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Interest group resources, access, and influence: An empirical review

Jonas A. H. Whittlestone¹  | Michael B. Klitgaard² 

¹Department of Political Science and Public Management, University of Southern Denmark, Odense, Denmark

²Department of Politics & Society, Aalborg University, Aalborg, Denmark

Correspondence

Jonas A. H. Whittlestone, Department of Political Science and Public Management, University of Southern Denmark, Campusvej 55, 5230 Odense, Denmark.

Email: jeri@sam.sdu.dk

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Abstract

Do informational and financial resources systematically secure interest group access and influence on public policy, and is one of these two resources politically more valuable than the other? Answering these questions is vital to understanding some of the drivers of political inequality and skewed influence associated with contemporary democratic politics. Focusing on two of the most commonly mobilized types of resources—money and information—this article reviews 60 studies on the topic. First, a qualitative synthesis shows that informational resources tend to be at least partially positively related to access and influence, while financial resources are more ambiguously associated with both outcomes. Second, a series of meta-regressions support this conclusion since they tend toward showing that informational resources are significantly stronger associated with these outcomes than financial resources. Thus, these results paint a more nuanced picture than the literature suggesting that moneyed interests subvert democratic politics.

KEYWORDS

access, influence, information, money, resources, review

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INTRODUCTION

Understanding which political resources interest groups can successfully translate into access to the policymaking process and deploy to achieve policy influence is perennial in analyzing fundamental aspects of democratic politics. Political resources are here understood as anything that can be used to sway the choices or strategies of another individual (Dahl, 1961, p. 226) and seem indeed to be positively associated with interest group access to and influence on public policymaking (see Dür & De Bièvre, 2007). Indeed, literature suggests that special interest groups dominate policymaking at the expense of middle-class preferences (see Gilens & Page, 2014).

The types of resources that interest groups are commonly held to invest in the political process are information and financial resources that can be exchanged for access to decision-making processes and, eventually, policy influence (Bouwen, 2004; Pfeffer & Salancik, 1978). The theorized political value of information and financial resources extends well beyond the exchange perspective and has been treated amply in a formal literature on the topic (e.g., Bernhagen, 2007; Hall & Deardorff, 2006). Nonetheless, little cross-study evidence exists regarding information and financial resources' absolute and relative usefulness in policymaking. We do not know whether and how information and financial muscles are systematically associated with access and influence for interest groups, and if one of these resources is more valuable than the other. In this article, we review the interest group literature and conduct a meta-analysis of a sample of studies to answer these questions.

Answering these questions are relevant for furthering our understanding of interest group politics in Scandinavia and beyond. As noted, it is broadly held in the literature that information and financial resources are important precursors of interest group influence. The Scandinavian countries are a case in point. Specifically, these countries have experienced a gradual weakening of neo-corporatist institutions and a shift toward a more pluralistic mode of interest intermediation (Öberg et al., 2011; Rommetvedt et al., 2012). This has had implications for interest group politics both concerning information and financial resources. On the one hand, interest groups' success in influencing public policy increasingly depends on their ability to provide relevant information to policymakers (Arnesen, 2023). Yet, on the other hand, a more plural interest group environment has also incentivized many groups to professionalize and scale up on financially demanding lobbying capabilities (see Binderkrantz et al., 2015). Thus, while it is plausible that lobbying has become more resource-demanding overall, it remains a significant contribution to examining the relative importance of said two resource types for influence-seeking groups.

We answer, as mentioned, the questions raised by conducting a review and meta-analysis of the interest group literature. We first searched databases for

relevant literature and ended up including 60 studies in the review that examined the impact of informational and/or financial resources on access and/or policy influence. The empirical review of these studies is divided into two parts. In the first part, we synthesize the empirical literature narratively, allowing us to examine the usefulness of the two resource types in isolation. In the second part, we conduct a series of meta-regressions using standardized effect sizes extracted from the included studies. By doing so, we can measure if one resource type is more strongly associated with access or influence than the other.

Our literature synthesis shows that informational resources tend to be at least partially positively related to access and influence, while financial resources are more ambiguously associated with both outcomes. The meta-regressions corroborate this conclusion since they tend to show that informational resources are significantly stronger associated with these outcomes than financial resources. Several checks show that the results from the meta-regressions are reasonably robust, albeit with some level of sensitivity to outliers and effects from single studies.

Our results contribute generalized knowledge to a rich and diverse literature. Based on the review, we suggest that while both financial and informational resources matter for influence-seeking groups, information appears to be a more potent tool for influence than finances. Interestingly, this difference becomes insignificant when the geographic context is included as a covariate in the meta-regressions. While we cannot be sure why this is the case, one possibility is simply that the scholarly emphasis on the two types of resources is correlated with geographical context—for example, that, say, US-centered studies tend to study financial resources more than European studies. In the conclusion, we elaborate and discuss some of the implications of these findings.

THE POLITICAL USEFULNESS OF INFORMATION AND FINANCIAL RESOURCES

Can organized interest groups translate information and/or financial assets into access to policymaking? And is information and/or financial resources efficient when interest groups seek influence on public policy? Raising these questions suggests that information and financial resources can be used as political resources, which we define as anything that can be used to sway the choices or strategies of another individual (Dahl, 1961, p. 226).

Theoretically, there are clear reasons to expect financial and informational resources to be positively associated with access and influence. Financial resources are valuable to policymakers in a mediatised and professionalized political reality where running for office is costly, and scholars have for a long time been occupied with whether money can buy public policy (Baumgartner et al., 2009). Further, as electorates are increasingly volatile and shift parties

with a higher frequency (Drummond, 2006) combined with declining turnouts in elections (Hooghe & Kern, 2017), politicians demand persuasive informational resources to attract an increasing number of independent voters. If interest groups can “analyze, synthesize, and summarize [information]—in a politically user-friendly form...” it can be exchanged for influence on public policy (Hall & Deardorff, 2006, p. 74). Or they can be granted access by acting as *service bureaus* for legislators by providing information about policies and potential electoral consequences (Hansen, 1991).

Financial resources can be defined as the passive control of, for example, budgets or more active use of lobbying expenditure, campaign contributions, or other financial assets toward achieving some policy goal. Informational resources are less tangible but like their financial counterparts, they are understood here as the passive possession or active use of information toward achieving some policy goal. The passive possession of information can be a by-product of participating in for example an expert group or special committees, while the active form can be related to seeking control of which information types are conveyed to policymakers and so on.

Access and policy influence can likewise be understood differently, and the latter concept is challenging to study empirically (Baumgartner & Leech, 1998). Binderkrantz et al. (2017, p. 307) define access as being present when a group enters a political arena by passing a threshold controlled by gatekeepers. However, this definition is too restrictive for the present purpose since many existing studies do not explicitly define access (Binderkrantz et al., 2017), which could lead to the exclusion of otherwise relevant studies. Therefore, we relax the part about gatekeepers and simply define access as being when a group enters a political arena. On the other hand, policy influence is defined as the ability to shape policy decisions in line with one's preferences (Dür, 2008, p. 561). In operational terms, we define policy influence as an alignment between groups' (stated or assumed) preferences and some policy output or outcome.

Identifying the relevant literature

We review the interest group literature to identify whether interest groups control over information and/or financial resources systematically affects their chances for gaining access to decision-making and achieving influence. The interest group literature is large and diverse—theoretically and methodologically—so how do we identify the relevant literature to include in the review?

First, the politics of interest groups in the United States, Europe, and the European Union have been converging since the 1990s (cf., Baumgartner & Leech, 1998; Woll, 2006) and we decided to include research from all three contexts. The main outcome of interest is policy influence. Yet studies that

examine access to policymakers and the policymaking process are also included since access is often considered a key precursor of influence.

Database searches were conducted in *Scopus* and *Web of Science* to identify potentially relevant studies. A subsequent backward search was conducted to identify studies that were not found in the initial searches. No journal restrictions were made since relevant work has also been done outside political science (Burstein, 2019). We did not specify a timeframe for the literature search. To be included, a manuscript had to be a peer-reviewed article, a book chapter, or a book; have resources as a main independent variable (i.e., not just as a control variable¹); contain an empirical part; focus empirically on one or more advanced democracies or a supranational collection of them such as the European Union; have access or public policy influence as a dependent variable; and be in English. We subsequently restricted the research focus to only include studies with informational and/or financial resources as an independent variable, meaning that 60 studies ended up being included in the review.

More information about the literature search, the data extraction, and the included studies can be found in the Supporting Information. We do not claim that the included studies are exhaustive, or even a random sample, of the universe of relevant studies. We still believe, however, that the set of included studies allows for a comprehensive review of the literature (see Burstein, 2020) and that this task is manageable with 60 included studies.

Having outlined how we identified the included studies, we now turn to the empirical review. We begin by synthesizing the literature narratively, aiming to identify cross-cutting areas of (dis)agreement in the literature and showcasing its breadth regarding empirical approaches and findings. This narrative account indicates that informational resources may be more valuable for access- and policy influence-seeking groups than financial resources. We subsequently test this proposition in a series of meta-regressions.

Information as a political resource

There is a near-consensus in the literature that information tends to be positively associated with access and influence. However, this conclusion is based on relatively few studies.

Access

A prominent example of a study which shows that informational resources matter for access is Hansen (1991) in a case study of the privileged access that the US farm lobby had to Congress during much of the twentieth century. Hansen shows that if an interest group has a competitive advantage, that is, that it can provide information more cheaply and efficiently than its competitors, and that circumstances that created this competitive advantage are likely to

recur, then it will enjoy privileged access to reelection-oriented legislators (Hansen, 1991).

Other studies have also examined the link between interest groups' informational capacity and access. Eising (2007) looks at business groups in the European Union and finds that providing policy-relevant information increases access to all the major EU institutions. Chalmers (2013) examines a broader set of interest group types and finds that access primarily results from providing information that meets EU policymakers' presumed demands and that information tactics appear more important than information types (Chalmers, 2013, pp. 47–54). In a different study of the European Union, Arras and Beyers (2019) also find that informational resources are positively associated with access. Specifically, they find that interest groups that hold a seat in a European Commission expert group are more likely to gain access to closed consultations held by EU agencies. Turning to the US context, the evidence suggests that increases in lobbyists' technical knowledge are associated with increased access to Congress (Esterling, 2004, p. 41).

Influence

The results linking informational resources to public policy influence also indicate a positive association. Two elaborate studies in the context of the European Union are Dür et al. (2019) and Klüver (2013b). Both rely on a preference attainment approach to influence but differ with respect to data and measurement. Despite these differences, their conclusions are similar since both studies find that the aggregate informational resources on the part of interest groups lobbying on the same side of an issue are more important for attaining influence than the informational resources held by individual groups (Dür et al., 2019; Klüver, 2013b). Thus, both studies underline that information provision is a crucial way to achieve influence in the European Union and that lobbying is a collective endeavor (see also Bernhagen et al., 2015).

Other studies investigating the link between informational resources and influence in the European Union include Hermansson (2016) who examines whether policy recommendations from interest groups with issue-specific expertise are more likely to be adopted in a European Commission proposal. Hermansson shows that issue-specific expertise is an important predictor of adoption, but only if the interest group also enjoys privileged access to the European Commission (Hermansson, 2016, p. 192).

The importance of providing technical information as a means to achieve preferred policy is echoed by Taghizadeh (2015) in a Swedish subnational context. Taghizadeh shows that loosely organized social movements mobilized around preventing school closures can provide technical information to legislators which, in turn, reduced the probability of school closure. Relatedly, Flöthe (2019) investigates lobbying success at the national level in five West

European countries and finds that expert information is significantly associated with self-assessed lobbying success while information about public preferences is not (see also Rasch, 2018).

Turning to the US context, a thorough examination of the link between informational resources and influence is Rich (2004), who studies a specific type of organized group, namely think tanks. Rich finds that relevant expertise from think tanks has the most considerable potential to be influential in the agenda-setting stage of the policymaking process since it usually reduces to argumentative ammunition in the decision-making stage (Rich, 2004, p. 108). Likewise, in a US context, Hoefler and Ferguson (2007) find that access to policymakers and information is associated with higher self-perceived effectiveness in influencing regulation among human service groups. Another study finds that providing scientific expertise is key to achieving influence in the preproposal stage at the agency level (Rinfret, 2011). Relatedly, Hansen (2011) finds that the higher the quality of the information in a comment letter provided by a lobbyist to the International Accounting Standards Board (IASB), the more likely the lobbyist's desired position is to figure in the final standard issued by the board.

Bernhagen (2007) is an example of a study that takes an entirely different empirical approach than those presented above. Specifically, Bernhagen studies business firms and theorizes that their influence emanates from structurally privileged positions in national economies and a comparatively superior endowment with private information vis-à-vis policymakers about the potential economic consequences of public policies (Bernhagen, 2007, pp. 55–63). This information asymmetry argument is tested country-comparatively but only partial support is found (Bernhagen, 2007, p. 129).

In sum, the empirical evidence shows that interest group informational resources are at least partially positively associated with access and influence over public policy. It should be noted, however, that this area of near-agreement is based on a limited number of studies.

Financial assets as political resources

A longstanding public and scholarly concern is that narrow interests can attain favorable public policy by using their financial resources in policymaking (Brown, 2016). The results from the existing literature reveal a more complex picture, however, and there is currently no consensus about the role of money in politics. Overall, financial resources are generally positively associated with access while the association with influence is more ambiguous.

Access

In the United States, interest groups spend vast financial resources on political campaigns. Campaign contributions have implications for the election-chances of candidates, and hence the granting of access due to campaign contributions has been theorized to be a *quid pro quo* exchange between donors and legislators (Kalla & Broockman, 2016). But, there is conflicting evidence as to whether this is the case. Chin et al. (2000) examine in a laboratory experiment whether schedulers for members of the House of Representatives were more responsive in the scheduling decision to meeting requesters being constituents (vis-à-vis non-constituents) of or political action committee (PAC) contributors (vis-à-vis non-PAC contributors) to the given representative. The results show that PAC contributor status was less important for scheduling decisions than was constituent status (Chin et al., 2000).

On the other hand, Kalla and Broockman (2016) examine in a field experiment whether an actual meeting was granted to a requester and the seniority of the official participating in the meeting. The experimental condition revealed in the meeting request was that the requester was a political donor (Kalla & Broockman, 2016, p. 548). Clear evidence was found that meeting requests from political donors were more successful at securing meetings with more senior officials compared to non-donors (Kalla & Broockman, 2016).

In an European Union context, campaign contributions are less important (Woll, 2006, p. 462); hence, other types of financial resources are often examined, including budgets, lobbying expenditure, and so on. Chalmers (2014), for example, shows that the more money an interest group devotes to interest representation at the European Union level, the more seats the group has on European Commission expert groups. Similarly, Rasmussen and Gross (2015) show that the lobbying budget of an interest group is associated with stronger access to European Union advisory committees. Looking specifically at business groups, larger budgets are associated with more access to both European Union political institutions (Eising, 2007; Kohler-Koch et al., 2017) and national level institutions (Kohler-Koch et al., 2017). In Denmark, Binderkrantz et al. (2015) show that higher annual incomes for interest groups are associated with better access to the bureaucracy but not to parliament.

Influence

A long line of scholarship, primarily from the United States, has sought to establish an empirical link between interest group financial resources and public policy influence. But the results are ambiguous and some scholars have asserted that "... the direct correlation between money and outcomes that so many political scientists have sought simply is not there" (Baumgartner et al., 2009, p. 214). This point is prominently showcased in the literature on the effects of

campaign contributions (see Ansolabehere et al., 2003, for a review). Indeed, the studies included in this review that look at this link find mixed results regarding the link between interest groups' campaign contributions and favorable roll call votes from legislators (Ansolabehere et al., 2003; Bronars & Lott, 1997; Chappell, 1982; Cohen & Hamman, 2003; Evans, 1996; Keiser & Jones, 1986; Langbein & Lotwis, 1990; Mallinson, 2014). But mixedness does not mean that campaign contributions never matter. Indeed, a meta-analysis of this literature finds that around one in three roll calls are significantly impacted by campaign contributions (Roscoe & Jenkins, 2005, p. 64).

Others have approached the campaign contribution question differently by looking at other empirical indicators of influence, incorporating more nuance and complexity and so on. Among studies that look at alternative indicators of influence is Kamieniecki (2006, p. 97) who finds that campaign contributions are essentially uncorrelated with different indicators of agenda-building. Studies looking at public policy outcomes rather than roll call voting also tend to find no relationship (Kluttz, 2019). Finger (2019) examines the effects of campaign contributions on perceived influence and whether a bill was proposed, blocked, or passed but, once again, no significant relationships are found. Another indicator of influence is government contracting where campaign contributions are positively associated with the number of contracts awarded to firms (Witko, 2011), but less strongly with the value of such contracts (Dusso et al., 2019; Hadani, Munshi, et al., 2017). Finally, Hansen (2011) finds that financial contributions to the IASB's oversight board are positively associated with having preferences reflected in auditing standards issued by the IASB.

Other studies incorporate more nuance and complexity. Constant (2006), for example, finds that contributions from teachers' unions are positively associated with favorable legislative roll call voting only for issues that feature prominently on such unions' agendas. Marianno (2019) also studies teacher policy and examines the relative resource expenditure (of which campaign contributions are a part) between teachers' unions, school choice advocacy groups and business groups (the latter two are assumed to be in opposition to the former) over time. Marianno's results show that when the relative resource expenditures of opposing groups increase, policies proposed and enacted in state legislatures tend to become more unfavorable to teachers (Marianno, 2019).

Aside from campaign contributions, there are other ways in which interest groups can spend their money. Junk (2019), for instance, examines in a European national-level context how the amount of economic resources that an interest group mobilizes around an issue affects self-assessed influence over public policy outputs in relation to the given issue. Junk's results reveal no significant direct relationship. In the US context, one study examining the determinants of influence in the form of earmarks from Congress to academic institutions finds some support that lobbying expenditure significantly increases the size of academic earmarks (de Figueiredo & Silverman, 2006, pp. 614-615). However, another, larger-scale study finds clear

evidence that lobbying expenditure (and financial resources more generally) of both individual lobbyists and lobbying sides are essentially unrelated to public policy influence (Baumgartner et al., 2009, pp. 203, 207).

Yet another financial resource is an interest group's budget size. Here, McKay (2012), who looks at revenue (for businesses) and budgets (for nonprofit organizations), finds that these are essentially unrelated to both subjective and objective measures of influence. This null-finding is supported by Mahoney (2007) who looks at both European Union and the United States and Buffardi et al. (2017) who look only at the United States. On the other hand, Steel et al. (1996) finds that budget sizes matter somewhat. Namely, they find that increases in budget sizes are associated with more self-perceived influence over legislators but not over agencies.

The results presented above indicate that financial resources are weakly associated with different measures of influence on public policy at best. However, there are important exceptions to this when looking at the European Union. Klüver (2013b), for example, approaches the question somewhat differently by theorizing, based on models of economic voting, that legislators need good economic performance in their aspirations for reelection and hence need support from economically powerful actors (Klüver, 2013b, p. 49). Economic power is here defined as the control over business investment and job creation and it is hypothesized that the more jobs and revenue a firm or a business group controls, the more influential it is likely to be (Klüver, 2013b, p. 57). Empirical support for this expectation is found for lobbying sides at both the policy formulation- and the decision-making stages in the European Union (Klüver, 2013b).

In sum, there is conflicting evidence as to whether financial resources are positively associated with access in United States politics while they are generally positively associated with access in Europe. Regarding influence, this is mainly explored in a United States context where the association is ambiguous.

Summing up, this qualitative literature synthesis indicates that informational resources may be more valuable overall than financial resources for groups seeking access and/or policy influence. This conclusion, however, is based on both qualitative and quantitative studies and does not consider the sizes of the relevant effects, nor can direct comparisons between the usefulness of the two resource types be made. Thus, to test more systematically whether information is more important than financial resources for gaining access and attaining policy influence, we now analyze standardized effect sizes through a series of meta-regressions.

Data extraction and estimation strategy for the meta-regressions

Meta-regression is a meta-analytic technique where the dependent variable is standardized effect sizes—in the present case, the effect of a resource variable on an access or policy influence variable—and the independent variables are factors that may moderate the conditional expectation of these standardized

effect sizes (Ringquist, 2013, pp. 144–145). Before running a meta-regression, we must extract effects from the studies included in the review and standardize these to make them comparable. Furthermore, it is necessary to extract the data for the independent variables. This section outlines how these data were extracted and coded, and how the meta-regressions were estimated.

In extracting the relevant data from the studies included in the review we relied largely on the recommendations by Ringquist (2013, pp. 102–140). The procedure was as follows: All 60 studies included in the review were searched for relevant effects (correlation coefficients, regression coefficients etc.). The searches were done in the main text of the study, the appendices, and in the online supplemental material (if the latter two existed). All relevant effects (meaning the effect of an informational or financial resource variable on an access of policy influence variable) were extracted. No distinction was made between main variables and control variables, meaning that effects of relevant control variables were also extracted. Effects were not extracted when the resource variable of interest was included in an interaction term with another variable in the given model since interactions change the main effects making them of little value here.

Most of the extracted effect sizes were unstandardized, so they needed manual standardization. Specifically, the extracted effects are standardized to partial correlation coefficients since these only require information on the sample size, the (absolute) *t*-score, and the degrees of freedom. The *t*-score and degrees of freedom were seldom reported in the studies. Hence, they often had to be calculated from the parameter estimates, sample size, and so on.² The degrees of freedom were calculated as $df = n - k^*$ where *n* is the sample size and *k** is the number of estimated parameters including intercept(s) in a given model.³ After the partial correlation coefficients had been calculated, they were transformed to Fisher's *Z* and their variances were calculated⁴ as this measure is often more appropriate in meta-regressions than are partial correlation coefficients (see Ringquist, 2013, p. 109). Finally, the proper sign was assigned to each standardized effect size such that a positive standardized effect size indicates that the resource variable is positively correlated with access or influence, and a negative standardized effect size indicates a negative correlation.⁵

It was not possible to extract effect sizes from all 60 included studies. Some studies were qualitative and did therefore not contain any quantitative effects. Other, quantitative studies did (to the best of our judgment) not contain sufficient information to standardize the reported raw effects. In total, we extracted and standardized 418 effects from 39 studies. Table 1 contains a list of the 60 studies included in the review, how many effects were extracted and standardized for the meta-regressions from each study, and whether a study is about financial, informational, or both types of resources.

Turning to the coding of effect-level independent variables, the primary variable of interest here is whether the resource variable associated with an effect was informational or financial. Second, and in line with our definitions of

TABLE 1 Included studies and number of extracted standardized effect sizes from each study.

Study reference	Number of extracted effects	Resource type in study
Ansolabehere et al. (2003)	0	<i>Financial</i>
Arras and Beyers (2019)	5	Informational
Baglioni (2015)	0	<i>Financial</i>
Baumgartner et al. (2009)	20	Financial
Bento et al. (2013)	15	Financial
Bernhagen (2007)	10	Informational
Bernhagen (2012)	0	<i>Financial</i>
Bernhagen et al. (2015)	6	Informational
Binderkrantz et al. (2015)	3	Financial
Blau et al. (2013)	16	Financial
Bronars and Lott (1997)	0	<i>Financial</i>
Buffardi et al. (2017)	6	Both
Chalmers (2013)	18	Informational
Chalmers (2014)	1	Financial
Chappell (1982)	0	<i>Financial</i>
Chin et al. (2000)	0	<i>Financial</i>
Cohen and Hamman (2003)	12	Financial
Constant (2006)	6	Financial
de Figueiredo and Silverman (2006)	1	Financial
DeGregorio (1997)	0	<i>Financial</i>
Dusso et al. (2019)	9	Financial
Dür et al. (2019)	0	<i>Informational</i>
Eising (2007)	12	Both
Esterling (2004)	2	Informational
Evans (1996)	2	Financial
Finger (2019)	26	Financial
Flöthe (2019)	50	Informational
Furlong (1997)	2	Financial

(Continues)

TABLE 1 (Continued)

Study reference	Number of extracted effects	Resource type in study
Gerber (1999)	24	Financial
Hadani, Bonardi, et al. (2017)	0	<i>Financial</i>
Hadani, Munshi, et al. (2017)	0	<i>Financial</i>
Haeder and Yackee (2015)	1	Financial
Haider-Markel (2006)	1	Financial
Hansen (1991)	0	<i>Informational</i>
Hansen (2011)	3	Both
Hermansson (2016)	3	Informational
Hoefer and Ferguson (2007)	1	Informational
Junk (2019)	10	Financial
Kalla and Broockman (2016)	0	<i>Financial</i>
Kamieniecki (2006)	0	<i>Financial</i>
Keiser and Jones (1986)	4	Financial
Kluttz (2019)	26	Financial
Klüver (2013a)	6	Both
Klüver (2013b)	16	Both
Kohler-Koch et al. (2017)	16	Financial
Langbein and Lotwis (1990)	1	Financial
Mahoney (2007)	0	<i>Financial</i>
Mallinson (2014)	3	Financial
Marianno (2019)	8	Financial
McKay (2012)	6	Financial
Mutter et al. (1999)	0	<i>Financial</i>
Orach et al. (2017)	0	<i>Informational</i>
Rasch (2018)	60	Informational
Rasmussen and Gross (2015)	4	Financial
Rich (2004)	0	<i>Informational</i>
Rinfret (2011)	0	<i>Informational</i>

TABLE 1 (Continued)

Study reference	Number of extracted effects	Resource type in study
Roscoe and Jenkins (2005)	0	<i>Financial</i>
Steel et al. (1996)	0	<i>Financial</i>
Taghizadeh (2015)	3	Informational
Witko (2011)	0	<i>Financial</i>
In total	418	

Note: Italicized rows denote studies that are not included in the meta-regressions.

TABLE 2 Overview and examples of operationalizations of resources and access/influence.

Variable	Operationalization type	Examples of measurement
Resource measure	Passive	Budget size (Haider-Markel, 2006; Kohler-Koch et al., 2017; McKay, 2012; Steel et al., 1996). Expertise of stakeholders and/or lobbyists (Esterling, 2004; Hermansson, 2016).
	Active	Expenditure on lobbying (Dusso et al., 2019; Junk, 2019). Campaign contributions (Finger, 2019; Marianno, 2019). Frequency of information provision, or amount of information provided, to policymakers (Chalmers, 2013; Eising, 2007; Klüver, 2013b).
Access/ Influence	Subjective	Self-assessed lobbying success or influence on policy outcomes (Buffardi et al., 2017; Flöthe, 2019; Junk, 2019). Self-assessed frequency of contact with E.U. decision-making institutions (Chalmers, 2013).
	More objective	Number of appearances in public committees (Binderkrantz et al., 2015). Decision to close, or partially close, a school (Taghizadeh, 2015). The value of awarded federal contracts (Dusso et al., 2019).

Note: More details about operationalizations can be found in the Supporting Information.

resources, it was coded whether the resource measure indicated the passive possession, or the active use of resources. We would generally expect larger effects for active uses of resources since it is ultimately the active use of resources that can affect policymakers (Burstein, 2019, 2020). Third, we coded

whether the dependent variable was a subjective measure or a more objective one. Table 2 shows examples of passive/active resource measures and subjective/more objective measures of access/influence found in the reviewed literature. Fourth, we coded whether the dependent variable was an access or influence variable. Fifth, the geographical context of a given effect was coded with the categories being the United States, the European Union, and European nations. We do not have strong expectations regarding the latter three independent variables but include them for more exploratory purposes. Descriptive statistics can be found in the Supporting Information.

Meta-regression models typically assume that the standardized effect sizes are independent of each other (Hedges et al., 2010; Ringquist, 2013, p. 191). This assumption is unlikely to hold here since many studies yielded more than one standardized effect size (cf., Table 1). Hence, it is necessary to account for potential non-independence in the form of within-study correlated standardized effect sizes. Therefore, we use the *robu* function in the *robumeta* package in the statistical software *R* to estimate all meta-regression models (Fisher & Tipton, 2015). The resulting models are meta-regressions with variance estimations that are robust to the potential within-study correlation of standardized effect sizes—even when the dependence structure is unknown.⁶ Specifically, the estimator assumes an equal correlation between standardized effect sizes from the same studies (see Hedges et al., 2010). We set this correlation value to 0.8 which is also the default value in the *robu* function.⁷ Furthermore, a small sample correction is used in all models to account for that the sample of studies from which we were able to extract and standardize effect sizes is relatively small (see Tipton, 2015).

Is information more valuable than financial resources?

A total of eight meta-regression models are estimated and the results are reported in Table 3. Model 1 contains only an intercept. The intercept indicates that, on average, informational and financial resources are positively and significantly ($p < 0.01$) associated with access and influence. Model 2 contains only the main independent variable of interest, that is, the resource type with the informational resource being the reference category. The coefficient is negative and significant ($p < 0.05$) thus supporting our main expectation. Specifically, this coefficient indicates that standardized effect sizes are smaller, on average, for financial resources than for informational resources. Additional covariates are added sequentially in Models 3–6. The main finding that financial resources are associated with lower standardized effect sizes than informational resources, on average, remains in all specifications, except when geographical context is added as a covariate in Model 6. In this model, the resource type variable becomes insignificant ($p = 0.134$), although the coefficient

TABLE 3 Meta-regression results.

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
<i>Resource:</i> Information	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)
<i>Resource:</i> Financial		-0.073** (0.032)	-0.075** (0.034)	-0.073** (0.035)	-0.072** (0.034)	-0.062 (0.038)	0.012 (0.036)	-0.107** (0.044)
<i>Resource measure:</i> Passive	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)
<i>Resource measure:</i> Active			0.008 (0.040)	0.016 (0.043)	0.017 (0.043)	0.021 (0.044)		
<i>Outcome:</i> Access	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)
<i>Outcome:</i> Influence				-0.034 (0.038)	-0.032 (0.039)	-0.029 (0.030)		
<i>Outcome measure:</i> Subjective	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)
<i>Outcome measure:</i> More objective					-0.021 (0.035)	-0.010 (0.035)		
<i>Geography:</i> US	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)	(Ref.)
<i>Geography:</i> E.U.						0.009 (0.041)		
<i>Geography:</i> European countries						0.050 (0.036)		
Intercept	0.072*** (0.017)	0.122*** (0.024)	0.118*** (0.035)	0.138*** (0.034)	0.150** (0.050)	0.119 (0.072)	0.096** (0.022)	0.141*** (0.035)

(Continues)

TABLE 3 (Continued)

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
<i>N</i> studies	39	39	39	39	39	38	8	31
<i>N</i> effects	418	418	418	418	418	408	61	357
<i>I</i> ²	86.199	86.338	86.508	86.305	86.586	87.176	74.790	87.717
τ^2	0.009	0.010	0.010	0.011	0.011	0.012	0.004	0.013

Note: Standard errors are reported in parentheses. The dependent variable in all models is standardized effect sizes. All models are estimated with the *robiv* function in the *robumeta* package in R. The models are estimated with within-study correlated effects and the correlation is set to the default $\rho = 0.8$. Small sample correction is used in all models.

**** $p < 0.01$; *** $p < 0.05$; * $p < 0.1$.

maintains its negative sign, as expected. The other covariates do not reach conventional levels of statistical significance.

A common criticism of meta-analyses in general is that they compare the incomparable which may lead to essentially meaningless results (Card, 2012, p. 25). To alleviate this concern, at least to an extent, we test the main effect of resource type on the two outcomes (access and policy influence) separately. Specifically, in Models 7 and 8 we split the sample of standardized effect sizes such that Model 7 only includes standardized effect sizes where access is the outcome, and Model 8 only includes those standardized effect sizes where policy influence is the outcome. Model 7 is based on just 61 standardized effect sizes from eight studies which likely makes the model underpowered. The resource type coefficient in Model 7 is positive, which contradicts the main expectation. However, the coefficient is far from significant at conventional levels. In Model 8 the main effect is once again negative and significant ($p < 0.01$) thus supporting our main expectation. Overall, however, the main takeaway from Models 1–8 is a tendency toward support for the proposition that informational resources are more valuable than financial resources concerning gaining access and attaining policy influence with the latter outcome being the main driver of this result.

To assess the robustness of the results, we first tested the sensitivity of the coefficients in the models to different levels of within-study standardized effect size correlations using the *sensitivity* function in the *robumeta* package. The results are not reported due to their verbosity, but they showed that the coefficients and standard errors changed very little under different within-study correlations. Second, Models 1–8 were re-estimated with potential outliers excluded. Here, a potential outlier is defined using the *inter quartile range method*, that is, if as a standardized effect size falls more than 1.5 times the interquartile range below the first quartile or more than 1.5 times the interquartile range above the third quartile, it is considered a potential outlier. This led to the exclusion of 34 potential outliers. Generally, the results from the models excluding potential outliers were like those in Models 1–8, albeit with the p -values increasing in many of the models such that the resource type variable is only significant with $p < 0.1$. Further, the coefficient sizes decrease somewhat compared to the main models. Finally, a leave-one-study-out analysis was conducted to check whether a single study's effects drove the results. This entailed re-estimating Model 2 39 times, where each estimation had all standardized effect sizes from one study excluded. In all 39 models, the main effect maintained a negative sign and was significant with at least $p < 0.1$. However, some degree fluctuations in the coefficient sizes is present which indicates that some studies were more influential than others. The results from these robustness checks can be found in the Supporting Information.

While the robustness checks do not pull the rug from under the findings from the main models, they do indicate that they are to some extent sensitive to

outliers and effect estimates from single studies. Thus, the results presented above should only be seen as indicative.

CONCLUSION

The purpose of this paper was to review the empirical literature that examines whether and how informational and financial resources are empirically associated with access and policy influence—both in isolation and in comparison—by using both a qualitative synthesis and a series of meta-regressions. Overall, the results support the commonly held assumption in the literature that resources tend to matter for access- and influence-seeking groups. Indeed, we find that both financial resources and informational resources are positively associated with access and influence. However, this association seems clearer for informational resources than for financial resources, where the results are generally more ambiguous. Further, when comparing the two types of resources, we find that information seems to be more strongly associated with access and influence than are financial resources.

Despite calls for stronger coherence and cognizance in the interest group literature by for example Baumgartner and Leech (1998), meta-analyses like the present one are not very common. However, when comparing this article to other recent meta-analyses, it is arguably the case that we address a more narrow research question with a more sophisticated methodology. It has a more narrow focus since we are concerned with one specific explanation of interest group access and influence, that is, groups' informational and/or financial resources. This is different from other recent meta-analyses that are considerably broader in terms of the set of theories they test (e.g., Burstein, 2021). However, by using meta-regressions we are able to test the overarching hypothesis more systematically (i.e., by standardizing the effect size, including covariates in the meta-regressions and so on) than is possible with the more commonly used descriptive ways of summarizing the results from a set of studies (e.g., by counting the number of studies that support/go against the overarching hypothesis).

Despite this, this article and its conclusions still have limitations of which we now address some of the most important ones. Specifically, the literature is highly diverse and heterogeneous regarding methodology, operationalizations etc., which is common in the interest group literature (see Baumgartner & Leech, 1998), and across the social sciences more broadly (see Ringquist, 2013, p. 73). This can be problematic in meta-regressions since it increases the risk of comparing studies and empirical results that are not easily compared. Furthermore, the results from the meta-regression are inherently correlational, so causal conclusions should not be made. In addition, since the included studies are not a random sample from the universe of relevant studies, we cannot generalize the results from the meta-regressions to studies beyond those

included in the regression models. Finally, the robustness checks showed some level of sensitivity to outliers and effects from single studies which led to a call for caution regarding interpretations.

In addition, given that this article is a literature review, it is worth briefly considering the potential impact of the scholarly knowledge production process itself on the empirical results. First, as also noted in the introduction, the difference between informational and financial resources became insignificant in the meta-regression models when geographical context was included as a covariate. There can be many reasons for this, but one likely possibility is that the studies in different geographical contexts tend to emphasize different resource types. If true, this would introduce collinearity between the geographical context and the resource type variable thus increasing the size of the standard errors. We know, for instance, that US-centered studies have a long tradition of studying financial resources due to factors such as the perceived importance of money in politics, data availability, and so on. This is, arguably, less so the case in Europe. However, whether this potential difference in research agendas across geographical contexts reflects a true underlying difference in the relative importance of resource types in different geographical contexts remains an open question.

Second, and related, it is possible that publication biases in the existing literature affect our results. Specifically, it could be the case that it is easier to publish null-findings about financial resources than informational resources because it may be considered more surprising when money do *not* matter for influence considering the commonly held beliefs about the importance of money in politics, the power of big business and so on. If this type of publication bias exists, it would drive the results toward what we find in the analyses—that information is a stronger predictor of influence than financial resources. However, it should be noted that only published studies are included in this review and therefore we cannot meaningfully gauge the extent of such a potential publication bias.

These caveats and considerations aside, this article's conclusion that information tends to beat money suggests important nuances for the interest group literature. First, on a broad scale, the conclusions reach into the debate about political inequality in contemporary democracies (see Elkjær & Klitgaard, 2021). A concern in this debate is that moneyed interests attain disproportionate influence over policy (e.g., Gilens & Page, 2014). We do not directly corroborate nor refute this concern, but our results indicate that policymakers tend to value informational resources more than financial ones, on average.

In a Scandinavian context, our findings are not overwhelmingly surprising. Information is increasingly important in corporatist exchanges between organized interests and policymakers (Arnesen, 2023), and these exchanges are proximate to policymaking. However, we have shown that policymakers'

relative valuation of these two types of resources is not particular to the Scandinavian context. Indeed, the results show that information tends to be more important across interest intermediation systems spanning the pluralist-corporatist spectrum.

Finally, we suggest two avenues for future research. First, the number of studies examining the effects of groups' informational resources remains small compared to those considering financial resources. We acknowledge that the intangibility of informational resources makes them challenging to study empirically. Nonetheless, the literature would benefit from theoretical and methodological innovation in studying the policy consequences of how informational resources are distributed between groups, how groups deploy information, and so on. Second, it would be fruitful to explore the interplay between informational and financial resources to facilitate access and influence. In connection with this, it would significantly broaden the scope by going beyond Western, liberal democracies to, say, hybrid- or autocratic regimes.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflict of interest.

ORCID

Jonas A. H. Whittlestone  <https://orcid.org/0000-0002-6954-2719>

Michael B. Klitgaard  <https://orcid.org/0000-0001-6561-8774>

ENDNOTES

¹ We chose this inclusion criterion to maximize the theoretical and empirical relevance and coherence in the set of included studies.

² The absolute value of the *t*-score for a regression coefficient $\hat{\beta}$ with standard error $SE_{\hat{\beta}}$ estimated with any statistical method was calculated as $t = |\hat{\beta}/SE_{\hat{\beta}}|$. The only case where this equation was not used was when odds ratios (OR) were reported. In this case, the *t*-score was calculated as $t = |\ln OR|/(SE_{OR}/OR)$. Sometimes only *p*-values were reported in which case the *t*-score was calculated based on this value together with the degrees of freedom. In some cases, *p*-values were reported as for example, 0.000. In these cases, we padded with a 1 such that a *p*-value of 0.000

was extracted as 0.0001. Furthermore, in some studies, the standard errors were not reported, and statistical significance was only indicated with significance symbols or an absence thereof. In these cases, we followed the advice by Ringquist (2013, p. 108) and set the t -score to the value of t at the symbol threshold given the degrees of freedom, and if an effect was insignificant we set t to 0. Importantly, the resulting standardized effect sizes using these t -scores will be lower bounds (Ringquist, 2013, p. 108).

- ³ The degrees of freedom were rarely reported which is why they usually had to be calculated manually. However, it was not always entirely clear how many parameters were estimated in a given model (if, for example, fixed effects or random intercepts were used) which sometimes made it necessary to make judgment calls on this account given the available information in the study. This also means that there will likely be some amount of measurement error in the calculation of the degrees of freedom which is, however, unlikely to be systematic.
- ⁴ The partial correlation coefficients r were calculated as $r = \sqrt{t^2/(t^2 + df)}$ with variance $V[r] = (1 - r^2)^2/(n - 1)$. These were then transformed to Fisher's Z using the formula $Z_r = 0.5 \cdot \ln[(1 + r)/(1 - r)]$ with variance $V[Z_r] = 1/(n - 3)$. Some special cases included if a regression coefficient was already standardized in the original study or if the effect size was a correlation coefficient. In these cases, r was simply set to these values. If Z -scores were reported in a study the calculation of the partial correlation coefficient was $r = \sqrt{Z^2/n}$ (Ringquist, 2013, pp. 103-110).
- ⁵ To assign the proper sign to r , three steps were necessary. The first step was to code the direction of the effect in a study, that is, to code whether or not a positive coefficient meant that increases in the resource variable were associated with increases in the access/influence variable. The second step was to base the calculation of r on the absolute t -score as detailed above. The third step was to assign a positive sign (+) to r if the effect extracted from a study corresponded to the direction from step one and a negative sign (-) if the extracted effect did not correspond to the direction from step one (see Ringquist, 2013, pp. 74, 138). As an example, if, in some study, a *negative* coefficient would indicate a positive relationship between a resource variable and an access variable, then we would assign a *positive* sign to the calculated r if the extracted coefficient is indeed negative (as expected) and a *negative* sign to r if the extracted coefficient is positive (counter to expectation).
- ⁶ The dependence structure is unknown here (as is usually the case) since it requires knowledge about the covariance structure of the effect size estimation errors which, in turn, requires the raw data of each individual study (Hedges et al., 2010, p. 40).
- ⁷ We test the sensitivity of the results to using different levels of within-study correlation. More details below.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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