settlements in Sample		
	Mean (1)	Std. Dev. (2)	Obs. (3)	Mean (4)	Std. Dev. (5)	Obs. (6)
Panel A. General Information						
Year Established	1901.556	6.525	396	1899.926	6.935	190
Has Address	0.947	0.224	396	0.995	0.073	190
N. Residents	6.396	7.972	265	8.271	9.817	107
N. Women Residents	4.804	6.303	265	6.000	7.491	107
N. Volunteers	28.906	35.196	265	37.234	39.775	107
N. Women Volunteers	21.106	27.853	265	25.776	31.332	107
Panel B. Region						
North East	0.520	0.500	396	0.700	0.459	190
Mid West	0.338	0.474	396	0.263	0.442	190
South	0.139	0.346	396	0.037	0.189	190
West	0.000	0.000	396	0.000	0.000	190
Panel C. Area of Activity						
Nursery	0.425	0.495	372	0.445	0.498	173
Kindergarten	0.565	0.496	372	0.578	0.495	173
Further Education	0.640	0.481	372	0.572	0.496	173
Professional Classes	0.806	0.396	372	0.769	0.423	173
Recreational Classes	0.288	0.453	372	0.318	0.467	173
Health	0.110	0.314	372	0.116	0.321	173
Assimilation	0.188	0.391	372	0.237	0.426	173
Financial Relief	0.374	0.484	372	0.462	0.500	173
Panel D. Target Group						
Italians	0.195	0.396	329	0.259	0.439	147
Russians	0.015	0.123	329	0.007	0.082	147
Poles	0.061	0.239	329	0.041	0.199	147
Jews	0.222	0.416	329	0.293	0.456	147
Irish	0.167	0.374	329	0.218	0.414	147
Germans	0.122	0.327	329	0.143	0.351	147
All Immigrants	0.523	0.500	329	0.694	0.462	147
Blacks	0.103	0.305	329	0.122	0.329	147
Americans	0.207	0.406	329	0.122	0.329	147
Working Class	0.179	0.384	329	0.116	0.321	147
Panel E. Religious Affiliation						
Catholic	0.109	0.312	211	0.162	0.370	105
Protestant	0.777	0.417	211	0.724	0.449	105
Jewish	0.114	0.318	211	0.114	0.320	105
Non denominational	0.133	0.340	211	0.133	0.342	105

Notes. This table reports descriptive statistics on the universe of settlements (columns 1–3) and the settlements in the analysis sample (columns 4–5) established between 1882 and 1911. Each row reports the mean of a variable (columns 1 and 4), its standard deviation (columns 2 and 5), and the total number of observations where the variable is not missing. In panel C, the area of activity is constructed from the set of activities the settlement performs; in panels D and E, the target group and religious affiliations are directly reported in the *Handbook of Settlements*. In panels B–E, the variables are equal to one if the settlement falls within the given category, and zero otherwise. Referenced on page(s) 13, A2.

Table B.4. Presence of Settlements and Demographic Characteristics in 1880: Tabular Evidence

	Without Controls				Controlling for Population			
	Unconditional		City FE		Unconditional		City FE	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Shares Relative to Entire Population								
Population	0.6538***	(0.0551)	0.3649***	(0.0437)				
Share of Women	-0.0038	(0.0036)	-0.0057**	(0.0028)	-0.0073***	(0.0018)	-0.0067***	(0.0017)
Share of Black	-0.0051	(0.0148)	0.0073	(0.0146)	-0.0087	(0.0139)	0.0037	(0.0138)
Share in Employment	0.0207***	(0.0061)	0.0008	(0.0054)	-0.0088**	(0.0045)	-0.0120***	(0.0044)
Share of Women in Employment	0.0050	(0.0046)	-0.0058	(0.0062)	-0.0073*	(0.0040)	-0.0098**	(0.0046)
Share in White Collar Employment	0.0022	(0.0126)	-0.0156	(0.0158)	-0.0112	(0.0111)	-0.0186	(0.0117)
Share in Blue Collar Employment	0.0134	(0.0094)	0.0171**	(0.0079)	0.0053	(0.0064)	0.0102*	(0.0052)
Income per Capita	0.4306***	(0.1253)	0.1479	(0.1045)	-0.1047	(0.1099)	-0.1244*	(0.0718)
Share of Children	-0.0060	(0.0108)	0.0058	(0.0108)	0.0081	(0.0087)	0.0116	(0.0083)
Share of Children at School	-0.0012	(0.0037)	0.0002	(0.0032)	0.0049*	(0.0030)	0.0036	(0.0028)
Share of Children at Work	0.0025	(0.0021)	0.0023	(0.0015)	0.0002	(0.0014)	0.0011	(0.0011)
Panel B. Shares Within Immigrant Population								
Share of Immigrants	0.0588***	(0.0153)	0.0209***	(0.0034)	0.0381***	(0.0125)	0.0115***	(0.0024)
Immigrants from North-Western Europe	0.0070	(0.0494)	-0.0276	(0.0396)	0.0163	(0.0420)	-0.0085	(0.0343)
Immigrants from Southern Europe	0.0155***	(0.0024)	0.0097***	(0.0037)	0.0101***	(0.0035)	0.0077**	(0.0034)
Immigrants from Eastern Europe	-0.0113	(0.0608)	0.0249	(0.0431)	-0.0439	(0.0543)	-0.0042	(0.0397)
Share of Women	0.0023	(0.0073)	-0.0094*	(0.0057)	-0.0123***	(0.0033)	-0.0159***	(0.0033)
Share in Employment	0.0127	(0.0086)	-0.0125*	(0.0070)	-0.0230***	(0.0065)	-0.0271***	(0.0049)
Share of Women in Employment	-0.0062	(0.0109)	-0.0282**	(0.0126)	-0.0163*	(0.0093)	-0.0275***	(0.0092)
Share in White Collar Employment	-0.0031	(0.0162)	-0.0299	(0.0202)	-0.0208	(0.0137)	-0.0321**	(0.0142)
Share in Blue Collar Employment	0.0049	(0.0105)	0.0161	(0.0107)	0.0025	(0.0097)	0.0099	(0.0086)
Income per Capita	0.2902***	(0.1045)	0.2877***	(0.0973)	-0.3380**	(0.1531)	-0.0968	(0.0623)
Share of Children	0.0018	(0.0048)	0.0032	(0.0025)	0.0062**	(0.0028)	0.0036*	(0.0019)
Share of Children at School	-0.0001	(0.0019)	0.0001	(0.0012)	0.0027**	(0.0011)	0.0007	(0.0008)
Share of Children at Work	0.0006	(0.0010)	0.0007	(0.0008)	0.0004	(0.0007)	0.0003	(0.0006)
Observations	11,941	11,941	11,941	11,941	11,941	11,941	11,941	11,941

Notes. This table reports the correlation between the presence of a settlement between 1882 and 1911 and hexagon-level demographic characteristics in 1880. In each line, I report the correlation between the row variable and the presence of a settlement unconditionally (columns 1), controlling for city fixed effects (columns 3), controlling for population (column 5), or both (column 7). In panel A, the variables are computed over the entire population and—except for population—are expressed as shares of the population. In panel B, the variables are computed over the foreign-born population and are expressed as shares of the foreign-born population, except for the share of immigrants, which is defined as the share of foreign-born individuals over the entire population. All variables are standardized for readability. Columns (2), (4), (6), and (8) report standard errors clustered at the neighborhood level. Referenced on page(s) 15, A4. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.5. Family and Fertility Dynamics After the Establishment of Settlements

	D.V. Normalized by # Immigrant Women			D.V. Normalized by # Immigrant Children	
	(1) Is Mother	(2) N. Children	(3) Is Married	(4) At School	(5) At Work
Post Establishment of Settlement	0.016	0.124***	-0.006	0.084**	-0.026
	(0.026)	(0.029)	(0.026)	(0.039)	(0.021)
Hexagon FE	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes
Number of Hexagons	11,246	11,246	11,246	11,246	11,246
Number of Observations	67,476	67,476	67,476	67,476	67,476
Mean Dep. Var.	0.074	0.074	0.074	0.074	0.074

Notes. This table reports the relationship between the establishment of settlement houses and a set of demographic and labor-market variables. The unit of observation is a hexagon at a (census) decade frequency between 1880 and 1940, except 1890. The treatment variable equals one after a settlement is established in the hexagon and zero otherwise. The dependent variable is the share of foreign-born women with at least one child (column 1), the number of children per foreign-born woman (column 2), the share of married foreign-born women (column 3), the share of foreign-born children attending school (column 4), and the share of foreign-born children at work (column 5). An individual is treated as a child if they are less than 15 years old when the census takes place. Hexagons are weighted by population. All regressions include hexagon and city-by-decade fixed effects. Standard errors are reported in parentheses and are clustered at the hexagon level. Referenced on page(s) 18, A5. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.6. Native Population and Settlement Houses

	All Natives		Native Men		Native Women	
	(1) Number	(2) Share	(3) Number	(4) Share	(5) Number	(6) Share
Post Establishment of Settlement	-1575.699**	-0.131***	-625.397**	-0.136***	-950.302***	-0.017
	(630.999)	(0.032)	(306.330)	(0.032)	(327.435)	(0.066)
Hexagon FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Hexagons	11,246	11,246	11,246	11,246	11,246	11,246
Number of Observations	67,476	67,476	67,476	67,476	67,476	67,476
Mean Dep. Var.	0.007	0.007	0.007	0.007	0.007	0.007

Notes. This table reports the relationship between the establishment of settlement houses and the native-born population at the hexagon level. The unit of observation is a hexagon at a (census) decade frequency between 1880 and 1940, except 1890. The treatment variable equals one after a settlement is established in the hexagon and zero otherwise. The dependent variable is the number of natives (column 1), the share of the population that is native-born (column 2), the number of native-born men (column 3), the share of men that are native-born (column 4), the number of native-born women (column 5), and the share of women that are native-born (column 6). Hexagons are weighted by population. All regressions include hexagon and city-by-decade fixed effects. Standard errors are reported in parentheses and are clustered at the hexagon level. Referenced on page(s) 18, A5. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.7. The Labor Market Effects of Settlement Houses: Robustness Regressions

	Full Population				Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Labor Force Participation								
Post × Settlement × Young	0.009**	0.006*	0.007**	0.009*	0.010**	0.008	-0.002	-0.001
	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.564	0.564	0.564	0.564	0.898	0.898	0.196	0.196
Panel B. White-Collar Employment								
Post × Settlement × Young	0.010***	0.010***	0.010***	0.008**	0.016***	0.011*	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.080	0.080	0.080	0.080	0.138	0.138	0.017	0.017
Panel C. Blue-Collar Employment								
Post × Settlement × Young	-0.006	-0.007*	-0.008*	0.000	-0.009	-0.002	-0.004	0.001
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.004)	(0.006)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.251	0.251	0.251	0.251	0.427	0.427	0.057	0.057
Panel D. Occupational Income Score								
Post × Settlement × Young	0.032**	0.021*	0.024**	0.031*	0.040***	0.035*	-0.012	-0.009
	(0.015)	(0.012)	(0.012)	(0.017)	(0.015)	(0.020)	(0.017)	(0.026)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	2.148	2.148	2.148	2.148	3.529	3.529	0.626	0.626
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls × Cohort	No	No	Yes	No	Yes	No	Yes	No
Propensity Score Weights	No	No	No	Yes	No	Yes	No	Yes

Notes. This table reports the effect of settlements on labor-market variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In columns (1–4), the sample comprises all individuals between 15 and 65 years old; in columns (5–6) and (7–8), the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual works (panel A), if they work in a white-collar occupation (panel B), if they work in a blue-collar occupation (panel C), and the inverse hyperbolic sine of the occupational income score (panel D). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects. In columns (2), (6), and (8), I include individual controls—sex, birthplace, race, and immigration year. In column (3), I interact the controls with cohort-specific dummies. In columns (4), (6), and (8), individuals are weighted by their propensity score. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 22, A6. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.8. The Assimilation Effects of Settlement Houses: Robustness Regressions

	Full Population				Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Married to Immigrant from Same Origin Country								
Post × Settlement × Young	0.037***	0.039***	0.039***	0.014***	0.046***	0.018***	0.032***	0.010*
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.618	0.618	0.618	0.618	0.620	0.620	0.616	0.616
Panel B. Married to Immigrant from Other Origin Country								
Post × Settlement × Young	-0.002	-0.001	-0.001	-0.004	0.000	-0.007	-0.006	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)
Number of Individuals	4,351,176	4,351,170	4,350,364	4,350,408	2,348,378	2,348,739	1,935,845	1,936,614
Mean Dep. Var.	0.086	0.086	0.086	0.086	0.081	0.081	0.093	0.093
Panel C. Married to Native-Born								
Post × Settlement × Young	-0.034***	-0.036***	-0.034***	-0.013***	-0.042***	-0.016***	-0.024***	-0.011***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Number of Individuals	4,351,176	4,351,170	4,350,364	4,350,408	2,348,378	2,348,739	1,935,845	1,936,614
Mean Dep. Var.	0.166	0.166	0.166	0.166	0.200	0.200	0.126	0.126
Panel D. (IHS) Foreign Name Index of Children								
Post × Settlement × Young	0.003***	0.004***	0.004***	0.002	0.005***	0.002	0.003*	0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Number of Individuals	4,193,543	4,193,535	4,192,743	4,192,947	2,145,566	2,146,025	1,980,262	1,981,046
Mean Dep. Var.	0.560	0.560	0.560	0.560	0.562	0.562	0.558	0.558
Panel E. Speaks English								
Post × Settlement × Young	0.009*	0.010**	0.010**	0.023***	0.008	0.020*	0.014**	0.030***
	(0.005)	(0.005)	(0.005)	(0.008)	(0.006)	(0.010)	(0.006)	(0.010)
Number of Individuals	4,739,467	4,739,460	4,739,006	4,738,587	2,450,672	2,450,525	2,232,978	2,233,222
Mean Dep. Var.	0.887	0.887	0.887	0.887	0.900	0.900	0.871	0.871
Panel F. Speaks and Writes English								
Post × Settlement × Young	0.008**	0.008**	0.007*	0.022***	0.010**	0.021***	0.005	0.025***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.005)	(0.008)	(0.005)	(0.009)
Number of Individuals	5,263,045	5,263,039	5,262,319	5,262,150	2,725,793	2,726,024	2,475,431	2,476,007
Mean Dep. Var.	0.907	0.907	0.907	0.908	0.924	0.924	0.889	0.889
Panel G. Naturalized Citizen								
Post × Settlement × Young	-0.002	-0.002	0.001	-0.001	-0.011	-0.008	0.013***	0.010
	(0.006)	(0.005)	(0.005)	(0.008)	(0.008)	(0.012)	(0.005)	(0.008)
Number of Individuals	4,861,684	4,861,678	4,861,256	4,860,793	2,526,534	2,526,360	2,278,891	2,279,111
Mean Dep. Var.	0.445	0.445	0.446	0.446	0.692	0.692	0.175	0.175
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls × Cohort	No	No	Yes	No	Yes	No	Yes	No
Propensity Score Weights	No	No	No	Yes	No	Yes	No	Yes

Notes. This table reports the effect of settlements on cultural assimilation variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In columns (1–4), the sample comprises all individuals between 15 and 65 years old; in columns (5–6) and (7–8), the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual is married to an immigrant from the same country (panel A), a different country (panel B), or to a native (panel C), the inverse hyperbolic sine of the foreign name index (panel D), an indicator equal to one if the individual speaks (panel E) or writes (panel F) English, and an indicator for naturalized citizens (panel G). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects. In columns (2), (6), and (8), I include individual controls—sex, birthplace, race, and immigration year. In column (3), I interact the controls with cohort-specific dummies. In columns (4), (6), and (8), individuals are weighted by their propensity score. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 26, A6. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.9. The Family and Fertility Effects of Settlement Houses: Robustness Regressions

	Full Population				Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. At Least One Child								
Post × Settlement × Young	0.002	0.005	0.004	0.006	0.004	0.000	0.014***	0.019***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.006)	(0.004)	(0.006)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.787	0.787	0.787	0.787	0.792	0.792	0.785	0.785
Panel B. Number of Children								
Post × Settlement × Young	0.011	0.015**	0.014*	0.023**	0.016*	0.017	0.032***	0.049***
	(0.008)	(0.008)	(0.007)	(0.010)	(0.009)	(0.013)	(0.010)	(0.013)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	1.495	1.495	1.495	1.495	1.498	1.498	1.500	1.500
Panel C. Age at First Children								
Post × Settlement × Young	-0.208***	-0.189***	-0.133***	-0.295***	-0.138**	-0.325***	-0.128**	-0.268***
	(0.055)	(0.052)	(0.048)	(0.065)	(0.064)	(0.079)	(0.062)	(0.083)
Number of Individuals	3,990,773	3,990,768	3,990,117	3,990,318	2,089,445	2,089,956	1,848,902	1,849,500
Mean Dep. Var.	26.038	26.038	26.036	26.037	27.624	27.626	24.136	24.139
Panel D. Is Married								
Post × Settlement × Young	-0.005	-0.003	-0.003	0.005	-0.009*	-0.003	0.002	0.008
	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)	(0.007)	(0.004)	(0.006)
Number of Individuals	5,280,296	5,280,290	5,279,572	5,279,397	2,736,157	2,736,382	2,482,469	2,483,043
Mean Dep. Var.	0.805	0.805	0.805	0.805	0.788	0.788	0.827	0.826
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls × Cohort	No	No	Yes	No	Yes	No	Yes	No
Propensity Score Weights	No	No	No	Yes	No	Yes	No	Yes

Notes. This table reports the effect of settlements on family and fertility variables. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. In columns (1–4), the sample comprises all individuals between 15 and 65 years old; in columns (5–6) and (7–8), the samples exclude women and men, respectively. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual has at least one child (panel A), the inverse hyperbolic sine of the number of children (panel B), the age when individuals have their first child (panel C), and an indicator for married individuals (panel D). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects. In columns (2), (6), and (8), I include individual controls—sex, birthplace, race, and immigration year. In column (3), I interact the controls with cohort-specific dummies. In columns (4), (6), and (8), individuals are weighted by their propensity score. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 28, A6. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.10. Heterogeneous Responses to Settlements by Origin Country's Gender Norms

	Male Dominance Index		Fertility in 1900	
	(1)	(2)	(3)	(4)
	Labor Force Participation	Number of Children	Labor Force Participation	Number of Children
Post × Settlement × Young	-0.070***	0.102***	-0.017	0.233***
	(0.010)	(0.027)	(0.012)	(0.033)
Male-Dominance Index	0.061***	1.156***		
	(0.021)	(0.047)		
Post × Settlement × Young × Male-Dominance Index	-0.330***	0.155		
	(0.045)	(0.139)		
Fertility Rate (1900)			-0.166***	0.317***
			(0.003)	(0.007)
Post × Settlement × Young × Fertility Rate (1900)			-0.011	0.170***
			(0.009)	(0.027)
Neighborhood-Cohort FE	Yes	Yes	Yes	Yes
Neighborhood-Census FE	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Number of Individuals	1,169,829	1,169,829	2,042,582	2,042,582
Mean Dep. Var.	0.221	1.490	0.188	1.536

Notes. This table reports the effect of settlements on women's labor market outcomes. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all women between 15 and 65 years old. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The baseline treatment is interacted with two measures of conservative gender norms: the Male Dominance Index of Guarnieri and Tur-Prats (2023) (columns 1–2), and the total fertility rate in 1900 (panel B) of Coale and Treadway (1986) (columns 3–4). The dependent variable is one if the individual works (columns 1 and 3) and the inverse hyperbolic sine of the number of children (columns 2 and 4). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects and individual controls—sex, race, and immigration year. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 30, A6. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.11. The Intergenerational Effects of Settlement Houses: Robustness Regressions

	Full Population				Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Labor Force Participation								
Settlement	0.013***	-0.003	-0.004	-0.000	0.004**	0.004*	-0.017***	-0.013*
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.007)
Number of Individuals	274,178	274,174	274,061	273,936	184,693	184,644	89,222	89,283
Mean Dep. Var.	0.719	0.719	0.719	0.719	0.931	0.931	0.281	0.281
Panel B. Occupational Income Score								
Settlement	0.088***	-0.007	-0.007	0.002	0.016**	0.017**	-0.052**	-0.047*
	(0.019)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.023)	(0.026)
Number of Individuals	274,178	274,174	274,061	273,936	184,693	184,644	89,222	89,283
Mean Dep. Var.	2.849	2.849	2.849	2.849	3.743	3.743	0.999	1.000
Panel C. Has Children								
Settlement	0.052***	0.011***	0.012***	0.005	0.009**	0.005	0.015**	0.004
	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)
Number of Individuals	274,178	274,174	274,061	273,936	184,693	184,644	89,222	89,283
Mean Dep. Var.	0.779	0.779	0.779	0.779	0.800	0.800	0.734	0.734
Panel D. Number of Children								
Settlement	0.116***	0.022***	0.024***	0.008	0.016*	0.006	0.036**	0.015
	(0.013)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.014)	(0.014)
Number of Individuals	274,178	274,174	274,061	273,936	184,693	184,644	89,222	89,283
Mean Dep. Var.	1.346	1.346	1.346	1.346	1.364	1.364	1.311	1.310
Panel E. Married								
Settlement	0.041***	0.009***	0.010***	0.005	0.004	0.002	0.020***	0.014**
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)
Number of Individuals	274,178	274,174	274,061	273,936	184,693	184,644	89,222	89,283
Mean Dep. Var.	0.819	0.819	0.819	0.819	0.846	0.846	0.764	0.764
Panel F. Married to Immigrants from Other Countries								
Settlement	0.022***	0.011***	0.010***	0.005	0.007*	0.002	0.022***	0.014*
	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)	(0.008)
Number of Individuals	225,883	225,878	225,750	225,692	161,780	161,783	63,792	63,899
Mean Dep. Var.	0.108	0.108	0.108	0.108	0.105	0.105	0.114	0.113
Panel G. Married to Native-Born Americans								
Settlement	-0.070***	-0.008**	-0.011**	-0.007*	-0.013***	-0.007	-0.002	-0.006
	(0.008)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)
Number of Individuals	225,883	225,878	225,750	225,692	161,780	161,783	63,792	63,899
Mean Dep. Var.	0.348	0.348	0.348	0.347	0.360	0.359	0.318	0.317
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Census FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls × Cohort	No	No	Yes	No	Yes	No	Yes	No
Propensity Score Weights	No	No	No	Yes	No	Yes	No	Yes

Notes. This table reports the effect of childhood exposure to settlements. The unit of observation is an individual immigrant observed in one census between 1920 (i.e., after the last settlement house is established) and 1940. In columns (1–4), the sample comprises all individuals between 15 and 65 years old; in columns (5–6) and (7–8), the samples exclude women and men, respectively. Regressions further exclude individuals above 15 years old when the first settlement house is established in their neighborhood. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. The dependent variable is: one if the individual works (panel A) and the (IHS) of the occupational income score (panel B), one for individuals with children (panel C), the (IHS) number of children (panel D), one for married individuals (panel E), and individuals married to immigrants from other countries (panel F) or natives (panel G). All regressions include city-by-census wave, city-by-cohort, and cohort-by-census wave fixed effects. In columns (2), (6), and (8), I include individual controls—sex, birthplace, race, and immigration year. In column (3), I interact the controls with cohort-specific dummies. In columns (4), (6), and (8), individuals are weighted by their propensity score. Standard errors are clustered at the neighborhood level and are reported in parentheses. Referenced on page(s) 33, A6, A6. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.12. Alternative Standard Errors: Labor Market Responses

	Labor Force Participation	White-Collar Employment	Blue-Collar Employment	Occupational Income Score
	(1)	(2)	(3)	(4)
Panel A. No Clustering				
No Adjustments				
Men	(0.016, 0.004)	(0.020, 0.007)	(0.004, -0.014)	(0.066, 0.018)
Women	(0.006, -0.008)	(0.004, -0.001)	(0.003, -0.006)	(0.017, -0.033)
White Correction				
Men	(0.016, 0.004)	(0.020, 0.006)	(0.004, -0.015)	(0.066, 0.018)
Women	(0.007, -0.009)	(0.004, -0.001)	(0.004, -0.007)	(0.019, -0.034)
Panel B. Clustered Standard Errors				
District				
Men	(0.016, 0.004)	(0.021, 0.005)	(0.004, -0.015)	(0.067, 0.017)
Women	(0.007, -0.009)	(0.004, -0.002)	(0.005, -0.008)	(0.021, -0.036)
City				
Men	(0.013, 0.007)	(0.026, 0.000)	(0.010, -0.021)	(0.057, 0.027)
Women	(0.007, -0.009)	(0.003, -0.001)	(0.005, -0.008)	(0.021, -0.037)
Census Wave				
Men	(0.013, 0.007)	(0.020, 0.007)	(0.003, -0.013)	(0.052, 0.032)
Women	(0.001, -0.003)	(0.004, -0.001)	(0.004, -0.007)	(0.002, -0.018)
Cohort				
Men	(0.015, 0.005)	(0.020, 0.006)	(0.006, -0.017)	(0.061, 0.023)
Women	(0.006, -0.008)	(0.004, -0.001)	(0.004, -0.007)	(0.015, -0.031)
District, Census Wave				
Men	(0.015, 0.005)	(0.022, 0.005)	(0.005, -0.015)	(0.061, 0.023)
Women	(0.005, -0.007)	(0.004, -0.002)	(0.006, -0.009)	(0.013, -0.029)
District, Cohort				
Men	(0.016, 0.004)	(0.021, 0.005)	(0.007, -0.017)	(0.063, 0.021)
Women	(0.007, -0.009)	(0.004, -0.001)	(0.005, -0.008)	(0.019, -0.034)
District, Census Wave				
Men	(0.016, 0.004)	(0.013, 0.013)	(-0.005, -0.005)	(0.065, 0.019)
Women	(-0.001, -0.001)	(0.001, 0.001)	(0.005, -0.008)	(0.020, -0.036)

Notes. This table reports the effect of settlements on labor-market variables using alternative standard error estimators. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old; within each panel, the table reports the estimates for men and women separately. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual works (column 1), if they work in a white-collar occupation (column 2), or if they work in a blue-collar occupation (column 3), and the inverse hyperbolic sine of the occupation-based imputed income (column 4). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are either unclustered (panel A), or clustered at different levels, as shown in panel B. The table reports 90% confidence bands around the baseline treatment effect estimate. Referenced on page(s) 18, A7. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.13. Alternative Standard Errors: Assimilation Responses

	Married Immigrant (Same Country)	Married Immigrant (Other Country)	Married Native	Foreign Name Index	Speak English	Writes English	Naturalized Citizen
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No Clustering							
No Adjustments							
Men	(0.050, 0.033)	(0.006, -0.005)	(-0.033, -0.047)	(0.008, 0.003)	(0.014, 0.002)	(0.015, 0.005)	(-0.005, -0.023)
Women	(0.039, 0.020)	(0.001, -0.012)	(-0.017, -0.030)	(0.005, 0.000)	(0.023, 0.008)	(0.013, 0.001)	(0.018, 0.008)
White Correction							
Men	(0.050, 0.033)	(0.006, -0.005)	(-0.033, -0.047)	(0.008, 0.003)	(0.015, 0.001)	(0.016, 0.004)	(-0.005, -0.023)
Women	(0.039, 0.020)	(0.001, -0.012)	(-0.017, -0.030)	(0.005, 0.000)	(0.023, 0.008)	(0.014, -0.000)	(0.019, 0.008)
Panel B. Clustered Standard Errors							
District							
Men	(0.051, 0.033)	(0.006, -0.006)	(-0.033, -0.047)	(0.008, 0.003)	(0.018, -0.002)	(0.018, 0.002)	(-0.001, -0.027)
Women	(0.037, 0.021)	(0.001, -0.012)	(-0.017, -0.030)	(0.005, 0.000)	(0.025, 0.006)	(0.015, -0.002)	(0.021, 0.006)
City							
Men	(0.053, 0.031)	(0.004, -0.004)	(-0.026, -0.054)	(0.009, 0.002)	(0.017, -0.001)	(0.015, 0.005)	(-0.004, -0.024)
Women	(0.041, 0.018)	(-0.001, -0.010)	(-0.011, -0.036)	(0.005, 0.001)	(0.030, 0.001)	(0.021, -0.007)	(0.025, 0.002)
Census Wave							
Men	(0.052, 0.031)	(0.002, -0.002)	(-0.032, -0.048)	(0.007, 0.004)	(0.020, -0.005)	(0.017, 0.003)	(0.000, -0.028)
Women	(0.039, 0.019)	(-0.001, -0.009)	(-0.013, -0.034)	(0.003, 0.002)	(0.025, 0.006)	(0.009, 0.005)	(0.033, -0.006)
Cohort							
Men	(0.052, 0.032)	(0.006, -0.006)	(-0.031, -0.049)	(0.008, 0.003)	(0.015, 0.001)	(0.015, 0.005)	(-0.000, -0.027)
Women	(0.040, 0.019)	(0.000, -0.011)	(-0.016, -0.031)	(0.006, -0.001)	(0.023, 0.008)	(0.013, 0.000)	(0.020, 0.007)
District, Census Wave							
Men	(0.054, 0.030)	(0.004, -0.004)	(-0.031, -0.049)	(0.007, 0.003)	(0.022, -0.007)	(0.019, 0.001)	(0.003, -0.031)
Women	(0.040, 0.018)	(0.000, -0.011)	(-0.013, -0.035)	(0.005, 0.001)	(0.028, 0.003)	(0.013, 0.000)	(0.032, -0.005)
District, Cohort							
Men	(0.052, 0.031)	(0.007, -0.007)	(-0.030, -0.050)	(0.008, 0.003)	(0.019, -0.003)	(0.018, 0.003)	(0.002, -0.030)
Women	(0.039, 0.019)	(0.001, -0.012)	(-0.015, -0.032)	(0.006, -0.001)	(0.026, 0.005)	(0.015, -0.002)	(0.022, 0.005)
District, Census Wave							
Men	(0.051, 0.032)	(0.000, 0.000)	(-0.031, -0.049)	(0.008, 0.003)	(0.019, -0.003)	(0.018, 0.002)	(0.000, -0.028)
Women	(0.038, 0.020)	(-0.005, -0.005)	(-0.024, -0.024)	(0.003, 0.003)	(0.015, 0.015)	(0.015, -0.002)	(0.013, 0.013)

Notes. This table reports the effect of settlements on assimilation variables using alternative standard error estimators. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old; within each panel, the table reports the estimates for men and women separately. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. In columns (1–3), the dependent variable is one if individuals are married with immigrants from the same country (column 1), immigrants from other countries (column 2), or natives (column 3). In column (4), the dependent variable is the foreign name index. In columns (5) and (6), the outcome is one for individuals who can speak (column 5) or both speak and write (column 6) in English. In column (7), the dependent variable is one if the individual is a citizen (column 8). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are either unclustered (panel A), or clustered at different levels, as shown in panel B. The table reports 90% confidence bands around the baseline treatment effect estimate. Referenced on page(s) 18, A7. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.14. Alternative Standard Errors: Family and Fertility Responses

	Has Children	Number of Children	Age at First Child	Married
	(1)	(2)	(3)	(4)
Panel A. No Clustering				
No Adjustments				
Men	(0.010, -0.005)	(0.028, -0.003)	(-0.003, -0.015)	(-0.047, -0.296)
Women	(0.021, 0.007)	(0.051, 0.020)	(0.007, -0.005)	(-0.058, -0.310)
White Correction				
Men	(0.010, -0.004)	(0.028, -0.003)	(-0.003, -0.016)	(-0.037, -0.306)
Women	(0.021, 0.007)	(0.051, 0.019)	(0.007, -0.005)	(-0.051, -0.317)
Panel B. Clustered Standard Errors				
District				
Men	(0.009, -0.004)	(0.027, -0.002)	(-0.001, -0.018)	(-0.062, -0.281)
Women	(0.021, 0.007)	(0.051, 0.019)	(0.008, -0.006)	(-0.077, -0.291)
City				
Men	(0.012, -0.007)	(0.031, -0.006)	(-0.001, -0.017)	(-0.036, -0.307)
Women	(0.025, 0.002)	(0.061, 0.010)	(0.011, -0.009)	(-0.033, -0.335)
Census Wave				
Men	(0.006, -0.001)	(0.024, 0.001)	(0.003, -0.022)	(0.076, -0.420)
Women	(0.020, 0.008)	(0.050, 0.021)	(0.006, -0.004)	(0.104, -0.472)
Cohort				
Men	(0.009, -0.004)	(0.028, -0.004)	(-0.001, -0.018)	(-0.062, -0.281)
Women	(0.020, 0.008)	(0.049, 0.022)	(0.007, -0.004)	(-0.093, -0.275)
District, Census Wave				
Men	(0.009, -0.004)	(0.027, -0.003)	(0.004, -0.023)	(0.073, -0.416)
Women	(0.021, 0.006)	(0.054, 0.017)	(0.008, -0.006)	(0.094, -0.462)
District, Cohort				
Men	(0.010, -0.005)	(0.030, -0.005)	(0.001, -0.020)	(-0.054, -0.290)
Women	(0.021, 0.006)	(0.052, 0.019)	(0.008, -0.006)	(-0.086, -0.282)
District, Census Wave				
Men	(0.010, -0.005)	(0.012, 0.012)	(-0.009, -0.009)	(-0.053, -0.290)
Women	(0.021, 0.006)	(0.053, 0.018)	(0.008, -0.006)	(-0.067, -0.301)

Notes. This table reports the effect of settlements on family and fertility variables using alternative standard error estimators. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old; within each panel, the table reports the estimates for men and women separately. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. The dependent variable is one if the individual has at least one child (column 1), the inverse hyperbolic sin of the number of children (column 2), the age when they have their first child (column 3), and indicator equal to one if they are married (column 4). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are either unclustered (panel A), or clustered at different levels, as shown in panel B. The table reports 90% confidence bands around the baseline treatment effect estimate. Referenced on page(s) 18, A7. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

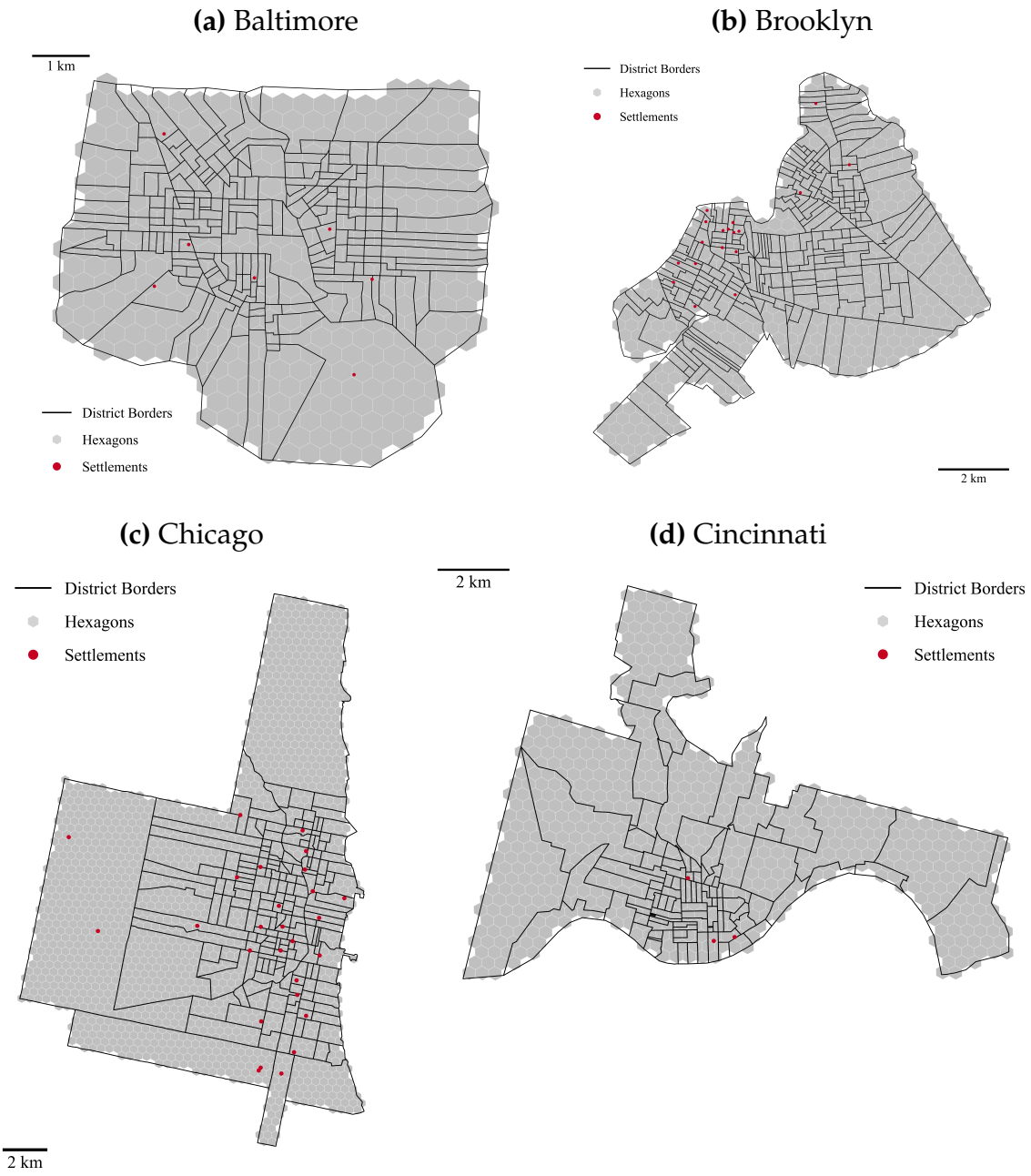
Table B.15. Alternative Standard Errors: Intergenerational Responses

	Labor Force Participation	Occupational Income Score	Has Children	Number of Children	Married	Married Immigrant (Same Country)	Married Immigrant (Other Country)	Married Native
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. No Clustering								
No Adjustments								
Men	(0.006, 0.000)	(0.027, 0.002)	(0.013, 0.004)	(0.024, 0.005)	(0.008, 0.000)	(0.013, 0.001)	(0.009, 0.002)	(-0.004, -0.015)
Women	(-0.010, -0.026)	(-0.026, -0.085)	(0.022, 0.007)	(0.054, 0.022)	(0.027, 0.012)	(-0.005, -0.022)	(0.029, 0.015)	(0.006, -0.012)
White Correction								
Men	(0.006, 0.000)	(0.027, 0.002)	(0.013, 0.004)	(0.023, 0.005)	(0.008, 0.000)	(0.013, 0.001)	(0.010, 0.001)	(-0.004, -0.015)
Women	(-0.010, -0.026)	(-0.025, -0.085)	(0.022, 0.007)	(0.053, 0.022)	(0.027, 0.013)	(-0.004, -0.022)	(0.030, 0.014)	(0.005, -0.011)
Panel B. Clustered Standard Errors								
District								
Men	(0.006, 0.000)	(0.027, 0.001)	(0.015, 0.003)	(0.028, 0.000)	(0.009, -0.001)	(0.015, -0.001)	(0.012, -0.001)	(-0.001, -0.017)
Women	(-0.007, -0.028)	(-0.017, -0.094)	(0.025, 0.004)	(0.061, 0.014)	(0.029, 0.010)	(-0.002, -0.025)	(0.033, 0.011)	(0.009, -0.014)
City								
Men	(0.005, 0.001)	(0.025, 0.003)	(0.013, 0.005)	(0.026, 0.002)	(0.010, -0.002)	(0.015, -0.001)	(0.014, -0.002)	(0.003, -0.022)
Women	(-0.013, -0.022)	(-0.037, -0.074)	(0.021, 0.007)	(0.050, 0.026)	(0.023, 0.016)	(0.000, -0.027)	(0.027, 0.017)	(0.004, -0.010)
Census Wave								
Men	(0.004, 0.002)	(0.016, 0.012)	(0.012, 0.006)	(0.019, 0.010)	(0.006, 0.002)	(0.010, 0.004)	(0.009, 0.002)	(-0.007, -0.012)
Women	(-0.014, -0.021)	(-0.037, -0.074)	(0.020, 0.009)	(0.044, 0.031)	(0.021, 0.018)	(-0.012, -0.014)	(0.028, 0.016)	(-0.002, -0.004)
Cohort								
Men	(0.007, -0.001)	(0.030, -0.001)	(0.013, 0.005)	(0.022, 0.006)	(0.008, -0.000)	(0.016, -0.002)	(0.011, 0.000)	(-0.000, -0.018)
Women	(-0.009, -0.027)	(-0.024, -0.086)	(0.025, 0.004)	(0.055, 0.020)	(0.027, 0.013)	(-0.001, -0.026)	(0.039, 0.005)	(0.009, -0.015)
District, Census Wave								
Men	(0.003, 0.003)	(0.017, 0.012)	(0.014, 0.003)	(0.027, 0.001)	(0.008, 0.000)	(0.014, 0.000)	(0.012, -0.000)	(-0.002, -0.016)
Women	(-0.009, -0.026)	(-0.022, -0.088)	(0.024, 0.004)	(0.059, 0.017)	(0.027, 0.012)	(-0.005, -0.022)	(0.033, 0.011)	(0.007, -0.013)
District, Cohort								
Men	(0.007, -0.000)	(0.029, -0.001)	(0.014, 0.004)	(0.025, 0.003)	(0.009, -0.001)	(0.016, -0.002)	(0.012, -0.001)	(0.000, -0.019)
Women	(-0.008, -0.028)	(-0.021, -0.090)	(0.025, 0.003)	(0.058, 0.017)	(0.028, 0.012)	(-0.000, -0.027)	(0.039, 0.005)	(0.009, -0.015)
District, Census Wave								
Men	(0.006, 0.000)	(0.028, 0.001)	(0.015, 0.003)	(0.028, 0.001)	(0.009, -0.001)	(0.015, -0.001)	(0.012, -0.001)	(-0.001, -0.018)
Women	(-0.008, -0.028)	(-0.018, -0.092)	(0.026, 0.003)	(0.061, 0.015)	(0.029, 0.010)	(-0.002, -0.025)	(0.035, 0.009)	(0.009, -0.014)

Notes. This table reports the effect of childhood exposure to settlements. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old; within each panel, the table reports the estimates for men and women separately. Regressions further exclude individuals above 15 years old when the first settlement house is established in their neighborhood. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. The dependent variable is: one if the individual works (column 1) and the inverse hyperbolic sine of the occupational income score (column 2), an indicator equal to one if they have children (column 3) and the inverse hyperbolic sine of the number of children (column 4), and an indicator if they are married (column 5), married with an immigrant from their same country (column 6), married with an immigrant from a different country (column 7), and married with a native (column 8). All regressions include city-by-census wave, city-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are either unclustered (panel A), or clustered at different levels, as shown in panel B. The table reports 90% confidence bands around the baseline treatment effect estimate. Referenced on page(s) 18, A7. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

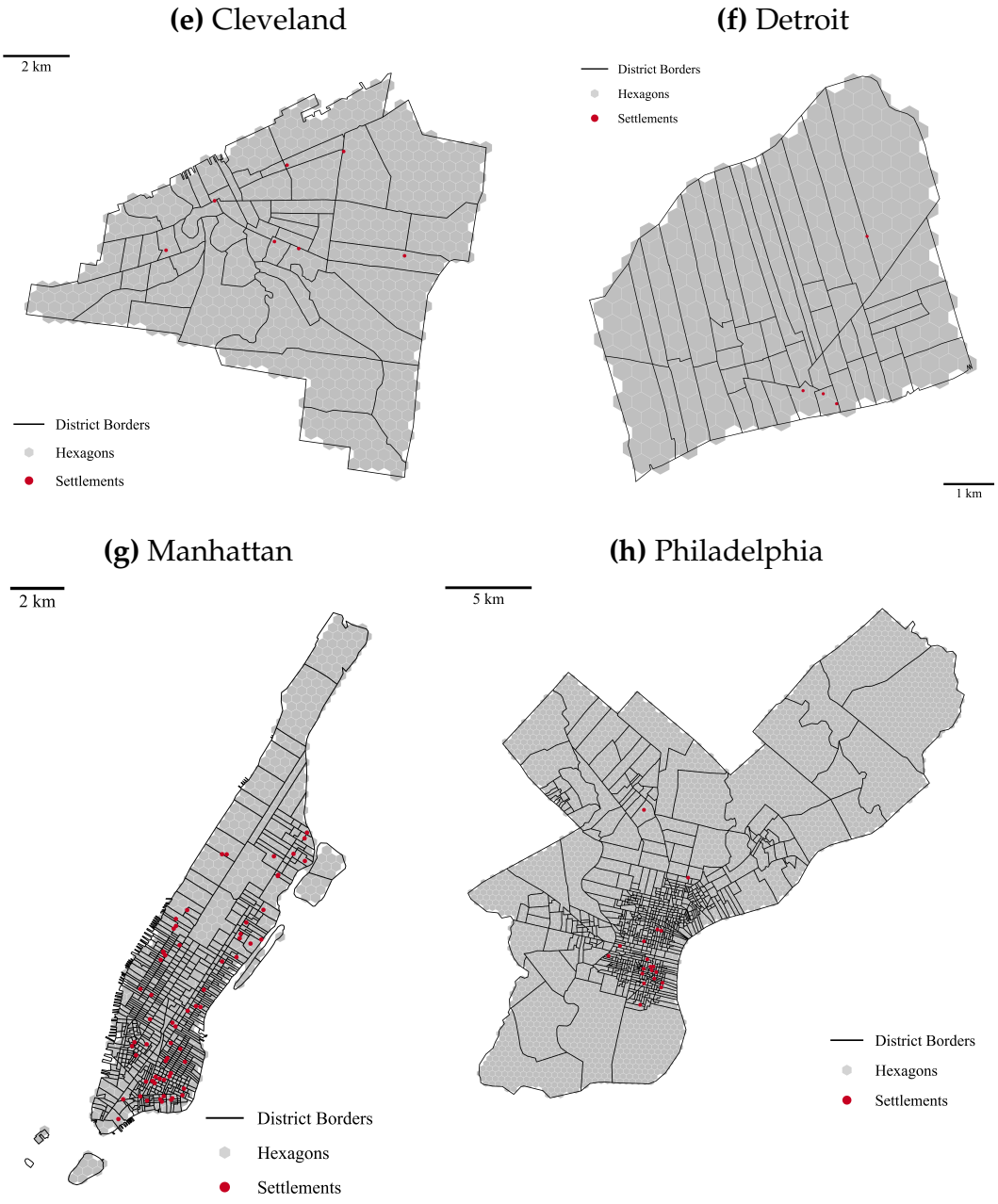
C ADDITIONAL FIGURES

Figure C.1. Spatial Distribution of Settlement Houses Within Cities



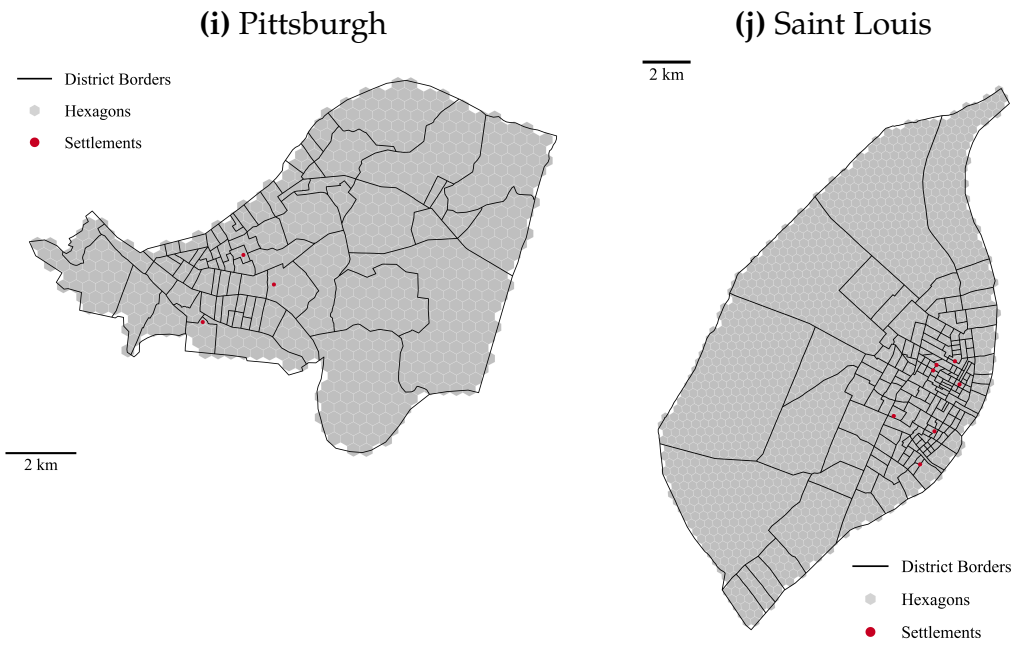
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 10, A7.

Figure C.1. Spatial Distribution of Settlement Houses Within Cities



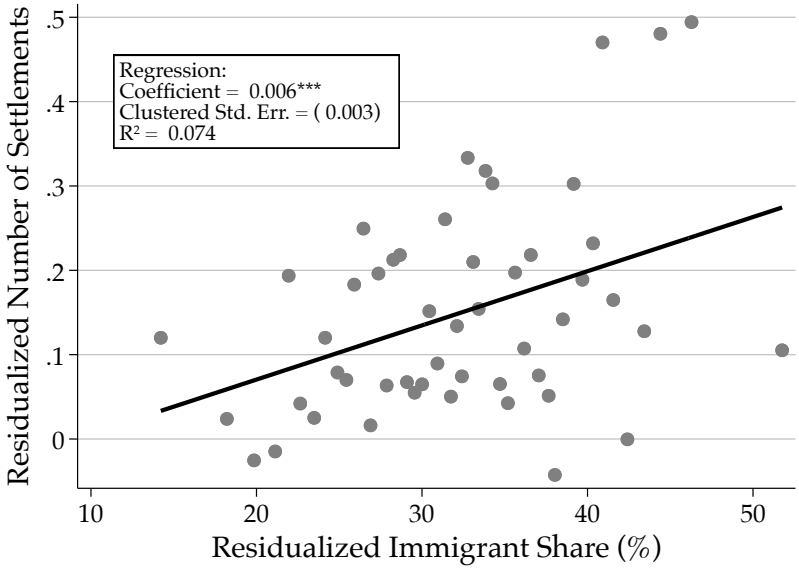
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 10, A7.

Figure C.1. Spatial Distribution of Settlement Houses Within Cities



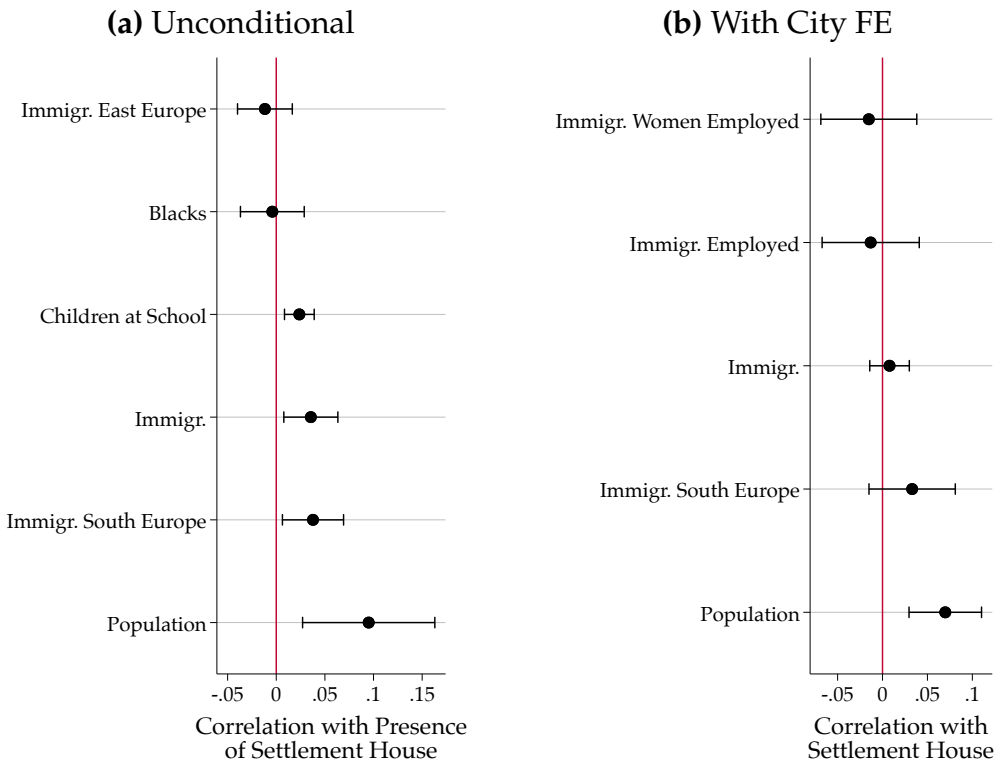
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 10, A7.

Figure C.2. Correlation Between Immigrant Share and Presence of Settlements



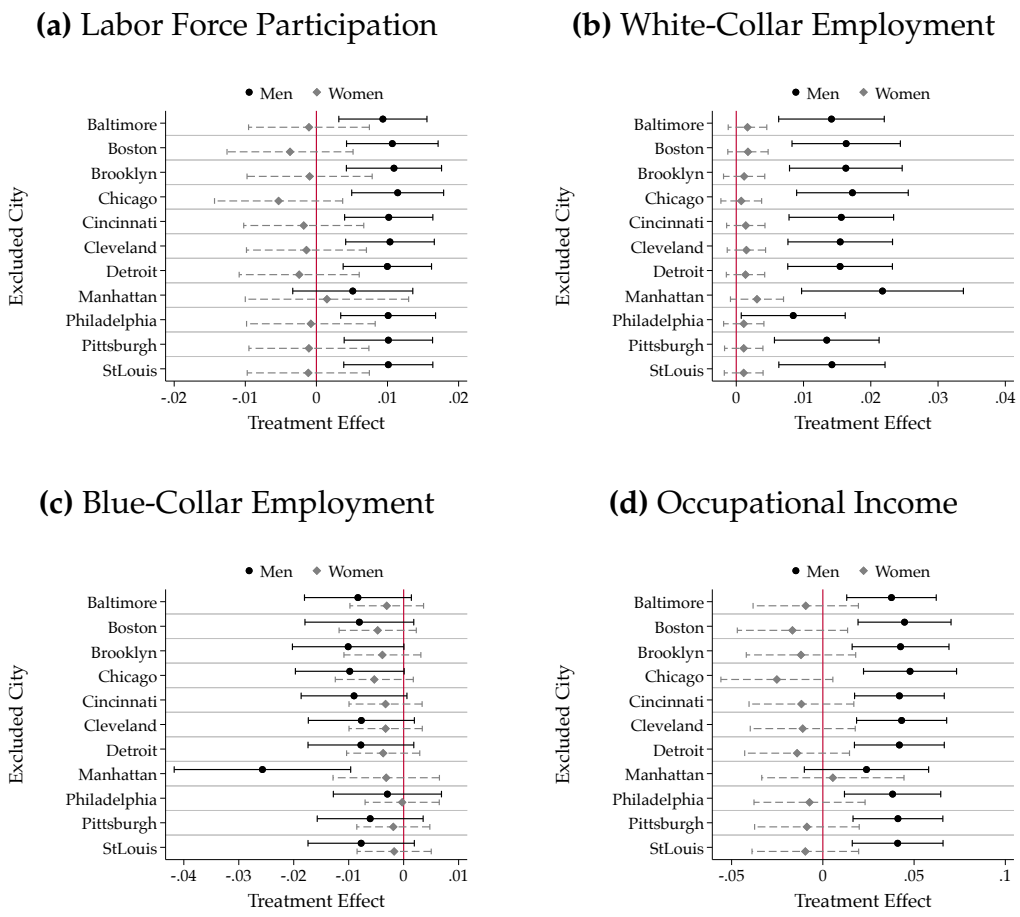
Notes. This figure reports a binned scatter between the number of settlement houses established between 1892 and 1911 (on the y-axis) and the share of immigrants (x-axis). Both variables are residualized against city-fixed effects. The figure reports the coefficient of a regression between the two variables along with its standard error clustered at the neighborhood level and the regression R^2. Referenced on page(s) 15, A5.

Figure C.3. Settlement Presence and Baseline Characteristics: LASSO Variable Selection



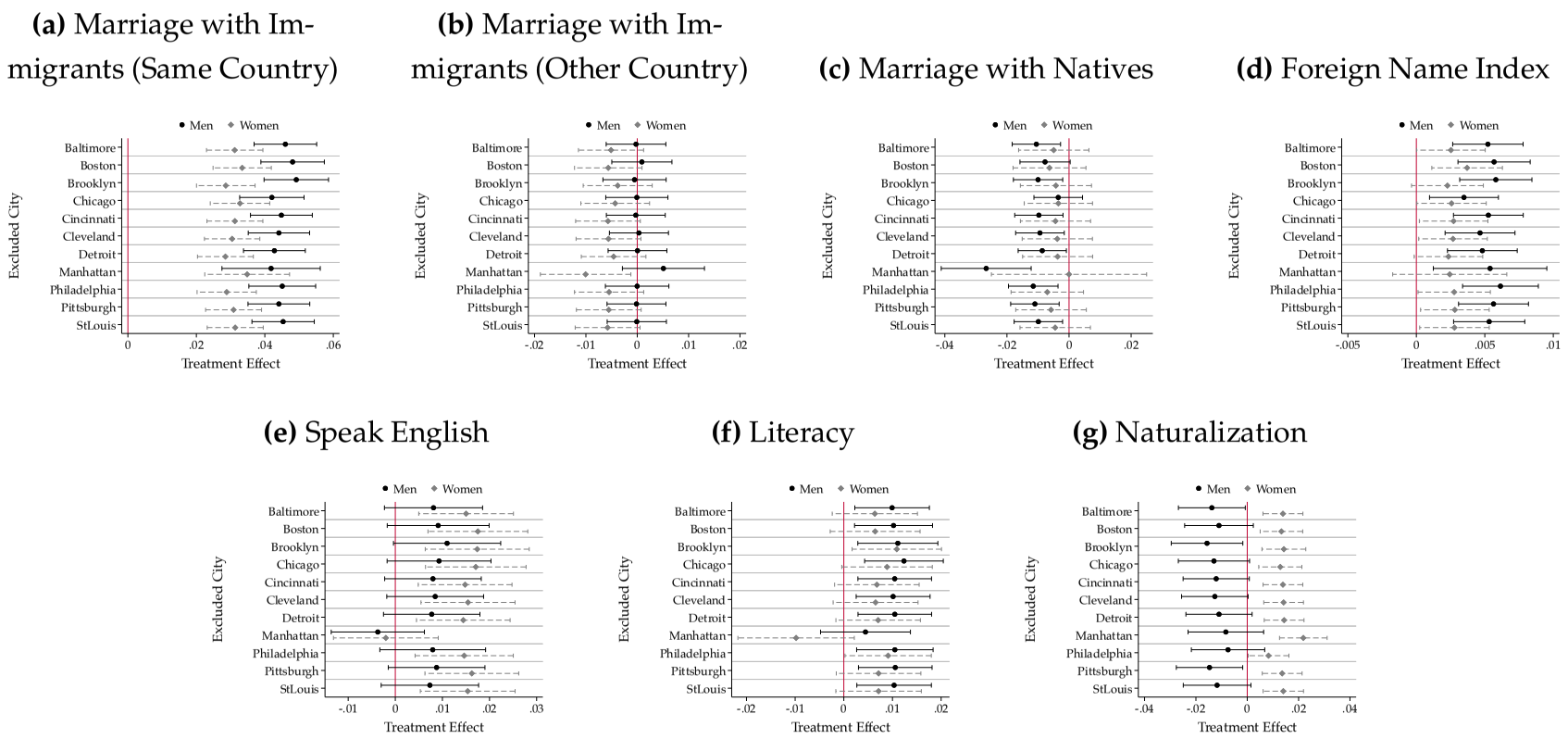
Notes. This figure reports the correlation between the presence of a settlement between 1882 and 1911 and hexagon-level demographic characteristics in 1880. Unlike in figure II, all co-variates are included in a single regression, and the dots report the corresponding coefficients. I employ the LASSO penalized regression to select which variables to include in the regression. Hexagons are weighted by population. In panel C.3a, the variables are constructed over the entire population and expressed as population shares, except for the first row. In panel C.3b, the variables are constructed over the immigrant population and are normalized by the number of immigrants, except for the first row. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 15, A5.

Figure C.4. Leave-out Estimates: Labor Market Effects of Settlement Houses



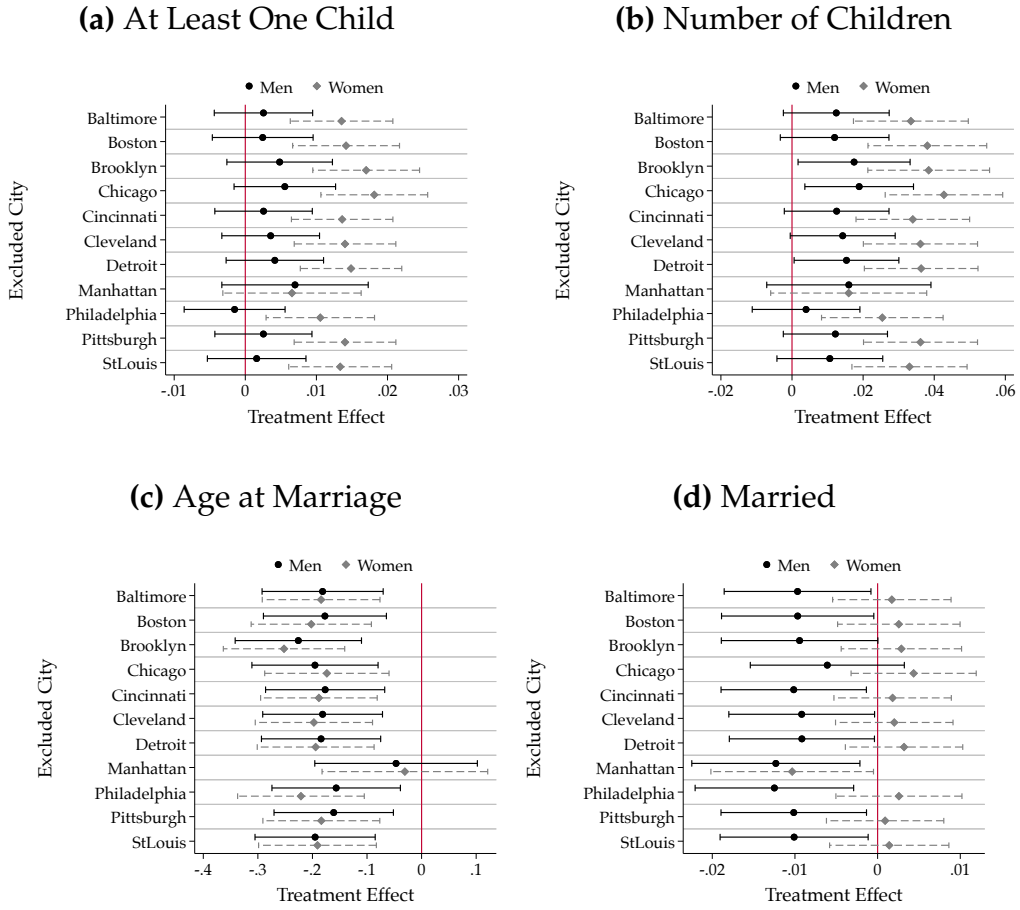
Notes. This figure reports the effect of settlements on labor-market variables dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is labor force participation (panel C.4a), white-collar employment (panel C.4b), blue-collar employment (panel C.4c), and occupational income score (panel C.4d). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

Figure C.5. Leave-out Estimates: Assimilation Effects of Settlement Houses



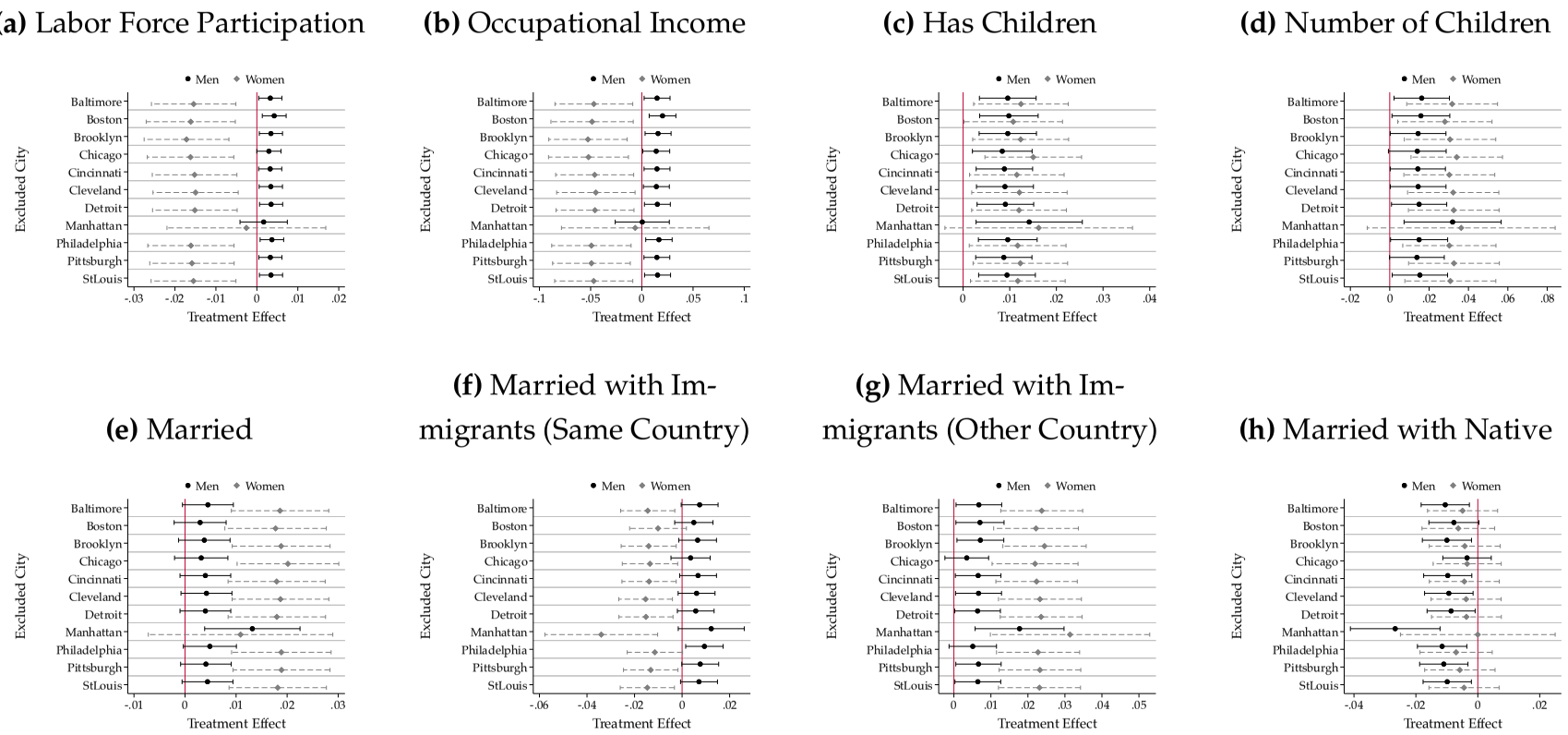
Notes. This figure reports the effect of settlements on assimilation variables, dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is an indicator for individuals married with immigrants from the same country (panel C.5a), from another country (panel C.5b), or natives (panel C.5c), the foreign name index (panel C.5d), an indicator for individuals who can speak (panel C.5e) and write English (panel C.5f), and a dummy for those with naturalized citizenship (panel C.5g). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

Figure C.6. Leave-out Estimates: Family and Fertility Effects of Settlement Houses



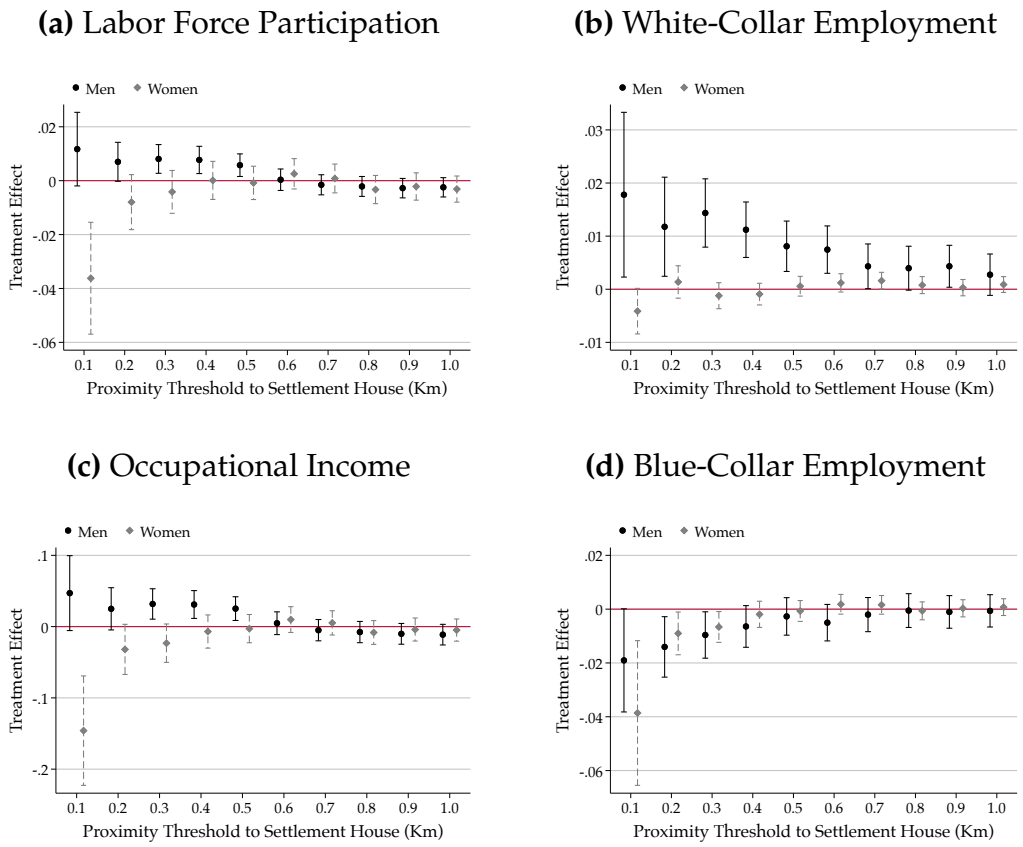
Notes. This figure reports the effects of settlements on family and fertility variables, dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods with a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is one for individuals with at least one child (panel C.6a), the (IHS) number of children (panel C.6b), the age at first marriage (panel C.6c), one for married individuals (panel C.6d). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

Figure C.7. Leave-out Estimates: Intergenerational Effects of Settlement Houses



Notes. This figure reports the intergenerational effects of settlements, dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed in one census between 1920 (i.e., after the last settlement house is established) and 1940. Black dots refer to men; gray dots refer to women. Regressions further exclude individuals above 15 years old when the first settlement house is established in their neighborhood. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. All regressions include city-by-census wave, city-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

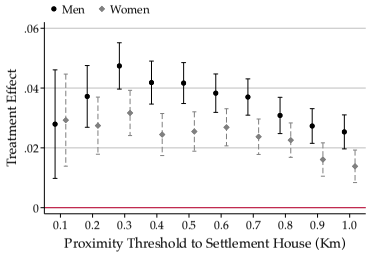
Figure C.8. Robustness to Distance Threshold to Settlement Houses: Labor Market Effects



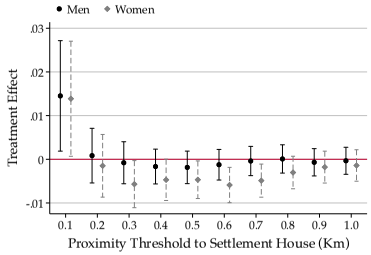
Notes. This figure reports the effect of settlements on labor-market variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the "proximity" of the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods in the "proximity" of a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. To define "proximity," I report ten different distance thresholds between the settlement and the centroid of the neighborhood of the individual. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is labor force participation (panel C.8a), white-collar employment (panel C.8b), occupational income score (panel C.8c), and blue-collar employment (panel C.8d). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

Figure C.9. Robustness to Distance Threshold to Settlement Houses: Assimilation Effects

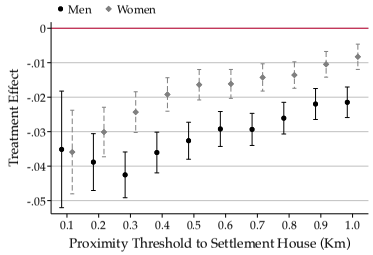
(a) Marriage with Immigrants (Same Country)



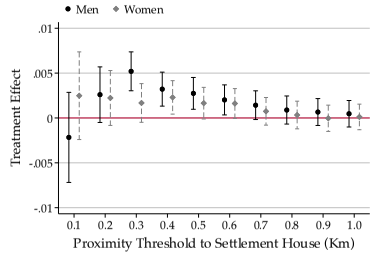
(b) Marriage with Immigrants (Other Country)



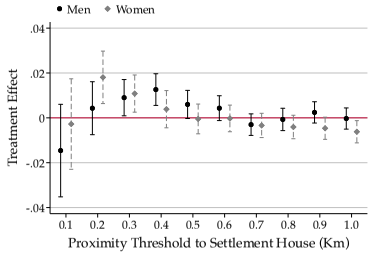
(c) Marriage with Natives



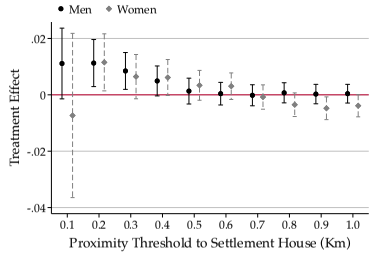
(d) Foreign Name Index



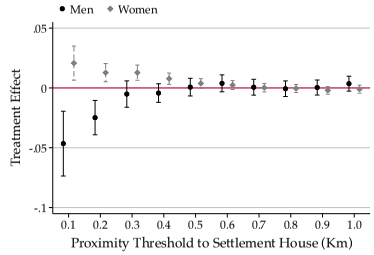
(e) Speak English



(f) Literacy

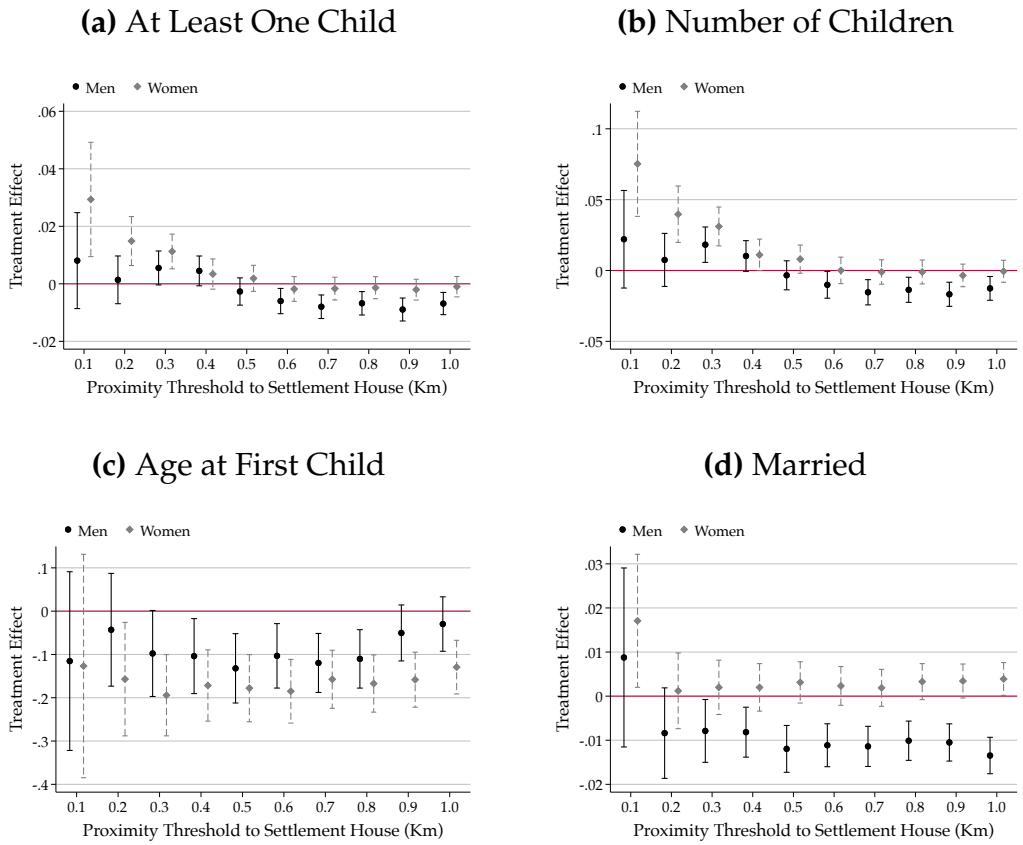


(g) Naturalization



Notes. This figure reports the effect of settlements on assimilation variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the "proximity" of the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods in the "proximity" of a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. To define "proximity," I report ten different distance thresholds between the settlement and the centroid of the neighborhood of the individual. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is an indicator for individuals married with immigrants from the same country (panel C.9a), from another country (panel C.9b), or natives (panel C.9c), the foreign name index (panel C.9d), an indicator for individuals who can speak (panel C.9e) and write English (panel C.9f), and a dummy for those with naturalized citizenship (panel C.9g). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

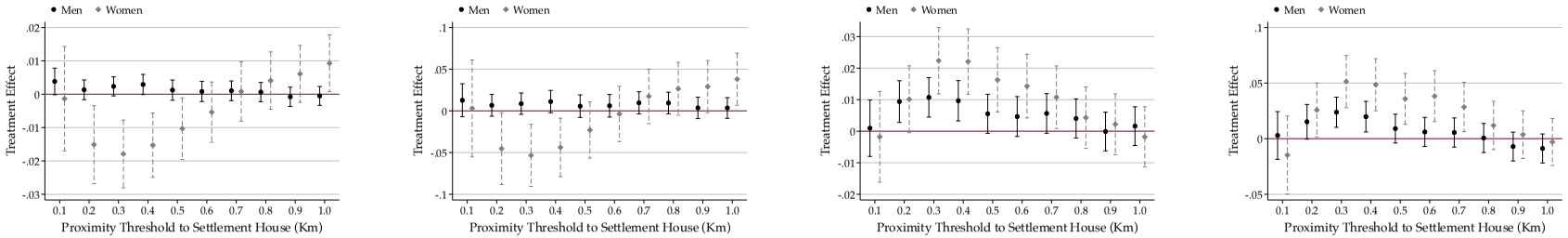
Figure C.10. Robustness to Distance Threshold to Settlement Houses: Family and Fertility Effects



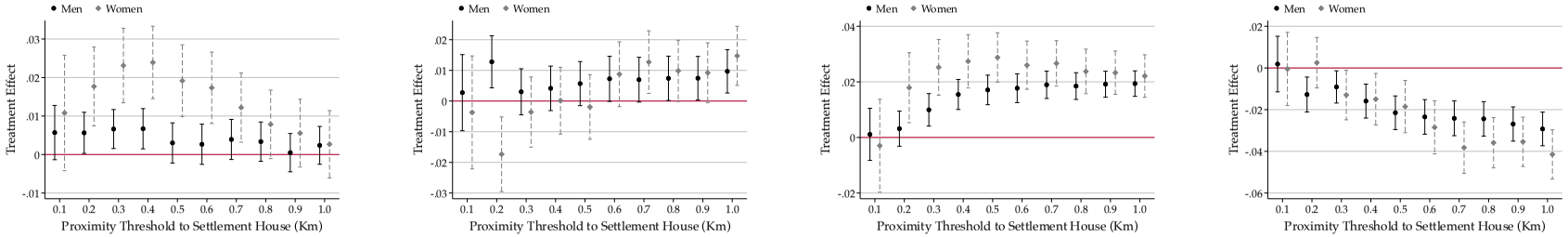
Notes. This figure reports the effects of settlements on family and fertility variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed in one census between 1880 and 1940. The sample comprises all individuals between 15 and 65 years old. The treatment is an interaction of three terms: (i) a dummy for census waves that follow the establishment of a settlement in the "proximity" of the neighborhood where the individual resides in 1900, (ii) a dummy equal to one for neighborhoods in the "proximity" of a settlement house and zero otherwise, and (iii) a dummy equal to one for individuals younger than 40 when the settlement is established. To define "proximity," I report ten different distance thresholds between the settlement and the centroid of the neighborhood of the individual. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is one for individuals with at least one child (panel C.10a), the (IHS) number of children (panel C.10b), the age at first child (panel C.10c), and an indicator for married individuals (panel C.10c). All regressions include neighborhood-by-census wave, neighborhood-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

Figure C.11. Robustness to Distance Threshold to Settlement Houses: Intergenerational Effects

(a) Labor Force Participation **(b)** Occupational Income Score **(c)** Has Children **(d)** Number of Children



(e) Married **(f)** Married with Immigrant (Same Country) **(g)** Married with Immigrant (Other Country) **(h)** Married with Native



Notes. This figure reports the intergenerational effects of settlements using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed in one census between 1920 (i.e., after the last settlement house is established) and 1940. Black dots refer to men; gray dots refer to women. Regressions further exclude individuals above 15 years old when the first settlement house is established in proximity to their neighborhood. The treatment is one for individuals who grew up in neighborhoods in "proximity" to a settlement and zero otherwise. To define "proximity," I report ten different distance thresholds between the settlement and the centroid of the neighborhood of the individual. The dependent variable is labor force participation (panel C.11a), occupational income (panel C.11b), a dummy for individuals with at least one child (panel C.11c), the (IHS) number of children (panel C.11d), a dummy for married individuals (panel C.11e), a dummy for individuals married with immigrants from the same country (panel C.11f), from a different country (panel C.11g), and with natives (panel C.11h). All regressions include city-by-census wave, city-by-cohort, and cohort-by-census wave fixed effects, and individual controls—sex, birthplace, race, and immigration year. Standard errors are clustered at the neighborhood level; bands report 90% confidence intervals. Referenced on page(s) 18, A7.

APPENDIX REFERENCES

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