Do Housing Prices Change with Building Codes?*

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Abstract

Building codes reduce losses from natural hazards, improve life safety, and increase community resilience, but opponents often argue that updated codes increase housing prices and worsen affordability. This paper examines whether state-level adoption of newer editions of the International Residential Code is associated with changes in home sale prices. Using ATTOM home sales data from 2011 through 2022 and code adoption histories for twenty-six states, we analyze forty-five adoptions of the 2009, 2012, 2015, and 2018 IRC editions. We estimate hedonic regression models of logged sale prices, controlling for home size, lot size, bedrooms, bathrooms, home age, sale timing, and ZIP Code fixed effects. For newly constructed homes, most code adoptions are not associated with statistically significant price changes, and the aggregate estimated effect is statistically indistinguishable from zero. In an expanded sample that includes resales of homes up to ten years old, statistically significant price increases following code adoption are rare, while price decreases are more frequent. Robustness tests addressing uncertainty in year-built data produce similar conclusions. We also examine border counties in North Carolina and South Carolina and find no evidence that builders shift construction activity to neighboring jurisdictions following code updates. Overall, the results do not support the claim that modern building code adoption systematically increases home prices or constrains new housing supply. Given the established resilience and safety benefits of modern codes, delaying code adoption is unlikely to improve affordability and may increase vulnerability to future hazards.

JEL Codes: **R31, R32, R38**

Keywords: **Building Codes, House Prices, Real Estate**

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1. INTRODUCTION

Exposure to catastrophic perils is on the rise throughout much of the United States. Although many of the hazards contributing to the increased exposure are beyond our immediate control, policymakers have direct access to the built environment through code adoption and implementation. As with all policy decisions, there are tradeoffs in benefits and costs that must be considered. This report focuses on quantifying the cost of building code updates to homebuyers.

Building codes describe the minimum technical requirements for design and construction. Building codes are adopted by state and local municipalities based on legislative authority set at the state level, with differing mandates on adoption. For the construction of most one- and two-family residential structures, many jurisdictions adopt editions of the model International Residential Code (IRC). The IRC specifies details such as building planning and construction, building systems including mechanical, electrical, and plumbing, and energy conservation, among other aspects of building design, construction, operations, and performance. New editions of these codes are published on a three-year cycle, although jurisdictions have broad authority to adopt any edition they choose and to make changes through code adoption amendments or legislation as they see fit to meet local needs. Due to this broad authority, states and local jurisdictions are not required to apply the same version and substance of the IRC at the same time.

For this study, we focus on the relationship between housing prices and potential cost changes correlated with the 2009, 2012, 2015, and 2018 editions of the IRC. We discuss in more detail later in this report that code updates are limited to these editions due to data limitations and market extremes during the 2007-2010 housing crisis. We also focus on versions of the IECC referenced by these IRC editions in each state, as cost increases due to energy provisions are often discussed. The Home Innovation Research Labs (HIRL) provide estimates of cost impact (savings or increase) based on provisions included in these focus editions of the IRC as compared to the respective previous edition.[1] The HIRL estimate the costs of the 2009 IRC ranging from a $6,100 to $16,100 increase over the 2006 IRC, primarily driven by an added residential fire sprinkler mandate, revised minimum insulation and fenestra-

[1] https://www.nahb.org/advocacy/top-priorities/building-codes/construction-codes-standards-research/estimated-costs-of-building-code-changes

tion requirements, and arc-fault circuit interrupter requirements.[2] To understand the tradeoff between the cost impacts and benefits, some key hazard resistance provisions of each IRC edition are discussed here, although we note that this is not an exhaustive list of such provisions. The 2009 IRC introduced key hazard resistance provisions such as required residential fire sprinklers and carbon monoxide alarms, emergency escape and rescue opening updates for basements and window well access, and updates to load requirements for glazing. The HIRL estimate the costs of the 2012 IRC ranging from a \$4,900 to \$13,800 increase over the 2009 IRC, attributed mainly to additional stringency in the energy efficiency provisions, followed by flashing and mechanical ventilation requirements.[3] Key hazard resistance provisions incorporated into the 2012 IRC include updated wind and seismic loading criteria and provisions for assurance of indoor air quality through ventilation and self-closing devices on garage and home doors. HIRL estimates for the costs of the 2015 IRC range from a \$2,000 increase to a \$600 savings over the 2012 IRC, with cost increases driven primarily by foundation footing sizes and the savings primarily from reductions in hot water pipe insulation and less conservative wall bracing.[4] Provisions for hazard resistance introduced in the 2015 IRC include the requirement of freeboard (or additional height of the first floor above ground level) for homes built in flood zones, enhanced protection in windborne debris regions, enhanced foundation design requirements, and adjustments to lumber structural load capacity. For the 2018 IRC, the HIRL estimates range from a \$100 increase to a \$600 savings, with cost increases driven by significant increases for homes built in moderate or high-seismic regions and coastal flood zones and savings attributed to new options for attic insulation.[5] Key hazard resistant provisions of the 2018 IRC include an increase in some structural framing requirements, updated wind and seismic loading criteria, and required breakaway components in some flood zones. Based on the adoption authorities of states and local jurisdictions discussed previously, provisions of the IRC may be changed or removed in the adopting jurisdiction. We make note of some known changes in the IRC editions adopted by states

[2] https://www.nahb.org/-/media/NAHB/advocacy/docs/top-priorities/codes/code-adoption/irc-2009-cost-study

[3] https://www.nahb.org/-/media/NAHB/advocacy/docs/top-priorities/codes/code-adoption/irc-2012-cost-study

[4] https://www.nahb.org/-/media/NAHB/advocacy/docs/top-priorities/codes/code-adoption/irc-2015-cost-study

[5] https://www.nahb.org/-/media/NAHB/advocacy/docs/top-priorities/codes/code-adoption/irc-2018-cost-study

analyzed in this study to understand the cost effects of these actions, but note that these are not an exhaustive accounting of code changes made in these states.

To understand the cost effectiveness of the IECC, HIRL published studies comparing the incremental construction cost and energy use cost savings of a house compliant with the 2009 edition of the IECC relative to the 2006 edition[6] and the 2012 edition relative to the 2009 edition.[7] The 2009 IECC incremental construction cost was estimated at a nationally weighted average of $1,365 more than the 2006, with a nationally weighted energy cost savings of 13.9% and simple payback of 5.6 years, meaning that it would take 5.6 years for the annual energy savings to exceed the incremental construction cost. The 2012 IECC incremental construction cost was estimated at a nationally weighted average of $5,668 more than the 2009, with a nationally weighted energy cost savings of $427 and simple payback of 13.3 years.[8] Across the four code editions, HIRL estimated increases ranging from $14,300 to $40,400. Builder surveys have similarly pointed to cost increases of roughly $26,000 to 32,000.[9]

Indirect costs to communities that experience disasters exacerbated by poor residential building performance are often much more difficult to quantify, but dislocation of community residents, business and social disruption as well as impacts to job markets, future health and mental health impact people and communities long past the initial damage caused by these events. The evidence for these positive life-cycle benefits of building codes should be weighed against potential costs added from the newer versions of building codes.

In addition to quantifying code change cost impacts, it is important to understand the role of codes among other cost factors that affect home prices. A study published in 2005 found that building codes increase housing costs by 5% or less and that other regulations such as zoning and subdivision requirements and impact fees are more consequential to new construction than building codes,[10] although the study

[6] 2009 IECC Cost Effectiveness Analysis. https://www.homeinnovation.com/documents/Insights/Reports/2012/2009-IECC-Cost-Effectiveness-Analysis-2012.PDF

[7] 2012 IECC Cost Effectiveness analysis. https://www.homeinnovation.com/documents/Insights/Reports/2015/2012-IRC-Cost-Analysis.pdf

[8] https://www.nahb.org/-/media/NAHB/advocacy/docs/top-priorities/codes/code-adoption/2012-iecc-cost-effectiv eness-analysis.pdf?rev=26b53db9053349a6b4f245d3d54a2bfe&hash=6F74EABAF209A746C494D7B86

[9] https://www.nahb.org/-/media/F927CA599F4343F19541BD9F929E50ED.ashx; https://www.nahb.org/-/media/NAHB/news-and-economics/docs/housing-economics-plus/special-studies/2021/special-study-government-regulation-in-the-price-of-a-new-home-may-2021.pdf?rev=29975254e5d5423791 d6b3558881227b&hash=9D2D9ABF37021181CD449DF9856446EF

[10] Listokin, D., & Hattis, D. B. (2005). Building codes and housing. Cityscape, 8(1), 21–67.

suggests more research is needed to better understand the connection between codes and housing. Subsequent research was conducted in limited geographic regions. For instance, one study found that updating codes in Memphis, Tennessee would add less than 1% to construction costs while reducing annualized loss—in terms of repair cost, collapse probability, and fatalities—by approximately 50%.[11] Another study compared the effects from Moore, Oklahoma adopting stronger codes in comparison to Norman, Oklahoma which did not. The authors found the updated code had no effect on price per square foot or home sales.[12] As it concerns the energy code, researchers could not find evidence that the implementation of California's energy code caused higher home price appreciation.[13]

The study most recent and relevant to ours does not consider building codes. Rather, it measures the relationship between building costs and house prices. After studying the last 75 years of housing prices and building **costs**, the authors conclude "that building costs have never had all that much explanatory power over US housing prices, but even the imperfect correlations of the past have weakened further in recent decades along multiple dimensions."[14] Their findings are intuitive, given the myriad factors that affect housing prices, including location, size, features, supply, and demand.

Regardless of any potential effects of building codes on home sale prices, building codes have measurable benefits when it comes to life-cycle costs. A study published by the National Institute of Building Sciences in 2019 shows that effective implementation of modern codes has an overall benefit cost ratio of 11:1.[15] The Federal Emergency Management Agency's (FEMA) Building Codes Save report calculates that disaster losses avoided by the adoption of modern codes over a 20-year period could be over $130 billion.[16] FEMA's Mitigation Assessment Team reports continuously show improvements in performance for buildings employing modern building codes compared to older vintages of codes; for example, see a recent report for Hur-

[11] NEHRP Consultants Joint Venture, Cost Analyses and Benefit Studies for Earthquake-Resistant Construction in Memphis, Tennessee, NIST GCR 14-917-26 (2013).

[12] Simmons, K. & Kovacs, P., Real Estate Market Response to Enhanced Building Codes in Moore, OK, Investigative Journal of Risk Reduction (Mar. 2018).

[13] https://efiling.energy.ca.gov/GetDocument.aspx?tn=220992&DocumentContentId=27172.

[14] Potter, B. and C. Syverson, 2025. "Building costs and house prices" Working Paper 33958 http://www.nber.org/papers/w33958, National Bureau of Economic Research

[15] https://www.nibs.org/projects/natural-hazard-mitigation-saves-2019-report

[16] https://www.fema.gov/sites/default/files/2020-11/fema_building-codes-save_study.pdf

ricane Ian.[17] The Insurance Institute for Business & Home Safety estimated between $1 and $3 billion in structural damage was prevented in Hurricane Ian in homes that were built to the modern Florida Building Code[18]. PNNL has studied the relationship between energy efficiency and thermal resilience and found that, in comparison to existing building stock, modern editions of the IECC improve shelter-in-place capability and reduce mortality and property damage in extreme temperature scenarios.[19] IBHS has also noted that this increase in habitability during extreme weather events that cause power outages, such as the 2021 Deep South arctic air event that caused extensive power grid failures in Texas, can lower the risk of plumbing failures due to freezing pipes.[20]

Although building codes can impose cost on builders, several factors mute the direct impact on large scale housing affordability. First, buyers often cannot directly observe most code effects and do not have a mechanism to internalize the value (e.g., greater structural resistance to seismic loads, impact resistant windows, etc.). For example, if choosing between two similar houses, one that is brand new and built to a newer code edition, and the other one year old and built to a previous edition, the buyer is likely indifferent. When building codes change, changes in cost are accounted for by a combination of the buyers and the builders. The builders' cost could be from decreased profit margins, or from reduced building activity. A home's "affordability" for a given buyer depends on its price and that buyer's income. For this reason, both affluent and budget-constrained buyers may self-select into the housing option that best fits their means. Therefore, even if the average price of new houses changes, affordability for individual buyers could remain unchanged.

To understand the relationship between building code changes and housing prices, we compare the sale prices of houses built under various code regimes in specific jurisdictions through time. We present a correlation analysis for all states and for all adopted IRC editions that offer sufficient data. We employ two empirical strategies, the first considers correlation between code adoption and newly constructed home

[17] https://www.fema.gov/sites/default/files/documents/fema_rm-hurriance-ian-mat-report-12-2023.pdf

[18] https://ibhs1.wpenginepowered.com/wp-content/uploads/HurricaneIan_PartII_FBC.pdf

[19] https://www.energycodes.gov/sites/default/files/2023-07/Efficiency_for_Building_Resilience_PNNL-32727_Rev1.pdf

[20] State of the Risk: Interior Water Damage. IBHS Research Advisory Council Residential Committee. December 2024.

pricing while the second considers correlation between building code adoption and home pricing in general. We also compare the number of houses built along contiguous state borders to determine if code adoptions change building patterns. For example, if the additional cost of a building code is sufficiently high, contractors in border counties could choose to build in a neighboring state which imposes less stringent building codes. Prior analyses focused on energy code adoption and found no correlation between code adoption and permitting trends.[21] Our analysis considers the IRC in its entirety and tests for differences in building patterns between North Carolina and South Carolina.

For newly constructed homes, we most commonly observe no change in house prices following adoptions of new IRC editions. In a few instances, it appears that home prices increased or decreased slightly, but considered in total across years and locations, the correlations between building codes and new house prices are roughly a wash. For housing prices in general (not just newly constructed housing), we find that prices may increase or decrease following code adoptions, and that the decreases are more frequent and larger than the increases. We find no discernible pattern of price changes following code adoptions. Our results also show that factors other than code changes exhibit strong correlations with home prices (e.g., house size, lot size, bathrooms per bedroom, etc.). In the cross-border analysis, we find a few instances in which the volume of new houses built differs significantly from year to year. There are no cases where building volume increases in a neighboring state's border counties at the same time as a new code edition is adopted. These results suggest that building code increases do not drive homebuilders from locales with more stringent codes to neighboring jurisdictions with less stringent codes.

In light of this empirical evidence, we conclude that any potential effects of code adoption on home prices are likely insignificant and dwarfed by other market influences.

2. EMPIRICAL ANALYSIS

Our goal is to test for correlation between enactment of a new code edition and the sale price of houses. *A priori*, there is no single best econometric test or model

[21] https://www.mwalliance.org/sites/default/files/meea-research/do_stronger_energy_codes_move_development_to_neighboring_jurisdictions.pdf.

specification. Rather, we follow a process to determine the best statistical tests possible given the available data. Our objective in this process is to isolate the effects of code adoptions on house prices by controlling for as many other factors as possible. We begin with standard hedonic regression models applied in the literature, subject to available data. We optimize model specification toward the best fitting model based on the adjusted R^2 value, which balances explanation of variance in the dependent variable against concerns of overfitting the model.

Importantly, we do not claim to identify a causal relationship between building codes and house prices in our model. We are not aware of a robust causal inference strategy that applies to the research question. Such a methodology would require identification of treated and control home sales with parallel pre-trends. For example, if neighborhood or community of similar houses implemented a building code for certain houses, but not for others, and then built a critical mass of new houses with and without enforcing a building code. Instead, as a second-best strategy, we look for correlations that would likely appear if there were a causal relationship between building codes and house prices. We analyze data on house prices across forty-five code enactments in twenty-seven states, controlling for common factors that affect price such as location, year sold, size, number of bedrooms and bathrooms, and lot size. If building codes have a positive causal effect on house prices, we would expect to find positive correlations between them in this large sample across time and location.

2.1. Data and sample selection

We do not have access to a complete repository of building code adoption data for the United States through the set of studied years. Instead, we gathered code adoption data used in this study from several sources including FEMA's Building Codes Adoption Tracking project[22] and PNNL's Building Energy Codes Program.[23] PNNL provides annual IECC-equivalent adoption per state which considers changes made to the model code editions through amendment or legislation as well as states that adopt custom residential energy codes. We use house price, house construction date, house sale date, and house characteristics data from ATTOM Data Solutions.

Code adoption differs across states due to legislative authorities mentioned previ-

[22]Building Code Adoption Tracking | FEMA.gov https://www.fema.gov/emergency-managers/risk-management/building-science/bcat

[23]https://www.energycodes.gov/sites/default/files/2021-07/2018IECC_CE_Residential.pdf

ously. In states that do not mandate code adoption in all jurisdictions, code adoption can vary widely within a given state. Therefore, we limit our sample to states where we find clear records of the statewide code adoption history. These include California, Florida, Georgia, Indiana, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Tennessee, Utah, Virginia, and Washington. Although these states mandate statewide code adoption, California allows minimal local code amendments, while Kentucky and Tennessee allow local jurisdictions to alter the adopted code. Additionally, the included states have altered versions of the codes that they adopt. For example, Louisiana and New York removed a requirement in the 2015 IRC requiring one foot of freeboard (additional elevation above the base flood elevation) in flood zones, North Carolina has altered wind design maps that allow homes to be built to resist lower wind speeds than required in the IRC, and Kentucky and South Carolina have weakened seismic requirements in their adoption. The states included in each step of the analysis differ based on available data and due to adoption timelines. Table 1 shows the enactment dates of each code in each state that we study.

We limit the sample to houses sold in 2011 or later to avoid bias from the global financial crisis from 2007 through 2010. This sample of states and years allows us to measure correlations between building code adoption and house prices for the 2009, 2012, 2015, and 2018 editions of the IRC. There is a lag of up to three years in reporting home construction and home sales in commercial data sources. Therefore, we could not analyze subsequent IRC editions due to data limitations.

We apply several screens to the data to address the inconsistencies in municipal tax and housing transaction records. Many observations would introduce bias to the analysis because they represent less-than-arms-length transactions or missing data. For example, when a house changes hands in estate probate or divorce proceedings, the price listed may not represent the value of the house. We discard observations below the fifth percentile of price in each state to avoid such bias. We also discard observations above the ninety-fifth percentile of price to avoid influential observations.[24] Further, we drop houses with more than six bedrooms or six bathrooms, and houses on more than one acre of land because these are not representative of typical homes,

[24]Screening at the first and ninety-nineth percentiles left many influential observations in the sample.

Table 1: Code Enactment Dates

	IRC Edition			
State	2009	2012	2015	2018
California		1/1/2014	1/1/2017	1/1/2020
Florida	3/15/2012	6/30/2015	1/1/2018	
Georgia		1/1/2014		1/1/2020
Indiana				1/1/2020
Kentucky		1/1/2014	1/1/2019	
Louisiana		1/1/2015	2/1/2018	
Maryland		7/1/2012	1/1/2016	
Massachusetts			10/20/2017	
Michigan		10/9/2014	4/20/2017	
Minnesota		1/24/2015		3/31/2020
Nebraska		5/29/2015		9/1/2019
New Hampshire			9/15/2019	
New Jersey			9/21/2015	9/3/2019
New Mexico			11/15/2016	
New York			10/3/2016	5/12/2020
North Carolina			1/1/2019	
Ohio	1/1/2013			7/1/2019
Oklahoma			11/1/2016	
Oregon	10/1/2014		1/1/2018	
Pennsylvania			10/1/2018	
Rhode Island			10/1/2018	
South Carolina		7/1/2013	7/1/2016	1/1/2020
Tennessee				7/16/2020
Utah			7/1/2016	
Virginia		7/14/2014	9/4/2019	
Washington			7/1/2016	

including those most sensitive to affordability considerations.[25] Finally, we discard observations of houses that are smaller than 500 and larger than 5,000 square feet of living space. Each of these screens improves the fit of the models. Where possible, we also screen for houses based on age to increase the similarity of houses in the sample and to ensure robust comparisons.

Table 2 shows descriptive statistics for all of the states and years in our analysis. A separate table describing the sample used for each test appears in the Appendix. However, Table 1 provides an overall description of the data we use.

2.2. Regression analysis

We employ a multiple regression model, which explains the variation in a dependent variable with the variation in multiple independent variables. The dependent variable in our model is the sale price of houses. We take the natural log of sale price because all models we tested achieve a better fit with the logged price. The logged variable also allows us to interpret coefficients as percentage changes.

Our independent variable of interest is an indicator variable equal to one in years when the building code edition we are testing is in effect and is otherwise equal to zero. We limit the sample to two years before and two years after the code enactments. This sample screen minimizes bias by limiting the time in which houses are sold. For example, California followed the 2009 IRC from 1/1/2011 until 12/31/2013. They followed the 2012 IRC from 1/1/2014 until 12/31/2016. In this case, the sample would include the years 2012 through 2015, and the 2012 IRC variable would be equal to zero in 2012 and 2013, and equal to one in 2014 and 2015. The coefficient estimate for the IRC 2012 variable will capture the average correlation between the change in building code and house prices.

The other independent variables in our model serve as control variables to approach an "all-else-equal" comparison. They include other factors that are likely to affect the price of houses. First, we control for the size of the house in square feet in the variable *House Size*. Like the dependent variable, this variable fits the data better in log form. Next, we control for the size of the lot on which a house sits. *Lot Size* is

[25] Results are not sensitive to applying this cutoff at five or seven. Six bedrooms was the 99th percentile of the sample, and it seemed like a natural break in the distribution. Some observations were as high as 43 bedrooms, which is likely a reporting error (intending to report four beds and three baths). Single-family houses with more than six bedrooms are quite rare.

Table 2: Summary Statistics

Variable	Average	St. Dev.	Minimum	Maximum
Price	332,883	195,663	50,000	1,615,000
House Size	2,318	734	500	5,000
Age	3.211	3.573	0	10
Lot Size Unknown	0.114	0.317	0	1
Lot Size	0.224	0.195	0	1
Bedrooms	2.976	1.550	0	6
Bedrooms Unknown	0.174	0.379	0	1
Baths per Bed	0.644	0.367	0	6
Baths/Bed Unknown	0.179	0.384	0	1
Sold in Q1	0.210	0.407	0	1
Sold in Q2	0.271	0.444	0	1
Sold in Q3	0.272	0.445	0	1
Sold in Q4	0.247	0.431	0	1
Year Built	2013	5	2001	2022
Year Sold	2016	3	2010	2022
Sample size (N= 2,715,607)				

Price is the sale price of houses. We use the log of *Price* in the regression models *Age* is the difference between the year a house sold and the year it was built. *House Size* is the size of a house in square feet. We use the log of *House Size* in the regression models. *Lot Size* is the size of a lot in acres. *Bedrooms* is the number of bedrooms in a house. *Baths per Bed* is the is the number of bedrooms divided by the number of bathrooms. Variables followed by "unknown" are indicator variables equal to one if the variable is not reported in the data. *Sold in Q1-4* are indicator variables equal to one if a house sold in each quarter of the year. *Year Built* is the year a house was built. *Year Sold* is the year a house sold.

the size of the lot in acres. We control for the number of bedrooms and bathrooms in a house using two variables, *Bedrooms* and *Baths per Bed*. *Bedrooms* is the number of bedrooms in a house. *Baths per Bed* is the ratio of bathrooms to bedrooms. The ratio gives the model a better fit because the number of bathrooms is highly correlated with the number of bedrooms, whereas the ratio measures the number of bathrooms relative to the number of bedrooms. In addition to measures of lot size, bedrooms, and bathrooms, we also include indicator variables equal to one when each of these variables is equal to zero. Because single-family houses do not exist without lots, bedrooms, and bathrooms, we assume that observations of zero for any of these variables indicate that the data are missing or unknown. Inclusion of the unknown indicators gives our model a better fit, suggesting that unknown observations may be systematically different than known observations.

Next, we control for the age of the house. *Age* is equal to the number of years between construction and sale of the house. As we explain below, *Age* serves slightly different purposes depending on the sample specification employed for each model. We also control for the year and quarter when a house is sold with two series of indicator variables, the first is equal to one for each year included in the sample, and zero otherwise, and the second is equal to one for each quarter of the year, and zero otherwise. These controls are important because house prices change annually and quarterly due to economic factors. Without these controls the analysis would have omitted variable bias. Finally, we include ZIP Code fixed effects to control for the location of houses. The ZIP Code fixed effects average out the location effects of each ZIP Code in our sample. The ZIP Code provides a better fit than alternative location variables, including county, city, census block, and census tract.

The regression model takes the following form:

$$\begin{aligned} Price_{t,z} = \beta_0 + \beta_1 House\ Size + \beta_2 Lot\ Size + \beta_3 Bedrooms + \beta_4 Baths\ per\ Bed \\ + \beta_5 Age + \sum_q^Q \beta_Q Quarter + \varepsilon_{tz} + \sum_t^T \beta_t Year\ Sold + \sum_z^Z \beta_z ZIP\ Code + \varepsilon_{tz}, \end{aligned} \tag{1}$$

Where βs are coefficients, q indexes quarters in which houses were sold, t indexes years in which houses were sold, z indexes ZIP Codes, and ε_{tz} is an error term clustered at the ZIP Code level to mitigate serial correlation and multicollinearity.

In the first model specification, we restrict the sample to houses sold for the first time within two years of when they are built. Returning to the California IRC 2009 example, we include only the houses that were built and sold in the years 2012 through 2015. The "treated" group would be the houses built and sold in 2014 and 2015. Because the model includes sale-year fixed effects, the comparison being made is between the treated houses and the houses built in 2012 and 2013 but sold in 2014 or 2015 (i.e., the control group). In this example, there were 30,536 observations in the treated group, and 3,962 observations in the control group. The ratio of treated to control observations is similar for all the states and years in which we use this model.[26]

We include additional observations beyond the treatment and control groups to enhance the validity of our estimates. For example, in the sample testing California IRC 2009, we include two years before the code enactment (2012 and 2013), even though the treatment and control groups are both contained in the two subsequent years. These observations help pin down the coefficients on control variables such as house characteristics, location fixed effects, and time trends, thereby reducing statistical noise in the estimation. This approach is particularly crucial when demonstrating null results, as precision becomes paramount to distinguish true null effects from imprecise estimates that fail to reject the null hypothesis due to large standard errors.

The age variable in this model is restricted to zero, one, or two. Houses older than two years are dropped from the sample. Age controls for any potential difference in sale price between houses that have been on the market for less than one year, compared to those that sell after up to two years.

The first model specification works well when new code editions are adopted on January 1 of a given year. However, when codes are adopted mid-year, we cannot assign houses built in that year to either code regime. If we exclude the adoption year, the control group sample becomes unreasonably small (less than 1% of the treated sample), and it only compares houses built and sold within one year to houses that have been on the market for more than one year. The strength of this specification is that it allows comparison of the most similar houses, those that were built and sold for the first time as new houses. The weakness is that it can only be used for 15 of the

[26] We also note that there is uncertainty in the definition of the year built in our data (NIBS, 2019). We address this issue with robustness tests in Section 3 below.

45 code adoptions we identify in states requiring uniform statewide code adoption.

Figure 1 shows the coefficient estimates and 95% confidence intervals for the code-adoption variables in 10 states and 15 code editions that we test with the first model. Details of the full regression results and diagnostics appear in the Appendix. If the confidence interval crosses the zero line, the coefficient estimate is not statistically different from zero, indicating that codes statistically do not influence housing prices. This is the case in nine out of fifteen tests. In two tests, California's adoption of the 2012 IRC and 2018 IRC, the coefficient estimates are significant and negative, suggesting that the average house price decreased when the new code was adopted. The coefficient estimate is -2.1% for the 2012 IRC and -1.1% for the 2018 IRC. Three coefficient estimates are positive and significant, suggesting that average prices increased when the new code editions were adopted. In Kentucky (2012 IRC, 5.4%), Maryland (2015 IRC, 1.8%), and Oregon (2015 IRC, 3%), the code changes are associated with price increases.[27]

We use the seemingly unrelated regression (SUR) procedure to estimate a Z-test of the hypothesis that the sum of all the *Code Adoption* coefficient estimates is equal to zero. The sum of the coefficient estimates is 0.138 and the clustered standard error is 0.488, indicating that the sum of the estimates is not statistically different from zero.[28] We omit this result from Figure 2 because including it reduces the level of precision we can show for individual code enactments.

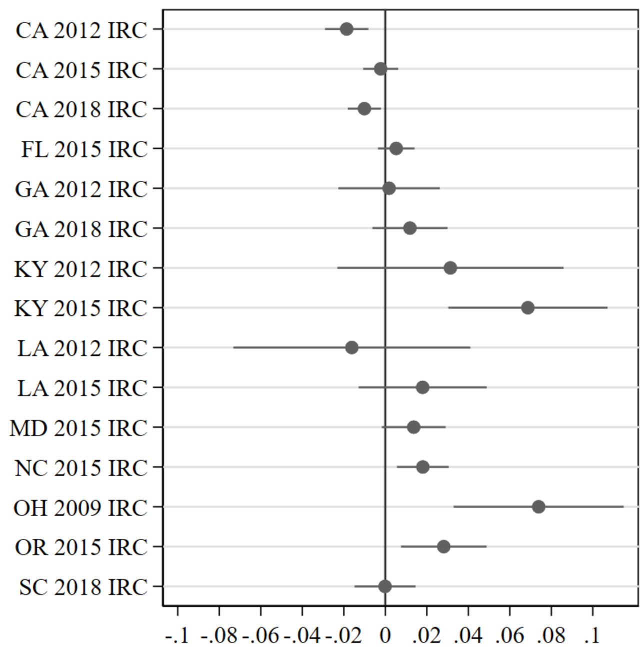
The sample of home sales and code adoptions does not facilitate estimation of a causal-analysis model. Therefore, we cannot say the building code adoptions caused the decreases or the increases in average home sale price. However, we do not observe large increases in home sale prices around the adoption of new IRC editions, even after controlling for house size, lot size, bedrooms, bathrooms, location, year sold, quarter sold, and age.

In summary, fluctuations in new house prices following adoptions of new IRC editions are not statistically different from zero. In a few instances, it appears that house prices increase or decrease slightly following adoptions of new IRC editions. Considered in total across years and locations, the effects of building codes on new house prices are roughly a wash.

[27]The p-value for the 1.3% coefficient estimate in North Carolina is 0.055.

[28]Standard errors are clustered at the ZIP Code level.

Figure 1: Regression Results Using the New-Construction Sample



Notes: The coefficients are estimated separately for each state and code edition and summarized here for parsimony. The regression model fits the natural log of sale price to a variable equal to one in years when the new code edition is effective, while controlling for measures of age, house size, lot size, bedrooms, bathrooms per bedroom, quarter sold, year sold, and location. Complete tables and discussion of results and regression diagnostics appear in the Appendix.

Next, we broaden the sample selection criteria and estimate a second specification of the regression model so that we can test more instances of code adoption. In this specification, we include houses up to 10 years old.[29] This allows us to test the effects of building code editions that were adopted mid-year. Using this test, we can expand our set of codes from 15 to 45. Because the houses in the sample are less similar, we rely more on the control variables in the regression model to approach an "all-else-equal" comparison. However, the increased sample size gives the model more power to control for observed factors.

In this model, the treated group is the same as in the previous model. It includes houses built and sold in the two years following code adoption. In the California 2012 IRC example, the treated group includes the houses built and sold in 2014 and 2015. The control group includes houses built up to 10 years before the adoption of the code and sold after the code adoption. By including sales of existing houses, we increase the size of the control group from about 4,000 in the first model to about 50,000 in the second model.

The increase in the sample size is most important in years when a code edition is adopted mid-year. For example, Florida adopted the 2012 IRC on June 30, 2015. Because the year built data is only available in whole years, we cannot identify the code under which houses were built in 2015. If we drop houses built in 2015 using the first model, the control group includes only 970 observations, compared to about 79,000 observations in the treatment group. Although the model would still compare the two samples, it is difficult to argue that the control group is representative of the treatment group, but for the treatment. By expanding the control group to include houses up to 10 years old, the number of observations grows from 970 to 47,130. The control group also becomes more representative of the treatment group. The treatment group includes houses in 691 ZIP Codes. The smaller control group (970 observations) includes houses in 289 of the 691 ZIP Codes in the treatment group. However, the larger control group (47,130 observations) includes houses in 672 of the 691 ZIP Codes in the treatment group.

Increasing the control group also provides a test of a more realistic scenario than the sample of only new houses. When we include the resale of existing houses, potential homebuyers in our model can choose between new houses and existing houses

[29] Our conclusions do not change if we include only houses up to five years old, or if we include houses up to fifteen years old.

when purchasing a home. Instead of testing if building code adoptions are correlated with the cost of new houses, we test the hypothesis that building code adoptions change the cost of houses.

Coefficient estimates and 95% confidence intervals from the second model specification appear in Figure 2. Using the second model specification we can test for price changes following 45 adoptions of four editions of the IRC (2009, 2012, 2015, and 2018) in 26 states.

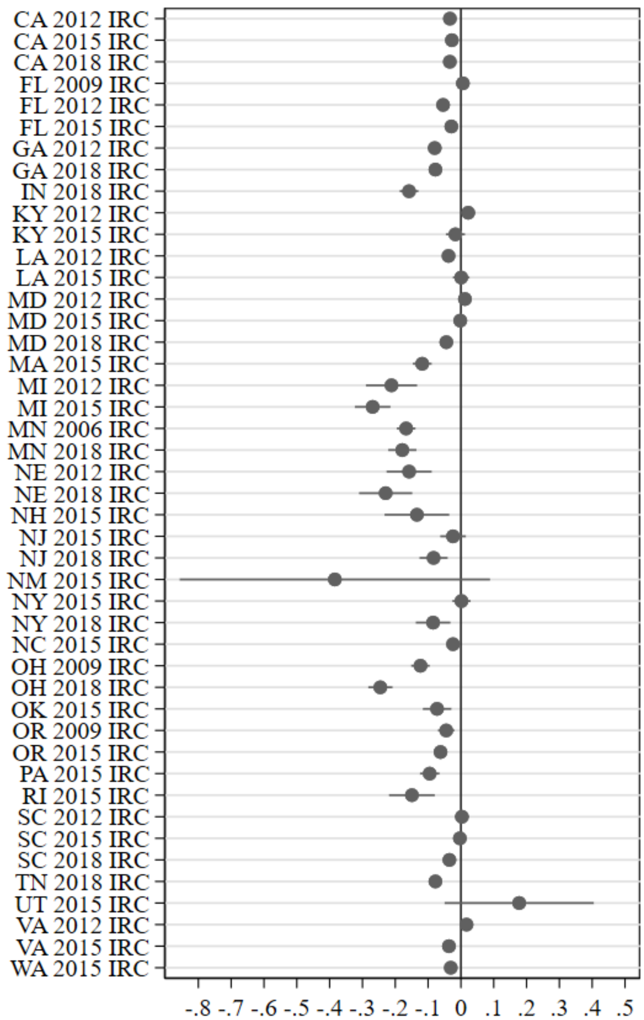
The second model appears to fit the data as well as or better than the first model. For each of the code adoptions in both figures, the confidence intervals in Figure 2 are similar to or smaller than those in Figure 1. Of the coefficient estimates in Figure 2, 32 are negative and significant, 2 are positive and significant, with the remaining 11 statistically indistinguishable from zero. Results for several code adoptions are different in Model 1 than in Model 2. In California (2015 IRC), Florida (2015 IRC), Georgia (2012 IRC and 2018 IRC), Louisiana (2012 IRC), North Carolina (2015 IRC), Ohio (2009 IRC), and South Carolina (2018 IRC) the coefficient estimate was insignificant in Model 1 and significantly negative in Model 2.

In the 2 instances where a building code update is followed by an increase in house prices, the smaller is a 1.8% increase when Virginia adopted the 2012 IRC, and the larger is a 2.4% increase when Kentucky adopted the 2012 IRC. Similar to results of the first model, these findings could be attributed to local conditions.

In 43 of the 45 code adoptions we study, the average price of houses does not increase when the building code changes. In 32 instances, the price of houses decreases significantly when the building code changes. The decreases range from -26.7% when Michigan adopted the 2015 IRC, to -2.5% when North Carolina adopted the 2015 IRC. In the remaining 11 instances, house prices do not increase or decrease significantly when new building code editions are adopted.

We use an SUR procedure to calculate a Z-test of the hypothesis that the sum of the *Code Adoption* coefficient estimates is greater than zero. The sum of the *Code Adoption* coefficient estimates is -3.47 and the clustered standard error from the Z-test is 0.76. Thus, we can reject the null hypothesis that the sum of the coefficients is positive. The negative coefficient is statistically significant at less than the 0.001 level indicating a less than 1-in-1,000 probability of randomly observing this outcome. This result is omitted from Figure 2 because its inclusion reduces the level of precision

Figure 2: Regression Results using the Expanded Sample



Notes: The coefficients are estimated separately for each state and code edition and summarized here for parsimony. The regression model fits the natural log of sale price to a variable equal to one in years when the new code edition is effective, while controlling for measures of age, house size, lot size, bedrooms, bathrooms per bedroom, quarter sold, year sold, and location. Complete tables of results and regression diagnostics appear in the Appendix.

that can be shown for individual code enactments in the figure.

In summary, when we expand the sample to include resales of existing houses we are able to test for correlations between building code adoptions and house prices in a larger set of states and years. Although we cannot claim a causal relationship between code adoptions and house prices, we find that prices can increase or decrease following code adoptions, and that the decreases are more frequent and larger than the increases.

3. ROBUSTNESS TESTS FOR CODE APPLICATION DATES

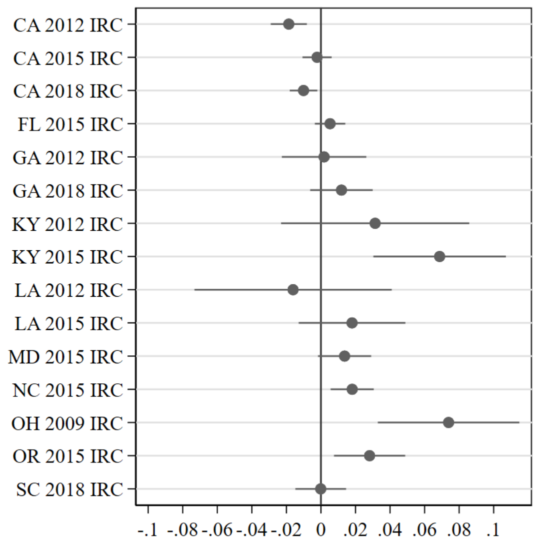
NIBS (2019) notes that the year-built data in the database "have considerable uncertainty because the year-built date could represent an initial building permit at the design stage, the final inspection of the completed building when the certificate of occupancy is issued, or something in between." U.S. Census data indicate that the average duration between a permit date and completion of a home is eight months. Therefore, depending on how year built is defined in each reporting municipality and the distribution of completion times across states, our primary specification could be incorrect for some percentage of the observations.

We use two sample and treatment specifications to address the potential uncertainty in application of new code editions. First, we shift the code enactment date and the sample period forward one year in Model 1. If it is more common to assign year built based on the date a house is completed than on the date a permit is filed, then this specification will be more accurate than the original specification. Figure 3 compares the coefficient estimates and 95% confidence intervals from using the original and alternative specifications for Model 1.

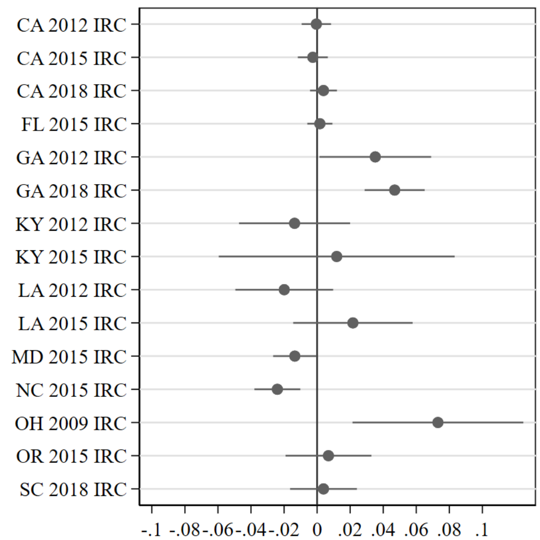
Results in Figure 3 are quantitatively different from those in Figure 1, but they support the same general conclusion. When we adjust the code-adoption dates and the sample window to accommodate the assumption that year built is defined as the year when the house was finished, rather than the year when the building permit was approved, we again find that three code adoptions are positively correlated with house prices and two code adoptions are negatively correlated with house prices.

In the robustness model, Geogia (2012 IRC, 3.5% and 2018 IRC, 4.7%) and Ohio (2009 IRC, 7.3%) are positively correlated with prices, and Maryland (2015 IRC,

Figure 3: Regression Results Using the New-Construction Sample and Alternative Construction Date



(a) Original Model 1



(b) Robustness Model 1

•Coefficient Estimate — 95% Confidence Interval

Notes: The coefficients are estimated separately for each state and code edition and summarized here for parsimony. The regression model fits the natural log of sale price to a variable equal to one in years when the new code edition is effective, while controlling for measures of age, house size, lot size, bedrooms, bathrooms per bedroom, quarter sold, year sold, and location. Complete tables of results and regression diagnostics appear in the Appendix.

-1.3%) and North Carolina (2015 IRC, -2.4%) are negatively correlated with prices. Results from the SUR procedure are a coefficient estimate of 0.131 and a standard error of 0.389, indicating that the overall average correlation between building code changes and houses prices is not statistically different from zero (p-value=0.737). This is not surprising given that most of the coefficient estimates are insignificant. The important takeaway from this robustness test is that the definition of year built is important when estimating correlations between building codes and house prices.

Next, we employ a "doughnut" style sample specification as a robustness test for Model 2. We implement the same analysis as Model 2, but we move the building code edition enactment date forward one year, and we drop observations from the original enactment year. Using the California implementation of the 2012 IRC edition as an example, the official enactment date is 1/1/2014. We drop all houses built in 2014. We assume that the new IRC edition was applied to houses built in 2015 and 2016.

For code editions enacted mid-year, we exclude observations from the enactment year and the following year. This conservative process should remove nearly all the houses built under the previous code from the treatment sample.

Figure 4 compares the coefficient estimates and confidence intervals from estimating Model 2 using the original specification and the "doughnut" specification. The coefficients remain negative on average, with only Maryland (2012 IRC, 2.7%) demonstrating a positive coefficient. Results from an SUR test find the average correlation in the robustness model is -3.59 with a standard error of 1.08. Like those of the original Model 2, these estimates reject the null hypothesis that building codes are positively correlated with house prices at less than the 1-in-1,000 level.

Results for the robustness specification of Model 2 are very similar to those in the original specification. The estimates follow similar patterns and magnitudes. This suggests that the specification and sample window used in Model 2 were more robust to the definition of year built. Nonetheless, the small but significant differences indicate the importance of robustness testing around this issue.

4. CROSS-BORDER ANALYSIS

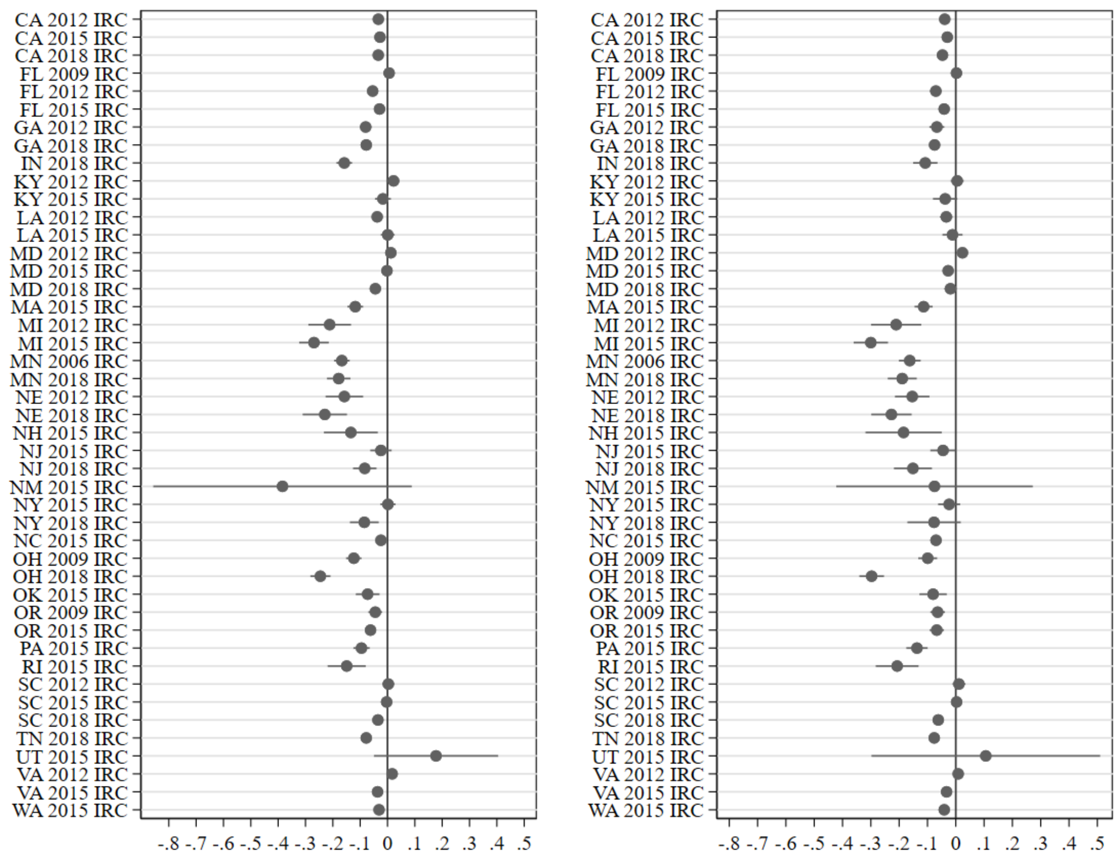
In this section we test for changes in building activity around changes in code adoption. Specifically, we compare the number of new houses per ZIP Code built and sold per year in the border counties of North Carolina and South Carolina. If building code enactment increases the cost of construction above what the market will bear, contractors in border counties could potentially focus their efforts across the nearby state line. In this case, we would expect the number of houses built to increase in one state and decrease in the other.

We use a regression model to compare the average number of houses built per ZIP Code in each state. First, we sum the number of houses built in each year and ZIP Code in the bordering counties. Then we estimate a regression model as follows:

$$Houses\ Built_{tz} = \alpha + \sum_{t=2010}^{2022} \beta_t t + \sum_{z=1}^{Z} \gamma_z z + \varepsilon_{tz}$$

Where $\boldsymbol{t}$ are year fixed effects, $\boldsymbol{z}$ are ZIP Code fixed effects, α, β, and γ are coefficient estimates, and ε is an error term. The coefficient estimates from this

Figure 4: Regression Results Using the Expanded Sample and "Doughnut" Specification



(a) Original Model 2 **(b)** Robustness Model 2

•Coefficient Estimate — 95% Confidence Interval

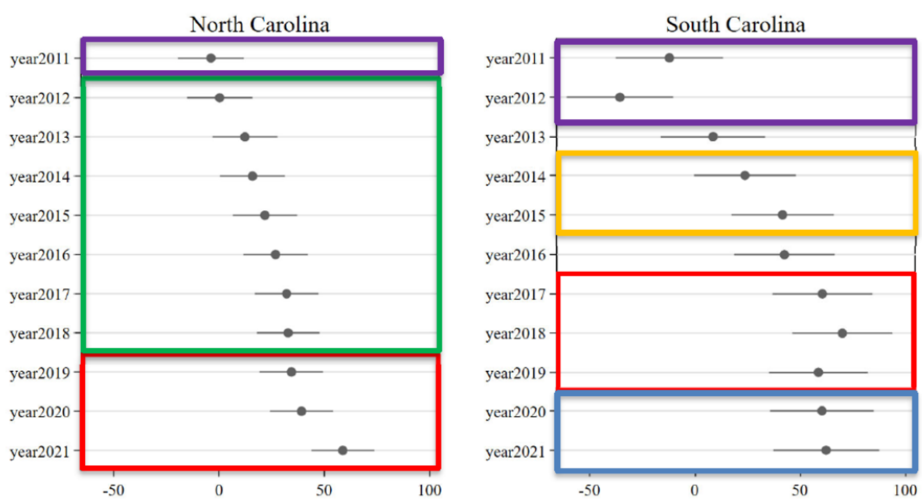
Notes: The coefficients are estimated separately for each state and code edition and summarized here for parsimony. The regression model fits the natural log of sale price to a variable equal to one in years when the new code edition is effective, while controlling for measures of age, house size, lot size, bedrooms, bathrooms per bedroom, quarter sold, year sold, and location. Complete tables and discussion of results and regression diagnostics appear in the Appendix.

model represent the average number of houses built per ZIP Code in each year. The coefficient estimates and standard errors tell us if the number of houses built changes significantly over time.

The border counties with sufficient building activity in North Carolina include Anson, Brunswick, Cleveland, Columbus, Gaston, Henderson, Jackson, Mecklenburg, Polk, Robeson, Scotland, Transylvania, and Union. The border counties with sufficient building activity in South Carolina include Cherokee, Horry, Lancaster, Oconee, Pickens, Spartanburg, and York.

Figure 5 shows the coefficient estimates and 95% confidence intervals from the regression equation. The coefficient estimates represent the average number of houses built in each year. By observing the difference between the coefficients for each consecutive year we see that the average number of houses built per ZIP Code in North Carolina increased significantly (at the 5% level) from 2020 to 2021. In South Carolina, the average number of houses built per ZIP Code increased significantly (at the 5% level) from 2012 to 2013.

Figure 5: Border County Building Activity in North Carolina and South Carolina



Notes: A purple box indicates years when the 2006 IRC was in effect. A green box indicates years when the 2009 IRC was in effect. A yellow box indicates that the 2012 IRC was in effect. A red box indicates that the 2015 IRC was in effect. A blue box indicates that the 2018 IRC was in effect. In the South Carolina figure, 2013 and 2016 are excluded from the boxes because building codes were adopted mid-year. South Carolina has consistently had building codes in effect.

Neither of the significant changes in building volume coincides with a new code adoption, adding to the evidence that builders do not cross state lines to avoid updated building codes.

5. CONCLUSIONS

Building codes are an effective tool that policymakers can use to improve life safety, economic resilience in the wake of the next hurricane, wildfire, earthquake, flood, or tornado and to improve the energy efficiency of homes, which represent the largest category of buildings in the United States. Building code adoption is often met with resistance because of the prevailing sentiment that they often drive broad increases in the price of homes and are a critical factor that exacerbates the housing affordability crisis already felt across the country.

Although the expected benefits of building codes have been studied extensively and are widely known (NIBS, 2019), the scientific literature has less to say about the relationship between building codes and housing affordability. A series of reports by the Home Innovation Research Labs estimates changes in the costs of materials and labor associated with some changes in the IRC and IECC; however, these estimates do not consider market forces and trends affecting the supply of, and demand for single-family housing.

Regardless of the cost of construction, a house is only worth what someone will pay for it. In many cases, it is not possible to determine if a house was built to code after construction or repairs are complete. Because buyers cannot visibly see many of the components and effects of building codes at the time of buying, such as potential reductions in damage during natural hazards or reductions in energy consumption, it is common for decisions to be driven by visible components such as interior finishes. Given the choices available to buyers (i.e., buying a new house versus renting or buying an existing house), we believe the relationship between code adoption and house prices is best addressed empirically. This report begins to fill that void in the scientific literature.

We use data on home sales and code adoption dates in 26 states from 2011 to 2022 to estimate correlations between code adoption and house prices, while controlling for location, time trends, and observable house characteristics. Our analysis includes robustness testing for ambiguity in the definition year built in our data. We also test for effects of code adoption on building activity by comparing the average number of houses built per ZIP Code in border counties of adjacent states.

We test for statistically significant price changes for new houses following 15 code adoptions in 10 states. We find three instances where prices increased in a statistically significant manner. The states and code editions depend on the definition of year built in our database. The increases range from 5.4% when Kentucky adopted the 2012 IRC to 1.7% when Maryland adopted the 2015 IRC We also find two instances where new-house prices decrease significantly following code adoption. The decreases range from 2.1% to 1.1%, when California adopted the 2012 and 2018 IRC, respectively.

We add resales of existing houses to our sample to expand the states and code adoptions we can test for price changes. Using data for houses up to ten years old in the comparison sample, we test for correlations between code adoption and the cost of houses following forty-five adoptions of four editions of the IRC (2009, 2012, 2015, and 2018) in twenty-six states.

In 43 of the 45 code adoptions we study, the average price of houses does not increase when the building code changes. In 32 instances, the price of houses decreases significantly when the building code changes. The decreases range from -26.7% when Michigan adopted the 2015 IRC, to -2.5% when North Carolina adopted the 2015 IRC. In the remaining 11 instances, house prices do not increase or decrease significantly when new building code editions are adopted.

In the 2 instances where a building code update is followed by an average increase in house prices, the smaller is a 1.8% increase when Virginia adopted the 2012 IRC, and the larger is a 2.4% increase when Kentucky adopted the 2012 IRC.

In summary, we find that in most instances house prices do not change following the adoption of a new edition of the IRC. In cases where we find statistically significant changes, they are more likely to be price decreases than price increases. Moreover, the decreases are larger than the increases on average. Although we cannot claim a causal relationship between code adoptions and changes in house prices, the correlations we observe do not present a clear pattern of price changes following code adoptions. These price changes could be driven by local effects of major disasters or other local market factors that are not captured by our methodology.

Results from the border county comparisons of North Carolina and South Carolina are inconsistent with the idea that contractors might choose to build in under-regulated markets when code changes increase construction costs. We find only a handful of consecutive years in which the average number of houses built per ZIP

Code changes significantly. None of the significant changes in new home volume coincide with changes in building codes.

In conclusion, the results shown here do not support any notions that home prices increase, or new home supply is limited or displaced when states adopt new building codes. In addition, there is no evidence to support that code adoption has any detectable effect on large scale housing prices either by state or at the national level. Moreover, given the known benefits of building code adoption for homeowners and society, it seems prudent for states and local jurisdictions to continue adopting building codes as they are made available.