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Economic perspectives on leadership: Concepts, causality, and context in leadership research

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ABSTRACT

The fields of leadership and economics have interacted very little until now. The aim of this special issue on “economics and leadership” is to demonstrate the potential benefits of paying attention to and incorporating economic perspectives and methods into leadership research. Specifically, we argue that the field of economics can advance leadership research on three main topics: concepts, causality and context. For each topic, we summarize the state-of-the-art literature and showcase important insights, tools and findings. We also discuss possible critiques of the economic perspective on leadership. The timeliness of this issue is evident from recent discussions on the three C's within the field of leadership research, not least within *The Leadership Quarterly* itself. Our paper as well as the other six papers in this issue demonstrates that narrowing the gap between economics and leadership is not only beneficial and feasible, but also crucial for the progress of leadership research.

1. Introduction

Traditionally, leadership scholars and economists have tended to ignore each other's work. With a few notable exceptions, research on the same phenomenon, leadership, is still done in two parallel universes. In our view, however, this state of ignorant bliss that characterizes each field's views on leadership is counter-productive, and limits the extent to which we can better understand a complex phenomenon of broad interest and relevance in the social sciences. That is, our understanding of leadership and its importance for organizations and societies is more limited than if the two fields were to engage in more frequent and productive dialogues. The main aim of this special issue is to demonstrate the potential benefits of such dialogues and in particular the benefits of taking an economic perspective on leadership.

To provide a common platform for this discussion, leadership can be thought of as a means to an organizational or social end. More formally, leadership can be defined as “a formal or informal contextually rooted and goal-influencing process that occurs between a leader and a follower, groups of followers, or institutions.” (Antonakis & Day, 2017, p. 5). The idea that leadership is a means to an end is clearly of interest to leadership scholars, who have investigated a variety of ways in which the characteristics and behaviors of leaders interact with the organizational context in which leadership takes

place in order to determine how it can be influential in the pursuit of desirable objectives. Similarly, economic research is also often focused on obtaining desirable, or efficient, outcomes. Where incomplete contracts, imperfect information or other forms of market failure create challenges to efficiency, an improved understanding of leadership has the potential to greatly benefit economic research on the functioning of organizations and institutions. Indeed, a growing body of research in economics seeks to understand the influence of leadership in public and private organizations (Bloom & Van Reenen, 2007; Chattopadhyay & Duflo, 2004; Hermalin, 1998; Jones & Olken, 2005).

Despite this shared focus, the fields of leadership and economics have remarkably interacted very little until now (Zehnder, Herz, & Bonardi, 2017). Economists studying leadership pay limited attention to large areas of leadership research that investigate similar questions, while leadership scholars typically treat economic research as if it is irrelevant for understanding leadership. One can, of course, argue that specialization in science means that researchers working on related concepts can safely ignore each other's work—for example, a clinical psychiatrist evaluating medical treatments may find it unnecessary to know the precise chemical and neurobiological basis underlying a medicine's effects. But what is striking about the gap between leadership and economics is that researchers often investigate essentially the same question, but without doing a sufficiently careful job of looking

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for related work in the other field.¹

It is worth noting that one of *The Leadership Quarterly's* principal objectives is to bridge leadership research across different academic fields, and with time it is reifying this objective (see, for a quantitative breakdown by disciplines, Gardner et al., 2020). Accordingly, the main motivation for this special issue is to show that economics and leadership research have much to learn from and offer to each other, in multiple ways. We are obviously not the first to make this observation, not even in this journal (Antonakis, 2017; Sieweke & Santoni, 2020; Zehnder et al., 2017). Even though both fields could benefit from taking more notice of each other, this special issue focuses on showcasing why and how economics can be useful in studying leadership. Moreover, we aim to do so by providing inspiring recent examples of research which highlight how cooperation between the two fields can benefit leadership research in general.

The timeliness of a special issue on “economics and leadership” is evident from recent discussions within the field of leadership research, not in the least within *The Leadership Quarterly* itself. Three main topics are central in our view:

1. **Conceptualization.** Several issues are at stake here. Some authors suggest that popular leadership concepts like transformational leadership are invalid (Van Knippenberg & Sitkin, 2013) and that concepts like charismatic leadership require radical rethinking (Antonakis, Bastardo, Jacquart, & Shamir, 2016). Moreover, others argue that leadership research suffers from construct redundancy (Banks, McCauley, Gardner, & Guler, 2016; DeRue, Nahrgang, Wellman, & Humphrey, 2011; Schmidt, 2010), because there is a substantial theoretical and empirical overlap both within and between leadership styles (Avolio & Gardner, 2005; Banks et al., 2016; Judge & Piccolo, 2004; Van Dierendonck, Stam, Boersma, De Windt, & Alkema, 2014)². In contrast, economics has traditionally relied on a unified analytical framework in which concepts are defined formally, typically making it easier to identify and prevent overlap.
2. **Causality.** If the central idea of leadership is that it is a means to an end, establishing causal relations between different forms of leadership and outcomes is crucial. However, as it turns out, much current leadership research can be questioned with regard to whether causal relations are truly identified (Antonakis, 2017; Martin, Hughes, Epitropaki, & Thomas, 2019; Sieweke & Santoni, 2020). In economics, the problem of identification of causal relationships in empirical research has been acknowledged for some time now, along with possible solutions and their limitations (Angrist & Pischke, 2008; Deaton, 2019); it is increasingly difficult for researchers to publish findings in top journals if the design does not lend itself to robust causal interpretation.
3. **Context.** Although one of the most important findings in leadership concerns the idea that leader effectiveness depends on the context (Yukl, 2013), leadership research has typically defined context at the level of the individual leader within her team. Although scholars generally acknowledge that the context should also be studied at more meso- and macro levels (Liden & Antonakis, 2009; Oc, 2018), with few exceptions (e.g. Jacquart & Antonakis, 2015; Stoker, Garretsen, & Soudis, 2019) the context of leadership is still almost solely empirically studied at the micro-level, a state of affairs that holds for organizational research more generally (Johns, 2018). Economics on the other hand, focuses strongly

on the relevance of the organizational context and varying conditions in the external environment.

On all three topics—conceptualization, causality, and context—the field of economics offers important and relevant insights, tools and findings that could be used to advance leadership research. These topics and their importance for leadership research are the main motivation for the current special issue.

At the same time, the study of leadership by economists is certainly not without its own limitations and problems—in particular, when it comes to studying leadership *traits* and *behaviors* as well as observer *perceptions* and *attributions* of leaders, which are arguably key points of leadership research (cf. Antonakis & Day, 2017). Here, the simplicity on which economic theory is built, and which empirical economic research takes as a starting point, can often be detrimental to understanding complex psychological processes, behaviors, and their consequences. To a leadership scholar, what economists define as “leadership” in their research may at first seem oversimplified and too abstract to represent a meaningful way of understanding the phenomenon. Nevertheless, and despite clear differences in research traditions, this special issue will show that narrowing the gap between economics and leadership is not only beneficial and feasible, but also crucial for advancing the field of leadership research.

The appropriateness of this special issue arises partly from the fact that the infusion of economics into leadership research, and the growing attention that economists are paying to the topic of leadership, is starting to yield important new research and is shaping the approaches used by researchers in both areas. The special issue is also timely because current state-of-the-art research in economics and the adjacent field of strategic management, which we also include as representative for an economic perspective on leadership, convincingly shows that the heterogeneity of performance in teams, organizations or even nations can only be partly attributed to contextual (i.e., external) factors; so ultimately, a substantial portion of the variation in quality or performance is likely driven by individual leaders or managers (Bloom et al., 2019; Quigley & Hambrick, 2015).

From a societal or policy point of view, a “meeting” between the fields of economics and leadership research seems more relevant than ever, given the increasing importance that is attached to individual leaders and leadership, both in politics and organizations. This importance of leadership is nicely illustrated by the fact that *The Economist*, perhaps the most widely read magazine among economists and economic policymakers, concluded that when looking back at their 175-history: “We are now not only more worldly; we are also more people-y. Even accounting for the length of the paper, Margaret Thatcher was mentioned three and a half times more often at her peak than was William Gladstone at his, part of a general trend towards reporting on personalities” (December 22nd, 2018). This position is visualized in Fig. 1, which shows for the case of American presidents how often that person was mentioned in the magazine, showing a clear increase over time in the focus on individual leaders in the political context.

Our introduction to this special issue is organized as follows. To better understand whether and how economics deals with leadership, the next section provides a brief introduction to the relevant literature. It zooms in on those elements that seem especially important from the abovementioned discussions in current leadership research, namely on *conceptualization*, *causality* and *context*, respectively, in Sections 2.1, 2.2, and 2.3. This section also serves as a backdrop for some critiques of the economic perspectives on leadership (Section 2.4). Subsequently, and armed with the brief survey of the view on leadership from economics, we will discuss the feasibility of a productive meeting between economics and leadership research in Section 3, by using and showcasing the six papers from our special issue. We demonstrate how these papers, in different ways, actually do merge economics and leadership research. From this discussion we will distill the main contributions economics could make to leadership research and vice versa, and also

¹ To provide anecdotal support for this point, one of us set out to find an example of where economists studying leadership had failed to cite a prominent paper in leadership research that addressed very similar questions. It didn't take long, as he quickly realized that one of his own papers (Brandts, Cooper, & Weber, 2015) should probably have done a better job of identifying and acknowledging relevant work in the other discipline (Van Vugt & De Cremer, 1999).

² These critiques apply to the field of (social) psychology more generally (see Banks et al., 2016). Barclay (1973) already compared the situation in psychology with an explosion in a confetti factory, implying a myriad of seemingly disconnected findings and theories.

Fig. 1. Frequency of American presidents mentioned over time in *The Economist* (© The Economist Group Limited, London, December 22nd, 2018).

discuss potential frictions or stumbling blocks. To round up the introduction to the special issue, Section 4 concludes and points out a number of future directions for research.

2. The economic perspectives on leadership

2.1. Conceptualization of leaders and leadership in economics

What is so important about leadership to begin with? This seems like a rather odd question for readers of *The Leadership Quarterly*, but it is a useful starting point for a discussion on how economists view leadership. Economics is a field with strong shared assumptions about the world and actors, often referred to as “agents”. A common starting point for thinking about such a world is a purely hypothetical environment involving perfect information and highly sophisticated individuals, where all eventualities about the possible “states of the world” and the actions of other economic agents are perfectly known or can be anticipated and, therefore, in which all actions can be clearly prescribed in binding and enforceable state-contingent contracts. In such settings with “complete markets”, there is no need for leadership—or even for organizations or firms—because the exchange of products and services via frictionless decentralized markets typically yields outcomes that are maximally desirable from the perspective of society’s welfare. The scope and applicability of such general equilibrium models, pioneered formally by Nobel-prize winning economists Kenneth Arrow and Gerald Debreu, means that they still serve as a starting point for much of today’s economic research.

Starting from such an extreme hypothetical world, economists often introduce frictions, or realistic imperfections, to the above idealized setting in order to understand why in the real world we observe inefficient outcomes or behaviors; they also examine the policies and institutions, such as firms, intended to remediate these inefficiencies. For example, introducing the concept of “transaction costs” rationalizes the existence of firms and organizations as an additional coordination and incentive device next to the market mechanism (Coase, 1937; Williamson, 1975). One critical source of transaction costs results from incomplete or imperfect information, where economic agents may not know or may not be able to observe everything that is relevant for their decision making. For example, a chip manufacturer may not know what kinds of PCs or tablets a firm that it supplies will produce in the future, and specifying every possible contingency in a contract may be too costly. Alternatively, members of a project team may have an incentive

to shirk and pursue their personal interests when their behavior is unobserved by others, and this lack of observability makes it impossible to specify the required behaviors in a contract (Kosfeld, this issue). Such settings—involving a need for coordination in the first case, and for cooperation in the second case—create not only a need for organizations and firms but often also for leadership, or individuals who can take a central role in motivating or guiding the behavior of agents who would otherwise not know what to do or might want to do the wrong thing. This perspective on leadership is highly *functional*, because the existence and actions of leaders only have a role as mechanisms to solve coordination or cooperation problems.

Importantly, from the perspective of leadership research, most of the forms of leadership investigated by economists as solutions to the above problems are solely of a *transactional* nature. We use this term in its pure form to denote real contractual obligations with real incentives and disincentives (unfortunately, this term is used rather loosely in the leadership literature and measurement or manipulation of transactional leadership hardly ever involves real stakes). Rather than focusing on broad forms of leadership, economists typically study a much narrower form of management (Kotter, 1990), relying on monitoring, explicit performance targets and extrinsic incentives for attaining those targets (Bloom, Genakos, Sadun, & Van Reenen, 2012; Zehnder et al., 2017). Thus, leadership in economics often takes the perspective of a principal-agent relationship, in which the leader (the “principal”) attempts to design work tasks or incentives in a way to get the follower (“agent”) to do what is desirable from the leader’s perspective (Gibbons, 1998; Hart, 1995; Holmstrom & Milgrom, 1994; Jensen & Meckling, 1976). Even in cases where the leader is not assumed to be directly manipulating financial incentives—as when leaders decide which tasks to delegate to subordinates (Aghion & Tirole, 1997) or when a leader takes a visible action that signals information to followers (Hermalin, 1998)—the ultimate channel through which leaders are effective is by shaping the incentives confronting followers. These views on leadership are rather well-suited for analyses using standard economic approaches, such as game theory, in which financial incentives can be easily mapped into game payoffs. But it means that when economists talk about “leadership”, they are often only referring to a very small part of how most readers of this journal understand this concept.

Nevertheless, there are examples in economics that explore ways in which leaders influence followers independently of directly or indirectly changing the extrinsic incentives they face. For an early example, see the work by Rotemberg and Saloner (Rotemberg & Saloner,

1993) in which the authors make a distinction between two different leadership styles (participatory and autocratic) and relate the appropriateness of these two styles to the external context of the organization. Moreover, by making a classification of leaders as motivators or communicators, Bolton, Brunnermeier, and Veldkamp (2012) also define leadership as a function that goes beyond the transactional role of a leader. More specifically, a growing strand of research in economics investigates how communication from leaders alone can influence follower behavior in resolving issues of coordination (Brandts et al., 2015; Brandts & Cooper, 2007; Cooper, Hamman, & Weber, 2020) or cooperation (Antonakis, d'Adda, Weber, & Zehnder, 2019; Boulu-Reshef, Holt, Rodgers, & Thomas-Hunt, 2020). Crucially, much of the economic research on leadership ignores the psychological make-up of the leader in terms of traits or rich forms of leadership behaviors, which are of course central in leadership research; though, even here, there are a few notable exceptions that study simple individual characteristics of leaders (d'Adda, Darai, Pavanini, & Weber, 2017; Graham, Campbell, & Puri, 2013; Kosfeld & Rustagi, 2015; Malmendier & Tate, 2005).

These brief introductory remarks on the foundation of leadership in economics are meant to drive home the following points to leadership researchers (see also Hiller, Piccolo, & Zaccaro, this issue). First, leaders often exist and matter because they are a possible, though imperfect, means to deal with very specific obstacles to efficiency, such as cooperation and coordination problems that arise in organizations. Second, in this highly functional view of leadership, the traits and behaviors of the leader are mostly ignored or merely taken as given; a leader *does* certain things and those are ultimately responsible for the leader's success or failure in solving the problem leadership is meant to address (and economic theory also assumes that firms appoint the most competent individuals as leaders; obviously this assumption is easily challenged). Third, leaders do not operate in a vacuum, their existence and effectiveness is always context-specific, that is, based on the rules of the underlying "game." Thus, it is not possible to analyze or conceive of leadership without analyzing at the same time the prevailing organizational or market constraints and the preferences and motives of others (e.g., followers). As a final observation, this literature is highly theoretical and much of the empirical research on leadership involves very abstract lab-experiments. These experiments are a valuable tool for establishing causal relationships regarding how a leader influences followers' behavior (Brandts & Cooper, 2007; d'Adda et al., 2017), but by definition ignore the real-world context in which leaders operate.³ This observation raises the more general issue of what economic research has to offer when we turn to leadership outside the lab, in more complex field settings. This is the topic to which we turn next focusing mostly on non-experimental research.

2.2. Leadership in economics: Causality

In a seminal paper in the field of finance within economics, Bertrand and Schoar (2003) investigate for a large sample of (US) firms the main determinants of the substantial variation in financial performance. The usual suspects in this line of research turn out to matter: the age, size, and sector of the firm explain part of the variation in performance. But a substantial part of the "between-firm" variation in firm performance is left unexplained. When they add a dummy variable for the CEO of the respective firm, it turns out that this "CEO dummy" is to a large extent responsible for the unexplained residual variation in firm performance. Crucially, what characterizes the CEOs or what they actually do is not part of the analysis. The conclusion of Bertrand and Schoar (2003) is therefore simply that CEOs and hence (business) leaders matter (and quite a lot) for firm performance, but we do not know why or how.

³ However, economists also use field experiments which have a higher external validity, much more so than do the fields of management and applied psychology (Lonati, Quiroga, Zehnder & Antonakis 2018).

In sharp contradiction to much of the theoretical perspectives we discussed in Section 2.1, the empirical approach to leadership in economics takes the existence and conceptualization of leaders for granted. However, it focuses on two questions:

1. Do leaders matter for some outcome variable?
2. Can the direction of this relationship between leaders and outcomes be determined such that it can be shown that the outcome is, indeed, *caused* by the leader?

The second question is of particular importance, because it clearly differentiates from how leadership scholars often study the relationship between leadership and outcome variables. Such scholars tend to take the existence and influence of leadership as a given and no effort is made to go beyond studying associations. Economists, however, are keen to inform policy by establishing causal relations and have thus worried more about issues like reverse causality, omitted variables or, more generally, about the endogenous nature of leadership with respect to outcome variables (Antonakis, Bendahan, Jacquart, & Lalive, 2010). Because leader behavior or its perceptions are observed and not necessarily exogenous, economists think that it is folly to use these behaviors or perceptions as explanatory variables of other endogenous outcomes, unless the appropriate estimator is used to identify the causal effect. These issues were identified a decade ago by Antonakis et al. (2010), yet only recently the topic of endogeneity becomes quite prominent in the fields of leadership and management research, as is shown in the review by Sieweke and Santoni (2020).

For economists, who are critically concerned with the identification of causal factors, research designs have to deal in a proper way with issues of causality. Does, as Bertrand and Schoar (2003) imply, causality indeed run from leadership to firm performance? This question is important, because it could just as well be the case that a relationship between leadership and performance either runs the other way around, or that both variables are influenced by an omitted third variable (e.g., leader intelligence, company resources, cultural or legal factors), which also makes inference of causality problematic.

A seminal paper by Jones and Olken (2005) addresses this issue, investigating the relationship between national political leaders and economic growth. In their study, they exploit the unexpected, natural death of political leaders in a large sample of countries to infer the causal relation between leadership and macroeconomic growth. The exogenous variation in the change in leadership is critical for establishing a causal relationship, because it enables the researchers to rule out that the change in leadership was caused by the state of the economy, or expectations about the future state of the economy, or by the economic policies implemented by the leader. The paper by Rizio and Skali (this issue) can be seen as an extension to Jones and Olken (2005). Similarly, Quigley, Crossland and Campbell (2017) employ a comparable approach to investigate how unexpected CEO deaths translate to market reactions. More generally, economics offers numerous examples of how causality between leader(ship) and outcome variables could be inferred, not only by using (quasi-) natural experiments as in Jones and Olken, (2005), but also by employing leader fixed effects (as essentially in the Bertrand and Schoar (2003) study), or alternative methods like difference-in-difference (Adams, 2016; Yang, Riepe, Moser, Pull, & Terjesen, 2019), regression discontinuity designs (Arvate, Galilea, & Todescat, 2018), matching designs (Bechtoldt, Bannier, & Rock, 2018) or event studies (Stoker et al., 2019). Given the new editorial policy of the journal (since mid-2016), which has been open to economic points of view, these approaches have already appeared in *The Leadership Quarterly* (Sieweke & Santoni, 2020).

Research in economics on the second question mentioned above also invokes presumably exogenous leader characteristics to see how these influence their actions as leaders. More specifically, this line of research typically focuses on the role of the CEO. The goal of these studies is to be able to create exogenous variation in the "CEO

variable”, which enables the identification of the impact of different kinds of CEOs on firm outcome variables. This approach has led to innovative research in economics, whereby exogenous features or traits of future leaders, as well as exogenous external events, are invoked to link current firm outcome variables to present-day CEOs. [Graham et al. \(2013\)](#), for instance, use the Big Five personality traits of US CEOs to explain variation firm performance, while [Malmendier and Tate \(2005\)](#) investigate how the degree of overconfidence exhibited by individual CEOs affects corporate investments. [Goldfarb and Xiao \(2011\)](#) argue that CEOs' strategic sophistication—or the ability to think about opponents' behavior in a game-theoretic setting—drives successful entry into markets with fewer competitors. It is important, though, to account for correlations with other traits, to ensure these do not drive the relationships. Thus, such research typically controls for the “usual suspects” of traits including general intelligence, sex, personality (see [Zaccaro, Green, Dubrow, & Kolze, 2018](#)), in addition to correcting for measurement error in these variables and controlling also for firm, industry, country, and time fixed-effects ([Antonakis, House, & Simonton, 2017](#)).

In addition, there is a rapidly growing literature in (mainly) the field of finance that uses early-life information on “future-to-be” CEOs, to assess the impact of allegedly exogenous early-life conditions on firm actions and performance after these individuals became CEOs later in their lives ([Adams, Keloharju, & Knüpfer, 2018](#)). A prominent example is the work by [Malmendier and Nagel \(2011\)](#) which shows that CEOs who grew up during times of economic depression display less risk-averse strategies later in life as CEOs. In a similar vein, early life events or traits can be linked to future risk taking in firms. [Bernile, Bhagwat, and Rau \(2017\)](#) find, for a sample of US CEOs, that CEOs who experienced natural disasters in their hometown during their formative years as children, became CEOs later in life of firms that are more risk-averse (see also [Malmendier & Nagel, 2011](#)). Note that most of these studies do *not* focus on specific individual characteristic of CEOs that pertain to their role or time as actual leaders, let alone their actual behaviors as CEOs. Psychologists have been more active in this area, see for instance [Reitan and Stenberg \(2019\)](#), who look at traits that predict leader emergence in the military.

To summarize, the examples discussed in this section have in common that the conceptualization of leadership (what is it, and why it would matter) is largely ignored. Instead, the focus is on testing causal relationships between a leader (which often is only identified by a dummy variable) to some objective outcome variable. If the leader herself is analyzed at all, the analysis focuses on very specific (exogenous) leader traits and characteristics. Clearly, the specific ways in which leaders behave or the reflection of these acts by followers are not taken into account, mainly because of two reasons. First, it is difficult to collect objective data on a wide variety of rich behaviors, whereas traits like gender and age, which can be used to identify early-life experiences, are more easily measured. Moreover, causality becomes a much more complicated matter if one really wants to suggest that the relationship flows from leader behaviors, via complex social relationships with followers to organizational performance. Finally, a specificity of this line of research is that various contextual characteristics, in terms of type of the organization, sector, internal or external changes et cetera, are mostly used as control variables and are not the focal point of the analyses (for an exception, see [Bastardo, Monney, Tur, & Antonakis, 2018](#) who exploit the exogenous variation of a terrorist attack on a country president's signaling of charisma and approval rating).

2.3. Context and the organization-centric view

In an adjacent field to economics—specifically, in the strategic management literature—the idea that leaders and notably CEOs matter for firm performance has led to a vast amount of research that tries to

link an array of CEO and firm or external environment characteristics to firm performance. The main theory in use is upper echelons theory ([Hambrick & Mason, 1984](#)). This theory stipulates that CEOs and also the firm's top management team (TMT) have leverage on firm performance via their strategic decision making. Including a range of firm-specific (e.g., firm size and age, ownership) and external (e.g., the sector) variables, the aim is to show how various biographical and objective CEO (and TMT) characteristics have predictive power for firm performance, given the context. In doing so, the “existence” of the CEO and the TMT are typically taken as given. It is only very recently that more subjective measures or features of the leader (i.e., the CEO) are included in the analysis (see e.g. [Harrison, Thurgood, Boivie, & Pfarrer, 2019](#)).

Interestingly, this line of research has now come down to the observation that—echoing [Bertrand and Schoar \(2003\)](#)—the variation in firm performance is to a considerable extent due to unobserved between-firm variation in the CEO. So, several firm-specific characteristics matter, but there is a large unexplained residual that [Quigley and Hambrick \(2015\)](#) have dubbed the “CEO-effect.” This conclusion does, of course, raise the question: what drives this effect? It is maybe for this reason that [Hambrick \(2019\)](#), the founding father of upper echelons theory and the ensuing empirical research agenda, in a keynote address on the future of strategic management research has called “leadership” the missing variable that would be needed to open the black box of this CEO-effect (see also [Neely, Lovelace, Cowen, & Hiller, 2020](#)).

The empirical research based on upper echelons theory has much to offer to leadership scholars. It puts a leader (the CEO) at the heart of the analysis and then takes the firm or organizational context in which the CEO (or the TMT) has to function as an integral part of the analysis about leader/CEO effectiveness. Given that several leadership scholars are themselves critical about the limitations of the micro-centered level of their analysis (where leadership analysis typically does not extend beyond the team level and is devoid of organizational context), the empirical research by strategic management scholars offers a source of inspiration (see [Hiller, Piccolo & Zaccaro, this issue](#)). Now that strategic management research on leadership has started to take the person (ality) of the leader more seriously, by analyzing issues like leader narcissism or over-confidence ([Chatterjee & Hambrick, 2007](#); [Chen, Crossland, & Luo, 2014](#)), there seems to be a growing common ground between strategic management and leadership research.

Although the strategic management literature on CEO leadership and its impact on firm performance could serve well as a source of inspiration for leadership scholars, this literature is less useful when it comes to the conceptualization of leadership and causality issues. In terms of conceptualization, strategic management studies often take the existence of CEOs (and TMTs) for granted. When it comes to causality, this literature pays far less attention to the endogeneity issues that we noted in the previous sub-section on the role of causality in economics.

In economics, explanations for firm performance have traditionally been based on differences in external conditions or situations confronting the firm—for example, the classic distinction in introductory economics highlights differences in “market power” between a firm in a perfectly competitive industry and a monopolist. Moreover, these differences relate to macro- and meso-levels, and thus not to the internal organization of the firm. However, this focus notably has changed over the last decade, largely thanks to the research by [Bloom, Lemos, Sadun, Scur, and Van Reenen \(2014\)](#). The focal point of this line of research is that the quality of firm management practices is seen as a key explanation for variation in firm performance. The basic idea ([Bloom, Sadun, & Van Reenen, 2016](#)) is that just like a stock of physical capital, a firm also has a stock of managerial capital in the guise of the knowledge and training of its managers and their practices with respect to operations-, strategic and HR-management. [Bloom et al. \(2014\)](#) show for both private (manufacturing, retail) and public (schools, hospitals) organizations, and for a wide range of countries, how the variation in

management practices is associated with variation in organizational performance.

Moreover, Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013) and Bloom, Mahajan, McKenzie, and Roberts (2018) also address the issue of causality, since merely documenting a positive association between management practices and performance will obviously not do. Specifically, they (2013, 2018) use a carefully controlled experiment with Indian textile firms in Mumbai in which the treatment varies exposure to better management practices. Applying what is in essence a difference-in-difference approach to compare the treatment group with a control group of firms before and after the intervention (and by revisiting firms five years after the intervention was conducted), this research offers insights into how causality might actually run from management practices and the “treated” individual managers to firm performance (see also Bruhn, Karlan, & Schoar, 2018).

In line with the strategic management literature, the relevance of this so called “new empirical management” literature for leadership research is that it provides a handle on how to deal with contextual variables on organizational and external levels more generally. This literature admittedly does not deal with (the conceptualization of) individual leaders (hip) yet, but this is on the cards as the next step. For example, Bloom et al. (2019) conclude that the between-firm as well as, crucially, the substantial *within*-firm variation in management practices can only be explained by (indirectly) acknowledging heterogeneity in the quality of individual managers or leaders. Also, a recent paper by Bandiera, Hansen, Prat, and Sadun (2017) employs machine learning to classify the actions and behaviors of approximately 1100 CEOs from six countries as measured by their calendars and then investigates how much of the variation in firm performance is due to variation in individual CEO behavior. It turns out that 30% of this variation is due to differences in types of CEO behavior (Bandiera et al., 2017). Leadership scholars might object that this measure is still far from an adequate representation of actual leadership behaviors, but this paper at least provides an example of where the organizational or firm level, and hence the context, meets the individual leader level.

The positive association between management practices and firm performance as summarized by Bloom et al. (2014) offers a potential model to include individual leadership as a next step, see above. Indeed, a very promising attempt to combine the organization- and leader-centric approaches in economics in a single unified framework is provided in a recent model by Dessein and Prat (2019). This model has the advantage of combining conceptualization of the leader by individual characteristics and traits, and context—in the form of the firm's environment, which can determine the optimal characteristics of the firm's leadership. It also yields a range of testable implications about the relevance of CEO leadership and management for firm performance, including interactions between the firm's environment and its leadership for determining performance. This approach offers the potential to generate models that potentially close the gap between economics and leadership research, combining contingency rich settings with individual leadership.

A further promising approach to study leadership integrates economics with social psychology or sociology to better understand how leaders influence followers. An example of this approach is the work on identity economics (Akerlof & Kranton, 2010). Here, individual economic behavior in general is seen as best understood by acknowledging that social identity is a pivotal driver of individual actions. In this line of reasoning, Steffens, Haslam, Peters and Quiggin (this issue) use the example of elevated CEO pay for their empirical study on how identity economics meets identity leadership. Another example is the incorporation of social norms as a potentially powerful influence on behavior into economic research (Breza, Kaur, & Krishnaswamy, 2019; Krupka & Weber, 2013), including the possibility that shared norms between leaders and followers may matter for organizational performance (Burks & Krupka, 2012).

2.4. Critiques of economic perspectives on leadership

It is of course beyond the goal of the introduction to this special issue to offer a full-fledged assessment of the economics (and strategic management) view on leadership. As we have argued above, we see three main topics where leadership scholars could or should take economics more seriously: conceptualization, causality, and context. But clearly, economic approaches to leadership also come with a few notable downsides.

First of all, linking individual leaders—be it CEOs or political leaders—and their traits or characteristics, to organizational or even national performance is a huge step that crosses multiple levels (Liu, Fisher, & Chen, 2018). Although this research can offer important policy implications, to better understand leadership we must also understand the precise processes or mechanisms through which a leader has leverage on higher-level outcomes. This question is largely left unanswered in these fields, which confirms our earlier observation that the economic approach to leadership—particularly once it leaves the lab and turns to actual organizations—still largely ignores the behaviors of leaders or other psychological processes.

Second, and notwithstanding the fact that the context or contingencies are directly or indirectly part of at least some economic approaches to leadership, the theoretical foundation is often meagre or lacking. The focus is very much an empirical one when context enters the analysis, and a proper (and much-needed) multi-level theoretical perspective is often not provided – for a valid exception see Staw, Sandelands, and Dutton (1981) on threat-rigidity.

A third critique of the empirical approaches to leadership in economics is that they are strongly leader-centric. More work on the role of and interaction with followers is warranted (see Ahmad & Loch, 2019 for an attempt in this direction).

A fourth critique is that the economics literature on leadership tends to emphasize the performance of leaders, whereas it has much less to say on the dynamics of leader emergence, selection or dismissal, which are central topics in leadership research (although, for exceptions on leader emergence see Besley, Folke, Persson, & Rickne, 2017; Born, Raneyhill, & Sandberg, 2019). Interestingly, the question of selection has recently led to some research that incorporates methods not only from economics but also from social psychology and political science. This literature shows how under imperfect information, business and political leaders are selected using various informational cues that are at best indirectly but at worst totally unrelated to the actual competencies of leaders and hence to their performance. In particular the idea that leaders that somehow look “good” are more prone to be selected has been tested extensively by now (for an overview see Todorov, 2017), even when set against economic or other motives for leader selection (Garretsen, Stoker, Alessie, & Lammers, 2014; Graham, Campbell, & Puri, 2016). Irrespective of the relevance of attractiveness of leaders for selection, research also clearly shows that attractiveness is not linked with leaders' performance (Antonakis & Eubanks, 2017; Stoker, Garretsen, & Spreuwerts, 2016; Todorov, Olivola, Dotsch, & Mende-Siedlecki, 2015; see also the special issue of *The Leadership Quarterly* on facial appearance of leaders, Poutvaara, 2014). These findings offer interesting avenues for future cross-disciplinary research.

Finally, as we explained before, the economic view of leadership is a rather functional one. Leadership is consequently often equaled to a transaction, where leaders' main instrument to exert influence on their followers is by providing (financial) extrinsic incentives to act. This assumption clearly cannot explain how more transformational or charismatic leadership features can also lead to positive outcomes, without using incentives (Antonakis et al., 2019; Hermalin, 1998), let alone how inspiration or charisma could lead people to perform “beyond expectations”, which was the key starting point and claim of Bass's concept of transformational leadership (Bass, 1985). Moreover, this functional view also concerns the dependent variable. There is a strong focus on (organizational) performance as the main or even single

target outcome of leadership, whereas it can be debated if performance really is always the main desirable result of leadership; a point which is also been taken up in economics via the work on happiness (Layard, 2011).

Against the background of the economic view on leadership, and also given the fact that there thus still remains quite a gap between the economic and leadership approach, we will now turn to the six papers in this special issue, that go at least some way in fusing or linking the strengths of the economic approach to leadership (as summarized by our 3C's) to leadership research.

3. Advancing conceptualization, causality and context in leadership research: the papers in this issue

As we argued above, the field of economics has made valuable contributions to the field of leadership in conceptualizing leadership, dealing with issues of causality, and integrating the meso- and macro context into the predominantly micro- or individual level of analysis that characterizes mainstream leadership research. This section will describe what these contributions look like in this special issue.

3.1. Conceptualizing leadership

One of the key economists studying leadership for over almost a decade is Michael Kosfeld. As he summarizes in his paper (Kosfeld, 2019), the central question in his work has been to analyze the role of leaders in human cooperation (Kosfeld & Rustagi, 2015; Kosfeld & Von Siemens, 2009; Kosfeld & Von Siemens, 2011; Rustagi, Engel, & Kosfeld, 2010). Clearly, this line of research takes a functional perspective on leadership: the leader is a facilitator of cooperation between group members. The starting point in Kosfeld's (this issue) work is the assumption that motivations of individuals to cooperate with others are heterogeneous. Some individuals primarily follow their own interest (the so-called non-motivated types or free-riders), whereas others are willing to cooperate voluntarily (the motivated). However, the latter group members typically only cooperate if others cooperate as well, so they are best described as *conditional cooperators* – the “CC type”.

Such a behavioral heterogeneity has strong implications for the behavior of leaders. Contrary to most leadership studies in economics, Kosfeld (this issue) argues and shows that decision-making should not always be guided by material incentives or punishments (Zehnder et al., 2017). Instead, starting from a game-theoretical framework, in a range of both lab and field studies Kosfeld and colleagues convincingly show how leaders should especially adjust their behaviors to positively direct the behaviors of the CC-type group members into cooperation, which is labeled the “CC-strategy”. This strategy consists of three elements: trust (to not demotivate the motivated), punishment (to motivate the non-motivated), and the attraction of as many CC types as possible.

But just as group members have their preferences, so do leaders themselves, especially when it comes to punishment. Kosfeld (this issue) distinguishes three types of preferences for punishment, and shows that these leader-types are associated differently with group cooperation outcomes. Leaders who reveal an antisocial motive to punish are leaders of groups who perform poorly, whereas leaders who punish inequality or inefficiency lead groups with significantly higher performance. Kosfeld's paper not only documents nicely how analytical and empirical methods can be borrowed from economics to study leadership, but more importantly, in line with assumptions from contingency models in leadership, it shows that different group member types need to be treated differently by their leaders.

In a similar vein, the paper by Boulou-Reshef, Holt, Rodgers & Thomas-Hunt (this issue) also takes a functional perspective on the role of leaders for solving cooperation problems by mitigating free-riding behavior. It studies this question using a paradigm regularly employed in experimental game theory—a laboratory public goods game with voluntary contributions (Chaudhuri, 2011; Ledyard, 1995). Like

Kosfeld, Boulou-Reshef et al. (this issue) investigate how leaders can get group members to contribute more in such games. But they take their research one step closer to the field of leadership than Kosfeld (this issue) does, because they specifically study leadership *behaviors*. Instead of investigating the classic economic options for a leader in such a game (such as the possibility to punish or reward), by building on leadership literature (Lorinkova & Pearsall, 2013; Martin, Liao, & Campbell, 2013; Sims Jr., Faraj, & Yun, 2009) they study how empowering and directive messages from leaders influence contributions of followers to a public good. Moreover, they introduce a well-known mechanism from economic game theory into their experiment, that is the possibility for leaders to personally also communicate (“chat”) with their followers. Their results clearly show that in situations where leaders cannot personally interact with their followers, leaders who use an empowering style are more effective in mitigating free-riding than do leaders who use directive messages. However once leaders are able to interact—that is, to chat with their followers—the differences between the effect of empowering and directive leadership disappear, which suggests that interpersonal contact can help directive leaders be more effective in mitigating free-riding.

Despite this richer conceptualization of leadership in Boulou-Reshef et al.'s study (this issue), however, it is worth noting that leadership researchers will still likely find that the kind of leadership which Boulou-Reshef et al. study, is rather rudimentary for their tastes. Leadership here consists of sending only simple messages or engaging in brief electronic communication. Followers do not see the leaders or any actual actions by the leaders, and some of the experimental conditions involve no possibility of communication between followers and leaders. Moreover, the only thing that followers do—and that leaders can therefore influence—is select a number, which serves as an abstract representation of some kind of costly contribution to the group. These features reflect standard practices of laboratory experimentation in economics, which typically take as a starting point very simple and abstract representations of complex situations and add the minimal elements necessary, one at a time, to understand their influence on outcomes. This approach of studying a stripped-down version of reality thus has many advantages in identifying precise effects and mechanisms free from confounding effects that would be prevalent in the field. But, from the perspective of studying “real” leadership, it will be far too sparse for many readers of *The Leadership Quarterly*. Nevertheless, we consider highly valuable the opportunity to further develop leadership research that begins from such stark settings and adds richer elements of leadership.

Both the paper by Kosfeld and Boulou-Reshef et al.'s paper showcase how the field of economics conceptualizes leadership in a rigorous and non-ambiguous manner. First, and contrary to concepts like transformational leadership or charismatic leadership, leadership is defined not by its outcomes (see also Antonakis et al., 2016; Van Knippenberg & Sitkin, 2013), but by its function with respect to followers. Second, the papers contribute to leadership research by demonstrating how incentivized experimental designs can be used to empirically study actual behavioral outcomes of leadership, instead of perceptual dependent variables.

3.2. Causality

Two papers in this issue offer strong illustrations of how causality issues in leadership studies can be addressed. First, the paper by Delfgaauw et al. (2020) presents an incentivized field-experiment. The authors study causal effects of team incentives on task assignment and performance, to test whether the possibility to use such incentives induces teams or their managers to reallocate tasks in a performance-enhancing way. Because of the focus on incentives and contract design, their experimental set-up can be placed in the tradition of organizational economics. Additionally, by hypothesizing about the way in which leaders assign tasks to employees in such a context, they build on

literature from the leadership field.

In a set-up with random subsets of 108 stores of a Dutch retail chain, the study (this issue) shows no significant effect of team incentives on performance, and also no changes in task allocation to enhance performance. Apparently, in the experimental condition the leaders did not allocate tasks differently than in the control groups in order to achieve the best possible performance outcomes. The authors therefore conclude that “business continued as usual” despite the treatment. They give several possible explanations for their null-effects, such as the fact that the bonus offered may have been too low. Additionally, they propose that the results might be caused by the fact that the employees concerned had a lack of skills to identify and act on sales opportunities.

The paper by Rizio and Skali (this issue) is a perfect example of a macro-economic perspective on the relationship between national leaders and economic growth. Following in the footsteps of the Jones and Olken (2005) paper that we discussed in Section 2.2, Rizio and Skali (2020) investigate for a panel of 133 countries whether well-intentioned dictators can be identified as drivers of economic growth. They test the so-called “benevolent autocrat hypothesis,” which argues that autocratic leaders can be good for the economy and the society when they are benevolent. The authors use a large data set on economic growth, political regimes and political leaders over the 1858–2010 period. Moreover, because it might be possible that there is reverse causality from economic conditions to political transitions, the authors apply a regression discontinuity design.

Their results clearly do not support the benevolent autocrat hypothesis. First, Rizio and Skali (2020) show that the “growth-positive” autocrats—leaders whose countries experience larger than average economic growth—are present as often as would be predicted based on chance alone, and also that the “growth-negative” autocrats are found significantly more often than what would have been expected by chance. Second, based on the results from the regression discontinuity design estimations, the authors show that the occasional growth-positive autocrats are basically merely lucky, because they find themselves at the right place at the right time, simply riding the wave of pre-existing growth.

Although the papers in this issue by Delfgaauw et al. and by Rizio and Skali provide limited insights on actual leader behaviors, both papers are great examples for leadership researchers because of their research designs. Their main contributions lie in the ways in which both studies maximize causal inference. Delfgaauw et al. do so by implementing an incentivized and randomized field-experiment; Rizio and Skali show how to properly work with panel-data sets using 8431 observations from 133 countries, including the application of regression discontinuity designs and several robustness analyses.

3.3. Context

Our special issue also includes two papers that address the context of leadership. More specifically, the paper by Hiller, Piccolo and Zaccaro (this issue) offers an innovative framework on how to conceptualize the economic context of leadership. Being prominent leadership scholars themselves, the authors discuss some general assumptions about leadership from the economics literature. They show how these insights can add to core topics and trends in mainstream leadership research. But more importantly, they investigate the relevance of the economic context and argue that improving the incorporation of economic contextual factors has strong implications for leadership selection, behavior, (performance) outcomes, and evaluation. In their view, taking the economic context as a starting point changes several aspects about how mainstream leadership literature considers the nature of leadership.

Building on the concepts of the omnibus and the discrete context by Johns (2006), they give some useful examples to support this position. They explain how the omnibus context can be divided into a macro (macroeconomic, national-level factors such as GDP growth,

unemployment levels), meso (industry level factors such as competition, dynamism, market structure) and local context (organization-level factors such as recent firm performance or a firm's current access to resources), including a selective review of relevant literature for each contextual level. Moreover, they apply economic assumptions and context to come up with new questions in three areas of current leadership research, namely shared leadership, value-based leadership, and leader's discretion.

The paper by Steffens, Haslam, Peters, and Quiggin (2020) takes up the challenge laid out by Hiller et al. (2020), to study the impact of an economic factor in the local context, namely CEO pay. In their paper, identity economics meets identity leadership by studying the implications of pay beyond the individual leader, in this case the CEO. Interestingly, they confront the more traditional economic approach to the effects of high pay with identity-based approaches, both in economics (Akerlof & Kranton, 2005; Akerlof & Kranton, 2010) and leadership (Haslam, Reicher, & Platow, 2011; Steffens et al., 2014).

The traditional line of reasoning at present e.g. in principal-agent theory (recall our discussion in Section 2.1), would argue that high levels of CEO performance pay might be a useful tool that can induce the CEO to effectively motivate her employees to work hard to achieve performance goals. In contrast, based on social identity theory (Tajfel & Turner, 1979) the exact opposite expectation is that high CEO pay can prevent a shared identity between leaders and followers, and consequently would have negative performance effects. By studying the effect of a high discrepancy between CEO compensation and the compensation of other organizational members, the paper by Steffens et al. (this issue) is among the first to empirically align ideas from identity economics with those from identity leadership literatures.

In an experimental and in a field study, the relationship between CEO pay and identification with the CEO is examined (Steffens et al., 2020). The authors investigate whether personal identification with the CEO has a relationship with perceptions of that CEO's identity leadership and charisma. In line with principles of identity economics and identity leadership, results show that individuals indeed identify less with a high-paid CEO compared to a low-paid CEO. Moreover, the lack of identification reduces a CEO's perceived identity leadership and charisma, supporting identity-based approaches.

The papers in our issue by Hiller et al. and Steffens et al. clearly complement each other. The conceptual work by Hiller et al. strongly argues for not only using or testing assumptions from economics in leadership research, but also for more carefully accounting for the economic context. The paper by Steffens et al. empirically does both. These authors confront assumptions from mainstream economics on the one hand with identity-approaches within economics and leadership on the other, by testing the effect of a highly relevant and topical economic contextual variable: CEO pay. In combination, the two papers provide evidence of the importance of (economic) context.

4. Conclusions and future research directions

In this introduction, we have tried to provide a bird's eye view of various economic perspectives on leadership. We further described what economics may have to offer to leadership research, while also keeping in mind the limitations of an economic approach to leadership. Just as one has to be careful not to caricature or oversimplify the field of leadership research, the same goes the field of economics. Just as much as leadership scholars do not like to be reduced to “people who study leadership behaviors or attitudes”, it is also counter-productive to look upon “economics” as a field that is only focused on markets, prices and extrinsic incentives or enamored with the idea of an all-knowing, rational *homo economicus*. Economics has developed as a field in a way that has brought the topic of leadership to the heart of modern empirical and theoretical analysis. The latter has in no small part been due to the rise and development of behavioral economics (Thaler, 2015), which has also stimulated the study of actual behavior at the individual

level. In addition, economic research no longer relies as strongly on utility or profit maximization as witnessed by the research in economics on topics like happiness, well-being, identity, and job satisfaction (e.g. Akerlof & Kranton, 2010; Layard, 2011).

In our view, the common ground between economics and leadership scholars has increased significantly in recent years. This confluence of disciplines is not only made possible by the ascent of behavioral approaches in economics, it is also stimulated by the fact that leadership scholars increasingly seem to acknowledge that for their field to move forward insights and methods from economics are required (Antonakis, 2017; Sieweke & Santoni, 2020). This potential for inter- or cross-disciplinary cooperation is mirrored by empirical findings in both economics and strategic management, as documented in Section 2 of this introduction. These findings illustrate that in order to get a grip on the substantial between- as well as within-organizational variation in performance, one has to confront the fact that this variation, *ceteris paribus*, is due to variation in effectiveness or quality of individual managers or leaders, which necessitates a better understanding of leadership.

Our goal is not for a self-contained special issue that showcases economic approaches to studying leadership, but instead that these perspectives and methods become more regular tools for leadership research. Indeed, three recently published papers in *The Leadership Quarterly* further illustrate how our three C's can be used to narrow the gap between economics and leadership. With respect to the *conceptualization* of leadership, the paper by Sloof and Von Siemens (2019) on the motivational effect of delegation demonstrates in an economic experiment how delegation can be an effective leadership behavior to motivate followers, measured by incentivized behavioral outcomes. The paper by Martin et al. (2019) gives a review and pragmatic recommendations on how to infer *causality* in leadership training research. Finally, the paper of Li and Patel (2019) on the relevance of functional and generalist experience for firm performance shows how this relationship can be investigated while taking *context* at multiple levels into account. We view these as valuable contributions of the kind that we advocate and hope that, in combination with the articles in this issue, this work represents a growing trend toward more research at the intersection of our fields.

When it comes to future research directions, it is tempting to use this opportunity to make very specific recommendations. However, such an attempt would go beyond the main message of our introduction, namely that leadership is a means to an end, and narrowing the gap between economics and leadership allows for much more precise and at the same time richer research questions on a large set of topics. The overarching question to be answered is: “how much of the variation in outcome variables operating at different levels of analysis can be attributed to variation in characteristics of leadership, while acknowledging that these outcome variables are also driven by potentially many other factors that are context-specific?” Whereas complex and broad, this question raises several potential channels through which economics can inform leadership research. In essence, narrowing the gap between economics and leadership is no more but also no less than just another step in advancing our knowledge about the age-old question that underlies the field of leadership research about effective leadership. Notably, in the very first issue of *The Leadership Quarterly* 30 years ago, Hunt, Boal, and Sorenson (1990) already argued for opening up the “black box” of (top management) leadership, linking the psychological view on leadership to strategic management. Our special issue shows that, by using methods, tools and insights from economics in the field of leadership, leadership research is uniquely and well placed to put the phenomenon of this notorious “black box” behind us in the next three decades.

Having laid out the potential for a fruitful research “meeting” of economics and leadership research, it is important to be realistic as well: large scale cooperation will not happen overnight. To single out just one but crucial stumbling block, whereas leadership research does

make heavy use of questionnaire data, in economics there is a general distrust of such data: “Yet despite easy availability, this is one data source that economists rarely use. In fact, the unwillingness to rely on such questions marks an important divide between economists and other social scientists” ((Bertrand & Mullainathan, 2001), page 67). At the same time, merely copying methods from economics is also not without problems. Within economics, the use of for instance quasi-natural experiments, randomized controls or instrumental variables is not without controversy. The main objection to be raised (see also the discussion in (Angrist & Pischke, 2010)) is that these “cool” methods are ultimately only a means to an end, and are not very useful without relevant research questions and underlying theories (Deaton, 2009; Heckman, 1997; Heckman & Urzua, 2010; Sieweke & Santoni, 2020). Or as one of the today's leading scholars in economics, Raj Chetty warned us in this respect (as quoted in Angrist & Pischke, 2010, page 24): “People think about the question less than the method”. Accordingly, we feel that the way forward for leadership research in this respect is not merely to only use “better” methods, because it includes the danger that we end up with great methods but uninteresting research questions (Deaton, 2019; Ravallion, 2018). Therefore, the greatest need is for collecting and combining better datasets, preferably at all relevant levels—that is, the individual, organizational, and external level—and then using the appropriate design and analysis method to get causal identified results.

To put some action to these words, the editorial policy of *The Leadership Quarterly* now not only includes a methods review process aimed at helping improve the validity of causal claims, but also includes as a prerequisite for each future empirical paper to be published that the method-section provides rich contextual information regarding the study, such as sector, time, country, level of management, economic situation, et cetera (see <https://www.elsevier.com/journals/the-leadership-quarterly/10489843/guide-for-authors#txt2003>). By doing so, and as a collective effort, this new policy supports the gradual build-up of a dataset where all leadership scholars can, at an aggregate level, compare findings. Such data would enable researchers to give greater prominence to contextual factors. And in the end, such better data also would allow for progress when it comes to the conceptualization of leadership and the inferences of causality (for a very similar plea, see Johns, 2018).

To conclude, we hope that this special issue will fulfil a role in bringing the fields of economics and leadership closer together. The timing seems right: methods and data are more available, and also there is more common ground in theorizing about leadership. As is showcased by our line-up of authors involved and by the research summarized in this introduction, scholars from both fields increasingly succeed in executing rigorous and relevant research that crosses the traditional boundaries between the fields. Surely, the best is yet to come!

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