The elusive rich: Piketty's data battles and the power of absent evidence

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Introduction

'We should have studied the rich.'

- Erving Goffman

Rumour has it these were Goffman's last words. Whether or not the rumour is true, the wry disappointment, the acknowledgement of a missed opportunity, continues to haunt the social sciences today.

On the one hand, wealth and its production have obviously been an overarching, if not *the* defining preoccupation of the social sciences since the nineteenth-century onwards, from Marx and Weber through to theorists such as Veblen and Bourdieu. That preoccupation clearly animates social scientists today. Compelling articles within economic anthropology have explored how the promise of future wealth through immersion in global circuits of capital can generate unexpected political alliances and hopes (Miyazaki 2006); reveries of untapped resources can both invigorate and deflate a populace regardless of whether dreams of wealth are realized or not (Weszkalnys 2011).

And yet, the lived experiences of the wealthy – as well as how they earn and maximize their wealth – often remains inscrutable, veiled from investigation thanks to the rich shielding themselves and their money through gated enclaves or offshore tax havens.

There have been notable exceptions to the lacuna of sociological studies on the political economy of wealth and the lived experiences of the rich, in particular from scholars such as Scott (1996), Domhoff (1979), and Keister (2005), to name just a few. There is also growing attention to the importance of elites in poor and middle-income economies (see Whitfield and Buur 2014). Despite these efforts, attention to the global wealthy has not kept pace with the vast attention paid to poverty, deprivation and the lives of the global poor – a neglect that scholars such as Mike Savage, Karel Williams and Andrew Sayer have emphasized, suggesting, in Savage and Williams' memorable phrase, that elites have been 'remembered in capitalism and forgotten by social science' (Savage and Williams 2008: 1; Sayer 2012). Savage and Williams argue that methodological preoccupations have entrenched this neglect – especially a longstanding use of national sample surveys which were incapable of capturing the lifestyles and sources of wealth of an ultra-elite segment of the rich who through the 1980s and 1990s were obscured as a result of their relative invisibility in national surveys.

Neglect of the underlying causal factors of growing wealth concentration is even more marked when one looks outside sociology to mainstream economic theory. As a number of heterodox and mainstream economists have underscored recently, the problem of wealth concentration largely ceased being a primary focus of mainstream economists from the 1950s onwards (see Piketty 2014; Wisman 2013). One reason in economics for this neglect is disciplinary tension among those specializing in microeconomics versus macroeconomics. Economists such as James Galbraith have pointed out that most discussions of inequality from the mid-century onwards tended to focus on individual-level characteristics in order to explain inequality, suggesting that factors such as uneven access to higher education play the strongest role in growing wealth divides. It is only more recently, as marked patterns of inequality grow more pronounced across the world, that economists have begun to treat inequality as classical and early neoclassical economists once did – as a problem rooted in political economy (Galbraith 2014).

One challenge making it harder to develop a full picture of global wealth inequality is the fact that each type of methodology for measuring wealth has its own distinctive weaknesses and strengths, and any methodological approach purporting to offer a conclusive understanding of global wealth divides is plagued by the problem of known uncertainties over the quality of global wealth statistics (Keister and Moller 2000; Piketty 2014).

Thomas Piketty's emergence as a celebrity economist bearing some of the most comprehensive data sets on global inequality ever compiled has therefore been timely and tenuous at the same time. As soon as his stardom seemed granted it was imperilled by the very thing that earned him global accolades: the quality of his data.

In this article, I take recent debates over Piketty's wealth data as an inspiration in order to consider two related questions. First, why did attention to wealth disparities cease to be a major preoccupation of neoclassical economic theory during the twentieth century, and what role did dominant theories of income distribution play in this neglect? Secondly, how can attention to 'absent' data help to shed light on the limits and advantages of Piketty's current argument – that levels of inequality are troubling, unjust and likely to exacerbate global strife unless reined through redistributive policy measures?

The article has two main sections. In the first half, I turn to an almost forgotten late-nineteenth-century debate over income distribution – a debate that was launched when John Bates Clark, an influential US economist, introduced his marginal theory of income distribution. This debate, which pitted incipient 'neoclassical' thinkers against more historically oriented political economists – does not get mentioned much by social scientists today. And yet, attending to its nature, scope and implications is, I suggest, vital for understanding some of the limits and strengths of Piketty's argument.

In the second half, I explore recent challenges to the robustness of Piketty's wealth data. I suggest that situating those recent data challenges next to my earlier discussion of Clark's work helps to underscore a problem facing not simply Piketty but neoclassical scholars more generally. That problem is the displacement of earlier, nineteenth-century preoccupations with illegitimate rent extraction, and the related absence of data tracking different forms of 'earned' versus 'unearned' income. Data in this case are not simply missing: they have never been of mainstream interest and never come into existence, thus remaining shadows of potential, unrealised research trajectories.

A fair wage for a fair day's pay?

One of the most vexing questions facing US academic economists at the turn of the twentieth century was the question of whether the industrial market economy that had flourished over the previous century in British and the United States was morally defensible or not. From the end of the US war of independence in the late eighteenth century to the start of the twentieth century, the US economy underwent a rapid transition from a primarily agricultural economy to a developed industrial one. Both socialists and laissez-faire proponents alike were acutely aware that the gains of industrialization were not reaped equally by capital-owners and labourers.

The most outspoken critics of the distribution of economic gains under advanced industrialisation were obviously Marxist thinkers. But increasingly non-socialist thinkers joined in the denunciation of the rapidly expanding market economy. The most notorious of these was Henry George, a self-taught economist who is little known today but who was once one of the most popular writers of the late nineteenth century – when he died in 1897, over 100,000 mourners gathered in the street of New York to pay their respects at his funeral, the largest number of congregants to amass since the death of Abraham Lincoln two decades earlier.

George was not a socialist – he was a strong believer in the benefits of open trade. But he also thought that growing wealth concentration threatened the fair distribution of economic profits. Observing the rapid expansion of the oil and rail sectors, he pointed out in his bestselling book, *Progress and Poverty*, that while 'some get an infinitely better and easier living… others find it hard to get a living at all' (George 2012[1880]: 4). For many of his adherents, he offered an attractive middle terrain between socialism, on the one hand, popularized by union leaders such as Eugene Debs, and free trade on the other, with its famous proponents such as the industrialist Andrew Carnegie – the steel baron who supressed his workers' right to unionize even as he built a philanthropic empire ostensibly aimed at lessening their poverty.

In a climate of growing labour militancy and both surreptitious and overt efforts by industrialists to expunge unions from their factories, academic economists of the period were torn between their admiration for abstract theories of political economy handed down through Smith, and their growing recognition that labourers were not appeased by the maxims of Smith's nineteenth-century apostles: thinkers such as Herbert Spencer who proclaimed that a labourer's economic deprivation under industrialization was a natural, even munificent result of evolutionary development. '[It] seems hard that a labourer incapacitated by sickness from competing with his stronger fellows, should have to bear the resulting privations. It seems hard that widows and orphans should be left to struggle for life or death,' Spencer proclaimed in his landmark book, *Social Statics*, published in 1850. 'Nevertheless, when regarded not separately, but in connection with the interests of universal humanity, these harsh fatalities are seen to be full of the highest beneficence (Spencer 2012[1896]: 150).

This line of thought might have been attractive to the industrialists of the day, but it was hardly palatable to working men, women and children.

It was in this tense climate of debate over the morality of capital accumulation that John Bates Clark first devised his theory of the marginal productivity of income distribution. Today, Clark's legacy is honoured through the John Bates Clark Medal – a highly prestigious award granted by the American Economic Association to American scholars under the age of 40 who are viewed to have made significant contributions to the field. He is seen as one of the neoclassical pioneers of the marginalist revolution in economic thought – a period when economists shifted to a largely psychological, 'subjective' understanding of the value of different commodities, in contrast to the 'objective' labour theory of value held by Marxist scholars and classical political economists such as Ricardo and Mill. Clark's contributions to economics are celebrated through the medal in his honour. But what is surprising and little known about his legacy is that a key aspect of his work – his theory of the marginal productivity of income distribution – was largely refuted even by Clark's mainstream sympathizers, in particular George Stigler and Paul Samuelson. Curiously, the spirit and principle of his idea continues to be taught as orthodoxy within neoclassical macroeconomic theory despite longstanding challenges to its legitimacy.

Clark was born in Rhode Island in 1847. He attended Amherst College, and then, like other notable scholars of his generation, spent time as a student in Germany, before returning to the US and eventually teaching at Columbia University. In his earliest work, he was sceptical of the role of market competition in allocating resources in a just manner. Unhampered competition, in his view, was not compatible with promoting societal ideals of good will. He later reversed this position, advocating something that he termed "potential competition" – the idea that if monopoly powers abuse their own position by raising prices above competitive levels, new competitors will take advantage of unnaturally high prices by offering more competitive rates. Clark's influential argument helped to arm business interests against calls for stricter anti-trust polices (see Morgan 1993; Groenewegen 1999; Leonard 2003; Fioriti 2012).

Clark also developed a theory of income distribution that has since become a central pillar of neoclassical economic thought. Developed as a direct rebuttal of Marx's theory of surplus value, Clark's marginal productivity theory suggested that the owners of capital, on the one hand, and labourers, on the other hand, each receive an income that is directly proportionate to the amount of wealth that they create.

Just over a decade after publishing *The Philosophy of Wealth* (1886) – an essay which lamented the corrosive effect of free market competition – Clark published *The Distribution of Wealth: A Theory of Wages, Interest and Profits* (1899), a book which advanced a powerful defense of the profits earned by capital owners.

> In his introduction to this highly influential second book, Clark takes aim at Marx's belief that workers are unfairly deprived of the full value of the product of their labour: The indictment that hangs over society is that of 'exploiting labor.' 'Workmen,' it is said, 'are regularly robbed of what they produce'… If this charge were proved, every right-minded man should become a socialist; and his zeal in transforming the industrial system would then measure and express his sense of justice (2012[1899]: 4).

Clark goes on to insist that not only is that charge unfounded – but that the exact *opposite* is the case:

> [T]he distribution of the income of society is controlled by a natural law, and…where natural laws have their ways, the share of income that attaches to any productive function is gauged by the actual product of it. In other words, free competition tends to give to labor what labor creates, to capital what capital creates (3).

This second passage of Clark's helped to indoctrinate a sentiment that has become entrenched in neoclassical theories of distribution since: the idea that remuneration levels are an accurate reflection of one's economic contribution.

The first obvious target of Clark's tract was Marxist thought. But he was also writing in response to the growing influence of Henry George. It is useful, thus, to understand the connection between George and Clark's ideas in order the grasp the importance today of ongoing debates over the measurement of capital – in particular questions over whether some wealth is 'earned' rather than 'unearned.'

The main line of thought animating George's *Progress and Poverty* – a book which sold millions of copies during the 1890s, second only to sales of the bible in the United States – is his concern that growing land shortages would lead wages throughout the nation to plummet. George realized that with the impending closure of the American frontier, workers were literally losing their bargaining leverage: they could not point to the existence of uncultivated land as a rationale for receiving better wages in urban regions (see Stabile 1995; Persky 2000).

Why did George think that the erosion of land availability would inevitably lead to lower wages in non-agrarian regions?

In George's view, wages throughout the nation were set by what workers could earn for themselves if given the chance to cultivate their own plots of land. In determining what work they would accept, they could, in theory, consider other opportunities – such as the ability to homestead land themselves. George suggested that the general level of wages was therefore firmly anchored to the highest possible product that individuals could earn on what he called 'no-rent' land. The theoretical possibility of cultivating existing land reserves was thus, in George's reading, a powerful restraint on the ability of capital to slash wage levels.

As the availability of surplus land shrank, George was certain that wages would necessarily fall.

Clark was sympathetic to George's argument. He thought that George's general perception of the *origin* of wages was accurate. But where he differed was in the belief that the *allocation* of wages was unjust. Clark thought that George had described labour's opportunity for increasing their own "marginal opportunity" – their ability to press for higher wages – too narrowly. Building on George's thesis, he put forward a novel concept, a phenomenon that he termed the 'zone of indifference.'

Clark suggested that a 'zone of indifference' could be found in virtually every industrial sector. It consisted of the surfeit of wasted or dilapidated materials of production, from outdated tools to poorly placed factories or mills – areas where the possibility for significant economic rents was limited due to the poor quality of materials but where, nonetheless, labour had the theoretical possibility of using such outdated materials in order to meet its own economic needs. This "zone of indifference" created, in his view, a sort of ad infinitum bargaining chip for the working classes. Clark believed that workers could always point to the existence of such depreciated goods as a rationale for receiving fair wages. From this observation, he derived the "natural law" of income distribution cited above: the idea, as he put it, that "where natural laws have their way…free competition tends to give to labor what labor creates, to capital what capital creates" (Clark 2012(1989): 3).

Clark took pains to point out that such a model rarely holds true in practice. He emphasized that 'frictions' and other market imperfections 'delayed the effects of the workings of static laws.' But, as Morgan points out, this did not deter him from suggesting that under *ideal* conditions, labour had the same ability to receive economically justified financial rewards as capital-owners (see Morgan 1992: 31).

This belief – that under ideal conditions, labourers receive an economic contribution that reflects their 'natural' contribution to the production process – is the founding kernel of marginal productivity theories of income distribution, theories which continue to be championed today as evidence that 'wealth-creators' deserve whatever windfalls they reap.

A second repercussion of Clark's notion is that his 'law' essentially extinguishes the problem of illegitimate rent-seeking by positing that under ideal market conditions, rent-seeking cannot exist: all proceeds to capital-owners are a natural reflection of the economic contribution they have made. The spectre of rentier profiteering disappears – at least in theory.

What is remarkable about Clark's theory – and why it is so relevant to debates over wealth distribution today – is that very few of his contemporaries or later generations of economists were, at face value, persuaded by it. Some of the fiercest criticism of Clark's theory came from mid-twentieth-century Chicago School economists who felt that Clark's theory was overly simplistic, and thus ceded too much ammunition to *critics* of neoclassical economics. The economist George Stigler, for example, has lamented the fact that Clark's 'naïve productivity ethics' could logically lead to a defence of Marxist economics: if workers managed to negotiate large pay-outs, Clark's thesis could equally be used to support whatever gains they receive – economically efficient or not (Stigler 1980: 165). Stigler adds that one of the strongest criticisms of Clark came from Frank Knight, who published a refutation of Clark's thesis in 1923. Stigler notes that one of Knight's main concerns was that economic competition, far from being ethically 'just', often 'distributes income largely on the basis of inheritance and luck' (Stigler 1980: 166).

Ever since the 1920s when Knight first expressed his reservations about Clark's notion, leading neoclassical thinkers have called attention to the limits of Clark's idea. One example is Paul Samuelson, the recipient of the first ever Bates Clark Medal, awarded to Samuelson in 1947. During that same year, Samuelson published his ground-breaking book, *Foundations of Economic Analysis*, which advanced the mathematical modelling of utility maximization in economic theory. In that volume, he raised concerns about the plausibility of Clark's notion of income distribution (see Persky 2000). He reiterated those concerns a few years later, noting his own surprise that other scholars had not recognized the weaknesses in Clark's formulation: 'To my astonishment I find that the arbitrariness of J.B. Clark's views on the deservingness of competitively determined rewards is not universally recognized' (Samuelson 1966: 1577, quoted in Sen 1997: 101).

Joseph Schumpeter raised similar concerns. In his *History of Economic Analysis*, Schumpeter makes his admiration of Clark's contributions to marginal utility theories clear. But he also castigates Clark for conflating a theory of income distribution with a theory of morality. Clark's problem, according to Schumpeter, is that he 'asserted that distribution according to the 'law' of marginal productivity is 'fair.' And this, in the eyes of the [economics] profession…created an association between 'Clarkian marginalism' and capitalist apologetics' (Schumpeter 1972 [1954]: 870).

The main concern of neoclassical theorists, in summation, is that the naivety and arbitrariness of Clark's theory made it easy for critics of marginalism to dismiss its explanatory power. And dismiss it they did. From scholars such as Joan Robinson onwards, heterodox thinkers have mounted persuasiveness critics of the theory's validity (Robinson's argument is described in more depth in the next section). In some fields, these criticisms *have* been influential. As the heterodox economist Fred Moseley points out, they have led to the gradual elision of the notion of marginal productivity theories of income distribution from microeconomics textbooks (Moseley 2012: 115).

But the opposite is the case when it comes to macroeconomics, a field where, as Moseley has emphasized, scholars such as Gregory Mankiw, the author of the one of the most influential macroeconomic textbooks in the world, continue to the teach the principle of Clark's notion of income distribution as if there was a general consensus in neoclassical economic theory about its validity. As I describe below, drawing on Mankiw's writing, versions of Clark's theory remain highly influential to this day, embedded throughout undergraduate macroeconomics teaching; influencing policymaking; and figuring in recent debates over the merits of Piketty's criticisms of wealth inequality.

Piketty's work offers a useful lens to explore the ongoing influence of Clark's notion. In this next section, I turn to Piketty's work and debates he has raised, starting first with an introduction of Piketty's main theoretical points, then a discussion of his data and its limits – before concluding with a discussion of the twin problems of income distribution and economic rent.

Piketty and his critics

To understand Piketty's arguments about inequality, it is necessary to first examine how he defines capital: any form of wealth – including land wealth; housing assets; wages; dividends from shares in public owned companies; returns from patents on intellectual property – that have a current or a historical market value. In other words, his definition and measure of capital is a financial denomination based on the pricing or valuing of a good or service once it has been accorded a market price.

This definition has a number of strengths. The first is that it is broad enough to allow him considerable scope for comparing varied sources of global inequality data – something that has historically been unavailable. Not simply because much wealth is hidden from sight in offshore tax havens, although that's a major problem. But because different countries, different regions, and different historical periods have varied methods for collecting wealth data, and therefore data comparisons across different regions and historical periods are always bound to be limited and imperfect.

Piketty's accomplishment is to bring together some of the most robust sources of wealth data available worldwide into one collective picture: from tax records, census data, inheritance and estate records he develops a robust corpus of evidence on wealth divides in some of the world's largest and most influential national economies, with a focus on France, the UK, United States, and, to a lesser extent, Germany, Sweden, Japan and Canada. From this data, he's able to estimate that returns to private capital exceed general economic growth levels to an extent that signals a return to nineteenth century levels of inequality.

His definition of capital also has its weaknesses – two in particular – one of which has been readily acknowledged by Piketty and one of which largely hasn't. This first is that Piketty's definition of capital excludes 'human capital' – a broad term referring to the general store of less tangible assets such as social networks and educational opportunities that can be important to one's life chances for social mobility but which Piketty necessarily excludes because he is restricting his analysis to things that be exchanged in global markets and therefore have a financial value. Piketty has conceded this point, effectively neutralizing it through accepting its accuracy: in an interview with media, he acknowledged that 'human capital' is key to inequality, adding – 'next time I will write an even bigger book!"

The second perceived limitation is that Piketty's definition of capital is that it largely ignores treatments of capital from scholars such as Marx, Ricardo and Smith – scholars who came before the neoclassical turn. Piketty's definition is firmly rooted in the neoclassical tradition. Why does this matter? Because his definition effaces questions of rent and unearned income that occupied Marx and his contemporaries (for excellent summary of controversies of the definition of capital in mainstream economics, see Cochrane 2011).

One of the clearest illustrations of this limitation comes from the work of the economist James Galbraith. Like Piketty, Galbraith has been at the forefront of the small group of mainstream economists who have spent the past two decades calling attention to the problem of wealth disparities. And his findings are broadly similar to Piketty: inequality levels in wealthy nations *have* been rising steadily since the 1980s. But Galbraith and Piketty's explanations for *why* inequality is growing are notably different.

A 2014 review by Galbraith of Piketty's book teases out these differences.

Galbraith sees Piketty's reliance on a neoclassical definition of capital as a serious limitation of his argument. He offers the example of Piketty's assessments of the capital-national income ratio in the Anglo-American world. Since the 1970s, the market value of capital assets has grown from an estimated 250-300 percent of national income in Anglo-American nations combined, to 500-600 percent of national income. Piketty uses such figures to underscore his argument that a small group of capital owners are increasingly capturing a disproportionate amount of national revenue. And yet, the problem is, as Galbraith underscores, rather than questioning *where* the rate of return to capital comes from in the first place, Piketty largely accepts prevailing neoclassical wisdom – inadvertently contributing to the neoclassical belief that income distribution largely reflects economic contribution.

Although Piketty does often state that he thinks that marginal productivity theories of income distribution are crude (see, for example, pages 304 to 306 of *Capital*), he never explores their history. Crucially, Piketty ignores the fact that ever since Clark put forward his idea, marginal productivity theories of income distribution have been successfully refuted time and again by successive generations of thinkers – most successfully by a group of Cambridge-based scholars writing in the 1940s and 1950s, spearheaded by the economist Joan Robinson.

Robinson's criticism of marginal productivity theories of income remains highly relevant today. Writing in 1953, Robinson suggested that 'The dominance in neo-classical economic teaching of the concept of a production function… has been a powerful tool of miseducation' (Robinson 1953-54: 81). She saw marginal productivity theories of income distribution as a tautological illusion, with the end financial pay-out received by capital or labour used to defend the idea that the pay-out represented a 'natural' measure of one's economic contribution. In a way, her criticism is not dissimilar to Stigler or Knight, although Robinson wrote from a leftist perspective, and Stigler and Knight did not.

In *Capital*, Piketty briefly acknowledges Joan Robinson and her colleagues' challenge to the neoclassical school's definition of capital. He also occasionally points out throughout his book that neoclassical theories of income distribution are crude and limited; he allows, for example, for the strong role of luck in determining wealth distributions, an observation that echoes the earlier concerns voiced by Frank Knight.

But despite offering occasional nods to the limits of marginal theories of income distribution, Piketty's definition of capital ultimately strengthens the hand of the economic theorists who he aims to refute.

The limitations of Piketty's analysis are clear from a short passage in *Capital* where his mistakenly interprets the importance of Robinson's criticism of marginal theorists. Piketty writes that 'economists working in Cambridge, England…saw in [Robert] Solow's model a claim that growth is always perfectly balanced, thus negating the importance Keynes had attributed to short-term fluctuations. It was not until the 1970s that Solow's so-called neoclassical growth model definitively carried the day' (Piketty 2014: 231).

Galbraith objects to Piketty's take on Robinson and her colleagues for two important reasons. First, Piketty gets their main objections wrong. Robinson and her colleagues' primary concern was not Solow's treatment of Keynes's writing on short-term fluctuations. Their *primary* argument centred on scepticism with neoclassical conceptions of capital and how it is measured and distributed. Second, Piketty gets the *outcome* of battle wrong. As Galbraith underscores: "Solow's model did *not* carry the day. In 1966, Samuelson conceded the Cambridge argument!" (Galbraith 2014; see also Syll 2014).

Why does this matter?

Because by dismissing the importance of Robinson's challenge to the marginal theorists, Piketty manages to entrench a false picture of both the stakes of the challenge and the end result. In doing so, he unintentionally *undermines* the very sort of policy mechanisms he explicitly calls for. Piketty proposes more taxation on the world's highest earners, but he does not consider the possibility that their wealth was ill-earned in the first place. In other words, he implicitly sides with conservative economists whose views on inequality he ostensibly aims to contest.

The best example of a conservative economist who Piketty inadvertently aligns himself with is Mankiw, a professor of macroeconomics at Harvard who drew headlines a few years ago when a group of Harvard students staged a walkout of his undergraduate economic modules; their aim was to express their solidarity with the Occupy movement and to criticise what they saw as Mankiw's conservative bias in his economics teaching. I suggest that this bias is visible in two main areas: Mankiw's macroeconomics textbooks and his scholarly writing in defence of wealth inequality.

A survey of recent editions of Mankiw's popular, intermediate-level textbook, *Macroeconomics*, underscores the aptness of Moseley's observations of undergraduate economics teaching. Writing in 2012, Moseley points out that Mankiw's bestselling textbook 'presents marginal productivity theory as if there were no logical problems whatsoever.'

It is an accurate observation. Mankiw's 8^{th} edition, published in 2013, does not mention Clark by name. But Mankiw adopts the precepts underpinning Clark's idea, and even uses language that directly echoes Clark's wording in *The Distribution of Wealth.* 'If all firms in the economy are competitive and profit maximizing,' Mankiw writes, 'then each factor of production is paid its marginal contribution to the production process' (2013: 55). He describes this approach as the *neoclassical theory of distribution* (italics are Mankiw's), and states that this theory is 'accepted by most economists today as the best place to start in understanding how the economy's income is distributed from firms to households' (2013: 49).

Mankiw's most recent 9th edition of his textbook, published in 2016, includes the exact same definition and wording as in the prior paragraph. But notably, directly following his definition of the neoclassical theory of distribution, Mankiw tacks on a discussion that is absent from his 8th edition – a section titled 'the growing gap between rich and poor.' In this section, Mankiw briefly addresses growing debates over widening income inequality, before concluding that growing income gaps are attributable predominantly to differences in education attainment. He adds that 'some policymakers advocate a more redistributive system of taxes and transfers, to take from those higher on the economic ladder and give to those on the lower rungs. Such an approach treats the symptoms but not the underlying causes of rising inequality' (2016: 65).

Mankiw's firm faith in the accepted and uncontroversial status of what he calls the 'neoclassical theory of distribution' is not limited to textbooks: it pervades his highly cited peer-reviewed work. In a recent article, 'Defending the one percent', Mankiw invokes marginal theories of income distribution in order to defend growing wealth divides. Referring to such theories as the 'economist's standard framework,' Mankiw argues that in a 'standard competitive labor market, *a person's earnings equal the value of his or her marginal productivity*' (Mankiw 2013: 30, emphasis added).

Mankiw *does* acknowledge that there may be occasional instances where, in his words, the 'real world might deviate from this classical benchmark. If, for example, a person's high income results from political rent-seeking rather than producing a valuable product, the outcome is likely to be both inefficient and widely viewed as inequitable.' But he then goes on to insist that in nations such as the US, examples of political rent-seeking are rare. 'My own reading of the evidence is that most of the very wealthy got that way by making substantial economic contributions, not by gaming the system or taking advantage of some market failure or the political process' (Mankiw 2013: 30).

Mankiw argues, in other words, that the 'one percent' earned their fortunes through the sheer dint of their hard work and economic contributions rather than from rent, something that he insists is likely to be rare. What Mankiw does *not* acknowledge is that the mainstream wording of this 'standard framework' can be traced to an economist, Clark, who insisted himself that his so-called 'law' only held under perfect conditions which rarely if ever appeared. Despite Clark's own caveats, his notion is now ingrained in neoclassical macroeconomics scholarship: taught and retaught to students as if it was an uncontested maxim.

Clark took a major problem of his day – the question of why industrialists such as Carnegie were getting richer and richer while the wages of his labourers often stayed stagnant or rose only incrementally – and he invented an economic 'law' that was remarkably favourable to the wealthy: the idea that capitalists receive an economic pay-out that is proportionate to the economic value which they generate. It's a powerful argument, one that's understandably appealing to the rich. But as Robinson, Samuelson and Stigler each insisted: it's also naïve and tautological.

When Mankiw argues that according to the 'economist's standard framework,' it is accepted that 'a person's earnings equal the value of his or her marginal productivity,' he is appealing to the power of a refuted economic theory. He also raises an open-ended question which he does not have the data to answer – just *how* small or large a role does rent-seeking actually play in wealth divides?

Rent-seeking is defined as the effort to procure economic rent (excess income paid to a factor of production) through favourably manipulative a legislative or a political environment rather than contributing directly to the production of economic value. Leading economists such as Robert Solow have recently suggested that political rent-seeking may be playing a growing and detrimental role in income allocation – a problem that Solow acknowledges is difficult to measure precisely because, in his words, there is 'no direct measurement of rent in this sense' (Solow 2015).

In the passage cited above, Mankiw does admit that the problem of rent-seeking can lead to unfair outcomes. He points out that in situations where a 'person's high income results from political rent-seeking rather than producing a valuable product' the outcome is typically seen as both 'inefficient' and 'inequitable.' But he hastens to add that such instances are rare.

Is he justified in doing so? It is accurate for Mankiw to suggest that most wealth today results from individual effort and value creation, rather than politically engineered rent extraction?

Unfortunately, that question is an unresolved and largely ignored one. Current economic tools and market-based estimates of wealth divides do not provide, as I show below, an answer to this question. This absence of appropriate tools of measurement makes it easy for someone like Mankiw to suggest that political rent-seeking is extremely rare. In reality, we do not know how rare or pervasive the problem of rent-seeking is in today's advanced economies. Few mainstream economists seem encouraged to measure a problem that orthodox theory suggests does not exist: a persistent source of ambiguity that is highly useful to thinkers such as Mankiw.

Divisive data in plain sight

How have conservative economists such as Mankiw managed to successfully withstand decades of persuasive challenges to marginal productivity theories? To address this question, my last section engages with debates over the quality and comprehensiveness of Piketty's data on wealth distributions. While valuable, much of this debate has centred on efforts to track and to measure wealth *after* it reaches the hands of private individuals or corporations. Piketty's data might, therefore, obscure understandings of inequality even as it purports to indict it.

Vociferous debates over data that *is* publicly available tends to legitimate the belief that what matters most in studying wealth inequality is the painstaking effort to chart the visible dispersion of wealth, rather than to explore something that remains even more difficult to measure: the question of how much of that wealth was legitimately earned or not.

Early political economists were preoccupied with that question. Mill, Smith and Ricardo each saw the problem of economic rents as central to understanding economic transformation (Khan 2000; see also Sayer 2014; Milonakis and Fine 2008). But the neoclassical turn, in stark contrast to classical preoccupations, treats all income as equally productive. As a result, current statistical sources do not themselves distinguish between something classical economists once differentiated as 'earned' versus 'unearned' income. Michael Hudson is one of the few leading US economists to stress this point. As he notes, 'any accounting format reflects the economic theory that defines its categories.' Given that the current neoclassical canon does not distinguish between earned and unearned income, neither do our statistical measures. Hudson goes on:

> Neither the National Income and Product Accounts (NIPA) nor the Internal Revenue Service's Statistics on Income in the United States define the specific form that the wealth buildup takes…A byproduct of this value-free view of wealth is that Piketty suggests an equally value-free remedy for inequality: a global estate tax with a progressive wealth and income tax. Not only is this almost impossible to enforce politically, but a general tax on wealth or income does not discriminate between what is earned "productively" and what is squeezed out by rent extraction or obtained by capital gains (Hudson 2014: 123).

Hudson, like Galbraith, is making a considerably different point than most of Piketty's interlocutors. Whether they agree or disagree with Piketty's findings, many of his readers have emphasized a somewhat obvious but still important point: all data on wealth equality is affected by known difficulties in collecting reliable evidence. Nations differ in their historical attention to wealth divides; some countries have longer and more historically robust databases than others. Each type of methodology, from sample surveys, to national censuses, to tax returns, has its own weaknesses and strengths. The uncertain quality of wealth data raises interesting political implications in itself. Just as uncertainty over the effects of ecological change, industrial pollutants or genetically modified products can be politically useful in thwarting regulatory action (see author 2009), ongoing uncertainty over the reliability of wealth data helps to cosset elites against demands for change.

But such uncertainty also veils a more deeply rooted problem: the fact that spirited nineteenth-century debates over the legitimacy of wealth have been silenced as a result of empirical models which suggest that, in efficient markets, the problem of unearned rent does not exist at all.

Let me offer two examples. This is not an exhaustive list, but it helps to highlight the areas where most criticism of Piketty's data has focused on to date. One of the most well-publicized criticisms has come from Chris Giles of the *Financial Times,* who suggested that Piketty seemed to have invented some figures, as well as cherry-picking data sources that favour his argument. Giles also charged Piketty with underestimating the importance of data from the UK Office of National Statistics (ONS) Wealth and Assets survey, which Giles argued offers a more reliable and far less pessimistic picture of wealth divides in Britain than Piketty's data (Giles 2014).

Unfortunately for Giles, his suggestion that ONS data offers a clearer understanding of wealth patterns is widely seen as highly objectionable. The ONS Wealth and Asset Survey relies on household interviews. As sociologists have long pointed out, self-reported assessments of personal wealth tend to paint a self-serving picture of personal assets. As reported by *The Independent*, one of the UK's leading daily newspapers, Piketty has called Giles' suggestion 'ridiculous,' as ONS data is 'based on self-reported data and is very low quality.' Gabriel Zucman of the LSE makes the same point, also quoted in *The Independent*: "The FT seems to take that survey as gospel, and I think that's a mistake' (Armitage 2014).

The second main perceived limitation – discussed above – is Piketty's failure to include human capital in his wealth estimates.

These two criticisms alone have fuelled dozens of headline-grabbing debates over the disciplinary tensions underpinning Piketty's research. One recent survey carried out by the University of Chicago suggests that 81 percent of economists surveyed disagree with Piketty's concept of r > g (his belief that wealth inequality is marked by growing returns to capital in disproportionate degree to overall national growth). Statistics like this, in turn, convey an image of economics as a realm of spirited dissensus, one where, for better or worse, it is difficult for any one theory or school to remain dominant for long.

Debates over the merits of Piketty's wealth data are, in a way, resonant of Bourdieu's analysis of the Barthes-Picard Affair in France, a dispute that he draws on in order the demonstrate the ways that ostensible controversy can often solidify underlying disciplinary preoccupations rather than unsettle them. The Barthes-Picard affair was rooted in Barthes and Picard's differing views on the writing of Racine. Despite their differences, their shared belief in the importance of studying Racine at all helped to cement perceptions of Racine's importance, and the value of literary criticism as an intellectual field. Thus behind their ostensible dispute lay a particular complicity, 'the *consensus* in *dissensus* which forms the unity of the intellectual field' (Bourdieu 1966; Lane 2000: 73). In a similar manner, vociferous contestation over Piketty's available data belies an underlying consensus, one that has grown more entrenched since the turn of the last century. That consensus is the neoclassical *rejection* of earlier concerns over rentier profits in favour of a Pollyanna-ish assumption that all wealth is equally productive and equally deserved.

Interestingly, one of the few public figures to call attention to this problem is Bill Gates. In a recent review on Piketty's book, Gates makes an argument similar to Hudson and Galbraith. He argues, rightly, that Piketty 'doesn't adequately differentiate among different kinds of capital with different social utility.' Gates then goes on to suggest, in an interesting reversal of Galbraith and Piketty's conclusions, that the problem of *rentier* exploitation condemned by nineteenth-century economists simply does not exist today:

> I fully agree that we don't want to live in an aristocratic society in which already-wealthy families get richer simply by sitting on their laurels and collecting what Piketty calls "rentier income" – that is, the returns people earn when they let others use their money, land, or other property. But I don't think America is anything close to that. Take a look at the Forbes 400 list of the wealthiest Americans. About half the people on the list are entrepreneurs whose companies did very well (thanks to hard work as well as a lot of luck)…I don't see anyone on the list whose ancestors bought

> a great parcel of land in 1780 and have been accumulating family wealth by collecting rents ever since.[i]

There is a flaw in Gates's argument. Economic rents are not defined as Gates defines them: as 'returns people earn when they let others use their money, land or other property.' Rents are defined as an excess of payment to a factor of production in excess of the cost needed to bring the factor into production. Such payments can accrue from the economic positioning of an asset, such as geographical land placement, that increases irrespective of any expenditure by an owner, as well as payments created through legal privileges, such as intellectual property rights. It is payment earned whenever a government or other party grants exclusive use of a factor of production through an artificial advantage such as a patent.

Gates gets part of the problem right – he correctly points out that rents are rooted in the problem of excessive profits created by legal or governmental privileges. And he's also correct to note that the lending of money or property *can* be a source of economic rent. But he gets the directionality wrong. Rent does not necessarily stem from the wealthy "letting" the less wealthy have access to their property. They come as well from the state permitting individuals to have exclusive property protections over commodities such as software, pharmaceutical drugs and land. Gates may be right that we have relatively few people on the Fortune 400 list whose wealth can be traced to eighteenth-century landholdings. But surely he does not need to look too far to find individuals whose fortunes are rooted in intellectual property protections.

What actually distinguishes today's tech billionaires from eighteenth or nineteenth-century land owners? The answer may be more uncomfortable for Gates than he admits.

In the nineteenth century, patents were seen as a worrying source of rentier income. They were condemned as an infringement of free trade. Today, while such views are still audible, they tend to linger outside the economics mainstream. As Andrew Sayer has pointed out, concepts such as 'unearned income' 'functionless investors' and 'improperty' – highly politicized terms that were far more commonly used in earlier decades – 'have fallen out of use over the last 40 years – just at the time they were becoming more relevant.'[ii] Although Piketty mentions the concept of 'rentier' throughout his book, his treatment of capital doesn't offer a means to disaggregate rentier income.

Conclusion: blame the absent data

I have drawn on a specific historical example – Clark's development of a theory of the marginal productivity of income distribution – in order to explore the historical roots of cultural maxims that are pervasive in western societies: incomes reflect talent; top managers deserve top pay; hard work pays off.

I have discussed a peculiar triumph of Clark's theory – the fact that it thrives today despite being castigated for decades by mainstream and heterodox scholars for its naivety.

By shedding light on the curious longevity of Clark's disputed theory, this article contributes to a perennial question raised by economic sociologists: why do economic theories that have long been dismissed by leading economists on both sides of the political spectrum continue to be taught and retaught as if they are universally true? Or to quote the economists Yanis Varoufakis and Christian Arnsperger, "how does mainstream economics get away with it?" (2009: 15; see also Gross and McGoey 2015).

One common response to that question is to accuse mainstream economists of simply ignoring countervailing evidence. This response is plausible. But it is also insufficient. To suggest that a dominant group simply ignores inconvenient evidence is to underestimate the effort, expertise and material resources needed to sustain the ongoing avoidance of uncomfortable truths.

In the case I've detailed above, one of the most useful resources for individuals such as Mankiw who insist that rent-seeking is 'rare' is the absence of a strong theoretical position to prove them wrong. The dearth of data – fomented by the lack of a persuasive theory of value – is a source of authority. Piketty's thesis is focused on 'open' data: evidence that lives in the databases of national treasuries or the offices of national statistics. His Herculean effort was to bring these databases together – and it is a major accomplishment. But, as Michael Hudson argues, it is a 'value-free' (and thereby value-laden from an STS perspective) catalogue of data: one that does not distinguish, and is incapable of distinguishing, between wealth is that is earned productively – through making economic contributions – and wealth that is extracted illegitimately through rent-seeking.

The data that Piketty would need to study this important problem is not collected in a way that disaggregates 'unproductive' from 'productive' wealth. It is not just inaccessible: it was never there. One of the reasons *why* it doesn't exist empirically is because generations of neoclassical economists did not see the need to generate it. And one reason for *their* lack of attention is because just over a century ago, a man named John Bates Clark came up with a 'law' asserting that, under ideal market conditions, illegitimate rent extraction was an impossibility. And powerful people wanted to believe that he was right.

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[i] Available at Gatesnotes, The Blog of Bill Gates: http://www.gatesnotes.com/Books/Why-Inequality-Matters-Capital-in-21st-Century-Review

[ii] Andrew Sayer, ‘We need to challenge the myth that the rich are specially-talented wealth creators’ January 27, 2015. http://blogs.lse.ac.uk/politicsandpolicy/we-need-to-challenge-the-myth-that-the-rich-are-specially-talented-wealth-creators/