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"It's Obvious, It's Wrong, and Anyway They Said It Years Ago"? Paul Krugman on Large Cities

Andrew M. Isserman

Regional Research Institute
West Virginia University
Morgantown WV 26506 USA

Paul Krugman does many important things extremely well. Whether he has created the basis for a new development economics (Krugman 1993b; Stiglitz 1993) and a new regional economics or economic geography (Krugman 1991a, 1991b, 1991c, 1993a, 1994) remains to be seen. His research and proclamations have infused these fields with new enthusiasm and energy. Krugman argues that because mainstream economists could not treat the messy problems of regional and development economics with formal mathematical models, they ignored those fields. Through his own research, he demonstrates that those problems can be studied with today's formal mathematical models. Hence, the fields should grow in prestige and importance. This promise of recognition and renaissance is heady stuff to practitioners of the old development and regional economics. Already Krugman has drawn more research, more students, and more attention to the problems of regional economics.

Yet some charge Krugman with ignoring prior work and, worse yet, with presenting no new insights. To use his own words, "I am having a terrible time with my current work on economic geography; referees tell me that it's obvious, it's wrong, and anyway they said it years ago" (quoted in Gans and Shepherd 1994, p. 178). How valid are these criticisms with respect to his article on large cities in developing countries (Krugman 1996)? First, I discuss whether Krugman's model falls short by being too simple. Second, I apply a very demanding criterion: "Does it teach us new things?" Third, I put Krugman's new economic geography into the broader intellectual context of the history of regional science.

Is the Model So Simple That It Yields Misleading Conclusions?

Krugman's model can be distilled even further than his excellent summary. People concentrate in cities because cities offer a greater variety of goods, and firms concentrate in cities because cities offer larger markets for their goods. In perhaps a spatial wrinkle on Say's law, firms create their own markets by concentrating their locations. Now wages must be lower in

the countryside because firms have to absorb the costs of transporting their goods to the city market, and prices are higher in the countryside because of the cost of transporting goods from the city. These lower wages and higher prices do not drive everyone to the city, because higher rent and commuting costs there balance the city's higher wages and lower prices.

This model suggests that free trade can reduce the size or, at least, slow the growth of primate cities by opening up alternative markets for producers and new sources of goods for consumers. The firms concentrated in primate cities no longer need to maximize their access to the national market, and (over)urbanization is no longer needed to create a market. Likewise, with access to goods from around the world, consumers need not locate in the city and pay the high commuting and land costs there.

How might the model and its predictions fall short? One possible argument is to recite the many determinants of urban growth that the model ignores. Such an argument misses the very point of modeling, however. The main advantage of simplification or modeling is that the focus on key attributes, to the exclusion of others, can bring new insights. A more interesting argument is to explore whether the chosen attributes are perhaps misattributes and whether the new insights are perhaps erroneous implications of ill chosen simplification. In other words, let us evaluate the model and its predictions on its own grounds, that is, by examining how it defines its key components: cities, countryside (or smaller cities), traded goods, transportation infrastructure, and migration.

Primate cities in the model are production points that can offer wage premiums because they are the market centers and have low transport costs to reach that market. They are monocentric, with commuting costs increasing and rents decreasing with distance from the center. They have more people than other places. That's about it for the primate city. The countryside (or a smaller city) must offer lower wages to absorb the costs of transporting goods to the city market. That's about it for the countryside. As Krugman says, the model is simple.

How might this characterization of city and countryside be mischaracterization and oversimplification? First, the primate city is not only a manufacturing center. Typically, it is the government center, the financial center, the educational center, the intellectual center, the transportation and communication center, the manufacturing center, and the center of just about everything. Thus, even if a greater proportion of its production is sold to foreign markets as a result of free trade, only one part of its economic base is freed from dependence on the primate population concentration. Its other functions, particularly finance, government, trade, and communication, might expand and reinforce its dominant position. Although Krugman acknowledges some of these roles, his formal model does not. Consequently, the

model may yield erroneous conclusions regarding the overall effects of free trade on population concentration.

The posited manufacturing trade effect, moreover, might not happen. Its basis is the substitution of foreign markets and goods for primate city markets and goods. For that substitution to occur, primate city manufacturers must be able to produce for the international market, and city residents must be able to afford the goods produced by the international market. To the extent that primate city manufacturers are unable to enter the world market and world goods are unable to pervade the city market, the posited trade effect will be weaker. To the extent that local producers will be eliminated by foreign competitors or by competitors newly located at now optimal points that serve both the world and primate markets, the trade effect will be stronger. In the model's theoretical extreme, the primate city will cease to exist if all manufacturing moves to the third point, that is, out of the country to the rest of the world. In short, we cannot know the nature or size of the trade effect on population concentration until we know more about what is produced and producible in the nation, what goods for what markets, as well as more about the nation's income levels and distribution systems.

Meanwhile, what is going on in the countryside? Presumably, some small cities and rural places will grow because of their production for world markets. Rural areas, however, produce food, natural resources, and other goods and services that are tied to place-specific attributes. Many of these goods cannot be produced in cities. Leaving these goods out of the model becomes a problem when drawing conclusions about the effects of trade on urbanization. Trade liberalization may have very big effects on agriculture, for example. Unlike manufactured goods, agricultural goods might be perfect substitutes for goods now consumed in the world market. Agricultural trade may evolve precisely as Krugman postulates for the primate city's manufactured goods.

If so, trade liberalization might have serious repercussions for primate cities, but opposite to those Krugman posits. Imports might destroy domestic agriculture, as is feared in France, Japan, the Republic of Korea, and many other countries. A plausible outcome in developing-country settings, therefore, is more, not less, migration from the countryside to the primate city. Alternately, trade liberalization might enable the country to expand its exports, but quite plausibly, by making agriculture more capital intensive in order to increase output for world markets. This change also might destroy the current agricultural system and cause additional rural unemployment and migration to the primate city.

The nature of transportation infrastructure in the model is important, too. There appears to be an inconsistency between what is assumed in setting up the model and what is implicit in the trade liberalization predictions. Crucial to Krugman's predicted trade effect is the ability to produce for

world markets from locations outside the primate city. Yet, the model argues that production is concentrated in the city largely because of high transportation costs to the city from elsewhere in the country. Those transportation costs push people to the city as consumers and push firms there as producers. Presumably, alternative locations are too costly in transportation terms to be served by the goods from the primate city or to produce goods to serve that city. That is why we have the primate city, transportation savings—at least, according to the model.

Enter trade liberalization, and the transportation system seems suddenly different. Now it suffices to serve more points. For Krugman's Mexican border case, that argument is easy: the transportation system that matters is outside the country once the border or port is reached. For other cases, the story becomes a bit mysterious. We must now assume an adequate transportation infrastructure from a place to the world market, whereas before we assumed no adequate system from that place to the primate city (adequate meaning sufficient to take away the primate city's locational advantage). Thus, for the predicted trade effect to occur, we must change the world of the model or at least the country's transportation infrastructure. Creating additional markets through trade liberalization will weaken the domination of the primate city only if those markets can be reached at reasonable cost from several points within the nation. Again, the actual outcome will depend on the particulars of each national case.

Finally, migration is implicit in the model. People move to the primate city until increases in land rents and commuting costs bring the urban system into equilibrium. In reality people also move abroad. Trade liberalization that fosters greater production at home might reduce emigration—as NAFTA supporters argued. Thus, trade liberalization might spur migration within the country, particularly to places where output and employment demand increase. Those places might well include the primate city, again confounding Krugman's posited trade effect.

I would be delighted to learn that the messy issues raised here can be readily resolved within the model or that the model's implications hold despite these messy thoughts. I fear, however, that the model is probably too simple. The true answer on the trade effect has a lot to do with the particular circumstances of the country concerned, including what it can offer to international trade, what transportation systems and other infrastructure it can muster, what will happen to its rural sectors, the spatial distribution of its resources and population, and so on.

Krugman posed the question, "What can we learn from looking at urbanization and regional inequality in developing countries through the lens of the specific approach to economic geography that has emerged out of the new trade and growth theories?" (Krugman 1996, p. 7). A look through this "new

lens" may not give us an accurate view of the world, but it does help us identify what we do not know. Our blindspots concern the nature of cities, countryside, production, infrastructure, and migration in individual countries—all standard elements of the old economic geography. By concluding that the knowledge of the old economic geographers, the area specialists, can teach us a lot about the effects of trade liberalization on urban concentration, I am calling not for a return to the old economic geography but for recognition of the virtues of both old and new. Understanding the relationship between trade and urbanization in developing countries calls for still richer models that are rooted in critical "ground truth" on key parameters and variables.

Does Krugman's Model Teach Us New Things?

Continuing with Krugman's "what can we learn" question, this section examines his policy conclusions regarding trade, centralization, taxes, and infrastructure. Are they surprising, new insights into our world, do they flow directly from the model, or are they actually the product of "older" less formal reasoning? In short, just what have we learned from that new lens of economic geography?

Krugman states, "One wants to be very careful about drawing policy implications from any discussion of urbanization and regional growth" (Krugman 1996, p. 22). He proceeds, nonetheless, perhaps because policy implications might well be the whole point for a World Bank audience. He concludes:

> That said, the general moral of the models described here seems to be that if you want Third World cities to be not quite so big, this desire may be served indirectly by the kind of economics policies currently favored by the Washington consensus for other reasons. Liberal trade policy appears likely to discourage primate city growth; so does a reduction in state intervention and a decentralization of power. Investment in better transportation infrastructure—part of the traditional role of government—also seems to work in the same direction.

Note the qualifiers, "may be fulfilled," "appears likely," and "seems to work" (Krugman 1996, p. 23). Perhaps we hear in them the wise author's confession: Maybe the world does not work this way, but it is the best my models can do at this point.

The fact that changes in trade policy can create production advantages for certain regions is conventional knowledge, not just of economists (e.g., Ruane 1983; Williamson 1986), geographers (Sheppard 1982; Warf and Cox 1993), and sociologists (Portes 1989), but also of French farmers and other folk. That trade liberalization will discourage the growth of primate cities can be predicted simply by pointing to opportunities that will be created elsewhere in the country. Yet neither such common sense reasoning nor Krugman's formal

model ensures that the prediction will be correct. Too many factors, absent from both, will determine what will happen in a particular country.

Krugman's conclusion that a reduction in state intervention and a decentralization of power should discourage primate city growth does not draw directly on the formal model. It follows from Krugman's less formal discussion of political centralization and regional inequality. Political centralization is "almost surely the most important reason" why Third World cities grow so large, according to Krugman. Yet, political centralization does not appear explicitly in the model, meaning that any conclusions regarding its effect do not result from the model. Centralization can be linked to the model, as Krugman does, by pointing out that government is itself a major employer. He also asserts that if the government is more interventionist, access to government is more important, so more lobbyists will be contributing to the economic base and size of the city. Thus, decentralized government and less-interventionist government will mean fewer people in the primate city. That argument may well be true, but it does not qualify as a new insight *provided by the model.* Furthermore, trade liberalization need not be accompanied by decentralized and less interventionist government, despite being part of the same Washington consensus.

Krugman's political section contains an interesting formal modeling exercise. It shows that if the government taxes rural income and spends the revenues in the city, further concentration in the city will result. That illustration, a nifty by-product of the model, is also not a new insight. Agricultural economists and others have argued for a long time that overtaxation of the countryside through low food prices has subsidized cities and stimulated urban and industrial growth. It was a key concept of Soviet economic planning, it underlies the Chinese economic system, not to mention feudal economies, and it is a shibboleth of the urban bias literature. In this case, the model merely illustrates accepted knowledge, a nice result if not a new insight.

Finally, there is Krugman's argument for investment in infrastructure to discourage primate city growth. This recommendation comes from the model and is a direct reflection of the role played by transportation costs. Yet it is also an old point in regional and development economics (Hansen 1965; Hirshman 1958; Williamson 1965). Poor transportation linkages within the country and transportation systems focused on the primate city reinforce the city's dominant role. As Krugman notes, these points seem intuitively obvious, as does the related policy recommendation.

Perhaps the most important contribution of the article, even if it is not based on the model, is that a whole constellation of popular policies can contribute to urban deconcentration. That the Washington consensus policies can lead to population decentralization is a striking, intriguing observation. Does it matter that it does not follow directly from the model? Does it matter

that each piece of that conclusion can be found in earlier work? In short, does it matter whether Krugman here is guilty of his referees' charges: "it's obvious" and "they said it years ago"? No!

The fact that Krugman's quest for rigorous analytical tools leads to attention, insights, and connections that are not based on those tools is just one of the virtues or byproducts of modeling. To require that a model teach us new things is to set a very demanding standard. Most models do not meet it. Usually, we are content if our modeling exercises reproduce known things or focus our attention on important parameters. Even if some of Krugman's conclusions are wrong, others obvious, and none virginal, his article focuses our inquiry, raises stimulating questions, and makes us appreciate the limits of our modeling capabilities and our knowledge.

Lessons for the Second New Economic Geography From the First

Krugman is not the first American economist to launch a new economic geography based on mathematical economic theory. Walter Isard did so in the 1950s. He called it regional science, but it was in fact a new economic geography or geographical economics based on applying the contemporary tools of economics to spatial analysis. Isard offered formal theoretical models of location, especially of the firm (Isard 1956, 1990a), and empirical methods of analysis, including most prominently regional input-output models (Isard et al. 1960; Isard 1990b). Krugman's work arguably is a continuation of Isard's concern with the mathematical modeling of economic behavior in a spatial context. It incorporates space in the same way, that is, primarily through transportation cost. Isard focused on the substitution principle as Krugman does on increasing returns. Isard (Isard et al. 1969) and Krugman (1994) also share a fascination with the search for general theories and the potential of theoretical frameworks originating outside economics.

More striking similarities exist. Both Isard and Krugman have written several influential books published by MIT Press, and each attained early success within mainstream economics. Isard, for example, had nineteen pieces published between 1942 and 1953 in the *Quarterly Journal of Economics*, *Review of Economics and Statistics*, *Journal of Political Economy*, *American Economic Review*, and *Econometrica*. (In the same period, he also published papers in *The New Republic*, *The American Scholar*, *Social Forces*, *Yale Review*, *Harvard Business Review*, *American Sociological Review*, and *Economic Geography*, among others.) These two established, precocious mainstream economists criticized the economics of their time in very similar terms: for confining "itself to a wonderland of no spatial dimensions" in which "everything within the economy is in effect compressed to a point" (Isard 1956, pp. 25–26) and for modeling "countries as dimensionless

points within which factors of production can be instantly and costlessly moved from one activity to another" (Krugman 1991a, p. 2).

These similarities make it worthwhile to review briefly the intellectual history of Isard's regional science to gain some perspective on Krugman's new economic geography. Isard's regional science succeeded, but far more spectacularly among geographers than economists. In the 1960s and 1970s regional science was arguably geography's hottest field. Numerous presidents of the Association of American Geographers were active regional scientists. Regional science offered the old economic geography a message not unlike that of Krugman's. It sought laws of behavior over space, focused on the universal, offered new mathematical techniques, and promised greater respectability for geography within the social sciences. In fact, geography could now become a true social *science*.

The story of regional science is too long to tell here (see Isard 1990a, ch. 12; Isserman 1995), but the regional science revolution, the quantitative revolution in geography, led to counter-revolution. Probably the majority of geographers today views regional science as too theoretical, too general, too mathematical, and too neoclassical with too little region and too much science. Regional science has even been ridiculed as an intellectual "cul-de-sac" whose "influence has been perverse, obstructing relevant theory, technique, and policy" (Holland 1976, p. 18). Nonetheless, the analytical, methodological side of regional science thrives within geography's mathematical, economic, and demographic specialties. Research foci include spatial statistics, locational analysis and modeling, spatial interaction modeling, and aspects of geographical information systems.

Within economics, Isard's *Journal of Regional Science* became, and still is, the leading journal of regional economics. Yet, until very recently, papers on regional economics were rare in the major economics journals, such as the *American Economic Review*, *Journal of Political Economy*, and *Quarterly Journal of Economics*. Krugman (1991a) offers an explanation:

> The neglect of spatial issues in economics arises for the most part from one simple problem: how to think about market structure. Essentially, to say anything useful or interesting about the location of economic activity in space, it is necessary to get away from the constant returns, perfect competition approach that still dominates most economic analysis. As long as economists lacked the analytical tools to think rigorously about increasing returns and imperfect competition, the study of economic geography was condemned to lie outside the mainstream of the profession. Indeed, as standards of rigor in economics have risen over time, the study of location has been pushed further and further into the intellectual periphery (p. 4).

Krugman (1993b) makes much the same argument for development economics: "Development theorists were unable to formulate their ideas with the precision required by an increasingly model-oriented economic mainstream and were thus left behind" (p. 28).

Krugman's new economic geography is still in its early stage, perhaps comparable to Isard's new regional science circa 1956 after publication of Isard's first theoretical book. Here are three macro lessons from the subsequent evolution of regional science:

1. The discipline of geography and its field of economic geography cannot and will not be reformed on the basis of principles from economics.
2. Regional economics can benefit from the insights of economic geography, particularly its ground truth and facts.
3. Although the models and tools of theoretical inquiry change, many theoretical issues in regional economics still end up being empirical questions.

The new economic geography need not learn the first macro lesson because Krugman, unlike Isard, shows no signs of wanting to start a new discipline or even to establish colonies within geography departments. He just likes the term economic geography (Krugman 1991a, p. xi)—but Krugman's word choice is probably as poor as Isard's was. Krugman's work is economics, not geography. The cumbersome "geographical economics" (similar to the JEL category demographic economics), if not the familiar regional economics or location theory, or even the JEL category spatial economics, would have made clear that Krugman lacks imperialistic ambitions and can ignore the first macro lesson.

The second lesson is a more difficult one. Students being trained in formal modeling perhaps lack the time and training to read enough social sciences and history relative to their mathematics and computing. That is a pity. Modeling requires simplification but without being simplistic. Modelers need to know about the things that they are modeling in order to avoid unwittingly forcing their subjects to fit the properties of the model. They must know enough to appreciate the tension between the properties of the model and the properties of the thing being modeled. If the new economic geography combines study of the real world with study of mathematical economics, it may be able to avoid the "cul-de-sac" of unreal modeling and unfounded conclusions.

The third lesson is related to the second. Accompanying the theoretical thrust of regional science was a strong emphasis on applied empirical work. Perhaps the enduring strength of the field is its data-driven hypothesis testing and its tradition of operational modeling. Regional science eventually found a useful niche between theoretical excess, on the one hand, and descriptive excess, on the other. It learned to appreciate the role of theory in disciplining inquiry and the role of the world, data, and statistical analysis in

disciplining theory. If the new economic geography does not eventually share that emphasis on empirical testing, it too may in time be ridiculed as simplistic modeling.

Early signs suggest that empirical testing will be a very strong component of the new economic geography. Barro and Sala-i-Martin (1992), Blanchard and Katz (1992), and Glaeser et al. (1992) are three prominent examples. Sometimes the new folks have not learned textbook basics of the old field (for example, Glaeser et al. reinvent the location quotient, a 60-year-old, standard measure of employment specialization). Nonetheless, the burden of having to teach new folks old things and the pain of seeing earlier work ignored are richly compensated by the exciting new work and the mainstreaming of regional economics again after some 30 years.

In summary, although the new economic geography misses some of the substantive lessons and basic concepts of the earlier regional economics and economic geography, it shows a healthy empirical orientation that in time will support the drive to richer theoretical models. Theory alone won't do—particularly when making that important stretch to policy implications. There is much to say for formal empirical regional modeling that is rooted in an understanding of the region. I look forward to the clever empirical research needed to examine and verify the policy implications of stimulating theoretical work such as Krugman's paper. I look forward confidently because nothing focuses the attention of economists better than articles published in prominent journals by prominent economists. Krugman's posited trade effect on population concentration is a good candidate to spawn empirical research in search of evidence of that effect. When that research happens, the new economic geography will have practiced the primary lesson of the regional science experience, namely, that theory alone does not create an enduring and valued field.

Conclusion

Yes, perhaps Krugman is right. His style, "maybe by claiming more originality than I really have" (quoted in Gans and Sheppard 1994, p. 178), not to mention his many achievements, does invite referees and commentators to apply unusual standards when evaluating his work. I propose another unusual challenge. At the 1992 conference, Bank President Lewis Preston stressed that research into why development takes place in some settings, and why poverty persists in others, is central to the Bank's mission (Preston 1993). Krugman's article has refocused our attention on markets, transportation costs, and government centralization to explain why development occurs in some places and not in others. Krugman has not yet focused his remarkable skills and talents on why poverty persists. I encourage the World Bank to

invite him to speak at its conference again, but this time on spatial aspects of poverty. Poverty economics, too, will benefit immensely from being stirred up by his ideas.

Paul Krugman's work and his models are worthy and exciting additions to the classic location theory of von Thunen, Weber, Losch, and Christaller. I agree wholeheartedly with his "definite conclusion...that whatever the changes made in economic policies, their implications for urban and regional development within countries are an important, neglected issue." Note that this time Krugman offers no qualifiers. This conclusion is "definite." Although we in regional economics and economic geography have not neglected these issues, to our chagrin others in academia and public agencies have ignored them. The stimulating work of Paul Krugman will make it more difficult for them to continue to do so.

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