A Report on "Protective Effects of Dog Ownership Against the Onset of Disabling Dementia in Older Community-Dwelling Japanese: A Longitudinal Study" by Taniguchi et al. (2023)

Reviewer 2

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> I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.
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> Plato, *The Apology of Socrates*, 21d

> To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.
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> Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Taniguchi, Y., Seino, S., Ikeuchi, T., Hata, T., Shinkai, S., Kitamura, A., Fujiwara, Y. (2023). Protective Effects of Dog Ownership Against the Onset of Disabling Dementia in Older Community-Dwelling Japanese: A Longitudinal Study. *Preventive Medicine Reports*. Vol. 36, 102465.

Abstract Summary: This prospective study examined the associations of dog/cat ownership with incident disabling dementia in older community-dwelling Japanese using propensity score matching and also investigated the interaction between dog/cat ownership and exercise habit and social isolation. The study found that current dog owners had a significantly lower risk of disabling dementia compared to past and never owners, particularly those with a regular exercise habit and no social isolation.

Key Methodology: Longitudinal study using propensity score matching and inverse probability of treatment weighted logistic regression model on data from 11,194 older adults in Japan, with a follow-up period of approximately 4 years.

Research Question: Does dog/cat ownership have a protective effect against the onset of incident disabling dementia in older community-dwelling Japanese, and do exercise habit and social isolation interact with this association?

Summary

Is It Credible?

Taniguchi et al. present a longitudinal study involving over 11,000 older Japanese adults, concluding that current dog ownership is associated with a significant reduction in the risk of incident disabling dementia. The authors report that current dog owners had an odds ratio (OR) of 0.60 compared to past and never owners, implying a 40% reduction in risk over a four-year period (p. 4). They attribute this protective effect to mechanisms involving "exercise habit and social participation," specifically noting that dog walking contributes to physical activity and opportunities for social interaction (p. 5). While the study utilizes a large sample and attempts to correct for confounding variables using inverse probability of treatment weighting (IPTW), the headline claim of a protective effect warrants closer scrutiny regarding its robustness and the interpretation of the mechanisms involved.

A primary point of discussion is the sensitivity of the main finding. A major concern in research on pet ownership and health is reverse causality—the "healthy owner effect"—whereby individuals with better physical and cognitive health are more likely to acquire or retain a dog. To address this, the authors conducted a sensitivity analysis excluding dementia cases that occurred within the first year of follow-up. In this analysis, the statistical significance of the protective effect disappeared, yielding an OR of 0.67 with a 95% confidence interval of 0.40–1.14 (p. 4). It is important to note that the point estimate (0.67) remains very close to the main finding (0.60), suggesting that the effect size is relatively stable. However, the confidence interval crossing 1.0 indicates a loss of statistical power and a failure to reject the null hypothesis in this stricter subsample. Consequently, the authors' assertion in the discussion that the association was "confirmed using sensitive analysis" is a statistical overstatement (p. 4). It would be more accurate to state that while the direction of the association

remained consistent, the result was not statistically significant when early cases were excluded, leaving open the possibility that prodromal dementia plays a role in the observed relationship.

Furthermore, the outcome measure used—"incident disabling dementia"—introduces a specific context. The study does not rely on clinical diagnoses but rather on certification for Long-Term Care Insurance (LTCI) at Level II or higher (p. 2). As the authors note, this is a validated administrative measure of functional dependence and care need in the Japanese context. However, it is distinct from a direct measure of neuropathology. Consequently, the results could reflect a delay in the *application* for formal care rather than solely a delay in disease onset. Dog owners, who the study notes are more likely to live in multi-person households and have better social networks, might be better equipped to manage early-stage cognitive decline at home without seeking state support (p. 3). This social buffering could contribute to the observed protective health effect.

Finally, the proposed mechanisms—exercise and social isolation—are difficult to disentangle from the ownership variable itself. The study found that dog owners with an exercise habit had the lowest risk (OR 0.37), but since "walking" is the primary form of exercise for dog owners, the variables are intrinsically linked (p. 4). The study essentially confirms that active, socially connected people are less likely to require long-term care for dementia. Whether the dog is the causal agent or merely a marker of this active lifestyle remains an open question, particularly given the lack of baseline cognitive data to rule out pre-existing differences between owners and non-owners.

The Bottom Line

The claim that dog ownership reduces the risk of disabling dementia by 40% is a provocative finding that requires careful interpretation regarding selection bias and

the "healthy owner effect." While the effect size remained relatively stable in sensitivity analyses, the loss of statistical significance when excluding the first year of data suggests that the result is sensitive to sample size and potentially reverse causality. Additionally, because the outcome is based on insurance certification rather than clinical diagnosis, the study may be capturing a delay in the need for formal care due to better social support among dog owners, alongside any potential biological prevention of dementia.

Potential Issues

Fragility of the main finding and selective reporting: The article's primary conclusion—that dog ownership has a protective effect on incident disabling dementia (OR 0.60, 95% CI: 0.37–0.977)—appears sensitive to the analytical sample. A key sensitivity analysis, designed to mitigate reverse causality by excluding cases from the first year of follow-up, yielded a statistically non-significant result (OR 0.67, 95% CI: 0.40–1.14) (p. 4). While the point estimate remains directionally consistent with the main finding, suggesting the loss of significance may be due to reduced statistical power rather than the absence of an effect, the authors' claim in the discussion that "This association was confirmed using sensitive analysis" is an overstatement (p. 4). A result with a confidence interval that crosses the null value of 1.0 does not statistically confirm an effect. Furthermore, the abstract reports only the significant main finding and omits the non-significant result from this key sensitivity check, which presents a more definitive conclusion to the reader than the full results may warrant (p. 1).

Unaddressed reverse causality and confounding: A central challenge in this area of research is the "healthy owner effect," where healthier, more functional, and socially connected individuals are more likely to be able to own a dog in old age. The dog may be a marker of pre-existing health, not the cause of future health. The authors acknowledge this possibility as a limitation, noting the lack of baseline cognitive function data could lead to reverse causality (p. 6). While propensity score methods were used to address confounding, they can only account for observed variables. Key unmeasured confounders, such as personality traits or baseline cognitive engagement, could still bias the results. The article's design cannot fully disentangle the protective effect of dog ownership from the pre-existing health of those who choose to be dog owners.

Lack of evidence for covariate balance: The article's claim to have controlled for

confounding rests on an inverse probability of treatment weighted (IPTW) model, which the authors state "enabled us to balance baseline characteristics" (p. 3). However, the article provides no diagnostic evidence to support this claim. Standard practice for this methodology involves presenting a table of standardized mean differences before and after weighting to demonstrate that the adjustment successfully balanced the numerous covariates between the owner and non-owner groups. Without such evidence, the unadjusted differences shown in Table 1—where dog owners are younger, wealthier, and more socially connected—remain a concern (p. 3). The effectiveness of the statistical adjustment is therefore unverifiable, leaving the validity of the causal claims in question.

Nature of the outcome measure: The study defines its outcome as "incident disabling dementia," but this is measured as a certification of "level II or higher" in Japan's Long-Term Care Insurance (LTCI) system, not as a direct clinical diagnosis of dementia onset (p. 2). While this is a standard and validated metric for functional need in Japanese gerontology, it is an administrative measure. The onset of dementia as a disease and the point at which functional decline becomes severe enough to require and successfully apply for state-funded care are distinct events. The decision to apply for LTCI is also influenced by non-clinical factors like family support and socioeconomic status. While the authors are transparent about their definition, this means the study is measuring a delay in the need for formal care rather than necessarily a direct protective effect against the onset of dementia pathology itself.

Potential for selection bias in the comparison group: The study's design compares "current" dog owners to a heterogeneous reference group of "past and never" owners (p. 2). This grouping may introduce a bias. The "past owner" category could be disproportionately composed of individuals who relinquished a pet due to declining health, cognitive function, or other life events that are themselves risk factors for dementia. Including these potentially higher-risk individuals in the reference group could artificially inflate the incidence of dementia in that group, thereby making

current dog ownership appear more protective than it is. While the article does not specify the proportion of "past" versus "never" owners in the reference group, the potential for this bias remains unaddressed (p. 3).

Ambiguity in the analytical sample size: There is a lack of clarity regarding the number of participants included in the final analyses. The methods section states that 11,194 participants provided complete data, and this number is used as the denominator for the "Incident dementia" column in Table 2 (pp. 2, 4). However, a footnote in the same table indicates that the primary analysis for dog ownership was conducted on "n = 8,323" (p. 4). This discrepancy is not explained in the text. It is possible this figure represents the sum of weights or an effective sample size derived from the IPTW method, rather than a raw exclusion of participants. However, without explicit clarification, the reader is left to guess the nature of this smaller n, which reduces the transparency of the statistical reporting.

Differential model specification for dog versus cat ownership: The study contrasts the significant protective effect of dog ownership with a null effect for cat ownership. The statistical models used for these two analyses differ: the propensity score model for dog ownership adjusted for 22 variables, while the model for cat ownership adjusted for only 12 (p. 3). While it is standard practice to select covariates that specifically predict the treatment (ownership type), and thus the models need not be identical, the exclusion of several important health and psychological covariates from the cat model (such as frailty status and motor fitness) makes direct comparison of the results more complex. It raises the question of whether the null finding for cats is due to the lack of effect or the differential adjustment strategy.

Issues with the interpretation of mechanisms: The article proposes that physical activity and social participation are the key mechanisms for the observed effect. However, the link is based on interaction analyses with general measures, not direct measurement of dog-related activities. For instance, the interaction between dog ownership and a "regular exercise habit" is difficult to interpret because the definition of

exercise includes "walking" (p. 3), the primary activity associated with dog ownership. This creates a degree of circularity. Furthermore, the measure of exercise relies on self-report (engaging in an activity "more than once per week"), which is a standard but limited approach that may not capture meaningful differences in activity intensity. The causal chain from ownership to the proposed mechanisms is therefore assumed rather than demonstrated.

Minor reporting and transparency issues: Several minor issues in the article's presentation collectively reduce methodological clarity. The abstract refers to "propensity score matching," while the methods section describes "inverse probability of treatment weighting," which are distinct techniques (pp. 1, 3). While likely a terminology error in the summary, it creates confusion. The article also lacks clarity on the handling of 124 participants who owned both a dog and a cat (p. 3), and does not explicitly state the rationale for using a Generalized Estimating Equation (GEE) model, though clustering by district is a plausible reason (pp. 2–3). While these individual points are minor, they contribute to a lack of overall transparency.

Future Research

Objective cognitive assessment: Future studies must move beyond administrative claims data and employ validated cognitive testing (such as the MMSE or MoCA) at both baseline and follow-up. This would allow researchers to control for baseline cognitive function—a critical unmeasured confounder in the current study—and detect clinical dementia onset regardless of whether the individual applies for long-term care insurance.

Extended washout periods: To rigorously test the direction of causality, future research should design prospective studies with a longer "washout" period, excluding incident cases from the first two or three years of follow-up. This would more effectively minimize the influence of prodromal dementia on pet ownership status. Such a design requires a larger initial sample size to maintain statistical power, addressing the issue where the current study lost significance after excluding only one year of data.

Device-based activity monitoring: To validate the mechanism that dog walking provides the protective exercise benefit, future work should utilize accelerometers rather than self-reported exercise habits. While self-reports are standard in large epidemiological studies, objective measurement would quantify whether dog owners actually achieve higher intensities or durations of physical activity compared to non-owners and allow for a mediation analysis to determine how much of the "dog effect" is strictly attributable to increased movement.

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