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Home Ownership and Political Participation:

Longitudinal Evidence Suggests There is No Causal Relationship

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Abstract

The effect of home ownership on political participation is examined using longitudinal data from New Zealand. We study political party membership, and if and how individuals voted in six General Elections, using validated data. There is a positive correlation between being a home owner and ever voting, and between political party membership and the local home ownership rate. Individual home ownership and the home ownership rate negatively correlate with voting for left-wing political parties. But once the longitudinal nature of the data are exploited, using fixed effects specifications, these significant correlations disappear. This change in the results suggests that unobserved factors that predispose people to be a home owner also affect their political participation.

1. Introduction

Public policy in many countries is biased in favour of home owners, in part due to the belief that home ownership has important externalities. Ownership is claimed to boost investment in local amenities and social capital because owners benefit from capitalization into house prices, and the lower mobility of owners raises the present value of their stream of expected benefits from enhanced local social capital (DiPasquale and Glaeser 1999). A variety of studies find that higher home ownership is associated with greater neighbourhood stability (Johnson 2003) and safety (Brounen, Cox and Neuteboom 2012), less crime (Glaeser and Sacerdote 1999), fewer social problems (Galster, Quercia, and Cortes, 2000), better educated children (Boehm and Schlottmann 1999 and Haurin, Parcel and Haurin 2002), more involvement in local civic groups (McCabe, 2013) and higher electoral participation (Rossi and Weber 1996 and Leviten-Reid and Matthew 2017).

The policy biases for home ownership are costly so the evidence for externalities needs to be quite firm. One bias is from tax deductibility for mortgage interest payments; in the United States this housing subsidy was worth over US$160 billion in 1990 (over two percent of GDP) with spatially concentrated benefits (Gyourko and Sinai 2001), while a similar subsidy in the Netherlands is equivalent to three percent of GDP (Brounen *et al.* 2012).[1] These subsidies are counter-productive since housing markets with inelastic supply – which may be the only ones where home ownership creates positive externalities (Hilber 2010) – will have higher prices due to a tax-induced rise in demand for owner-occupied housing. Potential buyers who have difficulty making a minimum deposit may be forced out of the market, and these may be people such as the young or low-paid who policy makers have in mind when trying to increase home ownership rates. Hilber and Turner (2014) show that mortgage interest deductibility only increases home ownership rates in elastically supplied housing markets, and these are places where no positive externality from home ownership is likely to occur.

Yet despite the need for firm evidence on externalities from home ownership, the existing literature often relies upon observed correlations that may not reveal causal effects. Since home ownership is likely to depend somewhat on idiosyncratic and unobservable factors, and those same factors may influence civic participation and other forms of investment in local social capital, it is possible that observed relationships between ownership and civic activity are affected by omitted variable bias. Indeed, reviews of the literature on the external effects of home ownership conclude that the empirical approaches used in early studies may not give reliable results because of omitted variables and unaccounted for selection effects (Dietz and Weinberg 2003 and Haurin, Dietz and Weinberg 2003). In the specific context of reviewing research on social participation and home ownership, Rohe, van Zandt, and McCarthy (2013, p.208) note that 'certain persons may have an underlying propensity for social involvement that leads them to both participate in voluntary and political activities, and to buy a home' and

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[1]   The implicit subsidy from not taxing imputed rental income also favours rich households (Saarimaa 2011).

further note that 'none of the studies on this topic have totally ruled out the possibility that the association between home ownership and social and political participation is spurious.'

In order to provide firmer evidence on whether home owners are more likely to vote, on how they vote, and on whether they are more likely to join political parties, we use longitudinal survey data that enable us to control for any 'underlying propensity' towards social involvement and home ownership. Intuitively, we are able to identify effects of home ownership by observing people who make transitions between renting and owning (or *vice versa*). Our longitudinal data come from the New Zealand Election Study (NZES), which surveys at General Elections on a three-yearly cycle (Vowles 2010); the surveys we use cover the period from 1996 to 2011. Of the almost 7,000 observations available to us, over 3,300 are in two waves of the survey, over 2,200 are in three waves of the NZES, and 884 are in four waves. While the main housing pattern is for owners to stay owners throughout the longitudinal record (the home ownership rate in the NZES is over 75 percent) there are 534 observations that transition between renting and owning, so regression models with individual fixed effects models can be estimated to see if the correlations between home ownership and participation in political activity are due to omitted variables bias. We also use local area fixed effects (at two different levels of spatial granularity), to deal with any potential biases that result from neighbourhood sorting. Finally, we allow for both direct and indirect effects of home ownership, by considering individual level ownership and local ownership rates.

Another strength of our study is the quality of the dependent variables from the NZES. First, the self-report by each respondent on whether they voted is *validated* by comparing with the lists of votes cast that are held by the registrar of electors in each electorate. Few prior studies use validated data even though voting is prone to over-reporting since it is seen as socially desirable.[2] Systemic overstatement of voting generates a non-random misclassification error, so when voting is the dependent variable it will induce bias in all regression coefficients (Hausman *et al.* 1998).[3] Second, voter registration is compulsory in New Zealand so variation in individual turnout is not driven by differences in voter registration, as happens in the U.S.[4] Third, under New Zealand's Mixed Member Proportional (MMP) voting system, each voter has the same weight in setting party proportionality in the Parliament regardless of the marginality of the electorate they live in. Finally, New Zealand voters have just two votes, one for a Party and one for an electorate Member of Parliament; referenda are rare and there are no

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[2] For example, Matsusaka and Palda (1999) use surveys from four national elections in Canada where the rate of self-reported voting in their samples was 16 percentage points higher, on average, than the actual turnout in each election. Similarly, 22 percent of non-voters in local government elections in Sweden claimed to have voted when they were surveyed (Karp and Brockington 2005).

[3] Even more non-randomness may come from the tendency for false reports of voting to be more likely for some demographic groups than others (Silver *et al.* 1986).

[4] While registration is compulsory, there is no legal compulsion to vote, unlike the case in neighbouring Australia.

other jurisdictional positions to fill. Thus, the cognitive costs of voting should nor vary, unlike in other countries where some voters face many more choices than do others.

While the NZES is for national elections, some studies in the literature are for the relationship between home ownership and voting in local elections so comment is needed on whether capitalization into house values and transactions costs that make owners less mobile also applies to national elections. International mobility is very high in New Zealand, with over one quarter of residents foreign-born. On the other side of the ledger New Zealand, along with Ireland, has the highest rate of skilled emigration in the OECD, at almost one-quarter (Gibson and McKenzie 2011). A lot of the international mobility is due to the fact that New Zealand residents have free entry to Australia under the Trans-Tasman Travel agreement, and this access to an economy that is over five times larger sees New Zealand normally lose about one percent of its residents per year to Australia, which it then replaces with new immigrants (who can vote after one year of residence). In these circumstances the capitalization argument also may apply at the national level; good policies that make a country more attractive cause fewer people to emigrate and more to immigrate and this raises housing demand, giving an incentive for home owners to be politically active. It is also the case that our analysis of political party membership can inform the literature that considers home ownership and political participation since political parties are normally nationally based.

Our results show that, in the cross-section, voting is correlated with both personal and area-level home ownership rates. However, no causal relationships are found once we control for neighbourhood fixed effects or focus on the impact of changes in home ownership using individual fixed effects. The potential bias due to unobservable factors that correlate with home ownership and voting is seen clearly by examining the type of political parties for whom individuals vote. There is a robust negative correlation between owning a home and voting for left-wing political parties that persists even after conditioning on neighbourhood home ownership rates and on neighbourhood fixed effects. But once the longitudinal nature of the data are exploited by using individual fixed effects, this significant negative correlation disappears. Evidently, the unobserved factors that predispose people to being a home owner also make them less likely to vote for left-wing political parties. Once these unobserved factors are controlled for there is no causal effect of home ownership on voting patterns.

The remainder of the paper is structured as follows: Section 2 provides a brief review of previous literature; Section 3 discusses the data and empirical methods; Section 4 provides the results, and Section 5 concludes.

2. Previous Literature

The basic intuition for the hypothesis about externalities from home ownership is well known but there are some important assumptions that are rarely discussed and that may limit the applicability of the hypothesis. The usual story is that unpaid civic participation improves the quality of community life and this improved quality is reflected in increased property values.

This capitalization effect means that the positive externalities from civic participation are most likely to be found amongst home owners and in neighborhoods with a high rate of home ownership. In contrast, for renters the improved community quality will be reflected in higher rents (unless there is rent control) so they receive no net benefit, while owners get an increased service flow component to their imputed rents and also a windfall gain when they sell. Furthermore, since home owners are less easily able to move than are renters, given that they face greater transactions costs from selling a property, owners will have a longer expected tenure in a given location, and this longer time horizon increases the present value of the expected stream of neighbourhood benefits (DiPasquale and Glaeser 1999).[5]

Yet hidden in the background in this story are assumptions about the supply conditions in the housing market which may make the relationship between home ownership and civic participation less widespread. Hilber (2010) shows how the elasticity of new housing supply affects the likelihood that people will make social capital investments. In a built-up area with little developable land for new housing, the incumbents deciding whether to invest in social capital are largely protected from inflows of newcomers who could dilute (due, for example, to congestion effects) the net benefit from that social capital. In contrast, in areas with elastic housing supply the rising attractiveness of an area that has civic-minded residents who are investing in local social capital will give landowners an incentive to develop new housing. The new residents who come to live in this new housing may dilute the benefits of the social capital, while the new housing supply will limit any rise in local house prices. In such a setting nobody has an incentive to make much investment in neighborhood specific social capital in the first place and so the hypothesized link between ownership and social capital will break down. To test this proposition, Hilber (2010) uses U.S. community survey data on social capital, which is measured with proxies such as the number of social interactions with immediate neighbors, participation in neighborhood associations, and participation in service and fraternal organizations. He finds that the positive link between home ownership and the decisions of individuals to invest in this type of social capital investment largely occurs in more built-up neighborhoods that will be where there is more inelastic supply of new housing.[6]

In addition to the need for inelastic supply, another caveat about the externalities from home ownership is that even if these are statistically significant they may not be substantively important at the home ownership rates that are typically observed. Evidence for this claim comes from Brounen *et al.* (2012) who find that a 10 percent increase in home ownership rates

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[5] There may be some negative impacts of this lower mobility of home owners which we do not consider in our empirical analysis. For example, equilibrium unemployment rates may be higher because lower market mobility of home owners adds friction to the spatial matching of labor supply and labor demand; this effect is found to hold for middle-aged households but not for the young and the old, in the United States (Green and Hendershott 2001).

[6] However, Pfeiffer and Morris (2017) found that the observed cross-sectional effects in volunteering by home ownership status did not vary over the recent drastic housing market cycles in the United States, which suggests that volunteering differences by home ownership status could be due to unobservable time-invariant factors.

would raise their measure of externalities (neighborhood safety) by just 0.6 percent in a city (Rotterdam) that has low home ownership rates of around 30 percent. Moreover, the external effects of higher ownership rates become even smaller once the ownership rate reaches levels of around 55 percent, which are below the level observed in many English-speaking countries.

2.1 Omitted Variables Bias

As noted above in the Introduction, reviews of the literature on the external effects of home ownership suggest that some results from early studies may suffer omitted variables bias, due to imperfectly controlling for unobservable factors that are correlated both with home ownership and with the outcomes studied (such as political participation, child welfare, and so forth). Even when instrumental variables (IV) strategies are used by authors such as DiPasquale and Glaeser (1999), to provide estimates that should be free of omitted variables bias, the available instruments based on group average home ownership rates may not be exogenous since unobservable factors that affect the outcomes could also affect either group membership or the group averages.[7]

Firmer evidence comes from research designs that randomly assign treatments that lead to exogenous variation in home ownership. Although there are far fewer of these studies, they provide grounds for doubting some earlier findings. For example, Engelhardt, Eriksen, Gale and Mills (2010) study political involvement of a sample in Tulsa, Oklahoma where a randomly assigned treatment group had their saving for home purchase subsidized; four years after the randomization the ownership rate of the treatment group was approximately one-quarter higher than in the control group. If the assignment to treatment is used as an IV, home ownership is found to have a significant negative effect on political involvement – the opposite of the usual pattern reported in the literature. However, if probit regressions are used that ignore the exogenous variation in home ownership resulting from the randomization, there appears to be the usual positive effect of home ownership on political involvement. This switch in the sign for the effect of home ownership suggests that some prior studies finding positive associations between home ownership and political participation may have been biased by incomplete controls for the endogenous choice to become a home owner. Another result from a randomized program that reverses previous non-experimental findings comes from Navarrete and Navarrete (2016), who found that families receiving a house through a randomized program do not show lower market mobility, contrary to what was shown by non-experimental studies like Green and Hendershott (2001).

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[7] Another research design is based on the argument that if investments by home owners in civic participation and social capital increase neighbourhood quality, that higher quality should be reflected in dwelling prices (assuming inelastic supply). Coulson and Li (2013) use this method to find a positive association between the neighborhood home ownership rate and the self-reported prices of single-family homes in the United States while Kortelainen and Saarimaa (2015) find no such relationship using actual sales prices for apartments in a built-up area of Finland (where built-up areas are the most favorable for capitalization into dwelling prices given inelastic supply).

2.2 Home Ownership and Voting Patterns

While there are several studies on the relationship between home ownership and the decision to vote, very little attention has been paid by economists to the question of whether owning a home changes an individual's political orientation. This is surprising since there are economic consequences from political preferences, and there are studies in economics of the role of other factors – such as having daughters (Oswald and Powdthavee 2010) – on whether people vote for left-wing political parties. Moreover, amongst other social scientists and probably also amongst housing practitioners and policy makers there are widely held beliefs that promoting home ownership will lower the chance of people supporting left-wing ideology and left-wing political parties:

> 'The belief that homeownership will cause individuals to rethink their politics and move to the right of the political spectrum has transcended academic boundaries and become conventional wisdom.' Gilderbloom and Markham (1995, p.1602).

The only recent economics study on this issue that we are aware of uses Latin-Barometer survey data to examine the relationship between home ownership and intentions to vote for left-wing, moderate, or right-wing parties in 17 Latin American countries (Pecha and Ruprah, 2010). Using propensity score matching (PSM) to create samples of owners and renters that balance on observed covariates, these authors find no differences in voting intentions by home ownership status nor do they find any housing-related differences in self-reported voting in the last Presidential elections.[8] However, PSM estimation can only account for the effects of omitted variable bias if the selection into home ownership is based just on the same observable variables that are used to estimate the propensity scores, which seems unlikely. Otherwise, PSM is intuitively just a more flexible way of running an ordinary least squares regression (OLS), and while it performs a little better than OLS when compared with benchmarks based on actual experimental designs it still falls well short of fully ameliorating bias due to selection effects (McKenzie, Gibson and Stillman 2010).

A more plausible research design, which is in keeping with the quotation above from Gilderbloom and Markham (1995), is to use longitudinal data and to identify home ownership effects from people who transition from renting to owning or *vice versa*. Indeed, this research design is implied by the phrase 'rethink their politics and move to the right' since it suggests that some people who become owners will, as a consequence, have a change of heart and reduce their likelihood of voting for a left-wing party. Such changes are potentially observable from longitudinal observations on the same person over time as they move between various states of home ownership.

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[8] The matched samples are less than one-third of the full sample (going from 18,848 respondents down to 6,049), with the rejection of common-support assumptions appearing to especially matter for home owners (almost three-quarters of whom are removed by the matching). In other words, there are many home owners with observable characteristics that cannot be found amongst the sample of renters. After the loss of observations needed for implementing PSM, the country-level samples average 216 owners and 140 renters.

3. Data and Methods

Since 1990, the New Zealand Election Study (NZES) has surveyed individuals of voting age (18 and over) after each triennial General Election, using self-completion postal questionnaires and telephone survey top-ups (Vowles 2010). We use six waves of the NZES data, from 1996 to 2011. As noted above, self-reports of voting are validated by checking against the lists of votes cast that are held by the registrar of electors. The surveys also provide a variety of information on respondents' home ownership status, and their demographic details (age, gender, ethnicity), educational qualifications, employment, household income (in eight brackets), and location.[9]

While exact address details are not released in the public use datafiles, the files do report the census meshblock within which their address is located. There are almost 40,000 meshblocks in New Zealand, each having an average of just 110 residents, so this is a very finely scaled spatial unit.[10] To explore the sensitivity of the results to using different types of neighborhood fixed effects, we also link the meshblocks to the next largest spatial level, the Area Unit (AU), which is somewhat like a census tract in the United States. There are almost 2000 Area Units in New Zealand, with an average population of 2,300 people and a median area of 1.2 square miles (three square kilometers). These Area Units can be thought of as a suburb, and they give a slightly broader definition to what might be considered as a 'neighborhood' for looking at the effect of local home ownership rates.

In addition to the information on whether the NZES respondent owned their dwelling we use neighborhood data from the quinquennial Census of Population that reports home ownership rates and the share of public housing in each neighborhood. Table 1 provides a description of the variables we use, which show that the voting rate is 88 percent, the membership rate for political parties is less than seven percent, and amongst voters, 47 percent voted for left-wing parties.[11] The home ownership rate of NZES respondents is 78 percent, which is a little higher than the Census reports (70 percent, whether averaged at Area Unit or meshblock level); this is to be expected since NZES respondents have an average age of 50, which is older than for the average person completing a dwelling questionnaire in the census. The other main characteristics described in Table 1 that are controlled for in the regressions are ethnicity, education, employment, and household income.

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[9] Since the NZES has only bracketed data we use New Zealand Income Survey (NZIS) data to estimate median income within each bracket (which differs from the midpoint). The NZIS is an annual supplement to New Zealand's main labour market survey (the Household Labour Force Survey).

[10] In terms of physical area, the median meshblock has a perimeter of less than one mile (around 1300 meters) and a square area of 0.03 square miles (0.07 square kilometers).

[11] All sample statistics and econometric results are weighted, using the post-sampling weights provided in the NZES data files that are designed to ensure national representativeness (hence, dealing with any uneven sampling probabilities and non-response bias).

Table 1. Descriptive Statistics

	Observations	Mean	Std Deviation
Voted in General Election	6548	0.876	0.330
Voted for left-wing party	6056	0.467	0.499
Political party member	6548	0.065	0.246
NZES respondent owns their dwelling	6548	0.781	0.414
Area unit ownership rate	6548	0.695	0.116
Meshblock home ownership rate	4852	0.703	0.172
Meshblock local public housing rate	4852	0.032	0.101
Age	6548	49.871	16.356
Ethnic group: Maori	6548	0.078	0.268
Ethnic group: Maori	6548	0.014	0.119
Ethnic group: Maori	6548	0.062	0.241
School qualifications	6548	0.444	0.497
Post-school qualifications	6548	0.284	0.451
University qualified	6548	0.174	0.379
Employed	6548	0.641	0.479
Log real household income	6548	10.753	1.327
Household income imputed	6548	0.229	0.420

Note
Means and standard deviations are weighted by NZES sampling weights.

The three dependent variables that we study – whether the person voted, and if so did they vote for a left-wing party, and are they a member of a political party – are all dichotomous but we do not use probit or logit models. Instead we use variants of OLS that are designed to absorb very many fixed effects and to work with short and wide panels; thus, the estimates reported below are effectively from linear probability models (LPMs). One reason for this choice is that with approximately 1,300 fixed effects at Area Unit level, and over 3,000 at the meshblock level, non-linear models like probits are not only slow to converge, they also drop observations because of predicting with perfect success. Moreover, none of the outcomes we study are so rare that they are at highly non-linear parts of the cumulative distribution function used to convert a linear index term from probit or logit models into the probabilities that lie between zero and one. Outside of the extreme tails for rare events the slope of the distribution function is approximately linear and so there is less harm in using LPMs. Finally, one of the other drawbacks of LPMs, that they are inherently heteroscedastic, is dealt with using robust standard errors that are clustered at either the Area Unit or meshblock level, depending on the spatial scale of the neighborhood home ownership effects that we introduce.

Given the above considerations, we first estimate:

$$Y_{ij} = \beta' Z_{ij} + \delta X_{ij} + \varepsilon_{ij} \tag{1}$$

where Y_{ij} equals one if person i in meshblock j (or Area Unit j) voted (or voted for a left-wing party, or was a member of a political party) and zero otherwise and X_{ij} equals one if (s)he is a home owner. The covariates in Z_{ij} are other potential determinants of political activity, including household income, employment, education, age, and ethnicity, and ε_{ij} is a random error term. We subsequently supplement the model with the neighbourhood home ownership rate, X_j as in equation (2).

$$Y_{ij} = \beta' Z_{ij} + \delta X_{ij} + \gamma_1 X_j + \varepsilon_{ij} \qquad (2)$$

The total effect of individual home ownership on voting can be decomposed into a direct effect and an indirect effect via the local home ownership rate:

$$\frac{\partial Y_{ij}}{\partial X_{ij}} = \delta + \gamma_1 \frac{\partial X_j}{\partial X_{ij}} \qquad (3)$$

In other words, we can ask whether an individual home owner has an indirect effect on political activity via their effect on the local home ownership rate (keeping in mind that our smallest unit, the meshblock, averages just 110 people so ownership status of a single dwelling easily affects the local ownership rate). Meanwhile, the parameter on own-ownership, δ indicates the extent to which the direct effect persists after controlling for one form of neighbourhood effect coming from the local home ownership rate. Partitioning into direct and indirect effects is useful for evaluating pro-owner public policy since a case can be made that if unpaid civic duties are valued and if there is a positive indirect effect of ownership, the market level of ownership may be too low and a Pigouvian subsidy could be justified. For example, when an individual decides to become a home owner, they are unlikely to take account of the effect that their ownership has on the civic participation decisions made by other residents in their neighbourhood.

However, the approach in equations (1) to (3) is unable to reveal causal effects of home ownership if unobservable factors affect both ownership and political participation (that is, ε_{ij} is not random and instead contains fixed effects that are correlated with home ownership and with the outcomes of interest). Moreover, even if one were to aggregate individual-level data into a group (area) level analysis, averaging may not ameliorate the omitted variable bias if households with similar unobservable characteristics tend to sort into particular neighbourhoods. A neighbourhood fixed effect could help control for this neighbourhood sorting, even when a neighbourhood's residents are not fixed over time; for example, the group attitude of a neighbourhood to civic duties could be assumed to be unchanged under Tiebout's sorting conjecture (Tiebout 1956). But even with area fixed effects, for regressions at individual level it is important to control for idiosyncratic characteristics of individuals with fixed effects, which is one of the advantages provided by our longitudinal data.

4. Results

We begin by examining the effect of home ownership on whether the individual voted in the most recent General Election. The results in column (1) of Table 2 from a pooled OLS regression suggest that home owners are more likely to vote, by 5.7 percentage points ($p<0.05$), after controlling for age, ethnicity, qualifications, employment, income, and the local home ownership rate. In contrast to previous studies in the literature that have purely cross-sectional analyses, our data mix both cross-sectional (between) and longitudinal (within) variation in these results. Something more akin to a pure cross-sectional analysis is in column (2), which uses between estimation (BE), where the variables are averaged over the (varying) periods that each of the 2672 unique individuals are observed by the NZES.

The between-estimate is larger than the pooled estimate (the voting rate of owners is 7.1 percentage points higher, all else the same) and this is consistent with some of the observed cross-sectional effect being due to unobservable individual characteristics. Specifically, while the results in column (1) have a mix of within-person and between-person variation, those in column (2) only have between-person variation. That the column (1) estimates are smaller than the column (2) estimates implies that the effect of within-person variation (e.g. when someone changes homeownership status) on voting is smaller than the cross-sectional effect, since including the within variation drags down the total effect.

Before considering direct estimates of models with individual fixed effects, we consider what happens once neighborhood fixed effects (at the Area Unit level) are introduced. In these results (in column (3)) the direct home ownership effect becomes statistically insignificant, while the effect of the local home ownership rate increases but is imprecisely estimated. Next, when individual fixed effects are introduced, in the model reported in column (4), with the time-invariant idiosyncratic factors of the individual effectively differenced out and the identification coming from people who change ownership status, neither own-ownership nor the local home ownership rate have any significant effect on voting.

The results in the second panel of Table 2 introduce the area fixed effects at the finer spatial scale of meshblocks, which leads to a smaller estimation sample due to some missing data. These models also include the (small) share of housing in the meshblock that is public housing. In these results, the estimated effects of own-ownership on the decision to vote follow the same patterns as in columns (1) to (4) when local areas were defined more broadly. Thus the lack of relationship shown in the column (3) and (4) results is not an artefact of using fixed effects that come from too broad of an area to capture the influences on individual behavior. Indeed, the results with individual fixed effects (with the area fixed effects at either the Area Unit or meshblock level) show that none of the time varying characteristics, such as changes in age, employment, income, or home ownership, have any effect on the decision to vote. Evidently the propensity to vote depends on fixed (but often unobservable) characteristics of individuals rather than the various changes in the economic circumstance that they experience.

Table 2: Effect of Home Ownership and Local Home Ownership Rates on Voting in the Most Recent General Election

	Local Areas=Area Units					Local Areas=Meshblocks				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Is a homeowner	0.0568**	0.0709**	0.0254	-0.00287	0.0229	0.0481*	0.0791**	-0.00221	-0.026	-0.0156
	(0.023)	(0.028)	(0.022)	(0.034)	(0.045)	(0.028)	(0.030)	(0.070)	(0.049)	(0.064)
Local homeown rate	0.0101	-0.0182	0.284	0.0145	0.154	0.0675	-0.0193	0.124	0.233**	0.389**
	(0.057)	(0.059)	(0.279)	(0.096)	(0.116)	(0.053)	(0.048)	(0.282)	(0.034)	(0.139)
Local public housing						0.134	0.0509	1.114	0.228	0.421**
						(0.095)	(0.085)	(0.750)	(0.182)	(0.187)
Age	0.00145**	0.00120***	0.0011**	0.0609	0.0229	0.00143**	0.0012**	-0.00139	0.0628	0.0328
	(0.000)	(0.000)	(0.004)	(0.047)	(0.075)	(0.000)	(0.000)	(0.002)	(0.068)	(0.145)
Ethnic: Maori	-0.0615**	-0.0511**	-0.0615**			-0.061**	-0.0399**	-0.033		
	(0.026)	(0.028)	(0.025)			(0.035)	(0.029)	(0.117)		
Ethnic: Pacific	0.0559	0.0664*	0.0129			0.0434	0.0536	-0.467		
	(0.042)	(0.038)	(0.059)			(0.051)	(0.039)	(0.336)		
Ethnic: Other	-0.0504	-0.0423	-0.0451			-0.075*	-0.0596*	-0.191		
	(0.031)	(0.034)	(0.032)			(0.041)	(0.037)	(0.119)		
School qualifications	-0.0364	-0.0241	-0.0174			-0.0358	-0.0204	-0.0299		
	(0.025)	(0.026)	(0.023)			(0.030)	(0.027)	(0.070)		
Post-School quals	0.00539	0.0321	-0.0016			0 .00715	0.00445	-0.0417		
	(0.026)	(0.028)	(0.027)			(0.032)	(0.032)	(0.078)		
University quals	0.0362	0.0522	0.02			0.0529	0.0365	0.0586		
	(0.029)	(0.029)	(0.029)			(0.034)	(0.029)	(0.106)		
Employed	-0.00992	-0.0171	0.0136	0.0144	0.0158	-0.00836	-0.0141	0.0103	0.0215	0.0285
	(0.014)	(0.019)	(0.015)	(0.022)	(0.037)	(0.019)	(0.020)	(0.046)	(0.037)	(0.061)
Log real household income	0.00765	-0.00171	0.0059	0.00712	0.00653	0.00636	-0.00373	0.00886	0.00984	0.00927
	(0.004)	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)	(0.005)	(0.008)	(0.007)	(0.010)
Household income is imputed	-0.0100	-0.0251	0.0095	0.0316	0.0415	-0.00771	-0.0206	0.0259	0.0274	0.0644
	(0.015)	(0.015)	(0.015)	(0.020)	(0.031)	(0.020)	(0.027)	(0.040)	(0.035)	(0.055)
Year FEs	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Local Area FEs	No	No	Yes	No	No	No	No	Yes	No	No
Panel Method	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE
R-squared	0.041	0.052	0.365	0.668	0.681	0.035	0.054	0.776	0.713	0.722
Observations	6548	2672	6548	6548	2636	4852	2419	4852	4852	1869

Notes: ***, **, * represent levels of statistical significance of 1%, 5% and 10%. Robust standard errors accounting for geographical (AU or MB) clustering in ( ).

Table 3: Effect of Home Ownership and Local Home Ownership Rates on Voting for a Left-Wing Party in the Most Recent General Election

	Local Areas=Area Units					Local Areas=Meshblocks				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Is a homeowner	-0.0719***	-0.0839**	-0.0752**	-0.0266	-0.0485	-0.0728**	-0.0787**	0.0139	-0.0532	-0.0802
	(0.026)	(0.033)	(0.033)	(0.037)	(0.046)	(0.030)	(0.036)	(0.103)	(0.049)	(0.065)
Local homeown rate	-0.321***	-0.322***	-0.810*	-0.124	-0.0609	-0.177**	-0.186**	-0.0598	-0.0931	-0.0648
	(0.088)	(0.095)	(0.429)	(0.116)	(0.161)	(0.070)	(0.077)	(0.283)	(0.094)	(0.151)
Local public housing						0.114	0.15	-0.327	-0.07	-0.276
						(0.112)	(0.108)	(0.892)	(0.178)	(0.222)
Age	-0.000234	-0.000137	0.0000275	0.002	-0.0672	-0.000788	-0.000272	-0.000277	-0.0509	-0.111
	(0.001)	(0.001)	(0.001)	(0.047)	(0.062)	(0.001)	(0.001)	(0.003)	(0.050)	(0.115)
Ethnic: Maori	0.185***	0.188***	0.108***			0.244***	0.215***	0.22***		
	(0.029)	(0.036)	(0.039)			(0.041)	(0.041)	(0.105)		
Ethnic: Pacific	0.232***	0.259***	0.192			0.185**	0.211***	0.319		
	(0.067)	(0.078)	(0.107)			(0.078)	(0.080)	(0.322)		
Ethnic: Other	0.0453	0.0516	0.0405			0.0208	0.035	-0.0752		
	(0.033)	(0.046)	(0.044)			(0.041)	(0.051)	(0.119)		
School qualifications	-0.0564**	-0.0298	-0.00121			-0.0423	-0.023	0.00856		
	(0.032)	(0.045)	(0.036)			(0.036)	(0.046)	(0.051)		
Post-School quals	-0.0483*	-0.0167	-0.00137			-0.0312	0.00703	0.0709		
	(0.035)	(0.047)	(0.040)			(0.040)	(0.049)	(0.075)		
University quals	0.0274	0.0797	0.098**			0.0547	0.118**	-0.0017		
	(0.038)	(0.051)	(0.045)			(0.042)	(0.051)	(0.114)		
Employed	-0.0504***	-0.0494***	-0.0503**	-0.0489*	-0.0799*	-0.0558**	-0.0505*	-0.106**	-0.0544	-0.0971
	(0.020)	(0.026)	(0.024)	(0.028)	(0.043)	(0.023)	(0.027)	(0.053)	(0.037)	(0.059)
Log real household income	-0.0337***	-0.0475***	-0.0332***	-0.00308	0.00126	-0.0306***	-0.048***	-0.00393	0.0000382	0.00963
	(0.006)	(0.009)	(0.007)	(0.007)	(0.008)	(0.007)	(0.009)	(0.009)	(0.008)	(0.010)
Household income is imputed	0.0576***	0.0777***	0.0661***	-0.00734	-0.0214	0.0559**	0.0892**	0.0166	0.0275	0.0001
	(0.020)	(0.030)	(0.022)	(0.021)	(0.029)	(0.024)	(0.033)	(0.041)	(0.029)	(0.036)
Year FEs	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Local Area FEs	No	No	Yes	No	No	No	No	Yes	No	No
Panel Method	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE
R-squared	0.062	0.069	0.434	0.815	0.802	0.062	0.073	0.876	0.871	0.859
Observations	6056	2626	6056	6056	2391	4457	2383	4457	4457	1678

Notes: ***, **, * represent levels of statistical significance of 1%, 5% and 10%. Robust standard errors accounting for geographical (AU or MB) clustering in ( ).

Amongst the individuals who did vote in the General Election, there appears to be a statistically significant effect of home owners being less likely to vote for left-wing political parties (Table 3). This effect is substantively large, with being a home owner seeming to reduce the likelihood of voting left-wing by about seven percentage points according to the pooled OLS results in column (1). Since the local home ownership rate also has a large negative coefficient, (about -0.32 in columns (1) and (2)) there is a further indirect effect of own home ownership, by making left-wing voting by neighbors less likely, following equation (3).

Similarly to the pattern in Table 2, the pooled estimate in column (1) (or column (6)) comes out smaller than the between estimates in column (2) (or column (7)). If fixed effects at the Area Unit level are included, there is a slight strengthening of the own-ownership and local ownership effects (although not when meshblock-level fixed effects are used) compared to the pooled estimates in column (1).

However, these apparent effects of home ownership on not voting left-wing disappear once individual fixed effects are included in column (4) (or in column (9)). Thus, in contrast to the Gilderbloom and Markham (1995) quotation noted above, which suggested that becoming a home owner would cause people to rethink their politics and move to the right, it seems that becoming a home owner has no effect on changing what sort of political party one votes for. Crucially, in the models with individual fixed effects the identification comes from the people who change their home ownership status over time, and so this corresponds to the thought experiment of seeing what happens when a renter becomes an owner or *vice versa*. In fact, none of the time-variant factors show any statistical significance in the models with individual fixed effects, which suggests that the decision to vote for a left-wing party is more of an inherent feature of an individual and not something that changes as their observed circumstances change.

The results for whether the individual is a member of a political party are reported in Table 4. There is no significant direct effect of being a home owner in any of the models reported. When neighbourhood fixed effects are included at the Area Unit level, the home ownership rate shows a significant positive association with political party membership (although not when meshblock fixed effects are used). This effect disappears once the individual fixed effects are added, in the results reported in column (4). Thus these results provide no support for the hypothesis that home owners, and those living in areas with higher home ownership rates, are more likely to join political parties.

Table 4: Effect of Home Ownership and Local Home Ownership Rates on Being a Member of a Political Party

	Local Areas=Area Units					Local Areas=Meshblocks				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Is a homeowner	0.00568	0.0165	-0.00247	0.0144	0.00373	-0.00424	0.00503	-0.0187	-0.00659	-0.0142
	(0.008)	(0.010)	(0.012)	(0.021)	(0.026)	(0.007)	(0.010)	(0.044)	(0.017)	(0.024)
Local homeown rate	-0.0206	-0.00578	0.525***	-0.0202	-0.0165	0.00713	0.00934	0.0803	0.02	0.0154
	(0.030)	(0.033)	(0.188)	(0.065)	(0.059)	(0.020)	(0.025)	(0.153)	(0.052)	(0.053)
Local public housing						-0.0172	-0.00292	0.186	0.0283	-0.072
						(0.025)	(0.035)	(0.264)	(0.077)	(0.085)
Age	0.002***	0.00163***	0.002***	0.0141	0.0644	0.00196***	0.00183***	0.00198	0.0281	0.0867
	(0.000)	(0.000)	(0.000)	(0.038)	(0.050)	(0.000)	(0.000)	(0.001)	(0.040)	(0.079)
Ethnic: Maori	0.0431***	0.08***	0.0295*			0.0225	0.0496***	-0.031		
	(0.013)	(0.016)	(0.019)			(0.015)	(0.017)	(0.081)		
Ethnic: Pacific	0.00731	-0.00827	0.0175			-0.0173	-0.00654	-0.0184		
	(0.023)	(0.020)	(0.037)			(0.018)	(0.020)	(0.038)		
Ethnic: Other	-0.0188**	-0.0161	-0.0248			-0.0278***	-0.0205	-0.0233		
	(0.009)	(0.013)	(0.015)			(0.006)	(0.013)	(0.038)		
School qualifications	0.00429	0.0199	-0.00224			0.0074	-0.0183	-0.0233		
	(0.013)	(0.019)	(0.016)			(0.014)	(0.019)	(0.025)		
Post-School quals	0.0345**	0.0494**	0.0208			0.029*	0.0446**	0.00914		
	(0.015)	(0.020)	(0.018)			(0.015)	(0.020)	(0.039)		
University quals	0.0299**	0.0460**	0.0126			0.0329**	0.0459**	0.0262		
	(0.015)	(0.020)	(0.020)			(0.015)	(0.021)	(0.066)		
Employed	0.00523	-0.0229	0.00163	0.0178	0.00799	0.00965	0.00935	-0.00259	0.0177	0.0098
	(0.007)	(0.010)	(0.010)	(0.016)	(0.021)	(0.008)	(0.009)	(0.032)	(0.020)	(0.027)
Log real household income	0.00352	-0.0000861	0.003	0.00386	0.00251	0.00368**	-0.00165	0.0085	0.00608	0.00314
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.006)	(0.004)	(0.003)
Household income is imputed	0.00471	0.0222	0.00711	0.0159	0.0146	-0.00166	0.00902	0.017	0.0148	0.0274
	(0.009)	(0.012)	(0.011)	(0.015)	(0.021)	(0.009)	(0.012)	(0.027)	(0.020)	(0.031)
Year FEs	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Local Area FEs	No	No	Yes	No	No	No	No	Yes	No	No
Panel Method	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE	Pooled	BE	Pooled w/ Area FE	Individual FE	Trimmed FE
R-squared	0.039	0.035	0.273	0.576	0.535	0.021	0.029	0.677	0.678	0.605
Observations	6548	2672	6548	6548	2636	4852	2419	4852	4852	1869

Notes: ***, **, * represent levels of statistical significance of 1%, 5% and 10%. Robust standard errors accounting for geographical (AU or MB) clustering in ( ).

In Tables 2, 3, and 4 there are results in columns (5) and columns (10) that we have not yet discussed. These results are for trimmed samples, and are based on the strategy suggested by Crump *et al.* (2009), of first estimating a propensity score for the likelihood that each observation is a homeowner, and dropping observations with estimated scores outside the range [0.1, 0.9]. The reason for this trimming is that our main results (in columns (4) and (9)) with individual fixed effects are reliant on the within variation for movers. The key assumption is that someone who is an owner would have showed similar trends in the outcome variables in the absence of being an owner. This is less credible if the people who switch from owning to renting and *vice versa* (that is, observations whose within variation over the sample period provides the identification) have very different characteristics to non-switchers (the persistent homeowners and persistent renters). The systematic approach to pre-screening the sample that Crump et al (2009) suggest helps to ensure similar characteristics for the non-switchers and the switchers, and brings the analysis closer to the ideal of a randomized control trial.

Using the restricted range [0.1, 0.9] of estimated propensity scores reduces the number of observations from 6548 to 2636. The magnitude of this reduction is a sign of how difficult it is to get covariate balance in observational data where the 'treatment' (home ownership in this case) is self-selected. When we control for the observable covariates distribution by this pre-screening, the insignificant estimates of home ownership impacts in columns (4) and (9) that used individual fixed effects are also observed in columns (5) and (10). This robustness check holds for all three outcome variables, and so this implies that the sort of unobserved factors that predispose people to being a home owner also make them more likely to vote and less likely to vote for left wing political parties.

5. Conclusions and Discussion

A widespread and costly bias in public policy in favor of home ownership is sometimes justified by the claimed externalities, where higher home ownership is meant to be a cause of greater civic and political participation. Despite the potential caveats to these relationships, due to variation in housing supply, and the possibility of diminishing effects as home ownership rates rise, a large body of published social science research broadly supports claims that these externalities exist. However, much of this research is hampered by relying on data and research designs that may not identify causal effects from observed correlations.

In this study we have used longitudinal data from New Zealand on whether people voted in General Elections (where the self-reports are validated against polling records), whether voters voted for left-wing parties, and whether individuals were members of a political party. In the cross-section, whether someone voted is correlated with whether they are a home owner, while political party membership is correlated with local home ownership rates. Voting for a left-wing political party is negatively correlated with both own home ownership and the local home ownership rate. However, all of these relationships disappear once we using individual fixed effects where models are effectively identified by changes in home ownership status.

Evidently, the unobserved factors that predispose people to being a home owner also make them more likely to vote, to join political parties, or to vote for right-wing political parties. Once these unobserved factors are controlled for there is no causal effect of home ownership on political activity. This finding weakens the justification for maintaining costly policy biases in favour of home ownership.

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