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The Resurgence of Élite Research: Promise and Prospects

A Comment on the Symposium

by Mark S. Mizruchi

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It took more than a decade after C. Wright Mills published *The Power Élite*, in 1956, for scholars to initiate a concerted effort to build on his work. Most famously, G. William Domhoff authored two seminal books, *Who Rules America* [1967] and *The Higher Circles* [1970], which attempted to address a major shortcoming of Mills' book: its inattention to actual politics, including the decisions of political actors. In the 1970s, a group of sociologists in North America and Europe began to apply a new approach - social network analysis - to the study of élites, with striking results.¹ Using (and in many cases, developing) a sophisticated series of techniques, and taking advantage of improved computing capability, these scholars published several important works, relying primarily on board of director links among corporations. These works provided several important findings: leading corporations were tied together in extensive webs of interlocks, suggesting the existence of a highly cohesive business community. At the center of these networks were the major commercial banks, suggesting the possibility that the prominence of "finance capital," noted by Hilferding [1910] and Lenin [1917] in the early Twentieth century, had reasserted itself in subsequent decades. Scholars also noted the possible existence of cliques of firms, tied together through shared financial connections, family relations, or geographic proximity. As Stokman, Ziegler, and Scott [1985] showed in the first major comparative study, these networks were pervasive in virtually all of the leading capitalist nations.

¹ See Fennema and Heemskerk [2016], for an illuminating discussion of the history of this field.

As valuable as these studies were, one problem remained: what I call the “so what?” question.² Much of the early work on corporate networks was largely descriptive. Although authors occasionally asserted or implied that the ties among firms had behavioral consequences, few actually demonstrated those consequences. This began to change in the late 1980s and early 1990s, and there is now a broad literature attesting to the effects of network ties on firm behavior.³ Ironically, however, just as researchers were beginning to demonstrate the behavioral effects of interfirm network ties, the focus shifted to firm-level strategies rather than larger questions about the role of corporate élites in the society. Although a handful of scholars continued to study corporate networks in a political context [see, for example, Burris 2005; Dreiling 2001; Windolf 2002], interest in this area among sociologists, at least in North America, appeared to have declined by the turn of the century.

Fortunately, this is no longer the case. After a seeming lull, research on corporate networks is back, with better data and more sophisticated techniques. Evidence of this resurgence is plentiful. Responding to growing concerns about economic inequality, scholars have begun to study the role of business élites in a broad range of nations.⁴ Conferences have been organized to address the topic. Student interest has increased. And even the popular press has directed its attention to the issue. Yet this resurgence has had a paradoxical outcome: those of us who study corporate élites seem to have returned to our earlier roots, with both positive and less-positive consequences. The promise and potential perils of the new research on corporate networks are illustrated by the four excellent papers on which I have been asked to comment. The authors of these works uncover fascinating new data from a range of countries. They deal with critically important substantive issues. They use sophisticated analytical techniques. And they provide a series of important findings. Where I hope the authors will go next – and what I eagerly await from their subsequent work – is a fuller response to the “so what?” question.

There is nothing wrong with descriptive work *per se*. In the hard sciences, the ability to describe a phenomenon such as protein folding or a process such as creating a cell line often constitutes a major contribution. We need to understand what exists and what occurs in the world before we can develop and test hypotheses about their consequences. What these authors are doing is therefore a necessary step. It is only a partial one, however, and I am hopeful that they will continue with their work and

² Other sociologists have also used this term, although to my knowledge I was the first to apply it to the area of corporate interlock research.

³ See Mizruchi [1996] for an overview of the earlier work; but much has been done since that article appeared.

⁴ See, for example, the book-length treatments by Kogut [2012] and David and Westerhuis [2014].

move it in a more analytical direction. In the remainder of this essay I will discuss these works, praise their authors, and raise some questions that I would like to see the authors consider as their work progresses.

I begin with the paper by Glucksberg and Burrows [2016] because it in some ways is the most preliminary of the four, although this makes it no less important. Increasing inequality is one of the most central developments of our time, and the fact that most of the increase has been due to the enormous growth of income and wealth at the top makes the issue of particular interest for elite scholars. How, Glucksberg and Burrows ask, do the super-rich manage their wealth? One answer is the rapid growth of an old institution: the family office, private offices that “take care of the family finances from cradle to grave” [Shelby White, cited in Dunn 1980, 45]. The most interesting feature of this paper is the discussion of Glucksberg’s fieldwork, a result of her ability, as an anthropologist, to penetrate the world of the super-rich and provide a detailed look at its inner workings. Using a contact she had made in a wealthy London neighborhood, Glucksberg secured an invitation to attend a conference of family officers. Her descriptions are revealing. She notes, for example, that dealing with personal conflicts among family members is often as important as managing the family’s wealth, that wealthy family members typically travel with no luggage (because all of their residences are stocked with everything they need), and the importance of having staff who live on-site, to ensure their loyalty and trustworthiness. The authors’ main conclusion, however, is simply that we need more research on family offices, and here I believe they understate the importance of what they found.

Family offices in the modern era (as opposed to their medieval incarnation) arose in the late Nineteenth century to handle the wealth of the emerging business elite, particularly in the United States. At the time, corporations were dominated by their founders, either by stock ownership or through complex financial instruments. Although the dominance of family ownership has persisted in much of the world outside the United States and Great Britain [La Porta, Lopez-de-Salines and Shleifer 1999], a system of management control emerged in the United States after World War I and remained largely in place into the 1980s. Glucksberg and Burrows are unclear on whether the older family offices declined during the managerial period, as well as the extent to which a new class of experts has emerged to service the super-wealthy, on both sides of the Atlantic.⁵ In fairness, as the authors note, data on the subject are scarce, and their call for further work on the topic is therefore fully warranted. A greater focus on the institution of family offices will undoubtedly shed further light on the ways in which the wealthy maintain their positions. Gaining

⁵ See Winters [2011] for a discussion of this industry.

access will continue to be difficult, however, and we will ultimately want to know whether (and if so, the extent to which) members of these organizations engage in collective action in the political arena.

Unlike Glucksberg and Burrows, who rely on ethnographic data, the authors of the other three papers employ quantitative network techniques. Two of the articles, one by Lebaron and Dogan [2016] and the other by Hjellbrekke and Korsnes [2016], use multiple correspondence analysis (MCA), an approach widely-used in Europe by scholars influenced by Pierre Bourdieu, who himself used the technique. In Lebaron and Dogan's case, MCA is used to account for differences in monetary policy preferences among members of the European Central Bank. In Hjellbrekke and Korsnes' case, it is used to examine differences between women and men (and among women) in the Norwegian "field of power." Both articles share similar strengths, and both raise similar questions.

Lebaron and Dogan are interested in the extent to which élites' worldviews and positions on economic policy are affected by their personal biographies. They address this question in two ways. First, they examine the extent to which officers of central banks worldwide fall into clearly identifiable clusters based on background characteristics. Second, they examine the extent to which the backgrounds and experiences of officers of the European Central Bank can account for whether they adopt "hawkish" or "dovish" positions on monetary policy, where hawkishness refers to strongly anti-inflationary policies and dovishness the reverse. The authors begin at a global level, conducting a multiple correspondence analysis of 312 bank governors from 158 countries between 2000 and 2016, using a series of demographic variables such as age, place of birth, gender, education, and professional experience. The multiple correspondence analysis operates much like factor analysis, in which the researcher extracts a linear combination of variables attempting to maximize the explained variance and then repeats the process to extract as much as possible of the remaining variance. The focus is on identifying differences among the actors' profiles (their values on the variables) in order to extract a distinct set of clusters. In this case, Lebaron and Dogan examined the first two components, which together accounted for about 25 percent of the variation. They then used the clustered variables to classify the subjects into groups, which included academic economists, legal professionals, non-financial civil servants, "pure" politicians, and both national and international financial bureaucrats. The authors' primary goal here was to use the bankers' personal characteristics to identify a series of distinct "types," each of which, according to the authors, exhibits a particular *habitus*, in Bourdieu's sense.

Having identified clusters of bankers at the global level, Lebaron and Dogan then attempt to account for the policy positions of 62 members of the Governing

Council of the European Central Bank, since 1999. Relying again on a multiple correspondence analysis, the authors identify a series of groups based on personal background and experiential variables. They find that those with academic connections tend to be more dovish, while those from Germany tend to be more hawkish. None of the other variables was statistically significant.

This study deals with an important topic – the role of one’s social background in economic policymaking, an issue that has been debated among elite researchers since the early 1970s. The study is based on an exhaustive and painstaking collection of data, and it contains two impressive analyses. I wonder, however, whether it was necessary for Lebaron and Dogan to conduct such an elaborate analysis to produce their findings. I suspect that they could have generated the same results and conclusions by simply taking the original background variables from their MCA, developing hypotheses about them, and inserting them directly into a regression equation. It is not clear to me that the authors’ findings – that German bankers tend to be relatively conservative in their monetary policy and academics tend to be relatively liberal – warranted the enormous effort that it took to produce them.

Similarly, Hjellbrekke and Korsnes rely on an impressive data set to address the distribution of forms of capital among women elites in Norway. In 2000-2001 the Norwegian Power and Democracy Project conducted a leadership survey that included 1,710 respondents, more than 87 percent of those contacted (a strikingly high response rate). The authors identified 30 variables, based on a Bourdieusian conception of types of capital – economic, cultural/educational, social (personal), and social (inherited) – and subjected them to a multiple correspondence analysis. The analyses yielded three principal components (called “axes” in MCA) that accounted for about 73 percent of the total variance. The results of the authors’ MCA are complex, and the descriptions of these findings are sometimes difficult to follow in both the Hjellbrekke and Korsnes and the Lebaron and Dogan papers. Several important ones emerge, however. Women clearly face disadvantages, but not in every respect. They are only slightly more likely than men to experience closure, for example, in terms of having connections to those in similar positions, and the women had worked in as many sectors as the men, indicating a comparable breadth of experiences. On the other hand, the men appear to be more prominent in organizations (especially economic ones) with a global rather than a local orientation, and the women within the business sector tended to have less breadth in their experiences than the men. Ultimately, power in Norway appears to follow the dominant global pattern, with women facing a series of disadvantages.

As with the Lebaron and Dogan paper, Hjellbrekke and Korsnes deal with an important issue, using a valuable data set and sophisticated techniques. As with the

former paper, however, I wonder how necessary it was to perform the analytical gymnastics that Hjellbrekke and Korsnes undertook in order to reach the conclusions they did. I suspect that a set of regression models using demographic variables and forms of capital to predict one's location in various organizations would have yielded similar results. As for the findings themselves, it does not surprise me that elite Norwegian women experienced disadvantages in 2001 – although Hjellbrekke and Korsnes do an excellent job of demonstrating the systematic inequalities among the women themselves – nor would it surprise me if those disadvantages continued to occur today. I have no illusions about the difficulty involved in replicating such a study, but it would be interesting to know the extent to which things have changed over the past fifteen years.

The final paper on which I was asked to comment, the work by Heemskerk and his colleagues [2016], deals with a topic that would require an additional paper to adequately address: the possible emergence of a global corporate elite. The vast majority of studies of business elites have involved a single country. The classic work by Stokman, Ziegler, and Scott [1985] and more recent ones by Windolf [2002] and David and Westerhuis [2014] contain studies of elites in several countries, but in most cases the countries are examined one at a time. Some scholars have studied elite networks in two or more countries simultaneously. A recent compilation by Kogut [2012] contains several such studies. The question of whether the corporate elite transcends national boundaries is an entirely separate issue, however. The issue of a global elite was first introduced by Meindert Fennema [1982]. More recently, William K. Carroll [2010] has published an exhaustive study, and others [Kentor and Jang 2004; Carroll and Fennema 2002] have debated the issue. Heemskerk, who is well-known for his path breaking study of the Dutch elite [2007] and has also authored, with Fennema, an important history of research on corporate interlocks [Fennema and Heemskerk 2016], is well-positioned to address this topic.

In a sense, the early corporate network researchers in Europe and North America were among the first users of “big data.” The data with which these scholars worked in the 1970s pale compared to what Heemskerk and his group use in their current study, however. Drawing on a major new database, the authors identified the boards of more than 18 million firms worldwide, which contained more than eight million directors who created ties between two or more of the firms. After the necessary data cleaning, Heemskerk *et al.* analyzed a network of more than 5.2 million firms connected by approximately 37 million interlocks. The authors' goal in this paper was to examine the relative centrality of cities, by focusing on ties between the cities in which the firms are headquartered. Although Heemskerk *et al.* acknowledge that the data on the United States might not have been as complete as the data from

Europe, they still found, interestingly if not surprisingly, that London was by far the most central city. American cities, including New York, by contrast, were, if not completely peripheral, certainly not a part of the core. Not a single North American city was among the 25 most central in the number of interlocks, and only one, ironically Washington and not New York, was among the 25 most central in terms of betweenness centrality [Freeman 1978-79]. One might argue that geographic distance was the main cause of the United States' relative isolation, but Sydney, Melbourne, Auckland, and New Delhi were among the 25 most interlocked cities. Most of the action in the global network is taking place in Europe.

Space considerations, as well as the reader's patience, prevent me from launching into a full-fledged discussion of the debates over the globalization of the *élite*. I am persuaded by Carroll that however globalized the corporate *élite* has become, most *élite* activity continues to take place within national boundaries, and Heemskerk *et al.*'s findings seem consistent with that notion. The implications of a globalized *élite* are of great potential significance, however, so this is an issue that will require further attention. In a recent work [Mizruchi 2013], for example, I argued that the corporate *élite* in the United States has largely abandoned its concern with the well-being of American society, an issue that had preoccupied the *élites* of the post-World War II era. The possibility that this *élite*'s loyalties lie with its global counterparts rather than its own nation is one conceivable explanation for this abandonment of concern for the conditions of the larger public. Ultimately, we will need to examine this issue at a more grounded level, by focusing on the extent to which specific domestic *élites* are oriented primarily toward their own nations, or toward no one nation in particular.

Conclusion

The papers in this volume demonstrate that research on corporate *élites* is alive and well. This topic, which appeared headed toward extinction in the 1990s, has spawned a flurry of activity. The focus today is far less North American-centric than in earlier years, the attention to global processes is a welcome development, and the introduction of sophisticated techniques and valuable new data sources promises to greatly advance the field. We do need to be careful, however. In our enthusiasm to take advantage of these new opportunities, we run the risk of repeating some of the practices of the past. Two in particular are cause for concern: the focus on description rather than explanation and the tendency for techniques and data analysis to run ahead of our ideas. As I've already noted, there is nothing wrong with descriptive studies *per se*. We need to understand what the world looks like before we can begin

to explain the processes within it. The elite researchers of the 1970s and early 1980s eventually began to address the “so what?” question. There is therefore good reason for optimism. Research on corporate élites is once again a hotbed of activity. Given the worldwide surge of inequality and the élites’ locus at the top of the distribution, the resuscitation of this field has come not a day too soon.

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The Resurgence of Élite Research: Promise and Prospects

A Comment on the Symposium

Abstract: Research on business élites has experienced a resurgence in recent years. Although the methods used by contemporary scholars are more sophisticated than those of their predecessors, the current research shares many of the strengths, but also some of the pitfalls, of the earlier work. I discuss four papers from this volume, highlighting their contributions to our understanding of both national and international élite networks. I conclude with a plea that the authors use their subsequent work to more fully address the theoretical implications of their findings.

Keywords: *Élites; Networks; Corporations; Interlocks.*

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