The Financial Psychology of Shrinking Families: How Demographic Decline Shapes Investor Behavior*

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Abstract

Fertility rates are declining worldwide, producing a growing share of only children. We exploit China's One-Child Policy in a regression discontinuity design to study how family structure shapes investor attitudes and their consequences for financial innovation adoption. Using nationally representative data covering over 30,000 household-year observations, we first establish that only children are significantly more risk-averse, less trusting, less socially engaged, and less competitive than individuals with siblings, extending a landmark psychology finding to the field. We then show that these attitude differences reduce participation in FinTech (internet lending, digital wealth management, and online payments) but not traditional finance, with economically large effects that survive controls for wealth, income, and internet access. The effect is concentrated among less educated individuals and in regions with weaker investor protection. Only children also react more negatively to FinTech platform scandals, confirming trust as the dominant channel. As the financial system becomes increasingly digital, demographic decline may impose a psychological constraint on financial modernization.

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I Introduction

Whether individual investors adopt digital financial products is a first-order determinant of how financial systems modernize. Adoption depends on preferences that the household finance literature has shown to be shaped early in life: risk tolerance, generalized trust, and willingness to engage with unfamiliar institutions. Yet a demographic transformation—what demographers call a "demographic winter"—is quietly altering the very environment in which these preferences form. Global fertility rates have fallen sharply, and by 2050 over three-quarters of the world's population will live in countries below replacement fertility.[1] Each successive cohort contains a rising share of only children who grew up without the sibling interactions that developmental psychology identifies as formative for these traits. Despite extensive research on the determinants of financial market participation and on the psychological consequences of family structure, no study has connected the two. This paper asks whether growing up without siblings alters investor attitudes and, if so, whether these attitudes affect the adoption of financial technology.

China offers the sharpest setting in which to study this question. 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 less trusting, less trustworthy, more risk-averse, less competitive, more pessimistic, and less conscientious than those who grew up with siblings. Their lab experiments with 421 Beijing residents, born just before and just after the OCP, established a striking psychological fact, but left open the question that matters most for financial economics: do these traits shape how people actually save, borrow, and invest? China's parallel emergence as the world's largest financial technology (FinTech) market makes it possible to answer this question directly, testing whether the attitudes forged in sibling-less households affect the adoption of the digital financial products that are now redefining household finance worldwide.

[1] In the United States, 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 (Pew Research Center, 2023), and the average number of children that adults aged 20–39 plan to have has fallen to 1.8, below replacement for the first time. Across the European Union, about half (49.4%) of families with children have a single child, with the share exceeding 50% in Germany, Italy, Spain, and Portugal (European Large Families Confederation, 2022).

To test this, we need data that capture both investor attitudes and FinTech behavior for the same individuals—a combination rarely available in household surveys. The China Household Finance Survey (CHFS) provides exactly this: a nationally representative biennial survey of over 30,000 household-year observations across 24 provinces that pairs granular FinTech participation measures with survey-elicited risk aversion, generalized trust, social network engagement, and competitiveness. 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 grew up with siblings under the prior regime. We exploit this sharp, policy-driven variation in a regression discontinuity design (RDD) that separates 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 attitudes. Using the RDD, we find that only children exhibit significantly higher risk aversion, lower generalized trust, weaker social network engagement, and lower competitiveness than individuals with siblings. The magnitudes are economically meaningful: only-child status increases risk aversion by approximately 11% of its sample mean and reduces trust by 3.5%. These findings extend Cameron et al.'s (2013) laboratory results to nationally representative field data and are consistent with established developmental theories in psychology linking parental overprotection to risk aversion and sibling interaction to trust formation.

The second set of results turns to FinTech adoption, where these attitude 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. RDD estimates indicate that only-child status reduces digital wealth management participation by 31 percentage points and online payment adoption by 6.8 percentage points—magnitudes large enough to reshape aggregate FinTech penetration as only-child cohorts age into their prime financial years. The family

socialization channel, not the network substitution channel, dominates: the effect survives controls for wealth, income, education, and internet access, confirming that the barrier is dispositional rather than resource-based. Heterogeneity analysis sharpens the interpretation. The effect is concentrated among less educated individuals and in regions with weak investor protection, and it vanishes among the university-educated and in well-protected regions—consistent with education and strong institutions providing the analytical tools and safeguards that compensate for the trust deficit only children carry from childhood.

To pin down the mechanism, we exploit a wave of FinTech platform scandals that followed regulatory gaps 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. The interaction between only-child status and provincial scandal exposure implies that only children reduce digital wealth management participation roughly three times 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 does not scale with sibling count, ruling out resource dilution; it is specific to FinTech and absent for traditional financial products, ruling out general financial disengagement; and it persists despite comparable digital access, ruling out technological barriers. 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 two 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 attitudes to their children (e.g., Barnea, Cronqvist and Siegel (2010) and Black, Devereux, Lundborg and Majlesi (2017)). Additionally, Ke (2021, 2025) show that horizontal dynamics between spouses shape portfolio decisions within the household. We add a novel dimension: horizontal socialization among siblings during childhood. Our results reveal that the presence or absence of this early peer interaction produces persistent differences in investor attitudes that 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 attitudes 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 digital, algorithmic, and platform-based, the interaction between demographic structure and financial innovation adoption becomes a first-order question. Understanding the financial psychology of shrinking families is essential for anticipating how the demographic transformation now underway will shape the evolution of financial markets.

II Related Literature and Hypothesis Development

Our paper connects two strands of research. The first examines how family structure during childhood shapes investor attitudes, drawing on developmental psychology and the economics of intergenerational transmission. The second studies the determinants of FinTech adoption, with an emphasis on the role of risk attitudes and trust.

A Family Structure and Investor Attitudes

Cameron, Erkal, Gangadharan and Meng (2013) exploited China's OCP to identify the behavioral consequences of growing up without siblings. Using lab experiments, they documented that only children are significantly less trusting, less trustworthy, more risk-averse, less competitive, more pessimistic, and less conscientious. Their findings build on two foundational ideas in developmental psychology: Bowlby's (1969) *attachment theory*, whereby concentrated parental investment fosters security but also overprotection that cultivates caution over exploration, and Piaget's (1932) *theory of moral judgment*, whereby children develop trust and cooperation through peer interactions among equals, with siblings providing the primary arena for this learning.[3]

These trait differences have direct implications for financial economics. In the household finance

[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.

[3]See, e.g., Falbo and Polit (1986) for a meta-analysis of existing evidence on personality differences between only children and those with siblings.

literature, both risk aversion and trust are first-order determinants of financial market participation (Guiso, Sapienza and Zingales, 2008). A growing body of work has begun to trace these attitudes to their origins within the family. Models of cultural transmission predict that parents shape children's risk preferences and trust through socialization (Bisin and Verdier, 2001), and Dohmen, Falk, Huffman and Sunde (2012) provide empirical confirmation using parent-child pairs. Several studies reinforce the importance of the family environment: Barnea, Cronqvist and Siegel (2010), Cesarini, Johannesson, Lichtenstein, Sandewall and Wallace (2010), and Black, Devereux, Lundborg and Majlesi (2017) find that the shared family environment, i.e., "nurture," contributes meaningfully to investor behavior. This body of work documents what we term *vertical* socialization, the transmission of attitudes from parents to children. More recently, Ke (2021, 2025) shows that *horizontal* dynamics within the adult household also shape portfolio decisions.[4] Yet no study has examined horizontal socialization during childhood, specifically how the presence or absence of siblings shapes the investor attitudes that adults carry into financial decisions.

> **Hypothesis 1 (Investor Attitudes):** *Only children exhibit higher risk aversion, lower generalized trust, and lower willingness to compete than individuals with siblings.*

B Family Structure and FinTech Adoption

FinTech platforms make unusually high demands on the psychological traits that distinguish only children from those with siblings: trust in opaque systems, willingness to experiment with unfamiliar products, and comfort with technological novelty. Family structure may therefore shape FinTech adoption through two competing channels.

B.1 The Family Network Substitution Hypothesis

In environments with incomplete financial markets, family networks partially resolve contracting frictions that limit access to formal credit, insurance, and savings, and Rosenzweig (1988) shows that the family functions as an implicit contract in precisely such settings. Siblings can provide two economically distinct services analogous to those documented at the village level: risk sharing across

[4]See also Love (2010) on marital status and portfolio choice, Bertrand, Kamenica and Pan (2015) on gender identity norms and relative income, and Knüpfer, Rantapuska and Sarvimäki (2023) on intergenerational transmission of security holdings.

members with imperfectly correlated income streams (Townsend, 1994) and informal credit in which loan repayment adjusts to borrower shocks, exploiting the information advantages of repeated, high-trust relationships (Udry, 1994). Evidence from China confirms that these mechanisms operate within the sibling network specifically. Zhou (2014) finds that an additional brother reduces savings rates through risk sharing, informal borrowing, and shared parental support costs, Niu, Wang, Li and Zhou (2020) show that having more brothers increases stock market participation through informal risk sharing, and Li and Wu (2018) document that siblings alleviate credit constraints by substituting for formal intermediation.

As fertility declines and sibling networks thin, these informal services erode and the marginal benefit of formal alternatives rises (Kranton, 1996). FinTech platforms offer precisely such alternatives: scalable digital substitutes for the credit, investment, and transfer services that siblings informally provide (Jack and Suri, 2014). More broadly, Yang (2025) shows that when trust in incumbent banks erodes, borrowers redirect demand toward FinTech lenders, demonstrating that households are willing to substitute across financial channels when existing relationships weaken. If this economic channel dominates the psychological barriers that only children carry into adulthood, demographic decline becomes a catalyst for FinTech adoption.

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

B.2 The Family Socialization Hypothesis

FinTech platforms differ fundamentally from traditional financial institutions in requiring users to trust algorithms and brands with limited track records rather than physical branches, long-standing reputations, and personal relationships. Trust shocks from fraud durably reduce financial market participation and shift households away from intermediaries that lack personal relationships (Giannetti and Wang, 2016; Gurun, Stoffman and Yonker, 2018). Because FinTech intermediaries lack the face-to-face relationships that traditional financial advisors use to build client trust (Gennaioli, Shleifer and Vishny, 2015), the opacity and novelty of digital platforms amplify these trust demands. D'Acunto, Ghosh and Rossi (2026) provide direct evidence that deep-rooted cultural attitudes can

suppress FinTech adoption, even when access to technology is unrestricted. The attitudes shaped by growing up without siblings should therefore disproportionately suppress FinTech adoption.[5]

Adoption requires not only trust but also willingness to experiment with unfamiliar products whose risk-return profiles are difficult to evaluate ex ante. Hong, Lu and Pan (2025) show that repeated use of FinTech payment technology builds familiarity and trust, progressively lowering the psychological barriers to platform-based investment. This virtuous cycle, however, presupposes a willingness to begin: individuals must clear an initial adoption threshold before habit formation can take hold. For only children, whose socialization was exclusively vertical, with intensive parental investment that cultivates caution, but lacked the horizontal sibling interactions through which trust and social confidence develop among equals, this threshold is higher at the outset.[6]

This prediction is distinct from a standard resource-based explanation: it holds even when only children possess comparable wealth, income, and internet access, because the barrier operates through preferences, not budget constraints.

> **Hypothesis 2b (Family Socialization):** *Only children are less likely to participate in FinTech than individuals with siblings, because the absence of sibling interactions during preference formation produces heightened risk aversion, lower generalized trust, and a higher adoption threshold for novel and opaque financial platforms.*

III Institutional Setting and Identification Strategy

We exploit China's OCP as a source of exogenous variation in family structure (Zhang, 2017). In response to rapid population growth, from 540 million in 1949 to 830 million in 1970, China launched a voluntary family planning campaign in 1971 under the slogan "Later, Longer, Fewer," which permitted couples to have two children. In October 1978, the government replaced this two-child norm 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

[5] In a parallel corporate finance setting, 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, consistent with the same psychological channel operating at the firm level.

[6] The psychology literature documents that siblings provide a setting for repeated experimentation under low-stakes conditions, fostering risk tolerance and social confidence (Wu and Zhao, 2020). Without this peer arena, concentrated parental attention and overprotective behavior reinforce caution (Parker, 1983).

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 total fertility rate dropped to 1.1 by the end of that year, implying near-complete compliance in cities (Tang, 2007). Crucially for identification, only-child status for the post-1980 urban cohort is overwhelmingly policy-imposed rather than reflecting endogenous parental choice, a limitation that affects studies relying on cross-sectional variation in family size in settings without binding fertility constraints.

The policy's implementation features support a regression discontinuity design 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.[7] Enforcement was strictest among urban residents and government employees, generating the sharpest variation in these populations, while known exceptions, including 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. One potential concern is that the OCP affected not only sibling count but also the concentration of parental resources per child (Rosenzweig and Zhang, 2009). We address this by examining whether results survive controls for wealth, income, education, and internet access, which would absorb any effect operating through resource concentration rather than preference

[7] citetZhang2017 identifies several empirical challenges in OCP studies: concurrent economic reforms may confound cohort differences, cross-regional enforcement variation is endogenous to local preferences, and China's fertility decline had begun prior to the OCP. Our design addresses all three. We exploit temporal variation at a single birth-year cutoff within uniformly enforced urban areas, not cross-regional enforcement differences. Economic reforms affected all cohorts continuously and cannot produce the discrete jump in family structure our RDD identifies. And while fertility had been declining since the early 1970s, the 1980 drop was the largest single-year decline in Chinese history outside the 1959 famine, confirming a sharp discontinuity at the cutoff.

formation.

China's FinTech market provides an ideal setting for studying the financial consequences of these attitude differences. Beginning in 2013, the market expanded rapidly across three segments that correspond to our outcome variables: mobile payments, online lending, and digital wealth management. By the start of our sample period, mobile payment platforms had achieved widespread penetration among urban smartphone users, P2P lending platforms numbered in the thousands, and digital wealth management products had attracted hundreds of millions of retail investors. The post-1980 OCP cohort, aged roughly 28 to 39 during this period, entered prime financial decision-making years precisely as these platforms scaled, generating meaningful within-cohort variation in adoption. Equally important for testing the trust channel implied by the family socialization hypothesis, the FinTech sector experienced a concentrated wave of platform scandals after 2015 due to regulatory gaps and business model failures, producing an adverse-event shock to platform credibility that allows us to test whether only children exhibit differentially negative responses.

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, and 2019 waves, which are the first to include detailed FinTech participation and attitudinal modules. 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 in each wave. It employs a stratified, three-stage probability-proportional-to-size sampling design with face-to-face interviews conducted by trained professionals who passed a certification exam. Sampled households were excluded only after three documented refusals, and the survey achieves a refusal rate of approximately 11%, comparable to or lower than leading international household surveys such as the U.S. Survey of Consumer Finances (SCF) and the Survey of Health, Ageing and Retirement in Europe (SHARE).

We restrict the sample to household heads aged 16 to 60 (males) or 16 to 55 (females), cor-

responding to China's statutory retirement ages. Consistent with our identification strategy, we exclude five provinces where the One-Child Policy was not widely enforced (Yunnan, Qinghai, Ningxia, Hainan, and Xinjiang), as well as rural areas where enforcement was lax, leaving 24 provinces in the sample. After dropping observations with missing key variables, we obtain an unbalanced panel of 21,681 households and 31,868 household-year observations. The panel is unbalanced because the CHFS is a tracking survey that adds refreshment households in each wave to maintain representativeness, while some original households attrite between waves.

B Variable Construction

We organize the variables into four groups: treatment status, investor attitudes, FinTech participation, and controls.

Only-child status. Our treatment variable, *Only Child*, equals one if the respondent reports having no siblings and zero otherwise. China relaxed its two-child policy in 2016, but over 90% of our sample is aged 25 or above, with parents predominantly over 50, making new siblings after the policy change negligibly unlikely. We also construct *Siblings*, a count variable measuring the number of siblings, to examine intensive-margin effects.

Investor attitudes. We measure four 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 household total assets, capturing social network engagement.[8] *Competitiveness* is the log of the difference between the respondent's stated minimum acceptable monthly salary and the average minimum wage in their province (observations where the difference is non-positive are excluded), capturing willingness to compete for returns above the regional floor.

FinTech participation. We measure participation along three dimensions corresponding to

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

the primary FinTech applications in China: lending, wealth management, and payments. We construct an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100 for readability). *Online Credit* equals one if the respondent borrows from or lends through an online platform; *Online Credit Share* is the ratio of annual online lending amounts to total household assets.[9] *Online Financial Products* is an indicator for holding digital wealth management products, with *Online Financial Products Share* as the corresponding asset ratio. *Online 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: number of siblings (*Siblings*), gender (*Male*), marital status (*Married*), log age (*Age*), education level (*Education*, coded 1 to 9),[10] employment status (*Employed*), whether the respondent has children (*Offspring*), log household income (*Total Income*), self-reported health relative to peers (*Health*, coded 1 to 5), financial industry employment (*Fin Job*), and homeownership (*Owns Property*).

Supplementary data. Several analyses require province-level measures of institutional quality, FinTech development, and scandal exposure. For institutional quality, we use the legal environment sub-index from the NERI provincial marketization index, which captures the density of professional services, commercial dispute resolution, and contract enforcement.[11] For FinTech development, we use two complementary measures: the cumulative number of registered FinTech companies by province, compiled from Tianyancha business registration data, and the Peking University Digital Financial Inclusion Index, which captures the coverage and usage depth of digital financial services at the provincial level. For scandal exposure, we obtain FinTech-related news coverage from the China Stock Market and Accounting Research (CSMAR) database.

[9] 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.

[10] 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.

[11] This sub-index is one of five components of the NERI marketization index, the most widely used measure of provincial institutional development in China.

C Descriptive Statistics

Table 1 reports summary statistics for our sample. Panel A describes individual characteristics. Only children account for 17% of observations, consistent with the population structure under the OCP given that individuals born after 1980 represent approximately 30% of the sample. The average respondent has 2.3 siblings and is approximately 40 years old. The sample is gender-balanced (48.8% male) and predominantly married (82.9%). Average education falls between high school and vocational school, and three-quarters of respondents are employed. Average log household income is 11.03, corresponding to approximately RMB 61,000. Nearly 88% of respondents own property, consistent with an established urban sample, while 2.4% work in the financial industry.

Panel B reports the investor attitude variables. Average *Risk Aversion* is 3.78 on a five-point scale, indicating that the typical respondent is quite cautious. Average *Trust* is 2.27, 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 highly skewed nature of informal social transfers. *Competitiveness* averages 8.18 in log terms. The attitude variables have somewhat lower coverage due to item non-response on the relevant survey questions; we examine whether this non-response is systematic in the robustness analysis.

Panel C describes FinTech adoption. The most notable feature is the wide dispersion across product categories: *Online Payment* adoption reaches 51.3%, *Online Financial Products* stands at 16.2%, while *Online Credit* stands at 1.2%. The continuous measures are correspondingly small, with *Online Credit Share* averaging 0.003% and *Online Financial Products Share* averaging 0.36% of total household assets. 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 assets. Portfolios are dominated by *Cash*, which averages 6.1% of total assets. *Stocks* average 0.4%, *Mutual Funds* 0.1%, and *Bonds* are negligible. Other risky instruments (derivatives, foreign exchange, gold) are similarly close to zero. 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 attitudes and FinTech adoption using two approaches. We begin with ordinary least squares (OLS):

$$Y_{i,t} = \beta_0 + \beta_1 \, \text{OnlyChild}_{i,t} + \boldsymbol{\gamma}' \mathbf{X}_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ is the outcome of interest for individual i in year t, $\text{OnlyChild}_{i,t}$ is the only-child indicator, $\mathbf{X}_{i,t}$ is the vector of controls, and μ_p and δ_t are province and year fixed effects. Standard errors are clustered at the individual level to account for serial correlation across survey waves.[12]

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 egression discontinuity design (RDD) exploiting the discontinuous increase in only-child probability at the 1980 birth-year cutoff (Hahn, Todd and Van der Klaauw, 2001).[13] 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. A McCrary (2008) density test confirms no evidence of manipulation at the cutoff, consistent with the fact that parents cannot precisely control their child's birth year.

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

$$\text{OnlyChild}_{i,t} = \pi_0 + \pi_1 d_i + f(\text{Birth} - c)_{i,t} + \boldsymbol{\gamma}' \mathbf{X}_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{2}$$

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

$$Y_{i,t} = \beta_0 + \beta_1 \widehat{\text{OnlyChild}}_{i,t} + f(\text{Birth} - c)_{i,t} + \boldsymbol{\gamma}' \mathbf{X}_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \tag{3}$$

[12] Results are robust to clustering at the province level to address spatial correlation, and at the birth-year level, following the recommendation of Lee and Card (2008) for RDD with a discrete running variable.

[13] 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 rural exemptions, ethnic minority exceptions, or non-compliance, while some born before 1980 are only children by parental choice.

where d_i equals one if individual i was born after the 1980 cutoff c, and $f(\text{Birth} - c)_{i,t}$ is a linear polynomial in the running variable estimated separately on each side of the threshold. $\mathbf{X}_{i,t}$ includes the same controls as equation (1) except log age, which is collinear with the running variable in the RDD specification.

The coefficient β_1 in equation (3) captures the local average treatment effect (LATE) of only-child status. We restrict the sample to individuals born within five years of the 1980 cutoff, a standard bandwidth choice that balances precision against bias from observations far from the threshold (Lee and Lemieux, 2010). Instrument relevance is assessed using the Kleibergen and Paap (2006) F-statistic, evaluated against the Stock and Yogo (2005) critical values for weak instruments.

B Family Structure and Investor Attitudes

Table 2 reports second-stage RDD estimates of the effect of only-child status on four investor attitude variables. Only-child status significantly increases *Risk Aversion* (column (1)) and significantly reduces *Trust* (column (2)), *Socialization* (column (3)), and *Competitiveness* (column (4)), with all four coefficients significant at the 10% level or better. These results confirm, within an RDD framework and using nationally representative data, that the psychological trait differences identified by Cameron, Erkal, Gangadharan and Meng (2013) in a Beijing lab sample extend to the broader population. Having established that only-child status shapes investor attitudes, we next examine whether these differences translate into financial technology adoption decisions.

C Family Structure and FinTech Adoption

C.1 OLS Estimates

Table 3 reports OLS estimates of the effect of only-child status on FinTech adoption. Across all five specifications, only-child status is associated with significantly lower adoption. For online credit, the coefficient implies a 1.3 percentage point reduction in the probability of participation (column (1)), and *Online Credit Share* declines by 0.065 standard deviations (column (2)). For online financial products, only children are 5.4 percentage points less likely to hold digital wealth management products (column (3)), with *Online Financial Products Share* lower by 0.086 standard

deviations (column (4)). *Online Payment* adoption is 4.5 percentage points lower (column (5)). These patterns are consistent with the family socialization hypothesis but may reflect unobserved parental or regional characteristics correlated with only-child status.

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, building the specification progressively: column (1) includes year fixed effects only, column (2) adds province fixed effects, and column (3) adds the full set of controls. The coefficient on d_i remains positive and significant across all three specifications. The estimate declines from 0.099 without controls (column (1)) to 0.071 in the fully saturated specification (column (3)), a 28% reduction, indicating that part of the raw discontinuity in only-child status reflects observable differences in demographic composition across cohorts that the controls absorb. The fully saturated estimate indicates that the policy increased the probability of being an only child by approximately 7 percentage points. Relative to the baseline only-child probability of 23% in the five-year bandwidth sample, this 7 percentage point increase represents approximately a one-third shift in the probability of being an only child.

Panel B presents the second-stage estimates. The coefficients capture the local average treatment effect for compliers, individuals whose only-child status was determined by the policy. Only-child status significantly reduces adoption across all five specifications, confirming the OLS patterns. To gauge economic magnitudes, we scale the LATE estimates by the first-stage coefficient of 0.071 to obtain implied reduced-form effects. Being born just after the 1980 cutoff reduces *Online Credit Share* by 0.8 percentage points (column (2)), *Online Financial Products Share* by 31 percentage points (column (4)), and *Online Payment* adoption by 6.8 percentage points (column (5)). The RDD estimates are uniformly larger in magnitude than OLS across all FinTech categories, reinforcing the positive selection interpretation: parents who voluntarily chose one child before the policy tend to be wealthier and more educated, characteristics positively associated with FinTech adoption, attenuating OLS estimates toward zero. The larger RDD magnitudes are consistent with the LATE isolating compliers whose only-child status was policy-imposed, for whom the family socialization channel operates most directly.

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.[14] Online Credit (Panel A), Online Financial Products (Panel B), and Online 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. The discontinuity is sharp, with the fitted value dropping by approximately 13 percentage points at the cutoff.

C.3 Validity and Robustness

We evaluate the validity of the RDD and the robustness of the baseline estimates through a series of tests. Figure 2 presents the McCrary (2008) density test for manipulation of the running variable. The test statistic is 0.918 ($p = 0.359$), indicating no evidence of sorting around the cutoff, consistent with the fact that parents could not precisely control their child's birth timing relative to the policy.

Panel A of Table 5 tests whether predetermined covariates vary smoothly through the 1980 cutoff. If the RDD is valid, individuals born just before and just after the threshold should be comparable on characteristics determined before the policy. No predetermined covariate exhibits a significant discontinuity, supporting the identifying assumption. The one exception is siblings, which jumps sharply at the cutoff, confirming that the policy affected family structure as intended. Panels B and C report results using alternative bandwidths of four and six years. The point estimates are stable or larger in magnitude across both alternatives, with the four-year bandwidth producing the largest coefficients, consistent with a sharper treatment contrast closer to the cutoff.

Table 6 reports placebo tests using artificial cutoffs at 1978 and 1982, two years before and after the true policy cutoff. If the baseline results reflect the OCP rather than a pre-existing trend, the placebo estimates should be insignificant. None of the ten placebo coefficients is statistically significant, and several reverse sign, indicating that the effects are specific to the 1980 cutoff. In unreported tests, results are also robust to using a quadratic polynomial in the running variable.

Beyond these formal tests, three aspects of our empirical design merit discussion. First, the RDD identifies a local average treatment effect for compliers near the 1980 cutoff. The OLS esti-

[14] 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.

mates, which use the full sample without relying on the discontinuity, show the same qualitative pattern with smaller magnitudes, suggesting that the relationship between only-child status and FinTech adoption extends beyond the complier population. Second, investor attitudes are measured by self-reported survey items. The CHFS trust and risk-aversion questions follow established protocols from the World Values Survey and standard investment-preference elicitations, and classical measurement error would attenuate our estimates, biasing against finding the effects we document. Third, we observe the financial decisions of household heads but not the bargaining process within households. To the extent that only-child household heads are married to spouses with siblings, intrahousehold negotiation may dilute the attitudes we measure, again working against our findings.

D Addressing Alternative Explanations

The preceding analysis establishes that only-child status reduces FinTech adoption under both OLS and the RDD. Three alternative explanations could generate similar patterns without implicating the psychological channels. We address each in turn.

Sibling resources. If resource advantages from having siblings drive the result, the only-child effect should scale monotonically with sibling count. We split the sibling sample into those with exactly one sibling and those with two or more, and estimate the RDD separately for each group. Table 7 Panel A reports the results. All ten coefficients are negative, and eight of ten are statistically significant. For online credit, the coefficients are nearly identical across the two comparisons. For online payment, the coefficient is substantially larger in the one-sibling group than in the two-or-more group, the opposite of what the resource channel predicts. The pattern suggests that the presence of any sibling during childhood, rather than the number of siblings, drives the effect.

Digital access. Only children may face higher costs of accessing digital platforms. We test this using two proxies: an indicator for internet access and the ratio of household communication expenditures to total household assets. Table 7 Panel B reports the results. Neither coefficient is statistically significant, and the point estimate on internet access is positive. Only children have comparable or superior digital access, confirming that the FinTech adoption gap reflects a dispositional barrier rather than a technological one.

FinTech-specific avoidance. Only children may simply participate less in all financial markets, not FinTech specifically. Table 8 tests this by reporting RDD estimates for both a composite FinTech indicator (column (1)) and a relative preference measure comparing FinTech and traditional finance participation.[15] The composite FinTech indicator confirms the baseline, with a coefficient of -1.029 ($p < 0.05$, column (1)). The coefficient on the relative preference measure is larger in absolute value (-1.211, $p < 0.05$, column (2)), indicating that the effect is concentrated in the shift away from FinTech relative to traditional finance rather than reflecting general financial disengagement.

These tests narrow the set of viable explanations. The only-child effect is not proportional to sibling-based resource differences, is not explained by differential digital access, and is not a symptom of general financial disengagement but is specific to FinTech.

E Cross-Sectional Heterogeneity

If the adoption barrier is dispositional, operating through preferences rather than 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. Table 9 Panels A and B report the results. Among individuals with below-median education, all five only-child coefficients are negative and significant, with magnitudes comparable to or larger than the baseline estimates. Among individuals with above-median education, none of the five coefficients is statistically significant. Education does not change only-child status, but it appears to offset the psychological barriers that only-child status creates. 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. Table 9 Panels C and D report the results. In provinces with below-median investor protection, all five only-child coefficients are negative and significant at the 5% level. In provinces with above-median investor protection, four of five coefficients are statistically insignificant. Stronger

[15] The relative preference measure 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 or having bank loans.

legal institutions appear to compensate for the trust deficit that only children carry into financial decisions.

The two dimensions reinforce each other. The only-child effect is not uniform but conditional, binding most tightly where neither human capital nor institutional quality provides a counterweight. This pattern is difficult to reconcile with resource-based or access-based explanations, which predict no interaction with education or investor protection, and is consistent with the dispositional channel identified by the family socialization hypothesis.

VI Economic Mechanisms

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

A Mediation Analysis

The investor attitudes documented in Table 2 are the proposed channels through which family structure shapes FinTech adoption. Controlling for these traits should therefore attenuate the only-child coefficient. We test this by augmenting the second-stage RDD specification with all four attitude variables: *Risk Aversion*, *Trust*, *Socialization*, and *Competitiveness*. A key advantage of our setting is that the attitudes 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 10 reports the results. The sample is smaller than the baseline because the specification requires non-missing values on all four investor attitude variables. The attenuation is striking: across all five FinTech outcomes, the only-child coefficient loses statistical significance, with complete attenuation across every specification. The magnitude declines are large, ranging from 48% to 79% across the five specifications. For example, the coefficient on *Online Credit* falls from -0.223 to -0.052, a 77% reduction that cannot be attributed to the smaller sample alone. Because the attitude 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, consistent with preferences rather than network-based resource differences driving the adoption gap.

The investor attitude variables enter with the expected signs, and the pattern has economic structure. For example, *Trust* is the most broadly significant attitude, predicting adoption across three of five specifications, including online credit and online payment, the segments where counterparty risk is most salient and users must rely on unfamiliar platforms to handle their money. *Risk Aversion* significantly reduces wealth management participation, where financial exposure is greatest and the stakes of experimentation are highest. That each attitude predicts the FinTech segments it theoretically should strengthens the interpretation that the mediation reflects genuine economic channels rather than a generic correlation.

Taken together, these results support the family socialization hypothesis: only-child status shapes FinTech adoption through the psychological traits that sibling interactions develop during childhood, not through the loss of informal financial networks that siblings provide in adulthood.

B The Trust Channel: Evidence from FinTech Scandals

The mediation analysis identifies trust as the most broadly significant investor attitude. This provides an opportunity for a sharper test: if trust is the operative mechanism, only children should respond more negatively than individuals 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 P2P 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. Let $S_{p,t}$ denote this measure. We estimate an augmented RDD that interacts instrumented only-child status

with scandal exposure:[16]

$$Y_{i,t} = \beta_0 + \beta_1 \widehat{\text{OnlyChild}}_{i,t} + \beta_2 S_{p,t} + \beta_3 \widehat{\text{OnlyChild}}_{i,t} \times S_{p,t} + f(\text{Birth} - c)_{i,t} + \boldsymbol{\gamma}' \mathbf{X}_{i,t} + \mu_p + \delta_t + \varepsilon_{i,t} \quad (4)$$

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 the interaction β_3 captures the dynamic response: how much more only children revise their participation when adverse information about platform credibility arrives.

Table 11 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 in all five specifications and statistically significant for online credit intensity (-0.221, $p < 0.10$, column (2)), wealth management participation (-2.763, $p < 0.01$, column (3)), and wealth management intensity (-11.437, $p < 0.01$, column (4)). The interaction compounds the baseline gap when platform credibility is challenged. That the effect concentrates in lending and wealth management, the product categories where financial exposure is greatest and trust in platform integrity most consequential, reinforces the interpretation. The interaction is not significant for online payment, consistent with the low-stakes, high-frequency nature of payment transactions, which are less sensitive to trust shocks.

The decomposition of the only-child effect is informative. The main effect of only-child status on wealth management participation in this specification (-0.515, column (3)) is approximately 30% smaller than the corresponding baseline estimate in Table 4 (-0.739). The difference represents the portion of the adoption gap that operates through the dynamic trust channel, the component that activates when platform credibility is shocked. The remaining 70% reflects the static preference differences captured in the mediation analysis.

These results confirm that trust is a central channel through which only-child status shapes FinTech adoption. Only children withdraw more sharply when platform credibility is challenged,

[16] 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.

consistent with their lower baseline trust producing larger revisions in willingness to participate when adverse information arrives.

C Market-Level Evidence

The preceding analysis documents individual-level effects: only children have different investor attitudes 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 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:

$$FinTech_{p,t} = \beta_0 + \beta_1 \, OnlyChildShare_{p,t} + \boldsymbol{\gamma}' \mathbf{Z}_{p,t} + \mu_p + \delta_t + \varepsilon_{p,t} \tag{5}$$

where $FinTech_{p,t}$ measures regional FinTech development in province p in year t, $OnlyChildShare_{p,t}$ is the provincial share of only children, $\mathbf{Z}_{p,t}$ is a vector of province-level controls (GDP, industrial structure, trade openness, patent activity, industrialization, and financial development), 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 its sub-indices, and entrepreneurial activity, captured by the number of FinTech firms operating in the province.

Table 12 reports the results for 121 province-year observations. The provincial only-child share negatively predicts the aggregate FinTech development index (-30.905, $p < 0.05$, column (1)) and its coverage breadth component (-25.650, $p < 0.10$, column (2)). The usage depth coefficient is negative but imprecisely estimated (column (3)), suggesting that the effect operates primarily through the extensive margin of FinTech coverage rather than the intensity of use among existing adopters. These adoption patterns are consistent with the individual-level RDD estimates aggregating to the provincial level.

The only-child share also predicts FinTech entrepreneurial activity. The coefficient is negative and significant for both the log number of incumbent FinTech firms (-1.199, $p < 0.05$, column (4)) and the number of new FinTech entrants (-2.216, $p < 0.05$, column (5)). A 10 percentage point increase in the only-child share corresponds to approximately 11% fewer incumbent firms. That

the effect extends to new entrants is notable: it suggests that the demand environment shaped by demographic composition deters entry, not merely that existing firms contract.

These results extend the individual-level findings to market equilibrium. The demand-side preference channel documented in the preceding sections has consequences for FinTech entrepreneurial activity: when a larger share of the population carries the psychological barriers associated with only-child status, fewer platforms find it viable to enter and operate. While the province-level regressions capture correlations rather than the effects identified by the individual-level RDD, the consistency between the two levels of analysis reinforces the economic significance of the family socialization channel and suggests that demographic policy can shape the pace of financial innovation through its effect on investor preferences.

VII Conclusion

This paper establishes that the family structure in which an investor grows up has lasting consequences for financial behavior. Exploiting China's One-Child Policy in a regression discontinuity design, we show that only children are more risk-averse, less trusting, and less competitive than individuals with siblings, revealing that what shapes investor attitudes 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 attitude 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 preferences formed decades before the adoption decision. Trust is the dominant channel: only children retreat more sharply from digital platforms following 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. Our 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 preferences 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—the absence of horizontal sibling socialization and the concentration of vertical parental investment—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. This matters because the constraint is not easily remediated. Financial literacy can be taught; institutional quality can be reformed; digital access can be expanded. But the preference 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. The implication for policymakers is twofold. 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 not merely desirable but 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 the algorithmic opacity that characterizes many current platforms.

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 from the opposite direction, 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, our results raise a design question that extends to business disciplines beyond finance: if the barrier to adoption is dispositional rather than resource-based, can digital services be engineered to build trust incrementally, replicating through technology the graduated exposure to uncertainty that sibling interactions once provided?

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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.

Panel A: Treatment and Instrument	
Variable	Definition
Only Child	Indicator equal to one if the individual has no siblings.
Sibling	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 level, 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 Attitudes	
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.
Competitiveness	Log of the difference between the individual's stated minimum acceptable monthly salary and the average minimum wage in their province. Observations where the difference is non-positive are excluded.

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Appendix A – Continued

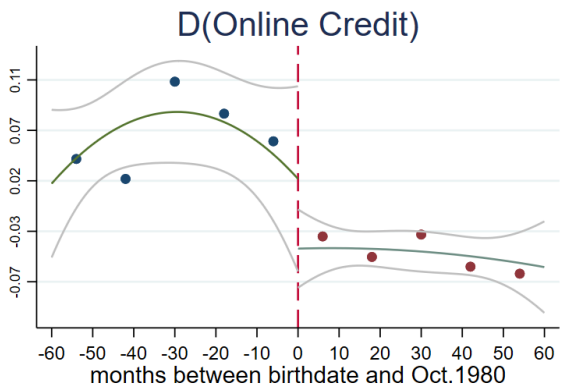
Panel D: FinTech Adoption	
Online Credit	Indicator equal to one if the individual borrows from or lends through an online platform.
Online Credit Share	Ratio of annual online lending amounts to total household assets, multiplied by 100.
Online Financial Products	Indicator equal to one if the individual holds digital wealth management products.
Online Financial Products Share	Ratio of online financial product holdings to total household assets, multiplied by 100.
Online Payment	Indicator equal to one if the individual holds a third-party payment account (e.g., Alipay or WeChat Pay).
FinTech	Composite indicator equal to one if the individual participates in any of Online Credit, Online Financial Products, or Online Payment.
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 is defined as holding stocks or having bank loans.

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.

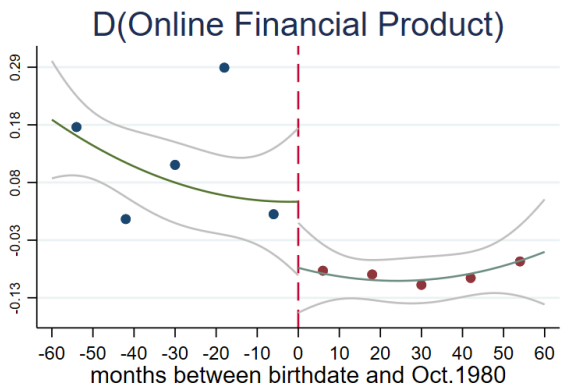
Figure 1: Family Structure and FinTech Adoption (RDD)

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 outcome variable against the running variable (birth month relative to the cutoff), with local polynomial fits and 95% confidence intervals estimated separately on each side of the cutoff. Panel A: Online Credit. Panel B: Online Financial Products. Panel C: Online Payment. Panel D: FinTech (composite indicator). The sample is restricted to individuals born within five years of the cutoff. The data are from the China Household Finance Survey.

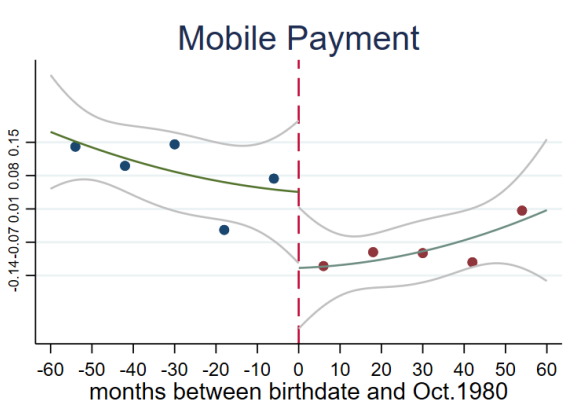
Panel A: Online Credit



Panel B: Online Financial Products



Panel C: Online Payment



Panel D: FinTech (Composite)

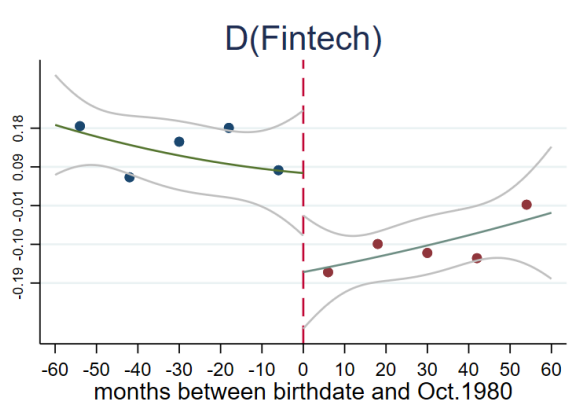
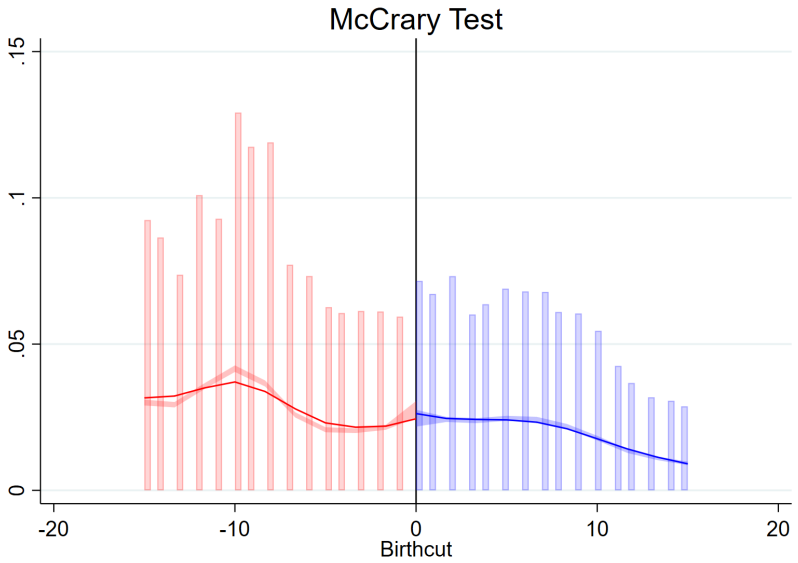


Figure 2: Density of the Running Variable

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 cutoff, which would suggest that individuals sort around the threshold. The sample is restricted to individuals born within five years of the cutoff. The data are from the China Household Finance Survey.



Method	T	P>|T|
local polynomial estimators	0.9177	0.3588

Table 1: Descriptive Statistics

This table reports summary statistics for the estimation sample. The sample includes urban household heads within the OCP-relevant age range across 24 provinces where the policy was binding. Panel A describes individual characteristics. Panel B reports investor attitude variables. Panel C reports FinTech adoption measures, including extensive-margin indicators and continuous measures scaled by total household assets (multiplied by 100). Panel D reports holdings of traditional financial products as shares of total household assets. The data are from the China Household Finance Survey. All variables are defined in Appendix A.

Panel A: Individual Characteristics						
Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Only Child	31,868	0.170	0.000	0.376	0.000	1.000
Siblings	31,868	2.320	2.000	1.782	0.000	13.000
Male	31,868	0.488	0.000	0.500	0.000	1.000
Married	31,868	0.829	1.000	0.376	0.000	1.000
Age (log)	31,868	3.689	3.761	0.281	2.944	4.094
Education	31,868	4.362	4.000	1.786	1.000	9.000
Employed	31,868	0.752	1.000	0.432	0.000	1.000
Offspring	31,868	0.148	0.000	0.355	0.000	1.000
Total Income (log)	31,868	11.026	11.190	1.240	6.217	13.467
Health	31,868	2.388	2.000	0.892	1.000	5.000
Fin Job	31,868	0.024	0.000	0.154	0.000	1.000
Owns Property	31,868	0.878	1.000	0.327	0.000	1.000

Panel B: Investor Attitudes						
Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Risk Aversion	26,933	3.781	4.000	1.225	1.000	5.000
Trust	26,587	2.268	2.000	0.883	1.000	5.000
Socialization	31,626	0.004	0.000	0.015	0.000	0.109
Competitiveness	24,174	8.181	8.161	0.612	5.456	9.904

Panel C: FinTech Adoption						
Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Online Credit	31,868	0.012	0.000	0.108	0.000	1.000
Online Credit Share	31,868	0.003	0.000	0.034	0.000	0.463
Online Financial Products	31,868	0.162	0.000	0.368	0.000	1.000
Online Financial Products Share	31,868	0.361	0.000	1.862	0.000	18.783
Online Payment	31,868	0.513	1.000	0.500	0.000	1.000

Panel D: Traditional Finance Products						
Variable	N	Mean	Median	Std. Dev.	Min.	Max.
Stocks	31,868	0.004	0.000	0.023	0.000	0.233
Mutual Funds	31,868	0.001	0.000	0.009	0.000	0.096
Bonds	31,868	0.000	0.000	0.000	0.000	0.031
Cash	31,868	0.061	0.009	0.135	0.000	0.969

Table 2: Family Structure and Investor Attitudes

This table reports second-stage estimates from a fuzzy RDD of the effect of only-child status on four investor attitude variables: Risk Aversion, Trust, Socialization, and Competitiveness. 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 cutoff. The data are from the China Household Finance Survey. Standard errors clustered at the individual level are reported in parentheses. *, **, and * denote significance at the 1%, 5%, and 10% levels. All variables are defined in Appendix A.

	(1)	(2)	(3)	(4)
Dependent Variable	Risk Aversion	Trust	Socialization	Competitiveness
Only Child	5.1568*	-0.9935*	-0.0196**	-0.5815*
	(2.6786)	(0.5796)	(0.0095)	(0.3408)
Controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	5,564	6,765	7,993	5,941

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

This table reports OLS estimates of the relation between only-child status and FinTech adoption. The dependent variables capture three segments: online credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). 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.

Dependent Variable	(1) Online Credit	(2) Online Credit Share	(3) Online Financial Products	(4) Online Financial Products Share	(5) Online Payment
Only Child	-0.0130***	-0.0022**	-0.0540***	-0.1598***	-0.0447***
	(0.0025)	(0.0009)	(0.0080)	(0.0430)	(0.0091)
Siblings	-0.0004	-0.0001	-0.0020	-0.0026	-0.0096***
	(0.0004)	(0.0002)	(0.0013)	(0.0068)	(0.0022)
Male	0.0045***	0.0010*	-0.0002	0.0366	-0.0075
	(0.0014)	(0.0006)	(0.0043)	(0.0230)	(0.0056)
Married	-0.0022	-0.0001	-0.0458***	-0.1985***	0.0447***
	(0.0024)	(0.0009)	(0.0066)	(0.0390)	(0.0081)
Age	-0.0104***	-0.0002	-0.1676***	-0.3789***	-0.3267***
	(0.0031)	(0.0011)	(0.0101)	(0.0490)	(0.0132)
Education	0.0007	0.0005**	0.0444***	0.1146***	0.0427***
	(0.0005)	(0.0002)	(0.0015)	(0.0083)	(0.0018)
Employed	0.0023	0.0009*	0.0107**	0.1244***	0.0428***
	(0.0014)	(0.0005)	(0.0044)	(0.0219)	(0.0062)
Offspring	-0.0002	0.0016**	-0.0029	-0.0389	0.0299***
	(0.0023)	(0.0008)	(0.0061)	(0.0282)	(0.0068)
Total Income	0.0008	0.0001	0.0230***	0.0298***	0.0327***
	(0.0006)	(0.0002)	(0.0017)	(0.0093)	(0.0022)
Health	0.0014	0.0003	0.0014	0.0180	-0.0177***
	(0.0008)	(0.0003)	(0.0022)	(0.0113)	(0.0030)
Fin Job	0.0040	0.0003	0.0412**	0.1882*	0.0129
	(0.0055)	(0.0020)	(0.0174)	(0.1060)	(0.0162)
Owns Property	-0.0108***	-0.0025***	0.0015	-0.7846***	0.0076
	(0.0026)	(0.0009)	(0.0062)	(0.0568)	(0.0078)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	31,868	31,868	31,868	31,868	31,868
R-squared	0.026	0.009	0.138	0.056	0.317

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 credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the cutoff, and the sample is restricted to individuals born within five years of the 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	0.0993***	0.0948***	0.0713***
	(0.0247)	(0.0239)	(0.0189)
Controls	No	No	Yes
Province FE	No	Yes	Yes
Year FE	Yes	Yes	Yes
RDD Polynomial Controls	Yes	Yes	Yes
N	8,192	8,192	8,192

Panel B: Second-Stage Regressions

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.2226**	-0.1064**	-0.7388**	-4.3823***	-0.9533**
	(0.1090)	(0.0420)	(0.3220)	(1.5591)	(0.3906)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,192	8,192	8,192	8,192	8,192

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 alternative bandwidths of four and six years around the 1980 cutoff. The dependent variables in Panels B and C capture three segments: online credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the 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 Balance

	RDD Coefficient
Siblings	-1.7407***
	(0.6464)
Male	-0.0558
	(0.2968)
Married	0.0154
	(0.1453)
Education	-0.9071
	(1.0992)
Employed	-0.0316
	(0.1902)
Offspring	0.1769
	(0.1887)
Total Income	-0.6201
	(0.6714)
Health	-0.0790
	(0.4185)
Fin Job	-0.0084
	(0.0998)
Owns Property	-0.0887
	(0.1622)

Panel B: Four-Year Bandwidth

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.3885*	-0.1606*	-1.4284*	-6.5866**	-1.0465
	(0.2154)	(0.0849)	(0.7565)	(3.2442)	(0.6721)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	6,675	6,675	6,675	6,675	6,675

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Table 5 – Continued

Panel C: Six-Year Bandwidth

Dependent Variable	(1) Online Credit	(2) Online Credit Share	(3) Online Financial Products	(4) Online Financial Products Share	(5) Online Payment
Only Child	-0.2230**	-0.1052***	-0.8551***	-5.0781***	-1.0913***
	(0.0886)	(0.0351)	(0.2729)	(1.3645)	(0.3290)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	9,819	9,819	9,819	9,819	9,819

Table 6: Placebo Tests

This table reports placebo tests using artificial cutoffs at 1978 (Panel A) and 1982 (Panel B), two years before and after the true 1980 policy cutoff. If the baseline results reflect the OCP rather than a pre-existing trend or other confounding discontinuity, the estimates should be insignificant at these placebo cutoffs. The dependent variables capture three segments: online credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the 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: Placebo Cutoff at 1978

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	0.0911	0.0460	-0.1297	-1.9251	-0.7750
	(0.2225)	(0.0984)	(0.4022)	(2.5967)	(0.6173)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,358	8,358	8,358	8,358	8,358

Panel B: Placebo Cutoff at 1982

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.0770	0.1276	0.3535	4.5577	-0.3737
	(0.2809)	(0.1471)	(0.7846)	(4.9800)	(0.9841)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,373	8,373	8,373	8,373	8,373

Table 7: Addressing Alternative Explanations

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 ("Less") and individuals with two or more siblings ("More"). The dependent variables capture three segments: online credit (columns 1–2), online financial products (columns 5–6), and online payment (columns 9–10), with extensive-margin indicators and continuous measures scaled by total household assets (multiplied by 100) for the first two segments. 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 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 Credit		Online Credit Share		Online Financial Products		Online Financial Products Share		Online Payment	
	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings	1 Sibling	≥2 Siblings
Only Child	-0.1360*	-0.1534*	-0.0721*	-0.0704**	-0.0872	-0.0710	-3.1942**	-2.2475**	-0.9015**	-0.4546*
	(0.0818)	(0.0815)	(0.0381)	(0.0290)	(0.2524)	(0.1764)	(1.5580)	(0.8722)	(0.4311)	(0.2357)
Control	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	4,491	5,613	4,491	5,613	4,491	5,613	4,491	5,613	4,491	5,613

Panel B: Digital Access

	(1)	(2)
	Internet Access	Communication Expenditure Share
Only child	0.2785	0.0013
	(0.3169)	(0.0015)
Controls	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	8,192	8,192

Table 8: FinTech-Specific Avoidance

This table tests whether only-child status reduces FinTech adoption specifically or financial market participation generally. Column (1) reports RDD estimates for FinTech, a composite indicator equal to one if the individual participates in any of online credit, online financial products, or online payment. Column (2) reports estimates for FinTech vs. Traditional, which 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 or having bank loans. All specifications include a linear polynomial in the running variable estimated separately on each side of the 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)
Dependent Variables	FinTech	FinTech vs. Traditional Finance
Only Child	-1.0286**	-1.2111**
	(0.3999)	(0.5330)
Controls	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	8,192	8,192

Table 9: 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 credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the 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 Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.2896*	-0.1374**	-1.7685**	-5.3191**	-1.4406**
	(0.1716)	(0.0650)	(0.7581)	(2.4598)	(0.6974)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	6,118	6,118	6,118	6,118	6,118

Panel B: Above-Median Education

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.1694	-0.0780	0.7571	-3.5325	-0.2555
	(0.1470)	(0.0651)	(0.5256)	(2.1993)	(0.4018)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	2,074	2,074	2,074	2,074	2,074

Panel C: Below-Median Investor Protection

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.3452**	-0.1361**	-1.2268**	-4.2603**	-1.5520**
	(0.1647)	(0.0581)	(0.4909)	(1.8552)	(0.6433)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	3,584	3,584	3,584	3,584	3,584

Continues on next page

Table 9 – Continued

Panel D: Above-Median Investor Protection					
	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.0789	-0.0749	-0.2545	-4.4014*	-0.4309
	(0.1168)	(0.0468)	(0.4027)	(2.2694)	(0.4250)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	4,608	4,608	4,608	4,608	4,608

Table 10: Mediation Analysis

This table reports second-stage RDD estimates of the relation between only-child status and FinTech adoption after augmenting the specification with four investor attitude variables: Risk Aversion, Trust, Socialization, and Competitiveness. The dependent variables capture three segments: online credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the cutoff, and the sample is restricted to individuals born within five years of the 1980 cutoff with non-missing values on all four attitude 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 Credit	Online Credit Share	Online Financial Products	Online Financial Product Share	Online Payment
Only Child	-0.0518	-0.0223	-0.3807	-1.8037	-0.2340
	(0.0355)	(0.0165)	(0.2898)	(1.2288)	(0.3191)
Risk Aversion	-0.0022*	-0.0009	-0.0301***	-0.0521	-0.0090
	(0.0013)	(0.0006)	(0.0068)	(0.0341)	(0.0079)
Trust	0.0032**	0.0015**	0.0074	0.0116	0.0295**
	(0.0016)	(0.0008)	(0.0102)	(0.0536)	(0.0118)
Socialization	0.0057	-0.0204	0.9420*	9.2746*	1.4471***
	(0.0872)	(0.0231)	(0.5394)	(5.3649)	(0.5490)
Competitiveness	0.0000	-0.0003	0.0457***	0.1724**	-0.0412**
	(0.0013)	(0.0007)	(0.0157)	(0.0817)	(0.0180)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	3,519	3,519	3,519	3,519	3,519

Table 11: The Trust Channel: Evidence from FinTech Scandal

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 credit (columns 1–2), online financial products (columns 3–4), and online payment (column 5). For the first two segments, we report an extensive-margin indicator and a continuous measure scaled by total household assets (multiplied by 100). All specifications include a linear polynomial in the running variable estimated separately on each side of the 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)	(4)	(5)
Dependent Variable	Online Credit	Online Credit Share	Online Financial Products	Online Financial Products Share	Online Payment
Only Child	-0.1908**	-0.0895***	-0.5147**	-3.4742***	-0.9059***
	(0.0826)	(0.0296)	(0.2132)	(0.9883)	(0.2709)
FinTech Scandal	0.1215*	0.0321	0.3892**	1.6157	0.3942*
	(0.0678)	(0.0240)	(0.1945)	(1.1693)	(0.2165)
Only Child ×	-0.4039	-0.2207*	-2.7630***	-11.4370***	-0.7657
FinTech Scandal	(0.3339)	(0.1146)	(0.8720)	(4.3584)	(1.0085)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	8,138	8,138	8,138	8,138	8,138

Table 12: 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), and *Financial Development* (total deposits and loans of banking financial institutions divided by GDP). The data are from the China Household Finance Survey and China Stock Market and Accounting Research. Standard errors 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	FinTech Aggregate Index	Coverage Breadth	Usage Depth	Incumbent FinTech firms	New FinTech Entrants
Only Child Share	-30.9046**	-25.6497*	-34.5481	-1.1986**	-2.2163**
	(14.7274)	(14.0960)	(28.9916)	(0.5625)	(1.0465)
GDP (log)	37.7229**	59.4341***	10.8871	0.9171	3.1530
	(17.3483)	(10.0673)	(34.1921)	(0.7167)	(2.0600)
Industrial Structure	-3.2249	0.1039	-3.9542	0.1767	0.4402
	(4.8291)	(3.8520)	(8.8182)	(0.2004)	(0.4109)
Trade Openness	-20.2809*	6.7474	-50.9580**	0.3163	1.9464*
	(10.2412)	(6.1690)	(21.3544)	(0.3220)	(1.1678)
Patents	0.0053	0.4208	4.5788	0.1649	0.3474
	(2.1393)	(1.5394)	(4.1728)	(0.1171)	(0.2403)
Industrialization	4.5303	-2.1589	3.6537	0.6895	0.6707
	(33.7341)	(24.6014)	(62.9368)	(1.1211)	(2.3280)
Financial Development	0.0259	6.4689***	-2.7119	0.2275	0.8594
	(3.8666)	(2.2448)	(7.7813)	(0.1623)	(0.5576)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	121	121	121	121	121
R-squared	0.997	0.998	0.988	0.982	0.900