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## Article

# The effect of house prices on fertility: Evidence from Canada

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## The effect of house prices on fertility: evidence from Canada

*Jeremy Clark and Ana Ferrer*

### Abstract

Persistent house price increases are a likely candidate for consideration in fertility decisions. Theoretically, higher housing prices will cause renters to desire fewer additional children, but home owners to desire more children if they already have sufficient housing and low substitution between children and other “goods”, and fewer children otherwise. In this paper, the authors combine longitudinal data from the Canadian Survey of Labour Income and Dynamics (SLID) and averaged housing price data from the Canadian Real Estate Association to estimate the effect of housing prices on fertility in a housing market that has historically been less volatile and more conservative than its American counterpart has. They ask whether changes in lagged housing price affect the marginal fertility of homeowner and renter women aged 18–45. They present results both excluding and including those who move outside their initial real estate board area, using initial area housing prices as an instrument in the latter case. For homeowners, but not renters, the authors predominantly find evidence that lagged housing prices have a positive effect on marginal fertility and possibly on completed fertility. These pro-natal effects are confined to non-movers.

**JEL** D13 J13 J18 R21

**Keywords** Economic determinants of fertility; housing prices; wealth effects; home ownership

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## 1 Introduction

Most Western societies are concerned their fertility rates are far below replacement levels, while others are concerned their rates are far above. A key question in the face of such concerns is the extent to which fertility rates respond to various price signals. Researchers in economics and demography have tried to estimate the extent to which families' fertility rates respond to the effects of changing wages or income, or to changes in tax and welfare policies that affect wages or income. Within this context, housing prices are a key variable of interest since housing is the major store of wealth for most families, as well as one of the main determinants of child-raising costs. Hence, the large increases in housing prices experienced in most OECD countries over the 2000's have sparked concern about housing affordability, family formation and the adequacy of neighbourhood amenities (Adsera and Ferrer 2018). In Canada, average house prices more than doubled between 2000 and 2010, with some large urban areas such as Toronto, Vancouver or Calgary experiencing annual housing price growth rates of over 30% at some points in this period. Such price increases have drawn academic, public and policy attention to the effects of housing prices on demographic trends.<sup>1</sup>

The dual role of housing as a major store of wealth and key component of the "price" of raising children creates ambiguity regarding the influence of housing price on fertility. An exogenous increase in the price of housing may reduce family fertility by making the space needed for raising children more expensive, or by requiring both parents to work full time to service a mortgage. Yet for families who already own housing, an increase in the price of housing creates wealth effects, accessible by moving or by home equity extraction via mortgage refinancing or opening lines of credit. Such wealth effects may increase homeowners' fertility, particularly if their willingness to substitute between children and other "goods" is reasonably limited, and they already have sufficient housing. Thus, increases in the price of housing could be expected to have potentially very different effects on the fertility of home owners and renters, and among home owners, between those who own much or little housing, and those who are flexible or inflexible about desired family size. The net effect of housing prices on fertility is thus an empirical question.

The empirical literature looks at this question conceding that, while housing and fertility are long term decisions, they are sufficiently fluid that large house-price increases may change the fertility decisions of people at the margin. A small number of primarily US-based studies have looked into the effect of housing price on fertility.<sup>2</sup> Walker (1995) attempts to explain variations in Swedish fertility as a function of its "shadow price", which includes the additional expenditures on housing that children pose for families. In descriptive analysis, Walker finds a strong negative correlation between fertility and its comprehensive "price", though the effect of housing expenditures alone is not identified. Curtis and Waldfogel (2009) employ a similar conceptual framework as Walker, but use panel regression based on the U.S. Fragile Families and Child Wellbeing study to test whether unmarried mothers in cities with higher housing price indices are less likely to have additional children, and find this to be the case. Simon and Tamura (2009) use individual public use micro data (IPUM) from successive waves of the United States census

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<sup>1</sup> See Hulchanski (2005), Government of Canada (2011), or Bryan et al. (2017).

<sup>2</sup> A related literature examines the effect of housing price on household formation (Borsch-Supan (1986), Giannelli and Monfardini (2003), Hughes (2003), Clark (2012)) and dissolution (Farnham et al., 2009).

linked with the median CMSA rental rate per room. They too find a significant negative relationship between the price of living space and the number of children living in households. Simon and Tamura also distinguish whether rental price affects spacing (delay) vs. total fertility by examining the number of children ever born to women aged 40 or greater. They find that a higher rental price per room both delays mother's age at first birth, and reduces completed fertility for older women. In contrast, Feyrer et al. (2008), who use the Office of Federal Housing Oversight's repeat sales index at state or MSA level, along with IPUM data from the 1980 and 2000 census, find a positive or no relationship between total fertility and housing price. None of these four studies, however, distinguishes between home owners and renters, for whom changes in housing price could be predicted to have different effects. They also cannot follow individuals over time to control for household-specific unobserved factors such as differing intrinsic desire for children, or willingness to substitute between children and other "goods".

There are, however, four recent papers by Lovenheim and Mumford (2013), Dettling and Kearney (2014), Atalay et al. (2017) and Aksoy (2016) that recognize the distinction between home owners and renters, and are able to use some form of fixed effects. Lovenheim and Mumford (2013) follow panels of individual women using restricted geo-coded data between 1985 and 2007 from the American Panel Study on Income Dynamics (PSID). They estimate the effect of changes in house price on the subsequent likelihood of having a child, controlling for the number of other children already in the household. For home owners house prices are derived from self-reported household values, whereas for home renters, changes in MSA level average housing price growth are used. Under most specifications, results indicate a positive relationship between house price and fertility for homeowners (meaning wealth effects dominate substitution effects), but surprisingly no significant negative relationship for renters (where only substitution effects should be in operation). Though not conclusive, Lovenheim and Mumford argue that house prices are likely raising homeowners' total (completed) fertility, rather than just reducing their spacing of births, since positive effects are found even for women aged 35–39 and 40–44. In a paper concerning the Australian case, Atalay et al. (2017) carry out a similar approach, but using individual level panel data from the Household Income and Labour Dynamics in Australia. Focussing on non-movers, and using pooled cross section estimates with fixed effects for local economic conditions, they also find a positive relationship between house price and fertility for homeowners, but not a significant negative relationship for renters.

Dettling and Kearney (2014) similarly recognize that housing prices may have different effects for home owners and renters. These authors use US vital statistics to follow aggregated MSA's over time rather than individuals. They estimate the effect of lagged MSA level house price levels (using the HPI on repeat sales transactions on homes with conforming, conventional mortgages securitised through Fannie Mae or Freddie Mac) and of MSA level home-ownership rates on MSA group level fertility. They include year and MSA level fixed effects as well as measures of time-varying MSA conditions such as unemployment. Across numerous specifications, Dettling and Kearney find that the main effect of an increase in lagged house price on the MSA fertility rate is negative, reflecting the negative effect of high housing costs on the fertility of renters. In contrast, an interaction term of house price and home ownership rate is positive, and of a greater magnitude than the main effect of house price. Thus, an increase in the aggregate home ownership rate raises the effect of house price on fertility, so that for MSA's with even moderate levels of home ownership, higher house prices have a net positive effect on

fertility. In a similar vein, Aksoy examines the effects of house prices indices on fertility at the county level in the United Kingdom, instrumenting for house price with county measures of planning restrictiveness. Similar to Dettling and Kearney with aggregated data, Aksoy finds that higher house prices are pro-natal for homeowners, and reduce fertility for renters. However, unlike the United States' case, Aksoy finds that rises in the house price index are anti-natal overall for the United Kingdom.

Our contributions to this literature is to extend the study of housing prices on fertility to Canada, a setting similar to the United States, but with greater stability in the housing and mortgage markets over the time period considered here. We believe that studying the Canadian case offers an interesting insight into the effect of housing prices on fertility. The differences between the home financing markets between Canada and the United States during the period of study suggest that to the extent that children are a normal good, house prices should have a larger effect on Canadian markets. This is because Canadian home buyers tend to make higher down payments and receive no tax benefits from mortgage interest, so are likely to have more equity in their homes, which would increase the income effect of house price changes.

The main challenge of studying the Canadian case resides in the level of aggregation of available housing price data. Most Canadian surveys do not collect self-reported information on housing prices (although it is included in the 2010 Canadian Household Survey). An HPI on repeat sales transactions is only available for recent years, and for a limited number of Canadian cities.<sup>3</sup> Other than this promising recent HPI, the Canadian Real Estate Association (CREA) is the only source of historic house pricing data, averaged by real estate board and without control for type or quality of residential housing unit. Our paper is the first to collect this price data directly from CREA to study the effect of house price on fertility using rural and urban areas across Canada. We build empirically on the approach of Lovenheim and Mumford (2013), using confidential geo-coded longitudinal Canadian data for women aged 18–45 from successive waves of the Survey of Labour and Income Dynamics (SLID), matched to time-series data on average housing price at the real estate board level from the Canadian Real Estate Association's Multiple Listing Service data set (CREA MLS). We distinguish between home owners and renters when examining the effects of changes in house price on fertility, and examine price effects across both urban and rural areas. We take various steps to address the challenges posed to identification of location self-selection. Our first strategy, similar to Atalay et al. (2017), is to focus on women whose families have not moved outside their initial real estate board boundary during the six years they are included in the SLID. These "non-movers" are more likely to experience house price changes in their areas as exogenous shocks. In addition, we also include women whose families move across real estate boards and address potential endogeneity of house prices for those who move, by instrumenting housing prices using initial location housing price.

The rest of the paper proceeds as follows. In Section 2, we briefly summarize theoretical predictions regarding housing price and fertility, and the differences in housing and its related credit markets that exist between Canada and the United States. In Section 3 we describe our data

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<sup>3</sup> The Teranet–National Bank House Price Index measures the rate of change of Canadian single-family home prices, based on the property records of public land registries using repeated sales estimation methodology. The monthly index covers eleven Canadian metropolitan areas and has only very recently been released dating back to 1999. Although of higher quality than the CREA data we use, this price data has only limited geographical coverage and would reduce the sample size considerably, given that the SLID oversamples rural areas.

and empirical estimation strategy. In Section 4 we present our results and robustness checks, and provide a final discussion and conclusions in Section 5.

## **2 Housing Prices and Fertility in Theory, and Housing Markets in Practice**

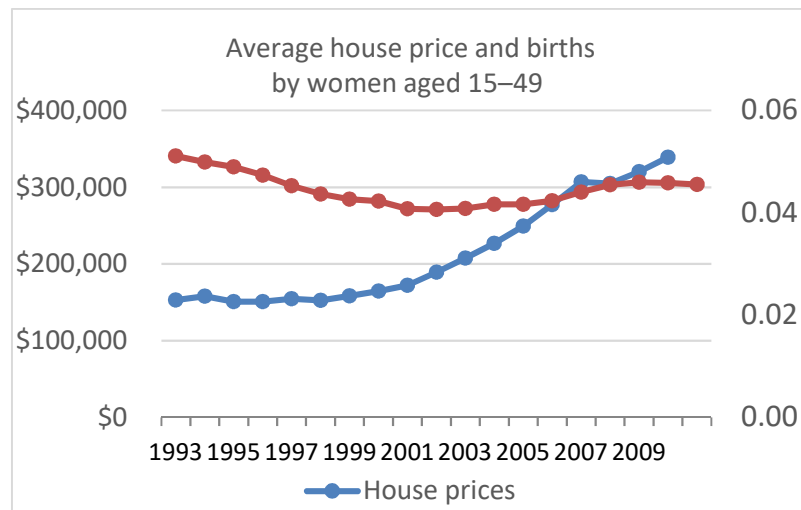
Liu and Clark (2016) provide a recent theoretical treatment of the relationship between house prices and fertility. The paper assumes that unitary households have constant elasticity of substitution preferences over number of children, leisure, and a composite good. Fertility is modelled using a Cobb Douglas household production requiring time and housing. Households choose whether to rent or buy housing based on the price of each, and maximise utility subject to time and budget constraints. Liu and Clark's model predicts that renters will respond to an increase in the cost of housing by desiring smaller families. For homeowners, those who have a low willingness to substitute between family size and leisure or consumption of other goods, and who already own substantial housing, are predicted to respond to an increase in house prices by desiring more children. However if such homeowners' initial quantity (size) of housing is sufficiently small, they may desire fewer children. In contrast, those homeowners who are more willing to substitute between family size and other things are predicted to respond to higher housing prices by desiring fewer children, regardless of how much housing they own. This model emphasizes the importance of a family's renter/owner status, as well as its (generally unobserved) elasticity of substitution between children and other 'goods', and for owners, the physical size of housing already owned.

Our contribution to the literature lies in the examination of the Canadian housing market. This offers a useful complement to the US studies, given the two countries' overall institutional similarities, paired with idiosyncratic differences in their housing markets. In Canada (as in the US), housing prices remained stable over the late 1990's and increased steadily over the 2000's, while fertility diminished over the period (see Figure 1 using Canadian data from Cansim). However, the institutional arrangements, incentives and outcomes of housing markets have been more stable and conservative in Canada than in the United States. House prices rose more precipitously in the United States than Canada prior to their peak in 2006, and fell more precipitously during the subprime mortgage lending crisis of 2007 to 2009 even as Canadian house prices quickly recovered (Carney 2011).<sup>4</sup> North American housing policy promotes home ownership as a social and economic end, but using different means. In the United States

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<sup>4</sup> By 2016, United States housing prices were still 20% below 2006 peak levels in real terms ([www.economist.com/blogs/graphicdetail/2016/08/daily-chart-20](http://www.economist.com/blogs/graphicdetail/2016/08/daily-chart-20)).

*Figure 1: Canadian average house price and births by women aged 15–49, 1993–2010*



**Source:** Authors' calculations using CREA-MLS data and CANSIM Table 051-0001 and Table 051-0004 Statistics Canada.

mortgage interest is tax deductible, and government sponsored housing agencies Fannie Mae and Freddie Mac have the explicit objective of enabling lower income/higher risk families to achieve home ownership. In Canada homeowners may instead make interest free tax withdrawals from retirement savings accounts, and neither capital gains nor imputed rents are taxed. These differences create incentives for Canadian home buyers to make relatively larger down payments.<sup>5</sup> Another difference is that securitisation of mortgage debt is widespread in the United States, via the debt bundling and on-selling activities of Fannie Mae (for banks) and Freddie Mac (for savings and loans). Capital markets rather than bank depositories thus make up the dominant source of funds for US mortgages (Green and Wachter 2005), with the reverse holding in Canada. As a result, the 10, 15 or 20 year fixed term mortgages funded by securitisation in the United States are not available in Canada. Canadian mortgages also tend to be “full recourse,” meaning banks can pursue defaulting borrowers beyond the mortgaged property itself, and they tend to include penalties for early repayment, unlike in the United States (Green and Wachter, 2005). Canadian mortgage lenders have also been more conservative, and operated in a more conservative regulatory framework, than their US counterparts (Concetta-Chiuri and Japelli 2010). As a result of these differences, typical loan-to-value (LTV) ratios and regulated LTV maximums have been lower in Canada than in the United States, as have the proportion of mortgages made to sub-prime borrowers or those without mortgage insurance (Tsatsaronis and Zhu 2004, Green and Walker 2005).

A final difference with potential relevance for fertility is a lag in the prevalence of uptake in home equity extraction (i.e. borrowing against home equity) by homeowners in the two countries.

<sup>5</sup> According to a study by the Bank of Montreal, the average Canadian first time home buyer in 2013 paid 16% of the house value as down payment (Genworth Canada), compared to 11% in the United States in 2016 (Realtor Magazine 2017). Overall, Canadian home equity was estimated to be between 66% and 72% during our sample period (Cooper, 2017), compared to the US where it was estimated to be between 38% and 63% (Board of Governors of the Federal Reserve, 2018).

Greenspan and Kennedy (2005) report increased prevalence of such extraction by American homeowners in the late 1990's and early 2000's, whereas as of 2004 Tsatsaronis and Zhu report that mortgage equity withdrawal was available but "unused" in Canada. However, Bailliu et al. (2011–2012) find this reticence among Canadian home owners to have abated subsequently, driven partly by continuously rising housing prices and household debt in major urban areas such as Toronto and Vancouver.<sup>6</sup>

What effect might these institutional differences in housing markets have on the relationship between housing price and fertility? We expect that larger down payments and higher equity in Canada could magnify the income effects associated with rises in house price. It is also possible that the greater volatility of US house prices could make US families less likely to immediately adjust their desired fertility (a long term 'investment/consumption good') to changes in housing price if they view these changes to be more transitory. Either effect could magnify the effect of rising house prices on fertility in Canada, relative to the United States.

### **3 Data and Empirical Methodology**

Our two main sources of data for this paper are the Canadian Survey of Income and Labour Dynamics (SLID) and house price data at real estate board level constructed from the Canadian Real Estate Association (CREA).<sup>7</sup> We use the confidential files of the SLID to obtain panel information about Canadian households from 1994 to 2010. The SLID is a household survey that covers all individuals in Canada, excluding residents of Indian Reserves, northern territories, or of institutions. The survey is designed as a series of two overlapping panels, each panel consisting of roughly 17,000 households surveyed for six consecutive years. A new panel is introduced every three years, so two panels always overlap. Besides ample information on household composition or income, the SLID also provides information on a broad selection of human capital variables, labour force experience and demographic characteristics such as education and family relationships. Its richness of information and relatively large sample size make the SLID a valuable dataset for our purposes. Its six year panel nature allows us to control for stable but unobserved household characteristics that may influence family size, albeit not for the same length as longer panels such as the PSID used by Lovenheim and Mumford in the United States.

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<sup>6</sup> Given differences in equity held, it would have been interesting to explore the effects of both house price and equity on fertility. This would be possible in theory with the SLID survey, as it includes a question on monthly mortgage payments. Preliminary tests found a mortgage indicator variable was not significant in fertility regressions, however question non-response meant we had less than half the observations than in our current sample.

<sup>7</sup> With regards to data availability, the analysis in this paper is carried out using (non-confidential) real estate data on Canadian housing prices linked to restricted-access files of the Survey of Labour and Income Dynamics (SLID), conducted by Statistic Canada and accessed at the Canadian Research Data Centre Network (<https://crdcn.org/>) facilities. Only vetted results are allowed outside the centre to preserve confidentiality. Therefore, we are not at liberty to make the SLID data publicly available. We are however committed to help interested researchers to access the data at one of the CRDC facilities across Canada. Application process and guidelines can be found here (<https://www.statcan.gc.ca/eng/rdc/process>). Our STATA syntax codes and the data on Canadian housing prices can be made available to researchers working at any CRDC through the network.



However, because the SLID does not ask home owners to estimate the value of their homes, a key challenge in this analysis was to obtain a consistent measure of housing price that was as detailed geographically as possible. There is no official source of resale house prices in Canada. The Teranet National Bank House Price Index (HPI) has recently released information dating back to 1999, but covers only 11 Census Metropolitan Areas (CMAs). The best information available for an extended period of time, for all regions of the country, comes from the Canadian Real Estate Association's Multiple Listing Service data set (CREA MLS), which we collected for the period 1991–2010. This data set provides mean house prices, (i.e. total sales value over total number of residential units sold) for 92 urban and rural 'boundaries' in Canada, generally the geographic boundaries of 92 real estate boards (REBs).<sup>8</sup> While 92 real estate board prices offer greater coverage than 11 MSA's, for a country the size of Canada some real estate board boundaries are quite large, and will likely contain sizeable variation in house prices within them. For panel regressions, the relatively small number of REBs should not present a problem if there are sufficient co-movements in house prices in adjacent low and high price neighbourhoods within REB's over time. While we know of limited evidence regarding this question, research by Clapp and Ross (2004) finds this to be true between the towns of labour market areas (similar to metropolitan statistical areas) in the American state of Connecticut.

Unfortunately, the CREA MLS boundaries do not match official boundaries, such as census tracts or dissemination areas used by government agencies. In order to match prices to house owners or renters, we use the census subdivision of a SLID respondent – which translates roughly into the first 3 digits of their 6 digit postal code – to assign respondents into the 92 urban and rural boundaries of CREA MLS. For the matching procedure, we collected images of the real estate boundaries from the various provincial real estate board websites across Canada (Alberta Real Estate Association, 2013; British Columbia Real Estate Association, 2013; Nova Scotia Association of Realtors, 2013; Ontario Real Estate Association, 2013; Winnipeg Realtors, 2013). When this data was not publically available, we consulted with real estate board representatives in order to define the provincial real estate boundaries on hard copy maps (Saskatchewan, New Brunswick, and the Toronto Area within Ontario). We obtained digital boundary data for the Real Estate Boards by rectifying the images to their geographic location and digitizing polygon files using Esri ArcGIS 10.0 software (Esri, 2013). We used Statistics Canada's census subdivision (CSD) as the aggregate geographic level of the census data. The CSD level of Canadian census data corresponds to "a municipality or an area that is deemed to be equivalent to a municipality for statistical reporting purposes" (StatsCan, 2001). Because this area corresponds generally to the size of the real estate areas, this level of aggregation seemed appropriate. The sales data for a particular Real Estate Board Area was linked to a unique CSD when the geographic centre of the CSD area fell within that particular Real Estate Boundary.

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<sup>8</sup> We also collected a secondary data set - called CREA MLS II – which provides median house prices for roughly 123 boundaries. Unfortunately, this secondary data set was only available for the years 2005–2010, and was limited to 14 urban centres in Canada. A full list of regions contained in CREA MLS and CREA MLS II is provided in the appendix.

Alberta, British Columbia, Saskatchewan, Manitoba, Ontario, Nova Scotia and New Brunswick were all spatially linked as described above.<sup>9</sup> In some cases, the available real estate board maps did not provide complete coverage of a province, and in those cases we created an ‘other’ category to represent the rest of the province (in Saskatchewan, Manitoba, and New Brunswick). According to experts these were mostly scarcely populated rural areas that have not seen great variation in prices. Within the Province of Quebec, the Quebec Federation of Real Estate Boards provided sales data by Census Metropolitan Area (CMA), which unlike in the rest of Canada could be linked directly to the CSDs within the six CMAs. Unfortunately, this was not available for the final three years of the sample. Those parts of Quebec outside these CMA’s were classified into a single ‘other’ category. The real estate data for the two provinces of Prince Edward Island and Newfoundland/Labrador were each a single value, and thus all unique CSD identifiers within each province were linked to a single province-wide price. Real estate data for northern territories was collected, but not used because the SLID does not cover these areas.

We measure marginal fertility with an indicator variable that equals one if the woman gave birth last year. Our empirical analysis focuses primarily on the effect of (lagged) housing price levels, though we also consider lagged change in price in Section 4.3.<sup>10</sup> In addition, because the proportion of women who give birth each year is small, we emphasize logit rather than linear probability model results. For homeowners and renters separately, we estimate the likelihood that family  $i$  (containing a female head or spouse aged 18–45) will have an additional child in year  $t$  (Fict) as a function of the mean REB housing price (HPct-1) as reported by the real estate board for  $i$ ’s city or rural region  $c$  with a one year lag ( $t-1$ ). More precisely, fertility is measured over the period between surveys - for example our dependent variable is equal to 1 if the woman had a baby between May 1995 and May 1996 – while our one year lagged house price variable corresponds to the average for the year previous to the survey, i.e. calendar 1995. This lag should provide appropriate information about housing price trends surrounding conception for most births in 1996, particularly if families forecast prices later in a calendar year from its earlier months. However, depending on ease of conception or the rate of house price increase in-year, a one year lag might not be sufficient – perhaps the likelihood of women giving birth between May 1995 and May 1996 is affected by calendar 1994 average house price. We therefore try lags of two years in robustness checks (see Section 4.3). The model estimated is the logistic transformation of

$$F_{ict} = \beta_0 HP_{ct-1} + \beta_1 X_{ict-1} + \beta_2 UR_{ct-1} + i + t. \quad (1)$$

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<sup>9</sup> A complication arose regarding Canada’s largest city Toronto in Ontario – whose real estate board sets intra-city boundaries that diverge from those for the Toronto area set by the Ontario association of real estate boards. We opted to use the price/boundaries provided by the provincial association, which necessitated imputing a house price for the combined area of Toronto and Brampton using provincially-sourced data.

<sup>10</sup> Lovenheim and Mumford (2013) argue that using price levels may represent different changes in wealth for households who own more or less of the equity in their homes, whereas using changes in price should represent the same degree of wealth increase for all types of owners. We believe that using lagged price levels is appropriate for person fixed effects analysis, and that the more conservative nature of mortgage markets in Canada (with lower LTV ratios and higher equity) lessen the force of this argument. We thus adopt Dettling and Kearney’s (2014) focus on price levels, though we compare the results with price change in Section 4.3.

We include a set of standard controls in the fertility regressions ( $X_{ict-1}$ ), including the woman's age, family income – to reflect current economic conditions – marital status, and labour force status – to account for time constraints – education and previous number of children born – which are related to fertility preferences. We also include the overall provincial unemployment rate ( $UR$ ) to capture local economic conditions in the area, and year dummies to capture time trends. We use first simple pooled cross section models that pool all observations from all SLID panels to estimate the coefficients, and later turn to panel data models that take individual effects ( $i$ ) into consideration. Individual fixed effects control for within panel time invariant characteristics, such as fertility preferences or degree of financial awareness, and so on. Observations are clustered to the REB level at which house prices are available, to yield cluster-robust standard errors.

Our marginal fertility measure cannot itself identify whether changes in housing price are affecting total fertility, or merely its timing. We cannot provide conclusive evidence either way, but we will extend our analysis to add age – house price interaction terms, to see how price effects may differ over women's childbearing years. If, for example, the same effects are found for older women as are found for the sample overall, we will take this as suggestive evidence that house prices are affecting total fertility. We similarly test for heterogeneity of effects by previous number of children, and household income.

We select a sample of women aged 18 to 45, who are married or live common law in the first year of the panel. This selection aims to capture relevant fertile years of women who have already selected a spouse/partner. Given our selection criteria, note that our control for married/common law status vs. separated/single status can vary only in the second or higher year of each panel. We distinguish between women who live in homes owned by one or more members of the household ("owners") and women living in non-owner occupied households ("renters"). In order to observe any differential effects of house price changes on the fertility of the two groups, we focus only on women who do not change their 'owner'/'renter' status during their six year panel. Hence, we exclude a number of changes in ownership status that are likely to be associated with major life transitions, such as a couple's purchase of a first house or a couple's separation, which may have an independent effect on fertility. We acknowledge this limitation, but consider that a proper analysis of these transitions requires a different treatment and possibly data with specific questions on fertility intentions and preferences. This restriction reduces our initial number of observations by 25% overall.

We initially restrict our sample to those women who remain at the same residence over the six years of their panel, or who move only within their initial real estate board area, and thus were assigned the same REB house price. By focusing on individuals who remain in their REB, house price increases will more resemble "exogenous" changes in household wealth. Ideally, however, we would like to account for the decision to move. Movers could potentially be trying to realize a(n observable) change in wealth, which might be related to their child-bearing decisions. For example, if women who do not want children stay in expensive areas, but those who want more children leave expensive REB's to afford bigger houses, omitting them from the sample would bias our estimates downwards, reflecting a spurious correlation between house price increases and *reduced* fertility. Seeking causal effects while including movers has the potential confounding effect of assuming that women who move from expensive to less expensive areas with a desire to increase fertility are unaffected by house price increases, when in fact they are. Hence, when including movers across REB's in the sample we employ an instrumental variable methodology

(IV) to address the decision to move. We use as an instrument for (say)  $t-1$  house price the  $t-1$  house price of the initial REB area in which the woman began the panel. Intuitively, this instrument effectively assigns women who move to realize an increase in wealth originating from rising house price to the treatment – rather than the control – group. The validity of this instrument requires the assumption that the past house price at origin affects the fertility of movers only through the changes in wealth it might bring about (i.e. through changes in prices) and not via other channels. Such changes in wealth are removed from the instrumented price, so that the IV estimates using these predicted house prices are unbiased by such wealth changes.<sup>11</sup>

### 3.1 Descriptive Statistics

We begin in Figure 2 by illustrating the behaviour over time of real (CPI deflated 2002 = 100) average housing prices for real estate boards in Canada, divided into regions, for the 18 years of combined panel data (1993–2010). We plot all REB prices by region, in order to show the variation in house prices that exists within and across regions, even if it makes for a cluttered figure. Note that the vertical scale differs between regions, reflecting strong variation in housing prices between regions in Canada. Prices are higher in British Columbia and Alberta in Western Canada, and in Ontario in central Canada. There is also strong variation within regions, usually between large urban, suburban, and rural areas. Prices are higher for urban centres: Vancouver, Victoria, Calgary, Saskatoon, the regions of Toronto, Montreal, and Halifax. Also of note within each region is the strong – but not universal – co-movement in prices between REB's, with price growth generally strongest in major urban centres.

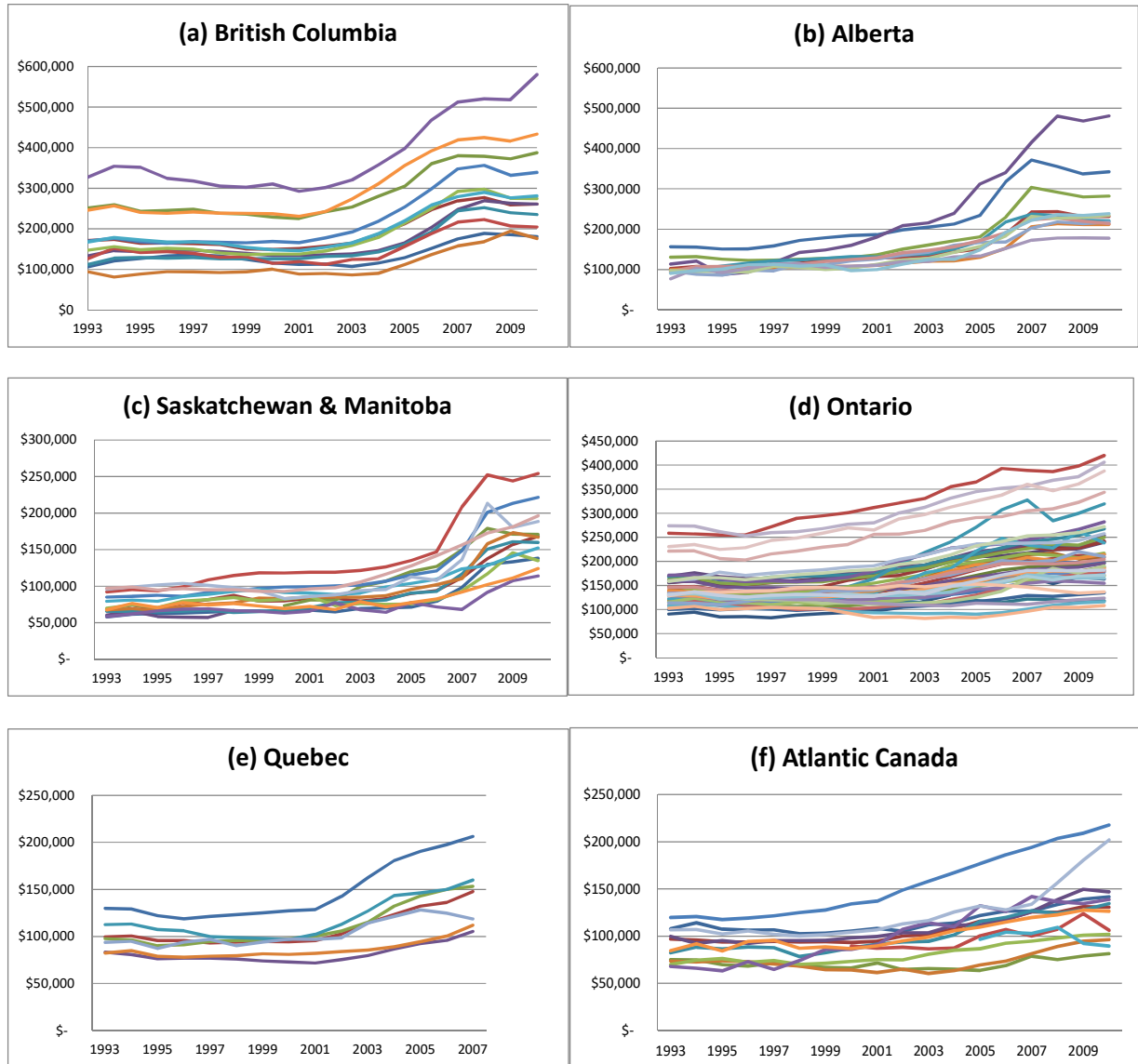
As previously mentioned, there has been a general decline in marginal fertility over the SLID sample years (see Appendix Table 1 along with Figure 1). The mean proportion of women reporting a birth dipped then recovered between 1995 and 2005 before falling again by 2010, broadly consistent with models predicting that fertility moves with the business cycle (Adsera, 2011). Table 1 shows marginal fertility separated by tenure and mover status, but pooled over years. Overall, 6.1% of homeowner women in our subsample gave birth in the year, whether with or without movers, while 7.4 to 7.7% of renting women did so. Finally, in Figure 3 we illustrate how the total number of children born per woman varies according to lagged real housing prices for homeowner women who reported a birth over the previous year. We focus here on women who remained within their REB over their six year panels. There is a positive correlation between house price and total children born, but only up to three children, with a negative correlation thereafter. This correlation, however, does not take into account individual characteristics, such as the influence of the life cycle on saving and investment, or characteristics of the house such as location or neighbourhood amenities. Fortunately, the SLID collects a broad range of information that we can use to control for the main economic determinants of income, savings and fertility. Table 1 shows the sample statistics for the main variables used in our analysis, broken down by owners vs. renters, and excluding or including movers to other REB's who kept their owner/renter status. In general, renters are younger, have lower household income, are less likely to be working

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<sup>11</sup> The IV strategy is similar to that used by Currie and Rossin-Slater (2013) to address the effect of hurricane-related maternal stress on infant health for mothers who subsequently move from the affected area.

(part time or full time), to have a post-secondary degree or to be married/common law, and have had fewer children in total. Yet renters are slightly more likely to have had a child in the last year (7 to 8% vs. 6%).

*Figure 2: Real average housing price by Real Estate Board,<sup>a</sup> 1993–2010, by region*



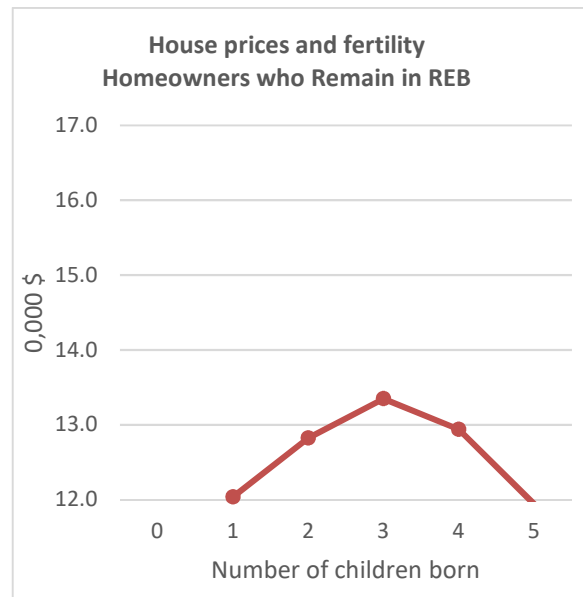
<sup>a</sup>Each line represents an individual REB average residential house price within the region.

Source: Authors calculations using CREA-MLS house price data.

*Table 1: Summary statistics (by home ownership and moving status)*

	Owners			Renters		
	N	Mean	SD	N	Mean	SD
<b>A. Home Owners and Renters Remaining within Real Estate Board Area</b> (keeping owner/renter status)						
Child born last year	36,046	0.061	0.240	3,385	0.074	0.262
House price (Real \$0,000)	36,046	17.85	8.60	3,385	18.42	9.10
House price change (R \$0,000)	27,840	0.601	1.590	2,580	0.605	2.139
Age	36,046	36.80	4.73	3,385	33.96	5.71
Full Time job	36,046	0.553	0.497	3,385	0.420	0.494
Part Time job	36,046	0.231	0.421	3,385	0.165	0.371
Family income (Real \$000)	36,046	70.28	42.27	3,385	40.00	18.91
# Children born	36,046	1.926	1.036	3,385	1.696	1.267
# Bedrooms in home	24,436	3.391	0.819	2,259	2.543	0.914
Provincial UR	36,046	7.70	2.48	3,385	8.05	2.39
Married /CL	36,046	.903	0.300	3,385	0.750	0.432
Post-Secondary education	36,046	0.220	0.414	3,385	0.119	0.324
<b>B. Home Owners and Renters including movers outside REB</b> (while keeping owner/renter status)						
Child born last year	36,562	0.061	0.239	3,534	0.077	0.267
House price (Real \$0,000)	36,562	17.87	8.59	3,534	18.38	9.06
House price change (R \$0,000)	28,518	0.596	1.638	2,720	0.612	2.158
Age	36,562	36.71	4.74	3,534	33.88	5.79
Full Time job	36,562	0.555	0.497	3,534	0.434	0.496
Part Time job	36,562	0.230	0.421	3,534	0.175	0.380
Family income (Real \$000)	36,562	70.31	42.12	3,534	40.13	19.00
# Children born	36,562	1.942	1.045	3,534	1.657	1.245
# Bedrooms in home	24,807	3.389	0.826	2,351	2.581	0.890
Provincial UR	36,562	7.70	2.46	3,534	8.04	2.377
Married /CL	36,562	0.884	0.320	3,534	0.741	0.438
Post-Secondary education	36,562	0.218	0.413	3,534	0.134	0.341
Sample: women aged 18 to 45, who are married or live common law in the first year of the panel. SLID (1993–2010)						

*Figure 3: Average housing price and total number of children born for women who gave birth in the previous year*



Source: see Appendix Table 2.

## 4 Regression Results

We look first in Table 2 at homeowners who remain within their real estate board boundaries. Pooled cross section analysis in model (1) shows that a \$10,000 increase in lagged house price is significantly associated with a 2.0% increase in the odds of having a birth. To put this in perspective, this would raise the sample mean odds of having a birth from  $(.061/.939 =) .0650$  to  $.0662$ , or raise the likelihood of birth from 6.10% to 6.21%. Covariates (significantly) positively associated with marginal fertility for non-moving homeowners are “continuing to be married/common law”, and “post-secondary education”. Covariates (significantly) negatively associated with marginal fertility (and thus with estimated odds ratios less than one) are “age”, “previous number of children born”, “household income”, and our proxy for macroeconomic conditions – “provincial unemployment rate.”

In some specifications for homeowners we also proxy for the physical quantity of housing owned using information on number of bedrooms along with its interaction with house price. Liu and Clark (2016) identify the potential importance of physical quantity of housing owned for the effect of housing price on homeowner fertility. Note however that the number of observations drops because the number of bedrooms variable is not available for all years of the SLID sample, and even when it is included not all individuals report it. Nevertheless, to test this prediction, we add number of bedrooms and its interaction with lagged house price in column (2). We do not find the interaction to be significant. However, we also suspect that house quantity proxies, such as number of bedrooms, suffer from endogeneity problems as individuals may select the size of a

*Table 2: Effect of house prices on marginal fertility of owners (proportional effect on odds ratios, p-values in parentheses)*

	Logit - Pooled Cross Section			Logit - Panel – Fixed Effects		
	Non Movers		With Movers	Non Movers		With Movers
	(1)	(2)	(3)	(4)	(5)	(6)
1-yr lag house price (\$0000)	1.020*** (0.000)	1.022 (0.158)	1.019*** (0.000)	1.118** (0.023)	1.101 (0.496)	1.060 (0.234)
# of bedrooms		1.221** (0.025)			1.266 (0.711)	
# bedrms x lag house price		0.998 (0.740)			1.004 (0.893)	
1-yr lag FT job	0.897 (0.220)	0.827* (0.093)	0.903 (0.268)	0.636 (0.136)	0.416** (0.017)	0.650 (0.154)
1-yr lag PT job	1.038 (0.687)	1.059 (0.637)	1.046 (0.637)	0.812 (0.400)	0.566* (0.092)	0.844 (0.480)
1-yr lag real family income (x \$000)	0.997*** (0.005)	0.995*** (0.000)	0.998*** (0.009)	0.983*** (0.000)	0.983*** (0.001)	0.984*** (0.000)
Married /CL <sup>a</sup>	1.404** (0.019)	1.486** (0.018)	1.394** (0.023)	--	--	--
Previous # of children	0.572** (0.000)	0.550*** (0.000)	0.573*** (0.021)	0.00005*** (0.000)	0.00001*** (0.000)	0.00008*** (0.000)
Post-Sec. education	1.801*** (0.000)	1.771*** (0.000)	1.809*** (0.000)	--	--	--
Age	0.832*** (0.000)	0.835*** (0.000)	0.833*** (0.000)	--	--	--
Provincial UR	0.975* (0.079)	0.980* (0.089)	0.976 (0.106)	0.892 (0.284)	0.830 (0.268)	0.903 (0.341)
Year effects	YES	YES	YES	YES	YES	YES
Observations	35,420	23,900	35,970			
Number of Individuals				7,420	4,620	7,980
Pseudo R <sup>2</sup> or Chi <sup>2</sup>	0.177	0.177	0.176	540.8	243.1	539.3

Dependent variable is an indicator for “child born last year”.

REB clustered robust SE in models (1)–(3) and replicated bootstrapped standard errors clustered to REB in models (4)–(6)

<sup>a</sup>CL stands for common law

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.



house based on fertility preferences.<sup>12</sup> Hence, we consider full model (1) without bedrooms our preferred specification.

Columns (4) and (5) perform a similar analysis while exploiting the panel nature of our data. Fixed effects allows us to remove any bias due to the existence of unobserved individual traits that do not change over the sample period, most importantly fertility preferences.<sup>13</sup> The main specification for the panel estimates, shown in column (3), finds that a \$10,000 increase in lagged house price is associated with an 11.8% increase in the odds of having a birth (raising the sample mean odds from .0650 to .0726, or the likelihood of birth from 6.10% to 6.77%). Thus the estimated size of effect is far greater under fixed effects than pooled cross section, with the coefficient significant at the 5% level. We shall discuss the robustness of this surprisingly large estimated effect subsequently. Column (5) shows the results when we consider the interaction of bedrooms and house prices. Once more, the interaction is not statistically significant.

Having found positive pro-natal effects of house prices for owners (at least those who stay within their REB), we move in Table 3 to analogous models for renters. Unlike for home owners, theory unambiguously predicts that if higher housing prices translate into higher rental costs, they will be negatively associated with the fertility of renters. The first item of note is that we have a much smaller number of observations for renters than for homeowners. This is in part because we are restricting our sample to renters who do not change REBs, nor their renter/owner status over a six-year period (columns (1) and (3)). Also, given that quantity of housing owned is not relevant for renters we exclude it from all renter specifications.

In contrast to theory, we find no significant effect of rising REB house prices on the fertility of renters. The point estimate of the odds ratios for house price is not significant in any of the models in Table 3. Overall, we have less insight on how to interpret these slightly surprising results, as we have no data on how closely movements in rental costs track changes in housing prices. However we note that this lack of significant negative correlation between housing price and fertility for renters is similar to what was found by Lovenheim and Mumford (2013) for the United States and Atalay et al. (2017) for Australia.

#### **4.1 Including Movers**

We move next to results when we retain in our sample women who moved to other REB's in Canada, but retained their renter/owner status. In principle, retaining movers has the virtue of including in the analysis those who care sufficiently about desired family size to change location, perhaps to access more space in less expensive areas. Beginning with homeowners, in Table 2 we include movers in model (3) (pooled cross section) and (6) (fixed effects). For cross section we find similar positive though smaller effects as when movers were excluded, with a \$10,000 increase in lagged house price now raising the odds of having a child by 1.9% (rather than 2.0%).

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<sup>12</sup> In particular, when we regress number of bedrooms on lagged house price, we find that lagged house price has large significant effects in explaining variation in number of bedrooms. This holds across sparse or full specifications, and is consistent with housing in more sought-after areas containing fewer bedrooms.

<sup>13</sup> The fertility literature generally considers that fertility preferences are part of the social norm and are formed at a relatively young age. See for instance, Adsera and Ferrer (2014) and Adsera et al. (2012).

In the FE panel regression in model (6) however, the increase in the odds ratio with movers is now only 6.0% and not significant (p-value .234), rather than 11.8% and significant. This suggests that for women who move, increases in house prices are associated with a reduction in fertility. We discuss some potential implications of this finding in Section 5. Results for renters when movers are included – Table 3, columns (2) and (4) – continue to be non-significant.

While including movers retains those who might differ in their views of desired family size, it also heightens endogeneity concerns, since by choosing location, individuals may be playing a greater role in influencing the change in wealth associated with the housing price they experience. In order to account for the endogeneity of house prices (if house prices are correlated with unobservable fertility preferences), we consider an instrumental variable (IV) approach through a two-step least squares (2SLS) regression framework. We instrument the current REB house price with the current housing price for the REB in which the woman is first observed in the SLID panel. Such prices will be identical to experienced prices for those who remain in their REB, but differ for those who move. Appendix Table 5 shows that house prices in the woman's initial REB

*Table 3: Effect of house prices on marginal fertility of renters (proportional effect on Odds Ratios, p-values in parentheses)*

	<b>Logit - Pooled Cross Section</b>		<b>Logit - Panel – Fixed Effects</b>	
	<b>Non Movers</b>	<b>With Movers</b>	<b>Non Movers</b>	<b>With Movers</b>
	(1)	(2)	(3)	(4)
1-yr lag house price (\$0000)	1.002 (0.818)	1.003 (0.742)	0.998 (0.984)	1.009 (0.912)
1-yr lag FT job	0.703 (0.209)	0.730 (0.255)	0.992 (0.231)	0.480 (0.185)
1-yr lag PT job	1.100 (0.671)	1.066 (0.772)	1.269 (0.458)	0.768 (0.613)
1-yr lag real family income (x \$000)	1.002 (0.626)	1.001 (0.748)	1.005 (0.713)	1.003 (0.874)
Married /common law	2.058*** (0.005)	1.904*** (0.006)	--	--
Previous # of children	0.808** (0.024)	0.801** (0.015)	--	0.001*** (0.000)
Post-Sec. education	1.273*** (0.374)	1.235 (0.411)	--	--
Age	0.880*** (0.000)	0.884*** (0.000)	--	--
Provincial Unemployment	0.957 (0.346)	0.950 (0.254)	1.086 (0.669)	0.921 (0.790)
Year effects	YES	YES	YES	YES
Observations	3,300	3,440		
Number of Individuals			760	950
Pseudo R <sup>2</sup> or Chi <sup>2</sup> (19)	0.104	0.096	39.58	62.68

Dependent variable is an indicator for "child born last year".

REB clustered robust SE in models (1)-(2) and replicated bootstrapped standard errors clustered to REB in models (3)-(4)

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.

are strongly correlated with those in her actual REB (its coefficient in the first step regressions is always large and significant at 1%). It is also hard to imagine how initial REB's ongoing housing prices should affect a woman's marginal fertility other than its effect through her current REB's housing price. This strategy alleviates concerns about endogenous moving as a result of house price changes that are related to fertility, since women who move are correctly assigned to the prices of areas they were originally in. Therefore, these predicted prices reflect the exogenous change in wealth for movers, rather than the endogenous response through the move. Our IV strategy is linear as there are difficulties implementing IV procedures for nonlinear models. To bridge the results between the non-instrumented logit regressions and our linear IV estimates, we also provide the results for the linear probability model in rows (2) and (5).

Table 4 provides our results as we instrument for house price, focussing on our key coefficients regarding the effect of lagged house price on marginal fertility. Rows (1) through (3) show the pooled cross section specifications as we move from non-instrumented logit, to analogous linear probability model, to 2SLS results. Rows (4) to (6) show analogous results for fixed effects specifications. The first column of coefficients refers to homeowners and the second to renters. Starting with pooled cross section for homeowners, we find that the bridging linear model in row (2) finds a similar magnitude of effect as did the logit model in row (1); a \$10,000 increase in lagged house price raises the probability of having a child by .1 percentage point (from 6.1% to 6.2% at sample mean). Similarly, with the instrument in row (3), a \$10,000 increase in predicted lagged house price again raises the probability of birth by .1 percentage point, significant at the 1% level. Thus, our estimated effects of higher house prices on the fertility of homeowners when we use pooled cross section is stable to the inclusion of movers, with or without instruments.

Our results for homeowners under fixed effects remain weaker with movers when we instrument for house price. We report the point estimate from Table 2, without instrumenting, in row (4) – the non-significant effect on the odds ratio with movers is 6.0%, which raises the sample mean probability of birth from 6.1% to 6.4%. In the analogous linear model in row (5), the similar point estimate of .2 percentage points (from 6.1% to 6.3% at sample mean) is still not significant, with a p-value of .155. When house price is instrumented in row (6), the point estimate rises to .5 percentage points from .2 (or from 6.1% to 6.6% at sample mean), which is larger in magnitude but still statistically insignificant.

For renters, we find that higher lagged house prices continue to have no significant effect on fertility when we include movers and instrument for house price. Whether we use pooled cross section in rows (1) to (3), or fixed effects in rows (4) to (6), and whether we use logit, linear, or 2SLS, we find no significant association between lagged REB housing price, and the marginal fertility of renters.

In summary, when we include home owners who move to other REB's but retain their owner status, we continue to find that higher lagged house price is positively associated with marginal fertility, though these effects are not significant in fixed effect models.

*Table 4: Effect of house prices on marginal fertility of owners and renters (including movers outside REB, p-values in parentheses)*

	<b>Homeowners</b>		<b>Renters</b>	
	<b>1-yr lag house price</b>	<b>N</b>	<b>1-yr lag house price</b>	<b>N</b>
<b>Pooled Cross Section (<sup>a,b,c</sup>)</b>				
(1) Logit – effect on Odds Ratio	1.019*** (0.000)	35,970	1.003 (0.742)	3,440
(2) LPM Coefficient	0.001*** (0.000)	35,970	-0.0001 (0.862)	3,440
(3) Linear 2SLS coefficient	0.001*** (0.005)	29,340	-0.001 (0.265)	2,760
<b>Panel (<sup>a, c, d</sup>)</b>				
(4) Logit FE - effect Odds Ratio	1.060 (0.234)	7,980	1.009 (0.912)	950
(5) Linear FE Coefficient	0.002 (0.155)	35,980	0.001 (0.764)	3,450
(6) Linear 2SLS FE Coefficient	0.005 (0.646)	29,270	0.018 (0.855)	2,790

Dependent variable is an indicator for “child born last year”. Year effects always included.

<sup>a</sup>REB clustered robust SE in models (1) – (3). Replicated bootstrapped standard errors clustered to REB in Models (4)-(6).

<sup>b</sup>Models (1) and (2) include control for Provincial Unemployment, 1year Lagged Real Family Income, Number of Previous Children and indicators for 1year-Lagged Full Time and 1year-Lagged Part Time job, Married/Common law, Age and Post-secondary Education, unless otherwise specified.

<sup>c</sup>The first stage of 2SLS models (3) and (6) regress lagged house price on lagged house price at woman’s initial REB and on the controls used in the second stage estimation. See Appendix Table 5.

<sup>d</sup>Models (4)–(6) include controls for Provincial Unemployment, 1year Lagged Real Family Income, Number of Previous Children and indicators for 1year-Lagged Full Time Work Status, 1year-Lagged Part Time Work Status, Married/Common Law indicator and individual fixed effects.

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.

## 4.2 Completed Fertility, and Heterogeneity of Effect by Income and Existing Family Size

So far we have mixed evidence that an increase in lagged average REB house price may increase the odds of home-owning women giving birth, more so if we restrict the sample to those who remain within an REB for the six years they are followed up. Such positive effects could, however, simply reflect a timing issue. A price increase might cause home-owning families to wish to have more children in total, or merely to have the same number sooner. Our marginal fertility measure does not enable us to address this ambiguity conclusively, but we try to get suggestive evidence by focussing on the effects of house prices on women nearing the end of their potential child-bearing years. Because effects seem most evident for home owners who remain in their initial REB over 6 years, we focus on this sample.

We first replace our continuous age variable with a dummy for whether the woman is 35 or older, together with an interaction of this dummy with lagged house price. We conduct joint tests

of the lagged house price plus interaction term to determine whether an increase in house price affects fertility for women aged 35 or older. We second consider a more flexible specification for the effect of age, using instead multiple age bracket dummies, each interacted with lagged REB house price. Under this approach, our omitted age bracket is women aged 23–27, the peak fertility bracket. Our results are provided in the upper section of Table 5, for pooled cross section in columns (1) and (2), and fixed effects in columns (3) and (4). For ease of interpretation, we present the effects as percentage point increases in the likelihood of giving birth from the overall sample mean of .0610. These show the effect of price increases on women by age, relative to women of the same age who do not experience a price increase.

Under pooled cross section in column (1), a \$10,000 increase in lagged house price raises the probability of women aged 35 or more giving birth by .14 percentage points (or from .0610 to .0624 at sample mean). In the equivalent fixed effects model (3), it raises the probability of older women giving birth by .63 percentage points (or from .0610 to .0673). We thus find that rising house prices have a pro-natal effect even for older home-owning women.

We glean more nuanced results when we move to multiple age bracket dummies and interactions. Beginning with pooled cross section in model (2), relative to other women their own age, a \$10,000 increase in price increases the probability of women aged 33–37 giving birth by .16 percentage points, which is significant in a joint test at the 1% level. It increases the probability by .87 under fixed effects in column (4), again significant at the 1% level. Do these pro-natal effects persist even for women aged 38 and above? The answer is yes, but at a reduced magnitude. A \$10,000 increase raises the probability of women aged 38 plus giving birth by .10 percentage points under pooled cross section, or by .60 percentage points under fixed effects. Both estimates are significant at the 5% level.

Putting these results together, higher house prices are associated with higher marginal fertility even of older home-owning women, though with the effect diminishing with age. To put this in context, under fixed effects a \$10,000 increase raises the probability of birth for women aged 28 to 32 by 1.25 percentage points. While our fixed effects estimations for home-owning women who remain in their REB's may be implausibly high, positive effects even for older women suggest that part of the positive effect we find may be raising the total fertility of homeowners, rather than just bringing forward the timing of their births. At the same time, the fact that the magnitude of price effects drops for older cohorts suggests that a part of the pro-natal effect of house price increases might simply be on birth timing.

While our prime concern in Table 5 was examining whether fertility effects of house prices varied by age, we can use an analogous approach to ask if they vary by the number of children a woman already has, or by household income. The middle and lower panels of the Table 5 indicate that fertility effects do not vary between those (REB-stable home-owning) women who have no previous children and those who have one-to-two, or those who have three or more. On the other hand, we find evidence that fertility effects of house prices do vary by family income. In particular, they are significantly positive for women whose family incomes are in the second to top quartile, and for women in the third to top quartile in cross section specifications. There are no fertility effects for women in the bottom or top family income quartiles.

*Table 5:* Predicted effect of \$10,000 rise in house price by age, previous children, household income, and increasing time lag<sup>a</sup> (excluding movers outside REB, p-values in parentheses)

	Logit Pooled CS		Logit Panel FE	
	Single age interaction	Multiple age interaction	Single age interaction	Multiple age interaction
	(1)	(2)	(3)	(4)
Age 35 up	0.0014*** (0.000)		0.0063** (0.016)	
Age 18-22		0.0067** (0.012)		0.0084 (0.808)
Age 28-32		0.0003 (0.433)		0.0125*** (0.006)
Age 33-37		0.0016*** (0.000)		0.0087*** (0.005)
Age 38 up		0.0010** (0.013)		0.0060** (0.025)
<b>Pseudo R<sup>2</sup> or Chi<sup>2</sup></b>	0.162	0.183	585.2	578.6
<b>Observations</b>	35,420	35,420		
<b>Number of Individuals</b>			7,420	7,420
Previous 1-2 children		0.0004 (0.136)		0.0018 (0.363)
Previous 3 + children		0.0000 (0.995)		0.0005 (0.843)
<b>Pseudo R<sup>2</sup> or Chi<sup>2</sup></b>		0.060		523.42
<b>Observations</b>		35,420		
<b>Number of Individuals</b>				7,740
Income 25 <sup>th</sup> -50 <sup>th</sup> percentile		0.0010*** (0.002)		0.0027 (0.408)
Income 50 <sup>st</sup> -75 <sup>th</sup> percentile		0.0006* (0.075)		0.0060* (0.075)
Income 75 <sup>th</sup> -100 <sup>th</sup> percentile		0.0001 (0.694)		0.0055 (0.121)
<b>Pseudo R<sup>2</sup> or Chi<sup>2</sup></b>		0.096		622.10
<b>Observations</b>		35,420		
<b>Number of Individuals</b>				7,750
<b>Year effects</b>	YES	YES	YES	YES
<b>Controls as in Tab 2:(2) or (5)</b>	As (2)	As (2)	As (5)	As (5)
<b>(5):</b>				

Dependent variable is an indicator for “child born last year”.

Standard errors clustered to REB in models (1) and (2) and replicated bootstrapped standard errors clustered to REB in models (3) and (4).

<sup>a</sup> Effect of a \$10,000 increase in the price of the house on the marginal fertility of women of a given age relative to the overall sample mean of .0610.

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.

### **4.3 Robustness Checks of Longer Lags, Changes in House Price, and Geographic Restrictions**

One possible objection to our general approach is that a one-year lag between measures of average annual REB house price and a woman giving birth may not allow sufficient time for families' fertility to respond to housing price changes. Increasing the house price time lag beyond one year allows more time for fertility responses, but at the cost of losing additional year(s) of observation from each six year SLID panel.<sup>14</sup> For panel regressions in particular, this compression further reduces potential within-family variation in house prices and fertility. We have thus repeated the analysis of our preferred specification, but using REB house prices lagged by two years. The results are reported in columns (2) and (4) of Appendix Table 3 for cross section and panel data respectively. For pooled cross section, increasing the lag of price slightly increases its positive effect on marginal fertility (a 2.2% increase in odds of giving birth rather than 2.0%). For fixed effects, increasing the lag results in a smaller, more plausible point estimate (6.1% increase in odds rather than 11.8%) but one that loses statistical significance.

A second robustness check concerns whether our results are sensitive to regressing lagged house price *changes* rather than lagged house price *levels* on marginal utility. While person fixed effects specifications using within-REB variation in house prices may already be viewed as testing for the effects of changes in house price on marginal fertility, the same cannot be said for pooled cross section. Lovenheim and Mumford (2013) argue that, even if price changes have greater noise to signal ratios than levels, they more clearly represent wealth effects for homeowners with varying degrees of home equity. We thus repeat our main specification in Tables 2 and 3 using lagged (from  $t-2$  to  $t-1$ ) house price changes rather than lagged ( $t-1$ ) house price levels. The results are provided in Appendix Table 3. We note first from Table 1 descriptive statistics that the mean of house price change (.60) for home owners is much smaller than the mean of house price level (17.85), yet its standard deviation is proportionately larger (1.59 vs. 8.60). In addition, the inclusion of  $t-2$  in the construction of the lagged change again results in a loss of an additional year (and thus within-person variation) from each six year panel, and reduced sample size compared to level specifications. Hence, while the point estimate of the odds ratio effects shown in Appendix Table 3 remain greater than 1 in model (1) (1.013), the result is no longer significant. The fixed effects specification (2) has a similar drop in magnitude (from 1.118 to 1.033) and loss in significance. We also find instability for renters when moving from house price levels to changes. The house price estimates for renters now show a marginally significant reduced effect on fertility in the cross-section models (3), but no significant effect in the panel estimate (4).

A final set of robustness checks concerns the sensitivity of our results to the exclusion of the major urban centres that have seen the greatest price growth during our study period, Vancouver, Toronto, and Calgary. We focus again on the case where fertility effects were most evident, home owners who remained in their REB's during their six year panels (Table 2). Not reported here, we replicated Table 2 without these REB's, and find effects similar to before. In particular, pooled cross section estimates rise from a 2.0% increase in odds to 2.8% that is still significant at the 1%

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<sup>14</sup> While we have the REB average housing price in the years preceding the start of each new SLID panel, we cannot know if a woman and her family lived in that REB in those pre-survey years.

level, while analogous fixed effects estimates rise from 11.8% to 13.7%, now only significant at the 10% level.<sup>15</sup>

## **5 Discussion and Conclusions**

In this paper, we have joined a limited number of recent investigations into the effects of housing price on the marginal fertility of homeowners separate from renters. We investigate house price effects on fertility specifically in Canada, where the housing market has historically been less volatile, mortgages backed more by bank deposits than securitisation, and lending/borrowing practice and regulation more conservative. We have used individual level data from the Canadian Survey of Labour Income and Dynamics (SLID) merged with more aggregated average housing price data at real estate board (REB) level from the Canadian Real Estate Association (CREA). Theoretically, higher housing prices that translate into higher rental costs should depress fertility among renters. Higher prices could raise fertility among homeowners if their elasticity of substitution between children and other goods is low and they already own sufficient housing, but depress their fertility otherwise.

Empirically, we find that the effect of housing price on marginal fertility does vary by homeowner status. For homeowners, we find that higher lagged REB average house price may likely raises the odds of a woman giving birth, though the effect is significant in some credible specifications, and not in others. In particular, for home-owning women who have not moved outside their REB during the 6 years of their panel, we find – using pooled cross section logit regressions with standard errors clustered to the REB level – that a \$10,000 real increase in lagged REB average housing price raises the odds of a woman giving birth by 2.0%. This is equivalent to raising the annual likelihood of giving birth by 1.1 percentage points, or from a sample mean of 6.10% to 6.21%. The pro-natal effect appears even larger under fixed effects, where with bootstrapped standard errors a \$10,000 increase raises the odds of giving birth by a whopping 11.1%. These pro-natal effects of rising house prices hold even for women aged 33-37 and 38 or higher, using either pooled cross section or fixed effects, suggesting that at least part of the effect of rising house prices on REB-stable homeowners is on their total fertility, and not just on the timing of their births. We find that these pro-natal effects concentrate in the 50 to 75<sup>th</sup> quartile of family income, and possibly in the 25<sup>th</sup> to 50<sup>th</sup>, rather than the top or bottom quartile. The results are not driven by cities experiencing the greatest price growth during our sample period (Vancouver, Toronto, and Calgary).

One of the limitations of our results is that we lose significance in many of the additional panel data specifications we consider. Specifically, when we include those homeowners who move outside their REB, the large effect found for movers in panel data models diminishes and loses significance. This suggests that families who move tend to have lower fertility than those who do not. This counter-natal effect of fertility for the movers is unlikely due to families moving

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<sup>15</sup> We alternatively try restricting our analysis to women living in the province of Alberta, where changes in house prices are tied mostly strongly to exogenous changes in the world price of oil. We find positive effects on the fertility of homeowners for the cross section and fixed effect models, but these results are not significant. However, the sample size is greatly reduced when analysis is restricted to this single province.



to cheaper areas in order to increase fertility, since our point estimates imply the same rough increase in the probability of having a child whether or not we instrument house prices to account for such effects. However, the number of movers is very small and the estimates are not precise enough to offer a definite conclusion one way or another.

In robustness checks for REB-stable homeowners, when we increase the lag in price levels to  $t-2$ , we find price increases remain pro-natal to a similar magnitude, although we lose significance in the panel estimates. Similarly, when we regress likelihood of birth on lagged *changes* in house price ( $t-2$  to  $t-1$ ) rather than lagged *levels* ( $t-1$ ) we continue to observe positive effects (1.3% increase in odds under pooled cross section and 3.3% under fixed effects), but these are no longer significant. We are unsure if this loss of significant effect of house prices on the fertility of REB-stable homeowners is due to behavioural causes (e.g. that homeowners contemplating family size pay more attention to rising price levels than to rising price changes), or statistical ones (e.g. that there is less relative variation in house price changes than house price levels). Nonetheless, it suggests a certain degree of caution is appropriate in concluding from our results that higher house prices definitely raise fertility among Canadian homeowners during the years of our study. Many reasonable specifications find they do, while others find a sizable effect that is not significant.

Higher average REB house prices, however, do not have a significant effect on renters' likelihood of giving birth. This result is contrary to theory, given that such price rises should bring only substitution effects, but has also been found by Lovenheim and Mumford (2013) for the United States, and by Atalay et al. (2017) for Australia.

By way of comparison to other studies, Lovenheim and Mumford (2013) find for American homeowners that a US \$10,000 increase in homeowners' own house price raises the .05 baseline probability of giving birth by .00085 percentage points, a 1.7% increase. Atalay et al. (2017) find that a AUS \$10,000 increase raises the .081 baseline probability by .00061 percentage points, a more modest .75% increase. Using our pooled cross section estimates, we find a Can \$10,000 increase in REB price raises the .061 baseline probability of giving birth for REB-stable homeowning women by .0011 percentage points, a 1.8% increase. This magnitude of effect is thus similar to Lovenheim and Mumford (2013), notwithstanding the more conservative nature of the Canadian housing market.

The near-impossibility of convincingly modelling all major life transition decisions such as marriage, separation, adoption, etc. drives much of the choice of outcomes and sampling selection in this paper, as well as in the literature. In particular, considering other family outcomes requires separate analysis (see for example Farnham et al. 2011). We have also not been able to consider here the fertility of single mothers, as very few births occur among single mothers in our SLID sample. Other authors who have focused on the effects of house prices on single mothers (Curtis and Waldfogel, 2009) have used specialised surveys that contain enough non-marital births for meaningful analysis. Another significant restriction in our sample involves tenure stability. Considering individuals who change tenure status requires more complex modelling than what was considered here. In particular, the decision to purchase a first home (and how house prices affect this decision) would require different analysis, such as a hazard rate model that better captures the timing of the decision.

Our findings provide some tentative conclusions for those regions of Canada that experienced strong increases in housing price over recent decades, such as Vancouver and Toronto. They do not suggest that areas experiencing rapid price growth will see significantly depressed fertility

among those families who rent there, even if rapidly rising prices force more young families into renting rather than home ownership. Among families who already own homes and remain within their REB boundaries, our findings suggest they may even have slightly more children than they otherwise would have, though with some concerns about the robustness of these pro-natal effects. Combining these two findings, if the number of school-aged children is falling in high price growth locations such as Vancouver or Toronto, it is not because extant homeowners and renters there are choosing to have fewer children. Such declines might instead be caused by other factors, such as migration into high growth urban centres by people with preferences for fewer children.

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## Appendix

*Appendix Table 1: Annual trends in marginal fertility*

	% children born last year	
	Mean	SD
1995	7.7	(0.072)
2000	6.5	(0.059)
2005	7.6	(0.071)
2010	3.4	(0.035)

Source: SLID 1993–2010, married or Common Law women aged 18–45 in the first year of the panel.

*Appendix Table 2: Average house price and fertility for homeowners (used for Figure 3)*

Total # children born	Average house price (0,000\$)					
	By child		If no child born last year		If child born last year	
	All	Non Movers	All	Non Movers	All	Non Movers
0	14.95	13.52	14.95	13.52		
1	14.24	12.92	14.28	12.98	13.88	12.04
2	14.43	13.37	14.42	13.40	14.63	12.83
3	14.14	13.25	14.09	13.25	14.84	13.35
4	14.11	12.66	14.04	12.64	15.01	12.94
5	13.63	12.19	13.39	12.21	15.86	11.94

Source: SLID 1993–2010, married or Common Law women aged 18–45 in the first year of the panel. Non Movers refers to homeowners who remained within their REB boundary for all six years of their panel.

*Appendix Table 3: Effect of house price change and 2-lagged house prices on marginal fertility.  
Proportional effect on odds ratios (excluding movers outside REB, p-values in parentheses)*

	Homeowners				Renters	
	Cross-section		Panel - FE		Cross-section	Panel - FE
	(1)	(2)	(3)	(4)	(3)	(4)
2 t Lagged house price t-2		1.022*** (0.000)		1.061 (0.445)		
1-yr lag Δhouse price (\$0000)	1.013 (0.671)		1.033 (0.744)		0.921* (0.088)	1.010 (0.960)
1-yr lag FT job	0.856 (0.169)	0.870 (0.221)	0.838 (0.653)	0.911 (0.814)	0.764 (0.425)	1.018 (0.970)
1-yr lag PT job	0.968 (0.768)	0.974 (0.812)	1.166 (0.746)	1.286 (0.599)	0.996 (0.987)	0.857 (0.773)
1-yr lag real family income (x \$000)	0.998* (0.095)	0.998** (0.028)	0.984** (0.015)	0.985*** (0.009)	1.001 (0.811)	1.012 (0.559)
Married /CL <sup>a</sup>	1.287* (0.082)	1.238 (0.148)	--	1.395 (0.803)	1.617* (0.005)	--
Previous # of children	0.589*** (0.000)	0.592*** (0.000)	0.00001 (0.164)	0.00001** (0.030)	0.858 (0.160)	--
Post-Sec. education	2.044*** (0.000)	2.028*** (0.000)	--	--	1.849** (0.029)	--
Age	0.822*** (0.000)	0.819*** (0.000)	--	--	0.867*** (0.000)	--
Provincial Unemployment	0.951*** (0.079)	0.960*** (0.000)	1.039 (0.871)	1.021 (0.919)	0.966 (0.504)	1.318 (0.525)
Year effects	YES	YES	YES	YES	YES	YES
Observations	27,840	27,860			2,580	
Individuals			4,410	4,405		440
Pseudo R <sup>2</sup> / Chi <sup>2</sup>	0.182	0.184	184.15	267.1	0.120	9.10

Dependent variable is an indicator for “child born last year”.

REB clustered robust SE in models (1) and (3) and replicated bootstrapped standard errors clustered to REB in models (2)–(4)

<sup>a</sup>CL stands for common law

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.

*Appendix Table 4: First stage regressions for 2SLS in Table 4 Rows (3) and (6) (includes movers outside REB, p-values in parentheses)*

	Pooled Cross Section		Panel Fixed Effects	
	Proportional effect on Odds Ratios		Proportional effect on Odds Ratios	
	Lag House Price	Lag House Price	Lag House Price	Lag House Price
	Homeowners	Renters	Homeowners	Renters
Lag original REB house price <sup>a</sup>	0.860*** (0.000)	0.801*** (0.000)	0.742*** (0.000)	0.702*** (0.000)
1-yr lag FT job	-0.186*** (0.005)	-0.135 (0.521)	-0.044 (0.358)	-0.196 (0.150)
1-yr lag PT job	-0.120 (0.124)	-0.785*** (0.003)	0.012 (0.833)	-0.478*** (0.004)
1-yr lag real family income (x \$000)	0.007*** (0.000)	0.013** (0.018)	0.002 (0.489)	0.021*** (0.010)
Married /common law	0.185*** (0.008)	0.663*** (0.000)	--	--
Previous # of children	-0.019 (0.482)	-0.221*** (0.007)	0.103*** (0.004)	-0.152* (0.067)
Post-Sec. education	-0.128* (0.066)	0.602 (0.176)	--	--
Age	0.064*** (0.000)	0.019 (0.239)	--	--
Provincial Unemployment	-0.075*** (0.000)	-0.205*** (0.000)	-0.116** (0.049)	-0.169 (0.111)
Year effects	YES	YES	YES	YES
Observations	29,340	2,760		
Number of Individuals			29,730	2,840
F statistic/ Chi Square	1453.84	213.88	1339.18	370.31

<sup>a</sup> in tens of thousands of dollars.

Dependent variable is an indicator for “child born last year”.

REB clustered robust SE in models (1)–(2) and replicated bootstrapped standard errors clustered to REB in models (3)–(4)

(\*), (\*\*) and (\*\*\*) indicates that the coefficient is significant at 10%, 5% and 1% level, respectively.

*Appendix: Full List of 92 CREA Boundaries for MLS and 123 CREA Boundaries for MLS II*

*MLS I: (1993–2010)*

**British Columbia:** Northern, Chilliwack, Fraser Valley, Kamloops, Kootenay, Northern Lights, Okanagan- Mainline, Powell River, South Okanagan, Vancouver, Vancouver Island, Victoria

**Alberta:** Calgary, Central Alberta, Edmonton, Fort McMurray, Grande Prairie, Lethbridge, Lloydminster(AB), Medicine Hat, North Eastern Alberta, South Central Alberta, Alberta West

**Saskatchewan:** Battlefords, SE Saskatchewan, Lloydminster (SK), Moose Jaw, Prince Albert, Regina, Saskatoon, Swift Current, Yorkton

**Manitoba:** Brandon, Portage La Prairie, Thompson, Winnipeg

**Ontario:** Bancroft, Barrie, Brantford, Cambridge, Chatham Kent, Northumberland Hills, Cornwall, Georgian Triangle, Grey Bruce Owen Sound, Guelph, Hamilton-Burlington, Huron Perth, Kawartha Lakes, Kingston, Kitchener-Waterloo, London and St Thomas, Muskoka& Haliburton, Niagara Falls–Fort Erie, North Bay, Oakville-Milton, Orillia, Ottawa, Parry Sound, Peterborough & the Kawarthas, Quinte, Sarnia-Lambton, Sault Ste. Marie, Simcoe, Southern Georgian Bay, St. Catharines, Sudbury, Thunder Bay, Tillsonburg, Timmins, Toronto+Brampton, Durham Region, Mississauga, Orangeville, York Region, Welland, Windsor-Essex, Woodstock-Ingersoll

**New Brunswick:** Fredericton, Moncton, Northern New Brunswick, Saint John

**Nova Scotia:** Annapolis Valley, Cape Breton, Halifax-Dartmouth, Highland, Northern Nova Scotia, South Shore, Yarmouth

**Prince Edward Island**

**Newfoundland & Labrador**

**Yellowknife**

**Yukon**

*MLS II: (2005–2010)*

**Victoria:** Victoria, Oak Bay, Esquimalt, View Royal, Saanich East, Saanich West, Sooke, Longford, Metchosin, Colwood, Highlands, North Saanich, Sidney, Central Saanich, Gulf Islands

**Vancouver:** Burnaby, Coquitlam, Delta, Maple Ridge, North Van, New Westminster, Port Moody/Belcarra, Port Coquitlam, Richmond, Van East, Van West, West Van/Howe Sound

**Fraser Valley:** North Delta, North Surrey, Surrey, Cloverdale, White Rock+District, Langley, Abbotsford, Mission, Chilliwack

**Calgary:** North West, North East, South West, South East

**Edmonton:** Northwest, North central, Northeast, Central, West, Southwest, Southeast, St. Albert, Sherwood Park



**Regina:** Area 1, Area 2, Area 3, Area 4, Area 5, Area 6

**Saskatoon:** Area 1, Area 2, Area 3, Area 4, Area 5, Area 6, Area 7, Area 8, Area 9, Area 20

**London/St.Thomas:** London East, London North, London South, Middlesex County, Elgin County, St. Thomas, Strathroy

**Hamilton:** Hamilton West, Hamilton Centre, Hamilton East, Hamilton Mountain, Burlington, Dundas, Ancaster, Stoney Creek, Grimsby

**Toronto:** Central, East, North, West

**Ottawa:** Area A&B, Area C&D, Area E&F, Area G&H, Area I, Area J, Area K, Area L, Area M, Area N, Area O

**Saint John:** Grand Bay Westfield, West & Musquash, North Saint John, East Saint John, Rothesay & Quispamsis, Hampton and Sussex, Kingston Peninsula, Other Areas, City Centre and South, Charlotte County

**Halifax:** Areas 1/2/3/4, Areas 5/6, Areas 7/8/9/40, Areas 10/11, Areas 12/13, Areas 14/30, Areas 15/16/17, Areas 20/21, Area 25/26, Area 31/35

**St. John's:** Conception Bay North, Conception Bay South, East Extern, Mount Pearl, St. John's, Southern Shore, All Other Areas

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The Editor