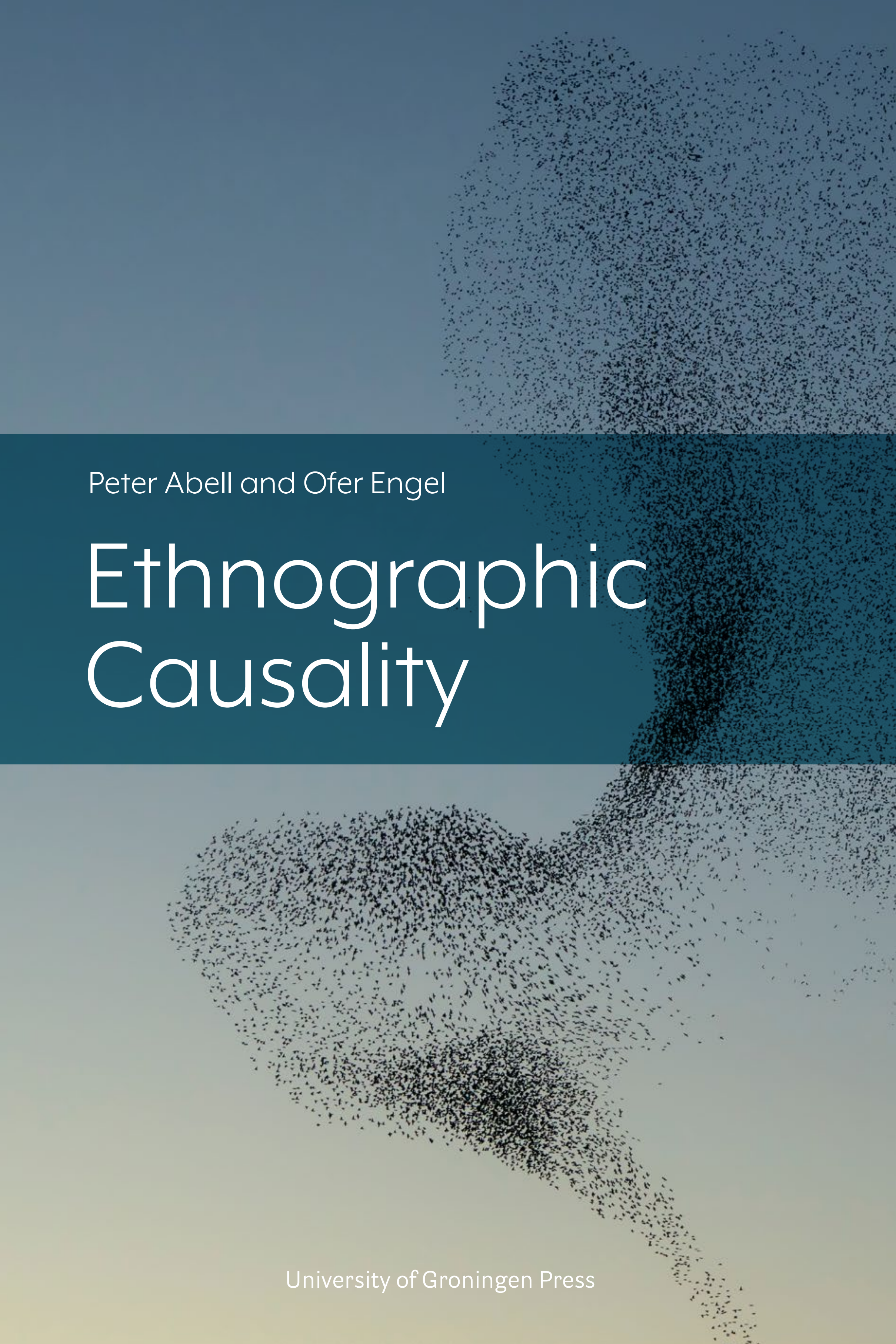

Peter Abell and Ofer Engel

Ethnographic Causality

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Preface

The concept of causal inference advocated in this brief book which, perhaps somewhat controversially, we label as ethnographic causality, allows singular causal inferences to be furnished when the number of repeated comparative observations dwindles, thus ruling out statistical treatment based upon relative frequencies in either cross-sectional or longitudinal, large *N*, samples (or both in the case of a panel). A salient consequence of searching for causal conclusions in the absence of comparative observations is that we dispense (at least initially) with all the paraphernalia of a relevant population and an appropriately drawn sample. Any usually limited generalisation (i.e., moving from a singular "token" to a general "type" of causal inference in the philosophical jargon) is logically posterior to the establishment of a singular causal explanation. We shall anoint such inferences as ethnographic causal inductions, which pose the question as to how generalisable the claims of an already established singular causal explanation may prove to be.

As a word of caution with regard to possible criticisms, we do not purport to write a book in the tradition of ethnography nor intend to draw solely on the literature of ethnography. We have largely failed to engage with the vast literature on ethnographic methodology. Other scholars, better versed in the literature of ethnographic methods, have written ably about the contribution of ethnography to causal inquiry (e.g., Katz 2001, Wedeen 2010, Mathias, Doering-White, Smith, Hardesty, 2021). In contrast, the purpose of this book is to introduce a new way of thinking about the problem of small-*N* causality. This problem is manifested in ethnographic research, a field of inquiry that relies primarily on case studies, where information is elicited through testimony and observation. These constraints pose specific methodological challenges, but they also reveal the limitations of both "quantitative" (large-N) and "qualitative" (small-N) traditions, leading us to find common ground, one which acknowledges their complementary roles in causal identification and estimation.

Causal inference is often made difficult by the recognition that much social science has multiple level aspects, inviting the interplay of causal analysis at the various levels (e.g., amongst and between groups, individuals, etc.). Drawing samples of units at varying levels often proves unfeasible from a cost and empirical availability standpoint. Furthermore, units of analysis at each level can rarely be treated as independently drawn since they are often located in networks of one sort or another. Each of these issues complicates, though does not rule out, attempts to establish causal inferences based on statistical frequencies as long as an appropriate number of comparative cases is available. However, in the absence of comparators, researchers are inevitably led either to dispense with a causal explanation or to find a way of identifying causality prior to any generalisation.

The authors are both committed to statistical modelling and only advocate a reliance upon ethnographic causality when the lack of available comparative units of analysis/cases/observations rules out statistical reasoning based on frequencies. However, we remain somewhat sceptical about any cumulative achievements of the social sciences regarding establishing reliable and replicable causal explanations by the exclusive routine use of off-the-shelf statistical techniques. Each of the authors has encountered situations where wanting to know what caused what could not be statistically derived for the lack of a suitably drawn sample of comparative observations. An almost universal orthodoxy in observational social science and some historiography suggests that in such circumstances, any aspirations to find causal explanations must be surrendered in favour of detailed descriptions. Attempts have been made to promote causal explanation when only a few or even a single "case study" is available and where investigation often relies heavily upon in-depth "ethnographic" interviews and observation. Focusing attention upon subjective degrees of belief and judgement, Bayesian techniques have also been promoted. But any widely accepted inferential procedures have not been adopted by social scientists. We have attempted in the following pages to rectify this situation by developing a transparent and communicable method of causal inference when the number of observations/cases, *N*, dwindles. Although the underpinnings of the procedures we advocate do invite some formal exposition, we have attempted to keep such to a minimum.

Many complex descriptive "qualitative" social phenomena do not easily lend themselves to classification into exclusive and exhaustive nominal equivalence classes – the minimal measurement requirement of most statistical procedures. Rather, they only permit the identification of similarity and dissimilarity between cases or units of analysis. Comparative case studies, which may permit generalisation across very restricted sets of comparators inevitably involve the comparison of groups of similar rather than identical cases which further imposes limitations upon any possible generalisation of causal analyses. We locate ethnographic causal inferences within the framework of small *N* Bayesian Narratives where networks of events are always causally connected by paths of generative individual and collective actions/forbearances and interactions and any limited ethnographic induction/ generalisation is usually constructed across similar Narratives. Narrative causal connections tend to lend themselves to generalisation where the generative actions and interactions causally follow from networks of normative expectations defining institutionalised social roles. Since both Bayesian Narratives and what we shall call large *N* frequency based investigations generate networks of causally connected events we can envisage the future of social science as finding a symbiotic relationship between the two.

There is an extensive philosophical literature attempting to tie down a precise definition of causality but, which unfortunately, appears not to have arrived at a secure resolution amongst many competing conceptions except, perhaps, for the stipulation that a cause always comes before an effect and, therefore an effect comes after a cause. But even this has been debated. The philosophical debate has, of course, deep roots and ultimately engages with disputes about objective as opposed to Humean and Kantian mind dependent conceptions of causality. Our concept of ethnographic causality, in some respects, brings a rather different perspective to this debate by taking subjective causal statements as evidential.

Several metaphysical and epistemological causal ideas compete for our attention, prominent amongst which are: lawful (Nomothetic) deterministic connection, various counterfactual reasoning models, transference of a conserved property, probabilistic causality, intervention conceptions and process models. The issues that appear to have caused most philosophical problems are omissions (i.e. absence of events creating a causal connection), preventive, pre-emptive and alternative (simultaneous overdetermination) causality where token/singular causality is under scrutiny. In addition, and particularly pertinent to our concept of ethnographic causality, is the query as to the relationship between type and token causal connections: do or do not singular "token" causal relations depend upon general "type" connections and if so in which direction does the dependence flow? These various philosophical issues must be carefully handled if we are to coin an acceptable conception of ethnographic causality. However, the disagreements amongst philosophers have hardly had any impact upon the way in which different scientific disciplines approach causal analysis whilst, almost invariably, adopting a large *N* comparative perspective. Nevertheless, as social scientists we need to tread warily, but our concept of ethnographic singular (i.e. token) causality will be contrasted with a large *N* statistical concept which in practice searches for generalised (i.e. type) causal connections.

By picturing causal mechanisms in terms of human agency, which creates connections between causes and their effects, there is a certain flavour of both transference of energy (i.e. intentional motivation and beliefs/cognitions) and intervention in our conception. However, counterfactual (or potential outcomes) reasoning lies at the centre of our deliberations and will loom large in our exposition. Despite the criticism that counterfactual reasoning has attracted and the lack of resolution as to whether causality should be defined in terms of counterfactuals or the other way around, we adopt the former standpoint. This is licenced by the fact that counterfactuals become, in the context of ethnographic causality, an item of evidence for the same unit of analysis as that for which the indicative causal statements are obtained. In this respect ethnographic causality has an advantage over most frequency-based and singular concepts of causality where this is difficult to achieve. However, this advantage has to be balanced against the possible lack of credibility and limitations of subjective causal statements.

1. Introduction

What role can the analysis of a single, or perhaps only just a few, case(s) play in a systematic social science? More particularly, what role can the study of a handful of comparative cases (based upon individuals or social groupings of one sort or another) play in discovering causal relationships between designated causes and effects with a view to furnishing explanations and perhaps even modest predictions? These questions may perhaps be more precisely expressed by replacing the term "cases" with the term "observations". Causal inferences can with caution be broached by gathering repeated longitudinal observations pertaining to a single case/unit of analysis, or just a few cases/units of analysis. However, single case studies are rarely based upon the collection of repeated data (i.e. time series). They more often than not, embrace chronologically ordered sequences (or even parallel sequences) of multiple types of events, actors, actions and forbearances amongst which various causal connections, generating an unfolding story or narrative may be sought. The appropriate question then is whether or not causal inferences can be vouchsafed by the observation of one or just a few such sequences or narratives. We shall, nevertheless continue to use the term case–study (sometimes alternatively, small *N* and qualitative study) and contrast such studies with those that centre attention around statistically drawn cross sectional or longitudinal samples (alternatively, large *N* and quantitative study). However, large *N* versus small *N*, where *N* is indicative of the number of comparative observations, assembled either cross-sectionally or longitudinally, is the term we shall tend to favour.

This brief book, thus, addresses the problem of establishing causal inferences in small *N case* studies by assuming that causal inference lies at the heart of the social scientific enterprise. We are fully aware that not everybody will agree with this objective. Many, particularly those coming from the small *N* qualitative tradition, may object to the term "scientific" itself; arguing that the term carries an unwarranted incorporation of the precepts of Positivism. However, hopefully we may set some readers' minds at rest by noting that there is a significant intellectual distance between our concept of ethnographic causality and some of the standard precepts of Positivism. We still, despite this distance, prize adherence to transparent, communicable and replicable procedures which we, like many others, find lacking amongst those few ethnographic studies and case studies which do seek to make causal claims. Although the terms ethnographic studies and case studies can be differentiated, for our purposes we will use them interchangeably. Our analysis will emphasise the role of subjective causal statements as providing evidence for ethnographic causal links whilst still acknowledging that other sorts of textual evidence may be important.

Many social scientists remain content with detailed descriptive comparative case studies (Geertz, 1973). Indeed, some case study enthusiasts (often labelled as adherents of a qualitative or small as opposed to large *N* approach) resist the application of causal thinking to human affairs all together (i.e. where actions, forbearances and interactions are involved in driving things along (March et al 1991). To put it succinctly, they ask – how is causality to be made compatible with the assumed voluntary actions and forbearances of human actors/agents? Such queries do, of course raise the thorny philosophical issue of "free-will" versus determinism (List, 2019). We are not going to settle this issue here but we will argue that our concept of ethnographic causality can, through the agency of what we term counter-potentials, preserve a voluntary interpretation of human actions. All agree that the analysis of individual and collective actions, forbearances and interactions (or perhaps some would say decisions or choices) reside at the heart of social science. This latter point we take as un-contestable, whether or not a voluntary interpretation of actions is adopted.

Ethnographic causality, as we define it, derives from the observation that actors may themselves often furnish causal explanations of what they are doing, have done and anticipate they will or might or even should do in the future. Thus, statements like: "I did *Y* because of *X* to realize *Z*" may be elicited, from actors, by ethnographers. Furthermore, well informed observers/informants of others' actions may also provide subjective causal explanations of this sort although they, themselves, are not the authors of the action. It is these sorts of "subjective causal explanations" which we will promote as providing the evidence for ethnographic causal inferences as long as appropriate levels of credibility (Schum, 1994) can be attached to them by ethnographic investigators. Such varying credibility, we shall argue, should be "socially constructed[1]" on the basis of in-depth social interactions between ethnographers and the investigated actors (informants). Thus, the examination of the process of ethnographic elicitation, which will generally comprise of a question and answer interaction, is brought into full focus as a possible path to causal inference. This in some respects reflects the often remarked statement that a fundamental human capacity is the ability to formulate and judge causal connections in their physical and social environment (Byrne, 2005). In addition in assigning credibility to elicited statements about the reasons and causes for actions (we shall controversially in some circles construe reasons as causes-see below) the ethnographer may be regarded as deriving her conclusions from an empathetic cultural understanding of the statements.

Attribution theorists, (Kelly 1967; Weiner, 1986) have studied how people attribute causes to their own and others' actions. The key distinction embodied in the theory is between "dispositional" (internal) causes like intentions and beliefs and "situational" (external) causes, comprising of events and others' actions. An interesting question follows from the standpoint of ethnographic causality; which sort of subjective causal statement is an investigating ethnographer likely to elicit? Tentative results tend to suggest that if the outcome/effect of an action perceived as negative by a prosecuting actor then a situational attribution is likely, but, if positive, then the attribution will, more often than not, be dispositional (Campbell and Sedikides 1999). However, our conception of ethnographic causality covers dispositional and situational attributions both of which may be elicited by ethnographers. Furthermore, the method of in-depth interactive ethnographic elicitation, in pursuit of credibility, goes well beyond any initial remarks that actors may make when queried by an ethnographer about the causes and consequences of their actions

1 We take a social construct to be an entity that comes to existence, continues to exist, or assumes its properties in virtue of the actions of social actors or their states of mind (Mallon 2019)

and forbearances. In particular interactive elicitation may explore possible alternative causal possibilities which may cross the mind of the informant.

Issues of credibility run both ways between the ethnographic investigator and the observed actor's reports of their actions and informants reports of others' actions. Each must trust the other party for mutual respect in their personal credibility to evolve and, thus, the credibility of any elicited statements about causality which are made by the actor/informant. They must also both estimate that their partner to the interaction is competent, on the one side, to elicit and, on the other, to impart causal information. It is important to acknowledge at the outset that the ethnographer adopts the role of "a measuring instrument" in any ethnographic inquiry. This attribution cannot be avoided but should not, as it is in some quarters, provide grounds for dismissing subjective statements as a possible credible source of information. In fact, affording such credibility underlies the every-day interactive mechanisms in human populations.

The ethnographer computes, on the basis of her ascribed degree of credibility of elicited statements, the likelihood ratio that a particular causal connection has or has not been established. This ratio when matched, in a Bayesian framework, with any legitimate prior odds entertained by the ethnographer of the existence of a causal connection, enables the posterior odds of the connection to be inferred. Actors may, however, initially be somewhat uncertain about how to causally account for their own and others actions until they face the determined elicitation of an ethnographer? It is in this sense that we may conceive of the social construction of the causal statements. Actors/informants rarely fail to provide an initial account of their actions but may adjust or elaborate their accounts on further inquiry by an ethnographer. Furthermore, the final, socially constructed, account can and should be mutually acknowledged, as the best account by both the actor and the ethnographer. Disagreements should be explicitly acknowledged and all statements should be reported as attributable to an ethnographic/informant pair.

Indeed, it may be worth noting, in passing, that Psycho-Analysis, amongst a number of psychological theories, rests upon a similar interpretation of the in-depth relationship between the analyst and the patient which hopefully procures the "truth" about the patient's unconscious motivation. Recent developments in neuroscience suggest that our real-world perceptions, even those of our own motives, are partially conditioned by our "predictive brains" which, using Bayesian up-dating, try to minimise errors between categorising our current experience and our prior similar experiences. This suggests that when an ethnographer queries, "why did you do that?", the personal construction of the eventually negotiated response is commenced, which eventuates in a subject's construction of self-understanding.

Ethnographers, under this interpretation of their involvement, play the role of an "expert", in the sense that analysts of uncertain judgements use this term. That is to say, they attach a probability to the credibility (i.e. truth) of the statements negotiated with the actor/informant. However, as we shall see in Chapter 3, the actors may, themselves, only surrender probabilistic statements to the ethnographer and in so doing may be conceived as playing the role of "expert" in respect of their own introspective objectives. In this situation the ethnographer may then be pictured as playing the role of what is usually termed the "facilitator", by merely prompting and reporting the probabilistic statements of the actor. However, alternatively, the actor and ethnographer may both contribute probabilistic attributions. The ethnographer then finds herself estimating the probability that the actor's probabilistic statements are of sufficient credibility in order to make a tentative causal inference.

It is the subjective aspect of ethnographic technique which will, no doubt, alarm both large *N* statistical and even perhaps some small *N* case study scholars. But this alarm arises largely because ethnography has produced no systematic replicable and communicable procedures which facilitate a clear understanding of how causal inferences should actually be made transparent when *N* is small because comparators are scarce and, what few there, are descriptively probably only similar, not identical. As a consequence, the social sciences are beset by conflict between the advocates of what are often termed "quantitative" and "qualitative" studies. Although the juxtaposition of these two terms is not entirely fortunate as quantitative statistical studies utilizing non-metric variables is routine and case based qualitative studies can sometimes embrace metric variables. They, nevertheless, have become the banners under which styles of research are promoted, each grounded in very different philosophical precepts and research methods, which occasion much mutual disparagement and dismissal. It does seem to us that much of this mutual disparagement could be dissipated if a cogent conception of causal inference could be formulated when *N* is low (i.e. based upon "qualitative" studies).

The paramount virtue of subjective causal statements is that they apparently, if they are deemed reasonably credible, solve the so called "impossibility of singular causality" (Holland, 1986, Rubin, 2005) which documents how the same unit of analysis cannot be both exposed and not exposed to a given causal variable on a specific occasion. However, ethnographic causal inquiry can, when directed at a particular action on a specific occasion, surrender counterfactual evidence for the same unit of analysis as that for which the causal connection is sought.

Thus, ethnographers may elicit statements, of varying credibility, from an individual actor, like, "I did *Y* because of *X* to realize *Z*" and the counterfactual "if *X* had not been the case I would not have done *Y* to realize *Z*". If such statements can be negotiated by the ethnographer and actor then, net of any motive to mislead, or lack of self-understanding, a generative causal connection, in virtue of action *Y*, lying between *X* and *Z* may be surmised.

Counterfactuals and associated counter -potentials, developed in the large *N* tradition, as we shall observe in chapter 3, have attracted the accusation that they are metaphysical in nature (Dawid, 2000, 2010). However, subjective counterfactuals, in the ethnographic framework are empirical, though they only carry varying levels of credibility. Furthermore, the voluntary nature of human actions, which many scholars wish to protect (List, 2019), may often be preserved in elicitation, as actors may acknowledge that, even if they did, on a specified occasion, do *Y* because of *X* they could have, nevertheless, forborne to do so, even in the presence of *X*. Similarly, they could have done *Y* in the absence of *X*. Clearly, for this reasoning to go through the action or forbearance must be feasible. We call such acknowledged statements counter-potentials (Abell and Engel 2019). Ethnographic inquiry may thus elicit, alongside positive causal statements, both counter-potential and counterfactual causal evidence from a given actor/informant concerning a focal action. This we believe should accord them some significant standing in appropriate circumstances.

The currently predominant large *N* paradigm to causal inference, which is largely based upon observational as opposed to experimental studies, would give rather short shrift to any causal ambitions attached to a single or few observations. They relegate the study of a small number of cases (i.e. low *N*) to a strictly subordinate role; usually as an exploratory device prefatory to eventual frequentist statistical inquiry (Lieberson,1985). Indeed, the paradigm promotes the idea that any causal inference is ultimately predicated upon the application of across case comparative method to a sample of units of analysis (cases) pointing to a generalization (or even law) connecting the "type" cause to the effect when protected against confounding measured and unmeasured covariates. No causal explanation, it is often implicitly implied, without the invocation of inter-unit comparison and generalization. We should recall though that a large *N* can be achieved either in cross-section, by observing and comparing many units of analysis at one point in time, or longitudinally by repeatedly observing a single or few units (a panel) over time.

However, singular causality, devoid of any reference to generalization and inter-unit comparison has been defended by a number of philosophers (notably, Woodward, 1984). Woodward points out that in every-day use of causal explanations there is often no apparent sense in which they are deduced from (associated with) generalized propositions. Rather, singular causal claims gain their explanatory potential in terms of the contrast that is supplied by comparison with counterfactuals. Counterfactual comparison has also been used to draw a distinction between law-like and accidental generalisations. Whatever we make of this distinction the question remains-how are we to guarantee that the counterfactual, in this particular case, is correct? We have noted that our conception of "singular" ethnographic causal inference provides a possible answer to this query by allowing for the elicitation of subjective counterfactuals pertaining to an action on a particular occasion. However caution must be exercised as actors may entertain alternative possible motivations for a given action on a specific occasion and, if so, the counterfactual must cover the conjunction of both motivations.

Subjective counterfactuals may also go some way to address another criticism, sometimes levied at a counterfactual interpretation of causality, which suggests that the reversal of a proposed causal factor in a counterfactual – say gender or race for instance – may not be possible to observe (Goldthorpe, 2007). No causality without the possibility of manipulation into the counterfactual state, as Holland (1986) expressed it. However, subjective estimation of the consequences of such variables is perfectly possible, thus appreciably liberating the powerful counterfactual perspective from this line of criticism (Woodward 2005; Greiner and Rubin, 2011). Thus, for instance, if gender is elicited as a cause of an action then the counterfactual response to "what if you were not a woman" is most probably elicitable. Or to put it another way the variable gender may be subjectively open to manipulation by the actor/informant in the form of a thought experiment, for example. Only the manipulation variables that are unconceivable to the actor/informant will fail to fit the bill. Furthermore, such failures will encourage elicited statements revealing the inconceivability which amounts to valuable information.

Nevertheless, it is true to say that the sorts of causal variables often found in many large *N* studies, are not those that interrogated actors are likely to provide under elicitation. Large *N* studies frequently make use of summary aggregate causal variables (e.g. socio-economic status) which combine a number of variables into a compendium. It is the constituent variables that elicitation is likely to procure. If ethnographic causality is to complement studies, where statistical (large *N*) investigations also prove feasible, then some method of reconciling the two becomes necessary (Chapter 3). Generally speaking singular ethnographic studies will generate more finely grained paths of causal connections explaining a particular target effect (outcome). These can then, with care, be interposed into a statistical derived connection thus providing more detailed causal paths. This procedure may be described as providing a theoretical causal mechanism showing how the events are connected. The mechanism will inevitably be either singular or of limited generalisation (i.e. based upon ethnographic induction) but may provide information for a subsequent elaboration of an intervening variable in order to further elaborate the statistical study. Indeed, any large N generalisation involving aggregate variables may in principle be reduced to a finite number of singular cases which may differ in terms of the details of their causal connections. The generalisation is conceived so as to dispense with this causal detail in order to establish, by comparison of similar cases, embodied in the generalisation. But following Pearl (20000) : "As we move up to macroscopic abstraction by aggregating variables and introducing probabilities to summarize omitted variables, we need to decide at what stage the abstraction has gone too far and where useful properties of causation are lost". Surely it is the careful symbiotic analysis of Bayesian Narratives and large *N* causal networks that should determine the acceptable level of abstraction or aggregation. It may be important to signal here that many causal networks in the large N framework are often deemed to carry a Markovian property whereby the specified causal indegree (parents of) into any node/variable renders it independent of all other prior variable/nodes in the network (Chapter 2). When this condition is simplifying causal structures. It is also important to recognise that aggregation which may discard potential causal differences may arise by virtue of the aggregation of variables (e.g. socio economic status) applied to each individual unit of analysis and aggregation of units into a collective actor where a mean level of an aggregate variable may be invoked. (e.g. a group) (Chapter 4).

From a social science standpoint, whether or not singular causal inferences are entertained, investigators will still pose the question as to how far any proposed causal explanation may be generalized beyond the currently available observations? Those operating in the large *N* tradition, characteristically seek generalisations in order to both validate any causal explanations they may wish to entertain and to locate the limits of those generalisations (sometimes called external validity). Ethnographic inquiries, on the other hand, inductively seek generalisations only in order to test the limits of the established singular causal explanation. They, as it were, pose the question as to how generalisable an already established causal explanation may prove to be?

Causal inferences can be achieved by the application of an empirically established generalization to a single or just a few additional cases/observations within the Large *N* perspective. However, since any generalisation is likely to be probabilistic, unless the additional cases/observations are statistically plentiful little can be inferred about a single case. Of course, many will, philosophically speaking, see the birth of the large *N* perspective on causal explanation in terms of Hempel's (1965) famous Nomothetic Covering Law Model which was in fact promoted as a framework for explanation in historical scholarship (Roberts, 1996). Although the model has experienced considerable philosophical buffeting and few these days would ascribe to laws of social science or of history (Cartwright, 1989), nevertheless the detection of generalization does continue to hold court in the large *N* statistical approach to causality (Morgan and Winship, 2015). Such causal connections are, these days, more often than not, embedded in networks (often depicted as Directed Acyclic Graphs, or DAGS, see Chapter 2) of causally connected variables, usually providing a number of causal paths between any pair of variables (Pearl, 2009).

We will suggest in the following chapters that a more ambitious role can – indeed should – be conceived for case studies (small *N* studies) in making causal inferences than that which is warranted by large *N* advocates who limit case studies to an entirely subordinate suggestive role. We do not, however, wish to be associated, at all, with those who reject statistical models. On the contrary, we believe that such models are manifestly an essential element of most of the advances we have witnessed in sociology in recent decades. Nevertheless, one of the authors of this volume, with a firm commitment to statistical procedures, encountered situations where empirical study was hampered for the lack of inter-unit comparative cases and the unfeasibility of conducting a longitudinal study (Abell, 1988). This led him to the formulation of the theory of Comparative Narratives (Abell, 2004) which coincidentally happened as others were developing parallel ideas (Abbot, 1992). It is remarkable how frequently the word narrative appears in many essays into the nature of social inquiry (science?) (Goldthorpe 2000; Little, 2011) though the systematic analysis of narratives has not thrived. This frequency is also apparent in historiography (Danto 1985, Roberts 1996) where the application of statistical causality has proved equally problematic; again often because of the scarcity of comparative cases unless over heroic simplifications are resorted to, which attempt to draw similarities between otherwise rather disparate cases (e.g. revolutions). A parallel issue can arise in the large *N* statistical framework where copious use of binary (dummy) control variables can render inferences hazardous (Breiman, 2001).

Narrative ideas (i.e. the recounting of causal stories connecting actions and events), expressed in natural language, are fundamental to every-day explanations and the ways in which individuals memorize, recount and account for their past activities. It is difficult, however, to conceive of narratives without the invocation of causal connections which in some sense transcend the mere documentation of sequences or chronologies. The analysis and comparison of narratives embodying singular causal connections should, in our view, play a central role, parallel to statistical large *N* procedurs, in the analysis of causality where running variables essential to various time series models are equally problematic. In order to achieve this objective it will prove imperative for small *N* studies to acquire transparent procedures for causal inference in singular cases.

Although any attempt to classify types of causal relations is fraught with philosophical difficulties (Paul and Hall, 2013) it will prove helpful in future chapters to distinguish between four types of causal link depending upon the nature of the causes and effects.

1. Events/processes cause other events/processes,

2. Events/processes cause actions/forbearances by individual and/or collective actors,

3. Actions/forbearances by individual and/or collective actors cause events/processes,

4. Actions/forbearances by individuals and/or collective actors cause further actions/forbearances (i.e. social interactions).

Philosophers have debated at length whether causal connections of type1 should be conceived as operating between unique spatial-temporal “singular events” or possibly repeatable non-unique “kinds of/properties of events”. All spatial-temporal events are, in some sense, unique in terms of their location in time and space. Token-level causal relations are deemed to link unique events whereas type-level causal relations link generalisable event kinds. This distinction is thus based upon whether the connected events are unique or not. The question then arises as to how, if at all, the causal connections at the type and token levels are related? Unfortunately, philosophers differ in the answers they give. Eells (1991), for example argues for independence whereas Cartwright (1989) and Hausman (2005), amongst many others, find that type-level causality depends upon the token- level, a conception with which most Large *N* social scientists may well agree. We will interpret ethnographic causality involving events as connecting either singular events or event kinds/ properties by actions/forbearances (see below). The connecting actions may also either be unique or open to some level of generalisation.

Most published social science research tends to focus on causal connections of types (2), (3) and (4) involving actions. Although we have formulated causal connections as running between events and actions most studies in the large *N* social sciences are based upon inferred causal relations between either discrete or continuously distributed random variables. Various covariation measures between these variables, when protected against measured and unmeasured co-variation, enable a conception of the strength of causal connections rather than merely their presence or absence. Suppes (1973) provides an intellectual bridge between causality formulated in terms conditional probabilities of events and the co-variation of variables. Binary variables are indicative of the presence or absence of an event. Also, each value, attributed to any unit of analysis, using either a discrete or continuously distributed variable, may be construed as an event.

Many causal connections involving actions only prove feasible because of a more or less conscious understanding, on the actors' behalf, of the implicit physical causal connections whereby actions engage with and propel the physical world. Thus, the action of opening a closed window to let in cool air clearly involves various physical causal connections which are implicitly assumed by the actor. They are, though, not usually documented by social scientists but clearly contribute to the context of the action. Thus, the causal explanation of the window being opened by an actor will not normally explicitly evoke the physical causality. Rather, the causal account will take the form that the closed window caused the actor to open it with the objective of letting in the cool air. However, any unintended consequences of actions may also entertain the social scientist especially if they have further causal consequences generating a simple narrative.

Various types of singular causal connection are brought together in narrative structures which trace out complex networks (appendix) of causality (Abell, 2001; Small, 2013) running between actions and events. They bear some comparison with the directed, acyclic graphs (DAGS) recently popularized in large *N* statistical studies (Pearl, 2009) though, not surprisingly, because of their singular nature, there are differences attributable to the fact that narrative structures are depicted as "directed acyclic digraphs" (DAGS) where the causal arrows both incident into and out of all the nodes are conjunctive ("and") causes consistent with the basic ethnographic mechanism (below), where, in a single case, alternative causal paths are not possible. Large *N* causal graphs, on the other hand, tend to be "or-digraphs" indicating alternative causal connections (paths) derived from a sample or population, though the recent non-parametric analysis of DAGs somewhat blunts this distinction (Chapter 2).

We shall, as we have already indicated, interpret the social sciences, including sociology, as a quest for causal mechanisms which will, in the most basic version, be structured in the following manner:

$$(C \text{ and } X) \rightarrow (\text{mechanism } Y) \rightarrow Z,$$

Where X, Y and Z, in the statistical large N tradition, are variables of one sort or another, $\rightarrow$ stands for causal connections and C stands for the context (often rather implicit) in which the presence of X is deemed ultimately to cause Z in virtue of the mechanism. We ignore for the moment a possible additional causal link running directly between C and X and Z. We also assume a chronological ordering of the causally connected elements.

However, this formulation apparently replaces one causal connection with two – perhaps they both now invite the insertion of additional mechanisms and so on (Blackburn, 1995). Some subtlety has been used in "bottoming out" causal connections such that mechanisms are described as "generative" which apparently puts a stop to the possible regress (Morgan and Winship, 2015). Thus, generative mechanisms are perhaps correctly conceived as more than just additional intervening variables? They, as it were, reach out to both X and Z – they construct or make possible the causal connection? The statistical large N treatment usually adds an additional intervening variable, Y, between X and Z. Y may, of course, be derived from some, non-observed theory about the connecting mechanism, but how it constructs or generates the connection between X and Z often does call for a deeper analysis.

The ethnographic perspective does, we believe, begin to address this issue when the basic formulation now takes the general form:

$\{C\}$ and $\{X\} \rightarrow$ (α's action /forbearance $\{Y\}$) $\rightarrow_T \{Z\}$.

Where $\{C\}$,$\{X\}$,$\{Y\}$ and $\{Z\}$ stand for place-holders of sets of conjoined descriptions of events, and additional actions/forbearances, α is a designated individual or collective actor and $\rightarrow_T$ stands for what we term a teleological causal connection (explained below).

We shall often drop the word forbearance, though it may be that α's forbearances (i.e. not doing something that could be done) that constructs causal connections. An actor, α's, forbearance may cause subsequent actions by α or other actors generating social interaction (type 4 causality above).

The sets of conjunctive elements, $\{C\}, \{X\}, \{Y\}\{Z\}$ are usually, at least initially, expressed in α's own natural language/discourse and are elicited by a specific identified ethnographer from α. The unadorned arrow continues to represent a causal link (i.e. between the conjunction of the elements of $\{C\}$ and $\{X\}$ and the action $\{Y\}$). The mechanism is, thus, characteristically an action or sequences of actions (i.e. interactions comprising a narrative, see below).

The action(s) show how the connection between the combination of the context $\{C\}$ and cause $\{X\}$ delivers the outcome, $\{Z\}$ and is, thus, constructed by the actor(s) establishing the mechanism. The action, by α, provides the motivational energy (directed intention) and cognition (beliefs) whereby action $\{Y\}$ procures $\{Z\}$. In this respect the causal account takes on a feature of the transference model of causality mentioned in the preface.

Here actions, as it were, look both backwards and forwards when linking $\{C\}$ and $\{X\}$ to $\{Z\}$, inviting us to regard the two implicit causal relations as a single analytical entity. This, importantly, permits a spanning counterfactual (subjunctive conditional) statement of the form, if α had not acted $\{Y\}$ (or some equivalent action) then $\{X\}$ in conditions $\{C\}$, would not have caused $\{Z\}$ on the occasion in question. Actions, by their very nature, unlike events, do tend to reach out both backwards to selected causal motivators and forwards to intended objectives and, moreover, often carry the name of their expected objectives. Then the formulation runs as follows:

$\{C\}$ and $\{X\} \rightarrow$ (α's action $\{Y\}$) $\rightarrow_T \{Y\}$.

Where $\{X\}$ may also be $\{not\ Y\}$

Then,

$\{C\}$ and $\{not\ Y\} \rightarrow$ (α's action $\{Y\}$) $\rightarrow_T \{Y\}$

The distinction between $\{C\}$ (i.e. conditions/context) and $\{X\}$ (i.e. causes) calls for some comment, in both the large N and ethnographic framework. It is usually proposed, in the large N tradition, that in conditions C, X causes Y and in identical conditions the counterfactual holds, whereby the absence of X does not cause Y, or at least a significant probability of Y. The absence of any of the positive components or presence of any of the negative components of set $\{C\}$ would also prevent $\{Y\}$.

The separation of causes X, from attendant conditions, C, has created much philosophical debate in the large N tradition (Ells. 1996; Spirities et al, 2000; Pearl 2019). Mackie (1965, 1980) has gained some prominence when he identifies a necessary component of a sufficient but not necessary set of conditions as a cause. The conjunction of sets $\{C\}$ and $\{X\}$ in the above formulation of ethnographic causation is a case in point.

But two compelling questions arise, first, why pick out $\{X\}$ and, second, can $\{C\}$ be exhaustively described (Pearl 2019)? It is useful in the large N approach to causality to regard C as designating the defining aspects of a population in which the presence and absence of variable X has an identified causal effect. Thus, the potential impact of the presence and absence of X exists in the population independently of any sampling process (Paul and Hall, 2013). However, a population is rarely definable when we turn to ethnographic singular causality.

We are not going to settle the important issue of separating $\{C\}$ and $\{X\}$ here, but note that in an ethnographic causal explanation of a singular causal connection both $\{X\}$ and $\{C\}$ will actually be selected by the actor/informant and elicited by an identified ethnographer. In this respect $\{X\}$ may or may not describe factors which have recently changed whilst $\{C\}$ remains constant. The actor may on another occasion, neither select $\{C\}$ nor $\{X\}$ when acting $\{Y\}$ and, thus, invalidate any tentative generalization of the causal connection connecting $\{X\}$ and $\{C\}$ and action $\{Y\}$. Nevertheless, the causal connection will still stand on the occasion in question. This, rather nicely, brings out the difference between ethnographic small N and large N causality.

Ethnographic causes may not be open to inductive generalization but can nevertheless stand as credible causes on a specific occasion. Furthermore, an ethnographer may elicit from the actor, on the focal occasion, the counterfactuals that had either $\{C\}$ or $\{X\}$ or both not been the case then the action would not have been pursued by the actor. Similar reasoning applies to set $\{Z\}$ the components of which will also actually be picked out by the actor in statements like "I did $\{Y\}$ to realize $\{Z\}$".

The set $\{Z\}$ is construed as a conjunction of events and perhaps further actions by α or other actors may also contain properties of these events or actions. For example, contrast opening a window, act $\{Y\}$, to let in fresh air and expel stale air – two events in set $\{Z\}$ – and opening a window so it is wide open, a property of the window. The teleological explanation of the latter requires specification of "wide open" to be included in the description of the action $\{Y\}$ whereas the former does not. It is this conceptual distinction that has animated the so-called logical connection argument whereby some philosophers have rejected the contingent (i.e. causal) connection between intentional actions and their consequences. We shall nevertheless, whilst keeping an eye on this issue, maintain a causal interpretation of the teleological connection in our basic formulation of ethnographic causal linkages (Davidson 1967).

It will not have escaped the readers' attention that there is much to suggest that the basic ethnographic causal model, thus far outlined, and the statistical large N model of mediation have some basic sequential features in common. However, the explicit introduction of actions as intervening between $\{X\}$ and $\{Z\}$ invites an analysis of how the actions actually generate the connection between the context, $\{C\}$, causes, $\{X\}$, and the intended outcome $\{Z\}$.

Ethnographically we may propose, for any attempted action that the generative connection between $\{X\}$ and $\{C\}$ and outcome $\{Z\}$ may be conceived in terms of a contingent practical syllogism (von Wright, 1970; Abell,1987) which provides a very flexible framework which permits the application a variety of theories of action:

(1) α intended to realize $\{Z\}$,

(2) In situation $\{C\}$ and $\{X\}$ both selected by α, α believed that by acting $\{Y\}$ that $\{Z\}$ would (probably) be realized,

(3) α acted $\{Y\}$, having selected $\{X\}$ and $\{C\}$,

(4) Action $\{Y\}$ brings about (realizes) $\{Z\}$ (i.e. a successful action) does not bring about $\{Z\}$ (i.e. an unsuccessful action).

Thus, all actions are experimental. As such the action driven connection between, on the one hand, causes $\{X\}$ and context $\{C\}$ and, on the other, effect $\{Z\}$ is generated by the contingent belief (2) and the teleological intention (1) but the latter remains causally undetermined (autonomous action!). However, alternatively, the intention may also be conceived as determined by the selected context $\{C\}$ and causes $\{X\}$. Then (1) would read along the lines:

(1*) Having selected $\{C\}$ and $\{X\}$, α intended to realize $\{Z\}$.

Either formulation, in virtue of $\{C\}$ and $\{X\}$ being selected (i.e. constructed as causes) by α, is consistent with an action providing a common antecedent (not mediating) cause generating the causal connection between $\{C\}$ and $\{X\}$ and effect $\{Z\}$, as follows:

$\{C\}$ and $\{X\} \leftarrow$ (α's action$\{Y\}) \rightarrow_T \{Z\}$.

This may be read as:

(5) α intended $\{Z\}$,

(6) α selected the causal environment $\{C\}$ and cause $\{X\}$,

(7) α – believed that doing $\{Y\}$, in $\{C\}$ and $\{X\}$, would (probably) realize $\{Z\}$,

(8) α did $\{Y\}$,

(9) $\{Z\}$ was realised.

Thus α's action involves the selection of a subjective context, C, subjective causes, X, and the formulation of a belief about the outcome of the proposed action, $\{Y\}$, bringing about (teleologically causing) the outcomes Z. Unlike with the statistical large N treatment the distinction between intermediation and confounding is not a sharp one. In either situation we have formulated beliefs, parenthetically, in probabilistic terms allowing that actors/informants may well express their beliefs in this manner (Chapter 3). Actors and Informants may also deliver elicited statements about causal consequences that were not intended which themselves may have causal consequences generating a narrative sequence (Chapter 3).

Unpacking actions in terms of the syllogism invites an exploration of possible counterfactuals which enrich a causal explanation. Both the cognitive (belief) and intentional components of the syllogism can generate subjective counterfactuals in respect of the causal connection. Thus, statements may be elicited along the lines of, “I would not have intended $\{Y\}$ if either $\{C\}$ or $\{X\}$ had not been the case” and “I would not have believed that doing $\{Y\}$ would realize $\{Z\}$”. Importantly note here that the cognitive counterfactual bridges the two stage causal connection further substantiating the idea that basic ethnographic causal connections should be treated as an analytical unities.

The explanatory schemes (1) or (1*) to (4), do not depend upon any explicit generalization or comparison to enable explanatory causal inferences to be drawn. They may however depend upon implicit generalisable physical causes, as we noted above, which are embodied in the beliefs about the effectiveness of the action in a physical environment.

Statistical (large N) models, based upon observational data, can only be tested if, ideally, random samples of an appropriate size can be drawn from a defined population, comparing, in the simplest version where X is binary, cases that are exposed to X and $\neg X$. Though to speak of samples and populations would be inappropriate, ethnographic comparisons may be sought inductively over a limited number of comparative cases or repeated similar actions by α and other actors. Repeated actions tend to occur in the context of institutionalized actions reflecting role expectations which will be explored in Chapter 4.

Limited inductive comparison and generalization associated with ethnographic causality is, however, logically distinct from the statistical, large N approach to generalisation. In effect, any limited generalization answers the question how generalizable is an already established singular causal explanation; not what is the generalization that warrants the supposition of a causal explanation in a singular case? Thus, the logic of generalization in ethnographic causal inquiry reverses the standard statistical (large N) conception whereby comparison and generalization are regarded as logical prerequisites of any explanation. Ethnographic explanatory causality is logically prior to any possible generalization. The term generalized causal explanation (GCE) captures this distinction (Abell, 1987).

The formulation of actions in terms of the practical syllogism is open to many differing theoretical interpretations about the nature, description and derivation of actions (and forbearances). Multiple theoretical interpretations of actions have been proposed at least since Weber (1949) and the various hermeneutic schools (Koppl and Whitman 2004). However, currently the disputes between "rational choice" and "behavioural theory", both deriving from economics, has dominated debate and penetrated the other social sciences. In this respect we may seek, in the context of ethnographic causal inference, to derive the causes of the intention to realize $\{Z\}$ (proposition (1) and (1*) above) in terms of preferences (if appropriately formulated in terms of indifference curves derivative of a utility function) over an opportunity set constrained by a budget set. Behavioural theorists reject what they regard as the overly simplified descriptions of actors in rational choice and propose replacing the "arbitrary categories of economics" by more realistic categories derived from the "physiology" of the individual (Camerer et al, 2005). However, our model of ethnographic causality is consistent with either theory and does not exclusively tie us to any particular theory of action.

Systematic causal studies of a few cases have been pursued by many scholars, but noticeably by (Mahoney, 2000, 2013) resorting to Mill's methods and Ragin (1989, 2000) who has developed both a deterministic and fuzzy Boolean approach to a handful of cases, each scored in terms of binary variables. Neither of these approaches however, seeks to reverse the logical relationship between comparison, generalization and causal explanation which we are advocating here.

If an ethnographer manages to elicit a credible subjective statement from an actor, that, on a specified occasion she acted $\{Y\}$, because of $\{X\}$ (in $\{C\}$), to realize $\{Z\}$ then we may tentatively conclude that $\{C\}$ and $\{X\}$ comprised, on the occasion in question, sufficient ethnographic causes for α's action $\{Y\}$ which, in turn was sufficient for $\{Z\}$ and, by deduction, $\{C\}$ and $\{X\}$ were sufficient for $\{Z\}$. Nothing follows about action $\{Y\}$ being a cause on other identical/similar occasions, nor that other actors would do the same in similar circumstances. The sufficiency is entirely occasion and actor specific, bringing into prominence the singular non-comparative nature of the inferences. If now the ethnographer further elicits the counterfactual where the actor,α, offers a credible statement that if either $\{X\}$ and/or $\{C\}$ had not been the case on the occasion in question then s(he) would not have acted $\{Y\}$ to realize $\{Z\}$ then, $\{X\}$ and $\{C\}$ are, on the occasion in question, necessary for action $\{Y\}$ and the realization $\{Z\}$. These occasion-specific conclusions hold true even if there are possible, but not realized, alternative subjective causes of α's action to realize Z. These alternatives, whist not operative on the occasion in question, may be recognised by the actor/informant and, if so, stand available for elicitation. This then invites the question as to why action $\{Y\}$ was chosen rather than an alternative course of action which, once again, is potentially open to exploration by elicitation (Chapter 3).

Turning now to the possibility of elicited probabilistic statements; uncertainty is most likely to enter α's belief whereby action $\{Y\}$ will realize $\{Z\}$. So, an ethnographer may elicit the statement revealing that the actor, α, believed that action $\{Y\}$ would, on the occasion in question, only probably realize $\{Z\}$. Thus, the action explicitly becomes experimental (or an attempt). If the ethnographer can observe whether the outcome of the action, Z, does or does not occur then s(he) is in the position to construe the action as successful or not As we noted earlier the ethnographer is found, when eliciting probabilistic statements, to be in a position of ascribing credibility to subjective probabilistic statements. Probabilistic subjective statements connecting $\{C\}$ and $\{X\}$ to act $\{Y\}$ may also be elicited and will raise the same issues (Chapter 3).

A case-study, be it about an individual, or a group (collective) or even an historical period, often implies repeated observations, usually organized longitudinally, conceived as a single unit of analysis. The unit may, of course, be constructed from a large number of sub-units, but predicates at the level of the major unit are characteristically deployed (Abell 2001). This implies that the interplay of causal connections at differing levels of abstraction (e.g. micro, meso and macro) become of central concern. In particular how is macro-ethnographic (e.g. "group action") causality related, if it is at all, to micro ethnographic causality (Chapter 4)?

Many case study enthusiasts appear to take the view that the more extensive and detailed descriptions prove to be, then the more revealing is the case. But surely locating what causes what is the most revealing feature of a case. However, focusing upon causal connections in case studies, and our conception of ethnographic causality mandates concentration upon chronologies of actions and events which are constructed in a very particular manner. Events only feature to the degree that they are the causes and consequences of an identified ethnographic causal connection created by an action/forbearance or sequences thereof. In this respect the methodology we will advocate departs from some apparently similar procedures like process tracing (Bennett and Checkel 2015). This restriction, to a degree, redirects case studies. The case will comprise of a chronology of actions/forbearances and events but the latter derive solely from credible subjective causal statements about the causes and consequences of the actions which are elicited from actors or informants. When the paths of causal connections are inserted amongst the actions and events, using Bayesian methods to be outlined below, we arrive at a Bayesian Narrative; narratives are then conveniently depicted as directed acyclic and graphs.

Since case studies are almost invariably prosecuted by gathering qualitative data (often couched in a natural language format), the issues surrounding their analytical potential have become entrammelled in the debate about the relative virtues of quantitative and qualitative analysis. The essential point explored in this volume, though, is not so much one of quantity versus quality, but rather whether and how causal analysis can be convincingly prosecuted in the absence of comparison when either $N = 1$ or is low.

2. Large and Small *N* Causal Inference: The Role of Comparison and Generalisation

The social sciences, perhaps with the exception of economics (e.g., North, 2005), are, as we noted in Chapter 1, beset by a continuing debate, sometimes rather rancorous, between the advocates of "quantitative" (i.e. broadly speaking statistics) studies and "qualitative" (often in the form of in-depth case-based studies) investigative approaches. But as we also noted a more revealing expression is large *N* versus small *N* where *N* stands for the number of observations gathered, either in cross-section or longitudinally, from which causal connections may be both identified and estimated. The question we wish to pose and answer in this Chapter is why many social scientists are persuaded to take a large *N* perspective to causality when others, including philosophers, argue that singular causal inferences are feasible? Many of course settle this issue by resisting the possibility of singular causality, finding singular inferences highly questionable.

But why search for causality in the first place? Many find richly descriptive historically based case studies to be much more revealing of "what was actually going on". Further they feel that attempts to generalise inevitably ignore details that in some manner impair our deep understanding. If this contains a kernel of truth then advocates of causal analysis must find ways of addressing this issue. Our claim is that counterfactual reasoning in the context of multiple conjunctive causes achieves this but singular counterfactuals must also be rendered tractable.

When we ask why an observed event, B, pertaining to a particular unit of analysis, happened/came about/was brought about/is the case, it seems entirely natural to search for a cause, A, of B and to retort, "because A caused B" maybe with the help of some descriptive context C. Though the predicates A, B and C each refer to a singular event, they may be descriptively complex, comprising of conjoined descriptors. However, the retort “because A causes B” suggests some generalisation of the relationship, possibly implying the prediction that “A will always cause B” (again in context C). In the context of generalisation we might say that “A is a cause of B” implying there may be other possible causes of B along-side A. This, in turn, invites the query as to whether the causes are conjunctive or alternatives, a situation that does not normally arise in the singular case where alternative possible causes cannot be operative (but see subjective over-determination in Chapter 3).

The contextual counterfactual (sometimes called a subjunctive conditional) definition of causality (Lewis, 1986), $A \rightarrow B$ we shall adopt is as follows, when observing the occurrence of *A* prior to *B*: All other things equal in *C*, *B* causally depends upon *A*, if and only if *A* had not occurred in *C*, then the probability of B occurring would be significantly diminished compared with its probability if *A* occurred. Thus, the occurrence of *A* normally increases the probability of *B* occurring.[1] This sort of definition is not without its critics (Hall 2004, Woodward 2005) but fits well with the practice of social science (Eells 1991).

Events *A* and *B* are distinct observed events or actions/forbearances (Chapter 1). The phrase other things equal in *C* (ceteris paribus) confers a variety of important restrictions upon the definition which enable refinement of the simple definition.

Although more often than not, counterfactuals are conceived in terms of the absence of a cause (often labelled as a treatment or assignment) the concept is essentially symmetric. Thus, prior to a unit of analysis being exposed to a cause it is possible to entertain the proposition that, if the unit were, in *C*, being exposed to *A* would bring about *B*. Thus, the presence of the cause is the counterfactual.

1 Event *A* may decrease the probability of event *B* if it counteracts the impact of an alternative cause.

The counterfactual definition suggests a conditional probabilistic interpretation of causality, $P(B|A,C) > P(B|\neg A,C)$, of which the deterministic version is a special case, when these probabilities are respectively one and zero. Casting the definition in probabilistic terms immediately enjoins a consideration of comparison and generalisation (i.e. large *N*), if the conditional probabilities are to be interpreted in terms of observed frequencies. We shall, however, eventually entertain a Bayesian, strength of belief interpretation of probabilities, which may not be so dependent, making way for singular/token causality not depending upon frequencies. We can thus pose the fundamental issue to be addressed in this Chapter – why is a comparative generalising, large *N*, perspective regarded as essential for the identification and eventual estimation any causal connection?

Expressing this question in philosophical terms – why is type level causal inference (events of type A cause events of type B) given priority as an objective over token level causality (this *A* caused this *B*)? We noted earlier that philosophers have promoted the possibility of singular or token causal inferences, but many social scientists of the Large *N* persuasion seem to remain unconvinced that non-comparative singular case studies can surrender any causal conclusions. The reasons behind this are manifold but largely reside with the phrase "all other things equal" within the counterfactual definition. The observation of counterfactuals, covariant variables, conditioning variables and confounding variables, usually entail comparisons which, in turn, enjoin a large *N* (either across observed units of analysis or across time or both). But how can case study advocates evade these sorts of considerations if and when they promote singular causality?

Social scientists often wish to make predictions where comparisons derivative of type level general causal relations are usually involved. As we noted in Chapter 1 causal connections were, until quite recently, interpreted as derivative of causal laws, thus a token causal connection was always to be deduced from a general causal law and social science was more or less consciously conceived in terms of establishing such laws and then predictively applying them. And, indeed, counterfactuals were often construed as demarcating the distinction between general laws, implying a counterfactual and "accidental generalisations" which do not. This way of looking at things, following Hempel (1965) became known as the Deductive-Nomological Model. However, laws, although not entirely unknown in social science, are hard to come by and social scientist (along with philosophers) now rarely indissolubly tie causality to this model. Nevertheless, establishing and applying causal generalisations – even if not laws- has remined the central preoccupation of most social scientists who seek to establish causality. A notable implication of this is that the quest for causal connections between variables, based upon frequencies, implies a definition of the population of comparative units of analysis across which the causal connection operates. In effect, the context *C* may involve the definition of the population but defining populations in the social sciences does not prove to be an uncontroversial matter (Rosenbaum, 2002). Let us start, however, with Small *N* causal inference.

2.1 Small *N* causal Inference

A starting point for small *N* studies is the recognition that actors/agents can themselves often provide descriptions and sometimes even causal explanations of what they are doing, have done and may, in the future, do (and forbear to do etc.) often expressed in their own, culturally derived, vocabulary (discourse). Further, particularly in situations where actions are repetitive and socially institutionalised (i.e. follow established normative expectations, see Chapter 5), informants may also provide causal explanations of others' actions. Clearly any causal explanations, which may be delivered by actors/informants will be predicated upon their own descriptive and explanatory resources which may vary across actors, cultures and circumstances. If subjective causal explanations are deemed to be at all credible, then this carries the additional assumption that agents have a contextual self-understanding of what drives their activity. If they possess such understanding (or perhaps can be coaxed into it by an ethnographer), then their causal explanations should arguably engage the attention of social scientists. As we intimated in Chapter 1, the ethnographer and actor/informant may be conceived as socially constructing the description/explanation which is revealed in the process of elicitation initiated by the ethnographer.

Although ethnographic case studies are not indissolubly tethered to ideas of the social construction of the semantics of an agents' vocabulary, they are often closely allied. The social construction of a vocabulary implies that its embodied concepts are derived and negotiated within the framework of social interactions. In the present context this implies the interaction between a subject/informant and an investigative ethnographer. Since the conception of ethnographic causality developed in later chapters endows the ethnographer with the responsibilities of estimating the credibility of informants' causal statements it is probably desirable that any estimation procedure is consistent with constructionists' precepts (Berger and Luckmann, 1966). Although we are not firmly committed to an out and out "constructionist" position the inferential models outlined in Chapter 3 are formulated in a manner which, we believe, can be made consistent with this standpoint, whilst not explicitly enjoining it.

Inter-case comparisons in pursuit of generalisations (regularities) are, to a significant degree, played down by case (small *N*) analysts in favour of unique or, at best, a few similar in-depth descriptive cases. It is the virtue of descriptive depth or detail which is deemed to be important and which, is often claimed, to be lost in large *N* studies. Demanding descriptive detail creates little intellectual friction if the objective is only to describe what is going on, usually chronologically in selected cases. But causal explanation is another matter.

Many small *N* case analysts avoid causal inference and believe that searches for generalisations in large *N* studies necessarily involve unwarranted over-forced comparisons across otherwise descriptively unique or not sufficiently similar cases. In this respect case-based studies tend to find common ground with some historical scholarship (Carr, 1987) though judicious comparisons, across historical periods, are sometimes entertained by historians. The rich literature on qualitative analyses (Miles and Huberman, 1994) like constant comparison, keywords in context and domain analysis with attendant software like NVIVO and CAQDAS barely approaches issues of causal analysis (Fielding and Lee, 1998). Comparison however does feature, centred around finding similar descriptions (codes) in "natural settings". Since, however, causal analysis is always dependent upon prior description the literature prompts the question as to how much local complexity may be surrendered to enable causal analysis? The obvious retort would seem to be that descriptive complexity should be surrendered only to the degree that causal predictions can be secured which once again implies some generalisation.

Peter Winch's book, The Idea of a Social Science (1990) strongly influenced by the late Wittgenstein (1953), lead the charge against the idea of a generalising social science but did advocete the idea of "family resemblance". Indeed, much may not be lost by interpreting social construction as the fabrication of family resemblance or similarity and consequently, dissimilarity between cases (i.e. in our terms, sequences of causally connected actions, events etc. in narratives, Chapter 3).

Most large N studies, are constructed around finding identity/equivalence, rather than merely similarity/resemblance, when comparing units of analysis. Thus, when ascribed identical scores on a given variable, units always fall within mutually exclusive and exhaustive equivalence classes of one sort or another. The nominal level then provides the basic level of measurement and the ordinal level ranks the equivalence classes and the metric levels (interval and ratio) introduce distance between them. So, those units which fall under a particular score are all deemed to be in a reflexive, symmetric and transitive relationship one to another. However, similarity is, a reflexive and symmetric but not necessarily transitive relationship. Thus, if A is similar to B and B is similar to C then it is not necessarily the case that A is similar to C. Similarity may therefore not unequivocally assign units into equivalence classes with clear boundaries. In the context of the generalisation of singular ethnographic causal inferences, we find ourselves in a situation where sets $\{X\}$, $\{Y\}$ and $\{Z\}$, which recall characteristically contain conjunctions of natural language components, may only be similar across cases. Ethnographic causal analysis is, thus, propelled in the direction of causal inference in tolerance or similarity spaces (Chapter 3).

Furthermore, as we argued earlier, ethnographic causality leads us in the direction of narrative causal networks embracing multiple causal links. Thus, the similarity between such networks, (i.e. a mapping between networks preserving similar features) which claims to show how similar outcomes/effects are generated, becomes the focus of attention. Colloquially, this amounts to a search for sufficiently similar, but not necessarily identical, causal stories which enable us to generalise that the "same sort" of causal generation is present (Chapter 3).

There have been many attempts to bridge the gap between "qualitative" and "quantitative" analysis in the social sciences. Qualitative Comparative Analysis (QCA) (Ragin, 1989, 2000) is perhaps the most successful attempt, applying a Boolean analysis to a handful of cases each exhibiting the presence or absence of a set of conjunctive binary causal variables and the presence or absence a binary outcome (effect) variable. A Boolean equation then relates a Boolean outcome (effect) to the sum of alternative sets of Boolean causal variables, each set comprising of a conjunction of the presence and absence of sets of Boolean causal variables. Thus, each alternative set represents one or more causally identical case(s). Reducing a set of such Boolean models to its prime implicants can then surrender a parsimonious, but deterministic, causal structure. The presence and absence of a binary variable can appear in alternative conjunctive causal sets. However the other variables in such sets must contain different binary variables which is consistent with our contextual definition of causality. An illustrative example may help:

$$\text{Effect} = (A \wedge B \wedge C) \vee (D \wedge B \wedge \neg C).$$

Here, the set comprising the presence of *A* and *B* and *C* or the set comprising the presence of *D* and *B* and absence of *C* are each sufficient for the effect.

Although this may appear far from a social constructionist's picture of things, QCA does enable a limited form of causal generalisation with only a few comparators, where binary variables can be carefully extracted from ethnographically rich descriptions. Abell (1987) suggested a version of this approach where the Boolean structure is generated in terms of the presence and absence of paths of causality between named variables.

A ready criticism of QCA was its apparent deterministic nature, but which is, nevertheless, sometimes justified by asserting that detailed ("thick description") case studies are observed without error. QCA has, however, been extended into fuzzy set analysis (Ragin, 2000) where this controversial assumption is relaxed. But both the deterministic and fuzzy versions do clearly involve systematic comparison of cases in order to estimate any causal connections. They maintain the large *N* mantra that causal explanation is posterior to comparison of cases.

2.2 Small and Large *N*

The purpose behind this book is to fashion some common ground about the limits of both "quantitative" (large *N*) and "qualitative" (small *N*) analysis, leading to an acknowledgement of their complementary roles in causal identification and estimation. Large *N* observational studies embrace explicit standards for both causal identification and estimation. Many observations are required either longitudinally or in cross-section, hence *N* is "large". Indeed, *N* is large enough to enable the control of confounding variables (i.e. identification) and to render tests of significance operative (i.e. estimation) when only a sample is observed. This leaves little room for one or just a few (*N* is "low") case studies, net of their possible exploratory role as a preface to statistical modelling. Any repeated observation internal to a case does not, as we have observed (Chapter 1), characteristically generate a time-series of identical events, but rather a chronology of diverse events and individual and collective actions which is not always amenable to time series analysis and Granger (1969) causality. The insertion of action driven ethnographic causal links into a chronology of events produces a narrative (Abell 2007).

However, in order to warrant the validity of ethnographic causal inferences it will prove necessary to address the issues solved by large *N* identification of causal influence in terms of appropriate comparison, counterfactuals and generalisation. Namely, identifying counter-factuals, defending against confounders and controlling the impact of alternative causes in the absence of, or at least with only very limited comparisons and generalisation across cases.

Before introducing narrative ideas in a little more depth (and more completely in Chapter 3), we shall examine the nature of causal inferences in large-*N* studies and then explore the apparent achievements and limitations of such studies.

2.3 Large *N* Causal Analysis

Here we are not concerned with the details of statistical techniques, but rather with the underlying logic of which ever technique is adopted to identify causal links given the available data. We shall predominately assume that the focus is upon observational studies, not upon carefully controlled randomised experiments, since the number of observational studies far outreaches the number of experimental studies in the social sciences. Experimental studies do allow for careful causal inferences, sometimes even with only a few observations (low *N*), through the device of randomised controls where the investigator is able to assign the experimental treatment. In observational studies the investigator is not in a position to select treatments, though the distinction between experimental and observational studies has been eroded by recent developments of the do-calculus (Pearl 2009) which allows the investigator to intervene and identify and estimate causal links in the context of observationally based studies, often depicted as directed acyclic graphs (DAGs) operating under strong (Markov) assumptions about the independence of its error terms (Fig 2.2 below). The a-cyclicity rules out the possibility of causal feedback.

A necessary, though far from sufficient, component of any inference of a direct causal effect, in the large *N* tradition, is a systematic co-variation of the cause and effect variables conditional upon controlling for any measured and unmeasured confounders and possible alternative causal paths between the cause and effect. As Pearl (2009) has observed, additional causal assumptions are always required, over and above knowledge of the joint probability distribution of the variables involved, for any secure causal inference to be made. The elementary treatment of both observed and unobserved confounding variables is a case in point (see below). The joint probability distribution of cause and effect, conditional upon any potential confounders will alone not distinguish between alternative DAGs, such as between: $X \rightarrow Y \rightarrow Z$ and $X \leftarrow Y \leftarrow Z$. Distinguishing between these models is usually achieved by invoking an additional theoretical assumptions, such as the time ordering of the variables.

Whilst the statistical estimation for causal inference depend upon sample size, causal assumptions are not so dependent; they rather contribute to the possibility or otherwise of the identification of a causal link (i.e. being assured that that any estimation of the parameter is indeed causal) prior to any estimation procedure.

Despite accusations mounted against its supposed metaphysical foundations (Dawid, 2000), one of the most influential conception of causality in the statistical, large *N* tradition is the potential outcomes model due to Rubin (2005). To establish a causal link connecting *X* (cause) and *Y* (effect) for a particular case (unit of analysis), evidence is required for that particular unit, giving the value of *Y*, both in the presence and the absence of treatment, i.e., different values of *X*. That is to say, counterfactual ideas are inevitably invoked. However, in neither a cross-sectional observational nor in an experimental context are both immediately accessible on the same occasion and in longitudinal studies (including test-retest experiments) inevitable dependence of values of the effect variable at different points in time rapidly creates analytical complications. That is, we can rarely observe the exposure and the non-exposure of the same unit in the very same conditions. Moreover, we can rarely guarantee that a unit's exposure is independent of its potential outcome.

Estimating statistical expectations across an assumed homogeneous population of units in either the presence or the absence of *X*, whilst controlling for confounders, is the standard inter-unit (as opposed to intra-unit) comparative and generalising way around this problem. However, this procedure is usually adopted under the auspices of the Stable Unit Treatment Value Assumption (SUTVA) (Cox 1958, Heckman, 2005). SUTVA is however, especially with human subjects, a rather fragile assumption; it requires, among others, that the potential outcome of a causal exposure for each unit under investigation is not influenced, one way or the other, by the causal exposure of the other units.

When we search for the causes of human actions/forbearances then what one person does is often dependent upon what others do and what drives them. If people self-select into the factors that drive their own actions then both others' actions and the causes of their actions may determine the person's causes and actions. In addition, self-selection makes it very difficult to ensure that treatment is independent of potential outcome.

If, however SUTVA fails then, in an experimental context, individual units subject to a treatment variable ($X = 1$) and a possible network of interactions with other actors must be compared with units exposed to neither ($X = 1$) nor the network. In observational studies this requires a conditioning to remove any network or group/macro effects in order to estimate the individual level effect. Needless to say, this imposes substantial constraints upon the ease with which statistical, large *N* models can surrender causal conclusions. It is salutary to realise that social science often involves the study of causal processes in the context of contagion when individuals copy or avoid copying the actions of others. So that which causally generates the action of one individual may have an impact upon others. If an individual observes others doing *Y* in *X* then this may not only cause further action, on the individuals behalf, generating social interaction, but induce the actor to, either more likely or less likely, copy the original action herself.

In the face of this complexity it is perhaps appropriate to pose the question as to whether individual case studies may offer some help (Chapter 3). Such studies would factor in subjective reports of the causal impact of networks of social interactions upon human actions (Chapter 4). Ethnographic causality may allow actors/informants to identify network relationships (interactions) as influencing their actions.

Recently there have been some misgivings aired (Deaton, 2010, Morgan and Winship, 2015) as to the sometimes-careless assumption whereby putative causal variables, comprising of an aggregate of a number of constituent "indicator variables" given a "theoretical" designation by the investigator, are the most apposite starting point from which to infer stable causal connections. For instance, the variable socio-economic-status (SES) which characteristically combines several "indicator variables" (e.g. income, education, etc) could be a candidate case. The argument advanced is that only with a detailed understanding of the causal mechanism's connecting each of the constituent indicator variables to the effect can causality be properly understood (Reed, 2011). In this respect Cartwright (2007) is particularly critical of establishing causal relations in the social sciences, writing of "imposter counterfactuals" derivative of aggregated causal variables. No doubt many case study enthusiasts would concur. Those who do embrace causal inferences urge that detailed case-based studies can more effectively address causal complexity, though they are rather silent as to how causality is to be reliably identified and inferred. However, insofar as investigators commence ethnographic studies using the vocabulary of the actors themselves, then aggregated concepts are unlikely to be elicited.

The impractical consequences of unpacking all the constituent variables in a theoretically aggregated causal variable are clear. To use the social scientists' terminology, each of the "operational variables" entering a theoretical causal variable would need to be separated out and stand alone in a highly complex causal structure (Pearl, 2009). It is difficult to envisage large *N* analysis taking a path devoid of aggregate variables. However, if we dispense with the requirement that causal analysis necessarily involves generalisation across inter-unit comparison and habilitate a singular causal concept then complex causal stories can become the focus. Fairfield and Charman (2017) have argued persuasively that iterated Bayesian inference can play a central role in this respect. They contrast deductive theory testing and inductive theory building, prominent in the large *N* framework, with the accumulated assembly of evidence (ignoring order) modifying prior judgements of the odds of competing hypotheses in a Bayesian inference. If one takes the view that causality can only be studied through the establishment of generalisation then a focus upon a specific problem must be located in a large *N* study. If, however, causal inference can be vouchsafed by appropriately studying a focal case then social science takes on a different mantle.

Whatever the sophistication of the statistical large *N* model chosen, the underlying logic of any explanatory causal inference is manifestly clear: Both inter-unit comparison and generalisation are necessary prerequisites for any causal inference; no explanation without comparison and generalisation. A particular ordering of a trinity is established whereby the determination of a causal explanation is always posterior to comparison induced generalisation (co-variance). As we shall see below, ethnographic causality will reverse this ordering (Abell 2009).

2.4 What has the Large *N* Approach Achieved?

In our view the answer to the question posed is; most of what we have learned about the social world. Notwithstanding, Figure 2.1 depicts the average variance explained (when reported), in empirical papers published from 1960 onward, in the American Sociological Review; one of the world's leading social science journals. For good reasons, maximising variance explained is not the usual objective of empirical researchers, nor are many studies explicitly directed at causal explanation. It is worthy of note, nevertheless, that despite the unprecedented advances in statistical analysis and the burgeoning availability of data since 1960, the average variance explained has not improved and remains rather modest. With the accumulation of published studies and subsequent incorporation of control variables into models, might we not expect an upward trend? Alternatively, it could be conjectured that statistical modelling of social phenomena is about as good as it is going to get, reflecting the fundamentally stochastic nature of social phenomena. Note this would, if we are to sustain the standpoint of universal causality, imply that there is a multitude of additional causal variables in operation. Maybe also early low hanging fruit is partially responsible for the flat profile. But whatever interpretation we care to put upon the analysis, it invites the question as to what we might reasonably expect in the future? Will the recent development of big data and perhaps improved statistical modelling likely improve the situation? This query is difficult, to give a definitive answer to. But these observations do, we believe, give us ample grounds for thought. If we add to this the recent head-scratching about the adoption of standard levels of significance (McShane, Gelman and Tackett, 2019), which have become accepted as a guide to explanatory success and thus warranting the publication of all the papers which pass the test, it is perhaps not unreasonable to wonder as to where large *N* social science is headed.

Perhaps a refocus upon the detailed causal analysis of singular complex contemporary historical causal stories rather than general causality may play a more prominent role? To put it another way, whilst still preserving the concept of universal causality, increased emphasis may be given to the idiosyncratic rather than general. Furthermore, do we believe that causal connections involving the generative power of actions will, in the future, show the stability that warrants treating any derived parameters as fixed (rather like physics)? We can conceive of causal analysis as seeking to explain general outcomes, general causes and general connecting actions providing a causal mechanism. Many large *N* studies centre attention upon explaining general outcomes – by posing the question: "what are the causes of the repeated outcome?" Occasionally the question becomes what are the outcomes of a general repeated cause? Ethnographic causality is best interpreted as seeking to answer the question as to how general an action (or sequence of actions) is in connecting a cause and an outcome (Chapter 3)?

Advocates of the virtues of case studies are deeply sceptical about the existence of large populations of causally homogeneous cases open to statistical sampling and treatment. They prefer carefully selected case studies, using detailed ethnographic techniques, which make no claim to be representative of a population. As we have noted above it is, however, difficult to comprehend how causality (if it is embraced at all) is inferred in such situations, beyond the instantiation of already established generalisations (from large *N* studies).

How should we react to the apparent limitation of current statistical practice and the sceptical claims of ethnographic case study researchers? Firstly, we should always be open to the incorporation of improved statistical modelling and developments in causal analysis. Nevertheless, we shall argue below that we need to search for an alternative and complementary approach to causal analysis which is appropriate in small-*N* situations. Let it be clear we are, unlike others, not seeking to substitute small-*N* approaches for statistical modelling but rather pondering whether each could play a significant role under their respective appropriate applications. It might be useful in this general context to distinguish between (1) large *N* studies that assume that causal inferences can be achieved without resort to small *N* cases (2) Large *N* studies which embody

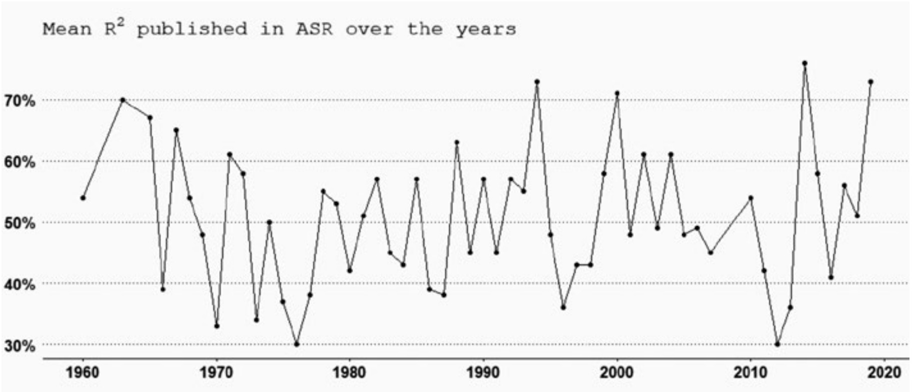

Figure 2.1 *Mean R^2 values published in the American Sociology Review over the years (based on Abell and Koumenta, 2019)*

in various ways small *N* case studies which "fill the gaps" between variables and (3) small *N* studies with little pretention to any generalisation.

Can case-based studies, usually longitudinally formulated, play a role in causal analysis which is more than merely exploratory or "gap" filling? There is little disagreement that exploratory case studies can prove to be highly suggestive, providing causal insights which can subsequently be embodied in large *N* statistical models. Such insights are usually delivered in a manner where causal events comprise of complex conjunctions of sub-events. The logical prerequisites for a single or just a few cases to directly surrender causal information are, however, clear; the large *N* "trinity", whereby inter-unit comparison and generalisation (co-variance) are prerequisites for any causal explanation, needs to be inverted. Thus, causal explanation is logically prior to any possible inter-case comparison and thence tentative generalisation. So, we may then cogently ask how general a given case based causal explanation is, as revealed by inter-unit comparison across, a limited set of similar explanatory case studies. Note, that even if *N* were to be large enough, this is not a procedure equivalent to statistical induction because the question being asked is not whether there are sufficient grounds for inferring a causal connection but rather how general the already established causal connection is. It is, thus, important to draw a line between this procedure and the standard inductive and deductive nomological models of scientific explanation. Any limited induction is rather established across a small N number of established singular causal explanations. We shall argue in more detail below (Chapter 3) that Bayesian Narratives (Abell, 2009) provide the appropriate vehicle in this respect but now briefly introduce the central ideas here.

2.5 An Introduction to Bayesian Narratives

Ethnographic Bayesian causal analysis offers a method where estimates of the odds of each causal link are inserted into an evolving chronology of events and their connecting actions (Chapter 1 and Chapter 3). A chronology along with inserted causal links generates a Narrative Network where the nodes are both the actions and the events and the directed edges are the causal relations. The events and possibly further actions are, as we noted in Chapter 1, usually derived from subjective causal statements of the form "I/we acted $\{Y\}$ because of $\{X\}$ to realize $\{Z\}$" elicited by ethnographers, from the actors themselves or maybe from informants. Thus sets $\{X\}$ and $\{Z\}$ will generally contain actions and events derived from the actor/informant reporting upon the focal action described as $\{Y\}$.

The derived Narrative can then stand alone as a singular causal explanation of how the starting events/actions causally generate the final events/actions. However, if a small number of case-based narratives are available the question may arise as to whether they can be generalised (small N induction of singular explanations). This amounts to posing the question as to whether two or more narratives are sufficiently similar (essentially recounting the same story) to warrant a limited generalisation (Chapter 3).

Although our target in this volume is the analysis of ethnographic causality we have suggested in chapter 1 that there is some significant similarity between the basic ethnographic causal structure, as we see it, and the standard mediation and common antecedent (spurious-variation) models in the large N statistical tradition. The distinction may be caught by contrasting the implications of adopting (from chapter 1) generalisable statistical causal models: ($X \rightarrow Y \rightarrow Z$) and ($X \leftarrow Y \rightarrow Z$),

where X, Y and Z are variables (possibly ranging from simple binary variables to counts to continuous ratio level measures) and where there may be additional direct causal links running from X to Z. These models were compared with:

$\{X\} \rightarrow (\alpha$ action or forbearance $\{Y\}) \rightarrow_T \{Z\})$,

$\{X\} \leftarrow (\alpha$ action or forbearance $\{Y\}) \rightarrow_T \{Z\}$,

where $\{X\}$, $\{Y\}$ and $\{Z\}$ are, more likely than not, sets of conjunctions of natural language descriptions and α is a designated actor. In the second model the causality of $\{X\}$ may be described as self-selection into the causal state (see Chapter 3). The singular ethnographic causes (explanations) may or may not, post explanation, permit limited generalisation. For the moment we ignore the important possibility of an additional direct causal connection running between $\{X\}$ and $\{Z\}$. But note that if this were to exists then from the ethnographic perspective, if $\{X\}$ and $\{Z\}$ are events, then further action driven mechanisms must be introduced to complete the picture depicting the operation of the generative mechanisms. Since the causal structure is singular any such introduction would involve an additional actor though the causal mechanisms may involve complex narrative networks. In the large N tradition the basic variable based models are also strung together into networks of causal relations, often DAGs (see below).

All the statistical models, running from the best designed random experimental trials to various versions of causal inference based on observational data (e.g. instrumental variables, regression discontinuity, matching, difference in differences etc.) depend upon drawing a sample of comparative cases (units of analysis) and generating inferences from multiple observations. It proves instructive to see how extensive comparison and concomitant generalisation arises within a potential outcomes and counterfactual framework, which may become unfeasible, making way for complementary singular ethnographic causal inference.

2.6 Potential Outcomes and Counterfactual Causal Analysis in large *N* Studies: The Role of Inter-Unit Comparison

Full expositions of the Potential outcome/counterfactual approach to causality are available in many places, but Morgan and Winship (2015) probably best serve social scientists. Unlike in standard expositions, we shall explicitly introduce time into the argument to enable eventual application to occasion specific analysis inherent to ethnographic causality. Consider, in the context of a large N study, a simple causal connection $X \rightarrow Y$ where X can randomly take on one of two values (0 or 1) and Y may be a random variable with distributed scores at any level of measurement. The causal variable X may take on more than two values when the following reasoning applies to any pair of its values.

Adopt a potential outcome (or counterfactual) perspective to causality. Then we may conceive of every unit in the defined population (technically infinite in size from an estimation standpoint) as capable of being either exposed to the cause, $X = 1$, generating a potential value of the effect, Y^1, and not exposed to the cause, $X = 0$, generating a potential value Y^0. Y^1 and Y^0 are thus latent random potential variables each varying in value across all the individual units in the population. It is often difficult to define the population and indeed whether potential outcome values should be deemed as fixed for all time across the individual units (Rosenbaum, 2002). With an eye to comparisons we might thus allow for within unit variation of the unit level potential outcomes (Y_i^1 and Y_i^0) or expectations, ($E[\![Y^1]\!]$ and $E[\![Y^0]\!]$). Intra-unit variation of potential outcomes may arise when the causal analysis is occasion specific (Chapter 3).

Consider a single unit, i, drawn at random from a given population on a particular occasion at time t. Then, using the lower case to depict individual scores:

Y_{it}^0 is the potential outcome, for unit i, if X_i were to be equal to 0 at time t

Y_{it}^1 is the potential outcome, for unit i, if X_i were to be equal to 1 at time t

The singular causal effect (CE) of X on Y, for unit i, on the occasion at time t, could then be given by the linear relation:

$$CE_i = Y^1_{it} - Y^0_{it}$$

$$CE = E[\![Y^1 - Y^0]\!]$$

That is, the difference in potential values of the effect variable for the unit in question. However, since both potential values cannot be observed on the same occasion the causal effect is not immediately identifiable. This remains true for all the other sampled units (cases), only one of the potential outcomes can be observed for each unit on a specific occasion, t.

There are then two possible strategies to address this problem – compare units of analysis and compare the same unit on more than one occasion, assuming the potential outcomes are fixed. Both strategies thus propel analysis in the direction of increasing the number of observations beyond the singular case.

Consider, first, the comparison between just two units of analysis (cases), i and j, where i has potential outcomes as detailed above and j has potential outcomes, again on occasion t, as follows:

Y^0_{jt} is the potential outcome, for unit j, if X_j were to be equal to 0 at time t

Y^1_{jt} is the potential outcome, for unit j, if X_j were to be equal to 1 at time t

Assume the actual observed outcomes are $Y^1_{it} = y^1_{it}$ and $Y^0_{jt} = y^0_{jt}$ then, if we compare i and j's values, with the perspective of making a causal inference, we arrive at

$$CE_i = y^1_{it} - y^0_{jt} = \left(y^1_{it} - y^0_{it}\right) + \left(y^0_{it} - y^0_{jt}\right)$$

Thus, only if $y^0_{it} = y^0_{jt}$ (i.e. the absence of selection bias) will the difference in the observed values, y^1_{it} and y^0_{jt}, potentially reveal a reliable indication of the causal connection, for i, between variables X and Y. Even if separate samples of units which are exposed and not exposed to X are drawn and the average values of Y^1 and Y^0 computed then unless the average difference between Y^0 for the two samples are zero then any causal inference is defeated.

Observing a single unit over time when the value of X varies may deliver a singular causal results (*CE*). After all many small *N* case studies take this form though as we commented in Chapter 1, systematic time series of X and Y are rarely achieved. If they are then for a single unit, if Y is not persistent, then a singular causal effect may be estimated.

Drawing a sample rather than just two units to compare, which puts the analysis firmly in a large *N* framework, would not solve the problem of selection bias as taking averages over the above equation for *CE* would still involve an average selection bias.

Eradicating average selection bias brings into prominence the problem of guaranteeing that "other things are equal." when comparing units in order to achieve a comparative counterfactual causal inference. When other things are not equal, this eventuality may, from a causal standpoint, be interpreted as being indicative of:

- Other causes (covariates) of Y, either in conjunction with X or as alternatives to X,

- Confounding causes of both X and Y, (i.e. common causes of X and Y).

These causes may be observed or unobserved and the latter may, in practice, be potentially observable or not. So the issue arises as to how these additional causes should be incorporated into the picture? The initial answer is that X causing Y will characteristically be embedded in a network of further causes.

Figure 2.2 depicts a causal network (see Appendix) with "error terms" (U terms) which would indicate unmeasured/unobserved causes (see below). C is a covariate of X and D's impact on Y and D confounds X and Y. Notice that variable X now mediates between variables D and Y and C mediates between

X and *Y*. It is clear that if this is to be considered credible causal model then in order to estimate the direct causal impact of *X* upon *Y*, the cofounding and mediating paths between *X* and *Y* must be controlled. Thus, inviting gathering data in order to make the appropriate comparisons (i.e. enter large *N*).

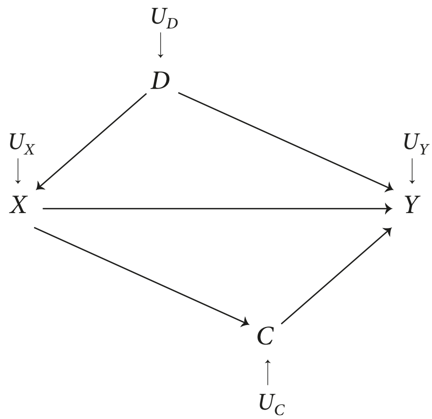

Figure 2.2 *A causal network with error terms*

The causal impact both C and D must be taken into account when identifying and estimating our target $X \rightarrow Y$. They must in some way be controlled or randomised out (i.e. their causal effects eradicated) if causality between *X* and *Y* is to be revealed. This inevitably involves extending the comparisons. We need enough comparative observations/cases to control or randomise out the other causes. Randomised controlled experiments provide the prominent procedure for so doing though in practice only observational techniques are feasible.

Causal inference in the large *N* tradition is usually regarded as most easily achieved in experimental randomised trials which, for many, stand as the gold standard. The difference in the sub-sample average *Y* scores for those assigned to *X* and to not *X* is taken to be equal to the average causal effect of *X* upon *Y*. Interpreting randomised assignment causally, we may say that the assigned sub-samples will be identical in terms of all observable and unobservable causes, apart from *X*, or at least sufficiently similar in this respect.

The virtues of experimental randomized trials, despite attracting the accolade of the gold standard for causal inference, have not gone entirely uncontested. Several authors have expressed reservations (Cartwright 2007, Deaton 2010, Cox 1958) particularly in respect of their generalisability (sometimes referred to as external validity) beyond the actual experimental context. Manski and Garfinkel (1992) suggest "[…] there is at present no basis for the particular belief that extrapolation from social experiments is less problematic than extrapolation from observational data". Attempts to address the criticisms of randomised trials have involved post-trial subgroup analysis which in effect combine regression with trial results.

Pawson and Tilley (1997) argue that it is a combination of a mechanism and a context that delivers generalisable conclusions beyond a specific experiment. Those who are sceptical about the application of experimental results emphasis the theoretical understanding of mechanisms which show why any treatment has the impact it does (Deaton, 2010). An examination of the literature about connecting mechanisms propounded by the advocates of large *N* suggest that even if the mechanism (theoretical or observed) is expressed as a variable (often binary) the thinking behind the insertion is more often than not derived from conceptions about human activity. Indeed, chains of actions (i.e., interactions) are contemplated which in the ethnographic framework we refer to as narratives (Chapter 3). This reveals some common ground between ethnographic and statistical causal inference but also invites the query; if the mechanisms connecting the cause and effect are complex and multiple (i.e. multiple causal paths) can enough comparative cases be located to enable a large *N* inquiry?

2.7 Causal Analysis in Large *N* Observational Studies

The basic large *N* mediation model $X \rightarrow Y \rightarrow Z$, post Lazarsfeld and Rosenberg's (1995) initial development, was eventually analysed, almost invariably, in terms of additive linear regression equations. The linear additive recursive structure for the basic mediation model, with a possible additional causal link also running directly from X to Z became the standard framework for estimating mediated causal connections (VanderWeele 2015). The analysis is easily extended to more complex networks of causal relations like the one depicted in Figure 2.2. The variables may be standardized enabling comparison of the relative impact of the alternative causes.

It is logically possible if the direct effect of X on Z is positive (negative) and the indirect effect, through Y, is negative (positive), then there may be no unconditional covariation between X and Z. it is often urged that covariation is not necessarily indicative of causality but also lack of covariation is not necessarily indicative of lack of causality. The error terms are each assumed to be pairwise independent of each other, normally distributed with zero means and constant variances. They can conveniently be regarded as depicting the impact of all the causal variables not explicitly incorporated into the model. They impact the appropriate dependent variable, controlling for the independent variables incorporated into the equation. Their pairwise independence serves a parallel purpose to randomisation in experimental studies in respect of the variables included in the model. Models of this sort involving many variables always allow both the identification and estimation of the causal links given the requisite comparative data (i.e. large N). However, the more causal variables that need to be incorporated into the model to render the causal error environment effectively random, then the larger the sample N needs to be.

The equations may always be represented diagrammatically as in Figure 2.2 which is a directed acyclic graph (DAG). In such causal diagrams the absence of an arrow (edge) represents the constraint that the origin node/variable has no direct causal impact upon the destination node/variable. The presence of a link relaxes this assumption, indicating the possibility of a direct causal link.

Causal diagrams of this nature and indeed some cyclic di-graphs allowing identifiable causal feedback involving more than three variables have become a convenient way of depicting causal models. The directed paths (i.e. following the direction of the causal arrows) running between any pair of variables represent alternative causal connections between them. The product of standardised coefficients down any directed path indicates the relative causal impact of the path upon the terminal variable.

The pair-wise uncorrelated errors rule out any unmeasured cofounders and any causal connection between component causal variables in the error terms. In any given regression equation selection bias is only ruled out to the degree that the included variables cover all the causal variables other than the random variables included in the error term. This absent we encounter unobserved variable biases which causally speaking would cover any impact of the excluded causal variables on the dependent variable along with any causal impact they might have on the included causal variables in the equation.

The appealing feature of linear regression models was that they allowed a simple correspondence between linear additive equations and causal diagrams (directed graphs). The ease with which such correspondence could be made lead unfortunately to applications where the various assumptions underlying the model were not always closely guarded.

The assumption that variables X and Y could interact in addition to, or alternatively to, each variables' additive causal impact upon Z also became prominent. So, thinking causally, in addition to the independent linear effects of both X and Y upon Z (where both variables will have a causal impact even in the absence of the other variable) these two variables may also combine such that both must be present to have an additional (additive) causal impact upon Z. It is perhaps worth noticing that even though X and Y are alternative causes of Z when one poses the question what caused Z, the un-cautious response might be X and Y, rather than X or Y, thus conflating interactive and additive effects.

No standard way of graphically depicting interactions has emerged in the literature, so the estimation equations become a better guide to causal inference than path diagrams (networks). A depiction we favour is to relabel interacting variable, X and Y, as a new additional variable, D. D then becomes a deterministic function of the two variables (e.g. $D = XY$) which does not introduce any additional variation into the model (Bollen 1995). However, the separation of additive and multiplicative interactive effects arises in virtue of the initial model being parametrically constructed.

More recently non-parametric models have become popular, where no explicit commitment to functional form is required. We are largely indebted to Pearl (2000, 2009) for enabling the construction of a correspondence between non-parametric models and causal directed a-cyclic graphs (DAGs). Pearl urges that causal DAGs enable us to draw causal conclusions without resource to the underlying non-parametric equations. As with the parametric models, for causal links to be identifiable then one must guarantee that the error (other causes) terms are independent of each other, independent of all the other causal variables in the model and have no common causes. Pearl refers to this as a Markovian condition. The movement from linear additive to non-parametric specification can, however, alter the interpretation of the causal arrows in the corresponding DAGs. Non-parametric arrows depict the total effect of the causal variable upon the effect variable. That is to say, unlike with the linear additive model, they fail to discriminate between additive ("or") and interactive ("and") effects, running the two together also with any possible non-linear effects. The various types of effect can be separated by conditioning on various variables but this, with increasing number of variables in the model, requires increasing number of comparisons and observations (Morgan and Winship, 2015). DAGs based upon non parametric equations may be regarded as embodying the state of the art for large N causal analysis. However, attempting to extract a set of causally connected variables, satisfying a Markovian error environment, often necessitates multiplying the number of variables explicitly involved and, thus, the number of comparative cases/ observations. Although non-parametric structural modelling and DAGs are most obviously tied to a large *N* causal framework they have been used by Pearl (2000) to analyse singular or, to use Pearl's term, actual causation (see Chapter 3).

The links in any DAG are each given an explicit counterfactual interpretation by the possible excision of the causal links incident into a node/variable(s) and then fixing the value of that variable(s) (i.e. the do-calculus, Pearl 2000). Pearl accordingly denotes causality, in terms of an intervention in a DAG, as $P(Y|\text{ do } X)$ rather than $P(Y|X)$. This allows, by comparing the causal impact of two fixed values of X and assuming that this fixation has no impact upon the rest of the causal links in the model (modularity), an estimation of the average treatment effects (ATE) for chosen causal variable in the underlying causal model (DAG).

The deletion of all the so-called backdoor paths between a chosen pair of variables in a DAG by deleting the causal arrows incident into the causal variable, without deleting any front door paths will surrender the causal impact down all the front door paths connecting the two variables. The direct impact of the causal variable upon the effect variable can then be delivered by deleting any indirect front door paths (i.e., those paths incident out of the causal variable) connecting them both as long as they do not contain collider variables (i.e. variables with two or more causal arrows incident into the variable/node) or causal consequences of a collider. Controlling on a collider variable, or a causal descendent of two variables, sets up a non-causal covariation of the two variables, incident into the collider. The great virtue of the front and backdoor path procedure is that any causal link in a complex causal DAG can be subjected to scrutiny.

The problem with this cosy picture is of course well known – what should we do if it is reasonable to assume the existence of perhaps unknown and unmeasured confounders causing the co-variation between events X and Z? In any large N statistical study unmeasured confounders are ubiquitous (Pearl 2009). Does locating a causally connecting mechanism, apart from satisfying the social science epistemic condition that all co-variations between events should be interpreted in terms of their connecting mechanism, provide any help in combating unmeasured confounders?

2.8 The Role of Causal Mechanisms In Large Studies

An unmeasured confounder (U) will often prevent the identification of a causal connection between X and Z. The backdoor path (Pearl 2009, Morgan and Winship, 2015) from X to Z in the model, $X \leftarrow U \rightarrow Z$ creates spurious co-variation between X and Z which cannot be conditioned or controlled away in virtue of U not being measured or observed, in order to identify $X \rightarrow Z$ (Figure 2.3a). The causal co-variation of X and Z is compounded of any possible direct causal link between X and Z and the spurious confounding effect of U. Furthermore, X and Z may not covary if contributing causal paths are of different signs.

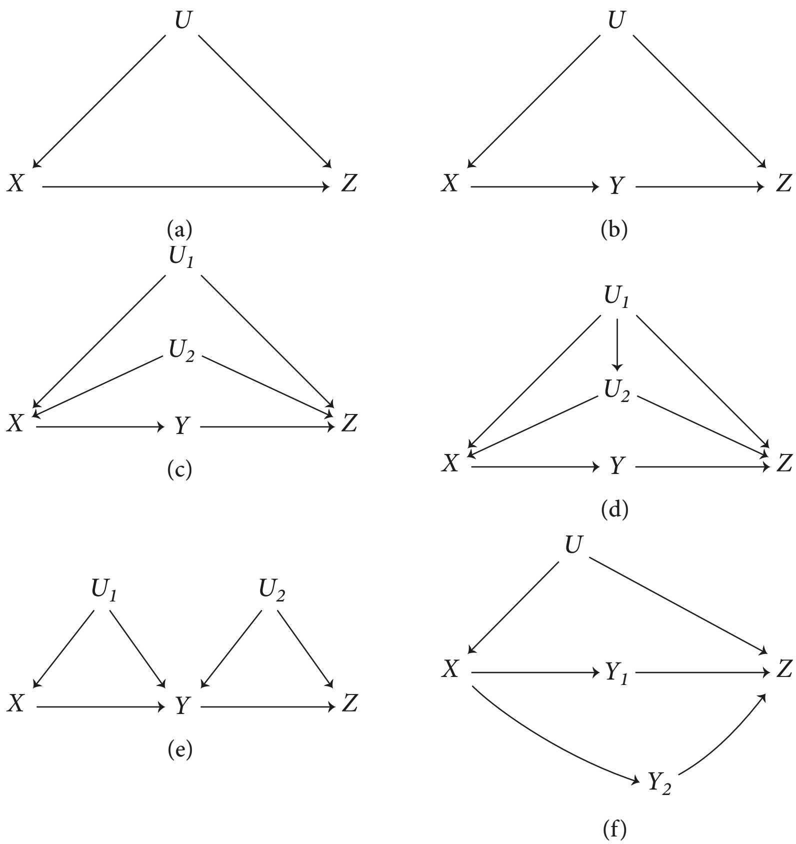

Figure 2.3 *The Role of Causal Mechanisms*

Throughout Figure 2.3 the error terms are suppressed for purposes of clarity and the attendant interpretative argument makes the standard assumption about these terms.

However, consider now the causal connection $X \rightarrow Y \rightarrow Z$ where an unmeasured confounder U still causes both X and Z as in Figure 2.3b (Morgan and Winship, 2000). We wish to identify, using Pearl's language of back-door paths, both of the causal connections $X \rightarrow Y$ and $Y \rightarrow Z$ without being able to condition on the unmeasured confounder between X and Z. Furthermore, Figure 2.3b assumes no alternative direct or undirected paths between X and Z. Consider, first, $X \rightarrow Y$; the backdoor path from Y to X namely, $X \leftarrow U \rightarrow Z \leftarrow Y$ is blocked by the collider Z, therefore the causal effect of X upon Y is identifiable (and estimable) in the face of the unmeasured confounder, as long as Z is not controlled, and this would remain true with any number of independent confounders between X and Z. Now consider $Y \rightarrow Z$, with the backdoor path $Y \leftarrow X \leftarrow U \rightarrow Z$, then by controlling X, once again in the face of the unmeasured confounder between X and Z, $Y \rightarrow Z$ is identified.

Thus, introducing the mechanism Y (between X and Z) not only satisfies the epistemic conditions of the mechanism approach to causality (Chapter 1) but also protects the initial co-variation against the unmeasured confounders. This attractive result holds for both independent (Figure 2.3c) and causally dependent multiple unmeasured confounders (Figure 2.3d). Inspection of the path between X and and Z surrenders this result.

If both causal links in $X \rightarrow Y \rightarrow Z$ are each separately beset by an unmeasured confounder, this happy result is lost (Figure 2.3e). Neither $X \rightarrow Y$ nor $Y \rightarrow Z$ is identifiable. Backdoor paths $X \leftarrow U_1 \rightarrow Y$ and $Y \leftarrow U_2 \rightarrow Z$ cannot be controlled. However, confining the direct impact of unmeasured covariates solely to X and Z but now with multiple paths between these variables, each containing an independent generative mechanism (Figure 2.3f), will still permit identification of all the network of constituent causal relations running between X and Z. This result holds if there are multiple unobserved mechanisms generating multiple paths between X and Z, proving particularly important when we come to explore the possible matching between large N causal networks with ethnographic networks in Chapter 3.

Consider now Y as a measured confounder rather than an intermediary variable, still in the presence of a further unmeasured confounder U (Figure 2.4a). We remarked in Chapter 1 that mechanisms as confounders are possible, particularly in the ethnographic context, where X is selected by an actor and Z is the teleological objective.

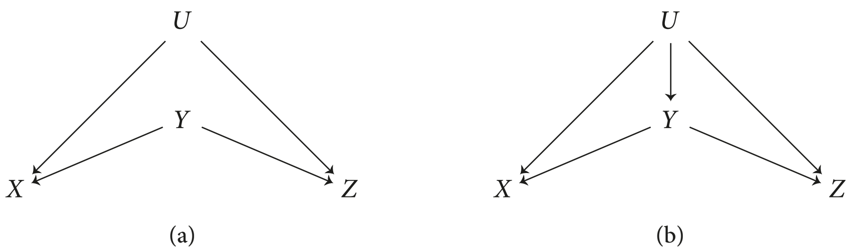

Figure 2.4 *Mechanism Y as a measured confounder*

In figure 2.4, $Y \rightarrow X$ is identified by virtue of the collider, Z, in $Y \rightarrow Z \leftarrow U \rightarrow X$. $Y \rightarrow Z$ is identified in virtue of the colliders, X, in $Y \rightarrow X \leftarrow U \rightarrow Z$. Once again these happy results do not hold up if U and Y are causally connected and both the links $Y \rightarrow X$ and $Y \rightarrow Z$ are confounded (Figure 2.4b).

The general conclusion we may draw is that the introduction of either a mediating or confounding mechanisms (variables) when exploring a causal relation $X \rightarrow Y$ can in many situations protect large N causal inference from unobserved confounders. However, in the social sciences both mediating and confounding "variables" are usually human actions or sequences of actions (Narratives) and we now turn to consider the implications of this observation in the context of singular counterfactual causality.

3. Ethnographic Causality: and Bayesian Narratives

This chapter explores the role which subjective statements about causality and their associated subjective counterfactuals and counter-potentials, elicited by identified ethnographers from actors and informants, may be allowed to play in the social sciences, where ethnographic techniques and the "social construction" of causality are appropriately invoked. The background to this exploration is the theory of Bayesian Narratives (Abell and Engel, 2019) where subjective statements may be used as evidential items in Bayesian Causal inference. Such inferences are required, as we have noted in previous chapters, when standard statistical (large *N*) approaches to causality prove to be inapplicable because of the limited number of comparative observations available. In such cases, a singular ethnographic concept must inevitably be deployed if causality is to be preserved as an intellectual objective. Bayesian causal inference has also recently been promoted in political science though not specifically in the context of narratives and not closely tied to subjective statements (Bennet and Checkel 2015, Fairfield and Charman, 2017). If Bayesian Narratives, which plot causal networks generated by human actions and forbearances, are given legitimacy then the issue arises as to how, if at all, they can symbiotically inform and be informed by Large *N* causal networks. Clearly large *N* causal networks, like those introduced in the previous chapter, can be given a Bayesian rather than frequentist interpretation (perhaps based upon judgement of "experts") but if the comparative data is forthcoming then few would advocate such a procedure. However, combining Bayesian and frequentist interpretations of causal links in complex causal networks may provide a way of reaping the complementary benefits of small and large *N* perspectives in causal analysis.

The concept of causality is itself, somewhat controversial amongst ethnographers, who may disavow the concept altogether, remaining content with "an understanding of the meaning of human actions" which is largely conceived as a descriptive exercise and rejecting "why questions" all together (Small, 2013). Nevertheless, Abend et al. (2013) find that many ethnographic studies do entertain some conception of causality, though the precise method of making a causal inference from ethnographic data, including subjective statements, remains rather difficult to fathom. In addition, the extensive literature on qualitative, small *N* case-based research has engaged with concepts of causality but almost invariably in a comparative perspective where $N > 1$ and where the language of variables (if only nominal dichotomies) is resorted to (Mahoney, 2000, 2012; Mahoney et al 2013; Ragin, 1987).

In this chapter we concentrate upon situations which ethnographers might wish to describe as unique and where the logic of cross sectional or limited longitudinal comparison across cases is initially absent. If a concept of causality can be found which is reasonably faithful to the precepts of ethnographic causality then this will require us to discard the standard explanatory procedure whereby generalised comparison across observations (or units of analysis) is a necessary prerequisite for any causal explanation. Indeed, in so far as comparison may be involved, the approach developed here places any comparison as posterior to the prior establishment of a causal explanation (Abell, 2009a). This may be conveniently labelled as ethnographic causal induction. Thus, the appropriate query becomes: is a given established singular causal link (i.e. explanation) open to some level of generalisation beyond the specific case? Ethnographic induction consequently is not derived from prior understanding of a general causal linkage. We thus draw a line between ethnographic causality and what is sometimes labelled as actual causality where the objective is to attribute causal connections in situations where it is difficult adjudicate between alternative causes in situations of pre-emption and symmetric over-determination (Hall 2004, Pearl 2000). In Pearl's (2000) formulation the actual cause is sought in the context of a non-parametric set of equations (laws) as introduced in Chapter 2.

Initially, basic ethnographic causal mechanisms will be examined which take the earlier introduced general form $\{C\}$ and $\{X\} \rightarrow \alpha$ (act$\{Y\}) \rightarrow_T \{Z\}$ where $\{C\}, \{X\}, \{Y\}$ and $\{Z\}$ are sets, usually comprising of conjunctions of propositions describing events and/or additional actions, commissioned by α or other actors; α stands for a specified actor (individual/collective) and (act $\{Y\}$) describes an action or forbearance designed by α to realise $\{Z\}$. The arrows continue to stand for causal and teleological causal relations (Chapter 1). The term (act $\{Y\}$) is used rather than do$\{Y\}$ to avoid any confusion with the do calculus (Pearl, 2009). In many instances the absence of $\{Z\}$ prompts an action to realize $\{Z\}$ (Chapter 1). Some may feel the term ethnographic causality should be used more broadly than merely associating it with subjectively elicited statements but we limit our perspective in this respect.

The basic causal structure, thus, closely parallels the statistical, Large N, mechanisms introduced in Chapter 2, though the symbols, C, X, Y and Z are not studied as variables but as sets of conjunctive propositions. They may, however, be envisaged as conjunctions of binary variables taking the values of 0 an 1. Causal links of this basic sort may subsequently be strung together into Narratives depicted as directed acyclic and-graphs, DAGs (see below).

Chapter 1 proposed that basic action and forbearance driven causal mechanisms link together the causes of actions/forbearances and their teleological consequences into a single analytical unit, promoting a bridging counterfactual of the following form; "if $\{C\}$ and $\{X\}$ had not been the case then α would not have acted $\{Y\}$ to realise $\{Z\}$."

Such mechanisms, thus, deliver concurrently both causes of effects and effects of causes. The inferential issue we face is whether or not elicited statements, claiming: (1) that an action/forbearance $act\{Y\}$ was caused by $\{C\}$ and $\{X\}$ which results in $\{Z\}$ and (2) that if $\{C\}$ and $\{X\}$ had not been the case then forbearance $\{Y\}$ would have not realised $\{Z\}$, provides sufficient evidence that we may conclude that there is a high enough odds of a causal connection between the conjunction of $\{C\}$ and $\{X\}$ and the consequence $\{Z\}$.

Note that even if the causal statement is elicited in deterministic form any causal conclusions which may be drawn will nevertheless always reflect uncertainty and be probabilistic in nature. The question we may raise is, given the analysis of large *N* type causality in Chapter 2, how counterfactuals, covariates and confounders may be addressed within an ethnographic singular (token) causal framework (Chapter 2)?

3.1 Elicited Subjective Causal and Counterfactual Statements

Assume, initially, that a specific ethnographer, *E*, directly observes α acting (i.e. doing) something on specific occasion. Now, further, assume *E* elicits, after an appropriately intense and extensive interaction and negotiation with α, statements, by α about her/his action/forbearance, along the following lines:

> (1) "In situation/occasion {*C*}, I did {*Y*} because of {*X*} to realise {*Z*}". (i.e. a subjective, first person, singular, past tense, causal/teleological statement);
>
> (2) "I would not have done {*Y*} to realise {*Z*}, in/on situation/occasion {*C*} if {*X*} had not happened (or not been the case)". (i.e. a subjective, singular, first person, past tense, causal/teleological counterfactual statement).

In the simplest situation each set will comprise of a single descriptive proposition. However, they may in general contain multiple conjoined descriptive propositions. Statements like these can most probably be expressed using many alternative locutions, each of which, we may assume, will carry, in virtue of any negotiated agreement between *E* and α, the same meaning and attract the same credibility estimation on *E*'s behalf (see below). Such alternative locutions will, thus, be regarded as evidentially equivalent. This assumption could be relaxed when the ethnographer may explore the implications of differing locutions.

It should be acknowledged here that the conditions, {*C*}, and consequence, {*Z*}, may be rather poorly comprehended by the reporting actor (c.f. large *N* causality in respect of *C*) but nevertheless, if elicited by *E*, they will be the actual cognised factors which the actor selects and reveals under negotiated elicitation, pertaining to the occasion and action under scrutiny. In this respect they may be conceived as derivative of the practical syllogism outlined in Chapter 1. $\{C\}$, $\{X\}$ and $\{Z\}$ may or may not be observed by E. If they are observed, the credibility afforded to the statements by E will be strengthened. The descriptive contents of (act $\{Y\}$), on the other hand, are only revealed by the elicited statements. It is also important to acknowledge that the temporal ordering of $\{C\}$, $\{X\}$, $\{Y\}$ and $\{Z\}$, normally requisite of any causal inference, will be implicit in subjective statements.

Since we are dealing with singular causes then any causal conclusions bear no necessary connection to the same or similar actions on other occasions. We may make also, what seems to be a natural assumption, that even though alternative courses of action (what to do in $\{C\}$ and $\{X\}$ to realise $\{Z\}$)may pass through the mind of the actor it is logically not tenable to consider an actual alternative as causally operative on the occasion in question. The ethnographer may entertain an alternative causal procedure and be tempted to balance this against the negotiated elicited statements. However, as we shall argue below, a basic, though contestable and certainly difficult to realize ethnographic principle is that the ethnographer should initially bring nothing to the table beyond the evidence at her/his disposal; that is for the moment the subjective causal and counterfactual statements elicited from the actor. It may be useful to re-emphasise here that the structure depicting the causal connections between the sets $\{C\}$, $\{X\}$, $\{Y\}$ and $\{Z\}$ is always conjunctive (i.e. depicting "and" causes), not alternative (i.e. depicting "or" causes). Thus, in a network sense the in-degree and out-degree to an action refers to conjunctions of causes (c.f. additive specification in large N model in Chapter 2). We thus refer to directed acyclic graphs (DAGs)

Singular causal investigation may be tied to various choice theories but to ask what actually caused the actor to act in a certain way, on a specific occasion is more restrictive than asking why the particular action is chosen from a possible menu of actions. However, the ethnographer may still ask, does the actor fully understand her motives (Chapter 1 reference to psychological analysis) as elicited in subjective statements? Tversky and Kahneman (1980) have documented biases in peoples causal reasoning which should give us grounds for caution and invites a deeper analysis of the motivation behind actions. What is required here is not an allegiance to a particular theory of action (e.g. rational choice) but a general framework which embraces how subjective statements may be reasoned by the actor which then may or may not be matched to a particular theory. Elicited statements we may assume, in this respect, derive from the very general framework of the contingent practical syllogism (von-Wright, 2004) introduced in Chapter 1. The syllogism provides a framework for the interpretation of singular teleological causality. Thus, the implicit reasoning takes the form:

- In situation/occasion $\{C\}$ and $\{X\}$, α intended that $\{Z\}$ should be the case,
- In situation/occasion $\{C\}$ and $\{X\}$ α believed that acting $\{Y\}$ would realise $\{Z\}$,
- In situation/occasion $\{C\}$ and $\{X\}$ α acted $\{Y\}$,
- $\{Z\}$ occurred (successful action) $\{Z\}$ did not occur (unsuccessful action).

Since an action driven ethnographic causal mechanism runs across both causal links; one running from sets $\{X\}$ and $\{C\}$ to (act $\{Y\}$) and the other, teleological cause, running from (act $\{Y\}$) to $\{Z\}$, the two implied constituent counterfactuals, in addition to (2) above, may be elicited. That is, on the occasion in question the following may be elicited by E from α:

(3) "I would not have done $\{Y\}$ if either (or both) not $\{X\}$ or not $\{C\}$ had been the case".

(4) "if I had not done $\{Y\}$ I would not have realised $\{Z\}$".

In addition, the counterfactual belief and intentional statements may be elicited. Thus,

(5) "If I had not intended $\{Z\}$ I would not have done $\{Y\}$".

> (6) "If I had believed that doing {Y} would not realise {Z} I would not have done {Y}".

Thus, actions lend themselves to a variety of subjective counterfactual statements which enrich and support of causal conclusions and may be estimated by the ethnographer to carry varying credibility.

The counterfactuals, in virtue of their singular (occasion specific) focus have the effect of ruling out alternative explanatory causal paths running between {X} and {Z}. However, alternative informants may offer alternatives and indeed alternatives may be elicited by alternative ethnographers (see below, meta-ethnography). Thus, any direct parallel with the Large N intermediation model, where controlling for Y eradicates the particular causal path in $X \rightarrow Y \rightarrow Z$ fails, as this model still leaves room for alternative paths between X and Z.

The ethnographer may also elicit information, from the actor about any future anticipated course of action, by α, along the following lines:

> (7) "In situation/on occasion {C} if {X} happens I will do {Y} to realize {Z}", i.e. a subjective first person singular future tense causally/teleological related statement;
>
> (8) "If either {C} or {X} does not happen then I will not do {Y} to realise {Z} "i.e. a subjective first-person singular future tense causal/ teleological counterfactual.

These, once again, may be supported by future tense belief and intentional counterfactual statements derivative of the practical syllogism all of which may be indicative of potential generalisation across occasions. The credibility afforded to future tense statements by the ethnographer, may well be significantly lower than for past tense statements rendering predictions rather hazardous. However, predictive statements may be particularly suggestive in developing large N causal networks where comparative data is scarce.

In the context of collective actions first-person plural statements along the following lines may be elicited:

> (9) "In situation/occasion $\{C\}$ we did $\{Y\}$ because of $\{X\}$ to realise $\{Z\}$".

Once again, appropriate counterfactuals may be elicited. The statement may also be tensed. The contents of sets $\{C\}$, $\{X\}$ and $\{Z\}$ may vary across those involved in the collective action (Chapter 4).

Ethnographic principle seems to enjoin that, at least initially, any elicited statements should be couched in the informants' language. Or to put it another way *E* should not impose any conceptualisation upon the various descriptive sets. This constraint also entails that the ethnographer should not interpret current evidence in terms of pre-conceived concepts and the contents of sets $\{C\}$, $\{X\}$, $\{Y\}$ and $\{Z\}$ should be derived from the actor/informant. The ethnographer may nevertheless reserve the right to explicitly and transparently translate from the actors' own conceptualisation to an alternative one – especially if *E* is in search of theoretical generalisations based upon similarity or even identity of ethnographic causal connections (see below).

The statements of types (1) to (9) may also reflect the actor's/informant's uncertainty and consequently be implicitly expressed in a probabilistic terms. They may particularly be uncertain about what an action may realise. Indeed, one may interpret all actions as experimental attempts to realise objectives when statement (1) may now take the form:

> (10) "In situation/occasion $\{C\}$ I did $\{Y\}$ because of $\{X\}$ hoping (but being uncertain) that my act $\{Y\}$ will realise $\{Z\}$".

The corresponding counterfactual is not straightforward.

> (10a) "if $\{C\}$ and $\{X\}$ had not been the case I would not have done $\{Y\}$ to probably realise $\{Z\}$"

Even though the causal connection between $\{C\}$ and $\{X\}$ and action $\{Y\}$ may be conceived as deterministic, uncertainty only arising in respect of the teleological connection between action $\{Y\}$ and the realisation $\{Z\}$, the bridging (transitive) counterfactual will be probabilistic. Thus "the inferred causal connection between $\{X\}$ and $\{Z\}$ will be probabilistic. This, once again. supports our contention that ethnographic causal links $\{X\} \rightarrow$ action $\{Y\} \rightarrow \{Z\}$ should be treated as a unified analytical unit deriving from the bridging counterfactual.

Again, the statements may be future tensed. Past tense statements may be elicited after the observation that Z is or is not realised thus reducing the statement to the deterministic form (1). But future tense statements will retain their uncertainty. How this uncertainty will affect the credibility of statements in the ethnographer's estimation is a moot point (see below).

Whilst picking out a set $\{Z\}$ as the objective of their action, actors may reveal, under elicitation by E, that they occasioned/will occasion further "unintended" consequences (actions and events) of their actions. These may then become the prompt for yet additional actions leading to narrative paths.

Statements may also derive, not from the actor commissioning the action, but from alternative informants claiming information about the focal action. Such procedures may be resorted to where the action type is culturally common. The elicitation will generate third person subjective statements along the lines of:

(11) "In $\{X\}$ and $\{C\}$ he/she/they did $\{Y\}$ to realise $\{Z\}$".

Again, various attendant subjective counterfactuals may be elicited. In addition, plural statements reflecting collective actions (see Chapter 4) may be collected from either participants or observers of the collective action. It should perhaps be acknowledged that with multiple informants giving statements about a particular action the number of observations is enhanced (see below).

The key point is that subjective elicitation surrenders information about both the causes of and the counterfactual for the same action, elicited either from the actor commissioning the action or observers of the action. In this respect, if credibility can be assigned, there is an advantage attached to such information especially where comparative analysis is not feasible..

The important question is: under what assumptions may an analyst, who might or might not be the ethnographer, allow elicited subjective statements to stand as credible evidence for a justified inference that:

> (12) In $\{C\}$, $\{X\}$ caused the actor α to (act $\{Y\}$) which teleologically caused $\{Z\}$.

Or prediction derivative of future tense subjective statements that:

> (13) $\{C\}$ and $\{X\}$ will cause the actor α to (act $\{Y\}$) which will teleologically cause $\{Z\}$.

It is important to note that, from an ethnographic standpoint, the subjective evidence, namely the causal and counterfactual statements themselves, must explicitly be associated with the elicitation by a specified ethnographer. Thus, all statements should be indexed by the informant ethnographic pair. Ethnographic principles require an acknowledgement that the statements by an informant are generated by virtue of the social interaction of the informant and a specific ethnographer wherein the credibility of the informant's statements come to be assessed by the ethnographer. Since the ethnographer becomes the measuring instrument it is important that any variation of the instrument is acknowledged. Any biases of an ethnographer may thus become evident.

3.2 Subjective Counter-Potentials

Some ethnographers may cavil at the inferences to causality between sets $\{C\}$ and $\{X\}$ and act $\{Y\}$, suggesting that voluntary actions and causality are incompatible (Campbell, 2020). However, we may assume that the above subjective statements can, in principle, be supplemented by elicited subjective counter-potential statements which run somewhat as follows:

(14) "I (she/he, we, they) could have forborne to (act $\{Y\}$) to realise $\{Z\}$ even when $\{X\}$ and $\{C\}$ are the case",

(15) "I (she/he, we, they) could have (acted $\{Y\}$ to realise $\{Z\}$ even if $\{C\}$ and $\{X\}$ are not the case".

Similarly, the future tense subjective statements can also be accompanied by elicited counter-potentials which run across both causal links but imply the two-constituent counter-potentials. Switching attention to probabilistic versions of subjective causal statements does not materially alter the role of counter-potentials.

We may assume that credible counter-potentials preserve the voluntary nature of human action/forbearance whilst maintaining the possibility of an inference to causality. That is to say, though informants can speak of why actors did/will act $\{Y\}$ (forbear to act $\{Y\}$), they fully recognise that the actor could always have done (may do) otherwise.

3.3 Singular Causality

The attraction of the various subjective statements outlined above is that they can all potentially be elicited by an ethnographer relating to an action (forbearance), connecting events or further actions, on a specific occasion. Thus, if they are mutually understood by an ethnographer and an informant and deemed as credible by the former, they open a route to singular causal inference without the need to generalise across comparative occasions. That is to say, causality can then, in principle, be justified in the absence of comparators and statistical co-variation. This allows that single case studies, if they can be constructed from basic ethnographic causal connections, may surrender causal information about sequences of actions and events, namely a narrative. It is probably important to emphasise that the term singular causality derives from the concentration upon a single occasion and a specific action/forbearance but which, nevertheless may have multiple conjunctive causes, set $\{X\}$ and consequences, set $\{Z\}$. The singularity arises from the connecting mechanism which shows how the causal connection is, on the occasion in question, generated. Everything depends, however, upon the credibility afforded to the subjective statements. How should they be elicited and then treated as credible evidence to warrant a justified causal inference? Further in the face of possible differing statements elicited from diverse informants and ethnographers, how should causality be assigned? Under what conditions may we assume the informant understands what the causes and objectives of his own and others actions are and is able to impart this understanding reliably to the ethnographer.

Consider, first a single commissioning actor. Certainly, if the sets $\{C\}$ and $\{X\}$ (act $\{Y\}$) and $\{Z\}$ are selected by the commissioning actor and expressed in her/his vocabulary then understanding is more likely to be the case and ethnographic principle, as we noted above, enjoins precisely this as the starting point for any research. Scepticism always remains, however, as to whether social scientists can assume a causal understanding, amongst actors, of their own and others actions. This scepticism may be particularly acute in respect of the counterfactuals and counter-potentials. Do people know what they would have done in the absence of $\{C\}$ and $\{X\}$ and are they capable of conveying this information to the ethnographer? These problems are clearly magnified when the informant is not the commissioning actor but only an "observing informant" of the action. It is part of the "art" of the ethnographer, as a "measuring instrument", to assign appropriate credibility to elicited statements. The assignment may also be fruitfully, complemented by field notes explaining the level of credibility afforded. The literature on causal reasoning suggests that peoples resort to causal reasoning is widespread (Waldmann, 2017). They tend to be less secure about diagnostic inference from observed effects to causes and more secure when inferring from observed causes to effects. Our model of ethnographic causality covers both aspects. The rather diffuse conclusion drawn in the literature appears to be that subjective causal reasoning about observed relationships between events tend to be prompted by more factors than the estimation of conditional probability amongst which, not surprisingly, is temporal ordering. The time lapse between cause and effect also tends to render inferences more hazardous and the complexity of the connected events carries a similar hazard. Most of this literature centres attention upon the cognition of the causal connection between events external to the individual, whereas ethnographic causality concern how people conceive their own and others causal agency.[1]

If we switch attention to prediction, rather than retro-diction, then things are not quite as problematic because the ethnographer can treat subjective statements as predictive and test this assumption if and when appropriate circumstances arise. Nevertheless, the conditions under which subjective causal, counterfactual and counter-potential statements can be relied upon as sources of credible evidence are far from transparent. Furthermore, when multiple ethnographers are introduced alongside multiple informants then the problems of comparing the likely varying elicited statements, with a view to a compendious causal inference, clearly exacerbates the inferential problems.

3.4 Generalising Singular Ethnographic Causal Explanations

Even if the elicitation of the various causal statements, as outlined above, is only appropriate when statistical comparison and generalisation are not feasible because of the scarcity of inter-unit comparative cases/observations, nevertheless generalisation may still be sought post a singular (occasion specific) explanation (i.e. ethnographic induction). Any such generalisation may be severely limited to a few occasions but may be sought when the costs of mounting a large N study prove to be prohibitive. Ethnographic induction will usually seek to answer the question as to how frequently the action $\{Y\}$ connects sets $\{X\}$ and $\{Z\}$. In other words a putative generalisation will centre attention upon the connecting causal mechanism. A few comparators may be available: (a) across actors on different occasions pursuing the same action, (b) repeated action by a given actor or (c) the occasion specific action involving many actors (i.e. collective action).

1 Also known as the self-serving bias (Campbell and Sedikides 1999), observers are found to be biased in the way they ascribe reasons to observed action, tending to attribute an observed action to an agent's character while attributing their own action to external situational factors. This and other biases may be relevant in judging the causes of action.

The Inductive question becomes, how generalisable does a credible singular causal explanation of a specific action and its consequences prove to be? Firstly, subjectively generalised causal claims may be directly elicited taking the form:

> (16) "I/she/we/they will always do{Y} in {X} and {C} to realise {Z}".

In addition the corresponding generalised counterfactuals and counter potentials may also be elicited including the counterfactual intentional and belief statements. These various statements, if they are deemed at all credible, may serve as direct evidence for a predictive/retrodictive general causal explanation. Their accuracy may then be tested if the appropriate circumstances arise. However, in the absence of any elicitation of such generalisation we may still wish to compare a few ethnographically derived basic causal sequences. Since, as we noted earlier, subjective evidence should also identify the ethnographer this also invites possible generalisation across ethnographers (i.e. meta ethnography) – but we put this complication to one side until later in the chapter.

Actors and informants may entertain and impart statements indicating possible alternative causal sequences. Thus, the following type of general statement may be elicited:

> (17) "If either {X} or { X 1} were to be (had been) the case then I/she/we/they would do {Y} to realise {Z}."

The counterfactual then takes on a conjunctive form:

> (18) "I/we/she/they would not have done/do (forborne to do) {Y} if both {X} and { X 1} had not been the case".

We noticed earlier that both "qualitative" social science and comparative case studies tend to engage with similarity rather than identity and equivalence running across cases. The question then becomes – are several ethnographic causal accounts sufficiently similar to warrant any ethnographic induction of the implied causal process? This amounts to asking whether sets $\{X\}$, $\{Y\}$ and $\{Z\}$ are sufficiently similar across occasions to warrant doing so?

Since basic causal connections are studied by the ethnographer initially identifying actions, elicited as $\{Y\}$, and then eliciting the causes and consequences of the identified actions, the starting point of the analysis will usually be: are the action descriptions sufficiently similar to warrant the assumption that the same action is involved? If so this will then promote the further questions:

- Do the similar actions have sufficiently similar causes $\{X\}$ to warrant the causal connection $\{X\} \rightarrow \{Y\}$?

- Do the similar actions have sufficiently similar effects $\{Z\}$ to warrant the teleological causal connection $\{Y\} \rightarrow T\{Z\}$?

If both conditions are satisfied then the basic action showing how $\{X\}$ and $\{Z\}$ are causally connected is generalisable under triple similarity. We should like to emphasise that this procedure, in practice, seeks generalisable causal actions. If an action $\{Y\}$ always connects different causes and effects then the action though generalisable is not causally generalisable.

It may be worthwhile to consider a little more deeply the significance of basing investigation upon similarity rather than identity/equivalence. In the large N tradition the nominal level of measurement is the lowest practical level available and units of analysis can be assigned to exhaustive and exclusive equivalence classes of one sort or another. Any ambiguity in assignment is then assumed away as errors of measurement enabling analysis to continue with clear category boundaries. However, in the qualitative small N tradition similarity appears to be intrinsic and any ambiguity must be explicitly acknowledged and reported, enabling the consumer of the research to understand the accuracy with which causal mechanisms may be generalised. Unfortunately coding techniques recommended in the standard literature do not always make such provisions.

Elicited credible deterministic statements of the form "I/ we /she/they did $\{Y\}$ in $\{C\}$ and $\{X\}$ to realise $\{Z\}$" may be interpreted as indicating double, occasion specific, subjective causal sufficiency: $\{C\}$ and $\{X\}$ were, on the occasion in question, subjectively sufficient for the actor to act $\{Y\}$ which in turn was, also on the occasion in question, subjectively sufficient for $\{Z\}$ to be realized. Similarly, elicited credible counterfactual statements "I/we /she/ they would have forborne to do $\{Y\}$ to realise $\{Z\}$ if $\{C\}$ and $\{X\}$ were not the case" surrenders subjectively necessary causal conditions for both action $\{Y\}$ and $\{Z\}$. These sorts of statements may, as we noted above, both be tensed. Despite the transitive subjective necessary and sufficient conditions of set $\{X\}$ for set $\{Z\}$ it is important to acknowledge that this does not imply any direct causality – there is no direct causal connection between sets $\{X\}$ and $\{Z\}$. Set $\{Z\}$ is counterfactually dependent upon set $\{X\}$ because of the action $\{Y\}$. The action $\{Y\}$ is necessary and sufficient for set $\{X\}$ to cause $\{Z\}$ on the occasion in question.

A singular, that is to say occasion specific, causal account will rule out alternative causes, though conditions $\{C\}$ and $\{X\}$ may on other occasions suggest alternative action mechanisms to realize $\{Z\}$. Thus, the necessity implications of singular counterfactuals will not rule out alternative causal stories on other occasions. It is also possible that $\{Z\}$ can be realised by alternative actions caused in different conditions. indeed, alternatives, as we have noted, may be elicited; thus an informant may suggest that an alternative course of action to realize $\{Z\}$ could have been pursued even if $\{X\}$ were to be absent. The possibility of simultaneous overdetermination has plagued the analysis of singular causality (Paul and Hall 2013) but is significantly resolved by ethnographic causality, as we may assume that, on a particular occasion, though the actor may acknowledge the possibility of alternative actions, they will be clear that a particular alternative was chosen. This then licences the causal account as surrendering both necessary and sufficient, occasion specific causes connecting sets $\{X\}$ and $\{Z\}$.

The subjective statements of causality will, as analysed below, be afforded a probability of their credibility by the ethnographer (i.e. the probability that they are true) but, in addition as we have noted, the statements themselves may only be elicited in probabilistic terms. In this respect the analysis of subjective statements requires a way of conceiving the probability of the necessity and the sufficiency of the inferred causal connection between sets {X} and {Z}. The above statement (10a) "I did {Y} because of {X} to probably realise {Z}" and "I would not have done {Y} to probably realise {Z} had not been the case" may attract interpretations that {X} was necessary and sufficient for {Y} and {Y} was probably sufficient for {Z}; thus {X} was probably sufficient for {Z} (Pearl, 2000, provides a formulation of such statements).

We noted in Chapter 1 that the full specification of the set C has proved troublesome for philosophers when working in the context o f a generalising large N framework. Set C normally comprises a number of conjunctive conditions all of which must obtain for the causal inference to stand and the absence of any one of which will suggest a counterfactual. Separating X from C also proves difficult. Ethnographic causality, however, provides a significant resolution of this problem. Sets {C}, {X}, {Y} and {Z} are selected by the actor/informant and if elicited as credible by E they demarcate the limits of the various sets. Set {Z} will not cover any unintended consequences of the action but such may be acknowledged by an informant. Unintended outcomes call for some analytical ingenuity. They may be further actions and/or events and we shall deal with them below.

Caution is necessary in the interpretation of the causal implications of any counterfactuals. Evidence for a counterfactual does not rule out alternative causes of an effect on other occasions. Indeed, in the large N framework it is routine for there to be sets of alternative conjunctive conditions each separately sufficient but not necessary for a dependent variable (Mackie, 1965). But in a single case, of course, only one of these alternative sets is logically possible and is formulated by the informant. The elicited counterfactual is, thus, confined to the particular cluster of conditions (C and X) operative in this case and on this occasion. As we have noted any modest generalisation is logically posterior to a singular causal explanation. Collective actions may complicate this picture as E may elicit different subjective causal sequences from alternative members of the collective (see Chapter 4). Having set the broad features of ethnographic causality let us now introduce an illustrative empirical example.

3.5 An Introduction to an Illustrative Empirical Example

We introduce here an illustrative empirical example which will be explored in more detail later in the Chapter. In a study of producer cooperatives in developing countries explanations were sought as to why many cooperatives failed whereas very few indeed prospered (Abell, 1988). Attempts to find a statistical model to account for this asymmetric distribution, which could be generalized across cases, proved elusive. To put it succinctly, each case appeared to be rather historically unique and a subsequent in-depth study of a single highly successful cooperative lead to the theory of Bayesian Narratives (below) and an attempt to take subjective causal statements and their counterfactuals as serious sorts of evidence. Here we concentrate upon a single action when the collective governing board appointed an external professional manager. Such an appointment is unusual as cooperatives usually appoint from amongst their membership.

A senior member of the governing body was asked the question, after a great deal of exploratory discussion (interaction), "why was an independent manager appointed"? The answer (whilst improving the expressed English) was as follows:

"Because sales were dropping, the quality of the products was not competitive, and the problems of discipline were uncontrolled a manager was appointed to improve the all-round performance whilst making the cooperative an attractive place to work."

It is important to recognise that this statement was mutually constructed in the interaction of the ethnographer/author and the informant and was endorsed by the informant as a perfectly acceptable causal explanation of the action taken.

The ethnographer was now faced with (1) assessing the credibility of this statement and, thus (2), inferring a causal link as follows: where inverted commas indicate subjective expressions extracted from the interaction.

Set $X = \{x_1, x_2, x_3\}$

x_1 = "sales dropping",

x_2 = "uncompetitive quality of products",

x_3 = "discipline problems".

Set $Y = \{y_1, y_2, y_3\}$

y_1 = "appoint an independent manager",

y_2 = "to realize improved performance".

y_3 = "realize an attractive place to work".

Set $Z = \{z_1, z_2\}$

z_1 = "improved performance,"

z_2 = "improving attraction of the place of work".

Thus, the possible causal inference takes the form:

$$\{x_1, x_2, x_3\} \rightarrow \text{Governing body (act } \{y_1, y_2, y_3\}) \rightarrow_T (z_1, z_2)$$

When faced with this inferred causality the informant (with prompting) stated;

"If falling sales, uncompetitive products and discipline problems had not been the case then we (the governing body) would not have appointed an independent manager";

Evidence of a subjective counterfactual. Data on counter potentials was unfortunately not gathered. Additional informant statements will be introduced later in the chapter.

3.6 Constructing a Case in Accordance with Ethnographic Causality

Much has been written about the construction of case studies but our claim is that in concentrating upon causal inference generated by actions and forbearances, such a construction takes a special form, giving priority to actions as causal mechanisms. Cases are often reported as a chronology of events and decisions/actions distributed in time (sequence analysis) between which causal connections may or may not be sought. Our reading of ethnographic causality initially centres attention upon actions/forbearances as mechanisms generating causal connection between sets $\{C\}$ and $\{X\}$ and then set $\{Z\}$ each of which are selected by the commissioning actor(s) or informants. The various sets may contain reference to events and or additional actions which deliver three of the four types of causality outlined in Chapter 1.

> (1) Causal connections between actions/forbearances and consequential events,
>
> (2) Causal connections between events and consequential actions/forbearances,
>
> (3) Causal connections between actions/forbearances and either prior actions or consequential actions/forbearances (i.e. social interactions).

Thus, in the basic causal/teleological model $\{C\}$ and $\{X\} \rightarrow (\mathrm{act}\{Y\}) \rightarrow_T \{Z\}$ the first causal link is either of type (2) or (3) and the teleological link may be of type (1) and (3). Since the causality generally involves sets of conjoined causes and effects the causal structure will be depicted as a directed acyclic and-graph (DAG). Fig 3.1, depicts the DAG for the above example where (act$\{Y\}$) is a node and the elements of set $\{X\}$ provide the nodes generating the indegree and set $\{Z\}$ the outdegree to (act$\{Y\}$). Set C is not depicted and is assumed to operate across the causal DAG.

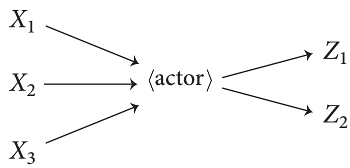

Figure 3.1 *The basic causal/teleological model {C} and {X}* $\rightarrow$ *(“act” {Y})* $\rightarrow_T$ *{Z}*

A case is constructed in accordance with the precepts of ethnographic causality, by initially documenting a chronology of actions/forbearances terminating in the event(s) or action(s) to be finally causally accounted for (Z). Any events involved are those selected by actors as causing and teleologically consequential upon their actions. When a number of action driven basic sequences of this sort are put together a Bayesian Narrative is constructed (see below).

Let us assume α intends to realise set Z, then α can encounter four types of situation:

(1) $\neg Z$ will become $\neg Z$ (i.e. $\neg Z$ persists) and α acts Y to realise Z (prevent $\neg Z$ persisting).

(2) $\neg Z$ will become Z and α forbears to act Y to realise Z (allow Z).

(3) Z will become $\neg Z$ and α acts Y to realise Z (prevent $\neg Z$)

(4) Z will become Z (i.e. Z persists) and α forbears to act Y to realise Z (allow Z)

Observe that (2) is the corresponding counterfactual to (1) and vice versa and (4) to (3) are similarly related. A parallel set of situations will arise if α intends not to realise Z rather than Z.

3.7 Bayesian Inference to Credible Causal Beliefs

Assume an occasion specific, ethnographic causal mechanism is under investigation by an identified ethnographer, E, who observes an initially unnamed action/activity by α. E's objective is to elicit a natural language description of the action and of the causal and effect sets in response to a query as to why and with what objectives the action was prosecuted. Mention of set $\{C\}$ will be dropped for the sake of clarity throughout this chapter and the commissioning actor is assumed to be an individual person (Chapter 4 introduces collective actions).

Let us initially assume that the elicited referent descriptions in sets $\{X\}$ and $\{Z\}$ are also observed by E in "the real world". E can, thus, ascertain that the contents of both sets actually occurred. Recall that they may contain reference to events or further actions by α or other actors. Assume also that $\{X\}$ precedes $\{Z\}$ and act $\{Y\}$ may or may not precede $\{X\}$ but will precede $\{Z\}$ depending upon which of the two basic models ($\{X\} \rightarrow \{Y\} \rightarrow \{Z\}$ and $\{X\} \leftarrow \{Y\} \rightarrow \{Z\}$) we adopt. In the following analysis we shall assume the former model.

The ethnographer's, E, eventual objective is to ascertain whether or not the observed action – elicited as $\{Y\}$ – probably did or did not generate a causal connection between sets $\{X\}$ and $\{Z\}$? E may also entertain the possibility that alternative action(s) connect the sets $\{X\}$ and $\{Z\}$. However, put this to one side for the moment. Any conclusions E may Initially draw will depend upon the credibility that E affords to the elicited statements (i.e. the probability that they convey reliable information about the causal connection) which may then, along with any additional relevant evidence, allow an inference to be made, by E, to the probability of the existence of the causal connection. As we noted in Chapter 1 the assignment of credibility by E (i.e. by the measuring instrument) will be made in the context of the detailed interaction between E and the actor/informant.

The objective of the ethnographer may be envisaged, in the context of an observed action, as involving a sequence of investigative steps as follows:

(a) Observe (or at least gather information about) the focal action to be explored in terms of its causes and consequences and in so doing identify the commissioning actor.

(b) Elicit the description of the focal action, set $\{Y\}$ from the commissioning actor/informant.

(c) Elicit the actor's/informant's subjective causes, set $\{X\}$, and consequences, set $\{Z\}$, of the focal action. This to include subjective counterfactuals and possibly the intentions and beliefs derivative of the practical syllogism (Chapter 2).

(d) Confirm that sets $\{X\}$ and $\{Z\}$ occurred.

(e) Estimate the odds that various statements carry credibility; that is the probability of their truth in delivering causal information.

(f) Involving the assigned credibility, estimate the odds that the action, so described, generated the causal connection between sets $\{X\}$ and $\{Z\}$.

Let s stand for the subjective causal and counterfactual statements elicited by E concerning an actor's action. For the sake of clarity in the formalism to follow assume that s is allowed to cover the conjunction of all the appropriate elicited statements; that is to say, the various possible causal and counterfactual statements. The following analysis could however, be applied to each separately.

To commence the analysis, assume these statements are expressed in deterministic form and, thus, do not contain any probabilistic (uncertain) reasoning on the informants behalf. Initially, let $B\ e$ denote E's belief in the credibility of the subjective elicited statements (i.e. E's estimation that statements express the truth about causal links), while $\neg Be$ denotes E's belief in the in the lack of credibility of the subjective elicited statements (i.e. E's estimation that statements fail to express the truth about causal connections).

Assume that the $P(B\ e) = 1 - P(\neg B\ e)$. These beliefs will be treated as binary but it would be possible to introduce ordinal measures of belief.

The beliefs entertained by E reflect E's estimation that the actors/informants can, under elicitation, both understand and impart reliable information about causal reasoning. Ascriptions of credibility may, thus, derive from both the ethnographers familiarity with the target actor/informant (personal credibility) and the culture (general cultural credibility) within which the actor operates.

Then by Bayes' rule, E's estimate of the posterior odds of the credibility of the elicited statements s is given by:

$$\frac{P(B_e|s)}{P(\neg B_e|s)} = \frac{P(B_e)P(s|B_e)}{P(\neg B_e)P(s|\neg B_e)}$$

$$\text{Odds}(B_e : \neg B_e|s)) = \text{Odds}(B_e : \neg B_e) \cdot L_s$$

$$\log(\text{Odds}(B_e : \neg B_e|s)) = \log(Odds(B_e : \neg B_e)) + \log\ (L_s)$$

Where, L_S is the likelihood ratio (to be estimated by E) of the elicitation of statement s given E's belief about the credibility or lack of credibility of such statements. Thus,

$$L_s = P(s|B_e)\ /\ P(s|\neg B_e)$$

From E's prior odds and estimation of the likelihood ratio L_S his/her posterior odds of the credibility afforded to the elicited statement can be derived on a log interval scale. This allows multiple evidential statements to be added and subtracted (see below). Note, that whatever E's prior credibility assignment happens to be, if L_S is greater than one then the posterior credibility is strengthened and vice versa.

It is, however, imperative to realise what the estimation by E of L_S in practice involves. It assumes that E estimates the odds of securing the statement s when the odds of credibility to lack of credibility is set at unity.

The subjective estimate of L_s, which comes in addition to the subjective nature of the statement *s*, may concern some, though the first estimate derives from *E* and the second from the informant. But subjectivity is an inevitable consequence of ethnographic technique, however investigative procedures are conceived, which always empowers *E* as the "measuring instrument" in assessing the credibility of evidence. All we can demand is a technique which makes procedures as transparent as possible, for the consumer of the research. We are ultimately concerned with inferences from subjective statements to causal hypotheses but it seems a reasonable requirement that the intervening subjective inference of the credibility ascribed to informants and their statements be made explicit. The consumer of the research is put in the position of knowing how any results are dependent upon the degree of credibility ascribed by the ethnographer to statement *s*. This becomes particularly pertinent when statements, elicited from multiple informants, have to be balanced for their credibility (see below).

Much obviously depends upon how any prior odds of credibility are ascribed by the ethnographer. If the prior odds is set at unity then the posterior odds is numerically equal to the likelihood ratio. Ethnographic researchers are often enjoined to engage in research without, at least initially, bringing any preconceived ideas to the research site. This is sometimes enjoined in terms of getting rid of any pre-conceived theories (Strauss and Corbin, 1997). Whether this is feasible or indeed desirable, is, of course, very much a moot point but it does apparently invite the suppression of any prior odds, other than they be set at unity, when the initial elicitation takes place. This amounts to being neutral between the credibility and lack of credibility of the elicited statements. In this situation E has merely to estimate the likelihood ratio. Setting the prior odds at unity derives from the objective of treating the first encounter with statements about an occasion specific action as unique. It in effect enjoins an ethnographer not to generalise when initially analysing the causal implications of statements about the focal action.

It may, however, in many situations prove desirable that any prior odds should be made explicit and then the current evidence in improving the odds can be computed. This is particularly so where the actions are institutionalised in terms of role expectations (Chapter 5). Some Bayesians may not be impressed with these ethnographic precepts but the inferential techniques outlined are not immutably tied to such precepts. Indeed, ethnographers when coming to an apparently new action may be inclined to ask whether it is sufficiently similar to previously experienced actions to licence an appropriate assignment of prior odds, other than unity, to the credibility of any elicited causal statements.

Estimation of likelihood ratios might still appear rather demanding, of the ethnographer. If, however, the estimate is reported alongside the explicit provenance of the statement of *s*, nothing is lost and much may be gained on behalf of the consumer of the research, who is enabled to understand how any inferences are grounded in the relative credibility assigned. This may be important when ethnographers, other than *E*, also elicit statements about the action when the question arises as to how their likely differing assignment of credibility should be combined. If in addition, there are also multiple informants then one may further ask how each of their statements should be combined. It is clear that a systematic procedure is required that enables combinations across ethnographer informant pairs (see below).

Let us stay with a single ethnographer, *E*, but now introduce *M* subjective statements, $s_1, s_2 \ldots s_m$ (again each to cover counterfactuals etc.) deriving from *M* informants observing or witnessing (having information about) the focal action. Initially assume each informant provides the appropriate causal subjective statements entirely independently of each other. So, *E* can assume that the statements are independently elicited, conditional on B_e. Then:

$$\frac{P(B_e|s_1,\ldots,s_m)P(s_1)\cdot\ldots P(s_m)}{P(\neg B_e|s_1,\ldots,s_m)P(s_1)\cdot\ldots P(s_m)} = \frac{P(B_e)\cdot P(s_1|B_e)\cdot\ldots P(s_m|\mathrm{B_e})}{P(\neg B_e)\cdot P(s_1|\neg B_e)\cdot\ldots P(s_m|\neg B_e)}$$

The log odds of *E*'s beliefs about the credibility of the *M* informants' statements will take the form:

$$\log\left(odds\left(B_e{:}\neg B_e\middle|s_1,\ldots,s_m\right)\right) = \log\left(odds(B_e{:}\neg B_e)\right) + \log\left(LS\right),$$

where

$$\log(LS) = \textstyle\sum_{i=1..m}\log\left(L_{s_i}\right).$$

Once again, adopting the advocated ethnographic precept, we may cautiously assume that the prior odds may be set at unity. If this is deemed feasible the posterior odds are then equal to the likelihood ratio (*LS*) and *E* can estimate the posterior odds directly rather than inferring such from the prior odds and the likelihood ratio.

Ethnographers often assemble evidence in a sequential manner drawing a line at the point when new evidence (informants' statements) does not appreciably alter the conclusions to be drawn. It should be noted that a sequential elicitation will permit the ethnographer to recursively apply the Bayesian ideas. Thus, for the first elicitation, the prior odds may be set at unity, the posterior odds computed, from the estimated likelihood ratio, and in turn the posterior odds become the prior odds for the second elicitation and so on. This enables the estimation of cumulative credibility. Note that the order in which the statements are incorporated into the overall credibility should not influence the conclusions but may do so depending upon which statement is initially selected with a prior of unity. If so then it may be useful to compare the conclusions when adopting each alternative statement as the one attracting the ethnographic assumption of odds at unity. The procedure is entirely consistent with the ethnographic directive that no prior evidence should initially be brought to the investigation. The adjustment of prior odds away from unity is entirely confined to the accumulated subjective evidence pertaining to the investigation at hand.

Dropping the assumption that the evidential statements are independent conditional on B_e does not materially alter the situation except that (LS) must now acknowledge the pattern of dependence amongst the subjective statements (Abell, and Engel 2009b). The recursive use of previously computed posterior odds as a subsequent prior still holds. Such dependencies are, of course, to be expected when the evidential statements are obtained for a particular action from multiple informants all of whom may interact in their observation the focal action. Where the actor, is collective then dependent statements are particularly likely (Chapter 4).

Clearly, it may prove difficult for the ethnographer to estimate each of a string of m likelihood ratios be they independent or not. Thus, a direct estimate of (LS), rather than its component likelihood ratios, may perhaps be all that can be demanded. However, if inferences are also made recursively then the consistency of (LS) computed from a string of inferences can be compared with the direct computation. Lack of consistency may then prompt further investigation.

So far, we have assumed the elicited causal or counterfactual statements s_1, s_2, … s_m are deterministic in nature. However, does dropping this assumption alter the ascription of credibility by the ethnographer? Now the ethnographer elicits a past tense statement of the general form:

"In $\{C\}$ and $\{X\}$ I/ (s)he/ we /they acted $\{Y\}$ to probably realise $\{Z\}$"

In most situations we may perhaps assume that the probability is reasonably high for if it were not the actor would not have pursued this course of action. In the absence, in the literature, of a precise theory of how ascription of credibility is established it is difficult to answer this query. If E also observes Z then the credibility is enhanced but if we switch to future tense statements then this check is not available. The informant may also pick out outcomes not intended by the actor, but as long as the ethnographer can infer a causal connection between the intended and not intended outcomes then the probability should not impact the ascription of credibility when compared with deterministic statements. Action driven causal mechanisms and, thus, narratives are nearly always embedded in networks of physical causes (event causes event). Credibility ascribed by E will as we note in the opening chapter depend upon E's estimate of the trust worthiness and knowledge of the informant. If the informant's use of probability (uncertainty) of Z is a signal of honesty then this may enhance credibility.

But, once again, why should the ethnographer go through the exercise of ascribing odds at all? The answer is twofold First, as we have observed, the aggregation of evidential items is made explicit but, second, differing estimates can be aggregated across ethnographers. So, alternative ethnographers are each endowed with a disciplined framework within which to debate their differing credibility assessments of informants' statements.

3.8 From Credible Causal Beliefs to Justified Belief in Causal Connections

If we now allow a direct inference, by a given ethnographer, from the elicited evidence to the probability of the actual existence of the causally generated mechanism $\{X\} \rightarrow \alpha(act\{Y\}) \rightarrow_T \{Z\}$ and label this as hypothesis H and its absence as $\neg H$. Interest now centres upon:

$$\text{Odds}\left(H{:}\neg H \middle| s_1, s_2, \ldots, s_m\right) = \text{Odds}(H{:}\neg H) \cdot L$$

Where,

$$L = \frac{P\left(s_1, s_2 \ldots s_m \middle| H\right)}{P\left(s_1, s_2 \ldots s_m \middle| \neg H\right)}$$

Thus, as per the above, the ethnographer may estimate the likelihood ratio at the aggregate level across all evidential statements. But just as with inference to credibility the calculation may also be made recursively.

It is possible that the evidential statements may instance mutually exclusive alternative causal hypotheses. H_k and H_L, which promote alternative action(s) as generating the causal connection between sets $\{X\}$ and $\{Z\}$ (Fairfield and Charman, 2017). Then:

$$\text{Odds}\left(H_k{:}H_l\right) \middle| s_1, s_2, \ldots, s_m\right) = \text{Odds}\left(H_k{:}H_l\right) \cdot L_a$$

$$L_a = \frac{P\left(s_1, s_2, \ldots\ldots s_m \,|H_k\right)}{P\left(s_1, s_2, \ldots\ldots s_m \,|\neg H_k\right)}$$

When attempting to adjudicate between alternative generating mechanisms it may prove constructive also to appeal to intentional and cognitive counterfactuals as evidential items which will enrich the evidence for the causal connections (Chapter 2).

The above direct inferences from the subjective statements to causal hypotheses either connecting H and $\neg H$ or H_k and H_l both fail to explicitly incorporate the ethnographer's credible beliefs about the statements. We are interested in how beliefs in the credibility of the available subjective evidential statements do or do not licence causal conclusions.

It is convenient to revert to a single item of evidence, s, rather than m items and to drop the designation of the ethnographer E, thus reducing the complexity of the notation. We shall also concentrate upon the odds of H and $\neg H$.

We, thus, need to examine the likelihood ratio, L, with a single item of evidence s and with the objective in mind of showing how the inference to the hypothesis of the existence of a causal link from the statement involves the ascription of credibility, B, to s. The components of the likelihood ratio are :

$$P(s|H) = \frac{P(s,H)}{P(H)} \tag{1}$$

$$P(s|\neg H) = \frac{P(s,\neg H)}{P(\neg H)} \tag{2}$$

The numerators are given by:

$$P(s, H) = P(s, B, H) + P(s, \neg B, H), \tag{3}$$

$$P(s, \neg H) = P(s, B, \neg H) + P(s, \neg B, \neg H) \tag{4}$$

The components of these two expressions are given by:

$$P(s, B, H) = P(H)\, P(B|H)\, P(s|B, H), \tag{3a}$$

$$P(s, \neg B, H) = P(H)\, P(\neg B|H)\, P(s|\neg B, H), \tag{3b}$$

$$P(s, B, \neg H) = P(\neg H)\, P(B|\neg H)\, P(s|B, \neg H), \tag{4a}$$

$$P(s, \neg B, \neg H) = P(\neg H)\, P(\neg B|\neg H)\, P(s|\neg B, \neg H), \tag{4b}$$

Thus, substituting these in the previous equations and rearranging we arrive at:

$$P(s|H) = P(B|H)P(s|B,H) + P(\neg B|H)P(s|\neg B,H) \qquad (5)$$

$$P(s|\neg H) = P(B|\neg H)P(s|B,\neg H) + P(\neg B|\neg H)P(s|\neg B,\neg H). \qquad (6)$$

The odds ratio is thus given by the ratio of these two equations and shows how it depends upon the ascribed credibility of s.

However, the two equations assume there is an inference from s to H and to $\neg H$ independently of credible belief (B and $\neg B$). Ethnographers appear to discount (often implicitly) this possibility, urging that the evidential support for H is (or perhaps, should be) entirely in virtue of the credibility of beliefs about the evidential support. Thus, under this assumption the equations become:

$$P(s|H) = P(B|H)P(s|B) + P(\neg B|H)P(s|\neg B) \qquad (7)$$

$$P(s|\neg H) = P(B|\neg H)P(s|\neg B) + P(\neg B|\neg H)P(s|B). \qquad (8)$$

Surrendering:

$$L = \frac{P(s|H)}{P(s|\neg H)}.$$

Then,

$$\log\left(Odds(H{:}\neg H|s)\right) = \log(Odds(H{:}\neg H)) + \log(L).$$

It is immediately obvious that this sort of analysis can be extended to multiple independent elicited statements.

The aggregate estimate by the ethnographer of the ratio L is, thus, logically constituted from constituent likelihood ratios. In a deep analysis these could be estimated by the ethnographer to unpack L but this would, of course, be a rather demanding and is an unlikely empirical procedure except perhaps when differing ethnographers reach inconsistent conclusions about L. Then some unpacking may reveal wherein differences lie. Ethnographers may exhibit some reluctance to make estimates of this sort but implicitly, if they venture to draw conclusions about causal mechanism they are implicitly doing so – therefore why not make it explicit then we can all observe what they are doing?

3.9 Meta-Ethnography

The analysis outlined above may surrender multiple but varying ethnographic estimates of the posterior odds for a given causal connection each deriving from multiple ethnographers and informants. In the context of narratives there may be multiple estimates for each such action driven causal connection located in in a narrative network (see below). Since the ethnographer and the informant produce a "measurement," we may appropriately ask the question as to whether any causal conclusions vary by informant and ethnographer pairs? Whereas the large N approach to causality implicitly adopts the standard empiricist assumption about the subject matter being independent of the observer, this is not the case in ethnographic study, where the ethnographer is necessarily part of the system which is studied. Effective social interaction of the ethnographer and the informant/actor, in pursuit of elicitation, inevitably requires this to be the case. In this respect a little pretention might be forgiven by drawing a parallel with the same issue in quantum mechanics.

How should varying posterior odds, for a given causal connection, be aggregated into an overall estimation of the odds of the link? If the ethnographers furnish alternative hypotheses (i.e. alternative actions connecting the sets $\{X\}$ and $\{Z\}$ then each contending hypothesis, as outlined above, will be aggregated across the appropriate supporting estimates. But let us concentrate upon a single hypothesis.

The natural extension of the Bayesian analysis is to adopt a Supra-Bayesian method (Clemen and Winkler 1999) whereby a meta-ethnographer treats all of the posterior odds estimated by each of the primary ethnographers as providing ethnographic evidence, alongside any prior she herself might entertain. Then the Supra-ethnographer would make an estimate of the likelihood ratios, and thence calculates her posterior odds. We might, however, in the light of our earlier precepts require the meta ethnographer not to bring any priors to the analysis. This overall procedure will of course involve all the unwieldy complications encountered above in estimating both the independent or dependent likelihood ratios. It, therefore, seems an unlikely aggregation candidate. Furthermore, it is difficult to adumbrate the criteria for the selection of the meta-ethnographer.

A linear pooling of the odds ratios of all the primary ethnographers with equal weightings which sum to 1 is probably more promising in this respect and where there is no reason to elevate one ethnographer above another. This then amounts to simple weighted averaging of the posterior odds ratios across all the primary ethnographers. An alternative is a normalised geometric pooling also with exponent weights when, once again, no primary ethnographer is given priority over any others. Whichever aggregation is chosen if each primary ethnographer is enjoined to set the prior odds at unity then the aggregation is solely across the likelihood ratios.

Ethnographers are scrupulously careful in comparing and contrasting (i.e. aggregating and separating) subjective reports in order to arrive at an estimation of "what is going on". There is, however, as far as we can see, no available framework within which this procedure can be systemized. However, as we noted above the Bayesian approach enables a common language of odds whereby comparisons may be made. Theories of probabilistic or odds pooling usually require that any aggregation technique should surrender unanimity (i.e. if all agree then this becomes the aggregate value), event wise independence (i.e. the aggregate only depends on the individual values) and Bayesian externality (i.e. it does not matter whether odds are updated before or after aggregation). Should these properties be taken as guides, on the meta-ethnographer's behalf, for causal inference where the basic evidence is subjective? Linear aggregation is unanimity preserving and event wise independent though fails to be Externally Bayesian. Geometric aggregation, on the other hand, is externally Bayesian and unanimity preserving, but not event-wise independent (Dietrich and Spiekermann, 2013).

We might start with a situation where all the primary ethnographers are in possession of the same set of statements $\{s_1, s_2, \ldots, s_m\}$. This could be achieved either as a natural consequence of their research or by what is sometimes called behavioural aggregation where the primary ethnographers are brought together as a group where they share the statements they have elicited in interaction with various informants. They will of course not necessarily attribute the same credibility to identical statements, nor to the estimated Posterior Odds of the causal link. However, behavioural aggregation usually searches out unanimity of the aggregate estimate then, if achieved, this would seem to provide the strongest grounds for inferring the odds of a causal link. Failure to achieve unanimity using behavioural aggregation might, however be taken to invite either additional linear or geometrical aggregation. It is difficult, at this stage, to advocate any particular aggregation technique – the issue warrants further research, if the whole ethnographic Bayesian framework towards causality is to be taken seriously.

A related approach is to make use of what we might call an Ethnographic Delphi -Technique where multiple ethnographers initially make independent estimates of the odds of a causal link. These are then are all made available to each of the ethnographers (without any interaction) and re-estimation invited. The procedure may then be repeated until a given level of consensus on the posterior odds is achieved. In the absence of such a consensus, alternative strengths of evidence will be entertained. Notice also that the Delphi technique could be used to try and settle differences between hypotheses rather than just to estimate the strength of evidence for a particular hypothesis. There is evidence that Delphi estimation sometimes provides more accurately predictive generalisations than statistical estimation (Landeta, 2006). This may prove particularly important in the context of our argument, to be introduced in Chapter 4, that multi-level network problems may prove to be impossibly daunting, in terms of the data required, when construed in terms of statistical frequencies (i.e., large N).

3.10 An Illustrative Empirical Example

Returning now to the illustrative empirical example introduced above. Both subjective causal and counterfactual statements $\{s_i\}$ were elicited from five members of the governing body (including the one examined above) of the producer cooperative in respect of the collective action of the appointment of an independent manager. One of the five was the chairman of the governing body (informant 1) and another the ex-chairman (informant 2). The first author of this book continues in the role of ethnographer. With significant prompting the informants agreed upon the causally connected sets X_c and X_o. The ethnographer assumed that the prior odds of the credibility of the five conjoined statements of subjective and counterfactual causality was 1:1 and estimated the likelihood ratios of the credibility of this evidence. With the assumption of the prior odds at 1:1 the likelihood ratios are then identical to the posterior odds of the credibility of the evidence. Table 1 gives the credibility estimates, by the ethnographer, of each informant.

Table 1: *Estimates of the credibility of statements by informants*

	$i = 1$	$i = 2$	$i = 3$	$i = 4$	$i = 5$	
L_{si}	10:1	10:1	5:1	8:1	4:1	$\overline{L_{si}} = 6.6{:}1$
$\log L_{si}$	2.30	2.30	1.61	2.08	1.39	$\sum_{i=1}^{m} \log\left(L_{si}\right) = 9.68$

The credibility derived likelihoods of the causal hypothesis that were elicited from the five informants by the ethnographer and are depicted in Table 2.

Table 2: *Justified belief in a Causal Relation*

	$i = 1$	$i = 2$	$i = 3$	$i = 4$	$i = 5$	Average
$\text{Odds}\left(\boldsymbol{H}{:}\neg\boldsymbol{H} \mid \boldsymbol{s}_{i1}, \boldsymbol{s}_{i2}, \ldots, \boldsymbol{s}_{i5}\right)$	10:1	10:1	5:1	8:1	4:1	6.6:1
$\mathbf{log}\left(\text{Odds}\left(\boldsymbol{H}{:}\neg\boldsymbol{H} \mid \boldsymbol{s}_{i1}, \boldsymbol{s}_{i2}, \ldots, \boldsymbol{s}_{i5}\right)\right)$	2.30	2.30	1.61	2.08	1.39	2.00

The credible evidence thus surrenders the odds that the causal link under investigation is correctly inferred at an average 6.6:1 across informants. Inspection of Table 1 and Table 2 enables any audience of the research to appreciate how this overall support for the causal link is constructed by the ethnographer.

3.11 Constructing Bayesian Narratives

We now turn to the construction of Bayesian Narratives, which string together multiple singular causal links of the by now familiar form, depicted as a Directed a-cyclical Graph (DAG)/network as, for instance in Figure 3.2. We describe the Narrative network as a DAG to emphasise that the causal arrows are explicitly conjunctive not alternative causes. The nodes in the network represent events and actions distributed left to right to indicate the passage of time. Event nodes must be connected by actions or sequences of actions showing how the events are causally connected. Figure 3.2 depicts a more complex version of the Narrative depicted in Figure 3.1. Attention is restricted here to individual actions, collective actions are addressed in Chapter 4.

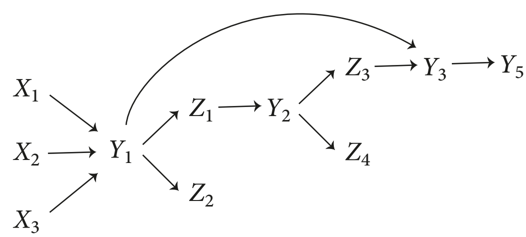

Figure 3.2 *A Bayesian narrative strings together multiple singular causal links*

Narratives contain several types of causal links corresponding to the distinctions drawn in Chapter 1.

Event → Action (e.g. $\{X_1\} \rightarrow \{Y_1\}$). Elicited as “I/she did $\{Y_1\}$ because of X_1”.

Action $\rightarrow_T$ Event (e.g. $\{Y1\} \rightarrow Z_1$). Elicited as “I/she did $\{Y_1\}$ to realise Z_1”.

Action $\rightarrow_T$ Action (e.g. $\{Y_1\} \rightarrow \{Y_3\}$. Elicited as "I/she did $\{Y_1\}$ to realise $\{Y_3\}$".

Action $\rightarrow_T$ Action (e.g. $\{Y_1\} \rightarrow \{Y_3\}$). Elicited as "I/she did $\{Y_3\}$ because of $\{Y_1\}$".

Causal links directly connecting actions (social interactions) may be both teleological and indicative.

3.12 Comparative Bayesian Narratives

If our finished product is a Bayesian Narrative comprising of multiple singular causal links (mechanisms) as depicted in Figure 3.2, the question may still be raised as to how similar two or more Narratives are to one-another, each explaining the occurrence of a sufficiently similar sets $\{Z\}$. Here we are seeking to detect any possible generalizations which may exist between "similar causal Narratives" rather than similarity between specific singular causal mechanisms. The intellectual objective is clear – to find whether or not two or more Bayesian Narratives may be regarded as sufficiently similar to warrant drawing the conclusion that essentially the same causal story is being conveyed by the two Narratives?

Let us assume they all connect sufficiently similar sets $\{X\}$ and $\{Z\}$. Then in general, in any single narrative, there will be multiple paths of causality running between these sets. Two or more of these connecting networks need to be regarded as sufficiently similar to warrant the conclusion that they convey the essentially the same causal story.

Consider first a single narrative, where the "parts" of the "whole" story are its constituent causal paths – perhaps commissioned by multiple actors (e.g. collective action). Furthermore, each part/path may be considered to be composed of sub-parts (i.e. sub-sequences in a path). Thus, when comparing narratives, in order to say that they are telling essentially the same story, this becomes a matter of comparing causal paths comprising parts of the whole story. To put it succinctly, are the part-whole relationships, that is the pattern of causal paths, in the comparative narratives of sufficient similarity to draw this conclusion?

Part-whole relationships (Epstein 2015) are usually regraded as reflexive, transitive and anti-symmetric generating a partial-order. A set of causal paths and parts of those paths may thus generate a partial-order. Depending upon the degree of refinement of the constituent paths one wishes to impose the partial order will be more or less extensive. Given a desired level of refinement (granularity) then the comparative analysis reduces to comparing partial orders. That is to say, mapping from one partial order to another. We shall not take this further here, but merely flag the potential of mereological analysis of Narratives which will be taken up empirically elsewhere.

3.13 The Interplay of Large *N* Causal Networks and Narratives

How can large *N* causal networks and Bayesian Narratives complement each other in pursuit of causal analysis? If *N* is intrinsically low, such that comparative analysis is impossible, then Narratives (or small *N* case studies) come entirely into their own. Also if Large *N* causal analysis encounters no comparative limitations then the question does not arise beyond using narratives in a purely exploratory role. But clear-cut situations like these are not always encountered.

We should first recognise how large causal networks may be given a Bayesian rather than frequentist interpretation. Although large *N* modelling is in practice closely allied, in social science, to regression models, as outlined in Chapter 2, a Markov network of causal links can be expressed in a probabilistic Bayesian manner. Thus, we may write for the network in Figure 3.3 the joint probability distribution:

$$P(X1, X2, X3. X4) = P(X1)\ P(X2)\ P(X3|X1, X2)\ P(X4|X1, X3)$$

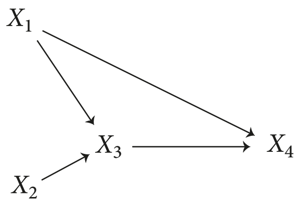

Figure 3.3 *3 A Markov model of causal links*

These probabilities might be given a Bayesian, rather than frequentist, interpretation as in Bayesian Networks. This procedure is clearly close, but not identical, to the elicitation of Bayesian Narratives. The latter seek direct elicited evidence for the causal links from which conditional probabilities may implicitly be calculated.

Consider a Bayesian who carries a prior on a causal link $X \to Y$ and observes a single case where Y follows X and attaches a likelihood to the probability of X given Y and updates his prior appropriately. Now assume she/ he elicits a subjective statement "I did Y because of X" (and the corresponding counterfactual(s)) to which she/he attaches, as outlined above, an appropriate credibility. Then the claim of ethnographic causal analysis is that the likelihood attached to this credibility weighted statement will increase the likelihood of the causal link by more than the merely observing X as in and Y. This is rather like saying that the "because" in the elicited statement carries additional causal credibility. A similar conclusion may also be drawn with respect to the ethnographic evidence for the relation between an action and its realisation.

Let us start by asking how Bayesian Narrative might contribute to Large N causal analysis. We have already seen how Narratives may fill what we might term the epistemic causal gaps between events in large N causal networks, by showing how the causal connections are actually generated. This may, in turn, guide the elaboration and selection of mediating action derivative variables between events. Such procedures may also invigorate a deeper theoretical understanding of a causal link. But these sorts of contribution have long been recognised and promoted by both the advocates of large and small N studies. However Narratives may help in addressing the ever present problem, encountered in

Large *N* studies, associated with the occurrence of unmeasured confounders. Case studies/ Narratives may be suggestive in sorting out possible candidates to be measured. We have also seen, in Chapter 2, how mechanisms inserted between observed variables can help to alleviate the problem of unmeasured confounders by allowing identification of the causal links between the observed variables. Narratives may be helpful guide in finding ways of conceiving such mechanisms, particularly where the mechanism may involve several paths of causally generating actions.

Pearl and Halpern (2005) have argued that situations which involve a choice between two alternative singular/actual/token causes, each of which might be sufficient for the effect, may be resolved by placing the basic causal model within a more extended network. Their solution has been contested by VanderWeele (2009, 2012), and others, but the debate clearly demonstrates the symbiotic relationship between large and small *N* causal networks. The famous singular case, which has animated endless philosophical debate (Hall, 2004) concerns two projectiles, both of which could have smashed a glass target. Of the two, which was the actual cause? We might note that ethnographic inquiry would attempt a resolution, which may not be decisive, involving statements by those observing the projectiles.

4. Multiple Levels of Causality

Social scientists frequently study groups, communities, organisations and other collective entities – even total societies – as "objects" or units of analysis. In so doing, they document their properties (e.g. group cohesion), their relationships to each other (e.g. inter-group competition) and the causal mechanisms, apparently operating at the collective level, which bring about changes in their properties and relationships. Call this the macro (or group) level of causal analysis. For many decades, issues have been debated which draw connections between this sort of analysis and a focus upon the individual units to be found within the collective entities, their properties (e.g. gender), their relationships to each other (e.g. interpersonal trust and normative expectations) and the causal mechanisms which drive changes in them. Let us call this the micro (or individual) level of analysis. In this context it is difficult to evade the conclusion that it is the motor energy of individual actions which ultimately provides the causal force at both levels.

When examining the relationship between the micro and macro levels the Coleman diagram (or boat, Figure 4.1) has gained a notable reputation. The diagram was anticipated by a number of authors (Epstein, 2015, Raub and Voss, 2017) but has become indissolubly associated with James Coleman (1990). The arrows in the figure are often, though not invariably, interpreted as causal connections between events of one sort or another. If they are to be so interpreted and we follow the arguments in the previous chapters, this will imply the location of connecting social mechanism, formulated in terms of actions or interactions which may be studied in ether a large or small *N* framework, depending upon the availability of comparators. This then implies a concept of macro or group action associated with arrow 4 and micro or individual action associated with arrow 2. How to treat the across levels arrows 1 and 3 is not as obvious (see below).

The "boat" rather than rectangular shape is used to suggest the passage of time from left to right. Furthermore, repeated diagrams whereby the "macro cause" is the outcome/effect of the macro effect in a previous cycle may be conceived. In addition, it is possible to think of more than two levels distinguishing, for instance, between the macro, meso and micro levels which would imply action derivative mechanisms at each level. We shall however largely concentrate upon two levels often designated as group and individual.

Unlike Coleman, we have labelled the four corners of the diagram as causes and effects as causal analysis is our focus. It should be made explicit at the outset that, in the large *N* statistical tradition, if causal inferences are to be made then samples of units of analysis at both levels must be available. The demand for comparators is accordingly multiplied at both levels.

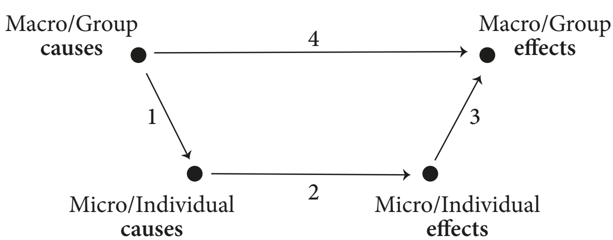

Figure 4.1 *The Coleman diagram*

Coleman was rather insistent that the explanatory (causal?) objective of the sociologist should always be to explain the macro outcome/effect or "social organisation" at the top right-hand corner of his diagram, not the micro level effect (i.e. the bottom right hand corner). Furthermore, he proposed that "the minimal basis for a "social system" is two (micro) actors each having control over resources of interest to the other" (Coleman, 1990; p29). This then implies that the micro level is inevitably concerned with interdependent (i.e. interactive) individual units which immediately brings into prominence the need to analyse the impact of networks at this level. This observation then raises the issue as to whether interdependent group/macro level units must also be involved, invoking group level networks? If so then in the large *N* framework we face the problem associated with sampling from networks at both levels. In the ethnographic, small N context, on the other hand, actions and narratives at both levels become the centre of attention.

Coleman was a very early advocate of what he termed, "structural research which will represent a truly sociological methodology". In this respect he was somewhat ahead of his time and it is only with the subsequent flowering of social network analysis that the "sociological methodology", as he conceived it, has begun to bear fruit. However, a casual inspection of the empirical literature in sociology suggests that traditionally most studies have taken explaining the bottom right hand corner of his diagram, in terms of supposedly independently sampled micro units (e.g. individuals), as the research objective. This objective is, of course, often propelled by the use of survey techniques which licence a large *N* statistical treatment and which, almost invariably, assumes independently drawn samples of individuals when making causal inferences (Goldstein 2011).

Clearly Coleman's diagram invites rather complicated research designs linking the micro (individual) and macro (group) levels. This is especially so if we continue to respect his direction to explain the top right hand corner of his diagram. As he poignantly observes, "…it is one thing to trace the development of social organisation in a particular instance, as a historian might do, and quite another to develop generalisations about such processes". Such difficult to study generalisations, we might suppose, would be causal. We shall argue that as a consequence of the inherent complexity in research designs, sociologists may often have to resort to ethnographic causality, rather than large *N* models of causality based upon comparison and generalisation (Abell and Engel 2019). Or, at least, they need to restrict their attention to ethnographic induction (Chapter 3) in any search for causal accounts of outcomes at the macro level.

The Coleman diagram inevitably generates heated debates about reduction, methodological individualism, top down and bottom up causality, in contrast to emergence and the conceptualisation of "social wholes" and collective causality (Epstein 2015). These issues have been widely debated in both the social and physical sciences often under the title of complex systems though no overarching analytical approach has emerged. The debate in the social sciences has though largely been pursued in the context of a large rather than small *N* perspective.

Reduction implies that any concept deployed at the macro level can be "derived from" or, "reduced to" concepts at the micro level, implying that the macro is defined in terms of the micro. This applies equally to the cause, and effect events and to any connecting action/interaction driven causal mechanism at the macro level. Emergence, on the other hand, denies that reduction should always be the case and then macro concepts may become, in some sense, sui generis. These issues may also be debated at more than two levels.

Particularly problematic are issues of causality. Can causal relations and mechanisms at the macro level (arrow 4 in Figure 4.1) always be reduced to micro causality, arrow 2, with the additional help of the between level arrows 1 and 3? Or, what amounts to the same thing, can macro causal mechanisms be faithfully constructed from micro causal connections? It is helpful in this context to distinguish macro concepts that might be emergent as a matter of principle and those that are pragmatically construed as emergent because of inherent micro-complexity.

Coleman (1990) argued, in the context of his diagram, that "no assumption is made that explanation of systemic (i.e. macro) behaviour consists of nothing more than individual action and orientation taken in aggregate. The interaction among individuals is seen to result in emergent phenomena at the system level, that is phenomena that is neither intended nor predicted….". Coleman, thus, construes emergent macro outcomes as the unintended and unpredicted consequences of micro actions/ interactions.

The hazards of making causal inferences when adopting the large *N* approach to multiple level analyses are often studied in the context of issues surrounding the ecological fallacy and correlation (King et al. 2005) where the macro variables are defined as the variation in mean values of the micro variables within a sample of macro units. Arrow 1 in Figure 4.1 is now in a sense inverted and interpreted as a definitional connection deriving the macro level from the micro as is arrow 3. Whether these should be regarded as causal is an open philosophical question as causes and effects are usually regarded as contingently separated and therefore not definitionally connected.

If we continue to demand, as we have argued in the previous chapters, that events must be connected by intervening action based mechanism variables then in the large N framework, the micro level causal relations take the form $X \rightarrow Y \rightarrow Z$ where Y is defined as a micro action derivative variable. At the macro (m) level the causal structure takes the form $X_m \rightarrow Y_m \rightarrow Z_m$. Action based variables are thus inserted into causal arrows 4 and 2 in Figure 4.1.

The X_m variable(s) may have a macro to micro across level (sometimes called contextual) causal impact upon the micro action, $X_m \rightarrow Y \rightarrow Z$. Thus, the Coleman diagram may be modified as depicted in Figure 4.2.

$$\begin{tikzcd}
X_m \arrow[r] \arrow[dr] & Y_m \arrow[r] & Z_m \\
X \arrow[r] & Y \arrow[u, dashed] \arrow[r] & Z \arrow[u]
\end{tikzcd}$$

Figure 4.2 *An elaboration of Coleman's diagram*

Figure 4.2 also allows for additional micro level causal variables, X, impacting Y. In a large N interpretation there may be multiple causes incident into Y deriving from both multiple macro to micro and micro level causes. If so this will generate a complex causal network running across levels incident into Y and thence causing Z.

One possible interpretation (e.g. ecological correlation) of the micro to macro arrow running from Z to Z_m is that it represents aggregations (e.g. mean values) of the micro Z scores across the individuals within a given macro unit; Z_m then varies across groups.

A questionable causal link (i.e. dashed) is included between Y and Y_m in Figure 4.2 indicating that micro actions may, in some manner, constitute (aggregate to) or cause the macro action. If a causal interpretation is adopted then the link between X_m and Y_m in Figure 4.2 may be regarded as spurious in terms of the micro action variable. We shall discuss these issues more fully below under the heading of ethnographic causality.

Turning now to an ethnographic inferential standpoint, it is rather straight forward to interpret the basic singular causal connections at the micro level along, by now, familiar lines:

$$\{X\} \rightarrow \alpha \text{ actions}\{Y\} \rightarrow_T \{Z\}.$$

Where α designates an individual (micro) actor (Chapter 3).

The sets $\{X\}$ may contain natural language descriptions of the group(s) to which the individual refers under elicitation. along with micro level descriptions. Set $\{Z\}$ will also contain micro level descriptions.

Set $\{C\}$ is dropped for clarity and $\{Z\}$ may include unintended effects which are acknowledged by the actor/informant (Chapter 3).

More complex micro causal relations will then take the form:

$$\{X\} \rightarrow \text{Micro Narrative } \{Y\} \rightarrow_T \{Z\}.$$

The intervening narrative may involve multiple paths of interactive causality – namely constituting a micro level Bayesian network – involving a number of micro actors.

It is tempting to adopt a parallel picture at the macro (m) level where elicited subjective macro/group action- "we acted $\{Y_m\}$ because of $\{X_m\}$ to realize $\{Z_m\}$" is elicited (Chapter 3) delivering the causal structure:

$$\{X_m\} \rightarrow \text{Macro actor } \{Y_m\} \rightarrow_T \{Z_m\}.$$

More generally:

$$\left\{X_m\right\} \to \text{Macro Narrative} \left\{Y_m\right\} \to_T \left\{Z_m\right\}.$$

Ethnographic causality can also be conceived (Chapter 2) as $\{X\} \leftarrow$ action $\{Y\} \to_T \{Z\}$ where $\{X\}$ is selected by the actor. This formulation which may be adopted at both levels does not, however, materially alter the above analysis which continues to apply.

Although Coleman urged that both the micro and macro units of analysis are characteristically interdependent, at their own level of analysis, it is only recently become widely acknowledged that drawing samples of units as if they are independent is likely to lead to entirely misleading causal inferences. This implies that we have to take account of structures (networks) of units, at all levels in the context of any causal inferences. This, as we shall see, introduces very demanding requirements in order to enable the application of large *N* statistical treatment to extract reliable results.

Given the demanding data requirements and assumptions of large *N* multi-level studies, it is unlikely that many of the multilevel problems, conforming with the Coleman diagram, which social scientists may wish to address can easily be pursued in a large N statistical manner. A multi-level analysis when only a few or even a single macro unit is available (either pragmatically or in fact) needs to be fashioned. But before exploring the implications of such an adoption it will prove helpful, from a comparative point of view to review the Large N approach to multi- level causal analysis where units of analysis are independent of each other and then introduce networks of interdependence.

4.1 The General Framework, for large *N* Multi Level Analysis

The statistical hierarchical linear model, where units of analysis at both the micro and macro levels (e. g. individuals and groups) can be independently drawn from, often implicit hypothetical populations, is now well developed (Snijders and Lazega 2016). The model can accommodate situations where the individuals are nested in a single group or in several groups (i.e. crossed membership). The standard models explicitly involve the impact of both fixed and random effects with the familiar standard assumptions about the distribution of disturbances at both levels. The model's powerful analysis is easily extended to more than two levels (e.g. individuals, groups and organisations). This extension though inevitably increases the burdens of comparison and generalisation.

If causal generalizations are to be inferred then it is assumed that the various parameter estimates are derived from samples drawn from suitably sized (technically infinite) populations of units at each level. Indeed, levels of analysis (two or more) are effectively defined as populations of units of analysis that can allow for independent random variation. The major issue is immediately evident: N must be of sufficient magnitude at all levels to warrant statistical estimation. The issue is magnified as the number of causal variables at each level multiplies (i.e. macro and micro causal networks). Whilst social scientists often find sufficient comparator cases at the micro level the same is not true for the macro level.

The basic hierarchical linear model centres attention upon the causal explanation of the outcome at the micro level taking account of macro causal (contextual) variables and thus fails to address Coleman's injunction that it is the macro level outcome that should be the focus of social scientists. The model can, however, be straightforwardly elaborated (Lüdtke et al. 2008) to make way for causal explanation of both the macro and micro effects. In terms of the Coleman Diagram, Figure 4.1, the model amounts to the elaboration of arrows 1, 2 and 4 but does not elaborate arrow 3. So, the macro variables drive outcomes at both the individual (along-side any additional micro level causal variables) and group levels. If arrow 3 in the Coleman diagram is interpreted as the aggregation of the micro effects to a macro effect for the macro unit of analysis.

The basic hierarchical linear model takes the analysis of sociological phenomena a long way in addressing a multiple level research design but always assumes that that the units at both the micro and macro levels can be drawn independently.

This is rarely the case and as Coleman observed we need to find "a genuine sociological analysis" which fully acknowledges the ways in which units at all levels are embedded in networks. That is to say, embracing both inter-individual and inter-group relations. An abstract framework, which acknowledges the non-independence of units of analysis, places the analysis of the causal relationships, in either the small or large *N* framework, between the micro and macro levels as running between two types of multi-relational networks:

1. Micro level networks, with various relation types between some or all pairs of micro units each carrying micro properties including micro actions;

2. Macro level networks, with various relation types between some or all pairs of macro units each carrying macro properties including macro actions.

The networks may be conceived as a multi-relational di-graphs or graphs (or matrices) each defined upon vector labelled nodes. Both networks may have both directed and undirected relationship types (edges) which may carry weightings (at various levels of measurement i.e. ordinal, interval and ratio.

In addition a bipartite mapping may be defined assigning each micro unit to one or more macro units.

The bipartite mapping may, thus, allow micro-units to be assigned to more than one macro-unit (e.g. multiple group membership of individuals). Also relations between micro units may run between macro units. In either situation then micro units may contribute to inter-macro unit relations in virtue of their bridging role between macro units. In addition, macro-units may be related by macro level relations (e.g. group relations not derivable from inter-individual relations). Group properties may also be derived from micro properties or be defined independently. The framework can easily be extended to three or more levels. The important conclusion to draw is that multi-level causality will operate in the complex connections of units of analysis within and between levels.

4.2 Large *N* Causality Between Micro and Macro Networks

The variables involved in large *N* causal analysis, amongst networks at both the micro and macro levels may be derived from:

(a) distributions of node properties, including actions across networks,

(b) the distribution of the position of the nodes in networks (e.g. node in-degrees)

(c) global properties of networks (e.g. network degree of completion).

At the micro (individual) level some inter-individual relations may run across macro (group) boundaries. Furthermore, micro units may have a place in more than one macro unit.

In virtue of the networks the causal analysis cannot be accomplished assuming SUTVA (Chapter 2) at either the micro or macro levels. That is to say, the causal impact of a variable upon an effect variable for any given unit, at both at the micro and macro levels of analysis, will not necessarily be independent of the values of the causal variables for other units. Referring back to Figure 4.2 the variables labelled as X and Z are best conceived as derivative of units of analysis embedded in networks of one sort or another. The variables Y and Y_m refer respectively to individual (micro) and group (macro) actions creating the connection between the X and Z variables Each of these possibly vectors of variables may contain variables of the types listed above. Thus, at the individual level we can envisage individuals (micro-nodes) carrying various exogenous properties acting, Y. in the context of an exogenous network of inter-individual relationships and exogenous contextual group properties and relationships, X_m to realise the Z variable. Similarly at the macro level the group carrying various properties will be embedded in a network of inter- group relations causing the group action, Y_m, realising Z_m. At both levels the X and Z variables may indicate changes in properties of the level specific nodes or their relationships (or both). In this respect the variables are derived from what we might term structural distributions where node properties are distributed across networks of relationships (Figure 4.3). The relationship between Y and Y_m, if subject to scrutiny, will detail how networks of related individual actions constitute as a macro action.

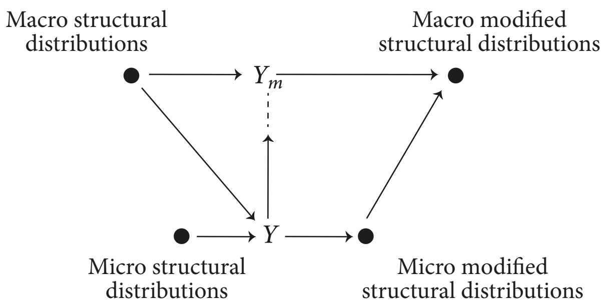

Figure 4.3 *The distribution of node properties across networks of relationships*

It is immediately evident that research in the large N tradition requires the investigator to sample from networks at both the macro and micro levels. Such sampling, even at a single level, is fraught with difficulties (Heckathorn and Cameron 2017). But let us start at the macro level – a network of groups each carrying group properties (emergent or otherwise). Groups may be selected for study across node property sampling or via macro – edge (relation type) sampling or by random walk sampling or by snowball sampling. Then at the micro level each selected group with an internal network of relations and micro node properties may be of a size that allows all the micro nodes to be studied. If, however, the groups are large then network sampling at the micro-level may once again be necessary. The statistical (large N) approach to the inherent complexity in the analysis of multiple level networks, despite being in early stages of development, is already rather impressive (Wang, Robins, Pattison & Lazega 2013). However the practical constraint of gathering comparative data with the objective of establishing causal regularities which match the inevitable complexity still looms large. It is difficult to see how many of the macro outcomes we might be interested to causally explain can be approached in a fully-fledged statistical (large N) manner. Observing samples of connected macro and micro units, each drawn from defined populations, is clearly daunting. Rather a small number or even a single case of the macro unit is a more likely focus. This implies that we will study a few macro units and perhaps seek to achieve a meta-analysis across other similar but not identical studies whilst retaining Coleman's ambition to explain macro outcomes. A limited sort of statistical analysis can be achieved with only a handful of cases/units using, for instance, Fisher's (1958) method, but any causal inference remains hazardous. Coleman (1990) himself acknowledges these issues, both in his analysis of the classical Weberian Thesis about Calvinism and Capitalism and in the early "qualitative" chapters of his monumental book. The nature of causal inference in these "qualitative" endeavours still remains rather obscure. How can we address the complexity of multilevel network analyses where units are not independently sampled and where data only upon a limited number of macro units is available or comparators are scarce? In either eventuality this rules out systematic comparison and statistical generalisation each of which are the standard ingredients of any causal inference.

To express the problem succinctly, we need to furnish a method of causal inference, charting the role which social actions and interactions play, that depends neither upon systematic comparison nor statistical generalisation across cases. The Coleman diagram when matched with narratives can then be depicted with narratives lying on causal links 2 and 4 providing the connective generative mechanisms. Thus, two types of narrative provide, as it were, the connecting causal mechanisms, respectively at the macro and micro levels. Before however exploring the conception of multi-level ethnographic causality it is important to take a view on the nature of arrow 4 in Coleman's diagram, namely upon macro causality.

4.2 Macro-Causality in The large *N* Framework?

Do macro causal effects (arrow 4) exist, or can they always, at least in principle if not necessarily in practice, be reduced to the conjunction of arrows 1, 2 and 3 in Coleman's diagram? This is of course a thorny issue in the history of sociology at least since Durkheim's time. If we stand by the assumption that causal links between events must involve intervening mechanisms, featuring actions and interactions (i.e. driving mechanisms), then to assert the existence of independent macro causes seems to necessitate a concept of irreducible collective action. This would imply that the narrative connecting the exogenous macro- structural distribution to its modification could only be constructed in terms of collective actors embedded in the macro networks. Reductionists would, however, say that when collective actions are correctly conceived they ultimately imply actions by individuals in the collective (group) taken in recognition of and on behalf of the group. Such statements seem to imply that the individual actions are, at least partially caused by the collective level (arrow 1). Although we favour in principle reduction we do not want to take a definitive position on this issue here. However as we shall see ethnographic causality provides a satisfactory interpretation of this problem. In the context of the Coleman diagram it is worth noting that the causal connection between the exogenous macro cause through arrows 1, 2 and 3 to the macro effect/outcome comprises a complex intervening mechanism running between the macro variables which are also, in addition, directly connected by arrow 4. The now standard way of thinking about the impact of direct causes, in observational studies, is due to Pearl (2009) and his concept of causality derivative of a- non-cyclic directed graphs (Chapter 2). By fixing the value of the intervening variable (in Pearl's analysis, by deleting all the causal arrows incident into the intervening variable) can surrender an estimate if the direct effect, here the macro causal effect, if it exists. Thus, the emergent standpoint requires that there is no such reduction available that eradicates the direct macro causal connection. This procedure, as we shall see, has implications for the ethnographic causality.

4.3 Ethnographic (Small-*N*) Causality in Multilevel Networks

Can singular causal connections, generated between node (actor) properties and the networks in which they are embedded, be derived from ethnographic investigation of those involved in the generation of the causal links which produce transformations in the structural distributions? The informants will inevitably be at the micro (individual) level. It is essential to recall that ethnographic studies initially centre attention upon the analysis of actions and interactions, not the events they connect (Chapter 3). The latter are then empirically derived from the ethnographic elicitations about the former. Although an investigation may commence by the ethnographer observing an outcome (e.g. Z_{mi} in Figure 4.2) the question posed is, how and why did the actions deliver this outcomes which must be acknowledged by the actor/informant under elicitation.

It is important to recognise the interpretation that must be given to the expression small N, as opposed to large N, in the context of investigation of causality at multiple levels. Research will usually centre attention upon a single macro (group) unit probably embedded in a network of macro level (inter-group) relations (i.e. small N) but may involve multiple micro units (i.e. possibly large N). For example, a single organisation with many individual members.

We shall motivate the argument for small N ethnographic inferences by examining the causal inferences of the type displayed in Figure 4.2 in the context of cooperative performance (Chapter 3). The macro unit (i.e. co-operative) was one of the few that managed to improve and maintain high performance. However, the data examined below is only partly based upon genuine elicited statements and uses language both imposed upon and negotiated, by the ethnographer, with the respondent. The model should therefore be treated as illustrative only.

In accord with the analysis to be found in Chapter 3 the derivation of the macro level causality may be sought from elicited subjective statements of the form:

"In situation where we were wanting to improve the performance of the coop so we decided, because of the intensity inter-group competition $\{X_m\}$, to improve group cohesion $\{Y_m\}$ in an attempt to realise improved performance of the cooperative $\{Z_m\}$".

Such statements may be supported by an elicited counterfactual:

"If the competition had been less intense and the performance better then we would not have sought to increase the group cohesion".

These elicited statements apparently provide macro -level evidence of a causal connection (mechanism) which may be, though not necessarily so, elicited independently of any singular statements, though they inevitably imply some micro level actions (see below).

Thus, the statements provide evidence for the basic macro level causal mechanism (Figure 4.2):

{low performance, inter-coop competition}→ macro action
{group cohesion} $\rightarrow_T$ {improved performance }.

More generally, the causal connection might involve a sequence of several actions by the focal macro actor generating a narrative.

$$\{X_m\} \rightarrow \text{macro narrative } \{X_m\} \rightarrow_T \{Z_m\}.$$

If the narrative involves more than one macro actor then there may be multiple paths of actions in the narrative.

Similarly, at the micro level in Figure 4.2 a number statements of the following form may be elicited:

"I attempted to establish strong trusting relationships $\{Y\}$ with several members because of the competition we were experiencing $\{X_m\}$ and recognising their commitment to co-op principles $\{X\}$. I hoped to improve my performance $\{Z\}$ ...".

$$\{X_m\} \text{ and } \{X\} \rightarrow \text{macro to micro and micro narrative } \{Y\} \rightarrow_T \{Z\}$$

Where the narrative may comprise of either a sequence of actions by a specific micro actor or a number of paths of action commissioned by various micro actors.

The connection between $\{Z\}$ and $\{Z_m\}$ is, as with the large *N* approach, usually one of aggregation rather than causality, showing how the micro outcomes (individual performances) combine to produce the macro outcome (group performance). In general. however, such aggregation will take place across a structural distribution. at the micro-level. Since both $\{Z\}$ and $\{Z_m\}$ are event sets it may be that a connecting causal action based mechanism should be invoked. This again would be a narrative showing how the micro actors build a collective outcome from their micro outcomes.

Fig 4.2 also depicts and aggregation between micro narrative $\{Y\}$ and macro narrative $\{Y_m\}$. this will amount to mapping a multiple path micro causal structure onto a single path macro ("we") structure. Abell (1987) proposes a homomorphic technique for achieving such mappings.

As we noted in Chapter 3 both future tense and general subjective statements may be elicited by ethnographers. This enables a tentative predictability of the results of an ethnographic study (Contrast what we concluded about large *N* studies with complex phenomena). Prediction is probably most robust where actions are institutionalised in terms of normative role expectations. Informants steeped in a culture are then likely to agree about the circumstances of anticipated future actions (Chapter 5). We must recall that ethnographic causal connections (explanations) are non-comparative singular in nature and the issue of generalisation only arises in the context of prediction not in deriving an explanation.

We may ask what conclusions may be drawn if a conflict occurs between subjectively defined causality and causal inferences suggested by a statistical large *N* study? Which is more reliable? If the subjective attributions of causality are highly credible, survive an Ethnographic Delphi Procedure (Chapter 3) and are adamantly retained by the informants when faced with the statistical results, then we may conclude that the action chosen for study is a statistical outlier though we should be aware of the above voiced misgivings that may be attached to statistical results in highly complex situations.

4.4 Conclusions

The Coleman diagram provides an indispensable guide to the construction of causal analyses of macro level outcomes. It has recently invited statistical (large *N*) analyses which are probably now best interpreted in terms of developing network hierarchical models which run faithful to Coleman's structural perspective in recognising networks at multiple levels. Such models should always be the first choice of sociologists, but often impose such demanding data requirements and challenging assumptions upon the investigator to render them impractical. When studies require the specification of detailed causal conditions, the number of available comparative cases often dwindles to the point where statistical techniques become difficult to apply. Causal analysis has been almost exclusively associated with a generalising, comparative large *N* perspective which has led many small *N* "qualitative researchers" to dispense with the concept altogether. However, Bayesian Narrative Analysis based upon ethnographic causality begins to open up a systematic way of inferring causality based upon subjective causal, counterfactual and counter potential evidence, where any limited generalisation across cases is posterior to singular causal explanation, not a presumption of explanation.[1]

1 Some readers may have seen a parallel between the issues raised by the Coleman diagram and the fraught debate about group selection in evolutionary theory. Indeed, some authors use the term co-evolution to describe the implied dynamics connecting the micro and macro in Coleman's diagram. However, dynamic processes are not necessarily the same as evolutionary selection and loose parallels are dangerous. It is not at all clear from the Coleman diagram what the selective units would be.

5. Role Theory, Social Norms and Ethnographic Causality

A few decades ago role theory stood at the foundation of social theory (Parsons 1951, Linton 1936, Biddle 1986, Winship and Mandel, 1983). Recently however it seems to have lost some of its early lustre. Here we want to revive the theory by drawing possible connections to ethnographic causality and narrative explanations. Since the conception of a role attempts to explain social actions at a generalised institutionalised level, it should lend itself to causal analysis.

A role is defined as a set of normative expectations, designed to influence the actions/forbearances of role incumbents, and which originate from other individuals occupying counter roles. However normative expectations can also be conceived as running between groups (collectivities), between individuals and between groups and individuals. Thus, the problems of multiple level analysis raised in Chapter 4 occur in respect of normative expectations embodied in group roles. Furthermore, the problems identified there concerning the difficulties in applying large *N* causality are inevitably also encountered here. It could be argued that the relative decline in interest in role theory is attributable to the difficulties of making it amenable to large *N* causal analysis.

Normative expectations, define networks where the nodes are roles (not role occupants) and the directed edges represent the normative expectations running between the nodes specifying appropriate actions/forbearances, in given circumstances, on the role incumbent's behalf.

If the set (in -degree) of normative of expectations deriving from counter roles are inconsistent then intra role-conflict occurs. In addition, since individuals can occupy many roles we can also speak of inter-role conflict, where incompatible normative expectations are operative across their role set (i.e. the number of roles occupied by an individual). Role expectations are empirically derived from incumbent individuals but it is important to acknowledge that the expectations are interpreted as institutionalised and pertain to roles not individuals. Role theory, thus abstracts away from individual networks to role networks and In so doing claims to enable analysis to move away from the idiosyncrasies of individual actions in favour of institutionalised actions.

The concept of normative expectation inevitably entails some appreciation of the philosophical debate about Deontic logic. However, the debate does not provide entirely firm grounds for the social scientist to work from, since there is much controversy about many aspects of the theory (McNamara, 2019). Nevertheless, concepts can be withdrawn which clarify the nature of norms and roles. The standard model deals with norms that are either obligations or permissions or prohibitions though other operators have been introduced (see below). Definitions in the standard model run across standard truth functional propositional variables and the truth of functional connectives of the propositional calculus. Thus, connected propositions are truth functional. Below we show that permission and prohibitions can always logically be defined in terms obligations. We shall use $OB(act\{Y\})$ for "it is obligatory to act Y". Specification of the actor is also dropped as obligations apply to any role incumbent.

We can in addition also specify ($OB(act\{\neg Y\})$, $\neg OB(act\{Y\})$ and $\neg OB(act\{\neg Y\})$. Recall that ($act\{Y\}$) may be named such that Y describes the direct consequence of what is done or something (causally) more remote. In the former case the action is defined in terms of the objective Y. Entirely parallel expressions rendering forbearances as obligatory are also possible.

The obligations may only run across the teleological causal link $OB(actY \rightarrow_T \{Z)\})$; that is, it is obligatory to act Y to realise Z.

Sometimes also the obligation may operate across the total causal mechanism; thus, using our received notation $OB(\{C\} \cdot \{X\} \rightarrow act\ \{Y\} \rightarrow_T \{Z\})$ alternatively expressed as $OB(act\{Y\} \rightarrow_T \{Z\} \mid \{C\} \cdot \{X\}$, that is, "if $\{C\}$ and $\{X\}$ it is obligatory to act $\{Y\}$ to realise $\{Z\}$". Despite the possible differences in the scope of obligation we shall assume that norms take the form – it is obligatory if $\{C\}$ and $\{X\}$ to act $\{Y\}$ to realise $\{Z\}$. A parallel formulation could cover forbearances. Thus, norms oblige our basic causal mechanisms. So conceived they are doubly contingent: contingent upon the causes of action/forbearances and contingent upon teleological consequences.

5.1 The Logic of Norms

Let us adopt the simplest formulation of a causal mechanism

$$\{C\} \cdot \{\neg Z\} \rightarrow act\ \{Y\} \rightarrow_T \{Z\}$$

Norms, if acknowledged, either maintain the state of the world at Z or change it from $\neg Z$ to Z. They can also take a form embodying any of the types specified at the end of the last section of the chapter. The basic normative operators are:

It is obligatory, *OB*, that…, (1)

It is permissible, *PE*, that…, (2)

It is impermissible, *IM*, that…, (3)

It is omissible, *OM*, that…, (4)

It is optional, *OP*, that…, (5)

Some may be rather surprised that *OM* and *OP* are included as norms. However, normative expectations may invite omissibilty and alternatives. Note now that:

$$PE(act\ \{Y\}) \leftrightarrow \neg OB(act\ \{\neg Y\}) \qquad (6)$$

$$IM(act\ \{Y\}) \leftrightarrow OB(act\ \{\neg Y\}) \tag{7}$$

$$OM(act\ \{Y\}) \leftrightarrow \neg OB(act\ \{Y\}) \tag{8}$$

$$OP(act\ \{Y\}) \leftrightarrow OB(act\ \{\neg Y\}) \tag{9}$$

Thus, obligation, *OB*, can be regarded, from an analytical point of view, as the sole normative operator. The rather neat implication of this is that analysis can proceed purely in terms of *OB* as any conclusions can then be translated into prescriptions in terms of the other normative operators. Furthermore, any conflicting prohibitions or permissions will imply conflicting obligations. Thus, role conflict may also be studied entirely in terms of obligatory norms.

The causal structure takes the form:

$$\left(OB\left(\{C\} \text{ and } \{X\} \rightarrow (act\ Y) \rightarrow_T \{Z\}\right)\right).$$

Where the promulgated normative expectation would be interpreted by the role incumbent and elicited by an ethnographer along the lines:

"I/ we /they should (ought) in situation $\{C\}$ and $\{X\}$ to do $\{Y\}$ to realise $\{Z\}$".

$\{C\}$ and $\{X\}$ may contain reference to playing a particular role.

"When occupying role $\{X\}$ I/we/they should --- etc".

The corresponding counterfactuals will take the form:

"I/we/they would not have done $\{Y\}$ if the obligation to do $\{Y\}$ had not been the case".

Elicited future tensed statements are also likely enabling prediction of actions causally connecting expectations and circumstances to outcomes (Chapter 3).

Since obligations often run both ways between types of roles (i.e. mutual obligations) and the structures so generated will, in turn, embody patterns of interactions (i.e. narratives) as causal Narratives.

Institutionalised normative actions have always been the natural focus of anthropological ethnographic inquiry where Institutionalisation implies the occurrence of repeated action types contingent upon a given context. They, thus, promote generalisation of singular causal connections. Elicited causal statements, taking the plural tense may then be elicited running along the following lines :

"I/we/they should (ought) always in situation {*X*} and {*C*} do {*Y*} to realise {*Z*}".

Repeated actions may thus enable the sampling of sufficient comparative cases to enable Large *N* causal analysis but the complexity issues still stand.

Of course, ethnographic causality may be applied to transient and evolving actions when comparators are scarce, but the strength of the conception may really show dividends in an institutionalised context, where complex patterns of actions are causally driven the normative expectations.

5.2 Role Expectations

Turning now to the promulgation of normative expectations within a defined community/collective; distinguish between individual (micro) and collective (macro) promulgators and recipients of role expectations. Thus, there are four possible situations:

> (1) Both the promulgators and recipients of the normative expectations are at the macro level. A normative network will be generated at the macro -level (Chapter 4). The elicited subjective causal statements, though inevitably deriving from individuals in the promulgating groups, will be expressed at the plural level – "members of the recipient group ought, in circumstances {*C*} and {*X*} to do {*Y*} to realise {*Z*}". Such expectations

will generate a network of expectations running between groups at the macro level. In turn, the expectations will, net of any role conflict, causally generate group actions/forbearances connecting a groups situation and its behaviour.

(2) The promulgator is at the macro level and the recipient is at the micro level (e.g. groups have normative expectations about individuals either within the group or in another group). The elicited subjective causal statements will be expressed along the following lines "We think that individuals (as role incumbents) in {*C*} and {*X*} ought to do {*Y*} to realise {*Z*}".

(3) The promulgator is an individual role with expectations directed at the macro level. Here the causal structure runs from the micro to the macro. "I think in {*C*} and {*X*} the group ought to do {*Y*}".

(4) Both the promulgators and recipients are at the micro level.

These alternatives enable multi -level ethnographic causal analysis as outlined in Chapter 4. Since institutionalised normatively expected role behaviour is inherently generalised this opens up a mode of analysis whereby singular ethnographic causality may be extended beyond the specific case.

5.3 Role Structures and Causal Analysis

We defined a role in the standard manner as attracting a set of normative expectations and also issuing normative expectations to counter roles. Any individual (or even group) may occupy several roles in a defined community.

Role theory is attractive because it can reduce the complexity of networks amongst individuals to manageable degree by reducing the number of nodes. But it only makes sense to invoke the theory where normative constrains on behaviour are strong. It is however generally recognised that many social organisations and communities are highly rule governed, usually by instrumental norms. Large networks invite an exploration of samples from norm structures (not an easy venture) with a view to inference to the population to enable causal inference. However, many rule governed collectives are too small to enable this procedure. The theories of ethnographic causality and comparative Bayesian narratives may make way for causal inference in these situations.

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7. Appendix: A non-Technical Introduction to Networks and Graph Theory

A network comprises of: A set (usually finite) of nodes $\{N\}$, (e.g. individuals). A relation R connecting some or all pairs of nodes. (e.g. which pairs of individuals interact). If all pairs are connected then the network is complete. If every node can be reached from every other node by tracing at least one path of relations then the network is connected.

The network may be depicted as a graph $G = (N, R)$ where the nodes are points on the page and lines (edges) joining the points are the pairwise symmetric relations as follows.

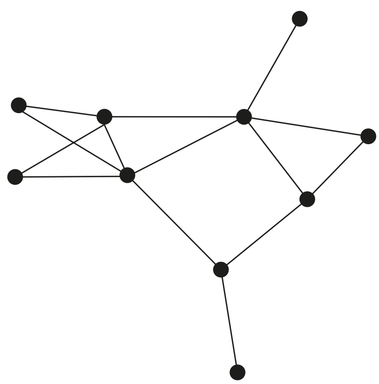

Figure 7.1 *A simple network*

The position of the points on the page does not change the graph, it is the connections that matter. The edges may carry arrows [be directed] then we have a di(rected)-graph. The in-degree of a node is the number of edges incident into anode and the out -degree the numbered edges incident out of a node.

The relation pairs may carry varying values (e g intensity of interaction).

The values may be at varying levels of measurement (e.g. ordinal, ratio).

Each node may have a variety of properties (vector labelled) (e.g. gender and types of actions).

The network (graph/digraph) may have multiple types of relationships (e.g. interaction and who trusts whom). These may be termed a multi (di)graphs.

Networks may be depicted as matrices with rows and columns representing the nodes and the entries the values of pairwise relations Multi (di)graphs will generate a matrix for each type of relationship.

Many other concepts describing aspects of a network can be derived from this simple picture (Borgatti, Everett, & Johnson, 2018).

In directed acyclic graphs (DAGS) the nodes are variables (including exogenous "error terms") and the relationships are directed causal links. The di-graph is acyclic when it contains no cycles. The links may convey the total direct effect of one variable upon another (i.e. alternative and conjunctive causes). DAGS are directed acyclic and graphs where the directed relationships are explicitly conjunctive causes.

This book explores the problem of causal inference when a sufficient number of comparative cases cannot be found, which would permit the application of frequency based models formulated in terms of explanatory causal generalisations.

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