All bark, no bite: A reply to Jens von Bergmann

Josh Gordon, School of Public Policy, Simon Fraser University
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In mid-June, 2019, I released a short research report titled “Solving Wozny’s puzzle: Foreign ownership and Vancouver’s decoupled housing market”. The report is available here. About a week later, Jens von Bergmann posted a blog that criticized the report. That blog post is available here. In what follows, I will respond to several of the criticisms made by von Bergmann.

Writing a reply to von Bergmann is a strange task, because it is clear that the audience for such a reply is small. So there is naturally the question of how much effort (and ink) to put into a response. I have chosen to respond because it is important to set the record straight, in order to encourage evidence-based debate.

Healthy debate about the causes of housing unaffordability is welcome – however the blog post does not represent that. Its sole purpose is to demean and obfuscate in order to discredit. Properly understood, the points made in the blog do not amount to a serious critique.

Von Bergmann’s criticisms

There are a number of criticisms that von Bergmann raises. They can be broadly summarized here.

1. The affordability metric I use is “terrible”.
2. The numbers I use for incomes are “wrong”.
3. There is a “mismatch in metrics”: we shouldn’t be looking at the incomes of all municipal residents, as my paper does, we should only look at the incomes of homeowners.
4. Once we adopt this different metric, our understanding of the issue changes considerably, and the conclusions of my paper are no longer reliable.
5. The correlations my paper finds are potentially subject to an “ecological fallacy”.

Let me take each criticism in turn.

Culicism 1: The affordability measure [price to income] “is pretty terrible unless you live in a fantasy world.”

The measure of housing affordability I used in my report was the average house price to average income ratio – or the price to income ratio, for short. I used this metric because my taking off point was the 2017 report by Richard Wozny, Low Incomes and High House Prices in Metro Vancouver. I was trying to explain the patterns that Wozny set out in that report, as I made clear in my own paper. Moreover, Wozny was not adopting some obscure metric. The price to income ratio is a very common measure of affordability, since affordability is a relationship between prices and incomes (e.g., see various BMO reports, Demographia, countless peer reviewed articles, the recent report on money laundering in B.C., etc.). There are other measures of affordability, such as RBC’s measure,
but they correlate nearly perfectly with a standard price to income metric, as I note in my paper (fn. 1). In short, the use of this metric is standard in the field and it is consistent with other approaches.

He also wonders why I looked mainly at detached houses. This is for three reasons. First, that is where the most extreme price to income ratios are found, which help drive Vancouver’s outlier status in cross-city comparisons – not least because of their “downstream” effects in the housing market, which I describe in my report. Second, because factors such as development charges can be held constant in this domain, since they will not apply to detached houses, thus negating one possible alternative explanatory factor that would be difficult to control for otherwise. Third, and related, their supply is inelastic (there cannot be more net new detached houses built without encroaching on the Agricultural Land Reserve), so this again introduces distinct dynamics, which can be held constant. All three reasons are noted in the report.

Von Bergmann is silent on the question of an alternative measure of affordability. If there was an obviously preferable metric that should have been used, what is it?

In this respect, it should be noted that criticisms of the price to income metric have been common among some in the real estate industry in Vancouver. Bob Rennie, for example, often argued that analysts should ignore the top 20 percent of sales in discussions of affordability – and thus disregard price to income metrics, which would include them. (Unspoken was that this top-end market wasn’t for locals anyway – which only reinforces my point in the report about foreign demand.)

Something similar is going on here, and it is typical of the YIMBY movement (of which von Bergmann is a part): “don’t even bother trying to own a detached house, young person, they’re not for you”. Von Bergmann’s dismissive invocation of “fantasy world” is part of this dynamic – that underlying my choice of metric is the view that “every household should be able to buy a single-detached home in the municipality they currently live in”. That is not at all my view, and nowhere have I said this. It is my view, though, that having price to income ratios of over 20 in some municipalities makes it virtually impossible for young working people to have a shot at something their elders did – owning a detached house close to work – and that if the extreme unaffordability of detached houses is due in substantial part to foreign ownership, then we should do something about it, policy-wise.

**Criticism 2: The numbers going into the affordability metric are “wrong”**.

Von Bergmann claims that the numbers that I used for household income are “wrong”, since he generated different numbers using Statistics Canada data. My figures, he says, are in the case of West Vancouver off by “almost a factor of two”.

This would be damning, were it true. But it isn’t.

In my report, I used median income figures – i.e., the income of the “middle” household, with 50 percent of the distribution above and 50 percent of the distribution below. In the report itself, I used figures that (the late) Wozny had gathered from the CRA – as I made crystal clear – however I also double checked my results using Census data (which I show in a moment).
Von Bergmann accuses me of getting the numbers “dramatically” wrong by using a different metric: the “mean” household income figures – i.e., all household incomes summed and then divided by the number of households.

Now here is the key point: both figures, median and mean, are averages.¹ Von Bergmann construes my discussion of “average household income”, to signify a “mean” figure, not a “median” figure. In some ways, that might be understandable, since average is often taken to signify “mean”, and in hindsight I could have been more explicit about which type of figure I used. But that would be to ignore the benefits of the internet.

Upon finding disparities in the numbers, von Bergmann had a few options: (i) he could accuse me of using the wrong numbers, and try to discredit me by doing so, or (ii) he could write me an email and ask me how I arrived at different figures. (von Bergmann and I have exchanged emails before.) He also could have gone to read the report by Wozny, option (iii), and checked which type of “average” he, and therefore I, was using, since the link to the income data is in Wozny’s report.² He chose option (i).

Before he tweeted his blog post, von Bergmann emailed me with a link to it. Presumably this was to get some feedback – if, for example, he had misconstrued anything I wrote. I then informed him, among other things, that he had in fact done so: I had used median figures, not mean figures. I then suggested that he re-do the analysis to take that into account. He did not adjust his analysis in response to my information. He kept his post basically the same, and then simply added an “Addendum” right at the very bottom where he again accused me of using flawed numbers.

From his blog: “We already establish above that the paper did not use average household income numbers, and the numbers don’t match median household income either, adjusted for 2016 or otherwise, as a quick comparison shows. We still don’t know what income statistic was actually used in the paper.”

This is unfounded. The median figures were drawn by Wozny from the CRA and I told von Bergmann this by email. (Again, a link to the data was in Wozny’s paper.) I also sent him the Census median income data that could be used to double check the results with another median figure. (Available here.) But von Bergmann pretends in the post as if none of this happened. Readers can judge whether this is a good way of going about a respectful and evidence-based debate.

Now let’s turn to the substance: which metric should we use for household incomes, median or mean? We should use a median figure for two reasons. First, that is what the vast majority of studies do when it comes to looking at the price to income ratio. If a study were to use a different metric, then its results and analysis would not be comparable. This is especially the case since I had taken Wozny’s report as my taking off point. Second, when there is a “skew” in the underlying data, due to the presence of extreme values at one end of the distribution, then statisticians urge the use of a median figure. This is because these extreme values can distort our sense of the “central tendency”.

To see this, consider descriptions of the average wealth of patrons in a bar. If Jeff Bezos walks into that bar, everyone might become “on average” a billionaire, according to the “mean” figure. But of

¹ Here is the definition on Google: “a number expressing the central or typical value in a set of data, in particular the mode, median, or (most commonly) the mean…”
² Wozny imputed 2016 median income data by extrapolating income growth from 2010 to 2013.
course this is misleading. A median figure would give us a better sense of the average wealth of the bar patrons. This “skew” is known to exist with income data – since very high incomes “skew” the data – which is why researchers typically use median figures.

I explained this to von Bergmann by email, and as a mathematician he is well aware of the issue. But he proceeded to disregard it in his core analysis – and he retained the flatly false statements in his original post – despite having the chance to rework it.

One final point: when we use Census figures for median household incomes, the implications of Figures 1-4 in my report do not change. Figure A, below, is what Figure 1 would look like if we used Census figures instead of Wozny’s CRA data. The result remains the same: there is a very weak relationship between these figures. I demonstrate the same thing for Figure 3 in Appendix A.

Figure A: Benchmark house prices vs median household incomes, by municipality, 2016

![Benchmark House Prices vs Median Household Incomes](image)

Source: REBGV; FVREB; Statistics Canada.

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3 The information behind Figure A is available here and here and here. I have given the 2015 income data a 2.85 percent bump for 2016, as von Bergmann does in his post. Not giving an inflation “bump”, or picking a slightly different number, doesn’t materially change the conclusions. I have used September 2016 price data, consistent with Wozny.
Criticism 3: There is a “mismatch of metrics”.

This is one of the stranger parts of the critique. Von Bergmann argues that instead of comparing average (or median) incomes in a municipality to the average house price – as I have done – we should instead compare only the incomes of those who already own a house to the average house price. This makes little sense, as I made clear to von Bergmann by email.

If we’re looking to explain patterns of “de-coupling” in relation to affordability, it makes little sense to include only the people who have already afforded a house. To paraphrase Ian Young, that’s like looking at whether Teslas are affordable by looking at the incomes of those who own them. Rather, what we want to know is if a person or household with a typical or median income could purchase a house in that area – and that will inevitably include people who are currently non-owners or renters. Since non-owners are potential buyers, then surely their incomes have some influence on where a market will settle in terms of price. After all, owners don’t only sell to other owners – at some point in time there has to be sales to non-owners, if only as people age out of ownership. So the incomes of the non-owners matter too: they help determine what those exiting the “ownership phase” (or downsizing or leaving the region) can achieve in terms of price. And if we’re trying to understand whether houses are affordable to non-owners, as is a big problem in Vancouver, then it makes especially little sense to exclude them from the analysis altogether.

Four of the final five graphs, which are ostensibly used to undercut my claims, rest on this dubious methodological decision. As such, I suggested to von Bergmann that he alter his methodological choice to match up with the one I used – and the one that has always been used when we are constructing price to income ratios. I suggested that his results would likely change, but that if they didn’t then he could show that his critique was robust to the standard methodological decisions in the field. I also suggested he do the same thing with respect to median incomes. He did not do either. This is perhaps because if he tried the standard methodological decisions that I had made he would have found, unsurprisingly, that he arrived at essentially the same results. (See below.) That would have sharply undercut whatever force his critique possessed.

Criticism 4: Once we adopt different methodological decisions, in order to be consistent with the “real world”, then my conclusions are no longer reliable.

Using the new metrics that he created – using “mean” figures and only looking at owners’ incomes – von Bergmann then looked at whether the relationship between high price to income ratios and non-resident ownership still holds by municipal region. His reformulated puzzle or question might be stated like this: are detached house prices high relative to owners’ average (mean) incomes in municipalities with higher rates of non-resident ownership?

Von Bergmann first calculates the price to income ratio of house prices relative to owners’ incomes. He notes, correctly, that even in this case – taking a skewed “mean” figure and only looking at owners’ incomes – there exists “very high price to income ratios”. In Richmond, for example, the (von Bergmann) price to income ratio is around 13 for detached houses. In City of Vancouver, it’s around 15. Banks typically lend about 3-5 times a household’s income in a mortgage. The idea that, on average, prices are 13-15 times a (skewed) average (owners’) income should surely give one pause: how is this happening? Something other than domestic, declared income is almost surely driving this pattern. Instead of grappling with this, von Bergmann makes a hand wave to “wealth playing a
significant role for people purchasing homes” – but that merely raises further questions: where does this wealth come from, why don’t things happen the same way in other urban centers, and why does it happen in such a pronounced fashion in some Metro Vancouver municipalities and not others?

As I explained in my report, all of these questions could potentially be answered with reference to foreign (undeclared) income or wealth, but von Bergmann does not venture there – presumably as this would be to grant my broad point. He does, however, document the relationship between his version of “de-coupling” and non-resident ownership – a highly revised “replication” of my Figure 3. This is copy and pasted below (Figure B).

Before interpreting the chart, it is important to explain that there are three possible definitions of “non-resident ownership”: where there are only non-resident owners on title (undiluted definition), where non-resident owners constitute a majority of owners on title (majority definition), and where there is at least one non-resident on title (participation definition). The rates of non-resident ownership according to the first two definitions are lower than the participation definition, but they correlate highly. This is why the choice of non-resident metric doesn’t change the nature of the relationship dramatically in Figure B, though the first two definitions do show a weaker relationship than the latter.

**Figure B: von Bergmann’s “replication”**

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4 Ironically, 2016 work by von Bergmann himself showed a striking and unique pattern of ostensible “poverty” in high house value areas in Metro Vancouver, consistent with Wozny’s contention that (declared) incomes and house prices are not well aligned.
Now let’s cut to the chase: properly understood, this chart is in fact strong evidence for the robustness of the claims I make in the paper, which is that there is a very strong relationship between foreign ownership and de-coupling.

According to the participation definition, which is the most accurate definition of foreign ownership of the three, there remains a correlation of $r = 0.88$ between von Bergmann’s alleged “real world” de-coupling and non-resident ownership. In short, even if we make two strange and questionable methodological decisions, as von Bergmann has done, we merely reduce the correlation of $r = 0.96$ that I found in my report to a correlation of $r = 0.88$.

And a close look at the chart reveals a likely source of that modest discrepancy. In my Figure 3 chart (see Appendix A), West Vancouver sits very close to the trendline. In von Bergmann’s “replication”, it sits far away, weakening the correlation. We know from earlier, though, that very high incomes are likely to skew “mean” figures, and give a misleading indication of the “central tendency”. West Vancouver is likely to have those kinds of very high incomes, and, as noted above, it is the case of the biggest disparity between “mean” and “median” figures, as von Bergmann himself notes. So, in a price to income ratio, this skewed “mean” figure in the denominator will push the ratio down. Were the ratio higher, then the West Vancouver dot would move “up” in the chart and closer to the trendline, improving the correlation measurably.

Ultimately, if this is supposed to be some kind of devastating critique, then that will come as news to most social scientists. Yet some in the debate are pretending as if von Bergmann’s analysis has “blown away” or “slaughtered” my report (actual quotes). And von Bergmann’s own tone gives the impression that something major has taken place, with his repeated invocation of the words “fantasy world” to describe my analysis and so on. Yet the strident tone belies the weakness of the critique.

Criticism 5: The analysis in the report is potentially subject to the “ecological fallacy”.

Von Bergmann claims that my report relies on “a textbook example of the ecological fallacy”. What is an “ecological fallacy”, you ask? Well, ever so generously, von Bergmann directs us to a Wikipedia page about the topic. He then asserts that this could mean that the correlations I find are not indications of a causal relationship. Von Bergmann provides no evidence that this is happening. Instead, he merely asserts that this could be a problem. If his desire is to meaningfully critique the report, then he should show that it is a problem. Any social science effort could have a range of potential problems. The job of a critic is to show that a particular study suffers from them, not to assert that they might.

Hand-waving to a potential problem is not a critique, then, it is obfuscation. Especially if the potential issue is beyond the technical knowledge of most people. The only thing going on here is the use of technical terms to try to introduce doubt and thereby “discredit”.

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5 The participation measure is the most accurate because it will capture many (but not all) cases of the so-called satellite family dynamic: where the breadwinner earns abroad and the homemaker or family resides in Canada. In the satellite family scenario, it will often be the case that both the breadwinner and the homemaker are on title – if perhaps with different official “ownership shares”. So a measure that counted such a pattern as “resident” – since non-residents were not a “majority of owners” (e.g., the majority definition) – would miss a lot of foreign ownership, properly defined. The participation measure, which would count this as “non-resident” ownership, is therefore more accurate.

6 The inclusion of Bowen Island and Lions Bay is curious (and dubious), but I leave that to one side. I assume that West Vancouver’s data mostly “overwhelms” the data from the two other locations.
Indeed, this is consistent with the entire thrust of his post. As von Bergmann notes, “this post is not aimed at offering an alternative analysis…” Yes, quite. Von Bergmann makes no attempt to explain what factors would make the strong correlations I’ve found spurious.\(^7\) Surely, if the claim is that the correlations are not causal – despite a clear causal mechanism (foreign ownership) and corroborative data in both the report (e.g., Figure 5) and peer-reviewed venues – then some alternative causal story should be provided, since I already eliminated some of the main plausible alternatives in my report. But no, the reader is provided with none.

Indeed, it is notable that von Bergmann makes no attempt to explain Figure 5, which shows that the markets with the highest rates of foreign ownership are currently – due to various restrictions on foreign demand – the softest (or slowest), with the most substantial price declines. If foreign demand is not a prime mover in the detached house market, how do we explain this pattern? These pieces of inconvenient evidence in the report are simply ignored in von Bergmann’s critique.

**Figure 5: State of detached market in early 2019 vs. non-resident ownership, select cities**

![Figure 5: State of detached market in early 2019 vs. non-resident ownership, select cities](image)

Source: REBGV; FVREB; CHSP.

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\(^7\) Some of the “confounders” mentioned in the post and in the Twitter debate, such as distance from the central business district, share of renters, and share of SFD properties, were investigated to see if the relationship between non-resident ownership and “de-coupling” was significantly weakened or made spurious by their inclusion. Non-resident ownership remained statistically significant when we controlled for these other factors – and the coefficient for non-resident ownership remained large (i.e., the bulk of the variation in decoupling was explained by this factor). This conclusion was robust to different specifications.
What’s it all mean?

Given the above, the reader might be wondering why von Bergmann wrote the post. The critique is based on dubious and misleading methodological choices, no alternative account of Vancouver’s strange and extreme pattern of de-coupling is offered, and some criticisms are simply thrown into the mix to give the reader a sense that something important is happening. None of it stands up on close examination, and the ultimate conclusions von Bergmann arrives at with his “replication” are not substantially different from mine, properly understood.

From the standpoint of good faith debate, the post makes little sense. But seen from a more cynical vantage point, the post makes sense, and is part of a pattern.

Von Bergmann is part of Abundant Housing Vancouver, a local YIMBY group. Their ambition is to rezone large parts of the metro region and to thereby dramatically increase housing supply. In their favored interpretation, zoning constraints are at the root of Vancouver’s housing affordability problem, and the housing crisis provides ample evidence that policy-makers need to follow their prescriptions.

Their emphasis on supply-side solutions means that analyses which emphasize demand-side problems are anathema. Such analyses strip their claims of their urgency and relevance. This is especially problematic for YIMBYs because their underlying policy prescriptions – mass rezoning and deregulation – are widely unpopular with the local population. If alternate, more popular, courses of action are proposed – for example, tackling speculation and limiting foreign ownership – then their prescriptions risk being left in the dust.

As the reader might know, I have been a proponent of such alternate courses of action, including the Foreign Buyers Tax and the Speculation and Vacancy Tax. I have also publicly questioned the merit in YIMBY proposals. There is in my view no good reason to follow them, since the housing crisis is not a product of zoning problems, but rather mostly the product of a massive, sudden influx of foreign capital.

Considering that I had just written a report which illustrated that foreign ownership had played a central role in Vancouver’s de-coupled housing market, people like von Bergmann who disagreed with the analysis could counter it with analytically robust critiques. He chose to pursue an attack that I have shown is disingenuous and based on unsubstantiated criticism.

This is part of a pattern. Similar “hit pieces” have been penned in the past. Their main point is to muddy the waters and, most importantly, provide an online link that can be used by YIMBY twitter activists in the housing debate, even if they are subsequently debunked. This would explain why von Bergmann left the

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8 That is, in the “real world”, and not surveys which take hypothetical and abstract openness to housing form diversity as evidence for “YIMBYism”. In the last municipal election in the City of Vancouver, the YIMBY candidate for Mayor, Hector Bremner, received 6 percent of the vote, and 9 of the 10 council members are from parties historically hostile to rezoning single-detached neighborhoods (NPA, Greens, COPE).

9 See for example the post by von Bergmann and his writing partner, Nathan Lauster, in reaction to Terry Glavin’s article “Dirty Money”. Among other questionable arguments, they tied themselves to an estimate of $100 million for money laundering, in order to minimize its impact. According to the recent government-commissioned report on money laundering, the figure is likely closer to $6 billion annually. That is, they were off by a factor of 60. There has been no “mea culpa”, to my knowledge. Another example is the “critique” written by Nathan Lauster of John Rose’s 2017
post largely intact, and did not seek to rework the post according to the feedback I provided over email. To have done so would have greatly reduced the rhetorical and strategic value of the post in the Twitter debate.

Discrediting the report would have taken on added significance for von Bergmann for another reason. For a couple of years now, both he and Nathan Lauster, a UBC sociology prof and frequent writing partner, have used the Canada Housing Statistics Program data on non-resident ownership to downplay the role of foreign ownership. Their claims have always revolved around the relatively modest share of property owned by non-residents, suggesting that this modest share could not possibly have had a large impact on Vancouver’s housing market. (See for example here.) They thus posed as the people “following the data” to “fact-check the swamp-drainers”. However, my report used that very same data to show that this analytical approach was wrong – modest ownership share figures could very well be consistent with major effects on housing prices, and that’s what the data indicated. The report, in short, served to shut down these attempts at misdirection.

As I concluded in the report: “We should be thankful that this data has been gathered, so that we can cut through some of the misinformation that has surrounded the topic. Those who have cited modest “stock” figures for foreign or non-resident ownership, in order to allege its “minimal role”, have been leading us astray.”

This still holds today, and those who were trying to lead us astray in the past are simply continuing the same routine now, hoping that we don’t notice the flimsiness of their case.

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Lauster’s critique was subsequently dismantled by Rose (available here), but again the initial blog critique served its purpose to “muddy the waters”.
Appendix A: Figure 3 (from my report), first with Wozny’s data, then Census income data

Source: CRA; Statistics Canada; CHSP; REBGV; FVREB.