**Success, proof, fairness, causes and consequences: five unanswered questions for the future of widening participation in higher education in the UK**

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**Introduction**

This chapter has its roots in a paper delivered at the 2012 FACE conference examining the misspecification of statistical performance indicators around the widening participation (WP) agenda in UK higher education (HE). However, rather than reflecting the main content of the paper itself (largely published as Harrison, 2012), this chapter re-poses the five ‘looking forwards’ questions posed at the end. This chapter develops some of the themes explored in the discussion following the paper and elsewhere in the conference, including the (de)construction of the key WP outcomes, the contested nature of ‘what works’ in educational research, fairness and equality, and the wider societal impacts of WP policy and practice.

**Context**

The original paper addressed the demise of the national Aimhigher programme, launched in 2004 and cancelled in 2011. Approaching £1 billion was expended to increase the proportion of young people from identified under-represented groups (Higher Education Funding Council for England [HEFCE], 2004; HEFCE, 2007) applying to (and, by implication, entering) HE. This was primarily intended to close the so-called ‘social class gap’ (National Audit Office [NAO], 2008) that saw participation among the highest socio-economic group being around six times that of the lowest. Aimhigher offered a wide range of interventions at national, regional and sub-regional levels, including summer schools, university taster days, tutoring/mentoring, work experience, enhanced information/advice/guidance and similar. Aimhigher was the main exposition of the WP agenda across this period, although individual universities and other organisations have made their own contribution; before, during and since.

One can analyse the cancellation of Aimhigher through a variety of lenses. A critical perspective might assert that the incoming Coalition government was ideologically hostile to the challenge to the *status quo* of wealthy families using their cultural, economic and social capital to secure university places for their children, especially in elite institutions. This is challenged by continuing policy around social mobility within the 2011 White Paper (Department for Business, Innovation and Skills [DBIS], 2011a), alongside initiatives led by the Deputy Prime Minister (Cabinet Office, 2011) and the social mobility ‘tsar’ (Cabinet Office, 2012). A more pragmatic analysis might conclude that Aimhigher was collateral damage in the ‘bonfire of quangos’, cutting costs in a period of austerity. Indeed, the Labour government had nearly halved Aimhigher’s budget in 2008.

Alternatively there was simply no evidence that it was working; at least, not under the official statistic designed to track progress. Harrison (2012) argues that problems with data definitions and processing, coupled with a poor mission specification and understanding of what constituted ‘policy success’ (McConnell, 2010), made it inevitable that Aimhigher’s impact was contested. A new performance indicator (‘Full-time Young Participation by Socio-Economic Class’ or FYPSEC) was created in 2008 (Kelly and Cook, 2008) and then abandoned in 2011 (DBIS, 2011b). The official reason was that there were data issues – which there undoubtedly were – but FYPSEC was showing very modest returns on the government’s investment in Aimhigher.

However, a number of other official sources (notably HEFCE, 2010) suggested that the social composition of HE was shifting and that the change was accelerating through the latter years of Aimhigher’s life. This chimed with the perceptions of chalk-face practitioners (Moore and Dunworth, 2011), academic researchers (Chilosi et al., 2010) and the early national evaluations (e.g. Emmerson et al., 2005). The trend was even more marked in applications than admissions (2012).

So, was Aimhigher a success? Its supporters could not point to cast-iron evidence that it was. There was a consensus around the organisation and its partners that, after a rocky and ill-focused birth, Aimhigher had found its feet and was delivering the agenda asked of it. However, the ‘hard’ evidence to support this view was partial and poorly grounded. The national evaluation designed to quantitatively isolate Aimhigher’s contribution failed to fulfil its brief, citing data problems and falling back on ‘soft’ descriptive data (Passy and Morris, 2010). Aimhigher’s sceptics could point to other possible causes for positive trends in HE applications and admissions.

**Five questions in search of answers**

The conference paper posed five questions that are re-posed, in a slightly amended form, below:

1. Is WP about school attainment, aspirations, applications, admissions or something else?
2. Is it enough to know that young people enjoy and value WP interventions – what about behaviour change?
3. Are area-based approaches to WP fair?
4. Is social class the *cause* or a *symptom* of educational inequalities – and which?
5. Is participation without economic regeneration *solving* or *worsening* the problem?

We will take each of these questions in turn, develop the thinking that underpins them, review the evidence and attempt to provide pathways towards answering them.

**Question 1: outcomes and measures of success**

Aimhigher’s mission shifted markedly during its seven years of existence. While the Dearing Report (National Committee of Inquiry into Higher Education [NCIHE], 1997) and the 2003 White Paper (Department for Education and Skills [DfES], 2003a) focused on those already studying, Aimhigher was originally charged to focus on aspirations for HE, as manifested in young people making applications (HEFCE, 2004), with a tacit assumption that applications would inexorably lead to admissions. Many WP practitioners demurred from seeing admissions as their concern, as these decisions were firmly within the remit of individual universities. However, there are many ways in which young people can be taught to ‘play the game’ of university admissions, whether by making tactical choices or enriching the personal statement that looms so large, especially for elite universities (Jones, 2012).

The essential problem, though, was that the relationship between applications and admissions began to break down in the late 2000s. Coleman and Bekhradnia (2011) note that from 2009 onwards, demand for HE began to seriously outstrip the supply, constrained by government funding levers. There was a new competitiveness bubbling up, signalling the end of the Robbins principle of a university place for every qualified person who wanted one. As a result, the growth of applications from lower socio-economic groups radically outpaced that of their admissions by 2010 (Harrison, 2012). In other words, schools (and probably Aimhigher) produced too many inspired and qualified young people for the sector to accommodate.

Through its life, Aimhigher’s focus changed gradually, to the point that raising attainment became its key objective (HEFCE, 2008). This was partly due to feedback from practitioners who were increasingly finding better results in terms of challenging traditional educational patterns when working with younger age groups, even down to primary school in some cases; more a codification of evolved practice than a sea change in policy. However, it also reflected a growing belief that the most effective way to increase progression from lower socio-economic groups was to improve their educational outcomes, promoting progression into Level 3 study. This gained support from DfES (2003b), Coleman and Bekhradnia (2011) and Department of Children, Schools and Families (2010) which found that the proportion of young people progressing to HE with various GCSE and A Level profiles were broadly constant and independent of social class.

Various studies (Chowdry et al., 2008; Marcenaro-Gutierrez, Galindo-Rueda and Vignoles, 2008; Anders, 2012) have reinforced this view arguing that while issues such as social class may impact on lower levels of educational attainment, it is a young person’s performance at 11 or 16 and their subsequent decision to pass into Level 3 study (especially A Levels) that largely dictates behaviour at 18. Crudely, if they are still on the academic conveyor belt at 16, they will continue to university almost regardless of family background. This conclusion is generally grounded in quantitative analysis of national figures and has been received by policymakers as robust and objective evidence providing easily-understood and apparently compelling conclusions.

However, this ground perhaps needs contesting more than it has to date. As Figure 1 shows, the headline GCSE pass rate has indeed risen sharply since 2002. This main measure is constructed from the proportion achieving five passes at grades A\* to C, including so-called ‘equivalents’ – vocational and skills-based qualifications undertaken alongside, or instead of, GCSEs (e.g. BTECs). Figure 1 also shows the pattern of GCSE passes once these ‘equivalents’ are removed and once the condition that the five passes includes both English and mathematics (generally required for HE entry) is imposed. It is immediately obvious that the majority of improvement in GCSE results during the last decade as actually been not in GCSEs, but rather in the ‘equivalents’; analysis and commentary often makes the category error of confounding the two. This is important as the main focus of the ‘equivalents’ has been in state schools serving deprived areas and pupils not well-suited to traditional GCSE courses – although many have gone on to HE.

*Figure 1: GCSE pass rates (2002 to 2011) using different threshold definitions*

Source: Department for Education

Not only was Aimhigher engaged in interventions designed to directly impact on attainment such as mentoring/tutoring (Goodlad, 1996), but it was also contextualising attainment for young people in deprived areas in new ways. Initiatives like summer schools and university taster days carried the message that strong school achievement could lead to university, even where this was traditionally unusual. There is good evidence (Moore and Dunworth, 2011) of Aimhigher activities raising motivation for school, which is likely to have been one contributory factor in improved outcomes; indeed, teachers consistently reported this (Hatt, Baxter and Tate, 2008). Finally, Aimhigher helped to raise the status of vocational qualifications and work-based learning as valid routes into HE, promoting these pathways as alternative reasons to ‘stay on’ at 16.

Aimhigher was thus providing at least three forms of impetus towards attainment at 16 and staying-on. Rather than sitting alongside improving GCSE results as a competing (and losing) explanation for increases in HE demand, it underpinned these improvements by adding value to other initiatives. Harrison (in press) finds that local authority areas with the lowest demand in 2004 showed the greatest improvement by 2009, even once GCSE improvements were accounted for. As these were the very areas targeted by Aimhigher and other WP initiatives, parsimony suggests a link between intervention and outcome.

How do these observations lead us towards an answer? Firstly, an appreciation is needed of the complex processes leading to HE admission. Aimhigher suffered from a dissonance between its mission (to raise attainment at 16, from 2008 onwards) and the measure by which it was effectively judged by the main published performance indicator (entry to HE and persistence for the first two months). Furthermore, a more nuanced appreciation of the role of WP interventions not only in raising aspirations, but also in raising attainment both directly and indirectly, is needed. This link needs evidencing more closely to become ingrained within the discourse around GCSEs and university demand.

Secondly, it would be a fallacy to assume that the connection between GCSE results and entry to HE will remain static indefinitely. Given a period of excess demand where the competition for places is more cutthroat than previously, it can be hypothesised that young people from lower socio-economic groups may increasingly lose out. The extension of the compulsory leaving age to 18 will also impact on achievement patterns as the concept of ‘staying on’ changes.

Thirdly, a possibly more seismic change will result from the removal or downgrading (e.g. from three GCSEs to one) of most of the ‘GCSE equivalents’ from 2014 (Department for Education, 2012). Schools are hurrying to divest themselves of the very qualifications which have been at the heart of their improved results. We may well find that the schools with the greatest improvements in results since 2002 will see a reversal in the mid 2010s as pupils struggle to adapt back into the GCSE curricula; the abolition of GCSEs themselves is another spectre on the horizon.

The final thread to weave in is the radical growth of apprenticeships undertaken by the Coalition government. If apprenticeships gain credibility with young people as a means to ‘get on’ into a high-quality career, the choice envelope surrounding HE will change significantly, especially with rising university costs.

So, the future needs both more clarity about specifically what outcome measures WP initiatives are intended to impact upon and over what timescales, alongside a healthy scepticism that the past will be a good predictor for future demand patterns.

**Question 2: causality and proof**

Causality in educational research has been a hot topic since the late 1990s, when there was a strong move towards a more (arguably) rigorous and quantitatively-based epistemology to underpin evidence-based practice – crudely, a ‘what works’ approach to research. Supporters (e.g. Hillage et al., 1998; Hargreaves, 1999) claimed that too much research derived from subjective self-reports from practitioners with a vested interest in proving the success of an intervention, while too little effort was expended on demonstrating objective changes to outcomes or behaviours. In particular, there was a strong call for greater use of experimental research designs incorporating control groups. Opponents (e.g. Hodkinson, 2004; Lather, 2004) argued that such approaches are highly mechanistic and simplistic in what are naturally complex fields and that a ‘stimulus-response’ approach is not always, if ever, appropriate. Furthermore, they argued that it devalued the role of the reflexive practitioner and implied an inability to be objective. For example, Doyle and Griffin (2012) reject reductionist epistemology that misses the wider and indirect value in WP work.

This debate continues: as former senior civil servant Jon Coles explored in his keynote address to the 2012 British Educational Research Association conference, education policy is littered with initiatives that were massively popular and well-regarded by stakeholders, practitioners and/or participants, but which were either unresearched or where research was unable to find any positive impact (Coles, 2012).

WP activities have not been immune to this critique. Early reviews (e.g. Universities UK, 1998) tended to be compendia of perceived good practice rather than a critical evaluation of results. Since then, there have been misgivings about the robustness of WP research. Gorard et al.’s (2006) critique of the literature characterised it as small-scale, descriptive and overly reliant on ‘soft’ measures from practitioners, teachers and young people. Their assessment initially sent shockwaves through the WP community, although little changed as a result. Many practical difficulties (e.g. with data availability and comparability) meant it proved difficult for practitioners and researchers to adopt a more quantitative approach. As mentioned earlier, even Aimhigher’s independent evaluators succumbed to these difficulties (Passy and Morris, 2010).

Moore and Dunworth’s (2011) end-of-life review of 62 research studies from Aimhigher partnerships synthesised an impressive corpus of positive opinions of activities from stakeholders. Aspirations had been raised, motivation for school was higher, career pathways were better defined in young people’s minds – overall the story was of an enjoyable and efficacious portfolio of activities. There were some quantitative studies suggesting raised attainment and demand for HE (echoing Chilosi et al., 2011), but these suffered from being small-scale and/or of dubious robustness; there were no truly experimental studies.

What it does not evidence is any changes in *behaviour* – essentially, that any individual changed their educational trajectory as a result of Aimhigher. Given known targeting difficulties (Thomas, 2001; Hatt, Baxter and Tate, 2005), apparently positive outcomes could simply have reflected those already *en route* to university – ‘deadweight’, in social policy terms.

So, there is paucity of evidence, but is this the same as evidence of paucity? Should a seven-year multi-million pound national programme not be reasonably expected to provide compelling, robust and reliable research to prove itself a success? Should the absence of such an evidence base not be taken as a strong signal of failure?

Firstly, there is a danger of blaming the victim. Aimhigher’s birth was marked by a confusion about mission, performance measures and data (Harrison, 2012). While not exempting it from responsibility, this did weigh heavily on the ability of Aimhigher partnerships to engage in high-quality and compelling research – as did the removal of the regional infrastructure in 2008.

Secondly, there is a continuing issue with research design, with no easy solution. Some suggest that educational research should learn from the field of medical research, with its rigorously controlled experimental testing that enables ‘what works’ to be deduced with a high degree of certainty. This is often advanced by those with a misunderstanding of both medical and educational research.

There are a number of features that are simply not transferable. In experimental medical trials, the symptoms are known and only those displaying them are involved in the trial; there is no deadweight. The dose of the experimental treatment is not only known, but carefully controlled, usually to the exclusion of other treatments. Schools, on the other hand, are messy places with multiple overlapping and interacting inventions, while the ‘dose’ of WP that they receive is hard to measure, let alone control: is one summer school more effective than three taster days or ten mentoring sessions? Medical trials include placebos in order to provide a non-treatment control, but replicating this in schools would be mindlessly perverse! Medical trials also rarely last over timescales measured in years and, when they do, their statistical power declines rapidly as participants fall away.

This is not to argue for ‘business as usual’. It is clear that something needs to change if WP practitioners are to have their work and voices adequately respected in future. Clarity of mission and desired outcomes will provide a useful step forwards. Another would be to robustly evidence small stages from intervention to outcome, rather than trying to show large-scale impact on behaviour over many years. A third approach has to be a move towards a more quantitative approach to research design. In some instances, experimental design can be appropriate, albeit with an appreciation of the ‘messiness’ of educational research. However, other approaches – some of which are also employed in medical research – can offer value, such as quasi-experimental studies that retrospectively examine behaviours over time.

Perhaps the biggest sticking point is the trade-off between the aims of professional practice and the aims of research design. In the former instance, practitioners strive to provide activities for the most ‘deserving’ or ‘receptive’ individuals, effectively violating the preconditions for experimental research designs by removing the scope for a randomised control group. Conversely, a randomised approach to the allocation of places (e.g. on a summer school) would increase the deadweight. This would dull and disguise any causal effect from the activity – the equivalent of including people without symptoms in a medical trial and then being surprised that they showed no improvement! There are constructive compromises to be made, but they require a much closer and earlier co-operation between practitioner and researcher than has generally been the pattern to date.

**Question 3: geography and fairness**

There has always been a geographical component to WP activity, both in terms of targeting interventions and the pragmatics of delivery; these continue to frame ongoing questions about fairness and equality of opportunity.

The idea that the ‘right’ young people for WP activities are concentrated in certain geographical areas is deeply ingrained for policymakers and practitioners. This has revolved around the concept of ‘low participation neighbourhoods’ (LPNs) with a lower-than-average propensity to send young people to university, particularly as specified by HEFCE’s POLAR statistic (HEFCE, 2005). This was augmented by the targeting of deprived neighbourhoods, as defined by the Index of Multiple Deprivation (HEFCE, 2007), to enable LPNs to be ranked further.

Harrison and Hatt (2009b; 2010) critiqued this approach, pointing out fundamental problems with the conceptualisation. LPNs are not neighbourhoods at all, being based around artificial local government wards that often contain 10,000 people or more. The level of granularity is far lower than presumed, with very large areas in most cities being included. Furthermore, the approach tends to privilege urban forms of deprivation, both in terms of what constitutes deprivation and how it is concentrated. For example, rural wards cover vast areas, perhaps taking in ten villages or more, with the effect of averaging deprivation, such that few areas reach the concentrations found in cities.

Perhaps the most important finding was that, crudely, while richer people tend only to live in affluent areas, poorer people are to be found much more widely, especially in rural areas and those urban areas with a dispersed approach to social housing. The conceptual problem, therefore, is that area-based initiatives miss as many ‘right’ people as they hit; this finding is congruent with similar studies in other areas of social policy (Tunstall and Lupton, 2003; Rees, Power and Taylor, 2007).

Targeting by LPN remains the dominant paradigm. It is ubiquitous within university Access Agreements, echoing the annual data published by the Higher Education Statistics Agency (e.g. HESA, 2012). Indeed, as other forms of targeting (e.g. by social class) have waned in popularity, the role of LPNs has become firmer. A third phase of POLAR data was released in late 2012 and is being absorbed intently by the sector. We are likely to see, therefore, an even more reductionist approach to targeting activity in the future.

The danger is that this is magnified by the pragmatics of delivery. WP activities are, by their nature, human-intensive and based on social interaction. They typically involve a person from the world of HE (lecturer, outreach worker or student) travelling to a school or for pupils to travel to a university. The practicalities of this are obvious – it is always easier and cheaper to make this occur where the school and university are near to each other.

One of the undervalued roles of Aimhigher was to ensure a degree of equitability about the rationing of access to WP activities. Universities were required to spread their work into areas beyond their normal sphere of influence, ensuring at least some coverage in areas distant from HE provision. Funds were directly or indirectly provided to schools to enable them to meet travel costs for them or their pupils’ parents.

One effect of the demise of Aimhigher is an inevitable retraction of the geographical reach of universities’ WP operations. With less money in the system, there will be an increasingly close concentration on the local to the detriment of those living further afield; this is also being seen in bursary programmes. In areas with multiple universities, we should expect to see a new ‘game’ of chasing well-qualified young people from LPNs (whether or not they are deadweight) to meet Access Agreement promises. This supply-chain focus will see some schools and young people being courted by multiple suitors while others get little or no attention.

This analysis poses serious questions about fairness. WP is in danger of being commoditised, with more concern about where someone lives (close and deprived being preferred) than whether any outcomes, attitudes or behaviours have actually been changed.

**Question 4: social class as symptom or cause**

The Dearing Report (NCIHE, 1997) established social class as a causal factor in HE participation in the minds of policymakers and Aimhigher was established with a clear remit to challenge social class differences as experienced by young people (HEFCE, 2004). In a sense, this was a positive step forwards in describing the obvious inequalities, but it missed a vital conceptual link which may have led to fifteen years of misdirection away from deeper understanding.

As noted earlier, a number of researchers use quantitative analysis assert that social class (as they define it) is not a good proximal predictor for HE participation. Following this logic, despite the apparently deterministic relationship enshrined in the Dearing Report, social class is a mere proxy variable. The bald statistics suggest that those from the lower socio-economic groups go on to HE in roughly the ‘right’ numbers in relation to their school attainment. If this is true, social class differences at 18 are a *symptom* (of deep-rooted inequality) and not a *cause* (of low participation).

They are a symptom of the fact that school-based outcomes are not evenly distributed. There is not space here to investigate this in any depth, but factors including parental support for education, school ‘choice’, school quality and leadership, and the ability of families to lever in additional resources are all salient. For example, Raphael Reed et al. (2007) found that a drop-off in school performance in a deprived area was associated with a reduction in parental involvement in homework and parent-teacher evenings. Similarly Lupton (2005) finds that schools serving deprived neighbourhoods have lower resources available to them when compared to those in richer areas; an inequality now targeted by the ‘pupil premium’. Both these examples show a clear pathway by which elements of classed lives can impact on school outcomes.

If it is during school that social class differences in HE participation are formed, then the WP community has, to an extent at least, being labouring under a misapprehension for the last fifteen years with its structuralist discourse of ‘barriers’ to overcome, alien ‘habitus’ and aspirational deficits. Instead, they needed to be focusing more on how social class becomes a symptom and less on its role as cause.

However, there remain misunderstandings and unanswered questions about these studies. All neglect the role of WP programmes in mitigating social class inequalities in earlier school and at 18. Most use data that is old, some predating Aimhigher completely. Some do find a small social class effect; Chowdry et al. (2010) estimate it at around ten percent, while Anders (2012) find that household income and parental education (both correlates for social class) are significant predictors for HE applications. These findings suggest that remains some classed inequality at 18 which is causal to participation and on which WP practitioners can get to work on; social class is thus both symptom *and* cause.

Another problem is how we measure social class. The dominant method has been through the occupation of a young person’s parents. This has a wide-range of pitfalls that touch on data collection, reliability, gender assumptions and the changing nature of the British workforce. The categorisation that underpins both official statistics and individual research projects is often so strained that it is difficult to draw any strong conclusions from it (Harrison and Hatt, 2009a). This alone is grounds to question any denial of social class as a cause.

More broadly, the structuralist approach to social class underplays individual agency. Young people from lower socio-economic groups have progressed to university for many decades and every family with HE experience has had one pioneer who was ‘the first’. Much thought has been given to barriers to progression (Gorard et al., 2007), but little to why some have always been able to overcome those barriers and thrive.

Looking forward, then, there needs to be more clarity about what social class actually means in terms of participation, rather than using it as a poor and unreliable proxy for a complex web of factors that may only exert a meaningful influence prior to 16. The WP community needs to plug into the literature around class and school outcomes more closely to understand what the right levers are and when/how they are best pulled. They also need to think more about why seemingly similar individuals do and do not participate in HE, respecting individuality alongside structural constraints.

**Question 5: the wider impacts**

The underpinning philosophy of WP for most practitioners has been around challenging social inequalities by providing fairer access to HE and graduate careers. The question is whether the second part of the assertion is truly associated with the first: could the impact actually be the opposite?

There is a significant gap in the evidence base concerning what happens after graduation to those exposed to WP activities. While we have reasonable information about where they came from, we are far less informed about where they went next. For example, we do not know how many return to the communities from which they came. We do know that students from lower socio-economic groups are more likely to live at home while they are studying (Universities and Colleges Admissions Service, 2008). Were these able to find graduate employment locally or did they move away into richer areas? Did those that went away to study remain in their university city or did they return to their original community to pursue their career?

One view of HE is that it is training for entry into the middle classes, offering new cultural and social capital and the qualifications to access increased economic capital. Students from lower socio-economic groups talk about a social class transition or having ‘a foot in two camps’ (e.g. Clayton, Crozier and Reay, 2009) as they gain middle class capitals and traits alongside their working class ones, sometimes leading to tensions within their family, friends and community.

This final question draws from this observation and hypothesises that for a significant proportion of such students, it is the middle class ‘foot’ that wins out. They do not return to their deprived neighbourhoods with their degrees and find graduate work. Rather, they enter the middle classes, live in affluent neighbourhoods and begin to sever ties with their pre-university past, perhaps remaining conscious of a duality or fluidity in their identity. Clearly this is a choice for an individual empowered by education.

One driver for this behaviour would be the distribution of graduate jobs. They are heavily concentrated in certain locations – nationally, regionally and locally. London and the other major cities are obvious critical masses. Within cities, graduate jobs are generally to be found in the central area, affluent suburbs and the out-of-town business/science parks that have flourished of late. Where they are not, generally, is in the poor urban neighbourhoods and the rural/coastal areas targeted by WP initiatives (Raphael Reed et al., 2007; Shucksmith, 2000). In short, it may be challenging for a graduate to find work at an appropriate level and live within a deprived or remote neighbourhood, even if they want to. Clearly practical considerations like transport links and ability to commute are factors.

Under this hypothesis, it is possible to interpret WP as an exercise in well-intentioned ‘cream-skimming’. The highest attaining and most ambitious young people from each generation are given various helping hands to get to university from the LPNs in which they grew up. However, the majority take their new-found middle class capitals and never return. They do not inspire a new generation of working class applicants and they do not attract new business investment into their neighbourhood. Instead, they move to a middle class area, get a middle class job and rarely look back.

Through this lens, WP becomes an exercise in upward social mobility, but it fails to address the underlying social divisions. It is a means by which a number of ‘deserving’ individuals are ‘rescued’ from their deprivation and given access to the ranks of educated professionals. It is a social ladder lowered down by those that have gone before.

What it is not, therefore, is a true challenge to structural social inequality. Indeed, it may even serve to worsen inequalities by permanently removing some of the most intelligent, motivated and talented individuals from their communities. Dorling (2011) suggests that those born into deprived communities with ‘get up and go’ are prone to have ‘gotten up and gone’ as soon as they can, leaving their neighbourhood with less collective capability to drive social or economic change. By lowering a community’s skill base, aspirational employers are even less likely to move into the area and create well-paid jobs. The wheel turns for another generation.

This is the least well-explored of the five questions presented. It is crying out for an improved evidence base around how WP initiatives might best fit into an holistic programme for regeneration and renewal in particular communities with multi-faceted and multi-generational forms of disadvantage, rather than standing apart and potentially exacerbating the problems. Some progress was made through the four reports commissioned by HEFCE into some of the most deprived urban areas (Raphael Reed, Gates and Last, 2007), but this focused methodology was not pursued, let alone backed with appropriate policy and resourcing.

**Conclusions**

The purpose of this chapter has been to explore some outstanding unanswered questions from the realm of WP research and practice. It does not seek to answer them in detail, largely as much of the required evidence base missing. However, it is hoped that posing them will give pause for thought before the HE sector moves into the post-Aimhigher world with less inter-university co-ordination, less resource for research and a more confused policy horizon.

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