



A Critique of an *Urban Studies* Article on the Housing Supply Impact of Land Use Reforms¹

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[LINK TO ABSTRACT](#)

The movement for liberalizing policies that affect housing supply is based in part on the claim that such liberalization will induce augmented supply, reducing housing prices and rental rates. Few researchers would deny that claim. But then there is the empirical question of the size of the effects. If those positive effects are small, then perhaps they do not compensate for potential negative effects of such liberalization.

In 2023, *Urban Studies* published an article titled “Land-Use Reforms and Housing Costs: Does Allowing for Increased Density Lead to Greater Affordability?” (Stacy et al. 2023). The study represents one of the most visible recent efforts to quantify the relationship between local zoning changes and housing supply. Using a machine-learning model to identify news articles about land-use reforms across eight U.S. metropolitan areas, the authors report 180 “major” reforms between 2000 and 2019. They classify each as either more or less restrictive and link

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those classifications to changes in housing supply measured through U.S. Postal Service address-count data. The study concludes that “less restrictive” reforms are associated with roughly a 0.8 percent increase in the housing stock within three to nine years, implying that zoning reform yields only modest gains.

Because Stacy et al. (2023) has already attracted substantial academic and policy attention, the credibility of its underlying data, methods, and conclusions is of central importance. Measuring zoning reform requires not only large-scale data collection, but also accurate interpretation of what constitutes a genuine, material policy change. The approach in Stacy et al.—while innovative—rests on machine-learning outputs that were never publicly validated and a binary classification that obscures the nuance of local land-use policy. These design choices risk mistaking symbolic or procedural actions for meaningful reforms and conflating cyclical market shifts with structural policy effects.

To evaluate the robustness of Stacy et al.’s findings, we independently reviewed all 180 reforms included in their study. Using the same news sources—supplemented by municipal ordinances, zoning maps, assessor records, building-permit data, and the AEI Housing Center’s practical experience from an extensive library of zoning case studies—we assessed each entry along three key dimensions:

1. Factual accuracy – whether the cited policy event occurred as described, in the correct location and year;
2. Classification validity and significance – whether the reform was coded correctly in direction and whether it was substantively large enough to affect housing supply; and
3. Methodological integrity – whether the data construction and measurement approach can credibly support the study’s conclusions.

Our independent review reveals that 60 of these articles should be disqualified outright due to duplication, incorrect geographic attribution, or policies that only affected commercial or industrial areas or properties. Among the remaining 120 entries, 118 are either incorrectly classified in direction (more vs. less restrictive), are either not major municipality-wide reforms, or have insufficient information to accurately determine policy direction. After a thorough and painstaking review of all 180 cases, we found only two that plausibly qualify as “major” reforms—and even these warrant caution given uncertainty about whether their supply impact was measurable using Stacy et al.’s methodology.

These data problems are compounded by structural flaws in the study’s research design. These include the use of a binary “more/less restrictive” classification that obscures policy nuance, limitations in the underlying address data, and the inability to account for policy changes not captured in media coverage. Collectively,

these weaknesses call into question the validity of the study’s headline conclusion—that zoning reforms only modestly increase housing supply—and highlight the need for a more transparent, verifiable, and context-sensitive approach to measuring land-use change.

Overview of the Stacy et al. Study

Stacy et al. (2023) seeks to quantify how local zoning reforms affect housing-supply outcomes across eight large U.S. metropolitan areas. The authors’ stated aim was to capture “major, municipality-wide” enacted zoning reforms—such as density increases, height-limit adjustments, minimum-lot-size reductions, or new allowances for accessory dwelling units (ADUs)—while excluding small, parcel-specific changes. The analysis combines a new dataset of local land-use changes with U.S. Postal Service address counts (from the HUD-USPS Vacancy Dataset) as a proxy for housing supply. The authors estimate city-level fixed-effects panel regressions using city–year and city–quarter data. They find less-restrictive reforms increase housing supply by about 0.8 percent within three to nine years, while more-restrictive reforms produce modest declines.²

Stacy et al. avails itself of a dataset of news reports that catalogues land-use reforms in 40 US metropolitan areas. To build the dataset, Stacy et al. relied on Access World News, a commercial archive of local and regional newspapers. The decision to base the analysis on secondary reporting—rather than on primary legislative or planning records—represents a methodological innovation in land-use research. The authors assembled roughly 76,000 articles published after 2000—a choice of starting date reflecting both the relative scarcity of digitized news coverage before that year and the authors’ goal of focusing on contemporary reforms. They identified potentially relevant stories using approximately 60 targeted keywords, such as “zoning,” “density,” “reform,” and “single-family.”

To distinguish genuine policy reforms from unrelated zoning references, Stacy et al. trained a supervised machine-learning classifier (a random forest model) on a manually labeled sample of 568 articles coded by policy experts. The probability model was trained to predict key attributes of each article—its jurisdiction, the type and timing of the reform, and whether it made local regulations more or less restrictive. The final dataset of 180 “major” reforms was the output of the high-probability model’s predictions.

However, the “reforms” were identified algorithmically and then reviewed by researchers “reading each article and correcting machine-coded data” (Stacy et

² Because the panel effectively ends in 2020, reforms enacted late in the sample period contribute little or nothing to the “Post Period” estimates (defined as three or more years after reform passage), mechanically limiting the long-run identifying variation from the 55 reforms adopted in 2017–2019.

al. 2023, 2925). The machine-learning model inferred policy direction and scope based on textual patterns rather than a full review of the underlying legislative record. Furthermore, licensing restrictions on the Access World News database prohibit the retention or redistribution of article text, complicating the process for outside reviewers to check on the accuracy of the Stacy et al. estimation sample.³

Although this approach represents an innovative attempt to scale up land-use analysis, it also rests on a series of questionable assumptions:

1. that short newspaper summaries can accurately capture the timing, direction, and magnitude of potentially complex zoning actions;
2. that USPS address data adequately proxy for new housing construction; and
3. that binary “more / less restrictive” labels can meaningfully summarize the magnitude of reforms.

These limitations raise questions about both data quality and empirical validity. Because the study’s regression results depend entirely on the accuracy of its machine-coded reform dataset, any misclassifications, duplications, or omissions in the data will directly bias its estimates. The following sections examine these issues in detail by reconstructing the Stacy et al. dataset and reassessing what types of reforms the model actually captured.

Our Method and Data

Stacy et al.’s study relies on local news coverage as the primary source of evidence for identifying land-use reforms. However, media reports vary widely in accuracy, detail, and precision. They are written for general audiences, often by journalists unfamiliar with the technical language of zoning and land-use regulations, and can be shaped by the editorial priorities of their publications. As a result, such sources are prone to incomplete context, factual inaccuracies, and selection bias—issues that are further amplified when filtered through automated text-mining models.

To evaluate the reliability of Stacy et al.’s study, we independently reviewed its dataset of 180 policy changes. The authors provided the list of article URLs and associated metadata (publication date, outlet, reform type). We retrieved the same articles from Access World News (the original news database) and carefully read each one—often multiple times—to determine what policy change, if any, it described.

³ For a more in-depth overview of the database used by Stacy et al., see Zheng (2020).

When the articles were ambiguous or incomplete, we conducted supplementary verification to determine whether each cited reform met the standard stated in Stacy et al.: it constituted a citywide, substantive zoning reform that is expected to meaningfully affect housing supply. This verification process involved:

- Primary municipal sources: city-council agendas, ordinance archives, and planning-department websites to confirm whether and when a measure was formally adopted;
- Spatial data: zoning maps and parcel-level zoning layers from *Zoneomics* to establish geographic coverage and current regulatory context;
- Market evidence: building-permit data from *housingdata.app* (U.S. Census Building Permit Survey) and assessor data from *First American*—including year built, land-use code, lot size, and location—to detect any measurable pre-/post-reform change;
- Visual verification: Google Earth and Street View imagery to observe on-the-ground development patterns; and
- Places dataset: Census Bureau geographic data used to match the Stacy et al. listed reform location to the corresponding jurisdiction. We then compared this assigned location to the place actually referenced in each article to identify any mismatches and miscoding.

Because the meaning and significance of zoning reforms are inherently contextual, we supplemented our document review with background research on local economic and planning conditions and, when necessary, informal consultation with practitioners. For transparency and replication, the complete list of reviewed articles, their URLs, and our and Stacy et al.'s classifications are provided as an Appendix spreadsheet.⁴ Throughout this report, each example references its corresponding article number from the master list in the footnotes.

We assessed each entry along three dimensions:

1. Factual accuracy: Whether the cited policy event as described occurred in the correct city location and year;
2. Classification validity: Whether the reform was coded correctly in its direction and whether it was substantively large enough in applicability to affect a city's housing supply; and

⁴ Footnotes in this paper citing any of the 180 articles include the AEI Article ID, enabling readers to easily cross-reference the cited article with its classification and URL in the data appendix. All quotations are drawn from articles that were either open-source or purchased directly from the originating newspaper or an authorized distributor.

3. Methodological integrity: whether the data construction and measurement approach can credibly support the study's conclusions.

We reviewed the dataset in a sequential “waterfall” process, progressing from cases involving clear disqualifying errors to those involving classification or policy issues. For instance, if an article is factually inaccurate—such as when the reported reform's location in the Stacy et al. spreadsheet does not match the actual location mentioned in the article—we exclude it at that stage without further evaluation of its classification validity or policy significance. This stepwise approach ensures that only verifiable entries advance to the subsequent in-depth levels of review. The table in the next section summarizes this evaluation waterfall.

The contrast between Stacy et al.'s computer-intensive approach and our labor-intensive approach raises a methodological question: if the goal is to develop an authoritative panel dataset of zoning reforms, can that be accomplished using only news reports or other secondary sources? Or must researchers engage in detailed examination of specific zoning amendments and their on-the-ground consequences? In our view, scholarly care is required to identify whether reforms genuinely expand development capacity or merely alter nomenclature or procedural details while leaving substantive barriers intact. Case studies provide the necessary detailed context to distinguish symbolic changes from structural ones. Keyword-driven models are more vulnerable to the vagaries and chicaneries of journalism and political packaging.

Assessing the Validity of Stacy et al.'s Data and Methodology

Stacy et al. claims to have constructed the “first cross-city panel dataset of land-use reforms.” The authors report that “reforms that loosen restrictions are associated with a statistically significant 0.8 percent increase in housing supply within three to nine years of reform passage, accounting for new and existing stock.”

The authors acknowledge that their automated classification was imperfect:

While the machine-learning procedure successfully tagged many articles, the algorithm identified many articles merely discussing zoning reforms that did not indicate reform passage. Though we optimized the machine-learning algorithm to eliminate false negatives, this continued lack of reliability necessitated that the team verify all variables within the dataset for each article. A team of land-use experts including the authors of this study and analysts from a national research organization with experience researching land-use reforms then hand-coded the data by reading each article and correcting machine-coded data. (Stacy et al. 2023, 2925)

However, the numerous factual and classification errors that we identified in our review make this statement difficult to reconcile with the evidence. If the dataset was truly verified through detailed human review, the inconsistencies that we found would not be present.

Table 1 below summarizes our reassessment of each coded policy change. The waterfall approach applies—only the first identified error is recorded for each article, even though many exhibited multiple, overlapping issues.

TABLE 1: Results from AEI Reexamination of Stacy et al. 2023

| Total | 180 | % of total |
|--|------------|------------|
| Do not meet the authors' criteria/not a qualifying reform | 60 | 33% |
| Policy changes attributed to the wrong city | 36 | 20% |
| Duplicative and overlapping policy entries (kept non-duplicative ones) | 17 | 9% |
| Policy change pertains to commercial or industrial use | 4 | 2% |
| Policy change did not pass | 1 | 1% |
| Op-ed | 1 | 1% |
| Informational meeting, not legislative | 1 | 1% |
| Classifying the remaining articles | 120 | 67% |
| Misclassified policy direction (more or less restrictive) | 14 | 8% |
| Not municipality-wide: affected only a single development site | 26 | 14% |
| Not municipality-wide: affected only one or two neighborhoods | 14 | 8% |
| Not major / did not significantly affect the city as a whole | 43 | 24% |
| Article has insufficient information to determine a policy direction | 21 | 12% |
| Major reform, but effect not readily measurable using Stacy et al.'s methodology | 2 | 1% |

Source: AEI Housing Center.

Do Not Meet the Authors' Criteria/Not a Qualifying Reform (60 cases)

Policy change attributed to the wrong city

In 36 articles (20 percent of the total) the reported reform locations do not match the city discussed in the article.⁵ This creates a significant methodological

⁵ The dataset includes both a reformlocationID and a city field, but these often do not align. When we

problem: the regression would be trying to identify housing supply effects from cities that did not implement the cited policy change. This measurement error would bias downward the estimate of a supply response and thereby undermine the validity of any estimated relationship between “reforms” and local housing supply outcomes.

This geographic mismatch likely occurs because a newspaper’s dateline or title city does not always match the jurisdiction actually affected—particularly when the article covers suburban areas or when the algorithm flags a nearby city mentioned incidentally in the text.

Consider the example of a policy change Stacy et al. attributed to the city of Miami. The relevant article, published in the *Miami Herald* on February 19, 2006, discusses a zoning change concerning building height limits. The first sentence states: “Developers who had plans to build in Aventura on hold can now proceed, but face stricter zoning laws.”⁶

The text makes clear that the reform occurred in Aventura, not in Miami. Stacy et al. assigned the reform to Miami because the article appeared in the *Miami Herald*. As a result, the dataset links a nonexistent Miami policy change to subsequent housing supply outcomes in Miami—a clear case of geographic misattribution. (The City of Aventura offers an interesting stand-alone case study as discussed in Appendix 2).

A similar error occurs in an article from the *Main Line Times* (Ardmore, PA) titled “Lower Merion adopts new City Avenue zoning amid Community Protest.”⁷ The article opens with: “It has been called the most massive and significant zoning change ever in Lower Merion Township, and now it is done – almost.” Yet the Stacy et al. dataset attributes this reform to Philadelphia, likely because the *Main Line Times* article later notes that, “The rezoning corresponds in some aspects to a new zoning overlay driven by similar goals that was adopted by Philadelphia for the other side of City Avenue more than two years ago.”

Duplicative and overlapping policy entries (kept non-duplicative ones)

Another 17 observations (over 9 percent of the total) were duplicative. In such cases, we retained only the primary or most comprehensive article describing the reform and removed secondary reports or near-identical duplicates to avoid

contacted the authors for clarification, they advised us to rely on the reformlocationID as the correct reference. When we compare the city field to the location mentioned in the article, the wrong location indicator jumps from 36 to 87 instances.

⁶ Article 60: Teproff, C. “New Laws Strengthen Restrictions.” *The Miami Herald* (FL), 19 Feb. 2006, p. 10NE.

⁷ Article 159: Amid, C. “Lower Merion Adopts New City Avenue Zoning amid Community Protest.” *Main Line Times* (Ardmore, PA), 16 Dec. 2011.

double-counting.

Duplications appear in several distinct forms. In some cases, Stacy et al. list separate “reforms” for cities that adopted multiple related zoning amendments—such as minimum lot size and mixed-use residential provisions—within the same legislative package. For example, in Flower Mound, TX, two sets of entries (2013⁸ and 2015⁹) describe the same council action recorded twice under different policy types.

In other cases, Stacy et al.’s dataset sometimes records separate entries for articles published within days of one another but referring to a single policy action. In Santa Monica, CA, two articles dated June 2 and June 3, 2018 both discuss the same Accessory Dwelling Unit (ADU) ordinance. Interestingly, the authors classify one case as “less” and the other case as “more” restrictive.¹⁰ The article from June 2 describes the “less restrictive” ADU ordinance, but notes that “a second reading of the ordinance will be heard on June 12, 2018.”¹¹ This leaves open the possibility that the reform was pending at the time of publication, an important timing distinction when evaluating post-reform effects.

In other cases, Stacy et al. counts separate articles providing various updates on a given policy reform as unique policy events. This issue, for example, arises in Costa Mesa, CA, where five separate articles between October 2017 and January 2018 all concern the city’s single ADU ordinance—tracking the progression from planning commission discussions through final council approval.¹² Or consider an example from Arcadia, CA, where local newspapers reported on the same zoning change at different stages of deliberation. The Stacy et al. database’s three articles on density or floor area ratio between April 4–21, 2016 describe suc-

⁸ Article 169 and 170: Roark, Chris. “Council Briefed on Tree Removal Plan; Ninth-Grade Center Approved.” *Flower Mound Leader* (TX), 6 Feb. 2013.; and Roark, Chris. “Council Briefed on Tree Removal Plan; Ninth-Grade Center Approved.” *Flower Mound Leader* (TX), 6 Feb. 2013.

⁹ Article 171 and 172: Roark, Chris. “Flower Mound P&Z Recommends River Walk Changes, High-Density Residential Project.” *Flower Mound Leader* (TX), 13 Jan. 2015.; and Roark, Chris. “Flower Mound P&Z Recommends River Walk Changes, High-Density Residential Project.” *Flower Mound Leader* (TX), 13 Jan. 2015.

¹⁰ Article 54 and 55: Eden, Jennifer. “Got a Granny Flat? New Rules Are Here.” *Santa Monica Mirror* (CA), 2 June 2018.; and Donald. “Regulations for Accessory Dwelling Units.” *Canyon News* (Beverly Hills, CA), 3 June 2018.

¹¹ Article 54: Eden, J. “Got a Granny Flat? New Rules Are Here.” *Santa Monica Mirror* (CA), 2 June 2018.

¹² Articles 14-18: Casiano, Louis. “Affordable housing – Changes to allow more granny flats inside Costa Mesa moving forward.” *The Orange County Register weekly* (*Newport Beach-Costa Mesa, CA*), 19 Oct. 2017, p.3.; Casiano, B. Louis. Casiano, Louis Jr. “Housing – Costa Mesa Gives Final Approval for ‘Granny Flats.’” *The Orange County Register* (Santa Ana, CA), 19 Jan. 2018, p. 1.; Casiano, Louis. “City gives final approval for additional ‘granny flats.’” *Coastal Current: Newport Beach, Costa Mesa, Laguna Beach* (CA), 25 Jan. 2018, p.1.; Casiano, Louis. “costa mesa – City approves rules to loosen requirements for ‘granny flats.’” *Coastal Current: Newport Beach, Costa Mesa, Laguna Beach* (CA), 11 Jan. 2018, p.6.; Casiano, L. “Costa Mesa Advances New Regulations Loosening Requirements for ‘Granny Flats.’” *The Orange County Register: Web Edition Articles* (CA), 3 Jan. 2018.

cessive coverage of the same ordinance from preliminary hearings through final approval.¹³ Counting each stage of the same legislative process as a separate reform can misstate the policy timeline and multiply a single ordinance into several artificial “reforms,” inflating measured reform exposure and potentially diluting estimated supply responses.¹⁴

A related problem arises in San Clemente, CA, where two *Sun Post News* articles from 2006 and 2008 describe the same dispute over a 16-foot height limit in the small neighborhood of Shorecliffs.¹⁵ Stacy et al.’s dataset treats these as separate “more restrictive” reforms, even though the later article confirms that the policy was finalized by voter referendum in 2008, not in 2006. The result is a reform misdated and double-counted. Moreover, the affected area—roughly 500 homes out of a city of 64,000—is likely too small to influence overall housing supply.¹⁶

Collectively, the duplications inflate the count of unique policy actions and distort statistical estimates of zoning reform effects. Counting multiple news articles or reform components as separate “reforms” reinforces already-measured policy changes and muddles any link between zoning actions and subsequent housing outcomes. This pattern underscores the need for a stricter rule for identifying unique and major reforms—ideally anchored to verified ordinance numbers or adoption dates—before drawing any quantitative conclusions.

Policy change pertains to commercial or industrial use

Four articles classified by Stacy et al. as land-use reforms actually pertain to non-residential use (e.g. the parking of trucks on residential property) or non-residential development (commercial properties, hotels, shopping centers, or light

¹³ Articles 8–10: Tompkins, C. “Mansionization Debate Spurs Arcadia to Adopt New Home-Size Law.” *San Gabriel Valley Tribune* (West Covina, CA), 4 Apr. 2016, p. 1.; “Arcadia’s Planned Anti-Mansionization Ballot Initiative Hits a Wall.” *KPCC – 89.3 FM: Web Edition Articles* (Pasadena, CA), 6 Apr. 2016.; and Tompkins, C. “Arcadia New Housing Standards Approved.” *San Gabriel Valley Tribune* (West Covina, CA), 21 Apr. 2016, p. 5.

¹⁴ Of the 17 duplicative entries we identified, 13 reflected unique underlying instances. Of them, four would be recorded in different quarter/year pairs and one in a different city, implying both timing inflation (multiple quarter “reforms” for one ordinance) and, in one case, outright treatment misassignment that can attenuate or otherwise distort estimated supply effects.

¹⁵ Articles 45 and 46: Swegles, F. “Voters Approve Shorecliffs Building-Height Restriction – Read What Both Sides Have to Say About the Result at www.sunpostnews.com.” *Sun Post News: Orange County Register Weekly* (San Clemente, CA), 5 June 2008, p. A.; and Swegles, F. “Shorecliffs Ht. Limit OK’d 3-2.” *Sun Post News: Orange County Register Weekly* (San Clemente, CA), 13 July 2006, p. Cover.

¹⁶ While such errors are relatively easy to identify when multiple articles exist in the Stacy et al. dataset, they are much harder to detect when only a single article is available. This leaves open the possibility that some policy reforms never became effective—or became effective at a date that differs from what Stacy et al. claim. We identified at least five articles (Articles 5, 7, 29, 40, and 137) that reference reforms which, at the time of publication, had not yet been fully implemented.

industrial uses).

For example, an article in the *Daily Herald* from 2007 describes a commercial zoning adjustment pertaining to overnight parking of commercial trailers in residential driveways in Schaumburg, IL.¹⁷ The measure clearly concerns commercial use in a residential area. Another article in the *Main Line Times* from 2014 covers a downtown business district reform in Narberth, PA: “Narberth Borough Council took a step this week that may make a coffee shop or new restaurants—a frequently heard request from residents—more likely options in the downtown.”¹⁸ This policy explicitly targets retail and restaurant uses, not housing. Such examples highlight the limitations of keyword-based article identification where terms like “zoning” or “development” are interpreted as housing-related even when the underlying policy concerns commercial land use. Including these cases as part of a housing supply dataset introduces additional measurement error, overstating the number of meaningful housing reforms identified by the study and exacerbating downward bias in the estimated supply response.

Miscellaneous

In addition, our review discovered one article where the policy did not pass,¹⁹ one opinion piece (with another judged sufficiently factual to retain²⁰) which reflects subjective commentary rather than objective reporting, and one unrelated article which merely described an informational meeting between residents and a city commissioner rather than any actual policy change.²¹

Summary

Our review finds that at least 60 of the 180 articles—one-third of the sample—appear to be factually inaccurate or misrepresented and, therefore, do not qualify

¹⁷ Article 91: “News.” *Daily Herald* (Arlington Heights, IL), 9 May 2007, p. 3.

¹⁸ Article 157: Callison, C. “Ordinance May Clear Way for Coffee Shop, More Restaurants in Downtown Narberth.” *Main Line Times* (Ardmore, PA), 15 May 2014.

¹⁹ Our review identified at least one false positive—a “reform” that was never enacted. The 2011 *Oregonian* article reported Wilsonville’s City Council had “accepted” the Old Town Neighborhood Plan, but a 2017 *Wilsonville Spokesman* article clarified it was never adopted. Stacy et al.’s dataset nonetheless codes both as separate restrictive reforms, misdating and double-counting an unenacted proposal. Such errors highlight the risk of confusing legislative intent with legislative action, biasing estimates of housing-supply effects. Article 151 and 152: Stark, R. “Wilsonville’s Old Town Residents Applaud Plan.” *The Oregonian* (Portland, OR), 24 Sept. 2011; and Green, C. “Planning for Old Town Charm.” *Wilsonville Spokesman* (OR), 11 Oct. 2017.

²⁰ Articles 52: Melkonians, A. “Community Benefits: Sizzle Sells the Bacon – Our Town.” *Santa Monica Daily Press* (CA), 8 Oct. 2013, p. 4.

²¹ Article 61: Veldemoro, T. “City Planners Calm ‘Upzoning’ Worries.” *The Miami Herald* (FL), 19 Oct. 2006, p. 7GR.

as proof of valid policy reforms. Collectively, these data inaccuracies would lead to a downward biased estimate of the housing supply response to zoning reforms.

Classification and substantive significance of the remaining 120 cases

Stacy et al. assert that: “We identified 180 major reforms during the study period in the eight regions’ 1136 cities... We intentionally excluded small scale reforms, since our interest is in municipality-wide impact. We did not include reforms affecting only one or two neighbourhoods, but we did include reforms that, for example, reduced minimum lot sizes on all parcels” (Stacy et al. 2023, 2925).

In the technical appendix to the paper, one of the coauthors says:

Although to some degree labeling a “major” reform relied on taggers’ subjective judgement, we did train the taggers on how to differentiate among reforms. Generally, a major reform either significantly affected the whole city or was an extreme departure from existing practices. For example, reforms that unprecedentedly disallowed or allowed a whole type of housing, or doubled or halved housing height or density would be considered major. (Zheng 2020)

If the article represented a true reform, the taggers would record when and where the reform took place, the geographic extent to which the reform applied, if the reform was major, if the reform made the regulation more or less restrictive, and the specific topic of the reform. (Zheng 2020)

Taken together, these statements indicate that the authors intended to capture citywide, substantive zoning reforms—that is, reforms that would be expected to meaningfully affect housing supply.

Our independent assessment of the remaining 120 policy changes identified by Stacy et al. shows that the vast majority fail to meet the study’s own threshold for “major” or “municipality-wide” land-use reforms.

Specifically, our detailed reclassification yielded the following:

- Wrong classification (14 cases): These entries were coded in the wrong policy direction—for example, reforms that increased regulatory stringency were labeled “less restrictive,” or vice versa.
- Not municipality-wide (40 cases): Of these, 26 affected only a single development site, and 14 applied to only one or two neighborhoods. Such localized actions clearly do not meet the study’s stated standard of “city-wide” reform.

- Not major / did not significantly affect the city as a whole (43 cases): The model detected linguistic cues of regulatory “tightening” or “loosening” but lacked the context to judge whether changes were substantive enough to affect housing supply. Direction is not magnitude—many identified reforms were merely procedural or symbolic, with little real impact.
- Unclear policy direction or article needs more information (21 cases): In eight cases, the direction of reform (more vs. less restrictive) could not be determined from the article text, while in 13 cases, the information provided was insufficient to establish the nature or scale of the reform without additional legislative context.
- Major reform but not suitable for reliable supply measurement (two cases): Only two entries qualified as major, citywide zoning reforms, and even in these instances, the research design makes it difficult to credibly estimate their impact on housing supply within the study’s framework.

As a result, the portrayal by Stacy et al. of its dataset as representing “major, municipality-wide reforms” appears to not be supported by the evidence. In our judgment, only two of the 180 entries could plausibly be considered both factually accurate and representative of major reforms—and even these exhibit timing or data-quality issues that compromise their analytical value.

By treating small, localized, or symbolic zoning adjustments as equivalent to sweeping deregulations, the authors inflate the count of meaningful reforms while diluting the estimated effects of genuine policy changes. This biases the analysis toward finding small and statistically insignificant supply responses, further weakening its broader conclusions about the relationship between zoning reform and housing affordability.

The following sections provide detailed examples illustrating these classification and measurement errors.

Misclassified policy direction

Our examination reveals that at least 14 articles involved potential misclassifications concerning the direction of policy changes by Stacy et al.’s machine-learning algorithm. Take, for instance, a June 1, 2010 article in the *Gloucester County Times*. The authors categorized this as more restrictive, possibly influenced by the usage of the word “restricts” later in the article. However, the initial sections clearly describe a rezoning intended to facilitate development, suggesting a less restrictive nature. Key details from the article include:

Several tracts of land have been rezoned under an ordinance adopted by the township council to become the new ‘Regional Growth Mixed Use District.’

Members of the council said the zoning change could open doors for more development on the Black Horse Pike, which is also U.S. Route 322 through Monroe Township.

The ordinance restricts the size and type of buildings within the mixed-use district. Single-family attached townhomes and detached housing each must not exceed 50 percent of what is built within the district. Single-family semi-detached homes are limited to 30 percent, and multi-family/apartment flats can't surpass 25 percent of the construction there. Institutional buildings and single-family attached structures must make up at least 25 and 20 percent, respectively, according to minimum requirements.²²

Similarly, a November 11, 2017 article from *The Orange County Register: Web Edition Articles* was categorized under ADU reform and classified by Stacy et al. as more restrictive. The key excerpt from this article is:

FOUNTAIN VALLEY In his last full City Council session before he hands over mayoral duties to Michael Vo, John Collins and the rest of the council reluctantly approved several changes to the city's housing ordinances—including a loosening of requirements for so-called granny flats—forced on cities by changes in state law.²³

These paragraphs suggest an easing of restrictions, contrary to the Stacy et al.'s classification. Such instances underscore the challenges and potential inaccuracies inherent in using machine-learning algorithms for policy classification without a deeper, contextual understanding of the content that often only analysts (at this point in time) can provide.

We encountered cases where land use reforms were potentially mis-categorized due to the machine-learning algorithm's failure to differentiate the baseline context of the changes. Reforms intended to ease restrictions and increase housing density were sometimes coded as "more restrictive," likely because the article described a tightening relative to an earlier draft proposal, even though the final version remained less restrictive compared to the pre-existing baseline.

Consider a case from Long Beach, CA categorized under "Density or Floor-Area-Ratio (FAR)" and dated November 11, 2017. The *Long Beach Press-Telegram*, as described by Stacy et al., classified this reform as more restrictive. Key excerpts from the article reveal the complexities involved (*italics added for emphasis*):

After contentious meetings, Long Beach *releases revised development maps* with less density.

²² Article 115: McAneny, DJ. "Monroe Township announces tracts eyed for new redevelopment project." *Gloucester County Times*, 1 June 2010.

²³ Article 26: Mellen, Greg. "Fountain Valley loosens 'granny flat' and other development restrictions required by state." *The Orange County Register: Web Edition Articles (CA)*, 22 Nov. 2017.

But, officials say, it's the most balanced plan they could accomplish after 18 months of work and community feedback, including four community meetings that at times turned contentious.

When maps were first released to the Planning Commission in June, commissioners were uncomfortable with the proposed density and declined to recommend approval by the City Council. The plan was slated to go to the council for consideration in August, but that decision was met with opposition by some of the city's most vocal constituents.

Many at those meetings were upset about proposed maps that would allow buildings seven stories high in Alamitos Beach, six stories high around the Traffic Circle, and four to five stories high in a lot of other places.

Some of those heights still remain in the proposed plan, but many have been scaled back by at least one or two stories. And in some cases, areas that were proposed as mixed use have been returned to their current commercial use.²⁴

The challenge lies in appropriately classifying a policy change relative to its established baseline. In this example, despite more restrictive revisions, the final plan was less restrictive than the pre-existing conditions, a nuance not captured by the Stacy et al. machine-learning algorithm. Well trained analysts still have a superior capacity to assess pre- and post-regulation scenarios comprehensively. Algorithms track changes sequentially and can miss the broader context of their implementation.

Not municipality-wide: affected only a single development site

Despite the authors' assertion that they "intentionally excluded small-scale reforms" and omitted "reforms affecting only one or two neighbourhoods," our analysis challenges this claim by finding 26 instances of one-off developments. Take for example, a 2014 policy change reported in the *Skokie Review*, which facilitated construction, but affected only 27 housing units in a town with 25,000 units—hardly qualifying as a major reform.²⁵

Additional examples include a report from the *Sun Post News: Orange County Register Weekly*, which notes that the reform pertains only to a "small section of

²⁴ Article 34: Tompkins, Courtney. "LONG BEACH PROPOSAL - Revised land use maps released." *Long Beach Press-Telegram*. 11 Nov, 2017. Another example for case is an article from Nov. 19, 2014 in *The Mecklenburg Times* labeled as less restrictive. The described policy change in the article may have added around 400 units in Charlotte City with a population of 900,000. See: Fuchs, Roberta. "Council hears Quail Hollow plans; Halvorsen, Marsh projects get the green light." *Mecklenburg Times*. 19 Nov, 2014.

²⁵ Article 92: Isaacs, Mike. "Floral Site Plan Tweaked, Construction Set for This Month." *Skokie Review*, 13 Mar. 2014.

town.”²⁶ Another piece from *The Daily Item* addresses a zoning change affecting between 11 and 46 parcels in Lynn, MA.²⁷ These instances underscore a significant shortfall in the machine-learning algorithm’s ability to correctly discern the effective scope of reforms.

Not municipality-wide: affected only one or two neighborhoods

Our review found 14 articles that only applied to a small area, and not the entire city. A noteworthy example from Philadelphia, PA, as reported in the *Philadelphia Inquirer* on May 7, 2017, falls under the “Rezoning” category, and is marked as more restrictive. The text notes that the ordinance “decreases the density of certain areas bounded by North Broad Street and Girard, Ridge, and Cecil B. Moore Avenues in an effort to preserve more single-family housing here.”²⁸ As illustrated in Figure 1, the area affected by this ordinance constitutes less than 1 percent of the city’s total area. This case underscores the necessity for a more nuanced approach in Stacy et al.’s methodology, one that more accurately reflects the true extent and potential impact of the included land use reforms.

Consider also a June 10, 2016 article from *Orange County Register* titled, “Dana Point voters limit development.”²⁹ It states that “Measure H, a grass-roots effort to require voter approval on any development changes to a 2008 plan for the downtown Lantern District, appears to have bested a rival, city-approved measure.” As seen in Figure 2, the downtown Lantern District is only a small part of the town.³⁰

²⁶ Article 46: Swegles, Fred. “Voters approve Shorecliffs building-height restriction.” *Sun Post News: Orange County Register Weekly*. 5 Jun, 2008.

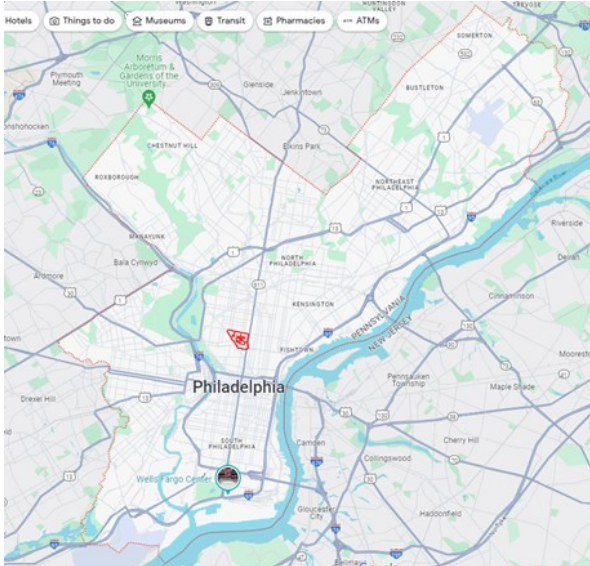
²⁷ Article 106: Amaral, Jenny. “Peabody residents get zoning amendment passed – PEABODY.” *The Daily Item*, 3 Oct. 2008.

²⁸ Article 160: McCabe, Caitlin. “As North Philly continues to gentrify, City Council rezones to promote single-family homes.” *The Philadelphia Inquirer*, 7 May 2017.

²⁹ Article 20: Ritchie, E. I. “Dana Point Voters Limit Development.” *The Orange County Register* (Santa Ana, CA), 10 June 2016, p. Class1_B.

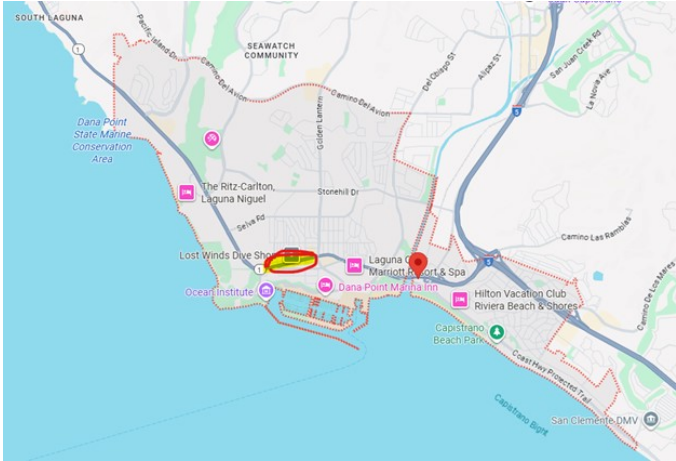
³⁰ <https://www.danapoint.org/City-Government/Community-Development/Planning/Long-Range-Planning/Lantern-District-Town-Center>

Figure 1: Philadelphia City and the area affected by the “Rezoning” policy change (colored in red)



Source: Google Maps and AEI Housing Center.

Figure 2: City of Dana Point, CA and the area affected by the “Rezoning” policy change (circled in red)



Source: Google Maps, City of Dana Point, and AEI Housing Center.

Not major / did not significantly affect the city as a whole

For 43 of the articles, the Stacy et al. machine-learning algorithm correctly identified the direction of a policy change—whether it was more or less restrictive—but failed to assess the substantive significance of the change. In other words, the model could detect linguistic signals of “tightening” or “loosening” regulations, but lacked the contextual understanding necessary to determine whether the reform was material to housing outcomes. This is a crucial distinction: direction is not magnitude. In zoning, many reforms that appear “liberalizing” or “restrictive” on paper are not so in reality and, consequently, have negligible real-world effects. We evaluate significance along two dimensions:

1. Extent: How broad the change was geographically; and
2. Intensity: How substantial the change was in altering development rights.

An example comes from Thousand Oaks, CA, in an article by *Thousand Oaks Acorn* titled “City Council Prevents Lot Split, Acts to Protect.”³¹ The Stacy et al. dataset classifies this case as more restrictive because the City Council increased the minimum lot size from 10,000 to 20,000 square feet and imposed a 10-month moratorium on new subdivisions. Yet the article itself indicates that the action was prompted by a single subdivision request, and that the affected area had little to no ongoing development activity. The change, therefore, merely formalized a condition that already existed in practice rather than altering future development capacity in any measurable way.

Another case in point is Lake Forest, IL, as described in a November 20, 2014 *Lake Forester* article.³² Stacy et al. codes the new policy as less restrictive, describing it as an ordinance to allow accessory dwelling units (ADUs).³³ However, the text of the article reveals that the policy’s practical impact was highly constrained: To address council members’ concerns, the city capped unrelated occupants at two, limited ADUs to large-lot areas and existing structures, required owner-occupancy, and imposed setback rules. These cumulative restrictions—large-lot eligibility, owner-occupancy, and parking mandates—render the reform practically meaningless. A human reviewer should recognize this as administratively and technically less restrictive but substantively inconsequential.

Another set of instances involves anti-mansionization rules. An April 21, 2016 *San Gabriel Valley Tribune* article, for example, describes controversy over “de-

³¹ Article 1: “City Council Prevents Lot Split, Acts to Protect Neighborhood.” *Thousand Oaks Acorn* (CA), 1 Nov. 2007.

³² Article 86: Dorfman, David. “Lake Forest Will Allow ‘Granny Flats’ with Some Restrictions.” *Lake Forester* (Lake Forest, IL), 20 Nov. 2014, p. 17.

³³ The same largely applies to ADU policies in California, described later.

molishing older, smaller homes and replacing them with large ones that often dwarf surrounding dwellings.”³⁴ The dataset correctly codes the change as “more restrictive,” but the substantive impact on housing supply is zero: The regulation constrains the size of replacement homes, not the number of homes, so one unit is still replaced by one unit.

A similar case appears in Tustin, CA (*Tustin News / Orange County Register*, September 17, 2015)³⁵, also coded as less restrictive. The article reports that the new rule “quadrupled the number of eligible properties, with all 194 Old Town locations theoretically allowed to offer backyard rentals.” Yet Tustin contains roughly 28,000 housing units, meaning the reform in Old Town applied to less than one percent of the city’s already existent housing stock—a de minimis change in scale.

In these examples, the Stacy et al. model successfully detected permissive phrasing—terms like “allow,” “permit,” or “increase eligibility”—but failed to recognize that these reforms were too encumbered by other restrictions and/or too limited in scope to have any meaningful housing supply effect.

The problem is particularly evident in Stacy et al.’s treatment of California’s ADU laws, which account for about 17 percent of all reforms in the dataset. While state legislation in 2017 mandated broader statewide ADU allowances, many cities embedded “poison pills” such as minimum-lot sizes, setback requirements, owner-occupancy clauses, and design caps that sharply limited where ADUs could actually be built. Without reading ordinance text, classifying these measures as “less restrictive” overstates their effect.

Data from California’s Department of Housing & Community Development confirm the limited initial impact. Constrained by local filters statewide, ADU permits rose only modestly—from 8,000 to 11,000 per year between 2018 and 2020. After the 2020 state law forced compliance with legislative intent (outside the Stacy et al. observation window), cities removed many of these barriers, and permits subsequently surged to 18,000–28,000 annually (2021–2024).

The issue is especially stark in Hermosa Beach (*Easy Reader*, July 27, 2018). The article announces a new ordinance allowing ADUs only on lots of 4,000 square feet or larger, yet assessor and Zoneomics data show that less than 30 percent of the city’s single-family parcels meet this requirement.³⁶ Thus, most homeowners remained ineligible. Only after 2020 were these restrictions lifted, making the earlier reform largely nominal (see Figures 3 and 4). This pattern repeats across much of the California ADU policy changes in Stacy et al.’s database.

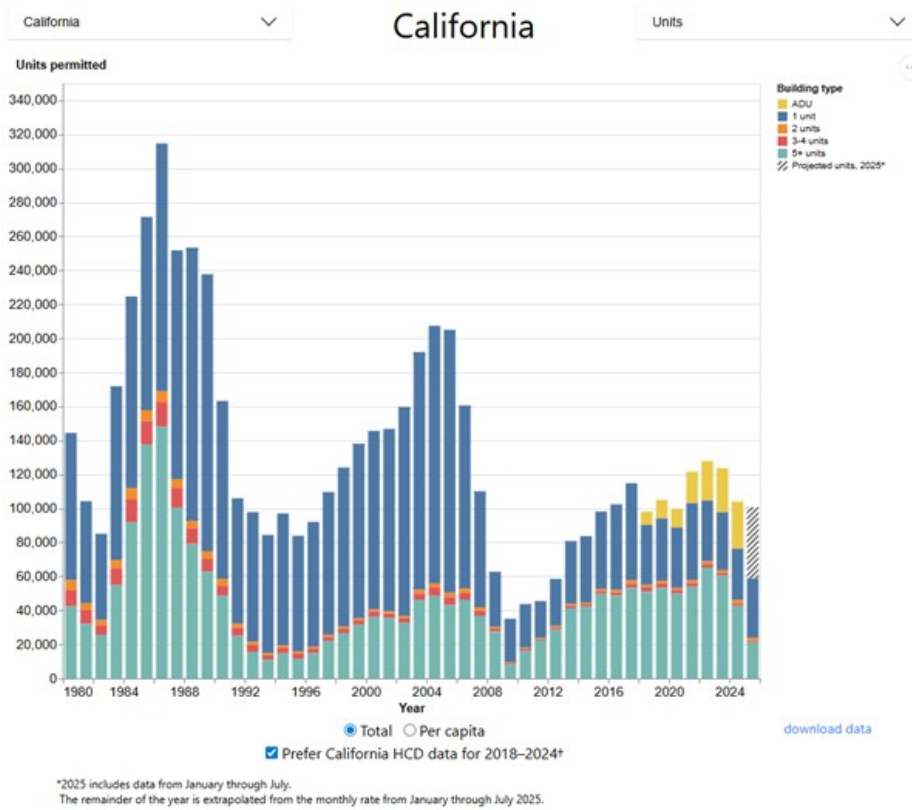
³⁴ Article 10: Tompkins, Courtney. “Arcadia New Housing Standards Approved.” *San Gabriel Valley Tribune* (West Covina, CA), 21 Apr. 2016, p. 5.

³⁵ Article 57: Edwards Staggs, Brooke. “New Rules for Old Town.” *Tustin News: Orange County Register Weekly* (CA), 17 Sept. 2015, p. Cities.

³⁶ Article 24: McDonald, R. “Council Approves New Rules for ‘Granny Flats’ in Hermosa Beach.” *Easy Reader* (Hermosa Beach, CA), 27 July 2018.

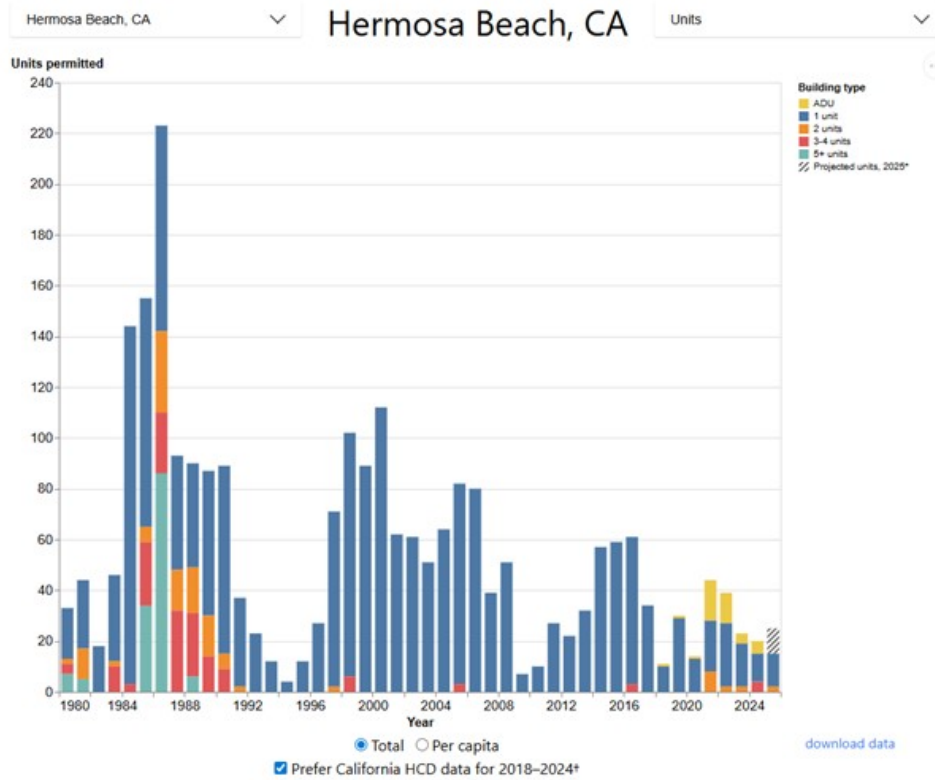
Together, these findings highlight a central flaw in Stacy et al.’s approach: its machine-learning model recognizes key words, but not the context surrounding these words. Although the authors report that researchers reviewed each article, these examples suggest that critical features, context, and nuances were still missed. Without ordinance verification or attention to geographic and economic scale, this approach is prone to systematically overstate the magnitude of local reforms.

Figure 3: Building Permits in California by Type and Year



Source: HousingData.app.

Figure 4: Building Permits in Hermosa Beach, CA by Type and Year



Source: HousingData.app.

Article has insufficient information to determine a policy direction

We identified 21 articles with insufficient information to determine a policy direction. Media coverage typically consists of brief articles targeted at a general audience which often fail to capture the detailed nuances found in complex and technical legal documents, such as enacted ordinances and amendments. Consequently, the information provided in news articles is frequently insufficient for making definitive determinations as to the direction of a policy change.

Consider the following examples:

1. Costa Mesa, CA

On January 19, 2018, *The Orange County Register* published an article titled

“Costa Mesa gives final approval for ‘granny flats’”. The article highlights the crucial stipulations that were enacted:

The 3–2 vote establishes new rules that allow accessory units on smaller properties. The council reduced the minimum lot size for accessory units in single-family areas to 7,900 square feet from 8,500 square feet. City staff had originally recommended a minimum size of 7,200 square feet to increase the number of units that could be built.³⁷

Without knowledge of how many lots are affected by this change, it is difficult to assess whether this policy could have any significant impact.

2. Union County, NC

A brief 80-word article titled “Union Co. updates UDO” [referring to a uniform development ordinance] that merely describes technical amendments to the county’s new UDO without providing any details, including any direction as to the changes.³⁸ From such a brief description, it is challenging to discern how Stacy et al.’s machine learning algorithm could categorize this policy change as more restrictive regarding ADUs.

3. San Clemente, CA

A February 4, 2010 *Orange County Register* article³⁹ describes a zoning change along a short stretch of Avenida Victoria, which Stacy et al. coded as more restrictive. The City Council reduced the available floor-area ratio (FAR) bonus from 2.0 to 1.5, but simultaneously made the bonus by right, eliminating a discretionary “public-benefit” review process. The article itself suggests this could be interpreted as less restrictive: “On Tuesday, the council scrapped the public-benefit allowance while increasing the floor-area ratio allowance by half, feeling the original standard was too restrictive.” In either case, the geographic scope of the reform is extremely limited, covering only a single block along Avenida Victoria with just two vacant parcels available for potential development. Given this narrow footprint, the policy’s broader impact on housing supply or affordability would likely be minimal. Moreover, the regulatory change presents an ambiguous mix of restrictiveness. Additional information would be needed to determine the net effect

³⁷ Article 15: Casiano Jr., Louis. “Costa Mesa gives final approval for ‘granny flats’”. *The Orange County Register*, 19 Jan. 2018.

³⁸ Article 121: Guion, Payton. “Union Co. updates UDO.” *Mecklenburg Times*, 20 May 2015.

³⁹ Article 24: McDonald, R. “Council Approves New Rules for ‘Granny Flats’ in Hermosa Beach.” *Easy Reader* (Hermosa Beach, CA), 27 July 2018.

of this trade-off—whether the reform ultimately expanded or constrained development capacity.

4. Long Beach, CA:

At first glance, a *Long Beach Press-Telegram* article published on November 11, 2017⁴⁰ appears to describe a clear-cut “more restrictive” density policy. The opening line states, “Revised land use maps released Friday include a reduction in height and density across the city.” However, a closer look reveals a more complex story. Additional reductions in density were made after the article was written but before the plan’s adoption four months later. More importantly, the proper baseline for evaluating restrictiveness is the 1989 Land Use Element, not the scaled-back 2017 draft described in the article. When compared with the longstanding 1989 plan rather than the earlier 2017 proposal, the adopted update arguably represented a partial liberalization, not a straightforward tightening, illustrating the importance of accurate policy baselines.

5. Munster, IN:

A March 2017 FAQ⁴¹ published by the town describes a planned upzoning around future transit stops “along the South Shore Line and proposed West Lake extension.” The article concludes by noting that “the final, compromise version of the legislation then must be re-approved by both the House and Senate to go to Gov. Eric Holcomb for his signature or veto.” The bill was ultimately enacted, authorizing the creation of Transit Development Districts near proposed rail stations. However, the two stations in Munster—Ridge Road and Munster/Dyer Main Street—were expected to be opening in September 2025.⁴² Given that Stacy et al.’s study period ends in 2020, it is implausible that any measurable housing-supply response could have occurred in these areas before the stations became operational. As such, the article does not provide enough information to form a clear judgment about the reform’s timing or potential impact.

Major reform but supply impact was likely not accurately measured using Stacy et al.’s methodology

Only two reforms on ADUs in Portland, OR appear to have been substantive,

⁴⁰ Article 34: Tompkins, Cynthia. “Long Beach Proposal: Revised Land Use Maps Released — After Contentious Public Meetings, City Reduces Height and Density Allowed in Proposed Building Guide.” *Long Beach Press-Telegram* (CA), 11 Nov. 2017, p. 1.

⁴¹ Article 55: Donald. “Regulations for Accessory Dwelling Units.” *Canyon News* (Beverly Hills, CA), 3 June 2018.

⁴² https://www.munster.org/egov/documents/1743605669_90006.pdf

though even these have issues. Just like in California, Portland’s ADU success story took multiple attempts before reaching full traction. Stacy et al. identifies three ADU-related reforms in Portland, all classified as less restrictive:

- 2010: Allowed ADUs up to 75% of the primary unit’s size or 800 sq. ft., whichever is smaller.
 - We classified this as a “non-major change” for the reasons outlined below.
- 2015: Further liberalized standards, easing setbacks, fees, and permitting requirements.
 - We classified as a “major reform, but unclear that the supply impact can be accurately measured”.
- 2016: Mentioned ADUs only in passing within a broader planning update.
 - We classified this as “a major reform, but unclear that the supply impact can be accurately measured”.
 - The article states:

“The plan also calls for 20 percent of future growth to be in existing residential neighborhoods. Among other things, it envisions rezoning parts of single-family neighborhoods for such so-called ‘missing middle’ housing as duplexes, triplexes, fourplexes, accessory dwelling units, cottage clusters and small apartments.”

- We classified the 2015 and 2016 as a “major reform but unclear that the supply impact can be accurately measured.” Both changes are only seven months apart and confounded by other policy changes, which makes it difficult to independently evaluate them.

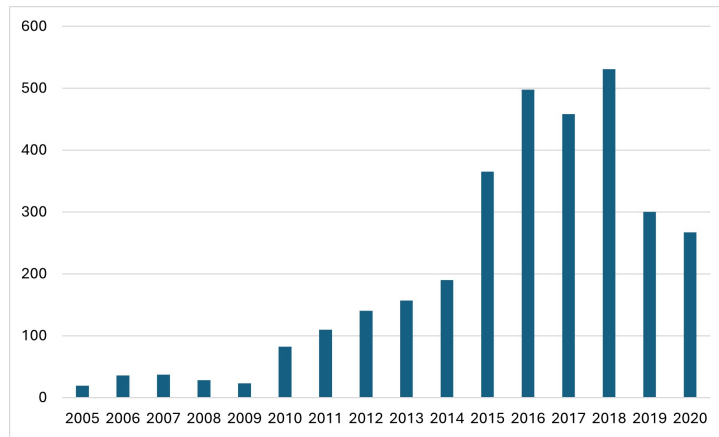
These entries align broadly with Portland’s timeline of ADU policy evolution and the observed surge in permit activity, but the accompanying articles omit key details. In 2010, the City Council waived System Development Charges (SDCs)—one-time permit fees—for ADUs—initially a three-year waiver—and updated the zoning code, actions widely credited with jump-starting ADU construction.

The SDC waiver was later extended through at least 2018, and in 2015, the Council approved a streamlining package to simplify land-use reviews and clarify design standards. As a result, ADU permits climbed sharply—from 23 in 2009 to more than 500 by 2018, with particularly strong growth visible by 2015–2016 (see chart).

This case underscores that not all policy changes are equal. The 2010 reforms laid the groundwork for ADU growth, but it was the 2015–2016 measures that provided the administrative clarity and regulatory certainty necessary to accelerate production. Stacy et al., however, attributes much of this later surge to the initial 2010 reform, thereby overstating the impact of the early policy change and understating the cumulative, iterative nature of Portland’s ADU liberalization.

Moreover, Portland’s ADU experience illustrates a simpler and more reliable way to measure the effects of reform: directly analyzing permit data that the city itself publishes and tying it back to policy events.⁴³ Such data (see Figure 5) clearly capture the timing and magnitude of policy-driven changes—something that cannot be achieved through keyword-based inference from newspaper coverage.

Figure 5: ADU Permits Issued in Portland, OR by Year



Source: AccessoryDwellings.org

However, Portland also had three additional reforms listed by the Stacy et al. that could have influenced residential growth beyond the ADU-related changes:

- 2015 – Height Limit (classified as less restrictive)
- 2016 – Density/FAR (classified as more restrictive)
- 2018 – Height Limit (classified as less restrictive).

⁴³ See for example: <https://accessorydwellings.org/2021/07/22/portland-oregons-adu-permit-data-for-2020/>.

The USPS data cannot distinguish ADUs from other housing types (and may not capture all ADUs, see discussion below). While ADU permits rose from fewer than 40 annually before 2010 to roughly 500 per year between 2016 and 2018, permits for units in five-or-more-unit buildings nearly doubled during the same period—from about 2,500 to 4,500. This broader surge in multifamily construction likely overstates the specific effect of the ADU policies.

Other concurrent factors likely conflated the measured effects of these policy changes. By the mid-2010s, Portland’s housing permits—especially for 5+ unit buildings—were already rising sharply, reflecting both a post-Great Recession construction rebound and broader citywide planning initiatives. During this period, it is also our understanding that planners expanded support for multifamily housing along transit corridors, particularly on SE Division, N Williams, and MLK Blvd, consistent with the Portland 2035 Comprehensive Plan (adopted 2016, effective 2018) and related Mixed Use Zones reforms.⁴⁴

Overall, among the 180 reforms coded by Stacy et al., only two—both on ADUs in Portland—qualify as genuinely substantive. Yet even these examples illustrate the limitations of the authors’ dataset and interpretation. In Portland, OR, successive adjustments to ADU regulations between 2010 and 2016 spurred measurable increases in permits, but the study misattributes later growth to early reforms and overlooks concurrent citywide planning changes and a broader post-recession recovery. The Tigard Triangle Plan (2018) represents a clear deregulatory initiative, simplifying zoning and eliminating parking and FAR limits over a 500-acre area. However, it was adopted at the end of the study window, meaning most of its housing impact lies beyond the observed period. In both cases, Stacy et al. captures genuine reform moments but misses critical context—timing, overlapping policy changes, and implementation lags—highlighting the pitfalls of treating complex, iterative zoning processes as single, isolated “events.”⁴⁵

Methodological Challenges

Beyond likely article-level misclassifications, Stacy et al. suffers from several methodological weaknesses in its broader research design, measurement strategy, and interpretive framework.

Binary classifications

Stacy et al. employs a binary framework to categorize land-use changes as either “more restrictive” or “less restrictive.” This approach oversimplifies what

⁴⁴ See for example: [Link](#)

⁴⁵ The Tigard Triangle Plan offers an interesting stand-alone case study as discussed in Appendix 2.

are often complex, multi-dimensional policy reforms that vary widely in scale, intent, and practical impact. By collapsing all reforms into just two categories, the analysis censors the magnitude of policy effects, leading to potentially misleading conclusions about their influence on housing supply and affordability.

A major shortcoming of this approach is its failure to account for qualifying conditions that often accompany nominally deregulatory actions—so-called “poison pills.” These caveats can nullify a reform’s intended impact, converting what appears to be a liberalization into a largely symbolic gesture. As discussed earlier with ADU reforms in Portland and California, a policy may look permissive in text but, in practice, remain highly restrictive due to layers of local limitations.

For example, a December 9, 2018 article in the *Mesquite News* describes an apparent easing of regulations on ADUs in Mesquite, TX. Yet the same article reveals that the ordinance simultaneously imposed stringent design and occupancy requirements, substantially limiting the feasibility of ADU construction.

Planning and Zoning suggested approval by special exception with the following requirements:

The minimum lot size shall be 21,780 square feet (one-half acre).

Property must have a minimum of four off-street parking spaces that are behind the front and exterior side building lines.

Property owner must live on the property.

The maximum area of the structure shall be 500 square feet, provided that on lots exceeding one acre and zoned agriculture, the maximum area shall be 1,000 square feet.⁴⁶

Each of these stipulations alone is burdensome, and they collectively compound the regulatory hurdles making widespread ADU adoption less likely. Without accounting for such countervailing provisions, Stacy et al.’s binary classification framework may misrepresent the real-world regulatory environment and overstate the extent of meaningful reform.

Additionally, consider a March 2, 2017, article from the *Cambridge Chronicle* on reforms in Cambridge, MA, which states: “To take full advantage of these boosts to the density limit, called the floor area ratio, developers would need to include a significant amount of housing in their new building, including affordable housing units.”⁴⁷ The specifics of the affordability requirements are not detailed, but they

⁴⁶ Article 178: Sivilay, Anny. “Mesquite approves ordinance regulating accessory.” *Mesquite News*, 9 Dec. 2018.

⁴⁷ Article 99: Sanna, James & Sennott, Adam. “CENTRAL square - council approves rezoning package.” *Cambridge Chronicle*, 2 Mar. 2017.

are likely substantial in Cambridge. Such mandates increase costs for the remaining units and may render some projects financially unviable.⁴⁸

The subtleties of these legislative texts are crucial—some of which might not be fully captured in news reports. Consider the following examples where cities or states seemingly loosened land use regulations but with stipulations that led to negligible impact on housing supply:

- Minneapolis abolished single-family zoning in 2018, but did not increase FAR or building heights significantly, thereby limiting its effectiveness (Hamilton 2020).
- Arlington, VA's 2023 upzoning permits only a total of 285 building permits over five years, effectively limiting the potential supply response (Barthel 2023).
- Regulations in some areas impose owner-occupancy requirements for accessory dwelling units (ADUs), making adding such a unit a less attractive option (Lemar 2022).
- Seattle legalized ADUs in 1994 and revised these regulations in 2009, but the stringent rules limited any significant increase in supply until further reforms were passed in 2019 (Carder 2023).

This lack of detailed consideration of restrictive caveats leads Stacy et al. to misunderstand the true impact of reforms on housing supply, emphasizing the need for a more comprehensive analysis of policy changes and their practical implications.

The authors' effort to create an authoritative panel dataset falls short by not accurately reflecting the true impact of different scale changes, comprehension of which requires a deeper contextual understanding than is often available from media sources.

For example, consider the likely varied impacts of the following less restrictive policy changes:

- Increasing building height from 1.5 to 2 or 3 stories,
- Raising the floor area ratio (FAR) from 50 percent to 70 percent or 140 percent,
- Reducing setbacks from 20 feet to 10 or 5 feet.

⁴⁸ For a review on the literature on inclusionary zoning (IZ), see <https://www.aei.org/wp-content/uploads/2024/05/Case-study-housing-abundance-traditional-housing-subsidy-programs-and-inclusionary-zoning-by-regulatory-mandate-5.13.24-FINAL-v2-1.pdf?x85095>.

It is noteworthy that the upzoning efforts in Houston, TX—widely considered a success (Hamilton 2020; Wegmann et al. 2023)—might have been less impactful had the minimum lot size reduction been from 5,000 sq. ft. to 2,500 sq. ft. (allowing two units per parcel), rather than to 1,400 sq. ft. (allowing three units or more per parcel) as was enacted.

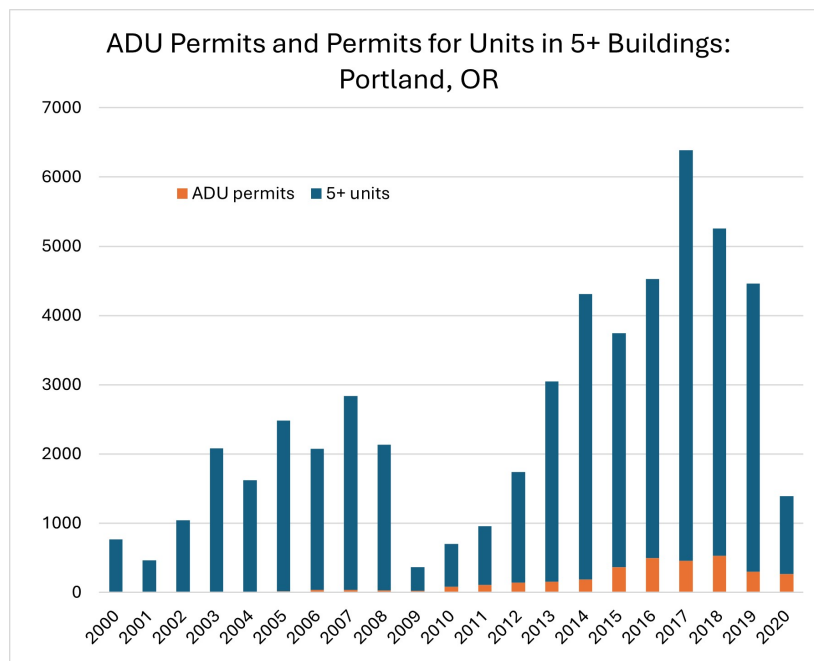
Problems with the USPS data

Stacy et al.’s reliance on United States Postal Service (USPS) address data (from HUD’s USPS Vacancy Dataset) introduces several important limitations that further weaken its ability to measure how zoning reforms affect housing supply.

1. Lack of Granularity

The USPS dataset reports only aggregate counts of deliverable residential addresses. It does not distinguish between housing types or identify whether new addresses stem from new construction, redevelopment, or conversions. Because most reforms in the Stacy et al. dataset are small-scale, single-family changes, their effects are likely too subtle to register in such coarse data.

For example, Portland’s ADU reforms led to a sharp rise in ADU permits starting in 2010 (as described above), but these permits represent a tiny fraction when compared to the total number of multifamily permits issued over the same period. Stacy et al.’s research design, which does not distinguish between the building type, risks conflating the ADU reform with broader unrelated multifamily permitting activity (see Figure 6). And while the authors assume their identified reforms are citywide, we have shown earlier that they are likely not. Although USPS data could be matched to finer geographies, the authors do not perform such disaggregation, missing potentially meaningful local variation.

Figure 6: Select Building Permits in Portland, OR by Type and Year

Source: AccessoryDwellings.org and HousingData.app.

2. Incomplete Coverage of ADUs

Roughly one-third (57 of 180) of the coded major reforms involve ADUs, yet ADUs are poorly captured in the USPS data. It is our understanding that the USPS tracks only active, deliverable addresses, typically assigned to detached, separately metered units. Most ADUs—especially attached or internal units—share an address with the main dwelling and, therefore, would go uncounted by USPS.⁴⁹

In short, while USPS address data can track broad housing trends, it is a blunt instrument for evaluating fine-grained zoning reforms and, therefore, likely understates the true effects of policies enabling incremental infill housing.

⁴⁹ We inquired with the relevant people at HUD on this issue, and they were unable to provide further explanation. Our own investigation reveals that ADUs are inconsistently captured in USPS-based datasets, as address assignment varies by jurisdiction and practice. In Los Angeles, detached ADUs often receive unique addresses, but Junior ADUs (JADUs) appear to be treated as part of the primary dwelling. Reviewing 20 visually verified Los Angeles ADUs completed in 2019–2020 from <https://www.aducalifornia.org/>, we found that a private dataset derived from USPS Address Management System records identified only 58% of them, confirming uneven representation of ADUs in USPS–HUD data.

This problem is compounded by Stacy et al.'s heavy concentration of ADU-related cases. Roughly 30 of the 180 total articles—nearly one in six—pertain to California's ADU laws, and 28 of those occur in 2017–2018, reflecting the wave of state-mandated compliance ordinances. As discussed earlier, many of these local measures contained restrictive “poison pills” which sharply limited their real impact. By counting each article as a separate “less restrictive” reform, Stacy et al.'s dataset gives disproportionate weight to these nominal changes—inflating the apparent number of pro-housing reforms while overstating their policy significance.

Other considerations

Even if every article in Stacy et al.'s dataset was correctly classified, the analysis would still face serious challenges from overlapping and contradictory policy actions. In many cases, multiple zoning amendments occurred within short intervals—sometimes moving in opposite directions—making it impossible to isolate the effect of more or less restrictive changes. For example, in Miami, FL, Stacy et al. record nine reforms between 2006 and 2011—two less restrictive and seven more restrictive—reflecting a complex mix of policy shifts during the housing boom and bust.

The authors also fail to account for important policy interactions outside its primary database, potentially omitting influential reforms not captured in the Access World News archive. In Philadelphia, PA, for instance, the authors identify two more restrictive land-use changes in 2006 and 2017, both occurring amid a major construction boom driven by the [city's well-designed tax abatement program](#) initiated in 2000. This example underscores the difficulty of disentangling the effects of zoning changes from other concurrent economic and policy forces.

A similar problem arises with ADU laws in California. Because the state's ADU legislation applied statewide, there was effectively no untreated control group within the state. Labeling jurisdictions without newspaper coverage as “no reform” creates a false contrast—these cities were equally bound by state mandates and may have implemented them quietly or with minimal press attention. This undermines the study's assumption that treatment and control areas differed only in local reform timing.

Conclusion

In sum, overlapping reforms, contradictory classifications, and missing contextual factors introduce substantial measurement error into Stacy et al.'s dataset. This biases down the estimated supply response and dilutes the statistical precision of the study's results. Together, this undermines the authors' central claim that zoning reforms can be cleanly quantified through automated media analysis.

A credible causal assessment of land-use policy requires verified sequencing, contextualization, and ground-truth validation—elements that Stacy et al.’s design falls short of providing.

After a thorough and painstaking review of all 180 cases, we found only two that plausibly qualify as “major” reforms—and even these warrant caution given uncertainty about whether their supply impact was measurable using Stacy et al.’s methodology.

These data problems are compounded by structural flaws in the study’s research design, including the use of a binary “more/less restrictive” classification that obscures policy nuance, limitations in the underlying address data, and the inability to account for policy changes not captured in media coverage. Collectively, these weaknesses call into question the validity of the study’s headline conclusion—that zoning reforms only modestly increase housing supply—and highlight the need for a more transparent, verifiable, and context-sensitive approach to measuring land-use change.

Taken together, we believe that these methodological weaknesses render Stacy et al.’s results unreliable as a measure of zoning reform impacts. Policy makers should be cautious in relying on Stacy et al. for insights into how to incentivize more housing supply. A credible analysis requires verified policy data, appropriate outcome metrics, and sensitivity to the economic and institutional environments in which reforms occur. For now, this combination is best accomplished by detailed case studies.

The Case for Case Studies

Our critique demonstrates some of the limitations of relying solely on news sources and machine-learning algorithms, even when they are reportedly reviewed by researchers to identify reforms and assess their outcomes. We have presented examples that illustrate the need to verify specific details and conditions of any land use changes. A superior method for accomplishing this objective is the case study approach. Carefully conducted, case studies provide the depth and continuity necessary to understand how reforms actually function, evolve, and influence housing outcomes over time. They also allow for multi-year evaluation windows, capturing both short-term adjustments and longer-run supply responses that large datasets tend to obscure.

No approach is without its vulnerabilities. We recognize that a case-study approach is vulnerable to charges of cherry-picking the cases selected for study. The concern suggests that inferences should be based on a wide range of case studies. One might contend that machine-learning algorithms operating on a population or a reasonable sample avoid such problems. But those methods, too, can be cherry-

picked and misrepresented; the semblance of random selection may be false. The upshot is that every approach is vulnerable, making critical review and replication essential.

At the AEI Housing Center, we have conducted over [two dozen detailed case studies](#) of local land-use reforms across U.S. jurisdictions (see Appendix 4 for the full list), along with additional instructive cases we identified while evaluating Stacy et al. (see Appendix 2). These analyses have illuminated how design details, administrative discretion, and market conditions shape the success—or failure—of reform efforts. These cases provide a useful template for other researchers interested in examining the effects of other local zoning reforms.

Accurate measurement of zoning-reform effects depends on getting the many details right—knowing what was passed, when it took effect, where it was applicable, and how it was implemented. At present, case studies remain the most reliable way to achieve this, offering the necessary acuity and depth to understand how land use reforms truly affect housing supply over time. We outline best practices for such case studies in Appendix 3.

We have conducted studies of 30 years (Palisades Park, NJ and Seattle, WA) and 50 years (Denver, CO), amongst others (for the full list see the Appendix 4). Such longitudinal case studies have shown that short and simple, by-right, density-enhancing reforms can produce sustained 1 to 2.5 percent annual growth in housing supply. In contrast, reforms that are procedural, symbolic, or highly conditional tend to have only negligible impacts.

Over time, the accumulation of careful case studies will allow researchers to build a more authoritative panel dataset on local housing policy. Advances in information technology may assist in this process by improving document retrieval and classification. Judgment by knowledgeable researchers will always be essential to assure contextual accuracy and interpretive depth.

Appendices

Appendix 1: Article Master List and Classification Comparison

The Stacy et al. Multi-City Panel of Land Use Reforms for the 180 policy changes—originally containing metadata and classification details for its 180 articles.⁵⁰ To ensure transparency and replicability, our appendix spreadsheet reproduces the core Stacy et al. dataset and supplements it with additional information, including each article’s verified URLs to Access World News, a comprehensive database of major newspapers by Newsbank (as provided by Stacy et al. 2023), our classification “waterfall,” notes on duplicative entries, and corrections

⁵⁰ See: <https://datacatalog.urban.org/dataset/multi-city-panel-land-use-reforms>.

for misattributed locations.⁵¹ The complete spreadsheet is available at the following link: <https://www.aei.org/research-products/one-pager/critique-of-panel-study-on-land-use-reforms-data-appendix/>.

Appendix 2: Where Stacy et al. Highlighted Some Interesting Cases that Were Misclassified

While the Stacy et al. dataset misclassifies many reforms, its raw coverage nonetheless surfaced several instructive cases that merit deeper study.

1. Aventura, FL:

The article related to this case should be disqualified as part of the Stacy et al. study because it was attributed to the wrong city. But as a stand-alone case study, it is interesting.

The relevant article, published in the *Miami Herald* on February 19, 2006, discusses a zoning change concerning building height limits.⁵² Local officials in Aventura argued that the city was “98% built out” and that remaining development capacity was straining traffic, infrastructure, and water resources. The mayor summarized the intent succinctly: “If anything, we are making it harder to redevelop.” A city commissioner added: “Everything we are doing is to limit growth in the future.”

The reform was clearly more restrictive, reducing allowable building heights and densities by sizeable margins. Given that Aventura was already largely built out by 2006, the policy primarily affected potential high-rise redevelopment projects. This pattern is consistent with the residential year-built data, which show far fewer multi-family structures constructed in the 2010s compared to earlier decades—particularly relative to the 1990s (purple) and 2000s (pink) cohorts in Figure 7. Surrounding cities, by contrast, show substantial new multifamily activity (many red dots), suggesting a promising difference-in-differences setting which is, however, beyond the scope of this paper.

2. Tigard, OR (Tigard Triangle Plan):

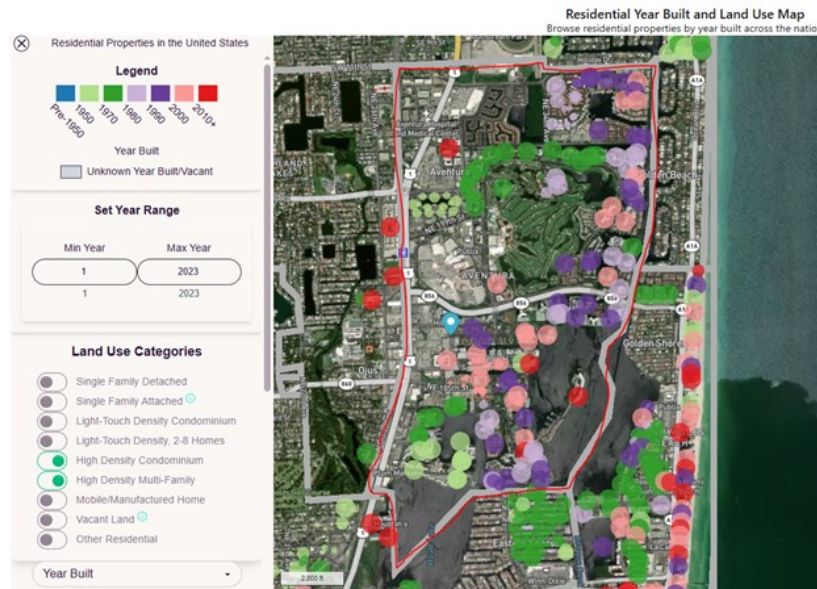
The Tigard Triangle Plan (2018) was correctly classified by Stacy et al. as a “less restrictive” reform relating to density and Floor Area Ratio (FAR). It represents genuine deregulatory zoning reform—one of few examples in the Stacy et al.

⁵¹ The articles are available through a full or trial license to Access World News.

⁵² Article 60: Teproff, C. “New Laws Strengthen Restrictions.” *The Miami Herald* (FL), February 19, 2006, p. 10NE.

dataset of a city intentionally removing regulatory barriers to facilitate mixed-use residential development. It was, however, not municipality-wide, and only covered approximately 500 acres out of a city of approximately 7,500 acres. Its “Lean Code” for the Triangle was adopted in August 2018 and sought to simplify zoning, streamline approvals, and eliminate several long-standing constraints on development intensity.

Figure 7: High Density Condominium and Multi-Family Buildings by Decade Built



Source: AEI Housing Center.

As the *Tigard-Tualatin-Sherwood Times* reporting explained:

The city of Tigard is moving ahead with plans to implement a new zoning district designed specifically for mixed-use zoning in what’s known as the Tigard Triangle.

The new zoning, adopted in August, is referred to as a so-called ‘lean code,’ making it easier for developers to receive approval for their projects. It specifically regulates development in the new Tigard Triangle mixed-use zone.

It’s lean because the approval process has been streamlined and development standards have been reduced or eliminated, e.g., no minimum parking requirement and no floor area ratio (FAR), which limited how big a building could be relative to the size of the lot.

One highlight of the lean code is that it will allow individual developers to determine how much parking they will need, something that is unique in planning circles.⁵³

Tigard's Lean Code created true by-right capacity short and simple process in a fairly large, contiguous redevelopment area. The reform applied to a relatively small area and was enacted in 2018, near the end of the study period. Because implementation lagged and most new development involved multifamily housing, the resulting increase in units likely occurred after the Stacy et al. observation window, meaning its effects are not fully captured in their dataset.

Satellite imagery confirms that no major residential structures were completed in the Tigard Triangle between the 2018 zoning change and the end of 2020. Since then, however, at least eight multifamily buildings have been completed, with another under construction—together adding roughly 750 housing units to a census tract that had only about 1,600 residents in 2020.⁵⁴

It is also important to note that the area was designated as a federal Opportunity Zone under the 2017 Tax Cuts and Jobs Act. This designation likely influenced the timing and financing of subsequent development, further complicating efforts to isolate the specific effects of the zoning reform itself.

3. Waxahachie, TX

According to the article “City amends lot size rules” in the *Waxahachie Daily Light*, the city reportedly adopted a zoning amendment in 2013 that increased the minimum lot size in the SF-1 (single family, large lot) district from 12,500 to 16,000 square feet and stated that SF-2 (medium lot) and SF-3 (small lot) also face higher minimum lot-size standards under the update.⁵⁵ Stacy et al. classifies this as a major, more restrictive reform.

However, a closer review of zoning maps, municipal records, and residential development patterns reveals little evidence that the change affected actual development outcomes. The SF-1, SF-2, and SF-3 zones together cover only about a quarter of the city's land area, and most post-2013 housing construction occurred on smaller lots.⁵⁶ This suggests that developers relied primarily on Planned Unit Developments (PUDs), which provide flexibility with base zoning standards.

⁵³ Article 149: Pitz, R. “City Moves Forward with ‘Lean Code’ for Tigard Triangle.” *Tigard-Tualatin-Sherwood Times* (OR), 1 Feb. 2018.

⁵⁴ Due to data limitations of the tax assessor data, we rely solely on satellite images and data from various rental sites for the unit count in each newly built building.

⁵⁵ Article 179: Staff Reports. “City Amends Lot Size Rules.” *Waxahachie Daily Light* (TX), 31 Jan. 2013.

⁵⁶ Using today's zoning map from Zoneomics, we estimate that 23% of land area and about 30% of residential units are in areas zoned SF-1, SF-2, or SF-3.

Permit data (Figure 9) and aerial time-lapse imagery overlaid with assessor records show no discernible shift in development patterns following 2013. Moreover, a search of city records found no confirmed 2013 ordinance, though a 2018 zoning amendment was verified. The city’s current zoning map (Figure 10) further indicates that the SF-1, SF-2, and SF-3 districts today encompass only small pockets of the municipality.

Figure 8: Satellite images from the Tigard Triangle in 2025



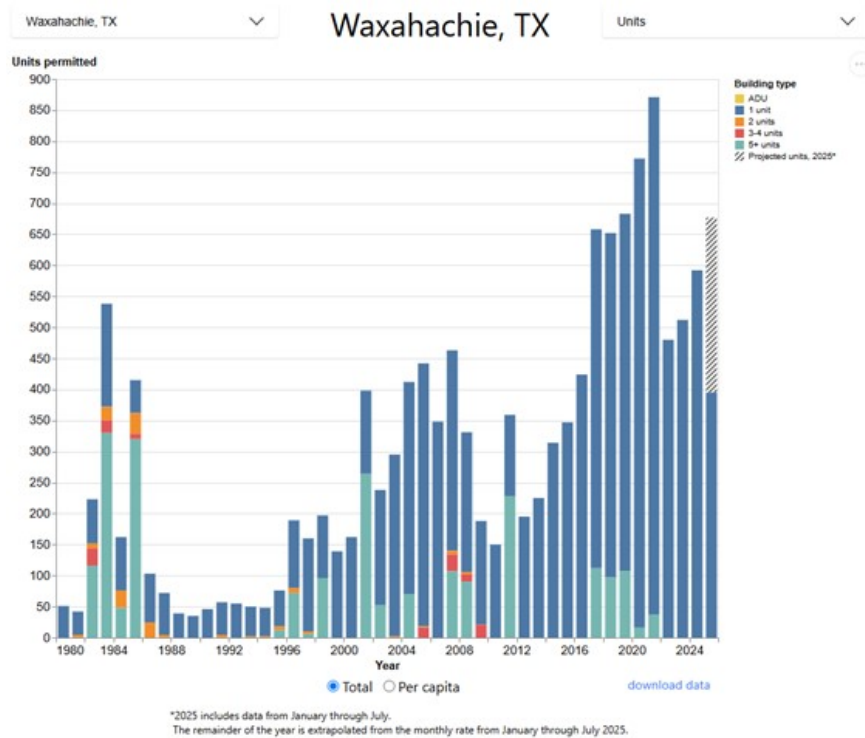
Legend: Red dots refer to structures built 2020–2025, yellow dot refers to a structure under construction. Numbers next to each dot refer to the approximate unit count in each building. Note that no structures were completed from the time of the zoning change to the end of the Stacy et al. sample period in 2020.

Source: Google Earth, Apartments.com, RentCafe, Reach Community Development, Fore Property, and AEI Housing Center.

A visual inspection of housing completions by year built and lot size (2000–2012 vs. 2013–2025) demonstrates that most new homes continue to be built on lots well below 16,000 square feet, reinforcing that any 2013 change—if enacted—had minimal practical effect (See Figures 11a and 11b). In all likelihood, Waxahachie’s development patterns were not shaped by the purported 2013 tightening reform, but rather by steady growth driven through PUD-based projects and other zoning categories outside the affected districts. As such, the reported 2013 amendment—if it was ever formally enacted—appears nominal rather than consequential.

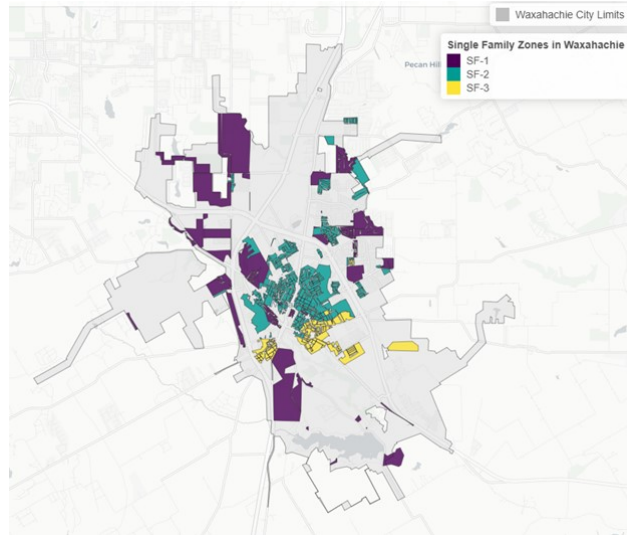
This case also illustrates how the AEI Housing Center’s freely available data tools can help researchers quickly analyze and visualize complex local housing patterns. Within minutes, users can identify zoning inconsistencies, assess development trends, and gain insights that would otherwise require significant time and technical effort to replicate independently.

Figure 9: Permit data for Waxahachie, TX



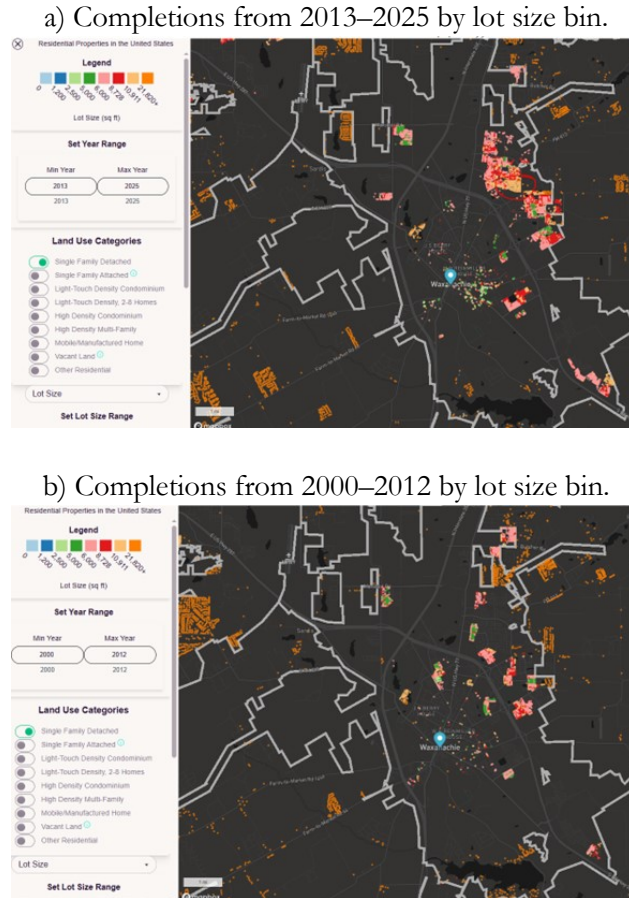
Source: HousingData.app.

Figure 10: Current zoning map for SF-1, SF-2, and SF-3 in Waxahachie, TX



Source: Zoncomics, First American, and AEI Housing Center.

Figure 11: Single family Detached Residential Buildings by Year Built and Lot Size



Source: AEI Housing Center.

Appendix 3: Best practices on case studies

Our approach combines the best available data, careful document verification, and deep contextual research. This is a time-intensive process that integrates legal, economic, and empirical analysis to ensure that every identified policy change represents a substantive and verifiable reform.

Criteria for a Major Policy Change

For a reform to qualify as major, it must meet several core conditions:

- **Verifiable adoption:** The policy must be traceable to an officially adopted ordinance, resolution, or amendment, with clear pre- and post-periods that can be reviewed for observable outcomes.
- **Meaningful scope:** The change must be significant enough—either in the size of the area affected, regulatory change, or deregulatory effect—to potentially influence housing supply or development patterns.
- **Supporting evidence:** There must be empirical data or credible documentation to confirm that the change occurred, had the potential to alter development incentives, and resulted in a supply response.

To verify this, we rely on the following:

- Legal verification: We begin by confirming each reform through primary legal sources, such as city legislative or planning websites. The devil is in the details:
 - Identify the ordinance number, adoption date, and effective date.
 - * If no adopted ordinance can be located, the reform is not considered real.
 - Retrieve the amended zoning text or the prior version for comparison.
 - Review zoning maps before and after the reform to determine its geographic reach.
 - Use Zoneomics and other databases to verify current zoning designations and confirm alignment with reported changes.
- Checking on actual implementation: Even when passed, zoning changes often have limited or delayed implementation. We therefore:
 - Examine the effective date and geographic scope (citywide or certain areas) of each reform.
 - Assess whether the change is by right or discretionary.
 - Examine whether the rules are short and simple or complex.
 - Look for restrictive caveats or “poison pills,” such as affordability mandates or parking requirements that may offset intended flexibility.
 - Consider economic feasibility: A reform allowing high-rise towers “by right” in a rural or low-demand area has little practical relevance.

- Classify as genuine a reform that is by-right, broad enough in scope, applies the “keep it short and simple” (KISS) principle by minimizing permitting complexity, regulatory hurdles, and construction costs and is economically meaningful.
 - When possible, consult local builders.
 - Consider local and macro market conditions.
- Evidence of uptake: Finally, we look for empirical evidence that the reform had an observable impact. Using the best available data for the affected area, we:
 - Compare building permit volumes before and after reform adoption.
 - Examine assessor data to determine whether parcel characteristics (e.g., lot sizes, unit counts) changed over time.
 - Use historical assessor datasets or lot shapefiles to trace long-term patterns.
 - Map new housing construction or permits pre- and post-reform in affected and nearby areas.
 - Use Google Street View or Google Earth imagery to visually confirm redevelopment activity.

We then assess whether homes built after the reform differ meaningfully from those built before—confirming that zoning changes, rather than unrelated trends, drove observed development.

- Historical and textual context:
 - Each case study is grounded in a review of contemporaneous sources—including news articles, city council hearing minutes, and planning reports—to understand the historical and political motivations behind the change. This context helps ensure that each identified reform is accurately characterized and properly timed.

In short, our case study methodology blends legal documentation, market analysis, and on-the-ground verification to distinguish real, consequential zoning reforms from symbolic, procedural, or economically irrelevant actions. This ensures that only policies capable of influencing housing outcomes are treated as meaningful reforms.

Appendix 4: List of AEI Housing Center case studies

List of AEI Housing Center case studies that have quantified the supply response as a percent of the existing single-family housing stock in the same areas:

| Area | Period Studied | Supply Response (per year) |
|-------------------------------|----------------|---|
| Cherry Creek, CO | 1970–2022 | 1.5% |
| Palisades Park, NJ | 2000–2019 | 1.1% |
| Tokyo, Japan | 1963– | 1.5% |
| Seattle, WA | 1994–2024 | 2.5% |
| McMansionization – nationally | 1990–2024 | Depending on the land share, we estimate that between 0.33% to 1.33% of economically viable single-family detached homes are converted to a McMansion each year. Similar percentages should apply to light-touch density infill conversion. ⁵⁷ |

⁵⁷ Note that if 1% of economically viable parcels are converted from a single-family house to four town-houses each year, the housing stock expands by 3% each year. If 0.5% are converted at a rate of 3:1, the housing stock expands by 1%.

List of other case studies that have quantified the impact of light-touch density supply additions but not as a percent of the existing single-family housing stock in the same areas (from AEI Housing Center unless otherwise noted):

| Area | Period Studied |
|---|-----------------------|
| California | 2000–2022 |
| Nashville, TN | 2010–2023 |
| Philadelphia, PA | 1980–2024 |
| Sarasota, FL | 1980–2023 |
| Anaheim, CA | 2005–2023 |
| Charlotte, NC | 2000–2020 |
| City of Denver, CO | 2000–2021 |
| Houston, TX | 2005–2018 |
| McMansionization in Los Angeles Metro, CA | 1950–2020 |
| San Jose Metro, CA | 2012–2022 |
| Sarasota, FL | 1990–2023 |
| Minneapolis, MN | 1900–2023 |
| Phoenix, AZ | 2000–2023 |
| San Diego, CA ADU Construction | 2021–2023 |
| Utah | 2000–2023 |
| Los Angeles, CA | 1875–2023 |

List of other AEI Housing Center case studies:

| Area | Period Studied |
|--|----------------|
| Denver, CO | 1980–2024 |
| Fargo, ND | 2000–2023 |
| Filtering: Theory and Practice | 1960–2022 |
| Houston, TX Townhome Reforms | 1998–2020 |
| Institutional Landlords | 2000–2021 |
| Los Angeles, CA | 1980–2024 |
| Raleigh, NC | 1960–2023 |
| Seattle, WA and Charlotte, NC | 1990s–2023 |
| Short-Term Rentals | 2015–2023 |
| Single-Room Occupancy Units (SROs) | 1910–2023 |
| Traditional Housing Subsidy Programs and Inclusionary Zoning | 2000–2023 |
| Unleashing the Swarm | 1990s–2023 |
| Vienna, Austria | Pre-1919–2022 |
| Houston, TX Lot-Size Reform | 1998–2025 |

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