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# Demography's theory and approach: (How) has the view from the margins changed?

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*Around the time that Population Studies celebrated its 50th anniversary in 1996, Susan Greenhalgh published 'An intellectual, institutional, and political history of twentieth-century demography'. Her contribution described a discipline that, when viewed from its margins, prompted scholars in other disciplines to ask the following questions: 'Why is the field still wedded to many of the assumptions of mid-century modernization theory and why are there no critical... perspectives in the discipline?' (Greenhalgh 1996, p. 27). Those questions still arise today. Similarly, Greenhalgh's observation that 'neither the global political economies of the 1970s, nor the postmodernisms and postcolonialities of the 1980s and 1990s, nor the feminisms of any decade have had much perceptible impact on the field' (pp. 27–8), remains a fairly accurate depiction of research published in Population Studies and other demography journals. In this contribution, focusing predominantly on feminist research and insights, I discuss how little has changed since 1996 and explain why the continued lack of engagement concerns me. Demographers still often fail to appreciate the impossibility of atheoretical 'just descriptive' research. Our methods carry assumptions and so rely on (often) implicit theoretical frameworks. Not making frameworks explicit does not mean they do not exert an important influence. I end by proposing that the training of research students should be part of a strategy to effect change.*

**Keywords:** feminist theory; modernization theory; sex role theory; gender; situated knowledge

## Introduction

While planning and working on this 75th anniversary issue, I have often looked back to the first half of the 1990s, thinking about the state of the discipline in the years leading up to the 50th anniversary issue of this journal. Early on, the other guest editors and I discussed what we most admired about that issue, what it would be feasible and desirable to try to replicate, and what we thought we'd try to do differently. While our aim was to put together papers that were as thoughtful and as reflective as those that made up the 1996 issue, our methods and priorities were somewhat different and more resonant, I suppose, with the current zeitgeist. Noting that back in 1996 all the authors were men from the Global North and all were well-established scholars, we agreed to pay particular attention to the politics of presence and decided that representing a wider variety of perspectives would be one of our top priorities. As white scholars who benefit from our affiliations with 'prestigious' universities, we were extremely sensitive to concerns about the epistemic injustice that can

result from hegemonic perspectives put forward by those who are most privileged to speak.

We had hoped to get one or more contributions from scholars working and residing in the Global South. Unfortunately, the scholars we approached and who initially agreed to contribute were, in the end, unable to deliver their papers. We also decided that we wanted to provide a platform for scholars who will be responsible for the direction the discipline takes in future years. We thought carefully about what new issues had emerged over the past 25 years and which emerging stars would be best able to give us an idea of what might be on the horizon. Aiming to deliver both a celebration of *Population Studies* and a critical reflection of the state of the discipline, as well as a hint of things to come—something that looks back and looks forward at the same time—we asked this question: With all the benefits of our 20/20 hindsight, what issues and topics should probably have been given more attention 25 years ago (a time when we ourselves were graduate students or thinking about doing a PhD in demography)?

Of particular interest to me were the reflections on the political and ethical dimensions of demographic research that were being published in the years leading up to and just after 1996. I was also interested in whether and how scholars were grappling with the methodological implications of the critiques of science that were transforming scholarship across the social sciences, often through the work of feminist, critical race, and postcolonial scholars. The 50th anniversary issue was being produced in the years following the International Conference on Population and Development in Cairo, when transnational feminist activists had succeeded in shifting the formal focus of policy from population control to reproductive health. Despite a good deal of (often) vocal resistance to the feminist project—including from some of the authors who contributed papers to the 50th anniversary issue (quoted in Presser 1997, pp. 315–16)—and despite much debate about the substantive effect such a change of ambition would have on the ground, the contributions to the 50th anniversary issue said little about that conference or the potential impact of its platform for action. John Cleland (1996) mentioned the possible effects a focus on reproductive health might have on data collected in ‘developing’ countries, but most of the authors were remarkably silent about this development. There were also no reflections on feminist scholarship or feminist research methodologies and what their distinct contributions might be to understanding demographic processes.

What I personally would like to have seen in the 1996 anniversary issue is the kind of critical intellectual history that Susan Greenhalgh (1996) published in the same year (although not in a mainstream demography journal) or that Harriet Presser (1997) would publish—with a more explicit feminist orientation—a year later in *Population and Development Review*. Susan Greenhalgh’s project described an insular discipline that, when viewed from its margins, prompted scholars in other disciplines to ask the following questions: ‘Why is the field still wedded to many of the assumptions of mid-century modernization theory and why are there no critical—that is, politically oriented—perspectives in the discipline?’ (Greenhalgh 1996, p. 27). The MSc and PhD students who take my Gender, Population and Policy course at the London School of Economics and Political Science often express surprise (and sometimes dismay) that those questions are about as pertinent today as they were a quarter century ago. Greenhalgh’s (1996) observation

that ‘neither the global political economies of the 1970s, nor the postmodernisms and postcolonialities of the 1980s and 1990s, nor the feminisms of any decade have had much perceptible impact on the field’ (pp. 27–8), while in need of a bit more qualification 25 years later (especially if we consider the important scholarly interventions in anthropological demography) remains a fairly accurate description.

In this 2021 contribution, my aim is to establish how much the view from the margins has changed in the last quarter century or so and to explain why I think what never really happened is so very important to address. I will first outline the legacies of modernist and functionalist thinking and then, with reference to the ethical and political implications, make the case for why I think urgent change is needed.

### **Still wedded to many of the assumptions of mid-century modernization theory?**

Writing in 1996, Greenhalgh reported that modernization theory had, in fact, already been ‘heavily criticized’ for several decades and ‘largely abandoned by the mainstream of social science’ (Greenhalgh 1996, p. 27). Demography, in contrast, had failed to make such a break from modernization theory. When demographers thought about social change, they tended to cling to a number of assumptions—many of them background assumptions—that had been shown to be problematic methodologically (Thornton 2001), ethically, and politically (Greenhalgh 1996). Drawing on the metaphor of society as a biological organism, the idea that societies develop through a series of similar stages from traditional to modern depicts the Global North as ‘more developed’ than the rest of the world, and so the adoption of European family patterns is assumed to be both inevitable and desirable (Thornton et al. 2015; Sigle 2021). In frameworks informed by modernization theory (e.g. structural functionalism in sociology), history is understood as a series of punctuated equilibria with huge changes—such as industrialization—requiring institutional adaptation. Viewed this way, Western family patterns were, in the mid-twentieth century, often explicitly assumed to have arrived at the new *industrial* equilibrium and so to represent the ‘pinnacle of family life’ (Cherlin 2012, p. 585).

The functionalism of prominent and influential scholars such as the sociologist Talcott Parsons had,

by the 1990s, been replaced by the more economic but equally functionalist assumption that institutions emerge to solve collective action problems, evolving or adapting to maximize efficiency, much as the invisible hand of competition leads to market equilibria (Sigle 2021). In this way, the idea that the less developed world, the 'demographic Other' (Greenhalgh 1996, p. 27), would inevitably become more like us—and importantly, that this was a good thing—persisted. In retrospect, the idea that mid-twentieth-century social institutions were ever in equilibrium was clearly wrong.

I agree with Andrew Cherlin (2012) that we need to show some historical empathy when describing the oversights and errors in the work of scholars so 'steeped in structural functionalism'. However, I think we can be a bit more critical and ask why these often implicit but highly problematic assumptions could still be detected in the way demographers, in the second half of the twentieth century, depicted and explained social change. Consider, for example, the Second Demographic Transition. A series of 'revolutions'—including a gender revolution—is posited to have disturbed the industrial equilibrium of the mid-twentieth century. Moreover, the theory assumes that the Nordic forerunners can be used to predict the future of the rest of Europe and, indeed, the rest of the world (Lesthaeghe 2010). When the Second Demographic Transition framework was recently critiqued and modified from a gendered perspective, it was not the 'almost unbelievable ethnocentrism' (to quote one of my anonymous reviewers) of the modernization assumptions that underpin it—the idea of stages of development and the corollary assumption that institutions move towards a uniform adaptive or competitive equilibrium—that they challenged. Instead, the authors worked within that framework and suggested that the form and function of families in the past few decades should not be interpreted as an equilibrium, but rather as part of the transition to the real new (Nordic) equilibrium of gender-equal families (Goldscheider et al. 2015). To be fair, the authors were cautious about predicting that the Swedish case is one that all others will move towards, but they did draw, rather uncritically, on conceptual material which suggests just that (Zaidi and Morgan 2017).

Thinking critically about these conceptual frameworks—their implications and legacies—is important because their adoption is not innocuous. They have effects that are disempowering in a number of ways. They impose a blueprint of social change for the 'less developed' societies, and once

'developed' status is attained, they depict further change as disruptive and undesirable. Rather than provide ideas for how to effect change, these frameworks reproduce the idea that once societies attain the new equilibrium—the right 'institutional fit'—efforts to effect further change would be 'dysfunctional', throwing the whole social system out of whack (Sigle 2021).

Even less explicit in these theoretical frameworks is their reliance on the (also long discredited) sex/gender role model in sociology that was, along with modernization theory, integral to mid-twentieth-century functionalist conceptualizations of social institutions and which remains integral to their legacy. The sex role model conceptualizes gender as an individual-level characteristic, produced primarily in the (separate sphere) of families during childhood (Ferree 1990, 2010; Sigle 2021). When social institutions and gender are conceptualized in such a limited way, it is very difficult to even think about how gender relations can change, except through cohort replacement. The only option is to socialize the current generation of boys differently and to hope that their 'lagged adaptation' (Goldscheider et al. 2015; Sullivan et al. 2018) will lead to better outcomes in the next generation (Deutsch 2007). The sex role model also suggests that the problem of gender inequality and its solution rest primarily with mothers (Stanley and Wise 1993). This narrow and overly simplistic conceptualization of gender relegated it to the private, separate sphere of the family, where it could be studied without reference to the institutions of the public sphere and vice versa. By invisibilizing the ways gender might interact with or be embedded in public institutional structures (Ferree 1990; Acker 1992; Riley 1998, 1999; Williams 2010; Riley and Brunson 2018), these frameworks obviated the need to consider the gender system as stratifying the public social world.

I wonder whether the persistence of such a narrow and restrictive conceptualization of gender can perhaps be understood not as a theoretical commitment but a methodological one. We continue to rely on analytic strategies that are well aligned with the simplifying assumptions of the integrated modernization and sex role frameworks. As Joan Acker observed:

The notion of gender (or sex) as a characteristic of individuals or as a social category, which is related to the idea of gender as role and/or identity, was easily adapted to conventional models of investigation in which theories were tested through

examining the relationships between variables. Gender can be used as an independent variable. For example, researchers studying wage determination add the worker's gender, or the proportion female in various occupations, as a variable to a list of other variables thought to determine the outcome—wages. Alternatively, populations are often divided into two groups, female and male, which are then compared on an aspect of gender roles—for example, the distribution of time spent on household responsibilities (Acker 1992, p. 566).

When certain theories fall out of fashion but the methods and approaches associated with them do not, we might find that we are inadvertently carrying some unwanted (and not very explicit) conceptual baggage.

In comparative analyses, the idea of stages of development does important but often unacknowledged—and extremely problematic—work. It makes it possible to assume that institutional variation can be ignored, as long as the contexts being compared are at the same stage of development. The idea that there is one 'right' institutional equilibrium suggests that once this equilibrium has been reached, it is possible to treat the meso and macro levels as more or less similar (as we might expect with human bodies at a particular stage of development) and to invoke the *ceteris paribus* assumption when generalizing from one context to the other. They are not expected to change and, indeed, the background assumption is that change would be dysfunctional (Sigle 2021). When contexts with different levels of development are compared, the assumption that societies progress in a linear fashion through the same stages of development makes it possible to treat a particular set of population measures—and gaps between 'forerunners' and 'laggards'—as valid indicators of the level of development, regardless of context. Similarly, it allows us to assume that those indicators (family size, education levels, women's labour market participation) carry the same meaning regardless of where or when they are measured/observed. Consequently, we can focus on the indicators rather than the processes they represent in order to promote 'progress' and improve lives in 'underdeveloped' countries. This approach can mean that individual behaviours take pride of place while the role of (more expensive) structural change and investment is minimized (Desai 2000; Chatterjee and Riley 2018).

Despite a paucity of empirical support and the compelling critiques of their ethnocentrism (see

e.g. Seth 2014), functionalist assumptions of institutional convergence continue to exert an influence in disciplines that demography draws on, including economics. For example, when David Soskice set out the motivation behind the Varieties of Capitalism model developed in the early 2000s, he described a question that was, in the late 1980s, puzzling his colleagues in the Economics Department at Oxford: Why are we not witnessing a global convergence to the United States (US) model of free market capitalism? (Clife 2014). Nor have those predictions completely disappeared in those multidisciplinary fields of study that, like demography, also draw heavily on the work of economists. Social policy is a good example.

Unlike demography, however, social policy has developed a theoretical literature that has opened up space for alternative ways of thinking about institutional change. The welfare regimes literature explicitly confronted the assumptions of modernization theory—the idea of linear, unidirectional development with convergence towards a single equilibrium (Emmenegger et al. 2015)—and, by providing authors with a new independent variable (a country's welfare regime) to examine, it offered an alternative approach that was compatible with existing quantitative approaches (which previously relied on levels of expenditure, assuming that 'welfare effort' meant the same thing in all times and places). Even those authors who were not interested in and did not understand all the nuances of its underlying theoretical framework could make use of its conceptual contribution in their empirical studies. The welfare regime typology provided researchers with a convenient way to introduce systematic institutional variation as an independent variable (with dummies for each of the regime types), which could be used to test the predictions of persistent systematic differences in policies or policy outcomes using standard statistical techniques. While I am inclined to agree that in social policy, the typology-based welfare regime approach has 'probably reached the point of diminishing returns' (Orloff 2009, p. 330), the regime framework prompted some much-needed and productive theoretical reflections.

The literature that treated welfare regimes as a dependent variable (conceptualizing the source and trajectory of institutional variations that distinguish different regimes) opened up the space for theorizing institutional variety, and once scholars acknowledge that institutions are varied, assumptions of uniform trajectories of development and *ceteris paribus* no longer hold. Consequently, the role and

influence of history and institutions can no longer be so easily ignored. To the extent that gender is acknowledged as 'an organizing principle in all societies ... deeply entwined with other social institutions such as the economy, the state, education or the labor force' (Riley and Brunson 2018, p. 2) and so as a source of institutional variation, it is no longer tenable to treat gender as a merely individual characteristic. Indeed prior to the embrace of the conceptual framework underpinning *The Three Worlds of Welfare Capitalism* (Esping-Andersen 1990), feminist welfare state scholarship was quite independent from quantitative work aligned with a modernization framework (the logic of industrialism), which assumed trajectories of convergence.

It was welfare regime theory's rejection of convergence and the acknowledgement of different institutional combinations and path dependencies that provided an entry point for feminist engagement with mainstream scholarship. Their first efforts focused on demonstrating how gender provided an important source of variation within the three regime types (liberal, social-democratic, conservative), because the core concepts used to distinguish the regimes (decomposition and stratification) were androcentric in their conceptualization and operationalization. Engaging directly with mainstream scholars, these early feminist interventions made visible the 'mutually constitutive relationship between systems of social provision and regulation and gender' (Orloff 2009, p. 317) and stressed its importance.

With his compelling demonstration of the distinctiveness of the institutional trajectory of the North European family model, John Hajnal (1965, 1982) problematized the assumptions of linear institutional development, but unlike Esping-Andersen's (1990) welfare regimes in social policy, this important intervention had a limited impact on the intellectual trajectory of the discipline. Similarly, Robert Frank's (1985, 1988) incisive demonstrations that people do not behave as rational actors—an assumption that underpins demographic thinking and approaches—offered no alternative conceptual framework that could provide a direction for change (see also Kahneman et al. 1991). The anomalies were noted, but research approaches were not revised in a way that integrated this insight and prompted further conceptual reflection as the welfare regime concept did in social policy. As Thomas Kuhn (1962) might have put it, demography remained in its 'normal stage': no crisis, revolution, or paradigm shift took place in response to this intervention.

The Ready, Willing, Able (RWA) framework, which was used to describe what might have

been going on when fertility declined in different European settings (Coale 1973, in Lesthaeghe and Vanderhoeft 2001), could have been used to think about how to model different context-dependent trajectories of social change more explicitly. However, the framework was not widely adopted and to my knowledge it was never evaluated as providing an important corrective to some of the flaws associated with modernization theory. Indeed, a search on Google Scholar returns a very limited number of papers in mainstream population journals that mention or attempt to apply the RWA framework in a methodologically aligned way (e.g. Simonsson and Sandström 2011). A few papers have attempted to operationalize the concepts 'ready', 'willing', and 'able' but treat them as additive and separable determinants (in ordinary least squares (OLS) or OLS-like regressions:  $R + W + A$ ), rather than dimensions which need to be considered in combination and have the potential to create bottlenecks ( $\text{MIN}(R, W, A)$ ) (see e.g. Lesthaeghe and Vanderhoeft 2001). This framework requires more methodological innovation (such as 'fuzzy sets': see e.g. Ciccio 2016) than was the case when the idea of welfare regimes was introduced in the social policy literature.

Similarly, life course theory has drawn attention to the importance of context but has had more impact on literature reviews than on methods. More recently, Christine Bachrach's Population Association of America (PAA) 2013 presidential address suggested ways to conceptualize and operationalize culture. Her network model of culture has, I think, had limited impact on theory and practice, at least in part because it is poorly aligned with predominant methods. Using language which I read as an attempt to put the gap between theory and predominant methods in the best possible light, she wrote that her contribution '*invites* new kinds of modelling' (Bachrach 2014, p. 20) [emphasis mine]. Although she suggested alternative methods that could accommodate her model of culture better, demography and culture continue to be 'reluctant bedfellows' both conceptually and methodologically. At least part of the explanation may once again be methodological incompatibility: 'regression models provide an awkward fit to modeling culture because culture so rarely acts as an exogenous independent variable' (p. 20). I would be inclined to add that it rarely acts as an additive separable variable (as in the fixed effects regression models) (Sigle 2016). In social policy research, the same methods (e.g. regression analysis) could have been used, even as scholars replaced the functionalist linear development assumption (implied by measures of welfare state

spending) with frameworks that emphasized the politics and path dependencies in the design and delivery of welfare state policies and so rejected the idea of institutional convergence (implied by the use of welfare regime dummies).

### Changing the subject but not the story ...

To provide a framework for thinking about the (limited) influence of feminist and critical theories on demography's intellectual trajectory, I will borrow from an excellent contribution by Marjorie DeVault (1996) that I suspect many readers of the 50th anniversary issue might not have come across (despite so many demographers having been trained in sociology). In her paper 'Talking back to sociology', DeVault defined feminist methodology as a commitment to three goals:

1. Feminists seek a methodology that will do the work of 'excavation', shifting the focus of standard practice from men's concerns in order to reveal the locations and perspectives of (all) women.
2. Feminists seek a science that minimizes harm and control in the research process.
3. Feminists seek a methodology that will support research of value to women, leading to social change or action beneficial to women (DeVault 1996, pp. 32–3).

In the years leading up to and following the publication of the 50th anniversary issue of *Population Studies*, research interest in gender issues grew and I think it would be right to conclude that demographic researchers had already begun to do some important work of excavation (see Riley 1999). This largely involved changing the subject of demographic research that had previously focused primarily on one sex: turning attention to women migrants and, perhaps to a lesser extent, at least early on, to men's fertility and sexual health. The importance of this work should not be underestimated.

The following passage from the introduction to a 1975 book demonstrates how men's experiences of migration were seen to provide the representative story, with women as a special case:

Among the migrant workers in Europe there are probably two million women. Some work in factories, many work in domestic service. To write of their experiences adequately would require a book in

itself. We hope this will be done. Ours is limited to the experiences of the male migrant worker (Berger and Mohr 1975, quoted in Pessar 1999, p. 54).

Pessar (1999) also quoted a 1985 study of Cuban and Mexican migrants in the US which claimed that its almost exclusive focus on male heads of families (I say 'almost' because the men were asked about their wives) was justified because studying women would make the project 'excessively complex'.

We can see gendered assumptions more subtly reflected in conceptual frameworks and study designs that presume potential parents—implicitly mothers— assess circumstances and changes only within their own country when they contemplate having children (e.g. designs that introduce time-varying measures of policy and country-specific fixed or random effects). With free migration in the European Union and a political culture of benchmarking and best practice (with reputational and representational effects), it is perhaps not surprising that people living in Poland were using some idea of a European average rather than the Polish past to assess the increased generosity of family policies in recent years (Marczak et al. 2018). However, differences in how parents and migrants are implicitly—and differentially—gendered, may have made it more difficult to immediately see the relevance of insights (such as the importance of reference groups) developed in the migration literature that implicitly assumed migrants were men when assessing different wages and income opportunities.

In the case of fertility, for example, the important intervention by Fran Goldscheider and Gayle Kaufman, published in the same year as the 50th anniversary issue, made a compelling case for bringing men into the study of fertility and commitment (Goldscheider and Kaufman 1996). The work of excavation in demography did not, however, inspire the same critical discussions that could be discerned in other disciplines and which resulted in a recognition of the importance of the second and third goals in DeVault's (1996) paper. In sociology, for example, the work of excavation unearthed not just new findings but a range of conceptual problems so challenging that nothing short of a Kuhnian paradigm shift would solve them (see also Ferree et al. 2007):

The initial period is one of filling in gaps—correcting sexist biases and creating new topics out of women's experiences. Over time, however, feminists discover that many gaps were there for a reason, i.e. that

existing paradigms systematically ignore or erase the significance of women's experiences and the organization of gender. This discovery ... leads feminists to rethink the basic conceptual and theoretical frameworks of their respective fields. The process of paradigm shifting, by which we mean changes in the orienting assumptions and conceptual frameworks which are basic to a discipline involves two separable dimensions: (1) the transformation of existing conceptual frameworks; and (2) the acceptance of those transformations by others in the field (Stacey and Thorne 1985, p. 302).

In Sociology, the 'rethinking' resulted in a much richer and more complex understanding of gender as a primary source of social stratification: a social structure (Risman and Davis 2013) or regime (Walby 2004) operating at the micro, meso, and macro levels. Ferree et al. (2007) described these developments as nothing short of a paradigm change because they involved a rejection of the predominant modernization–sex role frameworks. Demography did not experience the same conceptual crisis and did not embark on the same kind of rethinking described here, perhaps because the papers published in one of the top journals could be 'not very explicit about theory' (Watkins 1993, p. 561). Without much explicit attention to theory and a 'cumulative research record' which is noteworthy for 'the shortness of the list of variables that appear consistently' (Watkins 1993, p. 561), it may have been more difficult in demography than in other disciplines to see the potential for conceptual incompatibility when we 'add women and stir' or, in the case of fertility, we 'add men and stir' (see Sigle and Kravdal 2021). Often the subject was shifted without much attention to how what was going on for that new subject might be different: the same variables and the same methods (aligned with previous conceptual frameworks) were typically used.

When conceptual problems are more visible and the inadequacies and omissions of conceptual frameworks are brought to light, it is easier to see how the perspective of the powerful shapes our accepted understanding of the world. This is important because institutional development is informed and guided by these same frameworks and so shapes the reality they purport to describe. Once it is clear that conceptual frameworks don't just describe realities but contribute directly to their construction, issues of power in the research process come into stark relief. Whose partial and situated understandings of the world are accepted and acted on and so made (more) real? What are the material effects for the more and the less powerful? The insight, and the

questions it prompts, challenge conventional understandings of what research ethics means and what it involves. At the same time, it offers the radical and inspiring possibility that things could be different.

### **Why is the lack of critical perspectives (still) important?**

Critical perspectives invite us to think carefully about the conceptual frameworks we use and the work that they do. As our conceptual frameworks are reproduced (implicitly or explicitly) in policy discourses and policy logics, they have the potential to seep into the public consciousness. In this way, frameworks that were at first inaccurate descriptions of what was going on can become internalized prescriptive scripts, producing (and reproducing) rather than depicting a particular social reality. For example, when we depict childbearing decisions as a cost–benefit analysis and draw attention to the career-related opportunity costs of having children—even as we are advocating for policies that reduce those costs—we should not be surprised that potential parents internalize these discourses (that having children is extremely disruptive and costly for parents and should not be contemplated before they are financially secure and ready) and reproduce them when they offer explanations for their own behaviour, be it delayed first births or smaller families (Marczak et al. 2018).

I would argue that these discourses have constructed the decision makers they purported to describe. And these constructions of subjects are not inconsequential: they exert an influence on what happens (the timing of childbearing and family size) and how it is understood and then responded to. Similarly, in 2001, in his presidential address to the PAA, Arland Thornton posited that the developmental paradigm of modernization theory had been internalized by people who were repeatedly exposed to its discourse of 'developmental idealism' (Thornton 2001). He also drew attention to how the modernization framework was aligned with the method of 'reading history sideways', which Hajnal's work on the family clearly problematized. This has been followed by a subsequent body of research demonstrating how these ideas—once internalized—have tangible, material, and political effects. Research that perpetuates and promotes certain ways of thinking can have a powerful impact, and researchers who do not think carefully about their conceptual frameworks and background assumptions can reinforce and naturalize hegemonic representations and their



effects. Ethical research requires that we are attentive to the ways the worlds we build with our ideas can oppress people who lack the power to challenge us.

We could argue that modernization theory's idea that institutional change is inevitable—that things will eventually improve if only we are patient—creates the very 'stalled revolution' and 'lagged adaptation' that it claims to describe. It provides no resources—no vocabulary and no narrative—that can be drawn on to think about how to effect (or even expedite) change. We see this reflected in a recent study of mothers in the UK who decided to withdraw from paid employment and become 'CEOs of their home and family' (Orgad 2019). Orgad's otherwise articulate and well-informed respondents were at a loss for words when she asked them whether they thought the world would be different for their children. Many expected their children to live more gender-equal lives, but when she asked them to describe how that change might come about, they stuttered, hesitated, and were unable to offer any plausible narrative, eventually concluding that 'it just has to happen'. Other respondents reported wanting their daughters to manage their career expectations and to think strategically about the kind of paid work they could easily combine with their family responsibilities (as most assumed that their daughters would want to have children). For example, they would advise their daughter to be a GP rather than a heart surgeon, a schoolteacher rather than a university professor. When asked whether she would want her son to do the same thing, it was with some embarrassment that a former television producer admitted she did not. These accounts revealed an internalized and disempowered notion that there was nothing they could do but wait and/or adapt to structures that they had no power themselves to change. A lack of language and narrative can be disempowering. The nature of the language and narrative we use in our research is a social intervention influencing how people think, how they behave, and how they understand and explain their circumstances.

While conceptual discussions in the extant literature reveal the ongoing attachment to modernization theory and its elitist, ethnocentric simplifying assumptions, many demographers have instead insisted that theory is not very relevant to the work demographers do. I think Suzanne Bianchi articulated this position well in a recent paper:

Developing theory is a process, often begun when we observe something we do not fully understand. Bengtson, Acock, Allen, Dilworth-Anderson, and

Klein (2005) suggested, 'Theory is the attempt to move beyond the what of our observations... to the questions of why and how what we have observed or examined has occurred' (Bianchi 2014, p. 5).

Demographers often engage in documenting the 'what'—the rates of marriage formation, the divorce rate, fertility rates, and so on (Casper and Bianchi 2002). This rich description can be thought of as the first and essential step in developing theory (Bianchi 2014, p. 38)

The idea that there is an atheoretical world that can be described before it can be explained illustrates how demography has remained impervious to decades of critiques of assumptions that the world can be accessed and described in the same way, regardless of who is doing the observing and when in historical time they are doing it. By presenting work as merely the description of 'disinterested, objective observers' (Greenhalgh 2012, p. 122) rather than knowledge which is constructed using taken-for-granted conceptual and metaphorical repertoires and so 'crafted' rather than revealed, demographers could be implicitly relying on outdated conceptual frameworks without making them explicit and open to scrutiny (by them and their readers).

The following quote by Dorothy Smith illustrates the logic underlying rejections of the possibility of a 'view from nowhere' and the generally accepted notion that knowledge production is 'situated' (Haraway 1988), by which I mean mediated by the social location of the knower:

Riding a train not long ago in Ontario I saw a family of Indians, woman, man, and three children standing together on a spur above a river watching the train go by. There was (for me) that moment—the train, those five people seen on the other side of the glass. I saw first that I could tell this incident as it was, but that telling as a description built in my position and my interpretations. I have called them a family; I have said they were watching the train. My understanding has already subsumed theirs. Everything may have been quite other for them (Smith 1974, p. 12).

She went on to highlight the implications for how the less powerful—those who do not have the privilege of speaking—are represented and understood:

My description is privileged to stand as what actually happened, because theirs is not heard in the contexts in which I may speak. If we begin from the world as

we actually experience it, it is at least possible to see that we are located. There are and must be different experiences of the world and different bases of experience. We must not do away with them by taking advantage of our privileged speaking to construct a sociological version which we then impose upon them as their reality. We may not rewrite the other's world or impose upon it a conceptual framework which extracts from it what fits with ours (Smith 1974, p. 12).

Developmental idealism provides an excellent example, I think, of just such an imposition of conceptual frameworks by scholars located largely in the Global North.

These impositions illustrate how our research approaches—descriptive or otherwise—need to be understood not as ‘a way of opening a window on the world, but a way of interfering with it’ (Mol 2002, in Bacchi 2017, p. 22). It should not have taken me so long to grasp this important insight and its ethical implications. As an undergraduate economics student, I recall reading the work of Robert Frank and others (see e.g. Frank et al. 1993), which showed that studying economics appeared to change behaviour. Exposure to economic frameworks seemed to justify as rational—and acceptable—strategies that were less co-operative and more selfish. Rather than describing the world, as Foucault argued in the 1970s, economists were shaping and constructing it through the imposition of a market logic (Lemke 2001). We see this same economic logic used to describe women's approach to family formation.

Depicting the disadvantage associated with certain social categories (ethnicity, nationality, social class) as resulting from the ‘wrong’ demographic life choices—choices which differ from the ‘correct’ ones that the (more privileged, typically white, middle-class) researcher would have made—is another example of an imposition. Our conceptual frameworks—as well as all the work we do to establish causal relationships between demographic variables (such as early parenthood, non-marital childbearing, or divorce) and poor outcomes—reproduce the idea that the appropriate policy response is to change behaviours by making the ‘wrong’ choice more costly. In a collaborative project with Alice Goisis, we explored how the economic, social, and health implications of persistent racism might mean that efforts to encourage ethnic minority women to attain the (implicitly white) ‘ideal’ age of motherhood could cause harm (Goisis and Sigle-Rushton 2014). In a subsequent

paper, we demonstrated how the ‘meaning’ of marital status varies by maternal nativity and that parental marriage is far more a marker of advantage and resources for the children of white and UK-born mothers than for other children (Sigle and Goisis 2019).

To return to a previous example, conceptual frameworks which present the responsible parent (mother) as one who decides the ‘right’ time to transition to parenthood and policy discourses which emphasize the costs associated with having children (see e.g. Cigno and Ermisch 1989)—ever, but particularly at the wrong time—might shape and construct rather than simply describe and explain continued postponement and persistent low fertility. At the 2013 International Union for the Scientific Study of Population meeting, I was asked to participate in a plenary panel on the future of the family. Presenting my views on the future of the European family, I argued that family policy would become more and more generous, but fertility would remain at low levels as individuals internalized conceptual frameworks and models where strategies of postponement are rational. Such frameworks might also normalize the lower earnings and poorer career prospects of mothers as inevitable rather than a structural problem of labour market institutions. Individual adaptations—such as egg freezing—can then be presented as rational and sensible.

In contrast, in research I conducted early in my career, I attempted to problematize causal interpretations of associations between marital status and poorer child outcomes by appealing to the importance of unobserved heterogeneity (Sigle-Rushton and McLanahan 2002, 2004; Steele et al. 2009). It only later occurred to me that I was reproducing the idea that it would be alright to attempt to make divorce more difficult (Sigle-Rushton and McLanahan 2002; Steele et al. 2009) or to encourage unmarried parents to marry (Sigle-Rushton and McLanahan 2004)—often by making their poor decisions more economically disadvantageous—if only we were presented with evidence that the relationship was causal. I have grown increasingly uncomfortable with the way this analytic strategy perpetuates disparaging racist, classist, and colonialist representations and normalizes intrusive policy interventions as sensible with sufficient evidence.

In a similar vein, the first Rostock Debate on Demographic Change, which took place on 21 February 2006, asked the following question: ‘Should governments in Europe be more aggressive in pushing for gender equality to raise fertility?’. Gerda Neyer's ‘No’ position, subsequently published

in the journal *Demographic Research*, explained how demographic researchers shape and construct the reality they claim to be merely describing. Taking a critical view of the ‘low fertility hype’ in Europe, she reminded us that:

Demographic measures are subject to interpretation and are not immutable facts of reality. It is thus a matter of convention whether fertility is regarded as ‘low’ or ‘high’ or ‘normal.’ In other words, it is demographers, politicians, the media, or other groups of people or public institutions who produce the perception that fertility levels are too ‘low’ or too ‘high’ or ‘normal.’ Likewise, it is they who construct the social, economic, and political consequences of fertility levels by transforming demographic measures into ostensibly negative outcomes for the future. It is therefore necessary ... to look at the images which dominate this discourse, and to uncover the aims of those driving the discussion (Neyer 2011, p. 227).

That Neyer (2011) needed to spell this out for her audience (which she did remarkably well) is, I think, indicative of the ongoing ignorance of and resistance to critical and feminist perspectives in mainstream demographic research.

I have often wondered why a number of important and compelling interventions published shortly before and after the publication of the 50th anniversary issue of *Population Studies* appear to have had little discernible impact on the mainstream of the discipline in subsequent years. With demography described at the time as ‘a science short on theory, rich in quantification’ (Kirk 1996, p. 361) that ‘spend[s] too little time trying to explain and understand, rather than to quantify and to describe’ (Hobcraft 1996, p. 488) and that hasn’t acknowledged that ‘some theory, perhaps barely articulated, must underlie all analysis’ (Caldwell 1996, p. 309), it is perhaps not particularly surprising that the 1996 anniversary issue did not include a discussion of the impact of the post-positive, epistemological interventions of feminist—as well as critical race and postcolonial—scholars (e.g. Harding 1986; Hartsock 1987; Haraway 1988; all cited in Watkins (1993) discussed shortly; see also Mohanty 2003). With the exception of economics, contemporaneous reviews of the state of research in other, closely related, social science disciplines would almost certainly have been incomplete without such a reflection. Although in his contribution, Jack Caldwell (1996) referred to demographers ‘as the inheritors of nineteenth-century positivism’ (p. 311) and noted demographers’ tendency to ‘equate ... statistical

categories, defined in the first place in order to make measurement possible, with the underlying social reality’ (p. 312), as a reader, I did not get the sense that he thought this quirky discipline was going to, or even should, change any time soon.

John Hobcraft’s (1996) contribution, more than the others, didn’t just describe the discipline, but also made the case for change. He went so far as to suggest that demography students should be trained in qualitative methods as a way of improving our ability to explain demographic processes and change. I completely agree. As it was only very recently that *Population Studies* first published the findings of a qualitative study, John Hobcraft seems, in 1996, to have been a bit ahead of the (mainstream) curve. Similarly, that Heather Joshi, whose work on women’s employment has been so important and influential, was, from 1996 to 2005, on the editorial board of a journal described in 2001 as still ‘unreceptive’ to papers on gender issues (Desai 2000; see also Caldwell 1996), suggests that more progressive views and research contributions of members of the editorial board did not result in substantial changes to the discussions and debates that were taking place on the pages of *Population Studies*.

In the 30th anniversary issue of *Demography* in 1993, Susan Cotts Watkins made observations that were similar to those made by Caldwell (1996), but her cleverly crafted intervention—‘If all we knew about women was what we read in *Demography*, what would we know?’—also made a strong, and I think compelling, call for demographers to adopt a more critical perspective. She invited readers to consider the ways our assumptions (in her intervention, our gendered assumptions) underpin the research questions we ask, the way we interpret quantitative evidence, and the way we act on that evidence. Watkins (1993) noted the small number of variables we examine in our analyses (e.g. education) and how our interpretation of relationships between these variables is affected by the assumptions we make about the people we are studying and what we think the variables and categories that we use to distinguish them mean. Her paper has been cited more than 200 times, but I cannot see that her intervention has had a discernible impact, at least on the kind of research published in mainstream demography journals (Williams 2010; Sigle 2016).

We can see Watkins’ influence in Sonalde Desai’s paper, ‘Maternal education and child health: A feminist dilemma’ (Desai 2000). Desai described how measures of maternal education were interpreted by researchers in ways that made sense to them (more educated mothers were able to read the

instructions on packets of oral rehydration therapy and so their children were less likely to die of diarrhoea—something I recall hearing when I was a PhD student) but did not match the lived experiences of the women being studied. Desai expressed concern that quantitative analyses, which are often based on secondary survey data and rely on measures of individual-level characteristics (see also Crimmins 1993), diverted attention from many of the structural interventions, such as improving access to clean water, that would directly promote child health and which individual-level measures, such as maternal education, might be a proxy for.

Describing the 'feminist dilemmas' facing demographers like her, Desai explained why she had not abandoned ship and sought refuge in another discipline: 'This is a field that deals with issues of vital concern to women such as sexuality, marriage, family, employment, and health. Moreover, it is a field which supplies most of the empirical data fuelling the policy discourse, so why give up feminist claims to this terrain? The dilemmas that I outlined above simply increase the challenges and require greater creativity and reflexivity than we have demonstrated so far' (Desai 2000, p. 443). Like Greenhalgh's social history and other critical evaluations of the demographic project (see e.g. Greenhalgh and Li 1995; McDaniel 1996; Riley 1999), this study was not published in a demography journal and so was unlikely to have reached the desks or monitors of many mainstream demographers.

There were, in the 1990s, some opportunities for and tolerance of critical and feminist engagement with the discipline, mostly at demography conferences. At the 1993 annual meeting of the PAA, for example, Nancy Riley presented the paper 'Is feminist demography an oxymoron?' (cited in Desai 2000). Most of these critical and feminist interventions did not eventually find their way into the pages of our flagship mainstream journals. At the 2007 meeting of the PAA, I presented a paper with the title 'Critical and feminist demography' in a session chaired by Susan McDaniel, whose 1996 paper 'Toward a synthesis of feminist and demographic perspectives on fertility' was published in *The Sociological Quarterly*, not a demographic journal (McDaniel 1996). The paper I presented in that session was eventually published—many more years later than I care to admit!—in *The Sage Handbook of Feminist Theory* (Sigel-Rushton 2014). Feminist demographers, it seems, often found that feminist scholars were more receptive to their efforts to introduce and explain the importance of demographic topics than mainstream

demography scholars were receptive to efforts by feminist scholars to act as the ambassadors of gender studies. (As an example, shortly after presenting my 2007 PAA paper, I asked the (then) managing editor if it would be suitable for *Population Studies*; the answer was an unambiguous 'no'. I am not sure it would be such an unambiguous 'no' in 2021, but I am guessing some associate editors and reviewers might question whether it would be of interest to a large share of our readers.) Similar to my description of the research in social policy prior to the 1990s, the result has been the development of a separate body of feminist literature in migration studies (see Ronald Skeldon's contribution to this issue) and a separate body of feminist literature on fertility and reproductive health (see Elspeth Graham's contribution to this issue), neither of which has received much attention in the demographic mainstream.

The marginalization of critical and feminist perspectives from the demographic mainstream is—after so many years—now rather difficult to redress. In putting together this special issue, we wanted to include a wide range of perspectives and issues, and we were thrilled when Rishita Nandagiri—an emerging critical scholar—agreed to contribute a piece on the politics of family planning. One of the reviewers applauded our decision to solicit such an explicitly feminist contribution:

That *Population Studies* is looking for—has invited—this kind of perspective on population/demography is to be lauded. These kinds of critiques have been underway for decades now, and so it is a good moment in *Population Studies* history to publish a piece that looks critically at the population program and endeavor from an explicitly feminist perspective. So I want to begin comments with an explicit point: it is a good time to publish this kind of article (anonymous reviewer).

At the same time, this reviewer cautioned:

Much of the critique of demography has to come from outside the field. And that is a major issue for anyone writing about it. Over the last decades, demography has been unfriendly toward real or deep critiques of its project, including feminist critiques. There were critics who spoke from within the field ... often at demography conferences. But, notably, some of the most cogent critiques were published not in central demography journals, but in journals outside the field who were more willing to allow such debates to happen (e.g. *Anthropology and Medicine*, 2012 volume, edited by Krause and De Zordo; *Medical Anthropology* 2015 volume (34

(3)); Krause 2001, 2006; Desai 2004; Connelly 2008). This presents a problem for anyone trying to write for a mainstream demography audience, because much of that audience may not be familiar with that literature (anonymous reviewer).

### Creating a new critical juncture?

A key contribution of Susan Greenhalgh's (1996) 'social constructionist approach to knowledge production in twentieth-century demography' was to emphasize that 'other demographies could have emerged; other scripts for population studies could have been staged' (p. 29). By implication, this suggests that in the period after 1996, other demographies were *and still are* possible ... In an earlier work, I pondered how the discipline might be different if it had been more influenced by Kimberle Crenshaw's work on intersectionality (Crenshaw 1989, 1991) than by the methods and priorities of microeconomics (Sigle 2016). Like those who came before me, I tried to make the case that we should pay more attention to the conceptual frameworks and background assumptions that underpin our analyses and that we think about how our biases influence the research process and what the impact of our research (especially on those being researched) might be.

I had originally thought I might make similar recommendations in this contribution, but on reflection, I decided against it. I think it is essential that those of us committed to the transformative project (continue to) raise awareness of these issues, both through our own interventions and through a collective commitment to amplify and create outlets for critical voices as conference organizers, journal editors, and reviewers. Nonetheless, I decided I wanted to think a bit more about what else we can do. If the history of American sociology is anything to go by, these voices may end up forming an echo chamber (Burawoy 2005). Despite substantial work on how gender was conceptualized and understood, Ferree et al. (2007) also acknowledged that previously over-simplistic and discredited frameworks continued to be used, even in papers appearing in the top journals of the discipline. To explain why step two of Stacey and Thorne's paradigm shift (wide acceptance across the discipline; Stacey and Thorne 1985) was not yet complete, they pointed to the persistence of scholars who were unaware of these developments, because they had learned the previous paradigm as students and were not subsequently held accountable for their ignorance by the gatekeepers of the discipline. A recent study by

Jena Zarza (2018) showed that introductory sociology textbooks continue to present the previous paradigm to students. Zarza reported that:

A 'Girl Scout' depiction of gender was found across the majority of books, characterized by simplistic definitions of gender, the conflation of gender and women, and the relegation of gender to chapters stereotypically linked to women... Of the definitions of gender available across all textbooks in the sample, the majority was simplistic and dichotomized the concept. This study also found that discussions of gender were most likely to happen in the chapters on Gender and Family (Zarza 2018, p. 112).

The intellectual history of demography suggests that issues of ignorance and gatekeeping would be even greater obstacles than they were in sociology. Even when critical perspectives are presented in the most accessible and compelling way (I can think of no better example than Watkins (1993)), the impact on research practices of already established colleagues has thus far been limited.

At a time when we are thinking about the state of the discipline, how it has changed in the past 25 years, and where it might be going, and also against a backdrop of calls to decolonize the curriculum in universities, it seems like an opportune time to think about intervening earlier and to focus some attention on how we train research students. We need to do more than teach the application of advanced statistical and econometric techniques. We need to encourage our students to think carefully about the conceptual frameworks and background assumptions that are aligned with the methods they use. We also need to empower them to evaluate the ethical and political nature of their research decisions.

How did I come to conclude that issues of training should be addressed? As I was reflecting on the state of the discipline in the early to mid-1990s, I could not help also looking back to what I was doing and thinking at the time of the 50th anniversary issue in 1996. I completed an undergraduate degree in economics in 1993 and went straight into a PhD programme, with little understanding of what the transition from undergraduate student to postgraduate researcher meant or would require. I was learning implicitly, as it was never named or presented to me formally, the predominant research paradigm and the formal and informal rules governing knowledge production that would allow me to contribute to the 'normal science' (Kuhn 1962) of my discipline and of the social sciences more generally. In the second year of my PhD, I was introduced to the Population

Studies and Training Center at Brown University, an extremely fortunate event in my academic career (nothing short of a turning point, to use the terminology of life course theory). About the time that authors were submitting their first drafts for the 50th anniversary issue of *Population Studies*, I was just starting to learn—through the lens of my emerging understanding of economic research paradigms—what demographers ask and what they do.

Echoing what I had already started to grasp as an undergraduate whose training was heavily influenced by the Chicago school of economics, I was also getting the message, primarily from other PhD students in my home department, that the methodological approaches in economics were more rigorous and superior to those of other social science disciplines. Consequently, as an economist who conceptualized the social world as made up of rational actors and market mechanisms that, if left alone, would lead to efficient outcomes, I could explain the social world better than sociologists. My econometric methods would allow me to identify causal relationships (the academic equivalent of the holy grail) better than sociologists could. In other words, while qualitative sociologists and anthropologists could provide valuable descriptions of social phenomena that needed to be explained as somehow rational or efficient, my undergraduate education had already conveyed the message that quantitative sociologists working in the field of demography had little to teach me over and above providing me with questions that I was better equipped to answer than they were. I hope it goes without saying that I came to reject this position as arrogant, chauvinistic, and simply untrue.

Although by the end of my PhD I was already disenchanted with some aspects of economic priorities and approaches, I didn't have the vocabulary or confidence to articulate my unease. In subsequent years, I acquired the concepts and the language that allowed me to start to think critically about how I had learned to make sense of the world and what could be known. I think that my training in economics made me more sensitive than I otherwise would have been—especially in a period where so much empirical work presented reduced-form models, without much attention to the underlying data generation process (Sigle 2016)—to be attentive to how conceptual frameworks (utility maximization models) and methods are aligned. But other aspects of my training made it difficult to put these skills to use, to critique the aspects of the disciplinary culture that I had internalized as part of my academic socialization.

A key turning point for me took place in 2002 when I was asked to take over the core teaching and administration of a degree in Gender and Social Policy (subsequently renamed Gender, Policy and Inequalities), which was offered jointly by the Department of Social Policy and the (then) Gender Institute at the London School of Economics and Political Science. Although I considered myself a feminist scholar because I was interested in addressing, understanding, and combating gendered oppression, I was not at all well versed in feminist research methodologies and, if I am honest, I did not even know what the word epistemology meant. Drawing only on what I read in my undergraduate economics textbooks, I thought positivism simply meant 'not normative'. As objectivity was a measure of good research, positivism was clearly a 'good thing'.

I have learned so much from my generous and supportive—and often very patient—colleagues in the Department of Gender Studies. However, the path I have followed owes far more to serendipity than design. It is just as likely that I would never have had reason to question the epistemological commitments that I adopted as I internalized what it meant to think like an economist and then as an economic demographer. In the absence of instruction about issues of epistemology and critiques of science, our students cannot make an informed decision about the kinds of research they want to conduct and the moral and ethical issues that might be at stake. We should not teach our students the 'right' (mainstream) way to conduct research in demography but instead expose them to the variety of ways scholars think about demographic processes and population issues and give them the space to form their own view. That means asking and expecting our students to justify the conceptual frameworks that guide their thinking, explicitly or implicitly. Just as we expect students to justify their methods as an appropriate way of answering their research question, we should expect students to justify the conceptual frameworks that are aligned with their methods.

The advanced quantitative methods that are more typically attended to as the highest, if not only, training priority are necessary but not sufficient. Additional training in philosophy of science and in social theory should be required as well. As well as doing what we can to amplify the (already) critical voices of feminist scholars, we should also work within our departments and institutions to encourage, if not require, MSc and PhD students to take courses in Philosophy of Science as part of their

methods training. Recent calls for universities to decolonize the curriculum—calls which some universities are seeking to respond to—might provide opportunities to make the case for this sort of radical change (thanks to Rebecca Sear for mooted this idea when commenting on an earlier draft). The decolonization project could provide a critical juncture by prompting questions about the political and ethical implications of how we choose to ‘do’ demography. Giving students the opportunity to see how and why research practices might be unintentionally racist, classist, and colonialist could inspire the extraordinary science that is needed to challenge the social injustices that remain so prevalent in our world today.

At first glance, it might appear ironic that having earlier critiqued the socialization explanation for the slow pace of change in gender relations, I am now suggesting we focus on the socialization of the next generation as a strategy for effecting disciplinary change. An anonymous reviewer of the first draft of this paper made the following insightful suggestion:

It seems to me that some change can happen from within if there is a critical mass of scholars in the field who are working on related topics (in feminist scholarship, for example), and who have the drive to work together, year after year, to present their research at major conferences, publish it in top journals, and support each other by serving as referees, writing ... letters of rec [*sic*], and so forth. It would need to include scholars of different generations based at different institutions in different countries (anonymous reviewer).

I could not agree more. I see my suggestion of earlier intervention as *part of* rather than *instead of* such an initiative. Those of us who are more established in our careers can develop and contribute to the development and delivery of courses which introduce students to critical perspectives. We can take up gatekeeping roles in mainstream journals, funding panels, scholarship committees, recruitment committees, and promotions panels, and we can work together to change the reward structure and make space for a wider variety of contributions in our mainstream journals. As journal reviewers, conference session organizers, and paper discussants, we can (generously and collaboratively, in line with a feminist ethics of care) draw attention to the implicit assumptions that authors use and suggest more critical perspectives be added to the literature review and discussion of the results.

If widespread, systematic changes to the training of all students are unrealistic right now—as

suggested by one reviewer of an earlier draft—a shared commitment to co-authoring with early career researchers and our own PhD students would be a start. I always offer to co-author at least one paper with my PhD students. I do this because I think learning-by-doing is an effective and inclusive approach to training students in research practice. It also provides a point of focus for introducing students to critical perspectives. Expecting students to learn how to do research by ‘osmosis’—by reading research and emulating it—is an approach that favours students with a better sense of what they need to look for and emulate. More often than not, those are the students who have had more exposure to highly educated people in their families and social networks. As someone who lacked this cultural capital, I really struggled to understand what was expected of me. (Had my PhD supervisor, Mark Pitt, not provided me the opportunity to co-author a paper with him, I honestly think I would have dropped out; I nearly did.)

While a collective and multipronged strategy is necessary (the search for and expectation of a silver bullet represents another positivist conceptual legacy that, sadly, I do not have the time or space to get into here), a specific focus on early interventions could help establish the critical mass necessary to effect the kinds of transformative changes that, now more than ever, are so urgently needed in research and policy.

## Notes and acknowledgements

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- 3 We were sorely disappointed that Susan Greenhalgh was not able to accept our invitation to contribute to this anniversary issue, ideally by updating her excellent intellectual history to consider changes over the past 25 years, and it is with great sadness and much gratitude that I pause to acknowledge the contribution that the late Harriet Presser would have been able to make.
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