# Does Legal Marriage Change Financial Behaviors?
## Quasi-Experimental Evidence from Taiwan

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## Abstract

This paper studies whether entering legal marriage changes household financial behavior. The empirical challenge is that marriage timing is strongly selected: couples who marry differ in preferences, resources, expectations, and risk tolerance, so a simple comparison of married and unmarried individuals confounds the institutional content of marriage with selection into it. We address this challenge using Taiwan's 2019 legalization of same-sex marriage, which abruptly extended access to the legal marriage contract to couples previously excluded from the institution while leaving the option set of opposite-sex couples unchanged. We link this policy shock to confidential Taiwanese administrative data covering household registration, income, and individual asset records, which allow us to measure portfolios directly rather than through self-reports. Using opposite-sex married individuals matched on sex, age, birthplace, pre-reform assets, and pre-reform wage income as controls, we estimate difference-in-differences and event-study models around the 2019 reform. We find that legal marriage increases stock market participation by about 1.55 percentage points, but reduces the equity share of liquid financial wealth (by about 2.45 percentage points) and of total measured assets (by about 2.40 percentage points) among individuals who were already stock market participants before the shock. Both responses are robust to alternative control-group constructions, weighting, and inference, and the event-study paths display no differential pre-trends. The findings suggest that marriage affects financial behavior differently across margins: it relaxes entry barriers into risky asset markets while inducing portfolio rebalancing toward lower equity exposure among existing participants. The pattern is consistent with marriage shifting financial decision-making from an individual to a household problem, in which risk sharing lowers the cost of entry while household-level responsibility tempers concentrated equity risk.

**Keywords:** Marriage; household finance; portfolio choice; stock market participation; same-sex marriage; legal recognition; Taiwan.

**JEL classification:** G11, G51, D14, J12, K36.

# 1. Introduction

Household financial decisions shape individual welfare and aggregate financial stability. Choices about stock market participation, saving, asset allocation, borrowing, and insurance determine how households absorb shocks, smooth consumption, and accumulate wealth over the life cycle. A large household finance literature documents that these choices vary enormously across otherwise similar households, and that the variation is shaped by information, participation costs, background risk, income, and family structure. Classic work shows that many households do not participate in stock markets at all, that stockholders and non-stockholders differ sharply in their consumption and wealth dynamics, and that background risk and life-cycle resources are first-order determinants of portfolio choice (Mankiw and Zeldes 1991; Haliassos and Bertaut 1995; Campbell 2006; Calvet et al. 2007; Fagereng et al. 2017; Gomes et al. 2021). At the same time, models of the family emphasize that households are not unitary decision makers. Allocations are negotiated within the family, and the sharing rule that governs them depends on legal institutions, outside options, and the enforceability of commitments (Becker 1973; Chiappori 1988; Lundberg and Pollak 1993).

These two literatures intersect at marriage. Most households make their largest financial decisions—how much to save, whether to enter the stock market, how much housing to buy, how much risk to bear—inside a marriage, and yet the financial consequences of the institution of marriage itself are surprisingly hard to pin down. The difficulty is that marriage is both a private relationship and a legal contract, and the two are usually bundled together. When researchers compare married and unmarried people, or follow individuals as they marry, they capture the joint effect of the relationship, the selection of particular people into marriage at particular times, and the legal contract. Isolating the contribution of the legal institution—the bundle of rights and

obligations that the state attaches to marriage—requires variation in access to that contract that is not driven by the same forces that determine who marries and when.

From this perspective, marriage is not only a demographic transition; it is a change in the institution within which financial decisions are made. Legal marriage bundles a standardized set of rights and obligations that can alter commitment, insurance, bargaining, and long-horizon planning inside the household. The marriage contract creates default property claims, inheritance rights, next-of-kin status, joint-filing options, and legally enforceable obligations that are absent, or far less certain, outside marriage. These institutional features can change how individuals bear financial risk, and they do so in offsetting ways. On one hand, marriage may *increase* risk-taking by allowing spouses to pool income, share idiosyncratic background risk, and spread the fixed cost of participating in financial markets across a larger resource base. On the other hand, marriage may *reduce* concentrated risk-taking if spouses internalize household-level downside risk and rebalance toward safer, more diversified portfolios once the consequences of a bad realization fall on a partner as well as on themselves. Whether the net effect of marriage is to raise or lower financial risk-taking is therefore theoretically ambiguous, and the answer may differ across the margins of behavior that make up a household portfolio.

Identifying the causal effect of marriage on these behaviors is difficult precisely because marriage timing is highly selected. Individuals who marry at a particular moment may differ from those who do not in patience, risk preferences, wealth, family expectations, and unobserved financial sophistication. People who are more financially organized may be both more likely to marry and more likely to invest; people anticipating a large income gain may time both their marriage and their entry into the stock market to the same year. Standard comparisons between married and unmarried individuals therefore confound the institutional effect of marriage with

selection into marriage, and the direction of the resulting bias is rarely obvious. This identification problem is not merely a technical nuisance; it is the central reason that the financial consequences of legal marriage remain unsettled despite the centrality of marriage to household finance.

This paper addresses the problem by exploiting Taiwan's legalization of same-sex marriage in 2019. The reform abruptly extended access to the legal marriage contract to same-sex couples who had previously been excluded from the institution, while the option set available to opposite-sex couples was unchanged. It therefore generates variation in the *timing of access* to a common legal institution that is far less tightly linked to unobserved household traits than the timing of marriage in settings where marriage has long been universally available. Same-sex couples who married in 2019 were newly able to register a marriage that had simply not existed for them a year earlier; many of them are likely to have been in committed relationships well before the reform and to have married promptly once the law allowed it. The timing of their entry into the contract is therefore driven in large part by the date of the legal change rather than by an idiosyncratic, individually chosen moment.

The setting is useful because it shifts access to a legal contract without requiring the researcher to compare ordinary married and unmarried individuals at a point in their life cycle. Opposite-sex couples, who had long had access to marriage, provide a natural benchmark for financial behavior over the same macroeconomic period, absorbing market-wide movements in equity prices, interest rates, and the business cycle that would otherwise contaminate a pre-post comparison. The design does not assume that same-sex and opposite-sex couples are identical. Instead, it uses the reform-induced timing of access to legal marriage, combined with pre-reform matching on observable financial characteristics, to isolate changes in financial behavior around entry into the marriage contract. We are careful throughout to interpret the estimates as the effect of entering legal

marriage for the couples induced to marry by the reform, and we discuss in detail the conditions under which this local effect speaks to the general economic content of marriage.

We combine this institutional setting with confidential Taiwanese administrative data linking household registration, income, and asset records at the individual-year level. The data let us observe marriage dates, spouse identifiers, demographics, taxable income from multiple sources, and the components of wealth, including equity holdings, housing, land, savings proxies and other liquid asset[1] proxies constructed from interest income. We define the treated group as individuals in same-sex couples who married in 2019. The control group consists of individuals in opposite-sex marriages who were already married before the reform and who are matched to treated individuals on sex, age, birthplace, pre-reform total assets, and pre-reform wage income, yielding a one-to-five treated-to-control sample. The research design compares the change in financial behavior after legalization for treated individuals relative to their matched controls, both on average, through difference-in-differences, and dynamically, through event-study estimation that allows us to inspect pre-trends and the timing of the response.

The findings reveal a sharp distinction between the extensive and intensive margins of risky asset holding. Entering legal marriage increases stock market participation by about 1.55 percentage points in the baseline specification, consistent with marriage relaxing participation constraints or lowering the relative cost of market entry. Against a pre-reform participation rate of about 23 percent, this is an increase of roughly six percent in the propensity to hold equity. Conditional on pre-reform participation, however, equity exposure declines: the equity-savings ratio falls by about 2.45 percentage points and the equity-asset ratio by about 2.40 percentage points. The event-study estimates show flat pre-reform paths and post-reform responses that build

[1] Other liquid asset include Treasury and corporate bonds.

over time, supporting a causal reading. Taken together, the results indicate that the same legal institution can bring new individuals into risky asset markets while leading existing participants to hold less concentrated equity risk. This opposite-signed pattern across margins is the paper's central empirical finding, and it is difficult to reconcile with simple selection stories, which would tend to push both margins in the same direction.

The paper makes four contributions. The first is to household finance. Prior research has studied why households participate in risky asset markets, why so many hold no stocks at all, and how wealth, background risk, entrepreneurial risk, and demographics shape portfolio allocations (Mankiw and Zeldes 1991; Haliassos and Bertaut 1995; Heaton and Lucas 2000; Campbell 2006; Calvet et al. 2007; Calvet and Sodini 2014; Fagereng et al. 2017; Gomes et al. 2021). A related literature relates marital status and children to saving and portfolio choice (Love 2010). We contribute by using a policy shock to access, rather than cross-sectional variation in marital status, to ask whether entering the legal institution of marriage changes financial behavior. The distinction between stock market entry and portfolio intensity is central to our results and is often blurred in studies that examine a single summary measure of risk-taking; our data and design let us separate the two and show that they respond differently.

The second contribution is to research on marriage, intra-household allocation, and commitment. Collective models of the household emphasize that family allocations depend on bargaining power, outside options, and enforceable rules rather than on a single household objective (Chiappori 1988; Lundberg and Pollak 1993). Empirical work on marriage contracts and family law shows that legal institutions affect household behavior by changing commitment, risk sharing, and intertemporal incentives (Matouschek and Rasul 2008; Mazzocco 2004, 2007; Stevenson and Wolfers 2006; Voena 2015). We extend this line of work to financial portfolios.

Risky-asset participation and equity shares are direct, high-frequency measures of how households bear financial risk, making them a natural setting for testing whether the legal marriage contract changes risk sharing and household-level planning. Our finding that participation rises while concentration falls is consistent with a view of marriage as simultaneously an insurance arrangement and a commitment device that internalizes risk to a partner.

The third contribution is to the literature on same-sex marriage recognition. Existing evidence shows that marriage equality affects labor-market outcomes, tax filing, credit access, mortgage demand, and health (Sansone 2019; Miller and Park 2018; Hagendorff et al. 2022; Carpenter et al. 2021; Friedberg and Isaac 2024). We shift the outcome margin from labor, tax, and credit-market access to individual-level portfolio choice. This matters because portfolios are not merely downstream consequences of legal recognition; they are forward-looking decisions that reveal how individuals manage risk once they have entered a legally enforceable partnership. Although our identifying variation comes from same-sex couples, the mechanism of interest is the economic content of legal marriage rather than sexual orientation per se: the rights and obligations activated by the contract are the same as those governing opposite-sex marriages, so the behavioral responses we identify reflect general marriage mechanisms rather than population-specific effects.

Finally, the paper contributes empirically by using administrative records rather than survey-reported portfolios. Many household finance studies rely on surveys that are indispensable but limited in sample size, reporting precision, and timing. The Taiwanese administrative data allow us to observe marriage dates, spouse links, income, and asset components precisely and at annual frequency, with full coverage of the relevant population rather than a sample. This measurement advantage is what allows us to separate the extensive margin of stock market participation from

intensive-margin changes in equity exposure, a separation that is at the heart of our findings and that is difficult to achieve with the smaller and noisier portfolios reported in surveys.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and develops the hypotheses. Section 3 describes the data, sample construction, and outcome variables. Section 4 presents the empirical strategy. Section 5 reports the results. Section 6 discusses identification, robustness, and the threats the design must address. Section 7 concludes.

## 2. Institutional Background and Hypothesis Development

### 2.1 Taiwan's 2019 legalization of same-sex marriage

Taiwan legalized same-sex marriage through the Act for Implementation of J.Y. Interpretation No. 748, which took effect on May 24, 2019. The reform was the culmination of a multi-year legal and political process. In May 2017, the Constitutional Court held in Interpretation No. 748 that the provisions of the Civil Code restricting marriage to a man and a woman were unconstitutional, on the grounds that excluding same-sex couples from marriage violated the constitutional guarantees of freedom of marriage and of equality before the law. The Court gave the legislature two years to amend the law and specified that, if it failed to act, same-sex couples would be able to marry under the existing framework. A set of referenda in late 2018 then shaped the form the reform took, leading the legislature to enact a dedicated special act rather than amend the Civil Code directly. With the Act's entry into force in May 2019, Taiwan became the first jurisdiction in Asia to legalize same-sex marriage.

For identification, the central feature of the reform is that it changed access to the legal marriage contract for same-sex couples while leaving the institution, and the option set, of opposite-sex couples unchanged. The special act granted same-sex married couples essentially the same legal rights and obligations as opposite-sex married couples, including the statutory marital-

property regime, inheritance rights, next-of-kin and medical-decision authority, and eligibility for joint tax treatment. The economic content of the contract activated in 2019 is therefore the same content that has long governed opposite-sex marriages. This is what allows us to interpret the behavioral responses of newly married same-sex couples as evidence on the general economic effects of legal marriage, rather than on something specific to the same-sex context.

The reform also produced a discrete and well-dated shock. In the administrative data, no same-sex marriages are recorded before May 2019, and a pronounced spike occurs in the first month of eligibility, with 670 same-sex couples (1,335 individuals)[2] registering in May 2019 alone, followed by a steady flow over the remainder of the year. The sharp bunching of registrations at the moment the law took effect is consistent with the presence of a stock of couples who were ready to marry and who had been waiting for legal access. Opposite-sex marriages, by contrast, follow their usual seasonal pattern throughout the year, governed by the same seasonal and macroeconomic forces before and after the reform. Figure 1 plots monthly marriage counts by couple type and makes the timing of the shock visible. This discontinuity in access is the source of our identifying variation.

### 2.2 What the marriage contract changes

To fix ideas about why legal marriage might alter financial behavior, it is useful to be concrete about what the contract does. First, marriage establishes a default property regime. Under Taiwan's statutory regime, property acquired during the marriage is subject to division upon divorce or death, which converts the implicit promise of mutual support into a state-enforced claim and gives each spouse a stake in the assets accumulated by the other. Second, marriage confers inheritance and

[2] Individuals without Taiwanese citizenship are excluded from the administrative dataset. As a result, 5 people from the 670 couples are missing, leaving 1,335 individuals on record.

next-of-kin status, so that a spouse becomes a statutory heir and acquires authority over medical and financial decisions if the other is incapacitated. Third, marriage changes the tax environment through joint-filing options that alter the after-tax return to different sources of income. Fourth, marriage creates enforceable obligations of mutual support that persist beyond the relationship itself. Each of these features can plausibly change the way individuals save and bear financial risk, and none of them is available, or is available only imperfectly and at high transaction cost, to unmarried cohabiting partners.

### 2.3 Channels through which marriage may affect financial behavior

Marriage can affect financial behavior through several economically distinct channels, which differ in their predictions for the extensive and intensive margins of risky asset holding. The first is *risk sharing*. Marriage allows spouses to pool resources and partially insure one another against idiosyncratic income, health, and liquidity shocks. The legal contract reinforces this informal insurance by creating enforceable claims: marital-property rules require that gains accumulated during the marriage be divided upon divorce or death, which converts an implicit promise into a state-backed obligation that both parties can rely on when making long-horizon decisions. If individual market participation is constrained by background risk or limited liquidity, this enhanced risk-bearing capacity raises the willingness to enter the stock market and, by the same logic, could raise equity exposure conditional on participation. Risk sharing therefore predicts a higher extensive margin and, on its own, a higher intensive margin.

The second channel is *income and resource pooling*. Once spouses combine household resources, the fixed cost of acquiring financial information, opening and maintaining brokerage accounts, and bearing short-term portfolio volatility falls relative to household wealth. Because participation costs are a leading explanation for limited stock market participation in the household

finance literature, pooling predicts higher participation after marriage. Its prediction for the intensive margin is ambiguous: a larger and more stable resource base can support more equity, but the same pooling may also fund competing uses of household wealth, most importantly housing, which can crowd out equity in the portfolio. Pooling therefore predicts a higher extensive margin but an uncertain effect on intensity.

The third channel is *financial responsibility and household-level risk management*. Marriage can make individuals internalize the downside risk borne by a spouse and by the joint balance sheet. A married person may be less willing to hold highly concentrated equity exposure when an adverse return would impair the household's liquidity or a partner's welfare, and may therefore rebalance toward safer or more diversified positions. The legal contract sharpens this channel, because the spouse now has an enforceable claim on the household's assets, so a bad investment outcome is no longer borne by the investor alone. This channel predicts a decline in intensive-margin equity exposure even as participation rises, because the unit of decision-making shifts from the individual to the household and the relevant objective becomes the stability of joint, rather than individual, resources.

These channels need not operate in the same direction on both margins, and that is the conceptual core of the paper. Risk sharing and resource pooling lower the barriers to entering risky-asset markets, predicting a higher extensive margin. Financial responsibility, by contrast, can compress the intensive margin among those already in the market. The net effect on observed portfolios is therefore an empirical question, and the administrative data, which let us measure entry and intensity separately, are well suited to answering it. A further implication is that the two margins may be driven by different people: entry is most likely to respond among those previously

constrained from participating, while rebalancing is most relevant for those who already hold substantial equity. Table 1 summarizes the predicted sign of each channel on each margin.

These considerations lead to three hypotheses, which organize the empirical analysis.

*H1. Entering legal marriage increases stock market participation (the extensive margin).* Risk sharing and resource pooling both predict that marriage relaxes the constraints that keep some individuals out of the stock market.

*H2. Conditional on pre-marriage participation, entering legal marriage changes equity exposure (the intensive margin), with the sign determined by the balance between the risk-sharing and financial-responsibility channels.* If responsibility dominates, exposure falls even as participation rises.

*H3. The financial effects of entering legal marriage are larger among individuals with greater financial capacity, and may differ by gender, reflecting differences in baseline portfolios and within-household financial roles.* Households with more resources have more scope both to enter and to rebalance risky positions.

## 3. Data and Sample

### 3.1 Data sources

The analysis uses confidential Taiwanese census data linked to comprehensive individual-level tax and asset records, maintained by the relevant fiscal and registration authorities and accessed under strict confidentiality protections. The linked dataset has three components. The household registration data include individual identifiers, spouse identifiers, parents' identifiers, sex, age, place of birth, current residential address, and marriage information; these records allow us to date marriages precisely, link spouses to one another, and reconstruct household and kinship networks across the population. The income records include wage income, interest income,

dividend income, rental income, and other sources of taxable income. The asset records include holdings of equities, housing, land, and vehicles. Asset records are available from 2008 to 2022, while household-registration and income records currently span 2008 to 2024, and updates for later years are expected to become available as they are released.

From these raw records we construct the financial variables used in the analysis. We combine equity-holding records with market valuation data to construct each individual's equity wealth at annual frequency. Because the asset registry does not directly report the value of bank deposits, we proxy for the value of savings by capitalizing observed interest income at prevailing interest rates, a standard approach for inferring deposit balances from administrative tax data. We apply the same method to proxy for other forms of liquid asset, such as Treasury and corporate bonds. Housing and land are valued from the asset registry. The result is a panel of individual balance sheets, with separate components for equity, savings, housing, and land, that we can follow before and after the reform.

Relative to the survey data on which much of the household finance literature relies, these records offer three advantages that are important for our research design. First, marriage timing is observed exactly, from the registration date, rather than recalled by respondents. Second, asset components are measured from administrative sources rather than self-reported, which substantially reduces measurement error in the outcomes and avoids the well-known tendency of households to misreport financial wealth in surveys. Third, the panel structure and population coverage let us trace the same individuals before and after the reform at annual frequency, and let us construct large, well-matched control groups that would be infeasible in a survey of a few thousand households. Throughout, we report monetary values in U.S. dollars, converted at

approximately NT$31.5 per U.S. dollar, to ease interpretation; all statistical analysis is invariant to this rescaling.

### 3.2 Treatment and control groups

The treated group consists of individuals in same-sex couples who married in 2019, the first year in which legal same-sex marriage was available. These individuals are the most directly exposed to the legalization shock, and, as discussed above, many of them are likely to have intended to marry earlier but to have been legally unable to do so. Defining treatment by the 2019 marriage cohort captures precisely the group whose entry into the contract was enabled by the reform.

The matched control group is drawn from individuals in opposite-sex marriages who were already married before the reform. The logic of the comparison is counterfactual: in a world without legal constraints on same-sex marriage, some of the same-sex couples who married after 2019 would likely have married earlier, and would already have been married, like the controls, during the sample period. We therefore use already-married opposite-sex individuals who are observationally similar in financial terms as a proxy for the counterfactual financial behavior of treated individuals had they already had access to legal marriage. Because the controls are already married throughout, they are not themselves treated by the reform, and they experience the same aggregate environment as the treated group, which is what allows the difference-in-differences design to net out market-wide movements.

Matching uses pre-treatment characteristics measured in 2018, the year immediately before legalization. For each treated individual, the procedure first restricts the pool of potential controls to individuals in opposite-sex marriages who married before 2017 and who share the same sex, age, and birthplace, an exact match on these demographic cells. Within that cell, it retains the ten

individuals closest in total assets and then, among those ten, selects the five closest in wage income. This sequential nearest-neighbor procedure yields a matched sample with a one-to-five treated-to-control ratio, balancing the desire for close matches against the need for a sufficiently large control group for precise inference. In Section 6 we show that the results are robust to reversing the order of the matching variables—selecting on wage first and assets second—and to alternative pre-reform cutoffs for the control pool.

Table 2 reports summary statistics for the matched treated and control groups in 2018. The two groups are closely balanced on every dimension used for matching and, importantly, on the pre-reform levels of the outcome variables themselves. They have nearly identical age (about 34 years on average), the same sex composition (about 28 percent male), and the same baseline stock market participation rate (0.230 in both groups). They are also very similar in the equity-savings ratio (0.162 versus 0.160), the equity-asset ratio (0.082 versus 0.085), equity and savings values, housing and land values, wage income, and total gross assets. The close balance on the pre-reform levels of the outcomes is what gives the difference-in-differences design its credibility: post-reform divergence between groups that started from the same financial position is naturally attributed to the change in access to marriage rather than to pre-existing differences in financial circumstances. The event-study leads, reported in Section 5, confirm that the groups were not only similar in levels but also on parallel paths before the reform.

### 3.3 Outcome variables

We study three outcomes that jointly capture the extensive and intensive margins of risky asset holding. *Stock market participation* is an indicator equal to one if an individual holds positive equity wealth in a given year; it captures the extensive margin, that is, whether the household is in the stock market at all. The *equity-savings ratio* is equity wealth divided by the sum of equity

wealth and savings, and the *equity-asset ratio* is equity wealth divided by total measured assets, where total assets comprise equity, savings, housing, land, and other liquid asset. The two ratios capture the intensive margin: the concentration of the portfolio in risky equity, measured against liquid financial wealth and against the full asset base, respectively. The two denominators are complementary. The equity-savings ratio isolates the allocation between the two most liquid components of the portfolio, while the equity-asset ratio places equity in the context of the household's entire balance sheet, including illiquid real estate, and therefore reflects rebalancing toward housing as well as toward cash.

Because the intensive-margin ratios are defined only for individuals with positive equity, the intensive-margin regressions are estimated on the subsample of individuals who were already stock market participants before the shock. This sample restriction is also conceptually appropriate: the question on the intensive margin is how marriage changes the portfolios of people who are already investing, which is a different question, and a different population, from the extensive-margin question of who enters the market. Keeping the two analyses separate is what allows the opposite-signed responses to emerge clearly.

## 4. Research Design

The baseline design estimates difference-in-differences models on the matched sample. Let $T_i$ equal one for treated individuals in same-sex couples who married in 2019, and let $Post_t$ equal one for years *t* at or after 2019. The baseline estimating equation is

$$Y_{it} = \alpha + \gamma T_i + \lambda_t + \beta(T_i \times Post_t) + X'_{it}\delta + \varepsilon_{it}, \qquad (1)$$

where $Y_{it}$ denotes one of the three financial outcomes, $\alpha$ is the intercept, and $\lambda_t$ is the year fixed effect. $T_i$ captures time-invariant heterogeneity between the treated and control groups,

including fixed differences in preferences or circumstances between same-sex and opposite-sex individuals. The year fixed effects absorb aggregate shocks common to all individuals in different years, such as movements in equity prices, interest rates, and the business cycle. The vector $X_{it}$ includes time-varying controls, entering in successive specifications: a quadratic in age together with a sex indicator; birthplace indicators; total income in the year before the shock; and indicators for the number of years since marriage (the marriage stage). Standard errors are clustered at the individual level to allow for arbitrary serial correlation in the outcomes within person over time.

The coefficient $\beta$ is the parameter of interest. It measures the differential change in the treated group after legalization relative to the matched controls and is the average post-reform effect of entering legal marriage for the couples induced to marry by the reform. Because the controls are already married and experience the same calendar-year shocks, $\beta$ is identified from the additional post-2019 change among treated couples relative to the contemporaneous change among controls, after accounting for baseline differences between the two groups and the average post-legalization shift common to both groups. We report the estimates as controls are added across columns, both to show that the effect is not an artifact of any particular set of covariates and to gauge the sensitivity of the magnitude to controlling for income and marriage duration.

To evaluate dynamics and assess pre-trends, we also estimate an event-study specification that interacts treatment status with event-time indicators relative to 2019, omitting 2018 (the year immediately before legalization) as the reference:

$$Y_{it} = \alpha + \gamma T_i + \lambda_t + \sum_{k \neq -1} \beta_k (T_i \times 1\{t - 2019 = k\}) + X_{it}'\delta + \varepsilon_{it}. \qquad (2)$$

The lead coefficients $\beta_k$ for $k < 0$ test whether treated and control individuals were on parallel paths before the reform; coefficients close to zero support the identifying assumption that,

absent the reform, the two groups would have continued to move together. The lag coefficients for $k \geq 0$ trace the dynamic response after legal marriage became available, and reveal whether the effect appears immediately at registration or builds gradually as households adjust their portfolios. We estimate the event study over a window from four years before to three years after the reform, which balances the desire for a long pre-period to test pre-trends against the constraint that the asset data end in 2022. In all specifications, inference is based on individual-clustered standard errors, and the figures plot 95 percent confidence intervals.

The key identifying assumption is the standard parallel-trends condition: absent the 2019 reform, treated and matched control individuals would have followed parallel trends in each outcome. This assumption is not directly testable, but the design provides three forms of support for it. The pre-reform balance in Table 2 shows that the groups start from the same financial position; the flat event-study leads show that they were on parallel paths before the reform; and the use of already-married opposite-sex controls differences out the aggregate environment. Section 6 discusses the residual threats to this assumption and the additional design features that address them.

## 5. Results

### 5.1 Stock market participation

The first result is that entering legal marriage raises stock market participation. Table 3 reports the baseline difference-in-differences estimates as controls are added across columns. The coefficient on the interaction of treatment and the post-reform indicator is positive, stable, and statistically significant throughout. The estimated effect is 1.53 percentage points with demographic controls and clustered standard errors (column 1), 1.55 percentage points when using a slightly different control-group formation method (column 2), 1.53 percentage points after

adding birthplace controls (column 3), and 1.55 percentage points after further adding total income in the year before the shock as additional controls (column 4). The stability of the estimates after altering the matching procedure or adding birthplace and income controls indicates that the effect is not driven by the specific method used to construct the control group or by compositional differences in location or earnings. Adding indicators for the marriage stage in column 5 raises the estimated interaction to 1.88 percentage points, because these controls change the comparison by absorbing duration-of-marriage effects; we treat columns 1 to 4 as the baseline and report column 5 for completeness. Our preferred estimate, with demographic, birthplace, and income controls, is an increase of about 1.55 percentage points.

This effect is economically meaningful. Baseline participation in the matched sample is about 23 percent, so a 1.55 percentage point increase represents a rise of roughly six percent relative to the pre-reform mean. The result is consistent with marriage relaxing participation constraints (H1). When spouses pool income and liquidity, the fixed cost of stock market entry falls relative to household resources, so individuals for whom the cost previously exceeded the benefit are pushed over the threshold into participation. Legal marriage may also make long-horizon financial planning more salient and prompt households to formalize investment decisions, for example by opening accounts in anticipation of joint goals such as housing or children. Both interpretations point to the same prediction—a higher extensive margin—which is what the data show.

Figure 2 reports the event-study estimates for participation and supports a causal reading. The pre-reform coefficients are small and statistically indistinguishable from zero; the interaction terms at event times four, three, and two years before the reform are -0.0056, -0.0004, and -0.0001, respectively, indicating no differential pre-trend between treated and control individuals. The post-reform coefficients then turn positive and grow over time, rising from near zero in the reform year

to about 0.8 percentage points one year out, 1.8 percentage points two years out, and 1.7 percentage points three years out. The delayed build-up is plausible and indeed reassuring: portfolio entry requires opening accounts, acquiring information, and adjusting household financial routines, and would not be expected to change mechanically at the registration date. A response that appeared abruptly and only at the reform year, by contrast, would have raised concerns about a mechanical or anticipatory artifact.

### 5.2 Equity exposure among market participants

The second result is that equity exposure declines among individuals who were already participating in the stock market before the shock. Tables 4 and 5 report the equity-savings ratio and the equity-asset ratio, respectively, both estimated using the subsample of individuals who held equity prior to treatment, as measured in 2018. The treated-by-post interaction is negative and statistically significant across the baseline specifications. The equity-savings ratio falls by about 2.45 percentage points (2.51, 2.26, 2.51, and 2.51 percentage points across columns 1 to 4), and the equity-asset ratio falls by about 2.40 percentage points (2.38, 2.47, 2.38, and 2.38 percentage points). As with participation, the estimates are essentially unchanged when using a slightly different control-group formation method or adding birthplace and income controls. This stability indicates that the intensive-margin response is not an artifact of the specific matching procedure or of compositional differences in location and income. The two ratios, which use different denominators, move together, which suggests that the rebalancing reflects a genuine reduction in the portfolio weight on equity rather than a mechanical change in any single component.

The same legal institution that increases market entry therefore reduces the intensity of risky-asset holding among existing participants. This pattern is consistent with marriage changing the unit of financial decision-making. Individuals who already hold equities appear to rebalance once

they internalize spouse-level risk, anticipated household obligations, or a desire for more stable household liquidity. In this reading, marriage does not simply raise risk-taking through better insurance; it also shifts portfolio choice from an individual optimization problem toward household-level risk management, and the two forces leave opposite imprints on the extensive and intensive margins. The decline in the equity-asset ratio, which includes housing and land in the denominator, is also consistent with married households moving wealth toward real estate, a possibility we return to in the discussion of mechanisms.

Figures 3 and 4 report the event-study estimates for the two intensive-margin outcomes and again show flat pre-trends followed by a clear post-reform response. The pre-reform coefficients are close to zero, indicating no differential pre-trend, while the post-reform coefficients move below zero. For the equity-savings ratio, the post-reform interactions are about -2.2, -2.4, -2.1, and -3.6 percentage points at event times zero through three; for the equity-asset ratio, they are about -1.8, -0.9, -2.3, and -2.9 percentage points. The decline is visible immediately and, like the participation response, tends to grow over the post-reform window. Figure 5 combines the three outcomes in a single dynamic plot and summarizes the central finding compactly: after legalization, stock market participation rises while equity exposure declines among participants. The opposite movement of the two margins is the paper's main result and is robust across specifications and outcome definitions.

### 5.3 Heterogeneity

The effects are not uniform across individuals, and the heterogeneity is informative about which households drive each margin. Table 6 reports treated-by-post coefficients from subsample regressions by gender, by asset level, and by wage level. On the extensive margin, both men and women become more likely to enter the stock market after marriage, with a larger point estimate

for men (1.96 percentage points) than for women (1.35 percentage points), and both statistically significant. The broad-based nature of the entry response is consistent with risk sharing and resource pooling operating for a wide range of households, not just a narrow subgroup.

On the intensive margin, however, the decline in equity exposure is concentrated among women. The equity-savings ratio falls by 3.16 percentage points and the equity-asset ratio by 3.62 percentage points for women, while the corresponding estimates for men are small and statistically insignificant (a 1.07 percentage point decline in the equity-savings ratio and essentially no change in the equity-asset ratio). This asymmetry is consistent with differences in household bargaining, financial responsibility, or baseline portfolio composition by gender: if women in these households take on a larger share of responsibility for household-level financial stability after marriage, or hold systematically different baseline portfolios, the rebalancing channel would load more heavily on them. We interpret this gender pattern cautiously, because men and women in the sample differ in baseline assets, income, and within-couple financial roles, and disentangling these explanations is a natural direction for further work; but the basic fact that the intensive-margin response is female-driven is robust.

Financial capacity also matters, consistent with H3. The extensive-margin response is concentrated among individuals with above-median assets and above-median wages: the participation effect is 1.91 percentage points for the above-median-asset group and 2.40 percentage points for the above-median-wage group, both significant, while the below-median groups show small effects that are weak or insignificant (1.17 percentage points for below-median assets and an insignificant 0.65 percentage points for below-median wages). The intensive-margin declines are likewise stronger for higher-capacity individuals; for example, the equity-savings and equity-asset ratios fall by 2.91 and 2.28 percentage points among above-median-wage individuals, with

smaller and insignificant effects below the median. These patterns indicate that marriage changes portfolio behavior most where individuals have the resources to enter or rebalance risky positions, which is exactly where the channels in Section 2 have the most scope to operate.

### 5.4 Robustness

The baseline results are robust along several dimensions. First, the estimates are stable across the two matching procedures: selecting controls on assets first and wages second, or on wages first and assets second, yields very similar interaction coefficients on all three outcomes, indicating that the findings do not depend on the precise order in which the continuous matching variables are applied. Second, weighting the regressions and moving from conventional to individual-clustered standard errors leaves the point estimates and their statistical significance essentially unchanged, so the conclusions are not sensitive to the choice of weighting or inference. Third, the results are not sensitive to the precise pre-reform cutoff used to define the control pool of already-married opposite-sex individuals. Fourth, and most importantly, the event-study leads are flat for all three outcomes, which indicates that the post-reform divergence is not the continuation of a pre-existing trend but a genuine response that begins at the reform. Section 6 discusses the remaining identification concerns and the design features that address them.

## 6. Identification: Threats and Robustness

The identifying assumption is that, absent the reform, treated and matched control individuals would have followed parallel trends in each outcome. The flat event-study leads in Figures 2 to 4 provide direct support, and the close pre-reform balance in Table 2 reduces the scope for differential dynamics. This section discusses the two most important threats to the interpretation and the design features that address them.

### 6.1 Selection into marriage timing

A first concern is that couples who married immediately after legalization may differ from other couples in unobserved, time-varying ways that matching on observables does not capture. For example, couples who married in 2019 might have been on the cusp of some other life or financial transition that independently changes their portfolios. The design mitigates this concern in several ways. The control group is constructed entirely from pre-reform information and is balanced on the pre-reform levels of the outcomes themselves, so any residual selection would have to operate through trends that are orthogonal to the matched financial characteristics yet visible only after 2019. The event-study leads show no such differential dynamics in the pre-period, which is the most direct evidence that the groups were not already diverging. And the use of opposite-sex married controls, who experience the same aggregate environment, differences out market-wide movements in equity prices and interest rates that might otherwise be mistaken for a treatment effect.

Perhaps the strongest argument against a simple selection story is the structure of the results themselves. The extensive and intensive margins move in *opposite* directions. A selection account in which more financially aggressive individuals time their marriage to 2019 would tend to push both margins in the same direction—such people would both enter the market and hold more equity—and so would not naturally generate the pattern we observe of rising participation alongside falling concentration. The opposite-signed responses are, by contrast, exactly what the combination of a risk-sharing channel on entry and a responsibility channel on intensity predicts. This makes a mechanical or selection-driven explanation for the joint pattern difficult to sustain.

### 6.2 Legal contract versus social recognition

A second concern is that the 2019 legalization may affect behavior through social recognition—by reducing stigma and increasing public acceptance—rather than through the legal rights and obligations of the marriage contract. The two channels carry different interpretations. The legal-contract channel speaks to the economic content of marriage that generalizes to all couples, whereas a pure recognition channel would be specific to the population whose social status changed. Distinguishing them is therefore important for interpreting the estimates as evidence on the general effects of marriage.

The institutional history helps separate the channels. Before nationwide legalization, several Taiwanese counties introduced same-sex partnership registration on a staggered schedule. These registrations conferred public and administrative recognition of same-sex relationships but did not grant the legal rights of marriage. They therefore changed social recognition while holding the legal contract fixed. The reform we exploit, by contrast, activated the full bundle of marital rights at a single national date. The combination of a discrete, dated change in legal rights and the absence of differential pre-trends in the period when only recognition was changing is consistent with the behavioral responses operating through the legal content of marriage rather than recognition alone.

Two further features of the results support a contract-based interpretation. First, the responses are concentrated among higher-capacity households and load on portfolio intensity—margins that map naturally onto the property, inheritance, and risk-sharing provisions of the marriage contract rather than onto generalized changes in social attitudes, which one might expect to be more broadly distributed. Second, the opposite-signed extensive and intensive margins are precisely what a model of marriage as a risk-sharing-cum-responsibility contract predicts, and are not the signature one would expect from a uniform shift in sentiment, which would tend to move risk-taking in a single direction. Taken together, the evidence indicates that entering legal marriage changes

financial behavior through the economic content of the contract, and that the estimated effects reflect a general marriage mechanism identified through variation that happens to be specific to same-sex couples.

## 7. Conclusion

This paper uses Taiwan's 2019 legalization of same-sex marriage to study whether entering legal marriage changes financial behavior. By exploiting a discrete, well-dated shock to access to the marriage contract, and by comparing newly eligible individuals with closely matched opposite-sex married controls who experience the same aggregate environment, the design isolates the institutional content of marriage from the selection that ordinarily confounds comparisons of married and unmarried people. The administrative data make it possible to measure portfolios precisely and, crucially, to separate the decision of whether to hold equity from the decision of how much equity to hold.

The evidence shows that legal marriage affects both who enters risky-asset markets and how much equity risk existing participants hold. Stock market participation rises by about 1.55 percentage points after marriage—roughly five percent relative to the pre-reform participation rate—while equity exposure falls by roughly 2.40 to 2.45 percentage points among individuals already in the market. The event-study paths display no differential pre-trends and responses that build over time, and the effects are concentrated among higher-capacity households and, on the intensive margin, among women. These results point to a nuanced role for marriage in household finance: the same institution can relax entry constraints through risk sharing and resource pooling while inducing more conservative portfolio rebalancing through household-level financial responsibility. A single summary measure of risk-taking would mask this, because the two margins move in opposite directions.

The opposite-signed responses also speak to how we model households. They are consistent with collective rather than unitary models of the family, in which marriage changes the objective that governs financial decisions—from the stability of an individual's resources to the stability of joint resources shared with, and legally claimable by, a partner. In such a framework, marriage simultaneously expands risk-bearing capacity, which encourages entry, and internalizes downside risk to the spouse, which discourages concentration. The fact that both forces appear in the data, on different margins, suggests that they coexist rather than that one dominates, and that the financial footprint of marriage cannot be summarized by a single direction of effect.

The findings also underscore the value of administrative data for household finance. Survey data often lack precise measures of assets, income, and timing, which are exactly what is needed to separate the extensive margin of participation from intensive-margin changes in exposure. By linking household-registration records to income and asset data, this setting makes that separation possible and reveals that the two margins respond to marriage in opposite directions. More broadly, the results suggest that the legal architecture of marriage, and not only the demographic event, shapes how households bear financial risk—a conclusion with implications for understanding portfolio behavior as the prevalence and legal form of partnership continue to evolve, and as a growing share of couples organize their finances outside, or before, the legal institution of marriage.

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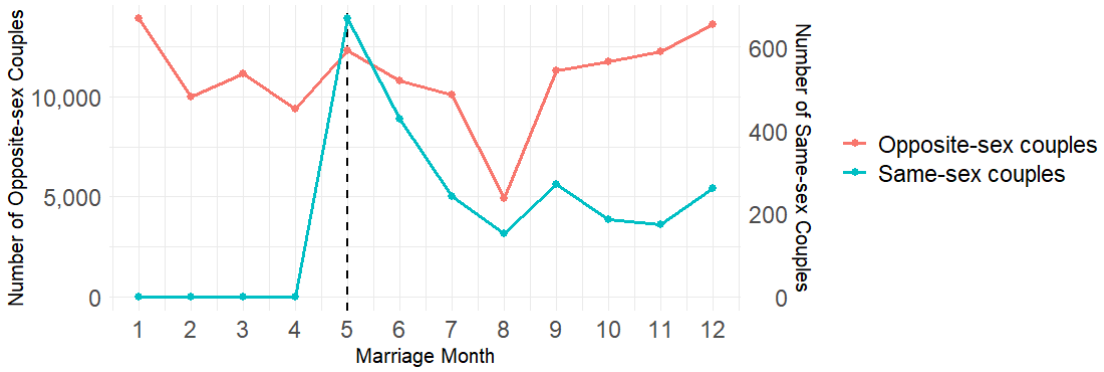



**Figure 1. Number of Married Couples by Month in 2019**

This figure plots monthly marriage registrations in Taiwan in 2019 separately for opposite-sex and same-sex couples. The vertical dashed line marks May 2019, the first month in which same-sex couples could legally marry following the May 24 reform. The figure shows no same-sex registrations before legalization, a sharp first-month spike of 670 same-sex couples (1,335 individuals) in May, and continued registrations thereafter; opposite-sex marriages provide the contemporaneous seasonal benchmark.

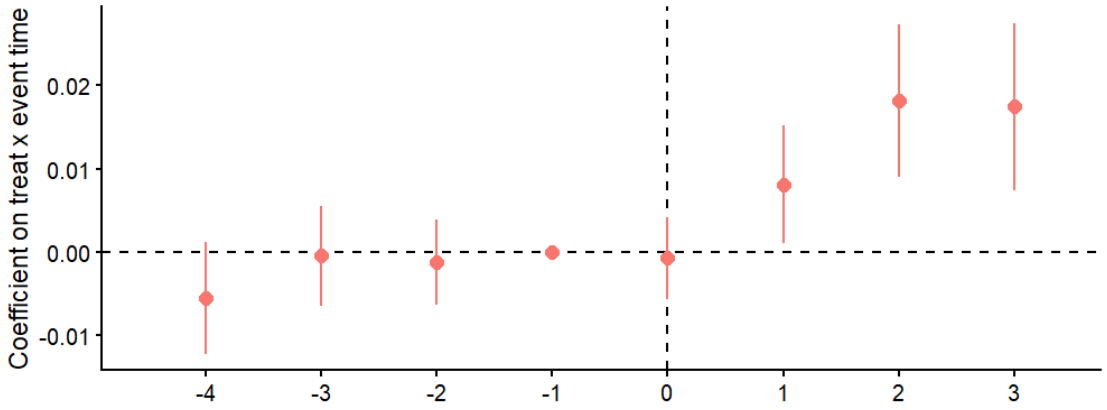



**Figure 2. Dynamic Effects on Stock Market Participation**

This figure reports event-study estimates of the effect of entering legal marriage on stock market participation. Event time is measured relative to 2019, with 2018 omitted as the reference year, and the dependent variable is an indicator for holding positive equity wealth. The plotted coefficients are treatment-by-event-time interactions from equation (2); vertical bars are 95 percent confidence intervals based on individual-clustered standard errors. The pre-reform coefficients are close to zero, while the post-reform coefficients turn positive and increase over the subsequent years.

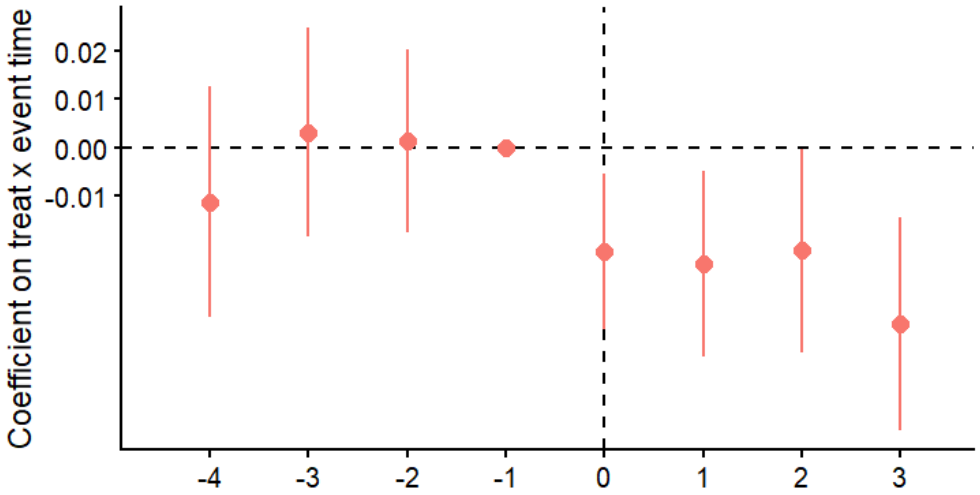



**Figure 3. Dynamic Effects on the Equity-Savings Ratio**

This figure reports event-study estimates for the equity-savings ratio, defined as equity wealth divided by the sum of equity wealth and savings. Event time is measured relative to 2019, with 2018 omitted as the reference year, and the sample is restricted to individuals who participate in the stock market. The plotted coefficients are treatment-by-event-time interactions from equation (2); vertical bars are 95 percent confidence intervals based on individual-clustered standard errors. The estimates show no differential pre-trend and a post-reform decline in equity intensity among individuals who were already participating in the stock market before the shock.

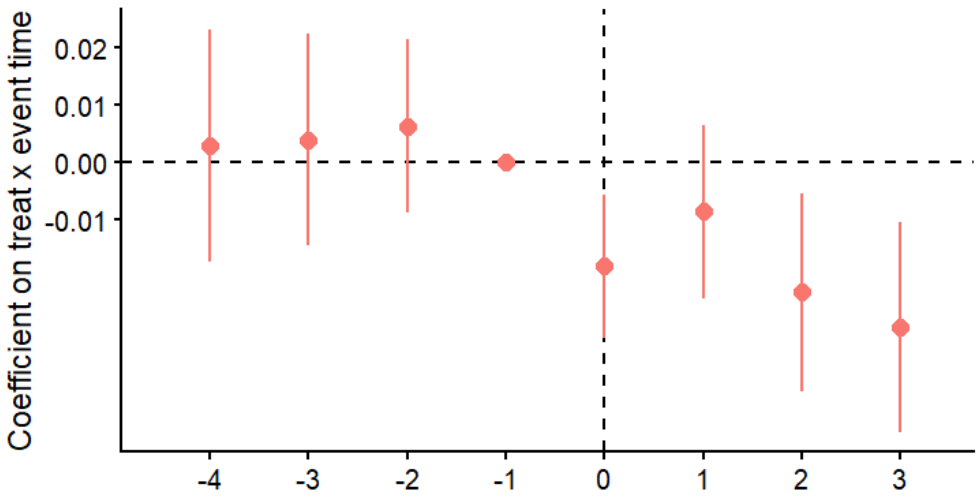



**Figure 4. Dynamic Effects on the Equity-Asset Ratio**

This figure reports event-study estimates for the equity-asset ratio, defined as equity wealth divided by total measured assets, including equity, savings, housing, land, and bonds. Event time is measured relative to 2019, with 2018 omitted as the reference year, and the sample is restricted to individuals who were already participating in the stock market before the shock. The plotted coefficients are treatment-by-event-time interactions from equation (2); vertical bars are 95 percent confidence intervals based on individual-clustered standard errors. The figure shows near-zero pre-reform coefficients and a post-reform decline in the equity share of the full balance sheet.

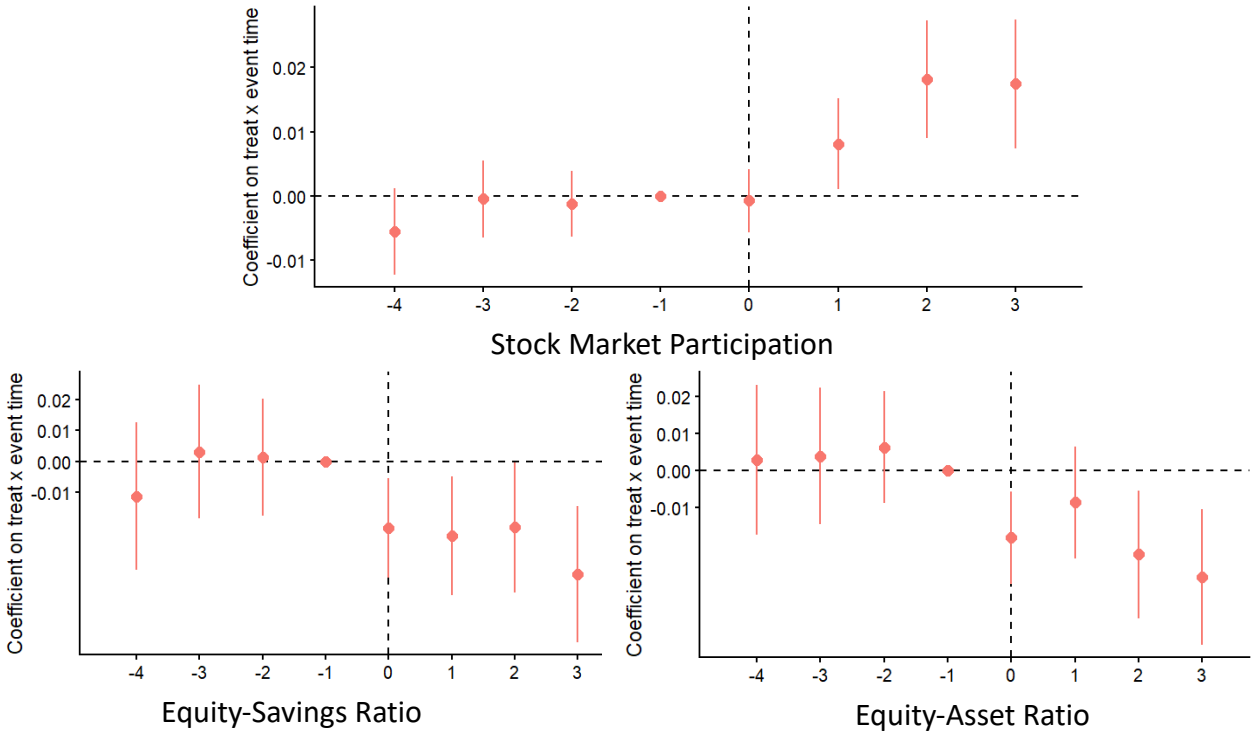



**Figure 5. Dynamic Treatment Effects Across Outcomes**

This figure summarizes the dynamic treatment effects across the three main outcomes: stock market participation, the equity-savings ratio, and the equity-asset ratio. Estimates use the matched one-to-five sample with demographic, birthplace, and income controls and individual-clustered standard errors. The top panel shows that participation rises after legalization, while the lower panels show that both intensive-margin measures decline among individuals who were already participating in the stock market before the shock. The combined pattern highlights the paper's central result: legal marriage raises entry into equity markets while reducing equity concentration among existing investors.

## Table 1
## Predicted Effects of Marriage by Mechanism

This table reports the conceptual predictions for the mechanisms developed in Section 2. The extensive-margin outcome is stock market participation, and the intensive-margin outcome is equity exposure conditional on participation before the marriage. A plus sign denotes a predicted increase after marriage, a minus sign denotes a predicted decrease, and a question mark denotes an ambiguous prediction. The table highlights that risk sharing and resource pooling predict higher participation, while household-level financial responsibility can reduce concentrated equity exposure among existing participants.

| Channel | Stock market participation (extensive margin) | Equity exposure (intensive margin) |
|---|---|---|
| Risk sharing | + | + |
| Income / resource pooling | + | ? |
| Financial responsibility | ? | – |

## Table 2
## Summary Statistics by Treatment Status in the year 2018

This table presents pre-reform summary statistics for treated individuals and matched controls in 2018, the year before Taiwan legalized same-sex marriage. Treated individuals are members of same-sex couples who married in 2019. Controls are drawn from the pool of individuals in opposite-sex marriages before 2017 and are matched on sex, age, and birthplace. For each treated individual, the matching procedure first retains the ten controls closest in pre-reform total assets and then, among those ten, selects the five controls closest in pre-reform wage income. The table reports means and medians for demographic characteristics, stock market participation, asset components, portfolio ratios, wage income, total assets, and sample size. Monetary values are converted to U.S. dollars using an exchange rate of approximately NT$31.5 per U.S. dollar.

| Variable | Treated mean | Treated median | Control mean | Control median |
|---|---|---|---|---|
| Age (years) | 34.38 | 34 | 34.37 | 34 |
| Share male | 0.28 | — | 0.28 | — |
| Stock market participation | 0.244 | 0 | 0.230 | 0 |
| Equity value (USD) | 6,557 | 0 | 6,290 | 0 |
| Savings value (USD) | 22,814 | 0 | 24,419 | 0 |
| Equity-savings ratio | 0.160 | 0 | 0.153 | 0 |
| Equity-asset ratio | 0.09 | 0 | 0.078 | 0 |
| House value (USD) | 5,858 | 0 | 6,840 | 0 |
| Land value (USD) | 36,395 | 0 | 34,755 | 0 |
| Wage income (USD) | 10,201 | 8,413 | 13,009 | 8,643 |
| Total gross assets (USD) | 86,870 | 1,165 | 90,129 | 1,542 |
| **Observations (N)** | **5,387** | | **26,908** | |

## Table 3
## Baseline Difference-in-Differences: Stock Market Participation

This table reports baseline difference-in-differences estimates for stock market participation. The dependent variable is an indicator equal to one if the individual holds positive equity wealth in a given year. $T_i$ equals one for treated individuals in same-sex couples who married in 2019, and $Post_t$ equals one for years 2019 and later. The coefficient on $T_i \times Post_t$ is the difference-in-differences estimate corresponding to $\beta$ in section 4. Columns differ in the procedure used to form the matched control group. Columns (1), (3), (4), and (5) use the 10a5w procedure, which first selects the 10 individuals with total assets closest to those of the treated individual and then retains the 5 among them with wage income closest to the treated individual. Column (2) uses the 10w5a procedure, which first selects the 10 individuals with wage income closest to that of the treated individual and then retains the 5 among them with total assets closest to the treated individual. All columns include age, a quadratic term in age, and a sex indicator; birthplace, total income in the year before the shock, and marriage-stage controls are added sequentially across columns. Standard errors, clustered at the individual level, are in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

| | **Stock Market Participation** | | | | |
|---|---|---|---|---|---|
| **Variable** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| $T_i$ | -0.0144*** | -0.0108** | -0.0144*** | -0.0248*** | -0.0502*** |
| | (0.0055) | (0.0055) | (0.0055) | (0.0056) | (0.0066) |
| **$T_i \times Post_t$** | **0.0153*** ** | **0.0155*** ** | **0.0153*** ** | **0.0155*** ** | **0.0188*** ** |
| | **(0.0040)** | **(0.0040)** | **(0.0040)** | **(0.0040)** | **(0.0052)** |
| Control group formation | 10a5w | 10w5a | 10a5w | 10a5w | 10a5w |
| Birthplace controls | No | No | Yes | Yes | Yes |
| Total income in year -1 | No | No | No | Yes | Yes |
| Marriage-stage controls | No | No | No | No | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes |
| Observations | 257,647 | 257,636 | 257,647 | 257,647 | 257,613 |
| Adjusted $R^2$ | 0.10980 | 0.11943 | 0.11686 | 0.13522 | 0.13598 |

## Table 4
## Baseline Difference-in-Differences: Equity-Savings Ratio

This table reports baseline difference-in-differences estimates for the equity-savings ratio among individuals who were already participating in the stock market before marriage. The dependent variable is equity wealth divided by the sum of equity wealth and savings. $T_i$ equals one for treated individuals in same-sex couples who married in 2019, and $Post_t$ equals one for years 2019 and later. The coefficient on $T_i \times Post_t$ is the difference-in-differences estimate corresponding to $\beta$ in section 4. Columns differ in the procedure used to form the matched control group. Columns (1), (3), (4), and (5) use the 10a5w procedure, which first selects the 10 individuals with total assets closest to those of the treated individual and then retains the 5 among them with wage income closest to the treated individual. Column (2) uses the 10w5a procedure, which first selects the 10 individuals with wage income closest to that of the treated individual and then retains the 5 among them with total assets closest to the treated individual. All columns include age, a quadratic term in age, and a sex indicator; birthplace, total income in the year before the shock, and marriage-stage controls are added sequentially across columns. Standard errors, clustered at the individual level, are in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

| | **Equity-Savings Ratio** | | | | |
|---|---|---|---|---|---|
| **Variable** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| $T_i$ | 0.0196*<br>(0.0112) | 0.0059<br>(0.0112) | 0.0195*<br>(0.0112) | 0.0299***<br>(0.0112) | 0.1555***<br>(0.0202) |
| **$T_i \times Post_t$** | **-0.0251**<br>(0.0104)** | **-0.0226**<br>(0.0104)** | **-0.0251**<br>(0.0104)** | **-0.0251**<br>(0.0104)** | **-0.0964***<br>(0.0183)** |
| Control group formation | 10a5w | 10w5a | 10a5w | 10a5w | 10a5w |
| Birthplace controls | No | No | Yes | Yes | Yes |
| Total income in year -1 | No | No | No | Yes | Yes |
| Marriage-stage controls | No | No | No | No | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes |
| Observations | 59,174 | 59,152 | 59,174 | 59,174 | 59,174 |
| Adjusted $R^2$ | 0.01529 | 0.01744 | 0.01859 | 0.03018 | 0.03327 |

## Table 5
## Baseline Difference-in-Differences: Equity-Asset Ratio

This table reports baseline difference-in-differences estimates for the equity-asset ratio among individuals who were already participating in the stock market before marriage. The dependent variable is equity wealth divided by total measured assets, defined as the sum of equity, savings, housing, land, and other liquid asset. $T_i$ equals one for treated individuals in same-sex couples who married in 2019, and $Post_t$ equals one for years 2019 and later. The coefficient on $T_i \times Post_t$ is the difference-in-differences estimate corresponding to $\beta$ in section 4. Columns differ in the procedure used to form the matched control group. Columns (1), (3), (4), and (5) use the 10a5w procedure, which first selects the 10 individuals with total assets closest to those of the treated individual and then retains the 5 among them with wage income closest to the treated individual. Column (2) uses the 10w5a procedure, which first selects the 10 individuals with wage income closest to that of the treated individual and then retains the 5 among them with total assets closest to the treated individual. All columns include age, a quadratic term in age, and a sex indicator; birthplace, total income in the year before the shock, and marriage-stage controls are added sequentially across columns. Standard errors, clustered at the individual level, are in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

| | **Equity-Asset Ratio** | | | | |
|---|---|---|---|---|---|
| **Variable** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| $T_i$ | -0.0081<br>(0.0101) | -0.0199**<br>(0.0100) | -0.0081<br>(0.0100) | -0.0001<br>(0.0100) | 0.0945***<br>(0.0185) |
| **$T_i \times Post_t$** | **-0.0238***<br>(0.0083)** | **-0.0247***<br>(0.0083)** | **-0.0238***<br>(0.0083)** | **-0.0238***<br>(0.0083)** | **-0.1100***<br>(0.0157)** |
| Control group formation | 10a5w | 10w5a | 10a5w | 10a5w | 10a5w |
| Birthplace controls | No | No | Yes | Yes | Yes |
| Total income in year -1 | No | No | No | Yes | Yes |
| Marriage-stage controls | No | No | No | No | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes |
| Observations | 59,174 | 59,152 | 59,174 | 59,174 | 59,174 |
| Adjusted $R^2$ | 0.05035 | 0.05100 | 0.05575 | 0.06439 | 0.06575 |

## Table 6
## Heterogeneity by Gender, Asset Level, and Wage Level

This table reports heterogeneity in the baseline difference-in-differences estimates by gender, asset level, and wage level. Each cell reports the coefficient on the treated-by-post interaction from a separate subsample regression that includes time fixed effects and controls for a quadratic in age, sex, and birthplace. Standard errors are clustered at the individual level. Asset and wage groups are defined by splitting the sample at the median. The outcomes are stock market participation, the equity-savings ratio, and the equity-asset ratio. Standard errors, clustered at the individual level, are in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. The results show that, by gender, stock market entry increases for both men and women, while the intensive-margin decline is concentrated among women. By contrast, when heterogeneity is examined by asset and wage levels, the estimated effects are stronger among households with greater financial capacity.

| | **Subsample** | | | | | |
|---|---|---|---|---|---|---|
| | **Gender** | | **Asset Level** | | **Wage Level** | |
| **Outcome / statistic** | **Male** | **Female** | **Above-med. asset** | **Below-med. asset** | **Above-med. wage** | **Below-med. wage** |
| Stock participation | 0.0196*** | 0.0135*** | 0.0191*** | 0.0117** | 0.0240*** | 0.0065 |
| | (0.0070) | (0.0048) | (0.0062) | (0.0049) | (0.0061) | (0.0050) |
| Observations | 72,660 | 184,987 | 128,767 | 128,478 | 128,880 | 128,367 |
| Equity-savings ratio | -0.0107 | -0.0316** | -0.0243** | -0.0330 | -0.0291** | -0.0187 |
| | (0.0178) | (0.0128) | (0.0107) | (0.0378) | (0.0134) | (0.0165) |
| Observations | 18,311 | 40,863 | 52,738 | 6,388 | 36,236 | 22,938 |
| Equity-asset ratio | 0.0038 | -0.0362*** | -0.0229*** | -0.0295 | -0.0228** | -0.0253* |
| | (0.0138) | (0.0103) | (0.0080) | (0.0387) | (0.0108) | (0.0131) |
| Observations | 18,311 | 40,863 | 52,738 | 6,388 | 36,236 | 22,938 |