Natural Experiments in Leadership Research: An Introduction, Review, and Guidelines

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Abstract

Endogeneity is a serious challenge for leadership research. To overcome the problem, researchers increasingly rely upon experimental designs, such as laboratory and field experiments. In this paper, we argue that natural experiments—in the form of standard natural experiments, instrumental variable, and regression discontinuity designs—offer additional opportunities to infer causal relationships. We conduct a systematic, cross-disciplinary review of 87 studies that leverage natural experimental designs to inquire into a leadership topic. We introduce the standard natural experiment, instrumental variable, and regression discontinuity design and use topic modelling to analyse which leadership topics have been investigated using natural experimental designs. Based on the review, we provide guidelines that we hope will assist scholars in discovering natural exogenous variations, selecting the most suitable form of natural experiment and by mobilizing appropriate statistical techniques and robustness checks. The paper is addressed to leadership and management scholars who aim to use natural experiments to infer causal relationships.

Keywords: causal inference; leadership; instrumental variable design; natural experiment; regression discontinuity design; topic moeling.

1 Introduction

The community of leadership scholars has started to take important steps to advance causal empirical research (Antonakis 2017; Antonakis et al., 2010; Banks et al. 2018). Along this line, Podsakoff and Podsakoff (2019) refer to a methodological turn towards 'experiments,' documented by the recent surge in the number of publications that adopt an experimental research design (e.g., Delfgaauw, Dur, & Souverijn, 2018; Slater, Turner, Evans, & Jones, 2018; Yeow & Martin, 2013). Expanding on this turn, Podsakoff and Podsakoff (2019) also provide a comprehensive introduction and guidelines regarding three types of experimental design: laboratory experiments; field experiments; and quasi-experiments.

Our review article aims to enrich the 'experimental toolbox' available to leadership scholars by emphasizing a fourth type of experiment, namely, *natural experiments*. The key feature of natural experiments is the presence of 'naturally' occurring events—such as new regulations and laws, natural disasters, or economic and political crises—which heterogeneously affect the units of a population (Dunning, 2012; Harrison & List, 2004; Robinson, McNulty, & Krasno, 2009).[1] Given that these events generate random or as-if random variations in the environment, natural experiments mimic the experimental ideal in which units (e.g., individuals, teams, organizations) are split into a treatment and a control group, *or*, alternatively, receive different levels of treatment. This setting enables causal inference even when the substantive relationship at hand is difficult to investigate in a laboratory setting and/or would require operating costly, impractical, or unethical field experiments. Typical examples are the impact of political leaders on the economic growth of a country (Jones & Olken, 2005) or the queen bee phenomenon (Arvate, Galilea & Todescat, 2018).

Although natural experiments are popular in economics (e.g., Angrist & Pischke, 2009) and the political sciences (e.g., Dunning, 2012), they have received less attention from management scholars (notable exceptions are Arvate et al., 2018; de Vries, 2012; Flammer & Bansal, 2017; Haack & Sieweke, 2018; Stoker, Garretsen, & Soudis, 2019). The present study aims to create momentum around causal empirical research on leadership through a *systematic*, *cross-disciplinary* review of 87 studies regarding the field of leadership (Dinh, Lord, Gardner, Meuser, Liden & Lin, 2014; Gardner, Lowe, Moss & Mahoney, 2010) and which use natural experiment designs. In so doing, we pursue three analytically distinct, yet interrelated goals. Firstly, we aim to understand how prior natural experiments map onto the space of leadership topics in terms of the total of 1,156 research articles published in *The Leadership Quarterly* between January 2000 and March 2019. Secondly, we introduce the different types of natural-experimental designs—i.e., the standard natural experiment, the instrumental variable design, and the regression discontinuity design[2]—using concrete examples. Thirdly, we provide guidelines to

[1] Natural experiments differ from other forms of experiments that involve observational data, such as quasi-experiments and field experiments. Specifically, in the context of quasi-experiments, units self-select into the treatment or control condition (Shadish, Cook, & Campbell, 2002). On the contrary, in natural experiments the assignment procedure is random or as-if random (Dunning, 2012). In other words, units in a natural experiment should not have: (i) information about the treatment; (ii) incentives and capacity to self-select into one of the experimental conditions. Also, natural experiments differ from field experiments, wherein scholars have control over the experimental manipulation (for a review of field experiments in the social sciences see (Baldassari & Abascal, 2017).

[2] We borrow this categorization from Dunning's (2012) book.

assist leadership scholars in 'discovering' relevant sources of natural experiments, identifying the most appropriate form of natural experiment to operate, and, finally, in analyzing the data that come from the diverse forms.

2 Methods

2.1 Retrieving Natural Experiment Studies in the Field of Leadership

This systematic review focuses on 87 studies that leverage a natural experiment design to inquire into phenomena or theoretical relationships regarding the field of leadership (e.g., Gardner et al. 2010; Dinh et al. 2014). We identified candidate studies via an electronic search conducted within Scopus. We searched for business and management, psychology, social sciences, or multidisciplinary journal articles with the keywords 'natural experiment,' 'regression discontinuity design,' or 'instrumental variable' in the title, abstract, *or* set of author's generated keywords.[3] For January 2000 - March 2019 —the timespan of our review—the search resulted in 6,917 unique items.[4]

The two authors independently went through each abstract and retained all the *empirical* studies that fulfilled two criteria: First, the work adopted at least one of the three forms of natural experiment, namely, 'standard natural experiments,' 'instrumental variables,' and 'regression discontinuity designs' (Dunning 2012). Second, the work addressed at least one leadership topic/theory included in Gardner and colleagues' review of the theoretical conversations that characterize *The Leadership Quarterly* journal.[5] Having completed the independent screening phase, we validated the coding decisions. We thus considered the full papers of the 87 studies that were temporarily filtered-in. Any disagreement about the theoretical focus of each individual study was reconciled through discussion and by evaluating the focal study against the conceptual categories and examples presented in Gardner and colleagues' (2010) review. This led to the exclusion of five studies. Finally, the results achieved via the Scopus database were complemented with a Google Scholar search combining the above-mentioned keywords with the term 'leadership.' This led to the inclusion of five additional studies. Hence, our review was based on a set of 87 published articles.[6]

Figure **??** illustrates the distribution of the studies with respect to the form of natural experiment (panel A) and the time period (panel B) involved. Standard natural experiments (N = 40) and instrumental variable designs (N = 41) are the most popular forms of natural experiments in leadership research, and, overall, their diffusion seems to grow over time. Regression discontinuity designs are the least popular form (N = 8) and their use in the field of leadership is scattered over the last ten years.

[3]The query we operated was as follows: TITLE-ABS-KEY ("natural experiment*") OR TITLE-ABS-KEY ("regression discontinuity design") OR TITLE-ABS-KEY ("instrumental variable*") AND (LIMIT-TO (DOCTYPE , "ar")). The outcome of the query—in the form article-level metadata—is available in the supplemental materials.

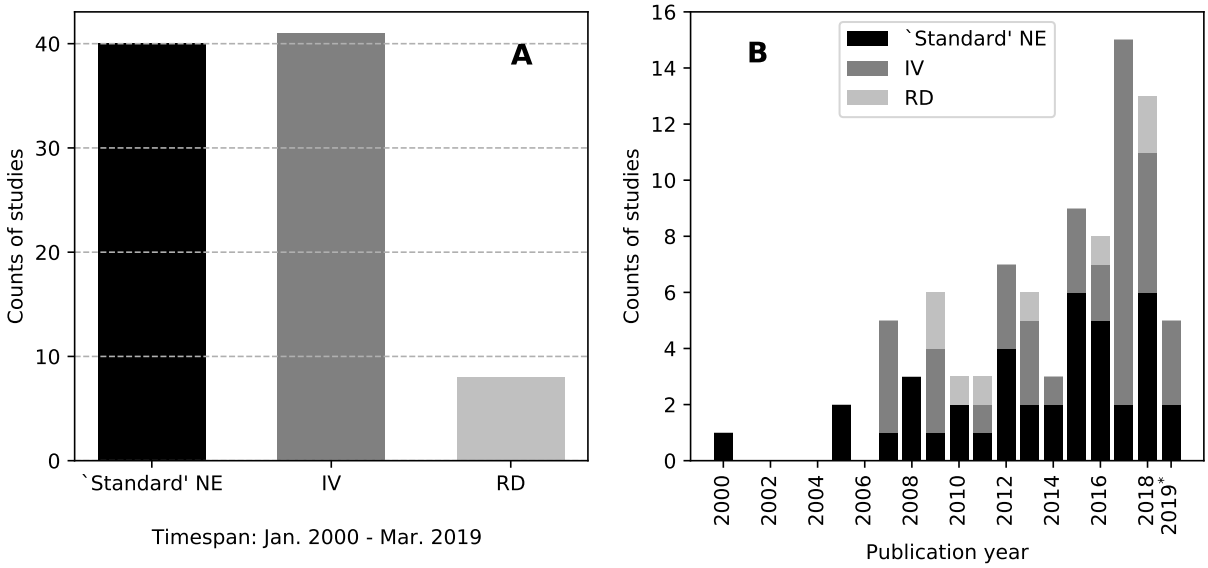
[4]Data were retrieved on April 5, 2019. Scientific publications may be affiliated with multiple subject areas. For example, an article published in *The Leadership Quarterly* is associated with the 'Business, Management and Accounting,' 'Psychology,' and 'Social Sciences' categories. Hence, conducting a separate, subject-by-subject search results in a higher number of publications than a search that concatenates the multiple subjects together.

[5]The supplemental materials report the categories associated with each individual study.

[6]The number of paper-research design instances we considered is higher than the number of studies, because two studies (Dal Bó et al. 2009; Dasgupta 2018) use two research designs.

Appendix A provides further descriptive elements regarding the set of studies.

Fig. 1: Counts of Retrieved Studies across Forms of Natural Experiment and Time



Notes.—'Standard NE' denotes the group of studies that use an average treatment on the treated approach; 'IV' denotes the group of studies using the instrumental variable design; 'RD' denotes the group of studies using a regression discontinuity design. Panel 'A' illustrates the group of studies across forms of NE experiments for the whole timespan; Panel 'B' accounts for the formation of the stock data reported in Panel 'A'. * '2019' data concern the first quarter of the year only. The number of individual studies is 87; Dal Bó et al. (2009) is included in both the IV and RD categories; Dasgupta (2018) is included both in the SNE and IV categories.

2.2 Characterizing the Natural Experiment Studies

As stated in the introduction, one of this paper's goals is to understand 'how' natural experiment methods intersect with substantive topics in the field of leadership. In order to do this, we used topic modeling—a text mining tool rooted in computational linguistics and natural language processing[7]—to assess how the studies retained map onto the topics dealt with in *The Leadership Quarterly*. In our case, a topic modeling approach has some advantages over manually coding papers. First, it lets the 'data speak' as the study-to-topic pairing is revealed inductively by analyzing the corpus of texts. Hence, the researcher does not need to subjectively assign a study to an established, theoretically derived topic. Second, topic modeling offers a nuanced characterization of the substantive focus of a study. Not only is the assignment of a document to a topic probabilistic, a document is also related to multiple topics. In other words, an article that investigates the firm level implications of gender diversity in top management teams may reflect both the 'strategic leadership' and 'gender diversity in groups' categories that Gardner and colleagues map.

In terms of design, our topic modeling involves two phases. In the first phase we trained a Latent Dirichlet Allocation (LDA) model (Blei, Ng & Jordan, 2003) on the abstracts of the 1,156 research

[7]For a non-technical introduction to the topic see Mohr and Bogdanov (2013).

TABLE I
Term-Topic Matrix

Topic Number	Terms as Lemmas									
1	context (0.042)	woman (0.03)	difference (0.024)	power (0.024)	role (0.02)	gender (0.017)	practice (0.017)	female (0.016)	culture (0.013)	position (0.012)
2	perception (0.034)	affect (0.033)	role (0.03)	emotion (0.028)	positive (0.028)	emotional (0.026)	negative (0.026)	network (0.017)	influence (0.016)	collective (0.016)
3	transformational (0.056)	subordinate (0.043)	rating (0.026)	trait (0.021)	associate (0.017)	experience (0.016)	significant (0.016)	personality (0.016)	high (0.014)	analysis (0.014)
4	development (0.046)	perspective (0.027)	develop (0.017)	political (0.015)	include (0.015)	purpose (0.013)	interest (0.013)	view (0.012)	multiple (0.012)	year (0.01)
5	employee (0.054)	work (0.048)	lmx (0.034)	job (0.023)	supervisor (0.023)	perceive (0.021)	authentic (0.019)	mediate (0.018)	hypothesis (0.016)	satisfaction (0.015)
6	understand (0.024)	effective (0.02)	vision (0.019)	problem (0.017)	cognitive (0.017)	strategy (0.015)	lead (0.014)	dynamic (0.013)	proposition (0.013)	identify (0.013)
7	performance (0.106)	team (0.085)	member (0.033)	ceo (0.03)	management (0.021)	skill (0.019)	firm (0.018)	decision (0.018)	share (0.017)	strategic (0.015)
8	level (0.029)	increase (0.024)	ethical (0.023)	develop (0.019)	impact (0.017)	moral (0.016)	structure (0.015)	reveal (0.014)	practice (0.013)	integrity (0.012)
9	charismatic (0.051)	change (0.041)	manager (0.035)	time (0.03)	charisma (0.025)	style (0.018)	content (0.014)	crisis (0.013)	type (0.012)	managerial (0.01)
10	group (0.073)	effectiveness (0.04)	task (0.03)	condition (0.023)	identity (0.022)	individual (0.021)	emergence (0.019)	show (0.017)	response (0.015)	characteristic (0.014)

Notes.— Estimations achieved with Mallet software and the Gensim library for Python; number of documents = 1,156; number of topics = 10; terms are arranged in descending order of likelihood to appear in topic i; the optimal number of topics to retain is based on the comparison and contrast of the coherence value of 29 competing models in the 1-29 topics range—see Appendix **??** for further details about the estimation procedure.

articles[8] published in the *The Leadership Quarterly* between January 2000 and March 2019. Table 1 illustrates key estimates obtained using Mallet (McCallum, 2002) and the Gensim (Řehůřek & Sojka, 2010) and spaCy (Honnibal & Montani, 2017) libraries for Python. Each cell in the table indicates the probability of a term w (e.g., 'CEO') of occurring in a topic τ (e.g., Topic 2). The analysis of the term-topic pairs included in the model reveals a series of substantive categories that seem consistent with Gardner and colleagues' review. The models emphasize topics such as 'female leadership' (Topic 1); 'emotions and leadership' (Topic 2); 'transformational leadership' (Topic 3) 'development of leadership' (Topic 4); 'dyadic relations' (Topic 5); 'cognition and leadership' (Topic 6); 'strategic leadership' (Topic 7); 'ethical leadership' (Topic 8); 'charismatic leadership' (Topic 9); 'leadership in team and decision groups' (Topic 10).

In the second phase, we used a folding-in strategy by 'projecting' each of the 87 natural experiments onto the trained LDA model. We thus represented each retained study in terms of the very same ten topics that represent the corpus of 1,156 abstracts published in *The Leadership Quarterly* between January 2000 and March 2019. This enabled us to characterize a natural experiment in terms of one or a few salient topics (i.e., those that occur with a higher likelihood in the document) or to allocate it as being leadership research. Appendix **??** provides further descriptive elements about the estimation procedure behind our LDA model. The supplemental materials contain the data and the Python code to reproduce the set of exhibits reported in the paper.

3 Literature Review

3.1 Standard Natural Experiments

The standard natural experimental design was used in the first-ever natural experiment, in which Snow (1855) analyzed the transmission of cholera in mid-19th century London. In its 'simplest' form, the standard natural experiment resembles the design of a randomized experiment in that there are two groups—the treatment and control group with a pre- and a post-treatment observation per group.[9]

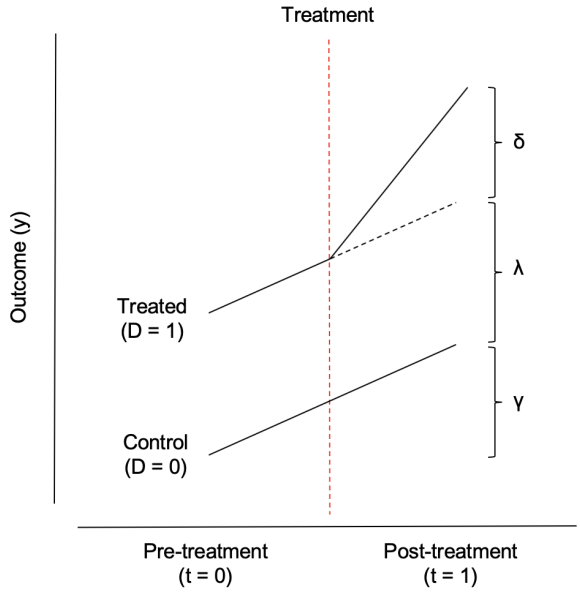
The standard natural experimental design was already used in the first-ever natural experiment, in which Snow (1855) analyzed the transmission of cholera in London. In its 'simplest' form, the standard natural experiment resembles the design of a randomized experiment in that there are two groups—the treatment and control group—with two observations per group—a pre- and a post-treatment observation.[10] As shown in Figure **??**, we can estimate the causal effect of the treatment by comparing the average change of the outcome variable y for the treated units ($\lambda + \delta$) and controls (γ). Of course, the 'simple' form of the standard natural experiment can be extended in several ways, such as by adding additional treatment groups or by adding additional time periods before and/or after the treatment (e.g.,

[8] Editorial notes were not used to train the model.

[9] The standard natural experiment is also often referred to as the "difference in difference (DID)" design; however, not all DID designs represent natural experiments, e.g., if the assignment is based on self-selection or unobserved covariates and not on an as-if randomization (Wing et al., 2018).

[10] The standard natural experiment is also often referred to as "difference in difference (DID)" design; yet, not all DID designs represent natural experiments, e.g., if the assignment is based on self-selection or unobserved covariates and not on an as-if randomization (Wing et al., 2018).

Fig. 2: Visual Representation of the Standard Natural Experiment



Notes.—The underlying population regression function is $y = \gamma t + \lambda D + \delta tD$, where γ, λ, and δ represent the systematic difference in the outcome across the treated and control cases, the trend effect and the difference in the outcome that is due to the treatment. For sake of clarity we represent the case in which $\delta > 0$.

Matsa and Miller, 2013).

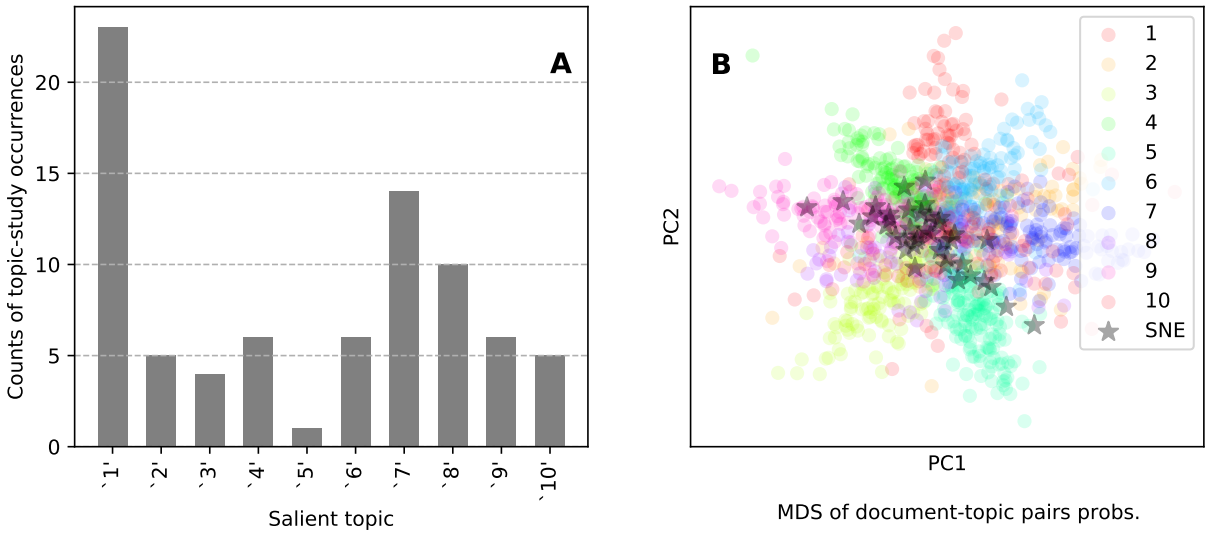
Whether the standard natural experiment provides a causal estimate mainly depends on the qualities of the treatment. In the case of random variations, such as lotteries, the assignment process needs to be truly random (e.g., Starr, 1997). In the case of as-if random variations, the assignment process needs to be independent of factors that are related to the outcome and not affected by the unit's self-selection into treatment or control conditions (Dunning, 2012). The second part of the document deals with these aspects in more detail.

3.1.1 Standard Natural Experiment and Leadership Research

Table **??** reports the set of studies that draw upon the standard natural experiment design to address a leadership-related topic. The left-hand column indicates the short reference for the study; the remaining columns provide a substantive characterization of the study. The right-hand columns present the inductive categorization of studies as emerging from the topic modeling described in the previous section. In the interest of clarity, we have just reported the two most salient topics of each study—i.e., the topics with the highest chance of being paired with the focal document.

Our topic model highlights the focus of the standard natural experiments and consists of three core topics—'female leadership' (Topic 1); 'strategic leadership' (Topic 7); and 'ethical leadership' (Topic 8)—together with a series of other themes that, although less central, still receive significant attention (see Topics 2, 3, 4, 6, 9, and 10). The prominence of Topics 1, 7 and 8 is clear from Figure **??**, Panel A,

Fig. 3: Standard Natural Experiments—Topic Characterization



Notes.— Panel A pictorially depicts the information reported in Table **??**; Panel B: Data points marked with a star denote natural experiments that are folded in the topic model trained on the 1,156 articles published in The Leadership Quarterly; Topic labels: Topic 1—'female leadership'; Topic 2—'emotions and leadership'; Topic 3—transformational leadership'; Topic 4—'development of leadership'; Topic 5—'(neo-)charismatic leadership'; Topic 6—'cognition and leadership'; Topic 7—'strategic leadership'; Topic 8—'ethical leadership'; Topic 9—'nature of managerial work'; Topic 10—'leadership in teams and decision group.' 'MDS' stands for multdimensional scaling; 'PC1' and 'PC2' refer to the components returned from the MDS analysis.

which shows the frequency with which salient topics appear in the documents.

The scatter plot in Panel B of Figure **??** expands on the outcome of the topic model by positioning each natural experiment in the topic space that characterizes the population of articles published in *The Leadership Quarterly*. The coordinates of each data point are produced via multidimensional scaling. This allows us to create a shallow representation of the 10-dimension space underlying the topic model. 'Stars' are associated with natural experiments. *The Leadership Quarterly* articles are represented with circles that have been color-coded to reflect the dominant topic of the document. The diagram highlights that: (i) standard natural experiments map onto a narrow portion of the space, whereas vast areas of leadership research have been barely or not impacted at all by this form of causal research; (ii) an initial cluster of studies emerges at the intersection of 'female leadership' and strategic leadership (see the bottom right of the chart); (iii) a second cluster of studies jointly investigate 'ethical leadership' and 'nature of managerial work' subjects (see the middle left of the chart).

3.1.2 Standard Natural Experiment Examples

Our review shows there is a significant number of studies using standard natural experiments to address selected leadership-related topics. We concentrate on three examples in order to provide leadership scholars with insights into the application of the standard natural experiment for inferring causal relationships.

The first study, by Beaman and colleagues (2012), uses a standard natural experiment to analyze

TABLE II
Standard Natural Experiments—Substantive Focus

	Salient topics			
	1^{st} topic		2^{nd} topic	
Study	Topic label	Prob.	Topic label	Prob.
Bae & Yi (2008)	Charismatic leadership	0.134	Ethical leadership	0.133
Beaman et al. (2012)	Transformational leadership	0.2	Female leadership	0.165
Belloc et al. (2016)	Cognition and leadership	0.143	Development of leadership	0.134
Bhavnani (2017)	Charismatic leadership	0.165	Female leadership	0.148
Breda & Ly (2015)	Female leadership	0.204	Emotions and leadership	0.148
Brockman et al. (2015)	Strategic leadership	0.332	Ethical leadership	0.102
Byrd et al. (2012)	Strategic leadership	0.174	Female leadership	0.165
Bækgaard (2011)	Leadership in teams	0.18	Female leadership	0.167
Chauchard (2014)	Female leadership	0.137	Leadership in teams	0.127
Chen et al. (2016)	Strategic leadership	0.181	Female leadership	0.171
Cheng et al. (2005)	Strategic leadership	0.173	Development of leadership	0.113
Cohen & Wang (2013)	Leadership in teams	0.139	Development of leadership	0.128
Coman (2018)	Development of leadership	0.149	Female leadership	0.131
Cox et al. (2000)	Strategic leadership	0.155	Leadership in teams	0.14
Cuñat & Guadalupe (2009)	Strategic leadership	0.194	Ethical leadership	0.159
Dahya & McConnell (2005)	Female leadership	0.227	Emotions and leadership	0.108
Dasgupta (2018)	Female leadership	0.123	Charismatic leadership	0.119
De & Scoppa (2015)	Female leadership	0.233	Leadership in teams	0.125
De et al. (2010)	Female leadership	0.322	Cognition and leadership	0.104
Gittell et al. (2008)	Dyadic relations	0.18	Emotions and leadership	0.13
Gormley et al. (2012)	Strategic leadership	0.202	Charismatic leadership	0.135
Guadalupe & Wulf (2010)	Strategic leadership	0.179	Ethical leadership	0.127
Han & Zhang (2018)	Female leadership	0.194	Strategic leadership	0.155
Hidalgo et al. (2016)	Female leadership	0.186	Emotions and leadership	0.126
Huber & Arceneaux (2007)	Charismatic leadership	0.214	Ethical leadership	0.136
Jayaraman & Milbourn (2015)	Strategic leadership	0.274	Emotions and leadership	0.109
Jiraporn & Lee (2018)	Female leadership	0.205	Development of leadership	0.113
Jiraporn et al. (2018)	Female leadership	0.19	Ethical leadership	0.128
Kahn et al. (2015)	Cognition and leadership	0.157	Female leadership	0.141
Laustsen & Petersen (2017)	Development of leadership	0.15	Cognition and leadership	0.128
Matsa et al. (2013)	Female leadership	0.224	Strategic leadership	0.177
Poulos (2019)	Female leadership	0.169	Ethical leadership	0.126
Rickman & Witt (2008)	Transformational leadership	0.128	Strategic leadership	0.122
Shea & Solis (2018)	Ethical leadership	0.127	Female leadership	0.12
Siming (2016)	Female leadership	0.161	Cognition and leadership	0.158
Tabvuma et al. (2014)	Charismatic leadership	0.138	Female leadership	0.128
Tosun (2016)	Strategic leadership	0.238	Ethical leadership	0.118
Valdini (2012)	Female leadership	0.271	Transformational leadership	0.115
Vo & Canil (2019)	Strategic leadership	0.217	Ethical leadership	0.134
Wyrwich (2015)	Transformational leadership	0.165	Cognition and leadership	0.154

whether female leadership has an impact on girls' career aspirations and educational attainment. The authors hypothesize that a female leader will act as a role model for girls and young women, and will thereby affect their career aspirations and educational attainment. The authors argue that analyzing this relationship in laboratory experiments is difficult, because participants are exposed to the role model for a short period of time, whereas in observational studies, people may self-select to certain role models based on observed and unobserved characteristics. Beaman and colleagues (2012) thus exploit the enactment of a law in India in 1993 that determined that in some randomly selected villages, the position of chief councilor was reserved for women. The law resulted into two treatment groups and a control group. The first treatment group consists of villages in which this position was reserved for women in one election (either 1998 or 2003); in the second treatment group, the position was reserved for women in two elections (in 1998 and 2003); and in the control group, the position was never reserved for women. The authors collected survey data from 15 randomly selected households in each village in 2006 and 2007. Their difference-in-means analyses show that the gender gap in parents' career aspirations for their children was much lower in villages in which the council positions had been reserved for women twice compared to villages in which the position had been reserved for women once or never. The analyses also indicate that the gap in educational aspirations between boys and girls was much lower in villages with female leaders than in villages with male leaders. Based on some additional analyses, the authors conclude that the effects are mainly caused by a role model effect; that is, female leaders provide a role model both for parents and for girls. Overall, the standard natural experiment by Beaman and colleagues (2012) provides important insights into the causal effect of female leadership on (female) followers' aspirations.

In the second study, Matsa and Miller (2013) exploit the introduction of 'gender quota' policies in Norway to investigate how female leadership influences strategic choices and outcomes, for example corporate downscaling. The gender quotas forced all publicly listed firms in Norway to increase the proportion of women on the board of directors to 40% within two years. Because the gender-quota policy applied to all listed firms, it was not a random variation in the regulatory environment. However, the authors argue that the policy targets companies that are part of a broader population of Scandinavian firms, that is, organizations facing relatively similar cultural and institutional factors. The Norwegian policy may therefore have an as-if-random interpretation. Matsa and Miller (2013) used a matching approach in which they first pair treated (i.e., publicly listed) and untreated (i.e., unlisted) Norwegian firms, and, in the second step, linked Norwegian firms to listed and unlisted firms located in Denmark, Finland, and Sweden. The summary statistics showed that the treatment and control group were similar in terms of most firm characteristics. To analyze the causal effect of female leadership on strategic choices and outcomes, the authors used the difference-in-differences and difference-in-difference-in-differences[11] analytical frameworks. They found that the gender quota had a negative impact on firm profitability. In further analyses, the authors showed that profit differentials were mainly attributable to the fact that companies treated with the quota policy tended to cut less jobs than their counterparts. To back up the causal interpretation of their results, Matsa and Miller (2013) conducted several robustness checks, such

[11] The difference-in-difference-in-differences framework is also referred to as the 'triple diff-in-diffs.'

as testing for trends before the introduction of the quota and testing whether the effects were stronger for firms with fewer women on their board of directors. Overall, the study provides causal empirical evidence supporting the effect of female leadership on firm performance as mediated by key strategic choices.

The third study, by Shea and Solis (2018), analyzes the relationship between leader tenure and countries' creditworthiness. The authors argue that higher leader tenure will reduce uncertainty in the sovereign credit market and will therefore increase a country's creditworthiness. Since leader tenure is endogenous (e.g., effective leaders tend to have a higher tenure), the authors backup their panel data analysis with a natural experiment in which leader tenure is exogenously determined. They focused on countries characterized by attempts to assassinate the political leader. While such events are not random, as confirmed by the authors' balance tests (see also the discussion in Jones and Olken, 2005), the outcome is as-if random. The authors provide anecdotal evidence for this claim (e.g., the successful assassination of President Kennedy versus the unsuccessful assassination of President Reagan) and excluded all assassinations in which the success was not determined by chance (e.g., *en coup d'etat*). Shea and Solis (2018) used a two-step approach in their analysis. First, they regressed sovereign bond yields on leader tenure, assassination success, and an interaction term. The interaction term was positive and significant, which indicated that assassination success had a stronger effect on bond yields at higher levels of leader tenure. Second, the authors accounted for a potential selection in the assassination sample (i.e., assassination attempts are likelier in poorer, non-democratic states) by applying the Heckman selection model, which supported the findings from the OLS. Overall, the study used an unusual exogenous variation of leader tenure to provide robust evidence that leader tenure influences a country's creditworthiness.

To sum up, the three examples of standard natural experiments explore important leadership topics (e.g., consequences of a leader's tenure and female leadership) and provide robust causal inference. The three studies use very different exogenous variations, such as laws or even assassination attempts, and focus on leadership in the contexts of villages, firms, and states. Thus, the three examples highlight both the potential and the variability of standard natural experiments for leadership research.

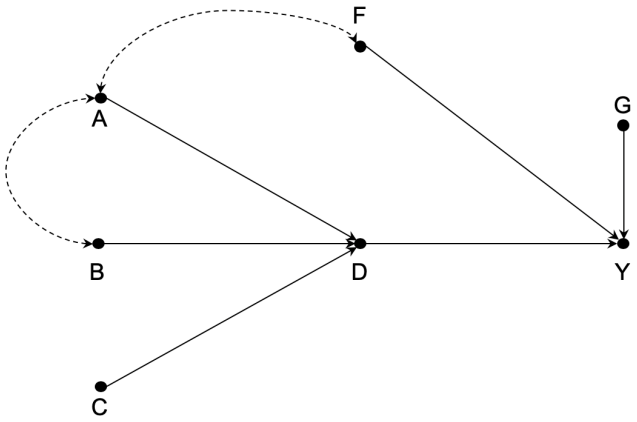
3.2 Instrumental Variable Designs

Instrumental variable (IV) designs have already received some attention in management research, as several researchers recommend their use to correct for endogeneity in the relationship between an independent variable and a dependent variable (e.g., Bascle, 2008; Semadeni et al., 2014). The basic idea of the IV design is shown in Figure **??**: The treatment variable D is influenced by covariates A, B, and F. Because at least one of the covariates is unobserved, we cannot directly estimate the causal effect of the treatment D on the outcome Y. The IV design 'solves' the endogeneity problem, which results from the omitted variable bias, by leveraging an instrument C to which subjects are (as-if) randomly assigned (Dunning, 2012).

A valid instrument needs to fulfill three conditions (Angrist & Pischke, 2009). First, it needs to

be exogenous, which means that it is uncorrelated with other causes of the dependent variable except for the treatment. Second, the instrument needs to influence the assignment of the treatment (i.e., it influences the probability of receiving the treatment). Third, the instrument has no relationship with the dependent variable except through the treatment. A violation of the conditions can lead to a severe bias in the estimates (Bound, Jaeger, & Baker, 1995; Semadeni et al., 2014). It is therefore important that researchers thoroughly scrutinize any candidate instrument and check whether and to what extent it fulfills these three conditions.

Fig. 4: Visual Representation of the Instrumental Variable Framework



Notes.— Continuous, oriented arrows denote the causal effect linking two variables; Dashed edges denote the presence of a common cause between two variables; C is an instrumental variable for D; A, B, and F are observables that influence D; G is a variable that affects the outcome but it is not causally related to D, so it does not affect the presumed causal path linking D to Y; Source: Morgan and Winship (2015, page 30).

3.2.1 Instrumental Variable Designs and Leadership Research

Table **??** reports the set of studies that draw upon the IV design. Our topic model—whose insights are summarized in Figure 5, Panel A—reveals that 'female leadership' (Topic 1) and 'strategic leadership' (Topic 7) tend to dominate the focus of attention of this group of studies. In fact, the core topics are even more core in this case than in standard natural experiments, whereas the number of documents that build on the remaining topics is relatively small. Panel B, showing the positioning of IV designs in terms of the articles published in *The Leadership Quarterly*, confirms that the data-points are concentrated in the bottom right of the chart.

3.2.2 Instrumental Variable Design Examples

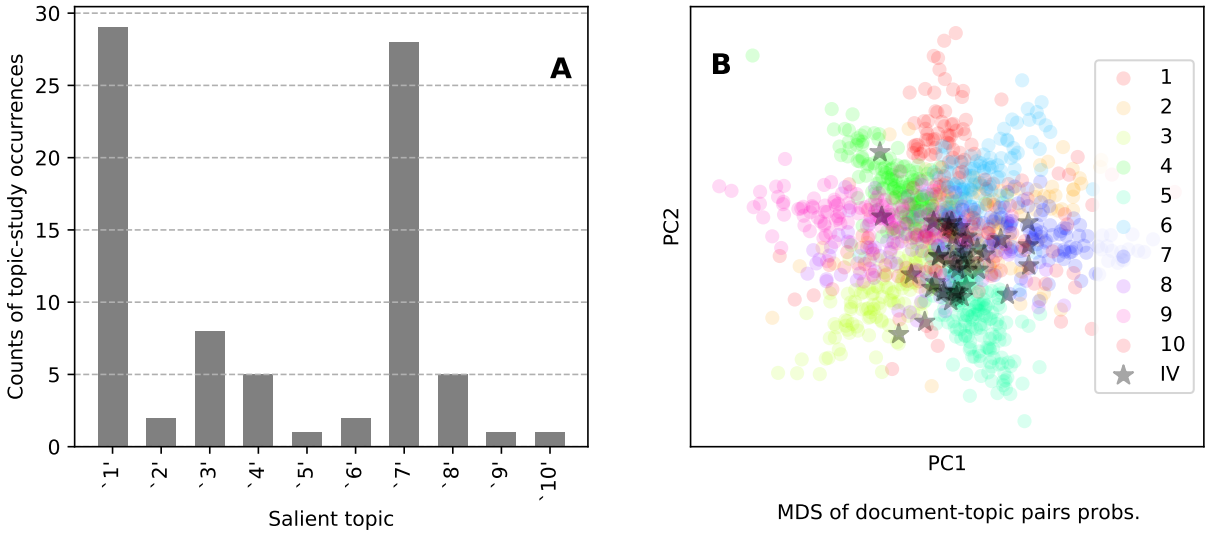
Our review shows that several studies in leadership research have applied the IV design, but only to address very few topics. In this section, we will focus on three example studies and explain their approach in more detail in order to provide leadership scholars with insights into how the IV design can be applied.

The first study was conducted by Bennedsen and colleagues (2007). It analyzed the relationship

TABLE III
Instrumental Variables Designs—Substantive Focus

	Salient topics			
	1^{st} topic		2^{nd} topic	
Study	Topic label	Prob.	Topic label	Prob.
Adams et al. (2009)	Strategic leadership	0.214	Female leadership	0.113
Adhikari (2018)	Female leadership	0.177	Strategic leadership	0.134
Adkins et al. (2007)	Strategic leadership	0.162	Female leadership	0.134
Aghion et al. (2013)	Strategic leadership	0.151	Female leadership	0.148
Akyol & Cohen (2013)	Female leadership	0.186	Strategic leadership	0.181
Amore et al. (2014)	Female leadership	0.181	Strategic leadership	0.142
Amore et al. (2017)	Strategic leadership	0.156	Transformational leadership	0.122
Arora (2018)	Female leadership	0.176	Strategic leadership	0.144
Artz et al. (2017)	Transformational leadership	0.185	Dyadic relations	0.163
Azoulay et al. (2017)	Transformational leadership	0.139	Development of leadership	0.135
Barros & Nunes (2007)	Strategic leadership	0.136	Ethical leadership	0.134
Bennedsen et al. (2007)	Strategic leadership	0.283	Transformational leadership	0.181
Bernile et al. (2018)	Female leadership	0.179	Ethical leadership	0.171
Chen et al. (2017)	Female leadership	0.226	Development of leadership	0.127
Chintrakarn et al. (2017)	Cognition and leadership	0.156	Strategic leadership	0.144
Conroy & Weiler (2016)	Strategic leadership	0.219	Female leadership	0.14
Conyon & He (2017)	Strategic leadership	0.198	Female leadership	0.191
Dal et al. (2009)	Female leadership	0.208	Development of leadership	0.179
Dasgupta (2018)	Female leadership	0.123	Charismatic leadership	0.119
Delis et al. (2017)	Female leadership	0.239	Strategic leadership	0.139
Driver & Guedes (2017)	Strategic leadership	0.155	Development of leadership	0.144
Frantz & Stein (2017)	Female leadership	0.151	Strategic leadership	0.138
Gabel & Scheve (2007)	Female leadership	0.141	Development of leadership	0.121
Harjoto & Rossi (2019)	Female leadership	0.15	Strategic leadership	0.124
Hearn & Filatotchev (2019)	Ethical leadership	0.156	Female leadership	0.128
Hooghiemstra et al. (2017)	Female leadership	0.141	Strategic leadership	0.132
Izgi & Akkaş (2012)	Strategic leadership	0.203	Female leadership	0.151
Khwaja (2009)	Strategic leadership	0.17	Female leadership	0.126
Kılıç & Kuzey (2016)	Female leadership	0.232	Strategic leadership	0.174
Li et al. (2018)	Strategic leadership	0.19	Transformational leadership	0.156
Lin et al. (2011)	Strategic leadership	0.189	Cognition and leadership	0.183
Markussen & Røed (2017)	Female leadership	0.196	Transformational leadership	0.148
Nicolosi & Yore (2015)	Strategic leadership	0.203	Female leadership	0.129
Pascal et al. (2017)	Strategic leadership	0.209	Female leadership	0.172
Rouse (2012)	Transformational leadership	0.167	Strategic leadership	0.128
Sabatier (2015)	Female leadership	0.165	Ethical leadership	0.138
Shue et al. (2017)	Strategic leadership	0.172	Emotions and leadership	0.167
Sun & Hovey (2013)	Strategic leadership	0.273	Female leadership	0.115
Yang et al. (2019)	Female leadership	0.186	Emotions and leadership	0.144
Wu (2015)	Female leadership	0.175	Ethical leadership	0.124
de Vries (2012)	Transformational leadership	0.234	Leadership in teams	0.219

Fig. 5: Instrumental Variable Designs—Topic Characterization



Notes.— Panel A pictorially depicts the information reported in Table **??**; Panel B: Data points marked with a star denote natural experiments that are folded in the topic model trained on the 1,156 articles published in The Leadership Quarterly; Topic labels: Topic 1—'female leadership'; Topic 2—'emotions and leadership'; Topic 3—transformational leadership'; Topic 4—'development of leadership'; Topic 5—'(neo-)charismatic leadership'; Topic 6—'cognition and leadership'; Topic 7—'strategic leadership'; Topic 8—'ethical leadership'; Topic 9—'nature of managerial work'; Topic 10—'leadership in teams and decision groups.' 'MDS' stands for multdimensional scaling; 'PC1' and 'PC2' refer to the components returned from the MDS analysis.

between CEO succession decisions, particularly the decision of family firms to hire a family or an external CEO, and firm performance. Testing the causal effect of CEO succession decisions on firm performance is difficult, because family members have in-depth knowledge regarding the characteristics of other family members (e.g., human capital), which will probably affect their decision to hire an external candidate. To infer a causal relationship, the authors use the gender of the departing family CEO's firstborn child as an instrument. They provide evidence that (i) the instrument is exogenous, because gender is randomly assigned; (ii) the instrument is relevant, because in the case of a family transition, it is about 10% higher when the firstborn child is male; and (iii) the instrument is unlikely to affect firm performance through other channels than CEO succession decisions, because a first child's gender is not related to firm-level attributes (e.g., age, size, and profitability). The authors conducted supplemental statistical analyses (e.g., using CEO deaths as an alternative instrument; ruling out changes in governance structure as alternative explanations) to back up their finding that appointing a family CEO leads to a decline of ca. 4% in firm profitability. Overall, the study adopts a creative instrument to estimate the causal impact of hiring professional managers on firm-level outcomes.

In the second study, Yang et al. (2019) investigated the relationships between students' centrality within a social network, gender, and attainment of leadership positions. The authors apply a two-study design in which they first test their hypotheses based on observational data, then infer the causal relationship by means of an IV design. The correlational study shows that a student's ego-network is related to her or his job placement in leadership positions. Network centrality is positively related to job placement both for male and female students—however, female students especially benefit from more

women-dominated networks and from relatively even communication with peers. Because the observational study provides no insights into the causal focal relationship, the authors exploit an exogenous variation in the context. When students start their MBA program, they are randomly assigned to home sections. Students take their first-quarter classes only with students from their home section, which is why their home-section-mates initially represent their most important friends. Later in their studies, students bid for second-quarter classes. Since the enrollment of students into classes is relatively unpredictable (i.e., many students even end up in classes they did not bid for), students have limited influence on the inter-personal ties they will develop. The authors used a student's degree of exposure to same-gender classmates from other home sections as the instrument. The findings of the IV design mostly support the correlational study. Female students' job placements are influenced by having an inner circle, whereas no effect was found for male students. Overall, the study exploits the as-if random assignment of students to networks as an instrument to estimate the causal effect of networks on attaining leadership positions, particularly for female leaders.

Finally, Chintrakarn et al. (2017) investigated the relationship between managers' religious piety and firms' anti-takeover provisions. For the instrument, the authors used the degree of religious piety from 1971 in the community surrounding a company's headquarters. First, the authors regressed the number of anti-takeover defenses on the non-instrumented current religious piety variable and found a positive effect. In the second step, they analyzed the same relationship based on instrumented values of current religious piety. The two-stage least squares analysis supported the conclusion of the correlational analysis that current religious piety affects corporate governance, although the coefficient of the instrumented treatment variable was smaller (b = 0.849) than the coefficient of the non-instrumented treatment variable (b = 1.256). As a robustness check, the authors used another instrumental variable—the degree of religious piety in the population in 1952. The result of the two-stage least squares supported the prior findings. Overall, the authors provide evidence that religious piety substitutes for corporate governance and reduces the conflict between managers and shareholders.

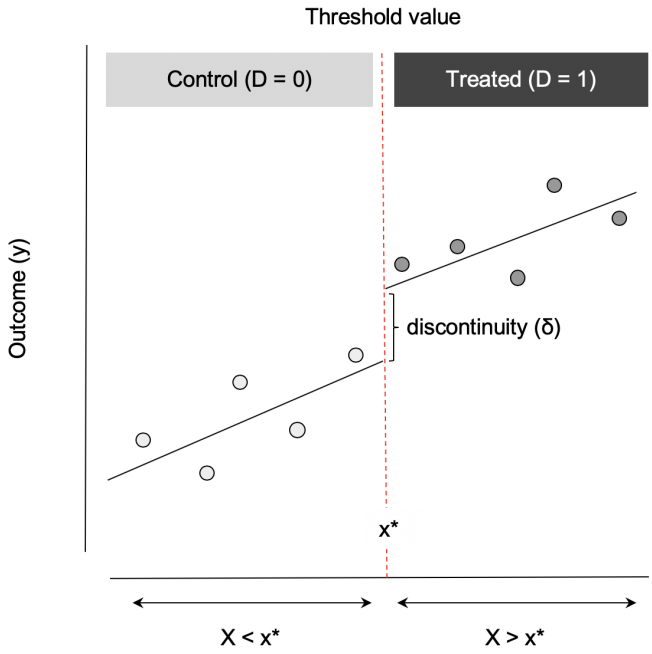
To sum up, the three examples highlight creative ways of applying instruments to estimate causal relationships involving a variety of leadership topics—including the consequences of leader selection for firms (Bennedsen et al., 2007), leadership development (Yang et al., 2019), and the consequences of culture on strategic leadership (Chintrakan et al., 2017).

3.3 Regression Discontinuity Design

The Regression Discontinuity (RD) design was initially developed by Thistlethwaite and Campbell (1960). It capitalizes on the fact that in many settings (e.g., business and education) a unit's score—above or below a certain threshold on a continuous variable—determines the treatment status of the unit. The basic idea of the RD design is shown in Figure **??**. When the assignment variable X is greater than or equal to x^*, which represents the threshold, units receive the treatment; if X is smaller than x^*, units receive no treatment. The RD design builds on the assumption that in the neighborhood of the

threshold (x^*) the assignment process is almost random (Dunning, 2012; Lee & Lemieux, 2010). Then, the variance in the outcome variable y across the $X < x^*$ and $X \geq x^*$ regimes is caused by the treatment (Antonakis et al., 2010), represented by the quantity δ.

Fig. 6: Visual Representation of the Regression Discontinuity Design



Notes.—The underlying population regression function is $y = \alpha + \delta D + \beta(X - x^*)$, where y is the response variable, α is the intercept, δ denotes the systematic difference in y across control and treated units, whereas β is the regression slope of the mean centered X scores.

So far, we have assumed that the probability of treatment assignment changes from 0 to 1 when $X > x^*$. This so-called 'sharp' RD design is probably the most common form in empirical research. However, some studies also apply a 'fuzzy' RD design. Here, the change in the probability of receiving the treatment is much smaller than in the sharp RD design when $X \geq x^*$ (Lee & Lemieux, 2010). For instance, the probability of receiving the treatment may just increase by several percentage points at the threshold (see, e.g., Grönqvist & Lindqvist, 2016). Although the sharp and the fuzzy RD differ to some extent, researchers can use both RD designs to estimate the average causal effect of the treatment.

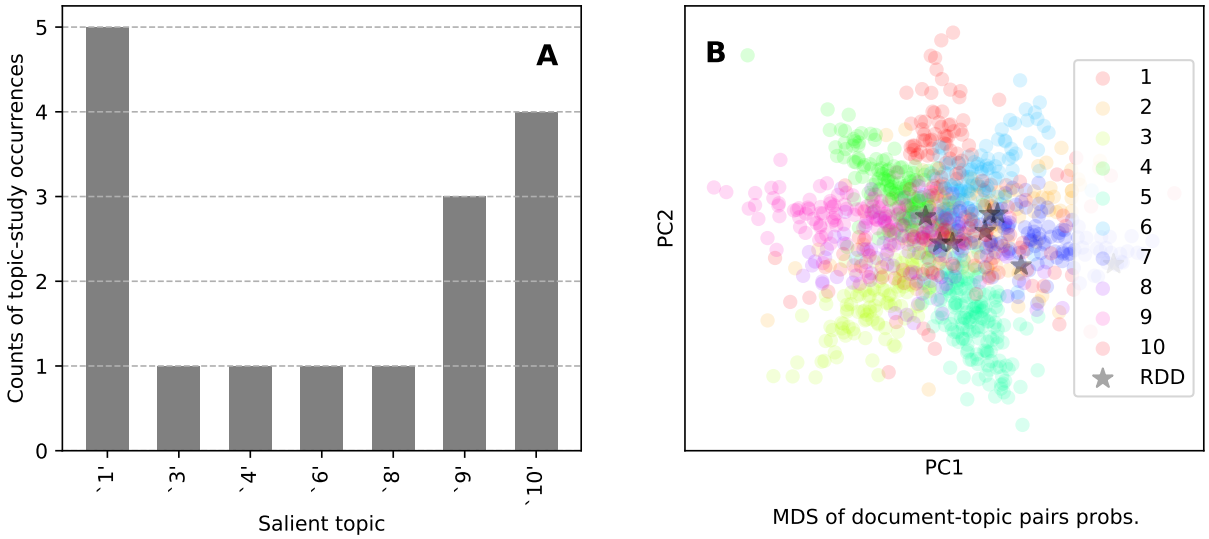
3.3.1 Regression Discontinuity Designs and Leadership Research

Table **??** shows the set of leadership studies drawing upon an RD design. The distribution of leadership topics across the documents (see Figure **??**, Panel A) confirms causal methods—irrespective the specific estimation framework—are core to the study of female leadership (in fact, Topic 1 has the highest number of occurrences among 'salient topics'). In addition, the RD design seems to associate to another two topics, namely 'nature of managerial work' (Topic 9) and 'leadership in teams and decision groups' (Topic 10).

TABLE IV
Regression Discontinuity Designs—Substantive Focus

	Salient topics			
	1^{st} topic		2^{nd} topic	
Study	Topic label	Prob.	Topic label	Prob.
Arvate et al. (2018)	Female leadership	0.373	Charismatic leadership	0.129
Boas & Hidalgo (2011)	Leadership in teams	0.177	Charismatic leadership	0.16
Butler (2009)	Charismatic leadership	0.203	Female leadership	0.127
Dal Bó et al. (2009)	Female leadership	0.208	Development of leadership	0.179
Dunning & Nilekani (2013)	Leadership in teams	0.15	Female leadership	0.148
Grönqvist & Lindqvist (2016)	Transformational leadership	0.14	Leadership in teams	0.123
Heck & Moriyama (2010)	Cognition and leadership	0.129	Ethical leadership	0.123
Lechler & McNamee (2018)	Leadership in teams	0.16	Female leadership	0.132

Fig. 7: Regression Discontinuity Designs—Topic Characterization



Notes.— Panel A pictorially depicts the information reported in Table **??**; Panel B: Data points marked with a star denote natural experiments that are folded in the topic model trained on the 1,156 articles published in The Leadership Quarterly; Topic labels: Topic 1—'female leadership'; Topic 2—'emotions and leadership'; Topic 3—transformational leadership'; Topic 4—'development of leadership'; Topic 5—'(neo-)charismatic leadership'; Topic 6—'cognition and leadership'; Topic 7—'strategic leadership'; Topic 8—'ethical leadership'; Topic 9—'nature of managerial work'; Topic 10—'leadership in teams and decision group.' 'MDS' stands for multdimensional scaling; 'PC1' and 'PC2' refer to the components returned from the MDS analysis.

3.3.2 Regression Discontinuity Design Examples

Our review indicates that to date only a few leadership studies have used the RD design. We will discuss three studies and analyze their approach in more detail in order to provide leadership scholars with insights into applying the RD design for inferring causal relationships.

The first study that we have selected is by Arvate, Galilea and Todescat (2018). They adopted a sharp RD design to analyze the 'queen bee' phenomenon (Staines, Tavris, & Jayaratne, 1974), which states that women in leadership positions do not support—and may even penalize—female followers. The authors point out that prior studies on the phenomenon are affected by reverse causality and omitted variable biases, and, therefore, do not have a causal interpretation. To overcome these problems, the authors use an RD design focusing on close-run elections in Brazilian municipalities. Empirical data indicate that those municipalities in which women are elected as mayors over a male candidate by a close margin do not differ from municipalities in which women just lost the election against a male competitor. Thus, near the threshold (i.e., 50% of votes), it is almost random as to whether a woman or a man is assigned to the leadership position. The results do not provide clear evidence for the queen bee hypothesis. In public organizations, which are under the influence of mayors, the ratio of female to male workers is reduced for middle management (anti-women) but increased for top management (pro-women) in municipalities ruled by female mayors. Overall, the study applies a sharp RD design to test the causal effect of female leaders on the career opportunities of female followers.

The second study is by Heck and Moriyama (2016), who used a sharp RD design to analyze the indirect relationship between improvement-focused school leadership and student learning outcomes via school instructional practices. The authors exploit the discontinuity which results from a cut-off date for students for starting kindergarten. In the study setting , students who were 5 years old by December 31 were assigned to the treatment (i.e., one year further schooling), whereas students who were 4 years old by December 31 were assigned to the control group (i.e., one year less schooling). Again, we can argue that near the cut-off, the student assignment to the treatment and control groups is as good as random, because parents cannot precisely manipulate the birth date of their children. Due to students' nesting within schools, the authors applied a multilevel RD design. The results provide causal evidence for the benefits of one additional year of schooling (i.e., the added-year of schooling effect). The authors further show that the added-year of schooling effect is influenced by the effect of improvement-focused school leadership on school instructional practices.

Finally, Grönqvist and Lindqvist (2016) used a fuzzy RD design to analyze how receiving military officer training influences the probability of attaining a civil leadership position. Directly testing this relationship is difficult as individuals who receive officer training differ from individuals who do not receive the training with regard to observable and unobservable characteristics (e.g., abilities). To infer the causal effect of the officer training, Grönqvist and Lindqvist (2016) used discontinuities in test scores as the identification strategy. That is, all individuals who were drafted in the Swedish military had to complete a cognitive ability test in which their abilities were ranked according to four dimensions on a

scale from 1 (lowest) to 9 (highest). Although the test score did not determine whether or not a person received the training, it significantly increased the probability of being treated. For instance, receiving the officer training 'jumps' from only 2% of the recruits with a score of 17 to 28 % for recruits with a score of 18 (Grönqvist & Lindqvist, 2016). Because of the fuzzy RD design, the authors used two-stage least squares to estimate the relationship between receiving officer training and attaining a civil leadership position after the military service. Their results indicate that officer training clearly influences the attainment of a civil leadership position. Individuals who received the officer training have a 75% higher likelihood of attaining a civil leadership position compared to the controls. Overall, the study provides causal evidence for the effectiveness of general leadership training.

To sum up, the three examples show that both the sharp and the fuzzy RD designs can provide answers to important questions in leadership research. The studies were conducted in a variety of contexts (e.g., schools, military, public administration) and used very different assignment variables (e.g., age, voting margins, test scores). However, all three studies exploited the almost random assignment of units near the cut-off point for causal inference.

4 Natural Experiments in Leadership Research: Guidelines

Our review of studies from various leadership-related disciplines, including economics, business and management, political sciences, and social sciences, suggests that natural experiments are very effective in identifying causal relationships. This section provides some guidelines on further facilitating the use of natural experiments in leadership research. The key phases of the research design are presented, starting with the discovery of a natural experiment moving on to the actual form of the natural experiment, and then finishing with the statistical analysis.

4.1 Discovering Natural Experiments

A major challenge for leadership scholars is to discover natural experiments. Unlike laboratory and field experiments, researchers cannot actually design natural experiments. Instead they need to discover contexts in which a random or as-if random variation has taken place. Discovering these contexts is difficult. For instance, Dunning (2012, p. 41) argues that discovering natural experiments is 'as much art as science.'

We believe a good way to discover natural experiments is by learning from prior examples. Firstly, novel research questions can often be answered by re-using a known/established natural experiment. For example, the natural experiment of the German reunification has been exploited to analyze several research questions, such as the impact of income on health (e.g., Frijters, Haisken-DeNew, & Shields, 2005), the transmission of preferences for entrepreneurship from parents to children (e.g., Wyrwich, 2015), or the legitimation of inequality (e.g., Haack & Sieweke, 2018). Secondly, even when known/established experiments may not be perfect for addressing a new research question, by analogical reasoning, researchers can be inspired by such experiments and discover more appropriate naturally-occurring events for their

research question.

Tables 5, 6, and 7 summarize the research questions, exogenous variations, and treatments of each individual study included in our review. Regarding standard natural experiments (see Table 5), many studies have exploited the introduction of new laws or regulations, such as a legal reform in Sweden that discontinued the conferral of state orders of merit (Siming, 2016), or new anti-takeover legislation (Cheng et al., 2005). Others have used laws that set certain quotas, such as the proportion of women on the boards of Norwegian firms (Matsa and Miller, 2013), or that reserved leadership positions for members of minorities in randomly selected villages (Beaman et al., 2012). Finally, some studies leverage sudden, exogenous events, such as earthquakes (Belloc et al., 2016), the successful assassination of political leaders (Shea and Solis, 2018), or the division of Germany into two states after 1945 (Wyrwich, 2015).

Instrumental variables (see Table 6) can be categorized into four main groups: i) macro-level variables pertaining to cultural, institutional, or societal properties, such as the degree of religious piety within a population (Chintrakarn et al., 2017) or the ratio of voters in favor of divorce within a region (Amore et al., 2017); ii) random or as-if random events, such as the gender of a CEO's first born child (Bennedsen et al., 2007) or the proportion of a firm's founders that are dead (Adams et al., 2009); iii) spatial distance or related variables, e.g., the distance of companies from executive recruiting firms (Akyol and Cohen, 2013) or the existence and intensity of one-stop flight connections between the locations of potential director home addresses and firm headquarters (Bernile et al., 2018); and finally iv) personal or team attributes, such as CEO age and tenure (Driver and Coelho Guedes, 2017) or board size (Kilic and Kuzey, 2016).

Concerning the RD design (see Table 7), the highest number of studies focus on the margin of victory in an election as an assignment variable (e.g., Arvate et al., 2018; Boas and Hidalgo, 2011). These studies exploit the fact that in close elections, the assignment of individuals to the leader position is almost random.

Ultimately, scholars have discovered natural experiments in a variety of leadership-related contexts and we recommend leadership scholars to analyze whether their research questions can be analyzed using the same natural experiment. Furthermore, leadership scholars may focus on current or historical institutional changes (e.g., the introduction of new laws) or try to identify contexts in which assignment to training, jobs, or ranks are based on or influenced by a unit's score on an observed variable to identify a new natural experimental context.

4.2 Deciding about the Form of the Natural Experiment

Once an exogenous, naturally-occurring variation has been discovered, scholars need to decide the form of natural experiment to adopt. This choice is key to a design's internal validity as standard natural experiments, IV, and RD designs build on specific assumptions about the mechanisms that are presumed to generate the observed data (Dunning, 2012; Imbens & Wooldridge, 2009). Our decision tree (Figure **??**)

TABLE V
Standard Natural Experiments—Research Questions, Exogenous Variations, and Treatments

Authors	Research question	Context	Exogenous variation	Treatment
Bae & Yi (2008)	Do mutual fund managers time the market?	Equity mutual funds	Introduction of the "short-short rule" for mutual funds	The short-short rule hinders mutual fund managers from timing the market
Baekgaard (2011)	Whether and how two different organizational leadership models affect the interaction between politicians and administrators	Danish municipalities	Amalgamation of 232 municipalities into 65 new municipalities	Decision to use different administrative leadership models
Beaman et al. (2012)	Does growing up under female leadership raises aspirations and educational attainment for girls?	Indian Villages	Law that reserves the chief councilor position for women in a random sample of villages	Female leader
Belloc et al. (2016)	Do natural catastrophes impact the stability of institutional regimes?	Medieval Italian cities	Earthquakes	Earthquakes interpreted as manifestation of God's outrage
Bhavnani (2017)	Do the effects of temporary ethnic group quotas persist?	Indian Villages	Quasi-random declaration of reserved seats to be "open" in elections in 1974 and 2008	Discontinuation of ethnic group quotas
Breda & Ly (2015)	Does the level of male-domination influence gender bias?	French higher education	Examiners in oral examinations are aware of the candidate's gender, whereas examiners are unaware of the gender in written exams	Proportion of women among professors within a scientific field
Brockman et al. (2015)	Does CEO compensation risk influences managers' risk-seeking behavior?	U.S. public corporations	Passage of FAS 123R	Reduction in CEO compensation risk level
Byrd et al. (2012)	Which governance mechanisms are associated with firm survival and failure?	U.S. firms	Thrift crisis of the late 1980s	Unitary leadership (single CEO/Chairman) vs. dual leadership (two leaders)
Chauchard (2014)	Can descriptive representation for a stigmatized group change the beliefs and intentions of members of dominant groups?	Indian villages	Discontinuity in the implementation of reservation of political leadership positions for minorities	Members of stigmatized groups hold political leadership positions
Chen et al. (2016)	Do controlling shareholders hold CEOs accountable to corporate fraud behavior?	Chinese companies	Split Share Structure Reform in China	Greater incentive for powerful shareholders to monitor managers
Cheng et al. (2005)	What value do managers place on the control rights conferred by stock ownership?	U.S. companies	Introduction of second-generation anti-takeover legislation	Weakening of outsiders' takeover power
Cohen & Wang (2013)	Do staggered boards negatively affect firm value?	Firms in Delaware	Unexpected court rulings in Delaware that affected for a subset of firms the extent to which staggered boards can impede shareholders seeking to replace a majority of directors	Weakening of the anti-takeover force of staggered boards
Coman (2018)	How does the party affiliation of local elites influence the distribution of central government funds to territorial units?	Romania	2008 Romanian electoral reform	Change from a closed-list proportional system to a system that requires all members of the parliament to run in single-member districts
Cox et al. (2000)	Does politicians' career goals influence their decision to join a faction?	Japanese bicameral parliament	Different electoral rules in the two houses of the Japanese parliament	Lower level of electoral competition when politicians' join a faction

Cont'd Table 5

Authors	Research question	Context	Exogenous variation	Treatment
Cunat & Guadalupe (2009)	Do deregulation and increased product market competition influence the compensation packages that firms offer to their executives?	U.S. financial sector	Deregulation laws that reduced entry barriers into the financial sector	Increased product market competition
Dahya & McConnell (2005)	Do boards with significant outside directors make different decisions than boards dominated by inside directors?	U.K. firms	Publication of the "Cadbury Report" which coerced firms into adding outside directors	Appointment of outside directors
De Paola & Scoppa (2015)	Is gender discrimination affected by the gender of evaluators?	Italian universities	Random assignment of evaluators	Degree of committee gender composition
De Paola et al. (2010)	Do gender quotas influence women involvement in political activity?	Italian local administration	Some municipalities did not vote under the gender quota regime	Gender quotas
Gittell et al. (2008)	Does job design affect the coordination of work?	Massachusetts hospital	Some patients were assigned to hospitalist physicians while others remained under the care of their own private practice physicians	Stage- and site-based specialization
Gormley et al. (2013)	How do boards adjust incentives in response to firms' risk and how do these incentives affect managers' risk-taking?	U.S. listed corporations	Workers being exposed to chemicals that have just been found to be toxic	Increase in left-tail risk (i.e., material risk)
Guadalupe and Wulf (2010)	Does product market competition influence organizational design?	large U.S. firms	Canada-United States Free Trade Agreement of 1989	Increase in competition
Han & Zhang (2018)	What is the net effect of a politically connected board for firms?	Chinese companies	Regulatory change in China	Bureaucrats were forbidden from sitting on the board of public firms
Hidalgo et al. (2016)	Does auditor appointment affect political accountability?	Brazilian state audit courts	Variation in the appointment mechanisms for choosing auditors	Auditors insulated from political influence
Huber & Arceneaux (2007)	Do presidential campaign advertisements mobilize, inform, or persuade citizens?	U.S. presidential elections	Some individuals living in non-battleground states accidentally received different advertisement because they resided in a media market adjoining a competitive state.	High levels or one-sided barrages of campaign advertisements
Jayaraman & Milbourn (2015)	Do CEO equity incentives influence financial misreporting?	U.S. public corporations	Collapse of Arthur Andersen	Level of auditor expertise
Jiraporn & Lee (2018)	How do co-opted directors affect dividend policy?	U.S. public corporations	Passage of the Sarbanes–Oxley Act	Increase in board independence
Jiraporn et al. (2018)	Do independent directors influence corporate innovation?	U.S. public corporations	Passage of the Sarbanes–Oxley Act	Increase in board independence
Kahn, Li and Zhao (2015)	Do shifts in evaluating local officials for promotion affect their efforts to reduce water pollution?	Chinese municipalities	Change in the local political promotion criteria	Higher incentives for local political leaders to reduce border pollution
Knott (2001)	Does hierarchy provide a dynamic advantage? Having self-employed parents who encountered a great deal of resistance due to their self-employment	Quick-printing industry	Establishments leave a franchise system	Establishments lose their hierarchical manager

Cont'd Table 5

Authors	Research question	Context	Exogenous variation	Treatment
Laustsen and Petersen (2017)	Are political candidates and leaders with dominant, masculine physical features more preferred under conditions of conflict than of cooperation?	Poland and Ukraine	Crimea crisis in 2014	Condition of conflict
Matsa & Miller (2013)	Does female leadership affect corporate decision making?	Norwegian listed firms	Law that sets a quota for women in the board of directors	Increase of female leaders
Poulos (2019)	Does personal wealth cause individuals to select into public office?	Georgia	1805 and 1807 Georgia land lotteries	Increase in wealth due to lottery
Rickman & Witt (2008)	Do principals who exercise favouritism towards certain agents harm other agents?	English soccer	Introduction of professional referees to the English Premier League	A group of referees was retained for the whole soccer season on a full salary
Shea and Solis (2018)	Does leader tenure influence country's creditworthiness?	Cross-country analysis	Successful executive assassination	Leader death
Siming (2016)	Do orders of merit function as an external form of perquisite through which the government can supplement the compensation given by a publicly listed firm to the CEO?	Swedish companies	1974 legal reform in Sweden	Discontinuing the conferral of orders of merit to citizens
Tabvuma et al. (2014)	Does change in political leadership influence job satisfaction in the public sector?	British public sector	Change in the political party which is governing at the national level	The ruling political party matches the political preference of the public sector employee
Tosun (2016)	Do CEO option compensation changes influence firm leverage changes?	U.S. public corporations	Internal Revenue Code 162(m) tax law	Increased option compensation for CEOs
Valdini (2012)	Do electoral systems affect candidate selection, especially female political leaders?	Elections in Japan	Electoral reforms of the Japanese House of Representatives in 1994	Move towards a greater orientation towards issues and parties
Vo & Canil (2019)	Is CEO pay disparity due to efficient contracting or CEO power?	U.S. public corporations	Introduction of FASB ASC 718 in 2005	All accounting benefits associated with option grants were removed
Wyrwich (2015)	Do parents transmit preferences for entrepreneurship to their children?	Entrepreneurs in Germany	German divide in 1945 which leads to some parents of entrepreneurs growing up in a socialist country	Having self-employed parents who encountered a great deal of resistance due to their self-employment

TABLE VI
Instrumental Variable Designs—Research Questions, Exogensous Variations, and Treatments

Authors	Research question	Context	Exogenous variation	Treatment
Adams et al. (2009)	Do founder-CEOs influence firm performance?	Family firms	(1) proportion of the firm's founders that are dead; (2) number of people who founded the company	Founder-CEO
Adhikari (2018)	Do firms led by female top executives hold more cash?	U.S. listed companies	Fraction of registered men between the ages of 18 and 44 who were drafted or enlisted for WWII in a state	Number of female executives
Adkins et al. (2007)	Do managerial compensation and ownership influence the use of foreign-exchange derivatives by U.S. bank holding companies	Large bank holding companies	(1) number of employees, (2) number of subsidiaries; (3) number of offices; (4) CEO age; (5) 12 month maturity mismatch; (6) market-to-book ratio; (7) foreign interest income dummy	Managerial compensation and ownership
Aghion et al. (2013)	Does institutional ownership influence firm innovation?	S&P 500 firms	Firms' addition to the S&P 500 index	Institutional ownership
Akyol & Cohen (2013)	Does the use of executive search firms for board member search influence corporate governance?	U.S. public corporations	Geographic distance of companies to executive search firms	Use of executive search firms
Amore et al. (2017)	Does leadership by couples affect the profitability of family firms?	Italian family firms	Regional ratio of voters in favor of divorce	Leadership by couples
Amore et al. (2014)	Do gender interactions at the top of the corporate hierarchy affect firm performance?	Italian family firms	(1) gender composition of the pool of potential family heirs; (2) geographic variations in gender stereotypes	Gender interactions
Arora (2018)	Does the effort of financially linked independent directors enable firms to reemerge from bankruptcy?	U.S. firms	(1) Board meeting fees; (2) prime interest rate movement; (3) board size	Effort of financially linked independent directors
Artz et al. (2017)	Does the competence of supervisors influence the quality of employees' lives?	US and UK employees	(1) whether the supervisor has a college degree; (2) whether the supervisor worked his or her way up in the organization	Supervisor competence
Azoulay et al. (2017)	Do young scientists adopt their advisers' orientations toward commercial science?	Academics from the U.S.	(1) proximity between scholars' undergraduate institutions and the universities where they might become postdoctoral fellows; (2) shared nationality between the scholar and a potential mentor	Social matching
Barros (2007)	Which factors influence pay and performance of CEOs?	Portuguese non-profit organizations	(1) number of stockholders in the company; (2) father's education	Board composition
Bennedsen et al. (2007)	Does the appointment of family CEOs negatively affect firm performance?	Danish family firms	Gender of CEO's first born child	Hiring of an external CEO
Bernile et al. (2018)	Does board diversity influence corporate policies and risk?	North American listed companies	Existence and intensity of one-stop flight connections between the locations of potential director home addresses and firm headquarters	Board diversity
Chen et al. (2017)	Does gender diversity in boardrooms influence dividend payouts?	S&P 1500	(1) fraction of male directors linked to female directors; (2) female-to-male labor force participation ratio	Board gender diversity

Cont'd Table 6

Authors	Research question	Context	Exogenous variation	Treatment
Chintrakarn et al. (2017)	Does religious piety substitute for corporate governance?	U.S. firms	degree of religious piety in the past in the population surrounding a corporate headquarter	Current religious piety in the population surrounding a corporate headquarter
Conroy & Weiler (2016)	Does female ownership influence firm performance?	U.S. startups	(1) change in divorce rate; (2) growth in female labor force participation	Female owner
Conyon & He (2017)	Does gender diversity in boardrooms influence firm performance?	US firms	Percentage of female residents in the US state where the given company has its headquarter	Board gender diversity
Dal Bó et al. (2009)	Why do political dynasties persist?	U.S. congress	Successful first reelection attempt	Long tenure in power
Dasgupta (2018)	Does technological change contribute to political turnover?	Indian agriculture	Share of district land with a naturally occurring aquifer interacted with a dummy variable that "switches on"' for all districts with the introduction of HYV crops	Technological change
Delis et al. (2017)	Do board members from countries with different genetic diversity levels influence corporate performance?	North American and U.K. listed companies	(1) migratory distance from East Africa; (2) the level of ultraviolet exposure in the directors' country of nationality	Genetic diversity within a country
Driver & Coelho Guedes (2017)	Is R&D expenditure reduced in cases of imminent departure of the CEO?	UK service and manufacturing firms	(1) CEO age; (2) CEO tenure; (3) profit shock	CEO departure
Frantz & Stein (2017)	Do institutionalized leadership succession rules influence the likelihood that dictators confront coups?	Dictatorships	A regime took power at independence	Institutionalized leadership succession rules
Gabel & Scheve (2007)	To what extent does elite opinion about policy shape public opinion?	European countries	Change in electoral laws	Elite polarization
Harjoto & Rossi (2019)	Do religiosity and female representation on the board influence corporate social responsibility?	Italian listed companies	(1) number of words in the encyclicals and other writings that are related to "religiosity"; (2) number of words in the encyclicals that are related to gender diversity	Religiosity in the population and women on the board of directors
Hearn and Filatochev (2019)	Does private equity ownership influence the probability of the founder's retention as CEO?	IPOs in emerging markets	Numbers of private equity investors	Private equity ownership
Hooghiemstra, Kuang and Qin (2017)	Does 'readability' of a remuneration report influence the level of shareholder say-on-pay voting?	UK-listed firms	Readability of CEO letter	Readability of remuneration report
Izgi & Akkas (2012)	Does top management gender diversity influence firm performance?	Turkish firms	(1) CEO MBA; (2) Big 4 Audit	Female CEO
Khwaja (2009)	Can project design compensate for community-specific constraints in social capital?	Community-maintained infrastructure projects in Northern Pakistan	Characteristics of hereditary leader households (e.g., does the leader household has a young and healthy male member?)	Having a project leader
Kilic & Kuzey (2016)	Does gender diversity in boardrooms improve firms' economic performance?	Turkish listed companies	(1) board size; (2) board independence; (3) firm size; (4) leverage	Board gender diversity
Li et al. (2018)	Does superior environmental, social and corporate governance disclosure affect firm value?	FTSE 350 firms	Firm-level initial value of the ESG disclosure score	Environmental, social and corporate governance disclosure
Lin et al. (2011)	Do managerial incentives and CEO characteristics influence a firm's innovation activities?	Chinese manufacturing firms	Industry-location averages	Provision of managerial incentive schemes

Cont' Table 6

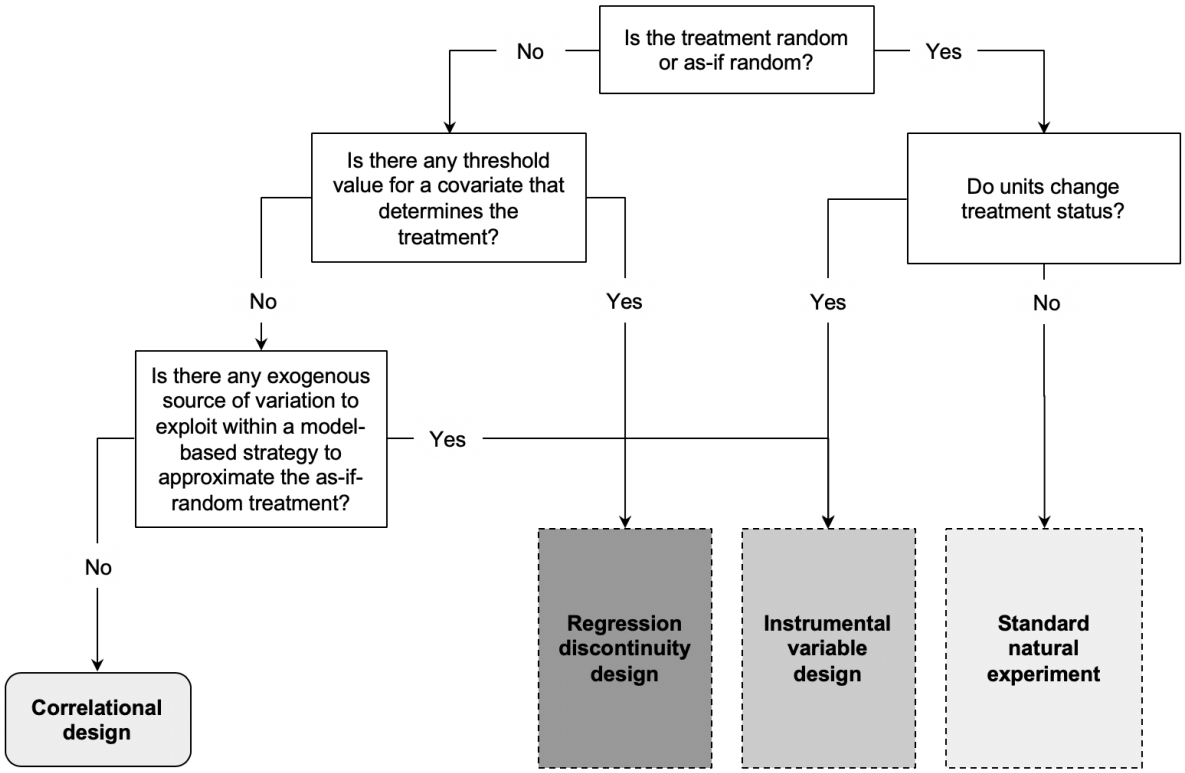
Authors	Research question	Context	Exogenous variation	Treatment
Markussen & Roed (2017)	Do gendered peer influences affect early career entrepreneurship?	Norwegian entrepreneurs	Entrepreneurship activity among the schoolmates' parents	Peer influences
Nicolosi and Yore (2015)	Does a CEO's marital status influence their firm's investment and compensation policies	S&P 1500	Religious CEO	CEO marital status
Pascal et al. (2017)	Does a CEO's business education influence the financial and social performance of microfinance institutions?	Global microfinance institutions	Microfinance institution age	CEO business education
Rouse (2012)	Does high school leadership affect subsequent educational attainment?	U.S. high schools	(1) school-level measure of leadership opportunities; (2) oldest child in the family	High school leadership
Sabatier (2015)	Does gender diversity in boardrooms improve firms' economic performance?	French publicly listed companies	Average ratio of women in connected boardrooms	Board gender diversity
Shue & Townsend (2019)	Does an increase in stock option grants affect CEO risk-taking?	North American listed companies	(1) whether each CEO-year is predicted to be the first year of a new fixed-value cycle; (2) variation in the value of options granted within fixed-number and fixed-value cycles	Increase in stock option grants
Sun & Hovey (2013)	Does executive compensation influence management discretionary behavior over financial reporting?	Australian Securities Exchange (ASX) listed companies	Median value of discretionary accruals for a portfolio of firms	Executive compensation
Vries (2012)	Does personality influence leadership styles?	Large municipality organization	Different-source personality ratings	Leader personality
Wu (2015)	Does inequality influence trade openness in authoritarian regimes?	Authoritarian regimes	Ratio between two age groups	Income inequality
Yang et al. (2019)	Do networks influence persons' placement into leadership positions of varying levels of authority	MBA programme	degree of expose to same-gender classmates from other home sections	Composition of students' inner circle

TABLE VII
Regression Discontinuity Designs—Research Questions, Exogensous Variations, and Treatments

Authors	Research question	Context	Exogenous variation	Treatment
Arvate et al. (2018)	Do female leaders harm the career of female followers?	Mayoral elections in Brazil	Margin of victory in election	Female leader
Boas & Hidalgo (2011)	Does incumbency influence politicians' ability to control the media and does media control affect their future electoral prospects?	Elections in Brazil	Margin of victory in election	Incumbency
Butler (2009)	Does the incumbency advantage enjoyed by freshmen differ from the incumbency advantage enjoyed by non-freshmen incumbents?	U.S. house elections	Margin of victory in previous election	Incumbency
Dal Bó et al. (2009)	Why do political dynasties persist?	U.S. congress elections	Margin of victory in election	Long tenure in power
Dunning & Nilekani (2013)	Do ethnic quotas induce distribution of material benefits to members of disadvantaged groups?	Elections in India	Proportion of the local population comprised of marginalized castes or tribes	Reserved seats in the council for members of minorities
Grönqvist and Lindqvist (2016)	Does training during the Swedish military service influence the probability of attaining a civil leadership position?	Swedish army	Score on a cognitive ability test	Received military officer training
Heck & Moriyama (2010)	Does school leadership influence students' educational performance?	School	Student age	Additional year of schooling
Lechler & McNamee (2018)	Does colonial rule influence support for democracy?	Namibia	Location of the Police Zone boundary	Form of colonial rule

is designed to help scholars select the most suitable research design.

Fig. 8: Decision Tree Linking Treatment Attributes, Assumptions, and Forms of Natural Experiments



The first question leadership scholars need to answer concerns the assignment process: is the assignment of units to the treatment truly random, 'as-if random' (i.e., the assignment process resembles a true randomization, Dunning, 2012), or non-random? Although a true randomization is a hallmark of laboratory and field experiments (Podsakoff & Podsakoff, 2019), it is seldom found in natural experiments—except for studies that use lotteries (e.g., Angrist, 1990).

In fact, most natural experiments are characterized by an as-if random assignment. The as-if random assignment poses some challenges for leadership scholars, because they need to evaluate the quality of the as-if randomization; that is, the extent to which it is plausible to assume that the assignment process (closely) resembles a true randomization. Dunning (2012) recommends assessing the quality of the as-if randomization based on three criteria. First, researchers should investigate whether units had *information* that they would or would not receive the treatment. Second, researchers need to check whether units had *incentives* to self-select into the treatment group or control group . Third, researchers should analyze whether not only units had incentives but also *capacity* to self-select into a treatment status. For the assessment, Dunning (2012) suggests using both qualitative evidence (e.g., documents, interviews) and quantitative evidence (e.g., balance tests).

Jones and Olken (2005), for example, jointly used qualitative and quantitative evidence to evaluate the plausibility of as-if randomization. They used qualitative evidence, such as leaders' biographies, to

determine whether the nature of death of political leaders was truly exogenous (e.g., due to health issues or accidents). At the same time, they provide quantitative evidence, such as the result of a logistic regression, to back up the assumption that economic conditions do not predict the death of political leaders.

If the assignment process is random or as-if random, then researchers need to check the second question: are units allowed to change their treatment status, moving from the treatment (control) to the control (treatment) group? This check is important, because units that comply with the assignment ('compliers') probably differ from units that do not comply with the assignment ('non-compliers'). For instance, in Angrist's (1990) study on the effect of military service on lifetime earnings, subjects were assigned to the treatment (military service) and control group (no military service) based on their date of birth and the result of a draft lottery. However, some subjects who were eligible based on the result of the lottery did not serve in the military, because they went to college or moved outside the U.S. We can assume that these non-compliers differ from the compliers (i.e., those citizens who were eligible and did serve and those who were not eligible and did not serve in the military) regarding knowledge, values, attitudes etc., and that these differences are probably correlated with their lifetime earnings. Therefore, leadership scholars should, if possible, focus on the compliers in their analysis (Dunning, 2012) or should try to estimate the ratio of compliers within a population (see, e.g., Angrist, 1990).

If researchers answer the first question positively (i.e., random or as-if random assignment) while negatively answering the second one (i.e., change of treatment status), then the natural experiment at hand represents a standard natural experiment, whose data can be analyzed either using Neyman's potential outcome framework or by adopting model-based adjustments (we discuss the analysis in more detail below). If both answers are positive, then the natural experiment is an IV design.

If the assignment to the treatment is neither random nor as-if random, then leadership researchers need to check whether a unit's score on an observed variable influences the assignment. An assignment based on a unit's score on a covariate is a hallmark of the RD design. The assignment may take two forms (Lee & Lemieux, 2010). First, in the sharp RD design, a unit's score on a covariate *determines* the assignment. For instance, in the study by Arvate and colleagues (2018), the assignment of women to political leadership positions was determined by their share of votes in mayoral elections. Second, in the fuzzy RD design, a unit's score on a covariate affects the *probability* of receiving the treatment. For instance, in Grönqvist and Lindqvist's (2016) study, a person's score on a cognitive test influenced the probability of receiving leadership training.

Finally, if the assignment to the treatment is not based on a unit's score on an observed variable, then leadership researchers need to determine whether they exploit an exogenous source of variation that they can use to approximate an as-if random treatment. If they answer 'yes' to the questions, then this exogenous source of variation represents an instrument and the natural experiment can be classified as an IV design. For instance, in the study by Bennedsen and colleagues (2007), the hiring of a family CEO versus an external CEO is neither random nor as-if random. However, the authors use an exogenous

source of variation—the gender of the firstborn child as an instrument to estimate the causal effect of hiring professional managers on firm performance. If researchers cannot exploit such an exogenous source of variation, then their context represents a correlational design, which does not support causal inference.

4.3 Analyzing Natural Experiments

After leadership scholars have determined the form of the natural experiment, they need to analyze the experiment. Although the different forms may require different types of data analysis, simplicity and transparency are the underlying factors (Dunning, 2012). Simplicity means that leadership scholars do not necessarily need to apply complex statistical techniques to analyze natural experiment data. A simple difference-of-means or difference-of-proportions test is often sufficient to estimate the average causal effect of the treatment. Simplicity in data analysis also generally implies greater transparency. For instance, difference-of-means or difference-of-percentages tests provide a more transparent estimate of the average causal effect than multivariate regression models that contain several covariates. Although researchers cannot always follow these principles due to the specific circumstances of a natural experiment and they need to use model-based adjustments, such as including covariates or adjusting standard errors, we believe that they should consider the general principles and prefer 'simpler' models over more complex ones.

4.3.1 Analyzing Standard Natural Experiments

Estimation of the Average Causal Effect in the Standard Natural Experiment. The analysis of standard natural experiments is reasonably simple. Since the assignment of units to the treatment is random or as-if random, it is possible to infer a causal effect within Neyman's potential outcome framework (also called the Neyman-Rubin model; see Rubin, 2005). Neyman's framework is both simple and transparent (Dunning, 2012). Scholars may want to use the difference-of-means or difference-of-proportion tests that are widely applied in laboratory experiments. Therefore, either a t-test (for smaller sample sizes) or z-test (for larger sample sizes, see Dunning, 2012) could suffice to conduct causal research with observational data.

Alternatively, a regression-based approach could be desirable when qualitative evidence and/or institutional knowledge on the part of the researchers indicate the treatment may not be random or as-if random. Such an analytical strategy would enable a comparison between the findings from the difference-of-means or difference-of-percentages test with the estimates obtained through a multivariate regression containing the covariates that are presumed to correlate with the treatment. In the regression-based approach, the population regression function is:

$$y = \beta X + \gamma t + \lambda D + \delta t D + u \tag{1}$$

In Equation **??**, β represents the coefficient for a vector of covariates X; γ represents the coefficient

for the time trend common for treatment and control group; λ represents the systematic difference in the outcome across the treated and the control cases (group-specific time-invariant difference); and δ represents the coefficient for the interaction of the group and time variable, which estimates the average causal effect of the treatment on the outcome y; and u represents the error term (Imbens & Wooldridge, 2009).

Plausibility of the Assumptions of the Standard Natural Experiment. The analysis of the standard natural experiment using the Neyman framework builds on two assumptions. First, units are randomly or as-if randomly assigned to the treatment. In order to provide evidence of the quality of the (as-if) randomization, Dunning (2012) recommends conducting balance tests. Currently, many researchers use mean-difference tests (t-tests) to analyze variations between units in the treatment and control group along relevant pre-treatment covariates. However, this approach is sensitive to the sample size, i.e., even small differences become statistically significant if the sample size is large. To reduce this problem, scholars can use normalized differences, which are unaffected by sample size (for a detailed discussion of how to calculate the normalized differences, see Imbens & Wooldridge, 2009). A further disadvantage of mean-difference tests is that it is difficult to determine whether the control and treatment groups are balanced. For instance, if researchers conduct t-tests for 20 covariates, we would expect to find at least one statistically significant difference simply as the result of chance. Therefore, we suggest conducting joint hypothesis tests, for example by regressing the binary treatment variable on the covariates and using a χ^2 test to analyze whether the coefficient of the covariates differs from zero (McKenzie, 2015).

Second, the Neyman framework builds on the assumption that the outcomes of a unit are only influenced by the unit's treatment-assignment status. This assumption is also called the noninterference assumption or the "stable unit treatment value assumption (SUTVA)" (Imbens & Rubin, 2015). SUTVA refers to a situation in which the treatment status of other units affects a unit's outcome. For instance, a company aims to analyze whether leadership training increases the effectiveness of their leaders. For this reason, the company assigns some leaders to a leadership training programme (treatment group), whereas other leaders receive no training (control group). However, because leaders work together within the same company and interact with each other, leaders in the treatment group may share some of the knowledge they learned in the training programme with leaders in the control group. This spillover violates the SUTVA and leads to an underestimated average causal effect (Morgan & Winship, 2015). Unfortunately, there is currently no clear solution to SUTVA violations, though Belloc et al. (2018) used corrected standard errors to deal with such possible violations, and Selb and Munzert (2018) excluded units in the control group which were in close spatial proximity to treated units, because these units were particularly likely to be treated by accident.

Since there is no 'solution' to the SUTVA violation, we recommend that (i) both the statistically adjusted and unadjusted estimation results are reported, as this may provide insights into the extent to which a possible SUTVA violation affects the results, and (ii) the role of social interactions needs to be

to explicitly taken into account, which could reveal important boundary conditions for the effect under examination (see Sinclair, 2011).

Testing the Robustness of the Results of the Standard Natural Experiment. There are several options to test the robustness of the results of a standard natural experiment. Firstly, the results can be tested for robustness if they include pre-treatment covariates in the analysis. For instance, Beaman and colleagues (2012) showed the results of both difference-in-means tests and ordinary least squares coefficients adjusted for covariates despite random assignment of women to a leadership position. Although the inclusion of covariates is unlikely to have a high impact on the average causal effect in a natural experiment with high plausibility of as-if random assignment, adding covariates—especially those that are unbalanced between the treatment and control group—increases the transparency and credibility of the results in the eyes of other researchers. Therefore, the best approach is to estimate the average causal effect with and without covariates and if there are significant differences then potential reasons for these differences should be explored in more detail.

Secondly, if the passing of a new law, regulation or quota is at the basis of the experiment, then Matsa and Miller's (2013) approach should be followed, i.e., by conducting additional tests for different types of units—e.g., units that almost complied with a quota or law before the inception, and units that had a large distance from compliance. Matsa and Miller argue that they would expect greater effects for firms with a greater distance from compliance with the gender quota than for firms that almost complied with the quota. Such an additional test is important because it can provide further evidence that the observed effect is caused by the new law, regulation, or quota and not by an unrecognized event that affected the units in the treatment group.

Thirdly, Matsa and Miller's (2013) also use matching methods (e.g., propensity score matching). Matching methods should be used if units in the treatment and control group differ from each other with regard to several (observable) covariates. The covariates can be used to match treated units with control units that are highly similar with regard to the observed covariates. Although matching methods are no replacement for an as-if randomization, because they do not ensure that units in the treatment and control groups do not differ with regard to observed *and* unobserved covariates, these methods can provide further insights into the robustness of the initial results.

4.3.2 Analyzing Instrumental Variable Designs

Estimation of the Average Causal Effect in the IV Design. In the IV design (see Figure **??**), we estimate the causal effect of an endogenous treatment variable D by identifying an instrument z that affects D but is otherwise unrelated to the outcome y (Abadie & Cattaneo, 2018). The key idea of the IV design is that we only retain the variation in the treatment that is caused by an exogenous variation in the instrument, while ruling out the association between the treatment and possible covariates A and B (see Figure **??**). For instance, in hist study on the effects of military service on future earnings, Angrist (1990) isolates the variation in the treatment (i.e., military service) that was caused by an exogenous

variation (i.e., Vietnam draft lottery) of the instrument (i.e., eligibility of being drafted for military service).

The Wald estimator is the simplest and most transparent way to estimate the average causal effect in the IV design (Dunning, 2012):

$$\rho = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} \tag{2}$$

The IV estimate ρ in Equation **??** equals the difference in the outcome variable between treated $E[Y_i|Z_i = 1]$ and $E[Y_i|Z_i = 1]$ controls divided by the ratio between treated $E[D_i|Z_i = 1]$ and controls $E[D_i|Z_i = 1]$. Such an equation provides some first insights into the IV estimate ρ.

While the Wald estimator should be applied for estimations with a single instrument without any covariates, researchers recommend using the two-staged least squares (2SLS) estimator for multiple instruments or when covariates are added (Angrist & Pischke, 2009). In the latter case, we use two steps to estimate the causal effect of the treatment. In the first-stage (equation 3), we regress the treatment variable D on a vector of covariates X and on the exogenous instrument Z:

$$D = \gamma_1 X + \gamma_2 Z + v \tag{3}$$

In the second-stage Equation **??**, we regress the outcome variable (Y) on the covariates (X) and predicted values of the treatment from the first-stage regression:

$$y = \beta_1 X + \beta_2 \widehat{D} + u \tag{4}$$

where β_2 denotes the average causal effect of the treatment.

Evaluation of the Plausibility of the Assumptions of the IV Design. The IV design relies on several assumptions. In line with Angrist & Pischke (2009), we emphasize the following elements: (i) exogeneity of the instrument; (ii) relevance of the instrument; and (iii) the exclusion restriction.[12] The first assumption—exogeneity of the instrument—means that the instrument is uncorrelated with the error term u, included in Equation 4 . In other words, the instrument is assumed not to be related to the causes of the dependent variable (Sovey & Green, 2011). Arguing this assumption is consistent with a target population regression function, and the dataset at hand is particularly problematic. In fact, it is not possible to empirically test the exogeneity of an instrument (Wooldridge, 2009). Furthermore, recent simulation studies indicate that endogenous instruments produce causal effect estimates "that are inferior to those reported by OLS regression" (Semadeni et al., 2014, p. 1071). These concerns

[12]Please note that assumption (i) and (iii) are often combined. In the interest of clarity, we follow Angrist and Pischke (2009) and separately consider each assumption.

become less accentuated when units are randomly assigned to the instrument (Dunning, 2012), as in the case of lotteries (Angrist, 1990). Even if the assignment is not truly random, one can still argue for the exogeneity of the instrument. For instance, Bennedsen and colleagues (2007) point out that their instrument—the gender of the firstborn child—is as-if random and they expand on qualitative evidence by observing that technologies to identify the child's gender before the birth were not widespread at the time, so that abortion due to the child's gender was unlikely. These arguments provide convincing evidence for the exogeneity of the instrument, despite the lack of a direct test.

The second assumption—the relevance of the instrument—implies that Z and D are correlated. Specifically, instruments can be categorized into weak, moderate and strong according to the magnitude of the $Z-D$ correlation. In order to assess an instrument's strength, the canonical test can be used which is based on the F-statistic of the first-stage regression (Semadeni et al., 2014).[13] Exploiting the canonical test, Olea and Pflueger (2013) developed a weak-instrument test, which is robust to heteroscedasticity, autocorrelation, and clustering, and which is more efficient than the standard Stock and Yogo (2005) test. Strong instruments should be the norm in the IV design, because weak and moderate instruments lead to inflated standard errors, although the estimated regression slopes are unbiased (Semadeni et al., 2014).

The third assumption—exclusion restriction—means that Z has no influence on y apart from the effect that is conveyed through D (Sovey & Green, 2011). Similarly to the exogeneity assumption, the exclusion restriction cannot be assessed based on the available data. In fact, it cannot be proved that the instrument—even when it results from a true randomization— does not affect the dependent variable through alternative causal pathways (Morgan & Winship 2015). Instead, the best approach would be to: (i) critically analyze possible theoretical mechanisms through which the instrument may be related to the dependent variable, and (ii) provide both logical arguments and qualitative evidence that help to rule those mechanisms out (Sovey & Green, 2011).

Testing the Robustness of the Results o f the IV Design. Leadership scholars have at least two alternatives to assess the robustness of an IV design's results. First, estimated regression slopes could be compared across models building on alternative instruments. For instance, Chintrakarn et al. (2017) use the degree of religious adherence in the population surrounding a firm's corporate headquarters in 1971 as instrument in their main analyses; then, they provide a second model using a twenty-year lag of the original instrument.

Second, sometimes the instrument influences the assignment to the treatment, whereas it does not capture the timing of the treatment. For instance, Bennedsen et al. (2007) explained that observed differences in firm performance between firms with a family CEO and firms with an outside CEO may also result from differences in the timing of CEO succession (e.g., CEOs may retire at a different age). Therefore, the authors check their initial results by estimating the model on a sub-sample of the data—i.e., the instances in which the CEO transition occurred while the incumbent CEO is in the 'normal' retirement

[13] Interested readers find an overview of critical values for the weak instrument test in Stock and Yogo (2005).

age. They even identified an additional instrument, CEO death, to test whether their findings are robust when the timing of CEO succession is credibly exogenous. These rigorous robustness checks help to rule out alternative explanations for the observed relationships and strengthen the causal inference.

4.3.3 Analyzing Regression Discontinuity Designs

Estimation of the Average Causal Effect. Before starting to analyze the regression discontinuity design, leadership researchers need to check whether the natural experiment represents a sharp or a fuzzy regression discontinuity design. The sharp RD design is characterized by a perfect compliance of the units; that is, all units above the threshold receive the treatment and all units below the threshold are assigned to the control group (or vice versa). In the fuzzy RD design, not all units with a score above the threshold receive the treatment; instead, a score above the threshold merely influences the probability that a unit will receive the treatment (Lee & Lemieux, 2010). Our discussion in this section focuses on the sharp RD design, because the fuzzy RD design has already been discussed in the section "Analyzing Instrumental Variable Designs."[14]

The analysis of the sharp RD design follows the principles of simplicity and transparency (Dunning, 2012). As shown in Figure **??**, we require two variables for the analysis: X and x^*. X represents the assignment variable and X_i represents the score of unit i on the assignment variable; x^* denotes the threshold or cut-off point for the assignment of the treatment. Based on X_i and x^*, we can create a binary treatment variable, tD, that equals 1 if $X_i >= x^*$ and 0 if $X_i < x*$ (please note that whether the treatment is assigned if $X_i >= x^*$ or $X_i <= x^*$ depends on the empirical context; we assume here that the treatment is assigned if the score on the assignment variable exceeds the threshold). The treatment variable is crucial for determining the discontinuity. The regression model for the RD design is shown in Equation **??**:

$$y_i = \alpha + \delta tD + \beta(X_i - x*) + u \tag{5}$$

The crucial quantity of interest is the treatment effect, tD. The coefficient δ indicates the discontinuity at the threshold ($x*$), which represents the average causal effect of the treatment (Dunning, 2012). The coefficient β represents the continuous effect of the assignment variable, which is centered around the value of the threshold x^*. Researchers suggested that the slope of the regression should be allowed to differ between the control group and the treatment group (e.g., Antonakis et al., 2010; Lee & Lemieux, 2010) by including an interaction between the treatment variable and the assignment variable as shown in Equation **??**:

$$y_i = \alpha + \delta tD + \beta(X_i - x^*) + \tau tD(X_i - x^*) + u \tag{6}$$

[14] Please note that the fuzzy RD design resembles an instrumental variable design in which the assignment variable is the instrument (Lee & Lemieux, 2010).

The coefficient τ represents the interaction between the assignment variable and the treatment assignment and indicates whether the slope differs between the control group and the treatment group.

Evaluation the Plausibility of the Assumptions of the RD Design. Compared to the instrumental variable design and the standard natural experiment design, the RD design is based on mild assumptions (Lee & Lemieux, 2010). The crucial assumption underlying the RD design is that units cannot *precisely* manipulate their score on the assignment variable (Lee & Lemieux, 2010), because it ensures that units close to the threshold are almost randomly assigned to the treatment and the control condition. This assumption cannot be directly validated. However, leadership scholars can conduct various tests that may falsify the assumption. For instance, Arvate and colleagues (2018) tested (i) the balance on covariates between units closely below and above the threshold, and (ii) the density of the assignment variable.

Firstly, testing the balance on pre-treatment covariates between units closely below and above the threshold provides insights into the quality of the as-if randomization. If units cannot precisely manipulate their score on the assignment variable, we would expect that units closely above the threshold do not systematically differ from units closely below the threshold with regard to pre-treatment covariates (Cattaneo et al., 2019). The balance test as described for the standard natural experiment can be used to test the plausibility of the as-if randomization. Although we suggest using different bandwidths around the threshold to check the robustness of the test, leadership scholars should consider that observations in the treatment and control group with a greater distance from the threshold will be more likely to differ from each other than observations close to the threshold.

Secondly, to further check the assumption that units were not able to precisely manipulate their score on the assignment variable, McCrary's (2008) test can be carried out. The test assumes that if units have imprecise control over their score, we would expect to find that the density of the assignment variable would be continuous. Conversely, a jump in the density around the threshold could be a sign of a unit's ability to manipulate the assignment variable (McCrary, 2008). Together, the results of these two tests can help leadership scholars to provide evidence for the validity of the identifying assumption in the RD design.

Testing the Robustness of the Results of the RD Design. A further important step in the RD design is to check the robustness of the results given that various decisions may affect the size of the average causal effect. Specifically, three decisions have received considerable attention in the literature: (i) the inclusion of covariates; (ii) the selection of the bandwidth; and (iii) the inclusion of higher-order polynomials.

First, leadership researchers need to decide whether they should include covariates in the analysis. In RD designs, the inclusion of covariates is not as straightforward as in correlational studies. Due to the as-if random assignment of units into treatment and control groups near the threshold, a consistent estimate of the discontinuity can still be obtained without including covariates (Lee & Lemieux, 2010).

Nevertheless, the current consensus is that covariates should be included in RD designs— especially if covariates are discontinuously distributed at the cut-off of the assignment variable—because they may reduce variance and eliminate bias in the average causal effect (Frölich & Huber, 2018).

We recommend scholars to estimate the average causal effect both with and without covariates. In a 'strong' RD design, the difference in the average causal effect in both settings should be minimal given the as-if random assignment. Yet, including covariates may provide insights into the robustness of the results and is especially recommended if the whole range of observations is included, i.e. even observations far away from the threshold (Imbens & Lemieux, 2008).

Second, leadership researchers need to select a bandwidth, i.e., a range of values around the threshold that should be included in the analysis. Selecting the bandwidth is a crucial decision, because the results are often sensitive to the bandwidth (Cattaneo et al., 2019; Imbens & Lemieux, 2008). Larger bandwidths have the advantage of reducing the variance in the coefficient of the discontinuity, because of the use of more observations, whereas smaller bandwidths reduce the likelihood of mis-specifying the local polynomial (Cattaneo et al., 2019).

Although in most empirical contexts there is no 'objective' bandwidth, there are, however, several statistical approaches for selecting an optimal bandwidth (e.g., Cattaneo & Vazquez-Bare, 2016; Imbens & Kalyanaraman, 2012).Whatever statistical approach or bandwidth selection criteria, we strongly recommend following Imbens and Lemieux (2008) and testing the sensitivity of findings in relation to different bandwidth choices (e.g., twice and half the size of the original bandwidth). This approach will reveal the level of robustness of the average causal effect and may increase the credibility of the findings (see, e.g., Arvate et al., 2018).

Thirdly, researchers debate the use of higher order polynomials of the assignment variable in RD designs. Some studies include high-order polynomials (e.g., fourth- or fifth-order polynomials) of the assignment variable in the regression to smoothen the regression function (Lee & Lemieux, 2010). However, Gelman and Imbens (2018) argue against this practice—unless there are strong theory-based reasons—because estimates become noisier and the results are sensitive to the choice of the high-order polynomials. Instead, they recommend using local low-order polynomials (linear or quadratic) in RD designs, which have a much lower variation in the estimates. We recommend the approach described by Lee and Lemieux (2010), who suggest analyzing the robustness of the average causal effect to changes in the inclusion of higher order polynomials both for a small and wide window around the threshold. Again, this approach provides further insights into the robustness of the findings and may increase the credibility of the RD design.

In addition to checking the robustness of the RD results for the three decisions, two types of placebo tests are also worth conducting. First, placebo cut-off tests check for a discontinuity at cut-off points where no treatment should have been assigned. Finding a discontinuity at a placebo cut-off may indicate confounding effects in the RD design. To test for the presence of multiple treatments, Imbens

and Lemieux (2008) found a good approach by splitting their sample into two sub-samples: sub-sample 1 includes all observations on the left of the initial cut-off point (x^*); sub-sample 2 includes all observations on the right of x^*. In each sub-sample, scholars should use the median value of the assignment variable as the placebo cut-off, as this approach maximizes statistical power. The same regression function can be used to run the placebo test as shown in equation 6. In this case, however, tD, which represents the placebo treatment variable, ideally does not differ from zero in either of the sub-samples, which would indicate that no discontinuity is found at the placebo cut-off point.

5 Concluding Remarks

Identifying causal relationships is becoming increasingly important for leadership scholars and experimental designs play a key role in this endeavour (Antonakis 2017; Antonakis et al., 2010; Podsakoff & Podsakoff, 2019). The aim of this paper was to complement the recent experimental turn in leadership research by introducing natural-experimental designs and discussing their potential for inferring causal relationships in leadership research.

Although this paper focuses on the potential of natural experiments and their implementation, it is also important to discuss some limitations (see also Harrison & List, 2004; Sekhon & Titiunik, 2012). First, a natural experiment is only as good as the plausibility of the as-if randomization. If the as-if randomization is plausible, we can assume that the internal validity of a natural experiment is almost as high as the internal validity of a laboratory or field experiment. However, if the as-if randomization is not plausible, then the internal validity of a natural experiment is rather low. Therefore, leadership scholars need to critically evaluate the quality of the as-if randomization—based on quantitative and qualitative evidence (Dunning, 2012).

Second, although natural experiments take place in a natural field setting, which guarantees their ecological validity, the external validity of natural experiments could be open to question. Often, the setting of a natural experiment is unique, or the interventions apply to a very specific group, which poses the question as to whether the findings can be generalized to other populations in other contexts (Dunning, 2012). To overcome this limitation, natural experiments can be combined with observational studies.

Finally, we need to emphasize that a 'good' natural experiment is no replacement for a 'good' research question. That is, leadership scholars need to consider that a natural experiment is just a tool to infer causal relationships; it is not an end in itself.

To sum up, the aim of this paper was to introduce the natural-experimental design to leadership research. Although we have tried to cover important parts of the literature regarding natural experiments, we urge scholars interested in applying a natural experiment to additionally consult the literature on the specific design (i.e., standard natural experiment, IV design, RD design). We hope that this paper will stimulate the use of natural experiments in leadership research and will be a useful addition to the

'experimental tool box' of leadership scholars.

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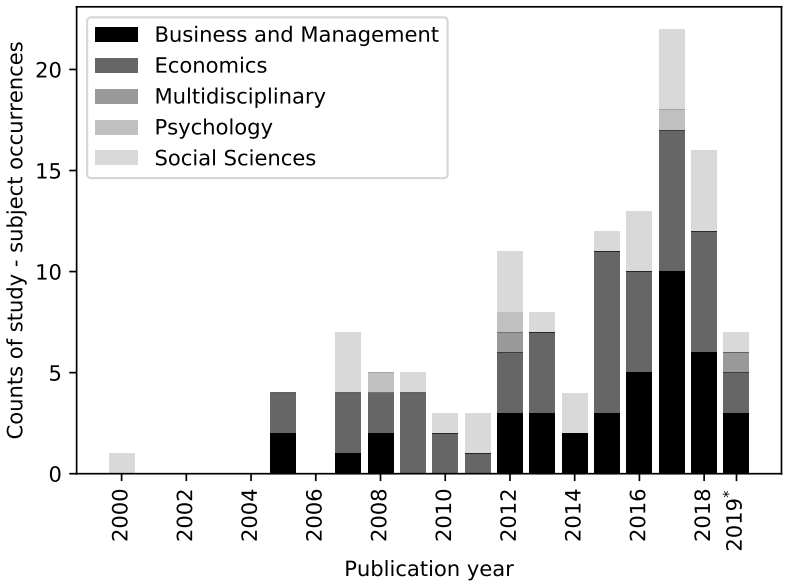
Appendices

A Retrieved Set of Studies

The set of exhibits included in this Appendix A provides some descriptive information on the reviewed articles. Specifically, Figure **??** illustrates the distribution of the studies with respect to the disciplinary domain (as per the Scopus categories) and time. Table **??** details the source of the studies.

Fig. A.1: Counts of Retrieved Studies - Disciplinary Subjects Occurrences over Time



Notes.—Disciplinary subjects as per the Scopus database. * '2019' data concern the first quarter of the year only.

TABLE A.1
Counts of Retrieved Studies by Journal (Alphabetical Order)

Journal	Count of Studies
Accounting Review	1
Accounting and Business Research	1
Accounting and Finance	1
Advances in Financial Economics	1
American Economic Journal: Applied Economics	4
American Economic Journal: Economic Policy	1
American Economic Review	1
American Journal of Political Science	4
American Journal of Sociology	1
American Political Science Review	3
Applied Economics	1
Applied Economics Letters	2
British Accounting Review	1
Comparative Political Studies	3
Corporate Governance: An International Review	1
Corporate Ownership and Control	1
Economica	2
Electoral Studies	3
European Journal of Economics, Finance and Administrative Science	1
Financial Management	2
Financial Review	1
Gender in Management	1
Human Resource Management	1
Industrial and Corporate Change	1
Industrial and Labor Relations Review	1
International Interactions	1
International Journal of Social Economics	1
International Studies Quarterly	1
Journal of Accounting and Economics	1
Journal of Banking and Finance	2
Journal of Business Ethics	2
Journal of Business Finance and Accounting	2
Journal of Business Research	2
Journal of Business Venturing	1
Journal of Comparative Economics	1
Journal of Corporate Finance	4
Journal of Economic Behavior and Organization	1
Journal of Empirical Finance	1
Journal of Finance	1
Journal of Financial Economics	2
Journal of Financial Research	1
Journal of Labor Economics	1
Journal of Management	1
Journal of Public Economics	2
Leadership Quarterly	2
Management Science	2
Political Psychology	1
Proceedings of the National Academy of Sciences	1
Public Administration	1
Public Administration Review	1
Public Choice	1
Quarterly Journal of Economics	2
Review of Economic Studies	1
Review of Financial Studies	1
School Effectiveness and School Improvement	1
Science	1
Small Business Economics	3
Social Science Quarterly	1

B Topic Modelling of Abstracts

The natural language processing pipeline behind our topic model comprises several steps (see the Jupyter notebook attached to the submission as supplemental material). In the first step, we use the Python library spaCy to pre-process the data. Specifically, we perform the following set of string manipulations:

- tokenization—sentences invo lved in the 1,156 abstracts are segmented into words, numbers, punctuation;
- lemmatization—base form of a word are applied. For example, the lemma of 'had' is 'have';
- token removal—numbers and stop words (i.e., words that provide limited information about the meanings conveyed by a piece of text) are filtered-out.

In the second step, we leverage the Python library Gensim to create the input for the topic model, namely the *dictionary* (i.e., the set of unique tokens involved in the corpus of abstracts) and the *corpus* (i.e., a matrix containing the numeric transformation of each individual abstract in terms of the set of unique tokens included in the dictionary).

In the third step we use Mallet software to estimate a set of competing topic models, each of which retains a unique number of topics (ranging from 10 - 29). We set the maximum number of topics equal to the number of categories included in Garnder and colleagues (2010). Considering the internal validity of the models (DiMaggio, Nag, & Blei, 2013) and the coherence score—a statistical metrics that expresses the face validity of the inductively derived topics (see Mimmo, Wallach, Talley, Leenders & McCallum, 2011)—we retain the model with ten topics. The pattern of topics associated with the best fitting model is shown in Figure B1. The left-hand side of the chart employs multidimensional scaling to offer a low-dimensional representation of the relationships among the eleven topics. The right-hand side of the visualization reports the set of the thirty most salient terms involved in the topic model. The live version of this visualization—available in html format as supplemental materials—provides additional insights regarding the pairing structure linking terms and topics. For example, the set of the most salient terms changes as one moves the cursor over the bullets associated with the topics.

Finally, we use the topic model trained with Mallet to characterize each of the 87 studies included in the review along the various topics. This enables us to see how natural experiment methods map onto the space of leadership phenomena and theories (at least as represented in The Leadership Quarterly).

The data and Python code behind the paper are publicly available within the Github repository natural-experiment.

References included in the Appendix

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Fig. B.1: Visualization of the Topic Modelling Outcome