ARITHMUS

TRANSCENDING METHODOLOGICAL NATIONALISM THROUGH A TRANSVERSAL METHOD? ON THE STAKES AND CHALLENGES OF COLLABORATION

Working Paper Number 1

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Abstract: This paper reflects on the challenges and pitfalls of doing collaborative ethnography in a research project (ARITHMUS) that studies the enactment of populations through statistics. Successful collaboration is essential in order to translate the idea of a transnational field of statistical practices – the conceptual starting point through which the researchers of the ARITHMUS team seek to overcome methodological nationalism – into a corresponding methodology and research practice that transcend nationally bounded case studies. Hence, the question arises as to how we make collaboration work in practice. In the first part of this working paper we explain why we seek to transcend methodological nationalism and why the conceptual starting points of the enactment of a European population and a transnational field of statistical practices require what we call a transversal method. In the paper's second part we reflect on five interrelated pitfalls and challenges of collaboration in order to tease out possibilities for negotiating these to make collaboration work in practice.

Keywords: Collaboration, methodological nationalism, multi-sited ethnography, politics of method, transversal methods

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Introduction (*SS*)

In this paper we reflect on the challenges and pitfalls of doing collaborative ethnography. The reason for this endeavour is that making collaboration work is easier said than done. Diverging research interests, different working practices and disciplinary backgrounds, varying modes of field access or the question of how to produce field notes that are meaningful to others are only a few of the issues that render collaborative ethnography a challenging methodology. While collaborative ethnographies are often proposed as the solution to the challenge of following the trajectories, frictions and connections of increasingly complex social phenomena and processes across a range of sites and scales in an interconnected, 'globalised' world (Marcus, 1995; Ong & Collier, 2005; Tsing, 2005), the practicalities of making collaboration work have rarely been considered.

Yet, collaboration can – for a variety of reasons – fail. This is in particular the case in the social sciences, which (in contrast to the natural sciences) have never really overcome the conception of research as an individualistic enterprise through which the production of knowledge rests on the "interpretive and authorial virtuosity of an individual" (Collier, Lakoff, & Rabinow, 2006, 5). Needless to say, the neoliberal restructuring of higher education and science, which calls on researchers to compete with one another over ever scarce funding and to develop a 'research track record' based on single authored publications, does certainly not facilitate collaborative research either (on this point see in particular: Guillaume, 2014). Given this challenging nature of collaborative research, we – a team of six researchers on a social science research project called ARITHMUS – reflect in this paper on some of the pitfalls and issues involved in doing a collaborative ethnography.[3] The aim is not to compile a set of 'best practices' and ready-made recipes guaranteeing successful collaboration. Rather, we highlight and reflect on some of the questions and challenges fellow scholars may encounter if they decide to embark on a similar odyssey. In this introduction we provide a summary of our overall argument, which we then elaborate in the sections below.

In the context of ARITHMUS the decision to do a collaborative ethnography arose from two key conceptual starting points. That is, our methodological choice – as in the case of any choice – is founded on key premises about our object and subjects of research. For one, it is based on an initial proposition that changes in statistical methods affect the enactment of a European population (Ruppert, 2013). The premise is that statistical methods do not just account for or reflect populations that already exist 'out there'. Rather, statistical methods and the data they produce help to bring populations into being and constitute them as tangible, quantifiable and actionable entities and objects of government. Furthermore, enactment is conceived as happening through the daily and specific work practices of statisticians rather than through legislative and regulatory edicts alone. Amongst other things, and especially as part of the European project, these practices involve national and international practitioners who need to juggle national autonomy, professional independence and European comparability to produce timely and policy relevant European statistics and they need to do this in the context of reduced resources and the related pressure to innovate technologically and methodologically (Ruppert, 2013). These specific practices, forces and dynamics cut across numerous national statistical institutes and international statistical

[3] For more detailed background information on ARITHMUS refer to the project's website www.arithmus.eu.

organisations.[4] Taken together, how statistics are done and a European population is enacted are not determined or given, but the outcome and accomplishment of these innumerable distributed practices, negotiations, struggles, tensions, constraints, and so on.

A second conceptual starting point is that following this dynamic also calls for moving beyond the study of nationally bounded case studies, a research practice that has been described and problematised as 'methodological nationalism' (Wimmer & Glick-Schiller, 2003). Instead, and following from the first premise, ARITHMUS starts from the idea of a transnational field of statistical practices in Europe in which the meaning and force of the national is not given but treated as an empirical question. In this way, ARITHMUS seeks to move beyond the presumption and usage of nationally bounded case studies as quasi naturally given units of research, analysis and theorisation – the very essence of methodological nationalism. Importantly, this transnational field of statistical practices is understood as a transversal field of power in which scales of the local, the national and the transnational overlap and intersect, as we detail below.

How to translate these conceptual starting points into a corresponding methodology and set of field work practices that transcends methodological nationalism is our challenge. In brief, we developed an ethnographic approach that involves multiple sites, a team of researchers, multiple disciplines and research techniques that we conceive of as a transversal method. It is a method that cuts across national and disciplinary boundaries, spatial scales, individual(ised) projects, and standardised or pre-defined research techniques. Crucially, the realisation of such a transversal method hinges on the successful collaboration of a team of researchers during all stages of the research process, from the identification of research themes guiding field work all the way to analysis and writing. Hence, the question how to collaborate successfully sits at the core of our transversal research method.

That in brief are both the premises and corresponding method that we want to reflect upon in this working paper. In the following sections we elaborate on a set of interrelated questions: why is methodological nationalism a problem and how can we overcome it through a transversal method? Why is successful collaboration essential if we want to move beyond the study and comparison of nationally bounded case studies? And how do we make collaboration work in practice in order to be able to write and research about themes that cut across sites defined as 'national'? Asking these questions is particularly relevant in a context in which thinking within the mind-set of national containers is still hegemonic and so pervasive that it keeps on haunting our actual research practices. We will elaborate on these questions in the following sections that have each been written by two, three or more members of ARITHMUS. While this working paper thus constitutes an example of (mostly) successful collaboration, we have nevertheless decided to make visible who of us was involved in the research and writing of which section. This decision acknowledges that to do otherwise – for instance by attributing

[4] The national statistical institutes include Estonia, Finland, the Netherlands, Turkey and ONS, the England and Wales statistical office. The international organisation include Eurostat, the statistical agency for the European Union and the statistical division of the United Nations Economic Commission for Europe (UNECE).

the whole working paper to a collective identity like 'ARITHMUS team' – would make our voices indistinguishable and erase the authors' answerability to their writing.[5]

Finally, it is important to note that this paper was written over a period of 12 months during the field work phase of our project. This had implications for both the writing process as well as the paper's content. Regarding the former, collaborative writing turned out to be a patchy and choppy process since most of us were often scattered all over our field sites across Europe, making it at times difficult to meet and coordinate. Regarding the content, this paper focuses more on field work related issues of collaboration than on challenges that are related to collaborative analysis and writing, such as coding our material in NVivo (qualitative data analysis software).

Readers who are more interested in the conceptual framework through which we try to transcend methodological nationalism should focus on the first, more conceptual part of the paper. Here we explain why we seek to overcome methodological nationalism by elaborating on what is at stake in analytical, epistemological and political terms. Subsequently, we argue that the concept of a transnational field of statistical practices provides a conceptual starting point for accomplishing this but that it hinges on successful collaboration through what we call a transversal method. Readers who are more interested in the pitfalls and challenges of making collaboration work can immediately start reading the paper's second part (starting on page 14), where we reflect on five interrelated challenges in regards to achieving this in practice. These challenges include the need to deconstruct the influential image of the individual(ised) researcher; the unease collaboration may initially generate in relation to established working practices; the practical difficulties of making research findings available to each other; how our socialisation in particular national cultures affects our research practices in terms of field access and available methods in particular sites; and finally, how to account for and work with the differences of ethnographic techniques used at different sites. Given the growing interest in collaborative ethnography to trace social phenomena and processes across national boundaries and spatial scales and to move beyond individualised research, we are convinced that these reflections are of interest to the growing community of researchers that tries to transcend, to paraphrase Judith Butler (2004), these methodological anachronisms that refuse to die.

For a Transversal Method: Moving Beyond Methodological Nationalism (ER, SS)

Methodological nationalism has been challenged within different disciplines of the social sciences. From imaginaries and epistemologies to practices and methods, the containment of people and populations in administrative and political boundaries of the nation has not only underpinned the workings of states but also that of the social sciences. In this section we first summarise how these challenges have been conceived and then outline the possibilities and difficulties of developing an approach that transcends methodological nationalism. We propose the concept of a transnational field of statistical practices and a transversal method

[5] On this note we acknowledge the lead role that Stephan Scheel took in coordinating and writing this paper. We use acronyms of the initials of members of the ARITHMUS team which are: Baki Cakici (BC), Francisca Grommé (FG), Evelyn Ruppert (ER), Stephan Scheel (SS), Ville Takala (VT) and Funda Ustek-Spilda (FUS).

as our answer to this challenge. Finally, we explain why collaboration is essential for this endeavour.

Methodological Nationalism and its discontents: a remake

Methodological nationalism generally refers to a research practice that analyses societies by using nation-states as quasi naturally given, unquestioned units of analysis (Dumitru 2014; Wimmer & Schiller 2003). This is done to the extent that '[t]he nation-state and modern society become conceptually as well as historically indistinguishable' (Chernilo, 2011, 99). The result is an 'explanatory reductionism' which uses the nation-state or one of its main-features to analyse and explain social phenomena (ibid).

This observation already indicates that methodological nationalism is not reducible to a 'superficial problem or a small error' of the social sciences (Beck, 2014, 110). It constitutes a complex 'cognitive bias' (Dumitru, 2014, 9) that 'is a key, if not the key, feature of the history of the social sciences' (Chernilo, 2011, 99). As such, methodological nationalism entails at least three distinct, but interrelated aspects: first, a disinterest in nationalism as a phenomenon worth to be studied which drives, in turn, the naturalisation of the nation-state as the universal mode of political organisation. Secondly, a 'taking for granted that the boundaries of the nation-state […] delimit the unit of analysis' (Wimmer & Schiller 2003, 578). This feature results in 'container-thinking' – a conception of societies as nationally bounded containers and the unquestioned usage of these containers as analytical units (ibid). This container-thinking results, third, in the limitation of social scientific inquiry into phenomena that occur within national boundaries (ibid, 579), which may be complemented by comparisons of nationally bounded case studies.

To underline the complexity of the problematic Speranta Dumitru (2014) distinguishes three versions of methodological nationalism that each require a distinct set of conceptual moves and research strategies in order to be addressed. Criticising the usage of the nation-state as a unit of analysis can refer to at least three logically distinct epistemological biases. First, it can refer to a state-centrism which makes the state the primary angle and principle explanatory variable in the analysis of the social (Dumitru, 2014, 19). Second, it can refer to what Roger Brubaker calls groupism: the tendency to conceive of groups, often along ethnic, national or racial lines, as 'internally homogeneous and externally bounded entities' and 'fundamental units of social analysis' (Brubaker, 2002, 164). A third form of methodological nationalism understands space as a mosaic of national territories with the effect that 'one develops concepts, asks questions, constructs hypotheses, collects and interprets evidence and draws conclusions in a spatial frame that is entirely territorial' (Scholte, 2000, 66). This 'methodological territorialism' (ibid) or 'territorial trap' (Agnew, 1994) continues to shape research questions, theories and debates of entire disciplines, as John Agnew has famously argued in regards to International Relations (IR).

These epistemological biases of state-centrism, groupism and methodological territorialism have underpinned the historical formation and scientific protocols of both social science methods and the state in post war Britain, as Mike Savage (2010) has shown. This connection between social scientific inquiry and statecraft and nation-building is critical to understanding the force and durability of methodological nationalism not only in thought but in practice. In

the UK context, Savage attends to how nationalism requires specific techniques and methods and their consecration to generate legitimised accounts. Especially in an era of de-colonialisation and loss of empire, providing accounts of the nation through statistics such as national unemployment rates was crucial to assess the health of the nation. This was accomplished through methods such as sampling, questionnaires, interviews, and surveys, which took national boundaries as given and were powerful in not only telling about but making up the modern nation. As Benedict Anderson (1991) famously argues, nationalism requires specific practices that can generate an 'imagined community'. For Savage, the sample survey is one such practice built on scientific concepts such as sampling error and confidence levels to generate rational statistical accounts "in which the truth of the nation is guaranteed not by storytelling but through the protocols of science itself" (2010, 189). That is, the connection between nation and society depended on the use of surveys and was a central feature of what later was defined as methodological nationalism.

This brief account of how social science methods feature in techniques of statecraft for constituting, naturalising and knowing the nation underscores how methods are formed in relation to particular contexts, concerns, purposes, politics and advocates that are not only of the academy (Law, Ruppert, & Savage, 2011). Methods are of the social insofar as it is sets of relations that form them. This conception of methods as being of the social helps to explain the persistence of methodological nationalism in its various forms. It equally highlights that overcoming the epistemological biases of methodological nationalism through alternative concepts and methods that do not adopt 'the national order of things' (Malkki, 1995) as the methodological starting point of research will prove challenging.

The challenge is in part because of what Law et al. (2011) call the 'triple lock' of methods. In brief, methods have certain advocates which promote and use them. Secondly, methods produce certain findings and representations, which correspond, third and finally, to the realities that these methods do not only describe, but also help to enact, as we explain below. This "triple lock at work here […] makes it very, very, difficult to know differently, to shape new realities, or to imagine different 'methods assemblages' or modes of knowing" (13).[6] This understanding is illustrated by methods founded on nationalism, which are now the mainstay of the workings of government departments, social science education and training and research funding. These institutional investments and the related predominance of findings and representations based on methodological nationalism and the 'national order of things' have reified the national and made it very difficult to think and do otherwise.

The 'triple lock' of methods points to the second feature of what Law et al. (2011) name the 'social life of method': not only are methods of the social as outlined above; they also are performative and come to make it up. Methods based on methodological nationalism also come to reproduce, circulate and reinforce the national as a territory and people. While narratives and imaginaries of the national are significant forces of nationalism, methods play their role in reproducing particular representations and realities that are bound up with an assemblage of advocates, purposes, practices, technical settlements and commitments. That the findings of methods then come to be the basis of governing decisions further reinforces and reproduces the reality of national territories and peoples. As renowned sociologist Ulrich

[6] We explain the notion of the 'method assemblage' in detail in footnote 11 on page 21.

Beck argues, methodological nationalism produces protocols of data collection and production and shapes definitions of fundamental concepts of modern sociology, including that of society, inequality, the state, democracy, imagined communities, multi-culturalism and, for us Europeans, our understanding of the European Community' (Beck, 2014, 110).

What the work on the social life of methods thus highlights are the epistemological and political implications of social scientific methods and analyses that implicitly or explicitly take the national as given. It suggests, furthermore, that there are no easy answers to the question of how to transcend methodological nationalism especially if, as we have argued, methods are tied up with a complex locked-in assemblage of actors and practices. In the next section we review some of the propositions that have been made to transcend methodological nationalism before making an alternative proposal.

Moving beyond methodological nationalism: A transnational field of statistical practices

We are of course not the first scholars to struggle with the question of methodological nationalism. Several authors provide valuable starting points for moving beyond social scientific approaches that reify the national order of things by organising research around nationally bounded units of analysis. John Urry (2000), one of the most prominent proponents of mobility studies, urges sociology for instance to transcend the assumption that nations are containers of societies. He questions this containment by foregrounding "the diverse mobilities of peoples, objects, images, information, and wastes; and of the complex interdependencies between, and social consequences of, these diverse mobilities" (185). Following Urry, methods are needed that transcend national conceptions and attend to what are increasingly global networks and flows and the "geographical intersections of region, city and place, with the social categories of class, gender and ethnicity" (186).

Whereas Urry (2000, 185) suggests that "a global civil society might constitute the social base of a sociology of mobilities", Beck engages the problematic of methodological nationalism from his famous hypothesis of a 'world risk society'. Accordingly, humans have to negotiate man-made, uncontrollable dangers and hazards, like terrorism or nuclear disasters, against which no insurance is possible and that exceed national boundaries (Beck, 1999, 2002). This diagnosis leads Beck (2014) to propose a methodological cosmopolitanism that starts from the existence of a plethora of transnational phenomena, most notably risks ranging from the environmental to the informational. Isabelle Delpla (2014) suggests in turn to engage in a methodological internationalism that highlights the immanence of the international to the national, most notably in the form of international law and the reliance of sovereign nation-states to be recognised as such by other nation-states. Finally, John Agnew (2017) proposes a geo-sociology of politics that addresses the territorial version of methodological nationalism by starting from the idea "that territories or places are not necessarily state-dependent at all and that spatial interaction links places across space without the necessity of territorial propinquity." In sum, each author proposes, albeit in different ways, to unbound methods and concepts from the national and its imagination as a container by adopting the global, the cosmopolitan or the international as alternative spatial frameworks of research.

However, these propositions do, in our view, not break with what Engin Isin criticises as 'scalar thought'. What all these propositions come down to is that replacement of one 'level' or 'scale' (the national) for another one, most notably the international or the global, which are imagined as spheres or scales above the national. In this way, the propositions cited above tend to remain caught up in scalar thought insofar as they subscribe to an imagination of different scales (the local, the national, the international, the global etc.) as neat, distinct layers that are mutually exclusive (i.e. contagious and non-overlapping) and stacked in a vertical hierarchy (Isin, 2007). Instead of just replacing one scale for another one, we thus follow critical IR scholars who propose a methodological approach that makes the force, relevance and meaning of any scale a matter of investigation by examining how practices operate at a great diversity of scales and sites (Aradau, Huysmans, Neal & Voelkner, 2015). Rather than assuming the predominance of a particular scale, an attention to practices opens up a methodology that operates transversally. Such an approach studies how practices traverse, travel between and connect different sites and scales. It thereby facilitates a clear break with traditional scalar thought because it permits imagining scales as no longer hierarchically layered and mutually exclusive, but as overlapping, nested and topologically interwoven.[7]

A conceptual framework which focuses our attention on practices while simultaneously providing a topological figure that transcends the predominance of the national is offered by Pierre Bourdieu's notion of the 'field'. As it is well known, Bourdieu understands a field as a relatively autonomous social space that is thought of as a dynamic configuration of relational positions that are occupied by various actors who compete with one another over influence and authority in this field. Hence, within any given field each actor tries to maintain or improve her position in relation to other actors through the investment of different forms of capital, which feature both as an instrument and as a stake of struggle. It is the composition and combined quantity of capital, including cultural, economic, social, and symbolic capital that an agent has accumulated, which determines the relative position of that agent within the field (Bourdieu & Wacquant, 2006, 127–128). While most of Bourdieu's investigations concern fields within a particular nation-state (namely France) – ranging from the field of literary production (1996), to academia (2010), bureaucracy (2004) all the way to Bourdieu's seminal analysis of the entire social space as a relational field of dispositions and distinctions (1984) – his concepts have been successfully mobilised by some scholars for the investigation of inter- and transnational fields. This has in particular been the case in international law (Madsen, 2011, 2014) and the emerging discipline of international political sociology (Bigo, 2006, 2011).

In the following we borrow from Didier Bigo's theorisation of a transnational field of (in-) security professionals to sketch out our understanding of a transnational field of statistical practice. This conceptual starting point permits us to transcend methodological nationalism in a more unequivocal manner because it moves us beyond scalar thought. The crucial difference to the propositions outlined above is that Bigo's understanding of a transnational field does not simply undo the force of the category of the 'national' by replacing it with the 'transnational' or the 'global'. Rather, the transnational can only exist through the national, in the form of transnational networks of security professionals (in the case of ARITHMUS principally statisticians) who enter the transnational field as experts of their national states.

[7] See John Allen (2016) for a discussion of topological orderings of power.

Hence, these professionals "play simultaneously in domestic and transnational fields" (Bigo, 2011, 251). The transnational field of statistical practice is, as a consequence, a transversal field of power that cuts across and merges the transnational, the national, and the local into a topological figure that resembles a Moebius ribbon rather than a neat stack of hierarchically ordered scales.

To account for this important feature of the transnational field of statistical practice requires adopting a relational approach that apprehends and analyses practices as relations that connect actors across various sites and scales, instead of conceiving of them as interactions between organisations located at different, mutually exclusive scales. The important point is to begin the analysis with the relation – the practices – instead of starting from a particular actor (like a national statistical institute) or scale (like the national or the transnational) (Bigo, 2011, 235). In this way it becomes possible to follow practices across actors, scales and sites and to understand how they traverse, crisscross, connect and concern various actors, scales and sites. Ultimately, it thus becomes possible to grasp that the same practice can emerge as national in a trans- or international setting and vice versa, just like in a Moebius ribbon, where it depends on the position of the observer to determine whether a particular point is on the inside or on the outside of the strip (on the figure of the Moebius strip see: Bigo & Walker, 2007).

The second important feature of the transnational field of statistical practices that we want to highlight through Bigo's work is that it constitutes a field of struggle. Bigo (2006) understands the transnational field of (in-)security as a terrain of struggles in which various transnational networks of security professionals compete with one another over budgets and influence through the definition of threats and the proposition of particular solutions for their abatement. Likewise, we understand the transnational field of statistical practices as a veritable battleground for what we call – in reference to Mike Savage's (2010) work – 'politics of method'. In these politics of method, statisticians and other stakeholders (demographers, data scientists, domain specialists etc.) struggle over the devices, truth claims, budgets and methods involved in the production of official statistics in order to advance their relative position in the transnational field of statistical practices. These politics of method are not reducible to language games in which the best arguments gain acceptance. They rather include a plethora of material-semiotic practices, devices and acts of demonstration through which statisticians and other stakeholders try to provide evidence for and legitimise their truth claims (on this point see: Ruppert & Scheel, forthcoming). Importantly, these politics of method render the transnational field of statistical practices a dynamic space whose topology and boundaries are the momentary outcomes of the struggles over methods, truth claims, budgets and influence. The second reason why we adopt the notion of the field over alternative concepts like Foucault's notion of the 'dispositif' or that of the 'assemblage' is that it "fits so well with any approach insisting on struggle and change, trying to understand continuities as fragile moments, and analysing […] the emergence of new kinds of practices" (Bigo, 2011, 240–41). Hence, the notion of the field offers a promising conceptual starting point for the ARITHMUS project's endeavour to investigate changes in statistical methods and

enumeration practices and how related struggles play themselves out in what we call politics of method.[8]

Nevertheless, it is important to note that we take up the concept of a transnational statistical field of practices to transcend methodological nationalism through a focus on specific practices that traverse sites and scales and that connect organisations and other actors. That is, our objective is not to do a full-fledged, Bourdieusian 'field analysis' and mapping of the relative positions of old and new stakeholders in the production of official statistics. Nor do we aim to study the 'habitus', forms of capital and relative power position of each stakeholder within the field. As said, ARITHMUS is concerned with changes in statistical methods and enumeration practices, the politics of those methods and the consequences they have for the European populations they enact. The concept of a transnational field of statistical practices is an opening for us to examine what kinds of orderings are being done through specific practices that traverse, connect and operate across various sites and scales. Yet, this conceptual starting point still requires a corresponding method that can transcend methodological nationalism in our daily research practices. How then can we translate this concept of relations and struggles in the making of population statistics into a method that does not fall back into methodological nationalism and the thinking in and study of nationally bounded case studies? Just because we start with a strong conception does not simply translate into a method that transcends the epistemological biases of methodological nationalism in practice. How can we trace this transversal field of power relations and relational practices?

Studying a transnational field of practices: Transversal methods and collaboration

There are of course many different methods through which this could be done. The social science repertoire includes a number, such as discourse analysis to trace and follow concepts, logics, arguments, problematisations and so on in reports and documents. Others include surveys of key actors to inquire about how they conceive of the power relations involved in the making of an object. These two examples already highlight that there is no inevitable match between a conception of an object and a particular method. At the same time, all methods are bound up with particular assumptions and premises about their object of investigation as we note below. Bringing these two points together, what then matters is to grasp how a methodological choice does not simply stand apart but is active in the making of an object to be researched. This performative understanding of research methods, that they help to bring into being that which they try to study, describe elucidate and represent, is an

[8] There is a third important feature of the transnational field of statistical practices that we adopt from the works of Bigo and Bourdieu which is its conception as a magnetic field that is held together by a shared habitus of statisticians. This shared habitus is the outcome of similar professional socialisation and career trajectories as well as common interests and practices (Bigo, 2006). It also features what Bourdieu has called 'illusio', that is a common sense of what is at stake in the production of official statistics and the shared interests to maximize once influence and capital within a field of professionals who compete with one another over the definition of what official statistics are, what kind of criteria they need to fulfil as well as the best practices and methods to produce them. But since this feature of the transnational statistical field is not relevant for the arguments we want to advance in this paper, we will not elaborate any further here.

epistemological starting point of ARITHMUS.[9] In this view, all methods (discourse analyses, surveys, focus groups, expert interviews, participant observation etc.) involve researchers who generate representations in relation to putative realities being described. They also feature often tacit assumptions about the context within which an object is formed, the people, institutions, organisations, and so on. Such explicit or implicit assumptions are impossible to escape and for these reasons any method can be said to be part of the enactment of the object under study: representations ('findings'), realities and contexts are not separate but part of methods. We thus need to attend to the effects of our own methods not as technical instruments but part of what we enact, that the specific choices we make (or don't make) will shape what we discover.

So what kind of method is needed to study (and enact) a transnational field of statistical practices as we have conceived it? We have already established in the previous sections that the study of a transnational field of statistical practices requires understanding practices as involving relations that traverse and connect various actors and scales. To make relational practices the starting point of research is reflected in a core assumption of ARITHMUS that it is through practices that population statistics are made and a European population is enacted. 'Enacting' signifies the contingent, situated and practical efforts involved in simultaneously representing and bringing populations into being. This involves not only what is said, but also what is done. In other words, it requires to study the multiple practices that involve "practical knowledge, informal processes, and improvisation in the face of unpredictability" (Scott, 1998, 6). For us, this calls for following, observing, studying and detailing the daily, practical work of statisticians that is usually out of public sight: their debates, struggles, tensions, discourses, techniques, material devices, logics, rationalities, and so on. Such an ethnographic approach has been conceptually and methodologically well established in STS (Bowker & Star, 1999; Latour & Woolgar, 1986; Law, 2009; Lury & Wakeford, 2012; Mol, 2002). Following such an ethnographic approach, we need to develop methods for following how population statistics are made through the practical and situated efforts of statisticians across a range of moments, scales and sites that make up the transnational field of statistical practices. In brief, we need an ethnographic methodology that follows practices across situations, scales and sites that are not conceived a priori as national, global or international.

Our answer to this very practical problem is what we call a transversal method in at least four senses. First, it is traversal because it aims to follow statistical practices across a range of scales and sites. In brief, it is transversal because it is multi-sited and cross-scalar in a classical sense (Marcus, 1995). Second, the method is transversal because practically it requires a team of researchers. Practices occur across multiple sites and scales, ranging from national statistical institutes to international meetings, conferences, and task forces, thereby exceeding the capacities of an individual researcher. It calls for a team of researchers who work collaboratively on shared questions and concepts and protocols for sharing and combining research findings. Such an ethnography, and the research practices and techniques it involves, are not transversal and collaborative in the sense that they dissolve the distinction between the researcher and the researched (Lassiter, 2005). Rather, the method we propose is transversal in the sense that it breaks with the traditional conception of ethnographic research

[9] This draws on the works of STS scholars Annemarie Mol (2002) and John Law (2004) and the work on the 'double social life of methods' (Law, Ruppert & Savage, 2011; Ruppert, Law & Savage, 2013).

as an individual(istic) enterprise, which "rests on a myth of *sui generis* intellectual production" (Collier et al., 2006, 1), a point to which we return in more detail in the second part of this paper.

Yet, multi-sitedness also implies that our method is transversal in relation to the specific research techniques it comprises. Instead of relying on a pre-defined set of research techniques (such as interviews, focus groups, shadowing, participant observation, auto-ethnography, provocations etc.), a team of researchers doing a multi-sited ethnography calls for a pragmatic mix of research techniques. The reason is simple: the opportunities and types of practices to follow vary from one field site to the next. What research techniques can be mobilised in each site depends on a variety of factors, ranging from the mode and degree of field access granted to the language capacities, training and discipline of the researcher. For these reasons, a transversal method breaks with what we might call 'methodological purism'. What a transversal method embraces and does instead is a pragmatic mix of heterogeneous research techniques that can be adjusted to the requirements of a particular field site, an approach, that has also been advocated by Pierre Bourdieu (Bigo, 2011, 245).

Fourth and finally, the method is transversal because it challenges the boundaries of particular academic disciplines. Put differently, the method is transversal because it is trans-disciplinary in the sense that it seeks to actively transgress and dissolve the boundaries between scientific disciplines. It does so by rearticulating their particular histories, literatures, methods and perspectives as a holistic approach that engages a set of shared problems and research questions which cut across the concerns of a particular discipline, such as the identified need to overcome methodological nationalism.[10] Again, the reason for this is rather self-evident. Because a multi-sited ethnography has to – at least in the context of ARITHMUS – involve a team of researchers, each team member will inevitably bring different skills, interests, disciplinary backgrounds, research cultures, literatures, experiences and domains of expertise, which will certainly shape their research techniques, interpretations and so on. Additionally, the variety of practices to be followed – from the highly technical to the political – calls for researchers with different and complementary skills. For ARITHMUS, different disciplinary backgrounds and formations certainly create tensions, but these are of a productive nature as researchers are forced to explain and make explicit their tacit assumptions, basic premises and routinised working practices. For ARITHMUS, a project situated in a sociology department, this translated into a research team with backgrounds in anthropology, sociology, computing, political science and international relations (IR) who previously worked in fields of study ranging from border and migration studies to labour, science and technology studies (STS) and surveillance studies. Since doing a collaborative ethnography requires sharing research findings and conceptual work, the method must also

[10] It is thus precisely the 'transgressiveness' of our method in relation to the techniques, disciplines, knowledges and literatures it mobilises which makes our method 'transdisciplinary'. This is in line with recent discussions which regard transdisciplinarity as going beyond multi- and interdisciplinarity. The former draws on knowledge and techniques from different disciplines but leaves the boundaries between them intact. Interdisciplinarity, in turn, tries to establish and synthetize links between disciplines, but nevertheless remains within the framework of disciplinary research (for a detailed discussion of the differences between the three see: Alvargonzalez 2011).

be transversal insofar as it involves translation of concepts across disciplinary boundaries as well as of findings gained through particular research techniques.

What these features highlight is that for us collaboration is essential to a transversal method that cuts across national and disciplinary boundaries, spatial scales, individual(ised) projects, and standardised or pre-defined research techniques. Moreover, collaboration emerges as a multi-faceted and complex challenge as it concerns all stages of the research process, from the identification of research themes to analysis and writing. It therefore involves a wide range of negotiations and stakes that concern various questions, ranging from rather banal sounding issues like the kind of platforms and research tools we want to use (repositories, software, databases etc.), or the degree of standardisation of working practices and common understanding of concepts that is needed to make collaboration work. It also concerns more complex issues like questions of authorship or how to share research findings through field notes that are meaningful and understandable to each other. In other words, successful collaboration is an achievement that requires constant investment, coordination, orchestration and negotiation and which, consequently, also features misunderstandings, disagreements and (at least temporary) failures. Hence, the question of how to collaborate successfully sits at the core of the endeavour to transcend methodological nationalism through a transversal research method. In the second part of this paper we therefore focus on collaboration to reflect on some of the questions, pitfalls and challenges we have encountered so far in this endeavour.

Collaboration? What do you mean? On the pitfalls of doing collaborative ethnography

In the following five sections that, taken together, constitute the second part of this working paper, we reflect on five issues that the endeavour of doing a collaborative ethnography has raised within the ARITHMUS project. These challenges concern the need to overcome the myth of the individual researcher as the source of knowledge, the unease that collaboration may initially trigger through the need of each team member to reconsider routinised working practices, the difficulties of making research findings available to each other, the question of how our socialisation in particular national cultures affects our research practices in terms of field access and available methods, and finally, the challenge of how to account for and work with the differences of ethnographic techniques used at different sites.

1 Beyond the individual project: towards an ethic of care (ER, FG, FUS)

> This model – that of the 'individual project' – rests on a myth of *sui generis* intellectual production. The individual project model assumes that interpretive and authorial virtuosity is the mainspring of good work. (Collier, Lakoff & Rabinow 2006, 1)

Arguably all research and writing is in a broad sense collaborative and never the creation of an individual researcher. Researchers are subjects who are not independent of the inspirations and muses of other researchers or insights and knowledge produced by peers and broader fields of scholarly work. Yet, the heroic or influential researcher is an imaginary that

still haunts academic production, in particular in ethnographic field work and writing where, more often than not, “a complex cultural experience is enunciated by an individual” (Clifford, 1983, 120). However, as Donna Haraway (1997) famously stated it, research is situated and a view from somewhere. For us the challenge is thus to disentangle our multiple situations as researchers: within different conceptual and theoretical repertoires of disciplines (e.g., politics, sociology, anthropology, science and technology studies, computing); within different methods with their specific histories, trajectories, theoretical foundations; within diverse field sites of statistical offices with their always varying configurations, demands, constraints; and within a collaborative team that creates demands and expectations and pulls and pushes research in particular albeit negotiated directions. Each of these situations are modes of collaboration in that they make demands upon us and have consequences for what we enact through the project.

Yet, one of our challenges is that intellectual production still rests on the ‘individual project.’ But this is not only a myth as suggested in the opening quote; it is also constituted through practices such as the training and making of the social science researcher. From forms of recognition and cultural capital to mechanisms of competition, evaluation and ranking such as the UK Research Excellence Framework, researchers occupy subject positions that are individualised and entrepreneurial. They are products of social, cultural, political and historical situations as Bourdieu has argued in his concepts of professional fields and forms capital (Bourdieu, 1984, 2010).

That said, and at the same time, intellectual production is moving to greater ‘openness’ and sharing of not only research outputs but also data. Yet this changing regime is still firmly placed in the historical tradition of individual authorship and struggles over attribution, plagiarism and copyright. To be sure, the notion of ‘author’ persists and continues to be privileged ‘in the history of ideas, knowledge, literature, philosophy, and the sciences’ (Foucault, 1998). Like methodological nationalism, it is not easy to abandon in part for the reasons noted above. We highlight this to emphasise that our method of doing collaborative ethnography is part of these tensions between what is collective and what is individual research. Here we draw on a few resources to identify what we call an ‘ethic of care’ in doing collaborative ethnography.

The quote at the beginning of this section comes from the work of a group of social scientists who, in the first decade of this century, sought to innovate techniques of ethnographic collaboration through the Anthropology of the Contemporary Research Collaboratory (ARC). Key to their approach was a critique of the dominant model of academic production in the social sciences that focuses on individual work and the heroic researcher. Their critique of the individual project led to the search for a collaborative project of “concept development, collective reflection, and shared standards of evaluation” (Collier, Lakoff, & Rabinow 2006, 1). An opportunity to experiment with this approach was taken up in relation to the formation of biosecurity, a complex field developing across numerous sites including scientific labs, public health organisations, security think tanks, the U.S. military, international organisations, etc. The extensity and heterogeneity of these sites could not be studied following the traditional model of the single ethnographer in the field and thus, they argued, the object demanded collaboration and an approach quite different from the individual project model.

Based on this initial formulation ARC went on to experiment with how to do such collaboration which in no small part involved ongoing reflection on the intellectual and practical challenges of not only rethinking how knowledge is generated but also how it is credited. This initiative has been taken up and referenced in later work such as the International Collaboratory on Critical Methods (Aradau & Huysmans, 2013) and the CRESC Encounters Collaborative (2013). Both of those initiatives well document both the longstanding debates and concerns about collaboration and offer specific examples of the challenges that a critical approach encounters. The principle focus of the latter, however, is on examples of individual projects that sought to collaborate with others in and outside of the academy. As for the former, the concern is with collaboratories as spaces of collective concept development across dispersed sites, people and activities through which a variety of smaller projects can be bound together by "key problematisations – e.g., critical methodologies – without requiring a massive coordination effort to reach a cohesive collective voice" (Guillaume 2014, 202).

There are considerable differences between ARITHMUS, ARC and the collaboratories outlined by Guillaume. Beyond collaboration on concepts, problems and methods, ARITHMUS involves doing and sharing ethnographic research data about transnational statistical practices that involve relations between sites and actors. As such, collaboration is not only that happening through our encounters, meetings, discussions, and collective writing with each other, but in the making and sharing of field notes, observations, reflections and interpretations (we discuss the issue of sharing field notes below in section 3). This is different from collaborating on large externally funded projects, where researchers typically undertake individual projects or case studies. There are of course exceptions such as in large scale quantitative data collection projects or co-authored papers, but the sharing of ethnographic field work observations and notes and doing transversal analyses rather than comparative case studies are not.

Collaboration through a transversal method gives rise to many practical challenges that we elaborate below, but here we want to focus on the ethical callings that it engenders. While solutions such as agreements on authorship and recognition are possible as Guillaume notes, they cannot capture all of the different situations and nuances of attribution that arise during the research process and especially analysis, interpretation and writing up. Attributing ideas, concepts, interpretations and data becomes problematic and difficult especially as the project extends over a long period of time and will continue after it has officially ended. In addition to practical solutions we thus propose that a transversal method calls for an 'ethic of care.'[11] This proposition has many inspirations from especially within the field of feminist technoscience, such as in the work of Karen Barad (2007) and Isabel Stengers (2010). It is, however, in particular Maria Puig de Bellacasa's (2012) work that we draw from. She takes up Haraway's (1988) understanding of situated knowledge to capture the embeddedness of thought in "the ideas, notions or affects" that nourish our thinking (Puig de la Bellacasa, 2012, 202). She calls this 'thinking with care', which involves recognising the collectivity of knowledge-makers, however loosely defined, that make up our thoughts. 'Care' is understood, at once, as "a vital affective state, an ethical obligation and a practical labour"

[11] We draw here from a policy paper of a research project that defined an 'ethic of care' as that which attends to relations of connectedness and interdependence between persons and data. It calls for an attentive concern for the social relations of data composition (Socialising Big Data Project, 2015).

(197). Thinking with care thus implies, rather than disregarding the specificities of specific authors and their contribution, to engage in 'connected thinking and writing' that explicitly acknowledges "the collective webs one thinks with, rather than using the thinking of others as a mere 'background' against which to foreground one's own" (202).

What Puig de la Bellacasa calls for is care of relations themselves which feed and maintain our ideas and thinking. It is an ontological ethic that imposes the responsibility to act in ways that account for and are accountable to our relationality. If knowing is to be ethical then it must take care and address the situated relations through which it has emerged. This is an ontological rather than moral argument; it attends to the dependence on the relations and labours of others to "create, hold together and sustain" (Puig de la Bellacasa, 2012, 198) social worlds that is not a choice but a condition of knowledge.

Practically, what this calls for – throughout the whole research process – is being attuned to how our thoughts and ideas are related and connected in ways that may not be immediately accessible to us. Taking care is thus not to just do proper citations but to acknowledge and be reflexive about how our relations and connections are also part of our method. This is best exemplified through two vignettes of situations we have encountered. The first involves how we sometimes introduce and narrate our situation to our research subjects. Francisca often introduces herself at Statistics Netherlands as being part of a team working in various countries in Europe. Talking about another site such as the Turkish Statistical Institute, for instance, becomes a mode of doing field work across sites. That some of her research subjects know other team members also has a role in connecting her field work situation to other sites. Because one of her objects of study is the innovation lab at Statistics Netherlands (where experiments with Big Data take place) she often mentions the journal Big Data & Society. While the journal is not strictly part of ARITHMUS, Evelyn is the Editor; referring to the journal demonstrates how both she and the team operate as part of larger network of relations. The journal is also a useful introduction not only for establishing credibility, but because it communicates our sensitivity to and interest in various issues of concern to her research subjects. Such referencing has also supported negotiating access to various sites such as her attendance at the Statistics Netherlands and University of Twente Data Camp, which in part also occurred as a result of a concurrent and not coordinated set of email communications between Evelyn and a statistician at Statistics Netherlands. Some topics of conversation also travel between different team members and research subjects; an example is the Caribbean Netherlands, which has been part of different combinations of conversations between Evelyn, Francisca and different statisticians in the Netherlands and elsewhere. That is, through a topic both researchers and field sites have been transversally connected. Rather than an independent researcher Francisca's subjectivity, status and legitimacy are that of a connected member of a team. She generates reactions from her research subjects by connecting her work to that of other researchers and field sites and certain topics travel and conjoin her work to other field sites, team members and research subjects. Her field site becomes an 'attachment site' (Haraway, 2007) and her field work performatively establishes relations between sites and other researchers.

A second situation. Francisca has been in contact with Stephan about a project to write about the Caribbean Netherlands and Estonia. The two come from different disciplinary backgrounds: Francisca orients more to STS and Stephan to politics and migration studies. Stephan sensitises Francisca to national migration statistics and their schemes of classification

and how they are interpreted and analysed within his discipline and opens her up to that literature through readings shared on a collaborative bibliographic referencing platform and to conferences she might otherwise never attend. Conjoining both though is the translation work they do between their disciplinary backgrounds and interests and the central thesis of ARITHMUS that methods and data make a people. When Francisca is at her field site she carries these insights and the awareness of disciplinary sensitivities and some of the theoretical resources of Stephan and other colleagues who are part of the project.

In these ways collaboration does not simply provide a greater volume of data about practices involved in the formation of a transnational field of statistics. Controlling for time, language, financial and other possible differences, the research done by a single researcher would not yield similar results (on this point see, in particular, the next section). Collaborative ethnography is not simply the sum of the work of individual researchers with different backgrounds working at different field sites. Rather, it is about relations amongst each other such as those identified above. This can be extended to include matters of intellectual trust, agreements on shared concepts, the influence of informal and formal contact, lines of authority for project decision-making, negotiations over authorship, discussion and argumentation, and experimentation. Rules and agreements alone cannot address these; rather, an ethic of care calls for acting responsibly in light of particular experiences and actual situations that are not external to but part of the many relations that compose our ideas, thinking and the data and writing that we come to produce.

2 Working with other people's categories (BC, FUS)

What does it take to make collaboration work in practice? Apart from the epistemological and ontological dilemmas of establishing a common understanding of concepts and their meanings (Levinas, 1987), the question remains, of how to manage intergroup politics when working on different topics across different field sites with varied modes of access (Kimble, Grenier & Goglio-Primard, 2010). Hence, the question of how collaboration works in practice is both a methodological and a political one.

In this section, we focus on the example of working with other people's categories in software to illustrate the conflicts, disagreements, and politics of collaboration in practice. Through a modest auto-ethnography of the unsettling and mundane moments of collaboration via software, we argue that collaboration is a complex endeavour involving constant investment, coordination, negotiation, and the will to learn from misunderstandings, disagreements and failures. Intergroup politics and other group dynamics arising from technical skill, theoretical knowledge, hierarchical positions within the group, and the backgrounds of researchers all influence how collaboration unfolds.

Collaboration requires establishing a common ground, and our past experiences, future expectations and personal preferences combine in unexpected ways to create challenges, conflicts and disagreements. The starting point for any common ground involves agreeing on categories and what these categories stand for, along with how to apply these categories to the kinds of data we work with. As a team, we work with a variety of documents, field notes, academic research articles, bureaucratic reports, and legal reports, among others. Working with a large volume of heterogeneous data requires systematic and careful organisation. We

use two software packages to systematically categorise the documents we collect during our research: Zotero, a bibliographic database, and QSR NVivo 10, a qualitative analysis software package. Early in the project, before starting to collect documents, we agreed on detailed guidelines for labelling our documents and organising them into primary and secondary literature, field work notes, memos and the like. However, these initial common grounds were constantly debated and contested when we started working with them.

We explore two moments of methodological contestation in collaboration to exemplify this point: unsettling moments and mundane tasks. Unsettling moments are where the established practices of an individual researcher become disturbed while she tries to accommodate the working practices of others. The state of being unsettled refers to when the researcher feels the need to shape, alter or outright change her own working practices to adapt to those agreed by the other team members. In one sense, the establishing of common categories becomes a minor form of 'torque', defined by Bowker and Star as the friction that appears when an individual's biographical trajectory, memberships, or location are mismatched with a classification system (1999, 223), as individual researchers are compelled to submit themselves, willingly or unwillingly to the categories and practices of others, by changing their own practices. For example, when we created classification categories in NVivo, we tried to keep the categories simple to avoid increasing the workload of team members, while allowing enough detail to categorise the collection sufficiently for later analysis. We created different classification categories for primary and secondary data; field work notes and bureaucratic and legal reports respectively. We also came up with attributes to further classify the documents into sub-categories and to provide some descriptive analysis of our data. Although these were established after discussions at multiple team meetings, once we started the actual categorisation practice, various unsettling moments emerged. Whereas some of us started categorising our documents early on, others waited to do their collections, creating an imbalanced document load with respect to our field sites. Moreover, the amount of categorisation each member performed on uploaded documents to the software varied, sometimes out of uncertainty about the categories, and sometimes due to unfamiliarity with the software. A brief survey into how the software was used during the first 12 months of the project showed that the majority of us still had a large backlog of documents not yet uploaded or not yet categorised in NVivo. We also discovered that some of the categories we initially agreed upon were redundant as none of us actually used them. Practical problems with the software, such as slow performance, difficulty in accessing the data from outside the university campus network also contributed to this unsettling aspect of collaboration. It sometimes divided us, between those who had already committed to the software and hence wanted others to also upload and categorise their documents, and others who were unsure of the eventual utility of the software and postponed committing to it until a later stage in the project.

The second moment, that of mundane tasks, is best exemplified through tracing the repetitive categorisation and classification work that all of us perform in our roles as researchers. Although these repetitive tasks are usually invisible when working individually, they attain a degree of visibility in collaboration because of the need to sustain a degree of standardisation to be able to share and access the collected documents. For example, the bibliographic software Zotero requires a number of fields to be filled depending on the entry to be added to the database. The practice of entering bibliographical details involves several mundane tasks, such as changing the capitalisation of titles, removing personal tags, avoiding

unnecessary attachments, and assigning references to the correct collections. Failing or postponing these mundane tasks would not only result in confusion over whether certain texts have been added to our database, but also consume additional time, and further unsettle other team members when they try to locate documents or write up research findings. The list can be expanded further with other technical and mundane tasks that we rarely discuss but have to perform for the sake of collaboration and shared experience, to ensure that everyone can find what they need. In an individual project, these tasks can be postponed indefinitely, but in a collaborative project the seemingly trivial tasks become both visible and essential to the functioning of the project.

Both unsettling moments and mundane tasks can be considered as a by-product of the software systems that we use in our project. They facilitate collaboration by making it possible to store and analyse large collections of ethnographic material, but they also shape how we collaborate through the functions they provide and lack. In other words, both software packages perform as 'boundary objects', originally proposed by Star & Griesemer (1989) as scientific objects which inhabit several intersecting social worlds while satisfying the requirements of each, in our case serving as an interface between different researcher practices, paralleling the boundaries of different communities of practice (Koskinen & Mäkinen 2009; Star 1999). They provide workable sets of categories and classification systems while remaining robust enough to maintain a common understanding across researchers (Koskinen & Mäkinen 2009). At the same time, the software packages lead to unsettling moments and require mundane tasks when working with other people's categories. In doing so, they also reveal intergroup politics (Kimble et al., 2010), which might otherwise remain hidden. Even when categories are established in negotiation through consensus within the team, those who are more familiar with the software or the classification systems find themselves arguing for the usefulness of the systems while others wait to see if the approach is likely to be beneficial before committing to it. This negotiation forms an integral part of collaboration, and for this reason, collaboration requires transversal methods. The negotiations are challenging to conceive of a priori, and they are shaped by the individual researchers' backgrounds, research interests, as well as their own interpretations of the demands of collaboration.

3 Lost in translation? Field notes as catalysts of collaboration (FG, FUS, SS)

Doing collaboration requires learning to work with the working practices of others, and in collaborative ethnography this includes producing and working with the field notes of other researchers. But this is easier said than done. One main issue is whether what an individual researcher thinks as meaningful about her field site is also regarded as relevant by other team members. There are also other important questions: what sort of things get lost in translation (linguistic translation, as well as across disciplines and personal interests)? How do we work with the unsettling feeling of exposure when we share our field notes with other researchers and present them for their use?

In this section we reflect on an experiment we conducted to answer two such questions: first, how comprehensible and meaningful are our field notes to other researchers, provided that they have limited, if any, background information about the field site and topics we study? Second, how do we make sense and relate to each other's notes in order to go beyond a

nationally bounded case study approach and build theoretical and empirical links between our field sites? We focused on these two questions to tease out and construe possible issues and difficulties of working with the field notes of others and identify possibilities to deal with and negotiate potential frustrations, misunderstandings and confusions. It is important to underline that we saw this experiment as a starting point to address these questions and issues, rather than a method for finding a prescription.

The experiment was structured around team members' reading the field notes of three researchers working at three different field sites, loosely revolving around the topic of migration. The selected field notes differed in style, length, formatting and detail. The first field note was a bilingual text, with an English abstract and notes written in the native language of the researcher and the field site. The second field note was an almost verbatim transcript of an (unrecorded) interview conducted in the native language of the researcher and the field site, translated by the researcher. The third field note was a detailed summary of an interview with the researcher's comments accompanying it. The third researcher conducted his field work in English, but this was not the spoken language of his field work site.[12] We limited the duration of the experiment to two hours and allocated this time to reading and discussing these three texts. Four pages of text (up to a thousand words each) were selected from each field note. Team members had ten minutes to read through each field note and subsequently we had a twenty minute discussion on each text. At the end of the experiment, we had half an hour for a general discussion on the challenges and opportunities working with other researchers' field notes create for our collaborative ethnography.

One of the main outcomes of our experiment is that team members focused more on the content of our notes than individual styles of note-taking. Despite the format differences of the three texts, team members noted that they were "able to understand almost everything". Instead of discussing the comprehensibility of other team members' field notes, participants (including ourselves) questioned the author of the field notes, asking for clarifications about the topic or additional information. They also related the ethnographic material to their own field sites and offered possible interpretations and theorisations of the material. In the final discussion, a consensus emerged that exercises like these would help us facilitate dialogues between and amongst field sites and enable co-production of ideas, theorisations of the ethnographic material beyond a nationally-bounded case study approach.

This answers the first question: how comprehensible and meaningful are our field notes to other researchers, provided that they have limited, if any, background information about the field site and topics we study? Instead of trying to impose a standard style cutting across all field notes, team members rather treated the texts as entry points that facilitate a dialogue and collaboration on interesting aspects of the topic of study at hand.

This observation makes the second question of how we make sense and relate to each other's notes in order to go beyond a nationally-bounded case-study approach all the more relevant.

[12] The first field note is Francisca's, the second is written by Funda, the third by Stephan. We return to the theme of the researchers' positionalities in the field in terms of language and familiarity in Section 4.

Our experiment pointed out two answers to this question. First is the realisation that our field notes operate as much as catalysts for collaboration as information providers. Rather than inhibiting and complicating collaboration, it is precisely the necessarily subjective style, incompleteness and – from the standpoint of desiring a full understanding – imperfection of our field notes that facilitates collaboration between us. Our field notes initiated a dynamic that was nothing but collaboration in action: the imperfection and incompleteness of field notes instils curiosity in the readers and the urge to know more, thus prompting them to ask the author for additional information, contextualisation, and clarifications. In this way the field notes facilitate dialogue and co-production of ideas, interpretations, theorisations of the ethnographic material which might result in co-authored working papers and other outputs. In other words, our field notes function as catalysts of collaboration precisely because they are not immediately accessible and fully understandable for team members.

Second, theory helps us to look beyond the particularities of our own field notes. In our discussions, theory was part of a 'method assemblage' (Law, 2004) that helped us relate to each other's field notes.[13] For instance, one of the texts we read gave information about the recordings of deaths in an NSI. After reading the text, we asked the other team members what they would do with these notes. The ARITHMUS researchers first gave brief instrumental answers ("I would go back to you, I would have some questions to ask and be excited about giving you suggestions to ask more"), but these answers were quickly followed by a discussion about what this means for subjectivity and the state. As one researcher commented: "The dead don't need the state anymore, but the state needs them". Another team member followed up: "Content-wise I got intrigued by death rates. It is not because the state needs you after you die, but […] it is more like the culture is in the data. I would like to know more about why they move people. Death is also about bordering the population."

Given the prominence that Michel Foucault's work has gained across all disciplines and fields of study in the social sciences it was probably no coincidence that concepts of Foucault's 'tool-box' feature heavily in the discussions of our interdisciplinary team. One thing we share despite our different disciplinary backgrounds and diverse research interests is an appreciation of Foucault's work on biopolitics (2003, 2009, 2010). This shared understanding of a particular theoretical approach helps us to relate to and make sense of each other's field notes. We attribute this connective quality of theory to its functions to abstract beyond the individual case and to ascribe meaning to empirical data. Making sense of empirical data involves developing particular narratives with and on this data. The crucial point is that these

[13] With the notion of the 'method assemblage' Law seeks to underline that methods do not operate in isolation as techniques that permit researchers to extract data on particular research objects. Rather, methods are performative and help to enact the very realities they are meant to study and describe because they are part of wider assemblages that comprise material artefacts and devices, a corpus of established theories, a political economy of knowledge production, certain communities of practice and other stakeholders, laws and regulations, institutions, regimes of truths and so on. This why methods have a 'double social life': they are part of the social worlds they are meant to describe and they help to constitute these social worlds, as we have explained in the first part. Hence, method assemblages are "enactments of relations that make some things (representations, objects, apprehensions) present 'in-here', whilst making others absent 'out-there'' (Law, 2004, 14), thus enacting and strengthening certain realities while occluding and marginalising others. It is then "the emphasis on presence that distinguishes method from any other form of assemblage" (84).

narratives facilitate dialogue between team members, irrespective of whether they provoke disagreement or agreement. In this way, we incite and inspire each other with interpretations and theorisations of the ethnographic material, as the above example illustrates. This reminds us that theory, method and data are intertwined in what we have called with John Law a method assemblage. And it is this interwovenness which explains why theory helps us to connect our field sites to each other.

The first conclusion that we draw from this small experiment is that variability of styles of taking field notes does not necessarily constitute a problem for collaborative ethnography; and that standardisation of working practices is not an absolute necessity to be able to work with each other's notes. This finding has important implications for ethnographic studies like ours: rather than investing too much time and energy on the standardisation of working practices, our experiment suggests that it is more productive to tolerate the unique styles of individual researchers as these provide rich materials through which we can debate, contextualise, actualise and situate our collaboration. This finding might sound contradictory to the claims made earlier on the need for some level of standardisation in categorisation and organisation of documents (see Section 2 on working with other people's categories). However, the point made here is different, as the issue of interest is the content of our field notes, and not how we classify and 'sort out' our field notes and other documents in order to assure that we can find them again in vast database containing thousands of files. And regarding the content of field notes, the experiment revealed that despite heterogeneous styles of doing field notes, the latter offer valuable entry points for dialogue and further collaboration between team members.

Here it is important to underscore, once more, one rather surprising outcome of our experiment: our field notes operate at least as much as catalysts of collaboration as they function as information providers. Or, to put it in less conceptual terms: the main function of our field notes may not so much reside in the provision of immediately accessible and fully understandable information on our field sites to other team members, but in the facilitation of dialogue and collaboration between researchers. This was also reflected in the questions that our experiment sparked on the practicalities of collaboration. For instance, how do we account for the workload some of our clarification requests might bear on the producers of field notes? What happens with our collaboratively produced material after the ARITHMUS project ends? What if a team member interprets the material of another team member in a way the latter does not agree with? And at what point will a team member become a co-author if her field notes are used in the work of another team member?

It was our field note reading experiment that sparked these questions, questions that pertain to what we may call the 'micro-politics of collaboration'. And again, a viable answer to the last question (which is maybe the most crucial one in regards to the political economy of the academy where publications function as a currency to be amassed on a CV) was implied by the incompleteness, subjective style and 'imperfection' of our field notes. As Evelyn, the principal investigator (PI), noted: "If you are seeking clarification from someone this might be a litmus-test for engaging in co-authorship." Hence, our necessarily subjective and always incomplete field notes may not only function as catalysts of collaboration but also as facilitators of co-authorship, arguably the most intimate and intense form of intellectual collaboration.

4 Practicing a transversal method within and against the national order of things (SS, VT)

This section reflects on how the 'national order of things' (Malkki, 1995) haunts and challenges the endeavour of ARITHMUS to transcend methodological nationalism through the conceptual starting point of a transnational field of practice and a transversal method. As we have indicated in the introduction, thinking within the mind-set of national containers is still hegemonic, not only among social scientists, but also, and even more so, in our field sites and among research participants. It is therefore no miracle that the 'tyranny of the national' (Noiriel, 1991) keeps on haunting our traversal method and actual research practices in various ways. For instance, each of us has varying degrees of familiarity with the national cultural contexts in which our field sites are located, and this inevitably has an impact on the types of access and methods we have at our disposal in our field sites.[14] Moreover, in our field work, we are – to varying degrees – confronted with practices and devices that work to enact the very methodological nationalism that we want to transcend, the most striking example of this being of course the fact that national statistical institutes (NSIs) constitute social phenomena as national phenomena through the production of national statistics (Dumitru, 2014, 9). Through short interviews with each team member, we wanted to reflect critically on the ways in which the 'national order of things' – in form of national cultures, language capacities as well as the methodological nationalism of our research participants – tends to challenge and undermine our attempts to move beyond the methodological nationalism.

The idea to conduct interviews with team members emerged after we had discovered that we were all very differently positioned in relation to the social and cultural contexts of our respective field sites. Whereas three of us have been socialised in the national contexts of our field sites (of which two had spent long periods abroad), Baki, the researcher covering the statistical institute of England and Wales (ONS), has only recently moved to the UK. Stephan, the researcher in charge of field sites in Estonia, was not only unfamiliar with the national-cultural context of his field sites, but was also not in command of Estonian.[15] Finally, Evelyn

[14] By problematising how the (non-)familiarity of researchers with the national-cultural contexts of their field-sites affects their research practices and possibilities we do not want to invoke a reading of 'national cultures' as a homogenous, stable and clearly bounded entities which are 'rooted' in particular places. It is precisely such a territorialised understanding of (national) culture that underpins some forms of methodological nationalism as well as the 'national order of things' (Malkki, 1992). Hence, transcending methodological nationalism requires to study and write against such an understanding of (national) culture, to paraphrase Lila Abu-Lughod (1991). We thus understand 'national cultures' as historically contingent enactments that cannot be thought independently of the emergence of territorialised nation-states as the dominant form of political organisation and the related build-up of specific administrative systems, the homogenisation and suppression of dialects to create a national language or the invention and maintenance of 'national' traditions, histories and particularities through specific schooling and education systems which, taken together, help to bring into being a particular national culture that in turn nurtures the belief to be a member of a particular imagined community (Anderson, 1991).

[15] It should be noted that this was unintended and the result of a necessary change in field sites that occurred after researchers had been hired. Estonian is considered as one of the most difficult languages in Europe. To learn Estonian to a level sufficient for conducting interviews and observations would have required at least one year of intense, immersive learning. The project's funding did,

covered two international statistical organisations, the United Nations Economic Commission for Europe (UNECE) and Eurostat, the statistical office of the European Commission. Yet, it was precisely this varying positionality of researchers in relation to the (national) socio-cultural contexts of their field sites that offered us the opportunity to explore, if and how the national order of things shapes research practices in order to reflect, in a second step, on how far it is possible to transcend methodological nationalism through a collaborative ethnography.

To address these issues, we asked team members to describe their positionality in relation to the socio-cultural context of their field sites. More specifically, we wanted to hear about situations in which this positionality plays a role in their field work and about instances where it proved to be advantageous or disadvantageous in terms of field access and methods available. Furthermore, we wanted to hear about other factors that may shape team members' practices, experiences and possibilities during field work (the full questionnaire is attached as Appendix to this paper).

What the responses we have received indicate, in sum, is that the degree of familiarity with the national-cultural context of a field site indeed has an impact on research experiences, practices and possibilities. Interestingly, the advantages mentioned by those who were familiar or very familiar with the national culture of their field site mirrored the disadvantages cited by those lacking this familiarity: the three of us who have been socialised in the national contexts of our field sites stated that this familiarity implies the ability to understand nearly everything we hear or see written at our field sites. Furthermore, this familiarity seems to include a certain 'savoir-faire' or 'practical knowledge' on how to deal with the local bureaucratic culture and how to approach prospective research participants.[16] This also entailed a sense of what we feel we can ask and do at our field sites in particular situations without putting off or offending our participants. One of us noted that due to this familiarity, "people relate to you a lot easier".

These advantages largely mirror the disadvantages which were cited by those of us who lacked this degree of familiarity with the national-cultural contexts of their respective field site. A lack of practical knowledge of how to negotiate with officials at a field site was cited as the most important factor shaping research practices and opportunities. Stephan reported, for example, that he was at one point during his research at Statistics Estonia frustrated by not receiving replies to his e-mails, only to learn later on that a low response-rate is common in the local public sector. After that, he adapted his strategy to get field access by contacting people by phone and by writing shorter interview requests more frequently.

However, having a different national background was not only described as being entirely disadvantageous. In fact, instead of speaking of (dis)advantages, it is better to speak of the varying challenges related to one's attachment and familiarity with a field site. Baki and

however, not provide enough resources – in this context primarily time – to start learning Estonian to a sufficient level.

[16] What we mean with 'savoir faire' here is close to James Scott's notion of 'mētis' i.e. a form of local practical knowledge that refers to a set of practical skills whose mastery is based on practical experience and which are therefore "exceptionally difficult to teach apart from engaging in the activity itself" (Scott, 1998, 313).

Stephan stated, for example, that they experienced their status as relative 'outsiders' at ONS and Statistics Estonia also as liberating insofar as it would grant them the freedom to ask questions that might otherwise have been perceived as 'stupid', politically radical or outright offensive. Francisca, who had been living in the Netherlands before she joined ARITHMUS, spoke about the challenges of sharing a national background with her research subjects at Statistics Netherlands (CBS). An example she cited was doing research on innovation. Her Dutch background lead her to read the institutionalised eagerness at CBS to stand out as innovative as "part of Dutch national bureaucratic discourse", noting that those employing the discourse are often also aware of this. Having encountered similar discourses in previous research, she noticed that she was initially "so used to it that you don't question it [the narrative] any further". She therefore had to push herself to study innovation as being variably enacted in practices both in- and outside this discourse. Moreover, her Dutch background meant that she, at times, employed more diplomacy because she felt that questioning the self-representation of CBS as innovative might be considered as offensive by research participants, or as asking 'stupid' questions about self-evident practices.[17]

Furthermore, it is important to note that most of us consider other factors as equally or even more important than (non-)familiarity with the national-cultural context of our field sites. While female researchers emphasised the importance of gender-relations, if not sexism, others mentioned age and professional as well as disciplinary boundaries as important factors having an impact on their field work possibilities and experiences. What these answers illustrate is the well-established point that we as researchers are always situated in our field sites in multiple and intersecting ways in terms of class, 'race', gender, age, disability, sexual orientation, nationality, professional background and so on (Abu-Lughod, 1991; Clifford, 1986; Gupta & Ferguson, 1997; Haraway, 1988). Consequently, our multifarious and shifting entanglements in a web of intersecting power relations of locally and temporally varying sexisms, racisms, capitalisms, ableisms, nationalisms and ageisms cannot be captured by simplistic insider/outsider binaries. Rather, it appears as more adequate to think of our positionality in terms of differential inclusion to highlight both the continuity and simultaneity of multiple, interwoven processes of inclusion and exclusion as well as the productivity of the multiple differences and tensions generated by them (Casas-Cortes et al., 2015, 79–80). Thus, the knowledge we produce can only be a partial and situated knowledge, as Haraway (1988)

[17] While we lack the space to expand on this point, it is important to note that this example highlights how deeply methodological nationalism is engrained in everyday practices and thinking, thus shaping field-work encounters and interviews on a very fundamental level. What this 'banalisation of methodological nationalism' (Dumitru, 2015) – in this case a form of methodological nationalism that we have described as 'groupism' in the first part of this paper – underscores is that any attempt to engage in methodological transnationalism is challenged by the everyday methodological nationalism of research participants who consider the researcher either as an 'outsider' or as a 'member' of their respective imagined community. The assignment of this in- or outsider status in everyday interactions – including those of fieldwork – comes with the allocation of certain roles and scripts that the person concerned (in this case: the researcher) is expected to perform as well as certain things she is expected 'to know' (and not to ask). Overcoming this mundane methodological nationalism of research participants requires (at least by people who are considered as 'members' of a given imagined community) nothing less than to disappoint the expectations of a research participant who assumes a mutual familiarity with the researcher because the participant considers her as a 'compatriot'.

has famously argued, because researchers are embodied subjects who, rather than seeing everything from nowhere, always come, look and speak from somewhere.

From acknowledging that we are all inescapably positioned in multiple and shifting ways in relation to our field sites follows, in turn, that "positionality is always part of any ethnographic work", as Evelyn aptly noted about studying international statistical agencies in her written answers to our questions (on this point see also: Gupta & Ferguson, 1997).[18] Our positionality should therefore "not be problematised but turned into a strategy where difference opens up the possibility of knowing and understanding differently", she added. This suggestion seems to offer, in fact, the most viable way forward to transcend methodological nationalism in practice: if the limits and challenges implicated by a researcher's non-familiarity with the national-cultural context of her field site do not constitute insurmountable obstacles, but also imply potential advantages in terms of research possibilities, as we have shown above, then it seems indeed a viable strategy to make the differences and tensions implied by our differential inclusion in relation to our field sites productive for opening up alternative ways of knowing. To put it in less abstract terms: instead of bemoaning language-barriers and non-familiarity with our field site's national administrative culture, we suggest apprehending the implications of being a 'non-national' not just as disadvantages, but as a starting point for inventing and experimenting with alternative modes of doing ethnographic field work.

In Estonia, for instance, Stephan was dependent on a local research assistant to negotiate language-barriers and his non-familiarity with the national administrative culture. This dependency on the support of a research assistant resulted in a productive collaboration and interesting research constellations (on this point see also: Cons 2014). The employment of a research assistant also implied that the researcher could follow-up on discussions of important working groups in a more continuous way since his research assistant continued attending meetings and writing reports while the researcher was not present in Estonia. The collaboration with a research assistant also proved to be productive for the very construction of the 'field' to be researched as it forced the researcher to reflect on his research priorities at a very early stage of his field work in order to 'induct' the research assistant who needed to know on what topics and issues he was supposed to focus on when taking notes of statisticians' discussions. What this example illustrates is that non-familiarity with the national-cultural context of a field site does not impose insurmountable obstacles for ethnographic research but rather invites experimentation with alternative modes of doing field work.

Regarding the endeavour to transcend methodological nationalism a research strategy that tries to make a researchers' differential inclusion productive (rather than bemoaning the difficulties it implies) constitutes, in fact, the best way forward. This becomes apparent if one acknowledges that the alternative – to only deploy researchers who have been socialised in

[18] It should be noted that the researcher responsible for international statistical agencies (our PI) was an 'outlier' in regards to the answers she provided because she did encountered multiple national cultures in her field sites. Hence, she noted that "[t]he subjects I research are not much differently positioned as me – they work within multinational, multicultural contexts, have diverse and varying language capacities, and have varying experience in statistics, computing, censuses, governing, and so on. Where we differ more is in years of experience working within the EU and national census contexts, and disciplinary and professional experience and interests."

the national-cultural context of the field site to be studied – constitutes a form of methodological nationalism in itself, a form that we have described as groupism in the first part of the paper. This is the tendency to conceive of groups along national lines as "internally homogeneous and externally bounded entities" and "fundamental units of social analysis" (Brubaker, 2002, 164). Hence, we call for a transnational research practice that, rather than buying into territorialised understandings of culture and identity which underpin the national order of things, transgresses and destabilises the boundaries and limitations implied by nationalism in its various forms and disguises. In other words, our method also has to be transversal (and transgressive) in relation to the boundaries that are implied by the territorialised logic of national belonging.

5 Diversity in data collection methods (BC, FG, VT)

We have used the term ethnography to cover a variety of methods that we deploy across the sites we study. These include semi-structured interviews, open-ended interviews, participation in conferences, and following mailing lists. The methods we are able to use also differ depending on the site, some restrict access to meetings, while others allow us to shadow our research participants throughout the day. Our engagement in some sites is limited due to language differences as well, requiring us to seek assistance from others who can translate, and in others we are required to be accompanied by an employee during our stay. Furthermore, the ways we produce data at each individual site change over time. Building a relationship of trust with research subjects takes time, and once those relationships have formed they change quite significantly what is possible in terms of ethnographic methods.

Social science methodologies often account for diversity in methods of data collection by arguing that each method is likely to add more data about a singular phenomenon. Triangulation is a good example.[19] Frequently used as a 'passe-partout' for justifying variety in data collection, it ascertains peers and readers that a phenomenon has been accurately measured by combining two or more methods. Along this mode of reasoning, data are 'integrated', 'added up' or serve to highlight different parts of an otherwise single phenomenon (Moran-Ellis et al., 2006). Using examples of collaboration in our research project, we explain why we do not resort to triangulation.[20] We argue that triangulation is incompatible with a transversal method because it does not allow for taking into account the team as a transversal entity that produces difference.

[19] Some usages of 'mixed methods' may serve as another example of combining methods to establish the characteristics of a single research object. In the textbook version, the epistemological claim is that some types of data (often quantitative) establish the phenomenon at the macro level, while qualitative data indicate the mechanics at the micro-level. We acknowledge, however, that many different usages of mixed methods are practiced, incorporating a variety if epistemological standpoints. Our critique is directed at using these methods in collaborative projects without reflecting on the implications of collaboration.

[20] We acknowledge that triangulation may imply varying epistemological viewpoints. Again, our critique is directed at using it in collaborative projects without reflecting on the implications of collaboration.

Using different methods at different sites enacts our research objects, statistical practices, in different ways. Our first example concerns the consequences of using different methods of data collection at different sites. At Statistics Estonia, Stephan attended meetings (represented through his research assistant) and performed follow-up interviews. One of his initial impressions was that statisticians in Estonia talked about data, but rarely seemed actually to work on them. In his case, doing an ethnography using interviews and team meetings enacted statistical practice as collaborative work, a more sociable activity than he imagined it to be, although it is possible that this impression is influenced by the particularities of his field access. Baki shadowed some key statisticians and other members of ONS in the UK in relatively high positions within the institutional hierarchy. Therefore, his impressions point to the relevance of the work of more senior ONS employees for innovation and the production of statistics, enacting statistical practice as a hierarchically structured work led by important individuals.

The ideas described in the examples are highly provisional and do not do justice to the full width of activities of these researchers. We use them here to illustrate a basic principle of the ARITHMUS project: methods are performative; they help bring into being what they want to describe. At our sites, they necessarily produce differences, and triangulation is not suitable for handling them. If we used triangulation, we would have to brush over the differences between individual researchers and their connections to the sites. Such data would simply not add up, and we do not intend them to.[21] We argue that we do not simply compare phenomena or investigate them with different methods from different angles to highlight different aspects; we create different phenomena with different methods.

In addition to the challenges introduced by a diverse group of researchers, triangulation also does not take into account our team as a transversal entity. In line with what we have argued about the field of statistics, our team can also be understood as a transversal entity that adapts methods differently across sites. For example, shadowing was initially used mostly by Baki, our researcher at ONS. Following his activities, a book about shadowing, Shadowing, and Other Techniques for Doing Fieldwork in Modern Societies (Czarniawska, 2007), started circulating in the team. This led to the application of this method by Evelyn at Eurostat, and later by Francisca at Statistics Netherlands as well. Yet it was not used by every team member, used to shadow statisticians of varying ranks, and it was used to learn about a diversity of activities such as management, statistical practice or building professional networks.

We argue that a collaborative ethnography is not just the sum of the work of individual researchers, but also includes their relations with each other. Similar insights about working in a team have been elaborated on in an earlier section describing our practices (see section 1: Beyond the Individual Project: Towards an Ethic of Care). Acknowledging these relations is part of our method. In other words, our team practice is just as transversal as the practice of statistics that we study.

[21] These insights are not new. Many ethnographers understand their field to emerge with their research activities. This way of thinking leaves little room for applying basic ideas of triangulation that understand research methods to provide a perspective to an independently existing field. We contribute an extra layer of complexity to thinking about these issues: variety produced by the different individuals in a team.

To conclude, our data cannot integrate or add up. Ideas about an emerging transversal field need to take into account our differences and connections both with the field and amongst ourselves. Consequently, we cannot resort to methodological 'passe-partouts'; we have to perform the work of accounting for our limitations and differences.

Conclusion (SS)

This working paper sprang from the urge to transcend the cognitive bias of methodological nationalism through a collaborative ethnography of a team of researchers who study and trace social phenomena and processes – in our case: changes in statistical methods – across various sites and scales. In the paper's first part we have explained why successful collaboration is vital to the success of this endeavour. In brief, our conceptual starting point – the assumption of a transnational field of statistical practices – calls for what we have called a transversal method. We start from the idea of a transversal field of power in which scales of the local, the national and the transnational are not thought of as hierarchically layered and mutually exclusive, but rather overlap and intersect.

To account for transversality requires an understanding and analysis of practices as relations which connect actors across various sites and scales. In order to translate this conceptual framework into a methodology that does not fall back to thinking and research of nationally bounded case studies we have proposed an ethnographic approach that is transversal in at least four senses: with regard to the sites and scales under study; with regard to the individual researcher who becomes part of a team that works collaboratively on shared questions, concepts and protocols; with regard to using a variety of research techniques, applied to the requirements of a particular field site; and finally, our method is transversal because it seeks to transgress and dissolve the boundaries between academic disciplines.

These features highlight that successful collaboration is essential if we want to transcend methodological nationalism through a transversal method that moves beyond thinking in and individual(ised) research of nationally bounded case studies. In the second part of the paper, we reflected on five practical collaboration issues and shared the lessons we learnt. The first lesson is that making a transversal method work takes moving beyond the heroic individual researcher and towards an ethic of care. Second, it requires working with other people's categories and adjusting to the unsettling moments this might produce. Third, it needs a situation of trust and understanding in which field notes can be shared, and varieties between note taking styles can be worked as catalysts for collaboration rather than as limitations. The fourth lesson is that a transversal method implies acknowledging the different positionalities of the researchers regarding the national and cultural contexts of their field sites. The fifth and final lesson is that we need to move beyond methodological 'passe-partouts' and instead account for our limitations and differences.

The accounts of our challenges emphasise a rather simple, but crucial point: there are no easy or final answer to the many issues and questions that make collaboration an ongoing challenge. Simply introducing standardised and homogeneous methods will not work. The reason is that the issues and questions which make collaboration challenging will vary from one context to the next, or more precisely, from one research project and team of researchers to the next. What kind of issues and pitfalls a collaborative project will encounter depends on the coming together of a variety of factors, including (but not limited to) the personalities and

expectations of the people involved, the questions and issues to be researched, the kinds of research techniques as well as (if ethnographic research is done) the challenges of the field sites to be studied. Hence, collaboration will always involve and require negotiations, compromises and (dis-)agreements as well as (hopefully productive) disputes, tensions and lose ends. In brief, collaboration requires a constant investment on behalf of the people involved. Anyone who toys with the idea of doing a collaborative project should therefore consider that collaboration does not come for free and that there is – unfortunately – no guarantee to success. If this over-lengthy working paper ever proves to be useful it might be for citing it in a grant application for a collaborative project to point out to prospective funders that it is essential for the project's success to allocate additional resources (in particular: time) to the task of making collaboration work.

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Appendix: Questionnaire for interviews in section 4: Practicing a transversal method within and against the national order of things (SS, VT)

1. How would you describe your positionality as a researcher in relation to the social and cultural context of the field site that you study in terms of familiarity with the social and cultural context, language capacities, national (non-)belonging etc.?

2. In which instances does your positionality as an in-/outsider in relation to the social and cultural context of the site that you study play a role in your field-work? Please provide examples.

3. How does this positionality as an insider/outsider play itself out in your research practices in regards to field access, the methods you use etc.? What advantages and disadvantages does your positionality implicate in terms of field-work possibilities and practices? Please provide examples.

4. Do you think reflecting on how national background impacts on your field-work and research practices is relevant or are there more important factors shaping your field-work and research practices which should be prioritized in exercises of ethnographic reflexivity?

5. The ARITHMUS is a social science research project with an explicitly European scope (it studies how a European population is enacted by means of statistical practices) which is funded by a EUropean body (the ERC) and starts from the assumption of a transnational statistical field in EUrope. In how far do we, as researchers of the ARITHMUS project, help to enact 'EUrope' through our own research?

6. Is there anything you would like to add regarding this reflection on how our positionality as in-/outsiders impacts on our research practices?