Home Prices, Fertility, and Early-Life Health Outcomes

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Abstract

We estimate the effect of housing price changes on fertility and early-life infant health in Denmark. Using rich population registry data among women aged 20-44 who own a home, we find that for each 100,000 DKK increase in home prices (equivalent to $15,000), the likelihood of giving birth increases by 0.27 percentage points or 2.32%. These estimates are similar to findings from the US per dollar of home price change, which is surprising given the strong pro-natalist policies and generous government programs in Denmark. We also present the first estimates of the effect of home prices on infant health. Our findings indicate that housing price increases lead to better infant health at birth in terms of low birth weight and prematurity, however some of these effects reflect changes in the composition of births. There is no evidence of an effect on health during the first five years of life. These findings are consistent with a lack of credit constraints among homeowner families and with both children and child health being normal goods that are similarly-valued in the US and Denmark.

KEYWORDS: Housing wealth, Fertility, Child health, Birth outcomes

1 Introduction

The past several decades have witnessed an historical amount of volatility in housing markets around the globe, driven by the run-up to and aftermath of the Great Recession. In most developed countries, housing wealth is a large component of individual households' wealth portfolio, and thus volatility in housing prices leads to substantial variation in household resources. A growing body of evidence examines household responses to home price variation, showing that it affects educational investment (Lovenheim 2011; Lovenheim and Reynolds 2013; Charles, Hurst and Notowidigdo 2018; Hotz et al. 2018), adult health (Fichera and Gathergood 2016), retirement behavior (Zhao and Burge 2017), and consumer debt (Brown, Stein and Zafar 2015). These studies all demonstrate the central relevance of housing wealth in driving a range of household behaviors.

Fertility decisions are among the most important made by a household. Children affect every dimension of household behavior and outcomes, and decisions about whether and when to have children have long-run implications for the individuals making these decisions as well as for society more broadly. Several papers have found that housing market variation strongly affects fertility decisions (Lovenheim and Mumford 2013; Dettling and Kearney 2014), both in terms of whether a women gives birth and the age at which a woman gives birth. These analyses focus exclusively on the US, which is important given the large housing boom and bust that occurred in the late 1990s to mid-2000s. However, the US context is also limiting because of the relatively small social safety net programs and the relative lack of pro-natalist policies like paid maternity leave that prevail in many other developed nations. It therefore is unclear to what extent fertility decisions in the US are particularly sensitive to housing wealth variation and whether the US estimates are representative of what one might find in a country with less inequality and a more expansive public sector. There also is no existing evidence on how housing market variation affects health outcomes at birth and during early childhood. Changes in such health outcomes have important implications for the social cost of housing market volatility and provide insight into some of the mechanisms underlying the relationship between home prices and fertility behavior.

In this paper, we extend the literature on household responses to housing wealth variation by studying the effect of home prices on fertility and infant health in Denmark. Denmark is an interesting case study because it has one of the most generous social safety nets in the world, combined with 52 weeks of parental leave, generous cash payments to families with children, heavily subsidized child care, and free and universal health care. It hence is interesting to understand whether the findings from the US extend to countries with larger pro-natalist policies or, rather, whether the effects in the US are driven by the need to self-finance the associated costs of childbearing. We discuss a theoretical framework that allows us to interpret similarities and differences between housing wealth effects across settings. We argue that in the absence of credit constraints, home price changes will affect fertility and health only through an income effect and hence reflect household preferences. In the presence of credit constraints, the higher net price of children in the US as well as any differences in the liquidity of housing lead to a prediction that the effect should be larger in the US than in Denmark.

Denmark also is an ideal setting in which to examine the effect of housing market variation on fertility because the existence of registry data for all Danish residents. These data allow us to probe the validity of the approach used by prior research with richer data and permit a thorough analysis of how short-run home price changes impact health outcomes of babies and young children. We use Danish registry data from 1992 to 2006 that contains detailed information on home purchases, home values,[1] birth outcomes, demographic background, and health outcomes for all residents in Denmark. Our focus is on women who were not home owners at the beginning of 1992-1993 and who became a homeowner between 1993 and 2004.[2] For each homeowner, we calculate the one-year change in home value and estimate how this one-year change affects the likelihood of giving birth in the subsequent year as well as health outcomes at birth and in the first five years of life.

Because of the rich data to which we have access, we include a number of household demographic and income controls as well as municipality and year fixed effects. This is essentially the model used by Lovenheim and Mumford (2013), however with a somewhat more expansive control set. In addition, we examine the sensitivity of the estimates to controlling for age-of-home-purchase by year-of-home-purchase fixed effects and municipality-by-year fixed effects.

[1] Throughout this paper, we use the terms "home values," "home prices," and "housing wealth" interchangeably.

[2] We do not require that a woman has to own the home herself. We refer throughout the paper to women who own a home, but we define this as the woman, her partner, or both being a home owner.

These are potentially important controls that prior researchers could not include because of data constraints and small sample sizes. The former set of fixed effects account for the fact that women who are in a given home longer mechanically have more exposure to home price changes and have more time to have a child. The latter set of fixed effects control for unobserved municipality-specific shocks that are correlated both with home price changes and fertility (e.g., economic conditions).

In addition to fertility, we examine whether short-run home price fluctuations affect early-life health outcomes. This is the first analysis to examine such effects. Prior work has shown that housing wealth affects adult health (Fichera and Gathergood 2016), but no research has estimated impacts on infant health due in large part to data constraints. It is important to identify the effect of housing wealth on infant health not only because these outcomes are of interest in their own right, but also because it provides insight into the social costs of the housing wealth-fertility link. We further argue that these estimates inform the mechanisms driving the housing wealth-fertility relationship.

Our findings suggest that there is a positive effect of home price increases on fertility: a 100,000 DK increase in home prices in the prior year increases the likelihood of giving birth by 0.27 percentage points, or 2.32% relative to the mean.[3] This effect size is almost identical to that found in Lovenheim and Mumford (2013), whose estimates imply that a $15,000 increase in home prices would increase fertility by about 2.5%. Dettling and Kearney (2014) find an effect size of 7.5% per $15,000 of home price increase,[4] so our estimates are qualitatively similar to prior results and are almost identical to the most closely related US-based studies. In levels, we find that effects are largest among 25-39 year olds and among first-time mothers, but effect sizes relative to the mean are relatively consistent across ages and birth parity groups. We also show that women in the middle of the income distribution are most responsive to home price changes in their fertility decisions, but there is little evidence of heterogeneity by completed eduction.

The similarity of our estimates to those in the US are surprising. We present evidence using US data from the Consumer Expenditure Survey (CEX) that indicates US homeowners are

[3]There are about 0.15 DK per dollar, so 100,000 DK is about $15,000.
[4]When we use similary-aggregated data to Dettling and Kearney, we find an effect size of 6.8%.

not credit constrained surrounding the birth of a child. Homeowners with children under two spend more overall than observationally-similar childless households, which is due to higher expenditures on housing and health. Other expenditure categories are extremely similar across groups. That US homeowners do not appear to be credit constrained suggests that Danish homeowners are not constrained either given the lower net cost of children in Denmark. In the absence of credit constraints, our results most likely reflect household preferences through an income effect. A core conclusion is that US and Danish household have similar fertility responses to home price changes because they have similar preferences. In a setting with unconstrained households and very low cost health care in terms of out-of-pocket expenditures, we also expect there to be little effect on health.

We examine several measures of fetal and early childhood health: birth weight, low birth weight, very low birth weight, prematurity, number of days hospitalized, whether there are any hospitalizations, number of emergency room (ER) visits, and whether there are any ER visits. We analyze each outcome separately but also standardize each variable by subtracting off the mean and dividing by the standard deviation. We then create a negative health index that is the standardized sum of each standardized variable. The results suggest that home price increases lead to somewhat healthier births, generated by small positive effects on health at birth. There is a statistically significant reduction in the likelihood of being born premature of 3.31% per 100,000 DKK of home price increase and a non-statistically significant reduction in low-birth weight of 1.73%. We also find a marginally statistically significant increase in birth weight, which is economically small. However, we show that home price increases lead more-advantaged women to give birth. While some of the health effects at birth reflect changes in composition, the compositional shifts cannot explain the entire effect: at least part of the increased health outcomes at birth reflect home-price-induced increases in health of inframarginal births. We do not find any evidence that health in the first five years of life is affected by home price variation. When we aggregate up all health measures from the first year of life into an index, we can rule out health increases greater than 1.09% of a standard deviation. Taken together, these results indicate that any health benefits of home price increases are modest and do not translate into better health in the longer run. This lack of a long-run health effect suggests our results are driven by income effects rather than a relaxation of credit constraints among homeowner families.

Our paper makes several contributions to the literature. First, we provide additional evidence on the effect of family resource shocks on fertility. Whether children are normal goods is an old question in economics, dating back at least to Malthus (1798). A long literature has demonstrated a strong negative cross-sectional relationship between income and fertility, which exists both across countries and across individuals within a country (Jones and Tertilt 2008; Jones, Schoonbroodt, and Tertilt 2010). Recent evidence has challenged the causal interpretation of these negative cross-sectional correlations using plausibly exogenous changes to family resources. Black et al. (2013) show positive fertility effects from the substantial income increases that accompanied the West Virginia coal boom of the 1970s, while Lindo (2010) shows that income reductions associated with job displacement reduce total fertility.[5] Brueckner and Schwandt (2015) use plausibly exogenous oil shocks and show that oil-induced country-level income growth leads to higher fertility. Similarly, Kearney and Wilson (2018) show that male wage increases driven by the fracking boom led to higher marital and non-marital fertility. There also is evidence that fertility is pro-cyclical (Chatterjee and Vogl 2018; Currie and Schwandt 2014), which is consistent with a positive income effect.[6]

The most related studies to this one are the two aforementioned US-based analyses of how housing price changes affect fertility. Lovenheim and Mumford (2013) use data from the Panel Study of Income Dynamics (PSID) from 1985-2007 and estimate models with year and MSA (i.e., city) fixed effects. Dettling and Kearney (2014) use a similar model but with aggregate MSA-level home price data and vital statistics birth data. This paper contributes to this prior work by showing that the effects found in Lovenheim and Mumford (2013) and Dettling and Kearney (2014) extend to a country with very generous social safety net and child support policies. As discussed above, our estimates also probe the sensitivity of the results to a set of fixed effects that account both for a potential form of bias from how long women are exposed to home price shocks and for municipality-year unobserved shocks. That our results are robust

[5] Huttunen and Kellokumpu (2016) re-examine the job displacement effects of fertility and find that female job displacement has a larger negative effect on fertility than does male job displacement, even though male job displacement has a larger effect on family resources. This finding suggests that the negative effect of job displacement on fertility may not be driven by income shocks.

[6] A related literature examines price effects, finding that fertility is declining in female wages (Butz and Ward 1979; Schultz 1985; Heckman and Walker 1990) but increases due to government child-based subsidies (Cohen, Dehejia, and Romanov 2013).

to these additional controls supports the validity of prior research.[7]

We also contribute to the literature by providing the first estimates of how home price changes affect infant health. This is a reduced form effect that operates both through changes in the extensive margin of who gives birth and changes in health outcomes among inframarginal births. Our findings that infant health is marginally better due to home price increases is important given the growing body of work that shows infant health has large long-run consequences for life outcomes (e.g., Figlio et al. 2014).[8] However, our results are in the opposite direction of those in Dehejia and Lleras-Muney (2004), who show that health outcomes at birth increase during periods of higher unemployment. Because home price variation does not tend to be driven by labor markets, it is likely that our estimates are picking up different phenomena. This underscores the importance of separately examining labor market and housing market effects. Our results indicate that home price increases reduce the social costs associated with childbearing by inducing healthier children to be born. Of course, this finding also suggests that home price declines, as occurred in the great recession, likely reduced infant health. That infant health varies cyclically with the housing market even in a developed country like Denmark with free and universal health care and low socioeconomic inequality is a novel finding that highlights the importance of the housing market in driving important life outcomes.

2 Background

2.1 Danish Institutional Setting

Denmark is country where social safety net programs substantially reduce the monetary burden of having a child (see Online Appendix Table A-9). To begin with, the majority of Danish health care services, including prenatal care and all birth related procedures, are free of charge and all residents have equal access (Danish Ministry of Health and Prevention, 2008). Second, Denmark has a generous paid parental leave program. Mothers are entitled to 4 weeks of leave before the due date and 14 weeks of leave after birth. Fathers can take 2 weeks of leave during the first

[7] Lovenheim and Mumford (2013) could have included these controls but the PSID is too small a sample to obtain reliable fixed effects estimates along these dimensions. Dettling and Kearney (2014) use MSA-year aggregate data and thus are unable to control for these potential confounders.

[8] See also the reviews of the large fetal origins hypothesis literature in Almond and Currie (2011) and Almond, Currie, and Duque (forthcoming).

fourteen weeks after the birth of the child. Furthermore, parents can take an additional 32 weeks of paid leave, which can be divided freely between the mother and the father. Parents receive full or partial compensation during leave, depending on their employment contract and collective bargaining agreement.[9] Families in Denmark also have access to highly subsidized child care. The responsibility of the organization of child care institutions falls on the municipalities, and parents are entitled to a place in the public child care system when their child turns 6 months old. The out of pocket costs of child care is at most 30 percent of the actual cost. Low-income households are eligible for additional subsidies. Parents are also paid a lump sum transfer to assist with costs; the exact amount varies with the child's age.

Several aspects of the Danish housing market also are worth noting. Like the US, Denmark has a mortgage credit system that allows borrowers access to relatively cheap and flexible financing of housing.[10] Mortgages are financed through covered bonds (i.e., bonds using a pool of mortgages as collateral) issued by a small number of specialized mortgage banks. Individuals can apply to these institutions for a loan of up to 80% of the home's value. The remaining 20% is a down payment that is either paid out-of-pocket or by using a (partial) bank loan. Due to Danish regulation, there is a strict matching of cash flows from loans to funding known as the "balance principle," meaning that payments by mortgage borrowers are passed through directly to the covered bond investors. Therefore, the investors rather than the mortgage bank bear the interest rate risk and prepayment risk. At the same time, the mortgage bank retains the ownership of the mortgages and bears any credit losses. This is quite similar to the structure of the mortgage-backed securities market in the US.

Fixed-rate mortgages are widely available to mortgage borrowers both in Denmark and the US. Unlike the US system, however, the Danish system allows mortgage borrowers to repurchase their own mortgages from the covered bond pool at the current market price, to transfer the mortgage to a buyer during a property sale, or to refinance at par with the same mortgage bank even if their home equity has declined because of a drop in home prices (Berg et al. 2018).

Mortgages in Denmark also have lower credit risk due to the 20% down payment, to the fact that the interest rate risk and prepayment risk are borne by the investors rather than by

[9] The Danish Childcare Leave act, LBK nr 822 af 20/06/2018, `https://www.retsinformation.dk/Forms/r0710.aspx?id=202000` (in Danish).

[10] Kjeldsen (2004) and Berg et al. (2018) provide detailed comparisons of the Danish and the US mortgage credit systems.

the mortgage banks, and to the credit friendly legal system in case of foreclosure (Berg et al. 2018). As a result, the degree of creditworthiness of the loan applicants plays a smaller role in Denmark than in the US. In particular, all Danish borrowers who are deemed to be creditworthy face the same interest rate, with household income and wealth influencing only the size of the loan. In contrast, borrowers with higher credit scores in the US typically face lower interest rates than those with lower scores. Overall, the Danish system makes it possible to provide low and stable interest rates for homeowners, resulting in higher rates of homeownership across the income distribution than in the US.

2.2 Theoretical Predictions

Why might housing wealth affect childbearing and what drives differences across countries in the wealth effect of fertility? The answer to these questions is important for interpreting our estimates and understanding why the estimates in the US are so similar. In a simple household utility maximization framework, children can be seen as consumption goods. The decision to have a child will be a function of the structure of preferences, prices for child-related goods and services, prices for non-child goods and services, total household resources, and the opportunity cost of time. Because variation in housing wealth does not affect the opportunity cost of time for raising children, we can abstract from labor market effects on fertility.

Theoretical predictions for how housing wealth should influence fertility and early child health rely on the extent to which households are credit constrained. If the household faces credit constraints, then increases in housing wealth can relax those constraints both because housing is a large component of overall household savings portfolios but also because housing wealth is relatively liquid (Mian and Sufi 2011). This liquidity is critical for relaxing credit constraints as it allows the household to more easily smooth consumption surrounding the birth of a child. Two factors are particularly important for credit constrained households: 1) the net price of having a child and 2) the liquidity of housing. If net prices are higher, it increases the likelihood credit constraints bind and raises the importance of sources of liquid wealth. When housing is more liquid, it becomes a more relevant resource with which to relax credit constraints for a given home price increase. Health care is a particularly important source of costs: if constrained households are unable to optimally invest in health care when a woman is pregnant or in the first years of a child's life, health outcomes may degrade. Hence, the effect of housing wealth on early-life health outcomes is linked to the extent to which households are credit constrained.

If households are not credit constrained, then the liquidity of the asset and the net price of having children should have little influence on fertility decisions and health outcomes. Instead, housing wealth will affect fertility and health because of preferences for children and their health. If children and child health are normal goods, then increasing wealth should lead to increased fertility and to better child health outcomes. But these effects will be muted among unconstrained relative to constrained households, as income effects are present for both but only the latter experiences an increased ability to consume optimally.

This framework is important in interpreting estimates of housing wealth effects across different countries (or across different people within a country). Focusing in particular on the US-Denmark comparison, one of the most salient differences across the two countries is in the net price of having childen. Online Appendix Table A-9 presents comparisons of child-related government subsidies in the US and Denmark for the four largest policy categories outside of housing: cash subsidies, child care, parental leave, and health care.[11] Unsurprisingly, subsidies in Denmark are substantially higher than in the US. Denmark has a generous "family allowance" that ranges from \$2,704 for 0-2 year olds to \$562 for 15-17 year olds. The closest analogue in the US is the child tax credit, which families with children under 17 receive and is equal to \$1,000 per year. Including the Earned Income Tax Credit makes the US comparably generous in terms of child-related cash payments, but families that receive the EITC (and in particular the maximum EITC) tend not to be wealthy enough to own homes. For homeowner families, the Danish cash subsidies are much larger than their US counterpart. This difference is even larger for child care subsidies. The Danish government subsidizes upwards of 75% of child-care costs, while the US government only provides a non-refundable tax credit of up to \$3,000 for one child. As a result, out-of-pocket expenditures on child care are much higher in the US than in Denmark.

[11] We do not consider housing subsidies because such subsidies do not change among homeowners upon the birth of a child in either country.

Parental leave differences also are stark across the two countries. In Denmark, there is a total of 52 weeks of paid family leave when a child is born, with a maximum weekly parental leave benefit of $645. In contrast, there is no national paid family leave policy in the US; American women are entitled to 12 weeks of *unpaid* leave, however many employers have more generous paid and unpaid leave policies. Virtually no employers have 52-week paid leave policies, though. Finally, there are differences in the cost of health insurance surrounding premiums and co-pays. It is important to stress that almost no children are uncovered in the US due to Medicaid and SCHIP. But most home-owning families in the US have private insurance, and they not only experience co-payments for birth and any subsequent medical care but also experience a premium increase upon the birth of the first child. Danish families pay nothing out-of-pocket for a birth and do not pay premiums. Taken together, it is clear that having a child in Denmark is far less expensive than having a child in the United States in terms of net out-of-pocket expenditures. This institutional framework suggests that housing wealth should more strongly influence fertility and child outcomes in the US than in Denmark because US households face higher net costs that make them more likely to be credit constrained than their Danish counterparts.

Absent credit constraints, it is much more difficult to make ex-ante predictions of how housing wealth should affect fertility and child health outcomes across countries. The reason for this ambiguity is that the size of any effect is based on preferences for children, health, and other goods. To the extent any of these preferences differ, there will be different wealth effects across countries.

3 Data

We use Danish registry data from 1992 to 2006. The data include individual-level records with household linkages, allowing us to follow the universe of Danish households for over a decade. Our outcome variables of interest concern fertility and early-life infant health. We use two complementary data sets to define our outcome variables. The first is the *Birth Registry* from 1995 to 2006,[12] which includes all (hospital and home) births in Denmark as well as measures

[12] For the variables covering the child's first year of life, data from 2007 are used as well.

of infant health at birth such as birth weight and gestational age. We also use data on hospital admissions and ER visits from the *National Patient Registry* to study effects on infant health within the first year of life and in years 1-5. These data cover the universe of hospitalizations in public and private hospitals.

Our main independent variable concerns short-term housing price changes. The housing data are obtained from *The State's Sales and Valuation Registry* in 1992 to 2005, which includes detailed information on public valuations, sales prices, ownership, and housing type. We rely on public valuation data to construct our measure of once-lagged one-year housing price changes from 1995 to 2006. Public valuations are used as the taxable value for almost all properties in Denmark.[13] All privately owned properties are valued in uneven years and adjusted in even years, which yields estimated values in every year. While these valuations account for an extensive set of observable housing characteristics (e.g., geographic location, year of construction, size, type of heating, type of roof), they have been criticized for being unable to precisely reflect the market value of houses.

To obtain a more accurate measure of the market value of properties not traded in the market, we use a method similar to how the equal-weighted sale price appraisal ratio (SPAR) index is calculated (Bourassa et al. 2006). We use the public valuation of homes in each year corrected by the mean over or undervaluation of houses actually sold in the same year/municipality/housing type/valuation quartile cluster c as an estimate of the market value of homes not traded in the market. As a hypothetical example, consider a house that is valued at 1,000,000 DKK in the municipality of Horsens in 2000, and assume that this valuation is in the third valuation quartile of all houses in Horsens in 2000. If houses that are actually traded in the market in Horsens in 2000 in the third valuation quartile are sold at a price that is 20% higher than their public valuation, a better estimate of the house would be to multiply the public valuation with an adjustment factor of 1.2 to obtain an estimated market value of 1,200,000 DKK.

More formally, denote the public valuation by V, the share of the house an individual owns by S, the sales price of the $k \in K$ houses sold in a cluster c by P. Among homeowner households, home value for woman i with partner j is then calculated as:

[13] Very few properties are exempt from public valuations, e.g., churches.

$$HV_{it} = (V_{itc}S_{it})\left(\frac{1}{K}\sum_{k=1}^{K}\frac{P_{ktc}}{V_{ktc}}\right) + (V_{jtc}S_{jt})\left(\frac{1}{K}\sum_{k=1}^{K}\frac{P_{ktc}}{V_{ktc}}\right). \quad (1)$$

We assign to each woman the value of HV_{it} from her first purchased home regardless of whether she subsequently moves. Hence, HV_{it} is based on the same house for each woman throughout the sample period.

In order to shed light on the accuracy of our constructed home price value, we compare in Figure 1 mean purchase price for homes that are actually sold and the mean estimated home value in the same year calculated using Equation (1) as well as the associated confidence intervals. The figure shows that the two lines are almost completely overlapping, which indicates that estimated home value is on average closely aligned with market value. In Figure 2, we show the mean difference between estimated home values and actual sales prices. This is close to being horizontal, which indicates that short-run home price variation is not driven by time-varying differences in the ability of Equation (1) to estimate the market value of the home.

Finally, we use data from the *Population Registry* and other relevant registries to obtain information on demographic characteristics (age, years of completed schooling, household income, having a partner). All monetary variables are in 100,000 Danish Kroner (DKK) deflated to 2006 prices using the consumer price index (CPI). The exchange rate is approximately 0.15 USD per DKK .

3.1 Analysis Sample

We impose two types of restrictions on the data: one for the sample of houses used and one for the sample of women.[14] We only include houses and apartments with residential use that are located in clusters with at least five sales. The other cases represent particular types of homes, e.g., very expensive apartments in rural areas that do not provide us with sufficient variation to estimate the home value. We only consider normal sales between individuals.[15] We exclude homes with a negative public valuation, negative sales price, or multiple addresses at the same

[14]Restrictions on the sample of homes used to calculate the adjustment factors also apply to the construction of the sample of home owners.

[15]For example, sales between family members are not included since these might not reflect the actual market value.

location. Finally, we omit homes that are sold for more than 300% or less than 40% of the public valuation.[16]

Turning to the sample selection criteria for women, we focus on individuals who were not home owners in the beginning of 1992-1993 and who became a homeowner between 1993 and 2004.[17] This gives us a sample of potential first time home owners. We only focus on women who purchase a home between the ages of 18 and 42.[18] We also drop 2,085 observations with incomplete data. We next omit women whose partners purchased a home after 1993 but before the woman and partner became a couple. Finally, we exclude individuals who bought houses whose public valuation increased by more than 50% from one year to the next and homes in the 1^{st} and 99^{th} percentile of lagged one-year price change. These fluctuations are likely driven by major changes to the property (i.e., additions, selling off land) and not by local housing market variation. This leaves us with a final sample of 1,105,559 observations on 198,435 women aged 20 to 44 who gave birth to a total of 125,903 children in the period 1995 through 2006.

Table 1 presents the summary statistics for our analysis sample. The first group of variables are outcome variables. The fertility rate among homeowners is 11.66%, which is somewhat higher than the national fertility rate of around 7% for the same age group (Online Appendix Table A-8). The latter is similar to the average birth probability in the US (see, e.g., Dettling and Kearney (2014), who report a fertility rate of 70 births per 1,000 women in a reasonably comparable period and age range). Women in our sample are likely to have a higher fertility rate than the average population due to selection into home ownership prior to the decision to have a child. Turning to early-life health outcomes, the average infant in the sample has a birth weight of around 3,570 grams. Over 3% of these children have low birth weight (birth weight below 2,500 grams), 0.58% have very low birth weight (birth weight below 1,500 grams), and 4.67% are premature (gestational age less than 37 completed weeks). The births in our sample are healthier on average than in the US, where over 8% of births are low birthweight, 1.4% are very low birth weight, and 9.85% are premature.[19] However, they are closely aligned with birth outcomes among the full sample of Danish births for women aged 20-44 (Online Appendix

[16] These restrictions exclude 8,581 women, which is 4.3% of the final analysis sample.

[17] Individuals who moved out of their newly purchased home before the end of the year are not included.

[18] Due to the use of a once-lagged one-year change independent variable, this leads to an age range of 20 to 44 year olds. Around 98% of all births in Denmark are to women in this age group.

[19] Source: `https://www.cdc.gov/nchs/fastats/birthweight.htm`.

Table A-8). On average, children in the sample are hospitalized for 1.69 days during their first year of life, and around 6% have an ER visit.[20] These means are slightly lower than those for the broader sample of Danish births.

The second group of variables in Table 1 are housing variables. Consistent with the evidence in Figure 1, we see that the mean estimated home value at the time of purchase is very close to the actual mean purchase sum of houses sold. The average lagged one-year home price change is about 71,000 DKK. Compared to prior studies from the US, women in our sample are subject to smaller and less varied housing price changes. This is in part due to differences in the timespan used to construct the main independent variables but likely also reflects the differences in the credit mortgage markets. Approximately 53% of homes in Denmark were occupied by the owner in 2000 (not included in the table), which is somewhat higher than the overall ownership rate in the US (44% in 2000 for a sample of women between 20 and 44, see Dettling and Kearney 2014).

The final group of variables in the table summarizes characteristics of homeowners. The average homeowner is around 32 years old with 14 years of completed schooling, 88% are married or cohabiting with a partner, and 96% are employed. Overall, demographic characteristics of homeowners in our sample are comparable to those in the US (see Lovenheim and Mumford 2013).

4 Empirical Approach

Our empirical approach relates short-run home price changes to fertility and infant health outcomes over the years 1995-2006. Specifically, we estimate models of the following form:

$$Birth_{iaymt} = \alpha + \beta \Delta HV_{i,t-1} + \gamma X_{it} + \phi_{mt} + \psi_{ay} + \epsilon_{iaymt}, \quad (2)$$

where $Birth_{iaymt}$ is an indicator for whether woman i whose household purchased a house when she was age a in year y and who lives in municipality m gave birth in year t. The model controls for a wide array of observed individual-year level characteristics that are available in the

[20]The number of inpatient days excludes the admission related to the childbirth and the following four days. During this period it was not uncommon to spend two to four days in the hospital after childbirth to recover and adjust to the new role as parent.

rich Danish data, including women's age fixed effects, woman's years of education, number of children in the household, an indicator for having a partner (being married and/or cohabiting), an indicator for being unemployed at least 6 months in a given year, and total CPI-adjusted real family income (woman + partner) in 100,000 DKK units.

The main variable of interest in the model is $\Delta HV_{i,t-1}$, which is the once-lagged one-year home value change experienced by woman i: $HV_{i,t-1} - HV_{i,t-2}$. The variation in this variable comes from two sources. The first is municipality-level changes in home prices that affect all homes similarly. The second is within-municipality changes in home price that are likely to be neighborhood specific.

We include in our preferred estimates two types of fixed effects that restrict the identifying variation in $\Delta HV_{i,t-1}$ that is used. Municipality-by-year fixed effects (ϕ_{mt}) account not only for fixed differences across years and municipalities but also for any unobserved year-specific municipality level shocks that may be correlated with home prices and fertility decisions or outcomes. For example, municipality economic conditions could affect birth outcomes and home prices, as could changes to municipality services such as child care. One may object to the use of these fixed effects, as the municipality-by-year level changes are arguably more likely to be exogenous than across-household home price changes within a municipality. We therefore show estimates that include just municipality and year fixed effects akin to Lovenheim and Mumford (2013) and Dettling and Kearney (2014). The comparison of estimates across these two specifications shows how accounting for any municipality-specific shocks in a given year affects the results.

The second set of fixed effects we include that are new to this literature are age-of-purchase-by-year-of-purchase fixed effects. These controls account for a potential mechanical bias in prior work that has not been addressed because of data limitations. Women who have been in a house longer are more exposed to housing market changes and are more likely to have a baby as they age. Similarly, women who purchase houses at a younger age are more likely to give birth at some point after purchase and have more time of exposure to home price changes. There therefore is a potential bias stemming from the interaction of a housing tenure effect and an age-of-purchase effect. The age-of-purchase-by-year-of-purchase fixed effects fully account for any bias coming from this source, and comparisons of estimates with and without these controls show the size of the bias in prior work from not accounting for this source of variation.

The variable ϵ_{iaymt} in Equation (2) is a random error term. Errors are likely to be correlated within household over time and within municipality over time because of the strong within-municipality correlation of home price changes. Standard errors are clustered at the municipality level throughout the analysis, which handles both sources of error correlation.[21]

The assumptions under which β is identified in this model are similar to those in Lovenheim and Mumford (2013) and Dettling and Kearney (2014) given the similarity in the models. The changes in home prices must be unrelated to unobserved household- or municipality-specific shocks that also correlate with the likelihood of giving birth and with birth outcomes. Given prior work finding procyclical fertility behavior (e.g., Currie and Schwandt 2014; Kearney and Wilson 2018), biases from macroeconomic conditions are a first-order concern. Municipality-by-year fixed effects account for this source of bias, but then the identification assumption is that *differences* in home price growth across houses in the same municipality are uncorrelated with unobserved trends in or shocks to fertility behavior and child health outcomes. Here, the age-of-purchase-by-year-of-purchase fixed effects also are important, as we essentially are comparing fertility behavior and outcomes of households within a municipality in the same year that purchased the home in the same year with women who were identically aged. In order for there to be a bias in these estimates, it must be that higher fertility households or those with better underlying infant health are better at predicting future home price growth when they purchase a home in a way that is uncorrelated with the rich set of observables in the model and with the age at purchase and year of purchase. While possible, we emphasize that this is a weaker set of identifying assumptions than what has been used in prior research on this question.

In order to assess the validity of the identification assumptions, we split households into quartiles of the once-lagged one-year home value change and show how observed characteristics that are residualized to the fixed effects vary with the treatment dosage. Table 2 shows that while home prices and home price changes vary considerably across quartiles (by construction),

[21] While women can move across municipalities, we assign each women the home price changes of her first purchased home throughout even if she moves. Thus, each woman's municipality is fixed and is based on the location of the first purchased home after 1992. Household clusters hence are fully subsumed by the municipality clusters.

there is little systematic variation in key observables across the home price change distribution.[22] We examine real household income, years of education, number of children, whether the woman is unemployed and whether she has a partner. While there are differences across quartiles, these differences are both economically insignificant and idiosyncratic insofar as they do not move unidirectionally across quartiles. For example, there is a small U-shape in mean income across quartiles, while years of education and number of children exhibits no discernible pattern across groups.

To see more directly how the observables vary with home price variation, we use all observed characteristics and calculate predicted fertility for each woman. This is essentially a summary measure of observed characteristics as they relate to fertility. The last row of means in Table 2 shows that predicted fertility changes little across home price growth quartile, and there is no discernible pattern across quartiles. In contrast, actual fertility increases across quartiles. Taken together, these results indicate that fertility is related to home price growth but observed characteristics correlated with fertility are not correlated with home price changes.

Online Appendix Table A-1 shows raw means (i.e., unadjusted for fixed effects) of these variables. Here, neither predicted fertility nor actual fertility varies across quartiles. It is striking that the observables are uncorrelated with home price changes, even in the raw data. In Online Appendix Table A-2, we show means that are residual to year and municipality fixed effects. Once these fixed effects are included, there now is a clear relationship between home price change quartile and fertility. A similar (though weaker) relationship exists for predicted fertility, especially comparing quartile 4 to the other quartiles. A comparison of Tables A-2 and 2 indicates that the additional fixed effects we include in this analysis may be useful in accounting for sorting of homeowners with different underlying fertility patterns into homes with different price growth.

It also is important to emphasize that our estimates are less sensitive to bias from parent mobility than are those from prior work. Rather than focus on a sample of "stayers" who do not move (which is potentially endogenous), we examine a sample of women who first purchase a home after 1992. If they move, they remain in the sample, but we assign everyone the price

[22] We can reject the null hypothesis that the means are equal across quartiles for all variables in Table 2 at high levels of significance. However, this finding reflects the large sample size in our data rather than economically meaningful differences across groups.

changes of their first home even if they move. Conceptually, this is the same as using price changes of one's first home as an instrument for the actual price changes women experience. Our estimates represent the reduced form version of this IV model. Mobility rates are low in Denmark, but our approach still is more robust to endogenous mobility behavior than the approaches used by prior research on this question.

5 Results

5.1 Baseline Fertility Results

Table 3 presents baseline estimates of Equation (2). We alter the set of fixed effects and controls across columns to assess the relative importance of different modeling assumptions. In column (1), we control only for year and municipality fixed effects. The estimate indicates that a 100,000 DKK increase in home value is associated with a 0.16 percentage point increase in the likelihood of giving birth. This estimate is statistically significantly different from zero at the 10% level, and it represents a 1.34% increase relative to the mean fertility rate of 0.1166 (Table 1). As Table 1 shows, the standard deviation of one-year lagged home price growth is about 1, so this also has the natural interpretation of the percent effect of a 1 standard deviation increase in home value.

In column (2), we use municipality-by-year fixed effects. The estimate decreases substantially in magnitude, and it no longer is significant at even the 10% level. This change likely reflects the existence of unobserved shocks at the municipality-year level that are positively correlated both with home price changes and fertility (e.g., local business cycle variation). While suggestive that the estimated effect of home price changes on fertility is not robust to the inclusion of municipality-by-year fixed effects, in column (3) we add in age-of-purchase-by-year-of-purchase fixed effects and the estimate increases substantially. We find in column (3) that a 100,000 DKK increase in home prices leads to a 0.0036 increase in the likelihood of giving birth, which is a 3.07% effect relative to the mean. This estimate is significant at the 1% level. Thus, controlling for municipality-by-year fixed effects reduces the size of the estimated effect, but conditional on those controls accounting for age-of-purchase-by-year-of-purchase fixed effects substantially increases the size of the coefficient. This occurs because the age and year of purchase is associated both with underlying fertility likelihood and with exposure to home price shocks. For example, women who have owned a house for longer may experience larger home price increases but are less likely to have a child because of prior fertility decisions. On net, the more extensive fixed effects that we are able to use here relative to previous studies has little impact on the estimated fertility effect of home price changes.

The final column of Table 3 adds controls for observed characteristics and shows our preferred estimate. We find that a 100,000 DKK increase in home prices leads to a 0.27 percentage point increase in the likelihood of giving birth. Relative to the mean fertility rate, this is a 2.32% effect. Comparing columns (3) and (4), the observables exert very little influence on the estimate. As Online Appendix Table A-3 demonstrates, this is because the fixed effects soak up similar variation to the observables. If only municipality and year fixed effects are included in the model, adding our set of observed characteristics has a very similar effect on the estimates as adding municipality-by-year and age-of-purchase-by-year-of-purchase fixed effects without observables. The estimate from such a model (column (2) of Online Appendix Table A-3) is almost identical to the estimate in column (3) of Table 3. To explore which observables matter most, we include them one-by-one in Online Appendix Table A-4. The table makes clear that age fixed effects and the number of children are the controls that cause the largest increase in the estimated effect.[23] Because these variables are highly related to fertility patterns and to the types of homes families occupy, this is a sensible result. Together, the estimates in Tables 3, A-3, and A-4 indicate that it is necessary either to control for observed homeowner characteristics such as number of children, age, education, and family composition or to control for the expanded set of fixed effects we employ. Controlling for both simultaneously produces effects that are quite similar to, if somewhat smaller than, estimates that use either set of controls separately.

Our preferred estimate indicates that fertility increases by 2.32% for each 100,000 DKK increase in home prices. How does this finding compare with the prior literature? Lovenheim and Mumford (2013) find that a $15,000 increase in home value (equivalent to 100,000 DKK) leads to a 2.64% higher fertility rate. Put on a monetary scale, our results thus are virtually

[23] Conversely, controlling for years of education and whether one has an identified partner substantially attenuates the estimate.

identical to theirs.[24] However, there is much more home price variation in the US than in Denmark: a standard deviation change in home prices in Denmark is equivalent to \$15,702 using one-year changes and \$22,081 using two-year home price changes (Online Appendix Table A-5). In the US, a standard deviation in two-year home price changes is \$73,130 (Lovenheim and Mumford 2013). As shown in Table 4, for a standard deviation increase in 2-year home price change, we find an effect of 1.11% (1.63/1.47). Lovenheim and Mumford (2013) find an effect of 24.1% (17.6/0.7313), which is a sizeable difference.

While our estimate is very similar to Lovenheim and Mumford (2013), at least per dollar of home price change, it is smaller than the estimate reported by Dettling and Kearney (2014). They report an effect of 7.5% per \$15,000 of home price increase. One reason for this difference is that Lovenheim and Mumford estimate regressions using individual data and Dettling and Kearney (2014) estimate models using city-by-year aggregate data. When we aggregate our data up the municipality-by-year and estimate models with our set of aggregated observables as well as municipality and year fixed effects, we obtain an estimate on the one-year lagged home price change of 0.0045(0.0023). This translates into a 6.8% increase in fertility per \$15,000 of home price increase. Hence, our estimates are very similar to those in Dettling and Kearney (2014) when we use a similar data structure and empirical approach. These results also are of independent interest because they show the aggregate effect of home price changes on fertility in a municipality. The aggregate effect is larger than the effect on homeowners, but we underscore that these differences also could reflect upward bias in the aggregate estimates due to an inability to include the expansive set of fixed effects in the individual model.

The fact that housing price increases significantly raises fertility in a country like Denmark is independently interesting. That home price changes affect fertility similarly per dollar in both Denmark and the US suggests that the pro-natalist policies of Denmark (See Online Appendix Table A-9) may not mitigate wealth effects related to fertility. At the same time, the larger magnitude of housing price variation in the US means that the housing market has a larger aggregate effect on fertility behavior in the US than in Denmark.

A core identification assumption embedded in our approach is that there are no contempora-

[24] Lovenheim and Mumford (2013) focus on two-year and four-year changes in home prices rather than the one-year changes we use here. Table 4 shows estimates from our data using two-year and four-year home price changes. We find that a 100,000 DKK increase in home prices over two years leads to a 1.63% and 1.29% increase in fertility, respectively. Thus, examining home price changes over one or two years leads to very similar results.

neous, unobserved shocks in municipalities that are both correlated with the timing, magnitude, and sign of home price changes and with fertility outcomes. One way to test for such shocks is to examine renters, who are subject to such local shocks but who do not experience wealth effects from home price changes. In column (1) of Table 4, we present estimates using a sample of renters who were not homeowners in the prior two years, current year, or subsequent year and who do not live with their parents. We define the sample in this way to avoid problems associated with renters purchasing homes right before they have a child. In addition, we want to avoid bias from spillovers from parents to children that could be induced by home price changes. The resulting sample of renters generally is composed of those who are younger, have fewer years of education, are lower income, have fewer children, and are less likely to have a partner than the those in the homeowner sample. Nonetheless, these estimates provide a check on the results and the existence of bias from contemporaneous shocks. Table 4 shows that the effect of municipality-wide changes in home prices on renter fertility is very small, at -0.00003, and it is not statistically significant at conventional levels. This translates into an effect size of -0.04%. There is no evidence that renters respond to home price changes, supporting our identification strategy.[25]

Table 4 contains several additional estimates that help validate our empirical approach. One main concern with our identification strategy is the existence of unobserved heterogeneity that is correlated both with home price changes and with underlying fertility preferences. In column (2) of Table 4, we include woman fixed effects in the model. The estimate is very similar to baseline and suggests that we are not biased by unobserved attributes of woman and households. In columns (3)-(5) we use two-, three-, and four-year lags to assess the robustness of our estimates to the use of one-year lagged home prices. The estimates are similar to one another and to our baseline result. Finally in Table 4, we assess the sensitivity of our results to more carefully aligning the timing of home price valuation and births. For woman who give birth, we use the gestational length and birth date to calculate a one-year home price change from time of conception. Among women who do not give birth, we conduct a similar calculation using a randomly-assigned birth month. We assign these "control" birth months such that the

[25]These findings are similar to the null results for renters in Lovenheim and Mumford (2013), while Dettling and Kearney (2014) actually find a slight negative effect for renters.

distribution of birth months is the same across women who do and do not give birth. The estimate in column (6) is larger than baseline and indicates a 5.66% increase per $15,000 of lagged home price increase. Note, however, that the sample is smaller because we can only engage in this exercise for women whose conception dates are more than a year after the start of our home price data. These results suggest that our main estimates are conservative.

5.2 Heterogeneous Fertility Results

We next examine how the baseline fertility effect we find varies according to important household and mother characteristics. Panel A of Table 5 shows separate estimates by age group: 20-24, 25-29, 30-34, 35-39, and 40-44. Each set of results is from our preferred model that includes observables, municipality-by-year and age-of-purchase-by-year-of-purchase fixed effects. The effects are quite stable across age groups for those below 40. The differences in baseline fertility rates by age, however, lead to small differences in the percent effect by age. The least responsive group in percent terms are those aged 25-29 (1.47%), while the most responsive group is those aged 35-39 (2.89%). We also note that the effect is not statistically significant at even the 10% level for 20-24 year olds,[26] but the estimate is quite similar to those for older ages. The effect declines substantially among those over 40 and is no longer statistically significantly different from zero at conventional levels, though the base fertility rate is so small among this group that the point estimate still points to a sizable impact of home prices on fertility among 40-44 year olds.

Panel B of Table 5 shows effects by parity. The effect of home price on fertility is largest for first-time mothers, at 0.56 percentage points per 100,000 DKK increase in home price, or 3.65% relative to the mean. Statistically significant and sizable impacts also are evident for families with one and two existing children, while the effect for 3+ children families is not statistically significant. However, the effect size for this group is quite large at over 3%.

Table 6 explores heterogeneity by socioeconomic status of the household. In Panel A, we present results separately by family income quartile. The point estimates suggest that women in the middle of the income distribution are most responsive to home price changes in their

[26]The lack of statistical significance among this group is driven in part by the fact that few 20-24 year olds live in a home that they or their partner own.

fertility decisions. In Panel B, we estimate effects by educational attainment quartile of the mother. There is little evidence of heterogeneity by completed education. The effects are similarly-sized and are statistically significant in all four groups.

The heterogeneous treatment effects in Tables 5 and 6 broadly align with the findings from Lovenheim and Mumford (2013) in the US. What is striking about this concordance is that Denmark has much less socioeconomic inequality than the US, has long parental leave, heavily subsidized child care, free and universal health care, and a very generous social safety net. Despite these equalizing policies, the fertility responses to housing wealth changes are similar in size and character to those in the United States.

5.3 Interpretation and Discussion

One of the strongest conclusions from the fertility analysis is that the effect per dollar is similar in the US and Denmark. This is a surprising finding, since as discussed in Section 2.2, child-related government subsidies and costs are much lower in Denmark. Under what conditions might we expect these estimates to be the same? As the framework in Section 2.2 highlights, housing wealth effects will be similar across contexts when households are not credit constrained and when they have similar preferences for children.

While it is undeniable that the net costs of children are higher in the US than in Denmark, costs are an important mechanism underlying our results only if households are credit constrained. To get a sense of the extent of credit constraints surrounding child birth, we examine expenditure differences across observationally-similar households with and without a child under the age of two using the 2015-2018 Consumer Expenditure Surveys. We take all two adult household and estimate regressions of expenditures on the number of children under two, the number of children aged 2-16, total family income, age and education fixed effects for both adults in the household, Census Division fixed effects, and quarter and year fixed effects. Coefficient estimates on the number of children under the age of 2 are presented in Online Appendix Table A-10. While the assumptions underlying a causal interpretation of these estimates are strong, they provide suggestive evidence of how expenditures among very similar families vary with the presence of young children. In Panel A, we show estimates for the sample of homeowners. Contrary to the existence of credit constraints, there is an increase in expenditures associated with having a young child in the household of 5-6%. This increase is driven by housing and health, while food expenditures decline slightly (possibly because new parents eat out less). Critically, "other" expenditures that reflect general consumption is unchanged. In contrast, in Panel B there is a large decline in total expenditures among renters, and for many large categories the effects for homeowners and renters are statistically significantly different at the 5% level. Hence, these estimates show little evidence of credit constraints among homeowners, even in the US where costs of having children are high. This is a sensible finding because most homeowners already have secured extensive credit to purchase a home. Although we cannot produce similar estimates for Denmark,[27] we expect Danish parents to be less credit constrained than US parents because of the cost differences associated with childbearing.

Evidence from the extant literature also suggests that housing wealth is similarly liquid in Denmark and the US, although this is most relevant in the case of binding credit constraints. The period of our analysis incorporates an unprecedented expansion in the liquidity of home equity through cash out refinances, home equity loans and home equity lines of credit. Mian and Sufi (2011) estimate that each dollar of home equity led to an increase in equity extraction of $0.25 in the US, and a similar study in Denmark found an extraction rate of 21% (De Stefani and Hviid 2018). Combined with the lack of evidence on credit constraints among homeowners, these studies underscore that the there are not consequential differences in the liquidity of housing wealth across Denmark and the US.

That the US and Denmark do not differ in ways that would suggest a difference in the effect of housing wealth on fertility implies that variation in estimates across countries reflects preferences for children. It thus is interesting that the effect per dollar are so similar across countries: holding all else equal, this will only occur when children enter similarly into parental objective functions in the two countries. The finding that parents respond similar to home price changes in their fertility decisions has important implications for uncovering fertility preferences, which would be difficult to do without this comparison. The fact that the income effects we document reflect preferences rather than market conditions or credit constraints implies that

[27] The pattern of results we document are also inconsistent with credit constraints. Credit constraints should bind more for lower-income families, but we do not find that the fertility response to home prices is higher for these families. Furthermore, if credit constraints are the main mechanism we would expect home prices to affect the timing of births rather than the total number of births. However, we find that overall fertility increases.

there also should be at most a small effect on health. If health is a normal good, health expenditures should increase with home prices, but the effect should be small unless home price increases relax credit constraints that impede optimal health investments. We now turn to the first analysis in the literature of the effect of housing wealth on health outcomes to shed light on this question.

5.4 Infant Health Results

Table 7 presents the first estimates in the literature of how housing wealth affects infant health outcomes. Panel A shows estimates of birth outcomes: birth weight (in grams), low birth weight (birth weight < 2,500 grams), very low birth weight (birth weight < 1,500 grams), and prematurity (born before gestational week 37). We find that birth outcomes are positively impacted by home price increases. A 100,000 DKK increase in home prices in the past year leads to a 0.05 percentage point reduction in the likelihood of low birth weight and a 0.15 percentage point reduction in the likelihood of being premature. These are both modest effects (1.71% and 3.31% relative to the mean, respectively) but only the latter is significant at the 5% level. Note that the incidence of low birth weight in Denmark is extremely low, at 3.16%, so detecting an effect on this outcome is difficult. The estimate on birth weight in Table 7 is positive and marginally significant (p-value of 0.1157), but at 0.10 percent of the mean it is economically insignificant. We thus focus on the prematurity and low birth weight outcomes.

It is important to highlight that the estimates in Table 7 reflect both changes in the composition of births and changes in health outcomes among inframarginal births. In Table 8, we estimate regression in the spirit of those in Deheji and Lleras-Muney (2004) to provide direct evidence on compositional shifts. Among those giving birth, we regress observed characteristics on lagged home price change as well as the full set of fixed effects. The results in Table 8 indicate that home price increases cause shifts in the composition of births towards older, more educated, higher income, and two-parent households. These changes should lead to increases in health outcomes, so at least some of the health effects we find are driven by changes in composition.[28] At the same time, the birth weight and prematurity effects we document in Table

[28] We control for each of these observables, so changes in these observables do not drive our results. However, there likely are other characteristics for which we cannot control that also change with home prices and can be related to underlying fertility.

7 are unlikely to be solely due to compositional changes. We can calculate an upper bound of the effect of the change in composition of births by dividing the health effects by the birth effect in Table 3. Essentially, this calculation treats housing prices as an instrument for birth, assuming all of the change in birth outcomes is driven by the change in births that occur. This calculation leads to an "effect" of -18.5 percentage points (-0.0005/0.0027) in the likelihood of low birth weight and -55.6 percentage points (-0.0015/0.0027) in the likelihood of prematurity. These effects are implausibly large given the low prevalence of these outcomes in Denmark, which suggests that some of the health effects we document in Table 7 is due to health changes among inframarginal births.

In Online Appendix Table A-6, we show low birth weight and prematurity effects of home price changes by mother's age, parity, income and education.[29] The estimates are imprecise, but they provide suggestive evidence that the low birth weight and prematurity reductions are predominantly concentrated among the youngest (20-24) and oldest (40-44) mothers in our sample and among new mothers and those with 3 or more children. While none of the estimates is statistically significant at conventional levels, they are economically significant for these groups. There also is suggestive evidence that mothers in the bottom half of the income distribution and who have below-median education attainment experience the largest changes in the likelihood of having a premature birth due to home price changes. Online Appendix Table A-7 shows similar results for our two health indices. The estimates are less precise, but the point estimates indicate that younger mothers and those who are having their first birth and who have large families experience reductions in negative child health outcomes due to home price increases.

Do the increases in health at birth translate into better longer-run health? Panel B of Table 7 shows results for health outcomes in the first year of life: number of days hospitalized, ever hospitalized, number of ER visits, and ever had an ER visit. While these are rather extreme health outcomes, a large portion of the sample (35%) has been hospitalized, and ER visits are not that uncommon. However, we find no statistically significant evidence that health in the first year of life changes. The point estimates in the first two columns are negative, but they are not statistically significantly different from zero at even the 10% level. Furthermore, we can

[29] Estimates for other health outcomes are available from the authors upon request.

rule out a decline of more than 4% in column (1) and a decline larger than 0.9% in column (2) at the 95% level. The point estimates in columns (3) and (4) for ER visits are positive but are modest in size and also are precisely estimated.

The final panel in Table 7 shows the effect on a normalized negative health index. To obtain this index, we first standardize each variable such that it has a mean of zero and a standard deviation of one. To ensure that all health outcomes have the same negative interpretation, we use the negative of birth weight. We then add each standardized variable together, divide by the number of variables that make up the index, and standardize. The result is an aggregate standardized measure of negative health outcomes with a mean of zero and a standard deviation of one.[30] The estimate in column (1) is negative, consistent with better infant health due to home price increases, but the effect size is quite small at -0.44% of a standard deviation. We can rule out an effect size larger than -1.09% at the 95% level, which suggests that any health effects are at most quite modest. In column (2), we examine whether home price increases reduce the likelihood of any adverse health event. Again, we find a small estimate that is precisely estimated and is not statistically significant.

In Table 9 we estimate effects of home price increases prior to birth on health outcomes in years 1-5 of life. These estimates provide information on medium-run effects of housing wealth on child health. The estimates are small, precisely estimates, and none is statistically significantly different from zero. Combined with the estimates from Table 7, we find little evidence of changes in health among 0-5 year olds due to home price increases prior to birth. Hence, the reduced prematurity and higher birth weight do not translate into longer-run health outcomes that we can detect in our data.

Taken together, the results in Table 7-9 and A-6 to A-7 suggest that home price increases lead to somewhat healthier births but have little effect on health measures in the first five years of life. Measures of health at birth have been shown to be strong measures of long-run life outcomes like academic achievement (Figlio et al. 2014) as well as later-life outcomes (Currie and Rossin-Slater 2015). That we find no effect of home prices on health outcomes among 0-5 year olds combined with the evidence that home price increase induce births among more advantaged households suggests that there are few real health implications of home price

[30]See Finkelstein et al. (2012) for a similar use of indices to measure health outcomes.

variation. This finding is aligned with our argument in Section 5.3 that homeowner households are not credit constrained. If the effect of housing wealth on fertility simply reflects and income effect, we would not expect much of an effect on child health because families are already optimizing health investments. While there is likely to be a positive income effect in terms of child health, since health is a normal good, the massive government subsidies in Denmark that lead health care to ostensibly be free likely mutes any income effect. Estimating effects on health in other settings with less generous health care subsidies would be an interesting direction for future work to shed further light on this question.

6 Conclusion

This paper examines whether short-run housing price variation generates changes in fertility as well as in infant health. We contribute to existing studies on the link between housing markets and fertility as well as the larger literature on how fertility responds to household resource variation by examining this question using a large dataset with a rich conditioning set in Denmark. Our results show that even after controlling for potential sources of bias from municipality-by-year shocks or from an interaction of age-of-purchase and year-of-purchase housing tenure effects, the effect of home price changes on fertility are very similar to those found in the United States by Lovenheim and Mumford (2013). The results are robust to additional fixed effects that account for potential sources of bias that appear important ex-ante. That they are unimportant ex-post supports the validity of prior research that was unable to include these controls. We also find on the whole a similar pattern of heterogeneity as in the US. Our finding that the effect of housing wealth on fertility is of a similar size and character in Denmark as in the US suggests the pro-natalist policies of Denmark (e.g., long parental leave, heavily subsidized child care, and free and universal health care) do not mitigate wealth effects related to fertility. We argue the similarity of effects across the US and Denmark reflect a lack of credit constraints among homeowners combined with similar preferences for children across countries.

We present the first estimates in the literature on the effect of home price changes on infant health. We find modest but economically significant effects on health at birth in terms of a lower likelihood of low birth weight and prematurity. Some but not all of this effect reflects changes in the composition of births. Our results thus indicate that home price increases lead to healthier births, but we do not find any evidence that health outcomes among 0-5 year olds are affected. The infant and young child health results generally support our argument that homeowner households do not face credit constraints with respect to the birth of a child; infant health increases modestly due to an income effect, but the heavily subsidized nature of health care in Denmark renders any effect quite small in the absence of significant constraints. The effect of housing wealth on health of young children in a setting without such high levels of government subsidy, such as the U.S., may be much larger. We view this as an important area for future research.

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Table 1: Summary Statistics

Variable	Mean	S.D.
Outcome variables:		
Birth (0/1)	0.1166	0.3210
Birth weight (grams)	3,571.1825	571.9259
Low birth weight (birth weight < 2,500 grams, 0/1)	0.0316	0.1750
Very low birth weight (birth weight < 1,500 grams, 0/1)	0.0058	0.0759
Premature (born before gestational week 37, 0/1)	0.0467	0.2109
Number of days hospitalized in first year of life	1.6918	7.1661
Hospitalized in first year of life (0/1)	0.3494	0.4768
Number of ER admissions in first year of life	0.0647	0.2802
ER admission in first year of life (0/1)	0.0576	0.2331
Housing variables:		
Lagged one-year home value change (100,000 DKK)	0.7084	1.0468
Purchase sum at time of purchase (100,000 DKK)[a]	9.9528	5.0229
Estimated home value at time of purchase (100,000 DKK)[a]	9.7807	4.7646
Control variables:		
Real household income	5.8532	3.3496
Years of education	14.0773	2.2306
Number of children	1.3003	1.0494
Partner (married and/or cohabiting, 0/1)	0.8820	0.3226
Unemployed (0/1)	0.0358	0.1857
Age	32.6639	5.3914
20-24 years old (0/1)	0.0594	0.2364
25-29 years old (0/1)	0.2457	0.4305
30-34 years old (0/1)	0.3317	0.4708
35-39 years old (0/1)	0.2367	0.4250
40-44 years old (0/1)	0.1265	0.3324

Number of observations = 1,105,559, number of women = 198,435, number of births = 125,903 (125,291 with non-missing information on birth weight). 100,000 DKK ≈ 13,000 EUR ≈ 15,000 USD. a) Includes only homes where there is sufficient variation in the associated cluster to calculate the correction factor in the year of purchase to compare it to the estimated home value.

Table 2: Mean Mother and Household Characteristics by Quartile of Lagged One-year Home Value Change Conditional on Year-by-Age and Age-by-Year-of-First-Purchase Fixed Effects

	Quartile of One-year Lagged Home Value Change			
	1st quartile	2nd quartile	3rd quartile	4th quartile
Variable	(1)	(2)	(3)	(4)
Lagged one-year home	-0.5771	-0.1530	0.0426	0.6875
value change	(0.5006)	(0.4129)	(0.4614)	(0.8973)
Total adjusted housing	0.3422	-0.5350	-0.2110	0.5081
value in year of purchase	(2.9330)	(2.6303)	(2.7687)	(3.8469)
Purchase sum in	-0.0969	-0.6477	-0.2224	0.9993
year of purchase	(3.2118)	(2.9271)	(3.1508)	(4.2629)
Real household income	-0.0144	-0.2431	-0.1350	0.3925
	(2.6383)	(2.2739)	(2.9355)	(4.4087)
Years of education	-0.0285	-0.0593	-0.0320	0.1198
	(2.0465)	(2.0354)	(2.0573)	(2.1217)
Number of children	0.0079	-0.0472	-0.0120	0.0514
	(0.9032)	(0.8894)	(0.8680)	(0.8632)
Unemployed	-0.0001	0.0015	0.0001	-0.0015
	(0.1942)	(0.1966)	(0.1831)	(0.1625)
Partner	-0.0015	-0.0325	-0.0083	0.0423
	(0.2833)	(0.3317)	(0.3172)	(0.2948)
Fertility	-0.0014	-0.0023	-0.0003	0.0041
	(0.3146)	(0.3191)	(0.3160)	(0.3116)
Predicted fertility	0.1172	0.1207	0.1174	0.1112
	(0.0791)	(0.0780)	(0.0779)	(0.0796)
Number of observations	276,390	276,392	276,388	276,389
Year FE				
Municipality FE				
Year-by-municipality FE	X	X	X	X
Age-by-year of FP FE	X	X	X	X

Means are residual to year-by-municipality and age-by-year-of-first-purchase fixed effects. Standard deviations in parentheses. Predicted fertility is obtained from the main specification in Table 3, column (4) estimated without lagged one-year home value change in the regression.

Table 3: Linear Probability Model Estimates of the Effect of Housing Prices on Birth Probability

	Dep. Var.: I(Birth) in Prior Year			
Independent Variable	(1)	(2)	(3)	(4)
Lagged one-year	0.0016*	0.0006	0.0036***	0.0027***
home value change	(0.0008)	(0.0006)	(0.0009)	(0.0006)
Real household				-0.00002
income				(0.0002)
% Effect	1.34	0.49	3.07	2.32
R^2	0.002	0.005	0.035	0.060
Year FE	X			
Municipality FE	X			
Year×mun FE		X	X	X
Age×year of FP FE			X	X
Controls				X

Columns (2) through (4) include controls for the woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). Both lagged one-year home value change and real household income are in 100,000 DKK. Dependent variable mean = 0.1166; Number of observations = 1,105,559. The % Effect shows the effect of a 100,000 DKK change in home prices relative to the mean fertility rate. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 4: Robustness Checks

	Dep. Var.: I(Birth) in Prior Year					
	Renters	Woman FE	Lagged 2-Year Change	Lagged 3-Year Change	Lagged 4-Year Change	Align Valuation & Conception
Ind. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Lagged home	-0.00003	0.0019***	0.0019***	0.0020***	0.0015***	0.0066***
value change	(0.0006)	(0.0006)	(0.0005)	(0.0004)	(0.0003)	(0.0004)
Real household	0.0012***	-0.0010***	-0.0001	-0.0001	-0.0002	-0.0003*
income	(0.0004)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
% Effect	-0.04	1.63	1.63	1.72	1.29	5.66
R^2	0.0470	0.374	0.059	0.059	0.059	0.076
Observations	3,763,425	1,105,559	904,309	730,195	580,914	850,123

The sample in column (1) consists of all women between 20 and 44 in years where they did not live in the same household as their either of their parents, were not a homeowner in the previous two years, the present year, and the subsequent year. The one-year lagged home price change is at the municipality level and include observed characteristics as well as municipality and year fixed effects. The estimates in column (2) include women fixed effects as well as municipality-by-year fixed effects. The results in columns (3) through (5) use different home price lags and include observed characteristics, age-of-purchase-by-year-of-purchase fixed effects municipality-by-year. In Column (6), for women who give birth the 1-year home price change is measured relative to the time of conception using birth date and gestation length. Women who did not give birth are assigned a random conception date that replicates the overall distribution of conception dates among homeowners. Observed characteristics include woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). Both lagged one-year home value change and real household income are in 100,000 DKK. The % Effect shows the effect of a 100,000 DKK change in home prices relative to the mean fertility rate. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 5: Linear Probability Model Estimates of the Effect of Housing Prices on Birth Probability, by Age Group and Parity

Panel A: Fertility Effects by Age Group

	Dependent Variable: I(Birth) in Prior Year				
	20-24 years	25-29 years	30-34 years	35-39 years	40-44 years
Independent Variable	(1)	(2)	(3)	(4)	(5)
Lagged one-year	0.0025	0.0025*	0.0030**	0.0020**	0.0005
home value change	(0.0019)	(0.0012)	(0.0010)	(0.0007)	(0.0005)
Real household	0.0010	-0.0008	-0.0003	-0.0001	-0.0001
income	(0.0007)	(0.0007)	(0.0002)	(0.0002)	(0.00009)
% Effect	2.20	1.47	2.02	2.89	3.25
Dep. Var. Mean	0.1137	0.1723	0.1489	0.0683	0.0156
R^2	0.081	0.039	0.058	0.047	0.032
Observations	65,666	271,643	366,700	261,662	139,888

Panel B: Fertility Effects by Parity

	Dependent Variable: I(Birth) in Prior Year			
	0 children	1 child	2 children	3+ children
Independent Variable	(1)	(2)	(3)	(4)
Lagged one-year	0.0056***	0.0032**	0.0010**	0.0009
home value change	(0.0009)	(0.0012)	(0.0005)	(0.0006)
Real household	0.0002	0.0017***	-0.0002**	-0.0004**
income	(0.0005)	(0.0004)	(0.0001)	(0.0001)
% Effect	3.65	1.52	2.20	3.16
Dep. Var. Mean	0.1532	0.2079	0.0475	0.0279
R^2	0.053	0.056	0.024	0.041
Observations	318,993	281,096	383,080	122,390

Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children (only in panel A), indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition, municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 6: Linear Probability Model Estimates of the Effect of Housing Prices on Birth Probability, by Quartile of Real Household Income and Quartile of Mother's Education

Panel A: Fertility Effects by Mother's Income Quartile				
	Dependent Variable: I(Birth) in Prior Year			
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile
Independent Variable	(1)	(2)	(3)	(4)
Lagged one-year	0.0012	0.0037***	0.0036***	0.0017**
home value change	(0.0012)	(0.0009)	(0.0009)	(0.0007)
Real household	0.0042***	-0.0120***	-0.0085***	0.0002
income	(0.0010)	(0.0027)	(0.0018)	(0.0001)
% Effect	1.18	2.67	3.03	1.66
Dep. Var. Mean	0.1043	0.1374	0.1197	0.1050
R^2	0.042	0.071	0.085	0.089
Observations	276,385	276,384	276,384	276,384

Panel B: Fertility Effects by Mother's Education Quartile				
	Dependent Variable: I(Birth) in Prior Year			
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	Top Quartile
Independent Variable	(1)	(2)	(3)	(4)
Lagged one-year	0.0029***	0.0023***	0.0037***	0.0026***
home value change	(0.0008)	(0.0007)	(0.0011)	(0.0010)
Real household	0.0008**	-0.0008***	0.0002	-0.0002
income	(0.0003)	(0.0003)	(0.0002)	(0.0002)
% Effect	2.97	1.97	2.93	1.85
Dep. Var. Mean	0.0969	0.1172	0.1256	0.1387
R^2	0.049	0.070	0.079	0.094
Observation	351,354	348,000	172,601	233,604

Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 7: The Effect of Housing Prices on Birth and Early Life Health Outcomes

Panel A: Birth Weight and Prematurity				
Independent Variable	Birth Weight (g) (1)	Low Birth Weight (2)	Very low Weight (3)	Premature (4)
Lagged one-year	3.5267	-0.0005	0.00004	-0.0015**
home value change	(2.2225)	(0.0006)	(0.0003)	(0.0007)
Real household	1.2186*	-0.0005**	-0.0001*	-0.0003
income	(0.7342)	(0.0002)	(0.00006)	(0.0002)
% Effect	0.10	-1.73	0.69	-3.31
Dep. Var. Mean	3,571	0.0316	0.0058	0.0467
Observations	125,291	125,291	125,291	125,903

Panel B: Hospitalization and ER Visits				
Independent Variable	Number of Days Hospitalized (1)	Ever hospitalized (2)	Number of ER Visits (3)	Ever ER Visit (4)
Lagged one-year	-0.0023	-0.0005	0.0007	0.0005
home value change	(0.0270)	(0.0016)	(0.0009)	(0.0009)
Real household	-0.0182***	-0.0015***	-0.00004	0.00006
income	(0.0058)	(0.0004)	(0.0002)	(0.0002)
% Effect	-0.13	-0.14	1.13	0.80
Dep. Var. Mean	1.6918	0.3494	0.0647	0.0576
Observations	125,903	125,903	125,903	125,903

Panel C: Health Index		
Independent Variable	Negative Health Index (1)	Any Health Issues (Health Index > 0) (2)
Lagged one-year	-0.0044	-0.0011
home value change	(0.0033)	(0.0016)
Real household	-0.0030**	-0.0018***
income	(0.0011)	(0.0005)
% Effect		-0.31
Dep. Var. Mean	0	0.3687
Observation	125,903	125,903

Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Estimates are conditional on giving birth. Multiple births are excluded. Columns (1) through (3) in Panel A have smaller sample sizes than the remaining columns due to missing information on birth weight. Information with missing information on birth weight are assumed to be neither low or very low birth weight when calculating the health index used in Panel C, column (1) and the health indicator used in column (2). Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 8: The Effect of Housing Prices on the Composition of Mothers Who Give Birth

	Dependent Variable:				
		Years of	Number of	Household	
	Age	Education	Children	Income	I(Partner)
Independent Variable	(1)	(2)	(3)	(4)	(5)
Lagged one-year	0.398***	0.087***	0.024***	0.347***	0.031***
home value change	(0.056)	(0.011)	(0.005)	(0.056)	(0.008)

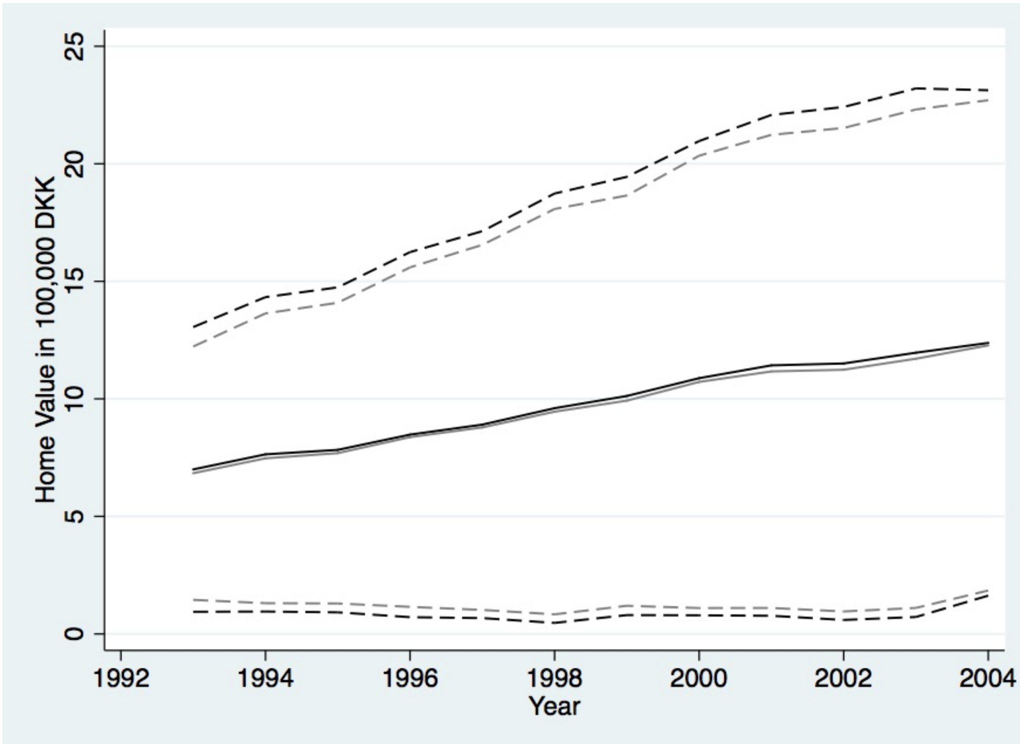
Each column is a separate regression and includes women from our main analysis sample who give birth. The controls include municipality-by-year and age-by-year-of-first-purchase fixed effects. Dependent variables are measured in the year of birth. Lagged one-year home value change is in 100,000 DKK. Multiple births are excluded. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 9: The Effect of Housing Prices on Health Outcomes in Years 1-5

	# Days	Ever	Number of	Ever ER
	Hospitalized	hospitalized	ER Visits	Visit
Independent Variable	(1)	(2)	(3)	(4)
Lagged one-year	0.0019	0.0010	-0.0005	0.0010
home value change	(0.0348)	(0.0019)	(0.0057)	(0.0018)
Real household	-0.0209***	-0.0012***	-0.0003	-0.0009**
income	(0.0101)	(0.0004)	(0.0022)	(0.0004)

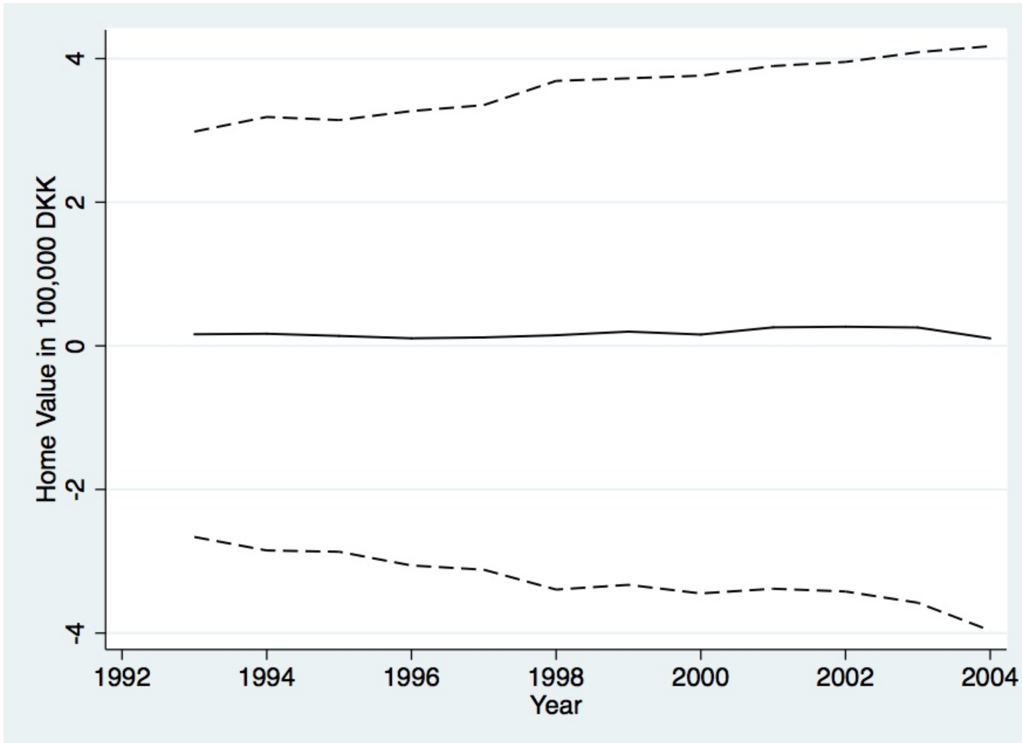
Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Estimates are conditional on giving birth. Multiple births are excluded. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Figure 1: Comparing Mean Estimated Home Value with Mean Actual Purchase Price



The solid black line illustrates mean actual purchase price in year of purchase. The solid gray line illustrates the mean estimated home value in the year of purchase for the homes actually sold. The black and gray dashed lines illustrate upper and lower bounds of the 95% confidence intervals associated with mean actual purchase price and mean estimated home value, respectively.

Figure 2: Mean Residual (Purchase Price Minus Estimate Home Value)



The solid line illustrates mean difference in actual purchase price and estimated value in the year of purchase for homes actually sold. The dashed lines illustrate upper and lower bounds of the associated 95% confidence interval.

Online Appendix: Not for Publication

Table A-1: Raw Means of Mother and Household Characteristics by Quartile of Lagged One-year Home Value Change

	Quartile of One-year Lagged Home Value Change			
	1st quartile	2nd quartile	3rd quartile	4th quartile
Variable	(1)	(2)	(3)	(4)
Lagged one-year home	-0.3776	0.2892	0.8179	2.1039
value change	(0.3408)	(0.1424)	(0.1748)	(0.9495)
Total adjusted housing	9.5192	8.1007	9.4418	12.1272
value in year of purchase	(4.4018)	(3.8623)	(3.8901)	(5.7493)
Purchase sum in	9.2939	8.1645	9.6036	12.7505
year of purchase	(4.5394)	(4.0626)	(4.1721)	(5.9063)
Real household income	5.6924	5.3653	5.6860	6.6690
	(2.7702)	(2.4028)	(3.0672)	(4.5898)
Years of education	13.8714	13.7931	14.0343	14.6104
	(2.1771)	(2.1718)	(2.1931)	(2.2864)
Number of children	1.3781	1.2216	1.2433	1.3582
	(1.0588)	(1.0601)	(1.0435)	(1.0258)
Unemployed	0.0397	0.0409	0.0352	0.0274
	(0.1953)	(0.1980)	(0.1842)	(0.1631)
Partner	0.9062	0.8630	0.8675	0.8912
	(0.2915)	(0.3438)	(0.3390)	(0.3114)
Fertility	0.1165	0.1193	0.1168	0.1140
	(0.3208)	(0.3241)	(0.3211)	(0.3178)
Predicted fertility	0.1145	0.1188	0.1193	0.1138
	(0.0751)	(0.0741)	(0.0739)	(0.0748)
Number of observations	276,390	276,392	276,388	276,389
Year FE				
Municipality FE				
Year-by-municipality FE				
Age-by-year of FP FE				

The tabulations are raw means and are not residual to any fixed effects or controls. Standard deviations in parentheses. Predicted fertility is obtained from regressing fertility on all control variables without the lagged one-year home value change or fixed effects.

Table A-2: Mean Mother and Household Characteristics by Quartile of Lagged One-year Home Value Change, Conditional on Year and Municipality Fixed Effects

	Quartile of One-year Lagged Home Value Change			
	1st quartile	2nd quartile	3rd quartile	4th quartile
Variable	(1)	(2)	(3)	(4)
Lagged one-year home	-0.6861	-0.2125	0.0373	0.8613
value change	(0.5656)	(0.4737)	(0.4883)	(0.8852)
Total adjusted housing	0.3178	-0.6717	-0.3140	0.7820
value in year of purchase	(3.2877)	(3.0316)	(3.1352)	(4.3252)
Purchase sum in	-0.1301	-0.8020	-0.3180	1.2911
year of purchase	(3.5579)	(3.3160)	(3.5233)	(4.7277)
Real household income	-0.0158	-0.3176	-0.1529	0.4864
	(2.6940)	(2.3382)	(2.9993)	(4.4873)
Years of education	-0.0297	-0.1197	-0.0460	0.1954
	(2.1068)	(2.0971)	(2.1276)	(2.1998)
Number of children	0.0133	-0.0928	-0.0258	0.1053
	(1.0395)	(1.0276)	(1.0009)	(0.9859)
Unemployed	0.0002	0.0012	-0.0006	-0.0009
	(0.1947)	(0.1972)	(0.1836)	(0.1628)
Partner	-0.0017	-0.0341	-0.0082	0.0441
	(0.2839)	(0.3326)	(0.3183)	(0.2971)
Fertility	-0.0021	0.0001	0.0003	0.0017
	(0.3205)	(0.3238)	(0.3208)	(0.3174)
Predicted fertility	0.1175	0.1212	0.1181	0.1096
	(0.0765)	(0.0752)	(0.0749)	(0.0756)
Number of observations	276,390	276,392	276,388	276,389
Year FE	X	X	X	X
Municipality FE	X	X	X	X
Year-by-municipality FE				
Age-by-year of FP FE				

The tabulations are all residual to year and municipality fixed effects. Standard deviations in parentheses. Predicted fertility is obtained from regressing fertility on all control variables and year fixed effects and municipality fixed effects without the lagged one-year home value change.

Table A-3: Linear Probability Model Estimates of the Effect of Housing Prices on Birth Probability Using Different Sets of Controls

	Dep. Var.: I(Birth) in Prior Year		
Independent Variable	(1)	(2)	(3)
Lagged one-year home value change	0.0016*	0.0037***	0.0031***
	(0.0008)	(0.0006)	(0.0007)
Real household income		-0.00004	-0.00001
		(0.0002)	(0.0002)
% Effect	1.34	3.20	2.67
R^2	0.0020	0.0557	0.0572
Year FE	X	X	X
Municipality FE	X	X	X
Controls		X	X
Age×year of FP FE			X

Columns (2) through (5) include controls for the womans age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). Both lagged one-year home value change and real household income are in 100,000 DKK. Dependent variable mean = 0.1166; Number of observations = 1,105,559. The % Effect shows the effect of a 100,000 DKK change in home prices relative to the mean fertility rate. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table A-4: Linear Probability Model Estimates of the Effect of Housing Prices on Birth Probability Controlling Separately for Each Observable Characteristic

	Dependent Variable: I(Birth) in Prior Year						
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lagged one-year	0.0016*	0.0056***	0.0055***	0.0016**	0.0003	0.0002	0.0016*
home value change	(0.0008)	(0.0010)	(0.0011)	(0.0007)	(0.0007)	(0.0006)	(0.0008)
Number of children			-0.0532***				
			(0.0022)				
Total household income				0.00002			
				(0.0004)			
Years of education					0.0081***		
					(0.0002)		
Partner						0.0421***	
						(0.0012)	
Unemployed							-0.0610***
							(0.0014)
Year FE	X	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X	X
Age FE		X					
R^2	0.002	0.033	0.030	0.002	0.005	0.004	0.003

Each column is a separate regression; N=1,105,559. Column (1) replicates column (1) in Table 3. Both lagged one-year home value change and real household income are in 100,000 DKK. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table A-5: Summary Statistics Using Two-Year Changes

Variable	Mean	S.D.
Individual- and household-level variables:		
Birth	0.1101	0.3131
Lagged two-year home value change	1.2190	1.4721
Real household income	5.9143	3.3526
Years of education	14.0907	2.2190
Number of children	1.3793	1.0426
Partner (married and/or cohabiting)	0.8810	0.3237
Unemployed	0.0353	0.1846
Age	33.1596	5.2104
20-24	0.0378	0.1908
25-29	0.2288	0.4201
30-34	0.3416	0.4742
35-39	0.2550	0.4359
40-44	0.1368	0.3436

Number of observations = 892,482, number of women = 198,435. 100,000 DKK ≈ 13,000 EUR ≈ 15,000 USD.

Table A-6: Linear Probability Model Estimates of the Effect of Housing Prices on Low Birth Weight and Prematurity, by Age Group, Parity, Quartile of Real Household Income, and Quartile of Mother's Education

Panel A: Low Birth Weight and Prematurity by Age Group					
	20-24 years	25-29 years	30-34 years	35-39 years	40-44 years
	(1)	(2)	(3)	(4)	(5)
Independent Variable	Dependent Variable: Low Birth Weight				
Lagged one-year	-0.0032	-0.0010	-0.0003	0.0014	-0.0026
home value change	(0.0061)	(0.0013)	(0.0009)	(0.0018)	(0.0079)
Independent Variable	Dependent Variable: Premature				
Lagged one-year	-0.0088	-0.0021	-0.0010	0.0008	-0.0062
home value change	(0.0062)	(0.0015)	(0.0012)	(0.0021)	(0.0075)

Panel B: Low Birth Weight and Prematurity by Parity				
	0 children	1 child	2 children	3+ children
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Low Birth Weight			
Lagged one-year	-0.0015	0.0004	-0.0013	-0.0053
home value change	(0.0014)	(0.0008)	(0.0016)	(0.0081)
Independent Variable	Dependent Variable: Premature			
Lagged one-year	-0.0025*	-0.0002	-0.0020	-0.0047
home value change	(0.0014)	(0.0010)	(0.0020)	(0.0115)

Panel C: Low Birth Weight and Prematurity by Mother's Income Quartile				
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Low Birth Weight			
Lagged one-year	-0.0002	-0.0002	-0.0011	-0.0005
home value change	(0.0018)	(0.0014)	(0.0014)	(0.0011)
Independent Variable	Dependent Variable: Premature			
Lagged one-year	-0.0032	-0.0021	-0.0015	0.0002
home value change	(0.0026)	(0.0016)	(0.0016)	(0.0014)

Panel D: Low Birth Weight by Mother's Education Quartile				
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	Top Quartile
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Low Birth Weight			
Lagged one-year	0.0001	-0.0011	0.0022	-0.0004
home value change	(0.0015)	(0.0015)	(0.0018)	(0.0011)
Independent Variable	Dependent Variable: Premature			
Lagged one-year	-0.0008	-0.0048***	0.0031	-0.0003
home value change	(0.0020)	(0.0017)	(0.0030)	(0.0012)

Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children (not in panel B), indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table A-7: Linear Probability Model Estimates of the Effect of Housing Prices on Negative Health Index and Any Negative Health Issues, by Age Group, Parity, Quartile of Real Household Income, and Quartile of Mother's Education

Panel A: Negative Health Index and Any Negative Health Issues by Age Group

	20-24 years	25-29 years	30-34 years	35-39 years	40-44 years
	(1)	(2)	(3)	(4)	(5)
Independent Variable	Dependent Variable: Negative Health Index				
Lagged one-year	-0.0427	-0.0051	-0.0048	0.0101	-0.0291
home value change	(0.0299)	(0.0069)	(0.0056)	(0.0111)	(0.0369)
Independent Variable	Dependent Variable: Any Negative Health Issues				
Lagged one-year	0.0008	0.0037	-0.0053*	-0.0029	0.0007
home value change	(0.0140)	(0.0034)	(0.0028)	(0.0049)	(0.0309)

Panel B: Negative Health Index and Any Negative Health Issues by Parity

	0 children	1 child	2 children	3+ children
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Negative Health Index			
Lagged one-year	-0.0111*	0.0021	-0.0071	-0.0642
home value change	(0.0065)	(0.0048)	(0.0109)	(0.0456)
Independent Variable	Dependent Variable: Any Negative Health Issues			
Lagged one-year	-0.0042	0.0005	-0.0053	-0.0200
home value change	(0.0034)	(0.0023)	(0.0069)	(0.0186)

Panel C: Negative Health Index and Any Negative Health Issues by Mother's Income Quartile

	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Negative Health Index			
Lagged one-year	0.0016	-0.0075	-0.0040	0.0005
home value change	(0.0100)	(0.0075)	(0.0071)	(0.0071)
Independent Variable	Dependent Variable: Any Negative Health Issues			
Lagged one-year	0.0075	0.0004	-0.0011	-0.0018
home value change	(0.0048)	(0.0041)	(0.0037)	(0.0038)

Panel D: Negative Health Index and Any Negative Health Issues by Mother's Education Quartile

	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	Top Quartile
	(1)	(2)	(3)	(4)
Independent Variable	Dependent Variable: Negative Health Index			
Lagged one-year	-0.0058	-0.0087	0.0097	0.0035
home value change	(0.0100)	(0.0082)	(0.0118)	(0.0063)
Independent Variable	Dependent Variable: Any Negative Health Issues			
Lagged one-year	0.0008	-0.0012	-0.0030	0.0002
home value change	(0.0054)	(0.0037)	(0.0042)	(0.0031)

Each column is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children (not in panel B), indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + partner). In addition municipality-by-year and age-by-year-of-first-purchase fixed effects are included. Both lagged one-year home value change and real household income are in 100,000 DKK. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table A-8: Summary Statistics on Fertility and Infant Health Outcomes for Children born by Women between 20 and 44 in 1995-2006 in Denmark

Variable	Mean	S.D.
Birth (0/1)	0.0661	0.2484
Birth weight (grams)	3,531.4617	578.3909
Low birth weight (birth weight $<$ 2,500 grams, 0/1)	0.0363	0.1870
Very low birth weight (birth weight $<$ 1,500 grams, 0/1)	0.0066	0.0811
Premature (born before gestational week 37, 0/1)	0.0495	0.2168
Number of days hospitalized in first year of life	1.9304	7.5242
Hospitalized in first year of life (0/1)	0.3874	0.4872
Number of ER admissions in first year of life	0.0912	0.3471
ER admission in first year of life (0/1)	0.0774	0.2673

Number of women = 11,402,379, number of births = 752,228 (748,627 with non-missing information on birth weight), number of women giving birth = 479,349.

Table A-9: Comparison of Child-Related Subsidies in the US and Denmark

Policy	United States	Denmark
Child Cash Subsidies	Child Tax Credit: $1,000 for ages 0-17 (phases out at incomes above $110,000; non-refundable) EITC: Max credit increases by $2,942 with birth of first and by $2,255 with birth of second child. One-child families earning over $40,320 and two-child families earning over $45,802 are ineligible.	Family Allowance: $2,704 for ages 0-2 $2,140 for ages 3-6 $1,685 for ages 7-14 $562 for ages 15-17 Benefit is reduced by 2% of the amount if either partner's income is in excess of DKK 765,800 ($114,870).
Child Care	$3,000 child care credit (non-refundable) for one child or $6,000 for two children under the age of 13. Daycare costs for babies and toddlers average $972/month and for pre-schoolers $733 per month (NACCRRA 2015).	Children are guaranteed a place in a day care facility. The government pays 75% of the cost and families pay 25% out of pocket. Higher subsidies for families earning under $80,970 and if there are multiple siblings. Prices after subsidy range across Denmark, but typically fall between $400 and $500 per month.
Parental Leave	12 weeks of unpaid leave if you work for a company with 50+ employees. Many states and companies have paid leave policies.	A total of 52 weeks of paid leave between mother and father. The final 32 weeks can be split between the mother and the father. Maximum weekly benefit of $645.
Health Insurance	Near-universal coverage for children because of Medicaid and SCHIP. Premiums increase with birth of first child only.	Universal health insurance coverage with no premiums or co-pays.

Child tax credits in the US use rules from prior to the 2018 TCJA that increased the child tax credit and the phase-out cutoff. The US-based fertility estimates are prior to the TCJA. Details on Danish benefits can be found at: https://ec.europa.eu/social/BlobServlet?docId=13746&langId=en. When possible, 2018 program details are used. Danish program parameters are converted into US Dollars using a 0.15 exchange rate. NACCRRA report can be found here: http://usa.childcareaware.org/wp-content/uploads/2016/05/Parents-and-the-High-Cost-of-Child-Care-2015-FINAL.pdf.

Table A-10: The Relationship Between Expenditures and the Presence of a Young Child in the Household

Panel A: Homeowners				
Expenditure Category	Expenditure (Dollars) (1)	Log Expenditure (2)	Mean (Dollars) (3)	Percent Effect (Column 1) (4)
Total	436.27	0.060**	8734.45	5.00%
	(282.83)	(0.025)		
Food	-81.79*	-0.018	1234.88	-6.62%
	(44.04)	(0.028)		
Housing	398.51**	0.101**	2994.65	13.31%
	(158.95)	(0.033)		
Health	161.04***	0.375**	876.61	18.37%
	(39.67)	(0.055)		
Education	-81.48***	-0.673**	140.55	-57.97%
	(21.25)	(0.227)		
Transportation	50.43	0.062	1318.44	3.82%
	(111.24)	(0.044)		
Other	-10.43	0.013	2169.32	-0.48%
	(138.50)	(0.033)		

Panel B: Renters				
Expenditure Category	Expenditure (Dollars) (1)	Log Expenditure (2)	Mean (Dollars) (3)	Percent Effect (Column 1) (4)
Total	***-814.30****	***-0.037***	1816.45	-44.83%
	(171.04)	***(0.032)***		
Food	-113.67**	0.013	299.46	-37.96%
	(29.67)	(0.037)		
Housing	***-199.96*****	***-0.015***	648.63	-30.83%
	(59.62)	***(0.043)***		
Health	***-42.57*****	***0.009***	153.54	-27.73%
	(23.04)	***(0.104)***		
Education	-114.10***	-0.398	37.14	-307.21%
	(33.54)	(0.271)		
Transportation	-99.05**	0.001	276.74	-35.79%
	(40.47)	(0.062)		
Other	-244.95**	-0.06	400.94	-61.09%
	(57.34)	(0.045)		

Source: Authors calculations using the 2015-2018 Consumer Expenditure Surveys. The estimates come from an OLS regression of the given expenditure category on the number of kids under 2, the number of kids 2-16, total family income, age and education fixed effects for both adults in the household, Census Division fixed effects, and quarter and year fixed effects. Estimation sample consists of all household with exactly two adults and household with two adults and at least one child under the age of two. Bolded and italicized estimates are statistically different at the 5% level across the renter and homeowner samples. Standard errors in parentheses: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.