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# Financial Literacy Externalities*

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**Abstract**

This paper uses unique administrative data and a quasi-field experiment of exogenous allocation 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 exposure and of a social multiplier of financial knowledge, but also of unfavorable distributional aspects of externalities. Exposure promotes saving in private retirement accounts and stockholding, especially when neighbors have economics or business education, but only for educated households and when interaction possibilities are substantial. Findings point to transfer of knowledge rather than mere imitation or effects through labor, education, or mobility channels.

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

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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] Existing literature has sought to measure own financial literacy and to study the role that it can play for household outcomes.[3] Even when an exogenous influence of financial literacy is fully established, the cost effectiveness of suitable programs may be challenged if they can only reach limited segments of the population (e.g., school children).[4] Thus, understanding financial knowledge spillovers across peers is probably as important as the exogenous influence of financial literacy on own behavior, but largely ignored in discussions to date.

[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] 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 attributes 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.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).

[4] 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.

This paper 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 over the medium and longer term. 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, by showing not only a lasting exogenous effect but also greater cost effectiveness resulting from a social multiplier.

Our approach is first to establish an exogenous peer effect on financial behavior and then to argue that it involves successful transfer of relevant knowledge rather than confounding effects through indirect channels. We then proceed to a third level, where we suitably vary factors influencing knowledge transmission. This is useful to understand whether financial literacy externalities are equally operative across individuals or mainly across the more educated and more connected, pointing to distributional consequences and the need for targeted programs.

In the context of establishing exogenous influences on peer financial behavior, 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]

[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.

We consider the effect of exposure to neighbors with business or economics education and some college attendance on participation in private retirement accounts (as distinct from social security contributions and occupational pension plans), and stockholding. 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 being exposed to financially literate neighbors. 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. These include pure imitation, a labor market channel, an encouragement of (relevant) refugee education, and a mobility channel.

As educational attainment and business or economics content are related to stockholding and to saving for retirement, there may be a concern that we are simply re-discovering an imitation peer effect rather than an externality arising from the transfer of relevant information. Our analysis finds that the share of participating neighbors has smaller effects than that of knowledgeable neighbors when entered on its own; and the initial share of neighbors with business and economics education who do not hold the financial asset in question still has a statistically significant effect, even when the share of holders is additionally included in the regression.

Further, we do not find evidence that knowledgeable neighbors improve labor market prospects for refugees, who then choose to participate in assets because of their better 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 standing; or that they influence financial behavior by encouraging refugees to obtain more education or more economics-related education. We also show that the effect on financial behavior does not manifest itself through encouragement of refugees to move to another neighborhood, conducive to financial market participation.

We then subject our argument, that the exogenous peer effects on financial behavior we study reflect transmission of knowledge, to various tests through variations in factors influencing such transmission. First, we vary the knowledge of financially literate neighbors, from business and economics education to quantitative education, and then to college education in fields other than business or economics. We find that content rather than the level of education matters. 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. When we vary the likelihood of interactions between neigbhors and refugees, we find that effects are operative in areas where Swedish neighbors are more positively predisposed to immigrants, and where there is a critical mass of knowledgeable neighbors. Moreover, effects are operative for refugees who had children, and thus more impetus to interact, at the time of initial allocation. Finally, we present some evidence that the initial share of financially literate neighbors affects not only whether refugees participate in certain assets but also their degree of portfolio inertia and diversification. All in all, our findings strongly point to the conclusion that financial literacy externalities involve the transfer, processing, and salience of relevant information.

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]

[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

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 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.

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. (2017) 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. (2017).

[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.

# 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 permit. 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.

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

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 officers, 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 O.A.1 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

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

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:

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

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

[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.

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 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. For example, 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

[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.

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 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 $\gamma_I$. In order to control for conditions in the broader area of placement, the parish, relevant for financial market behavior, we introduce fixed effects $\gamma_j$, 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, $\gamma_k$, to capture language- and culture-related factors; and fixed effects for the year of arrival, $\gamma_0$, and the year of observation, $\gamma_t$, 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.

[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 Arctic 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 $http://val.se/$.

# 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

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

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" 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.2. 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 The measure of 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. In order to ensure a high potential for random encounters, we consider a small neighborhood around the apartment where the refugee household was placed, namely 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 discussed.[17] The potential of refugees for experiencing financial literacy externalities and improving their own financial behavior through such interactions is assumed to be an increasing function of the share of financially literate people living in their electoral district.[18]

There is no single way to define financial literacy.[19] The most widely adopted definition of financial literacy focuses on knowledge of basic financial concepts and familiarity with the economic environment. Such knowledge is most often proxied by answers to the "Big Three" questions of financial literacy, capturing knowledge of interest compounding, the difference between real and nominal interest rates, and risk diversification.

Not surprisingly, our dataset does not include scores of financial literacy of the initial neighbors to the refugees. However, it provides detailed information on the level of educational attainment and content of education for people living in each electoral district. Our benchmark measure of financial literacy in the neighborhood refers to the share of neighbors in the electoral district of initial allocation who have business or economics education and have attended (but not necessarily completed) college.

Our education-based measure of financially literate neighbors correlates closely to measures based on the Big Three survey questions. In particular, the survey work of Almenberg

[17] This parallels the distance-based approach of Pool et al. (2015) in analysis of mutual fund managers. For information on the size of electoral districts, see section 3.

[18] 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 apartment, such as those we consider, the immediate neighbors are the most likely contact points.

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

and Söderbergh (2011) on a Swedish sample finds that almost half of the respondents with a major in economics or engineering answered all three questions correctly, compared to about one quarter among each of the other majors (social sciences, arts and humanities, and medicine).[20] The largest difference was found in the first and second questions, where 68 percent and 82 percent of those with an economics major answered correctly, compared to 36 percent and 66 percent among other college majors. There is no large difference in answering question 3 correctly among the college majors.

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. In terms of gender composition, neighbors with business or economics education and college attendance were reasonably gender-balanced, with 53 percent of them male and 47 percent female in the 1991 sample. In what follows, we will also vary the education content of the measure in order to shed light on the role that content and educational attainment play.

# 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,

[20] Almenberg and Söderbergh (2011) use a slightly more demanding variant of the first question, which has been found to work better with European samples: "Suppose you have 200 SEK in a savings account. The interest is 10 per cent per year and is paid into the same account. How much will you have in the account after two years?".

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.

Table II 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 III 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.[21] Separating the two "runs" allows not only the effect of financial literacy externalities but also the relationship of participation probability to household

[21]Table O.A.3 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.

characteristics and other factors to differ across the two periods of observation of financial behavior.

When considering only the medium run since 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, both in estimated size and in statistical significance, than the medium-run effect. The effects are also economically meaningful. 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, the 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 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.

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.

# 6 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.

## 6.1 Correlated effects: Characteristics of the environment

It is important 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 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 II, 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.

## 6.2 A pure imitation effect

A common finding in participation literature is that higher education levels (especially college education) are correlated with a greater tendency to participate in risky assets and to save for retirement (through private accounts). Our findings link educational attainment and content of neigbhors to participation outcomes of refugees. Is this a simple restatement of the known (economics) education-participation link, as in Christiansen et al. (2008), combined with an imitation peer effect in financial behavior, as in Duflo and Saez (2002)? Are financial literacy externalities exhausted in imitation of asset holding or do they crucially encompass transfer of knowledge regardless of neighbor asset market participation?

This is an important question, as the answer could have very different implications for policy: pure imitation could lead us to incentivize asset holding among particular groups, in the hope that such behavior will spread to their neighbors and peers; transfer of knowledge could lead us to promote business and economics education, aimed at transfers of relevant knowledge that would enable neighbors and peers to hold stocks and individual retirement accounts. We address this question by introducing alternative or additional controls in our

benchmark specification to allow for the presence of asset holders in the electoral district of initial placement, and by considering effects across the two assets.

Table IV introduces the share of stockholders in the neighborhood in different ways and examines whether the significance of our base financial literacy variable is due to the presence of stockholding neighbors. Column (i) introduces in the benchmark regression the share of stockholding initial neighbors alone, while column (ii) presents estimates when the share of stockholders appears together with the share of business or economics educated neighbors.[22] In both specifications, the share of stockowners bears no statistically significant relationship to the stock market participation behavior of refugee households in either the medium or the longer run. By contrast, our education-based measure of financial literacy of neighbors retains its significance, even in the presence of the share of stockholding initial neighbors.

We consider further breakdowns. In column (iii), we control for the neighbors with business or economics education without any stock investments and find that removal of stockholders does not invalidate the significance of this measure. In column (iv), we restrict attention to those neighbors with both business or economics education and stock ownership, but find an insignificant estimate. Column (v) controls for the share of neighbors with stock ownership but no education in business or economics, and the estimate turns out to be insignifcant.

Further support for the relevance of knowledge transfer rather than imitation is provided by considering cross-asset effects. Table V modifies the benchmark educational variable to include only neighbors with business or economics education, some college attendance, but no stockholding. Columns (i) and (ii) show that the share of stockowners in the initial electoral district alone has no statistically significant effect, neither on stock market participation nor on participation of immigrants in private retirement accounts, in either run. By contrast, columns (iii) and (iv) demonstrate that the education variable has signifi-

[22] Our data do not report stock holdings directly for that early period, but we are able to observe the share of relevant neigbhors who receive dividend income.

cant effects, both on refugee stockholding (despite the absence of neighbor stockholders) and on participation in the other asset, private retirement accounts.[23]

The above findings strongly suggest that the patterns of statistical and economic significance we find in our base runs are not a mere product of the presence of stock owners in the electoral district who happen to have the educational qualifications we consider. Nevertheless, we do not regard our findings as proof of irrelevance of the stockholding neighbors: indeed, this would be contrary to existing literature on the subject. We view the lack of significance as resulting from the very small shares of stockholding neighbors satisfying the requirements of these cuts of the data. This conjecture is supported when we next consider the more substantial share of private retirement savers. In this case, estimated effects of retirement savers in the neighborhood are significant, allowing us to extend our results to the case of substantial presence of asset holders in the neighborhood.

Table VI focuses on private retirement savers,[24] rather than on those participating in stocks, but otherwise repeats the exercise of Table V. The first two columns shed considerable light on the issue of imitation versus knowledge transfer. The first shows that the share of private retirement savers in the initial electoral district does influence the medium- and longer run probabilities of refugee participation in private retirement accounts, suggesting a possible imitation effect. However, comparison with our base results also shows that this imitation effect, to the extent that it is present, is smaller in estimated magnitude than that of the share of financially literate neighbors. Column (ii) takes us a step further and shows that the share of private retirement account participants also affects participation in stocks, in both runs, and to a larger extent than in the retirement asset held by neighbors. This cross-asset effect is very hard to reconcile with pure imitation. Columns (iii) to (vi) show further that the share of neighbors with business or economics education has a large

[23]No statistically significant effect is found only for participation in private retirement accounts over the longer run, as was also the case in our baseline regressions with business or economics educated neighbors.

[24]Our data do not include the shares of neighbors who participate in private retirement accounts exactly at the time of entry of the refugees, but a few years later, namely in 1994. Based on the literature on participation inertia, we use these figures on the assumption that they capture the distribution of private retirement savers across electoral districts at the somewhat earlier time of entry.

and significant effect, even when this variable excludes participants in private retirement accounts; that the effect is larger than that of the presence of retirement savers; and that the effect extends to the other asset (stocks) and is larger there.

As a final check on whether imitation of financial behavior, as opposed to knowledge transfer, has been a major force guiding the subsequent behavior of refugees, we have considered whether these refugees were influenced by their initial neighbors owning a house or having any debt outstanding at the time of entry.[25] We replace the share of financially literate neighbors with the shares of homeowners and borrowers among initial neighbors, in turn, and consider the effect of each share on refugee participation in homeownership or debt in the medium and longer runs. We find that early exposure to homeowners or to borrowers produces no significant effect on the corresponding choices of immigrants to own real estate or to borrow in the medium- and longer runs.

All in all, our findings suggest that, while teaching by example may influence financial behavior, it produces smaller effects than social interactions with knowledgeable people regardless of the assets they hold. More strikingly, business or economics education does have significant effects on participation in both assets, even when we exclude participants in either one of the assets from the regressor. The combination of these results provides strong support against the argument that our findings simply reflect imitation of asset holding of peers. The estimated size and significance of neighbor education variables point to a process of information and knowledge transfer that goes well beyond imitation.

## 6.3 A local labor market explanation

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

[25] Homeownership is measured as paying property tax on single-family homes (we are missing apartments). Having debt is measured as deducting interest rate payments in the tax form.

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.[26]

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 VII 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,[27] and whether the respondent is unemployed.

[26]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.

[27]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)) including 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.

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. We only find an effect on the probability that the refugee ends up working in the financial sector over the longer run, but this is relevant for a very small number of refugees.

## 6.4 An effect through encouragement of further education

It is possible that being immersed in an environment with people educated in business or economics significantly encourages refugees to acquire further education, possibly in the fields of business or economics. The participation literature typically finds a statistically significant relationship between educational attainment and asset market participation. So, maybe the link between neighbors educated in business or economics and stockholding or private pension participation runs through encouragement of (relevant) education rather than through the direct transfer of knowledge relevant for financial asset participation.

In order to test econometrically the presence of such a channel, we consider the cross section of immigrants in 1999. We first examine whether refugees were systematically encouraged to acquire business or economics education by neighbors who had similar education. In unreported regressions of a dummy pointing to refugees who had business or economics education by 1999 on the measure of financial literacy in the initial neighborhood and on the other controls, results show no statistically significant effects of the configuration of neighbors on the likelihood that refugees would have business or economics education by 1999.

We then consider whether the presence of neighbors with relevant education may have encouraged refugees to reach higher levels of any type of education by 1999. We regress their total years of education by that time on the other controls and fixed effects of our benchmark model. Table O.A.8 shows that we find no statistically significant relationship of the share of financially literate neighbors on years of schooling attained by refugees until 1999.

To sharpen results further, we next consider, in unreported regressions, only the extra years of education obtained after entering Sweden. In descriptive statistics, the median years of education reported by refugees upon arrival and in year 1999 are actually the same, namely 11. When we regress additional years of education since entry on the relevant controls, we obtain insignificant coefficients on the peer financial literacy proxies. Taken together, our results do not support an operative channel from the share of financially literate neigbhors to stock market participation or participation in private retirement accounts which goes through number of years or relevant focus of education.

## 6.5 A mobility effect

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. As shown above in Table VII, 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.

Now, mobility points to the length of exposure to financially literate neighbors: it is reasonable to suppose that, if the effects we argue for are relevant, then longer exposure to neighborhoods with a larger share of financially literate neighbors should produce a bigger effect on asset participation probabilities of refugees ten to twenty years after entry. Indeed, our data allow us to compute total exposure to financially literate neighbors between entry and the time of observation of financial behavior, as it tracks the location of refugees throughout the period. We do so, by weighting the relevant shares of financially literate neighbors in each location by the length of time spent in that location.[28]

In assessing effects of cumulative exposure, we need to tackle the fact that subsequent

[28]The definition of such a cumulative exposure variable is motivated by various articles on neighborhood effects (Kling et al., 2007; Åslund et al., 2011; Ludwig et al., 2013).

location of refugees is endogenous. In lieu of exogenous instruments for each subsequent move of each refugee, we consider the effect of the part of subsequent cumulative exposure attributable to the exogenous initial exposure to financially literate neighbors.[29]

The 2SLS estimation model for cumulative exposure to financially literate neighbors takes the following form:

$$Y_{iklj0t} = \alpha_1 \cdot X_{it} + \alpha_2 \cdot X_{l0} + \beta \cdot \overline{Ext}^*_{iklj0t} + \gamma_I + \gamma_j + \gamma_k + \gamma_0 + \gamma_t + \epsilon_{iklj0t} \tag{2}$$

$$\overline{Ext}^*_{iklj0t} = a \cdot X_{it} + b \cdot Ext_{l0} + \gamma_I + \gamma_j + \gamma_k + \gamma_0 + \gamma_t + e_{iklj0t} \tag{3}$$

$\overline{Ext}^*_{iklj0t}$ is the fitted value for the cumulative financial literacy externalities from Equation 3, where we use the share of financially literate people in the initial electoral district, $Ext_{l0}$, as an instrument.[30]

Results are reported in Table VIII. For the medium run, we find statistically significant and economically much more sizeable effects on participation in both assets than in the base model. The same holds true for longer-run effects on stockholding behavior, but we do not find statistically significant, longer-run effects of subsequent cumulative exposure on participation in private retirement plans. The magnification of estimated effects when we consider the part of subsequent cumulative exposure attributable to initial exogenous exposure provides further support for the relevance of financial literacy externalities for asset participation outcomes.

[29] In computing subsequent cumulative exposure, we exclude exposure to financially literate neighbors in the initial neighborhood. Considering cumulative exposure instrumented by initial exposure requires that the share of financially literate neighbors in the initial electoral district is indeed excludable, and that its effect runs through determining the subsequent cumulative exposure and possibly through observable factors that we control for, but not through any unobservable factors. See also Åslund et al. (2011).

[30] Note that the instrument of business or economics education enters positively and significantly in the unreported first stage regressions, and it is highly significant. The F-statistic for the first stage regressions is far greater than 10, which is used as a rule-of-thumb threshold to evaluate whether the excluded instrument suffers from a weak instrument problem. Note that we do not introduce fixed effects for the parish in each year of the refugee's presence in the sample, as these would be endogenous and there is no possibility to instrument all of them given the available data. On this, see also Edin et al. (2003).

Taken together, our numerous findings in this section do not support the idea that the 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, pure imitation of the financial behavior of neighbors, or to indirect effects of financially literate neighbors through employment prospects, encouragement of further education, or relocation of refugees. In what follows, we try to shed light on the process through which the financial literacy of initial neighbors is transmitted to greater participation probabilities of refugees in stocks and in private retirement accounts much later in their lives.

# 7 On the Process of Transmission

In this section, we probe into channels through which the relevant knowledge of close initial neighbors is transmitted to refugees, so as to influence their financial behavior over the medium and longer run. This is a challenging task, as we do not observe the interactions between initial neighbors and refugees directly. Our approach here is three-pronged. First, we vary the degree of relevant knowledge of initial neighbors and examine whether greater knowledge of relevance to financial behavior is linked to greater estimated effects on the subsequent financial behavior of refugees. Second, we vary the ability of refugees to interpret and apply financial knowledge, and we examine whether the effects on the financial behavior of more able refugees are indeed more pronounced. Third, we vary the likely intensity of interactions and examine whether estimated effects are indeed stronger when the likelihood of interaction is greater. We find a very consistent pattern of results: the estimated size of effects is larger when initial neighbors are more knowledgeable in matters relevant for financial behavior, when immigrant household heads are more able to interpret the signals, and when interaction between refugees and initial neighbors is more likely.

## 7.1 Varying the education of initial neighbors

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 O.A.6. 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.

We have rerun our entire set of regressions in this paper, replacing the share of neighbors with business or economics education and some college attendance with the corresponding share for quantitative education. This substitution produces the same pattern of significance as our base measure of financially literate neighbors, but smaller coefficient estimates. The smaller effects with the broader and less demanding notion of financial literacy among neighbors across the board lend considerable support to the view that the process through which financial literacy externalities operate is one in which knowledge content matters.

Further support to the importance of knowledge content is provided by a different exercise. Since both sets of significant results above 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.

## 7.2 Varying the education of refugee household heads

Another 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.[31]

Before we compare estimates based on the sample split, we want to confirm that they are not an artefact of the more educated 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 O.A.1 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 split in this section, Table O.A.10 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, as well as comparable variation in this exposure.

Table IX 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

[31] 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.

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.[32]

We have also experimented with the content of refugee education, and in particular whether refugees had business or economics education. We introduced a dummy to our base regressions for the full sample, alongside our financial literacy proxies in the neighborhood. Table O.A.11 presents the results. The variables for the business or economics content of refugee education turn out to be insignificant for both assets and runs, while the original variables for neighbor financial literacy retain their pattern of significance, as in the benchmark regressions.

Our findings on the education sample split 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 households, tends to overcome relative unwillingness to consult others because of overconfidence, thus making financial literacy externalities operative.

## 7.3 Varying the likely intensity of interactions

The likelihood of fruitful interaction is a function of the availability of knowledgeable neighbors in the initial electoral district, and of their willingness and opportunities for interaction with refugees. In this section, we vary the likely intensity of interactions in several different ways, and we examine whether the econometrically estimated effects on financial behavior of refugees are generally greater in situations where interactions with knowledgeable neigh-

[32]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 is weaker, consistent with the importance of content. Specifically, Table O.A.12 shows that 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.

bors are plausibly more likely.

### 7.3.1 Do attitudes towards immigrants matter?

It is plausible that social interactions between refugees and their neighbors are less likely to take place where locals are more negatively predisposed towards refugees. 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,[33] which include responses to an important question regarding attitudes, 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 bad".[34] We focus on respondents with college education and classify them as having a positive attitude to immigrants if they pick the first or second option. Otherwise the respondent is classified as not having a positive attitude. The share of positive respondents per county is then calculated.[35] 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, as reported in O.A.13, 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.[36]

[33]Weibull, Lennart, Sören 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.

[34]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.

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

[36]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.

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 is consistent with the view that the process of transmission involves social interactions between refugees and their initial neighbors.

### 7.3.2 Does having children matter?

We next split the sample of refugee immigrants by having children at the time of initial allocation (i.e., 1987-1991). When we split the sample by having children at the time of allocation[37] and analyze the effects of early interactions with financially sophisticated neighbors on the subsequent financial behavior of refugees, we find different results. The sample splits are reported in Table X. Effects are only operative in the subsample of refugees with children at the time of initial arrival, consistent with the view that financial literacy externalities are operative when repeated interactions are more likely.[38]

Could it be that the effects of having children at the time of initial location refer to financial aspects of having children relevant for the two assets, rather than to the interaction opportunities they create for refugees? First, we do control for the numbers of adults and of children in all of our participation regressions. Second, we have experimented with sample splits controlling for the presence of children in the refugee households during the observation period for financial behavior (1999-2007), but we did not find that such presence mattered for whether financial literacy externalities were operative.

### 7.3.3 Does the size of knowledgeable neighbor share matter?

In our data, the share of initial neighbors with business or economics education and some college attendance ranges from zero to 22.5 percent in an electoral district, while the average

[37] Having a child at the time of entry refers to having at least one child under the age of 18. The median age of the youngest child at the time of entry is 5 years.

[38] Table O.A.14 shows that the result is robust to using the share of neighbors with quantitative education and some college attendance.

share is two percent. We want to see if having a critical mass of financially literate neighbors is important for the presence of financial literacy externalities, as seems plausible. We create four dummy variables, one for each quartile of the distribution of shares of financially literate neighbors in the initial neighborhood and introduce those in the regression instead of the (inverse hyperbolic sine of the) share of financially literate neighbors.[39] Table XI shows that the estimated effect of financial literacy externalities monotonically increases in the density of financially literate initial neighbors. In this cut of the data, the externalities in the bottom three quartiles are not precisely estimated, and we get statistically significant effects in the top quartile.[40] The effects are also economically significant, ranging from 3.3 to 4.7 percentage points, depending on the outcome that we consider.

### 7.3.4 Introducing more recent neighboring immigrants

We now focus on neighbor immigrant status and recency of entry as influencing the probability of interaction with the refugees. Specifically, we broaden the relevant circle of neighbors to the refugees, by including also neigbhoring recent migrants that have stayed in Sweden between 10 and 20 years. The greater similarity in degree of assimilation to the local culture between the newly arrived refugees and these recent migrants should increase the overall likelihood of interaction.[41]

Table O.A.16 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 signifi-

[39]The IHS transformation introduces some allowance for non-linearity: for example, in a linear probability model, the coefficient on this variable is very close to the semi-elasticity of the share. Moreover, in unreported regressions, we have found that using the level of the share instead of its IHS transformation produces similar implications as those discussed in the text.

[40]When focusing on neighbors with business or economics education and some college attendance, the top quartile ranges from shares of 3.67 and 22.53 percent. In the case of quantitative education, it ranges from 6.26 to 39.18 percent.

[41]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.

cance 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 to considering quantitative education as the basis for defining the share of financially literate neighbors.

This mixed result looks intriguing at first, but is actually quite consistent with the importance of social interactions. If greater intensity of interactions with neighbors were the only relevant factor, we would expect to observe higher estimated effects on participation in both financial instruments (private retirement accounts and stocks) when we introduce more recent immigrants. However, earlier work on the Swedish native and migrant population has found that the period between ten and twenty years of stay in Sweden is quite important for assimilation of migrant stockholding behavior to that of Swedes (Haliassos et al., 2017). Thus, although, inclusion of recent immigrants is likely to raise the probability of interaction of refugees with the circle of neighbors, it also lowers the probability that stocks will be salient for a random neighbor. The smaller estimated effect on stockholding despite higher likely interaction of refugees with neighbors is consistent with the interplay of these two factors.

## 7.4 From participation to market behavior

So far, we have focused on effects relating to participation in asset markets. The case for financial literacy externalities could be further strengthened if we found that the shares of knowledgeable initial neighbors tend to influence also how immigrants handle their portfolios in information-intensive markets, such as the stock market: do they tend to exhibit less inertia or under-diversification, for example? The survey results of Almenberg and

Söderbergh (2011) would be consistent with such effects, in view of the documented superior knowledge of interest compounding and of real versus nominal interest rates among those with business or economics education.

A full-fledged analysis of such issues is beyond the scope of the present study. However, we present some encouraging indicative results in Tables XII. Specifically, we focus on the full estimation period and consider as outcomes portfolio inertia and a simple measure of under-diversification. We proxy for portfolio inertia with the absolute value of the change in the risky portfolio share (Calvet et al., 2009). For under-diversification, we consider the portfolio share of directly held stocks in the overall stock portfolio.[42] We do find that being placed in an electoral district with higher shares of financially literate neighbors tends to reduce portfolio inertia (in the sense of encouraging trades to limit changes in portfolio shares) and to improve diversification based on our (admittedly coarse) measure. Interestingly, the role of quantitative education among neighbors is elevated here relative to that of basic business and economics education, possibly pointing to the more involved mathematical concepts underlying these aspects of behavior. Once again, this is consistent with content being transmitted.

# 8 Conclusions and Policy Implications

This paper uses unique administrative data and a quasi-field experiment of exogenous allocation of refugees to estimate the effect of access to financially literate neighbors on two important aspects of financial behavior: saving for retirement through private accounts, and participation in stockholding. As we can track refugee households over twenty years, we can estimate the effects of the exogenous component of exposure to knowledgeable initial neighbors (over an average length of stay of 5.4 years) as it influences financial behavior ten to twenty years later. The nature of the experiment allows us to address thorny causality

[42]Following Calvet et al, we only consider those households with direct stock investments, and this is what reduces the number of observations in these tests.

issues related to endogenous choice of neighborhood.

We find evidence of statistically and economically significant effects of the share of initial neighbors with business or economics education at college level. Benchmark estimation controls for 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. Could our results be due to simultaneous exposure of refugees and neighbors to environmental characteristics favoring asset market participation? We address this issue of correlated effects by controlling for relevant economic conditions in the electoral district, as well as for unobserved factors in the broader parish. Could they represent a mere imitation effect? We show that the share of participating neighbors has smaller effects than that of knowledgeable neighbors, and that the latter have pronounced effects even when not participating. Could the effect be due to knowledgeable neighbors improving labor market prospects for refugees, who then choose to participate in asset markets because of their better standing? We do not find evidence of significant effects of initial knowledgeable neighbors on labor market outcomes of refugees. Could the effect arise because knowledgeable neighbors encourage refugees to get additional years of education or to be more likely to have business or economics education when we start to observe their financial behavior? We do not find evidence of either type of influence. Could knowledgeable initial neighbors be encouraging refugees to move to areas more conducive to asset participation? We do not find evidence of a significant effect on the probability that refugees will have moved by the start of the observation period for financial behavior.

We next explore the nature of the process of transmission from knowledgeable initial neighbors to the refugee households. Our approach is to vary factors influencing the knowledge of initial neighbors, the ability of refugees to interpret information, and the likelihood

of interactions between them. We find that content rather than the level of neighbor education matters. Financial literacy externalities are operative only for the subsample of refugee household heads with at least a high school degree. We confirm that these results are not plausibly due to sorting of more educated refugees to areas with greater financial literacy nor to the choice of financial literacy concept. Then, we vary the likelihood of interactions between neigbhors and refugees. Effects are operative in areas where Swedes are more positively predisposed to immigrants, where there is a critical mass of knowledgeable neighbors, and for refugees who initially had children, and thus more impetus to interact.

All in all, our findings provide considerable evidence of influences from financially knowledgeable neighbors on the financial behavior of households that were placed next to them, and support for a mechanism that involves transmission of knowledge rather than imitation. Yet, our results have nuanced implications for the spread of financial knowledge. Financial literacy externalities are operative when both sides have the ability to process content, and are willing and able to interact in this dimension. The spread of financial knowledge is unlikely to be automatic or homogeneous, and is likely to be most limited for people with low education and limited opportunities to interact with knowledgeable peers. Campaigns to spread financial knowledge need to focus not only on generating content, but also on its distribution where it is needed most.

Finally, the focus on refugees 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 |
| Share of neighbors with stockholding | 36,513 | 0.113 | 0.086 | 20,303 | 0.113 | 0.086 | 20,303 | 0.113 | 0.086 |
| Share of neighbors with stockholding but no economics/business education | 36,513 | 0.11 | 0.08 | 20,303 | 0.11 | 0.08 | 20,303 | 0.11 | 0.08 |
| Share of neighbors with economics/business education but no stockholdings | 36,513 | 0.015 | 0.02 | 20,303 | 0.015 | 0.02 | 20,303 | 0.015 | 0.02 |
| Share of neighbors with economics/business education and stockholdings | 36,513 | 0.008 | 0.02 | 20,303 | 0.008 | 0.02 | 20,303 | 0.008 | 0.02 |
| *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: 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) |
| Disposable Income (IHS) | 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* | *36,513* | *36,513* | 34,354 | 35,185 |
| *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 III: 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) |
| Disposable Income (IHS) | 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* | 20,303 | 20,303 | 16,210 | 16,210 |
| *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). 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 IV: A Pure Imitation Effect? Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term - Breakdown of the Neighbors' Financial Literacy

| | Stockholding | | | | |
|---|---|---|---|---|---|
| *Panel A: Medium-Term* | (i) | (ii) | (iii) | (iv) | (v) |
| Initial Share of Stockowners | -0.0746 | -0.15790 | | | |
| | (0.1131) | (0.1141) | | | |
| Initial Fin Lit Ext (Base) | | 0.82091*** | | | |
| | | (0.3070) | | | |
| Initial Fin Lit Ext (Alternative I) | | | 0.93198*** | | |
| | | | (0.3591) | | |
| Initial Fin Lit Ext (Alternative II) | | | | 0.32099 | |
| | | | | (0.4988) | |
| Initial Fin Lit Ext (Alternative III) | | | | | -0.10552 |
| | | | | | (0.1186) |
| *Observations* | 20,303 | 20,303 | 20,303 | 20,303 | 20,303 |
| *Panel B: Longer-Term* | (i) | (ii) | (iii) | (iv) | (v) |
| Initial Share of Stockowners | -0.03211 | -0.16538 | | | |
| | (0.1152) | (0.1187) | | | |
| Initial Fin Lit Ext (Base) | | 1.31871*** | | | |
| | | (0.3169) | | | |
| Initial Fin Lit Ext (Alternative I) | | | 1.74087*** | | |
| | | | (0.3753) | | |
| Initial Fin Lit Ext (Alternative II) | | | | 0.2449 | |
| | | | | (0.5327) | |
| Initial Fin Lit Ext (Alternative III) | | | | | -0.05158 |
| | | | | | (0.1216) |
| *Observations* | 16,210 | 16,210 | 16,210 | 16,210 | 16,210 |
| *Household Controls* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Clustering* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* |
| *Time Fixed Effects* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Country-of-Origin Fixed Effects* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Arrival-year Fixed Effects* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Neighborhood Fixed Effects* | *Parish* | *Parish* | *Parish* | *Parish* | *Parish* |
| *Industry Composition of the Initial Elec. Dist.* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Time-varying Initial Elec. Dist. Controls* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |

*Note:* This table presents coefficient estimates from linear probability models of participation in stockholding (direct or indirect) for various sample periods: 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. 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. In (ii), we use our base financial literacy measure, (iii) focuses the neighbors with business/economics education without any stock investments, (iv) considers neighbors with both business/economics education and stock ownership, and (v) considers on the neighbors with stock ownerhsip but no education education in business/economics. 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: A Pure Imitation Effect? Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term - Cross-Asset Effects considering the Initial Share of Stockowners

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|---|---|---|---|---|---|---|
| *Panel A: Medium-Term* | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Initial Share of Stockowners | 0.13172 | -0.07460 | | | 0.12651 | -0.08245 |
| | (0.1017) | (0.1131) | | | (0.1019) | (0.1127) |
| Initial Fin Lit Ext (Alternative I) | | | 0.63188* | 0.93198*** | 0.62100* | 0.93904*** |
| | | | (0.3601) | (0.3591) | (0.3593) | (0.3584) |
| *Observations* | 20,303 | 20,303 | 20,303 | 20,303 | 20,303 | 20,303 |
| *Panel B: Longer-Term* | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Initial Share of Stockowners | 0.12798 | -0.03211 | | | 0.12263 | -0.04613 |
| | (0.1249) | (0.1152) | | | (0.1256) | (0.1160) |
| Initial Fin Lit Ext (Alternative I) | | | 0.66295* | 1.74087*** | 0.65266 | 1.74467*** |
| | | | (0.3985) | (0.3753) | (0.3991) | (0.3758) |
| *Observations* | 16,210 | 16,210 | 16,210 | 16,210 | 16,210 | 16,210 |
| *Household Controls* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Clustering* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* |
| *Time Fixed Effects* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Country-of-Origin FEs* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Arrival-year FEs* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Neighborhood FEs* | *Parish* | *Parish* | *Parish* | *Parish* | *Parish* | *Parish* |
| *Industry Composition of the Initial Elec. Dist.* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Time-varying Initial Elec. Dist. Controls* | *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 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. *Initial Fin Lit Ext (Alternative I)* refers to the share of financially literate neighbors in 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 but no stock investments. 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 VI: A Pure Imitation Effect? Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term - Cross-Asset Effects considering the Initial Share of Retirement Savers

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|---|---|---|---|---|---|---|
| *Panel A: Medium-Term* | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Initial Share of Retirement Savers | 0.19549* | 0.28530** | | | 0.18747* | 0.27658** |
| | (0.1078) | (0.1155) | | | (0.1086) | (0.1152) |
| Initial Fin Lit Ext (Alternative I) | | | 0.78877** | 0.86576** | 0.76390* | 0.82896** |
| | | | (0.3905) | (0.4064) | (0.3901) | (0.4048) |
| *Observations* | 20,303 | 20,303 | 20,303 | 20,303 | 20,303 | 20,303 |
| *Panel B: Longer-Term* | (i) | (ii) | (v) | (vi) | (iii) | (iv) |
| Initial Share of Retirement Savers | 0.26358** | 0.39585*** | | | 0.25517** | 0.38291*** |
| | (0.1214) | (0.1224) | | | (0.1218) | (0.1219) |
| Initial Fin Lit Ext (Alternative I) | | | 0.82192* | 1.27380*** | 0.78754* | 1.22263*** |
| | | | (0.4232) | (0.4218) | (0.4231) | (0.4230) |
| *Observations* | 16,210 | 16,210 | 16,210 | 16,210 | 16,210 | 16,210 |
| *Household Controls* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Clustering* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* | *Electoral District* |
| *Time FEs* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Country-of-Origin FEs* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Arrival-year Fixed FEs* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Neighborhood Fixed FEs* | *Parish* | *Parish* | *Parish* | *Parish* | *Parish* | *Parish* |
| *Industry Composition of the Initial Elec. Dist.* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| *Time-varying Initial Elec. Dist. Controls* | *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 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. *Initial Fin Lit Ext (Alternative I)* refers to the share of financially literate neighbors in 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 but no private retirement saving plans. 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: 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) | (0.2938) |
| *Observations* | 19,342 | 19,342 | 17,671 | 4,061 |
| *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* | 15,697 | 15,697 | 14,377 | - |
| *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 VIII: A Mobility Effect: Cumulative Exposure to Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term - 2SLS Estimates

| | Saving for Retirement | Stockholding |
|---|---|---|
| *Panel A: Medium-Term* | (i) | (ii) |
| Cumulative Exposure to Fin Lit Neighbors | 3.93492** | 5.02125** |
| | (2.0034) | (2.2455) |
| *Observations* | 20,268 | 20,268 |
| *Panel B: Longer-Term* | (i) | (ii) |
| Cumulative Exposure to Fin Lit Neighbors | 4.45941 | 11.42939*** |
| | (2.9861) | (3.7700) |
| *Observations* | 16,187 | 16,187 |
| *Household Controls* | *Yes* | *Yes* |
| *Clustering* | *Electoral District* | *Electoral District* |
| *Time Fixed Effects* | *Yes* | *Yes* |
| *Country-of-Origin Fixed Effects* | *Yes* | *Yes* |
| *Arrival-year Fixed Effects* | *Yes* | *Yes* |
| *Neighborhood Fixed Effects* | *Parish* | *Parish* |
| *Industry Composition of the Initial Elec. Dist.* | *Yes* | *Yes* |
| *Time-varying Initial Elec. Dist. Controls* | *Yes* | *Yes* |

*Note:* This table presents coefficient estimates from the second stage of 2SLS regressions 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). 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. *Cumulative Exposure to Fin Lit Neighbors* refers to the weighted share of financially literate neighbors in each electoral district by the length of time spent in that location between entry and the time of observation of financial behavior. Note that we exclude the share of financially literate neighbors in the initial electoral district and used it as an excluded instrument in the first stage regressions. We define *Fin Lit Neighbors* as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics 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 IX: 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* | 14,392 | 14,392 | 5,911 | 5,911 |
| *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* | 11,936 | 11,936 | 4,274 | 4,274 |
| *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 X: Sample Split By Having Children: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

| | With Children | | No Children | |
|---|---|---|---|---|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| *Panel A: Medium-Term* | (i) | (ii) | (iii) | (iv) |
| Initial Fin Lit Ext | 0.71020* | 1.03403*** | 0.28094 | 0.23583 |
| | (0.3999) | (0.3997) | (0.4445) | (0.4481) |
| *Observations* | 11,029 | 11,029 | 9,274 | 9,274 |
| *Panel B: Longer-Term* | (i) | (ii) | (iii) | (iv) |
| Initial Fin Lit Ext | 0.67203 | 1.52950*** | 0.46594 | 0.54924 |
| | (0.4251) | (0.4084) | (0.4898) | (0.4600) |
| *Observations* | 8,809 | 8,809 | 7,401 | 7,401 |
| *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 having children at the initial time of allocation. 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 XI: Varying the Intensity of Interactions: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Full Observation Sample - Size of Knowledgeable Neighbors

| | Saving for Retirement | Stockholding |
|---|---|---|
| | (i) | (ii) |
| I{Q2≥Initial Fin Lit Ext>Q1} | 0.00613 | -0.01112 |
| | (0.0202) | (0.0232) |
| I{Q3≥Initial Fin Lit Ext>Q2} | 0.02251 | -0.01001 |
| | (0.0168) | (0.0177) |
| I{Initial Fin Lit Ext≥Q3} | 0.03276* | 0.04665** |
| | (0.0190) | (0.0218) |
| *Observations* | 36,513 | 36,513 |
| *Household Controls* | *Yes* | *Yes* |
| *Clustering* | *Electoral District* | *Electoral District* |
| *Time Fixed Effects* | *Yes* | *Yes* |
| *Country-of-Origin Fixed Effects* | *Yes* | *Yes* |
| *Arrival-year Fixed Effects* | *Yes* | *Yes* |
| *Neighborhood Fixed Effects* | *Parish* | *Parish* |
| *Industry Composition of the Initial Elec. Dist.* | *Yes* | *Yes* |
| *Time-varying Initial Elec. Dist. Controls* | *Yes* | *Yes* |

*Note:* This table presents coefficient estimates from LPM regressions of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for the full sample period (1999-2007). 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. We define *Initial Fin Lit Ext* as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics and at least some college attendance. We create and use 4 indicator variables, one for each quartile of the distribution of shares of financially literate neighbors in the initial neighborhood, and introduce those instead of the share of financially literate neighbors. 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 XII: From Participation to Market Behavior: Long Shadow Effects of Having Financially Literate Neighbors: Full Observation Sample

| | Economics/Business Education | | Quantitative Education | |
|---|---|---|---|---|
| | Inertia | Underdiversification | Inertia | Underdiversification |
| | (i) | (ii) | (iii) | (iv) |
| Initial Fin Lit Ext | -0.45616** | -0.04706 | -0.35666*** | -0.67946** |
| | (0.2059) | (0.6161) | (0.1338) | (0.3353) |
| No of Obs | 5,531 | 5,531 | 5,531 | 5,531 |
| *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 OLS regressions of portfolio inertia and underdiversification for the full sample period (1999-2007). Portfolio inertia is defined as the absolute value of the changes in the risky share of a household's portfolio. Underdiversification is measured as the share of direct stocks in the equity portfolio. 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. We define *Initial Fin Lit Ext* as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics and at least some college attendance and and quantitative education and at least some college attendance in (i)-(ii) and (iii)-(iv), respectively. 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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