SVERIGES RIKSBANK
WORKING PAPER SERIES 333

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December 2016 (Updated October 2017)

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Financial Literacy Externalities*

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October 25, 2017

Abstract

This paper uses unique administrative data and a quasi-field experiment of exogenous allocation to apartments in Sweden to estimate medium- and longer-run effects on financial behavior from exposure to financially literate neighbors. It contributes evidence of causal impact of financial literacy and points to a social multiplier of effective programs to enhance it. Exposure promotes saving in private retirement accounts and stockholding, especially when neighbors have economics or business education, but only for educated or male-headed households. Findings point to relevant knowledge transfer through social interactions rather than to labor market or other channels linked to local economic conditions.

Keywords: Household finance, financial literacy, social interactions, refugees
JEL Codes: G11, E21, D14, F22, I28

*We would like to express our thanks to the many colleagues who provided comments and suggestions. Special thanks go to Dionissi Aliprantis, Tobias Berg, Tabea Bucher Koenen, Hector Calvo Pardo, Dimitris Christelis, Henrik Cronqvist, Erik Hurst, Arthur Kennickell, Theresa Kuchler, Ekaterini Kyriazidou, Wenlan Qian, and Johannes Stroebel. We would like to thank participants in the 2016 meetings of the International Association of Applied Econometrics, the 2016 C.R.E.T.E. conference, the CEPR European Workshop on Household Finance, the CEPR Second Annual Spring Symposium in Financial Economics, the Second Israel Behavioral Finance Conference, the NETSPAR International Pensions Workshop, the Eurozone Household Finances and Consumption Network, the Research Workshop in Financial Economics at the University of Bonn, the DFG SPP Conference, and the Sheffield Workshop on Household Finance; as well as seminar participants at the Bank of Spain, Collegio Carlo Alberto, ETH Zurich, the Joint Research Committee of the European Commission, and the National University of Singapore for their comments and suggestions. The views described in this paper are the authors' own and do not necessarily reflect those of the Sveriges Riksbank. Haliassos acknowledges support from the German Research Foundation (DFG).

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1 Introduction

Confronted with the demographic transition and rapid financial innovation, households make complicated financial choices with important and lasting consequences for their economic well being. Research on financial literacy, developed over the past decade, has established widespread presence of financial illiteracy, as well as a strong correlation between low financial literacy and negative financial outcomes at the household level.[1] Establishing causality from financial literacy to economic outcomes has been more challenging but of profound importance for policy choices in the presence of competing approaches to empowering households.[2] In principle, unobserved factors can lead an individual both to become financially literate and to have good financial outcomes, without a direct causal link between the two. Reverse causality is also plausible, e.g., from saving for retirement to learning about financial concepts. Moreover, even if there is an exogenous influence of financial literacy on outcomes, the cost effectiveness of suitable programs for its promotion may be challenged if they can only reach limited segments of the population (e.g., school children) and their long-lasting impact on behavior is not firmly established.[3]

Existing literature has sought to measure own financial literacy and to study the role that it can play for household outcomes. Calvet et al. (2009) use observable characteristics, such as household size and financial wealth, as well as education and financial experience proxies, to measure own financial sophistication of households by relating household at-

[1] See Lusardi and Mitchell (2014) for an excellent survey. Outcomes include lack of saving for retirement, lower wealth, stock market non-participation, use of higher cost credit, being in credit arrears, and recently also wealth inequality (Lusardi and Mitchell, 2007; van Rooij et al., 2011; Disney and Gathergood, 2013; Lusardi and Mitchell, 2014; Lusardi et al., 2016).

[2] These include financial regulation, financial advice, and default options in addition to financial education.

[3] Hospido et al. (2016) recently found that financial education programs are effective in improving financial literacy test scores of treated school children. Alan and Ertac (2016) conduct experiments with an educational program in primary schools and find an educational program in primary schools effective for encouraging school children to exhibit greater patience when making intertemporal choices in incentivized experimental tasks, also three years later. Brown et al. (2015) exploit variation in the enactment of financial and economics education reforms in high school curricula within and across US states to show that reforms have significant (though moderate and opposite) effects on the debt-related outcomes of 19- to 29-year-olds: the tendency to hold debt and to run into repayment difficulties are somewhat reduced by financial education and increased by economics education.

tributes to investment mistakes. A number of papers have used scores on the "Big 3" financial literacy questions of Lusardi and Mitchell (2007) to measure own financial literacy, and instruments for such literacy in order to estimate its effects on financial behavior, either going back to early life events or looking at environmental factors.[4]

This paper takes a different approach and is the first to study financial literacy externalities, defined as the potential for financially literate neighbors to have an exogenous (positive) influence on economic choices of households. Financial literacy externalities reinforce and extend the notion of human capital externalities, in the spirit of Acemoglu (1996) and Acemoglu and Angrist (2001). Establishing the presence of financial literacy externalities for behavior over a longer horizon can strengthen significantly the case for promoting financial literacy, not only by showing a lasting exogenous effect on outcomes but also by demonstrating greater cost effectiveness of relevant programs resulting from a social multiplier.

In the context of establishing financial literacy externalities, a thorny identification issue is posed by the typically endogenous choice of neighborhood. Sorting into neighborhoods with greater financial literacy may arise from unobserved characteristics of people (such as interest in financial matters) and of the area (such as availability of financial services and advice) and thus correlate with good financial choices without implying causality. We are able to tackle this issue by utilizing high-quality administrative data and tracking over a twenty-year period a group of people initially allocated to apartments by a government agency: refugees assigned to specific apartments through a nation-wide placement program. This natural experiment has been fruitfully used in existing literature because of its attractive properties, but for very different purposes than ours.[5]

[4]Instruments have included understanding of financial matters by parents as perceived by the respondent, self-reported mathematics grades at age 10, institutional changes affecting early education, or introduction of financial education requirements interacted with State spending on education. See, for example, Lusardi and Mitchell (2009), van Rooij et al. (2011), and Jappelli and Padula (2013).

[5]Edin et al. (2003) study the consequences of living in enclaves for labor market outcomes. Åslund and Fredriksson (2009) study peer effects in welfare use among refugees, while Åslund et al. (2011) focus on the extent to which immigrant school performance is affected by the characteristics of neighborhoods in which they grew up.

Exploiting exogenous variation in financial literacy at the neighborhood of initial placement, we study financial behavior ten to twenty years later to uncover lasting causal effects of financial literacy in the neighborhood of initial placement over the medium and longer runs. As we know the precise location of refugee immigrants, we are able to control both for economic conditions in the immediate neighborhood of placement (electoral district), as well as for unobserved features of the greater area of placement (parish)[6] to which the initial neighborhood (electoral district) belongs. We also control for a wide range of household characteristics at the time of observing financial behavior, as well as for macroeconomic conditions. We investigate possible confounding factors that could generate effects through channels other than social interactions and dissemination of financial knowledge (such as labor market channels or correlated effects) and argue that these are not responsible for the observed effects. We also explore the channels through which financial literacy externalities operate, including importance of content of neighbor education, ability of refugee household heads to process relevant information, likelihood of interaction, and the importance of imitation.

We consider the causal effect of exposure to financially literate neighbors on two aspects of financial behavior: participation in private retirement accounts (as distinct from social security contributions and occupational pension plans); and stockholding. We find more sizeable effects of the share of neighbors with specialized knowledge of economics or business rather than of those with quantitative education in general, and no evidence that effects are linked to the level rather than the content of neighbor education. We also find that externalities are operative only for the more educated refugees and for male-headed households, and that their size is plausibly linked to the likely intensity of interactions. We find no influence of the initial share of financially literate neighbors on future employment and locational prospects of the refugees, except in encouraging them to get a job in the fi-

[6]Relevant features of the broader shared environment include the quality of public amenities and the penetration of the financial sector in a given neighborhood (Oreopoulos, 2003; Manski, 1993). In the terminoloy of Manski (1993), these would be 'correlated' effects rather than social effects. See also (Damm and Dustmann, 2014).

nancial sector. All in all, our findings suggest that financial literacy externalities involve the transfer, processing, and salience of information, as well as confidence to use it.

In addition to financial literacy, our paper links to two other strands of literature. One studies peer effects on financial behavior, following seminal work by Duflo and Saez (2002), who found evidence that observing a higher share of workplace peers invest in a particular retirement product increases the probability that the respondent will also invest in the product.[7]

The other strand studies immigrant financial behavior with an emphasis on establishing links to culture (see Guiso et al. (2006) for a useful framework).[8] Our use of a refugee sample serves as a useful identification device of long lasting effects of exogenous placement on economic behavior in a modern developed economy, while the time distance of ten to twenty years from initial entry ensures that financial behavior is observed at an advanced stage of the assimilation process.[9] The location in a highly advanced country, the time distance to initial entry, and our controls (e.g., for attitudes towards immigrants) make it unlikely that our findings are specific to refugee status and inapplicable to the wider population.

Nevertheless, and while identification has been our primary motivation, a focus on refugees is of interest in its own right, given the current intense debate on accepting and placing them. In this different context, our analysis points to long-lasting effects of the initial placement of refugees on their subsequent economic behavior. This implication parallels

[7]Hong et al. (2004) found that sociability, proxied by church attendance, participation in social clubs and similar activities, is related to greater tendency to hold stocks. Kaustia and Knuepfer (2012) found that the stock market performance of neighbors influences stock market entry. Georgarakos et al. (2014) found that those who perceive themselves as earning less than the average of their peers are more likely to borrow, to borrow larger amounts, and to worsen their indicators of potential financial distress.

[8]In a pioneering paper, Carroll et al. (1994) examined the role of culture for saving patterns at the individual level, while Guiso et al. (2006) looked at national saving rates. Osili and Paulson (2008) found a link between the degree of investor protection in the country of immigrant origin and the probability of the immigrant to participate in the stock market. Guiso et al. (2004) focused on use of basic financial instruments, such as writing a check or purchasing a share, and found that this is affected by the level of social capital. Guiso et al. (2006) provided evidence that trust is influenced by ethnic origin in US data, while Guiso et al. (2003) found evidence that trust is influenced by religion, both pointing to the relevance of culture. Haliassos et al. (2016) found that financial behavior differs across cultural groups of migrants, controlling for a range of characteristics, but these differences diminish with exposure to host country institutions.

[9]For evidence on the speed of assimilation of financial behavior of immigrants to Sweden, see Haliassos et al. (2016).

and extends work on long term implications of interventions to allow disadvantaged families to move to better neighborhoods.[10] Our work also links to the literature on early-life influences on financial behavior inspired by the paper of Malmendier and Nagel (2011) with the important difference that subsequent exposure to macroeconomic variables is plausibly exogenous, while staying in the neighborhood is endogenous.

Section 2 describes features of the refugee settlement program relevant for our analysis. Section 3 presents the estimation model, while section 4 describes the data, our sample construction, and our proxies for financial literacy externalities. Section 5 reports our findings and investigates alternative interpretations. Section 6 explores the channels of influence, while section 7 concludes and presents policy implications for programs intended to enhance financial literacy and for placement of refugees. The Online Appendix contains descriptive statistics and more detailed presentation of estimates and robustness exercises.

2 Relevant features of the refugee placement policy

We exploit a rare natural experiment, a Swedish policy of exogenously allocating refugees to apartments, which has not previously been applied to financial behavior and financial literacy.[11] Between 1985 and 1994, the Swedish Immigration Board had the task of placing refugees who moved to Sweden for reasons other than family reunification in particular apartments. The policy was implemented in response to complaints from certain municipalities that they were bearing disproportionate burdens of absorbing immigrants in the 1980s, and was most strictly applied between 1987 and 1991, the period of our attention. 277 out of Sweden's 284 municipalities participated in the program. Placement by the municipal officers to a specific apartment occurred shortly after the refugee obtained a residence per-

[10] See in particular a recent paper by Chetty et al. (2016). They analyze the long-term effects of the Moving-to-Opportunity (MTO) program that offered randomly selected families the opportunity to move from high-poverty neighborhoods to lower-poverty neighborhoods and document that the children who moved to lower-poverty areas at a younger age are more likely to attend college and have higher earnings as adults. The paper also includes references to work on other outcomes of the program.

[11] For further details about this policy experiment, used in another context, see Edin et al. (2003) pp. 333-335.

mit. STATIV data, described in section 4.1 below, allows us to identify precisely the refugees among migrants to Sweden in the relevant period who were not being reunited with family members, had limited resources and therefore little choice but to accept the allocation decision of municipal officers.

Our causal analysis relies on the assumption that, given the observed characteristics of the refugees, the characteristics of initial location on which we focus (share of financially literate neighbors by different metrics) are independent of unobserved refugee characteristics determining the probability of outcomes we study (saving for retirement through private accounts, or holding stocks) ten to twenty years later in life.

The way in which the placement program assigned refugees to particular apartments is important for the validity of this identification assumption. If refugees were placed in those neighborhoods on the basis of applicant characteristics unobserved to us, and these characteristics both contributed to subsequent stockholding and private retirement saving of refugees and accounted for the presence of a larger share of financially literate neighbors, then our identification assumption would be violated.

How could refugee characteristics for which we do not control enter the determination of initial placement? One channel might be provision of information regarding characteristics to the placement officers outside what is recorded in the data and observable to us. This issue does not arise in our sample, as there was no interview and no further contact between the officers and the refugees: the immigration officers observed the same refugee characteristics as we do.

Second, observable refugee characteristics might influence the allocation to a particular apartment, because they were used for this purpose by immigration officials, but they are not included in our estimation. Narratives of the allocation process[12] make clear that the dominant factor influencing allocation by immigration officers was whether an apartment became available or not. In cases where some choice was available to the immigration offi-

[12]See Åslund and Fredriksson (2009) and Åslund et al. (2011).

cers, the narratives state that program officers might take into account the education level of the refugee, whether others speaking the same language lived in the area of placement under consideration, and whether the refugee was married or single, given limited availability of small apartments. Accordingly, in our estimation model we control for the country of origin and year of arrival of the refugee, the refugee's education level, marital status, household size, and number of children, as well as for other observable characteristics relevant for financial behavior (see section 3).

Third, as a further check of possible sorting, we regress the share of financially literate neighbors in the initial location on initial characteristics of the refugees observable to municipal officers. Table II presents results for two alternative definitions of financially literate neighbors, the first based on those with economics or business education and some college attendance, and the second based on the share of those with quantitative education (including business and economics but not confined to this) and some college attendance. In each case, we include gender, marital status, household size, number of children, educational attainment, and age group controls, as well as parish, country of origin, and arrival year fixed effects. As indicated in the Table, the measures of neighborhood financial literacy on which we rely are independent of initial refugee household characteristics observable to municipal officers, including those they might have reportedly taken into account.

Refugees were also asked to state their preferences, despite the apartment availability constraints under which the placement program was operating. This raises the possibility that refugee preferences (unobserved by us) had some influence on placement, and these locational preferences were themselves linked to unobserved factors relevant for asset participation. A number of considerations counter this possibility. First, descriptions of the process and interviews with placement officers (Åslund et al., 2011) make it clear that the key limiting factor in placement was the availability of an apartment and not the preferences of refugees. Second, this is corroborated by revealed preferences of refugees. Refugees tended to apply for placement in the largest and better known cities, but the economic boom

meant that very few places were available there. The allocation of refugees through the program differed from the pre-existing endogenous allocation across the country, as well as from the allocation that was observed after sufficient time had elapsed for refugees to relocate on their own. Such relocation was initially precluded by short-term benefits available at the initial location (e.g., being able to enroll in language classes), but about 75 percent of refugees had relocated from the place of initial placement by year 1999. We should stress here that this last figure should not be interpreted as suggesting that the refugees had only limited exposure to their initial neighborhood. In fact, refugees spent an average of 5.4 years in their parish of initial allocation and 8.7 years in the (broader area of the) initial municipality.

Finally, as we describe in detail in section 3, we also include in our model controls for economic conditions in the electoral district of initial location, and for time-invariant factors in the broader area of the parish, and we estimate the effect of the share of financially literate neighbors net of those conditions.

3 The Estimation Model

We focus mainly on two aspects of financial behavior, participation in stocks (directly or in vehicles other than those linked to retirement) and active saving for retirement through private accounts (as distinct from social security and occupational pension schemes) in the period of observation. In modeling outcomes, we estimate the impact of measured exposure to financial literacy externalities in the initial neighborhood of assignment (electoral district), controlling for a wide range of observable household characteristics, economic characteristics of the immediate neighborhood (electoral district), and a number of fixed effects, including one for conditions in the broader area of placement, the parish.

In our benchmark regression (1), we estimate a model of the following form:

$$Y_{iklj0t} = \alpha_1 \cdot X_{it} + \alpha_2 \cdot X_{l0} + \beta \cdot FLShare_{l0} + \gamma_I + \gamma_j + \gamma_k + \gamma_0 + \gamma_t + \epsilon_{iklj0t} \quad (1)$$

where Y_{iklj0t} refers to the relevant aspect of financial behavior of household i from country of origin k that arrived in year $0 \in \{1987, 1988, 1989, 1990, 1991\}$, was initially placed in electoral district l and parish j and is observed in period t. $FLShare$ is the (inverse hyperbolic sine function, IHS, of the) share of financially literate neighbors in the household's initial electoral district, l, in the year of arrival, 0.[13] For our medium run analysis, the observation years are $t = 1999, .., 2003$, while for the longer-run analysis, the corresponding years are $t = 2004, .., 2007$.

The coefficient of interest is that on the share of financially literate neighbors. As placement in the initial electoral district is exogenous to the refugee, we do not use instrumental variable estimation but can use OLS or probit estimators for the causal effect of interest. Such estimation allows financial literacy in the initial location to influence subsequent financial behavior through various channels other than those for which we explicitly control.

We are able to control for a wide array of observable household characteristics, denoted by X_{it}. These include disposable household income, age categories, gender, occupational status (unemployed, retired, employed, student), marital status, number of adults in the household, number of children in the household, educational attainment (less than high school, high school and college graduate), position of the household in the distribution of net wealth (except that, when we consider stocks, we exclude the asset class in question from the computation of net wealth), and working in the financial sector or working for the government, all measured in the year of observation of financial behavior, t. We use the inverse hyperbolic sine (IHS) transformation of household disposable income and of the financial literacy share.

As our household controls include labor market outcomes, a possible concern may be that

[13] Essentially, the coefficient on an IHS can still be thought of as a semi-elasticity, but the IHS transformation is less restrictive than the logarithmic one.

the share of financially literate neighbors operates by influencing such outcomes and the latter should be replaced by initial characteristics of refugees at the time of allocation. We do not opt for such an approach for two reasons. First, the initial labor market characteristics of refugees are very special because of their refugee status: unemployment or very low incomes are standard, without being very relevant to the subsequent labor market status of such migrants. Secondly, we test for the relevance of financially literate neighbors in the original electoral district for subsequent labor market outcomes, and we find no such evidence, except for encouraging work in the financial sector.

We also control for relevant characteristics both of the immediate neighborhood of initial location, the electoral district, and the broader area, the parish. Parishes represent the smallest administrative and political subdivision in Sweden. In 2000, there were 2,482 parishes[14] and approximately 5,700 electoral districts in Sweden, each typically with 200 to 2000 people. In the Stockholm municipality, with total area of 187.17 square kilometers, there are 537 electoral districts. This suggests an average size of 590x590 meters (for a reference case of square electoral districts) in Stockholm. Sizes for other areas can be constructed, confirming the notion of a small neighborhood in a typically much bigger parish.[15]

We consider immediate neighbors in the electoral district where the refugee was initially placed, but also recognize that workers typically cross electoral district borders in order to access their workplace, and their financial behavior can be influenced by conditions in a broader area. Economic characteristics of the electoral district at the time of initial allocation for the respondent, period 0, are denoted by X_{l0}. These include median household income, median taxable wealth, and median household debt-to-income ratio in the electoral

[14]The median individual lived in a parish with 8,660 inhabitants, while the median refugee lived in a parish with 14,148 in 2000, suggesting more concentration in metropolitan areas.

[15]The implied average size of electoral district is the same for the much smaller city of Lund, which has 74 electoral districts and 25.75 square kilometers. The average size for a particular Stockholm parish in the inner city with 5-storey buildings (Hedvig Eleonora, depicted in our Figure O.A.I) is only 274x274 meters with an average population of 1368 people. Finally, a city close to Artic Line (Lulea) has 44 electoral districts and an area of 29.09 squared kilometers, implying an average electoral district size of 813x813 meters. Information is available at $http://www.scb.se/sv_/Hitta-statistik/Regional-statistik-och-kartor/Statistikatlasen/Valen-2010-i-interaktiv-kartform/$. For the number of electoral districts, see $http://val.se/$.

district as a proxy for financial development (analogous to the often-used private-credit-to-GDP ratio). We also introduce fixed effects for the most important industry in the electoral district at the time of initial placement, denoted by γ_I. In order to control for conditions in the broader area of placement, the parish, relevant for financial market behavior, we introduce fixed effects γ_j, where j denotes the initial parish. Parish fixed effects are identified, both because the arrival year of refugees to that initial parish is not the same, and because the parish typically includes more than one electoral districts. Both factors create variation in the initial share of financially literate (electoral-district) neighbors for refugees at the same initial parish.

Further, we introduce fixed effects for the country of origin, γ_k, to capture language- and culture-related factors; and fixed effects for the year of arrival, γ_0, and the year of observation, γ_t, to capture macroeconomic or institutional factors prevailing at the time of initial entry and the period of observing financial behavior. We correct standard errors by clustering at the initial electoral district level.

4 Data and Measurement of Externalities

4.1 Data and sample construction

We use the LINDA and STATIV databases from Statistics Sweden for the years 1987 to 2007 to identify refugee immigrants and their reasons for immigration, characteristics of the households in the neighborhood of each respondent, and household financial behavior.

LINDA consists of an annual cross-sectional sample of around 300,000 individuals, or approximately 3% of the entire Swedish population, and an annual immigration sample of around 200,000 individuals, or approximately 20% of all immigrants in Sweden. The data contain detailed and highly accurate information on financial and demographic characteristics of each sampled household as well as characteristics of their place of residence for the period from 1999 to 2007. This dataset is key to observing refugee financial behavior over

the medium and longer runs.

The STATIV database contains the entire Swedish population and combines a large number of different variables from different registers in Sweden. We use the information from STATIV as a supplementary database to LINDA, as STATIV provides very detailed and rich information about immigrants. These include special coding for reasons for residence (e.g., refugee immigrant or labor immigrant) and the type of refugee immigrant.

When constructing the working sample, we adopt a conservative strategy in order to minimize potential misclassification or measurement errors. We restrict our attention to immigrants who entered Sweden between 1987 and 1991.[16] Unlike some previous work, we are able to identify refugees among immigrants with great accuracy through use of the STATIV data and include in the sample only those immigrants who were registered as refugees. We exclude from the sample those refugees who have been recorded as coming to Sweden for work reasons, family ties and other extensions, studies, other reasons, as well as refugees who are flagged as having enough living supplies. In other words, we only consider those refugees who are indicated as being in need of protection, or having been admitted for humanitarian reasons, i.e., those who find themselves in a particularly weak situation and present no doubt that they had to comply with the location instructions given by the immigration authorities.

We take further precautions in minimizing the probability of misclassifications. Specifically, to exclude family reunification cases from the analysis, we drop refugees who at the time of their first appearance in the LINDA dataset belong to a household with an adult (i.e., 18+) already residing in Sweden or holding a Swedish citizenship. Finally, we only keep those refugee immigrants who were first sampled in LINDA in the year of immigration or in the following year.

Out of this conservatively constructed sample, we drop households with missing information on the initial place of residence or the current place of residence (where by "current"

[16] See also Edin et al. (2003).

is meant the 1999-2007 period) of the refugee, or the year of immigration, or the country of refugee origin. As we need to match refugees to their environment, we also exclude observations if there is missing information on the share of neighbors who have particular educational qualifications (described below) or who save for retirement.

Despite this conservative approach, we end up with 4,061 refugee immigrants in the final sample in any given year. Descriptive statistics for the pooled sample of 36,513 observations are presented in Table I. The breakdown of refugees by country of origin and by year of immigration is shown in Table O.A.1. Slightly more than a quarter of the refugees came from Iran, 13.22 percent from Chile, while Iraq and Lebanon have about 9 and 8 percent, respectively. As shown in Panel B, more than half the refugees in the sample entered Sweden in 1988 or 1989, while the rest entered in 1987 or 1990, with only a few entering in 1991.

4.2 Proxying for financial literacy externalities

Our basic premise, following Pool et al. (2015), is that individuals have the greatest scope for interaction with people in their immediate environment, here being proxied by their electoral district. The idea is that refugees have a high probability of random encounters with geographically close neighbors, some of which can lead to non-random encounters where relevant financial content is exchanged.[17] The potential of refugees for improving their own financial literacy through such interaction is assumed to be an increasing non-linear function of the share of financially literate people living in their electoral district.

In order to ensure a high potential for random encounters, we consider the share of financially literate neighbors in a small neighborhood around the apartment where the refugee household was placed. In modern societies, it is possible for well-connected people to be interacting mainly with peers living at some distance rather than with their immediate neighbors. However, for unconnected refugees newly allocated to a particular area and

[17]This parallels the distance-based approach of Pool et al. (2015) in analysis of mutual fund managers.

apartment, such as those we consider, the immediate neighbors are the most likely contact points. We choose the electoral district as the relevant notion of a neigbhorhood. As described in section 3, the electoral district is a typically small area around the apartment of each refugee household.

There is no single way to define financial literacy, and different researchers and organizations have done so in different ways.[18] The most widely adopted definition of financial literacy focuses on knowledge of basic financial concepts and familiarity with the economic environment. We take advantage of detailed available information on the level of educational attainment and content of education for people living in each electoral district to consider two alternative measures of financial literacy among initial neighbors. Our benchmark measure refers to the share of neighbors in the electoral district of initial allocation who have business/economics education and have attended (but not necessarily completed) college. To make sure that theoretical knowledge is combined with knowledge of Swedish institutions, we exclude from the set of relevant neighbors in the base runs migrants who have less than 20 years in Sweden. Validity of this proxy for financial literacy is reinforced by the observation that stock ownership rates among such neighbors are typically of the order of 85 percent.

Our second measure of financial literacy is the share of electoral district neighbors who have a quantitative educational background, regardless of whether they have been trained in economics and finance, and who have attended college. A quantitative background typically facilitates the processing of information relevant for financial behavior and could also yield useful externalities. In our robustness analysis below, we are also able to check whether effects come from the neigbhors having a particular level of education or they are related to content.

[18] For an overview, see Lusardi (2008) and Lusardi and Mitchell (2007).

5 Externalities from Financially Literate Neighbors

We begin our analysis by focusing on causal effects of exposure to neighbors with at least some college education and a business or economics background. We estimate the effect of the share observed in the initial electoral district of exogenous placement, controlling for refugee characteristics, some of which might have influenced that placement, economic characteristics of the electoral district, time-invariant relevant factors in the greater area of the parish, macroeconomic and other year-specific factors in the year of arrival and in that of observation, as well as for considerations that might be specific to refugees from the particular country of origin. We consider behavior over different runs: the full sample, the medium run (1999-2003), and the longer run (2004-2007). Tables in the main text are indicated by roman numerals, and those in the online appendix by the prefix OA.

5.1 Presence and time dimension of effects

Table III presents the estimation results for the full set of years during which financial behavior is observed, 1999-2007. We present coefficient estimates of a linear probability model in columns (i) and (ii), and average marginal effects from a probit model in columns (iii) and (iv), both using specification (1). We see that when the period is taken as a whole, the share of neighbors who had attended college and had economics or business education in the initial electoral district of placement has a statistically significant positive effect both on the tendency to save for retirement in private accounts and on the tendency to hold stocks. This positive effect is present, controlling for household and initial electoral district characteristics, as well as for country of origin, year of immigration, year of observation, and initial parish fixed effects, as described in section 3.

We find somewhat larger estimated effects and greater statistical significance for the probability of holding stocks than for the probability of saving for retirement. Expressing results in terms of a one-standard-deviation increase in the share of initial neighbors with

business or economics education and some college attendance, the resulting increase in the probability of participation in private retirement accounts is 1.34 percentage points, while that for stocks is 2.65 percentage points. This is consistent with the idea that stock investment is more involved, because of its informational intensity and its riskiness, compared to saving for retirement. In such a case, respondents are more likely to benefit from knowledge transfers to them from the environment.

Table IV distinguishes between effects of financial literacy externalities in the initial neighborhood over the medium run (1999-2003) and over the longer run (2004-2007), using a linear probability model.[19] Separating the two "runs" allows not only the effect of financial literacy externalities but also the relationship of participation probability to household characteristics and other factors to differ across the two periods of observation of financial behavior.

When considering only the medium run from the initial placement, we find a positive and statistically significant coefficient estimate for participation both in private retirement accounts and in stocks. For financial behavior over the longer run, we find an effect of the share of financially literate neighbors only on stockholding, and that effect is larger in estimated size and more strongly statistically significant than the medium-run effect. The effects are also economically significant. A one-standard-deviation increase in the share of initial neighbors with business or economics education and some college attendance raises the probabilities of medium-run participation in private retirement accounts and in stocks by 1.47 and 2.03 percentage points, respectively. Over the longer run, probability of participation in stocks increases by 3.43 percentage points.

The sign and statistical significance of other controls in our estimation model is largely consistent with what has been found in household finance regressions for these variables to date. It is noteworthy that educational attainment of the household head continues to

[19]Table O.A.10 presents average marginal effects for the medium, the longer run, and the full period of observation of financial behavior using probit estimation. We see that these estimates of average marginal effects exhibit the same signs and pattern of statistical significance as the corresponding estimates from the linear probability model, confirming robustness to the estimation method used.

be statistically significant and to correlate with investment in stocks and saving through private retirement accounts even when the role of a financially literate neighborhood is acknowledged. On the other hand, the role of having a household head that works in the financial sector is not precisely estimated, probably given the small number of such occurrences in the data. Having a larger number of children is negatively associated with saving for retirement through private retirement accounts but is insignificant for stockholding in most cases.[20]

We will further examine possible changes in relevance of initial exposure to financial literacy externalities between the medium and the longer run below. The difference we found between effects on medium- and on longer-run behavior, however, is consistent with financial literacy externalities being more relevant for the riskier and more informationally intensive asset but also with a longer "gestation period" during which information is absorbed and the idea of stockholding gradually gains salience.

5.2 Robustness to alternative interpretations

In this section, we discuss possible sources of a statistically significant coefficient on the share on financially literate neighbors alternative to financial literacy externalities and how we tackle them. Key for our interpretation of the estimates as reflecting financial literacy externalities is that informational exchanges were part of social interactions with immediate neighbors rather than of labor market activities or of access to financial industry branches and facilities located in the neigbhorhood.

It is important in this context to guard against the possible presence of “correlated effects”. This is a case in which the environment in the initial area of refugee placement influences positively both the quality of (electoral-district) neighbors and refugee financial behavior, without a direct link between the two. In our context, the share of financially

[20]The presence of children might also encourage social interactions and could in principle strengthen the effect of any given share of financially literate neighbors. Nevertheless, in unreported regressions, we have not found statistically significant interaction effects.

literate neighbors might then simply reflect the availability of financial institutions (e.g., banks or insurance companies), advisors or brokers, that in turn contribute to the financial decisions of refugee households as well as causing a higher share of financially literate neighbors to be present in the relevant area. These supply-side factors would influence both the number of financially literate households in the neighborhood and the financial behavior of refugees without a direct causal relationship between the two.

We address this possibility of correlated effects in a number of ways. First, we recognize that the overall presence of financial and related institutions in the greater region to which refugees were initially allocated could be relevant for their financial choices later on. To control for any such regional influences, we include parish fixed effects in regression (1), capturing conditions in the smallest administrative unit in Sweden.

Second, in order to control for a financial or labor market environment in the immediate neighborhood (electoral district) favorable to stockholding or private retirement accounts over and above what holds for the region as a whole, we also introduce explicit indicators of financial development and well-being in the initial electoral district at the time of refugee placement. Specifically, we control for median household income, median taxable wealth, and median household debt-to-income ratio in the electoral district, as well as introducing fixed effects for the largest industry in the electoral district.

We find that these electoral-district controls, some of which are statistically significant, do not weaken at all our (unreported) estimates of the effects of the share of financially literate neighbors in a specification omitting those factors. Moreover, we note that the estimated coefficients on the electoral-district controls are either insignificant or negative instead of positive (see Table III, for example). This is the opposite of what one would expect if electoral district conditions were in fact responsible for better financial behavior in initial neighborhoods with a larger share of financially literate households.

A further potential consideration is that the share of financially literate neigbhors is highly correlated with employment conditions in the electoral district and it is these labor

market conditions in the immediate neighborhood that are critical for employment outcomes and ultimately the financial behavior of refugees. We should first recall that we control for labor income, labor market status, and occupation of refugees in our benchmark model. Further, available independent evidence on the distance between workers' places of residence and of work in Sweden challenges this argument. A survey carried out by the Swedish Transport Analysis Agency (TRAFA) shows that 75.7 percent of survey respondents either declare working in a different municipality than where they live or respond that they work in the same municipality but more than 5 km away from their place of residence. Even in less densely populated areas than the big cities, this distance should be sufficient to place their place of work outside their electoral district.[21]

Another possible concern is that the effect of financially literate neighbors on subsequent financial behavior of refugees does not run through social interactions but through the influence of such neighbors on career prospects of the refugees, possibly through close friendships or intermarriage. The idea here is that close interactions with a financially alert neighbor open doors for your future professional placement.

In addition to controlling for labor income, labor market status, and occupation of refugees in our benchmark model, we run regressions of labor market outcomes of refugees in 1999-2007 (more than ten years after entry) on the share of financially literate neighbors in the initial electoral district, controlling for other relevant features of the refugee households. Table V reports estimated effects of the initial share of financially literate neighbors (proxied by business or economics education) on three labor-market outcomes in the period 1999-2007; and on location of the refugees by the year 1999. We consider whether the respondent ends up working in the financial sector, the level of earnings attained,[22] and whether the

[21]Out of 22,088 respondents, 9,818 declare that they work in another municipality. Hence, we know that they do not work in the electoral district where they live (electoral districts are parts of parishes which are part of municipalities). Further, we know that 12,270 respondents are working and living in the same municipality. If we assume that the maximum distance within an electoral district is 5 km, we know that 6,910 of them work outside their electoral district. Hence, we get 19,180 respondents who can be considered as working outside their electoral districts. See also the discussion of electoral districts in section 3.

[22]We report results using the broad income definition that includes labor income, income from entrepreneurship, and employment related transfers (see also Edin et al. (2003) and Åslund et al. (2011)) in-

respondent is unemployed.

We find no evidence of an effect of the initial share of financially literate neighbors on the level of earnings and on the tendency to be unemployed, either in the medium or in the longer run. The corresponding Table O.A.12 shows that these findings are also robust to using a broader notion of financial literacy, namely quantitative education and college attendance of neighbors (more on this below). We only find an effect on the probability that the refugee ends up working in the financial sector over the longer run, but such statistical significance is not present for the financial literacy definition based on quantitative education.

We also consider the possibility that financially literate neighbors in the initial location influence subsequent financial behavior mainly by affecting the probability that the refugee eventually moves to another location. As mentioned above, the average time spent by a refugee in the initial parish is 5.4 years, while the number rises to 8.7 years for staying in the same municipality. We find that the share of financially literate initial neighbors is not relevant for whether the refugee migrant will have remained in the same parish by year 1999.

All in all, our findings in this section do not support the idea that our estimated effects of the share of financially literate neighbors are in fact attributable to other features of the immediate neighborhood or broader region of original location, or to indirect effects of financially literate neighbors through employment prospects or future location of refugees. In what follows, we try to shed light on the channel through which the presence of immediate neighbors with financial literacy affects future financial behavior of immigrants allocated to their neighborhood. Such analysis is interesting in its own right but can also provide further support for considering the estimated effect as reflecting financial literacy externalities.

cluding only people with positive earnings, as is standard in the labor literature. These results are robust to using different earnings definitions, and defining the income at the household or individual level.

6 Exploring Channels of Influence

In this section, we probe into channels through which close initial neighbors influence the financial behavior of refugees over the medium and longer run. The first subsection studies particular characteristics of neighbors and then of refugee household heads. The second subsection studies the role of the likely intensity of social interactions.

6.1 Content of neighbor education

Important insights into the channel of transmission can be obtained by varying the qualifications of the initial neighbors considered relevant for influencing subsequent financial behavior of refugees. In our first such exercise, instead of considering neighbors with business or economics education as potential sources of externalities, we consider a broader set that includes all neighbors with quantitative education and at least some at the college level. Results for the full observation sample, the medium run, and the longer run are reported in Table VI.

When considering this broader group of neighbors with ability to process quantitative information but not necessarily with specialized knowledge of economics or business, we find smaller corresponding estimated effects of financial literacy externalities, regardless of whether we focus on the medium or the longer run. We confirm the significance pattern of effects on private retirement saving over the medium run and on stockholding over the medium and the longer run. The smaller effects with the broader and less demanding notion of financial literacy among neighbors suggest that the process through which financial literacy externalities operate is one in which content and knowledge matter.

The view that business or economics knowledge content matters is strengthened by a further exercise. Since both sets of significant results refer to neighbors who share at least some college attendance as their educational attainment, we ask whether educational attainment per se, rather than content, is responsible for the results. Specifically, we rerun

the estimation using the share of neighbors who have at least some college attendance but who do not have as their major business or economics. We do not find a statistically significant effect of this share of generally literate neighbors on refugee financial behavior over any run. The finding is particularly strong, given that this subset of neighbors with at least some college education includes also neighbors with quantitative education (other than in business or economics). This result reinforces the view that content, rather than the level of education per se, matters for the observed effects on financial behavior.

6.2 Education and gender of refugee household head

A third argument for the importance of content, but also one that is interesting in its own right, is the role of educational attainment of the refugee household head for an operative channel of effects on financial behavior. We now split the sample into refugee households with heads that had high school or college education and those that did not. We then consider the role of the gender of the household head in a similar way.[23]

Before we compare estimates based on sample splits, we want to confirm that they are not an artefact of the more educated or the males sorting into neighborhoods with either high or low financial literacy. One might suspect that the nature of the allocation process performed by immigration officials, focused as it was on education, language, and family size, might result in differences across subsamples in exposure to externalities, and these might confound findings on the operativeness of different channels.

We have already presented evidence in Table II that a number of initial refugee characteristics are not correlated with our measures of financial literacy in the electoral district. Focusing on the criteria for the sample splits in this section, Table O.A.2 verifies that allocation of refugees by immigration officials did not result in different exposures of the two subsamples to financially literate neighbors, regardless of the financial literacy measure used. The two subsamples exhibit comparable average exposure to financially literate neighbors,

[23]Obviously, by splitting the sample and carrying out separate estimations, we also allow the relationship of other factors to the probability of participation to differ across subsamples.

as well as comparable variation in this exposure.

Table VII shows that the effect of financial literacy externalities is present only for the more educated subsample, namely those households whose heads have high school education or more, but not for those with less than high school education. This is true regardless of whether we examine medium- or longer-run effects, and it extends to both assets: the more educated benefit from financial literate neighbors across the board, while we find no statistically significant effect for refugees with less than high-school education. Taken together with our previous results on the content of education among the neighbors, our findings suggest that the operativeness of the channel of influence depends crucially on what the initial neighbors know about business and economics and on the ability of refugee household heads to process information, as reflected in their educational attainment.

Are financial literacy externalities important in influencing participation behavior of both female- and male-headed households? The question does not have an a priori obvious answer in light of existing research. On the one hand, existing literature on financial literacy draws attention to more limited financial literacy among women and lower self-confidence in dealing with finances. These are evidenced by lower scores of women in financial literacy tests, and by greater tendency to opt for the "Don't Know" answer and to give a wrong answer if they attempt the question (Lusardi and Mitchell, 2014; Bucher-Koenen et al., 2016). On the other hand, there is considerable evidence that males are more likely to exhibit overconfidence, with stock trading as a prominent example (Barber and Odean, 2001). In principle, overconfidence and more limited willingness of males to consult with others may limit the scope for financial literacy externalities to affect financial behavior of males, so that such externalities can contribute to mitigating the gender gap found in the literature so far. However, this is not what we find.

When we split the refugee sample by gender (Table VIII), we find that financial literacy externalities are operative for households headed by males but fail to have statistically significant effects on participation of females. Female-headed households who find themselves

in a neighborhood with a larger share of economics or business-educated neighbors are not systematically influenced by them in their saving for retirement or stock market participation decisions, either over the medium- or over the longer run. Male-headed households, on the other hand, are more likely to participate in stocks over the medium run when they have been exposed to a greater share of financially literate neighbors, and the effect persists and becomes larger over the longer run. Thus, instead of financial literacy externalities providing a mitigating mechanism, they can actually contribute to the gender disparity in asset market participation.

In addition to verifying that there was no sorting by immigration officials (see Table O.A.2), we have explored the possibility that the result is due to a tendency of women to talk more to women, especially given the cultural composition of refugees. In unreported regressions, we find no effect when we restrict attention to the share of financially educated women in the initial parish.

When we apply the sample split to the estimations involving the share of neighbors with quantitative education and at least some at the college level, the pattern of results (Table O.A.3) is weaker, consistent with the importance of content.[24].

Our findings on the education and gender sample splits are consistent with the view that financial literacy of neighbors influences household choices through transfer of knowledge and information that needs to be received, processed, understood, and acted upon by the household in question. In view of existing literature on participation, our findings point to the conclusion that ability to process new financial information from neighbors, found to be larger for more educated and for male-headed households, tends to overcome relative unwillingness to consult others because of overconfidence, thus making financial literacy

[24]Specifically, educated refugee household heads benefit from financially literate neighbors only in their retirement account participation in the medium run and only in their stockholding over the longer run, unlike the broader set of effects in the benchmark definition of financially literate neighbors. Splitting by gender, we find that male-headed households who came in contact with a larger share of neighbors with quantitative education in the initial location are more likely to be holding stocks in the longer run, but they are not influenced in the short run or in retirement saving. Exceptionally, and unlike neighbors with business or economics education, quantitatively educated neighbors influence saving for retirement decisions of females rather than of males in the medium run (see Table O.A.4)

externalities operative.

6.3 Intensity and nature of interactions

A further angle from which to explore the validity of the claim that the estimated effects reflect social interactions is to examine the role of the likely intensity, as well as nature, of interactions. In particular, we examine whether factors likely to limit or enhance the likelihood and informativeness of interactions between refugees and their neighbors are reflected in smaller and larger estimates, respectively, of the effect of financial literacy externalities. The first factor is attitudes that initial Swedish neighbors with some college education had towards immigrants. The second is the composition of the initial neighbor pool with respect to immigrant status and recency of immigration and its implications for salience of different assets. The third factor is whether the potential for pure imitation of financial behavior of neighbors was present and decisive for the effect.

6.3.1 Attitudes towards immigrants

We posit that social interactions between refugees and their neighbbors are more likely to take place in neighborhoods where locals are more positively predisposed towards refugees. Thus, if the channel of influence does involve mainly social interactions (as we argue), then the influence of financial literacy externalities should be stronger in those regions.

In order to assess the attitudes of Swedes towards immigrants in the initial neighborhood, we make use of unique survey data from the SOM survey,[25] which include responses to an important question regarding attitudes, asked only to individuals with university or college studies, as well as recording their county of residence for the years 1988 and 1991. The question of interest is: "Should we accept more refugees in Sweden?". Respondents can choose between five ways to characterize this suggestion, ranging from "very good" to "very

[25]Weibull, Lennart, SÃ¶ren Holmberg, Henrik Ekengren Oscarsson, Johan Martinsson, Elias Markstedt and Frida Vernersdotter 2016. Super-Riks-SOM 1986-2014, v2016.1 Gothenburg University: SOM-Institute, www.som.gu.se. We are grateful to the owners of the dataset for sharing their data with us.

bad".[26] If someone picks the first or second characterization, we classify the respondent as having a positive attitude towards immigrants and code the response as 1. Otherwise the respondent is classified as not having a positive attitude, with a code of 0. The share of positive respondents per county is then calculated.[27] The mean county value is 33.9 percent and the median is 35.2 percent.

If the share of positive responses in the county is above this median share of 35.2 percent, that county is classified as positive towards immigrants and is differentiated from those with a below-median share. When implementing this split, we find that significant estimates of the effect of financial literacy externalities in regression (1) always refer to the subsample of (electoral districts in) counties with positive attitudes towards immigrants.[28]

Our finding that the share of financially literate neighbors in the initial electoral district has a statistically significant effect on subsequent financial behavior only if there is an above-average share of neighbors positively predisposed to immigrants further reinforces the view that our estimates reflect social interactions between refugees and their initial neigbhors.

6.3.2 Neighboring recent immigrants

We now turn to a second exercise aimed at examining the relevance of the likelihood of interaction. This time, the exercise focuses on neighbor immigrant status and recency of entry as influencing the probability of interaction with the refugees. Benchmark estimation has proceeded so far by including in the set of relevant neighbors those who are Swedes or immigrants with more than 20 years of stay in Sweden. We now broaden the relevant neighbor circle to include also more recent migrants that have stayed in Sweden between 10

[26] Specifically, for question fc900a, edu3 == 3, the options are: 1. Very good suggestion; 2. Pretty good suggestion; 3. Neither good nor bad; 4. Pretty bad suggestion; 5. Very bad suggestion.

[27] In total, we have 530 individual respondents for the 24 counties.

[28] Coefficient estimates on the share of financially literate neighbors are always statistically significant for short-run and for medium-run stockholding behavior of refugees initially placed in counties positively predisposed to immigrants, regardless of whether we focus on neighbors with business/economics or quantitative education. Estimates are smaller for quantitative education than for business or economics education, as in the rest of the paper. Estimates are available on request.

and 20 years, and we examine the estimated size and significance of the coefficient on the proxy for financial literacy externalities.

By expanding the relevant social circle to include more recent immigrants, we increase the probability that the refugee respondent interacts with the neighbors, in view of the likely greater tendency of recent migrants to associate with refugees sharing similar levels of assimilation to the local culture.[29]

Table O.A.5 presents estimation results for all samples. We find that an increase in the share of neighbors with economics or business education and some college attendance among this expanded circle of neighbors results in the same pattern of statistical significance of the effects as for the original circle of neighbors, but in somewhat greater estimated increases in the probability of participation in retirement saving, and in smaller estimated increases in stock market participation. This combination of greater intensity of the effect on retirement saving and reduced intensity of the effect on stockholding under the expanded circle of neighbors is remarkably robust across all runs (full sample, medium, longer run), as well as to undertaking (in unreported regressions) sample splits by education and gender and to considering quantitative education as the basis for defining the share of financially literate neighbors.

This mixed result is intriguing. If greater intensity of interaction with the relevant circle of neighbors were the full explanation, we would expect to observe higher estimated effects on participation in both financial instruments (private retirement accounts and stocks). However, the estimated effect of financial literacy externalities on stocks is now smaller. This suggests the presence of a second factor, that mitigates the heightened probability of interaction imparted by recent immigrant status. Earlier work on the Swedish native and migrant population finds that the period between ten and twenty years of stay in Sweden is

[29]As we condition on the immigrants having spent at least 10 years in Sweden, we are still unlikely to include immigrants lacking knowledge of the Swedish institutional setup in the financial sector. An alternative way to manipulate probability of interaction might be to consider neighbors from the same country. This is not feasible, however, due to the small numbers involved. We have also experimented with including only refugees and neighbors having children at school, as this tends to facilitate contact among adults. We did not find that this was a decisive factor for the initial interaction between refugees and immediate neighbors.

quite important for assimilation of migrant stockholding behavior to that of Swedes (Haliassos et al., 2016). This suggests that a plausible mitigating factor is the changing salience of stocks as a function of the time the migrant neighbor has spent in Sweden. Although recency of entry makes the additional neighbors now included in the relevant circle more likely to interact with the refugees, their lower stock market participation makes them less likely to find stocks salient and worthy of discussion than the benchmark circle of neighbors did. Salience considerations can thus be consistent with social interactions being the key channel for transmission of financial literacy externalities, and with content being important.

6.3.3 Financial choices of neighbors

We have presented evidence above suggesting that content of knowledge of neighbors and salience of financial products for them are relevant for whether financial literacy externalities are operative. In this last section, we examine whether the potential for imitation of asset holding by the neighbors is a prerequisite for transmission of financial literacy externalities to the refugees. In other words, are financial literacy externalities exhausted in imitation or do they also encompass transfer of knowledge regardless of neighbor participation in an instrument?

Table IX presents estimates of the influence of the share of neigbhors who were saving for retirement through private accounts in the initial electoral district on the medium- and longer-run tendency of refugees to participate in such retirement saving or in stockholding. We see that greater presence of retirement savers in private accounts in the district of initial placement has statistically significant positive effects on refugee participation in both instruments over both runs. However, the estimated size of these effects is generally smaller than the size we estimated for the share of neighbors with economics or business background (Table III) and for the share with quantitative background (Table VI), despite the fact that the share of neighbors in the initial electoral district who were participating

in private retirement accounts is larger than either of the two shares based on neighbor education.[30]

These findings suggest that, while teaching by example does influence financial behavior, it produces smaller effects than social interactions with knowledgeable people regardless of the assets they hold. Moreover, teaching by example influences not only participation in the item neighbors hold (private retirement accounts) but also in the other financial instrument under consideration (stocks), even to a larger extent than in the asset neighbors hold. Both findings are consistent with informative discussion rather than mindless copying of behavior being at the heart of the financial literacy transmission process.

7 Conclusions and Policy Implications

This paper uses unique administrative data on refugees to Sweden and a quasi-field experiment of exogenous allocation of refugees in order to estimate the effect of access to financially literate neighbors on two important aspects of household financial behavior: saving for retirement through private accounts, and participation in stockholding. As we can track refugee households over twenty years, we are able to estimate the effects of the exogenous component of exposure to financial literacy externalities (over an average length of stay of 5.4 years) as it influences financial behavior in the medium and longer runs (ten to twenty years). The nature of the experiment and of the data allow us to address thorny causality issues related to "correlated effects" arising from endogenous choice of neighborhood.

We find evidence of statistically and economically significant effects of the share of initial neighbors who had business or economics education at college level on the financial behavior of refugees in the medium and in the longer run. Estimation controls for a range of

[30] In unreported regressions, we have also included the share of retirement savers together with either the share of neighbors with business or economics education, or the share of those with quantitative education. We found no case (whether for the full sample, the medium run, or longer run behavior) in which the share of retirement savers in the electoral district was significant but the corresponding education share was rendered insignificant. Sample splits by education or by gender confirm the same channels as we found for the financial literacy proxies based on educational background: the effect is operative only for refugee household heads with at least high school education, and for males (see Tables O.A.7 and O.A.8).

observed refugee characteristics, unobserved features of their broader location (parish), economic conditions in their immediate neighborhood (electoral district), macroeconomic and institutional factors, as well as unobserved cultural and other factors related to the country of origin.

We consider a number of alternative explanations for our results in the process of validating their interpretation as financial literacy externalities. We address the issue of correlated effects having to do with environmental factors in the electoral district or the broader parish driving the results in a number of ways. We are able to control for relevant economic conditions in the electoral district at the time of initial allocation, as well as for unobserved factors in the broader parish, which we show to be more relevant for the labor market experience of refugees. We also show that the share of financially literate neighbors does not influence the future labor market outcomes of refugees or their tendency to move away from the initial neighborhood. We find no evidence of operative labor market or other effects, as opposed to financial literacy externalities, in producing the observed estimates.

We next explore the nature of financial literacy externalities and channels through which they operate. First, we vary the definition of financially literate neighbors to include all those with quantitative education, regardless of business or economics content. We find the same pattern but a smaller estimated size of effects across all runs and sample splits. We also show that it is content rather than level of education that is relevant for the effects. Second, we find evidence that financial literacy externalities are operative for the subsample of refugee household heads with at least a high school degree, but not for those with less than high school education. We also find that the effect tends to operate through male- rather than female-headed households. We confirm that these results are not plausibly due to sorting of more educated and male refugees to areas with greater financial literacy nor to the choice of financial literacy concept.

Then, we vary the probability and nature of social interaction between refugees and their initial neighbors in three ways: by considering attitudes towards immigrants in the county

of location, by expanding the relevant circle of neighbors to include more recent migrants than in the base model, and by exploring the importance of imitation of financial behavior.

We find no effects of the share of financially literate neighbors in counties with below-median share of Swedes positively disposed towards immigrants. We also find that inclusion of more recent immigrants in the relevant circle of neighbors results in bigger effects on participation in private retirement accounts and smaller effects on stockholding, an asset less salient for the recent immigrants. Conflict between more likely interaction - which should strengthen effects on both assets - and smaller salience of stocks is consistent with the opposite effects we observe on the two assets.

On imitation, we do find that the initial share of neighbors participating in private retirement accounts has an effect on asset participation of refugees. Contrary to what one expects in case of pure imitation, the effect of observing neighbors with retirement saving extends to refugee participation in both assets, and it is actually more pronounced for the other asset (stocks).

All in all, our findings suggest the presence of significant financial literacy externalities that extend beyond imitation to the transfer of knowledge relevant for financial behavior, and that are increasing in likelihood and relevance of interactions.

These findings contribute to the long-time search for evidence of a causal impact of financial literacy on economic behavior and outcomes, and to policy discussions on the reach of effective financial education programs. Taken together, our results in this study have nuanced implications for these debates. While financial literacy externalities are likely to lower the cost of effective financial education initiatives for given effects on financial literacy, their uneven impact is likely to widen disparities in financial literacy that should not be ignored in policy design. To the extent that externalities are operative only for people who have the educational background and confidence to receive and process relevant financial knowledge, they can widen the observed gap in financial behavior and outcomes if the asymmetry is not taken into account.

Finally, the focus on refugees for econometric reasons generates some implications for the ongoing refugee crisis. Our results highlight the importance for medium and longer-term refugee behavior of being placed where they can benefit from the knowledge and (financial) literacy of others. The finding that it is the more educated and financially confident refugees that are likely to benefit from financial literacy externalities seems promising, as such refugees are typically more welcome to more educated communities.

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Table I: Summary Statistics

	Full Sample			*Medium-Term*			*Longer-Term*		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel A: Dependent Variables									
Saving for Retirement	36,513	0.26	0.44	20,303	0.23	0.42	16,210	0.30	0.46
Stockholding	36,513	0.37	0.48	20,303	0.37	0.48	16,210	0.36	0.48
Panel B: Financial Literacy Externalities (at initial Placement)									
Share of neighbors with economics/business education	36,513	0.02	0.03	20,303	0.02	0.03	16,210	0.02	0.03
Share of neighbors with quantitative education	36,513	0.05	0.05	20,303	0.05	0.05	16,210	0.05	0.05
Share of neighbors who save for retirement	36,513	0.19	0.10	20,303	0.19	0.10	16,210	0.19	0.10
Panel C: Household Controls									
Disposable Income (IHS)	36,513	12.99	0.57	20,303	12.89	0.56	16,210	13.11	0.57
Age 30-45	36,513	0.51	0.50	20,303	0.58	0.49	16,210	0.41	0.49
Age 45-60	36,513	0.39	0.49	20,303	0.32	0.47	16,210	0.49	0.50
Age 60-75	36,513	0.07	0.26	20,303	0.05	0.22	16,210	0.09	0.29
Male	36,513	0.67	0.47	20,303	0.67	0.47	16,210	0.67	0.47
Unemployed/Uncategorized	36,513	0.32	0.47	20,303	0.35	0.48	16,210	0.29	0.45
Retired	36,513	0.09	0.29	20,303	0.09	0.28	16,210	0.10	0.30
Employee	36,513	0.56	0.50	20,303	0.52	0.50	16,210	0.60	0.49
Married	36,513	0.60	0.49	20,303	0.59	0.49	16,210	0.60	0.49
Number of Adults	36,513	1.96	0.95	20,303	1.89	0.91	16,210	2.05	1.00
Number of Children	36,513	1.01	1.27	20,303	1.10	1.31	16,210	0.91	1.22
High School Graduate	36,513	0.41	0.49	20,303	0.41	0.49	16,210	0.42	0.49
College Graduate	36,513	0.31	0.46	20,303	0.30	0.46	16,210	0.32	0.47
Working in the Financial Sector	36,513	0.003	0.05	20,303	0.003	0.05	16,210	0.003	0.05
Working for the Government	36,513	0.20	0.40	20,303	0.18	0.38	16,210	0.22	0.42

Note: This table presents descriptive statistics for the variables employed in the empirical analysis. The sample is a balanced sample of 4,061 refugee immigrants. The medium-term refers to the time period from 1999 to 2003, and the longer-term refers to the period from 2004 to 2007, respectively. The mean and standard deviation are calculated on the full pooled sample. The monetary variables are defined in SEK. For variable definitions, see Online Appendix A. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table II: The Relationship between Shares of Financially Literate Households in Initial Location and Initial Refugee Household Characteristics

	Share of neighbors with …	
	business/econ education	quantitative education
	(i)	(ii)
Male	0.00012	0.00052
	(0.0009)	(0.0017)
Married	-0.00060	0.00016
	(0.0010)	(0.0015)
Household size	0.00052	0.00004
	(0.0008)	(0.0013)
Number of children	-0.00048	-0.00016
	(0.0009)	(0.0015)
College degree and more	0.00009	-0.00041
	(0.0011)	(0.0017)
High school degree and more	-0.00011	-0.00169
	(0.0009)	(0.0015)
Age 30-45	0.00016	-0.00035
	(0.0009)	(0.0014)
Age 45-60	0.00177	0.00073
	(0.0017)	(0.0029)
Age 60-	-0.00714	-0.00439
	(0.0054)	(0.0098)
Observations	4061	4061
Neighborhood FEs	Parish	Parish
Country-of-origin FEs	Yes	Yes
Arrival Year FEs	Yes	Yes

Note: This table presents the results of the OLS regressions where the share of financially literate neighbors in the initial electoral district (proxied by either business/economics or quantitative education and some college attendance) is regressed on the household characteristics at the initial time of allocation. In both specifications, we account for country-of-origin and arrival year fixed effects, as well as for time-invariant neighborhood characteristics through parish fixed effects. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table III: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Full Observation Period (1999-2007)

	LPM Estimates		Probit Estimates	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.47043*	0.93210***	0.41074*	0.93425***
	(0.2713)	(0.2869)	(0.2501)	(0.2904)
Local Financial Development in the Elec. Dist.	0.03963	-0.05371*	0.03929	-0.05902**
	(0.0288)	(0.0287)	(0.0279)	(0.0286)
Median Taxable Wealth in the Elec. Dist.	-0.00254*	-0.00317**	-0.00205	-0.00279*
	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Median Income in the Elec. Dist.	-0.05026	0.03268	-0.04233	0.03613
	(0.0328)	(0.0331)	(0.0310)	(0.0329)
Income	0.18163***	0.20299***	0.18767***	0.21638***
	(0.0127)	(0.0122)	(0.0141)	(0.0136)
Age 30-45	0.03230*	-0.02532	0.06914**	-0.02783
	(0.0181)	(0.0241)	(0.0272)	(0.0247)
Age 45-60	0.05978***	-0.05897**	0.09183***	-0.06022**
	(0.0214)	(0.0267)	(0.0294)	(0.0273)
Age 60-75	-0.04554*	-0.10665***	-0.03145	-0.12213***
	(0.0258)	(0.0325)	(0.0352)	(0.0345)
Male	-0.04113***	-0.05420***	-0.04689***	-0.05918***
	(0.0128)	(0.0133)	(0.0123)	(0.0131)
Unemployed	-0.01394	-0.01608	-0.01317	-0.02200
	(0.0173)	(0.0197)	(0.0211)	(0.0200)
Retired	-0.03666	-0.06638***	-0.04384	-0.07661***
	(0.0230)	(0.0248)	(0.0280)	(0.0266)
Employee	0.04776**	0.05562***	0.05264**	0.04611**
	(0.0189)	(0.0210)	(0.0221)	(0.0210)
Married	0.01313	0.02606*	0.00946	0.01851
	(0.0127)	(0.0133)	(0.0129)	(0.0131)
Nbr of adults	-0.02023***	0.00047	-0.02053***	-0.00171
	(0.0070)	(0.0069)	(0.0068)	(0.0067)
Nbr of children	-0.02179***	-0.00450	-0.01854***	-0.00100
	(0.0048)	(0.0050)	(0.0050)	(0.0050)
High school Dummy	0.04815***	0.06376***	0.05756***	0.07446***
	(0.0130)	(0.0139)	(0.0142)	(0.0143)
College and more Dummy	0.09725***	0.16857***	0.09347***	0.16314***
	(0.0154)	(0.0175)	(0.0159)	(0.0170)
Net wealth quartile II	-0.01180	-0.02083*	-0.01864*	-0.02270**
	(0.0104)	(0.0119)	(0.0105)	(0.0113)
Net wealth quartile III	-0.00242	-0.02214*	-0.01495	-0.02140*
	(0.0113)	(0.0131)	(0.0114)	(0.0127)
Net wealth quartile IV	0.10322***	0.13856***	0.08009***	0.11775***
	(0.0144)	(0.0139)	(0.0123)	(0.0123)
Financial sector Dummy	0.04519	-0.05843	0.01424	-0.05425
	(0.0888)	(0.0852)	(0.0713)	(0.0742)
Government sector Dummy	0.00519	-0.04306***	0.00029	-0.04313***
	(0.0136)	(0.0144)	(0.0119)	(0.0134)
Observations	*36513*	*36513*	34354	35185
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time FEs	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood FEs	*Parish*	*Parish*	*Parish*	*Parish*
Country-of-origin FEs	*Yes*	*Yes*	*Yes*	*Yes*
Arrival Year FEs	*Yes*	*Yes*	*Yes*	*Yes*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from a linear probability model and average marginal effects from pooled probit regressions of participation in saving for retirement through private accounts, and in stockholding (direct or indirect). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants and financial behavior in the period 1999-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table IV: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term

	Medium-Term		Longer-Term	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
	(iii)	(iv)	(v)	(vi)
Initial Fin Lit Ext	0.51858*	0.71541**	0.43506	1.20876***
	(0.2779)	(0.3045)	(0.3087)	(0.3091)
Local Financial Development in the Elec. Dist.	0.03770	-0.05710*	0.04075	-0.05052
	(0.0300)	(0.0298)	(0.0320)	(0.0317)
Median Taxable Wealth in the Elec. Dist.	-0.00245	-0.00351**	-0.00259	-0.00276
	(0.0015)	(0.0016)	(0.0018)	(0.0017)
Median Income in the Elec. Dist.	-0.06825**	0.03007	-0.03016	0.03759
	(0.0333)	(0.0340)	(0.0385)	(0.0369)
Income	0.15839***	0.21128***	0.20692***	0.19295***
	(0.0134)	(0.0147)	(0.0171)	(0.0146)
Age 30-45	0.04624***	-0.02050	-0.04092	-0.04318
	(0.0173)	(0.0247)	(0.0625)	(0.0644)
Age 45-60	0.08909***	-0.05638**	-0.02724	-0.07446
	(0.0212)	(0.0279)	(0.0635)	(0.0641)
Age 60-75	-0.00799	-0.11674***	-0.12752*	-0.12370*
	(0.0276)	(0.0362)	(0.0666)	(0.0679)
Male	-0.04184***	-0.06195***	-0.04156***	-0.04636***
	(0.0129)	(0.0138)	(0.0157)	(0.0152)
Unemployed	-0.00471	-0.00670	-0.01746	-0.03177
	(0.0188)	(0.0219)	(0.0332)	(0.0397)
Retired	-0.04821**	-0.05794**	-0.02136	-0.07544*
	(0.0245)	(0.0279)	(0.0406)	(0.0456)
Employee	0.03885*	0.07031***	0.06577*	0.03419
	(0.0201)	(0.0234)	(0.0346)	(0.0411)
Married	0.02360*	0.03085**	0.00453	0.01929
	(0.0134)	(0.0148)	(0.0165)	(0.0156)
Nbr of adults	-0.02726***	0.00093	-0.01598*	0.00139
	(0.0083)	(0.0086)	(0.0088)	(0.0085)
Nbr of children	-0.02181***	-0.01029*	-0.01957***	0.00081
	(0.0051)	(0.0057)	(0.0061)	(0.0061)
High school Dummy	0.04024***	0.06414***	0.05589***	0.06144***
	(0.0130)	(0.0148)	(0.0159)	(0.0158)
College and more Dummy	0.09584***	0.15759***	0.09791***	0.17699***
	(0.0157)	(0.0178)	(0.0186)	(0.0205)
Net wealth quartile II	-0.01034	-0.03614***	-0.00825	-0.00718
	(0.0124)	(0.0139)	(0.0142)	(0.0147)
Net wealth quartile III	-0.02323*	-0.05503***	0.01725	0.01949
	(0.0136)	(0.0154)	(0.0149)	(0.0164)
Net wealth quartile IV	0.10321***	0.11737***	0.10079***	0.16950***
	(0.0161)	(0.0158)	(0.0176)	(0.0178)
Financial sector Dummy	0.04855	0.00648	0.03227	-0.13975
	(0.0904)	(0.0919)	(0.1134)	(0.0933)
Government sector Dummy	0.00579	-0.04298***	0.00499	-0.03771**
	(0.0150)	(0.0160)	(0.0168)	(0.0172)
Observations	20303	20303	16210	16210
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time FEs	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood FEs	*Parish*	*Parish*	*Parish*	*Parish*
Country-of-origin FEs	*Yes*	*Yes*	*Yes*	*Yes*
Arrival Year FEs	*Yes*	*Yes*	*Yes*	*Yes*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for various sample periods: the medium term (1999-2003), and the longer term (2003-2007). The dependent variables, Saving for Retirement, is a binary variables that takes the value 1 if the household saves for retirement. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. The standard errors that are clustered at the electoral district level (1,428 cells) are reported in parentheses. When defining the financial literacy externalities, we consider the share of neighbors (both natives and immigrants who have been in Sweden for at least 20 years) who have both business/economics education and college attendance in the initial neighborhood. The sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table V: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance on Various Outcomes: Medium-Term and Longer-Term

	Working in the Financial Sector	Labor Income	Unemployed	Mover
Panel A: Medium-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.06139	0.05526	-0.18489	-0.35184
	(0.0390)	(0.4733)	(0.2803)	-0.35184
Observations	19342	19342	17671	4061
Panel B: Longer-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.09056**	-0.00826	0.29114	-
	(0.0432)	(0.4450)	(0.3436)	-
Observations	15697	15697	14377	-
Household Controls	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Country-of-Origin Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Arrival-year Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood Fixed Effects	*Parish*	*Parish*	*Parish*	*Parish*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*
Time-varying Initial Elec. Dist. Controls	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents estimates of the determinants of different labor market outcomes and residential location choice estimated using a linear probability model. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. Financial literacy externalities are defined in terms of the share of neighbors (natives and immigrants who have been in Sweden for at least 20 years) in the electoral district of initial placement who had a business/economics education and had attended college. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Earnings are defined as the sum of labor income, entrepreneurial income and taxable employment-related transfers. In specifications (i)-(iii), we condition on having positive earnings. The original sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for effects on outcomes over the medium-term (1999-2003), while Panel B reports results for the longer-term (2003-2007). Statistical significance at the 10, 5, and 1 percent levels are indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VI: Long Shadow Effects of Having Neighbors with Quantitative Education and College Attendance: Full Observation Period, Medium-Term, and Longer-Term

	Full Sample		Medium-Term		Longer-Term	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Initial Fin Lit Ext	0.32433*	0.48621***	0.41943**	0.34233*	0.21603	0.66555***
	(0.1758)	(0.1788)	(0.1874)	(0.1875)	(0.1936)	(0.1970)
Financial Development in the Elec. Dist.	0.03457	-0.05504*	0.02866	-0.05639*	0.04072	-0.05424*
	(0.0293)	(0.0294)	(0.0305)	(0.0307)	(0.0326)	(0.0326)
Median Taxable Wealth in the Elec. Dist.	-0.00254*	-0.00297*	-0.00255*	-0.00331**	-0.00248	-0.00255
	(0.0015)	(0.0015)	(0.0015)	(0.0016)	(0.0018)	(0.0017)
Median Income in the Elec. Dist.	-0.05127	0.03306	-0.07034**	0.03082	-0.02979	0.03764
	(0.0331)	(0.0338)	(0.0337)	(0.0345)	(0.0386)	(0.0378)
Income	0.18191***	0.20354***	0.15857***	0.21159***	0.20736***	0.19404***
	(0.0126)	(0.0123)	(0.0133)	(0.0147)	(0.0172)	(0.0147)
Age 30-45	0.03155*	-0.02659	0.04522***	-0.02148	-0.04126	-0.04403
	(0.0181)	(0.0239)	(0.0173)	(0.0246)	(0.0622)	(0.0631)
Age 45-60	0.05923***	-0.05987**	0.08839***	-0.05713**	-0.02738	-0.07474
	(0.0214)	(0.0266)	(0.0212)	(0.0278)	(0.0631)	(0.0629)
Age 60-75	-0.04609*	-0.10772***	-0.00881	-0.11798***	-0.12764*	-0.12375*
	(0.0257)	(0.0324)	(0.0276)	(0.0362)	(0.0663)	(0.0668)
Male	-0.04124***	-0.05427***	-0.04199***	-0.06196***	-0.04162***	-0.04660***
	(0.0128)	(0.0133)	(0.0129)	(0.0138)	(0.0157)	(0.0152)
Unemployed	-0.01409	-0.01628	-0.00504	-0.00688	-0.01765	-0.03214
	(0.0173)	(0.0197)	(0.0188)	(0.0219)	(0.0332)	(0.0396)
Retired	-0.03608	-0.06533***	-0.04743*	-0.05703**	-0.02114	-0.07459
	(0.0230)	(0.0248)	(0.0245)	(0.0279)	(0.0406)	(0.0456)
Employee	0.04758**	0.05522***	0.03867*	0.07018***	0.06528*	0.03298
	(0.0188)	(0.0210)	(0.0201)	(0.0234)	(0.0345)	(0.0411)
Married	0.01329	0.02638**	0.02390*	0.03109**	0.00469	0.01966
	(0.0127)	(0.0133)	(0.0133)	(0.0148)	(0.0165)	(0.0156)
Nbr of adults	-0.02034***	0.00015	-0.02740***	0.00071	-0.01618*	0.00091
	(0.0070)	(0.0069)	(0.0083)	(0.0086)	(0.0088)	(0.0085)
Nbr of children	-0.02164***	-0.00435	-0.02160***	-0.01022*	-0.01949***	0.00110
	(0.0048)	(0.0050)	(0.0051)	(0.0057)	(0.0061)	(0.0061)
High school Dummy	0.04853***	0.06434***	0.04073***	0.06457***	0.05617***	0.06224***
	(0.0130)	(0.0139)	(0.0130)	(0.0148)	(0.0159)	(0.0158)
College and more Dummy	0.09779***	0.16930***	0.09658***	0.15811***	0.09824***	0.17796***
	(0.0154)	(0.0174)	(0.0157)	(0.0178)	(0.0186)	(0.0205)
Net wealth quartile II	-0.01160	-0.02059*	-0.01022	-0.03618***	-0.00793	-0.00642
	(0.0104)	(0.0119)	(0.0124)	(0.0139)	(0.0142)	(0.0147)
Net wealth quartile III	-0.00231	-0.02204*	-0.02319*	-0.05483***	0.01738	0.01950
	(0.0113)	(0.0131)	(0.0135)	(0.0154)	(0.0149)	(0.0165)
Net wealth quartile IV	0.10322***	0.13888***	0.10304***	0.11762***	0.10094***	0.17005***
	(0.0144)	(0.0139)	(0.0161)	(0.0159)	(0.0176)	(0.0178)
Financial sector Dummy	0.04547	-0.05607	0.04725	0.00737	0.03458	-0.13415
	(0.0887)	(0.0855)	(0.0899)	(0.0922)	(0.1132)	(0.0936)
Government sector Dummy	0.00490	-0.04340***	0.00550	-0.04306***	0.00477	-0.03844**
	(0.0136)	(0.0144)	(0.0151)	(0.0160)	(0.0168)	(0.0172)
Observations	*36513*	*36513*	20303	20303	16210	16210
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time FEs	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood FEs	*Parish*	*Parish*	*Parish*	*Parish*	*Parish*	*Parish*
Country-of-origin FEs	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Arrival Year FEs	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for various sample periods: the full sample (1999-2007), the medium term (1999-2003), and the longer term (2003-2007). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have quantitative education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VII: Sample Split By Education: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

	High school and more		Less than high school	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
Panel A: Medium-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.76109**	1.10336***	0.07489	-0.31025
	(0.3476)	(0.3733)	(0.4967)	(0.4636)
Observations	14392	14392	5911	5911
Panel B: Longer-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.69484*	1.66823***	-0.49097	0.17723
	(0.3912)	(0.3746)	(0.6391)	(0.5524)
Observations	11936	11936	4274	4274
Household Controls	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Country-of-Origin Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Arrival-year Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood Fixed Effects	*Parish*	*Parish*	*Parish*	*Parish*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*
Time-varying Initial Elec. Dist. Controls	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for two subsamples based on educational attainment. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VIII: Sample Split By Gender: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

	Female		Male	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
Panel A: Medium-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	-0.07749	0.31652	0.71963**	1.04624***
	(0.5282)	(0.5584)	(0.3418)	(0.3549)
Observations	6620	6620	13683	13683
Panel B: Longer-Term	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	-0.29269	0.78593	0.74686*	1.51874***
	(0.5497)	(0.5428)	(0.3979)	(0.3805)
Observations	5285	5285	10925	10925
Household Controls	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Country-of-Origin Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Arrival-year Fixed Effects	*Yes*	*Yes*	*Yes*	*Yes*
Unobserved HH Heterogeneity	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood Fixed Effects	*Parish*	*Parish*	*Parish*	*Parish*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*
Time-varying Initial Elec. Dist. Controls	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for two subsamples based on gender of the household head. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table IX: Long Shadow Effects of Having Retirement Savers as Neighbors: Medium-Term, and Longer-Term

	Medium-Term		Longer-Term	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
	(iii)	(iv)	(v)	(vi)
Initial Fin Lit Ext	0.19549*	0.28530**	0.26358**	0.39585***
	(0.1078)	(0.1155)	(0.1214)	(0.1224)
Local Financial Development in the Elec. Dist.	0.01839	-0.08643**	0.00740	-0.08531**
	(0.0337)	(0.0358)	(0.0359)	(0.0385)
Median Taxable Wealth in the Elec. Dist.	-0.00272*	-0.00393**	-0.00319*	-0.00314*
	(0.0015)	(0.0017)	(0.0018)	(0.0017)
Median Income in the Elec. Dist.	-0.07475**	0.02025	-0.04093	0.02611
	(0.0338)	(0.0338)	(0.0384)	(0.0367)
Income	0.15807***	0.21076***	0.20724***	0.19401***
	(0.0134)	(0.0147)	(0.0171)	(0.0147)
Age 30-45	0.04413**	-0.02356	-0.04614	-0.05184
	(0.0172)	(0.0247)	(0.0624)	(0.0635)
Age 45-60	0.08686***	-0.05961**	-0.03336	-0.08411
	(0.0211)	(0.0279)	(0.0634)	(0.0634)
Age 60-75	-0.01155	-0.12186***	-0.13438**	-0.13471**
	(0.0275)	(0.0362)	(0.0665)	(0.0671)
Male	-0.04182***	-0.06193***	-0.04161***	-0.04624***
	(0.0129)	(0.0138)	(0.0157)	(0.0152)
Unemployed	-0.00524	-0.00748	-0.02080	-0.03742
	(0.0187)	(0.0219)	(0.0328)	(0.0392)
Retired	-0.04800**	-0.05766**	-0.02398	-0.07968*
	(0.0244)	(0.0280)	(0.0403)	(0.0452)
Employee	0.03850*	0.06980***	0.06210*	0.02737
	(0.0200)	(0.0233)	(0.0341)	(0.0406)
Married	0.02346*	0.03063**	0.00453	0.01975
	(0.0134)	(0.0148)	(0.0164)	(0.0156)
Nbr of adults	-0.02713***	0.00114	-0.01586*	0.00103
	(0.0083)	(0.0086)	(0.0088)	(0.0085)
Nbr of children	-0.02161***	-0.00997*	-0.01937***	0.00092
	(0.0051)	(0.0057)	(0.0061)	(0.0062)
High school Dummy	0.04038***	0.06433***	0.05605***	0.06172***
	(0.0130)	(0.0148)	(0.0159)	(0.0158)
College and more Dummy	0.09546***	0.15703***	0.09767***	0.17643***
	(0.0157)	(0.0178)	(0.0186)	(0.0205)
Net wealth quartile II	-0.01059	-0.03664***	-0.00766	-0.00618
	(0.0124)	(0.0139)	(0.0142)	(0.0147)
Net wealth quartile III	-0.02325*	-0.05511***	0.01749	0.01970
	(0.0136)	(0.0154)	(0.0149)	(0.0164)
Net wealth quartile IV	0.10315***	0.11736***	0.10023***	0.16923***
	(0.0161)	(0.0159)	(0.0175)	(0.0178)
Financial sector Dummy	0.05001	0.00833	0.03748	-0.12564
	(0.0908)	(0.0925)	(0.1127)	(0.0922)
Government sector Dummy	0.00599	-0.04270***	0.00540	-0.03697**
	(0.0150)	(0.0159)	(0.0168)	(0.0171)
Observations	20303	20303	16210	16210
Clustering	*Electoral District*	*Electoral District*	*Electoral District*	*Electoral District*
Time FEs	*Yes*	*Yes*	*Yes*	*Yes*
Neighborhood FEs	*Parish*	*Parish*	*Parish*	*Parish*
Country-of-origin FEs	*Yes*	*Yes*	*Yes*	*Yes*
Arrival Year FEs	*Yes*	*Yes*	*Yes*	*Yes*
Industry Composition of the Initial Elec. Dist.	*Yes*	*Yes*	*Yes*	*Yes*

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. We also control for median income, median taxable wealth, median credit-to-income ratio, and fixed effects for the major industry of occupation of the residents in the initial electoral district of allocation. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who save for retirement in a private retirement account. We consider a balanced sample of 4,061 refugee immigrants. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Data Appendix: Variable Descriptions

- *Stockownership:* A binary variable that is set to one if the household holds stocks in period t, and zero otherwise. Stocks include all forms of direct and indirectly held stocks, except stocks held through retirement accounts in year t. The latter are not included in the data.

- *Saving for Retirement:* A binary variable that is set to one if the household makes in year t a contribution to a tax-deferred private retirement account. If no contribution is made, even to an already open account, the variable takes the value zero.

- Household disposable income: Household disposable income in year t. This variable includes labor income, capital income (if any), student aid (if any), pension income (if any), unemployment benefits (if any), and welfare support net of taxes.

- *Age<30:* Household head is younger than 30 years old in year t.

- *30≤Age<45:* Household head is (equal to or) older than 30 years old and younger than 45 years old in year t.

- *45≤Age<60:* Household head is (equal to or) older than 45 years old and younger than 60 years old in year t.

- *60≤Age:* Household head is or is older than 60 years old in year t.

- *Male:* Household head is male.

- *Unemployed/Uncategorized:* Household head has received unemployment benefits, registered as unemployed or does not qualify for any other occupation category in year t.

- *Retired:* Household head has received pension greater than labor income and does not qualify for any other occupation category in year t.

- *Student:* Household head has received student aid at least equal to one semester government student aid in year t.
- *Employed:* Household head is not retired nor student and has received positive labor income in year t.
- *Married:* Household head is married in year t.
- *Number of adults:* Number of household members at least 18 years old in year t.
- *Number of children:* Household members younger than 18 years old in year t.
- *High school graduate:* Household head has a high school education in year t.
- *College graduate:* Household head has a college (or more) education in year t.
- *Household net wealth:* Household net wealth in Swedish Kroners (SEK), calculated as the sum of all real and financial assets minus all debt, except student loans
- *Working in the financial sector:* Household head has worked in the financial sector in year t.
- *Working for the government:* Household head has worked for the local or central government in year t.
- *Economics/business education share:* The share of households who had business and economics related topics as their major during their studies in a given given electoral district in year t. The business and economics related topics include Economics and Economic history, and Business Administration (i.e., Banking, insurance, and finance, Accounting and taxation, Management and administration, Marketing, etc.)
- *Quantitative education share:* The share of households who have a quantitative educational background in a given given electoral district in year t. The quantitative

education includes Science, mathematics, computing, and Commerce, administration, law, etc.

- *Retirement savers share:* The share of households who save for retirement in a given electoral district in year t.

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