

Two Cheers for Social Mobility: comments on Bukodi and Sturgis

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Social Mobility Grinding to a Halt? New Evidence from the Census and Birth Cohort Studies

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I should begin by thanking our speakers for giving us two excellent, thought provoking, talks. New data and new results are always seductive, especially I might add, when the results confirm what one is already predisposed to believe!

More seriously, there is no lack of chit-chat in the press, in think-tanks and governmental circles about declining social mobility, stalling social mobility or the UK's social mobility problem. In fact there have probably been many more words written about the subject of late than there are data points to base those words on. So we should not underestimate the value of empirical work that brings new evidence to the table or under appreciate the considerable effort that goes in to making it intelligible. If social policies are to be made they should be made in the light of **all** the relevant evidence not merely the bits that happen to suit a narrative that has already been decided upon. So here we have two new pieces of the jigsaw. How and where do they fit in?

Making sense of the evidence is actually no easy task, as both our speakers are well aware. We have to face the fact that all data sources are imperfect, have strengths and weaknesses some of which may be unknown or even undiscoverable. We almost never observe what ideally we would want to observe and we usually never have sufficient data to estimate the quantities we are interested in with great precision. When it comes to establishing what the facts of the matter are about trends in social mobility in the UK, for now and for the foreseeable future we are all looking through a glass darkly and trying to do the best we can with what we have to hand. It should go without saying that this should entail being tough on ourselves and not shrinking from addressing difficult questions about the quality of our sources. I'll say more about this issue as I proceed.

So what have we learned? I've no intention of reiterating all of the various results that have just been shown to us. But let me pick out just a few things to comment on.

Figure about here

In this graph I pull together as best I can – the trends for men with respect to absolute mobility rates – total mobility, upward mobility and downward mobility reported in the two papers we have heard, up to age 27 for the cohort data and age 30 for the LS. In neither case, as both our speakers are aware would we really want to speak of these men as having reached the stage of “occupational maturity” which is to say that some of the action will be and is in fact observed at slightly later ages, but this graph will do for my illustrative purposes.

You might expect the patterns revealed by the LS and the cohort studies to have a basic similarity but at least in this case they don't (let's simplify radically by ignoring all considerations of estimation uncertainty).

Though both sources agree that the level of total mobility doesn't change much during the period under observation there is a roughly 10% point difference in the estimate of what that level actually is (NB this probably is because of a definitional difference as to what counts as total mobility in the two papers).

If we turn to upward and downward mobility the two sources also tell a different story. Whereas the cohort studies suggest a convergence in the rates of upward and downward mobility with the latter becoming more relatively common and the former less common, the LS suggests little change during its observational window. So if, hypothetically, we wish to present a convergence of upward and downward mobility amongst those born in the early 1980s as a stylised fact we should be aware that its facticity is rather fragile.

But what if we asked the good (or bad) fairy to take away the LS and erase all memory of it from our consciousness. Would we still be surprised by the pattern revealed by the cohort studies? Yes, I think there are grounds for surprise. The apparent convergence between the upward and downward rates for the 1970 and the 1982 cohort is dwarfed in magnitude by the convergence between the 1946 and the 1958 cohorts. Apparently those born in 1958 experienced less upward mobility and more downward mobility than those born in 1946. It could be true but I'm

not sure I would have predicted this [in the discussion Bukodi gave an account of why it might be plausible]. After all we've been led to believe that the 1958 cohort represented the lucky generation – at least compared to the cohorts that came after it.

Let's turn from absolute rates to relative rates. Here in a sense the story is simpler. Neither of today's papers supplies any evidence that would support the view that, in the big picture, relative rate of mobility are declining or as sociologists sometimes like to put it, that social fluidity is increasing. For men there is some slight evidence of an increase in fluidity between the 1946 and the 1958 cohort up to the age of 27. But this apparent difference disappears if we take these men up to the age of 38. Otherwise, though we can produce so called unidiff parameter estimates that are monotonically declining - indicating more fluidity - both in the cohort studies and in the LS the model of constant social fluidity fits rather and is not significantly improved upon by fitting a trend.

When it comes to the women though there is a major divergence between the LS and the cohort studies. Whereas the cohort studies suggest a considerable increase in social fluidity – ie a decline in the association between social origin and social destination, the LS finds no strong evidence of change either at age 32 or age 42.

Which of these accounts should we believe, assuming that they both can't be true? Using the cohort data Bukodi and her colleagues go on to give us an intriguing explanation of the trend they find in terms of the selection process that leads some women into part-time work. This all sound and looks to me very plausible but how robust are the findings? I can't answer that question, but another piece of evidence from the LS should perhaps give us food for thought.

Consider the following figure presented to us by Professor Sturgis:

Figure about here

In the LS the sample size is so large that estimates for single year birth cohorts can be produced and this is one of the figures that shows the results of doing so.

To remind you, what is plotted here are the so called unidiff parameters that give a global estimate of the proportional extent to which fluidity is either increasing or decreasing relative to a reference year. Numbers less than 1 or points below the horizontal line indicate increased fluidity.

With the exception of the values at age 42 we've not been given information about the uncertainty around these numbers – the relevant test at age 42 actually suggests no difference – but what strikes me powerfully is the year on year variability. Visually at least you would get a very different impression if you made the comparison at age 40 or at age 43. But common sense suggests that a difference of 3 years of age should be neither here nor there. Again we are forced to conclude that some of what we think we know may have rather fragile foundations.

My last piece of scepticism concerns something much simpler than estimating the parameter of a somewhat complex model. Let's look at one very important piece of the evidence, but something which for entirely understandable reasons is not really in the foreground of the papers we have heard – the marginal distribution of social class destinations. This is, of course, controlled for in the analysis of relative rates but can have an influence on the calculation of absolute mobility rates.

Let's look at the distribution across NS-SECs in 2011 for men as estimated in the Annual Population Survey using the grossing up weights. This should give us pretty good estimates of the population values – at least for those who were at the time employed or self-employed (I was too lazy to write the code that would have taken into account the unemployed and those out of the labour market). There are three columns that correspond to 3 age groups which are also represented in 2011 in one of the cohort surveys - the UK Household Longitudinal Survey and the LS.

	APS 24-30	APS 30-36	APS 40-46
Higher managerial and professional	14.3	22	22.5
Lower managerial and professional	23.2	26.6	25.9
Intermediate occupations	11	8.45	6.5
Small employers and own account workers	9.3	10.7	14.7
Lower supervisory and technical	12.3	10.8	10.2
Semi-routine occupations	14.8	10.4	9.3
Routine occupations	14.7	11.2	10.9

These numbers look broadly sensible. Now here are the equivalents from the cohort study and the LS.

Table about here

There are some minor, but possibly surprising differences. The cohort/LS seems to overestimate the percentage of higher managers and professionals at younger ages and underestimate them at higher ages. It also tends to overestimate the percentage of small employers and own account workers and the cohort study seriously under-estimates the percentage of lower supervisors and technical workers at age 24-30. Of course the sample sizes— at least for the cohort survey are relatively small – but even so, what is being estimated is not particularly demanding.

So what is going on here? Why do we get differences between the cohort studies, the LS and other data sources? It's not difficult to think of reasons why we might find these apparent differences if we rummage around enough in the repertoire of ad hockery.

One possibility might be differences in the quality of the basic occupational data. I'll give you an anecdote to illustrate what I mean. The census occupational coding basically depends on whatever people happen to write down and that is usually brief and sometimes misleading.

A little while ago I had occasion to look up Ramsey MacDonald in the 1911 Census. The 1911 Census is very nice because the original census schedules still survive and you can see what the householder wrote in their own hand. Ramsey Macdonald who at the time was living with his wife and children in a flat in Lincoln's Inn Field gave as his occupation the single word: Secretary. This was not untruthful. He was at the time Secretary of the Labour Representation Committee. He was also an MP and a freelance journalist. Scribbled across the schedule in red ink – presumably by some well-informed Census official are the words: "This is the leader of the Labour Party".

Generally speaking information collected in a complete population enumeration tends to be more prone to measurement error than information collected in sample surveys by face-to face interview. How big an impact this will have will depend on the level of aggregation we consider. At 7 category NS-SEC level one wouldn't expect it to be so big, but one can't completely dismiss it.

Another obvious difference between the LS and the cohort surveys is that in the latter it is possible to use information about previous jobs to assign a class position to those who are currently unemployed or out of the labour market. In the LS the best that could be done would be to go back 10 years to the previous census to fill in missing information. Common sense suggests that this problem will have its

biggest impact on the analysis of female mobility patterns and that its significance will not be constant over time.

While on the topic of female social mobility it is also worth making one further observation. This really is an observation and not a criticism because I know that our presenters are well aware of the difficulty but entirely constrained by a limitation of their data. People live in households and the level of welfare they experience is partly determined by who else is in the household and what they are doing. Measuring female social class mobility simply in terms of a woman's own occupation can only give us a very partial picture. A female part-time clerical worker married to a call-sector worker is in a rather different class situation to a female part-time clerical worker married to a doctor. It is my understanding that neither the cohort studies nor the LS can effectively deal with this problem because they do not have sufficient information about the spouses and partners of respondents that are followed.

There are a number of other points I could make about relevant differences between the cohort studies, the LS and other relevant data sources all of which may play a small part in generating the differences we observe. But I'll leave those for others to take up.

Our basic problem is that the data we have is barely adequate to address the issues we are really interested in. This need not have been the case. In 1997 a government was elected that claimed to have issues of social mobility on its social policy agenda. The current government makes similar claims. Imagine if in 1997 somebody in Whitehall had had sufficient clout to force the politicians to see that without relevant good quality data social policy is shooting in the dark. All it would have taken is somebody in 1997 to have insisted that a couple of questions of social origins were included in just one quarter of the annual Labour Force Survey. By now we would have 17 years of cross-sectional data. Even if we were still unable to say as much as we would like about the future we would today be in a much better position to talk confidently about the past.