

RANDOMISING DEVELOPMENT: GEOGRAPHY, ECONOMICS AND THE SEARCH FOR SCIENTIFIC RIGOUR

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ABSTRACT

Development economics has become something of an innovator within the discipline of economics, due to its adoption of experimental and statistical analysis techniques. In this paper I give examples of this new trend in development economics: randomised-control trials, natural experiments, specialist analytical techniques like pre-analysis plans, and evidence-driven policy evaluation. I explore this novel experimental development economics in conversation with current argumentation in economic/development geography about economics. I do this in order to ask whether this experimental trend responds to any of these geographical critiques. Although I find that this new development economics repeats many of the tendencies of economics that geographers find so specious, it does pose challenges to economic/development geography, which I explore.

Key words: Development economics, economic geography, randomised control trials, experiments

INTRODUCTION

Development economics is back. At least according to development economists themselves (Banerjee & Duflo 2010). Studies of the 'strange ways of the world outside high-income countries' (Banerjee & Duflo 2010, p. 61) were once sidelined within mainstream economics, but development economics is now recognised as a sub-discipline in which some of the most exciting and innovative economic work is being conducted (Buchanan 2010; Parker 2010; Green 2012). Many economists, even those who do not label themselves development economists, have begun to apply and test their theories in 'developing' country contexts. The reemergence of vanguard development economics, Banerjee and Duflo (2010, 2011)

suggest, is due to the subfield's potential for the vigorous integration of empirical and theoretical thinking. As development economists have historically collected rich primary data in order to test their theories, they are well suited to thinking about the empirical and theoretical together. Within this subfield 'a new economics is being born' (Parker 2010, p. 78).

In particular, development economics has emerged as an academic trailblazer because of its recent emphasis on economic field experiments. 'By enabling the researcher to precisely control the variation in the data, field experiments allow the estimation of parameters and testing of hypotheses that would be very difficult to implement with observational data' (Banerjee & Duflo 2010, p. 62). The use of field experiments, including randomised control

trials, natural experiments, and specialised analytical techniques such as pre-analysis plans, has led to new theories and creative policies, and has facilitated decision-making.

This paper considers this recent re-ignition of development economics via field experiments, in conversation with economic and development geography. In doing this, I aim to assess whether these innovations in development economics undercut geographical critiques of economics, but I also consider what they may mean for geography. This paper, then, is a step towards a 'trading zone' (Barnes & Sheppard 2010) at the juncture of development, economics and geography. A 'trading zone' is an engaged-pluralist approach to conversations between different fields of thought, an approach that does not aim for consensus and agreement, but for a dialogue that can probe and enhance our academic theories and thoughts (Barnes & Sheppard 2010). Engaging in dialogue between economic/development geography and development economics, therefore, has the potential to problematise and thus enrich both of these subfields.

The trading zone explored here is one between economic and development geography and development economics. However, this paper is also working with the conviction that the distinction between development and economic geographies is false (Vira & James 2011), and is an experiment in what such a development/economic geography might look like and provide. This essay is '[lying] down with the lion' despite warnings from others, and hoping not to become prey (Amin & Thrift 2000, p. 8). This first step into this trading zone is largely one-sided, from the perspective of an economic/development geographer reporting back from considerable time spent engaging with development economics and development economists. There are, therefore, no comments here about how economists think geographers talk about economics, but what follows is informed by such feedback in frequent exchanges. Despite these limitations I maintain that this is a necessary step towards such a 'trading zone' – just one statement in a much larger conversation – because, in reporting back to economic/development geography, I aim for openness (in contrast, for example, to some

geographers' disdain for economics; see for example Peet 2011). Although this is, in many respects, a geographical assessment of the new development economics, I specifically try to remain open to the idea that new scientific and empirical approaches in development economics may represent a serious, and perhaps progressive, revision in economics, particularly in comparison to the abstracted, mathematical models of the world that have characterised the discipline. This analysis, therefore, if not a complete transaction between development economics and geography, is in the spirit of a trading zone.¹

Influential economic geographers warn against attempting such an engagement. Amin and Thrift (2000) originally predicted an intellectual 'death' were economic geographers to even make contact with formal, neoclassical economics. Peck (2005, 2012) continues this 'defensive pluralism' (Barnes & Sheppard 2010), recommending that economic geography build its broad practices and positions within the 'archipelago' of heterodox economics, and avoid altogether the 'icy continent' of formal economics. He summarises: 'concerning if and how to fashion a constructive dialogue with orthodox economics: Don't' (Peck 2005, p. 149). Peck's (2012, pp. 113–114) misgivings stem from the (mere) epistemological and ontological divides between economic geography and orthodox economics: 'theoretical pluralism versus theoretical monism; cultural-institutionalism versus market centrism; methodological eclecticism versus methodological individualism; grounded explanation versus parsimonious reasoning; open theorizing versus closed modeling; and so forth'. Any open dialogue with orthodox economics would require that geography abandon many of its core propositions and insights – the 'sociospatially constitutive character and conjunctural historical specificity' of regional economies (Peck 2012, p. 118). And, importantly, orthodox economics, Peck (2012, p. 120) argues, is not only different to economic geography, it threatens to 'marginalize, erase or obliterate' its foundations.

Of course, there is heterodoxy within economics, including within the subdiscipline of interest here. However, Peck (2012, p. 129) suggests that those who challenge the ortho-

doxy – for instance by using ‘non-canonical methods’ like the experimental development economics explored here – risk being marginalised from key journals and graduate programmes. This is not the case with the growing and increasingly influential development economics. So popular has this experimental and evidence driven development economics become that, the world over, doctoral students are scrambling ‘for something to randomize’ (Ravallion 2009, p. 1). Indeed, its ‘doyenne’ (Green 2012), Esther Duflo, has recently won the John Bates Clark Medal (a ‘Nobel in waiting’; Parker 2010) and a MacArthur ‘genius’ award, and was posted to the White House’s new Global Development Council (Gross 2013). Easterly (2010) dubs this ‘Esther-Mania’.

Despite objections (Amin & Thrift 2000), there are several reasons for continuing to work within this trading zone and to examine the new development economics. As noted, the experimental development economics heralds an economic heterodoxy at the very core of economic orthodoxy. As such, these *Randomistas* – as the development experimenters have been nicknamed – have begun examining real world economics and economic problems ‘in the wild’, narrowing their research practices to smaller, answerable questions (demonstrating at least a little humility), and they are committed, in their own way, to social justice. In addition, it might also be worth reflecting on how these changes could affect economic geography. What might it mean for economic geography that economics is beginning to get its hands dirty (cf. Peck 2005), comparatively and historically at least, the intellectual terrain of geography?

The paper is laid out as follows. I discuss what critical economic/development geography currently knows about economics in the following section. I then present some examples of field experiments, analysing them in conversation with this geographical knowledge of economics. I show that, although some of the tendencies of economics that geography has critiqued are still evident, this empirically-focused development economics represents a partial shift away from the abstracted mathematical approaches that characterise other parts of the discipline. I conclude by reflecting

on these changes and their consequences for geography.

WHAT WE KNOW AND THINK ABOUT ECONOMICS IN GEOGRAPHY

In this section I set out some of the characteristics that geographers have argued against in economics. These characteristics are certainly not applicable to all economics, nor are these the opinions of all economic geographers. I draw from (largely Anglophone) critical economic geography – this is the ‘we’ and ‘us’ here – and outline their misgivings about formal, mainstream economics. Importantly this is not my assessment of the right-ness or wrong-ness of economics, but rather a distilling of previous geographical observations. I do this so as to later assess whether these characteristics are still evident in reinvigorated development economics. These insights into economics come from two key debates between economics and geographical political economy over the last few years: the examination of geographical economics and economic geography (Martin & Sunley 1996, 2001; Maki & Marchionni 2011; Sheppard 2011a 2012), and the (somewhat hostile) reception of, and debate about, the 2009 World Bank World Development Report (WDR), which focused on economic geography (Harvey 2009; Scott 2009; Deichmann *et al.* 2010a, 2010b; Peck & Sheppard 2010). I use these two conversations as a heuristic tool to narrow and concentrate a long and at times heated debate, and to clearly exemplify and structure the identified economic characteristics. The WDR is particularly useful in this respect, because there are explicit responses to and assessments of economic geography from geographical economists, unlike many previous conversations which are largely one-sided arguments and to which economists are mostly indifferent.

Amid these arguments critical economic geographers have characterised economics as having (at least) four key attributes, or ontological tendencies, that are distinct from geography: (i) that the economy can be separated from space (and social, political, environmental spheres); (ii) that economies are not related to each other and that uneven development is incidental; (iii) that economic development

follows established trajectories; and (iv) that analytically rigorous, mathematical methodologies are the most appropriate ways of theorising economies. Of course, there are other things geographers know about economics, and the four tendencies are related, overlapping, and could be summarised in different combinations and vernaculars. Nonetheless, each of these four attributes is described below in relation to the two debates between economics and geography (geographical economics versus economic geography, and discussions about the WDR).

The economy can be separated from spaces and places – Consider here an example of the ways in which economists abstract the economy and economic processes from the messy actualities of a more-than-economic and more-than-capitalist world. In the neoclassical free trade doctrine that epitomises mainstream trade theory, several complications are conveniently disconnected and simplified (Sheppard 2012). Different economic activities and commodities are disconnected and individualised into sectors. Other kinds of global flows are eliminated from analyses, including migration, movements of knowledge, financial trades, and foreign direct investments, and non-capitalist logics of production and exchange are ignored. Although empirical examples have shown errors in the assumptions that encourage such disentanglements (see Sheppard 2012 for examples), in trade theory, trade remains separated from the differentiated and uneven spaces in which it is co-produced. That is, the economy is ‘assume[d] to exist on the head of the proverbial pin’ (Martin 1999, p. 67). Mainstream trade theories could benefit from the kind of entangled alternative theories that characterise the sociospatial ontology of geographical political economy.

Geography has recently been added to the mix of relevant factors influencing trades within mainstream trade theory. Geographical economics is concerned with understanding – and modelling – the interactions between space and economies, but these researchers understand such interactions very differently to critical economic geographers. The trade theories of geographical economists like Krugman (1990, 1991) are illustrative examples (on

Krugman and economic geography, see Knox & Agnew 1994; Martin & Sunley 1996; Martin 1999). These trade theories include factors such as transportation costs, the size of the national economy, and distance. Adding geography to trade theories has improved empirical performance and has advanced the two-country, two characteristics, and two-commodity models. Yet, Krugman’s is an impoverished geography, one in which the extra-economic and extra-capitalist forces are sequestered from those processes of market exchange, rather than these being understood as co-produced. Moreover, space exists at only two scales: either as an individual or as a country, where both make autonomous and rational decisions (these are methodological individualism and nationalism respectively; Martin 1999).

These extractive and simplifying tendencies are also evident in the 2009 World Development Report. In her examination of the WDR, Lawson (2010) demonstrates the ways in which the geographical economics of the report separates economy not only from space, but also from its environmental, political, cultural, and gendered contexts. In response to Lawson’s criticism, the WDR team insists that a focus on economy is necessary:

To be tractable it requires parsimony, and a report on economic development should obviously put economics front and center. Economists also tend to be more comfortable with boiling down complex issues to their essence – often using simple models that yield useful insights for policy design. Geographers, in contrast, tend to give more attention to what is unique about a place or situation. (Deichmann *et al.* 2010a, pp. 375–376)

This position echoes the original WDR, which prefaces the report as omitting the ‘the *social and environmental* effects of a changing economic geography’ (World Bank 2009, p. 34, emphasis in original). Although the WDR is clear on the reasons for this focus, and acknowledges that these extra-economic forces are important (they should be considered in place-based policy assessments), this position assumes that such separations are meaningful ones. For critical geographers, as Lawson

(2010) demonstrates, economies are always and 'everywhere embedded' in complex spatial contexts, reproducing these contexts along with environmental, political, social and cultural forces. To sever economy from the spaces in which they are produced is thus an ontological separation, which geographers find fallacious.

Economies are not (productively) related to each other and uneven development is incidental – Economic ontologies separate space from economy, but they also separate societies/economies from each other. Lawson (2010) notes this in her commentary on the WDR. Labour mobility and remittances are expected to bring convergence between cities and regions according to the WDR policy rubric. Yet, Lawson's investigations of contemporary migration patterns of caregivers between richer and poorer countries demonstrate that migration decisions and processes are inherently asymmetrical (on this production of uneven development, see Amin 1974). In short: rich, receiving countries benefit immensely from the caregivers at the cost of the caregivers themselves, their families, and the poorer, sending countries. The processes of migration and new caregiving careers erode 'the skills and earning potential of migrating caregivers, undermining the functioning of care-economies in the third world, and compromising the capacity of remittances to compensate for this loss of vital labor' (Peck & Sheppard 2010, pp. 337–338). But the ways in which these two economies and spaces – sending and receiving – are related, and the ways in which the poverty of one is intrinsically connected to the wealth of another are overlooked in the WDR.

In addition, the WDR policies call for the acceleration of agglomeration economies – increased spatial/social inequity, that is – in order to encourage migration from poorer to wealthier areas (Harvey 2009; Scott 2009). This, according to WDR reasoning, will increase opportunities in the sending areas, as labour markets will be less crowded, as well as increase the wealth of sending areas due to remittances from migrants in the cities. At the same time, earning potentials in cities will decrease as labour markets become more crowded. These processes (with some others) will lead to con-

vergence; that is, less uneven development between core and peripheral regions. What the report is arguing for, then, is policies that allow increasing density in the cores, that encourage the migration of people, economic goods and activities to the core, and that permit the market to take charge; or, that planners need to stop trying to plan (Harvey 2009). But, as Harvey shows in his examination of the report, the increasing social and economic inequality between core and peripheral locations under such neoliberal, market-led policies continues to increase, and does not, in fact converge. Simply the 'rich get richer and the poor get poorer' (Harvey 2009, p. 1276).

Economic development follows established trajectories – Another trend that geographers have identified in economics is the assumption that economic development follows an established trajectory. Sheppard (2011b) demonstrates this in his potted history of development economics and its treatment of geography and nature. Sheppard sees three groups of development economists: those who treat geography and nature deterministically, those who use a 'second nature' account of geography, and those 'new development economists' who, like geographical political economists, are staunchly critical of neoliberal globalisation.

The first group, including development economists such as Sachs, has argued that the environment – climate, and size, location, and orientation of continents, for instance (Diamond 1997) – is a significant and limiting factor in economic and social development. Sachs (2005) argues that geography – an exogenous, yet deterministic 'backcloth' – must be flattened so that competitive capitalism can spread across this newly levelled playing field. Accordingly, geography is a factor that must be overcome in order to reach the development promised land – American-style capitalism. A second group of economists, including Krugman, considers some economic activities to be geographical, yet many other geographical relations remain endogenous. Sheppard's third group of development economists – which includes Rodrik (1997) and Stiglitz (2002, 2006) – make critical intervention into business-as-usual development economics by 'softening' some negative impacts of neoliberal

globalisation, so that its capitalist systems are not completely rejected (Sheppard 2011b; Sheppard & Leitner 2010). For instance Rodrik (2006) argues that institutions are a determinant of economic development, and the development policies should be shaped to local and institutional contexts. Thus, Rodrik calls for – somewhat progressive – reforms to the World Trade Organization that would allow nation-states to have greater control over the movement of economic goods and services into their territories. Within this collective of ‘new development economists’, arguments are about ‘*how* capitalism can bring prosperity, not *whether* it can’ (Sheppard 2011b, p. 58, emphasis in original). Despite differences between economists, the end goal and the general path they advocate is the capitalism of the global North.

This teleological developmentalism is also evident within the WDR. Even when the WDR recognises the unevenness of economic development and spatial differentiation, it maintains a belief in, and the practices of, a ‘unidirectional development trajectory’ (Hart 2010). In its urbanisation policy prescriptions, as mentioned previously, the WDR advocates rural to urban transitions, encouraging poorer rural citizens to move into the city to promote economic convergence. This linear and universal model of urbanisation is promoted, as ‘no country has grown to middle income without industrialising and urbanising’ (World Bank 2009, p. 24). These teleological framings

conjure up a Rostowian conception of the world in which ‘advanced’ national economies have progressed to a higher stage of a unilinear development path along which their ‘less developed’ brethren must learn to travel. To push Rostow’s aeronautical metaphors further, WDR 2009 in effect portrays countries as akin to aircraft lining up on a runway, with the best-behaved most likely to edge to the head of the queue (Hart 2010, p. 343).

Rodriguez-Pose (2010), despite a sympathetic critique of the WDR, challenges the voluntarism of this stageist approach to development. The WDR argues that there is no reason to expect that ‘other parts of the world [i.e. developing countries] will not experience the same patterns’ (World Bank 2009, p. 10) of

development as nineteenth century Europe. But Rodriguez-Pose (2010) counters that this is neither demonstrated, nor is it logically consistent, given that the world of the nineteenth century and that of today differ significantly and that, empirically, development trajectories have differed spatially and across scales.

Analytically rigorous mathematical methodologies – A fourth attribute of development economics concerns its methodological tendency towards quantification and insistence on analytical parsimony. Whereas geographical political economy shifted away from quantification in the 1960s in search of radical economic theorisations (although there is nothing inherently neoclassical about quantification; Barnes 2009; Plummer & Sheppard 2001; Wyly 2009), development economics has adhered to its methodological territorialism and individualism. Compared to the fragmented and pluralist approach of critical economic geography (Peck 2012), mainstream economics is a monist field, ‘defined by the study of rational economic choice and the price-based optimal allocation of resources using a body of analytically rigorous and mathematically recondit theory and techniques’ (Barnes & Sheppard 2010, p. 200; Mirowski 2002). Within this analytically rigorous and mathematically committed economics, the two scales that come to matter are the individual and the nation-state (Martin 1999). The methodological individualism that underlies this tendency dictates that individuals are free to make perfectly informed, self-interested, and rational choices. Moreover, social and macro-scale phenomena are all decomposable to the intentions and actions of individuals. For economics, there is a singular reality, which can be – must be – sequestered through mathematical modelling.

The effect of these four tendencies: (i) that the economy can be separated from space (and social, political environmental spheres); (ii) that economies are not related to each other and that uneven development is incidental; (iii) that economic development follows established trajectories; and (iv) analytically rigorous quantitative epistemologies – is to reinforce ‘market-oriented patterns of development, logics of diagnosis/analysis, and modes of intervention’ (Peck 2012, p. 116). These four econ-

omic inclinations underwrite a voluntarist conception of individual and territorial agency, which promises to bring the economic development of democratic American capitalism (Hart 2010). Failure to achieve this marker of development success becomes the failure of individuals and territories themselves, who have chosen not to be triumphant capitalists. Is this also the case within experimental development economics?

AN EMERGENT ECONOMIC EMPIRICISM

To be sure, all four of these tendencies remain within traditional development economics. However, this essay is concerned with the recent interest in scientifically rigorous, 'objectivity'-oriented statistical experiments in development policies – a shift away from analyses of large territorially scaled observations. This trend in development economics aims to interrogate empirical evidence and theoretical models together, and it is expected to spread throughout economics (Banerjee & Duflo 2010). There is evidence of this empiricism beyond development economics, through for instance, the influential behavioural economics of Nudge (Thaler & Sunstein 2009) and Freakonomics (Levitt & Dubner 2005). But here I focus on the experimental tendency within development economics, using empirical examples to demonstrate four analytical trends, summarising the examples as the authors present them, and analysing the trends in the following subsection.

Randomised control trials – One analytical technique that brings theory and empirics together in an attempt at 'objectivity' is the randomised control trial. By bringing the 'gold standard' of medical trials (Deaton 2009; Barrett & Carter 2010) to development economics to cure the sickness of poverty, randomised control trials claim to reliably identify the effect of development programmes. Randomised control trials (RCTs) randomly assign populations to treatment or control groups, and distribute a development intervention as such, thus identifying causality by eliminating other correlations among variables (Glennester & Kremer 2011). The 'power and promise' of RCTs lies in their ability to resolve

long standing econometric problems with selection effects (associated with participants choosing their involvement or not), and the endogeneity of policies and observable variables like household or community characteristics to development outcomes (Barrett & Carter 2010). Causality can be identified and these econometric problems resolved as the control and treatment populations are identical and perfectly comparable due to randomisation – the only difference between the two groups is the development intervention of interest. RCTs also allow for multiple treatment experiments, so as to identify the key components of a broader development program that are working rather than assessing entirely the broader policy intervention (Banerjee & Duflo 2008). The RCT movement aims to be open 'to let the data lead the researcher, rather than vice versa' and as a consequence this has 'opened up important new areas of inquiry in development economics' (Barrett & Carter 2010, pp. 518–519) by forcing the results, rather than the theory, to speak for themselves (Banerjee & Duflo 2008).

In investigating credit markets in the global South, where the poor do not typically have access to formal lending and borrowing and rely instead on informal credit, a randomised control trial aims to help explain some observations about credit markets. In an attempt to examine the effects of introducing microcredit institutions, Banerjee *et al.* (2010) conducted a randomised control trial in 104 slums in Hyderabad in order to test claims, by development players like the World Bank, that microcredit can cure poverty. In this experiment, microfinance institutions (MFIs) were opened in some (randomly selected) slums, but not in others. Fifteen to 18 months after the opening of the MFIs, about 65 households were surveyed in each slum (Banerjee *et al.* 2010). The results indicated that those who had loans were more likely than other households to have new businesses or more profitable businesses, but there was no effect on measures of poverty or human development. Therefore, while claims that microfinance changes lives receive some support from the results, there appears to be little impact on empowering women and children. In the long run, Banerjee and colleagues find that the gap between those who

have businesses and those who do not increases with the provision of microcredit, and there is no effect on human development indicators.

Evaluating microcredit as a poverty reduction tool is complicated, and randomised control trials, Banerjee *et al.* (2010) and Banerjee and Duflo (2010) claim, are effective in allowing for comparing and estimating the effects of MFIs. Accordingly, the benefit of using experiments is that they can absolve the statistical analysis of observational problems inherent in comparing clients and non-clients (who self-select) and villages in which MFIs operate and those in which they do not (as MFIs choose which villages are suitable). Variables that are otherwise correlated in the real world can be accounted for in experimental design. In the experiment in Hyderabad, the only difference between those who took up microcredit and those who did not was the access to the microcredit. Moreover, conducting an observational bias-free experiment can parse the 'facts' between proponents and critics of microcredits abilities: a randomised control trial settles the score, 'bringing more scientific rigour to development work' (Buchanan 2010, p. 2).

Natural experiments – In the case where experiments are not feasible, it may be possible to investigate a natural experiment – a case in which places were (accidentally, or serendipitously) pseudo-randomly assigned to treatment and control groups. Beginning in the 1980s, natural experiments – like RCTs, in fact the precursor to them – emphasise study design and internal validity to identify clear and causal sources of variation (Imbens 2010). However, unlike RCTs, the natural randomisation of places or peoples may be correlated with the outcomes of interest, so natural experiments rely on instrumental variables to identify causality (Deaton 2009). One frequently utilised natural experiment is the history of colonialism, and the varied experiences and processes of colonialism across different countries (Acemoglu *et al.* 2001, 2011; Nunn 2008). For instance, in trying to decide whether geography or institutions are the determinant of economic development, Acemoglu *et al.* (2001) examined the wealth of countries that were settler or extractive colonies. In particular,

Acemoglu *et al.* (2001) studied the different institutions that European colonisers implemented, expecting that the places with 'good' institutions (that uphold private property rights, restrict the power of elites, and include some sense of equal opportunities for all) would fare better economically. Apparently, the colonisation of the Caribbean and Latin America was extractive, and so poor institutions either remained or were introduced. This compares with settler colonies such as North America, New Zealand, and Australia, where Europeans transplanted their 'good' property-rights regimes. Acemoglu *et al.* (2001) argue that if geography were the determinant of prosperity, this would not explain the relative decline of wealthy pre-Colombian Latin American empires after colonisation.

Acemoglu *et al.* (2001, 2006) used a natural experiment approach to test their hypothesis of the effect of institutions on development. In particular, using statistical tools, they claim to establish causality.² The authors argue that it was not colonisation per se that caused underdevelopment; rather, the institutions of colonialism caused relative wealth and poverty. To formally test this hypothesis and establish 'causality', the authors must account for the correlation between those countries that were settler colonies and that then were wealthy; perhaps Europeans chose to settle in colonies that had greater natural endowments. To do this, Acemoglu *et al.* (2001, 2011) introduce a statistical instrument – a variable independent of the dependent variable (economic development), but highly correlated with the causal factor (institutions); in this case the instrument is European mortality rates in the colonies. A statistical instrument is introduced in order to control for confounding variables, giving natural experiments power to identify causality. Using these statistically rigorous methodologies – natural experiments and instruments – the authors claim to prove that institutions are a greater determinant of economic development than physical endowments.

Pre-analysis plans – In its push towards even greater observational 'objectivity', development economics is beginning to utilise the pre-analysis plan, a tool common in medical experiments. A pre-analysis plan details how

researchers will analyse their data, written before they begin to collect it in order to avoid data mining and specification, and associated problems (McKenzie 2012). Such a plan might include: a description of the sample to be used, key data sources, hypotheses to be tested and how these will be measured, how variables will be constructed, the treatment effect equations, and a plan for dealing with multiple hypotheses, survey attrition, and outcomes with limited variation (McKenzie 2012). While there are only a few published papers exploring the use of this analytical technique (Casey *et al.* 2012; Finkelstein *et al.* 2012), there is rapidly building interest in it, both within and beyond economics (McKenzie 2012).³

Building on the work of Acemoglu *et al.* (2001, 2011), which claims to establish that institutions are key in economic performance, Casey *et al.* (2012) seek to measure how institutions can be successfully reformed to raise economic prosperity. To do this, Casey and colleagues conducted an experiment in 236 villages in Sierra Leone on the effects of including minority participation requirements in decision-making institutions. The randomised control trial tested whether institutions improved when minority participation was required, using surveys to measure decision-making during and after the project. The randomised control trial shows that the development intervention did little to substantively and permanently change institutional structures.

Of relevance here, however, are Casey *et al.*'s (2011) methodological conclusions related to the use of pre-analysis plans. Institutions are multifaceted and can therefore be measured in a number of different ways, leading to numerous indicators of institutional improvement. Measuring changes in institutions might be biased, Casey *et al.* (2011, p. 1) argue, as they could 'cherry-pick' indicators which suited their analyses. 'To address these measurement challenges, [the authors] exploit a randomly assigned governance intervention, develop objective real-world measures of institutions, and use a pre-analysis plan to bind our hands against data mining'. The pre-analysis plan involved generating a set of hypotheses prior to the project to which were then added indicators and econometric specifications as the

project continued. Importantly, the methods for measuring change and analysing the data were outlined prior to investigating the follow-up data. Sticking to this plan meant eliminating some of the methodological risks mentioned, and 'generated correctly sized statistical tests, [and bolstered] the scientific credibility of the findings' (Casey *et al.* 2011, p. 3). 'Within the discipline of the pre-analysis plan and mean effects approach, [the authors] could have instead selected an assortment of individual treatment effects . . . to tell basically any story' (Casey *et al.* 2011, p. 31). In short, pre-analysis plans ensure 'objectivity' by requiring researchers to outline hypotheses and indicators to measure these hypotheses prior to undertaking the research and analysis, and thus preventing data mining for desired results. Such methods are particularly important when attempting to measure the impact of interventions with a wide variety of potential different outcomes.

Evidence driven policy evaluation – Perhaps the most obvious indication of the trend towards statistical and observational 'purity' is the arrival and success of Banerjee and Duflo's (2011) *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty* – the 'latest Big Book on development' (Green 2012). The book came out of the wildly lauded Poverty Action Lab, which, by 2010, had completed over 240 development experiments in 40 countries. Complete with descriptive website, accompanying data, coursework guidelines, and potential syllabi, Banerjee and Duflo's book – geared to a popular audience – urges us to re-think poverty and development. In particular, the authors argue that examining the causes and remedies for underdevelopment in grand theories (like the economists examined above) is misguided, and instead development economics and policies should be focused on specific 'sticking points' for poverty (Banerjee & Duflo 2011, p. x). This book focuses not on 'big picture' development problems, such as whether foreign aid is important – that Sachs (2005) and Easterly (2006) argue about – and instead focuses on small questions, such as how to reduce the prevalence of dengue fever and diarrhoea. And these small questions should be interrogated with a mind for developing policies, because

‘talking about the problems of the world without talking about some accessible solutions is the way to paralysis rather than progress’ (Banerjee & Duflo 2011, p. 6).

These narrower, policy-focused questions should be answered with evidence that can be more easily interpreted than multi-country comparisons, Banerjee and Duflo contend. The major assertions of the book are: forget grand theories of economic development and conduct experiments that can rigorously test policies. Questions – such as, what is the best way to ensure children sleep under a mosquito net? – should be answered by observing the ‘behavior of comparable groups of people facing different levels’ of control and treatment (Banerjee & Duflo 2011, p. 7). It does not make analytical sense to compare groups of people who have chosen to buy or not buy mosquito nets, as these groups are socio-economically different, which may account for or confound their choices. Instead, an experiment that assigns similar people to different groups can ‘objectively’ decipher whether mosquito nets should be subsidised, free, or sold at market rates in order to ensure their most effective uptake. With these experimental results, the best policies can be implemented. In development policy, the authors argue,

the best anyone can do is to understand deeply the specific problems that afflict the poor and to try to identify the most effective ways to intervene . . . there is no general rule here . . . It is the body of knowledge that grows out of each specific answer and the understanding that goes into those answers that give us the best shot at, one day, ending poverty (Banerjee & Duflo 2011, p. 15).

Thus while calling for an end to grand theories, the authors instead argue for scientific rigor that can objectively identify the best development policies.

ANALYSING EXPERIMENTAL DEVELOPMENT ECONOMICS

Before assessing these novel inclinations, it should first be noted that the experimental emphasis is not without criticism from within development economics itself. Precautionary (economist) critics warn that experimental

techniques should not be the only source of knowledge on development (Deaton 2009; Ravallion 2009, 2012). Ravallion (2009) summarises his concerns thus: some development decisions cannot be randomised, for example, locating key infrastructure or designing macro-economic policy; RCTs can only ask small questions and one at a time, with the risk that we end up with many social experiments with evidence on a single parameter; spill-over effects are under-studied; and there are profound ‘external validity’ problems such that experiments lose the ability to generalise to other settings and scales. Sharing many of Ravallion’s reservations, Copestake (in Green 2011) is also concerned that researchers are choosing projects based on their desire to use experimental tools, and not instead focusing on the most important issues and then choosing the best tool for analysing the problems. Also, experiments are very expensive ways to evaluate policies. Another well-known pessimist, Deaton (2009) cautions that experimental methods are more complex than they are made out to be, and they are riddled with technical and practical problems that undermine their statistical superiority. Harvard development economist Pritchett declares that randomised experiments have played no role in the major social transformations of his lifetime (in Parker 2010) and that they are a tool for cutting development funding (in Green 2013). Analytically, Pritchett objects to RCTs as they test projects against non-projects where in reality, development interventions are overlapping, multiple and more than the sum of their parts. Echoing others concerns, Pritchett notes there is no evidence that RCTs are replicable, which is a key condition of causality and validity in the sciences.

Perhaps these criticisms herald the intellectual arrival of this new brand of development economics; as Duflo (in Parker 2010, p. 84) herself concludes: ‘then it became controversial, which, in a sense, is even better.’ Notwithstanding the misgivings of senior contributors to development economics, does this new empirical emphasis really represent a significant shift in the sociospatial ontology of development economics? Consider again Banerjee and Duflo’s (2011) book on *Poor Economics*. The book wants to understand the economic lives of

the poor, in all their complexity. As such, each case described in the book illuminates the intricacies of these lives; in some cases we meet individuals in Indonesian slums, or Indian markets. This messiness and place-based complexity is unlike economics more generally, which frequently aims for simplified models in the name of parsimony.

However, each of these detailed instances exists to provide a literary entrance into broader development concerns; enter the Indonesian slum to learn how the poor make decisions about family planning; enter the Indian market to learn about how informal money-lending operates. Each of the complicated experiments detailed in Banerjee and Duflo's book builds on others, adding up to a complicated, yet generalisable, theory of the way the lives of the poor operate. Thus, studies in the United States about class sizes and learning outcomes are augmented with those from Kenya to establish why schooling is delivering little to the poor (Banerjee & Duflo 2011). While complexity is recognised in these policy experiments, such messiness is generalisable; a universal state of being independent from the spaces in which the experiments operate. By definition, and if we recall the mosquito-nets example, experiments aim to account for and then eliminate sociospatial differentiations to provide 'objective' answers to policy and theoretical questions. Micro-economies (because, as critics of randomised evaluation note randomised evaluations cannot yet answer macro-economic questions) – of markets for mosquito nets, of familial decision-making around education – are therefore independent of the societies in which they are produced.

Natural experiments also rest on the assumption that societies are independent of each other. There are several specious assumptions underlying Acemoglu *et al.*'s (2001) analyses that even economists observe (Glaeser *et al.* 2004; Olsson 2004), which there is not sufficient space to detail here. Note, however, that the connections between societies – settler, extractive, and colonising – are glaringly absent. Instead, economic performance is found to be a consequence of endogenous factors – the presence of certain types of good institutions within the countries of each category. Underdevelopment, according to

Acemoglu and colleagues seems to be the result of serendipitous historical circumstances that only placed some institutions in some places (see McAnany 2011). An assumption of independence is also fundamental to randomised control trials; independence within the population of interest (people, communities, regions, nations, etc.) is necessary for randomisation to work – to be random. Thus, it is presumed that members of a population do not influence each other either before or after being assigned to control or treatment groups, contravening the very foundations of economic geography; that geography matters.⁴

Within the emergent empiricism of development economics, economies are separate from each other and from space, and so economies are expected, as in mainstream economics, to follow pre-established development trajectories.⁵ For instance, Banerjee and Duflo's (2010, 2011) microfinance studies suggest subsidising the poor's banking services, and providing government sponsored insurance schemes for the poor. But the ultimate aim of these government interventions is to create efficiently and effectively functioning markets to provide financial services for the poor. Conducting scientific experiments simply suggests the 'best' ways to provide credit to the poor (Banerjee *et al.* 2010), in pursuit of fully marketised societies. This is also evident in *Poor Economics*, where, although the authors write in demand of a new poor-centred development economics, their focus remains on problems and policies for the global South without reflecting on policies in (or relations with) the global North. This includes how to best provide credit, health care, education, employment, and food through properly functioning – perhaps government supported – markets and institutions (Banerjee & Duflo 2011). Thus, the poorly performing economies of the global South are independent of the economies in the global North; inequality is absent.

Perhaps the most evident change in development economics is its methodological shift away from abstracted mathematical models. The stories and the data from Banerjee and Duflo's (2011, p. viii) wide travels left the authors 'both fascinated and confused, struggling to fit what [they] were hearing and seeing into the simple models that (often Western or

Western-trained) professional development economists and policy makers have traditionally used to think about the lives of the poor'. Primarily, experimental approaches discard abstract theories and models, or at least urge significant modifications. But, the authors admit, they 'are no exception' (Banerjee & Duflo 2011, p. 10) when it comes to economists' penchant for simple, or simplistic theories and diagrams. While they are no longer mathematically sophisticated, experiments do not 'call [into question] the entire set of hard-core propositions' of economics (Sheppard 2011b, p. 60), such as rationality and the ability to extract truth from our observations of the world.

The abstracted mathematical models of the world, that economics is known for, are founded on the actions of hypothetical, rational, equally empowered and knowledgeable individuals (or their simple sum which is a nation-state). However, statistical analyses are not. Rather than based on the actions of the poor that would result from rational choice models, policies derived from experimental studies are founded on the assumption that the unobserved poor in diverse places act in the same way that the average, observed poor do in an experimental circumstance. And such evidence – cries this experimental collective – can be collected, analysed, and used to identify the best policies for poverty reduction. This collection of assumptions – that experiments can extract data to describe how the poor act independent of any biases (through pre-analysis plans for instance), that this data and subsequent analysis will lead to better policies and to more coherent, truthful theories, and that these policies and theories will help reduce poverty for the future poor – is also herculean.

CONCLUSIONS

This paper has demonstrated the emergence of a new experimental trend in development economics. This trend is put into conversation with observations of the sociospatial ontology of economics, according to geographical political economics. I demonstrate four tendencies of economics that geographers have critiqued, drawing from two instances of exchange between economists and geographers – the

2009 World Development Report and the arguments about geographical economics and economic geography. I show that the assumptions of development economics more generally are reproduced within this new policy-oriented version of the sub-discipline. In particular, I demonstrate that three of the four tendencies that geographers have identified in economics – that economy is separate from space, that economies are independent, and that economic development follows established trajectories – are also tendencies of experimental development economics. However, and importantly, this experimental trend marks a shift away from the abstracted and mathematical models that have characterised development economics. Although the objectivity-oriented studies within experimental development economics are founded on the same view that truth can be extracted unproblematically, the experimental vein assumes this truth is to be extracted from observations that are then averaged, rather than from assumptions about the average person. This is no small difference in approach.

But, although the assumptions of this new policy-oriented and experimental approach require large leaps of faith, should geographers applaud this change? On the one hand, books such as Banerjee and Duflo (2011) make a case against neoliberal development policies, and they argue for a rethinking of development economics that plays out within New York City and Boston, rather than solely within the global South (as evidenced by the recent opening of the Poverty Action Lab North America offices). On the other hand, this approach is vigorously un-relational, founded on the conviction that poverty is to be ameliorated by creating better policies for the global South, or poor enclaves of New York City. Accordingly poverty is found, caused, and fixed within bordered and contained poor communities, which require 'technical' and 'best practice' interventions (Eyben & Roche 2013). This approach ignores at least two vital relationalities. First, it ignores the role that previous interventions have played in creating 'problems'; just as it overlooks the linked processes of conceptualising and problematising the poor in need of poverty-fixes, and suggesting solutions in policy and project form (Li 2007). More importantly, the experimental

development economics position denies the production and relationality of poverty from the global North and among the rich. The 'exploitative effects of capital accumulation, processes of unequal sociospatial categorisation, and political and discursive systems that limit or exclude the poor' are ignored (Lawson 2012, p. 1). Instead, experimental development economics reproduce 'authoritative (dominant) accounts of poverty ... as a self-contained problem in need of management and intervention' (Lawson 2012, p. 2). Moreover, this account depoliticises poverty, abstracting it from power relationships, and rendering intervention as a 'technical' problem (Harriss 2009; Lawson 2012; Eyben & Roche 2013).

In addition, although some of the conclusions of the empirical studies and policies may challenge economic foundations, in some respects this new development economics signals an aggressive empiricism, veiled as progressive development policies. This is one of the major criticisms of the new development economics from orthodox economics. Duflo (in Parker 2010, p. 86) declares 'I'm never unhappy with the results. I haven't yet seen a [statistical] result I didn't like'. But some of her fellow development economists are wary, liking this atheoretical experimental approach to alchemy (Green 2013). Whereas Duflo's quote indicates that an explanation and analysis simply jump from the page when an experiment presents numbers, Deaton (2009) is not so sure. Generalising and analysing statistical results, Deaton (2009) counters, requires inference, which in turn is dependent on thinking and theory. And if theory and inference is the backbone of experiments and informed decision-making, then 'do we need to test the idea that parachutes are useful to people who jump out of planes' (Parker 2010, p. 88)?

In the spirit of trading zones, we might also reflect on whether and how these changes in development economics might ripple into geography and geographical political economy. There are at least three things of particular note to geographers and future geographical research. The first, and one I shall not delve into here in detail but that geographers are primed to evaluate in the future, is related to the ethical and political implications

and commitments of such research. What might it mean for developmental subjects to become 'laboratory' subjects, as development economists take medical trial-inspired techniques to communities? What is at stake – ethically and politically – when these subjects are divided into control and treatment, or their reactions and decisions observed by researchers who present them with gifts? Such experiments on the poor have also been subject to criticisms, with sometimes-flippant responses from randomistas. For instance, when discussing a project in which teachers' pay was adjusted according to their attendance as measured by time-sensitive cameras that document teachers' arrival and departure from the classroom: Duflo (in Parker 2010, p. 80) responds 'Who do you care about? Lazy teachers who show up sixty per cent of the time, or the kids? O.K., I care about the kids.' But the history of the poor becoming research subjects rather than politically empowered citizens (Eyben & Roche 2013) ought to at least give us pause.

Geographers should investigate the discursive and material processes through which experimental subjects are produced, and the impacts of these processes. Geographers might also historicise the production of 'laboratory' subjects alongside the 'complex articulation of backwardness' and 'underdevelopment' that Gupta (1998, p. ix) terms the postcolonial condition (see also Escobar 1995). Finally, geographers ought to interrogate how randomised development evaluations might reproduce or depart from contentious medical experiments on unconsenting and often marginalised 'easily accessible' populations, including Holocaust victims and dispossessed Indigenous peoples. Such investigations may insightfully demonstrate the inequalities and disciplinary apparatuses underlying such experimentation. They will also reveal the innocence (or not) of these randomisation techniques, and whether they might indeed be used in productive, spatially explicit, and theoretically informed ways.

Second, failing to consider what such changing commitments in development economics might mean for geographical research is perilous. Development economics has begun to get its 'hands dirty', spending time investigating poverty among the poor. This is typically the terrain of development geography, but

development economists have approached questions of poverty and development with research budgets, collaborations and thus capabilities far beyond those that geographers could muster. For instance, Casey *et al.*'s (2012) study using a pre-analysis plan was conducted with the (financial) collaboration of seven other institutions including the Gates Foundation and the World Bank and (from my count) employed more than a dozen research assistants.

This is suggestive of the imperialism that many economic geographers so fear, as economics intrudes into 'our' disciplinary boundaries (Maki & Marchionni 2011). But what might we make of this 'imperialism' if it indeed exists? As Maki and Marchionni question: should it be resisted, or celebrated, and how? This paper does not have the complete answers to these questions, in part because they depend on the ethical and political queries outlined above. Nonetheless, it is futile to remain possessive about geographical concepts or methodologies, for instance spatial phenomena or grounded investigations of poverty (Scott 2004). But, given that experimental development economics contravenes key geographical precepts, we might begin by defending these principles, and critiquing their misuse or disregard, as I have begun to do here. Maki and Marchionni (2011, p. 656) also suggest that one route from this disciplinary imperialism is the 'improved articulation scenario' where economic geographers reflect on and articulate 'the foundations of theories and standards ... thereby making [geographical] claims better justified'. Returning to the idea of a trading zone: conversations, debates, and critical exchanges with economics might improve geographical research and theorising, although as this paper has demonstrated such trading zones are necessarily asymmetrical.

Consequently, therefore, not only should experimental economics be recognised, but also, perhaps geographers stand to gain from thinking about methodologies in this development economics way. To be clear, this does not automatically imply that geographers begin randomising, but that we can strengthen our theories and methodologies from such reflections. To reiterate, if development economics is beginning to ask 'our' geographical questions,

we should have good answers. This might mean: focusing our perilously pluralist and 'magpie-like' approach to studying the world (Tickell *et al.* 2007); using powerful statistical techniques accompanied with rigorous theorising about causality and associated with macro-structural and relational questions of poverty; and turning more attention to questions of development and poverty. The experimental methodology emphasises collaboration and comparison in investigations, perhaps encouraging geographers to extend from the case-study approach to refine foundational theories (Tickell *et al.* 2007). Experiments implement small steps, creating hypotheses that can be empirically tested (perhaps overly so) and thus proven to the world beyond the novel sub-field. And most importantly, especially compared to the methodological approaches in economic geography, such hypotheses, collaborations and comparisons are cumulative, building on one another in order to make and sustain policies and theories.

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Notes

1. Some disclaimers: this is a conversation about mainstream development economics and about Anglophone economic geography. There are other lessons from economics that geographical political economy has taken to its core – notably Marxian economics (for instance the work of Resnick & Wolff (1989) who in fact take many of geography's concerns about economics – for example, embeddedness – very seriously). However, for this paper, I focus on mainstream, generally quite neoclassical, economics. In some

senses, therefore, this trading zone is plagued by what may seem like ideological differences, with economics broadly rightist and geographical political economy generally leftist (though as I discuss in the paper, development economics has been shifting towards the centre).

2. This causality does rely on finding a plausible story or theory for explaining why institutions matter, but the pudding – in which the proof lays – is the statistical analysis. This is the inverse to the critical realism of many economic geographers, where causality is found in robust theorizing that explores how questions; for Sayer (2001, p. 2981), ‘explanation does not require repeated observations: what makes something happen has nothing to do with the number of times it has or has not been observed to happen’ – causality should not be presumed from regularity.
3. Thanks to an informed reviewer for pointing this out and clarifying it to me. See Center for Effective Global Action (2012) for some interesting examples of pre-analysis plans.
4. Again, I must thank a generous reviewer for this forceful phrasing.
5. This is perhaps most evidently the case – as one of the insightful reviewers of this paper noted – in the behavioural economics that is a close relative to the experimental approaches considered here. In that family, economists try to get the incentives right such that an individual’s behaviour will mimic the *homo economicus* and methodological individualism of mainstream economic theory (see Thaler & Sunstein, 2009). That is, then, an individual’s personal economic growth is to follow expected rational trajectories.

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