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# Income inequality and housing prices in the very long-run

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**Abstract**

We examine the relationship between income inequality and house prices for a panel of 17 OECD countries over the period 1870 to 2015. Our identification strategy takes advantage of exogenous variation in culturally weighted communist influence to instrument for within-country variations in income inequality. Controlling for endogeneity, time, and country fixed effects, our results suggest that an increase in income inequality has a significant negative effect on real house prices. This finding is robust to the use of both the Gini coefficient and top income share as measures of income inequality and the use of absolute and relative income inequality measures, as well as a range of other robustness checks. We examine crime as a mechanism through which income inequality influences housing prices and find that the theft rate is a channel via which higher income inequality contributes to lower house prices.

**KEYWORDS**

house prices, income inequality, OECD

**JEL CLASSIFICATION**

D31; E25; E31

  

## 1 | INTRODUCTION

Much research has focused on the antecedents, and implications, of the global increase in house prices that raises concerns about housing affordability (Canarella et al., 2021; Knoll et al., 2017). The literature has examined the effects of factors such as infrastructure, neighborhood characteristics, and immigration, among others, on house prices (Halvorsen & Pollakowski, 1981; Tsui et al., 2016; Caudill et al., 2014; Zhu et al., 2018; Gong et al., 2015; Duncan, 2010). At the same time, there has been a steady increase in income inequality around the world (Alvaredo et al., 2017; McCall & Percheski, 2010; Cohen & Ladaique, 2018; Tridico, 2018; Atkinson, 2003; Atkinson et al., 2011). This has led to growing interest in understanding the implications of the widening income gap (Panizza, 2002; De Dominicis et al., 2008; Reyes-García et al., 2019; Ngamaba et al., 2018; Kondo et al., 2009; Patel et al., 2018; Chong & Gradstein, 2007).

What is the effect of the increase in income inequality around the world on the housing market? Inferences from the theoretical literature remain inconclusive as a priori, the effect of income inequality on house prices could either be negative or positive depending on the mechanism of influence. Empirically, while a few studies have started to examine the relationship between income inequality and access to housing or housing prices, several questions remain.

Most of what we do know is for individual countries, limiting generalizability. Zhang (2015) use data from the Chinese Urban Household Survey covering 186 cities in 16 provinces over the period 2002 to 2019. Using ordinary least squares (OLS) and fixed effect models, they report that higher income inequality within Chinese cities is associated with higher house prices, smaller per capita living space, and lower quality housing. In a related study using the same dataset and methods, Zhang et al. (2016) show that income inequality is positively associated with the house price-to-income ratio. Määttänen and Terviö (2014) use data from the American Housing Survey for six major cities covering the period 1998 to 2007 and find that an increase in income inequality is negatively related to house prices. Hassani et al. (2019) use U.K. quarterly data over the period 1975 to 2016. They apply a series of time-series univariate and bivariate linear and nonlinear models and find no evidence of causality between inequality and house prices. Özmen et al. (2019) use regional data from Turkey covering the period 2014 to 2017. They apply a panel fixed effects model and find that income inequality is negatively associated with house prices, while heterogenous effects are reported for different quintiles. Specifically, they find that while income shares of the bottom three quintiles are positively associated with house prices, the top income quintile is associated with negative effects.

Where there are studies that use data on multiple countries, the timeframe is relatively short. Using data for 28 European Union (EU) countries over the period 2003 to 2018, Dewilde and Lancee (2013) apply multi-level models and find that higher income inequality is associated with increased probability that low-income groups will not be able to afford housing. Goda et al. (2019) use data on 15 OECD countries for the period 1975 to 2010 to provide short- and long-run evidence on the determinants of house prices. Using a dynamic panel fixed effects approach, they examine the effects of absolute and relative inequality and find that only absolute inequality was positively correlated and cointegrated with house prices.

A further issue is that we know little about the transmission channels through which income inequality influences housing prices. Together, these are significant shortcomings in the existing literature on the relationship between income inequality and housing prices when policymakers are increasingly concerned about the implications of growing income inequality for housing affordability and the probability of homeownership (Coulter, 2017; Dewilde & Lancee, 2013; Zhang, 2015).

We examine the effects of income inequality on house prices using data on a panel of 17 OECD countries from 1870 to 2015.[1] We make three main contributions to the literature. The first is that we use long historical panel data. We employ an extended sample of OECD countries over about 150 years, thus allowing us to understand how evolution of income inequality, in what is now the world's richest countries, has influenced house prices over time. The advantage of using a long panel for the OECD is that we are able to capture considerable variation in housing prices and income inequality over time. House prices in the OECD were relatively constant from the nineteenth to the mid-twentieth century, but rose strongly, and with substantial cross-country variation, in the second half of the twentieth century (Knoll et al., 2017). Income inequality was relatively high prior to World War II, but then fell in the three decades after World War II (Atkinson & Leigh, 2013). This reflected the introduction of progressive taxation on capital accumulation and the expansion of social security schemes following the New Deal in the United States and the rise of Keynesianism in western democracies (Piketty, 2003). There was a further reversal in income inequality in the late 1970s, reflecting tax cuts at the top of the scale (Atkinson, 2003) and financial deregulation in the 1980s that favored high income earners (Brandolini, 1998) and contributed to higher income inequality.

Our second contribution is that we use variations in culturally weighted foreign communist influence, constructed by Madsen et al. (2018), as a novel instrumental variable (IV) for income inequality. When examining the relationship between income inequality and housing prices, income inequality is likely to be endogenous. Potential causes of endogeneity are reverse causality running from housing prices to income inequality, measurement error, and omitted variables are given that it is difficult to control for all variables that could influence housing prices, especially over such a long period. We expect that culturally weighted foreign communist influence will be correlated with income inequality because workers' wage aspirations and calls for greater welfare provision, both of which lower income inequality, are more likely to be accommodated by employers or political elites when the threat of communist influence is more severe. A potential threat to the exclusion restriction would be if countries that have higher communist influence have more social housing, rent controls, and tenant protection, each of which affects housing prices directly. We argue below, though, that this is unlikely to be the case for two reasons. The first is that several of the countries with the lowest mean communist influence have had the most extensive record of rent controls and tenant protection. The second is that the timing of the introduction of rent controls and social housing policies coincided with the World Wars, not the threat of communism, and that many countries were dismantling their rent controls at the greatest threat of communist influence in the Cold War. In robustness checks, we also use a series of long-run estimators that are robust to endogeneity.

Our third contribution is to examine the role of crime rates as a potential mediator of the relationship between income inequality and house prices. Previous studies have shown that income inequality is associated with higher crime rates (Brush, 2007; Choe, 2008; Elgar & Aitken, 2011) and that higher crime rates are an antecedent of lower house prices (Lynch & Rasmussen, 2001; Tita et al., 2006). While we do not have data on crime for the OECD countries in our sample back to 1870, we construct an unbalanced panel, consisting of homicides, robberies, and thefts per 100,000 people dating from 1900 to examine if crime rates are a mechanism through which the effect of inequality transmits to house prices.

[1]Our sample period ends in 2015 given that for some variables included in this analysis, we do not have data beyond 2015. Further details on data sources are discussed below.

Our results suggest that an increase in income inequality is associated with a decrease in house prices. Our preferred results, which account for endogeneity of income inequality, suggest that, on average, a 1% increase in the relative Gini coefficient is associated with a decline in real house prices in the range of 1.1% to 1.4%, depending on the exact specification. This general finding of a negative relationship is robust to measuring income inequality by the absolute Gini coefficient, income share of the top 10%, and employing a range of alternative model specifications, as well as other sensitivity checks. We find that the theft rate is an important channel through which higher income inequality leads to lower house prices.

The remainder of the paper is structured as follows. The next section presents a conceptual overview of how income inequality could affect house prices. Section 3 discusses the data and empirical methods, while Section 4 presents the results. Section 5 concludes.

## 2 | WHY SHOULD INCOME INEQUALITY AFFECT HOUSE PRICES?

Theoretically, income inequality can have both direct and indirect effects on housing prices. One direct effect is that income inequality has been associated with increasing willingness to pay for houses (Gyourko et al., 2013; Määttänen & Terviö, 2014). When income inequality increases, wealthy people at the top end of the demand side have more money to buy properties; thus, increasing demand. Faced with a limited supply of housing stock, as is the case in many high-income countries, this puts upward pressure on housing prices, pushing low income earners out of the market. Moreover, as a direct result of the bidding process, which increases the value of houses, homeowners and investors expect house prices to keep increasing and hold on to properties which otherwise would have been put onto the market, further restricting supply and pushing prices up. A related factor is that as people earn more, they become less open to properties being built in close vicinity to them and are more willing to pay to limit construction in spaces bordering them, further restricting supply (Gyourko et al., 2013).

A second direct way in which income inequality and housing prices are related is that income inequality has been linked with investing in housing, increasing the demand for houses (Nakajima, 2005; Zhang, 2015). This relates to housing consumption for "conspicuous" or "emulative" purposes (Dwyer, 2009), in which housing becomes a yardstick for social and economic success. Conspicuous consumption explains the increased consumption of housing by the rich as an ostentatious display of wealth, while housing consumption for emulative reasons describes mimicry attempts of lower income earners. The cumulative result is increased demand for available housing stock resulting in higher prices. For internationally appealing cities, which have in place policies designed to attract foreign investment, increased pressure from consumption flows stems not only from locals, but also from international investors who may not necessarily occupy those houses for long periods of time.

A third direct channel is that higher house prices resulting from income inequality generate socio-economic sorting, in which houses in more desirable areas are owned by the rich and those in less desirable locations are concentrated in the hands of lower income earners (Lupton & Power, 2004). This leads to sharp price increases in desirable locations because high-income earners compete for housing in those areas, considering it an exclusive good. High housing prices in desirable locations tend to increase the overall average house price.

Income inequality, though, can also influence house prices negatively through various channels. Income inequality is accompanied by lower social trust, lower social mobility, and higher crime rates (Brush, 2007; Choe, 2008; Elgar & Aitken, 2011; Gustavsson & Jordahl, 2008), which are associated with undesirable neighborhood characteristics that negatively influence house prices (Lynch & Rasmussen, 2001; Taylor, 1995; Tita et al., 2006).

The pricing impact of crime is complex and varies by type of crime, property type, and location (McIlhatton et al., 2016). Most of the literature that studies the effect of crime on house prices is about local areas and finds that crime leads to a decrease of house prices in crime hotspots –less demand– and an increase in house prices in neighboring areas, which tend to see an inflow of residents that leave crime hotspots (Ceccato & Wilhelmsson, 2020).

If crime leads to less housing demand in localized areas and higher housing demand in neighboring areas, this suggests that at higher levels of aggregation—including at the national level—these effects should cancel out. Some studies have found this to be the case. For example, Lynch and Rasmussen (2001) find that for Jacksonville as a whole crime had virtually no effect on house prices overall, while Ceccato and Wilhelmsson (2020) reached the same conclusion for Stockholm. Gibbons (2004) finds that for London as a whole, criminal damage lowers housing prices, while burglary and housing prices are not related. Cigdem-Bayram and Prentice (2019) find that crimes against the person has no effect on property prices in metropolitan Melbourne, in the state of Victoria in Australia, but it reduces property prices in the much broader geographical area of regional Victoria. More generally, the majority of studies of the relationship between crime rates and property prices, where the unit of analysis is large metropolitan cities, find some evidence that crime is negatively related to property prices. Ihlanfeldt and Mayock (2009) review 18 hedonic price studies for U.S. cities and regions and find that 14 report a negative, statistically significant, relationship between one or more measures of crime and house prices. Studies at the national level are rare. One such study is Braakmann (2017), who finds that violent and nonviolent crime is associated with lower property prices, while burglary, robbery, and vehicle crime are unrelated to property prices for the whole of England and Wales. Hence, while it varies by crime, there is evidence of a negative relationship between crime and house prices at higher levels of geographic aggregation.

Why might crime rates be negatively related to housing prices at the national level? Within regions, the price effects of crime will only cancel out if the price decrease in the crime-affected area is symmetric with the price increase in the neighboring area. But, the negative effect of crime on demand for housing in areas affected by crime is likely to be greater than the increase in demand for housing in neighboring areas. The potential exists for crime induced poverty traps (Mehlum et al., 2005), in which people moving from crime affected areas to neighboring areas are likely to have lower purchasing power, meaning that the price increase in neighboring areas is not offsetting the price decline in the crime affected area. Crime rates also tend to be higher in inner suburbs where population density is higher, while incidence of crime tends to be lower in the outer suburbs. Moving house to escape crime may involve moving to the outer suburbs. The inner suburbs, though, tend to be closer to where most people work. The increased cost of the daily commute is likely to reduce the purchasing power available to spend on housing in the outer suburbs.

There might be temporal variation in the relationship between crime rates and housing prices within countries. If differences in crime rates across time are reflected in broader neighborhood effects, such as collective efficacy and social cohesion (Gibbons, 2004), in time periods in which crime rates are high, housing prices may be lower, controlling for other factors that influence housing prices. The same reasoning applies to differences in the relationship between crime rates and housing prices across countries, with countries that have higher crime rates depressing housing prices in these countries, relative to countries with lower crime rates.

Access to mortgage credit is another channel through which income inequality could influence house prices. With higher inequality, credit constraints are more binding for households with lower income. The cost of entry into financial markets or to get access to credit is often higher, as credit is often channeled to wealthier households at the top of the income distribution (Greenwood & Jovanovic, 1990). Given that the share of population at the lower end of the income distribution tend to be higher, lack of access to mortgage credit induced by inequality engenders lower demand in the housing market, thus negatively affecting house prices.

Overall, this suggests ex ante that higher income inequality may be associated with either higher or lower housing prices. The direct effect of income inequality on housing prices suggests that higher income inequality should be associated with higher housing prices. However, the indirect effect of income inequality could result in lower housing prices.

## 3 | DATA AND EMPIRICAL STRATEGY

### 3.1 | Data

We use a historical dataset for major OECD countries spanning from 1870 to 2015 on house prices and financial conditions constructed by Jordà et al. (2017) and Jordà et al. (2019). Data on short-term interest rate, mortgage credit, real GDP per capita, and private sector credit are sourced from Jordà et al. (2017) and are available in the Jordà -Schularick-Taylor Macrohistory Database.[2] Data on the age dependency ratio, communist influence, income share of the top 10%, and Gini coefficients are from Madsen et al. (2018). Based on data availability, our sample consists of the following 17 OECD countries for the period 1870 to 2015: Australia, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and the United States.

We examine the role of crime rates as a channel through which income inequality influences house prices. While we do not have data on crime rates that date back to 1870, we constructed an unbalanced panel from 1900, capturing robberies, theft, and homicides per 100,000 people. Data for all 17 OECD countries from 1900 to 1974 are taken from Archer and Gartner (2006) and updated using data from Von Hofer et al. (2012), the US Department of Justice UCR Database, the UK Government Statistics Office, and the Global Economy Database.[3]

We conduct a simple check to examine how the crime statistics from Archer and Gartner (2006) compare with those from other sources that we use to update the data. To do so, we compared the statistics for the overlapping years for which data are available for Archer and Gartner (2006) and the other sources. We find that the statistics are generally similar with very minor deviations except that, in the case of the Nordic states, the Von Hofer et al. (2012) statistics suggest relatively higher crime rates in some cases, which could reflect underreporting in Archer and Gartner (2006). The most similar rates were for homicides, which have a mean of 5.87 homicides per 100,000 in Archer and Gartner (2006) and a mean of 5.72 in the other merged sources. The widest gap in average crime rates for the overlapping years was observed for

[2]http://www.macrohistory.net/data/.

[3]For the United Kingdom, see, https://www.gov.uk/government/statistics/historical-crime-data; for the US, see https://www.ucrdatatool.gov/Search/Crime/State/RunCrimeStatebyState.cfm; for the Global Economy Database, see https://www.theglobaleconomy.com/download-data.php.

the theft rate with a mean of 602.88 thefts per 100,000 people in Archer and Gartner (2006) and a mean of 686.20 thefts per 100,000 people in the other sources.

Table 1 provides a description and summary statistics. We use the log of the various indicators of inequality; namely, the relative and absolute Gini coefficient and the income share of the top 10%. The average relative Gini coefficient, absolute Gini coefficient, and income share of the top 10% for our sample are 3.49, 12.13, and 3.55 in log scales, respectively. There are significant variations in income inequality and house prices across OECD countries.

## 3.2 | Cross-sectional dependence and panel unit root tests

We begin our analysis by examining the time series properties of our variables. We first test for cross-sectional dependence (CD). The CD test results, which are presented in the last column of Table 2, reject the null of cross-section independence. Hence, to test the stationarity of the variables we employ the cross-sectionally augmented IPS (CIPS) test proposed by Pesaran (2007). This test accounts for cross-sectional dependence using the simple average of the individual cross-sectionally augmented Dickey–Fuller (CADF) tests.

**TABLE 1** Description and summary statistics of variables

| Variable | Description | Mean | *SD* | Min. | Max. |
|---|---|---|---|---|---|
| Log real house prices | Log of the ratio of real house prices to income per capita | 0.64 | 4.43 | −8.17 | 7.98 |
| Log net Gini coefficient (Gini) | Log of relative Gini | 3.49 | 0.25 | 2.91 | 4.39 |
| Log top 10 percent income shares (Top10) | Log income share of the top 10% | 3.55 | 0.22 | 2.89 | 4.33 |
| Log absolute Gini | Log of absolute Gini | 12.13 | 0.84 | 10.20 | 14.13 |
| Communist influence | Communist influence external to the country | 14.82 | 12.96 | 0.21 | 49.19 |
| Mortgage loans to GDP ratio | Mortgage loans to nonfinancial private sector (% GDP) | 0.26 | 0.25 | 0.00 | 1.44 |
| Financial development (credit) | Private sector credit to GDP ratio | 54.47 | 41.66 | 0.21 | 227.75 |
| Short-term interest rate | Short-term interest rate (percent per year) | 4.84 | 3.21 | −2.00 | 21.27 |
| Log GDP per capita | Real GDP per capita (purchasing power parity) | 8.66 | 0.92 | 6.60 | 10.48 |
| Log population | Log number of people (000) | 16.52 | 1.25 | 14.33 | 19.59 |
| Age dependency ratio | Fraction of the population with age higher than 65 and younger than 15 | 57.49 | 7.98 | 42.48 | 82.77 |
| Log robberies | Log of robberies per 100,000 people | 2.56 | 1.85 | −1.58 | 7.64 |
| Log thefts | Log of thefts per 100,000 people | 6.03 | 1.67 | 1.52 | 9.07 |
| Log homicides | Log of homicides per 100,000 people | 0.18 | 0.93 | −3.46 | 4.09 |

**TABLE 2** Unit root and cross-sectional dependence (CD) test results

| Variable | Unit root test | | | | CD-test | |
|---|---|---|---|---|---|---|
| | Levels | | First difference | | | |
| | $Z(\bar{t})$ | *p*-value | $Z(\bar{t})$ | *p*-value | Test-stat | *p*-value |
| Real house prices | 0.226 | .589 | −11.992*** | .000 | 91.22 | .000 |
| Net Gini | −1.164 | .122 | −17.373*** | .000 | 45.91 | .000 |
| Absolute Gini | 0.265 | .605 | −14.909*** | .000 | 134.77 | .00 |
| Mortgage/GDP | −0.546 | .293 | −9.105*** | .000 | 73.67 | .000 |
| Credit | 1.622 | .948 | −12.869*** | .000 | 62.93 | .000 |
| Interest rate | −4.939*** | .000 | −18864*** | .000 | 86.53 | .000 |
| GDPPC | −1.038 | .150 | −15.584*** | .000 | 134.13 | .000 |
| Population | 5.628 | 1.000 | −3.119*** | .001 | 129.79 | .000 |
| Age dependency | −2.329** | .010 | −3.840*** | .000 | 80.79 | .000 |

*Note:* The critical values for the CIPS test statistics are provided by Pesaran (2007) in table 2(a–c).
**Indicate rejections of the null of a unit root at 5% significance levels.
***Indicate rejections of the null of a unit root at 10% significance levels.

The results of the CIPS unit root test, which are presented in Table 2, show that we cannot reject the null hypothesis of a unit root in our series in levels except for the interest rate and the age dependency ratio. However, the null of a unit root process is rejected in the first differences for all variables. The rejections of the unit root hypotheses in the first differences of the variables suggest that house prices and income inequality are integrated of order 1, I(1).

## 3.3 | Empirical strategy

To examine the effects of income inequality on the housing market, based on the test results in Section 3.2, we estimate a first-differenced equation of the following form:

$$\Delta hp_{it} = \alpha_i + \beta_1 \Delta I_{it} + \beta_2 \Delta \mathrm{MGDP}_{it} + \beta_3 \Delta (I \times \mathrm{MGDP})_{it} + \gamma \Delta X_{it} + \mu_t + \varepsilon_{it} \tag{1}$$

Where $\Delta$ is the first difference operator, $hp_{it}$ is log of real house price to income ratio and $I_{it}$ denotes our measure of income inequality for country $i$ in year $t$. Our measure of the dependent variable follows studies such as Adam et al. (2012), De Stefani (2020), Himmelberg et al. (2005), Jordà et al. (2015), Zhang (2015), and Zhang et al. (2016), who employ the log of the ratio of real house prices to income per capita. This measure is a widely accepted indicator of housing cost and affordability in OECD countries (André, 2010).

In the main results, we use the relative Gini coefficient to denote income inequality, although we use the absolute Gini coefficient and income share of the top 10% in robustness checks. MGDP denotes total mortgage credit as a ratio of GDP. We include the interaction term between inequality and mortgage credit ($I \times$ MGDP) as the effect of inequality may be conditional on access to credit (Madsen et al., 2018). $X_{it}$ denotes control variables - the interest rate, the age dependency ratio, and financial development. $\alpha_i$ are country fixed effects that control for cross-country differences in time-invariant determinants of income inequality and house prices. $\mu_t$ are year fixed effects that capture common time shocks. Examples of common time shocks are common global shocks that affect OECD countries, such as business cycles or oil price shocks. $\varepsilon_{it}$ is the error term that captures all other omitted factors.

Empirically, identifying the effect of income inequality on house prices by estimating $\beta_1$ in Equation (1) requires a strategy to isolate changes in income inequality that are plausibly uncorrelated with shocks in house prices in the current year. The main concern is that income inequality may be endogenous due to reverse causality or measurement error. Given that housing constitutes a significant proportion of household income and wealth in many countries, a change in housing prices may also influence income inequality (Piketty & Zucman, 2014). Further, given that the dataset used in this study is compiled from multiple sources, measurement error is a likely source of endogeneity. For instance, previous research suggests that existing measures of house prices tend to underestimate the house price appreciation (Davis & Heathcote, 2005; Knoll et al., 2017). Reverse causality and measurement error will result in a downward bias in estimates that do not control for endogeneity.

To address endogeneity, we use exogeneous variations in communist influence, constructed by Madsen et al. (2018), as an instrument for income inequality. Communist influence is constructed using linguistic distance (as a proxy for cultural distance) between country *i* in each of the OECD countries and communist regime *j*, number of common nodes in the languages of countries *i* and *j,* and the size of their population. The communist influence index has been used in other contexts as an instrument for income inequality (El Herradi & Leroy, 2020; Uddin et al., 2020), and also shown to be correlated with income inequality in other contexts (Anna & Weller, 2020). The manner in which the variable is constructed and the data sources are discussed in detail in Madsen et al. (2018) and in the online supplementary materials provided by Madsen et al. (2018). The baseline data for communist rule back to 1917 is *Encyclopedia Britannica*, which is supplemented by additional country-wise data.

As Madsen et al. (2018) discuss, prior to 1917 no country was under communist governance, although the labor movement was gaining momentum from the 1850s, following publication of Marx and Engels' 1848 *Manifesto*. Madsen et al. (2018) use union density to backdate communist influence before 1917. The data on union density comes from a number of country-specific sources documented in Madsen et al.'s (2018) supplementary materials.

The orthogonality of the culturally weighted communist influence relies on the assumption that changes in foreign communist influence only affect real house prices through their effect on income inequality. While none of the OECD countries in our sample have had a communist government, as argued by Madsen et al. (2018), it is reasonable to expect that the ideological threat from communist influence will help shape relative income inequality through the behavior of elites, governments, unions, and center-left politics.

We use communist influence to instrument both relative income inequality and absolute inequality. Madsen et al. (2018) instrumented relative income inequality with communist influence. For absolute income inequality, not only the distribution of income, but also the level of mean income matters (Goda et al., 2019; Goda & Torres García, 2017). We contend that communist influence will be correlated with absolute income inequality, in addition to relative income inequality as posited by Madsen et al. (2018), through its effect on the behavior of governments and unions. One reason is that when people refer to income inequality, surveys suggest that they are referring as much to absolute income differences as relative income differences (Harrison & Seidl, 1994). Hence, to the extent that communist influence exerts pressure on elites to reduce income inequality in response to their constituents, one expects this to apply to both concepts. A second reason is that, historically, absolute within-country inequality declined between 1929 and 1950 (Goda & Torres García, 2017). An important reason for this was the strengthening of trade unions and labor rights and the creation of the social welfare state (Goda & Torres García, 2017). Collier (1999) suggests that much of the increased agitation for worker rights in the 1920s and 1930s in Europe in countries such as Belgium, Finland, Germany, and Sweden can be traced to the increased threat of communism following the Russian Revolution in 1917. Moreover, absolute income inequality was relatively stable in the 1950s and 1960s at the height of the Cold War, when communist influence in all countries in our sample was elevated, but it increased markedly after 1985 (Goda & Torres García, 2017), when the threat of communism was much lower.

With regard to the validity of the instrument, if countries that have higher communist influence have more social housing, rent controls and tenant protection, each of which affect housing prices directly, the exclusion criteria will not be valid. We argue that this has not been the case for two reasons. The first is that while each of the countries in our sample have had some sort of rent controls and tenant protection at different points during the time we study, several of the countries with the lowest mean communist influence over the period have had the most extensive record of rent controls and tenant protection. Two of the countries with the lowest mean communist influence over the period are the Scandinavian countries of Norway and Sweden, each of which has had extensive social housing programs. Norway has had rent controls from 1916 to 1936, 1940 to 1969, and for limited periods and certain types of housing thereafter (Eitrheimigstad & Erlandsen, 2004). Sweden had rent controls from 1917 to 1923 and 1942 to 1978 with public housing accounting for about 20% of all houses and half of the rental sector (Svensson, 1998). Germany is another country with low mean communist influence, but has had rent controls and tenant protection in the 1920s, from 1936 to 1960 and from the mid-1970s (Homburg, 1993; Knoll et al., 2017). At the same time, the United States, which has the third highest mean communist influence of countries in our sample, has had a limited record of rent control and tenant protection. Apart from World War II, rent control in the United States has been restricted to a few locations such as New York and the District of Columbia in the 1920s and a few cities since the first oil price shock (Arnott, 1995; Willis, 1950).

The second reason why we argue that higher communist influence did not lead to more social housing, rent controls, and tenant protection is that the timing of the introduction of rent controls and social housing policies, which was predominantly during the World Wars and in response to the first oil price shock, does not coincide with the peak in communist influence in each of the countries, which was at the height of the Cold War in the 1950s and 1960s. With the exception of Japan and the United States, each of the countries introduced rent controls in World War I and these persisted to differing degrees into the 1920s and 1930s. The speed with which they were relaxed was linked to the extent to which specific countries were affected by inflation following World War I (Willis, 1950), not communist influence. Similarly, each of the countries introduced rent controls in World War II. The extent to which they were relaxed in Europe following World War II depended on the extent to which the countries were affected by the War (Arnott, 1995; Knoll et al., 2017). Tellingly, countries such as Australia and the United States that had among the highest threat of communist influence, either had little rent control and tenant protections, or were dismantling it, at the height of the Cold War. In the United States, apart from New York, rent controls were relaxed after World War II and were absent from the early 1950s to the 1970s (Arnott, 1995). In Australia, rent controls and tenant protection legislation was dismantled in most states through the 1950s and in New South Wales it was abolished by the end of the 1960s (Albon, 1981). Other countries that had lower mean communist influence over the entire period, but still, in a time series sense, had elevated threat of communist influence in the Cold War, had either eliminated rent controls after World War II or shortly thereafter and had nothing in place through the 1950s and 1960s. For example, France eliminated rent controls in 1948 (Friggit, 2002), the Netherlands in 1950 (Knoll et al., 2017), and Canada immediately after World War II (Arnott, 1995).

Another potential threat to the exclusion restriction is if communist influence is driven by external forces that are common to all countries. However, as Madsen et al. (2018) argue, this seems unlikely. Communist regime changes have occurred for different reasons and in countries at different levels of economic development. The choice of cultural weights in the weighting scheme underpinning the variable also serves to minimize the influence of non-communist external forces that are common across countries. A third threat to the exclusion restriction would be if governments responded to communist influence by providing goods and services to the poor that do not affect inequality. Again, this seems unlikely. As Madsen et al. (2018) remark, almost all government activity has a redistributive component.

We use two-stage least square estimation to address endogeneity of income inequality. The corresponding first-stage estimation equation is given by Equation (2):

$$\Delta I_{it} = \theta_i + \phi_t + \delta \Delta \mathrm{comm}_{it} + \rho \Delta X_{it} + \mu_{it} \tag{2}$$

where, comm denotes culturally weighted communist influence and the coefficient $\delta$ captures the effects of variations in communist influence on income inequality.

# 4 | MAIN RESULTS

## 4.1 | Benchmark estimates

We first present OLS and fixed effects estimates as a benchmark in Table 3. To alleviate issues of reverse causality in the baseline model, we include income inequality with a lag. The results in Column (1) show that the OLS estimates of the relationship between income inequality and house prices are negative and statistically insignificant. This is also the case, in Column (2), when we control for time-invariant determinants of housing prices using the fixed effect estimator.

## 4.2 | Endogeneity-corrected estimates

Table 4 presents the two-stage least squares (2SLS) estimates. In Column (1), we regress real house prices on only the relative Gini coefficient, while other columns increasingly add more controls concluding with Column (7) which includes the full set of control variables. The first stage results support the validity of communist influence as an instrument. The Kleibergen Paap F-statistic is always well above 10, rejecting the null hypothesis of weak instrument bias in all regressions. The first-stage results also show that the coefficient on the communist influence variable is negative and statistically significant, suggesting that greater communist influence is associated with a decrease in income inequality in OECD countries.

Another important identifying assumption is that the instrument is not correlated with the second-stage regression errors, so that variations in communist influence can be used as an exclusion restriction in our IV estimates. Since our model is exactly identified, the Sargan test for over-identification restrictions cannot be computed. An alternative way of testing the

**TABLE 3** Baseline results

| | OLS (1) | Fixed Effects (FE) (2) |
|---|---|---|
| ΔLog Gini$_{(t-1)}$ | −0.054 | −0.089 |
| | (0.103) | (0.093) |
| ΔMortgage loans to GDP ratio | 0.766*** | 0.845*** |
| | (0.181) | (0.202) |
| ΔMortgage loans x Gini | −0.009 | −0.006 |
| | (0.005) | (0.006) |
| ΔCredit | 0.001*** | 0.002** |
| | (0.000) | (0.001) |
| ΔInterest rate | 0.002 | 0.002 |
| | (0.004) | (0.005) |
| ΔAge dependency ratio | −0.423* | −0.347 |
| | (0.236) | (0.414) |
| ΔLog population | 0.520* | 0.503 |
| | (0.294) | (0.360) |
| Observations | 1493 | 1616 |
| R-squared | 0.445 | 0.468 |

*Note:* The dependent variable is the log of the ratio of real house prices to income per capita. All regressions include country and time fixed effects. Robust standard errors in parentheses.
Abbreviation: OLS, ordinary least squares.
$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

exogeneity assumption in the case of exactly identified models, is by examining the sensitivity of the estimates to the exclusion, and inclusion, of control variables (Altonji et al., 2005). The incremental addition of control variables across Columns (1) to (7) show that the 2SLS estimates are insensitive to the inclusion and exclusion of control variables.

The results from Table 4 are consistent with our conjecture that endogeneity is causing a downward bias in the fixed effects estimates. The coefficient on relative income inequality is negative and statistically significant at the 1% level in all regressions, suggesting that rising inequality has a significant negative effect on house prices. Specifically, on average, a 1% increase in relative income inequality as measured by the Gini coefficient is associated with a decline in real house prices in the range of 1.1% to 1.4% depending on the exact specification. Note that this result differs from Goda et al. (2019) who find that the coefficient on relative inequality is insignificant in OECD countries over the period 1975 to 2010. That our results differ from Goda et al. (2019) likely reflects that they only look at a relatively recent period in which income inequality and housing prices have both increased in tandem.

The coefficient of the interaction term between inequality and mortgage is positive and statistically significant at the 5% level, suggesting that the effect of inequality on house prices is conditional on access to mortgage credit. More precisely, the marginal effect of income inequality, conditional on mortgage credit, is given by $\beta_1 + \beta_3 MDGP$. For example, considering the results from the most complete specification in Column (6) of Table 4, the

**TABLE 4** IV estimates of the effect of relative income inequality on house prices

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| 2SLS-second stage regressions | | | | | | |
| ΔLog Gini | −1.167** | −1.295*** | −1.356*** | −1.199*** | −1.155*** | −1.137*** |
| | (0.469) | (0.452) | (0.425) | (0.412) | (0.399) | (0.395) |
| ΔMortgage loans to GDP ratio | | −0.325 | −0.629 | −0.419 | −0.367 | −0.336 |
| | | (0.490) | (0.483) | (0.465) | (0.452) | (0.448) |
| ΔMortgage loans x Gini | | 0.030** | 0.035** | 0.030** | 0.029** | 0.028** |
| | | (0.014) | (0.014) | (0.013) | (0.013) | (0.013) |
| ΔCredit | | | 0.002*** | 0.002*** | 0.002*** | 0.002*** |
| | | | (0.001) | (0.001) | (0.001) | (0.001) |
| ΔInterest rate | | | | 0.001 | 0.002 | 0.001 |
| | | | | (0.004) | (0.004) | (0.004) |
| ΔAge dependency ratio | | | | | −0.379 | −0.414 |
| | | | | | (0.267) | (0.269) |
| ΔLog population | | | | | | 0.624* |
| | | | | | | (0.360) |
| Observations | 1741 | 1630 | 1630 | 1619 | 1619 | 1619 |
| R-squared | 0.309 | 0.431 | 0.434 | 0.442 | 0.446 | 0.448 |
| Kleibergen Paap F-statistic | 11.384 | 16.705 | 16.922 | 16.741 | 17.405 | 17.485 |
| First stage regression | | | | | | |
| Communist influence | −0.004*** | −0.004*** | 0.004*** | −0.004*** | −0.004*** | −0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| F-statistic | 11.38 | 16.71 | 16.92 | 16.74 | 17.41 | 17.49 |

*Note:* The dependent variable is log of the ratio of real house prices to income per capita. All regressions include country and time fixed effects. Robust standard errors in parentheses.
*$p < .1$, **$p < .05$, ***$p < .01$.

marginal effect of inequality at the mean value of MDGP (0.26) is −1.037, which is only slightly lower (in absolute values) than the estimated value of $\beta_1$. This result suggests that the negative effect of inequality is slightly higher in countries in which access to mortgage credit is limited.

## 4.3 | Extensions and robustness checks

### 4.3.1 | Heterogeneity and cross-sectionally dependent robust long-run estimates

The results in Table 4, based on Equations (1) and (2), are in first differences. Given that the CD test results in Table 2 show that our series has cross-sectional dependence and that two of the variables (interest rate and age-dependency ratio) are I(0), as a first robustness check, we estimate Equation (1) in levels to examine the long-run relationship between real house price and income inequality. We pretest for cointegration among the variables using the two-step procedure proposed by Pedroni (2004). The results of the tests, reported in Table A1 of the Appendix, show that the null hypothesis of no cointegration is rejected at the conventional significance levels, indicating the existence of a long-run relationship between the variables. Next, we employ the mean group (MG), augmented mean group (AMG), common correlated effects (CCE), and the dynamic common correlated effects MG (CCEMG) estimators to estimate the long-run effects of inequality on house prices. These long-run estimators have the advantage that they address the issue of heterogeneity in the slope and are robust to cross-sectional dependence and endogeneity (Chudik & Pesaran, 2015).

The results for each of these estimators, which are reported in Table 5, suggest a negative long-run relationship between income inequality and real house prices. This suggests that it is unlikely that our 2SLS estimates are biased by heterogeneity or cross-sectional dependence.

#### 4.3.2 | Bounding values and omitted variable bias

Our 2SLS and long-run estimates consistently suggest that income inequality has a negative effect on real housing prices. In our discussion of instrument quality, we show that the instrument satisfies a suite of instrument validity tests and that the exclusion restriction assumption is plausible. We also demonstrate that the 2SLS estimates are not sensitive to the inclusion or exclusion of variables. However, given that we cover a long historical timeframe that includes both World Wars and various waves of economic and social transformations, one might be worried that these will lead to unobserved country heterogeneity, generating coefficient instability. For example, Knoll et al. (2017) note that residential construction declined rapidly with a severe and long-lasting effect on house prices during World War I. In addition, the advent of World War II marked a significant decline in the property market. Hence, we examine the robustness of our results to omitted variable bias that may arise from unobserved country heterogeneity, such as these heterogeneous effects of economic shocks.

To do this, we employ a method for assessing estimation bias from unobservable factors proposed by Oster (2019), which is useful in estimating the degree of selection on unobservables

**TABLE 5** Estimates of heterogeneous model accounting for CSD

| | (1) MG | (2) AMG | (3) CCE | (4) CCEMG |
|---|---|---|---|---|
| Log Gini | −1.499** | −0.617** | −1.354*** | −1.374*** |
| | (0.608) | (0.315) | (0.478) | (0.462) |
| Controls | Yes | Yes | Yes | Yes |
| N | 1625 | 1625 | 1625 | 1625 |

*Note:* All regressions include full set of control variables, as well as country and time fixed effects. Robust standard errors in parentheses.

Abbreviations: AMG, augmented mean group; CCE, common correlated effects; CCEMG, common correlated effects MG; MG, mean group.

**$p < .05$, ***$p < .01$.

and establishes the lower bound that would confound the effect. Oster's (2019) approach exploits information on coefficient and R-squared movements to calculate the bounding values for the treatment effect. Since observable covariates are assumed to be a random subset of all covariates that are relevant in the model, the underlying assumption is that the selection of the observable and unobservable covariates is assumed to be the same. Therefore, a lower bound estimate can be computed from movement in the coefficients following the inclusion of additional observable covariates.

To provide a brief overview of the approach, consider the following regression model:

$$\Delta hp = \beta \Delta Gini + \gamma \omega^o + W_2 + \varepsilon$$

where $hp$ is real house prices, $Gini$ is the treatment variable, $\omega^o$ is a vector of the observed covariates and $W_2$ is a vector of unobserved covariates. Denoting $W_1 = \gamma\omega^o$, where all elements of $\omega^o$ are assumed to be orthogonal to $W_1$, and hence $W_1$ and $W_2$ are also orthogonal, the proportional selection relationship is $\delta\frac{\sigma_{1Gini}}{\sigma_1^2} = \frac{\sigma_{2Gini}}{\sigma_1^2}$, where $\sigma_i Gini = cov(W_i, Gini)$ and $\sigma_i^2 = var(W_i)$), for $i \in \{1, 2\}$. The parameter $\delta$ is the coefficient of proportionality that captures the relative importance of observables and unobservables. For example, $\delta = 1$ implies a degree of proportionality where observed and unobserved factors have equal importance.

We define the estimated coefficient and the R-squared from the unconditional regression of $hp$ on $Gini$ as $\dot{\beta}$ and $\dot{R}$, respectively, and the respective estimates from the controlled regression of $hp$ on $Gini$ and $\omega^o$ as $\tilde{\beta}$ and $\tilde{R}$. $R_{max}$ denotes the R-squared from a hypothetical regression of $hp$ on all observable and unobservable covariates, including $Gini$. For the OLS estimates of $\dot{\beta}$ and $\tilde{\beta}$, the omitted variable bias is determined by the auxiliary regressions of each element of $\omega^o$ on $Gini$; $W_2$ on $Gini$; and $W_2$ on $Gini$ and $\omega^o$. Based on the proportional selection relationship, $\delta\frac{\sigma_{1Gini}}{\sigma_1^2} = \frac{\sigma_{2Gini}}{\sigma_1^2}$, the bias-adjusted treatment effect is calculated as

$$\beta^* \approx \tilde{\beta} - \delta\left[\dot{\beta} - \tilde{\beta}\right]\frac{R_{max} - \tilde{R}}{\tilde{R} - \dot{R}}$$

We use the suggested bounds for $\delta$ and $R_{max}$ in Oster (2019); that is, we choose $\delta = 1$, as it is unlikely that the unobservables have a greater impact than that of observables included in the model. In addition, Oster (2019) suggests a bound for $R_{max}$, given by $R_{max} = min\left\{1.3\tilde{R}, 1\right\}$.

Table 6 reports results for assessing estimation bias. Column (1) reports the estimated effect of $Gini$ for the baseline uncontrolled model. Column (2) presents the estimates from the controlled model that includes the full set of observed covariates. Standard errors and R-squared are given in parentheses and in brackets, respectively. Column (3) shows the identified set for the impact of income inequality on house prices, $\left[\tilde{\beta}, \beta^*\left(\min\left\{1.3\tilde{R}, 1\right\}, 1\right)\right]$, 1)], and Column (4) shows whether the identified set excludes zero. We also compute the ratio of the impact of unobserved covariates, relative to the observed control variables (denoted as $\bar{\delta}$) that would be needed to drive the estimated coefficient on $Gini$ to zero. This result is reported in Column (5).

Based on Table 6, we find that the identified set, [−0.307, −0.297], excludes zero, suggesting that the estimates from the controlled regressions are robust to omitted variable bias. This implies that the bias adjusted coefficient on $Gini$, $\beta^*$, is consistently negative and does not change sign relative to $\tilde{\beta}$. The estimated baseline and controlled effects suggest that the movements in the estimated coefficient and R-squared are fairly modest. The

**TABLE 6** Parameter stability and robustness to omitted variable bias

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Treatment variable | Baseline effect, $\dot{\beta}$ (Std. error)[$\dot{R}$] | Controlled effect, $\tilde{\beta}$ (Std. error)[$\tilde{R}$) | Identified set $[\tilde{\beta}, \beta^* (\min\{1.3\tilde{R}, 1\}, 1)$ ] | Exclude zero? | $\bar{\delta}$ for $\beta = 0$ given $R_{max}$ |
| ΔLog Gini | $-0.365^{***}$ (0.097) [0.314] | $-0.297^{***}$ (0.103) [0.456] | [−0.307, −0.297] | Yes | 3.688 |
| N | 1656 | 1496 | | | |

*Note:* Results of the uncontrolled and controlled models are from the static OLS regressions. The controlled regression includes full set of control variables, as well as country and time fixed effects. Robust standard errors in parentheses.
$^{***}p < .01$.

results reported in Column (5) of Table 6 show the estimated value of $\bar{\delta}$ for which the estimated coefficient on *Gini* would be zero. The estimated value of 3.688 implies that the impact of the omitted variables has to be more than 3.7 times larger than the impact of the observed explanatory variables, which is clearly unlikely. Therefore, our estimates are robust to omitted variable bias.

### 4.3.3 | Are the results sensitive to the measure of income inequality?

Our main results are based on a relative income inequality measure. However, Goda et al. (2019) suggest that the effects of income inequality on house prices might be sensitive to whether income inequality is measured in absolute or relative terms. Thus, to examine the sensitivity of our results, we follow Bandyopadhyay (2018) and Niño-Zarazúa et al. (2017) and employ absolute income inequality, which we calculate as $A(y) = \bar{y}G(y)$, where A denotes the absolute Gini coefficient, $y$ is the income distribution of a given country, $\bar{y}$ denotes the mean of the income distribution $y$, and G is the relative Gini coefficient. The results are reported in Table 7. The coefficient on the absolute Gini coefficient is negative and significant, which is consistent with our main results in Table 4. Moreover, we find that the coefficient on the logarithm of the absolute Gini is quantitatively lower in all regressions in Table 7 compared to the respective coefficient on the logarithm of the relative Gini reported in Table 4.

The results in Tables 4 and 7 are consistent with employing the Gini coefficient as a measure of income inequality, which captures variations in income inequality that arise from differences in lower- and middle-income households (Hailemariam et al., 2020). However, evidence suggests that the effect of income inequality on house prices depends on the shape of the income distribution (Määttänen & Terviö, 2014). Hence, as an alternative to the Gini coefficient, we employ the income share of the richest 10%. The results, which are presented in Column (1) of Table 8, are consistent with our main results in Table 4.

In our main results, we use the log of the ratio of real house prices to income per capita as the dependent variable. In a further robustness check, we also examine the sensitivity of our results to employing the log of real house prices as in Goda et al. (2019) and Özmen et al. (2019). The results, which are reported in Columns (2) and (3) of Table 8, also suggest that income inequality has a negative effect on house prices, consistent with our main results in

**TABLE 7** IV estimates of the effect of absolute income inequality on house prices

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| 2SLS-second stage regressions | | | | | | |
| ΔLog absolute Gini | −0.985*** | −1.077*** | −1.115*** | −0.975*** | −0.925*** | −0.902*** |
| | (0.338) | (0.318) | (0.310) | (0.307) | (0.299) | (0.288) |
| ΔMortgage loans to GDP ratio | | −0.509 | −0.745 | −0.498 | −0.421 | −0.372 |
| | | (0.483) | (0.473) | (0.460) | (0.448) | (0.433) |
| ΔMortgage loans x Gini | | 0.028** | 0.031*** | 0.026** | 0.025** | 0.024** |
| | | (0.012) | (0.011) | (0.011) | (0.011) | (0.011) |
| ΔCredit | | | 0.002*** | 0.001*** | 0.001*** | 0.001*** |
| | | | (0.001) | (0.001) | (0.001) | (0.001) |
| ΔInterest rate | | | | 0.000 | 0.001 | 0.001 |
| | | | | (0.004) | (0.004) | (0.004) |
| ΔAge dependency ratio | | | | | −0.535** | −0.585** |
| | | | | | (0.271) | (0.270) |
| ΔLog population | | | | | | 0.950* |
| | | | | | | (0.498) |
| Observations | 1741 | 1630 | 1630 | 1619 | 1619 | 1619 |
| R-squared | 0.338 | 0.425 | 0.422 | 0.448 | 0.457 | 0.462 |
| Kleibergen Paap F-statistic | 9.446 | 11.046 | 11.104 | 10.916 | 11.638 | 12.099 |
| First stage regression | | | | | | |
| Communist influence | −0.005*** | −0.005*** | −0.005*** | −0.005*** | −0.005*** | −0.005*** |
| | (0.002) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| F-statistic | 9.45 | 11.05 | 11.10 | 10.92 | 11.64 | 12.10 |

*Note:* Regressions includes country and time fixed effects. Robust standard errors in parentheses.
$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Table 4. In Columns (2) and (3), we further alternate between relative and absolute income inequality as the indicators of income inequality. That the coefficients are negative and significant lends support to the robustness of our results to alternative indicators of income inequality.

In Column (4) of Table 8, while the coefficient on top income share is negative in the log of real house prices regression, it is statistically insignificant. Thus, when we employ the top income share to measure income inequality, in Columns (1) and (4) of Table 8, the effects are weaker than when we employ the Gini coefficient. We attribute this result to the fact that top income share captures a specific aspect of income inequality; that is, income concentration in the top 10% of income earners. The weaker, and statistically insignificant effect of top income share on the log of real house prices in Column (4), could be linked with the neighborhood characteristics of individuals in this income group. Crime is one of the main channels through which income inequality transmits to house prices. However, crime rates tend to be much lower in neighborhoods inhabited by the extremely wealthy (Levitt, 1999). Put differently, the

**TABLE 8** Alternative measures of income inequality and house prices

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| 2SLS-second stage regressions | | | | |
| ΔLog income inequality | −1.442** | −4.361*** | −1.462** | −0.431 |
| | (0.653) | (1.369) | (0.727) | (0.987) |
| Observations | 1593 | 1496 | 1619 | 1470 |
| First stage regression | | | | |
| Communist influence | −0.003*** | −0.001*** | −0.004*** | −0.003*** |
| | (0.001) | (0.0003) | (0.001) | (0.001) |
| F-statistic | 14.23 | 17.01 | 16.38 | 17.64 |

*Note:* Regressions include country and time fixed effects as well as a complete set of control variables consistent with Column (8) of Table 4. The dependent variable in Column (1) is the log of the ratio of real house prices to income per capita. The outcome variable in Column (2) to Column (4) is the log of real house prices. All variables are first differenced consistent with the conclusions from the unit root test. The measures of income inequality are top income inequality (top 10%) in Column (1), absolute Gini in Column (2), relative Gini in Column (3) and top 10% in Column (4). In Column (2), the measure of income inequality is consistent with Table 7 (i.e., absolute Gini) whereas in Column (3) the inequality measure is consistent with Table 4 (relative Gini). We control for GDP per capita on the right-hand side in addition to the full set of covariates. Robust standard errors in parentheses.

**$p < .05$, ***$p < .01$.

observed differences in the results for top income share could reflect the fact that this indicator does not take into account the holistic distribution of income inequality.

### 4.3.4 | Alternative model specifications

Next, we examine the sensitivity of our results to alternative model specifications. Specifically, we perform sensitivity checks to examine the robustness of our results to the control of country-specific linear time trends, the control of the lagged value of real house price, and Gini coefficient. Table 9 presents the results. The specification in Column (1) includes country-specific trends, Column (2) includes the lagged value of real house prices, Column (3) includes lags of the Gini coefficient, and Column (4) includes lags of both house prices and the Gini. In all cases, the coefficient on income inequality is negative and statistically significant at the 1% level. The coefficient of the lagged Gini is statistically indistinguishable from zero.

### 4.3.5 | Sensitivity to instrument construction

We examine the sensitivity of our results to differences in data sources used for the construction of our instrument. As Madsen et al. (2018) discuss, there were no communist governments before the Russian Revolution in 1917. Before 1917 they use union density to measure communist influence. To show that our results are not sensitive to this, we follow Madsen et al. (2018) and conduct our analysis for a subsample covering the years 1917 to 2015. The results, which are reported in Appendix Table A2, show that our main results are robust.

**TABLE 9** Robustness results with alternative model specifications

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| 2SLS-second stage regressions | | | | |
| ΔLog Gini | −1.103*** | −1.025** | −1.073*** | −0.975** |
| | (0.396) | (0.417) | (0.381) | (0.396) |
| ΔLog Gini$_{t-1}$ | | | 0.044 | 0.071 |
| | | | (0.098) | (0.096) |
| Controls | Yes | Yes | Yes | Yes |
| Observations | 1496 | 1479 | 1493 | 1479 |
| R-squared | 0.438 | 0.452 | 0.440 | 0.455 |
| First Stage regression | | | | |
| Communist influence | −0.004*** | −0.004*** | −0.004*** | −0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| F-statistic | 17.71 | 17.03 | 17.15 | 16.71 |

*Note:* Column (1) includes country-specific trends, Column (2) includes lagged value real house price, Column (3) includes lag of Gini, and Column (4) includes lags of both house price and Gini. All regressions include country and time fixed effects. Robust standard errors in parentheses.
$^{**}p < .05$, $^{***}p < .01$.

## 4.4 | What is driving the results?

In this section, we explore crime as a potential channel through which income inequality influences house prices. As discussed in Section 2, income inequality tends to be accompanied by higher crime rates (Choe, 2008; Brush, 2007), which is an undesirable neighborhood characteristic that negatively influences house prices (Lynch & Rasmussen, 2001; Taylor, 1995; Tita et al., 2006). To examine if crime is a potential channel, we use an approach consistent with previous studies (Alesina & Zhuravskaya, 2011; Awaworyi Churchill et al., 2019). For crime to qualify as a channel of influence, in addition to being correlated with income inequality, it should also be correlated with house prices, and the inclusion of crime as an additional covariate in the regression linking house prices to income inequality should decrease the magnitude of the coefficient on income inequality. We use an unbalanced panel from 1900, capturing robberies, theft, and homicides per 100,000 people.

One might be concerned that if countries with a greater threat of communist influence spend more on reducing inequality, this might directly affect crime rates via having a higher degree of social cohesion, stricter law enforcement, and more police officers per capita. We posit that this is not very likely for the following reasons. First, countries such as the United States and United Kingdom, which are each in the top five countries for mean communist influence over the period studied, are routinely regarded as countries with low social cohesion (Green et al., 2011; Reich, 2001). At the other end of the spectrum, Sweden, Norway, and Denmark, which have the lowest mean communist influence, are universally considered to be countries that rank high in social cohesion (Delhey & Newton, 2005; Green et al., 2011). Second, from a time series perspective, social cohesion in the United States, and United Kingdom, and most other OECD countries, declined significantly throughout the Cold War when communist influence was at its highest (Green et al., 2011). Third, there does not appear to be any direct

TABLE 10 The effect of income inequality on crime

| | (1) Robberies | (2) Thefts | (3) Homicides |
|---|---|---|---|
| Panel A: Relative Gini | | | |
| ΔLog Gini | 3.614*** | 2.192*** | 4.434 |
| | (1.341) | (0.592) | (5.568) |
| Observations | 755 | 887 | 898 |
| Panel B: Absolute Gini | | | |
| ΔLog absolute Gini | 3.487*** | 2.935** | −21.910 |
| | (0.940) | (1.223) | (90.299) |
| Observations | 755 | 887 | 898 |
| Controls | Yes | Yes | Yes |

*Note:* All regressions include relevant controls, country and time fixed effects. Robust standard errors in parentheses.
** $p < .05$, *** $p < .01$.

relationship between the threat of communist influence and expenditure on law and order. While the Nordic countries spend a low percentage of their GDP on police, so do countries, such as Japan, that rank high on communist influence, while the United States and United Kingdom are about mid-table among OECD countries in terms of expenditure on police as a percentage of GDP (Cheatham & Maizland, 2020). Fourth, there does not appear to be a direct inverse relationship between communist influence and actual crime rates, which you would expect if the communist influence was influencing crime through expenditure on law and order and higher social cohesion. For example, among countries in our sample, homicides per capita are highest in the United States, while Australia is mid-table and Japan is low. Fifth, regardless of expenditure on police and law enforcement and actual crime rates, in several of the countries that have the lowest mean communist influence in our sample, citizens report feeling among the safest. This is not only true for the Nordic countries, but also countries such as Germany, Netherlands, and Switzerland, which have relatively low communist influence (OECD, 2014).

Panel A of Table 10 reports results for the effects of income inequality using our main indicator of inequality (relative Gini) on crime. Column (1) reports effects on robberies, Column (2) reports effects on thefts, while Column (3) reports effects on homicides. We find that while there is no significant effect of income inequality on the rate of homicides, an increase in income inequality is associated with an increase in the rate of robberies and thefts. In robustness checks, the results reported in Panel B of Table 10 show that the effects of income inequality on crime is robust to the use of absolute Gini as the measure of inequality.[4]

Given that the effect of income inequality is statistically significant only in Columns (1) and (2), we estimate house prices regressions, with robberies and thefts as additional covariates. To ensure that the same subsample is used in the potential channel analysis, we first rerun

[4]The difference in the theft rate, at the upper bound, for overlapping years across the different datasets is less than 0.1% per 100,000 people, which is still relatively low. Nevertheless, to ensure our results are not due to minor differences in crime rates from different data sources, we conduct a subsample mediation analysis for each of homicides, robberies, and thefts for two subperiods; (1) 1900 to 1970 (i.e., employing the Archer and Gartner (2006) data) and (2) post-1970 (i.e., using crime rates from other data sources). The results, reported in Tables A3 and A4, show that our results remain robust.

**TABLE 11** The effect of crime (potential channel analysis)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Log Gini | −1.231*** | −1.122** | −1.384*** | −1.010*** |
| | (0.453) | (0.451) | (0.364) | (0.363) |
| ΔLog robberies | | −0.043 | | |
| | | (0.033) | | |
| ΔLog thefts | | | | −0.030*** |
| | | | | (0.003) |
| Observations | 750 | 750 | 717 | 717 |

*Note:* All regressions include relevant controls, country and time fixed effects. Robust standard errors in parentheses.
**$p < .05$, ***$p < .01$.

regressions with house prices as an outcome variable using the subsample of observations simultaneously available for the crime, house prices, and control variables. Columns (1) and (3) of Table 11 rerun regressions for house prices, while Columns (2) and (4) add robberies and thefts as additional covariates, respectively. The coefficient on robberies is statistically insignificant, although with the inclusion of robberies as an additional covariate in the model in Column (1), there is a marginal decline in the magnitude of the coefficient on income inequality. In Column (4), however, an increase in the rate of thefts is associated with a decline in house prices. Comparing the coefficient on income inequality in Columns (3) and (4), with the inclusion of theft as an additional covariate, the size of the coefficient on income inequality reduces. This result confirms that theft is a channel through which income inequality lowers house prices.

As discussed in Section 2, mortgage credit is another potential channel through which inequality could influences house prices. Empirically, though, we find that in all our specifications reported in Table 4 that the association between mortgage credit and house prices is insignificant. This suggests that mortgage credit is not a valid mediator or transmission channel through which income inequality influences house prices. However, we find that mortgage credit does moderate the relationship between income inequality and house prices, given that its interaction with income inequality is statistically significant. Thus, the effect of inequality on house prices is conditional on access to mortgage credit such that the negative effect of inequality is slightly higher in countries where access to mortgage credit is limited. This suggests that access to credit dampens the effect of income inequality on house prices.

## 5 | CONCLUSION

Rising income inequality across many countries has drawn attention from policymakers. Concurrently, house price growth, which has outpaced income growth in many countries in the past few decades, has inspired a body of literature that seeks to understand the determinants of house prices. We contribute to this literature by examining the role played by income inequality, taking a historical perspective. We have examined the effect of income inequality on house prices in 17 OECD countries over the period 1870 to 2015 and the transmission role played by crime rates from 1900 to 2015. We find that income inequality has had a negative effect on house prices and that this negative effect has channeled through theft rates.

These findings have important implications for economic policy and urban planners. For many people, the majority of their wealth is tied to their investment in their home and they have taken out large mortgages to purchase their home (Cobb-Clark & Hildebrand, 2006). Our results suggest that if increasing income inequality lowers house prices this could lead to loss in household savings and wealth and increase mortgage overhang. This also has negative consequences for consumption and long-term growth (Campbell & Cocco, 2007). As such, at a national level, our results point to a negative implication of income inequality that has not been recognized and provide support for calls to implement redistributive policies.

Our results also have implications for local councils and city/urban planners. Our results for crime as a transmission mechanism lends support to the argument that income inequality engenders undesirable neighborhood characteristics, which negatively influence house prices. This suggests the need for policies that seek to prevent the clustering of households at the lower end of the income group. This is particularly true, given that the effect of rising income inequality on lowering house prices is likely to be concentrated in low income suburbs, often at the urban fringe where households have large mortgages and, as such, are most vulnerable to lower house prices (Valadkhani & Smyth, 2016). It is exactly in these suburbs that income inequality is likely to result in undesirable neighborhood characteristics, such as higher crime rates, that contribute to lower house prices. An important policy to ensure that income inequality does not lead to the clustering of low-income earners is inclusive zoning, which requires that developers include cheaper or subsidized units in new developments. Other policy considerations include tax incentives and other social financial support that could be given to low income earners to allow for relocation in specific neighborhoods.

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## APPENDIX

**TABLE A1** Pedroni (2004) Cointegration test

| | $\rho$ | $t$ | ADF |
|---|---|---|---|
| Panel test statistic | 2.699 | 4.459 | 3.857 |
| Group test statistic | 3.412 | 4.376 | 2.163 |

*Note:* All test statistics are distributed N(0,1), under a null of no cointegration. Optimal lags are determined by AIC.

**TABLE A2** IV estimates with subsample from 1917 onwards

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| 2SLS-second stage regressions | | | | | | |
| ΔLog Gini | −1.230*** | −1.402*** | −1.430*** | −1.272*** | −1.222*** | −1.201*** |
| | (0.472) | (0.477) | (0.442) | (0.422) | (0.405) | (0.403) |
| ΔMortgage loans to GDP ratio | | −0.454 | −0.675 | −0.462 | −0.402 | −0.374 |
| | | (0.507) | (0.490) | (0.464) | (0.449) | (0.447) |
| ΔMortgage loans x Gini | | 0.036** | 0.038** | 0.033** | 0.031** | 0.030** |
| | | (0.016) | (0.015) | (0.015) | (0.014) | (0.014) |
| ΔCredit | | | 0.002*** | 0.002*** | 0.002*** | 0.002*** |
| | | | (0.001) | (0.001) | (0.001) | (0.001) |
| ΔInterest rate | | | | 0.001 | 0.002 | 0.002 |
| | | | | (0.004) | (0.004) | (0.004) |
| ΔAge dependency ratio | | | | | −0.425 | −0.464* |
| | | | | | (0.274) | (0.278) |
| ΔLog population | | | | | | 0.482 |
| | | | | | | (0.409) |
| Observations | 1314 | 1265 | 1265 | 1255 | 1255 | 1255 |
| R-squared | 0.285 | 0.423 | 0.428 | 0.439 | 0.444 | 0.447 |
| First stage regression | | | | | | |
| Communist influence | −0.004*** | −0.004*** | −0.004*** | −0.004*** | −0.004*** | −0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| F-statistic | 11.16 | 15.62 | 15.7 | 15.6 | 16.16 | 16.15 |

*Note:* All regressions include country and time fixed effects. Robust standard errors in parentheses. To further check on the quality of the instrument, we re-estimated Table 4 using subsample data from 1917 to 2015 since there were no communist governments before the Russian Revolution in 1917. Madsen et al. (2018) use union density as a proxy measure to communist influence before 1917.

*$p < .1$, **$p < .05$, ***$p < .01$.

**TABLE A3** Effects of income inequality on crime (subsample analysis)

| | 1900 to 1970 | | | Post 1970 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Robberies | Thefts | Homicides | Robberies | Thefts | Homicides |
| ΔLog Gini | 4.229** | 1.775*** | 7.388 | 4.513** | 1.795*** | 7.467 |
| | (1.713) | (0.588) | (6.122) | (1.875) | (0.591) | (5.935) |
| Observations | 395 | 484 | 456 | 360 | 403 | 442 |

*Note:* All regressions include relevant controls, country and time fixed effects. Robust standard errors in parentheses.
** $p < .05$, *** $p < .01$.

**TABLE A4** The effect of crime (subsample analysis)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ΔLog Gini | −0.963*** | −0.944* | −1.147** | −1.030* |
| | (0.317) | (0.548) | (0.470) | (0.559) |
| ΔLog robberies | −0.023 | | −0.021 | |
| | (0.023) | | (0.022) | |
| ΔLog thefts | | −0.028*** | | −0.025* |
| | | (0.011) | | (0.014) |
| Observations | 345 | 468 | 335 | 357 |

*Note:* All regressions include relevant controls, country and time fixed effects. Robust standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$.