Shrinking Families:

How Does Demographic Decline Shape Investors?*

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Abstract

Fertility rates are declining worldwide, producing a growing share of only children. Exploiting China's One-Child Policy in a regression discontinuity design, we show that only children are significantly more risk-averse, less trusting, and less socially engaged than individuals with siblings. These trait differences reduce participation in FinTech (online lending, digital wealth management, and mobile payments) but not traditional finance. The effects are economically large, persist after accounting for differences in economic resources and digital access, and concentrate among less educated individuals and in regions with weaker investor protection. Only children also retreat more sharply from digital platforms following scandals, confirming trust as an operative mechanism. As the financial system becomes increasingly digital, demographic decline may impose a psychological constraint on financial modernization.

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I Introduction

Shrinking families are one of the defining socioeconomic trends of our time. Global fertility rates have fallen sharply (Doepke, Hannusch, Kindermann and Tertilt, 2023), and a recent *Lancet* study projects that by 2050 more than three-quarters of countries will be below replacement fertility (GBD Fertility And Forecasting Collaborators, 2024).[1] Each successive cohort contains a rising share of only children, raised without the sibling interactions that developmental psychology identifies as formative for risk tolerance, trust, and social engagement. Yet we do not know how this demographic transformation will reshape investor behavior, and the question is especially pressing because the financial system has simultaneously moved online: adoption of financial technology now demands the trust in opaque platforms, willingness to experiment with unfamiliar products, and exposure to peer networks that the very traits formed in sibling-less households may erode. This paper asks whether growing up without siblings alters investor traits and, if so, whether these traits shape the adoption of financial technology.

China offers the sharpest setting in which to study these questions. The One-Child Policy (OCP) compressed into a single generation a demographic shift that the rest of the world is only now beginning to experience, creating a large population of adults who grew up entirely without siblings. In a landmark study published in *Science*, Cameron, Erkal, Gangadharan and Meng (2013) exploited this policy to show that it produced "little emperors": individuals who are significantly more risk-averse, more pessimistic, less trusting, less competitive, and less conscientious than those who grew up with siblings. Their experiments with Beijing residents, born just before and just after the OCP, established that the sibling interaction channel dominates the parenting channel for these traits, but left open the question that matters most for financial economics: do these traits shape how people actually make financial decisions? China's parallel emergence as the world's largest financial technology (FinTech) market makes it possible to answer this question directly, testing whether the traits forged in sibling-less households affect the adoption of the digital financial products that are now redefining household finance worldwide.

[1] For example, in the U.S., the share of families with children that have only one child has roughly doubled in two generations, from 11% in 1976 to approximately 20% today, and U.S. birth rates have declined steadily since the Great Recession across nearly all demographic groups (Kearney, Levine and Pardue, 2022). The average number of children that adults aged 20–39 plan to have has fallen from 2.3 in 2012 to 1.8 in 2023, below replacement for the first time (Pew Research Center, 2025).

To test this, we need data that capture both investor traits and FinTech behavior for the same individuals. The China Household Finance Survey (CHFS) provides exactly this: a nationally representative biennial survey of over 30,000 household-year observations across 26 provinces that pairs granular FinTech participation measures with survey-elicited risk aversion, generalized trust, and social network engagement. This allows us to trace the complete chain from childhood family structure to adult psychological traits to financial technology adoption. A fundamental identification challenge remains: family size is endogenous, since parents who choose to have one child may differ systematically from those who have more. The OCP resolves this challenge because the policy imposed only-child status on an entire urban generation beginning in 1980, so individuals born just after this cutoff are overwhelmingly only children for reasons unrelated to parental preferences, while those born just before tended to grow up with siblings. We exploit this policy-driven variation in a regression discontinuity design (RDD) to isolate the effect of sibling absence from the parental selection that confounds cross-sectional comparisons.

Our first set of results establishes that family structure during childhood has lasting effects on investor traits. Using the RDD, we find that only children exhibit significantly higher risk aversion, lower generalized trust, and weaker social network engagement than individuals with siblings. These findings extend Cameron et al.'s (2013) results to nationally representative field data and are consistent with established theories in psychology linking parental overprotection to risk aversion and sibling interaction to trust formation (Piaget, 1932; Bowlby, 1969).

The second set of results turns to FinTech adoption, where these trait differences should matter most. Digital financial platforms make unusually high demands on precisely the traits that only children lack: trust in opaque algorithmic systems, willingness to experiment with unfamiliar products, and comfort with technological novelty. The data confirm this. The policy-induced shift in only-child status at the OCP cutoff reduces online lending participation by 3.9 percentage points, digital wealth management participation by 4.6 percentage points, and mobile payment adoption by 5.9 percentage points. The family socialization channel, not the substitution channel (lost sibling risk-sharing increasing demand for formal alternatives), dominates: the effect survives controls for wealth, income, education, and internet access, confirming that the barrier is dispositional rather than resource-based. Consistent with this interpretation, the effect is concentrated among less ed-

ucated individuals and in regions with weak investor protection, where neither human capital nor institutional safeguards compensate for the trust deficit only children carry from childhood.

To pin down the mechanism, we exploit a wave of FinTech platform scandals in China after 2015. If the socialization channel operates through trust, only children should retreat more sharply when platform credibility is damaged, and the data bear this out. Only children reduce digital wealth management participation following scandals more than individuals with siblings, precisely in the product category where platform opacity places the greatest demands on trust. Three further tests tighten the interpretation: the effect is specific to FinTech and absent for traditional financial products, inconsistent with general financial disengagement; it does not scale with sibling count, weighing against information diffusion; and it persists despite comparable digital access, confirming that the barrier is dispositional rather than technological. These individual-level patterns aggregate: provinces with higher only-child shares develop fewer FinTech firms and lower adoption of digital financial services.

Our paper contributes to several literatures. In household finance, recent work has begun to open the family black box. Prior studies document vertical socialization, showing that parents transmit risk and trust traits to their children (Barnea, Cronqvist and Siegel, 2010; Black, Devereux, Lundborg and Majlesi, 2017). Additionally, Ke (2021) shows that horizontal dynamics between spouses shape portfolio decisions within the household. We add a novel dimension: horizontal socialization among siblings during childhood, whose effects on investor traits persist into adulthood and shape financial decisions decades later. Our paper also speaks to the emerging FinTech literature, where recent work shows that trust shapes borrowers' choice of digital lenders (Yang, 2025), that deep-rooted cultural traits distort financial decisions on FinTech platforms (D'Acunto, Ghosh and Rossi, 2026), and that repeated digital payment use can progressively lower psychological barriers to risk-taking, but only once an initial adoption threshold is cleared (Hong, Lu and Pan, 2025).[2] We identify a fundamentally different class of demand-side determinant: the psychological composition of the investor base, shaped by family structure decades before the adoption decision.

As fertility continues to decline worldwide, and as the financial system becomes increasingly

[2]For overviews of the FinTech literature, see Philippon (2016), Goldstein et al. (2019), and Thakor and Merton (2024). The vast majority of existing research focuses on the supply side of FinTech markets, including lending, screening, and competition among intermediaries. Individual-level studies of demand-side adoption remain rare.

digital, algorithmic, and platform-based, the interaction between demographic structure and financial innovation adoption becomes a first-order question. Understanding the investor psychology of shrinking families is essential for anticipating how the demographic transformation now underway will shape the evolution of financial markets.

II Conceptual Framework and Hypothesis Development

Family socialization during childhood may shape the psychological traits that drive investor behavior. We develop a simple framework that distinguishes two types of childhood socialization — vertical interactions with parents and horizontal interactions with siblings — and use it to organize the related literature and derive testable hypotheses. This framework clarifies why the presence or absence of siblings during childhood may have long-lasting consequences for investor traits and, through these traits, for the adoption of financial technology.

A Framework for Vertical and Horizontal Childhood Socialization

Consider a child i who grows up with $n_i \geq 0$ siblings. Parents are endowed with a finite stock of parental attention A, which is divided equally among children, so each child receives:

$$v_i = \frac{A}{n_i + 1} \tag{1}$$

Total childhood socialization consists of two components:

$$T_i = v_i + h_i \tag{2}$$

where v_i denotes *vertical* socialization with parents and $h_i = h(n_i)$ denotes *horizontal* socialization with siblings, with $h(0) = 0$, $h'(n_i) > 0$, and $h''(n_i) \leq 0$. Vertical interaction is characterized by authority asymmetry: the parent holds superior resources, information, and power, so the child has fewer opportunities to practice the reciprocal negotiation, trust formation, and social experimentation that peer-level interaction requires. Horizontal interaction occurs among equals,

creating the conditions for genuine reciprocity, experimentation, and social learning.[3]

As the number of siblings increases, the two components move in opposite directions: each additional sibling reduces the share of vertical socialization available per child while simultaneously adding a new source of horizontal peer interaction. Formally:

$$\frac{\partial v_i}{\partial n_i} < 0 \qquad \frac{\partial h_i}{\partial n_i} > 0 \tag{3}$$

Only children represent the limiting case: they receive the maximum vertical socialization $v_i = A$ and, by construction, zero horizontal socialization $h_i = 0$.

Adult psychological traits ψ_i are partially formed during childhood as a function of both interaction types:

$$\psi_i = \psi(v_i, h_i) \tag{4}$$

where v_i and h_i are measured over the childhood period, consistent with the developmental psychology literature documenting that sibling interactions shape personality and social skills.

This framework shows how the absence of siblings shapes traits through two conceptually distinct channels. The first is the *parenting channel*: because parental attention is undivided, only children receive more intensive vertical socialization, which may shape traits indirectly through concentrated attention, overprotective behavior, and a home environment structured around a single child, independent of anything a sibling would have contributed directly. The second is the *sibling interaction channel*: only children lack the horizontal peer interactions through which trust, risk tolerance, and social network engagement develop directly, through repeated practice with equals. Both channels operate simultaneously, and our empirical design identifies their combined effect.

The net effect of only-child status on any given trait depends on the relative magnitudes and directions of the two channels, which we characterize for the traits most relevant to financial decisions in the next subsection. Formally, the net effect on trait ψ_i, defined as the difference in

[3]Two simplifying assumptions are maintained for tractability. First, parental attention is divided equally among children. In practice, allocation may vary systematically, for example male offspring may receive preferential attention in patriarchal societies, and firstborn children may receive disproportionate attention in hierarchical family structures. Second, the additive specification assumes that vertical and horizontal socialization are substitutes and that total socialization is fixed. In principle, the two types may be complements, and total socialization may vary with family size.

trait levels between an only child and an otherwise identical individual with at least one sibling, decomposes as a first-order approximation into the contributions of each channel:

$$\Delta\psi_i = \underbrace{\frac{\partial\psi_i}{\partial v_i} \cdot \Delta v_i}_{\text{parenting channel}} + \underbrace{\frac{\partial\psi_i}{\partial h_i} \cdot \Delta h_i}_{\text{sibling interaction channel}} \tag{5}$$

where $\Delta v_i > 0$ and $\Delta h_i < 0$ capture the differences in parental attention and horizontal socialization between an only child and an otherwise identical individual with at least one sibling, holding all other characteristics constant. The signs of the partial derivatives depend on the psychological mechanisms through which each channel operates, which we establish in the next subsection. When the two terms have the same sign, the effect of only-child status on the trait is unambiguous. When they have opposite signs, the net direction depends on which channel dominates. We assume that the traits shaped during childhood persist into adulthood, consistent with evidence that early-life experiences have lasting effects on preferences and behavior (Cameron et al., 2013).

B Family Socialization and Investor Traits

We now apply the framework to establish predictions for investor traits.[4] For the parenting channel, concentrated parental attention cultivates security but also overprotection that reinforces caution over exploration, suggesting that only children are more risk averse than those with siblings (Bowlby, 1969; Parker, 1983). The effect on trust is ambiguous. Intensive parental attention builds attachment and security, which may support trust formation within close relationships (Putnam, 2000), but the absence of horizontal socialization may limit the development of generalized trust toward strangers. The effect on social network engagement points more clearly in one direction: a home environment structured around a single child substitutes parental bonds for peer network development, limiting the breadth of social engagement (Dunn, 1988).

For the sibling interaction channel, the evidence points in one direction. Horizontal socialization among equals reduces risk aversion, develop trust, and strengthen social network engagement. Siblings provide the primary arena in which children practice reciprocity, perspective-taking, and

[4]Falbo and Polit (1986) provide a meta-analysis of personality differences between only children and those with siblings.

negotiation that parental relationships may not fully replicate (Piaget, 1932; Dunn, 1983), and children with siblings outperform only children on tasks requiring assessment of others' intentions (McAlister and Peterson, 2007).[5] Each of these traits has implications for investor behavior: risk aversion and trust are first-order determinants of financial market participation (Guiso, Sapienza and Zingales, 2004, 2008), and the social engagement fostered by horizontal socialization shapes the information networks through which financial decisions are made (Hong, Kubik and Stein, 2004).

A growing body of work in household finance has traced investor traits to their origins within the family. Models of cultural transmission predict that parents shape children's risk preferences and trust (Bisin and Verdier, 2001), and Dohmen, Falk, Huffman and Sunde (2012) provide empirical confirmation using parent-child pairs. Knüpfer, Rantapuska and Sarvimäki (2023) document intergenerational transmission of security holdings. Several studies document that the shared family environment contributes meaningfully to investor behavior (Barnea, Cronqvist and Siegel, 2010; Cesarini, Johannesson, Lichtenstein, Sandewall and Wallace, 2010; Black, Devereux, Lundborg and Majlesi, 2017). More recently, Ke (2021) and Gu, Peng and Zhang (2024) show that spousal interactions within the adult household, another form of horizontal socialization, though operating within the adult household rather than during childhood, also shape portfolio decisions.[6] This body of work focuses on vertical transmission from parents to children or horizontal dynamics among adult spouses, leaving the horizontal peer channel during childhood unexamined.[7]

> **Hypothesis 1 (Investor Traits):** *Only children exhibit higher risk aversion, lower generalized trust, and lower social network engagement than individuals with siblings if the horizontal socialization deficit outweighs any countervailing effect from the parenting channel.*

[5] Wu, Cai, Xiong, Dong, Guo, Han and Ye (2021) provide neuroscientific support using brain imaging in a sequential trust game, finding that only children exhibit lower repayment, less reciprocation, and weaker interpersonal brain synchronization than non-only-children.

[6] See also Love (2010) on marital status and portfolio choice.

[7] Detlefsen et al. (2024) document that birth order and sibling gender composition shape risk aversion and trust, and Agarwal, Cochardt and Orlov (2025) find that later-born fund managers trade more aggressively, attributing the pattern to sibling rivalry during childhood. Both confirm that sibling dynamics shape financial behavior but examine variation within sibling groups rather than the presence versus absence of siblings.

C Family Socialization and FinTech Adoption

The investor attributes shaped by family socialization predict lower FinTech adoption among only children. A competing economic explanation predicts the opposite: the absence of informal sibling networks may increase demand for formal digital alternatives.

C.1 Family Socialization Hypothesis

FinTech platforms make different trust demands than traditional financial institutions. Rather than relying on physical branches, long-standing reputations, and personal relationships, users must trust algorithms and brands with limited track records. Okat, Paaso and Pursiainen (2025) confirm that trust in incumbent financial institutions does not predict FinTech adoption, establishing that FinTech requires a distinct form of trust. FinTech intermediaries lack the face-to-face relationships through which traditional financial advisors build client trust (Gennaioli, Shleifer and Vishny, 2015), and financial innovation more broadly embeds decision-making in algorithms that are opaque to households (Reuter and Schoar, 2024), making generalized trust the binding input for platform adoption. D'Acunto, Ghosh and Rossi (2026) provide direct evidence that deep-rooted cultural traits toward trust suppress FinTech adoption even when access to technology is unrestricted. The trust deficit that the sibling interaction channel produces should therefore disproportionately raise the barrier to FinTech adoption among only children.[8]

Beyond trust, FinTech adoption requires willingness to experiment with unfamiliar products whose risk-return profiles are difficult to evaluate ex ante, and exposure to peer networks through which information about novel platforms diffuses. On risk aversion, Hong, Lu and Pan (2025) show that repeated use of FinTech payment technology progressively lowers psychological barriers to investment, but this virtuous cycle of familiarity and trust-building presupposes a willingness to begin, a threshold that heightened risk aversion raises. On social network engagement, individuals embedded in denser peer networks receive more word-of-mouth exposure to FinTech platforms, lowering the information costs of adoption (Brown, Ivković, Smith and Weisbenner, 2008). Only

[8] In parallel corporate finance settings, Chemmanur, Tang and Wu (2025) show that single-child CEOs shaped by the OCP invest less in innovation and foster cultures that discourage trust and collaboration, and Wang, Gonzalez, Hagendorff and Sila (2025) find that only-child CEOs are associated with worse firm performance, particularly when industry competition is stronger. Both are consistent with the same psychological channels operating at the firm level.

children, whose family socialization was exclusively vertical, enter adulthood with all three deficits simultaneously: lower trust, heightened risk aversion, and thinner social networks, each of which raises the adoption threshold independently.

> **Hypothesis 2a (Family Socialization):** *Only children are less likely to adopt FinTech than individuals with siblings if the psychological traits shaped by childhood socialization — heightened risk aversion, lower generalized trust, and reduced social network engagement — raise the adoption threshold for novel and opaque financial platforms.*

C.2 Family Substitution Hypothesis

A competing explanation operates through adult economic networks. In environments with incomplete financial markets, family networks partially resolve contracting frictions that limit access to formal credit, insurance, and savings (Rosenzweig, 1988). Siblings provide informal risk sharing and credit that exploit the information advantages of repeated, high-trust relationships (Townsend, 1994). Evidence from China confirms that these mechanisms operate within the sibling network specifically: Niu, Wang, Li and Zhou (2020) show that having more brothers increases stock market participation through informal risk sharing.

As sibling networks thin, these informal services erode and the marginal benefit of formal alternatives rises (Kranton, 1996). FinTech platforms are particularly well-suited as substitutes, offering scalable digital alternatives for the credit, investment, and transfer services that siblings informally provide (Jack and Suri, 2014), and Yang (2025) shows that households readily redirect demand toward FinTech lenders when existing financial relationships weaken. If this substitution channel dominates, only children would adopt FinTech at higher rates precisely because they lack the informal financial networks that siblings provide.

> **Hypothesis 2b (Family Substitution):** *Only children are more likely to participate in FinTech than individuals with siblings if the absence of informal sibling-based financial networks increases demand for formal digital financial services.*

III Institutional Setting and Identification Strategy

We exploit China's OCP as a source of exogenous variation in family structure (Zhang, 2017). In October 1978, the government replaced the voluntary "Later, Longer, Fewer" family planning campaign, which permitted two children, with strict one-child fertility control. That same year, China's constitution incorporated family planning as state policy, elevating fertility control from administrative guidance to legal mandate. Sanctions for non-compliance, including dismissal from employment and substantial fines, were implemented nationwide beginning in January 1980, while compliant families received long-term subsidies and priority access to social services. The policy was enforced through a comprehensive governance system linking fertility behavior to household registration, children's school enrollment, and social security eligibility. Provincial and prefectural officials were evaluated on OCP compliance alongside economic growth and social stability, giving local administrators strong career incentives to enforce the mandate. The demographic response was immediate: China's total fertility rate fell 18.6% in 1980, the largest single-year decline in Chinese history outside the 1959 famine, and the urban rate dropped to 1.1 children per woman by year-end, implying that only-child status for the post-1980 urban cohort is overwhelmingly policy-imposed rather than reflecting endogenous parental choice (Tang, 2007).

These policy features support a regression discontinuity design (RDD) around the 1980 birth-year cutoff. The identifying assumption is that individuals born just before versus just after the cutoff are comparable in unobservable characteristics, so that any discontinuity in outcomes reflects the policy-induced shift from a two-child to a one-child regime rather than a pre-existing difference between adjacent cohorts. Three features of our design address the principal identification concerns in OCP studies (Zhang, 2017). First, because China's market-oriented economic reforms began contemporaneously, cohort differences could reflect changing economic conditions; however, reforms affected all birth cohorts continuously and cannot produce the discrete jump in family structure that our RDD requires. Second, cross-regional variation in enforcement intensity may reflect local fertility preferences, but we exploit temporal variation at a single birth-year cutoff within areas where enforcement was strictest, and known exceptions — ethnic minorities, rural households, and parents in high-risk occupations — are well documented (Guo, Zhang, Gu and Wang, 2003),

allowing us to construct a sample in which the policy binds tightly. Third, the OCP affected not only sibling count but also the concentration of parental resources per child (Rosenzweig and Zhang, 2009); we address this by testing whether results survive controls for wealth, income, education, and internet access, which would absorb any effect operating through resource concentration rather than preference formation.

The OCP thus provides an ideal setting for studying both the investor psychology of shrinking families and how demographic decline shapes investor behavior. On the psychology side, the policy created a large population of adults whose traits — risk aversion, generalized trust, and social network engagement — were formed entirely without sibling interaction, allowing us to test whether these differ systematically from those of individuals who grew up with siblings. On the investor behavior side, China's rapid emergence as the world's largest FinTech market, spanning online lending, digital wealth management, and mobile payments, allows us to test whether these trait differences shape the adoption of financial innovation.

IV Data

A Data Sources and Sample Construction

Our data come from the China Household Finance Survey (CHFS), a nationally representative biennial survey conducted by the Survey and Research Center for China Household Finance (Gan et al., 2014). We use the 2015, 2017, 2019, and 2021 waves, the four waves that include detailed FinTech participation and attitudinal modules, with 2021 the most recent wave currently available.[9] The survey collects micro-level data on demographic characteristics, financial assets, liabilities, income, and financial decision-making from approximately 35,000 households across 29 provinces, autonomous regions, and municipalities in each wave. It employs a stratified, three-stage probability-proportional-to-size sampling design with face-to-face interviews conducted by trained professionals, and achieves a refusal rate of approximately 11%, comparable to leading international household surveys such as the U.S. Survey of Consumer Finances and the Survey of Health, Ageing and Retirement in Europe.

[9]The CHFS has previously been used by other financial economists, including Chen, Chen and He (2019) and Niu, Xiang, Yang and Zhou (2026).

We restrict the sample to household heads aged 16 to 60 (males) or 16 to 55 (females), corresponding to China's statutory retirement ages. Consistent with our identification strategy, we exclude three provinces where the OCP was not universally enforced or where higher fertility was permitted (Guangxi, Ningxia, and Inner Mongolia), leaving 26 provinces in the sample. After dropping observations with missing key variables, we obtain an unbalanced panel of 36,780 household-year observations.

B Variable Construction

We organize the variables into five groups: only-child status, investor traits, FinTech adoption, controls, and province-level variables. Detailed definitions are provided in Appendix A.

Only-child status. Our treatment variable, *Only Child*, equals one if the respondent reports having no siblings and zero otherwise. We also construct *Siblings*, a count variable measuring the number of siblings, to examine intensive-margin effects.

Investor traits. We measure three psychological traits through which family structure may shape financial behavior. *Risk Aversion* is based on the question "Suppose you have an asset available for investment, which type of project would you choose?" with responses coded from 1 (high risk, high return) to 5 (unwilling to bear any risk). *Trust* is measured by "To what extent are you willing to trust strangers?" on a five-point scale from very unwilling (1) to very willing (5), consistent with the generalized trust measure in the World Values Survey. *Socialization* is the ratio of total cash and in-kind exchanges with relatives and friends (including banquets, gifts, and red envelopes) to total household assets, capturing social network engagement.[10]

FinTech adoption. We measure participation along three dimensions corresponding to the primary FinTech segments in China. Our primary measure for each segment is an extensive-margin indicator capturing the threshold decision to adopt. For online lending and digital wealth management, we additionally construct a continuous share scaled by total household financial assets as a supplementary intensive-margin measure. *Online Lending* equals one if the respondent borrows from or lends through an online platform; *Online Lending Share* is the ratio of annual

[10] In the Chinese context, reciprocal gift-giving is a primary mechanism for maintaining interpersonal relationships and accumulating social capital (Hudik and Fang, 2020).

online lending amounts to total household financial assets.[11] *Digital Wealth Management* is an indicator for holding digital wealth management products; *Digital Wealth Management Share* is the ratio of annual digital wealth management product holdings to total household financial assets. *Mobile Payment* equals one if the respondent holds a third-party payment account (e.g., Alipay or WeChat Pay).

Control variables. Following the household finance literature, we control for: gender (*Male*), marital status (*Married*), log age (*Age*), education level (*Education*),[12] employment status (*Employed*), whether the respondent has children (*Offspring*), log household income (*Total Income*), self-reported health relative to peers (*Health*), financial industry employment (*Fin Job*), and homeownership (*Owns Property*). We also control for the individual's internet use (*Internet Access*) and the ratio of household communication expenditures to total household assets (*Communication Expenditure Share*). Continuous control variables are winsorized at the 1% level in each tail.

Province-level variables. Several analyses require province-level measures. *Investor Protection* is the legal environment sub-index from the NERI provincial marketization index. *FinTech Scandal* is the ratio of negative to total FinTech news coverage in the province, constructed from CSMAR data. FinTech development is measured by the Peking University Digital Financial Inclusion Index (*FinTech Aggregate Index*) and its sub-indices (*Coverage Breadth* and *Usage Depth*), as well as *Incumbent FinTech Firms* (log) and *New FinTech Entrants* from Tianyancha business registration data. *Only-Child Share* is the provincial proportion of only children in our CHFS sample. Province-level regressions also control for GDP, industrial structure, trade openness, patent activity, industrialization, financial development, and telecom service.

C Descriptive Statistics

Table 1 reports summary statistics for our sample. Panel A describes individual characteristics. Only children account for approximately 20% of observations, consistent with the population struc-

[11] We include both borrowing and investing through online platforms (including peer-to-peer and crowdfunding). For loan amounts, we prioritize detailed amounts when reported and use interval midpoints otherwise. For open-ended intervals, we follow standard conventions: above 10,000 yuan is coded as 15,000 yuan and below 5,000 yuan as 2,500 yuan.

[12] The education scale corresponds to: 1 = never attended school; 2 = elementary school; 3 = junior high school; 4 = high school; 5 = secondary vocational school; 6 = junior college; 7 = university undergraduate; 8 = master's degree; 9 = doctoral degree.

ture under the OCP. The average respondent has 2.1 siblings and is approximately 40 years old. The sample is gender-balanced (48.8% male) and predominantly married (82.9%). Average education falls near the high-school level, and roughly three-quarters of respondents are employed. Average log household income is 11.06, corresponding to approximately RMB 64,000. Nearly 87% of respondents own property, consistent with an established urban sample, while 2.5% work in the financial industry.

Panel B reports the investor trait variables. Average *Risk Aversion* is 3.80 on a five-point scale, indicating that the typical respondent is quite cautious. Average *Trust* is 2.28, well below the scale midpoint of 3. *Socialization*, which measures the ratio of reciprocal gift and cash exchanges with relatives and friends to total household assets, averages 0.4% with a median of zero, reflecting the skewed nature of informal social transfers.

Panel C describes FinTech adoption. The most notable feature is the wide dispersion across product categories: *Mobile Payment* adoption reaches 59.0%, *Digital Wealth Management* stands at 16.2%, while *Online Lending* stands at 3.7%. The two continuous share measures, scaled by total household financial assets, average a small fraction with median zero in both cases. The zeros are not censored or top-coded values but reflect actual non-participation. Our sample period covers the early phase of FinTech adoption, the segment of the technology diffusion curve in which a heavy mass at zero is the expected feature (Brynjolfsson and McElheran, 2016; Hong, Lu and Pan, 2025). The binary participation indicator captures the first-order economic decision facing each household — whether to engage with a new financial technology at all — while the share allocated conditional on participation is second-order. This heterogeneity across product categories generates the variation that our empirical analysis exploits.

Panel D reports holdings of traditional financial products as shares of total household financial assets. Portfolios are dominated by *Cash & Bank Deposit*, which averages 69.5% of household financial assets. *Stocks* average 4.0%, *Mutual Funds* 1.3%, and *Bonds* are negligible. We return to these variables when testing whether only children exhibit a general aversion to risky financial products or one specific to FinTech.

V Empirical Results

A Empirical Methodology

We estimate the effect of only-child status on investor traits and FinTech adoption using two approaches. We begin with ordinary least squares (OLS):

$$Y_{i,t} = \beta_1 \, \text{OnlyChild}_{i,t} + \boldsymbol{\gamma}' X_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{6}$$

where $Y_{i,t}$ is the outcome of interest for individual i in year t, $\text{OnlyChild}_{i,t}$ is the only-child indicator, $X_{i,t}$ is the vector of controls, and μ_p and δ_t are province and year fixed effects. Individual fixed effects are not included because only-child status is time-invariant and would be absorbed. Standard errors are clustered at the individual level to account for serial correlation across survey waves.[13]

OLS estimates of β_1 may be biased if only-child status correlates with unobserved parental or regional characteristics that independently affect outcomes. To address this, we implement a fuzzy regression discontinuity design (RDD) exploiting the discontinuous increase in only-child probability at the 1980 birth-year cutoff (Hahn, Todd and Van der Klaauw, 2001).[14] The RDD compares individuals born just before and just after the cutoff who are otherwise similar, using the policy-imposed discontinuity in only-child probability to isolate the effect of family structure from the parental selection that confounds cross-sectional comparisons. The LATE interpretation requires a monotonicity assumption: the policy weakly increased only-child probability for all individuals near the cutoff, ruling out defiers. This is credible given that the OCP paired strict sanctions for non-compliance with subsidies for compliance.

The first stage models the probability of being an only child as a function of the policy cutoff:

$$\text{OnlyChild}_{i,t} = \pi_1 d_i + f(\text{Birth}_i - c) + d_i \cdot g(\text{Birth}_i - c) + \boldsymbol{\gamma}' X_{i,t} + \mu_p + \delta_t + \nu_{i,t} \tag{7}$$

[13] Results are robust to clustering at the province level to address spatial correlation and at the birth-year level following Lee and Card (2008).

[14] The design is fuzzy rather than sharp because the policy did not deterministically assign only-child status: some individuals born after 1980 have siblings due to ethnic minority exceptions or non-compliance, while some born before 1980 are only children by parental choice.

The second stage estimates the effect of instrumented only-child status on outcomes:

$$Y_{i,t} = \beta_1 \widehat{\text{OnlyChild}}_{i,t} + f(\text{Birth}_i - c) + d_i \cdot g(\text{Birth}_i - c) + \boldsymbol{\gamma}' X_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{8}$$

where d_i equals one if individual i was born after the 1980 cutoff c. The functions $f(\text{Birth}_i - c)$ and $f(\text{Birth}_i - c) + d_i \cdot g(\text{Birth}_i - c)$ are linear polynomials in the running variable estimated separately on the left and right sides of the threshold, respectively, following standard practice for discrete running variables (Cattaneo et al., 2020).[15] $X_{i,t}$ includes the same controls as equation (6) except log age, which is collinear with the running variable in the RDD specification. Because the CHFS is a pooled panel with respondents observed across survey waves, the RDD bandwidth sample pools all available observations for individuals born within five years of the cutoff. Standard errors clustered at the individual level account for the resulting within-person correlation.

We restrict the sample to individuals born within five years of the cutoff as our baseline bandwidth. Because the running variable is discrete, taking only integer values by birth year, data-driven optimal bandwidth procedures designed for continuous running variables can yield unreliably narrow bandwidths with insufficient support points for local smoothing (Cattaneo et al., 2020). We therefore rely on fixed bandwidths and examine robustness to alternatives of four and six years.[16] Instrument relevance is assessed using the Kleibergen and Paap (2006) and Cragg and Donald (1993) F-statistics for weak instruments.

B Family Structure and Investor Traits

Table 2 reports the effect of only-child status on three investor traits: risk aversion, generalized trust, and social network engagement. Panel A presents OLS estimates. Only-child status is associated with significantly higher risk aversion (0.126, $p < 0.01$) and significantly lower trust (-0.041, $p < 0.10$) and socialization (-0.001, $p < 0.01$), with signs consistent with Hypothesis 1 across all three investor traits.

Panel B reports the first-stage estimates from the fuzzy RDD. The coefficient on the post-cutoff

[15] Replicating the specification using a quadratic polynomial yields consistent discontinuities at the 1980 cutoff.

[16] Applying the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth procedure to the subsample with birth month information, which supports a continuous running variable, yields results consistent with the baseline estimates.

indicator d_i is positive and statistically significant in all three subsamples, confirming that the 1980 cutoff generates a discrete increase in the probability of being an only child. The Kleibergen and Paap (2006) and Cragg and Donald (1993) F-statistics exceed conventional thresholds for instrument relevance.

Panel C presents the second-stage estimates. The magnitudes are larger than OLS, consistent with the LATE isolating compliers whose only-child status was policy-imposed. Only-child status increases *Risk Aversion* by 1.852 ($p < 0.01$, column (1)), consistent with concentrated parental attention cultivating caution over exploration. *Trust* declines by 1.060 ($p < 0.05$, column (2)), reflecting the absence of the sibling interactions through which generalized trust develops. *Socialization* declines by 0.021 ($p < 0.05$, column (3)), indicating that only children maintain substantially weaker social networks as adults. That the RDD estimates exceed OLS in magnitude is consistent with positive selection: parents who voluntarily chose one child before the policy tend to be wealthier and more educated, attenuating OLS toward zero.

These results support Hypothesis 1 and extend the laboratory findings of Cameron, Erkal, Gangadharan and Meng (2013) to nationally representative field data. The psychological consequences of growing up without siblings persist into adulthood and shape the investor traits that underpin financial decisions.

C Family Structure and FinTech Adoption

C.1 Baseline Estimates

Table 3 reports estimates of the effect of only-child status on FinTech adoption. Panel A presents OLS estimates. Across all five specifications, only-child status is associated with significantly lower adoption. For online lending, the coefficient implies a 1.3 percentage point reduction in participation ($p < 0.01$, column (1)). For digital wealth management, only children are 4.2 percentage points less likely to participate ($p < 0.01$, column (3)). Mobile payment adoption is 5.8 percentage points lower ($p < 0.01$, column (5)). The share measures in columns (2) and (4) are negative and significant, confirming that the effect extends to the intensive margin.

Panels B and C confirm these patterns using estimators tailored to the structure of each outcome. Panel B reports Logit estimates for the three binary participation indicators, addressing

functional form concerns at the extensive margin.[17] Panel C reports PPML estimates for the two continuous share variables, the standard specification for non-negative intensive-margin outcomes. Only-child status remains negative and significant across all specifications. The PPML estimates imply that only-child status reduces the expected online lending share by approximately 40% and the expected digital wealth management share by approximately 26%.

These patterns point uniformly toward the family socialization hypothesis: only children adopt less FinTech, not more, suggesting that the psychological barriers from growing up without siblings outweigh any increased demand for formal financial alternatives. Because only-child status may correlate with unobserved characteristics, we turn to the RDD to establish causality.

C.2 RDD Estimates

Table 4 reports the RDD estimates. Panel A presents the first-stage relationship between the post-1980 cutoff indicator d_i and only-child status. The coefficient on d_i is positive and significant across three progressively saturated specifications, declining from 0.091 with year fixed effects only (column (1)) to 0.069 in the fully saturated specification with province fixed effects and the full set of controls (column (3)). The fully saturated estimate implies that the policy increased the probability of being an only child by approximately 7 percentage points, roughly a one-quarter shift relative to the baseline only-child probability of 27% in the five-year bandwidth sample.[18] The Kleibergen and Paap (2006) and Cragg and Donald (1993) F-statistics exceed conventional thresholds for instrument relevance.

Panel B presents the second-stage estimates. Only-child status significantly reduces FinTech adoption across the three participation measures, confirming the OLS patterns under a design that addresses endogeneity. The LATE coefficients are large in magnitude, reflecting the fuzzy first stage in which the policy shifts only-child probability by approximately 7 percentage points at the cutoff. To compare the RDD and OLS estimates on the same scale, we compute the implied reduced-form effects as the product of the first-stage and second-stage coefficients, expressing the policy-induced changes in the same percentage-point units as the OLS estimates in Table 3. The

[17] A small number of province–years exhibit no variation in the dependent variable, and are therefore excluded from the Logit model specifications due to perfect prediction.

[18] The only-child share is higher (27%) in the narrower 5-year bandwidth sample than in the full sample (20%) because it contains a larger proportion of post-1980 cohorts.

1980 cutoff reduces online lending participation by 3.9 pp (column (1)), digital wealth management participation by 4.6 pp (column (3)), and mobile payment adoption by 5.9 pp (column (5)). For digital wealth management and mobile payment, the two largest segments, these reduced-form effects are close to their OLS counterparts (4.2 and 5.8 pp), indicating that selection bias in the cross-sectional estimates is limited and that the OLS association largely reflects the causal effect of family structure rather than parental selection. The online lending effect is larger than its OLS value (1.3 pp), consistent with greater attenuation in the smallest and most specialized segment, where adoption is rarest.

We treat the extensive-margin coefficients as our primary causal estimates: they capture the threshold adoption decision that defines whether a household participates in FinTech, and they are unaffected by the corner-solution problem inherent in the share variables. The digital wealth management share estimate in column (4) corroborates the participation results at the intensive margin, with only-child status reducing the amount allocated to digital wealth management as a fraction of household financial assets; the online lending share estimate in column (2) is negative but not statistically significant.

Figure 1 provides visual confirmation. Each panel plots FinTech adoption against birth month relative to the 1980 cutoff, with local polynomial fits and 95% confidence intervals.[19] *Online Lending* (Panel A), *Digital Wealth Management* (Panel B), and *Mobile Payment* (Panel C) all show a visible downward shift at the cutoff. Panel D plots a composite indicator, *FinTech*, equal to one if the individual participates in any of the three segments, with the fitted value dropping at the cutoff.

C.3 Validity and Robustness

We evaluate the validity of the RDD and the robustness of the estimates through a series of tests.

Density test. A fundamental requirement of the RDD is that individuals cannot precisely manipulate their position relative to the cutoff. We test for such sorting using the McCrary (2008) density test, reported in Figure 2. The test statistic of -0.041 ($p = 0.967$) indicates no evidence of manipulation, consistent with the fact that parents could not precisely control their child's birth

[19] We use months as the running variable in the graphical analysis because finer granularity enhances the visual presentation. In the regression analysis, we use years because a large proportion of CHFS respondents have missing birth month information.

timing relative to the policy.

Covariate smoothness. A valid RDD requires that predetermined characteristics vary smoothly through the cutoff. Panel A of Table 5 tests this directly. No predetermined covariate exhibits a significant discontinuity, supporting the identifying assumption. The only exception is *Siblings*, which is significant, as expected: it drops sharply at the cutoff, confirming that the policy reduced family size as intended.

Placebo cutoffs. If the baseline results reflect a pre-existing cohort trend rather than the OCP, artificial cutoffs away from 1980 should produce similar estimates. Panels B and C of Table 5 report results using placebo cutoffs at 1978 and 1982. None of the ten placebo coefficients is statistically significant, and several reverse sign, confirming that the effects are specific to the 1980 cutoff.[20]

Donut RDD. A potential concern is that parents who anticipated the OCP may have timed births around the 1980 cutoff. We address this by excluding individuals born in 1980 itself. Panel D reports the results. The coefficients remain negative across all five FinTech outcomes and statistically significant in four of five specifications, confirming that birth timing around the cutoff does not drive our findings.

Alternative bandwidths. The choice of a five-year bandwidth involves a bias-variance tradeoff. Panels E and F report results using alternative bandwidths of four and six years. The six-year bandwidth produces estimates close to the baseline, with all five coefficients significant. The four-year bandwidth yields larger magnitudes for several outcomes, consistent with a sharper treatment contrast closer to the cutoff, though two coefficients lose significance due to the reduced sample size. The qualitative pattern is stable across both alternatives.

Four additional aspects of our empirical design merit discussion. First, investor traits are measured by self-reported survey items, and classical measurement error would attenuate our estimates, biasing against finding the effects we document. Second, the trait variables have substantially smaller samples than the FinTech outcomes due to item non-response. We test whether non-response is systematic around the 1980 cutoff by estimating the RDD using a non-response indicator as the outcome; the cutoff indicator is insignificant across all three trait variables, con-

[20] As an additional placebo test, we replicate the RDD design centered on 1965, a birth year predating any family planning intervention in China. The estimated discontinuity is negligible and statistically insignificant across all FinTech outcomes.

firming that item non-response is not differentially concentrated among individuals born just before or just after the policy. Third, the LATE captures the effect for compliers near the 1980 cutoff, but the OLS estimates show the same qualitative pattern across the full sample, consistent with the limited selection bias documented above and suggesting that the relationship between only-child status and FinTech adoption extends beyond the complier population. Fourth, the FinTech participation coefficients remain statistically significant after applying the Romano and Wolf (2005) step-down correction for multiple hypothesis testing.[21]

D Addressing Alternative Explanations

We address three alternative explanations that could generate lower FinTech adoption among only children without implicating the psychological channels of the family socialization hypothesis.

D.1 Traditional Finance Participation

A natural question is whether only children avoid financial markets broadly or FinTech specifically. The investor traits we document — risk aversion, trust, and social network engagement — are not unique to FinTech adoption; prior work has established their role in traditional financial market participation as well. However, traditional financial institutions mitigate trust and risk concerns through physical branches, established reputations, regulatory guarantees such as deposit insurance, and personal relationships with advisors. FinTech platforms lack these safeguards, so the psychological barriers that only children carry should bind more tightly in digital finance.

Table 6 tests this prediction using three outcomes: a composite *FinTech* indicator, a relative preference measure that directly contrasts FinTech and traditional finance participation, and traditional finance participation alone.[22] The composite *FinTech* indicator confirms the baseline (-1.652, $p < 0.01$, column (1)). The relative preference measure yields a larger coefficient in absolute value (-1.742, $p < 0.01$, column (2)), confirming that the only-child effect involves a shift away from FinTech rather than a shift away from traditional finance. The coefficient on traditional

[21]The Romano-Wolf adjusted p-values are 0.002 for *Online Lending*, 0.008 for *Digital Wealth Management*, and 0.008 for *Mobile Payment*. The online lending share coefficient is not statistically significant in the baseline RDD and is therefore excluded from this correction.

[22]The relative preference measure, *FinTech vs. Traditional*, equals 1 if the individual participates in FinTech but not traditional finance, -1 if in traditional finance but not FinTech, and 0 otherwise. Traditional finance participation is defined as holding stocks, mutual funds, bonds, and bank deposits.

finance participation alone is statistically insignificant (column (3)), confirming that the only-child effect is specific to the segment where trust demands are highest. The lack of significance on traditional finance further reinforces the dispositional channel.

D.2 Information Diffusion

The family socialization hypothesis posits that siblings shape investor traits during childhood — the disposition to trust, to tolerate risk, and to engage socially — and that these traits persist into adulthood. An alternative channel operates not through childhood development but through siblings' role as *current* peer informants. The peer effects literature documents that social networks facilitate financial market participation through word-of-mouth information sharing (Hong, Kubik and Stein, 2004; Brown, Ivković, Smith and Weisbenner, 2008). Under this channel, only children adopt less FinTech because they have fewer family members transmitting information about financial products today, not because growing up without siblings shaped their preferences decades earlier. These two channels generate a distinct prediction: if current information flow drives the result, the adoption gap should scale with the number of siblings available as peer informants, whereas if childhood socialization drives it, the presence of any sibling during the formative years should suffice.

Panel A of Table 7 tests this prediction by splitting the sibling sample into those with exactly one sibling and those with two or more. The coefficients are negative and significant in the majority of specifications across both groups, and we cannot reject the equality of coefficients between the one-sibling and two-or-more-sibling groups in any specification, providing no evidence that the adoption gap grows with sibling count. What matters is whether an individual grew up with a sibling, not how many are available as informants in adulthood, consistent with the dispositional channel and ruling out information diffusion as the primary driver.

D.3 Technological Barriers

Only children may face higher costs of accessing digital platforms, so that the adoption gap reflects a technological rather than a psychological barrier. If this channel operates, only children should exhibit measurably lower digital access than individuals with siblings.

We test this using two proxies that capture both the extensive and intensive margins of digital engagement: an indicator for internet use and the ratio of household communication expenditures to total household assets. Panel B of Table 7 reports the RDD estimates using each proxy as the outcome. Neither coefficient is statistically significant. Only children have comparable digital access to individuals with siblings, confirming that the barrier to FinTech adoption is not the ability to access digital platforms but the willingness to use them for financial transactions, a dispositional constraint, not a technological one.

E Cross-Sectional Heterogeneity

If the adoption barrier operates through psychological traits rather than resource constraints, it should be most binding where neither human capital nor institutional safeguards lower the threshold for engaging with novel financial platforms. We test this prediction along two dimensions.

Education. We split the sample at the median education level and estimate the RDD separately for each group. Panels A and B of Table 8 report the results. Among individuals with below-median education, the only-child coefficients are negative across all five outcomes and statistically significant in four of five, with magnitudes comparable to or larger than the baseline estimates. Among individuals with above-median education, only one of the five coefficients is statistically significant, and only at the 10% level. This pattern is consistent with formal human capital substituting for the risk tolerance and trust that sibling interactions develop informally during childhood.

Investor protection. We divide provinces at the median of the NERI legal environment subindex and estimate the RDD separately for each group. Panels C and D report the results. In provinces with below-median investor protection, the only-child coefficients are negative across all five outcomes and significant in four, each at the 5% level or better. In provinces with above-median investor protection, none of the five coefficients is statistically significant. Stronger legal institutions appear to compensate for the trust deficit that only children carry into financial decisions.

The only-child effect is not uniform but conditional, binding most tightly where neither human capital nor institutional quality provides a counterweight, consistent with the dispositional channel

identified by the family socialization hypothesis.[23]

VI Economic Mechanisms

The preceding sections establish that only-child status shapes investor traits and reduces FinTech adoption. This section investigates the economic mechanisms linking these two findings.

A Mediation Analysis

The investor traits documented in Table 2 are the proposed channels through which family structure shapes FinTech adoption. Adding these traits to the second-stage specification should therefore attenuate the only-child coefficient. We test this by augmenting the second-stage RDD with *Risk Aversion*, *Trust*, and *Socialization*. A key advantage of our setting is that traits and FinTech outcomes are measured for the same individual in the same survey wave, so the mediation operates within-person rather than relying on cross-sectional proxies from different samples or periods.

Table 9 reports the results. Adding the three traits substantially attenuates the only-child coefficient: relative to the baseline RDD, the magnitude declines by approximately 56% for online lending, 40% for the digital wealth management share, and 48% for mobile payment, and by a smaller margin for digital wealth management participation. Because the trait variables enter alongside the instrumented only-child indicator rather than replacing it, this absorption reflects genuine channel overlap rather than a mechanical reduction in residual variation. The traits enter with the expected signs. *Trust* predicts adoption in the online lending specifications, where counterparty risk is most salient. *Risk Aversion* significantly reduces digital wealth management participation, where financial exposure is greatest. *Socialization* positively predicts the online lending share and mobile payment adoption, consistent with social network engagement lowering the threshold for experimenting with novel platforms.

Two caveats apply to the mediation interpretation, both of which strengthen rather than weaken our conclusions. First, the trait variables are themselves outcomes of only-child status, so their

[23] Augmenting the second-stage RDD with interactions between instrumented only-child status and indicators for above-median education and above-median investor protection yields positive and significant interaction terms for both moderators, confirming that the attenuation of the only-child effect by human capital and institutional quality is statistically robust.

inclusion as controls introduces post-treatment bias that may affect the magnitude of attenuation (Imai et al., 2010). Second, because traits and FinTech outcomes are measured contemporaneously in the survey, reported traits may partially reflect prior adoption experience rather than preceding it, which would itself produce attenuation. Both concerns are conservative, making our estimates a lower bound on the true importance of psychological traits as mediators of the only-child effect.

The only-child coefficient remains significant after the traits are included, indicating that the three traits absorb a substantial but not complete share of the effect. Each trait is captured by a single survey item, and measurement error in a mediator mechanically attenuates its estimated contribution, understating the effect of the traits. The residual is also consistent with the parenting channel, which may shape adoption through dispositions such as autonomy, tolerance of ambiguity, and openness to novelty that the survey does not measure. The results thus support the family socialization hypothesis: only-child status shapes FinTech adoption through the psychological traits that sibling interactions develop in childhood, rather than through the absence of sibling-provided financial networks in adulthood.

B The Trust Channel: Evidence from FinTech Scandals

The mediation analysis points to trust as one of the channels linking only-child status to FinTech adoption. The behavior of FinTech platforms during periods of damaged credibility provides a sharper, design-based test of this channel: if trust is operative, only children should respond more negatively than those with siblings when platform credibility is shocked, as their lower baseline trust implies that a given negative signal produces a larger revision in willingness to participate.

The wave of platform scandals that struck China's FinTech sector after 2015, when regulatory gaps and business model failures caused a large number of online lending and digital wealth management platforms to collapse or be exposed for fraud, provides the setting for this test. We measure provincial scandal exposure as the ratio of negative FinTech news to total FinTech news, denoted $S_{p,t}$.[24] Provincial fixed effects and time-varying province-level controls for GDP, financial development, and FinTech market size absorb the most plausible confounds. We estimate an

[24]We identify FinTech-related news reports in the CSMAR database using keyword matching and assign reports to provinces based on geographic references. We classify report sentiment using a Bayesian text classification model trained on Chinese-language corpora (SnowNLP), which assigns each text a probability score between 0 (most negative) and 1 (most positive). Reports scoring below 0.5 are classified as negative.

augmented RDD that interacts instrumented only-child status with scandal exposure:

$$Y_{i,t} = \beta_1 \widehat{\text{OnlyChild}}_{i,t} + \beta_2 S_{p,t} + \beta_3 \widehat{\text{OnlyChild}}_{i,t} \times S_{p,t} + f(\text{Birth}_i - c) + d_i \cdot g(\text{Birth}_i - c) + \gamma' X_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{9}$$

The coefficient of interest is β_3, which captures whether only children's FinTech adoption responds differentially to regional scandal exposure. The main effect β_1 captures the static adoption gap in a province with no scandal exposure, while β_3 captures the dynamic response. Standard errors are clustered at the province level in this specification to account for the province-year variation in scandal exposure.[25]

Table 10 reports the results. The main effect of only-child status (β_1) remains negative and significant across all five specifications, confirming that the adoption gap persists even in the absence of scandal exposure. The interaction term (β_3) is negative and statistically significant for online lending participation (-1.150, $p < 0.01$, column (1)), digital wealth management participation (-0.750, $p < 0.10$, column (3)), digital wealth management share (-0.513, $p < 0.01$, column (4)), and mobile payment (-0.524, $p < 0.10$, column (5)). The differential retreat among only children is thus broad-based across FinTech segments. The one exception is the online lending share (column (2)), the least precisely estimated outcome throughout our analysis.[26]

These results provide direct evidence that trust is an operative channel through which only-child status shapes FinTech adoption. Only children do not merely adopt less, but they retreat more sharply when platform credibility is challenged, exactly as the family socialization hypothesis predicts.

C Market-Level Evidence

The preceding analysis documents individual-level effects: only children have different investor traits and adopt less FinTech. If these demand-side effects are economically meaningful, they should propagate to market structure: provinces with a higher share of only children should exhibit

[25] Clustering at the individual level, as in the baseline specifications, yields qualitatively and quantitatively similar results.

[26] As a placebo test, we replace contemporaneous scandal exposure with values randomly drawn from pre-2015 years. The interaction term between only-child status and placebo scandal exposure is statistically insignificant across all five specifications, confirming that the differential response is specific to the period when actual platform failures occurred.

not only lower FinTech adoption but also fewer FinTech firms, as depressed demand reduces the incentive for platform entry. We test this prediction using province-by-year OLS regressions:

$$FinTech_{p,t} = \beta_1 \, OnlyChildShare_{p,t} + \gamma' Z_{p,t} + \mu_p + \delta_t + \varepsilon_{p,t} \tag{10}$$

where $FinTech_{p,t}$ measures regional FinTech development in province p in year t, $OnlyChildShare_{p,t}$ is the provincial share of only children, $Z_{p,t}$ is a vector of time-varying province-level controls (GDP, industrial structure, trade openness, patent activity, industrialization, financial development, and telecom service), and μ_p and δ_t are province and year fixed effects. We measure FinTech development along two dimensions: adoption, captured by the Peking University Digital Financial Inclusion Index, and entrepreneurial activity, captured by the number of FinTech firms in the province. Because the regional FinTech development indices are available at the province level for years in which household-level FinTech participation is not surveyed, this analysis incorporates the 2011 and 2013 waves in addition to the four waves used in the individual-level tests.[27]

Table 11 reports the results for 147 province-year observations. The provincial only-child share negatively predicts the aggregate FinTech development index (-28.605, $p < 0.05$, column (1)), its coverage breadth component (-21.683, $p < 0.10$, column (2)), and its usage depth component (-47.029, $p < 0.05$, column (3)). The only-child share is thus associated with lower FinTech development on both the extensive margin of coverage and the intensity of use. The direction and magnitude of these adoption effects are consistent with the individual-level RDD estimates aggregating to the provincial level. The only-child share also negatively predicts the stock of FinTech firms. The coefficient on the log number of incumbent FinTech firms is negative and significant (-1.201, $p < 0.05$, column (4)); a 10 percentage point increase in the only-child share corresponds to approximately 12% fewer incumbent firms. The coefficient on new FinTech entrants is negative but not statistically significant (column (5)).

The consistency between the individual-level RDD estimates and the province-level patterns reinforces the economic significance of the family socialization channel: when a larger share of the population carries the psychological barriers associated with only-child status, fewer platforms find

[27] The results are qualitatively and quantitatively similar when the analysis is restricted to the four waves used in the individual-level analysis.

it viable to operate, and digital financial services reach fewer users.

VII Conclusion

This paper establishes that the family structure in which an investor grows up has lasting consequences for financial decision-making. Exploiting China's One-Child Policy in a regression discontinuity design, we show that only children are more risk-averse, less trusting, and less socially engaged than individuals with siblings, revealing that what shapes investor traits is not only the *vertical* transmission of parental preferences but the *horizontal* socialization among siblings, a dimension the household finance literature has not previously examined. These trait differences suppress the adoption of FinTech but not traditional financial products, implying that the demand side of financial innovation has a demographic component that operates through traits formed decades before the adoption decision. Trust is the dominant channel: only children retreat more sharply from digital platforms following FinTech scandals, and the effect is concentrated where platform opacity is greatest. Strong investor protection and higher education attenuate the effect, suggesting that institutions and human capital can partially compensate for the socialization that siblings provide.

Our evidence challenges a central premise of FinTech: that technology can substitute for human relationships in financial intermediation. The results point in the opposite direction. When the human relationships that build trust and risk tolerance during childhood are absent, the psychological barriers to adopting financial technology rise, even when access to technology itself is unrestricted. The constraint is not technological but developmental, rooted in how traits form within the family long before any financial decision is made.

These findings speak beyond China. The OCP provides identification, but the psychological channels we document operate through childhood development, not through the policy itself. A child growing up without siblings experiences the same absence of formative peer interactions whether family size was mandated or chosen. Consistent with this, our OLS estimates, which do not rely on the policy discontinuity, show the same qualitative pattern as the RDD, suggesting that the relationship extends beyond policy compliers to the broader population of only children. The demographic forces that the OCP compressed into a single generation are now unfolding voluntarily

across Asia, Europe, and increasingly the United States, making the dynamics we document a preview rather than a special case.

Our results reveal a cost of demographic decline that existing research has not identified. Policy discussions of low fertility typically focus on dependency ratios, pension sustainability, and labor supply. We document a different consequence: demographic decline permanently alters the psychological composition of the investor base. Financial literacy can be taught; institutional quality can be reformed; digital access can be expanded. But the trait formation that occurs through sibling interaction takes place during a childhood window that cannot be reopened. Our provincial-level evidence confirms that this individual-level mechanism aggregates: regions with higher only-child shares develop fewer FinTech firms and experience lower adoption of digital financial services. On the demand side, the populations most in need of financial innovation — aging societies with thin family networks — may be precisely the populations least psychologically disposed to adopt it, making investments in financial education and investor protection necessary to close the gap that shrinking families create. On the supply side, the design of digital financial products may need to accommodate the trust profiles of a generation raised without siblings, favoring transparency, simplicity, and graduated engagement over algorithmic opacity.

More broadly, our findings contribute to a growing agenda on how the internal structure of the family shapes financial behavior, and suggest several directions for future work. One direct step would test the sibling socialization mechanism in reverse, exploiting Romania's Decree 770, which banned abortion and contraception in 1966 and forced an entire cohort into larger families (Pop-Eleches, 2006). A second frontier concerns the evolving nature of digital finance. As intermediation moves from platform-based FinTech toward AI-driven advisory services, the trust demands on investors will shift from trusting a brand to trusting an algorithm, a transition that may interact with childhood socialization in ways our data cannot yet capture. Finally, we have opened the family black box by documenting how one dimension of its internal structure — the presence or absence of siblings — shapes investor traits and financial behavior. Childhood development within the family is multidimensional, and other aspects of its internal structure may operate through similar developmental channels, offering a broader agenda for understanding how the family environment in which investors are raised shapes their financial decisions in adulthood.

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Appendix A: Variable Definitions

This table defines all variables used in the empirical analysis. Variables are organized by their role in the analysis.

Variable	Definition
Panel A: Treatment and Instrument	
Only Child	Indicator equal to one if the individual has no siblings.
Siblings	Number of siblings.
d_i	Post-cutoff indicator equal to one if the individual was born after the 1980 OCP cutoff.
Birth – c	Running variable measuring the individual's birth year (or birth month in figures) relative to the 1980 cutoff.
Panel B: Individual Characteristics	
Male	Indicator equal to one if the individual is male.
Married	Indicator equal to one if the individual is married.
Age	Log of the individual's age.
Education	Highest education, coded 1 (never attended school) to 9 (doctoral degree).
Employed	Indicator equal to one if the individual is currently employed.
Offspring	Indicator equal to one if the individual has children.
Total Income	Log of total household income.
Health	Self-reported health relative to peers, coded from 1 to 5.
Fin Job	Indicator equal to one if the individual is employed in the financial industry.
Owns Property	Indicator equal to one if the individual owns residential property.
Internet Access	Indicator equal to one if the individual uses the internet.
Communication Expenditure Share	Ratio of household communication expenditures to total household assets.
Panel C: Investor Traits	
Risk Aversion	Response to "Suppose you have an asset available for investment, which type of project would you choose?" coded from 1 (high risk, high return) to 5 (unwilling to bear any risk).
Trust	Response to "To what extent are you willing to trust strangers?" on a five-point scale from 1 (very unwilling) to 5 (very willing).
Socialization	Ratio of total cash and in-kind exchanges with relatives and friends (including banquets, gifts, and red envelopes) to total household assets.
Panel D: FinTech Adoption	
Online Lending	Indicator equal to one if the individual borrows from or lends through an online platform.
Online Lending Share	Ratio of annual online lending amounts to total household financial assets.
Digital Wealth Management	Indicator equal to one if the individual holds digital wealth management products.
Digital Wealth Management Share	Ratio of annual digital wealth management product holdings to total household financial assets.
Mobile Payment	Indicator equal to one if the individual holds a third-party payment account (e.g., Alipay or WeChat Pay).
FinTech (Composite)	Composite indicator equal to one if the individual participates in any of Online Lending, Digital Wealth Management, or Mobile Payment.
FinTech vs. Traditional	Equals 1 if the individual holds FinTech assets but not traditional finance assets, –1 if the individual holds traditional finance assets but not FinTech assets, and 0 otherwise. Traditional financial assets are defined as holding stocks, mutual funds, bonds, and bank deposits.
Total Household Financial Assets	Sum of stocks, mutual funds, bonds, cash and bank deposits, digital wealth management, and other financial assets (including non-digital wealth management, foreign exchange, and gold).

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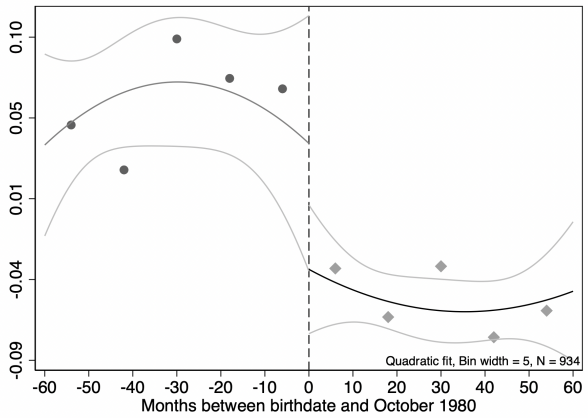
Appendix A – Continued

Panel E: Province-Level Variables	
Only-Child Share	Provincial proportion of only children in the CHFS sample in the survey year.
FinTech Scandal	Ratio of negative FinTech news to total FinTech news in the province, based on CSMAR news reports classified using SnowNLP sentiment analysis.
Investor Protection	NERI legal environment sub-index, capturing the density of professional services, commercial dispute resolution, and contract enforcement at the provincial level.
FinTech Aggregate Index	Peking University Digital Financial Inclusion Index, composite measure.
Coverage Breadth	Peking University Digital Financial Inclusion Index, coverage breadth sub-index.
Usage Depth	Peking University Digital Financial Inclusion Index, usage depth sub-index.
Incumbent FinTech Firms	Log of cumulative number of registered FinTech firms in the province, from Tianyancha business registration data.
New FinTech Entrants	Number of newly registered FinTech firms in the province in the survey year, from Tianyancha business registration data.
Log GDP	Log of provincial GDP.
Industrial Structure	Ratio of tertiary industry output to secondary industry output.
Trade Openness	Total goods imports and exports multiplied by exchange rate, divided by GDP.
Patents	Log of invention patent applications accepted.
Industrialization	Total industrial value divided by GDP.
Financial Development	Total deposits and loans of banking financial institutions divided by GDP.
Telecom Service	Total telecom services revenue divided by GDP.

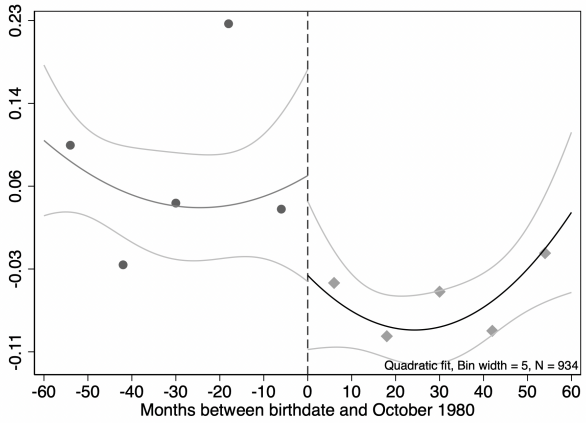
Figure 1: Family Structure and FinTech Adoption

This figure presents regression discontinuity plots for the relation between only-child status and FinTech adoption, using the 1980 OCP birth-year cutoff as the instrument. Each panel plots the mean outcome within 10-month bins against birth month relative to the 1980 cutoff, with local linear polynomial fits and 95% confidence intervals estimated separately on each side of the 1980 cutoff. The sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey (CHFS).

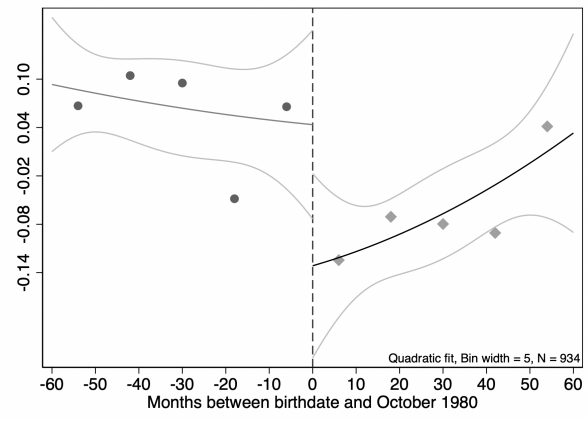
Panel A: Online Lending



Panel B: Digital Wealth Management



Panel C: Mobile Payment



Panel D: FinTech (Composite)

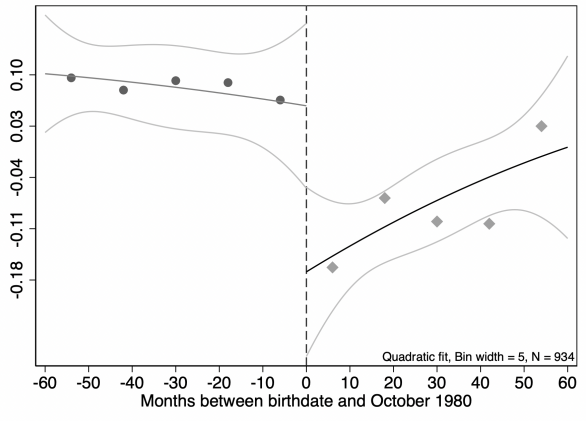


Figure 2: Density of the Running Variable

This figure presents the McCrary (2008) density test for manipulation of the running variable (birth month relative to the 1980 cutoff). The test examines whether there is a discontinuity in the density of the running variable at the 1980 cutoff, which would suggest that individuals sort around the threshold. The sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey.



Table 1: Descriptive Statistics

This table reports summary statistics for the estimation sample. The sample includes household heads within the OCP-relevant age range across the provinces where the policy was binding. Panel A describes individual characteristics. Panel B reports investor trait variables. Panel C reports FinTech adoption measures, including extensive-margin indicators and continuous measures scaled by total household financial assets. Panel D reports holdings of traditional financial assets as shares of total household financial assets. The data are from the China Household Finance Survey, including the 2015, 2017, 2019, and 2021 waves. All variables are defined in Appendix A.

Panel A: Individual Characteristics

Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Only Child	36,780	0.199	0.000	0.400	0.000	1.000
Siblings	36,780	2.112	2.000	1.715	0.000	7.000
Male	36,780	0.488	0.000	0.500	0.000	1.000
Married	36,780	0.829	1.000	0.377	0.000	1.000
Age (log)	36,780	3.697	3.784	0.278	2.833	4.094
Education	36,780	4.420	4.000	1.790	1.000	9.000
Employed	36,780	0.753	1.000	0.431	0.000	1.000
Offspring	36,780	0.843	1.000	0.364	0.000	1.000
Total Income (log)	36,780	11.063	11.223	1.247	6.217	13.467
Health	36,780	2.373	2.000	0.895	1.000	5.000
Fin Job	36,780	0.025	0.000	0.156	0.000	1.000
Owns Property	36,780	0.870	1.000	0.337	0.000	1.000

Panel B: Investor Traits

Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Risk Aversion	34,184	3.800	4.000	1.176	1.000	5.000
Trust	28,255	2.277	2.000	0.886	1.000	5.000
Socialization	36,585	0.004	0.000	0.013	0.000	0.081

Panel C: FinTech Adoption

Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Online Lending	36,780	0.037	0.000	0.189	0.000	1.000
Online Lending Share	36,780	0.006	0.000	0.042	0.000	0.364
Digital Wealth Management	36,780	0.162	0.000	0.368	0.000	1.000
Digital Wealth Management Share	36,780	0.050	0.000	0.173	0.000	0.979
Mobile Payment	36,780	0.590	1.000	0.492	0.000	1.000

Panel D: Traditional Financial Assets

Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Stocks	36,780	0.040	0.000	0.160	0.000	0.952
Mutual Funds	36,780	0.013	0.000	0.075	0.000	0.589
Bonds	36,780	0.001	0.000	0.014	0.000	0.282
Cash & Bank Deposits	36,780	0.695	1.000	0.408	0.000	1.000

Table 2: Family Structure and Investor Traits

This table reports second-stage estimates from a fuzzy RDD of the effect of only-child status on three investor trait variables: Risk Aversion, Trust, and Socialization. The instrument is the 1980 OCP birth-year cutoff. All RDD specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: OLS Estimates

	(1)	(2)	(3)
Dependent Variable	Risk Aversion	Trust	Socialization
Only Child	0.1263***	-0.0411*	-0.0007***
	(0.0236)	(0.0210)	(0.0002)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	34,184	28,255	36,585
R-squared	0.148	0.087	0.077

Panel B: RDD Estimates (First Stage)

	(1)	(2)	(3)
Dependent Variable	Only Child	Only Child	Only Child
d_i	0.1091***	0.1130***	0.0681***
	(0.0186)	(0.0212)	(0.0177)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
RDD Polynomial Controls	Yes	Yes	Yes
Kleibergen-Paap F-statistic	34.3	28.4	14.7
Cragg-Donald Wald F-statistic	61.5	55.4	24.5
N	8,682	7,436	9,639
R-squared	0.438	0.412	0.468

Panel C: RDD Estimates (Second Stage)

	(1)	(2)	(3)
Dependent Variable	Risk Aversion	Trust	Socialization
Only Child	1.8519***	-1.0604**	-0.0211**
	(0.5993)	(0.4639)	(0.0094)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	8,682	7,436	9,639

Table 3: Family Structure and FinTech Adoption (OLS, Logit, PPML Estimates)

This table reports estimates of the relation between only-child status and FinTech adoption using three estimation approaches. Panel A reports OLS estimates. Panel B reports Logit estimates. Panel C reports Poisson pseudo-maximum likelihood (PPML) estimates. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: Ordinary Least Squares (OLS) Estimates

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.0129***	-0.0035***	-0.0415***	-0.0100***	-0.0577***
	(0.0041)	(0.0009)	(0.0069)	(0.0034)	(0.0075)
Siblings	0.0016**	0.0001	-0.0030**	-0.0008	-0.0152***
	(0.0006)	(0.0002)	(0.0013)	(0.0006)	(0.0022)
Male	0.0046**	0.0009*	-0.0014	-0.0014	-0.0066
	(0.0022)	(0.0005)	(0.0040)	(0.0019)	(0.0049)
Married	-0.0026	-0.0007	-0.0397***	-0.0089***	0.0459***
	(0.0034)	(0.0008)	(0.0063)	(0.0031)	(0.0071)
Age	-0.0381***	-0.0083***	-0.1237***	-0.0467***	-0.2720***
	(0.0050)	(0.0013)	(0.0098)	(0.0045)	(0.0119)
Education	0.0034***	0.0002	0.0444***	0.0140***	0.0355***
	(0.0007)	(0.0002)	(0.0014)	(0.0007)	(0.0016)
Employed	0.0037	0.0012**	0.0086**	0.0082***	0.0384***
	(0.0023)	(0.0005)	(0.0041)	(0.0020)	(0.0054)
Offspring	-0.0160***	-0.0010	-0.0241***	-0.0138***	-0.0411***
	(0.0037)	(0.0008)	(0.0071)	(0.0037)	(0.0063)
Total Income	0.0036***	0.0002	0.0262***	0.0061***	0.0271***
	(0.0009)	(0.0002)	(0.0016)	(0.0008)	(0.0019)
Health	0.0032***	0.0008***	0.0010	0.0006	-0.0139***
	(0.0012)	(0.0003)	(0.0020)	(0.0010)	(0.0026)
Fin Job	0.0056	0.0008	0.0702***	0.0196**	0.0126
	(0.0086)	(0.0017)	(0.0164)	(0.0078)	(0.0139)
Owns Property	-0.0218***	-0.0043***	0.0038	-0.0066**	0.0098
	(0.0038)	(0.0009)	(0.0057)	(0.0029)	(0.0065)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	36,780	36,780	36,780	36,780	36,780
R-squared	0.113	0.050	0.134	0.067	0.380

Panel B: Logit Estimates

	(1)	(2)	(3)
Dependent Variable	Online Lending	Digital Wealth Management	Mobile Payment
Only Child	-0.5197***	-0.4380***	-0.1758***
	(0.1007)	(0.0513)	(0.0583)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	36,780	36,780	36,780

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Table 3 – Continued

Panel C: Poisson Pseudo Maximum Likelihood (PPML) Estimates		
	(1)	(2)
Dependent Variable	Online Lending Share	Digital Wealth Management Share
Only Child	-0.5198***	-0.3019***
	(0.1172)	(0.0555)
Controls	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	36,780	36,780

Table 4: Family Structure and FinTech Adoption (RDD Estimates)

This table reports fuzzy RDD estimates of the relation between only-child status and FinTech adoption, using the 1980 OCP birth-year cutoff as the instrument. Panel A reports first-stage estimates, where the dependent variable is the only-child indicator. Panel B reports second-stage estimates. The dependent variables capture three segments: Online Lending (columns 1–2), Digital Wealth Management (columns 3–4), and Mobile Payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household financial assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: First-Stage Regressions

	(1)	(2)	(3)
Dependent Variable	Only Child	Only Child	Only Child
d_i	0.0912***	0.0901***	0.0689***
	(0.0237)	(0.0228)	(0.0177)
Controls	No	No	Yes
Province FE	No	Yes	Yes
Year FE	Yes	Yes	Yes
RDD Polynomial Controls	Yes	Yes	Yes
Kleibergen-Paap F-statistic	14.8	15.6	15.2
Cragg-Donald Wald F-statistic	27.8	28.3	25.6
N	9,813	9,813	9,813
R-squared	0.133	0.171	0.466

Panel B: Second-Stage Regressions

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.5723***	-0.0444	-0.6738**	-0.5050***	-0.8495**
	(0.1938)	(0.0310)	(0.2997)	(0.1681)	(0.3410)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	9,813	9,813	9,813	9,813	9,813

Table 5: Validity and Robustness

This table reports validity tests and robustness checks for the RDD estimates. Panel A tests for covariate balance by estimating the RDD specification with each predetermined covariate as the dependent variable. Panels B and C report second-stage RDD estimates using placebo cutoffs at 1978 and 1982. Panel D reports a donut RDD that excludes individuals born in 1980. Panels E and F report second-stage RDD estimates using alternative bandwidths of four and six years around the 1980 cutoff. The dependent variables in Panels B–F capture three segments: Online Lending (columns 1–2), Digital Wealth Management (columns 3–4), and Mobile Payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household financial assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: Covariate Smoothness	(1)
	RDD Coefficient
Siblings	-1.6290***
	(0.5876)
Male	-0.0203
	(0.3018)
Married	0.1289
	(0.1581)
Education	-0.7973
	(1.1045)
Employed	-0.0581
	(0.1934)
Offspring	0.2757
	(0.1907)
Total Income	-0.3974
	(0.6607)
Health	-0.1749
	(0.4190)
Fin Job	-0.0207
	(0.0998)
Owns Property	-0.0585
	(0.1697)

Panel B: Placebo Cutoff at 1978

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.1765	-0.0742	-0.2061	-0.0691	-0.8149
	(0.4377)	(0.1046)	(0.5451)	(0.2526)	(0.7918)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	10,115	10,115	10,115	10,115	10,115

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Table 5 – Continued

Panel C: Placebo Cutoff at 1982

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.4930	0.0088	0.2829	0.2831	-0.2199
	(0.4076)	(0.0655)	(0.5669)	(0.2897)	(0.6548)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	9,855	9,855	9,855	9,855	9,855

Panel D: Donut RDD

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.8836**	-0.1341**	-0.5489	-0.4518**	-2.0730***
	(0.3630)	(0.0657)	(0.3451)	(0.1902)	(0.7218)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,848	8,848	8,848	8,848	8,848

Panel E: Four-Year Bandwidth

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.8204*	-0.0346	-1.4235*	-1.1240*	-1.1194
	(0.4584)	(0.0553)	(0.8635)	(0.6001)	(0.7568)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,016	8,016	8,016	8,016	8,016

Panel F: Six-Year Bandwidth

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.7028***	-0.0711**	-0.8442***	-0.5827***	-1.0083***
	(0.1937)	(0.0302)	(0.2873)	(0.1646)	(0.3212)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	11,747	11,747	11,747	11,747	11,747

Table 6: FinTech Adoption vs. Traditional Finance Participation

This table tests whether only-child status reduces FinTech adoption specifically or financial market participation generally. Column (1) reports RDD estimates for FinTech (Composite), an indicator equal to one if the individual holds FinTech assets. Column (2) reports estimates for FinTech vs. Traditional, which equals 1 if the individual holds FinTech assets but not traditional finance assets, –1 if the individual holds traditional finance assets but not FinTech assets, and 0 otherwise. Traditional finance assets are defined as holding stocks, mutual funds, bonds, and bank deposits. Column (3) reports estimates for the indicator Traditional Finance Assets (it equals 1 if holding traditional finance assets, and 0 otherwise). All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

	(1)	(2)	(3)
Dependent Variables	FinTech (Composite)	FinTech vs. Traditional Finance	Traditional Finance Assets
Only Child	-1.6516***	-1.7424***	0.0908
	(0.4953)	(0.5719)	(0.2864)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	9,813	9,813	9,813

Table 7: Information Diffusion and Technological Barriers

This table reports tests of alternative explanations for the relation between only-child status and FinTech adoption. Panel A tests whether the relation scales with sibling count by estimating the RDD separately for individuals with one sibling and individuals with two or more siblings. The dependent variables are Online Lending (columns 1–2), Online Lending Share (columns 3–4), Digital Wealth Management (columns 5–6), Digital Wealth Management Share (columns 7–8) and Mobile Payment (columns 9–10). Within each pair of columns, the first reports estimates for the one-sibling subsample and the second reports estimates for the two-or-more-siblings subsample. Panel B tests whether only-child status affects digital access using two dependent variables: Internet Access, an indicator equal to one if the individual has access to the internet, and Communication Expenditure Share, the ratio of household communication expenditures to total household assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: Sibling Count

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	Online Lending		Online Lending Share		Digital Wealth Management		Digital Wealth Management Share		Mobile Payment	
	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings
Only Child	-0.5571***	-0.5317***	-0.0376	-0.0675***	-0.0826	-0.0203	-0.2819**	-0.2659***	-0.7270**	-0.4017*
	(0.2097)	(0.1508)	(0.0290)	(0.0240)	(0.2229)	(0.1706)	(0.1353)	(0.0991)	(0.3263)	(0.2139)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5,883	6,558	5,883	6,558	5,883	6,558	5,883	6,558	5,883	6,558

Panel B: Digital Access

	(1)	(2)
Dependent Variable	Internet Access	Communication Expenditure Share
Only Child	0.2449	0.0012
	(0.3021)	(0.0012)
Controls	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	8,049	9,813

Table 8: Cross-Sectional Heterogeneity

This table reports RDD estimates of the relation between only-child status and FinTech adoption across subsamples defined by education level and provincial investor protection. Panels A and B split the sample at the median education level. Panels C and D split the sample at the median of the NERI legal environment sub-index. The dependent variables capture three segments: Online Lending (columns 1–2), Digital Wealth Management (columns 3–4), and Mobile Payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household financial assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Panel A: Below-Median Education

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.5770**	-0.0506	-1.4704**	-0.6928***	-1.1658**
	(0.2541)	(0.0429)	(0.5799)	(0.2677)	(0.5280)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	7,307	7,307	7,307	7,307	7,307

Panel B: Above-Median Education

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.6559*	-0.0472	0.7910	-0.1740	-0.2618
	(0.3573)	(0.0536)	(0.5813)	(0.2207)	(0.4066)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	2,506	2,506	2,506	2,506	2,506

Panel C: Below-Median Investor Protection

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.4586**	-0.0487	-0.7033**	-0.5449***	-0.9353**
	(0.1910)	(0.0363)	(0.3121)	(0.1847)	(0.3879)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	5,035	5,035	5,035	5,035	5,035

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Table 8 – Continued

Panel D: Above-Median Investor Protection					
	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.8635	-0.0380	-0.7280	-0.5106	-0.7875
	(0.5730)	(0.0610)	(0.7323)	(0.3962)	(0.7476)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	4,778	4,778	4,778	4,778	4,778

Table 9: Mediation Analysis

This table reports second-stage RDD estimates of the relation between only-child status and FinTech adoption after augmenting the specification with three investor trait variables: Risk Aversion, Trust, and Socialization. The dependent variables capture three segments: Online Lending (columns 1–2), Digital Wealth Management (columns 3–4), and Mobile Payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household financial assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff with non-missing values on all three trait variables. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.2500***	-0.0109	-0.6040***	-0.3017***	-0.4437**
	(0.0782)	(0.0139)	(0.1969)	(0.0878)	(0.2010)
Risk Aversion	-0.0031	-0.0006	-0.0208***	-0.0020	-0.0055
	(0.0023)	(0.0004)	(0.0054)	(0.0024)	(0.0058)
Trust	0.0096**	0.0016**	0.0036	-0.0005	0.0124
	(0.0042)	(0.0007)	(0.0076)	(0.0035)	(0.0082)
Socialization	0.2151	0.0935*	0.5149	0.1904	0.7181*
	(0.2285)	(0.0520)	(0.4292)	(0.2115)	(0.4334)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	6,739	6,739	6,739	6,739	6,739

Table 10: The Trust Channel: Evidence from FinTech Scandals

This table tests whether only children respond more negatively to FinTech scandals. The specification augments the second-stage RDD with FinTech Scandal, defined as the ratio of negative FinTech news to total FinTech news in the province, and its interaction with the only-child indicator. The dependent variables capture three segments: Online Lending (columns 1–2), Digital Wealth Management (columns 3–4), and Mobile Payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household financial assets. All specifications include a linear polynomial in the running variable estimated separately on each side of the 1980 cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the province level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Lending	Online Lending Share	Digital Wealth Management	Digital Wealth Management Share	Mobile Payment
Only Child	-0.3963**	-0.0541	-0.5608**	-0.4277***	-0.7870***
	(0.1562)	(0.0328)	(0.2686)	(0.0933)	(0.2241)
FinTech Scandal	0.4376**	-0.0420	0.2621	0.1926**	0.0403
	(0.1638)	(0.0271)	(0.2102)	(0.0875)	(0.1227)
Only Child ×	-1.1496***	0.0569	-0.7501*	-0.5126***	-0.5242*
FinTech Scandal	(0.3819)	(0.0673)	(0.4361)	(0.1315)	(0.2879)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	9,764	9,764	9,764	9,764	9,764

Table 11: Market-Level Evidence

This table reports province-by-survey-year OLS regressions of FinTech market development on the provincial share of only children. The dependent variables capture FinTech adoption and entrepreneurial activity at the province level: the Peking University Digital Financial Inclusion Index *FinTech Aggregate Index* (column 1), *Coverage Breadth* (column 2), *Usage Depth* (column 3), the log number of *Incumbent FinTech Firms* (column 4), and the number of *New FinTech Entrants* (column 5). The key independent variable is the provincial proportion of only children. All regressions include province-level controls: *GDP* (log), *Industrial Structure* (tertiary industry output divided by secondary industry output), *Trade Openness* (total goods imports and exports multiplied by exchange rate divided by GDP), *Patents* (log invention patent applications), *Industrialization* (total industrial value divided by GDP), *Financial Development* (total deposits and loans of banking financial institutions divided by GDP) and *Telecom Service* (total telecom services revenue divided by GDP). The data are from the China Household Finance Survey and China Stock Market and Accounting Research, including six waves of surveys: 2011, 2013, 2015, 2017, 2019, and 2021. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Appendix A.

Dependent Variable	(1) FinTech Aggregate Index	(2) Coverage Breadth	(3) Usage Depth	(4) Incumbent FinTech firms	(5) New FinTech Entrants
Only Child Share	-28.6048**	-21.6825*	-47.0291**	-1.2013**	-1.4528
	(13.5896)	(12.6137)	(22.4789)	(0.5198)	(1.0812)
GDP (log)	12.7239	18.2197***	0.0885	0.8602*	0.2371
	(7.9234)	(6.7815)	(12.7228)	(0.4444)	(0.8522)
Industrial Structure	-4.4305	-2.5933	-3.8318	0.2686	0.1886
	(3.8607)	(3.6122)	(6.4673)	(0.2334)	(0.4993)
Trade Openness	-30.2080***	-5.9566	-63.1195***	1.2479***	2.5125***
	(8.2410)	(5.7484)	(17.2230)	(0.3565)	(0.6935)
Patents	1.9324	2.3055	4.1833	0.1730*	0.2310
	(2.0673)	(1.6484)	(3.6747)	(0.0997)	(0.2351)
Industrialization	-30.7113	-33.1110	-38.0851	1.1201	0.1562
	(28.4630)	(24.6249)	(48.7123)	(1.0312)	(2.1192)
Financial Development	-1.8285	-1.3579	-1.6732	0.1946	0.1426
	(2.0939)	(1.5216)	(4.1314)	(0.1450)	(0.2291)
Telecom Service	-69.1151**	53.3862**	-129.0769**	0.7353	2.5197
	(33.8581)	(24.5802)	(64.8160)	(1.5460)	(3.9577)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	147	147	147	147	147
R-squared	0.997	0.998	0.991	0.979	0.902