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 child health in Denmark. Using rich population register data among women aged 20-44 who own a home, we find that for each 100,000 DKK increase in home prices (equivalent to $12,000), the likelihood of giving birth increases by 0.27 percentage points or 2.35%. 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 child health at birth in terms of low birth weight and prematurity, however most 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 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

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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 the single most important component of wealth for all but highest-resource households (Saez and Zucman 2016). As such, understanding how this volatility affects household decisions and outcomes is of high importance. In this paper, we examine the effects of home prices on fertility and child health using rich register data from Denmark for the period 1992 to 2011. 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. Investigating effects on child health is equally important given the large body of research linking early-childhood health to long-term socio-economic outcomes (Currie 2009; Currie and Rossin-Slater 2015).

While several prior studies have examined the effect of housing wealth on fertility, none has studied a country like Denmark that has an extensive social welfare system and none has estimated effects on child health. Denmark has one of the most generous social safety nets in the world and has a wide range of strongly pro-natalist policies, such as 52 weeks of combined parental leave, sizable cash payments to families with children, heavily subsidized child care, and free and universal health care. Comparing the housing wealth effect on fertility across settings allows us to provide new insight into the underlying mechanisms driving the fertility response. These mechanisms have not been a focus of prior research, and they are important to articulate in order to understand the interplay between government policy and how households respond to home price variation. Effects on child health are important for understanding the social costs and benefits of home price variation, and they also provide information about the mechanisms through which housing wealth affects household behaviors.

Combining insights from former studies on determinants of fertility as well as the housing finance literature, we present a conceptual framework that highlights that if housing is similarly-liquid across countries and if credit constraints do not bind, home price changes affect fertility and infant health only through an income effect and thus reflect household preferences. In the presence of credit constraints, in contrast, there should be larger fertility and childhood health responses in countries with less generous child-related government supports (i.e., a higher net price of children). Hence, our theoretical framework provides a more systematic way to interpret variation in housing wealth effects across countries that allows us to provide insight into *why* home price effects do or do not vary across countries and the roles played by government policies, household preferences, credit constraints, and the structure of the housing finance market.

Denmark also is an informative setting in which to examine the effects of housing market variation on households because of the existence of rich register data. These data allow us to probe the validity of the approach used by prior research by controlling for a larger array of fixed effects than previously has been possible because of data constraints and small sample sizes. Register data further permits a thorough analysis of how short-run home price changes impact health outcomes of babies and young children. We present the first estimates in the literature on how home price variation affects 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.

Investigating the effects of housing wealth on fertility and child health is complicated by the endogenous choice of home ownership, fertility and child investment decisions. In order to account for such endogeneity, we follow Lovenheim and Mumford (2013) and relate short-run home price changes to fertility and child health outcomes.[1] In particular, we focus on women who were not home owners at the beginning of 1992-1993 and who became a homeowner during the period 1993 to 2004 when they were between the ages of 18 and 42.[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.[3]

Our findings suggest that there is a positive effect of home price increases on fertility: a 100,000 DKK (approximately $12,000 in 2006, adjusting for purchasing power parity) increase

[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.

[3]Due to the use of a once-lagged one-year change independent variable, this leads to an age range of 20 to 44 years when we investigate fertility effects.

in home prices in the prior year increases the likelihood of giving birth by 0.27 percentage points, or 2.35% relative to the mean. Effects are similar across age groups and are largest among first-time mothers. Using the age-specific pattern of effects, we calculate that each 100,000 DKK increase in home prices leads to an increase of 0.057 in lifetime fertility. 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 education. These results are in line with former studies using micro data from the United States, Australia, and Japan (Lovenheim and Mumford 2013; Mizutani 2015; Atalay, Li, and Whelan 2017).[4] Our paper is methodologically closest to Lovenheim and Mumford (2013), who use a similar empirical strategy and data from a similar period from the US. They find an effect of 2.11% for each $12,000 of home price increase, which is very close to the 2.35% effect we document in Denmark. The heterogeneous treatment effects in Denmark also broadly align with the findings from Lovenheim and Mumford (2013) in the US.

The similarity of fertility responses between Denmark and countries with less generous welfare systems such as the US is surprising. Our theoretical discussion indicates that if households are not credit constrained and if households in the two countries have similar preferences for children, then we would expect housing wealth effects to be similar across the two settings. We therefore turn to the extent of credit constraints. We first present evidence using US data from the Consumer Expenditure Survey (CEX) that suggests US homeowners are 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. We provide suggestive evidence to support this assertion in Denmark using the consumption imputation method from Danish income and wealth flows developed by Browning and Leth-Peterson (2003). 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 households have similar fertility

[4]These estimates range from a 1.28% to 2.11% fertility increase per $12,000 of housing wealth. Dettling and Kearney (2014) employ a similar strategy to these micro-data studies using city-by-year aggregate data from the US for the period 1990 to 2007 and find an effect of 6.0% per $12,000 of home price increase.

responses to home price changes because they have similar preferences.

In a setting with credit access and very low cost health care in terms of out-of-pocket expenditures, we also expect there to be little effect on health. We turn to this question in the second part of the paper. Our results suggest that home price increases lead to small positive effects on health at birth. There is a statistically significant reduction in the likelihood of being born premature of 3.95% per 100,000 DKK of home price increase and a non-statistically significant reduction in low-birth weight of 0.97%. However, we show that home price increases lead more-advantaged women to give birth, and these composition effects can explain most of the health impacts we document. We do not find any evidence that health in the first five years of life is affected by home price variation. Taken together, these results indicate that any health benefits of home price increases at birth are modest and do not translate into better health in the longer run.

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 using plausibly-exogenous changes to family resources has highlighted that these negative cross-sectional correlations are not causal. 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] We contribute to this body of work by

[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).

presenting new estimates using rich register data from a high-income country with strong social welfare programs.

Second, we contribute to the growing body of research examining household responses to home price variation. Previous studies show that housing wealth 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). Most relevant for our paper are the studies linking housing wealth to fertility (Lovenheim and Mumford 2013, Dettling and Kearney 2014, Mizutani 2015, Atalay, Li, and Whelan 2017). We contribute to this prior work in three ways. First, we show that the fertility responses documented in previous studies extend to a country with a very generous social safety net. Second, we probe the sensitivity of the results to an expanded set of fixed effects and shed light on the validity of prior research. Finally, we present the first analysis of how housing market variation affects child health outcomes.

Third, we provide a conceptual framework that allows us to interpret *why* home price effects on fertility do or do not vary across settings. This framework allows us to perform a cross-country comparison that provides new insight into the mechanisms that underlie the home price effects the prior literature and we document. In particular, we highlight the role of credit constraints, household preferences, the liquidity of housing wealth, and government policies in understanding why home prices have similar effects on fertility across countries. Our analysis hence allows us to reconcile many of the different findings in the literature.

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 Appendix Table 7). 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 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.[7] 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.[8] 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

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

[8] 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

In this section, we discuss the theoretical foundations for how housing wealth affects fertility and child health. We then use this framework as a basis for understanding why these effects may be similar or different across countries. A long line of literature in economics has examined the determinants of fertility. As discussed in Dettling and Kearney (2014), children can be seen as consumption goods. The decision to have a child and the investments in children will be a function of the structure of preferences for children and other goods, prices for child-related goods and services, prices for non-child goods and services, total household resources, and the opportunity cost of time. Changes in housing wealth affect total household resources because housing is a large component of overall household savings portfolios and because housing is a relatively liquid asset (Mian and Sufi 2011). Because variation in housing wealth does not affect the opportunity cost of time for raising children, we abstract from labor market effects on fertility in this discussion and in our analysis.

Housing wealth can affect fertility through two main mechanisms: a home equity effect and a price effect (Dettling and Kearney 2014). The price effect is driven by the fact that housing is one of the main inputs to the cost of raising children. Hence, an increase in housing prices should lead to a reduction in fertility if the price effect dominates. The home equity effect refers to the fact that one's home is a large store of wealth that is increasingly liquid. People can use this wealth to fund current consumption, including children, thus generating an income effect. The extent to which housing wealth is used to fund consumption has garnered much attention in the literature (e.g., Campbell and Cocco 2007; Browing, Gørtz, and Leth-Peteren 2013; Aladangady 2017; Berger et al. 2018). Berger et al. (2018) provide a simple "rule-of-thumb" formula for characterizing how home price changes affect consumption: the consumption effect is equal to the marginal propensity to consume (MPC) times the home price. We use this formula as a basis to understand variation in home price effects across countries below. For several reasons, it is not obvious that the MPC out of home prices is positive. First, when home prices rise, the cost of living rises proportionally, and so real wealth does not change (Sinai and Souleles 2005). Second, home price changes may be transitory rather than permanent and thus may not increase long-run household resources. However, even a transitory shock that does not increase real household wealth can lead to an increase in current consumption if households are credit constrained.

As this discussion illustrates, the effect of housing wealth on fertility is theoretically ambiguous both because of confounding variation from the price effect as well as because of the possibility that the MPC is zero. That prior research has found extensive evidence of a positive consumption effect from home price variation, including the positive fertility estimates discussed above, suggests that the equity effect dominates the price effect and that the MPC out of housing wealth is positive.

A main focus of this paper is on understanding why home price effects are similar across two different contexts: Denmark and the US. The Berger et al. (2018) rule-of-thumb formula provides a framework for understanding these cross-country differences. Because home prices enter into this formula, it is important to compare the effect of similar changes in home prices across countries. Countries differ in the net price of having children as well as in the prices of other goods and services. As a result, the same nominal wealth change will translate into different real wealth differences across countries. We thus will consider the effects of real housing wealth changes by scaling the estimated effects by a purchasing power parity exchange rate.

Differences across countries in the effect of a real wealth shock are driven by differences in the marginal propensity to consume. In turn, variation in the MPC is determined by the structure of household preferences, the extent to which liquidity constraints bind, the liquidity of housing, and the transitory versus permanent nature of the shock. Among these mechanisms, the role of credit constraints is particularly important. If households face credit constraints, then increases in housing wealth can relax those constraints, allowing the households to more easily smooth consumption and investments surrounding the birth of a child. Hence, housing wealth effects on fertility will be muted among unconstrained relative to constrained households, as the MPC out of housing wealth will be higher for constrained households.

The price of having children (net of government subsidies) is a main determinant of the extent to which households are credit constrained. If the net price of having a child is high, credit constraints are more likely to bind, which will increase the responsiveness of households to wealth changes. The effect of a *real* wealth increase on fertility will not be impacted by price levels for unconstrained households, however. Because government subsidies and family policies primarily act to alter the net price of childbearing and rearing, these polices will influence the effect of housing wealth variation on fertility only in the presence of credit constraints. This suggests that, all else equal, households living in countries with high net prices of having a child are more likely to face binding credit constraints and thus will have stronger fertility and child investment responses to changes in housing wealth.

In the specific case of the US-Denmark comparison, we document that the net price of having children is significantly different between the two countries. Appendix Table A-7 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.[9] 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.[10] Including the Earned Income Tax Credit (EITC) 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 counterparts. 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-

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

[10] This tax credit was non-refundable during the period of our analysis, which limits the ability of many households to claim the full credit.

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.

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 and raising a child in Denmark is far less expensive than having a child in the US in terms of net out-of-pocket expenditures. This institutional framework suggests that US households are more likely to face credit constraints because of the higher net price of having children, which should make those households respond more strongly than their Danish counterparts to housing wealth changes.

Responses to housing wealth variation across countries also could differ by the liquidity of the wealth itself. As the difficulty of extracting wealth to fund current consumption declines, the MPC should increase. Evidence from the extant literature suggests that housing wealth is similarly liquid in Denmark and the US. 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). These studies underscore that there are no consequential differences in the liquidity of housing wealth across Denmark and the US that would drive any differences in responses to housing wealth.

In a setting with relatively liquid housing wealth, the transitory versus permanent nature of a home price shock matters differentially depending on whether there are binding credit constraints. If housing wealth changes are transitory, then they will only affect fertility behavior if households are credit constrained. Furthermore, the relaxation of credit constraints should lead to a change in the timing of fertility rather than a change in lifetime fertility. In contrast, a permanent housing wealth increase should lead to an increase in lifetime fertility if children are normal goods. Below, we estimate lifetime fertility effects using the age pattern of fertility responses and compare lifetime fertility effects across countries. This provides evidence on the permanent nature of the home price variation we employ and how this varies across Denmark and the US.

Absent credit constraints and holding constant the liquidity of housing wealth across countries, it is much more difficult to make ex-ante predictions of how housing wealth should affect fertility. The reason for this ambiguity is that the size of any effect is based on preferences for children and other goods. To the extent any of these preferences differ, there will be different wealth effects across countries. Importantly, our theoretical discussion underscores that similar responses to housing wealth across countries reflect similar preferences for children if credit constraints do not bind, housing wealth is similarly liquid, and home price shocks raise lifetime wealth of the household.

A similar set of forces governs the relationship between housing wealth and child health. If child health is a normal good, housing wealth increases should lead to better health outcomes. Credit constraints will increase the magnitude of this relationship, as housing wealth can be used to relax such constraints. Because health care is heavily subsidized in most countries, including in Denmark, we would not expect a large effect of housing wealth changes on child health absent credit constraints.[11] Hence, the effect of housing wealth on health provides some additional insight into the extent of binding credit constraints faced by households.

3 Data

We use Danish register data from 1992 to 2011. The data include individual-level records with household linkages, allowing us to follow the universe of Danish households for almost two

[11] Even with universal health insurance and zero copayments, housing wealth could influence child health through changes in a range of choices such as nutrition, exercise, and risky behaviors. These effects should be larger if credit constraints limit the ability of households to invest in their children's health in these ways.

decades. Our outcome variables of interest concern fertility and early-life child health. We use two complementary data sets to define our outcome variables. The first is the *Birth Registry*, which includes all (hospital and home) births in Denmark as well as information on infant health at birth. We use these data to construct an indicator for giving birth and, for those who have live births, we construct separate indicators for having a low birth weight (birth weight below 2,500 grams), very low birth weight (birth weight below 1,500 grams), or premature (gestational age less than 37 completed weeks) baby. We also use birth weight as an outcome on its own. We supplement these with data on hospital admissions and ER visits from the *National Patient Registry*, which cover the universe of hospitalizations in public and private hospitals. Using the National Patient Register, we construct separate indicators for the child having hospital or ER admissions during the first year of life and during ages 1-5. Our outcomes also include the number of days hospitalized and number of ER visits during the first five years of life.[12]

Our main independent variable concerns short-term housing price changes. The housing data are obtained from *The State's Sales and Valuation Registry*, 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. Public valuations are used as the taxable value for almost all properties in Denmark. All privately owned properties are valued in uneven years and adjusted in even years, which yields estimated values in every year.[13] 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

[12]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.

[13]The public valuations occur in January of the prior year until the end of 2003. Afterwards, they occur in October of the prior year. Very few properties are exempt from public valuations, e.g., churches.

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:

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

We assign to each woman the value of HV_{it} from the first house we see her purchase in the data 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 in 2006 was approximately 0.17 USD per DKK. If we take into account differences in purchasing power, the exchange rate in 2006 becomes 0.12.[14] The latter exchange rate is what we use for international comparisons throughout this paper.

[14]Source: https://data.oecd.org/conversion/purchasing-power-parities-ppp.htm.

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.[15] 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.[16] We exclude homes with a negative public valuation, negative sales price, or multiple addresses at the same location. Finally, we omit homes that are sold for more than 300% or less than 40% of the public valuation.[17] Appendix Table A-1 shows the number of observations excluded due to each of these conditions.

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.[18] 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.[19] We also drop 8,574 observations with incomplete data on household income, home price, and home ownership. 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 1,133,795 observations on 204,507 women aged 20 to 44 who gave birth to a total of 133,450 children in the period 1995 through 2006. Panel B in Appendix Table A-1 shows how many observations are affected by each of these sample restrictions.

Table 1 presents the summary statistics for our analysis sample. The first group of variables are outcome variables. The fertility rate among homeowners is 11.49%, which is somewhat higher than the national fertility rate of 6.41% for the same age group (Appendix Table A-2).

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

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

[17] These restrictions exclude 7,434 observations.

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

[19] Due to the use of a once-lagged one-year change independent variable, this leads to an age range of 20 to 44 years when we investigate fertility effects. Around 98% of all births in Denmark are to women in this age group.

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 3,573 grams. Over 3% of these children have low birth weight, 0.51% have very low birth weight, and 4.81% are premature. The births in our sample are healthier on average than in the US, where over 8% of births are low birth weight, 1.4% are very low birth weight, and 9.85% are premature.[20] However, they are closely aligned with birth outcomes among the full sample of Danish births for women aged 20-44 (Appendix Table A-2). On average, children in the sample are hospitalized for 1.69 days during their first year of life, and 5.7% have an ER visit. 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 time span 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). In Figure 3, we present the evolution of the average one-year home price change by quartile of the home's value in the initial year. Price changes are sizable and stable across the home value distribution, and the housing boom of the early- to mid-2000s is clearly evident. Home prices vary across municipalities as well: Figure 4 shows the average home price change for each municipality in 2006. While home price increases tend to be larger around the urban areas, home price increases are not concentrated in one part of the country and are not localized to urban centers.

The final group of variables in Table 1 summarizes characteristics of homeowners. The

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

average homeowner is around 33 years old with 14 years of completed schooling, 88% are married or cohabiting with a partner, and 96% are employed. Compared to the full population of women aged 20 to 44, homeowners in Denmark tend to be older, more likely to be married or cohabiting and have higher socio-economic status (see Appendix Table A-2). However, demographic characteristics of homeowners in our sample are comparable to those in the US (Lovenheim and Mumford 2013).

4 Empirical Approach

Our empirical approach relates short-run home price changes to fertility and early-life child health outcomes.[21] Specifically, we estimate models of the following form:

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

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 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 model controls for a wide array of observed individual-year level characteristics that are available in the rich Danish register 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 key identification assumption in this model is that changes in home prices are unrelated to unobserved characteristics that also correlate with the likelihood of giving birth and with birth outcomes.[22] In order to assess the validity of this assumption, Table 2 shows average observed characteristics across the distribution of the lagged one-year home price change. We

[21] Our register data includes years 1992 to 2011. We use data from 1995-2006 to examine the effects on fertility. When investigating health outcomes within the first year and during ages 1-5, we expand the sample period accordingly.

[22] One may also be concerned that home price variation could lead to differences in tax revenue that in turn could affect the quality of health care available. In Denmark, hospital care (including emergency and psychiatric care), as well as health services provided by primary care physicians and specialists in private practice are the responsibility of five Danish regions. These services are financed through income taxes. Danish municipalities, on the other hand, are only responsible for local and elderly health services, such as rehabilitation outside hospital, home nursing, school health services, child dental treatment, physiotherapy, and alcohol and drug abuse treatment. As such, local property tax variation is unlikely to affect the health care services relevant for our setting.

examine real household income, years of education, number of children, whether the woman is unemployed and whether she has a partner. Home price changes vary considerably across the quartiles (by construction). While there also is some evidence that real household income and educational attainment increase slightly across quartiles, what matters for our identification strategy is whether they change in ways that predict our outcomes of interest. To see more directly how the observables vary with home price variation, we calculate predicted outcomes for each woman based on all observed characteristics. These are essentially summary measures of observed characteristics as they relate to our outcome variables. Table 2 shows means of each predicted outcome by home price change quartile. For each of the thirteen outcomes in the table, the predicted outcomes are remarkably similar across home price growth quartile. There is no discernible pattern across quartiles for any outcome, and for no outcome do we observe predicted changes that would generate spurious treatment effects.

While it is reassuring to see that average observable characteristics are unrelated to the changes in housing prices, there still can be some sources of bias remaining. The variation in housing prices 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. 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. In order to address this, we include municipality-by-year fixed effects (ϕ_{mt}) that 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 well as 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. Hence, there 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 addition of these two sets of fixed effects considerably reduces the possibility of a remaining bias. Once we add municipality-by-year fixed effects, the identification assumption is that *differences* in home price growth across houses in the same municipality (and year) 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 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 still 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.

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 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.

Finally, 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. Therefore, standard errors are clustered at the municipality level throughout the analysis, which handles both sources of error correlation.[23]

5 Home Prices and Fertility

5.1 Baseline Results

Table 3 presents the effect of short-run home price changes on fertility. 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 5% level, and it represents a 1.39% increase relative to the mean fertility rate of 0.1149 (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 include 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

[23]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.

(3) that a 100,000 DKK increase in home prices leads to a 0.37 percentage point increase in the likelihood of giving birth, which is a 3.22% 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 2.35% effect. Comparing columns (3) and (4), the observables exert very little influence on the estimate. As 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 Appendix Table A-3) is almost identical to the estimate in column (3) of Table 3.[24] To explore which observables matter most, we include them one-by-one in 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.[25] 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.

[24] We also estimate a model similar to that in column (2) of Appendix Table A-3 that includes women who purchase a home prior to 1993. The estimate of 0.0033(0.0005) is similar to baseline and suggests focusing on women who purchase a home in 1993 or after does not drive our results.

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

5.2 Robustness Checks and Heterogeneous Effects

In this section, we first investigate the robustness of the estimated effects on fertility along several dimensions. A core identification assumption embedded in our approach is that there are no contemporaneous, 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 a 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, (approximately 0.04% at the mean), and it is not statistically significant at conventional levels. Lack of evidence that renters respond to home price changes supports our identification strategy.[26]

In column (2) of Table 4, we include woman fixed effects in the model. We confirm that short-run housing price changes significantly increases fertility. This suggests that our baseline results are not driven by unobserved attributes of women 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 Column (6), we assess the sensitivity of our results to more carefully aligning the timing of home price valuation and births. In our baseline model, we link births in a given year to the house price change in the previous year. In doing so, we ignore the fact that some conceptions occur before the parents observe the change in the home price. In order to

[26]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.

address this concern, we use the gestational length and birth date to calculate the conception date. We then scale the one-year home price change by the fraction of the year until the 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 distribution of birth months is the same across women who do and do not give birth. The resulting estimate, presented in column (6), is very similar to the baseline result.[27]

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, particularly in percent terms. As discussed in Section 2, a core question is whether our results reflect changes in the timing of births or total lifetime fertility. That each age-specific estimate is positive points to home price increases raising lifetime fertility. Following Lovenheim and Mumford (2013), we calculate the total lifetime fertility effect by multiplying each age-specific coefficient by 5 and then adding them together. This calculation yields an estimate of 0.057, which is similar in size to the effect Lovenheim and Mumford (2013) calculate for these age groups. These results have two important implications. The first is that the home price shocks households experience have a permanent component. The second is that the lifetime fertility effect is similar in the US and Denmark, which suggests the persistence of home price shocks in the two countries also are similar.

Panel B of Table 5 shows effects by parity. The effect of home price on fertility is largest for first-time mothers, at 0.57 percentage points per 100,000 DKK increase in home price, or 3.93% relative to the mean. Statistically significant and sizable impacts also are evident for families with one, two, and 3+ existing children. The effect size for families with 3+ existing children is quite large, at over 3%.

In Panels C and D, we investigate treatment heterogeneity by the socioeconomic status of the household. Panel C presents results separately by family income quartile.[28] The point estimates suggest that women in the middle of the income distribution are most responsive to home price

[27] We also have explored the robustness of the results to using log home prices. We favor the level model, as we argue that the income effect should be proportional to the value of the home increase rather than the percentage change. Our results are robust to using log home prices, indicating an effect on fertility of 1.82% per 100,000 DKK of home value increase.

[28] Income quartiles are defined over the full sample period.

changes in their fertility decisions. In Panel D, 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.

Finally, we study whether households respond differently to home price increases and decreases. In order to do so, we include in equation (2) an interaction term between home price change and an indicator for a price decline (there are no observations with a home price change of exactly zero). We obtain estimates of a 100,000 DKK increase in home prices on fertility of 0.0038(0.0006) and of a 100,000 DKK decrease in home prices on fertility of -0.0038(0.0017). The effects are equal and opposite in magnitude, suggesting that home price effects on fertility are symmetric.

5.3 Interpretation and Discussion

In this section, we compare the magnitude of our fertility result to those in the previous literature. Using data from 1985 to 2007, Lovenheim and Mumford (2013) find that a $12,000 increase in home value leads to a 2.11% higher fertility rate in the US. Estimates from a similar model in Australia using micro data from 2001-2014 show a fertility effect of 1.36% per $12,000 (Atalay, Li, and Whelan 2017), while results from Japan during the housing market downturn of the early 1990s indicates a fertility effect of 2.8% per $12,000 among those with home loans (Mizutani 2015). There is no effect among those without home loans, such that the weighted average effect is a 1.28% increase in fertility per $12,000 of lagged home price increase.[29]

Our preferred estimate indicates that fertility increases by 2.35% for each 100,000 DKK increase in home prices, which is $12,00 taking into account purchasing power differences. Put on a monetary scale, our results are in line with those found in previous studies and virtually identical to that of Lovenheim and Mumford (2013), who use data from a similar period.[30] Additionally, the lifetime fertility effect and the pattern of heterogeneous treatment effects across mother characteristics documented in Table 5 broadly align with the findings from

[29]Two studies find larger fertility effects. Using city-by-year aggregate data for the period 1990 to 2007, Dettling and Kearney (2014) find an effect of 6.0% per $12,000 of home price increase. Clark and Ferrer (2019) examine lagged home price levels rather than home price changes and document a fertility effect of 9.6% for each $12,000 increase in lagged home prices. That home price levels are more strongly correlated with household unobservables than are changes is likely the reason why their estimates are so large.

[30]Lovenheim and Mumford (2013) focus on two-year and four-year changes in home prices rather than the one-year change 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 and four years leads to a 1.79% and 1.64% increase in fertility, respectively.

Lovenheim and Mumford (2013) in the US.[31]

The similarity of the fertility responses in Denmark and in the US to housing wealth changes is a surprising finding given the strong pro-natalist policies of Denmark. While it is undeniable that the net costs of children are higher in the US than in Denmark, our discussion in Section 2 highlights that costs would be an important mechanism behind fertility responses only if households are credit constrained. We argue that housing wealth is similarly-liquid and the persistence of home price variation as indicated by lifetime fertility effects are similar in the two countries. These similarities highlight the potentially important roles played by credit constraints and household preferences in understanding why the effects are similar in the US and Denmark. In particular, if households are not credit constrained and if households in the two countries have similar preferences for children, then we would expect housing wealth effects to be similar across the two settings.

In order to shed some light on the reasons for the similarity of fertility responses, we bring suggestive evidence on the extent of credit constraints surrounding child birth. We first examine expenditure differences across observationally-similar households with and without a child under the age of two using the 2015-2018 Consumer Expenditure Surveys in the US. We take all two-adult households 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 Appendix Table A-5. While the assumptions underlying a causal interpretation of these estimates are strong, they provide suggestive evidence of how expenditures among 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 (possibly because new parents eat out less). Critically,

[31] In contrast to the similarity of the effects per dollar, comparing effects per standard deviation leads to large differences because 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 104,000 DKK ($12,480) using one-year changes and 152,000 DKK ($18,240) using two-year home price changes (the lagged two-year home value change has a mean of 1.2076 with a standard deviation of 1.5197). As shown in Table 4, for a standard deviation increase in 2-year home price change, we find an effect of 2.7% (152,000*1.79/100,000). In the US, a standard deviation in two-year home price changes is $73,130 (Lovenheim and Mumford 2013). Thus, a standard deviation increase in housing prices result in a fertility effect of 12.8% (73,130*2.64/15,000), which is a sizable difference.

"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 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.

It is difficult to produce similar estimates for Denmark, as there are no consumption measures in the register data. As a means to compare the consumption effects of birth across Danish and US homeowners, we use the consumption imputation method developed by Browning and Leth-Peterson (2003). They show that imputing consumption using income and year-to-year wealth changes matches reported consumption in a sample of linked consumption data. Using their method for imputing household consumption and applying this measure to the same model used with the CEX survey data discussed above, we find that an additional child under the age of two reduces consumption by a non-statistically-significant 467 DKK. This is a little more than a third of a percent reduction in consumption. Similar to US homeowners, Danish homeowners change their consumption little when they have young children. These estimates suggest that both Danish and US homeowners have access to sufficient credit to smooth consumption surrounding childbirth.[32]

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 effects 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 similarly to home price changes in their fertility decisions across the two countries is useful for uncovering fertility preferences, which would be difficult to do without this comparison.

[32]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.

6 Home Prices and Early-Life Child Health

We now turn to the first examination in the literature of how child health is affected by home prices. There are two mechanisms that would generate a relationship between home prices and child health. The first is that households may use the additional resources to invest in child health. As discussed in Section 2, we would expect this effect to be larger if credit constraints bind, which is less likely in Denmark given the heavy subsidization of health care. Furthermore, the lagged home price increases we examine lead to changes in household resources during the in-utero period. This is a particularly important period in child development, and there is extensive evidence that in-utero conditions have long-run developmental implications (Almond and Currie 2011; Almond, Currie, and Duque 2018). The second mechanism is driven by the fertility effect documented above: the composition of births may change in a way that would alter the aggregate health capital of children. In this section, we first estimate home price effects on child health outcomes and then provide evidence on these underlying mechanisms.

Table 6 presents estimates of how housing wealth affects early-life child health outcomes. Panel A shows estimates of birth outcomes: birth weight (in grams), and indicators for low birth weight (birth weight $<$ 2,500 grams), very low birth weight (birth weight $<$ 1,500 grams), and prematurity (born before gestational week 37).[33] 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 0.03 percentage point reduction in the likelihood of low birth weight and a 0.19 percentage point reduction in the likelihood of being premature. These are both modest effects (0.97% and 3.95% 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 3.08%, so detecting an effect on this outcome is difficult. The estimate on birth weight in Table 6 is positive but not statistically significant, nor is it economically significant at 0.08 percent of the mean. Hence, in the remainder of the paper, we focus on the likelihood of prematurity and low birth weight.

Appendix Table A-6 shows the effects of home price changes on low birth weight and prematurity by mother's age, parity, income and education. The estimates are imprecise, but they

[33] We also have examined macrosomia, which is defined as having a birthweight over 4,000 grams. The mean of this variable is 0.2110 and its standard deviation is 0.4080. We estimate an effect of 0.0008(0.0016), or 0.38%, on this outcome.

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 2 or more children. While only some of the estimates are 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.

Housing price changes can impact infant health outcomes both by changing the composition of births and by changing health outcomes among inframarginal births. We conduct two types of analyses to shed light on the importance of these channels. First, we estimate regressions in the spirit of Dehejia and Lleras-Muney (2004) to provide direct evidence on compositional shifts. In particular, among those who give birth, we regress observed characteristics on lagged home price changes as well as the full set of fixed effects and find that home price increases cause shifts in composition of births towards older, more educated, higher income, and two-parent households. These results are presented in Table 7. The last two columns of the table uses predicted prematurity and low birth-weight to assess to overall implications of these compositional changes for our infant health estimates. The estimates are negative, of similar magnitude to the treatment effects in Table 6, and are statistically significant, confirming that at least some of the health effects we find are driven by changes in composition.[34] One way to account for endogenous changes in the composition of mothers is to include mother fixed effects. When we estimate models that include mother fixed effects, we find percent effects that are roughly half of those found for families with at least 2 children (see Panel B of Table A-6). The sensitivity of the estimates of the inclusion of mother fixed effects as well as the reduction in magnitudes of the effects suggest that the baseline results likely reflect selection rather a causal effect of housing wealth on infant health.

Second, we investigate effects of housing price increases prior to birth on short-run and medium-run health outcomes. Panel B of Table 6 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

[34]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.

ER visit. While these are rather extreme health outcomes, a large portion of the sample (35%) has been hospitalized in the first year of life, and ER visits are not that uncommon. However, we find no statistically significant evidence that health changes in the first year of life. 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 rule out a decline of more than 5% in column (1) and a decline larger than 1% in column (2) at the 95% level. The point estimates in columns (3) and (4) for ER visits are small in size and also are precisely estimated. In Panel C, we estimate effects of home price increases on the same health outcomes in years 1-5 of life. The effects are again small, precisely estimated, and statistically insignificant.

Taken together, our results suggest that home price increases lead to somewhat healthier births largely due to a change in the composition of births, and they 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 variation. This finding is consistent with our argument that homeowner households are not credit constrained. If the effect of housing wealth on fertility simply reflects an income effect, we would not expect much of an effect on child health because families are already optimizing health investments and because health care in Denmark is so heavily subsidized. 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.

7 Conclusion

This paper examines the effects of short-run housing price variation on fertility and early-life child health. Using administrative data from Denmark, we first show that short-run home price increases lead to significant increases in fertility. The magnitudes of our estimates are in line with those established in prior literature and are virtually identical to those found by Lovenheim and Mumford (2013) using a similar strategy in the US for a similar period. Our analyses also reveal a similar pattern of heterogeneity as in the US. The similarity of the fertility responses in Denmark and the US suggests that 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 that the similarity of effects across the US and Denmark reflect a lack of credit constraints among homeowners combined with similar preferences for children across the two countries.

In the second part of the paper, 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. However, most of this effect reflects changes in the composition of births. Overall, we do not find any evidence that housing wealth has a causal impact on early life health. This finding generally supports our argument that homeowner households do not face credit constraints with respect to the birth of a child; the heavily subsidized nature of health care in Denmark renders any income effect quite small in the absence of significant credit constraints. The effect of housing wealth on the health of young children in a setting without such high levels of government subsidy, such as the US, may be much larger. We view this as an important area for future research.

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Table 1: Summary Statistics

	Mean	S.D.
Outcome variables:		
Birth (0/1)	0.1149	0.3190
Birth weight (grams)	3,572.7667	566.7671
Low birth weight (birth weight < 2,500 grams, 0/1)	0.0308	0.1728
Very low birth weight (birth weight < 1,500 grams, 0/1)	0.0051	0.0713
Premature (born before gestational week 37, 0/1)	0.0481	0.2140
Number of days hospitalized in first year of life	1.6933	7.0657
Hospitalized in first year of life (0/1)	0.3498	0.4769
Number of ER admissions in first year of life	0.0645	0.2794
ER admission in first year of life (0/1)	0.0574	0.2327
Number of days hospitalized ages 1 to 5	1.1588	5.0371
Hospitalized ages 1 to 5 (0/1)	0.3803	0.4855
Number of ER admissions ages 1 to 5	0.4332	0.8751
ER admission ages 1 to 5 (0/1)	0.2759	0.4470
Housing variables:		
Lagged one-year home value change (100,000 DKK)	0.7068	1.0398
Purchase sum at time of purchase (100,000 DKK)[a]	9.9096	5.0041
Estimated home value at time of purchase (100,000 DKK)[a]	10.6108	4.9087
Control variables:		
Real household income	5.8105	3.3319
Years of education	14.0781	2.2338
Number of children	1.2776	1.0538
Partner (married and/or cohabiting, 0/1)	0.8781	0.3271
Unemployed (0/1)	0.0359	0.1860
Age	32.7237	5.4343
20-24 years old (0/1)	0.0593	0.2362
25-29 years old (0/1)	0.2444	0.4297
30-34 years old (0/1)	0.3286	0.4697
35-39 years old (0/1)	0.2359	0.4246
40-44 years old (0/1)	0.1318	0.3383

Number of observations = 1,133,795, number of women = 204,507, number of births = 133,450 (131,287 with non-missing information on birth weight). 100,000 DKK ≈ 12,000 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: Average Observed 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
	(1)	(2)	(3)	(4)
Housing variables:				
Lagged one-year home	-0.3732	0.2899	0.8184	2.0920
value change	(0.3390)	(0.1426)	(0.1741)	(0.9424)
Total adjusted housing	9.8903	8.8926	10.1531	13.5071
value in year of purchase	(4.3325)	(4.0385)	(4.1326)	(5.6703)
Purchase sum in	9.2383	8.1401	9.5670	12.6825
year of purchase	(4.5159)	(4.0492)	(4.1538)	(5.8969)
Control variables:				
Real household income	5.6478	5.3384	5.6367	6.6192
	(2.6909)	(2.5483)	(3.1949)	(4.4192)
Years of education	13.8696	13.7984	14.0365	14.6081
	(2.1803)	(2.1783)	(2.1960)	(2.2877)
Number of children	1.3553	1.2025	1.2197	1.3329
	(1.0640)	(1.0628)	(1.0472)	(1.0322)
Unemployed	0.0397	0.0407	0.0355	0.0275
	(0.1953)	(0.1976)	(0.1851)	(0.1635)
Partner	0.9017	0.8600	0.8627	0.8882
	(0.2977)	(0.3470)	(0.3441)	(0.3151)
Predicted outcomes at birth:				
Predicted fertility	0.1131	0.1169	0.1175	0.1123
	(0.0740)	(0.0731)	(0.0729)	(0.0740)
Predicted birth weight	3,549.9740	3,542.4501	3,545.1164	3,555.6703
	(96.3152)	(95.9874)	(95.4343)	(96.4703)
Predicted low birth weight	0.0397	0.0405	0.0403	0.0392
	(0.0153)	(0.0152)	(0.0152)	(0.0157)
Predicted very low birth weight	0.0065	0.0067	0.0066	0.0064
	(0.0028)	(0.0028)	(0.0028)	(0.0029)
Predicted premature	0.0572	0.0582	0.0580	0.0570
	(0.0153)	(0.0152)	(0.0151)	(0.0154)
Predicted outcomes in the first year:				
Predicted # days hospitalized	4.3509	4.4276	4.4181	4.3411
	(1.0008)	(0.9929)	(0.9783)	(0.9879)
Predicted ever hospitalized	0.9842	0.9846	0.9845	0.9839
	(0.0062)	(0.0061)	(0.0061)	(0.0063)
Predicted # ER visits	0.0640	0.0655	0.0643	0.0619
	(0.0127)	(0.0135)	(0.0126)	(0.0114)
Predicted ever ER visits	0.0570	0.0583	0.0573	0.0555
	(0.0103)	(0.0108)	(0.0102)	(0.0095)
Predicted outcomes in years 1 to 5:				
Predicted # days hospitalized	1.1815	1.2080	1.1688	1.0860
	(0.2661)	(0.2844)	(0.2679)	(0.2452)
Predicted ever hospitalized	0.3835	0.3884	0.3812	0.3660
	(0.0499)	(0.0530)	(0.0502)	(0.0462)
Predicted # ER visits	0.4372	0.4443	0.4334	0.4116
	(0.0763)	(0.0813)	(0.0763)	(0.0690)
Predicted ever ER visits	0.2778	0.2810	0.2760	0.2656
	(0.0361)	(0.0383)	(0.0362)	(0.0330)

The tabulations are raw means and are not residual to any fixed effects or controls. Standard deviations in parentheses. Predicted outcomes are obtained from the main specification estimated without lagged one-year home value change or fixed effects in the regression, with standard errors in parentheses.

Table 3: Baseline Results – Fertility

	(1)	(2)	(3)	(4)
Lagged one-year	0.0016**	0.0007	0.0037***	0.0027***
home value change	(0.0008)	(0.0006)	(0.0009)	(0.0006)
Real household				0.0004*
income				(0.0002)
% Effect	1.39	0.61	3.22	2.35
R^2	0.002	0.005	0.035	0.059
Year FE	X			
Municipality FE	X			
Year×mun FE		X	X	X
Age×year of FP FE			X	X
Controls				X

Column (4) includes 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 = 0.1149; Number of observations = 1,133,795. 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 – Fertility

	Renters	Woman FE	Lagged 2-Year Change	Lagged 3-Year Change	Lagged 4-Year Change	Align Valuation & Conception
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged home	0.0000	0.0018***	0.0019***	0.0019***	0.0015***	0.0023***
value change	(0.0006)	(0.0006)	(0.0005)	(0.0003)	(0.0002)	(0.0007)
Real household	0.0018***	-0.0010***	0.0003	0.0002	0.0000	0.0004***
income	(0.0004)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
% Effect	0.04	1.58	1.79	1.90	1.64	2.00
R^2	0.0476	0.3587	0.0585	0.0581	0.0584	0.0656
Observations	3,900,644	1,109,961	926,268	747,126	593,834	1,133,795

The sample in column (1) consists of all women between 20 and 44 in years who were not a homeowner in the previous two years, the present year, and the subsequent year and who did not live in the same household as their parents. The one-year lagged home price change for renters is calculated 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. 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 and municipality-by-year fixed effects. 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: Heterogenous Effects – Fertility

	Panel A: Mother's Age					Panel B: Parity			
	20-24 years (1)	25-29 years (2)	30-34 years (3)	35-39 years (4)	40-44 years (5)	0 children (6)	1 child (7)	2 children (8)	3+ children (9)
Lagged one-year	0.0030	0.0027*	0.0029***	0.0020***	0.0007	0.0057***	0.0030**	0.0009*	0.0010*
home value change	(0.0019)	(0.0012)	(0.0010)	(0.0007)	(0.0005)	(0.0008)	(0.0013)	(0.0005)	(0.0006)
Real household	0.0010	-0.0004	0.0000	0.0002*	0.0001	0.0013**	0.0017***	-0.0002**	-0.0004***
income	(0.0007)	(0.0006)	(0.0003)	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0001)	(0.0001)
% Effect	2.66	1.58	1.95	2.96	4.64	3.93	1.46	1.85	3.56
Dep. Var. Mean	0.1124	0.1710	0.1482	0.0673	0.0147	0.1457	0.2075	0.0475	0.0278
R^2	0.0800	0.0391	0.0562	0.0437	0.0285	0.0555	0.0564	0.0238	0.0396
Observations	67,153	277,069	372,535	267,359	149,333	340,917	283,782	385,666	123,205

	Panel C: Mother's Income Quartile				Panel D: Mother's Education Quartile			
	Bottom Quartile (1)	2^{nd} Quartile (2)	3^{rd} Quartile (3)	4^{th} Quartile (4)	Bottom Quartile (5)	2^{nd} Quartile (6)	3^{rd} Quartile (7)	Top Quartile (8)
Lagged one-year	0.0015	0.0037***	0.0038***	0.0015**	0.0030***	0.0023***	0.0032***	0.0027***
home value change	(0.0012)	(0.0010)	(0.0009)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0009)
Real household	0.0056***	-0.0109***	-0.0089***	0.0002	0.0013***	-0.0004**	0.0007**	0.0002
income	(0.0012)	(0.0026)	(0.0017)	(0.0001)	(0.0003)	(0.0002)	(0.0003)	(0.0002)
% Effect	1.51	2.71	3.17	1.46	3.18	1.99	2.60	1.95
Dep. Var. Mean	0.0989	0.1367	0.1196	0.1046	0.0954	0.1158	0.1238	0.1366
R^2	0.0422	0.0701	0.0834	0.0875	0.0488	0.0684	0.0765	0.0909
Observations	283,429	283,430	283,373	283,305	360,960	355,716	177,298	239,548

Each column of each panel is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children (except 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 6: The Effect of Home Price Changes on Infant Health

Panel A: Health at Birth				
	Birth Weight (g)	Low Birth Weight	Very low Weight	Premature
	(1)	(2)	(3)	(4)
Lagged one-year	2.8002	-0.0003	0.0002	-0.0019**
home value change	(2.2186)	(0.0006)	(0.0003)	(0.0007)
Real household	1.2993*	-0.0005***	-0.0001***	-0.0004*
income	(0.7176)	(0.0002)	(0.0000)	(0.0002)
% Effect	0.08	-0.97	1.96	-3.95
Dep. Var. Mean	3,573	0.0308	0.0051	0.0481
Observations	125,308	125,308	125,308	127,274

Panel B: Short-Run Health (age 0-1)				
	Number of Days Hospitalized	Ever hospitalized	Number of ER Visits	Ever ER Visit
	(1)	(2)	(3)	(4)
Lagged one-year	-0.0062	-0.0007	-0.0001	0.0002
home value change	(0.0235)	(0.0015)	(0.0009)	(0.0008)
Real household	-0.0187***	-0.0017***	-0.0002	-0.0001
income	(0.0058)	(0.0004)	(0.0002)	(0.0002)
% Effect	-0.37	-0.20	-0.16	0.35
Dep. Var. Mean	1.6933	0.3498	0.0645	0.0577
Observations	127,274	127,274	127,274	127,274

Panel C: Medium-Run Health (age 1-5)				
	Number of Days Hospitalized	Ever hospitalized	Number of ER Visits	Ever ER Visit
	(1)	(2)	(3)	(4)
Lagged one-year	0.0141	0.0008	-0.0002	0.0007
home value change	(0.0151)	(0.0017)	(0.0029)	(0.0014)
Real household	-0.0037	-0.0008**	-0.0010	-0.0007*
income	(0.0034)	(0.0004)	(0.0008)	(0.0003)
% Effect	1.22	0.21	-0.05	0.25
Dep. Var. Mean	1.1588	0.3803	0.4332	0.2759
Observations	127,274	127,274	127,274	127,274

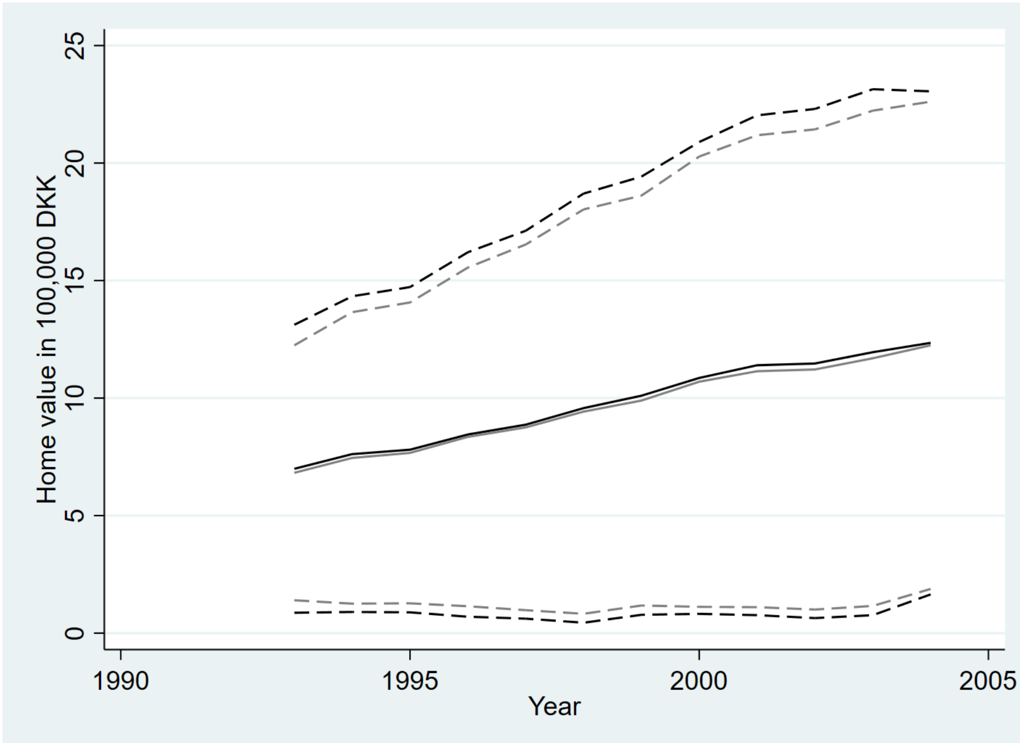
Each column of each panel is a separate regression. The controls include woman's age (age fixed effects), years of education, number of children, indicator for being married and/or cohabiting, indicator for being unemployed at least 6 months within a given year, and total real family income (for woman + 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. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Table 7: The Effect of Housing Prices on the Composition of Mothers Who Give Birth

	Age (1)	Years of Education (2)	Number of Children (3)	Household Income (4)	I(Partner) (5)	Predicted Prematurity (6)	Predicted Low BW (7)
Lagged one-year	0.4026***	0.0864***	0.0225***	0.3539***	0.0311***	-0.0008***	-0.0008***
home value change	(0.0560)	(0.0095)	(0.0045)	(0.0585)	(0.0076)	(0.0001)	(0.0001)

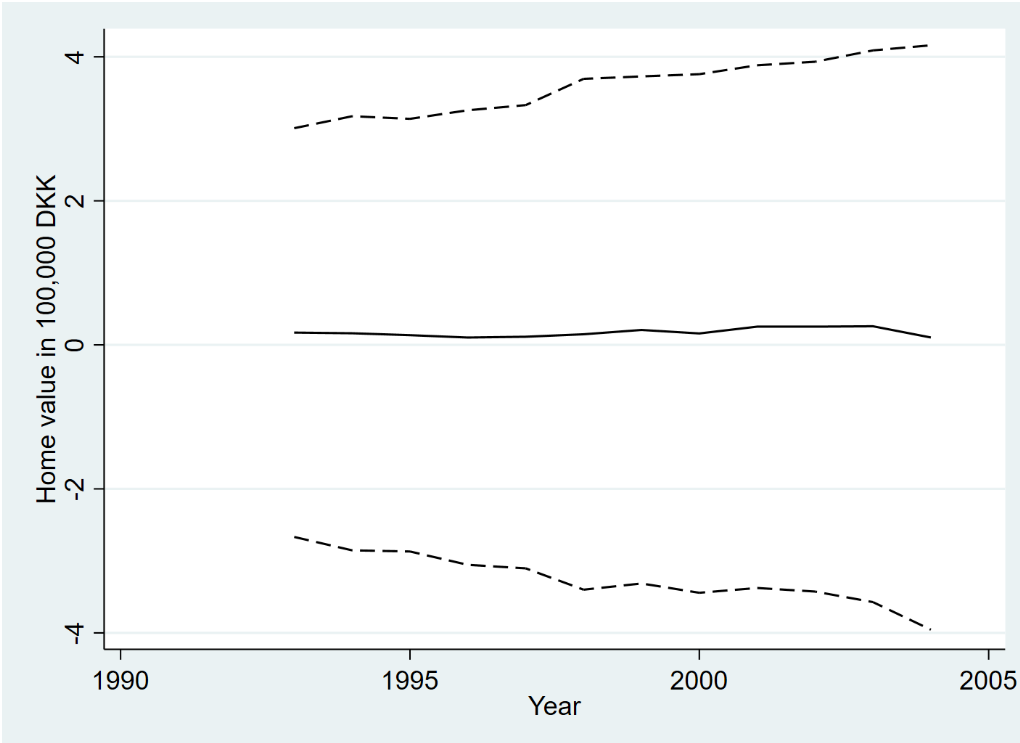
Each column is a separate regression and includes women from our main analysis sample who give birth. The controls include municipality-by-year and age-by-year-of-first-purchase fixed effects. Dependent variables are measured in the year of birth. Lagged one-year home value change is in 100,000 DKK. Multiple births are excluded. Predicted Prematurity and Predicted Low BW are predicted values from a regression of each dependent variable on age, years of education, number of children, household income, and partner status. Standard errors clustered at the municipality level in parentheses: significant at *10%, **5%, and ***1%.

Figure 1: Estimated Home Value versus Actual Purchase Price



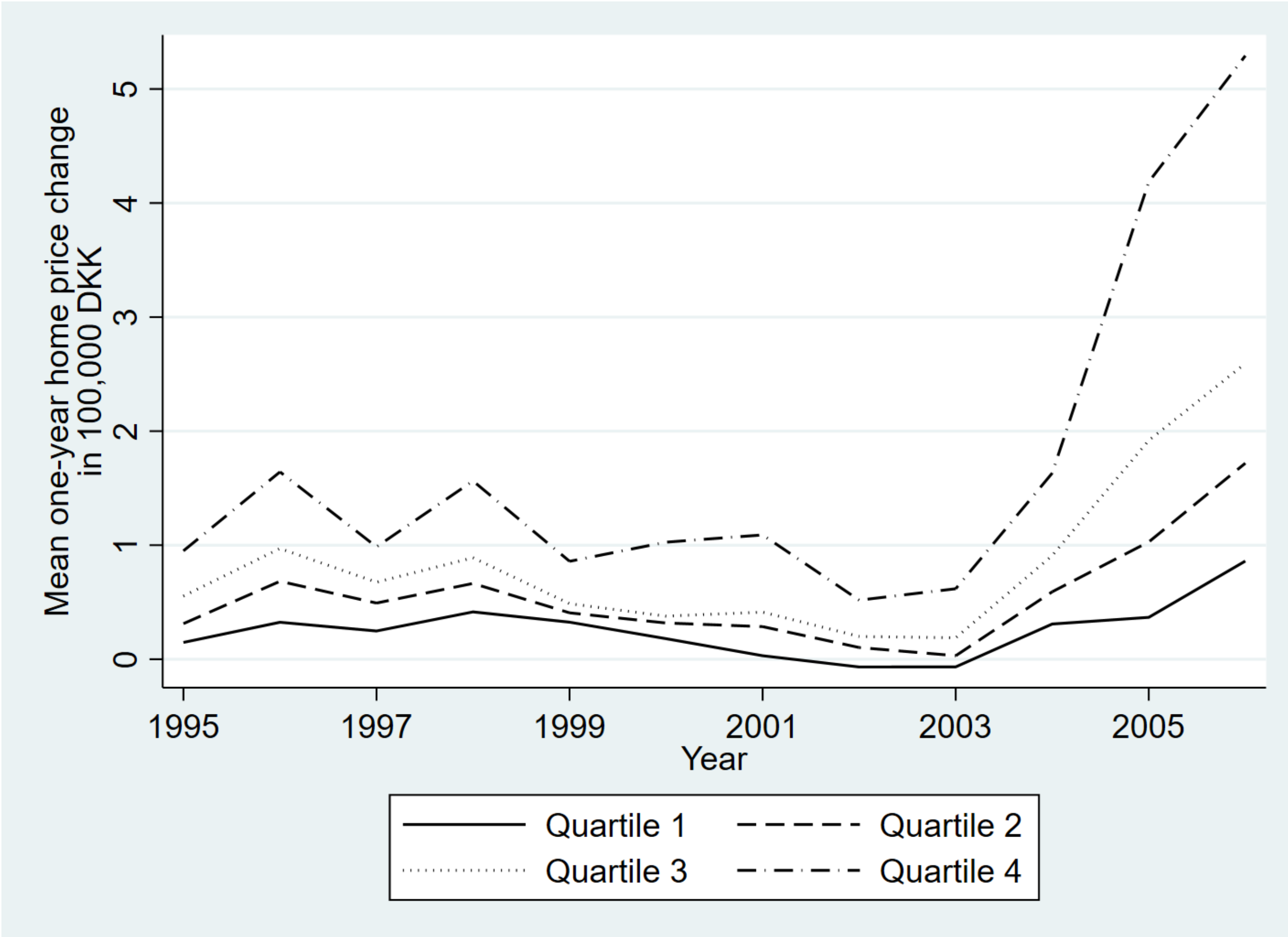
The solid black line illustrates mean actual purchase price in year of purchase. The solid gray line illustrates the mean estimated home value in the year of purchase for the homes actually sold. The black and gray dashed lines illustrate upper and lower bounds of the 95% confidence intervals associated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

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



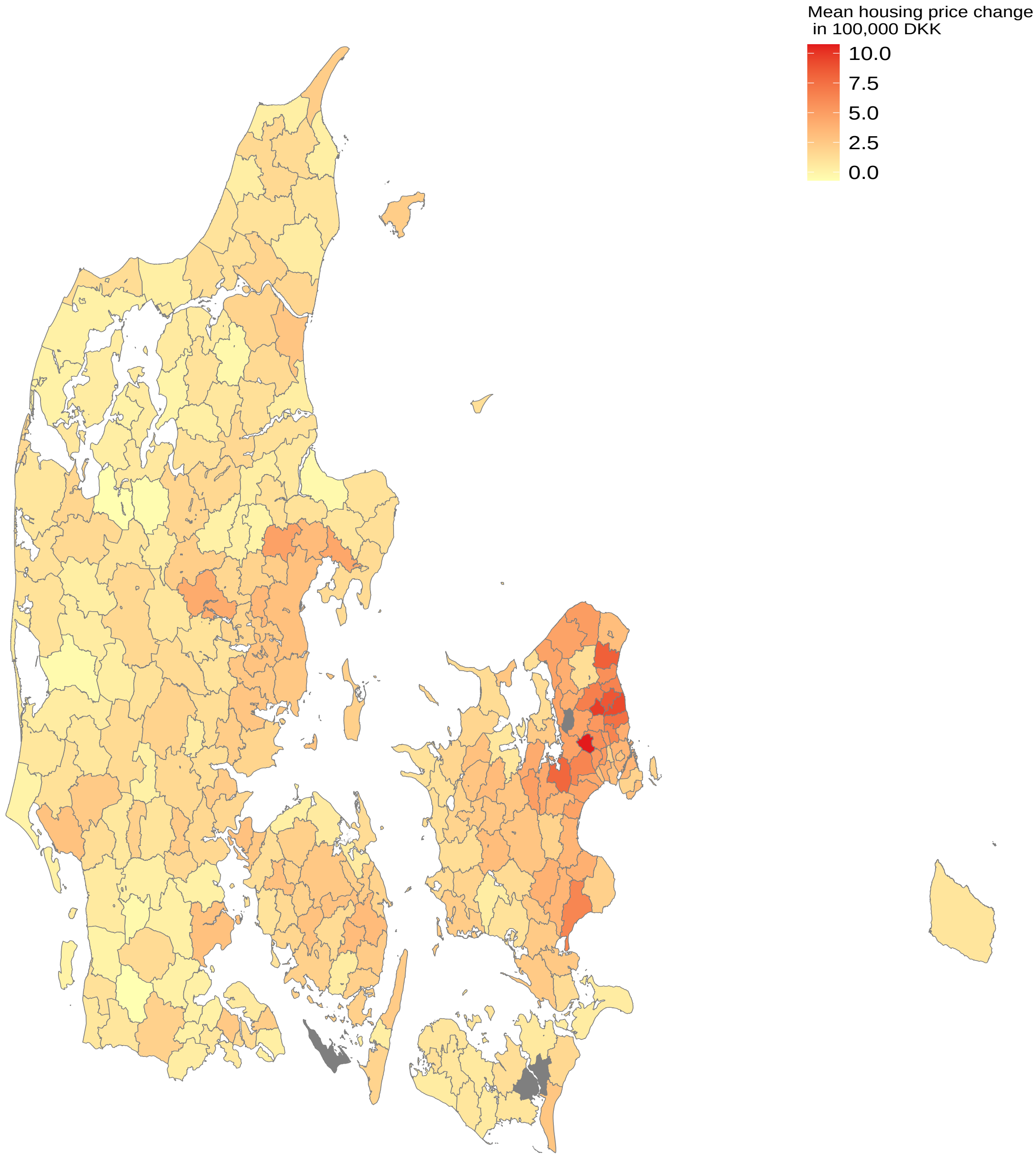
The solid line illustrates mean difference in actual purchase price and estimated value in the year of purchase for homes actually sold. The dashed lines illustrate upper and lower bounds of the associated 95% confidence interval.

Figure 3: Trends in One-Year Home Price Change by Home Price Quartile



Each line shows the average one-year home value change by quartile of the home's value in the initial year (t-1).

Figure 4: One-Year Home Price Changes by Municipality, 2005-2006



The figure presents a heat map of one-year home price changes from 2005 to 2006 by municipality.

Online Appendix: Not for Publication

Table A-1: Sample Construction

Panel A: Sample of House-years	
Houses and apartments with residential uses, 1992-2006	20,345,781
With non-negative public valuation	20,345,710
With non-negative sales price	20,342,427
With a single address	20,286,362
For private use only	20,282,101
With at least 5 houses sold in the cluster	20,273,275
With sales price at least 40% of public valuation	20,254,219
With sales price less than 300% of public valuation	20,252,160
Panel B: Sample of Women	
Women ages 18-44, 1992-2006	1,604,633
Either woman or partner own a single house (individually or jointly)	1,587,602
First purchased a house between 1993 and 2005	224,661
With sufficient data to calculate lagged one-year house price changes*	221,946
With non-missing data on household income	219,028
With non-missing data on house price and ownership	213,372
With sufficient data to calculate municipality correction factor	213,248
Purchased a house whose public valuation changed by less than 50% in the prior year	206,084
Exclude those in 1st and 99th percentile of lagged one-year house price change	204,507

In all cases, the houses mentioned in Panel B must meet all of the sample screens described in Panel A. Sample size in Panel A corresponds to house-years and not individual houses.

* This screen drops 18 and 19 year olds and omits the first two years of the treatment period.

Table A-2: Summary Statistics, Women Aged 20-44 During 1995-2006

	Mean	S.D.
Outcome variables:		
Birth (0/1)	0.0641	0.2448
Birth weight (grams)	3,532.7833	574.7735
Low birth weight (birth weight < 2,500 grams, 0/1)	0.0356	0.1853
Very low birth weight (birth weight < 1,500 grams, 0/1)	0.0061	0.0780
Premature (born before gestational week 37, 0/1)	0.0505	0.2189
Number of days hospitalized in first year of life	4.5789	7.7839
Hospitalized in first year of life (0/1)	0.9823	0.1318
Number of ER admissions in first year of life	0.0911	0.3469
ER admission in first year of life (0/1)	0.0773	0.2670
Number of days hospitalized ages 1 to 5	1.5170	5.7881
Hospitalized ages 1 to 5 (0/1)	0.4633	0.4987
Number of ER admissions ages 1 to 5	0.5849	1.0308
ER admission ages 1 to 5 (0/1)	0.3510	0.4773
Control variables:		
Real household income (100,000 DKK)	4.2577	4.0853
Years of education	13.2132	2.3315
Number of children	1.0774	1.1637
Partner (married and/or cohabiting)	0.6385	0.4804
Unemployed (0/1)	0.0418	0.2001
Age	31.6117	7.5701
20-24 years old (0/1)	0.1721	0.3775
25-29 years old (0/1)	0.1960	0.3970
30-34 years old (0/1)	0.2128	0.4093
35-39 years old (0/1)	0.2136	0.4098
40-44 years old (0/1)	0.2055	0.4041

Number of women = 11,767,245, number of births = 753,716 (741,873 with non-missing information on birth weight), number of women giving birth = 479,780. 100,000 DKK $\approx$ 12,000 USD.

Table A-3: Effects of Housing Prices on Fertility Using Different Sets of Controls

	(1)	(2)	(3)
Lagged one-year	0.0016^{**}	0.0037^{***}	0.0031^{***}
home value change	(0.0008)	(0.0006)	(0.0007)
Real household		0.0004^{*}	0.0004^{**}
income		(0.0002)	(0.0002)
% Effect	1.39	3.22	2.70
R^2	0.002	0.055	0.056
Year FE	X	X	X
Municipality FE	X	X	X
Controls		X	X
Age×year of FP FE			X

Columns (2)-(3) 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 = 0.1149; Number of observations = 1,133,795. 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: Effects of Housing Prices on Fertility
Controlling Separately for Each Observable Characteristic

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lagged one-year	0.0016**	0.0057***	0.0054***	0.0015**	0.0004	0.0002	0.0016**
home value change	(0.0008)	(0.0010)	(0.0011)	(0.0007)	(0.0007)	(0.0006)	(0.0008)
Number of children			-0.0502***				
			(0.0022)				
Total household income				0.0003			
				(0.0004)			
Years of education					0.0080***		
					(0.0002)		
Partner						0.0431***	
						(0.0012)	
Unemployed							-0.0605***
							(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	0.034	0.028	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: The Relationship Between Expenditures and the Presence of a Young Child in the Household

Expenditure Category	Expenditure (Dollars) (1)	Log Expenditure (2)	Mean (Dollars) (3)	Percent Effect (Column 1) (4)
Panel A: Homeowners				
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				
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.

Table A-6: Heterogeneous Effects of Housing Prices on Low Birth Weight and Prematurity

	Panel A: Mother's Age				
	20-24 years	25-29 years	30-34 years	35-39 years	40-44 years
	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Low Birth Weight					
Lagged one-year	-0.0035	-0.0003	-0.0003	0.0016	-0.0038
home value change	(0.0054)	(0.0012)	(0.0009)	(0.0017)	(0.0083)
Dependent Variable: Prematurity					
Lagged one-year	-0.0104*	-0.0022	-0.0012	0.0006	-0.0086
home value change	(0.0057)	(0.0014)	(0.0012)	(0.0022)	(0.0082)
	Panel B: Parity				
	0 children	1 child	2 children	3+ children	
	(1)	(2)	(3)	(4)	
Dependent Variable: Low Birth Weight					
Lagged one-year	-0.0009	0.0005	-0.0019	-0.0047	
home value change	(0.0013)	(0.0008)	(0.0017)	(0.0096)	
Dependent Variable: Prematurity					
Lagged one-year	-0.0035***	-0.0003	-0.0019	-0.0078	
home value change	(0.0014)	(0.0010)	(0.0021)	(0.0117)	
	Panel C: Mother's Income				
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile	
	(1)	(2)	(3)	(4)	
Dependent Variable: Low Birth Weight					
Lagged one-year	-0.0004	0.0007	-0.0015	0.0002	
home value change	(0.0019)	(0.0014)	(0.0013)	(0.0011)	
Dependent Variable: Prematurity					
Lagged one-year	-0.0048*	-0.0018	-0.0019	0.0004	
home value change	(0.0027)	(0.0016)	(0.0016)	(0.0014)	
	Panel D: Mother's Education				
	Bottom Quartile	2^{nd} Quartile	3^{rd} Quartile	Top Quartile	
	(1)	(2)	(3)	(4)	
Dependent Variable: Low Birth Weight					
Lagged one-year	0.0003	-0.0011	0.0031	-0.0006	
home value change	(0.0015)	(0.0014)	(0.0020)	(0.0010)	
Dependent Variable: Prematurity					
Lagged one-year	-0.0015	-0.0057***	0.0036	-0.0000	
home value change	(0.0019)	(0.0016)	(0.0031)	(0.0013)	

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: 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.