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ORIGINAL RESEARCH

The Causal Effect of Pet Ownership on Health and Well-being

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Abstract

Despite a large strand of literature investigating how pet ownership is associated with different health and well-being outcomes, almost all studies are cross-sectional and use non-random samples. Using Australian longitudinal data and leveraging a policy change that allowed renters in the state of Victoria to get pets but did not apply to renters in other parts of the country, we estimate the causal effect of obtaining a pet on life satisfaction, loneliness, mental health, and general health. Using the stability-controlled quasi-experiment (SCQE) estimator that provides partial identification within a quasi-experimental study, under plausible assumptions we find no significant effects of pet ownership on any of the outcomes considered. To obtain a significant effect, one would require underlying trends in the outcomes to be order of magnitudes greater than those observed in benchmark groups: renters in unaffected states and non-renters in Victoria. The null effects from our causal identification contradict existing work, based primarily on selective cross-section data, as well as the general belief that pets improve individual well-being.

Keywords Well-being · Health · Pets · Pet ownership · Australia

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Introduction

For many individuals, pets are an integral part of daily life. In the US, Australia, and the UK, more than 60% of households have at least one pet (Applebaum et al., 2023; Wilkins et al., 2025; UKPF, 2025). The belief that pet ownership enhances well-being is widespread, with organizations such as the American Heart Association stating that having a pet "helps you have a happier, healthier mind" (American Heart Association, 2024). Veterinary clinics, rescue shelters, and social media communities frequently promote the emotional and physical benefits of pet ownership. This sentiment has been encapsulated by American cartoonist Charles M. Schulz, who famously wrote, "Happiness is a warm puppy."

Pets are often considered "social facilitators" (Rijken & van Beek, 2011) because they enable casual interactions between individuals, particularly other pet owners. They are also considered "social resources" because they provide comfort, companionship, and emotional support to their owners (Damberg & Frombling, 2022). However, empirical findings on the relationship between pet ownership and well-being remain mixed. Some studies report positive associations (Headey & Grabka, 2007; Utz, 2014), while others suggest negative (Rijken & van Beek, 2011) or null effects (Antonacopoulos & Pychyl, 2010). Some research also indicates that health outcomes differ by pet type, with dog owners benefiting more than cat owners (Rijken & van Beek, 2011; Bao & Schreer, 2016; Taniguchi et al., 2024).

Despite widespread perceptions that pets improve well-being, empirical research on this question remains limited. One of the main challenges is the reliance on cross-sectional data, often from small, non-representative samples, which raises concerns about con- founding and generalizability. Few studies have used longitudinal data, with notable exceptions being Headey and Grabka (2007) and Taniguchi et al. (2024). Headey and Grabka (2007) found that lifelong pet owners had fewer doctor visits than non-owners, while Taniguchi et al. (2024) linked pet ownership to lower mortality rates among dog owners. However, no study to date has credibly identified the causal effect of pet ownership on health and well-being outcomes. Our study aims to fill this critical gap using nationally representative household panel data from Australia.

The introduction of tenant protections in the state of Victoria, Australia, provides a unique opportunity to study this question using a quasi-experimental design. In 2020, the state government introduced additional protections for tenants, including the right to own a pet, with landlords unable to unreasonably refuse such requests. This policy change was associated with a substantial increase in pet ownership among renters in Victoria who did not previously own pets (see Table A.4). In 2022, 37% of these renters reported having a pet, compared to 27% of non-renters in Victoria – a statistically significant difference of 10% points. Importantly, as a fundamental placebo test, pet ownership rates were nearly identical (and not significantly different) for non-renters in Victoria (27%) versus non-renters elsewhere (26%), and for renters (29%) versus non-renters outside Victoria (26%) (Table A.4). The introduction of this rental policy at roughly the same time as the first nationally representative data collection on pet ownership allow us to explore causal effects more accurately. We leverage this quasi-experimental setting to examine the down- stream effects of pet ownership on sev-

eral health and well-being outcomes, including life satisfaction, loneliness, general health, and mental health.

More traditional methods of causal identification are inadequate within the context of this policy setting. First, traditional difference-in-differences comparisons are likely to fail: a within-state comparison (renters-vs-owners after Victoria's 2020 rule change) is likely confounded by renter-specific economic shocks that deepened during the COVID- 19 pandemic, while an across-state comparison (Victorian renters-vs-renters elsewhere) is likely affected by Victoria's uniquely severe lockdown. Second, even if such trends were parallel, pet uptake remains a choice - presumably, people adopt a pet in expectation that their well-being would improve as a result. Traditional difference-in-difference designs pool people that do and do not select into pet ownership. In a setting where only part of the "treated" group (renters who can acquire a pet under new regulation) actually gets a pet, difference-in-differences can only recover the Intent-to-Treat (ITT) effect, diluting the result. We care about the effect on "compliers" - people that adopt a pet under the new policy. In our setting, it is 39% for the main specification (renters in Victoria that did not have a pet in 2018 but obtained one by 2022) (Table A.4). However, treating the reform's timing as an instrument does not solve the problem: "time" moves well-being through COVID restrictions and other concurrent tenancy protections, violating the exclusion re- striction and leaving a Wald estimator contaminated. Together, pandemic-driven shocks, tenure-specific selection into treatment, and the exclusion breach mean neither standard difference-in-differences nor IV can deliver a credible causal effect of pets on renter well- being.

To estimate the effects of the increase in pet ownership induced by this policy change, we use the stability-controlled quasi-experiment (SCQE) estimator (Hazlett, 2019). This approach transparently makes assumptions about the difference in average outcomes, be- tween periods, people would experience without the treatment, i.e., a "baseline trend" in the outcome. By doing this, it accounts for non-random selection into pet owner- ship. Difference-in-differences methods assume the existence of pre-specified "treatment" and "control" groups with exactly the same trends in non-treatment potential outcomes. SCQE, by contrast, informs one about the required baseline trends to make the impact of the treatment be significantly positive or negative. It is up to researchers to evaluate if such baseline trends are plausible. Since the true baseline trends cannot be inferred from the data, we examine the trends in two never-treated groups, each of which share different and important traits of the treatment group. Specifically, rely on the trends in health and well-being outcomes for homeowners in Victoria (who were unaffected by the pol- icy), renters in other states, and synthetic renters in other states and territories. Unlike traditional difference-in-differences methods, which assume that treated and untreated groups followed parallel trends, SCQE relaxes this assumption by incorporating potential deviations from parallel trends. Hence, we do not posit that either non-renters in Victoria or renters elsewhere serve as a perfect control group. Yet, the trends observed for these groups could inform us about the direction and approximate magnitude of the true baseline trend for renters in Victoria. In this sense, SCQE functions as a sensitivity analysis, generating causal estimates under varying assumptions about baseline trends – with parallel trends as a special case. By systematically adjusting

these assumptions, SCQE provides a range of plausible causal estimates and clarifies the baseline trends required for a given conclusion to hold.

Overall, we find no discernible effects of pet ownership on life satisfaction, loneliness, mental or general health under plausible assumptions about baseline trends in Victoria. To obtain a significant effect, one would require underlying trends in the outcomes to be order of magnitudes greater than those observed in benchmark groups: renters in un- affected states and non-renters in Victoria. These findings suggest that individuals who acquire a pet primarily for its expected instrumental benefits – rather than out of an intrinsic appreciation for animals – may ultimately be disappointed. This challenges the prevailing assumption that pet ownership universally enhances health and well-being.

Data

We use data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. Conducted since 2001, the HILDA Survey is an annual longitudinal survey of a representative sample of roughly 7,500 households. All household members aged 15 and over are interviewed. We use data from waves 18–22 (2018–2022), with pet ownership measured in waves 18 and 22—the only waves that included questions on pet ownership. In wave 18, pet ownership questions were included in the self-completion questionnaire (SCQ), whereas in wave 22 these questions were elicited in the person-questionnaire (PQ).

Sample Restrictions

We restrict the sample to individuals observed in both 2018 and 2022 who did *not* have a pet in 2018. The restriction to non-pet-owners in 2018 is necessary because our identification strategy exploits the 2020 Victorian rental legislation as a source of exogenous variation in pet ownership (see Sect. "Identification Strategy"). This policy change would only affect renters who did not already have pets—those with pets in 2018 were presumably already in pet- friendly housing or had negotiated permission from their landlord.

Our main analysis focuses on individuals who were Victorian renters in 2020, when the policy took effect (N=495). This definition ensures that all individuals in the policy-exposed group were actually subject to the new legislation at the time it was enacted. Some of these individuals (14%) were not Victorian renters in 2018, meaning their base- line outcomes were measured in different housing situations or locations. By 2022, 65% remained Victorian renters, while others had transitioned to home ownership, moved interstate, or were lost to follow-up. We deliberately do not condition on post-policy housing status, which could induce selection bias. Figure A.2 and Table A.3 in the SI Appendix provide detailed breakdowns of sample composition across waves.

Pet Ownership Measure

The relevant pet ownership question asks: “Do you have any pets?” Respondents who answer affirmatively are then asked to report what type of pet they have (e.g., dog, cat, bird, fish, horse). Following prior work, we measure pet ownership at the household

level (Wilkins et al., 2020, 2025). That is, if anyone in the household reports having a pet, all respondents in that same household are assumed to have a pet. This household-level measurement means we cannot distinguish effects for individuals who personally engage with pets versus those who simply reside in pet-owning households.

Outcome Variables

As outcomes, we use mental health, general health, loneliness, and life satisfaction. Because pets tend to provide support and companionship (Damberg & Frombling, 2022), theoretically we expect that – if there is a significant effect of pet ownership – pets will reduce loneliness and improve life satisfaction, general health, and mental health.

Mental health is measured by the Mental Health Inventory (MHI-5), a five-item sub-scale of the SF-36 instrument. Respondents are asked to report how often during the past four weeks they: (i) have been a nervous person, (ii) felt so down in the dumps that nothing could cheer you up, (iii) felt calm and peaceful, (iv) felt down, and (v) have been a happy person. Each response is scored from 0 to 5 and the responses are linearly trans- formed so that the measure ranges from 0 (poor mental health) to 100 (excellent mental health).

The general health sub-scale of the SF-36 consists of the items: (i) self-assessed health, and the extent of agreement with the statements (ii) "I get sick a little easier than other people", (iii) "I am as healthy as anybody I know", (iv) "I expect my health to get worse", and (v) "My health is excellent". As with the MHI-5, the scores from these responses are transformed to make up the general health scale ranging from 0 (poor general health) to 100 (excellent general health).

Loneliness is measured on a 1–7 scale based on respondents' agreement with the statement: "I often feel very lonely". Higher scores indicate greater loneliness. Life satisfaction is measured on a 0–10 scale based on the question: "All things considered, how satisfied are you with your life?" Higher scores indicate greater life satisfaction.

Missing values for outcome variables are handled through listwise deletion; we do not impute missing data due to question non-response or panel attrition. Table A.2 in the SI Appendix reports descriptive statistics for all outcome variables, showing population- level trends among Victorians between 2018 and 2022.

Identification Strategy

We use changes in rental tenancy laws in the state of Victoria as a source of exogenous variation in pet ownership (see Consumer Affairs Victoria, 2021; Residential Tenancies Authority, 2022). Implementation of these tenancy laws prevented landlords from unreasonably refusing a tenant's application to own a pet. The law came into effect on 2 March 2020.[1] Renters who previously were not allowed to have any

[1] A similar law took effect in Queensland on 1 October 2022. Of the 337 renters surveyed in Queensland in 2022, 82 were interviewed after the policy change and could plausibly have been affected by it. We exclude these 82 individuals from our analysis.

pets during their tenancy could obtain a pet once the law was enacted, with landlords unable to refuse without a valid reason.

This setup – a policy affecting renters in Victoria but not non-renters in Victoria or renters elsewhere –would traditionally be analyzed using difference-in-differences. How- ever, difference-in-differences recovers only the intent-to-treat effect and requires the parallel trends assumption. Moreover, selection into pet ownership is likely driven by anticipated benefits, meaning that people who acquire pets differ systematically from those who do not. An instrumental variable approach, using time as an instrument, would ad- dress selection but violates the exclusion restriction due to baseline trends in potential outcomes.

We therefore utilize a Stability-Controlled Quasi-Experiment (SCQE), introduced by Hazlett (2019). SCQE compares outcomes between a high-use cohort (Victorian renters after the policy change, in 2022) and a low-use cohort (Victorian renters before the policy change, in 2018). Rather than assuming parallel trends, SCQE estimates treatment effects across a range of assumed baseline trends (δ), making transparent what assumptions are required to support any given conclusion.[2]

SCQE Framework

SCQE: Key objects, assumptions, and estimand.

Cohorts. $T \in \{0, 1\}$ with $T = 0$ for 2018 (low-use) and $T = 1$ for 2022 (high-use) among Victorian renters.

Treatment. $D = 1$ if adopted a pet by 2022 (restricted to those without pets in 2018); eligibility/exposure to the 2020 reform is an encouragement (shifts take-up), not the treatment itself.

Potential outcomes. $Y(1), Y(0)$ are outcomes with and without treatment for each well-being measure.

Baseline trend. $\delta \equiv \mathbb{E}[Y(0) \mid T = 1] - \mathbb{E}[Y(0) \mid T = 0]$, the expected change in non-treatment outcomes between cohorts.

High-use take-up. $\pi_1 \equiv \Pr(D = 1 \mid T = 1)$, the share of the 2022 cohort that acquired pets.

Estimand. $\text{ATT} = \mathbb{E}[Y(1) \mid D = 1, T = 1] - \mathbb{E}[Y(0) \mid D = 1, T = 1]$.

SCQE estimator. $\text{ATT}(\delta) = \dfrac{\mathbb{E}[Y \mid T = 1] - \mathbb{E}[Y \mid T = 0] - \delta}{\pi_1}$, which equals the Wald estimator when $\delta = 0$.

Assumptions. SUTVA; monotonicity (no defiers); no conditioning on post-treatment variables; δ reported via transparent benchmarks rather than assumed known.

The key insight of SCQE is that δ – the baseline trend in outcomes that would occur absent treatment – cannot be directly observed. Traditional difference-in-differences assumes δ is equal across treated and control groups (the parallel trends assump-

[2] Event-study specifications examining intent-to-treat effects show no consistent significant effects across outcomes (see SI Appendix Section A.4).

tion). SCQE relaxes this assumption at the cost of not yielding a single point estimate. Instead, we re- port ATT estimates across a range of plausible δ values and use empirical benchmarks to assess which values are defensible.

First-stage Effects

SCQE does not assume the absence of treatment-takers in the low-use cohort, but it does require a meaningful change in treatment probability between cohorts. Pet ownership was prevalent across all groups even before 2020, so the raw change in ownership rates was modest. We address this by restricting the sample to individuals who did not have a pet in 2018, focusing on those most likely constrained by pre-existing rental restrictions.

The SCQE estimator is algebraically equivalent to a modified Wald (IV) estimator, where time serves as an encouragement to treatment and δ adjusts for deviations from the standard IV exclusion restriction (see Hazlett, 2019, Sect. 4.2). However, the weak- instrument concern familiar from 2SLS does not apply here for two reasons. First, weak-IV bias arises because small first stages amplify bias from violations of the exclusion restriction. SCQE does not assume the exclusion restriction holds; instead, δ explicitly parameterizes the extent to which average non-treatment outcomes differ across cohorts. There is no "hidden" bias to amplify. Second, while a smaller π_1 reduces precision (because the term δ/π_1 scales sampling uncertainty), this is a power concern rather than a validity concern. In our data, take-up among Victorian renters is substantial ($\pi_1 \approx 0.39$), and first- stage F -statistics exceed the conventional threshold of 10 (Stock et al., 2002; Angrist & Jorn-Steffen, 2009) across all policy-relevant comparisons. The relevant diagnostic for SCQE is not whether the first stage is "strong enough" to avoid bias, but whether confidence intervals at each benchmarked δ are sufficiently narrow to support substantive conclusions.

Among our restricted sample, Victorian renters were 9–12% points more likely to acquire pets than comparison groups, with F -statistics ranging from 11.5 to 24.7 across specifications (see SI Appendix Table A.4). Uptake patterns are flat in placebo com- parisons – non-renters in Victoria versus non-renters elsewhere, and renters versus non- renters outside Victoria. This supports the assumption that the policy change, rather than other factors, drove differential pet adoption. For consistency, we impose the same sample restriction (no pets in 2018) on comparison groups. This means we estimate the effect of acquiring a pet among individuals who did not already own one – the population for whom the policy change was most relevant.

Benchmarking δ

We use three empirical benchmarks to establish plausible values of δ, each reflecting different assumptions about how untreated outcomes evolve:

DiM: Renters elsewhere. The change in outcomes from 2018 to 2022 among renters out- side Victoria.[3] This benchmark is most informative if secular, calendar-time shocks affect renters similarly across Australia. An important limitation is that Vic-

[3] Queensland is excluded because it was also treated during the observation window.

toria introduced strict COVID-19 lockdowns, so using renters elsewhere may not capture Victoria-specific pandemic effects.

DiM: Non-renters in Victoria. The change in outcomes among non-renters (owner-occupiers) in Victoria. This benchmark captures state-level shocks, including pandemic-related disruptions, that affected all Victorians regardless of tenure. While renters and non-renters may differ in baseline trends, they experienced the same policy environment apart from the pet legislation.

TJBAL: Synthetic renters. A synthetic control constructed via trajectory balancing (Hazlett & Xu, 2018), which re-weights renters outside Victoria to match the pre-treatment out- come trajectory of Victorian renters. This approach relaxes the assumption that any single comparison group shares the same trend as Victorian renters, instead constructing a weighted combination that replicates the treated group's pre-policy trajectory.

Each benchmark produces different δ values that can differ in magnitude and sign. Rather than privileging one benchmark, we present ATT estimates across the full range of δ values implied by all three. This makes explicit what assumptions about baseline trends would be required to conclude that pet ownership has significant positive or negative effects on well-being.

Results

Because the SCQE method does not identify a single average treatment effect on the treated (ATT), we evaluate a range of ATT estimates under different assumptions about the counterfactual trend (δ) among renters in Victoria – the group affected by the 2020 policy change. Under SCQE, δ represents the *baseline trend*: the change in outcomes from 2018 to 2022 among Victorian renters who adopted pets, in the counterfactual world where they had *not* adopted. It is measured in the same units as the outcome. We consider three empirical benchmarks as plausible values for δ (summarized in Table 1): (1) *DiM: Non- renters*, the change in outcomes among non-renters in Victoria; (2) *DiM: Renters elsewhere*, the change among renters in untreated regions outside Victoria; and (3) *TJBAL: Renters elsewhere*, based on a synthetic control group of renters outside Victoria constructed using trajectory balancing (see Hazlett & Xu, 2018).

Figure 1 plots the estimated ATTs of pet ownership for each outcome measure across a range of assumed δ values. The horizontal axis reflects the assumed counterfactual trend; the vertical axis shows the corresponding ATT estimate. Each panel displays sensitivity curves spanning all observed benchmark deltas from Table 1, along with the minimum and maximum values of δ that would yield statistically significant increases or decreases in the outcome. These curves extend 30% beyond the significance thresholds to show that even substantially more extreme assumptions than required for significance remain implausible given our benchmarks. We summarize the results for each of the outcomes below, and provide the underlying estimates, standard errors, 95% confidence intervals, and P-values in tabular form in SI Appendix Table A.5.

Table 1 Plausible values of δ estimated under three benchmarking approaches

	Loneliness	Life satisfaction	General health	Mental health
	(1–7)	(0–10)	(0–100)	(0–100)
DiM: Non-renters[a]	0.05	0.01	−1.71	−1.28
DiM: Renters elsewhere[b]	0.07	0.00	−2.18	−1.04
TJBAL: Renters elsewhere[c]	0.18	0.00	−3.08	−1.11

δ represents the estimated change in outcomes (2018–2022) for comparison groups, used as counterfactual benchmarks. Missing values due to survey non-response and panel attrition are not imputed

[a]DiM Non-renters – Loneliness: $N = 883$ (2018), 778 (2022); Life satisfaction: $N = 913$ (2018), 836 (2022); General health: $N = 891$ (2018), 781 (2022); Mental health: $N = 897$ (2018), 786 (2022)

[b]DiM Renters elsewhere – Loneliness: $N = 1{,}114$ (2018), 914 (2022); Life satisfaction: $N = 1{,}147$ (2018), 993 (2022); General health: $N = 1{,}117$ (2018), 919 (2022); Mental health: $N = 1{,}126$ (2018), 924 (2022)

[c]TJBAL Renters elsewhere (balanced panel 2018–2022) – Loneliness: $N = 1{,}035$; Life satisfaction: $N = 1{,}282$; General health: $N = 1{,}029$; Mental health: $N = 1{,}052$

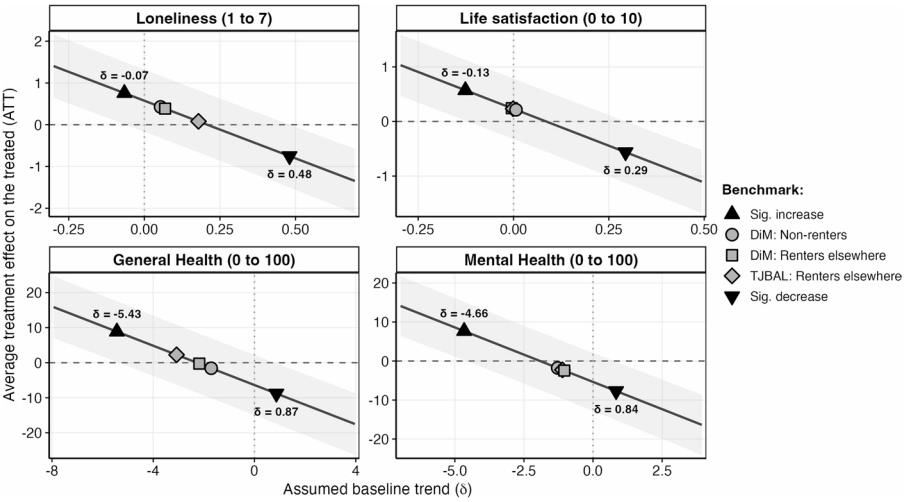

Fig. 1 SCQE estimated effects of pet ownership on well-being outcomes across δ values. SCQE estimated ATTs plotted across a range of assumed baseline trends (δ). The dashed horizontal line at zero indicates no effect; shaded regions denote 95% confidence intervals. δ represents the counterfactual trend in outcomes among Victorian renters (N=495) had the pet ownership policy not changed. Plausible values for δ include: DiM: Non-renters (change in outcomes from 2018 to 2022 among non-renters), DiM: Renters elsewhere (same change among renters in untreated regions), and TJBAL: Renters elsewhere (change among a synthetic comparison group constructed via trajectory balancing). ATT estimates for the minimum value of δ that would yield a statistically significant increase, and the maximum value that would yield a statistically significant decrease are shown for reference

Loneliness (1–7; higher = worse) To conclude pet adoption significantly *reduced* loneliness ($P < 0.05$), one must believe $\delta \geq 0.48$–that Victorian renters who adopted pets would have experienced loneliness increases of 0.48 points had they not adopted. Our empirical benchmarks are $\delta = 0.05$ (DiM: non-renters), $\delta = 0.07$ (DiM: renters elsewhere), and $\delta = 0.18$ (TJBAL). As shown in Fig. 1, the required threshold is 2.7–9.6 times larger than these benchmarks. At benchmark values, estimated ATTs are +0.43

at $\delta = 0.05$ ($P = 0.26$), +0.39 at $\delta = 0.07$ ($P = 0.31$), and +0.08 at $\delta = 0.18$ ($P = 0.83$)–all statistically insignificant and, in two cases, suggesting potential increases rather than decreases. For context, the broader Victorian population experienced loneliness changes of between −0.02 and 0.10 points during this COVID-impacted period (see Table A.2), making the required $\delta = 0.48$ implausibly large.

Life satisfaction (0–10; higher = better) A significant *improvement* would require $\delta \leq -0.13$; a significant *decline* would require $\delta \geq 0.29$. Benchmark estimates cluster near zero: $\delta = 0.01$ (DiM: non-renters), $\delta = -0.00$ (DiM: renters elsewhere), and $\delta = -0.00$ (TJBAL), indicating stable trajectories across comparison groups. At benchmark values, ATTs are small and non-significant: +0.21 at $\delta = 0.01$ ($P = 0.45$), +0.24 at $\delta = -0.00$ ($P = 0.39$), and +0.23 at $\delta = -0.00$ ($P = 0.41$). No plausible counterfactual trend supports a significant effect in either direction. The broader Victorian population experienced virtually no change in life satisfaction during this period (−0.00 to 0.03 points; see Table A.2), consistent with the near-zero benchmarks and reinforcing the implausibility of the required thresholds.

General health (0–100; higher = better) A significant *benefit* would require $\delta \leq -5.43$; a significant *harm* would require $\delta \geq 0.87$. Benchmarks suggest moderate declines: $\delta = -1.71$ (DiM: non-renters), $\delta = -2.18$ (DiM: renters elsewhere), and $\delta = -3.08$ (TJBAL). All benchmarks fall between the two significance thresholds. At benchmark values, ATTs are −1.58 at $\delta = -1.71$ ($P = 0.72$), −0.26 at $\delta = -2.18$ ($P = 0.95$), and +2.27 at $\delta = -3.08$ ($P = 0.60$). The benefit threshold would require counterfactual declines 1.8–3.2 times steeper than observed benchmarks, exceeding even the 1.4–2.4 point declines seen in the broader Victorian population (see Table A.2).

Mental health (0–100; higher = better) A significant *benefit* would require $\delta \leq -4.66$; a significant *harm* would require $\delta \geq 0.84$. Benchmarks indicate modest declines: $\delta = -1.28$ (DiM: non-renters), $\delta = -1.04$ (DiM: renters elsewhere), and $\delta = -1.11$ (TJBAL). All benchmarks fall between significance thresholds. At benchmark values, ATTs are −1.78 at $\delta = -1.28$ ($P = 0.64$), −2.45 at $\delta = -1.04$ ($P = 0.52$), and −2.27 at $\delta = -1.11$ ($P = 0.55$). The benefit threshold would require counterfactual declines 3.6–4.5 times steeper than observed benchmarks, exceeding even the 1.0–2.5 point declines in the broader Victorian population (see Table A.2).

Overall, our empirical benchmarks consistently yield null findings across all outcomes. For loneliness and life satisfaction, significant effects would require counterfactual base- line trends 2.7–9.6 times more extreme than any observed benchmark. For general and mental health, all benchmarks fall between the positive and negative significance thresh- olds, indicating that Victorian renters' health trajectories are statistically indistinguishable from comparison groups. To conclude the 2020 pet policy had significant effects in any direction, one must defend counterfactual assumptions substantially more pessimistic or optimistic than what any empirically grounded benchmark suggests. The SCQE frame- work makes these inferential boundaries explicit: our analysis identifies precisely which δ values would be necessary to reach each substantive conclusion and demonstrates that none are empirically plausible.

Robustness checks Our main analysis includes all individuals who were Victorian renters in 2020 when the pet policy took effect ($N=495$), ensuring the sample comprises those who were eligible to be affected by the changes in Victorian legislation that removed re- strictions on pet ownership for renters. However, 14% of this sample were not Victorian renters in 2018, meaning their baseline (pre-policy) outcomes and pet-ownership status were measured in different housing situations or locations. To address this measurement concern, we conduct two robustness checks with progressively stricter sample restrictions (see SI Appendix Tables A.7 and A.8).

First, we restrict to individuals who were Victorian renters in both 2018 and 2020 ($N=425$). This ensures baseline outcomes are measured while individuals were already Victorian renters, thereby eliminating measurement issues from housing transitions be- tween 2018 and 2020. As with the main analysis, we do not condition on 2021 or 2022 renter status, avoiding possible selection bias from post-policy housing decisions. Results remain substantively identical: under the TJBAL benchmark, the loneliness ATT estimate is −0.08 [95% CI: −0.91, 0.75] compared to 0.08 [−0.66, 0.82] in the main analysis.

Second, we impose the most conservative restriction to obtain a balanced panel of stable Victorian renters across all periods – 2018, 2019, 2020, 2021 and 2022 ($N=291$) – eliminating any compositional changes from housing transitions. Despite 41% sample loss and correspondingly wider confidence intervals, point estimates remain near zero. Across both robustness checks and all four outcomes, findings are consistent with the main analysis. This strengthens confidence that null results are not artifacts of sample composition or measurement timing. These checks also demonstrate robustness to potential concerns about housing transitions during the COVID-19 period, as the stable-renter specification ($N=291$) retains only individuals who remained Victorian renters throughout 2018–2022.

Finally, while our DiM: Non-renters benchmark explicitly controls for Victoria-specific COVID-19 effects – since both renters and non-renters in Victoria experienced the same lockdown policies – we conduct an additional robustness check excluding Melbourne to address potential geographic heterogeneity in policy effects. Melbourne experienced the country's longest lockdowns (262 days), substantially more severe than other Australian cities. This raises questions about whether policy effects might differ across regions with varying pandemic severity. We therefore replicate the analysis restricting to renters in regional Victoria when the policy took effect in 2020 ($N=124$; see SI Appendix Table A.6). Across all four outcomes, we find no statistically significant ATT estimates under any plausible δ values, consistent with our main findings. Confidence intervals are gain wider due to reduced sample size – approximately 80% of Victorians reside in Melbourne – but point estimates remain near zero across all benchmark scenarios. These null findings, combined with administrative data from RSPCA shelters showing no evidence of pandemic-specific pet relinquishment patterns in Victoria (see SI Appendix Section A.1), provide additional confidence that our results are not artifacts of Melbourne's lockdown conditions or unstable pet ownership patterns during the pandemic period.

Discussion and Conclusion

Using a novel quasi-experimental identification strategy coupled with an exogenous change in rental laws, this paper used nationally representative longitudinal data from Australia to study the causal effect of pet ownership on mental health, general health, life satisfaction, and loneliness. Across all four outcomes, we find no evidence of significant effects under plausible assumptions about counterfactual trends. These findings are robust to alternative sample restrictions, geographic specifications, and benchmarking approaches. They suggest that much of the positive association documented in prior work may reflect selection rather than average causal effects among those who chose to get pets. The absence of detectable effects on health and well-being is consistent with some previous research (Antonacopoulos & Pychyl, 2010), but stands in contrast to the widespread belief that pets improve well-being (Utz, 2014; Bao & Schreer 2016; American Heart Association 2024). We offer three explanations for these findings.

First, our design addresses selection bias that pervades the existing literature. Most prior studies rely on cross-sectional comparisons (e.g., Antonacopoulos & Pychyl, 2010; Bao & Schreer, 2016; Damberg & Frombling, 2022) or inadequately control for selection into pet ownership (Gmeiner & Gschwandtner, 2025). People who acquire pets likely differ systematically from those who do not – in ways correlated with both the decision to get a pet and subsequent well-being. When the causal arrow runs from anticipated benefits to adoption (rather than from adoption to benefits), observational associations do not identify causal effects. Our approach explicitly accounts for non-random selection into pet ownership. It does so by showing what effects would be estimated under a range of assumptions about the baseline trends required to support a particular ATT. In the current setting, the assumptions required to justify a positive effect of pet ownership on well-being outcomes are not empirically or substantively plausible.

Second, even if pets provide genuine benefits to some individuals, these benefits may be offset by the responsibilities and burdens of ownership. Pets require significant investments of time, effort, and money. For renters – who face tighter budgets and housing constraints than homeowners – these trade-offs may be especially salient. The demands of pet care could create stress or limit time for other social activities, potentially counter- balancing any companionship benefits. Null effects on average are consistent with heterogeneous responses: benefits for some individuals, burdens for others, averaging to zero.

Third, we cannot rule out that positive effects of pet ownership are short-term in nature. Our outcome measures capture effects up to two years after some individuals obtained a pet, by which time initial benefits may have dissipated. The HILDA survey only measured pet ownership in 2018 and 2022, so we cannot observe the exact timing of adoption or distinguish individuals with six months of ownership from those with two years. This measurement limitation means our estimates may average across individuals with varying exposure durations, potentially attenuating effects that depend on the length of ownership.

A related concern is that benefits of pet ownership might have been short-lived if many “pandemic pets” were ultimately relinquished once lockdowns lifted and

daily routines resumed. If so, we would expect a surge in shelter intakes in Victoria beginning in 2021, reversing earlier declines. However, administrative data from the Royal Society for the Prevention of Cruelty to Animals (RSPCA), Australia's largest animal welfare organization, provide no evidence of such a pattern (see SI Appendix Section A.1). Shelter intake volumes continued to fall across nearly all jurisdictions through 2023. In Victoria specifically, intakes declined by 50% between 2019 and 2023 – a reduction that exceeded most other states. These trends suggest that pet relinquishment remained low in Victoria and are inconsistent with the hypothesis that unstable ownership patterns explain our results. The paper has several limitations. First, because pet ownership is only measured in 2018 and 2022, we cannot observe annual changes in ownership status, the precise timing of adoption, or whether individuals acquired and subsequently relinquished pets between survey waves. This limits our ability to distinguish effects that depend on exposure du- ration. Second, the data do not indicate how many pets a respondent has, only whether they have pets. We therefore cannot estimate the effect of a "marginal" pet for those who already own one. Similarly, pet ownership is measured at the household level, so we can- not distinguish effects for individuals who personally engage with pets versus those who simply reside in pet-owning households.

Third, although SCQE has important advantages over traditional difference-in-differences methods, it does not yield a unique point estimate. The method relies on the plausibility of assumed baseline trends, which requires transparent benchmarking against empirically grounded comparison groups. We have provided three such benchmarks, and our conclusions hold across all of them. Fourth, our sample sizes yield wide confidence intervals for some outcomes. We cannot rule out small positive or negative effects that fall within these intervals. Finally, these findings derive from a single policy context in Australia, and the extent to which they generalize to other settings remains an open question.

Several potential concerns have been addressed through our research design and robustness checks. First, the coincidence of the 2020 pet policy with the COVID-19 pandemic raises questions about confounding. Our comparison of Victorian renters to Victorian non-renters explicitly controls for Victoria-specific pandemic effects, since both groups experienced the same lockdown policies but only renters were affected by the pet legislation. Additional analyses excluding Melbourne – which experienced substantially longer lockdowns than other Australian cities – yield consistent null findings. Second, our main analysis defines treatment status based on Victorian renter status in 2020, when the policy took effect. This ensures that all individuals in the treatment group were actually subject to the new legislation. For some of this sample, baseline outcomes were measured in 2018 when they resided elsewhere or in different housing tenure. Robustness checks restricting to individuals who were Victorian renters in both 2018 and 2020, and to stable Victorian renters across all periods, produce substantively identical results. This strengthens confidence that findings are not artifacts of sample composition or measurement timing.

The general absence of significant effects of pet ownership on health and well-being does not imply that individuals do not benefit from animal companionship. Many people derive genuine satisfaction from their pets, and some individuals in our sample may have experienced meaningful improvements in well-being. However,

the average treatment effect among those who adopted pets was indistinguishable from zero under all plausible counterfactual assumptions. This suggests that acquiring a pet is unlikely to be an effective strategy for improving health or reducing loneliness, on average, for individuals who would not otherwise obtain one. The effects of losing a pet – whether through death, relinquishment, or policies that prohibit pet ownership – may be quite different. If attachment to an existing companion is stronger than the benefit of acquiring a new one, policies that force pet relinquishment could have negative effects on well-being that our design cannot detect. The widespread belief that pets improve well-being may thus partly reflect selection, but may also reflect asymmetric effects of gaining versus losing a pet. Future research should examine heterogeneity in treatment effects, the consequences of pet loss, and whether these findings replicate in other policy contexts. More broadly, our findings illustrate the value of quasi-experimental approaches for evaluating widely held beliefs about lifestyle factors and well-being.

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Data Availability The data can be accessed following approval by the data custodian. The findings and views reported in this paper, however, are those of the authors and should not be attributed to the Australian Government, DSS, or any of DSS' contractors or partners.

Declarations

Ethics Approval and Consent to Participate This paper uses unit record data from the Household, Income and Labour Dynamics in Australia Survey (HILDA) conducted by the Australian Government Department of Social Services (DSS). DOI: https://doi.org/10.26193/R4IN30. All participants provided informed con sent before completion of the survey.

Conflicts of interest The authors declare that they have no competing interests.

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