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Community re-studies: lessons and prospects.

Graham Crow, University of Southampton.

Abstract: Community re-studies have played an important role in the development of knowledge about social relationships and social change although they have had a chequered history. Assessments of their value vary according to whether they are approached as replications of earlier studies or as a different type of project. Re-studies have the potential to undermine the credibility of earlier studies but they can also fill in gaps and provide the basis for assessment of social change at a local level. They are generally characterised by methodological and theoretical innovation, learning from and building on what has gone before and contributing to an accumulating body of knowledge about community relations and how to study them.

Keywords: community studies; re-studies; replication; re-visiting

## Introduction

Community studies have been the subject of numerous reviews over the years (Bell and Newby 1971, Blackshaw 2010, Brunt 2001, Bulmer 1985, Cohen 1985; Crow 2002; Crow and Allan 1994, Day 2006, Frankenberg 1969, Stacey 1969, Stein 1964, Vidich *et al.* 1971). These reviews portray the field of community studies as marked by considerable diversity, in terms of how community studies have been conceived and undertaken, in terms of the critical assessments that have been made of them both individually and as a body of work, and in terms of the assessment of the long-term direction of the field's development. In these reviews reference is made to the contribution of community re-studies to the field's trajectory, although there is little agreement about the nature of that contribution. From one angle the capacity of re-studies to cast doubt on the veracity of the findings of the original research stands as an indictment of the whole community studies approach as unreliable. By contrast, another line of argument advances a more progressive narrative in which methodological and theoretical evolution has led to better, more representative and more valid results. Such diametrically opposed assessments are possible because there is no underlying consensus about what community re-studies are or what their purpose is. Clarifying the nature of these issues is the aim of this short (and inevitably incomplete) review of the lessons to be learned from the history of community re-studies. This will be done through a consideration of key issues rather than through a straightforward chronology, although the conclusion will offer an overall assessment of how the field has come to be where it is to-day, and its future prospects.

Many community studies have been assessed as falling short of social science benchmarks of best practice relating to representativeness, reliability and validity, and concerns about these issues figure prominently in the discussion of community re-studies. The unrepresentativeness of community studies' findings is bound to be a recurrent charge since they are often motivated by a desire to capture the nature of some unusual or distinctive community. Where by contrast the community under investigation has been framed as being in some sense typical, this has proved to be equally controversial. Either way, whether or not researchers are looking to generalise from a particular case, re-studies have the potential to give voice to social groups excluded in original pieces of research; examples of such groups have included minority ethnic groups, and men as well as women. With regard to reliability, re-studies have been at the heart of the debate about whether community studies methods produce results consistently. Several celebrated re-studies have involved different researchers going back to communities and finding things at odds with the contents of original research reports. Where these discrepancies cannot be accounted for simply by the passage of time, questions are inevitably raised about the methods, the researchers, or both. This issue of reliability connects to concerns about the validity of a re-study relative to that of an original study, because conflicting accounts necessarily prompt discussion of whether community relations have

changed in the interim period, or whether one approach has managed to get closer to the essence of community.

Underlying these debates about representativeness, reliability and validity is a discussion about the extent to which researchers undertaking community re-studies should aspire to replicate an original study. The language of replication is often associated with research employing experiments and related methods, but in general community researchers have not followed Lloyd Warner's conception of communities as a 'laboratory' (1974: 273) or a 'clinic' (Warner *et al.*, 1964: xi), even if some have used the word replication (Seale 1999: ch.10). Warner's ambitions for community research were remarkable, but negative reactions to them steered the development of the field in quite different directions. His Yankee City project investigating social stratification in Newburyport, Massachusetts in the 1930s was conducted 'on a scale that can only be described as prodigious'. Over a period of a decade it employed 'some 30 research assistants' to fulfil his ambition to record 'millions of social facts' (Thernstrom 1974: 295), while his study *Democracy in Jonesville* (Warner *et al.* 1964) lists no fewer than ten collaborators and co-authors. Although the scale of these ambitions did not prevent researchers elsewhere from following the "'Warner approach" to community studies' (Bell and Newby 1971: 111), scale is bound to be a factor making replication difficult to achieve. This would have been an obvious consideration in Erving Goffman's decision as a lone postgraduate researcher on the remote Scottish island of Unst to pursue a different research strategy to that which Warner was expecting. Goffman had been sent there to conduct a community study in the 'Yankee City' tradition, but instead wrote 'Communication conduct in an island community', whose fieldwork featured prominently in his (1956) *The Presentation of Self in Everyday Life*. For Goffman this was a study not 'of a community' but 'in a community' (quoted in Winkin 2000: 200, emphasis in original), concentrating on interpersonal behaviour and everyday dramas rather than on Warner's concerns with stratification. Goffman's implicit rejection of Warner's framework echoed numerous explicit attacks (Platt 1996: 102). These were so withering that the 'methodological blunders' (Thernstrom 1974: 294) and 'ahistorical assumptions of the old equilibrium school of social anthropology' (Bell and Newby 1971: 110-11) that they exposed killed off any aspirations to replicate work in the Warner tradition. Expressed more generally, the point is that pioneering research is rarely if ever perfect, and subsequent generations of researchers face a choice over how faithfully to stick to replicating their predecessors' agendas, and how far they should develop and revise them.

### Representativeness

One important justification of a re-study is that it may capture more fully the population of a community than a previous study did. This issue has featured since Robert and Helen Lynd developed their analysis in *Middletown in Transition* (1937) of the power of the family whose business interests were dominant locally. This discussion had not featured in their original (1929)

*Middletown* study, but the re-study saw a whole chapter devoted to 'the X family' (in fact the Ball family) as a case of 'business-class control' (1937: 74). According to Robert Lynd, the changes between the first and second studies came about through soliciting the views of local people about the accuracy or otherwise of the original publication, and through researcher reflexivity. In the Preface to *Middletown in Transition*, which had not been intended to be a comprehensive re-study, he mentioned the inevitability of subjectivity when using the written record of the previous study's findings 'as a base line against which to analyse the broad changes' of the intervening period, 'the dramatic ten years of boom and depression' (Lynd and Lynd 1937: x-xi) that had befallen America. Others have suggested that the inclusion of the Ball family in the re-study also had something to do with the fact that between the first and second studies he read Marx (Bell and Newby 1971: 90), but additional factors such as the greater visibility of large capitalist corporations in the 1930s depression have also been noted (Burawoy 2003: 663; Caccamo 2000: 14). The focus on Muncie's previously overlooked African-American inhabitants in Eric Lassiter and his colleagues' (2004) account of *The Other Side of Middletown* provides a still more striking example of how fruitful it can be to use re-studies to extend the range of people covered in research on a community, notwithstanding the fact that this takes the agenda away from strict replication.

Class and ethnicity also feature in discussions of what can be learned from re-studies of the well-known investigation by Michael Young and Peter Willmott, *Family and Kinship in East London* (1957). The original study focused principally on Bethnal Green and its people, particularly their family relationships and housing, and it became a classic community study, reflected in sales of over half a million copies. It served as a model for community studies conducted elsewhere (such as Colin Rosser and Christopher Harris's (1965) study of Swansea) as well as a reference point for many later studies conducted in East London. None of these can be considered a replication. Geoff Dench, Kate Gavron and Michael Young's *The New East End* comes closest to being a re-study of the original. Described as 'a sequel to Family and Kinship', a 'second study', it set out with the aim 'to repeat the 1953 enquiry' but it 'ended up with a different focus from the 1957 book' (2006: 2, 236). Not quite 40 years separated the follow-up survey of Bethnal Green residents that Dench and his colleagues conducted in 1992, with Michael Young among the re-study team (although he died before the publication of *The New East End* in 2006). Given that the Institute of Community Studies (later the Young Foundation) was located in Bethnal Green, this re-study had never been far from Young's mind. A new introduction to the 1986 edition of *Family and Kinship* reflected on how 'In the last thirty years a great deal has happened to the locality' (Young and Willmott 1986: xiii). These changes were associated primarily with the new wave of immigration from Bangladesh, and with the transformation of the East End's economic profile as the old docks gave way to the new Docklands, which attracted into the area more affluent groups than the traditional working class that had dominated the scene at the time of the original study. Dench and his colleagues rightly took

representativeness to require researchers undertaking a re-study to focus on the newly-arrived as well as the long-established residents, and a similar philosophy informed Nickie Charles and her colleagues re-study of Swansea, although there the proportion of ethnic minority residents was only 2%, compared to around one third in Bethnal Green (Charles *et al.* 2008: 38; Dench *et al.* 2006: 237).

Selectivity concerning who features in community studies matters because of the inevitable influence that this has on who is given voice in the research findings. The original Young and Willmott study has been criticised amongst other things for its focus on women and corresponding neglect and marginalization of men (Phillipson *et al.* 2001: 9). Jocelyn Cornwell's *Hard-Earned Lives* develops this theme. Although her study drew its participants from Bethnal Green, she questioned what might be learned from comparisons of people living in the same place at different times because the original study had in her view generated 'the post-war myth of Bethnal Green of urban village life, a place of huge families centred around Mum, of cobbled streets and terraced cottages, open doors, children's street games, and always, and everlastingly, cups of tea and women gossiping on the doorstep' (1984: 24). Only with determination did she achieve a gender distribution of 15 women and 9 men, a ratio of 5:3 that is close to the 6:4 reported by Charles and her colleagues for their Swansea research, in which they too found it 'more difficult to find men than women who would agree to be interviewed' (2008: 43). Other researchers revisiting the East End of London have reproduced accounts in which '[t]he mother's point of view.... predominates' (Holme 1985: 11; see also Mumford and Power 2003: 18), reflecting on-going difficulties in recruiting men as participants in community research.

Where more of a gender balance among research participants has been achieved, as in Dench and his colleagues' study, men are not necessarily more sympathetically portrayed. The fact that more attention is paid to men's lives in this study does not especially work to their advantage, and one of the three men among the seven character profiles highlighted in the book is subject to particular criticism from the authors. In a section of the book entitled 'marginal men and disappearing fathers', the comment is made that 'Tony Hicks.... considered himself to be something of a New Man, on the basis that his teenage children came to stay on a Saturday night. But his use of time did not appear seriously impeded by parenting'. Consequently he was bracketed with another male respondent who 'fell well short of the role-reversing house-husband which he liked to regard himself as being' (2006: 111-2). There is thus a more complicated lesson here than that of accounts produced by male-led research teams producing reports favourable to men, and female-led teams producing more sympathetic accounts of women, even if the contribution of Phyllis Willmott (Peter's wife) to the original *Family and Kinship* study is properly acknowledged (Gavron and Mulgan 2007: ix-x).

A contrasting story unfolded around the research conducted in the mining village of 'Ashton' (Featherstone) in Yorkshire that was the basis of *Coal Is Our Life*

(Dennis *et al.* 1956) and of Dennis Warwick and Gary Littlejohn's (1992) re-study, *Coal, Capital and Culture*. The re-study acknowledged that the revolution in thinking ushered in by second-wave feminism meant that reproducing the sexist biases of the original all-male research team of Norman Dennis, Fernando Henriques and Cliff Slaughter would be untenable, and that the question 'Coal is whose life?' (Frankenberg 1976: 37) would need to be answered by doing more than providing another version of what was unequivocally a man's eye view. Warwick and Littlejohn did this by concentrating their data collection on women and by building up a research team in which women predominated (1992: xiv-xv, 87). The result is undeniably a more balanced account which dispels, for example, '[m]yths about women in mining households being tied to the kitchen sink' (1992: 88). There is also in the book an attempt to give a more nuanced portrayal of social class differences in the community compared to the relatively undifferentiated picture presented in the original study. Even so, Warwick and Littlejohn describe their work an attempt at replication 'to some extent' (1992: xii); despite the fact that *Coal Is Our Life* had operated with unsustainable stereotypes, it still provided a baseline of sorts with which the results of a more rounded process of data gathering could be compared.

### Reliability

Re-studies that raise questions about the incompleteness of an original study's coverage of a community's population can take steps to achieve greater representativeness by including previously overlooked or marginalised groups. Re-studies that raise questions about the reliability of a previous study pose a challenge that is less easily managed. The classic case of this issue concerns the doubts that arose about the veracity of the baseline fieldwork when Oscar Lewis revisited Tepoztlán in Mexico seventeen years after Robert Redfield's initial visit. The passage of time between 1926 and 1943-8 was an insufficient explanation of the differences between Redfield's (1930) and Lewis's (1951) accounts. Lewis's observation that 'Tepoztecs are not always pleasant to each other' (1963: xvii) notably clashes with Redfield's more romantic image of community. Redfield relied upon far fewer informants than Lewis did, perhaps as few as six (Bell and Newby 1971: 77), and distorted pictures are always a danger with such small numbers, but this is not only a matter of sample size. It, and cases like it, 'remind us time and again of the subjectiveness and onesidedness of social perception' (Brunt 2001: 84). In Lodewijk Brunt's view what this suggests is that it is unrealistic to expect different researchers to come up with the same findings because the research methods used in community research are not standardised ones that bracket out the individual researcher's unique creativity and imagination.

In the UK, the study and re-study about which there has been most discussion of discrepancies are those conducted in Banbury by Margaret Stacey and her teams. *Tradition and Change* (Stacey 1960) and *Power, Persistence and Change* (Stacey *et al.* 1975) were based on fieldwork conducted between 1948-51 (supplemented by knowledge derived through Stacey's residence in the town

from 1944) and 1966-8 respectively (by which time Stacey had moved away but Banbury hosted her three researchers). In principle, the gap was long enough for the earlier study's ideas about continuity and change to be examined empirically, but was short enough for some original study participants to be re-interviewed. Herein lay unexpected difficulties as discrepancies emerged between what people told the re-study team and what was on record about them in the original study material (Savage 2010: 156). This made problematic Stacey's ambition of the re-study achieving the goal of 'filling gaps in the first study' (Stacey and Thompson 2008: 111), because its credibility as a secure foundation for such a project was brought into question. According to Bell (Bell and Thompson 2008: 113) the re-study team were mindful of the discrepancies between Redfield's and Lewis's accounts of Tepoztlán, and the ensuing difficulties over how to treat the original Banbury study's findings were never sufficiently satisfactorily resolved to allow the ambition 'of testing the predictions of the first study' (Stacey *et al.* 1975: 4) about change in local social systems to be realised (Bell 1977). As a result, the contribution to cumulative social scientific knowledge was more modest than had been anticipated, and the case for re-studies being undertaken elsewhere was not advanced.

Other inauspicious starts in the field of community re-studies can be noted. One is Conrad Arensberg and Solon Kimball's (1968) *Family and Community in Ireland* which had its portrayal of County Clare in the early 1930s systematically challenged (Brody 1973; Gibbon 1973), not least its assertion (reflecting the direct influence of Warner), that social life comprised 'an integrated system of mutually interrelated and functionally interdependent parts' (1968: xxx). Another is Art Gallaher's return to 'Plainville' fifteen years after James West had studied the town. This came about because Gallaher had been encouraged by West telling him that 'to the best of his knowledge, with one outstanding exception most Plainvillers had taken his report "with relative composure"'. Gallaher was to discover that West underestimated the number of his critics amongst the townspeople, although this was not a bar to the re-study going ahead. Local people prepared to talk to him included critics of the original study who took issue with the way in which that study had been approached: 'one man, now deceased, but still remembered as "a notorious loafer", was identified by most critics as West's main informant'. This fitted with their assessment that West's study had 'stressed only the negative side of the community' and 'exaggerated their poverty and their backwardness' (1971: 286, 294, 292). Aware that his findings were at odds with West's, Gallaher could attribute this, at least in part, to the fact that he and his wife had developed better rapport with local people than West had, ironically enough as a result of having West's study as a point of reference for their discussions.

Re-studies have thus played a part in exposing what Nancy Scheper-Hughes has described as 'a long (and distinguished) tradition of anthropological ostriches, beginning with Robert Redfield, who weathered the Mexican revolution in Tepotzlan without writing a word about it, lest it disrupt his description of a placid,



harmonious peasant village' (2001: 29). The capacity of re-studies to do this was used as a basis for bringing into question the methodological basis of community studies, and this critique contributed to community studies going out of fashion for a time in the 1970s (Crow and Allan 1994: 13). What followed, however, was a period of reconsideration of this methodology, and the realisation of the potential of re-studies to improve on the methods used in original studies has contributed to the revitalisation of the field. A good example is provided by Warwick and Littlejohn's more systematic treatment of survey data, along with their awareness of the dangers of anecdotal evidence which the original *Coal Is Our Life* researchers had been guilty of relying on too much. Returning to the area of the original 1952-4 fieldwork thirty years later, Warwick and Littlejohn were still able to trace some of the informants from the first study, some of whom suggested that the book's portrayal of miners reflected not only the information that they had provided but also the stereotypes that the researchers had brought with them. In addition, attention was drawn to the one-sidedness that arose from the researchers spending 'a lot of time in the pubs and clubs' and becoming over-reliant on informants who were 'able to keep up an endless stream of anecdotes and information, so long as the beer flowed' (Warwick and Littlejohn 1992: 31-2). The usefulness of key informants in ethnographies is well-known but the information that they supply does need to be treated with caution.

Over-reliance on anecdotes is a criticism that also features in Jennifer Platt's assessment of the work of the Institute of Community Studies. Platt praised the intention 'to combine the strengths of rigorous quantitative method with those of personal involvement and observation in depth', but was critical on many counts of actual research practice, such as the tendency to over-interpret 'personal anecdotes' (1971: 92). The brief of community studies to capture the reality of 'how "ordinary people" lived' (Stacey and Thompson 2008: 109), to 'provide a picture of life of "average citizens"' (Bryson and Winter 1999: 59), does not restrict researchers to anecdotal evidence, and case studies of individuals selected to convey the position of wider groups of people can be much more than anecdotal, as case studies of social polarization in Ray Pahl's *Divisions of Labour* (1984: ch.11) illustrate. Developments in methodological matters figure prominently in Raymond Kent's account of the increasing 'professionalism of sociological inquiry' (1981: 141), and the expectation that re-studies will exhibit greater methodological sophistication compared to the originals is borne out. Similarly, as Savage has documented, the 'protocols involved in interviewing "ordinary" people' (2010: 5) have gained in certainty as social research practice has become more professionalised. In general, data collection has become more systematic than it was, for example, in the first Banbury study, which (looking back) Stacey described as 'amateur' (Stacey and Thompson 2008: 111).

Chris Phillipson and his colleagues' use of social network analysis as 'a practical and flexible method for studying community life' (2001: 27) exemplifies how a re-study can move beyond the methodology of original studies (in this case including *Family and Kinship in East London*) without reliability issues being an

obstacle to arriving at important new findings about social change. Another example of a re-study responding to criticisms of original research practice is provided by Janet Foster's *Docklands* which broadened the research base beyond the 'handful of streets' (Gavron and Mulgan 2007: xii) of Young and Willmott's Bethnal Green to other parts of the borough of Tower Hamlets. In particular its historical narrative goes back beyond a comparison with the 1950s to an appreciation of the longer history of community change in the area, including that history's 'unpleasant habit of repeating itself'. One of Foster's purposes in tracing the history of the East End over two centuries was to demonstrate how easily that history can be distorted through the construction of 'images of close-knit past communities' which on closer inspection turn out to be 'more imagined than real' (1999: 45, 43). Foster's inclusion of photographs in her monograph are another way in which methodological innovation has augmented the power of her study.

### Validity

Photographs also figure in *The Other Side of Middletown*, as part of Lassiter and his colleagues' approach of collaborative ethnography. By working with community members throughout the research process, a different type of knowledge can be generated which it is argued gets closer to the realities of community life than more conventional research conducted by outsiders is able to achieve. At the same time, Lassiter and his team adopted a framework 'to remain consistent with the classic study' (2004: 9). Focusing on the six broad substantive areas of work, home, education, leisure, religion, and community activities that were at the heart of the Lynds' original investigations some eight decades previously makes good sense for comparative purposes, even if social and social scientific developments in the interim period warrant the adoption of revised research questions and methods. It is instructive that work, home, education, leisure, religion, and community activities figure prominently as core themes in many contemporary community re-studies including Bryson and Winter (1999), Charles *et al.* (2008), Dench *et al.* (2006), Foster (1999), Mumford and Power (2003), Phillipson *et al.* (2001) and Warwick and Littlejohn (1992), as does the concern to convey their interconnectedness. There are thus good reasons to believe that developments in methodological and theoretical underpinnings of community studies are compatible with continuity and thus comparability in terms of the substantive areas of community life on which researchers focus.

Such thinking informs Charles and her colleagues' re-study of Swansea. Harris's presence in the research teams that undertook the original Swansea study and the re-study is relevant to the continuity and change that characterised these two investigations. In the original study Rosser and Harris anticipated that future research would follow new lines of investigation because they 'did not explore every avenue in Swansea, and there are of course still many stones that need a careful turning over in future research' (1965: 39). In addition, Harris had in the 1983 edition of *The Family and Social Change* flagged up the availability of new theoretical perspectives based on Marxism and feminism that had not figured in

the original, and the opening chapter of *Families in Transition* extends this list to include theories of individualisation and the concept of social capital. The chapter nevertheless concludes with the useful reminder that a re-study 'by definition asks, wherever possible, the same questions as the original study', and notes the possibility that '[t]he story is the same but the terminology is different' (Charles et al. 2008: 22). They conclude that 'although exact replication of a study is neither desirable nor feasible' (Charles et al. 2008: 41), useful comparisons are still possible and can provide the basis for confident statements to be made about community change.

The second Swansea study provides another important lesson about the ability of re-studies to capture 'community', namely that the relationship between community and place is not fixed. This became apparent because the re-study did not stick rigidly to revisiting the same locations in Swansea to collect ethnographic data as the original study had. Rosser and Harris had undertaken ethnographies in two localities, Morrision and Sketty, but the original plan had been to include more, and so the fact that the re-study has ethnographic material from four localities is in keeping with the earlier project's spirit. Indeed, the prioritisation of this spirit over concerns about precise replication led the re-study team to re-locate their data collection to 'Fairview', 'whose middle-class character is probably closer to that of 1960s Sketty than is the Sketty of today' (Davies and Charles 2002: 4.2). But it is the inclusion of inner-city 'Parkfields' as an additional area of ethnographic data collection that allows the multi-ethnic character of contemporary Swansea to be captured, through observations such as 'amongst the Bangladeshi population, men were central to kinship networks as providers and heads of extended family households' (Charles et al. 2008: 82). Had the re-study team simply returned to Morrision and Sketty their findings would have been much less interesting, given the socio-spatial changes of the intervening period.

Similar points about the risks of following an original study's geographical identification of community can be made with regard to re-studies of Bethnal Green. In their Introduction to the 2007 edition of *Family and Kinship*, Kate Gavron and Geoff Mulgan discuss the enduring influence of this fifty-year-old study, noting that what was essentially a report on 'the lives of a handful of streets' has proved of lasting interest because 'it touched honestly on universal questions of belonging and identity, family and community situated in a real place and a real time' (2007: xii). But Bethnal Green has changed dramatically since the 1950's, so much so that it is worth asking whether researchers undertaking a re-study are actually studying the same place. In the 1960s the old Metropolitan Borough of Bethnal Green was merged with the neighbouring Boroughs of Stepney and Poplar to form the London Borough of Tower Hamlets, and although it was possible for Dench and his colleagues to collect data from electoral 'wards corresponding to the old borough of Bethnal Green', the political re-organization materially affected important aspects of local life, including the allocation of housing which is a core concern in both studies. The study's survey had followed

'as far as possible the original methods and questions used in the 1950s' (2006: 236), but some accommodation had to be made with the fact that the in the interim geographical basis for the collection of population and other data had changed. The value of exploring beyond the area studied in an original piece of research is nicely illustrated by Lassiter and his team venturing to *The Other Side of Middletown*, to rectify the earlier neglect of the town's African-Americans. This was a metaphorical as well as an actual journey, but the map in the Lynds' *Middletown in Transition* attests to the ethnic segregation of the town, as does the discussion in the book (1937: 463).

This point about the spatial fluidity of community was not lost on Willmott, who developed a useful distinction between territorial communities, interest communities and communities of attachment (Willmott 1989). If community studies are understood as attempts to capture interconnected aspects of ordinary people's everyday lives then there is no requirement that they have to be place-based, as studies of occupational, ethnic, religious and internet communities testify. In this context it is interesting that community re-studies continue to be re-studies of place-based communities. This may in part reflect the greater logistical challenges that would face a researcher seeking to re-study a highly mobile community such as Douglas Harper's (1992) 'tramp community'. But it also is plausible to suggest that place is prominent in providing the context in which people's lives are lived, and that the notion of 'local knowledge' (Geertz 1993) conveys literally what many researchers set out to capture. Brunt argues that, despite everything that has been written about the development of new types of communities, 'we have to be careful in concluding that the relationship between communities and place has come to a definite end'. His discussion of how in the Netherlands the distinction commonly made between 'our kind of people' and 'that kind of people' (2001: 83, 85) has geographical reference points is echoed in other studies, such as Tim Butler and Garry Robson's (2003) *London Calling* in which notions of 'people like us' among gentrifiers are very readily mapped.

Another reason for the continuing importance of place in community re-studies lies in the theoretical perspectives researchers bring to the research process. This is almost the first thing mentioned by Warwick and Littlejohn when they highlight 'Alain Touraine, Pierre Bourdieu, Manuel Castells and Raymond Murphy' as writers whose work on 'contemporary social change and conflict' allows them to pose new questions. The notion of 'local cultural capital' derived from Bourdieu shaped how they went about collecting and analysing their data, opting to present their findings for the four localities of 'Oakton', 'Willowby', 'Ashby' and 'Beechthorpe' separately rather than as an undifferentiated collectivity. This allowed them to show that even though all four localities were in the same geographical area as Dennis and his colleagues' 'Ashton' and 'Fullwood', there were significant variations between the four in terms of rates of owner-occupation (between 58 and 83%), age of leaving school (women who had left school by age 15 ranged from 43 to 84%), and proportions of men ever having worked in the coal industry (between 22 and 60%). These localities are all

within one Parliamentary constituency, but they are 'socially and spatially differentiated' and they have distinct 'senses of division and coherence' (1992: xii, 73, 15, 88, 90, 165). On these grounds they argue that community is still very much about where people live and their local social networks. The same conclusion is reached by Lois Bryson and Ian Winter (1999) in *Social Change, Suburban Lives*, their re-study of the working-class Melbourne suburb first investigated three decades earlier, even though the theories informing their analysis are different to those acknowledged as agenda-setters by Warwick and Littlejohn.

### Conclusion

The fact that this article has been able to discuss several recent book-length community re-studies including those by Bryson and Winter (1999), Charles *et al.* (2008), Dench *et al.* (2006), Lassiter *et al.* (2004), Phillipson *et al.* (2001) and Warwick and Littlejohn (1992), as well as numerous other publications that have a bearing on the subject, is an indication that the field of community re-studies has proved more resilient than it might have been appropriate to expect a generation ago. The reasons for this situation include the capacity of community researchers to respond to criticisms of work in the field by showing that it is not necessary for re-studies to be strict replications in order for them to have useful things to say about the contemporary world and patterns of social change. Indeed, it can be argued that as community studies have resurfaced they have done so with a clearer rationale and more robust methodological and theoretical underpinnings. In terms of representativeness, there are good reasons for thinking that more groups are given voice through community research than was the case when whole classes, whole genders, and whole ethnic groups could be omitted. There are also grounds to believe that on a more micro scale Geoff Payne's argument that in community studies '[a]lmost everybody seems to be so nice' (1996: 21) is less likely to stand up to scrutiny to-day, reflecting responses to the criticism that community research was characterised by vulnerability to romanticism.

Regarding the issue of reliability, the case for studies employing methodological innovations producing better results than their predecessors is a compelling one. To take just one example, Young and Willmott lamented that their methods meant that 'for the most part we can only report what people say they do, which is not necessarily the same as what they actually do' (2007: xxvii). In response to this, Cornwell's method of re-interviewing people to get beyond their 'public accounts', accessing their 'private accounts', enabled her to overcome the 'selective and partial' character of the common sense things that people say for public audiences and to get at 'those parts of people's experience and opinions that might be considered unacceptable and not respectable' (1984: 205) in which moral judgements about responsibility and blame are expressed. As Cornwell notes, there are clear links between this approach and Goffman's notions of how people present themselves publicly, illustrating how theoretical developments feed into methodological innovation and vice versa. As a result of such

advances, confidence can also be expressed about the issue of validity, that is, whether attempts to get at the reality of community are succeeding. Partly this confidence rests on the use in community re-studies of an expanding range of mixed methods approaches rather than reliance on a single method. The methodological options have also been extended by improvements in archiving and the number of archives available to researchers wishing to revisit the bases on which the analyses of original studies were made (McLeod and Thomson 2009).

Of course, the research involved in a community re-study is a significant undertaking, and it is salutary to note that the six monographs listed above all involved teams. If *Docklands* (Foster 1999) is included as a re-study (which it arguably is), this would be an exception, but it took a full decade of Foster's time to complete. Michael Burawoy's (2003) notion of 'the focused revisit' does not necessarily entail narrowing down the range of activities that a re-study might involve, but the approach allows for this possibility, and Cornwell (1984) is a good example of what can be achieved within the time and resource constraints of a research degree, as is Fiona Devine's (1992) re-study of Luton. Another lone researcher, Steve Bruce, limited the scope of his (2011) re-study of Banbury to the place of religion in that community, but it also serves to demonstrate that scaled down re-studies can produce valuable results. Teams have the capacity to do more than individuals, but individuals can still follow up usefully on the work of teams. Burawoy's key points about focused revisits are that returning to the site of a previous ethnographic research project is much more common in anthropology than it is in sociology, and that this highlights an opportunity for useful work to be done in the latter discipline. The proviso that he makes is that the success or otherwise of these projects depends on the theoretical purpose underlying such revisits, and the sensitivity of the researcher to the challenges relating to historical processes that revisiting must entail.

Of course, not all ethnographies are community studies, and not all community studies are ethnographies, and this suggests that the reasons for undertaking community re-studies may be shared with other types of research that involves going back. The adults traced in Howard Williamson's (2004) *The Milltown Boys Revisited* do not constitute a community conventionally understood, but the interview material collected from them a quarter of a century on from their troubled youth allows a compelling story of individual trajectories and social change to be told (including chapters on the Middletown themes of work, home, education and leisure). Similarly, Julia Johnson and her colleagues' (2007) re-study of Peter Townsend's *The Last Refuge* revisits care organizations rather than communities, but it conveys the value of charting change, and the methodological challenges of doing so, as well as the opportunities that are opened up by archives making available material from previous studies for secondary analysis. Thirdly, Ronald Frankenberg's revisiting of the text of his community study more than thirty years on from its original publication shares with several community re-studies the ambition to reassess theoretical ideas and

explanations. His conclusion that community is 'too large, too macro' (1990: 188) as a unit of analysis appears to sit uneasily alongside Burawoy's (2000) promotion of *Global Ethnography*, although both are rooted in the desire to understand ordinary, everyday life and its wider ramifications. Further community re-studies will be required to resolve this debate, but the balance of opinion based on the material considered here is that, like the community studies of which they are a sub-set, community re-studies remain 'good to think with' (Morgan 2008).

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