Market Effects of IECC 2009 Adoption in Wisconsin

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ABSTRACT

Affordability is often at the center of conversations around development and adoption of building energy codes, especially for residential buildings where there are concerns that increased energy efficiency will lead to increased home prices that will in turn be passed directly to home-buyers. We used a Difference-in-Differences approach to evaluate home transaction data, building permit data, and tax assessment data to estimate causal effects of energy code adoption on the prices and quantities of new homes. The 2016 adoption of IECC 2009 in Wisconsin, the most significant code update that Wisconsin has undergone since 2009, did not show statistically significant market effects among new single-family homes. Point estimates suggest a slight decrease in home prices and permits for new homes, as well as heterogeneous price effects across income quartiles; however, the precision of these estimates is not sufficient to infer causal effects. While affordability should remain a priority in the development and adoption of new energy codes, this work suggests that there are other, larger market forces which greatly outweigh the effect of a newer, more stringent energy code.

Introduction

Buildings consume nearly 37% of the primary energy in the United States (EIA 2026). Since 1975, building energy codes have been developed and adopted across the United States as a way to reduce the energy consumption of new buildings and those which undergo major renovations. It is estimated that residential buildings built to meet the standard of the 2024 International Energy Conservation Code (IECC) use roughly half of the energy of homes built to the 1975 standard from the American Society of Heating, Refrigerating and Air-Conditioning Engineers (ASHRAE)(PNNL 2026). Opponents of more stringent energy codes cite affordability, claiming that those changes will increase home prices, despite the fact that the United States Department of Energy (DOE) publishes cost-effectiveness projections for each new version of IECC (Salcido et al. 2024). This work builds on the existing literature which investigates the price and quantity impacts of energy code adoption on new single-family homes. We look at Wisconsin's 2016 adoption of IECC 2009 and compare it against a control group of Midwestern states which had already adopted IECC 2009 or were using an amended version of IECC which was similar to IECC 2009. We analyze effects on the prices and quantities of new homes and the major characteristics of new homes (size, bedroom count, bathroom count), as well as whether or not there were heterogeneous price effects between ZIP-codes in different income quartiles.

Background

Effective January 1, 2016, Wisconsin adopted IECC 2009 for new residential construction permits (Wisconsin DSPS 2026). Compared to IECC 2006 — the energy code which was in effect in Wisconsin before the adoption of IECC 2009 on January 1, 2016 — IECC

2009 requires more insulation in portions of the envelope as well as reduced air infiltration and duct leakage. The states used as controls for the study were selected because they are nearby states in the Midwest, they either did not undergo new energy code adoption during the study period, or they adopted amended codes which retained their energy efficiency to most resemble IECC 2009 (PNNL 2026).

In microeconomic theory, an increase in the cost of producing a good causes an upward shift in the supply curve, resulting in a higher equilibrium price (Mankiw 2024). In the context of housing markets, these prices can be broken down into the price of land and the price of the structure (in this case, a single-family home) which sits on the land. Between 1975 and 2006, the inflation-adjusted price of residential land nearly quadrupled, while the real price of structures increased by only 33%. This finding suggests that fluctuations in home prices are primarily driven by changes in the price of land rather than changes in the price of structures (Davis et al. 2007) which in turn implies that a change to the energy code which results in a marginal increase to the structure might not result in a significant change to the price of a home. Additional evidence suggests that it is regulation such as minimum lot sizes and density restrictions, not construction cost which drive high housing prices (Glaeser and Gyourko 2018). Previous work evaluating the impacts of energy codes includes evidence that energy codes do save energy (Jacobsen and Kotchen 2013; Kotchen 2017) and that homebuyers are attentive to future energy costs in United States markets (Myers 2019) and in England (Fuerst et al. 2015). In California, there is evidence that homebuyers are willing to pay a premium for homes labeled as "green" by a third party (Kahn and Kok 2014). Based on these findings, we might expect that – even if builders pass marginal costs of energy efficiency on to homebuyers – expected savings through reduced energy bills would convince homebuyers to pay more for a more energy efficient home. There is also evidence from California that energy codes can have distributional effects in the housing market in the form of changes to home characteristics (square footage and number of bedrooms) across income quintiles (Bruegge et al. 2019). Recent work in Massachusetts has also investigated the price and supply impacts of adopting the base energy code versus the more stringent stretch and specialized codes, finding that the most stringent energy code was associated with increased home prices and reduced home supply (Muzio et al. 2025). An earlier investigation in Illinois did not find evidence of a strong or direct correlation between the timing of energy code adoption and the number of single-family residential permits compared to peer states in the Midwest (Lindburg et al. 2024). This paper contributes new findings in the Midwest using a quasi-experimental design to investigate income heterogeneity impacts as well as simultaneous price and quantity effects.

Data

We paired housing transaction data (ATTOM Data 2025b) with historical tax assessment data (ATTOM Data 2025a) to build the dataset for the study. Assessor data contains homes characteristics including square footage, bedroom count, bathroom count, as well as the year the home was built, which we were able to match to the transaction data to find the first arm's-length sale after construction. We restricted the sample to the first transactions within 3 years of the year built and 67% of homes had their first transaction in the year of construction. We used building permit data (U.S. Census Bureau 2024) for quantity effects and ZIP-code median household income (U.S. Census Bureau 2026) from 2014 to investigate potential distributional effects in home prices. We applied a 0.2% and 99.8% winsorization on transaction prices within each state, replacing extreme values above and below those thresholds with the values at the thresholds to handle outliers. Descriptive statistics of the home data used in the price studies can be found in Table 1.

Table 1: Descriptive statistics of homes

Group	Period	N homes	Sale price, median [IQR]	Area (sq ft), median [IQR]	Bedrooms, mean (SD)	Bathrooms, mean (SD)
WI	Pre (2014-2016)	2,714	$259,900 [$177,233, $337,789]	2,780 [2,115, 3,604]	3.43 (0.73)	2.83 (0.74)
WI	Post (2017-2020)	3,856	$324,900 [$240,826 $409,900]	2,492 [1,848, 3,432]	3.31 (0.70)	2.74 (0.72)
Pooled controls	Pre (2014-2016)	60,121	$272,800 [$185,633, $379,000]	2,957 [2,142, 3,940]	3.45 (0.86)	3.04 (1.06)
Pooled controls	Post (2017-2020)	95,492	$292,700 [$210,000, $389,900]	2,400 [1,818, 3,223]	3.45 (0.87)	2.97 (1.18)

For descriptive statistics, missing values for area, bedroom count, and bathroom count were omitted from the calculations

Identification Strategy

We used an event-study difference-in-differences (DiD) (Angrist and Pischke 2008) framework to estimate the price effect of Wisconsin's 2016 adoption for new single-family homes, with Illinois, Indiana, Iowa, Michigan, and Ohio as controls. We refer to these states as "controls", but more precisely they are states which had already received the treatment before the analysis window of the study. Some of the control states adopted new residential building energy codes during the analysis period, however, they all contained weakening amendments which kept them closer to IECC 2009 in terms of energy efficiency than the nominal code version (PNNL 2026). Minnesota was excluded because they adopted a new code in 2015, and the staggered adoption would require more complex methods. Missouri was excluded because they did not have a state energy code at the time. Although Wisconsin adopted IECC 2009 in 2016, we assumed that homes built in 2017 were the first cohort of homes with full exposure to the new code because of the lag between permit application and construction. Because of the period of the analysis (2014-2020), this identifies the short-run compliance cost capitalization in home prices, but does not estimate the long-run equilibrium price effect. Before estimating, we detrended prices for each state by estimating a linear slope of log P during the pre-treatment period to remove state-specific linear trends. Each observation is a sale transaction i in ZIP-code z, state s, and year of construction cohort t. The primary specification is as follows:

$$\log P_{izst} = \mu_z + \mathbf{X}'_{\mathbf{it}}\gamma + \sum_{k=-3,\, k\neq -1}^{+3} \beta_k \left(\mathbf{1}_{\mathbf{relyear_{it}=k}} \cdot WI_i\right) + \varepsilon_{izst} \tag{1}$$

where:

- $\log P$ = detrended log first-sale price
- μ_z = ZIP-code fixed effect
- X'_{it} = home characteristics vector (log area, number of bedrooms, number of bathrooms, pool, fireplace, ZIP-level income at year built)
- β_k = event-study coefficient for cohort k
- Treatment indicator: $\mathrm{W}I_{i1} = [\mathrm{state}_i = \mathrm{WI}]$
- Running variable: $\mathrm{relyear}_{it} = \mathrm{YEARBUILT}_{it} - 2017$ (construction cohort relative to first full post-adoption year
- Reference year: $\mathrm{relyear} = -1$ (YEARBUILT 2016; last pre-adoption cohort, normalized to zero)
- Window: $k \in \{-3, -2, 0, +1, +2, +3\}$

The causal estimand is average treatment effect on the treated (ATT) which is the average price effect on Wisconsin homes built under IECC 2009 compared to the counterfactual of IECC 2006:

$$\widehat{ATT} = \frac{1}{4}\sum_{k=0}^{3} \widehat{\beta_k} \tag{2}$$

We implemented a full null restricted wild cluster bootstrap (WCB) to improve the p-value and confidence interval (CI) estimates given the small number of states (Cameron et al. 2008) using Webb weights (Mackinnon and Webb 2018) with 999 repetitions.

To address whether the price effects are uniform across income groups, we extended equation (1) to include income quartile interactions:

$$\log P_{iqzst} = \mu_z + \boldsymbol{X}'_{\boldsymbol{it}}\gamma + \sum_{q,k} \beta_{qk}\left(\mathbf{1}_{\boldsymbol{Q_i=q}} \cdot \mathbf{1}_{\boldsymbol{relyear_{it}=k}} \cdot WI_i\right) + \varepsilon_{iqzst} \tag{3}$$

where $q \in \{1,2,3,4\}$ and $k \in \{-3, -2, 0, +1, +2, +3\}$.

The per quartile post-treatment average effect is:

$$\widehat{ATT_q} = \frac{1}{4}\sum_{k=0}^{3} \widehat{\beta_{qk}} \tag{4}$$

We ran an additional event study using county-level permit data to estimate the effect of Wisconsin's 2016 adoption of IECC 2009 in new single-family home starts using the following specification:

$$\log(1 + permits_{ct}) = \alpha_c + \lambda_t + \sum_{k=-3,\, k\neq -1}^{+3} \beta_k\left(\mathbf{1}_{\boldsymbol{WI_{c=1}}} \cdot \mathbf{1}_{\boldsymbol{relyear_{ct}=k}}\right) + \varepsilon_{ct} \tag{5}$$

where α_c = county fixed-effects and λ_t = year fixed-effects. Similar to the price study above, we implemented a WCB method because of the small number of state-clusters.

Results

As shown in Figure 1, we found that Wisconsin homes had a post-treatment average price effect of −0.050 log points ≈ −4.9% price change compared to the cohort of control states with a WCB 95% CI of [−11.8%, +2.6%], WCB p = 0.252. The wide confidence interval is a result of the small number of clusters (states) and the large p value means that we cannot reject the null hypothesis that the adoption of IECC 2009 had no effect on the prices of new homes. In other words, we cannot infer that adopting IECC 2009 caused home prices to change in Wisconsin relative to the control states, but point estimates suggest that prices in Wisconsin fell relative to the control states after the adoption of IECC 2009. Home prices in all states rose during the study period. The pre-treatment parallel trends assumption fails since home prices in Wisconsin rose slightly faster in Wisconsin during the pre-treatment period (relative year -2) than in the control states. Therefore, the post-treatment effect is positively biased, meaning that the true effect of observed lower home prices in Wisconsin could be even larger in magnitude, but this is suggestive rather than formally established. The estimate of −4.9% is under the minimum detectable effect (MDE = $q_{0.975} \times \widehat{SE_{\text{post-avg}}}$) from the WCB of ≈7.9%, which reflects the limited precision of the model. For all figures, conventional asymptotic 95% confidence intervals are for reference, but WCB confidence intervals are used for inference because of the small number of state-clusters.

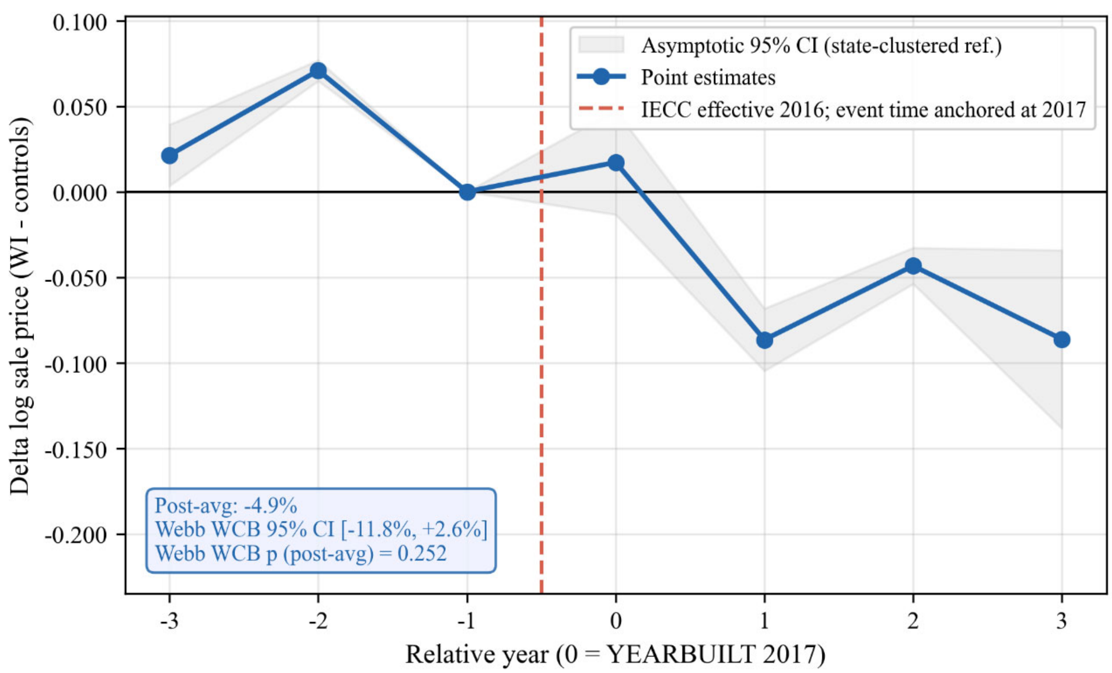

Figure 1: Price effect of IECC 2009 adoption in Wisconsin

We conducted a series of robustness checks to validate the primary specification. These include a wider window (2012-2022), the donut specification which drops 2016 and 2017 to avoid contamination issues of homes built in those years which may have been permitted before the 2016 code adoption, excluding the Milwaukee MSA, which may have distinct housing market dynamics, and removing home characteristic controls (area, bedroom count, bathroom count) to account for potential bad controls (Angrist and Pischke 2008). These home characteristics could have been bad controls if the composition of homes changed significantly post-treatment (e.g., homes got smaller, homes had fewer bedrooms). Point estimates for average treatment effects on treated of the robustness checks can be found in Table 2. The point estimates of all of the robustness checks move, but none yield p values low enough to reject the null hypothesis at conventional levels. The sign of the point estimate flipped for two of the robustness checks: the wider window and the exclusion of the Milwaukee MSA. The wider window extends from 2012 to 2022, so it is very likely that the sample was impacted by both the recovery from the 2008 financial crisis and the COVID-19 pandemic. The narrower, short-term window helps avoid the bulk of these effects. Removing the Milwaukee MSA removes nearly one third of all samples in the treated state and cannot precisely attribute the primary estimate to Milwaukee MSA alone. The donut estimate drops too many homes which are driving identification and therefore results in an unstable estimate. We also ran state-assignment placebos to test if we would get similar effects to those found in Wisconsin by running the same model 5 times with each of the control states as the treatment state instead of Wisconsin. Post-average prices were −21.6% (IL), +8.9% (IN), +3.2% (IA), +0.5% (MI), and +7.2% (OH). Wisconsin's result of −4.9% is not reproduced by treating the control states which can be interpreted as qualitative support that Wisconsin's estimate is not a mechanical by-product of the specification of the study alone.

Table 2: Robustness checks for the price effect event study

	N	$\widehat{\beta_{ATT}}$	WCB 95% CI	WCB p
Primary Specification	162,183	−0.050 (−4.9%)	[−11.8%, +2.6%]	0.252
Wider Window	267,584	+0.107 (+11.3%)	[−0.8%, +24.8%]	0.132
Donut	112,188	−0.345 (−29.2%)	[−67.4%, +53.9%]	0.253
Ex-Milwaukee MSA	159,662	+0.022 (+2.2%)	[−0.3%, +4.9%]	0.105
No Controls	161,709	−0.069 (−6.7%)	[−13.5%, +0.6%]	0.132

In addition, we ran a modified version of equation (1), replacing $\log P$ with each home characteristic to see whether there were compositional shifts in the homes. Directional shifts suggest that homes in Wisconsin might have gotten smaller and had fewer bedrooms relative to the homes in control group, but the WCB p values are high and the WCB CIs are wide and contain zero for all three characteristics, so these estimates do not have a causal interpretation. Note that Table 1 shows that homes in both groups got smaller on average, so it is possible that there was a change in the data coverage from ATTOM. Point estimates for average treatment effect on treated and WCB 95% confidence intervals for the home characteristics can be found in Table 3.

Table 3: Point estimates and WCB confidence intervals for home characteristics event study

	$\widehat{\beta_{ATT}}$	WCB 95% CI	WCB p
Log square feet	−0.135 (−12.6%)	[−23.8%, +0.3%]	0.12
Bedroom Count	−0.066	[−0.23, +0.10]	0.39
Bathroom Count	−0.006	[−0.016, +0.005]	0.25

For the quantity study using BPS data, we found that single-family permits in Wisconsin changed by −0.093 log points (−8.9%) after the 2016 adoption of IECC 2009 relative to the control states. However, the WCB joint pre-trend test fails and the WCB 95% confidence interval includes 0, so we cannot reject the null hypothesis that the adoption of IECC 2009 had no impact on single-family home permits in Wisconsin.

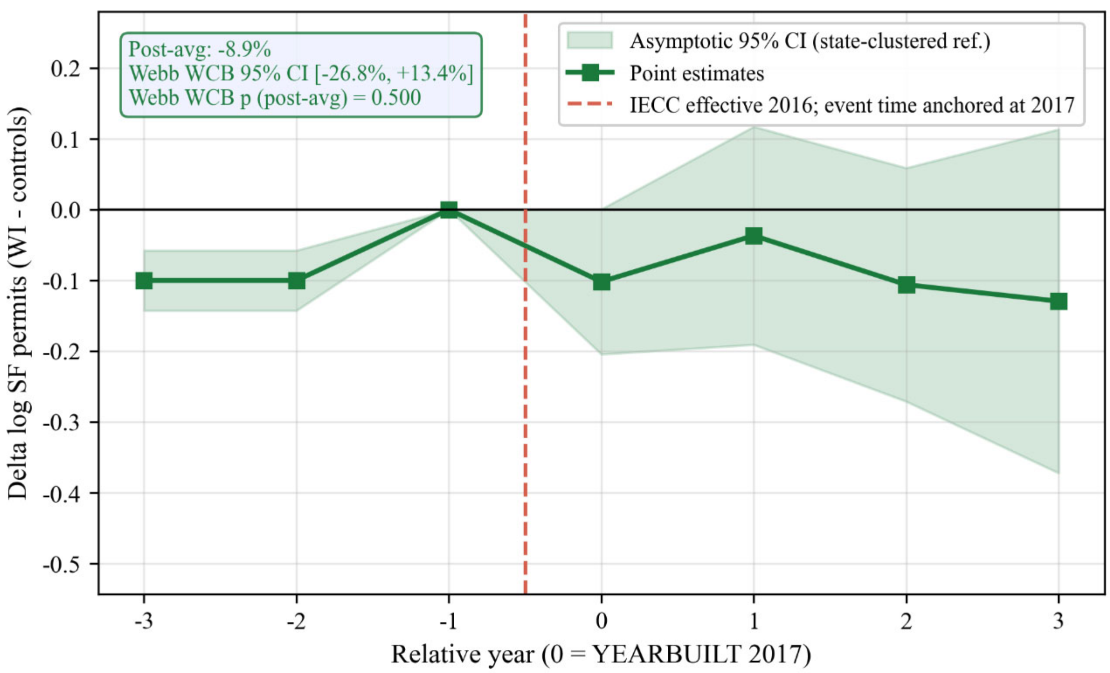

Figure 2: Quantity effect of IECC 2009 adoption in Wisconsin

We did not find significant income heterogeneity in price treatment effects. That is, wide confidence intervals and high p values produced by the WCB did not allow us to reject the null hypothesis that home prices did not change in different ways across income quartiles as the results of Wisconsin's adoption of IECC 2009. Point estimates show large, negative effects in the highest income quartiles. However, with only 6 state-clusters, there is not enough statistical power to reject the null hypothesis of no income heterogeneity at conventional levels.

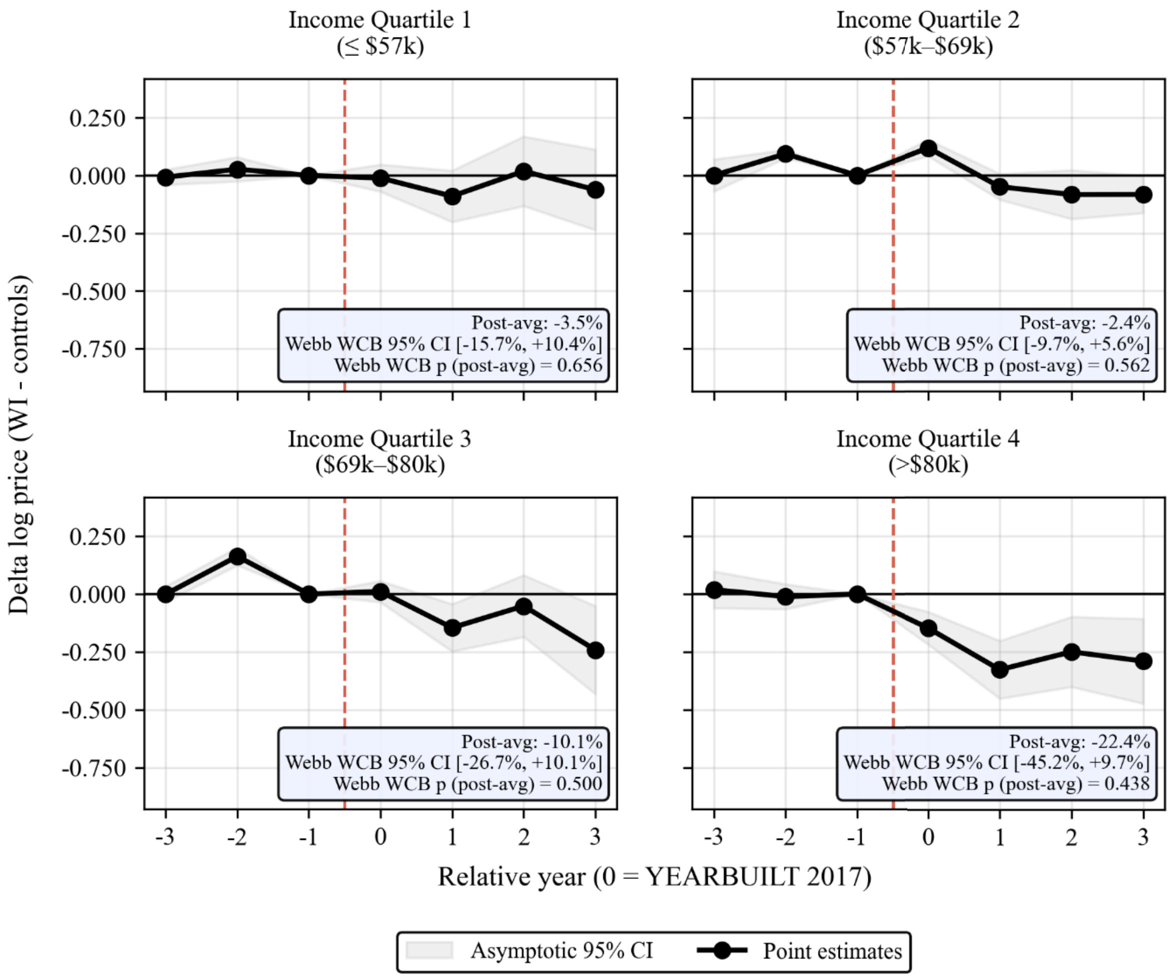

Figure 3: Heterogeneity of price effect of IECC 2009 adoption in Wisconsin

Discussion

We did not find a statistically significant price effect due to the adoption of IECC 2009 in the primary specification. Because home prices in Wisconsin were rising faster relative to the control states before the adoption of IECC 2009, the point estimates serve as a conservative estimate because we would expect them to be positively biased. These results are surprising given that there were in fact marginal construction cost increases for builders for increased insulation, air sealing, duct sealing, and other energy efficiency measures which were present in IECC 2009, but absent in IECC 2006. Estimates of these costs range from $525 to $2,336 depending on the climate zone, foundation type, and data source (NAHB 2012; Mendon et al. 2013). However, this range (0.2-1% for a home ~$260,000) is significantly below the MDE of ≈7.9%. One explanation for the seeming absence of these marginal price increases could be that – if buyers are not fully attentive to energy efficiency measures such as marginal increases in envelope insulation – builders might not be able to pass this cost along to consumers and instead absorb this cost themselves. While it is reasonable to assume that a homeowner is attentive to different heating fuels which differ significantly in cost (Myers 2019), it is unlikely that homeowners would be similarly attentive to the amount of insulation in the walls. This could be supported by the suggestive findings that newly built homes got smaller and had fewer bedrooms after the adoption of IECC 2009. Builders also could have been reducing costs in other ways which are unobservable to us, such as using less expensive finishes or fixtures. We also cannot rule out other changes in market conditions in Wisconsin that may have coincided with the adoption of IECC 2009. It is also possible that some builders in Wisconsin had already adapted to the requirements of IECC 2009 and were building compliant homes before the code was formally adopted, given that neighboring states had already adopted and construction practices might have already shifted. Importantly, we did not find that the adoption of IECC 2009 caused a significant reduction in single-family construction activity in Wisconsin.

These findings suggest that Wisconsin's 2016 adoption of IECC 2009 — its only change to the energy code since 2009 — did not cause shocks to the single-family housing market, but rather that builders adapted and continued to build homes and those homes sold at prices which did not diverge from the control states. If anything, point estimates suggest that prices might have gotten slightly lower in Wisconsin after adoption. However, it is worth noting the income heterogeneity that exists on visual inspection of the event study (Figure 3). Because the point estimates are noticeably lower for the highest quartile, further research should investigate if there is similar heterogeneity among income groups in the permit data.

The statistical power of the methods in this paper is limited because there are only 6 state-clusters (1 treatment, 5 control). The observations are clustered and therefore they are not truly independent and their errors are correlated. Cluster-Robust Standard Errors (CRSE) are not reliable with such a small number of clusters, so WCB methods are employed to prevent over-rejection of the null hypothesis. In practice, this might be a rather conservative estimate, but it is difficult to add more states because either the timing of their code adoption or significantly different climate zones which can change the costs of energy code implementation. Further research using more states and a staggered DiD methodology (Callaway and Sant'Anna 2021) could provide more clusters and therefore increase the statistical power of this approach. It is also difficult to examine longer-run effects because of the 2008 financial crisis and the COVID-19 pandemic which both caused dramatic shifts in housing markets.

Conclusion

This study used a quasi-experimental Difference-in-Differences framework to evaluate the market impacts of Wisconsin's 2016 adoption of IECC 2009 for new single-family homes compared to a control group of Midwestern states. The analysis looked at home transaction prices, permit quantities, and home characteristics. Our findings indicate that the transition to IECC 2009 did not produce statistically significant market disruptions. However, the statistical power is limited by the study design. The evidence suggests that homebuilders adapted to the new energy efficiency requirements without significantly increasing the prices for new homes, reducing the number of new homes, or making the new homes smaller. Broader market dynamics such as land prices and land regulation remain the primary driver of costs rather than the incremental costs of updated energy efficiency requirements. For policymakers, these results suggest that building energy code updates can be pursued to meet decarbonization goals without significant disruptions to the single-family housing market.

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AI-assisted tools used: Github Copilot, Cursor
Used for: Coding.
Extent: Generating code, tables, and figures.
Human review: The authors reviewed and verified all content, calculations, and citations.