SYMPOSIUM – AUTHOR'S REPLY

PROMISES AND LIMITS OF THE MODES OF COORDINATION PERSPECTIVE

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I am grateful to my critics and to symposium editor Manuela Caiani for the oppor-
tunity to address some of the core themes of my book, and indeed of my work over the
years. Hopefully, this exchange will clarify at least some of the persisting areas of ambi-
guity in my argument. My response will unfold in two stages. First, I will recap the basic
tenets of my approach to collective action, which will enable me to address some of the
limitations of the book and identify some roads ahead; then, I will move to more meth-
odological issues, concerning the translation of the theoretical model in empirical re-
search.

As Bassoli correctly notes, *Cement* displays a constant tension between two foci: it is
framed, starting from its title, as a book on civil society, but it is also a book on social
movements, and on their peculiarity vis-a-vis other ways of promoting collective inter-
ests. Bassoli asks if this is not actually a book on social movements that tries to address
a broader theme like civil society but ultimately does so in a limited way, maybe because
of the background of its author and his main research interests. He may have a point,
but let me argue why it is so difficult to disentangle the study of social movements from
that of civil society, regardless of whether the analyst belongs in the social movement
studies community or not. The most obvious, if purely practical, reason is that the ex-
pression "social movement" is frequently used loosely to designate the actors operating

within civil society in pursuit of public or collective goods, i.e., as synonymous to any form of political organizing other than political parties or special interest groups. The literature on the renaissance of civil society in Eastern European countries exemplifies such a use (Diani and Cisar 2014, 182–183). Another reason is that social movements are an inherently difficult phenomenon to theorize, given their complex and multidimensional structure. Because of this, their systematic analysis requires greater engagement with unresolved theoretical and methodological issues than interest groups might need (not because studying them is easier, but because there is a more consolidated tradition in the study of organizations than in that of more complex collectivities).[1]

These remarks lead us to the third reason, most directly linked to my work, namely, that (a) it is impossible to understand social movements without looking at broader processes of collective action; and (b) civil society is the relational setting in which such processes occur. More specifically, civil society is viewed as an organizational field consisting of "all the voluntary organizations engaged in the promotion of collective action and the production of collective goods" (Diani 2015, 12–13). Let me add two important clarifications here. First, although I refer to civil society as a field in a way which borrows from DiMaggio and Powell's (1983) definition, I do not make any assumption about isomorphic tendencies within that field, as Pavan suggests in her comment. Nor do I attach any normative value to the concept of civil society, in contrast to Bassoli who differentiates between civil and uncivil society. His is obviously a legitimate view, yet one which leads us to ignore the contested boundaries of what is civil (and thus legitimate or at least admissible) and what is not – as Charles Tilly (1989) among many others noted, a crucial step in the emergence of the modern social movement was the shift from a view of challengers as deviants to a view of challengers as carriers of an alternative political project. The distinctive feature of civil society is in my opinion the production of collective goods as indivisible outcomes that originate from collective action, regardless of their substantive positive or negative implications: for example, if anti-abortion movements managed to ban abortion again in Italy that would still represent a collective "good" (as no Italian resident could escape from the resulting situation) even if by my, and many others', ethical standards it would be a very bad "good" indeed.

Bassoli's distinction between civil and uncivil organizations is one example of the way in which an aggregative approach takes precedence over a relational approach, even among the staunchest advocates of social network analysis, a crew in which Bassoli clearly belongs (with even more enthusiasm than this author, I have to say). Looking at civil society, Bassoli decides a priori who the relevant actors are, according to criteria of

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[1] That's also incidentally the reason why I pay relatively less attention to the links between civic organizations and institutions, another of Bassoli's criticisms.

civicness, and then maps the ties among them (actually, mostly between them and political institutions). Ditto for social movements, defined by certain properties such as orientation to protest or challenges to authorities. In a revealing passage, Bassoli asks "Can we foresee a city with no contentious politics, no protest and no power challenger in which we can identify the social movement modes of coordination? If yes, as it seems throughout the book, what are we facing? It is not a social movement (lacking some of the aspects) or a coalition (lacking the relational aspects); it is probably some kind of interest group and nothing more." My reply to this very good question would be that what we are seeing might well be a social movement, in particular, one close to the "consensus movement" model. I should have probably devoted some space to this issue, which I have referred to in earlier work (Diani and Bison 2004, 285) and which can be detected for example among animal rights groups in my early study on Milanese environmentalism (Diani 2012, 116–117). Saunders is similarly embedded in an aggregative view of social movements (more on this when I move to her methodological critiques). Let us be clear about this: the identification of the units of a field is always the result of some combination of nominalist and realist criteria. That's to say, of some compromise between a choice dictated by theoretical criteria and one driven by the (discovery of) actual relations linking specific actors irrespective of their traits. But both Bassoli and Saunders seem to move from an aggregative view to a relational perspective rather than the other way around, as I do in the book.

In order to clarify what I mean by a relational perspective it may be useful to look at my conception of collective action. I see collective action as the set of practices oriented to the production of collective goods. This may take the form of pressure on authorities and power holders more broadly, but also that of direct production of the goods (e.g. in the case of action oriented to achieve changes in lifestyles, or to deliver specific services) or that of enforcing the adoption of certain behaviours by individuals through targeting and pressurizing. The pursuit of such goals relies at the very minimum, I suggest, on two broad classes of mechanisms, some focusing on resource allocation, the others, on boundary definition (what we might also call the construction of collective identities). Both mechanisms can develop in different relational settings, sometimes involving sustained exchanges between different actors, at others taking place primarily within the confines of specific actors. Some organizations may be inclined to exchange resources with other groups or associations, other organizations may work primarily on their own. Likewise, some organizations may rely primarily on their own identity when it comes to defining the "us and them" involved in collective action processes; other organizations, however, may use broader symbolic referents, identify with broader collectivities than

those represented by their own constituencies, and develop sustained feelings of solidarity with other groups operating in the same field. An implication of this approach is that, contrary to Bassoli's concerns, looking at civil society from a relational point of view does not mean to suggest that civil society is a network (this may or may not happen in different contexts or times). It means taking the question of the relational patterns between the actors mobilizing to produce public goods as a starting point.

The typology of "modes of coordination" (henceforth, MoC) summarizes the analytic properties of such patterns. The social movement MoC displays dense network exchanges referring to both resource allocation and boundary definition; the coalitional MoC implies dense networks of resource exchanges but sparse or absent links when it comes to boundary definition; conversely, a subcultural/communitarian MoC connects actors through dense networks of boundary construction (e.g., shared identities, shared members, etc.) but few resource exchanges; finally, an organizational MoC does not display dense networks, as mechanisms of both resource allocation and boundary definition operate largely within the boundaries of each specific group or association. Let me grasp the chance to clarify what is admittedly, despite my best efforts, a potential source of confusion: in the book I refer to "groups and associations" when I mean "organizations" as we conventionally know them (i.e., as bounded sets of coordinated people pursuing specific aims); I refer to "organizational MoC" when I mean a way of coordinating collective action that tends to concentrate within the confines of specific organizations. This may apply to highly diverse instances, ranging from radical organizations fighting each other for control of the same field, to interest groups focusing on ownership of specific sub-issues in mutual indifference to each other. As such, organizations (i.e., groups and associations) may be involved in any of the MoC identified by the typology. It is also worth noting that a "social movement" MoC does not refer to a specific social movement, mobilized around a specific cause. In both cities, the incumbents of the social movement block identify with a number of causes; they are best characterized as a social movement sector.

There is an important limitation to the way in which this conceptualization has been translated into empirical research in *Cement*. The book assigns organizations an even more central role than I would be inclined to assign them in theoretical terms. Collective action can be seen as a process that involves at least three elements: two have the capacity for agency, namely, organizations but also individuals; the other (events) lacks such a capacity, yet provides the context in which the process of formation of new agents may develop. Multiple levels of relations result: individuals perform acts (attending demonstrations, practicing lifestyles, participating in organizations, etc.) that often nest with other individual acts in larger interactive sets. Sometimes these sets of interactions

take a more stable form, follow rules and scripts that individuals take into account, and define boundaries: they take, in other words, the form of groups or associations. At other times, sets of interactions take a looser form, as they are usually associated with a specific time and space: they take, in other words, the form of events. Of course, both individuals and organizations – that are not simply bundles of individual acts, as Martin (2009) seems to suggest, but develop their own capacity for agency – can create links between events through involvement and processes of symbolic attribution. At the same time, events themselves may spur the development of new ties between individuals and/or organizations.

Therefore, it has to be clear (as I state in the concluding section of the book) that a proper relational design for the analysis of collective action processes within civil society is inevitably multilevel. And it is similarly clear that *Cement* is just one step in that direction (although, I may dare say, and my critics seem to agree, a substantial one). It covers organizations quite fully, but treats events in a partial way and individuals to a very limited extent. Crossley is correct in suggesting that events may be regarded as a constitutive element of collective action processes. They are important in at least two other ways. On the one hand, they may be looked at as a source of links between organizations: as synchronic ties other than those based on the perception of organizations as relevant partners, but also as a basis for a diachronic analysis of network change over time. On the other hand, organizations' involvement in multiple events tells us about the connections between events and therefore the possibility of seeing them as part of broader campaigns (on the relation between single events and campaigns, see also Diani and Kousis 2014). In Chapter 6 I use data based on events to all these effects, but I concur with Crossley that this is done to a limited extent and that this line of analysis represents an important way to expand our understating of contentious processes.

Moving to individuals, their role is treated very partially, as links between the organizations interviewed created by their core members. In other words, my data only enable me to chart personal ties between core members of organizations that I have interviewed. Pavan is right in exposing the limitations of this take, as the spread of new technologies draws our attention once again to the opportunities for individual action outside any organization (see e.g. Bennett and Segerberg 2013; Earl and Kimport 2011; Pavan 2012). An optimal dataset would actually consist of all the most active individuals operating in an area and would match those data against those on inter-organizational relations. A second best option would include at least data on all individuals active within organizations operating in a given field. If organizations were defined broadly, including loose informal groups of activists, then it would be possible to trace the ties created by individuals even among groups that are not working together, or look at the involvement

of individuals in events in which their organizations do not participate. This was the type of data I managed to collect for my early study of Milanese environmentalism (Diani 1995), even though unfortunately at the time I did not have the MoC model in mind (see, however, Diani 2012). I tried the same design in Bristol and Glasgow, but alas response rates were too low for me to use individual data meaningfully in the analysis. Nonetheless, I was able to collect data about almost one hundred core organization members in Bristol. While this was not enough to support a systematic analysis comparable to the one conducted in Milan, where I had information about 50% of environmental groups' core members, it enables an illustrative analysis of multimode networks involving individuals, organizations, and events.[2]

Whatever the type of actors (or, if we include events, elements) that we are able to include in our analysis, a proper understanding of their relations requires, as Crossley has repeatedly reminded us with his work (Crossley 2011; Krinsky and Crossley 2014), systematic attention to the cultural dimension. Of course, culture being such a polysemic word, a cultural approach to networks may take quite different meanings. In theoretical terms it is clearly impossible to separate the two terms, as network ties "exist" only inasmuch as they are constituted through cultural interpretation (see e.g. Mische 2003). Therefore, one promising line of inquiry might be to dig deeper into the processes through which different actors identify important alliance partners, define the properties and the implications (e.g. in terms of behavioral expectations, mutual commitments, etc.) of the ties in which they are involved, and the like (see e.g. Mische 2008). Another way would be to include networks of meanings and symbols in the analysis of collective action fields. Linking to recent developments in the analysis of cultural networks (e.g. Breiger and Pachucki 2010), it would be interesting to apply a 2-mode logic of analysis to the relation between civic actors and cultural elements. This would enable us to explore to what extent actors share symbolic systems and how this is reflected (if at all) in the identification of their most important partners and interaction patterns. From the reverse perspective, it would also enable us to explore the structure of the symbols used/generated by civic actors, based on the latter's co-occurring use of similar rhetorical figures (for a preliminary exploration, focusing on protest claims, see Diani and Kousis 2014). Yet another line implies exploring how dominant cultural traits of the context in which civic actors operate may shape their practices of tie building. That's what I do in the book, albeit quite superficially, as Crossley rightly notes.

Cultural mechanisms also linger behind de Fazio's remark that the civil societies I describe are remarkably free of internal conflictual tensions. It is certainly possible that if

[2] I am currently working on those data as part of a broader project on multimode political networks (Christopoulos, Diani, and Knoke in progress).

937

we looked more closely at the specific initiatives promoted by each group we would find a higher proportion of organizations engaging with specific opponents, trying to stop others' plans or to achieve their own goals against others' opposition. Yet, when looking at contentious politics we should not just look for specific conflictual interactions, we should try to capture how the actors perceive their role in relation to other social actors, and their capacity to conceptualize their opponents in social or political terms. If one group challenges, say, the specific behaviour of a specific public agency on a specific issue, it is certainly involved in a conflictual relation, but that does not necessarily mean it identifies itself in relation to political opponents broadly defined. The behaviour of the specific adversary in that specific case could be explained in the light of the misbehaviour or ineptitude of specific officials rather than as a result of institutionalized patterns of behaviour. Only recognizing the latter would imply the presence of a conflictual identity. Having said that, and moving to inter-organizational relations, it is certainly possible that the way the sample was selected discouraged the inclusion of radical groups, and in particular of groups holding opposite views to the democratic, inclusive principles dominant in the British voluntary and community sector. While there is no reason to think that the inclusion of such groups would have altered the structure of the network analysed here, it would have certainly enriched the analysis with the addition of a distinct component to the network.

One more possible line of expansion of the analysis is identified by Nicholls. He is probably too generous in acknowledging my attention for the spatial dimension. While it is true that the book makes what I regard as a quite compelling case that there can be no "civil society" and no understanding if local peculiarities are not taken into account, it is also true that the potential for a sustained analysis of the link between networks and space is largely left unexplored. This is a pity in consideration of the fact that both Bristol and Glasgow present features of the urban environment that may contribute to shape civic networks in non-negligible ways. The motorway in Bristol, and the river Clyde in Glasgow divide the cities not only on the physical level but also in terms of social status, which calls for a careful treatment of the political dimensions of urban space. The lack of familiarity with the tools of urban spatial analysis when I was conducting the crucial analysis for the study certainly contributed to this unfortunate omission. My current work on civic networks in Cape Town, South Africa, where I can count on the expertise of principal investigator Henrisk Ernstson and fellow researcher Lorien Jasny (Diani, Ernstson, and Jasny 2015; Ernstson, Sörlin, and Elmqvist 2008), will hopefully address these shortcomings.

And now to methodological issues. Saunders has voiced her reservations on my work for ages (Saunders 2007; 2008; 2011), and – to use her own words – does not miss the

opportunity to "indulge" herself one more time. Some of her criticisms are simply due to misinterpretations.[3] Others seem to me to rely on quite unreasonable expectations of the clustering procedures that are used to detect structural equivalence. Saunders seems extremely worried by the fact that some actors in the network are not located in the positions where she thinks they should be. In particular, she wonders "how many fairly poorly networked organisations end up being misallocated to the most densely connected block, and how many well-networked organisations are misallocated to the organisational modes of coordination block". Such a question is inspired by her exploration of figures 4.1 and 4.2. My understanding of cluster analysis is that there are always cases that do not fit neatly in one group or the other. I cannot see why this should not happen to an analysis of networks: the goal of structural equivalence analysis in the book (and I dare to say, in any scientific work using this approach) is to try and assign some – provisional and approximate – meaning to complex webs of exchanges. More generally, having always thought that any quantitative procedures yield at best approximations, I am a bit baffled by Saunders' insistence that any reference to structural equivalence should be preceded by the word 'approximate'. To me it sounds like referring to "wet rain" rather than simply "rain". In short, I am perfectly happy to go ahead with approximate equivalence as long as this yields meaningful results. Which the analysis presented in *Cement* does: the interplay between structural positions and attributes of the incumbents suggests significant differences between the two cities. The analysis shows in particular that some organizations that one would not immediately associate with social movements (e.g. because their members do not fit the profile of radical grassroots activists, or because they do not focus on protest) are actually involved in a relational social movement pattern, while the traditional traits of social movements do not necessarily matter, as I found in Bristol. Of course, this is a problem for Saunders – and indeed to some extent for Bassoli – but not for me as it points at the fact that the social movement MoC may well not merely involve 'the usual suspects'.

Saunders' most serious reservations have to do with the particular procedure I used to detect structural equivalence, Concor. She feels that the blocks it identified might be too artificial and too approximate to provide a "true" representation of local networks in Glasgow and Bristol. I have to say that I find her insistence on the discovery of the

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[3] For those who are really interested in these details (or petty skirmishes, if you prefer), Saunders claims for example, in critical reference to Diani and Rambaldo (2007), that "isolated actors were mistakenly allocated to different blocks of 'structurally equivalent' actors ……. (see Figures 1-3 in Diani and Rambaldo, 2007)." The problem is, as the captions of the figures and the text both clearly state (Diani and Rambaldo 2007, 775–777), only ties within blocks were shown in those graphs, for the sake of clearer illustration.

"true" structure a bit disconcerting. True in respect to what? As far as I am aware, clustering procedures always display a certain amount of variation in the partitions they generate. In general terms, as long as the blocks that Concor generates enable me to build a credible interpretation of the data, I am happy with that. Saunders seems to have problems with the fact that I work on three blocks and wonders how this is possible given that Concor proceeds by consequent splits, that should result in even numbers of blocks. But this is not necessarily the case, as blocks can be split at different levels, depending on substantive considerations. Let me recapitulate the procedure for those who may be unfamiliar with this particular routine. The structural equivalence analysis, the results of which were originally published in 2004 (Diani and Bison 2004), was conducted with Ucinet III that treated isolates as one of the first blocks in the split, and then ignored them. The second split applied to the main component and differentiated between a densely connected block (including the incumbents of blocks 1 and 3 in the book) and a poorly connected one (block 2 in the book, corresponding to the organizational MoC). The third split generated, in the case of the densely connected block, two blocks that were still densely connected in terms of resource exchanges, but differed in terms of the distribution of social bonds (i.e., ties which included personal contacts between members of two organizations or multiple memberships). As there were different structural dynamics going on in the two blocks, it made sense to keep them distinct. Instead, the third split of the block designated as block 2 in the book generated two new blocks that did not differ in structural properties (nor, it turned out later, in the characteristics of their incumbents). Therefore, there was no reason to keep them as distinct. Finally, the decision to include the isolates in block 2 made perfect theoretical sense because both isolates and incumbents of block 2 shared the same properties of the organizational MoC: they were not densely connected to each other, either in terms of resource exchanges or in terms of personal ties and shared memberships. Moreover, isolates shared the same characteristics as the incumbents of block 2, which further suggested the opportunity of a merge. It is also worth noting that the structural differences between the blocks were subject to a series of homophily tests (see Diani 2015, ch.4). The density of a block was considered to be relatively high anytime the chance that two organizations within the same position were connected was higher than random. It is a weak criterion, but a clear one. Saunders argues that I should have differentiated between high, medium, and low density blocks, but does not provide any criterion for the choice of a cut off point.

Having said all that, I have no difficulty in conceding that the clustering procedures that support an inductive approach to the identification of structural patterns are not exempt from problems. And I can easily see Saunders' reasons for opting for an alternative strategy, what she calls "deductive blockmodelling", a version of the 'generalized

blockmodelling' approach (Doreian, Batagelj, and Ferligoj 2004). Starting from predefined assumptions about the blocks amongst which a network should be partitioned may certainly help us to test some hypotheses about homophily mechanisms, on top of sparing us the instability of clustering procedures. I have myself applied this strategy on occasion, in particular when revisiting my data on Milanese environmentalism (Diani 2012), even though the exercise did not alter the fundamental findings of the original analysis, conducted with Concor from an inductive perspective (Diani 1995). However, I have two reservations about deductive blockmodelling. First, it seems to me that by starting with blocks of actors, grouped on the basis of some characteristics, we once again assign prevalence to an aggregative rather than a relational perspective. Second, and more seriously, it seems to me that the expectation that a good partition of the network maximize ties within blocks and minimize ties across blocks, treated as 'errors', leads us to overlook substantial structural properties. Focusing on the density of the blocks to the detriment of ties between blocks takes out the distinctive principles behind structural equivalence, namely, that similarity of position depends primarily on actors being linked to similar third actors rather than being tied to each other. For example, if a Concor-based analysis found two internally connected blocks of environmental organizations, one focusing on conservation, the other on combining environmental protection with political change, why should the ties between the two blocks be regarded as 'errors'? The presence of ties across blocks or their absence would point at two equally interesting dynamics going on: the persistence of conventional ideological divisions between environmental organizations, and the growing integration of organizations coming from a different background into a broader network of collaboration. I fail to see why the latter should be considered an 'error'.

On one point I certainly agree with Saunders: *Cement* will not settle the issue of what constitutes a social movement. As she writes, the line of thinking developed in the book "goes against the grain of many other existing definitions of social movement forms." In particular, the view of social movements as the set of actors embedded in a specific structural position within civil society, as opposed to linked to a specific issue or campaign, will need time to be digested. More modestly, I hope that Cement contributes to consolidate a different and genuinely relational line of conversation about social movements that I myself have contributed to start, along with others (Bearman and Everett 1993; Gould 1995; Mische 2008; Krinsky and Crossley 2014; Eggert and Pavan 2014). In particular, if I have managed to convince a few more people that fields of citizens' organizations may be studied (and indeed have long been studied: Diani 1995) with the tools of network analysis, I'd already be happy. Given the current state of research on

fields (most illustriously represented by Fligstein and McAdam 2012), that would be no minor achievement.

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