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Peer performance and stock market entry ☆

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ABSTRACT

Peer performance can influence the adoption of financial innovations and investment styles. We present evidence of this type of social influence: recent stock returns that local peers experience affect an individual's stock market entry decision, particularly in areas with better opportunities for social learning. The likelihood of entry does not decrease as returns fall below zero, consistent with people not talking about decisions that have produced inferior outcomes. Market returns, media coverage, local stocks, omitted local variables, short sales constraints, and stock purchases within households do not seem to explain these results.

1. Introduction

> "That others have made a lot of money appears to many people as the most persuasive … evidence that outweighs even the most carefully reasoned argument…"
>
> Robert Shiller, Irrational Exuberance, 2000

Investor sentiment—the collection of beliefs not justifiable by economic fundamentals—influences asset prices.[1] Although a variety of studies show the price impact of sentiment, the microfoundations are not well understood. In this paper, we analyze the role of social influences. Our aim is to explain individuals' stock market entry decisions using recent stock market experiences of local peers.

Market entry is an interesting decision to analyze, as asset price bubbles tend to be associated with sharp increases in participation rates.[2] Fig. 1 uses data from

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[1] Investor sentiment is related to blatant violations of the law of one price (Mitchell, Pulvino and Stafford, 2002; Lamont and Thaler, 2003), closed-end fund discounts (Lee, Shleifer and Thaler, 1991), retail investor trading (Kumar and Lee, 2006; Barber, Odean and Zhu., 2009), initial public offering (IPO) pricing patterns (Cornelli, Goldreich and Ljungqvist, 2006; Derrien, 2005), investment (Baker, Stein and Wurgler, 2003; Polk and Sapienza, 2009), and stock return predictability (Baker and Wurgler, 2007).

[2] The low rates of stock market participation have attracted attention from both academics and policy makers (see, e.g., Mankiw and Zeldes, 1991; Haliassos and Bertaut, 1995; Guiso, Haliassos and Jappelli, 2003). Most of the evidence on stock market participation comes from

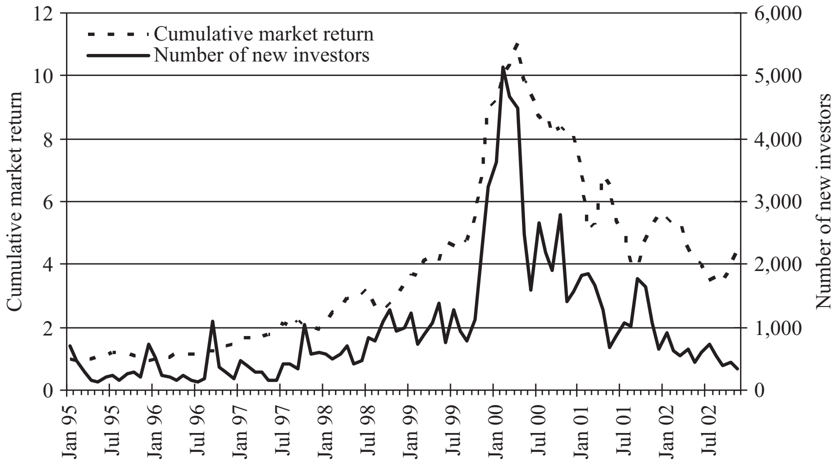

Fig. 1. Stock market entry and market returns. The solid line plots the monthly number of investors who enter the stock market; the dotted line plots the cumulative market return in the Finnish stock market from 1995 to 2002. The stock market entry date is the first day on which an investor buys stocks of publicly listed companies. We exclude entries through equity offerings, gifts, inheritances, divorce settlements, and other transactions that do not represent an active stock market entry decision. The market return is based on the HEX Portfolio Index.

Finland during the Internet and technology boom of the late 1990s to illustrate this point. At the peak of the market, entry rates were about five times the average. Shiller (1984, 1990, 2000), among others, suspects that investment success stories spreading in networks of peers partly explain the pattern of new investors flocking into the market in times of high sentiment.

Stock market outcomes of peers could influence entry decisions through two plausible channels. First, individuals could use peer outcomes to update beliefs about long-term fundamentals, such as the equity premium. However, compared with more deterministic environments, the stock market is exceptional in that learning about fundamentals from peer outcomes is difficult to rationalize. Peer outcomes represent small data samples that come with various biases. The random components in returns are large and unobservable factors are involved, such as investment skill and exposure to risk.[3] For these reasons, information on peer outcomes should be discounted heavily. A large literature on probabilistic judgment shows, however, that people typically fail to fully adjust for small sample biases (e.g., Tversky and Kahneman, 1971; Nisbett, Borgida, Crandall and Reed, 1976; Einhorn, 1980).

Second, people cannot directly observe peer outcomes and have to rely on indirect cues, such as verbal accounts, instead. Communication could be biased toward positive outcomes if appearing to be a successful investor carries private benefits.[4] If such selectivity is present and people do not know whether their peers participate in the market, communication reveals not only the outcomes but also the participation status of peers. This mechanism could, in turn, make people enter the market if they care about their wealth relative to their peers (Abel, 1990; Gali, 1994; Bakshi and Chen, 1996; DeMarzo, Kaniel and Kremer, 2004).

We analyze the influence of peer outcomes using reliable and accurate microdata that cover the stock holdings and trades of the entire population of individual investors in Finland from 1995 to 2002.[5] These individual-level stock market transactions are an exceptional source of outcome data, as we can identify the true outcomes of neighboring investors for each individual explicitly (as opposed to self-reported information in survey data), measure them at a high frequency without error, and relate them to individual decisions.

We identify peer outcomes by taking advantage of the large geographical variation in stock holdings and returns. We aggregate direct stock holdings at the zip code level and measure the monthly returns of these zip code portfolios, henceforth called neighborhood returns.[6] We find that high neighborhood returns are associated with an increase in the number of new investors entering the stock market in the same neighborhood the following month. The effects are economically meaningful: A one standard deviation increase in the monthly neighborhood return increases stock market entry rate by 9–13%.

Alternative mechanisms that do not involve social influence could generate these results. Panel data techniques, that is, fixed effects and the use of lagged returns,

(*footnote continued*)
cross-sectional snapshot data. An exception is Brunnermeier and Nagel (2008), who study the dynamics of wealth and participation.

[3] See models of social learning; for example, Ellison and Fudenberg (1993, 1995), McFadden and Train (1996), Persons and Warther (1997), Banerjee and Fudenberg (2004), and Cao, Han and Hirshleifer (2011).

[4] Such behavior is modeled by Hirshleifer (2011) and is also in line with the communication model by Bénabou and Tirole (2002).

[5] The data source, the Finnish Central Securities Depository, is an official ownership registry that covers the whole Finnish stock market. More description of a subset of the data is provided in Grinblatt and Keloharju (2000).

[6] The variation in portfolio returns across zip codes is remarkably high. The time series average of the cross-sectional standard deviation of neighborhood returns equals 3.4%, while the average monthly neighborhood return equals 1.2%.

eliminate concerns about common unobservables and reverse causality. The zip code fixed effects control for systematic regional differences. The inclusion of province-month fixed effects (there are 20 provinces, containing 132 zip codes on average) removes the influence of market returns and market-wide news releases, as well as the effects of local media coverage and other local economic shocks that operate at the provincial level. A possibility nevertheless remains that the residents of a particular zip code would experience shocks in a month that are positively correlated with neighborhood returns. We assess this possibility by analyzing subsamples in which such influences are unlikely.

Any shocks that involve the stocks of local companies cannot explain our results, as the estimates are similar in areas containing no local stocks. An analysis in which we look at the peer influences in individuals' decisions to participate in initial public offerings, which, by definition, are not held by any investors before listing, rules out any other special roles for stocks local investors hold. Wealth effects from local stock market returns to nonparticipants' wealth are an unlikely explanation, as our results are practically the same in areas where wealth is more closely tied to the local economy. We rule out the effect of household heads purchasing shares for other household members by looking at the interactions in the behavior of individuals of the same gender and age.

We perform additional tests to understand the nature of social influence. We decompose the outcome variable into negative and positive regions to analyze whether unfavorable peer outcomes influence entry similarly to good outcomes. We find that the negative returns do not affect entry and that the relation between peer returns and entry comes solely from the positive region of returns. This pattern is consistent with selective communication: people are more likely to talk about favorable experiences.[7] We also find that the neighborhood return effect is particularly strong in areas with high stock market participation rates. The amplifying effect of stock market participation is consistent with social influence being more likely when more opportunities are available to learn from peers.

We can summarize our contribution as follows. First, we find evidence of outcome-based social influence in the stock market, a setting in which peer outcomes should not be very informative. Second, we uncover a peculiar pattern in social influence consistent with the idea that people communicate selectively, that is, refrain from discussing bad outcomes. Third, we find evidence consistent with individuals extrapolating from peer outcomes, with implications for understanding how sentiment develops and propagates in the economy.

[7] The asymmetry does not appear to be driven by short sales restrictions that make it difficult for prospective new investors to act on negative information. In our regressions, the constant is positive; that is, the unconditional entry rate is positive. Hence, if communication was equally likely for negative and positive returns, the negative peer returns would bring down the entry rate from the positive unconditional average. The asymmetry also seems to hold in a Tobit regression that explicitly accounts for the nonobservability of the negative entry rates.

Our paper is most closely related to studies that analyze the influence of peer actions in the stock market. Hong, Kubik and Stein (2004) and Brown, Ivković, Smith and Weisbenner (2008) provide evidence consistent with the notion that individuals are more likely to participate in the stock market when their geographically proximate peers participate. Hong, Kubik and Stein (2005) also show that investors tend to buy stocks their local peers have been buying in the recent past. Unlike these studies, we show that peer outcomes have incremental explanatory power over peer actions, making it easier to attribute our findings to sentiment instead of valuable information exchange.

Our paper also connects to a recent literature that examines how individuals form their beliefs about asset returns. Kaustia and Knüpfer (2008), Choi, Laibson, Madrian and Metrick (2009), and Malmendier and Nagel (2011) argue that personally experienced outcomes are an important influence in investment decisions, over and above general statistical information. Such behavior is consistent with reinforcement learning; that is, repeating behavior that has produced good outcomes in the past. Lacking any personal experiences, the new investors we analyze could turn to the next most cognitively accessible source of information: the experiences of their peers.

Only a handful of other empirical studies analyzes the influence of peer outcomes, and they are within the fields of agricultural and development economics (Munshi, 2004; Kremer and Miguel, 2007; Conley and Udry, 2010). We provide evidence on the influence of peer outcomes in the stock market, in which the environment is much less deterministic and consequently the impediments to social learning are much stronger than in the settings involved in previous studies (planting of wheat variants, de-worming drugs, and fertilizer use).

We outline the remainder of the paper as follows. Section 2 reviews the literature on different forms of social interaction and develops the hypotheses. Section 3 discusses relevant institutional background and the data sources, and Section 4 presents the empirical strategy. Section 5 presents the results, and Section 6 assesses alternative explanations. Section 7 concludes.

2. Literature and hypotheses

This section first reviews prior literature on social learning, making a distinction between action-based and outcome-based influence. It then discusses two broad channels through which peer outcomes might influence actions in the stock market.

2.1. Earlier literature on social interaction

Empirical studies on social interaction come from various fields and settings. One manifestation of the multitude of the studies is the vocabulary they use. The social mechanism can be referred to as social influence, peer effects, community effects, neighborhood effects, network effects, herding, mimicking, conformity, or observational learning. A common problem in empirical studies is identification. A finding that individuals' choices are related to peers' choices is not necessarily due to social interaction (Manski, 1993, 2000). Many empirical studies nevertheless argue, and use varying identification strategies to show that social interaction is driving the observed relation between average behavior of a peer group and an individual's behavior.

Research has found evidence for action-based social learning in farmers' crop choices (Foster and Rosenzweig, 1995), criminal activity (Glaeser, Sacerdote and Scheinkman, 1996), labor market participation of married women (Woittiez and Kapteyn, 1998), use of welfare benefits (Bertrand, Luttmer and Mullainathan, 2000), membership of social groups (Sacerdote, 2001), pension plan participation (Duflo and Saez, 2002), stock market participation (Hong, Kubik and Stein, 2004), stock market trading (Shive, 2010), choice of a health plan (Sorensen, 2006), automobile purchases (Grinblatt, Keloharju and Ikäheimo, 2008), choice of workplace (Bayer, Ross and Topa, 2008), and choice of dishes from a restaurant menu (Cai, Chen and Fang, 2009).

Sacerdote (2001) and Zimmerman (2003) show that the performance of a randomly assigned roommate positively affects academic performance in college. One interpretation of this result is that being assigned to a high-achieving roommate helps one emulate good study practices. This interpretation is consistent with outcome-based social learning. It could, however, be due to other externalities, such as motivation or competitive pressure.

2.2. *Outcome-based social influence*

Early work by social psychologists has shown the importance of observing outcomes as children copy new behaviors (Bandura and Walters, 1963). In the field of animal studies, Call and Tomasello (1994) find that orangutans do not merely copy what other orangutans are doing when they are trying to obtain out-of-reach food. Instead, they adopt techniques that yield best results, paying attention to both orangutan and human demonstrators' success. Theorizing based on these findings informs the development of hierarchical models of learning in the behavioral brain sciences (for a review, see Byrne and Russon, 1998). Economic theorists have extensively modeled outcome-based social learning (Ellison and Fudenberg, 1993, 1995; McFadden and Train, 1996; Persons and Warther, 1997; Banerjee and Fudenberg, 2004; Cao, Han and Hirshleifer, 2011).

Despite the theoretical interest, empirical research on the outcome-based dimension of social influence has been limited. The studies of which we are aware are in the areas of agricultural and development economics. Munshi (2004) finds that Indian farmers planted more of a new high-yielding variant of wheat in the early 1970s if farmers in the neighboring districts had received good yields from that variant. The farm-level results are based on a single snapshot of data in 53 villages. Kremer and Miguel (2007) test for peer effects in the decision to undergo drug treatment against intestinal worms in Kenya, and they find that people are less likely to take the de-worming drugs if their peers have taken them. The authors argue that this is consistent with peer effects due to learning from others about the benefits of the technology. Conley and Udry (2010) investigate fertilizer use in 47 pineapple farms in the Akwapim South district of Ghana at a time when pineapple was a new produce in the area. Figuring out the right amount of fertilizer requires some experimentation, as the optimum amount depends on local conditions. The results show that the amount of fertilization used by farmers depends on their neighboring farmers' usage and the profits that their peers achieved.

2.3. *Naïve extrapolation and selective communication*

In addition to our main hypothesis about outcome-based social influence, we investigate two broad channels through which peer outcomes might influence actions in the stock market. First, if people extrapolate from peer outcomes when forming their return expectations (as in Shiller, 1984, 1990), outcomes exert an incremental influence on entry decisions beyond other sources of information. Given the small and biased samples drawn from peer outcomes, such behavior appears anomalous. Second, if more communication follows good peer outcomes, and the participation status of peers is not fully known prior to communication, people could enter the market due to relative wealth concerns. People could want to imitate their peers due to a "keeping up with the Joneses" effect (Abel, 1990; Gali, 1994; Bakshi and Chen, 1996). Such an effect could arise from conformity to social norms (Akerlof, 1976) or competition for local resources (DeMarzo, Kaniel and Kremer, 2004).

People might be more willing for several reasons to discuss their stock market experiences after they have experienced good returns. First, they could simply enjoy discussing their positive stock market experiences more than the bad ones. Second, appearing to be a competent investor can carry private benefits. Third, various theories in psychology (under the conceptual umbrellas of motivated cognition, self-deception, or attribution) predict, and experiments confirm, that people have a self-serving bias in recalling and interpreting the factors involved in their successes and failures.

People tend to take credit for good performance while blaming external factors for poor performance. Cognitive dissonance theory (Festinger (1957); see Akerlof and Dickens (1982) for an economic model) argues that a discrepancy between one's actions and self-image causes discomfort and that people try to act and think in ways that reduce the discomfort. Bénabou and Tirole (2002) present a general economic model in which agents protect their self-esteem by engaging in self-deception through selective memory and awareness. Hirshleifer (2011) provides a model of investor communication in which investors are more likely to discuss their profitable investments than their losing ones, and people fail to adjust for this bias when deciding which investment style to adopt. Strategies with higher variance thus tend to be reported more often, making people overestimate the value of active investment strategies.

The naïve extrapolation and selective communication stories can alone account for a positive relation between past neighborhood returns and the tendency of new investors to enter the market. Selective communication makes a further testable prediction the extrapolation story does not share: peer outcomes should have a stronger influence on actions when the outcomes have been better. The stories are not mutually exclusive, however. In particular, communication might be selective, but once communication takes place, extrapolation still occurs.

3. Institutional background and data

This section describes the data and the construction of key variables. We begin by discussing the main institutional features of stock market investing in Finland at the time of the sample period.

3.1. Stock market participation in Finland

The means of participating in the stock market in Finland are directly held stock, mutual funds, and voluntary pension products. In this section we briefly discuss their key institutional features, as well as illustrate their relative importance in households' asset holdings.[8]

The first mutual funds were introduced in 1987, but their use by households remained limited for several years. Total household assets in all kinds of mutual funds were 5.2 billion euros in 2000, but only 0.5 billion euros at the beginning of our sample period in 1995. Discussions with bankers reveal that people without any direct stock holdings did not commonly purchase equity mutual funds during the 1990s. Since 2002, the popularity of mutual funds has grown.

All employees are automatically included in a government-sponsored defined benefits pension plan in which the benefits do not depend on stock returns. An individual employee has no influence on the amount of her own contribution or the selection of investments in these government-sponsored plans. Pensions are mostly financed by the contributions of the current workforce. Making additional voluntary pension investments with some tax benefits is also possible. Such investments totaled 10 billion euros in 2000. The vast majority of these assets were invested in the money market.

Comparing the different means of stock market participation shows that directly held stock has been the dominant channel. In 2000, households held 25.8 billion euros in direct stock, 2.6 billion euros in equity mutual funds, and 0.75 billion euros in equity through voluntary pension investments. This totals 29.2 billion euros, of which the share of directly held stock was 89%.[9]

From 1995 to 2002, the stock market participation rate through directly held stock increased from 9.3% to 13.9%, which implies an annual increase of 50 basis points.[10] The largest increases occurred from 1998 to 2000, and they coincided with high market returns and many equity offerings that attracted new investors to participate in the stock market. Privatizations of government-owned companies played an important role in increasing stock market participation. Individuals made about 240,000 subscriptions in these offerings (Keloharju, Knüpfer and Torstila, 2008).

3.2. Data

The data come from an established source for investor level data. The data set is derived from the Finnish Central Securities Depository (FSCD), an official registry that includes every stock market transaction of every stock market participant in the whole Finnish stock market. The data span a time period from January 1995 to November 2002. The data also include a number of investor characteristics. For our purposes, the most important is the place of residence, which is available at three times: January 10, 1997, June 30, 2000, and November 27, 2002. From this data set, we extract two types of data:

(1) *Entry dates*: For every investor in the sample, we determine the stock market entry date as the first day on which an investor buys stocks of publicly listed companies. We require that no other transactions with positive volume take place on that day to exclude entries through equity offerings, gifts, inheritances, divorce settlements, and others that do not represent an active stock market entry decision. Our data explicitly identify such cases. This definition of stock market entry also leaves out investors that have a stock market position at the beginning of the sample period. Some of the investors that enter the stock market during our sample period might already have participated in the stock market earlier but perhaps exited before the beginning of our sample period. If anything, this possibility is likely to bias our results in favor of the null, because investors with previous personal experiences of stocks probably pay less attention to peer outcomes than individuals with no such experiences.
(2) *Neighborhood returns*: In the absence of a direct mapping from an individual to his or her neighbors, we use zip codes as the neighborhoods. In total, there were about 27 hundred zip codes in Finland at the end of 2002. For each zip code in the sample, we define neighborhood return as the equally weighted average return on the portfolio the investors residing in a zip

[8] The estimates in this subsection are based on data provided by the Finnish Bankers' Association and the Finnish Association for Mutual Funds.

[9] This calculation is based on the following statistics and assumptions: The share of mutual fund assets invested in equity has been between 30% and 50% during 1998–2002. We lack accurate statistics from earlier sample years and assume that 50% of mutual fund assets are generally invested in equity. Statistics on pension assets show that only about 15% is invested outside of the money market, of which we assume 50% is invested in equity. The share of directly held stock of the total stock market investments has ranged from 82% to 92% during the sample years. The range is 88% to 95% if, instead of 50%, we assume that only 30% of mutual funds and nonmoney market pension assets are invested in equity.

[10] In our analysis, as in Fig. 1, we leave out stock market entries through equity offerings and other transactions in which the investor cannot fully determine the timing of entry.

code held at the beginning of a month. We also use the value-weighted portfolio return in some of the analysis.

We merge this data set with socioeconomic census data from Statistics Finland. These data come from a data product called SuomiCD. The database includes various socioeconomic variables from different times, depending on the timing of the census. We use the 2002 version, in which some of the variables come from the end of 2000 and some from the end of 2002. For ease of exposition, we refer to all of these data coming from 2002 throughout the paper.

Table 1, Panel A, summarizes descriptive statistics of the zip codes in the sample. The average stock market entry rate implies that on average 1.6% of the inhabitants of a zip code entered the stock market during the sample period. This number is different from the aggregate entry rate, 1.8%, which effectively weights the zip code entry rates with the number of inhabitants in each zip code. For illustrative purposes, Panel B reports results of regressions of the determinants of stock market entry rates aggregated at the zip code level. Entry rates are higher in areas with higher wealth, income, and level of education. Members of the Swedish-speaking minority, who hold a disproportionately high amount of wealth, are also more likely to enter the stock market. Fig. 2 plots the stock market entry rates across the whole country. Stock market entry rates are higher in Southern and Western Finland, reflecting the concentration of urban areas in the South and the Swedish-speaking communities in the West.

4. Methods and identification

This section discusses our identification strategy utilizing panel regressions of the monthly zip code level entry rates.

4.1. Regressions

In an effort to test our main hypothesis, we run regressions of zip code entry rates at the monthly level.

Table 1
Descriptive statistics.

This table reports descriptive statistics of zip code-level socioeconomic characteristics and stock market entry rates. Panel A summarizes the socioeconomic characteristics of the 2649 zip codes based on census data available at the end of the sample period in 2002. Stock market entry rate, measured in percentage points, is the proportion of inhabitants entering the stock market during the sample period of 1995–2002. Population density is the number of inhabitants divided by the area of a zip code (in kilometers squared). Age measures the average age of all the inhabitants of a zip code. College degrees is the proportion of people with higher academic education. Swedish-speaking measures the proportion of people whose mother tongue is Swedish (Finland has two official languages, Finnish and Swedish). Income and wealth are based on official tax filings of all the individuals living in a zip code. Self-employed is the proportion of inhabitants working as an entrepreneur and homeownership is the proportion of housing units owned by the occupier. Panel B explains stock market entry rates with socioeconomic characteristics. The regressions include decile dummies (except one) for population density, not reported for brevity. Heteroskedasticity robust *t*-values are reported in parentheses below coefficients. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: socioeconomic characteristics of zip codes

Characteristic	Mean	Standard deviation	Minimum	25%	Median	75%	Maximum
Stock market entry rate (%)	1.6	1.2	0.0	1.0	1.5	2.0	35.0
Number of inhabitants	1911	2801	101	272	609	2423	24,734
Area (km^2)	110	226	0.1	21	55	114	3511
Population density (per km^2)	299	1010	0.1	4	10	59	23,464
College degrees (%)	55.4	10.0	25.0	48.0	55.0	62.0	92.0
Swedish-speaking (%)	7.0	21.3	0.0	0.0	0.0	1.0	98.0
Income (thousand of euros)	15.6	4.1	9.1	13.0	15.0	17.3	66.4
Wealth (thousand of euros)	52.6	25.2	10.0	39.0	48.0	60.0	397.0
Self-employment (%)	3.4	1.4	0.0	2.5	3.3	4.1	14.6
Homeownership (%)	77.3	15.6	0.0	70.0	82.0	89.0	100.0

Panel B: regressions of stock market entry rate

Variable	(1)	(2)	(3)
Ln(Income)	0.413	1.107	
	(1.85)*	(4.72) ***	
Ln(Wealth)	0.496		0.583
	(6.14)***		(6.78)***
College degrees	0.014	0.009	0.018
	(2.80) ***	(1.67)*	(4.38)***
Swedish-speaking	0.014	0.014	0.015
	(7.66) ***	(7.59)***	(8.14)***
Number of observations	2,649	2,649	2,649
Adjusted R^2	0.153	0.148	0.151

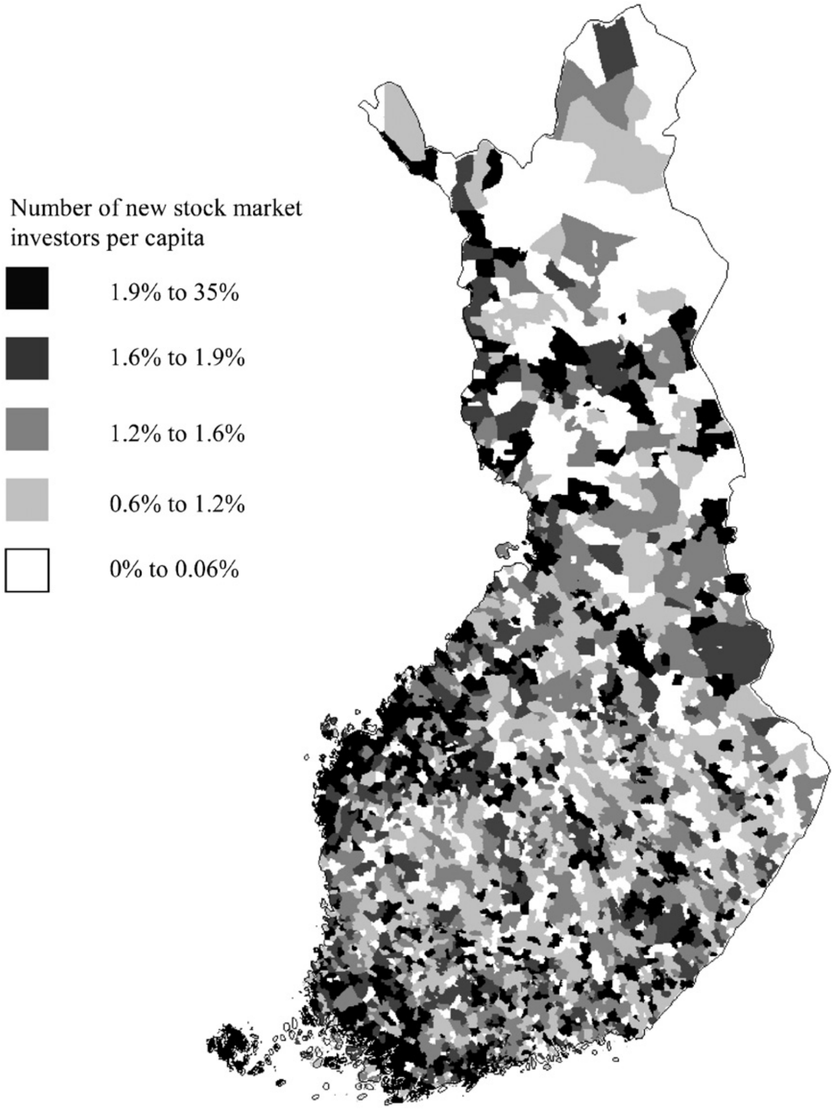

Fig. 2. Distribution of stock market entry rates across zip codes in Finland. This figure plots the stock market entry rates, that is, the number of new investors entering the stock market during the sample period of 1995 to 2002 divided by the total number of inhabitants at the end of 2000, across Finnish zip codes.

This approach introduces significant cross-sectional and temporal variation in our key variables of interest: neighborhood returns and entry rates. Our identification of the effect of experienced outcomes on neighborhood entry relies on the panel features of the data. Another important feature is our ability to unambiguously identify the shareholders and nonshareholders in an area at any given time. Because we lack register data on individuals who never became shareholders during the sample period, we aggregate the number of shareholders to zip codes, for which we have the total number of inhabitants available. This aggregation enables us to trace the changes in the exact number of participating and nonparticipating individuals between any two times.

We construct a panel of observations consisting of 2648 zip code areas and 93 calendar months. This panel data set is not typical in that it has an unusually large number of both areas and time periods. In estimating the models, we rely on methods developed in political science for analysis of political connections between countries over time. Beck and Katz (1995, 2004) show that such models can be estimated with simple ordinary least squares (OLS) techniques, and more complex techniques provide minor or no improvements.

The main regression model is

$$y_{it} = a + br_{i,t-1} + cp_{i,t-1} + u_{it}, \tag{1}$$

where the subscripts i and t refer to zip code areas and months, respectively. The dependent variable, y_{it}, is the stock market entry rate in a zip code in a month. We use two definitions of this variable. The first definition divides the number of new investors by the total number of inhabitants of a zip code. The second definition uses as the scaling variable the number of inhabitants who do not participate in the stock market at the beginning of the month. The neighborhood return $r_{i,t-1}$ is the equally weighted average of the portfolio return of all investors in a zip code in the preceding month. It is our main explanatory variable, and we expect it to be positively related to the stock market entry rate. We include stock market participation rate $p_{i,t-1}$ as a control variable to account for differences in entry rates that arise from differences in stock market participation. Areas with very high stock market participation rates tend to have lower entry rates by construction, as fewer new investors can enter the stock market. On the other hand, a larger pool of existing investors might allow more possibilities for social interaction, encouraging an even larger number of people to participate. The residual, u, absorbs the error term and fixed effects.

4.2. Standard errors

The error terms in our regressions are likely to be cross-sectionally correlated as social networks extend beyond zip code boundaries. That is, people can hear success stories also from their peers in adjacent zip codes. Such dependence is not necessarily eliminated by the inclusion of month fixed effects and would cause a bias in regular (unadjusted) standard errors (Petersen, 2009).

We address the cross-sectional dependence by implementing two alternative assumptions about the cross-sectional correlation. First, we assume that zip codes that are in the same province have correlated residuals in a month. There are 20 provinces in our sample, and an average of 132 zip codes per province. This assumption is consistent with our economic story in which the likelihood of social influence is geographically determined. Alternatively, we assume a more general (and conservative) form of correlation in which residuals are allowed to be arbitrarily correlated across the zip codes in a month.

We implement these correlation structures by allowing for clustering at the appropriate level, that is, at the province-month level in the first approach and at the monthly level in the second approach. We also experiment with a structure in which we add a second dimension to the clustering and assume clustering at the month as well as zip code level. This produces standard errors that are practically identical to those obtained with monthly clustering alone.

4.3. Unusual observations

The distribution of entry rates has a high skewness parameter of 9.4. Some unusually large observations seem to generate this pattern, highlighted by the maximum entry rate of 274 basis points (which is three hundred times the mean). Given these numbers, we must be careful in making sure that the unusual observations do not unduly influence the estimates.

The majority of the unusual observations comes from areas with a very small number of inhabitants. The following example illustrates the mechanism. Consider zip code A with a population of ten thousand and zip code B with a population of one hundred. Assume that the latent average monthly entry rate is one basis point in both areas. The realizations of the entry rates look very different from the latent rate, however. The minimum value of a strictly positive entry rate is 1/10,000 = 1 basis point for A. But for B it is 1/100 = 100 basis points. A small area thus generates a time series of observations with an extremely large variance and skewness.

We use three alternative procedures to make sure that the observations that appear to be outliers do not drive our results. We first discard observations for which the entry rate is above the 98th percentile (32.5 basis points). We also experiment with cutoffs of 90%, 95%, and 99%. Second, we run regressions that weight the observations by the inverse of the estimated variance of the zip code level entry rate, as in Greene (2008, p. 167). This regression gives little weight to the extreme observations and identifies the neighborhood return effect mainly from zip codes with less variable and less skewed entry rate distributions. Third, we run weighted regressions in which the weight is the number of inhabitants in a zip code. This approach is more restrictive as it does not address extreme values arising from large areas. All three procedures produce results that are qualitatively similar. We use the OLS specifications as the baseline due to their parsimony.

4.4. Identification

The empirical model attempts to capture social influence in which communication takes place between two groups of people living in the same area (stock market participants and nonparticipants). This structure, combined with the significant amount of temporal and cross-sectional variation, rules out alternative mechanisms based on reverse causality and common unobservables. First, consider reverse causality—the possibility that stock market entry in a particular area causes higher neighborhood returns, not vice versa. The introduction of new stock market participants could result in price pressure on stocks owned by existing investors in an area. Although this mechanism might affect the contemporaneous relation between entry and returns, it does not explain the relation between lagged returns and entry. We, therefore, rule out this alternative mechanism using the lagged neighborhood return as a regressor.

Common time-invariant unobservables might also generate a positive relation between neighborhood returns and entry. As an example, consider an area where residents are financially more sophisticated. Existing investors might enjoy high returns in such an area, and high-quality advice from their peers (rather than the returns) might encourage new investors to participate.[11] This scenario involves social interaction but not necessarily the outcome-based mechanism we posit. We

[11] As a group, individual investors are known to under-perform the market portfolio and would thus be better off investing in a passive market portfolio (Odean, 1999; Barber and Odean, 2000).

nevertheless eliminate this type of influence from our analysis, as the zip code fixed effects control for common time-invariant unobservables.

Common time-varying shocks might also produce a positive relation between local returns and entry. For example, high market returns are likely to be associated with increased visibility of stocks in the media, which could make some investors enter the market. We control for this possibility and any other market-wide time-varying influences by including month fixed effects in the analysis.

A remaining issue involves time-varying shocks that are unique to a particular zip code. The majority of these influences, such as changing prospects of the local economy, work mainly through the stock returns of local companies. Another potential story is one in which investors follow locally held companies and decide to participate after observing their good returns. Yet another, though perhaps less plausible, possibility is that local stock market wealth shocks generate spillovers to nonparticipants' wealth. Finally, household heads might purchase shares for other household members after experiencing high returns, or regional media coverage of the stock market might increase after high neighborhood returns.

Our empirical strategy for assessing these alternative explanations is, first, to use fixed effects for each province-month cluster in the regressions. The inclusion of province-month fixed effects eliminates the influence of shocks that work at the level of the 20 provinces in our sample. Such reasoning applies to local media coverage that should operate at the provincial level in our sample, as we explain in Section 6. Second, we rely on the rich cross section of data we have on different types of areas and investors. We identify subsamples in which the alternative explanations imply a weaker or a nonexistent neighborhood return effect. We also discuss this evidence in Section 6.

In this section, we have argued our empirical framework is immune to many of the econometric challenges that plague empirical studies of social interaction. In influential papers, Manski (1993, 2000) argues that identifying social interaction in most of the available data sets is difficult due to reverse causality, common unobservables, or common responses to shocks. We believe our data and method are exceptionally well suited to overcome these issues.

5. Results

This section first presents the results of the main analysis on the effect of neighborhood returns on stock market entry in the following month. It then discusses tests that utilize variation in opportunities for social learning. Finally, it presents results that speak to the relevance of two possible mechanisms underlying the main effect: naïve extrapolation and selective communication.

5.1. Past neighborhood returns and stock market entry

In each month and each zip code, we explain the stock market entry rate with the returns existing investors experience. Table 2, Panel A, reports descriptive statistics of the key variables in the regression. The average monthly stock market entry rate calculated over all zip codes and all months equals 0.9 basis points. The average return on the portfolio of existing investors equals 1.2%, and the mean stock market participation rate is 9.5%. Considerable variation in these numbers exists both across time and across zip codes. For example, half of the monthly zip code returns fall between –3.3% and 5.2%. In the majority of the zip code–month observations no new investors enter the market, whereas the maximum monthly entry rate equals 32.5 basis points. Panel B reports the cross-sectional standard deviation of neighborhood returns calculated across zip codes within each month and then averaged over all the months in the sample. This standard deviation equals 3.4%, so there is considerable heterogeneity in portfolio returns across zip codes at any time.

Panel C of Table 2 reports the results of Regression 1. Columns 1 and 2 use the population-scaled entry rate, and Columns 3 and 4 present results for the entry rate based on the number of non-investors. All specifications provide support for our main hypothesis, that is, that past peer outcomes influence individual entry decisions. In the full model, past neighborhood return enters the regression with a positive coefficient of 1.04. The *t*-value assuming clustering at the province-month level equals 4.3, and the corresponding statistic with an assumption of clustering at the month level is 2.0. The coefficient appears to be statistically significant even with the most conservative approach.[12]

We assess economic significance by providing marginal effects of the coefficients. Marginal effects are calculated as a change in the stock market entry rate resulting from a 1 standard deviation change in the neighborhood return. With a 1 standard deviation increase in the return, the increase in the stock market entry rate equals $0.08 \times 1.04 = 0.085$ basis points, which corresponds to a 9.4% relative increase from the mean entry rate. Given that the vast majority of the population never participates in the stock market (the average participation rate is 9.5%), the marginal effect appears to be large.

5.2. Variation in the opportunities for social learning

The social influence hypothesis makes a further testable prediction that we analyze in this section: The neighborhood return effect should be stronger in areas where people have more opportunities for social learning. Studies of the spreading of innovations find that diffusion is more rapid in populations in which the probability of becoming "infected" is higher (Griliches, 1957; Coleman, Katz and Menzel, 1966). We operationalize this prediction

[12] The baseline results discard observations for which the entry rate is above the 98th percentile (32.5 basis points), as explained in Section 4.3. We also apply weighted regressions to reduce the impact of outliers by weighting the observations by the inverse of the estimated variance of the entry rate or, alternatively, weighting by the number of inhabitants. These approaches produce coefficient estimates (and associated *t*-values) of 1.47 (1.88) and 0.59 (2.17), respectively.

Table 2
Past neighborhood returns and stock market entry.

This table shows the results of regressions of the number of investors entering the stock market in a month in a zip code. The dependent variable is the number of new investors entering the stock market divided by either the total number of inhabitants or the total number of inhabitants who do not participate in the stock market, and it is measured in basis points (bps). Neighborhood return is defined as the return on the portfolio of all investors in a zip code, calculated as the average portfolio return of investors living in a zip code. Participation rate is the number of stock market participants divided by the number of inhabitants in a zip code. The regressions include fixed effects for each province-month and each zip code. The regressions are estimated with ordinary least squares, and t-values are robust to clustering at the province-month level (in parentheses) or at the month level (in brackets). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: descriptive statistics

Variable	Mean	Sd	Minimum	25%	Median	75%	Maximum
Entry rate (bps)	0.90	2.72	0.00	0.00	0.00	0.00	32.51
Entry rate for non-investors (bps)	1.05	3.18	0.00	0.00	0.00	0.00	39.53
Neighborhood return (%)	1.18	8.13	−40.05	−3.31	0.94	5.20	99.92
Market return (%)	1.88	10.04	−26.88	−3.66	2.84	8.36	29.37
Participation rate (%)	9.47	6.59	0.00	5.41	7.91	11.24	62.18

Panel B: cross-sectional standard deviation of neighborhood return

	Mean	Standard deviation	Minimum	25%	Median	75%	Maximum
	3.41	1.81	0.99	2.14	2.94	4.00	9.47

Panel C: regressions

	Entry rate (bps)		Entry rate for non-investors (bps)	
Variable	(1)	(2)	(3)	(4)
Neighborhood return	1.056	1.042	1.190	1.190
	(4.38)***	(4.29)***	(4.14)***	(4.15)***
	[2.03]**	[1.98]**	[1.99]**	[1.98]**
Participation rate		−1.522		0.005
		(−3.24)***		(0.01)
		[−3.14]***		[0.01]
Month-province fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Number of zip codes	2,648	2,648	2,648	2,648
Number of observations	246,174	246,174	246,181	246,181
Overall R^2	0.119	0.125	0.121	0.130

by investigating how the neighborhood return effect varies as a function of the stock market participation rate in a neighborhood.

Considerable variation exists in participation rates across time and space, as shown by Table 2. For example, the participation rate in the bottom quartile ranges from the minimum of zero to 5.4%, and the rates in the top quartile range from 11.2% to 62.2%. We use both the cross-sectional and temporal variation in sorting the observations to the participation rate quartiles, using the lagged beginning-of-month participation rate as the sorting variable.

Table 3, Panel A, shows that the coefficient in the top quartile equals 1.72 (t-value 2.0), and in the bottom quartile it equals 0.64 (t-value 1.1). The coefficients in the second and third quartiles indicate that the impact of the neighborhood return increases monotonically with the participation rate. These results support the idea that areas with higher participation rates are associated with stronger social influences, a result also established in earlier studies of social interaction in the stock market (see Hong, Kubik and Stein, 2004).

We also sort the sample according to participation density, that is, the number of investors divided by the area of a zip code. These unreported results are similar to the estimates obtained using the participation rate, except for the top quartile that yields an insignificant coefficient (t-value 0.75). The disappearance of the effect in the top quartile is somewhat surprising. It might, however, reflect the lack of local social ties in urban communities that mostly populate the top quartile.

We also experiment with a related idea: The influence of peer outcomes should be more widespread when many investors in a neighborhood have experienced favorable outcomes. Accordingly, we have thus far used a neighborhood return measure that equally weights each investor in the area. Applying value weights instead, that is, weighting each investor's portfolio return with its proportional value in the neighborhood portfolio, should produce weaker results. Specification 1 in Table 4, which

Table 3
Participation rate interactions and impact of negative and positive returns.

Panel A estimates the model in Table 2 in each participation rate quartile using the total number of inhabitants in scaling the dependent variable (entry rate). The observations are sorted into participation rate quartiles using the beginning-of-month participation rates. Panel B estimates a piecewise linear model that employs a single change in the slope of neighborhood return at zero. The dependent variable is the number of new investors entering the stock market divided by either the total number of inhabitants or the total number of inhabitants who do not participate in the stock market. The regressions include fixed effects for each province-month and each zip code. The regressions are estimated with ordinary least squares, and *t*-values are robust to clustering at the province-month level (in parentheses) or at the month level (in brackets). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Participation rate interactions

	Participation rate			
Variable	Bottom quartile	Second quartile	Third quartile	Top quartile
Neighborhood return	0.641	0.688	1.385	1.717
	(1.74)*	(1.60)	(2.80)***	(3.65)***
	[1.12]	[0.94]	[1.89]*	[1.98]**
Participation rate	−0.419	−6.897	−20.032	−1.538
	(0.24)	(2.40)**	(8.30)***	(2.20)**
	[0.14]	[1.30]	[5.53]***	[2.49]**
Month-province fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Number of zip codes	1,190	1,535	1,440	1,050
Number of observations	61,158	61,672	61,694	61,650
Overall R^2	0.089	0.098	0.113	0.135

Panel B: Regressions employing a change in the slope at zero

P	Entry rate (bps)	Entry rate for non-investors (bps)
Variable	(1)	(2)
Max (Neighborhood return, 0)	1.434	1.653
	(4.65)***	(4.55)***
	[2.44]**	[2.50]**
Min (Neighborhood return, 0)	0.302	0.318
	(0.69)	(0.64)
	[0.29]	[0.27]
Participation rate	−1.634	−0.128
	(−3.32)***	(−0.20)
	[−3.28]***	[−0.22]
Province-month fixed effects	Yes	Yes
Zip code fixed effects	Yes	Yes
Number of zip codes	2,648	2,648
Number of observations	246,174	246,181
Overall R^2	0.131	0.138

includes other additional results and robustness checks that we discuss later, reports this result. The coefficient on the value-weighted return equals 0.56 (month-clustered *t*-value 2.0), which is about 45% smaller than the baseline coefficient that uses equally weighted returns. This supports the idea that many investors experiencing good outcomes matters over and above the aggregate performance of the neighborhood portfolio.

5.3. Naïve extrapolation and selective communication

Section 2 outlines two broad channels through which peer outcomes could influence stock market entry. Under naïve extrapolation, return expectations of potential new investors adapt to the outcomes their peers experience. Under selective communication, communication that is more likely after good outcomes reveals the participation status of peers, which triggers relative wealth concerns. The stories are not mutually exclusive (naïve extrapolation could occur after selective communication), but analyzing how the neighborhood return effect varies in the region of good and bad outcomes can test for the presence of selective communication. In defining good and bad outcomes, we look at returns above and below zero, a reference point investors commonly use when defining gains and losses.

In Table 3, we estimate a piecewise linear model in which we break down the neighborhood return into two variables that separately capture the slope estimates for positive and negative returns. The results show that the effect on entry rates comes exclusively from positive returns. The coefficient

Table 4
Additional tests and robustness checks.

This table reports additional tests and robustness checks that modify the baseline regression in Table 2. Specification 1 in Panel A weights the individual portfolios within a neighborhood with portfolio value instead of giving them equal weights. Specification 2 estimates the model in a subsample of individuals of the same age and gender, that is, males born between 1948 and 1968. Specifications 3 through 6 interact the neighborhood return variable with an indicator for municipalities with no local stocks, zip codes in which the distance to a local stock is less than 30 km (km), zip codes with above median homeownership rates, and zip codes with above median ratios of self-employed people. The *t*-values are robust to clustering at the province-month level (in parentheses) or at the month level (in brackets). Panel B reports the results of a Tobit regression that assumes censoring at entry rate of zero. Many province-month blocks have no variation in the entry rate across zip codes, which would require dropping a considerable number of the observations, as required by the Tobit model. Province-month fixed effects are thus replaced with month fixed effects. Zip code fixed effects are included as in the baseline analysis. The model is estimated using unconditional fixed effects for zip codes and months as in Greene (2004). The *t*-values are robust to clustering at the month level (in brackets). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: interactions and robustness checks

	Neighborhood return		Interaction variable		Participation rate		Number of observations
Specification	Coeff.	*t*-value	Coeff.	*t*-value	Coeff.	*t*-value	
(1) Value-weighted return	0.560	(3.71)*** [2.00]**					246,174
(2) Males born between 1948 and 1968	0.230	(4.07)*** [1.91]*			−3.441	(11.02)*** [9.33]***	241,292
(3) No local stocks	1.042	(2.67)*** [1.12]	0.024	(0.08) [0.02]	−1.522	(3.15)*** [3.27]***	246,174
(4) Distance to local stocks less than 30 km	1.021	(3.28)*** [1.52]	0.040	(0.17) [0.09]	−1.524	(3.15)*** [3.28]***	246,174
(5) Above median homeownership	0.980	(3.62)*** [1.45]	0.190	(0.83) [0.25]	−1.516	(3.13)*** [3.19]***	246,174
(6) Above median self-employment	1.007	(4.17)*** [2.00]**	0.133	(0.94) [0.87]	−1.518	(3.13)*** [3.23]***	246,174

Panel B: Tobit regressions

	Maximum of (Neighborhood return, 0)		Minimum of (Neighborhood return, 0)		Participation rate		Number of observations
Specification	Coeff.	*t*-value	Coeff.	*t*-value	Coeff.	*t*-value	
(7) Tobit assuming censoring at zero	3.452	[1.59]	0.094	[0.02]	−3.616	[2.87]***	234,038

on negative returns is statistically insignificant and small in magnitude, while the slope of the positive returns equals 1.43 (month-clustered *t*-value 2.4), about 40% greater than the coefficient in the baseline analysis.

Fig. 3 shows a graphical illustration of the impacts of negative and positive peer returns. We divide the neighborhood return into categories representing 5 percentage point intervals between −10% and 15%, as well as categories for returns less than −10% and in excess of 15%. We assign the categories dummy variables that replace the neighborhood return variable in a regression similar to the full model in Table 2, omitting the 0–5% category.

The graph, which plots the coefficient estimates, confirms the results from the piecewise linear specification that employed a single change in the slope estimate at zero. The graph further shows that the influence of peer returns is particularly strong for returns in excess of 15%. They increase the entry rate by 0.30 basis points.

These results are consistent with selective communication. That is, people are more likely to discuss their stock market experiences with others when the experiences have been favorable, maybe because appearing to be a successful investor carries private benefits. Self-serving bias in recall and attention can also cause people to talk about their investments more when performance is good.

The asymmetry could also be due to short selling constraints. Under this alternative explanation, people would be equally likely to discuss their stock market performance with their peers regardless of the sign of the return. Given short sale restrictions, the prospective new investors could not easily act on the negative information and would continue to stay out of the market. This mechanism would produce behavior consistent with our findings.

Short selling was relatively difficult for individual investors and, thus, was rare during our sample period. This hypothesis nevertheless faces difficulty in accounting for the asymmetric relation we find. A strong upward trend in stock market participation exists in our sample, similar to many other countries, making the unconditional entry rate per month positive (0.9 basis points). Therefore, the monthly entry rate could come down to the extent that the prospective new investors are affected also by their peers' negative experiences. The positive unconditional entry rate should thus allow a significant coefficient also in the region of negative returns, a result we do not find.

One could still wonder whether the magnitude of the unconditional entry rate is large enough to allow for the entry rate to drop when peer returns have been negative. We investigate this possibility by running a Tobit model that explicitly accounts for the fact that we do not observe

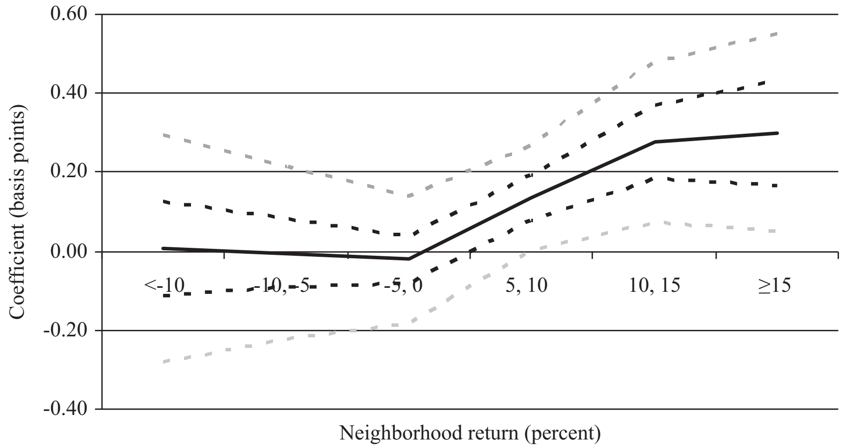

Fig. 3. Influence of neighborhood returns on stock market entry. This figure plots coefficients of dummy variables that each represents a 5% interval of the neighborhood return (0–5% omitted). The dummy variables replace the neighborhood return variable in a regression similar to that in column 2 of Table 2. The black solid line plots the coefficients on the dummy variables, and the dotted lines show the upper and lower bounds of the confidence intervals at the 5% significance level. The black dotted line comes from the standard errors clustered at the province-month level; the gray dotted line from clustering at the month level. The coefficient values indicate, for each return category, the absolute change in the monthly entry rate from the omitted category in basis points.

negative entry rates. A general drawback of the Tobit model with fixed effects is the incidental parameters problem that occurs when the number of observations within a group is small. Fortunately, in our case, this number exceeds the bounds (five or more) derived in Greene (2004), and we thus proceed with estimating the model with unconditional fixed effects, assuming censoring at zero. However, although the incidental parameters problem does not have an impact on the coefficient estimates, censoring deflates the standard errors by an unknown quantity (Greene, 2004).

The model is estimated using month fixed effects instead of province-month fixed effects as the latter approach would mean dropping a large number of observations. The exclusion of the observations follows from the requirement that, in a Tobit model, some variation must exist within the fixed effect block. Even with month fixed effects the number of observations is somewhat lower than in the baseline analysis.

Table 4, Panel B, reports the results of the Tobit regression. The coefficient on the positive return equals 3.45 (t-value 1.59), and the coefficient on the negative return is 0.09 (t-value 0.02). The larger coefficient estimate for the positive region is consistent with the argument that short sales constraints do not explain the asymmetric relation. However, this model lacks the statistical power needed to formally reject the equality of the two coefficients.

6. Alternative explanations

This section presents analysis that addresses various alternative explanations, and discusses results from further robustness checks.

6.1. Purchases within households

Household heads purchasing shares for other members of the household after experiencing positive returns could drive our results. Such a mechanism does not necessarily involve any social influence. We address this possibility by relating individuals' entry decisions to outcomes of individuals who are not likely to be members of the same household but live in the same zip code. People who are old enough to have moved from their childhood home and who are of the same gender and same age only rarely live in the same household, so we analyze the influence of peer returns within this subsample of investors. Because most investors are male, we choose the men in the sample. We choose the maximum birth year for this analysis to be 1968, that is, the youngest investors are 30 years old in the middle of the sample period. The oldest investors are born in 1948, that is, they are less than 20 years older than the youngest investors in the subsample. We calculate neighborhood returns and entry rates for each zip code and each month using data on these investors.

Specification 2 in Table 4 shows that, for males born between 1948 and 1968, the coefficient estimate equals 0.23 (month-clustered t-value 1.9). One should not directly compare the size of the coefficient with that of the baseline coefficient, as the unconditional entry rates in the two samples are very different. Instead, the marginal effect relative to the mean entry rate provides a meaningful point of comparison. A 1 standard deviation increase in the return increases the entry rate from its average value of 0.25% by 8.0%, which is comparable to the baseline estimates. These results provide reassurance that purchases within households do not fully generate the neighborhood return effect.[13]

6.2. Local wealth shocks

If neighborhood returns are positively correlated with changes in the wealth of individuals who do not participate

[13] Unreported analysis shows that this marginal effect is similar in a subsample of all men. The marginal effect is considerably smaller, 4.1%, for female investors.

in the stock market, some of these individuals could enter the stock market after high neighborhood returns, due to either changes in risk aversion or lower per-period costs of participation (Abel, 2001; Vissing-Jørgensen, 2003; Brunnermeier and Nagel, 2008). Here the positive relation between neighborhood returns and market entry would arise from the local shock to nonparticipants' wealth and would not be due to social influence.

Either local stocks or stock market wealth effects could create a channel through which local stock market wealth shocks could influence entry. Changes in the prospects of the local economy could influence both the returns on local stocks (which are predominantly held by local investors; see Coval and Moskowitz (1999), Huberman (2001), and Grinblatt and Keloharju (2001)) and the wealth of the nonparticipants through higher demand for local products and services or higher salaries and bonuses paid to employees of local companies.

We put together several pieces of evidence that collectively speak against local wealth shocks. We first run the baseline analysis (as in Table 2) but now add a variable that interacts the neighborhood return with an indicator that takes the value of one if there are no local companies with listed stocks in the municipality. We use municipalities (that include on average four zip codes) here as they are administrative units that better capture local economic areas. The majority of our sample, about 80%, comes from municipalities with no local stocks.

The interaction variable in Specification 3 in Table 4 enters the regression with a statistically insignificant coefficient of 0.08 (t-value 0.02). This finding supports the conjecture that the neighborhood return effects are similar in areas with and without local stocks. The main effect remains identical to the baseline coefficient but becomes statistically insignificant. This is perhaps not surprising as the interaction variable draws some of the identifying variation away from the main effect. When interpreting the other interaction analyses in Table 4 we also put more emphasis on the sign and size of the coefficient than on statistical significance.

Municipalities without any local stocks can be surrounded by municipalities that do have local stocks. Economic shocks could spill over to the nearby municipalities and also have an impact on portfolio returns in a municipality with no local stocks. We address this concern by indicating observations for which there are no local stocks within a given radius from the population-weighted centroid of the zip code. The average distance from a zip code that is located in a municipality with no local stocks to a zip code that does have a local stock equals 55.3 km (km), and the minimum distance is 3.6 km. We define an interaction variable that now takes the value of one if there are no local stocks within 30 km and zero otherwise. About 55% of the sample comes from such areas.

Specification 4 in Table 4 reports the results of this regression, showing a small and statistically insignificant coefficient for the interaction variable. In addition to the 30 km radius we use radii of 50, 100, and 200 km and find similar results (not reported). These estimates suggest that it is unlikely that local wealth shocks operating through local stocks explain our results.

Even in the absence of local stocks, changes in stock market wealth could influence the real economy through wealth effects from changes in stock market wealth to stockholders' consumption (Poterba, 2000). If stockholders increase their consumption after observing positive returns on their portfolios, the effect could generate a spillover to the local economy and, ultimately, to the wealth of nonparticipants.

How much individuals would alter their consumption in response to month-to-month fluctuations in stock prices is unclear. Even with longer estimation periods than ours, microdata-based empirical evidence on stock market wealth effects is mixed and many of the estimates are insignificant. The estimates are especially small for households that do not participate in the stock market (Starr-McCluer, 2002; Dynan and Maki, 2001).

Even less evidence is available to guide our expectations about the speed at which a wealth shock might propagate from the stock market to nonparticipants' wealth. In unreported analysis, we find that lagged returns beyond the past two months are neither statistically nor economically significantly related to stock market entry, which is consistent with social influence but perhaps difficult to reconcile with wealth effects. We also noted that negative neighborhood returns do not discourage entry. Wealth effects have a hard time explaining this result, whereas it is naturally related to social influence and communication.

We provide additional results on wealth effects by investigating how the neighborhood return effect relates to the composition of wealth and income in an area. Wealth effects from the stock market to nonparticipants' wealth can operate through either housing wealth or income and should thus be higher in areas where housing wealth represents a larger proportion of wealth and in areas where income is more closely tied to the local economy. We implement these ideas by looking at how the neighborhood return effect varies in areas with a high proportion of homeowners and a high proportion of self-employed people (defined here as the proportion of entrepreneurs).

We run the baseline regressions augmented with an explanatory variable that is an interaction between the neighborhood return and an indicator for higher-than-median homeownership (row 5 in Table 4) or self-employment (row 6 in Table 4). Table 1, Panel A, reports that the medians for homeownership and self-employment equal 77.3% and 3.4% and that these variables have an interquartile range of 19.0% and 1.6%, respectively. The variation in homeownership is likely large enough to produce a meaningful sorting into high and low homeownership areas, but this might not be the case with self-employment. The results in Table 4 nevertheless show that the interaction terms in both regressions, one for homeownership and the other for self-employment, are small in magnitude and statistically indistinguishable from zero. These results suggest that the neighborhood return effect is not confined to areas with high homeownership and self-employment rates, which goes against the predictions of the wealth effect story. Taken together, we believe local wealth shocks are not driving the relation between neighborhood returns and stock market entry.

Table 5
Past neighborhood returns and stock market entry through initial public offerings.

This table reports the results of the regressions on the number of investors entering the stock market through an IPO. The dependent variable is the number of new investors entering the stock market through an IPO divided by either the total number of inhabitants or the total number of inhabitants who do not participate in the stock market, and it is measured in basis points (bps). The sample contains 57 IPOs. Independent variables are from the beginning of the subscription period of an IPO. Neighborhood return is the return on the portfolio of all investors in a zip code, calculated as the average portfolio return of investors living in a zip code. Participation rate is the number of stock market participants divided by the number of inhabitants in a zip code. The regressions are estimated with ordinary least squares, and include IPO-province and monthly fixed effects. *t*-values are robust to clustering at the province-IPO level (in parentheses) or at the IPO level (in brackets). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: descriptive statistics

Variable	Mean	Sd	Minimum	25%	Median	75%	Maximum
Entry rate (bps)	0.61	2.93	0.00	0.00	0.00	0.00	27.01
Entry rate for non-investors (bps)	0.70	3.40	0.00	0.00	0.00	0.00	51.15
Neighborhood return (%)	2.22	8.94	−43.10	−3.09	1.50	8.31	163.45
Participation rate (%)	9.58	6.68	0.25	5.42	7.96	11.38	51.20

Panel B: regressions

	Entry rate (bps)		Entry rate for non-investors (bpp)	
Variable	(1)	(2)	(3)	(4)
Neighborhood return	1.834	1.795	1.852	1.805
	(2.57)**	(2.47)**	(2.33)**	(2.22)**
	[0.70]	[0.67]	[0.64]	[0.61]
Participation rate		0.855		1.689
		(0.64)		(1.03)
		[0.47]		[0.75]
IPO-province fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Number of zip codes	2,648	2,648	2,648	2,648
Number of observations	148,713	148,713	151,448	151,448
Overall R^2	0.284	0.284	0.279	0.281

6.3. *Locally held stocks*

If prospective investors more closely follow stocks existing investors tend to hold, they could buy these stocks after seeing them produce high returns. Given that differences in local ownership generate the observed cross-sectional variation in neighborhood returns, cases in which local stocks have been performing well are likely to coincide with a high return on the corresponding neighborhood portfolio. New investors would thus enter the stock market after observing good performance of the locally held stocks, irrespective of observing any outcomes of their neighbor's investments.

We investigate if following of local stocks explains our results by analyzing market entry in stocks with no prior history, namely, initial public offerings (IPO). These stocks are not part of any local portfolio, and following their performance prior to the listing is impossible. If prospective investors follow only stocks the existing investors hold, past returns on them should not influence market entry via IPO stocks.

In Table 5, we perform an analysis of the demand for IPOs. The sample is a time-zip code panel with the full set of zip codes we used in the main analysis. We now define time as the starting date of the subscription period of an IPO. Details of the sample IPOs and their characteristics appear in Kaustia and Knüpfer (2008). The dependent variable in the regression is the number of new investors participating in a particular IPO in a zip code divided by the total number of inhabitants of the zip code. Independent variables are similar to those in Table 2, and we measure them at the beginning of the subscription period of each IPO. We measure the neighborhood return from a period of 30 days before the beginning of the subscription period. We estimate the regressions by including zip code fixed effects. We control for time effects with province-IPO fixed effects, which pick up the influence of market returns, other contemporaneous effects, and time-varying shocks influencing a province at a particular point in time.[14]

The results show that recent neighborhood returns influence the likelihood of entering the stock market via an IPO. The coefficient estimate 1.80 translates into a 26.5% increase in the entry rate $[(0.61+1.80\times 0.089)/0.61-1]$, which is larger than obtained in the baseline analysis.

[14] An alternative would be to use month-province fixed effects as in the main analysis. Such models yield coefficient estimates that are about two times the size of the estimates that we report as the baseline results. We choose to err on the side of caution and report results using IPO-province fixed effects.

The t-values assuming clustering at the month level are statistically insignificant, while at the province-month level they remain significant. This discrepancy is likely an artifact of the small number of IPOs. The magnitude of the coefficient, however, is large enough to cast serious doubt on the idea that a tendency to follow locally held stocks would drive our results.

Besides providing a possibility to address an important alternative explanation, the results on IPOs make an independent contribution to the IPO literature. Derrien (2005) and Ljungqvist, Nanda, and Singh (2006) model the impact of investor sentiment on IPO demand and pricing patterns. Empirical studies find that these demand and pricing patterns are correlated with measures of investor sentiment (Lee, Shleifer and Thaler, 1991; Rajan and Servaes, 1997; Lowry, 2003; Cornelli, Goldreich and Ljungqvist, 2006). Kaustia and Knüpfer (2008) argue that investors' past personal experiences with IPOs provide a microfoundation for the role of sentiment in IPO demand. The results of this paper show how the positive stock market experiences of peers influence the inexperienced investors' IPO demand. Existing investors' good performance thus introduces a positive externality in the form of new investors, something the issuers and their investment banks are likely to value (Cook, Kieschnick and Van Ness, 2006).

6.4. Local media coverage

Neighborhood returns might correlate positively with the extent of media coverage on stocks. A local journalist might, for example, decide to write a story promoting stock market investment after experiencing good returns.

The institutional characteristics of the Finnish media industry suggest such a mechanism is not likely to drive our results. Finland has practically no local TV stations. Instead, four national TV channels are followed throughout the country. Having only month fixed effects in our regressions would thus absorb the shocks in TV coverage. One national newspaper dominates the newspaper market, but several smaller newspapers do exist. The market areas of the newspapers that regularly cover business news follow provincial borders; that is, each of the 20 provinces typically has its own newspaper. As for radio stations, the national broadcasting company has one station in each of the provinces.

The geographical segmentation of the media market suggests shocks to media coverage should operate at the level of provinces. The baseline regression reported in Table 2 includes province-month fixed effects and thus identifies the neighborhood return effect solely from the variation between zip codes in a particular province in a particular month. Because the coefficient estimates are large in magnitude and statistically significant, we believe news coverage is an unlikely driver of the results.

7. Conclusion

The returns the existing investors in a neighborhood experience in a given month encourage new investors to enter the market the following month. This neighborhood return effect is asymmetric: only positive returns are related to entry. These results are consistent with a type of social influence in which the stories of positive outcomes from stock market investing make people more likely to enter the market.

The strength of the neighborhood return effect increases monotonically with the level of stock market participation in an area. Given that even the highest participation rates in Finland in the late 1990s were moderate by today's international standards, the results are likely to be stronger in populations with more widespread stock market participation.

We have outlined two channels through which peer outcomes could have an impact on individual actions—extrapolative expectations and selective communication with relative wealth concerns—but have so far remained agnostic about their explanatory power. As a side product of one of our robustness checks, we find that the neighborhood return effects are practically invariant to the regional levels of homeownership and self-employment. Although these tests are by no means decisive, they suggest the results could be difficult to reconcile solely with relative wealth concerns, as we should expect them to be the strongest in areas where income and wealth are tied more to the local economy.

Extrapolative beliefs influenced by success stories of peers can explain how financial innovations and investment styles spread. For example, a local tilt toward certain types of stocks could partially reflect good experiences of local investors in these stocks, irrespective of the location of the firms' headquarters. Peer experiences could also influence the adoption of specific approaches to trading, such as a high-turnover day trading style. These questions are potential topics for future research. Extrapolative beliefs could also contribute to asset price bubbles, especially in markets with limits to arbitrage (see Scheinkman and Xiong, 2004; Hong and Stein, 2007). A prime example of such a market is the housing market in which an analysis along the lines of our paper would be interesting. Extrapolation from others' outcomes can also play a part in explaining the success of Ponzi-type securities scams and other frauds, as favorable peer outcomes can persuade individuals to overcome their initial suspicions.

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