# By Generation and by Design: A Review of the Empirical Rent Control Literature

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**Abstract**

This work extends Kholodilin's (2024) review of 169 studies by coding each of the studied regimes by its design features. The relatively loose conventional terms "first and second generation" compress real variation in rent control regimes, and I propose coding on design features as a refined alternative.

*Chapter 2 of the author's dissertation, Second Generation Rent Control and New Construction, The Graduate Center, City University of New York, 2026. Page numbers below are the chapter's own.*

# 1 Introduction

There has been a recent revival in rent control policies across the world, in the United States, Berlin, Catalonia and Ireland. These policies, like many post-war rent regulations, have been characterized as second-generation, being more lenient than the more strict first-generation policies which had fewer allowances for increases and exemptions. There has long been a consensus among economists about the effects of rent control more broadly (Arnott 1995), but there has not been a systematic review of the literature of this kind that distinguishes the evidence between these two broad regime classifications, although some reviews with different structures do draw attention to this distinction (cf. Turner and Malpezzi 2003). Moreover, the design features of rent control policies can vary widely, and the coarse generation framing does not appropriately distinguish these features. Exemptions for new construction, allowable yearly rent increases, and vacancy decontrol are all hallmarks of a more lenient second-generation rent control policy, but many policies considered to be second generation have only some of these elements and not others, making them more similar to nominally first-generation policies than they are to one another. Policymakers would benefit from knowing the evidence of rent control when distinguished first from second generation, and furthermore how the reported evidence differs across particular policy elements.

In this paper I extend work from Kholodilin (2024), which summarizes the literature of rent control, by coding the rent control policies studied in each paper at the design feature level. For example, noting whether a given policy has a full exemption for new construction and for how long. To my knowledge, this is the first systematic study-level design-feature coding of this corpus (cf. the regime-level typologies of Lind 2001; Weber and Lee 2020), and the first statistical moderation and exemption tests on it. I also run a sequence of sensitivity tests to better establish the confidence in the effects measured throughout the literature.

The literature finds controlled rents fall, uncontrolled rents rise, mobility falls (stability rises), and quality falls in each generation, coding, and collection of the literature studied, and homeownership tends to rise. Under regime-level generation coding, mixed becomes the largest category, and new

construction exemptions are widespread even in what would typically be considered first-generation rent control. Rent control generations are too coarse to capture the variation in policy design, suggesting generation terms are less generally useful than previously thought. In comparisons of prominent design features (exemption strength, vacancy decontrol, rent cap form, and eviction protection bundling), the hardest versions align with the theory-predicted signs, while the softer versions do not consistently align, particularly when it comes to new construction. A check of reported signs by publication status finds no differences, though the test has little power and cannot speak to work never circulated.

This chapter complements my causal empirical work (Chapter 1) studying the passage of Oregon's SB 608, a rent control law with new construction exemption, lenient allowances for yearly rent increases, and allowance for vacancy decontrol. My political economy work (Chapter 3) explores the proliferation of rent control preemption laws throughout the United States and the factors that may have influenced their passage.

## 2 Background: three generations of rent control

The introduction of the term second-generation rent controls was in Blumberg et al. (1974), defined as peacetime rent controls that were not absolute rent freezes, and instead allowed restricted rent increases. Arnott (1995) popularized the dichotomy between first- and second-generation controls (or hard and soft controls) within the economics discipline. While freezes were the hallmark feature of the harder first-generation controls, a set of other design features "governing not only allowable rent increases, but also conversion, maintenance, and landlord-tenant relations" (Arnott 1995, 102) became associated with softer second-generation controls. Arnott (2003) extends the tenancy rent control of Basu and Emerson (2000), which introduced it as a special case of second-generation controls, into a generation of its own: third generation. These are policies where "rent increases are controlled within a tenancy but are unrestricted between tenancies" (Arnott 2003, 92), or "vacancy decontrol" in the language of this paper. Kettunen and Ruonavaara (2021) apply this classification

in a survey of European rental regulation: if both initial rents and rent increases are regulated, the policy is coded as second generation, and if only rent increases are regulated, it is coded as third generation (Kettunen and Ruonavaara 2021, 1451). While the use of generation labels is commonplace, their implementation is ambiguous at times. Kettunen and Ruonavaara (2021) find that the "distinction between second- and third-generation rent control was not clear-cut in all cases" (Kettunen and Ruonavaara 2021, 1451), an ambiguity that also arose in the generation coding for this paper. For alignment with Kholodilin (2024), this paper sticks to the two-generation framework with third generation already implicitly being a subdivision of second-generation controls (I include the main results under this specification as well, Figure 7). In cases where design features combined elements of both first and second generation, I coded the regime as mixed.

The study of rent control has developed around several different generalized descriptors, which refer to similar classes of rent control policies. If a rent control policy has strict caps on the nominal rent price, and few avenues to reset these nominal rent prices to the market rate, it would be described as first generation, or hard, rent control. This is in contrast with second generation, or soft, rent control which generally does not have firm price caps, and has many potential avenues to reset prices to the market rate, such as when a tenant moves out, which is referred to as vacancy decontrol. On occasion the first versus second generation split is referred to as "rent control" for the more strict first-generation policies and "rent stabilization" for the more lenient second-generation policies; to avoid confusion this paper will not use these terms.

While the generation terminology does gesture towards some important key policy details, there are other important features which can be silently obscured. The first being the coverage of a rent control policy within a region, even a very strict first-generation policy may only affect a relatively small proportion of the rental units within a city depending on its design, or indeed the rental market may be a relatively small proportion of the total housing stock of the region. As a consequence, the particular design features which dictate a rent control policy's coverage could be more influential on policy outcomes than the first- versus second-generation decision.

The second partially obscured design detail is the "bite" of a rent control policy, meaning the difference between the maximum allowable rent for a given apartment under a rent control policy and an unobserved counterfactual; the market rate rent that would be charged absent rent control. While it might be fair to assume most first-generation rent control policies tend to have a larger bite than second-generation rent control policies, that might not always be true. A first-generation rent control might set nominal caps just marginally below the market rent for a particular unit, and its bite would be quite small, whereas a second-generation rent control policy may allow for 2 percent yearly increases during a period of high inflation or housing shortage, and its bite could be much more significant.

Lastly, many rent control policies combine features of both first and second generation. For example, a rent control policy might allow for yearly price increases, but not offer any opportunities to reset to the market rent. While variation in coverage and bite is largely not captured in this paper, many other particular features of rent control policies have been included to give a more textured view of the existing evidence the literature has of the effects of rent control. To highlight some particular examples, many modern European rent control policies do not include vacancy decontrol; the German Mietpreisbremse and the Irish Rent Pressure Zones do not allow for the market price to reset to the current market price after a tenant leaves (Mense et al. 2023; Ahrens et al. 2019). These policies would otherwise fall fairly cleanly into a second-generation categorization, but lacking vacancy decontrol leaves them straddling these two generation definitions. This is unsatisfactory both in that these policies cannot be coded cleanly with the current terminology, and that the literature cannot guide policymakers without presenting evidence on the measurable effects of similar mixed or hybrid rent control policies. This paper consolidates this evidence which to this point is not decomposed by generation and by design feature.

One particular feature of interest is the exemption of new construction. Many rent control policies allow for new construction of rental units to be exempted entirely and indefinitely, or allow for exemption for a specific period of time. In practice, the former is often implemented by defining rent

control coverage by the year a building was built. In such cases, rent control would apply to rental units in buildings built before, say, 1979, but not after. This means that a newly constructed building would never be hit by this rent control policy. In the latter case, there might be an exemption for 30 years, and thus has 30 years to charge market rates before being affected by the rent control law. There are numerous other forms of partial exemptions, such as for small buildings with less than a particular number of units, while all others are covered by rent control. These exemptions are of particular note because of an already well-documented effect of many rent control policies: rents in uncontrolled properties will tend to rise (Fallis and Smith 1984; Early and Phelps 1999). Rent control in these instances, and with new construction exempted, increases the revenues that can be earned building and renting out a new building. This release valve in the housing market may reduce some of the negative unintended effects of rent control, namely a reduction in the quantity of housing supplied. This potential for new construction can only be realized if supply is relatively unencumbered, meaning that other constraints to new construction, like restrictive zoning, FAR restrictions, minimum parking requirements, historic designation, or height requirements, do not bind too tightly. In other words, these exemptions rely on supply responding to the increases in price seen in the uncontrolled sector. The effect of rent control in this case, and all others, is therefore contingent on the existing regulatory framework.

The mechanism above describes a situation where new construction could be made to increase as a consequence of a particular form of rent control; however, even in this case there is an effect cutting in the opposite direction. When a new rent control law is passed, even if new construction is currently exempt, there might be concerns that the law will be changed or a new law passed that makes previously exempted buildings now covered. If these fears are genuine, it may reduce new construction when a rent control policy is implemented even though these new buildings would be exempt. A long-standing rent control policy with clearly defined coverage and scope would likely suffer less from this effect than a rapidly changing policy regime. Beyond that, there may be attempts at credible commitment devices to go even further still, binding the ability, or making it more politically costly, to implement changes to a rent control policy. In Chapter 3, I consider one

such device, rent control preemption laws which states across the United States have implemented to prevent cities within those states from adopting rent control policies.

# 3 The corpus and the design coding

This paper follows and extends Kholodilin (2024) which reviews the state of the rent control literature and presents the signs associated with many key categories of outcomes from each of 169 studies. These recorded outcomes were verified against each paper; this surfaced a small number of discrepancies and double-listings (documented in the replication materials), but these few did not meaningfully change the interpretations and conclusions found in Kholodilin (2024). Relevant excerpts from each paper were gathered throughout this process, to provide reference for verification.

Each paper was then grouped and coded with a regime key for papers that studied the same rent control regime. Generally this meant that each region with a distinct rent control regime would have one regime key that each paper studying that regime would inherit characteristics from. On occasions when there was a major change to the rent control regime, papers studying the new regime received a distinct key from the old regime to preserve those new characteristics. For each regime several key features would be coded:

1. Exemption presence, window and strength: These codes capture whether new construction is exempt and for how long that exemption lasts. Strength indicates whether new construction is fully exempt or only partially exempted from the rent control.

2. Vacancy and renovation decontrol: These two codes note whether rents are allowed to reset to the market rate when a tenant leaves or when renovations are done to the unit or building.

3. Cap form and level: These codes describe the way a rent controlled unit's price is governed; this could be a nominal price freeze in the simplest case to percentage allowances, CPI plus a percentage allowance or more bespoke indices.

4. Coverage and intensity: These two roughly capture the coverage and bite, described earlier, if reported and measured by a paper for a given rent control regime. These codes are most affected by data availability and are more sparsely populated than the other features.

5. Generation: Generation coding is done at the regime level assigning first, mixed or second generation. Where market rents are reachable between tenancies, the regime is generally considered second generation. For regimes where control persists across tenancies, the balance of the remaining design features, adjustable caps versus effective freezes and new construction exemptions, determines whether the regime is placed as first generation or mixed. Headline analyses continue to use the Kholodilin-coded generation tags, with this regime-level coding entering the generation comparisons of later sections.

Oregon's SB 608 (2019) is an exemplar of softer rent control design. New construction is completely exempt for a rolling 15-year window: market prices are untouched both in how they are set initially and in the rate of increase. For buildings older than 15 years, landlords cannot raise rents by more than 7 percent plus CPI for a given year while a tenant remains in the unit. There is vacancy decontrol; thus a tenant leaving resets the rent for that unit to the market rate. This is paired with just-cause eviction such that tenants cannot be evicted without cause to reset rents to the market price. While there is an option for no-cause eviction within the first year, this eviction does not trigger the vacancy decontrol, and rent increases remain controlled by the 7 percent plus CPI limit. Only when a tenant voluntarily leaves does the vacancy decontrol activate. SB 608 does not provide a renovation or improvement decontrol, and renovation does not affect the 15-year exemption period for new construction. This collection of design features would comfortably fall under the definition of second-generation rent control, but would this still be considered second generation absent the vacancy decontrol feature? Alternatively, what if the exemption for new construction was eliminated? If prices were capped rather than allowed to increase, could it still be considered second generation? Of the 93 regimes coded throughout the literature, 31 were coded "mixed" generation under the design coding described above, so this is a practical issue with analysis that solely relies

on an imprecise definition of rent control regime generation.

Each study's findings are coded by outcome family with the effect sign recorded. These families are inherited from Kholodilin (2024) and the eight most common are presented in Table 1. The coverage of the literature in terms of the outcome families measured is unevenly distributed. Controlled rents are well represented but construction and rental supply, the outcome families on which many public policy debates hinge, are relatively thin.

This paper, following Kholodilin (2024), treats the sign in an outcome family as the main finding. While magnitudes would be theoretically preferable, the diversity of the types of studies, contexts and methodologies would make this impractical. By way of example, new construction could be measured in number of permits, units, buildings, bedrooms or square feet of housing to say nothing of the various contexts each study is based in. Therefore the sign, positive, negative, or null, is taken as this paper's main set of outcomes from each study coded here. Every coded sign corresponds to a specific result from each paper associated with a family of outcomes; some papers may result in multiple coded signs across several families, but never multiple in the same family, apart from a handful of cells where Kholodilin's own table records two signs for one study and family; U1, defined below, preserves those as published, and the pooled tests collapse them to one vote. Excluded here are studies without a clear empirical basis for their estimates, as are implied or assumed results. For example, while lower mobility might imply misallocation, if misallocation itself is not empirically estimated, it would not be coded. Meta-studies and literature reviews are also excluded to avoid issues of double-counting, and to make the main results of this paper more easily interpretable. The signs coded from the literature are then compared within families, presented visually in Figure 1 and using the Fisher exact test (Freeman and Halton 1951; Mehta and Patel 1983) which is appropriate for small sample sizes to measure the association of design features with the main outcome families. Appendix B contains a pooled summary model and a sensitivity battery (Figure 5). U1 contains the cells published in Kholodilin's Table 2, with my sign corrections, and provides the paper's main statistical comparison. U2 was built by recoding the

same studies as Kholodilin (2024) codes, essentially a verification of the existing coding from the studies themselves, termed Kholodilin-coded and Thorne-coded throughout this paper. U3 adds a sweep of new studies that were not included in Kholodilin (2024) as they were largely produced after its writing and publication; I code these in the same manner as U2 (Table 7). These labels of a collection of papers should not be confused with the coding of the design feature or rent control generation. For instance, a paper might be in the Kholodilin collection of papers, but have the design features of its rent control policy coded by me.

Table 1: The eight headline outcome families: cells, studies, and Kholodilin-coded generation tags.

| Outcome family | Cells | Studies | Gen-1 | Gen-2 | Other/unknown |
|---|---|---|---|---|---|
| Controlled rents | 57 | 55 | 24 | 29 | 4 |
| Uncontrolled rents | 18 | 18 | 8 | 7 | 3 |
| New construction | 14 | 14 | 3 | 7 | 4 |
| Rental supply | 18 | 18 | 7 | 7 | 4 |
| Quality | 23 | 21 | 13 | 9 | 1 |
| Mobility | 27 | 26 | 15 | 10 | 2 |
| Misallocation | 13 | 13 | 7 | 6 | 0 |
| Homeownership | 22 | 22 | 10 | 9 | 3 |

# 4 The evidence by outcome family

Figure 1 shows the Kholodilin (U1) collection, split by Kholodilin generation coding. While this generation coding was present, pooling the results by generation had not been done in Kholodilin (2024). New construction and supply have the largest shift in distribution from first to second generation: less clearly negative, and closer to a null effect overall.

As verification, the same Kholodilin (2024) (U1) papers were reanalyzed in line with how the new Thorne-coded papers would be done to form U2. Sign disagreements were rare, with three corrections among roughly 275 verified cells. U2 is not a superset of U1: it adds 103 verified

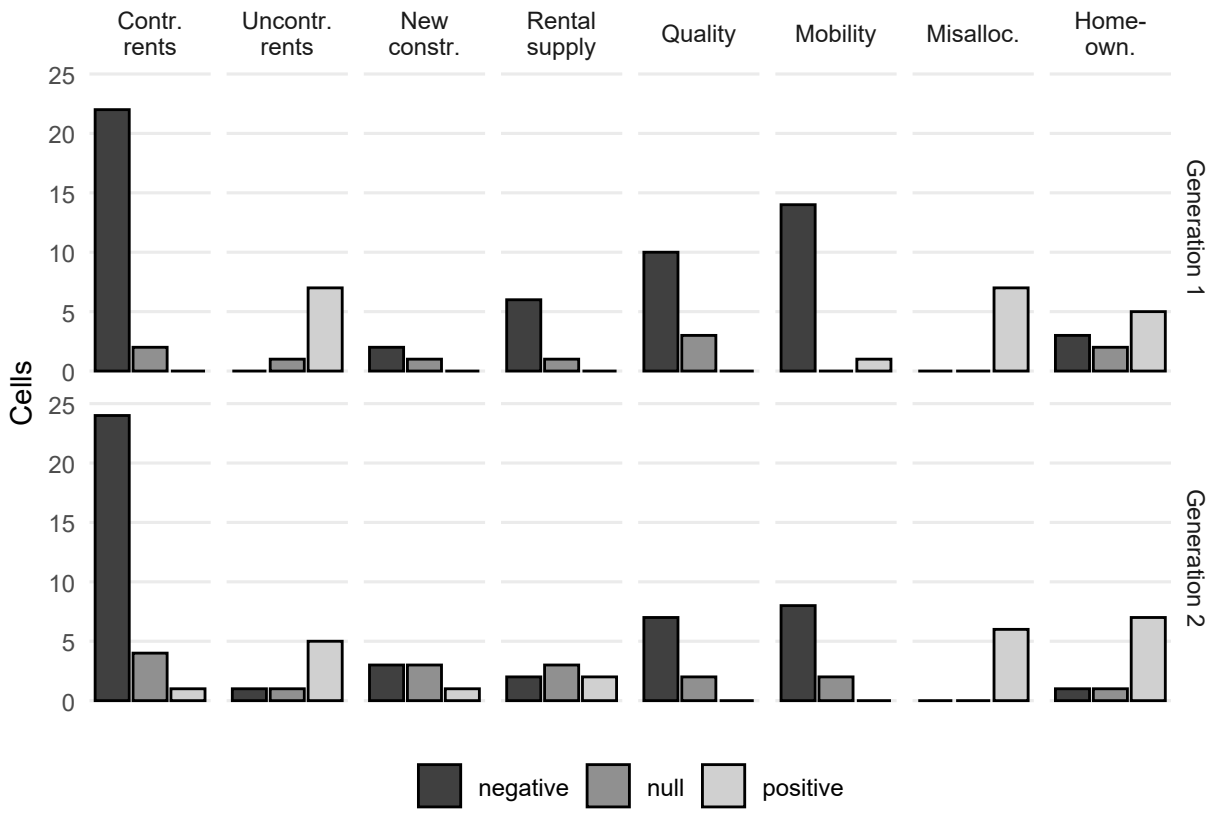


Figure 1: Sign distributions by outcome family and generation.

Notes: The corpus as published, under Kholodilin's generation tags. Bars count the reported direction of each estimate, not desirability; a sign's meaning differs by outcome family.

findings that Kholodilin's table did not carry, but it does not carry 34 of the U1 cells. Most of these cells do not contain a countable empirical estimate and therefore do not meet the inclusion rule. The remainder come from two studies I could not obtain, a handful of recorded claims I could not locate in the cited papers, and one corrected sign. The added layer leans less negative than the published cells in several families, consistent with a published table favoring signed results, but no family's direction reverses; Table 8 gives the family-by-family comparison.

New findings in U2 inherit the rent control generation Kholodilin assigned to those papers' other findings, provided all of the study's tagged findings carry the same generation. In cases where no generation is coded, or multiple are, it is largely because those studies cover multiple areas, regions or regimes and cannot be consistently coded; these are left as "Untagged" in Figure 2.

Overall Figure 2 shows that the family and sign patterns observed in U1 carry over into U2 (the strict as-published version, without inheritance, is Figure 6 in Appendix B). Each paper still contributes at most one finding per outcome family. The additional findings in U2 are in outcome families the U1 record did not include.

The first two rows of Figure 3 show generation coding differences on the same set of papers, between my coding and Kholodilin's. I introduce a "mixed" regime coding where there is a substantial blend between canonical first-generation and second-generation design features.

After the recoding, roughly half of the totals for first and second generation fell under the new mixed category (Table 3). This highlights that the generation terminology, while useful rhetorically, does not fully capture the variation across existing rent control regimes.

Row C includes the papers from Kholodilin (2024), with expanded coverage (U2) and the addition of new papers not included in the Kholodilin 2024 study. This encompasses the broadest collection of studies known to me and available at the time of writing. The largest growth in coverage is in the second generation and mixed categories, which aligns with the view that second-generation rent control policies are becoming more common.

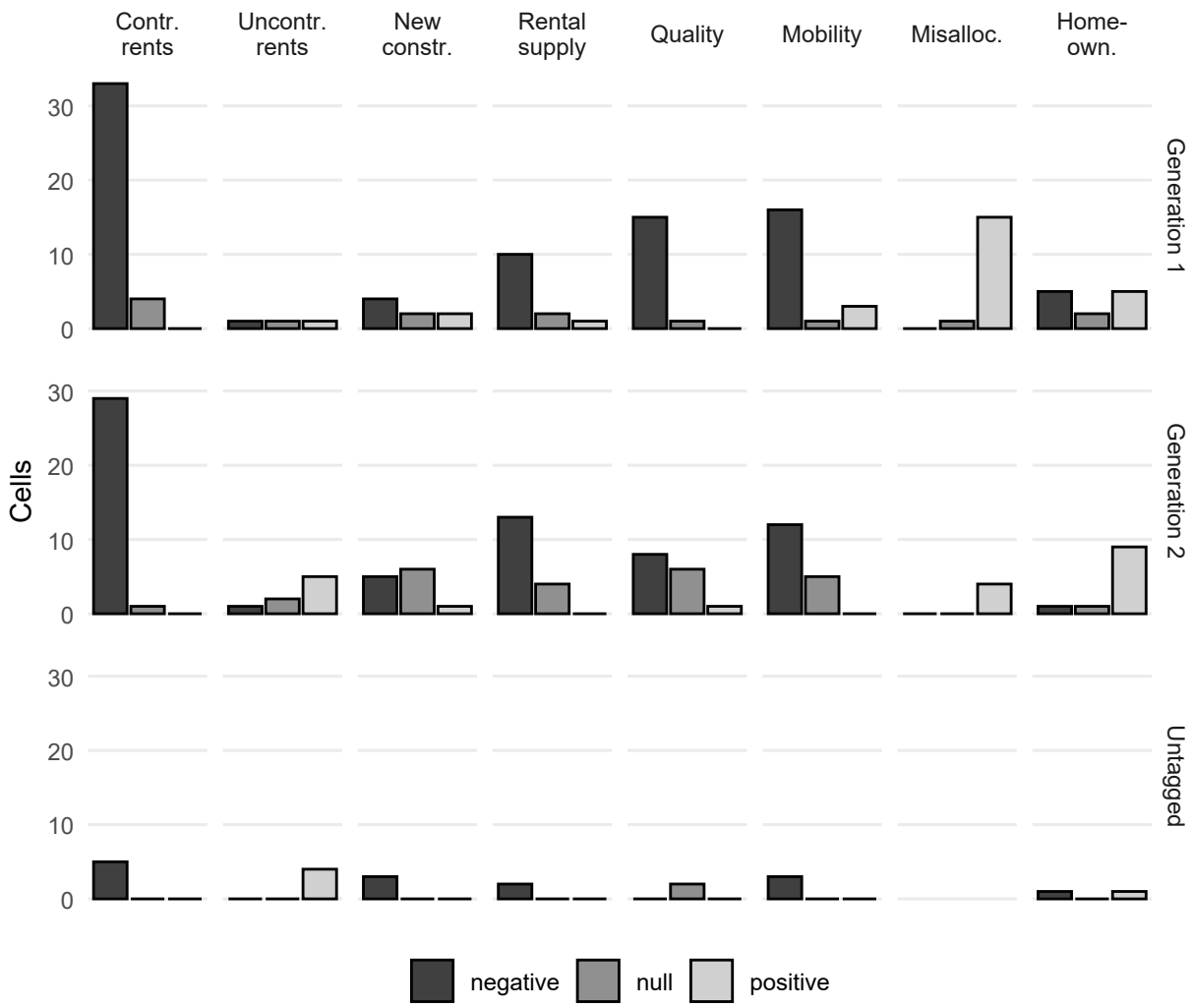


Figure 2: Sign distributions on the verification-expanded universe. U2 by outcome family and Kholodilin-coded generation with unanimous-study inheritance: an untagged cell takes its study's generation when all of the study's tagged cells agree (93 of 114); cells of multi-generation or never-tagged studies stay in the untagged row. A descriptive panel; bars count the reported direction of each estimate. The strict as-published version is Figure 6 in the appendix.

Table 2: U1 represents Kholodilin's published coding, broken down by his regime coding.

| Outcome family | Dir. | Gen-1 | Mixed | Gen-2 | Uncl. | p (1 v 2) | p (1/m/2) |
|---|---|---|---|---|---|---|---|
| **Kholodilin coding, U1 (as published)** | | | | | | | |
| Controlled rents | − | 22/24 | – | 24/29 | 3/4 | .436 | .436 |
| Uncontrolled rents | + | 7/8 | – | 5/7 | 3/3 | .569 | .569 |
| New construction | − | 2/3 | – | 3/7 | 4/4 | 1.000 | 1.000 |
| Rental supply | − | 6/7 | – | 2/7 | 4/4 | .103 | .103 |
| Quality | − | 10/13 | – | 7/9 | 0/1 | 1.000 | 1.000 |
| Mobility | − | 14/15 | – | 8/10 | 2/2 | .543 | .543 |
| Misallocation | + | 7/7 | – | 6/6 | – | 1.000 | – |
| Homeownership | + | 5/10 | – | 7/9 | 1/3 | .350 | .350 |
| **Thorne coding, U1 (the same cells)** | | | | | | | |
| Controlled rents | − | 12/13 | 27/30 | 8/9 | 2/5 | 1.000 | 1.000 |
| Uncontrolled rents | + | 5/5 | 7/9 | 3/4 | – | .444 | .559 |
| New construction | − | 3/3 | 3/4 | 1/5 | 2/2 | .143 | .116 |
| Rental supply | − | 6/6 | 2/8 | 3/3 | 1/1 | 1.000 | .005 |
| Quality | − | 5/7 | 7/8 | 4/6 | 1/2 | 1.000 | .682 |
| Mobility | − | 6/6 | 8/10 | 7/8 | 3/3 | 1.000 | .763 |
| Misallocation | + | 2/2 | 6/6 | 4/4 | 1/1 | 1.000 | – |
| Homeownership | + | 2/5 | 6/8 | 4/7 | 1/2 | 1.000 | .537 |
| **Thorne coding, U2 (verification-expanded)** | | | | | | | |
| Controlled rents | − | 15/16 | 31/34 | 11/12 | 10/10 | 1.000 | 1.000 |
| Uncontrolled rents | + | 3/3 | 3/7 | 3/4 | 1/1 | 1.000 | .371 |
| New construction | − | 3/4 | 4/8 | 1/7 | 4/4 | .088 | .146 |
| Rental supply | − | 7/7 | 10/14 | 7/9 | 1/2 | .475 | .417 |
| Quality | − | 7/8 | 7/11 | 4/8 | 5/6 | .282 | .273 |
| Mobility | − | 7/9 | 12/17 | 8/10 | 4/5 | 1.000 | 1.000 |
| Misallocation | + | 6/7 | 9/9 | 2/2 | 2/2 | 1.000 | .500 |
| Homeownership | + | 2/4 | 7/12 | 4/6 | 2/3 | 1.000 | 1.000 |
| **Thorne coding, full pool (U2+U3)** | | | | | | | |
| Controlled rents | − | 18/19 | 33/38 | 17/23 | 10/12 | .105 | .181 |
| Uncontrolled rents | + | 6/6 | 7/11 | 6/8 | 1/1 | .473 | .407 |
| New construction | − | 5/7 | 5/9 | 2/8 | 4/4 | .132 | .205 |
| Rental supply | − | 9/9 | 13/19 | 9/14 | 2/3 | .116 | .146 |
| Quality | − | 7/10 | 10/14 | 6/11 | 5/6 | .659 | .667 |
| Mobility | − | 7/9 | 12/16 | 13/15 | 4/5 | .615 | .784 |
| Misallocation | + | 7/8 | 12/12 | 5/5 | 3/3 | 1.000 | .520 |
| Homeownership | + | 3/6 | 7/12 | 4/9 | 2/3 | 1.000 | .884 |

Notes: Thorne coding U1 (the same cells) represents Kholodilin's cell results, broken down by my regime coding.

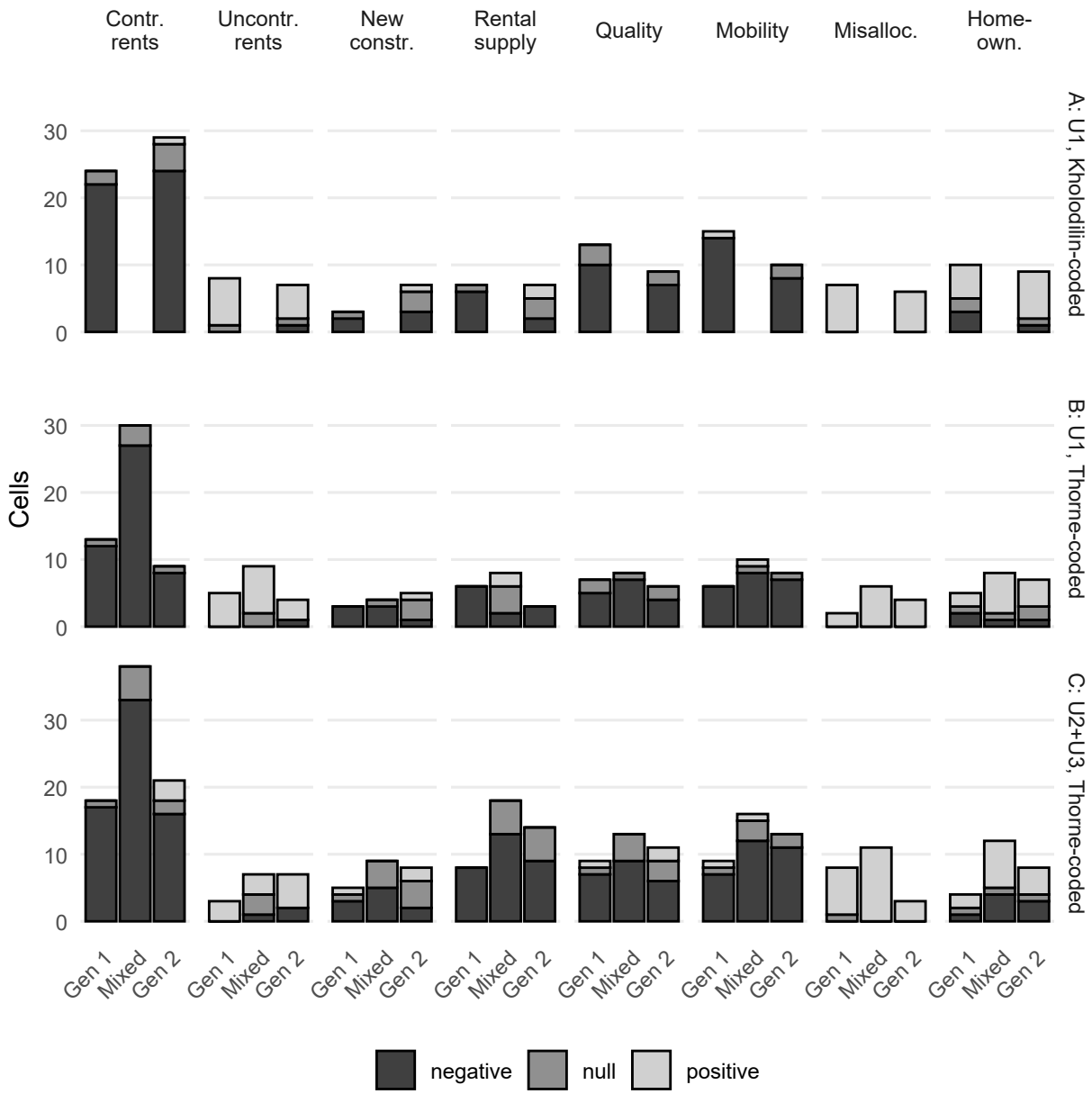


Figure 3: Evolution of the evidence, by outcome family and generation.

Notes: Top row: Kholodilin's Table-2 cells (U1) under his generation tags. Middle row: the same cells under Thorne regime-level coding – the only change is the recoding, and a mixed category emerges. The bottom row combines the verification-expanded findings with the studies added in the 2023 through 2026 update, or U2 + U3. Cells without a generation tag under the relevant coding are omitted, and effect signs remain within their outcome family rather than being pooled across families.

Thorne coding U2 (verification) are my coding of cells and my regime coding, of the same set of U1 papers. The full pool is U2 with the additional U3 cells (mostly from recent papers), plus the addition of 28 weak-basis or absent U1 cells restored with Kholodilin-coded signs. Weak basis is ultimately a subjective determination, and thus for studies that included cells that within an outcome family were entirely coded weak basis I restore these so as to not drop a study entirely from contributing to these tallies. Their inclusion does not push any p-values across the .05 threshold. For ease of presentation the numerator of each fraction represents the number of cells which are of the same sign as conventionally predicted, with the denominator being the total number of cells tallied in that outcome family. The unclassified column represents studies that do not have a generation tag, most frequently because they study multiple differing rent control regimes.

In rental supply, where there is the appearance of an effect, it does not follow the conventional wisdom that the softest forms will be least likely to find measurable negative side effects (Table 2).

## 4.1 Controlled rents

In the case of the intended purpose of rent control, controlled rents, there is near-unanimity across every generation, every coding, and every collection of evidence: rent control lowers rents for controlled units. Under the full pool with second generation, there is a suggestion that some regimes do not lower rents, which may mean that they are sufficiently loose to be effectively non-binding.

## 4.2 Uncontrolled rents

Uncontrolled rents, meaning rents in uncontrolled units within a rent controlled region (not to be confused with spillovers from rent controlled regions into non-rent controlled regions), rise in nearly every panel.

The common explanation of this effect (Fallis and Smith 1984; Early and Phelps 1999) is that households with lower rents in the controlled sector consume more housing than they would absent rent control, and this less intensive use of the controlled housing stock causes spillovers of excess demand into the uncontrolled housing stock, raising prices.

This effect does not appear to be affected by rent control generation.

## 4.3 New construction and rental supply

While there is no theoretical consensus on the effect of rent control on new construction, the basic story, as set out in Section 2, has three main components:

1. Higher uncontrolled rents raise the earnings of new units where new construction is exempt.
2. While new units may earn more during the exemption, depending on the length of the exemption and the bite of the rent control once it lapses, the lifetime earnings of the building could still decline.
3. Even where a given rent control law does not negatively affect developer earnings, concern and uncertainty over future laws governing rent and development might dissuade risk-averse developers from building.

Therefore the net effect of these three components could theoretically decrease new construction, increase new construction, or cancel out, leaving a null.

The effect of rent control on new construction, in particular second-generation rent control, is the most empirically contested outcome family. Unfortunately, the empirical evidence is also relatively thin. What the corpus shows is inconclusive, but there are some indications that might firm up with more evidence: under either coding, studies of first-generation policies mostly find negative effects on new construction, while studies of second-generation policies are majority null or positive. This suggests a potential relationship, but there is not sufficient evidence yet to form a firm conclusion.

Across every panel, rental supply tends to be negative. Practically speaking, this largely means landlords taking a housing unit out of the rental market and putting it into the owner-occupied market; it is not to be confused with a net decrease in housing supply. This shift is consistent with the corresponding rise in homeownership.

The clearest result under U1 comes from the nulls under mixed regimes, and these dissipate with additions that come in the U2 recoding (Table 2: under my regime coding of the same U1 cells, 2 of 8 mixed-regime rental-supply cells are negative, p .005 across the three groups, against 10 of 14

once the verification universe of cells is added in).

## 4.4 Quality

The effect on quality is broadly negative across universes and generations. This is largely a case of less maintenance or increased depreciation and degradation within controlled dwellings.

These problems tend to be minor or aesthetic concerns in the short run that can accumulate into what would be considered habitability failures in the long run (Sims 2007; Gyourko and Linneman 1989).

Quality does not appear to be affected by rent control generation.

## 4.5 Mobility and misallocation

Tenants in rent-controlled units move less frequently regardless of generation or literature universe. This consistent finding can be framed in two ways. It is either a successful intended result of the policy, allowing residents to stay in their communities and resist displacement as prices change, or a vehicle for misallocation, creating mismatches between units and the tenants who value them most.

Misallocation increases as a consequence of rent control in all but one instance. The primary story from Glaeser and Luttmer (2003) is that non-price rationing changes the allocation of housing substantially. Two different theoretical strands of misallocation have differing policy implications: loss of efficiency, measured as a departure from what the market allocation would be absent rent control, and poor targeting, meaning the benefits of rent control accrue to those with higher incomes. The former is well documented in the literature, but is true nearly by construction; the latter has mixed evidence: some studies find that the benefits of rent control accrue to those with higher incomes (e.g., Gyourko and Linneman 1989), while others find that lower income households receive the larger benefits. This appears to vary regime by regime, and no study in this literature empirically evaluates whether means-testing or some other set of eligibility requirements would improve targeting. In cases where misallocation is defined as a difference in allocation relative

to the market allocation, I note two important factors: first, there is no possibility of finding a "negative misallocation" in these cases, and thus the options are to find a null or a positive (increased misallocation). This is, in effect, a test of whether rent control is binding and changes the allocation of housing, which is the intended effect of the policy. Second, there is reason to believe that with housing in particular, market allocations might not always be preferable for maximizing social welfare (Weitzman 1977), particularly when income inequality is high and needs for the good are relatively uniform. Future literature that evaluates eligibility requirements directly, rather than measuring targeting after the fact, would give policymakers the missing half of this trade-off.

## 4.6 Homeownership

Homeownership has a slightly positive tilt across the literature unaffected by regime generation, but it is not necessarily clear-cut. The increase largely comes through conversions of rental units to owner occupancy, but in many rent control regimes this option is restricted or unavailable. In cases where decontrol was studied, as in Cambridge, Massachusetts (Autor et al. 2014), conversions rose sharply once rent control ended, in part because the regime itself had blocked them: rent control's removal permit requirement had made condo conversion an option that was not available to landlords until decontrol. Overall, it is not clear whether this outcome tells us a great deal in terms of broader housing outcomes: whether a family of three rents or owns the same two-bedroom unit may not have much effect on the broader housing market. Where it may play a bigger role is in understanding the incentives landlords face. In regimes where selling units into owner occupancy remained legal, take-up was modest, suggesting that many landlords still prefer to participate in rental markets even in the presence of rent control. Where conversion was restricted, low conversion counts reflect the law rather than landlord preferences.

Overall the literature tends to align with the theory, but there is some ambiguity on the supply-side measures, rental supply and new construction, when regime generation is taken into account. This is suggestive of the idea that design features might play a real role in developer and landlord incentives. In a sensitivity analysis reported in Table 9 in Appendix B, I remove ambiguous-flagged results

(included by default in the tables above), and the rental supply and new construction split becomes more stark: far fewer predicted-direction results for both families under second-generation rent controls. The flags were assigned during the data collection, before these tallies existed, but this cut of the data was elevated after the main analysis had been completed.

# 5 Which design features matter

Table 3 displays how the original universe of Kholodilin cells is coded under my regime-level coding, once mixed regimes are given their own category. The table excludes Gilderbloom (1986) and Heffley and Santerre (1985), which span many municipal New Jersey rent control policies that cannot be coded as one rent control regime. The mixed category pulls from both of Kholodilin's generation categories in comparable numbers, indicating a genuine gray area. The mixed coding becomes the largest category.

Table 3: Kholodilin-coded versus Thorne-coded generation (cells).

| Kholodilin-coded | Gen 1 | Mixed | Gen 2 | Unknown |
|---|---|---|---|---|
| Other/unknown | 8 | 0 | 4 | 8 |
| Generation 1 | 39 | 37 | 8 | 3 |
| Generation 2 | 0 | 46 | 34 | 3 |

Notes: Rows: Kholodilin-coded generation; columns: Thorne-coded generation.

For an example of the difficulty of these rent control regime codings, consider Vancouver, British Columbia. The new construction exemption was present from the program's first year, was made a permanent fixed date exemption in 1977, and continued until the end of the rent control regime in 1984. Rents did not reset at vacancy, though capped yearly increases were allowed. The program thus carried a second generation and a first generation feature for its duration. This presents a necessary difficulty in coding these regimes: the published generation tagging lands on both sides of it, first generation for Lazzarin (1990) and second generation for Marks (1984), the same Vancouver

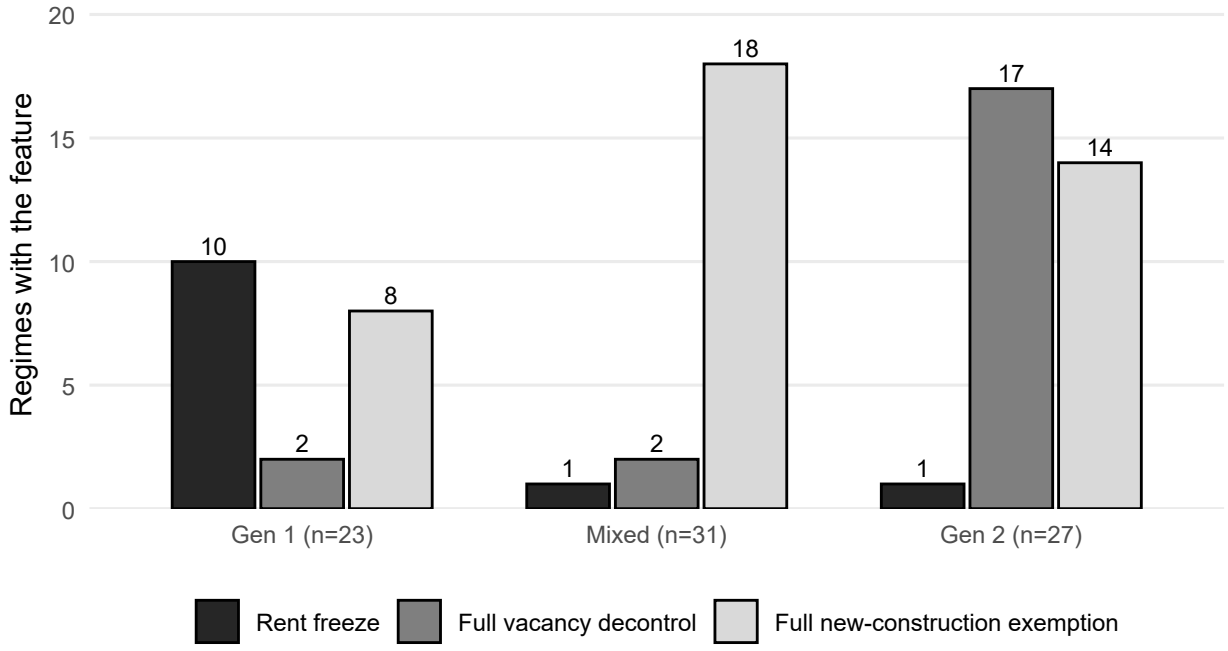


Figure 4: The three Arnott margins at their strongest settings. Of the 81 coded regimes classifiable as generation 1, mixed, or generation 2, how many carry a rent freeze, full vacancy decontrol, and a full new-construction exemption; one profile per regime, since cells would over-weight much-studied regimes, and 12 further regimes with indeterminate generation are excluded.

Notes: Freezes concentrate in generation 1 and full vacancy decontrol in generation 2, while the full exemption spreads across all three categories. The vacancy sorting is partly definitional, since market rents reachable between tenancies is itself the second-generation criterion, and the one generation-2 freeze (Scotland, 2022) binds within tenancies only, with rents resetting fully at vacancy.

regime, which I code as "mixed." However, even introducing a third category does not resolve all issues of compressing real regimes into the categories. Rio de Janeiro in 1980, studied in Silveira and Malpezzi (1991), had full vacancy decontrol and no exemption for new construction. Nominally, this could be coded under "mixed" with its blend of first generation and second generation features, but in practice the free margin was difficult to reach, since the same law abolished no-fault eviction and automatically extended sitting tenancies for as long as the tenant chose to stay. Both codings retain first generation here, a call the practical obstruction supports. These two examples suggest that two or even three rent control regime categories are not sufficient to describe the heterogeneity of rent control design features without loss due to compression.

The exemption for new construction, like all design features in this paper, was coded once per regime and inherited by each study of that regime, and in turn by each outcome family that study produces estimates for (Table 6). Among the eight headline outcome families there are a total of 192 cells, meaning one signed result per study per outcome family. Of these 192, 176 could be coded, with the uncoded cells largely being studies that pool many regimes, such as Malpezzi and Ball's (1993) 51-country index, plus two whose full text could not be obtained. Of these 176 coded cells, 151 were exempt and 25 unexempt. While the new construction exemption is considered a second generation hallmark, it is wide-reaching, and studies featuring unexempt regimes are relatively rare in this universe of studies (Figure 4). Even the wartime and post-war rent control regimes of Israel in 1953 (Werczberger 1988), Lyon in 1948 (Bonneval et al. 2022), and Norway in 1940 (Oust 2018b) have exemptions for new construction, so this is not a feature with exclusively modern origins. Of the seventeen unexempt regimes, ten are historic hard regimes, and two are modern: St. Paul (2021) and the Paris encadrement (2015, re-enacted 2019). The remaining five are unexempt by the letter, in that a brand new building must abide by the rent control law, but through other design features the law does not mechanically limit what a landlord may charge on a new lease. These are functionally similar to having an exemption with a very small window. With the vast majority of coded cells exempt, and several among the unexempt having other soft characteristics, there is little variation in this margin through which I can practically find the effect of exemption (Table 4). This is not

to say that exemption has little effect, or even that unexempt policies themselves are rare, just that there is little basis in the studies I have collected with which to test it. There is significant variation within regimes that have an exemption, however: I code strength (full or partial, judged on pricing freedom and durability) and window design (a rolling window or a fixed date).

Table 4: Reported-sign split by new-construction exemption status, for four outcome families.

| Outcome family | Exemption | N | Negative | Share negative |
|---|---|---|---|---|
| New construction | exempt | 9 | 4 | 44% |
| New construction | unexempt | 3 | 3 | 100% |
| Controlled rents | exempt | 43 | 40 | 93% |
| Controlled rents | unexempt | 9 | 7 | 78% |
| Rental supply | exempt | 15 | 9 | 60% |
| Rental supply | unexempt | 2 | 2 | 100% |
| Uncontrolled rents | exempt | 16 | 0 | 0% |
| Uncontrolled rents | unexempt | 2 | 1 | 50% |

Table 5a: Reported-sign splits across four design-feature axes: U1-only tests beside the full-corpus pool – the verification-expanded Kholodilin universe plus the 2023–26 update (U2+U3), one vote per study and family on the Kholodilin side.

| Family | Predicted / n, by level | p (pooled) | p (U1) | p (layer × level) |
|---|---|---|---|---|
| **New-construction exemption strength (none / partial / full; universal-soft excluded)** | | | | |
| Controlled rents | 6/9 ǀ 13/14 ǀ 42/48 | .214 | .153 | .441 |
| Uncontrolled rents | 2/2 ǀ 3/4 ǀ 12/16 | 1.000 | .542 | – |
| New construction | 4/5 ǀ 3/4 ǀ 5/13 | .253 | .124 | .020 |
| Rental supply | 5/5 ǀ 5/7 ǀ 19/28 | .463 | .580 | .163 |
| Quality | 1/3 ǀ 3/4 ǀ 19/27 | .447 | 1.000 | .444 |
| Mobility | 3/4 ǀ 4/7 ǀ 22/25 | .153 | 1.000 | .260 |
| Misallocation | 5/5 ǀ 7/7 ǀ 11/12 | 1.000 | 1.000 | – |
| Homeownership | 1/2 ǀ 3/3 ǀ 10/20 | .282 | 1.000 | 1.000 |
| **Vacancy decontrol (none / partial / full)** | | | | |
| Controlled rents | 35/41 ǀ 19/20 ǀ 12/17 | .157 | 1.000 | .480 |
| Uncontrolled rents | 9/10 ǀ 4/6 ǀ 4/7 | .342 | .184 | .027 |
| New construction | 7/11 ǀ 1/6 ǀ 1/4 | .148 | .016** | .014 |

Table 5a: *(continued)*

| Family | Predicted / n, by level | p (pooled) | p (U1) | p (layer × level) |
|---|---|---|---|---|
| Rental supply | 19/23 \| 5/7 \| 5/9 | .270 | .056* | .020 |
| Quality | 12/17 \| 9/13 \| 2/4 | .765 | 1.000 | .164 |
| Mobility | 14/20 \| 9/10 \| 7/8 | .499 | 1.000 | .628 |
| Misallocation | 12/13 \| 8/8 \| 4/4 | 1.000 | 1.000 | 1.000 |
| Homeownership | 9/15 \| 1/4 \| 4/8 | .440 | 1.000 | .138 |
| **Cap form (freeze / formula-administered / rate)** | | | | |
| Controlled rents | 11/11 \| 42/48 \| 15/21 | .098* | .663 | .371 |
| Uncontrolled rents | 3/3 \| 12/16 \| 3/5 | .635 | 1.000 | .064 |
| New construction | 0/1 \| 10/14 \| 2/9 | .036** | .072* | .338 |
| Rental supply | 6/6 \| 14/20 \| 9/14 | .316 | .182 | .530 |
| Quality | 3/3 \| 18/23 \| 2/9 | .005*** | .115 | .899 |
| Mobility | 3/4 \| 20/24 \| 8/11 | .728 | 1.000 | .903 |
| Misallocation | 3/3 \| 15/16 \| 6/6 | 1.000 | 1.000 | 1.000 |
| Homeownership | 1/1 \| 6/16 \| 7/10 | .168 | .170 | – |
| **Just-cause eviction bundled (yes / no)** | | | | |
| Controlled rents | 48/55 \| 19/21 | 1.000 | 1.000 | .919 |
| Uncontrolled rents | 12/16 \| 5/7 | 1.000 | 1.000 | .210 |
| New construction | 11/16 \| 1/7 | .027** | .045** | .105 |
| Rental supply | 17/22 \| 9/13 | .698 | .287 | .015 |
| Quality | 18/24 \| 4/10 | .112 | .140 | .534 |
| Mobility | 27/30 \| 2/7 | .002*** | 1.000 | .137 |
| Misallocation | 20/21 \| 2/2 | 1.000 | 1.000 | – |
| Homeownership | 11/23 \| 2/2 | .480 | – | – |

Notes: Within each design axis, the levels run from strictest to softest from left to right. The cap-form axis is different because it distinguishes policy machinery rather than degrees of strictness, as explained in the table note. The Fisher exact tests are reported only where the levels can be pooled.

Table 5b: Companion to Table 5a: each design axis reduced to two levels by the sparse-merge rule – the sparsest level merged into its strictness-adjacent neighbor, a choice made on level sizes. Two-sided Fisher exact tests on the same pooled full-corpus counts. Table 5a remains primary. Stars as in Table 5a.

| Family | Strict pole: predicted / n | Soft pole: predicted / n | p (pooled) |
|---|---|---|---|
| **New-construction exemption strength: none + partial vs full (universal-soft excluded)** | | | |
| Controlled rents | 19/23 | 42/48 | .718 |

Table 5b: *(continued)*

| Family | Strict pole: predicted / n | Soft pole: predicted / n | p (pooled) |
|---|---|---|---|
| Uncontrolled rents | 5/6 | 12/16 | 1.000 |
| New construction | 7/9 | 5/13 | .099* |
| Rental supply | 10/12 | 19/28 | .451 |
| Quality | 4/7 | 19/27 | .656 |
| Mobility | 7/11 | 22/25 | .167 |
| Misallocation | 12/12 | 11/12 | 1.000 |
| Homeownership | 4/5 | 10/20 | .341 |
| **Vacancy decontrol: none vs partial + full** | | | |
| Controlled rents | 35/41 | 31/37 | 1.000 |
| Uncontrolled rents | 9/10 | 8/13 | .179 |
| New construction | 7/11 | 2/10 | .080* |
| Rental supply | 19/23 | 10/16 | .264 |
| Quality | 12/17 | 11/17 | 1.000 |
| Mobility | 14/20 | 16/18 | .238 |
| Misallocation | 12/13 | 12/12 | 1.000 |
| Homeownership | 9/15 | 5/12 | .449 |
| **Cap form: freeze + formula-administered vs rate** | | | |
| Controlled rents | 53/59 | 15/21 | .071* |
| Uncontrolled rents | 15/19 | 3/5 | .568 |
| New construction | 10/15 | 2/9 | .089* |
| Rental supply | 20/26 | 9/14 | .469 |
| Quality | 21/26 | 2/9 | .003*** |
| Mobility | 23/28 | 8/11 | .663 |
| Misallocation | 18/19 | 6/6 | 1.000 |
| Homeownership | 7/17 | 7/10 | .236 |
| **Just-cause eviction bundled: yes vs no (two-level in both tables)** | | | |
| Controlled rents | 48/55 | 19/21 | 1.000 |
| Uncontrolled rents | 12/16 | 5/7 | 1.000 |
| New construction | 11/16 | 1/7 | .027** |
| Rental supply | 17/22 | 9/13 | .698 |
| Quality | 18/24 | 4/10 | .112 |
| Mobility | 27/30 | 2/7 | .002*** |
| Misallocation | 20/21 | 2/2 | 1.000 |
| Homeownership | 11/23 | 2/2 | .480 |

Table note (covers Tables 5a and 5b): exemption "none" = strength none with universal-soft cells (initial rents free for all units) excluded (no binding build-date margin exists there); vacancy "none" = no reset at vacancy; cap "admin./freeze" = reference-rent, board-administered, or freeze

systems, vs mechanical percentage-growth caps – a machinery distinction, not a strictness ranking (the administered pole includes cost-rent systems). These cells are directional counts of results coded from studies within an outcome family, so a given empirical paper could contribute several cells among different outcome families. I use Fisher exact tests (Freeman and Halton 1951) or a generalized Fisher exact test (Mehta and Patel 1983) in cases with more than two columns. Each p-value is the probability of a split at least this uneven under the null of no association between the design level and the reported sign; with three columns, a small p-value means the levels are not all alike, not that each differs from the others. The construction-family tests are fragile, because the number of empirical studies touching these sub-categories is quite small. Small changes to these cell counts could alter the results; more literature is needed to get a firm idea of the disambiguation these tests represent. The rows without a new-construction exemption draw on very few regimes, and the exemption's separate contribution cannot be identified because only one regime varies that feature independently of its vacancy rule. Findings from studies of the same regime are also not independent. Conditional and regime-level companion tests are included in the replication materials.

Across the four design feature categories, studies of harder rent control features tend to find the predicted result from theory, and softer features have more results that depart from the theory. This is most consistent in new construction, though there it rests on few cells and, as the table note records, is fragile to single cells; the general pattern can be seen in most of the outcome families, though not all. For sensitivity to the new literature added in this paper, the Fisher exact results from the original Kholodilin universe of studies are included. The original Kholodilin universe has seemingly indicative results for vacancy decontrol on new construction and on rental supply, and the layer-by-level test in Table 5a hits on those two rows, meaning those initial results were more a feature of that older universe of studies than a general finding. The exemption for new construction is null everywhere in Table 5a, meaning there is little evidence to show that exemption changes any outcome family's balance of reported signs, but I suspect this has more to do with the ubiquity of new construction exemptions and an underpowered test than with this design feature not having any effect. Table 5b does suggest, when pooling sparse levels together, that new construction might be

affected by the exemption feature. In practice, as is generally the underpinning of the idea of distinct rent control generations, these design features do move together, but not to such an extent that one could swap the generation coding for a combination of these features and extract the same results. These tests are primarily explorations of the literature that may be useful to policymakers; they are not all independent, nor necessarily representative, given that some regimes may not appear in these data and that some regimes have multiple studies (as sensitivity I have included the breakdown with only 1 vote per regime, Figure 8). This gives the reader an idea of where evidence is strong and in favor of theory, and where there are gaps or thinner areas.

The outcomes measured in this paper are directions of movement of particular variables in response to rent control implementation (or in some cases decontrol). With the exception of the effect on new construction, the compiled literature suggests that these effects do move in the direction predicted by the standard theory. These do not necessarily speak to the welfare of alternative policies, their efficiency or effectiveness, and should not be interpreted in those terms. The particular design of the price cap and the various options for resetting to market rate affect the bite of these rent control regimes, and thus policies aimed at reducing negative side effects may eat into the benefits granted to tenants. This is clearer under cap form in Table 5b, where less negative effects on quality and new construction also come with less positive effects in the form of lower controlled rents. Vacancy decontrol, exemption, and eviction do not suggest such a clear trade-off, but that may be due to a lack of power.

# 6 Limitations

One main limitation of this methodology is relying on signs. It gives equal weight to studies of differing levels of quality, and does not contain information about the magnitudes found for each outcome. Separately, published and unpublished studies report similar distributions of signs (the split of these two by family is in the replication materials), a low-power check that leaves the equal-weighting concern standing. Second, there is relatively thin evidence for many core claims;

this reflects the existing literature on rent control. Third, New York City is over-represented in the rent control literature, and so unique characteristics of the city and its policies may overpower the rest of the world's experience of rent control.

Generation also moves with study vintage and research design. First-generation evidence comes mostly from older and generally weaker designs, while second-generation evidence comes more often from modern quasi-experiments that are more likely to report nulls. These findings cannot fully separate a difference between generations from the broader change in empirical methods. The identification groups in the sensitivity battery provide only a rough check on this problem. A null sign means that the study reports no statistically distinguishable effect, unless the author clearly asserts another reading. It records the study's power and design as much as the regime's bite. There is predictably little new evidence of harder first-generation controls being produced, and so new studies on modern rent control regimes are unlikely to further sharpen these distinctions.

In general, the coding of a particular regime denotes the law as written and not necessarily the enforcement and compliance of said law, because that is less often measured. In cases where rates of compliance are documented by authors and conflict with the law as stated, I defer to the authors' documented account. Additionally, each rent control regime is given one coding, for the sake of consistency, but that means that regimes that changed or were updated throughout their life are necessarily simplified in coding. Parameters that changed over a regime's life, like the allowable increase, were coded to their longest-running value, so a short-lived initial freeze that was then updated to a yearly allowable increase for the rest of its implementation would be coded as a yearly allowable increase. A study whose identifying window fell during the freeze years could therefore sit under the later cap value; regimes were split into separate regimes only for major changes, and no subsequent check necessarily aligns each study's window with the parameter in force. Some additional measures that restrict landlords' and developers' exit from rent control regimes, like conversion and demolition controls, are uncoded in this paper. These, in addition to city-specific laws like zoning restrictions, historic building preservation, and similar legislation, may play a

meaningful role in the supply-side effects of rent control. To decompose how much of the change is the rent control law itself, versus a signal about the political and regulatory environment, would be very difficult.

The subject of this study is explicitly empirical studies of rent control; this is meaningfully different from a study of rent controls themselves. If a rent control law is left unstudied, it will necessarily be excluded from this paper. For the basis of this study, I inherit the main body of work studied in Kholodilin (2024), and I extend this universe of literature through my own replication of that search, which may leave gaps, particularly among non-English-language papers. The decision to include non-academic work or unpublished papers is an open question, and I follow Kholodilin (2024) in this: including but noting it for sensitivity analysis.

# 7 Conclusion

What does the literature find when it comes to rent control, across design features and generations? Rent controls do reduce rent for tenants in rent-controlled apartments, rents in uncontrolled apartments rise, there is a different allocation under rent control than would exist absent rent control (misallocation), and homeownership tends to rise. Tenants move less often (stability or immobility), and the quality of rent-controlled apartments tends to degrade, with the latter concentrated under harder design features. Rent control's effect is least consistent when it comes to new construction, and there is suggestive evidence, fragile to single cells, that design features matter for it. In this corpus the new construction exemption is too nearly universal to test: its presence alone cannot be linked to differences across regimes here, and whatever bite it has would have to be found in its strength, durability, and window design.

This paper finds that there is substantial heterogeneity in the design features of rent control regimes, and while speaking in terms of rent control generations is an improvement, there is rich variation even within generations that is lost through compression into this binary. Coding on design features captures more of the underlying variation and is potentially more practical for policymakers who

may need to understand the trade-offs associated with each design feature. Future work can extend the depth and coverage of these design-feature codings, and recover evidence in the form of numerical estimates with standard errors rather than signs alone. The rent control generation framework is not sufficient to describe the variety of rent control regimes that exist; analysis of rent control should lean towards these more specific design features.

# A Study-level evidence table

Table 6: Study-level evidence: Kholodilin's Table 2, extended.

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Ahern and Giacoletti (2026) | USA | value, misallocation | -1, 1 | 2 | mixed | 0 | none | Fixed percent |
| Ahrens et al. (2019) | IRL | controlled rents | -1 | 2 | mixed | 1 | none | Fixed percent |
| Albon (1978) | AUS | uncontrolled rents, controlled rents | 1, -1 | 1 | 1 | 0 | none | Formula or index |
| Ambrosius et al. (2015) | USA | construction | 0 | 2 | 2 | 1 | partial | Fixed percent |
| Appelbaum et al. (1991) | USA | homelessness | 0 | 2 | – | – | – | – |
| Asquith (2019) | USA | homeownership | 1 | 2 | 2 | 1 | full | CPI-linked |
| Assaad et al. (2021) | EGY | marriage | -1 | 1 | 1 | 1 | none | Freeze |
| Attia (2016) | EGY | controlled rents, uncontrolled rents | -1, 1 | 1 | 1 | 1 | none | Freeze |
| Ault and Saba (1990) | USA | misallocation, net welfare | 1, 1 | 1 | 1 | 1 | none | Formula or index |
| Ault et al. (1994) | USA | mobility | -1 | 1 | 1 | 1 | none | Formula or index |
| Autor et al. (2014) | USA | value | -1 | 1 | mixed | 1 | none | Formula or index |
| Autor et al. (2019) | USA | crime | -1 | 1 | mixed | 1 | none | Formula or index |
| Bailey (1999) | GBR | construction | -1 | unknown | 1 | 0 | NA | Formula or index |
| Ballesteros (2001) | PHL | rent burden, misallocation | -1, 1 | 1 | 2 | 1 | full | Fixed percent |
| Ballesteros et al. (2016) | PHL | misallocation | 1 | 2 | 2 | 1 | full | Fixed percent |
| Barton (2020) | USA | supply, homeownership | -1, 1 | 2 | mixed | 1 | none | CPI-linked |
| Baye and Dinger (2022) | DEU | rent burden | 1 | 2 | mixed | 1 | partial | Formula or index |
| Baye and Dinger (2024) | DEU | uncontrolled housing returns, controlled housing returns | 1, -1 | 2 | mixed | 1 | partial | Formula or index |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Bettendorf and Buyst (1997) | BEL | rent burden | -1 | 1 | – | – | – | – |
| Block (1989) | CAN | vacancy | -1 | unknown | – | – | – | – |
| Bonneval et al. (2022) | FRA | controlled rents, uncontrolled rents, mobility | -1, 0, -1 | 1 | mixed | 1 | full | Formula or index |
| Borck and Gohl (2021) | DEU | net welfare | -1 | 1 | 1 | 1 | none | Freeze |
| Bourassa and Hoesli (2010) | CHE | homeownership | -1 | 2 | 2 | 0 | full | Formula or index |
| Breidenbach et al. (2022) | DEU | controlled rents, housing quality | -1, -1 | 2 | mixed | 1 | partial | Formula or index |
| Caudill (1993) | USA | controlled rents, uncontrolled rents | -1, 1 | 1 | 1 | 1 | none | Formula or index |
| Causa and Pichelmann (2020) | AUS, AU... | mobility | -1 | unknown | unknown | NA | NA | NA |
| Chapelle et al. (2021) | FRA | misallocation | 1 | 2 | unknown | NA | NA | NA |
| Chen et al. (2023) | USA | inequality, controlled rents | 1, -1 | 2 | 2 | 1 | partial | Formula or index |
| Clark and Heskin (1982) | USA | mobility | -1 | 1 | 2 | 1 | full | Fixed percent |
| Coffey et al. (2022) | IRL | controlled rents | -1 | 2 | mixed | 1 | none | Fixed percent |
| Cuerpo et al. (2014) | BEL, BG... | volatility | 1 | unknown | – | – | – | – |
| DeSalvo (1971) | USA | rent burden | -1 | 1 | 1 | 1 | none | Formula or index |
| Diamond et al. (2019) | USA | mobility, uncontrolled rents, homeownership | -1, 1, 1 | 2 | 2 | 1 | full | CPI-linked |
| Dolls et al. (2021) | DEU | controlled rents, property price for controlled dwellings, uncontrolled rents, property price, supply | -1, -1, 1, 1, -1 | unknown | 1 | 1 | none | Freeze |
| Donner and Kopsch (2023) | SWE | misallocation, controlled rents | 1, -1 | 1 | mixed | 1 | none | Formula or index |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Dutta et al. (2022) | IND | mobility, inequality | -1, 1 | 1 | unknown | NA | NA | NA |
| Early and Olsen (1998) | USA | homelessness | -1 | unknown | – | – | – | – |
| Early and Phelps (1999) | USA | uncontrolled rents | 1 | unknown | 2 | 1 | NA | NA |
| Eckert (1977) | USA | homeownership, housing quality, tax base | -1, 0, -1 | 1 | 1 | 1 | none | Formula or index |
| Eichholtz et al. (2022) | BEL, FR... | rent burden | -1 | unknown | – | – | – | – |
| Ejarque and Kristensen (2015) | DNK | controlled rents, rent burden | -1, -1 | 2 | mixed | 1 | none | Formula or index |
| Engerstam (2017) | FIN, SWE | volatility | 1 | 2 | – | – | – | – |
| Enström Öst and Johansson (2023) | SWE | employment, earnings | -1, -1 | unknown | – | – | – | – |
| Enström Öst et al. (2014) | SWE | segregation | -1 | 2 | mixed | 1 | none | Formula or index |
| Fallis and Smith (1985a) | CAN | controlled rents, uncontrolled rents | -1, 1 | 1 | mixed | 1 | none | Formula or index |
| Fallis and Smith (1985b) | CAN | controlled rents, uncontrolled rents | -1, 1 | 1 | mixed | 1 | none | Formula or index |
| Fetter (2016) | USA | homeownership | 1 | 1 | 1 | 0 | none | Freeze |
| Field et al. (2008) | IND | mobility | -1 | 1 | 1 | 1 | NA | Freeze |
| Fisher (2022) | USA | property price, property sales | 0, 0 | 2 | – | – | – | – |
| Fitzenberger and Fuchs (2017) | DEU | controlled rents | -1 | 2 | 2 | 0 | full | Formula or index |
| Forouzandeh (2023) | USA | controlled rents, housing quality | -1, -1 | 2 | 2 | 1 | partial | Formula or index |
| Gaffney (2021) | USA | homeownership | 0 | 2 | 2 | 1 | full | Formula or index |
| Gandhi et al. (2022) | IND | vacancy | -1 | 1 | unknown | NA | NA | NA |
| Gardner and Asquith (2025) | USA | eviction | 1 | 2 | 2 | 1 | full | CPI-linked |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Geddes and Holz (2022) | USA | eviction | 1 | 2 | 2 | 1 | full | CPI-linked |
| Gelting (1967) | DNK | construction | -1 | 1 | 1 | – | none | Freeze |
| Gibb (1994) | GBR | construction | -1 | 0 | 1 | 0 | NA | Formula or index |
| Gilderbloom (1986) | USA | controlled rents | 0 | 2 | – | – | – | – |
| Gilderbloom and Markham (1996) | USA | construction, controlled rents, housing quality | 0, -1, 0 | 2 | 2 | 1 | partial | CPI-linked |
| Gilderbloom and Ye (2007) | USA | construction, housing quality | 0, 0 | 2 | 2 | 1 | partial | CPI-linked |
| Gissy (1997) | USA | homelessness | -1 | 2 | – | – | – | – |
| Glaeser (2003) | USA | segregation | -1 | 2 | – | – | – | – |
| Glaeser and Luttmer (2003) | USA | misallocation | 1 | 2 | 2 | 1 | partial | Formula or index |
| Goetz (1995) | USA | construction | 1 | 2 | 2 | 1 | full | CPI-linked |
| Grimes and Chressanthis (1997) | USA | homelessness | 1 | unknown | – | – | – | – |
| Gross (2021) | unknown | mobility, inequality | -1, -1 | 2 | 2 | 1 | NA | NA |
| Gyourko and Linneman (1989) | USA | homeownership, mobility | -1, -1 | 1 | 1 | 1 | none | Formula or index |
| Gyourko and Linneman (1990) | USA | housing quality | -1 | 1 | 1 | 1 | none | Formula or index |
| Hacamo et al. (2023) | PRT | earnings | 1 | unknown | – | – | – | – |
| Hager et al. (2026) | DEU | NIMBYism | -1 | 1 | 1 | 1 | none | Freeze |
| Hager et al. (2026) | DEU | NIMBYism | -1 | 1 | 1 | 1 | none | Freeze |
| Hahn et al. (2024) | DEU | controlled rents, uncontrolled rents, supply | -1, 1, -1 | 1 | 1 | 1 | none | Freeze |
| Heffley and Santerre (1985) | USA | controlled rents | 0 | unknown | – | – | – | – |
| Heskin et al. (2000) | USA | homeownership, mobility, controlled rents | 1, -1, -1 | 2 | mixed | 1 | none | CPI-linked |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Hirsch (1988) | USA | value | -1 | 1 | – | – | – | – |
| Iannello (2022) | ITA | inflation | -1 | 1 | – | – | – | – |
| Iannello et al. (2024) | ITA | controlled rents | -1 | unknown | 1 | 1 | full | Freeze |
| Jackson (1993) | USA | supply, housing quality | -1, -1 | 1 | 1 | 1 | none | Formula or index |
| Jacobo and Kholodilin (2022) | ARG | controlled rents | -1 | 1 | unknown | NA | NA | NA |
| Jacobs (1994) | USA | controlled rents, controlled rents, inequality | -1, 0, -1 | 1, 2, 1 | mixed | 1 | partial | Formula or index |
| Jarosiewicz (1984) | USA | controlled rents, misallocation | -1, 1 | 1 | mixed | 1 | none | Formula or index |
| Jiang et al. (2025) | USA | unemployment | 1 | 2 | 2 | 1 | partial | Formula or index |
| Jofre Monseny et al. (2023) | ESP | controlled rents, supply | -1, 0 | 2 | mixed | 1 | none | Formula or index |
| Karpestam (2022) | SWE | mobility | -1 | 2 | mixed | 1 | none | Formula or index |
| Kattenberg and Hassink (2017) | NLD | mobility, misallocation, homeownership | -1, 1, 0 | 1, 1, unknown | 1 | 0 | partial | CPI-linked |
| Kholodilin and Kohl (2023a) | AUS, BE... | construction | -1 | unknown | unknown | NA | NA | NA |
| Kholodilin and Kohl (2023b) | AUS, BE... | inequality | -1 | unknown | – | – | – | – |
| Kholodilin and Kohl (2023c) | AUS, BE... | homeownership | 1 | unknown | unknown | NA | NA | NA |
| Kholodilin et al. (2021) | RUS | controlled rents, mobility | -1, -1 | 1 | 1 | 0 | none | Freeze |
| Kholodilin et al. (2022) | ESP | controlled rents, uncontrolled rents, supply | -1, 0, 0 | 2 | mixed | 1 | none | Formula or index |
| Krol and Svorny (2005) | USA | commute times | 1 | 1 | – | – | – | – |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Lambie-Hanson (2008) | USA | homeownership, construction, controlled rents, mobility | 1, -1, -1, 0 | 2 | 2 | 1 | full | CPI-linked |
| Lauridsen et al. (2009) | DNK | homeownership | -1 | 1 | mixed | 1 | none | Formula or index |
| Lazzarin (1990) | CAN | supply, housing quality, homeownership, tax base | 0, 0, 0, -1 | 1 | mixed | 1 | none | Fixed percent |
| Levine (1999) | USA | supply | -1 | unknown | 2 | 1 | NA | NA |
| Levine et al. (1990) | USA | length of tenure, rent burden | 1, -1 | 1 | mixed | 1 | none | Formula or index |
| Lind (2003) | SWE | construction | -1 | 1 | 1 | 0 | none | Formula or index |
| Lind and Hellström (2006) | SWE | segregation | 0 | 1 | – | – | – | – |
| Linneman (1987) | USA | inequality, mobility, mobility | -1, -1, 0 | 1, 1, 2 | mixed | 1 | partial | Formula or index |
| Lyytikäinen (2008) | FIN | net welfare | -1 | 1 | – | – | – | – |
| MacLennan (1978) | GBR | supply | -1 | 1 | 1 | 0 | none | Formula or index |
| Malard and Poulhes (2020) | FRA | controlled rents | 0 | 2 | mixed | 0 | none | Formula or index |
| Malpezzi (1996) | USA | rent, property price, construction, homeownership, segregation, neighborhood quality | 1, 1, -1, -1, 0, 0 | unknown | unknown | NA | NA | NA |
| Malpezzi (1998) | EGY | controlled rents, side payments | -1, 1 | 1 | 1 | 0 | none | Freeze |
| Malpezzi and Ball (1993) | ARG, AU... | rent, property price, housing investment | -1, 1, -1 | unknown | unknown | NA | NA | NA |
| Malpezzi and Tewari (1991) | IND | controlled rents, net welfare | -1, -1 | unknown | 1 | 1 | none | Formula or index |
| Marks (1984) | CAN | value | -1 | 2 | mixed | 1 | none | Fixed percent |
| McClure (1978) | USA | profitability, inequality | -1, 0 | 1 | mixed | 1 | none | Formula or index |
| Mengle (1985) | USA | housing quality | -1 | 2 | unknown | NA | NA | NA |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Mense et al. (2018) | DEU | supply, controlled rents, uncontrolled rents, value | 1, -1, 1, 1 | 2 | mixed | 1 | partial | Formula or index |
| Mense et al. (2023) | DEU | uncontrolled rents, controlled rents, mobility, demolitions | 1, -1, -1, 1 | 2 | mixed | 1 | partial | Formula or index |
| Mildner (1991) | USA | welfare, welfare | -1, -1 | 1, 2 | – | – | – | – |
| Monras and Montalvo (2023) | ESP | controlled rents, supply | -1, -1 | 2 | mixed | 1 | none | Formula or index |
| Moon and Stotsky (1993) | USA | housing quality | -1 | 1 | mixed | 1 | partial | Formula or index |
| Moorhouse (1969) | USA | housing quality, housing quality | 0, -1 | 1 | 1 | 1 | none | Formula or index |
| Moorhouse (1972) | USA | housing quality | -1 | 1 | 1 | 1 | none | Formula or index |
| Morawetz and Klaiber (2024) | AUT | segregation | -1 | 1 | – | – | – | – |
| Morin et al. (2025) | FRA | controlled rents | -1 | 2 | mixed | 0 | none | Formula or index |
| Munch and Svarer (2002) | DNK | mobility | -1 | 1 | mixed | 1 | none | Formula or index |
| Murray et al. (1991) | USA | controlled rents, housing quality, supply, homeownership | -1, -1, -1, 1 | 1 | 2 | 1 | full | Fixed percent |
| Nagy (1995) | USA | mobility | -1 | 1 | 1 | 1 | none | Formula or index |
| Nagy (1997) | USA | mobility | -1 | 1 | 2 | 1 | partial | Formula or index |
| Nath (1984) | IND | tax base | -1 | unknown | – | – | – | – |
| O'Toole (2022) | IRL | controlled rents | -1 | 2 | mixed | 1 | none | Fixed percent |
| O'Toole et al. (2021) | IRL | controlled rents | -1 | 2 | mixed | 1 | none | Fixed percent |
| Olsen (1972) | USA | net welfare | -1 | 1 | – | – | – | – |
| Oni (2008) | NGA | controlled rents | 0 | 1 | 1 | 0 | none | Fixed percent |
| Oust (2018a) | NOR | controlled rents | 0 | 1 | mixed | 1 | none | Formula or index |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Oust (2018b) | NOR | misallocation | 1 | 1 | mixed | 1 | none | Formula or index |
| Peña and Ruiz-Castillo (1984) | ESP | misallocation | 1 | 1 | mixed | 1 | full | Freeze |
| Pollakowski (1997) | USA | mobility | -1 | 2 | 2 | 1 | partial | Formula or index |
| Pollakowski (2003) | USA | construction, housing quality | -1, -1 | 1 | mixed | 1 | none | Formula or index |
| Quigley (1990) | USA | homelessness | 0 | unknown | – | – | – | – |
| Rapaport (1992) | USA | vacancy | 0 | 2 | 2 | 1 | partial | Formula or index |
| Roistacher (1992) | USA | misallocation | 1 | 2 | 2 | 1 | partial | Formula or index |
| Rubaszek et al. (2025) | CAN, GB... | volatility | 0 | unknown | – | – | – | – |
| Rydell and Neels (1985) | USA | housing quality, controlled rents | -1, -1 | 2 | 2 | 1 | full | Fixed percent |
| Sagner and Voigtländer (2023) | DEU | controlled rents, supply, value | -1, -1, 0 | 1 | 1 | 1 | none | Freeze |
| Sánchez and Andrews (2011) | AUS, AU... | mobility | -1 | unknown | unknown | NA | NA | NA |
| Schweitzer et al. (2023) | USA | housing quality, housing quality | -1, -1 | 1, 2 | mixed | 1 | partial | Formula or index |
| Seko (2019) | JPN | mobility | -1 | 2 | 2 | 0 | full | Formula or index |
| Shulman (1981) | USA | controlled rents, value | -1, -1 | 1 | mixed | 1 | none | Formula or index |
| Silveira and Malpezzi (1991) | BRA | controlled rents | -1 | 1 | 1 | 0 | full | CPI-linked |
| Simmons-Mosley and Malpezzi (2006) | USA | mobility | -1 | 2 | 2 | 1 | partial | Formula or index |
| Sims (2007) | USA | construction, conversion, controlled rents, housing quality | 0, 1, -1, -1 | 1 | mixed | 1 | partial | Formula or index |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Sims (2011) | USA | segregation | 1 | 1 | – | – | – | – |
| Skak and Bloze (2013) | DNK | controlled rents, uncontrolled rents | -1, 1 | 1 | mixed | 1 | none | Formula or index |
| Smith (1988) | CAN | construction, controlled rents, uncontrolled rents, housing quality, homeownership | -1, -1, 1, -1, 1 | 2 | mixed | 1 | none | Formula or index |
| Smith and Tomlinson (1981) | CAN | construction, homeownership, vacancy | -1, 1, -1 | 2 | mixed | 1 | none | Formula or index |
| St. John (1990) | USA | value, value | 0, -1 | 2, 1 | – | – | – | – |
| Sternlieb and Hughes (1980) | USA | value, tax base | -1, -1 | 2 | – | – | – | – |
| Struyk (1988) | JOR | vacancy, net welfare | 1, -1 | 1 | – | – | – | – |
| Sung and Kim (2023) | KOR | uncontrolled rents | -1 | 2 | 2 | 0 | full | Fixed percent |
| Svarer et al. (2005) | DNK | mobility | -1 | 1 | mixed | 1 | none | Formula or index |
| Tan (2021) | USA | housing quality | -1 | 2 | 2 | 1 | partial | Formula or index |
| Teitz (1994) | USA | controlled rents, mobility, homeownership | -1, -1, 1 | 1 | mixed | 1 | none | Formula or index |
| Thomschke (2016) | DEU | controlled rents, misallocation | -1, 1 | 2 | mixed | 1 | partial | Formula or index |
| Thomschke (2019) | DEU | controlled rents, supply | -1, -1 | 2 | mixed | 1 | partial | Formula or index |
| Thornberg et al. (2016) | USA | controlled rents, uncontrolled rents, supply | 0, 1, -1 | 2, unknown, unknown | 2 | 1 | full | CPI-linked |
| Tucker (1991) | USA | homelessness | 1 | 1 | – | – | – | – |
| Turner (1990) | USA | controlled rents, profitability | -1, 0 | 2 | 2 | 1 | partial | CPI-linked |
| Vandrei (2018) | DEU | value | -1 | 2 | mixed | 1 | partial | Formula or index |
| Vitaliano (1985) | USA | housing quality | -1 | 1 | 1 | 1 | none | Freeze |

Table 6: *(continued)*

| Study | ISO | Outcome(s) | Sign(s) | K-gen | T-gen | Exempt | Vacancy | Cap form |
|---|---|---|---|---|---|---|---|---|
| Weber and Lee (2020) | AUS, AU... | controlled rents, controlled rents | -1, -1 | 1, 2 | unknown | NA | NA | NA |
| Werczberger (1988) | ISR | homeownership | 1 | 1 | 1 | 1 | none | CPI-linked |
| Werczberger (1997) | CHE | homeownership | 0 | 1 | 2 | 0 | full | Formula or index |
| Wilhelmsson et al. (2011) | SWE | vacancy | -1 | 1 | 1 | 0 | none | Formula or index |
| Willis et al. (1990) | GHA | controlled rents, supply | -1, -1 | 1 | 1 | 0 | none | Freeze |
| Zapatka and Castro Galvao (2022) | USA | rent burden | -1 | 2 | 2 | 1 | partial | Formula or index |

Notes: Kholodilin's study, country, outcome, sign, and generation columns, extended with the Thorne-coded generation and the design features this chapter adds. Outcomes and signs are listed in the same order, so the n-th sign belongs to the n-th outcome. In the T-gen and design-feature columns, – marks a cell left blank in the coding sheet and NA a cell coded as indeterminate, usually a study that pools many regimes, while unknown marks a study whose regimes span the generations so that no single label applies. The K-gen column reproduces Kholodilin's published tags, including his own unknown and the single 0 he gives Gibb (1994), a value his paper does not define. Cap form gives the increase rule in words. Long country lists are abbreviated, with the full coding in the replication materials. Study labels resolve by author and year in the references and give the published version where a working paper he cited has since appeared, so a few years differ from his table. One paper his list carries twice appears here as two identical rows, kept visible deliberately as an audit finding rather than silently merged.

# B Transparency and supplementary analyses

In cases where Kholodilin (2024) cites a working paper that has since appeared in a journal, the reference list gives the published version. The coding uses the study version cited by Kholodilin (2024). I do not code effect magnitudes. Where I have compared different versions of a study, the reported sign remains the same; the differences are in the sample and the magnitude.

Table 7 accounts for the universe assembled from Kholodilin's 169 listed studies to the 192 headline cells and the verification-expanded and updated universes, and Table 8 compares the Kholodilin published cells with the verification-expanded layer family by family: the added layer leans less negative in several families, and no family reverses.

Table 7: Corpus flow: from Kholodilin (2024) Table 2 to the analysis universes, the deferred magnitude synthesis, and the 2023–26 update corpus.

| Stage | N | Detail |
|---|---|---|
| Kholodilin (2024) Table 2 studies | 169 | 169 listed = 168 distinct: Hager et al. (2022) and Hilbig & Vief (2022) are one paper double-listed (the duplicate is in Kholodilin's own reference list, p. 17; counted once) |
| U1 headline universe: cells (all families) | 273 | |
| U1 headline universe: studies | 166 | |
| Cells in the 8 headline outcome families | 192 | |
| Cells in the 4 dissertation-grade families | 107 | |
| Exemption margin coded | 176 | |
| of which exempt / unexempt | | 151 / 25 |
| unexempt regime keys (hard / modern / universal-soft) | 17 | 10 / 2 / 5 |
| Margin ruled indeterminate (regimes pooled across jurisdictions) | 14 | |
| Quarantined studies (full text unobtainable) | 2 | Gilderbloom 1986; Heffley & Santerre 1985 |

Table 7: *(continued)*

| Stage | N | Detail |
|---|---|---|
| Effect-size extraction piloted | 5 | Ahrens 2019, Albon 1978, Attia 2016 (an alphabetical pilot batch) |
| pilot cells with a usable estimate and standard error | 1 | older studies rarely report a standard error, t-statistic or p-value |
| Magnitude synthesis: deferred (preregistered plan, amendment 3) | | |
| U2 verification-expanded universe: cells (8 families) | 261 | descriptive panel; U1 remains the sole inferential headline |
| of which verification-added (not in Kholodilin's U1) | 103 | within-study generation inheritance covers 93 of 114 untagged cells |
| U1 cells restored with Kholodilin-coded signs (pooled tests) | 28 | weak-basis or absent U1 cells restored to the pooled tests; one further pair (Moorhouse, quality) disagrees in sign and casts no vote |
| U3 update corpus (2023-26): candidates screened | 374 | replicated Kholodilin search, 2023-01 to run date (preregistered plan, amendment 4) |
| ruled in for U3 coding | 51 | 39 effective studies, 288 records, after exclusions |
| coded into the U3 ledger (kept separate from U1 and U2) | 39 | coded in batches by regime |

Table 8: U1 (Kholodilin's published cells) versus U2 (verification-expanded), cells and share reporting a negative effect by outcome family; verification-added cells separated in the final columns.

| Family | U1 cells | U1 % neg | U2 cells | U2 % neg | Added | Added % neg |
|---|---|---|---|---|---|---|
| Controlled rents | 57 | 86 | 72 | 93 | 22 | 91 |
| Uncontrolled rents | 18 | 6 | 15 | 13 | 5 | 20 |
| New construction | 14 | 64 | 23 | 52 | 11 | 45 |
| Rental supply | 18 | 67 | 32 | 78 | 16 | 75 |
| Quality | 23 | 74 | 33 | 70 | 15 | 60 |
| Mobility | 27 | 89 | 41 | 78 | 17 | 65 |
| Misallocation | 13 | 0 | 20 | 0 | 11 | 0 |
| Homeownership | 22 | 23 | 25 | 28 | 6 | 33 |

The supporting pooled model behind Figure 5 is a logit of whether a cell reports a negative

sign on the generation tag. I include controls for identification tier, publication status, method class and publication year, and outcome-family fixed effects, estimated on the headline cells (except misallocation) and standard errors clustered by study; the figure plots its second generation coefficient under each cut.

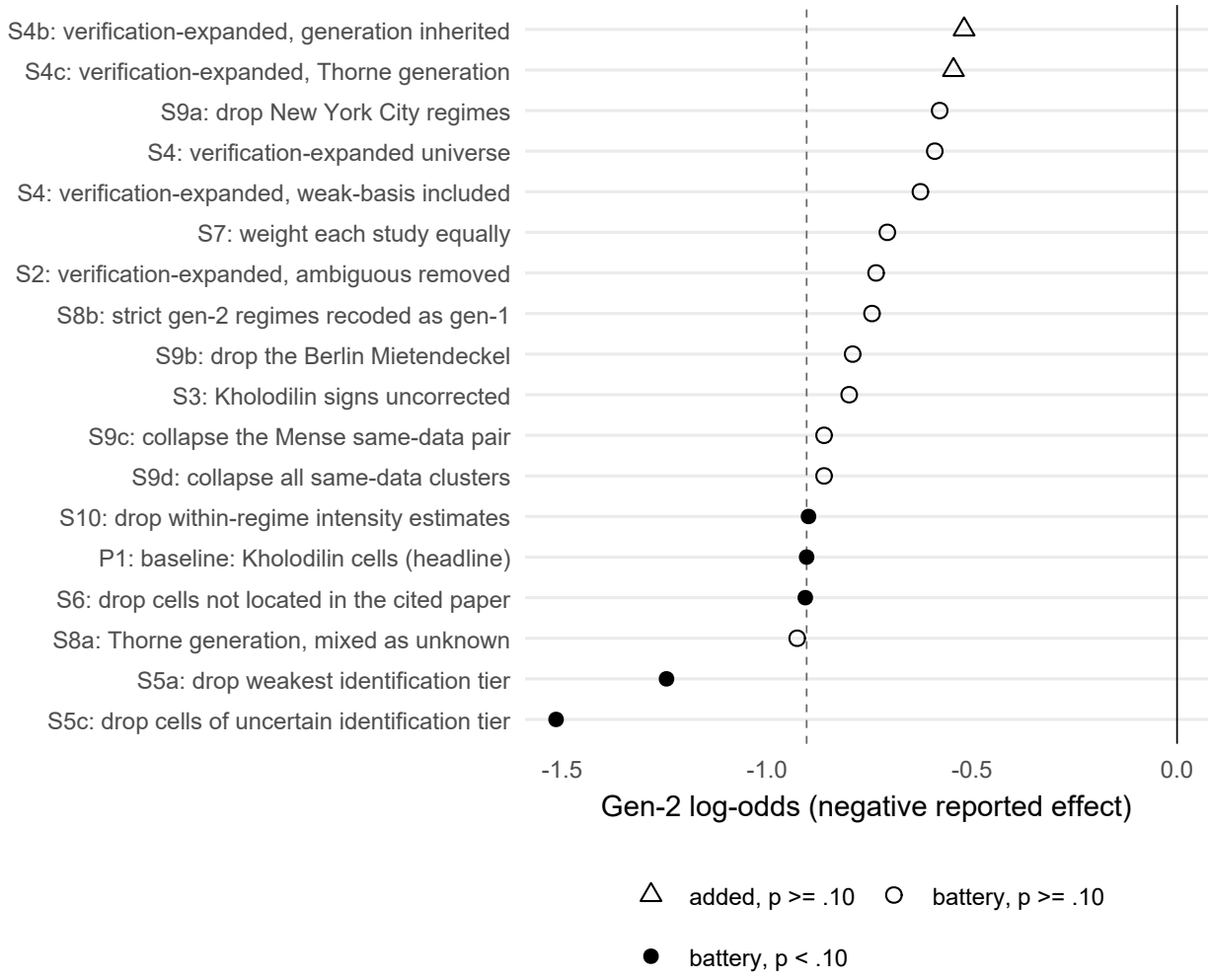


Figure 5: The generation-2 coefficient across the sensitivity battery.

Notes: Circles are the main battery; triangles are the added rows on the verification-expanded universe. Filled markers denote p below .10, and the dashed line marks the baseline estimate of the supporting pooled model.

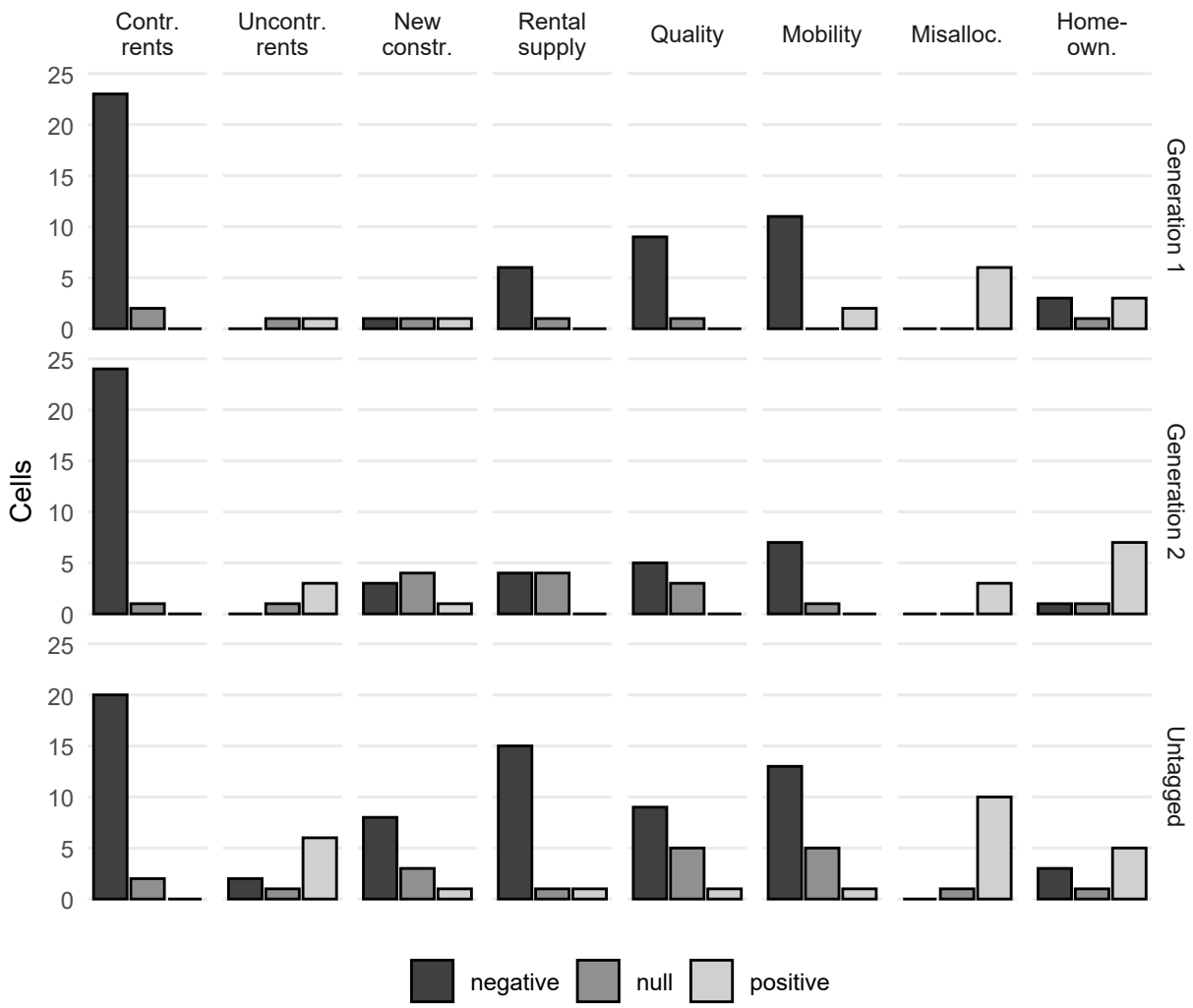


Figure 6: Companion to Figure 2: the verification-expanded universe (U2) under Kholodilin's generation tags exactly as published, with no inheritance – the untagged row holds every verification-added and never-tagged cell.

Notes: Bars count the reported direction of the estimate; sign meaning differs by outcome family.

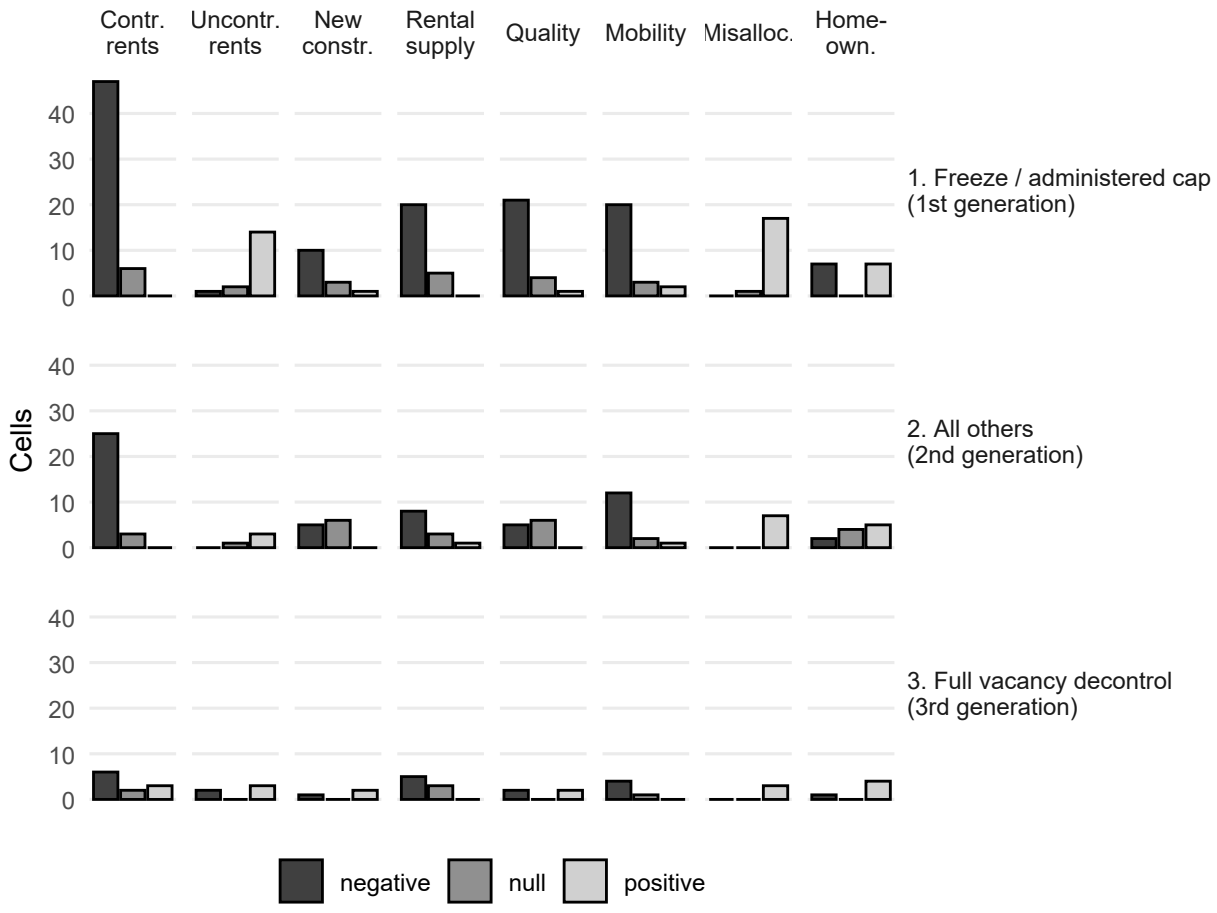


Figure 7: Alternate rule-based regime split. Group 1 is a rent freeze or an administered formula, the hallmark of first-generation control (Arnott 1995); group 3 is full vacancy decontrol, the third-generation criterion (Arnott 2003), which extends the tenancy rent control of Basu and Emerson (2000); group 2 is every other regime, and holds the regimes carrying both.

Notes: First generation carries the canonical rent control results; 2nd and 3rd generation do not have enough power to support the predicted direction.

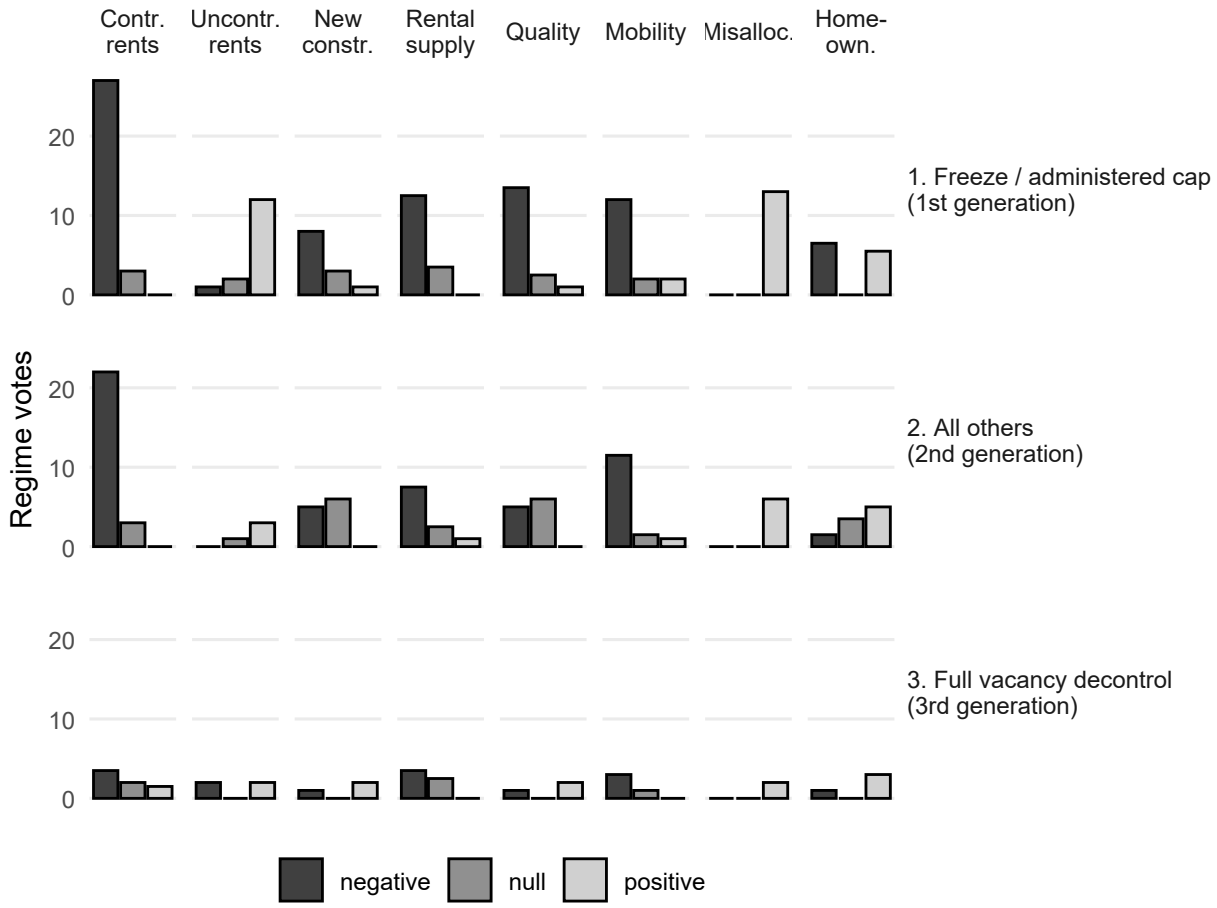


Figure 8: Alternate rule-based regime split. Each regime only casts 1 vote in total which removes influence of overrepresented regimes in the literature. Groups are defined as in Figure 7; within a regime and outcome family the majority sign takes the vote, and a tie splits it equally across the tied signs.

Notes: 2nd and 3rd generations do not hold much power to support canonical rent control results.

Table 9: The full-pool per-family generation contrasts of Table 2 recomputed with ambiguous-flagged cells removed. Cells count estimates in the family's conventional predicted direction (column Dir.) over the category total; both tests as in Table 2.

| Outcome family | Dir. | Gen-1 | Mixed | Gen-2 | Uncl. | p (1 v 2) | p (1/m/2) |
|---|---|---|---|---|---|---|---|
| **As published (full pool, U2+U3)** | | | | | | | |
| Controlled rents | − | 18/19 | 33/38 | 17/23 | 10/12 | .105 | .181 |
| Uncontrolled rents | + | 6/6 | 7/11 | 6/8 | 1/1 | .473 | .407 |
| New construction | − | 5/7 | 5/9 | 2/8 | 4/4 | .132 | .205 |
| Rental supply | − | 9/9 | 13/19 | 9/14 | 2/3 | .116 | .146 |
| Quality | − | 7/10 | 10/14 | 6/11 | 5/6 | .659 | .667 |
| Mobility | − | 7/9 | 12/16 | 13/15 | 4/5 | .615 | .784 |
| Misallocation | + | 7/8 | 12/12 | 5/5 | 3/3 | 1.000 | .520 |
| Homeownership | + | 3/6 | 7/12 | 4/9 | 2/3 | 1.000 | .884 |
| **Ambiguous-flagged cells removed** | | | | | | | |
| Controlled rents | − | 17/17 | 28/32 | 14/19 | 5/7 | .047 | .055 |
| Uncontrolled rents | + | 5/5 | 6/8 | 4/6 | 1/1 | .455 | .505 |
| New construction | − | 4/4 | 3/7 | 0/4 | 2/2 | .029 | .021 |
| Rental supply | − | 8/8 | 7/12 | 4/9 | 1/1 | .029 | .040 |
| Quality | − | 4/5 | 9/12 | 4/6 | 3/4 | 1.000 | 1.000 |
| Mobility | − | 4/5 | 8/12 | 12/13 | 4/4 | .490 | .229 |
| Misallocation | + | 5/6 | 10/10 | 5/5 | 3/3 | 1.000 | .524 |
| Homeownership | + | 2/5 | 3/5 | 3/6 | – | 1.000 | 1.000 |

Table note: the ambiguous flag marks admissible cells whose sign coding required judgment – authorial directional verdicts over conventionally insignificant estimates (the divergence guard), measured-but-descriptive tabulations, and borderline aspect-to-family mappings; weak-basis records are excluded from every table by default and are not at issue here. The filter removes 92 of the 335 cells in the combined set. I assigned the flags record by record during data collection using conventions stated before these totals were calculated. The U1 panels do not change because the flag applies only to the verification layer, which is why the table shows only the combined panel.

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