How Do Investment Behaviors Transmit Across Generations? The Effects of Parents and Neighborhoods*

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Abstract

Financial outcomes are known to be highly persistent across generations. However, less is known about the exact process through which specific investment behaviors develop based on features of one's childhood. Using Danish administrative data and two quasi-experimental designs, we find a significant effect of childhood exposure to the behaviors of one's own parents, and of the parents of one's peers, on a child's investment behaviors in adulthood. We perform tests to shed light on the underlying structure of the "production function" of investment behaviors and find evidence of substitutability of exposure to own-parents and peer-parents. In exploring mechanisms, we find that direct transfers of public equity and the sharing of financial intermediaries cannot fully explain the parent-child transmission, suggesting that other channels, such as risk preferences or financial savvy, play a role.

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1 Introduction

Extensive research in economics and finance has documented and sought to explain the cross-sectional heterogeneity in stock market participation and investment behaviors.[1] Recent work exploiting intergenerational linkages in administrative datasets has shown that wealth and asset holdings are correlated within the same family, across generations (Black et al. (2017), Fagereng et al. (2021), and Knüpfer et al. (2023)). However, much less is known about the exact process through which investment behaviors transmit from parents to their children. Is this transmission driven by inherited risk preferences, direct transfers of wealth, or the sharing of financial intermediaries? Do children inherit investment behaviors from their own parents alone, or also from the parents of their peers, and are there complementarities in exposure to different adults in the child's household and neighborhood? Whether children emulate specific investment behaviors, or whether a small set of parental characteristics jointly shape multiple investment behaviors, is also underexplored.

This paper exploits Danish registry data to document the population-level correlation of investment behaviors between parents and their children, and to quasi-experimentally isolate the causal effects of parental and neighborhood exposure during childhood on investment behaviors in adulthood. We use a novel combination of quasi-experimental designs, building on methods from Bakker et al. (2026) and Chetty and Hendren (2018). First, using plausibly exogenous variation in the timing of parental separations, we show that an extra year of exposure to a parent who participates in the stock market while the child is at home causally induces a 0.266 percentage point increase in the child's probability of stock market participation in adulthood. This effect is economically meaningful, as it indicates that spending the entirety of one's childhood (18 years) with a parent who participates in the stock market increases the child's probability of participating as an adult by about 4.8 percentage points. This is equivalent to a 15% increase from the baseline participation rate.

Next, we isolate transmission specific to parents from transmission related to environment by estimating the causal effect of being exposed to the *parent of a peer* who invests in the stock market, using a movers design that builds on Chetty and Hendren (2018) and Chetty et al. (2026). We show that children who move between Danish municipalities sharply converge to the average participation probability of parents of peers in their destination (relative to origin) municipality, with the effect size declining in the age of move. Combined, these two quasi-experimental designs show that exposure to both one's own parents and the parents of one's peers during childhood affects the development of investment behaviors.

Given this evidence of both own-parent and peer-parent transmission, we next study how these two channels interact. Combining our two quasi-experimental designs, we find evidence against the existence of complementarities in exposure to own-parents and peer-parents: a child with sufficient exposure to stock market participation from one channel gains less from exposure to the other channel. The result in our age-at-separation design with variation in municipality-level participation suggests an even stronger claim: that own-parents and peer-parents are substitutes.

[1] See Gomes et al. (2021) and Campbell and Ramadorai (2026) for recent reviews of this literature.

Having demonstrated these reduced form effects, we next explore the mechanisms underlying the parent-to-child transmission. First, we estimate the multivariate relationships between a broad set of parent and child investment behaviors: the share of liquid wealth invested in public equity (the "risky share"), the share of income held in savings, a measure of pension savings, homeownership, investment in funds, and holding bonds. The resulting matrix of child-on-parent investment behavior coefficients contains a strong diagonal, suggesting that children inherit or copy *specific* investment behaviors from their parents.

Finally, we re-run our age-at-separation quasi-experimental design on various subsamples: the subsets of children (1) without public equity in their name before age 18, (2) without a shared bank branch with their parents, and (3) without a parent who has received a degree in economics or finance. We find that neither direct transmission of public equity nor the sharing of financial intermediaries can fully explain the intergenerational transmission of stock market participation. The transmission also remains large and positive for children whose parents lack an economics or finance education, which we acknowledge is an imperfect measure of financial sophistication. These results suggest a strong role for the inheritance or copying of unobservable risk preferences or financial savvy—neither of which is perfectly measurable—in the transmission of investment behaviors from parents to children.

This paper makes three contributions to the literature. Our main contribution is that we provide a comprehensive picture of the effect of exposure to one's own parents, one's peers' parents, and the interaction of the two on the development of investment behaviors, using distinct quasi-experimental designs that disentangle these two mechanisms. We additionally decompose the parental transmission effect into channels related to direct transfers of public equity, the sharing of financial institutions, transfers of formal financial sophistication, and a residual. Second, unlike existing literature on the intergenerational transmission of wealth and behaviors that uses *contemporaneous* parent and child outcomes, we specifically focus on events and factors during childhood that influence investment behaviors in adulthood. Thus, our results speak to the underlying process through which investment behaviors take shape in the first place, which is not well-understood. Finally, our paper provides population-level estimates of the transmission of specific investment behaviors from Danish parents to their children.

Related Literature This paper relates to an established and active literature studying the transmission of finances and behaviors across generations. Foundational work includes Becker and Tomes (1979) and Becker and Tomes (1986), which model the transmission of income and human capital from parents to children. The advent of large administrative datasets in recent decades has allowed economists to empirically estimate population-level measures of *income* mobility, with notable examples in the U.S. context including Chetty et al. (2014), Chetty et al. (2026), and other work by the same authors. The intergenerational transmission of *wealth* has received slightly less attention in the U.S. context due to data limitations. Granular administrative data coupled with the historical existence of a wealth tax in Denmark and other Scandinavian countries enables detailed analyses of wealth transmission in these contexts.

Our paper bridges two strands of the literature on intergenerational transmission: one studying the transmission of *wealth*—including Charles and Hurst (2003), De Nardi (2004), Boserup et al. (2018), and Daysal et al. (2022)[2]—and the other studying the transmission of risk-related *preferences*—such as Dohmen et al. (2012), Almenberg et al. (2021), and Ebrahimian and Sodini (2024), where the latter uses a structural model to estimate preference parameters. We contribute by quasi-experimentally estimating the intergenerational transmission of specific investment behaviors—including share of assets held in stocks, an indicator for owning multiple properties, and contributions to a pension fund, among others—and by characterizing the ways in which specific behaviors are transmitted via exposure during childhood. Our paper thus helps uncover the behavioral mechanisms underlying the transmission of wealth across generations and also speaks to the "participation puzzle": a well-known empirical fact that surprisingly few individuals participate in the stock market (see e.g. Haliassos and Bertaut (1995)).

The paper closest to ours—and to which we attribute inspiration for our quasi-experimental designs—is Bakker et al. (2026), which shows that revolving credit card debt is a behavior that transmits from parents to kids. While we adopt key empirical methods from Bakker et al. (2026), our paper is aimed at answering a distinct question: we trace out the development of investment behaviors based on features of one's childhood. With two different tests of complementarities between own-parent and peer-parent exposure, along with several tests of the mechanisms underlying own-parent transmission, we shed light on the form of the "production function" of investment behaviors. Additionally, the richness of our data allows us to use parent behaviors while the child was young—rather than contemporaneous measures—as treatments, and to explore a broad range of investment behaviors that are not observable in the U.S. context studied in Bakker et al. (2026).

Existing work that studies how investment behaviors are causally transferred from parents to their children relies on samples of adopted children to isolate the effects of nature versus nurture.[3] Fagereng et al. (2021) use the quasi-random assignment of Korean adoptees to Norwegian families to isolate the role of environment from that of genetics; they find environmental factors to be important. Black et al. (2017) conduct a similar analysis with Swedish adoptees and find a role for both genetics and environment. Common to these studies is the potential for selection of adopting parents—who are often pointed out to be older, wealthier, and more educated—and of adopted children. Our paper uses two quasi-experimental designs that are new in this context and that allow for causal estimates with larger and potentially more representative samples. Furthermore, our two designs shed light on a new angle of the "nature versus nurture" distinction by isolating transmission that occurs through exposure to one's own parents versus to the parents of one's peers.

Another strand of literature that relates to ours explores the relationship between *contemporaneous* child and parent investment behaviors (such as Knüpfer et al. (2023)), or between the behaviors of an

[2]Other examples include Daysal et al. (2023), who focus specifically on housing wealth.

[3]Other designs include studies of twins, such as Cesarini et al. (2010). Existing work that uses a twins design is typically focused on the role of genetics, whereas our paper documents the importance of several distinct factors during childhood and explores complementarities.

individual and other family members or partners (such as Christensen and Olufsen (2025)). Our paper is distinct from this work in that it explores how parental and neighborhood exposure during childhood affects investment behaviors in adulthood, which speaks to the underlying process by which investment behaviors initially develop. Furthermore, our use of parental investment behaviors when the child is young, rather than contemporaneous measures, helps address econometric issues related to common shocks to finances.

Our quasi-experimental movers design relates to a separate strand of literature studying the effect of peers on various behaviors.[4] In the particular context of financial behaviors, Hong et al. (2004) show that households that are more "social" are also more likely to participate in the stock market, conditional on controls. Duflo and Saez (2003), Hvide and Östberg (2015), and Ouimet and Tate (2020) show that social interactions with colleagues at work influence retirement plan participation, stock purchase decisions, and participation and trading decisions, respectively, and Haliassos et al. (2020) find that exposure to financially literate neighbors increases both stockholding and retirement saving via the transfer of financial knowledge.[5] While our paper speaks to this literature on peer effects, we answer a different question that specifically addresses the effects of peer-parent exposure *during childhood* on investment behaviors in adulthood, and we provide new evidence on the lack of complementarities between own- and peer-parent exposure.

2 Institutional Background and Data

This paper uses administrative register data covering the universe of the Danish population from 1985 to 2025. Our analysis samples include children born in 1976 and later.[6]

The data are collected by government agencies for administrative purposes—taxation, civil registration, and the administration of public services—and are made available for research in anonymized form through Statistics Denmark. All registers are linked at the individual level through the personal identification number (cpr-number) assigned to every Danish resident at birth or immigration since 1968. This allows us to follow individuals over time, across registers, and across generations. Importantly, because the data are acquired predominantly through administrative reporting rather than surveys or self-reporting, there is less concern about attrition or self-reporting error, which is important for studies of household portfolios (Calvet et al. (2007)).

Demographic information comes from the population register (BEF), which is based on the Central Person Register (CPR) maintained by the civil registration authorities. It records each individual's date of birth, sex, municipality of residence, and identifiers of the individual's legal parents, which we use to construct parent–child links.

Income and wealth information comes from the records of the Danish tax authorities (SKAT). The

[4] See Kuchler and Stroebel (2021) for a review of the peer effects literature.
[5] See also Kaustia and Knüpfer (2012), who find that social learning is an important channel through which peer effects matter for financial behaviors.
[6] Each analysis sample imposes additional cohort restrictions such that the required outcome windows lie within the years of data.

income register (IND) contains third-party-reported information on labor income, transfers, and capital income for every taxpayer since 1980, where employers report wages, banks report interest and account balances, and financial institutions report securities holdings, directly to the tax authority. The same third-party reports underlie the year-end wealth components we observe through the personal wealth register (FORMPERS)—bank deposits, market values of stocks and bonds, housing assessments, and liabilities. From 2014 onward, we supplement the values obtained from the older income register with the personal wealth register, which provides a finer decomposition of financial assets.[7] All wealth variables are observed separately at market value on December 31.[8]

Education information comes from the education register (UDDA), compiled by Statistics Denmark from reports that educational institutions submit directly to the student register. We map Danish educational degrees into ISCED levels, UNESCO's International Standard Classification of Education, which orders qualifications on a common scale from primary schooling to doctoral degrees. We measure education as the highest completed level and classify it into fields.

Finally, information on banking relationships comes from the register of account-specific data on deposit interest for individuals (IRTEPERS), compiled by Statistics Denmark from reports that banks have to submit to the tax authorities for every deposit account held by an individual. We use the account holder's personal identifier and the registration number of the reporting bank branch to identify bank relationships.

3 Descriptive Statistics

Table 1 summarizes the demographic and financial characteristics of our full sample of children and parents. The children in our sample were on average born around 1985, have 1-2 siblings, and are fairly dispersed across Danish municipalities (with only 16% living in the four largest municipalities). Baseline stock market participation is in general fairly low (consistent with the literature), with just under 50% of parents ever participating while the child is ages 0-17, and about 30% of children ever participating in ages 18-30.

[7] We tested that the old and new definitions of stock market participation and the risky share are consistent with each other: in the years of overlap (2014 onward), the correlation between the old and new definitions exceeds 0.98 in each year.

[8] One important note is that the market value of housing is reported as tax value, which is considerably lower than market value; however, we only use this variable to create participation indicators and thus only need to observe whether this is non-zero.

Table 1: Characteristics of Children and Parents

	Mean	SD	N
Panel A: Child demographics			
Birth year	1985	6	1,148,053
Female	0.489	0.500	1,148,053
Number of siblings (same mother)	1.6	1.1	1,145,612
Lives in one of the 4 largest municipalities	0.164	0.371	1,148,053
Lived with both parents at 15	0.665	0.472	1,148,053
Lived with mother at 15	0.281	0.449	1,148,053
Panel B: Parents (household measures, child ages 0–17)			
Mother's age at child's birth	27.7	4.9	1,142,981
Father's age at child's birth	30.7	5.7	1,120,968
Household income (DKK, child ages 11–15)	445,585	360,627	1,148,021
Ever participates in the stock market	0.461	0.498	1,147,959
Average risky share	0.072	0.146	952,248
Ever holds bonds	0.156	0.363	960,369
Ever owns a home	0.811	0.391	1,148,022
Ever owns 2+ properties	0.131	0.337	1,148,022
Ever uses investment funds	0.074	0.262	1,147,959
Average bank saving rate	0.011	0.065	789,630
Ever contributes to a pension	0.431	0.495	1,147,960
Panel C: Child outcomes, ages 18–30			
Income (DKK, ages 25–30)	262,571	352,742	1,137,859
Ever participates in the stock market	0.323	0.467	1,148,045
Average risky share	0.051	0.120	1,145,099
Ever holds bonds	0.103	0.304	1,147,865
Ever owns a home	0.429	0.495	1,147,909
Ever owns 2+ properties	0.018	0.133	1,148,052
Ever uses investment funds	0.063	0.243	1,147,908
Average bank saving rate	0.021	0.067	1,145,082
Ever contributes to a pension	0.116	0.320	1,147,908

Notes: Table shows descriptive statistics for the sample of children and their parents. The four largest Danish municipalities are Copenhagen, Aarhus, Odense, and Aalborg; municipality is measured at age 15. Income is total pre-tax nominal income. Risky share is defined as public equity divided by the sum of public equity plus safe assets. "Ever uses investment funds" is measured as receipt of a fund distribution. Average bank savings rate is defined as bank flows divided by net income over the year. "Ever contributes to a pension" captures contributions to a privately-purchased scheme only.

4 Framework: A "Production Function" of Investment Behaviors

We model the data generating process for child i's investment behavior y_i as follows,

$$y_i = f(x_i, z_i, p_i) + \eta_i, \tag{1}$$

where x_i is the same investment behavior by child i's parents, z_i is the behavior by peer-parents in the child's municipality of residence, p_i is a set of own-parent characteristics (income, wealth, and education), and η_i is an idiosyncratic error term. Given the literature's interest in the determinants of stock market participation and the explanations behind the "participation puzzle" (see e.g. Haliassos and Bertaut (1995), Mankiw and Zeldes (1991), Campbell (2006)), we focus on extensive margin participation as the main behavior of interest, with later analyses broadening to a wider range of investment behaviors.

The function $f(\cdot)$ is unknown, but the richness of our data and quasi-experimental designs allows us to learn about its form. We thus proceed to our quasi-experimental strategies, which allow us to test whether $f(\cdot)$ is increasing in each of x and z, and—by combining our two sources of identifying variation—whether there are complementarities between own-parent and peer-parent exposure.

5 Empirical Strategy

5.1 Descriptive Analysis of Parent-Child Transmission

A correlational analysis of the transmission of investment behaviors from parents to children can be implemented with a simple regression of child on parent behaviors,

$$y_i = a + bx_i + X_i'\Gamma + \lambda_{c(i)} + e_i, \tag{2}$$

where y_i is the investment behavior of child i (for instance, an indicator for whether they ever invest in the stock market) between ages 18-30, and x_i is the investment behavior of their parents when the child is young (ages 0-17). We include a vector of controls X_i and child birth cohort fixed effects $\lambda_{c(i)}$. The coefficient b conveys—conditional on controls and within birth cohort—the extent to which a child's investment behavior in adulthood are correlated with their parents' behavior while the child was young.

While informative about raw correlations, this naïve regression ignores the fact that parental investment behaviors x_i are likely correlated with unobservable characteristics of both parents and children that also determine children's investment behaviors. To address this source of endogeneity, we next proceed to a number of quasi-experimental procedures that allow us to identify the causal effect of both own-parent exposure and peer-parent exposure on children's investment behaviors in adulthood.

5.2 Parental Exposure: Separation Design

First, to estimate the causal effect of exposure to one's own parents on children's investment behaviors later in life, we focus on families that experience a parental separation and exploit plausibly random variation

in child age at the time of the separation. Building on Bakker et al. (2026), we estimate the following regression,

$$y_i = \sum_m \alpha_m \mathbb{I}\{m_i = m\} + \delta x_i^\ell + \rho(m_i \times x_i^\ell) + X_i'\Gamma + \lambda_{c(i)} + \epsilon_i, \tag{3}$$

where y_i is child i's investment behavior between ages 18-30,[9] x_i^ℓ is the investment behavior of the leaving parent in the years prior to the separation event, m_i is the child's age in the year of the separation event, and X_i is a vector of controls. Birth cohort fixed effects $\lambda_{c(i)}$ absorb everything common to a birth year, such as the market conditions a cohort faces when it starts saving. The coefficient of interest is ρ, the causal effect of one additional year of exposure to the investment behavior of a leaving parent on the child's investment behavior as an adult.

Relative to the descriptive regression in equation 2, this quasi-experimental design allows us to compare children of investing parents with early separations to children of investing parents with late separations. As such, this design removes confounds that are common to investing parents relative to non-investing parents (independent of child age at separation), allowing us to isolate the causal effect of one additional year of childhood exposure to a parent who invests, relative to an additional year with a parent who does not invest.

Identification relies on the assumption that, conditional on birth cohort, the child's age at the time of the parents' separation is random. In other words, the child's investment behaviors should not vary with age at parental separation in the absence of exposure effects. The main threat to identification is that early- and late-separating families may be systematically different from each other (it could be, for instance, that "better" parents either delay or expedite their separation).

To address this concern, we complement our age-at-separation design with a strategy that instead exploits variation in the age *gap* between siblings: as a robustness check, we estimate

$$y_i = \sum_m \alpha_m \mathbb{I}\{m_i = m\} + \xi_f + \rho(m_i \times x_i^\ell) + X_i'\Gamma + \epsilon_i, \tag{4}$$

where ξ_f are family fixed effects. This is conceptually equivalent (in a first-differences sense) to regressing the difference in sibling outcomes on leaver parent exposure fully interacted with the gap in sibling ages. The coefficient of interest is once again ρ, which has the same interpretation as in equation (3).

In this second design, identification is based on the randomness of the age *gap* between siblings. The use of these two complementary designs helps provide reassurance that our estimates are picking up the causal effect of an additional year of parental exposure.

In equation (3), we implicitly assume that the effect of an additional year of exposure is linear. To allow for the effect to differ across childhood ages, we additionally estimate a version of equation (3) using age buckets. This tests whether particular childhood periods matter more for the transmission of investment

[9] In this design and the movers design discussed in the section that follows, we restrict the sample to children in birth cohorts that are observable through at least age 26, so that younger cohorts do not contaminate the analysis since their observed window is shorter/younger. For the full population of children in cohorts observable through age 30, 80% of those who participate at any time between ages 18-30 also participated at some point between ages 18-26, which validates our use of this cohort restriction.

behaviors. Concretely, we estimate

$$y_i = \sum_b \alpha_b \mathbb{I}\{b_i = b\} + \delta x_i^\ell + \sum_{b \neq 2\text{–}3} \rho_b \left(\mathbb{I}\{b_i = b\} \times x_i^\ell\right) + X_i'\Gamma + \lambda_{c(i)} + \epsilon_i, \tag{5}$$

where b_i is the child's age-at-separation bucket (0–1, 2-3, ..., 16-17).[10] Differential effects of parental exposure would be captured by variation in ρ_b across age buckets.

5.3 Peer Effects: Movers Design

Next, we exploit an alternative source of quasi-random variation to estimate the importance of peer effects during childhood in the formation of investment behaviors. We build on the movers design in Chetty et al. (2026) (along with earlier work by the same authors). While similar in approach to prior work, we are interested in the transmission of behaviors from the *parents* of peers rather than from peers directly. We restrict our sample to families with children who moved between Danish municipalities exactly once while the child was between ages 1-28. Then, following equation (9) of Chetty et al. (2026), we estimate the semi-parametric regression

$$y_i = \sum_{m=1}^{28} \mathbb{I}\{m_i = m\} \left[\alpha_m + \beta_m \bar{x}_{o(i),c(i)} + \zeta_m p_i + b_m \Delta_{o(i),d(i),c(i)}\right] + \lambda_{c(i)} + \varepsilon_i, \tag{6}$$

where $\bar{x}_{o(i),c(i)}$ is the cohort-specific mean of an investment behavior (e.g. propensity to participate in the stock market) among the parents of peers[11] in child i's origin municipality, p_i is the within-cohort household income rank of child i's parents, and $\Delta_{o(i),d(i),c(i)}$ is the difference in the average investment behavior among the parents of peers in child i's destination vs origin municipality. The outcome y_i is the investment behavior of child i between ages 18-30. The coefficient of interest is b_m, which captures the extent to which the adult investment behaviors of children who move at age m converge to the peer-parent mean investment behavior in their destination municipality, relative to their origin.

Following Chetty and Hendren (2018), we also estimate a parametric regression for our movers design. The estimates of equation 6 (in Figure 2 below) display kinks at roughly ages 7 and 20, motivating the use of a piecewise spline with three linear segments to estimate the slopes of the exposure effect over the age profile. These kinks are consistent with the intuition that parental moves after age 20 have no causal effect on children's investment behaviors; we follow Chetty and Hendren (2018) in assuming a constant selection effect of municipality of residence, and we use age 20 as the "knot" for estimating this selection effect.[12] Our

[10] We use bucket 2-3 as the reference since separations at earlier ages are rare.

[11] We restrict to parents of peers who are non-one-time-movers when calculating the municipality means, and the means are weighted by the number of years the peer family lived in that municipality. We use peers with birth years ±2 years around the mover child's birth year.

[12] Chetty and Hendren (2018) use age 23 as the selection cutoff; we use age 20 because this is the first age at which fewer than 50% of Danish children in any cohort lived with their parents in any year. See https://www.dst.dk/da/Statistik/udgivelser/NytHtml?cid=32669.

parametric specification is thus as follows,

$$
\begin{aligned}
y_i \;=\; & \sum_c \mathbb{I}\{c(i)=c\}\big(\alpha_c^1 + \alpha_c^2\,\bar{x}_{o(i),c(i)}\big) \;+\; \sum_{m=1}^{28} \mathbb{I}\{m_i=m\}\big(\zeta_m^1 + \zeta_m^2\,p_i\big) \;+\; \sum_{c\neq\bar{c}} \kappa_c\,\mathbb{I}\{c(i)=c\}\,\Delta_{o(i),d(i),c(i)} \\
& +\; \mathbb{I}\{m_i\leq 6\}\big(b_7 + (7-m_i)\,\psi\big)\Delta_{o(i),d(i),c(i)} \;+\; \mathbb{I}\{7\leq m_i\leq 19\}\big(b_{20} + (20-m_i)\,\gamma\big)\Delta_{o(i),d(i),c(i)} \\
& +\; \mathbb{I}\{m_i\geq 20\}\big(\delta + (20-m_i)\,\delta'\big)\Delta_{o(i),d(i),c(i)} \;+\; \varepsilon_i,
\end{aligned}
\tag{7}
$$

where the final three terms contain the three linear functions of $\Delta_{o(i),d(i),c(i)}$ for each age segment. The coefficient of interest is γ, which gives the peer-parent effect from one additional year of exposure to the destination municipality in ages 7-19: these are the ages when children are most extensively exposed to peer-parents, as they are in school and living in their parents' municipality of residence. We additionally report estimates of ψ and δ'.

In this design, identification relies on the randomness of the timing of childhood moves. We include a robustness check that adds time-varying controls for parent income rank and marital transitions to equation 7, to account for potential confounds.

6 Results

In this section, we present results from implementing the designs outlined in Section 5. We estimate the descriptive effect of parental stock market participation, followed by the causal effects of exposure to a leaving parent who participates in the stock market, and of exposure to peer-parents with high average participation in the stock market, on a child's probability of participating in the stock market as an adult. In a later section, we broaden our analysis to other investment behaviors beyond stock market participation.

6.1 Descriptive Transmission Results

As a first pass at documenting the intergenerational transmission of stock market participation, Table 2 shows estimates of b in our descriptive equation (2), with y_i an indicator for whether the child ever participates in the stock market at ages 18-30, and x_i an indicator for whether either parent ever participates while the child was ages 0-17.

The regression estimates show that, even after controlling for parent household income rank, wealth rank, and an indicator for any post-secondary education, the correlation between child and parent participation is strong. A child whose parent ever participated while the child was 0-17 is, conditional on parent characteristics, 15 percentage points more likely to participate between ages 18-30, which is nearly a 50% increase in the probability of participation from the baseline rate of 32% shown in Table 1.

Table 2: Descriptive Evidence of Parent-Child Participation Transmission

Dependent Variable:	Child ever participates (18-30), 0/100		
	Income rank	+ wealth rank	+ education
Model:	(1)	(2)	(3)
Parents ever participate (child ages 0-17)	19.411***	15.389***	15.349***
	(0.104)	(0.125)	(0.125)
Parent household income rank	13.575***	5.671***	3.624***
	(0.175)	(0.220)	(0.230)
Female	-9.287***	-9.709***	-9.778***
	(0.084)	(0.095)	(0.096)
Parent household financial wealth rank		21.887***	21.343***
		(0.231)	(0.233)
Parent has tertiary education			3.495***
			(0.121)
Fixed-effects			
Birth cohort	Yes	Yes	Yes
Observations	1,147,950	901,252	889,052
R^2	0.081	0.098	0.097

Notes: Table shows estimates of b in equation (2), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i is an indicator for whether the parents ever participated while the child was ages 0-17. Income and wealth ranks are calculated within cohort. Standard errors are clustered at the household level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

The remaining coefficients in the table indicate that a child is more likely to participate in the stock market between ages 18-30 if their parents have a higher household income rank, higher household wealth rank, or if either parent has any post-secondary education. Men are more likely than women to participate.

While the estimates of b in Table 2 are suggestive that stock market participation transmits from parents to their children, there are many unobservable characteristics of the child and parent that might bias the results. Thus, we next turn to our quasi-experimental designs, which provide estimates of the causal effect of parental exposure on the child's participation decision in adulthood.

6.2 Own-parent Transmission: Parental Separation Design

Table 3 shows regression estimates from equation 3, where the outcome y_i is an indicator for whether child i ever participates in the stock market in ages 18-30, and the variable x_i^{ℓ} is an indicator for whether the leaving parent ever participated while the child was living with both parents. Column (1) includes birth cohort and age at separation fixed effects. The coefficient of interest, ρ, is the interaction between age-at-separation and leaver parent participation: the estimate indicates that one additional year spent in the intact

household raises the child's probability of ever participating by 0.266 percentage points more if the leaver parent is a stock market participant relative to a non-participant. This effect is statistically significant and economically meaningful: over the full length of childhood (18 years), the participation gap widens by about 4.8 percentage points, which is a 15% increase in the probability of stock market participation over the full population baseline rate documented in Table 1. We therefore conclude that exposure to a participating parent has a meaningful causal effect on a child's stock market participation later in life.

In columns (2)-(5), we add controls—both in levels and as interactions with age at separation—for other characteristics of the leaver parent. As shown, after controlling for the leaver parent's income rank, wealth rank, and an indicator for whether they have post-secondary education, the estimate of ρ remains positive and significant and is in fact slightly larger in magnitude, indicating a 0.32 percentage point increase per year of exposure, corresponding to an 18% increase in the probability of participation with a full childhood of exposure. This result conveys that the transmission of stock market participation from parents to children persists even after controlling for exposure to higher income, wealth, or education.

Table 3: Effect of Exposure to a Leaving Parent on Child's Stock Market Participation in Adulthood

Dependent Variable:	Child ever participates (18-30), 0/100				
	Age at separation	+ leaver income	+ leaver wealth	+ leaver education	+ all three
Model:	(1)	(2)	(3)	(4)	(5)
Leaving parent participates while intact	11.583***	12.135***	8.326***	10.989***	8.192***
	(0.606)	(0.615)	(0.868)	(0.610)	(0.874)
Pre-separation household income rank	21.319***	26.168***	18.979***	18.652***	22.998***
	(0.382)	(0.680)	(0.492)	(0.399)	(0.834)
Female	-11.633***	-11.533***	-12.377***	-11.552***	-12.391***
	(0.197)	(0.198)	(0.239)	(0.200)	(0.241)
Leaving parent participates while intact × Age at separation	0.266***	0.231***	0.303***	0.307***	0.320***
	(0.056)	(0.058)	(0.074)	(0.057)	(0.075)
Leaving parent pre-separation income rank		-8.078***			-9.061***
		(0.933)			(1.413)
Leaving parent is the mother		1.075***	2.814***	1.821***	1.137***
		(0.325)	(0.337)	(0.303)	(0.368)
Age at separation × Leaving parent pre-separation income rank		0.259***			0.097
		(0.076)			(0.110)
Leaving parent pre-separation financial wealth rank			13.870***		12.922***
			(1.164)		(1.237)
Age at separation × Leaving parent pre-separation financial wealth rank			0.024		0.066
			(0.104)		(0.111)
Leaving parent has tertiary education				10.910***	12.330***
				(0.585)	(0.852)
Age at separation × Leaving parent has tertiary education				-0.391***	-0.553***
				(0.056)	(0.074)
Fixed-effects					
Age at separation	Yes	Yes	Yes	Yes	Yes
Birth cohort	Yes	Yes	Yes	Yes	Yes
Observations	214,990	214,786	150,253	210,815	147,851
R^2	0.068	0.068	0.075	0.071	0.079

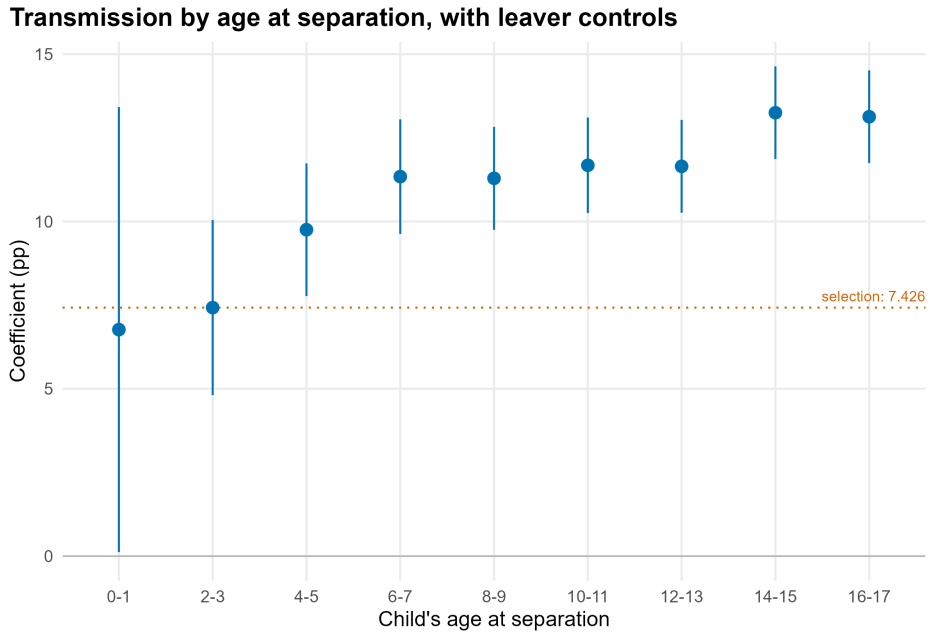
Notes: Table shows estimates of equation (3), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

To gauge whether the effect of exposure to parental investment behaviors varies with age, and to allow for non-linearity in the effect of parental exposure on stock market participation, Figure 1 shows estimates of $\delta + \rho_b$ in equation (5). The plotted point for the 2-3 age bucket is δ, which is the baseline (e.g. selection) effect, while the point for each of the other age buckets conveys the exposure effect relative to age 2-3, which is the estimate of ρ_b for that age bucket.

A visual comparison of the age 16-17 point to the age 2-3 point, for instance, shows that about 13 years of additional exposure to a leaving parent who participates (relative to a non-participating leaving parent) increases the child's probability of ever participating by about 6 percentage points.

Figure 1: Effect of Exposure to a Leaving Parent, by Age Bucket, with Controls



Notes: Figure shows estimates of $\delta + \rho_b$ in equation (5), for each age bucket b on the horizontal axis, where $\rho_b = 0$ for the age 2-3 category. Standard errors are clustered at the family level. Controls include leaver parent income rank, wealth rank, and tertiary education, where income and wealth ranks are calculated within cohort and separation year.

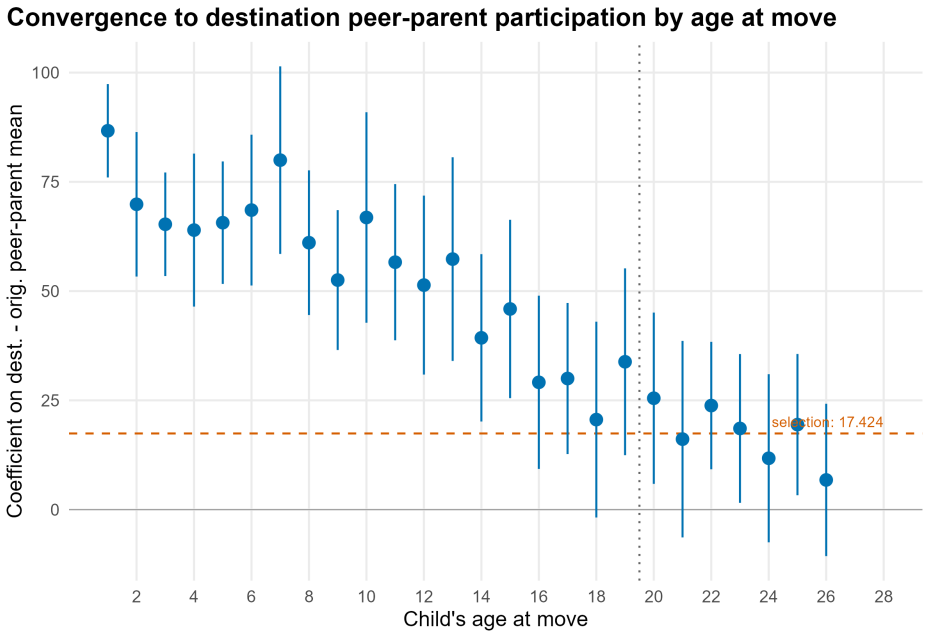
6.3 Peer-parent Transmission: Movers Design

Figure 2 shows estimates of b_m in equation (6) across the age profile, for the sample of children who move between Danish municipalities exactly once by age 28. The outcome y_i is an indicator for whether child i participates in the stock market in early adulthood (ages 18-30), and $\Delta_{o(i),d(i),c(i)}$ is the difference in the fraction of peer-parents who participate in the stock market between the child's destination and origin municipalities.

Much like Chetty et al. (2026)'s analysis—in which income rank is the variable of interest—we find a positive and significant coefficient estimate which declines over the age profile, indicating that children who move at younger ages converge more strongly to the participation probability of their destination municipality.

Furthermore, a comparison to the age-at-separation plot in Figure 1 highlights a commonality between own-parent and peer-parent exposure: a child's probability of stock market participation in ages 18-30 is shown to be causally related to both exposure to their own parents and to the parents of their peers. We test for interactions between these two sources of exposure in a later section.

Figure 2: Transmission of Stock Market Participation from Peer-parents to Children



Notes: Figure shows estimates of b_m in equation (6), for each age bucket m at the time of the move on the horizontal axis. Standard errors are clustered at the origin municipality level.

To obtain a single estimate for the exposure effect over the age profile, Table 4 shows estimates from our parametric regression outlined in equation 7. The slope for ages 7-19, which is γ, indicates that a single year of additional exposure to a destination municipality where all peer-parents participate—relative to an origin municipality where no peer-parents participate—increases the child's probability of stock market participation by 3.6 percentage points. This effect size is substantial.

Table 4: Effect of Peer-Parent Exposure on Child Stock Market Participation: Movers Design

Dependent Variable:	Child ever participates (18-30), 0/100
Model:	(1)
Raw dest. - orig. mean, moves at 1-6: level at 7	54.488***
	(6.359)
x (7 - age at move)	3.692***
	(1.072)
Raw dest. - orig. mean, moves at 7-19: level at 20	25.562***
	(7.601)
x (20 - age at move)	3.608***
	(0.514)
Raw dest. - orig. mean, moves at 20+: level at 20	19.890**
	(8.689)
x (20 - age at move)	1.225
	(1.421)
Fixed-effects	
Age at move	Yes
Birth cohort	Yes
Parent income rank x age at move	Yes
Origin peer-parent mean x birth cohort	Yes
Raw dest. - orig. mean x birth cohort	Yes
Observations	234,403
R^2	0.057

Notes: Table shows estimates of equation (7), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30. The right-hand-side is a three-piece spline with segments linear in ages 1-6, 7-19, and 20+, along with fixed effects. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Income ranks are calculated within cohort. Standard errors are clustered at the origin municipality level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

6.4 Testing for Complementarities Between Parents and Neighborhoods

Our semi-parametric plots of the own-parent and peer-parent exposure effects in Figures 1 and 2, respectively, provide suggestive evidence that $f(\cdot)$ in equation 1 is increasing in each of x and z. A separate, and potentially more interesting question, is whether there are complementarities between own-parent and peer-parent exposure, or whether these two channels of exposure act as substitutes. To answer this question, we perform tests of whether $f(\cdot)$ is the $\max(\cdot)$ or $\min(\cdot)$ function, which would mean that exposure to only one or both own- and peer-parent investment shapes the child's behavior, respectively, or whether there are complementarities in own-parent and peer-parent exposure.

First, we use the age-at-separation quasi-experimental design along with cross-municipality variation to test whether exposure to a participating parent affects a child's probability of stock market participation

in adulthood, over and above exposure to peer-parent participation in their municipality of residence. We perform this test in two ways: in columns (1) and (2) of Table 5, we estimate equation 3 separately for children living in municipalities with a peer-parent participation rate below the median and above the median, respectively. As shown, the coefficient ρ on the interaction between leaver parent participation and age at separation is large and significant in the below-median sample: this suggests that $f(\cdot) \neq \min(x_i, z_i)$, as it is clear that own-parent exposure alone is sufficient to generate stock market participation in adulthood. On the other hand, in the above-median sample, the estimate of ρ is less than 1/2 of the magnitude. This result suggests that exposure to a participating parent has the largest effect on a child's probability of stock market participation if the child is not already exposed to high rates of participation by other adults in their neighborhoods, which is evidence toward substitutability of own-parent and peer-parent exposure.

In column (3) of Table 5, we estimate a version of equation 3 that is fully interacted with the peer-parent participation rate in the child's municipality of residence. The coefficient on the triple interaction of leaver parent participation, peer-parent participation rate, and age at separation is negative and significant, suggesting that the effect of own-parent exposure is declining in the peer-parent participation rate in the child's municipality of residence.

To summarize, these results strongly suggest that $f(\cdot)$ does not take the form $\min(x_i, z_i)$ and also that there is no complementarity between own-parent and peer-parent exposure. The large gap between the estimates of ρ in columns (1) and (2), and the large negative estimate on the triple interaction in column (3), both suggest that own-parent and peer-parent exposure are substitutes.

Table 5: Test of Own- and Peer-parent Complementarity: Separation Design

Dependent Variable:	Child ever participates (18-30), 0/100		
	Peer rate below median	Peer rate above median	Peer rate interacted
Model:	(1)	(2)	(3)
Leaving parent participates while intact	10.193***	12.225***	10.866***
	(0.891)	(0.827)	(0.612)
x age at separation	0.401***	0.130*	0.260***
	(0.083)	(0.077)	(0.057)
Pre-separation household income rank	19.652***	22.530***	20.601***
	(0.521)	(0.551)	(0.382)
Female	-11.174***	-12.060***	-11.611***
	(0.270)	(0.287)	(0.197)
Pre-separation municipality peer-parent rate, centered			28.091***
			(3.733)
x age at separation			1.814***
			(0.406)
Leaving parent participates x peer rate			30.301***
			(9.307)
x age at separation			-2.123**
			(0.882)
Fixed-effects			
Age at separation	Yes	Yes	Yes
Birth cohort	Yes	Yes	Yes
Observations	107,658	107,319	214,977
R^2	0.063	0.063	0.071

Notes: Table shows estimates of equation (3), estimated separately for children who live in municipalities with below-median peer-parent participation rates in column (1) and above-median peer-parent participation rates in column (2), and with an interaction with peer-parent participation rate in column (3). The outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

We next perform the analogous test using the movers quasi-experimental design coupled with across-family variation in parent participation rates: results are displayed in Table 6. The coefficient on the interaction between the difference in destination vs origin participation interacted with years of exposure now captures the effect of peer-parent exposure for children whose own parents do *not* participate; the effect is positive and significant, which again suggests that the combination of both own-parent and peer-parent exposure is not required (i.e. $f(\cdot)$ is likely not the min$(\cdot)$ function). Furthermore, the coefficient on the triple interaction is negative (but insignificant): this suggests a lack of complementarity of own-parent and peer-parent exposure, and points toward substitutability.

Table 6: Test of Own- and Peer-parent Complementarity: Movers Design

Dependent Variable:	Child ever participates (18-30), 0/100
Model:	(1)
Raw dest. - orig. mean x (20 - age at move), ages 7-19	3.733***
	(0.563)
x parents participate pre-move	-1.644
	(1.238)
Fixed-effects	
Age at move x parents participate pre-move	Yes
Birth cohort x parents participate pre-move	Yes
Parent income rank x age at move x pre-move participation	Yes
Origin peer-parent mean x birth cohort x pre-move participation	Yes
Raw dest. - orig. mean x birth cohort x pre-move participation	Yes
Other spline pieces x pre-move participation	Yes
Observations	234,277
R^2	0.081

Notes: Table shows estimates of equation (7), where we additionally interact with an indicator for whether the parents participate in the stock market pre-move. The outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Income ranks are calculated within cohort. Standard errors are clustered at the origin municipality level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

This analysis provides several key takeaways about the functional form of $f(\cdot)$. Our results suggest that $f(\cdot)$ is not the $\min(\cdot)$ function, and that there are no complementarities between own-parent and peer-parent exposure. We find suggestive evidence that exposure to own- and peer-parents acts as substitutes in the production of investment behaviors.

7 Robustness Checks and Placebo Test

This section probes whether our main estimates are robust to various alternative specifications and the addition of controls and fixed effects.

7.1 Fixed Window of Parental Exposure

One concern with our main estimating equation in the age-at-separation design is that the indicator for leaver participation, x_i^ℓ, is computed over the length of the child's exposure to the leaving parent and is thus directly related to m_i. More concretely, a parent who leaves later in the child's life has more years to be observed as a stock market participant.

To address this concern, we re-estimate equation 3 using a fixed window, containing years -5 through -3 relative to the parental separation, to measure x_i^ℓ. We find similar (in fact, slightly larger) estimates compared to our main specification.

Table 7: Effect of Exposure to a Leaving Parent on Child's Participation: Fixed Parent Window

Dependent Variable:	Child ever participates (18-30), 0/100	
	Age at separation	+ leaver controls
Model:	(1)	(2)
Leaving parent participates pre-separation	9.715***	5.848***
	(0.643)	(1.022)
Pre-separation household income rank	22.177***	23.524***
	(0.381)	(0.836)
Female	-11.611***	-12.380***
	(0.197)	(0.242)
Leaving parent participates pre-separation × Age at separation	0.493***	0.533***
	(0.061)	(0.087)
Fixed-effects		
Age at separation	Yes	Yes
Birth cohort	Yes	Yes
Leaving parent controls + interactions		Yes
Observations	214,776	147,851
R^2	0.066	0.077

Notes: Table shows estimates of equation (3), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in years [-5, -3] relative to the year of parental separation. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Column (2) includes controls for leaver parent income rank, wealth rank, and post-secondary education, along with the interaction of each with m_i. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

7.2 Parental Separation: Sibling Design

To account for the possibility that a child's age at which their parents separate may be non-random, Table 8 implements our sibling comparison design summarized by equation 4. Namely, we now compare children *within families* and only require that the age gap between siblings in the same family is random. To ensure that we are comparing siblings across a fixed and fully observable window, we restrict to sibling pairs for which the youngest sibling's birth cohort is observable in the data through at least age 26, and we measure stock market participation for the child in ages 18-26.[13]

This alternative design returns a near-identical estimate of ρ as the one in Table 3 but is only marginally

[13] As noted in an earlier footnote, we find that, for our full sample of children in cohorts observable through age 30, among those who participate at some point between 18-30, 80% also participated at some point between 18-26. This allows us to use this younger outcome window at a smaller cost in terms of sample size. Chetty and Hendren (2018) use an analogous approach, in which their outcome is measured at younger ages in the sibling design.

significant given the smaller sample size of families with at least two children in cohorts observed through age 26. The comparability of magnitudes suggests that the participation gap holds between siblings within the same family and is not driven by cross-family differences. This stability across specifications is reassuring and points to the estimate capturing the causal effect of parental exposure.

Table 8: Effect of Exposure to a Leaving Parent on Child's Stock Market Participation: Sibling Design

Dependent Variable:	Child ever participates (18-26), 0/100	
	Sibling gap, family FE	+ cohort FE
Model:	(1)	(2)
Female	-8.279***	-8.223***
	(0.306)	(0.305)
Age at separation × Leaving parent participates while intact	0.251*	0.215
	(0.152)	(0.151)
Fixed-effects		
Family	Yes	Yes
Age at separation	Yes	Yes
Birth cohort		Yes
Observations	91,252	91,252
R^2	0.698	0.700

Notes: Table shows estimates of equation (4), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-26, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Column (2) adds birth cohort fixed effects. Standard errors are clustered at the family level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

7.3 Placebo Test: Interact Age at Separation with Stayer Parent

As a placebo test, we add to equation 3 an indicator for whether the *stayer* parent participates in the stock market, along with an interaction of this indicator with age at separation m_i. The coefficient on the interaction term should be zero: the child's amount of exposure to the staying parent is unrelated to the child's age at the time of parental separation.

Table 9 shows the results of running this placebo test.[14] The coefficient on age at separation interacted with the stayer participation indicator is insignificant and small in magnitude, as expected.

[14] In this regression, we use the fixed pre-separation window from Section 7.1 to measure leaver and stayer parent stock market participation, since the fact that the window length over which we measure x_i^ℓ is increasing in m_i could cause additional contamination when we include both leaver and stayer parent effects in the regression.

Table 9: Effect of Exposure to the Staying Parent on Child's Stock Market Participation (Placebo)

Dependent Variable:	Child ever participates (18-30), 0/100	
	Age at separation	+ leaver controls
Model:	(1)	(2)
Leaving parent participates pre-separation	7.834***	4.058***
	(0.663)	(1.052)
Staying parent participates pre-separation	11.340***	9.883***
	(0.791)	(1.128)
Pre-separation household income rank	20.668***	20.361***
	(0.383)	(0.840)
Female	-11.551***	-12.392***
	(0.197)	(0.241)
Leaving parent participates pre-separation × Age at separation	0.360***	0.406***
	(0.064)	(0.090)
Age at separation × Staying parent participates pre-separation	0.070	0.143
	(0.072)	(0.094)
Fixed-effects		
Age at separation	Yes	Yes
Birth cohort	Yes	Yes
Leaving parent controls + interactions		Yes
Observations	214,605	147,814
R^2	0.072	0.083

Notes: Table shows estimates of equation (3), where we additionally interact age at separation m_i with an indicator for whether the staying parent ever participated in the stock market in years [-5, -3] relative to the year of parental separation. The outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in years [-5, -3] relative to the year of parental separation. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Column (2) includes controls for leaver parent income rank, wealth rank, and post-secondary education, along with the interaction of each with m_i. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

8 Mechanisms

Having documented that the probability of participating in the stock market demonstrates strong transmission from both own-parents and peer-parents to children, we next explore the mechanisms underlying this transmission. We begin with a descriptive analysis in Section 8.1, followed by quasi-experimental results using our age-at-separation design.

8.1 Specific Behavior Transmission versus Common Factors

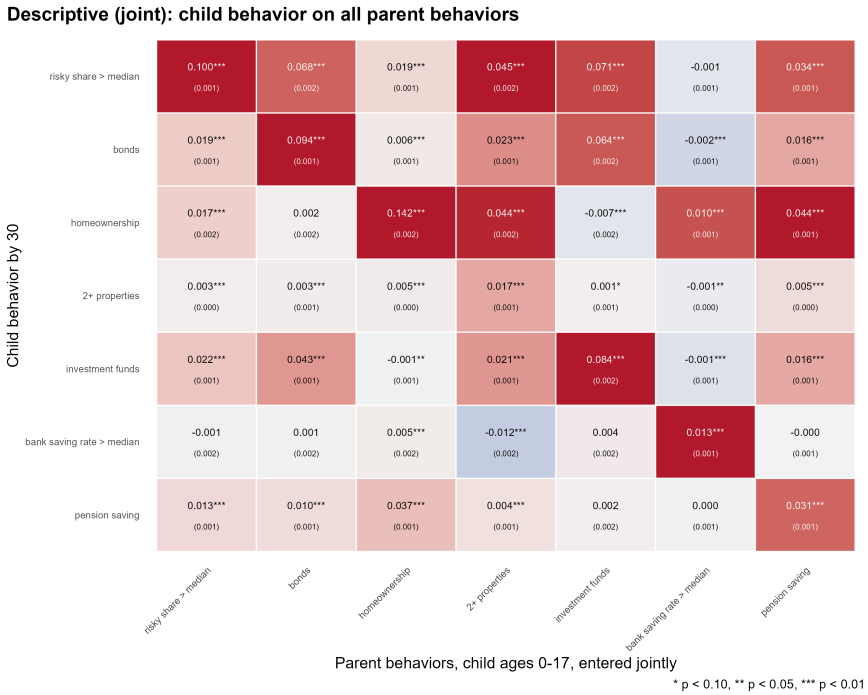
First, we broaden our set of outcomes to include a range of investment behaviors beyond extensive margin stock market participation, to answer the question of whether *specific* behaviors are transmitted from parents to kids, or whether a common set of parental characteristics determine several child behaviors.

To that end, Figure 3 shows estimates of b in a multivariate version of equation 2, for various child and parent investment behaviors. For instance, to obtain the entry in the second row and first column, the outcome y_i is an indicator for whether the child ever holds bonds between ages 18-30, x_i is an indicator for the parents' risky share averaged over the years that the child is ages 0-17 being greater than the median, and we additionally include, on the right hand side, the full set of other parental investment behaviors included in the columns of the matrix.

The figure shows strong positive correlations between nearly all parent and child investment behaviors, but the diagonal entries are generally largest in magnitude. This suggests a strong behavior-by-behavior transmission, providing descriptive evidence that children either observe and copy specific investment behaviors, or inherit preferences and knowledge related to specific investment behaviors, from their parents.

This finding—that transmission from parents to children is highly specific to the behavior—is consistent with studies of intergenerational linkages in other types of behaviors, ranging from credit card revolving (Bakker et al. (2026)) to criminal activity (Case and Katz (1991)).

Figure 3: Coefficient Matrix of Parent and Child Investment Behaviors



Notes: Figure shows estimates of b in equation 2 where we additionally control for the full set of parent investment behaviors on the horizontal axis along with household controls (household income rank, wealth rank, and an indicator for post-secondary education, where income and wealth ranks are calculated within cohort). The right-hand-side variable x_i is own-parent behavior while the child was ages 0-17. The outcome y_i is the child investment behavior indicated on the vertical axis, measured over the years that the child is ages 18-30. All variables are indicators; the median risky share is conditional on holding positive public equity. Standard errors are clustered at the family level. Colors are normalized by column.

8.2 Direct Transmission of Public Equity

Returning to extensive margin stock market participation as the main outcome, we next exploit variation within the population of children in our age-at-separation quasi-experimental design, to test various mechanisms through which stock market participation transmits from parents to children.

We begin with a test of whether this transmission is driven by direct transfers of public equity from parents to their kids. It is plausible that a child with a participating parent is more likely to participate in the stock market only because their parent buys public equity for them directly. To test whether this is the case, we shut down this direct transmission channel by estimating equation (3) after restricting to children with no public equity held in their name before age 18.

Table 10: Effect of Parental Exposure on Child's Participation, Among Children Without Holdings Before 18

Dependent Variable:	Child ever participates (18-30), 0/100	
	No equity before 18	+ leaver controls
Model:	(1)	(2)
Leaving parent participates while intact	6.395***	3.999***
	(0.621)	(0.902)
Pre-separation household income rank	15.989***	17.389***
	(0.373)	(0.834)
Female	-11.461***	-12.469***
	(0.196)	(0.244)
Leaving parent participates while intact × Age at separation	0.166***	0.202***
	(0.058)	(0.077)
Fixed-effects		
Age at separation	Yes	Yes
Birth cohort	Yes	Yes
Leaving parent controls + interactions		Yes
Observations	188,607	128,833
R^2	0.050	0.056

Notes: Table shows estimates of equation (3), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26, with the additional sample restriction that the child cannot have reported public equity in their name before age 18. Column (2) includes controls for leaver parent income rank, wealth rank, and post-secondary education, along with the interaction of each with m_i. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 10 shows the results: the estimate of ρ among this subset remains positive and significant and (after controlling for leaver parent characteristics) is about 60% of the estimate for the full sample in Table 3. This result indicates that direct transfers of public equity likely play a role but cannot fully explain the intergenerational transmission of stock market participation.

8.3 Sharing of Financial Intermediaries

Next, we explore whether the sharing of financial intermediaries might explain the intergenerational transmission of stock market participation. Children who use the same bank as their parents may be exposed to the same financial advisers and the same set of savings and investment instruments as their parents, which could explain the link between their probability of stock market participation. To shut down this channel, Table 11 shows estimates from equation (3) after restricting to children who do not share a bank with their

parents.[15]

As shown in the table, after controlling for leaver parent characteristics, the estimate of ρ is roughly equal in magnitude to the estimate for the full sample, indicating that sharing financial institutions contributes relatively little to the intergenerational transmission of investment behaviors.

Table 11: Effect of Parental Exposure on Child's Participation, Among Children and Parents Without a Shared Bank

Dependent Variable:	Child ever participates (18-30), 0/100	
	No shared bank	+ leaver controls
Model:	(1)	(2)
Leaving parent participates while intact	7.376***	5.439***
	(1.078)	(1.705)
Pre-separation household income rank	15.638***	21.318***
	(0.628)	(1.560)
Female	-8.861***	-9.947***
	(0.338)	(0.461)
Leaving parent participates while intact × Age at separation	0.272**	0.280*
	(0.112)	(0.156)
Fixed-effects		
Age at separation	Yes	Yes
Birth cohort	Yes	Yes
Leaving parent controls + interactions		Yes
Observations	64,455	36,481
R^2	0.037	0.048

Notes: Table shows estimates of equation (3), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26, with the additional restriction that the child cannot share a bank with their parent (defined as using a bank with the same registration number). Column (2) includes controls for leaver parent income rank, wealth rank, and post-secondary education, along with the interaction of each with m_i. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

8.4 Transmission of Financial Savvy

As a proxy for formal financial skill that might be transferable from parents to kids, we consider the type of educational degree obtained by the parents. Existing work has shown that individuals with a

[15] We use the bank registration number in the IRTEPERS registry to identify banks; each registration number corresponds to a bank branch. A child is classified as sharing a bank with the leaver parent if the child's bank registration number in any year corresponding to their ages 18-30 coincides with the leaver parent's bank registration number in any of those same years. If either the child or leaver parent is missing a bank registration number, the child is dropped from the analysis sample.

degree in economics or finance are more likely to participate in the stock market or exhibit other behaviors consistent with financial sophistication (Christiansen et al. (2008), Andersen et al. (2020)). We speculate that parents with an education in economics or finance might transmit financial savvy to their kids.

Hence, to shut down this channel of transmission, in Table 12, we omit from the sample children whose parents have a degree in either economics or finance. As shown, the estimate of ρ remains positive, significant, and nearly unchanged in magnitude, indicating that transmission of formal financial sophistication does not substantially contribute to the intergenerational linkage in investment behaviors. Of course, there may be other, unobservable forms of financial savvy that transfer from parents to kids.

Table 12: Effect of Parental Exposure on Child's Participation, Among Children Whose Parent Lacks an Economics/Finance Degree

Dependent Variable:	Child ever participates (18-30), 0/100	
	No econ/finance education	+ leaver controls
Model:	(1)	(2)
Leaving parent participates while intact	11.275***	7.696***
	(0.630)	(0.899)
Pre-separation household income rank	20.479***	22.834***
	(0.394)	(0.847)
Female	-11.570***	-12.303***
	(0.202)	(0.245)
Leaving parent participates while intact × Age at separation	0.255***	0.335***
	(0.058)	(0.077)
Fixed-effects		
Age at separation	Yes	Yes
Birth cohort	Yes	Yes
Leaving parent controls + interactions		Yes
Observations	204,030	142,570
R^2	0.063	0.074

Notes: Table shows estimates of equation (3), where the outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30, and x_i^ℓ is an indicator for whether the leaver parent ever participated in the years before the separation while the child was ages 0-17. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26, with the additional restriction that the child cannot have a parent with a degree in economics or finance. Column (2) includes controls for leaver parent income rank, wealth rank, and post-secondary education, along with the interaction of each with m_i. Income and wealth ranks are calculated within cohort and separation year. Standard errors are clustered at the family level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

9 Conclusion

Despite an abundance of evidence that financial behaviors are correlated across generations, the exact process through which investment behaviors form as a function of the features of one's childhood has been difficult to uncover.

We have shown, through two quasi-experimental designs and data on the full population of Denmark, that exposure during childhood to the behaviors of one's own parents, and of the parents of one's peers, causally affects a child's investment behaviors in adulthood. In testing for interaction effects between own- and peer-parents, our estimates suggest a lack of complementarities between these two sources of exposure and point toward substitutability. An analysis of mechanisms underlying the own-parent transmission revealed that direct transfers of public equity, sharing of financial intermediaries, and transfers of formal financial sophistication (proxied for by a parent's economics/finance education) cannot fully explain this transmission, suggesting an important role for correlated risk preferences or transfers of other forms of financial savvy between parent and child.

Our results shed light on the underlying model through which investment behaviors develop, which speaks to both the theoretical and empirical literature on the determinants of stock market participation. There are also implications of our results for economics research on parent and peer effects in other contexts: one fruitful avenue for future research would be to test whether the lack of complementarities between own- and peer-parent exposure extends to other contexts, such as education and occupation choice.

Our findings also have implications for the broader discussion of wealth inequality and upward mobility. The strong transmission of investment behaviors from parents to children documented in our paper provides evidence that such behaviors contribute to the persistence of wealth inequality across generations. Furthermore, our finding that direct transfers of public equity cannot fully explain the transmission of behaviors suggests that this persistence cannot be fully resolved through taxes and transfers.

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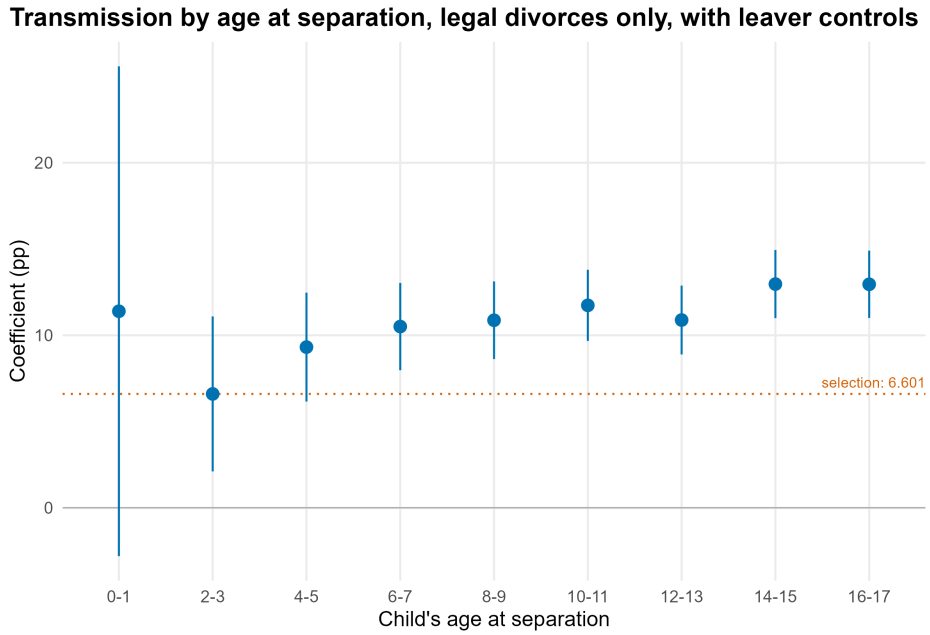
A Separation Design: Robustness Check with Legal Divorce

In our age-at-separation design, we include the full sample of families with parents that permanently separated: this includes both legal divorces and cohabitation dissolutions.

As a robustness check, Figure 4 shows estimates of equation 5 after restricting the sample to families where the parents were legally divorced, observed in our data via a change in civil status code. The estimates are very similar in magnitude.

Figure 4: Effect of Exposure to a Leaving Parent, by Age Bucket, with Controls



Notes: Figure shows estimates of $\delta + \rho_b$ in equation (5), for each age bucket b on the horizontal axis, where $\rho_b = 0$ for the age 2-3 category. Standard errors are clustered at the family level. Controls include leaver parent income rank, wealth rank, and tertiary education (where income and wealth rank are calculated within cohort and separation year). The sample is restricted to children whose parents were legally divorced (measured via a change in civil status code), and the x-axis measures age at the time of the divorce.

B Movers Design: Robustness Check with Time-varying Controls

The identifying assumption for our movers design is that the timing of childhood moves is random. To address possible confounds related to job loss and marital status changes that might coincide with the timing of a move, Table 13 shows the result of estimating equation 7 with the addition of two time-varying controls: parental income rank and marital transitions. The coefficient estimates are very similar in magnitude to our main estimates.

Table 13: Movers Design with Time-varying Controls

Dependent Variable:	Child ever participates (18-30), 0/100	
	Same-sample baseline	+ time-varying controls
Model:	(1)	(2)
Raw dest. - orig. mean, moves at 1-6: level at 7	54.487***	54.260***
	(6.343)	(6.427)
x (7 - age at move)	3.710***	3.716***
	(1.076)	(1.076)
Raw dest. - orig. mean, moves at 7-19: level at 20	25.736***	25.036***
	(7.625)	(7.495)
x (20 - age at move)	3.610***	3.508***
	(0.514)	(0.506)
Raw dest. - orig. mean, moves at 20+: level at 20	20.143**	20.366**
	(8.716)	(8.704)
x (20 - age at move)	1.224	1.336
	(1.420)	(1.443)
Fixed-effects		
Age at move	Yes	Yes
Birth cohort	Yes	Yes
Parent income-rank change at move, exposure spline		Yes
Marital transition at move x exposure spline		Yes
Parent income rank x age at move	Yes	Yes
Origin peer-parent mean x birth cohort	Yes	Yes
Raw dest. - orig. mean x birth cohort	Yes	Yes
Observations	233,913	233,913
R^2	0.057	0.059

Notes: Table shows estimates of equation (7), where we additionally interact with time-varying controls: parent income rank and marital status transitions. The outcome y_i is an indicator for whether the child ever participates in the stock market between ages 18-30. The sample is restricted to children in cohorts old enough to be observed in the data through at least age 26. Income rank is calculated within cohort. Standard errors are clustered at the origin municipality level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.