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Occupations and Inequalities in the 21st Century: What’s in your Wallet?☆

Kevin T. Leicht

Department of Sociology, University of Illinois Urbana-Champaign, 702 South Wright Street, Urbana, Illinois, 61801, USA

1. Introduction

In 1893, Emile Durkheim, one of the founders of sociology, attempted to grapple with how social organization was going to be produced in the modern world. That “modern world” was viewed by many as urbanized, anonymous, individualized, anachronistic, and anomistic. One of the potential sources of social organization Durkheim discussed was the occupational community, groups of likeminded people who performed similar tasks who would develop common interests, outlooks, and social norms. Like many intellectuals of his age, racial, ethnic, and gender discrimination in public spaces was taken for granted. What wasn’t taken for granted, and what animated the study of sociology in the first place, was whether there were sources of social organization beyond the family and village. Nobody knew. Nineteenth and early twentieth-century urban society was an experiment.

For much of that time (if not before) governments were very interested in how their citizens made a living (cf. Moskowitz 2017). This was viewed as a serious and important thing to know, so the systematic measurement of what would become occupations was born. For all of the 20th Century, occupational categories were refined and put to various uses (e.g., SOC/ISCO/ISCO-88/U.S. Census Occupational Categories and the U.S. Dictionary of Occupational Titles, now O’Net). The post-World War II west especially evolved toward a system that placed confidence in our ability to determine peoples’ aptitudes and then match them to occupational tasks and activities that would best use their skills and abilities.

Occupational measurement almost naturally spawned research into occupational attainment and mobility (for example, Blau and Duncan, 1967; Duncan et al., 1972; Featherman and Hauser 1978; Goldthorpe and Hope, 1974). Like most research in social inequality, research on occupational mobility had a strong normative component – it was one thing for people to attain a relatively prestigious occupation thru their own efforts, but persistent help from prior generations was viewed as a violation of meritocracy norms or, at minimum, something that needed to be explained.

Intragenerational mobility also became a major research area with its own normative implications. The well-placed worker was “not stuck” – their work career could (or should) be described as a series of orderly

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E-mail address: kleicht@illinois.edu.

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moves that could be charted by looking at changes in jobs and occupations. These moves should not be idiosyncratic but systematic. One should be able to predict with some (but not complete) certainty where somebody was likely to end up at some time in the future given their starting point (see, for example, Spilerman 1977; Spener et al., 1982). Occupational placement also had a strong cognitive/social psychological component (“when I grow up, I want to be a doctor/lawyer/soldier/sailor...” etc.) and an interpersonal, cross-generational component too— we talked with our children about different “walks of life” in occupational terms. The belief in an ordered career trajectory was important— what good was it to aspire to do a certain kind of work (defined by occupations) if it was here today, gone tomorrow, and one could not predict where it would ultimately lead economically and socially?

So several generations of social scientists have looked to occupation-based research to provide insights into how social inequality works. But this view is currently under scrutiny because of many of the almost seismic upheavals that have afflicted many developed economies over the past 30 years. Granted, the good of occupation is still here, like a 1960s VW Beetle— reliable, honest, a bit simple but, when you turn the key, it starts! Is it time to “trade in” the old reliable occupational analysis for a new model or should we send it to the shop for some refurbishing? The authors in this issue have different and very insightful answers to that question and in the process interrogate what occupational analysis can tell us now and in the future.

Before I assess the contributions themselves we should take a look back at why occupational analysis was so popular and what it has taught us. Then I will address just a few of the changes that have led some to question the primacy of occupations as a means of studying social inequalities. Then I will turn to our very insightful contributions and see what they can tell us about the current state of occupational analysis and provide some ideas about how to move forward from here.

2. Occupations and Inequalities: Appreciating What We Learned and the Context Where We Learned it

There are several things that occupation-based social inequality research taught us that are still at least a starting point for stratification and inequality research now. These insights are far from trivial but they are often overlooked because they have become (more or less) a staple or “cannon” of inequalities research.

We discovered that people really can rank occupations, not necessarily in ordinal groups but in matched pairs (Prestige Scores). We discovered that educational attainment and earnings predicted the relative rankings of occupations created by these matched pairs (Duncan SEI scores). And we discovered that the relative ranking of these occupations was culturally invariant. A physician really was physician really was a physician (Treiman, 1977; Hout and DiPrete, 2006).

But that’s not all we discovered. I think, especially in the Post-World-War II United States where belief in individualism and meritocracy reigned supreme, we have forgotten how revolutionary the original occupation-based social inequality research taught us. It’s very hard for hardened, cynical contemporary observers to realize that telling 1960s American policymakers that a substantial percentage of someone’s SES was inherited from one’s parents was tantamount to burning the American flag (!). Americans don’t DO status inheritance. We fought a revolutionary war and a civil war in part over the injustice of inherited social class and position. You’re telling us we’re doing it anyway?! How does that work?!

Occupation-based research also took maximum advantage of what governments wanted to measure. In this sense, government interests and expanding data collection resources aligned with a growing cadre of researchers who could use the data to answer important questions. Governments (especially after the traumas of World War II and the Great Depression) had an overriding interest in finding their citizens meaningful and remunerative work. Social scientists could study meaningful and remunerative work, which (it was assumed looking to the limitless future) everyone had some right to expect (Goldthorpe et al., 1968).

Similarly, it wasn’t at all clear that the measurement of anything else was really worth doing. There were many predictions that state-socialist and capitalist forms of economic organization would converge (see for example Schumpeter 1942; Galbraith 1967; O’Connor 1972). This would happen because firms in the capitalist world would become really large, employ hundreds of thousands of people, engage in relatively little competitive behavior, and work in concert with the state to foster the general welfare (Davies, 2016). Small firms would simply be swallowed up or not be created in the first place. General Motors was the wave of the future. The local mom-and-pop grocery store was not. The idea that an individual in a garage could invent a gadget that would earn hundreds of millions of dollars in just a few years (or less!) was viewed as impossible and possibly fraudulent. The idea that college students would do this would have produced laughter. Old people ran corporations. Young people worked for them. When young people became old people, then they would run corporations. Since everyone was going to work in a firm with 100,000 employees that made billions of dollars anyway (much of it passed on in higher wages and benefits), and those firms were all going to be the same, why study them?

In a booming, post-war economy in the United States, UK, much of Western Europe, and parts of Asia, figuring out who progressed and who was left behind occupation-wise was a big research and policy priority. The horizons seemed limitless. Unemployment and inequality were relatively low. Incomes were rising across the board. The ability to incorporate underrepresented people, integrate them into the labor market and then move them into the middle class (in the U.S. the “American Dream”) seemed within reach and any impediments to that seemed harsh, unjust, arbitrary, temporary, and easily addressed by a few tweaks in government policy.

In this environment, occupations represented bundles of activities and life chances, much as Durkheim envisioned them. Everyone would have a job and that job would pay well. The job would exist in a large bureaucracy subject to little direct economic competition and one bureaucracy was more-or-less exchangeable for another (for less sanguine views of this world, see Mills, 1951, 1956; Whyte 1956; and Jackall, 1988). The real question was whether occupations used people’s skills and abilities to their full extent. This was the variable component in the whole scenario.

3. Then the Context Changed: Recession, Inflation, New Ideologies and the Wholesale Disinvestment in Work

But then this sunny story of the full-employment Great Society began to unravel, most seriously in the English-speaking nations;

1) The manufacturing-based economy collapsed and the post-industrial society that followed seemed to systematically disinvest in some occupational skills and reinvest in others at a rate that was impossible for governments or labor market institutions to keep up with, let alone individual workers;

2) Mass layoffs, unemployment, and stagflation signaled that the attempt to find the right match between skills, aptitudes, and abundant opportunities was degenerating into a search for any job at all. We discovered (see Clogg 1979) that one of the major stratifiers wasn’t having a good or bad job, it was having any job at all that paid for 40 hours a week worth of work, around 50 weeks a year, paid above a poverty-level wage, and lasted a reasonable length of time. Only somewhere between 50-60 percent of labor market participants, even in highly developed economies, met this standard (Clogg, Eliasion, and Wahl, 1990). Well-ordered careers that fully used people’s skills and abilities seemed like a distant dream of the privileged few;

3) The retail attack on workers’ rights and organizations (especially in the English-speaking countries) left workers unable to change their
working conditions or “grow” their jobs. The lack of any presence of organized labor in some labor markets became so taken for granted that entire generations of workers assumed that employers would treat them badly, they would “never be able to retire...”, and that the workplace loyalties of older generations were misplaced (Haughey 1993);

4) Entire segments of the political elite disinvested in the world of work entirely and didn’t see it (or the people who occupied it) as central to their prosperity. Instead, we were to replace workers with robots or people from the endless global stream of desperate humanity that would work for pennies on the dollar. This change was fed by one of the principal “post-truth” myths of the late 20th century – “supply-side economics” – where investment and wealth for the few would translate into better jobs with better pay for the many. A second, half-truth/half-myth soon followed – workers would be better off under globalization because it would bring them cheaper consumer items and they could take consolation that the rising GDP that followed from “flexible specialization” would make them better off (cf. Hirst and Zeitlin, 1991; Stiglitz 2019). Almost nobody (and certainly few political elites) seemed to remember that even the most robust supporters of globalization advocated that victims (i.e. people who lost jobs and incomes due to outsourcing and automation) should receive compensation for their losses. In many parts of the world this compensation never arrived (Stiglitz, 2019; Roberts 2013).

Worse still, workers were viewed as suckers and the occupations and jobs they occupied simply things to be destroyed or changed in order to make stock prices jump in the rush to promote “shareholder value” (Lazonick 2011). Business school graduates were touting “creative disruption”, which usually translated into disruption for others and riches for themselves (Gobble 2015, some of the original advocates of these positions have since changed their minds, see Roberts 2013). More ominously in the U.S., the American corporation was shrinking and it was possible in a deregulated, globalized financial environment to make billions of dollars and employ almost no one (see Davies, 2016).

In summary, it was becoming difficult to see how jobs or occupations could be the basis for an analysis of social inequality if most members of the managerial class were trying to systematically eliminate everyone else’s job and occupation;

5) Jobs and occupations lost their position as the drivers of economic growth and prosperity through the stimulation of aggregate demand. Their place was taken by deregulated finance and consumer debt. One’s economic status seemed to derive from what one was able to consume, and what one could consume was increasingly divorced from any tie with work, earnings, or income (see Manning 2000; Crouch 2009; Porter 2012; Leicht and Fitzgerald 2014; Dwyer et al., 2011).

6) The ability to obtain services from a variety of vendors and firms around the world seemed to create pockets of “winner-take-all” labor markets, markets that rewarded a small subset of practitioners handsomely while leaving many in the same occupations with marginal employment prospects or none at all (see Frank and Cook, 1995). This change was happening within occupations.

7) (Correspondingly) firms seemed to benefit enormously from “first mover advantages” that created barriers to entry and competition, leaving many markets with one or a few providers of goods and services to enormous numbers of consumers, in many cases encompassing the entire planet (think Amazon, Microsoft and Facebook). Contrary to our hopes and predictions, these first-mover companies did not employ everybody. In fact, the largest companies listed on the U.S New York Stock Exchange and registered with the U.S. SEC drastically shrunk in employment size – the combined employment of Facebook, Twitter, Zynga, Zillow, LinkedIn, Uber and Google is around 80,000 – fewer employees than General Motors added to their payrolls in 1942 (Davies, 2016). Government anti-trust officials either paid no attention at all or had no means to fight back.

8) Income and wealth inequality increased alarmingly (Piketty 2017) - much of it within occupations, genders, and ethnicities. Social mobility seemed to stall as well (Chetty et al. 2015). Those temporary, 1960’s differences and injustices we thought were fixable seemed to be permanent fences around gated country clubs of privilege in 21st-century post-industrial society.

All of these changes have profoundly affected the macroeconomies of most of the developed and developing world. Yet the good ‘ol occupation (like that VW Beetle) is still with us and, with it, the measurement and analyses that have produced much of the bedrock knowledge of social inequalities that sociology claims to know. Our authors vary in where they think occupations can take us in the future in light of some of the changes outlined above. Let’s turn now to our stylized domestic dispute over the future of the occupational analysis.

4. The Old Reliable Still Starts: The “Fix and Refurbish” Group

On the “fix and refurbish” side of our domestic dispute are Yaish and Kraus (“On class and earnings mobility: should we try harder to collect intergenerational earnings data to study intergenerational mobility?”), Bjorklund/Jantti (“Intergenerational mobility, intergenerational effects, sibling correlations, and equality of opportunity: a comparison of four approaches”), and Houseworth/Fisher (“Measurement error in occupation and its effect on mobility tables”).

The closest thing to a defense of the status quo comes from Yaish and Kraus (“On class and earnings mobility: should we try harder to collect intergenerational earnings data to study intergenerational mobility?”). They point to the commonalities in the concepts of permanent income as measured by economists and sociologists. While economists focus on permanent income, they rarely have more than a few years of income measures that they either average or sum. While this helps to overcome temporary fluctuations in year-to-year incomes, Yaish and Kraus question whether these measures get much closer to permanent income than the original, if flawed, focus on cross-sectional differences in a single year. With data from Israel, Yaish and Kraus link individual income data over a substantial span of workers’ careers and compare those results with data on occupational classes, coded in the standard EGP fashion at a single point in time. They discover that the long-term income trajectories of Israeli workers mirror rather directly their social class positions. They conclude that the measurement of occupational classes is easier and more workable than the measurement of yearly incomes over long periods of time and that the measurement of social classes in this context is a useful proxy for permanent income.

Houseworth and Fisher (“Measurement error in occupation and its effect on mobility tables”) examine a problem that has received very little attention in the literature and that is important if the use of occupations in inequalities research is to continue – what to do about measurement error in the reporting and coding of occupations. Based on prior literature, three-digit occupation codes are misreported somewhere between 42 and 58 percent of the time. They also show that proxy reporting produces measurement error in the reporting of occupation (for example, cases where children report the occupation of their parents or vice-versa) but these errors are relatively small compared to the misreporting errors on which occupational coding is based (for more on errors in occupation coding, see Kim, Kim and Ban in this issue). Their recommendation, which is consistent with the “fix and refurbish” perspective, is to use fewer occupational classes and fewer proxy reports for occupation when doing mobility analysis.

Bjorklund and Jantti (“Intergenerational mobility, intergenerational effects, sibling correlations, and equality of opportunity: a comparison of four approaches”) approach the intergenerational mobility relationship differently, though their basic thrust is consistent with prior research on occupational mobility – what is it that produces the link
between parents’ and children’s occupational attainment? They examine four different approaches, including an intergenerational mobility approach, a sibling correlation approach that assesses the portion of parental effects that is shared across siblings, the intergenerational effect literature that examines how changes in parental circumstances produce changes in child outcomes, and an equality-of-opportunity approach that seeks to isolate family background effects that children are not responsible for from subsequent abilities and achievements. The intergenerational persistence effects or correlations vary widely depending on the approach, suggesting there is no one easy answer to the question of how much social status is passed on and what mechanisms are responsible. Of the three contributions summarized so far, this one goes the furthest in suggesting that the occupational mobility approach needs quite a bit of ‘fixing and refurbishing’ to answer central questions about intergenerational mobility and may need to be jettisoned. The ‘fix and refurbish’ faction in the family has made their case. It is now time to listen to the arguments of the others in this domestic dispute.

5. “Aw Dad…Mom…Occupations are for Old People and Nerds!”: The “Trade it in” Group

This brings us to the second faction in our domestic dispute. Among this group the old reliable Beetle’s time has come and gone and the junkyard awaits. On the less radical side of the ledger lies Kim, Kim and Ban (“Do you know what you do for a living? Occupational coding mismatches between coders in the Korean General Social Survey”). They take advantage of an opportunity to re-code occupations as they are reported in the 2010 Korean General Social Survey. Like Houseworth and Fisher, they find considerable misreporting and coding error in the coding of occupation with a 31 percent error rate at the 1-digit level and a 51 percent error rate at the 4-digit level using two independent coders. These differences, especially at the 1-digit level, suggest that occupation is not as reliably measured as many analysts have thought. The mismatch rates for EGP classes also are unsettling for sure. The coding errors are asymmetric and heteroskedastic, especially when one hits categories of managers, and income is negatively associated with the match rate suggesting that there is quite a bit of income dispersion in occupations that have higher unreliability in coding. Their quote is rather telling, ‘This result implies that the notion that occupation is easily distinguishable and a better proxy of permanent earnings than income is a bias of professionals who study occupations...’ Kim and colleagues go on to suggest that gradational treatments of occupations (concepts like SES) are more fruitful and are prone to less measurement error. To be fair, this contribution rightfully could be classified under the “I’d like to fix it, but I’m not sure it’s fixable” label.

The remaining contributions seriously question the contemporary value of occupations as a viable basis for inequalities research. Going from least to most radical, Bloome and Furry (“Lifetime inequality: income and occupational differences and dynamics in the United States”) examine lifetime income intragenerational mobility using the Panel Study of Income Dynamics. They use a life-course perspective of income inequality that suggests that sources of inequality equalize somewhat over the long term as the influence of social origins wanes and incomes plateau with age. They compare actual lifetime earnings mobility with estimates using occupational prestige and occupational income and the findings are a bit sobering for sociologists. Occupational prestige and occupational income measures overestimate the persistence of social origins in intergenerational income inequality as lifetime income dynamics equalize family incomes in ways that occupational income and occupational prestige don’t measure. The differences remained stable across cohorts because rising income inequality is not offset by increases in income mobility or rising occupational inequality. If Bloome and Furry had stuck with occupation-based inequality measures they would have missed a big portion of the intergenerational inequality dynamic entirely by ignoring changes in lifetime incomes (see also the Sakamoto and Wang discussion below).

Then we get to the two Avent-Holt contributions and the Janietz/Bol contribution. These authors argue for a different approach to inequalities that jettisons traditional occupational analysis for analyses that focus on jobs and firms. They analyze different contexts to be sure, but the results fit together nicely. Focusing on occupations alone will not get you to where the action really is, which is in the interface between people and their employers, jobs and firms. Janietz and Bol (“Occupations, organizations, and the structure of wage inequality in the Netherlands”) find that firms are more important in explaining earnings inequality between occupations than inequalities in the same occupation. This result happens through a dual-sorting phenomenon where high-paying occupations end up clustering in the highest paying firms, leaving relatively low-paying occupations in lower paying firms. They recommend moving away from a focus only on occupations to a focus on firms and occupations within firms as the way forward to explain contemporary labor market inequalities.

The pieces by Avent-Holt and colleagues are offspring of the very promising, multi-nationally funded COIN project (Comparative Organizational Inequalities Network). In the case of Sweden (“Occupational status and organizations: variation in occupational hierarchies across Swedish workplaces” Avent-Holt, Halsten, and Cort), the authors examine cases where occupational status within firms varies substantially from a national organizational hierarchy of the kind that Treiman (1977, see also DiPrete and Hout 2006) discuss. The authors find considerable firm-level variation in occupational hierarchies and suggest that the market power of individual level firms contributes substantially to these variations. They also find that the gender composition of occupations has counter intuitive effects – Swedish occupations with more men in them seem to move down the status hierarchy relative to their national position based on a national occupational hierarchy, suggesting a more complex organizational than one would suggest from a standard gender-devolution perspective. More importantly they call for more research and theorizing on the characteristics of firms that could explain these deviations. In the second piece (“Occupations, workplaces or jobs? An exploration of stratification contexts using administrative data” Avent-Holt, Henriksen, Häggland, Jung, Kodama, Melzer, Mun, Rainey, and Tomaskovic-Devey) the authors’ team uses administrative data from five countries (Denmark, Finland, Germany, Japan, and South Korea) to examine the relative role that firms, occupations and jobs play in explaining wages. The results show that there is wide variation in the ability of occupations to explain inequality with variance explained anywhere from 31 to 56 percent by country-year. Establishments in all but one case (Finland, more on that below) explain more variation than occupations do. These results suggest that the emphasis on occupations is somewhat misplaced and a renewed focus on jobs and work organizations is needed to fully explain inequalities (see also Tomaskovic-Devey and Avent-Holt, 2019; Tomaskovic-Devey et al. 2020).

This leaves us with Sakamoto and Wang (“The declining significance of occupation in research on intergenerational mobility”) the contribution that opens the issue but also the contribution that most stridently wishes to trade off the old reliable occupational analysis for a newer and sexier ride. In their sweeping analysis, they argue for the declining significance of occupations as sorters and producers of social inequality and they advocate for a move toward the economists’ focus on income mobility rather than reliance on occupational mobility tables. The most telling critique they level at studies using occupations is the claim that such research was “asleep at the switch” when actual inequalities started to rise in the 1980s and 1990s and that occupation-level research completely missed the Great U-turn and Gatsby Curves that affected many developed countries. They also claim that occupations have a great deal of heterogeneity within them (much as Kim, Kim, and Ban suggest), that occupations don’t even accurately tap non-monetary rewards or compensating differentials, that cross-sectional measures of
occupation don’t get at over-time and increasing income volatility (contrary to Yaish and Kraus), and that the separation of circulation from structural mobility was more apparent than real. By their reasoning, occupation-level research cannot be redeemed and must be replaced with more direct analyses of income and earnings inequality, fringe benefits, and longitudinal income volatility.

So where does this leave us? We have analyses of several different national contexts with different histories and different labor market institutions (U.S., Japan, Korea, Germany, Finland, Denmark, Israel, and Sweden). If I were to array these pieces on a continuum, they would look like Fig. 1.

6. Occupation-based Inequalities Research: Some Uncomfortable Questions

Before we get to our take-home message, some other more uncomfortable questions need to be addressed. These are directed more at researchers than government officials or policymakers, but they are still important.

First, did we initially study occupations because they were most important or did we study them because they were measured and available? Most social stratification research using occupations originated in the U.S. and U.K. In those countries, organizational measurement of any systematic sort was lacking completely – there were no national registries of firms, no widely-available Department of Commerce data on organizations and employment, and no central registry of organizational existence that told us anything about the characteristics of firms/organizations beyond (perhaps) an industry code. In short, even if firms and organizations were completely driving social inequality in the past, we were in no position to systematically study them.

Occupational measurement was well established, had a long tradition, and had considerable government infrastructure to back it up. Better still, you could count on the measurement to be consistent even if a bit flawed. This gave occupational-level research a considerable advantage and much to recommend it.

So we studied occupations because they were measured. The practical answer is we had no other alternative even if we wanted to go in other directions. One could attach nefarious political motives for this – firms are controlled by influential individuals and those individuals don’t want government agencies messing with their business – but one could simply say that the U.S./U.K. governments lacked the will and capacity to collect the types of data that would have allowed for more systematic analyses of firms and organizations as producers of social inequality.

Of course, just because something has always been measured doesn’t mean it is as important now as it has always been. We know how many horses need shoes, and our measurement of that is probably just as good as it has always been, but our economy doesn’t depend on it anymore. Yet we are still disturbingly dependent on what governments decide to measure and what is important to them, which may not be the dimensions of social inequality that are important or salient to analysts.1

Kevin Phillips, in his then famous book The Politics of Rich and Poor (1990) predicted that, in an era of rising economic inequalities, it would be easier to find government statistics on the ingredients in the typical frozen pizza than it would be to find government statistics on income inequality and, with regard to firm-level inequalities, he was not completely wrong.

Second, occupations sent social stratification research (perhaps inadvertently) in the direction of studying rankings. But income inequality is about gaps. The gaps have widened. The rankings have not. Our theories are tied to rankings. Occupations are ordinal measurement in a ratio world. Converting occupations into prestige and SES scores does not really alter that. In fact, it might make it worse because it gives the illusion of ratio measurement when, in fact, one has interval or ordinal measurement at best.

The net result is that many of our theories developed around ranks vs. incomes. The ranks were standardized to range from 1 – 100. The income distribution was under no such constraint. A Status point difference of 30 points might be worth $10,000 real dollars in 1970, $50,000 real dollars in 1990, and $100,000 real dollars in 2015 but it would still be 30 status points. The occupations involved might have earnings distributions that cross at the bottom of one and the top of the other, but they’re still separated by 30 status points. One form of measurement was picking up on these changes. The other was not.

While this was clearly not the intention, the real or implied focus on rankings has left us unable to explain much of what has been going on cross-nationally and within individual countries over the last 40 years. To use gender inequality as an example, we suspect that Mark makes more than Sally. We may know that Sally makes more than Rita. And we may know that Mark makes more than Rita. But we still have almost no idea why Mark makes $10,000 more than Sally and $30,000 more than Rita, and why Sally makes $20,000 more than Rita. That’s because our theories have developed around explaining relative rankings rather than the distance in gaps between locations and people. Recent research by the Pew Charitable Trust has begun investigating the relationship between these dimensions as a means of understanding absolute mobility across generations (Pew Charitable Trusts, 2015).

Third, while other researchers were sending out alarm bells that income and wealth inequality were rising at alarming rates and that we were headed for the “Gatsby Curve” and “Great U-Turn”, occupation based researchers kept claiming we were seeing trendless fluctuation. The United States and U.K. almost trendlessly fluctuated itself out of having a middle class. One wonders what the response would have been to the American Bankruptcy Survey, interviewing people standing in line at the county court house to file for bankruptcy, if the opening line from the research assistant was, “excuse me… we understand you’re a victim of trendless fluctuation. Could we interview you so we can understand how truly trendless your downward income mobility has been??”

In this context, occupations seemed like a flawed COVID-19 test that couldn’t pick up the virus or any of the antibodies. At minimum this result was a sign that we were looking in the wrong place.

Finally, occupation-based research accidentally fed into the not-inaccurate hallway commentary about (especially) American Sociology – our income and wealth inequality is the highest in the OECD and climbing, incomes for 80 percent of the population are flat or declining, marginality and labor market dysfunction afflicts a vast majority of the population so, of course, we don’t study any of those things! (Leicht, 2016 – an RC-28 participant made this observation to me at one of our receptions in Burno, Czech Republic). While occupational status could be viewed as an important marker of attainment in and of itself, and educational attainment could be viewed as important by itself, American sociology especially degenerated into a world of identities, bricolage, hybridity, intersectionality, microaggressions (question: isn’t widening social inequality a serious macroagression??!), sloppy postmodern

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1 I had a direct experience with this during the mid-2000s (2005). I had a grant from the U.S. National Science Foundation to study the relationship between local educational and economic contexts and successful transitions to adulthood. The running hypothesis was that there were parts of the country that were facilitating the creation of functioning, productive adults and other parts

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reasoning, and other absurdities that afflict the Mercedes/BMW set – anything but money. It was as if the captains of Wall Street dropped a nuclear weapon on somebody’s house and the victims claimed the local anti-war protesters with tattoos were responsible. Perhaps we really do need a social justice movement to diversify the demographics of hedge fund managers and day traders (and who wouldn’t want to join an occupation with a median starting income of $300,000??), but the net result is that several generations of American sociologists wouldn’t know what real economic inequality looked like if it hit them over the head with a cricket bat.

Granted, the prevailing historical focus on occupations is far from the only factor responsible for these trends and probably not the major factor. But the view that changing occupational attainment was a major (or only) key to producing a more just society is now seriously in question in a way that would not have seemed possible in the 1960s and 1970s.

So we studied occupations because the data were there. Our rankings of them led to theories of ranks. Ranks and incomes were highly correlated when we started doing this but they are less so now, and (as time passed and the context changed) the sociological focus on ranks and classes made it appear we didn’t care about money. There are worse sins. In spite of these problems our authors’ work here leads to some new questions that may keep the occupational analyses of inequalities vital and lively.

6.1. Occupations and Social Inequality: Questions that Still Need to Be Addressed

Occupational analysis of social inequalities has taught us a lot and there may still be a future for it with some refurbishing. Our analyses here point us to new issues and questions to be addressed. I outline just a few of these, most of which I would not have thought of without taking a comprehensive look of the outstanding contributions to this issue.

First, while occupations and occupational coding come under serious scrutiny here, job and firm-level analysis still depends on the core competency of assigning occupation codes. This is so because almost all analyses of jobs that make comparisons across firms or organizations are really analyses of occupations within those firms and organizations. The “occupation-within-firm” is, in effect, a “job.” This means that the problems with occupational coding highlighted by our authors do not magically disappear if we move our focus to jobs and firms. In order for the jobs to be comparable across firms, an “accounting manager” in firm A needs to be doing the same work as an “accounting manager” in firm B. Otherwise the comparison is compounded by the idiosyncratic job classifications of the firms themselves. These idiosyncrasies may be interesting, but if we move to a labor market level analyses of inequalities across firms and jobs (as Janietz/Bol, Avent-Holt and colleagues do) we will still need a robust occupational coding regime to do that. It is true one could abandon this for a more detailed analysis of the division-of-labor within firms as a major (and less studied) stratification dimension, but it would be nice if we could compare similarly situated jobs across firms as well.

It is here that I think that Artificial Intelligence/Machine Learning and Natural Language learning may help us. Training artificial intelligence programs to code occupations would seem to be an ideal use of these increased computing capabilities. Creating the “ground truth” that the programs would use would be laborious, and the ground truth would have to be updated often and the codes would have to be nested in ways to make them comparable, but one would think this is something AI could do. AI/Machine Learning may also spot trends in respondent language and suggest corrections to existing codes. As with everything else with AI, we would have to be alert to the biases and mislabeling we programmed into the algorithms, but an AI program could (in theory) also provide us with estimates of the confidence in the program’s assignments and a series of other possible codes for any given description (with confidence estimates around that). If we stick with occupations-within-firms = jobs coding, this might be one promising way forward.

Second, while most of the analyses by our authors are national (or in some cases cross-national) many labor markets for most occupational groups are local. Many of the inequalities that have led analysts to question the utility of occupational analysis (including the critiques mentioned above) play out as regional differences in economic vitality and economic opportunities. An engineer in Wichita may be left in the dust by her counterpart in Silicon Valley and still receive a quite comfortable income, plenty of social status and prestige, and be quite gainfully employed relative to others in the Wichita labor market. She increasingly suffers in comparison to her counterparts in larger cities on the coasts, but perhaps the entire national or global labor market for engineers with her skills is not the only comparison group that matters and it might not be the most important one.

The local labor market is far more salient for occupations that, in effect, have no national labor market and that is probably a majority of occupations we study. Is there really a national labor market for barbers, carpenters, restaurant cooks, janitors, county librarians, and local firefighters?? The overall stickiness and lack of geographic mobility many places would suggest there isn’t or, if there is, not enough people tap into to improve their job prospects.

So, if there are few truly national labor markets, why do we study occupational attainment and social inequality as if there were? Obviously the “nationalness” of labor markets varies from country to country, but our data collections have gotten to the point where we can study labor market areas and still have enough cases to make for a viable analysis. This might be one direction to take occupational inequalities research that would prove fruitful.

Third, only Yaish/Kraus and Bloome/Furey really directly compare the performance of an occupationally-based class scheme with lifetime income data. Their results don’t completely line up. In the Israeli case, occupational classes do provide a viable representation of income trajectories over life course. The real question is whether this result and analysis can be repeated elsewhere and what those results will show. In the Bloome/
Furey U.S. case, occupation-level measurement does not pick up important shifts in income over the life course. As data collection continues we need more direct comparisons of the relationship between occupations and lifetime income trajectories. It is totally possible that, in some places, occupational classes serve as a good proxy for lifetime income trajectories and in other places not so much. Governments are increasingly making data available on a restricted-use basis to address this issue and researchers should take full advantage of those opportunities to repeat analyses like these.

Fourth, I have yet to walk into a grocery store and spent a socioeconomic status point to buy food. Nor have I been confronted by a homeless person outside with the question, “brother, could you spare a status point?” (others will get their comeuppance in a minute, just wait!). In the world of the 1960s, one could perhaps argue that status, income, wealth, and prestige were interchangeable because the stakes were far lower. In the early 21st Century OECD, the income and wealth stakes are far too high, and I doubt very many people beyond a fairly well-off elite would trade some of their wealth and income for more occupational prestige. I’m virtually certain nobody from an underrepresented, impoverished group would do so.2

This would be true even if within-occupation inequality remained the same and between-occupation income/wealth inequality increased. Unless we ourselves are willing to trade income and wealth for something else, then we are partly acknowledging that these are more important and we should study them. I have complained in other quarters about this focus on anything but money (“Brother, can you spare an identity?”, Leicht 2016). It’s not a problem when income/wealth inequality are low but it is a big problem when it is high, especially if occupational measurement doesn’t change.

Similarly, I’ve never walked into a grocery store and spent a logged dollar. Nor has a homeless person approached to say, “God bless you sir! Could you spare a logged dollar?” This too is a problem and one of our authors addresses it directly (Bjorklund and Jantti). Randy Hodson (our recently deceased colleague and prior editor of the American Sociological Review) warned about the problem with logging earnings/income distributions some years ago (Hodson 1985), and Trond Petersen developed mechanisms for understanding different functional forms of earnings in inequality models (Petersen 1989). Logging earnings/income has both normative and methodological implications, but the biggest problem with it can be summarized simply:

1) income and wealth inequality have increased because of the growing distance between the top 10% and/or top 1% and the rest of us;
2) To eliminate this skewness, we “log” the distribution to pull the tails toward the middle;
3) But the fact that the tails have pulled away from the middle is the very phenomenon to be explained.

Logging a distribution of earnings and income in a context where income and earnings inequality is worsening is like saying to yourself, “...the rich are not like you and me, so we’ll make them less rich...” It’s like trying to understand Wall Street by lopping off hedge fund managers and day-trading by investment banks. It’s like studying police shootings and removing victims that were killed because we can’t interview them.

You get the idea. When analyzing monetary inequality, the skew is the story, not a methodological aberration. Only methods that deal with this skew in its full and unadulterated state are dealing with what is really going on. One promising development in this regard is the move toward quantile regression models (see, for example, Budig and Hodges, 2010; Lin 2015). Everything else turns the study of earnings and wealth into another analysis of rankings vs. gaps.

Fifth, Perhaps occupations mattered more at some time in the past, but matter less now. There are no analyses here that assess what occupations explained in the 1960s or 1970s compared to (say) the early 2000s. There are good reasons to think that occupations may have mattered more for inequalities right after World War II than they do now, but nobody has ever really addressed that issue. Part of the reason is that it isn’t clear what occupational analyses of the past would be compared to – occupations might have mattered more then, but that leads to the question, “…relative to what?” If the “what” is firms and jobs-within-firms, then that data in many places is flat out not there. If occupations are heteroskedastic in their prediction qualities across the occupational distribution, and this heteroscedasticity has changed quite a bit over time, that might be very difficult to detect but it would be well worth knowing.

Sixth, there is no particular reason to believe that occupational homogeneity/heterogeneity will be the same across countries and contexts. In some parts of the world, many skilled occupations are strongly associated with the nation-state. This tends to produce narrower earnings/income distributions and more uniform perceptions of social status. In other locations, these same occupations may be associated with the private sector rather than the state. This should produce much more heterogeneity in job-based income and status, and more within-occupation heterogeneity.

More generally, perhaps occupations matter more in some places than others. Correspondingly, perhaps firms/establishments/jobs matter more in some places than others. There may be no simple one-to-one link between these (situations where occupations automatically matter more and firms-jobs automatically matter less) but some variant of a “varieties of capitalism” framework is one promising place to start to theoretically address variation across national contexts (Esping-Anderson 1990; Avent-Holt in this issue; see also Tomaskovic-Devey et al., 2020).

7. CONCLUSION: The Study of Inequalities – What’s in Your Wallet?

America is nothing if not the land of credit cards. In a television advertisement for the new CapitalOne Venture Card, Jennifer Garner delivers the punchline slogan, “What’s in Your Wallet?” Those who study inequalities occasionally need to return to a variation of this basic question and apply it to our research.

In the end, the answer to the question of how useful occupations are comes down to whether occupations represent significant bundles of life chances, whether occupations are highly correlated with incomes, benefits, and work quality, and whether occupations explain more inequalities than other dimensions of social structure. But there is no escaping that, in an era of high economic inequality, the ultimate utility of any measure of welfare comes down to “What’s in Your Wallet?” If it cannot be spent or traded, if it cannot increase in value and if it is unavailable for use by the holder then, relative to cash, it doesn’t exist. It may be worthwhile to study occupational changes divorced from a connection to permanent incomes (or to claim that the attainment of some occupations produces less volatile permanent incomes), but those claims need to be justified rather than assumed. There is considerable evidence presented here that the assumption needs work and our measurement of occupation may need work as well. At minimum our coding of occupations needs to be cleaned up so that our transition to occupations-within-firms (i.e. jobs) does not suffer from the same problems our authors outline here. After all, a miscoded occupation-

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2 I experienced this directly at one of the institutions I was working for. In a Department Chair’s meeting an Associate Dean was lecturing us about the importance of and stress on diversity for the coming hiring season. The Department Chair of English, who was African American, promptly stood up and said, “What African American parent in their right mind in the early 21st Century would tell their child to get a PhD in English?” Obviously he was skeptical that the additional occupational prestige points were a perfect substitute for more money in another professional occupation employing people with advanced degrees, regardless of the status it received. Needless to say, this comment led to several minutes of awkward silence.
within-firm is just another mis-coded occupation. We need a better theoretical understanding of when and why firms might matter for explaining inequalities than occupations, and vice versa. And we need to analyze labor markets at the levels they actually operate rather than assuming that everyone is competing on a national labor market to find a job.

In the end the old VW Beetle representing occupational analysis may not survive this domestic dispute, but it has had a good run and served the family well. It will be interesting to see what you as readers think.

Declarations of Competing Interest

The authors report no declarations of interest.

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