

The Future of Global Economic History *Regional Comparisons to Address Global Questions**

Pim de Zwart

TSEG 15 (2-3): 129-142

DOI: 10.18352/TSEG.1025

Abstract

Economic history deals with the process of economic development across the globe in the long-run. In this essay, I put forth ideas about what economic historians should be doing in the next ten years in terms of content, methodology and the scale of analysis. In terms of content, I suggest that prospective research questions should generate what I consider 'useful knowledge' and provide two example topics: inequality and globalization. Regarding methodology I argue in favour of the comparative (quantitative) method in order to analyse processes of change in a variety of contexts. The scale of analysis should shift from the country to the regional level in order to be better able to tease out relevant relationships in a comparative analysis.

Introduction

The key issue of economic history is to understand what drove economic development across the globe in the long-run.¹ This includes questions of what has caused some economies to grow, but also what deter-

* I thank Bram van Besouw (Utrecht) and Dan Curtis (Leiden) for comments on an earlier version of this paper.

¹ I am aware of the fact that *TSEG*, as is obvious from its name, is a journal of both social and economic history. In this essay, however, I will focus on the field of economic history. This is not because I think that social history is not important, or totally unrelated to economic history; in fact, many subjects, like that of labour relations and the long-run development of wages, on which I have worked in the past, are shared between the two fields. It is rather because I do not think that my knowledge of what goes on in the field of social history is sufficient to sketch its future.

mined the stagnation or decline of other economies, and to what extent economic growth has led to greater well-being for the majority of the population. In this essay, I will address how I think how economic history (in the Low Countries) can best move forward in its attempts to come up with answers to those questions. Admittedly, the choices that I have made in writing this essay are rather personal as they derive mostly from my own historical interests as well as my current expertise. Nonetheless, I will put forward two themes – globalization and inequality – that I think will be important to address in the coming decade and I will put forward the arguments why. Furthermore, I will argue in favour of the analysis of *change* in various contexts (rather than attempts to discover *causality* under severe restrictions, or an examination of the *persistence* of specific patterns) and I consider the (quantitative) comparative approach – the old workhorse of economic history – well-suited for the task. In terms of the scale of analysis, I put forward arguments in support of comparing regional and local case-studies for methodological reasons.

Themes

It is clear that at the core of our field are subjects like welfare and its distribution, but also politics and conflicts, and that we should address these issues in terms of their *changes over long periods of time*. Long term analysis of change is crucial, because (1) there are many developments that have different short-run and long-run effects, and (2) economic and social history are the only disciplines that study long-run developments (while the short-term is being studied by all other social scientists). I think that the themes that economic historians may address in the coming decade (1) should have relevance for wider academic and societal debates, (2) should have potential for producing what one could consider ‘useful knowledge’ and (3) should allow economic historians to make a specific contribution to provide a deeper understanding of the topic. To briefly explain what I mean by ‘useful knowledge’; this is knowledge that, even if only indirectly, may have policy implications. Thus, I consider research that finds out how bad ecological disasters (just to name one currently fashionable topic of economic and social history research) have been in the past (e.g. how many deaths) a less ‘useful’ study, than research that investigates the mitigating impact of, for example, specific institutions, or the degree of inequality, on the effects of ecological disasters. While the first research may be interesting in itself to get a clearer picture of what

has happened in the past, the second has more important ramifications for how we think we can best organize society (also today). Now, while there are many themes one can think of, let me emphasize two topics that fulfill these three conditions, and which, I think, will (continue to) be important in the coming decade: within-country inequality and globalization.

Inequality

Since the beginning of the financial crisis in 2008, popular dissatisfaction with growing wealth and income disparities has been on the rise. In 2011, people across the world took part in the ‘Occupy’ movement to protest against the ‘1 per cent’ and the power and misconduct of those active on the financial markets. While the protestors were being put under surveillance by the US government, Barack Obama himself proclaimed the growth of within-country economic inequality as ‘the defining challenge of our time’ in 2013.² More recently, the election of Donald Trump and the vote for Brexit, have shown that inequality and the popular discontent related to this inequality are also causing dramatic political shifts. Better understanding the causes and consequences of economic inequality seems more urgent than ever. Also within economic history the importance of the topic is on the rise. Within-country inequality shares with the debate on the rising global economic inequality between Western Europe and Asia (the ‘Great Divergence’), which has stimulated so much of global economic history research over the past two decades, the publication of a book that has a big and provocative overall thesis; Thomas Piketty’s *Capital in the twenty-first century*.³ The book has received widespread attention in both the academic and the public domains,⁴ received rave reviews from Nobel laureates Robert Solow and Paul Krugman,⁵ but also elicited critical responses from prominent economists/economic historians, like Daron Acemoglu and James Robinson,⁶ and Deirdre McCloskey.⁷

2 B. Obama, 4 December 2013, on income inequality in the US: <https://obamawhitehouse.archives.gov/the-press-office/2013/12/04/remarks-president-economic-mobility>.

3 T. Piketty, *Capital in the twenty-first century* (Cambridge 2014).

4 With over 2.2 million copies sold by 2017: H. Boushey, J. Bradford DeLong and M. Steinbaum, ‘Introduction’, in: Idem (eds.), *After Piketty. The agenda for economics and inequality* (Cambridge 2017) 1.

5 R.M. Solow, ‘Thomas Piketty is right. Everything you need to know about “Capital in the twenty-first century”’, *The New Republic* 23 April 2014; P. Krugman, ‘Why we’re in a new gilded age’, *The New York Review of Books* 8 May 2014.

6 D. Acemoglu and J.A. Robinson, ‘The rise and decline of general laws of capitalism’, *Journal of Economic Perspectives* 29 (2015) 3–28.

7 D. McCloskey, ‘Measured, unmeasured, mismeasured, and unjustified pessimism. A review essay

Recent years have seen many new investigations into the long-term trends in inequality. Piketty and his colleagues charted wealth inequality across some Western (and a few non-Western) societies for the twentieth century using tax returns. Others have attempted to build social tables in order to sketch income inequality for the more distant past, like Peter Lindert and Jeffrey Williamson regarding inequality in the United States and Guido Alfani and Wouter Ryckbosch for Europe from the middle ages to the nineteenth century.⁸ In recently presented working papers, Alfani, Piketty and others have started to investigate inequality in colonial Africa and Asia.⁹ Branko Milanovic and his colleagues charted pre-industrial inequality across the globe and introduced the concept of the 'inequality possibility frontier', which dealt with the problem that in poorer societies inequality is necessarily limited as a result of the lesser absolute amount of surplus that could be extracted.¹⁰ Such societies could, however, still be highly extractive in the sense that the *percentage* of surplus that was extracted could be very high. Recent special issues of *Cliometrica* and *TSEG* from 2017 contain contributions that push the analysis of inequality in Europe and Latin America into the early modern era.¹¹

While research on the topic is clearly booming, perhaps relatively few detailed explanations of the rises and declines in inequality in the long-run have been given, besides such grand claims that suggest that capitalism, or the market, automatically leads to more inequality in the long-run.¹² Such

of Thomas Piketty's *Capital in the twenty-first century*, *Erasmus Journal of Philosophy and Economics* 7 (2014) 73-115.

8 P.H. Lindert and J.G. Williamson, *Unequal gains. American growth and inequality since 1700* (Princeton 2016); G. Alfani 'Economic inequality in northwestern Italy. A long-term view (fourteenth to eighteenth centuries)', *Journal of Economic History* 75 (2016) 1058-1096; W. Ryckbosch, 'Economic inequality and growth before the industrial revolution. The case of the Low Countries (fourteenth to nineteenth centuries)', *European Review of Economic History* 20 (2016) 1-22.

9 G. Alfani and F. Tadei, 'Income inequality in colonial Africa. Building social tables for pre-independence Central African Republic, Ivory Coast and Senegal', Paper presented at *AEHN Workshop*, 21-22 October 2016, Sussex University; F. Alvaredo, D. Cogneau, T. Piketty, 'Income inequality under colonial rule. Evidence from French Algeria, Cameroon, Indochine and Tunisia, 1920-1960', Paper presented at *AEHN Workshop*, 21-22 October 2016, Sussex University.

10 B. Milanovic, P.H. Lindert and J.G. Williamson, 'Pre-industrial inequality', *Economic Journal* 121 (2011) 255-272.

11 P.H. Lindert and J.G. Williamson, 'Inequality in the very long run. Malthus, Kuznets and Ohlin', *Cliometrica* 11 (2017) 289-295; *TSEG* Special issue on inequality 14:2 (2017).

12 Piketty, *Capital*; B. van Bavel, *The invisible hand? How market economies have emerged and declined since AD 500* (Oxford 2016). Dombrecht and Ryckbosch also note that trends in pre-industrial are often explained by focussing on deterministic forces and lack attention to political economy factors: K. Dombrecht and W. Ryckbosch, 'Wealth inequality in a time of transition. Coastal Flanders in the sixteenth century', *TSEG* 14:2 (2017) 63-84.

big theories are not very useful in explaining the large differences in inequality levels between different capitalist countries (that is: all countries with the possible exceptions of North Korea and Cuba). Another downside of these studies is that they do not consider their own policy advice likely to be implemented. Piketty suggests that the way to remedy capitalism's movement towards ever greater inequality is by implementing a global capital tax, but even he himself considers this a utopian idea. Van Bavel suggests that redistribution via the state, or family and kinship bonds, guilds and commons, may counteract the forces of the market, but he seems to be very pessimistic about the feasibility of remedying inequality as economic inequality also leads to political inequality and the beneficiaries from the system are unlikely to implement correction mechanisms. After a point of no return has been reached (as it has in advanced capitalist societies), ever growing political and economic inequality becomes inevitable. Also rather unhelpful is the suggestion that wars and disasters are the main drivers leading to a decline in inequality (as discussed in Walter Scheidel's recent book *The great leveler*, but it is also an element of Piketty's argument). It is hard to imagine that such results would lead to any likely policy implications. For Scheidel, all we can do is wait for what he calls the 'Four Horsemen' of levelling – mass mobilization warfare, transformative revolutions, state collapse, catastrophic plague – to relieve us from this terrible and ever-increasing inequality. Such arguments perhaps even stand in the way of thinking about more feasible, and less violent and destructive, ways of reducing inequality. More research in the coming years should be devoted to providing more detailed explanations for the newly unearthed trends in inequality: under what conditions did a country (or a region) become less egalitarian and when did it become more egalitarian in the past? How are these trends influenced by institutions and policies as they change over time?¹³

Further research should also be devoted to the consequences of inequality. Because of ethical or moral reasons many of us (especially in Europe) think that a high degree of inequality is a bad thing; yet whether it is actually bad for economic development remains a contentious issue.¹⁴ As is well-known, influential modernization literature from the 1950s and 1960s actually suggested that growing inequality was necessary to create eco-

13 See for good recent examples: Dombrecht and Ryckbosch, 'Wealth inequality'; B. van Bavel and E. Frankema, 'Wealth inequality in the Netherlands, c. 1950-2015. The paradox of a Northern European welfare state', *TSEG* 14:2 (2017) 29-62.

14 W. Easterly, 'Inequality does cause underdevelopment. Insights from a new instrument', *Journal of Development Economics* 84 (2007) 755-776.

conomic growth. More recently, seminal papers by Acemoglu et al. and Stanley Engerman and Kenneth Sokoloff have suggested a connection between inequality and long-term economic stagnation through its effect on institutions, but the evidence thus far presented has remained largely inconclusive.¹⁵ As so much is being written about inequality nowadays, both in academia as well as in the public domain, economic historians have an important task to investigate the long-run impact of inequality on economic development. Richard Wilkinson and Kate Pickett have suggested that high levels of inequality are correlated with a host of health and social problems, like life expectancy and infant mortality, obesity, mental illness, crime rates, and decreased levels of trust.¹⁶ But their results have also been challenged. A meta-study by Andrew Leigh et al. found that the evidence on the connection between inequality and health is currently too thin and shows no significant negative relationship.¹⁷

Because economic historians have been building databases on different indicators of well-being on a global scale,¹⁸ with relatively good evidence from the nineteenth century onwards, combined with long-term evidence on inequality, allows us to better assess the relationship between inequality and other indicators of well-being in a long-term perspective and on a global scale. Such long-run cross-country studies will be useful in pointing research in further directions, but the definitive tests will have to be based on regional studies that allow for a more detailed analysis of the different factors involved. The groundwork has been firmly laid, we know much more now about periods of rising and declining inequality for different areas of the globe than we did ten years ago. The next step involves conducting detailed analyses relating these trends to other factors that could influence inequality: like ecology, factor endowments, institutions, international trade etc. As economic historians, we are in an exceptional position to examine the relationship in the long-run: examining both the causes and the consequences of income and wealth inequality. Bringing the scale down to more local levels, we are able to see the different conditions under which these relationships are shaped.

15 D. Acemoglu, S. Johnson, and J.A. Robinson, 'The colonial origins of comparative development. An empirical investigation', *American Economic Review* 91 (2001) 1369-1401; S. Engerman and K. Sokoloff, 'History lessons. Institutions, factor endowments and paths of development in the New World', *Journal of Economic Perspectives* 14 (2000) 217-232.

16 R. Wilkinson and K. Pickett, *The spirit level. Why more equal societies almost always do better* (London 2009).

17 A. Leigh, C. Jencks, and T.M. Smeeding, 'Health and economic inequality', in: B. Nolan, W. Salverda, and T.M. Smeeding (eds.), *The Oxford handbook of economic inequality* (Oxford 2011).

18 J.L. van Zanden et al. (eds.), *How was life? Global well-being since 1820* (Paris 2014).

Globalization

Branko Milanovic's recent book *Global inequality* relates inequality to the second theme that I think will continue to dominate economic history: globalization.¹⁹ This is one of the most studied subjects in the social sciences of the past decades. Historians have also entered this discussion and many articles and books have appeared discussing different aspects of the process. These studies have charted the rise and fall of different periods of globalization. Different scholars have come to different views regarding the question of when the history of the world came best to be seen as an interconnected or a globalized world, with the answer to the question often depending on their preferred definition. In the coming years, the question of 'when did globalization begin?' should give way to the question of 'how did global interaction affect local social and economic developments in different parts of the world in various time periods?'

Globalization, or the opening of economies to the world market, has often been put forward as one of the major factors influencing inequality.²⁰ Has globalization made the world more or less equal? Among social scientists, opinions are highly divided on the issue: while some scholars have argued that globalization has led to economic growth and reduced poverty,²¹ others have stressed that it increased inequality both between and within nations.²² The long-run relationship between globalization and within-country inequality is complex and pends conclusive empirical investigation. So far, cross-country studies have yielded inconsistent results on the effects of openness on inequality within countries.²³ Generalizations are difficult as a result of the large differences in the underlying conditions among countries. Scholars from both sides of the debate have therefore called for in-depth case studies and historical investigations in order to acquire a proper understanding of this relationship.²⁴ Since long-run case-

19 B. Milanovic, *Global inequality. A new approach for the age of globalization* (Cambridge (MA) 2016).

20 A.B. Atkinson, *Inequality. What can be done?* (Cambridge (MA) 2015); M Ravallion, 'Growth, inequality and poverty. Looking beyond averages', *World Development* 29 (2001) 1803-1815.

21 J.A. Frankel and D.H. Romer, 'Does trade cause growth?' *American Economic Review* 89 (1999) 379-399.

22 F. Rodriguez and D. Rodrik, 'Trade policy and economic growth. A skeptic's guide to cross-national evidence', in: B.S. Bernanke and K. Rogoff (eds.), *NBER Macroeconomics Annual 2000* (Cambridge (MA) 2001).

23 B. Milanovic, 'Can we discern the effect of globalization on income distribution? Evidence from household surveys', *World Bank Economic Review* 19 (2005) 21-44.

24 T.N. Srinivasan, J. Bhagwati, 'Outward-orientation and development. Are revisionists right?', Center discussion paper #806 (September 1999); J. Bhagwati, *In defense of globalization* (New York 2004);



Illustration 1 *The consequences of globalization. Working for a sugar factory in Tegal, Java in the 1870s. Painting by Abraham Salm (source: Rijksmuseum Amsterdam. <https://www.rijksmuseum.nl/nl/collectie/SK-A-4777>).*

study comparisons are the speciality of the economic historian, we are exceptionally well-placed to contribute.

Many economic historians have set out to test for the integration of factor and commodity markets as the ultimate evidence of globalization.²⁵ More research should deal with the question whether the integration of markets, i.e. globalization, is actually good or bad in the long run.²⁶ What

D. Rodrik, *The globalization paradox. Democracy and the future of the world economy* (New York and London 2011).

²⁵ G. Federico, 'How much do we know about market integration in Europe', *Economic History Review* 65 (2012) 470-497; K.H. O'Rourke and J.G. Williamson, 'When did globalisation begin?' *European Review of Economic History* 6 (2002) 23-50; P. de Zwart, 'Globalization in the early modern era. New evidence from the Dutch-Asiatic trade, 1600-1800', *Journal of Economic History* 76 (2016) 520-558.

²⁶ For example, a study on market integration and economic development by Roman Studer assumes a positive link exists, but does not actually investigate the relationship: *The great divergence reconsidered. Europe, India and the rise to global economic power* (Cambridge 2015). Especially Jeffrey Williamson has tried to investigate the link in the long run, and examined the different effects of developed and developing countries. For the nineteenth century, see: J. Williamson, *Trade and poverty. When the third world fell behind* (Cambridge 2011). A recent study by Luigi Pascali suggests only a small group of countries, with inclusive institutions, benefitted for the nineteenth-century trade integration: 'The wind of change.

were the consequences of the integration into the world market for the welfare of a population? How was this determined by the broader political and institutional contexts in which this trade took place? This is one of the main issues in the debate on current globalization; different studies have come to different answers, as the answer to the question is largely dependent on the interaction of forces of globalization with local conditions.²⁷ Here, focussed historical case-studies may provide more clues about which countries, regions, and social classes have benefitted from the integration of markets in different periods and world regions. How was this different for countries in the ‘Global South’ as compared with those in Europe and North-America? How has this been affected by geography, factor endowments and social structures? What policies have been effective in mitigating the possible inequality effects of globalization?

Method and scale of analysis

Overall, there should be a shift from charting the periods of rise and decline of e.g. economic growth, inequality, global trade etcetera (as a lot of recent research has done), to explaining these developments, as well as analysing their consequences. I will suggest that this can be done by scaling down the level of analysis from the national, to the regional or the local level. This may allow teasing out the variables that are most relevant for the questions at hand, as many variables tend to differ as much within countries as they do between countries. Newly gathered quantitative data on such lower levels that can be analysed using statistics should be combined with careful qualitative analysis of the areas studied and subjected to extensive source criticism, in order to obtain reliable knowledge about the different relationships we are interested in. This will shed new light on what drove trends towards wealth and poverty in the past, as well as a better understanding of such developments in the present.

Maritime technology, trade, and economic development’, *American Economic Review* (2017) 2821-2854. For an overview of economics articles dealing with the relationship of market integration and development in the present, see: D. Donaldson, ‘The gains from market integration’, *Annual Reviews of Economics* (2015) 619-647.

²⁷ For the early modern era an overview surveying these issues is provided by P. de Zwart and J.L. van Zanden, *The origins of globalisation. World trade in the making of the global economy, 1500-1800* (Cambridge 2018).

In terms of methods, I do not think that we should try to copy the techniques of many economists practicing history (often in the United States), who use advanced econometric techniques to demonstrate that some variable in the past is related to economic outcomes in the present. Such studies have convincingly shown that the past matters for the future.²⁸ The sophistication of their methods is impressive and requires knowledge and skills that most historians simply do not possess.²⁹ This is not as problematic as one may think. The scarcity of historical data and the problems associated with them, as well as the necessary assumptions underlying such studies will always put doubts on their results, no matter how well-specified the model, how sophisticated the techniques to control for unwanted influences on their model, or how ingenious the instrument. While the efforts on behalf of these 'historical economists' will probably continue to be published in the prominent American economics journals in the foreseeable future, the fact that some historical variable that differed across the globe in 1700 has a small, but significant, effect on GDP, or some other measure of wealth or income, in 2010, in fact gives us very little useful information besides what we already know: what happened in the past is important for the present. While the relationships these economists have been investigating are highly interesting, the restrictions of their methods require them to specify their questions and models in such a way that it generally severely limits the conclusions that could be drawn from them. Furthermore, as historians we are more interested in how we got from 1700 to 2010 and what happened in between. Rather than the *persistence* of history, we, as historians, are interested in patterns of *change* over time.

At the same time, these economists are right about the fact that without some proper statistical technique to test for the relationships between variables, and controlling for factors we are uninterested in, we are unable to say something about *causality* between the factors of interest over time. Finding definitive proof of causal relationships is probably outside the realm of the historical sciences and perhaps is only really possible in fields like physics or chemistry, where researchers can conduct laboratory experiments. Social scientists interested in the present have more abundant and better quality data at their disposal that may be analysed using the

28 For example: Acemoglu et al., 'The colonial origins'; M. Dell, 'The persistent effects of Peru's mining mita', *Econometrica* 78 (2010) 1863-1903; N. Nunn, 'The long-term effects of Africa's slave trade', *Quarterly Journal of Economics* 123 (2008) 139-176.

29 That the education of quantitative techniques to bachelor students of history is almost non-existent (at least in the Netherlands), how problematic that is, and what to do about it, is subject matter for another discussion.

same techniques as the studies referred to above. Even better: such social scientists can conduct field experiments, like the Randomized Control Trials (RCTs) that are now often used by development economists (see, for example, the well-known book *Poor economics* by Abhijit Banerjee and Esther Duflo).³⁰ These RCTs have the benefit of establishing the causal effect of a particular treatment, because the treatment is randomly assigned to e.g. a sample of villages, while another, similar, sample of villages constitute the control group. Yet even laboratory and field experiments have limitations: if a RCT has shown that some particular ‘treatment’ (or policy) works in one context that does not automatically mean that it works in another. Economists are always looking for the effects of a specific variable on a particular outcome with all else held equal (*ceteris paribus*). Obviously, *ceteris paribus* conditions do not apply across the globe, which limits the lessons that could be drawn from these causal inferences. Finding out what contextual factors may influence the effect of the policy (and how) is just as important as figuring out the effect of a policy while ‘controlling’ for all other factors. Here seems to lay an important task for economic historians.

At the same time, it is true that historians are generally less precise in their argumentation. This is, for a large part, the result of the (lack of) data at our disposal. Especially for periods further back into the past, continuous good quality time-series data for a multitude of variables are generally lacking. Nonetheless, we can try to be a bit more systematic in our approach. Too often, economic historians make statements about causality after eyeballing a few trends. They tend to prefer to put forward grand theories or statements that are vaguely specified over well-defined and structured arguments with explicit assumptions. So how should we move forward? I follow Jared Diamond, James Robinson, Bas van Bavel and Daniel Curtis in suggesting that we should formulate testable hypotheses and conduct ‘natural experiments in history’ by using the comparative method.³¹ Theoretical work and global cross-country studies can provide the hypotheses that should be tested empirically at the regional or local levels. By comparing regions that are similar in many respects, but differ either in terms of one explanatory factor, or with regard to the observed outcome, one conducts a possible natural experiment in history. In general, as Diamond and Robinson note: ‘the more numerous are the potentially relevant independent

30 A. Banerjee and E. Duflo, *Poor economics, barefoot hedge-fund managers, DIY doctors and the surprising truth about life on less than \$1 a day*. (London 2011).

31 B. van Bavel and D.R. Curtis, ‘Better understanding disasters by better using history. Systematically using the historical record as one way to advance research into disasters’, *International Journal of Mass Emergencies and Disasters* 34 (2016) 143-169.

variables, the more cases must be compared to test for effects of those variables'.³² The case studies should be picked in such a way that their characteristics would allow for testing specific hypotheses.

Thus, in order to test our hypotheses in a multitude of settings, research should shift from comparing variables at the national/country level (which has dominated recent research, see e.g. Clio-Infra) to regional comparisons. The regional case studies should be put in a global comparative perspective and put to use to answer globally relevant questions. While global cross-country data are useful for providing general directions for research, more in-depth regional comparisons allow a more detailed study of the underlying dynamics. Such comparisons, obviously, require the collection of data at levels below the national level. For Europe, such data will probably be more easily found as much data was collected and written down, especially before the nineteenth century, at sub-national levels. For many other parts of the world where there was a writing culture and where many archival pieces have survived into the present, like China and Japan, it seems likely that regional data are similarly available for periods reaching further back into the past. For the colonial era, where my personal interests lie, regional data are also available as many statistics were collected at such levels by colonial civil servants. These data often have to be retrieved from archives, as the data were generally aggregated as they entered into official colonial reports that are widely available in academic libraries and on the internet. I think a systematic comparative analysis of local or regional data will contain the answers to the big questions that we have posed over the past decades.

The comparison of Haiti and the Dominican Republic as carried out by Ewout Frankema and Aline Masé is one example of a well-defined comparison,³³ even if it does not entirely fulfil the conditions as they are based on the country level. Currently, there are only a limited amount of studies that really fulfil the conditions necessary to really test hypotheses. One reason is of course that it is incredibly difficult to find cases that differ only in the variable of interest, but that are similar in all others. Even regions very close to each other, generally differ in a number of ways. Therefore, assumptions about the effects of other variables still need to be made. In my own future research, I will try and compare different regions within Indonesia, Malaysia and Vietnam in order to test the consequences of the growth of

32 J.M. Diamond and J.A. Robinson (eds.), *Natural experiments of history* (Cambridge (MA) 2011).

33 E. Frankema and A. Masé, 'An island drifting apart. Why Haiti is mired in poverty while the Dominican Republic forges ahead', *Journal of International Development* 26 (2014) 128-148.

international trade on inequality during the long-nineteenth century. The case-studies have been chosen on the basis of their varieties and similarities in terms of factor endowments and crop exported. Nonetheless, assumptions will have to be made about differences in variables for which no data are available. These should be made explicit.

In contrast perhaps to some other historians, economic historians are more willing to generalize from the patterns they observe. We already know that the world is complex; it is our task to try and unravel parts of this complexity. While we should perhaps be cautious in claiming definitive proof of causal relationships, I believe that once we have a substantial number of comparative case studies testing different aspects of a relationship, patterns will emerge that provide meaningful insights for some of the big challenges that we are facing in the twenty-first century.

Concluding

While the position of economic history within the broader field of history may have declined as a result of the ‘cultural turn’ in the 1980s and 1990s, it is likely that with the ‘digital turn’ of the early twenty-first century it will gain new prominence. Currently, even cultural historians are refocussing on the digital humanities and are starting to analyse ‘big data’. Economic history, as the most quantitative of the historical disciplines, stands to profit from these developments. In the meantime, economic history established itself as serious social science and, perhaps partially as a result of discontent with ‘regular’ economics since the most recent economic crisis, economic history has gained standing in economics. Books like Acemoglu and Robinson’s *Why nations fail* (2012), Piketty’s *Capital* (2014), Gordon’s *Rise and fall of American growth* (2016), Scheidel’s *Great leveller* (2017), and, closer to home, Van Bavel’s *The invisible hand?* (2016),³⁴ to give a few examples, have captured the attention of a wider audience. These books have powerful and relatively simple messages on the core topics of economic history: colonialism, inequality, and long-run economic growth, that speak to a general audience. Currently, public and academic debates in the Low Countries, and around the globe, are centred on issues for which we, as economic historians, are in a good position to contribute to. This includes discussions on the unequal distribution of the benefits and detri-

34 Recently translated in Dutch and published by Prometheus.

ments of globalization, which derives its relevance from the possibility to inform our understanding of current phenomena by analysing them in the past. But it may also include current debates on the role of the slave trade, slavery and colonialism in the rise of economic growth in the West and how we are to deal with this (perhaps uncomfortable) past at present (see for good examples the projects on slave trade, slavery and unfree labour as carried out at the International Institute of Social History in Amsterdam and Leiden University).

Let me conclude this essay by reiterating my main statements:

- 1 Economic and social history research should address relevant issues and lead to ‘useful knowledge’: insights that could – even if only indirectly – have policy implications.
- 2 The focus of our research should be on the patterns of historical change in various contexts and in the long-run, not on causation or persistence (while holding all other things equal).
- 3 The comparative method, combining quantitative techniques with extensive source criticism and qualitative research, remains our best tool for reliably uncovering relevant patterns of the past in order to shed light on the present.
- 4 This method is best applied at a scale of analysis below that of the nation-state: regions, provinces, and municipalities, that are equal in most respects, but differ in terms of the (left- or right-hand side) variable of interest.

About the author

Pim de Zwart (1986) is Assistant Professor at the Rural and Environmental History Group of Wageningen University. He obtained his PhD in economic history from Utrecht University in 2015 (*cum laude*). In 2017, he received a NWO Veni research grant for the project ‘Unfair Trade: Globalization, Institutions and Inequality in Southeast Asia, 1830-1940’. His recent publications include the book *The origins of globalization: world trade in the making of the global economy, 1500-1800*, with Jan Luiten van Zanden (Cambridge 2018).

E-mail: pim.dezward@wur.nl