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Cats, Dogs, and Babies: Quasi-Experimental Evidence on Substitutes or Complements from Linked Administrative Records

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Cats, Dogs, and Babies:

Quasi-Experimental Evidence on Substitutes or Complements from Linked Administrative Records

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Abstract

Falling fertility is one of the most pressing demographic and economic challenges, particularly in East Asia. Among the many popular explanations, pets are often portrayed as substitutes for children. By linking Taiwan’s universe of pet registry records with administrative tax files, we construct a ten-year panel covering 23 million individuals and millions of registered cats and dogs and provide the first large-scale empirical analysis examining the relationship between pet ownership and childbearing.

We first document descriptive patterns that both dog and cat ownership are rising rapidly while births decline, and in the cross-section dog ownership is positively correlated with having children—even after controlling for family background, income, assets, and home ownership—while cat ownership is negatively correlated. Second, we estimate cross-price elasticities of demand. Exploiting the staggered rollout of childbirth bonuses, we find that new pet acquisitions increase significantly in the year after a bonus is introduced. Combining this response with income elasticities we estimate from Taiwan’s receipt lottery and with measured expenditure shares through Slutsky symmetry, we recover the policy-relevant elasticity of child demand with respect to the cost of pets is -0.20 . This indicates that children and pets are net complements. Third, event studies show that timing and life stage matter: childbearing rises markedly after pet adoption—concentrated among those without prior children—consistent with a “starter family” mechanism, yet childbirth is followed by fewer pet acquisitions. Pet acquisition also rises after later-life events such as divorce or the death of a spouse or child.

Keywords: Fertility, Pet Ownership, Pro-natalist Policies

1 Introduction

Falling fertility rates have emerged as one of the defining demographic and economic challenges of the twenty-first century. Fertility has declined across much of the developed world, dropping from an average of 3.3 children per woman in 1960 to about 1.5 in 2022 among OECD countries. This decline has pushed fertility well below the replacement level of approximately 2.1 (OECD, 2024*b*). Across East Asia, the drop is even more dramatic: virtually every country has seen fertility fall to “ultra-low” levels, with South Korea and Taiwan among the lowest globally. A vast literature has examined the causes of this decline, emphasizing factors such as housing costs, labor market dynamics, shifting gender norms, and the rising opportunity cost of childbearing (Kim, Tertilt and Yum, 2024; Myong, Park and Yi, 2021; Doepke et al., 2023). Research on the determinants of fertility has grown rapidly, yet important features of contemporary households still sit outside its scope.

One salient and increasingly widespread feature of modern households is pet ownership. Pets are not only common but are often treated as family members, shaping daily routines, financial choices, and long-term planning: in the United States, 97% of pet owners say their pets are part of their family, and about half say they are as much a part of it as a human member (Pew Research Center, 2023). A recent survey shows that about 28.3% of households in Taiwan owned a pet in 2023: 13.8% had dogs and 10.4% had cats (Wu, 2024). Public commentators have speculated that pets may act as substitutes for children, delaying or reducing fertility. For instance, Pope Francis warned that “dogs and cats take the place of children” and argued this trend may “diminish us” and “take away our humanity” (Francis, 2022). Meanwhile, J. D. Vance remarked that “we are effectively run ... by a bunch of childless cat ladies ... miserable at their own lives ... so they want to make the rest of the country miserable too” (Vance, 2021). Nor has the idea stayed in the realm of commentary: it has entered actual policy debate. Campaigning for Taiwan’s presidency in 2023, Terry Gou proposed folding pet care into his pro-natalist platform—one subsidized pet for every child born, and a pair for two children—explicitly treating pets and children as jointly subsidizable family members (Central News Agency, CNA). Despite this prominence in public discourse and

policy proposals alike, systematic empirical evidence is virtually absent. The relationship between pets and children remains unsettled, influenced by anecdote, judgment, and cultural bias.

To the best of our knowledge, this paper provides the first large-scale empirical evidence on whether pets and children are substitutes or complements. Using newly linked administrative data from Taiwan, we combine the universe of pet registry records with individual-level tax files to construct a ten-year panel covering 23 million individuals and millions of registered cats and dogs. These data allow us to observe pet ownership, fertility, and socioeconomic outcomes in detail.

Our analysis proceeds in three steps. First, we document descriptive patterns. In the cross-section, the correlation between pets and children differs sharply by species: individuals with dogs are more likely to have children, individuals with cats less likely, and neither association is explained by controls for income, assets, home ownership, or demographics. The two decisions are also staggered over the life cycle: pet acquisition is most common in the twenties and early thirties, while childbirth peaks several years later (Appendix A).

Second, we move beyond correlation to estimate cross-price elasticities of demand. We exploit the staggered rollout of Taiwan’s newborn child bonus, which provided a lump-sum transfer per newborn, as an exogenous reduction in the cost of childbearing. Immediately following the introduction of bonuses, the annual rate of new dog acquisitions among individuals of childbearing age rises by roughly 20% relative to its pre-bonus level. From this, we estimate a Marshallian cross-price elasticity of dog demand with respect to the cost of children of $\epsilon_{\text{dog,child}}^M = -1.24$, suggesting that children and pets are gross complements.

To disentangle the price response from income effects, we use Taiwan’s unique receipt lottery, which randomly awards cash prizes to consumers based on purchase receipts and thereby generates quasi-random variation in disposable income; from it we estimate the income elasticity of dog ownership to be 0.11. Combining our bonus- and lottery-based estimates with measured expenditure shares through Slutsky symmetry, we recover the elasticity that matters for policy—the response of child demand to the cost of pets: $\epsilon_{\text{child,dog}}^M = -0.20$, negative and statistically distinguishable from zero, and robust across a series of alternative specifications. The corresponding Hicksian elasticity

$\epsilon_{\text{dog,child}}^H = -1.22$ suggests that pets and newborn children are net complements once income effects are accounted for. Cats display the same complementarity: cat acquisitions also rise when the cost of children falls, with a proportional response roughly half as large as for dogs.

Third, we investigate mechanisms. A popular view is that pets, especially dogs, serve as “starter family.” Our evidence supports this hypothesis. Using an event-study framework, we show that the timing is essential: the probability of childbearing rises after households adopt a dog, relative to households that adopt later. Because dog adoption is itself a choice, we read these event studies as descriptive of timing rather than as causal magnitudes. In the other direction, childbirth is followed by a marked decline in subsequent dog adoption. Heterogeneity analyses further reinforce the learning mechanism: the fertility-enhancing effect of dog ownership is concentrated among individuals without prior children, while for those who already have children, dog adoption is associated with fewer subsequent births. Moreover, dogs exert stronger effects than cats, consistent with the idea that raising dogs more closely mirrors child-rearing.

Finally, we emphasize that our results concern the arrival of a newborn, and that the relationship between pets and family members varies across life stages. Around the birth of a grandchild, dog acquisition moves in the same direction as around one’s own newborn, at roughly a fifth of the magnitude. Around three later-life events the sign reverses: pet acquisition rises after a divorce, after the death of a spouse, and after the death of an adult child. Read together, these patterns indicate that the complementarity we document is specific to the family-formation stage—pets enter households alongside prospective children—whereas in later life, after a divorce or the loss of a spouse or an adult child, pets act instead as substitutes for lost human companionship.

Our paper connects to research in sociology, psychology, and public health that examines how pets fit into family life. Sociologists emphasize that companion animals often function as family members and may even substitute for human companionship (Veevers, 2016; Charles and Davies, 2008). Psychologists highlight the diverse ways people relate pets to childrearing, with mixed evidence on whether attachment to pets discourages or complements fertility intentions (Gillet and Kubinyi, 2025; Guo et al., 2021). Public health studies further document that pets can improve

mental health and children’s socioemotional development (Marsa-Sambola et al., 2017; Zilcha-Mano, Mikulincer and Shaver, 2012). Together, these fields underscore the emotional and relational importance of pets but have rarely linked it to fertility behavior.

The study most closely related to ours is Schwarz, Troyer and Walker (2007), who use over twenty years of cross-sectional U.S. consumer expenditure data and conclude that pets and children are substitutes: households with young children are less likely to own pets and spend less on them. Our analysis extends this literature in several ways. We exploit rich administrative panel data from Taiwan, cover a broader time horizon, and apply quasi-experimental variation from policy reforms rather than cross-sectional comparisons. Chatterjee and Butler (1997) likewise treat pet ownership as a household choice, modeling the joint ownership of cats and dogs and finding that household size and composition are first-order determinants of both.

Beyond the pet literature, our study contributes to the economics of fertility. Existing research has emphasized the role of structural, cultural, and institutional determinants, particularly in Asia, where fertility decline has been most severe (Myong, Park and Yi, 2021; Kim, Tertilt and Yum, 2024). We complement this work by introducing pet ownership as a novel and previously overlooked dimension of household decision-making that interacts with fertility choices.

We also add to the evaluation of pro-natalist policies. Prior work has established that fertility bonuses, tax incentives, and transfer programs can affect birth timing and completed fertility (Milligan, 2005; González, 2013; González and Trommlerová, 2023). Our findings demonstrate that such policies can also generate spillovers to other family decisions, in this case influencing pet acquisition. This highlights the broader reach of pro-natalist measures beyond their intended target.

Finally, we expand the scope of family economics. Traditional models of the household focus on marriage, fertility, and intra-household allocation but largely omit companion animals. By explicitly incorporating pets into the analysis, we recognize their central role in modern families and show how decisions about children and pets are jointly determined. This perspective encourages a rethinking of the boundaries of family economics and calls attention to the growing importance of non-human family members in shaping household utility.

The remainder of the paper is organized as follows. Section 2 describes the data and sample. Section 3 presents descriptive evidence. Section 4 estimates cross-price elasticities of demand. Section 5 investigates mechanisms through dynamics and heterogeneity. Section 6 concludes.

2 Data

2.1 Pet Registry

Due to potential risks of rabies, the Taiwanese government has required all individuals to register their dogs since September 1, 1999 (Ministry of Justice, 1998; Council of Agriculture, Executive Yuan, 1999). Any individual age 20 or older (lowered to 18 after 2020) is eligible to register. The cost of registration is typically subsidized by local governments—in Taipei City, registration and chip insertion together cost NTD250 (USD 8.48)—and, importantly for the quality of the registry, compliance is largely incentive-compatible rather than enforced through fines alone. Registration comes bundled with services owners value: the implanted microchip lets anyone who finds a stray animal trace it back to its owner; governments routinely subsidize chip insertion and rabies vaccination, often delivered together with on-site registration (Animal and Plant Health Inspection Agency, Ministry of Agriculture, 2026); and the widely used dog and cat neutering subsidies are conditional on completed pet registration and a current rabies vaccination (Taipei City Government, 2025). Cat registration was phased in later and varies by county (Taipei City, for example, has required it since July 1, 2016), becoming mandatory nationwide only from August 2025, outside our analysis period.¹

The official survey estimates that the registration coverage rate is approximately 70.6% for dogs and 58.4% for cats as of 2023 (Ministry of Agriculture, Taiwan, 2024). Our pet registry data span from 1999 to 2020 and include variables on the registration date, species, and anonymized owner ID. When ownership changes, the registry also records the new owner, allowing us to track pet transfers

¹ Appendix C.8 presents results using a sample restricted to the post-2016 period in Taipei City. The findings suggest that these rule changes do not drive our main results.

across households. However, the registry does not reliably record whether a pet has passed away, which may introduce some measurement error in identifying active pet ownership.

Throughout the paper we study dogs and cats only. A survey of more than four thousand Taiwanese adults that we conducted in 2026, used throughout the paper as complementary descriptive evidence (Appendix G), indicates that this covers nearly all pet-keeping in Taiwan: among current pet owners who report species, 91.1% keep a dog or a cat, and only 8.9% keep exclusively other animals—predominantly turtles, rabbits, and fish, whose care demands little of the time and caregiving involvement central to the mechanisms we study.

2.2 Fiscal Information Agency (FIA) Data

Our primary individual-level data come from the administrative tax and registry records maintained by Taiwan’s Fiscal Information Agency (FIA). The dataset consists of two main components: a family and household information section, and an income and asset section.

The family and household information section provides unique individual identifiers, parental identifiers (mother and father), spouse identifiers, and exact birthdates. This structure allows us to reconstruct family trees, identify household composition, and link individuals across generations. The data include all individuals with household registration in Taiwan, regardless of tax filing status or taxable income, thereby covering the entire resident population.

The income and asset section records detailed information on income and assets. Each income and asset entry is available at the source level, allowing us to distinguish between different income types (such as wage, business, and capital income) and asset categories (such as deposits, property, and securities). The data also include geographic identifiers such as county and postal code of registration, which enable regional analyses.

We combine the pet registry and the FIA data using the unique individual identifier, matching each registered pet exactly to its owner’s demographic, family, and economic information, and construct an annual panel dataset spanning 2009 to 2020. The resulting linked panel serves as the foundation for our analysis of the relationship between pet ownership and fertility behavior.

3 Descriptives

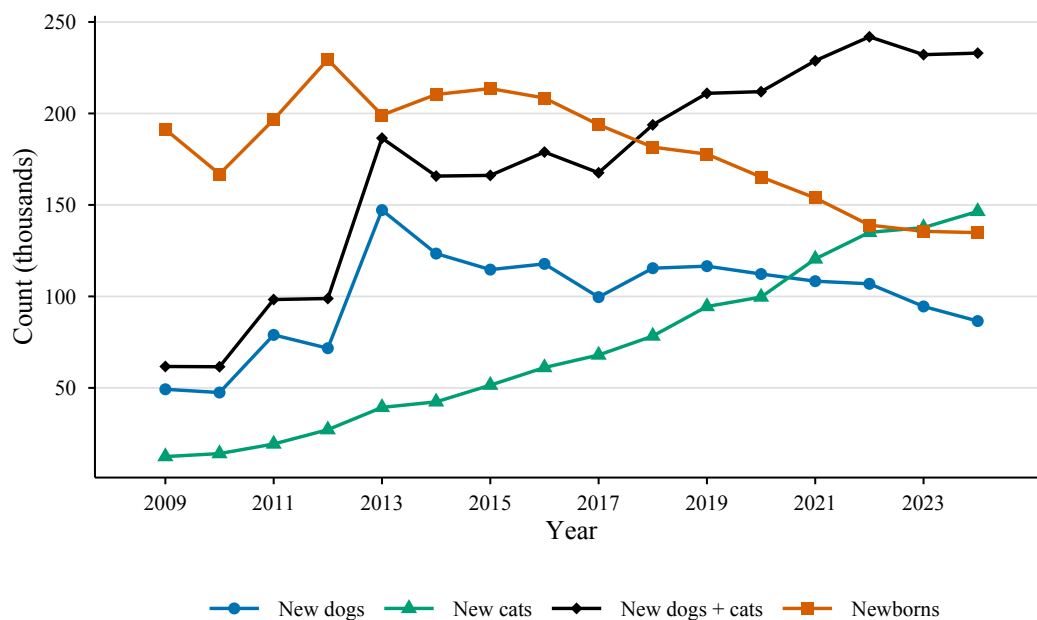


Figure 1: Trends for Registered New Cats, Dogs, and Babies.

Note: The figure displays the number of newly registered dogs and cats, their sum, and the official count of registered live births in Taiwan from 2009 to 2024. Pet counts are national annual totals from the Ministry of Agriculture’s pet registration system (Ministry of Agriculture, Taiwan, 2026); birth counts are from the Ministry of the Interior (Ministry of the Interior, Taiwan, 2026). Notable fluctuations in births appear in 2010 (Year of the Tiger) and 2012 (Year of the Dragon) under the lunar calendar, both of which are well known to influence fertility decisions in Taiwanese and Chinese culture. The sharp rise in pet registrations in late 2013 is likely attributable to a free registration campaign held from September to October 2013 (Council of Agriculture, Taiwan, 2013). As shown in Appendix C.1, our results are robust to excluding these observations. Our individual-level data end in 2020; the later years of the figure therefore describe the aggregate environment rather than our estimation sample.

3.1 Summary Statistics

We begin by illustrating the broad pattern of pet registration over time, as shown in Figure 1. The figure is built from published national aggregates—the Ministry of Agriculture’s registration totals and the Ministry of the Interior’s birth counts—which extend through 2024; our individual-level registry extract ends in 2020, so the later years of the figure describe the aggregate environment rather than our estimation sample. The flow of newly registered pets in Taiwan has grown steadily over this period, while the number of children born each year has gradually declined. Throughout,

“newly registered” dogs and cats refers to animals entering the registry in a given year; a registration may also reflect a transfer of ownership rather than a new acquisition. Because of these opposing trends, the flow of newly registered pets came to exceed the number of newborn children in 2018, and by 2024 new cat registrations alone exceeded registered births. These opposing aggregate trends do not by themselves speak to whether pets and children are substitutes or complements. Both series respond to forces other than each other’s price: rising incomes, since pets are a normal good, and a shrinking cohort at family-formation ages alongside a growing number of older and single-person households, for whom pets serve as companions rather than as a prelude to children (Section 5.4). Section 4 instead isolates the response of one margin to an exogenous change in the cost of the other, holding these forces fixed.

To better understand the individuals behind these trends, we next examine basic summary statistics comparing pet owners with non-owners matched on sex and age. Table 1 provides a side-by-side view of key characteristics, including gender, age, marital status, having children, household size, annual income, home ownership, and metropolitan residence. Three patterns stand out. Dog owners are more likely to own a house than their matched non-owners (0.49 versus 0.45), and the same holds for cat owners (0.36 versus 0.32), consistent with pet-friendly housing being easier to secure for owners than for renters. By contrast, dog owners and their matched comparisons are about equally likely to have children (0.67 versus 0.66)—the raw child margin scarcely differs once sex and age are matched—whereas cat owners are somewhat *less* likely to have children than their comparisons (0.45 versus 0.48). Pet owners of both species also have slightly smaller households and lower incomes than their matched counterparts.

3.2 Cross-Sectional Correlation

While these descriptive patterns provide useful context, they do not by themselves clarify how pet ownership relates to childbearing once we account for other household and individual characteristics. To address this, we turn to cross-sectional regressions: we regress pet ownership on a

Table 1: Summary Statistics: Pet Owners and Matched Non-Owners

Variable	With Dog	Without Dog	With Cat	Without Cat
Men	0.43	0.43	0.37	0.37
Age	48.66	48.66	39.03	39.03
Married	0.57	0.58	0.42	0.45
With Child	0.67	0.66	0.45	0.48
Household Size	3.66	3.87	3.75	3.98
Income (1000 NTD)	532	581	489	512
House Owner	0.49	0.45	0.36	0.32
City Resident	0.71	0.70	0.73	0.70
N	1,068,973	1,068,973	336,999	336,999

Note: For each individual owning a dog (cat), we randomly draw one individual of the same sex and age without a dog (cat) for comparison. “Men,” “Married,” “With Child,” “House,” and “City” indicate the means of binary variables for being male, being married, having at least one child, owning a house, and residing in one of the six major cities in Taiwan, respectively. Income is the annual total income measured in thousands of NTD. Owners and non-owners are exactly matched on sex and age, which is why the “Men” and “Age” rows coincide by construction.

dummy variable indicating whether an individual has children, controlling for economic and demographic characteristics. Table 2 reports the estimates for dogs and cats side by side, with the control sets indicated by checkmarks; the full coefficient tables underlying each column are available on request.

Several findings stand out when we compare these results to the descriptive patterns. First, we find a positive correlation between dog ownership and having children: having at least one child is associated with a 2.57 percentage point higher probability of owning a dog, against a baseline ownership rate of about 3 percent. Second, this association attenuates only modestly, to 2.07 percentage points, when we add controls for income, assets, and other economic conditions, indicating that economic status alone does not drive the pattern. Third, adding demographic characteristics such as age, gender, marital status, and household size cuts the estimate to 0.92 percentage points, though it remains positive and precisely estimated. Demographic composition therefore explains much of the raw association, but not all of it. Finally, cat ownership carries the opposite sign: having a child is associated with a 0.42 percentage point *lower* probability of owning a cat, and unlike

the dog estimate this coefficient is stable across specifications, moving only between -0.40 and -0.53 percentage points as controls are added.

Beyond the cross-section, the age profiles of the two decisions are staggered: pet ownership is most prevalent among individuals in their twenties and early thirties, while childbearing peaks several years later. Appendix A presents the age profile of pet ownership by parental status; we caution there that a single-year cross-section mixes birth cohorts at each age, so we read it as context for the timing patterns studied formally in Section 5 rather than as evidence in its own right.

Taken on their own, these descriptive patterns do not reveal whether pets and children are complements or substitutes, but they suggest the potential importance of unobserved characteristics that jointly influence both pet ownership and childbearing.

Table 2: Correlation Between Having a Child and Pet Ownership, by Species

	Has Dog (pp)				Has Cat (pp)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	3.040*** (0.006)	2.070*** (0.007)	3.470*** (0.010)	2.670*** (0.010)	1.610*** (0.003)	1.010*** (0.004)	2.350*** (0.007)	1.690*** (0.008)
Has Child	2.570*** (0.008)	2.070*** (0.009)	0.920*** (0.010)	0.900*** (0.010)	-0.420*** (0.005)	-0.400*** (0.005)	-0.530*** (0.008)	-0.460*** (0.008)
Economic Controls		✓		✓		✓		✓
Demographic Controls			✓	✓			✓	✓
R ²	0.0039	0.0068	0.0057	0.0082	0.0003	0.0034	0.0017	0.0047

Note: The dependent variable is an indicator for owning a dog (columns 1–4) or a cat (columns 5–8); coefficients are expressed in percentage points. Economic controls: asset holdings, stock holdings, and income, each measured per NTD million, home ownership, and work status. Demographic controls: age, gender, marital status, household size, and the age–gender interaction. “Has Child” indicates whether the individual has at least one child. Observations: 24 million individuals. Standard errors, assuming independent and identically distributed errors, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Cross-Price Elasticities of Demand

4.1 Theoretical Framework

The descriptive analysis in the previous section is informative and provides a picture of the correlational patterns. However, these patterns alone do not establish whether children and pets are substitutes or complements. Following basic economic theory, we define substitution and complementarity through the cross-price elasticity of demand. In our context, there are two relevant cross-price elasticities. One approach is to consider how changes in the cost of having a child affect the demand for pets. Alternatively, we can examine how changes in the price of pets influence the demand for children.

These two elasticities are generally different. Writing x_j and p_j for the quantity and price of good j , the two gross (Marshallian) elasticities are

$$\epsilon_{\text{pet,child}}^M = \frac{\partial \ln x_{\text{pet}}}{\partial \ln p_{\text{child}}}, \quad \epsilon_{\text{child,pet}}^M = \frac{\partial \ln x_{\text{child}}}{\partial \ln p_{\text{pet}}},$$

where the first subscript indexes the responding good and the second the good whose price changes. By the property of Slutsky symmetry, we have the following expression:

$$s_{\text{pet}} \epsilon_{\text{pet,child}}^M = s_{\text{child}} \epsilon_{\text{child,pet}}^M + s_{\text{pet}} s_{\text{child}} (\eta_{\text{child}} - \eta_{\text{pet}}), \quad (1)$$

where s and η denote expenditure shares and income elasticities.²

This identity highlights that the asymmetry in Marshallian cross-price elasticities is not arbitrary but follows directly from differences in expenditure shares and income elasticities. When two goods differ in how much households spend on them or how sensitive they are to income, the observed responses to price changes will naturally be asymmetric. The identity formalizes this by showing that the difference between the two cross-price elasticities, once weighted by budget shares, is entirely explained by the income elasticity gap.

²The identity follows from symmetry of the Slutsky substitution terms, $s_i(\epsilon_{ij}^M + s_j \eta_i) = s_j(\epsilon_{ji}^M + s_i \eta_j)$.

Applied to our setting, childbearing accounts for a substantially larger share of household expenditure than pet ownership, and its income elasticity is somewhat higher: 0.15, adopted from the lottery-fertility estimates of Tsai et al. (2022), against 0.11 for dog ownership, which we estimate from the same lottery design in Section 4.3. This structure implies that changes in the cost of children can have a noticeable impact on pet ownership decisions, both through substitution and through the induced change in real income. In contrast, changes in the cost of owning a pet are less likely to influence fertility behavior, as they represent a smaller share of the household budget and generate a smaller shift in effective purchasing power. The identity in Equation 1 thus clarifies why we expect the elasticity of pet ownership with respect to the cost of children to be larger than the reverse, even if both are jointly determined.

These two quantities provide a closely related but very different interpretation. Arguably, the more policy-relevant parameter is $\epsilon_{\text{child,pet}}^M$, as policy proposals have explicitly targeted the pet margin to stimulate childbearing—most prominently Terry Gou’s campaign pledge of a subsidized pet per newborn, discussed in the Introduction.³ Our plan is to estimate all the other quantities in the above Equation 1. We will use the child bonus as our main source of variation in identifying $\epsilon_{\text{pet,child}}^M$ (Section 4.2). We then use the receipt lottery to identify the income elasticities (Section 4.3). Finally, we use the expenditure surveys to obtain the expenditure share and apply Equation 1 to derive $\epsilon_{\text{child,pet}}^M$ from the remaining parameters (Section 4.4).

4.2 Identification of the Cross-Price Elasticity

The key challenge in identifying the parameters lies in finding exogenous variation in the cost of having a child. The descriptive analysis in the previous section can be interpreted as showing how certain factors affect this cost. For example, home ownership may lower the barriers to having a child. However, owning a home may also reduce the cost of having a pet. Even after controlling for the rich set of observables in the tax data, unobserved confounders could still influence both the

³See Helen Davidson, “Taiwan presidential hopeful offers pets for pregnancies in push to boost birth rate,” *The Guardian*, September 5, 2023, for international coverage of the proposal (Central News Agency, CNA).

cost of childbearing and other family-related choices.

The ideal research design would be an experiment in which bonuses for childbearing are randomly assigned, allowing for a direct comparison between those whose costs are reduced and those whose costs are not.

Although we do not observe such an experiment, we exploit multiple quasi-experimental policy changes to estimate how exogenous shifts in the cost of children affect pet ownership. In particular, we use the introduction of childbirth bonuses. We also draw on expansions of child tax deductions, which, despite more complex eligibility rules, effectively reduce the cost of raising a child. We present the results from this latter source of variation in Appendix C.3. The findings from both approaches are consistent.

Our main identification strategy is exploiting the one-shot child bonus offered by the local governments. Local governments across Taiwan provide one-time childbirth bonuses as part of broader efforts to alleviate the immediate financial burden of having a child and to address persistently low fertility rates. These lump-sum payments are typically disbursed upon a child's household registration, and are of the order of tens of thousands of NTD per birth. Eligibility criteria typically include a residency requirement, often requiring one or both parents to have lived in the city for at least one year, along with timely completion of registration.

The variation we exploit is the timing of these programs. The years in which counties introduce their bonus differ substantially, and we treat that introduction as a binary event. Figure 2 presents the rollout scheme. This staggered rollout provides a quasi-experimental setting in which the timing of local policy changes can be interpreted as a plausibly exogenous shock to the effective cost of childbearing.

From a theoretical standpoint, a one-time, lump-sum childbirth bonus directly reduces the upfront or “gross” price of having a child at the time the decision is made, without affecting ongoing childrearing costs. Unlike other commonly used sources of variation in the cost of children, such as changes in tax policy, housing prices, or childcare availability, this policy offers a clearly defined and measurable reduction in the monetary cost of childbearing. Because the payment is deliv-

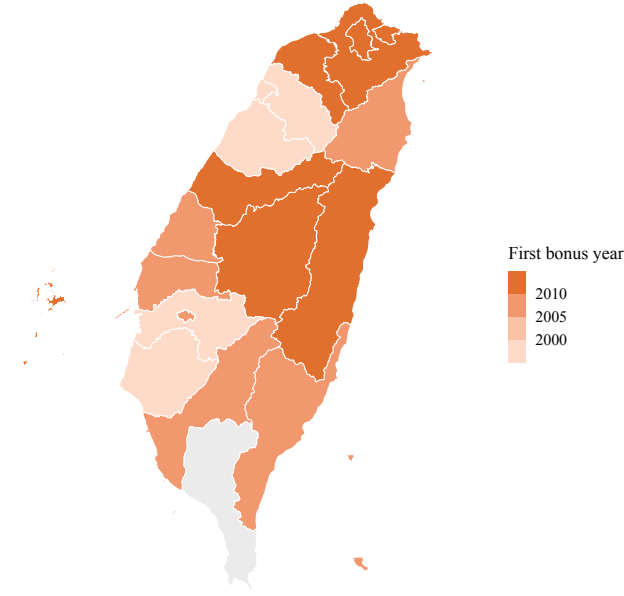


Figure 2: Child Bonus Across Counties Over Time

Note: Shading indicates the year in which each county introduced its childbirth bonus; darker shades denote later introduction. Pingtung county (grey) does not introduce a child bonus before the end of our sample period.

ered after birth and registration, it constitutes a discrete and well-timed financial incentive. In our framework, such variation allows us to examine how reductions in the immediate and quantifiable cost of childbearing influence household behavior, particularly in the allocation of resources across competing life decisions.

The effectiveness of this specific bonus has been heavily studied (Lai, 2024; Wang and Yang, 2022; Huang and Tsai, 2023). The consensus of the literature appears to be that this kind of bonus has a significant but small effect on births. Previous literature has used various event study methods, with staggered design, synthetic controls, and various sample selection criteria.

To estimate the effect of childbirth bonuses on pet decisions, we closely follow the event study framework developed by Callaway and Sant’Anna (2021), using counties that introduce the bonus later as the control group. We define the event as the year in which a county introduces its childbirth bonus, and define treatment at the county-year level. Because almost all counties eventually adopt the bonus, a never-treated comparison group would be both small and unrepresentative, so we use later-treated counties as comparisons instead. This approach has the advantage of comparing treated

and control units that are likely more similar in their long-run policy trajectories than comparisons using never-treated units.

The estimation sample is restricted to individuals born between 1974 and 1989, so that everyone in the sample is within the childbearing age range over our 2009–2020 window and the composition of the risk set does not shift mechanically with calendar time. All specifications include year-of-birth fixed effects alongside gender, which absorb the strong life-cycle gradient in both pet acquisition and childbearing documented in Section 3; without them, cohorts entering their late twenties during the sample period would drive much of the apparent policy response.

Formally, we define cohort g as the county-year of the bonus introduction, and r as the relative time since that event. For example, cohort $g = 2012$ includes individuals living in counties that introduced their bonus in 2012, and $r = 1$ corresponds to one year after the policy change (i.e., calendar year 2013). We estimate the ATT separately for each cohort and event time pair (g, r) , using a simple difference-in-differences specification. The estimation sample for each (g, r) includes counties treated in year g and counties that have not yet been treated by year $g + r$, observed in two periods: the year before treatment, $g - 1$, and the outcome year, $g + r$. On this two-period, two-group sample we estimate the following regression:

$$Y_{ict}^g = \alpha_i^g + \theta^{g,r} \mathbf{1}\{t = g + r\} + \gamma^{g,r} D_c^g \mathbf{1}\{t = g + r\} + \epsilon_{ict}^g, \quad (2)$$

where Y_{ict}^g denotes the outcome for individual i living in county c in year t , α_i^g is an individual fixed effect (allowed to vary across subsamples), $\theta^{g,r}$ is the cohort-specific time effect, D_c^g is an indicator for counties treated in year g , and $\gamma^{g,r}$ is the cohort- and event-time-specific ATT. We average the estimated $\gamma^{g,r}$ across cohorts, weighting by cohort size.⁴

In this setting, the unit of treatment is county, while outcomes are observed at the individual level. We cluster standard errors at the county level to account for serial correlation and shared policy exposure. Year-of-birth fixed effects are included to absorb time trends that vary across

⁴Counties whose only change was a reduction are excluded from the treatment definition, and Appendix C.2 re-estimates the specification dropping them from the sample entirely.

cohorts. A key identifying assumption is that the timing of bonus introduction is as good as random with respect to unobserved determinants of dog ownership.

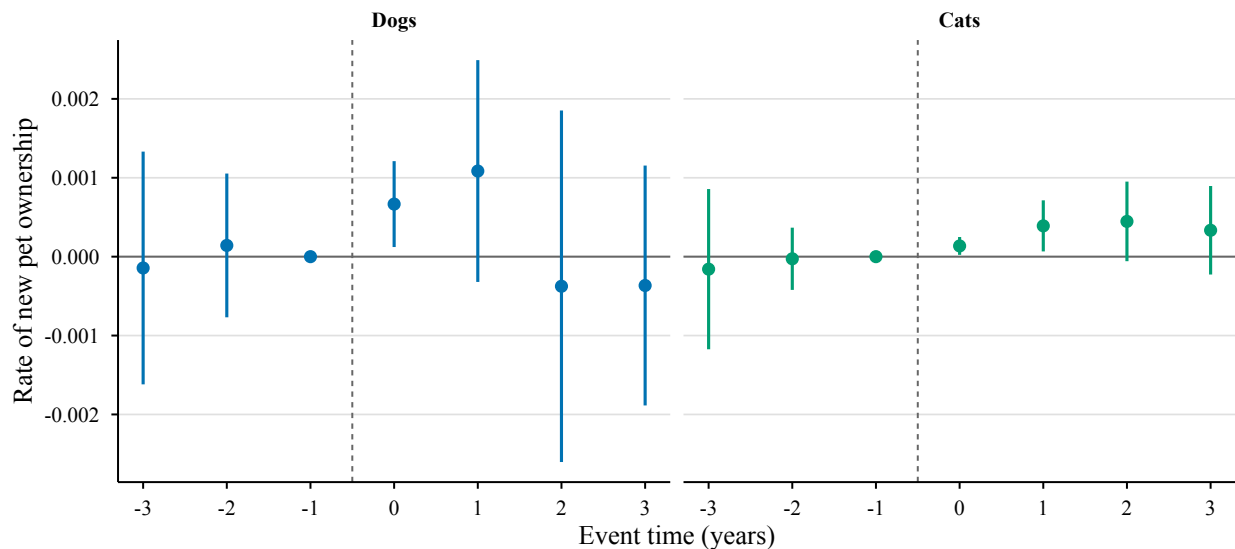


Figure 3: Effect of the Childbirth Bonus Introduction on the Annual Flow of Pet Ownership

Note: This figure plots the estimated coefficients from Equation 2, separately for dogs (left) and cats (right). The x -axis indicates years relative to the bonus introduction, and the y -axis shows the difference in rates of new pet ownership. The two panels share a common vertical scale, so magnitudes are directly comparable across species. Standard errors are clustered at the county level, and error bars denote 95% confidence intervals.

Figure 3 presents the event study estimates of the effect of the childbirth bonus introduction on the probability of acquiring a new pet, separately for dogs and cats.⁵ The pre-treatment coefficients are very close to zero and relatively flat, providing support for the validity of the later-treated comparison group and the parallel trends assumption. Immediately following the implementation of the bonus, we observe a clear increase in the flow of new dog ownership. The estimated effect is largest in the first two years.⁶ Relative to a pre-treatment mean of 0.33 percentage points in annual dog acquisition, the first-year increase of 0.067 percentage points corresponds to a 20.1% rise. This magnitude is nontrivial given the relatively low baseline rate of dog ownership and the modest size of the bonus.

⁵We analyze the cats and dogs separately, rather than pooling them into a single pet category, because they are largely kept by different households. In our survey, among the current pet owners, 4.5% keep both cats and dogs.

⁶The size of the standard errors varies across event times because the composition of the comparison group changes depending on how many counties remain untreated at each horizon.

Both cats and dogs respond, and they respond in an ordered way. In the first post-treatment year, new dog acquisitions rise by 0.067 percentage points on a pre-treatment base of 0.332 percentage points, a 20.1% increase ($t = 2.40$); new cat acquisitions rise by 0.0136 percentage points on a base of 0.125 percentage points, a 10.9% increase ($t = 2.36$). Cats are therefore complements with children as well, but the proportional response is roughly half as large as for dogs. This ordering—dogs above cats above zero—is what the starter-family mechanism of Section 5 predicts, since caring for a dog is the closer analog to childrearing, and it matches the gap we find in stated acquisition motives in our survey. Applying the same mapping used for dogs, and using the corresponding cat expenditure share of 0.026, the implied Marshallian cross-price elasticity for cats is approximately -0.68 , against -1.24 for dogs.

The effect appears quickly following the policy change but fades out within a few years. By the third and fourth post-treatment years, the point estimates decline and become statistically indistinguishable from zero. This pattern contrasts with the fertility response observed in related work, where the effect of the bonus tends to materialize more gradually and persist over time. The contrast in timing is itself informative about mechanism: a birth takes at least nine months to respond to a price change, whereas a pet can be acquired immediately. A fast, front-loaded pet response followed by a gradual fertility response is exactly the sequencing the “starter family” channel of Section 5 predicts, with prospective parents adjusting the pet margin first. The faster onset and shorter duration of the pet ownership response may reflect lower adjustment costs, fewer institutional frictions, or more flexible decision-making in pet-related behavior.

The fadeout raises a natural concern about retiming: the bonus might merely shift forward dog acquisitions that would have occurred anyway, leaving the long-run stock of dogs unchanged. The dynamic profile of the estimates speaks against this interpretation. Pure retiming requires the later flow coefficients to turn *negative*, as acquisitions pulled forward from the future are repaid; instead, the post-treatment coefficients decline toward zero but remain non-negative at every horizon, so the implied cumulative effect rises over the first two years and then plateaus rather than reverting. While the wider confidence intervals at longer horizons cannot formally rule out a partial offset,

we find no evidence of the negative post-period flows that pure retiming would require, and we therefore interpret the response as a genuine increase in the stock of dogs.

A related concern is that the bonus is cash conditional on a birth: for inframarginal families who would have had a child regardless, the payment operates as a pure income transfer, so part of the estimated dog response could reflect an income effect of the cash rather than a change in the relative price of children. Two pieces of evidence address this concern. First, and most directly, Section 4.3 uses the receipt lottery to estimate the pure income response of dog ownership, and Section 4.4 computes the Hicksian (compensated) elasticity that nets out precisely this income channel; the compensated estimate is essentially unchanged. Second, the joint-outcome decomposition in Appendix D splits the annual response into mutually exclusive child-by-dog flow cells. The earliest response comes from the “no child, dog” cell—dog acquisitions by individuals without a contemporaneous birth, and hence without newly received bonus cash—which is difficult to reconcile with a cash-in-hand income effect; the joint “child and dog” cell responds only at longer horizons, where households may hold bonuses received in earlier years. Both point to the same reading: the estimated effect reflects the change in the effective cost of children rather than the transfer itself.

We present the effect estimates on fertility of the child bonus in Appendix C.5. The fertility response carries the same positive sign found in prior studies, though it is less pronounced than the pet response, and it builds gradually over the post-treatment years, in contrast to the immediate pet adjustment documented above. A small fertility effect is also fully consistent with a sizable cross-price elasticity for dogs: as discussed earlier, the magnitude of the cross-response depends on the relative expenditure shares, and the pet share is small. Given that the expenditure share on pet ownership is small, a relatively modest bonus for children can still generate a substantial proportional change in pet behavior. In our setting, the implied Marshallian cross-price elasticity is approximately -1.24 , suggesting a relatively strong degree of complementarity. One possible interpretation, which we elaborate in Section 5, is that dog ownership may serve as a preparatory step within broader household planning, reflecting behavioral linkages between family formation

and pet acquisition.

We further conduct a range of robustness checks to assess the credibility of our findings. First, we re-estimate the model excluding counties that later reduced the bonus amount. As shown in Appendix C.2, the results remain largely unchanged, suggesting that our estimates are not driven by these policy reversals. These alternative models produce estimates that are consistent in sign and timing with our main event study results. Next, to address the possibility that individuals may relocate in response to bonus eligibility, we follow an approach similar to Wang and Yang (2022) in Appendix C.4, restricting the sample to non-movers. While this reduces statistical power, the estimates remain similar in magnitude and direction, providing additional support for our interpretation.

As an additional test of the cost-of-children channel, we examine a separate policy that also reduces the relative cost of childrearing: the 2018 expansion of the child tax credit. This reform introduced a NTD120,000 deduction per dependent child for households above a specified income threshold. We estimate the effect of this tax change on pet ownership and present the results in Appendix C.3. The estimates suggest a modest increase in pet ownership following the tax expansion, consistent with the interpretation that changes in the financial cost of children can influence decisions in adjacent domains such as pet acquisition. Although this design does not offer the same quasi-experimental variation as the staggered bonus rollout, the direction of the effect is broadly consistent with our main findings.

4.3 Identification of the Income Elasticity: Lottery Windfall

To implement Equation 1, we require estimates of the income elasticities of childbearing and pet ownership. These elasticities determine how household demand responds to changes in real income and are critical for interpreting the asymmetry in cross-price elasticities. Ideally, identifying income elasticity would require exogenous variation in household income. We leverage such a source through a unique institutional feature of Taiwan's tax system.

Taiwan's sales tax system provides a natural experiment for income shocks through its uniform

invoice lottery program. As part of a nationwide effort to reduce tax evasion, nearly all consumption receipts are eligible for bi-monthly lottery drawings, with prizes reaching up to NTD10 million. Receipt holders are randomly selected, and prizes are distributed regardless of income level or background. These awards thus constitute plausibly exogenous, positive income shocks.

We follow a similar identification strategy as in Tsai et al. (2022), who examine the fertility response to lottery winnings. In our case, we study how lottery windfalls affect pet ownership behavior (results for fertility outcomes are reported in Appendix C.6). Specifically, we use a later-treated event study design, comparing winners to those who win in future lottery periods. This approach allows us to isolate the effect of transitory income gains on pet ownership and thereby estimate the income elasticity of demand. The estimates generated from this analysis serve as inputs for the theoretical decomposition in Appendix H.1, linking the behavioral responses to changes in childbearing cost and pet ownership.

To estimate the income elasticity of pet ownership, we adopt the same event study framework used in the childbirth bonus analysis. The key difference is that the treatment here occurs at the individual level, rather than at the county level. Specifically, we define treatment as winning NTD2,000,000 or more in the uniform invoice lottery, and we compare early winners to later winners. The formal specification is given by:

$$Y_{ict}^g = \alpha_i^g + \theta^{g,r} \mathbf{1}\{t = g + r\} + \gamma^{g,r} D_i^g \mathbf{1}\{t = g + r\} + \epsilon_{ict}^g, \quad (3)$$

where Y_{ict}^g denotes the pet ownership outcome for individual i in county c and year t , and D_i^g indicates whether individual i won the lottery in cohort g . Individual fixed effects α_i^g absorb time-invariant heterogeneity, and standard errors are clustered at the individual level to account for within-person serial correlation. As before, we estimate the ATT separately for each cohort-event time pair and take a weighted average.

The results are presented in Figure 4. Following the receipt of the lottery windfall, there is a significant increase in dog ownership. In terms of magnitude, the first-year effect corresponds to an increase of approximately 24% in the annual flow of new dog acquisitions (1.4 per thousand

individuals on a pre-treatment base of 5.9 per thousand) in response to crossing the NTD2,000,000 prize threshold; the derivation of the implied income elasticity is detailed in Appendix H.1. Compared to the child bonus results, the response to lottery winnings is somewhat smaller in magnitude but consistent in direction. These findings reinforce the interpretation that pet ownership is positively income-elastic and responsive to household financial resources, providing a key input into the theoretical decomposition of substitution and income effects discussed earlier.

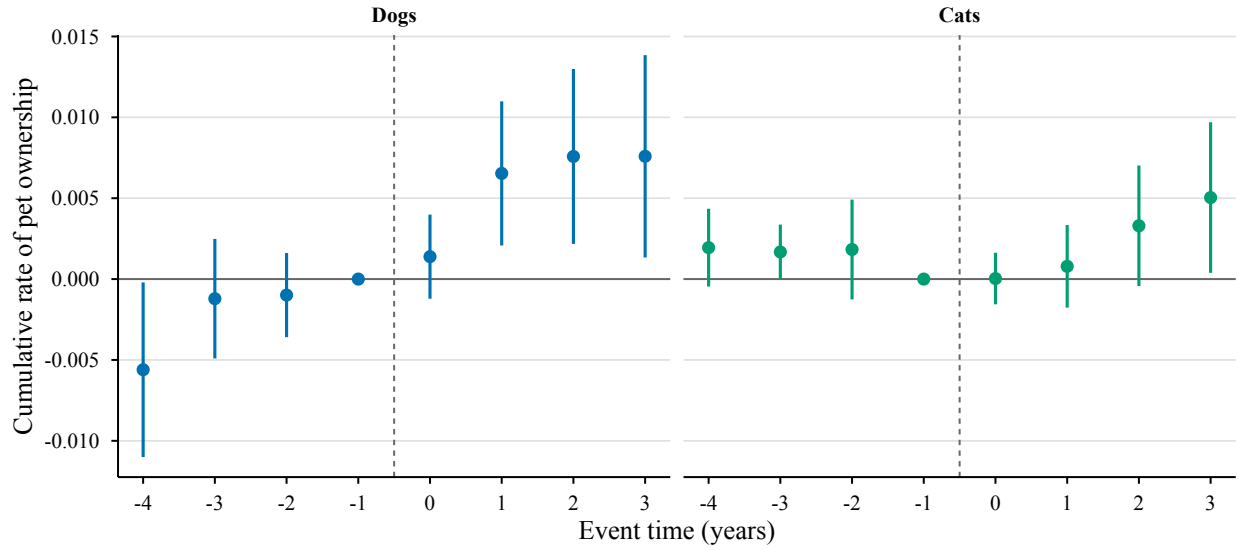


Figure 4: Effect of Lottery Winnings on Pet Ownership

Note: This figure plots the estimated coefficients from Equation 3, separately for dogs (left) and cats (right), on a common vertical scale. The x -axis indicates years relative to the lottery winnings, and the y -axis shows the difference in pet ownership between earlier and later winners. Standard errors are clustered at the individual level, and error bars denote 95% confidence intervals.

4.4 Estimating the Remaining Quantities

With the empirical estimates of the cross-price elasticity and income elasticities in hand, we now turn to quantifying the remaining parameters required to interpret Equation 1, in particular the expenditure shares for children and pets. While most of the underlying calculations and data sources are detailed in Appendix H.1, here we summarize the key figures and provide the resulting elasticity estimates.

According to the 2023 Taiwan pet industry report (Liu et al., 2024), the average annual expenditure per dog-owning household is roughly NTD30 thousand. Using data from the Family Income and Expenditure Survey, we estimate average annual disposable income of about NTD1,116 thousand and average annual consumption of about NTD930 thousand. This implies a pet expenditure share of approximately 0.032; the unrounded figures behind every step are reported in Appendix H.1.

Estimating the expenditure share for children is more complex, as detailed child-specific spending is not separately reported in household survey data. As a proxy, we use the equivalence scale approach, where a child under age 14 is assigned a weight of 0.3. For a three-person household with two adults and one child, this implies that the child accounts for 0.3 out of a total equivalized household size of 1.8, or roughly 17% of consumption. Alternative estimates in the literature suggest child expenditure shares as high as 22% in the United States. To remain conservative and context-appropriate, we adopt a baseline value of 0.20 in our calculations.

Both shares are calibrated from sources external to our own data collection; our survey (described in Appendix G) provides an independent, measured check on them. Among current pet owners who report their monthly pet spending, the median ratio of annualized pet spending to household income is 0.033, nearly identical to the 0.032 obtained from the industry report and the Family Income and Expenditure Survey. Among parents, the median ratio of reported child spending, summed across children, to household income is 0.22, with a mean of 0.30, bracketing our baseline value of 0.20. Reported spending levels are similarly consistent: the median pet expenditure of NTD3,000 per month annualizes to NTD36,000, close to the industry-report figure of NTD30,093. Using the survey medians in place of the calibrated shares would leave the implied elasticities below essentially unchanged.

Using these expenditure shares along with the income elasticities ($\eta_{\text{child}} = 0.15$, adopted from Tsai et al. (2022); $\eta_{\text{dog}} = 0.11$, our lottery estimate) and the cross-price elasticity ($\epsilon_{\text{dog,child}}^M \approx -1.24$), we can solve for the implied elasticity of child demand with respect to the price of dog ownership, $\epsilon_{\text{child,dog}}^M = -0.20$, using the identity in Equation 1. The details of this derivation are

presented in Appendix H.1.

To recover the Hicksian (compensated) cross-price elasticity between children and dogs, we apply the Slutsky decomposition that links Marshallian and Hicksian elasticities through the income effect. Formally,

$$\epsilon_{ij}^H = \epsilon_{ij}^M + s_j \eta_i,$$

where ϵ_{ij}^M is the Marshallian elasticity of good i with respect to the price of good j , s_j is the expenditure share of good j , and η_i is the income elasticity of good i . Applying this first to the estimated elasticity of dog demand with respect to the price of children, with $\epsilon_{\text{dog,child}}^M = -1.24$, $s_{\text{child}} = 0.20$, and $\eta_{\text{dog}} = 0.11$, we obtain

$$\epsilon_{\text{dog,child}}^H = -1.24 + 0.20 \times 0.11 = -1.22,$$

the compensated counterpart of our headline estimate: because the expenditure share of children and the income elasticity of dog ownership are both modest, netting out the income effect barely moves the elasticity. In the reverse, policy-relevant direction, using $\epsilon_{\text{child,dog}}^M = -0.202$, $s_{\text{dog}} = 0.032$, and $\eta_{\text{child}} = 0.150$, we have

$$\epsilon_{\text{child,dog}}^H = -0.202 + 0.032 \times 0.150 = -0.198.$$

The resulting Hicksian elasticity of -0.20 indicates that, after compensating for income effects, dogs and children remain complements. It is worth noting that the adopted income elasticity of childbearing cancels in this compensated calculation: substituting the identity in Equation 1 gives $\epsilon_{\text{child,dog}}^H = (s_{\text{dog}}/s_{\text{child}}) \epsilon_{\text{dog,child}}^M + s_{\text{dog}} \eta_{\text{dog}}$, so the net-complements conclusion rests only on quantities we estimate directly—the bonus response and the income elasticity of dog ownership—and not on the value of η_{child} borrowed from the literature. A similar calculation for cats gives $\epsilon_{\text{child,cat}}^H = -0.089$.

Because the shares and income elasticities entering these calculations are calibrated inputs,

Appendix H.2 reports a full sensitivity and uncertainty analysis. Varying every input over all alternative values from our sources, including the survey-measured shares, keeps $\epsilon_{\text{child,dog}}^M$ strictly negative, with bounds of $[-0.58, -0.13]$. Propagating sampling uncertainty as well (resampling the survey-measured shares, and drawing $\epsilon_{\text{dog,child}}^M$ and the income elasticities from their estimated standard errors) yields a combined 95% interval of $[-0.35, -0.04]$, which excludes zero. The conclusion of complementarity does not depend on any single input, and it is robust to the sampling uncertainty of the estimated components.

One caveat on interpretation, developed in Appendix J, deserves note: the Slutsky identity holds exactly for a static demand system, and with learning the implied $\epsilon_{\text{child,dog}}^M$ is best read as a policy-relevant summary of the total response rather than as a deep parameter of static preferences.

In summary, using quasi-experimental designs based on child bonuses and income lotteries, our estimates indicate a complementarity between childbearing and pet ownership. Stated-preference evidence from our survey points in the same direction: asked whether a hypothetical subsidy of NTD10,000 per registered pet would change their childbearing plans, respondents answering that children would become *more* likely outnumber those answering *less* likely by roughly two to one (25.7% versus 12.2%, with the majority reporting no effect). This is precisely the sign pattern implied by a negative cross-price elasticity $\epsilon_{\text{child,dog}}^M$, and it lends independent credibility to the complementarity result.

The estimated effect of -0.20 is sizable. However, a back-of-the-envelope calculation shows that even taking it at face value, promoting pet ownership cannot “solve” the fertility problem, as is sometimes suggested. Two adjustments discipline the extrapolation. First, handing a household a dog eliminates only the acquisition cost—a median of NTD15,000 in our survey, roughly 5% of the full cost of dog ownership once about NTD30,000 of annual upkeep over a ten-year lifespan is accounted for. Second, our elasticity is identified from households on the dog-ownership margin, and only such households can respond: in our survey, 9.1% of respondents currently own a dog and a further 14.7% do not own a pet but would like a dog, so we scale the aggregate response by this combined share of 0.24. Applying $\epsilon_{\text{child,dog}}^M = -0.20$ to Taiwan’s 2023 total fertility rate of 0.86

(Agency, 2025), handing every household a dog raises the total fertility rate by $0.86 \times 0.20 \times 0.05 \times 0.24 \approx 0.002$ children per woman—essentially zero. Even making dog ownership entirely free—a 100% price reduction far outside the range over which our local elasticity is identified—raises the total fertility rate by only about 0.04, to roughly 0.90, still far below the replacement level of 2.1. It is also important to emphasize that this estimate is inherently static, as it is identified from a one-time policy variation. The dynamic patterns and underlying mechanisms are much richer, as discussed in Section 5.

5 Mechanism

The preceding section establishes a causal cross-price relationship: reducing the cost of children increases pet acquisition. This section asks what behavioral mechanisms may generate that complementarity. Pet acquisition and childbirth are themselves choices, so the analyses below do not estimate the causal effect of one event on the other. Instead, we combine sequencing in the administrative data, survey evidence, and heterogeneity patterns to distinguish among mechanisms that make different predictions about when and for whom the two decisions coincide. At least three mechanisms could generate the complementarity. Pets may serve as a “starter family,” letting prospective parents rehearse caregiving and learn about their own taste for family life before a birth. This is a recurring theme in our survey, where a substantial share of respondents report acquiring a dog as a way to “practice” for having children. Children may instead create demand for pets, with parents acquiring an animal as a companion for a growing child. Or the two may be linked through selection, if family-oriented people simply tend to acquire both pets and children. We cannot conclusively rule out any of these channels, but each carries distinct predictions about which decision comes first, and for whom.

5.1 Dynamic Patterns

The causal estimates in Section 4 are primarily static in nature and do not reveal the sequencing of events between acquiring a pet and having a child. To explore the underlying mechanism of complementarity, we examine dynamic patterns in the data, focusing on the timing of births relative to pet acquisitions and vice versa. Specifically, we conduct descriptive event studies using two types of comparison groups. The first comparison group matches individuals on age and gender who have never experienced the event in question. The second uses a “later-treated” framework, comparing those who have experienced the event at different times. While these exercises are not intended to identify causal effects, because acquiring a pet or having a child is itself a choice, they serve as descriptive tools that shed light on the temporal relationship between the two events.

We begin by examining raw event-time averages. The left panel of Figure 5 plots the probability of having a newborn child for individuals who acquire a dog, relative to those who do not. The outcome variable is measured as a flow. The levels differ markedly before the event: throughout the pre-period, the event group’s fertility sits well below that of the matched comparison group, individuals of the same age and sex assigned a randomly drawn placebo event year. After dog acquisition, the probability of having a child rises sharply, overtakes the comparison group, and continues to increase in subsequent years.

The right panel of Figure 5 shows the reverse relationship: the probability of acquiring a dog for individuals who have a newborn child. Several patterns are noteworthy. First, future parents already acquire dogs at visibly higher rates before childbirth. This anticipatory pattern is consistent with the starter-family channel, though selection can produce it as well. Second, immediately after the birth of a child, the probability of acquiring a dog falls markedly. Examined more closely, however, the decline does not persist: the acquisition rate stops falling once the child is about two years old and edges up thereafter, hinting that some parents later acquire a dog as a companion for a growing child—the second channel listed above. Interestingly, the pre-event trends are close to parallel. These raw data show annual flows: the probability of a birth and the probability of a new dog registration in each event year. Appendix B presents the corresponding cumulative stocks, the

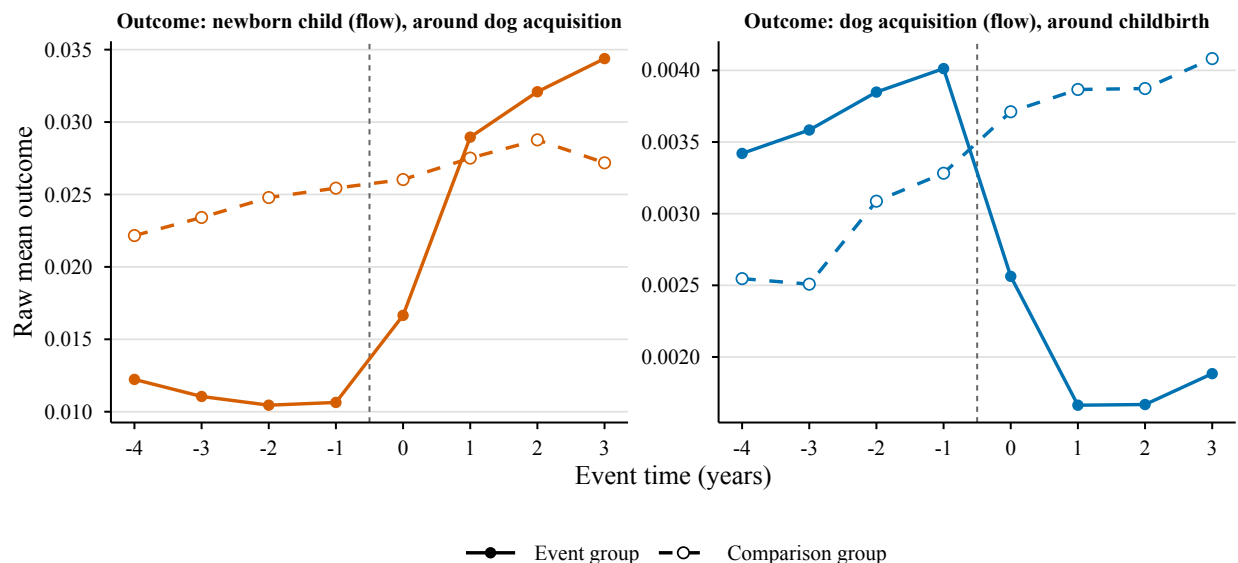


Figure 5: Raw Event-Time Averages Around Dog Acquisition and Childbirth

Note: Raw means by event time. The left panel plots the probability of having a newborn child (flow) around the year of dog acquisition; the right panel plots the probability of acquiring a new dog (flow) around the year of childbirth. Lines are colored by the outcome of each panel (orange for childbearing, blue for dog acquisition). In each panel the event group (solid line, filled markers) is aligned on the actual event year, while the comparison group (dashed line, open markers) is assigned a placebo event year drawn at random. Panels use separate vertical scales because the outcomes differ.

running totals of children and of dogs up to each event time.

The same qualitative patterns emerge when using a descriptive event study design with later-treated individuals as the control group. As shown in Figure 6, the probability of having a baby rises sharply after acquiring a dog, while the probability of acquiring a dog falls sharply after the birth of a child. The same pattern holds if we restrict our focus to first-time dog owners (Appendix C.7). Additional results using the number of new dogs as the outcome variable and those using the household as the unit of analysis are reported in Appendix C.9 and Appendix C.10, respectively.

Given the evidence presented above, we now consider a potential mechanism that can explain the observed complementarity between childbearing and dog ownership: pets as a “starter family.” By this, we mean that some households may acquire a pet, particularly a dog, as a way to prepare for the responsibilities, routines, and shared decision-making associated with raising a child. In this interpretation, pet ownership serves both as a test of caregiving capacity and as a way for couples

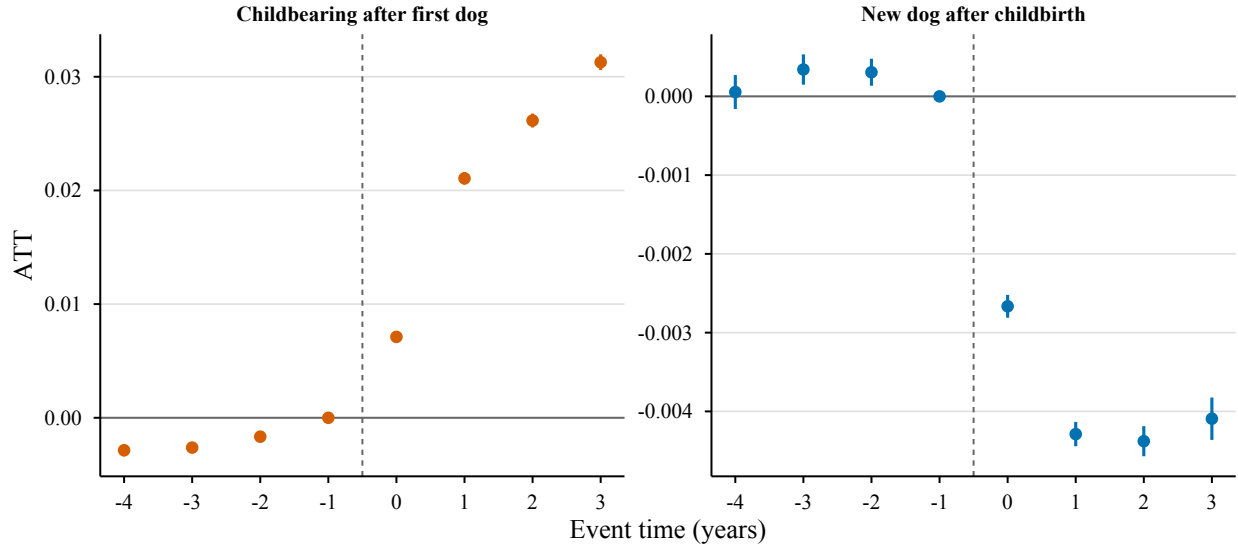


Figure 6: Event Studies of Childbearing and Dog Ownership, Both Directions

Note: Event-study estimates using later-treated individuals as the comparison group. The left panel shows childbearing around first dog acquisition; the right panel shows new dog ownership around childbirth. Panels use separate vertical scales because the outcomes differ. Standard errors are clustered at the individual level, and error bars denote 95% confidence intervals.

to gauge compatibility in managing joint responsibilities.

The first piece of evidence supporting this mechanism comes from direct self-reports in our survey. Among pet owners who answered the question on acquisition motives, 21.2% report at least one practice-type motive: practicing caregiving before having children, testing their own (or their partner's) capacity to care for a child, or building caregiving routines and shared responsibility with a partner. Consistent with the dog–cat asymmetry emphasized below, the share is 29.7% among current dog owners versus 18.5% among cat-only owners ($p = 0.002$). The stated direction of influence is also asymmetric: among respondents who acquired a pet before having children, those reporting that the experience made them want children *more* outnumber those reporting the opposite by roughly five to one. Similar motives have been documented in other settings (Gonzales et al., 2023). This explicit self-reported evidence points directly toward the starter-family interpretation.

Importantly, the starter-family channel does not require an ex-ante plan to operate. Even if a household acquires a pet for unrelated reasons, the caregiving experience itself generates information: 35.8% of survey respondents report that caring for a pet revealed traits about themselves or

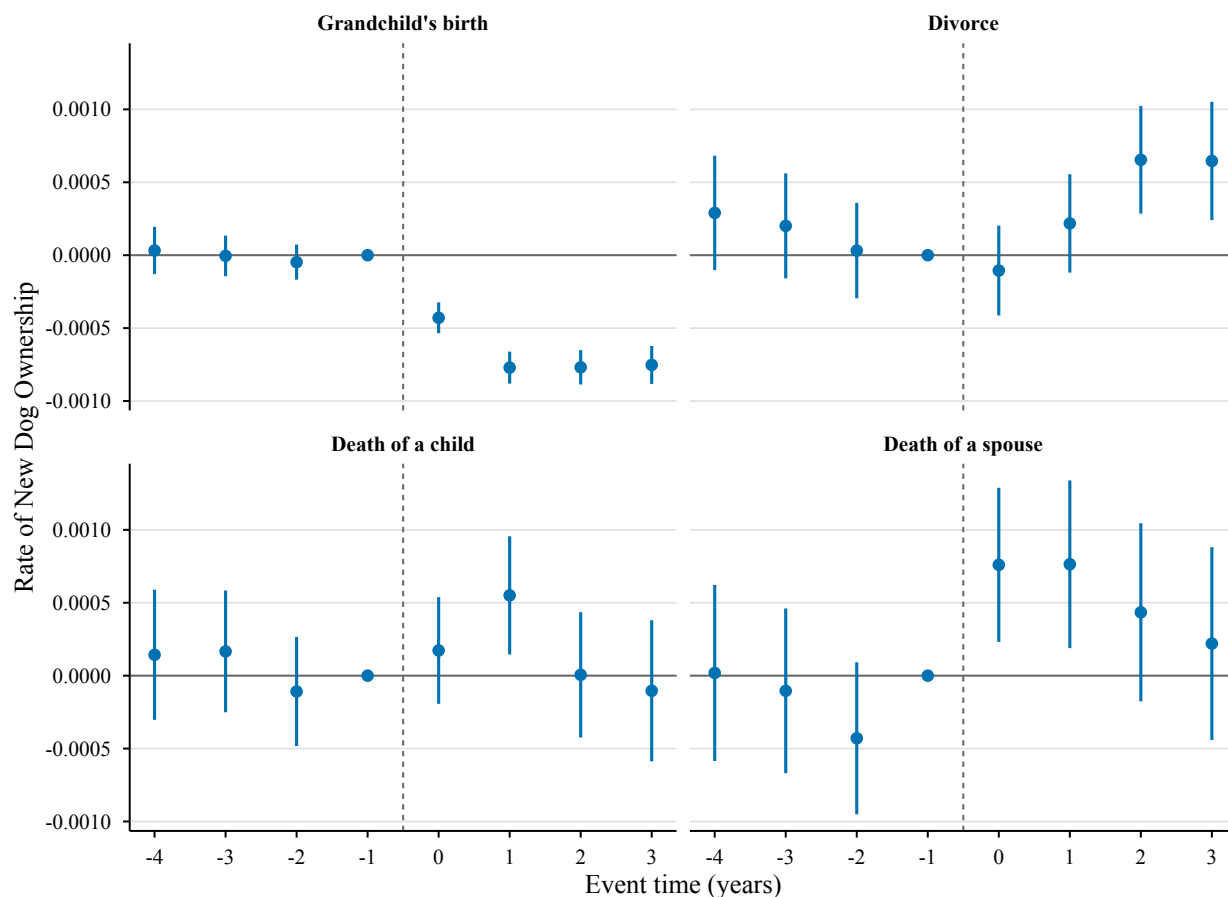


Figure 7: Dog Ownership Around Later-Life Family Events

Note: Event-study estimates of new dog ownership around the birth of a grandchild and a divorce (top row) and the death of a child and the death of a spouse (bottom row), each using later-treated individuals as the comparison group. All four panels share a common vertical scale, so magnitudes are directly comparable across events. Standard errors are clustered at the individual level, and error bars denote 95% confidence intervals.

their partner that influenced their childbearing decision, a considerably larger share than the one stating a practice motive up front. The timing is consistent with this learning being consequential: among owners who were not already considering children when they acquired the pet, roughly a third began seriously considering children within a year of the acquisition. The survey also quantifies the sense in which a pet is a scaled-down version of a child: owners report spending about 1.2 hours per day caring for pets, compared with 3.2 hours that parents report spending on children, a ratio that mirrors the gap in expenditure shares documented in Section 4.4 (0.03 versus 0.22). In both time and money, pets resemble a partial, lower-stakes trial of childrearing.

The dynamic patterns documented above are also consistent with this story. The probability of having a child increases sharply following the acquisition of a dog, and the rise in the flow of births continues over subsequent years. This pattern suggests that dog ownership may accelerate the transition to childbearing for households without children at baseline. The quasi-experimental bonus variation exhibits the same sequencing: when the response is decomposed into mutually exclusive joint child-by-dog flow cells (Appendix D), the “no child, dog” cell responds first, the joint “child and dog” cell catches up in later years, and the “child, no dog” cell—the largest—rises throughout, exactly the pattern implied by a pet-then-child sequence.

The heterogeneity between dogs and cats further reinforces this interpretation. Caring for a dog generally requires more time, involvement, and structured daily routines than caring for a cat, making it a closer analog to childrearing. Consistent with this, the childbirth-bonus response is roughly twice as large in proportional terms for dogs as for cats (20.1% versus 10.9% of the pre-treatment flow; Figure 3), a ratio close to the dog–cat gap in stated practice motives in our survey (29.7% versus 18.5%). Two independent sources of evidence thus deliver the same ordering.

Finally, if the starter-family channel is indeed central, then the response to acquiring a dog should differ markedly between households that already have children and those that do not: the increase in births should be concentrated among those without prior children, since existing parents have already experienced the responsibilities of childrearing and therefore have less to “learn” from pet ownership. Figure 8 supports this prediction: households with no children at baseline exhibit a sizable increase in births after acquiring a dog, whereas households with existing children experience no increase (and even a modest decline) in subsequent births. Part of the level gap between the two groups is mechanical, since those without prior children can only increase fertility. What the mechanical account cannot produce is the direction of the with-children path: their subsequent births do not merely rise less, they decline after dog acquisition, while births in the childless group rise. This pattern is not unique to the descriptive event studies. The same asymmetry appears from the child bonus analysis: splitting the childbirth-bonus event study by baseline parity, the bonus-induced increase in dog acquisition comes almost entirely from individuals without children

at baseline, while existing parents show no response (Table 5, Panel B).

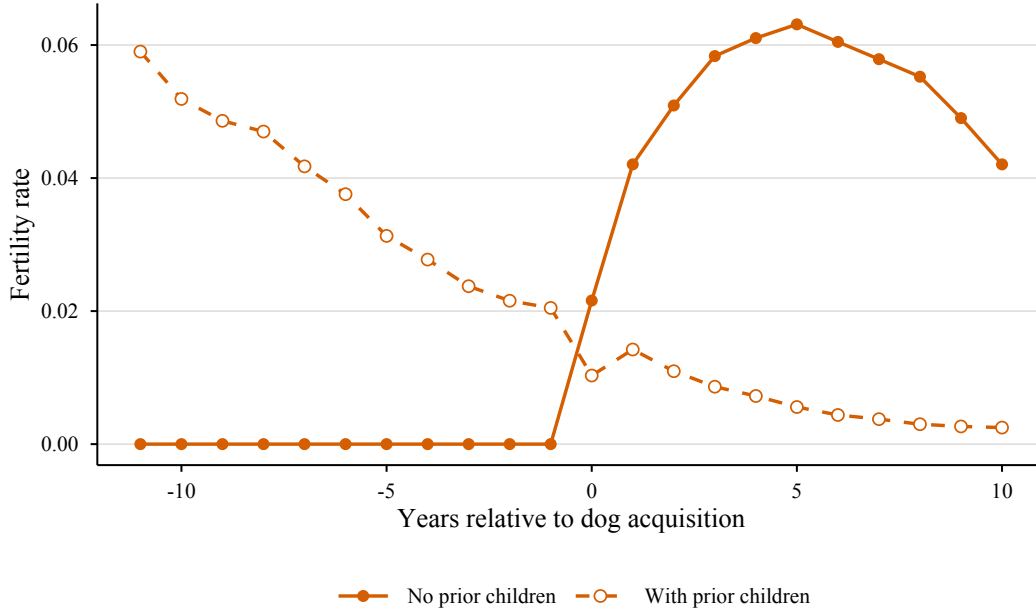


Figure 8: Raw Data Comparison of Additional Childbearing Around the Onset of Dog Ownership

Note: This figure plots the raw mean probability of having an additional child relative to the year of dog acquisition. Both lines are orange, the color of childbearing outcomes throughout the paper. The solid line (filled markers) shows individuals without a child prior to acquiring a dog; the dashed line (open markers) shows individuals who already have a child.

5.2 Alternative Explanations

While the descriptive patterns and heterogeneity analyses provide evidence consistent with the “starter family” interpretation, other explanations are also possible. In particular, the observed complementarity between dog ownership and childbearing could arise from differences in underlying preferences across individuals, or from indirect channels such as changes in partnership formation. Before concluding that the starter-family mechanism is the primary driver, we consider two alternative explanations: selection into a “type” that values both pets and children, and effects operating through marriage.

A straightforward alternative to the “starter family” interpretation is that there are simply different types of people. One “family-loving” type sequentially acquires a dog and then has children,

while another type does neither. This view is consistent with the dynamic patterns observed so far, and our descriptive event studies cannot rule it out. Two observations, however, weigh against it.

First, it must be the case that for the family-loving type, dogs always precede children, and when a dog is acquired later in life, it does not lead to additional children. Second, a fixed-type account has to be reconciled with the policy results, in which child bonuses and tax benefits raise dog ownership. Selection nonetheless remains possible even in light of the policy results: a bonus can change what a family-oriented type finds optimal without changing the type itself, so the policy response and a stable-type account can coexist. What the evidence does establish is that the two accounts differ in their predictions about timing and parity, and on both margins the data favor a channel operating at the point of family formation.

Another possible mechanism is that dog ownership indirectly affects fertility through marriage. For example, walking a dog may increase opportunities to meet potential partners, either through social interactions or by making owners appear more approachable. If dog ownership facilitates partner matching, this could increase marriage rates, which in turn may lead to higher fertility. This channel is not merely hypothetical: in our survey, 35.9% of the pet owners asked report that keeping a pet helped them meet a partner or strengthened their relationship with one, although this is descriptive stated-preference evidence.

To assess this, we estimate an event study similar to those presented earlier in this section, replacing births with marriage as the outcome in Appendix F. As shown in Figure 30, the pre-event trend for marriage differs markedly from the parallel patterns seen in the childbearing results, with divergence beginning well before the acquisition of a dog. The event study provides little support for dog acquisition itself increasing fertility through subsequent marriage formation.

Another related issue is health concerns for children. It is well known in Taiwan that cat feces pose a toxoplasmosis exposure risk for pregnant women (Taiwan Centers for Disease Control, 2018). This may help explain why cat ownership could relate to newborn outcomes. However, almost all patterns we find are common to dogs and cats, with smaller magnitudes for cats, so toxoplasmosis exposure is unlikely to be the main driver.

Appendix J presents a simple three-period framework that organizes these channels within a single decision problem: households allocate money and time across pets and children, caring for a first pet can reveal information about the household’s own taste for childrearing, and underlying tastes for pets and children may be correlated. Each channel discussed in this section has a counterpart in that framework—learning corresponds to the starter-family story, time competition and companionship demand shape when pets and children coexist, and correlated tastes capture selection—and the framework makes explicit why timing and parity are the margins on which the channels differ. It is an organizing device rather than an estimated model, but it shows that the channels can coexist without contradiction and why the sequencing evidence above speaks to their relative importance.

In sum, while we cannot rule out these alternative mechanisms, the combination of anecdotal evidence, dynamic patterns, and heterogeneity across pet types is more readily reconciled with the “starter family” interpretation than with the alternatives we have considered. Because these event studies are descriptive, we read them as narrowing the set of plausible explanations rather than as establishing a channel.

5.3 Heterogeneity Analysis

We present the event study results for different subsamples in Appendix I, summarized in Table 5. Specifically, we examine heterogeneity by sex, birth cohort, homeownership, and baseline parity.

We consider sex because it captures differences in household roles that are relevant for understanding the trade-offs between raising children and keeping dogs. We also examine homeownership, given that the rental market imposes constraints since many rental units do not allow pets.

We do not find substantial heterogeneity by sex, and we find little difference by homeownership status. Across birth cohorts, however, both margins are stronger for younger cohorts: childbearing after a first dog rises by 1.27 percentage points for the younger cohort versus 0.36 for the older one, consistent with the starter-family channel operating primarily at the family-formation stage of the

life cycle.

5.4 Pet Acquisition across the Family Life Cycle

So far, we have focused on the relationship between new births and dog ownership. How pets enter family life, however, differs across its stages. As the previous sections have shown, timing is crucial in understanding this relationship, and its importance extends more broadly.

Our data allow us to briefly explore how pet acquisition responds to family events beyond a birth: for instance, once children reach adulthood or when grandchildren arrive. Extending the event studies to the grandparent generation, Figure 7 shows that the probability of acquiring a dog declines modestly following the birth of a grandchild, mirroring the decline observed around one's own childbearing at roughly a fifth of the magnitude. The association between family formation and pet ownership thus extends beyond the nuclear family, albeit to a lesser degree.

We further examine three additional life events. Figure 7 shows that dog acquisition rises in the years following a divorce. The death of a child is likewise associated with an increased probability of dog ownership, and the death of a spouse has an even larger effect.

Further research is needed to establish the causal nature of these relationships, as the evidence we present is suggestive. The absence of strong pre-trends provides some reassurance, and reverse causality is hard to construct for several of these events—pet ownership is unlikely to cause a bereavement—but we do not claim that omitted confounders can be ruled out. Taking these results into account, the overly simplified public discussions of “complements” and “substitutes” may stem from differences in the life stages being considered. The same distinction offers one way to read the aggregate picture in Figure 1: as Taiwan's population ages, a growing share of pet acquisitions may come from households at stages where pets substitute for human companionship, so aggregate pet growth alongside declining births could be consistent with complementarity at the family-formation margin.

6 Conclusion

To the best of our knowledge, this paper provides the first large-scale empirical evidence on the relationship between pet ownership and fertility. Using rich administrative data from Taiwan, we document that pets interact with household decisions about childbearing in systematic and economically meaningful ways.

Our findings can be summarized along three dimensions. First, observationally, individuals with dogs are more likely to have children, while individuals with cats are less likely to. Dog ownership is positively correlated with characteristics associated with fertility, such as stronger economic conditions and more favorable positions in the marriage market, but these factors alone cannot explain the positive association—nor do they explain the negative association for cats, which is stable across specifications. Second, exploiting quasi-experimental variation in the cost of raising a child from child bonuses, together with income shocks from the receipt lottery, we map the reduced-form responses into Marshallian and Hicksian cross elasticities under a calibrated static demand system, and find that pets and children are complements. The causal content of this step rests on the policy variation; the mapping to elasticities additionally relies on calibrated expenditure shares. Third, dynamics matter. Individuals who adopt dogs subsequently exhibit higher probabilities of having children, a pattern consistent with dogs fostering routines of care, encouraging home-centered lifestyles, and serving as a form of “starter family,” though the event studies behind it are descriptive rather than causal. Conversely, after the arrival of a newborn, the likelihood of acquiring a new dog declines sharply. Later in life, however, pet ownership often rises following major life transitions such as divorce or the loss of a spouse or child, reflecting pets’ role as emotional and social companions.

Taken together, our analysis highlights the broader role of pets in shaping household behavior and well-being. Companion animals influence how people allocate time, form relationships, and adapt to life-course transitions. Beyond fertility, they represent an increasingly integral component of modern family life, mediating care, companionship, and emotional support in ways that intersect with demographic and social change. Understanding these dynamics can enrich family economics

and social policy, offering new insights into how modern households organize and adapt.

Declaration of Generative AI Use

During the preparation of this work the authors used Claude in order to improve the readability and language of the manuscript. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of this article.

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A Descriptive: Age Profiles of Pet Ownership

Figure 9 plots the cross-sectional age profile of pet ownership in 2020, separately for single and married individuals and stratified by whether they have children. Two patterns stand out. Pet ownership peaks in the twenties and early thirties in every group, several years before the peak of childbearing, consistent with the sequencing documented in Section 5. And within each marital status, individuals without children own pets at higher rates through the prime family-formation ages, with the gap largest among younger married individuals. We caution that a single-year cross-section mixes different birth cohorts at each age, so these profiles serve as context for the timing patterns studied formally in the main text rather than as evidence in their own right.

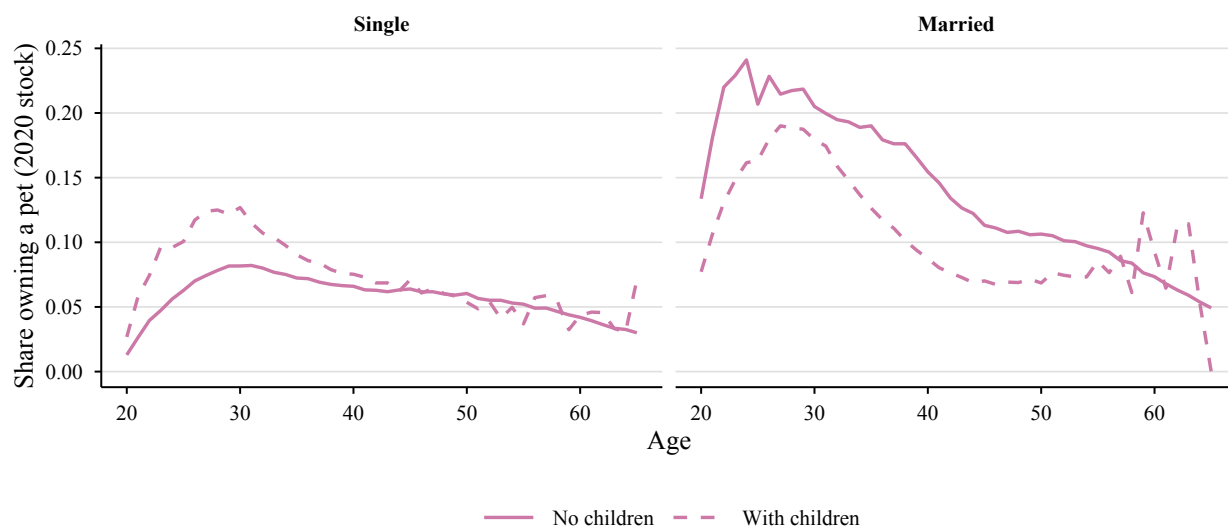


Figure 9: Pet Ownership by Age and Marital/Parental Status (Cross-Sectional Stock, 2020).

Note: Share of individuals owning a pet by age in 2020, for single (left) and married (right) individuals. Both lines are purple, indicating combined dog-and-cat ownership; the solid line represents individuals without children and the dashed line those with at least one child. Ownership peaks in the twenties and early thirties in every group.

B Raw Data Cumulative

Here we report cumulative, or stock, measures that mirror the flow patterns (Figure 5) in the main text. Figure 11 plots the stock of children around the event of acquiring a dog. Figure 10

presents the stock of dog ownership around the birth of a child.

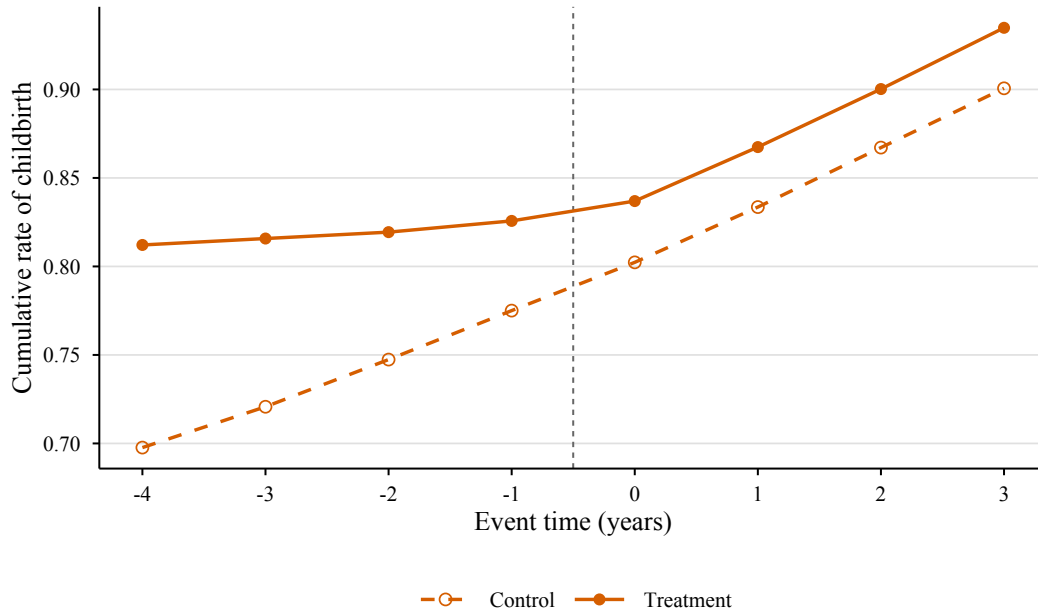


Figure 10: Raw Data Comparison of Cumulative Childbearing Around the Onset of Dog Ownership

Note: This figure plots the raw mean stock of children relative to the year of dog acquisition. The stock is constructed as the cumulative sum of the annual indicator for having a newborn up to each event time for each individual. Both lines are orange, the color of childbearing outcomes. The treatment group (solid line, filled markers) is aligned on the actual year of dog acquisition; the comparison group (dashed line, open markers) is assigned a placebo event year drawn at random.

C Robustness

C.1 Removing the 2013 Registration Spike

From September to October 2013, the Council of Agriculture implemented a one-time nationwide campaign that provided free microchipping, waived registration fees, and supplied rabies vaccines in epidemic and high-risk areas as well as private shelters. This temporary incentive generated a sharp, mechanical spike in registrations unrelated to underlying demand. To ensure our findings are not driven by this administrative shock, we re-estimate our specifications excluding observations from September–October 2013. Figure 12 shows the DiD results for childbirth-bonus effects on dog ownership; Figure 13 reports the corresponding event-study estimates for childbearing around

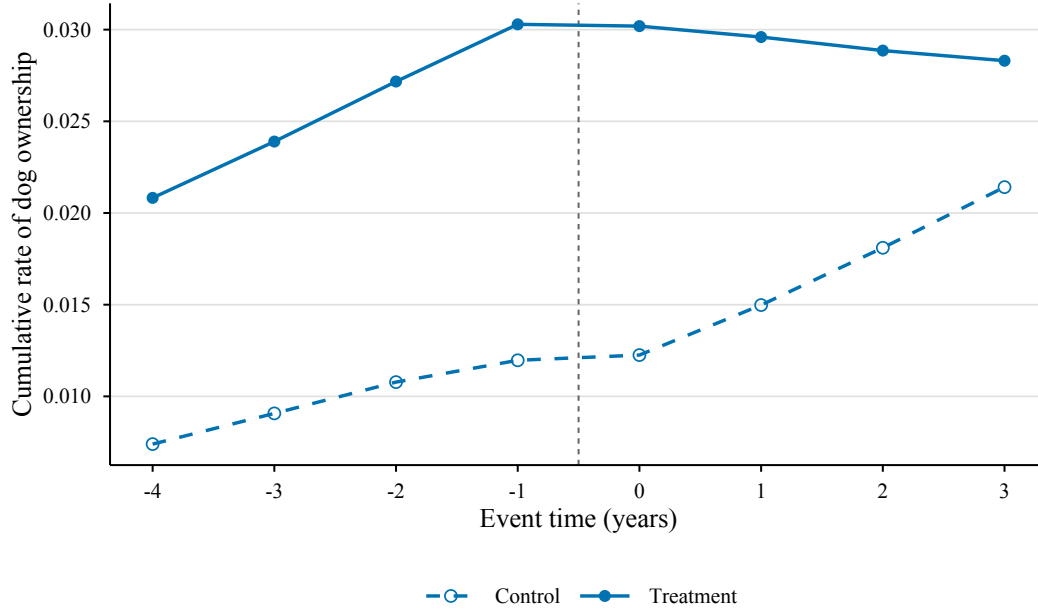


Figure 11: Raw Data Comparison of Cumulative Dog Ownership Around the Onset of Childbirth

Note: This figure plots the raw mean stock of dog ownership relative to the year of childbirth. The stock is constructed as the cumulative sum of the annual indicator for acquiring a new dog up to each event time for each individual. Both lines are blue, the color of dog-ownership outcomes. The treatment group (solid line, filled markers) is aligned on the actual year of childbirth; the comparison group (dashed line, open markers) is assigned a randomly drawn placebo event year.

dog acquisition and dog acquisition around childbearing, respectively. Across all three exercises, the results are robust to excluding the spike months.

C.2 Drop Decreases

Our treatment definition dates each county by the introduction of its bonus, so a subsequent reduction is not itself coded as an event. Three counties—Hsinchu, Miaoli, and Tainan—nonetheless cut their bonus at some point during the sample period, and their post-treatment observations could in principle be contaminated by that later reversal. Figure 14 re-estimates the main specification after dropping these three counties entirely. The signs and magnitudes are essentially unchanged, so the estimates are not driven by counties whose bonus later fell.

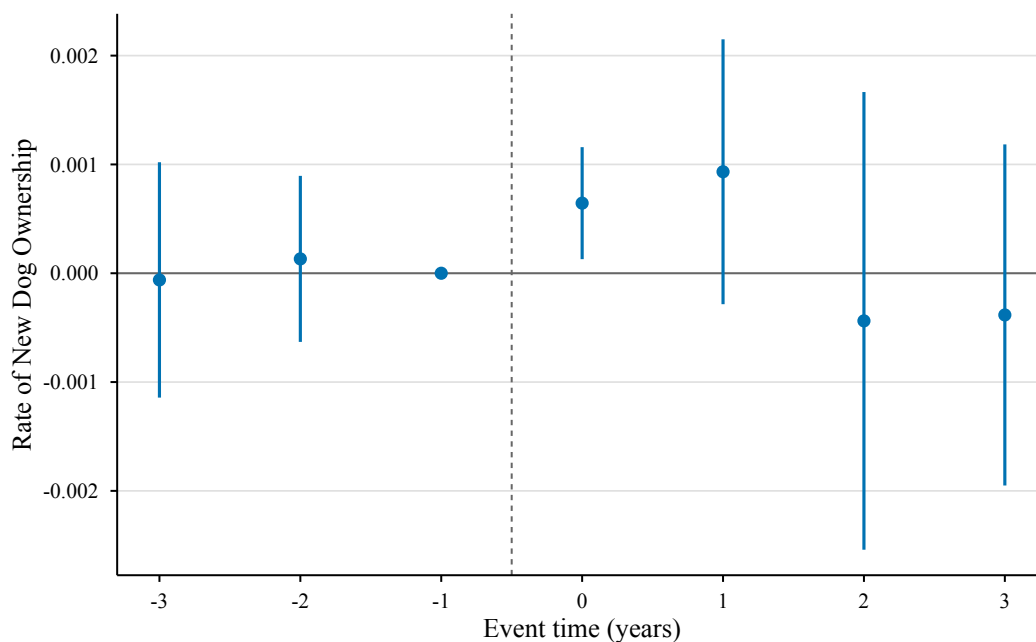


Figure 12: Effect of the Childbirth Bonus on New Dog Ownership, Excluding Sep–Oct 2013

Note: This figure reproduces the baseline bonus difference-in-differences estimates after removing observations from September–October 2013, when a national registration campaign caused a mechanical spike in registrations. The specification, controls, and clustering (county level) match the baseline.

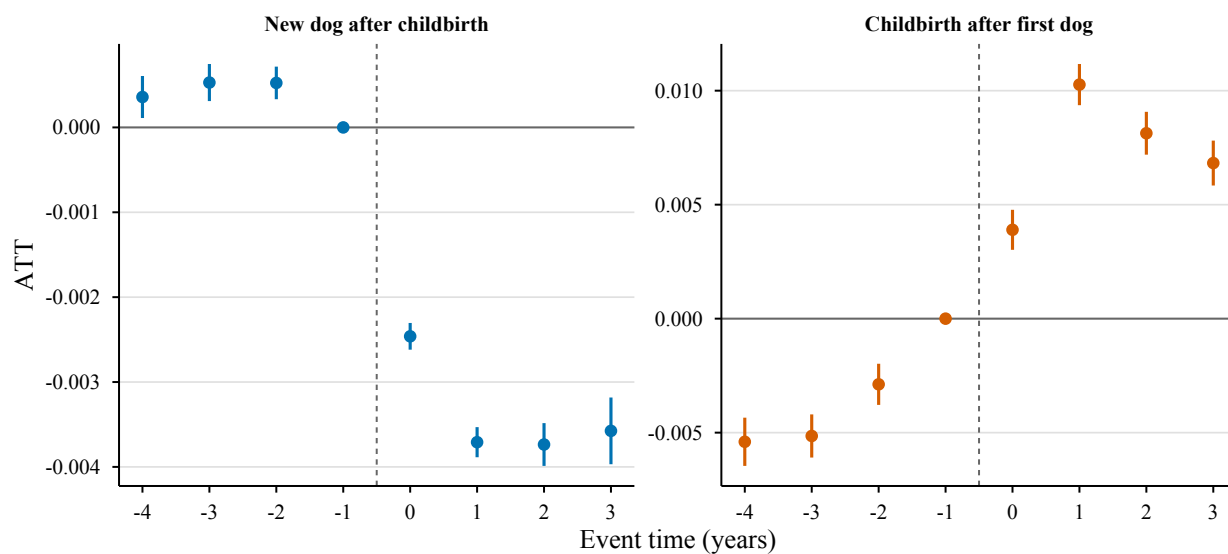


Figure 13: Event Studies Excluding the September–October 2013 Registration Campaign

Note: Both directions of the main event study re-estimated after excluding registrations from the 2013 free-registration campaign: new dog ownership around childbirth (left) and childbearing around first dog acquisition (right). Panels use separate vertical scales; standard errors are clustered at the individual level.

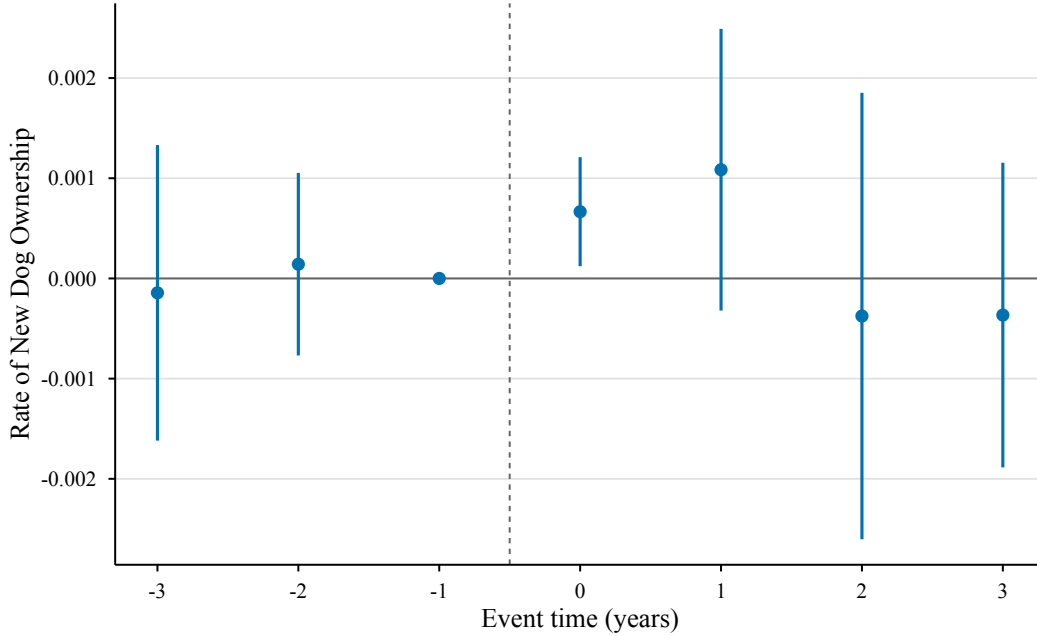


Figure 14: Re-estimating After Excluding Counties with Bonus Decreases

Note: The main specification re-estimated after dropping Hsinchu, Miaoli, and Tainan, the three counties whose bonus fell at some point during the sample period. The estimator, controls, and county-level clustering follow Equation 2.

C.3 Specifications Using Tax Deductibles

To alleviate childcare costs and encourage fertility, Taiwan introduced the *Special Deduction for Pre-school Children* in 2012 (NTD25,000 per eligible child) and substantially expanded it in 2018 to NTD120,000, alongside a broadened eligibility scope (Ministry of Justice, 2018). We leverage the 2018 expansion as a nationwide policy shock whose *effective* generosity varies mechanically with the taxpayer’s marginal tax rate (MTR), because the value of a deduction equals $\text{MTR} \times (\text{deduction amount})$.

We focus on individuals aged 20–44 and years 2013 onward to avoid conflating effects with the 2012 introduction. Let Y_{it} denote the outcome of interest (e.g., birth, marriage, or dog ownership) for individual i in year t . Define $\text{HighMTR}_i = 1$ for taxpayers whose *prior-year* taxable income places them in the 20% bracket and $\text{HighMTR}_i = 0$ for those in the 12% bracket. To enhance comparability, we restrict the sample to a symmetric income bandwidth of NTD100,000 around the relevant bracket cutoffs, using lagged taxable income for classification. Our baseline

DiD specification is

$$Y_{it} = \alpha_i + \lambda_t + \beta (\text{Post}_t \times \text{HighMTR}_i) + X'_{it}\delta + \varepsilon_{it},$$

where α_i are individual fixed effects, λ_t are year fixed effects, and X_{it} includes standard controls. Post_t equals one for $t \geq 2018$. Standard errors are clustered at the individual level.

To assess dynamics and pre-trends, we estimate an event-study:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq -1} \beta_k \mathbf{1}\{t - t_0 = k\} \times \text{HighMTR}_i + X'_{it}\delta + \varepsilon_{it},$$

with $t_0 = 2018$ and $k = -1$ as the omitted category. No pre-reform coefficient is individually distinguishable from zero, but we do not read this as a clean pre-trend test: the pre-period point estimates are uniformly positive and not negligible relative to the post-reform ones, so the design is best treated as suggestive corroboration rather than as independent identification. Because the reform is national, the 12% group serves as a *partially treated* comparison experiencing a smaller policy shock than the 20% group. This attenuation implies our DiD estimates are conservative: relative to a never-treated counterfactual, the true effects would be (weakly) larger in magnitude.

We classify MTR using prior-year taxable income to mitigate endogenous bracket movement. The estimation for this design is carried out within the Fiscal Information Agency's secure environment; bandwidth sensitivity ($\pm 50k$, $\pm 150k$) and alternative fixed-effects structures (county-year or cohort-by-year) were examined there and reported to be stable, though those variants are not reproduced in the replication materials accompanying this paper.

C.4 Border Sample as Wang and Yang (2022)

One concern with a design based on county-specific bonuses is that households may choose where to register their residence in order to claim a more generous bonus, without any real change in fertility. In that case the bonus would raise the number of births *registered* in a county without raising the number of births that occur.

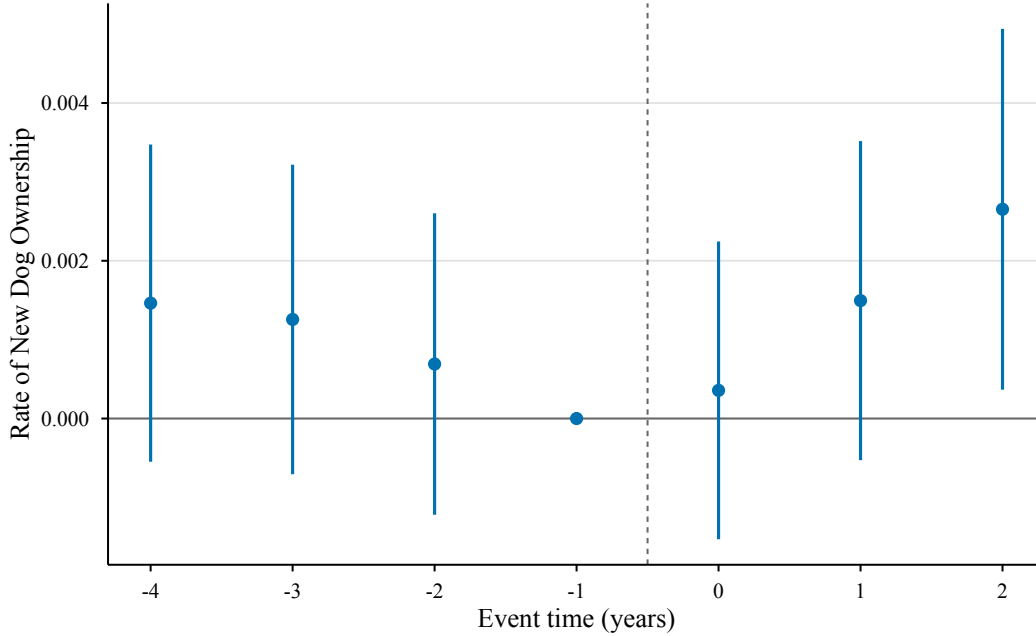


Figure 15: Tax-Deductible Expansion: Event-Study/DiD Estimates

Note: Event-time coefficients on HighMTR_i interacted with year, with $k = -1$ omitted; the comparison group is taxpayers in the 12% marginal-rate bracket, who are partially treated by the same national reform. The outcome is the rate of new dog ownership. All estimated coefficients are positive, and the post-reform path rises from +0.04 percentage points at $k = 0$ to +0.27 at $k = 2$. Because the 2018 expansion lowered the effective cost of a child by more for the high-rate group, a positive dog-ownership response is the sign predicted by complementarity. Mapping this design to an elasticity is less direct than in the bonus analysis.

To address this concern, Wang and Yang (2022) restrict attention to non-migrants in border townships, where the incentive to re-register is strongest but the population is otherwise comparable across the boundary. We follow their approach. Some of their covariates are unavailable in our data, so we cannot replicate their specification exactly; Table 3 reports the closest version we can estimate. Restricting to border townships cuts the sample substantially and with it the precision of the estimates, none of which is individually significant. The point estimates nonetheless retain the sign pattern of the main results, with the childbirth and pet responses moving in the same direction.

C.5 Bonus Effects on Children

We next estimate the effect of the child bonus on childbearing itself. We re-estimate Equation 2 with childbirth as the outcome; the estimator, covariates, comparison group, and clustering are

Table 3: Border-Sample Difference-in-Differences as in Wang and Yang (2022)

Dependent Variables: Model:	Child Birth (1)	Has Dog (2)	Has Cat (3)
<i>Variables</i>			
Age ²	−0.010** (0.005)	−0.0004 (0.0006)	−0.0016*** (0.0001)
Asset (NTD million)	0.000883 (0.001120)	−0.000272 (0.000219)	0.000088 (0.000393)
Real Estate	1.630*** (0.200)	0.050** (0.020)	0.090** (0.040)
Treat × Post	0.270 (0.310)	0.100 (0.130)	0.020 (0.100)
<i>Fixed-effects</i>			
Individual	Yes	Yes	Yes
Year	Yes	Yes	Yes
County	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	2,223,171	2,223,171	2,223,171
R ²	0.161	0.147	0.169
Within R ²	0.0010	0.0000	0.0001

Note: Coefficients are expressed in percentage points; asset holdings are measured per NTD million. Standard errors clustered at the county × year level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

otherwise identical to Section 4. Figure 16 plots the results in the same format as Figure 3.

We cannot reject a zero fertility effect at conventional levels, though the sign of the point estimates matches the small positive effects reported in the previous literature. The response is also slower than the pet response in Figure 3, which is what the biology of the two margins would suggest. We stress that this is a statement about the fertility response to a fertility bonus, and carries no direct implication for the cross-price elasticity of pet demand; that quantity is identified by the pet response in the main text.

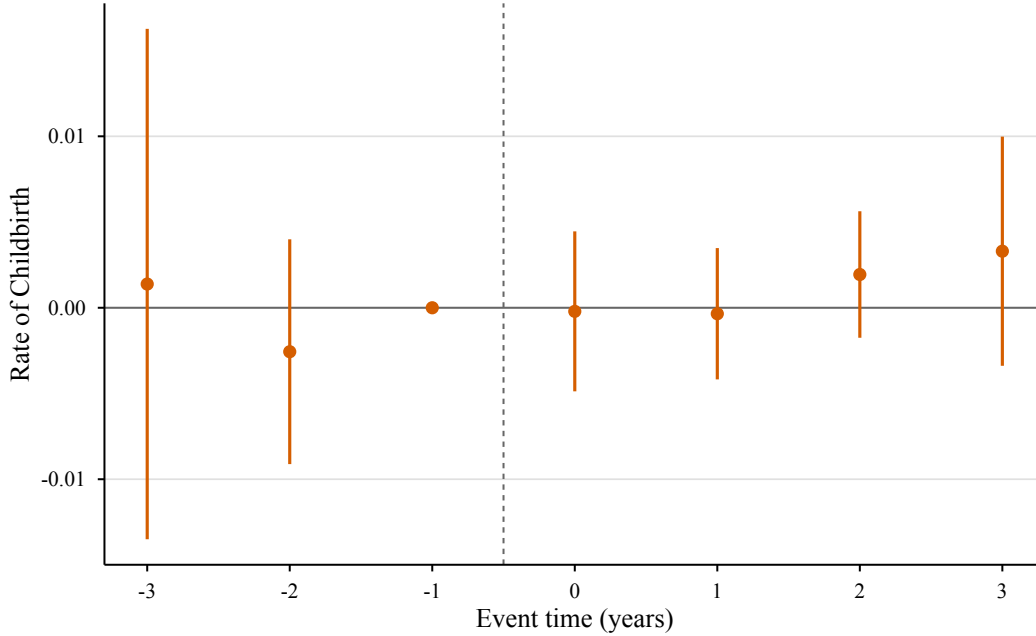


Figure 16: Estimated Effects of Child Bonus on Childbearing

Note: Event-time coefficients for childbearing around the bonus introduction, estimated from Equation 2 with childbirth as the outcome. Standard errors are clustered at the county level. The estimates are positive on average but not individually distinguishable from zero, and are slower to emerge than the corresponding pet response.

C.6 Lottery Effects on Children

We present the results of the lottery on children. We estimate Equation 3 and change the outcome variable into childbirth. Figure 17 plots the results, similar to Figure 4.

C.7 Event-Study Robustness for Dogs: Childbearing on First Dog Ownership

Here we present the results similar to the one in Figure 6 in Section 5. Different from the previous specifications, we restrict the outcome variable to the first time dog ownership in our data instead of any dog ownership in the left panel of Figure 18, and we change the event to first-time dog ownership in the right panel of Figure 18. The pattern is similar to the one above.

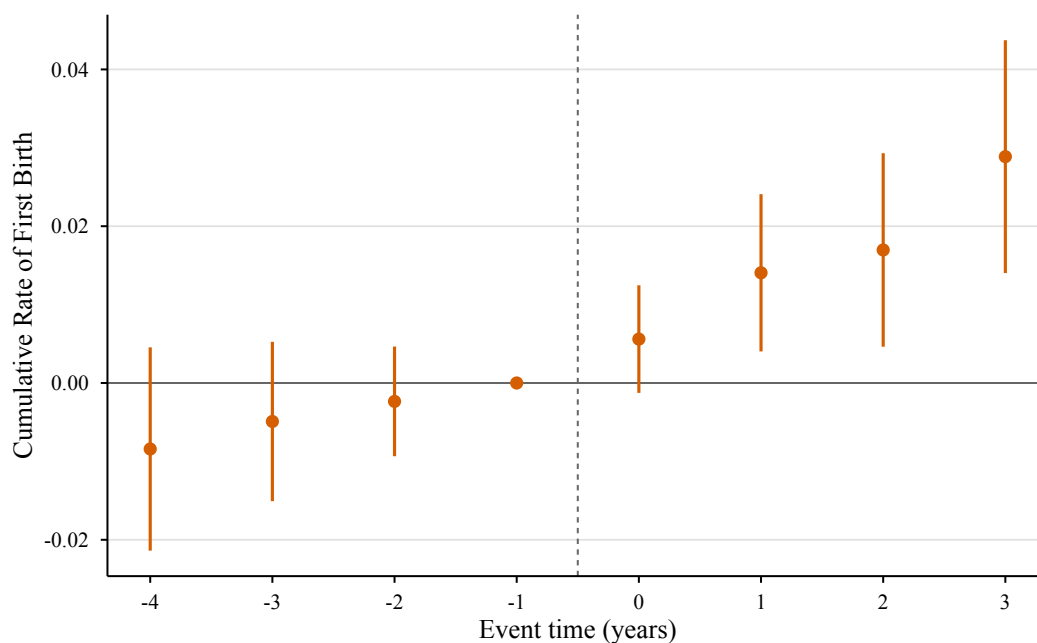


Figure 17: Lottery Shock and Childbearing

Note: Event-study around the lottery event (2×10^6 NTD) shows patterns consistent with the bonus evidence.

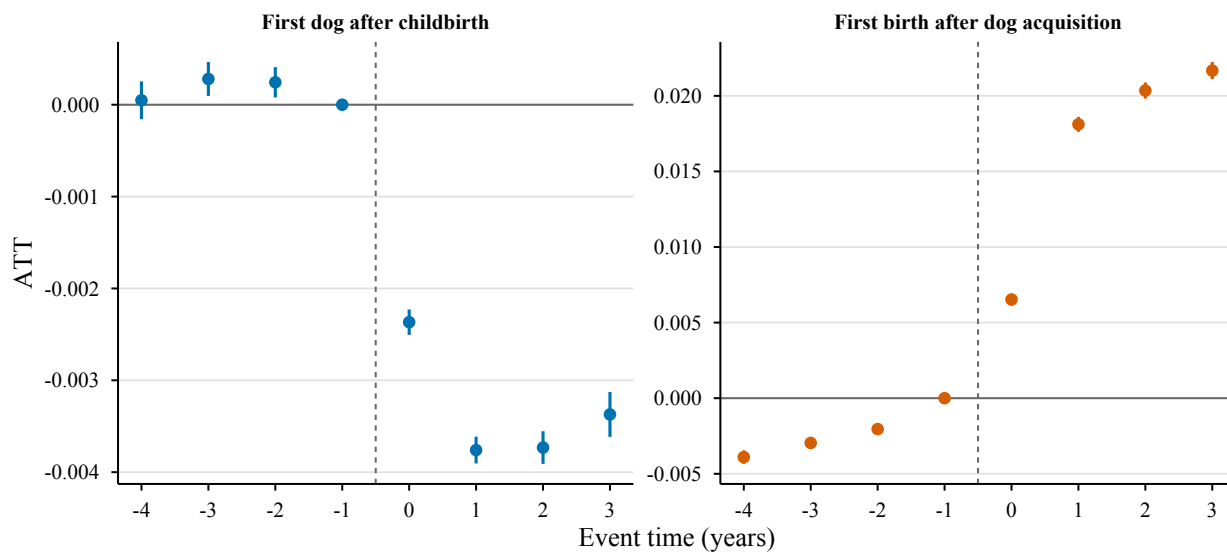


Figure 18: Event Studies Restricted to First-Time Pet and First-Birth Transitions

Note: First dog acquisition around childbirth (left) and first birth around dog acquisition (right), restricting attention to first transitions. Panels use separate vertical scales; standard errors are clustered at the individual level.

C.8 Post-2016 Taipei City Sample

One concern of the measurement with the registry data is that the registration rules may affect the results. To address this concern, we restrict our sample to the Taipei City as Taipei City requires both cats and dogs to register on 2016. As shown in Figures 19, the results remain unchanged for this sample.

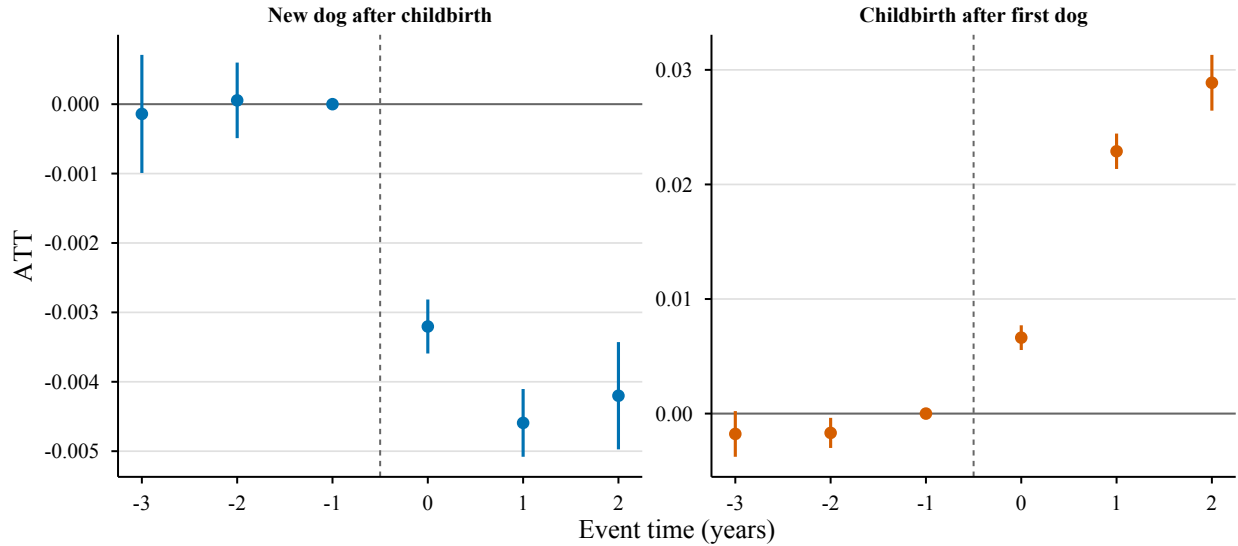


Figure 19: Event Studies Restricted to the Post-2016 Period

Note: Both directions of the main event study re-estimated on the post-2016 sample, when cat registration was required in Taipei City: new dog ownership around childbirth (left) and childbearing around first dog acquisition (right). Panels use separate vertical scales because the outcomes differ; standard errors are clustered at the individual level.

C.9 Intensive Margin: Number of Dogs

Our main analyses focus on the extensive margin, whether an individual owns any dog, using an event-study specification. To assess intensive-margin responses, we re-estimate the event study with the outcome defined as the number of registered dogs. The corresponding figure, Figure 20, shows that the qualitative conclusions remain unchanged.

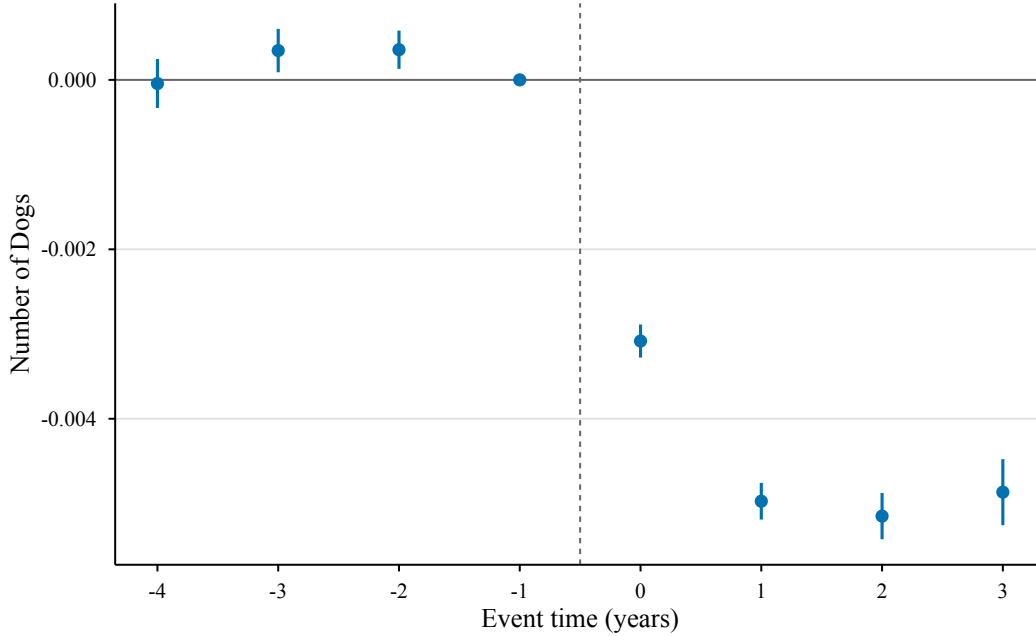


Figure 20: Event Study with Intensive-Margin Outcome: Number of Dogs

Note: This figure plots regression-adjusted event study coefficients where the outcome is the count of registered dogs rather than a binary ownership indicator. The control group consists of later-treated individuals. The x -axis is years relative to the event of interest, and the y -axis shows changes in the expected number of dogs. Standard errors are clustered at the individual level, with 95% confidence intervals shown.

C.10 Household Results

Our baseline analyses use individuals as the unit of observation, which facilitates comparisons among unmarried individuals and allows for rich individual-level controls (e.g., age). In this subsection, we re-estimate the event studies at the household level to gauge the scale and magnitude of responses in a more policy-relevant unit. We consider two complementary events: (i) first dog acquisition and (ii) childbirth within the household. The figures below report regression-adjusted event-time coefficients with later-treated households as the comparison group.

D Four-Way Joint Child–Pet Outcomes

The main analysis estimates the effect of the childbirth-bonus introduction on the marginal flow of new dog ownership. To see *which* joint transition the bonus moves households into, we re-

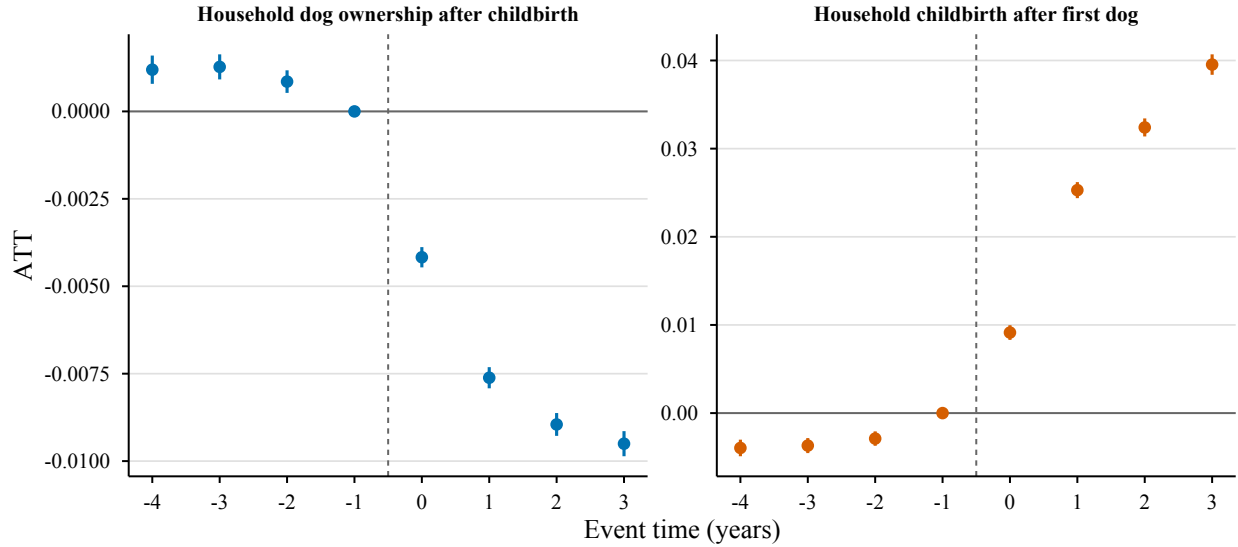


Figure 21: Event Studies at the Household Level

Note: Household dog ownership around childbirth in the household (left) and household childbirth around first dog acquisition (right). Panels use separate vertical scales; standard errors are clustered at the household level.

estimate the event study with the outcome decomposed into four mutually exclusive and exhaustive joint flow cells: Y_1 (new child and new dog this year), Y_2 (new child, no new dog), Y_3 (no new child, new dog), and Y_4 (neither). Figure 22 presents the results for dogs; Figure 23 shows the cat analogue.

The dynamics line up with the pet-then-child sequencing discussed in Section 5. The “no child, dog” flow (Y_3) responds first, rising immediately in the first two post-treatment years, and the joint “child and dog” flow (Y_1) catches up in later years. The “child, no dog” flow (Y_2)—by far the largest of the responding cells—increases throughout the post-treatment window, while the residual “neither” cell (Y_4) declines correspondingly. The decomposition also matters for interpretation: because the earliest dog response occurs among individuals without a contemporaneous birth, it cannot be an income effect of just-received bonus cash (see Section 4.2).

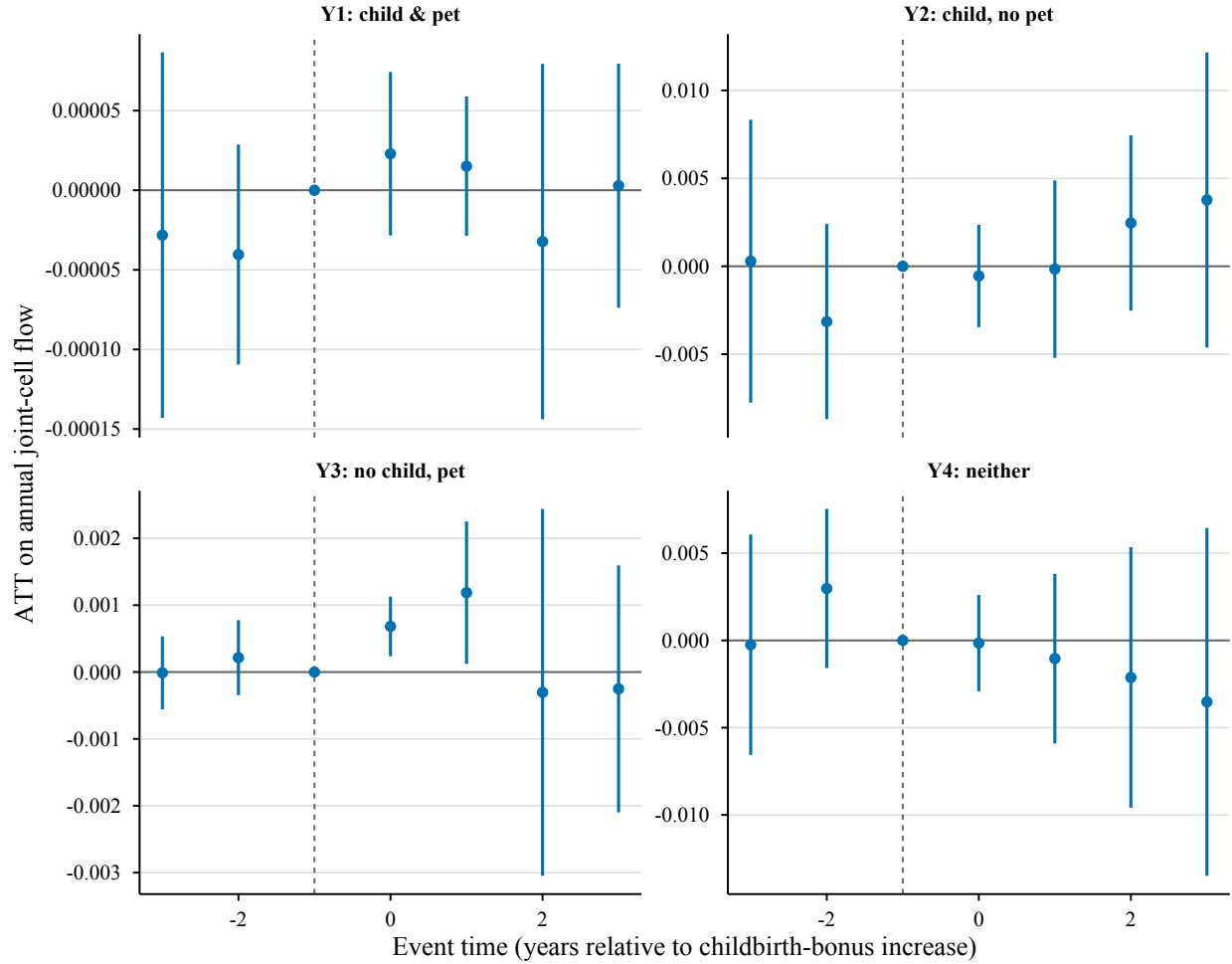


Figure 22: Childbirth-Bonus Effect on Joint Child×Dog Outcomes

Note: Event-study estimates of the effect of the childbirth-bonus introduction on the four mutually exclusive joint child-by-dog annual flow cells. The estimator, controls, and county-level clustering follow the main specification in Equation 2; the reference period is -1 , and error bars denote 95% confidence intervals. Note that the vertical scale differs across panels.

E Results on Cats

We next present results for cats, structured in parallel to the dog analyses. The two headline cat results—the childbirth-bonus and lottery event studies—appear alongside their dog counterparts in Section 4; this appendix collects the remaining cat analyses.

The cross-sectional OLS estimates for cat ownership are reported alongside the dog estimates in Table 2 in the main text (columns 5–8).

Figures 24–25 document the policy and lottery designs, while Figures 26–29 show raw event

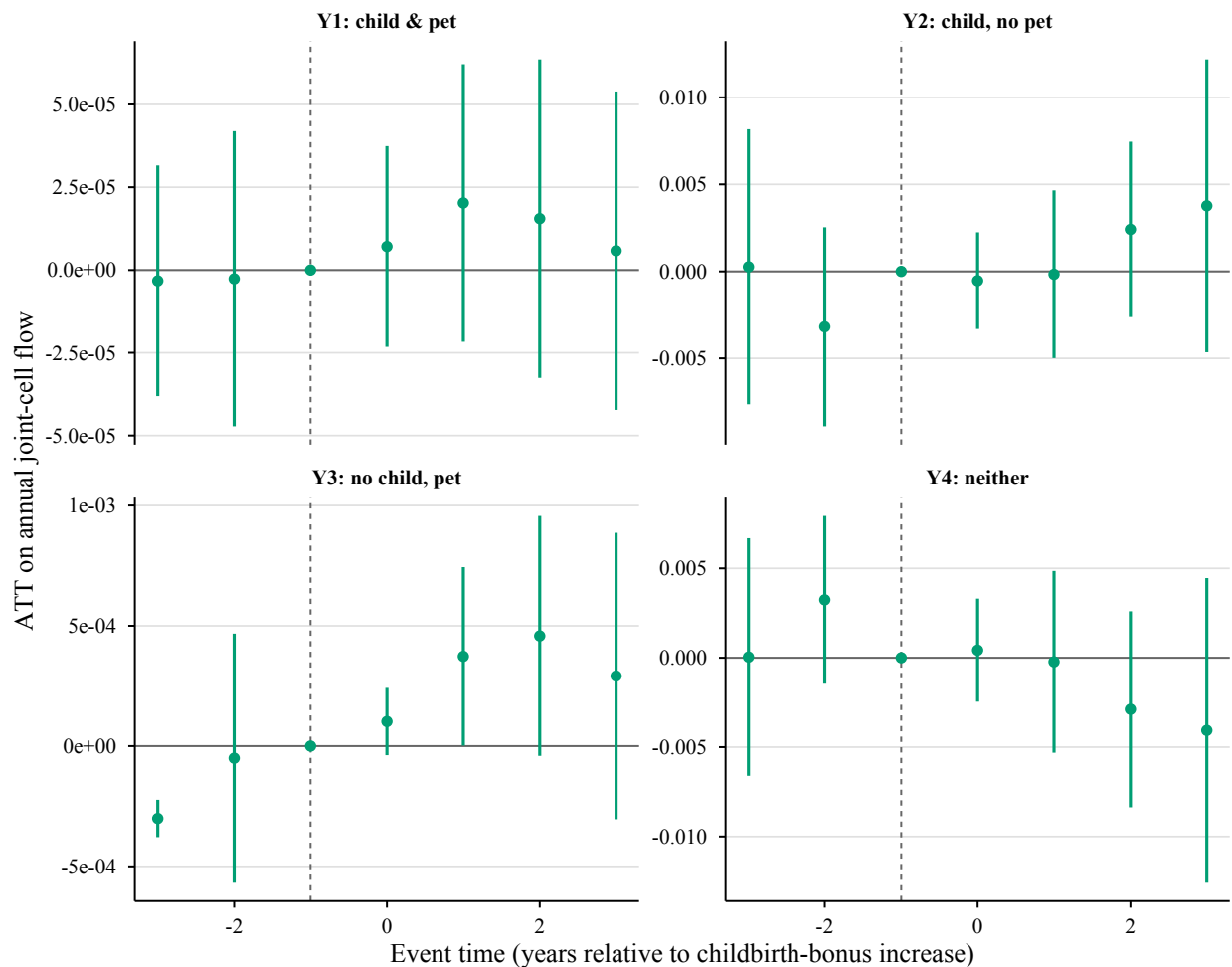


Figure 23: Childbirth-Bonus Effect on Joint Child×Cat Outcomes

Note: Same specification as Figure 22, with the pet leg defined by cat acquisitions.

patterns and event-study estimates for various life events. Overall, the causal estimates for cats resemble those for dogs, even though cats are negatively correlated with children in the cross-section. This contrast is worth stating plainly: the cross-sectional association in Table 2 is negative, whereas the policy and lottery designs recover a positive response of cat acquisition to a reduction in the cost of children. The two are different objects—one is a correlation across households that differ in unobserved ways, the other a response to exogenous variation in price—and only the latter speaks to whether cats and children are complements. The cat estimates are also typically smaller in magnitude than the dog estimates.

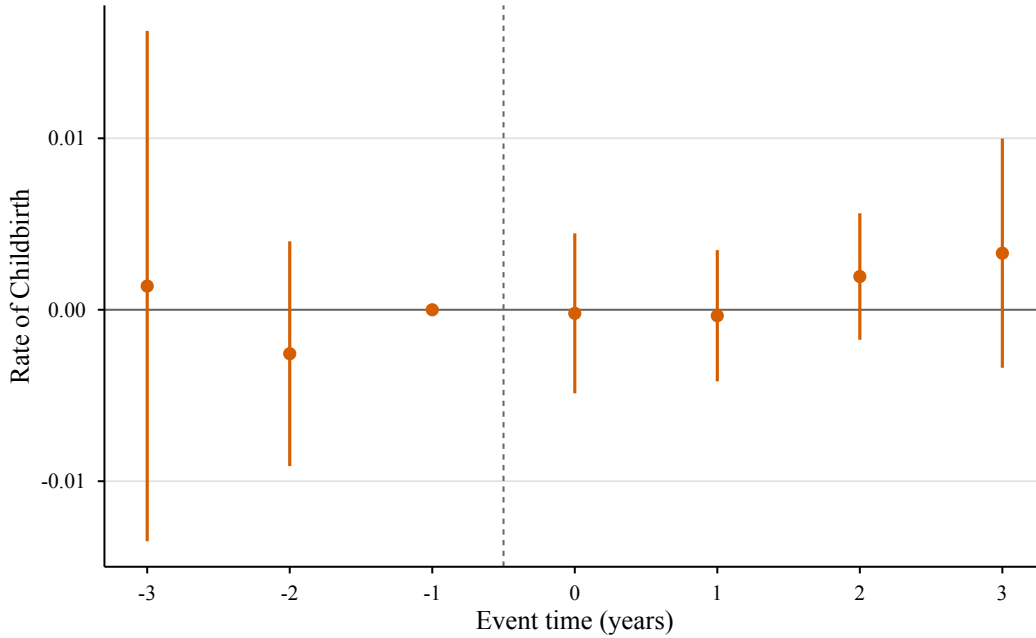


Figure 24: Effect of Childbirth Bonus Increases on Fertility

Note: This figure plots the estimated coefficients from Equation 2 with the indicator variable of having a new child as the outcome variable. The x -axis indicates years relative to the bonus introduction, and the y -axis shows differences in childbearing rates. Standard errors are clustered at the county level, with 95% confidence intervals shown. This serves as a benchmark for comparison with cat ownership responses.

We next turn to raw event-study plots and corresponding regression-adjusted estimates. Figures 26–28 show how cat ownership and childbearing co-move, while Figures 29 examine other major family shocks.

F Outcome on Marriage

We examine how marriage timing evolves around the first acquisition of a dog using a regression-adjusted event-study design. Figure 30 plots cohort-relative coefficients where later-treated individuals serve as the comparison group, and time is measured in years relative to the acquisition year.

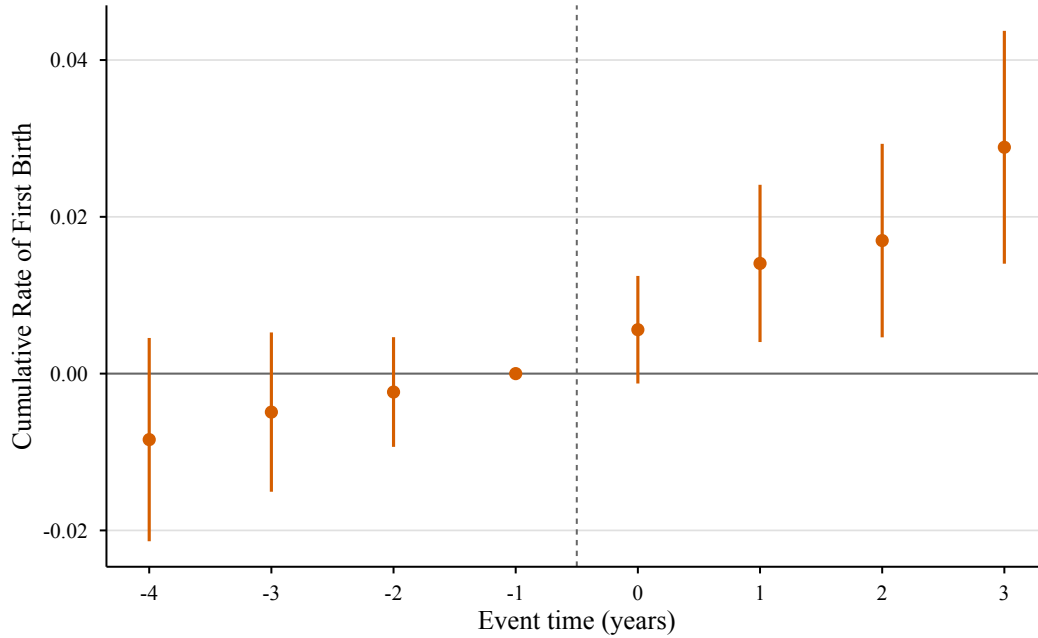


Figure 25: Effect of Lottery Winnings on Fertility

Note: This figure plots the estimated coefficients from Equation 3 with the indicator variable of having a new child as the outcome variable. The x -axis indicates years relative to the lottery winnings, and the y -axis shows differences in childbearing rates. Standard errors are clustered at the individual level, with 95% confidence intervals shown. This provides a direct income shock comparison for fertility outcomes.

G Survey Design and Sample

We fielded an original survey through the NTU Dynamic Polling Platform in two waves in June and July 2026, collecting 4,889 completed responses (1,009 in the first wave and 3,880 in the second), with stratified sampling over pet owners and parents. The questionnaire covers current and past pet ownership and species, acquisition motives and costs, spending on pets and on children, registration status, fertility intentions, and beliefs about the relationship between pets and childbearing.

Because of the stratified design, the sample is not population-representative, and responses are stated preferences. We therefore use the survey throughout the paper as descriptive, complementary evidence—for the expenditure shares that validate the calibration in Appendix H.1, for acquisition motives and species co-ownership, and for stated responses to hypothetical pet subsidies—rather than as a basis for population estimates. Reweighting respondents to the 2023 population margins

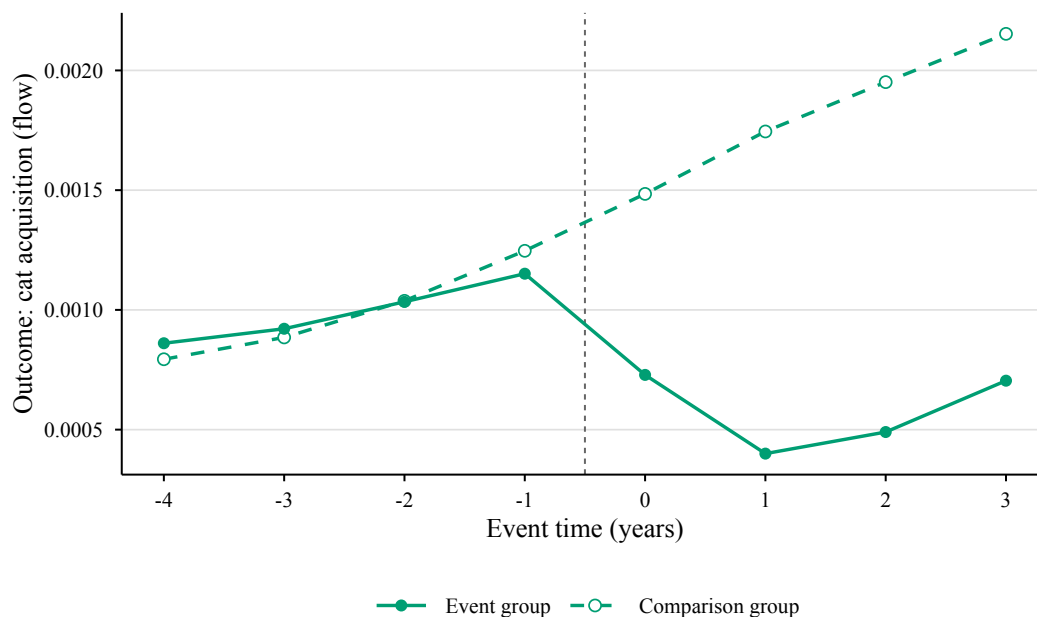


Figure 26: Raw Event Study: Cat Acquisition Around Childbearing

Note: This figure shows raw means of cat acquisition (flow) by event time relative to childbirth. The event group (solid line, filled markers) is aligned on the actual year of childbirth; the comparison group (dashed line, open markers) is assigned a placebo event year drawn at random. Lines are colored by the outcome (green for cat acquisition).

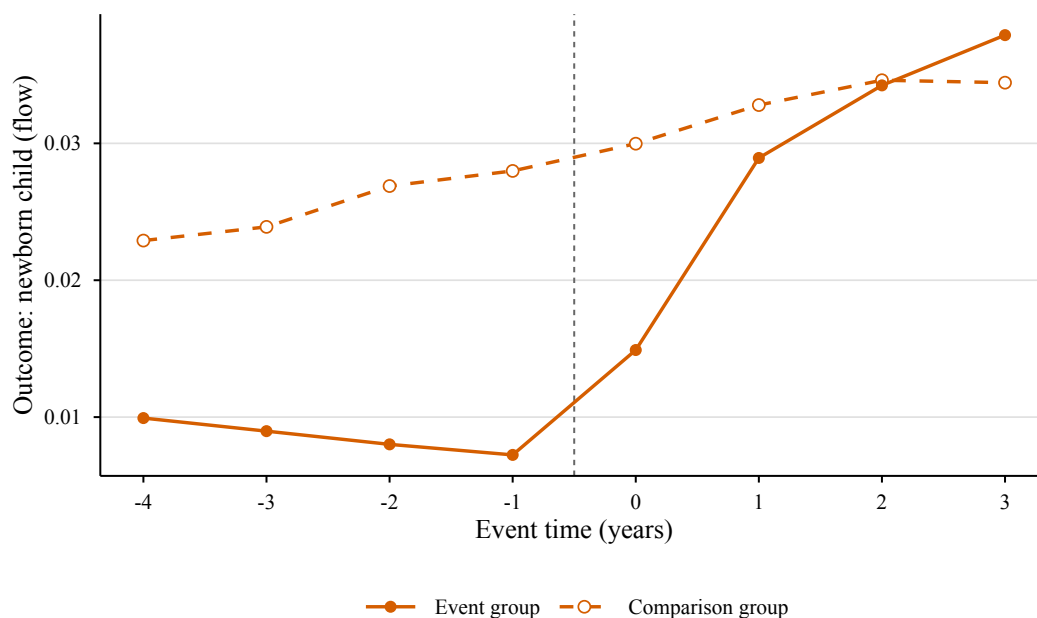


Figure 27: Raw Event Study: Childbearing Around Cat Acquisition

Note: This figure shows raw means of childbearing (flow) by event time relative to first acquiring a cat. The event group (solid line, filled markers) is aligned on the actual acquisition year; the comparison group (dashed line, open markers) is assigned a placebo event year drawn at random. Lines are colored by the outcome (orange for childbearing).

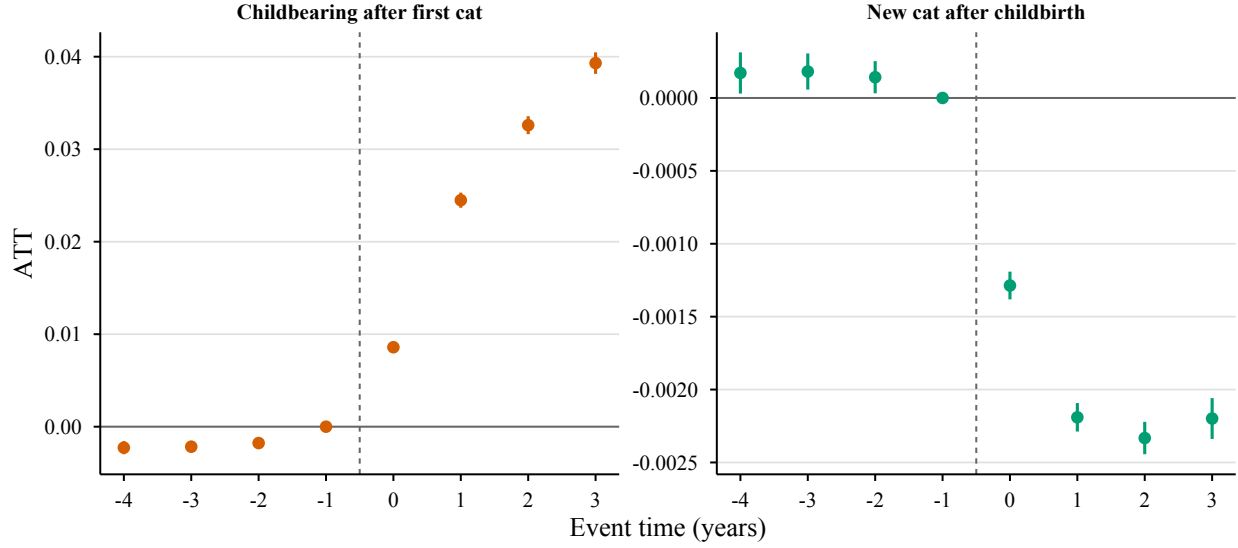


Figure 28: Event Studies for Cats: Both Directions

Note: Childbearing around first cat acquisition (left) and new cat ownership around childbirth (right), the cat counterparts of the dog event studies in the main text. Panels use separate vertical scales; standard errors are clustered at the individual level.

by sex, age, and region, and restricting to the fertile-age population, leaves every survey statistic we cite essentially unchanged or slightly stronger; details are in the replication package.

One belief question is worth recording. Asked whether pet-keeping is “replacing” childbearing in modern society, 67.3% of respondents agree. These are self-reported beliefs about other people’s behavior; on the margins we can measure, the quasi-experimental evidence in the main text points in the opposite direction.

H Other Details

H.1 Expenditure Shares

In order to translate the reduced-form effects into elasticities, we first calculate the expenditure shares of pets and children within household budgets. Our baseline shares are calibrated from sources external to our own survey. For the numerator, we rely on the 2023 Taiwan pet-industry report, which puts average annual expenditures at approximately 30,093 NTD for dog-owning house-

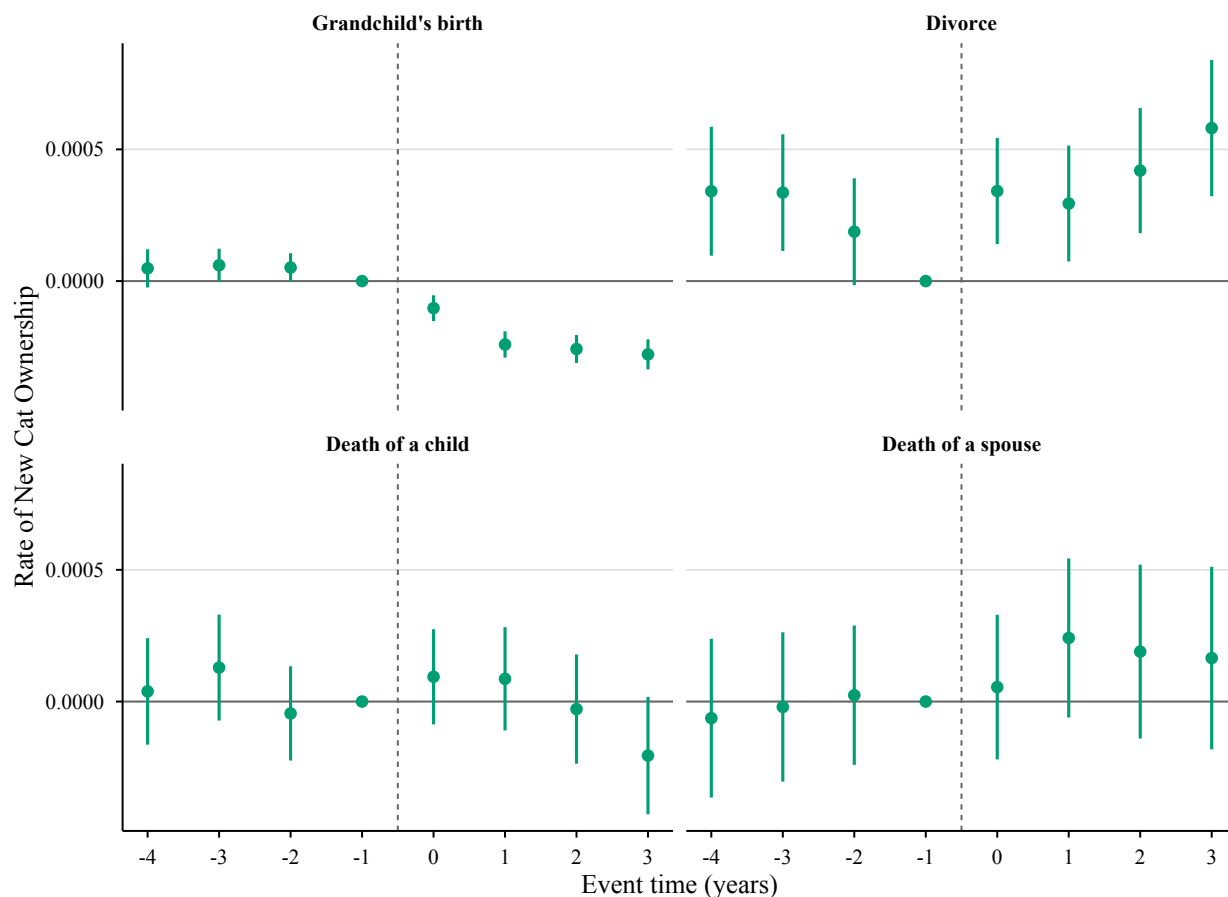


Figure 29: Cat Ownership Around Later-Life Family Events

Note: Event-time coefficients for new cat ownership around the birth of a grandchild and a divorce (top row) and the death of a child and the death of a spouse (bottom row), the cat counterparts of the dog results in the main text. All four panels share a common vertical scale; standard errors are clustered at the individual level.

holds and 24,577 NTD for cat-owning households (Liu et al., 2024). For the denominator, we use the official Family Income and Expenditure Survey (Directorate-General of Budget, Accounting and Statistics, Executive Yuan, 2025), which reports total average consumption of 930,374 NTD for households with an average disposable income of 1,116,195 NTD. Dividing pet expenditures by this consumption measure yields an expenditure share of about 3.23% for dogs. Our own survey provides an independent, measured check on these calibrated values (see Section 4.4 and Table 4); we deliberately keep the external calibration as the baseline so that the survey serves as validation rather than input.

Estimating the resource share devoted to children is more complex, as household consumption

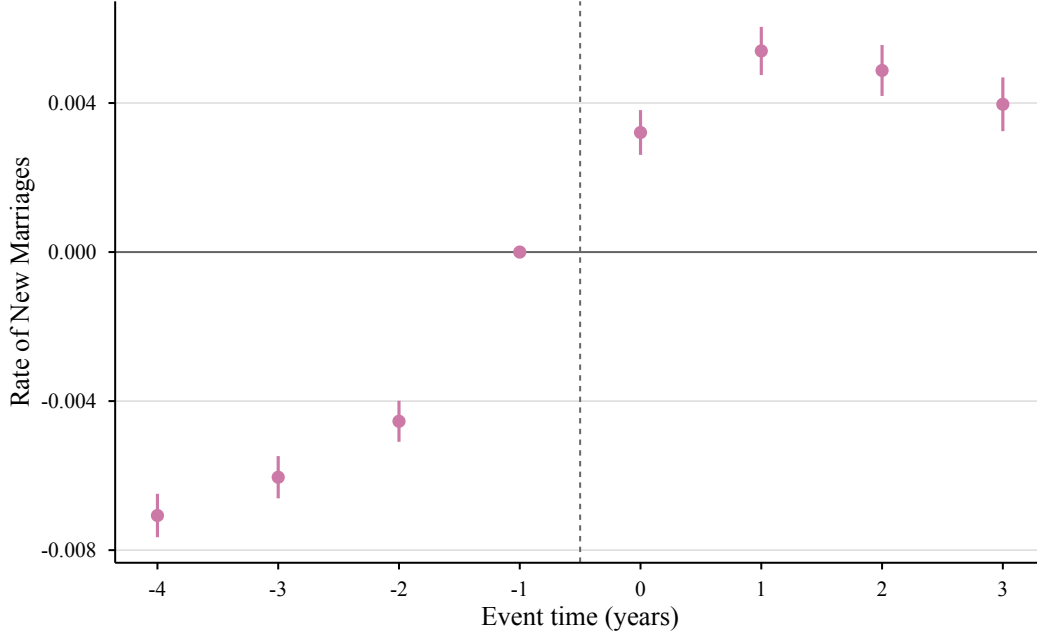


Figure 30: Event Study of Marriage Outcomes Around Dog Ownership

Note: This figure plots regression-adjusted event study coefficients of marriage relative to the year of first dog acquisition. The control group consists of later-treated individuals. The x -axis is years relative to dog acquisition, and the y -axis shows the difference in the probability of marrying. Standard errors are clustered at the individual level, with 95% confidence intervals shown.

on children is only partially captured in reported expenses. We therefore adopt an equivalence-scale approach that translates household composition into consumption weights. Following the conventional scale, a child under age 14 is assigned a weight of 0.3 relative to an adult (OECD, 2024a). In a household with two adults and one child, the implied share is $\frac{0.3}{2.3} \approx 13\%$, or $\frac{0.3}{1.8} \approx 17\%$ if two adults are given unit weight and the child is scaled accordingly.

With these expenditure shares in hand, we then proceed to combine them with the reduced-form estimates of how price variation in one domain affects the quantity demanded in the other. The mapping from reduced-form coefficients to elasticities is the definition of a cross-price elasticity,

$$\epsilon_{yx}^M = \frac{\Delta q_y / q_y}{\Delta p_x / p_x},$$

the proportional change in the quantity of good y divided by the proportional change in the price of good x . Two conventions make this operational in our setting. First, the numerator is the reduced-

form effect expressed relative to the pre-treatment base: the estimated change in the dog acquisition rate divided by that rate. Second, because a child is the natural unit of quantity ($q_c = 1$), the “price” of a child is the household’s total expenditure on children, $p_c = s_{\text{child}} \cdot C$; a childbirth bonus of b therefore represents a proportional price reduction of $\Delta p_c / p_c = b / (s_{\text{child}} C)$.

Applying this to the childbirth-bonus estimates, the event-time-0 effect on the dog acquisition rate is 0.0006663 on a pre-treatment base of 0.0033197, so $\Delta q_d / q_d = 0.2007$, while the NTD30,000 bonus reduces the price of a child by $30,000 / (0.20 \times 930,374) = 0.1612$. The implied Marshallian cross-price elasticity is $0.2007 / (-0.1612) = -1.24$. All inputs here are the unrounded estimates, so this figure reconciles exactly with the 20.1% first-year response reported in Section 4.2.

Finally, we compute the income elasticity of dog ownership from the lottery design. We divide the first-year (event-time-0) response among winners above the NTD2,000,000 threshold—0.139 percentage points on a pre-treatment acquisition rate of 0.587 percentage points among winners, a 23.6% increase—by the proportional income shock, measured as the prize over average household consumption ($2,000,000 / 930,374 \approx 2.15$). This yields $\eta_{\text{dog}} = 0.236 / 2.15 \approx 0.11$. We currently use the threshold value as the prize size; the mean prize conditional on winning is somewhat larger, which would make η_{dog} correspondingly smaller. For the income elasticity of childbearing we adopt $\eta_{\text{child}} = 0.15$ from the lottery-fertility estimates of Tsai et al. (2022), whose design measures the fertility response at horizons over which births can actually occur. Applying our own first-year calculation to childbearing yields approximately 0.045, but we treat this number only as a consistency check and a mechanical lower bound: a birth cannot follow a windfall within the same year, so an event-time-0 fertility response understates the income elasticity by construction—the same gestation-lag logic we invoke for the bonus timing in Section 4.2. Table 4 shows that the implied $\epsilon_{\text{child,dog}}^M$ is in any case insensitive to the choice, moving from -0.202 to -0.199 , and the Hicksian counterpart is algebraically independent of η_{child} altogether. By the Slutsky equation, the

total (Marshallian) price elasticity can then be expressed as

$$\epsilon_{yx}^M = \epsilon_{yx}^H - s_x \cdot \eta_y,$$

where ϵ_{yx}^H is the Hicksian (compensated) elasticity of good y with respect to the price of good x , s_x is the expenditure share of the good whose price changes, and η_y is the income elasticity of demand for the responding good y . This decomposition allows us to assess how sensitive our results are to assumptions about income responsiveness. The combination of expenditure shares, reduced-form coefficients, and income elasticities yields a consistent set of elasticity estimates that can be compared across specifications and data sources.

H.2 Sensitivity of the Implied Elasticity

Because the expenditure shares and income elasticities entering the identity are calibrated rather than estimated jointly, Table 4 reports how the implied $\epsilon_{\text{child,dog}}^M$ varies over every alternative input value that appears in our sources: the calibrated shares, the equivalence-scale variants, the survey-measured medians and means, and the alternative income-elasticity estimates. Two results stand out. First, the implied elasticity is dominated by the ratio $s_{\text{dog}}/s_{\text{child}}$; the income-elasticity gap is second order. Second, and most importantly, $\epsilon_{\text{child,dog}}^M$ remains strictly negative—children and dogs remain complements—for *every* combination of the sourced inputs, with bounds of $[-0.58, -0.13]$.

The lower panel of the table adds sampling uncertainty. Throughout, event-study estimates are aggregated according to the outcome type: for *flow* outcomes (annual acquisitions, as in the bonus design), where the response is concentrated immediately after treatment, we use the first post-treatment year; for *stock* outcomes (ever-ownership, as in the lottery design), where the effect cumulates, we use the average post-treatment ATT with the estimator’s own aggregated standard error. Each elasticity entering the identity is a fixed multiple of an estimated event-study coefficient, so its relative standard error carries over directly: $\epsilon_{\text{dog,child}}^M$ inherits the county-clustered standard error of the first-post-year bonus ATT ($t = 2.4$), and the income elasticities inherit the individual-

clustered standard errors of the post-period average lottery ATTs ($t = 3.3$ for dog ownership and $t = 3.8$ for childbearing). A Monte Carlo that draws these three estimates independently from their sampling distributions, holding the calibrated shares fixed, yields a 95% interval of $[-0.37, -0.04]$; resampling survey respondents to capture sampling error in the survey-measured shares alone yields $[-0.23, -0.17]$; and drawing both sources jointly yields a combined interval of $[-0.35, -0.04]$. The implied elasticity remains negative at the upper end of every interval.

Table 4: Sensitivity of the Implied Cross-Price Elasticity $\epsilon_{\text{child,dog}}^M$

Variation	Input value (source)	$\epsilon_{\text{child,dog}}^M$	$\epsilon_{\text{child,dog}}^H$
Baseline	preferred calibration	-0.202	-0.198
η_{child}	0.045 (own first-year calculation)	-0.199	-0.194
η_{dog}	0.15 (upper alternative)	-0.201	-0.196
s_{dog}	0.0328 (own-survey median)	-0.205	-0.201
s_{dog}	0.0606 (own-survey mean)	-0.380	-0.375
s_{child}	0.13 (equivalence scale 0.3/2.3)	-0.311	-0.306
s_{child}	0.17 (equivalence scale 0.3/1.8)	-0.238	-0.233
s_{child}	0.216 (own-survey median)	-0.188	-0.183
s_{child}	0.22 (U.S. literature)	-0.184	-0.179
s_{child}	0.303 (own-survey mean)	-0.134	-0.129
Grid bound (most negative)	all sourced combinations	-0.583	-0.578
Grid bound (least negative)	all sourced combinations	-0.129	-0.124
CI: survey shares	sampling error in s_d, s_c	[-0.228, -0.167]	—
CI: event-study SEs	sampling error in $\epsilon_{\text{dog,child}}^M, \eta_{\text{child}}, \eta_{\text{dog}}$	[-0.367, -0.037]	—
CI: combined	both sources	[-0.352, -0.035]	—

Note: Calibration rows (upper panel) hold $\epsilon_{\text{dog,child}}^M$ at its point estimate and reflect input-calibration sensitivity, not sampling error. Input sources: s_{dog} — 2023 industry report over FIES consumption (0.032), own-survey median (0.033) and mean (0.061) of annualized pet spending over household income; s_{child} — OECD equivalence scales (0.13, 0.17), paper baseline (0.20), own-survey median (0.216) and mean (0.303), U.S. literature (0.22); η_{dog} — 0.11 (derived from the first-year lottery response; see Appendix H.1) and 0.15 (alternative); η_{child} — 0.15 (lottery-fertility estimate of Tsai et al., 2022) and 0.045 (own first-year calculation). Confidence intervals (lower panel) are Monte Carlo percentile intervals ($B = 20,000$) around the baseline calibration: the survey-share interval resamples respondents and recomputes the share medians; the event-study interval scales each elasticity by normal draws of its underlying first-post-year ATT— $\epsilon_{\text{dog,child}}^M$ from the bonus event study (county-clustered SE), η_{dog} and η_{child} from the lottery event studies (individual-clustered SEs)—treated as independent across the three designs; the combined interval draws both sources jointly. Baseline calibration: $s_d = 0.032$, $s_c = 0.20$, $\eta_{\text{dog}} = 0.11$, $\eta_{\text{child}} = 0.15$. Confidence intervals are 95%. $\epsilon_{\text{child,dog}}^M$ remains negative in every calibration and at the upper end of the combined interval.

I Effect Heterogeneity

In this section we present the event study results by sex and by house ownership status. The results correspond to Figure 6 in the main text.

Table 5 summarizes the first-post-year estimates across subsamples. Panel A reports both directions of the main event studies by sex, birth cohort, and homeownership. Panel B reports the childbirth-bonus response by baseline parity: the bonus-induced increase in dog acquisition is concentrated entirely among individuals without children at baseline, with no response among existing parents.

Table 5: Heterogeneity of the Main Event-Study Estimates (First Post-Treatment Year)

	New dog after first childbirth		Childbearing after first dog	
	ATT (pp)	(SE)	ATT (pp)	(SE)
<i>Panel A: Demographics and housing</i>				
Full sample	-0.27***	(0.01)	0.71***	(0.02)
Male	-0.20***	(0.01)	0.77***	(0.04)
Female	-0.34***	(0.01)	0.67***	(0.03)
Older cohort	-0.19***	(0.01)	0.36***	(0.02)
Younger cohort	-0.31***	(0.01)	1.27***	(0.05)
Homeowner	-0.23***	(0.02)	0.40***	(0.03)
Non-homeowner	-0.27***	(0.01)	0.78***	(0.04)
<i>Panel B: Childbirth-bonus effect on new dog ownership, by baseline parity</i>				
No child at baseline	0.07***	(0.03)	—	
Has child at baseline	-0.01	(0.07)	—	

Note: Each cell reports the event-time-0 (first post-treatment year) ATT, in percentage points of the annual flow, from the corresponding later-treated event-study specification with covariates (gender, birth year; the gender splits control for birth year only); standard errors in parentheses are clustered at the individual level (Panel A) or the county level (Panel B). Columns in Panel A correspond to the designs in Figures 6 and 6. Panel B re-estimates the childbirth-bonus event study (Equation 2) separately by whether the individual has any child before the bonus increase. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I.1 Heterogeneity by Sex

We first present the results by sex. Figure 31 reports the event study of childbirth on dog acquisition for men and women side by side. Figure 32 reports the event study of dog acquisition on childbearing for men and women side by side.

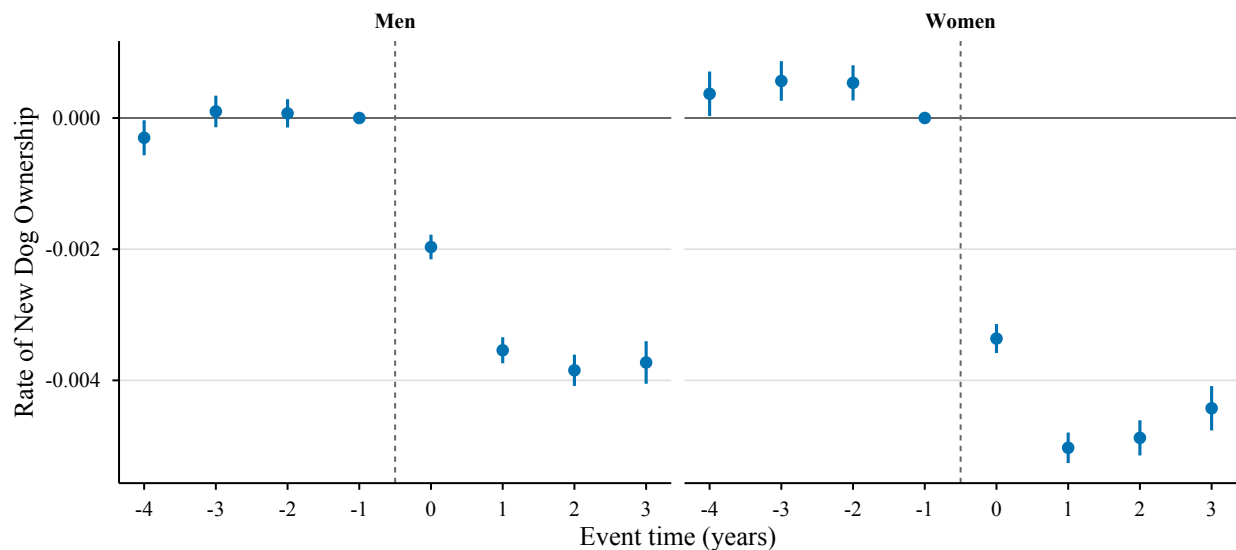


Figure 31: Heterogeneity by Sex: Dog Acquisition Around Childbirth

Note: Event-time coefficients for the probability of acquiring a new dog, estimated separately for men (left) and women (right) on a common vertical scale. Later-treated individuals within each group serve as the comparison group; standard errors are clustered at the individual level.

I.2 Heterogeneity by Homeownership

We also assess heterogeneity by homeownership status at baseline. Figure 33 presents event-study coefficients for dog acquisition around childbirth for non-owners (estate0) and homeowners (estate1), respectively. Figure 34 presents the corresponding coefficients for childbearing around first dog acquisition in the same housing strata.

In each panel, event time is centered on the relevant event and effects are shown relative to the omitted pre-event period. The comparison group consists of later-treated units within the same ownership stratum, and standard errors are clustered at the individual level. Together these figures

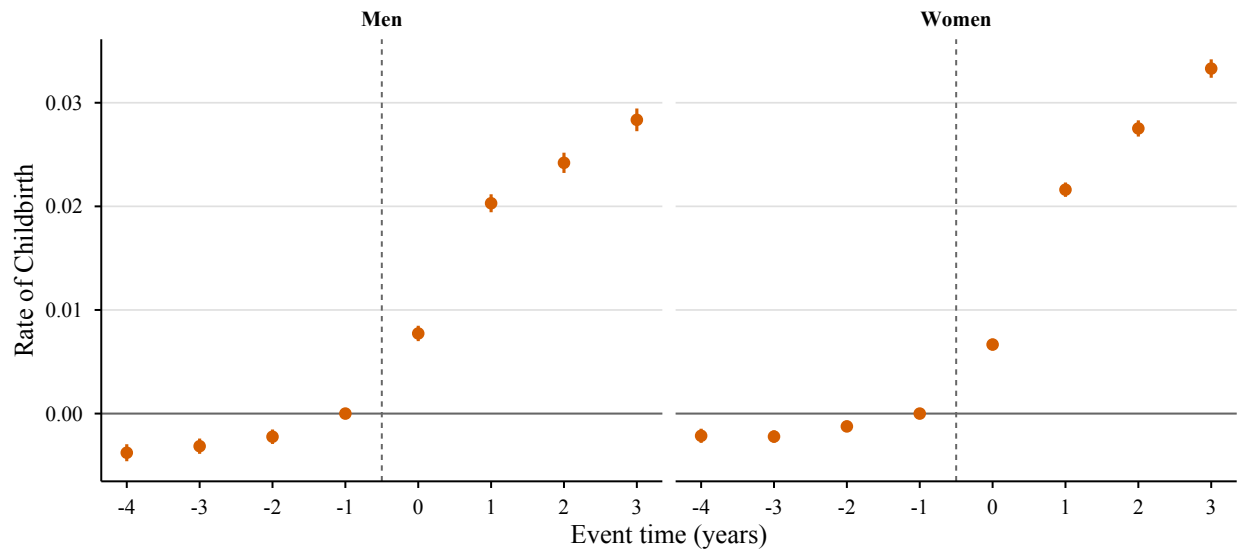


Figure 32: Heterogeneity by Sex: Childbearing Around First Dog Acquisition

Note: Event-time coefficients for the probability of childbirth, estimated separately for men (left) and women (right) on a common vertical scale. Later-treated individuals within each group serve as the comparison group; standard errors are clustered at the individual level.

facilitate side-by-side comparisons across non-owners and owners while keeping identification and scaling consistent.

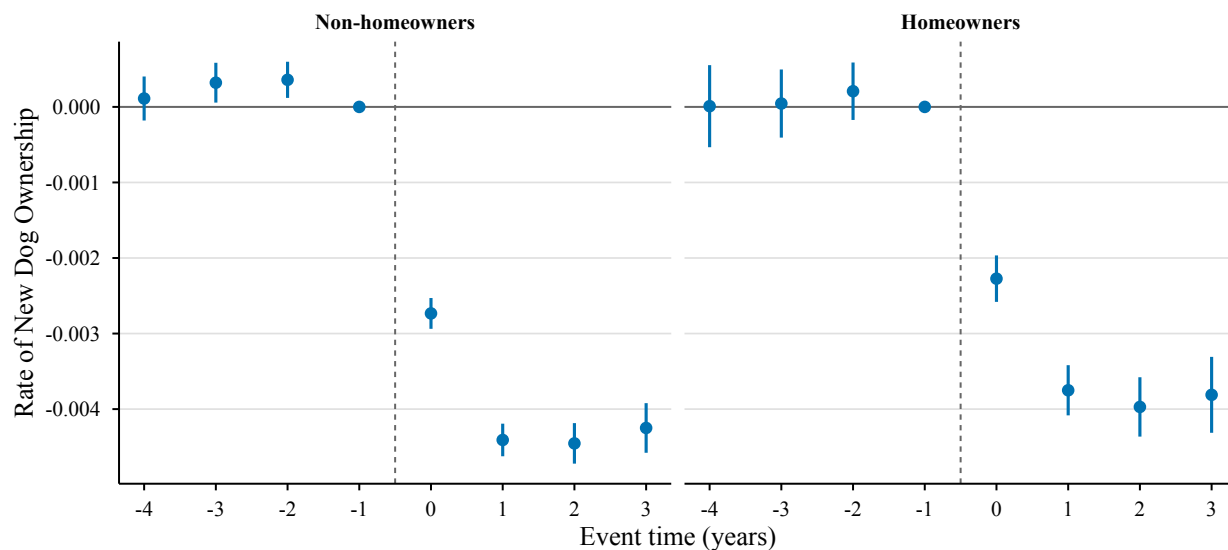


Figure 33: Heterogeneity by Homeownership: Dog Acquisition Around Childbirth

Note: Event-time coefficients for the probability of acquiring a new dog, estimated separately for non-homeowners (left) and homeowners (right) on a common vertical scale. Later-treated individuals within the same ownership stratum serve as the comparison group; standard errors are clustered at the individual level.

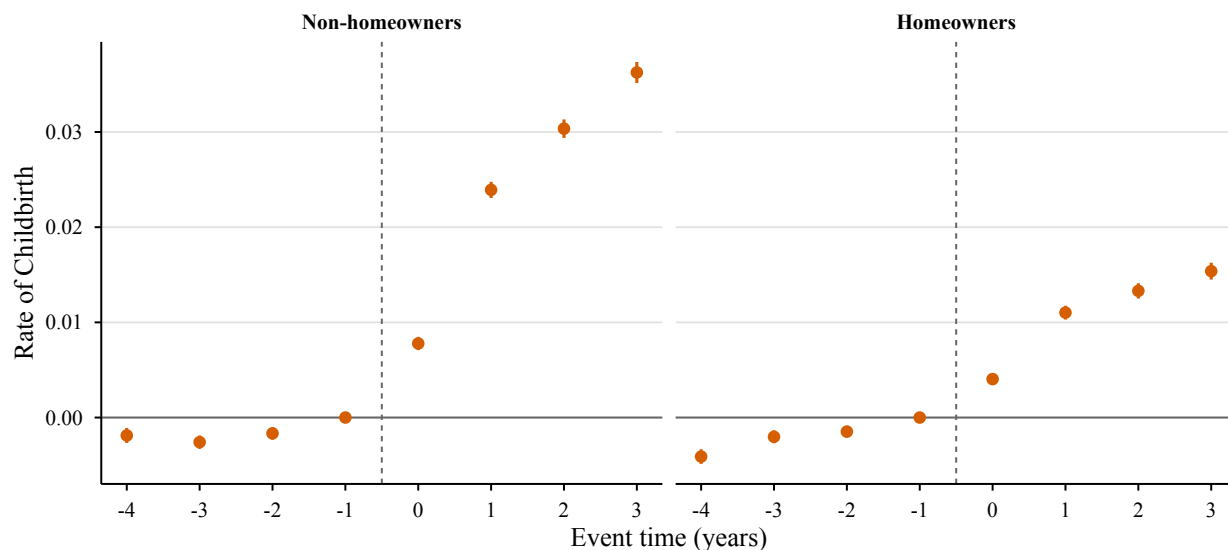


Figure 34: Heterogeneity by Homeownership: Childbearing Around First Dog Acquisition

Note: Event-time coefficients for the probability of childbirth, estimated separately for non-homeowners (left) and homeowners (right) on a common vertical scale; standard errors are clustered at the individual level.

J A Three-Period Framework for Pets, Children, and Learning

This appendix presents a short organizational framework for the empirical patterns in the paper. Its purpose is not to estimate a structural life-cycle model or to derive sharp quantitative predictions. Rather, it shows how three forces can coexist within one internally consistent decision problem: pets and children compete for money and time; caring for a first pet can reveal information about preferences for childrearing; and households differ in their underlying tastes for pets and children. Together, these forces can explain why dog ownership and childbearing are positively associated in the cross-section, why dog acquisition can precede childbearing, and why childbirth can reduce subsequent pet acquisition.

J.1 Environment and timing

Households live for three decision periods, $t \in \{0, 1, 2\}$, interpreted as early adulthood, the main family-formation period, and later adulthood. A household has permanent income Y , species-specific pet tastes (α_D, α_K) , and an initially uncertain taste for children θ . The household knows (Y, α_D, α_K) and begins with a conditional prior

$$\theta \mid Y, \alpha_D, \alpha_K \sim \mathcal{N}(\mu_0, \sigma_0^2).$$

Allowing the prior mean μ_0 to vary with income and pet tastes permits selection: households that are especially oriented toward family life may value both dogs and children, even in the absence of a causal effect of one on the other.

At the beginning of period t , the state is

$$s_t = (D_t, K_t, C_t, \mu_t, \sigma_t^2; Y, \alpha_D, \alpha_K),$$

where D_t , K_t , and C_t are the stocks of dogs, cats, and children already in the household, and (μ_t, σ_t^2)

summarize beliefs about θ . The household chooses

$$a_t = (d_t, k_t, c_t) \in \{0, 1\}^3,$$

where each coordinate indicates whether one dog, cat, or child is added in the current period. Post-decision stocks are

$$D_t^+ = D_t + d_t, \quad K_t^+ = K_t + k_t, \quad C_t^+ = C_t + c_t.$$

Not every bundle is feasible at every state. Let $\bar{D}$, $\bar{K}$, and $\bar{C}$ denote capacity bounds. Given a candidate bundle $a = (d, k, c)$, material consumption and leisure are

$$x_t(s_t, a) = Y - p_D d - p_K k - p_C c - m_D D_t^+ - m_K K_t^+ - m_C C_t^+, \quad (4)$$

$$\ell_t(s_t, a) = L - \tau_D D_t^+ - \tau_K K_t^+ - \tau_C C_t^+ - \phi_D D_t^+ C_t^+ - \phi_K K_t^+ C_t^+. \quad (5)$$

The state-dependent feasible action set is therefore

$$\mathcal{A}(s_t) = \left\{ a \in \{0, 1\}^3 : \begin{array}{l} D_t + d \leq \bar{D}, K_t + k \leq \bar{K}, C_t + c \leq \bar{C}, \\ x_t(s_t, a) > 0, \ell_t(s_t, a) > 0 \end{array} \right\}. \quad (6)$$

Thus low-resource households or households with large existing care burdens need not be able to choose every nominal bundle.

Expected current-period utility from a feasible bundle is

$$r_t(s_t, a) = u(x_t(s_t, a), \ell_t(s_t, a)) + \alpha_D D_t^+ + \alpha_K K_t^+ + (\mu_t + \kappa Y) C_t^+, \quad (7)$$

where u is increasing and concave and $\kappa \geq 0$. The interaction parameters $\phi_D, \phi_K \geq 0$ capture additional time pressure when pets and children live in the same household. The empirically motivated ordering $\phi_K \geq \phi_D$ permits cats to be harder to combine with children than dogs are, while

still recognizing that both species use time and money.

After the action is chosen, any signal generated by a first pet or first child is realized. Stocks then transition according to $D_{t+1} = D_t^+$, $K_{t+1} = K_t^+$, and $C_{t+1} = C_t^+$, and the updated beliefs become part of the next period's state. This timing means that a signal cannot change a child decision made simultaneously in the same period; it can affect only later fertility decisions.

J.2 Learning from first pets, including simultaneous signals

If a household with no previous dog chooses $d_t = 1$, it observes

$$z_D = \theta + v_D, \quad v_D \sim \mathcal{N}(0, \omega_D^2).$$

Likewise, a first cat generates

$$z_K = \theta + v_K, \quad v_K \sim \mathcal{N}(0, \omega_K^2).$$

We allow a dog to be more informative about the household's realized payoff from childrearing,

$$\omega_D^2 < \omega_K^2.$$

Experiencing a first child can be still more informative. When a household with $C_t = 0$ chooses $c_t = 1$, it observes $z_C = \theta + v_C$, with $v_C \sim \mathcal{N}(0, \omega_C^2)$; we assume $\omega_C^2 \leq \omega_D^2$. This captures the idea that direct parenting experience resolves much of the uncertainty that a pet could otherwise help reveal. All three signal noises are independent conditional on θ .

Let $J_t(s_t, a) \subseteq \{D, K, C\}$ be the set of first-time experiences generated by the current bundle. If the household adopts its first dog and first cat in the same period—or combines a first pet with a first child—all signals enter one joint Bayesian update. Posterior precision and the posterior mean

are

$$\frac{1}{\sigma_{t+1}^2} = \frac{1}{\sigma_t^2} + \sum_{P \in J_t(s_t, a)} \frac{1}{\omega_P^2}, \quad (8)$$

$$\mu_{t+1} = \sigma_{t+1}^2 \left[\frac{\mu_t}{\sigma_t^2} + \sum_{P \in J_t(s_t, a)} \frac{z_P}{\omega_P^2} \right]. \quad (9)$$

When $J_t(s_t, a)$ is empty, beliefs remain unchanged. These formulas avoid treating the values of simultaneous signals as mechanically additive: each signal is evaluated together with the others through their combined precision.

The information is valuable because the household can condition later fertility choices on the posterior. Holding the physical consequences of pet ownership fixed, a more informative signal has weakly greater value in the standard Blackwell sense: the household can always ignore information but may benefit when it changes a later decision. This information value is largest when a future fertility choice remains genuinely uncertain. It is zero in period 2 because there is no later decision in this three-period framework.

J.3 The three-period decision problem

Let $\delta \in (0, 1)$ and set $V_3 \equiv 0$. For $t = 0, 1, 2$, the household's finite-horizon problem can be written compactly as

$$V_t(s_t) = \max_{a \in \mathcal{A}(s_t)} \{r_t(s_t, a) + \delta \mathbb{E}[V_{t+1}(s_{t+1}) \mid s_t, a]\}. \quad (10)$$

The expectation integrates over every signal generated by the current bundle and hence over next period's beliefs.

Equation (10) makes the limited dynamic content explicit. A pet adopted today affects later utility through information, durable benefits, maintenance, time, and interactions with future children. We use this only to organize mechanisms; its primitives are not estimated, and the implications below are not structural estimates.

J.4 How the framework organizes the empirical patterns

Dogs before children. A first dog can be attractive in period 0 partly because it generates information useful for a fertility decision in period 1 or 2. A household that receives a favorable signal raises its posterior mean μ_{t+1} and becomes more likely to choose a child later. This produces a dog-then-child sequence without requiring every household to have adopted the dog expressly as practice. It also explains why the empirical response is concentrated among people without prior children: direct parenting experience is a more precise signal and leaves less uncertainty for a later pet to resolve.

Childbirth followed by fewer pet acquisitions. Once a child is present, a new pet requires resources in a state with lower available consumption and leisure. In equation (5), the marginal time cost of a dog rises with the child stock through $\phi_D D_t^+ C_t^+$, and analogously for cats. In addition, a household that has already learned through parenting receives less informational value from a first pet. These forces can reduce pet acquisition after childbirth even when dogs and prospective childbearing are complements earlier in the family-formation process.

Why a reduction in child costs can increase dog acquisition. A lower effective child price raises the value of future paths that include a child. For households near the fertility margin, this can also raise the value of acquiring a dog before the fertility decision, because the dog supplies information relevant to a now more attainable childbearing choice. The sign is not imposed: the positive continuation-value channel competes with the ordinary resource and time costs of keeping both a pet and a child. The childbirth-bonus estimates in the paper indicate which force dominates at the observed margin.

Dogs versus cats. The species ordering can arise from two differences. A dog signal is more precise ($\omega_D^2 < \omega_K^2$), while the joint time burden may be greater for cats ($\phi_K \geq \phi_D$). Thus the learning channel can be stronger for dogs and the crowd-out channel stronger for cats. The framework does not require dog-child and cat-child associations to have opposite signs in every population; it pre-

dicts that species differences depend on the balance of information, direct utility, and resource costs. In our cross-section the two associations do in fact have opposite signs (Tables 2 and 2), while both species respond positively to the childbirth bonus—a configuration the framework accommodates, since the cross-sectional association and the causal price response are different objects.

Cross-sectional selection. Because the initial belief μ_0 may be correlated with α_D and α_K , observed pet-child correlations need not equal causal effects. A positive dog-child association can reflect both selection into family-oriented households and the dynamic learning channel. This is why the paper uses the policy variation, rather than the cross-sectional regressions of Section 3, to study changes in relative costs.

J.5 Relation to the elasticity calculation

The Slutsky calculation in Section 4.4 is a static demand-system benchmark that maps the estimated response in one direction into a reverse-direction elasticity using expenditure shares and income elasticities. The framework in this appendix serves a different purpose: it organizes timing, learning, and heterogeneity and is not used to estimate the Slutsky inputs. With state dependence, exact Slutsky symmetry need not hold: the response to a price change can contain both a current resource effect and a continuation-value effect, and symmetry is restored when the latter vanishes. Accordingly, the mapped elasticity is best read as a local, static-equivalent summary of the policy response rather than as an estimate of the primitive parameters in equations (4)–(10). When dynamic learning is small at the relevant margin, the static benchmark is the natural approximation; when it is important, the framework clarifies the additional mechanism included in the observed reduced-form response. The survey evidence in Section 5—in particular, that over a third of owners report the caregiving experience revealed decision-relevant information about themselves or their partner—suggests the learning channel is present at a nontrivial share of the margin, which is why we read the mapped elasticity as a policy-relevant total response rather than a preference parameter.