Discussion of "The Macroeconomics Effects of Neighborhood Policies: A Dynamic Analysis"

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June 18, 2026

I. Introduction

How should macroeconomic policy evaluation tools be adapted when they are used to address traditional microeconomic policy questions, such as how should governments respond to disadvantaged neighborhoods? My discussion of an excellent paper by Alessandra Fogli, Veronica Guerrier, Mark Ponder and Marta Prato particularly focuses on the larger issues of translating macro tools to micro settings. The primary point of the discussion is that while an estimate of the welfare consequences of a policy may be sufficient in macroeconomic settings, such as setting benchmark interest rates where there is no obvious policy default, the level of certainty about the welfare associated with a policy change is critical in microeconomic settings where current policies are the natural status quo. Knowing the level of certainty is necessary when there is a default because economists and policy makers will only want to invest their time changing a policy when they are sufficiently confident that the alternative is an improvement.

The focus on the paper by Fogli, Guerrieri, Ponder and Prato (henceforth Fogli et al.) focuses on the ancient urban policy question of whether we should invest more in disadvantaged areas or make it easier for parents and kids to escape from those areas. A growing literature, associated above all with Chetty, Hendren and Katz (2016), finds strong causal effects of growing up in particular places. Does that mean that policies should move children out of those areas or provide those areas with transfers, investments and better incentives? The natural experiments that are the gold standard for modern empirical work have difficulty answering these questions. These typically involve small-scale interventions, such as providing a small number of parents with housing vouchers, rather

than the much larger policies that could actually reshape the destiny of communities and regions.

Alessandra Fogli, Veronica Guerrier, Mark Ponder and Marta Prato have addressed this intellectual shortcoming with a smart and sensible paper that uses a general equilibrium model to analyze the long run impacts of large-scale policies. They contrast a relocation policy, with a place-based transfer (PBT) policy and a place-based investment (PBI) policy. Their results cast doubt on the efficacy of massive relocation, largely because the relocated families change the nature of the places where they are moved. The transfer policy induces poor people to stay in poor neighborhoods, which echoes 50 years of complaints against policy that subsidizes dysfunctional areas (e.g. Kain and Persky, 1969). They are more optimistic about place-based investment programs, which "produces smaller short-run effects but, over time, resolves the trade-off by raising average welfare while simultaneously reducing inequality, lowering segregation, and improving intergenerational mobility."

In Section II of this discussion, I explain why I follow an 80 percent rule for making non-academic pronouncements, which means that I need to be at least 80 percent sure that my policy guidance is correct before I make any public statements. Section II argues that this rule is appropriate in settings where there is reasonably well-functioning default policy, which can be the policy status quo or just policy neutrality across places or industries. The 80 percent rule would be wrong in areas like the setting of interest rates where there is no natural default. I have no significant disputes with any of the choices of functional forms or parameter choices made in the Fogli et al. paper, but there is much uncertainty about their (or any) model's implications. This uncertainty both reflects the leaps of faith that are needed to accept any complex structural model and the lack of empirical evidence about the large interventions that are the subject of this paper.

The implication of this discussion is that policy simulation exercises, like those done in this paper, would benefit from more focus on the uncertainty about their model's predictions. The easy way to embed uncertainty would be to use the standard errors of the empirical moments that are used in the model. The more difficult, and assuredly more important, form of uncertainty relates to our uncertainty about the model itself. If differences in neighborhood outcomes reflect the spillover model assumed in the Fogli et al. paper then, as they show, dispersing lots of poor children into richer neighborhoods would tend to reproduce the conditions now found in poorer neighborhoods. If instead, poorly performing neighborhoods have gotten themselves into a bad equilibrium of some sort, then just disrupting the status quo might create something much better.

Section III argues that it makes more sense to focus on policy simulations that are plausible in the United States or the European Union, and to consider the political difficulty of implementing a policy when considering its merits. This section follows Djankov, Glaeser and Shleifer (2026) and discusses the value of focusing on reforms that face fewer veto points and are therefore in the "reform possibilities set." I then argue that large scale relocation is not in that set, although polices that permit more construction in better neighborhoods might be. One key distinction between expanding access to better neighborhoods with vouchers and expanding access by reducing housing regulations is the selection of who moves into the better neighborhood. Section III also contrasts reform possibilities today with the reform possibilities in the late 1960s, when Kain and Persky were writing in the shadow of the Fair Housing Act.

Some forms of place-based transfers and place-based investment are certainly in today's reform possibilities set, and the most policy-relevant conclusions from this paper relate to the comparison between these two approaches. A useful extension of this paper could be to focus on sufficient conditions for determining when a value of investment is better than a value of cash. Effective investments in poorer schools seem likely to reduce long-run inequities more than pure cash transfers, but many place-based investments are spent in ways that seem much worse than cash. Did public investment in Sheffield National Centre for Popular Music generate any sizable local benefits?

Section IV of this discussion focuses on place-based incentives, which are different from either place-based transfers or investments. These incentives could take the form of employment subsidies or payoffs to students who read more, as in Fryer, Devi and Holden (2016). Section IV discusses that conditions under which place-based incentives generate more benefits than person-based incentives and particularly highlights the role of complementarities in individual behavior. Section V concludes.

II. Public Policy Simulations and the Eighty Percent Rule

There is a legal distinction between the standard of proof needed in a criminal case ("beyond reasonable doubt") and the standard needed in a civil case ("preponderance of the evidence") that makes sense. In a criminal case, there is an obvious normal outcome (non-conviction) and we should expect a lot of evidence before we stray from that norm, especially given fears about public sector overreach. In a civil case, there is no equivalent normal outcome. Why should we assume that a supplier who allegedly failed to deliver a product did nothing wrong? Consequently, it makes sense to rely on one's best guess.

There is a similar distinction between different forms of economic policy advising. There are some fields in which there is an obvious normal outcome, typically non-intervention, and other fields, especially monetary policy, in which there is no equivalent norm. In the fields in which non-intervention is likely to be the right answer in the absence of significant market failure, then it may make sense to apply a relatively high standard before offering policy advice. Since I work in the fields of urban and housing economics, where non-intervention is often a reasonable option and it is costly to change the status quo, I try not to say something unless I am 80 percent sure that I am right. By contrast, there is no equivalent fallback when the Federal Reserve influences interest rates or money supply and consequently, I would expect monetary economists to make pronouncements based on their understanding of the "preponderance of evidence."

One way to understand why macroeconomists generally have so much more enthusiasm than microeconomists for welfare calculations generated from complex equilibrium models is that macroeconomists occupy a totally different policy-environment. Macroeconomists often address Central Banks who must make sensible decisions about monetary policy based on the preponderance of the evidence. Central Banks also employ sophisticated economists who are able to interrogate complex models. For microeconomists to impact policy, they typically need to change the status quo and that requires them to speak to non-economists. Consequently, they are bound by a standard that is more like "beyond reasonable doubt."

The Fogli et al. paper occupies a middle ground between pure scholarship and a policy advice. In an academic setting, bold hypotheses are welcome and an 80 percent rule makes no sense. In The *Logic of Scientific Discovery*, Karl Popper wrote that "theoretical science aims, precisely, at obtaining theories which are easily falsifiable," meaning that the theory "allows the empirical world only a narrow range of possibilities." Popper's statement, which I find compelling, can be interpreted as meaning that academic hypotheses are better when they are less likely to be correct, which suggests that we might impose a maximum probability of being correct for academic ideas rather than a minimum probability of being correct.

Moreover, there are some spatial policy debates that should require less certainly. For example, if a government was debating between two policies of equal cost, then typically the lower standard is appropriate, unless one of the policies is the status quo and is working well. If a government is having a debate between new, equally costly PBT and PBI interventions, then it surely makes sense to choose whichever policy appears to have the highest expected returns, presumably also factoring in our estimates of the distribution of possible returns.

Similarly, a lower standard of certainty is appropriate when discussing where to put investments that are going to be put somewhere. If a state government is already sending state money to particular schools, then we can rely on a lower level of certainty when providing advice about how to allocate those funds. Similarly, if a municipality is building a bus rapid transit system and wants to know where to put the routes, then it makes sense to use a structural spatial model like the International Growth Centre spatial model developed by Nick Tsivanidis (https://www.theigc.org/initiatives/cities-spatial-model), despite the significant uncertainty related to both parameters and functional forms. The current standard, which is done well by Folgi et al., is to look one by one at a few of the more controversial parameters. That procedure is helpful but doesn't really give us a full sense of the uncertainty surrounding the model's welfare predictions.

An alternative, and far more radical approach, would for all sophisticated policy simulation to focus far more on standard errors than on welfare point estimates. The relatively simple version of that is to use the standard errors that come with the empirical estimates that are being used for parameter values. A reasonable approach might be to assume that these parameters are drawn from a distribution suggested by the mean and standard error of the estimates. With these distributions, it would be easy to use Monte Carlo simulations with thousands or millions of draws. Presumably the parameter draws could be assumed to be independent, although it might also be interesting to think about simulations.

The harder form of uncertainty relates to the model itself, which relates both to functional forms and to the very nature of the underlying mechanism. For example, it should be relatively easy to generalize the spillover function in the Fogli et al., to allow for stronger or weaker diminishing returns to these spillovers. It is harder to think about how to sensibly allow for spillovers to reflect a totally different mechanism.

For example, assume that all teenagers make a one-zero choice about crime and there are strong complementarities across choices, as in Glaeser, Sacerdote and Scheinkman (1996). Further assume that the complementarities are so strong that neighborhoods become high crime or low crime areas. Over time, better educated parents leave the high crime areas, and consequently there is a strong association between crime and low levels of schooling. Moreover, the crime level has a negative treatment effect on children, so this does impact intergenerational mobility. In this case, you can move a lot of children from the high crime area to a low crime area with little chance of changing that equilibrium. In this case, moving children into the high opportunity area would do great things.

To be clear, I believe that model in Fogli et al. is far more sensible than the model that I just suggested. I further agree strongly with their conceptual point that moving large numbers of poorer kids into high opportunity areas is likely to have spillovers on the receiving

neighborhood and have impacts that are quite unlike what happens when one person moves. Indeed, I have heard (and made) that point in seminars for 30 years. But if we are to believe the welfare simulations generated by this model, or any similar equilibrium model, it would be good to have a better methodology for establishing standard errors.

III. The Reform Possibilities Set

Djankov, Glaeser and Shleifer (2026) investigate over 3,700 attempts at economic reform around the world. We document that proposed reforms that face more veto points, such as legislation, are far less successful than simple administrative or technological changes. We also document that the frequency of reform attempts across countries is strongly correlated with the success rate of reform attempts, but not with the effect of reform on related outcomes. We conclude from this that economists should think more about what reforms are likely to succeed, and not just about the social impact of reform. This conclusion also pertains more to microeconomists who are trying to significantly impact the status quo than to macroeconomists who have direct influence on monetary decisions that occur constantly.

What are the policies that are related to the Fogli et al. paper that are politically plausible, either in the U.S. or Europe? It is easy to imagine reforms that alter the voucher program modestly while keeping spending levels constant. Targeting housing vouchers towards parents with young children is definitely in the reform possibilities set. Imposing neighborhood constraints is also possible, but as the original Moving-to-Opportunity experiment observed, more constrained voucher recipients are less likely to use their vouchers. But I have trouble imagining a vast increase in the size of the voucher program.

I propose that doubling spending provides a reasonable upper limit for plausible permutations of existing programs that already cost more than $25 billion per year. As such programs only exist because they have advocates, and because those advocates have been pushing to expand those programs for many decades, it is almost inconceivable to me that even a Democratic electoral tidal wave could do more than that. A better approach than this rule of thumb would be to look at the scale of shifts in the program over the last 40 years and take the largest historical shift to be conceivable upper limit for policy simulations.

The hope would be that by concentrating on smaller and more plausible policy perturbations, it would be easier to focus on the standard errors associated with welfare simulations discussed in Section II. Even if the nature of simulations makes it almost as

cheap to simulate extreme policies as it is to simulate plausible policies, focusing on smaller simulations will produce a narrower range of welfare outcomes. A slightly different spillover functional form will have less impact when the change in neighborhood composition is less extreme.

Access to higher opportunity neighborhoods can also be produced by building more housing in those neighborhoods, especially if that housing is targeted towards lower income people. Such building can either be accomplished through subsidy programs, such as the Low-Income Housing Tax Credit (LIHTC), or simply by reducing the regulatory barriers to building in high opportunity areas. The Wharton Residential Land Use Regulatory Index, which measures the difficulty of getting a building permit in a place, is much higher in places that appear to offer more opportunity for lower income children, as measured by Chetty and Hendren (2018).

Some building policies could produce results that closely resemble the assumptions about mobility in Fogli et al., but others would not. For example, how different would inequality and upward mobility be if Dallas and Miami, or indeed all of America's metropolitan areas, had built as exuberantly after 2000 as they had built between 1980 and 2000. That extra building does not generate integration per se, but it would have perhaps made it more plausible for poorer parents to buy into middle income neighborhoods. This type of counter-factual would have to figure out what is different between old communities and new communities. One explanation for the much lower levels of racial segregation found in western metropolitan areas is that those areas, unlike eastern and midwestern cities, are not still straightjacketed by patterns developed during the mid-20th century peak of racial segregation (Cutler, Glaeser and Victor, 1999).

What housing policies are in the reform possibilities set? For an existing policy, the LIHTC, presumably the same doubling of current spending is a reasonable upper limit. For private sector construction or regulatory reform, the natural upper barrier is the building seen in the relatively recent past. This could be done either by assuming the same rates of construction as in the past, or assuming that the housing supply curve was the same as in the 1980s and 1990s and then estimating how much housing would have been produced if those supply curves continued into the 2000s and 2010s.

Fogli et al. treat Place-Based Transfers and Place-Based Investments as completely different concepts. In practice, there is a continuum of place-based policy possibilities that range from pure cash transfers to other policies that have different levels of local government control. The two most important ways in which these policies differ are (1) how much do parents value this policy (which will be maximized with cash transfers) and (2) how much does this policy directly impact the human capital of children. In the U.S.,

place-based transfers typically occur at the state level, combine some level of Federal and state-level funding and also condition on individual characteristics, such as being unemployed or being both sick and poor. Both the U.S. and the E.U. have programs that give aid after a natural disaster, but that seems like a very different.

As I have trouble imagining, any U.S. Federal cash transfer program that would differentiate between cities or neighborhoods within states, I doubt it makes sense to put more effort into analyzing the long-run impacts of such programs. Moreover, the relatively negative impact of such programs, suggested by Fogli et al. analysis, seems likely to be correct to me. Consequently, I will be happy if this paper becomes the definitive, last word on place-based cash transfers.

But place-based support for local government entities, like schools, is very much in the reform possibilities set and worthy of far more investigation. Not all place-based investments took the form of highly-productive improvements in education. D'Amico (2025) analyzes a major European Union regional policy, and finds that "EU transfers are less likely to be invested towards technological development and innovation in regions with many unskilled workers, which in turn leads to fewer jobs created per euro." As this finding suggests, the more that the policy grants locals control, the more likely the policy will cater to the preferences of the parents rather than investments that will primarily benefit their children.

What place-based investments are in the reform possibilities set? In the U.S., there is a sharp difference between Federal and State policies. The Federal government gives relatively little directly to local governments. Community-development block grants are one of the few such programs, but the most important Federal place-based program that resembles their simulated policy is the Title 1 funding of the Elementary and Secondary Education Act. Other Federal programs, such as the School Lunch Program or Head Start, are person, not place-based, so that if a child of poor parents were to move to a higher opportunity area, that child would still be eligible to participate in Head Start and to receive a free or reduced-price lunch.

Consequently, the most immediately policy-relevant place-based investment reform to evaluate in the U.S. context would be a plausible increase (again, perhaps doubling) in the Elementary and Secondary Education Act. Connecting with this or any concrete proposal would also make it easier to determine whether the quite strong results found by Biasi et al. (2025) are really appropriate for policy simulation under consideration. Another quite plausible reform is to imagine changes to the education policies of U.S. states, which often redistribute between school districts. Simulating state policies offers the particularly rich challenge of addressing migration between states.

The European Union offers a rich menu of spatial policies, although I don't know that any of them look exactly like the extremely benign place-based investments studied by Fogli et al. That proliferation of policies makes the value of applying the methods of Fogli et al. to E.U. policies particularly high. When considering the place-based interventions in the E.U. and U.S., it would be particularly valuable to think about why parameters are different in the U.S. and Europe and what explains those differences. Historically, Americans were much more mobile than Europeans, but the reduction in U.S. mobility since 2000 has eroded some of that difference.

IV. Place-Based Incentives

Austin, Glaeser and Summers (2018) focus on the joblessness in America's Eastern Heartland and discuss policies that change the incentives to create jobs and to work in difficult parts of the U.S. That analysis can, of course, also be faulted for discussing policies that may not have been particularly realistic either then or now. Nonetheless, the wide heterogeneity in regional conditions in the U.S. means that a one-size-fits all policy towards unemployment or disability insurance for housing may make far less sense than a policy than responds to local conditions.

Austin, Glaeser and Summers (2018) provided evidence that suggested that the regions differ in their elasticity of joblessness with respect to labor demand shocks, generated either by Federal spending or national industrial changes. A positive shock to a low joblessness area, like Seattle, appeared to do less to increase employment than a shock to a high joblessness area, like eastern Kentucky. In a variant of the Bailey-Chetty unemployment model, a greater responsiveness of joblessness to the returns to work implies either a lower unemployment payout or a higher subsidy for working. Responding to local conditions can either mean different payouts in some areas than in others, such as a large Earned Income Tax Credit in Eastern Kentucky, or a twisted payout structure that keeps average welfare constant, such as allowing people who are receiving disability insurance in Eastern Kentucky to keep more of their earnings but also lowering their base payout.

As future researchers evaluate policies for disadvantaged neighborhoods, it may also make sense to follow this logic and consider place-based incentives, as well as place-based investments. In principle, the right incentive scheme might be able to turn a payout going to parents into an investment that helps children. If the same incentives deliver a larger change in behavior in poorer places, for example, then it might make sense to have stronger incentives in those places.

For example, Brazil's *La Bolsa Familia* program provides cash transfers to poorer parents as long as they vaccinate their children and ensure that their children attend school without too many absences. In the spirit of the reform possibilities set, it is far more plausible to consider an actual reform of the Brazilian program in Brazil than to imagine bringing to the U.S. at a national level. At the local level, New York City experimented with a Bolsa Familia-like structure between 2007 and 2010 (Courtin et al., 2018).

Bolsa Familia targets parents, but there are also incentives strategy that target children, teachers and local government leaders. Fryer, Devi and Holden (2012) report on a series of randomized control trials looking at providing incentives for children in schools. The key finding of that paper was that providing incentives for inputs into learning, such as reading, was much more effective than providing incentives for final outputs, such as school grades. A national student incentive program is implausible, but it is certainly possible that a large philanthropy or even a state government might offer student incentives, and almost surely it would make sense for those incentives to differ by school.

No Child Left Behind was a federal education policy that both allocated funds to school districts and created incentives for schools to improve outcomes for their lowest performing students. As the policy created different incentive for different schools based on their test score distribution, it was innately a form of place-based incentive. While excellent work has evaluated the impact of No Child Left Behind (e.g. Dee, Jacob and Schwartz, 2012), much less has been done to work on the optimal structure of school-based incentives. Obviously, any test-based incentives must also reckon with the risk of teacher cheating.

V. Conclusion

The Fogli et al. paper is a sensible and strongly implemented equilibrium model-based policy simulation of three possible place-oriented policies. The implementation of those simulations highlights the advantages of place-based investments over place-based transfers, and that large scale housing voucher interventions that generate large-scale mobility will have some negative consequences. Those conclusions seem broadly correct to me, with two major caveats. First, policies that reduce regulatory barriers to building in high upward mobility neighborhoods could deliver policy outcomes similar to the voucher expansion but at a far lower cost to taxpayers. Second, I do not believe that most place-based investments, especially those that have nothing to do with schools, yield anywhere near the returns found by Biasi et al. (2025).

I take this discussion, however, to address the broader methodological issue of how macroeconomics policy evaluation tools should be adjusted when they are used to address microeconomic topics. A central difference between macroeconomic and microeconomic policy making is that with many macroeconomic questions there is no natural policy standard, and policy makers are often making a decision about a continuous policy action, such as setting interest rates. In these settings, a point estimate of the advantage of moving rates up or down may be sufficient. In microeconomic settings, there is often a natural reference policy that will prevail if nothing major changes.

When there is a natural reference policy, then changing that policy is likely to take a great deal of effort both by economists and policy makers. Consequently, it is important to know not just the expected payoff from that policy but the level of certainty about that payoff. In a sense, standard errors are needed because any would-be reformer should engage in Bayesian shrinkage when thinking about whether the costs of undertaking reform offset the benefits. As macroeconomic tools switch to microeconomic questions, it also makes sense to focus on reforms that are within the reform possibilities set rather than on anything conceivable. The extra work needed to determine the level of certainty as well as the mean effect is only justifiable on reforms that have any chance of being enacted.

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