

Financial Literacy Externalities*

MICHAEL HALIASSOS, THOMAS JANSSON, and YIGITCAN KARABULUT†

November 15, 2016

Abstract

This paper uses unique administrative data and a quasi-field experiment of exogenous allocation of refugees in Sweden to estimate effects of exposure to financially literate neighbors on household financial behavior. The paper contributes evidence of a causal impact of financial literacy on behavior and points to a social multiplier of financial education initiatives. Exposure promotes saving for retirement in the medium run and stockholding in the longer run, more so when neighbors have economics or business education. Significant effects are only observed on educated or male-headed households. Findings point strongly to transfer of knowledge rather than mere imitation. They are relevant both for financial education and for refugee placement policies.

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

*We are grateful to Dionissi Aliprantis, Dimitris Christelis, Arthur Kennickell, Theresa Kuchler, Ekaterini Kyriazidou, and Johannes Stroebel for helpful comments and suggestions. We would like to thank conference participants in the 2016 meetings of the International Association of Applied Econometrics and the 2016 C.R.E.T.E. conference, members of the Eurozone Household Finances and Consumption Network, and seminar participants at the Joint Research Committee of the European Commission for valuable comments and suggestions. The views described in this paper are the authors' own and do not necessarily reflect those of the Sveriges Riksbank. Haliassos acknowledges support from the German Research Foundation (DFG).

†Haliassos: Goethe University Frankfurt, CEPR, and Netspar; Jansson: Sveriges Riksbank; and Yigitcan Karabulut: Rotterdam School of Management, Erasmus University and CEPR.

1 Introduction

Confronted with the demographic transition and rapid financial innovation, households need to make complicated financial choices with profound and lasting consequences on their economic well being. Research on financial literacy, which has developed rapidly over the past decade, has established the widespread presence of financial illiteracy across countries at various stages of economic development, as well as a strong correlation between financial literacy and positive financial outcomes at the household level (see Lusardi and Mitchell (2014) for the state of the art). Establishing causality from financial literacy to economic outcomes has been more challenging but of profound importance for policy choices and priorities in the presence of competing approaches to enabling household financial decisions. These include financial regulation, financial advice, and default options that come into effect if the household takes no action.

Knowing whether and to what extent financial education programs should be part of this arsenal depends crucially on identifying a causal link between financial literacy and good financial outcomes for the household. Yet unobserved factors, including but not limited to cognitive abilities, can lead an individual both to become financially literate and to have good financial outcomes, without a direct causal link between the two. Furthermore, the observed correlation between financial literacy and outcomes may be due to reverse causality, e.g., from retirement saving to learning about financial concepts. In either case, the observed correlation would not imply that promoting financial literacy can be expected to improve financial outcomes for households.

Rather than focusing on the role that own financial literacy can play for household financial outcomes, this paper is, to the best of our knowledge, the first to study financial literacy externalities, i.e., the potential for financially literate neighbors to have an exogenous (positive) influence on financial choices of households. If the presence of such externalities is established, it can constitute a powerful argument for the relevance of financial literacy for economic well being, strengthen the case for introducing effective financial education pro-

grams, and imply wider reach and greater cost effectiveness as a result of social multiplier effects.¹

Financial literacy externalities reinforce and extend the notion of human capital externalities, in the spirit of Acemoglu (1996) and Acemoglu and Angrist (2001), as motivations for suitable educational programs but in this case with financial content. Beyond financial literacy, examination of such externalities has implications for two other budding literatures, related to peer effects on financial behavior, and to financial outcomes of immigrants and refugees. The former is relevant for the spread of (possibly flawed) financial behavior across broad segments of the population, while the latter has relevance for little appreciated aspects of refugee policies in the ongoing crisis.

The literature on (own) financial literacy has sought to overcome the econometric problems of reverse causality and unobserved heterogeneity through instrumental variable estimation. Researchers have been inventive in devising instruments for own financial literacy, either by going back to early life events or by looking at environmental factors.² An important concern is the potential influence of early life or environmental conditions on shaping financial behavior of the individual through unobserved channels, beyond the various included controls.

When studying financial literacy externalities, the identification issue is shifted to dealing with endogenous sorting of financially sophisticated households into neighborhoods with other sophisticated people, as this could induce a positive correlation between financial outcomes and neighborhood financial literacy without a causal influence of the latter. This problem, however, is overcome if respondents have been randomly allocated to specific ar-

¹This last factor is relevant even if thorough financial education programs are only possible and effective at younger ages, as their effects could spread to a wider range of the life cycle.

²Instruments that relate to early-life conditions 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). There has been limited experimentation with environmental factors as instruments for own financial literacy: political beliefs in the environment of the respondent have been used on the assumption that more right wing beliefs are associated with greater financial knowledge (Bucher-Koenen and Lusardi, 2011).

eas. With exogenous placement, the initial share of financially literate neighbors is not a matter of choice of the respondent and can be used as an exogenous control. Moreover, other potentially unobserved features of the broader initial environment of the respondent can be controlled for through fixed effects.

In this paper, we exploit a rare natural experiment, namely a Swedish policy of exogenously allocating refugees to parishes and apartments, and a unique data set tracking refugees and characteristics of their neighborhoods over twenty years, as well as details of their financial behavior ten to twenty years after entry. Specifically, refugees who moved to Sweden for reasons other than family reunification, mainly between 1987 and 1991, were subjected to a settlement policy, under which the Swedish Immigration Board (quasi-) randomly assigned refugees to an initial parish and apartment within that parish. Once the residence permit of an asylum seeker was approved by the immigration authorities, the municipal officers randomly allocated refugee immigrants to apartments based on characteristics observable to the officers and included in our dataset. It is important to note that there were no face-to-face meetings between placement officers and refugees, so that assignment of a refugee cannot have been based on unobserved individual characteristics beyond the observable ones included as controls.³

We use our detailed data set to study whether household financial behavior is influenced by the share of financially literate people in the neighborhood (electoral district) of the respondent or by the share of those saving for retirement, after controlling for a range of respondent characteristics, unobserved features of the parish of initial placement (a broader area than the electoral district), as well as unobserved heterogeneity of respondents.⁴ We

³For details about the policy experiment, see Edin et al. (2003) pp. 333-335. Edin et al. (2003) and Åslund et al. (2011) use this natural experiment to study different issues, namely the consequences of living in enclaves for labor market outcomes and to what extent immigrant school performance is affected by the characteristics of neighborhoods in which they grew up, respectively.

⁴Other relevant features of the shared environment include the quality of public amenities and the penetration of the financial sector in a given neighborhood (Oreopoulos, 2003; Manski, 1993). As noted by Manski (1993), these ‘correlated’ effects are not social effects, and are not created by social interactions (Damm and Dustmann, 2014). It is thus important to provide an estimate of the effect of financial literacy externalities on individual behavior that controls for the presence of unobserved environmental factors beyond the share of financially literate neighbors. Controlling for the presence of such factors over the broader area of the

consider both medium- and longer-run effects on refugee financial behavior. The effect of financial literacy of neighbors on the respondents' financial behavior is potentially a function of the likelihood of interaction and of the content conveyed by neighbors to respondents, which in turn depends on the knowledge neighbors have, on the ability of refugees to absorb, and their confidence to make use of that knowledge. We identify the effect of financial literacy externalities and explore the relevance of such factors in making them operative.

We find more sizeable effects of the share of neighbors with specialized knowledge of economics or business rather than of those with general ability to deal with quantitative topics. As we start observing financial behavior of refugees about ten years after they are granted asylum and we follow them until the end of twenty years, we consider whether effects of financial literacy externalities due to the exogenous initial allocation of refugees are present over the medium run and over the longer run. We find medium-run effects on participation in retirement accounts and longer-run effects on the more information-intensive and initially less salient asset, namely stocks.

We also consider the effect of financial behavior of neighbors, and in particular the share of neighbors who actively contribute to retirement saving on private accounts. We find that this share has significant, though smaller, effects on refugee financial behavior than having financially literate neighbors, suggesting that imitation is less important than transfer of relevant knowledge.⁵

Importantly, we find that externalities are operative only for the more educated refugees and for male-headed households. In the baseline runs, we focus on financially literate neighbors who are Swedes or immigrants with more than twenty years in Sweden. When we

parish rather than the electoral district is both plausible and useful in identifying such environmental effects separately from the share of financially literate neighbors defined over the electoral district.

⁵Observation could lead to adjustments of own financial behavior without information flows through the perception of norms (whether neighbors take financial risks or save for retirement), consistent with models of conformity (Bernheim, 1994): as individuals lack information, they mimic the behavior of peers. This need not be passive mimicking but inference that neighbors have knowledge that warrants such actions. Beyond rational inference, social comparison considerations, such as "role model" effects, may be present: an individual makes decisions based not just on his/her own preferences but also on whether his/her decisions deviate from choices made by others in the reference group (Glaeser and Scheinkman, 2001; Akerlof, 1997).

expand the relevant circle of neighbors to include more recent immigrants, i.e., those who have spent between ten and twenty years in Sweden, we find opposite effects of the additional neighbors on participation in the two assets. Our findings suggest that financial literacy externalities involve the transfer, processing, and salience of information, as well as confidence to apply it, rather than mere imitation of the actions of financially literate neighbors.

Our paper links to three strands of literature. Financial literacy and its relevance for financial behavior have been actively studied for more than a decade. Following the seminal paper of Lusardi and Mitchell (2007), several papers established widespread financial illiteracy in different countries, with regard even to simple concepts such as interest compounding, real versus nominal interest rates, and diversification. Concentration of financial illiteracy among certain demographic groups, as well as strong correlation between financial literacy and financial outcomes have been shown. These 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.⁶ There is important recent progress on the issue of the effectiveness of financial education programs at schools.⁷ The second strand of literature deals with effects of social interactions on household financial behavior, which is itself part of a broader literature on a range of economic and social outcomes.⁸ The third

⁶See (van Rooij et al., 2011; Disney and Gathergood, 2013; Lusardi and Mitchell, 2014; Lusardi et al., 2016).

⁷Hospido et al. (2016) found that such programs are effective in improving financial literacy test scores of treated school children. Alan and Ertac (2016) conduct experiments with implementing an educational program in primary schools and find it effective for encouraging school children to exhibit greater patience when making intertemporal choices in incentivized experimental tasks, even three years after the educational program was administered. 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 financial and economics education 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.

⁸In a seminal paper, Duflo and Saez (2002) focused on endogenous effects and 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. Hong et al. (2004) focused on the degree of sociability, as proxied by church attendance, participation in social clubs and similar activities, and they found that more sociable households are more likely to hold stocks. Kaustia and Knüpfer (2012) exploited geographic information and found that the stock market performance of peers influences stock market entry. On the debt side, Georgarakos et al. (2014) found that those who perceive themselves as earning less than the average of their

strand of literature is the study of immigrant financial behavior. Compared to other aspects, such as labor or fertility outcomes, financial behavior of immigrants has received more limited attention, focused on establishing links to culture.⁹

Section 2 describes the data, our sample construction, and our proxies for financial literacy externalities. Section 3 presents the estimation model and features of the placement policy relevant to it, while section 4 reports our findings. Section 5 concludes and presents policy implications for financial education programs and for placement of refugees. The Online Appendix contains variable definitions, robustness exercises, and some information on electoral districts.

2 Data and Measurement of Externalities

2.1 Data and sample construction

The policy experiment regarding the initial allocation of refugee immigrants to different neighborhoods took place between 1985 and 1994 but was most strictly enforced in the 1987 to 1991 period on which we focus. We use STATIV and LINDA databases from 1987 to 2007 to identify the refugee immigrants, characteristics of the neighborhood of each respondent, and the financial behavior of refugee immigrants. Both of these datasets are provided by Statistics Sweden for the observation period from 1985 to 2007.

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

peers are more likely to borrow, to borrow larger amounts, and to worsen their indicators of potential financial distress.

⁹In a pioneering paper, Carroll et al. (1994) examined the role of culture for saving patterns at the individual level, while Guiso et al. (2006) looked at national saving rates. Osili and Paulson (2008) found a link between the degree of investor protection in the country of immigrant origin and the probability of the immigrant to participate in the stock market. Guiso et al. (2004) focused on use of basic financial instruments, such as writing a check or purchasing a share, and found that this is affected by the level of social capital. Guiso et al. (2006) provided evidence that trust is influenced by ethnic origin in US data, while Guiso et al. (2003) found evidence that trust is influenced by religion, both pointing to the relevance of culture. Haliassos et al. (2016) found that financial behavior differs across cultural groups of migrants, controlling for a range of characteristics, but these differences diminish with exposure to host country institutions.

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. The data include detailed information on household assets and debt for the sample period from 1999 to 2007, allowing us to consider effects that arise from exposure to financially literate neighbors as these manifest themselves over the medium and the longer run. We are also able to observe features both of the parish and the electoral district where the refugee was originally assigned.¹⁰

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 refugee 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 1991.¹¹ 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, other extensions, studies, other reasons, as well as refugees who are flagged as having enough living supplies. In other words, we only consider those refugees who are indicated as being in need of protection, or having been admitted for humanitarian reasons, i.e., those who find themselves in a particularly weak situation and present no doubt that they had to comply with the location instructions received by the immigration authorities.

¹⁰There are approximately 5,700 electoral districts in Sweden, typically with 1000 to 2000 people in an electoral district. Information is available at <http://www.scb.se/sv/Hitta-statistik/Regional-statistik-och-kartor/Statistikatlasen/Valen-2010-i-interaktiv-kartform/>

¹¹See also Edin et al. (2003).

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 II. 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.

2.2 Proxying for financial literacy externalities

Our basic premise is that individuals have the greatest scope for interaction with people in their immediate environment, here being proxied by their electoral district. The potential of refugees for improving their own financial literacy through such interaction is assumed to be an increasing function of the (log) share of financially literate people living in their electoral district j . This is because, other things equal, their greater presence raises the probability of interaction of a refugee with somebody who has relevant information to convey

for household financial decisions.

We consider the "long shadow" of the share of financially literate neighbors in the initial electoral district of placement. Because of exogenous placement to that district, which is not based on refugee characteristics we do not observe in the data, we can uncover causal effects with OLS or probit estimation without instrumenting.

In addition to presenting effects of financial literacy externalities over the entire period in which we observe financial behavior (1999-2007), we are also able to distinguish between "medium-run" effects of financial literacy externalities, namely those on financial behavior of refugees in the period 1999-2003, and "longer-run" effects, that focus on financial behavior in the period 2004-2007. The main text presents OLS results from a Linear Probability Model (LPM), while Online Appendix C presents probit estimation results to examine robustness across estimation methods.

An important issue is whom to consider financially literate and thus in a position to generate the externalities relevant for neighbor financial behavior. There is no single way to define financial literacy, and different researchers and organizations have done so in different ways.¹² The most widely adopted definition of financial literacy focuses on knowledge of basic financial concepts and familiarity with the economic environment.

We consider three alternative measures of financial literacy among neighbors and examine robustness of our findings and relevant nuances of each alternative. Our benchmark measure of financial literacy among neighbors refers to the share of neighbors in the electoral district, defined as those who have business/economics education and have attended college. To make sure that theoretical knowledge is combined with knowledge of Swedish institutions, we exclude from the set of relevant neighbors in the base runs migrants who have less than 20 years in Sweden.¹³

An alternative measure of financial literacy we consider is the share of electoral district

¹²For an overview, see Lusardi (2008) and Lusardi and Mitchell (2007).

¹³In section 4.3 we also consider a broader set of relevant neighbors, which only excludes migrants who have spent less than 10 years in Sweden.

neighbors who have a quantitative educational background, regardless of whether they have been trained in economics and finance. A quantitative background typically facilitates the processing of information relevant for financial behavior and could also yield useful externalities. We test for the presence of such externalities and compare their magnitude to those generated by neighbors with business and economics education.

While the first two measures emphasize educational background, a third measure takes a different viewpoint of financial literacy externalities, namely that of neighbors providing an example that respondents can follow, whether or not they fully understand the information that led to this financial behavior. Specifically, this measure focuses on the share of electoral district neighbors who contribute to private retirement saving plans. It potentially encompasses cases where the respondent simply mimics the neighbor who holds retirement products, those where the respondent discusses with the neighbor and gains knowledge relevant for saving for retirement, as well as intermediate cases where observing that the neighbor holds retirement products is not mindless imitation but is seen by the respondent as an informative signal about the usefulness of these products.

Beyond testing for the presence of a causal influence of the share of financially literate neighbors on financial behavior over the medium and the longer term, we probe further to uncover operative links. We perform two sample splits - by education and by gender - and examine the size and significance of the coefficients on the financially literate share in each pair of subsamples. The nature of the allocation process performed by immigration officials, focused as it was on education, language, and family size, could a priori result in differences across subsamples in exposure to externalities, and these might confound results on the operativeness of different channels per se.¹⁴ Table III 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. For each sample

¹⁴As reported in Edin et al. (2003), the governing criteria for allocation of refugees to apartments were language, formal qualifications, and family size. Preference was given to highly educated individuals and to those who had common language with some members of the resident immigrants. Single individuals were more difficult to place, given the smaller availability of apartments for single occupancy.

splitting criterion, the two subsamples exhibit comparable average exposure to financially literate neighbors, as well as comparable variation in this exposure.

We also explore financial literacy externalities for different definitions of the relevant neighbor circle. In benchmark runs, we restrict our measure of financially literate neighbors to those who are native Swedes or immigrants with more than twenty years in Sweden. In Online Appendix B, we include all immigrants who have been at least ten years in Sweden. As we consider different groups, we essentially vary the likely intensity of interaction between refugees and their neighbors, while we ensure that neighbors have the necessary practical knowledge of the Swedish financial system by not considering immigrants with less than ten years of stay in the country.

3 Estimation

In what follows, we model two aspects of financial behavior, namely the participation in risky and information-intensive financial instruments, i.e., stocks, and saving for retirement. In modeling these decisions, we control for a sizeable number of observable household characteristics, introduce a number of fixed effects, and random effects depending on the estimation method, and estimate the impact of measured exposure to financial literacy externalities in the initial neighborhood of assignment (electoral district). We first consider the features of the quasi-field experiment that are most relevant for the estimation model, and we then describe the model.

3.1 Relevant features of the refugee placement policy

Our causal analysis of the effect of financially literate neighbors in the initial (electoral) district of placement on subsequent asset participation of refugees relies on the assumption that, given the observed characteristics of the refugee, the characteristics of initial location are independent of unobserved individual characteristics determining the probability

of saving for retirement or holding stocks. The way in which the refugee placement program assigned refugees to particular apartments is relevant for the validity of this assumption.¹⁵

In 1985, the Swedish Immigration Board was assigned the task of placing refugees in particular apartments, in response to complaints from certain municipalities that they were bearing disproportionate burdens of absorbing immigrants in the 1980s. Almost all municipalities participated in the program, which went on until 1994 but with strictest implementation during the period 1987-91, on which we focus. Given this motivating concern, the placement program did not assign refugees to apartments based on some lottery but mainly on the basis of availability of public housing (a suitable apartment). Program officers also took into account the education level of the refugee, whether others speaking the same language existed in the area of placement under consideration, and whether the refugee was married or single, with the latter being more difficult to place in view of limited availability of small apartments. Refugees were also asked to state their preferences. There was no personal interview with the refugee. Placement occurred shortly after the refugee obtained a residence permit.

In our estimation, we are able to control for all relevant observable characteristics of the refugees that may have influenced their initial assignment. How could unobserved refugee characteristics enter the determination of initial placement? One channel might be provision of information to the placement officers outside what is recorded in the data. Since there was no interview and no further contact between the officers and the refugees, this channel can plausibly be ruled out.

Another possibility is that the statement of refugee preferences influenced placement, and these preferences were themselves influenced by unobserved factors also relevant for asset participation. 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

¹⁵The placement program is described extensively in two papers, Edin et al. (2003) and Åslund et al. (2011), on which this subsection draws. Our use of STATIV allows us to identify precisely the refugees among migrants to Sweden in the relevant period.

of an apartment and not the 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. Further evidence that refugee preferences hardly influenced the outcome is that 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 without paying short-term costs (such as being able to enrol in language classes). All in all, it seems that the assumption that the characteristics of initial location are independent of unobserved individual characteristics determining the probability of saving for retirement or holding stocks seems warranted.

3.2 The estimation model

In our benchmark regression (1), we estimate the long shadow of the effect of financial literacy in the original electoral district over the medium and over the longer run, controlling for unobserved characteristics of that original electoral district. Given the panel nature of the dataset, we account for various fixed effects, as well as for household unobserved heterogeneity:

$$Y_{ikt} = \alpha \cdot X_{ikt} + \beta \cdot FLShare_{l0} + \gamma_{j0} + \gamma_k + \gamma_0 + \gamma_t + \epsilon_{ikt} \quad (1)$$

where Y_{ikt} refers to the relevant aspect of financial behavior observed for household i from country of origin k in period t over the horizon of interest (full period, medium run, or longer run) and $FLShare$ the share of financially literate neighbors that household i had in its initial electoral district, $l0$. For our medium run analysis, the observation years are $t = 1999, \dots, 2003$, while for the longer-run analysis, the corresponding years are $t = 2004, \dots, 2007$. The full period comprises both sets of years.

We are able to control for a wide array of observable characteristics, denoted by X . These

include disposable labor income (in logs), 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.

We introduce fixed effects for the parish of initial placement, γ_{j0} , where the initial year 0 was not the same for all refugees and it runs between 1987 and 1991; the country of origin, γ_k , the year of arrival, γ_0 , and the year of observation, γ_t . We also correct the standard errors by clustering at the initial electoral district level. Parish fixed effects should control for the possibility that a positive coefficient on the externalities variable reflects, at least in part, exposure to common factors in the greater neighborhood. For example, a higher share of financially literate neighbors in the electoral district may reflect characteristics of the initial parish (e.g., a larger number of banks or other financial institutions, or greater neighborhood ambience that attracts sophisticated individuals to the broader area of the parish), and these unobserved parish-specific factors tend to generate both a more knowledgeable base of neighbors in the electoral district and better financial outcomes for the refugees in that district. This is an instance of 'correlated effects', where the parish environment influences positively both the quality of (electoral-district) neighbors and refugee financial behavior, without a direct link between the two. Initial 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.

4 Externalities from Financially Literate Neighbors

We begin our analysis by focusing on effects that the share of neighbors with college education and a business or economics background has on refugee financial behavior. We estimate the effect of the share observed in the initial electoral district of exogenous placement by the immigration authorities, controlling for refugee characteristics, some of which might have influenced that placement, as well as for supply-side factors in the greater area of the parish, macroeconomic and other year-specific factors in the year of arrival and in that of observation, and considerations that might be specific to refugees from the particular country of origin. We consider behavior over different runs, the nature of operative links, and the role of the likely intensity of interaction. We present results from the Linear Probability Model in the main text, without and with random effects that control for unobserved heterogeneity. Online Appendix C presents corresponding probit estimates of average marginal effects for robustness.

4.1 Presence and time dimension of effects

Table IV presents the estimation results for the full set of years during which financial behavior is observed, 1999-2007, before we distinguish between the "medium run" effects on financial behavior in the period 1999-2003, and the "longer run" effects in the period 2004-2007. Columns (iii) and (iv) differ from (i) and (ii) in that they also control for unobserved heterogeneity through random effects. 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 positive effect both on the tendency to save for retirement and on the tendency to hold stocks. This positive effect is present, controlling for a wide range of household characteristics as well as for the fixed effects described above, which include unobserved relevant features of the parish of initial placement.

Regardless of whether we control for household heterogeneity through random effects

estimation or not, we find somewhat larger estimated effects and greater statistical significance for the probability of holding stocks than for the probability of saving for retirement. 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.

These results are consistent with initial placement casting a long shadow on the evolution of subsequent financial behavior of the refugee, even controlling for how the refugee's income, wealth, education, marital status and household size develop in the host country. This suggests that the quality of the initial placement of refugees (in our case, with regard to financial literacy of neighbors) matters for subsequent financial behavior, roughly between 10 and 20 years after the time of entry.

The sign and statistical significance of other controls is mostly consistent with what has been found in household finance regressions for these instruments 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, having a household head that works in the financial sector ceases to be significant for good financial outcomes when financial literacy externalities in the neighborhood are included in the regression. What seems to matter is exposure to financially literate neighbors rather than working in the financial sector per se. Having a larger number of children is negatively associated with saving for retirement, although estimates are not consistently significant. The sign of the relationship of the number of children to the tendency to hold stocks switches once we allow for unobserved heterogeneity through random effects.¹⁶

¹⁶The estimated sign is typically negative or insignificant in cross-sectional household finance regressions that do not allow for financial literacy externalities. Here we obtain a significant positive relationship when we allow for unobserved heterogeneity through random effects. These sign reversals highlight the multidimensional role that the number of children plays in the decision to participate in the stock market. On the one hand, for given resources, a larger number of children implies larger costs and greater committed expenditures that leave fewer resources for stockholding. On the other hand, a larger number of children may also provide

Tables V and VI distinguish between effects of financial literacy externalities in the initial neighborhood over the medium run (1999-2003) and over the longer run (2004-2007), respectively. Separating the two "runs" allows not only the effect of financial literacy externalities but also the relationship of participation probability to household characteristics and other factors to differ across the two periods of observation of financial behavior, rather than forcing them to be the same.

An interesting pattern emerges. When considering only the medium run from the initial placement (Table V), we do find positive coefficient estimates for participation in both assets, and somewhat higher ones when we control for unobserved heterogeneity through random effects. However, these effects of initial exposure to financially literate neighbors are only statistically significant (at the 10 percent level) for the simpler of the two assets, namely retirement saving, and not for the riskier and more informationally intensive stocks. When we consider the longer run effects of financial literacy externalities (Table VI), we find strongly statistically significant and quite sizeable effects on participation in stocks, but no remaining significant effects on retirement saving. Controlling for unobserved heterogeneity, a 10 percent increase in the share of neighbors with economics or business education in the electoral district of initial placement is estimated to add 10.4 percentage points to the probability that the household participates in the stock market over a horizon of 15 to 20 years. Interestingly, the pattern of signs and significance of the other household controls remains the same as in the regression spanning all years of observed financial behavior.¹⁷

We will further examine this change 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

a greater impetus to seek the wealth-generating potential of the equity premium and a more diversified safety net to parents facing stockholding risks.

¹⁷Table O.A.18 in Online Appendix C 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 are very close numerically to the corresponding estimates from the linear probability model and with the same pattern of statistical significance, confirming robustness to the estimation method used.

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 matures.

4.2 Exploring the links: education, gender, and content

In this section, we try to shed light on the underlying mechanism by investigating the strength of financial literacy externalities for different subsamples. We consider two important sample splits, by education and by gender, and we compare the coefficients on the exposure to financially literate neighbors across each pair of subsamples.¹⁸

Table VII shows that the effect of financial literacy externalities is present only for the more educated subsample, namely those whose heads have high school education or more, but not for those with less than high school education. This is true regardless of whether we examine medium- or longer-run effects. Interestingly, we find that refugees with high-school education or more benefit from financially literate neighbors in both asset participation decisions, retirement saving and stockholding, but the effect on stock market participation increases in estimated size and significance level as we move from the medium to the longer run. The estimated effect on retirement saving remains roughly the same, and it is only significant at the 10 percent level in the longer run. Given the absence of sorting, depicted in Table III above, these findings on the education split suggest that higher educational attainment is crucial if people are to benefit from financially literate neighbors. Higher educational attainment normally provides people with increased ability to ask the right questions and evaluate the information they obtain from others. More educated people also tend to be more likely to become interested in acquiring such information, as stocks and private retirement plans are more salient to them in the course of their daily occupation.

Are financial literacy externalities important in influencing participation behavior of both female- and male-headed households? The question does not have an a priori obvious

¹⁸Obviously, 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.

answer in light of existing research in household finance. On the one hand, existing literature on financial literacy draws attention to more limited financial literacy among women and lower self-confidence in dealing with finances. These are evidenced by lower scores of women in financial literacy tests, and by greater tendency to opt for the "Don't Know" answer and to give a wrong answer if they attempt the question (Lusardi and Mitchell, 2014; Bucher-Koenen et al., 2016). On the other hand, there is considerable evidence that males are more likely to exhibit overconfidence with respect to stock trading (Barber and Odean, 2001) and to be less willing to get or accept financial advice (Hackethal et al., 2012). In principle, overconfidence and more limited willingness of males to consult with others may limit the scope for financial literacy externalities to affect financial behavior of males, so that such externalities can contribute to mitigating the gender gap found in the literature so far. However, this is not what we find.

When we split the refugee sample by gender (Table VIII), we find that financial literacy externalities are operative for households headed by males but fail to have statistically significant effects on participation of females. Female-headed households who find themselves in a neighborhood with a larger share of economics or business-educated neighbors are not systematically influenced by them in their saving for retirement or stock market participation decisions, either over the medium- or over the longer run. Male-headed households, on the other hand, are more likely to participate in either asset over the medium run when they have been exposed to a greater share of financially literate neighbors, and the effect persists for stocks and becomes larger over the longer run. Thus, instead of financial literacy externalities providing a mitigating mechanism, they can actually contribute to the gender disparity in asset market participation.

Our findings on the education and gender sample splits are consistent with the view that financial literacy of neighbors influences household choices through transfer of knowledge and information that needs to be received, processed, understood, and acted upon by the household in question. In view of existing literature on participation, our findings point

to the conclusion that ability to process new financial information from neighbors and self-confidence to act upon it, both of which have been found to be larger for more educated and for male-headed households, tend to overcome relative unwillingness to consult others because of overconfidence, thus making financial literacy externalities operative.

Further insights into the channel of transmission can be obtained by varying the potential content of financial literacy externalities. Instead of considering neighbors with business or economics education as potential sources of such externalities, we next consider a broader set that includes all neighbors with quantitative degrees. Summary results for the full observation sample, the medium run, and the longer run are reported in Table IX, while full results are reported in the Online Appendix (Tables O.A.1, O.A.2, and O.A.3, respectively).

When considering this broader group of neighbors, with ability to process quantitative information but no specialized knowledge of economics or business, we find smaller corresponding estimated effects of financial literacy among neighbors, regardless of whether we focus on the medium or the longer run effects on financial behavior. We confirm the pattern of significant effects on retirement saving over the medium run and on stockholding over the longer run, but with a smaller size.¹⁹ Moreover, the pattern of results for the sample splits based on education and gender of the household head remains generally the same as for the case of neighbors with economics or business education (see Tables X and XI).²⁰

Our findings with this broader notion of financial literacy among neighbors suggest that financial literacy externalities from neighbors with college-level knowledge of economics or business tend to be larger than those generated by neighbors who have developed capacity for processing quantitative information in general but may not have specialized knowledge relevant for financial behavior. This finding reinforces the view that the process through

¹⁹Taking the observation period as a whole, the regression for the full sample shows very comparable effects of quantitatively educated neighbors on retirement saving and on stockholding participation.’

²⁰The exception is that, unlike in the case of neighbors with business or economics education, quantitatively educated neighbors influence saving for retirement decisions of females rather than of males in the medium run.

which financial literacy externalities operate is not mindless imitation but one in which content and knowledge matter.

4.3 Salience and intensity of interactions

Benchmark estimation so far has proceeded by including in the set of relevant neighbors those who are Swedes or immigrants with more than 20 years of stay in Sweden. In this section, we broaden the neighbor circle over which we measure financial literacy to include also more recent migrants that have stayed in Sweden between 10 and 20 years, and we examine the estimated size and significance of the coefficient on the proxy for financial literacy externalities computed for the new circle under consideration. By expanding the relevant social circle to include more recent immigrants, we increase the probability that the refugee respondent interacts with the neighbors of a given level of financial literacy. Such increase could be a result of simply including more neighbors in the pool, but it can also be reinforced by the likely greater tendency of recent migrants to associate with refugees sharing similar levels of assimilation to the local culture.

Table XII presents summary results, while full estimation results are presented in Online Appendix B (Tables O.A.4, O.A.5, and O.A.6). We find that a given increase in the share of neighbors with college-level economics or business education among this expanded circle of neighbors results in greater estimated increases in the probability of participation in retirement saving, which are even significant for longer-run behavior, but in smaller estimated increases in stock market participation (with similar patterns of significance as in the benchmark). 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 to undertaking sample splits by education and gender and to considering quantitative education as the basis for defining the share of financially literate neighbors.²¹

This robust mixed result is intriguing. If greater intensity of interaction with the rele-

²¹This is illustrated by the summary results on education and gender splits in Tables XIII and XIV, while full results are presented in the Online Appendix (Tables O.A.7 to O.A.11).

vant circle of neighbors were the full explanation, we would expect to observe higher estimated effects on participation in both financial instruments (retirement accounts and stocks). However, we observe that the estimated effect of financial literacy externalities on stocks is now smaller.

This suggests the presence of a second, offsetting factor, and results in earlier work on the Swedish native and migrant population provide an important clue. Haliassos et al. (2016) have shown that, as migrants spend time in the host country, they progressively raise their stock market participation probability to approach that of Swedes, controlling for household characteristics. There is considerable such adjustment between the tenth and the twentieth year of stay in Sweden, and this is the relevant period for the addition to the set of neighbors considered in this section. Thus, a plausible second factor that produces the particular pattern of results for stockholding is the changing salience of stocks, controlling for age and other relevant migrant characteristics, as a function of the time the migrant neighbor has spent in the host country. The expanded set of migrant neighbors we consider have significantly lower stock market participation rates than those more closely assimilated to Swedes, and are therefore less likely to find stocks salient and convey information on stockholding than the benchmark circle of neighbors does.

4.4 Financial choices of neighbors

We have focused so far on a key characteristic of neighbors, educational attainment, and its tendency to encourage participation in financial instruments. Existing literature on financial literacy has provided considerable evidence that those who are more financially literate are also more likely to participate in financial products, such as retirement saving (Lusardi and Mitchell, 2007) or stockholding (van Rooij et al., 2011). The separate literature studying peer effects on financial behavior has delivered strong evidence that the tendency of peers to hold a particular retirement product or to be successful in stockholding tends to make others in the peer group more likely to participate in retirement products (Duflo and

Saez, 2002) or in stocks (Kaustia and Knüpfer, 2012), respectively. Combining these results in our context of financial literacy externalities raises the possibility that the key channel through which such externalities operate is emulation of financial choices of neighbors with no significant role for transfer of knowledge useful for financial decisions. If this is so, then promoting participation in financial products may be left to marketing campaigns without any need for financial education initiatives. In this section, we investigate this possibility.

The literature on peer effects has recognized the potential presence of both types of influence, those arising from underlying characteristics of the peers (termed by Manski “exogenous” or “contextual” effects) and those resulting from emulation of financial choices of peers (termed “endogenous” effects). In their seminal study, Duflo and Saez (2002) investigated whether individuals are directly influenced by the financial choices of their peers, in a setup where participants and their peers were quite homogeneous in characteristics, and participation in only one (retirement) product was studied.²² In our data, we observe participation of electoral-district neighbors in retirement saving, their heterogeneous financial literacy according to the two criteria already examined, as well as participation of the respondents (refugees) in two financial instruments, one of which (stockholding) is distinct from the peer instrument observed (retirement saving).

Table XV presents summary results on the influence of the share of neighbors who were saving for retirement in the initial electoral district of placement on the subsequent tendency of refugees to participate in retirement saving or in stockholding over different time horizons²³. We see that greater presence of retirement savers in the district of initial placement has statistically significant positive effects on refugee participation in both instruments over the longer run, but not in the medium run (where effects are not significant). The estimated size of these effects is generally smaller than the size we estimated for the

²²Duflo and Saez (2002) used data on participation of librarians in each library of a large University in a particular retirement product. While this helped to isolate the effect of peers’ use of the retirement product, it provided little variation in educational characteristics among peers and no other asset to consider.

²³Full estimation results for retirement saving by refugees are in Appendix Table O.A.12 and for stockholding in Table O.A.13.

share of neighbors with economics or business background (Tables IV to VI) and for the share with quantitative background (Table IX). Moreover, the effects of retirement savers in the initial neighborhood take longer to be visible, as they are not significant over the medium run but only in the longer run.²⁴ This comparison implies that a greater share of peers with knowledge relevant for financial decisions had bigger and more immediate effects on refugee financial behavior than did a comparable percentage increase in the share of peers participating in retirement accounts.²⁵

These results support further the hypothesis that financial literacy externalities go beyond imitation and involve the transfer of relevant knowledge, reinforcing other types of evidence presented in our findings above. Additional evidence for this view comes from considering effects on refugee participation in two assets. If the effect of participating in retirement accounts were purely one of imitation, we would expect that it would only or mainly be visible for the asset held by the neighbors. What we find, however, is that it extends to the other asset (stocks), and it is actually estimated to be larger for refugee participation in this more information-intensive asset than for the retirement asset.

Online Appendix B presents results when we broaden the neighborhood circle to include migrants with between 10 and 20 years in Sweden (Tables O.A.16 and O.A.17). We see that the estimated effects of having neighbors who save for retirement in the initial district of exogenous placement are actually estimated to be larger than those for the benchmark circle of relevant neighbors, and even to turn significant in the medium run in the case of retirement saving. These findings are quite intuitive in the context of intensity of interactions and of salience. The expansion of the relevant circle of neighbors to include more recent

²⁴In unreported regressions, we have also included the share of retirement savers together with either the share of neighbors with business or economics education, or the share of those with quantitative education. We found no case (whether for the full sample, the medium run, or longer run behavior) in which the share of retirement savers in the electoral district was significant but the corresponding education share was rendered insignificant.

²⁵When we split the sample by education, we find a statistically significant effect on retirement account participation already in the medium run for the more educated (Table O.A.14). Results on the gender split are mixed, with short-run effects on retirement saving appearing for women and longer-run effects on stockholding only for men (Table O.A.15).

migrants with retirement accounts increases the likely intensity of interactions. Seeing a larger share of retirement savers in this expanded circle signals to the refugees that retirement saving is likely more relevant for them. Interestingly, when we control for unobserved heterogeneity through random effects, the estimated effect on stockholding is larger than that on participation in retirement saving, as for the smaller circle of neighbors described above.

5 Conclusions and Policy Implications

This paper uses unique administrative data on refugees to Sweden and a quasi-field experiment of exogenous allocation of refugees in order to estimate the effect of access to financially literate neighbors on two important aspects of household financial behavior, namely saving for retirement and participation in stockholding. As we can track refugee households over twenty years, we are able to estimate the effects of the exogenous component of exposure to financial literacy externalities over the medium run (ten to fifteen years after initial placement) and the longer run (about fifteen to twenty years). The nature of the experiment and of the data allow us to address thorny causality issues related to "correlated effects" arising from endogenous choice of neighborhood. We focus on the influence of financial literacy of neighbors on respondent financial behavior, which can be central for assessing social multipliers of programs that are effective in raising financial literacy in segments of the population. We also examine the relevance of the share of neighbors who save for retirement for the tendency of respondents to do the same, and (separately) for their tendency to participate in the stock market.

We find evidence of statistically and economically significant effects of financial literacy in the exogenous initial location of refugees on their financial behavior in the medium and in the longer run, controlling for unobserved features of their location and for a number of individual characteristics. Effects on the act of saving for retirement tend to be significant

in the medium run, while effects on stockholding are significant in the longer run.

We next turn to exploring the nature of financial literacy externalities and channels through which they operate. We find evidence that the effects are operative for the subsample of refugees who have at least a high school degree, but not for those with less than high school education. We also find that the effect tends to operate through male- rather than female-headed households. We confirm that these results are not plausibly due to sorting of more educated and male refugees to areas with greater financial literacy.

We then vary the definition of financially literate neighbors to include all those with quantitative education, without requiring economics or business education most pertinent to financial decisions. When we dilute content in this way, we find that the estimated size of effects is reduced across all runs and sample splits. We conclude from these exercises that content and ability to process financially relevant content matter for the transmission and size of financial literacy externalities.

We next expand the circle of relevant neighbors to include migrants who have spent between 10 and 20 years in Sweden, as opposed to only including in the base runs natives and migrants with more than 20 years in the country. This adds to the circle of neighbors migrants who are more likely to be interacting with the refugees and to be closer to their age and stage of assimilation. We find that the estimated size of financial literacy externalities on participation in retirement saving increases, while that on stock market participation decreases; and this holds for any run, sample split, and definition of financial literacy based on background we consider. We interpret this robust combination of effects as arising from two factors. First, greater intensity of interactions is likely to contribute to stronger effects on participation in both assets. Second, we cite evidence that the more recent migrants are less likely to participate in stocks, conditional on their characteristics, than those with longer time in Sweden, and thus less likely to discuss information about stocks with the refugees. The conflict between stronger interaction - which should strengthen effects on both assets - and greater salience of stocks is consistent with the two opposite effects we

observe.

We also consider the possibility that financial literacy externalities operate mainly through imitation of the asset participation behavior of financially literate neighbors, who are more likely to be saving for retirement and to be holding stocks. We do find that the initial share of neighbors participating in retirement saving has a longer-run effect on asset participation of refugees. The effect is, however, smaller than what we found for the share of financially literate neighbors under either definition, and it only becomes operative in the longer run. Moreover, contrary to what one expect in case of pure imitation, the effect of observing neighbors with retirement saving extends to refugee longer-run participation in both assets, and it is actually more pronounced for the other asset (stocks) than the one neighbors are observed to hold (retirement accounts). When we expand the circle of neighbors to include more recent migrants, effects of having more retirement savers in the initial neighborhood increase, as would be expected both from greater likely intensity of interaction between refugees and their neighbors and from greater similarity in the financial products that both find salient.

All in all, our findings suggest the presence of significant financial literacy externalities that extend beyond imitation to the transfer of knowledge relevant for financial behavior, and that are increasing in economics and business content. These externalities are operative for people who have the educational background and confidence to receive and process relevant financial knowledge, potentially widening the gap with those that lack financial literacy and confidence in financial matters.

These findings contribute to the long-time search for convincing evidence of a causal impact of financial literacy on financial behavior and outcomes, and to policy discussions on the usefulness of effective financial education programs. They suggest that programs that improve financial literacy through economics and business education but also through quantitative skills, can have significant social multiplier effects on financial behavior both in the medium and in the longer run. These effects are likely to be operative mostly for

people with the general educational background and confidence needed to process and apply the transmitted knowledge, and to be larger for greater exposure to, and salience of the transmitted knowledge. While the presence of financial literacy externalities is likely to lower the cost of effective financial education initiatives for given effects on financial literacy, their uneven impact is likely to widen disparities in financial literacy that should not be ignored in policy design.

The focus on refugees, albeit for econometric reasons, provides also some conclusions in a very different context, relevant for the ongoing refugee crisis. Our results highlight the medium and longer-term importance of placing refugees in neighborhoods where they can benefit from the knowledge and (financial) literacy of their neighbors. The finding that it is the more educated and financially confident refugees that are likely to benefit from financial literacy externalities does not suggest that including such background considerations in refugee placement is likely to meet with objections from highly educated local communities. Our findings also suggest that the size of financial literacy externalities does not depend only on the share of financially literate neighbors but also on the preconditions for intensive interactions with those neighbors. Placing more educated refugees in more educated neighborhoods and promoting interactions with their new neighbors significantly enhances their tendency to save for the longer run and to participate in information-intensive financial products, contributing funds to the stock market.

References

- Acemoglu, D. (1996). A microfoundation for social increasing returns in human capital accumulation. *The Quarterly Journal of Economics*, 779–804.
- Acemoglu, D. and J. Angrist (2001). How large are human-capital externalities? evidence from compulsory-schooling laws. In *NBER Macroeconomics Annual 2000, Volume 15*, pp. 9–74. MIT Press.
- Akerlof, G. A. (1997). Social distance and social decisions. *Econometrica: Journal of the Econometric Society*, 1005–1027.
- Alan, S. and S. Ertac (2016). Good things come to those who (are taught how to) wait: An educational intervention on time preference. *Working Paper*.
- Åslund, O., P.-A. Edin, P. Fredriksson, and H. Grönqvist (2011). Peers, neighborhoods, and immigrant student achievement: Evidence from a placement policy. *American Economic Journal: Applied Economics* 3(April), 67–95.
- Barber, B. M. and T. Odean (2001). Boys will be boys: Gender, overconfidence, and common stock investment. *Quarterly Journal of Economics* 116(1), 261–292.
- Bernheim, B. D. (1994). A theory of conformity. *Journal of political Economy*, 841–877.
- Brown, M., J. Grigsby, W. van der Klaauw, and J. Wen (2015). Financial education and the debt behavior of the young. *Review of Financial Studies*, Advance Access.
- Bucher-Koenen, T. and A. Lusardi (2011). Financial literacy and retirement planning in germany. *Journal of Pension Economics and Finance* 10(4), 565–584.
- Bucher-Koenen, T., A. Lusardi, R. Alessie, and M. van Rooij (2016). Women, confidence, and financial literacy. *Working Paper*.

- Carroll, C. D., B.-K. Rhee, and C. Rhee (1994). Are there cultural effects on saving? some cross-sectional evidence. *Quarterly Journal of Economics* 109(3), 685–699.
- Damm, A. P. and C. Dustmann (2014). Does growing up in a high crime neighborhood affect youth criminal behavior? *The American Economic Review* 104(6), 1806–1832.
- Disney, R. and J. Gathergood (2013). Financial literacy and consumer credit portfolios. *Journal of Banking and Finance* 37(8), 2246–2254.
- Duflo, E. and E. Saez (2002). Participation and investment decisions in a retirement plan: The influence of colleagues' choices. *Journal of Public Economics* 85(1), 121–148.
- Edin, P.-A., P. Fredriksson, and O. Åslund (2003). Ethnic enclaves and the economic success of immigrants - evidence from a natural experiment. *The Quarterly Journal of Economics* 118(1), 329–357.
- Georgarakos, D., M. Haliassos, and G. Pasini (2014). Household debt and social interactions. *Review of Financial Studies* 27(5), 1404–1433.
- Glaeser, E. and J. Scheinkman (2001). Measuring social interactions. *Social dynamics*, 83–132.
- Guiso, L., P. Sapienza, and L. Zingales (2003). People's opium? religion and economic attitudes. *Journal of Monetary Economics* 50(1), 225–282.
- Guiso, L., P. Sapienza, and L. Zingales (2004). The role of social capital in financial development. *American Economic Review* 94(3), 526–556.
- Guiso, L., P. Sapienza, and L. Zingales (2006). Does culture affect economic outcomes? *Journal of Economic Perspectives* 20(2), 23–48.
- Hackethal, A., M. Haliassos, and T. Jappelli (2012). Financial advisors: A case of babysitters? *Journal of Banking and Finance* 36(2), 509–524.

- Haliassos, M., T. Jansson, and Y. Karabulut (2016). Incompatible european partners? cultural predispositions and household financial behavior. *Management Science*, Forthcoming.
- Hong, H., J. D. Kubik, and J. C. Stein (2004). Social interaction and stock market participation. *Journal of Finance* 59(1), 137–163.
- Hospido, L., E. Villanueva, and G. Zamarro (2016). Finance for all: The impact of financial literacy training in compulsory secondary education in spain. *Working Paper*, Bank of Spain.
- Jappelli, T. and M. Padula (2013). Investment in financial literacy and saving decisions. *Journal of Banking and Finance* 37(7), 2779–2792.
- Kaustia, M. and S. Knüpfer (2012). Peer performance and stock market entry. *Journal of Financial Economics* 104(2), 321–338.
- Lusardi, A. (2008). Financial literacy: An essential tool for informed consumer choice? *NBER Working Paper Series*, 14084.
- Lusardi, A. and O. Mitchell (2014). The economic importance of financial literacy: Theory and evidence. *Journal of Economic Literature* 52(1), 5–44.
- Lusardi, A., O. Mitchell, and P.-C. Michaud (2016). Optimal financial knowledge and wealth inequality. *Journal of Political Economy*, forthcoming.
- Lusardi, A. and O. S. Mitchell (2007). Baby boomer retirement security: The roles of planning, financial literacy, and housing wealth. *Journal of Monetary Economics* 54(1), 205–224.
- Lusardi, A. and O. S. Mitchell (2009). How ordinary consumers make complex economic decisions: Financial literacy and retirement preparadness. *NBER Working Paper Series*, 15350.

- Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The Review of Economic Studies* 60(3), 531–542.
- Oreopoulos, P. (2003). The long-run consequences of living in a poor neighborhood. *The Quarterly Journal of Economics*, 1533–1575.
- Osili, U. O. and A. L. Paulson (2008). Does culture affect economic outcomes? *Review of Economics and Statistics* 90(3), 498–517.
- van Rooij, M., A. Lusardi, and R. Alessie (2011). Financial literacy and stock market participation. *Journal of Financial Economics* 101(2), 449–472.

Table I: Summary Statistics

| | <i>Full Sample</i> | | | <i>Medium-Term</i> | | | <i>Longer-Term</i> | | |
|---|--------------------|-------|-----------|--------------------|-------|-----------|--------------------|-------|-----------|
| | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. |
| <i>Panel A: Dependent Variables</i> | | | | | | | | | |
| 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 |
| <i>Panel B: Financial Literacy Externalities (at initial Placement)</i> | | | | | | | | | |
| 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 |
| <i>Panel C: Household Controls</i> | | | | | | | | | |
| Log disposable Income | 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 | 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 | 0.00 | 0.05 | 20,303 | 0.00 | 0.05 | 16,210 | 0.00 | 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 the 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: Households in the Sample by Country of Origin and Year of Immigration

| | Number of Households | Percentage of Households |
|-------------------------------------|----------------------|--------------------------|
| <i>Panel A: Country of Origin</i> | | |
| AFGHANISTAN | 22 | 0,54% |
| ANGOLA | 8 | 0,20% |
| BANGLADESH | 31 | 0,76% |
| BULGARIA | 40 | 0,98% |
| CAMBODIA | 6 | 0,15% |
| CHILE | 537 | 13,22% |
| CHINA | 13 | 0,32% |
| COLOMBIA | 19 | 0,47% |
| DEMOCRATIC REPUBLIC CONGO | 10 | 0,25% |
| (FORMER) CZECHOSLOVAKIA | 32 | 0,79% |
| EGYPT | 4 | 0,10% |
| EL SALVADOR | 53 | 1,31% |
| ERITREA | 66 | 1,63% |
| ESTONIA | 10 | 0,25% |
| ETHIOPIA | 280 | 6,89% |
| HUNGARY | 62 | 1,53% |
| INDIA | 4 | 0,10% |
| IRAN | 1169 | 28,79% |
| IRAQ | 375 | 9,23% |
| JORDAN | 6 | 0,15% |
| KUWAIT | 6 | 0,15% |
| LAOS | 7 | 0,17% |
| LEBANON | 332 | 8,18% |
| LIBYA | 5 | 0,12% |
| FYROM | 4 | 0,10% |
| MOROCCO | 4 | 0,10% |
| OTHER | 33 | 0,81% |
| PAKISTAN | 8 | 0,20% |
| PALESTINIAN AUTHORITY | 22 | 0,54% |
| PERU | 24 | 0,59% |
| POLAND | 70 | 1,72% |
| ROMANIA | 218 | 5,37% |
| SOMALIA | 77 | 1,90% |
| (FORMER) SOVIET UNION | 42 | 1,03% |
| SRI LANKA | 20 | 0,49% |
| SYRIA | 110 | 2,71% |
| TUNISIA | 13 | 0,32% |
| TURKEY | 117 | 2,88% |
| UGANDA | 11 | 0,27% |
| VIETNAM | 121 | 2,98% |
| (FORMER) YUGOSLAVIA | 70 | 1,72% |
| <i>Panel A: Year of Immigration</i> | | |
| 1987 | 965 | 23.76% |
| 1988 | 969 | 23.86% |
| 1989 | 1,309 | 32.23% |
| 1990 | 591 | 14.55% |
| 1991 | 227 | 5.59% |

Note: This Table presents the number and percentage shares of households in the final sample by country of origin and year of immigration, respectively. The sample includes 4,061 refugee immigrants as of 1999. The Other group includes refugee immigrants from countries from which there are less than 5 households in the final sample. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table III: Neighborhood Characteristics by Sample Splits

| <i>Panel A: By Education</i> | <i>High School and More</i> | | | <i>Less than High School</i> | | |
|--|-----------------------------|--------|-----------|------------------------------|--------|-----------|
| | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. |
| Share of neighbors with economics/business education | 26,328 | 2.39% | 2.88% | 10,185 | 2.16% | 2.75% |
| Share of neighbors with quantitative education | 26,328 | 4.71% | 5.17% | 10,185 | 4.29% | 4.94% |
| Share of neighbors who save for retirement | 26,328 | 19.51% | 9.81% | 10,185 | 18.38% | 9.41% |
| <i>Panel B: By Gender</i> | <i>Male</i> | | | <i>Female</i> | | |
| | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. |
| Share of neighbors with economics/business education | 24,608 | 2.28% | 2.80% | 11,905 | 2.40% | 2.95% |
| Share of neighbors with quantitative education | 24,608 | 4.49% | 5.02% | 11,905 | 4.80% | 5.29% |
| Share of neighbors who save for retirement | 24,608 | 19.20% | 9.73% | 11,905 | 19.19% | 9.68% |

Note: This table presents the mean and standard deviation for the neighborhood characteristics at initial neighborhood defined at the electoral district level for the following subsamples of refugee immigrants: Panel A presents the neighborhood characteristics for better versus less educated; and Panel B reports characteristics for male versus female, respectively. The sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

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

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.41405* (0.2442) | 0.61315** (0.2648) | 0.47840* (0.2560) | 0.70443** (0.2792) |
| Income | 0.18013*** (0.0127) | 0.20321*** (0.0123) | 0.08000*** (0.0085) | 0.08100*** (0.0076) |
| Age 30-45 | 0.03493* (0.0181) | -0.02578 (0.0240) | 0.04547*** (0.0164) | 0.00303 (0.0180) |
| Age 45-60 | 0.06202*** (0.0214) | -0.05972** (0.0266) | 0.06963*** (0.0181) | -0.00004 (0.0198) |
| Age 60-75 | -0.04522* (0.0257) | -0.10805*** (0.0324) | -0.01859 (0.0233) | -0.02188 (0.0253) |
| Male | -0.04132*** (0.0128) | -0.05365*** (0.0133) | -0.03309*** (0.0127) | -0.03189** (0.0134) |
| Unemployed | -0.01428 (0.0174) | -0.01542 (0.0197) | -0.00220 (0.0119) | -0.00675 (0.0114) |
| Retired | -0.03709 (0.0230) | -0.06787*** (0.0247) | -0.01561 (0.0152) | -0.04130*** (0.0146) |
| Employee | 0.04786** (0.0190) | 0.05625*** (0.0210) | 0.03096** (0.0132) | 0.02016 (0.0123) |
| Married | 0.01461 (0.0127) | 0.02582* (0.0134) | 0.00835 (0.0086) | 0.00863 (0.0093) |
| Nbr of adults | -0.02040*** (0.0070) | 0.00005 (0.0069) | 0.01402*** (0.0048) | 0.03995*** (0.0050) |
| Nbr of children | -0.02247*** (0.0048) | -0.00448 (0.0050) | -0.00372 (0.0038) | 0.02418*** (0.0039) |
| High school Dummy | 0.04946*** (0.0131) | 0.06410*** (0.0140) | 0.04123*** (0.0126) | 0.05155*** (0.0115) |
| College and more Dummy | 0.09739*** (0.0153) | 0.16755*** (0.0175) | 0.07744*** (0.0159) | 0.15253*** (0.0169) |
| Net wealth quartile II | -0.01189 (0.0104) | -0.02038* (0.0119) | -0.00462 (0.0069) | 0.00183 (0.0071) |
| Net wealth quartile III | -0.00281 (0.0113) | -0.02138 (0.0131) | 0.01087 (0.0077) | -0.00662 (0.0079) |
| Net wealth quartile IV | 0.10306*** (0.0144) | 0.13910*** (0.0140) | 0.03476*** (0.0092) | 0.02258** (0.0088) |
| Financial sector Dummy | 0.04859 (0.0893) | -0.05159 (0.0854) | 0.00212 (0.0743) | 0.03764 (0.0535) |
| Government sector Dummy | 0.00639 (0.0136) | -0.04285*** (0.0144) | -0.01385 (0.0095) | -0.01292 (0.0091) |
| <i>Observations</i> | 36513 | 36513 | 36513 | 36513 |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Country-of-Origin Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Arrival-year Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Unobserved HH Heterogeneity</i> | <i>No</i> | <i>No</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using Linear Probability Model (LPM). The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a business/economics education and college attendance in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table V: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term (1999-2003)

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.44802* (0.2526) | 0.35481 (0.2801) | 0.50404* (0.2613) | 0.43628 (0.2928) |
| Income | 0.15705*** (0.0134) | 0.21183*** (0.0147) | 0.07370*** (0.0099) | 0.11168*** (0.0103) |
| Age 30-45 | 0.04974*** (0.0172) | -0.02101 (0.0245) | 0.03840** (0.0150) | -0.00174 (0.0196) |
| Age 45-60 | 0.09116*** (0.0211) | -0.05696** (0.0277) | 0.06436*** (0.0172) | -0.01430 (0.0220) |
| Age 60-75 | -0.00611 (0.0275) | -0.11591*** (0.0361) | -0.03534 (0.0225) | -0.07698** (0.0316) |
| Male | -0.04176*** (0.0129) | -0.06071*** (0.0138) | -0.03287** (0.0129) | -0.03773*** (0.0139) |
| Unemployed | -0.00430 (0.0189) | -0.00548 (0.0218) | 0.01049 (0.0124) | -0.00899 (0.0125) |
| Retired | -0.04868** (0.0246) | -0.06024** (0.0278) | -0.00717 (0.0158) | -0.06000*** (0.0164) |
| Employee | 0.03927* (0.0202) | 0.07070*** (0.0233) | 0.03812*** (0.0138) | 0.01736 (0.0138) |
| Married | 0.02465* (0.0134) | 0.02968** (0.0148) | 0.00879 (0.0099) | 0.00948 (0.0111) |
| Nbr of adults | -0.02683*** (0.0083) | 0.00043 (0.0086) | 0.00477 (0.0053) | 0.03789*** (0.0065) |
| Nbr of children | -0.02279*** (0.0051) | -0.01018* (0.0057) | -0.00896** (0.0041) | 0.01650*** (0.0048) |
| High school Dummy | 0.04132*** (0.0130) | 0.06450*** (0.0149) | 0.03014*** (0.0114) | 0.05570*** (0.0128) |
| College and more Dummy | 0.09581*** (0.0156) | 0.15695*** (0.0178) | 0.07961*** (0.0149) | 0.15194*** (0.0168) |
| Net wealth quartile II | -0.01073 (0.0124) | -0.03577** (0.0140) | 0.00141 (0.0084) | -0.01473 (0.0090) |
| Net wealth quartile III | -0.02308* (0.0135) | -0.05382*** (0.0153) | 0.00342 (0.0096) | -0.01781* (0.0105) |
| Net wealth quartile IV | 0.10353*** (0.0161) | 0.11872*** (0.0159) | 0.04096*** (0.0105) | 0.02249** (0.0110) |
| Financial sector Dummy | 0.05251 (0.0908) | 0.01547 (0.0923) | -0.06720 (0.0764) | 0.06668 (0.0487) |
| Government sector Dummy | 0.00657 (0.0150) | -0.04295*** (0.0160) | -0.00831 (0.0105) | -0.00504 (0.0109) |
| <i>Observations</i> | 20303 | 20303 | 20303 | 20303 |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Country-of-Origin Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Arrival-year Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Unobserved HH Heterogeneity</i> | <i>No</i> | <i>No</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a business/economics education and college attendance in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2003. 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: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Longer-Term (2004-2007)

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.39095 (0.2798) | 0.93888*** (0.2833) | 0.44506 (0.2932) | 1.03961*** (0.2935) |
| Income | 0.20524*** (0.0171) | 0.19343*** (0.0146) | 0.08143*** (0.0092) | 0.08808*** (0.0098) |
| Age 30-45 | -0.03854 (0.0620) | -0.04372 (0.0647) | 0.05835 (0.0369) | -0.05553 (0.0409) |
| Age 45-60 | -0.02373 (0.0630) | -0.07511 (0.0644) | 0.07384* (0.0380) | -0.07231* (0.0417) |
| Age 60-75 | -0.12767* (0.0661) | -0.12632* (0.0683) | 0.01678 (0.0420) | -0.11226** (0.0441) |
| Male | -0.04238*** (0.0158) | -0.04662*** (0.0152) | -0.03601** (0.0155) | -0.03511** (0.0149) |
| Unemployed | -0.01821 (0.0337) | -0.03041 (0.0398) | -0.01824 (0.0177) | -0.00231 (0.0220) |
| Retired | -0.02130 (0.0409) | -0.07562* (0.0457) | -0.07221*** (0.0244) | -0.06773** (0.0280) |
| Employee | 0.06666* (0.0351) | 0.03597 (0.0411) | 0.00271 (0.0183) | 0.03366 (0.0231) |
| Married | 0.00655 (0.0165) | 0.01968 (0.0156) | 0.02150* (0.0115) | -0.00482 (0.0133) |
| Nbr of adults | -0.01695* (0.0088) | 0.00082 (0.0085) | 0.01983*** (0.0060) | 0.03643*** (0.0061) |
| Nbr of children | -0.01975*** (0.0061) | 0.00081 (0.0061) | 0.00711 (0.0049) | 0.03094*** (0.0055) |
| High school Dummy | 0.05747*** (0.0159) | 0.06200*** (0.0159) | 0.05541*** (0.0179) | 0.07301*** (0.0158) |
| College and more Dummy | 0.09832*** (0.0185) | 0.17569*** (0.0205) | 0.12286*** (0.0192) | 0.20671*** (0.0210) |
| Net wealth quartile II | -0.00829 (0.0143) | -0.00681 (0.0146) | -0.01695** (0.0086) | 0.01040 (0.0095) |
| Net wealth quartile III | 0.01601 (0.0149) | 0.01975 (0.0164) | -0.00526 (0.0090) | -0.00411 (0.0098) |
| Net wealth quartile IV | 0.09982*** (0.0176) | 0.16945*** (0.0178) | 0.02079* (0.0107) | 0.04065*** (0.0115) |
| Financial sector Dummy | 0.03533 (0.1138) | -0.13385 (0.0934) | 0.08240* (0.0448) | -0.06080 (0.0416) |
| Government sector Dummy | 0.00599 (0.0168) | -0.03773** (0.0171) | 0.00051 (0.0116) | -0.02696** (0.0121) |
| <i>Observations</i> | 16210 | 16210 | 16210 | 16210 |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Country-of-Origin Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Arrival-year Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Unobserved HH Heterogeneity</i> | <i>No</i> | <i>No</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a business/economics education and college attendance in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants for the years 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 VII: Sample Split By Education: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

| | High school and more | | Less than high school | |
|--|-----------------------|------------------------|-----------------------|----------------------|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| Panel A: Medium-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.78922** (0.3346) | 0.80131** (0.3570) | 0.08768 (0.4102) | -0.26196 (0.4296) |
| Observations | 14392 | 14392 | 5911 | 5911 |
| Panel B: Longer-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.70775* (0.3766) | 1.42920*** (0.3521) | -0.22101 (0.5802) | 0.29275 (0.4877) |
| Observations | 11936 | 11936 | 4274 | 4274 |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes |
| <i>Clustering</i> | Electoral District | Electoral District | Electoral District | Electoral District |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Unobserved HH Heterogeneity</i> | Yes | Yes | Yes | Yes |
| <i>Neighborhood Fixed Effects</i> | Parish | Parish | Parish | Parish |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by the level of education. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a business/economics education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (2003-2007). Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

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

| | Female | | Male | |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| Panel A: Medium-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.19307 (0.4782) | -0.32645 (0.5073) | 0.54860* (0.3226) | 0.80739** (0.3448) |
| Observations | 6620 | 6620 | 13683 | 13683 |
| Panel B: Longer-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.10904 (0.5189) | 0.26643 (0.4946) | 0.50389 (0.3762) | 1.37665*** (0.3672) |
| Observations | 5285 | 5285 | 10925 | 10925 |
| <i>Household Controls</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Country-of-Origin Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Arrival-year Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Unobserved HH Heterogeneity</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by gender. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a business/economics education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (2003-2007). Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

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

| | Saving for Retirement | | Stockholding | | Saving for Retirement | | Stockholding | |
|---|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|
| <i>Panel A: Full Observation Period</i> | (i) | (ii) | (iii) | (iv) | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.27525* (0.1520) | 0.26714* (0.1572) | 0.31259* (0.1601) | 0.31522* (0.1664) | 36513 | 36513 | 36513 | 36513 |
| <i>Observations</i> | | | | | | | | |
| <i>Panel B: Medium-Term</i> | (i) | (ii) | (iii) | (iv) | | | | |
| Fin Lit Share | 0.34296** (0.1635) | 0.09722 (0.1652) | 0.37303** (0.1693) | 0.13902 (0.1737) | 20303 | 20303 | 20303 | 20303 |
| <i>Observations</i> | | | | | | | | |
| <i>Panel C: Longer-Term</i> | (i) | (ii) | (iii) | (iv) | | | | |
| Fin Lit Share | 0.19884 (0.1696) | 0.47743*** (0.1708) | 0.24087 (0.1789) | 0.53712*** (0.1759) | 36513 | 36513 | 36513 | 36513 |
| <i>Observations</i> | | | | | | | | |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Unobserved HH Heterogeneity</i> | No | No | Yes | Yes | No | Yes | Yes | Yes |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a quantitative education and college attendance in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. In Panel A, the sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. In Panel B and C, the observation period is restricted to 1999-2003 and 2004-2007, 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.

Table X: Sample Split By Education: Long Shadow Effects of Having Neighbors with Quantitative 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) |
| Fin Lit Share | 0.60545*** (0.2145) | 0.38645* (0.2219) | 0.16838 (0.2589) | -0.25624 (0.2417) |
| Observations | 14392 | 14392 | 5911 | 5911 |
| Panel B: Longer-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.38988* (0.2303) | 0.81848*** (0.2232) | -0.01947 (0.3663) | -0.00647 (0.2891) |
| Observations | 11936 | 11936 | 4274 | 4274 |
| <i>Household Controls</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Country-of-Origin Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Arrival-year Fixed Effects</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Unobserved HH Heterogeneity</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by the level of education. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a quantitative education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (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: Sample Split By Gender: Long Shadow Effects of Having Neighbors with Quantitative Education and College Attendance: Medium-Term and Longer-Term

| | Female | | Male | |
|---------------------------------|-----------------------|---------------------|-----------------------|-----------------------|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| Panel A: Medium-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.52535* (0.2975) | 0.07839 (0.3218) | 0.28260 (0.2046) | 0.11269 (0.2142) |
| Observations | 6620 | 6620 | 13683 | 13683 |
| Panel B: Longer-Term | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.07605 (0.3249) | 0.29084 (0.3210) | 0.23416 (0.2320) | 0.55456** (0.2298) |
| Observations | 5285 | 5285 | 10925 | 10925 |
| Household Controls | Yes | Yes | Yes | Yes |
| Clustering | Electoral District | Electoral District | Electoral District | Electoral District |
| Time Fixed Effects | Yes | Yes | Yes | Yes |
| Country-of-Origin Fixed Effects | Yes | Yes | Yes | Yes |
| Arrival-year Fixed Effects | Yes | Yes | Yes | Yes |
| Unobserved HH Heterogeneity | Yes | Yes | Yes | Yes |
| Neighborhood Fixed Effects | Parish | Parish | Parish | Parish |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by gender. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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) with a quantitative education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (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 XII: Long Shadow Effects of Having Neighbors with Business/Economics Education: Full Observation Period, Medium-Term, and Longer-Term for Alternative Social Group

| | Saving for Retirement | | Stockholding | | Saving for Retirement | | Stockholding | |
|---|---------------------------|---------------------------|---------------------------|---------------------------|-----------------------|------|--------------|------|
| <i>Panel A: Full Observation Period</i> | (i) | (ii) | (iii) | (iv) | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.46570* (0.2424) | 0.59371** (0.2639) | 0.53023** (0.2537) | 0.67961*** (0.2777) | | | | |
| Observations | 36513 | 36513 | 36513 | 36513 | | | | |
| <i>Panel B: Medium-Term</i> | (i) | (ii) | (iii) | (iv) | | | | |
| Fin Lit Share | 0.48015* (0.2513) | 0.35321 (0.2768) | 0.53667** (0.2599) | 0.42907 (0.2890) | | | | |
| Observations | 20303 | 20303 | 20303 | 20303 | | | | |
| <i>Panel C: Longer-Term</i> | (i) | (ii) | (iii) | (iv) | | | | |
| Fin Lit Share | 0.46762* (0.2784) | 0.89860*** (0.2848) | 0.52318* (0.2915) | 0.99980*** (0.2942) | | | | |
| Observations | 16210 | 16210 | 16210 | 16210 | | | | |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes | | | | |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | | | | |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes | | | | |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes | | | | |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes | | | | |
| <i>Unobserved HH Heterogeneity</i> | No | No | Yes | Yes | | | | |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | | | | |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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 10 years) with business/economics education and college attendance in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. In Panel A, the sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. In Panel B and C, the observation period is restricted to 1999-2003 and 2004-2007, 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.

Table XIII: Sample Split By Education: Long Shadow Effects of Having Neighbors with Business/Economics Education and College Attendance: Medium-Term and Longer-Term for Alternative Social Group

| | High school and more | | Less than high school | |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| <i>Panel A: Medium-Term</i> | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.83723** (0.3323) | 0.82388** (0.3530) | 0.09694 (0.4005) | -0.33296 (0.4322) |
| <i>Observations</i> | 14212 | 14212 | 5821 | 5821 |
| <i>Panel B: Longer-Term</i> | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.82613** (0.3728) | 1.40685*** (0.3533) | -0.14698 (0.5758) | 0.16899 (0.4929) |
| <i>Observations</i> | 11785 | 11785 | 4211 | 4211 |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Unobserved HH Heterogeneity</i> | Yes | Yes | Yes | Yes |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by the level of education. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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 10 years) with a business/economics education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (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 XIV: Sample Split By Gender: Long Shadow Effects of Having Neighbors with Business/Economics Education and College Attendance: Medium-Term and Longer-Term for Alternative Social Group

| | Female | | | Male | | |
|--|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|
| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
| <i>Panel A: Medium-Term</i> | (i) | (ii) | (iii) | (iv) | | |
| Fin Lit Share | 0.15259 (0.4780) | -0.34859 (0.4980) | 0.58712* (0.3239) | 0.76180** (0.3434) | | |
| <i>Observations</i> | 6490 | 6490 | 13543 | 13543 | | |
| <i>Panel B: Longer-Term</i> | (i) | (ii) | (iii) | (iv) | | |
| Fin Lit Share | 0.16746 (0.5185) | 0.17516 (0.4879) | 0.56563 (0.3717) | 1.31635*** (0.3690) | | |
| <i>Observations</i> | 5182 | 5182 | 10814 | 10814 | | |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Unobserved HH Heterogeneity</i> | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM for sample splits by gender. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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 10 years) with a business/economics education and college attendance in the initial neighborhood. In all specifications, we control for unobserved household heterogeneity using random effects estimator. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for medium-term (1999-2003), while Panel B reports the results for the longer-term (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 XV: Long Shadow Effects of Having Retirement Savers as Neighbors: Full Observation Period, Medium-Term, and Longer-Term

| | Saving for Retirement | Stockholding | Saving for Retirement | Stockholding |
|--|---------------------------|---------------------------|---------------------------|---------------------------|
| <i>Panel A: Medium-Term</i> | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.11534 (0.0834) | 0.02162 (0.0838) | 0.14064 (0.0862) | 0.04769 (0.0869) |
| <i>Observations</i> | 20303 | 20303 | 20303 | 20303 |
| <i>Panel B: Longer-Term</i> | (i) | (ii) | (iii) | (iv) |
| Fin Lit Share | 0.17644* (0.0934) | 0.17982** (0.0872) | 0.20113** (0.0957) | 0.23120** (0.0902) |
| <i>Observations</i> | 16210 | 16210 | 16210 | 16210 |
| <i>Household Controls</i> | Yes | Yes | Yes | Yes |
| <i>Clustering</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> | <i>Electoral District</i> |
| <i>Time Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Country-of-Origin Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Arrival-year Fixed Effects</i> | Yes | Yes | Yes | Yes |
| <i>Unobserved HH Heterogeneity</i> | No | No | Yes | Yes |
| <i>Neighborhood Fixed Effects</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> | <i>Parish</i> |

Note: This table presents the estimates of the determinants of saving for retirement and stockholding that is estimated using LPM. The dependent variables, Saving for Retirement and Stockholding, are binary variables that takes the value 1 if the household saves for retirement or holds directly or indirectly stocks, respectively. 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. 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 actively save for retirement in the initial neighborhood. In columns (iii) and (iv), we also control for unobserved household heterogeneity using random effects estimator. In Panel A, the sample is a balanced sample of 4,061 refugee immigrants for the years 1999-2007. In Panel B and C, the observation period is restricted to 1999-2003 and 2004-2007, 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.