STOCKHOLM SCHOOL OF ECONOMICS
Department of Economics
5350 Master's thesis in economics
Academic year 2015–2016

House prices and fertility rates:

Empirical evidence on the wealth effect and the option value effect in Sweden

Agneta Berge (22044) & Amanda Olsson (21865)

Abstract

Previous literature suggests that fertility rates of house owners respond more positively to higher house prices than those of renters due to a housing wealth effect. The identification of such wealth effect relies on the assumption that house owners and renters respond equally to higher house prices as the investment cost of having an additional child. We add to this literature by recognizing that house ownership correlates positively with age. When house prices are high, there is an incentive to postpone fertility to wait and see if house prices will decrease. Option value theory predicts that the value of holding such option to postpone childbearing should decrease with age due to increased risks of fertility postponement, and thus that fertility rates of older women should respond more positively to higher house prices than those of younger women. We empirically test this prediction and the wealth effect by exploiting the regulated rental market in Sweden, allowing us to identify house prices as an investment cost of having an additional child. We use panel data on Swedish municipalities over the years 1993-2014 and fixed effects regressions, and address endogeneity concerns by using internal instruments for house prices and by controlling for various trends. Consistent with the option value effect, our results suggest that higher house prices are significantly associated with positive fertility rate effects for older women relative to younger. Our results give weaker support to the wealth effect, suggesting it is biased when an option value effect is not controlled for.

Keywords: Fertility, household behaviour, house prices, option value, wealth effect

JEL Classification: D10, D81, J13, R21

Supervisor: Kerem Cosar
Date submitted: January 2, 2016
Date examined: January 12, 2016
Discussants: Nils Fergin & Lu Yu
Examiner: Maria Perrotta Berlin

Acknowledgements

We are greatful to Roman Bobilev, Anna Dreber Almenberg, Karl Harmenberg, Dany Kessel, Erik Lindgren, Bo Malmberg, Hannes Malmberg, and Erik Öberg for helpful discussions and comments. We are especially thankful for the valuable comments, discussions and guidance from our supervisor Kerem Cosar. All errors are our own.

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1 Introduction

The decision to have a child is one of the most important decisions that a household makes and lies at the heart of demography. A long economic literature dating back to Malthus (1798) has focused on how these decisions are influenced by financial incentives and the key modern reference on fertility as an economic decision is Becker (1960), who argues that children should be analyzed as durable consumption goods. This implies that the demand for children should respond negatively to an increase in the price of a marginal child. Within the Becker framework, children are also assumed to be normal goods since there are few substitutes for children. This implies that the demand for children should respond positively to an increase in household income or wealth.

With this paper, we aim to test these key predictions from Becker's framework. In particular, we aim to investigate how house prices, that have the double characteristic of both reflecting the value of housing wealth as well as the cost of one of the major investments associated with having an additional child, namely that of buying a (larger) house (see e.g. Ström 2010; Chudnovskaya 2015), are associated with fertility behavior.[1]

Against the backdrop of the recent economic crisis, the association between house prices and household behavior has attracted increasing interest. Firstly, housing wealth constitutes a large part of the household balance sheet. Secondly, the experience from the recent economic crisis indicates that fluctuations in house prices affect household consumption. Thirdly, deregulated financial markets have spurred higher private debt-to-income ratios and simultaneously, strong house price growth has led to much speculation about the outlook for house values.

A large and growing body of research indicates that there is a significant and economically meaningful relationship between house price movements and consumption (see e.g. Case et al. 2006; Chen 2006; Campbell and Cocco 2007; Li and Yao 2007; Mian et al. 2013; Berger et al. 2015). This relationship is hypothesized to be explained by a wealth effect that operates either through a direct wealth channel and realized wealth gains for house owners, or through a credit channel and a relaxed borrowing constraint for house owners that can use their housing asset as collateral. A recent contribution to this literature investigates the relationship between housing wealth and the household consumption decision of having a child. Using rich U.S. micro data, Dettling and Kearney (2014) (D&K hereafter) and Lovenheim and Mumford (2013) (L&M hereafter) identify a housing wealth effect on fertility rates based on the assumption that while the wealth and/or the credit channel should affect the fertility behavior of house owners and not that of renters, the higher investment costs of having a child that is also reflected by higher house prices should affect potential first-time homeowners and current homeowners who might upgrade to a bigger house with the addition of a child equally. Both studies find that higher house prices are associated with a positive fertility rate premium for house owners relative to renters, which lends support to the wealth effect explanation of the co-movement between house prices and fertility rates.

However, as for example Attanasio et al. (2009) recognize, there may be systematic differences between owners and renters in terms of their age, which may in turn correlate with different responses to increasing costs of having a child. In particular, house owners are likely to be older

[1] This paper is limited to study only the effects that are associated with prices on single family houses and not those that are associated with prices on owned apartments, due to limited data availability. This is elaborated upon in section 3 when we discuss our limitations of scope.

than renters on average, which may have important implications for their fertility response to higher investment costs of having a child. This is a potential limitation to the identification of a wealth effect on fertility rates.

In this thesis, we argue that the option value theory can contribute to the understanding of the potential difference in how the fertility behavior of women of different ages responds to higher costs of having a child, and thus to the understanding of the potential limitation to identification of a wealth effect between house prices and fertility rates. In line with for example Ström (2010) and Chudnovskaya (2015), we consider house prices as the major investment cost of having a child. If house prices increase, and since their development is characterized by uncertainty, there should be a value associated with holding the option to postpone childbearing, to see if house prices will decrease in the future (see e.g. Dixit and Pindyck 1994; Iyer and Velu 2006; Bhaumik and Nugent 2011; Asphjell et al. 2013). Since fertility risks increase with age, the cost of postponing childbearing is however expected to increase with age, as also argued by Asphjell et al. (2013). The value of holding the option to postpone the fertility decision should thus decrease with age. Taken together, the option value theory predicts that there is a value associated with being able to wait and see if house prices will decrease in the future, and that this value is higher for younger women than for older. We argue that such value should be included in the potential parent's cost-benefit analysis of having a child, interpreted as a shadow price of having a child.

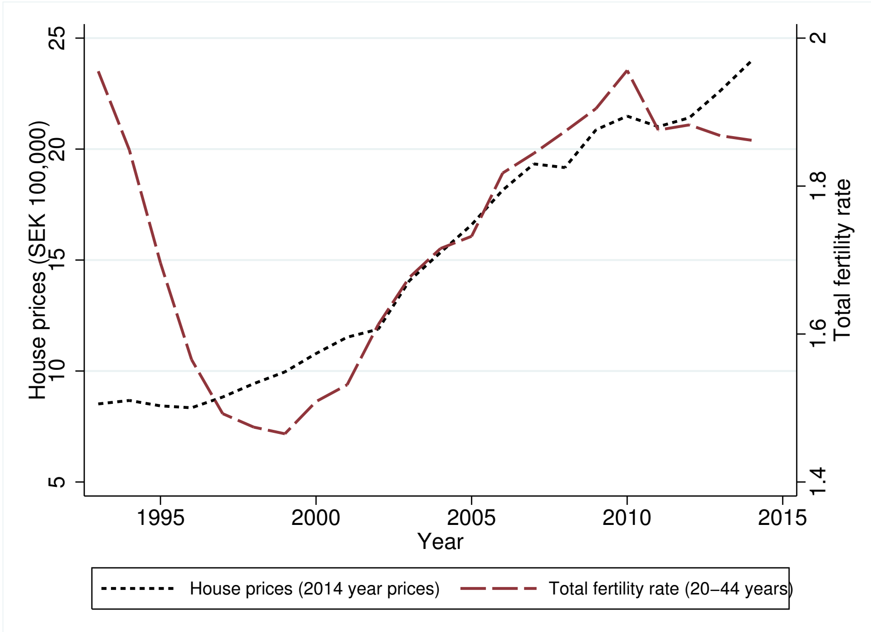
Iyer and Velu (2006) model the influence of uncertainty on fertility decision-making in an option value framework. Their model predicts that the value of postponing the fertility decision increases with the uncertainty in the net benefits of having a child. Using German micro data, Bhaumik and Nugent (2011) find empirical support for these predictions. Asphjell et al. (2013) develop a model that predicts that the option value of postponing fertility is higher for younger women than for older, and find support for this prediction using rich Swedish micro data.

Therefore, by recognizing that house ownership is likely to correlate positively with age, and by recognizing that owners and old women are predicted to respond similarly to increasing house prices relatively to renters and young women respectively, the assumption that house owners and renters react similarly to increasing costs of having a child appears to be strong. In other words: the housing wealth effects on fertility rates that are reported by D&K and L&M, may suffer from omitted variable bias since an option value effect is not controlled for.

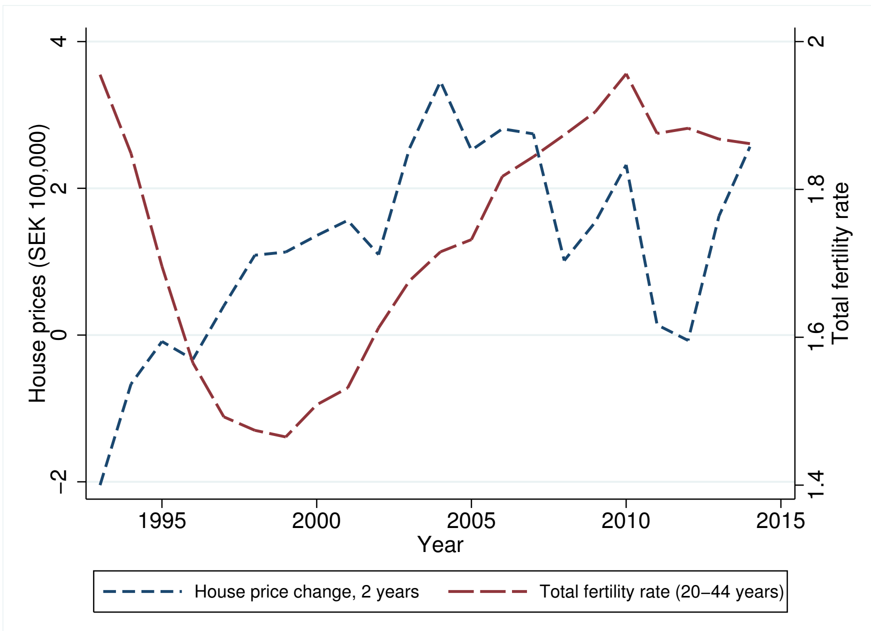
With this study, we aim to contribute to this literature by adding the value of the option to postpone the fertility decision to the analysis of how house prices affect fertility rates. As such, we also make a contribution to the option value literature, since this has, to the best of our knowledge, not studied the effect of house prices on fertility rates.

The identification of an option value effect is based on the assumption that house prices only reflect an *investment* cost for having a child, and not also *current* costs for any type of housing. In line with the arguments put forward by Chen (2006), we therefore choose to study the case of Sweden to be able to exploit the fact that the cost of living in rented housing is regulated in Sweden, and regulated rents are less likely to follow the market price for houses than rental rates that are determined on the regular housing market. Further, following a two decade long housing boom and rising debt-to-income ratios, understanding the relationship between house price fluctuations and real economy outcomes is of particular importance at present in Sweden. A first look at Swedish house price and fertility rate data in figure 1.1 also provides *prima facie* motivation a more formal and thorough investigation of their co-movement.

Figure 1.1: House prices and total fertility rates in Sweden, 1993-2014



(a) House price levels and total fertility rates

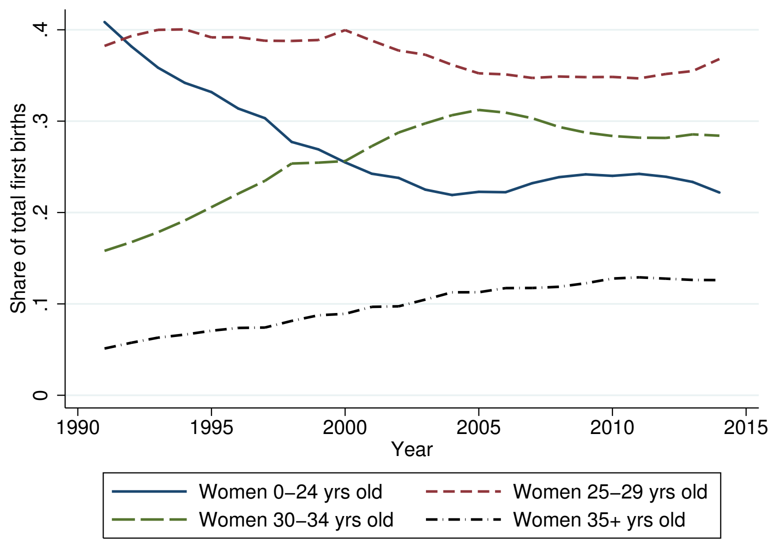


(b) House price changes and total fertility rates

Note: The figures display the development of house price levels (a) and two year house price changes (b) along with total fertility rates for women 20-44 years old in Sweden 1993-2014. Data source: Statistics Sweden, the authors' own calculations.

In particular, similarly to several other developed countries (Schmidt et al. 2012), the development of fertility in Sweden over the past two decades indicates a trend of postponement, as shown in figure 1.2. The increasing age at first birth and the overall comparatively high ages at which births occurs for many women, is coincident with the housing boom of the past two decades. This further motivates a formal analysis of the relationship between house prices and different fertility postponement behaviors between women of different ages, and, in other words, it motivates the addition of the option value framework to the study of housing wealth and fertility.

Figure 1.2: Women's age at birth of first child in Sweden, 1993-2014



Note: The figure displays the development of the age composition of the women that each year give birth to their first child in Sweden, over the period 1993-2014. The ages of the women are divided into four groups: 0-24 years old; 25-29 years old; 30-34 years old; and 35+ years old. Data source: National Board of Health and Welfare.

To empirically test the implications of both the wealth effect explanation and the option value effect explanation, we make use of a panel data set for Swedish municipalities over the years 1993-2014 and fixed effects specifications that are estimated using ordinary least squares (OLS) (the time period is chosen with respect to data availability). The main threats to identification of conditional exogeneity between house prices and fertility are the concerns that house prices and fertility rates may be jointly determined by some underlying unobservable factor such as productivity and expectations of future income, and that individuals sort between municipalities that are on different house price trends in a way that correlates with their fertility preferences. We address these concerns by a careful choice of control variables; by controlling for a set of different trends; by employing county-by-year fixed effects since shocks are arguably more likely to be exogenous at the county level; and by making use of internal instrument variables in an Arellano-Bond GMM model.

Our empirical analysis shows four main results. First, we find that especially higher house price levels but also house price changes are robustly associated with differential fertility rate effects between young (20-29 years old) and old (30-44 years old) fertile women. In all specifications, this differential effect is estimated to be a positive fertility rate premium for old fertile women relative to young, consistent with the option value explanation of the relationship between house prices and fertility rates. The most conservative estimates indicate that a SEK 100,000 (approx USD 11,775) increase in house price levels is associated with 2.7 percent increase in fertility rates among old women relative to young women, and that a SEK 100,000 house price change over the past two years is associated with a 2.3 percent increase in fertility rates among old women compared to young. The size of these estimates are relatively consistent with those reported by D&K and L&M for the wealth effect. Second, we find that house price changes appear to be associated with differential fertility rate effects between groups with different home ownership rates, which lends support to the wealth effect explanation. A SEK 100,000 change in house prices over the past two years is associated with a 2.5 percent increase in fertility rates going from a group with zero percent home ownership to one with 100 percent home ownership. More meaningfully, between a group with 25 percent ownership rate and one with 75 percent ownership rate, the fertility premium for the latter would be 1.2 percent. We do however draw careful conclusions from this result as it is not fully robust and since there are limitations to the ownership data, but we note that it appears to be somewhat lower than the estimates reported by D&K and L&M. Third, and in contrast to the wealth explanation, we do not find that higher house price levels are associated with significantly different fertility rate effects between groups with different ownership rates. Forth, we find that the wealth effect estimates are substantially upwards biased when they are estimated on their own, without controlling for the option value effect. A corresponding large upwards bias is not found for the option value effect estimates, when they are estimated without controls for the wealth effect.

These results indicate that the value of the option to postpone fertility contributes to explain the relationship between house prices and fertility rates, while the evidence on the wealth effect is mixed. In particular, our results add to the literature that studies how housing wealth affects fertility rates by suggesting that estimates of the wealth effect that are obtained from analyses that do not control for the option value effect may be biased. This paper also contributes to the literature on the effects of housing market fluctuations and to the literature on fertility responses to financial incentives by demonstrating that fertility choices are among the set of behaviors that are affected by changes in the housing market.

Our results are also important from a policy perspective. In the long run, completed fertility rates affect dependency ratios and a country's ability to finance its welfare system. According to the latest U.N. population estimates, fertility is now below the replacement level of 2.1 in for example Western Europe (1.66), Northern Europe (1.87), Southern Europe (1.41) and North America (1.89). In the short run, the timing of fertility matters for how factors such as housing, pre-schooling and schooling should be dimensioned. Indeed, short-run postponement of fertility have also been found to reduce completed fertility in the longer run (Schmidt et al. 2012).

The remainder of this paper is organized as follows. In section 2, we describe the theoretical framework in which we study the relationship of fertility and house prices. In particular, we describe both the wealth effect approach and the option value approach. Here we also present relevant empirical literature. In section 3 we distill the theoretical predictions into testable implications. The data and empirical strategy we use to test the implications is outlined in section 4. Here we expand in particular on our identification strategy and how we deal with the threats to it. The empirical results are presented in section 5 and section 6 discusses the results, their limitations, fruitful topics for future research, and then concludes.

2 Theoretical framework and relevant literature

In this section, we develop the theoretical framework for the study of how house prices may affect fertility rates and present the relevant literature. We begin by elaborating on the key implications from Becker's framework on fertility decision making, in which house prices are conceptualized to influence the fertility decision both as a wealth effect through the budget constraint and through an option value effect by constituting one of the major investment costs associated with having a child. We then proceed to describe the wealth effect in depth, and thereafter we expand on the option value effect.

In the presence of access to and knowledge about contraceptives, fertility can, as Becker (1960) noted, be argued to be a decision affected by other factors than coition[2]. Furthermore, children are, for most parents, a source of happiness and satisfaction, and thus considered a consumption good. Children may sometimes also provide money income or work in the household and are then a production good. In most developed countries, it is however reasonable to assume that children are net consumption goods. Moreover, as utility can be derived from children during a long period of time, they are assumed to be durable consumption goods within the Becker framework. This implies that the demand for children should respond negatively to an increase in the price of having a marginal child. Lastly, children are in addition assumed to be normal goods within the Becker framework, as they do not appear to be inferior member of any broader class of goods. This implies that the income elasticity of demand for children is positive, why demand for children should respond positively to rising income or wealth.

The most simple static economic approach to the question of fertility is then to view parents as consumers that maximize their lifetime utility subject to the price of children and the budget constraint they face. The standard approach in the economics literature on fertility has been to examine the fertility decision with regards to income and wages, and in particular to female wages (see e.g. Becker 1960; Heckman and Walker 1990; Lindo 2010; Milligan 2005; Cohen et al. 2013; Andersson 2000). The evidence is mixed. Heckman and Walker (1990) estimate a reduced-form neo-classical model of fertility dynamics using Swedish longitudinal data and find that rising female wages delay times to all conceptions and reduce total conception. They also find that the strongest effect on timing operates through the time to the first birth. Cohen et al. (2013), on the other hand, use micro data on over 300,000 Israeli women and find a small effect of income on fertility, which is negative at low and positive at high income levels.

The main hypotheses for explaining the negative relationship between income and fertility is the quantity/quality trade-off hypothesis (Becker 1960, Becker and Lewis 1974) and the opportunity cost of time hypothesis (Mincer 1963; Becker 1965). The quantity/quality trade-off refers to the observation that parents have preferences for both the quantity and quality of children[3]. The relationship between the income elasticity of demand for quantity of children and the income elasticity of demand for quality of children therefore determines if parents substitute towards higher quality of children or higher quantity of children as income rises. The opportunity

[2] In absence of contraceptives, some degree of decision making is introduced to the question of fertility through absenteeism and abortion. Abortion and absenteeism is however not perfectly available in all societies. Moreover, some individuals may have a narrowed scope of decision making due to difficulties of having a child. Modern technology the scope of decision making in these cases, probably in particular with regards to the number of children to have, but less so with regards to the timing of having a child. In practice, fertility control is thus imperfect.

[3] With higher quality referring to parents obtaining additional utility from spending additional resources on the child and not to some children being morally better than others.

cost of time, on the other hand, recognizes that increasing wage rates raise the shadow cost of parental time. Thus, a negative relationship between income and fertility can stem from either a trade-off between quality and quantity of children, or from a higher opportunity cost of time of having children, or from both. A major challenge to the identification strategy of studies on the relationship between income and fertility, and a common critique to them (see e.g. Lovenheim and Mumford 2013; Dettling and Kearney 2014), is that the effect of a quantity/quality trade-off is difficult to credibly disentangle from that of rising opportunity cost of time.

It is however not only rising income and wages that relax the household budget constraint; so does increasing wealth. Studying the relationship between wealth and fertility has merit in that the identification is not limited by the opportunity cost of time hypothesis. Further, while the study of the relationship between wealth and fertility has recently grown, it is still substantially smaller than that using income, why it is of relatively greater interest to make a contribution to the understanding of the role of wealth in the household fertility decision. This study therefore considers wealth instead of income in the study of fertility. Lastly, this study only considers housing wealth, which, along with stock-market wealth, form the major part of the household asset portfolio. This is motivated by the present interest in house prices that has grown in particular against the backdrop of the recent economic crisis, and by the value of comparability to the recent literature on housing wealth and fertility. Further, housing wealth conceptualized as house prices does not only affect the fertility decision through the household budget constraint, but housing is also a consumption good itself, and one of the major costs associated with having a child. House prices thus captures two dimensions of the question of how fertility behavior responds to financial incentives: the wealth and budget constraint dimension, as well as the dimension of the investment cost of having a child. House prices can therefore be used to test the two key implications from Becker's framework: that the demand for children should respond positively to an increase in wealth, and negatively to an increase in the cost of children.

In the next section, we develop the wealth dimension of house prices, and present the wealth effect approach to the study of house prices and fertility. We then proceed to cost dimension of house prices, and present the option value theory and investment cost approach to the study of house prices and fertility.

2.1 The wealth effect approach

Since fertility is considered to be a household consumption decision, we begin by noting that a large and growing literature suggests that consumption responds to house price movements in an economically and statistically meaningful way, suggesting an important role for housing wealth in household consumption dynamics (see e.g. Lettau and Ludvigson 2004; Case et al. 2006; Campbell and Cocco 2007; Attanasio et al. 2009; Li and Yao 2007; Mian et al. 2013; Berger et al. 2015). Case et al. (2006) compares the effect on consumption of stock market wealth and housing wealth using country level as well as U.S. state level panel data, and find a statistically significant and rather large effect of housing wealth upon household consumption. These effects appear to be heterogenous between different households. Li and Yao (2007) develop a life-cycle model that predicts that the non-housing consumption of young and old homeowners is more sensitive to house price changes than that of middle-aged homeowners. Both using micro U.K. data, Campbell and Cocco (2007) find that the consumption of older house owners are affected the most by rising house prices, while Attanasio et al. (2009) in contrast find that young households respond more.

A number of studies using Swedish data also point to a similar relationship between house prices and consumption (see e.g. Barot 1995; Berg and Bergström 1995; Johnsson and Kaplan 2001; Lyhagen 2001; Chen 2006). Chen (2006) studies the association between the Swedish housing market and consumption with a vector error correction and cointegration model. His main findings are that in particular long run associations between housing wealth and consumption are strong; the estimated long run elasticity of total consumption with regards to net housing wealth is 0.11. Chen further notes that a large part of the variance in housing wealth movements are transitory, and that short run variations are largely disassociated with consumer spending. According to his findings, a one percent increase in housing wealth in the short run is associated with a 0.064 percentage point increase in consumption.

2.1.1 Channels between housing wealth and consumption

There are two main channels that explain this co-movement between housing wealth and consumption: the wealth channel; and the credit channel. The wealth channel can broadly be said to capture two distinct effects. The first effect is that with higher house prices, more resources are available for life time consumption, in line with the life-cycle theory of consumption, first formulated by Modigliani and Brumberg (1954). This theory proposes that individuals want to smooth their consumption over their life time, with the underlying assumption that individuals are forward looking and plan their consumption after known and future expected incomes and financial and real assets. The life-cycle theory therefore implies that the full life-time value of wealth is important as it reflects potential life-time consumption.

The second effect that the wealth channel captures is that a current period increase in the market value of the housing asset leads to a net increase in housing wealth. The permanent income hypothesis (PIH) developed by Friedman (1957) states that only changes in permanent income and not changes in transitory income will affect consumption. In our conceptualization, house price changes that are viewed as permanent, and not those that are viewed as transitory, should affect fertility behavior through the wealth channel. This is in line with the findings presented by Lettau and Ludvigson (2004), who suggest that only permanent changes in housing wealth are associated with movements in consumption.

The credit channel, in turn, implies that a credit constrained household can increase its borrowing when it has access to higher value collateral, which will allow it to better smooth its consumption over the life cycle. Aoki et al. (2004), recognizing the strong relationship between net housing equity and mortgage equity withdrawal, point out that a rise in house prices increases the collateral available to home owners. This may encourage them to borrow more - in the form of a mortgage equity withdrawal - by raising the equity available to borrow against, enabling them to finance higher consumption.

For the credit channel to have explanatory power, functioning financial markets are required. If consumers cannot get access to credit markets, or if they are credit constrained, they are restricted in their abilities to react to asset price changes. Case et al. (2006) argue that institutional innovations in recent decades, relating to mortgages in particular, imply that housing wealth might be of particular importance for household consumption decisions, as they have facilitated withdrawal of cash from housing equity. In Sweden, financial deregulation starting in the mid-1980s has caused increasing lending and loan-to-value ratios (Turner 1999). Figure 8.2 in appendix shows the development of the debt to disposable income ratio for Swedish households, which has been increasing from 94 percent in 1996, to 171 percent in 2014. The financial deregulation has also facilitated the increasing use of Swedish housing equity for consumption. Since the year 2000 Swedish households' withdrawal of housing equity for consumption has been increasing. In 2005 the the withdrawal of housing equity to disposable income corresponded to around 5 percent (National Board of Housing, Building and Planning 2012). However, in the years following the Swedish financial crisis in the 1990s, the withdrawal of housing equity for consumption, as a share of disposable income, was negative.

The credit channel and the wealth channel complement each other in the sense that if households are forward looking, a wealth effect will occur once the house price change can be anticipated, not when it actually happens. On the other hand, as argued by Campbell and Cocco (2007), changes in house prices that have already been anticipated could still relax borrowing constraints once they occur.

Using U.K. micro data, Campbell and Cocco (2007) estimate the largest effect of house prices on consumption for older homeowners, and the smallest effect, insignificantly different from zero, for younger renters. This finding is consistent with heterogeneity in the wealth effect across these groups and in support of the wealth and credit explanations of the relationship between house prices and consumption. A recent paper by Mian et al. (2013) find that the marginal propensity to consume (MPC) out of housing wealth in the United States following the recent economic crisis was on average 5 to 7 cents for every dollar of house value, and that the MPC for durable goods was higher than for nondurables. Using aggregate data and an error correction model, Johnsson and Kaplan (2001) estimate that the MPC out of *net* housing wealth in Sweden over the period 1970-1998 was 0.04. Although lower than MPC out of financial assets (0.16) and income (0.8), these results are also in line with the wealth effect.

2.1.2 Housing wealth and fertility

A recent contribution to the literature on housing wealth and consumption investigates the relationship between housing wealth and fertility and presents evidence of a co-movement (Dettling and Kearney 2014; Lovenheim and Mumford 2013). In a study of U.S. metropolitan areas, using individual data and both ordinary least squares (OLS) and two stage least squares with an instrumental variable approach (IV), D&K find that rising house price levels are associated with differential fertility effects among renters and owners: a USD 10,000 increase in house price levels leads to a 7.2 percent increase in fertility rates among homeowners relative to that among non-owners. More meaningfully, moving from a demographic group with 25 percent ownership rate to one with 75 percent ownership rate, the results indicate a positive fertility rate premium of 3.6 for the latter relative to the former. Given underlying differences in home ownership rates, the predicted net effect of house price changes also varies across ethnic groups. These results are in line with the predictions of the life-cycle theory. However, as the study only captures fertility timing effects, these results should not be interpreted as the effect on completed fertility rates.

Unlike D&K, however, L&M do not find that house price levels have a significant impact on fertility rates in the U.S., but instead that house price changes appear to be relevant. As predicted by the PIH, changes in house prices can affect consumption if they are perceived as permanent. Just like D&K, however, the findings of L&M also point to a differential fertility effect between renters and homeowners in the U.S. Using a linear probability model and U.S. micro data, they find that a USD 100,000 increase in the value of a home over the prior two (four) years is associated with a 0.0089 (0.0082) percentage point increase in the likelihood of having a child among homeowners. This represents a 17.8 (16.4) percent increase in the likelihood of having a child, or an 18.8 percent increase of the underlying the total fertility rate for women 25-44 years old. Again, these effects are fertility timing effects. With respect to renters, the results reported by L&M suggest that housing price increases have little effect and may reduce the likelihood of giving birth. These results lend support to the wealth effect, and are in particular in line with the PIH if these house prices changes are believed to be permanent.

One concern with the identification of a wealth effect in this literature is that house prices and fertility, or consumption in general, can be linked together by underlying productivity changes and expectations of future incomes (Attanasio and Weber 1994). Attanasio et al. (2009) use U.K. micro data to study the link between housing wealth and consumption and report coefficients for young head of households (21-34 years old) that are positive and larger than those for middle-aged (35-59) and old (60-75) head of households. They argue that this pattern of coefficients does not appear to offer support for the wealth hypothesis; since households comprised by older people are more likely to own a house, less likely to want to trade up in the future, and have a shorter time-frame in which to enjoy their wealth gain, they should benefit the most from higher house prices under the wealth explanation. As previously described, the findings of D&K and L&M suggest that there is a differential fertility effect between owners and renters in terms of larger coefficients for owners. These findings do not lend support to the common causality explanation, since under that explanation, owners are not expectd to benefit the most from house price increases.

However, in a recent study, Cesarini et al. (forthcoming) use administrative data on Swedish lottery players to estimate the causal impact of substantial wealth shocks on players' own health and their children's health and developmental outcomes. They also test whether wealth shocks have an impact on fertility, and find no evidence that the lottery wealth impacts fertility of women below the age of 50 in their sample, which contradicts the prediction of the wealth channel. This question is however not the main focus of their study and the authors recognize it as a fundamental question in its own right.

Another concern with the identification of a wealth effect is that it is based on the assumption that while the wealth and/or the credit channel should affect the behavior of house owners and not that of renters, the higher investment costs of having a child that is also reflected by higher house prices should affect potential first-time homeowners and current homeowners who might upgrade to a bigger house with the addition of a child equally (Dettling and Kearney 2014; Lovenheim and Mumford 2013). The next section is dedicated to the discussion of why this may be a strong assumption to make.

2.2 The cost and option value approach

The previous section discusses the theory and relevant literature related to the wealth dimension of house prices and the study of housing wealth and fertility. We therefore now proceed by discussing the theory and relevant literature for the cost dimension of house prices. In particular, while the literature that studies housing wealth and fertility empirically examines fertility timing effects rather than completed fertility effects, the timing dimension has, to the best of our knowledge, not been dealt with explicitly in the frameworks or the empirical analyses of this literature.

We add to this body of literature by explicitly considering the fertility timing question in relation to house prices as the investment cost of having a child. We draw the insights to the timing question from the economics literature on irreversible investments under uncertainty. This section therefore starts with an introduction to the option value theory and to previous literature that studies fertility using it. We then outline the factors that impact the value of the option to postpone fertility, among which we are particularly interested in the investment cost of having a child and in the cost of postponement.

2.2.1 The option to postpone the fertility decision

The standard application of the option value theory considers a firm facing an investment choice. The investment decision is assumed to be (partially or completely) irreversible, there is uncertainty about the future returns from the investment, and the timing of the investment is flexible. These three features generate an option value on the investment decision in terms of the wedge between the net present value of the investment threshold under completely certain conditions and reversible investments, and the threshold under uncertainty and irreversible investments. By postponing actions, the firm can get more information about the future costs and rewards and reduce, although not completely remove, uncertainty. There is thus also a value associated with holding the option to postpone the investment decision (see e.g. Bernanke 1983; Bertola 1988; Dixit and Pindyck 1994).

The motive for turning to the literature on the option value theory for insights to fertility behavior is that the decision to have a child is also characterized by the three features of irreversibility, uncertainty, and choice of timing. First, having a child is irreversible once the child is born. Second, the economic environment in which the fertility decision is made is likely to be characterized by some degree of uncertainty, and third, women can influence the timing of childbearing up until the end of their childbearing years. Further, the theory is applicable to the fertility decision since the definition of an investment as "the act of incurring an immediate cost in the expectation of future rewards" (Dixit and Pindyck 1994) is compatible with incurring an expenditure on a durable consumption good, or in other words, with having a child[4].

Under the option value approach, a household with an opportunity to postpone childbearing is holding an "option" analogous to a financial call option of American style: it has the right but not the obligation to have a child at some future time of its choosing up until the time of expiry of the option (that is, when the woman reaches menopause). As there is uncertainty associated with the irreversible decision of having a child, there is a value associated with holding the option to postpone the decision. When a household decides to have a child, it exercises its option to postpone the fertility decision and wait for new information to arrive that might affect the desirability or timing of the child; it cannot "disinvest" should market conditions change adversely. This lost option value is an opportunity cost that must be included as part

[4]Dixit and Pindyck (1994) argue that not only traditional decisions such as buying a house or enrolling at a university, but also seemingly non-economic decisions such as to marry should be considered investments of this type.

of the cost of the investment to have a child; the value of the child must not only equal the investment and maintenance costs, as in the standard "net present value" theory, but exceed these costs by an amount equal to the value of keeping the investment option alive (Dixit and Pindyck 1994). The option value approach therefore has merit because it deals more explicitly with the timing considerations of the fertility decision than the wealth effect approach does.

Several studies use the option value approach to analyze fertility behavior (see e.g. Zuluaga 2011; Gete and Porchia 2011; Iyer and Velu 2006; Bhaumik and Nugent 2011; Asphjell et al. 2013). Iyer and Velu (2006) model the benefits of children as subject to uncertainty with the prediction that under increasing uncertainty, the value of the option to wait increases. On the other hand, children are in this model also thought of as an insurance against risk to family income and mortality, which predicts that the demand for children today increases with uncertainty. The net of these two effects cannot be determined theoretically, but Iyer and Velu argue that with improved education and health services, the insurance value of a child is reduced.

Bhaumik and Nugent (2011) use a probit model to empirically test the predictions made by Iyer and Velu (2006). They exploit differences in employment- and financial-related uncertainty between West and East Germany at reunification and find support in favour of the existence of an option to postpone the fertility decision under higher uncertainty. In particular, Bhaumik and Nugent (2011) argue that the impact of uncertainty on decisions about postponement of childbirth is at the margin and therefore contemporaneous rather than cumulative. To be specific, they argue that "if a woman was to lose her job in period t, it would make her more uncertain about her employment prospects and thus affect her decision to have (or not have) a child in period t. But, when the same woman would re-evaluate her situation in period $t+1$, her decision about conceiving a child in that period would be based primarily on her employment state in period $t+1$ instead of her state in period t".

Another contribution to this literature is made by Asphjell et al. (2013) who also apply an option value model to the timing of the investment in childbearing and analyze how social influences affect women's childbearing decisions. For any given benefit gain and investment cost of the child, their model predicts a positive fertility premium for having fewer compared to more years to maturity of the option to wait (i.e. menopause), and for less uncertain compared to more uncertain environments. They test these predictions empirically using a linear approximation of a proportional hazard model and data on 140,000 Swedish women over the years 1997-2004. The analysis provides support for their model predictions: the propensity to postpone the fertility decision appears to increase with uncertainty and to decrease with age.

2.2.2 Inputs for valuing the option to postpone

Against this understanding of how an option to postpone childbearing can influence the timing of a fertility decision, we now turn to describe the main factors that impact the value of holding this option. Three main factors determine this value; first, the value of having a child today, secondly, the value of postponing the decision in order to make a more informed decision in a future period when more information is available, and lastly, the costs of delaying the decision (Dixit and Pindyck 1994; Damodaran 2002).

The first factor, in line with Dixit and Pindyck (1994), is giving up the value of having the child today, which favors immediate childbearing. Households can however also derive utility from not having a child, which it would then give up instead. The net of these two utilities cannot be determined theoretically.

The second factor is that if there is uncertainty related to the investment costs of having a child, there is a value associated with being able to optimize the fertility behavior with respect to actual future outcomes, whereas immediate action must be based only on future expectation. In other words, if the investment cost is changing over time and the household expects the cost to get lower, this is a reason for waiting. In our framework, buying a (larger) house is the relevant investment cost to consider. Housing prices thus not only affect the available economic resources for private consumption through the wealth and credit channels as described in section 2.1, but housing is also a consumption good itself and one that is associated with the decision to have a child. Chudnovskaya (2015) uses register-based individual data to study childbearing and living patterns in Sweden over the years 1986-2006. She finds that compared to women living in apartments, women that live in single-family dwellings are 37-43 percent more likely to become mothers. Chudnovskaya argues that this indicates that living in a house is strongly associated with having a child, and that the desired size of the dwelling drives this link. Ström (2010) also uses Swedish data to study the relationship between housing and fertility and finds similar results: the size of a dwelling seems to be the housing factor with the strongest association to first-order births.

International evidence also provides support for studying housing as a relevant investment cost for having a child. House purchases in the Netherlands have been found to increasingly frequently happen before the birth of the first child (Feijten and Mulder 2002). Kulu and Vikat (2007) show that couples in single family houses in Finland have a likelihood of first conception that is 53 percent higher than couples living in apartments and that a large part of the initial fertility variation across housing types is associated with selective moves, indicating the importance of housing for the decision to have a child. Couples in Austria have in addition been shown to change their housing situation in the time period when they are waiting for their child to be born (Kulu 2008). Yi and Zhang (2010) consider all types of housing, both owned and rented, and hypothesize that many households may be constrained to have any children at all, to the extent that owning or renting a house of a sufficient size may be a precondition for having children. Using aggregate level data and a cointegration analysis, they find that a 1 percent increase in house prices is associated with a 0.45 percent decrease in total fertility rates in Hong Kong over the period 1971 to 2005.

While these studies support that studying housing as an investment cost of having a child is appropriate, they also suggest that housing demand may be driven by the desire to have a child. In the option value framework, the desire to have a child is however considered to be given, and just matter of timing with respect to house prices, which reduces the concern for reverse causality. However, such potential endogeneity will be important to deal with in our empirical analysis.

Since future house prices are relatively uncertain, and are likely to be considered to be so by many households, when house prices increase, households that expect them to decrease over time have an incentive to delay the fertility decision and its investment cost (Terrones 2004; Chen 2006). As Chen (2006) finds, when studying the associated effect between Swedish house prices and consumption, a large share of the short-run variation in house prices is transitory[5].

[5]Chen (2006) notes that short-run housing variation only has little effect on consumption, given that house-

There is thus scope to investigate the option value effects of house prices on the fertility decision.

The option value literature that studies fertility behavior has to the best of our knowledge not explicitly dealt with the aspect of the investment cost of having a child in the value of the option to postpone the fertility decision. There is however empirical evidence pointing to the relevance of doing so. Using individual level data, Clark (2011) finds that being on an expensive housing market is associated with a delay of first births by three to four years in U.S. metropolitan areas, after controlling for education, ethnicity and labor market participation. Enström Öst (2012) compares the housing market entry and family formation between three different cohorts in Sweden (born in 1956, in 1964 and in 1974). The cost of home ownership is found to be more negatively associated with having a child for young adults born in 1964 and 1974, who experienced a rather turbulent housing market and an increasing housing shortage at the time of entry to the housing market, compared to those born in 1956, who instead experienced a large housing supply.

The third and last relevant factor that impacts the value of holding the option to postpone fertility, following Damodaran (2002), is the cost of delaying fertility. This cost can be thought of as the increasing risks and difficulties of childbearing at older ages. Time to conception and the risk of spontaneous miscarriage both tend to increase with maternal age, and the effectiveness of assisted reproduction technology declines with maternal age; the two latter at least after age 30 years (see e.g. de Mouzon et al. 2012; Nybo Andersen et al. 2000; Leridon 2004). This implies that there should be different values of holding the option to postpone fertility between women of different ages: the older a woman is, the smaller is the value of the option to postpone the fertility decision since the cost of delaying is higher. Indeed, such differential value between young and old women is in line with the model prediction and empirical results presented by Asphjell et al. (2013)

Summarizing these factors, the value of holding the option to postpone fertility is expected to increase with the investment cost of children, if such is prone to uncertainty, which house prices are. Further, the value of holding the option to postpone fertility is expected to decline with the cost of delaying fertility, which increases with age. The prediction of a differential fertility response between women of different ages from rising house prices has support in the small literature that studies this topic: L&M report results that suggest that there is a positive fertility premium for older women compared to younger women associated with a four year house price change. These results are however not interpreted in light of an option value effect.

To sum up the theoretical framework, in a simple, static economic framework of fertility, in which children are considered to be durable consumption goods, the fertility decision is made subject to life time wealth and to the investment cost of having a child. House prices are thus expected to influence fertility behavior through either a wealth effect, operating through a wealth and/or a credit channel and influencing owners and renters differently; and/or through an option value effect, which affects young and old fertile women differently since the options to postpone fertility that they hold, differ in value. Against this framework and the previous literature presented in this section, we proceed by outlining the purpose and research questions of our study, and the testable theoretical predictions that constitute the hypotheses for our empirical analysis.

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holds have the right anticipation on house prices, i.e. that short run fluctuations are transitory.

3 Purpose, research questions and theoretical predictions

In the current literature on housing wealth and fertility, the identification of a wealth effect is based on the assumption that while the wealth and/or the credit channel should affect the behavior of house owners and not that of renters, higher house prices should affect potential first-time house owners and current house owners who might upgrade to a bigger house with the addition of a child equally (Dettling and Kearney 2014; Lovenheim and Mumford 2013). However, as is recognized by for example Attanasio et al. (2009), there may be systematic differences between owners and renters in terms of their age, which may in turn correlate with different behaviors or preferences. In particular, house owners are likely to be older than renters on average.

The option value theory suggests that holding the option of being able to postpone the fertility decision is a value that must be included in the cost-benefit analysis of having a child. Since the fertility risks increase with age, the cost of postponing childbearing also should increase with age, and therefore, the value of the option to postpone the fertility decision should decrease with age. Further, with higher house prices, there is an incentive to wait and see if the house prices, that are thought of as the investment cost of having a child, decrease. This implies that when house prices increase, young fertile women are more likely to postpone childbearing than old fertile women are.

While D&K and L&M control for age effects on fertility, they do in most specifications not control for such effects interacted with house prices, and when they do, it is not along with also controlling for house ownership rates interacted with house prices. Their specifications may thus suffer from omitted variable bias. In particular, the wealth effects may be overestimated if house ownership also captures an age and option value effect. Further, while both D&K and L&M investigate timing effects of fertility, explicit timing considerations that might be taken by households that are planning to have a child are not dealt with.

We aim to contribute to this literature by adding the value of the option to postpone the fertility decision, and thus to postpone the investment cost of having a child, to the study of how housing wealth affects fertility behavior. For brevity, we call this the option value effect. As such, we also add to the option value literature by studying house prices as the investment cost of having a child.

Identifying an option value effect based on the assumption that such effect should differ between women of different ages has merit in that age is not a choice variable. Further, the identification of an option value is also based on the assumption that house prices only reflect an investment cost for having a child, and not also current costs for any type of housing. As argued by Chen (2006), Sweden is therefore an attractive country to study since the cost of living in rented housing is regulated; regulated rents are less likely to follow the market prices for one family dwellings than rental rates that are determined on the regular housing market are. Studying Sweden is also motivated by the fact that high quality data in terms of its credibility and availability has been accessible within the financial and time constraints of this paper (although the used data has certain limitations, that are be discussed in detail below and iterated upon throughout the paper).

Further, a differential value of the option to postpone the fertility decision between young and old women is likely to be relevant for a study using Swedish data, as it has become increasingly difficult for young adults to establish themselves on the Swedish housing market and to finance the purchase of a house suitable as a family residence (Bergenstråhle 2009; SOU 2007; Statistics Sweden 2015a).

While previous literature has examined whether fertility rates respond to house price movements and find evidence of such relationship, this literature is small and relatively new. In particular, since no previous research, to the best of our knowledge, has presented evidence for such relationship for the case of Sweden with its special feature of a regulated renting market, we aim to investigate if house price movements affect fertility behavior in Sweden. In particular, and with respect to the key predictions from Becker's framework, we aim to answer the following two research questions:

1. *Can a wealth effect contribute to explain the relationship between Swedish house prices and fertility rates?*
2. *Can an option value effect contribute to explain the relationship between Swedish house prices and fertility rates?*

It should be noted that a wealth effect and an option value effect are not mutually exclusive. Instead, theory and previous literature gives reason to believe that both can contribute to explain the relationship between house prices and fertility rates. We will therefore not test one effect *against* the other, but instead test for both effects simultaneously.

Limitations of scope

A full treatment of the wealth effect would require an allowance for transaction costs, the cost and ability to finance house purchases, and the use of housing equity to finance fertility consumption. Given the potential for complicated interactions, and given the limitations in what data has been available for this study, we choose not to identify a single specification for each particular channel (the wealth and/or the credit channel) that the wealth effect can operate through. An additional delimitation that we do with regards to the wealth effect and also with regards to the cost effect of housing, is that we study permanent single family home dwellings and exclude other forms of housing wealth. Owned apartments constitute a large share of the housing stock in Sweden and it would have been interesting to include this wealth category also. However, data for house price developments of shared apartments does not exist in the same detail as for house price developments of single family home dwellings, and not with enough detail to fulfill the purpose of this study.[6] As such, due to data availability, we will only focus on housing wealth in the form of single family home dwellings. However, this contributes to the purpose of external validity, as this is the common approach in the literature.

A full treatment of the fertility behavior associated with higher house prices would require the study of both women and men; of both the timing of fertility and completed fertility effects; and of different orders of births (i.e. of the number and spacing between the children a woman has). Considering the value of comparability to previous literature that does not study the effect on men or on completed fertility, and given that our data does not allow us to study different

[6]This study would require a house price measure that is available at municipality level and that is adjusted for quality of the property, to ensure that house price fluctuations does not capture the variation in quality or size of the property. House prices of shared apartments is not available with this detail and we will therefore not include it in this paper.

order births, we do not study men's fertility, completed fertility effects or different order births. While the latter would be interesting, it does not constitute a limitation to identifying wealth and option value effects to not study it. Timing effects and completed effects are however likely to be linked (Schmidt et al. 2012), why our conclusions regarding the timing of fertility can lend some insight to the question of completed fertility.

A full treatment of the option value effect, in particular with respect to completed fertility, requires that it is also analyzed within a dynamic modelling framework. We will allow for dynamic relationships in our empirical investigation, but the model is somewhat limited by data availability, as will be discussed further in section 4.3.1. Lastly, while we will close the discussion section with a brief sketch on how the option value effect could be analyzed in a structural dynamic model, it falls outside the scope of this study to develop and solve such model.

3.1 Theoretical predictions

This section elaborates on the relevant theoretical predictions with respect to the research question of this study and condenses them into the testable implications that will be the focus for the empirical investigation. In total, four hypotheses are formulated: two regarding the wealth effect, and two regarding the option value effect. For each of the effects, one hypothesis deals with house price levels and one with house price changes. To be clear, and with respect to the limitations of scope of this study, all four hypotheses deal with timing effects of fertility and not with completed fertility effects.

Since D&K find that higher house price levels are significantly associated with wealth effects on fertility rates, while L&M reach the conclusion that house price levels are not significantly associated with wealth effects on fertility rates but that instead house prices changes are, we will consider both. The permanent income hypothesis predicts that individuals will only adapt their consumption in response to permanent changes in their wealth, while the value of the option to postpone the fertility decision predicts that individuals respond to the expectation of such changes being temporary. We make no prediction with respect to the degree of permanency of the house price changes. Instead, the estimates on fertility responses to house prices changes will lend insight to how permanent or transitory households perceive them to be.

Wealth effect approach

Since children are considered to be durable consumption goods for which the income elasticity is assumed to be positive, the demand for children should respond positively to an increase in housing wealth. Such wealth effect on fertility rates is predicted to operate through a wealth channel (a realized wealth effect), and/or through a credit channel (a relaxed borrowing constraint).

The credit channel and the wealth channel predict similar relationships between house prices and the household fertility decision: the budget constraint with respect to which the fertility decision is made is affected for house owners but not for renters when house prices change. Since there is no theoretical prediction as to which channel that would be dominating, nor any clear guidance from previous literature, we refrain from making such prediction. The results from the empirical analysis will however be discussed in the light of these channels.

Further, economic theory does not give any prediction on what the sign of the relationship between housing wealth and fertility rates should be. Rather, increased wealth could be associated with both higher and lower quantity of children, depending on the relationship between the income elasticities of demand for quality and quantity of children respectively. Previous literature does however suggest that the relationship of the income elasticities is such that increased wealth is associated with higher quantity of children (Lovenheim and Mumford 2013; Dettling and Kearney 2014).

Lastly, economic theory predicts that both increased life time wealth and short-run realized wealth effects are associated with higher demand for children. As previously mentioned, the empirical literature that investigates the relationship between housing wealth and fertility is not conclusive on this point. We therefore make no competing hypotheses between house price levels and house price changes regarding which that is expected to best capture a wealth effect on fertility.

Consequently, regarding the wealth effect, we formulate the following two hypotheses:

Hypothesis 1: Higher house price levels give rise to differential fertility rate effects between house owners and renters. This differential effect is in the form of a positive fertility rate premium for owners relative to renters.

Hypothesis 2: Positive house price changes give rise to differential fertility effects between house owners and renters. This differential effect is in the form of a positive fertility rate premium for owners relative to renters.

Option value effect

Since children are considered to be durable consumption goods for which the income elasticity is assumed to be positive, the demand for children should respond negatively to an increase in the investment cost of having an additional child.

The cost of buying a (larger) house is one of the major investment costs that is associated with having a child. House prices are however characterized by uncertainty. If a household is planning to have a child at a time when house prices are high, it has an incentive to wait and see if house prices will decrease. The option value theory thus predicts that there should be a positive value associated with the holding the option to postpone the fertility decision, which should be included in a cost-benefit analysis of having a child.

The option value theory also predicts that there is a negative value associated with holding the option to postpone the fertility decision, which are the risks of fertility that increase with age, and thus with postponement of childbearing. There should in other words be a higher value associated with holding an option to postpone fertility that has longer time to expiry date, or menopause. Therefore, with higher house prices, younger women should have a higher propensity to postpone their fertility decision than older women should have.

The theory and previous research does however give no guiding as to whether house price levels or changes to those levels are what matters for the fertility behavior.

Consequently, regarding the option value effect, we formulate the following two hypotheses:

Hypothesis 3: Higher house price levels give rise to differential fertility effects between young and old fertile women. This differential effect is in the form of a positive fertility premium for old fertile women relative to young.

Hypothesis 4: Positive house price changes give rise to differential fertility effects between young and old fertile women. This differential effect is in the form of a positive fertility premium for old fertile women relative to young.

4 Data and empirical approach

In this section the data approach, data and the estimation strategy that is used to empirically test the four hypotheses regarding the relationship between housing prices and fertility are described. Firstly the data approach is presented, and secondly the main variables of this study are described. Lastly, the empirical specification is presented, and we elaborate on the identifying assumptions underlying it, the threats to identification, and our means of dealing with these threats.

4.1 Data approach

Inferring causality between house prices and fertility rates is not straightforward. Relationships could run either way or be related to some other factor which affects both processes. While micro-level data facilitates disentangling causality and outcomes, and is used by both D&K and L&M, such data has unfortunately not been available for this study.

Broadly speaking, two types of data approaches remain: i) using aggregate times series data, or ii) using regional level panel data. As fertility rates and house prices vary between geographical regions, it is attractive to use a panel data approach in order to exploit this variation. The findings of Campbell and Cocco (2007) suggest that it is important to consider regional heterogeneity when estimating the effects of house prices on consumption. We therefore use time series data with a municipality level panel: our data set consists of a strongly balanced panel consisting of yearly observations at municipality level for a number of variables from the Statistics Sweden (Statistiska Centralbyrån, in Swedish). The number of years included in the analysis is restricted by data availability, in particular by the ownership data. With the full set of control variables, the data set contains observations from 290 municipalities for the years 1993-2014, with a total of 12,496 observations ($municipalities * years * agegroups$).

Sweden has 290[7] municipalities that are divided into 20 counties. Population density varies between the municipalities, from 0.2 to 5,074 inhabitants per square kilometer, with a mean 128 inhabitants per square kilometer. Five municipalities were created after 1993, thus data for those units will be missing before the year they were founded [8]. Furthermore, in the main time span 1993-2014 the county definition has changed, however, throughout the whole time series the 2015 county definition will be employed [9].

There are four main data requirements for the test of our four hypotheses. First of all, fertility data and house price data are needed. In addition, a measure of the ownership rate of one family houses is needed in order to test the wealth effect in hypothesis one and two. Lastly, the fertility and ownership rate data must be separable across different age groups in order to test hypothesis three and four related to the option value effect. These main data requirements are described in detail in the following subsections. In section 4.3.2 the control variables are presented. In table 4.1 a short description including mean and standard deviation, source, and

[7] The current municipality division is based on the 2011 definition. All municipality based data has been updated in order to match the 2011 definition.

[8] The municipalities are Bollebyggd and Lekeberg, created in 1995, Nykvarn, created in 1998, and Knivsta, created in 2003.

[9] Gothenburg, Bohuslän, Älvsborg, and Skaraborg counties formed Västa Götaland county in 1993. Skåne county was created in 1996 by merging Malmöhus and Kristianstads counties.

Table 4.1: Descriptive statistics of variables

Variable	Mean	Standard deviation	Source	Description	Geographical level
House price (SEK 100,000)	18.562	12.742	Statistics Sweden	House price levels, permanent one dwelling houses. Taxation value times the purchase-price coefficient.	Municipality
House price change (SEK 100,000)	1.245	2.481	Statistics Sweden	House price changes, over two years, one dwelling houses. Taxation value times the purchase-price coefficient.	Municipality
Age specific fertility rate (ASFR)	0.895	0.244	Statistics Sweden	Age specific fertility among young (20-29 yrs) and old (30-44 yrs) women. Calculated by the number of live births per woman in a certain age, divided by the average female population in that age.	Municipality
Ownership rate	0.175	0.112	Statistics Sweden/ FoB90	Ownership rates among young (20-29 yrs) and old (30-44 yrs) individuals: the number of individuals in each age group living in a one family dwelling house in 1990, divided by the total number of individuals in the age group in 1990. The ownership rate is corrected by the number of one family dwellings actually owned in 1990.	Municipality level
Post-secondary education	0.375	0.131	Statistics Sweden/ UREG	Share of women with any post-secondary education, per young (20-29 yrs) and old (30-44 yrs) women.	Municipality
Female employment rate, municipality	0.722	0.1054	Statistics Sweden/ RAMS	Share of women in employment per age group of fertile women (young (20-29 yrs) and old (30-44 yrs)), defined as working at least one hour per week during the month of November.	Municipality
Total employment rate, county	0.750	0.029	Statistics Sweden/RAMS	Number of total people in employment as share of total population on county level, defined as working at least one hour per week during the month of November.	County
Average income (SEK 1,000)	247.248	41.898	Statistics Sweden/ HEK	Average real income, deflated by the shadow-CPI, per 31 December each year.	Municipality

Note: All data is weighted by total female population in each age group per municipality and year.

geographical availability of all the data that is used in the empirical analysis is presented.

4.2 Main variables of interest: fertility rates, house prices and ownership rates

In this section the main variables of interest will be defined and presented. Firstly fertility rates, then house prices, and finally ownership rates.

Fertility rates

The standard fertility measure in the literature (see e.g. Dettling and Kearney 2014; Lovenheim and Mumford 2013; Yi and Zhang 2010) is the total fertility rate (TFR), or the age specific fertility rate (ASFR) for a certain age group of women. The fertility rate for women in a certain age, individual age ASFR, is given by

$$ASFR_{iat} = \frac{B_{iat}}{E_{iat}} \tag{4.1}$$

where B_{iat} represents the number of live births that women of a particular age a has given birth to, in a certain municipality i, and in a given year t. E_{iat} is the number of person years of exposure to the age a, in a certain municipality i, during the time period t. In other words, the fertility for women of a certain age, is given by the number of births to women of that particular age, divided by the total number of women of that age. For example, the ASFR for women in the age of 30 is given by the number of births to women aged 30, divided by the total number of women in the age of 30. In other words: the ASFR measures the propensity for a woman of a specific age to have a child, and not the actual number of children born by women of that specific age.

Consequently, the ASFR can also represent the fertility rate of a certain age group g, which is given by

$$ASFR_{igt} = \sum_{a} ASFR_{iat} \tag{4.2}$$

where $ASFR_{igt}$ is the fertility rate for women of age a belonging to age group g, which is given by taking the sum of the individual age specific ASFRs to women belonging to this age. For example, the ASFR for women in the age group 20-29 is given by summing the individual age ASFRs for women in the age 20-29.

Likewise, the TFR is given by summing all the individual age ASFRs for all fertile ages. The definition of fertile ages differ in the literature. L&M use the ages 15-49 due to data availability, whilst D&K use the ages 20-44. In this study, fertile ages are defined as the ages 20-44.

In order to test hypotheses three and four, the fertility data must be split between two age groups of young and old women. Following the definitions in D&K, the young age group is defined as the ages 20-29 and the old age group is defined as the ages 30-44. While somewhat arbitrary, this definition has merit since the risks and difficulties with childbearing increase from around 30 years of age for the woman, as discussed in section 2.2.2. Consequently, we calculate the ASFR for young women aged 20-29 and for old women aged 30-44 in each year and municipality.

$$ASFR_{young} = \sum_{a=20}^{29} ASFR_{iat} \tag{4.3}$$

$$ASFR_{old} = \sum_{a=30}^{44} ASFR_{iat} \tag{4.4}$$

Considering age groups has merit in that age is not a choice variable, unlike home ownership. Further, a strength of the ASFR measure is that it measures fertility independent of the age group composition of women: it captures the propensity to have a child and not the actual number of births. This is an important feature as the size of different age groups varies over time and could bias the fertility measure otherwise. In other words, by employing the ASFR we control for age structure effects. We therefore reduce the potential reverse causality in form of an effect on house price from changes in the age composition of the population. Consider for example the fertility of women 30 years of age compared to the fertility of women 40 years of age. The fertility rate of the former is higher than that of the latter. Thus, if for a specific year there are more 30 year old women than there are in other years, the fertility rate for old women will be boosted by this age effect. Instead, by controlling for such age structure effect, the ASFR measure instead gives a hypothetical picture of how many children a woman would have on average in a given year if she would adhere to the fertility pattern prevailing in that year[10].

As this data is based on the number of live births, it does not capture fertility decisions that result in miscarriages or for individuals that cannot reproduce. This is a limitation to the data albeit a small one. Another limitation with the fertility data is that we cannot distinguish between first and higher order births. As there is a correlation between buying a house and having the first child, as described in section 2.2.2, it would be interesting to distinguish the effects of a house price increase on first versus higher order births. This is however not possible with the available data.

House prices

The second important data source for this study is house prices. Malmberg (2010) uses the mean purchasing price for one family permanent housing in Swedish municipalities. This measure is however sensitive to differences in housing standard, house size and geographical location of the house. For example, in a given period, houses of a lower standard or in a more expensive area could be sold more than in another period. This can produce misleading data on the house price development as the sold objects are not comparable from one period to another.

D&K and L&M overcome this issue by using the Federal Housing Finance Agency (FHFA) house price index for the U.S. It uses repeat transactions on the same physical property units, which helps to control for differences in the quality of houses across the sample. For this reason, the FHFA house price index is described as a "constant quality" house price index. Also Campbell and Cocco (2007) use a constant quality measure of house prices for the UK to avoid the measure being affected by changes in the composition of sold houses.

The Swedish real estate house price index is also a "constant quality" house price index, as it is constructed by using actual house prices adjusted for the tax assessment values of houses. However, this index is only available at an aggregate national level, which is unsuitable for the pursue of this study. Chen (2006) uses an approach similar to other authors in order to construct an aggregate time series of Swedish house prices, trying to control for the composition of sold houses. The value of the housing stock is calculated as the tax assessment value [11] of owned permanent and seasonal homes respectively multiplied with a purchase-price-coefficient [12] of each type. This measure is used as the value of gross housing wealth. The variables used by Chen (2006) are available on municipality level on a yearly basis. We are therefore able to follow this approach, and thus also the principle used by D&K and L&M in using a "constant quality" measure, deflated by CPI. In section 8.1 in appendix, we describe in detail how this house price measure is calculated.

The adjusted house price measure employed in this study is used both as levels, reflecting housing lifetime wealth, and as a change between years. The house price change is calculated

[10] An alternative measurement of fertility is the completed fertility rate. This measurement takes into account the actual number of births women aged 50 years have during their reproductive years. A limitation to this measure is however that it does not capture fertility timing effects, as it is not influenced by short-run economic fluctuations. Instead it captures the actual life-time fertility of a woman born in a certain year. As the aim of this paper is to study the short run fluctuations in fertility, and not completed fertility, the ASFR measure is best suited. Another limitation of the completed fertility rate is that data would only be available for women born 1965 or earlier (as those women have past their fertile years) (Statistics Sweden 2002).

[11] The Swedish tax authority conducts general tax assessment of real estate periodically. Such tax assessments have been made in 1996, 1990, 2003, 2006, 2009 and 2012.

[12] The purchase-price-coefficient is calculated as the unweighted average of the sum of all ratios, for sold houses, between the purchase price and the most recent tax assessment value (Statistics Sweden 2015b).

as the change between two years, $2yr\Delta HP = HP_{it} - HP_{it-2}$, following the approach of L&M.

One limitation to using a gross measure of wealth is that it can upwards bias the effect of wealth on consumption. However, to produce the series of net housing wealth data, the series of mortgage home loan data is required. Restricted by data unavailability, we employ total housing wealth data.

Ownership rates

The third data requirement for this study is ownership rates for one family houses. A concern with including house ownership rates is that ownership rates are likely to be endogenous to fertility rates and child bearing outcomes. Therefore, in line with D&K, the ownership rates employed in the study will be taken prior to the first year of our empirical analysis. To be clear: the ownership rates used in our estimation do not change over the years but is fixed to the year of 1990, and the empirical analysis covers the years 1993-2014. This reduces endogeneity concerns, but does not fully eliminate them, as ownership rates at baseline are likely to predict ownership the following years rather well. In section 4.3.2 a robustness test for this is outlined, and we are in general careful to interpret the estimated effect of house ownership rate as causal.

To test hypotheses 1 and 2 about differential fertility effects among house owners and renters, ideally we would need individual level ownership data. This has however not been available. Instead we construct a measure of average ownership rate per municipality in 1990.[13] This measure is constructed using two separate data sets from the same database: FoB90 from Statistics Sweden. The first data set contains information on the number of individuals that live in a one family house per municipality in 1990. The data is available for five year small age groups for the ages 20-65+. Should only this data be employed to construct ownership rates, divided by the population size per age group and municipality in 1990, the ownership rate would be overestimated since not all individuals living in a one family dwelling are owners or affected by ownership status (either their own or their potential partner's).

To obtain more correct ownership measures, the ratios of people living in a house have to be adjusted. This adjustment is done by employing a data set on people's living patterns, including their ownership status. These two data sets have to be combined as the first data set contains important information on living patterns across age groups, whereas the second data set contains information on ownership status, however, not by age group. The adjustment is done by calculating an overestimation rate for all people living in a one family house,

$$Overestimation_{i,1990} = Living\_house_{i,1990} - Owning\_house_{i,1990} \tag{4.5}$$

$$Overestimation\_rate_{i,1990} = \frac{Overestimation_{i,1990}}{Living\_house_{i,1990}} \tag{4.6}$$

In other words, the overestimation rate corresponds to the difference between the total number of people living in a house and the total number of people owning a house, divided by the number of people living in a house. The types of one family dwellings that are not regarded to reflect

[13] The ownership rates are based on data from the 1990 census survey, but based on the 1992 municipality definitions. This implies that we lack ownership data for the five municipalities created after 1993: Nykvarn, Knivsta, Gnesta, Trosa, Bollebyggd and Lekeberg

ownership are for example rented ones. On average, the ownership rate in a municipality is overestimated by 20 percent, with a minimum overestimation rate of 10 percent and a maximum overestimation rate of approximately 50 percent.

Since the second data set does not contain any age group information, we make the assumption that the overestimation of ownership rates is equal across age groups within a municipality. Using this assumption, municipality level age group data of the number of people living in a one family home in 1990 is weighted by the overestimation rate per municipality. Through this procedure, ownership rate data per age group and municipality in year 1990 is obtained.

Following D&K, the ownership measure is constructed for different demographic groups. D&K use ownership rates for women 20-29 years old and for 30-44 years old respectively, and also depending on whether the woman is white, black or hispanic. They thus use six different ownership rates. Limited by data availability, we only use two different rates: for young and for old individuals respectively. The assumption we make is therefore that women that are young and old respectively are affected by their own ownership status and/or by the ownership status of their potential partner, that is assumed to be in the same age group as her. This assumption favors creating relatively large age groups.

The main limitation to this measure is that it does not capture the true ownership rate in the different age groups, which makes it likely to over- or underestimate ownership in the different groups. Another limitation is also that we do not capture the ownership status of only women. However, we believe this measurement error to be limited, and that it is reasonable to assume that women are affected by a wealth effect if their partner own the house in which she lives, and that our estimations would become more biased if we chose to exclude these ownership rates.

4.2.1 A first look at data

Table 4.2 presents detailed descriptive statistics of our main variables of interest, weighted by the female population per municipality, age group and year. Women 30-44 years of age will subsequently be called “old fertile women”, or just “old”, and similarly, women 20-29 years of age will be called “young fertile women” or “young”. During the period 1993-2014, the average ASFR for young women is 0.78 and it is 0.97 for old women. The average ownership rate of one family dwellings in 1990 was 24 percent for individuals aged 30-44, with a maximum ownership rate of 41 percent. For individuals aged 20-29, the corresponding values are 8 percent and 25 percent. In 2007 the average ownership rate of one family dwellings was 8 percent for individuals aged 20-29 and 46 percent for individuals aged 30-49 (Statistics Sweden/HEK). For women, the corresponding numbers were 9 and 45 percent[14]. The similar values between all individuals in an age group on the one hand, and only women in the age group on the other, indicates that our ownership rates for all individuals and not only for women in 1990 can be a relatively good proxy for female ownership rates. Further, the higher values in 2007 compared to 1990 indicate either that ownership rates have grown apart for young and old over the course of the years in our panel data set, or that our assumption of equal overestimation across all age groups underestimates the average among old individuals. However, in total, these figures confirms that there is a positive correlation between ownership rates and age.

[14]This data has not been possible to use in our econometric analysis for two reasons: it is not possible to disaggregate on a municipality level, and it is likely to be endogenous as it is taken in 2007.

Table 4.2: Descriptive statistics, main variables of interest by age groups, 1993-2014

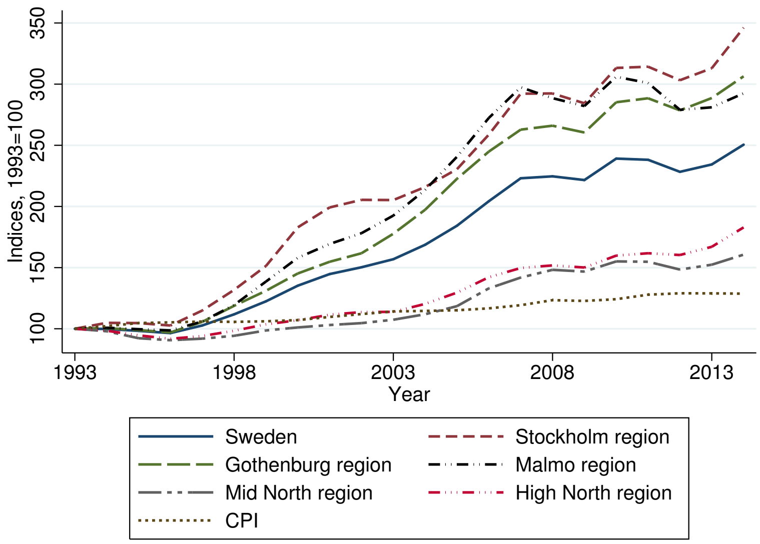
	Fertility rate		Ownership rate		House price (SEK100,000)		2 year house price change (SEK100,000)	
	mean	min,max	mean	min,max	mean	min,max	mean	min,max
Total	0.89 (0.24)	[0.15, 2.12]	0.17 (0.11)	[0.00, 0.41]	18.56 (12.72)	[2.29, 93.81]	1.25 (2.48)	[-10.81, 22.99]
Old	0.97 (0.22)	[0.20,1.99]	0.24 (0.10)	[0.01, 0.41]				
Young	0.78 (0.25)	[0.16, 2.12]	0.08 (0.05)	[0.00, 0.25]				

Notes: All data is weighted by the total female population in each age group per municipality and year. Standard deviation reported in parentheses.
Source: See table 4.1

The average house price in our data set is SEK 1,856,000 and the average two year house price change is SEK 125,000. However, the relatively large standard deviations imply substantial variation to these values in the data set. In particular, the main identifying variation in our study comes from the housing boom that began in the mid 1990s and that was characterized by large increases in house prices that occurred differentially across municipalities, as illustrated by figure 4.1. Compared to in 1993, the average house costs 2.5 times more today in Sweden. In the urban regions[15] around the three largest cities of Sweden (Stockholm, Gothenburg and Malmo), house prices are 3-3.5 times higher today than in 1993. In non-urban regions in the north of Sweden, house prices have instead increased by 60-80 percent over the same period. On average, house prices have increased substantially more than average real consumer prices, that are approximately 30 percent higher today compared to in 1993.

[15]The definition of the urban areas comes from Statistics Sweden, including the urban areas of Stockholm region (26 municipalities), Gothenburg (13 municipalities) and Malmo (12 municipalities)

Figure 4.1: Real house price indices (aggregate and for selected urban and non-urban regions) and real consumer price index (aggregate), 1993-2014



Note: The figure displays the development over the period 1993-2014 of real house prices indexed to year 1993 in selected regions in Sweden, along with the consumer price index (CPI) indexed to 1993. Data source: Statistics Sweden, the authors' own calculations.

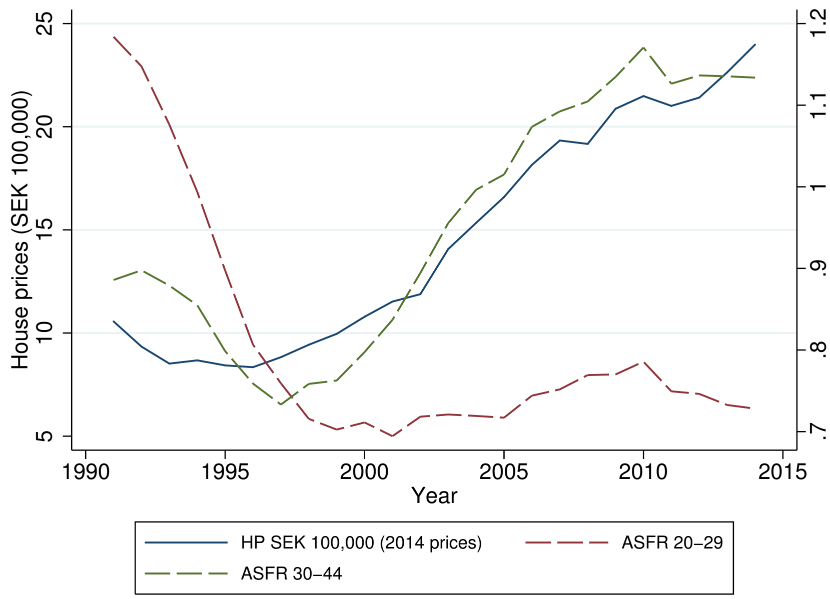
The simple correlation between fertility rates and house prices is -0.65 for young fertile women and 0.81 for old fertile women over the period 1993-2014, which are relatively strong correlations. For two year house price changes, the corresponding correlations are -0.44 and 0.37, which are moderately strong correlations. This correlation is also illustrated by figure 8.1 in appendix, which shows the linear prediction plot for age specific fertility rates on house prices. Following the argument made by Ström (2010), the house price variables (both for levels and house price changes) will be used with a one year lag in our econometric specifications. According to Ström (2010), it takes on average 6-7 months to become pregnant from the decision of wanting to become pregnant has been made. Then there is an additional 9 months until the birth of the child. Thus, there is a natural lag of on average 15 months between the event of the decision to have a child to the event of the birth. With a one year house price lag, we capture the house prices for most of the months during which a fertility decision is made on average. Further, a correlation table[16] for ASFR for young and old women and various lags of house prices shows that the correlation is strongest for one or two year lags in house prices, and for four years in house price changes and the ASFR of old women. As is explained in section 4.3.2, we will check the validity of the decision to use a one year lag by also running the regressions using various house price lags.

Related to this correlation, figure 4.2 shows the development of house price levels and house prices changes together with the development of fertility rates among young and old women respectively over the period 1993-2014. We firstly note that fertility rates among young and old women have had distinct developments since the early 1990s until today. Both graphs

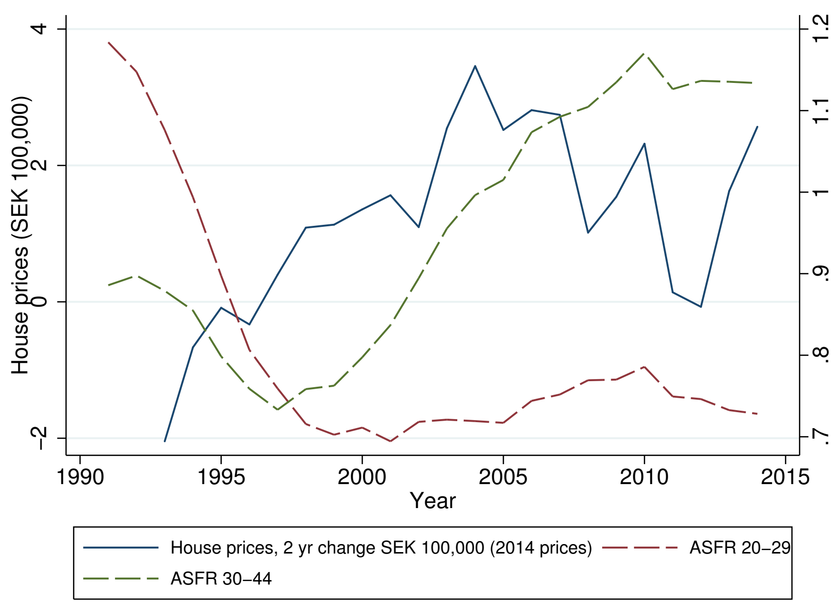
[16]Not displayed in the paper but available upon request.

in figure 4.2 display a trend shift, from high to low fertility among young women and from relatively low or volatile to high fertility among older women. This trend will be addressed in our empirical analysis. A second thing to note is that the development of house prices and fertility rates among old women exhibit a positive relation. Such positive relationship is also observed between house price changes and fertility rates among old women in figure 4.2. Since old women are assumed to be affected by higher ownership rates on average, this provides *prima facie* evidence for the wealth effect explanation of the relationship between house prices and fertility rates.

Figure 4.2: House prices and age specific fertility rates for young and old women, 1993-2014



(a) House price levels and age specific fertility rates



(b) House price changes and age specific fertility rates

Note: The figures display the development of house price levels (a) and two year house price changes (b) in 2014 prices along with the age specific fertility rates for women 20-29 years of age and for women 30-44 years of age. Data source: Statistics Sweden, the authors' own calculations.

In relation to the trend shift in fertility rates among young and old women, there has also been a trend shift in the distribution of at what age women give birth to their first child, as displayed in the figure 1.2 in the introduction. The figure shows the distribution of first birth across four age groups of women: 0-24, 25-29, 30-34 and 35- years of age[17], over the years 1993-2014. Since the beginning of the 1990s, there has been a clear shift of first births to occur at later ages. In 1993, women of 24 years of age or below gave birth to almost twice as big a fraction of all first births per year compared to today: approximately 40 percent in 1993 to approximately 20 percent in 2014. In a similar manner, women 30-34 years old almost doubled their share of all first births and now account for just below 30 percent of all first births per year. Also noteworthy is the constant upward trend for women 35 years of age and older, and a slight decrease in the share of total first birth that women 25-29 years of age account for each year. In total, figure 1.2 shows a clear pattern of increasing postponement of fertility. As mentioned in the introduction, the investment costs of having a child in terms of buying a house have increased substantially during the same time period, as shows by figures 4.1 and 4.2. This provides *prima facie* evidence for the option value explanation of the relationship between house prices and fertility.

These patterns with respect to age, ownership, fertility and house prices are however only suggestive; more formal and thorough analysis is indeed needed to investigate if the wealth effect and/or the option value effect can explain the relationship between house prices and fertility rates. This is the topic for the next section.

4.3 Empirical strategy

4.3.1 Econometric specifications

To empirically test our four hypotheses, we estimate the following two econometric specifications as our baseline specifications using OLS. They are identical in setup but differ in whether house price levels (equation (4.7)) or house price changes (equation (4.8)) are analysed in relation to age specific fertility rates. The first baseline specification is

$$\begin{aligned} lnASFR_{igt} = \beta_{l0} + \beta_{l1}HP_{it-1} + \beta_{l2}HP_{it-1} * Old_g + \beta_{l3}HP_{it-1} * Own_{ig} + \\ \beta_{l4}Old_g + \beta_{l5}Own_{ig} + \mathbf{X}'_{it}\gamma_l + v_t + u_i + e_{it} \end{aligned} \tag{4.7}$$

where $lnASFR_{igt}$ is the natural logarithm of age specific fertility rate for women in municipality i, year t, and the young/old age group g, which is our dependent variable. HP_{it-1} is the municipality average house prices with one year lag, $HP_{it-1} * Old_g$ is house prices interacted with an age group dummy that is equal to 1 if the age group is old fertile women and 0 if the age group is young fertile women, and $HP_{it-1} * Own_{ig}$ is a variable interacting house prices with house ownership rate per age group and municipality. The l index on the β and γ coefficients indicates *levels*, as these coefficients belong to the house price level specification.

The second baseline specification is

[17] These age groups do not perfectly match the young and old age groups that we study due to limitations in available data.

$$lnASFR_{igt} = \beta_{c0} + \beta_{c1}2yr\Delta HP_{it-1} + \beta_{c2}2yr\Delta HP_{it-1} * Old_g + \beta_{c3}2yr\Delta HP_{it-1} * Own_{ig} + \beta_{c4}Old_g + \beta_{c5}Own_{ig} + \mathbf{X}'_{it}\gamma_c + v_t + u_i + e_{it} \quad (4.8)$$

where $lnASFR_{igt}$ is the natural logarithm of age specific fertility rate for women in municipality i, year t, and the young/old age group g, which is our dependent variable. $2yr\Delta HP_{it-1}$ is the municipality average house prices changes over two years with one year lag, $2yr\Delta HP_{it-1} * Old_g$ is two year house price change interacted with an age group dummy that is equal to 1 if the age group is old fertile women and 0 if the age group is young fertile women and $2yr\Delta HP_{it-1} * Own_{ig}$ is a variable interacting the two year house price change with house ownership rate per age group and municipality. The c index on the β and γ coefficients indicates *changes*, as these coefficients belong to the house price change specification.

In both baseline specifications, Old_g is a dummy variable equal to 1 if the group is old fertile women and 0 if the group is young fertile women. It is therefore in practice interpreted as age group fixed effects. The variable Own_{ig} measure the average house ownership rate in a municipality for young and old respectively, and this measure does not vary over years. $\mathbf{X}_{it}$ is a vector of control variables that are presented and motivated below in section (4.3.2), v_t is panel-invariant unobservable effects for each year, u_i is time-invariant unobservable effects for each municipality, and e_{it} the error term.[18]

Since our dependent variable is based on averages with different sized denominators, or in other words, with different sized female populations per municipality and age group, a small change in a relatively small group of women will be given larger weight than a bigger change in a larger group. To correct for this, we weight all the regressions by the number of women per municipality, group and year, in line with D&K.

Further, we follow Bhaumik and Nugent (2011) that estimate the value of the option to postpone fertility as a contemporaneous rather than cumulative effect, which implies that a static year-by-year specification is appropriate. We will however also run a dynamic panel data model as robustness check.

Lastly, the ASFR is used in log-form and house prices are estimated in level-form in all specifications to follow D&K and L&M, which also gives the estimated coefficients intuitive interpretations. The overall estimated pattern of coefficients and the conclusions are however robust to using house prices in log-form[19].

4.3.2 Identification and threats to identification

The identifying assumption underlying equations (4.7) and (4.8) is that house prices in a municipality are conditionally exogenous to the fertility decisions in the municipality. In other

[18] A double interaction between house prices, ownership rates and age groups ($HP_{it-1} * Own_{ig} * Old_g$ and $2yr\Delta HP_{it-1} * Own_{ig} * Old_g$) are not included in the specifications since an ownership effect conditional on age and vice versa does not contribute to answering the research questions of this thesis and there are no theoretical predictions with regards to such conditional effects. The estimated pattern of coefficients are however robust to controlling for such double interaction variables. These results are not reported in this paper but are available upon request.

[19] The results are not reported in the paper but available upon request.

words, house prices and age specific fertility rates should be uncorrelated conditional on the observables in the model, except for the fact that house prices increase household wealth and the cost of buying a house.

There are two main threats to this assumption. The first is that the relationship between house prices and fertility rates may be driven by an underlying unobservable factor and the second is that it may be driven by systematic sorting between municipalities. Both of these threats and how we address them is discussed in detail below. In addition to this, we are concerned that our results may reflect underlying fertility trends that differ between young and old fertile women, as indicated by figure 4.2, and that main explanatory variables may be biased due to reverse causality, as discussed in section 2.2.2. We therefore employ additional checks for this, which are also presented in detail below. Lastly, we also deal with heteroskedasticity and dependence in the disturbances as well as potential regression misspecification, the strategies of which, too, are described in detail below.

However, despite employing several measures to reduce the threats to identification, the coefficients on house prices should not be interpreted as the causal effect of house prices on fertility, as they may be biased. In order to credibly test our four hypotheses, we want to capture an age effect net of an ownership effect, and likewise, we want to capture an ownership effect net of an age effect. We therefore compare the coefficients across groups, making the identifying assumptions that i) if house prices capture the direct wealth effect, we would expect the coefficient to be larger for demographic groups with higher ownership rates, and ii) if house prices capture the effect of the value of the option to postpone the fertility decision, we would expect the coefficient to be larger for old fertile women relative to young.

The in total four interaction variables with house prices in our baseline specifications - $HP_{it-1} * Old_g$ and $HP_{it-1} * Own_{ig}$ in equation (4.7) and $2yr\Delta HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Own_{ig}$ in equation (4.8) - are therefore our four variables of interest. In particular, in both baseline specifications we interpret β_{l2} and β_{c2} as the differential effect of house prices on fertility rates of old women relative to young, and thus as a test for hypotheses 3 and 4. Similarly, we interpret β_{l3} and β_{c3} in both specifications as the differential effect of house prices on fertility rates between demographic groups with 100 percent ownership rates relative to zero percent ownership rate, and as such, a test for hypothesis 1 and 2. Given our previous discussions and the four hypotheses presented in section 3, we expect $\beta_{l2} > 0$ and $\beta_{c2} > 0$, and $\beta_{l3} > 0$ and $\beta_{c3} > 0$.

The main threat to the identifying assumptions underlying the comparison of coefficients across groups is that age and house ownership are positively correlated. Old age can thus reflect higher probability of house ownership, and higher house ownership rates in a municipality can reflect an older population. Therefore, these two effects must be estimated jointly. To be explicit, the interaction variables with house prices and Old_g and Own_{ig} respectively must both be included in the specifications.

Further, regarding the value of the option to postpone fertility and hypotheses 3 and 4, the underlying assumption is that house prices reflect investment costs of having a child. If rents for rented housing are determined on the housing market and follow housing prices, this may not be the case. Swedish rents are regulated by law to be negotiated between landlords and the Tenants' Union instead of to follow market prices. According to the National Board of Housing, Building and Planning (2010) in Sweden (Boverket, in Swedish), rents increased with on average 1 percent per year in real terms over the period 1995-2009, compared to the yearly average house price increase over the same time of approximately 7 percent. It therefore seems plausible to assume that Swedish house prices reflect the investment costs of having a child.

Baseline strategy for dealing with threats to identification

In this section we describe how the baseline specifications in equations (4.7) and (4.8) are designed in order to deal with the threats to the identification of conditional exogeneity between house prices and fertility rates.

The first main threat regards factors of common causality between house prices and fertility rates that can drive the results, such as productivity shocks or expectations about future income. In order for the estimated relationship between house prices and fertility not to reflect time-invariant differences in preferences for children across municipalities or by time-variant national economic shocks, such as business cycle movements, it is imperative that the regression specifications control for municipality and year fixed effects. The regression estimates of the relationship between house prices and fertility rates are in other words identified off within-municipality changes in house prices[20].

Further, it is important to control for other time-varying municipality-level economic conditions that potentially covary with the demand for housing and children, and that can reflect the state of the local business cycle or expectations of future income. We therefore control for average income before taxes at the municipality level, where income is defined as all taxable income excluding capital income for the population aged 20-64 years, at December 31st each year. The income of the population younger than 20 years old and older than 64 years old is likely to be highly determined by the pension system and the education system, rather than by local business cycles, why they are excluded. The measure is deflated by CPI and we use the natural logarithm of the two year lag in the variable to obtain a well-behaved measure of the macro economy for the year during which the fertility decision is being made. Previous research has used similar control variables for this purpose, such as income per capita as well as average wage rates, unemployment rates and real family income (Dettling and Kearney 2014; Lovenheim and Mumford 2013). Introducing average income as a control variable is however not made without concerns of its own, since the variable may be biased. It could for example be correlated with how many children that are born in a municipality, through effects of maternity and paternity leave, part time work, etc. However, since our dependent variable does not directly measure the number of children in a municipality, but rather the propensity of the women to have a child, there is less worry about such bias. Using a one year lag in income further reduces such potential bias.

As an additional control for regional business cycles we also control for total employment rate in the county[21]. This is motivated since labour market regions are integrated across boarders: over the time period 1993-2014, on average 33 percent of the Swedish employed population commuted outside the municipality in which they live for work, whereas only 6 percent commuted outside the county in which they live (Statistics Sweden/RAMS). The employment data comes from the register based labor market statistics (RAMS) collected by Statistics Sweden from The Swedish Tax Agency and its tax administrative register and is available for the years 1985-2013[22]. It

[20] A test for overidentifying restrictions, using the additional orthogonality conditions that the regressors are uncorrelated with the group-specific error u_i (the "random effect"), i.e., $E(X_{it} * u_i) = 0$, confirms that a fixed effects strategy is appropriate.

[21] Optimally, we would use unemployment data. There is however no publicly available information on unemployment in RAMS, or in any other database on a municipality level.

[22] The data is retrieved from three different data sets corresponding to three different time periods between

covers the full adult population that resides in Sweden rather than a surveyed sample of it, as labor force surveys do, and the individual is defined as employed if the individual has performed, on average, at least one hour work per week during November in a given year.[23]

Our next main threat to identification regards sorting bias. The assumption underlying the identification of β_{l2} and β_{l3} in equation (4.7) and β_{c2} and β_{c3} in equation (4.8) is that households with higher or lower underlying fertility rates are not sorting into regions in which house prices are growing relatively fast or slow, driven by other amenities. A negative bias in the estimates could be caused by for example higher wages in larger, more expensive municipalities that induce career-oriented women to substitute away from having children, toward market work. A positive bias in the estimates could be driven by for example similar municipalities but that also have well-developed pre-school systems or similar services that induce career-oriented women that want to have children to sort into those municipalities. Given the aim of this study, we want to isolate the effect of house prices on fertility net of these sorting patterns.

For this purpose, we control for educational attainment among young and old fertile women respectively[24]. In particular, we control for the fraction of young and old fertile women in a municipality that have any post-secondary education[25]. Similar measures are used in previous literature: L&M control for the individual woman's educational attainment and D&K use the fraction of college educated women in each ethnicity-age-group in a metropolitan area. For similar reasons, we also control for the employment rate among young and old fertile women in each municipality and year. The employment data comes from the same register based labor market statistics (RAMS) as described for total county employment rates above. Lastly, it should be noted that both of these control variables serve as additional controls for common causality between house prices and fertility rates.

A last comment to the design of the baseline specifications is that we believe that the disturbances may be serially correlated within municipalities, and thus not independently distributed, and that they are likely to suffer from heteroskedasticity, and thus not be identically distributed. To obtain consistent and unbiased OLS estimators, we therefore employ the Huber-White clus-

1985 and 2013.

[23] An additional variable that potentially covaries with demand for both housing and children is immigration. Migration flows are uneven across municipalities and could therefore affect housing demand and house prices differently in different municipalities. Immigrants also have different fertility rate patterns depending on which country they migrant from (Statistics Sweden 2014). This could affect fertility rates differently across municipalities. Given the status of Sweden as a relatively large receiver of migrants, such variable would be relevant to control for. However, due to the time limitations of this thesis and given that the relevance of this variable came to our awareness at a late stage in the thesis writing process, we could unfortunately not include it in the specifications. If an inflow of immigrants creates different municipality specific trends in fertility and house prices, this will however be captured by our municipality specific time trends. Further, the pattern is on average that immigrants have higher fertility rates than native women in their 20's and early 30's, while the fertility rates are on average the same among foreign and native born women 40 years of age. This implies that if immigration causes some common causality between house prices and fertility rates that affects our estimated differential effects, it is likely to be a negative bias, which indicates that our estimates are a lower bound. Further, not explicitly controlling for immigration makes our specifications more comparable to those of D&K and L&M, that do not control for immigration in their studies.

[24] Optimally, we would employ income or wage data. Restricted by data availability, we instead use educational attainment, which should be highly correlated with income and wage.

[25] The education data we use is a register based data on the educational level of the full population residing in Sweden (UREG). We define the variable "post-secondary education" as the sum of the three groups "post-secondary education, less than 3 years", "post-secondary education 3 years or more" and "post-graduate education". In terms of the International Standard Classification of Education 1997 (ISCED97) developed by the United Nations Educational, Scientific and Cultural Organization (UNESCO) we sum the three groups ISCED97 4+5B, ISCED97 5A and ISCED97 6.

tered method developed by Rogers (1993) that is robust to both such characteristics of the residuals, and cluster the standard errors at the municipality level. The modified Wald statistics test for groupwise heteroskedasticity, following Greene (2000), and the Newey and West (1987) estimator including autocovariances confirm that such standard errors are appropriate.

Robustness checks

To evaluate the validity of our baseline specification design, we employ several robustness checks. In particular, we address concerns of regression misspecification; sorting bias; underlying fertility trends; reverse causality; cross-sectional dependence; and common causality.

Regression misspecification

To test how robust our baseline results are to alternative regression specifications we run the baseline specification for two year house price changes using one year and four year house price changes instead. Further, we split the sample into urban and non-urban municipalities based on a classification of municipalities developed by the Swedish Association of Local Authorities and Regions (SALAR) (Sveriges Kommuner och Landsting, in Swedish) to see if the estimated baseline pattern hold for both urban and non-urban areas. This is motivated by the trends observed in figure 4.1, showing stronger house price increases in urban areas compared to in Sweden on average, and compared to selected non-urban areas[26]. Lastly, we estimate the fertility effects associated with higher house price levels and house price changes jointly in one regression.

Sorting bias

As robustness checks for our baseline sorting bias strategy, we include municipality specific trends and municipality-group-specific trends in the model, following D&K, to allow for the possibility that individuals with plans to expand their families choose to locate in municipalities with upward or downward trending prices, and that old and young fertile women may behave differently in this respect.

Underlying trends

An additional concern is the diverging fertility trends among young and old women observed in figures 4.2a and 4.2b. Fertility rates among young women have been declining and fertility rate among old women have been increasing since the early 1990s until today. In order to see if the baseline estimations capture the age specific response in fertility to house prices, and not just these general trends in fertility for the two groups, we include a group specific time trend, which allows for a separate time trend development across young and old women.

Reverse causation

As discussed in section 2.2.2, it is possible that the decisions to have a child and to buy a house are simultaneous to some degree. If the decision to buy a house is driven by the decision to have a child, such reverse causality can induce endogeneity to the ownership rate variable. Since the ownership rates in 1990 are used for all years, such endogeneity is reduced. However, ownership rates in 1990 are likely to be a good proxy for ownership in later years. One indication of this is that the ownership rates in 2007 are not dramatically different from those in 1990, as discussed

[26]The exact definitions of urban and non-urban areas are found in Appendix A (section 8.1)

in section 4.2. Another indication of this is that the composition of the stock of houses in a municipality is rather stable over time (Statistics Sweden/Dwelling stock). This means that using lags in ownership rates as a way to deal with reverse causality concerns might not suffice.

We therefore run regressions using the ASFR of five-year small age groups[27] as the dependent variable, and the ownership rate of a five year younger age group as explanatory variable. For example, the ASFR among women 30-35 years of age in municipality i and year t is used as dependent variable, and the home ownership rate for the same municipality in 1990 among individuals 25-30 years of age is used as explanatory variable interacted with house prices in year $t-5$. In the specifications, the subscript g will in this specification refer to the groups 20-24, 25-29, 30-34, 35-39, 40-44 respectively, and the younger age group for which ownership rates is controlled for has the subscript $g-1$. With this setup, we impose the restriction that house ownership must occur before having a child, which allows us to check for potential reverse causality in the baseline design of the specifications. While this design makes the assumption that women can be affected by a partner's ownership status in the same age group stronger, it also allows for more variation in the ownership variable.

The potential reverse causality between fertility and house ownership however also implies there may be a similar relation between fertility rates and house prices, as increasing demand for house ownership may cause house prices to rise, inducing an upward bias in the relationship between fertility and house prices. Using panel data for Swedish municipalities from 1981-2006, Malmberg (2010) argues that house prices are influenced by the age structure of the population. In order to assess such potential endogeneity in house prices, we estimate the baseline regression using 2, 3, and 4 year lags of the respective house price variable.

Cross-sectional dependence

Further, we are concerned about cross-sectional dependence in the disturbances, such that

$E[e_{i,t}e_{j,s}|x] \neq 0$, where (i$\neq j$)

and where t and s potentially, but not necessarily, equal each other. In other words, the disturbances e_{it} may be correlated not only along t, as is commonly assumed, but also along i. By including year fixed effects, as we do in all specifications, we reduce potential cross-panel correlation. However, with municipalities in Sweden as the panel level, one reason for still being concerned about cross-sectional dependence is that the economic and financial integration of geographical and financial entities has grown, which implies interdependencies between cross-sectional units. Another reason is culture, norms or values that are shared within a region.

Pesaran's (2004) cross-sectional dependence test, Friedman's (1937) statistic, and the test statistic proposed by Frees (1995) give mixed indications as to whether our concern is justified. Pesaran's and Free's test indicate the presence of cross-sectional dependence while Friedman's test does not. As a robustness check to our baseline results we do however employ Driscoll and Kraay (1998) standard errors (DK-SE) to correct for potential cross-sectional dependence. This model is preferred to a linear regression with panel-corrected standard errors as developed by Beck and Katz (1995), as the DK-SE allow the cross-sectional dependence to have a temporal dimension and not merely being contemporaneous[28]. By employing the DK-SE, the

[27] We construct these groups to be as small as possible, which is five year age groups, due to limitations in the ownership data.

[28] The results are however similar comparing the two models. These are not presented in this paper but

autocorrelation is assumed to follow a moving-average process with some lag q[29].

To perform this robustness test, the sample has to be divided in two sub-samples: young and old women respectively. This is because the DK-SE does not allow repeated time values within a panel. This implies that the ownership variable will be captured by the municipality fixed effects as it is time invariant in each age group and municipality. Therefore, we also exclude the interaction term between house prices and ownership. This is a potential threat to the identifying assumption underlying the comparison of coefficients between age groups. We attempt to qualify and quantify the bias induced in the old-dummy interaction variables by estimating the baseline specification with and without the ownership-interaction variables.

Further, this model specification implies that we cannot estimate a coefficient on the interaction variable between house prices and the old-dummy. Instead, the house price coefficients will be compared across the the young and old sub-samples. If one or both of the house price variables are estimated as statistically significant in the sub-samples, and with opposing signs, this will indicate that there is differential effect of house prices on young and old women's fertility.

Common causality

To deal with concerns of an underlying common factor, such as productivity or the expectations of future incomes, that drives the demand of both children and housing, and thus house prices, a two stage least squares estimation using an instrumental variable (IV) is commonly applied. D&K use housing supply elasticity as measured by Saiz (2010) as an IV for house prices. Such housing supply elasticity IV has unfortunately not been available for this study, and has also been criticized for not being an exogenous instrument; house prices in land locked areas may be relatively high not only because they are landlocked, but also for example because individuals have strong preferences for living close to water or mountains (Davidoff 2015).

Kyriacou et al. (2015) use a demographic based variable as IV for construction industry measures. In particular, they use individuals between 25 and 49 years of age as share of total population since they are believed to have a net demand for housing, while those below this range may still live with their parents and those above may already own a house. Malmberg (2010) presents evidence that supports the idea of such relationship between population age structure and house prices in Sweden. Thus, a demographic based IV in line with that of Kyriacou et al. (2015) but with a ten year lag arguably satisfies the conditions expected of a good instrument for house prices levels in our study. First, the propensity of women to have a child in year t arguably does not affect the age structure of the population in year $t-10$. Second, since our dependent variable is the propensity to have a child and not the number of births, the size of this particular age cohort would directly affect the dependent variable only if there exists some cultural transmission channel through which an increasing number of children in the population causes women to increase their propensity to have a child. However, despite that this demographic based IV appears to be good one for our study *a prioi*, the F-statistics from the first stage of the two-step procedure are well below the critical value of 10 recommended by Staiger and Stock (1997) which means that the instrument is weak. We therefore do not make use of this demographic based IV for house prices in our study as the estimates are likely to be

available from the authors upon request.

[29] If the disturbances would be better captured by an autoregressive (AR) process with some lag p, Hoechle (2007) argues this is necessarily not an issue as AR processes normally can be well approximated by a finite-order moving-average process.

biased[30].

Instead of using an IV approach, L&M employ state-by-year fixed effects to control for common causality between house prices and fertility rates. The house price coefficients are then identified off of housing price growth differences among home owners within a state and year, as they use individual level data. L&M argue that while it is still possible for these within-state and year differences to be driven by economic shocks, the local dynamics of housing price changes within a state are more likely to be driven by exogenous factors, such as local supply constraints, that are less prone to bias from macroeconomic shocks than is within-metropolitan area variation over time. A similar approach can be applied to Swedish data, by instead employing county-by-year fixed effects. This can be motivated by the multilevel political systems, where municipalities have a large influence on factors that could drive the local economic developments, whereas the main responsibility of county level politics is to govern and manage the health care systems [31] (Swedish Government Offices 2008; Pettersson-Lidom and Wiklund 2002). As such, we employ a similar approach to that of L&M in our model, comparing estimates obtained using county-by-year fixed effects with the baseline estimates.

Further, when "external" instrument variables are not available, another strategy to deal with potential common causality is to make use of "internal" instruments, as recognized by Arellano and Bond (1991). They develop a Generalized Method of Moments (GMM) estimator that is designed for "small T, large N" panels; that allows for dynamic processes in the model, with current realizations of the dependent variable being influenced by past ones; and that instruments differenced independent variables that are not strictly exogenous with their lags in levels. Therefore, in addition to allowing us to instrument the potentially endogenous house prices by using internal instruments, and as such test how robust our results are to remaining common causality, another merit with employing a dynamic panel data model like the Arellano-Bond estimator is that we can relax the strictly static restrictions imposed on the relationship between house prices and fertility. This is particularly relevant for testing the value of the option to postpone the fertility decision.

The Arellano-Bond estimator is however not robust to serial correlation in the idiosyncratic errors. We therefore use the estimator developed by Roodman (2009) that fits the Arellano-Bond estimator while also dealing with error terms that are correlated within (but not across) panels[32]. This estimator is also robust to heteroskedastic disturbances. Consider the model:

[30] The two stage least squares results using this demographic based IV are not presented in the paper but are available upon request.

[31] By law, both municipalities and counties are autonomous and are entitled to collect taxes. At the county level the main responsibility is the health care systems. At the municipality level, main areas of responsibility concern housing, planning and building, infrastructure, social services such as education, child and elderly care, and services to develop the business structure of the municipality (SALAR 2015; Swedish Government Offices 2008). Such services provided at the municipality level are thus more likely to be correlated with local economic and house price developments.

[32] A problem with the original Arellano-Bond estimator is that lagged levels are poor instruments for first differences if the variables are close to a random walk. Arellano and Bover (1995) outline a dynamic panel data model that is efficient in this case, and which Blundell and Bond (1998) fully develops. As we will see in the next section, the Blundell-Bond estimator is not needed for our case, as the autoregressive coefficient is not close to one. The Blundell-Bond estimator also requires the additional assumption that the first differences that are used as instruments are orthogonal to the unobserved fixed effects, which arguably is a strong assumption for our model.

$$y_{it} = y_{it-1} + \mathbf{x}'_{it}\gamma + \mathbf{w}'_{it}\beta + \epsilon_{it} \tag{4.9}$$

$$\epsilon_{it} = u_i + e_{it} \tag{4.10}$$

$$E(u_i) = E(e_{it}) = E(u_i e_{it}) = 0 \tag{4.11}$$

where y_{it-1} is the lag of the dependent variable, $\mathbf{x}'_{it}$ is a vector of strictly exogenous covariates, $\mathbf{w}'_{it}$ is a vector of potentially endogenous covariates (house prices, in our case, which are thus defined as GMM-style instruments), and where the disturbance term ϵ_{it} has two orthogonal components: the fixed effects, u_i, and the idiosyncratic shocks, e_{it}.

One problem in applying ordinary least squares and mean deviations transformation to remove the fixed effects from the model in equation (4.9) is that y_{it-1} is correlated with the fixed effects in the error term, which gives rise to "dynamic panel bias", and within-groups does not eliminate dynamic panel bias (Nickell 1981). The Arellano-Bond estimator instead applies a first-difference transformation to the model in equation (4.9), producing the following model:

$$\Delta y_{it} = \Delta y_{it-1} + \Delta\mathbf{x}'_{it}\gamma + \Delta\mathbf{w}'_{it}\beta + \Delta e_{it} \tag{4.12}$$

The fixed effects are gone, but the lagged dependent variable is still potentially endogenous, because the y_{it-1} term in $\Delta y_{it-1} = y_{it-1} - y_{it-2}$ is correlated with the v_{it-1} in $\Delta v_{it} = v_{it} - v_{it-1}$. Likewise, any variable in $\mathbf{w}'$ that is not strictly exogenous becomes potentially endogenous because it may too be related to v_{it-1}. But unlike with the mean-deviations tranform, longer lags of the regressors remain orthogonal to the error and available as instrument. This is the main benefit of the Arellano-Bond estimator that we exploit.

Similarly to when we employ DK-SE to control for cross-sectional dependence, we now must split the sample into the two sub-samples for young and old women respectively, as the Arellano-Bond estimator does not allow repeated time values within a panel. This means that ownership rates are captured by fixed effects and that we exclude the house price interaction with ownership rates. We remind that we make an attempt at qualifying and quantifying the potential bias arising from this, which is also kept in mind when we interpret the results. Further, we make use of the two-step procedure as it is asymptotically more efficient than the one-step procedure. However, since the reported two-step standard errors tend to be severely downward biased (Arellano and Bond 1991; Blundell and Bond 1998), a finite-sample correction to the two-step covariance matrix derived by Windmeijer (2005) is employed.

Regarding the choice of what lags to use as internal instruments, an autoregressive process of order 1 (AR(1)) is expected in first differences of the residuals, because $\Delta e_{it} = e_{it} - e_{it-1}$ should correlate with $\Delta e_{it-1} = e_{it-1} - e_{it-2}$, since they share the e_{it-1} term. So to check for AR(1) in levels, we look for AR(2) in differences, and let this guide our choice of which lags of instruments to use. The Sargan statistic of the instruments' joint validity is not robust to heteroskedasticity or autocorrelation, and Roodman (2009) argues that the Hansen J statistic should not be relied upon too faithfully: it grows weaker the more moment conditions there are. Since these are quartic in T, it is important to restrict the number of instruments used.

In conclusion

We want to identify the relationship between house prices and fertility net of systematic sorting patterns and potential underlying common causality between the two variables. Our set of control variables is therefore chosen so as to minimize these two main threats to the identification of the conditionally exogenous relationship between house prices and fertility. We employ fixed municipality and year effects along with controlling for the fraction of young and old fertile women with some post-secondary education in a municipality; the employment rate per young and old fertile women on municipality level; the overall employment rate for adults (men and women) on the county level; and average income in the municipality. At baseline (and in most robustness models) the regression estimates are thus identified off within-municipality changes in house prices. We also use standard error estimators that are robust to heteroskedasticity and within-panel serial correlation.

To check the robustness of the baseline results we vary the length of the house price change, estimate the model for urban and non-urban sub-samples and estimate levels and changes in house prices jointly to check particular specification issues; we use municipality-specific time trends and municipality-group-specific time trends to check for remaining sorting bias and group-specific time trends to control for underlying fertility trends; we employ several house price lags to check for potential reverse causality in house prices and smaller age groups and ownership rates for the age group younger than that of the dependent variable to control for reverse causality in the ownership rates; we use DK-SE to check for potential cross-sectional dependence in the disturbances; and we make use of county-by-year fixed effects as well as internal instruments to control for remaining common causality (among which the latter also serves to check the robustness of the static relationship between house prices and fertility).

5 Results

In this section we present the results from our empirical strategy as outlined above. Firstly, we present the results on how house price levels and house prices changes are associated with fertility rate effects among groups with varying house ownership rates on the one hand, and among young and old women on the other, following the baseline design of the specifications. Secondly, our set of robustness tests to these baseline results are reported.

5.1 Baseline results

This section presents the results from estimating the fertility rates effect that is associated higher house price levels and changes. These are the baseline results and they are presented in table 5.1 for house price levels and in table 5.2 for house price changes, in which column 5 corresponds to the baseline equations 4.7 and 4.8 respectively (presented in 4.3.1).

The dependent variable is $lnASFR_{igt}$ in all specifications, which is the natural logarithm of the age specific fertility rate for young and old fertile women per municipality and year. In both table 5.1 and 5.2, column 1 contains only the two variables of interest - the ownership rates and the old-dummy interacted with house prices - along with a house price variable, an ownership variable and the old-dummy variable. As described in the previous section, the variables interacting house prices with ownership rate, $HP_{it-1} * Own_{ig}$ and $2yr\Delta HP_{it-1} * Own_{ig}$, test the wealth effect (hypotheses 1 and 2 respectively), and the variables interacting house prices with the old-dummy, $HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Old_g$, test the option value effect (hypotheses 3 and 4 respectively). Through column 2 to 5, control variables are added step by step to the specification in column 1, to provide an idea of the robustness of the estimates of our main variables. Fixed municipality and year effects are included in all specifications.

In column 6 the ownership variable interacted with house prices is removed from the full specification and in column 7 the variable interacting the old-dummy with house prices is removed instead. This gives an idea of how robust the variables of interest are for being analyzed on their own versus together with the other variable of interest. We make this comparison as an attempt to qualify and quantify the bias induced to the estimates when the variables are analyzed on their own[33].

In table 5.1, the baseline results of the fertility effects associated with rising house price levels are presented. The coefficient on house prices interacted with the old-dummy, $HP_{it-1} * Old_g$, which tests the option value effect is positive and statistically significant at the 0.1 percent level through all six specifications. The size of the estimate is very robust to the inclusion of control variables through columns 2-5. The coefficient on $HP_{it-1} * Old_g$ in the full specification in column 5 indicates that with SEK 100,000 higher house prices, the differential fertility effect for old relative to young women is an increase in the fertility rate by 2.58 percent, which indicates that there is a significant difference in fertility rate effects across two age groups in terms of a positive premium for old women. Comparing the results in column 6 to column 5 gives an

[33] The models that include the two main variables on their own, in columns 6 and 7, are only presented with the full baseline specification. These results are however very similar to those corresponding to columns 1-4, where control variables are added step by step for the respective variable of interest when estimated on its own. These results are not presented in this paper but are available upon request.

Table 5.1: House price levels and fertility rates, 1993-2014

Dep.var $lnASFR_{igt}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HP_{it-1}	-0.0178***	-0.0174***	-0.0170***	-0.0177***	-0.0156***	-0.0161***	-0.00435
	(0.00212)	(0.00229)	(0.00186)	(0.00185)	(0.00183)	(0.00155)	(0.00361)
$\text{HP}_{it-1} * Old_g$	0.0261***	0.0272***	0.0266***	0.0267***	0.0258***	0.0273***	
	(0.00221)	(0.00260)	(0.00267)	(0.00268)	(0.00233)	(0.00199)	
$\text{HP}_{it-1} * Own_{ig}$	0.0144	0.0158	0.00615	0.00445	0.0117		0.117***
	(0.00931)	(0.00986)	(0.00983)	(0.00972)	(0.00914)		(0.0109)
HighEduc_{igt-1}		-0.750*	-0.486*	-0.466*	-0.499**	-0.484*	-0.182
		(0.303)	(0.199)	(0.193)	(0.189)	(0.190)	(0.228)
EmpRate_{igt-1}			1.606***	1.727***	1.822***	1.842***	2.020***
			(0.323)	(0.319)	(0.312)	(0.314)	(0.290)
$\text{EmpRateCounty}_{it-1}$				-1.924***	-1.455***	-1.571***	-0.816
				(0.426)	(0.287)	(0.272)	(0.416)
$ln\text{AvgInc}_{it-2}$					-0.807***	-0.713***	-1.588***
					(0.230)	(0.211)	(0.363)
Old_g	0.0464	0.0397	-0.144	-0.161	-0.151	-0.193	0.641***
	(0.137)	(0.161)	(0.178)	(0.179)	(0.170)	(0.171)	(0.146)
Own_{ig}	-2.180**	-1.923*	-2.472**	-2.493**	-2.667**	-2.412**	-6.936***
	(0.677)	(0.821)	(0.877)	(0.882)	(0.810)	(0.816)	(0.708)
N	12496	12496	12496	12496	12496	12496	12496
Adj R-square	0.769	0.777	0.801	0.803	0.806	0.805	0.726

Notes: In column 1-5 control variables are added step-by-step. Column 5 reports the baseline specification. Column 6 reports the baseline specification with the house price interaction with ownership excluded and column 7 the baseline specification with the house price interaction with the old age group excluded. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

indication of how biased $HP_{it-1} * Old_g$ is when estimated on its own, without $HP_{it-1} * Own_{ig}$. The point estimate in column 6 is 0.0273, compared to 0.0258 in column 5, which points to a small upward bias in the house price and old-dummy interaction variable when it is estimated without the ownership interaction variable. This is the expected direction of the bias if age can proxy for house ownership. However, since it is a very small bias the old-dummy interaction does not appear to capture much ownership effect.

The coefficient on house prices interacted with ownership rates tests the wealth effect and is positive but only statistically significant in one of the six specifications: when it is estimated without the interaction between house prices and the old-dummy (column 7). In column 7, when it is estimated without house prices interacted with the old-dummy, the effect of ownership on fertility is positive, significant at the 0.1 percent level and a factor ten larger than in column 5, with a point estimate of 0.117 compared to 0.0117. The direction of the bias is the expected if ownership can proxy for age. Since the bias is large, ownership interacted with house prices estimated on its own seems to capture an age and option value effect.

All control variables are statistically significant, except from the Old_g variable estimated without the house price interaction. Against the concern of potential endogeneity in the income variable, it is noteworthy that the estimate of our main variables of interest do not change significantly from column 4 to 5, as income is included. Thus, the concern of introducing endogeneity with the inclusion of income should be minor. The negative coefficient on income potentially reflects its association to opportunity cost of time. Further, the house price variable without interactions is negative and statistically significant in all specification except for in column 7. Regarding the two variables are included to control for sorting effects, the coefficient for the share of fertile women with higher education is negative and the sign for employment rate among fertile women at the municipality level is positive. This is in line with results from previous literature. Total employment rate at the county level, included to control for common causality, has a negative coefficient. The effect of the ownership variable estimated alone, is statistically negative in all specifications. The statistical significance of most control variables reduces the concern for a misspecified model.

Proceeding to table 5.2, we comment on the baseline results for house price changes. The coefficient for house price changes interacted with the old-dummy, $2yr\Delta HP_{it-1} * Old_g$, tests the option value effect and is positive and statistically significant at a 0.1 percent level through all specifications. As control variables are added through columns 2-5, the estimate increases slightly, but in general gives a robust impression. In the full specification in column 5 the coefficient is 0.0677, which implies that a house price change over two years of SEK 100,000 is associated with a 6.77 percent increase in fertility among old women compared to young women, which indicates that there is a significant difference in fertility rate effects across two age groups in terms of a positive premium for old women. The estimated coefficient is slightly larger in column 6 which indicates a small upward bias when ownership interacted with house price changes is excluded, as would be expected if age can proxy for house ownership. This bias does however appear to be minor.

The estimate of house price changes interacted with the ownership rate per municipality, $2yr\Delta HP_{it-1} * Own_{ig}$, tests the wealth effect and is positive and statistically significant in all six specifications. The size of the estimate does however change both upwards and downwards as control variables are included and the statistical significance also varies a bit. In the full baseline specification in column 5, the estimate is statistically significant at the 1 percent level and indicates that a house price change over two years of SEK 100,000 is associated with a 4.09 percent higher fertility rate increase in a group with one hundred percent ownership rate compared to a group with zero percent ownership rate. Comparing column 5 to column 7 does however indicate that the ownership captures an age affect when it is estimated on its own. The point estimate is now 0.245, and highly statistically significant. The direction of this bias is the expected if ownership rates can proxy for age, and the size of the bias indicates that when estimating ownership rates interacted with house prices changes on its own, this variable is likely to capture an age and option value effect.

All control variables are statistically significant in the main specification in column 5. With regards to the income variable, the concern of introducing potential endogeneity with its inclusion appears minor since the coefficients of the main variables of interest change little to its inclusion (comparing column 4 to 5). The negative sign of income could potentially reflect its association with the opportunity cost of time. The house price variable without interactions is negative and highly statistically significant in all specifications. Furthermore, both variables included to control for municipality sorting are statistically significant, share of women with higher education with a positive sign, and the employment rate among fertile women at municipality level with a negative sign. Total employment rate at the county level, included to control for common causality, has a negative coefficient. Both coefficients for the old-dummy and the ownership rate are statistically significant and positive. The statistical significance of all control variables reduces the concern for a misspecified model with regards to the included controls.

Table 5.2: House price changes and fertility rates, 1993-2014

Dep.var $lnASFR_{igt}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2yrΔHP_{it-1}	-0.0409***	-0.0444***	-0.0490***	-0.0486***	-0.0471***	-0.0464***	-0.0241**
	(0.00603)	(0.00576)	(0.00478)	(0.00480)	(0.00493)	(0.00456)	(0.00832)
2yr$\Delta HP_{it-1} * Old_g$	0.0588***	0.0619***	0.0686***	0.0677***	0.0677***	0.0726***	
	(0.00697)	(0.00668)	(0.00665)	(0.00671)	(0.00640)	(0.00642)	
2yr$\Delta HP_{it-1} * Own_{ig}$	0.0502***	0.0558***	0.0304*	0.0373**	0.0409**		0.245***
	(0.0131)	(0.0113)	(0.0135)	(0.0133)	(0.0147)		(0.0297)
HighEduc$_{igt-1}$		0.634	0.971***	0.966***	0.982***	0.974***	0.726**
		(0.382)	(0.255)	(0.248)	(0.243)	(0.244)	(0.259)
EmpRate$_{igt-1}$			2.576***	2.709***	2.844***	2.854***	2.688***
			(0.321)	(0.313)	(0.303)	(0.302)	(0.325)
EmpRateCounty$_{it-1}$				-2.402***	-2.231***	-2.162***	-2.598***
				(0.392)	(0.362)	(0.349)	(0.434)
lnAvgInc$_{it-2}$					-0.810***	-0.796***	-0.817***
					(0.144)	(0.134)	(0.224)
Old$_g$	0.879***	0.837***	0.469***	0.453***	0.435***	0.423***	0.656***
	(0.102)	(0.0976)	(0.108)	(0.107)	(0.108)	(0.109)	(0.125)
Own$_{ig}$	-5.014***	-5.068***	-5.790***	-5.839***	-5.881***	-5.812***	-6.723***
	(0.569)	(0.495)	(0.573)	(0.583)	(0.579)	(0.589)	(0.599)
N	12496	12496	12496	12496	12496	12496	12496
Adj. R-squared	0.627	0.633	0.698	0.702	0.705	0.705	0.660

Notes: In column 1-5 control variables are added step-by-step. Column 5 reports the baseline specification. Column 6 reports the baseline specification with the house price interaction with ownership excluded and column 7 the baseline specification with the house price interaction with the old age group excluded. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In summary, the baseline results for the interaction between the old-dummy and house prices that test for the option value effect hypotheses suggest there is a positive and significant fertility effect for old women compared to young associated with higher house prices. The baseline results for the interaction between ownership rates and house prices suggest there is a positive fertility effect for groups with higher ownership rates compared to those with lower ownership rates when house prices rise, although this effect is only statistically different from zero in association with house price changes, and not with house price levels.

5.2 Robustness checks

We proceed with analyses of how robust these baseline results are. First, we check if the baseline specifications are sensitive to alternative specifications. Second, we check if the baseline results reflect potential remaining sorting bias and also an underlying fertility trend. Third, we test if the baseline results are driven by reversed causality. Fourth, we check how sensitive the precision of the estimates is to cross-sectional dependence in the error terms, and lastly, we check if the baseline results are driven by remaining common causality.

Regression misspecification

We begin by varying the construction of the house price change variable. Instead of using a two year change as in the baseline specifications, a one and a four year house price change are employed. The results are found in table 8.1 in appendix. This modification of the house price change variable does not alter any conclusions from the baseline results, which points to the robustness of the baseline estimates. However, the size of the coefficients become slightly larger with a one year change in house prices, and they decrease significantly with a four year house price change. This is not surprising as both changes are in terms of SEK 100,000, but over a shorter or longer time span, which means that the one-year change reflects a more dramatic and notable change than the four- and two-year changes do.

Next, we split the sample into two subgroups of urban and non-urban municipalities. The results are displayed in table 5.3. For house price levels, in panel A, the pattern of coefficients in the baseline results remains in both the urban and the non-urban sub-sample. The size of the coefficient on the old-dummy interaction with house price levels is larger in the non-urban sub-sample than in the urban, with 0.0352 compared to 0.0213, with the baseline result of 0.0258 in the middle. However, in panel B, for house price changes, the interaction term between house prices and ownership becomes statistically insignificant. This is a concern with regards to the robustness of its baseline estimates. The interaction with the old-dummy however remains positive and highly significant. Again, the size of the point estimate is larger in the non-urban sub-sample than in the urban, with 0.0983 compared to 0.0567, and again, with the baseline estimate of 0.0677 in the middle.

Table 5.3: House prices and fertility rates: urban and non-urban sub-sample, 1993-2014

Dep.var $lnASFR_{igt}$	Urban (1)	Non-urban (2)
A. House price level		
HP_{it-1}	-0.0129***	-0.0249***
	(0.00327)	(0.00120)
$\text{HP}_{it-1} * Old_g$	0.0213***	0.0352***
	(0.00211)	(0.00325)
$\text{HP}_{it-1} * Own_{ig}$	0.00502	0.0233
	(0.0107)	(0.0147)
N	1760	10736
Adj R-square	0.908	0.704
B. House price change		
2yrΔ HP_{it-1}	-0.0394***	-0.0649***
	(0.00369)	(0.00478)
2yrΔ $\text{HP}_{it-1} * Old_g$	0.0567***	0.0983***
	(0.00564)	(0.00664)
2yrΔ $\text{HP}_{it-1} * Own_{ig}$	0.0208	0.0199
	(0.0166)	(0.0202)
N	1760	10736
Adj. R-squared	0.858	0.584

Notes: In column 1 the baseline specification is reported for the urban sub-sample; municipalities in the Stockholm, Gothenburg and Malmö regions. In column 2 the baseline specification is reported for the non-urban sub-sample; municipalities that are not part of the urban sub-sample. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

As a final regression misspecification test, we include both house price levels and house price changes in one regression, along with their respective interaction terms with the old-dummy and the ownership rates. The results are presented in table 8.2 in appendix 8.3. The interaction between house price changes and ownership becomes insignificant also in this robustness specification. Beyond that, this test does not alter any conclusions from the baseline results.

Sorting bias and underlying fertility trend

In table 5.4 we present the results from addressing the concern that remaining sorting bias drives the baseline results. We include municipality-specific time trends to allow for the possibility that individuals with plans to increase or decrease their fertility move into municipalities with upward or downward trending house prices. Column 1 reports these results. The pattern remains largely the same as in the baseline results with positive coefficients for $HP_{it-1} * Old_g$, $2yr\Delta HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Own_{ig}$, and the magnitudes of the coefficients on the old-dummy interactions are similar to the specification without any trend terms included. However, the coefficient on $2yr\Delta HP_{it-1} * Own_{ig}$ is decreased and the coefficient on $HP_{it-1} * Own_{ig}$ is, in contrast to the baseline specification, now statistically significant. This is the only specification in which this variable is statistically significant once we also control for the option value effect. This statistical significance should therefore be interpreted with great caution and in general, these results give little reason to suspect that individuals with plans to increase or decrease their fertility systematically move into municipalities with upward or downward trending house prices.

If there exist trends that are distinct for old and young women, which on average also have different ownership rates, this may bias the estimated baseline results. We therefore control for group-specific time trends, which allow for old women and young women to on average be on different trends with regards to for example fertility preferences. The results are reported in column 2. These trends do not alter the pattern of the baseline estimates, but the point estimates for the effects associated with house price changes that are reported in panel B are decreased.

Lastly, if there exist sorting trends between municipalities that are controlled for in column 1, and that also are distinct for old and young women as the trend controlled for in column 2, the estimated β_{l2}, β_{c2}, β_{l3} and β_{c3} in the baseline specifications might be biased estimates of the conditional causal effect of interest. We thus additionally include in the model separate municipality-group-specific time trends. These trends allow, for example, old women in Stockholm to be on a different trend than young women in Stockholm. These trends do not alter the pattern of the baseline results regarding the option value effect, but the coefficient on $2yr\Delta HP_{it-1} * Own_{ig}$ is now statistically insignificant and the point estimates for the effects associated with house price changes that are reported in panel B are even more decreased. The coefficient on $2yr\Delta HP_{it-1} * Old_g$ of 0.0235 is substantially lower than the baseline coefficient of 0.0677 and is now similar to that on $HP_{it-1} * Old_g$ of 0.0271. This is arguably the most conservative estimate of $2yr\Delta HP_{it-1} * Old_g$. These results give us reason to suspect that the baseline results for house price changes reflect that young and old women with plans to increase their fertility systematically sort municipalities with upward trending house prices.

Table 5.4: House prices and fertility: various trends, 1993-2014

Dep.var $lnASFR_{igt}$	(1)	(2)	(3)
A. House price level			
HP_{it-1}	-0.0170***	-0.0134***	-0.0106***
	(0.00202)	(0.00201)	(0.00226)
$\text{HP}_{it-1} * Old_g$	0.0202***	0.0225***	0.0271***
	(0.00265)	(0.00267)	(0.00343)
$\text{HP}_{igt-1} * Own_{ig}$	0.0608***	0.0118	-0.0254
	(0.0182)	(0.00859)	(0.0135)
N	12496	12496	12496
Adj R-square	0.815	0.810	0.852
B. House price change			
$\text{2yr}\Delta HP_{it-1}$	-0.0467***	-0.0315***	-0.0186***
	(0.00456)	(0.00514)	(0.00425)
$\text{2yr}\Delta HP_{it-1} * Old_g$	0.0693***	0.0466***	0.0235***
	(0.00673)	(0.00657)	(0.00332)
$\text{2yr}\Delta HP_{it-1} * Own_{ig}$	0.0371*	0.0254*	0.0149
	(0.0157)	(0.0119)	(0.0120)
N	12496	12496	12496
Adj. R-squared	0.714	0.759	0.836
Municipality trend	Yes	No	No
Group trend	No	Yes	No
Municipality group trend	No	No	Yes

Notes: In column 1 the results are reported from estimating the baseline specification with municipality trends, in column 2 the baseline specification with group trends and in column 3 the baseline specification with municipality group trends. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Reverse causality

If households that plan to have a child increase their demand for owning a one family dwelling, and if the ownership rates in 1990 do a good job at predicting future ownership rates, the baseline estimates of the wealth effect may be biased by reverse causality. We therefore impose the restriction that ownership must occur before fertility, in line with the procedure that we outline in section 4.3.2. The results, in which women 20-24 years old are the base group, are reported in table 5.5. The baseline pattern is not changed by this rather demanding restriction. In particular, the pattern of coefficients is such that the size of the coefficient increases with the age groups, which is consistent with the option value hypotheses. Note that since women 25-29 years of age are defined as young women along with the base group of women 20-24 years of age in the baseline specification, it is of little concern that the coefficient on $2yr\Delta HP_{it-5} * (25-29)$ is not statistically different from zero. It is interesting to note that the size of the estimates remain larger for house price changes than for house price levels. This should however not raise any concern about endogeneity in ownership since this $HP_{it-5} * Own_{ig-1}$ remains statistically insignificant.

Table 5.5: Five year age groups with five year house price and ownership lags

(a) House price levels, 1995-2014

Dep.var $lnASFR_{igt}$	(1)
HP_{it-5}	-0.0247***
	(0.00355)
$\text{HP}_{it-5} * (25-29)$	0.0000820*
	(0.0000340)
$\text{HP}_{it-5} * (30-34)$	0.000261***
	(0.0000385)
$\text{HP}_{it-5} * (35-39)$	0.000387***
	(0.0000404)
$\text{HP}_{it-5} * (40-44)$	0.000457***
	(0.0000371)
$\text{HP}_{it-5} * Own_{ig-1}$	0.000212
	(0.000142)
N	28504
Adj. R-squared	0.925

(b) House price changes, 1997-2014

Dep.var $lnASFR_{igt}$	(1)
$2\text{yr}\Delta HP_{it-5}$	-0.0357***
	(0.00841)
$2\text{yr}\Delta HP_{it-5} * (25-29)$	0.00420
	(0.00786)
$2\text{yr}\Delta HP_{it-5} * (30-34)$	0.0340***
	(0.00841)
$2\text{yr}\Delta HP_{it-5} * (35-39)$	0.0603***
	(0.00869)
$2\text{yr}\Delta HP_{it-5} * (40-49)$	0.0817***
	(0.00914)
$2\text{yr}\Delta HP_{it-5} * Own_{ig-1}$	0.0681**
	(0.0253)
N	25654
Adj. R-squared	0.913

Notes: The two tables report the associated effect from estimating five year lagged house prices on the dependent variable age specific fertility rates for women in five year age groups (instead of the young and old age groups). Age group 20-24 is the base group. The other age groups are 25-29, 30-34, 35-39 and 40-44 years. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, dummy variables indicating each age group 25-29, 30-34, 35-39 and 40-44 years and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Although the results reported in table 5.5 give little reason to suspect reverse causality in ownership rates, we also check for potential reverse causality in house prices, since such endogeneity would bias not only the estimated β_{l3} and β_{c3} in the baseline specifications but also the estimated β_{l2} and β_{c2}. For this purpose, we run the baseline specifications with two, three and four year house price lags instead of with a one year lag[34]. The results are presented in table 5.6, with house price levels in 5.6a and house price changes in 5.6b. These house price lags do not change the baseline pattern: the coefficients on $HP_{it-1} * Old_g$, $2yr\Delta HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Own_{ig}$ remain positive and highly significant, and the coefficient on $HP_{it-1} * Own_{ig}$ remains positive and insignificant, and the magnitudes of the coefficients are similar to those in the specification with one year house price lags. These results give us no reason to suspect that the baseline results reflect reverse causality in house prices.

[34]We therefore loose some observations, as we, due to concerns of endogeneity in house ownership, do not want to use house prices prior to 1990.

Table 5.6: Different house price lags and fertility rates, 1993-2014

(a) House price levels

$Dep.var\ lnASFR_{igt}$	(1) 2-lag	(2) 3-lag	(3) 4-lag
HP_{it-2}	-0.0156***		
	(0.00199)		
HP_{it-3}		-0.0158***	
		(0.00206)	
HP_{it-44}			-0.0164***
			(0.00218)
$HP_{it-2} * Old_g$	0.0263***		
	(0.00250)		
$HP_{it-3} * Old_g$		0.0265***	
		(0.00261)	
$HP_{it-4} * Old_g$			0.0265***
			(0.00282)
$HP_{it-2} * Own_{ig}$	0.0133		
	(0.00968)		
$HP_{it-3} * Own_{ig}$		0.0140	
		(0.00964)	
$HP_{it-4} * Own_{ig}$			0.0143
			(0.00969)
Years	1993-2014	1993-2014	1994-2014
N	12496	12496	11928
Adj R-squared	0.794	0.780	0.773

(b) House price changes

$Dep.var\ lnASFR_{igt}$	(1) 2-lag	(2) 3-lag	(3) 4-lag
2yrΔHP_{it-2}	-0.0430***		
	(0.00558)		
2yrΔHP_{it-3}		-0.0408***	
		(0.00606)	
2yrΔHP_{it-4}			-0.0359***
			(0.00535)
2yr$\Delta HP_{it-2} * Old_g$	0.0643***		
	(0.00653)		
2yr$\Delta HP_{it-3} * Old_g$		0.0629***	
		(0.00693)	
2yr$\Delta HP_{it-4} * Old_g$			0.0561***
			(0.00656)
2yr$\Delta HP_{it-2} * Own_{ig}$	0.0487**		
	(0.0156)		
2yr$\Delta HP_{it-3} * Own_{ig}$		0.0481**	
		(0.0175)	
2yr$\Delta HP_{it-4} * Own_{ig}$			0.0431*
			(0.0170)
Years	1994-2014	1995-2014	1996-2014
N	11928	11360	10792
Adj. R-squared	0.712	0.739	0.752

Notes: The two tables report the associated effect of estimating house prices with different lags on the dependent variable fertility rates for young (20-29 years) and old (30-44 years) women. Estimates for 2 year lags are reported in column 1, 3 year lags in column 2 and 4 year lags in column 3. The years employed in each specification are adjusted to not include years prior to 1991 in order to avoid inducing endogeneity in the ownership variable, which is taken at the year 1990. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Cross-sectional dependence

If shocks to the disturbances are correlated across municipalities due to financial integration or common culture, the disturbances may be cross-sectionally dependent. We therefore run the baseline specifications using Driscoll-Kraay standard errors instead of Huber-White standard errors (HW-SE). In order to do so the sample must be split into the two sub-groups of young and old women. As described in section 4.3.2, the ownership rates are then captured by the fixed effects, why we do not include the interaction term with ownership rates. While this threatens the identifying assumption underlying the comparison between groups, we believe that this is a minor issue in this case. The reason for this is that we are not interested in the point estimates in this robustness test, but their standard errors, since this is the only aspect of the estimation that the use of DK-SE instead of HW-SE affects. Comparing columns 5 and 6 in the baseline tables, the statistical significance of the old-dummy interactions with house prices is not significantly affected by excluding the ownership interaction variable. We therefore begin by estimating the specification on the two sub-samples using HW-SE to provide an idea of the statistical significance associated with the baseline results, since combining these two specifications corresponds to the specification in column 6 in the baseline table 5.1. We then compare these standard errors to those obtained by using DK-SE, to see how the baseline standard errors are affected by potential cross-sectional dependence. The results are reported in table 5.7, with HW-SE in columns 1 and 3 (for young and old respectively), and with DK-SE in columns 2 and 4 (for young and old respectively).

Table 5.7: House prices and fertility rates: Controlling for cross-sectional dependence, 1993-2014

	Young		Old	
	(1)	(2)	(3)	(4)
Dep.var $ln(ASFR)_{igt}$	HW-SE	DK-SE	HW-SE	DK-SE
A. House price levels				
HP_{it-1}	-0.00301***	-0.00301***	0.00177	0.00177*
	(0.000695)	(0.000510)	(0.000918)	(0.000893)
N	6356	6356	6356	6356
R-squared		0.964		0.852
Adj. R-squared	0.934		0.861	
B. House price changes				
2yrΔ HP_{it-1}	-0.00410*	-0.00410***	0.00329***	0.00329*
	(0.00164)	(0.00118)	(0.000805)	(0.00151)
N	6344	6344	6344	6344
R-squared		0.963		0.852
Adj. R-squared	0.933		0.861	

Notes: Results are reported from estimating the baseline specification for the young sub-sample (women aged 20-29 years) and for the old sub-sample (women aged 30-44 years). The dependent variable is age specific fertility rates for young women in column 1 and 2 and age specific fertility rates for old women in column 3 and 4. In column 1 and 3 Huber-White standard errors clustered at the municipality level are in parentheses, and in column 2 and 4 Driscoll-Kraay standard errors are in parentheses. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

When we split the baseline specification using HW-SE into the sub-samples of young and old women, the coefficient on house price levels becomes statistically insignificant in the old-women sub-sample (column 3, panel A). The central question with regards to our hypotheses is however not whether the point estimates in these separate regressions are significant or not, but whether the coefficients on house prices are statistically different from each other in the young and old sub-samples. Since this is the case in column 6 in table 5.1, and since the combination of columns 1 and 3 in panel A correspond to this specification, we can conclude that the coefficients on house price levels that we report here in table 5.7 are statistically different from each other. Moving to columns 2 and 4 in panel A and the use of DK-SE, these coefficients are even more precisely estimated. Proceeding to panel B and the results for house price changes, the standard errors are smaller using DK-SE in the young sub-sample and slightly larger in the old sub-sample, both still being statistically significant. In summary, this gives us no reason to believe that the baseline standard errors cause incorrect rejection of true null hypotheses ("type 1 errors").

Common causality

Lastly, we address the concern that the baseline results reflect remaining common causality between house prices and fertility rates. First, municipality and year fixed effects in the baseline specifications are replaced with county-by-year fixed effects following the procedure suggested by L&M, which controls for uniform county level economic shocks. The results are presented in table 5.8.

Controlling for county-by-year fixed does not change the baseline pattern of positive and significant coefficients on $HP_{it-1} * Old_g$, $2yr\Delta HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Own_{ig}$, and a positive and insignificant coefficient on $HP_{it-1} * Own_{ig}$. The size of the estimates do however increase somewhat compared to the baseline regression, in particular for house price changes (table 5.8a). This implies that the concern for a confounding macroeconomic factor that is present at the municipality level but not at the county level is minor.

Table 5.8: House prices and fertility rates: County-by-year fixed effects, 1993-2014

(a) House price levels

Dep.var $lnASFR_{igt}$	(1)
HP_{it-1}	-0.0165***
	(0.00135)
$HP_{it-1} * Oldg$	0.0314***
	(0.00196)
$HP_{it-1} * Own_{ig}$	0.0117
	(0.00791)
County-by-year FE	Yes
N	12496
Adj R-square	0.747

(b) House price changes

Dep.var $lnASFR_{igt}$	(1)
$2yr\Delta HP_{it-1}$	-0.0615***
	(0.00205)
$2yr\Delta HP_{it-1} * Old_g$	0.0874***
	(0.00391)
$2yr\Delta HP_{it-1} * Own_{ig}$	0.0490***
	(0.0113)
County-by-year FE	Yes
N	12496
Adj. R-squared	0.306

Notes: Results are reported from estimating the baseline specifications with county-by-year fixed effects. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In lack of good external instruments that can be employed to control for factors that potentially influence house prices and fertility rates together, we make use of internal instruments and the Arellano-Bond GMM-model. With this estimator we are also able to check the validity of the static relationship estimated in the baseline specifications, as a lagged dependent variable can be controlled for. The results are reported in table 5.9. As when we use Driscoll-Kraay standard errors, the sample must be split into the young and old sub-samples in order to avoid repeated time values within the panel. This implies that the ownership variable is captured by the fixed municipality effects and that we exclude the ownership interaction from the model. Again, this is a threat to the identifying assumption underlying the comparison between groups.

We do however believe that the potential bias that this causes is minor and the reasons for this are twofold. First, the coefficient on $HP_{it-1} * Own_{ig}$ has been statistically insignificant in all but one specification in which also $HP_{it-1} * Old_g$ has been included, and the coefficient on $2yr\Delta HP_{it-1} * Own_{ig}$ has been less robust than that on $2yr\Delta HP_{it-1} * Old_g$. It is thus the least significant and robust of the two interaction variables in each specification that is dropped. Second, comparing column 5 to column 6 in the baseline tables 5.1 and 5.2 allows us to quantify this potential bias. The coefficients on house prices interacted with the old-dummy becomes slightly larger when the interaction with ownership is excluded from the specification for both house price levels (0.0258 compared to 0.0273) and changes (0.0677 compared to 0.0726). While this indicates that we should interpret the results when we split the sample and loose the ownership variable with caution, this upward bias in the estimates is small and should not constitute a major concern.

To begin with we note that the lagged dependent variable is statistically significant in only one out of four specifications: in the young sub-sample when we estimate the effect of house price changes. The coefficient on the lagged dependent is -0.124, which indicates that past fertility rates are negatively associated with current fertility rates for young. This is reasonable if having a child in $t-1$ reduces the likelihood of having a child in year t. However, that the lagged dependent variable is insignificant in the other three specifications indicates that past fertility rates have little explanatory power for the current fertility rates, which reduces the concern of omitted variable bias in the baseline specifications.

With regards to the house price coefficients, we again stress that the central question is not the point estimates of the house price variables in the separate regressions, but whether the coefficients on the house price variables are different between the regressions in the young and old sub-samples. In the specifications for house price levels (panel A), the coefficient on HP_{it-1} is negative and statistically significant at a 1 percent level in the young sub-sample, and positive but not statistically different from zero in the old sub-sample. This indicates that the differential fertility effect between young and old women associated with higher house price levels remains in this dynamic model specification. In the house price changes specifications (panel B), the coefficient on $2yr\Delta HP_{it-1}$ in the young sub-sample is positive but it is not statistically significant at a 5 percent level; the p-value is 0.056. It is thus statistically significant at the 10 percent level. The coefficient on $2yr\Delta HP_{it-1}$ in the old sub-sample is positive but not statistically different from zero. This indicates that the differential effect between young and old potentially remains when we control for common causality, but we should be careful with assigning too much weight to this effect.

Lastly, we fail to reject the null hypothesis of no remaining autoregressive process of second order, AR(2), in all specifications, which indicates that we have selected the lags of the instruments well. Although we have minimized the number of instruments used by using only one lag of the endogenous variables as instruments, the instruments amount to 67. The Hansen J test for over-identifying restrictions is therefore likely to give misleading results, as it suffers from low power when a large set of instruments is used. Consequently, the instruments' joint validity is rejected in three out of four cases[35]. However, as argued by Roodman (2009), one should not rely too faithfully on this statistic.

[35] Bowsher (2002) finds by conducting the Sargan test (used instead of Hansen test if homoskedasticity can be assumed), in Monte Carlo simulations of difference GMM on N=100 panels, that the test is clearly undersized once the time units reach 13. When the time units reach 15, the test never rejects the null of joint validity at the 5 or 10 percent level.

Table 5.9: Two-step Arellano-Bond GMM, 1993-2014

Dep.var $lnASFR_{igt}$	Young (1)	Old (2)
A. House price levels		
L.Depvar	0.151	-0.0171
	(0.473)	(0.469)
$\mathrm{HP}_{it-1}(100,000)$	-0.00410**	0.000296
	(0.008)	(0.849)
N	6066	6066
p-value Hansen J	0.000	0.003
p-value AR(1)	0.002	0.000
p-value AR(2)	0.152	0.102
laglimits	(2 2)	(1 1)
# instruments	67	67
B. House price change		
L.Depvar	-0.124***	-0.0225
	(0.001)	(0.324)
$2\mathrm{yr}\Delta HP_{it-1}(100,000)$	-0.00458	0.000659
	(0.056)	(0.510)
N	6054	6054
p-value Hansen J	0.000	0.103
p-value AR(1)	0.000	0.000
p-value AR(2)	0.532	0.112
laglimits	(1 1)	(1 1)
# instruments	67	67

Notes: Results are reported from estimating a two-step Arellano-Bond GMM-model with a lagged dependent variable and internal instruments. Column 1 is estimated for the young sub-sample (women aged 20-29 years) and column 2 for the old sub-sample (women aged 30-44 years). The dependent variable is age specific fertility rates for young women in column 1 and age specific fertility rates for old women in column 2. p-values are reported in parentheses. Standard errors adjusted for Windmeijer's finite sample correction, clustered at the municipality level, are employed but not reported. All specifications are weighted by female population per age group, municipality and year. Municipality and year fixed effects are included in all specifications. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5.3 A summary of the results

To sum up, there appears to be a differential response in fertility between young and old women associated with higher house prices. This effect appears to be most robust with respect to house price levels, but also relatively robust with respect to house price changes. There also seems to be a differential response in fertility across groups with different ownership rates in relation to house price changes, although this effect is not entirely robust to additional checks. In relation to higher house price levels, there does not appear to be a significant differential fertility effect between groups with different ownership rates.

However, three concerns about the conditional exogeneity between house prices and fertility rates, and thus about the validity of the baseline results, can not be ruled out with the robustness checks in mind.

First, the results from the dynamic panel data model with a lagged dependent variable and internal instruments do not completely rule out the existence of a common factor that drives both fertility rates and house prices. The differential effect across young and old women's fertility remains significant in the house price level specification, whereas such effect is less robust in relation to house price changes.

Second, the baseline results for house price changes interacted with both the old-dummy and with ownership rates appear to be biased upwards, since the point estimates are reduced in further checks. This bias appears to arise from a systematic sorting into municipalities with different house price trends among women with different fertility preferences. Such sorting in addition appears to differ between young and old women, which, themselves, appear be on different trends. This bias is more pronounced for the interaction with ownership rates, since this coefficient becomes statistically insignificant when we control for municipality-group-specific trends.

Thirdly, it should be stressed that while the effect of house price changes interacted with ownership rates is significant in the baseline specification and to several robustness checks, it is not robust to two specification checks as well as to the just mentioned sorting bias check. The baseline wealth effect should therefore be interpreted cautiously, especially since we, due to data limitations, cannot include this variable in all our robustness checks.

Lastly, although the coefficient on $HP_{it-1} * Old_g$ remains robust to all our specifications, it should not be interpreted as causal.

Effect analysis

We proceed by commenting on the effect size of our estimated results. Since the statistical power of a test increases with the sample size, it is possible to reject a null hypothesis even for very small economic differences should a sample be very large. Since our sample is large (12,496 observations in the baseline specifications), we see little reason to expand on the power of our tests, that arguably should be very high, but rather consider it relevant to discuss the economic significance of our results. We begin by commenting on the magnitude of our results and then compare them with the findings in previous studies.

We begin by reminding that we are interested in the differential fertility effects between young and old women on the one hand, and between groups with different house ownership rates on the other. Further, point estimates should not be interpreted as causal, since we are concerned that the coefficients may reflect remaining sorting bias or common causality as discussed above. As such, the comments that we make on point estimates in this section should be regarded as illustrative and as giving an idea of their economic significance, and not as true causal effects on fertility.

Through the specifications that use the full sample and (at least) all the "full baseline specification" variables (column 5 in the baseline tables), the coefficient on the house price levels interacted with the old-dummy varies between 0.0202 and 0.0314; that on house price changes interacted with the old-dummy varies between 0.0235 and 0.0874; and the coefficient on house price changes interacted with ownership varies between 0.0254 to 0.049 (for specifications where it is statistically significant). We refrain from commenting on the size of the coefficient on the variable interacting house price levels with ownership rates as it is statistically insignificant in close to all specifications.

Since the coefficient on $2yr\Delta HP_{it-1} * Old_g$ is less robust than that on $HP_{it-1} * Old_g$, we choose to illustrate the economic significance of our results using the coefficients from the specification that gives the most conservative estimation of $2yr\Delta HP_{it-1} * Old_g$. This is the baseline specification with a municipality-group-specific trend, reported in table 5.4 in column 3.

This specification indicates that a SEK 100,000 (approx USD 11,775) increase in house price levels is associated with a 2.7 percent marginal increase in the fertility rate of old women relative to young women. This estimate is in the middle of the relatively narrow region of estimates for this coefficient. Considering that the mean weighted fertility rate among old women is 0.97, a 2.7 percent increase implies a fertility rate of 0.996. This should however not be interpreted as the "true effect", especially since it is not adjusted for the effect on young women.

Further, a SEK 100,000 change in house prices over the past two years is associated with a 2.3 percent increase in the fertility rate of old women relative to young. The mean weighted fertility rate among old women is 0.97, and thus, a 2.3 percentage increase is associated with a fertility rate of 0.992. Again, this should not be interpreted as the "true effect".

These estimates indicate that the coefficients for levels and changes in house prices interacted with the old-dummy give similar marginal effects on fertility of about a 2.5 percent relative increase in the fertility of old women. This is plausible since neither theory nor empirical literature suggests that there should be different effects between house price levels and changes on the value of holding the option to postpone fertility.

In the specification for which we report the coefficients on $HP_{it-1} * Old_g$ and $2yr\Delta HP_{it-1} * Old_g$, the coefficient on the home ownership interaction with house price changes is not statistically different from zero. This coefficient is however, as noted above, significant in other specifications. We therefore use the most conservative estimate of the wealth coefficient when it is also statistically significant, to make an illustrative example of its economic significance, but stress that these results must be interpreted with caution. We exemplify with the coefficient from the specification in which we control for group-specific trends (table 5.4 column 2), and the size of the coefficient is 0.0254. Given the average female population weighted ownership rate of 0.17, this estimate indicates that there is a marginal increase in fertility rates of 0.43 percent associated with a house price change of SEK 100,000. Comparing the marginal effect between two groups with the ownership rates of 0.25 and 0.75, the fertility rate effect would be 1.2 percent higher in the group with the higher ownership rate. This conservative estimate appears to be of somewhat smaller economic significance than the conservative estimate regarding the option value effect.

We proceed by comparing our estimates to those reported by D&K and L&M. D&K find that a USD 10,000 increase in house price levels (comparable to SEK 100,000 $\approx$ USD 11,775), in an out of sample estimation of an ownership rate of 100 percent, is associated with a 7.2 percent increase in fertility relative to an ownership rate of zero percent. In a similar exercise to the one above, with ownership rates of 0.25 and 0.75 for two separate groups, D&K find the marginal effect in fertility rates to be 3.6 percent. The economic significance of such effect appears to be slightly larger but relatively comparable to that of the effects estimated for the option value on fertility, and larger than that of our most conservative wealth effect.

Furthermore, L&M find that a house price change of USD 100,000 is associated with an 18.8 percent increase in total fertility rates for women 25-44 years old, or, assuming a linear relationship, a 1.88 percent increase in total fertility rates from a USD 10,000 house price change. This appears to be a similar but slightly smaller effect than the conditional effect of house prices on the fertility rates among old women relative to young found in our analysis.

To sum up, the illustrated effects of house prices on the fertility rates of old women relative to young women appear to be of economic significance. The conditional effect of ownership on fertility associated with house price changes appears to be of less economic significance in this study, but we again stress that this effect must be interpreted carefully. Further, the size of the estimates of the fertility rate effects on old relative to young appear to be fairly consistent with the results reported for a wealth effect on fertility by D&K and L&M. Finally, we again stress that these effects are not considered to be causal and that all point estimates should be carefully interpreted.

6 Discussion and conclusion

With the results from the empirical analysis presented in the preceding section, and following a comparison of our estimated results to previous literature, we proceed by discussing our results in this section. We will discuss the channels through which the estimated effects may be operating and potential explanations as to why the results in this paper differ from those of the literature on housing wealth and fertility. Furthermore, we discuss potential limitations to our results and suggestions for future research. Lastly, we conclude.

6.1 Discussion of results

We begin by discussing the findings presented in the previous section. Firstly, we turn to the results related to hypothesis 1 and 2 regarding the wealth effect. In particular, we expand on the relatively weak evidence in support of a wealth effect in our estimations. Secondly, we discuss the results relating to hypothesis 3 and 4 regarding the option value.

Wealth effect results

Based on theory and previous literature, we hypothesized that both house price levels and house price changes would be associated with a positive fertility premium for groups with higher ownership rates relative to those with lower ownership rates. Such results would lend support to the wealth explanation of the relationship between fertility rates and house prices. In particular, for house owners, house price levels are expected to capture life time wealth and house price changes are expected to capture realized wealth changes, both of which are predicted to be important for household fertility behavior.

With regards to our hypotheses, the evidence is mixed. When ownership rates are interacted with changes in house prices, the estimated effect on fertility rates is positive and significant in the baseline specification as well as in several, although not all, of the robustness checks to it. Further, due to data limitations the variable of interest is not included in all robustness tests, which also gives reason to interpret its effect carefully. When ownership rates are interacted with house price levels, on the other hand, we find little support for the wealth effect explanation of the relationship between house prices and fertility rates. These latter results lend support to those of L&M, who find that house price changes and not levels seem to matter for explaining fertility rates in the U.S. At the same time, our findings contradict those of D&K, who find that house price levels are significantly associated with fertility rates in the U.S., and in particular with a positive fertility premium for higher house ownership rates. In conclusion, we do not reject that a wealth effect can contribute to explain the relationship between house prices and fertility rates. Rather, we find some support for a wealth effect associated with house price changes, albeit relatively weaker support than that we find for the option value effect (which is discussed below). One potential explanation to the relatively less robust wealth effect results can be the relatively well developed and, for the family, cheap child care system in Sweden along with child allowances, that can reduce the importance of wealth in affording the current costs of having a child.

One important observation with regards to the ownership interaction variables is that they seem to suffer from substantial upwards bias when they are estimated on their own compared to when an age effect interacted with house prices is also controlled for. As home ownership is positively correlated with age, it is possible that an ownership interaction with house prices that is estimated without a control for the age interaction with house prices captures part of the age effect. If this is the case, previous findings regarding a wealth effect on fertility, that do not control for age and option value effects, may be biased. This is potentially an important caveat to the studies made by L&M and D&K.

However, an important point for discussion regarding the variables that test for a wealth effect is the construction of the ownership variable. Since ownership data for housing in this study was constructed from ownership rates for different age groups in 1990, and corrected for a general municipality level overestimation rate, it is possible that the ownership variable is over- or underestimated for the group of young and old respectively. Further, we have not been able to construct ownership rates for only women. This is why we make the assumption that if there is explanatory power to the wealth effect, women's childbearing should not only be affected by their own ownership status but also by that of their potential partner's, and that the partners and the women are in the same age group. This may also induce measurement error in the variable. If so, this can contribute to explain why we do not find fully robust wealth effects from house price changes, or any effects at all from house price levels. In connection to this it should also be stressed that due to limitations in data availability, ownership rates have not been possible to include in all robustness checks, which also gives reason to interpret these results carefully.

As a last point for discussion, we should also discuss the relative importance of the the wealth and the credit channels respectively. The recent findings by Cesarini et al. (forthcoming) indicate that Swedish women do not change their fertility behavior in response to a positive wealth shock, which does not speak in favor of the wealth channel. On the other hand, while Berg and Bergström (1995) suggest that financial wealth matters more for consumption than housing wealth, these findings are made with respect to a period with the financial markets were more regulated. Since then, financial deregulation has facilitated households to withdraw housing equity, which is suggested to have led to increased loan-to-value ratios and higher house prices (Turner 1999; Sveriges Riksbank 2014). Table 8.2 in appendix shows how the ratio of credit to disposable income has increased at a high rate. This makes it plausible that the potential wealth effect could be operating through the credit channel.

Option value results

Based on theory and previous literature, we hypothesized that both levels and changes in house prices would be associated with a positive fertility premium for older fertile women compared to younger fertile women. Such results would lend support to the option value explanation of the relationship between fertility rates and house prices.

The pattern of the coefficients for the variables interacting house price levels and house price changes with the age group dummy variable are in line with our hypotheses and therefore lends support to the option value explanation of the relationship between house prices and fertility rates. Higher house price levels as well as house price changes are significantly associated with a positive fertility premium for old fertile women compared to young. These results are robust to various additional checks for regression misspecification, reverse causality, sorting bias and common causality. While we do not make predictions regarding the respective sign of the house price variables when the sample is split into young and old women respectively (when we employ Driscoll and Kraay standard errors and when we use internal instruments in the Arellano-Bond GMM model) the pattern of signs of the coefficients is in line with what could be expected if higher house prices would induce a postponement of fertility decisions: in the young subsample the coefficient is negative and in the old subsample the coefficient is positive (although small and statistically insignificant in the Arellano-Bond model). One note of caution should however be made with regards to house price changes interacted with the old-dummy, as this effect is not quite as robust to the use of internal instruments and a lagged dependent variable as those of house price levels interacted with the old-dummy are.

In general, for the results of age effects associated with primarily house price changes but to some extent also house price levels, the largest concern to conditional exogeneity appears to be an upward bias arising from both sorting bias and common causality. Therefore, the specification from which we report estimates is the one that produces the most conservative estimates for the interaction of house price changes and the age effect, which is when municipality-group-specific trends are controlled (table 5.4, column 3). In addition, it is noteworthy that the coefficients on house prices interacted with the old-dummy are relatively similar in this specification: 0.027 for house price levels and 0.023 for house price changes. This lends further credibility to these estimates as the option value theory makes no prediction of a varying effect across house price levels and house price changes.

The findings in this paper indicate that fertility effects associated with different rates of house ownership might be upward biased if an age effect interaction with house prices is not controlled for. In other words, our results suggest that it is important to identify wealth effects net of potential option value effects. While our findings are relatively more consistent with the option value effect than with the wealth effect, our empirical investigation does not test the wealth hypothesis against the option value hypothesis, but rather their respective explanatory power when also controlling for the other. Our findings should therefore not be interpreted as indicating that *either* the wealth explanation *or* the option value explanation contribute to explain the relationship between house prices and fertility rates; they are not mutually exclusive. Further, as discussed above, there are several limitations to our ownership rate data, which may also contribute to explain why we find relatively weaker support for the wealth effect explanation than for the option value effect explanation. In sum, however, these findings add to the recent literature on housing wealth and fertility effects as well as to the growing literature of the existence of a value of the option to postpone the fertility decision.

Despite our several measures to control for sorting bias and common causality, we lastly want to stress that although the robust and significant positive fertility rate premium for old fertile women relative to young as a response to higher house prices indicates that a causal relationship between house prices and fertility rates may exist, the estimated coefficients cannot be interpreted as reflecting the size of such causal effect. This is particularly the case for the age effect associated with house price changes, since the Arellano-Bond GMM model adds a question mark as to whether the differential effect between young and old women remains in a dynamic setting and with the use of internal instruments.

Transitory or permanent changes in house prices

One last point for discussion regarding the results is that house price changes are associated with positive fertility premiums in relation to both old women and higher ownership rates, although more robustly in relation to the former. Under the option value theory, households are expected to respond to house price changes if they are perceived as transitory, and in particular, if they are believed to be reversed. In contrast, under the permanent income hypothesis and the wealth explanation, house price changes are expected to affect fertility behavior if they are perceived to be permanent.

Previous research on Swedish house prices and consumption concludes that Swedish house price movements are largely disassociated with consumption in the short run, as short-run changes are transitory. In contrast, long-run changes are found to be associated with changes in consumption (Chen 2006). While this lends support to the findings regarding the option value explanation, it can also be the case that households perceive house price to be more permanent than they actually are. Which then would explain why also the wealth effect appears to have some explanatory power with regards to the relationship between house price changes and fertility rates. On the other hand, it could be that when studying short-run changes in house prices, as we are doing, the effects are perceived as more transitory, which better captures the option value effect. Perhaps wealth effects are better captured using more long-run changes in housing wealth, in line with findings of Chen (2006), and that potential wealth effect would come out more robustly in such a study.

Policy implications

While Sweden is a country with relatively high fertility rates compared to many European countries, the fertility rates are below the replacement level of 2.1. During the past decades, childbearing has become increasingly postponed in Sweden. Such postponement of fertility may impact the required housing, pre-schooling and schooling in the short run, and reduce completed fertility in the long run. Since our results are relatively consistent with the option value effect explanation, such potential relationship between house prices and postponement of fertility may therefore be valuable to add to the many discussions on house prices and their future outlook in Sweden at present.

6.2 Limitations and future research

There are a few limitations to this study and to the quality of the data, which have been iterated upon throughout this thesis, that should be discussed. The first limitation regards the data that is used to construct the ownership variable. Due to data limitations, as mentioned in the section for data description, the ownership variable had to be constructed from two different data sets to first estimate the number of women per age group that live in a house, secondly, to adjust that number with the number of women that own a house. The adjustment was done under the assumption that the ownership overestimation rate is equally distributed across all age groups. This assumption could potentially induce measurement error to the ownership variable, by overestimating ownership in the younger age groups, and underestimating ownership in the older groups. Further, due to data limitations the ownership variable is not constructed for women separately, but for all individuals in an age group. This is another potential source of measurement error. Thus, the relatively weaker support that we find for the wealth effect explanation compared to the option value effect explanation could partly be explained by these limitations to the data.

An additional potential explanation to this that stems from limitations in our data, is that we use gross measures of housing wealth. It is thus possible that we do not capture true wealth effects from changing house prices or house price inflation.

Due to the aforementioned limitations to the ownership data, we were unfortunately not able to include the interaction between ownership rates and house prices in the robustness checks controlling for cross-sectional dependence by employing Driscoll-Kraay standard errors on the one hand, and when we use a dynamic panel data model with internal instruments to control for remaining common causality. The results would have been more reliable if ownership could have been included in these models too. In addition, this limitation induces some bias to the age effect interaction with house prices. Although we attempt to quantify this bias, and it appears to be minor, it would have been preferred to be able to include the ownership rate interaction.

Furthermore, a central concern in this study is the threats to the identification of conditional exogeneity between house prices and fertility rates. The two main threats are, as iterated throughout the thesis, common causality and sorting bias. We have addressed this concern by carefully choosing our set of control variables; by employing fixed municipality and year effects; by including municipality specific time trends and municipality-group-specific time trends; by employing county-by-year fixed effects; and by making use of internal instruments and a lagged dependent variable. However, these controls would be potentially more powerful if employed on individual level data. It would also strengthen the analysis to find and make use of a good external instrument variable for house prices and to control for immigration effects.

A promising topic for future research is therefore to address these limitations by investigating the relationship between house prices and fertility rates using higher quality ownership data; *net* housing wealth data; micro data; an immigration control variable; and an instrumental variable approach.

The main limitation to the external validity of our study is that, due to data limitations, the estimated coefficients are not directly comparable to those in previous literature. A limitation to the generalizability of this study is that the econometric models are only tested on a period that is characterized by a housing boom and also in a country with regulated rents. If the results are robust to housing bust periods and to a market rents is a topic that future research will hopefully bring clarity too.

Lastly, since the option value effect explanation appears to contribute to our understanding of the relationship between house prices and fertility rates, we believe that another promising topic for future research is to take our study forward by 1) analyzing the double dimensions of house prices in relation to fertility behavior in a dynamic modelling framework, and by 2) taking the option value mechanism to the lab and study it in an experimental setting. For the first avenue along which we see that future research could take our study, such dynamic modelling framework could, in line with Damodaran (2002) and Dixit and Pindyck (1994), to begin with model house prices as the investment cost of having a child and subject to uncertainty. Without the option to postpone, the household would decide to have a child if the net value of having a child is larger than the immediate investment cost (or is indifferent between having a child or not if the net value equals the immediate investment cost). In the actual situation, where the opportunity to have a child remains available in future periods, the decision involves a different trade-off: to have a child today or wait and do what is best tomorrow. To assess this, the household would have to look ahead and take into account its expectations about the house prices tomorrow. The cost of postponement of the fertility decision that increase with age could be capture by an inverse of the time that the woman has left to expiry of the option to postpone fertility, of in other words, her time left to menopause. The household's optimal decision would be the one that maximizes the net present value and thus takes into account both the expected value of postponing the fertility decision until tomorrow and the cost of delaying it. The value of the extra freedom of holding the option to postpone the decision - which is the value of interest here - would be given by the value of being able to postpone less the value of not having this option. The wealth dimension of housing could be captured by exogenous assignment of house ownership (in a dynamic framework, this could perhaps be thought of as a helicopter drop of houses with some probability in each time period, given that the household is not already a house owner). An increase in house prices could be modelled as additional discretionary income under the assumption that households can withdraw credit on their collateral value, thus employing the credit channel to model the wealth effect. Although only a brief sketch, future research can hopefully take this forward in order to formalize the dynamic relationship between house prices and fertility rates.

As for the second avenue along which future research could take our study forward, it would be interesting to take the option value mechanism to the lab, to try and capture it in an experimental setting. This could bring insights into whether the actual mechanism operates in the way that theory, our empirical evidence and (future) dynamic models suggests that it should.

6.3 Conclusion

We study the relationship between house prices and fertility rates in Sweden over the time period 1993-2014. The aim is to investigate through which channels such relationship might operate. While a potential wealth effect explanation has been examined in previous literature, the identification of such effect has relied on the assumption that there are no systematic differences between the fertility response of house owners and renters to the increasing investment costs of having a child that higher house prices reflect.

We add to this literature by recognizing that house ownership is likely to be positively correlated with age, and that women of different ages are likely to respond differently to higher house prices. Since house prices are characterized by uncertainty, the option value theory predicts that if house prices are high, there is an incentive to wait and see if they will decrease, making the investment cost of having a child lower. In particular, the option value theory predicts that holding such option to postpone the fertility decision is more valuable to younger women than to older women, since the risks of postponing fertility increase with age.

Our contribution lies in a thorough empirical analysis of the role the option value effect and the wealth effect in the context of house prices and fertility behavior. Using a panel data approach, we empirically test the wealth effect explanation and the option value explanation of the relationship between house prices and fertility rates. We exploit the fact that rents are regulated in Sweden and thus less likely to follow house prices, which allows us to credibly identify house prices as the investment cost of having a child. The main identifying variation comes from the large housing boom that Sweden has experienced since the early 1990s and that has occurred differently in different geographical regions. Our main specifications are fixed effects panel regressions that are estimated using OLS. We also carry out a number of robustness checks to assert the validity of our results. In particular, as robustness checks we use internal instruments in a dynamic panel data setting as well as county-by-year fixed effects to address the concern that the relationship between house prices and fertility rates are driven by common causality; we employ several trends to control for sorting bias and underlying fertility trends that differ between young and old women; and we make use of lags in house prices and impose the restriction that ownership must occur before fertility to check for reverse causality.

The empirical analysis shows four main results. First, consistent with the option value explanation of the relationship between house prices and fertility, we find that higher house prices are associated with a positive fertility rate effect for old women relative to young women. SEK 100,000 higher house price levels are associated with 2.7 percent higher fertility rates for old women relative to young, and a SEK 100,000 house price change over the past two years is associated with 2.3 percent higher fertility rates for old women relative to young. The size of these estimates is relatively consistent with a small but growing body of literature that studies the housing wealth effect on fertility rates. Second, we find that house price changes appear to be associated with differential fertility rate effects between groups with different home ownership rates, which lends support to the wealth effect explanation. A SEK 100,000 change in house prices over the past two years is associated with a 2.5 percent increase in fertility rates going from a group with zero percent home ownership to one with 100 percent home ownership. More meaningsfully, going from a group with 25 percent home ownership to one with 75 percent home ownership is associated with a 1.2 percent relative increase in fertility rates. We do however draw careful conclusions from this result as it is not fully robust and since there are limitations to the ownership data, but we note that the effect appears to be smaller than those reported by D&K and L&M. Third, and in contrast to the theory of housing wealth and fertility, we do not find that higher house price levels are significantly associated with differential fertility rate effects between groups with different ownership rates. Forth, we find that the estimates that we use to capture the wealth effect are substantially upwards biased when they are estimated on their own, without controlling for the age effect that we use to capture the option value effect. A corresponding large bias is not found in the estimates that we use to capture the option value effect without controlling for the ownership rate effect that we use to capture the wealth effect.

Although our findings are not proof of causality, they do suggest that a differential value of holding the option to postpone childbearing between young and old women may play a role in explaining the relationship between house prices and fertility rates. The results add to the literature that studies how housing wealth affects fertility rates by suggesting that estimates of the wealth effect that are obtained without controlling for option value effects across different ages may be biased. This paper also contributes to the literature that studies how fertility behavior responds to financial incentives and to the literature that studies the effects of housing market fluctuations, by demonstrating that fertility behavior appears to be among the set of variables that responds to house price fluctuations.

Important gaps remain in the research field studying house price effects on fertility rates. First of all, we see the need for higher quality data to fully credibly identify wealth effects in a study that also addresses the option value effect. We also believe that future research could take our study forward by formalizing the double dimension of house prices in relation to fertility, with the option value effect and the wealth effect, in a dynamic modelling framework, and also by studying the option value mechanism in an experimental setting.

The dynamics of housing market fluctuations on the one hand, and fertility behavior on the other, are complex and multifaceted fields of research. Hopefully, this paper makes a small contribution to the important endeavor of understanding how housing market fluctuations affect household behavior in general, and fertility behavior in particular.

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8 Appendix

8.1 Appendix A: Data description

Fertility rates

The data we use to construct the fertility measure is based on high-quality register based data from the Swedish Tax Authority (Skatteverket), which is reported to Statistics Sweden. Register based data is usually more reliable compared to self-reported data and thus contains less reporting bias. The number of person years of exposure in an age group, E_{iat}, is defined as the average number of women in a particular age, in a particular year. This is computed by taking the average of women in a certain age at the beginning and at the end of the year.

House prices

The purchase-price-coefficient for a given year in a given municipality is provided by Statistics Sweden and is calculated as the unweighted average of the sum of all ratios of the actual price of a sold house and the latest available tax assessment value of that same house. The Swedish Tax Authority conducts general tax assessments of real estate periodically. Thus, the coefficient indicates if houses are sold to a price over, at or below their tax assessed value.

The purchase-price-coefficient is consequently multiplied by the average tax assessment value of houses in a particular municipality in a given year. As such, the house price measure indicates, given the tax assessed values of houses in a municipality in a given year, to what price an average house would be sold at in the market. The house price variable is calculated as

$$HP_{it} = \bar{T}_{it} * \frac{1}{N} \sum_{n=1}^{N} \frac{K_{nit}}{T_{nit-s}} \tag{8.1}$$

where HP_{it} is the average house price in municipality i in year t, $\bar{T}_{it}$ is the average tax assessment value of all one- or two-family buildings used as permanent housing in in municipality i in year t, K_{nit} is the price a house n is sold for in municipality i and year t, and T_{nit-s} is the tax assessment value of the sold house n in municipality i in year $t-s$ where $s = 0, 1, .., 7$ (indicating the number of years from the last tax assessment).

To obtain real house prices, this data is deflated by the shadow consumer price index (CPI), also provided by Statistics Sweden. The shadow CPI implies an adjustment to the originally published CPI in order to correct for mistakes after the original publishing that otherwise would bias the index and display incorrect results.

Urban and non-urban municipalities

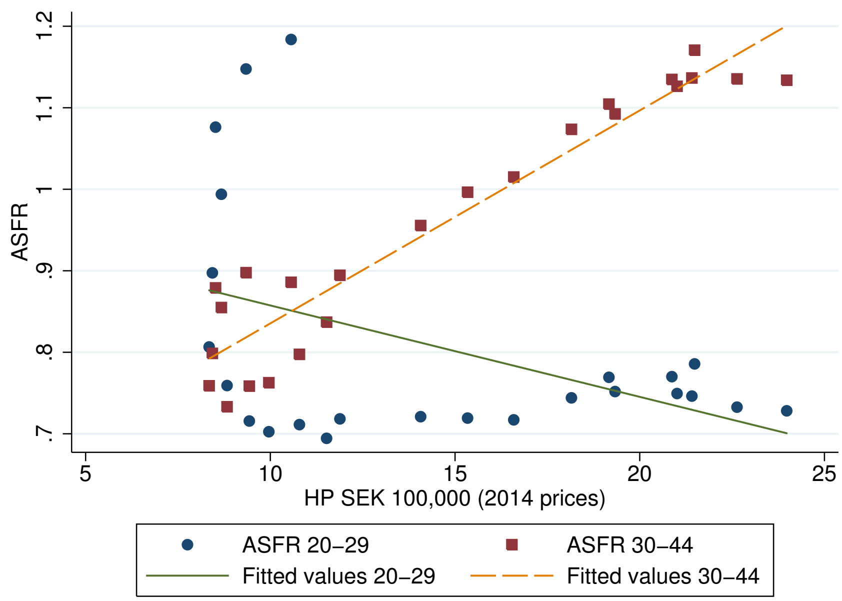
Table 5.3 reports results from the baseline specifications estimated on an urban and a non-urban sub-sample. The definitions of these sub-samples is based on a classification of municipalities developed by the Swedish Association of Local Authorities and Regions.

The following municipalities are defined as urban: Ale, Bollebygd, Botkyrka, Burlöv, Danderyd, Ekerö, Göteborg, Håbo, Haninge, Härryda, Huddinge, Järfälla, Kungälv, Kungsbacka, Lerum, Lidingö, Lilla Edet, Lomma, Malmö, Mölndal, Nacka, Nynäshamn, Öckerö, Österåker, Partille, Salem, Skurup, Sollentuna, Solma, Staffanstorp, Stockholm, Sundbyberg, Svedala, Täby, Tyresö, Upplands-Bro, Upplands Väsby, Vallentuna, Värmdö, Vaxholm, Vellinge.

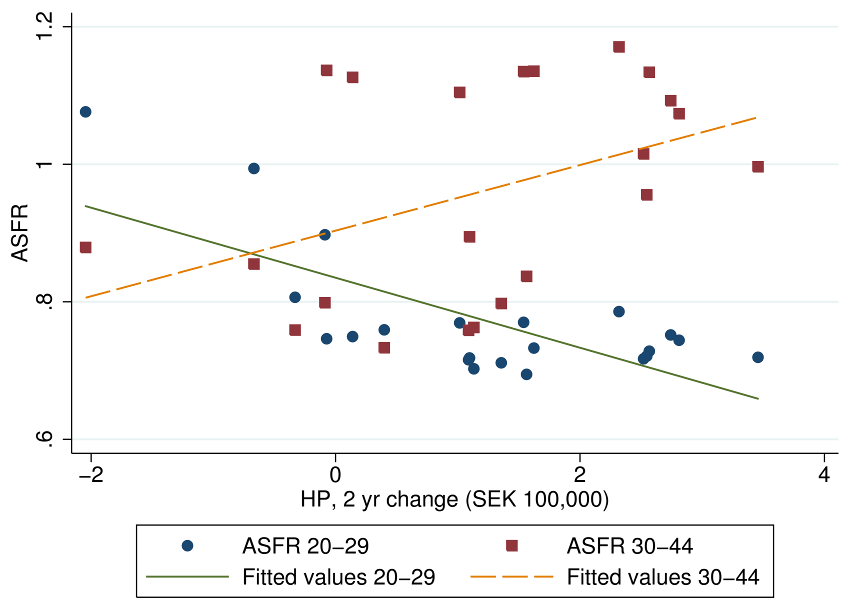
The following municipalities are defined as non-urban: Älmhult, Älvdalen, Alvesta, Älvkarleby, Älvsbyn, Åmål, Aneby, Ånge, Ängelholm, Arboga, Åre, Årjäng, Arjeplog, Arvidsjaur, Arvika, Åsele, Askersund, Åstorp, Åtvidaberg, Avesta, Båstad, Bengtsfors, Berg, Bjurholm, Bjuv, Boden, Bollnäs, Borås, Borgholm, Borlänge, Boxholm, Bräcke, Bromölla, Dals-Ed, Degerfors, Dorotea, Eda, Eksjö, Emmaboda, Enköping, Eskilstuna, Eslöv, Essunga, Fagersta, Falkenberg, Falköping, Falun, Färgelanda, Filipstad, Finspång, Flen, Forshaga, Gagnef, Gällivare, Gävle, Gislaved, Gnesta, Gnosjö, Götene, Gotland, Grästorp, Grums, Gullspång, Habo, Hagfors, Hällefors, Hallsberg, Hallstahammar, Halmstad, Hammarö, Haparanda, Härjedalen, Härnösand, Hässleholm, Heby, Hedemora, Helsingborg, Herrljunga, Hjo, Hofors, Höganäs, Högsby, Höör, Hörby, Hudiksvall, Hultsfred, Hylte, Jokkmokk, Jönköping, Kalix, Kalmar, Karlsborg, Karlshamn, Karlskoga, Karlskrona, Karlstad, Katrineholm, Kävlinge, Kil, Kinda, Kiruna, Klippan, Knivsta, Köping, Kramfors, Kristianstad, Kristinehamn, Krokom, Kumla, Kungsör, Laholm, Landskrona, Laxå, Lekeberg, Leksand, Lessebo, Lidköping, Lindesberg, Linköping, Ljungby, Ljusdal, Ljusnarsberg, Ludvika, Luleå, Lund, Lycksele, Lysekil, Malå, Malung-Sälen, Mariestad, Mark, Markaryd, Mellerud, Mjölby, Mönsterås, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Munkfors, Nässjö, Nora, Norberg, Nordanstig, Nordmaling, Norrköping, Norrtälje, Norsjö, Nybro, Nyköping, Nykvarn, Ockelbo, Ödeshög, Olofström, Örebro, Örkelljunga, Örnsköldsvik, Orsa, Orust, Osby, Oskarshamn, Östersund, Östhammar, Östra Göinge, Ovanåker, Överkalix, Övertorneå, Oxelösund, Pajala, Perstorp, Piteå, Ragunda, Rättvik, Robertsfors, Ronneby, Säffle, Sala, Sandviken, Säter, Sävsjö, Sigtuna, Simrishamn, Sjöbo, Skara, Skellefteå, Skinnskatteberg, Skövde, Smedjebacken, Söderhamn, Söderköping, Södertälje, Sollefteå, Sölvesborg, Sorsele, Sotenäs, Stenungsund, Storfors, Storuman, Strängnäs, Strömstad, Strömsund, Sundsvall, Sunne, Surahammar, Svalöv, Svenljunga, Tanum, Tibro, Tidaholm, Tierp, Timrå, Tingsryd, Tjörn, Tomelilla, Töreboda, Torsås, Torsby, Tranås, Tranemo, Trelleborg, Trollhättan, Trosa, Uddevalla, Ulricehamn, Umeå, Uppsala, Uppvidinge, Vadstena, Vaggeryd, Valdemarsvik, Vänersborg, Vännäs, Vansbro, Vara, Varberg, Vårgårda, Värnamo, Västerås, Västervik, Växjö, Vetlanda, Vilhelmina, Vimmerby, Vindeln, Vingåker, Ydre, Ystad. .

8.2 Appendix B: Figures

Figure 8.1: House prices and fertility rates for young and old women, fitted values



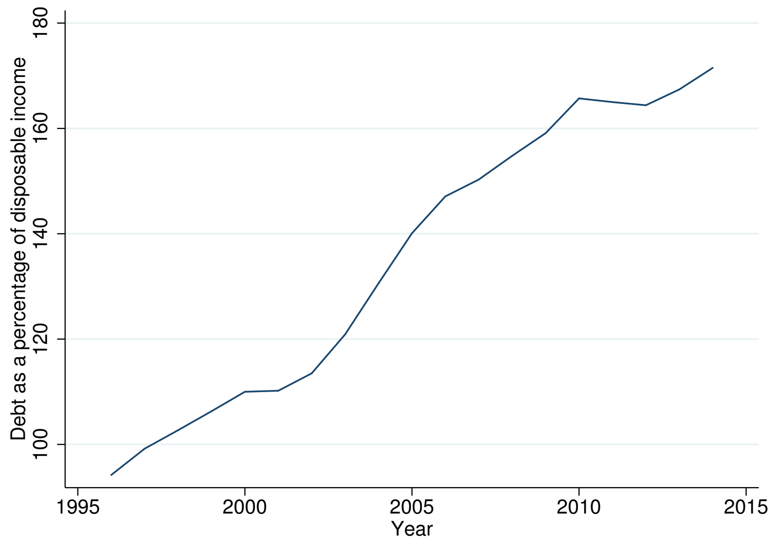
(a) House price levels and age specific fertility rates



(b) House price changes and age specific fertility rates

Note: The figures display age specific fertility rates for women 20-29 years old and 30-34 years old respectively plotted against real house price levels (a) and two years real house price changes (b), along with the respective fitted line. Data source: Statistics Sweden, the authors' own calculations.

Figure 8.2: Debt to disposable income ratio, 1996-2014



Note: The figure displays the development of the aggregate debt-to-income ratio in Sweden 1996-2014. Data source: Statistics Sweden/Sparbarometern and national accounts.

8.3 Appendix C: Robustness tests

Table 8.1: House price changes and fertility: Different number of years in changes, 1993-2014

Dep.var $lnASFR_{igt}$	(1)	(2)
1yrΔHP_{it-1}	-0.0560***	
	(0.00654)	
1yr$\Delta HP_{it-1} * Old_g$	0.0787***	
	(0.00921)	
1yr$\Delta HP_{it-1} * Own_{ig}$	0.0471**	
	(0.0178)	
4yrΔHP_{it-1}		-0.000311***
		(0.0000411)
4yr$\Delta HP_{it-1} * Old_g$		0.000469***
		(0.0000487)
4yr$\Delta HP_{it-1} * Own_{ig}$		0.000364**
		(0.000129)
N	12496	11360
Adj. R-squared	0.671	0.762

Notes: Results are reported from estimating the baseline specification with one year house price changes (column 1) and four year house price changes (column 2) as the main independent variable. Huber-White standard errors clustered at the municipality level are in parentheses. All specifications are weighted by female population per age group, municipality and year. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8.2: House price levels, house price changes and ownership in one regression

Dep.var $lnASFR_{igt}$	(1)
$2\text{yr}\Delta HP_{it-1}$	-0.0145***
	(0.00183)
HP_{it-1}	-0.0142***
	(0.00159)
$2\text{yr}\Delta HP_{it-1} * Old_g$	0.0219***
	(0.00184)
$\text{HP}_{it-1} * Old_i$	0.0236***
	(0.00219)
$2\text{yr}\Delta HP_{it-1} * Own_{ig}$	0.00895
	(0.00725)
$\text{HP}_{it-1} * Own_{ig}$	0.00999
	(0.00903)
N	12496
Adj. R-squared	0.812

Notes: Results are reported from estimating the baseline specification with both house price levels and house price changes and their respective interaction terms with the old age group dummy and the ownership variable. Huber-White standard errors clustered at the municipality level are in parentheses. The specification is weighted by female population per age group, municipality and year. Control variables are included but not reported: share of women in each age group with higher education, employment rate per age group, total employment rate at the county level, average income at the municipality level, a dummy variable indicating the old age group and a variable for the ownership rate per age group. Data source: See table 4.1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.