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 the assimilation of immigrants in the United States between 1880 and 1940. Settlement houses provided services such as education, childcare, and job training. Using a cross-sectional difference-in-differences strategy based on age at first exposure, I find that settlement houses increased labor force participation and income for men but reduced them for women. Childhood exposure increased human capital accumulation and earnings, but only for men. These gendered effects stem from increased in-group marriage and fertility 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 success and cultural assimilation, drawing on the Settlement movement. Robert Putnam (2000) famously listed settlement houses pioneered by Progressive reformers among the most significant social capital innovations in US history.[2] Settlement house volunteers, typically from middle- and upper-class backgrounds, provided critical support to the wave of immigrants who entered the United States during the Age of Mass Migration (1850–1920) and primarily settled in rapidly expanding urban centers, where living conditions were 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, primarily targeted at low-skill manufacturing occupations. Additionally, Progressive reformers explicitly aimed to foster the cultural assimilation of immigrants into American society through language and citizenship classes.

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

[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.

[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 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 measure exposure to settlement houses for immigrants observed in the 1930 and 1940 federal censuses based on their location in 1900. My empirical strategy consists of a cross-sectional difference-in-differences specification that compares immigrants who lived in districts with and without a settlement by age when they were first exposed to settlement houses. My hypothesis is that settlement houses affected relatively young immigrants who would have not yet completed their occupational and fertility decisions when they accessed the services provided by the settlements. The identification parallel assumption thus requires that relatively young and old immigrants, when exposed to settlement houses, would not have displayed diverging trajectories if the settlements had not been established. I assess the empirical plausibility of this assumption throughout the paper.

I find that immigrants younger than 30 when exposed to settlements display a 2% higher rate of labor force participation and a 3% increase in labor income. Additionally, they are 2% 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. Conversely, women exposed to settlement houses were 1.2% less likely to work—a 12% drop relative to the mean—earned 4% less, and were 1% less likely to be employed in white-collar jobs, which corresponds to a 13% drop relative to the mean. The divergence between men and women is confirmed when I estimate the treatment effect by cohort. The estimates provide evidence in support of the parallel trends assumption and indicate that the divergence between genders increases for immigrants exposed to settlements younger.

Cultural assimilation was an explicit purpose of many Progressive-era reformers.[4] My evidence indicates that immigrants exposed to settlement houses—regardless of their gender—were 2% more likely to be naturalized, 3% more likely to speak English, and 2% more likely to show both written and oral English command. However, they were also 3% less likely to marry natives, and the effect is larger for women (4.2%, equal to almost 25% of the mean) than for men (2%, or 10% of the mean). I find no significant effect of settlement houses on the probability of marrying immigrants from other countries. Overall, because marriage rates among exposed immigrants increase—as I detail below—exposure to settlement houses increased the probability of marrying in-group immigrants from the same country. In addition, I follow the methodology to measure cultural assimilation proposed by Abramitzky, Boustan and Eriksson (2020) and find that immigrants exposed to settlement houses give 0.5% more foreign-sounding names to their children. My findings thus provide mixed evidence on the effects of settlement houses on immigrant assimilation.

Why did exposure to settlements hurt women's ability to participate in the labor market? Throughout this period, married women and 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 had more children (a 5% increase), were significantly more likely to marry (by 2%), and the age gap between the husband and the wife—a measure of the bargaining power of the wife within the household—increased by 0.2 years, equal to 1% of the mean.

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

[4] 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).

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 probability of marriage and traditional gender norms is robustly positive. These patterns indicate that gender norms shaped the response of immigrants to settlement houses and, in particular, their implications for women's empowerment.[5]

Finally, I investigate the intergenerational effects on first- and second-generation immigrants exposed to settlement houses during childhood. Childhood exposure to settlements increases the probability of skilled employment by 4% (20% of the mean), labor income by 2%, and educational attainment by .17 years, equal to 5% of the mean. As in the previous analysis, these effects are entirely driven by men, and the effects of childhood exposure on women are either insignificant, as in the case of education, or negative, as with income. These results echo a large literature on the intergenerational transmission of values and norms and indicate that the beneficial effects of settlements on men transmitted to the younger generation but did not spill over to women (Bisin and Verdier, 2001; Fernández, Fogli and Olivetti, 2004). Early exposure increases the probability of marrying immigrants from other countries by 4% and decreases marriages with natives by 3.5%, suggesting that settlement houses fostered intergroup contact among immigrants from different countries.

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,

[5]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.

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 into American society.[6]

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 is associated with increased segregation 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.

[6]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 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.

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.[7] An overwhelming majority of volunteers in settlement houses,

[7] 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.

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 children 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).[8] 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,

[8]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.

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. 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.[9]

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

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

digitize the entire book, which covers 411 settlement houses.[10]

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 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.[11] 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,

[10] 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.

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

Goeken, Schroeder, Sobek *et al.*, 2024).[12] 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.[13] To study family and fertility decisions, I consider the probability of marriage, an indicator of whether an individual has at least one child, and the IHS of the number of children.[14] 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.[15]

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

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

[14] 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 that left the household before. I leverage intergenerational linked data to circumvent this issue.

[15] 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.

III.C Construction of the Samples

I construct three 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 two are individual-level cross-sections of individuals in 1930 and 1940. In this section, I explain how I construct these three 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 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 districts, 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 to 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.1 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 Sections

To explore the effects of settlement houses on immigrants' welfare, I restrict the attention to immigrants already in the United States when the Settlement movement emerged in 1900. The first dataset is a cross-section of immigrants living in one of the sample cities compiled from the 1930 population census. I link these individuals to 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.[16] The second dataset is analogous, except that it is compiled from the 1940 population census to study how those who were children when settlement houses were being established fared as adults. 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).

In both datasets, I observe the enumeration district where each individual lived in 1900. I thus 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.

Panels B and C of Appendix Table B.1 provide sample statistics for the 1930 and 1940 datasets 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.[17] 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.

IV.A Descriptive Evidence on Settlement Activity

Appendix Table B.2 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. Settle-

[16] 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.

[17] 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.

ments 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 city 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.3 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.[18] 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 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.[19]

[18] 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*.

[19] In Appendix figure C.3, I use LASSO to select the most relevant predictors of the establishment of settlement presence and

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, although I also report the coefficients of an analogous regression that substitutes city-by-decade with 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. 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 city level. 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 plau-

compute their correlation with the presence of a settlement. The estimates confirm the patterns highlighted here.

sibility 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.[20]

Figure III presents the results of the flexible difference-in-differences model (2). The black and gray dots report the estimates without and with city-by-decade fixed effects. 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. 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.4 displays the shift in the mean share of immigrant mothers (column 1), the num-

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

ber 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.[21]

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. Finally, I evaluate the effects of settlement houses on those who spent their childhood in their proximity.

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, which consists of a cross-section of first-generation immigrants compiled from the 1930 federal census. Using the intergenerational links constructed by the Census Tree Project, I follow those individuals back to the 1900 census to observe the enumeration district where they lived at the onset of the Settlement movement. I consider a neighborhood as "exposed" to settlement houses if at least one settlement is established within 250 meters (0.15 miles) from its centroid.[22] In this part of the analysis, I focus on the effects of settlements on adults. Hence, the sample only includes those aged 15 or above when their neighborhood is exposed to a settlement.[23]

[21] By contrast, in Appendix table B.5, I show that the non-migrant population shrunk in areas with settlements.

[22] 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.

[23] 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 increasing the threshold 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.

I estimate variations of the following empirical specification:

$$y_i = \alpha_{d(i)} + \alpha_{c(i)\times t(i)} + X_i'\Gamma + \sum_{\substack{k=15 \\ k\neq 35}}^{45} \beta_k \times I\left(t(i) - \tau_{d(i)} = k\right) + \varepsilon_i, \tag{3}$$

where i denotes an individual born in year $t(i)$ who, in 1900, lived in enumeration district $d(i)$ in city $c(i)$. The terms $\alpha_{d(i)}$ and $\alpha_{c(i)\times t}$ denote district and city-by-cohort fixed effects. The vector X_i contains a set of individual-level controls measured from the 1900 census: country of origin, marital status, relationship to the household head, race, gender (when applicable), and year of immigration. The variable $\tau_{d(i)}$ is the time when district $d(i)$ is first exposed to a settlement house. The terms $I(\cdot)$ denote a set of dummy variables that code the age of individual i when the settlement was established.[24] The age is binned into five-year windows, and the 35-40-year-old bin serves as the baseline category because 30 was approximately the mothers' median age at conception. Individuals living in districts without settlements are thus part of the control group. As in the rest of the analysis, standard errors are clustered at the city level.[25]

I also estimate variations on (3) where I substitute the event-time dummies with a $Young_i$ treatment variable such that $Young_i \equiv I\left(t(i) - \tau_{d(i)} \leq 35\right)$. In this case, the cross-sectional difference-in-difference estimator compares individuals younger than 35 when exposed to a settlement with older individuals and those not exposed to a settlement in the sample period.

The motivating rationale behind specification (3) is that the activities offered by settlement houses targeted relatively young immigrants. Kindergarten and nursery services benefitted young households. Professional classes similarly targeted 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. These conjectures motivate using the birth year as the timing variable in the cross-sectional difference-in-differences analysis.

Regression (3) allows me to evaluate the empirical plausibility of this claim, which implies that I should not detect statistically significant effects of settlement houses on relatively older immigrants. In most cases, my estimates confirm this prediction, and, in all cases, the magnitude of $\hat{\beta}_{40,45}$ is significantly smaller than for younger cohorts. This pattern provides supporting evidence for the identification assumption that, in the absence of the settlement houses, younger and older individuals would

[24] For immigrants who were not yet in the United States when the settlement opened, the age of exposure to the settlement is instead computed as their age when they immigrated and, thus, acquired access to the services provided by the settlement.

[25] In Appendix Figures C.12, C.13, C.14, and C.15, I show that the statistical significance of the estimates remains very similar when using alternative estimators for the standard errors.

not have displayed diverging trajectories in 1930.

A concern for regression (3) is that the non-random location of settlement houses across districts and the presence of residential segregation jointly imply that settlement houses targeted different individuals.[26] While the small and often statistically insignificant pre-treatment coefficients support the identification assumption, I cannot rule this concern out entirely. 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.[27] This procedure ensures that treatment and control individuals have similar probabilities of being treated and yields very similar results to the baseline estimates.[28]

V.B 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-cohort effect of settlement houses on labor force participation (Panel IVa), and the occupation-based income proxy (Panel IVb). Each dot reports the estimate of one coefficient $\hat{\beta}_k$ in regression (3); 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 for labor force participation and income. Men exposed to settlement houses in their twenties and early thirties are more likely to work in 1930 by approximately 5%, which corresponds to approximately 6.3% of the average labor force participation rate among men. On the other hand, women exposed to settlement houses at the same ages are approximately 2% less likely to have a job in 1930. This effect, while seemingly small, is large compared to an average employment probability of 9.7%, as it corresponds to a 20.1% drop in the likelihood of working. Income follows a similar pattern. Men earn approximately 7% more in 1930 when they are exposed to settlement houses before their late thirties. Women, on the other hand, earn 4% less. Reassuringly, the association between the presence of settlements and labor force participation and income for older cohorts is either statistically insignificant or quantitatively small.

Table II reports the estimates of a variant of specification (3) where I conflate treated and untreated cohorts 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 specification, which controls for neighborhood and city-by-cohort

[26] For a review of recent evidence on residential segregation, see Logan and Parman (2022).

[27] To compute the propensity scores, I include multiple variables that could influence the probability of being exposed to a settlement. These are the city, gender, race, country of origin, year of immigration, literacy status, married status, and relationship with the household. Importantly, all variables are measured in 1900.

[28] Appendix section A.II describes the additional robustness exercises mentioned in passing in the main text.

fixed effects and individual-level characteristics measured at baseline in 1900. Online Appendix table B.6 reports the estimates obtained from other specifications, including, in column (4), 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, whereas women are less likely to work in 1930 when they are exposed to settlement houses (panel C). Income follows a similar trajectory. In column 2, I focus on "high-skill" employment, that is, more skill-intensive occupations that presumably required a higher level of formal education.[29] The presence of settlement houses does not impact the probability of working in a high-skilled occupation. Presumably, even though settlement houses offered educational classes, high-skill occupations required skills that working-age immigrants could not acquire in settlement houses.

In columns (3) and (4), I split employment into white- and blue-collar occupations. White-collar occupations include liberal and managerial occupations, clerical and sales workers, and those employed in services. Blue-collar occupations, on the other hand, consist of low- and middle-skilled manufacturing jobs and farm laborers. White-collar employment increases among individuals exposed to settlement houses when young (panels A and B). Settlement houses, as discussed previously, offered educational services. While these could not build up the human capital necessary to take up high-skill occupations (column 2), my estimates suggest that they positively impacted the probability of obtaining a white-collar job. The estimates split by gender, however, reveal a substantial gender divide. Men exposed to settlement houses when young are, in 1930, 1.2% more likely to be employed in a white-collar occupation, which corresponds to a 3% increase with respect to the mean. Women, on the other hand, are 1% less likely to have a white-collar job. Since the share of women in white-collar occupations is only 7.5%, the average treatment effect corresponds to a 13% drop relative to the mean. On the other hand, settlement houses do not significantly affect the probability of being employed in a blue-collar manufacturing occupation (column 5). Since white-collar jobs commanded, on average, a higher imputed income (29.78) than blue-collar occupations (25.60), the shift to white-collar jobs explains part of the overall increase in income for men.

Settlement houses offered training and educational classes that could benefit the immigrants' employment probability and their ability to take up 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

[29] Consistently with this interpretation, the average imputed income of individuals employed in high-skill occupations (40.9) is almost twice that of individuals employed in other occupations (26.01).

white-collar, hence higher-skilled, jobs. These beneficial effects, however, are driven by men. Women exposed to settlement houses experienced deteriorating labor market conditions in terms of lower labor force participation, lower income, a lower probability of obtaining white-collar jobs, and, to some extent, blue-collar ones. In section VI, I explore some likely drivers of these gender differences.

V.C 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 of the flexible specification (3) using the probability of speaking English (panel IVc) and the probability of being naturalized (panel IVd) as the dependent variable. Exposure to social settlements during early adulthood results in statistically significantly higher probabilities of speaking English and being naturalized as a US citizen in 1930. The effect is larger for individuals exposed to settlement houses when younger, as this plausibly reflects a longer exposure to the settlements themselves and the community they gathered. Quantitatively, men (resp. women) exposed to settlement houses when they were between 15 and 20 are, in 1930, 4% (resp. 2%) more likely to speak English. These correspond to approximately 5% (resp. 2.5%) of the relevant population's mean. The probability of naturalization for this same group increases by 5% for women and 3% for men, corresponding to 6.3% and 1.4% of the average naturalization rates among immigrant women and men, respectively. By comparison, individuals exposed to settlement houses later in life do not display any systematic response in terms of English proficiency and naturalization rates.

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) and (2), I replicate the results of the flexible specification using the probability of being naturalized and speaking English. The probability of naturalization and speaking English increases for individuals exposed to settlement houses during their early adulthood (columns 1–2) irrespective of gender (panels B and C). In column (3), I broaden the scope of English proficiency to the command of the written language and find that the literacy rate, defined as the ability to speak *and* write in English, similarly increases following the exposure to settlements by 1.7% for men and 2.1% for women. These effects are quantitatively modest since most of the population was literate and correspond to 1.8% and 2.3% of the relevant populations' means.

While these patterns suggest that settlement houses fostered the assimilation of the immigrants, in columns (4) and (5), I explore the impact of settlement houses on the probability of marrying a US

native citizen and another 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, indicate 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.

Exposure to settlement houses decreased the probability that immigrants married natives by approximately 1.5% for men and 4.3% for women (column 4). These effects correspond, respectively, to 5.3% and 19.5% of the mean. I find no statistically significant evidence that settlement houses impacted the probability of marrying immigrants from other countries (column 5). Because the overall marriage probability for individuals exposed to settlement houses increased—see section VI—, settlement houses increased the probability of in-group marriages. From this perspective, settlement houses did not foster assimilation but rather further segregation along ethnic lines. This effect is more pronounced for women, who were already less likely to marry out-group members.

In a similar spirit, panel F reports the effect of settlement houses on the Foreign Name Index (FNI) of Abramitzky *et al.* (2020).[30] 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 (column 6). The effect is stronger for men (panel B) than women (panel C), although this may indicate men's higher bargaining power in naming decisions rather than women's preference for less non-native-sounding names.

Appendix Table B.7 replicates the results employing different layers of fixed effects, controls, and the propensity score weighting scheme. All results remain qualitatively unchanged.

[30] Formally, I follow Abramitzky *et al.* (2020) and define the FNI 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.

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 segregated women from the labor market. Naturalization rates and language proficiency of immigrants increased in response to settlement houses but so did in-group marriage probabilities and, plausibly, overall segregation along ethnic lines.

V.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. In this section, I thus focus on individuals who were exposed to settlement houses during childhood and study their later-in-life trajectories.

I employ data compiled from the 1940 census and linked to the 1900 census. The sample comprises all those born after 1890. I include all first- and, unlike the previous analysis, second-generation immigrants younger than 15 when they were first exposed to a settlement house, depending on the neighborhood where they lived in 1900.[31] I estimate variations on the following specification:

$$y_i = \alpha_{c(i)\times t(i)} + X_i'\Gamma + \beta \times Settlement_{d(i)} + \varepsilon_i, \tag{4}$$

where i denotes an individual born in year $t(i)$ in city $c(i)$. As in (3), I include city-by-cohort fixed effects ($\alpha_{c(i)\times t(i)}$) to leverage within-city variation. The term X_i collects the same controls as the previous analysis: country of origin, marital status, relationship to the household head, race, gender (when applicable), and immigration year, all measured in 1900. 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. As in the rest of the empirical analysis, standard errors are clustered at the city level.

In (4), 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 in 1940. 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

[31] I exclude individuals born before 1890 because they would not be children in 1900. I include second-generation immigrants because settlement houses targeted immigrants, hence also their children, even if they were not born outside of the United States.

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 IV reports the results on labor force participation (column 1), high-skill employment (column 2), imputed labor income (column 3), educational attainment (column 4), and out-group marriage probabilities, namely, with natives (column 5) and immigrants from other countries (column 6). Appendix Table B.8 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 (column 1). On the other hand, the probability of high-skill employment is substantially higher (3.2%) among individuals exposed to settlement houses during childhood. This difference corresponds to approximately 18.5% of the mean (panel A, column 2). The breakdown by gender, however, reveals that the high-skill employment rate among men (panel B) increased by 4%, or 19% of the mean, whereas it did not change among women (panel B). Labor income analogously increased by 2% among men (column 3, panel B) and, if anything, decreased among women (column 3, panel C), even if not statistically significantly so.

The 1940 census is the first to contain detailed information on educational attainment. Since settlement houses had clear educational purposes, in column (4), I focus on the association between exposure to settlement houses during childhood and completed educational attainment. The educational attainment of individuals who grew up near settlement houses is considerably higher. Growing up close to settlement houses results in 0.12 additional years of education (panel A), which corresponds to a 4% increase relative to the mean. Once more, however, this aggregate pattern conceals substantial heterogeneity between genders. Men display, on average, 0.17 additional years of education (panel B), equal to 5% of the mean. In contrast, I find no statistically significant effects of settlement houses on the educational attainment of women (panel C). It is plausible that higher educational attainment enhanced the ability of men to take up high-skill, hence education-intensive jobs, whereas women did not benefit from this human capital accumulation effect.

These results indicate that the gender disparities generated by the presence of settlement houses among adults—seen in sections V.B-V.C—trickled down onto their children. The wedge between men and women in education and, consequently, skill-intensive employment and income widened as the positive effects of settlement houses accrued solely to men. 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).

In columns (5) and (6), I explore the association between exposure to settlement houses during childhood and the likelihood of marrying natives and immigrants from other countries. Individuals ex-

posed to settlement houses were 4% more likely to marry immigrants from other countries and, on the other hand, 4% less likely to marry native-born citizens. In both cases, the effect is larger for women (panel C) than for men (panel B), even though the estimates are both statistically significant. These correlations 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. In unreported regressions, I find that childhood exposure to settlement houses has a positive but statistically insignificant ($p = 0.124$) effect on the Foreign Name Index of second-generation immigrants.

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. Then, I study how those responses vary in terms of the gender norms of the various immigrant groups. I conclude by providing a complementary perspective of settlement houses as a coordination device for public goods provision in ethnically diverse neighborhoods.

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.[32]

To estimate settlement houses' fertility and marriage impacts, I employ the 1930 dataset linked to

[32]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).

the 1900 census used in sections V.B-V.C. I estimate regression (3) using the probability of having children, the IHS of the number of children, and the marriage probability as the outcome variables. The sample is the same as in the previous analysis. I report the results in Table V. Column (1) refers to the probability of having children: the dependent variable is equal to one if the individual has at least one child living in their household in 1930 and zero otherwise. The estimates indicate a positive effect of settlement houses on the probability of having at least one child, even though the coefficient is quantitatively small (a 1.7% increase) relative to the average (0.9).

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 4% increase in the number of children (panel A). The effect is larger for women (5%, panel C) than for men (3%, panel B). Panel IVe in Figure IV reports the associated estimates where I compute the association between the presence of settlement houses and the number of children by age cohort windows. The number of children increases for men and women exposed to settlement houses before the 35-40 age bin, and the effect is more precisely statistically significant for individuals treated younger.

My results indicate that the effect of settlement houses on fertility was 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.

Marriage was a critical factor in shaping the labor supply decisions of women in this period. First, prevailing social norms heavily discouraged married women 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 or mandate that women be fired upon marrying (Goldin, 2021).[33] In column (3), I thus report the effect of exposure to settlement houses on the probability of marriage. I estimate a positive, robust effect of settlement houses on marriage rates. Individuals exposed to settlement houses are 1% more likely to marry, which corresponds to approximately 1% of the average; the effect is larger for women (1.1%) than men (0.8%). Panel IVf reports the associated cohort-level estimates. These reveal that the effect of settlement houses is larger for younger individuals at the time of exposure. Immigrants below 20 years old when the settlement is established are 2% more likely to marry, as opposed to a 0.5% higher probability among older cohorts. There is no statistically significant association between settlement houses and the probability of marriage of individuals who

[33] 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).

were exposed when they were older than 35 and had plausibly completed their marriage decision.

The age at marriage and the gap between the age of the husband and the wife can be interpreted as a measure of the women's agency within households and pre (e.g., see Field and Ambrus, 2008; Buchmann, Field, Glennerster, Nazneen and Wang, 2023). In panel D, I thus explore how the age at marriage reacts in response to the establishment of settlement houses.[34] In column (4), I estimate that the age at first marriage of immigrants exposed to settlement houses increases by approximately 0.2 years or, equivalently, 1% of the mean. The effect, however, is entirely driven by men (panel B), whereas I find no change in women's age at marriage (panel C). This response plausibly emerges due to higher labor force participation rates among men following the establishment of settlement houses. These patterns imply that the age gap between the husband and the wife increases.

Appendix Table B.9 reports the estimated effect of exposure to settlement houses on family and fertility 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 were more likely to marry and have more children. Moreover, they married older men, who were more likely to work and earn more. 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.

To provide more evidence in this direction, in Appendix Table B.10, I explore the heterogeneous treatment effects of settlements depending on the services they provide. Specifically, I contrast childcare and professional training services. I estimate the baseline model (3) and interact the treatment term with binary indicators for individuals exposed to settlements that provided (i) professional training but no childcare, (ii) childcare but no professional training, and (iii) both. My argument that settlements exacerbated the segregation of women into housework by providing training to men and reducing the cost of childcare requires that their effect should be larger when both services—childcare and professional training—are effectively provided. The results corroborate this hypothesis. Settlements have positive labor-market effects on men (columns 1 and 3) and negative effects on women (columns 2 and 4), especially when settlements provide training and childcare services. Similarly, the increase in marriage rates (columns 5–6) and the number of children (columns 7–8) are larger where settlements provided childcare *and* professional training.

[34] It is worth noting that, in this case, the sample excludes unmarried individuals.

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.[35]

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.[36] Second, I measure exclusionary gender roles as the total fertility rate in 1900. This approach 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.[37]

To explore how gender roles shaped the immigrants' responses to settlement houses, I employ the baseline 1930 dataset linked to the 1900 census. I estimate regression (3) and interact the baseline treatment with country-of-origin indicators:

$$y_i = \alpha_{d(i)} + \alpha_{c(i)\times t(i)} + \alpha_{o(i)} + X_i'\Gamma + \sum_{\omega} \beta^{o} \times I\left(t(i) - \tau_{d(i)} \leq 35\right) \times I\left(o(i) = \omega\right) + \varepsilon_i, \tag{5}$$

[35] 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.

[36] 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.

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

where, as in the previous analysis, i denotes an individual who, in 1900, lived in district $d(i)$ in city $c(i)$ and was born in year $t(i)$. The term X_i collects the same individual-level controls of (3)-(4): marital status, relationship to the household head, race, gender (when applicable), and year of immigration. Term $\alpha_{o(i)}$ denotes fixed effects for the immigrants' 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.[38] The term $I\left(t(i) - \tau_{d(i)} \leq 35\right)$ is the analog of the time dummies in (3) and considers as treated all individuals younger than 35 when their district was first exposed to a settlement house (in $\tau_{d(i)}$). Finally, $I\left(o(i) = \omega\right)$ is an indicator equal to one for immigrants born in country ω and zero otherwise. As in the rest of the paper, standard errors are clustered at the city level. Since this analysis focuses on women, I exclude men from the estimation sample.

Figure V reports the estimates of $\hat{\beta}^o$ 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 marriage probability (panels Vb and Vd).[39] 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 as France and Sweden—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 probability of marriage 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

[38] Country-of-origin fixed effects are included in all specifications. In (3), they are part of X_i. In (5), however, I include them more explicitly to emphasize that I control for such time-invariant factors.

[39] Appendix table B.11 explores the heterogeneous treatment effects on all labor-market outcomes for the two measures of gender norm conservatism.

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.[40] 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 in the top tercile as "highly diverse."[41] Then, I estimate the baseline regression (3) on the adult sample of immigrants observed in 1930 and linked to the 1900 census. I include an additional interaction term between the baseline treatment $I\left(t(i) - \tau_{d(i)} \leq 35\right)$ 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.

Table VI reports the results. I use a set of dependent variables that convey the general findings documented in the previous sections for labor market outcomes (columns 1–2), marriage and fertility (columns 3–4), and assimilation patterns (columns 5–6). In panel A, I look at the full population, whereas panels B and C focus on the male and female subsample. 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

[40] 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.

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

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.

VII CONCLUSIONS

An extensive literature views social capital as conducive to 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 the immigrants.

I study the Settlement movement, a characterizing 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. Exposure to settlement houses positively affected immigrants' labor force participation and income. These effects, however, are entirely driven by men. Women display lower participation in the labor market and, consequently, lower labor income. Settlement houses increased basic human capital indicators, such as literacy and naturalization rates, but decreased marriages between migrants and natives. These effects spilled over to the next generation. Those who grew up near settlement houses were more likely to hold skill-intensive occupations, earn more, and have higher educational attainment, but men entirely drive these effects. However, they were also 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, were more likely to marry, and the age gap between the husband and the wife increased. In this period, married women and mothers 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. Consistently 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, 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***
	(950.796)	(0.032)	(0.022)	(0.407)	(0.022)	(0.018)
Mean Dep. Var.	1097.266	0.281	0.483	13.428	0.238	0.315
Panel B. Men						
Post Establishment of Settlement	2049.745***	0.136***	-0.218***	-5.929***	-0.195***	0.085***
	(498.970)	(0.031)	(0.033)	(0.589)	(0.028)	(0.025)
Mean Dep. Var.	570.303	0.287	0.718	22.569	0.310	0.536
Panel C. Women						
Post Establishment of Settlement	1495.500***	0.121***	-0.021	-0.031	-0.054**	0.039**
	(455.846)	(0.033)	(0.025)	(0.399)	(0.022)	(0.017)
Mean Dep. Var.	526.964	0.273	0.233	3.879	0.160	0.076
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 city level. Referenced on page(s) 16.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table II. The Labor Market Effects of Settlement Houses

	Labor Force Participation	High-Skill Employment	Occupational Income	White-Collar Employment	Blue-Collar Employment
	(1)	(2)	(3)	(4)	(5)
Panel A. Entire Population					
Young × Settlement	0.006**	0.004	0.030***	0.004	0.002
	(0.002)	(0.003)	(0.008)	(0.003)	(0.003)
Number of Individuals	553,521	553,521	553,521	553,521	553,521
Mean Dep. Var.	0.460	0.092	1.805	0.214	0.244
Panel B. Men					
Young × Settlement	0.018***	0.006	0.068***	0.012**	0.006
	(0.004)	(0.006)	(0.017)	(0.005)	(0.004)
Number of Individuals	300,560	300,560	300,560	300,560	300,560
Mean Dep. Var.	0.765	0.157	3.054	0.330	0.431
Panel C. Women					
Young × Settlement	-0.012***	-0.002	-0.035**	-0.009***	-0.002
	(0.004)	(0.001)	(0.014)	(0.003)	(0.001)
Number of Individuals	252,868	252,868	252,868	252,868	252,868
Mean Dep. Var.	0.097	0.014	0.321	0.075	0.021
Neighborhood FE	Yes	Yes	Yes	Yes	Yes
City-Cohort FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	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 once in the 1930 census. In panel A, the sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any; in panels B and C, the sample excludes women and men, respectively. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. The dependent variable is one if the individual works (column 1), and if they work in a highly skilled occupation (column 2), the (IHS) occupation-based imputed income (column 3), and is equal to one if the individual is employed in a White collar (column 4) or Blue collar (column 5) manufacturing occupation. All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 19.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table III. The Assimilation Effects of Settlement Houses

	Naturalized Citizen	Speak English	Literacy	Married Native	Married Immigrant	Foreign Name Index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Entire Population						
Young × Settlement	0.021**	0.016***	0.020***	-0.029***	0.003	0.003**
	(0.007)	(0.003)	(0.003)	(0.006)	(0.003)	(0.001)
Number of Individuals	553,521	553,521	553,521	422,999	422,999	385,059
Mean Dep. Var.	0.810	0.948	0.930	0.225	0.103	0.526
Panel B. Men						
Young × Settlement	0.023**	0.017***	0.017***	-0.020***	0.002	0.003*
	(0.008)	(0.002)	(0.004)	(0.003)	(0.005)	(0.001)
Number of Individuals	300,560	300,560	300,560	254,993	254,993	211,408
Mean Dep. Var.	0.824	0.963	0.944	0.265	0.104	0.527
Panel C. Women						
Young × Settlement	0.013*	0.012***	0.021***	-0.042***	0.003	0.003
	(0.007)	(0.003)	(0.005)	(0.012)	(0.003)	(0.003)
Number of Individuals	252,868	252,868	252,868	167,826	167,826	173,449
Mean Dep. Var.	0.793	0.931	0.914	0.163	0.102	0.524
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	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 once in the 1930 census. In panel A, the sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any; in panels B and C, the sample excludes women and men, respectively. The treatment variable is equal to one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. The dependent variable is one if the individual is a citizen (column 1), if they speak English (column 2), if they can both read and write (column 3), if they are married to a native US citizen, or if they are married to an immigrant from another country (columns 4 and 5), and the Foreign Name Index (column 6). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 21.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table IV. The Intergenerational Effects of Settlement Houses

	Labor Force Participation	High-Skill Employment	Occupational Income	Educational Attainment	Married Native	Married Immigrant
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Entire Population						
Settlement	-0.005**	0.032***	-0.006	0.136**	-0.044***	0.043***
	(0.002)	(0.006)	(0.008)	(0.051)	(0.005)	(0.007)
Number of Individuals	222,487	222,487	222,487	217,994	183,171	183,171
Mean Dep. Var.	0.751	0.162	3.016	3.319	0.786	0.127
Panel B. Men						
Settlement	0.000	0.043***	0.020**	0.169***	-0.035***	0.037***
	(0.001)	(0.008)	(0.007)	(0.051)	(0.004)	(0.006)
Number of Individuals	154,516	154,516	154,516	151,309	133,149	133,149
Mean Dep. Var.	0.960	0.210	3.882	3.399	0.800	0.113
Panel C. Women						
Settlement	-0.020	0.000	-0.073	0.039	-0.076***	0.065***
	(0.012)	(0.003)	(0.043)	(0.054)	(0.012)	(0.014)
Number of Individuals	67,948	67,948	67,948	66,662	49,995	49,995
Mean Dep. Var.	0.278	0.053	1.047	3.139	0.747	0.164
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effect of childhood exposure to settlements. The unit of observation is an individual in the 1940 census. In panel A, the sample comprises all individuals 15 years old or younger when the first settlement was established in their neighborhood in 1900, if any; in panels B and C, the sample excludes women and men, respectively. 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 if they work in a highly skilled occupation (column 2), the (IHS) occupation-based imputed income (column 3), the highest completed schooling year (column 4), the dependent variable equals one for individuals married with immigrants from other countries or with US natives (columns 5 and 6). All regressions include city-by-cohort fixed effects and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 24.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table V. The Family and Fertility Effects of Settlement Houses

	Has Children	Number of Children	Married	Age at Marriage
	(1)	(2)	(3)	(4)
Panel A. Entire Population				
Young × Settlement	0.017***	0.038***	0.013***	0.213***
	(0.004)	(0.007)	(0.002)	(0.056)
Number of Individuals	553,521	553,521	553,521	380,049
Mean Dep. Var.	0.887	1.723	0.935	24.952
Panel B. Men				
Young × Settlement	0.016***	0.028***	0.010***	0.247***
	(0.005)	(0.007)	(0.002)	(0.066)
Number of Individuals	300,560	300,560	300,560	237,992
Mean Dep. Var.	0.875	1.676	0.938	26.259
Panel C. Women				
Young × Settlement	0.014**	0.045***	0.011**	0.124
	(0.005)	(0.006)	(0.004)	(0.076)
Number of Individuals	252,868	252,868	252,868	141,816
Mean Dep. Var.	0.901	1.779	0.931	22.761
Neighborhood FE	Yes	Yes	Yes	Yes
City-Cohort 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 once in the 1930 census. In panel A, the sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any; in panels B and C, the sample excludes women and men, respectively. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise. The dependent variable is one if the individual has at least one child (column 1), the (IHS) number of children (column 2), and it equals one if the individual is married (column 3), and the age at first marriage (column 4). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 25.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table VI. Heterogeneous Responses to Settlements by Neighborhood Immigrant Diversity

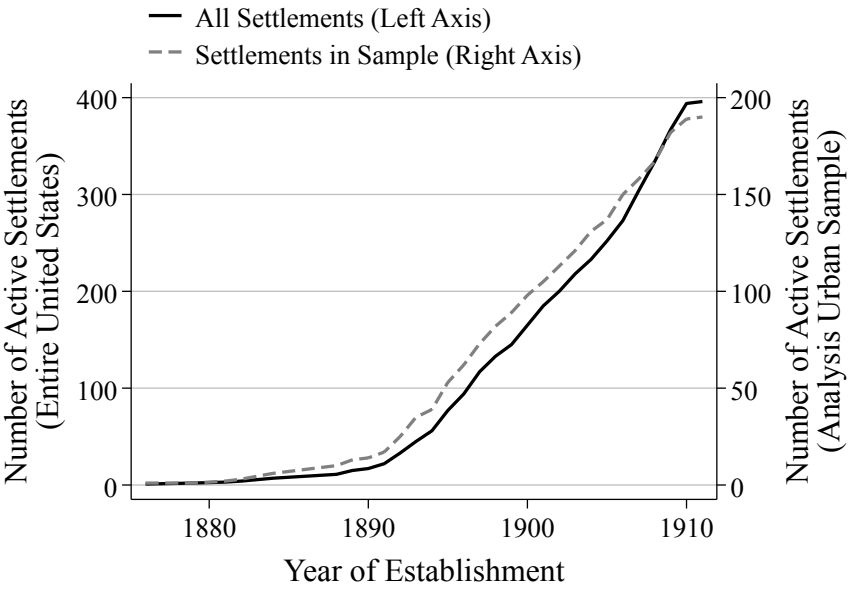
	Labor Market		Family		Assimilation	
	(1) Labor Force Participation	(2) Occupation-Based Income	(3) Has at Least One Child	(4) Is Married	(5) Speaks English	(6) Naturalized US Citizen
Panel A. Entire Population						
Post Settlement	-0.000	0.017	0.010	0.007**	-0.003*	-0.007
	(0.003)	(0.010)	(0.006)	(0.003)	(0.002)	(0.004)
Post Settlement ×	0.008*	0.035	0.018**	0.015***	0.049***	0.072***
High Diversity	(0.004)	(0.025)	(0.006)	(0.003)	(0.006)	(0.007)
N. of Individuals	553,521	553,521	553,521	553,521	553,521	553,521
Mean Dep. Var.	0.414	1.805	0.887	0.935	0.948	0.810
Panel B. Men						
Post Settlement	0.001	0.025*	0.010	0.006	0.004	-0.009
	(0.005)	(0.013)	(0.006)	(0.004)	(0.003)	(0.006)
Post Settlement ×	0.029***	0.109**	0.015*	0.011	0.031***	0.080***
High Diversity	(0.006)	(0.037)	(0.007)	(0.007)	(0.005)	(0.008)
N. of Individuals	300,560	300,560	300,560	300,560	300,560	300,560
Mean Dep. Var.	0.683	3.054	0.875	0.938	0.963	0.824
Panel C. Women						
Post Settlement	0.002	0.012	0.009	0.005*	-0.012***	-0.008
	(0.002)	(0.008)	(0.006)	(0.003)	(0.001)	(0.005)
Post Settlement ×	-0.037***	-0.131***	0.015**	0.017***	0.066***	0.059***
High Diversity	(0.007)	(0.030)	(0.006)	(0.005)	(0.010)	(0.010)
N. of Individuals	252,868	252,868	252,868	252,868	252,868	252,868
Mean Dep. Var.	0.096	0.321	0.901	0.931	0.931	0.793
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the heterogeneous effect of settlements in terms of the diversity of the immigrant community. The unit of observation is an individual immigrant observed once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. In panel A, the sample comprises the full population; in panel B, the sample is restricted to men; in panel B, the sample is composed of women. The first treatment variable is equal to one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. We then compute the diversity of the immigrant communities in each neighborhood in 1900 in terms of the Hirschman-Herfindahl index, construct a binary variable equal to one for neighborhoods in the top tercile of HHI diversity, and interact this binary variable with the main treatment variable. The dependent variable is one if the individual works and their (IHS) occupation-based income (columns 1–2), an indicator for individuals with children and married individuals (columns 3–4), and an indicator for individuals who speak English or are naturalized US citizens (columns 5–6). All regressions include neighborhood, city-by-cohort fixed effects; and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 30.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

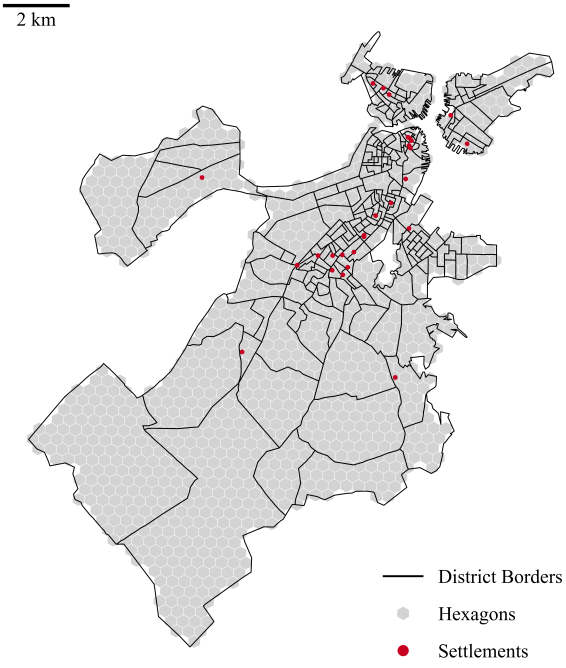
FIGURES

Figure I. Settlements over Time and Across Space

(a) Number of Settlements by Year of Establishment

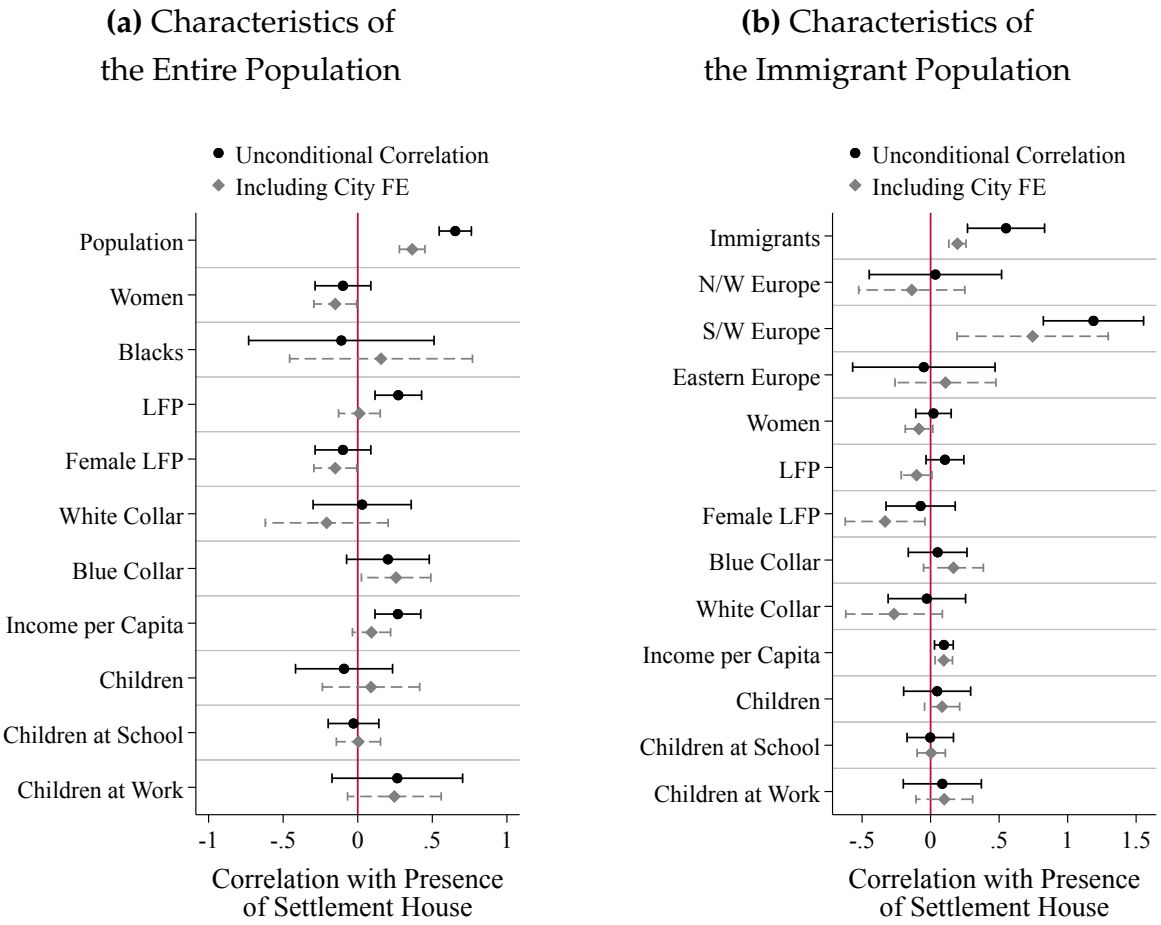


(b) Distribution of Settlements in Sample City (Boston)



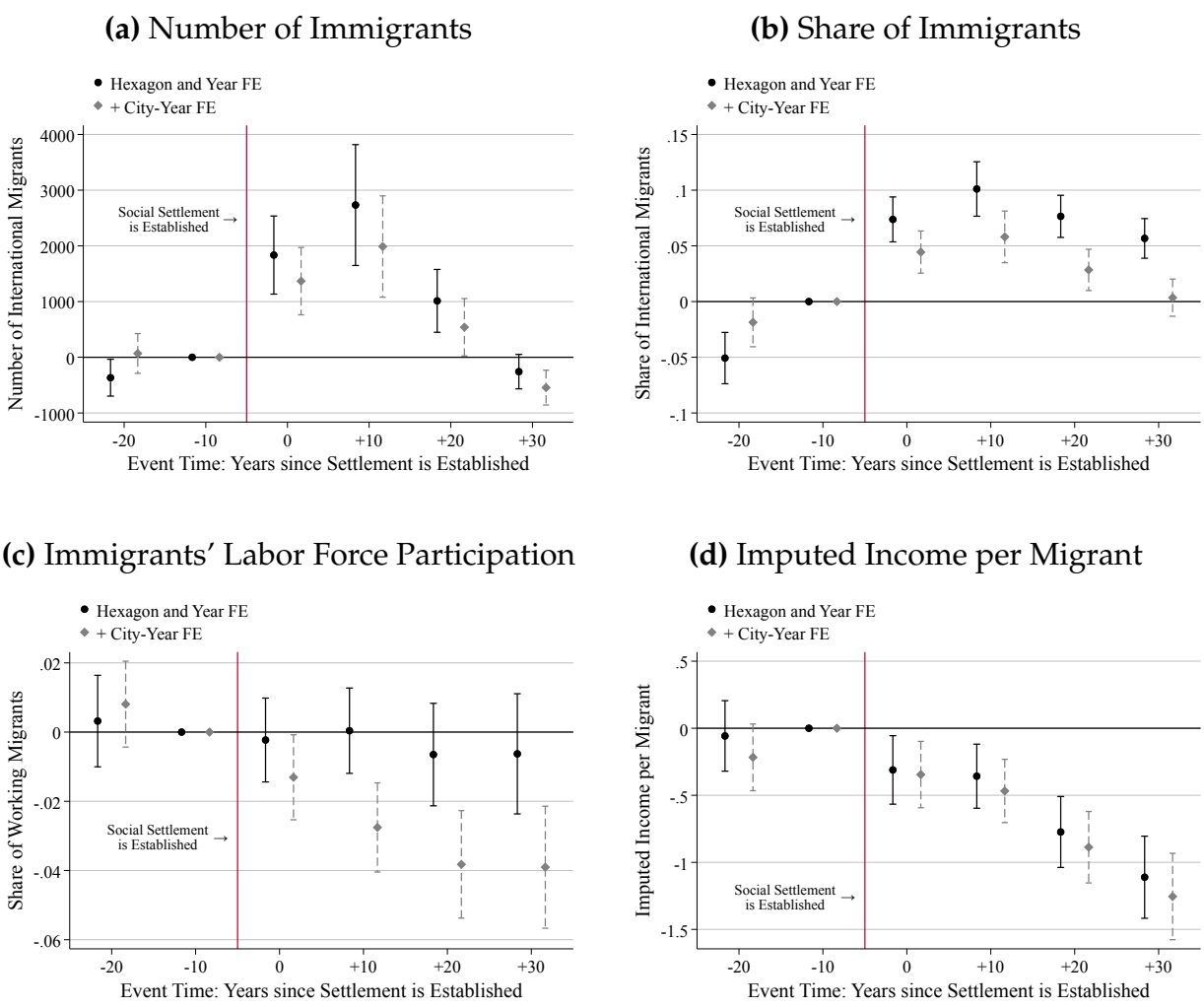
Notes. Panel Ia reports the total number of settlements between 1882 and 1911 in the United States (solid black line, left axis) and in the analysis sample (dashed grey line, right axis). Panel Ib plots the spatial distribution of settlements (red dots) in Boston. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 9.

Figure II. Presence of Settlements and Demographic Characteristics in 1880



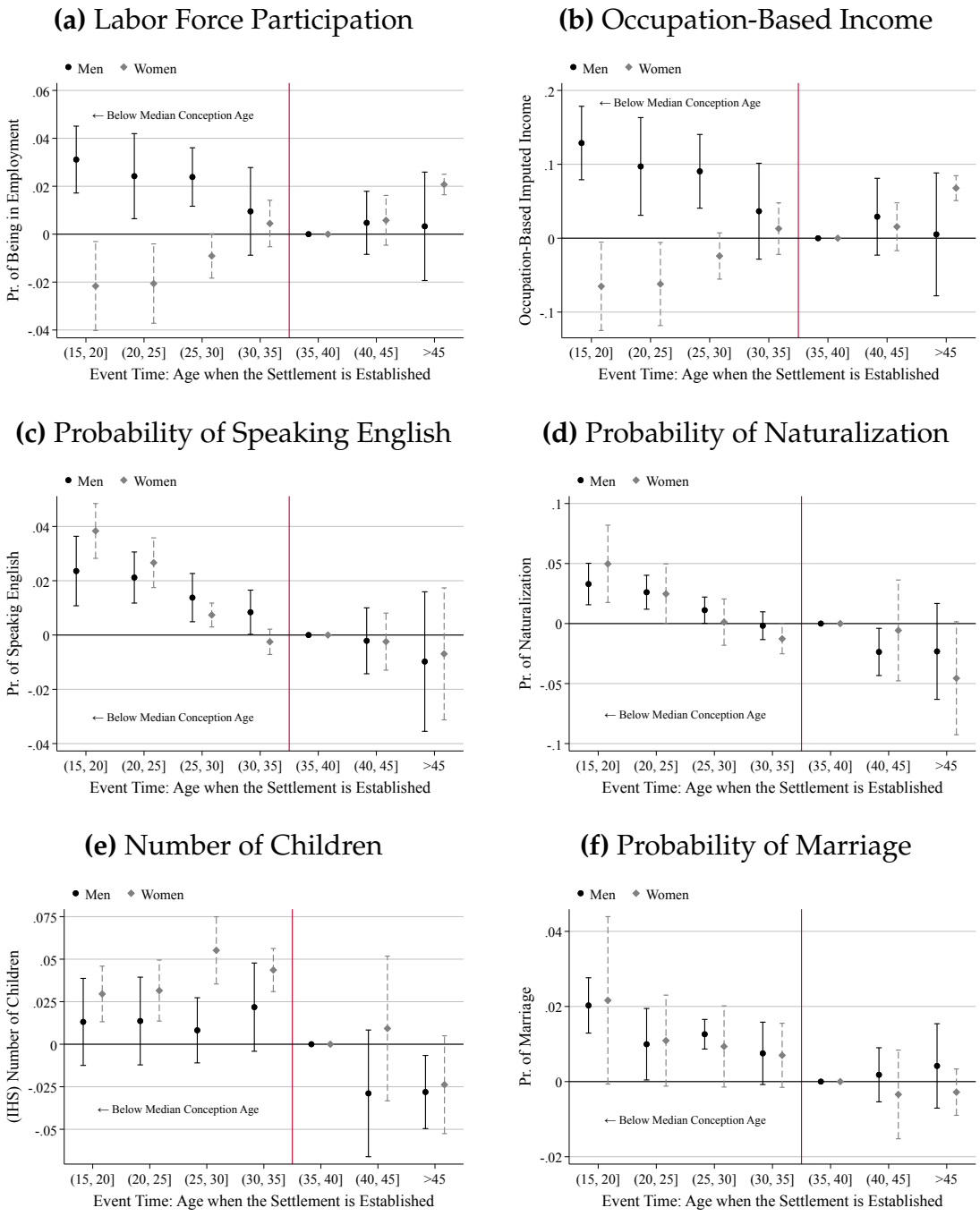
Notes. This figure reports the correlation between the presence of a settlement between 1882 and 1911 and hexagon-level demographic characteristics in 1880. Each dot reports the correlation between one variable and a binary indicator, which equals one for hexagons with at least one active settlement. 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 city level; bands report 95% confidence intervals. Referenced on page(s) 13, A5, C24, C24.

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 is 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). Black dots report regression coefficients including hexagon and census decade fixed effects; the gray dots include city-by-year fixed effects. Hexagons are weighted by population. Standard errors are reported in parentheses; bands report 95% confidence intervals. Referenced on page(s) 16.

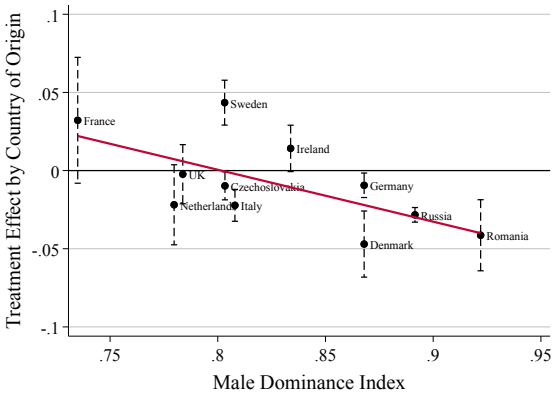
Figure IV. Individual-Level Effects of Settlements



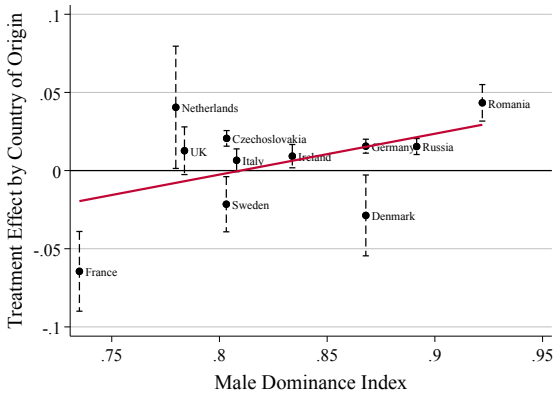
Notes. This figure reports the effect of settlements on labor force participation (IVa), (IHS) occupation-based imputed income (IVb), the probability of speaking English (IVc), the probability of obtaining US citizenship (IVd), the (IHS) number of children (IVe) and the probability of marriage (IVf). The unit of observation is an individual immigrant observed once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. The dots report regression coefficients associated with a set of dummies coding the age—binned in 5-year windows—of the individual when they were first exposed to a settlement, depending on where they were located in 1900. The bin 35-40 serves as the baseline category. The black dots refer to the male population, and the gray dots refer to the female population. All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level; bands report 95% confidence bands. Referenced on page(s) 19, 21, 26.

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

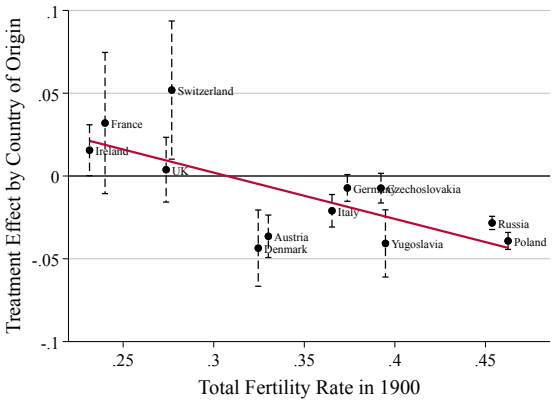
(a) Male Dominance Index and Labor Force Participation



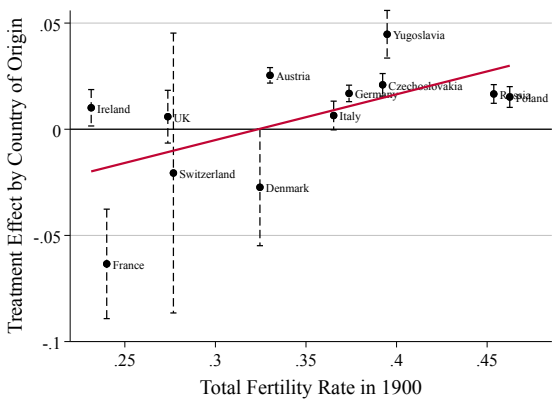
(b) Male Dominance Index and Marriage Probability



(c) Total Fertility Rate and Labor Force Participation



(d) Total Fertility Rate and Marriage Probability



Notes. This figure reports the response to the establishment of settlements by the country of origin of the immigrants. In both panels, the unit of observation is an individual immigrant observed once in the 1930 census. The sample comprises all women aged 15 or older when the first settlement was established in their neighborhood in 1900, if any. The dependent variable is labor force participation (panels Va and Vc) and the probability of marriage (panels Vb and Vd). On the y-axis, each dot reports the coefficient associated with an interaction term between a variable equal to one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise, and country-of-origin indicators. The regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level; the bands report 95% confidence intervals. 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) 29.

ONLINE APPENDIX

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

Davide M. Coluccia

April, 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.

In Appendix table B.10, I explore how different activities performed by settlement volunteers impacted the immigrants. 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.10, I thus concentrate on childcare and professional training services in light of the results displayed in the main text.

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 high-skill occupation 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 white-collar occupations as high-skilled occupations and "Clerical and Kindred" (OCC1950 between 300 and 390), "Sales workers" (OCC1950 between 400 and 490), and "Service Workers" (OCC1950 between 700 and 790). I classify as blue-collar occupations "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 1930 censuses. Hence, it is straightforward to check whether the individuals had children residing in their household in 1930 and 1900. However, this approach would not identify those who had children between those years and whose children left the household before 1930.[1] To circumvent this issue, I exploit the linked data produced by Price *et al.* (2021). Specifically, I link the 1930 census to the 1920, 1910, and 1900 censuses, so that I observe whether each individual who appears in the 1930 sample has children in each 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 either in 1930 or in one census linked to 1930 (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). 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).

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.

[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 1930 census, but it is likely that this naïve approach heavily underestimates fertility.

To construct the foreign-name index (FNI), I use confidential data from the 1900, 1910, 1920, and 1930 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 samples link immigrants between the 1900 and the 1930 and 1940 censuses. In the 1900-1930 sample, which I use to explore the effects of settlements on adults, I include individuals born after 1850. In the 1900-1940 sample, which I use to explore the effect of the settlements on children, I include individuals born after 1890. In both samples, an individual is exposed to a settlement if they lived within 0.250 meters of a settlement in 1900. In the 1900-1930 sample, adults are considered treated if they were younger than 35 when they were first exposed to a settlement. In the 1900-1940 sample, which only comprises immigrants exposed to settlements during childhood, the treatment is simply an indicator equal to one for individuals exposed to settlements in 1900 and zero otherwise.

A.II Summary of the Robustness Analyses

A.II.1 Hexagon-Level Analysis

Table B.3 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.4 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.5 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 complements evidence provided in the main text by exploring the effects of settlements depending on the services they provide. My main argument is that, in an environment characterized by exclusionary gender norms, settlement houses had positive labor-market effects on men and negative consequences on women because, by increasing men's employability and income, they segregated women into housework by decreasing the cost of childcare.

I thus contrast the effects of professional training and childcare services settlements provide. Specifically, I estimate the baseline difference-in-differences regression and interact the treatment with dummy variables for individuals exposed to settlements that provided (i) professional training but no childcare, (ii) childcare but no professional training, and (iii) both. Job market training benefits men and harms women in terms of their labor force participation (columns 1–2) and income (columns 3–4). By contrast, childcare services increased marriage rates (columns 5–6) and the number of children (columns 7–8), although in the latter case, the effect is not statistically significant. Importantly, when immigrants are exposed to childcare *and* training services, all the effects magnify. Male immigrants exposed to childcare and training services are more likely to work (column 1), earn more (column 3), and have more children (column 7). Women also have more children (column 8) and are more likely to marry (column 6) but are less likely to work and earn less (columns 2 and 4). These patterns suggest that the joint presence of employment training for men and childcare services supporting women reinforced the segregating effects of settlement houses on women.

Table B.11 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 (panel A), the total fertility rate (panel B), and

an interaction term between the two.[4] Female immigrants from countries characterized by more traditional gender roles—both in terms of MDI and fertility—were less likely to work (column 1), hold high-skill occupations (column 2), earn more (column 3), and have white-collar or blue-collar jobs (columns 4 and 5). 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, in 1930, increasingly less likely to work and, more generally, faced deteriorated labor market opportunities. This pattern holds regardless of the measure of gender conservatism. 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.6, B.7, B.8, 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 (3), (5), and (7) report the results of the main text, which further include individual-level fixed effects for gender, country of origin, marital status, race, and immigration year. 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, year of immigration, literacy status, married status, and relationship with the household—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.6, and C.7, 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

[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.8, the treatment varies at the neighborhood level, so I only include cohort fixed effects.

sample, but their sign and magnitude remain unaltered.

Figures C.12, C.13, C.14, and C.15 report various estimates of the standard errors for the baseline treatment effects of settlement houses on immigrants.[6] The black markers refer to men, and the gray ones refer to women. I adopt four different estimators and report nine different estimates for each outcome. The estimators are the unadjusted standard errors, the White (heteroskedasticity-robust) standard errors, and clustered standard errors by neighborhood and city (which I adopt as my preferred estimates). A plausible concern is that statistical significance may be inflated by spatial autocorrelation. I thus adopt the estimator developed by Conley (1999), which is robust to spatial autocorrelation under different thresholds (0.5 Km, 1 Km, 2 Km, 5 Km, and 10 Km). 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, I consider individuals as exposed to a settlement house if they lived within 250 meters of a settlement in 1900. In figures C.8, C.9, C.10, and C.11, I report the estimated treatment effects obtained 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 between residents and non-residents, did not impact immigrants beyond this area. In fact, 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.

[6] The estimates are obtained from the baseline specification without controls to avoid excessive computational burden. The results with and without the controls are quantitatively very similar.

B ADDITIONAL TABLES

Table B.1. 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. 1930 Cross Section Linked to 1900 Census (Immigrants Only)								
Has Settlement	0.122	0.327	0.000	583212	0.122	0.327	0.000	583212
Young when Settlement Established	0.088	0.283	0.000	583212	0.088	0.283	0.000	583212
Employed	0.669	0.470	1.000	315222	0.093	0.290	0.000	267990
High-Skill Employment	0.154	0.361	0.000	315222	0.013	0.115	0.000	267990
Occupation-Based Income	2.993	1.754	3.829	315222	0.311	0.989	0.000	267990
Literacy	0.944	0.230	1.000	315222	0.914	0.281	1.000	267990
Married	0.939	0.240	1.000	315222	0.932	0.251	1.000	267990
Has Children	0.706	0.456	1.000	315222	0.687	0.464	1.000	267990
N. of Children	1.065	0.824	0.881	315222	0.993	0.797	0.881	267990
Naturalized Citizen	0.824	0.381	1.000	315222	0.792	0.406	1.000	267990
Speaks English	0.962	0.191	1.000	315222	0.930	0.254	1.000	267990
Panel C. 1940 Cross Section Linked to 1900 Census (Immigrants Only)								
Educational Attainment	3.336	2.431	2.000	310535	3.178	2.124	2.000	149098
Married Immigrant from Other Country	0.153	0.360	0.000	310535	0.171	0.377	0.000	149098
Married Native-Born American	0.063	0.243	0.000	310535	0.078	0.268	0.000	149098

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 women population. Panel A refers to the panel at the hexagon-by-census decade level; the statistics in panel B are constructed from the 1930 census sample linked to the 1900 census as described in the main text; panel C refers to the 1940 cross-section linked to the 1900 census. The statistics of Panels B and C 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) 11, 12.

Table B.2. 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) 12.

Table B.3. 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 city level.Referenced on page(s) 13, A4.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.4. 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.	0.533	0.082	0.610	0.700	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 city level. Referenced on page(s) 16, A5.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.5. 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
	(630.831)	(0.032)	(306.228)	(0.031)	(327.367)	(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.	1903.219	0.719	936.467	0.713	966.752	0.729

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 city level. Referenced on page(s) 17, A5.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.6. 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								
Young × Settlement	0.015***	0.018***	0.006**	0.005**	0.018***	0.017***	-0.012***	-0.010***
	(0.005)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.004)	(0.002)
Mean Dep. Var.	0.460	0.460	0.460	0.459	0.765	0.765	0.097	0.097
Panel B. High Skill Employment								
Young × Settlement	0.017***	0.009***	0.004	0.003	0.006	0.003	-0.002	-0.001
	(0.003)	(0.002)	(0.003)	(0.002)	(0.006)	(0.004)	(0.001)	(0.001)
Mean Dep. Var.	0.092	0.092	0.092	0.092	0.157	0.157	0.014	0.014
Panel C. Occupation-Based Income								
Young × Settlement	0.069***	0.085***	0.030***	0.024**	0.068***	0.061***	-0.035**	-0.030***
	(0.019)	(0.016)	(0.008)	(0.008)	(0.017)	(0.012)	(0.014)	(0.008)
Mean Dep. Var.	1.805	1.805	1.805	1.805	3.054	3.054	0.321	0.321
Panel D. White Collar Employment								
Young × Settlement	0.022***	0.009***	0.004	0.003**	0.012**	0.009***	-0.009***	-0.007***
	(0.004)	(0.002)	(0.003)	(0.001)	(0.005)	(0.002)	(0.003)	(0.002)
Mean Dep. Var.	0.214	0.214	0.214	0.214	0.330	0.330	0.075	0.075
Panel E. Blue Collar Employment								
Young × Settlement	-0.007	0.009	0.002	0.002	0.006	0.008***	-0.002	-0.003***
	(0.007)	(0.005)	(0.003)	(0.002)	(0.004)	(0.002)	(0.001)	(0.000)
Mean Dep. Var.	0.244	0.244	0.244	0.244	0.431	0.431	0.021	0.021
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	–	–	–	–	–	–	–
City-Cohort FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Propensity Score Weights	No	No	No	Yes	No	Yes	No	Yes
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823

Notes. This table reports the effect of settlements on labor-market variables. The unit of observation is an individual immigrant observed once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. In columns (5–6) and (7–8), the sample is restricted to men and women, respectively. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. The dependent variable is one if the individual works (panel A), and if they work in a highly skilled occupation (panel B), the (IHS) occupation-based imputed income (panel C), and is equal to one if the individual is employed in a White collar (panel D) or Blue collar (panel E) manufacturing occupation. All regressions include neighborhood and cohort fixed effects; in columns (2–8), we substitute cohort with city-by-cohort fixed effects; in columns (3) and (5–8), we include individual controls—sex, birthplace, marital status in 1900, race, and immigration year. In column (4), individuals are weighted by their propensity score. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 19, A7.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

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

	Full Population				Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Naturalized US Citizen								
Young × Settlement	0.052***	0.027***	0.021**	0.017***	0.023**	0.016***	0.013*	0.015***
	(0.007)	(0.007)	(0.007)	(0.004)	(0.008)	(0.004)	(0.007)	(0.004)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	0.810	0.810	0.810	0.810	0.824	0.824	0.793	0.793
Panel B. Speak English								
Young × Settlement	0.031***	0.018***	0.016***	0.014***	0.017***	0.014***	0.012***	0.009***
	(0.006)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	0.948	0.948	0.948	0.948	0.963	0.963	0.931	0.931
Panel C. Can Read and Write								
Young × Settlement	0.031***	0.023***	0.020***	0.020***	0.017***	0.011***	0.021***	0.028***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.005)	(0.002)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	0.930	0.930	0.930	0.930	0.944	0.944	0.914	0.914
Panel D. Married to US Native Citizen								
Young × Settlement	-0.060***	-0.016*	-0.029***	-0.019***	-0.020***	-0.013***	-0.042***	-0.029***
	(0.012)	(0.008)	(0.006)	(0.003)	(0.003)	(0.002)	(0.012)	(0.006)
Number of Individuals	423,009	423,006	422,999	422,895	254,993	254,926	167,826	167,795
Mean Dep. Var.	0.225	0.225	0.225	0.225	0.265	0.265	0.163	0.163
Panel E. Married to Immigrants from Other Country								
Young × Settlement	0.012***	0.005	0.003	-0.002	0.002	-0.003	0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)	(0.003)	(0.001)
Number of Individuals	423,009	423,006	422,999	422,895	254,993	254,926	167,826	167,795
Mean Dep. Var.	0.103	0.103	0.103	0.103	0.104	0.104	0.102	0.102
Panel F. Foreign Name Index (IHS)								
Young × Settlement	0.006**	0.001	0.003**	0.003***	0.003*	0.005***	0.003	0.002
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
Number of Individuals	385,073	385,069	385,059	384,975	211,408	211,357	173,449	173,425
Mean Dep. Var.	0.526	0.526	0.526	0.526	0.527	0.527	0.524	0.524
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	–	–	–	–	–	–	–
City-Cohort FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
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 once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. In columns (5–6) and (7–8), the sample is restricted to men and women, respectively. The treatment variable is equal to one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. The dependent variable is one if the individual is a citizen (panel A), if they speak English (panel B), if they can both read and write (panel C), if they are married to a native US citizen, or if they are married to an immigrant from another country (panels D and E), and the Foreign Name Index (panel F). All regressions include neighborhood and cohort fixed effects; in columns (2–8), we substitute cohort with city-by-cohort fixed effects; in columns (3) and (5–8), we include individual controls—sex, birthplace, marital status in 1900, race, and immigration year. In column (4), individuals are weighted by their propensity score. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 22, A7.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.8. 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	0.051***	0.008	-0.005**	-0.006***	0.000	0.002***	-0.020	-0.031***
	(0.012)	(0.005)	(0.002)	(0.001)	(0.001)	(0.000)	(0.012)	(0.003)
Number of Individuals	222,497	222,497	222,487	222,342	154,516	154,418	67,948	67,909
Mean Dep. Var.	0.751	0.751	0.751	0.751	0.960	0.960	0.278	0.278
Panel B. High Skill Employment								
Settlement	0.070***	0.036***	0.032***	0.030***	0.043***	0.040***	0.000	-0.003
	(0.010)	(0.007)	(0.006)	(0.003)	(0.008)	(0.003)	(0.003)	(0.002)
Number of Individuals	222,497	222,497	222,487	222,342	154,516	154,418	67,948	67,909
Mean Dep. Var.	0.163	0.163	0.162	0.162	0.210	0.210	0.053	0.053
Panel C. Occupation-Based Income								
Settlement	0.226***	0.048**	-0.006	-0.005	0.020**	0.027***	-0.073	-0.112***
	(0.052)	(0.018)	(0.008)	(0.004)	(0.007)	(0.002)	(0.043)	(0.009)
Number of Individuals	222,497	222,497	222,487	222,342	154,516	154,418	67,948	67,909
Mean Dep. Var.	3.016	3.016	3.016	3.016	3.882	3.882	1.047	1.048
Panel D. Educational Attainment								
Settlement	0.302***	0.109*	0.136**	0.124***	0.169***	0.157***	0.039	0.018
	(0.050)	(0.051)	(0.051)	(0.026)	(0.051)	(0.024)	(0.054)	(0.030)
Number of Individuals	218,004	218,004	217,994	217,850	151,309	151,212	66,662	66,623
Mean Dep. Var.	3.319	3.319	3.319	3.319	3.399	3.398	3.139	3.139
Panel E. Married to Immigrants from Other Countries								
Settlement	0.075***	0.043***	0.043***	0.041***	0.037***	0.037***	0.065***	0.058***
	(0.008)	(0.007)	(0.007)	(0.003)	(0.006)	(0.002)	(0.014)	(0.006)
Number of Individuals	183,183	183,183	183,171	183,046	133,149	133,063	49,995	49,963
Mean Dep. Var.	0.127	0.127	0.127	0.127	0.113	0.113	0.164	0.164
Panel F. Married to Native-Born Americans								
Settlement	-0.113***	-0.058***	-0.044***	-0.040***	-0.035***	-0.034***	-0.076***	-0.069***
	(0.011)	(0.007)	(0.005)	(0.001)	(0.004)	(0.001)	(0.012)	(0.005)
Number of Individuals	183,183	183,183	183,171	183,046	133,149	133,063	49,995	49,963
Mean Dep. Var.	0.786	0.786	0.786	0.786	0.800	0.800	0.747	0.747
Cohort FE	Yes	–	–	–	–	–	–	–
City-Cohort FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
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 in the 1940 census. The sample comprises all those aged 15 or younger when the first settlement was established in their neighborhood in 1900, if any. The sample is restricted to men and women in columns (5–6) and (7–8). 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 if they work in a highly skilled occupation (panel B), the (IHS) occupation-based imputed income (panel C), the highest completed schooling year (panel D), one for individuals married with immigrants from other countries or with US natives (panels E and F). All regressions include cohort fixed effects; in columns (2–8), we substitute cohort with city-by-cohort fixed effects; in columns (3) and (5–8), we include individual controls—sex, birthplace, marital status in 1900, race, and immigration year. In column (4), individuals are weighted by their propensity score. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 24, A7, A7.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

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								
Young × Settlement	0.023***	0.022***	0.017***	0.018***	0.016***	0.016***	0.014**	0.018***
	(0.004)	(0.005)	(0.004)	(0.001)	(0.005)	(0.001)	(0.005)	(0.002)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	0.887	0.887	0.887	0.887	0.875	0.875	0.901	0.901
Panel B. Number of Children								
Young × Settlement	0.059***	0.049***	0.038***	0.041***	0.028***	0.032***	0.045***	0.048***
	(0.005)	(0.005)	(0.007)	(0.003)	(0.007)	(0.003)	(0.006)	(0.004)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	1.723	1.723	1.723	1.723	1.676	1.676	1.779	1.779
Panel C. Married								
Young × Settlement	0.020***	0.018***	0.013***	0.011***	0.010***	0.008***	0.011**	0.013***
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.001)	(0.004)	(0.001)
Number of Individuals	553,531	553,528	553,521	553,389	300,560	300,481	252,868	252,823
Mean Dep. Var.	0.935	0.935	0.935	0.935	0.938	0.938	0.931	0.931
Panel D. Age at First Marriage								
Young × Settlement	0.427***	0.237***	0.213***	0.125***	0.247***	0.170***	0.124	-0.012
	(0.051)	(0.060)	(0.056)	(0.034)	(0.066)	(0.034)	(0.076)	(0.045)
Number of Individuals	380,063	380,057	380,049	379,953	237,992	237,923	141,816	141,795
Mean Dep. Var.	24.952	24.952	24.952	24.952	26.259	26.259	22.761	22.761
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	–	–	–	–	–	–	–
City-Cohort FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
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 once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. In columns (5–6) and (7–8), the sample is restricted to men and women, respectively. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise. The dependent variable is one if the individual has at least one child (panel A), the (IHS) number of children (panel B), and it equals one if the individual is married (panel C). All regressions include neighborhood and cohort fixed effects; in columns (2–8), we substitute cohort with city-by-cohort fixed effects; in columns (3) and (5–8), we include individual controls—sex, birthplace, marital status in 1900, race, and immigration year. In column (4), individuals are weighted by their propensity score. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 27, A7.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.10. Heterogeneous Responses to Settlements by Settlements' Activities

	Labor Force Participation		Occupational Income		Marriage		Number of Children	
	(1) Men	(2) Women	(3) Men	(4) Women	(5) Men	(6) Women	(7) Men	(8) Women
Post Settlement × Young ×								
Training & No Childcare	0.008**	-0.022***	0.039***	-0.069***	0.003	0.018***	0.030	0.070**
	(0.003)	(0.005)	(0.008)	(0.018)	(0.002)	(0.003)	(0.022)	(0.024)
Childcare & No Training	-0.001	-0.014	0.028	-0.023	0.021**	0.033*	0.059*	0.053***
	(0.008)	(0.013)	(0.033)	(0.048)	(0.009)	(0.018)	(0.030)	(0.015)
Training & Childcare	0.019**	-0.023***	0.072*	-0.072***	0.008	0.019***	0.024*	0.076***
	(0.008)	(0.006)	(0.033)	(0.020)	(0.005)	(0.005)	(0.012)	(0.013)
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuas	302,841	253,962	302,841	253,962	302,841	253,962	302,841	253,962
Mean Dep. Var.	0.766	0.097	3.059	0.322	0.938	0.931	1.674	1.779

Notes. This table reports the effect of settlements depending on their activity. The unit of observation is an individual foreign-born observed once in the 1930 census. The sample comprises all men (columns 1, 3, 5, 7) and women (columns 2, 4, 6, 8) who are 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise. The baseline treatment is interacted with dummy variables coding whether the settlements in the neighborhood offer childcare services and no professional training, professional training and no childcare services, or both. The dependent variable is one if the individual works (columns 1–2), the (IHS) occupation-based imputed income (columns 3–4), is equal to one if the individual is married (columns 5–6), and is equal to the (IHS) number of children (columns 7–8). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 27, A2, A2, A6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table B.11. Heterogeneous Responses to Settlements by Origin Country's Gender Norms

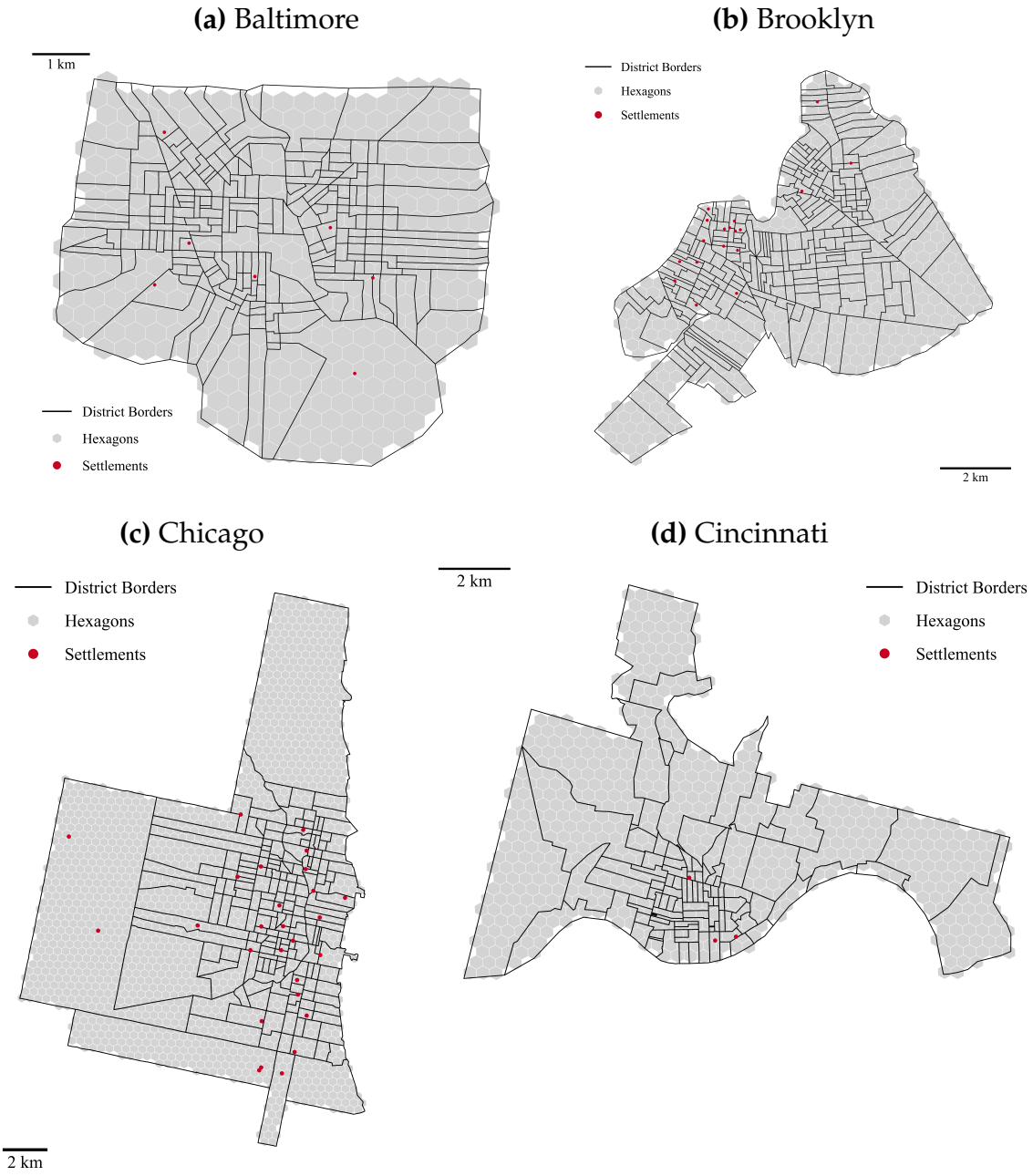
	Labor Force Participation	High-Skill Employment	Occupational Income	White-Collar Employment	Blue-Collar Employment
	(1)	(2)	(3)	(4)	(5)
Panel A. Male-Dominance Index					
Young × Settlement	0.194***	0.010	0.728***	0.097	0.102**
	(0.036)	(0.020)	(0.112)	(0.056)	(0.034)
Male-Dominance Index	-0.261***	-0.030**	-0.876***	-0.194***	-0.072***
	(0.060)	(0.012)	(0.190)	(0.046)	(0.017)
Young × Settlement × Male-Dominance Index	-0.244***	-0.015	-0.907***	-0.125*	-0.126***
	(0.040)	(0.023)	(0.124)	(0.065)	(0.039)
Number of Individuals	197,664	197,664	197,664	197,664	197,664
Mean Dep. Var.	0.096	0.012	0.318	0.076	0.021
Panel B. Total Fertility Rate in 1900					
Young × Settlement	0.037***	0.007	0.132***	0.030**	0.005*
	(0.011)	(0.004)	(0.033)	(0.011)	(0.003)
Total Fertility Rate (1900)	-0.218***	0.001	-0.637***	-0.214***	-0.007
	(0.045)	(0.006)	(0.117)	(0.042)	(0.005)
Young × Settlement × Total Fertility Rate (1900)	-0.141***	-0.023*	-0.477***	-0.111***	-0.025**
	(0.029)	(0.011)	(0.079)	(0.029)	(0.008)
Number of Individuals	214,099	214,099	214,099	214,099	214,099
Mean Dep. Var.	0.091	0.013	0.304	0.071	0.021
Neighborhood FE	Yes	Yes	Yes	Yes	Yes
City-Cohort FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effect of settlements on women's labor market outcomes. The unit of observation is an individual foreign-born woman observed once in the 1930 census. The sample comprises all individuals 15 years old or older when the first settlement was established in their neighborhood in 1900, if any. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise. The baseline treatment is interacted with two measures of conservative gender norms: the Male Dominance Index (panel A) of Guarnieri and Tur-Prats (2023), and the total fertility rate in 1900 (panel B) of Coale and Treadway (1986). The dependent variable is one if the individual works (column 1), and if they work in a highly skilled occupation (column 2), the (IHS) occupation-based imputed income (column 3), and is equal to one if the individual is employed in a White collar (column 4) or Blue collar (column 5) manufacturing occupation. All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level and are reported in parentheses. Referenced on page(s) 29, A6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

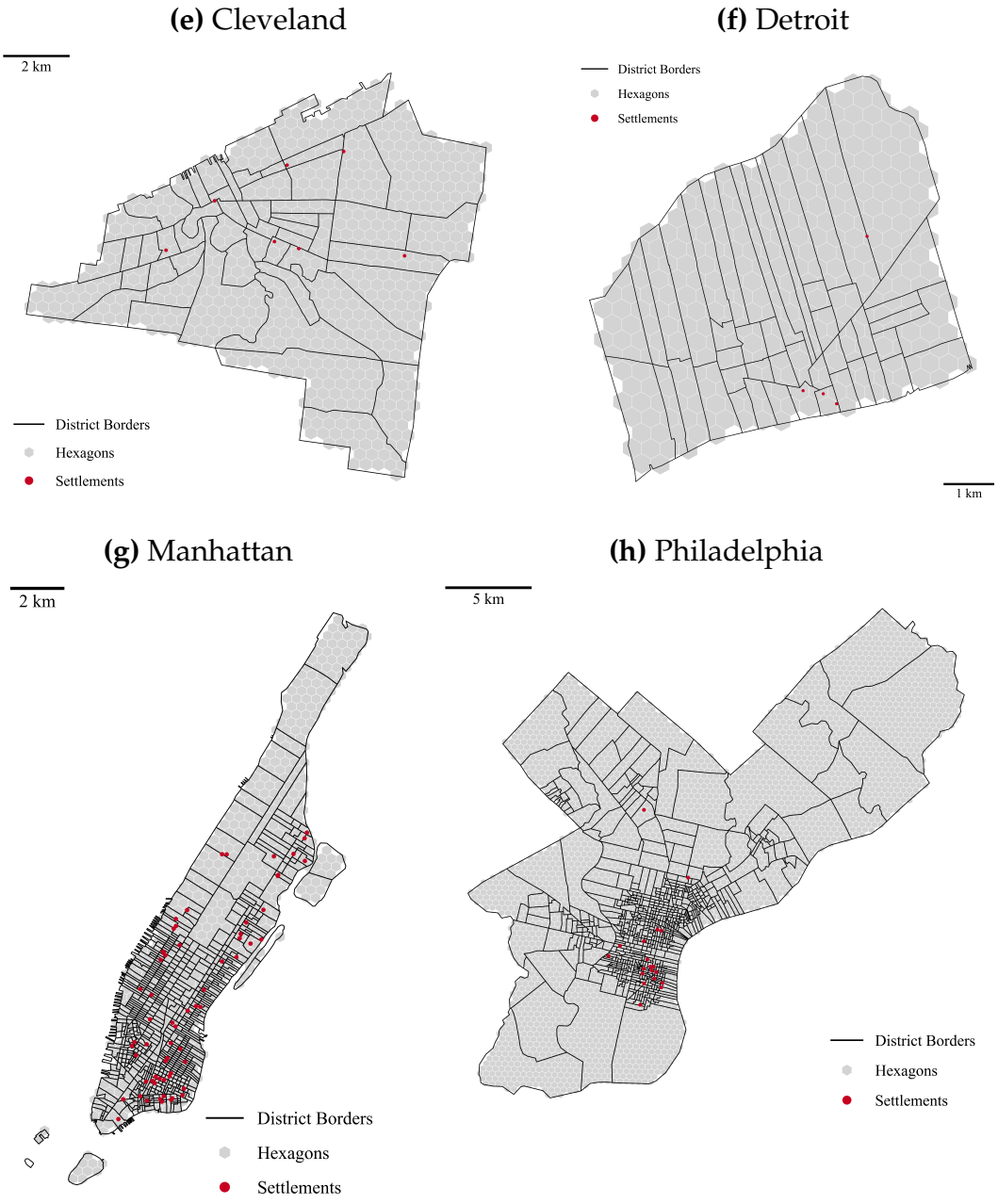
C ADDITIONAL FIGURES

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



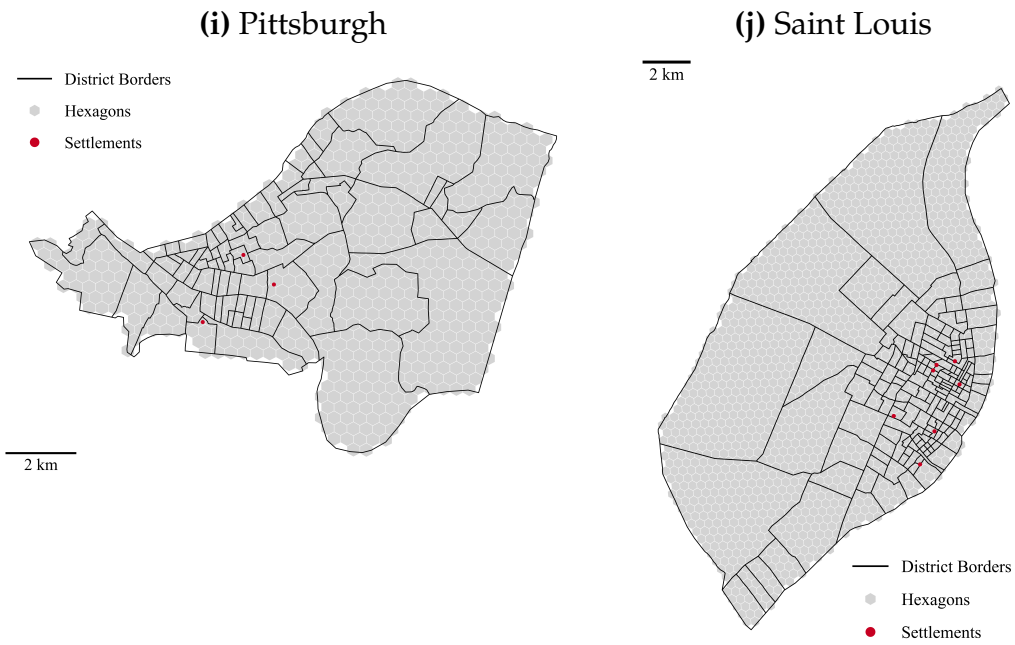
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 9, A8.

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



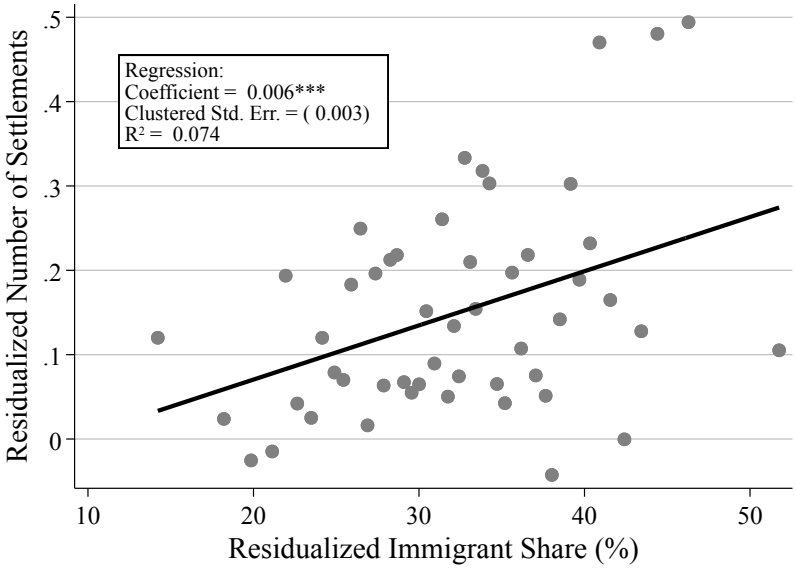
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 9, A8.

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



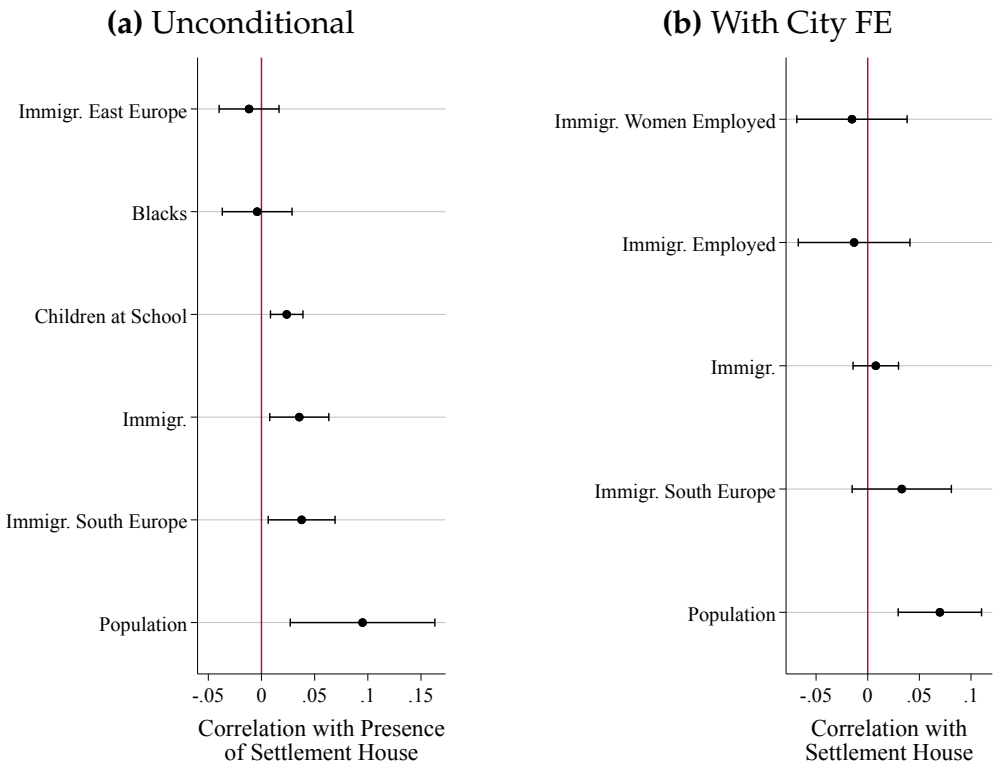
Notes. This figure plots the spatial distribution of settlements in each city within the analysis sample. The figure overlays the borders of 1880 neighborhoods (solid black lines), as well as the tessellation hexagons in gray. Referenced on page(s) 9, A8.

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 city level and the regression R^2. Referenced on page(s) 13, A5.

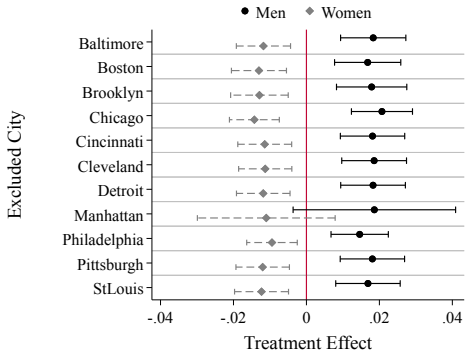
Figure C.3. Settlement Presence and Baseline Characteristics: LASSO Variable Selection



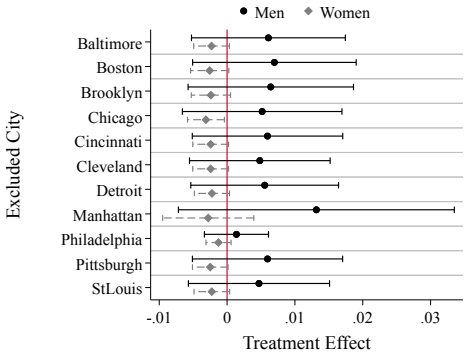
Notes. This figure reports the correlation between the presence of a settlement between 1882 and 1911 and hexagon-level demographic characteristics in 1880. Unlike in figure II, all co-variates are included in a single regression, and the dots report the corresponding coefficients. I employ the LASSO 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 city level; bands report 95% confidence intervals. Referenced on page(s) 14, A5.

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

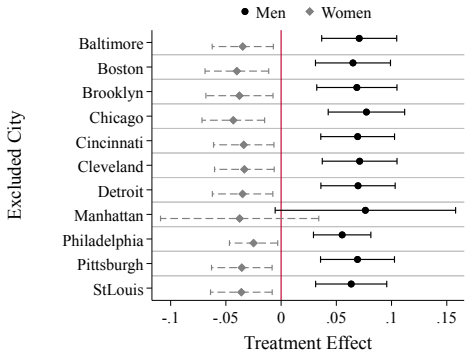
(a) Labor Force Participation



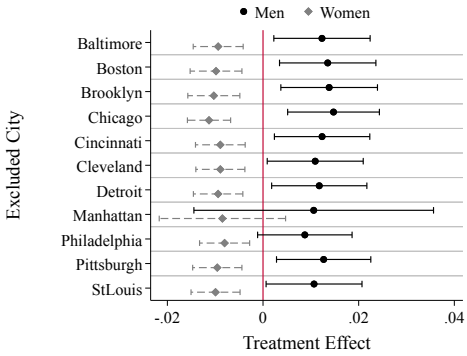
(b) High-Skill Employment



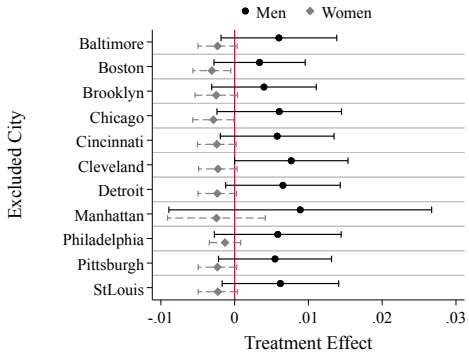
(c) Occupational Income



(d) White-Collar Employment

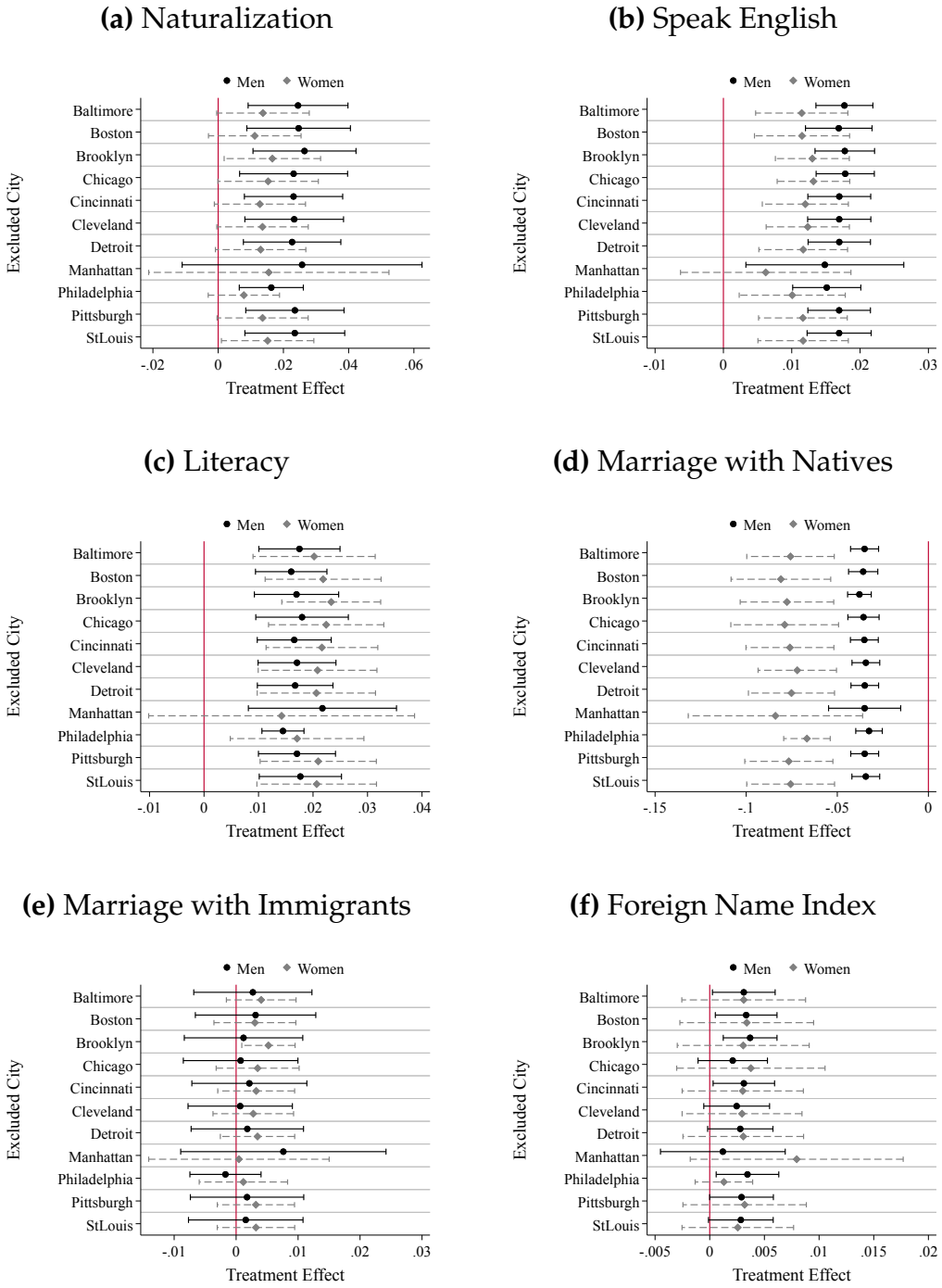


(e) Blue-Collar Employment



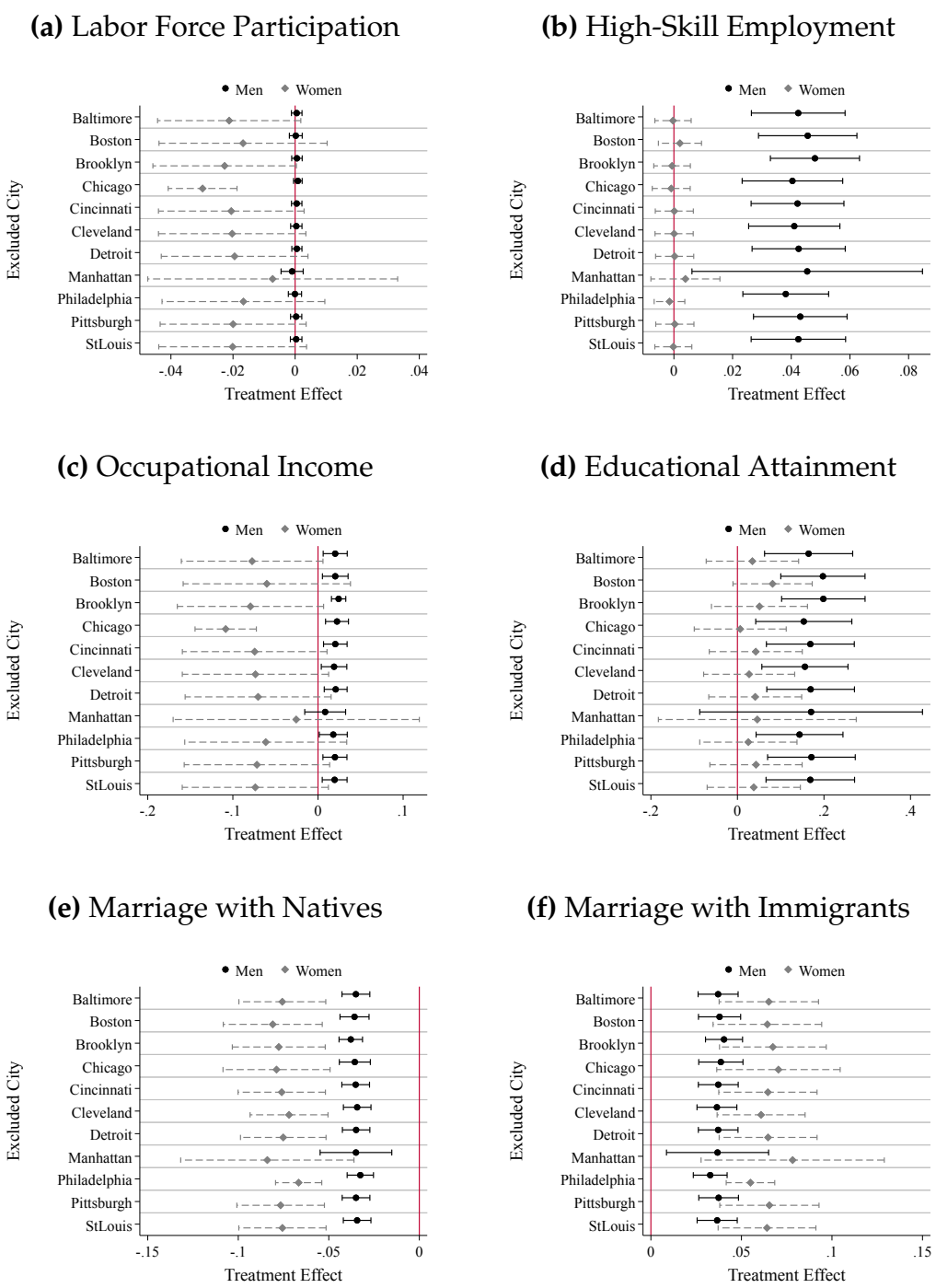
Notes. This figure reports the effect of settlements on labor-market variables dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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), high-skill employment (panel C.4b), income (panel C.4c), white-collar employment (panel C.4d), and blue-collar employment (panel C.4e). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level. Bands report 95% confidence intervals. Referenced on page(s) 17, A7.

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



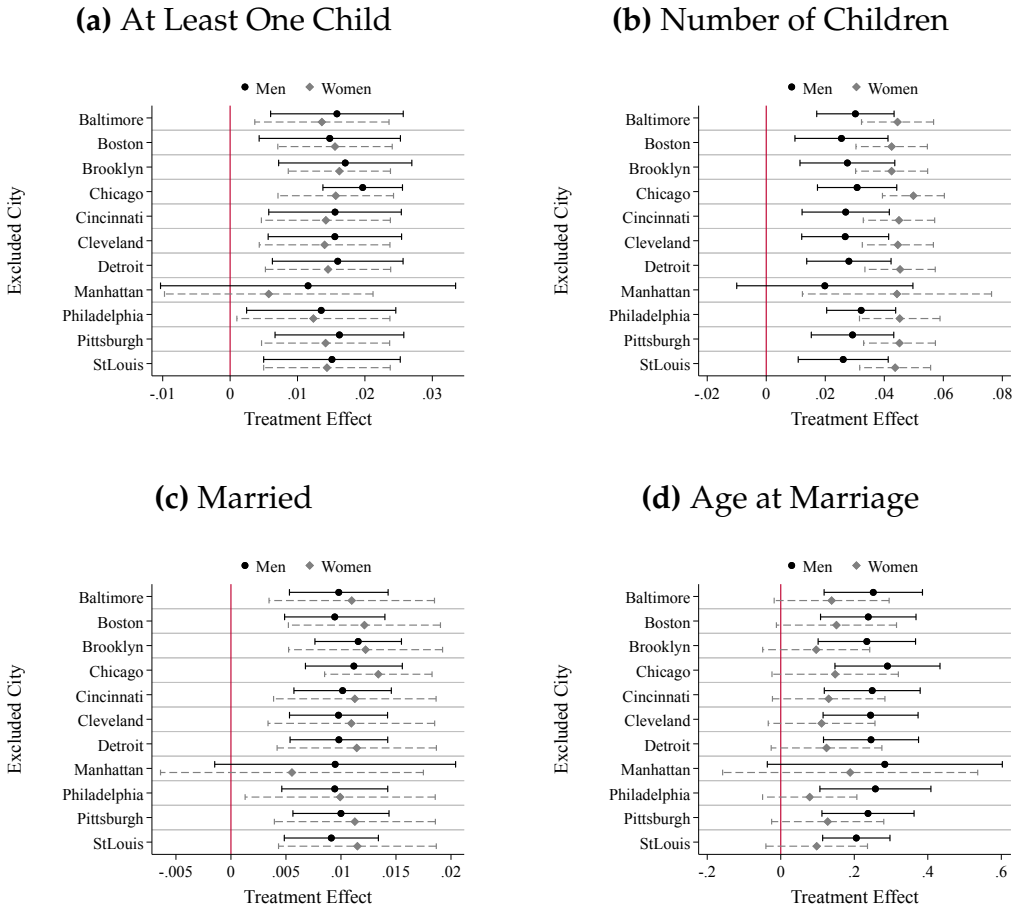
Notes. This figure reports the effect of settlements on assimilation variables, dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is naturalization (panel C.5a), English speaking (panel C.5b), literacy (panel C.5c), marriage with natives (panel C.5d), marriage with immigrants from other countries (panel C.5e), and the Foreign Name Index (panel C.5f). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level. Bands report 95% confidence intervals. Referenced on page(s) 17, A7.

Figure C.6. 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 once in the 1930 census. The sample comprises all individuals 15 years old or younger when the first settlement was established in their neighborhood in 1900, if any. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. 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.6a), high-skill employment (panel C.6b), occupational income (panel C.6c), educational attainment (panel C.6d), marriage with natives (panel C.6e), and marriage with immigrants from other countries (panel C.6f). All regressions include city-by-cohort fixed effects and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level. Bands report 95% confidence intervals. Referenced on page(s) 17, A7.

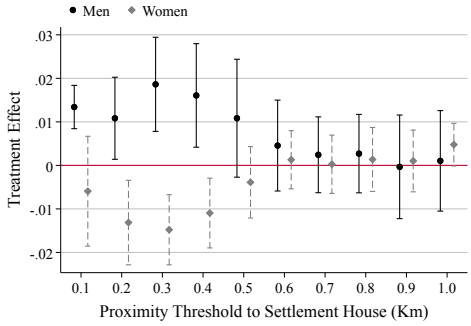
Figure C.7. Leave-out Estimates: Family and Fertility Effects of Settlement Houses



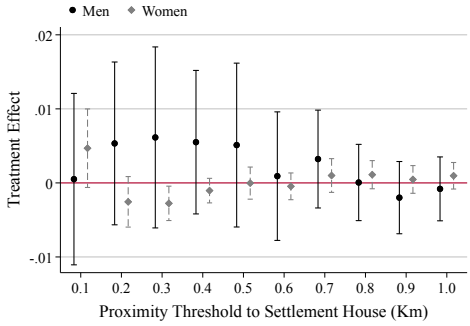
Notes. This figure reports the effects of settlements on family and fertility variables, dropping one city at a time from the estimation sample. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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.7a), the (IHS) number of children (panel C.7b), one for married individuals (panel C.7c), and the age at first marriage (panel C.7d). All regressions include neighborhood, city-by-cohort fixed effects, and individual controls—sex, birthplace, marital status in 1900, race, and immigration year. Standard errors are clustered at the city level. Bands report 95% confidence intervals. Referenced on page(s) 17, A7.

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

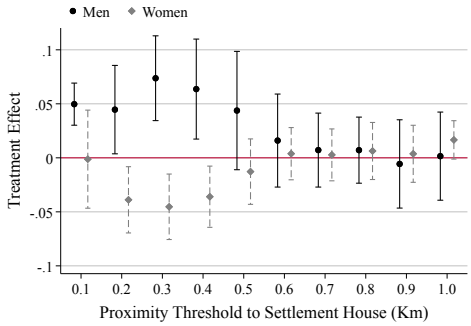
(a) Labor Force Participation



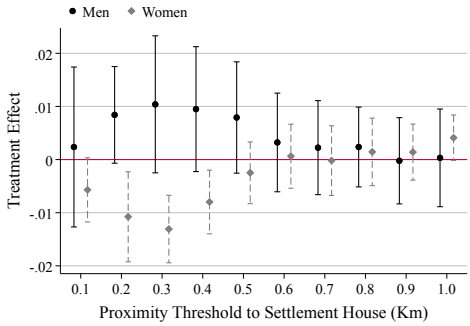
(b) High-Skill Employment



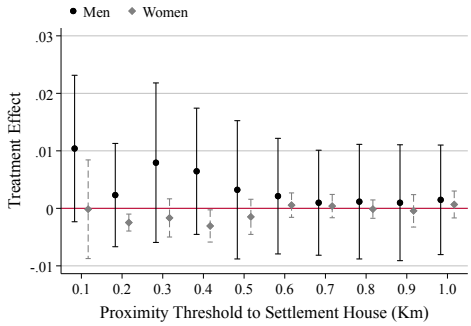
(c) Occupational Income



(d) White-Collar Employment

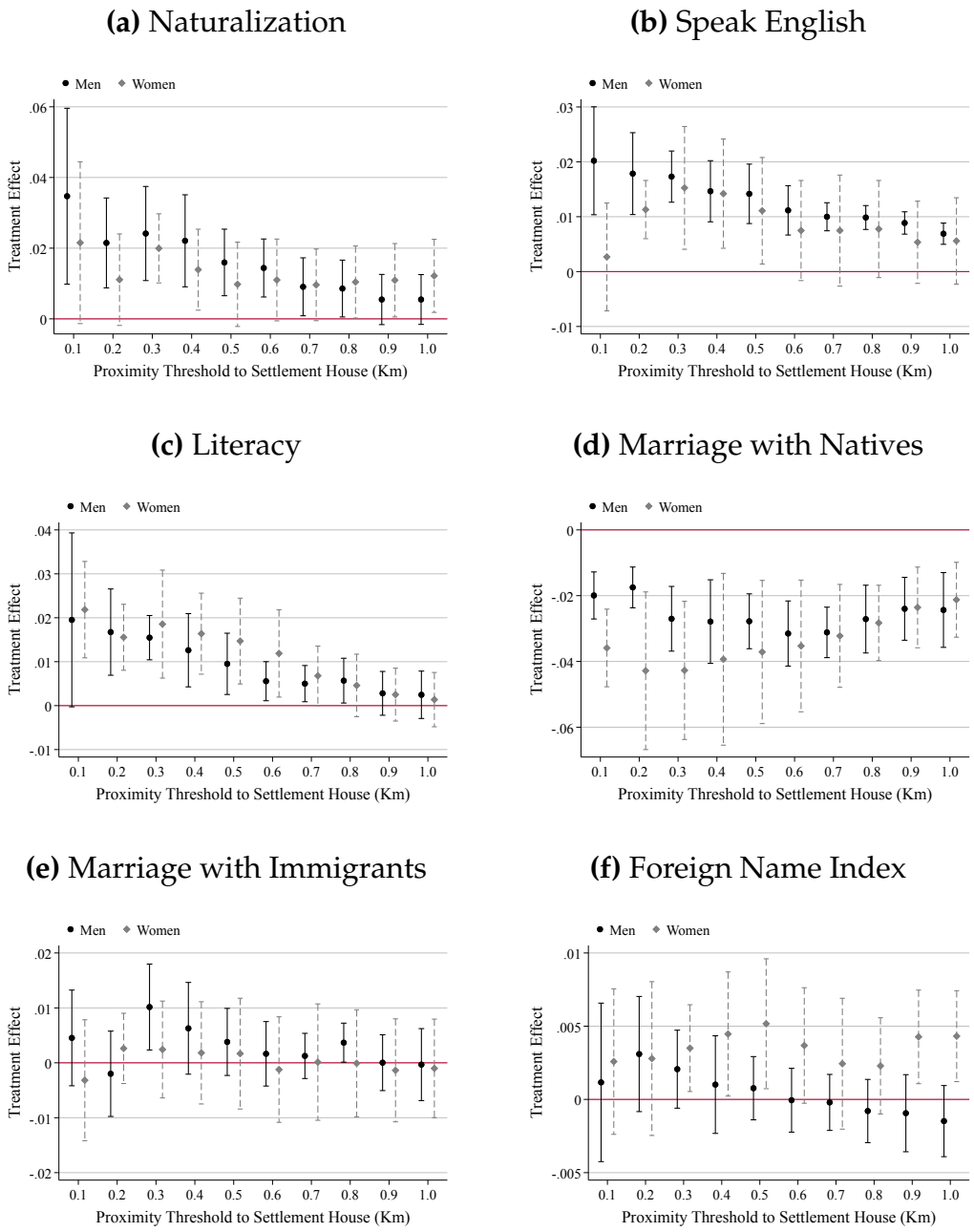


(e) Blue-Collar Employment



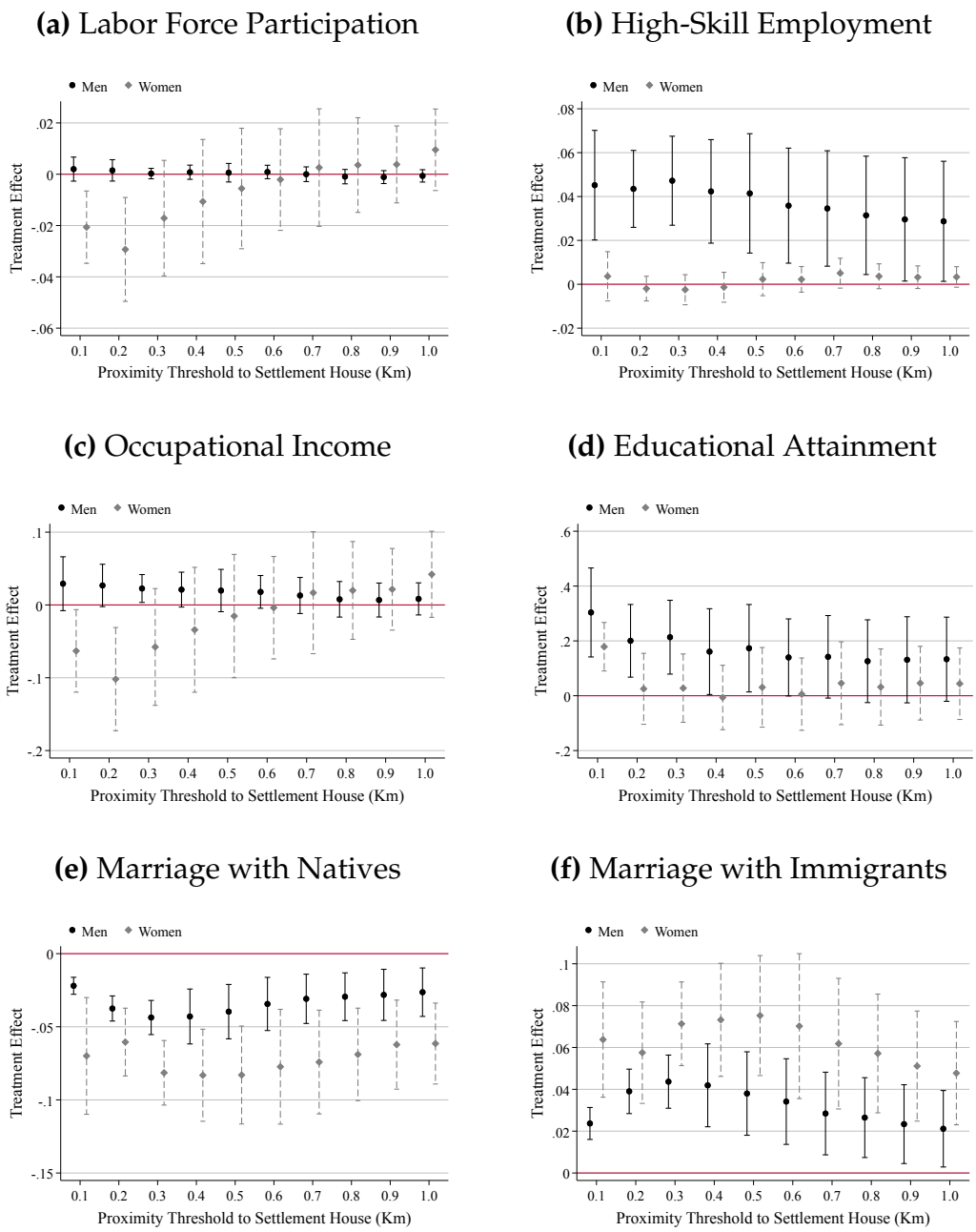
Notes. This figure reports the effect of settlements on labor-market variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood, if any, and zero otherwise. 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), high-skill employment (panel C.8b), income (panel C.8c), white-collar employment (panel C.8d), and blue-collar employment (panel C.8e). All regressions include neighborhood and city-by-cohort fixed effects. Standard errors are clustered by city. Bands report 95% confidence intervals. Referenced on page(s) 17, A8.

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



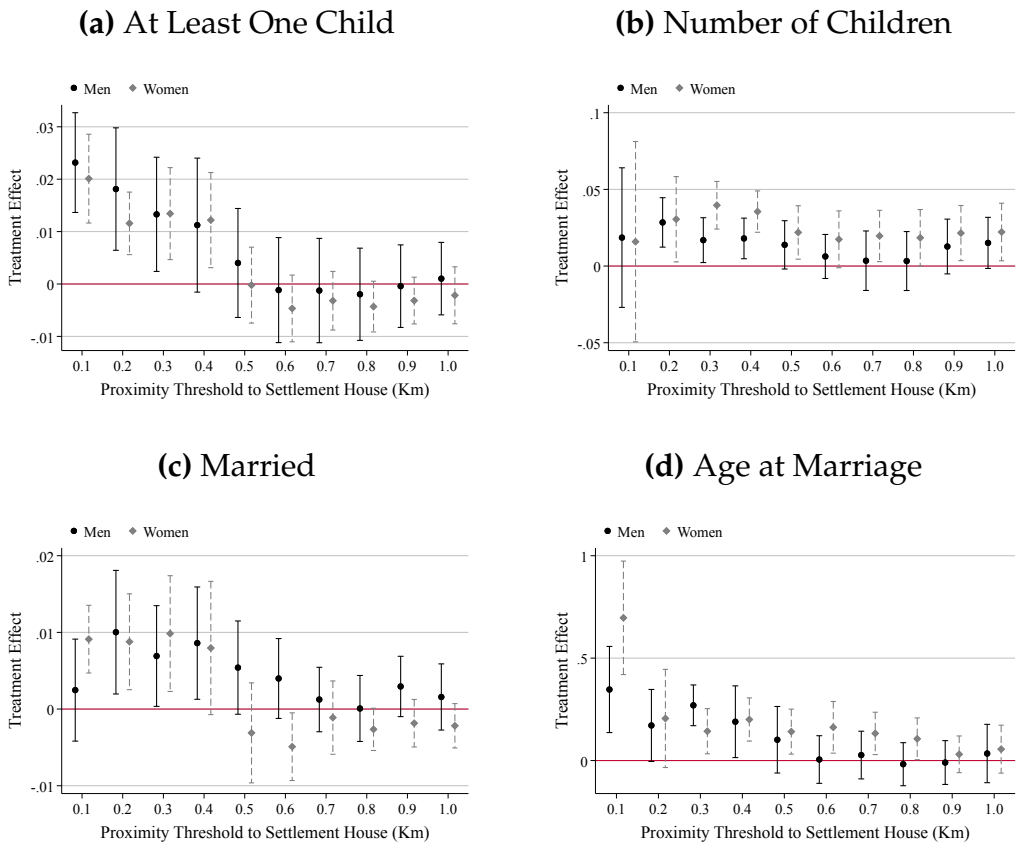
Notes. This figure reports the effect of settlements on assimilation variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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 naturalization (panel C.9a), English speaking (panel C.9b), literacy (panel C.9c), marriage with natives (panel C.9d), marriage with immigrants from other countries (panel C.9e), and the Foreign Name Index (panel C.9f). All regressions include neighborhood and city-by-cohort fixed effects. Standard errors are clustered by city. Bands report 95% confidence intervals. Referenced on page(s) 17, A8.

Figure C.10. Robustness to Distance Threshold to Settlement Houses: Intergenerational Effects



Notes. This figure reports the intergenerational effects of settlements using alternative proximity thresholds to settlements for the definition of the treatment. The sample comprises all individuals 15 years old or younger when the first settlement was established in their neighborhood in 1900, if any. The treatment is one for individuals who grew up in neighborhoods exposed 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. 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.10a), high-skill employment (panel C.10b), occupational income (panel C.10c), educational attainment (panel C.10d), marriage with natives (panel C.10e), and marriage with immigrants from other countries (panel C.10f). All regressions include city-by-cohort fixed effects. Standard errors are clustered by city. Bands report 95% confidence intervals. Referenced on page(s) 17, A8.

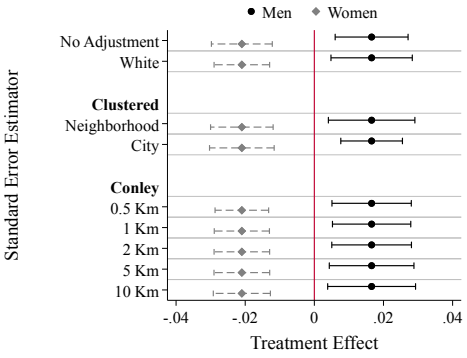
Figure C.11. Robustness to Distance Threshold to Settlement Houses: Family and Fertility Effects



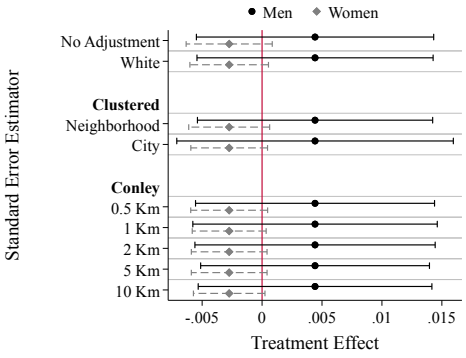
Notes. This figure reports the effects of settlements on family and fertility variables using alternative proximity thresholds to settlements for the definition of the treatment. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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.11a), the (IHS) number of children (panel C.11b), one for married individuals (panel C.11c), and the age at first marriage (panel C.11d). All regressions include neighborhood and city-by-cohort fixed effects. Standard errors are clustered by city. Bands report 95% confidence intervals. Referenced on page(s) 17, A8.

Figure C.12. Alternative Standard Errors: Labor Market Effects of Settlement Houses

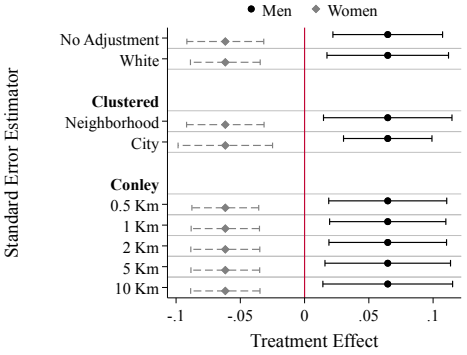
(a) Labor Force Participation



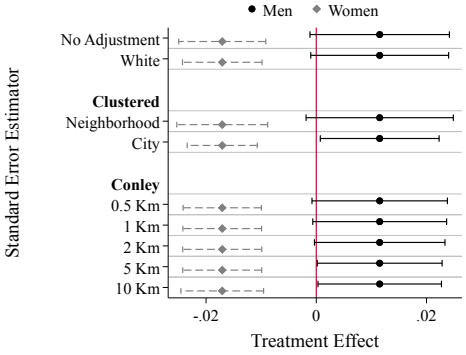
(b) High-Skill Employment



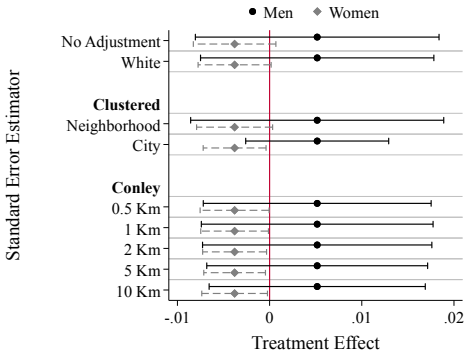
(c) Occupational Income



(d) White-Collar Employment

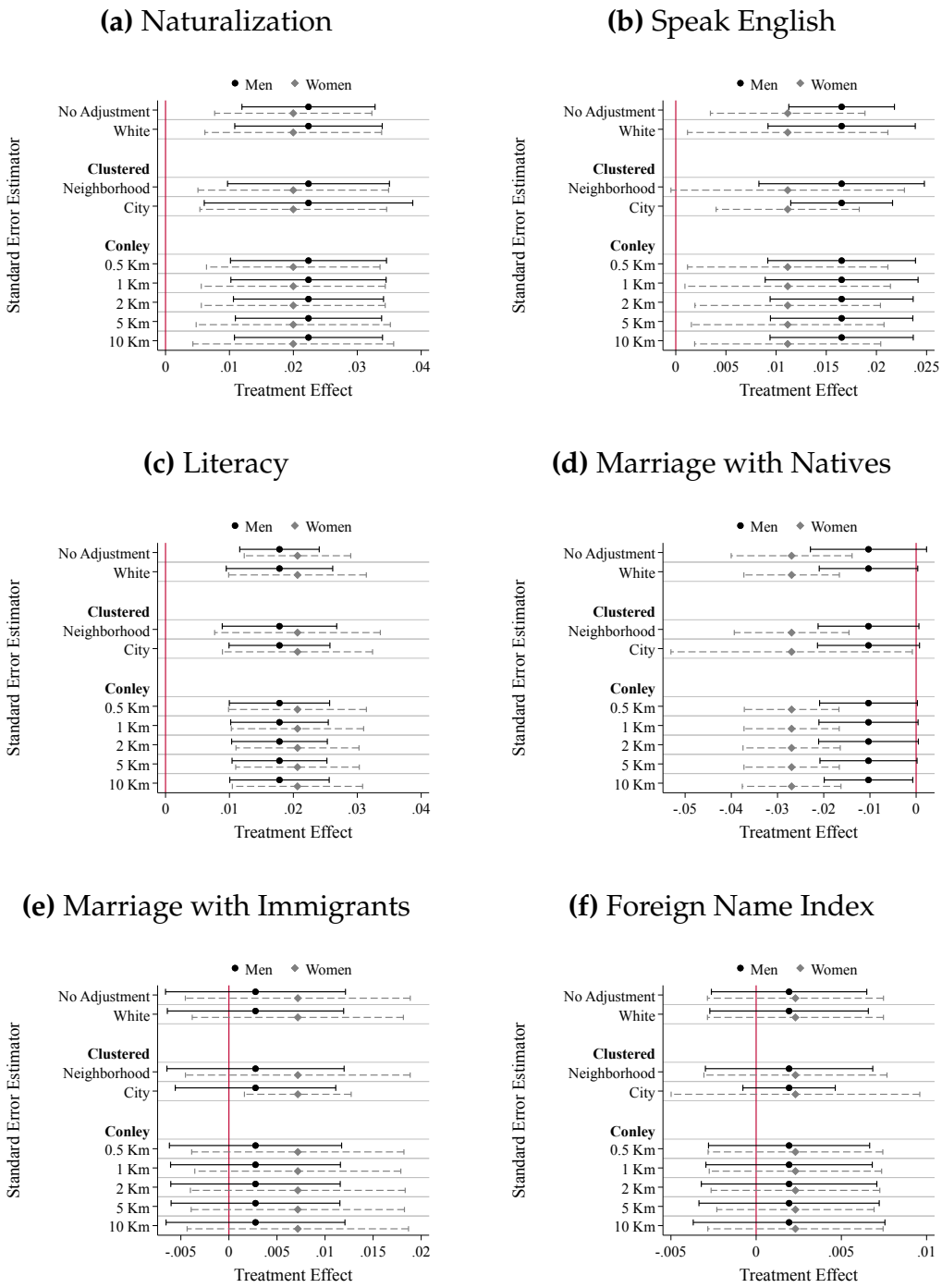


(e) Blue-Collar Employment



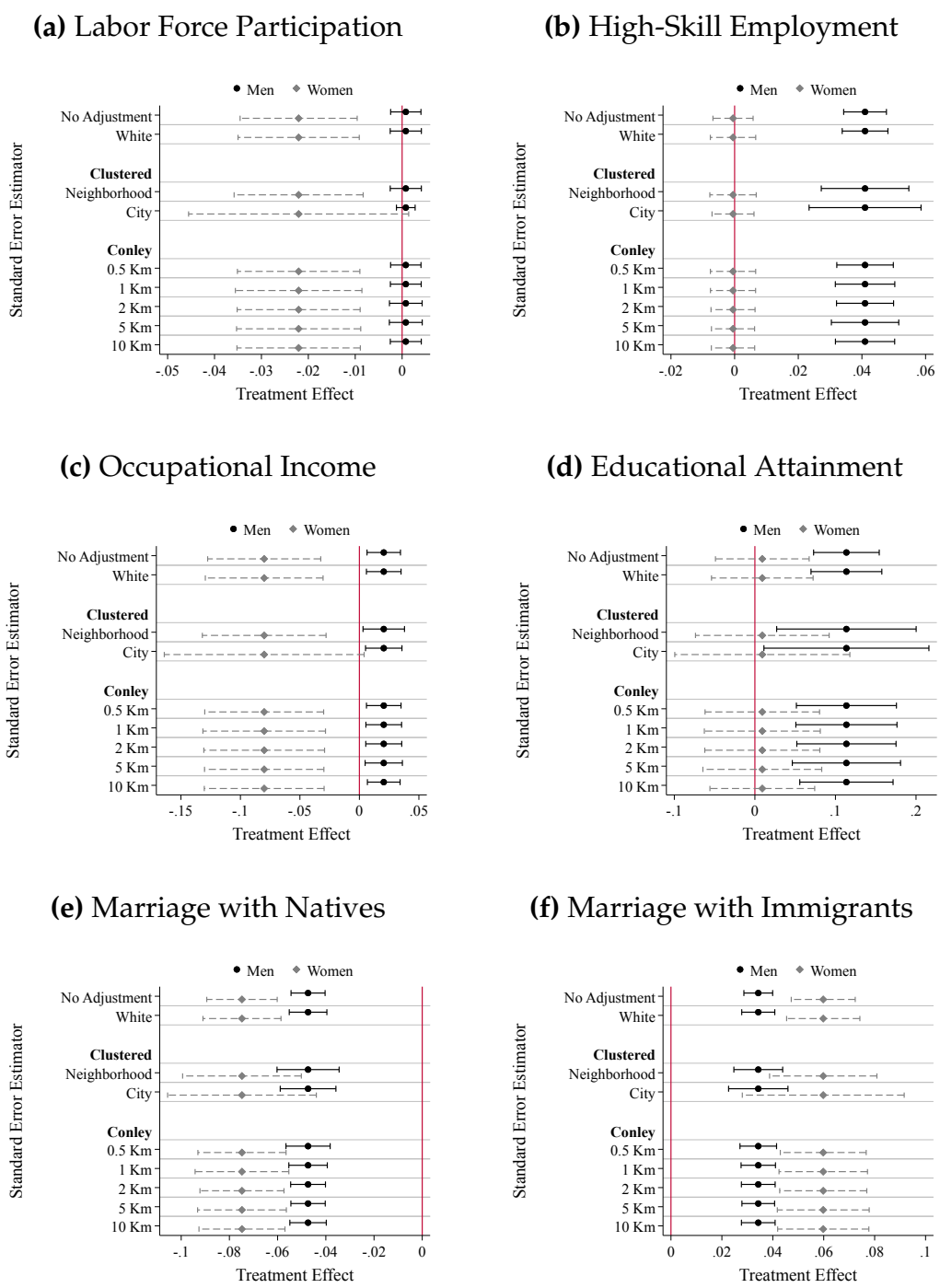
Notes. This figure reports the effect of settlements on labor-market variables using alternative estimators for the standard errors. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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.12a), high-skill employment (panel C.12b), income (panel C.12c), white-collar employment (panel C.12d), and blue-collar employment (panel C.12e). All regressions include neighborhood and city-by-cohort fixed effects. I report unadjusted, heteroskedasticity-robust, clustered—by district and city—and Conley (1999) standard errors. For Conley-corrected standard errors, I report various bandwidths between 0.5 and 10 kilometers of spatial autocorrelation. Bands report 95% confidence intervals. Referenced on page(s) 18, A8.

Figure C.13. Alternative Standard Errors: Assimilation Effects of Settlement Houses



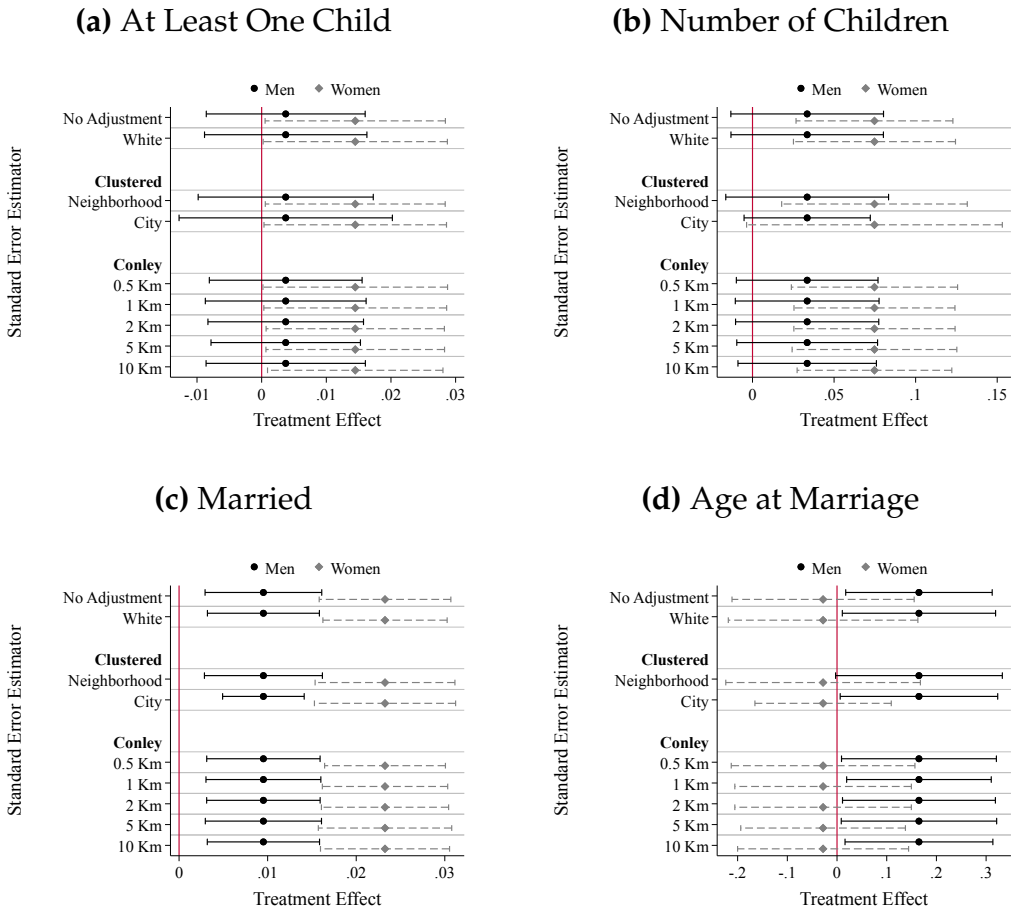
Notes. This figure reports the effect of settlements on assimilation variables using alternative estimators for the standard errors. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. Black dots refer to the sample of men; the gray dots report the treatment effect on women. The dependent variable is naturalization (panel C.13a), English speaking (panel C.13b), literacy (panel C.13c), marriage with natives (panel C.13d), marriage with immigrants from other countries (panel C.13e), and the Foreign Name Index (panel C.13f). All regressions include neighborhood and city-by-cohort fixed effects. I report unadjusted, heteroskedasticity-robust, clustered—by district and city—and Conley (1999) standard errors. For Conley-corrected standard errors, I report various bandwidths between 0.5 and 10 kilometers of spatial autocorrelation. Bands report 95% confidence intervals. Referenced on page(s) 18, A8.

Figure C.14. Alternative Standard Errors: Intergenerational Effects of Settlement Houses



Notes. This figure reports the intergenerational effects of settlements using alternative estimators for the standard errors. The sample comprises all individuals 15 years old or younger when the first settlement was established in their neighborhood in 1900, if any. The treatment is one for individuals who grew up in neighborhoods exposed to a settlement and zero otherwise. 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.14a), high-skill employment (panel C.14b), occupational income (panel C.14c), educational attainment (panel C.14d), marriage with natives (panel C.14e), and marriage with immigrants from other countries (panel C.14f). All regressions include city-by-cohort fixed effects. I report unadjusted, heteroskedasticity-robust, clustered—by district and city—and Conley (1999) standard errors. For Conley-corrected standard errors, I report various bandwidths between 0.5 and 10 kilometers of spatial autocorrelation. Bands report 95% confidence intervals. Referenced on page(s) 18, A8.

Figure C.15. Alternative Standard Errors: Family and Fertility Effects of Settlement Houses



Notes. This figure reports the effects of settlements on family and fertility variables using alternative estimators for the standard errors. The unit of observation is an individual immigrant observed once in the 1930 census. The treatment variable is one for individuals younger than 35 when the first settlement was established in the proximity of their 1900 neighborhood if any, and zero otherwise. 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.15a), the (IHS) number of children (panel C.15b), one for married individuals (panel C.15c), and the age at first marriage (panel C.15d). All regressions include neighborhood and city-by-cohort fixed effects. I report unadjusted, heteroskedasticity-robust, clustered—by district and city—and Conley (1999) standard errors. For Conley-corrected standard errors, I report various bandwidths between 0.5 and 10 kilometers of spatial autocorrelation. Bands report 95% confidence intervals. Referenced on page(s) 18, A8.

APPENDIX REFERENCES

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