

(consumers, money-lenders) did not adapt their behaviour to the new environment of a stable currency. In the meantime, however, they seem to have learned this lesson and, therefore, the prospects for the euro look much more positive. To see the EU as a community of law would also imply a role different from the one foreseen by Giddens, which is to be a new powerful actor on the global scene, on a par with the USA and China, and backed by military power. Rather, it would confirm the old idea of Europe as a 'Civil Power', focusing upon peaceful negotiations instead of military interventions and the strengthening of such methods and institutions (such as the United Nations) around the world. The EU has a world-wide positive image just because it limits itself (maybe willy-nilly) to such a role.

Thus, in my view there exist a series of objections against the vision that the EU should become a federal state. Nevertheless, the ideas proposed by Giddens have sharpened such a vision and they also force one to clarify the counter-arguments. In so doing, this book is an important contribution to an ongoing, pivotal question for Europe.

Max Haller
University of Graz
max.haller@uni-graz.at

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Wiemer Salverda, Brian Nolan, Daniele Checchi, Ive Marx, Abigail McKnight, István György Tóth and Herman van de Werfhorst (eds.): *Changing Inequalities in Rich Countries: Analytical and Comparative Perspectives*
Oxford 2014: Oxford University Press, 432 pp.

In his Gifford lectures (*If you're an egalitarian, how come you're so rich?* [Cohen 2000]), G.A. Cohen described a philosophical problem which polarised the philosophy departments at Harvard and Oxford. Members of those departments, headed by Quine and Ayer, prided themselves on their ability to think analytically and logically. Yet Cohen points out that what, without exception, determined their views of the issue was only which institution they belonged to. The book under review—*Changing Inequalities* (henceforth CI)—mentions a disagreement on trends in intergenerational mobility. Despite analysing exactly the same data from two UK birth cohort studies, economists and sociologists came to opposite conclusions as to whether social mobility had increased or decreased.

CI reports on a project funded by the European Commission. It analysed trends in income distribution, their causes, effects, and possible policy responses. Its contributors are predominantly economists but include a few sociologists. But over the last 30 years, research on the societal and health effects of wider or narrower income differences has come predominantly from specialists in public health and epidemiology. Disagreements between economists and epidemiologists on these issues have already attracted academic attention [Kawachi 2001]. Trained in economic history and epidemiology, I might claim impartiality, but because the damaging effects of inequality, which Kate Pickett and I showed in *The Spirit Level* (henceforth SL), are disputed in several chapters of CI, I am a protagonist.

In SL we show that most of the health and social problems that tend to be more prevalent lower down the social ladder are also much more common in countries with bigger income differences between rich and poor. We demonstrate this relationship internationally and among the 50 states of the USA. Our data cover poor physical and mental health, lower levels of child well-being, lower levels of social cohesion, violence, bullying in schools, teenage births, the proportion of the population imprisoned, low social mobility, drug abuse, and lower maths and literacy scores [Wilkinson and Pickett 2010].

CI also reports finding significant associations between greater inequality and: higher teenage birth rates, lower social mobility, low social cohesion, higher rates of imprisonment, status anxiety, happiness, infant and child mortality, lower political participation and lower support for democracy. But the authors usually find reasons to doubt causality. Sometimes relationships are sensitive to the inclusion of particular control variables, or they weaken when time series associations are analysed, or expected interaction effects are not found.

The disagreement boils down to conceptual models. As the editors of CI say, 'arriving at a causal interpretation of the consequences of inequality ... requires deductive theory building and hypothesis formulation and testing' (p. 6). Why then is there no proper discussion of what might lie on the causal pathway and should not be controlled out, or of what might be confounders and should be controlled? Why no discussion of lag times in time series analyses and no recognition of previous work on this? The best paper on lag times is Zheng [2012]. It uses a multilevel model controlling for income education, work status, age, gender, and marital status. Controlling in turn for a series of previous lagged changes in inequality, it finds the effects of inequality start to come in after

about 4 years, and continue to accumulate until around 12 years after a change in inequality. Although Zheng reviews—and makes sense of—previous findings from time series analyses, it is not referenced in CI. Nor is the largest meta-analysis of multilevel models of income inequality and health, many using panel data [Kondo et al. 2009]. And the question, even where inequality is increasing, is not simply whether health got better or worse, but whether it got better faster or slower than it otherwise would have done.

But there is a more fundamental problem. There are now over 60 papers on the relation between inequality and violence and at least 250 on inequality and health. Although a small proportion have negative findings, they overwhelmingly support the view that more unequal societies have more violence and worse health [Wilkinson and Pickett 2006]. More recently, there has been a growing literature on how other outcomes may also be related to inequality. Indeed, commenting on replications of the finding that inequality was related to teenage birth rates and to depression, Paul Krugman commented that 'this is another sign that Wilkinson-type views about the corrosive effects of inequality are going seriously mainstream' [Krugman 2012].

In this situation, it is likely that a better understanding would be gained from looking for a consistent interpretation of all the evidence rather than from producing another few analyses of data. Too often researchers think primarily about their own data and how their results fit with other work on the same outcomes—homicides, or teenage births, or anything else. But we are now seeing associations between inequality and a very wide range of outcomes. The common factor seems to be that they are all outcomes with social gradients making them more common further down the social ladder. We tested this using death rates from causes of death which do and do not have social gradients: those

with stronger social gradients were also more strongly associated with inequality [Wilkinson and Pickett 2008].

Is the task then to explain results concerning—separately—health, or homicides, or child well-being, or social cohesion? Or is it to explain why outcomes with negative social gradients are more common in more unequal societies? Take, for example, the USA. It is one of the most unequal of the rich nations and has amongst the lowest life expectancy, the highest homicide rates, the highest teenage birth rates, the highest imprisonment rates, the highest obesity, almost the lowest UNICEF child wellbeing scores, amongst the poorest mental health and amongst the lowest rates of social mobility. Many studies—though not all—show these outcomes graded internationally according to levels of inequality. At the opposite end from the USA are the more equal Scandinavian countries and Japan, which do well on most of these outcomes. Should we treat the USA as if it were just chance that it suffers such a high prevalence of so many apparently unrelated problems with social gradients? And is it merely luck that more equal countries tend to do well? Should we turn to unrelated explanatory factors for each problem—to nutrition and smoking, to school systems, family relationships, penal systems and so on? Or do we look for a more general explanation to do with social differentiation itself?

The prize is surely an understanding of why some countries do systematically better than others. If the only common factor, shared by all the apparently very different problems related to inequality, is that they all have common roots in low social status, then that has surely to be the focus of attention. (Despite ignoring key multi-level studies controlling for all individual incomes, CI does at least show that more unequal countries do not have more poverty—defined in terms of material standards.)

So what about causal models? Research on income inequality and population health arose from research on health inequalities and the social determinants of health. Measures of social position were initially assumed to be proxies for more specific underlying material determinants of health, but the evidence increasingly suggested that social status differences are themselves close to the real determinants of health [Marmot 2004]. That emerging picture has gone hand in hand with a recognition of the power of psychosocial influences on health. Working through the biology of chronic stress, these act as general vulnerability factors producing effects analogous to more rapid ageing [Pickett and Wilkinson 2015].

A meta-analysis of hundreds of experiments on how stress hormones respond to different situations shows that the most potent sources of stress are situations which include 'social evaluative threat'—'threats to self-esteem or social status ... in which others could negatively judge performance' [Dickerson and Kemeny 2004]. The insecurities and anxieties about how we are valued by others perhaps explains why the most powerful psychosocial influences on population health are so highly social—hinging on social status, lack of friendship networks, and a difficult early childhood social environment.

Across species, social ranking systems have determined differential access to resources and reproductive opportunities. It should not therefore surprise us that psychologists and neurologists have shown that the human brain has an evolved 'dominance behavioural system' [Zink et al. 2008]. A major review of psychological responses to hierarchy identifies various forms of psychopathology relating variously to the struggle for dominance, the struggle against inferiority, and to the acceptance of either dominance or inferiority [Johnson, Leedom and Muhtadie 2012]. Perhaps because of disciplinary bounda-

ries, none of this research is discussed or referenced in CI and there is little or no mention of social or psychological stress.

Bigger differences in material wealth make social hierarchy more important: they increase social distances and feelings of superiority and inferiority and strengthen the downward prejudices against those seen as inferior. Money, as the key to material differences, provides the framework in which the cultural markers of status and distinction grow. Most fundamentally, what the association between a society's income inequality and a range of health and social problems tells us is merely that problems which are related to social status within a society get worse if you increase social status differences. And the causal processes are simply that bigger material differences strengthen all the ways (known and unknown) in which social position imprints itself on us from early childhood onwards. The only possible surprise is that, although the effects of inequality tend to be greatest lower down the social ladder, the better off do not escape its effects. When status becomes more important as a social yardstick, it affects us all. Hence, if politicians want to reduce the salience of class in society, or to improve social mobility or strengthen community life, there is no more powerful policy lever than reducing the differences in income and wealth.

Relations of friendship are almost the opposite of relations of dominance and subordination. Friendship is based on relative equality and sharing, but dominance hierarchies are about gaining privileged access to resources. This is why gifts are a symbol of friendship. And just as low social status gives rise to poor health, friendship is highly protective of health: a review of 150 studies showed that friendship networks are as important to survival as whether or not you smoke [Holt-Lunstad, Smith and Layton 2010].

The philosopher of science, Professor Sir Karl Popper, taught that the mark of a

good theory is whether it makes novel predictions which are confirmed when tested. Early evidence that income inequality and population health were related internationally was first explicitly tested and independently confirmed by groups at Harvard and Michigan. Both looked to see if similar relationship existed among the 50 states of the USA [Kaplan et al. 1996; Kennedy, Kawachi and Prothrow-Stith 1996]. There are now very large numbers of replications of these findings in many different settings. The tendency for more unequal societies to have higher homicide rates has also been replicated many times.

Following evidence of association, it was suggested that the causal mechanism linking greater equality to better health, might be that more equal societies were more cohesive and enjoyed better social relations [Wilkinson 1996]. A year later that prediction was tested: path analysis showed that the relationship between greater equality and lower death rates among the US states was mediated by social capital [Kawachi et al. 1997]. The effects of inequality on various aspects of social cohesion have since been confirmed many times. The hypothesis that rising inequality would strengthen the 'social evaluative threat' causing narcissism to replace modesty has also been confirmed [Loughnan et al. 2011; Twenge et al. 2008].

Each time a new observation has arisen after a relationship between inequality and an outcome has been published, the new data are in effect a test of that relationship. When data on social mobility and mental illness rates became available for several additional countries, the new data were found to fit previously established relationships between those outcomes and inequality [Wilkinson and Pickett 2010]. Lastly, the first evidence that mental illness was more common in more unequal societies used general measures of mental illness. That relationship has since been confirmed by papers showing that more

specific forms of mental illness—depression, schizophrenia and psychotic symptoms—are more common in more unequal societies [Messias, Eaton and Grooms 2011; Burns, Tomita and Kapadia 2013; Johnson 2014].

There is a widespread assumption that economists are the right people to do research on income distribution. But when it comes to understanding its social effects, a deeper understanding of the social sources of stress, its psychology and physiology, is required. Economics cannot be neatly separated from all that surrounds it. As an example, take the tendency towards increasingly 'assortative' mating (the tendency to live with a partner with whom you share characteristics such as education, class, earnings capacity). This is often described (as it is in CI) as an important contributor to rising inequality because it leads to more households in which both partners earn good salaries and others where neither is employed. But if class and status become more important indicators of personal 'worth' in more unequal societies, then perhaps it is a response to, rather than an initial cause of, rising inequality. An analysis of data from 44 countries concluded that 'household income tends to have a much stronger effect on class identification in societies with a high level of income inequality compared to societies that are more economically equal [Anderson and Curtis 2012]. Similarly, a study of data from 56 countries showed a strong association between greater inequality and people placing more emphasis on status over love when choosing potential mates [Lim, Bond and Bond 2005]. This means that increasing assortative mating is a response to inequality, one which provides feedback, rather than being an independent cause.

Economic analysis seems most at home with the *mechanics* of how income differences widened and what might reduce them, but it is less at home with the likely

political and ideological forces which may be the real drivers of trends in inequality. The chapters of CI which set out to explain the causes of the almost universal increases in inequality show how much was due to widening earnings differences and how much to changes in the tax and benefit system. But there is little mention of the political forces behind the mechanics. If, however, you look at the major changes in income distribution throughout the 20th century, it is clear that inequality began to decline from the 1920s or 1930s and went on declining until sometime in the 1970s, after which the modern rise in inequality begins. This 'U' shaped pattern almost certainly reflects the strengthening and then the weakening of the labour and social democratic movement as a countervailing voice almost everywhere, probably supplemented by the rise and fall of the communist threat. That view is supported by sociological research showing very strong cross-sectional and time-series correlations between trade union membership (as an indicator of the waxing and waning of the labour movement) [Gustafsson and Johansson 1999]. Similarly, the World Bank's *Asian Miracle* puts the decline of inequality in the former 'tiger economies' during the 1960s and 1970s down to 'crises of legitimacy' in the face of communist rivals—South Korea in relation to North Korea, Taiwan and Hong Kong in relation to China, etc. [World Bank 1993]. If, as Krugman also argues [Krugman 2009], political forces were key to the major changes in inequality, then these issues should also have been discussed in the chapters on policies to reduce inequality. CI has very little to say about the political forces behind changes in inequality. While failing to give proper weight to the effect of politics on inequality, it has a whole chapter on the effects of inequality on politics—particularly the decline in support for democracy.

The discussion of policies to reduce inequality is divided in CI between a chapter

dealing with redistribution and another dealing with reductions in pre-tax incomes. As elsewhere in the book, there is interesting material on the mechanics—on the failure of increases in employment to reduce inequality and the difficulty of reducing in-work poverty. But conspicuous by its absence is a proper discussion of what can be done to bring down the inflated salaries at the top which have played such an important part in the rise in inequality. The book is silent on whether companies should publish their pay ratios, or on whether it would help to have more employee representatives on company boards or to ensure the recognition of trade unions. And what about policies to prevent tax avoidance? Where is the discussion of whether the OECD's attempts to close down tax havens is likely to be successful? Until progress is made there, governments will remain reluctant to raise top tax rates and income tax will continue to be unprogressive. As a subject, inequality is inherently political. Analyses of changes in inequality are not well served by a desire to stay out of politics. Subjects like this do not respect the boundaries between academic disciplines. But the academic fear of abandoning the appearance of impartiality is surely based on a misunderstanding of how science works. As Popper also pointed out, it is not the sources of theories which have to be unbiased, but the methods of testing them.

Richard Wilkinson
University of Nottingham (emeritus)
and The Equality Trust
richard@richardwilkinson.net

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Milton Lodge and Charles S. Taber:
The Rationalizing Voter

New York 2013: Cambridge University Press, 300 pp.

The ability of humans to reason, judge, and decide plays a key role in our understanding of social interactions and is a cornerstone for any conception of democratic rule. In this book, Milton Lodge and Charles S. Taber, two Stony Brook political psychologists, present a new perspective regarding the ways attitudes and behaviours are constructed in political and social contexts. The main argument, thoroughly presented, is that unconscious spontaneous feelings impressively condition subsequent conscious deliberative processes. In the authors' own words: 'This is a book about why the first 100 milliseconds of thought matters.'

The opening chapter presents the main argument. Political behaviour and judgement are driven by unconscious spontaneous affective reactions that condition consequent memory retrieval and reasoning. The authors review findings in psychology and neuroscience which attest to the strong effects of unconscious primes. Given that consequent reasoning only rarely and partly shapes decisions, its main function is to rationalise an existing judgment, rather than determine it. In the second chapter the authors present their theory—the 'John Q. Public' (JQP) model. Their starting point is the limited capacity of our working memory (conscious thought), which requires a highly selective retrieval process of information from long-term memory (LTM). This process is determined by the associative organisation of LTM. Their model rests on 'seven postulates'. The first proposition, 'automaticity', draws on the affect-driven, dual-process modes of thinking and reasoning [Evans 2008] that have crystallised over the recent three decades in cognitive and social psychology and in the neuroscience literature. Central to such dual-process models is the distinction between unconscious (system I) and