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Causality within the price elasticity of the housing supply: An instrumental variables approach

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Abstract

The goal of this research is to find the causal effect of housing prices on housing supply changes, captured within the concept of the price elasticity of the housing supply. This is attempted by applying an instrumental variables approach with municipal fixed effects to common empirical models used for estimating this elasticity. IV is utilized with the main aim of eliminating simultaneity bias, which is expected to be a major source of bias within the commonly used models. Municipal real estate taxes and local crime rates have been selected to serve as instruments for housing prices in the model(s). An elasticity of 8.9 was estimated within the preferred specification, which is clearly higher than elasticities found in other studies. This difference might be caused by the elimination of negative bias, but conclusions rely strongly on the unprovable validity of the instruments. Regardless, comparisons between the new IV models and standard variants imply that the latter yield inconsistencies likely caused by simultaneity. Furthermore, lagging price and cost variables does not seem to offer a solution to this as is suggested in previous research. In conclusion, no guarantee can be given on the causality of the found elasticity of 8.9, as assumptions are only plausible to hold. However, it can be argued that simultaneity does indeed seem to be a threat to the causal interpretation of current housing supply models, and different methodologies might be able to assist in finding more accurate ones.

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I. Introduction

In the recent years the housing market in the Netherlands has been characterized by rising house prices and demand surpluses. Asking prices for homes are regularly overbid, making it difficult for people to find and buy an house within their budget. It has been especially troubling for starters and lower income groups to acquire their first house as an owner, as most competition for houses appears in the somewhat lower price categories (Geyer & Sieg, 2013). The fact that mortgage terms have become stricter since the housing crisis of 2008 has also contributed to the development of these circumstances. At the end of the line, the main problem is that demand for houses is larger than housing supply, primarily for houses in medium to low price ranges. Theoretically, a demand surplus like this should be offset by an increase in housing supply. As house prices increase, the incentive for investors and housing corporations to build new homes increases as potential profits are larger. This is however held back by local and regional building restrictions, which could prohibit new construction as designated space for housing runs out or certain building requirements need to be met. Also, there is a substantial lag in the building of new houses as construction is a process over multiple years. This means that generally supply reactions are delayed as a result. The ability of housing supply to react to price fluctuations, is measured by the price elasticity of housing supply (Green & Malpezzi, 2005). This metric represents the relative increase in the housing supply proportional to the rise in prices. A better understanding of the causal inference of this concept could help aid governmental institutions in making decisions in terms of regulation to balance the housing market.

Multiple studies on this subject have used very similar models to estimate these supply elasticities, namely explaining housing supply by prices as well as some other related variables (CPB, 2017; Caldera & Johansson, 2013). The regular variables controlled for in these models consist of for example population size, construction costs and interest rates. Possible flaws in this model design could be the presence of omitted variable and simultaneity bias. The equilibrium house price is namely determined by demand and supply simultaneously. The market price reacts to demand as well as supply shocks, making it more difficult to estimate the elasticity assuming constant demand.

Omitted variable bias implies that one or more variables affecting both the independent and dependent variable are left out, resulting in a biased estimate for the relation between the two. This problem often holds back regression models as many of these omitted variables are either not observed or not quantifiable. Simultaneity bias on the other hand occurs if the dependent variable affects the independent variable at the same time when the opposite effect is tried to be identified. This type of bias could cause even bigger problems, as the probability that house prices are affected by the supply is rather large. Motive for this particular problem has been found in previous studies (Blom, 2019), but definitive identification of simultaneity bias in the price elasticity of housing supply has yet to be found. Both omitted variable bias and simultaneity bias lead to the violation of the zero conditional mean assumption in regular regression models, causing inaccurate estimates which cannot be interpreted as causal. The zero conditional mean assumes that the error term of the regression is uncorrelated with the dependent variable (Wooldridge, 2016).

The causal relationship between house prices and housing supply would yield the possibility for detailed calculation of policy effectiveness in terms of housing, as well as more accurately allocated local policies in the future. This all sustains the scientific and societal relevance for identifying the causal effect of pricing on new construction. Therefore, the research question addressed in this thesis will be stated as follows;

What is the causal effect of housing prices on the housing supply in the Netherlands?

Many researchers have looked into the price elasticity of housing supply, mainly aiming to provide specific numbers in terms of elasticities from which conclusions on the market can be made. Due to this broader objective of comparability between different countries or regions, as well as between different time periods, it is important to make accurate and justifiable claims on these real estate markets. However, the literature on this topic provides many different techniques to estimate the supply elasticity. This leads to the fact that elasticities for the same sample are often varying, and variations can most likely be explained by methodological differences. For example, the Netherlands Bureau for Economic Policy Analysis (CPB, 2017) estimates an elasticity between 0 and 1.4 depending on the economic conjuncture, the OECD (Caldera & Johansson, 2013) estimate an elasticity of 0.19

and Vermeulen and Rouwendal (2007) estimate a relation between price and supply of 0.04 in the short run a d 0.1 in the long run.

Most of these models used to estimate the price elasticity of housing supply are common in that they are based on multiple variable regression models. These models are generally not used to identify causal relationships, as the zero conditional mean assumption is very hard to satisfy. As a consequence, most estimations on the relationship between prices and supply fail to resemble their actual effect on each other. For this reason, this research will apply different methodological techniques to estimate the relationship, mainly by making use of instrumental variables (IV). The results pursued by this research are hence not necessarily meant to be comparable to other research on this subject, but are rather oriented at providing insights on what happens as a consequence of shocks in house prices. This could help policy makers in anticipating surpluses or shortages in demand or supply and pro-actively engage in the market to reduce these distortions. In order to do this as effectively as possible, the causal relationship is needed to accurately predict supply reactions.

Whereas plenty of research has been done on the determinants of house price, extensive research on the determinants of supply is still lacking (Green et al., 2005). For this reason Green et al. themselves researched the price elasticity of the housing supply. Similarly to research by Saiz (2008, 2010), their research concluded that spatial differences in supply elasticities are primarily the result of policy and geographical aspects. Both geographical differences and differences in terms of local policy are difficult to incorporate in the modelling of supply elasticities, either because they are not observed well enough or not quantifiable. This is one of the main reasons that this research is looking towards IV, as some omitted variables like these can theoretically be left out by construction and a good instrument could eliminate any simultaneity bias. The instruments proposed to predict house prices are criminality and real estate tax. The relevance and validity of these instruments will be argued later on in this paper.

This paper will further consist of the following components; a theoretical framework elaborating on existing scientific literature on related topics to create a foundation. Thereafter the data and methodologies used for estimating the relationship between house prices and supply will be discussed. Subsequently the results of these statistical methods will be presented, followed by their interpretation and a discussion of the results. Finally, a conclusion will be made in terms of the central research question, paired with a reflection on the analysis highlighting possible flaws and interesting findings.

II. Theoretical framework

As stated before in the introduction, the relationship between house prices and supply is usually captured within the price elasticity of the housing supply. To understand this term and the connection between prices and supply, firstly attention will be given to the determinants of house prices and supply individually. After that more in depth insights on the elasticity will be covered including possible biases, followed by how this elasticity is generally modelled. Afterwards the reasoning for flaws in the methodology of these models, including simultaneity bias, will be discussed providing the incentive for this particular research. Finally, the hypotheses to assess the research question will be presented.

House price dynamics

The prices of houses are often uncertain and fluctuate over time. Only for actual transactions, in which an house is bought, the exact market price of that property can be determined. The transaction price is equal to what people are willing to spend on that particular property at that moment in time. This price does not have to be equal to the real value of that property. Actually, a mismatch between real value and actual prices are very common in the real estate market. An example of this is the overvaluing of houses by (future) home-owners because of a temporary bubble in the market (Taipalus, 2006). Such a bubble occurred just before the financial crisis of 2008. Transaction prices did not represent any real worth of the properties anymore, but were inflated as demand kept rising due to financial incentives of banks making them want to issue more and more mortgages. These bubbles are hard to identify in advance as real property worth is not observed.

Another factor fueling these bubbles and making them more difficult to identify, is an often occurring misconception made by people that house prices will keep on rising when they have been in the past. This is demonstrated by the behavioral heuristic of mainly nonprofessional investors, what regular house buyers essentially are, to base future values on historical trends (Bikas et al., 2013). The expectation of rising or declining house prices mainly effects market wide prices, as expectations about future prices affect overall demand. If potential home owners and investors expect prices to rise, they are more likely to engage in the purchasing of a property regardless of where they are from or how much they earn. However, this effect is more true for wealthier people as they have more funds available for spending.

The supply of housing also effects these average market prices. As supply is lower, relative demand is high meaning people will outbid each other more easily. The supply of housing is largely dependent on actual construction cost and spatial policies. These factors do generally not influence differences in prices between properties, but more so affect the common price trend of all properties (DiPasquale & Wheaton, 1994). Policies aimed at specific housing types do although affect the relative prices of these house types. For example, governments tend to subsidize social housing projects to make those house types more affordable for lower income people. This is only possible because of the restrictions implemented for people to be considered for social housing, as relative prices of houses are predominantly determined by demand factors. More detail on the supply of housing and its determinants will be given further on.

The demand for housing on the other hand is determined by the characteristics of a property. These characteristics can be split up into 2 primary categories, namely factors based on physical attributes and factors based on location (Malpezzi, 2002). The physical attributes determine the type of dwelling. In reality almost all houses differ in at least a few of these characteristics, meaning that residential real estate consist of mainly heterogeneous goods. Examples of physical attributes are floor space, volume, energy label, age, but also appearance in terms of esthetical value and the incidence of light are valued by buyers. Locational factors take into account the surroundings of that property. Houses close to certain amenities are generally valued higher compared to houses equal in physical attributes but further from these amenities (Kiel & Zabel, 2008). Examples of such amenities are shopping facilities, beaches, bars and everything else people like living near to. Most of the locational factors are positive like the ones mentioned, but there can also be negative locational factors, like a neighborhood with a lot of crime or a plant in the vicinity.

Monocentric city model

Amenities often cluster in the center of the city. Therefore, a clear relation can often be observed between the price of houses and their distance to a major city center. This theory is captured within the monocentric city model, which is a well-known and intuitive model to explain house prices based on location (Alonso, 1964; Mills, 1967; Muth, 1969). As the name suggests, the monocentric city model assumes there is only one central business district (CBD) in which all amenities, facilities and jobs are situated. The intuition of the monocentric city model is that prices of houses decrease linearly with the distance to the center, compensating for the increasing commuting cost. The house prices at the edge of the city are determined by the yield of agricultural land which covers all space outside of the city edges. In equilibrium, the yield of farmland is equal to the house value per square meter on the edge of the city. There is no incentive to build new homes in equilibrium, as new houses would have to be build further from the city center, meaning that their value would be lower than the yield on farmland. However, when house prices would increase, there will be incentives to build new homes just outside of the current edge of the city as homes would have a higher value then the existing farmland. This demonstrates the effect of changing house prices on the supply of homes. An increase in overall house prices lead to increasing stimuli to construct more homes as their yield increases.

As mentioned the monocentric city model assumes that all jobs and amenities are located in the center of the city, of which there is only one. Furthermore, this model also assumes that the city approaches a circular shape. These assumptions are not often exactly met, but they do match a simplified version of most cities. An intrinsic assumption which the model also makes, is that farmland is always available outside the city borders and can be used as building space. Many cities in the world have however grown to their geographical or zoning limits. This means that if prices increase in those cities, supply cannot be increased by utilizing leftover space. What will happen in reality, is that prices drive up even faster as a result of the demand surplus, or that buildings start to reach higher to utilize space above the city. A final assumption which is not realistic, is that all houses in the monocentric city are exactly similar. However, the purpose of this is only to value the locational component of the house prices, which this model intends to do. Also, in practice this problem can be solved by applying hedonic pricing models which correct for al observed physical characteristics of homes, leaving only a value per square meter unrelated with house type (Li & Brown, 1980; Malpezzi, 2002; Sirmans et al., 2005). For these hedonic house prices, the monocentric city model should be able to predict values based on location more closely.

If the other assumptions regarding the circular shape and limited number of city centers are relaxed, this model resembles reality quite accurately. Of course cities are not exactly circular, but they do tend to be clustered around the (historic) center. If the shape of cities is more irregular, that can mostly likely be blamed on geographic factors restricting the growth of the city in one or more directions. For these irregular shaped cities, it theoretically still holds that houses become cheaper the further they are located from the city center. The amount of centers poses a more complicated discussion. Especially larger cities tend to have more centers in which jobs and amenities like shopping and services are clustered. Usually there is still one main CBD which is located very centrally relative to the rest of the city. In addition, there are a few secondary centers which facilitate for example a cluster of offices or shops, or even large shopping malls. Even most neighborhoods themselves have some sort of small center with a few shops for more primary needs. If these multiple centers are incorporated the monocentric city model turns into a polycentric city model. This model functions essentially in the same way. Prices become lower the further a location is from one any of the centers (Fujita & Ogawa, 1982). Living close to some centers is more valuable than to others, meaning the magnitude of a price increase towards a center depends on which of the centers a property is close to. All things considered, the monocentric city model discussed offers relatively realistic insights to how prices are affected by location and how those prices impact the housing supply.

Supply elasticity and simultaneity bias

In the previous section prove was granted on the effect of prices on supply and the intuition behind the price elasticity of housing supply. The supply of housing is very complex and not only reliant on house prices however. Other determinants of supply are required to create valid models for estimating this supply, as omitted variables related to price as well as supply create biases in multiple regression models (Clarke, 2005). Likewise, simultaneity bias is expected between price and supply as price is determined by demand as well as supply simultaneously. Both of these biases distort the statistical relationship found in regressions, disabling them from identifying actual causal relationships. An instrumental variable regression can overcome omitted variable biases as well as simultaneity bias internally, which is why this methodology will be primarily used to acquire a causal estimate (Wooldridge, 2016).

The problem of simultaneity bias within the price elasticity of the housing supply will be further elaborated on to provide literary evidence on its complications regarding the estimation of any causal relationship. As stated before, the main problem in terms of simultaneity is that the market price of houses is determined by demand and supply simultaneously. The supply of housing, which could be seen as the available housing stock, affects the market price through a demand effect. If less houses are available whereas demand stays equal, the relative demand per dwelling is increased meaning people compete more on buying houses. This increased competition results in higher prices as people try to outbid each other on individual properties. This mechanism follows the expectation of standard economic theory predicting increases in prices as a result of rising demand, which also holds for the opposite effect. The effect of supply on price thus actually corresponds to a more basic economic intuition, where the usual thought behind the supply elasticity is aimed at identifying the effect of price on supply as a result of changing incentives.

This simultaneity bias is very hard to obviate for the more basic and often used regression models. Bramley (1993) recognized this problem and concluded that previous models did not account for this bias. He therefore proposed a lagged response model in which the supply is determined by lagged values of price, whereas these prices are in turn determined by current demand and supply. According to Pryce (1999) this approach only moves the simultaneity problem towards a different period and is not a solution for this type of econometric problem. He argues that that a lagged response model could only work if it can be assumed that the lagged value of price is exogenous. This is highly unlikely as price is modelled as demand relation, and demand is determined by both price and supply. Pryce therefore concludes that using this type of model still yields inconsistent results caused by simultaneity.

However, many studies in more recent years still employ lagged response models similar to Bramley's, including the studies of the OECD (Caldera & Johansson, 2013) and the CPB

(2017). Therefore, this research will attempt to eliminate this simultaneity bias by using an instrumental variables regression. This type of setup predicts values for the house prices with the use of variables (instruments) exogenous to the dependent variable. The predicted values which are used as independents can therefore not be causally influenced by the dependent variable eliminating simultaneity bias or reverse causality. The success of this type of method is however reliant on the validity of the instrument used, as instruments which lack validity will still provide biased results. This will further be discussed in the methodology section. To evaluate whether the instrumental variables regression is actually more effective to estimate the causal relationship, the coefficients of the regular regression are required as comparison. The model which will be used as comparison and basis for the IV model(s) will be based on existing models used in previous research.

Model basis

As a standard model representing the mechanics of the supply elasticity of housing, the model used in studies of the OECD (Caldera & Johansson, 2013) and the Netherlands Bureau for Economic Policy Analysis (CPB, 2017) are chosen. Both of these studies are carefully conducted by trustworthy economic institutions on either the European or the Dutch national market. The CPB themselves used the model of Caldera and Johansson as bases for their own methodology, making their models relatively similar and a solid reference point for existing literature on this topic. In this model the newly developed real estate Δs is explained by house prices p, construction costs CC, the real interest rate r, population growth pop and quarterly dummies γ_t.

$$\ln(\Delta s_t) = \beta_0 + \beta_1 \ln(p_t) + \beta_2 r_t + \beta_3 \ln(CC_t) + \beta_4 \ln(pop_t) + \gamma_t + \varepsilon_t$$

The reason for adding these control variables is that according to the CPB (2017) corrections should be made for fluctuations in price as a consequence of supply shocks when estimating the supply elasticity. These control variables explain supply levels conditionally on price in this model to correct for such shocks in supply. House prices tend to be higher in metropolitan areas, and housing supply could be lower or higher for these specific locations depending on the availability of building spaces. Hence population is used to control for this variance. The interest rate on the other hand controls for economic circumstances. Higher interest rates affect the ability of people to afford mortgages, as well as the real profitability of new housing projects for developers. Finally, the construction costs are also important decision factors when starting housing projects, and could therefore help to explain some of the variation in the housing supply.

Usually for multiple regression to approach any sort of causality, far more extensive models are required with numerous amounts control variables. All control variables needed to account for omitted variables are most likely not all observed or not even known. The long term interest rate and capitalization rate are potentially two of those variables, which therefore will be added in the model used in this research. Earlier research found suspicion on these variables possibly solving a part of the bias in the regular model (Blom, 2019). No other omitted variables could be identified for which data is available, so these two will be the only additions. Reasoning for why the addition of these variables might reduce bias will now be given individually on each of the variables.

First of all the long term real interest rate, which does not create similar effects to the short term interest rate on the supply elasticity and should therefore be considered. Levin & Pryce (2009) argue the importance of the long term interest rate as construction is a long term project and thus mostly yielding returns on the long term. When evaluating the profits of constructing new real estate, the real return is establish by comparing the project return to the long term yield of capital. An interesting way to incorporate the long term interest rate is by taking the spread between that long term interest rate and the short term interest rate. This so called long term interest spread is often seen as a sign for recessions and economic circumstances in finance theory (Dotsey, 1998). Therefore this spread can affect the house prices as its predictive nature in terms of economic state influences the demand for housing. Also this spread can explain part of the variance in housing supply as the long term interest rate is taken into account when starting new projects, implying that it should be incorporated in the regression as it is theoretically correlated to both the price and supply.

Another factor mentioned by Levin and Pryce (2009) to be added to supply elasticity models is the capitalization rate, which is basically the ratio between rent levels and house price. This metric is also a vital part of the four quadrant model of DiPasquale and Wheaton (1992) in which the interaction between the different parts of the real estate market is captured in an intuitive framework. In this framework the capitalization rate is determined by demand in the market for space fueled by the economy, and in turn impacts construction market in terms of a changing incentive for developers to build new housing. As the capitalization rate increases, rent levels increase conditionally on price. If higher rents can be realized on the same investment, the incentive to build increases as potential profits are higher. The capitalization rate is therefore a key mechanism in the relation between demand and supply.

Other remaining bias

Although adding variables could reduce omitted variable bias, it is unlikely that all of this bias can be solved. Some omitted variables are not identifiable or not quantifiable. For example, one of the main limitations for construction and thus the housing supply are geographical and regulatory factors. These factors are generally hard to quantify and not used as explanation for housing supply within models, but rather to explain differences in found elasticities. Rough terrains restrict the possibility of building or make it that much more expensive that the profitability is severely harmed. Examples of such conditions are mountainous terrains, marshes or simply bodies of waters. Saiz (2010) concluded that for example sloped terrains constrain the development of new housing development making supply more inelastic. In the Netherlands, not much rough terrain restricts us from creating new homes. At some locations soil is a bit too soft leading to the sinking of buildings over long periods of times, and of course the sea, as well as multiple lakes, rivers and canals are deemed unable to build on.

What restricts construction in the Netherlands far more clear, is the existence of local and regional policy on new housing development. The regulatory bodies determine where can be build and where not, as well as what a potential building in a certain location must comply to. These policies distort the equilibrium on the real estate market, and result in slower reactions of supply on price fluctuations. Eventually this often causes a shortage of supply, mainly because of a lack of available space. Especially in larger cities there are shortages of land available for new housing development (Glaeser & Gyourko, 2003; Saiz, 2008, 2010). The lack of space is mainly due to rules preventing constructors to build in certain locations, for example to retain green areas or because those areas are destined for other purposes by law. These restrictions also influence the land cost as the value of that land is dependent on what can be developed on top of it. Grimes and Aitken (2010) concluded that land cost do also influence the reaction of supply to price shocks, thus affecting the supply elasticity. They find that higher land cost lead to a weaker supply response in case of such price shocks. The effect of local policy does therefore not only affect supply directly, but also through mechanisms like land costs.

Apart from these supply restrictions, there is also a lot of policy aimed at the demand side. The (local) governments often claim taxes on housing in many different ways, and these taxes vary depending on the type of house as well as the occupant. All policies regarding housing together influence the prices of housing as well as the construction of new housing. These regulatory factors also vary per province and municipality, as local governments make most of the decision in terms of housing in that particular area. Variations in empirically found supply elasticities are most often attributed to these differences in local policy, as those policies are known to influence the elasticities but cannot be incorporated in most models (Anundsen & Heebøll, 2016). This implies that these policies do in fact result in omitted variable bias. It is very hard, or even impossible, to quantify these regulations effectively, but leaving them out ensures that found elasticities can only be used to predict supply reactions under exactly equal circumstances. Policy is a very clear example of an omitted variable, but undoubtedly more omitted variables exist when creating regressions with just a few control variables. As stated before, it is simply impossible to identify and quantify all omitted variables to control for the bias created by them. This is the second reason that this research looks towards IV to potentially identify the actual causal estimate of reactions in supply resulting from price changes.

Hypotheses

The literature ratifies the plausibility of at least some sort of bias being present in previous research on the price elasticity of housing supply. This bias is mostly recognized either directly or indirectly, but often do not pose major implications for these researches as they are not aimed at providing the exact causal relationship between housing prices and supply. This research does however try to identify this causal relationship employing an Instrumental Variables regression. To analyze if this methodology has resulted in more accurate results in terms of causality, the following hypotheses have been formulated:

Hypothesis 1: The instruments used are (statistically) viable based on the assumptions of relevance and validity

Hypothesis 2: Standard models reflect biased results compared to instrumental variables models when estimating the price elasticity of the housing supply

Hypothesis 3: The price elasticity of the housing supply is positive and statistically significant

The first and second hypotheses are necessary to answer the third and final hypothesis, which is closely related to the central research question. Both of these first two hypothesis examine necessary conditions for the causal implications of the found elasticity. Hypothesis one provides evidence for the reliability of the instruments used. The expected relevance and validity of the instruments will be theoretically supported in the methodology section. The second hypothesis is installed to examine the improvement in the found estimate for the IV models in terms of causality compared to standard models used in previous research. It will determine whether the IV models are actually more accurate and effective when estimating the supply elasticity. The third and final hypothesis then assesses the actual (possibly causal) effect of housing prices on the housing supply. Like economic theory predicts, this final hypothesis states that the supply elasticity is positive, and thus implies a positive effect of housing prices on their supply. One of the instrumental variables models is expected to yield the most realistic estimates in terms of the price elasticity of the housing supply, but this is reliant on the outcome of the second hypothesis.

III. Data

The main variables used to estimate the price elasticity of housing supply are the house prices and the issued building permits for new houses. This research chooses to use data on these variables originating from Statistics Netherlands (CBS), which is the Dutch governmental agency for statistics. This data source contains an extensive amount of data on economic topics as well as on other general subjects like demography and education. A major upside of using data from Statistic Netherlands is that it is administrative data, making it reliable information from an independent source, consistently collected over multiple decades. Furthermore these datasets contain aggregated or location specific data taking into account every person, transaction or event relevant in contrast to private data providers like the NVM (a Dutch branch organization of real estate agents and appraisers), who only keep track of data coming from people or entities associated with them. This makes the data more complete and not likely to be skewed to any relevant sorting variable as it is not a sample of the total population. For example, if only elite real estate agents would be member of the NVM their data is more likely to include intelligence focused primarily on more expensive homes, neglecting another part of the market.

Many other variables necessary are also collected from Statistics Netherlands, for equal motives as discussed previously. Using multiple datasets from the same source also guarantees a good fit in terms of locational components of these data. The total list of variables retrieved from Statistic Netherlands consists of house prices for owner-occupied houses, number of building permits for owner-occupied housing, criminality figures, population, construction costs, average rent increases and inflation values/price indexes. The remaining used variables; real estate tax, the interest rate and the spread between the short and long term interest rate respectively originate from the Dutch central government, Eurostat and a combination of data from Eurostat and Trading Economics for the spread.

Data description and transformations

The final dataset consists of a balanced panel with annual data on all of the mentioned variables. The cross-sectional component of this panel is municipal specific, meaning yearly averages or totals per municipality are used. The timeframe contains the years 2012 up to and including 2019. There is a total of 2840 observations, divided into 355 subjects (municipalities) over eight years. Annual data is used as most of the variables were not documented on a quarterly basis. Therefore using quarterly data would restrict the estimation of the effects of these variables from quarters within the same year, decreasing the viability of the regression as a whole. The choice for municipalities as geographic areas has been made in the faith of improving accuracy, as this is the smallest geographical classification possible. Furthermore, many of the market dynamics of the real estate market operate on a very local level. These local dynamics include municipal taxes, local demand and geographical limitations, which mostly correspond to municipalities rather than larger areas. A relatively minor downside of such a small locational component is that spillover effects are not considered as much. The CPB (2017) mentioned these spillover effects as an advantage of using a larger geographical scale, in their case; COROP regions. Neighboring regions could possibly act as substitutes for construction, but this effect seems to occur on larger scales as well as is described by Beenstock & Felsenstein (2015). Overall, these spillover effects do not nearly weigh up against the lost accuracy and efficiency of larger locational components.

Although the most important variables are region specific, some of the other variables do not vary across locations but are determined on national level. The short term real interest rate as well as the spread between short and long term are based on yields of Dutch government bonds with maturities of two and ten years, which are equal for all locations. The construction costs are also assumed to be roughly equal across the country. These could practically differ between locations further apart, but data on regional specific construction costs are very limited and would deal more harm than they would contribute. Finally the rent increase was not available regionally as well, and therefore the annual maximum rent increase set by the government is used to proxy for this.

For the municipal specific variables, an important transformation had to be made to make the data complete and consistent across time. The problem causing this transformation is that the composition of municipalities in the Netherlands changes yearly. In the year 1900, there were a total of 1100 municipalities in the Netherlands, whereas there are only 355 since the beginning of 2019 (Government of the Netherlands, 2019). This process is called municipal reshaping, and is primarily initiated by local governments themselves as a measure to become more efficient. Along the timespan employed in this research (2012-2019), the number of municipalities has fallen from 415 to 355, a decrease of 60. This means that for every year, there was a different set of regions in the dataset. To resolve this issue, all regions in previous years are transformed to their current equivalent. For example, if two municipalities have merged in 2014, the data from that new municipality is used from 2014 onwards, and the sum or weighted average of those two initial municipalities is used in the years before. Only for housing prices, a weighted average had to be used, as other variables can be accumulated to a total across all previous components of a municipality. This weighting was based on populations of the previous municipalities, as this is comes closest to their relative share in the joint housing market. Finally, there have also been a few cases in which municipal borders have been adjusted. For these cases, weights have been calculated also based on populations of the regions within these border adjustments.

Price and supply of housing

As mentioned earlier, the two key concepts in estimating elasticities are the prices and the supply. As they can be captured by multiple differing metrics, the choices and types of variables used to quantify these concepts will now be elaborated on. First of all, the supply of housing, which is a relatively complicated term as the moment of actually initiating the project is very different from the moment it becomes part of the real estate market. Objectively speaking a house becomes part of the actual supply when it is finished and available. On the other hand, the intention to add supply to the market has been created much earlier in reaction to changing influences. Also, a lot of new houses or apartments are already sold during or before construction nowadays. The actual effect of housing supply on the market thus occurs with the intention to construct new housing. In this research, the (additions to the) housing supply will be defined as the amount of newly issued permits for new housing development. This matches with other national and European research from the CPB and OECD (CPB, 2017; Caldera & Johansson, 2013).

Building permits are an accurate indication of changing economic incentives to construct new housing, and are affected by current developments in the market rather than delayed effects like many other proxies would. Newly finished homes could for example also be a very logical variable to employ, as that variable literally measures how much the total supply of housing has been adjusted. However, as the building process of homes usually takes up multiple years, the amount of newly finished houses is more so a reaction on previous market circumstances. Finally, the total monetary investments of all permits can be argued to be a relevant indicator, but this variable can also respond to shifts in preferences and is therefore lesser of a pure supply indicator. In Table 1 some general statistics can be found on the permits as well as the other variables. The amount of issued permits varies from zero to a maximum of 2250 for one of the municipalities in a certain year, whereas the average is measured around 91 permits per region per year. This variance is both present between and within the municipalities, in depth panel statistics discriminating between these can be found in Appendix 1. The within variation is more important for this analysis, as municipal fixed effects are employed which means there will solely be looked at individual changes per municipality. The standard deviation within the municipalities is 81.84, which is a clear variation sufficient to be used for analysis.

Table 1: Descriptive statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Building Permits	2,840	91.42	151.31	0	2250
House Price (in €)	2,840	250808.50	71452.53	120209	872210
Population	2,840	47803.48	69873.53	919	862965
Construction Cost (in €/m2)	2,840	757.75	47.53	694	846
Rent increase (in %)	2,840	1.25	1.13	-0.1	3.4
2Y real interest rate (in %)	2,840	-3.11	0.79	-4.51	-2.27
2-10Y interest rate spread (in %)	2,840	1.20	0.40	0.58	1.77
Real estate tax (in %)	2,840	0.12	0.03	0.04	0.27
Criminal cases	2,840	2683.15	6877.97	25	101650

Similarly, the average house prices per municipality also varies widely, with a minimum of around 120,000.- euro and a maximum of over 870,000.- euro. Again, there is also a lot of variation within the municipalities themselves, with a standard deviation within municipalities of 27,898.- euro. The price of housing is set as the average real selling price of all transactions within the same municipality within a certain year. This real housing price is derived by taking nominal selling prices and deflating them according to inflation/consumer price indexes (CPI) retrieved from the CBS. The base year of these indexes is 2015, as provided by the CBS by default. These prices are well documented and easily comparable. Normally, hedonic prices would yield more accurate results as they compensate for different physical properties of a dwelling. Hedonic models take into account basic variables like floor area, number of rooms, amount of bathrooms, etc., but also more luxurious properties like access to a garage or swimming pool for example. The idea of such a model is that all attributes are priced, leaving only a per square meter value which should theoretically only be determined by location. The reason that this type of model is not necessary for this research, despite its usefulness, is that only price changes are used in the model. As real estate in a larger area like a municipality usually lasts for decades or even centuries, it could be argued that the composition of dwellings does not vary significantly over a few year time span. If this is roughly the case, changes in regional price averages should be equal to changes in hedonic prices. The logic behind this is that if the properties of dwellings do not change, the changes in price can be allocated solely to non-physical factors. The variables utilized to quantify price and supply in the real estate market are thus likely to be trustworthy of yielding accurate results based on economic theory.

Instruments and remaining variables

Apart from housing price and supply, the used instruments are also key variables for this research. These instruments have to comply to a few assumptions, and therefore require a careful selection process. The first instrument being crime, is defined as the yearly total amount of registered criminal cases in a municipality. The second instrument, real estate tax, is the pro rata tariff on residences as set by the local municipal government. Both vary significantly over time and between regions as can be seen in the Appendix 1. As the instruments need to predict house prices, variation in these variables is necessary or else price would be predicted to be nearly constant disabling the model to detect any realistic elasticities. Further explanations on these instruments and their specifications are given in the methodology section discussing the instruments' relevance and validity.

The remaining control variables are majorly quantified in accordance to previous research (see e.g. CPB (2017), OECD (Caldera & Johansson, 2013)). The population is defined as the average total population per municipality on the 1st of January. For the construction cost, the average real cost per square meter solely for owner-occupied houses is used. The two year real bond yield on Dutch government bonds is used as the short term interest rate, whereas the spread between short and long term rates is defined as the difference between the yields of this bond with a two year maturity compared to the same bond with a ten year maturity. Finally, the capitalization rate between rent and buying prices of homes is taken into account by adding the nationwide maximum allowed real rent increase set by the government. The construction costs, interest rates and rent increases are all deflated using the CPI in a similar fashion to the real housing price. The correlations between all of the variables employed are displayed in Appendix 2.

IV. Methodology

Model specification

As mentioned before, an instrumental variables regression will be used to estimate the price elasticity of the housing supply. More specifically, this will be a fixed effects model with instrumental variables, which will then be compared to a regular fixed effects model to analyze whether or not that model still contains any bias. The assumptions concerning the instruments' relevance and validity will later be discussed in this section. As stated before, the IV model will be based on the model used by both the CPB (2017) and OECD (2013). In this model the change in housing supply is explained by the housing prices *p*, the (short term) interest rate *r*, construction cost *CC* and the population growth *pop*. The original model of the CPB and OECD also accounted for quarterly dummies, but as the dataset employed in this paper is not compatible with those, they are left out.

$$\ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(p_{it-1}) + \beta_2 * r_t + \beta_3 * \ln(CC_{t-1}) + \beta_4 * \ln(pop_{it}) + \varepsilon_t$$

In this model logarithms are taken for supply, price, construction costs and population. As an elasticity is calculated the relative changes are necessary for the dependent and independent variable. Furthermore, it is economically more logical that construction costs and population influence this elasticity in relative terms than in nominally. Also, the single period lag of housing price and construction costs replace their current value. The reasoning for this is that it reduces the problem of simultaneity bias, which is very likely due to simultaneous determination of price in the real estate market. This problem was acknowledged by both papers amongst others on this subject, but is less of an issue for this research as an instrumental variables approach is taken. As established earlier, IV regression internally eliminates both simultaneity bias and reverse causality if the assumptions are met.

Lags are therefore theoretically not necessary, as lags were used solely for the purpose of counteracting simultaneity bias and are economically not very logical when applying yearly fixed effects. As the model uses the change from one year ago till now, it seems unreasonable that the changes between two and one year ago is more applicable to current values then changes between last year and now. Practically the housing supply cannot react instantly to housing price changes, and will also not react to every little fluctuation, but rather to broader trends. This would imply that there is indeed some sort of delay in a reaction. It is however not likely that this delay comes close to an entire year as companies evaluate economic decisions on the most recent information available, and even projections for the future which also aligns the 'reaction' with the future and these broader trend expectations. However, to get a complete comparison between new models introduced in this paper and previous models, both the model with and without lags should be estimated to inspect if these lags might have reduced bias.

Furthermore, two extra control variables could be added to possibly eliminate part of leftover omitted variable bias which otherwise could be identified as simultaneity bias when comparing with the IV model. These two controls are the capitalization rate *Δrent* and the spread between short and long term interest *rspread* . An earlier study has created reason to believe that these controls could diminish omitted variable bias when added to the standard model of the CPB and OECD (Blom, 2019). Similarly to using lags or not, both the cases with and without these extra controls should be reviewed. These two variations in model specification will generate a total of four different models for which fixed effects and instrumental variables can be compared. The Fixed Effects variants of these models are formulated as model 1 to 4.

$$(1) \quad \ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(p_{it}) + \beta_2 * r_t + \beta_3 * \ln(CC_t) + \beta_4 * \ln(pop_{it}) + \varepsilon_t$$

$$(2) \quad \ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(p_{it-1}) + \beta_2 * r_t + \beta_3 * \ln(CC_{t-1}) + \beta_4 * \ln(pop_{it}) + \varepsilon_t$$

$$(3) \quad \ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(p_{it}) + \beta_2 * r_t + \beta_3 * \ln(CC_t) + \beta_4 * \ln(pop_{it}) + \beta_5 * rspread_t + \beta_6 * (\Delta rent_t) + \varepsilon_t$$

$$(4) \quad \ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(p_{it-1}) + \beta_2 * r_t + \beta_3 * \ln(CC_{t-1}) + \beta_4 * \ln(pop_{it}) + \beta_5 * rspread_t + \beta_6 * (\Delta rent_t) + \varepsilon_t$$

The only difference between the models above and their respective IV counterparts, is that except for using the actual values for housing price, these prices are predicted by the instruments. Therefore, house price *p* is replaced with predicted house price $\hat{p}$. This predicted house price $\hat{p}$ is first explained by local amount of criminal cases *Crime* and municipal real estate taxes *OZB*. Theoretically, control variables are not necessary when using instrumental variables on the condition that those instruments are also exogenous to the controls. However, control variables will realistically still need to be added into the IV regression to help accuracy, but also to assess the relevance of the instrument conditional on these controls (Wooldridge, 2016). There are only two different first stages between the four models, as the first stages are equal for the first and third model as well as for the second and fourth model. The IV counterparts of model 1 to 4 are displayed below and formulated as model 1' to 4'.

(1') $\ln(\hat{p}_{it}) = \delta_0 * \ln(Crime_{it}) + \delta_1 * OZB_{it}$

$\ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(\hat{p}_{it}) + \beta_2 * r_t + \beta_3 * \ln(CC_t) + \beta_4 * \ln(pop_{it}) + \varepsilon_t$

(2') $\ln(\hat{p}_{it-1}) = \delta_0 * \ln(Crime_{it-1}) + \delta_1 * OZB_{it-1}$

$\ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(\hat{p}_{it-1}) + \beta_2 * r_t + \beta_3 * \ln(CC_{t-1}) + \beta_4 * \ln(pop_{it}) + \varepsilon_t$

(3') $\ln(\hat{p}_{it}) = \delta_0 * \ln(Crime_{it}) + \delta_1 * OZB_{it}$

$\ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(\hat{p}_{it}) + \beta_2 * r_t + \beta_3 * \ln(CC_t) + \beta_4 * \ln(pop_{it}) + \beta_5 * rspread_t + \beta_6 * (\Delta rent_t) + \varepsilon_t$

(4') $\ln(\hat{p}_{it-1}) = \delta_0 * \ln(Crime_{it-1}) + \delta_1 * OZB_{it-1}$

$\ln(\Delta s_{it}) = \beta_0 + \beta_1 * \ln(\hat{p}_{it-1}) + \beta_2 * r_t + \beta_3 * \ln(CC_{t-1}) + \beta_4 * \ln(pop_{it}) + \beta_5 * rspread_t + \beta_6 * (\Delta rent_t) + \varepsilon_t$

The price elasticity of the housing supply is captured by the estimated value of β_1 for all models**.** This elasticity can be interpreted as the expected % change in housing supply as a result of a 1% increase in housing prices. Finally, a Durbin-Wu-Hauman test will be used to determine whether the coefficients retrieved from the IV models are statistically different from the standard models. This test considers if a possible endogeneity issue is solved by employing IV and if IV does not suffer from reduced accuracy too much. The results from this test will give a decisive answer to whether or not IV is superior to regular Fixed Effects for that particular model. With the results of this test and the assessment of the estimations of all models, a model or group of models can finally be selected on the basis of which approaches causality best. The answer on the central research question can then be provided based on the selected model(s).

Instrument validity

In this section the proposed instruments for house prices will be discussed, mainly with regard to their validity and theoretical applicability. This research will utilize two instruments to predict house prices, which are the amount of criminal cases in the area and the local real estate tax. These instruments have been selected as they primarily influence the demand for housing, and are not causally expected to be related to supply changes. Validity is one of the two primary assumptions which have to be satisfied for instruments to be applicable and effective. Validity, also called exogeneïty, implies that the instruments used are not related to the dependent or other explanations. This assumption can essentially not be tested and relies heavily on qualitative arguments. Therefore, an in depth individual assessment of these instruments' exogeneïty and possible flaws will be discussed in this section, based on economic theory and logic. However, an overidentification test is often added to provide confirmation of the theoretical arguments. This test and its implications will be discussed later on. The second assumption is instrumental relevance. Relevance assesses to what extend the instruments actually are able to predict the instrumented/independent. This can be tested statistically in contrast to exogeneïty. The statistical part of examining relevance and validity will be covered in the results part, providing a final verdict on the first hypothesis.

Local crime

To assess whether crime is an exogenous instrument, first the exact origin of the data and what it comprehends should be considered. The data on local crime rates is retrieved from the Dutch Bureau of Statistics. The geographical scale is municipal and the crime figures are tracked yearly. Also, only total numbers of criminal cases are recorded, meaning no discrimination between what type of crime actually occurred is made. As mentioned earlier, crime is expected to decrease property value locally according to the literature. This relation can be assigned to a demand effect, as the attractiveness of real estate decreases as a result of the lower livability in the area surrounding it. In case of housing, people generally dislike living in neighborhoods or even entire cities if crime and robberies are more common. Additionally, just the fear of becoming a victim of crimes or theft is already enough for people to devalue properties.

Since crime is mainly known to influence the demand side of the equation, no effect on the supply of housing is expected other than through a price effect as a result of demand shifts. Furthermore, no compelling evidence on a causal effect between crime and housing supply exists, mainly as there is no theory to support such a relationship. Only if crime could directly impact the construction of housing in any way or impact incentives of developing real estate other than price, a direct relationship causing endogeneity can occur. For example, Berg and Hinze (2005) researched crime and theft on construction sites and concluded that theft and vandalism negatively affect firm performance of developers. Especially the theft of kitchen appliances is found to be more common and problematic. Construction firms are also found to be able to take measures against the threat of theft or vandalism during construction, meaning the rational constructer would take extra monetary measures to prevent this if that is more likely in a particular area. This would imply monetary incentives could actually decline housing supply if crime is an issue. Theft and vandalism on construction sites in the Netherlands seems to be less of an issue as there is a lack of studies addressing this problem, as well as a lack of data recording such events. Furthermore, there is no specific evidence that these incidents are linked to other types of robberies or burglaries.

If construction sites are specifically targeted, it could well be expected that the robbers do not necessarily have to come from that specific municipality. Additionally, one could also argue that theft on sites can only occur if the permit for construction has already been issued. Since housing supply in this study is measured in terms of issued construction permits, these permits are impossible to be influenced by thefts happening in the future. Only if theft risk is so high that a constructing company could foresee it and take it into account, then housing supply in a certain area could actually be influenced by this type of crime. Considering that these incidents do not occur often and are not specifically common in certain areas, it is unlikely that housing developers adjust their choices to them. Also no theoretical nor empirical evidence exist that developers of housing reason this way and actually quantify potential losses due to crime on specific locations affecting their decisions. Altogether, a direct causal effect of crime on housing supply is not to be expected.

However, not only direct effects can be the cause of endogeneity of the instrument. If crime in the area causes change in unobservable factors which in turn affects housing supply, that could also be a problem. This type of mechanism could result in an indirect effect of crime on housing supply possibly making crime endogenous as an instrument. The chance of such a mechanism affecting the validity is largely mitigated by the usage of municipal fixed effects, as all cross sectional time-invariant specific variables will be filtered out. Only mechanisms which would alter over time could harm the validity. It is hard to think of a such a variable which would be influenced by increasing or decreasing crime while also influencing the incentive to build any other way then through house prices themselves. The main effect which crime usually has is an increase of insurance and security expenses on individual level and changes in police activity on governmental level. Neither of these could imaginably have an impact on housing supply. In the contrary, increasing individual expenses might actually lead to lower house prices to offset these costs, contributing to the expected effect of crime on housing prices. If the overidentification test turns out to not be able to reject that the instruments are exogenous, and no clear claim exists to combat that, a reasonably certain conclusion could be made that the number of local criminal cases is exogenous to changes in the housing supply and is more likely than unlikely to be a valid instrument in this case.

Municipal real estate tax

Now the case will be made for the local property tax to be a valid instrument in addition to local crime levels. The main difference between these types of instruments is that the real estate tax is set by the government, and could thus be viewed as less random and more coordinated. To analyze whether or not this type of tax is directly correlated with housing supply, the decision factors of setting these tax levels will be of great importance. The factors taken into account when setting or increasing the municipal tax are not quantifiable or based on objectivity. Also, different municipalities could employ different logics when deciding upon the tax increase. Allers et al., (2001), as well as the COELO (2004), both mention that tax tariffs are significantly influenced by the political color of the local government. Allers et al. found that municipalities ruled by more right wing parties employ lower real estate taxes. This would imply that these taxes rather are a way of political bargaining than an economical tool to balance the market. This fact helps in convincing the exogeneïty of the variable, as a balancing function would have led to relation between supply, price and taxes, as these taxes would then likely be set in reaction to changes in these other market variables.

Furthermore, a previously set national maximum increase in the real estate tax has been abolished in 2008 (COELO, 2015). This contributes to the thought that these taxes are not based on other market factors, as these maxima were installed to protect the consumer and prohibit significant market inefficiencies. After this change, the only restriction on the municipal real estate taxes is that the macro-revenue of all municipalities cannot exceed the real growth of national GDP (COELO, 2015). Since this condition does not affect or control individual municipalities, it is not able to set hard limits on local governments choices and incentives. Everything considered, it looks like the real estate taxes by local governments are mainly determined by their political climate and their individual income and expenses, relieving the threat of any direct involvement with the real estate market.

Again, there could also be the possibility of a mechanism related to both property tax and the amount of construction permits issued. This mechanism would also have to be time-variant since the municipal fixed effects eliminate cross-sectional bias by default. The real estate tax basically results in a net cash transfer from house owners to the local government, who then use this money to invest in shared municipal benefits. Looking at the consumer side, the tax solely has monetary effects. Higher taxes result in higher expenses, decreasing property value to compensate for these expenses. The only effect on supply through this consumer side is thus through prices and poses no problem to validity. On the municipal side on the other hand, higher taxes increase budgets which in turn is likely to increase spending. A possible problem could appear if local governments would use this extra money to invest in housing development. In that case, an increase in taxes is positively correlated to the housing supply through budget effects. This could theoretically happen, but no motive can be found that this actually happens. It is more likely that municipalities choose to increase their taxes to compensate for an already existing budget deficit, and has little to no impact on the distribution of their expenses. Again, in addition to an overidentification test which cannot find any reason for significant concern, there is a plausible case for municipal real estate tax to be exogenous to housing supply and thus a valid instrument.

V. Results

In this section the hypotheses will be answered as objectively as possible in accordance with the statistical prove presented. First, hypothesis one will be addressed as it is actually part of the methodologic foundation of this research. This hypothesis is necessary for the instrumental variables models to actually be reliable and worth comparing to the standard models. These standard models will then be discussed followed by the instrumental variables models. Thereafter the second and third hypothesis can be answered, as they depend on the comparison between all of these models.

Hypothesis 1

The first hypothesis stated that the instruments used are statistically viable in terms of relevance and significance. The theoretical validity of the instruments has already been argued extensively, and can be supplemented by testing the overidentification restrictions. The implications of this test will be further discussed in the section elaborating on the test results. Relevance for these multiple instruments can be assessed by the Cragg-Donald F-statistic. As it is not known which model is superior, no definite verdict can be given based of the tests of one of them. Therefore, the test results of all models should be considered in collectively creating a statement on the relevance and validity of the instruments.

Relevance

Literature on these instruments has shown that there is a theoretical, as well as an empirical effect on house prices on local levels. The effect of crime has been researched extensively, as well as related measures like overall thefts and burglaries which are incorporated in crime figures. Most show a clear negative impact of crime on the house prices in metropolitan as well as non-rural areas (Thaler, 1978; Lynch and Rasmussen, 2001; Ceccato & Wilhelmsson, 2011; Wilhelmsson & Ceccato, 2015). The effect of local real estate tax on house prices has not been researched as much on the other hand. Sirmans et al., 2008 have concluded that tax differentials in terms of property tax are capitalized, meaning that these taxes have impacted local house prices. The expected relevance of both these instruments are thus theoretically backed up by existing research.

To statistically confirm relevance within the employed sample, usually the first stage regression is ran in which the instrumented variable is explained by the instruments conditional on all control variables. An F-test on the instruments then quantifies the ability to predict changes However, this test does only hold for single instruments (Staiger & Stock, 1997). A similar test which can be utilized in this case is the Cragg-Donald test. This test provides a Cragg-Donald F-statistic which considers that there are two or more instruments (Stock & Yogo, 2002). This test reported statistics between about 19 and 275 depending on the particular model, and are shown in Table 4. These all surpass their respective critical values (highest stated critical value is 10% maximal IV size). These results imply that for all models, both instruments are indeed relevant and can effectively predict house price.

Validity

The other assumption, validity, is a more difficult concept to comprehend and test intuitively. If the instrument is not exogenous, and thus endogenous, there is some sort of direct or indirect effect on the dependent variable other than through the independent. This would mean that the estimated effect of the independent on the dependent is biased as it is not solely reliant on the causal effect between the two. The hard part about proving validity is that it is not conclusive. There is no definite test which can be performed, so the main part of evaluating exogeneity is the theoretical argumentation which has been discussed before.

An overidentification test can however be performed to give an indication of whether the instruments are exogenous. This test does not only control for instrument validity, but also if the model is correctly specified. A rejection can thus also be caused by model misspecification. Also, the test already assumes that at least one of the instruments used is already valid. The overidentification can therefore not give a decisive answer on the validity issue, but is thought to be able to give support the claim of validity in addition to good argumentation. Although this test is widely used to support instrument validity, it is neither required nor necessary to guarantee exogeneïty. Parente & Silva (2012) discuss the overidentification test in relation to validity of instrumental variables, and argue that the overidentification test rather checks whether all instruments identify the same set of parameters. They come to this conclusion by extensively analyzing the conditions implied by the underlying economic model.

Nevertheless, the overidentification test is however still ran as it can also provide insights towards the quality of the models. The test provides a Hansen J-statistic and must not reject in order to ensure that there is no misspecification or endogeneity problem. The Sargan/Hansen test for overidentification reported p-values of 0.054, 0.000, 0.009 and 0.007 for models 1' to 4' respectively. These values are displayed in Table 4. Only the first p-value for model 1' rules out rejection at the usual 5% significance level and can thus satisfy the required conditions. This particular model does not consider the two newly found control variables and does not employ lags for price and construction costs. This is the most basic model, but could also well be the most accurate model as it is essentially the model of the CPB and OECD apart from removing the lags for which there is no theoretical backing that it actually alleviates any simultaneity bias . The test results for the other models can however not guarantee the same, implying that there is some sort of issue causing this. The origin of this problem is not explicitly known and could be due to different causes. For two of them (2' & 4'), misspecification could be the cause as they do use those same lags which economically do not make a lot of sense, especially for yearly changes. For the remaining model (3'), there is no relatively easy explanation to why the restrictions are not satisfied. The only difference compared to model 1' is the addition of two control variables being interest rate spread and real rent increases. These variables might have caused some sort of distortion in the model. Both of them turn out not to be significant in the model, which might be the result of effectively not being necessary controls, but rather mechanisms through which other variables affect the dependent.

No certain conclusion can be made off these overidentification test results, but it could imply that the model 1' is the superior model accuracy and efficiency. In terms of validity, the test results for model 1' do not create any more concern on the validity of the instruments. If this model is in fact the superior model, that could provide some extra proof on the applicability of crime rates and real estate tax as instruments for housing price. Additionally, since the same instruments are used in each model to predict the same independent, rejection of the overidentification test in some of the models appear to rather be the result of some kind of misspecification. Everything considered, including the theoretical background sustaining the exogeneïty of both instruments, a plausible case can be made for these instruments to in fact be exogenous/valid for predicting real house price fluctuations for the sample considered in this research. Finally, a joint conclusion can be made that crime rates and real estate tax at least seem to be viable instruments based on the assumptions of relevance and validity. It should however be noted that validity is not, and cannot be objectively proven.

Standard Models

To answer the two leftover hypotheses, the estimates generated by the different models need to be discussed first. Both the second and third hypothesis namely rely on the comparison between the standard models and the IV models, considering all differences between them, test results as well as the models individual interpretation. First of all, a look will be taken at the standard model and different variants of it. The CPB (2017) and OECD (Caldera & Johansson, 2013) both used a lagged response model in which price and construction cost were lagged one period and the other explanatory variables included population and interest rate. In this research, two complementary possible control variables are introduced, being the capitalization rate and long term interest rate. Also the IV models in this research are expected to be more accurate when not utilizing lagged variables. As the Durbin-Wu-Hausman test will only compare the IV model with its direct OLS counterpart, all possible models are taken into account to provide a full picture. This means that for example also IV models with lags are ran to compare those models with the standard models with lags, even though it is argued that these lags are not necessary when employing IV. All of the possible models differ in two dimensions; namely whether or not they lag price and costs and whether or not they include the two complementary controls. A total of four models that can function as standard model can therefore be distinguished. These four Fixed Effects models are displayed below in Table 2.

Table 2: Fixed Effects estimates of models on the supply elasticity

Model (FE)	(1)	(2)	(3)	(4)
log House price	2.252**		1.408**	
	(0.469)		(0.464)	
L1. log House price		-1.087*		0.017
		(0.430)		(0.481)
log Construction cost	-1.874**		-0.639	
	(0.512)		(0.838)	
L1. log Construction cost		4.385**		27.523**
		(0.834)		(3.056)
log Population	-1.807	-4.691*	-3.439	-5.887**
	(2.042)	(2.158)	(2.019)	(2.240)
2Y interest rate	0.260**	0.575**	0.250**	2.472**
	(0.028)	(0.040)	(0.057)	(0.255)
2Y-10Y interest spread			-0.345**	0.388**
			(0.059)	(0.091)
Rent increase			0.031	1.139**
			(0.054)	(0.149)
Cons.	*7.742*	*38.713*	*27.307*	*-111.517***
	(19.199)	*(20.541)*	*(20.218)*	*(29.087)*

Table notes. Coefficients displayed represent Fixed Effect estimates with the logarithm of added housing supply functioning as the dependent variable. Added housing supply is measured in issued building permits. (1) till (4) refers to the specification of the model as is described in the methodology. L1. refers to the first order lag of that specific variable. Standard errors are reported in parentheses. **, * denote significance at the 1%, 5%.

The coefficient for the housing price in these models represents the estimated supply elasticity of that particular model. Clearly different elasticities are found between the models, two being significantly positive, one significantly negative and the final one insignificantly different from zero. Furthermore, the coefficients of the other variables vary largely in sign and magnitude across the models as well. All of these differences can be explained by assessing the differences in modelling. First, the difference of adding the interest spread and rent increase will be discussed. The base model without these controls provides an elasticity of 2.3, implying that the number of issued building permits is expected to increase by 2.252%, given a 1% increase in house prices. The model with the extra two control variables finds a somewhat lower elasticity of 1.4. Both of these estimates are significant at 5% and 10% level, but the decrease in the found relationship is notable. The other explanatories change as well, construction cost mainly as the magnitude of this variable is decreased causing it to lose its significance. The added control of rent seems to not have much of an effect with its small insignificant coefficient. More interesting however is the interest spread. This variable reports a significant result, but what is possibly even more important, is that it does not seem to change the effect of the regular interest rate variable. This appears to imply that the addition of this variable could indeed be useful, as it is not found to be collinear to this relatively similar variable. Overall, these results might mean that at least one of these variables could be omitted, with the spread being the most likely candidate. Comparing the two lagged models at the other hand in terms of adding the extra two controls yields more extreme results. The elasticity changes from -1.1 to 0.0, removing almost all predictive power of the housing price. Furthermore, the interest rate is now affected largely, changing from 0.6 to 2.5 almost quintupling its effect. Moreover, an even more extreme increase in the effect of construction cost is reported. Compared to a coefficient of 4.4 in model two, model four estimates a coefficient of 27.5, which seems almost unreasonable. This would mean that an increase in costs of 1% would actually lead to a 27.5% increase in building permits. This positive relation between construction cost and supply would mean that rising costs lead to a larger incentive to construct new housing, which is contradictory to economic reasoning. Therefore this final model could be viewed as highly debatable in terms of accuracy.

The introduction of the lagged variables tells a different story compared to the addition of extra controls. In both cases with or without the two extra controls, the found elasticities completely overturn compared to the models without lags. The lagged models report elasticities of -1.1 and 0 for models two and four respectively, whereas the non-lagged models find positive elasticities. A negative elasticity would imply less housing development would be constructed as a result of increasing house prices, or an increase in housing construction together with decreasing prices. Other explanatory variables also change notably when lagged versions of price and construction cost are used. Where construction cost used to have more logical negative effects on the amount of building permits issued, the models with lagged variables report positive effects. As mentioned before this seems unlikely to be a causal effect as increasing cost would lead to decreasing profitability assuming constant price. In total, lagged variables seem to change the found effects of those specific lagged variables largely by changing their sign. Lags therefore have great effect on the intuitions that the model generates, and these significant differences could well imply that only one option of either employing lags or not is accurate. Non-lagged fixed effects models appear to be more in line with expectations based on theory and logic, but no conclusion can be made to whether or not one model is indefinitely better than the other. However, the implications of these results could be helpful when considering the added benefit of instrumental variables.

The effect of population is not yet mentioned in the comparisons as it does not vary much between models, the implications of its coefficient should however be discussed briefly. For all models, the effect of population seems to have a negative impact on the issuing of building permits. This effect is significant for the lagged models, but insignificant for the non-lagged models due to high standard errors. A negative effect of population could be explained by the fact that there is a lack of space in larger cities to develop new housing projects, resulting in lower permits. Population generally rises faster in metropolitan areas, and a lack of space is a common problem as the boundaries of surrounding villages or smaller cities are often reached. This is predominantly a problem for the really big cities in the Netherlands, like the four cities together forming the Randstad. The high standard deviation can however possibly occur as this effect is less or not present for lower density municipalities. In the contrary, quickly expanding villages or smaller cities would likely see relatively more permits being issued to create housing for these new people.

Instrumental variables models

The only difference between these IV models and the previously discussed FE models is that in the IV models the actual tracked value for housing price has been replaced by a price prediction estimated by the instruments crime and real estate tax. This methodologic addition is captured by the first stage of the two stage least squares (2SLS) regression, which is shown below in Table 3.

Table 3: First stage estimates IV models

Model (FE)	(1')	(2')	(3')	(4')
log Criminality	-0.240**		-0.240**	
	(0.008)		(0.008)	
L1. log Criminality		-0.200**		-0.200**
		(0.008)		(0.008)
Real estate Tax	-0.835**		-0.835**	
	(0.145)		(0.145)	
L1. log Real Estate Tax		-0.497**		-0.497**
		(0.138)		(0.138)
Cons.	*14.223***	*13.873***	*14.223***	*13.873***
	(0.069)	*(0.068)*	*(0.069)*	*(0.068)*

Table notes. Coefficients displayed represent first stage estimates of the instruments (municipal real estate taxes and local crime rates) with regard to the instrumented variable housing price. (1') till (4') refers to the specification of the model as is described in the methodology. L1. refers to the first order lag of that specific variable. Standard errors are reported in parentheses. $**$, $*$ denote significance at the 1%, 5%.

In contrast to before, there are actually two first stages which can be distinguished, as model 1' and 3' as well as model 2' and 4' essentially yield the same first stage. This is due to the fact that adding control variables to the model does not change their first stage, as that is solely based on the effect of the instruments on the instrumented. Furthermore, the only difference between the two first stages is that one employs lagged variants of all variables and one does not. In principle, these models are therefore still the same, apart from the fact that the lagged variant does not use the values for the most recent year. The expected similarity also shows in the found coefficients, as no extreme changes occur when introducing lags. For all models, the effect of criminality is around -0.2 and significant, whereas the effect of real estate tax varies between -0.8 and -0.5, and is also significant. Negative coefficients for all variables mean that increases in crime or tax result in a decrease in real housing price. This follows the projected relation between the instruments and independent, meaning no major causes for concern are found in the first stages.

The second stages, and effectively the direct comparisons for the fixed effects model can now be assessed. Again, there are four different models which correspond to the fixed effects models. The second stages of these models are displayed in Table 4 below.

Table 4: Instrumental Variable estimates of models on the supply elasticity

Model (IV)	(1’)	(2’)	(3’)	(4’)
log House price	8.942**		9.116**	
	(0.914)		(1.383)	
L1. log House price		9.469**		11.335**
		(3.385)		(3.239)
log Construction cost	-8.333**		-8.573**	
	(0.942)		(1.647)	
L1. log Construction cost		-12.725*		40.316**
		(5.398)		(5.152)
log Population	-14.205**	-12.816**	-13.915**	-14.453**
	(2.607)	(3.612)	(2.858)	(3.594)
2 year interest rate	0.028	-0.039	0.030	4.285**
	(0.041)	(0.196)	(0.078)	(0.597)
2-10 year interest rate spread			0.066	1.023**
			(0.099)	(0.214)
Rent increase			-0.005	2.537**
			(0.069)	(0.435)
Cragg-Donald F-stat	*275.061*	*19.318*	*121.639*	*23.468*
p-value overid test	*0.054*	*0.000*	*0.009*	*0.007*
p-value DWH test	*0.000*	*0.000*	*0.000*	*0.000*

Table notes. Coefficients displayed represent Instrumental Variables estimates with the logarithm of added housing supply functioning as the dependent variable. Housing price is instrumented by municipal real estate taxes and local crime rates. Added housing supply is measured in issued building permits. (1’) till (4’) refers to the specification of the model as is described in the methodology. L1. refers to the first order lag of that specific variable. Standard errors are reported in parentheses. ∗∗, ∗ denote significance at the 1%, 5%.

The first thing which can be noted is that for all models the found elasticities are relatively similar, especially when compared to the fixed effects models. More similar elasticities are also expected as IV should eliminate any omitted variable bias. This means that adding the two extra controls should not have much or any effect on the coefficients, which indeed does not seem to be the case. This helps arguing the validity of this methodologic strategy, as exogeneity of the instruments could have caused the similarity of these coefficients by eliminating bias. There could still be a difference between the lagged and non-lagged models, since their difference is based on their different used values for prices. The elasticities range from about 8.9 to 11.3 and are all significant at both 5% and 10%

significance level. With minor differences, the models including lags indeed provide somewhat higher elasticities, but also greater standard deviations.

Not only the similarity between the estimated elasticities is different compared to the FE models, but also their magnitude. They are all positive, which is in line with expectations, but their magnitude exceeds all FE elasticities. This implies that a downward bias would be present in the regular fixed effects estimates. This seems logical, as the reverse effect of housing supply on housing prices would likely be negative based on economical demand and supply assumptions. Would the housing supply expand, then the average price of houses is expected to drop due to higher availability. This counteracts the positive effect of prices on supply, possibly causing such a downward bias. Furthermore, housing supply elasticities of about 9%/10% imply far bigger supply reactions to changing house prices then what is found in existing literature on this topic. As mentioned earlier, supply elasticities usually range from being zero or slightly negative to a maximum of one or two percent. This does however not make it improbable that these high elasticities could represent reality. A 9% elasticity does not mean that total supply is increased by nine percent as a result of a one percent price increase, but that added supply in terms of building permits is 9% higher compared to the previous year as a result of this price change. This means that (the intention of) construction is actually increasing relatively to the previous year as a result of rising house prices. If (one of) these models could indeed represent causal relations as is intended, this could have implications for the way housing supply dynamics are examined.

Apart from the elasticities, some other interesting results have been generated by the IV models. First of all, the construction cost is fairly constant over the first three models, but is overthrown in the fourth model. Whereas the coefficient for the non-lagged models are -8.3 and -8.6, lagged models estimate coefficients of -12.7 and even a positive 40.3. This final relation has an unlikely high magnitude, even higher than the relation found in its FE counterpart (27.5). The same argument described for that coefficient holds here. An effect this high is economically unexplainable and is improbable to reflect causal inference. A similar story holds for the interest rate variable, for which the fourth model provides a significant coefficient of 4.3 whereas all other models estimate an insignificant coefficient statistically non-distinguishable from zero. Population changes seem to have a consistent effects whereas the two extra controls are inconsistent across the final two models.

Hypothesis 2

The final two hypotheses can now be answered as all of the models have now been discussed. The second hypothesis argues there to be bias in the estimates of standard models. It is mainly aimed at assessing the added value of employing instrumental variables as a causal estimation technique. A confirmation of this expectation implies that the IV models are more accurate in estimating causality, on the condition that the instruments are relevant and valid. To review this claim, the standard models need to be compared to their IV counterparts. The Durbin-Wu-Hausman test will give a decisive answer to whether this hypothesis holds. It tests if there is a significant difference in the estimated coefficient found in the IV model compared to the OLS model, or in this case, the Fixed Effects model as the IV model included Fixed Effects. The null hypothesis assumes that the coefficients for housing price are equal between IV and FE, indicating that no bias is solved by using IV and that OLS/FE is the superior model. The results of these DWH tests for all instrumental variable models can be found in Table 4. For all of the four instrumental variable models the DWH test reports a p-value of 0.000. This means that for all models the null hypothesis is rejected implying that the coefficient of the FE estimator is endogenous and that the IV estimator solves this bias for each of the models. This result is straightforward and also expected when reviewing the difference between the FE and IV models. The found elasticities when using instrumental variables are far higher compared to the elasticities found for their fixed effects counterparts, which usually results in a rejection of the DWH test. Assuming viable instruments, this implies that IV modelling should normally be used in this case as it solves an endogeneity issue. Therefore, the second hypothesis can be accepted on the condition that the instruments are in fact relevant and valid. Besides the DWH test, the IV models also seem to provide more consistent results across their variants compared to the standard models which vary more in found estimates for most coefficients. Also, these estimates seem to be more in line with economic expectation, as well as logic. These points seem to provide further backing for the accuracy of the IV models relative to fixed effects models, producing some sort of verification for the results of the DWH tests.

Hypothesis 3

The third and final hypothesis is directly linked to the main research question. It is focused on the actual causal relationship between the housing price and supply. Like economic theory on the housing market predicts, this hypothesis states that the causal supply elasticity is significantly positive. This implies that increases in the prices of housing would lead to a relative rise in housing supply through the increase of construction. To examine the found supply elasticities and assess their actual causal accuracy, a total of four Fixed Effects models and four Instrumental Variable models were created and reviewed. The results of these models imply that it is at least likely that employing an instrumental variable approach solves some sort of bias which was still present in the fixed effects models. This conclusion is based on DWH test as discussed in hypothesis two. Therefore, the IV models are expected to be more accurate when estimating the supply elasticity. These models estimate elasticities between 8.9 and 11.3.

This range of coefficient estimates can be narrowed down further by considering the differences between models. First of all, lagging housing price and construction costs results in the same endogeneity problem as in the non-lagged model variants for the standard models. The main reason for running these IV models with lags was to analyze whether they could solve simultaneity bias, which was described to be their purpose in previous research (Caldera & Johansson, 2013; CPB, 2017). These lags do thus not seem to offer much help on this issue considering yearly fixed effects in the employed sample. No other explanation for the use of the lags is mentioned and none can intuitively be derived using economic theory, at least not while using this particular sample with solely yearly recorded data. This was discussed in more detail earlier in the methodology section. Additionally, the results of the overidentification tests were also not in favor of the lagged models. These imply some sort of endogeneity problem or misspecification. Model 2’ and 4’ are therefore not considered as being capable of representing causal relationships, leaving 1’ and 3’ remaining as candidates.

On the other hand, the addition of the two new control variables creates ambiguous results in the standard models. The coefficients do change a little as a result of their addition, but these extra controls themselves lack either significance or economic logic in terms of sign. It is therefore dubious which of these variants is better than the other. The overidentification test results do however create separation between model 1’ and 3’. Model 1’ is actually the only model which passes this test, and does thus not attract concerns on either misspecification or endogeneity in contrast to model 2’. Considering these judgements, model 1’ seems to have at least a slight edge in approaching reality most accurately. This model does not lag any cost variables and does not consider the extra controls. A supply elasticity of 8.942 has been found by this model. This found elasticity reflects a clear positive relationship, hence the third hypothesis can be accepted. All other IV models find even higher elasticities, meaning that this hypothesis can be accepted regardless of which IV models is correct. Again, the acceptance of this hypothesis relies on the validity of the instruments used in this models. Finally, all three hypotheses can thus be plausibly accepted, but not with definite certainty. Mainly the validity of the instruments, which cannot be guaranteed, leaves room for error. Would the instruments be proven to be valid , these hypotheses could be easily accepted. Without this guarantee however, the results of these models and their implications should be carefully interpreted.

VI. Discussion

The results found in this research give reason to believe that the common models used for estimating the price elasticity of the housing supply cannot deal properly with simultaneity bias, despite attempts to combat it by lagging price and cost variables. The IV models introduced do however display statistically different elasticities compared to their fixed effects counterparts. This together with the proper testing and analysis implies that a certain bias has been removed from the models as expected, assuming correctly constructed IV models. All of these results do however rely heavily on the validity of the instruments as discussed before, as well as on all of the other assumptions made by the different models. These assumptions, the context of this paper, the data and other limitations and their implications need to be addressed to create a more complete picture of what the results can effectively tell us.

Data

First the employed dataset will be discussed in light of its implications. The main dimensions which can be addressed are the timespan and frequency, as well as the geographical classification. The geographical division into municipalities seems to be able to take care of accuracy, as sufficient data is available on these regions and values are sufficiently large not to be mainly affected by randomness. Also, municipalities hold a lot of power to influence the actual local market. Most places within a municipality could be considered as close substitutes to each other, whereas broader regions do not function similarly. The used yearly timespan of 2012 to 2019 is on the other hand rather small compared to other research. That does however not result in a lack of observations since the much more detailed geographical component compensates for that effectively. A longer timespan or higher frequency (quarterly instead of yearly) could benefit the accuracy and efficiency of the models in generating more consistent estimates. The main problem is that data did not seem to conform perfectly between this period and earlier periods. Therefore this would be a solid point for improvement. A final factor to consider is the difference between elasticities in different economic periods. The CPB (2017) found significantly different elasticities during periods of recession compared to other periods. This research considers a short period in which the recession has mostly passed and economic growth is consistently present. Different economic periods could be of influence on results on supply elasticities, raising attention to the causes of these differences. Quantifying this effect and the dynamics behind it could be of huge benefit to all kinds of models attempting to estimate the price elasticity of the housing supply.

Model specification

Another factor to discuss when reviewing the methodology of this research is the composition of the specifications. The four types of models estimated are for the most part very similar as they have the same basis. Two of them use lagged variants of certain variables and two have two extra controls added, creating four slightly different models. In the results it was concluded that both the addition of the extra controls; long term interest rate and the capitalization rate, and the implementation of lagging the cost variables of price and construction cost did not help or might even harm the accuracy and reliability. Also, using an instrumental variables regression appears to solve bias still present in the standard models. Hence, the most basic IV model, model 1', is selected as being most representative of reality. This model explains the relative change in issued building permits by construction cost, population, the short term interest rate and finally the predicted value of housing price, instrumented by local crime rates and the real estate tax. Municipal fixed effects were included in this model, just like in all of the other models to estimate local effects rather than national effects. Based on the models which were considered as possibly being "true", this model came out on top.

This does however not mean that this research argues this model to be the optimal method to estimate the supply elasticity. Based on a review of the literature this model has been formed in an attempt to isolate the pure causal effect of housing price on the housing supply. The supply elasticity could also be seen as a metric which resembles the interdependent relationship, instead of just the one way effect. This mutual two way relationship can also say a lot about the housing, and a lot of the previous research might not even have been interested in the isolated causal effect. This together with the aim to pursue comparability might be the reason that a lot of papers use similar methods, mostly differentiating in geographical aspects or time periods. When the goal is thus not to estimate actual causality, IV is not necessary and a simpler model can often suffice. The only problem then is whether the found elasticity is more dependent on the effect of price on supply or on the opposite effect, which has to be taken into account. Furthermore, more accurate modelling to find causal estimates is also possible as it cannot be assumed that the exact perfect model is already found in this attempt. Other yet unknown control variables could possibly benefit the model, despite the fact that a perfect IV setup should dismiss variation in controls. Also, a set of other instruments for housing price could increase the trustworthiness of the model if correctly aligned with the assumptions. More research on possible instruments could likely find an instrument or set of instruments for which the validity assumption can be argued more indefinitely, as the validity will always persist to be an issue when not completely certain. Finally, other types of regression methods instead of IV might also be able to isolate causal effects. So, while there are ways of improving the specification of the model for this purpose or for another, model 1' is currently the best approximation of a model intended to estimate the causal relationship within the price elasticity of the housing supply.

Instrumental variables assumptions

The two main requirements of an instrumental variables model, are the relevance and validity of the instruments. Without fulfilling these, the estimates cannot be trusted to isolate the causal effect and are likely to be affected by biases. Since IV is somewhat less accurate as a result of larger standard deviations by default, unviable instruments lead to very unpredictable and unreliable estimations. Formally there are a total of three assumptions related to IV. The first being a strong first stage, which is the same as relevance. The other two are independence and the exclusion restriction. These are essentially both captured within the term of validity. The independence assumption argues that the instrument is not related to any other determinants of the outcome, whereas the exclusion restriction states that the instrument itself is not directly related to the outcome. Both of these thus cover the relation between the instruments and the dependent, either direct or indirect. In the methodology the exogeneity of the instruments was discussed by arguing against a possible direct effect of crime rates and the real estate tax on issued permits, as well as indirect effects through the other controls like sorting based on population sizes. The second and third assumption have therefore both been covered in this section, and the final verdict on these is that the instruments are sufficiently relevant and plausibly exogenous as far as can be concluded based on the discussed arguments and tests. This means that there is still a chance that the instruments turn out not be completely exogenous. In that case, the methodological processes of these models are likely distorted, and no conclusions could effectively be made of their results. Since this possibility cannot be ruled out, the found elasticities should not be interpreted as a completely definite causal effect. It does however give an indication of how simultaneity bias and possibly other biases impact these elasticity estimates. All of the models suggest a negative bias in standard models, if this has indeed an effect about seven to eight percentage points in the final elasticities cannot be guaranteed. More research on the these instruments, as well as more research on supply elasticities using causal regression techniques like IV in different time and spatial settings is necessary to give more insight in the causal effect of housing price on the supply.

Besides the assumptions that should be met, there is one other major criticism on the application of IV. This is the fact that IV estimates a local average treatment effect (LATE). The LATE only considers compliers, which are subjects which change their treatment status. Never takers or always takers are thus not taken into account, and differences between these groups and the compliers can then result in the inability to extrapolate findings to other groups. This is however not a big problem for this research, as the independent variable housing price is not binary but continuous and there are no instances where the housing price does not change. For every municipality in every year, there is at least some sort of minor increase or decrease in housing prices. All of the observations are thus technically part of the compliers, as their treatment is changed over the years.

Model results and comparison

It is also interesting to review and compare the found elasticity and possibly the other coefficients with other research and find explanations for the differences. Model 1', which was found to be the most reliable option, estimated a supply elasticity of 8.9. Furthermore the coefficients for the effects of the construction cost, population and the short term interest rate are -8.3, -14.2 and 0.03 respectively. Both the housing price and interest rate thus have a positive impact on added supply, while the construction cost and population size have negative effects. All of these variables are relative and describe fixed effects per municipality. The signs seem to comply to the expectations for the most part. The effect of prices, construction costs and population can be illustrated as discussed in the results section. A positive effect of the interest rate is somewhat less explainable. Normally, when the interest rate rises, it is more costly to develop housing as money often needs to be borrowed. Also if that is not the case, it is still relatively less profitable to invest money in other projects as the profits from lending increases. This would lead to an actual decrease in projects started and permits requested. However, the coefficient estimated for the short term interest rate is not significant and is therefore not distinguishable from zero. As interest rates are related to other circumstances within the financial market and the state of the economy, the sole effect of changing interest rates could easily be distorted resulting in this insignificance. This does not pose a problem for the other results, as essentially more omitted variables would then be included within this variable.

Other papers on the price elasticity of the housing supply find elasticities of 0.19 (Caldera & Johansson, 2013), between 0 and 1.4 depending on the economic conjuncture (CPB, 2017), or 0.04 in the short run versus 0.1 in the long run (Vermeulen & Rouwendal, 2007). The magnitudes of all of these researches lay far lower than the estimated elasticity of 8.9 in this paper. These mentioned papers use similar definitions of the supply elasticity, also often referred to as the supply responsiveness. They consider the relative effect of prices on new construction, where the construction is either measured in new project starts or issued permits. This difference in metric does probably not result in the differences in elasticity, as permits result in a start about 99% of the time (Somerville, 2001).

This paper only considers a relatively short period, in which the economy is recovering from the crisis. The CPB differentiates the elasticity from different time periods, where a relationship of about zero is estimated between 1995 and 2007 in the period before the crisis, and an elasticity of 1.4 is found after the crisis between 2008 and 2016. They ascribe this increased elasticity to the fact that intended projects could easily be cancelled considering the decreasing prices due to the crisis. The lack of a relationship before could on the other hand be allocated to spatial limitations. Changes in housing market policy could finally also explain the differences in the found elasticities. IV modelling like executed in this paper, should theoretically not find different elasticities in different periods. As IV attempts to eliminate any bias, including omitted variable bias, changes in policy or economic circumstances should not affect the outcome. The fact that this is not the case in other papers, supports the concern of variables like policy, geographic limitations and economic state to be omitted. This in turn solidifies the search for different estimation techniques which can circumvent these problems. Another interesting finding, is that according to Vermeulen and Rouwendal (2007), the supply fails to react properly in the immediate short term, whereas changes occur more clearly in the longer term. Compared to the elasticities in this research, these differences are still very small, but it could imply that supply indeed needs some time to adjust to price changes. This could also confirm that supply only reacts to broader price trends instead of minor alterations. Since IV models with lags of housing price were also estimated, a careful comparison can be made between somewhat shorter and longer term. The model most similar to the preferred specification, but instead with lags, is model 2'. This model provides a higher elasticity than the non-lagged variant, namely

9.5 opposed to 8.9. This could suggest that there is indeed a stronger effect in the long run. However, as the relative unreliability of the lagged models should be considered, no conclusion can be made. Further research on the differences between short and long term supply adjustments can give more insight into this matter.

VII. Conclusion

The central research question: *What is the causal effect of housing prices on the housing supply in the Netherlands?* can be attempted to be answered with the results found in this research. Looking at the different models, there are clear differences between the regular fixed effects models and the newly introduced instrumental variable models. Based on the assessment of these models, a conclusion can be made that the IV models are more accurate as they likely eliminate the presence of simultaneity bias. Assuming that the instruments employed fulfill their assumptions in terms of relevance and validity, which is found to be plausible, these models are found to have significantly different coefficients in term of the elasticity compared to FE models. The IV models estimate supply elasticities of between 8.9 and 11.3, and thus seem to be relatively close in terms of prediction. It is however likely that one of these models is superior to the others and represents reality better. Based on the evaluation of these models this superior model seems to be model 1', in which the housing supply is explained by the non-lagged variants of housing price and construction cost, and to which the new possible control variables of interest rate spread and capitalization rate are not added. This particular model finds an elasticity of 8.942. This implies that the increase in the amount of issued building permits, reflecting the housing supply, is expected to increase by 8.942% as a result of a real housing price increase of 1% whilst keeping all other variables constant. If all conditions of IV are satisfied, this effect can be interpreted as causal. Where instrument relevance does not seem to pose a problem, the validity/exogeneity of the instruments is a subjective matter. As discussed extensively before, the validity of crime and the real estate tax as instruments for real housing price is plausible, but cannot be guaranteed. Both of these instruments seem to mainly effect housing demand, through which it effects the housing price. Direct relations to housing supply are unlikely as is argued at length, but are not able to be entirely dismissed with solid proof. Therefore, it can only be assumed that these conditions hold with probable cause, allowing for interpreting the found elasticity as at least possibly causal.

The found relationship between the real housing price and the added housing supply is very different compared to other studies which find elasticities with much lower magnitude or coefficients not statistically distinguishable from zero. This could be the result of IV reducing or eliminating negative bias still existing in other models. According to the Durbin-Wu-Hausman tests, an instrumental variables approach is superior to regular fixed effects regressions for the supply elasticity models considered. This finding is essentially more interesting than the found elasticity itself, as it could prove simultaneity bias to be a persistent threat for estimating the price elasticity of housing supply. This does not mean that the found elasticities in this research are exactly correct in reflecting the real relationships. Rather, it gives probable reason to believe that there is indeed bias present in many of the common models historically used. Instrumental variables may pose a solution to simultaneity bias, but remain reliant on subjective validity. It is not argued that IV should be employed by new studies on supply elasticity, but the intention is to raise awareness on the problem of simultaneity.

As the elasticity of 8.9 is clearly higher than expected, some alternate implications for policy can be deducted compared to other studies. Low elasticities usually imply that the supply does not react properly to rising demand and prices, leading to shortages in housing and unaffordability of a majority of properties. These effects are further emphasized in urban environments where land is scarce. Higher elasticities like the one found in this research would however yield opposite results. Supply should be able to react to demand shifts accordingly and balance the market. However, the supply elasticity estimated in this research resembles the purely isolated effect of price on supply. Other determinants of housing supply like geographical limitations, population density and policy itself are therefore not taken into account. Imbalances in the market can thus be allocated to these external factors, as the sole reaction of supply to price seems to compensate internally. The main problem is that governments are not able to simply adjust population or geographies. Policy in the contrary can theoretically be used effectively as a determinant of the supply elasticity while also being an outcome of it at the same time. Either affordability or availability is usually the main aim of policies in the housing market. Those two should logically work together quite well as an increase in availability should also decrease prices. Subsidies, which could be seen as tool to make housing more affordable, are deemed ineffective when elasticities are low as demand is stimulated while the supply cannot react, creating an even larger surplus of demand. In more elastic circumstances, a subsidy could actually result in a boost to construction. This would eventually compensate for the excess demand and balance out demand and supply. At the other end, housing prices should not actively be suppressed to keep housing more affordable, as this might now lead to limited supply fueling demand surpluses. What is most important to keep in mind when considering these implications, is that they rely on all other factors staying constant. Policies should therefore not be implemented solely based on what a supply elasticity is telling, but must be carefully weighed with regard to the specific circumstances of specific locations. Projections of different contributing variables could furthermore help in creating alternate and tailor-made solutions.

Finally, some solid points of improvement for further research on this topic could be met to increase accuracy and broaden the perspective on the impact of simultaneity bias and possibly the application of instrumental variables. First of all extending the dataset in terms of recorded periods could give more clear results and more definitive answers to which extend simultaneity bias can be solved by using instrumental variables. Another possibility to review the applicability of IV would be to evaluate similar relations in other countries. However, the primary caveat created by this research does not lie within the data, but is still mainly based around to which extend the instruments are exogenous. The most important recommendation would thus be to look for new possible instruments, and examine whether different instruments would yield comparable results. Extensive research on this could eventually lead to more unambiguous and more definite conclusions on the impacts of simultaneity bias within housing supply dynamics. Finally, if simultaneity bias does indeed prove to be a persistent problem in estimating supply elasticities, new solutions will have to be found for new studies to find reliable, consistent and comparable estimates. It does still depend on what the aim of the study is, as much of the previous research is conducted with the aim of comparability to other research. Finding the causal effect of housing price on supply could however provide new inputs towards housing market policy and might help to reduce demand surpluses often present in today’s market.

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Appendix

Appendix 1: Cross-sectional and time-varying panel statistics

Variable		Mean	Std. Dev.	Min.	Max.	Obs.
Building Permits	overall	91.42	151.30	0	2250	N = 2840
	between		127.42	0.125	1297	n = 355
	within		81.84	-687.58	1355.29	T = 8
House Price (in €)	overall	250808.5	71452.53	120209.3	872210	N = 2840
	between		65862.41	135821.4	657600.3	n = 355
	within		27898.08	90042.63	470691.2	T = 8
Population	overall	47803.48	69873.53	919	862965	N = 2840
	between		69935.21	936	827207.5	n = 355
	within		1853.689	10705.98	83560.98	T = 8
Construction Cost (in €/m2)	overall	757.75	47.53	694	845.89	N = 2840
	between		0	757.75	757.75	n = 355
	within		47.53	694	845.89	T = 8
Rent increase (in %)	overall	1.25	1.13	-0.1	3.4	N = 2840
	between		0	1.25	1.25	n = 355
	within		1.13	-0.1	3.4	T = 8
2 year interest rate (in %)	overall	-3.11	0.79	-4.51	-2.27	N = 2840
	between		~0.00	-3.11	-3.11	n = 355
	within		0.79	-4.51	-2.27	T = 8
2-10 year interest rate spread (in %)	overall	1.20	0.40	0.58	1.77	N = 2840

	between		0	1.20	1.20	n = 355
	within		0.40	0.58	1.77	T = 8
Real estate tax (in %)	overall	0.12	0.03	0.04	0.27	N = 2840
	between		0.03	0.04	0.24	n = 355
	within		0.01	0.06	0.17	T = 8
Criminal cases	overall	2683.15	6877.97	25	101650	N = 2840
	between		6836.63	34.38	92224.38	n = 355
	within		825.97	-7871.22	12108.78	T = 8

Appendix 2: Correlation table of all variables

		1	2	3	4	5	6	7	8	9
1	Permits	1.00								
2	Price	0.04*	1.00							
3	Population	0.04*	0.28*	1.00						
4	Construction Cost	0.74*	-0.05*	0.00	1.00					
5	Rent increase	-0.09*	-0.26*	-0.79*	-0.00	1.00				
6	2Y interest rate	0.13*	0.14*	0.16*	0.00	-0.67*	1.00			
7	2Y-10Y spread	-0.12*	-0.18*	-0.20*	-0.01	0.26*	-0.43*	1.00		
8	Real estate tax	-0.05*	-0.35*	-0.05*	-0.05*	-0.01	0.13*	-0.24*	1.00	
9	Criminal cases	0.67*	-0.03	-0.02	0.97*	0.02	-0.02	0.04*	-0.08*	1.00

* p < 0.05