

# Do quantitative and qualitative research reflect two distinct cultures? An empirical analysis of 180 articles suggests “no”.

published in *Sociological Methods & Research*

Gold open access: <https://doi.org/10.1177/00491241221082597>

This is the accepted, non-proofread preprint.

David Kuehn

(GIGA Institute of Global and Area Studies, Hamburg, Germany, david.kuehn@giga-hamburg.de)

<https://orcid.org/0000-0003-2593-0194>

Ingo Rohlfing

(University of Cologne, Cologne Center for Comparative Politics)<sup>1</sup>

<https://orcid.org/0000-0001-8715-4771>

## Abstract

The debate about the characteristics and advantages of quantitative and qualitative methods is decades old. In their seminal monograph, *A Tale of Two Cultures* (2012, *ATTC*), Gary Goertz and James Mahoney argue that methods and research design practices for causal inference can be distinguished as two cultures that systematically differ from each other along 25 specific characteristics. *ATTC*'s stated goal is a description of patterns in empirical quantitative or qualitative research, but it precludes a systematic empirical evaluation as to whether the 25 characteristics are relevant and valid descriptors of the applied research. In this paper, we derive five observable implications from *ATTC* and test the two-cultures hypothesis. With this, we contribute to a better understanding of the choices quantitative and qualitative scholars do in the course of their empirical research. We test the implications against a stratified random sample of 90 qualitative and 90 quantitative articles published in six journals between 1990 - 2012. Our analysis provides little support for the “two-cultures” hypothesis. Quantitative methods are largely implemented as described in *ATTC*, whereas qualitative methods are much more diverse than *ATTC* suggests. While some practices do indeed conform to the qualitative culture, many others are implemented in a manner that *ATTC* characterizes as constitutive of the quantitative culture. We find very little evidence for *ATTC*'s anchoring of qualitative research with set-theoretic approaches to empirical social science research. The set-theoretic template clearly applies to a fraction of the qualitative research that we reviewed, with the majority of qualitative work incorporating different method choices.

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<sup>1</sup> Alphabetical order. Both authors contributed equally. Ingo Rohlfing is author for correspondence. Reproduction material available at <https://doi.org/10.17605/OSF.IO/6UHPD>.

The debate about the relative pros and cons of quantitative and qualitative methods is decades old and continues to be lively (Gerring 2017; Mahoney 2010). Within this debate, the monograph, *A Tale of Two Cultures (ATTC)*, takes stock of and offers a novel perspective by mapping ‘typical practices’ in the application of qualitative and quantitative methods in political science and sociology (Goertz and Mahoney 2012, 10). ATTC argues that qualitative and quantitative research practices can best be characterized as two *cultures* that systematically differ from each other. Henceforth, we refer to this as the *two-cultures hypothesis* (or argument). The cultures are argued to be based on different ‘values, beliefs, and norms’ and are ‘associated with distinctive research procedures and practices’ (Goertz and Mahoney 2012, 1). Research practices are broadly construed, with five dimensions of empirical research subsuming 25 individual items that capture a variety of decisions and choices researchers must make in their work.<sup>2</sup> The 25 items comprise fundamental beliefs about the nature of causality (symmetric vs asymmetric), the formation of concepts, various research design elements and practical decisions such as how to choose cases. For ease of discussion, we refer to them as matters of method implementation, application or practice. The claim of fundamental differences between qualitative and quantitative methods is hardly novel in the social sciences (for example, Brady and Collier 2010). However, ATTC delivers by far the most articulate and comprehensive discussion, making one of the strongest claims about the scope and relevance of these differences.

In this paper, we perform a comprehensive empirical test of the two-cultures hypothesis with the goal of moving forward the debate about qualitative and quantitative methods practices. A test of the two-cultures hypothesis is important because there are three related reasons that ATTC influences empirical and methods-oriented research.<sup>3</sup> First, the two-cultures hypothesis is referenced in empirical and methods-related research as if it was an empirical fact rather than a hypothesis (Blatter and Haverland 2013; Koivu and Damman 2015; Schneider and Wagemann 2013).<sup>4</sup>

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<sup>2</sup> One could question whether ‘common practices’ qualify as a methods culture (Brady 2013, 253; Goertz and Mahoney 2013a, 282). For the sake of clarity, we follow ATTC’s terminology.

<sup>3</sup> According to Google Scholar, the foundational 2006 article that presented the two-cultures argument for the first time (Mahoney and Goertz 2006) has been cited 1329 times. The ATTC monograph has been cited 1194 times (as of 22 October 2021).

<sup>4</sup> We searched Google Scholar for article citations of ATTC in 2016 (search date: 2017-09-12). 2016 is a reasonable choice of year because ATTC was published in 2012. Taking into account review and journal production times, authors of articles published in 2016 should have had the opportunity to read ATTC and know the content. We collected 54 articles (we excluded other formats) and coded them according to whether they reference ATTC with regard to best methods practices, common practice or in a different context (ambiguous meaning of citation or another issue). 11 citations of ATTC are about common practices.

Any hypothesis needs to be evaluated in light of empirical evidence before it is accepted as being probably correct.

Second and related, discussants of ATTC express doubts that it correctly describes methods applications (Brady 2013; Elman 2013; Rohlfing 2013). These doubts, however, have not been substantiated with systematic empirical evidence. Arguments for and against the existence of two cultures have thus far been confined to illustrative examples that are easy to find for both proponents and sceptics of the two-cultures argument.<sup>5</sup> We aim to overcome this impasse with a comprehensive empirical test.<sup>6</sup>

Third, ATTC is at times cited as if it was suggesting *best practices* in the application of methods, and is criticized for promoting incorrect best practices (Beach and Pedersen 2013b).<sup>7</sup> ATTC cannot be blamed for such misreadings because it makes very explicit that its main focus is *description*, not *prescription*. Nonetheless, an empirical analysis of how methods are applied and whether this conforms to the two-cultures hypothesis is important because it highlights what ATTC is about and might help to correct any misinterpretations. Our article does not aim to make prescriptive arguments about method application in political science, of which there already quite a number, but to get a better understanding about how it is done.<sup>8</sup>

The present analysis goes significantly beyond a previous empirical test of the two-cultures hypothesis by Kuehn and Rohlfing (2016), both conceptually and empirically. In conceptual terms, the 2016 study focuses on a single observable implication: namely whether research practices in published qualitative and quantitative articles conform to the descriptions in ATTC. In this article, we formally derive four additional implications from the two-cultures hypothesis and test them. Empirically, the earlier paper is based on just 30 articles, 15 quantitative and 15 qualitative, from

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<sup>5</sup> Schwemmer and Wiczorek (2019) conducted an automated text analysis of the abstracts of over 8000 sociology articles. They found that qualitative and quantitative research differ systematically in regard to their preference for scientific practices; research questions; and the use of certain paradigms. These differences increase over time. This analysis hints at differences between qualitative and quantitative research, but does not provide evidence for the much more specific and involved two-cultures hypothesis.

<sup>6</sup> On a more general level, we take stock of the application of methods and present data on what researchers do when answering causal research questions. The analysis contributes to what is, to our knowledge, a small number of studies that have systematically evaluated method practices in the social sciences and its subfields (Bollen et al. 1993; Bunea and Baumgartner 2014; Coppedge and Kuehn 2019; Groeneveld et al. 2014; Munck and Snyder 2007; Pepinsky 2019). An evaluation of method practices in ethnographic research is presented by Abend, Petra and Sauder (2013).

<sup>7</sup> 16 citations of ATTC are about best practices (see footnote 4).

<sup>8</sup> Readers who are interested in prescription can use the results to compare their individually defined terms of best practice with current practices, as reflected in our data.

three journals (*Comparative Political Studies*, *European Journal of Political Research*, *World Politics*), published between 2008 and 2012. This article is based on a research sample of 180 articles plus a sensitivity test involving 30 additional qualitative articles. In terms of methods, the earlier analysis only cross-tabulates observed methods practices across all 30 articles against the type of method (qualitative or quantitative) that is applied in a study. The cross-tabulations cast doubt on the validity of the two-cultures hypothesis because the distribution of method practices deviates from the expectations. However, an analysis of the 30 articles can only be considered preliminary because of the small sample size, its limitation to a single observable implication, and the simple empirical approach.

We proceed as follows. After a brief summary of the two-cultures argument (section 2), we derive five observable implications for which we should find empirical evidence if the hypothesis was correct (section 3). The empirical analysis is based on an in-depth content analysis of a random sample of 90 qualitative and 90 quantitative empirical articles that are stratified by three time periods and six major journals covering different subfields of political science (section 4).<sup>9</sup> The authors coded each article independently of each other with respect to the 25 methods practices as defined in ATTC (see also Appendix C). The codes constitute the data that we use to assess the five observable implications in sections 5 to 9.

We find that none of the five observable implications is fully empirically supported. Quantitative methods practice largely meets ATTC's characterization of the quantitative culture. However, we also find a number of exceptions, such as the modeling of interactions in a sizeable share of quantitative articles, which is theorized to be a feature of qualitative research. Qualitative research is more diverse than expected and often does not display the characteristics of the qualitative culture. While some practices do indeed comply with the qualitative culture (e.g., the typical number of cases studied), many others are implemented in a manner that ATTC characterizes as constitutive for the quantitative culture. We believe that the disagreement between our empirical findings and the expectations formulated in ATTC is due to its fundamental assumption that qualitative research is anchored in set-theoretic approaches to research design. This assumption is reflected, for example, in how ATTC discusses the asymmetric conceptualizations of variables; causal rela-

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<sup>9</sup> The journals are the *American Journal of Political Science*, *American Political Science Review*, *Comparative Political Studies*, *European Journal of Political Research*, *International Organization* and *World Politics*. The periods are 1990-1994, 2000-2004 and 2008-2012.

tions in terms of necessity and sufficiency; and the conjunction of conditions that produce the outcome. Moreover, among the 25 indicators for the qualitative culture, ATTC includes several methods practices that are relevant to one specific subset of set theoretic methods, namely Qualitative Comparative Analysis (QCA), but are not relevant for research practices in non-QCA qualitative research. These include inter alia the organization of data in truth tables (item 15), and the interpretation of triangular data as evidence of set relations (item 16).<sup>10</sup> In total, nine of the 25 qualitative research practices are defined based on the equation of qualitative and set-theoretic research.

Section 10 summarizes the findings and discusses potential alternative explanations for those findings other than the two-cultures hypothesis being incorrect. In particular, we present evidence indicating that the findings are neither the result of a particular bias in the selected journals, nor that the finding is confined to the periods under review. We also address in detail why potentially implicit methods practices, especially in qualitative research, are unlikely to affect our results. The final section concludes by summarizing an exploratory empirical cluster analysis of the subset of all 90 qualitative articles. We find that empirical research in the qualitative tradition is considerably more diverse than the two-cultures hypothesis suggests, as we find evidence for the existence of at least three “sub-types” of qualitative methods applications, as seen in articles based on set-theoretic assumptions constituting the numerically smallest cluster.

## **2. The two-cultures hypothesis**

The core argument of ATTC is that the application of methods in the social sciences systematically differs in qualitative and quantitative empirical research. The claim of systematic differences has been made many times before (for example, Beach and Pedersen 2013a, chapter 2; Collier et al. 2004; George and Bennett 2005, chapter 1). ATTC goes significantly beyond previous work and contends that the two methods follow different, coherent practices that can be located on five dimensions, capturing a total of 25 individual design and method decisions (see Appendix A).

In a stylized perspective, the argument is that qualitative research is about explaining the outcome of individual cases; follows a set-relational understanding of causality; tends to engage in in-depth analysis of a small number of cases with the aim of generalizing findings to a narrowly defined context; understands the measurement of the operational variables as a semantic process

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<sup>10</sup>This is despite the fact that QCA per se does not play a prominent role in ATTC. We count only ten explicit references to QCA in the monograph.

of defining and bringing into logical relationships a multitude of concept dimensions; and develops asymmetrical causal arguments. In contrast, quantitative research does not primarily aim to explain the outcome of individual cases; understands causality in terms of the average treatment effect of individual variables and, only rarely, interaction terms; quantitative studies usually cover a large number of cases to allow for broad generalizations; conceives of variables as latent variables that are operationalized with indicators; and develops 'symmetric causal arguments in which the same variables and model explain the presence versus absence of an outcome' (Goertz and Mahoney 2012, 225).

ATTC is exemplarily clear in describing the two cultures and defining their dimensions and constitutive attributes. At the same time, the discussion falls short of demonstrating that the two cultures are empirically valid descriptions of qualitative and quantitative empirical research. The book provides well-chosen discussions of selected methods applications and examples that fully illustrate the two-cultures argument. However, these illustrations do not constitute the kind of empirical evidence that is necessary to making a convincing case for the empirical correctness of the two-cultures hypothesis.

The appendix of ATTC provides a survey of 216 articles from the *American Journal of Sociology*, *American Political Science Review*, *American Sociological Review*, *Comparative Politics*, *International Organization*, and *World Politics*. Substantively, it identifies whether an article uses predominately qualitative or quantitative methods and what type of method in particular (case studies, QCA, OLS regression, etc.). This survey is an insightful first step, but does not constitute a systematic evaluation of the much more nuanced thesis of two quantitative and qualitative cultures that differ from each other with regard to the 25 method practices.

### **3. Five observable implications of the two-cultures hypothesis**

In essence, the two-cultures hypothesis is a statement about common method practices in the social sciences that can be tested empirically. However, the main hypothesis is too general and broad for a direct empirical test. In this section, we use it as the basis for deriving five observable implications for which we should find empirical evidence if the two-cultures hypothesis was correct.

First, we should observe that the 25 method practices constituting the two cultures are conceptually valid. A methods practice is conceptually valid for the two-cultures hypothesis when it is frequently or, ideally, always observed in quantitative and qualitative research. What is barely

relevant in the practice of empirical research cannot be relevant for distinguishing cultures and is evidence for the inclusion of irrelevant practices into their definitions. The finding that one or multiple practices are rare in empirical work would not disconfirm that qualitative and quantitative methods differ on the subset of practices that are regularly exercised. However, it would suggest that the two methods cultures are not constituted by all 25 practices defined in ATTC. This first implication is the most general one because we only ask *whether* the design and method decisions are empirically relevant as a necessary prerequisite for asking *how* they are implemented. The *how*-question is asked in different ways to motivate the other four implications.

Second, if the two-cultures hypothesis was true, it implies that ‘methods culture’ is a single *latent* dimension underlying the use of methods in empirical qualitative and quantitative research. A confirmatory factor analysis should show that the 25 methods practices can be reduced to a single latent methods dimension.

Third, if the two-cultures hypothesis was true and the 25 methods practices differ systematically *between* and are uniformly implemented *within* quantitative and qualitative research, we should observe strong positive correlations for each pair of methods practices. For example, all qualitative articles should make modest generalizations (practice 12) and choose cases based on theoretical and substantive importance (practice 13), whereas all quantitative articles should generalize broadly and use random samples. Moderate or weak pairwise correlations would undermine the two-cultures hypothesis because it would show that more than a few qualitative articles follow the quantitative culture for a selected practice and that quantitative studies follow the qualitative culture. A test of this implication is useful, regardless of the result of the confirmatory factor analysis in the previous step. The two-cultures hypothesis could be incorrect even if we find a single dimension in our data because many different combinations of practices that do not reflect the two cultures can be reduced to a single dimension.<sup>11</sup> In the absence of evidence for a single dimension, a test of the third implication is also interesting because the pairwise correlation might indicate why there is not a single qualitative-quantitative dimension in the data.

Fourth, if the two-cultures hypothesis was true, we should observe that a large proportion of method and research design decisions are in line with the expectation. This general implication can be specified for the incidence of *one practice over multiple quantitative and qualitative articles*

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<sup>11</sup> For example, in the extreme case, all qualitative articles follow the quantitative culture and all quantitative articles, the qualitative culture. There would be a single dimension in the data, but method and design decisions would be exactly the opposite of what we expected.

and for the prevalence of *all 25 practices within one qualitative or quantitative study*. When we take method practices as the units of analysis, we should, for example, observe that qualitative articles mostly discuss equifinality in their empirical analysis, whereas quantitative articles do not (Goertz and Mahoney 2012, chapter 6). When our unit of analysis is the individual empirical study, we should observe that the 25 method and design decisions largely correspond to the expected culture. For example, we should observe a large share of qualitative articles that explain the outcome of a single case; analyze causal mechanisms; and so on (Goertz and Mahoney 2012, chapter 19). Unless both observable implications are fully correct and the observed proportion of expected practices is 1 across and within articles, there is added value to testing both implications empirically. Suppose we find that a qualitative practice is implemented in 80 percent of all qualitative articles under analysis. This would appear to serve as confirming evidence for the two-cultures hypothesis. However, a share of 0.80 for each practice, for example, allows for the possibility that only 60 percent of the articles display all the qualitative practices.<sup>12</sup> Since we cannot infer the incidence of culture-conforming practices within one article from their prevalence across articles, and vice versa, we formulate implications separately for individual practices and articles.

Both observable implications are important to test regardless of the finding for the third implication. Strong positive correlations between practices are not necessarily evidence of the presence of the expected cultures. A large number of patterns in qualitative and quantitative methods applications can give rise to strong positive correlations and still be in discord with the presence of the two cultures. If we find moderate or weak pairwise correlations, an analysis of the fourth implication would still be of value because the proportions of methods practices in qualitative and quantitative work would show why a correlation deviates from the expectations.

Fifth, following the idea of three waves of qualitative methods development (Goertz and Mahoney 2013b), we should expect the qualitative culture to become more pronounced over time (Goertz and Mahoney 2012, 226). Before the publication of *Designing Social Inquiry (DSI)* (1994), qualitative methods primarily centered on “the comparative method” (Collier 1993; Hall 2008, 308). The focus was on what is now called the cross-case level and causation was inferred via symmetric associations between cause and effect through Mill’s methods (Lijphart 1971). Until the mid-1990s, notions of ‘set theory’, ‘process tracing’ and ‘causal mechanism’ were largely absent.

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<sup>12</sup> To illustrate: Suppose we compare two articles across ten practices A, B, C, etc. to J. Then one article might display practices A to H and the second article, C to J. They would only share six practices, that is, C to H.

This implies that qualitative research should not consistently reflect the qualitative culture during the first wave.<sup>13</sup>

The publication of *DSI* inspired a new wave of work on qualitative methods, which pitted a set-theoretic and asymmetric view against a correlational, symmetric conception of causality. It also shifted attention from comparisons on the cross-case level to process tracing and the analysis of causal mechanisms (Beach and Pedersen 2013a, chap. 2; George and Bennett 2005; Mahoney 2010). These lines of thinking were developed in the 2000s and needed time to influence the practice of qualitative research. The implication is that qualitative articles should start reflecting the qualitative culture more in the early 2000s and most strongly around 2010 (our period of analysis ends in 2012, when ATTC was published).

In contrast, we expect the quantitative culture to be largely invariant over time. Quantitative methods changed substantially over the past 20 years because of an increased interest in causal identification and Bayesian statistics, and the development of new estimation techniques for different forms of data such as panel or multilevel data (Keele 2015; Lebo and Weber 2015). From the perspective of ATTC, however, these developments all occurred within an already established framework of quantitative research and are not related to the constitutive elements of the quantitative culture. We expect to find evidence for a quantitative culture at any point during our research period and little variation in methods applications over time. Taking our expectations as a whole, it follows that qualitative and quantitative methods practices should become more dissimilar over time because of a reorientation of qualitative methods. Table 1 summarizes the five observable implications that we will test empirically in the remainder of this paper.

Table 1: Summary of observable implications

<b>OI</b>	<b>Expectation</b>	<b>Expectation according to ATTC</b>
1	High validity and empirical relevance	All 25 practices used to describe differences between the two cultures are empirically relevant practices in qualitative and quantitative research.
2	Single dimension	Methods practices can be reduced to a single “methods culture” dimension.
3	Strong pairwise correlations	Individual practices should be strongly positively correlated with each other.

<sup>13</sup> The first publication on process tracing that is usually cited is from George and McKeown (1985). However, it took until the mid-2000s for process tracing to become central to empirical and methodological qualitative work (Mahoney 2010).

4	High shares of predicted practices	The proportion of qualitative and quantitative studies that display the expected methods practice should be high for individual practices. The proportion of expected practices should be high for individual articles.
5	Strengthening qualitative culture over time, constantly strong quantitative culture	Differences between quantitative and qualitative methods should become more pronounced over time. Qualitative work increasingly reflects the qualitative culture over time. Quantitative work consistently reflects the quantitative culture.

These observable implications are not equally relevant to the empirical test of the ‘two cultures’ hypothesis. Observable implications 1 (high validity and empirical relevance), 2 (a single dimension of methods practices), and 4 (high shares of predicted practices) are, in our view, implications that directly follow from the ‘two cultures’ hypothesis and should be confirmed. Consequently, we pay particular attention to the empirical results for these three implications in the following. Observable implications 3 (strong pairwise correlations) and 5 (changes over time), in contrast, can be conceived of as indirect implications of the ‘two cultures’ argument. They should also find empirical confirmation if the two-cultures hypothesis holds, but we take them to be less relevant in the context of our analysis.

## 4. Empirical strategy

### 4.1. Article collection<sup>14</sup>

We test the observable implications against a stratified random sample of 180 empirical research articles. In line with the perspective taken in ATTC, we limit the selection of articles to those answering causal research questions and include neither purely descriptive articles, nor those based on an interpretivist framework into the analysis (Goertz and Mahoney 2012, chapter 1). The sample of 180 articles is stratified on three levels. First, we stratify by the method that is employed in the article, selecting 90 articles that apply a qualitative method and 90 that implement a quantitative method. The 180 articles were randomly sampled from *journal-periods*, capturing that we additionally stratified articles by journal on a second level and by time period on a third level. We chose

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<sup>14</sup> This section presents a shortened summary of the sampling procedure. We provide a more in-depth discussion in Appendix C.

Anglo-American and European outlets that cover all the major sub-disciplines of political science:<sup>15</sup> *American Journal of Political Science (AJPS)*, *American Political Science Review (APSR)*, *Comparative Political Studies (CPS)*, *European Journal of Political Research (EJPR)*, *International Organization (IO)* and *World Politics (WP)*. For each method and journal, we distinguished three time periods to test the fifth observable implication. We expect a trend towards greater coherence within the two methods cultures over time, and especially in qualitative research. The first period, 1990-1994, captures the time before the publication of *DSI* and the dominance of “the comparative method” in qualitative research. The period 2000-2004 is an interim phase in which we might already see instances of new developments in qualitative methods covering process tracing, set theory, etc. The third period, 2008-2012, was chosen because it is most likely to show evidence for a qualitative culture (see previous section). Altogether, we selected ten articles per journal-period (five qualitative and five quantitative), which yielded 30 articles per journal, 60 articles per period, and 180 articles in total.<sup>16</sup>

We chose these journals for two reasons. First, the snap-shot analysis in the appendix of ATTC draws on APSR, CPS, IO and WP as the empirical basis for measuring the incidence of quantitative and qualitative research and specific methods. This makes these journals a straightforward choice for a test of the two-cultures hypothesis.<sup>17</sup> Second, these journals are considered premier outlets in political science across our period of analysis (Giles and Garand 2007). If two methods cultures exist, we should find evidence for them in journals that are arguably paradigmatic for setting the methods standard in the discipline. Consequently, we find it most likely to identify the expected method patterns in these journals.

Despite these arguments in favor of the chosen journals, there might be the concern that the six journals or a subset of them are least likely to produce confirming evidence for the two-cultures hypothesis. The concern could be rooted in the fact that two of them (AJPS, APSR) mostly publish quantitative work and that they or others might attract work following a “quantitative, correlational

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<sup>15</sup> We broadly distinguish between American politics, comparative politics and international relations as subdisciplines within political science (see Appendix A). The subfields have no substantive relevance for us with the exception of our intention to ensure some variation across the subfields. In total, our sample includes 24 articles on American politics (13.33%), 112 on comparative politics (62.22%), and 44 on international relations (24.45%).

<sup>16</sup> We are not interested in the journals per se, which would require a larger sample to reduce the uncertainty of the estimates. Our goal is the collection and analysis of a random sample of articles belonging to the population of quality quantitative and qualitative publications in premier outlets.

<sup>17</sup> In addition to the four political science journals, the brief analysis in ATTC’s appendix includes two premier sociological journals (*American Journal of Sociology*, *American Sociological Review*). We replaced those with AJPS and EJPR because we want to focus on political science and cover more journals from that discipline.

template” that is in discord with the qualitative culture. However, we find it conceptually difficult to argue that a qualitative culture exists except for articles published in any set of six journals — and ATTC does not make such an argument. We discuss the potential bias introduced by the selection of these journals as well as the details of the sampling procedure in Appendices B-D.

## 4.2. Coding of method practices

The coding of method practices in the 180 articles proceeded as follows. We first developed a coding scheme and instructions based on Chapter 18 of ATTC and the chapters dealing with one or more methods practices (see Appendix A). Guided by the coding scheme, each of the authors independently read and coded all 180 articles on each of the 25 items capturing a specific methods practice.<sup>18</sup> We coded an item “0” when it corresponded with the expectations of the quantitative culture and “1” when it conformed to the qualitative culture.<sup>19</sup>

As an example, consider item 4 (“process tracing”), which asks whether an article includes the empirical analysis of an historical process in at least one case. We coded the item “1” if a process was reconstructed in the empirical analysis that went beyond reporting isolated within-case evidence. Hsueh’s (2012) analysis of differences in the reregulation of economic sectors in China and India, which was sampled as one of the qualitative articles published in CPS in the third journal period (2008-2012), includes a detailed historical description of the reregulation schemes in both countries in the textile and telecommunication sectors, each spanning multiple pages. This was coded “1”. However, we also coded less detailed and extensive process narratives as conforming to the qualitative culture. Weyland’s (2010) article on the diffusion of anti-regime protests in the 1830-1940 period, drawn from the same journal-period as Hsueh’s, only provides a few sentences of qualitative evidence for each of the many cases discussed in the article. Nonetheless, all present a clear, albeit brief, historical narrative on how external influence affected the chances for successful pro-democratic mass mobilization in Europe. Hence, we coded the process tracing item as conforming to the qualitative culture. As an example of a qualitative article that did not include the

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<sup>18</sup> From here on, we use “method practice”, “design and method decision” and “item” interchangeably.

<sup>19</sup> For item 2 (cross-case vs within-case level of analysis), we introduced a code “3” for qualitative research. We expected, correctly, that many qualitative articles combine an integrated cross-case and within-case analysis that we would have to code as “99” if we followed the original scheme as defined in ATTC. We wanted to avoid the loss of too many qualitative articles for item 2 and opted for adding to a third code “3” to see how often this is done in practice. In our empirical analysis, codes “1” and “3” correspond to the qualitative culture. Item 18 (measurement) suffers from a similar problem because an article can follow both quantitative and qualitative practices. When this occurred, we coded the article in favor of the culture (quantitative articles = 0, qualitative articles = 1).

empirical tracing of processes, consider Hopf (1991), which was published in APSR. The article tests Waltz's theory on the impact of multi- vs. bipolarity in the international balance of power on the stability of the international order. While the article provides a vast amount of historical data on the power distribution in the 16<sup>th</sup> century, it does not include systematic process narratives as to how these measures of state power affected concrete cases of stability or instability. Hence, we coded the process tracing item 4 as conforming to the quantitative culture, i.e., "0".

We assigned a code of "99" (interpreted as "missing") when an empirical article followed a practice not predicted by ATTC, which is possible for items for which the qualitative and quantitative practices outlined in ATTC are not jointly exhaustive. For example, the indicator capturing the rules of case selection (item 13) was coded "99" when an empirical study offered no explicit explanation as to why the cases were chosen for analysis, as this reflected neither the quantitative nor the qualitative culture. Another use of the "99" code occurred when a method practice was simply absent in an empirical article. For example, item 21 asks for *how* a typology is constructed, which requires *that* an article features a typology in the first place (most articles do not; see Section 5). The possibility that the meaning of the "99" code differs across items and articles is not problematic for our analysis.<sup>20</sup> We are specifically interested in the incidence of methods practices complying with the quantitative culture, the qualitative culture or neither, and not in a general mapping of design and method decisions in political science.

Since each of us coded each article on all 25 items individually, there were items to which we assigned different codings. The intercoder reliability for all articles and items has a Krippendorff's alpha of 0.77, which is slightly below the conventional threshold of acceptability of 0.80.<sup>21</sup> After each coder had analyzed all articles, we discussed the items with different codes to decide on what code best aligned with the item. For many items, agreeing on a final code was not difficult because the deliberation and re-reading of the article convinced one coder that the other's coding decision was more plausible. When coder agreement was difficult to achieve, which usually occurred when the empirical study was ambiguous in regard to a methods practice, we coded conservatively, in favor of the culture. In that event, we assigned a "1" to the item in question for a

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<sup>20</sup> For items 5, 9, 16, 20, 21 and 24, a "99" signifies that the issue is not practiced because no counterfactuals are performed (items 5, 9, 24); triangular data are not discussed (16); data are not transformed (20); or the article does not include a typology (21). For the choice of cases (item 13), "99" can have several meanings, e.g., a convenience sample or the absence of any discussion of how cases were chosen.

<sup>21</sup> A breakdown of intercoder reliability by the type of method produces an alpha of 0.76 for qualitative articles and of 0.73 for quantitative studies. See appendix G. for a more detailed analysis of individual items and articles.

qualitative article and a “0” when it was a quantitative article.<sup>22</sup> As an example, consider the divergent coding of Traxler’s (1992) qualitative analysis of the determinants of state policy in Austria regarding the question of whether the article follows the qualitative or quantitative culture in the ontology of concepts (item 18). One of us read the conceptual discussion as including an explicit discussion of the concept’s defining characteristics and their relations and, in consequence, as following the qualitative culture (Goertz and Mahoney 2012, 128). The other saw the conceptual discussion as closer to what ATTC considers a “focus on issues of data and measurement, and less on semantics and meaning” (ibid.) and, thus, corresponding to the quantitative culture. After discussing our positions, we agreed to disagree and coded the item “1”, in favor of the culture.

The spreadsheet containing all sampled articles, coder-specific and agreed upon codes is available in the online repository.<sup>23</sup>

## 5. Observable implication 1: Relevance of method practices

The starting point of the tests of observable implication 1 is the screening of the definition of all qualitative and quantitative practices; this allows us to separate the items that are necessarily relevant from those that might not be important in practice. A method practice is relevant when the qualitative and quantitative practices are jointly exhaustive and an empirical study has to follow one or the other. We identify six items as potentially invalid indicators of method cultures because the corresponding practices might not be observed in practice: 5 (counterfactuals), 9 (counterfactuals), 16 (triangular data), 20 (data transformation), 21 (typologies) and 24 (counterfactuals). For all other items, it is always possible to assign a substantively informative code, meaning that the other items cannot be coded as missing.

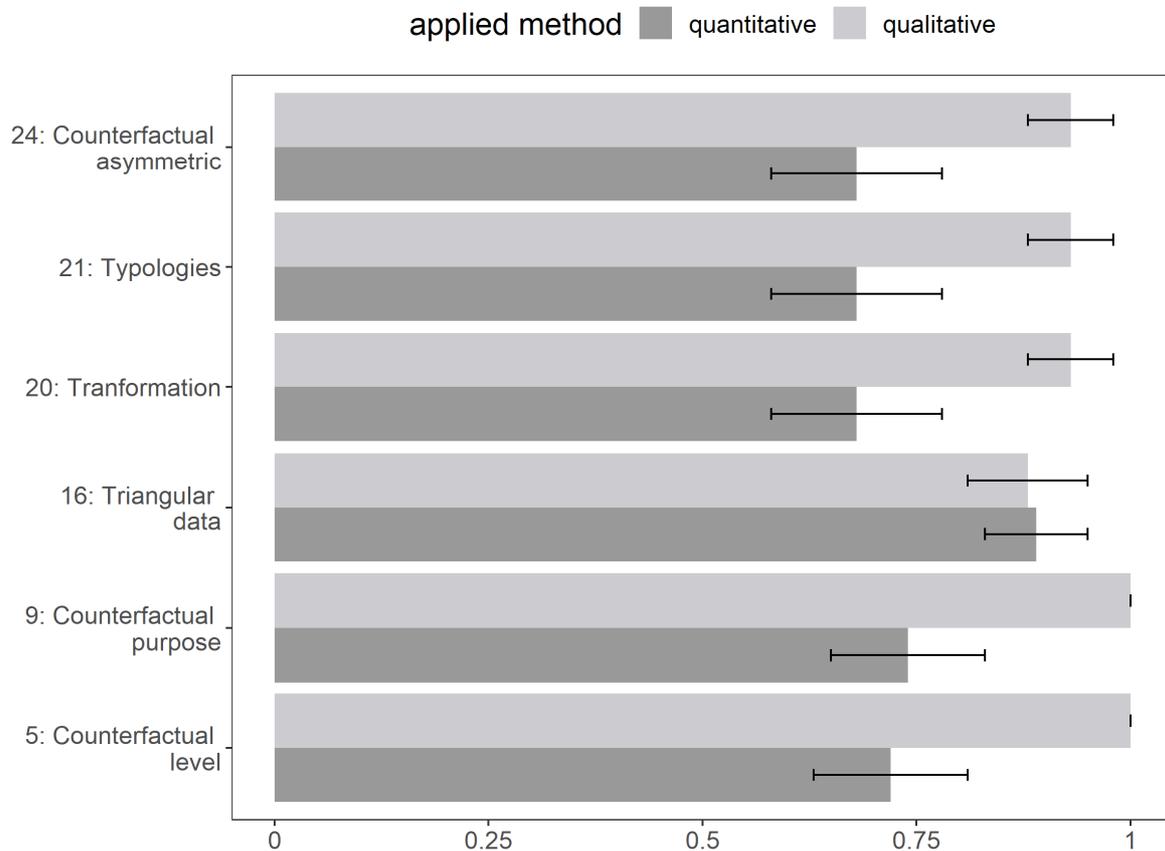
We assess the conceptual validity and empirical relevance of a methods practice by its incidence in empirical research. For the 90 quantitative and qualitative articles, we calculate the share of studies that do *not* display a methods practice and are coded “99”. We split the sample into the quantitative and qualitative part to detect possible differences in relevance between methods. In the absence of a convention to draw on, we find it reasonable to argue that, to be designated as relevant, a methods practice should at least be present in more than 50 percent of all empirical studies. Figure 1 presents the shares of missings for the six items that can be coded “99”.

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<sup>22</sup> Or a “3” for item 2. In the analysis of our data, we take “1” and “3” as evidence for a qualitative practice and collapse the codes.

<sup>23</sup> BLINDED repository: [https://osf.io/4fdcb/?view\\_only=5043c3787fd2418f984e4df35587b4a9](https://osf.io/4fdcb/?view_only=5043c3787fd2418f984e4df35587b4a9)

Figure 1: Proportion of missings and 95% confidence interval



All six items for which a “missing” code of “99” was possible are observed in less than half of all qualitative and quantitative articles. We observe variation in the extent of missingness, ranging from complete irrelevance in qualitative articles (understanding of triangular data, item 16, and data transformation, item 20) to the absence of practices in about 70% of all articles (counterfactuals in quantitative articles; items 5, 9, 24). Using our criterion of empirical importance, we observe that the six possibly irrelevant practices are not valid characteristics of the two cultures because they are rarely observed in practice. If there are qualitative or quantitative methods cultures, they are constituted by the subset of practices that are relevant to empirical work. For this reason, we limit the following analysis to the empirically important items unless we explicitly state otherwise. In figure 1, we left aside item 13 (case selection) in figure 1 because the “99” code denotes the non-discussion of how cases were chosen *or* that a case selection strategy other than those discussed in ATTC was followed. The analysis of case selection strategies shows that qualitative articles

follow the qualitative culture less often than quantitative articles and that both show a sizeable share of “99” codes. 34% of all quantitative articles do not sample cases randomly [95% confidence interval: 0.24-0.34] and that 66% of all qualitative articles do not display the expected practice [0.56-0.76].

## 6. Observable implication 2: A single dimension of method application

We run a confirmatory factor analysis (CFA) to test the second observable implication that the realization of qualitative and quantitative methods represents a single underlying dimension. The data for the 18 relevant items that we include in this analysis are categorical and do not follow a multivariate normal distribution, which is a standard assumption in CFA.<sup>24</sup> We take this into account by using a robust weighted least squares estimator for testing the null hypothesis that a single dimension exists. Table 2 summarizes the results.

Table 2: Fit measures for confirmatory factor analysis of 18 items

measure of fit (robust estimation)	Value
$\chi^2_{(df = 135)}$	562 ( $p < 0.001$ )
RMSEA	0.133 (90% CI: 0.118-0.148)
Lewis-Tucker index	0.890

The Pearson  $\chi^2$ -test for model misspecification is significant, allowing us to reject the null hypothesis that the data represent a single methods dimension. A second test is the root mean square of approximation (RMSEA) test that evaluates the degree of deviation from perfect model fit for a single dimension. According to the conventional interpretation, values below 0.1 are taken as acceptable and values below 0.05 are good (Hu and Bentler 1995). The RMSEA test for the existence of a single dimension yields 0.133, with a 90-percent confidence interval from 0.118 to 0.148. It fails to meet the standard benchmark for acceptable model fit. These findings are confirmed by the Lewis-Tucker index, which is an alternative test to the RMSEA. The estimate is 0.89, which fails

<sup>24</sup> In addition to the six items with a large number of missings, we exclude item 13 (case selection) because it has a sizeable share of “99” codes that are not relevant when testing for a single dimension. The Shapiro-Wilk test for multivariate normality rejects the null of normality at .05.

to meet the conventional threshold of 0.95 that signals good model fit. In sum, the three measures are not far from the conventional benchmarks indicating good model fit, but they fail to meet any of those benchmarks, thus indicating that there is not a single methods dimension.

When we reject the null hypothesis of a single methods dimensions, the follow-up question is what the dimensionality of methods practices is, if there is any? More broadly speaking, one might wonder whether there are any patterns in methods implementation in political science. We briefly return to this question in the conclusion to this article.

### **7. Observable implication 3: Strong pairwise correlations of items**

If the two-cultures hypothesis was true, the third implication predicts that we should observe strong correlations between all 18 empirically relevant items because we would consistently code quantitative practices as “0” and qualitative articles as “1”.<sup>25</sup> We calculate Cramér’s V and the 95-percent confidence interval for all pairwise correlations to test this implications (Figure 2).<sup>26</sup> We apply a permissive benchmark and designate a correlation as high when the lower bound of the confidence interval is larger than 0.5.

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<sup>25</sup> We exclude the six empirically irrelevant items discussed in Section 5 and again exclude item 13 because of the high number of qualitative articles coded “99” on this item. We are interested in correlations between substantively interesting codes, “0” and “1”, and high correlations resulting from “99” codes (such as for item 16 and 20). We exclude correlations of pairs of identical items because it is necessarily 1.

<sup>26</sup> Appendix E.1 includes a heat map for the correlation coefficients that links the degree of correlation to a specific pair of practices for the pooled data.

Figure 2: Pairwise correlations of individual items (Cramér's V) with 95% confidence intervals

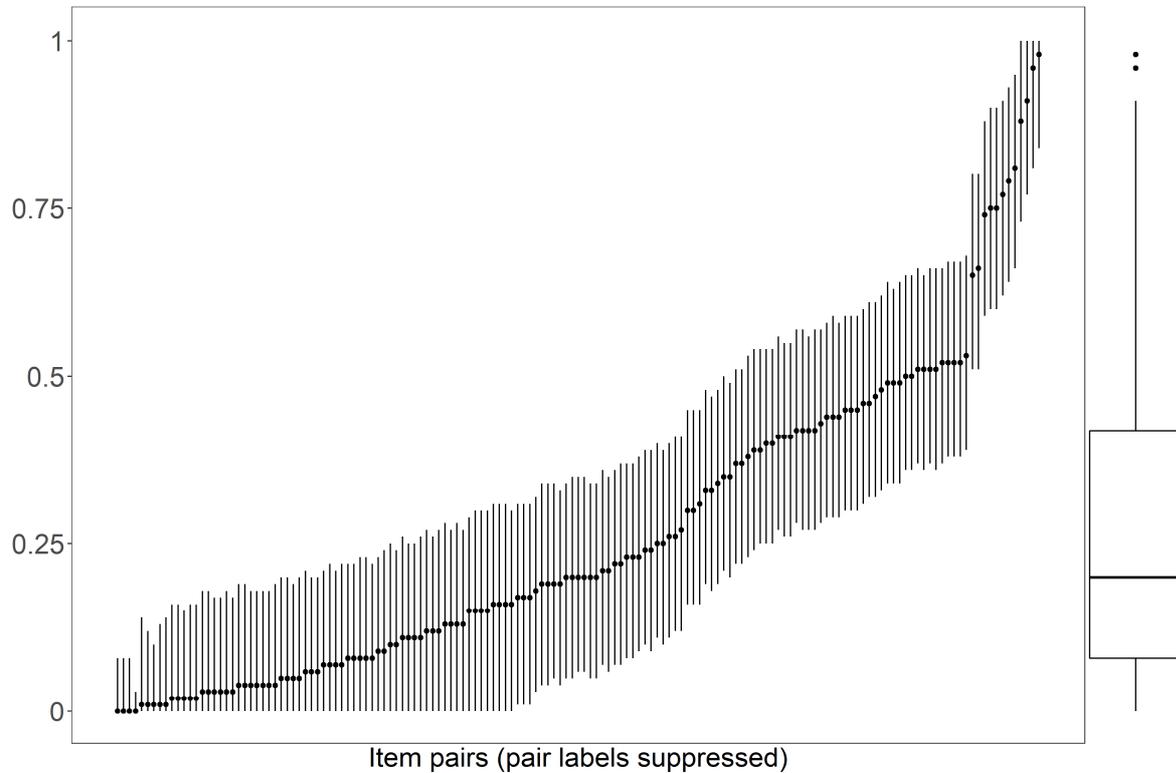


Figure 2 shows that 10 of the 151 pairwise correlations are at 0.75 or higher and that 14 are clearly above 0.5. The largest share of correlations is in the medium and lower ranges, with 91 correlations being below 0.25, which is also reflected in a median correlation of less than 0.25.<sup>27</sup> The pairs of items that correlate with each other at a level higher than 0.5 mainly come from two sets of items. First, items 1, 2, 3 and 4, which capture different method and design decisions about how individual cases are treated, are strongly correlated with each other. This is only partially surprising because these items are defined in ATTC such that a qualitative article must be coded “1” on all four items and that a quantitative article must be coded “0”. The other set of items comes from different dimensions and can be summarized as those related to asymmetric relations in the data, with ten items in total (items 6, 8, 11, 14, 17, 18, 19, 22, 23, 25). Again, some of these correlations are to be expected because items 22, 23, and 25 (symmetry/asymmetry of concepts and causal inferences) must all be coded either “0” or “1”. Altogether, we conclude that the third observable implication receives little empirical support. The third quartile of the distribution of correlations is less than

<sup>27</sup> We do not report statistical significance because one easily sees statistical significance for pairs that are weakly correlated.

0.5, and those relatively high correlations we found are mostly due to how the items are defined in ATTC.

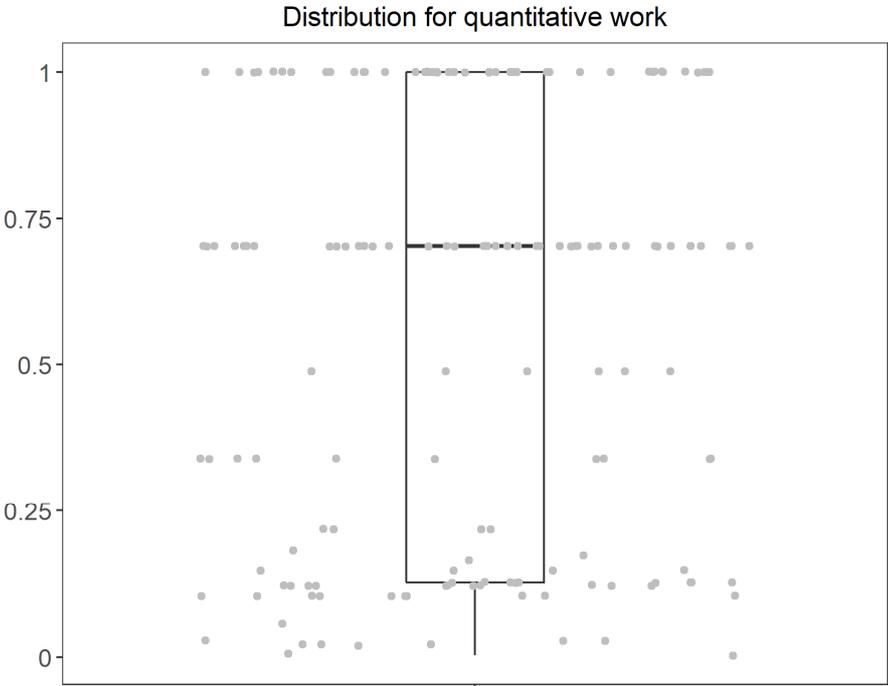
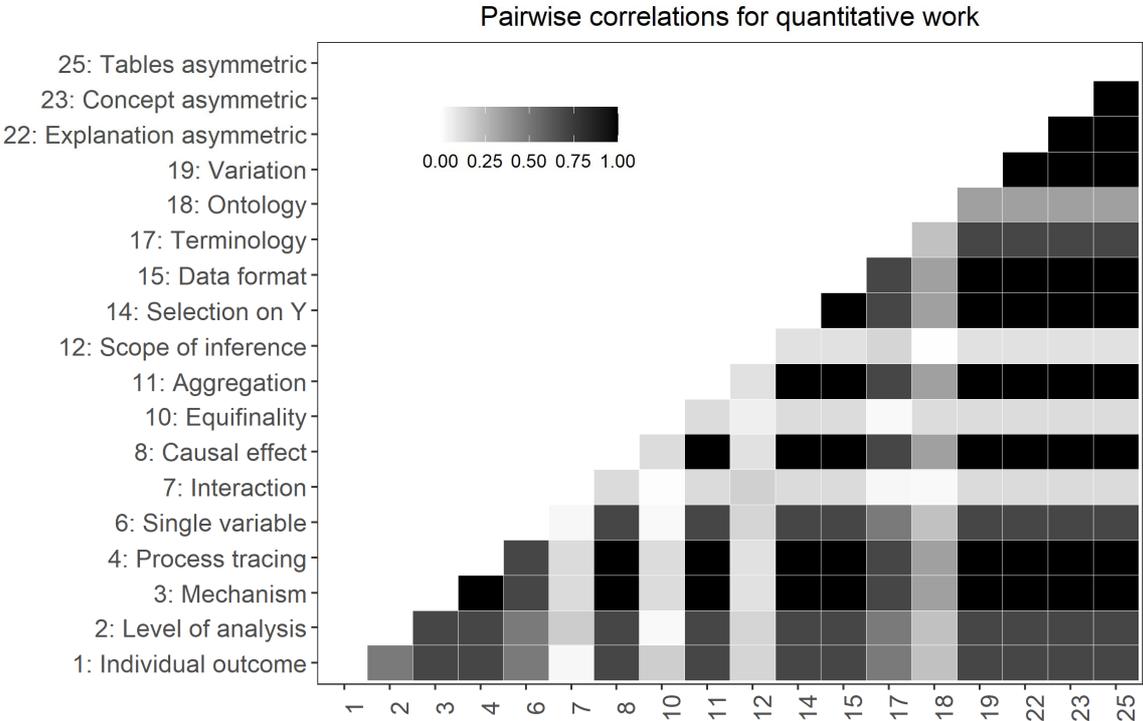
The findings for observable implication 2 and 3 suggest that the two-cultures hypothesis does not hold up against empirical evidence. Based on these insights, we find it necessary to take a closer look at the data. From this point on, we disaggregate the pooled data into the quantitative and qualitative subsample and test the implications separately against the two subsamples. This allows us to determine whether the reasons for the differences between expectations and findings lie with the quantitative or qualitative articles or both.

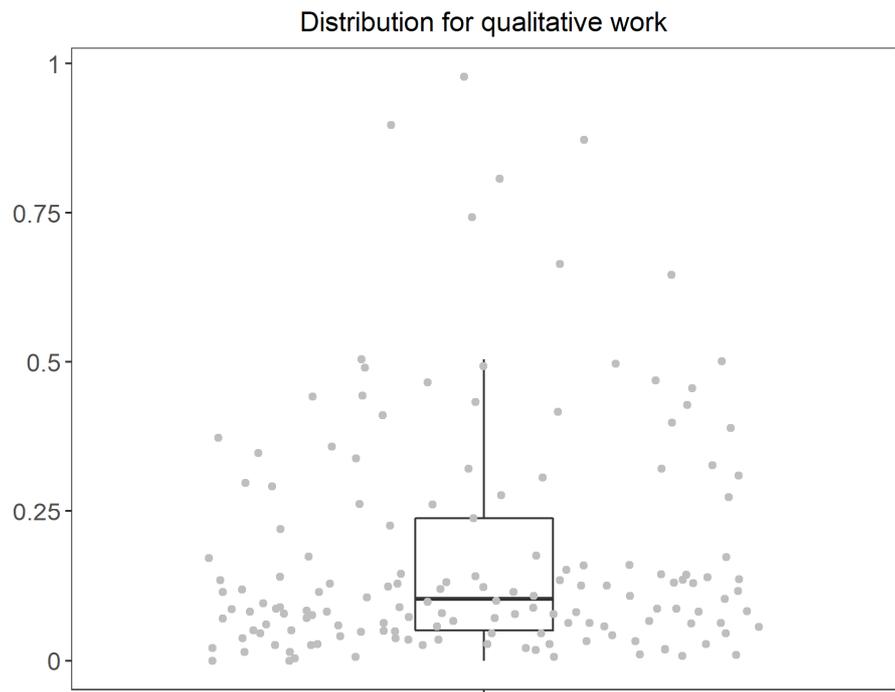
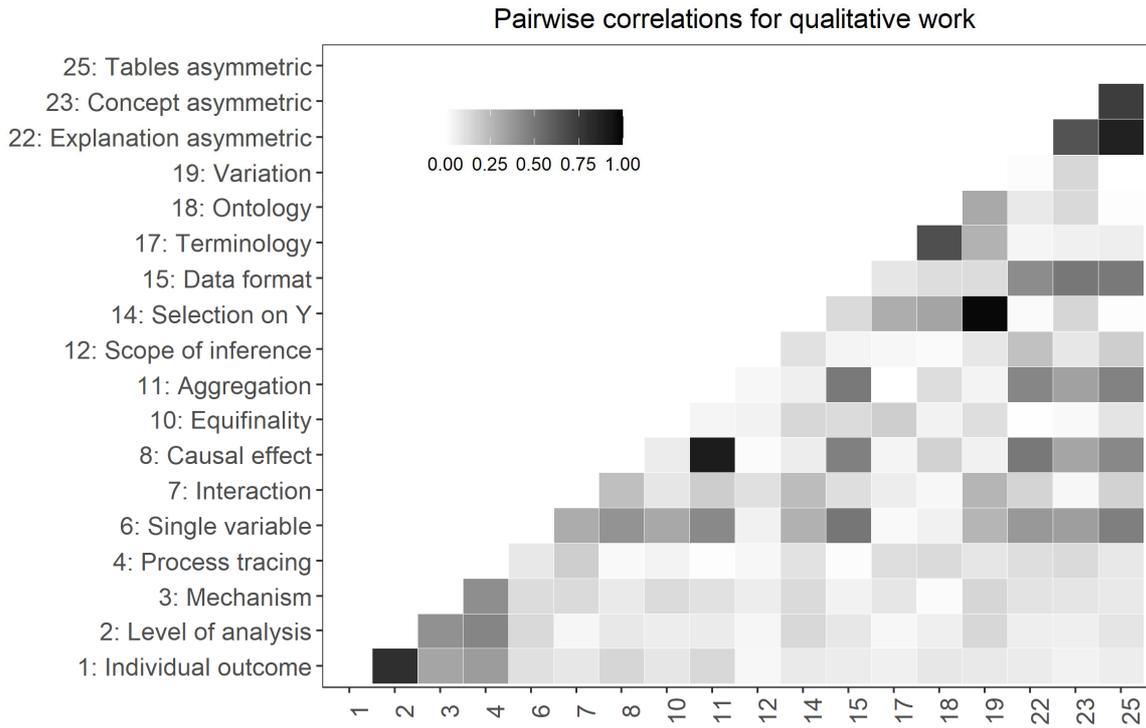
In figure 3, we present the pairwise correlations separately for the quantitative and the qualitative articles.<sup>28</sup> We observe a difference between the two types of articles because the correlations are, at least to some degree, in the upper range for quantitative work and mainly in the lower range for qualitative articles. The median correlation is about 0.7 for the quantitative articles and 0.1 for the qualitative articles. Among the pairwise correlations for the quantitative articles, items 7 (interactions), 10 (equifinality), 12 (scope of generalization) and 18 (nature of concepts) stand out because they are only weakly correlated to all other items. For the qualitative articles, in contrast, only a handful of items achieve a level of correlation that is higher than the median for quantitative work. The analysis of the third observable implication clearly indicates that quantitative and qualitative research do not fully follow the culture and that the difference between expectation and evidence is much larger for qualitative than quantitative articles.

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<sup>28</sup> We do not present uncertainty estimates here because we want to include information about the pairs of items and because they would grow large and less meaningful with a reduced sample size of 90.

Figure 3: Pairwise correlations by type of applied method



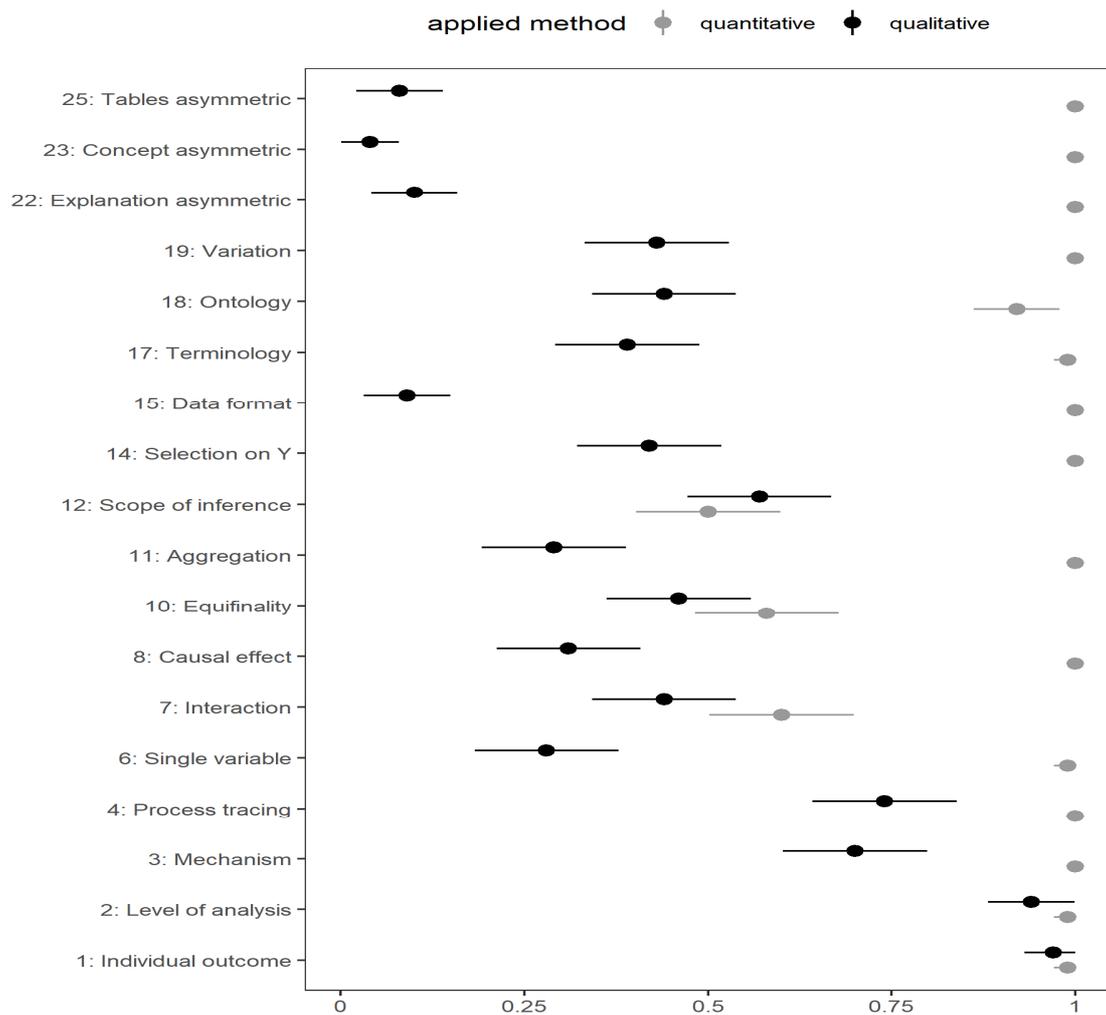


### 8. Observable implication 4: High shares of culture-conforming applications

We first assess the implication for the share of culture-conforming practices by taking *items as the unit of analysis*. For each item, we calculate the share of how often it is realized in accord with the

corresponding culture.<sup>29</sup> We present the data separately for qualitative and quantitative articles to detect possible differences between them.<sup>30</sup> We use a share of 0.75 as the threshold for evaluating the incidence of method practices. We infer that a method practice confirms the observable implication if the lower bound of the confidence interval is larger than 0.75 and as not confirming it otherwise.

Figure 4: Proportions of culture-conforming codes and 95% confidence intervals



<sup>29</sup> Again, we exclude the items with “99” codes because we can either not calculate a proportion because of too many “99” codes or the confidence intervals would be too large and pointless. We present simple cross-tabulations in *Appendix E.2*.

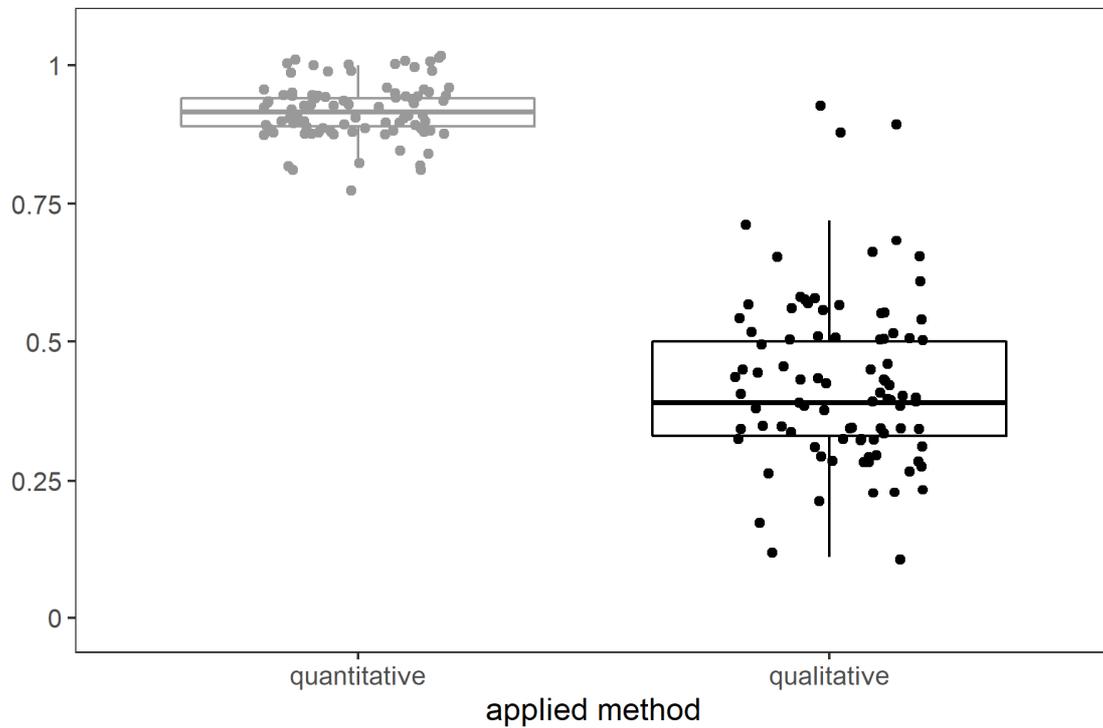
<sup>30</sup> We cut the bounds of the confidence intervals if the upper bound of the confidence is larger than 1 or the lower bound smaller than 0.

Figure 4 shows that, for quantitative articles, 15 practices out of 18 are statistically indistinguishable from 1 and support the expectation of a quantitative methods culture. The three practices that stand apart relate to items 7 (interaction effects), 10 (equifinality) and 12 (generalization) that are statistically indistinguishable from the share of qualitative practices. The results show that, for item 7, about 40% of the quantitative articles estimate an interaction effect; for item 10, about 45% of the quantitative articles discuss model equifinality; and, for item 12, only 50 percent of quantitative articles make a broad generalization claim.

The findings differ for qualitative articles wherein large proportions of culture-conforming codes can only be identified for the first dimension (items 1-5), which relates to the way in which individual cases are handled. 14 practices out of 18 are either as much quantitative as they are qualitative (confidence interval includes 0.5) or are actually closer to the quantitative culture. This includes items 22, 23 and 25 that capture whether the analysis is symmetric or asymmetric and indicate that qualitative articles usually take a symmetric view. In sum, the findings for this implication support the divergent findings for the third implication. The moderate to weak correlations of items for qualitative articles have their source in the heterogeneous implementation of methods practices in qualitative research.

The second test of the fourth implication again takes *articles as the unit of analysis* and calculates the share of the 18 practices that an article comprises in accord with the corresponding culture. In figure 5, the left distribution shows that quantitative method practices are mostly in line with the corresponding culture for all 90 articles. Whereas this is less surprising after having seen figure 4, figure 5 shows that qualitative articles have a higher degree of heterogeneity. Again, we take a share of 0.75 as the benchmark for distinguishing between articles that follow a culture and those that do not.

Figure 5: Proportion of culture-conforming practices per article by applied method



For the qualitative culture, we observe a much wider dispersion of practices, with the third quartile being at a share of culture-conforming practices of 50%. Only a handful of articles reach a share of culture-consistent method practices of about 80%, which is the median for quantitative work. In comparison, there are more articles that are more quantitative in nature, with a share of qualitative practices of less than 0.25, which means more than 75% do follow the unexpected, quantitative culture. This confirms the mixed conclusions we made before and indicates that the two-cultures hypothesis is principally valid for quantitative articles and has little validity for qualitative articles.

### 9. Observable implication 5: Method applications over time

The fifth implication is that the differences between quantitative and qualitative approaches should become more pronounced over time as qualitative methodology becomes more developed and formalized. Based on the results in the previous section, the most direct and, in our view, meaningful evaluation of this implication is to determine whether the proportion of articles corresponding with the two-cultures hypothesis increases over time (Figure 6).

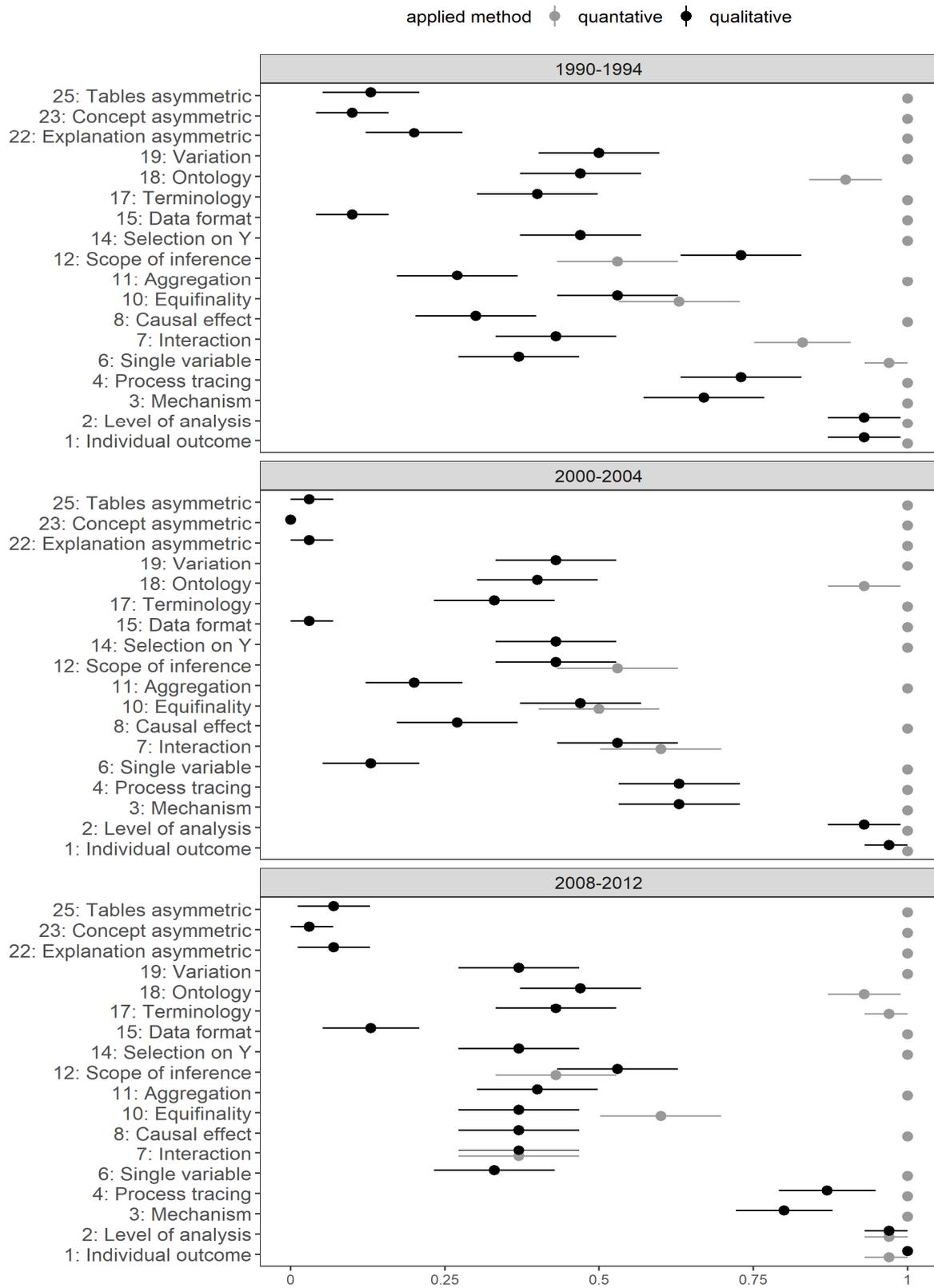
For quantitative articles, we see a very stable pattern across the three periods. The only change that stands out is an unexpected increase in the share of articles that estimate an interaction effect from 2000-2004 to 2008-2012 (item 7). Overall, Figure 6 confirms our expectation that there should be little temporal variance for quantitative methods practices; it consistently displays a high degree of conformity with the expected practices.

The development of qualitative methods practices over the three periods does not confirm our expectation of an increasingly pronounced qualitative culture. This implies that neither method became *more* distinct in comparison. The three panels indicate that the variance across qualitative practices decreases over time.<sup>31</sup> Method practices for 2008-2012 show that there are three clusters in the most recent period (item 15, capturing the format in which data are presented, is not part of a cluster of practices): first, one cluster with items from dimension 5 (items 22, 23, 25 which capture symmetry/asymmetry of concepts and causal arguments) that follow the quantitative culture; second, one cluster representing dimension 1 (items 1 to 4, which indicate how individual cases are treated), with items 3 (empirical analysis of causal mechanisms) and 4 (tracing of empirical processes) showing a trend toward becoming more qualitative over time and largely following the qualitative culture in terms of levels; and third, one “neither-nor” cluster that includes ten items from different dimensions that converged in the range of about 0.25 to 0.5 over time. Overall, there is no evidence for a strengthening qualitative culture over time, including the most-likely period 2008-2012.

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<sup>31</sup> See Appendix E.2 for slope plots more clearly tracing the development of items over the three periods.

Figure 6: Proportions of culture-conforming codes and 95% confidence intervals for subperiods



## 10. Summary and alternative explanations

The empirical evaluation of the five observable implications provides little support for the two-cultures hypothesis (Table 3). Some practices in quantitative research are as qualitative as they are quantitative, but the overall level to which individual articles using quantitative methods conform to the quantitative culture described in ATTC is high. This is very different for qualitative research wherein articles show a much greater degree of diversity in how design and method decisions are made and only a handful of practices meet the expected ideal of the qualitative culture. Taken together, this means the main reason that the two-cultures hypothesis is not confirmed rests in the misrepresentation of qualitative empirical research.<sup>32</sup>

Table 3: Results of the empirical tests of five observable implications

OI	Expectation	Results	OI corroborated
1	High validity and empirical relevance	Six out of the 25 practices that could not be relevant (no exhaustiveness of expected quantitative and qualitative practice) are not relevant in empirical research.	No
2	Single dimension	Method practices do not reflect a single dimension of method practices.	No
3	Strong pairwise correlations	Most method practices do not correlate strongly with each other. Some of those that do, do so by the way they are defined in ATTC.	No
4.	High shares of predicted practices	The proportion of items in line with the used method is high for quantitative articles and low for qualitative articles.	Yes (quant) and no (qual)
5.	Strengthening qualitative culture over time, constantly strong quantitative culture	Strong conformity with quantitative culture over time (with few exceptions). Most qualitative practices and articles are not following the qualitative culture. No discernible trend towards qualitative culture over time.	Yes (quant) and no (qual)

<sup>32</sup> The interpretation of our findings does not change if we take into account the difference between first and second order implications that we introduced in Section 3. Of the direct implications of the ‘two cultures’ hypothesis (OIs 1, 2, 4), two are refuted while OI4 is only corroborated for the subset of quantitative articles. The same is true for the second order expectations: OI2 is refuted while OI5 is only confirmed by quantitative research.

A look at our findings suggests that a central reason for qualitative articles differing from the expected culture is the underlying assumption inherent in ATTC that all (or at least most) qualitative research is based on the foundation of set theory. Our analysis shows that applied qualitative research is based on neither a set-theoretic modeling and handling of data (captured by items 15, 16, 23, 25), nor on set-relational and asymmetric modeling of causality (items 6, 8, 11, 22). Some of these items work well as valid descriptors of empirical research using Qualitative Comparative Analysis (QCA, Marx et al. 2014), but it seems that they cannot be successfully exported to qualitative empirical research per se (see also the results of a cluster analysis of qualitative practices in Appendix E.3.2).

There might be two alternative explanations for our findings other than that many qualitative articles do not follow the qualitative culture. First, qualitative research could be characterized by “implicit but quite common practices” (Goertz and Mahoney 2012, 9, 11) that are not amenable for identification through a standardized coding and evaluation process as applied in this article. If this was true, our disconfirming findings for qualitative research could be explained by having misread and miscoded qualitative practices. There are two reasons why we find this alternative problematic. The first is that, according to ATTC, despite being implicit, qualitative methods characteristics should be “readily identifiable” and can be “reconstructed” by “a broad reading of qualitative studies, including an effort at systematically coding qualitative research articles” (Goertz and Mahoney 2012, 2, 7). Moreover, whenever we were unsure about the coding, we coded conservatively “in favor of the culture”. This should offset at least to some degree the potential impact of ambiguousness due to implicit use of certain methods practices on our results. The second reason for rejecting the argument that the implicitness of qualitative research practices drives the results is that only a small subset of the 25 practices that we analyzed can be realized without making them explicit. In our reading, only six items could *potentially* capture implicit practices and lead to ambiguous codings.<sup>33</sup> If the implicit-practices counterargument was true and if we have miscoded method and design decisions<sup>34</sup>, this might change some of the findings we have presented, but would not overturn the overall conclusion of heterogeneous qualitative research practices.

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<sup>33</sup> Item 8 (correlation vs set theory), item 11 (linear-additive vs set-relational aggregation), item 15 (dataset vs truth table) and items 22, 23 and 25 on symmetry of concepts and causal reasoning.

<sup>34</sup> Our personal coding experience is not that practices were implicit, but were most often made explicit.

The second alternative explanation is that the two-cultures hypothesis is empirically true for methods practices in political analysis, but that our data are biased against uncovering them.<sup>35</sup> Our data collection could be biased for multiple reasons. One source of bias could be that we evaluate journal articles and not books.<sup>36</sup> It is easier to present the *evidence and results* of a qualitative study in a book-length format, but we argue that this is unrelated to the *implementation* and underlying principles of qualitative methods. The design and method decisions that characterize the qualitative culture can be presented in articles because they do not require extensive discussion. The justification of design and method decisions should require about the same space in a qualitative study as in a quantitative study. Those articles in our data that do largely correspond to the qualitative culture (Figure 5) demonstrate that one can follow the qualitative culture in journal articles.

A second potential source of bias could be the journals that we have chosen. The editorial policy of journals might be predisposed in favor of quantitative work or a certain type of research that is inspired by the quantitative template more generally (such as in King et al. 1994). There might also be regional biases, as five out of the six journals might follow an Anglo-American tradition that has traditionally been dominated by quantitative research and “symmetric” understandings of causality. In comparison, European political science has been described as being more open to qualitative research and methodological pluralism (Moses et al. 2005). If this alternative explanation was true, it would imply that articles following the qualitative culture are rejected and hence published in journals we did not select; or that they are revised throughout the editorial process to better conform to the quantitative culture; or that authors expect such preferences for the quantitative culture and refrain from submitting to these journals or follow the quantitative culture preemptively.

We try to assess journal-induced bias, broadly speaking, by comparing methods practices across the six journals. Figure 7 presents the proportions of practices that conform to the corresponding culture separately for quantitative and qualitative articles and each journal. We observe that the pattern found for the pooled data (figure 5) holds in very similar ways across all journals. IO and WP have a somewhat larger dispersion of qualitative articles, suggesting that there is no regional bias because the articles in these two journals are “more qualitative” than those sampled

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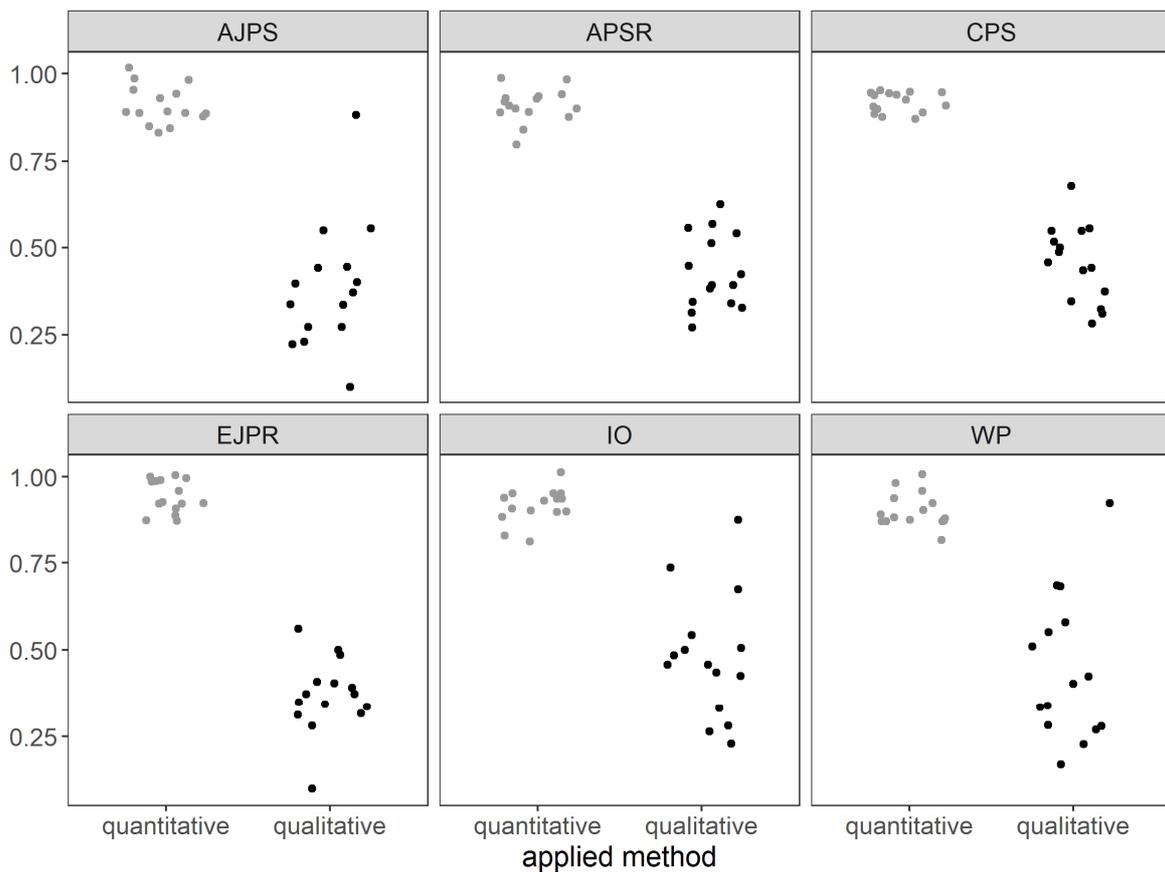
<sup>35</sup> We chose political science journals in a similar manner as in the appendix of ATTC. This does not render our choice correct, but this alternative explanation extends to the analysis of journals in ATTC.

<sup>36</sup> This possibility does not seem to be shared by ATTC that is agnostic about the medium of presentation.

from EJPR. We also think that the panels in Figure 7 do not support the conclusion that editorial policies account for a suppression of the qualitative culture. These journals are known for having a different orientation – AJPS and APSR being more quantitatively oriented than the others – and the data for each journal pool observations from different editorships who are likely to follow different editorial policies.<sup>37</sup>

Figure 7 does not conclusively refute the possibility that editorial policies might be biased against the qualitative culture. However, we believe that the presented evidence indicates that journal bias is not driving the empirical results. This is confirmed by the results of 30 additional qualitative articles published in 2018 in a large number of different journals other than the six that underlie the main analysis (see Appendix E.3).

Figure 7: Proportion of culture-conforming practices per article per journal



<sup>37</sup> We could not sample a sufficient number of qualitative articles for selected journal periods (see above and Appendices B-D). The clustering of the data for AJPS and APSR in figure 7 suggests that the sampling from other journals does not create problems.

Finally, our analysis could be problematic because it might end too early. This would be a valid claim if empirical articles only picked up on recent developments in qualitative methods after 2012. We address this possibility (as well as the possibility of a biased journal selection) by coding 30 additional qualitative journal articles published in 2018 and in 28 journals other than the six main journals. We present the sampling procedure and detailed results in Appendix E.3. We test observable implication 4 (share of culture-conforming method practices) against the 30 articles because the disaggregated perspective makes it easiest to see where the 30 additional articles differ from the 90 articles in the main analysis. We fail to find meaningful differences between the two samples when taking items and articles as the unit of analysis. These findings strengthen our main results and indicate that they are neither conditional on the chosen journals nor on the three periods of analysis and that is no qualitative culture that exists exclusively outside of the six premier journals included in the main dataset.

## **11. Discussion**

In this paper, we take the seminal hypothesis of “A Tale of Two Cultures” (ATTC) about the presence of two coherent and distinct quantitative and qualitative method cultures and derive five observable implications from it. The implications allow us to perform a comprehensive empirical test showing that the “two-cultures hypothesis” is not an empirically valid description of common methods practices in political science. The nuanced picture that we obtained from a stratified random sample of 180 empirical articles is that, with a small number of significant exceptions such as the analysis of interaction effects, the implementation of quantitative methods follows the expected culture. Qualitative methods, in contrast, neither conform to the qualitative culture nor do they show a clear trend to become “more qualitative” (as per the “two-cultures hypothesis”) over time, including the period 2008-2012, which is identified as a most-likely time frame. Since qualitative methods continue to be discussed along the dimensions of asymmetry, mechanisms, process tracing and set theory, it is possible that empirical research might reflect these characteristics more clearly in the future and that the height of the “qualitative culture” is yet to come. While we cannot forecast future method developments, our robustness analysis of 30 randomly selected qualitative articles published in 2018 casts doubt on this argument (see Appendix E.3). Overall, our analysis shows that qualitative research is not mainly based on the set-theoretic underpinnings stipulated by ATTC.

A set-theoretic framework for analysis with a distinct mode of causal inference is valuable and can be used in qualitative research. However, our results suggest that a set-theoretic perspective is not all there is to applied qualitative research and that a certain share of articles adopts a difference-making perspective on causal inference (broadly understood) as it underlies quantitative research.

If qualitative research does not conform to the qualitative culture as defined in ATTC, the follow-up question is whether our data suggest the existence of other patterns in qualitative practices. We address the question by running an exploratory cluster analysis on all 90 qualitative articles in our sample (see Appendix E.4 for the full procedure and results). In this, we draw on Koivu and Kimball Damman's (2015) conceptual distinction between three approaches within qualitative research to a priori specify three clusters: "quantitative emulation (QE)", "eclectic pragmatism (EP)", and "set-theoretic approaches (ST)". According to Koivu and Kimball Damman, these three approaches are based on different foundations that lead to different method and design decisions in causally oriented qualitative research. The analysis shows that the vast majority of qualitative articles in our sample are grouped into two clusters: one containing 48 articles (53.3 per cent of all qualitative articles) and one containing 35 articles (38.9 per cent). A third cluster is small and contains only seven articles (7.8 percent).

To determine whether the three clusters correspond to the three conceptual groups suggested by Koivu and Damman, we calculate the per-item means within each cluster and compare them across the three clusters: the closer the value is to 0, the closer the cluster comes to the quantitative ideal; the closer the value is to 1, the closer the cluster is to the set-theoretic qualitative ideal described in ATTC. The first cluster has a median value of 0.25; it thus corresponds well to Koivu and Kimball Damman's QE label – qualitative research that broadly follows the quantitative template. This includes an interest in the individual effect of individual variables and treating causal relationships as symmetric. The median of the second largest cluster is close to 0.5. This cluster thus corresponds to the EP category as articles in this cluster on average clearly follow neither the qualitative nor quantitative cultures, but show a more eclectic mixing of quantitative and qualitative practices. The smallest cluster, which includes only seven individual articles, has a median share of culture-conforming practices of 0.79. The seven articles in the third cluster are thus well-described by Koivu and Kimball Damman's ST label — and the set-theoretically defined qualitative culture according to ATTC.<sup>38</sup> However, and echoing Koivu and Kimball Damman's insight, our

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<sup>38</sup> Koivu and Kimball Damman (2015) identify an "empirical interpretivist" practice as a fourth variant of qualitative work that is not relevant here because of ATTC's and, thus, our focus on research aimed at identifying causal relations.

empirical analysis shows that the ST approach is but one of numerous “varieties” of qualitative research — and is not what the majority of qualitative researchers actually does.

Future research on the practice of empirical political research could proceed in four directions. First, there is room for conceptually defining more valid and empirically relevant indicators for mapping method practices in political science. Second, for the reason previously discussed, it would be useful to run a follow-up study to test for a stronger presence of the qualitative culture and to measure a potential prescriptive impact of ATTC on qualitative methods practices since 2012. Third, although we don’t believe that the selection of the six journals biases the results against the two-cultures hypothesis, it would be empirically insightful to analyze articles and possibly books covering a variety of substantive and empirical foci. This would broaden the empirical basis and could serve as a replication of the present analysis to deepen the insights and method practices in political science. Fourth, future research should address and further investigate the existence and causes of systematic differences in the application of methods, especially within the broad category of “qualitative research”.

### **Acknowledgements**

For excellent research assistance, we are grateful to Kevin Benger, Charlotte Brommer-Wierig, Nancy Deyo, Nils Holzbecher and Nils Hungerland. Earlier versions of this paper have been presented at the APSA Annual Meeting 2017, at the Research Seminar of the Cologne Center for Comparative Politics, and at the German Institute for Global and Area Studies. We appreciate the constructive feedback from the reviewers and Felix Elwert.

### **Declaration of Conflicting Interests**

We declare no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

### **Funding**

We received funding by the Fritz-von-Thyssen Foundation (grant number 20.14.0.058) and are grateful for the support that we received.

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## Appendices

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## A. Coding Guide

### *Dimension 0: Identification and general information*

<b>Item</b>	<b>Code</b>
<b>A</b> <b>Year</b>	Publication Year <i>Year</i>
<b>B</b> <b>Journal Title</b>	Journal in which the article was published <i>Journal title</i>
<b>C</b> <b>Volume</b>	Volume of the journal in which the article was published <i>Volume #</i>
<b>D</b> <b>Issue</b>	Issue of the volume in which the article was published <i>Issue #</i>
<b>E</b> <b>Page_numbers</b>	Page numbers of the article <i>Page #</i>
<b>F</b> <b>Title of Publication</b>	Title of the article <i>Article title</i>
<b>G</b> <b>Author(s)</b>	Author's name (if more than one author, list all authors' names separated by comma) <i>Surname, first name</i>
<b>H</b> <b>N_authors</b>	Number of authors <i>Number</i>
<b>I</b> <b>Gender</b>	Sex of authors (if more than one author, list all authors' genders separated by comma) <i>F: female</i> <i>M: male</i>

<b>J</b>	<b>Location</b>	Location of author's institutional affiliation (country). Based on ISO 3166 Alpha2 country codes (if more than one author, list country codes separated by comma) <i>ISO country code</i>
<b>K</b>	<b>Discipline</b>	Political science sub-discipline of the article <i>1: American</i> <i>2: Comparative</i> <i>3: International Relations</i>
<b>L</b>	<b>Paradigm</b>	Article applies a qualitative or quantitative method (to be distinguished from the culture)s <i>0: Quantitative article</i> <i>1: Qualitative article</i> <i>3: Multi-method article combining quantitative and qualitative methods</i>

Note : The term in brackets in the column "Item" is the label that we use for plotting.

*Dimension 1: Individual cases*

<b>Item</b>		<b>Code</b>	<b>Expectations according to ATTC</b>	
			<b>Quantitative</b>	<b>Qualitative</b>
<b>1</b>	<b>Explain outcome in individual case (Individual outcome)</b>	<p><i>No (0):</i> The outcome of no individual case is explained explicitly</p> <p><i>Yes (1):</i> The outcome of at least one individual case is explained explicitly</p> <p><u>Note:</u> Cases should be distinguished from observations. Following ATTC, quantitative studies are always coded</p>	No	Yes

		“0” no case is explained in detail. Cases are then defined as what receives the values and are in single-country survey studies the individuals. <sup>39</sup>		
2	<b>Cross-case vs. within-case level (Level of analysis)</b>	<p><i>Cross-case (0):</i> Empirical analysis of causal effects and cross-case regularities</p> <p><i>Within-case (1):</i> Empirical analysis of within-case processes and mechanisms</p> <p><i>Both (3):</i> Empirical analysis of cross-case regularities and within-case mechanisms</p> <p><u>Note:</u> Cross-case analysis is interested in an effect (present/absent, marginal effect, positive/negative, increasing/decreasing, necessity/sufficiency). Within-case analysis must have a temporal element in linking cause to effect and a focus on a process.</p>	Cross-case	Within-case
3	<b>Causal mechanism (Mechanism)</b>	<p><i>No (0):</i> Causal mechanism is not empirically analyzed</p> <p><i>Yes (1):</i> Causal mechanism is empirically analyzed</p> <p><u>Note:</u> Mechanism is empirically studied when the analysis is a within-case analysis trying to answer a why-question by linking cause to effect. Speaking of a “mechanism” in the analysis is not sufficient for coding “1” because the meaning of “mechanism” and might not coincide with meaning in ATTC.</p>	No	Yes

<sup>39</sup> In chapter 17 of ATTC, it says it is “rare” for quantitative studies to explain individual cases. A close reading of chapter 3 suggests that this can never be the case because it requires *qualitative* within-case evidence.

4	<b>Process tracing (Process tracing)</b>	<p><i>No (0):</i> Analysis of within-case processes is not part of the empirical analysis</p> <p><i>Yes (1):</i> Analysis of within-case processes is part of the empirical analysis</p> <p><u>Note:</u> Process tracing is given when a process is reconstructed in the empirical analysis. It is not sufficient to report isolated within-case evidence that does not constitute a process.</p>	No	Yes
5	<b>Counterfactual analysis (Counterfactual level)</b>	<p><i>Yes: Cross-case (0):</i> Explicit discussion of cross-case counterfactuals. Effect of extreme values in the sample.</p> <p><i>Yes: Within-case (1):</i> Explicit discussion of within-case counterfactuals. Discussion of possible alternative historical processes.</p> <p><u>Note:</u> Quantitative counterfactuals ask for marginal effects or predicted outcomes if variables were taking specific values not necessarily observed in the data. Qualitative counterfactuals “rerun the history of one or more specific cases” (ATTC, 223), indicated by considering alternative paths and “what-if” discussions. Need to be substantial and cover multiple sentences or one paragraph at least.</p>	Yes (Cross-case, 0)	Yes (Within-case, 1)

Dimension 2: Causality and causal models

Item		Code	Expectations according to ATTC	
			Quantitative	Qualitative
6	<b>Individual variable is at focus (Single variable)</b>	<p><i>Yes (0)</i>: Discussion of causal effect of individual variable(s) on the outcome</p> <p><i>No (1)</i>: No discussion of causal effect of individual variable(s) on the outcome</p> <p><u>Note</u>: A single variable and its effect need to be explicitly discussed.</p> <p>Overlaps with items 7 and 8 because the non-focus on individual variables implies the focus on set relations and conjunctions (224).</p>	Yes	No
7	<b>Configurations; interaction terms (Interaction)</b>	<p><i>No (0)</i>: Causal model does not contain causal configurations and interactions between variables</p> <p><i>Yes (1)</i>: Causal model does contain causal configurations and interactions between variables</p> <p><u>Note</u>: Interaction/configuration should be part of the empirical analysis, not only theorized. Squared multiplicative interaction terms count as ordinary interactions to be coded “1”.</p>	No	Yes
8	<b>Causal effect (Causal effect)</b>	<p><i>Average Treatment Effect (ATE) (0)</i>: Causal effect is conceptualized as Average Treatment Effect</p> <p><i>Set Logic (1)</i>: Causal effect is conceptualized as necessary and/or sufficient condition</p> <p><u>Note</u>: For ATTC, some treatment effect is more important than that it is the <i>average</i> treatment effect. We treat “treatment effect” synonymously with “marginal effect”, “causal effect”, “effect” and variants like the local average treatment effect. Thinking in terms of set logic should be explicit and use corresponding language: is sufficient for,</p>	ATE	Set Logic

		presence of X leads to presence of Y, X produces/generates/brings about Y, if-X-then-Y, only-if-X-then-Y, INUS condition, SUIN condition		
9	<b>Purpose of counterfactual (Counterfactual purpose)</b>	<p><i>After causal inference (0):</i> Explicit <i>counterfactual</i> has no relevance for causal inference. An explicit discussion of <i>counterfactuals</i> happens ex post for interpretation of results</p> <p><i>Prior to causal inference (1):</i> <i>Counterfactual</i> is central for causal inference</p> <p><u>Note:</u> Identical with item 5.</p>	After causal inference	Prior to causal inference
10	<b>Equifinality (Equifinality)</b>	<p><i>Implicit (0):</i> No discussion of multiple causal paths or equifinality (including alternative explanations and control variables)</p> <p><i>Explicit (1):</i> Explicit discussion of multiple causal paths or equifinality (including alternative explanations and control variables)</p> <p><u>Note:</u> We read ATTC ambiguously regarding equifinality. The text says that the complete quantitative model captures equifinality and represents an infinite number of paths. In contrast to qualitative research, this makes it useless to speak of a small number of specific paths. We find ATTC's reading of quantitative practices in discord with the usual understanding and with how empirical researchers discuss control variables in the articles. Multiple variables are part of one model, but they are rival variables. <i>Given</i> the estimated marginal effect, values on different variables can lead to the same value on the outcome and independently of the other variables. Thus, we find that equifinality in quantitative research is possible and meaningful to discuss. For assigning a code of "1", the discussion of equifinality</p>	Implicit	Explicit

		must be sufficiently deep and go beyond only mentioning control variables		
<b>11</b>	<b>Aggregation in causal model (Aggregation)</b>	<p><i>Additive, log-linear (0)</i>: Causal effects add up (log-)linearly or are linear in the <i>link</i>-function</p> <p><i>Set Logic (1)</i>: Causal effects/mechanisms follow set-theoretic rules of aggregation (logical AND/minimum; OR/maximum)</p> <p><u>Note</u>: Set-theoretic aggregation should be explicit and involve verbal “and” or “or” representing logical AND or OR, or reference to minimum or maximum scoring rule. Aggregation in causal model is coded “0” when variables are argued to have independent effects (implying additive effects). Linearity need not be explicitly mentioned because there are more link functions than log-linearity that do fall under the “0” code. Coded “99” if no multivariate model is used in the analysis (e.g., because model is bivariate).</p>	Additive/log-linear	Set Logic

*Dimension 3: Populations and data*

Item	Code	Expectations according to ATTC		
		Quantitative	Qualitative	
<b>12</b>	<b>Scope (Scope of inference)</b>	<p><i>Broad (0)</i>: Conclusions are generalized over a large population and for many different contexts</p> <p><i>Narrow (1)</i>: Conclusions are drawn only for a small number of cases and within narrowly defined contexts</p> <p><u>Note</u>: Broad scope should be explicitly claimed by mentioning a time period; another geographic region; other policy fields etc. or stating that results “should hold widely”, “are broadly generalizable” etc. Scope is coded as narrow when this is not mentioned or when article explicitly states that</p>	Broad	Narrow

		generalization is not intended; unlikely; one should be careful with generalizing etc.		
<b>13</b>	<b>Case selection (Case selection)</b>	<p><i>Representative (0)</i>: Sample is representative for population (selection of representative cases or random sampling)</p> <p><i>Specific (1)</i>: Sample focuses on theoretically or substantially important cases; set theoretic case selection</p> <p><u>Note</u>: Choice of cases based on data availability is coded “99”. No discussion of case selection strategy is coded “99”. Choice of cases because of good comparability or based on “Mill’s Methods” is coded “99” because choice is then guided by the goal to construct a specific comparison. Random sampling must be explicitly claimed or evident (survey data). “0” is assigned if data is population because generalization to more cases is not possible (unless article explicitly mentions idea of sampling from superpopulation).</p>	Representative	Specific
<b>14</b>	<b>Selection on dependent variable (Selection on Y)</b>	<p><i>No (0)</i>: Variation on the dependent variable (outcome) is not truncated</p> <p><i>Yes (1)</i>: Limited variance on the dependent variable (outcome)</p> <p><u>Note</u>: Authors need to explain explicitly that they focus on a limited range of values on the outcome.</p>	No	Yes
<b>15</b>	<b>Data Format (Data format)</b>	<p><i>Case-based (0)</i>: Each case is a „row“ in a dataset</p> <p><i>Configurative/typology (1)</i>: „Rows“ in the dataset are conceptual types/logical configurations of independent variables</p> <p><u>Note</u>: Configurational view (see item 6) implies configurative data format. If this view is not taken and data not in a configurational format, we code it “0”.</p>	Case-based	Configurative/typology
<b>16</b>	<b>Triangular data (Triangular data)</b>	<i>Transformation (0)</i> : Triangular data distribution is interpreted as heteroskedasticity. Solved by data transformation	Heteroskedasticity/ data transformation	Set-relation/ no data transformation

		<p><i>Interpretation (1):</i> Triangular data distribution is interpreted as evidence for a particular set relation. No data transformation</p> <p><u>Note:</u> We code “0” when data is transformed because of actual heteroscedasticity, or concerns about it, or when authors control for it without testing (e.g., via robust standard errors). Coded “1” when article states that triangular data pattern was detected and that it is taken as evidence for a set relation.</p>		
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*Dimension 4: Concepts and measurement*

Item		Code	Expectations according to <i>ATTC</i>	
			Quantitative	Qualitative
17	<b>Terminology (Terminology)</b>	<p><i>Variables/indicators (0):</i> Measurement in terms of variables and indicators</p> <p><i>Concepts/data (1):</i> Measurement in terms of concepts and their relationship to empirical data</p> <p><u>Note:</u> Quantitative means that latent variables cause indicators . This is usually not made explicit, but we code “0” when researchers speak of “operationalization” and “indicators”. Qualitative measurement is given when other terminology is used, e.g., “requirements” instead of indicators. “Operationalization” or “indicator” should not be used then. Concept definition should be significant and more than one or two sentences (e.g., a paragraph). If article uses terminology that can be subsumed under both cultures, we code in favor of the culture (quantitative culture for quantitative article and qualitative culture for qualitative article).</p>	Variables/indicators	concepts/data
18	<b>Ontology (Ontology)</b>	<i>constructs (0):</i> Concepts are unobservable/latent constructs that are operationalized by measurable indicators	Operationalization of latent concepts	Conceptualization

		<p><i>conceptualization (1)</i>: Measurement based on multi-dimensional and specified concepts</p> <p><u>Note</u>: To be coded “1”, the description of research variables needs to entail an explicit discussion of the attributes or characteristics constituting the concept. Further, the concept needs to include at least two constituent elements (attributes) and a discussion on how they are related. Else, we code “0”.</p>		
19	<b>Variation (Variation)</b>	<p><i>Complete (0)</i>: Analysis and explanation of the full range of variance</p> <p><i>Zones (1)</i>: Focus on certain “zones” of variance</p> <p><u>Note</u>: Same as item 14, but extends to variance on cause.</p>	Complete	Zones
20	<b>Variable transformation rationale (Transformation)</b>	<p><i>Yes (0)</i>: Values are transformed to conform to statistical assumptions and to allow for inference</p> <p><i>No (1)</i>: Variables are not transformed without conceptual reasons</p> <p><u>Note</u>: Transformation can be explicitly discussed or implicit, e.g., by simply noting that population size is logged because taking natural log is standard for transforming distribution. Conceptual transformation should be explicitly discussed.</p>	Yes	No
21	<b>Typologies (Typologies)</b>	<p><i>Exclusive (0)</i>: Categories are mutually exclusive</p> <p><i>Overlapping (1)</i>: Categories allow for overlapping membership of cases</p> <p><u>Note</u>: Self-explanatory.</p>	Exclusive	Exclusive or overlapping

*Dimension 5: Asymmetry*

Item		Code	Expectations according to ATTC	
			Quantitative	Qualitative
22	<b>Explaining 0s different than explaining 1s (Explanation asymmetric)</b>	<p><i>No (0)</i>: Symmetric explanation; the same model and variables explain absence and presence of the outcome</p> <p><i>Yes (1)</i>: Asymmetric explanation; absence and presence of outcome are explained by different models and variables</p>	No	Yes

		<u>Note:</u> Explanation is symmetric when it contains a contrast “Higher X, higher Y”, “X, as opposed to not-X, leads to Y, as opposed to not-Y”. Contrast on X or Y is enough because it implies contrast on the other variable. Asymmetric explanation otherwise.		
23	<b>Concept and its opposite (Concept asymmetric)</b>	<i>No (0):</i> Symmetric concept: One variable captures the complete continuum of the concept <i>Yes (1):</i> Different definitions and indicators are used to measure the concept and its opposite <u>Note:</u> Different definitions and indicators for concept and its opposite should be explicit. Otherwise coded “0”.	No	Yes
24	<b>Counterfactual <math>x_i \rightarrow x_j</math> different from counterfactual <math>x_j \rightarrow x_i</math> (Counterfactual asymmetric)</b>	<i>No (0):</i> <i>counterfactual</i> is symmetric <i>Yes (1):</i> <i>counterfactual</i> is asymmetric <u>Note:</u> Linked to item 5 and 9. Asymmetry of counterfactual should be stated explicitly (“the reverse does not hold”, etc.). Symmetric counterfactual establishes a comparison (“as opposed to”, “in comparison with” etc.).	No	Yes
25	<b>2x2 tables when exchanging (0, 1) and (1, 0) cells (Tables asymmetric)</b>	<i>Symmetric (0):</i> Relation between variables does not change when interchanging rows and columns of a 2x2 matrix <i>Asymmetric (1):</i> Relation between variables changes, when interchanging rows and columns of a 2x2 matrix <u>Note:</u> Correlates positively with items 22 and 23 and same notes apply.	Symmetric	Asymmetric

## B. Sample

Authors	Year	Title	Journal	Vol	Issue	Pages
Wood, B. D.; Anderson, J. E.	1993	The Politics of United States Antitrust Regulation	AJPS	37	1	1–39
Krause, G. A.	1994	Federal Reserve Policy Decision Making - Political and Bureaucratic Influences	AJPS	38	1	124–144
Hagle, T. M.	1993	“Freshman Effects” for Supreme Court Justices	AJPS	37	4	1142–1157
Berkman, M. B.	1994	State Legislators in Congress: Strategic Politicians, Professional Legislatures, and the Party Nexus	AJPS	38	4	1025–1055
Koubi, V.	1993	International Tensions and Arms Control Agreements	AJPS	37	1	148–164
Ethridge, M. E.	1991	Minority Power and Madisonianism	AJPS	35	2	335–356
Clinton, R. L.	1994	Game Theory, Legal History, and the Origins of Judicial Review: A Revisionist Analysis of Marbury v. Madison	AJPS	38	2	285–302
Chong, D.	1993	How People Think, Reason, and Feel about Rights and Liberties	AJPS	37	3	867–899
Golden, M.	1992	The Politics of Job Loss	AJPS	36	2	408–430
Blake, C.	1994	Social Pacts and Inflation Control in New Democracies: The Impact of “Wildcat Cooperation” in Argentina and Uruguay	AJPS	27	3	381–401
Berinsky, A. J.	2002	Silent Voices: Social Welfare Policy Opinions and Political Equality in America	AJPS	46	2	276–287
Durr, R. H.; Martin, A. D.; Wolbrecht, C.	2000	Ideological Divergence and Public Support for the Supreme Court	AJPS	44	4	768–776
Fox, R. L.; Lawless, J. L.	2004	Entering the Arena? Gender and the Decision to Run for Office	AJPS	48	2	264–280
Lee, F. E.	2003	Geographic politics in the United States House of Representatives: Coalition building and distribution of benefits	AJPS	47	4	714–728
Vanberg, G.	2001	Legislative-judicial relations: A game-theoretic approach to constitutional review	AJPS	45	2	346–361

Drezner, D. W.	2000	Ideas, bureaucratic politics, and the crafting of foreign policy	AJPS	44	4	733–749
Rudolph, C.	2003	Security and the political economy of international migration	AJPS	97	4	603-620
Boylan, D.	2001	Democratization and Institutional Change in Mexico	AJPS	34	1	3-29
Knill, C.; Lehmkuhl, D.	2002	The national impact of European Union regulatory policy: Three Europeanization mechanisms	AJPS	41	2	255-280
Koremenos, B.	2001	Loosening the ties that bind: A learning model of agreement flexibility	AJPS	55	2	289-325
Leifeld, P.; Schneider, V.	2012	Information Exchange in Policy Networks	AJPS	56	3	731–744
Lubell, M.; Henry, A. D.; McCoy, M.	2010	Collaborative Institutions in an Ecology of Games	AJPS	54	2	287–300
Trager, R. F.; Vavreck, L.	2011	The Political Costs of Crisis Bargaining: Presidential Rhetoric and the Role of Party	AJPS	55	3	526–545
Hirano, S.; Snyder, Jr. J. M.	2009	Using Multimember District Elections to Estimate the Sources of the Incumbency Advantage	AJPS	53	2	292–306
Corstange, D.; Marinov, N.	2012	Taking Sides in Other People’s Elections: The Polarizing Effect of Foreign Intervention	AJPS	56	3	655–670
Saurugger, S.	2010	The social construction of the participatory turn: The emergence of a norm in the European Union	AJPS	49	4	471-495
Cottrell, M.	2009	Legitimacy and institutional replacement: The convention of certain conventional weapons and the emergence of the mine ban treaty	AJPS	63	2	217-248
Rathbun, B.	2011	Before hegemony: Generalized trust and the creation and design of international security organizations	AJPS	65	2	243-273
Collins, K.	2007	Ideas, networks and Islamist movements: Evidence from Central Asia and the Caucasus	AJPS	60	1	64-96
Kelemen, R.; Vogel, D.	2010	Trading places: The role of the United States and the European Union in international environmental politics	AJPS	43	4	427-456
Dixon, W. J.	1994	Democracy and the Peaceful Settlement of International Conflict	APSR	88	1	14–32
Muller, E. N.; Dietz, H. A.; Finkel, S. E.	1991	Discontent and the Expected Utility of Rebellion: The Case of Peru	APSR	85	4	1261–1282
Richardson, B. M.	1991	European Party Loyalties Revisited	APSR	85	3	751–775

Cohen, C. J.; Dawson, M. C.	1993	Neighborhood Poverty and African American Politics	APSR	87	2	286–302
MacDonald, S. E.; Listhaug, O.; Rabinowitz, G.	1991	Issues and Party Support in Multiparty Systems	APSR	85	4	1107–1131
Hopf, T.	1991	Polarity, the Offense-Defense Balance, and War	APSR	85	2	475–493
Heimann, C. F. L.	1993	Understanding the <i>Challenger</i> Disaster: Organizational Structure and the Design of Reliable Systems	APSR	87	2	421–435
Geddes, B.	1991	A Game Theoretic Model of Reform in Latin American Democracies	APSR	85	2	371–392
Cox, R.	1992	After Corporatism: A Comparison of the Role of Medical Professionals and Social Workers in the Dutch Welfare State	APSR	24	4	532-552
Hey, J.; Kuzma, L.	1993	Anti-United States Foreign Policy of Dependent States: Mexican and Costa Rican Participation in Central American Peace Plans	APSR	26	1	30-62
Ansolabehere, S.; Gerber, A.; Snyder, J.	2002	Equal votes, equal money: Court-ordered redistricting and public expenditures in the American states	APSR	96	4	767–777
Schickler, E.	2000	Institutional change in the House of Representatives, 1867-1998: A test of partisan and ideological power balance models	APSR	94	2	269–288
Martin, C. J.; Swank, D.	2004	Does the organization of capital matter? Employers and active labor market policy at the national and firm levels	APSR	98	4	593–611
Peceny, M.; Beer, C. C.; Sanchez-Terry, S.	2002	Dictatorial peace?	APSR	96	1	15–26
Kahn, K. F.; Kenney, P. J.	2002	The slant of the news: How editorial endorsements influence campaign coverage and citizens' views of candidates	APSR	96	2	381–394
Pape, R. A.	2003	The strategic logic of suicide terrorism	APSR	97	3	343–361
Peterson, M. J.	2004	Diverging orbits: Situation definitions in creation of regimes for broadcast and remote sensing satellites	APSR	98	2	277–291
Narizny, K.	2003	Both Guns and Butter, or Neither: Class Interests in the Political Economy of Rearmament	APSR	97	2	203-220
Luong, P. J.; Weinthal, E.	2004	Contra coercion: Russian tax reform, exogenous shocks, and negotiated institutional change	APSR	98	1	139–152

Hacker, J. S.	2004	Privatizing risk without privatizing the welfare state: The hidden politics of social policy retrenchment in the United States	APSR	98	2	243–260
Bateson, R.	2012	Crime Victimization and Political Participation	APSR	106	3	570–587
Gerber, A. S.; Huber, G. A.; Doherty, D.; Dowling, C. M.; Ha, S. E.	2010	Personality and Political Attitudes: Relationships across Issue Domains and Political Contexts	APSR	104	1	111–133
Healy, A.; Malhotra, N.	2009	Myopic Voters and Natural Disaster Policy	APSR	103	3	387–406
Greenhill, B.; Mosley, L.; Prakash, A.	2009	Trade-based Diffusion of Labor Rights: A Panel Study, 1986-2002	APSR	103	4	669–690
Lindqvist, E.; Östling, R.	2010	Political Polarization and the Size of Government	APSR	104	3	543–565
Snyder, J.; Borghard, E. D.	2011	The Cost of Empty Threats: A Penny, Not a Pound	APSR	105	3	437–456
Favretto, K.	2009	Should Peacemakers Take Sides? Major Power Mediation, Coercion, and Bias	APSR	103	2	248–263
Haggard, S.; Kaufman, R. R.	2012	Inequality and Regime Change: Democratic Transitions and the Stability of Democratic Rule	APSR	106	3	495–516
Skarbek, D.	2011	Governance and Prison Gangs	APSR	105	4	702–716
Lee, T.	2009	The Armed Forces and transitions from authoritarian rule	APSR	42	5	640-669
Rosh, R. M.; Gonick, L. S.	1990	The Structural Location of Third World States within the International Division of Labor	CPS	23	3	355–380
Davis D. R.; Ward, M. D.	1990	The Entrepreneurial State: Evidence from Taiwan	CPS	23	3	314–333
Lee, A. R.	1993	Culture Shift and Popular Protest in South Korea	CPS	26	1	63–80
Togebly, L.	1994	Political Implications of Increasing Numbers of Women in the Labor Force	CPS	27	2	211–240
Iversen, T.	1994	The Logics of Electoral Politics: Spatial, Directional, and Mobilizational Effects	CPS	27	2	155–189

Kitschelt, H.	1994	Austrian and Swedish Social Democrats in Crisis: Party Strategy and Organization in Corporatist Regimes	CPS	27	1	3–39
Pontusson, J.	1993	The Comparative Politics of Labor-Initiated Reforms: Swedish Cases of Success and Failure	CPS	25	4	548–578
Zhang, B.	1994	Corporatism, Totalitarianism, and Transitions to Democracy	CPS	27	1	108–136
King, D. S.; Rothstein, B.	1993	Institutional Choices and Labor Market Policy: A British-Swedish Comparison	CPS	26	2	147–177
Garrett, G.	1993	The Politics of Structural Change: Swedish Social Democracy and Thatcherism in Comparative Perspective	CPS	25	4	521–547
Rohrschneider, R.; Whitefield, S.	2004	Support for foreign ownership and integration in Eastern Europe - Economic interests, ideological commitments, and democratic context	CPS	37	3	313–339
Harper, M. A. G.	2000	Economic voting in postcommunist Eastern Europe	CPS	33	9	1191–1227
Kreppel, A.	2002	Moving beyond procedure - An empirical analysis of European Parliament legislative influence	CPS	35	7	784–813
Hofmann, [SR]	2004	Islam and democracy - Micro-level indications of compatibility	CPS	37	6	652–676
Aguilar, E. E.; Pacek, A. C.	2000	Macroeconomic conditions, voter turnout and the working-class/economically disadvantaged party vote in developing countries	CPS	33	8	995–1017
Hamann, K.	2000	Linking policies and economic voting - Explaining reelection in the case of the Spanish Socialist Party	CPS	33	8	1018–1048
Henderson, S. L.	2002	Selling civil society - Western aid and the nongovernmental organization sector in Russia	CPS	35	2	139–167
Wood, E. J.	2001	An insurgent path to democracy - Popular mobilization, economic interests, and regime transition in South Africa and El Salvador	CPS	34	8	862–888
Thelen, K.; van Wijnbergen, C.	2003	The paradox of globalization - Labor relations in Germany and beyond	CPS	36	8	859–880
Kenney, C. D.	2003	The death and rebirth of a party system, Peru 1978-2001	CPS	36	10	1210–1239
Garland, M.; Biglaiser, G.	2009	Do Electoral Rules Matter? Political Institutions and Foreign Direct Investment in Latin America	CPS	42	2	224–251

Omgba, L.	2009	On the Duration of Political Power in Africa: The Role of Oil Rents	CPS	42	3	416-436
Kenyon, T.; Naoi, M.	2010	Policy Uncertainty in Hybrid Regimes: Evidence from Firm-Level Surveys	CPS	43	4	486–510
Jhee, B.	2007	Economic Origins of Electoral Support for Authoritarian Successors: A Cross-National Analysis of Economic Voting in New Democracies	CPS	41	3	362-388
Jordan, J.	2010	Institutional Feedback and Support for the Welfare State: The Case of National Health Care	CPS	43	7	862-885
Hsueh, R.	2012	China and India in the Age of Globalization: Sectoral Variation in Post-liberalization Reregulation	CPS	45	1	32–61
Weyland, K.	2010	The Diffusion of Regime Contention in European Democratization, 1830-1940	CPS	43	08-sep	1148–1176
Callaghan, H.	2009	Insiders, Outsiders, and the Politics of Corporate Governance: How Ownership Structure Shapes Party Positions in Britain, Germany, and France	CPS	42	6	733–762
Newman, A.	2010	What You Want Depends on What You Know: Firm Preferences in an Information Age	CPS	43	10	1286-1312
Zuber, C.	2011	Understanding the Multinational Game: Toward a Theory of Asymmetrical Federalism	CPS	44	5	546-571
Sørensen, R.	1992	Fiscal illusions: nothing but illusions?	EJPR	22	3	279-305
Väisänen, I.	1992	Conflict and consensus in social policy development	EJPR	22	3	307-327
Boreham, P.; Compston, H.	1992	Labour movement organization and political intervention	EJPR	22	2	143-170
Heidar, K.	1994	The polymorphic nature of party membership	EJPR	25	1	61-86
Høst, V.; Paldam, M.	1990	An international element in the vote?	EJPR	18	2	221-239
Traxler, F.	1992	Interests, politics, and European integration	EJPR	22	2	193-217
Grant, W.	1992	Models of interest intermediation and policy formation applied to an internationally comparative study of the dairy industry	EJPR	21	1.2	53-68
De Winter, L.	1993	The selection of party presidents in Belgium	EJPR	24	3	233-256
Yishai, Y.	1992	From an iron triangle to an iron duet?	EJPR	21	1.2	91-108
Bergqvist, C.	1991	Corporatism and gender equality	EJPR	20	2	107-125
Grendstad, G.	2003	Comparing political orientations: Grid-group theory versus the left-right dimension in the five Nordic countries	EJPR	42	1	1-21

Mishler, W.; Rose, R.	2002	Learning and re-learning regime support: The dynamics of post-communist regimes	EJPR	41	1	5-36
Garrett, G.; Mitchell, D.	2001	Globalization, government spending and taxation in the OECD	EJPR	39	2	145-177
Barrington, L. W.	2002	Examining rival theories of demographic influences on political support: The power of regional, ethnic, and linguistic divisions in Ukraine	EJPR	41	4	455-491
Van Deth, J. W.	2000	Interesting but irrelevant: Social capital and the saliency of politics in Western Europe	EJPR	37	2	115-147
Joppke, C.; Marzal, E.	2004	Courts, the new constitutionalism and immigrant rights: The case of the <i>French Conseil Constitutionnel</i>	EJPR	43	6	823-844
Compston, H.	2003	Beyond corporatism: A configurational theory of policy concertation	EJPR	42	6	787-809
Tews, K.; Busch, P.; Jørgens, H.	2003	The diffusion of new environmental policy instruments	EJPR	42	4	569-600
Braun, D.; Bullinger, A.-B.; Wälti, S.	2002	The influence of federalism on fiscal policy making	EJPR	41	1	115-145
Batory, A.; Sitter, N.	2004	Cleavages, competition and coalition-building: Agrarian parties and the European question in Western and East Central Europe	EJPR	43	4	523-546
Häusermann, S.	2010	Solidarity with whom? Why organised labour is losing ground in continental pension politics	EJPR	49	2	223-256
Traxler, F.	2010	The long-term development of organised business and its implications for corporatism: A cross-national comparison of membership, activities and governing capacities of business interest associations, 1980–2003	EJPR	49	2	151-173
Baekgaard, M.	2011	Committee bias in legislatures with a high degree of party cohesion: Evidence from Danish municipalities	EJPR	50	3	315-335
Shu, M.	2009	Coping with a two-dimensional political space: Party mobilization in referendums on European integration	EJPR	48	3	397-431
Elenbaas, M.; De Vreese, C.; Boomgarden, H.; Schuck, A.	2012	The impact of information acquisition on EU performance judgements	EJPR	51	6	728-755
Hepburn, E.	2011	‘Citizens of the region’: Party conceptions of regional citizenship and immigrant integration	EJPR	50	4	504-529
Doherty, B.; Hayes, G.	2012	Tactics, traditions and opportunities: British and French crop-trashing actions in comparative perspective	EJPR	51	4	540-562

Trampusch, C.	2010	Employers, the state and the politics of institutional change: Vocational education and training in Austria, Germany and Switzerland	EJPR	49	4	545-573
Neumayer, L.	2008	Euroscepticism as a political label: The use of European Union issues in political competition in the new Member States	EJPR	47	2	135-160
Blavoukos, S.; Pagoulatos, G.	2011	Accounting for coalition-building in the European Union: Budget negotiations and the south	EJPR	50	4	559-581
Oneal, J. R.	1990	The theory of collective action and burden sharing in NATO	IO	44	3	379-402
Eichenberg, R. C.; Dalton, R. J.	1993	Europeans and the European Community: the dynamics of public support for European integration	IO	47	4	507-534
Strang, D.; Chang, Patricia M. Y. C.	1993	The International Labor Organization and the welfare state: institutional effects on national welfare spending, 1960–80	IO	47	2	235-262
Nollen, S. D.; Quinn, D. P.	1994	Free trade, fair trade, strategic trade, and protectionism in the U.S. Congress, 1987–88	IO	48	3	491-525
Mansfield, E. D.	1992	The Concentration of Capabilities and International Trade	IO	46	3	731-764
Krauss, E. S.; Reich, S.	1992	Ideology, interests, and the American executive: toward a theory of foreign competition and manufacturing trade policy	IO	46	4	857-897
Tètreault, M. A.	1991	Autonomy, necessity, and the small state: ruling Kuwait in the twentieth century	IO	45	4	565-591
Burley, Anne-Marie; Mattli, W.	1993	Europe Before the Court: A Political Theory of Legal Integration	IO	47	1	41-76
Haas, P. M.	1992	Banning chlorofluorocarbons: epistemic community efforts to protect stratospheric ozone	IO	46	1	187-224
Stein, J. G.	1994	Political learning by doing: Gorbachev as uncommitted thinker and motivated learner	IO	48	2	155-183
Hallerberg, M.	2002	Veto Players and the Choice of Monetary Institutions	IO	56	4	775-802
Blaydes, L.	2004	Rewarding Impatience: A Bargaining and Enforcement Model of OPEC	IO	58	2	213-237
Herrmann, R. K.; Shannon, V. P.	2001	Defending International Norms: The Role of Obligation, Material Interest, and Perception in Decision Making	IO	55	3	621-654
Mansfield, E. D.; Milner, H. V.; Rosendorff, B. P.	2002	Why Democracies Cooperate More: Electoral Control and International Trade Agreements	IO	56	3	477-513
Gartzke, E.; Li, Q.; Boehmer, C.	2001	Investing in the Peace: Economic Interdependence and International Conflict	IO	55	2	391-438

Kydd, A.; Walter, B. F.	2002	Sabotaging the Peace: The Politics of Extremist Violence	IO	56	2	263-296
Tallberg, J.	2002	Paths to Compliance: Enforcement, Management, and the European Union	IO	56	3	609-643
Meunier, S.	2000	What Single Voice? European Institutions and EU–U.S. Trade Negotiations	IO	54	1	103-135
Schimmelfennig, F.	2001	The Community Trap: Liberal Norms, Rhetorical Action, and the Eastern Enlargement of the European Union	IO	55	1	47-80
Peceny, M.; Stanley, W.	2001	Liberal Social Reconstruction and the Resolution of Civil Wars in Central America	IO	55	1	149-182
Morrison, K. M.	2009	Oil, Nontax Revenue, and the Redistributive Foundations of Regime Stability	IO	63	1	107-138
Kucik, J.; Reinhardt, E.	2008	Does Flexibility Promote Cooperation? An Application to the Global Trade Regime	IO	62	3	477-505
Pelc, K. J.	2010	Constraining Coercion? Legitimacy and Its Role in U.S. Trade Policy, 1975–2000	IO	64	1	65-96
Baum, M. A.; Groeling, T.	2010	Reality Asserts Itself: Public Opinion on Iraq and the Elasticity of Reality	IO	64	3	443-479
Büthe, T.; Major, S.; M. Souza, A. De	2012	The Politics of Private Foreign Aid: Humanitarian Principles, Economic Development Objectives, and Organizational Interests in NGO Private Aid Allocation	IO	66	4	571-607
Caporaso, J. A.; Tarrow, S.	2009	Polanyi in Brussels: Supranational Institutions and the Transnational Embedding of Markets	IO	63	4	593-620
Walter, S.	2008	A New Approach for Determining Exchange-Rate Level Preferences	IO	62	3	405-438
Hayes, J.	2012	Securitization, Social Identity, and Democratic Security: Nixon, India, and the Ties That Bind	IO	66	1	63-93
Carpenter, R. C.	2011	Vetting the Advocacy Agenda: Network Centrality and the Paradox of Weapons Norms	IO	65	1	69-102
Branch, J.	2011	Mapping the Sovereign State: Technology, Authority, and Systemic Change	IO	65	1	1-36
Ember, C. R.; Ember, M.; Russett, B.	1992	Peace Between Participatory Polities: A Cross-Cultural Test of the “Democracies Rarely Fight Each Other” Hypothesis	WP	44	4	573-599
Kim, W.	1992	Power Transitions and Great Power War from Westphalia to Waterloo	WP	45	1	153-172

Mitra, S. K.	1991	Room to Maneuver in the Middle: Local Elites, Political Action, and the State in India	WP	43	3	390-413
Schonhardt-Bailey, C.	1991	Specific Factors, Capital Markets, Portfolio Diversification, and Free Trade: Domestic Determinants of the Repeal of the Corn Laws	WP	43	4	545-569
Huber, J. D.; Powell, G. B.	1994	Congruence between Citizens and Policymakers in Two Visions of Liberal Democracy	WP	46	3	291-326
Bratton, M.; Van De Walle, N.	1994	Neopatrimonial Regimes and Political Transitions in Africa	WP	46	4	453-489
Lepingwell, J. W. R.	1992	Soviet Civil-Military Relations and the August Coup	WP	44	4	539-572
Huang, Y.	1994	Information, Bureaucracy, and Economic Reforms in China and the Soviet Union	WP	47	1	102-134
Barnett, M.	1990	High Politics is Low Politics: The Domestic and Systemic Sources of Israeli Security Policy, 1967-1977	WP	42	4	529-562
Swenson, P.	1991	Bringing Capital Back In, or Social Democracy Reconsidered: Employer Power, Cross-Class Alliances, and Centralization of Industrial Relations in Denmark and Sweden	WP	43	4	513-544
Fish, M. S.	2002	Islam and Authoritarianism	WP	55	1	4-37
Iversen, T.; Cusack, T. R.	2000	The Causes of Welfare State Expansion: Deindustrialization or Globalization?	WP	52	3	313-349
Patterson, D.; Beason, D.	2001	Politics, Pressure, and Economic Policy: Explaining Japan's Use of Economic Stimulus Policies	WP	53	4	499-523
Sambanis, N.	2000	Partition as a Solution to Ethnic War: An Empirical Critique of the Theoretical Literature	WP	52	4	437-483
Golden, M. A.; Chang, E. C. C.	2001	Competitive Corruption: Factional Conflict and Political Malfeasance in Postwar Italian Christian Democracy	WP	53	4	588-622
Alcock, F.	2002	Bargaining, Uncertainty, and Property Rights in Fisheries	WP	54	4	437-461
Ziblatt, D.	2004	Rethinking The Origins of Federalism: Puzzle, Theory, and Evidence from Nineteenth-Century Europe	WP	57	1	70-98
Mares, I.	2003	The Sources of Business Interest in Social Insurance: Sectoral versus National Differences	WP	55	2	229-258
Cornell, S. E.	2002	Autonomy as a Source of Conflict: Caucasian Conflicts in Theoretical Perspective	WP	54	2	245-276

Rutherford, K. R.	2000	The Evolving Arms Control Agenda: Implications of the Role of NGOs in Banning Antipersonnel Landmines	WP	53	1	74-114
Blaydes, L.; Linzer, D. A.	2008	The Political Economy of Women's Support for Fundamentalist Islam	WP	60	4	576-609
Finkel, S. E.; Pérez-Liñán, A.; Seligson, M. A.	2007	The Effects of U.S. Foreign Assistance on Democracy Building, 1990-2003	WP	59	3	404-439
Cao, X.	2012	Global Networks and Domestic Policy Convergence: A Network Explanation of Policy Changes	WP	64	3	375-425
Chang, E.; Golden, M.; Hill, S.	2010	Legislative malfeasance and political accountability	WP	62	2	177-220
Cederman, L.; Girardin, L.; Gleditsch, K.	2009	Ethno-Nationalist Triads: Assessing the Influence of Kin Groups on Civil Wars	WP	61	3	403-437
Nalepa, M.	2010	Captured Commitments: An Analytic Narrative of Transitions with Transitional Justice	WP	62	2	341-380
McNally, C. A.	2012	Sino-Capitalism: China's Reemergence and the International Political Economy	WP	64	4	741-776
Taylor, M.	2009	Institutional Development through Policy-Making: A Case Study of the Brazilian Central Bank	WP	61	3	487-515
Narizny, K.	2012	Anglo-American Primacy and the Global Spread of Democracy: An International Genealogy	WP	64	2	341-373
Wohlforth, W.	2009	Unipolarity, Status Competition, and Great Power War	WP	61	1	28-57

### C. Sampling procedure

The general selection of articles that were coded and evaluated in our analysis is outlined in Section 4.1 of the main article. This section of the Appendix summarizes the five-step sampling procedure through which we identified the articles that underwent coding.

1. After having drafted the codebook, we first had student research assistants identify all original research articles published in the six journals included in this analysis (*American Political Science Review*, *International Organization*, *World Politics*, *American Journal of Political Science*, *European Journal of Political Research*, *Comparative Political Studies*), in the three research periods (1990-1994; 2000-2004; 2008-2012), excluding introductions to special issues, book reviews, review and other summary articles, research notes, as well as all editorial contributions. We chose Anglo-American and European outlets that cover all the major sub-disciplines of political science: American Journal of Political Science (AJPS), American Political Science Review (APSR), Comparative Political Studies (CPS), European Journal of Political Research (EJPR), International Organization (IO) and World Politics (WP). For each method and journal, we distinguished three time periods to test the fifth observable implication: 1990-1994, 2000-2004, and 2008-2012. The first period captures the time before the publication of DSI and the dominance of “the comparative method” in qualitative research. The period 2000-2004 is an interim phase in which we might already see instances of new developments in qualitative methods covering process tracing, set theory, etc. The third period was chosen because it is most likely to show evidence for a qualitative culture (see previous section). Altogether, we selected ten articles per journal-period (five qualitative and five quantitative), which yielded 30 articles per journal, 60 articles per period, and 180 articles in total.

We chose these journals for three reasons. First, an empirical analysis in the appendix of ATTC uses APSR, CPS, IO and WP to measure the incidence of quantitative and qualitative research and specific methods. This makes these journals a straightforward choice for a test of the two-cultures hypothesis. Second, these journals are often considered premier outlets in political science across our period of analysis (Giles and Garand 2007). If two methods cultures exist, we should find evidence for them in journals that are arguably paradigmatic for setting the methods standard in the discipline. Consequently, we find it most likely to identify the expected method patterns in these journals.

Despite these arguments in favor of the chosen journals, there might be the concern that the six journals or a subset of them are least likely to produce confirming evidence for the two-cultures hypothesis. The concern could be rooted in the fact that two of them (AJPS, APSR)

mostly publish quantitative work and that they or others might attract work following a “quantitative, correlational template” that is in discord with the qualitative culture. However, we find it conceptually difficult to argue that a qualitative culture exists except for articles published in six journals—and ATTC does not make such an argument. Empirically, we address the arguments against our choice of journals in two ways.

First, we evaluate whether there are systematic differences between methods practices in the six journals, and between EJPR as the arguably least quantitative outlet and any of the other journals in particular. We find that there are no differences between any pair of journals (see Section 10).

Second, as a robustness test we draw a sample of 30 qualitative articles published in 2018 from journals other than the six main journals (see below for details). If the choice of the main journals biases the analysis against finding evidence for a qualitative culture, we should observe evidence for the expected method practices in the 30 articles. We select articles published in 2018 to additionally take into account that the method cultures might not have fully materialized until 2012. We expect to find evidence for the two cultures in the period of 2008-2012 because ATTC was published in 2012 and it describes the state of the discipline as if the two cultures were existing already. However, one might argue that empirical research needs to catch up with the development of new tools such as process tracing and set-theoretic thinking and that the consequences of the second and third “wave” of qualitative methods can only be seen in the 2010s (Goertz and Mahoney 2013b). If this argument was correct, we should observe that method practices comply with the qualitative culture. On the other hand, the two-cultures hypothesis would be strongly invalidated if we neither find confirming evidence in the 180 articles from the six journals nor in the 30 qualitative articles in a sample of other journals.

2. For an initial test of the general applicability of the codebook and calibration of both authors’ coding routines, we drew a random sample of 10 articles from CPS published in the third research period (2008-2012). We chose CPS because it publishes both qualitative and quantitative work and the third period because we expected to find differences between both methods cultures to be particularly pronounced (see the discussion in Section 4.1 in the main article).
3. We decided to run a second test to further fine-tune the routines based on a new random sample of ten articles each from the three journals that publish a relatively balanced share of qualitative and quantitative articles – CPS, WP and EJPR – again exclusively from articles published in the third period. We realized that simple random sampling stratified by

journal and period was likely to produce skewed samples with too few articles of a given culture. Specifically, our random sample was dominated by quantitative research and included too few qualitative articles. Consequently, we changed the sampling procedure and had trained student research assistants screen all articles identified in Step 1 and to code them as either quantitative or qualitative research, based on an evaluation of the title, abstract and keywords. If these indicated that the article employed some kind of statistical analysis (reporting of effect estimates; quantitative technique; a long period of analysis or high number of cases), the article was coded as “quantitative”; if they indicated a small-n analysis or some form of case study technique (reporting of qualitative technique; small number of cases), the article was coded “qualitative”. We also included a code for mixed method research (MMR) for those articles that combined statistical and case-based analyses. While this pre-selection procedure raised the possibility of measurement error by falsely identifying qualitative research as quantitative and vice versa, this error should not be systematic and should not affect the results of our analysis. The misclassification of articles in the initial, abstract-based sampling stage and their exclusion in stage 2 of the sampling frame does not invalidate the argument that the remaining 180 articles are a random sample. The articles that we excluded once we looked at their content (false positives) have never been part of our target population of articles pursuing a causal research question. All 180 articles that we designated as belonging to the target population (true positives) at the end of the second sampling stage were accessible and could be coded by us. In terms of survey analysis, this means there is no “unit non-response” and no potential for introducing sampling bias. In the next step, we drew a random sample stratified by the journal-period and type of method we had assigned to an article in the previous step. We encountered the problem that for three journal-periods, AJPS1, AJPS2 and APSR1, there were less than 5 qualitative articles. We then drew on qualitative articles in other journals of the same period, selecting one article per journal in ascending order of the alphabet, until we reached five quantitative and qualitative applications. In total, we had to replace seven qualitative ‘slots’ with articles from other journals to meet the target number of 30 qualitative articles per period. Although seven replacement articles out of 90 only represent a small share, one might argue that this strategy is problematic because AJPS and APSR are often taken to be the “most quantitative” journals in the discipline following a “correlational” template (Garand 2005). If true, taking qualitative replacement articles from other journals would slightly bias the results in favor of the two-cultures hypothesis. An additional robustness test that we run shows that excluding the seven replacement articles does not change the results.

Besides, our discussion in Section 3 of the main article shows that we are interested in methods practices across the discipline and the three periods and not in methods cultures in specific journals. For the same reason, we argue that the relatively small number of 30 articles per journal and five articles per method-journal-period is unproblematic.

4. While coding, we realized that including MMR articles into the research sample as fill-ins for qualitative research was likely to bias our analysis against the two cultures hypothesis problems as qualitative research practices in MMR might not be representative for “pure” qualitative practices and should not be treated simply as such. This is in line with ATTC’s treatment of multi-method research, which does not specify clear expectations as to which culture MMR “belongs” (Goertz and Mahoney 2012, 26). Consequently, we discarded all codings of MMR articles, eliminated all further MMR applications from the list of articles to be sampled and relied exclusively on qualitative articles from other journals to fill the missing “slots”, as described in Step 4. It would be interesting to determine what share of MMR articles resembles the quantitative and the qualitative methods culture and to what degree. However, this is a separate research question and not relevant to our interest in whether single-method quantitative and qualitative articles follow their own distinct logic.
5. As noted in the description of Step 3 above, the pre-coding of articles as original research articles or either quantitative or qualitative was sometimes wrong, which we realized when both authors independently coded a sampled article. Whenever at least one of the coders identified a misclassified article, we briefly discussed it and took it out of the sample. We then moved on with the next article on the list as per Step 4, above. All in all, there were 89 wrongly pre-classified articles in the sample. These are listed in Appendix D together with the reason why we excluded them.

#### D. List of sampled and excluded articles

The following list shows those 89 articles that were sampled and pre-screened according to the procedure summarized in Appendix C and turned out to have been misclassified. The “excluded” column specifies the reason for the article’s exclusion as per the following guidelines:

“Excluded” code	Explanation of code	Number of excluded articles
descriptive	Description of event or process without making a causal argument.	26
interpretivist	Interpretivist/ethnographic article that does not follow ATTC’s definition of (neo-)positivist social science making causal arguments	5
no research article	Article does not aim to answer a specific research question, but is of a different genre (e.g., summary of state of the art, introduction to a special issue, presidential address)	4
qualitative	Article was identified as <i>quantitative</i> during the pre-screening, but was designated as a <i>qualitative</i> article upon closer inspection	4
quantitative	Article was identified as <i>qualitative</i> during the pre-screening, but was designated a <i>quantitative</i> article upon closer inspection	26
theory	Article that presents/discusses a concept or theoretical argument, but does not include a full-fledged empirical analysis going beyond illustration. The category of “theory” subsumes informal theory, formal theory, game theory and political theory.	24

<b>Authors</b>	<b>Year</b>	<b>Title</b>	<b>Journal</b>	<b>Vol</b>	<b>Issue</b>	<b>Page</b>	<b>Excluded</b>
Wilkerson, J.	1991	Analyzing Committee Power: A Critique	AJPS	35	3	613–623	quantitative
Sullivan, T.	1991	The Bank Account Presidency: A New Measure and Evidence on the Temporal Path of Presidential Influence	AJPS	35	3	686–723	quantitative
Riker, W. H.; Sened, I.	1991	A Political Theory of the Origin of Property Rights: Airport Slots	AJPS	35	4	951–969	theory
Feldman, S.; Zaller, J.	1992	The Political Culture of Ambivalence: Ideological Responses to the Welfare State	AJPS	36	1	268–307	quantitative
Bawn, K.	1993	The Logic of Institutional Preferences: German Electoral Law as a Social Choice Outcome	AJPS	37	4	965–989	quantitative
Runge, C. F.; Von Witzke, H.	1990	European Community Enlargement and Institutional Choice in the Common Agricultural Policy	AJPS	34	1	254–268	descriptive
Smith, D.; Wanke, J.	1993	Completing the Single European Market: An Analysis of the Impact on the Member States	AJPS	37	2	529–554	quantitative
Strøm, K.; Budge, I.; Laver, M.	1994	Constraints on Cabinet Formation in Parliamentary Democracies	AJPS	38	2	303–335	theory
Lawrence, E. D.; Maltzman, F.; Wahlbeck, P. J.	2001	The politics of Speaker Cannon's committee assignments	AJPS	45	3	551–562	quantitative
Gibson, J. L.	2001	Social networks, civil society, and the prospects for consolidating Russia's democratic transition	AJPS	45	1	51–68	quantitative
Mondak, J. J.; Sanders, M. S.	2003	Tolerance and intolerance, 1976-1998	AJPS	47	3	492–502	quantitative
Thies, M. F.	2001	Keeping tabs on partners: The logic of delegation in coalition governments	AJPS	45	3	580–598	quantitative
Botting, E. H.; Carey, C.	2004	Wollstonecraft's philosophical impact on nineteenth-century American women's rights advocates	AJPS	48	4	707–722	interpretivist
Frederking, B.	2003	Constructing Post-Cold War Collective Security	AJPS	97	3	363–378	interpretivist

Gillman, H.	2002	How political parties can use the courts to advance their agendas: Federal Courts in the United States, 1875-1891	AJPS	96	3	511-524	descriptive
Slantchev, B.	2003	The Power to Hurt: Costly Conflict with Completely Informed States	AJPS	97	1	123-133	theory
Hawkesworth, M.	2003	Congressional Enactments of Race-Gender: Toward a Theory of Raced-Gendered Institutions	AJPS	97	4	529-550	interpretivist
Skocpol, T.; Ganz, M.; Munson, Z.	2000	A Nation of Organizers: The Institutional Origins of Civic Voluntarism in the United States	AJPS	94	3	527-546	descriptive
Wantchekon, L.	2004	The Paradox of "Warlord" Democracy: A Theoretical Investigation	AJPS	98	1	17-33	theory
Hayward, C.	2003	The Difference States Make: Democracy, Identity, and the American City	AJPS	97	4	501-514	theory
Alexander, G.	2002	Institutionalized uncertainty, the rule of law, and the sources of democratic stability	AJPS	35	10	1145-1170	theory
Kitschelt, H.	2000	Citizens, politicians, and party cartellization: Political representation and state failure in post-industrial democracies	AJPS	37	2	149-179	theory
Schmidt, M.	2002	Political performance and types of democracy: Findings from comparative studies	AJPS	41	1	147-163	no research article
Moreno, L.	2003	Europeanisation, mesogovernments and 'safety nets'	AJPS	42	2	271-285	descriptive
Harmel, R.; Tan, A.	2003	Party actors and party change: Does factional dominance matter?	AJPS	42	3	409-424	quantitative
Van Kersbergen, K.	2000	Political allegiance and European integration	AJPS	37	1	ene-17	descriptive
Hiscox, M.	2001	Class versus industry cleavages: Inter-industry factor mobility and the politics of trade	AJPS	55	1	ene-46	quantitative
Magaloni, B.	2010	The Game of Electoral Fraud and the Ousting of Authoritarian Rule	AJPS	54	3	751-765	theory
Clinton, J. D.	2012	Congress, Lawmaking, and the Fair Labor Standards Act, 1971-2000	AJPS	56	2	355-372	quantitative
Fang, S.	2008	The informational role of international institutions and domestic politics	AJPS	52	2	304-321	theory

Pope, J. C.; Treier, S.	2011	Reconsidering the Great Compromise at the Federal Convention of 1787: Deliberation and Agenda Effects on the Senate and Slavery	AJPS	55	2	289–306	quantitative
Bauer, M.; Hartlapp, M.	2010	Much ado about money and how to spend it! Analysing 40 years of annulment cases against the European Union Commission	AJPS	49	2	202–222	quantitative
Barker, L.	1994	Limits of political strategy: A systemic view of the African American experience	APSR	88	1	ene-13	no research article
Baron, D.	1991	A spatial bargaining theory of government formation in parliamentary systems	APSR	85	1	137–164	theory
Peled, Y.	1992	Ethnic democracy and the legal construction of citizenship: Arab citizens of the Jewish state	APSR	86	2	432–443	descriptive
Bartels, L.	1991	Constituency opinion and congressional policy making: The Reagan defense build up	APSR	85	2	457–474	quantitative
Razi, G. H.	1990	Legitimacy, Religion, and Nationalism in the Middle East	APSR	84	1	69–89	theory
Strøm, K.; Leipart, J.	1993	Policy, Institutions, and Coalition Avoidance: Norwegian Governments	APSR	87	4	870–887	theory
Brisbin, R.	1993	Antonin Scalia, William Brennan, and the politics of Expression: A study of legal violence and repression	APSR	87	4	912–927	interpretivist
Stark, A.	1992	Corporate electoral activity, constitutional discourse, and conceptions of the individual	APSR	86	3	626–637	theory
Smith, R.	1993	Beyond Tocqueville, Myrdal, and Hartz: The Multiple Traditions in America	APSR	87	3	549–566	theory
Laitin, D.	1994	The Tower of Babel as a Coordination Game: Political Linguistics in Ghana	APSR	88	3	622–634	descriptive
Thompson, D.	1993	Mediated Corruption: The Case of the Keating Five	APSR	87	2	369–381	descriptive
King, D.	1994	The Nature of Congressional Committee Jurisdictions	APSR	88	1	48–62	descriptive
Schlesinger, J.; Schlesinger, M.	1990	The Reaffirmation of a Multiparty System in France	APSR	84	4	1077–1101	quantitative

Weaver, W.; Longoria, T.	2002	Bureaucracy that kills: Federal sovereign immunity and the discretionary function exception	APSR	96	2	335-349	descriptive
Jha, S.; Wilkinson, S.	2012	Does Combat Experience Foster Organizational Skill? Evidence from Ethnic Cleansing during the Partition of South Asia	APSR	106	4	883-907	quantitative
Warren, M.	2011	Voting with Your Feet: Exit-based Empowerment in Democratic Theory	APSR	105	4	683-701	theory
Walsh, K.	2012	Putting Inequality in Its Place: Rural Consciousness and the Power of Perspective	APSR	106	3	517-532	interpretivist
Stow, S.	2010	Agonistic Homegoing: Frederick Douglass, Joseph Lowery, and the Democratic Value of African American Public Mourning	APSR	104	4	681-697	theory
Widner, J.	1991	Interest Group Structure and Organization in Kenya's Informal Sector: Cultural Despair or a Politics of Multiple Allegiances?	CPS	24	1	31-55	descriptive
Selle, P.; Svåsand, L.	1991	Membership in Party Organizations and the Problem of Decline of Parties	CPS	23	4	459-477	descriptive
Kimmerling, B.	2002	Jurisdiction in an immigrant settler society - The "Jewish and democratic state"	CPS	35	10	1119-1144	descriptive
Abramson, P. et al.	2010	Comparing Strategic Voting Under FPTP and PR	CPS	43	1	61-90	quantitative
Mershon, C.; Shvetsova, O.	2008	Parliamentary Cycles and Party Switching in Legislatures	CPS	41	1	99-127	quantitative
Ignazi, P.; Ysmal, C.	1992	New and old extreme right parties: The French Front National and the Italian Movimento Sociale	EJPR	22	1	101-121	descriptive
Shugart, M.	1992	Electoral reform in systems of proportional representation	EJPR	21	3	207-224	qualitative
Togebay, L.	1990	Political radicalism in the working class and in the middle class	EJPR	18	4	423-436	descriptive
Raab, C.	1992	Taking networks seriously: Education policy in Britain	EJPR	21	01-feb	69-90	descriptive

Mendrinou, M.	1994	European Community: fraud and the politics of institutional development	EJPR	26	1	81-101	descriptive
Andersen, S.; Eliassen, K.	1991	European Community lobbying	EJPR	20	2	173-187	descriptive
Rhodes, R. A. W.; Marsh, D.	1992	New directions in the study of policy networks	EJPR	21	01-feb	181-205	no research article
Scarrow, S.	1994	The 'paradox of enrollment': Assessing the costs and benefits of party memberships	EJPR	25	1	41-60	theory
Rose, R.	1990	Institutionalizing professional political science in Europe: A dynamic model	EJPR	18	6	581-603	theory
Webb, P.	1992	Election campaigning, organisational transformation and the professionalisation of the British Labour Party	EJPR	21	3	267-288	descriptive
Leduc, L.	2002	Opinion change and voting behaviour in referendums	EJPR	41	6	711-732	no research article
Radaelli, C. M.	2004	The diffusion of regulatory impact analysis – Best practice or lesson-drawing?	EJPR	43	5	723-747	descriptive
Jones, P.	2003	Public choice in political markets: The absence of quid pro quo	EJPR	42	1	77-93	theory
Siaroff, A.	2003	Comparative presidencies: The inadequacy of the presidential, semi-presidential and parliamentary distinction	EJPR	42	3	287-312	theory
Cizre, U.	2004	Problems of democratic governance of civil-military relations in Turkey and the European Union enlargement zone	EJPR	43	1	107-125	descriptive
Curini, L.	2010	Government survival the Italian way: The Core and the Advantages of Policy Immobilism during the First Republic	EJPR	50	1	110-143	quantitative
Bowler, S.; Farrell, D.	2011	Electoral institutions and campaigning in comparative perspective: Electioneering in European Parliament elections	EJPR	50	5	668-688	quantitative
Helbing, M.; Hoeglinger, D.; Wüest, B.	2010	How political parties frame European integration	EJPR	49	4	496-521	quantitative
Genschel, P.; Jachtenfuchs, M.	2011	How the European Union constrains the state: Multilevel governance of taxation	EJPR	50	4	293-314	descriptive
Neto, O.; Costa Lobo, M.	2009	Portugal's semi-presidentialism (re)considered: An assessment of the president's role in the policy process, 1976–2006	EJPR	48	2	234-255	descriptive

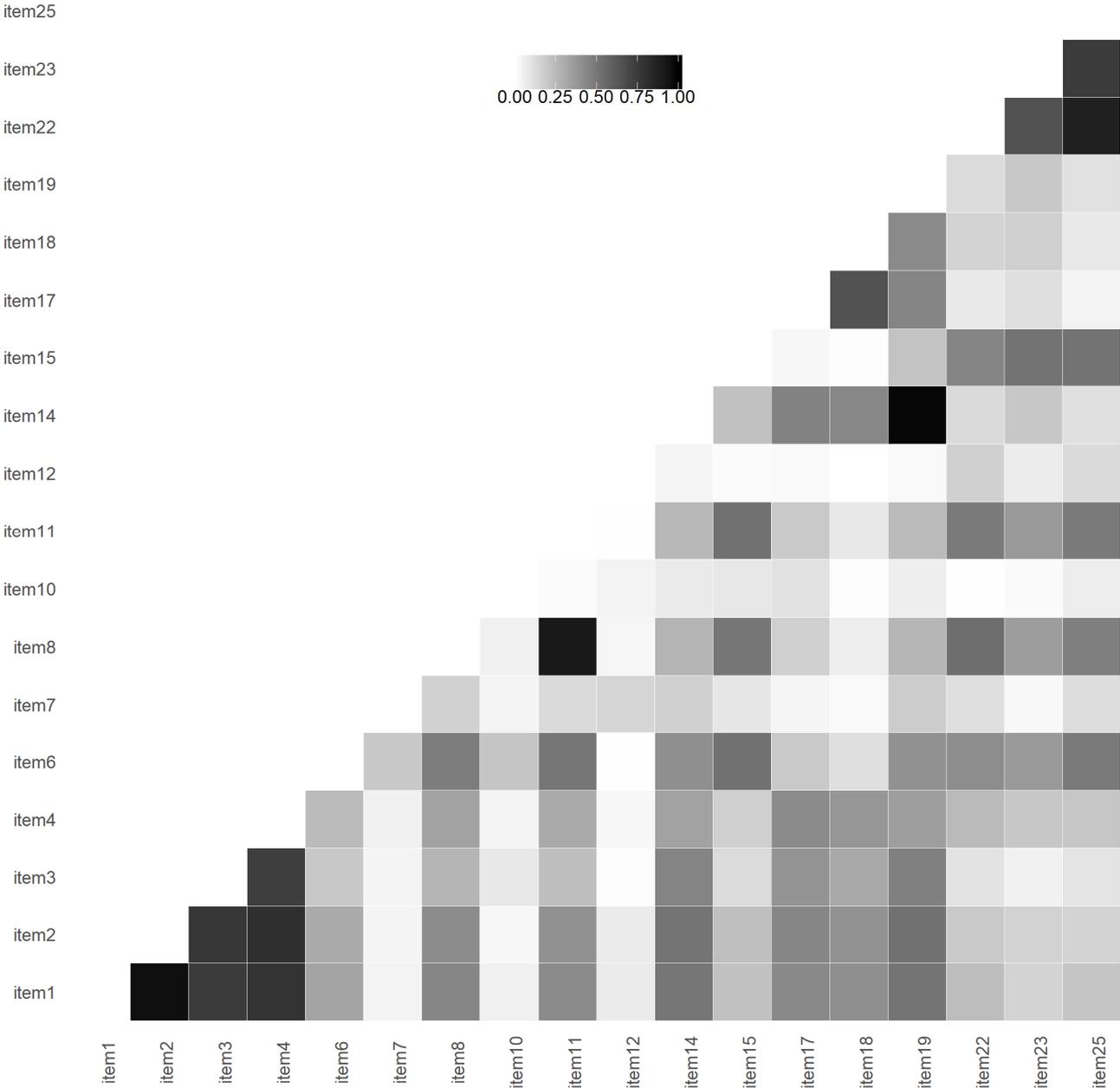
Kjaer, U.; Elklit, J.	2010	Party politicisation of local councils: Cultural or institutional explanations for trends in Denmark, 1966–2005	EJPR	49	3	337-358	quantitative
Vail, M.	2008	From ‘welfare without work’ to ‘buttressed liberalization’: The shifting dynamics of labor market adjustment in France and Germany	EJPR	47	3	334-358	descriptive
Smith, N.; Hay, C.	2008	Mapping the political discourse of globalisation and European integration in the United Kingdom and Ireland empirically	EJPR	47	3	359-382	quantitative
Corbetta, P.; Cavazza, N.; Roccato, M.	2009	Between ideology and social representations: Four theses plus(a new) one on the relevance and the meaning of the political left and right	EJPR	48	5	622-641	quantitative
Thompson, W. R.	1990	Long waves, technological innovation, and relative decline	IO	44	2	201-233	descriptive
Frieden, J. A.	1991	Invested interests: the politics of national economic policies in a world of global finance	IO	45	4	425-451	theory
Posusney, M. P.	1993	Irrational Workers: The Moral Economy of Labor Protest in Egypt	WP	46	1	83-120	qualitative
Stepan, A.; Skach, C.	1993	Constitutional Frameworks and Democratic Consolidation: Parliamentarianism versus Presidentialism	WP	46	1	ene-22	qualitative
Odom, W. E.	1992	Soviet Politics and After: Old and New Concepts	WP	45	1	66-98	theory
Nelson, J. M.	1993	The Politics of Economic Transformation: Is Third World Experience Relevant in Eastern Europe?	WP	45	3	433-463	theory
Kenworthy, L.	2001	Wage-Setting Measures: A Survey and Assessment	WP	54	1	57-98	descriptive
Murillo, M. V.	2000	From Populism to Neoliberalism: Labor Unions and Market Reforms in Latin America	WP	52	2	135-168	qualitative
Amorim Neto, O.; Cox, G. W.; McCubbins, M. D.	2003	Agenda Power in Brazil's Câmara Dos Deputados, 1989–98	WP	55	4	550-578	quantitative
Weyland, K.	2008	Toward a new theory of institutional change	WP	60	2	281-314	theory

**E. Additional analyses**

*E.1. Observable implication 3*

Figure A.1 shows the pairwise correlations for all non-missing items for all 180 articles. The analysis of the pooled data does not confirm the expectation of two coherent cultures because most correlations are at a low or moderate level as opposed to a high level.

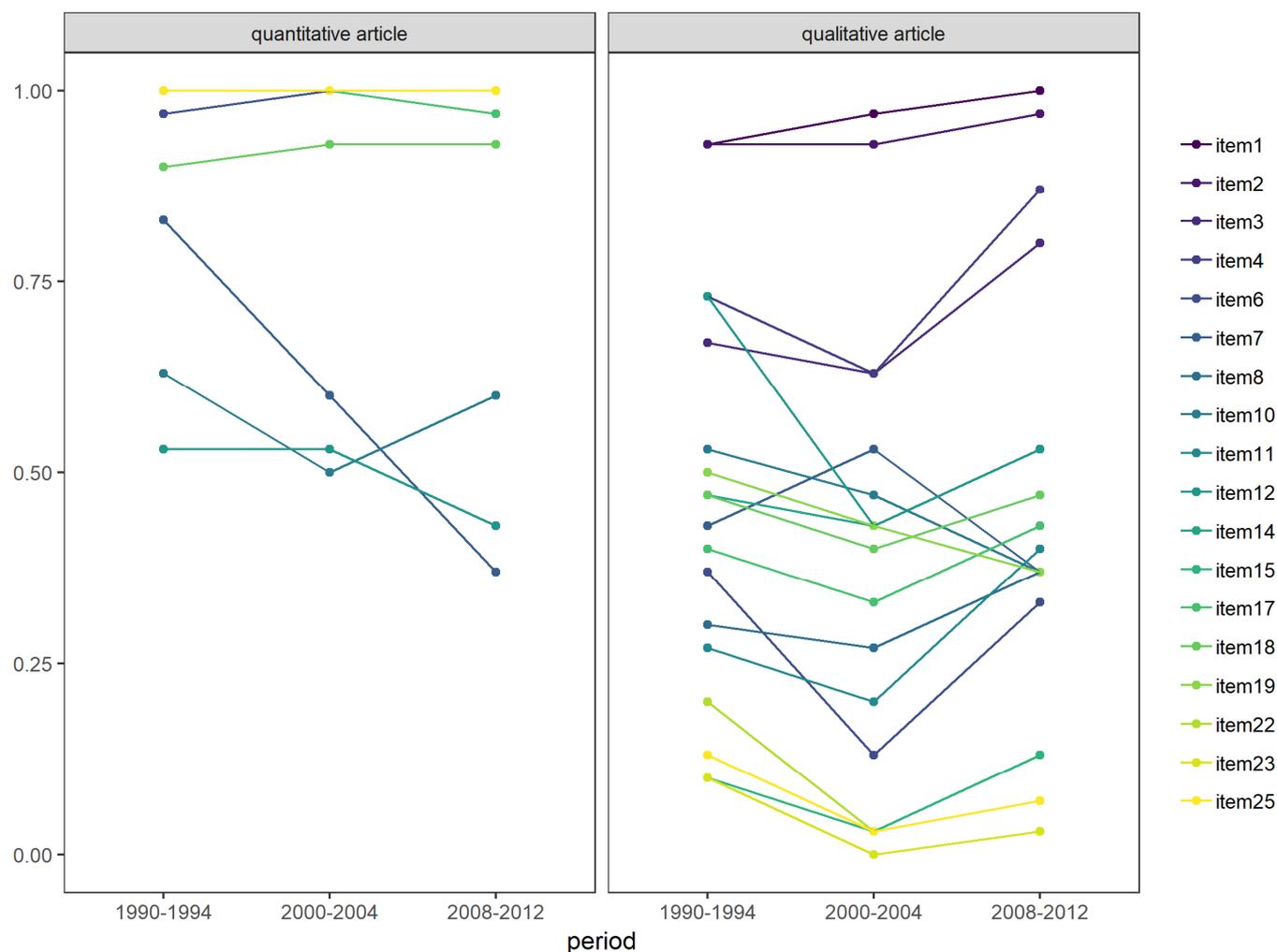
Figure A.1: Heatmap of pairwise correlations (Cramer's V) for pooled data



## E.2. Observable implication 5

Figure A.2 presents more clearly how the shares of culture-compliant practices developed over time. The left plot shows fewer than 18 lines because many shares are 1 in quantitative articles, such that their lines overlap.

Figure A.2: Development of share of culture-conforming practices over three subperiods



The analysis of the culture-compliant proportions of practices in the article focused on the items that cannot be coded “99”. In Table A.1, we present cross-tabulations for the seven items with 99 codes for a qualitative assessment of culture compliance. We present the absolute numbers here because the shares are small and become meaningless. Overall, the picture is mixed. For items 5, 9, 13 and 24, we see that the method practices largely comply with the expectation. For

items 16 and 20, we observe the expected share for quantitative articles and have no data for qualitative research. For item 21, both types of articles follow the quantitative culture.

Table A.1: Cross-tabulation of articles and practices for items with 99 code

		method practice	
		quantitative culture	qualitative culture
item 5	quantitative article	28	1
	qualitative article	1	5
item 9	quantitative article	27	2
	qualitative article	1	5
item 13	quantitative article	58	1
	qualitative article	1	26
item 16	quantitative article	25	0
	qualitative article	0	0
item 20	quantitative article	23	0
	qualitative article	0	0
item 21	quantitative article	10	0
	qualitative article	10	1
item 24	quantitative article	29	4
	qualitative article	0	2

### *E.3 Robustness tests toward the choice of journals and periods of analysis*

We address the possibility of post-2012 developments towards a more pronounced qualitative culture and that the six journals selected for analysis bias the results against finding a qualitative culture. We conducted a robustness test on 30 randomly chosen qualitative journal articles published in 2018. We used the *Web of Science* database to search for journal articles published in

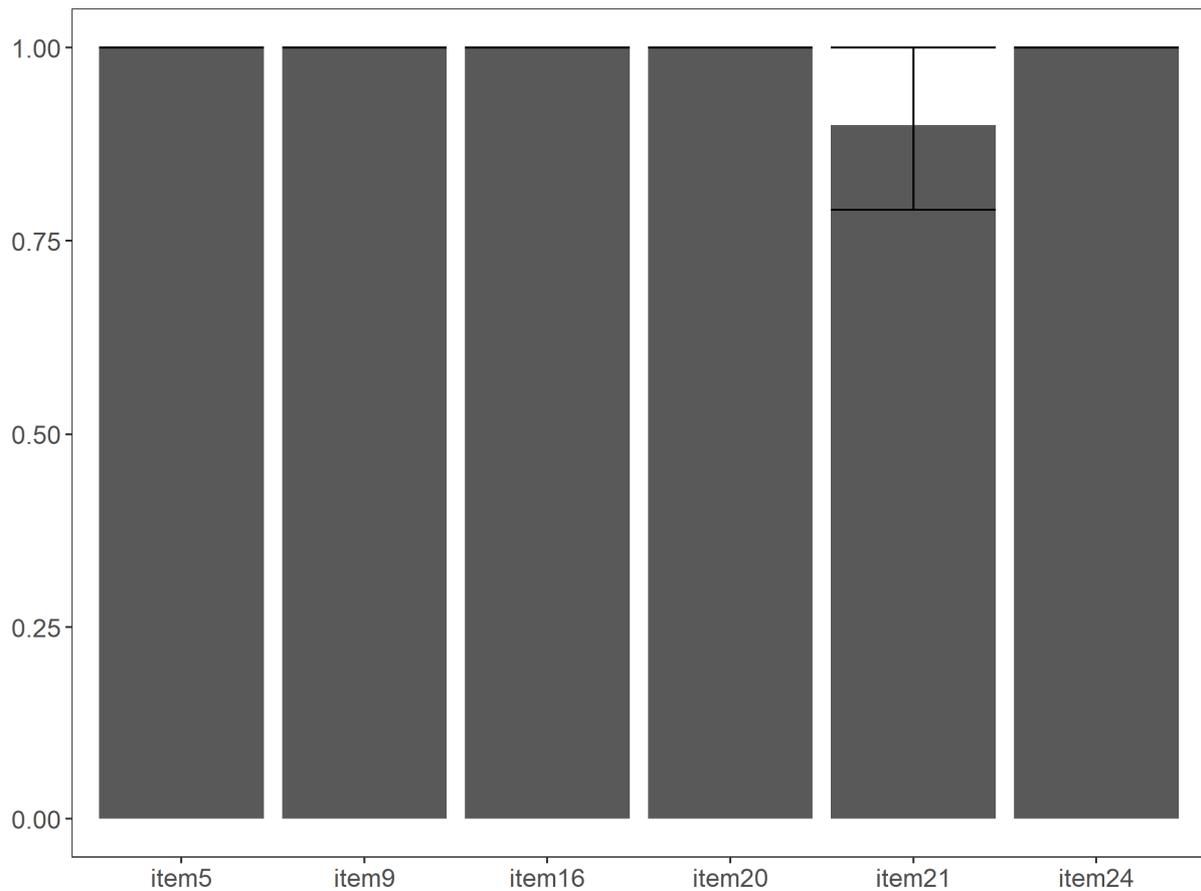
the field of Political Science in 2018 using the following keywords: “cross-case study” OR “cross-case analysis” OR “case study” OR “comparative case study” OR “process tracing” OR “within-case analysis” OR “paired comparison” OR “systematic process analysis”. From this list, we removed all obvious false-positives, i.e. articles that were sampled but did not meet the scope conditions of our analysis. In addition to the criteria outlined in Section 4.1 of the article and Appendix D, we had to exclude a number of articles for additional reasons, for instance because the article was published in one of the six journals from which we sampled the original 180 articles; because it was unavailable from our institutions; or had been published in a language different from English. The reproduction script presents a tally of the reasons of exclusion. In total, we pre-screened 129 articles to achieve our sample of 30 articles, which we then coded according to the procedures described in Section 4.2 of the article.<sup>40</sup> We empirically analyzed the data based on the two main observable implications (OI), namely 1 and 4. OIs 2, and 5 are irrelevant for the robustness sample of 30 qualitative articles published within a single year, as they require comparisons of qualitative versus quantitative articles (OI 2), or within qualitative articles across time (OI 5). The third implication is not assessed with the 30 articles because the correlations are not directly informative about the degree to which a method practices conforms to the qualitative culture (see manuscript). Overall, we find no meaningful differences between the 90 qualitative articles sampled from the six journals of our original dataset and the robustness sample of 30 qualitative articles.

In terms of OI 1, which asks whether all 25 items are empirically relevant, we find that the same method practices that we infer to be empirically irrelevant in Section 5 of the article for qualitative empirical research are also irrelevant in the 2018 sample of 30 qualitative articles. This means mainly that also in 2018 qualitative research typically does not engage in counterfactual reasoning (items 5, 9, 24); does not problematize triangular data (item 16) or the transformation of data (item 20); and does not include typologies (item 21) (see Figure A.3).

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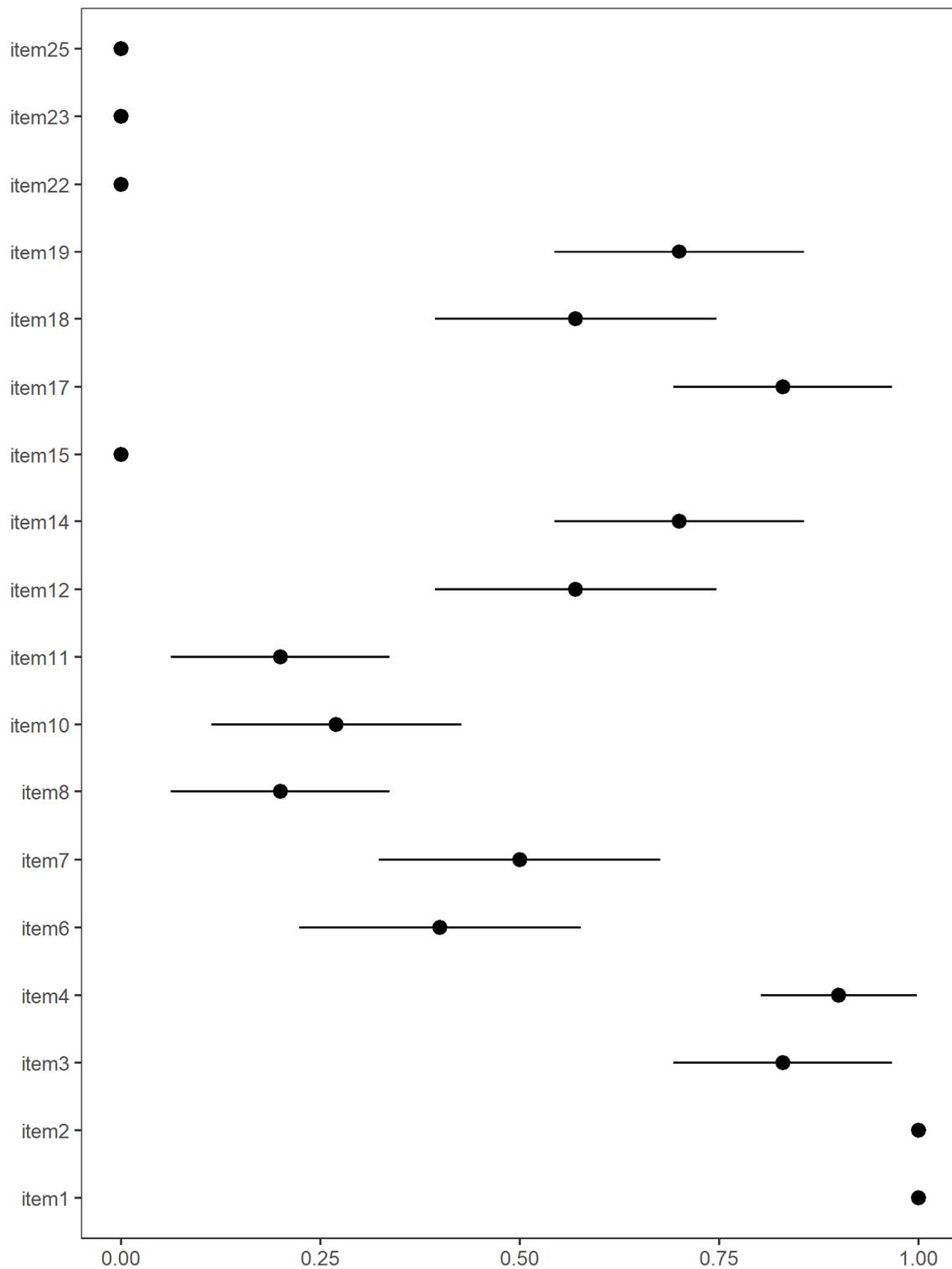
<sup>40</sup> The individual codings by each author yield a similar level of intercoder reliability as the coding of the original dataset, with a Krippendorff’s alpha of 0.75 and a Cohen’s kappa of 0.76.

Figure A.3: Items with proportion of missings and 95% confidence interval (if applicable)



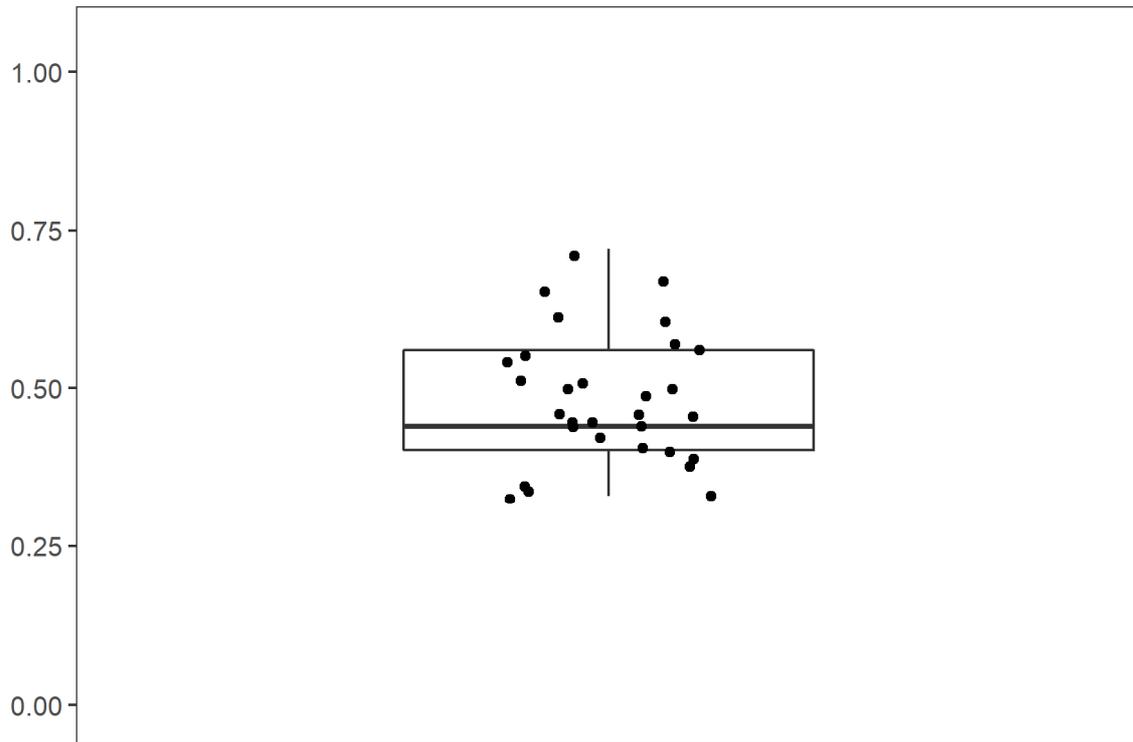
The second analysis of the 30 articles asks for the share of method practices that are implemented in line with the qualitative culture (OI 4 taking items as the unit of analysis). The results for the 30 articles mirror the findings for full sample summarized in Section 9 of the article. Figure A.4 shows the shares of culture-conforming codes across the 18 items without missings. It provides a very similar image as Figure 4 in the article: the 30 articles conform to the prescriptions of the qualitative culture mainly in the focus on individual cases (1-4), which relates to the way in which individual cases are addressed. For items relating to concepts and measurement (14, 17-19), the robustness sample tends to be a bit closer to the qualitative culture as described in ATTC than the articles in the original sample. However, the sample size of 30 introduces a higher level of uncertainty and the confidence intervals span a range of about 20 points. For items relating to causality and causal models (6-10), the robustness sample conforms less to the qualitative culture than the original data. But overall, the differences are substantially negligible, not least due to large confidence intervals.

Figure A.4: Proportions of culture-conforming codes and 95% confidence intervals



Finally, Figure A.5 shows the proportion of culture-conforming item codes per article, with a median of around 0.4 and the third quartile slightly above 0.6. This is very similar to the findings summarized in Figure 5 of the article (Section 8).

Figure A.5: Proportion of culture-conforming practices per article



#### *E.4 Exploratory analysis of alternative clusters in the data*

Based on the negative results of our analysis of the single dimension that represents qualitative and quantitative methods cultures, this section addresses the follow-up question whether methods practices in political science cluster in other interesting and systematic ways. We consider this question through two exploratory approaches, multiple correspondence analysis (MCA) and cluster analysis. The results are summarized in the article's conclusion.

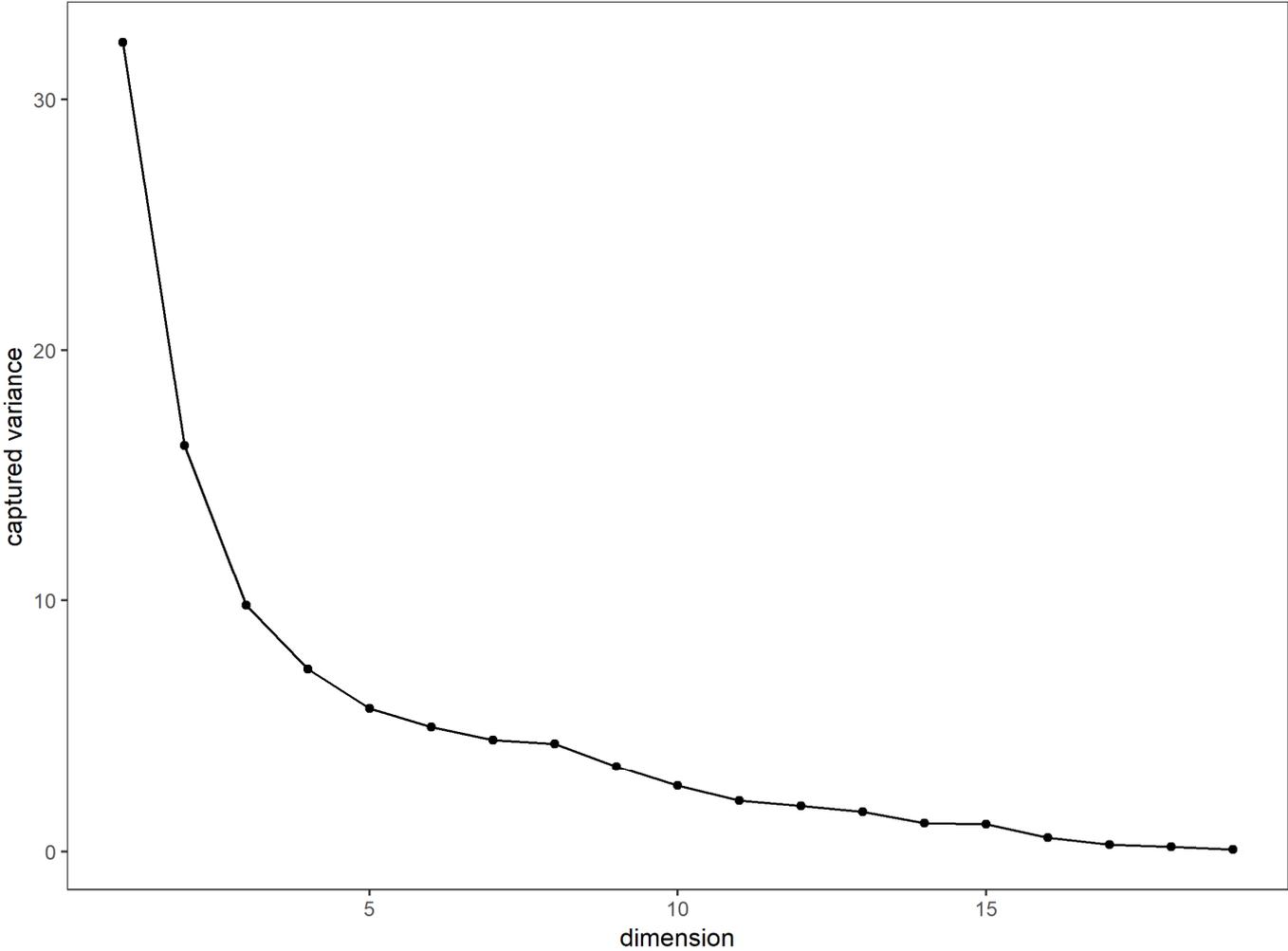
##### *E.4.1. Multiple Correspondence Analysis*

The analytical goal of multiple correspondence analysis (MCA) is to identify structures (dimensions) in datasets of multiple categorical variables (Greenacre 2007). As such, it can be used as another means to test the two-cultures hypothesis, but also to identify inductively alternative patterns in the data that could indicate similarities and differences among and between methods practices in applied political science.

We first address the additional test of the two-cultures hypothesis. The scree plot in Figure A.6 presents the degree of variance captured by each of the 19 dimensions derived from the MCA.

If the two-cultures hypothesis was true, we would expect that the first two dimensions (the quantitative-qualitative divide) explained a large share of all variance in the data. The plot shows, however, that this is not the case, with the first dimension capturing only 33 percent of the variance on the 18 items, reiterating the finding that methods practices in political science cannot be reduced to a single quantitative-qualitative distinction. The scree plot also shows that there are no particularly convincing alternative groupings in the data, with the third and all following dimensions capturing variances that are close to or below the average we would expect from random data, i.e., six percent.

Figure A.6: Scree plot for multiple correspondence analysis of 18 items



This is also shown by the MCA plot reproduced in 4, which presents the first two dimensions extracted from the MCA with a captured variance of greater than 10%.<sup>41</sup> If the two-cultures

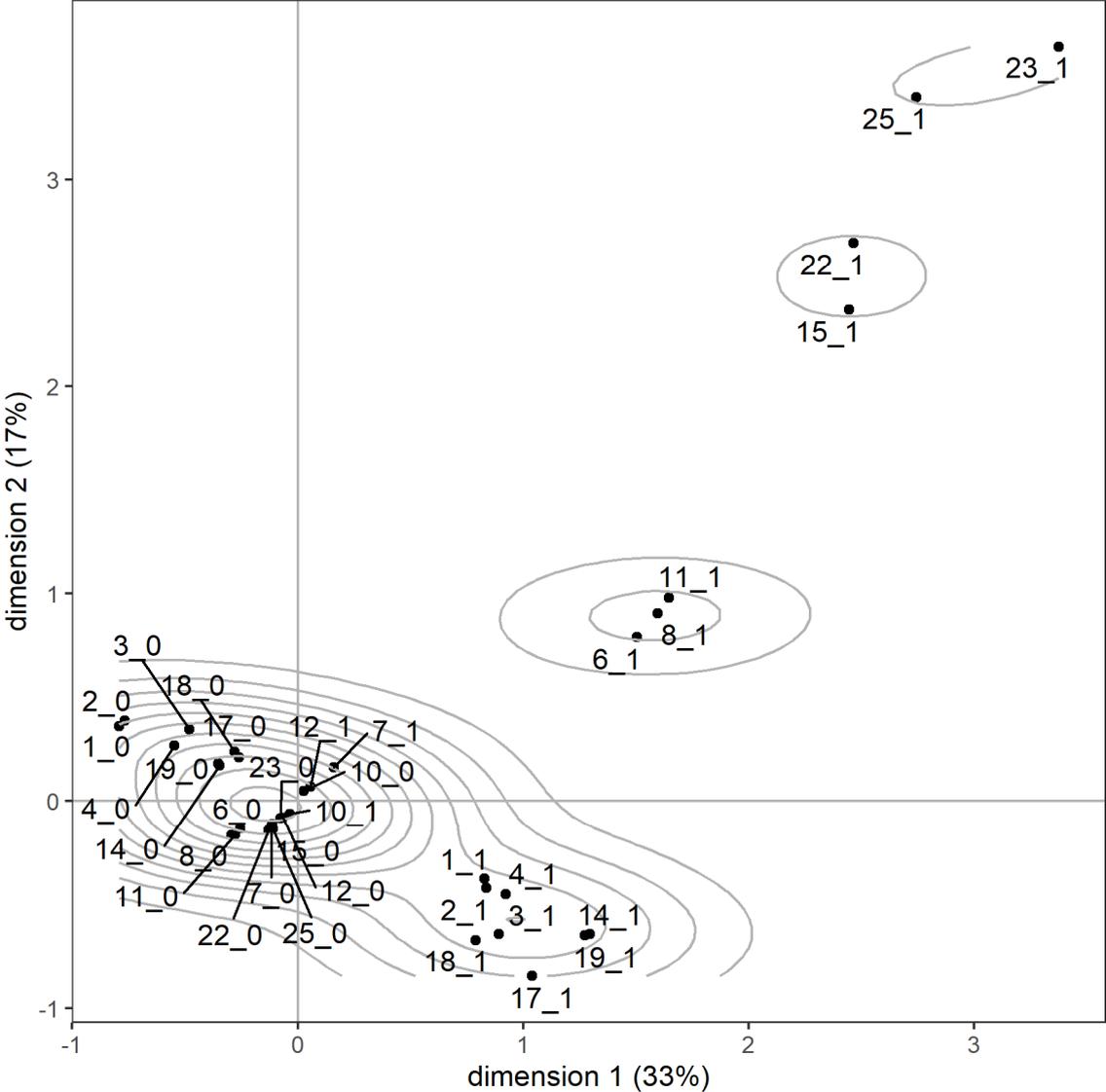
<sup>41</sup> Here, a „variable“ is an item in combination with the code we assigned. For example, 23\_1 represents the „1“-codes for item 23.

hypothesis was true, the MCA plot should show two centers of gravity in which the parameter values of the individual items are clustered together closely along their respective coding as either quantitative or qualitative. This is not the case. As was expected from our previous analyses, quantitative item codes cluster together closely in a single center of gravity around the centroid, reiterating that quantitative methods applications follow, in general, the patterns described in ATTC. The remaining four clusters include 15 out of the 18 items that have been coded as following the qualitative culture (“1”). While this provides some evidence that qualitative research is, as ATTC suggests, different from quantitative research, the fact that the majority of qualitative items are clustered in close proximity to the “quantitative cluster” around the centroid shows that these items do not differentiate strongly between qualitative and quantitative articles. They stand slightly apart from the bulk of articles coded as quantitative and make a limited contribution to the captured variance of both dimensions. Their location relative to the zero point and the seven items in the upper-right plot suggests that they are not related to each other, which is in discord with the two-cultures hypothesis.

Turning to the second aspect of the MCA, the data also does not suggest any substantively interesting clusters of methods practices. Figure A.7 shows that seven items in the upper-right corner of the plot are associated because they are closely located to each other and distant from the zero point. Substantively, Figure A.7 shows a clustering of items that measure whether an article includes conceptual or causal asymmetry. Items 6, 8, 11, 15, 22, 23 and 25 capture asymmetry-related issues such as making set-theoretic claims (item 8) and arranging data in a truth table (item 15). On first sight, this suggests that asymmetry indeed is indicative of the qualitative culture. However, as discussed in detail in Section 8 of the main article, while almost all articles that were coded “1” on these variables were indeed qualitative, the actual number of

articles that were thus coded is very small. This suggests that we should not overinterpret the results of the MCA (see also the results of the cluster analysis, below).

Figure A.7: MCA plot of 18 items



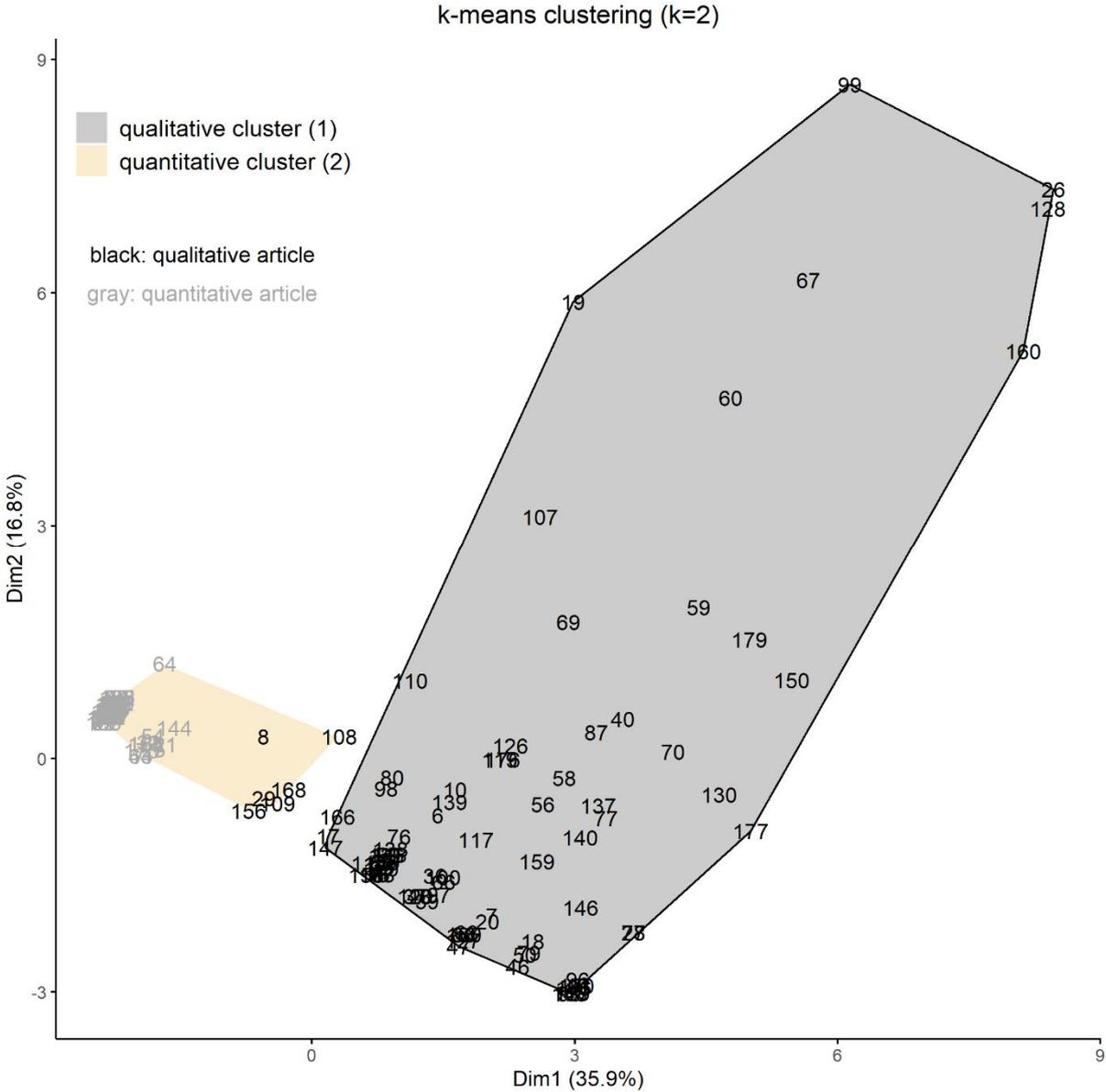
E.4.2. Cluster Analysis

The cluster analysis has an individual article as the unit of analysis. The focus then is on the share of methods practices within an article that follow the quantitative or qualitative culture. The cluster analysis can be used to test the two-cultures hypothesis once again against our dataset of 180 articles. Since the result should and does confirm the findings in the manuscript, the primary purpose of the cluster analysis is exploratory. It serves to detect groupings of methods practices *within* the 90 qualitative articles, which could be termed “methods sub-cultures”.

We report the results for the confirmatory and exploratory analysis in the following based on cluster analyses of the 18 items without missings.

If the two-cultures hypothesis was true, the cluster analysis should yield two relatively homogenous and equally large clusters on the highest hierarchical level, each containing exclusively qualitative and quantitative articles. On the penultimate hierarchical level, the two groups contain a similar number of articles; cluster 1 contains 96 articles and cluster 2 includes 84 articles. The clusters appear to be relatively homogenous with cluster 2 containing 84 qualitative articles, which amounts to 100% of all articles in this cluster and 93.3% of all qualitative articles in our data. Among the 96 articles in cluster 1, six qualitative articles (6.3%) are clustered with all the 90 quantitative articles in our analysis. However, the plot for the 2-dimensional k-means cluster represented in Figure A.8 shows that the two clusters are very dissimilar in size.

Figure A.8: k-means cluster plot for two clusters



The predominately quantitative cluster is dense and shows very little variance between the individual articles. This reflects that the articles in cluster 1 are similar in terms of the values on the 18 items. Again, this can be interpreted as evidence for a relatively coherent quantitative methods practices that largely comply with the quantitative culture (see manuscript). The mainly qualitative cluster is very large, demonstrating that the method applications in these articles are diverse. This supports the finding that qualitative research is much more diverse than quantitative research and we expect to be if the two cultures hypothesis was true. The location of the two clusters shows that many qualitative articles in the qualitative cluster are distant from its core, which is at the top of the cluster in close distance to the quantitative cluster.

Our exploratory cluster analysis of all 90 qualitative articles<sup>42</sup> draws on Koivu and Kimball Damman's (2015) work that makes a conceptual distinction between approaches *within* qualitative research: "quantitative emulation (QE)", "eclectic pragmatism (EP)", and "set-theoretic approaches (ST)".<sup>43</sup> Koivu and Kimball Damman suggest that these three approaches are based on different foundations that lead to different method and design decisions in causally oriented qualitative research: QE is variable-oriented; based on a probabilistic conceptualization of causality; has concerns about no-variance designs and confounders that need to be controlled for; and aims at generalizable results. EP is case-oriented; based on mechanistic conceptions of causality; uses process tracing as a method of causal inference; and utilizes „thick“ concepts to operationalize research variables. ST is also case-centered, but draws on set-theoretic conceptions of causality; is mainly interested in the effects-of-causes; as such, ST corresponds best with what is described as the qualitative culture in ATTC (Koivu and Kimball Damman 2015, 2622).

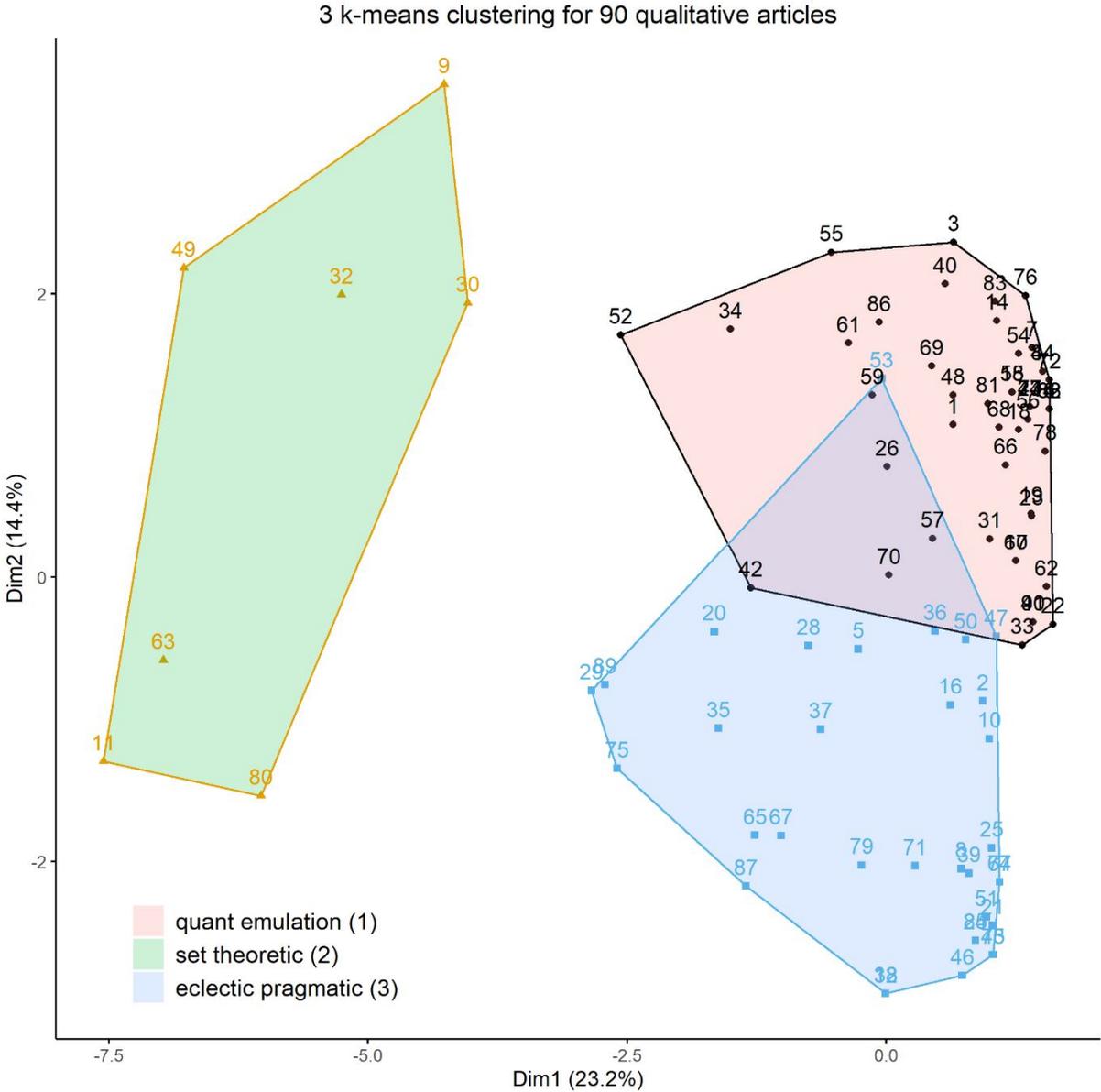
We assess whether this three-fold differentiation is useful for sorting the qualitative articles. We run a k-means cluster analysis on the 90 articles belonging to the "qualitative" cluster and set to k=3 (Figure A.9). In the discussion of Figure A.9 and A.10 below, we explain why we designate the three clusters as summarized in the legend.

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<sup>42</sup> We do not discuss quantitative subclusters because there is large homogeneity among the quantitative articles.

<sup>43</sup> Koivu and Kimball Damman (2015) define a fourth, "empirical interpretivist" qualitative approach which is based on different ontological and assumptions than the other three. Since we excluded all interpretivist articles from our dataset, we limit our analyses to the three approaches discussed by Koivu and Kimball Damman.

Figure A.9: k-means cluster plot of qualitative articles (3 clusters)

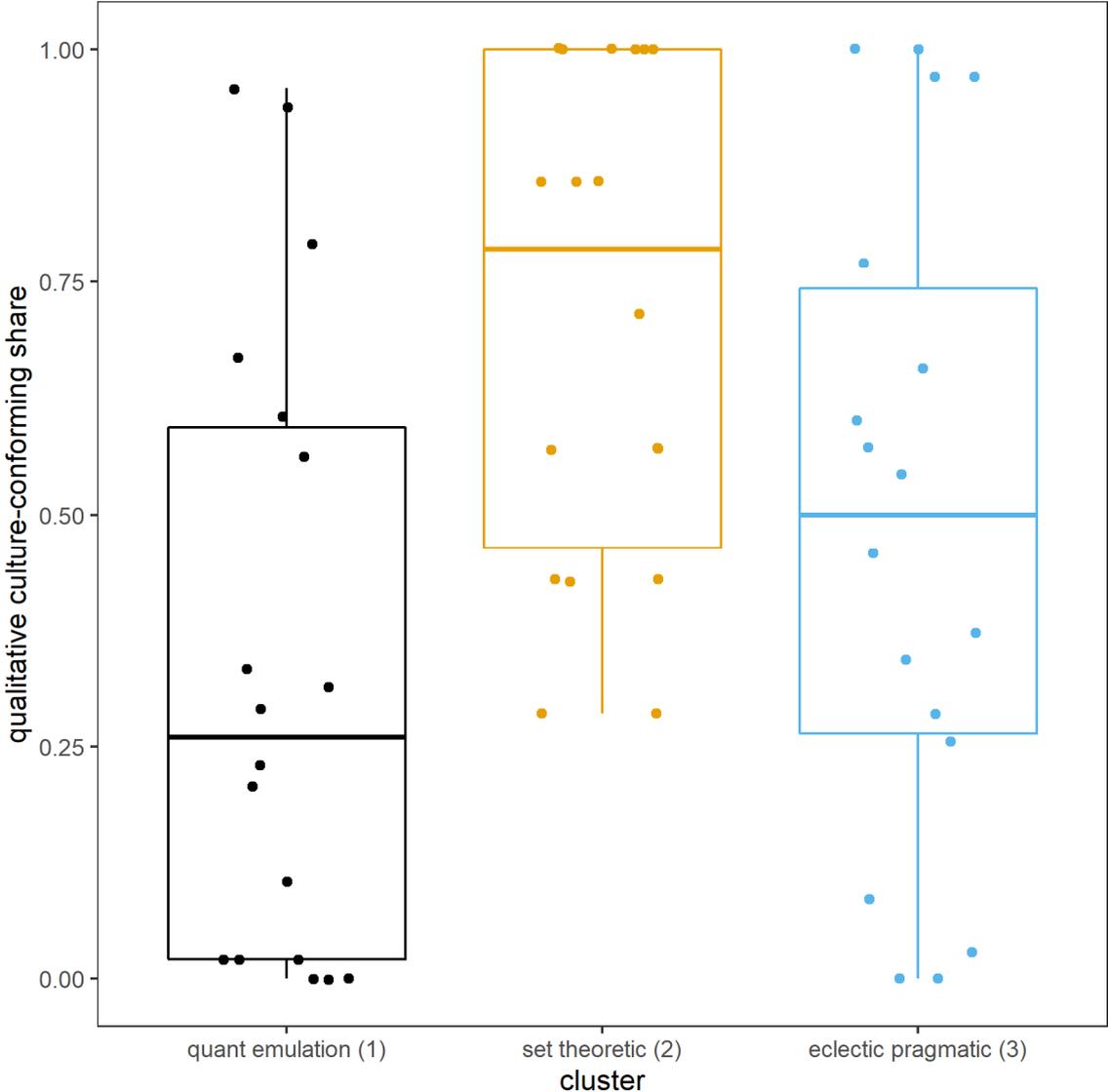


The figure shows that the vast majority of qualitative articles in our sample are grouped into cluster 1 (48 articles, 53.3 per cent) and cluster 3 (35 articles, 38.9 per cent). Cluster 1 contains seven articles (7.8 per cent). We calculate the per-item means within each cluster and compare them across the three clusters to check whether the cluster characteristics comply with how they are defined by Koivu and Kimball Damman. If their conceptualization is valid, we should observe variance across those items that capture these differences.

Figure A.10 illustrates how frequently each of the 18 items is practiced in accordance with the qualitative culture relative to all 18 items. A value of 1 means that all practices comply with the *qualitative* culture in all articles in the cluster at hand. A value of 0 means that all

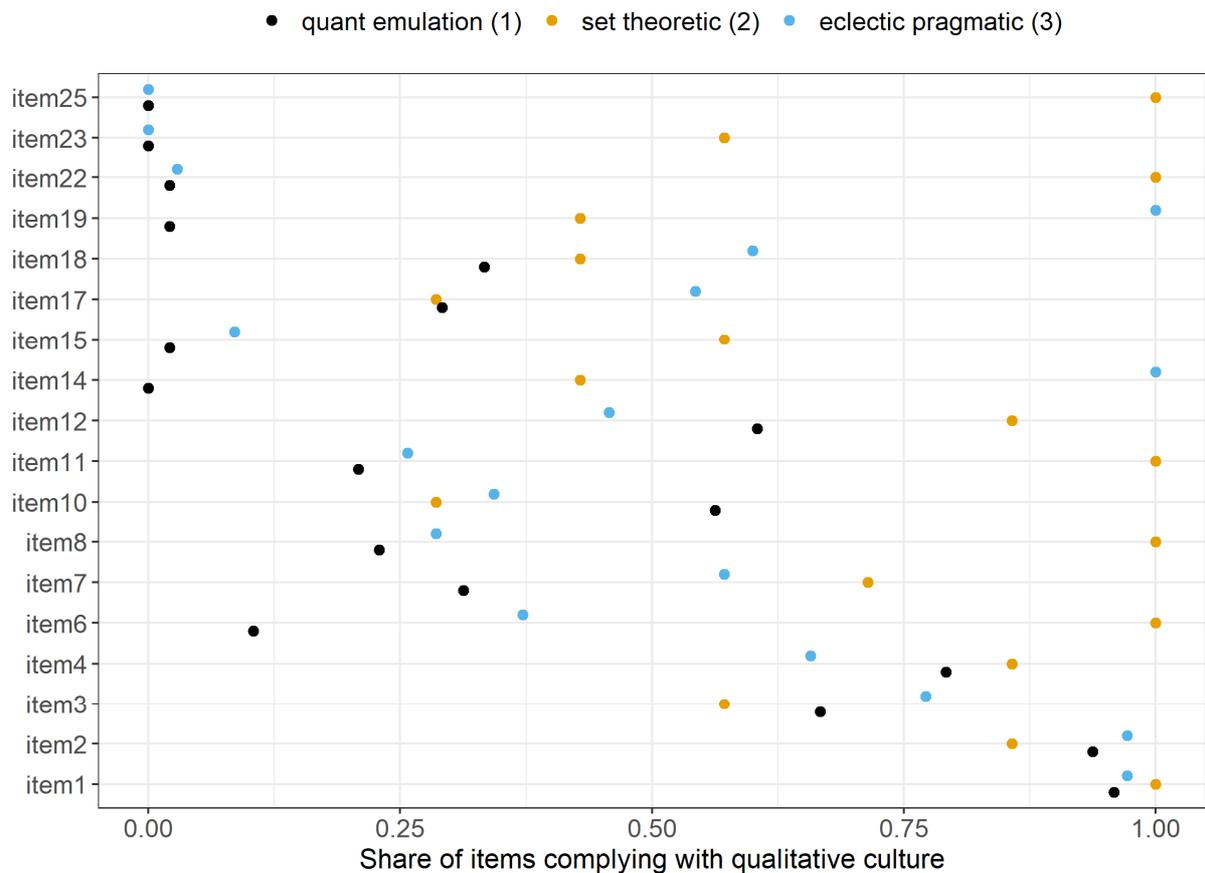
practices comply the *quantitative* culture in all articles in this cluster. Cluster 1, which we designate as “quantitative emulation” cluster, shows a wide dispersion of culture-complying practices, but has a median share of only 0.25 and a third quartile of less than 0.6. We label the second cluster as the set-theoretic cluster with a median share of culture-conforming practices of 0.79 and most items being located above 0.5. The third cluster is labeled as the eclectic pragmatic cluster because it shows the largest distribution of shares, reflecting that the articles in this cluster neither clearly follow the qualitative or quantitative culture, on average.

Figure A.10: Shares of qualitative culture-conforming practices per cluster



In Figure A.11, we compare the item-specific means for each item to see where two or all three clusters differ from each other and where they don't. We do include confidence intervals because the number of cases is too small for their meaningful interpretation.

Figure A.11: Shares of qualitative culture-conforming practices per item



The clusters differ substantively in the average values in the individual items. The set-theoretic cluster (cluster 2) corresponds well to Koivu and Kimball Damman’s discussion; the share is 1 for the relevant set-theoretic items 6 (no interest in identifying net causal effect of individual variables), 8 (set-theoretic understanding of causality in terms of necessity/sufficiency), 11 (set-theoretical logic of aggregation of causally relevant variables), 22, 23 and 25 (asymmetric, i.e., set-theoretic, explanation, concepts and data).

The item-means of cluster 1 correspond well with the model of quantitative emulation, not only in the aggregate (Figure A.10), but also in relation to the main indicators of the QE group that mark the quantitative logic of modeling and identifying causality (Figure A.11). In terms of causal models, the articles in cluster 1 are, on average, mainly interested in the net effect of individual variables (items 6 and 8), aggregate causal effects along the additive-linear model (item 11), and treat causal relationships as well as concepts as symmetric (items 22, 23 and 25). The average article in the QE cluster follows Koivu and Kimball Damman’s characterization by prizing variation on the dependent and independent variables (items 14 and 19), and treating cases as collections of variables (item 15). What we do *not* find in the QE cluster

is a strong interest in the broad generalization of findings because the share of articles with broad and narrow claims is almost even (item 12).

The methods practices in cluster 3 are best summarized as pragmatic and eclectic (PE). The item-means typically lie in between those of the articles in Clusters 1 and 3 and the QE and EP cluster partially overlap in Figure A.9. Articles in this cluster combine a preference for the quantitative logic in terms of the treatment of data (items 15 and 23) and the symmetry of causal relations (items 22 and 25) on the one hand, with a preference for the qualitative practice of not analyzing the full zones of variance of the dependent and independent variables (items 14 and 19).<sup>44</sup> As suggested by Koivu and Kimball Damman, articles in cluster 3 also seem to prize concept formation, with items 17 and 18 receiving the highest mean value across the three clusters (0.56 and 0.61, respectively), even if cross-cluster variation in these items is not particularly large. The supposed emphasis of articles in the eclectic pragmatic tradition on “the ability of within-case analysis to uncover mechanistic causation, or causal processes” (Koivu and Kimball Damman 2015, 2622) is not borne by the data. Cluster 3 has an, albeit slightly, lower mean in the empirical analysis of causal mechanisms (item 3) than the QE-Cluster 3, and the lowest of all cluster-wise mean scores in the use of process tracing (item 4).

In a comparison of the clusters, we observe for some items that the differences between the clusters are small. This is particularly the case for items 1 to 4 (dealing with the role of individual cases), but also to items 7 and 10 (configurative and equifinal causality), and items 17 and 18 (concepts and measurement).

In sum, Koivu and Kimball Damman’s three types of qualitative research provide a useful foil to map the variation in the application of qualitative methods in the 90 articles that we sampled. Especially the ST and QE types correspond well to the patterns in the three clusters, as they capture systematic variation in a number of items. In contrast, the PE type is mainly useful as a residual type complementing the other two, more clear-cut approaches. Taken together, the cluster analysis suggests *four substantive conclusions*. First, it highlights, again, that qualitative research is much more diverse than the ATTC hypothesis suggests. Second, the existence of a sub-approach that “emulates” quantitative research helps to explain the often rather small aggregate differences between the two methods cultures reported in the main analysis. Third, the three clusters do not vary much on some practices; it is mainly in the treatment of set-theoretic causality and variance on the research variables that meaningful differences between the clusters appear. Fourth, the type of qualitative research that does conform with the

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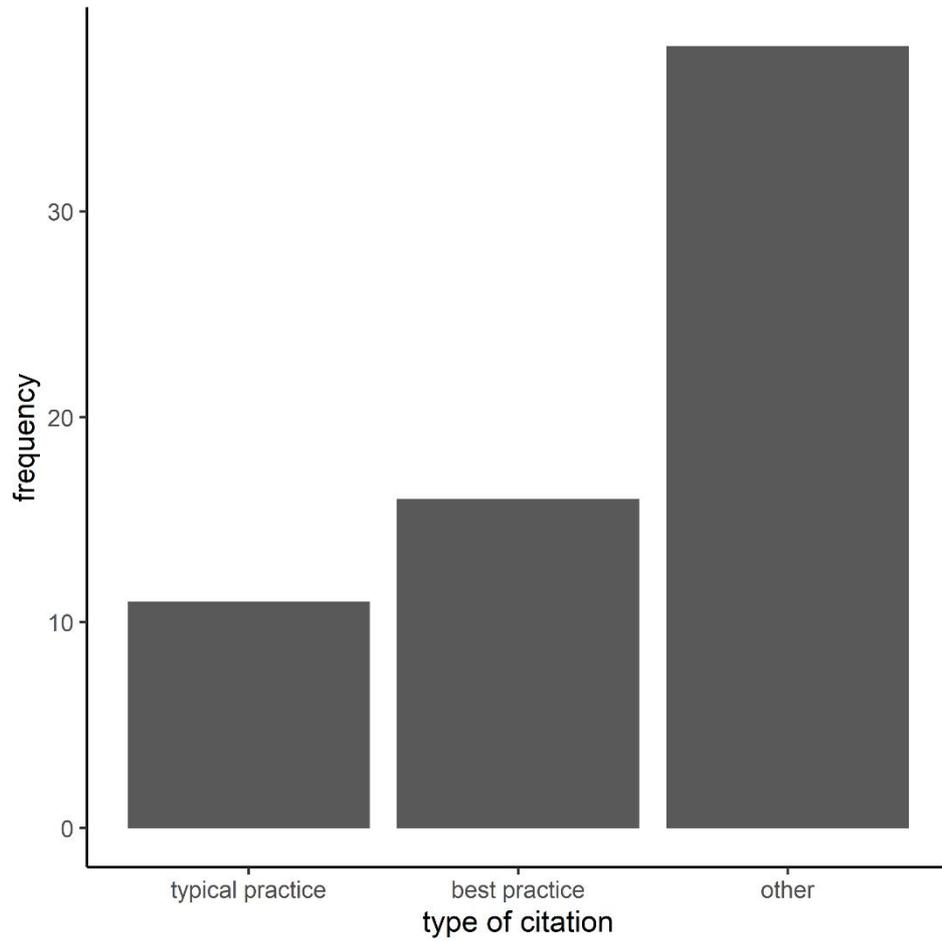
<sup>44</sup> We cannot test the presumed preference of PE for the combination of quantitative and qualitative research methods with our data, since we excluded MMR research from our data collection and evaluation (see Appendix C).

qualitative culture as defined in ATTC is hardly dominant with only seven out of 90 qualitative articles. What is defined as the qualitative culture describes only a tiny subset of qualitative research applications, which are otherwise much better described as either emulating quantitative or pragmatically mixing qualitative and quantitative practices.

## F. Analysis and references of publications citing ATTC

The sampling and coding procedure are described in the repository for this project. (<https://osf.io/6uhpd/>)

Figure A.12: Types of citations of "A Tale of Two Cultures" (Google Scholar, 2016)



List of journal articles that cite ATTC and are listed in Google Scholar as published in 2016.

- Ahonen, P. 2016. "Digital Public-Administration Research Drawing from Bayesian Inspiration: Latent Trait Scaling and Topic Modeling Examination of Budgetary Legislation in Thirteen Countries." *Administrative Culture* 17 (1): 47-70.
- Akyol, B. 2016. "The Qualitative Analysis of Turkish Adult Education System in Terms of Management Processes." *International Education Studies* 9 (11): 181-195.
- Altundal, U. 2016. "Regime Change within Defective Democracies: Turkey in the Early 1990s and 2010s." *Democracy and Society* 13 (1): 10-14.
- Anria, S., and S. Niedzwiecki. 2016. "Social Movements and Social Policy: The Bolivian Renta Dignidad." *Studies in Comparative International Development* 51 (3): 308-327.
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## **G. Analysis of coder agreement**

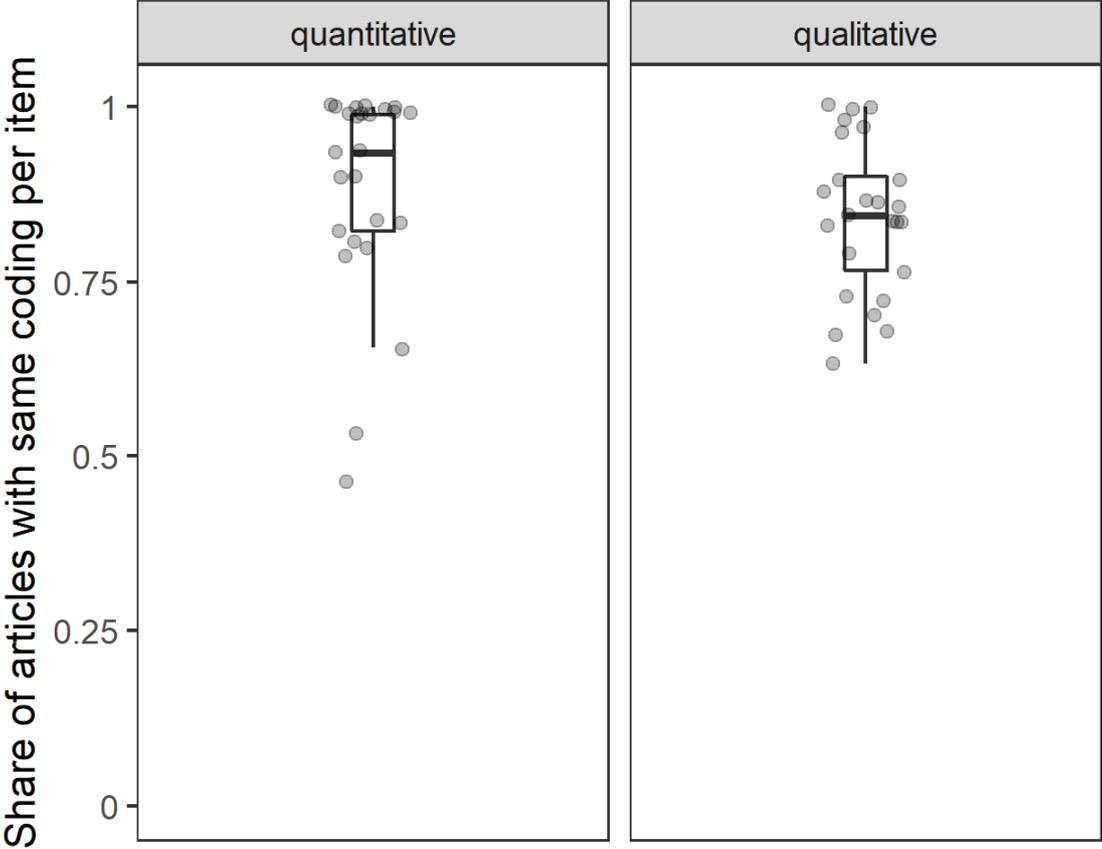
For the purpose of this analysis, the main question is whether a given item for a given article was coded the same by the coders or differently. The precise codings (i.e., whether the item was coded in accordance or contradicting the expectations concerning the methods culture) do not matter because we are only interested in agreement and disagreement. Codings are designated as agreeing (or being the same) when we both assigned the same value to the item (e.g., 0 and 0, or, depending on the item, 99 and 99). Two codings are considered as different (or disagreeing) when they are not the same (e.g., 0 and 1, or 1 and 0). We identified the nature of the codings for each individual method practice to determine whether there are particular items and articles where we particularly agree or disagree.

Figure A.13, reproduced below, presents the distribution and quartiles for the share of coding agreements per item aggregated over all quantitative and qualitative articles (left and right panel, respectively).<sup>45</sup> In this figure, a coding agreement of 0.9, for example, means that we assigned the same coding to this item in 81 out of the total of 90 (qualitative or quantitative) articles. The figure shows that, overall, the share of identical codings is higher for methods practices in quantitative articles, but that quantitative articles also saw the greatest degree of disagreement concerning individual items (the three outliers in Figure A.13). Overall, however, we infer from this that there are no substantial differences related to the reliability of our codings of method practices between quantitative and qualitative articles. For both types of articles, the third quartile is above 0.75, meaning that at least 18 items show coding agreements in about 70 articles out of 90.

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<sup>45</sup> This means the unit of analysis is the individual item.

Figure A.13: Coding agreement per item aggregated over 90 quantitative (left panel) and 90 qualitative articles (right panel)



In table A.2 (below), we reproduce the share of coding agreement for each item, differentiated by the classification of an article as quantitative (top half) or qualitative (bottom half). The table shows that there is no apparent clustering of items that belong to the same dimension (see above) at the lower range of shares for quantitative and qualitative articles.

Table A.2: Coding agreement per item aggregated over 90 quantitative (top half) and 90 qualitative articles (bottom half)

Method	Item	Share
quantitative	item3	1

quantita- tive	item15	1
quantita- tive	item19	1
quantita- tive	item22	1
quantita- tive	item23	1
quantita- tive	item25	1
quantita- tive	item1	0.99
quantita- tive	item4	0.99
quantita- tive	item8	0.99
quantita- tive	item11	0.99
quantita- tive	item14	0.99
quantita- tive	item17	0.99

quantitative	item6	0.93
quantitative	item21	0.93
quantitative	item2	0.9
quantitative	item18	0.9
quantitative	item16	0.83
quantitative	item20	0.83
quantitative	item7	0.82
quantitative	item9	0.81
quantitative	item24	0.8
quantitative	item5	0.79
quantitative	item10	0.66

quantitative	item12	0.53
quantitative	item13	0.47
qualitative	item1	1
qualitative	item16	1
qualitative	item20	1
qualitative	item24	0.98
qualitative	item5	0.97
qualitative	item9	0.97
qualitative	item21	0.9
qualitative	item23	0.9
qualitative	item25	0.88
qualitative	item4	0.87
qualitative	item15	0.87
qualitative	item22	0.86
qualitative	item3	0.84
qualitative	item2	0.83
qualitative	item14	0.83
qualitative	item17	0.83
qualitative	item18	0.83
qualitative	item10	0.79
qualitative	item19	0.77

qualitative	item7	0.73
qualitative	item11	0.72
qualitative	item8	0.7
qualitative	item6	0.68
qualitative	item12	0.68
qualitative	item13	0.63

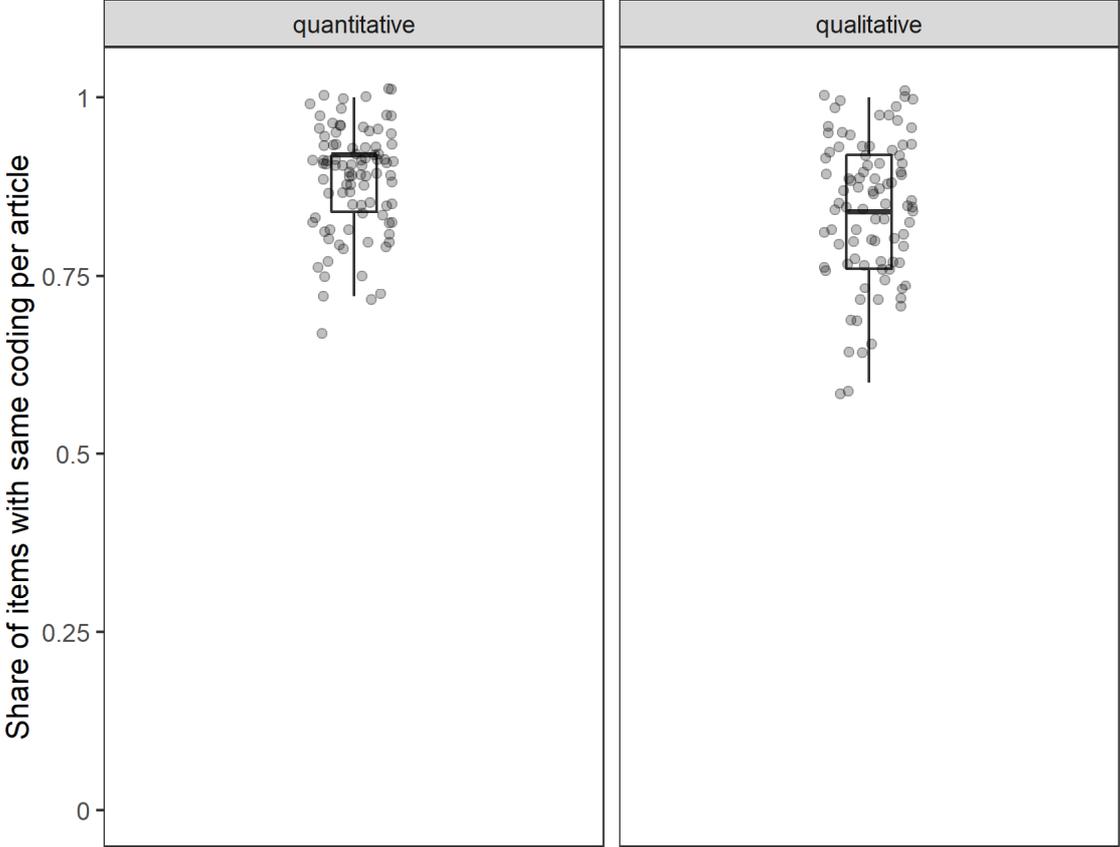
Finally, in Figure A.15 (below), we present the coding agreements per article aggregated over the 25 items for quantitative (left panel) and 90 qualitative articles (right panel).<sup>46</sup> A share of coding agreements of 0.8, for example, means that we assigned the same codings for 80% of all 25 items of an article, corresponding to a total of 20 items. The two panels demonstrate that the shares are higher for quantitative articles. However, the distribution for the qualitative articles shows that the third quartile is slightly above 0.75, which we take as a sufficiently

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<sup>46</sup> This means the unit of analysis is the individual article.

high share of coding agreements. Substantively, this means that the codings are the same for 19 items in about 68 articles.

Figure A.15: Coding agreement per article aggregated over 25 items for quantitative (left panel) and 90 qualitative articles (right panel)



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