Information Perspectives on Accounting and Taxation

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Abstract

This dissertation examines accounting's economic role by exploring how information is generated, distributed, received, and processed by economic agents. Building upon such information perspectives, I analyze how accounting and taxation shape (our comprehension of) the behavior of various economic agents, including non-entrepreneurs. The three chapters (A) - (C) of this dissertation, thus, are all embedded in a simple but powerful framework of sending and receiving information. In (A), the focus is on how intentional financial misrepresentation (i.e., accounting fraud) by a firm affects the financial situation of the individuals who establish the spatial community around which the firm operates. I investigate the influence of interest groups on the textual sentiment of press coverage of tax reforms in (B). Tax knowledge diffusion via strategic alliances is documented in (C) by identifying economically meaningful decreases in effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to pseudo treated high-tax firms in strategic alliances with other high-tax firms. Overall, my dissertation provides novel insights for practitioners and policymakers by employing information perspectives on accounting and taxation.

Information Perspectives on Accounting and Taxation

Abstract

Diese Dissertation befasst sich mit den ökonomischen Implikationen von Accounting und Taxation. Die einzelnen Analysen und Kapitel werden dabei in Informationsperspektiven *("Information Perspectives")* eingebettet, in denen der Einfluss des Generierens *("generate")*, des Verbreitens *("distribute")*, des Erhaltens *("receive")* und des Verarbeitens *("process")* von Informationen auf das Verhalten ökonomischer Akteure analysiert wird. Hierdurch entstehen vielfältige Beziehungen zwischen Sendern und Empfängern von Informationen, welche die thematisch diversen Kapitel (A) bis (C) miteinander verknüpfen. In Kapitel (A) liegt der Fokus auf lokalen finanziellen Implikationen unternehmerischer Bilanzmanipulation. In (B) untersuche ich den Einfluss von Interessensverbänden auf Nachrichtenberichterstattung zu Steuerreformen. Die Diffusion steuerlichen Wissens in strategischen Allianzen wird in (C) identifiziert. Die in dieser Dissertation getroffenen *Information Perspectives on Accounting and Taxation* erlauben somit nicht nur die Analyse des Verhaltens einer Vielzahl ökonomischer Akteure, sondern liefern auch innovative Erkenntnisse für Praxis und Politik.

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Introduction

The economic role of accounting is described to "increase welfare through its effects – in conjunction with complementary institutions – on firm and household behavior" (Ball 2024, 7). In this dissertation, I elucidate accounting's economic role by exploring how information is generated, distributed, received, and processed by economic agents.[1] Building upon such *information perspectives*, I analyze how accounting and taxation shape (our comprehension of) economic behavior and contribute research insights for practitioners and policymakers.[2] Consistently, the three chapters that follow are all embedded in a simple but powerful framework of sending ("generated, distributed") and receiving ("received, processed") information that underlies Weinrich and Kim (A)[3], Weinrich (B)[4], and Mueller and Weinrich (C)[5]. At first glance, however, the chapters appear thematically diverse. In (A), the focus is on how individuals' financial situations are affected by accounting fraud. I ask in (B) whether interest groups influence the textual sentiment of press coverage of tax reforms. Tax knowledge diffusion via strategic alliances is analyzed in (C). In this introduction I, therefore, explain the motivation for and discuss how (A) - (C) relate to the targeted contribution.

The motivation for (A) is rooted in recognizing that accounting fraud generates important economic consequences (Kedia and Philippon 2009). In particular, individual level analyses revolve around specific subsets of those directly tied to the firm, such as managers (Egan, Matvos, and Seru 2019, 2022) or employees (Choi and Gipper 2024). The core idea of (A), however, is to expand beyond individuals' specific, direct ties to the fraudulent firms to identify the comprehensive financial impact of accounting fraud on all eventually exposed individuals. Therefore, Weinrich and Kim ("we") examine in (A) how intentional financial misrepresentation (i.e., accounting fraud) by a firm affects the financial situation of the individuals who establish the spatial community around which the firm operates. Importantly, the financial situation of

[1]My conceptualization of accounting is inspired by the DFG funded collaborative research center *TRR 266 Accounting for Transparency* (Project ID 403041268). The work in this dissertation has greatly benefited from the resources and members of this initiative. Financial support from the foundation *Stiftung Prof. Dr. oec. Westerfelhaus* (Project ID P02) is also greatly acknowledged.

[2]*Perspective* for the purpose of this dissertation captures how information is considered in the behavior of economic agents. In other words, it is "the action of looking into or through something" (Oxford English Dictionary 2023): I look into accounting and taxation through a framework of information.

[3]Weinrich, Arndt, and Ji-Eon Kim. "Wiped Out? Financial Health of Individuals Affected by Accounting Fraud." *Working Paper.*

[4]Weinrich, Arndt. "Press Coverage of Tax Reforms and Interest Groups." *TRR 266 Accounting for Transparency Working Paper No. 129.*

[5]Mueller, Jens, and Arndt Weinrich. "Tax Knowledge Diffusion via Strategic Alliances." *TRR 266 Accounting for Transparency Working Paper No. 17.*

such a broad set of individuals is one of society's key economic indicators, and it has a direct pathway to important social outcomes (Butler et al. 2023; Sergeyev, Lian, and Gorodnichenko 2023). Upon fraud revelation, we identify significant increases in indicators of financial distress among individuals who reside in spatial proximity to fraudulent firms' headquarters. On average, we observe incremental increases in debt in collection, affecting approximately one in every one hundred to one in every two hundred individuals. Furthermore, we compare individuals' credit demand and supply under fraudulent 'good' information to that under truthful 'bad' information from firms headed for bankruptcy, revealing misinformed financial decisions before the fraud's exposure. Additional evidence indicates that such misinformed financial decisions are harmful, too.

The key innovation of (A) is that we can examine the connection between one of the roots of social outcomes, the financial health of a broad set of individuals in a spatial community, and accounting fraud. Thus, the results of our study also speak to policy makers and their agencies, when shaping, implementing, and enforcing policies related to accounting fraud. Importantly, we derive at these implications by empirically testing how information affects economic behavior. Ex ante to a fraud's revelation, a firm's management engages in regulatory violations but continuously disseminates contradictory, fraudulent 'good' information to the public through (mandatory) financial reporting. Such fraudulent yet seemingly valid information has been documented to affect financial decisions of firms (Beatty, Liao, and Yu 2013; V. Li 2016). Since fraudulent information could also be relevant for individuals' expectations about future economic conditions, we are interested in individuals' financial decisions during the fraudulent periods. In particular, financial decisions could diverge from those under truthful 'bad' information, appear misinformed in hindsight, and contribute to the effects upon revelation. Since there is anecdotal but little empirical evidence for individuals' misinformed financial decisions under fraudulent information (e.g., see Ornstein (March/4/2002)), we leverage comprehensive data on individuals' credit card limits to benchmark financial decisions under fraudulent information against those under truthful, adverse information.

This set of tests in (A), thus, already holistically speaks to how information are generated, distributed, received, and processed by economic agents. Notably, we include not only firms but also, somewhat unusually for business studies, individuals (i.e., non-entrepreneurs) among the economic agents of interest. Specifically, we tie firm level behavior to individual level financial outcomes to capture the economic role of accounting as proclaimed by Ball (2024). We do so by examining indicators of financial distress upon fraud revelation. We embed these analyses in a theoretical framework which suggests that economically meaningful accounting fraud may cause financial consequences upon revelation that might not sufficiently be captured when analyzing specific subsets of those with direct ties to the fraudulent firms. Instead, concerns about its broader impact arise as fraud eventually affects a wide range of stakeholders and individuals who fall beyond the SEC's shareholder-centric mandate (Velikonja 2012). Furthermore, comprehensive assessments of economic consequences frequently reveal effects that extend beyond the immediate financial impact on certain individuals through contagion and spillovers (Kleiner, Stoffman, and Yonker 2021; Gupta 2019). Consequently, a key difficulty for (A) is to identify and observe a diverse range of individuals who are exposed to a fraudulent firm, regardless of whether they have specific, direct ties to the firm. We address this challenge by analyzing highly granular panel data on indicators of financial distress of a random sample of all individuals who reside in the communities for which fraudulent firms are economically important.

We make several novel contributions to the literature, which are discussed in detail in (A). In essence, (A) contributes to the research on financial misconduct by providing a better understanding of its broader economic impact. Typically, existing insights address the consequences of financial misconduct to the extent that specific groups of share- and stakeholders directly experience bad realizations (e.g., see Egan, Matvos, and Seru (2019) and Egan, Matvos, and Seru (2022) for management implications). We add to these insights by providing an analysis that identifies and quantifies the impact of intentional financial misrepresentation on one of society's key economic indicators: the financial health of a broad set of individuals. Financial health has a direct pathway to important social outcomes, such as divorce probability (Butler et al. 2023), and psychological costs (Sergeyev, Lian, and Gorodnichenko 2023). We present robust evidence linking intentional financial misrepresentation with the financial health of individuals residing in the spatial communities for which fraudulent firms are economically important. Since our analyses reveal that a broad population is, on average, affected, our findings also speak to the roots of social costs of accounting fraud.

Furthermore, we analyze individuals' financial decisions when the fraud has not yet been revealed. This analysis not only addresses the eventual effects upon the revelation of fraud but also connects our study to the discussion on financial consumer prudence, where it remains a matter of debate whether precautionary saving is an (un-)important part of consumer behavior (e.g., see Dynan (1993) and Aydin (2022)). We contribute with (A) to this fundamentally important question from a different perspective and show that individuals' financial decisions appear misinformed under fraudulent information. Our findings, thus, underscore the critical role of reliable and verifiable information in financial decisions.

In (B), I turn to press coverage of tax reforms and interest groups. Specifically, the focus of (B) is on how information are generated and distributed by intermediaries (news/media/*the press*) when interacting with other economic agents, namely interest groups. A seemingly simple observation underlies this study: news is essential to elevate transparency. Individuals, for instance, often do not directly experience changes to public policy but learn about them from the news (Soroka and Wlezien 2019). Exerting influence over the way that individuals eventually perceive the presented information, therefore, allows for influence over the political discourse (Strömberg 2004). Such influence imposes an important source of political power (Gilardi et al. 2021) and should become a strategic objective for those with special interests (i.e., interest groups). Therefore, I ask in (B) whether interest groups influence the textual sentiment of press coverage of tax reforms, which are prominent in both news reporting and special interest seeking. My analysis, thus, also speaks to the ongoing discussion regarding transparency mandates in policy.

Calls for transparency typically center around immediate interactions between politicians and interest groups (i.e., inside tactics) and aim to mitigate the tension between providing expertise and seeking strategic influence by interest groups. If, however, outside tactics, through which interest groups publicly seek to influence policy outcomes, are important for realizing policy goals (as suggested by survey and interview evidence from Chalmers (2013); see also Bruycker and Beyers (2015)), then transparency mandates on inside tactics could ultimately be undermined and rendered less effective. One key challenge for an analysis, however, is to explore interest groups' outside tactics concerning economically significant policy actions and how these tactics determine an outcome variable of interest that, in turn, may influence a policy's outcome. Hence, Becker, Bischof, and Daske (2021) stress that endeavors to influence policy decisions remain obscure. I address this challenge in (B) by analyzing tax reforms (in Germany), press coverage of these reforms in quality outlets, interest groups' appearances in the articles, and their influence on the articles' characteristics, such as textual sentiment. Through 2SLS and OLS estimates and the application of a modified control function approach, I identify increases in the differences among represented opinions (subjectivity effect). This subjectivity effect translates into relative increases in both positive and negative textual sentiment within an article. Additional tests reveal that interest groups affect articles' textual sentiment on the negative margin. Furthermore, I find that staleness of texts increases with the appearance of interest groups. Taken together, these results suggest that articles are not balanced but ambiguous when interest groups appear in them, indicating that interest groups particularly provide redundant information.

I focus on textual sentiment in (B) because it captures the (un-)intended and latent value assignment of the underlying corpus. It is shown to distort the perceptions of recipients as they transform acquired information into new knowledge. Some exemplary consequences of these (mis)perceptions include biases in investor decisions, company strategies, stock markets, employment choices, investments in education, voting, tax evasion, and political trust (see Introduction and Conceptual Framework in (B)). Boydstun, Highton, and Linn (2018) highlight that textual sentiment "of economic news coverage has an independent, direct connection with economic attitudes." Furthermore, the media's concentrated and emotionally based coverage is consistently found to influence public policy by putting pressure on decision makers. For (B) it follows that interest groups, when they successfully influence the textual sentiment of tax reform press coverage, may influence the political discourse with an outside tactic that is nearly impossible to regulate. To gain institutional insights in how such a potential influence could actually materialize, I conduct an interview with a tenured journalist at a quality outlet before I turn to the data. The conjunction of this anecdotal evidence, the conceptual framework, and insights from prior literature suggest that interest groups are indeed incentivized to influence the textual sentiment of news on tax reforms. However, a journalist's choice, while itself possibly biased, constraints this influence, which underscores the tension when analyzing outcomes of the interaction of powerful sources of political influence.

Chapter (B) contributes to the literature which enhances our comprehension of the political process by which (tax) regulations are instituted (e.g., see Bischof, Daske, and Sextroh (2020)). Strategic interventions by interest groups are often considered successful, have important economic consequences, and rely on political attention generated by the media. Taxes, furthermore, receive substantial press and front page attention and are at the core of special interests (for a detailed discussion, see Introduction in (B)). Yet, we know little about how seeking influence on (tax) regulation is actually *performed* (for a literature review of the *effects* of influence seeking see Gipper, Lombardi, and Skinner (2013)). In (B), I focus on interest groups' outside tactics and empirically document how their press appearances determine an outcome variable of interest that can impact policy (e.g., see Strömberg (2004) for analytical evidence on how the media biases policy). Analyzing interest groups as a determinant of the textual sentiment of press coverage, thus, directly speaks to how interest seeking in tax reforms is performed. Thereby, my study also responds to the call by Gipper, Lombardi, and Skinner (2013), expanding beyond the conventional focus on comment letters in empirical research on strategic influence.

Furthermore, (B) contributes to a better understanding of the role of the press as an information intermediary in an economic context (Chen, Schuchard, and Stomberg 2019; Rees and Twedt 2022). In particular, my analysis enhances our understanding of how news outlets transpose information on tax reforms to the public. I elucidate the outcome of the press's interaction with other powerful sources of political influence and, thus, essentially capture how information are generated and distributed. Not only does this aspect tie (B) to the information perspectives of this dissertation, it also empirically tests the theoretical predictions by Shapiro (2016) and Sobbrio (2011) on how news outlets report under special interest seeking. The results of (B), thus, speak to an accounting audience but are rooted in political science and economics.

Chapters (A) and (B) not only build upon information perspectives in accounting and taxation but also consider various economic agents. While in (A) the focus is on firms and individuals, (B) considers information intermediaries (i.e., the press) and those with special interests, namely interest groups. This pattern continues through (C), in which firms in business cooperation are analyzed. In particular, Mueller and Weinrich ("we") provide a novel tax perspective on the question "when you work with a superman, will you also fly?" (Tan and Netessine 2019). Focusing on strategic alliances, a highly relevant form of contract-based collaboration between at least two firms (PwC 2018), we elucidate undersheltered "high-tax" firms' changes in tax planning. Specifically, our analyses reveal that high-tax firms increase their tax planning after establishing strategic alliances with tax aggressive "low-tax" firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. In essence, (C) documents the impact of close cooperation and continued exchange in strategic alliances on firms' willingness to engage in tax planning. Our empirical evidence, thus, complements the interview insights by Mulligan and Oats (2016), suggesting that informal private exchange may reduce the expected costs of tax planning. Analyzing changes in tax planning, as a matter outside the scope of an alliance's main business purpose, further highlights the complex tension between knowledge diffusion and protection in alliances (e.g., see Palomeras and Wehrheim (2021)). Taken together, our study reveals tax planning responses to "working with superwoman", offering an unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010).

We argue in (C) that tax planning, conceptually, results from a firm specific equilibrium of expected costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). Its key benefits, lower tax payments, are rather simple to predict, also because specific tax planning tools are mass-market tax advisory products. Low-tax firms, for instance, are particularly good in managing and reducing actual tax costs or expect low potential tax costs. If this tax knowledge diffused to high-tax firms, the assessment of tax costs by high-tax firms could change, too. Observing changes in tax planning in our analysis would then be the consequence of an updated equilibrium of expected costs and benefits of tax planning (see also the Conceptual Framework in (C)). Thus, the motivation for (C), consistent with (A) and (B) while thematically unique, builds upon the information perspectives in this dissertation, elucidating how accounting and taxation shape our comprehension of the economic behavior of various economic agents.

Strategic alliances are expected to foster their main business purposes and to facilitate (intended) transfers of related knowledge between the cooperating firms (K. Li, Qiu, and Wang 2019). In (C) we provide a conceptual framework which suggests that a strategic alliance could also stir the diffusion of tax knowledge as a matter outside the alliance's intended scope. In essence, information exchange, due to trust, and mutual commitment, as a consequence of collaboration, may exceed the initially intended scope. However, the ex ante unintended diffusion of tax knowledge would establish a valuable "private benefit" for the high-tax firm for which the low-tax firm is not compensated, e.g., in form of joint tax planning. Analyzing tax knowledge diffusion in strategic alliances, thus, is distinct and independent from intentional transfers of tax knowledge in peer-to-peer relationships to facilitate joint tax planning (Cen et al. 2017, 2020) and from intentional transfers and acquisitions via intermediaries (e.g., the client-bank-client relationships in Gallemore, Gipper, and Maydew (2019)). Empirically, we not only identify tax knowledge diffusion via strategic alliances but also find that elapsed time facilitates tax knowledge diffusion. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. Furthermore, we find that shared industry affiliation rather inhibits tax knowledge diffusion. Our inferences persist when analyzing shared audit firms and board ties as alternative channels. We also show that tax knowledge diffusion appears ex ante unintended by analyzing abnormal returns to the announcements of strategic alliances and differences between the partners' market shares.

Our primary contribution with (C) is to reveal that tax planning responses to "working with superwoman" (for a detailed discussion, see the Introduction of (C)). Our findings, thus, not only inform research but also offer valuable insights for practitioners and policymakers by elucidating how fostering collaboration through strategic alliances can influence firms' tax planning decisions. Tax *knowledge* diffusion, which we conceptually define as gaining access to and being willing to and capable of employing relevant tax knowledge (see the Conceptual Framework of (C)), thus, holistically speaks to the information perspectives in this dissertation. Specifically, we utilize an institutional feature of strategic alliances, the absence of mechanical tax effects at the firm level upon investment, that allows us to tie observed changes in tax planning to an update of the equilibrium of expected costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). The diffusion of tax knowledge (i.e., a specific type of *information*) explains these results, indicating that undersheltered firms either gain access to tax knowledge and are willing to adjust their tax planning strategies or reevaluate their tax planning strategies. In other words: high-tax firms *receive* and *process* information. Furthermore, we empirically test whether capital markets anticipate tax knowledge diffusion via strategic alliances and find that this is not the case when comparing returns at announcements for treated and pseudo treated firms. Consistent evidence from analyzing differences in the partners' market shares further suggests that tax knowledge diffusion is unintended and not power-induced. Thus, (C) also underscores the importance of considering tax knowledge diffusion as unique and economically important yet unintended effect of a relevant cross-firm connection: cooperation in strategic alliances. Overall, the evidence in (C) contributes to a deeper understanding of knowledge diffusion via strategic alliances, particularly concerning tax knowledge.

In conclusion, it easily becomes evident how chapters (A) - (C) are "all embedded in a simple but powerful framework of sending ("generated, distributed") and receiving ("received, processed") information" as indicated at the beginning of this introduction. There, I also explicitly inserted *comprehension* when describing how building on information perspectives in accounting and taxation allows for inferences about the behavior of economic agents. The applied empirical research methods in chapters (A) - (C) approach the *true* effects of accounting and taxation on economic behavior through inferences that are based on identification strategies. Consequently, I view my dissertation as taking one, although rigorously implemented and executed, further step in striving for empirical *evidence* on the economic role of accounting and taxation. The discussion of the chapters' motivation and contribution, however, highlights how valuable information perspectives are in this attempt. By recognizing that generating, distributing, receiving, and processing information establishes diverse relationships between senders and receivers, my dissertation expands beyond work that respectively considers a single economic agent. After all, behavior of economic agents is interconnected and does not occur in a vacuum.

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Wiped Out? Financial Health of Individuals Affected by Accounting Fraud*

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Wiped Out? Financial Health of Individuals Affected by Accounting Fraud

Abstract

This study examines how intentional financial misrepresentation (i.e., accounting fraud) by a firm affects the financial situation of the individuals who establish the spatial community around which the firm operates. Utilizing granular data from a consumer credit panel covering 10% of the U.S. population with credit histories, we analyze both pre-revelation financial decisions and post-revelation financial distress. Upon revelation, we identify significant increases in indicators of financial distress among individuals who reside in spatial proximity to fraudulent firms' headquarters. On average, we observe incremental increases in debt in collection, affecting approximately one in every one hundred to one in every two hundred individuals. Additionally, we compare individuals' credit demand and supply under fraudulent 'good' information to that under truthful 'bad' information from firms headed for bankruptcy, revealing misinformed financial decisions before the fraud's exposure. Overall, we offer critical insights into the connection between one of the roots of social outcomes, the financial health of a broad set of individuals in a spatial community, and accounting fraud.

- Working Paper -

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1 Introduction

"You can't make the argument that the public was harmed by anything I did [...] there were no victims." (Paul Bilzerian, who was convicted for violating disclosure regulation (Soltes 2019, 173))

Accounting fraud generates important economic consequences (Kedia and Philippon 2009), with individual level analyses focused on specific subsets of those directly tied to the firm, such as managers (Egan, Matvos, and Seru 2019, 2022) or employees (J. H. Choi and Gipper 2024). The core idea of our study, however, is to expand beyond individuals' specific, direct ties to the fraudulent firms to identify the comprehensive financial impact of accounting fraud on all eventually exposed individuals. Therefore, we examine how intentional financial misrepresentation[1] by a firm affects the financial situation of the individuals who establish the spatial community around which the firm operates. Importantly, the financial situation of such a broad set of individuals is one of society's key economic indicators, and it has a direct pathway to important social outcomes (Butler et al. 2023; Sergeyev, Lian, and Gorodnichenko 2023). We assess individuals' financial situation through examining indicators of financial distress and credit demand and show not only that individuals' financial distress increases upon fraud revelation but also that their financial decisions appear misinformed under fraudulent information.

For an economically meaningful case of intentional financial misrepresentation, the financial consequences upon revelation might not sufficiently be captured when analyzing specific subsets of those with direct ties to the fraudulent firm. Concerns about its broader impact arise as fraud eventually affects a wide range of stakeholders and individuals who fall beyond the SEC's shareholder-centric mandate (Velikonja 2012). Furthermore, comprehensive assessments of economic consequences frequently reveal effects that extend beyond the immediate financial impact on certain individuals. For instance, Kleiner, Stoffman, and Yonker (2021) and Gupta (2019) document how contagion and spillovers affect debt and mortgage defaults in spatial communities. Consequently, a key difficulty for our study is to identify and observe a diverse range of individuals who are exposed to a fraudulent firm, regardless of whether they have specific, direct ties to the firm. We address this challenge by analyzing highly granular panel data on indicators of financial distress of a random sample of all individuals who reside in the communities for which fraudulent firms are economically important. Thus, the key innovation

[1] *Intentional financial misrepresentation* is the most accurate description of the subject of analysis in this study. We use *corporate financial misconduct* and *(accounting) fraud* as synonyms throughout the text.

of our study is that we can examine the connection between one of the roots of social outcomes, the financial health of a broad set of individuals in a spatial community, and accounting fraud.

Intentional financial misrepresentation is, furthermore, orthogonal to other forms of corporate misbehavior ex ante to its revelation. A firm's management engages in regulatory violations but continuously disseminates contradictory, fraudulent 'good' information to the public through (mandatory) financial reporting. Such fraudulent yet seemingly valid information has been documented to affect financial decisions of firms (Beatty, Liao, and Yu 2013; V. Li 2016). Therefore, we are interested in individuals' financial decisions during the fraudulent periods, specifically focusing on credit card limits. The fraudulent information could be relevant for individuals' expectations about future economic conditions (e.g., through local news' cheer leading[2] (Gurun and Butler 2012), spending responses to financial information (Gipper et al. 2024), locally biased investment decisions (Seasholes and Zhu 2010), and gatekeepers who failed to identify the fraud). In turn, financial decisions, such as credit demand and granted supply, could diverge from those under truthful 'bad' information, appear misinformed in hindsight, and contribute to the effects upon revelation. Since there is anecdotal but little empirical evidence for individuals' misinformed financial decisions under fraudulent information (e.g., see Ornstein (March/4/2002)), we leverage comprehensive data on individuals' credit card limits not only in the spatial communities eventually affected by the revelation of fraud but also across the entire U.S. to benchmark financial decisions under fraudulent information against those under truthful, adverse information. Taken together, we document the conjunction of accounting fraud's eventual ex ante and ex post revelation effects on the financial situation of individuals.

To address our research question, we use information from a large consumer credit panel covering 10% of the U.S. population with credit histories. The information in this random sample are highly granular and include individuals' trade lines (e.g., limits on credit cards, auto loans, and mortgages), collections (e.g., payment delinquencies and debt sent to collection agencies), and public records (i.e., consumer bankruptcies) on a monthly basis. We track individuals between late 2000 and 2019 (to avoid COVID-19 impacts). Our analyses utilize information at the *person* × *quarter* level and other aggregated levels, focusing on individuals'

[2]News is an important source of information (Bushee et al. 2010) and textual sentiment is shown to affect perceptions of recipients as they transform information into new knowledge (Pentina and Tarafdar 2014; Tan, Ying Wang, and Zhou 2014). We analyze a large corpus of news on fraudulent firms in the Online Supplement (e.g., see Figure OS3) and find that fraudulent firms receive abnormal positive textual sentiment before a fraud is revealed.

financial situation during 'Boom' (pre-revelation) and 'Bust' (post-revelation) periods. We identify intentional financial misrepresentations from SEC Accounting and Auditing Enforcement Releases (AAERs). We separate errors from intent among the AAERs by applying natural language processing tools and gather information on additional formal events (delistings and corporate bankruptcies) to analyze large negative abnormal returns for the identification of fraud-revelation dates. Given that we are interested in a case's potential impact on a broad set of stakeholders, we turn to the financial situation of *all* individuals who reside in spatial proximity to fraudulent firms' headquarters.[3] Finally, we require that firms are economically important for these spatial communities.

For our main analysis, we leverage revelations of fraud cases (i.e., staggered "treatments") to implement a treated vs. not yet treated design at the $person \times quarter$ level (>120 million observations). We estimate canonical difference in differences (DiD) regressions which benchmark *post* and relative *months to treatment* indicators against single pre-event baselines. Our main outcome variable of interest measures whether a person's debt has been sent into collection. Utilizing debt in collection as an indicator of an individual's financial distress has the advantage that it excludes less severe payment delinquencies but is not as severe and rare as consumer bankruptcy. Both visual and empirical evidence consistently indicate that individuals' financial distress increases significantly upon the revelation of intentional financial misrepresentation. On average, we observe incremental increases in debt in collection, affecting approximately one in every one hundred to one in every two hundred individuals (upper- and lower-bound estimates). These estimates render our findings economically meaningful but are neither surprisingly large nor negligibly small.

We corroborate this finding by analyzing alternative measures of financial distress at the $person \times quarter$ level. We show that credit card delinquencies also increase. A credit card delinquency is a softer indicator of financial distress because it covers not only collections but also 30+, 60+, and 90+ days of payment delinquencies on outstanding balances. Consumer bankruptcies, instead, represent one of the most severe indicators of financial distress because individuals default on their debt under Chapter 7 (full) or Chapter 13 (partial). Although bankruptcy filings are relatively rare compared to experiencing debt being sent into collection

[3] Spatial proximity to financial misconduct is commonly considered to increase exposure (Parsons, Sulaeman, and Titman 2018; Giannetti and Wang 2016; Glaeser, Sacerdote, and Scheinkman 1996; Carnes, Christensen, and Madsen 2023); see also Section 2. Our analyses primarily consider the county (CT) as spatial community of interest.

(with averages of ~5% and ~30% across our sample population of individuals), we still observe a significant and economically meaningful increase in consumer bankruptcies following the revelation of fraud within a spatial community.

Next, we utilize consumer credit panel data that are collapsed at the ZIP code ($ZIP \times$ *quarter*) level. This aggregation allows us to apply alternative econometric estimators. We start by estimating an aggregated average treatment effect on the treated (ATT) by employing the methodology proposed by Callaway and Sant'Anna (2021). We are able to replicate our main finding under this approach, which is robust to econometric challenges from staggered treatment designs. Additionally, the aggregated data allow us to consider not only the counties of interest (for a treated vs. not yet treated design) but also the entire U.S. Consequently, we can estimate an ATT that utilizes never-treated communities as control observations. Considering these alternative counterfactuals enhances the credibility of our analyses because it allows us to assess whether our results apply more widely beyond the specific context of the (not yet) treated observations. We continue to identify the effect from our main analysis under this approach.

We then transition from staggered DiD models to a stacked DiD design at the $ZIP \times$ *quarter* level. Importantly, a stacked panel that covers the entire U.S. allows us to include granular *region* $\times$ *quarter* fixed effects (we implement these in different specifications at the state and the commuting zone (CZ) levels) to control for local economic trends (see Povel, Singh, and Winton (2007) and Beneish et al. (2023) on the association between financial misreporting likelihood and the economy). If intentional financial misrepresentation and its revelation were a product of a broader local economic downturn, our estimates for individuals' financial distress could reflect this trend rather than being impacted by accounting fraud. In conjunction with the *date* fixed effects, which capture trends for the entire population of the U.S., the inclusion of the *region* $\times$ *quarter* fixed effects allows us to capture these local economic trends. Commuting zones (~700 in the U.S.) are of particular interest in this regard because they establish clusters of counties that are characterized by strong within-cluster commuting ties and cover the entire landmass of the U.S. (see Keys, Mahoney, and Yang (2023) for an application of commuting zones in research on financial distress). We find that our results persist when controlling for these granular regional economic trends, reducing concerns about reflection confounding our insights.

In an additional analysis, we investigate the spatial dispersion of treatment effects by assigning treatment statuses at different spatial regions. We expand treatment status for $ZIP \times quarter$ observations from the county level (~3.9K in the U.S.), over commuting zones (~700 in the U.S.), to the state level. We find that the effect is substantially attenuated when considering state level treatment assignment (ca. one-tenth of the baseline effect). If fraud reflected deteriorating state economic conditions, we would have expected to find a stronger effect at this level of treatment assignment. Our results, consistent with the results from the stacked DiD analysis, mitigate this concern. We find stronger effects under treatment assignment at commuting zones. The estimate, though, still fall shorts of the estimate for treatment assignment at the county level. These findings suggest that the average treatment effect fades with increasing spatial distance to the fraudulent firm, underscoring the role of spatial proximity in accounting fraud exposure.

Our focus on the financial situation of all individuals who reside in spatial proximity to the fraudulent firms entails a trade-off: identification of effects that are driven by contagion and spillovers may preclude the identification of specific mechanisms operating at the individual level, such as employment. We are, however, interested in whether our results would primarily be an accumulation of the effects on those with direct ties to the fraudulent firms. Therefore, we employ information on Worker Adjustment and Retraining Notifications (WARNs) in cross sectional analyses. WARNs, which are provided by Stanford University's Big Local News initiative, are mandatory public announcements of large, scheduled layoff events. We find that increases in financial distress are consistent both within and outside the WARN cross section, suggesting that the consequences of intentional financial misrepresentation extend beyond cases in which effects originate primarily from those with direct ties to the fraudulent firm (i.e., employees). Consistently, we find a substantially amplified effect when we consider fraudulent firms from the finance industry, which likely affect many, including those with rather indirect ties to the firm. The SEC, for instance, described one case in the finance industry as having a "devastating regional impact" due to which "several regional banks failed"[4], underscoring the multitude of those potentially affected. Taken together, these results are consistent with treating the identified effects from our analyses to reflect the economic consequences on a broad set of individuals in a spatial region.

[4]SEC (May/04/2011) Brooke Capital; see also the Ponzi example in Section 2.

We then turn to the analysis of the financial situation of individuals ex ante to a fraud's revelation. We are interested in whether individuals' financial decisions appear misinformed in hindsight (for a firm-level analysis, see Beatty, Liao, and Yu (2013)). For identification, we compile a dataset of corporate bankruptcies in the U.S. Economic effects from corporate bankruptcies are documented to be deteriorating not only for the affected firms but also for a broad set of stakeholders (e.g., see Benmelech et al. (2019), Chava, Malakar, and Singh (2022), and John Graham et al. (2023)). We conclude that the ex post local economic effects from intentional financial misrepresentation and corporate bankruptcies share similarities. Corporate bankruptcies, however, are seldom surprising events. Eckbo, Thorburn, and Wang (2016) and Hertzel et al. (2008) document negative economic signals and effects already ex ante to the bankruptcy filing. In contrast, intentional financial misrepresentation is characterized by the concealment of a firm's true economic state. These differences in the ex ante revelation/filing periods allow us to analyze whether individuals' financial decisions diverge under fraudulent 'good' information from decisions under truthful 'bad' information (a behavior we theoretically refer to as "rational overconfidence"). We observe credit card limits of current credit cards, which place an upper bound on consumer purchases and borrowing (see Aydin (2022), T. Gross, Notowidigdo, and Wang (2020), D. B. Gross and Souleles (2002), and Agarwal et al. (2015)), as the outcome variable of interest. Our results indicate that credit card limits differ statistically and economically during the three years preceding the revelation/filing. In particular, we observe that individuals who live in communities that will be affected by the revelation of intentional financial misrepresentation expand their credit card limits. In contrast, the credit card limits of individuals in communities that will face a corporate bankruptcy filing by a locally economically important firm are substantially lower. We conclude that it appears that individuals are indeed misinformed in their financial decisions under fraudulent information. Together with our cross sectional evidence on the ex post effects, these insights also underscore that the financial impact of accounting fraud expands beyond the eventual effects from general company distress.

Finally, we analyze how post-revelation financial distress is impacted by pre-revelation financial decisions. In particular, we measure compound growth rates of credit card limits ex ante to the fraud revelation and include this measure in specifications of the main analysis for the ex post estimation (at the *person* $\times$ *quarter* level). Our results suggest that pre-revelation financial decisions incrementally aggravate individuals' financial distress upon fraud revelation.

However, we also find that expanding credit card limits during fraudulent periods initially creates a liquidity cushion upon fraud revelation. Taken together, we cautiously conclude that financial decisions ex ante to the revelation of accounting fraud not only appear misinformed but also become harmful to an individuals' financial distress upon fraud revelation.

Our study contributes to the research on financial misconduct by providing a better understanding of its broader economic impact. While previous work places strong emphasis on predictors of financial fraud (e.g., see Khanna, Kim, and Lu (2015) and the seminal work by Becker (1968)), research on its consequences often focuses on firm-level outcomes (e.g., see Karpoff, Scott Lee, and Martin (2008) or Heese and Pérez–Cavazos (2019)) and stock-market reactions which are interpreted as reputational costs (starting with Karpoff and Lott (1993)). These and individual level insights, however, typically address the consequences of financial misconduct to the extent that specific groups of share- and stakeholders directly experience bad realizations (e.g., see Egan, Matvos, and Seru (2019) and Egan, Matvos, and Seru (2022) for management implications).[5] Documented employment effects by J. H. Choi and Gipper (2024), and analyses by Parsons, Sulaeman, and Titman (2018) and Holzman, Miller, and Williams (2021) on criminal offenses around the revelation of financial misconduct address fraud's comprehensive economic and social consequences more directly. Consistently, Giannetti and Wang (2016) show that *all* households in a state reduce equity holdings upon the revelation of financial misconduct.

We add to these insights by providing an analysis that identifies and quantifies the impact of intentional financial misrepresentation on one of society's key economic indicators: the financial health of individuals. Financial health has a direct pathway to important social outcomes. For instance, financial distress is positively associated with divorce probability (Butler et al. 2023) and has been documented to raise psychological costs (Sergeyev, Lian, and Gorodnichenko 2023). We present robust evidence linking intentional financial misrepresentation with the financial health of individuals residing in the spatial communities for which fraudulent firms are economically important. Notably, our analyses extend beyond individuals with direct ties to these firms, revealing that a broad population is, on average, affected by such misrepresentation. Consequently, our findings also speak to the roots of social costs of accounting fraud. Furthermore, we analyze individuals' financial decisions when the fraud has not yet been revealed. This

[5] See also Table OS1 for an overview on research on (intentional) financial misrepresentation.

analysis not only addresses the eventual effects upon the revelation of fraud but also connects our study to the discussion on financial consumer prudence, where it remains a matter of debate whether precautionary saving is an (un-)important part of consumer behavior (e.g., see Dynan (1993) and Aydin (2022)). We contribute to this fundamentally important question from a different perspective and show that individuals' financial decisions appear misinformed under fraudulent information. Our findings, thus, underscore the critical role of reliable and verifiable information in financial decisions. Overall, we provide empirical evidence that is consistent with calls for a broad consideration of potentially affected share- and stakeholders when enforcing accounting regulations (Velikonja 2012).

2 Theoretical Framework

2.1 Revelation & Financial Distress

Consequences of economically meaningful events often extend beyond the immediate impact on certain individuals. They are also driven by contagion throughout a community. For instance, Kleiner, Stoffman, and Yonker (2021) and Gupta (2019) show that contagion and spillovers exist for mortgage defaults in local communities. On a more positive note, Bailey et al. (2018) suggest that social interactions drive housing market expectations, and Kalda (2020) shows that individuals are less likely to default during the period following peer distress caused by health shocks. These insights suggest that events with potentially broad economic consequences are inherently different from single bad realizations that a subset of individuals might face. Contagion appears as an amplifier in these scenarios.[6] For an economically meaningful case of intentional financial misrepresentation it follows that the immediate financial impact on specific subsets of individuals might not sufficiently describe the fraud's comprehensive consequences. Giannetti and Wang (2016) show, for instance, that not only those invested in the firm but all households in a state reduce equity holdings upon the revelation of financial misconduct. A broad set of stakeholders (such as household members, suppliers, local dependent commerce, and service contractors) is likely affected by the fraud (see below) and, through spillovers and contagion, they affect each other. Thus, intentional financial misrepresentation could entail negative externalities for all eventually (spatially) exposed individuals, which could translate to increased risk of financial distress among them. This broad set of potentially affected, though, would face difficulties in

[6] Evidence for contagion at the firm level is documented by Gleason, Jenkins, and Johnson (2008), Chaney and Philipich (2002), and Kedia, Koh, and Rajgopal (2015)

recouping losses because the SEC is particularly mandated to focus on shareholder interests. While this practice has been conceptually criticized by Velikonja (2012) to have "missed the significantly larger social welfare losses by securities fraud that fall outside financial markets", it is important to note that the Fraud Section of the Department of Justice (DOJ) has authority to pursue criminal actions against accounting fraud.[7] Analyzing the financial situation of a broad set of individuals, thus, not only addresses one of society's key economic indicators but also carries important implications for regulators and their agencies when implementing and enforcing regulations against accounting fraud.[8]

Defining spatial communities as communities potentially affected by firms' financial misbehavior assumes that spatial proximity increases exposure. Certainly, awareness of (e.g., through local media) and transactions with (e.g., through household employment) the respective firm are locally amplified. Spatial proximity is, therefore, commonly employed as a proxy for increasing exposure (Parsons, Sulaeman, and Titman 2018; Giannetti and Wang 2016; Glaeser, Sacerdote, and Scheinkman 1996; Carnes, Christensen, and Madsen 2023). There is also empirical evidence that individuals are particularly impacted by local firms (e.g., see Seasholes and Zhu (2010) on local stock trading, and Carnes, Christensen, and Madsen (2023) on job choices). Chava, Malakar, and Singh (2022) conclude that "local communities are a major stakeholder in public firms" and refer to firms' headquarters locations (at the county level) as communities of interest. We follow this notion and focus on spatial exposure to fraud committed by firms headquartered in the county in which individuals reside. Importantly, we broadly define the set of transactions through which a person may (financially) be impacted by a local fraudulent firm. We do not limit our analyses to specific sets of potentially affected share- or stakeholder groups but are particularly interested in how fraud comprehensively affects the average financial situation of a broad set of individuals. An anecdotal example for these indirectly, tangentially but meaningfully impacted individuals can include young adults, whose parents lost their college funds from (locally biased/underdiversified) investments in fraudulent firms (Edwards Feb/27/2000).[9]

[7] See DOJ (2012), p. b69, Tsao, Kahn, and Soltes (2023), Ch. 17 III, and § 9-47.110 of the DOJ's Justice Manual.

[8] We draw on existing research that is not related to accounting fraud's economic consequences to derive at theoretical expectations. Thus, we also complement and contribute to the literature on contagion and spillovers among individuals with the implications of our study on policy makers.

[9] Even the "original Ponzi scheme" by Charles Ponzi in the 1920s provides an example of these indirect effects. The freezing of Ponzi's remaining assets by prosecutors led to a run on smaller trust companies, of which several failed. Depositors, who did not invest any USD in the Ponzi scam, lost millions as a result (Wall Street Journal Aug/22/1922; León and Webber July/20/2023).

2.2 Rational Overconfidence

[Figure 1 about here.]

Ex ante to the revelation of intentional financial misrepresentation, other economic actors could be misguided by firms' fraudulent information. At the firm level, this effect is documented by Beatty, Liao, and Yu (2013) and V. Li (2016). At the individual person level, there is anecdotal but little empirical evidence for such misinformed decisions. One investor recalled that "beyond my stupidity, the main reason [for investing additional funds in Enron's stock] was that a slew of analysts was nearly shouting "buy, buy, buy!" at the time [...]" (Ornstein March/4/2002, content in square brackets added). Furthermore, evidence by Gipper et al. (2024) suggests that individuals, including those without specific ties to firms, adjust their spending in response to financial information of locally economically important firms. At a theoretical level, we describe misinformed financial decisions under fraudulent information as "rational overconfidence" by individuals. Figure 1 depicts this framework. In fraudulent periods, a firm reports a signal γ that mimics a truthful & good signal δ. However, if the firm truthfully reported, it would report the bad signal ε. Typically, an overconfident individual is characterized as someone who, when facing ε, would ignore the signal to the extent that she is overly optimistic about future realizations (e.g., see Skala (2008) for a nuanced definition of overconfidence in psychology and finance). Under γ, however, an individual faces a seemingly valid and good but actually fraudulent and bad signal. The perceived validity of this signal could further be reinforced because a firm's governance structure or gatekeepers, such as analysts, auditors, and regulatory agencies, failed to identify the fraud. Additionally, local news are also found to hype businesses as quid pro quo for advertising expenditures (Gurun and Butler 2012).[10] Consequently, individuals' financial decisions could be influenced under γ through expectations about future economic conditions (such as expected but unrealized income and wealth) and map into (in comparison to the state where the truthful signal was known) misinformed financial decisions. An individual under γ might thus effectively act as an overconfident individual under ε but is rational in doing so.

[10]See Footnote[2] and the analysis to Figure OS3 in the Online Supplement.

3 Institutional Details

U.S. federal regulation penalizes falsification of accounting records. Title 17 CFR and Title 15 U.S.C. codify the Securities and Exchange Act of 1934 ("SEC Act") and its relevant amendments to the accounting provisions by the Foreign Corrupt Practices Act of 1977 ("FCPA"):

> *"**Every issuer** which has a class of securities registered pursuant to section 78l of this title and every issuer which is required to file reports pursuant to section 78o(d) of this title shall [...] **make and keep books, records, and accounts, which, in reasonable detail, accurately and fairly reflect the transactions and dispositions of the assets of the issuer**"*[11]

The FCPA's accounting provisions, while intended to reflect transactions in conformity with accepted methods of recording economic events and thereby effectively preventing "off-the-books slush funds and payment of bribes" (House of Representatives 1977), do not mention bribery and do not require illicit payments to violate the statute. "As a result, the statute has been used by the SEC and DOJ to police all varieties of accounting fraud" (Tsao, Kahn, and Soltes 2023, Ch. 17 III).

Maintaining accurate records is an issuer's mandate and requires (publicly traded) companies subject to the jurisdiction of the SEC[12] to comply with the regulations of the SEC Act and the FCPA. The act of record-falsification, however, may be carried out by a natural person: "No person shall [...] knowingly falsify any book, record, or account"[13], whereas "books, records, and accounts" are broadly interpreted by the SEC and DOJ. A criminal violation requires the person to act "knowingly" and "willfully" and may be penalized with a USD 5m fine or imprisonment of up to 20 years.[14] Civil violations of the accounting provisions also allow the SEC to enforce against financial misrepresentation in the absence of proof of intent (Tsao, Kahn, and Soltes 2023).

[11] 15 U.S.C. § 78m(b)(2)(A), emphasis added.
[12] 15 U.S.C. § 78l and § 78o(d) contain a detailed definition of the term 'issuer'.
[13] 15 U.S.C. § 78m(5) and 17 CFR § 240.13b2-1.
[14] 15 U.S.C. § 78ff(a).

4 Data

[Table 1 about here.]

4.1 Intentional Financial Misrepresentation

SEC Accounting & Auditing Enforcement Releases

We utilize SEC Accounting & Auditing Enforcement Releases (AAERs) to identify cases of intentional financial misrepresentation.[15] Case-aggregated AAER data are provided and updated by Dechow et al. (2011) for the period between late 1999 and 2018 ("CFRM" data).[16] CFRM aggregates and provides data at the firm-event level because multiple AAERs may be allocated to one firm-specific event. For instance, Enron's accounting scandal (one case in CFRM) is associated with 49 distinct AAERs in CFRM. We fuzzy-match firms to Compustat's gvkey based on firm names (utilizing historical Compustat snapshots and WRDS linking tables).[17]

One assumption of our analyses is that individual i faces greater exposure to fraud committed by firms headquartered in the region in which she resides. Related research by Hong, Kubik, and Stein (2008) ("only game in town effect") and Chava, Malakar, and Singh (2022) ("most of the effect comes from HQ locations") supports this notion (see also Section 2.1). Therefore, we subsequently focus on publicly traded firms that are incorporated and headquartered in the U.S. and utilize firms' headquarters-counties (CT) as spatial communities of interest. We apply several sample selection steps which are summarized in Table 1 and described in greater detail in the following. In particular, we are interested in *intentional* financial misrepresentations, in *revelation* dates, and in firms' *local economic importance*.

Identifying Intent

Karpoff et al. (2017) summarize that intentional financial misrepresentations and errors are commonly separated for analyses. According to Hennes, Leone, and Miller (2008), errors and intent ("irregularities") are perceived differently by investors, regulators, boards, and other stakeholders, emphasizing "the importance of distinguishing errors from irregularities". Consistently, we

[15] Note that research on financial misconduct utilizes multiple sources to construct samples for investigation. Therefore, we provide an overview of data sources that have been utilized in research in Table OS1. AAERs appear as the most prevalent sources.

[16] AAERs were issued well before 1999 but are electronically not available at the SEC's website pre mid-1999. We utilize the AAERs' content in our analyses and therefore rely on their electronic availability.

[17] Recently, CFRM issued an updated version of the data which includes the gvkey identifier. We cross-validate CFRM's mapping of gvkeys to firms with our matching results and resolve few conflicts manually.

follow prior literature and separate intentional financial misrepresentation from errors. We do so because enforcing against errors does not require proving materiality, allowing enforcement actions to include economically less meaningful or even irrelevant cases (Tsao, Kahn, and Soltes 2023). More importantly, the Fraud Section of the DOJ has the authority to pursue criminal actions against financial misrepresentations, requiring proof of intent (see Section 3). *Intentional* financial misrepresentation's financial impact on a broad set of individuals is, thus, of particular interest, given that the DOJ, unlike the SEC, is not primarily concerned with shareholder interests.

[Figure 2 about here.]

We identify intent among the AAERs in our sample by employing simple natural language processing (NLP) tools. We start with the replication data by Call et al. (2018) ("CMSW")[18] to gather information on cases that are classified as fraudulent. The data, which basically overlap with Karpoff et al. (2017) and contain AAERs, include the variable $fraud$ which flags cases as intentional (in congruence with the institutional details presented in Section 3). We subset the data to observations for which $fraud$ equals one and then fuzzily match the data to our AAER/CFRM sample (based on firm names and event times). Successfully matched observations create a set of "true" fraud cases among our AAERs. These observations serve as a "training" set for classification of the remaining "test" AAERs in our sample (our and CMSW's sample periods are not identical, which makes the classification of our remaining AAERs necessary).

We then classify intent based on the content of the respective AAERs. In preparation, we scrape AAERs from the SEC's website and generate a corpus of all primary AAERs (the "primary" AAER per case is flagged by CFRM). We lemmatize the corpus, which allows us to subset the corpus to action words (verbs, adjectives, and adverbs) by means of POS tagging. Next, we remove the most common English vocabulary from the corpus and generate simple bi- and three-grams from the remaining action words in the training/fraud subset of the AAERs. Figure 2 depicts the most common tokens. One can easily observe that terminology such as "materially overstate", "fail [to] disclose", or "engage [in] fraudulent" dominate the set of action words, which suggests that this set of AAERs indeed represents *intentional* financial

[18] A replication package for this study is available at the online repository of the Journal of Accounting Research (JAR). We thank Jerry Martin and Jonathan Karpoff for additional guidance on how to interpret the data.

misrepresentations. Furthermore, we note that the content of these AAERs addresses financial misrepresentations because the word cloud precisely depicts the wording of the accounting provisions in 15 U.S.C. § 78m(b)(2)(A) "accurately [and] fairly reflect". We build regular expressions on selected tokens and count distinct hits of the expressions per AAER when performing pattern matching across all AAERs. We require a minimum number of hits in the test AAERs (p10) to classify them as intentional. Test AAERs that do not surpass this threshold are classified as unintentional and subsequently excluded from our analysis.[19]

Fraud Revelation

[Figure 3 about here.]

Next, we turn to the question of when fraud is publicly revealed. While AAERs contain release-dates, their publication can substantially lag a case's actual revelation-date. Dyck, Morse, and Zingales (2010), for instance, show how important outsiders such as the press are in revealing corporate misbehavior. One prominent example of intentional financial misrepresentation is the Adelphia Communications case, in which the owner family heavily channeled money for private purposes out of the company by understating its liabilities. After substantial disagreement with its audit firm Deloitte on how and whether to record these liabilities in the annual report, the analyst Oren Cohen, in an earnings conference call on March/27 2002, noted "That's an awful lot of debt" (Lowenstein Feb/1/2004). This mundane insight sent Adelphia's stock price into immediate and substantial turmoil and Deloitte ultimately refused to proceed with the audit of Adelphia's 10K. By the end of May 2002, Adelphias' stock was delisted. Figure 3 depicts how Adelphia's stock price crashed until its stock was delisted. The large negative jump on March/27 marks the fraud's revelation date (Panel A). The lower panel of the figure depicts daily abnormal returns in the long run until the company's delisting and it can be observed how extreme its turmoil became once the fraud was revealed. The primary (first) AAER for this case, however, was not filed until April/26/2005 (July/24/2002).

[Figure 4 about here.]

Motivated by the observations from the Adelphia case, we analyze large negative abnormal returns to identify revelation dates. We observe AAERs, delistings (from CRSP), and

[19] We provide additional guidance on the classification in the Online Supplement.

corporate bankruptcies (from Audit Analytics and BRD[20]) as formal events, which provide an initial approximation of the revelation dates. We then search for a firm's largest negative abnormal return in the weeks preceding the earliest formal event and mark the respective date in this period as the revelation date (see "CRSP revelation date" in Figure 4). For instance, this approach precisely classifies Adelphia's revelation date at March/27 2002.[21] Since we observe persons in the consumer credit panel at the end of every quarter, we consider the ceiling quarter-end date of this date as the revelation and treatment date ($t = 0$) for our analyses.

Local Economic Importance

Finally, we focus on fraudulent firms that are locally economically important because we are interested in whether intentional financial misrepresentation affects a broad set of individuals in a local community.[22] We remove extremely small firms by requiring that a firm shows at least USD 100m (in 2010 USD) in total assets on its balance sheet at some point during the sample period. Correspondingly, we remove firms that are in the top 1 percentile of this distribution. We also exclude firms that are headquartered in heavily populated (top 1 percentile) counties (see Table 1). We require that firms reach a minimum spatial market share among all publicly traded companies. We calculate the concentration measure HHI for all firms in the Compustat-CRSP merged universe (CCM; based on global sales) but do this over years and counties, not years and industries. We then require that a firm's average spatial market share (i.e., its share in the concentration measure) exceeds the bottom quintile to remain in our sample. These sample selection steps leave us with 104 cases of intentional financial misrepresentation. We merge these observations at the county-date level to the consumer credit panel data.

4.2 TransUnion Consumer Credit Panel

[Table 2 about here.]

We employ novel data on individuals' financial situation from an anonymized consumer

[20] BRD abbreviates the "Florida-UCLA-LoPucki Bankruptcy Research Database". We ensure that delistings and bankruptcies are fraud-connected by considering only those that occur in a time-congruent window around the SEC's enforcement actions.

[21] For a robustness check, we search through online archives of news outlets (e.g., WSJ, NYT, NexisUni) to collect dates at which the media reported on (rumors about) cases. We do this for all cases in our final sample and find that the median difference between our approach and the manual collection is only 7 days. A major advantage of our approach is that it can be scaled to samples of AAERs without handcollecting data. We run a regression analysis that utilizes the dates from the news searches as alternative revelation dates and find consistent results with our main analysis.

[22] Naturally, this restriction trades off the internal and external validity of our analyses. Note, however, that V. Li (2016) shows that small sample fraud-analyses may generalize to larger populations.

credit panel that covers 10% of the U.S. population with credit histories.[23] The information in this random sample are highly granular and cover individuals' trade lines (e.g., limits on credit cards, auto loans, and mortgages), collections (e.g., payment delinquencies and debt sent to collection agencies), and public records (i.e., consumer bankruptcies) on a monthly basis. We are able to observe individuals since late 2000 and decide to limit the sample period up until 2019 to avoid any impact of COVID-19.[24] For our main analysis, we utilize information at the $person \times quarter$ level and other aggregated levels and analyze individuals' financial situation during fraudulent periods ("Boom") and upon fraud-revelation ("Bust").

Panels A and B of Table 2 contain summary statistics of the variables of interest in our sample. Data at the $person \times quarter$ level (Panel A) are restricted to the eventually treated communities (CT) so that *treated* equals one throughout this sample. While raw data are available monthly, we collect quarterly information. We do not aggregate information over the monthly observations within a quarter but collect information at the ends of March, June, September, and December of a calendar year. In numerous analyses, we also utilize data that are aggregated at the $ZIP \times quarter$ level. In particular, this aggregation allows us to analyze never treated communities, i.e. the entire U.S. (Panel B). The count of all observations at a given date in a ZIP is summarized in *numobs*, which we use as weights in some regressions. We construct slightly different datasets for the respective analyses, which explains differences in the number of observations in the following. For instance, when employing the stacked DiD methodology, double counting of control observations substantially increases the sample size. Therefore, we explain the variables of interest and their construction in Section 5 (see also Table A1 for variable definitions).

5 Identification Strategy & Results

5.1 Bust: Financial Distress Upon Staggered Revelation of Fraud

For our main analysis, we utilize consumer credit data at the $person \times quarter$ level and employ a staggered identification strategy among counties in which fraud has been revealed and counties in which fraud has not yet been revealed but will be revealed at some point in time during the

[23] Data items are calculated (or derived) based on data provided by TransUnion through a relationship with the Kilts Center for Marketing at the University of Chicago Booth School of Business.

[24] Observations at the $person \times quarter$ level start in 2001, observations at the $ZIP \times quarter$ level start in 9/2000.

sample period. We estimate the following regression using OLS:

$$\begin{aligned} \textit{Financial Distress}_{i,t} = \alpha + \beta_1 \textit{ Accounting Fraud Revealed}_{CT,t} \\ + FEs + \varepsilon_{i,t} \end{aligned} \tag{1}$$

Accounting Fraud Revealed$_{CT,t}$ captures the effect of intentional financial misrepresentation on an individual's i measure of financial distress (*Financial Distress*$_{i,t}$). We estimate canonical difference in differences (DiD) regressions which benchmark *post* or relative *months to treatment* (*bin*) indicators at the $CT \times date$ level (i.e., the level of treatment assignment) against single pre-treatment baselines. We include all pre-treatment observations but limit the sample to 24 months of post-treatment.[25] We include varying sets of fixed effects (FEs) at the *person*, CT, and *date* (*date* equals the end of each *quarter*) levels in multiple specifications of equation (1). These fixed effects control for a wide range of potentially omitted variables. We cluster robust standard errors at the *person* (sampling unit) or CT (treatment assignment) level (Abadie et al. 2022). For counties with multiple treatments/events during the sample period, we retain the first event only to ensure that confounding treatments do not affect pre-treatment observations of subsequent events. Our measures of *Financial Distress*, which we describe subsequently, tightly follow those utilized by Keys, Mahoney, and Yang (2023) because we utilize identical underlying raw consumer credit data.

Debt in Collection

Our main outcome variable of interest is Debt in Collection (*unpdcol*) which is an indicator variable that equals 100 when an individual is flagged as having at least one debt sent to collection (third-party collectors report Debt in Collection to TransUnion). Collections provide an interesting angle to measure financial distress because this proxy has the advantage that it excludes less severe payment delinquencies but is not as severe and rare as consumer bankruptcy. However, it is substantially more likely for bankruptcy filers to have debt in collection ex ante filing (Dobbie, Goldsmith-Pinkham, and Yang 2017).

[Figure 5 about here.]

Before we turn to the regression analyses, we perform a simple visual inspection of the

[25] The Online Supplement depicts results from estimating an event study with symmetrical pre- and post-periods in Table OS2.

raw data. We plot *unpdcol* of the treated individuals against the relative months at which the fraud is revealed (treatment at $t = 0$). Pre- and post-averages (depicted for the -15 to -3 and 0 to +24 months periods) are marked with horizontal dashed lines. The dots mark the averages for the relative periods and the vertical lines mark the confidence intervals. One can observe that the pre-treatment point in time averages float closely around the multi period average. Upon the revelation of the fraud, however, we observe an economically meaningful increase. This increase does not "jump" at the date of treatment but accumulates over time. This observation fits to the institutional design of *unpdcol*. Before debt is sent to collection, it is typically marked as delinquent first. This provides debtors some leniency in coming up with the amount they own. If the debtors still fall short of their obligation, debt is sent to collection at approximately 180 days later (Keys, Mahoney, and Yang 2023).

[Table 3 about here.]

Next, we estimate multiple specifications of equation (1) with *unpdcol* as the dependent variable. The regressions allow us to control for fixed effects and standard error adjustments to corroborate the visual evidence from Figure 5. Table 3 depicts the results. In columns (1) and (2) we regress *unpdcol* on the indicator variable *post*, which switches from zero to one upon fraud revelation in the communities of interest. It captures the average effect the treated individuals experience from the treatment. We show that including CT fixed effects and clustering standard errors at the $person$ level (column (1)) and alternatively including $person$ fixed effects and clustering standard errors at the CT level (column (2)) both lead to economically meaningful and statistically significant estimates.[26] We repeat these analyses in columns (3) and (4) in which we regress *unpdcol* on the factor variable $months\ to\ treatment\ bin$ which captures the months (0 to 24) since fraud revelation (*bin* indicates that this measure is estimated against a single pre-treatment baseline). Our findings mirror the documented pattern in Figure 5. On average, incremental increases in debt in collection in the range between one in one hundred and one in two hundred individuals (upper and lower bound estimates in Table 3) render our estimates economically meaningful but are neither surprisingly large nor negligibly small since the sample average of *unpdcol* is ~30%. We interpret these findings as first evidence of increased

[26] In an untabulated test, we include $date$, $person$, and CT fixed effects in the regression (since there are movers between the communities in the sample; e.g., see Y. Li, Lin, and Zhang (2018) on "firm-movers" and fixed effects). We continue to find a meaningful and significant effect (β_1 of 0.5064 for $post$ with a p-value of 0.0498 when we cluster standard errors at the CT level). We find consistent evidence with $months\ to\ treatment\ bin$ as the independent variable of interest.

financial distress through intentional financial misrepresentation.

Credit Card Delinquencies

[Figure 6 about here.]

[Table 4 about here.]

We corroborate our main analysis by employing alternative measures of financial distress at the *person* $\times$ *quarter* level. We, therefore, turn to credit card delinquencies (*ccdq* and *ccdq cond*). A credit card delinquency is a softer indicator of financial distress because it not only covers collections but includes 30+, 60+, and 90+ days of payment delinquencies on outstanding balances. The trade line information in the consumer credit panel allow us to measure whether an individual owns credit card(s) and whether she is delinquent on at least one of them. The indicator variable *ccdq* captures credit card delinquencies by equaling 100 in these instances (*ccdq cond* is constructed identically but is conditional on owning a credit card). We utilize *ccdq* and *ccdq cond* as dependent variables and repeat the analyses from Table 3 by estimating four specifications of equation (1) in which we include *date* and *person* fixed effects and cluster standard errors at the *CT* level. The results, which are presented in Table 4, match not only those in Table 3 but also the visual trends in Figure 6. We observe meaningful and statistically significant increases which accumulate over time. We interpret these findings as additional evidence of increased financial distress through intentional financial misrepresentation.

Consumer Bankruptcies

[Figure 7 about here.]

[Table 5 about here.]

Next, we focus on consumer bankruptcies, which posit one of the most severe indicators of financial distress. When filing for bankruptcy, individuals default on their debt under Chapter 7 (full) or Chapter 13 (partial). We determine whether an individual's public records data indicate a bankruptcy filing. We construct the indicator variable *bkrt* and use it as the dependent variable when estimating specifications of equation (1). We cluster standard errors at the *person* level and include *date* and *CT* fixed effects in these analyses because Keys, Mahoney, and Yang (2023) highlight that " [...] place-based factors determine whether you use bankruptcy to get out [of financial distress]" (p. 46, content in square brackets added). Figure 7 depicts how raw values - consistent with our previous findings - increase upon fraud revelation. Table 5 depicts the regression results. Although filing for bankruptcy is a rarer event than having debt sent into collection (averages of ~5% and ~30% across the population of individuals in our sample), we still find an economically meaningful increase in consumer bankruptcies upon fraud revelation in a community. Overall, our findings at the *person* $\times$ *quarter* level indicate that upon revelation of intentional financial misrepresentation, the financial distress indicators of individuals who establish the local community around the fraudulent firm increase.

ZIP Aggregated Data

ATT at ZIP Level

[Table 6 about here.]

In an additional set of tests, we collapse the raw TU consumer credit data at the $ZIP \times$ *quarter* level. Data items are constructed identically to the data at the *person*$\times$*quarter* level but are averaged across a ZIP and cover the entire U.S. While this aggregation drastically reduces within ZIP variation of *unpdcol* at any given point in time, it allows us not only to consider never treated communities as control observations but also to apply alternative estimation techniques. We start with an application of the estimation suggested by Callaway and Sant'Anna (2021) for difference in differences with multiple time periods and staggered treatments. The authors, among others (e.g., see Sun and Abraham (2021)), highlight econometric challenges to canonical two way fixed effect designs with staggered treatments which are also relevant for our analyses.

Table 6 depicts the estimated Average Treatment Effect on the Treated (ATT) when employing the estimation technique and software by Callaway and Sant'Anna (2021). In the first column, we replicate our main analysis by employing a treated vs. not yet treated design that disregards never treated communities. In the second specification of Table 6, we estimate an ATT which utilizes never treated communities as control observations. Considering these alternative counterfactuals enhances the credibility of our analyses because it allows us to assess whether our results apply more widely beyond the specific context of the (not yet) treated observations. Both specifications include ZIP and *date* fixed effects and are weighted by the number of observations (*numobs*) in each ZIP code. The estimated ATTs from these two specifications are both significantly different from zero. Their magnitude is also consistent with the results from Table 3 where *post* is meant to capture the aggregate effect. Thus, we continue to identify the effect from our main analysis when applying alternative estimators which are designed to address challenges from canonical two way fixed effect designs.

Stacked Difference in Differences

[Table 7 about here.]

The ZIP collapsed data allow us to perform a stacked difference in differences analysis. Generally, stacked DiD is considered more robust to eventual econometric problems with staggered treatment designs (e.g., see Cengiz et al. (2019)). We create subdatasets by unique event times for treated and control (i.e., never treated) communities. In these subdatasets, events occur all at once, which allows us to construct pre- and post-indicators (*post stacked*) not only for the treated communities but also for the never treated control communities. We stack the subdatasets to one and estimate equation (2) with *unpdcol* as the dependent variable using OLS:

$$\begin{aligned} Debt\ in\ Collection_{ZIP,t} = \alpha &+ \beta_1\ post\ stacked_{ZIP,t} + \beta_2\ post\ stacked_{ZIP,t} \times treated_{ZIP} \\ &+ FEs + \varepsilon_{ZIP,t} \end{aligned} \tag{2}$$

The interaction term *post stacked* $\times$ *treated* captures the parameter of interest in this regression. It captures how Debt in Collection (*unpdcol*) develops in treated communities vis-a-vis its development in the stacked control communities. Any baseline differences between the *treated* and *control* communities are absorbed by including *ZIP* fixed effects. The *date* fixed effects control for time-variant influences across all observations. Because we are also interested in whether there are considerable trends among the control observations (i.e., *post stacked*) that could confound the identification of treatment effects, we respectively include *cohort* and *cohort* $\times$ *date* fixed effects, which subsume the *post stacked* indicator, in one specification (column (3) in Table 7).[27] One potential disadvantage of stacked DiD designs is their multiple usage of control observations. We address this in two dimensions. First, we provide specifications

[27]The results remain unchanged when we include *cohort* $\times$ *date* fixed effects but remove *cohort* fixed effects, in which the *ZIP* fixed effects of the treated communities are nested (β_2 of 0.5324 for *post stacked* $\times$ *treated* with a p-value of 0.0641 when we cluster standard errors at the *CT* level).

of equation (2) that are either weighted or unweighted by *numobs* to ensure that particularly large communities do not overproportionally impact the results through their weights. We find that coefficients for $post\ stacked \times treated$ are both positive and statistically significant in each specification (columns (1) and (2) in Table 7). Secondly, we account for duplication in the control observations by clustering standard errors not at the event level but at the unit of observation ZIP or at the CT level in which ZIP is nested (following Cameron, Gelbach, and Miller (2011) and Deshpande and Li (2019)). These adjustments are considered to reduce overrejection of the null hypothesis. Importantly, a stacked panel that covers the entire U.S. allows us to include granular $region \times quarter$ fixed effects (we implement these in different specifications at the *state* and the commuting zone (CZ) levels) to control for local economic trends (see Povel, Singh, and Winton (2007) and Beneish et al. (2023) on the association between financial misreporting likelihood and the economy). If intentional financial misrepresentation and its revelation were a product of a broader local economic downturn, our estimates for individuals' financial distress could reflect this trend rather than being impacted by a firm's fraud. In conjunction with the *date* fixed effects that capture trends for the entire population of the U.S., the inclusion of the $region \times quarter$ fixed effects allows us to capture local economic trends. Commuting zones (~700 in the U.S.) are of particular interest in this regard because they establish clusters of counties that are characterized by strong within-cluster commuting ties and cover the entire landmass of the U.S. (see Keys, Mahoney, and Yang (2023) for an application of commuting zones in research on financial distress). We find that our results persist when controlling for these granular regional economic trends (columns (4) and (5) in Table 7), reducing concerns about reflection confounding our insights (see also Section 5.2). Overall, we find evidence throughout all estimations at the $ZIP \times quarter$ level that is consistent with our analyses at the $person \times quarter$ level.

Parallel Pre-Trends

[Figure 8 about here.]

Our main analyses generally employ canonical DiD designs in which indicators for *post* and relative *months since treatment* are estimated against a single baseline reference indicator ("bin"). We do this for the tests at the $person \times quarter$ level because we employ a treated vs. not yet treated research design in which we keep all pre-treatment observations to ensure that the *date* fixed effects can be estimated on a sufficiently large number of observations at each point in time. Furthermore, relative month indicators ex ante to the treatment would be unbalanced for observations that are treated rather late in the sample period (because TU data are not consistently available ex ante to early treatments). Identification in these tests, nevertheless, relies on the assumption that the outcome variables of interest for the treated would develop just as they do for the controls if the treatment were absent for the treated. While this is an assumption by definition, one may gather support for this notion by observing that the dependent variable develops in parallel between treated and control observations ex ante to the treatment.

Therefore, we consider multiple event study designs that include relative month indicators ex ante to the treatment (i.e., not binned). In a first analysis, we limit the $person \times quarter$ data to a symmetrical $\pm$ 24-month period around the treatment. We estimate equation (1) with *unpdcol* as the dependent variable and include relative month indicators ex ante and ex post to the fraud revelation (with a reference baseline at the ultimate quarter before the revelation). We include $person$ and $date$ fixed effects and cluster standard errors at the CT level. Coefficients for the ex ante indicators and their 95% confidence intervals are presented in the upper panel of Figure 8 (the full set of estimates is depicted in the Online Supplement in Table OS2). Standard errors are large, and neither of the coefficients is significantly different from zero. Furthermore, the absolute value of the largest ex ante estimate is just one-fourth of the *post* estimate in Table 3. We then turn to ex ante event study estimates for the analyses at the $ZIP \times quarter$ level. While we present aggregated estimates for the ATT using the estimation technique by Callaway and Sant'Anna (2021) in Table 6 (which includes all pre- and 24 month of post-treatment observations, and ZIP and $date$ fixed effects), the authors also provide an estimation technique to derive dynamic event study estimates and their simultaneous confidence bands. We present the -24 to -3 months estimates in the lower two panels of Figure 8 (utilizing either never treated or not yet treated observations as controls). While we observe some variation among the estimates, parallel pre-trends also prevail in these analyses. Notably, the estimate for the ultimate pre-treatment indicator is a precise zero. Overall, we conclude that the event study estimates gather support for the parallel trend assumption.

5.2 Bust: Spatial Dispersion

[Figure 9 about here.]

[Table 8 about here.]

In an additional analysis, we assign treatment status at different regional levels to show how the treatment effect disperses spatially. We depict the communities of interest from our sample in Figure 9. The respective counties are not only located in states but also nested in commuting zones, which are characterized by strong within-cluster commuting ties. For our analysis, we alter the group of potentially affected $ZIP \times quarter$ observations. We expand treatment status from the baseline county level (~3.9K CTs in the U.S.), over commuting zones (~700 CZs in the U.S.), to the state level (Giannetti and Wang (2016) utilize the *state* level to proxy for increased spatial fraud exposure (see also Carnes, Christensen, and Madsen (2023))). We expect to find attenuated effects when expanding the local grid of treatment.

Table 8 depicts the results from estimating three different specifications in which *post spatial* is either assigned at the *state*, CZ, or CT level. We find that the effect of fraud revelation on the financial distress indicator *unpdcol* is substantially attenuated when considering *state* level treatment assignment (ca. one-tenth of the baseline effect). If fraud reflected deteriorating state economic conditions, we would have expected to find a stronger effect at this level of treatment assignment. Our results, consistent with the results from the stacked DiD analysis, mitigate this concern. In comparison to the state level treatment assignment, we find stronger effects under treatment assignment at commuting zones. The estimate, though, still fall shorts of the estimate for treatment assignment at the CT level. These findings suggest that the average treatment effect fades with increasing spatial distance to the fraudulent firm, underscoring the role of spatial proximity in accounting fraud exposure.

5.3 Bust: Intensity

[Table 9 about here.]

Our focus on the financial situation of all individuals who reside in spatial proximity to the fraudulent firms entails a trade-off: identification of effects that are driven by contagion and spillovers (i.e., we specifically include all spatially exposed individuals but disregard their potential ties to the fraudulent firms) may preclude the identification of specific mechanisms operating at the individual level, such as employment (e.g., see J. H. Choi and Gipper (2024)). We are, however, interested in whether our results would primarily be an accumulation of the effects on those with direct ties to the fraudulent firms. Therefore, we use cross sectional analyses to elucidate the intensity of the identified effects. We start by considering layoffs by fraudulent firms around the fraud's revelation. Data on layoff events come from Stanford University's Big Local News (BLN) Initiative and cover WARN Act Notices. A Worker Adjustment and Retraining Notification (WARN) is required by federal law and mandates a notice about large, scheduled layoff events.[28] Generally, a WARN must be issued 60 days in advance of the layoff and it has to be served to the affected employees and the state's dislocated worker unit, where the alerts are publicly available.[29] BLN scrapes WARNs from states' repositories and provides data at the layoff event level. We fuzzily match the firms from our sample to this dataset and identify whether a fraudulent firm issued a WARN in the aftermath of the fraud's revelation. Since BLN does not cover the entire U.S. but respectively 39 states, we continue to utilize the firms' headquarters as spatial regions of interest for our analysis. We then estimate a specification of equation (1) and regress Debt in Collection on a *post* indicator and on the interaction of *post* with the cross sectional indicator variable. We include *date* and *person* fixed effects and cluster standard errors at the CT level. The core idea of analyzing WARNs is to identify a set of cases in which a substantial number of exposed individuals are directly tied to the firm (here as employees). If our results were primarily driven by those with direct ties, our regression results should identify a particularly attenuated effect among those outside the WARN cross section. Our results, which are depicted in Table 9, however, do not suggest that this is the case. In contrast, we find that the estimates for *post* and $post \times WARN$ are economically nearly identical and both within and outside the cross section meaningful, suggesting that the consequences of intentional financial misrepresentation extend beyond cases in which effects originate primarily from those with direct ties to the fraudulent firms. This finding corroborates the interpretation of our main results.

We consider fraudulent firms from the finance industry as an additional cross section because revelation of fraud in the finance industry could particularly affect many, including those

[28] 29 U.S.C. § 2102(a)(2).

[29] An employer (i.e., any business that employs at least 100 employees) is mandated to issue a WARN either when a site is scheduled to be entirely closed or when a "mass layoff" is scheduled at the level of a site. The latter occurs under two conditions: either (i) 33% of the site's employees are about to be terminated, leading to at least 50 employees being affected, or (ii) at least 500 employees at the site are scheduled to be terminated. States host their own repositories of WARNs; e.g., see the website of California's EDD.

with rather indirect ties to the firm. The SEC, for instance, described one case in the finance industry as having "devastating regional impact", due to which "several regional banks failed", underscoring the multitude of those potentially affected. Consistently, we find a substantially amplified effect for the interaction term. In conjunction with the WARN cross section, our results are consistent with treating the identified effects from our analyses to reflect the financial impact on a broad set of individuals in a spatial community.

5.4 Boom: Ex Ante Financial Decisions

We now turn to an empirical analysis on how potentially misinformed individuals could act rationally overconfident when the fraud has yet not been revealed. Therefore, we shift our focus from indicators of financial distress to financial decisions, specifically focusing on credit card limits. As illustrated in Figure 1, one can distinguish between truthful & good signals δ, truthful & bad signals ε, and fraudulently good signals γ. The signal γ represents the unique mixture of regulatory violations and contradictory reporting under intentional financial misrepresentation. In essence, individuals in γ are factually but unknowingly closer to ε than they are to δ. Therefore, we are interested in whether these different signals map into differences in individuals' financial decisions when benchmarking observations under γ against ε.

We utilize corporate bankruptcies to proxy for the truthful & bad signal ε. Extensive research has documented the deteriorating economic effects ex post bankruptcy filing of firms (e.g., see Bernstein, Colonnelli, and Iverson (2019) and Hotchkiss (1995)). Recent research also approaches contagion and social costs as effects from corporate bankruptcies. For instance, Benmelech et al. (2019) address (firm-level) contagion by identifying "[...] a new channel through which bankrupt firms undergoing liquidation impose negative externalities on their nonbankrupt peers." The authors conclude that liquidation of a retail chain's local stores "weakens the economies of agglomeration in any given local area [...]". Chava, Malakar, and Singh (2022) analyze the effects of public firms' bankruptcies on local municipal bonds and find that "local communities are a major stakeholder in public firms" because "they are adversely affected by corporate financial distress." Similar to the employment effects of accounting fraud documented by J. H. Choi and Gipper (2024), John Graham et al. (2023) analyze consequences of bankruptcy on financial distress of employees (see also Wachter, Song, and Manchester (2009) and A. K. Agrawal and Matsa (2013)). These insights indicate that the ex post local economic effects from intentional financial misrepresentation and corporate bankruptcies share some similarities. The signals γ and ε ex ante to fraud revelation and bankruptcy filing, however, are different. Eckbo, Thorburn, and Wang (2016), for instance, show that invested CEOs, while they do not sell out when their firms approach bankruptcy, face substantial equity value losses already *ex ante* bankruptcy filing. Furthermore, Hertzel et al. (2008) document that the economic effects of bankruptcy filings along the supply chain occur *prior* to and at filing. The authors argue that corporate "financial distress is typically widely known well in advance of the filing of the bankruptcy petition, suggesting that substantial wealth effects for linked firms could also be evident in the pre-filing period." We conclude that the unsurprising nature of many corporate bankruptcies and the ex ante filing revealed economic conditions of these firms provide us with a valid proxy for the truthful & bad signal ε.

[Table 10 about here.]

We collect information on corporate bankruptcies from Audit Analytics and match case information to the Compustat-CRSP merged universe (CCM) utilizing CCM's linking table at the *cik* level. Next, we perform sample selection steps that essentially mirror those that we apply to our sample of cases of intentional financial misrepresentation. Table 10 summarizes these steps. In particular, we require identical criteria to a firm's local economic importance to be included in the benchmark sample. We do so to ensure that we are actually benchmarking comparable cases against each other so that the ex ante bankruptcy filing and ex ante fraud revelation signals remain unconfounded.

For identification, we stack events by unique event times to never treated communities as control observations and keep the 36 month period ex ante to each event in the subdatasets. We utilize 36 months as reference length because Eckbo, Thorburn, and Wang (2016) employ a three year pre bankruptcy filing period to analyze ex ante trends. The average difference between a misrepresentation's start and its revelation in our sample approximately equals three years, too. We utilize $ZIP \times quarter$ aggregated data for the entire U.S. and proceed consistent with our stacked DiD approach for the financial distress tests. Consequently, we are not only able to benchmark ex ante periods of fraud revelations and bankruptcy filings against each other but also include controls that capture time trends in the broader U.S. We estimate equation (3) which includes interactions of relative month indicators (*months to event*) with a factor variable *event type*, whose levels capture controls, corporate bankruptcies, and intentional financial misrepresentations. We include *ZIP* and *date* fixed effects in the regression and cluster robust standard errors by *CT* (in which *ZIP* is fully nested):

$$\begin{aligned} Credit\ Card\ Limit_{ZIP,t} = \alpha + \beta_n\ months\ to\ event_{ZIP,t} \times event\ type_{ZIP} \\ + FEs + \varepsilon_{ZIP,t} \end{aligned} \tag{3}$$

The available Credit Card Limit (*cclimit*) of current credit cards is the dependent variable of interest in our analysis. Credit card limits place an upper bound on consumer purchases and unsecured borrowing and are widely utilized in research on credit supply and demand. D. B. Gross and Souleles (2002) and Aydin (2022) show that credit consumption is sensitive to credit card limit expansions. T. Gross, Notowidigdo, and Wang (2020) find that credit card limits change with changes in credit scores when consumer bankruptcies are removed from reports. Importantly, Agarwal et al. (2015) conclude that the 2009 Credit Card Accountability Responsibility and Disclosure (CARD) Act, which falls into our sample period, "had a precise zero effect on credit limits." We conclude that *cclimit* describes individuals' credit demand and granted supply which renders it a suitable proxy for their financial decisions.

[Table 11 about here.]

The results are depicted in Table 11. For simplicity, we use three distinct columns to present the results from estimating one specification of equation (3). *X* marks the *event type* levels of control, corporate bankruptcy, and intentional financial misrepresentation. First, there is no meaningful trend among the control observations. Coefficients are marginally different from zero for all interactions and solely amount to a fraction of the estimates of the other two interactions. We are then interested in whether *cclimit* develops differently until bankruptcy filing/fraud revelation. The coefficients indicate that this is indeed the case. For corporate bankruptcies, the estimated coefficients are, perhaps surprisingly, negative. Individuals under ε could also be interested in expanding their *cclimit* to create a liquidity cushion. Instead, our results indicate that their demand may not meet the available supply. Importantly, estimates for the interactions of the ex ante indicators with the fraud indicator are positive (and, except one, all significantly different from zero). The differences between the bankruptcy and fraud estimates are not necessarily linear but move around USD 800 for most of the coefficients, before shrinking for the ultimate observation ex ante the events. Column (4) contains p-values for testing the difference between the estimates for corporate bankruptcies and intentional financial misrepresentations. All but one of the differences are significantly different at conventional levels. We interpret these findings to be consistent with our theoretical framework and conclude that financial decisions indeed appear misinformed under fraudulent information. Together with our cross sectional evidence on the ex post effects, these insights also underscore that the financial impact of accounting fraud expands beyond the eventual effects from general company distress.

5.5 Bust × Boom

[Table 12 about here.]

Finally, we are interested in whether (misinformed) financial decisions ex ante the revelation aggravate individuals' financial distress upon revelation. Therefore, we estimate equation (4) at the *person* × *quarter* level using OLS:

$$\begin{aligned} \textit{Debt in Collection}_{i,t} = \alpha &+ \beta_1 \, \textit{post}_{CT,t} + \beta_2 \, \textit{post}_{CT,t} \times \; \textit{z cclimit pre cgr}_{CT} \\ &+ \beta_3 \, \textit{z cclimit pre cgr}_{CT} + \beta_4 \, \textit{cclimit pre high}_{CT} \\ &+ FEs + \varepsilon_{i,t} \end{aligned} \tag{4}$$

The coefficients of interest in this specification are *post* and *post* × *z cclimit pre cgr*. They capture the average effect of the revelation of accounting fraud on individuals' Debt in Collection (*unpdcol*) and how this effect is incrementally impacted by *cclimit pre cgr*. We measure *cclimit pre cgr*, which is the compound growth rate ("*cgr*") of credit card limits ex ante ("*pre*") the revelation of the fraud (i.e., the $[t_{-36}; t_{-3}]$ period with revelation in t_0). We construct *cclimit pre cgr* at the ZIP level (because information on *cclimit* are aggregated at the $ZIP \times \textit{quarter}$ level) and then calculate the average per CT. Consequently, *z cclimit pre cgr* is a continuous, time-constant measure at the CT level, whereas z indicates standardization with a mean of zero and a standard deviation of one. We have shown in Section 5.4 that *cclimit* develops differently under fraudulent 'good' information vis-a-vis its development under truthful 'bad' information from firms headed for bankruptcy. The identified (regionally) expanding *cclimit* ex ante the revelation translates into, on average, a positive *cclimit pre cgr* (see Table 2). If *cclimit pre cgr* incrementally aggravated individuals' financial distress upon fraud revelation, one would expect a positive coefficient for both *post* and *post* $\times$ *z cclimit pre cgr*. However, it is unclear whether this relationship exists because an expanded *cclimit* could also serve as an initial liquidity cushion. In this case, one would expect an initially attenuated or even negative effect which becomes positive once the cushion is exhausted.

Results of multiple specifications of equation (4), in which we include *person* and *date* fixed effects and cluster standard errors at the *person* level, are depicted in Table 12. In column (2), we add *cclimit pre high* to the regression, which is an indicator variable that equals one when the average level of *cclimit* in a county exceeds the sample average during the pre-revelation period. Its inclusion controls for the *level* of demanded and granted credit supply pre-revelation. We find that the estimates for *post* are similar to those in our main analysis, both in magnitude and significance. We also find positive estimates for *post* $\times$ *z cclimit pre cgr*, which suggests that financial distress post-treatment incrementally increases with increasing growth of *cclimit* pre-treatment. Our results indicate that a two standard deviation increase in *z cclimit pre cgr*, which is considered to typically cover 95% of the distribution, adds more than 10% to the baseline effect of *post*. Given that we are also interested in whether *cclimit pre cgr* initially serves as liquidity cushion, we subsume the *post* indicator with relative *months to treatment bin* indicators and their interactions with *z cclimit pre cgr* in an alternative specification of equation (4) (column (3)). The estimates consistently indicate that the effect is indeed slightly negative early post-treatment and becomes positive once the cushion is apparently exhausted. Taken together, we cautiously conclude that financial decisions ex ante to the revelation of accounting fraud not only appear misinformed but also exert incremental harm to individuals' financial distress upon fraud revelation.

6 Conclusion

This study provides evidence on how intentional financial misrepresentation by a firm affects the financial situation of the individuals who establish the spatial community around which the firm operates. Utilizing granular data from a consumer credit panel covering 10% of the U.S. population with credit histories, we analyze both pre-revelation financial decisions and post-revelation financial distress. Upon revelation, we identify significant increases in indicators of financial distress among individuals who reside in spatial proximity to fraudulent firms' headquarters. On average, we observe incremental increases in debt in collection, affecting approximately one in every one hundred to one in every two hundred individuals. We corroborate these insights with analyses of alternative distress indicators, of how the effect spatially disperses, and of cross sections that allow us to assess whether the effects are primarily driven by those with direct ties to the firms. Additionally, we compare individuals' financial decisions under fraudulent 'good' information to those under truthful 'bad' information from firms headed for bankruptcy, revealing misinformed financial decisions, in form of credit demand and supply, before the fraud's exposure. Results from various identification strategies also mitigate concerns about reflection confounding our insights and suggest that accounting fraud and its impact on individuals' financial situation are distinct from the effects from general company distress. Finally, we show how (misinformed) financial decisions pre-revelation can aggravate post-revelation financial distress. Overall, we offer critical insights into the connection between one of the roots of social outcomes, the financial health of a broad set of individuals in a spatial community, and accounting fraud.

In conclusion, we encourage additional research on how accounting fraud eventually affects economic agents and the society at large. Our study provides evidence that suggests that a narrow focus on specific share- or stakeholders (as already conceptually criticized by Velikonja (2012)) appears insufficient when considering enforcement actions against corporate misbehavior. Future research might, for instance, explore alternative measures of exposure to and impact from fraud to provide an holistic assessment. Taken together, these insights can assist regulators and their agencies in shaping and implementing policies related to intentional financial misrepresentation.

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Appendix

Table A1: Variable Definitions

Variable	Definition [Source]
Sampling Units & Spatial Aggregations	
County (CT)	Time invariant U.S. county as defined by its countyfips; the crosswalk to CT is performed using the ZIP code from TU's *person* level summary information as seeding variable (a person's address information can be time variant); CT is the spatial community of interest for the treatment assignment; (fraudulent) firms' headquarters-counties are identified in the historical snapshots of Compustat. [TU & Compustat]
Commuting Zones (CZ)	Time invariant U.S. commuting zones; crosswalk to CZ is performed using the ZIP code from TU's *person* level summary information as seeding variable (a person's address information can be time variant). [TU]
Sampling Unit in TU Consumer Credit Panel (*person*)	Individual in the TU consumer credit panel with non-missing and minimum credit score of 300; for years after 2008 the sample is restricted to individuals between 20 and 80 years of age (birthdays are not available pre-2009); individuals are observed at the ends of March, June, September, and December of each year, creating a $person \times quarter$ panel; the panel contains information on (i) person level summary statistics ($person \times quarter$), (ii) trade level information on each credit item per person ($person \times trade \times quarter$), (iii) collections ($person \times trade \times quarter$), and (iv) public records ($person \times record \times quarter$). [TU]
U.S. state (*state*)	Time invariant U.S. state as defined by its statefips; the TU consumer credit panel contains information for individuals who live in U.S. oversea-territories/who are with the U.S. armed forces. We follow TU's recommendation and exclude individuals who live in the following states: 'AA', 'AE', 'AP', 'AS', 'FM', 'GU', 'MH', 'MP', 'PR', 'PW', 'VI'. [TU]
ZIP Code (ZIP)	ZIP codes come from TU's *person* level summary information (a person's address information can be time variant); the aggregated quarterly observation (i.e., average) of all individuals in a ZIP create a $ZIP \times quarter$ panel; see *numobs* for weights in some regressions. [TU]
Quarter (*date*)	Individuals in the panel are observed at the end of each *quarter* (i.e., March, June, September, December) throughout the sample period (i.e., *date*). "Months" in *months to treatment* and *months to event* refers to these dates, too. [TU]
Outcome Variables	
Debt in Collection (*unpdcol*)	Indicator variable, equals 100 when an individual's credit file indicates that she has 1+ debt in collection; zero otherwise. [TU]
Credit Card Delinquencies (*ccdq*)	Indicator variable, equals 100 when an individual's credit file indicates that she has 1+ delinquent credit card; zero otherwise. [TU]
Conditional Credit Card Delinquencies (*ccdq cond*)	Indicator variable, equals 100 when an individual's credit file indicates that she has 1+ delinquent credit card (identical to *ccdq*); zero otherwise; set to missing if individual does not own a credit card. [TU]
Consumer Bankruptcy (*bkrt*)	Indicator variable, equals 100 when an individual's credit file indicates that she has filed for bankruptcy under Chapter 7 and/or Chapter 13, zero otherwise . [TU]
Credit Card Limit (*cclimit*)	Sum of all credit card limits of current credit cards (in USD) which are shown in an individual's credit record. [TU]

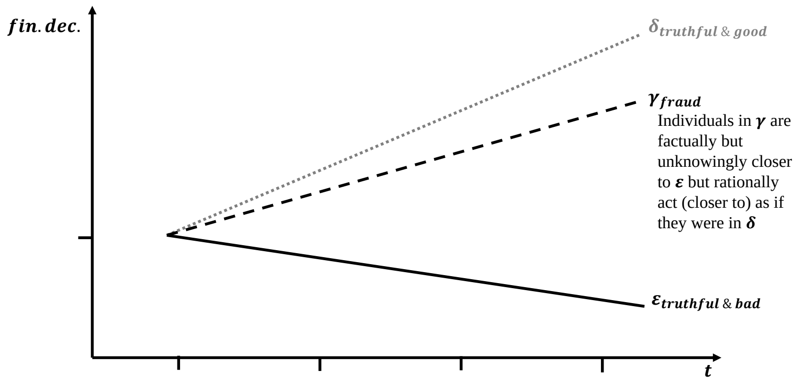
Variable	Definition [Source]
Credit Card Limit Growth (*cclimit pre cgr*)	*cclimit pre cgr* measures the county (for the fraud sample) average compound growth rate of *cclimit* (i.e., the compound growth rate ("*cgr*") is calculated at the *ZIP* level ($\sqrt[n]{cclimit_{t-3}/cclimit_{t-36}}$) and then averaged over the *CT* level) over the fraudulent t_{-36} to t_{-3} period ("*pre*"). [TU]
Credit Card Limit High (*cclimit pre high*)	Indicator variable, equals 1 when the average level of *cclimit* in a county exceeds the sample average during the fraudulent t_{-36} to t_{-3} period ("*pre*"), zero otherwise. [TU]
Other Variables	
Abnormal Return (*abret*)	Difference between the stock's daily return without dividends (*retx*) and a value-weighted market portfolio return without dividends (*vwretx*); winsorized at $5 \times p99/p1$. [CRSP]
Corporate Bankruptcy (*bkrpt*)	We compile data on corporate bankruptcies from Audit Analytics (AA) and the Florida UCLA LoPucki Bankruptcy Research Database (BRD). We fuzzily match corporate bankruptcies from AA and BRD to the cases of intentional financial misrepresentations in our sample to create a set of fraud-related formal events (see also *dlst*). Corporate bankruptcies from AA serve as benchmark for the tests in the pre-revelation periods (see *monhts to event*). [AA & BRD]
Credit Score (*score*)	VantageScore 3.0 credit score; we truncate the sample at minimum *score* of 300 and exclude all individuals with a score below this threshold (see *person*). [TU]
Delistings (*dlst*)	Information on and dates of delistings of corporate equities come from CRSP. We fuzzily match delistings to the cases of intentional financial misrepresentation in our sample to create a set of fraud-related formal events (see also *bkrpt*). [CRSP]
numobs	Number of observations in the TU consumer credit panel in a *ZIP* at a given point in time ($ZIP \times quarter$). [TU]
treated	A person (identified at the spatial community (*CT*) of her residence) who is eventually affected by the revelation of intentional financial misrepresentation. Our main analysis at the $person \times quarter$ level employs a treated vs. not yet treated research design so that *treated* equals one throughout this sample. [TU & CFRM]
post	Indicator variable, equals one once fraud is revealed in a community. Our analyses include all available pre-treatment observations (i.e., *post* equals zero) and are limited to a post-treatment period of 24 months (i.e., *post* equals one).
post sptl	We expand the spatial treatment assignment from the *CT* (baseline) to the *CZ* and the *state* levels (with units of observations at the $ZIP \times quarter$ level): *post sptl* equals one if there is a treated community (*CT*) within the spatially expanded community at any given point in time (*date*).; equals zero in these communities otherwise, creating a treated vs. not yet treated research design with varying sample sizes.
post stacked	We stack never treated communities (at the $ZIP \times quarter$ level) to subdatasets of each treatment for a stacked DiD analysis. Within each subdataset, treatment occurs all at once, which allows us to construct *post stacked* for the treated and the control (i.e., the never treated communities) observations; equals one upon revelation of intentional financial misrepresentation in a subdataset; zero otherwise.

Variable	**Definition [Source]**
months to treatment	Relative months to/since the revelation of intentional financial misrepresentation by locally economically important firms in the communities of interest. *bin*-version is utilized in regression analyses: relative months to (i.e., ex ante) revelation are binned at -1 and serve as baseline/omitted category in the analyses (this approach mirrors a canonical DiD with *post* as independent variable of interest). We provide a fully specified event study which includes relative month to treatment indicators ex ante and ex post treatment (i.e., not binned) in the Online Supplement.
months to event	Relative *month to event* indicators $[-36; -3]$. Events include controls, revelation of intentional financial misrepresentation ("fraud"), and corporate bankruptcies ("bkrpt"). The event type factor variable X captures the event type and is interacted with the monthly indicators. [CFRM & AA]
WARN	WARN abbreviates Worker Adjustment and Retraining Notification; a WARN is a mandatory notice of large, scheduled layoffs under U.S. law; indicator variable that equals one when we can identify at least one WARN for a sample firm in a scraped dataset of WARNs which is provided by Stanford University's Big Local News (BLN) initiative; zero otherwise. [BLN]

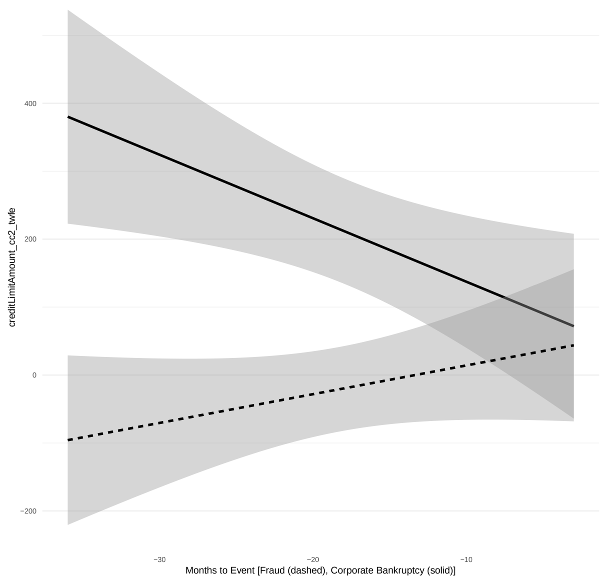
TU data items [TU]: Calculated (or derived) based on credit data provided by TransUnion, a global information solutions company, through a relationship with the Kilts Center for Marketing at the University of Chicago Booth School of Business. Indicators of financial distress (*unpdcol*, *ccdq*, *ccdq cond*, *bkrt*) are constructed by applying a slightly adjusted version of the replication package by Keys et al (2023).

Figure 1: Ex Ante Fraud Revelation - Rational Overconfidence

Panel A: Theory



Panel B: Evidence



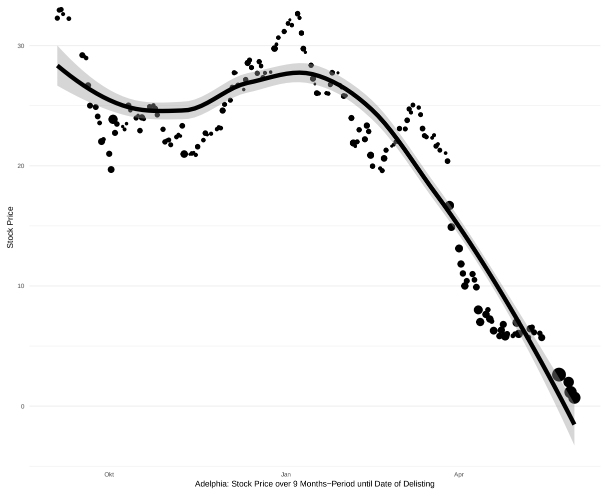
The upper Panel of this figure depicts the theoretical expectations on how financial decisions may be influenced by locally economically important firms. A firm in a deteriorating economic condition (e.g. a firm that will file for bankruptcy eventually) reports truthful & bad information ε. When engaging in intentional financial misrepresentation, a firm reports a fraudulent good (and credible) information γ. If these information were relevant for individuals' financial decisions (e.g. through expectations on future local economic conditions), one would expect diverging trends in individuals' financial decisions. The lower Panel of this figure provides initial empirical evidence for this theoretical expectation. We utilize credit card limits ($cclimit$) of current credit cards, which establish an upper bound on consumer purchases and unsecured borrowing. The y-axis presents residuals from regressing $cclimit$ on $date$ and ZIP fixed effects (with data aggregated at the $ZIP \times quarter$ level for the entire U.S.). The x-axis indicates the relative months to (i) the revelation of intentional financial misrepresentation and (ii) a corporate bankruptcy filing, whereas we use these events as proxies for (i) the fraudulent good information γ (dashed) and (ii) the truthful & bad information ε (solid) of locally economical important firms. See Table 11 for regression results.

Figure 2: Intentional Financial Misrepresentation: NLP Identification

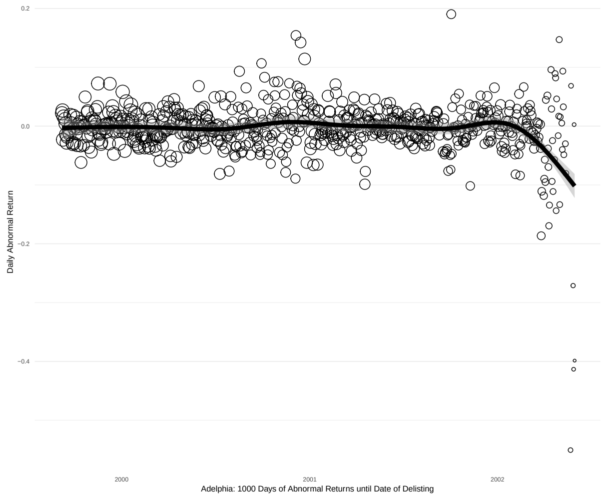
This figure depicts a word cloud of the 50 most common bi- and tri-grams in a set of AAERs in our sample that we identify as "true" fraud cases in the replication data by Call et al. (2018) (CMSW). We create a corpus of all primary AAERs in our sample (after having scraped them from the SEC), lemmatize tokens and only keep actions terms (verbs, adverbs, adjectives). We remove common English terminology and create bi- and tri-grams of the remaining tokens. Terminology, which is presented in this Figure for the set of true fraud AAERs, is descriptive of intentional financial misrepresentation. Therefore, we construct regular expressions on selected tokens and classify the remaining AAERs in our sample as intentional vs. error (see Sections 3 and 4). We only keep cases in our sample that are classified as intentional financial misrepresentations. The Online Supplement contains additional information on the classification process in Figure OS2.

Figure 3: Adelphia Communications: Fraud Revelation

Panel A: Stock Price

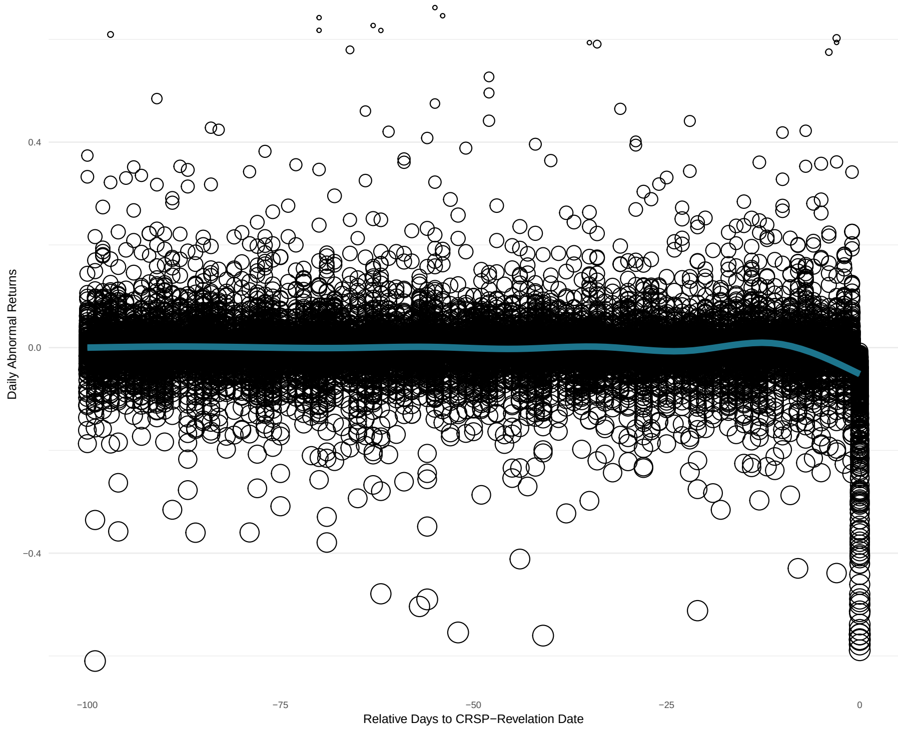


Panel B: Abnormal Returns



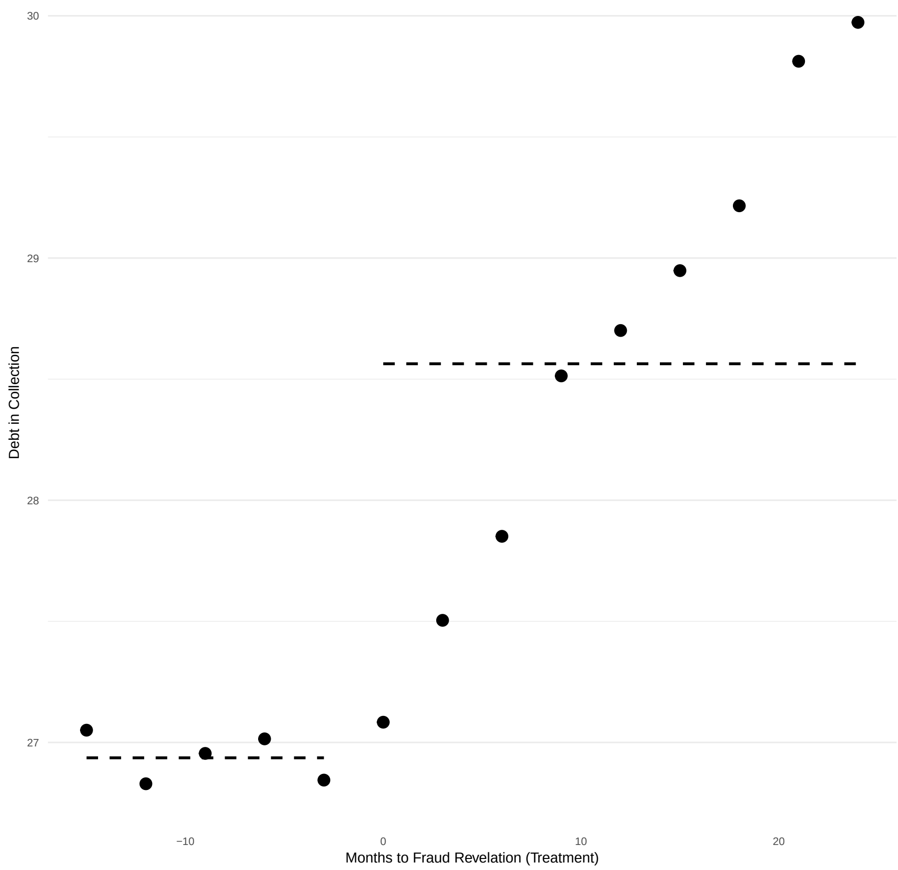
This figure depicts information on the Adelphia case, which we use to provide a stylized example of how we identify fraud revelation dates. Panel A depicts the stock price development of the company for the 9-month period which preceded Adelphia's delisting on May/31/2002. Dots in Panel A mark the stock price and the size of them marks the absolute values of the daily abnormal returns. We identify March/27/2002 as revelation date when an analyst pointed to inconsistencies in an earnings conference call (see Lowenstein Feb/1/2004 for the NYT). The lower Panel of this figure depicts 1000 days of abnormal returns for Adelphia before it was delisted. We use this long period to show that the firm's stock performance co-moved with the market before the fraud was revealed (the smoothed line and its 95% interval lay flat around zero). Only upon revelation we observe large and persistent negative abnormal returns (see the notch in the smoothed line). We utilize large negative abnormal returns for identification of revelation dates (see Figure 4 and Section 4).

Figure 4: Fraud Revelation & Large Negative Abnormal Returns



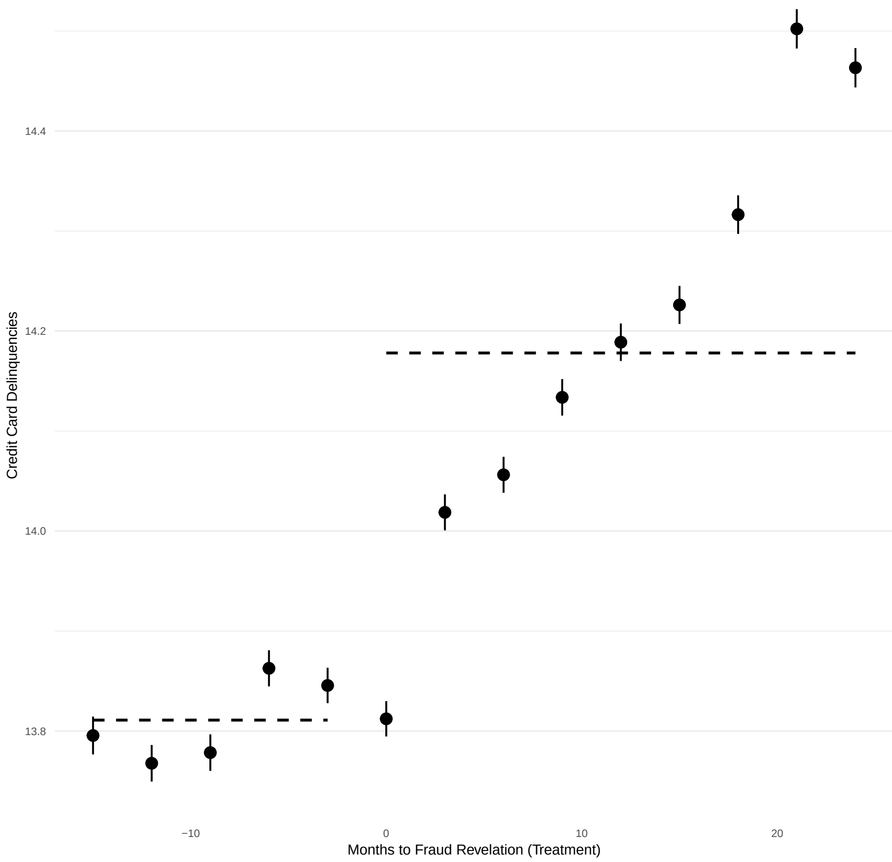
This figure depicts daily abnormal returns (*abret*) before and at the date which we classify as fraud revelation date. We compile information on the dates of (i) all AAERs per case, (ii) delistings, and (iii) bankruptcy filings of sample firms in our analysis and ensure that (ii) and (iii) are time congruent with the AAERs so that all these "formal events" are plausibly fraud-related. We then consider the first date of all formal events per case and analyze daily abnormal returns in the weeks preceding it $[-100; -10\ days]$. We mark the date with the largest negative abnormal return in this period as "CRSP-Revelation Date" of a case. We consider the ceiling quarter-end date of this date as the revelation and treatment date ($t = 0$) for our analyses.

Figure 5: Financial Distress Upon Fraud Revelation - Debt in Collection



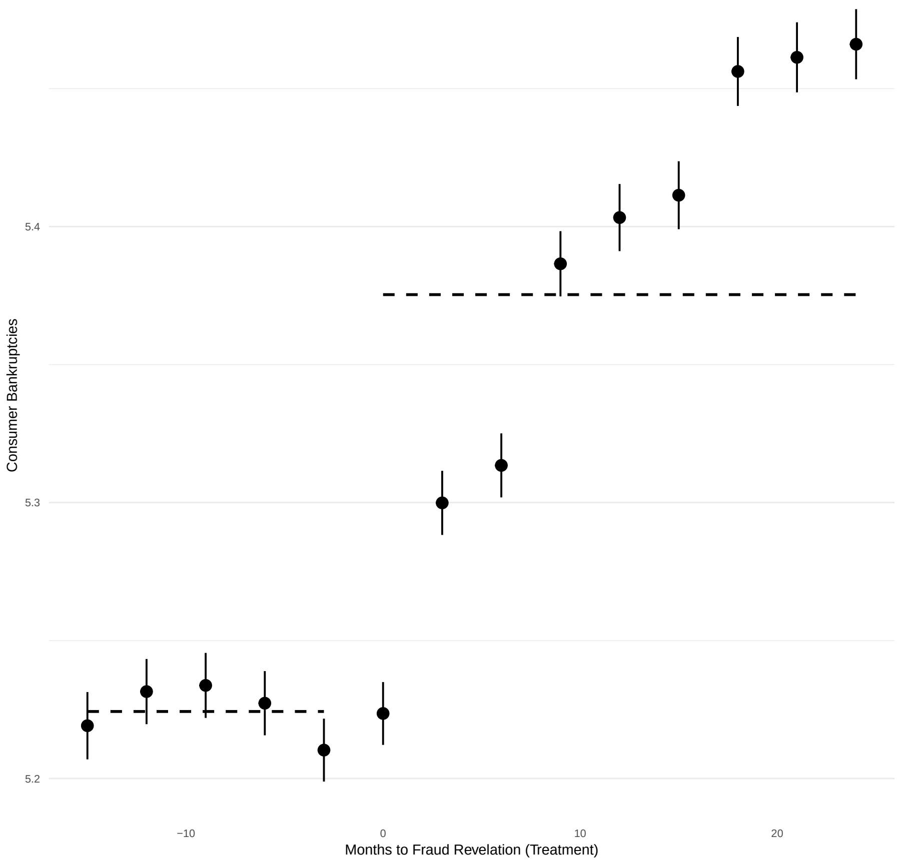
This figure depicts Debt in Collection (*unpdcol*, which is an indicator variable that equals 100 when an individual is flagged as having at least one debt sent to collection) against relative months to a case's revelation date (with data at the $person \times quarter$ level). We use the ceiling end of quarter date in which the revelation event falls as $t = 0$ throughout our analyses. The dots present the average in *unpdcol* among the treated (we utilize a treated vs. not yet treated research design) at the respective point in time. Vertical lines mark the 95% confidence intervals. The horizontal dashed lines mark the multiperiod averages of *unpdcol* (here $[-15; -3]$ and $[0; 24]$). Our empirical analyses at the $person \times quarter$ level include all pre-treatment observations (binned at a single reference baseline) and are limited to 24 months post-treatment.

Figure 6: Financial Distress Upon Fraud Revelation - Credit Card Delinquencies



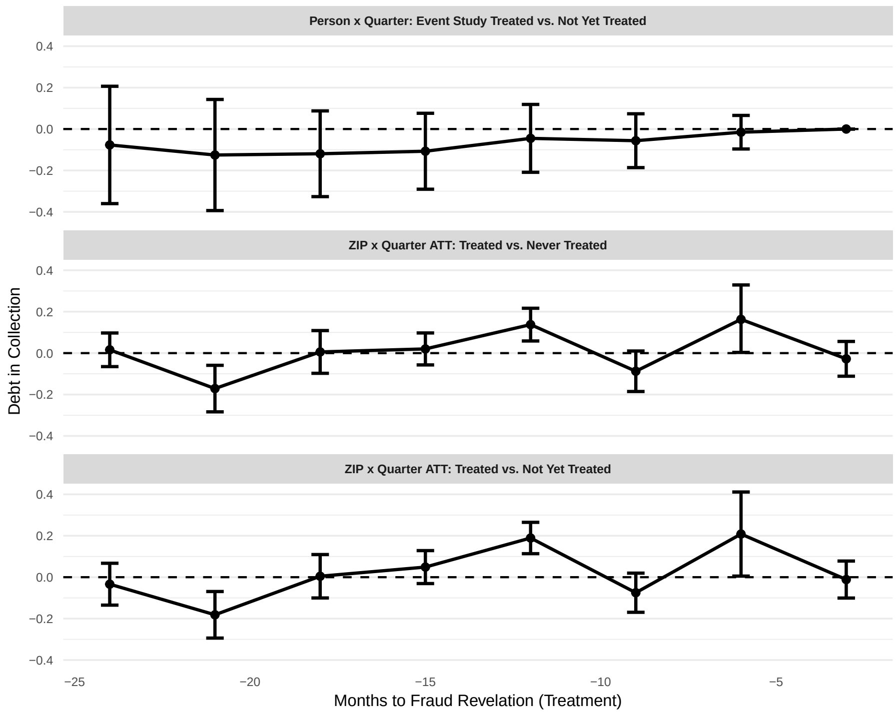
This figure depicts Credit Card Delinquencies ($ccdq$, which is an indicator variable that equals 100 when an individual's credit file indicates that she has 1+ delinquent credit cards) against relative months to a case's revelation date (with data at the $person \times quarter$ level). We use the ceiling end of quarter date in which the revelation event falls as $t = 0$ throughout our analyses. The dots present the average in $ccdq$ among the treated (we utilize a treated vs. not yet treated research design) at the respective point in time. Vertical lines mark the 95% confidence intervals. The horizontal dashed lines mark the multiperiod averages of $ccdq$ (here $[-15; -3]$ and $[0; 24]$). Our empirical analyses at the $person \times quarter$ level include all pre-treatment observations (binned at a single reference baseline) and are limited to 24 months post-treatment.

Figure 7: Financial Distress Upon Fraud Revelation - Consumer Bankruptcies



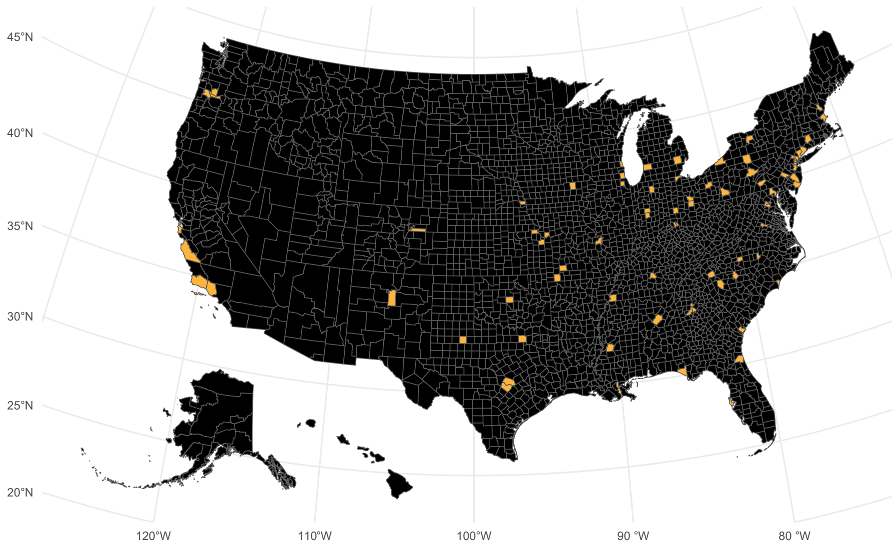
This figure depicts Consumer Bankruptcies ($bkrt$, which is an indicator variable that equals 100 when an individual's credit file indicates that she has filed for bankruptcy under Chapter 7 and/or Chapter 13) against relative months to a case's revelation date (with data at the $person \times quarter$ level). We use the ceiling end of quarter date in which the revelation event falls as $t = 0$ throughout our analyses. The dots present the average in $bkrt$ among the treated (we utilize a treated vs. not yet treated research design) at the respective point in time. Vertical lines mark the 95% confidence intervals. The horizontal dashed lines mark the multiperiod averages of $bkrt$ (here $[-15; -3]$ and $[0; 24]$). Our empirical analyses at the $person \times quarter$ level include all pre-treatment observations (binned at a single reference baseline) and are limited to 24 months post-treatment.

Figure 8: Pre Trends - Event Study Design



This figure depicts coefficients and their confidence intervals from three event study regressions which include relative *months to treatment* indicators in the pre- and post-treatment periods (i.e., not binned at a single baseline reference). The upper Panel depicts an event study at $person \times quarter$ level in which we limit the sample to a symmetric ± 24 months to/since treatment period. We include *person* and *date* fixed effects in this regression and cluster SE at the CT level. The ultimate period ex ante to the treatment is the omitted baseline reference indicator. The points mark the coefficient estimates and the error bars cover the 95% confidence intervals. The lower two Panels build on regressions at the $ZIP \times quarter$ level and depict relative months to treatment indicators for the $[-24, -3]$ months period under application of the estimation technique by Callaway/SantAnna (2021) (all pre-treatment periods are included in these regressions but not displayed here). The points mark the coefficient estimates and the error bars cover the simultaneous confidence bands (see Callaway/SantAnna (2021) on how these bands are calculated; note that this technique does not require a left out/baseline reference). All regressions in this figure include post-treatment indicators which are not depicted here (e.g., see Table OS2).

Figure 9: Spatial Dispersion - Communities of Interest



This figure depicts a map of the U.S.A with its county borders. Orange (light) fillings mark the treated counties in our sample. We use the spatial demarcation at the county (CT) level because we are particularly interested in locally economically important firms and how their intentional financial misrepresentations impact the financial situations of the individuals who establish the spatial community around which the firm operates. In additional tests, we investigate how treatment effects disperse spatially when expanding treatment level assignment from counties to (i) commuting zones (CZ) and (ii) states.

Table 1: Sample Selection - CFRM

Selection Step	Cases
SEC AAERs: financial misrepresentation at firm-case level (CFRM and AA)	$1,111$
./. U.S. Firms	725
./. Intentional financial misrepresentation	606
./. Firms in Compustat-CRSP universe upon revelation	305
./. FCPA bribes	290
./. Insider trading and other violations	286
./. Overlapping events per firm	285
./. Local economic importance: extremely large and small firms	243
./. Local economic importance: minimum spatial market share	185
./. Local economic importance: HQ not in county with population in top 1-pctl of U.S.	112
./. Revelation date in TransUnion sample period	104

SEC Accounting & Auditing Enforcement Releases (AAER) are aggregated and updated at the firm case level by Dechow et al. (2011) ("CFRM"). We start the sample selection with all electronically available AAERs at the SEC's website (as pdf and html files) between late 1999 and 2018 (we disregard the recent 2019-2021 update by CFRM due to the potential impact of the COVID-19 pandemic on our outcome variables of interest). CFRM assigns each case a "primary" AAER and - if applicable - allocates multiple other/secondary AAERs to a case. One AAER may be allocated to multiple cases (e.g., the 1,111 cases are identified by 1,048 AAERs). We manually collect the URLs to all primary AAERs and scrape the documents from the SEC's website. We utilize the recently introduced database on AAERs by Audit Analytics (AA) to collect the URLs to and dates of all secondary AAERs. The sample selection steps and other data sources are described in the main body. Our focus is on compiling a dataset of intentional financial misrepresentations by locally economically important firms.

Table 2: Summary Statistics - TU Consumer Credit Panel

Panel A: Person × Quarter

Statistic	Mean	St. Dev.	Min	Median	Max
score	668.10	111.69	301	679	839
unpdcol	26.81	44.29	0	0	100
ccdq	13.86	34.56	0	0	100
ccdq cond	18.66	38.96	0	0	100
bkrt	5.08	21.97	0	0	100
year			2001	2005	2018
treated	1	0	1	1	1
post	0.2564	0.4367	0	0	1
months to treatment	−42.53	52.42	−210	−27	24
months to treatment bin	2.21	6.72	−1	−1	24

Panel B: ZIP × Quarter

Statistic	Mean	St. Dev.	Min	Median	Max
score	663.9	41.84	301.0	665.9	838.0
unpdcol	29.20	15.44	0	27.66	100.00
cclimit	31,110	11,990.32	0	30,352	590,679
cclimit pre cgr	0.0129	0.0533	−0.6309	0.0114	0.8902
numobs	778.1	1296.78	1.0	179.0	16059.0
year			2000	2010	2019
treated	0.07	0.25	0	0	1
control	0.93	0.25	0	1	1

This table depicts summary statistics for data from the TU consumer credit panel. The upper Panel contains information at the $person \times quarter$ level. This sample starts in 3/2001 and respectively includes eventually treated individuals. Our analyses at the $person \times quarter$ level employ a treated vs. not yet treated identification strategy, which forces our sample to end with the ultimate treatment observation in 2018. Data at the $ZIP \times quarter$ level (lower Panel) cover the entire U.S. and stretch from 9/2000 to 12/2019. We also display summary statistics for panel information on treatment assignments in this table. All variables are defined in the Appendix. Note that different identification strategies (e.g., staggered and stacked DiD) cause alterations in sample compositions and sizes. Therefore, we describe samples and variables in the main body along with the analyses.

Table 3: Financial Distress Upon Fraud Revelation - Debt in Collection

	Dependent Variable:			
	unpdcol	*unpdcol*	*unpdcol*	*unpdcol*
post	0.7680	0.5164		
	(0.0219)	(0.2392)		
months to treatment bin 0			0.3128	0.2066
			(0.0192)	(0.1669)
months to treatment bin 3			0.4976	0.2836
			(0.0209)	(0.1861)
months to treatment bin 6			0.5997	0.3822
			(0.0223)	(0.2070)
months to treatment bin 9			0.7271	0.4825
			(0.0241)	(0.2331)
months to treatment bin 12			0.8392	0.5648
			(0.0257)	(0.2749)
months to treatment bin 15			0.9975	0.7179
			(0.0271)	(0.3000)
months to treatment bin 18			1.0526	0.7995
			(0.0288)	(0.3122)
months to treatment bin 21			1.1765	0.8657
			(0.0299)	(0.3526)
months to treatment bin 24			1.2185	0.9489
			(0.0312)	(0.3769)
Date FE	Yes	Yes	Yes	Yes
CT FE	Yes	No	Yes	No
Person FE	No	Yes	No	Yes
SE Cluster	Person	CT	Person	CT
Observations	122,732,436	122,732,436	122,732,436	122,732,436
Adjusted R^2	0.0451	0.7547	0.0451	0.7548

This table depicts analyses at the $person \times quarter$ level. We regress Debt in Collection (*unpdcol*, which is an indicator variable that equals 100 when an individual is flagged as having at least one debt sent to collection) on (i) a *post* indicator (columns (1) and (2)), and (ii) on relative *months to treatment* indicators (columns (3) and (4)). All specifications are estimated against single omitted baseline pre-treatment references ("bin"). Treatment is identified through staggered revelations of intentional financial misrepresentations by locally economically important firms. We utilize a treated vs. not yet treated identification strategy and include varying sets of fixed effects. Clustered, robust standard errors are in parentheses. Superscripts are not used to identify statistical significance.

Table 4: Financial Distress Upon Fraud Revelation - Credit Card Delinquencies

	Dependent Variable:			
	ccdq	*ccdq cond*	*ccdq*	*ccdq cond*
post	0.4060	0.6730		
	(0.1383)	(0.1239)		
months to treatment bin 0			0.1982	0.3351
			(0.0949)	(0.0856)
months to treatment bin 3			0.2517	0.4324
			(0.1053)	(0.0943)
months to treatment bin 6			0.3208	0.5326
			(0.1228)	(0.1120)
months to treatment bin 9			0.3925	0.6370
			(0.1356)	(0.1225)
months to treatment bin 12			0.4438	0.7150
			(0.1511)	(0.1338)
months to treatment bin 15			0.5048	0.8079
			(0.1668)	(0.1473)
months to treatment bin 18			0.5736	0.9297
			(0.1782)	(0.1605)
months to treatment bin 21			0.6419	1.0668
			(0.1897)	(0.1707)
months to treatment bin 24			0.7328	1.2190
			(0.2017)	(0.1841)
Date FE	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes
SE Cluster	CT	CT	CT	CT
Observations	122,732,436	91,188,312	122,732,436	91,188,312
Adjusted R^2	0.6098	0.6721	0.6098	0.6721

This table depicts analyses at the *person* × *quarter* level. We regress Credit Card Delinquencies (*ccdq*, which is an indicator variable that equals 100 when an individual's credit file indicates that she has 1+ delinquent credit cards; and *ccdq cond*) on (i) a *post* indicator (columns (1) and (2)), and (ii) on relative *months to treatment* indicators (columns (3) and (4)). All specifications are estimated against single omitted baseline pre-treatment references ("bin"). Treatment is identified through staggered revelations of intentional financial misrepresentations by locally economically important firms. We utilize a treated vs. not yet treated identification strategy, include *date* and *person* fixed effects, and cluster robust standard errors at the *CT* level (depicted in parentheses). Superscripts are not used to identify statistical significance.

Table 5: Financial Distress Upon Fraud Revelation - Consumer Bankruptcies

	Dependent Variable:	
	bkrt	*bkrt*
post	0.1012	
	(0.0117)	
months to treatment bin 0		0.0663
		(0.0097)
months to treatment bin 3		0.0999
		(0.0106)
months to treatment bin 6		0.0973
		(0.0114)
months to treatment bin 9		0.1089
		(0.0123)
months to treatment bin 12		0.1072
		(0.0133)
months to treatment bin 15		0.1170
		(0.0140)
months to treatment bin 18		0.1357
		(0.0150)
months to treatment bin 21		0.0938
		(0.0157)
months to treatment bin 24		0.1023
		(0.0164)
Date FE	Yes	Yes
CT FE	Yes	Yes
SE Cluster	Person	Person
Observations	122,732,436	122,732,436
Adjusted R^2	0.0078	0.0078

This table depicts analyses at the *person* $\times$ *quarter* level. We regress Consumer Bankruptcies (*bkrt*, which is an indicator variable that equals 100 when an individual's credit file indicates that she has filed for bankruptcy) on (i) a *post* indicator and (ii) on relative *months to treatment* indicators. All specifications are estimated against single omitted baseline pre-treatment references ("bin"). Treatment is identified through staggered revelations (treated vs. not yet treated) of intentional fin. misrepresentations by locally economically important firms. We include *date* and *CT* fixed effects and cluster robust (standard errors) at the *person* level. Superscripts are not used to identify statistical significance.

Table 6: Financial Distress Upon Fraud Revelation - ZIP Aggregated Data

	Dependent Variable:	
	unpdcol	*unpdcol*
ATT (Callaway & SantAnna)	0.7078	0.5228
	(0.1098)	(0.0928)
	[0.4927; 0.923]	[0.3409; 0.7048]
Data Aggregation	ZIP × Quarter	ZIP × Quarter
Control Communities	Not Yet Treated	Never Treated
Weighted	Yes	Yes
Date FE	Yes	Yes
ZIP FE	Yes	Yes
Estimation Method	Doubly Robust	Doubly Robust

This table depicts analyses at the $ZIP \times quarter$ level. We employ the estimation technique by Callaway/SantAnna (2021) to estimate the aggregated average treatment effect on the treated (ATT). We rely on the doubly robust estimation method and utilize default properties in the `did` R-package to derive estimates for the ATTs, (standard errors), and [simultaneous confidence bands] (see Callaway/SantAnna (2021) on how these calculations are performed). Column (1) employs a treated vs. not yet treated identification strategy (consistent with our analyses at the $person \times quarter$ level). Column (2) utilizes never treated communities (i.e., the entire U.S.) as controls. Regressions are weighted by the number of observations in the TU consumer credit panel in a ZIP at a given point in time ($numobs$). Superscripts are not used to indicate statistical significance. All variables are defined in the Appendix.

Table 7: Financial Distress Upon Fraud Revelation - Stacked DiD

	Dependent Variable:				
	unpdcol	*unpdcol*	*unpdcol*	*unpdcol*	*unpdcol*
post stacked	0.0001	0.0003		0.0001	0.0001
	(0.0001)	(0.0002)		(0.0002)	(0.0001)
post stacked $\times$ *treated*	0.5332	0.2897	0.5324	0.4661	0.4778
	(0.1562)	(0.1239)	(0.2875)	(0.2525)	(0.2309)
Data Aggregation	ZIP $\times$ Quarter	ZIP $\times$ Quarter	ZIP $\times$ Quarter	ZIP $\times$ Quarter	ZIP $\times$ Quarter
Panel Structure	Stacked	Stacked	Stacked	Stacked	Stacked
Weighted	No	Yes	No	No	No
Date FE	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	No	No	Yes	No	No
Cohort $\times$ Date FE	No	No	Yes	No	No
State $\times$ Date FE	No	No	No	Yes	No
CZ $\times$ Date FE	No	No	No	No	Yes
SE Cluster	ZIP	ZIP	CT	CT	CT
Observations	52,995,988	52,995,988	52,995,988	52,995,988	52,995,988
Adjusted R^2	0.7944	0.9399	0.7944	0.8059	0.8155

This table depicts analyses at the $ZIP \times quarter$ level. We utilize stacked DiD models for identification in this set of regressions. We create subdatasets for each unique treatment/event at a given date (i.e., cohort; treatment assignment does not change in comparison to the staggered tests) and allocate all never treated communities as controls to the treated observations. We then append/stack all subdatasets to one. Stacking has the advantage that within each subdataset treatment occurs all at once, which allows us to construct a *post stacked* indicator variable for both treated and control observations. The interaction term *post stacked* $\times$ *treated* captures the coefficient of interest in these analyses. All specifications include *date* and ZIP fixed effects with standard errors clustered at either the ZIP or the CT level (in parentheses). The regression in column (2) is weighted by *numobs*. In column (3) we include *cohort* and $cohort \times date$ fixed effects, which subsume the *post stacked* indicator. In columns (4) and (5) we include $region \times date$ fixed effects (at the *state* and CZ level) which capture changes in the local economic conditions around the revelation of intentional financial misrepresentations. Superscripts are not used to indicate statistical significance. All variables are defined in the Appendix.

Table 8: Financial Distress Upon Fraud Revelation - Spatial Dispersion

	Dependent Variable:		
	unpdcol	*unpdcol*	*unpdcol*
post sptl state	0.0643		
	(0.1168)		
post sptl CZ		0.1574	
		(0.2040)	
post sptl CT			0.7958
			(0.4125)
Data Aggregation	ZIP × Quarter	ZIP × Quarter	ZIP × Quarter
Weighted	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes
SE Cluster	CT	CT	CT
Observations	1,527,576	404,918	100,776
Adjusted R^2	0.9435	0.9533	0.9543

Tests in this table utilize data at the $ZIP \times quarter$ level. We analyze how the treatment effect disperses spatially when expanding the treatment assignment from the county (CT) level (default for the main analyses), to the CZ level ($\sim$ 700 commuting zones in the U.S.) and to the $state$ level (CT is nested in CZ and $state$ but CZ is not necessarily nested in $state$). The indicator variable *post sptl* equals one when there is a treatment within the respective spatial community at a given point in time (i.e., *post sptl CT* basically captures the treatment as in the main tests). We set *post sptl* to zero for the treated observations in a spatial community ex ante the treatment. We include $date$ and ZIP fixed effects and cluster standard errors at the CT level. Regressions are weighted by $numobs$. Superscripts are not used to indicate statistical significance. All variables are defined in the Appendix.

Table 9: Financial Distress Upon Fraud Revelation - Intensity

	Dependent Variable:	
	unpdcol	*unpdcol*
Cross section X equals	WARN	Finance
post	0.5257	0.4771
	(0.2544)	(0.2486)
post $\times$ X	0.4250	1.0156
	(0.5397)	(0.4489)
Date FE	Yes	Yes
Person FE	Yes	Yes
SE Cluster	CT	CT
Observations	122,732,436	122,732,436
Adjusted R^2	0.7548	0.7548

This table depicts analyses at the $person \times quarter$ level. We regress Debt in Collection ($unpdcol$, which is an indicator variable that equals 100 when an individual is flagged as having at least one debt sent to collection) on a $post$ indicator and on the interaction of $post$ with a cross section indicator variable X. The cross section X varies across the presented specifications and equals WARN (column (1)) and Finance (column (2)), respectively. The indicator variable WARN equals one when a firm in our sample issues a Worker Adjustment and Retraining Notification (i.e., the firm announces a large layoff event). Data on WARN come from the Big Local News initiative at Stanford University. Finance is an indicator variable that equals one when a firm in our sample is from the finance industry (i.e., Fama French 12 industry code 5). The baseline estimate for X is included in the regressions but not displayed in this table. Clustered, robust standard errors are in parentheses. Superscripts are not used to identify statistical significance.

Table 10: Ex Ante - Corporate Bankruptcies as Benchmark

Selection Step	Cases
Bankruptcies of publicly traded U.S. firms (AA matched to CCM)	1,019
./. Firms in fraud sample	927
./. Local economic importance: extremely large and small firms	688
./. Local economic importance: minimum spatial market share	450
./. Local economic importance: HQ not in county with population in top 1-pctl of U.S.	302
./. Bankruptcy filing/begin date in TransUnion sample period	269
./. Firms headquartered in fraud-sample counties	162

This table builds on data from Audit Analytics' (AA) database on corporate bankruptcies which are matched to the Compustat-CRSP merged (CCM) universe. We compile a dataset of corporate bankruptcies in the US because we are interested in whether financial decisions of individuals ex ante to the revelation of intentional financial misrepresentation (i.e., a firm overstates its economic condition) diverge from situations in which individuals observe a deteriorating signal by a locally economically important firm. We use corporate bankruptcies to proxy for this truthful & bad signal (ε in Figure 1; see also Section 2). To ensure that the underlying conditions are - everything else - comparable, we apply basically identical sample selection steps to the AA sample as we do for the sample of AAERs in our main analyses.

Table 11: Ex Ante - Credit Card Limits

	Dependent Variable:			
	cclimit			
Event type X equals	Control	Corp. Bkrpt	Acc. Fraud	(3) vs (2)
-36 *months to event* $\times$ X	0	0	0	
	-	-	-	
-33 *months to event* $\times$ X	0.0132	-73.1520	372.2758	[0.0364]
	(0.0285)	(125.1578)	(259.8390)	
-30 *months to event* $\times$ X	0.0042	-189.1482	621.8786	[0.0000]
	(0.0300)	(186.0082)	(354.5058)	
-27 *months to event* $\times$ X	0.0417	-73.1010	552.5096	[0.0153]
	(0.0447)	(193.5408)	(306.2503)	
-24 *months to event* $\times$ X	0.0284	-147.6663	478.0183	[0.0120]
	(0.0413)	(203.0022)	(204.8707)	
-21 *months to event* $\times$ X	-0.0252	-149.6814	701.8581	[0.0017]
	(0.0464)	(187.5738)	(221.9948)	
-18 *months to event* $\times$ X	-0.0342	-125.6617	536.2311	[0.0223]
	(0.0481)	(186.8502)	(257.2484)	
-15 *months to event* $\times$ X	-0.0073	-212.7471	587.6801	[0.0044]
	(0.0449)	(191.9649)	(284.6120)	
-12 *months to event* $\times$ X	0.0216	-144.1450	515.8637	[0.0233]
	(0.0371)	(221.0246)	(264.4237)	
-9 *months to event* $\times$ X	0.0360	-121.3058	547.2375	[0.0239]
	(0.0382)	(204.7134)	(281.9856)	
-6 *months to event* $\times$ X	0.0331	-237.6267	551.5805	[0.0088]
	(0.0418)	(229.2322)	(254.9046)	
-3 *months to event* $\times$ X	0.0886	-48.5498	370.8908	[0.1582]
	(0.0466)	(231.2243)	(210.6259)	
Data Aggregation	ZIP $\times$ Quarter			
Panel Structure	Stacked			
Weighted	No			
Date FE	Yes			
ZIP FE	Yes			
SE Cluster	CT			
Observations	30,721,176			
Adjusted R^2	0.7633			

The first three columns of this table depict estimates from *one* regression. We create a dataset of corporate bankruptcies to benchmark whether financial decisions ex ante to the revelation of intentional financial misrepresentations appear misinformed. We allocate and stack never treated communities as control observations to each event (i.e., corporate bankruptcy filing or fraud revelation) and keep each ex ante event period of 36 months per subdataset. We interact relative months indicators (with the baseline at -36) with a factor variable which contains the event-type (Control, Corp. Bankruptcy, Acc. Fraud). The outcome variable of interest in this test is the Credit Card Limit (*cclimit*) of current credit cards in USD, aggregated at the $ZIP \times quarter$ level. Superscripts are not used to indicate statistical significance. Clustered, robust standard errors are in parentheses. The square brackets in column (4) contain p-values for testing the difference between column (2) and column (3). All variables are defined in the Appendix.

Table 12: Financial Distress Upon Fraud Revelation × Ex Ante Decisions

	Dependent Variable:		
	unpdcol	*unpdcol*	*unpdcol*
post	0.5095	0.5085	
	(0.0171)	(0.0171)	
post × *z cclimit pre cgr*	0.0288	0.0286	
	(0.0158)	(0.0158)	
z cclimit pre cgr	0.0964	−0.0122	−0.0480
	(0.0517)	(0.0579)	(0.0580)
cclimit pre high		−0.4309	−0.4273
		(0.1032)	(0.1033)
months to treatment bin 0			0.2249
			(0.0146)
months to treatment bin 3			0.3051
			(0.0162)
months to treatment bin 6			0.3939
			(0.0175)
months to treatment bin 9			0.4934
			(0.0189)
months to treatment bin 12			0.5579
			(0.0204)
months to treatment bin 15			0.6995
			(0.0216)
months to treatment bin 18			0.7942
			(0.0232)
months to treatment bin 21			0.8366
			(0.0244)
months to treatment bin 24			0.9165
			(0.0256)
months to treatment bin 0 × *z cclimit pre cgr*			−0.0620
			(0.0138)
months to treatment bin 3 × *z cclimit pre cgr*			−0.0648
			(0.0153)
months to treatment bin 6 × *z cclimit pre cgr*			−0.0351
			(0.0165)
months to treatment bin 9 × *z cclimit pre cgr*			−0.0163
			(0.0178)
months to treatment bin 12 × *z cclimit pre cgr*			0.0725
			(0.0189)
months to treatment bin 15 × *z cclimit pre cgr*			0.1206
			(0.0198)
months to treatment bin 18 × *z cclimit pre cgr*			0.0775
			(0.0211)
months to treatment bin 21 × *z cclimit pre cgr*			0.1875
			(0.0219)
months to treatment bin 24 × *z cclimit pre cgr*			0.2073
			(0.0228)
Date FE	Yes	Yes	Yes
Person FE	Yes	Yes	Yes
SE Cluster	Person	Person	Person
Observations	122,732,436	122,732,436	122,732,436
Adjusted R^2	0.7548	0.7548	0.7548

This table depicts analyses at the *person* × *quarter* level. We regress Debt in Collection (*unpdcol*, which is an indicator variable that equals 100 when an individual is flagged as having at least one debt sent to collection) on a *post* indicator and on the interaction of *post* with *z cclimit pre cgr* (*z* indicates standardization). *cclimit pre cgr* measures the county average compound growth rate of *cclimit* over the fraudulent t_{-36} to t_{-3} period. Clustered, robust standard errors are in parentheses. Superscripts are not used to identify statistical significance. All variables are defined in the Appendix.

Online Supplement

Wiped Out? Financial Health of Individuals Affected by Accounting Fraud

Data on Financial Misrepresentations in Research

Table OS1: Data on Financial Misrepresentations in Research

Paper	Data
A. Agrawal and Chadha (2005)	LexisNexis for restatements, 2000 - 2001
Aharony, Liu, and Yawson (2015)	Litigation data from PACER, 2000 - 2007
Aitken, Cumming, and Zhan (2015)	Multicountry setting, suspected insider trading, 2003 - 2011
Alawadhi et al. (2020)	Enforcement actions for financial misrepresentation by the SEC and/or DOJ under provisions of Section 13(b) of the Securities and Exchange Act of 1934 from 1978 through 2017, for misconduct occurring from 1976 - 2014
Amiram et al. (2018)	review
Amiram, Bozanic, and Rouen (2015)	M-score approximation by Beneish (1999)
Antill (2022)	Moody's Ultimate Recovery Database (URD) for bankruptcy cases
Banerjee et al. (2023)	CFRM by Dechow et al. (2011), 1999 - 2015
Beatty, Liao, and Yu (2013)	SEC AAERs, 1999 - 2009
Bereskin, Campbell, and Kedia (2014)	SCAC, 1996 - 2011
Bernile and Jarrell (2009)	WSJ articles on misdated options
Blackburne and Quinn (2022)	SEC data recording all 14K investigations closed by the Division of Enforcement between 2000 and 2018 incl. information on whether the investigation resulted in an enforcement action
Blackburne et al. (2021)	FOIA requests to obtain investigations by the SEC that were closed (i.e. nonpublic) between 2000 and 2017
Bowen, Call, and Rajgopal (2010)	Whistleblowing cases: LexisNexis, requests at OSHA, 1989-2004
Bowen, Dutta, and Zhu (2018)	restatement data from AA, AA provides distinction between error, fraud, and GAAP misapplication, 2001 - 2014
Call, Kedia, and Rajgopal (2016)	SCAC, 1996-2011
Call et al. (2018) ("CMSW")	**AAERs and additional sources (observations in online appendix; see Karpoff et al. (2017) for sample composition), whistle-blowing information via FOIA, 2002-2010 (we utilize CMSW' replication package for identification of intent in our sample of AAERs)**
Carnes, Christensen, and Madsen (2023)	AAERs from CFRM, 1985-2009
Cecchini et al. (2010)	AAERs from SEC website, 1999-2005
Chapman-Davies, Parwada, and Tan (2013)	fraud committed by mutual fund managers, Disclosure Reporting Pages (DRPs) within the SEC's Investment Adviser Public Disclosure (IAPD) database.
Chen et al. (2016)	Chinese setting, China Centre for Economic Research (CCER/Sinofin) or China Stock Market and Accounting Research (CSMAR), 1999 - 2008
Cheung and Chan (2008)	Corruption Perception Index
S. Choi and Kahan (2007)	Wallstreet Journal Website for mutual funds scandals, 1994-2004

Paper	Data
H. M. Choi et al. (2023)	regulatory enforcement actions initiated by the SEC and DOJ, 1978 - 2015, see Karpoff, Scott Lee, and Martin (2008)
J. H. Choi and Gipper (2024)	AAERs from CFRM between 1989 and 2008
Chy, Khurana, and Kyung (2022)	Implementation of MMoU across countries
Coleman et al. (2021)	FOIA requests to identify when the SEC denies FOIA requests because of ongoing enforcement proceedings (2006-2016)
Cumming, Dannhauser, and Johan (2015)	review
Cumming, Leung, and Rui (2015)	Chinese setting, detected fraud cases, 2001 - 2010
Cumming, Peter Groh, and Johan (2018)	European setting; ESMA country-level report for Market Abuse Directive
Deason, Rajgopal, and Waymire (2015)	SEC investigations into Pyramid schemes, SEC website and LexisNexis, 1988-2012
Dechow et al. (2011)	**AAERs from LexisNexis and SEC: CFRM, data updated here with coverage 1982 - 2021**
Defond, Francis, and Hallman (2018)	AAERs issued against auditors
Dimmock, Gerken, and van Alfen (2021)	FOIA requests, Meridian IQ, BrokerCheck for data on misconduct by financial advisors, 1999-2013
Donelson, Glenn, and Yust (2022)	D&O insurance data and tax litigation cases from SCAC 1996-2016
Donelson et al. (2021)	SCAC, CFRM from Dechow et al. (2011), restatement data from AA, GAO data on irregularities Hennes, Leone, and Miller (2008), “sample differs significantly from Karpoff et al. (2017)”, 1998 - 2014
Dyck, Morse, and Zingales (2010)	SCAC 1996 - 20004, filters to reduce influence of frivolous cases: large domestic firms, exclude case dismissals, when settled at least \$3m, exclude IPO underwriter allocation cases (& more)
Dyck, Morse, and Zingales (2023)	Dyck, Morse, and Zingales (2010) & Dechow et al. (2011)
Efendi, Srivastava, and Swanson (2007)	GAO data from 1997 - 2002
Egan, Matvos, and Seru (2019)	FINRA individual/broker misconduct information (publicly available) at brokercheck 2005 - 2015
Egan, Matvos, and Seru (2022)	see Egan, Matvos, and Seru (2019)
Erickson, Hanlon, and Maydew (2004)	AAERs between 1996 and 2002
Erickson, Hanlon, and Maydew (2006)	see Erickson, Hanlon, and Maydew (2004)
Field, Lowry, and Shu (2005)	SCAC 1996-2000
Giannetti and Wang (2016)	see Karpoff, Lee, and Martin (2017), 1989 - 2009
Glaeser and Saks (2006)	Federal Corruption Convictions in the US: Justice Department’s “Report to Congress on the Activities and Operations of the Public Integrity Section”, waves: 1989, 1999, and 2002
J. Graham, Li, and Qiu (2008)	GAO, 1997-2002, subsection on fraud
Gurun, Stoffman, and Yonker (2018)	FOIA requests for SEC Form ADV; Madoff’s client list here; FDIC branch-level deposit data
Hail, Tahoun, and Wang (2018)	Multicountry setting, newspapers, 1800 - 2015
Heese and Pacelli (2023)	Violation Tracker, 2000-2017
Heese and Pérez-Cavazos (2020)	Violation Tracker, 2000-2017
Heese, Pérez-Cavazos, and Peter (2022)	Violation Tracker, 2000-2017
Heese and Pérez–Cavazos (2019)	Whistleblower events and litigation, 2000-2012
Hennes, Leone, and Miller (2008)	GAO data, distinguish between intentional misstatements and errors

Paper	Data
Holzman, Marshall, and Schmidt (2024)	see Blackburne et al. (2021)
Holzman, Miller, and Williams (2021)	see Dechow et al. (2011)
Huang, Roychowdhury, and Sletten (2020)	firm HQ in the Ninth Circuit, 1995-2003
Jennings, Kartapanis, and Yu (2021)	AAERs from CFRM, 1998-2012
Leuz et al. (2017)	Proprietary data on pump-and-dump schemes in German markets from the German Financial Supervisory Authority and handcollected touts from German websites and internet forums, 2002-2015
Karpoff (2021)	review
Karpoff et al. (2017)	**Comparison of GAO, AA, SCAC, CFRM: handcollected subset of enforcement actions by SEC and DOJ which includes charges of fin. misrepresentation under Sec. 13 (b)**
Karpoff, Lee, and Martin (2008)	LexisNexis FEDSEC:SECREL & FEDSEC:CASES libraries, SEC website, DOJ, DOJ's corporate fraud task force website, 1978-2002
Karpoff, Scott Lee, and Martin (2008)	LexisNexis FEDSEC:SECREL & FEDSEC:CASES libraries, SEC website, DOJ, DOJ's corporate fraud task force website, 1978-2002
Karpoff and Lou (2010)	see Karpoff, Lee, and Martin (2008), 1988-2005
Karpoff, Lee, and Martin (2017)	all enforcement actions initiated by the SEC and DOJ from January 1, 1978 through May 31, 2013 for foreign bribery under the Foreign Corrupt Practices Act of 1977; data from CCH Wolters Kluwer and the PACER database
Kedia and Rajgopal (2011)	see Karpoff, Scott Lee, and Martin (2008) 1976–2009
Kempf and Spalt (2022)	SCAC 1996-2017
Klimczak et al. (2022)	Case study
Khanna, Kim, and Lu (2015)	see Karpoff, Lee, and Martin (2017) 1996-2006
Marangoni (2021)	bribery: FCPA enforcement actions, website data from DOJ and SEC, 1978 - 2019
Mehta and Zhao (2020)	see Karpoff, Lee, and Martin (2008) & Karpoff et al. (2017) 2001-2010
Miller (2006)	SEC AAER, SEC website
Mućko and Adamczyk (2023)	setting in Poland
Nguyen (2021)	annual country-level corruption control indicator published by the World Bank, 1996-2018
Pittman and Zhao (2020)	restatement data from AA, 2000 - 2013
Raghunandan (2021)	Violation Tracker
Wang, Winton, and Yu (2010)	SEC AAER & SCAC, proxy for detected IPO fraud: filing of a securities lawsuit against an IPO firm for alleged financial misreporting during the IPO process, 1996 - 2007
Yue, Zhang, and Zhong (2022)	information for bank enforcement actions from Standard & Poor's (S&P) Global Market Intelligence's SNL Bank Regulatory dataset

Non-exhaustive list of data-sources in the literature. Most commonly applied: Government Accountability Office (GAO) database of restatement announcements; Audit Analytics (AA) database of restatement and non-reliance filings announcements; Securities Class Action Clearinghouse (SCAC) database of securities class action lawsuits; the Center for Financial Reporting and Management (CFRM; see Dechow et al. (2011)) database of Accounting and Auditing Enforcement Releases (AAERs).

ZIP Aggregated Data - Debt in Collection

[Figure OS1 about here.]

We utilize consumer credit data at the $person \times quarter$ and the $ZIP \times quarter$ levels throughout the study. For the analyses at the $ZIP \times quarter$ level, we consider the average of a variable (e.g., Debt in Collection (*unpdcol*)) across all individuals who have their place of abode in a ZIP at a given point in time (an individual's ZIP is included in TU's summary statistics at the $person \times quarter$ level). We visualize how raw data for our indicators of financial distress develop ex ante and ex post to the treatment (i.e., revelation of intentional financial misrepresentation at $t = 0$) in the main body of the study (e.g. see Figure 5 for *unpdcol*). We are interested in whether we observe consistent patterns when we aggregate the data at $ZIP \times quarter$ level and, therefore, replicate the visualization in Figure OS1. We indeed find that the aggregated data develop with significant overlap to what we observe in the main analysis. From this we conclude that our analyses at the $ZIP \times quarter$ level in the main body of the study gain additional credibility. In untabulated tests we also estimate equation (1) at the $ZIP \times quarter$ level with *unpdcol* as dependent and *post* as independent variables of interest. We include *date* and ZIP fixed effects, weigh the regression by *numobs*, and cluster standard errors at the CT level. The coefficient estimate for *post* is basically identical to what we observe in Table 3 (β_1 of 0.8566 with a p-value of 0.0436).

Event Study

[Table OS2 about here.]

Our main analyses generally employ canonical DiD designs in which indicators for *post* and relative *months since treatment* are estimated against a single baseline reference indicator ("bin"). We do this for the tests at the $person \times quarter$ level because we employ a treated vs. not yet treated research design in which we keep all pre-treatment observations to ensure that the *date* fixed effects can be estimated on a sufficiently large number of observations at each point in time. Furthermore, relative month indicators ex ante to the treatment would be unbalanced for observations which are treated rather late in the sample period (because TU data are not consistently available ex ante to early treatments). For an alternative design of the analysis, we perform a fully specified event study. We limit the $person \times quarter$ data to a symmetrical $\pm$ 24 months period around the treatment. We estimate equation (1) with *unpdcol* as dependent variable and include relative months indicators ex ante and ex post to the fraud revelation (with a reference baseline at the ultimate quarter before the revelation). We include *person* and *date* fixed effects and cluster standard errors at the *CT* level. Table OS2 depicts the results for this specification (note that the pre-treatment coefficients are included in Figure 8 in the main body of the study). We observe similar and consistent evidence with our main specification, for which results are presented in Table 3. We conclude that this finding corroborates our regression design choice for the main analysis. The findings from the event study also support the parallel trend assumption (see Figure 8).

AAER - Identifying Intent

[Figure OS2 about here.]

We provide a short description of how we separate between error and intent among the cases/AAERs of financial misrepresentation in Section 4, where we note that we build regular expressions on the most common tokens in a training set of AAERs and perform pattern matching among the to-be-classified/remaining/test AAERs. Figure OS2 provides additional details on how we identify intent. We count matched patterns for the training and test AAERs separately and plot distributions of these counts against each other in Panel A of the figure. One can observe that the test AAERs include a substantial amount of cases which are not described by tokens that are representative of intentional financial misrepresentation because the distribution (in comparison to the training AAERs) is skewed towards zero. For the classification of intent, we, therefore, require a minimum threshold of pattern matches in the test AAERs. We choose the first decile of matches in the training AAERs as threshold because this leads to a convergence of the means of the two distributions (see also Panel B of Figure OS2). AAERs/cases which are either (i) classified as intentional/fraudulent by Call et al. (2018) or (ii) surpass the threshold of tokens that are representative of intent are included in our sample as cases of intentional financial misrepresentation.

Ex Ante - Textual Sentiment of News

In Section 2.2 we discuss how intentional financial misrepresentation may impact individuals and their financial decisions ex ante to the fraud's revelation. One aspect that could be particularly important in this process is the (characteristics of) news because news is an important source of information (e.g., see Bushee et al. (2010)). Generally, Dyck, Morse, and Zingales (2010) document that news, even though it "do[es] not own any residual claim in the firms", plays a key role in fraud detection. Broader evidence on news, however, documents that firms can actively manage media coverage (Ahern and Sosyura 2014) and that local news "hype" businesses (i.e., firms receive positive textual sentiment) as quid pro quo for advertising expenditures (Gurun and Butler 2012). Since textual sentiment is shown to affect perceptions of recipients as they transform information into new knowledge (Pentina and Tarafdar 2014; Tan, Ying Wang, and Zhou 2014), we are interested in whether fraudulent firms receive rather positive textual sentiment ex ante to the fraud's revelation. Therefore, we collect news coverage of fraudulent firms from NexisUni. We apply search criteria, which basically mimic those in Shapiro, Sudhof, and Wilson (2022) ("SSW"): within the "News" classification by NexisUni, we focus on articles that are written in "English" and for which the location is coded as "United States" (note that SSW are interested in generic news on the U.S. economy while we focus on firm-specific news). We run an individual search per case and, consistent with our ex ante revelation tests, focus on news in the range of $[-36; -3]$ months to the fraud's revelation date. Further, we respectively include cases for which our manual news-based verification of revelation dates indicates a range of $\pm$ 100 days to the revelation dates in our main analysis (see Section 4 and Footnote[21]). Limiting our searches to the 1,000 most relevant (as indicated by NexisUni) hits per case, we analyze a corpus of 22,088 documents. We apply the bag-of-words by Loughran and McDonald (2011) ("LM") to measure *Textual Sentiment*:

$$Textual\ Sentiment = \frac{n_{pos} - n_{neg}}{N} \tag{OS1}$$

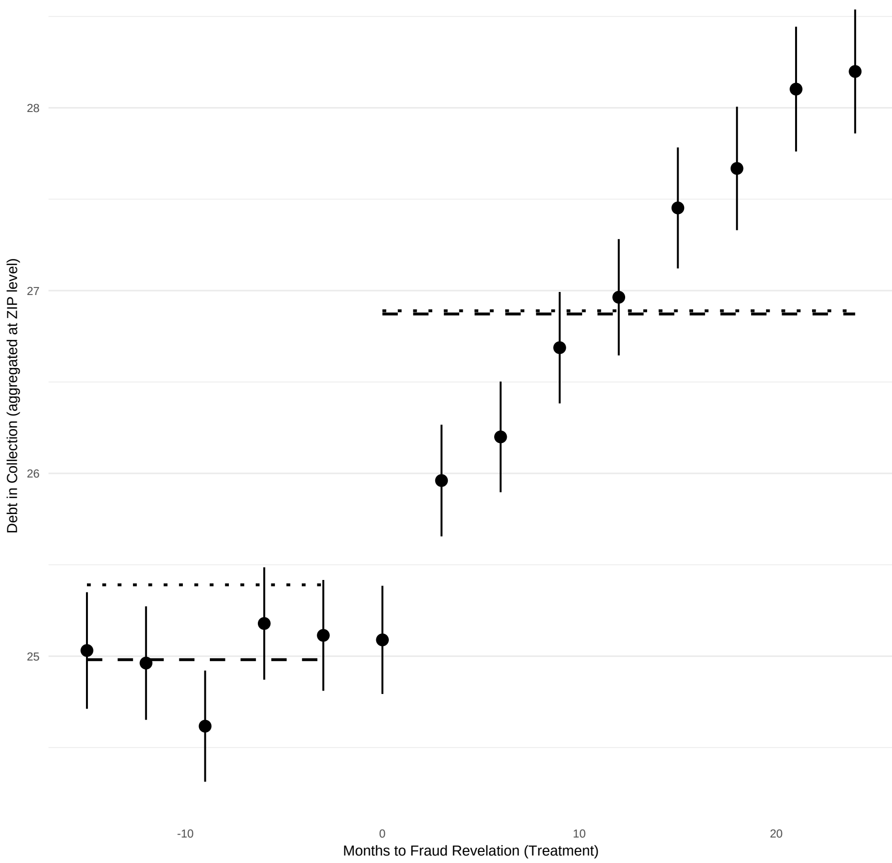
The terms n_{pos}, n_{neg}, and N in equation (OS1) refer to counts of positive, negative, and total words with n_{pos} and n_{neg} indicating inclusion in the bag-of-words by LM (a measure that SSW, for instance, also refer to as "net positivity"). We also follow the suggestion by SSW to include a negation-rule (i.e., we reverse sentiment of terms that follow "not").

[Figure OS3 about here.]

Analyzing whether fraudulent firms eventually receive rather positive textual sentiment in news ex ante to the fraud's revelation requires a benchmark. We decide to utilize the daily measure of textual sentiment of news by SSW for comparison and match this information to our corpus through the news' publication dates. In essence, SSW's measure captures the (expected) textual sentiment of generic news on the U.S. economy on any given date.[OS1] It is built on multiple bag-of-words (including the bag-of-words by LM) and follows the same interpretation as the measure that we apply: higher values indicate more positive textual sentiment. Benchmarking the textual sentiment of our news corpus against the generic measure by SSW, thus, allows us to measure abnormal (positive) sentiment for fraudulent firms. To ensure comparability of the two measures, we rescale both variables at a new minimum of -1 and a new maximum of 1. For illustration, we depict density plots of the textual sentiment as measured in our corpus (solid line) and by SSW (dashed line) in Figure OS3. The vertical lines mark the averages of the two measures. We then compare the average textual sentiment in our news corpus to the generic measure by SSW and find a statistically significant higher average of textual sentiment for the fraud-firm-news. We conclude that this evidence suggests that fraud firms receive rather positive textual sentiment in news ex ante to the fraud's revelation. Consequently, positive textual sentiment in news on fraudulent firms could be a relevant channel through which individuals receive and process information about these firms.

[OS1] Data by Shapiro, Sudhof, and Wilson (2022) are publicly available at frbsf.org/economic-research/indicators-data/daily-news-sentiment-index.

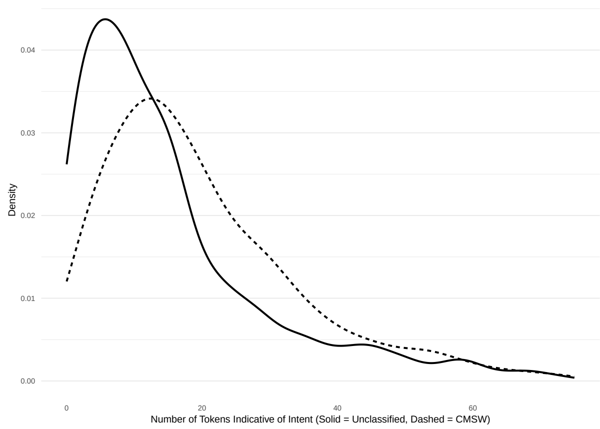
Figure OS1: Debt in Collection (ZIP × Quarter)



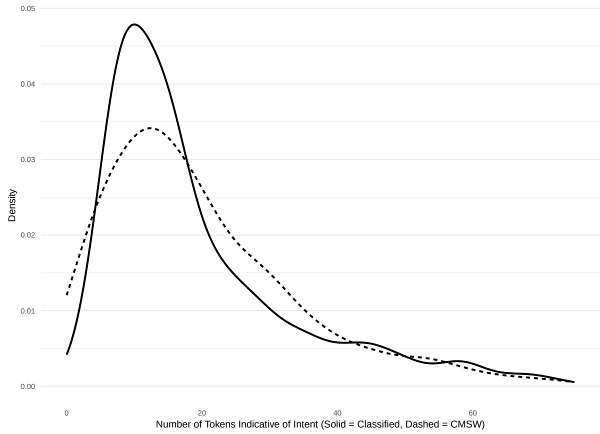
This figure mirrors Figure 5 but utilizes aggregated data at the $ZIP \times quarter$ level. The dots represent the average of Debt in Collection ($unpdcol$) among the treated communities at the relative point in time against the treatment (revelation of intentional financial misrepresentation) in $t = 0$. Vertical lines mark the 95% confidence intervals. The horizontal dashed lines mark the multiperiod averages of $unpdcol$ (here $[-15; -3]$ and $[0; 24]$). The horizontal dotted lines mark the averages which are weighted by $numobs$.

Figure OS2: AAER - Identifying Intent

Panel A: Unclassified AAERs

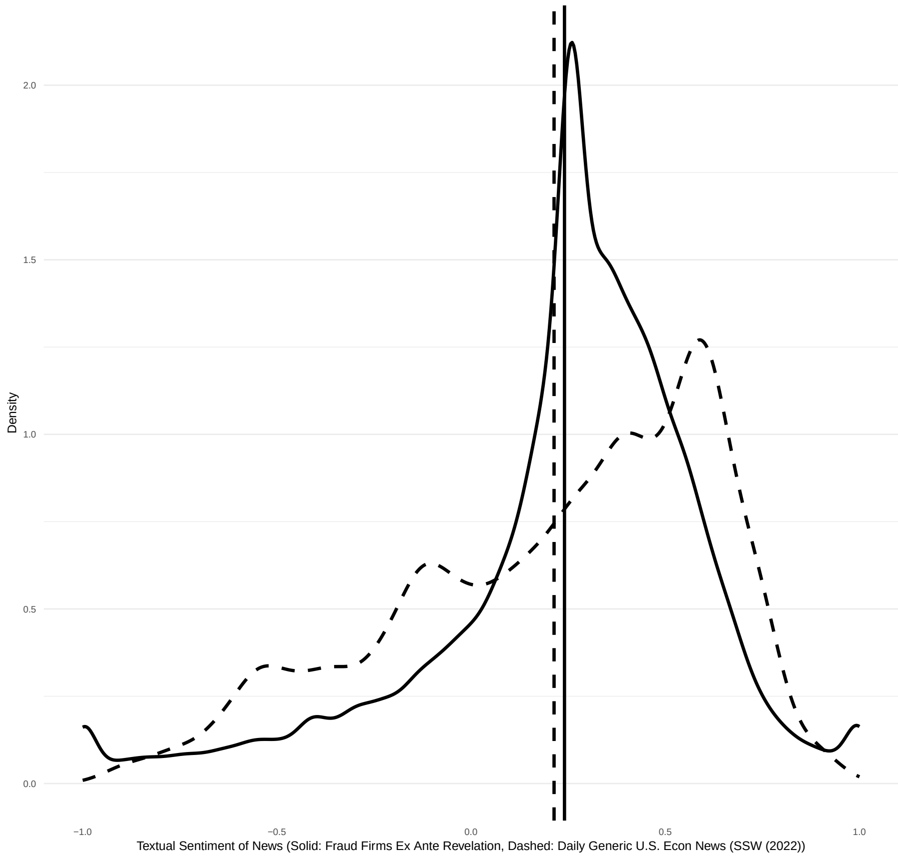


Panel B: Classified AAERs



This figure depicts distributions of tokens in the AAERs which relate to *intentional* financial misrepresentation before classification (Panel A) and after classification (Panel B). The dashed lines (identical in Panels A and B) relate to the training/true set of AAERs which we identify as cases of $fraud$ in CMSW (2018). The solid lines relate to the AAERs which we (need to) classify based on the most common action terms in the training set. We derive regular expressions from these action terms and use them to classify the remaining/test AAERs in our dataset (see Figure 2 for a word cloud of the most common action terms). We apply a minimum threshold (p10) in the number of identified tokens from regular expression searches in the test AAERs to separate between error and intent. Note that the displayed distributions are cut off to the right.

Figure OS3: Ex Ante - Textual Sentiment of News



This figure depicts density plots of textual sentiment of news. The solid line depicts the textual sentiment (measured by applying the bag-of-words by Loughran and McDonald (2011)) of firm-specific news on fraudulent firms ex ante to the fraud's revelation. The solid line depicts the textual sentiment (as measured by Shapiro et al. (2022) (SSW)) of generic news on the U.S. economy. We merge the daily measure by SSW to the fraud-firm-news based on the publication dates. To ensure comparability of the two measures, we rescale both measures at a new minimum of -1 and a new maximum of 1. The vertical lines mark the averages of the two measures.

Table OS2: Financial Distress - Event Study Design

	Dependent Variable:
	unpdcol
-24 *months to treatment*	-0.0767
	(0.1445)
-21 *months to treatment*	-0.1254
	(0.1368)
-18 *months to treatment*	-0.1194
	(0.1056)
-15 *months to treatment*	-0.1071
	(0.0935)
-12 *months to treatment*	-0.0450
	(0.0836)
-9 *months to treatment*	-0.0563
	(0.0663)
-6 *months to treatment*	-0.0153
	(0.0414)
-3 *months to treatment*	0
	-
0 *months since treatment*	0.0412
	(0.0542)
3 *months since treatment*	0.1088
	(0.0549)
6 *months since treatment*	0.1954
	(0.0709)
9 *months since treatment*	0.2803
	(0.0874)
12 *months since treatment*	0.3842
	(0.1053)
15 *months since treatment*	0.5425
	(0.1136)
18 *months since treatment*	0.5791
	(0.1295)
21 *months since treatment*	0.6885
	(0.1711)
24 *months since treatment*	0.7823
	(0.2019)
Date FE	Yes
Person FE	Yes
SE Cluster	CT
Observations	58,842,435
Adjusted R^2	0.8304

This table depicts estimates from an event study at the $person \times quarter$ level in which we regress Debt in Collection (*unpdcol*) on relative *months to/since treatment* indicators. The sample for this analysis is limited to a symmetrical period of $\pm$ 24 months around the treatment in $t = 0$. Coefficient estimates and 95% confidence intervals (based on standard errors clustered at the *CT* level) are depicted in the upper Panel of Figure 8 for the pre-treatment periods. Superscripts are not used to indicate statistical significance. All variables are defined in the Appendix.

Press Coverage of Tax Reforms and Interest Groups*

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Press Coverage of Tax Reforms and Interest Groups

Abstract

I investigate the influence of interest groups on the textual sentiment of press coverage of tax reforms. Through 2SLS and OLS estimates and the application of a modified control function approach, I identify increases in the differences among represented opinions (subjectivity effect). This subjectivity effect translates into relative increases in both positive and negative textual sentiment within an article. Additional tests reveal that interest groups affect articles' textual sentiment on the negative margin. Furthermore, I find that staleness of texts increases with the appearance of interest groups. Taken together, these results suggest that articles are not balanced but ambiguous when interest groups appear in them, indicating that interest groups particularly provide redundant information. Overall, I elucidate the press's role as an information intermediary and document how seeking influence on (tax) regulation is performed when interest groups' activities are nearly impossible to regulate.

- Working Paper -

Keywords: Tax Reform, Press Coverage, Interest Groups, Textual Sentiment

Data Availability: Sources identified in the text

Declaration of Interest: The author declares no conflict of interest

1 Introduction

News is essential to elevate transparency. Individuals, for instance, often do not directly experience changes to public policy but learn about them from the news (S. Soroka and Wlezien 2019). Exerting influence over the way that individuals eventually perceive the presented information, therefore, allows for influence over the political discourse (Strömberg 2004). Such influence imposes an important source of political power (Gilardi et al. 2021) and should become a strategic objective for those with special interests, namely interest groups. Therefore, this study asks whether interest groups influence the textual sentiment of press coverage of tax reforms, which are prominent in both news reporting and special interest seeking. As such, my analysis targets our understanding of how information are generated and distributed by intermediaries (news/media/*the press*) when interacting with other economic actors. I utilize a tax setting to, furthermore, address the ongoing discussion regarding transparency mandates in policy.

Calls for transparency, which received special attention after businesses paused or terminated their political spending in the aftermath of the Capitol riots of 2021 (Hirsch Jan/11/2021; Vigdor and Paybarah Jan/11/2021), typically center around immediate interactions between politicians and interest groups (i.e., inside tactics) and aim to mitigate the tension between providing expertise and seeking strategic influence by interest groups. If, however, outside tactics, through which interest groups publicly seek to influence policy outcomes, are important for realizing policy goals (as suggested by survey and interview evidence from Chalmers (2013); see also Bruycker and Beyers (2015)), then transparency mandates on inside tactics could ultimately be undermined and rendered less effective. One key challenge for an analysis, however, is to explore interest groups' outside tactics concerning economically significant policy actions and how these tactics determine an outcome variable of interest that, in turn, may influence a policy's outcome. Hence, Becker, Bischof, and Daske (2021) stress that endeavors to influence policy decisions remain obscure. I address this challenge by analyzing tax reforms (in Germany), press coverage of these reforms in quality outlets, interest groups' appearances in the articles, and their influence on the articles' characteristics, such as textual sentiment.

Textual sentiment captures the (un-)intended and latent value assignment of the underlying corpus. It is shown to distort the perceptions of recipients as they transform acquired information into new knowledge (Pentina and Tarafdar (2014); Tan, Ying Wang, and Zhou (2014); McCluskey, Swinnen, and Vandemoortele (2015)). Some exemplary consequences of these (mis)perceptions include biases in investor decisions, company strategies, stock markets, employment choices, investments in education, voting, tax evasion, and political trust (Abo-Zaid (2014); Ceron (2015); Blaufus et al. (2020); Crabtree et al. (2020); Mason, Utke, and Williams (2020)). Boydstun, Highton, and Linn (2018) highlight that textual sentiment "of economic news coverage has an independent, direct connection with economic attitudes." Importantly, the media's concentrated and emotionally based coverage is consistently found to influence public policy by putting pressure on decision makers ("CNN effect", e.g., see McCluskey, Kalaitzandonakes, and Swinnen (2016) and Robinson (2005)). In essence, these insights suggest that interest groups, when they successfully influence the textual sentiment of tax reform press coverage, can influence the political discourse with an outside tactic that is nearly impossible to regulate.

While tax reforms represent a specific form of political intervention, they are particularly suited for analysis in the context of press coverage and interest groups. Tax policy measures are associated with broad economic and social consequences. Consequently, tax reforms regularly affect large groups and thus provide a good fit for the incentives of outlets to provide news in accordance with their increasing return-to-scale advertising financing. Boydstun (2013) concludes that "nothing is certain on the front page, we might say, except death, taxes, sports and snowstorms on the eastern seaboard." Analyses of press coverage, however, reveal a significant amount of ideological heterogeneity across news outlets in the U.S. (Groseclose and Milyo 2005; Gentzkow and Shapiro 2010; Mankiw 2014; Puglisi and Snyder 2015). Thus, measures of textual sentiment could depend on the selection of specific U.S. outlets. This raises calls for the analysis of textual sentiment in non-U.S. outlets (Figueiredo and Richter 2014; Kearney and Liu 2014; Bombardini and Trebbi 2020). Friebel and Heinz (2014) show that outlet-selection bias is diminished when German outlets are analyzed and conclude that German quality outlets rather slant their articles in the same direction. Therefore, I analyze the press coverage of tax reforms in Germany. The press's important role in reform processes in Germany is further underscored by the findings of McLeay, Ordelheide, and Young (2000). The authors stress that setting (accounting) standards in Germany is not only viewed as a technical matter in which a group of experts should hold competence but also as an issue of public policy with a relatively broad social basis. This further renders my analysis to be of broad and general interest.

To address my research question, I construct a sample of tax reforms from German parliament sources between 1990 (German reunification) and 2019 (just before the impact of COVID-19 on politics and news) and apply natural language processing methods to the hand-collected documents to gain insights into the content of these tax reforms. Based on word frequencies (counts and term frequency inverse document frequencies) and the given titles of the reforms, I identify keywords that are representative of their content.[1] I utilize these keywords as search terms to manually collect press coverage from the two German quality outlets Frankfurter Allgemeine Zeitung ("FAZ") and Süddeutsche Zeitung ("SZ"). This allows me to capture the press's presentation of tax reform related content that surrounds a tax reform. I identify ~10,000 articles that align with the topics and content of 140 tax policy interventions. I measure the articles' textual sentiment and identify the articles in which interest groups appear. I then estimate the effect of an interest group's appearance in an article on its textual sentiment.

My primary result is that an interest group's appearance in an article is associated with its textual sentiment. In particular, I identify increases in the differences among the represented opinions (subjectivity effect). This subjectivity effect translates to relative increases in both the positive and the negative textual sentiment within an article. Generally, this finding aligns with the survey-evidence presented by Call et al. (2022), who state that journalists have strong incentives to develop high-quality articles that trade-off heterogeneous views. Andrews and Caren (2010) consistently highlight that interest groups that are strongly confrontational gain relatively little media attention, even though their incentives to intervene become pronounced when they take opposing positions (see also Bruycker and Beyers (2015)). I find that this effect is robust to using the accounting-related bag-of-words by Bannier, Pauls, and Walter (2019) and the politics-related bag-of-words by Rauh (2018) when measuring textual sentiment. Importantly, the evidence is also robust to controlling for appearances of other economic actors. Specifically, I collect the names, party affiliation, and political leaning of all members of the German federal parliament since 1990. I identify appearances of members of parliament in the articles and then construct a variable that controls for the appearance of right over left political leaning in the article.

I derive at the above findings by employing 2SLS and OLS estimates and under application of a modified control function. Given that the underlying estimators identify different treatment effects (e.g., LATE for 2SLS), I particularly focus on the direction of evidence, which

[1]Note that the tax reforms in my sample are not limited to specific tax reforms with a particular recognizable shift in the tax system (such as the TCJA in the U.S.). Instead, I strive to cover the multitude of tax policy interventions over time. For simplicity, I use the term "tax reform" to refer to these policies.

is consistent across the entire set of estimates. For the 2SLS analysis, I implement an instrumental variable strategy. I construct the instrumental variable, which must address both the effort of interest groups and the choices of journalists, by drawing on the suggestions of Georgiou (2004), who indicates that while effectiveness is perceived as diminishing over the course of a reform, interest groups typically engage in the process rather late and only once they see the likelihood of the new regulation being passed as being sufficiently high. I conduct an interview with a tenured journalist at a German quality outlet who not only highlights that interest groups approach journalists ("we are inundated with such input") but also stresses that articles in the daily press must be written in a very timely manner while relying on multiple sources. I conclude that if interest groups accelerated their effort to appear in the press somewhat shortly before the decision on a tax reform (i.e., the tax reform's "date"), they would be more likely to be chosen by journalists to appear in the respective articles. I test this notion by exploiting the plausibly exogenous time period between articles' publication dates and the date of a tax reform and find consistent patterns in the analyzed German press coverage: the appearance of interest groups in articles is especially pronounced in the weeks preceding a decision on a tax reform. The instrumental variable is an indicator variable that equals one for this exogenous time frame per tax reform. I find that the instrument is a relevant predictor of interest groups' appearances in articles and also assess whether journalists would confoundingly include (different actors to incorporate) different perspectives shortly prior to a tax reform's date. Additionally, I employ an alternative instrumental variable, a tax reform's budget impact, and find evidence that is consistent with the main 2SLS analysis.

For the modified control function approach (MCF), I employ the technique by Klein and Vella (2010), who present a control function estimator for models where it is difficult to find sufficiently powerful instruments that satisfy the exclusion restriction. MCF also found recent application in Armstrong, Nicoletti, and Zhou (2022), who highlight its strength in settings with two-sided endogeneity. In essence, the MCF approach relies on constructing a variable that controls for the endogenous relation using information about the unobserved variables (which are captured by the residuals) under heteroskedasticity. When controlling for this newly constructed variable, I identify consistent evidence with the 2SLS and OLS estimates. Taken together, 2SLS-, OLS-, and MCF-estimates for interest groups' press appearances are all positive and statistically significant when I analyze articles' subjectivity.

Since the main analysis points to differences in represented opinions in articles, I also investigate how interest groups contribute to these differences. For instance, an article could feature opposing and supporting views of different interest groups. In a first set of tests, I substitute the indicator variable from the main analysis with factors of a count variable and find that the subjectivity effect increases with the inclusion of additional interest groups within an article. While this could suggest that the identified main effect is attributable to the appearance of several interest groups, regressions results suggest that, in particular, it is an article's negativity that increases with multiple interest group appearances. I triangulate these findings by analyzing sections ("quasi-sentences") of the articles in which interest groups appear (a ± 1 sentence-window around the sentence in which an interest group is identified). I contrast the measures of textual sentiment contained within these quasi-sentences to those of the entire articles. It descriptively appears that quasi-sentences of interest groups within articles are particularly negative. Therefore, I rerun the main analysis with the IG-sections-narrowed measures of textual sentiment as dependent variables (for those articles in which interest groups appear). The results indicate that the appearance of interest groups on average influence the textual sentiment of these articles on the negative margin. Overall, this downward shift appears to be mitigated by the inclusion of differentiated views and perspectives, which explains the overall identification of the subjectivity effect. Importantly, this "balance", while in line with journalists' incentives to incorporate heterogeneous views (Call et al. 2022), can also introduce ambiguity. With the influence of interest groups persistently leaning towards the negative margin, press coverage of interest groups can lead to a inclusion of perspectives that might not necessarily reflect the actual uncertainty or controversy surrounding a topic (e.g., see Shapiro (2016)).

To provide empirical support for the latter notion, I analyze how stale the entire sample of articles and the quasi-sentences of interest groups are vis-à-vis each other. Staleness of text is considered to represent redundant information (Tetlock 2011). Since interest groups affect articles' textual sentiment persistently on the negative margin, their impact could particularly introduce ambiguity if they provided redundant information (i.e., if their quasi-sentences were particularly stale). I construct two textual staleness measures (Jaccard similarity and cosine similarity) for all articles against each other and for all quasi-sentences against each other. First, I verify whether these measures actually capture staleness by demonstrating that the similarity of articles and quasi-sentences increases within reforms (capturing the idea that articles that belong to one reform are likely to be more similar). Next, I test whether staleness is higher for quasi-sentences in comparison to the staleness for the articles and find consistent support for both applied measures and across multiple subsamples. Thus, staleness of texts increases with interest groups appearances, indicating redundant information by interest groups, inducing ambiguity rather than balance.

My study contributes to the literature which enhances our comprehension of the political process by which (tax) regulations are instituted (e.g., see Bischof, Daske, and Sextroh (2020)). Strategic interventions by interest groups are often considered successful (Mulligan and Oats 2016; Barrick and Brown 2019) and to have important economic consequences (Bertrand et al. 2020; Huneeus and Kim 2020). With regard to outside tactics, Bruycker and Beyers (2015) and Chalmers (2013) indicate that interest groups rely on political attention generated by the media. Taxes, furthermore, receive substantial press and front page attention (Boydstun 2013) and are at the core of special interests (Richter, Samphantharak, and Timmons 2009; Kerr, Lincoln, and Mishra 2014; Kim and Zhang 2016; Lin et al. 2018; Bertrand et al. 2020). Taken together, these insights suggest that interest groups are incentivized to strategically influence articles' characteristics, such as textual sentiment. Yet, Barrick and Alexander (2014) and Becker, Bischof, and Daske (2021) highlight that we know little about how seeking influence on (tax) regulation is actually *performed* (for a literature review of the *effects* of influence seeking see Gipper, Lombardi, and Skinner (2013)). I focus on interest groups' outside tactics and empirically document how their press appearances determine an outcome variable of interest that can impact policy (see also Strömberg (2004) for analytical evidence on how the media biases policy). Analyzing interest groups as a determinant of the textual sentiment of press coverage, thus, directly speaks to how interest seeking in tax reforms is performed. Thereby, my study also responds to the call by Gipper, Lombardi, and Skinner (2013), expanding beyond the conventional focus on comment letters in empirical research on strategic influence.[2]

Furthermore, my study contributes to a better understanding of the role of the press as an information intermediary in an economic context (Chen, Schuchard, and Stomberg 2019; Rees and Twedt 2022). In particular, my analysis enhances our understanding of how news outlets transpose information on tax reforms to the public. While Baker, Bloom, and Davis (2016) analyze press coverage to show that taxes significantly explain economic policy uncertainty, Bushee et al. (2010) indicate that articles generally reduce information asymmetries (see also Dai, Shen, and Zhang (2020)). Articles represent a flexible information source and

[2]Gipper, Lombardi, and Skinner (2013) underscore that "in spite of a relatively large volume of research on comment-letter lobbying in the standard-setting process, what we know is limited" (p. 544).

capture insiders' and outsiders' views and perspectives (Kearney and Liu 2014; Coyne, Kim, and Kim 2020). They also provide distinctive types of information that allow us to deepen our understanding of broad economic, political, and historical developments (Teoh 2018). However, Bybee et al. (2020) note that although articles reflect information that consumers rely on to make allocation and consumption choices, their structure remains opaque. My analysis elucidates the outcome of the press's interaction with other powerful sources of political influence and, thus, essentially captures how information are generated and distributed. As such, my study also empirically tests the theoretical predictions by Shapiro (2016) and Sobbrio (2011) on how news outlets report under special interest seeking. By showing that interest groups rather influence the textual sentiment of articles on the negative margin, I document evidence which is consistent with the prediction by Sobbrio (2011) that low costs of engaging in strategic influence seeking (see above: "we are inundated with such input") generally increase the probability of slant. While this downward shift appears to be mitigated by the inclusion of differentiated views and perspectives (i.e., subjectivity effect), I also find that staleness of texts increases with interest groups appearances, which is consistent with a (reputational) friction leading to ambiguous articles as suggested by Shapiro (2016).

2 Conceptual Underpinnings & Prior Literature

2.1 Interest Groups' Involvement in Public Policy

Expertise & Strategic Influence

Descriptions of interest group activities refer to the "transfer of information" (Figueiredo and Richter (2014)) but are also characterized as "vote buying" and "legislative subsidy" (Hall and Deardorff (2006)). Intuitively, interest groups activities can involve both costs (strategic influence) and benefits (providing expertise). With regard to the latter, the involvement of interest groups in setting regulation is commonly justified by treating them as providers of valuable expertise. Consistently, "notice and comment" is the prevailing form taken by legislative procedures:

> *"initially, an agency conducts an analysis for a new rule and then issues its policy proposal (the notice); next, the public (in reality, including many special interests) may provide commentary; and, ultimately, the agency promulgates its final rule after accounting for the comments"* (Bils, Carroll, and Rothenberg 2020).

Bertrand, Bombardini, and Trebbi (2014) find that providing expertise constitutes an important interest group activity by presenting evidence that their expertise is also valuable for the politicians of opposite political affiliations (see also Acemoglu et al. (2016); for a comprehensive review see Bombardini and Trebbi (2020)). In particular, commentaries serve as a common tool for these purposes (Georgiou 2004; Chalmers 2013). McLeay, Ordelheide, and Young (2000) analyze commentaries on German accounting draft legislation and find that interest groups indeed impact regulators under notice and comment. Public institutional structures, however, provide multiple channels through which interest groups can seek to strategically influence public policy (Mercado Kierkegaard 2005; Mykkänen and Ikonen 2019). These processes are particularly shaped by informal bargaining rather than formal procedures (Thomson 2011). Strategic influence seeking could, thus, also outweigh the benefits obtained from interest groups' expertise. For instance, Bertrand, Bombardini, and Trebbi (2014) also show that seeking influence enables interest groups to earn a monetary premium from their connections (see also Blanes i Vidal, Draca, and Fons-Rosen (2012)).

Strategic Influence: Outside Tactics

Notice and comment as a formalized channel and immediate interactions with politicians establish interest groups' "inside tactics", while mobilizing "citizens outside the policymaking community to contact or pressure officials inside the policymaking community" (Kollman 1998, 3) establish "outside tactics". Both types provide interest groups with tools for increasing the success of their influence seeking efforts (Chalmers 2013). Huneeus and Kim (2020) point to resource misallocation as an example of the important economic consequences of interest groups' strategic influence seeking and Bertrand et al. (2020) highlight that political influence may remain "undetected by voters and subsidized by taxpayers" (p. 2065). In the tax context, Barrick and Brown (2019) and Mulligan and Oats (2016) suggest that (the application of) tax law can become endogenous. Importantly, Chalmers (2013) provides interview evidence that the effects of news appearances by interest groups (i.e., outside tactics) are not nearly as marginalized as is commonly predicted and very important in granting access to decision makers. One exemplary finding for the impact of media attention is established by Strömberg (2001) who finds that redistributive spending is higher for those programs that are intensely covered by the media. Interest groups and the press, however, are distinct economic actors whose incentives may or may not converge. Understanding the press's impact on public policy, and how interest groups can make it to and influence the news, thus, is essential.

2.2 The Press's Impact on Public Policy

Bybee et al. (2020) state that the "media sector, as a central information intermediary in society, continually transforms perceptions of economic events into a verbal description that we call news" (p. 2). Individuals, for instance, often learn about changes to public policy from the news (S. Soroka and Wlezien 2019). Exerting influence over the way that individuals eventually perceive the presented information, therefore, allows for influence over the political discourse (Strömberg 2004) and imposes an important source of political power (Gilardi et al. 2021). Martin and Yurukoglu (2017) and DellaVigna and Kaplan (2007) provide exemplary findings for this outcome. They assess media-driven polarization and show that exposure to Fox News increases the proportion of Republican vote shares.

Media can invoke responses in their audiences through concentrated and emotionally based coverage. Such coverage influences public policy by placing pressure on decision makers to react (e.g., see McCluskey, Kalaitzandonakes, and Swinnen (2016) and Robinson (2005): "CNN Effect"). Strömberg (2004) shows that public policy distortions can occur even without changing voting intentions because politicians respond to changes in media coverage at the same time and in a similar way. The press's own "representation bias" could further add to these effects. For instance, articles tend to emphasize negative events (S. N. Soroka 2006; McCluskey, Swinnen, and Vandemoortele 2015; Heinz and Swinnen 2015; Friebel and Heinz 2014) and periods of prolonged economic growth or contraction are amplified by the press through increasing the level of coverage of the economy and reporting with an overly positive or negative textual sentiment (van Dalen, Vreese, and Albæk 2017; S. Soroka and Wlezien 2019). S. N. Soroka (2006) and Hawkins (2002) deduce that journalists regard negative information more important, which is not simply based on their own interests but also on the interests of their news-consuming audience. It emerges that the press is a particularly powerful institution that is itself, however, subject to biases in its reporting. While influencing the press's sentiment could, thus, establish a politically powerful outside tactic for interest groups, the press, acting as a filter, constraints this influence (Shapiro 2016; Sobbrio 2011).

2.3 Press Coverage of Interest Groups

"It is one thing to seek media attention; to make it to the news is another" (Binderkrantz 2012). For instance, Andrews and Caren (2010) point out that interest groups that are particularly confrontational gain relatively little attention in local media outlets. Call et al. (2022) provide consistent insights when analyzing the development of articles by financial journalists. The authors state that negative articles are among the most impactful kinds, but they may, in turn, lead to a backlash to unfavorable articles, which could impede important private communication. With regard to interest groups, Walton (2020) highlights that "the most successful political intervention could be defined as that which is not visible". I obtain consistent anecdotal/interview evidence from a journalist:

> *"While we have to ensure that readers find access to the topic, we refrain from giving platforms to the loudest or most polemic voices just because it would heavily be clicked at our web outlet."* (tenured journalist at a German quality outlet)

Interest groups, however, receive extensive media coverage (for instance, vis-a-vis citizen groups (Binderkrantz, Bonafont, and Halpin 2017) and across particular types of interest (Bruycker and Beyers 2015; Grossman and Helpman 1994; Klüver 2012)). Shanahan et al. (2008) suggest that articles provide narrative framing strategies for constructing a policy story. Beyers (2004) provides an example and states that

> *"an opinion letter in the Financial Times by the chair of the European Roundtable of Industrialists does not reach a very large audience, but it will be read by financial and business elites all over Europe"* (p. 214).

In essence, the conceptual framework and prior literature suggest that interest groups are incentivized to influence the textual sentiment of news on tax reforms. A journalist's choice, while possibly herself biased, constraints this influence, which underscores the tension when analyzing outcomes of the interaction of powerful sources of political influence. Thus, the eventual impact of interest groups on the textual sentiment of press coverage of tax reforms remains an empirical, albeit important, question.

3 Data

3.1 Tax Reforms in Germany

[Table 1 about here.]

I collect tax reforms from the central documentation and information system of the German federal parliament ("DIP Bundestag")[3] between 1990 and 2019. The reunification of West and East Germany in the aftermath of the fall of the Berlin wall in late 1989 establishes a natural starting point for my analysis. The sample period ends in 2019 because of the unprecedented pandemic situation that occurred from 2020 onward. Table 1 depicts the keywords that are used for the searches to identify a broad range of tax reforms throughout the sample period. In particular, the spectrum of identified tax reforms covers tax issues ranging from base modifications for corporations, motor-vehicle taxes for electric cars, tax relief for families, and tax enforcement measures targeting tax evasion and avoidance. The keyword searches at DIP deliver results at a document level, which may classify as proposal ("Gesetzesantrag"), draft ("Gesetzentwurf"), or resolution ("Gesetzesbeschluss") of a tax reform.[4]

[Table 2 about here.]

I utilize unique tax reform IDs at DIP[5] to group the collected documents at the tax reform level. A tax reform can consist of several "important" (as classified by DIP) documents (e.g. both an initial proposal of and a final resolution on a single tax reform). Reshaping data from the document level to the tax reform level leads to an initial sample of 444 distinct tax reforms from 1,137 documents. I manually walk through all of the distinct reforms and use supplemental information obtained from DIP to exclude false positives, reforms with an unclear status, and (amendments to) double tax treaties (see Table 2 for sample selection steps).[6] While Germany formally qualifies double tax treaties as tax law, the latter exclusion is necessary because the respective documents are multilingual and common language processing algorithms

[3]DIP abbreviates "Dokumentations- und Informationssystem für Parlamentarische Vorgänge".

[4]Ministerial discussion drafts ("Referentenentwurf") or other reform proposals are not included in my sample. These very preliminary information are not available at DIP Bundestag, which posits a natural boundary for the identification of tax reforms.

[5]DIP provides an "Archivsignatur", a "GESTA-Ordnungsnummer", and an "ID". Recently, permalinks were added to the information; e.g., see dip.bundestag.de/vorgang/jahressteuergesetz-1996-jstg-1996-g-sig-13020126/119219. To enhance the transparency of my research approach, I collected all permalinks to these reforms at which the input-documents for identification of the tax reform-content are downloadable.

[6]For instance, a tax reform would qualify as having an "unclear status" when DIP Bundestag information indicate that it was merged with another proposal. If the former reform was not excluded from the data, duplicates in the tax reforms would receive higher weights in the subsequent analyses.

cannot distinguish among different languages. Furthermore, I expect that amendments to double tax treaties primarily fall outside of the interest of the German press. These selection steps yield a sample of 146 tax reforms for press coverage identification.

3.2 Press Coverage

In preparation for collecting press coverage, I initially apply an exploratory data analysis of the tax reforms. I begin this procedure by applying several processing steps to the PDF documents from DIP:

i. documents are combined at the tax reform level (subsequently "tax reform document"),
ii. optical character recognition (OCR) is performed because some documents are respectively available as scans with embedded images,
iii. multiple columns of text are distinguished when extracting tokens from a tax reform document,
iv. the corpus is lemmatized and stop words, calendar references, names, and other less relevant words are removed,
v. the tax reform documents are tokenized using single- and bi-grams,
vi. the data are transformed to a document term matrix (at the tax reform level), and
vii. the most frequently occurring features per document are identified using both simple word counts and term frequency inverse document frequency weights.

This approach allows for the identification of keywords that describe the content and topics of the tax reforms in the sample. Barberá et al. (2021) advocate for the use of keyword searches rather than predefined categories provided by archives when identifying press coverage. Therefore, I select search terms from the identified keywords and information from the tax reforms' titles to manually search for press coverage in the online archives of the German quality outlets Frankfurter Allgemeine Zeitung and Süddeutsche Zeitung (subsequently "FAZ" & "SZ"). I apply a time frame for a search of six entire months preceding and three entire months subsequent to a respective tax reform. The *Tax Reform Date* refers to the latest date of a collected tax reform document per tax reform. For instance, the *Tax Reform Date* of an implemented tax reform refers to the date of the approval by the Federal Council.

I manually collect 13,684 articles (html-files) through the respective searches conducted in the two online archives. However, some of the articles do not represent full articles but rather, for instance, news flashes in the form of a collection of "relevant appointments of the week", which likely do not provide editorial content containing reporter analyses (see also Coyne, Kim, and Kim (2020) on selecting press coverage for analyses). Manual inspections of selected articles further indicate that some of the articles do not refer to tax reforms in Germany but to reforms in other jurisdictions, discuss trials for tax evasion of celebrities and managers, or do not cover generic but company-specific news (e.g., see Loughran, McDonald, and Pragidis (2019) on the separation of generic and firm-specific articles). Therefore, I manually check each article-headline to exclude false positive hits and create a sample of generic articles. I also exclude duplicates of articles per tax reform. These steps result in a sample of 10,733 articles (see Table 2) on 140 of the 146 tax reforms.

3.3 Focal Variable of Interest: *Interest Group*

[Figure 1 about here.]

In the absence of a comprehensive register for interactions between politicians and interest groups throughout the sample period, the identification of appearances of interest groups in press coverage of tax reforms is both relevant and difficult. The federal parliament hosts a publicly available list of interest groups that are officially accredited at the Bundestag. Since accreditation is voluntary and does not create specific rights or duties for the respective interest groups, self-selection biases its immediate usage. However, manual inspections of current and former lists emphasize strong patterns recurring in the names of the interest groups. I use language processing tools to identify the most common single- and bi-grams within the names of the interest groups because there is little reason to believe that interest groups select themselves on the list due to the specific characteristics of their own naming. Figure 1 visualizes a word cloud comprising 50 most common features among the name patterns.[7] I base regular expressions on selected features of these patterns to trade-off individual search-precision and universal validity in the identification of interest groups[8]. I utilize 31 regular expressions for the identification of interest groups in the collected press coverage. The regular expressions are fully depicted in Table OS1 in the Online Supplement and can span over multiple tokens (I also perform multiple

[7]Naturally, German name patterns prevail in this identification approach. For instance, "verband" translates to "association". Its derivatives such as "Bundesverband", "Verband deutscher [...]", or "Zentralverband" translate to "federal association", "association of German [e.g. manufacturers]", and "central association".

[8]Generally, I am interested in identifying generic interest group references in articles and not a (sub)set of selected, specific interest groups because this enhances universal validity in the identification of interest groups in the press coverage. This approach comes at the cost of not being able to distinguish between interest groups that (do not) share specific characteristics.

quality checks of this identification approach; e.g., see Table OS3). I use pattern matching to identify references to interest groups in the articles (subsequent level of analyses). I construct the indicator variable *Interest Group* (subsequently also *IG*), which classifies article i on tax reform r in my sample:

$$\boldsymbol{Interest\ Group}_{i,r} = \begin{cases} 1, & \textbf{min. one interest group identified,} \\ 0, & \textbf{otherwise.} \end{cases}$$

3.4 Textual Sentiment

Bannier, Pauls, and Walter (2019) (subsequently BPW (2019)) developed a dictionary to measure the textual sentiment of accounting-related texts in the German language. It builds on and translates the work of Loughran and McDonald (2011), who provided accounting and finance research with a comprehensive dictionary ("bag of words") of specific terminology of negative and positive sentiment. The bag of words (bow) approach and the derived calculations of textual sentiment (Das 2014) heavily dominate research on textual sentiment (e.g., see the comprehensive reviews by Loughran and McDonald (2015), Loughran and McDonald (2016), and M. C. Zhang, Stone, and Xie (2019)). Machine learning approaches offer alternatives to the bow approach and can be particularly useful when the employed textual sentiment analysis is aimed at prediction rather than causal inference.[9] Given each alternative's specific advantages and disadvantages, one might contend that "although all quantitative models of language are wrong, some are useful" (Grimmer and Stewart 2013). It appears that the application of either measure (or their hybrid, as in Hájek (2018)) depends on the investigated scenario. This study aims to deepen our understanding of how news outlets transpose information to their readers and focuses on *IG* as a determinant of textual sentiment. This target is different from using textual data to predict economic outcomes and requires the transparent identification of the terminology that explains the articles' textual sentiment. Therefore, I rely on the bow approach for measuring *Textual Sentiment*:

$$Textual\ Sentiment = \frac{n_{pos} - n_{neg}}{N} \tag{1}$$

The terms n_{pos}, n_{neg}, and N refer to counts of positive, negative, and total words with

[9] E.g., see Groth and Muntermann (2011); Hagenau, Liebmann, and Neumann (2013); Gentzkow, Kelly, and Taddy (2019); Adämmer and Schüssler (2020); Brown, Crowley, and Elliott (2020); García, Hu, and Rohrer (2023).

n_{pos} and n_{neg} refer to the respective inclusion and identification of a token in the respective bow. Intuitively, one can construct the specific measures of *Negativity* and *Positivity*:

$$Negativity = \frac{n_{neg}}{N} \tag{2}$$

$$Positivity = \frac{n_{pos}}{N} \tag{3}$$

Since journalists are incentivized to develop articles that contain different perspectives through inclusion of rather heterogeneous views (see Section 2), both an article's *Positivity* and *Negativity* could increase while overall *Textual Sentiment* would remain rather constant. Therefore, I measure *Subjectivity*, which indicates the proportion of textual sentiment to the frequency of occurrence (W. Zhang and Skiena 2010; Das 2014). In essence, *Subjectivity* approximates the degree to which differences among represented opinions are included (in the press coverage of tax reforms):

$$Subjectivity = \frac{n_{pos} + n_{neg}}{N} \tag{4}$$

In addition to the accounting language related bow of BPW (2019), I also utilize the bow of Rauh (2018), which was specifically developed to analyze German political language.[10] I take this additional approach because tax reforms originate in accounting language but are implemented as policy measures and are therefore characterized by the choices of specific expressions. Choosing two separate bows also challenges the robustness of my analyses and triangulates my findings to the commonly advanced criticism of subjective word-selections in the development of a bow.

3.5 Summary Statistics

[Table 3 about here.]

Table 3 depicts the summary statistics of the measures of textual sentiment that are derived from the bow by BPW (2019) and Rauh (2018). Bow-based sentiment measures are biased to zero because one expects that there are relatively few words in the bow in comparison to the total word count. Generally, this also holds for the sample of articles at hand. On average, 3 out of every 100 words in an article can be identified in the positive and negative word lists of BPW

[10] BPW's bow is available at uni-giessen.de and is utilized in R with the SentimentAnalysis-package. Rauh's bow is available through the quanteda-package by Benoit et al. (2018).

(2019). *Textual Sentiment* of an article is slightly negative on average, indicating that negative words appear more commonly than positive words. This is consistent with many accounting studies, in which the focus is often on the *Negativity* of the underlying corpus. Applying the bow of Rauh (2018) produces a slightly positive average for *Textual Sentiment* (0.0038). This appears to be consistent with Rauh's discussion of his bow, in which he states that the bow rather picks up the positive textual sentiment of texts (Rauh 2018).

Information that are derived from the articles in the sample are also presented in Table 3. Approximately 64% of the articles in the sample are extracted from the online archive of the FAZ. With regard to appearances of an *Interest Group* in articles, it can be observed that 18% of the articles in the sample are classified as pertaining to interest groups (i.e. *Interest Group* = 1). I observe that a maximum of 8 distinct interest groups can appear within one article (*Interest Group Count*). The last column of Table 3 depicts averages for which the sample is conditional on $IG = 1$. Articles are slightly longer but appear about as often on the title page of an outlet.[11] Across all articles, 18% appear on the title page, and the maximum word count of an article is 6584 (with a mean of 575 words per article). *IV* is the instrumental indicator variable for the 2SLS analyses and is constructed as described in Section 4.1.

4 Empirical Design

4.1 Instrumental Variable

Covering the positions of interest groups in articles is unlikely to be a random event but rather depends on the respective tax reform, the choices of journalists, and the efforts made by interest groups to appear. Consequently, endogeneity is a caveat when analyzing interest groups (LaPira, Thomas, and Baumgartner 2012; Figueiredo and Richter 2014). Instrumental variable strategies can be applied to solve endogeneity and allow for a causal interpretation of (local average) treatment effects. This, however, requires identifying an instrument that reliably predicts the endogenous independent variable but is itself not related to the dependent variable other than through its impact on the endogenous regressor. In particular, the identification of valid instruments often arises from specific characteristics of the institutional background (Cunningham 2021).

[11] The construction of my outcome variables of interest includes scaling through total word counts (see Section 3.4). To ensure that my results are not simply reflective of increasing article length, which could somewhat mechanically increase the likelihood of inclusion of interest groups and multiple opinions, I include, in untabulated tests, an article's length as additional control variable and find unchanged inferences to that presented in Section 5.1.

[Figure 2 about here.]

I construct the instrumental variable by drawing on the suggestions of Georgiou (2004), who indicates that while effectiveness is perceived to decrease with the course of a reform, interest groups typically engage rather late in the process and only do so once they see the likelihood of the new regulation being passed as being sufficiently high (see also Becker, Bischof, and Daske (2021) in the context of setting IFRS). I conduct an interview with a tenured journalist at a German quality outlet who not only highlights that interest groups approach journalists ("we are inundated with such input") but also stresses that articles in the daily press must be written in a very timely manner and always rely on multiple sources. I conclude that if interest groups accelerated their effort to appear in the press around a tax reform's date, they would be more likely to be chosen by journalists to appear in the respective articles. I test this notion by exploiting a plausibly exogenous time period between article publication dates and tax reform dates (the *Tax Reform Date* marks the latest date of a collected "important document" of that tax reform in DIP Bundestag and typically refers to the date of approval by the Federal Council) and find consistent patterns in the analyzed German press coverage. *"Days Between Publication of Article and Tax Reform Date"* in Figure 2 refers to this time-relation on the x-axis. In particular, Figure 2 depicts two density plots illustrating press coverage on tax reforms (by IG). The ice blue (dashed) facet depicts the density for articles that are classified as $Interest\ Group = 1$ and the red (solid) facet depicts the density when $IG = 0$ (I repeat this analysis by deciles of articles' word counts and find consistent patterns). It can be observed that interest group references in articles are especially pronounced in the weeks preceding a decision on a tax reform. The excess in the density of the ice blue facet (period between the two black vertical lines) marks this time-period. Since there is little reason to expect that the strengthened press appearance of interest groups in this period drives other factors that are related to textual sentiment (see also Section 5.1), the instrumental variable IV equals one throughout this exogenous time-period per tax reform and zero otherwise. Subsequently, IV serves as an instrumental variable in the regression analyses which use two stage least squares estimators.

4.2 Regression Design

2SLS

The causal relationship of interest is depicted in the structural equation (5), which cannot be directly estimated due to the endogeneity between the dependent and focal independent variables of interest:

$$Subjectivity_{i,r} = \alpha + \beta_1 \ Interest\ Group_{i,r} + \beta_n \ X + \tau_t + \delta_o + \gamma_i + \phi_i + \epsilon_{i,r} \tag{5}$$

Therefore, I estimate an instrumental variable regression using two stage least squares (2SLS). In the first stage, I regress *Interest Group* on the instrument IV:

$$Interest\ Group_{i,r} = \alpha + \pi_1 \ IV_{i,r} + \pi_n \ X + \tau_t + \delta_o + \gamma_i + \phi_i + \epsilon_{i,r} \tag{6}$$

The second stage (equation (7)) uses the fitted values $\widehat{InterestGroup}$ from equation (6) and employs the exogenous part of the variation in the variable to estimate the causal impact of *Interest Group* on *Textual Sentiment* (which I operationalize in the regressions with measures of *Subjectivity*, *Negativity*, and *Positivity*):

$$Subjectivity_{i,r} = \alpha + \beta_1 \ \widehat{Interest\ Group}_{i,r} + \beta_n \ X + \tau_t + \delta_o + \gamma_i + \phi_i + \epsilon_{i,r} \tag{7}$$

Control Variables

[Figure 3 about here.]

Panel A of Figure 3 depicts the *Textual Sentiment* of each article in the sample throughout the sample period. The smoothed white line displays time trends in *Textual Sentiment* (and its 95%-confidence interval) and indicates variation (i.e., a wavy course) over time. In particular, the aftermath of the dotcom and the financial crisis are aligned with low levels of *Textual Sentiment*. A plot of the annual changes in Germany's gross domestic product (GDP) shows a similar development over time (Panel B of Figure 3). It appears that my sample aligns with the common observation of co-movement between the press's *Textual Sentiment* and economic developments (van Dalen, Vreese, and Albæk 2017; S. Soroka and Wlezien 2019). For my analyses, I utilize article publication dates to include year fixed effects τ_t to control for this variation.

Furthermore, $\beta_n \ X$ depicts a vector of control variables. Primarily, I am interested in controlling for appearances of other economic actors in the sample of articles. Therefore, I construct *Right over Left MP*. Specifically, I manually collect the names, party affiliation, and political leaning of all members of the German federal parliament since 1990 (*Right*: CDU, CSU, FDP, AfD; *Left*: SPD, Green, Leftist). I then perform exact name matching within the articles to identify appearances of members of parliament. I count appearances of *Right* and *Left* leaning politicians and then construct *Right over Left MP* (as $(Right - Left)/(Right + Left)$)[12] to measure the leaning of right over left political views in the article. By controlling for appearances and the political leaning of members of parliament in the article, I intend to capture the effect of *Interest Group* on an article's textual sentiment conditional on the appearance of other important sources of influence. I also control for *BT Sitting*, which measures the absolute difference in days between the publication date of an article and the closest sitting of the federal parliament ("BT"). For its construction, I manually collect all dates of sittings of the federal parliament since 1990. Generally, sittings are scheduled way ahead in time and follow a repetitive pattern over the years, so that the information could serve as a predictor of press coverage. Furthermore, information from DIP indicate whether a tax reform was (i) implemented, (ii) rejected within its course (some tax reforms do not pass their stage as "proposal"), or (iii) later declared unconstitutional/void by the federal constitutional court. I include the respective *Tax Reform Outcome* as factor variable δ_o in the first and second stage regressions. Additional control variables include outlet (FAZ and SZ) γ_i and title page ϕ_i fixed effects.

5 Results

5.1 2SLS Results

[Table 4 about here.]

Table 4 depicts results of estimating instrumental variable regressions using two stage least squares (2SLS) with *Negativitiy*, *Positivity*, and *Subjectivity* as dependent variables. The results for estimating the first stage (equation (6)) are depicted at the bottom of Table 4. In the second stage, the coefficient for the endogenous regressor *Interest Group* is positive in all specifications. Basically, an increase in *Negativity* (*Positivity*) indicates a decrease (increase) in the textual sentiment of the press coverage of tax reforms when interest groups appear in the respective articles. Increases in both *Negativity* and *Positivity* are consistent with the conceptual expectations which suggest that journalists are incentivized to present rather heterogeneous

[12] In multiple untabulated robustness checks, I alternatively include counts of *Right* and *Left* jointly/separately.

views when interest groups appear in articles (see Section 2). This renders the analysis of *Subjectivity* particularly interesting because *Textual Sentiment* rather remains constant overall. I find the estimate for *Interest Group* to be strongly statistically significant (p-value 0.0141) when *Subjectivity* serves as the dependent variable. Generally, these local average treatment effects suggest that the exogenous variation of *Interest Group* that stems from *IV* can be associated with the textual sentiment of the press coverage of tax reforms. I interpret this finding as first evidence for interest groups influencing the textual sentiment conveyed in the press coverage of tax reforms.

[Table 5 about here.]

I find consistent evidence when employing the bow of Rauh (2018) for the main analysis. Table 5 depicts the results in which the 2SLS design is utilized to estimate equation (7). *Negativity*, *Positivity*, and *Subjectivity*, however, refer to the sentiment calculations under the bow for political language by Rauh. The coefficient for the endogenous regressor *Interest Group* is positive in all specifications with a p-value of 0.0191 when *Subjectivity* serves as the dependent variable. The analyses depicted in Table 5 also point to the observation from Section 3.5, namely that the bow by Rauh (2018) picks up positive words from the corpus particularly well. Given the consistency of the findings in Tables 4 and 5, it further appears that the selection of a specific bag of words does not drive the results. Additionally, the control variables displayed in Tables 4 and 5 are consistent across the specifications. For instance, *BT Sitting* is respectively marginally associated with an article's textual sentiment. The coefficient estimates for *Right over Left MP* are marginally negative. Interestingly, the coefficient estimates for the (untabulated) outlet fixed effects for Süddeutsche Zeitung (γ_i) are positive and highly statistically significant for all specifications in Table 4 and (Table 5). The estimates for γ_i of 0.0024 (0.0074) with *Negativity* as the dependent variable exceed the estimates for γ_i with 0.0005 (0.0046) when *Positivity* is the dependent variable. This suggests that articles on tax reform topics in SZ on average appear with stronger negative connotations than in their FAZ counterparts. Overall, applying the 2SLS estimator indicates that the press appearance of an *Interest Group* influences an article's characteristics, such as common measures of textual sentiment. In particular, the evidence suggests that differences in represented opinions, as approximated by *Subjectivity*, increase when interest groups appear in the press coverage of tax reforms.

Instrument Validity

[Figure 4 about here.]

Next, I turn to a discussion of the validity of the instrument IV. One testable criterion of an instrument's validity is its relevance. The results from estimating the first stage (equation (6)) are depicted at the bottom of Tables 4 and 5. The coefficient estimate for IV is 0.0247 and is highly statistically significant (p-value 0.0019). This suggests that the appearance of an *Interest Group* becomes 2.5% more likely when IV equals one. The F-statistic for the instrument of 10 indicates sufficient relevance of the instrumental variable because it meets the rule of thumb-threshold for the F-statistic. Limiting the sample to articles from the FAZ ("FAZ only") yields a stronger F-statistic of 12 while the significant influence of *Interest Group* on *Subjectivity* prevails.

While timing, which essentially determines the instrumental variable IV, is exogenous per se, it could influence journalists' incentives to the extent that including different actor groups to incorporate different perspectives becomes more likely shortly prior to a tax reform's date. Therefore, I visually inspect how *Subjectivity* of articles develops relative to the *Tax Reform Date* (and thus over the $IV = 1$ and $IV = 0$ periods) by IG (see Figure 4). In particular, I plot smoothed lines (ice blue facet (dashed) when $IG = 1$, red facet (solid) when $IG = 0$) and the 95% confidence intervals through the observations of *Subjectivity* in relation to the difference between publication dates of the articles and the *Tax Reform Date* (for the FAZ only sample). If inclusion of different perspectives became more likely in the run up to a tax reform's date, one would expect an effect on the articles' *Subjectivity* among the article for which IG equals zero, too. Notably, however, one can observe that *Subjectivity* is rather stable over time when articles do not feature an *Interest Group*. Instead, Figure 4 shows the increase in *Subjectivity* among the articles that feature an *Interest Group* when the IV (area between the two vertical lines) equals one. These insights suggests that journalists per se do not confoundingly include different actor groups to incorporate different perspectives shortly prior to a tax reform's date. Therefore, I interpret the absence of a meaningful relation between IV and an article's *Subjectivty* for "untreated" articles as supporting the validity of the instrumental variable.[13]

[13]Even if my instrument was not perfectly suited for the task, my instrumental variable approach probably improves upon OLS. This holds when the correlation between the endogenous variable and the OLS residuals is larger than the ratio of the correlation of the instrument and the residuals to the correlation of the instrument and the endogenous variable (e.g., see Gurun and Butler (2012)).

Alternative Instrument: Budget Impact

[Table 6 about here.]

Next, I turn to an alternative instrumental variable. In particular, I focus on a tax reform's absolute budget impact (*z Tax Reform Abs. Budget Impact*). Often, tax reforms are designed to be net-revenue-neutral. There are, however, notable exceptions with rather large impacts on the federal/state/local budgets. Given that interest groups typically serve special interests, their media presence could be driven by reforms with rather large absolute budget impacts. Therefore, I manually collect information on the expected budget impacts for the entire set of tax reforms in my sample from the documents at DIP (at the latest time possible, thus disregarding potential changes throughout the course of a tax reform's implementation). Information on a tax reform's budget impact are typically provided for different government entities (at the federal-, state-, and local-level) and over a time-horizon of a few years (typically three years). For the construction of the instrumental variable, I (i) calculate the average of the budget impact information per tax reform (across entities and periods), then (ii) take the absolute values (i.e. a large decrease equals a large increase in government revenue), and (iii) standardize the per-tax-reform measure at mean zero and SD one (z indicates standardization). I then estimate equation (7) with *z Tax Reform Abs. Budget Impact* as instrument for IG and *Subjectivity* as dependent variable (as calculated under both BOWs by BPW and Rauh). Results are depicted in Table 6 and suggest that an increase in the absolute budget impact of a tax reform is associated with an increasing probability of an *Interest Group* appearing in the press. The F statistic of 25 signals strong statistical relevance which also exceeds the strength of IV. Furthermore, the results (consistent with the results in Tables 4 and 5) suggest that *Interest Group* is positively and statistically significantly related to *Subjectivity*.

5.2 OLS Estimates

[Table 7 about here.]

I now turn to OLS estimates of equation (5). Table 7 presents the OLS results which throughout all specifications point to a significant effect of an *Interest Groups* on the textual sentiment of articles (e.g., p-value of IG when *Subjectivity* serves as dependent variable: 0.0000). This relation continues to hold when subsetting the analyzed sample to articles from the FAZ ("FAZ only") and to those observations, in which the IV equals one. The latter approaches the 2SLS regression design because it only utilizes variation in the observations of IG when the instrument is "turned on". Furthermore, there is no evidence of upwards bias for OLS throughout all specifications of equation (5).[14] Additionally, Table 7 presents the results from estimating the reduced form, which we can write as:

$$Subjectivity_{i,r} = \alpha + \beta_1 \, IV_{i,r} + \beta_n \, X + \tau_t + \delta_o + \gamma_i + \phi_i + \epsilon_{i,r} \tag{8}$$

Equation (8) subsumes the endogenous regressor *Interest Group* but includes the IV as a predictor of textual sentiment: it depicts the relationship between the outcome and the instrument itself. Because 2SLS relies on the assumption that IV does not affect the outcome directly (i.e. it is uncorrelated with the error term) but indirectly/only through IG, it should mechanically have an association with articles' textual sentiment. The last specification of Table 7 depicts results from estimating equation (8) and suggests that this mechanical relationship actually exists (p-value for IV of 0.0005). Note that the magnitude of the estimate is attenuated because the first stage regression indicates that the instrument does not one-to-one translate into interest group's appearances in articles.

5.3 Modified Control Function

[Figure 5 about here.]

[Table 8 about here.]

Next, I employ a modified control function (MCF) approach as alternative estimation technique to address endogeneity in the relationship of interest. I follow Klein and Vella (2010), who present a control function estimator for models where it is difficult to find sufficiently powerful instruments that satisfy the exclusion restriction. The MCF approach, which relies on homogeneous treatment effects, found recent application in Armstrong, Nicoletti, and Zhou (2022) who highlight the technique's strength in settings with two-sided endogeneity (here interest groups' effort to appear and journalists' choices).

The core idea of the MCF approach is to construct a variable that controls for the endogenous relation using information about the unobserved variables (which are captured by the residuals) under heteroskedasticity. For the implementation of the MCF approach, I first

[14] Results continue to hold when I estimate the specification within reforms (i.e., including tax reform fixed effects). Since a tax reform and its press coverage often occur within one specific year, I decide to rather include year fixed effects than reform fixed effects in the presented specifications in Table 7. Furthermore, most of the articles in my sample are written by unidentified authors and those articles that identify authors are often written by multiple authors. Therefore, I cannot include author fixed effects in the regressions.

regress *Interest Group* on all control variables from the main specification using an OLS estimator without standard error adjustment ("first-stage"):

$$Interest\ Group_{i,r} = \alpha + \beta_n\ X + \tau_t + \delta_o + \gamma_i + \phi_i + \xi_{i,r} \tag{9}$$

A second model regresses the measures of textual sentiment (e.g., *Subjectivity*) on the focal independent variable and identical controls without standard error corrections ("second-stage"):

$$Subjectivity_{i,r} = \alpha + \beta_1 Interest\ Group_{i,r} + \beta_n\ X + \tau_t + \delta_o + \gamma_i + \phi_i + \eta_{i,r} \tag{10}$$

For both specifications, residuals are predicted. In equation (10), β_1 is the estimate of interest but is biased through correlation between η and ξ. The MCF decomposes the error term η. It can effectively be substituted by $\frac{\sigma_\eta}{\sigma_\xi}\xi$ so that the (new) error term ω is uncorrelated (through construction) with the independent variable of interest (see Klein and Vella (2010) and Armstrong, Nicoletti, and Zhou (2022)). This requires to calculate the "standard deviation ratio" ($\frac{\sigma_\eta}{\sigma_\xi}$). It is the ratio of the standard deviations of the residuals of the first- and second-stage regressions. For the measure's construction, MCF requires that the standard deviation ratio varies in the cross section (i.e., MCF depends on sufficient variation in the ratio of the standard deviations of the first- and second-stage residuals). I assume that the standard deviation ratio varies across outlets and years (i.e., the standard deviations of the residuals are calculated by grouping the data over *Year* $\times$ *Outlet*). Figure 5 depicts a density plot of the standard deviation ratio (when the dependent variable is *Subjectivity*). It can be observed that there is indeed variation in the standard deviation ratio. The MCF regression then includes the standard deviation ratio interacted with the first-stage residuals (i.e., $\rho = \frac{\sigma_\eta}{\sigma_\xi}\xi$) as an additional control variable:

$$Subjectivity_{i,r} = \alpha + \beta_1 Interest\ Group_{i,r} + \beta_2\ \rho + \beta_n\ X + \tau_t + \delta_o + \gamma_i + \phi_i + \omega_{i,r} \tag{11}$$

I find that the estimate for ρ (see Table 8) is statistically significant (p-value 0.0496) when *Subjectivity* is the dependent variable, which indicates the existence of endogenous factors driving the relation between *Interest Group* and *Subjectivity* shown in the OLS estimates. The estimate for *Interest Group*, though, continues to identify the subjectivity effect from the main and OLS analyses (p-value 0.0000).[15] Taken together, 2SLS-, OLS-, and MCF-estimates for *Interest Group* are all positive and statistically significant when *Subjectivity* is the dependent variable.

5.4 Multiple Interest Groups

[Figure 6 about here.]

[Table 9 about here.]

Thus far, the evidence suggests that an *Interest Group* actually impacts the textual sentiment conveyed in the press coverage of tax reforms. The increases in both *Negativity* and *Positivity* suggest the existence of a subjectivity effect, which can be interpreted as an increase in the represented differences in opinions among articles that feature interest groups. Intuitively, this raises the question of who contributes to these different opinions. An article could feature opposing and supporting views on a tax reform by different interest groups or include different views from different groups of stakeholders. To address this, I investigate whether the inclusion of several interest groups in an article is associated with the increase in *Subjectivity*.

I create the variable *Interest Group Count* which counts the number of distinct interest groups within one article (for analyses binned at ≥ 4). Figure 6 depicts boxplots of *Subjectivity* which condition on the levels of *Interest Group Count*. Firstly, the graphic supports my main analysis because the depicted distributions of *Subjectivity* indicate higher values of the variable when at least one *Interest Group* appears in an article (the red/solid facet depicts a boxplot if $IG\ Count = 0$). Secondly, Figure 6 also suggests that the number of included interest groups per article contributes to the increase in *Subjectivity* as medians rise throughout the conditions of *Interest Group Count*. Averages in *Subjectivity* between conditions $IG\ Count = 1$ and $IG\ Count \geq 4$ also increase statistically significantly (p-value 0.0008). I corroborate this descriptive investigation by regressing textual sentiment on the factors of *Interest Group Count* (using the OLS estimator). Table 9 depicts the results for these estimations with *Negativity*, *Positivity*, and *Subjectivity* as dependent variables (all employing the bow from BPW (2019)). Generally, the inclusion of multiple interest groups within an article contributes to increases in the differences of represented opinions because coefficient estimates for *Subjectivity* increase with an increase in *Interest Group Count* (p-value for difference between β_1 and β_4 0.0002).

[15] I find identical evidence when I estimate equations (11) and (9) independently (as presented here) or as seemingly unrelated regressions.

A comparison between the effect-sizes for *Negativity* and *Positivity*, however, indicates that the effect is particularly driven by increases in *Negativity*. To better understand whether the identified effects are primarily driven by interest groups, I subsequently triangulate my findings to the "quasi-sentences" containing references to interest groups in the articles.

5.5 Quasi-Sentences of Interest Groups

[Figure 7 about here.]

[Table 10 about here.]

I construct quasi-sentences (a ± 1 sentence-window around the sentence in which an IG is identified) containing references to interest groups in articles. A quasi-sentence establishes a grammatical construct that is meant to contain a single argument. By measuring the textual sentiment of a quasi-sentence and contrasting this measure to that of the entire article, I strive to narrow down the actual impact of interest groups on an article's textual sentiment. Figure 7 depicts the difference between the *Negativity* of an article and the *Negativity* of the quasi-sentences of an *Interest Group* within this article in Panel A (see Panels B and C for *Positivity* and *Subjectivity*):

$$Negativity_{(Entire\ Article)} - Negativity_{(IG\ Quasi\ Sentences)} \tag{12}$$

Values of < 0 for the measures of equation (12) suggest that the respective sentiment measure is particularly pronounced in the quasi-sentences of interest groups. It can be observed that this is true for *Negativity*, as the mean of this calculation is < 0 (vertical dotted line in Figure 7, p-value 0.0000). This suggests that interest groups appear in conjunction with rather strong negative textual sentiment in an article. Thus, the increase in represented opinions (subjectivity effect), can also be attributed to the inclusion of text that mitigates the particularly negative impact of interest groups. This conclusion is further supported when replacing the dependent variables in equation (7) with the *Altered* measures of textual sentiment from equation (12) (i.e. when an *Interest Group* is identified in the article, the measure of textual sentiment is reduced to that of the quasi-sentence, e.g. $Negativity_{(IGQuasiSentences)}$):

$$Altered\ Negativity_{i,r} = \begin{cases} Negativity_{(IG\ Quasi\ Sentences)}, & \text{article with Interest Group} \\ Negativity_{(Entire\ Article)}, & \text{otherwise} \end{cases}$$

In comparison to the coefficient estimates in Table 4, the results for *Interest Group* listed in Table 10 are larger (smaller) when *Altered Negativity* (*Altered Positivity*) is the dependent variable. Thus, the results indicate that the appearance of interest groups on average influence the textual sentiment of these articles on the negative margin. Overall, this downward shift appears to be mitigated by the inclusion of differentiated views and perspectives, which explains the overall identification of the subjectivity effect. Importantly, this "balance", while in line with journalists' incentives to incorporate heterogeneous views (Call et al. 2022), can also introduce ambiguity. With the influence of interest groups persistently leaning towards the negative margin, press coverage of interest groups can lead to a inclusion of perspectives that might not necessarily reflect the actual uncertainty or controversy surrounding a topic (e.g., see Shapiro (2016)). In interest of empirical evidence for the latter notion, I next analyze how stale the entire sample of articles and the quasi-sentences of interest groups are vis-à-vis each other.

5.6 Staleness

[Table 11 about here.]

Staleness of text is considered to represent redundant information (Tetlock 2011). Since interest groups affect articles' textual sentiment persistently on the negative margin, their impact could particularly induce ambiguity if they provided redundant information (i.e., if their quasi-sentences were particularly stale). Therefore, I construct two textual staleness measures for all articles against each other and for all quasi-sentences against each other. In preparation, I first lemmatize the entire corpus to receive word stems of all tokens. Next, I remove the most common German vocabulary from the corpus and require a minimum term frequency of two.[16] I then measure Jaccard textual similarity *StaleJacc* and cosine textual similarity *StaleCos* (e.g., *StaleJacc* for one specific article is calculated against the remaining ~10K articles in the sample, creating as many observations). For both measures, values closer to one indicate a high degree of similarity, whereas those closer to zero indicate low similarity.

I then verify whether these measures actually capture staleness by demonstrating that the similarity of articles and quasi-sentences increases within reforms (capturing the idea that articles that belong to one reform are likely to be more similar). Panels A and B of Table 11 depict results from regressing *Stale* on several article and reform characteristics (reform, year,

[16] Removing the most common German terminology and requiring a minimum term frequency biases the similarity measures to zero but is done for computational feasibility (the article $\times$article construction leads to $>$ 94m observations). Also, I am interest in relative comparisons and not in absolute values of *StaleJacc* and *StaleCos*.

title page, and outlet fixed effects) and *Reform Match*. The indicator variable *Reform Match* equals one when staleness is calculated against an article/quasi-sentence that belongs to the same reform. I observe that staleness increases within reforms for both articles and the quasi-sentences, verifying that *StaleJacc* and *StaleCos* capture staleness of the texts in my sample.

Next, I test whether staleness is higher for quasi-sentences in comparison to the staleness for the articles. Panel C of Table 11 depicts results from Welch two sample t-tests that compare averages of staleness between quasi-sentences and articles (first row), when *Reform Match* equals one (second row), and when utilizing predicted values of staleness from the regressions in Panel A and Panel B. I find, consistently both applied measures and the multiple subsamples, that staleness of texts increases with interest groups appearances, indicating redundant information by interest groups, inducing ambiguity rather than balance. I conclude that press coverage of interest groups can indeed lead to a inclusion of perspectives that might not necessarily reflect the actual uncertainty or controversy surrounding a topic.

6 Conclusion

This study investigates how interest groups influence the textual sentiment conveyed in the press coverage of tax reforms. My analyses suggest that interest groups on average influence in both a positive and a negative direction the textual sentiment conveyed in the press coverage of tax reforms. Thus, interest groups appearances in the press coverage of tax reforms generally explain differences in represented opinions (subjectivity effect). However, the results of additional analyses suggest that the increase in the differences in represented opinions can only partially be explained by the inclusion several interest groups within an article. Instead, I find that interest groups rather affect articles' textual sentiment on the negative margin. This downward shift appears to be mitigated by the inclusion of differentiated views and perspectives, which explains the overall identification of the subjectivity effect. Importantly, this "balance" can also introduce ambiguity because I also find that staleness of texts increases with interest groups appearances, suggesting that press coverage of interest groups can lead to a inclusion of perspectives that might not necessarily reflect the actual uncertainty or controversy surrounding a topic (e.g., see Shapiro (2016)).

I analyze the media as an information intermediary and my study elucidates the outcome of the interactions that occur between media and interest groups. I contribute to the current discussion of the costs (influence seeking) and benefits (providing expertise) of interest groups activities in standard setting by analyzing an outside tactic of interest groups that is nearly impossible to regulate. Overall, I advocate for considering outside tactics of interest groups when aiming at efficiently designing transparency mandates in political processes. While the results of this study appear robust across multiple identification strategies and estimators, I encourage additional thoughtful attempts that shed light on how information are (chosen to be) transmitted by intermediaries. Furthermore, future research could leverage the insights of this study when documenting the effects of the interaction between different economic agents in setting (tax) standards (e.g., on how tax reform proposals are changed along their way to adoption or abolishment).

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Appendix

Table A1: Variable Definitions

Variable	Definition (Source)
Outcome Variables of Interest	
Textual Sentiment	Difference between counts of positive words and negative words per article, scaled by total word count. (bow by BPW (2019) and Rauh (2018))
Negativity	Count of negative words per article, scaled by total word count. (bow by BPW (2019) and Rauh (2018))
Positivity	Count of positive words per article, scaled by total word count. (bow by BPW (2019) and Rauh (2018))
Subjectivity	Sum of positive and negative word counts per article, scaled by total word count. (bow by BPW (2019) and Rauh (2018))
Sampling Unit	
Article	.html-file of an article from either Frankfurter Allgemeine Zeitung (“FAZ) or Süddeutsche Zeitung (”SZ”) which relates to a tax reform’s content/topic. Articles at SZ are available from Jan/2/1992. (licensed online archives of FAZ at faz-biblionet.de and of SZ at archiv.szarchiv.de)
Other Variables	
BT Sitting	Absolute difference in days between the release of an article and the closest sitting of the federal parliament. (DIP Bundestag)
Budget Impact	The absolute value of the expected budget impact of a tax reform as estimated in the tax reform documents. Estimates are provided by the body which submits the tax reform document (e.g., ministry of finance). Estimates are provided on a “mechanical” as is basis (i.e., ignoring behavioral effects). Estimates are typically provided for different government entities (Bund, Laender, Gemeinden) and over a time-horizon of about three years. I consider the average across entities and time-horizon per tax reform. (DIP Bundestag)
GDP Growth	Annual growth of the Gross Domestic Product (in %) in Germany. (World Bank)
Interest Group	Also *IG*; focal variable of interest; indicator variable; equals one when at least one interest group appearance is identified in an article; zero otherwise. (see Table OS1 for identification steps)

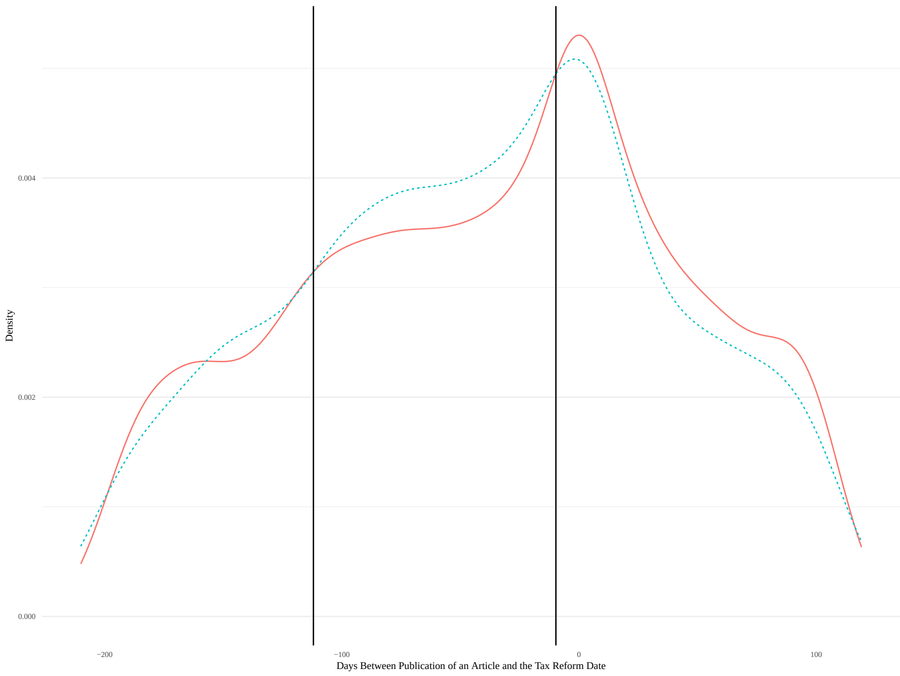
Variable	Definition (Source)
Interest Group Count	Count of the distinct interest group references per article; binned at maximum of four for analyses.
IV	Instrumental variable for the regression analysis. Indicator variable; constructed as described in Section 4.1.
Reform Match	Indicator variables; equals one when *Stale* is calculated against an article/quasi-sentence that belongs to the same reform, zero otherwise; see *Stale*. (Article and Tax Reform)
$\rho_{textual\ sentiment}$	Interaction of the ratio of the standard deviations of the predicted residuals from a first- and a second-stage regression with the predicted residuals from the first-stage; applied in the modified control function approach (see Section 5.3 for a detailed description).
Right over Left MP	Names of all members of the German federal parliament since 1990 and their political leaning according to their party affiliation (*Right*: CDU, CSU, FDP, AfD; *Left*: SPD, Green, Leftist); handcollected; exact name matching in articles to count appearances of rather right and left politicians and their views, constructed as: $(Right - Left)/(Right + Left)$, zero if missing.
Stale	I construct two textual staleness measures (Jaccard similarity (*StaleJacc*) and cosine similarity (*StaleCos*)) for all articles against each other and for all quasi-sentences (at the document level) against each other (e.g., *StaleJacc* for one specific article is calculated against the remaining ~10K articles in the sample, creating as many observations). In preparation, I lemmatize the entire corpus to receive word stems of all tokens. I also remove the most common German vocabulary from the corpus and require a minimum term frequency of two. (Article)
Tax Reform	A tax reform is any process ("Vorgang") which is identified via pattern searches in the central documentation and information system of the German federal parliament ("DIP Bundestag") and included under the criteria depicted in Tables 1 and 2. (DIP Bundestag)
Tax Reform Date	The date of a tax reform marks the latest date of a collected "important document" of that tax reform in DIP Bundestag. (DIP Bundestag)
Tax Reform Outcome	Factor variable of the tax reform outcome: implemented, rejected, or later declared unconstitutional/void by the federal constitutional court. (DIP Bundestag)

Variable	**Definition (Source)**
Title Page	Indicator variable; equals one when an article appears on the title page of that day's edition; zero otherwise. (Article)
Word Count	Total word count of an article. (Article)

Figure 1: Word Cloud of Interest Groups' Name Patterns

The federal parliament hosts a publicly available list of interest groups that are officially accredited at the Bundestag. Manual inspections of current and former lists emphasize strong patterns recurring in the names of the interest groups. I use language processing tools to identify the most common single- and bi-grams within the names of the interest groups. This figure visualizes a word cloud of the 50 most common features in the name patterns. German name patterns prevail. For instance, "verband" translates to "association". Its derivatives such as "Bundesverband", "Verband deutscher [...]", or "Zentralverband" translate to "federal association", "association of German [e.g. manufacturers]", and "central association". Table OS1 depicts the exact regular expressions that are used to identify the appearance of an *Interest Group* in the collected press coverage.

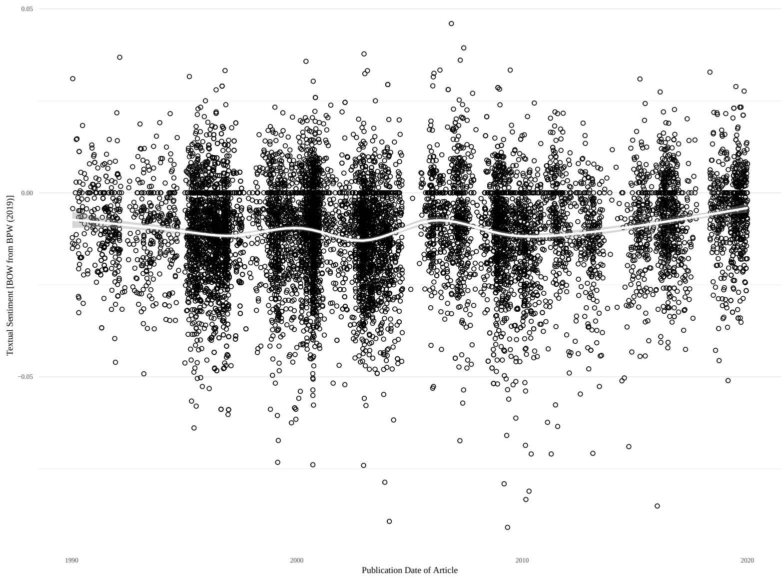
Figure 2: Interest Groups in Press Coverage - Instrumental Variable



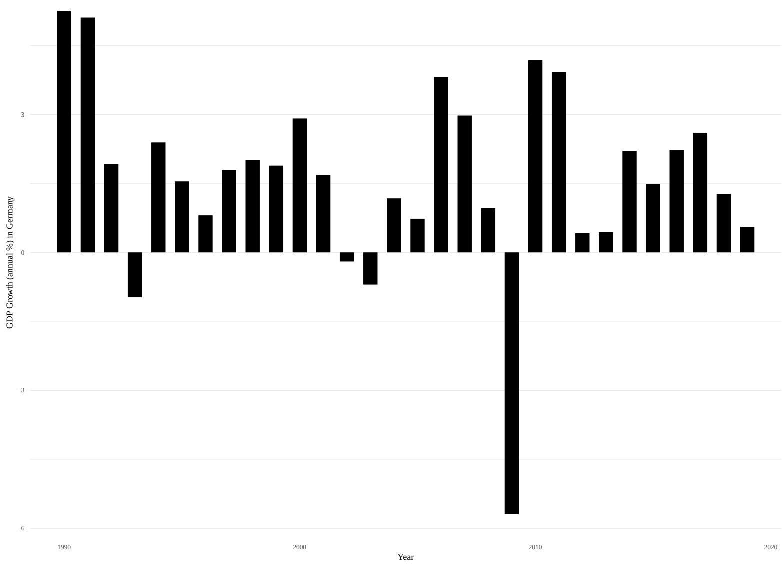
This figure depicts the density of articles around the date of a tax reform by Interest Group. The ice blue facet (dashed) depicts the density of articles when Interest Group equals one and the red facet (solid) depicts the density when Interest Group equals zero. The excess in the density of the ice blue facet (area between the two vertical lines) depicts the period when the instrumental indicator variable IV equals one for all tax reforms: IV marks the weeks preceding a tax reform's date.

Figure 3: Time Trends in Articles' Textual Sentiment and GDP

Panel A Textual Sentiment of Articles

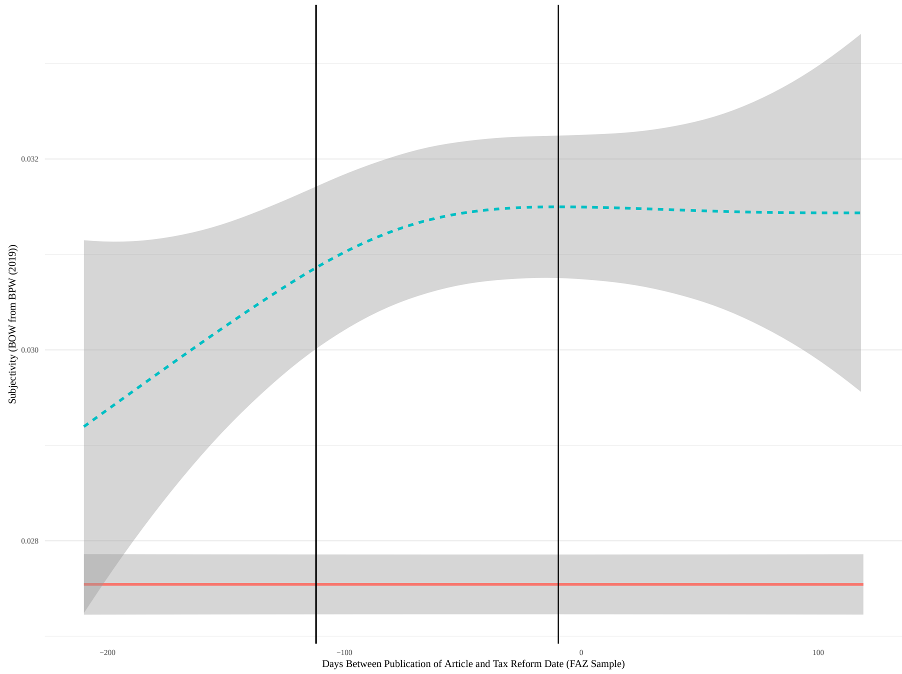


Panel B Annual GDP Growth in Germany



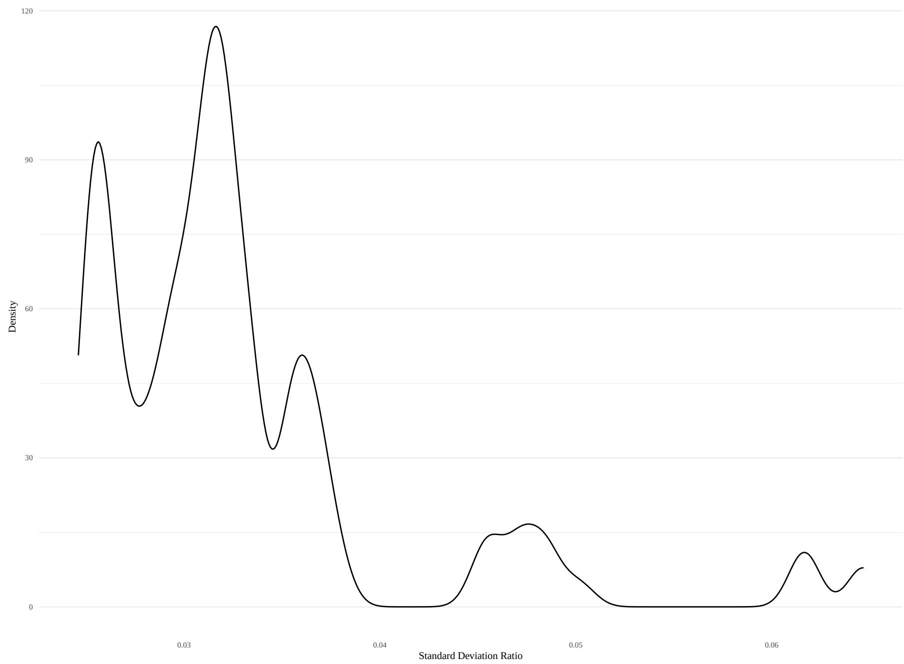
Panel A of this figure depicts the *Textual Sentiment* of each article in the sample throughout the sample period. The smoothed white line displays the time trend in *Textual Sentiment* (and its 95%-confidence interval) and indicates a rather wavy course throughout time. Panel B depicts the annual changes in Germany's Gross Domestic Product and shows a similar development over time.

Figure 4: Articles' Subjectivity - Instrument Validity



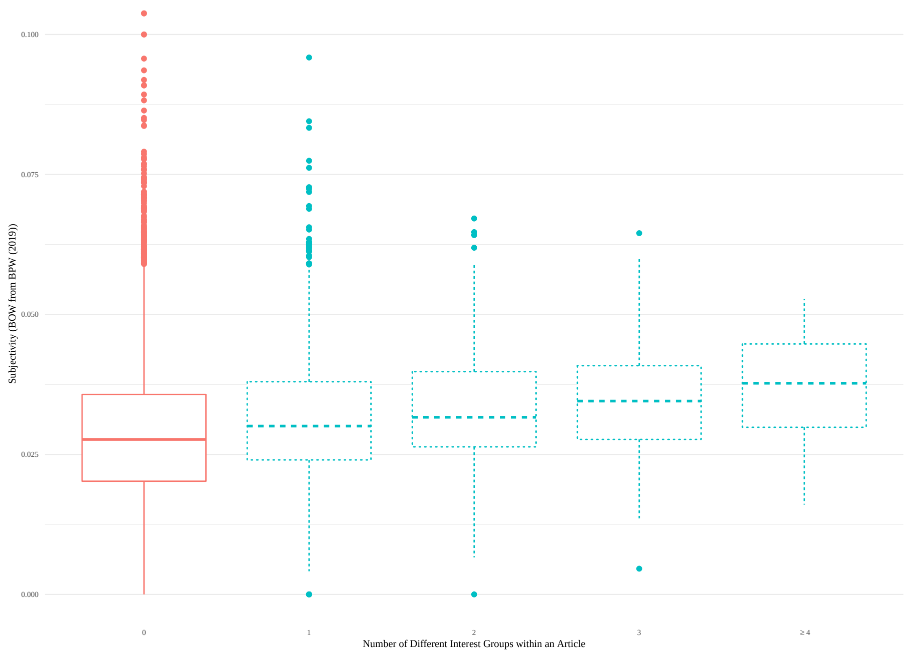
This figure relies on the FAZ only subsample. It depicts smoothed trend lines through the distributions of articles' subjectivity against the difference in days between an article's publication date and the date of the tax reform. The ice blue facet (dashed) depicts articles for which Interest Group equals one and the red facet (solid) depicts articles for which Interest Group equals zero.

Figure 5: Modified Control Function - Density of Standard Deviation Ratio



This figure depicts a kernel density plot of the ratio of the standard deviations of the residuals of two regressions. One regression (first-stage) regresses the focal independent variable of interest (IG) on the control variable. The other regression regresses the dependent variable of interest ($Subjectivity$) on the focal independent variable and the control variables. The interaction of this standard deviation ratio with the residuals from the first-stage regression is included as additional control variable in the regression of interest as $\rho_{Subjectivity}$.

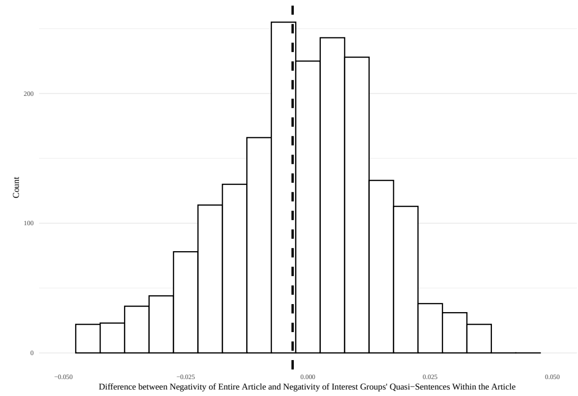
Figure 6: Multiple Interest Groups in Press Coverage



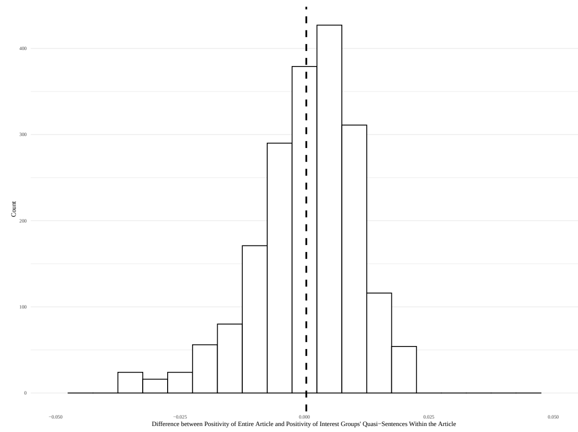
This figure depicts boxplots of *Subjectivity* which condition on the levels of *Interest Group Count*. *Interest Group Count* reflects the number of different interest groups within one article (for analyses binned at ≥ 4). Ice blue coloring (dashed lines) of the boxplots indicate at least one *IG* appearance in an article.

Figure 7: Interest Groups - Differences in Textual Sentiment Within Articles

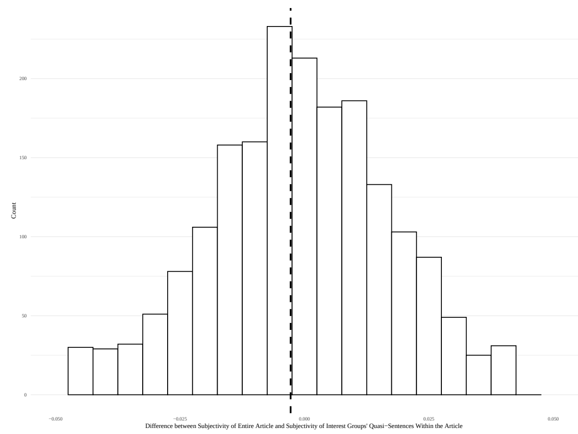
Panel A Negativity



Panel B Positivity



Panel C Subjectivity



This figure depicts the difference between the $Negativity$ of an entire article and the $Negativity$ of the quasi−sentences of an *Interest Group* within this article in Panel A (Panels B and C depict measures for $Positivity$ and $Subjectivity$). Quasi-sentences of interest groups in an article are measured as a ± 1 sentence-window around the sentence in which an IG is identified.

Table 1: Identification of Tax Reforms in Germany

<table>
<tr><th>Keyword</th><th>DIP Search Operator</th><th>Proposal</th><th>Draft</th><th>Resolution</th><th></th></tr>
<tr><td>Steuer</td><td>∗steuer | steuer∗</td><td>45</td><td>534</td><td>236</td><td></td></tr>
<tr><td>Steuergesetz</td><td>∗steuergesetz | ∗steuergesetzes</td><td>23</td><td>126</td><td>44</td><td></td></tr>
<tr><td>Steueraenderungsgesetz</td><td>∗steueraenderungsgesetz | ∗steueraenderungsgesetzes</td><td>0</td><td>22</td><td>12</td><td></td></tr>
<tr><td>Steuerreformgesetz</td><td>∗steuerreformgesetz | ∗steuerreformgesetzes</td><td>0</td><td>14</td><td>6</td><td></td></tr>
<tr><td>Abgabenordnung</td><td>abgabenordnung</td><td>8</td><td>18</td><td>5</td><td></td></tr>
<tr><td>Entlastungsgesetz</td><td>∗entlastungsgesetz | ∗entlastungsgesetzes</td><td>1</td><td>20</td><td>11</td><td></td></tr>
<tr><td>Additional</td><td>-</td><td>0</td><td>8</td><td>4</td><td></td></tr>
<tr><td>Total Tax Reform Documents</td><td></td><td></td><td></td><td></td><td>1,137</td></tr>
</table>

Search terms that are displayed with the Boolean operator | were collected in distinct searches.

Table 2: Sample Selection

Selection Steps	Tax Reforms	Articles
Tax Reform Selection		
Distinct Tax Reforms from 1137 Tax Reform Documents	444	
./. False Positives	31	
./. Unclear Status	104	
./. Missing Reform Identifier	27	
./. Redundancies	3	
./. Double Tax Treaties	133	
Tax Reform Sample for Press Coverage Identification	146	
Press Coverage Selection		
Handcollected Articles from FAZ and SZ between 1990 and 2019	140	13,655
./. Remove Duplicates within Tax Reforms	140	730
./. Manually screen and remove False Positives	140	2,192
Press Coverage of Tax Reforms	140	10,733

Table 3: Summary Statistics

Statistic ($N = 10,733$)	Mean	SD	Min	Median	Max	Mean $IG = 1$
BOW from BPW (2019)						
Textual Sentiment	−0.0102	0.0131	−0.0909	−0.0090	0.0460	−0.0120
Negativity	0.0196	0.0111	0.0000	0.0184	0.0909	0.0220
Positivity	0.0094	0.0064	0.0000	0.0087	0.0536	0.0099
Subjectivity	0.0291	0.0125	0.0000	0.0283	0.1038	0.0319
BOW from Rauh (2018)						
Textual Sentiment	0.0038	0.0251	−0.1404	0.0042	0.1359	0.0029
Negativity	0.0494	0.0176	0.0000	0.0485	0.1754	0.0518
Positivity	0.0532	0.0170	0.0000	0.0527	0.1514	0.0547
Subjectivity	0.1026	0.0238	0.0000	0.1029	0.2105	0.1065
Information on Articles						
Outlet FAZ	0.6419	0.4795	0	1	1	0.6643
Interest Group (IG)	0.1815	0.3854	0	0	1	
Interest Group Count	0.2336	0.5849	0	0	8	
IV	0.3668	0.4820	0	0	1	0.3907
Word Count	574	463	30	488	6,584	675
Title Page	0.1114	0.3147	0	0	1	0.1124
Right over Left MP	−0.0155	0.5671	−1	0	1	−0.0396
BT Sitting	4.6100	5.9428	0	3	36	4.5996

This table depicts the summary statistics of the measures of textual sentiment that are derived from the bows of BPW (2019) and Rauh (2018) in the upper two sections. Information that are derived from the articles in the sample are also presented in this table (lower section). The last column depicts means that are conditional on $IG = 1$. All variables are defined in the Appendix.

Table 4: Regression (2SLS) Results
BOW from BPW (2019)

	Dependent variable:			
	Negativity	*Positivity*	*Subjectivity*	
	FAZ & SZ	FAZ & SZ	FAZ & SZ	FAZ only
Interest Group	0.0288	0.0071	0.0360	0.0349
	(0.0123)	(0.0057)	(0.0146)	(0.0128)
Right over Left MP	−0.0006	−0.0002	−0.0008	−0.0008
	(0.0003)	(0.0001)	(0.0003)	(0.0004)
BT Sitting	−0.00004	0.00001	−0.00003	−0.0001
	(0.00002)	(0.00001)	(0.00003)	(0.00004)
Observations	10,733	10,733	10,733	6,889
Outlet Fixed Effect	Yes	Yes	Yes	-
Title Page Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes	Yes	Yes
First Stage Instrument				
IV	0.0247	0.0247	0.0247	0.0342
	(0.0079)	(0.0079)	(0.0079)	(0.0101)
F Statistic for IV in First Stage	10	10	10	12

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts 2SLS estimates for regressions of multiple measures of articles' textual sentiment on *Interest Group*. Analyses in this table rely on the bow from BPW (2019) when measuring textual sentiment. FAZ only refers to a reduction of the sample to articles from the Frankfurter Allgemeine Zeitung respectively. All variables are defined in the Appendix.

Table 5: Regression (2SLS) Results
BOW from Rauh (2018)

	Dependent variable:			
	Negativity	*Positivity*	*Subjectivity*	
	FAZ & SZ	FAZ & SZ	FAZ & SZ	FAZ only
Interest Group	0.0270	0.0338	0.0608	0.0522
	(0.0158)	(0.0171)	(0.0259)	(0.0214)
Right over Left MP	−0.0004	−0.0001	−0.0005	−0.0005
	(0.0003)	(0.0004)	(0.0006)	(0.0007)
BT Sitting	−0.00004	0.000000	−0.00004	−0.0001
	(0.00003)	(0.00003)	(0.0001)	(0.0001)
Observations	10,733	10,733	10,733	6,889
Outlet Fixed Effect	Yes	Yes	Yes	-
Title Page Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes	Yes	Yes
First Stage Instrument				
IV	0.0247	0.0247	0.0247	0.0342
	(0.0079)	(0.0079)	(0.0079)	(0.0101)
F Statistic for IV in First Stage	10	10	10	12

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts 2SLS estimates for regressions of multiple measures of articles' textual sentiment on *Interest Group*. Analyses in this table rely on the bow from Rauh (2018) when measuring textual sentiment. FAZ only refers to a reduction of the sample to articles from the Frankfurter Allgemeine Zeitung respectively. All variables are defined in the Appendix.

Table 6: Alternative Instrument: Budget Impact (2SLS)

	Dependent variable:	
	Subjectivity FAZ & SZ	*Subjectivity* FAZ & SZ
Interest Group	0.0294	0.0309
	(0.0079)	(0.0130)
Right over Left MP	−0.0008	−0.0005
	(0.0003)	(0.0004)
BT Sitting	−0.00003	−0.00004
	(0.00003)	(0.00004)
Observations	10,733	10,733
Outlet Fixed Effect	Yes	Yes
Title Page Fixed Effect	Yes	Yes
Year Fixed Effects	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes
Textual Sentiment: BOW from	BPW (2019)	Rauh (2018)
First Stage Instrument		
$z\,Tax\ Reform\ Abs.\ Budget\ Impact$	0.0228	0.0228
	(0.0049)	(0.0049)
F Statistic for Instrument in First Stage	25	25

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts 2SLS estimates for a regression of articles' *Subjectivity* on *Interest Group*. The analysis in this table relies on the bow from BPW (2019) in column (1) and on the bow from Rauh (2018) in column (2) when measuring textual sentiment. The tests utilizes an alternative instrumental variable: the absolute value of the expected budget impact of a tax reform. *z* indicates standardization. All variables are defined in the Appendix.

Table 7: OLS Estimator
BOW from BPW (2019)

	Dependent variable:					
	Negativity	*Positivity*	*Subjectivity*			
	FAZ & SZ	FAZ & SZ	FAZ & SZ	$IV = 1$	FAZ only	Red. Form
Interest Group	0.0029	0.0006	0.0035	0.0036	0.0035	
	(0.0003)	(0.0002)	(0.0003)	(0.0005)	(0.0003)	
IV						0.0009
						(0.0003)
Right over Left MP	−0.0006	−0.0002	−0.0008	−0.0013	−0.0005	−0.0008
	(0.0002)	(0.0001)	(0.0002)	(0.0004)	(0.0003)	(0.0002)
BT Sitting	−0.00004	0.00001	−0.00003	−0.00001	−0.0001	−0.00003
	(0.00002)	(0.00001)	(0.00002)	(0.00003)	(0.00003)	(0.00002)
Outlet Fixed Effect	Yes	Yes	Yes	Yes	-	Yes
Title Page Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,733	10,733	10,733	3,937	6,889	10,733
Adjusted R^2	0.0485	0.0269	0.0354	0.0349	0.0345	0.0252

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts OLS estimates. We regress measures of textual sentiment on our focal variable of interest (IG). Columns (4) and (5) depict results when subsamples of the articles are analyzed. Column (6) presents reduced form estimates in which we regress *Subjectivity* on IV. All variables are defined in the Appendix.

Table 8: Modified Control Function
BOW from BPW (2019)

	Dependent variable:		
	Negativity FAZ & SZ	*Positivity* FAZ & SZ	*Subjectivity* FAZ & SZ
Interest Group	0.0052	0.0015	0.0068
	(0.0016)	(0.0009)	(0.0017)
ρ_{neg}	−0.0851		
	(0.0584)		
ρ_{pos}		−0.0556	
		(0.0545)	
ρ_{sub}			−0.1053
			(0.0536)
Right over Left MP	−0.0006	−0.0002	−0.0008
	(0.0002)	(0.0001)	(0.0002)
BT Sitting	−0.00004	0.00001	−0.00003
	(0.00002)	(0.00001)	(0.00002)
Outlet Fixed Effect	Yes	Yes	Yes
Title Page Fixed Effect	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes	Yes
Observations	10,733	10,733	10,733
Adjusted R^2	0.0487	0.0269	0.0357

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts results for the main specification when employing the modified control function approach by Klein and Vella (2010), where ρ is added as an additional regressor to the analysis. See text and variable descriptions for details on its construction. All variables are defined in the Appendix.

Table 9: Multiple Interest Groups within Articles (OLS)
BOW from BPW (2019)

	Dependent variable:			
	Negativity	*Positivity*	*Subjectivity*	
	FAZ & SZ	FAZ & SZ	FAZ & SZ	$IV = 1$
Interest Group Count 1	0.0024	0.0006	0.0030	0.0030
	(0.0003)	(0.0002)	(0.0003)	(0.0006)
Interest Group Count 2	0.0043	0.0002	0.0045	0.0043
	(0.0006)	(0.0004)	(0.0007)	(0.0010)
Interest Group Count 3	0.0059	0.0008	0.0067	0.0099
	(0.0012)	(0.0006)	(0.0012)	(0.0016)
Interest Group Count ≥ 4	0.0064	0.0023	0.0087	0.0095
	(0.0018)	(0.0012)	(0.0015)	(0.0030)
Right over Left MP	−0.0006	−0.0002	−0.0008	−0.0012
	(0.0002)	(0.0001)	(0.0002)	(0.0004)
BT Sitting	−0.00004	0.00001	−0.00003	−0.00001
	(0.00002)	(0.00001)	(0.00002)	(0.00003)
Outlet Fixed Effect	Yes	Yes	Yes	Yes
Title Page Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Reform Outcome Fixed Effects	Yes	Yes	Yes	Yes
β_1 vs. β_4 (p-value)	(0.0246)	(0.1682)	(0.0002)	(0.0353)
Observations	10,733	10,733	10,733	3,937
Adjusted R^2	0.0498	0.0270	0.0366	0.0374

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts OLS estimates from specifications which regress measures of textual sentiment on a factor variable which captures the number of distinct interest groups that can be identified in an article (*Interest Group Count*, top coded at 4). All variables are defined in the Appendix.

Table 10: Quasi-Sentences of Interest Groups (2SLS)
BOW from BPW (2019)

	Dependent variable:		
	Altered Negativity FAZ & SZ	*Altered Positivity* FAZ & SZ	*Altered Subjectivity* FAZ & SZ
Interest Group	0.0401	0.0051	0.0452
	(0.0158)	(0.0066)	(0.0179)
Right over Left MP	−0.0009	−0.0001	−0.0010
	(0.0004)	(0.0001)	(0.0004)
BT Sitting	−0.0001	0.00001	−0.0001
	(0.00003)	(0.00001)	(0.00004)
Observations	10,733	10,733	10,733
Outlet Fixed Effect	Yes	Yes	Yes
Title Page Fixed Effect	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Reform Outcome FEs	Yes	Yes	Yes

Superscripts are not used to indicate statistical significance. Robust standard errors are in parentheses. This table depicts 2SLS estimates when altering the outcome variables to the measures of textual sentiment that can be identified in quasi-sentences around interest groups' appearances in press coverage. I construct quasi-sentences (a ±1 sentence-window around the sentence in which an IG is identified) of interest groups in articles and respectively measure textual sentiment of the quasi-sentences in these articles ($Altered$). When IG equals 0, measures of textual sentiment remain unchanged.

Table 11: Staleness

Panel A Quasi Sentences

	Dependent variable:	
	$StaleJacc_{(IG\ Quasi\ Sent.)}$	$StaleCos_{(IG\ Quasi\ Sent.)}$
Reform Match	0.0130	0.0329
	(0.0021)	(0.0032)
Outlet Fixed Effect	Yes	Yes
Title Page Fixed Effect	Yes	Yes
Year Fixed Effects	Yes	Yes
Reform Fixed Effects	Yes	Yes
Observations	513,710	513,710
Adjusted R^2	0.0393	0.0333
Average $Stale_{(IG\ Quasi\ Sent.)}$	0.0408	0.0856

Panel B Entire Articles

	Dependent variable:	
	$StaleJacc_{(Article)}$	$StaleCos_{(Article)}$
Reform Match	0.0085	0.0386
	(0.0016)	(0.0019)
Outlet Fixed Effect	Yes	Yes
Title Page Fixed Effect	Yes	Yes
Year Fixed Effects	Yes	Yes
Reform Fixed Effects	Yes	Yes
Observations	94,984,196	94,984,196
Adjusted R^2	0.2279	0.1035
Average $Stale_{(Article)}$	0.0299	0.0491

Panel C Comparison

	Difference *StaleJacc*	Difference *StaleCos*
$Stale_{(IG\ Quasi\ Sent.)} - Stale_{(Article)}$	0.0108	0.0365
	[0.0108; 0.0109]	[0.0363; 0.0367]
$Stale_{(IG\ Quasi\ Sent.)} - Stale_{(Article)}\ _{[Reform\ Match=1]}$	0.0160	0.0312
	[0.0154; 0.0165]	[0.0301; 0.0323]
$\widehat{Stale}_{(IG\ Quasi\ Sent.)} - \widehat{Stale}_{(Article)}$	0.0108	0.0365
	[0.0108; 0.0108]	[0.0365; 0.0366]

Superscripts are not used to indicate statistical significance. Robust standard errors [95% confidence intervals] are in parentheses [squared brackets]. I construct two textual staleness measures (Jaccard similarity (*StaleJacc*) and cosine similarity (*StaleCos*)) for all articles against each other and for all quasi-sentences (at the document level) against each other (e.g., *StaleJacc* for one specific article is calculated against the remaining 10K articles in the sample, creating as many observations). The upper two Panels test whether these measures actually capture staleness by demonstrating that the similarity of articles (Panel B) and quasi-sentences (Panel A) increases within reforms (capturing the idea that articles that belong to one reform are likely to be more similar). I regress (using OLS) *Stale* on article characteristics and *Reform Match* which is an indicator variable that equals one when staleness is calculated against an article/quasi-sentence that belongs to the same reform. Panel C depicts results from Welch two sample t-tests that compare averages of staleness between quasi-sentences and articles (first row), when *Reform Match* equals one (second row), and when utilizing predicted values of staleness from the regressions in Panel A and Panel B. All variables are defined in the Appendix.

Online Supplement

Press Coverage of Tax Reforms and Interest Groups

Identification of Interest Groups

[Table OS1 about here.]

[Table OS2 about here.]

Table OS1 depicts the regular expressions that are used to identify the appearance of an *Interest Group* in the collected press coverage. The term "Hits" listed in columns (2) and (3) refers to matches of the regular expressions in the manually collected articles before applying press coverage selection steps. To test the quality of the patterns, I run the regular expressions against a manually collected .txt-file (based on a randomly chosen interest-group-input list from DIP Bundestag) in which each line contains one name of an registered interest group. This exercise indicates sufficient quality of the hits with a hit-percentage of 41.67%, meaning that the 31 patterns in Table OS1 identify 1872 in this randomly chosen list of interest groups' names. Table OS2 provides a translation of the regular expressions to English.

Top Tokens in Context

[Table OS3 about here.]

Table OS3 depicts the 100 most common features (i.e. the regular expression plus a contextual window of 1-5 words) which are identified by applying the regular expressions in Table OS1 to the FAZ articles. The applied regular expressions are "broad" enough to allow for the identification of a varying number of different interest groups in the articles. The regular expressions also identified grammatical features, such as the attached "-s" or "-es" for the genitive cases, without obvious error. Furthermore, false positive identifications of interest groups are not depicted in this table. For instance, "verband deutscher maschinen und anlagenbau" is identified by using the regular expressions, while the standalone German term for bandage "verband" is not falsely classified as an interest group reference.

Table OS1: Identification of Interest Groups: Regular Expressions

<table>
<tr><th>Regular Expression</th><th>Hits in FAZ</th><th>Hits in SZ</th></tr>
<tr><td>\bbundesverband</td><td>613</td><td>260</td></tr>
<tr><td>\bbundesverbände</td><td>1</td><td>2</td></tr>
<tr><td>\bverband(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bdeutsch</td><td>129</td><td>53</td></tr>
<tr><td>\bdeutsch(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bgesellschaft</td><td>43</td><td>12</td></tr>
<tr><td>\bberufsverband</td><td>3</td><td>3</td></tr>
<tr><td>\bbundesarbeitsgemeinschaft</td><td>12</td><td>8</td></tr>
<tr><td>\bbundesvereinigung</td><td>98</td><td>19</td></tr>
<tr><td>\bzentralverband</td><td>161</td><td>55</td></tr>
<tr><td>\bdeutsch(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)verband</td><td>20</td><td>4</td></tr>
<tr><td>\bbund(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bdeutsch</td><td>8</td><td>7</td></tr>
<tr><td>\bgewerkschaft</td><td>606</td><td>352</td></tr>
<tr><td>\bfachverband</td><td>7</td><td>4</td></tr>
<tr><td>\bfachverbände</td><td>11</td><td>1</td></tr>
<tr><td>\bgesamtverband</td><td>39</td><td>26</td></tr>
<tr><td>\bgesamtverbände</td><td>0</td><td>0</td></tr>
<tr><td>\bspitzenverband</td><td>21</td><td>8</td></tr>
<tr><td>\bspitzenverbände</td><td>166</td><td>77</td></tr>
<tr><td>\bdachverband</td><td>24</td><td>8</td></tr>
<tr><td>\bdachverbände</td><td>1</td><td>1</td></tr>
<tr><td>\binteressengemeinschaft</td><td>1</td><td>5</td></tr>
<tr><td>\binteressenverband</td><td>13</td><td>3</td></tr>
<tr><td>\binteressenverbände</td><td>10</td><td>7</td></tr>
<tr><td>\barbeitsgemeinschaft(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bdeutsch</td><td>7</td><td>5</td></tr>
<tr><td>\bindustrieverband</td><td>38</td><td>9</td></tr>
<tr><td>\bindustrieverbände</td><td>7</td><td>4</td></tr>
<tr><td>\bwirtschaftsverband</td><td>8</td><td>0</td></tr>
<tr><td>\bwirtschaftsverbände</td><td>111</td><td>92</td></tr>
<tr><td>\bunternehmerverband</td><td>16</td><td>4</td></tr>
<tr><td>\bunternehmerverbände</td><td>4</td><td>1</td></tr>
<tr><td>\binstitut(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bdeutsch(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bwirtschaft</td><td>51</td><td>19</td></tr>
<tr><td>\binstitut(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bmakroökonomie</td><td>9</td><td>3</td></tr>
</table>

The term "Hits" refers to matches of the regular expressions in the manually collected articles before applying press coverage selection steps.

Table OS2: Identification of Interest Groups: Regular Expressions in English

<table>
<tr><th>Regular Expression</th><th>Hits in FAZ</th><th>Hits in SZ</th></tr>
<tr><td>\bfederal association</td><td>613</td><td>260</td></tr>
<tr><td>\bfederal associations</td><td>1</td><td>2</td></tr>
<tr><td>\bassociation(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bgerman</td><td>129</td><td>53</td></tr>
<tr><td>\bgerman(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bsociety</td><td>43</td><td>12</td></tr>
<tr><td>\bprofessional association</td><td>3</td><td>3</td></tr>
<tr><td>\bfederal workinggroup</td><td>12</td><td>8</td></tr>
<tr><td>\bfederal association</td><td>98</td><td>19</td></tr>
<tr><td>\bcentral association</td><td>161</td><td>55</td></tr>
<tr><td>\bgerman(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)association</td><td>20</td><td>4</td></tr>
<tr><td>\bfederal(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bgerman</td><td>8</td><td>7</td></tr>
<tr><td>\bunion</td><td>606</td><td>352</td></tr>
<tr><td>\bprofessional association</td><td>7</td><td>4</td></tr>
<tr><td>\bprofessional associations</td><td>11</td><td>1</td></tr>
<tr><td>\btotal association</td><td>39</td><td>26</td></tr>
<tr><td>\btotal associations</td><td>0</td><td>0</td></tr>
<tr><td>\bumbrella association</td><td>21</td><td>8</td></tr>
<tr><td>\bumbrella associations</td><td>166</td><td>77</td></tr>
<tr><td>\broof association</td><td>24</td><td>8</td></tr>
<tr><td>\broof associations</td><td>1</td><td>1</td></tr>
<tr><td>\binterest group</td><td>1</td><td>5</td></tr>
<tr><td>\binterest association</td><td>13</td><td>3</td></tr>
<tr><td>\binterest associations</td><td>10</td><td>7</td></tr>
<tr><td>\bworking group(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bgerman</td><td>7</td><td>5</td></tr>
<tr><td>\bindustry association</td><td>38</td><td>9</td></tr>
<tr><td>\bindustry associations</td><td>7</td><td>4</td></tr>
<tr><td>\beconomic association</td><td>8</td><td>0</td></tr>
<tr><td>\beconomic associations</td><td>111</td><td>92</td></tr>
<tr><td>\bentrepreneur association</td><td>16</td><td>4</td></tr>
<tr><td>\bentrepreneur associations</td><td>4</td><td>1</td></tr>
<tr><td>\binstitute(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bgerman(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\beconomy</td><td>51</td><td>19</td></tr>
<tr><td>\binstitute(\s+|\s+\w{1,3}\s+|.\s+|.\s+\w{1,3}\s+|..\s+|..\s+\w{1,3}\s+)\bmacroeconomics</td><td>9</td><td>3</td></tr>
</table>

This table shows regular expression that are, for illustration purposes. translated to English. Please note that German terminology may be nuanced so that some expressions translate to the same term in English.

Table OS3: Interest Groups: Top Tokens in Context

1	2	3
acht spitzenverbände	angestellten gewerkschaft	arbeitgeber und gewerkschaften
auch der bundesverband	beim bundesverband	beim bundesverband der deutschen
beim bundesverband der deutschen industrie	bericht der bundesverband	bonn eigener bericht der bundesverband
bundesarbeitsgemeinschaft	bundesverband	bundesverband der deutschen
bundesverband der deutschen gas und wasserwirtschaft	bundesverband der deutschen industrie	bundesverband des deutschen
bundesverband des deutschen groß	bundesverband des deutschen groß und außenhandels	bundesverband deutscher
bundesverband deutscher banken	bundesverband freier	bundesverband freier wohnungsunternehmen
bundesverband güterkraftverkehr	bundesverband güterkraftverkehr logistik	bundesverband güterkraftverkehr logistik und entsorgung
bundesverband mittelständische	bundesverband mittelständische wirtschaft	bundesverband mittelständische wirtschaft bvmw
bundesverbandes	bundesverbandes der deutschen	bundesverbandes der deutschen industrie
bundesverbandes der deutschen industrie bdi hans	bundesverbandes des deutschen	bundesverbandes des deutschen groß
bundesverbandes des deutschen groß und außenhandels	bundesverbandes deutscher	bundesverbandes deutscher banken
bundesverbandes freier	bundesverbandes güterkraftverkehr	bundesverbands
bundesverbands der deutschen	bundesverbands der deutschen industrie	bundesverbands freier
bundesverbands freier wohnungsunternehmen	bundesvereinigung	bundesvereinigung der deutschen
bundesvereinigung der deutschen arbeitgeberverbände	dachverband	deutsche gesellschaft
deutsche gewerkschaftsbund	deutschen angestellten gewerkschaft	deutschen gesellschaft
deutschen gewerkschaftsbundes	deutschen steuer gewerkschaft	deutschen verbandes
eigener bericht der bundesverband	fachverband	gesamtverband
gesamtverband der deutschen	gesamtverband der deutschen versicherungswirtschaft	gesamtverband der wohnungswirtschaft
gesamtverbandes	gewerkschaft	gewerkschaft der eisenbahner
gewerkschaft der eisenbahner deutschlands	gewerkschaft erziehung	gewerkschaft erziehung und wissenschaft
gewerkschaft verdi	gewerkschaften	gewerkschaften haben
gewerkschaften und bundesregierung	gewerkschaften und unternehmen	gewerkschafter
gewerkschafterin	gewerkschaftern	gewerkschaftsbund
gewerkschaftsbundes	gewerkschaftschef	gewerkschaftsmitglieder
gewerkschaftstag	großen wirtschaftsverbände	hauptgeschäftsführer des bundesverbandes
hauptgeschäftsführer des bundesverbands	industrieverband	institut der deutschen wirtschaft
instituts der deutschen wirtschaft	interessengemeinschaft	interessenverband
interessenverbände	jahrespressekonferenz des bundesverbandes	jahrespressekonferenz des bundesverbandes deutscher
kommunalen spitzenverbände	kommunalen spitzenverbänden	präsident der bundesvereinigung
präsident der bundesvereinigung der deutschen	präsident der bundesvereinigung der deutschen arbeitgeberverbände	präsident des bundesverbandes
präsident des bundesverbandes der deutschen	präsident des bundesverbandes der deutschen industrie	präsident des bundesverbandes des deutschen
präsident des bundesverbandes des deutschen groß	präsident des bundesverbands	präsident des zentralverbandes
präsident des zentralverbandes des deutschen	präsident des zentralverbandes des deutschen handwerks	präsident des zentralverbands
pressekonferenz des bundesverbandes	spitzenverband	spitzenverbände
spitzenverbände der deutschen	spitzenverbände der deutschen wirtschaft	spitzenverbände der wirtschaft
spitzenverbänden	steuer gewerkschaft	unternehmerverband
verband deutscher	verband deutscher hypothekenbanken	verband deutscher maschinen
verband deutscher maschinen und anlagenbau	verband deutscher maschinen und anlagenbau vdma	verband deutscher verkehrsunternehmen
verbandes deutscher	verbandes deutscher hypothekenbanken	vorsitzende des deutschen gewerkschaftsbundes
wirtschaft und gewerkschaften	wirtschaftsverbände	wirtschaftsverbänden
zentralverband	zentralverband des deutschen	zentralverband des deutschen baugewerbes
zentralverband des deutschen handwerks	zentralverbandes	zentralverbandes des deutschen
zentralverbandes des deutschen handwerks	zentralverbands	zentralverbands des deutschen
zentralverbands des deutschen handwerks		

This table depicts (in alphabetical order) the overlap of the 100 most common features ("top tokens") in FAZ and SZ (i.e. the regular expression plus a contextual window of 0-5 words) which are identified by applying the regular expressions in Table OS1 articles.

Tax Knowledge Diffusion via Strategic Alliances*

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Tax Knowledge Diffusion via Strategic Alliances

Abstract

We empirically identify tax knowledge diffusion via strategic alliances by documenting economically meaningful decreases in effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to pseudo treated high-tax firms in strategic alliances with other high-tax firms. Additional analyses reveal that elapsed time facilitates tax knowledge diffusion. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. Furthermore, we find that shared industry affiliation rather inhibits tax knowledge diffusion. Our inferences persist when analyzing shared audit firms and board ties as alternative channels. We also show that tax knowledge diffusion appears ex ante unintended by analyzing (i) abnormal returns to the announcements of strategic alliances and (ii) differences between the partners' market shares. Overall, our study documents the impact of close cooperation and continued exchange in strategic alliances on undersheltered firms' willingness to engage in tax planning.

- Working Paper -

Keywords: Tax Planning, Strategic Alliance, Knowledge Diffusion

Data: Sources identified in the text

Declaration of Interest: The authors declare no conflicts of interest

1 Introduction

This study provides a novel tax perspective on the question "when you work with a superman, will you also fly?" (Tan and Netessine 2019). Focusing on strategic alliances, a highly relevant form of contract-based collaboration between at least two firms (PwC 2018), we elucidate undersheltered "high-tax" firms' changes in tax planning. Specifically, our analysis reveals that high-tax firms increase their tax planning after initiating strategic alliances with tax aggressive "low-tax" firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. In essence, we document the impact of close cooperation and continued exchange in strategic alliances on firms' willingness to engage in tax planning. Our empirical evidence, thus, complements the interview insights by Mulligan and Oats (2016), suggesting that informal private exchange may reduce the expected costs of tax planning. Analyzing changes in tax planning, as a matter outside the scope of an alliance's main business purpose, further highlights the complex tension between knowledge diffusion and protection in alliances (e.g., see Palomeras and Wehrheim (2021)). Taken together, our study reveals tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010).

Tax planning, conceptually, results from a firm specific equilibrium of expected tax costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). Its key benefits, lower tax payments, are rather simple to predict, also because specific tax planning tools are mass-market tax advisory products (e.g., see Lisowsky (2010) and Wilson (2009)). Low-tax firms are particularly good in managing and reducing actual tax costs (e.g., transfer price documentation that has been accepted by the IRS) or expect low potential tax costs (e.g., audit or reputational costs). If this tax knowledge diffused to high-tax firms, the assessment of tax costs by high-tax firms could change, too. Observing changes in tax planning in our analysis would then be the consequence of an updated equilibrium of expected costs and benefits of tax planning. Importantly, strategic alliances are not subject to corporate income taxation because they do not establish a separate legal entity, but all partners of a strategic alliances individually. This institutional characteristic precludes confounding mechanical tax effects upon investment and allows us to isolate the effect of tax knowledge diffusion and overcomes a key challenge that occurs with other forms of investment, such as M&A.

Strategic alliances are expected to foster their main business purposes and to facilitate (intended) transfers of related knowledge between the cooperating firms (e.g., K. Li, Qiu, and Wang (2019) identify significant increases in firms' innovative capacity when investing in R&D strategic alliances). Conceptually, a strategic alliance could also stir tax knowledge diffusion because information exchange, due to trust, and mutual commitment, as a consequence of collaboration, may exceed the initially intended scope (e.g., see Boone and Ivanov (2012) and Yin and Shanley (2008) on increased mutual commitment in alliances). However, the ex ante unintended diffusion of tax knowledge would be a valuable "private benefit" for the high-tax firm (e.g., see Anderson et al. (2014)) for which the low-tax firm is not compensated, e.g. in form of joint tax planning. Analyzing tax knowledge diffusion in strategic alliances, thus, is distinct and independent from intentional transfers of tax knowledge in peer-to-peer relationships to facilitate joint tax planning (Cen et al. 2017, 2020) and from intentional transfers and acquisitions via intermediaries (e.g., the client-bank-client relationships in Gallemore, Gipper, and Maydew (2019)).

To address our research question, we utilize information on strategic alliances that are initiated between publicly traded US firms from 1994 to 2021. Given that accounting data are available for an alliance's partners, we reshape the data from the alliance to the partners' levels (i.e., one observation per partner of an alliance). Our variable of interest to measure tax planning is the effective tax rate as the commonly employed proxy for a firm's nonconforming tax planning behavior. We then classify the partners in an alliance as low-tax and high-tax firms depending on their industry-multiperiod-mean adjusted cash effective tax rates in the run-up to the initiation of an alliance. To tease out tax knowledge diffusion, we analyze changes in our tax planning measure of high-tax firms in strategic alliances with low-tax firms in comparison to high-tax firms in strategic alliances with high-tax firms. Our control-group, thus, represents pseudo treated firms with on average identical chances and challenges after initiating a strategic alliance as the treated firms except for the fact that their alliance has no low-tax partner.

Our main analysis reveals that high-tax firms increase their tax planning (i.e., decrease their (cash) effective tax rates) after initiating strategic alliances with low-tax firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. First, we employ descriptive statistics and a univariate analysis to understand whether eventual treatment effects would stem from unexpected variation in the outcome variables of interest among the control observations. We then corroborate this analysis by multivariable regression analyses in which we control, based on textual analysis, for the alliances' business purposes, partner characteristics, alternative channels, and within-firm determinants of tax planning. Because these covariates account for a broad range of alternative explanations, we find it plausible to associate the (relative) increase in tax planning for treated high-tax firms with the presence of a low-tax firm in the alliance. Additionally and even though data indicate that the high-tax firms in our sample are in very similar situations except for potentially experiencing tax knowledge diffusion, we employ, in the interest of caution and methodological thoroughness, entropy-balancing weighting (Hainmueller 2012; Hainmueller and Xu 2013) and use multiple calculations of entropy weights to re-estimate the main analysis. Overall, our findings indicate that tax planning responses to "working with superwoman", suggesting tax knowledge diffuses via strategic alliances. Importantly, the average treatment effect on the treated is neither surprisingly large nor negligibly small. Our estimates indicate, consistent across a broad set of specifications, a reduction of 2.5 to 3 percentage points in a treated firm's effective tax rate, supported by a 95% confidence interval of $[-0.0552, -0.0052]$ in the preferred specification. Considering a sample average of ~29% pretreatment, the identified decreases are both statistically and economically significant.

We then test whether our treatment assignment is indeed plausibly exogenous by analyzing abnormal returns to the announcements of strategic alliances. Our idea is that if tax knowledge diffusion could be anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study and find, consistent with the findings by Chan et al. (1997), that cumulative abnormal returns are on average positive across all announcements in our sample. Importantly, we observe, counter to the idea of anticipation of tax knowledge diffusion, that abnormal returns of control firms' announcements exceed the abnormal returns for the treated firms. We conclude that tax knowledge diffusion via strategic alliances is not anticipated, reducing remaining concerns about endogenous treatment assignment.

Next, we turn to the mechanisms of tax knowledge diffusion via strategic alliances. First, we focus on elapsed time because knowledge diffusion is a gradual, multi-stage process (Bresman, Birkinshaw, and Nobel 2010; Inkpen 2000; Szulanski 1996) and elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv 2003;

Isaksson, Simeth, and Seifert 2016). To test whether elapsed time facilitates tax knowledge diffusion, we extend the posttreatment period gradually over multiple specifications of our main analysis. The results indicate that the treatment effect particularly increases in magnitude with elapsed time since the initiation of an alliance. In particular, the estimates for our treatment variable of interest turn significant and expand economically with elapsed time. We conclude that elapsed time, increasing the probability of information exchange due to trust and mutual commitment, facilitates tax knowledge diffusion via strategic alliances. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. We find, broadly consistent with the inferences by Brown and Drake (2014) and Brown (2011), that a shared industry affiliation rather inhibits tax knowledge diffusion via strategic alliances. Finally, we turn to differences in the partners' market shares to test whether tax knowledge diffusion is indeed unintended or originates from power-dynamics. We find no evidence for a power-induced mechanism but, consistent with trust and mutual commitment, that tax knowledge diffusion is unintended. Throughout all cross sectional analyses the baseline estimate for the treatment persists in direction, magnitude, and statistical significance in comparison to the inferences from our main analysis.

Furthermore, we examine how tax knowledge diffusion via strategic alliances is impacted when partners share an audit firm because we are interested in whether the identified effects are robust to alternative channels of intentional tax knowledge transfers. We find that the results from our main analysis persist and that we do not spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances. The same holds when we analyze board ties between the firms in our sample. We then run a battery of robustness checks and analyze alternative tax planning measures, pretreatment volatility in effective tax rates, low-tax firms, and probabilities of valuation allowance releases. The results from these analyses support our identification strategy and inferences: we capture tax planning responses of the treated high-tax firms due to tax knowledge diffusion.

Our study reveals tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010). Specifically, we document the impact of close cooperation and continued exchange in strategic alliances on firms' willingness to engage in tax planning. Our empirical evidence not only complements the interview insights by Mulligan and Oats (2016), suggesting that informal private exchange may reduce the expected costs of tax planning, but also contributes to the literature that formalizes tax planning as an equilibrium of expected tax costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). We utilize an institutional feature of strategic alliances, the absence of mechanical tax effects at the firm level upon investment, that allows us to tie observed changes in tax planning to an update of this equilibrium. Our findings, thus, not only inform research but also offer valuable insights for practitioners and policymakers by elucidating how fostering collaboration through strategic alliances can influence firms' tax planning decisions.

Analyzing tax knowledge diffusion via strategic alliances, furthermore, contributes to the literature on cross-firm connections as determinants of tax planning. Importantly, our analysis is distinct and independent from intentional transfers of tax knowledge in peer-to-peer relationships to facilitate joint tax planning (Cen et al. 2017, 2020) and from intentional transfers and acquisitions via intermediaries (e.g., the client-bank-client relationships in Gallemore, Gipper, and Maydew (2019)). The focus in these and related studies (e.g., on board-ties (Brown 2011; Brown and Drake 2014), human capital turnover (Barrios and Gallemore 2024), auditors (McGuire, Omer, and Wang (2012), Klassen, Lisowsky, and Mescall (2016)), and law firms (Acito and Nessa 2022)) is on selective transactions that can be ex ante intended to affect a firm's tax planning. We empirically test whether capital markets anticipate tax knowledge diffusion via strategic alliances and find that this is not the case when comparing returns at announcements for treated and pseudo treated firms. Consistent evidence from analyzing differences in the partners' market shares further suggests that tax knowledge diffusion is unintended and not power-induced. Thus, our study underscores the importance of considering tax knowledge diffusion as unique and economically important yet unintended effect of a relevant cross-firm connection: cooperation in strategic alliances.

Finally, our study theoretically builds on and contributes to research that examines knowledge in the context of strategic alliances. Related findings frequently highlight the knowledge-related benefits of investments in strategic alliances but the focus is typically on knowledge in the context of a network's main business purpose (e.g., see K. Li, Qiu, and Wang (2019) on R&D alliances and their effects on firms' innovative capacities). Consistently, analyses of knowledge protection in strategic alliances suggest that firms especially attempt to safeguard themselves with respect to the main business purpose of the network (D. Li et al. 2008; Palomeras and Wehrheim 2021). We argue that close cooperation, trust, mutual commitment, and continued exchange with a low-tax firm in a strategic alliance could also stir the diffusion of tax knowledge, as matter outside the scope of an alliance's main business purpose, and find consistent evidence. Our evidence thus contributes to a deeper understanding of knowledge diffusion via strategic alliances, particularly concerning tax knowledge.

2 (Tax) Knowledge Diffusion: Conceptual Underpinnings

Generally, knowledge diffusion requires communication through channels over time among members of a social system (Rogers 2003). Moreover, a firm must not only gain access to knowledge but must also deploy an approach to utilize the knowledge. Otherwise, knowledge diffusion cannot contribute to a firm's knowledge profile (Kale, Singh, and Bell 2009; Mazloomi Khamseh, Jolly, and Morel 2017). We deduce that (tax) knowledge diffusion conceptually comprises gaining access to and being willing to and capable of employing relevant tax knowledge.

Within this framework, there are several aspects that speak in favor of tax knowledge diffusion via strategic alliances. Generally, strategic alliances force firms to commit investment and other support to common goals (Yin and Shanley 2008). Consistently, cooperation is found to mitigate cultural differences between partners (Kogut and Singh 1988). Furthermore, Kale, Singh, and Bell (2009) argue that firms should create a dedicated management structure to oversee and support their alliance activities. While research on knowledge protection in strategic alliances emphasizes firms' efforts to safeguard proprietary knowledge pertinent to the main business purpose of an alliance (D. Li et al. 2008; Palomeras and Wehrheim 2021), it is evident that strategic alliances facilitate transfers of such critical knowledge between the cooperating firms (e.g., K. Li, Qiu, and Wang (2019) identify significant increases in firms' innovative capacity when investing in R&D strategic alliances). We conclude that close cooperation, trust, mutual commitment, and continued exchange with a low-tax firm in a strategic alliance could also stir the diffusion of tax knowledge, as matter outside the scope of an alliance's main business purpose.[1] Consistently, Mulligan and Oats (2016) note that "sharing information, particularly about tax plans and technical advice about dealing with ambiguities in tax laws serves to provide legitimacy to preferred tax positions, yielding a form of power [...] when taking tax positions in

[1] Note that protection against tax knowledge diffusion is aggravated because tax knowledge comes with little to no legal protection, as exists, for instance, for intellectual property (for the general implications of weak knowledge protection, see Zhao (2006)).

dealing with Revenue Authorities" (p. 70). These insights suggest that informal private exchange may particularly reduce the expected costs of tax planning (including, for instance, the expected reputational costs (Austin and Wilson 2017; Gallemore, Maydew, and Thornock 2014; Graham et al. 2014; Hanlon and Slemrod 2009)). Tax planning might, thus, response as a consequence of an updated equilibrium of expected costs and benefits of tax planning (Jacob, Rohlfing-Bastian, and Sandner 2021).

However, "not all corporate practices diffuse in the same way" (Y. Cai et al. 2014, 1087). Exemplary barriers are knowledge-related factors, such as limits to a recipient's absorptive capacity (Dyer and Hatch 2006; Szulanski 1996). Additionally, imposing constraints on knowledge diffusion increases a firm's return from having a sophisticated knowledge profile (Akcigit and Ates 2019). Furthermore, tax knowledge diffusion is a private benefit (i.e., it is especially valuable outside the scope of an alliance). Such private benefits can harm the partners' efforts to strive for the alliance's common benefits (Khanna, Gulati, and Nohria 1998), shift partners' bargaining power, and finally induce instability to the alliance (Inkpen and Beamish 1997; Khanna, Gulati, and Nohria 1998). Additionally, Desai, Foley, and Hines (2004) suggest that shared ownership of equity joint ventures impacts the fine-tuning of tax planning of these entities, and both cooperation (Chen, King, and Wen 2015) and tax planning (Dyreng, Hanlon, and Maydew 2019) are found to induce uncertainty, which may induce marginal disutility from tax knowledge diffusion. Corporate culture and governance further impact a firm's decisions on tax planning (Armstrong et al. 2015; Klassen, Lisowsky, and Mescall 2017). Thus, it is an empirical question if and when tax knowledge diffuses via strategic alliances.

3 Data & Identification Strategy

[Table 1 about here.]

3.1 Strategic Alliances

We exploit data on strategic alliances from Refinitiv's SDC Platinum (SDC) database on strategic alliances over the 1994-2021 period. SDC is widely used in relevant research on corporate cooperation (Anand and Khanna 2000; Boone and Ivanov 2012; Y. Cai and Sevilir 2012; Chen, King, and Wen 2015; Ishii and Xuan 2014; K. Li, Qiu, and Wang 2019) and tracks a very wide range of agreement types (Schilling 2009). SDC issues data at the strategic alliance level. Initially, we deflate our sample to observations that are flagged as strategic alliances by excluding equity joint ventures from the data. We then reshape data from the alliance to the partner level (i.e., one observation per firm in an alliance) because strategic alliances are (unlike equity joint ventures) not subject to corporate taxation but the investing partners. To illustrate: a strategic alliance between *two* partners translates to one observation for each of the two partners (i.e., *two* observations). Compustat data (via Wharton Research Data Services) provide firm-year-level accounting information, and we merge SDC and Compustat data by using a firm's historical six-digit CUSIP number (at the level of the ultimate parent of the participant). Although SDC provides reliable network observations from the beginning of 1990 onward, we start in 1994, consistent with many tax studies. Furthermore, we respectively consider strategic alliances between publicly traded firms incorporated and headquartered in the US and in which all contracting parties are identified in Compustat data.

3.2 Measuring Tax Knowledge Diffusion

We argue in our conceptual framework (see Section 2) that tax knowledge diffusion via strategic alliances would impact a firm's equilibrium of expected costs and benefits of tax planning. Therefore, we operationalize tax knowledge diffusion by measuring changes to a firm's nonconforming tax planning behavior. The lingua franca in determining the degree to which a firm engages in tax planning is the effective tax rate, which puts tax expenses and pre-tax book income into perspective.[2] We base our inferences on the cash effective tax rate (*cash ETR*) because *cash ETR* also captures tax deferral strategies (Edwards, Schwab, and Shevlin 2016; Hanlon and Heitzman 2010). Furthermore, we apply a multiperiod (3-year) form of *cash ETR* (Barrios and Gallemore 2024; Brown and Drake 2014; Gallemore, Gipper, and Maydew 2019):

$$cash\ ETR3_{i,t=1} = \frac{\Sigma_{t=1}^{3}(txpd_{i,t})}{\Sigma_{t=1}^{3}(pi_{i,t} - spi_{i,t})} \tag{1}$$

The terms txpd, pi and spi in equation (1) correspond to their Compustat data item equivalents of cash taxes paid, pre-tax income and special items. Missing spi values are reset

[2]While nonconforming tax planning should be indicative of a low effective tax rate, a low effective tax rate is not indicative of tax planning by all means (Drake, Hamilton, and Lusch 2020; Schwab, Stomberg, and Xia 2021). However, the importance of effective tax rates as a measure of and proxy for tax planning continues to remain highly important in corporate practice. We employ effective tax rates as tax knowledge measure for the latter reason but carefully check the robustness of this choice by (i) considering the effects from presumable valuation allowance releases (Drake, Hamilton, and Lusch 2020), and (ii) utilizing a different measure of a firm's tax planning activities (e.g., the cash tax differential by Henry and Sansing (2018)).

to 0, while any *cash ETR3* with a negative denominator is reset to missing. Nonmissing *cash ETR3* are winsorized at 0 and 1.

For every t_1 in which a strategic alliance is initiated, we are interested in whether firms are rather undersheltered "high-tax" firms or rather tax aggressive "low-tax" firms. For the identification of low-tax and high-tax firms, we consider *pre cash ETR3*, which is constructed identically to *cash ETR3* but over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$:

$$pre\ cash\ ETR3_{i,t=0} = \frac{\Sigma_{t=-2}^{0}(txpd_{i,t})}{\Sigma_{t=-2}^{0}(pi_{i,t} - spi_{i,t})} \tag{2}$$

Next, we require to observe *cash ETR3* and *pre cash ETR3* of all partners for an alliance to be considered in our analysis.[3] We classify firms as high-tax or low-tax based on their *pre cash ETR3* which we industry-year-mean adjust for this purpose (i.e., we are interested in firms that show low/high multiperiod effective tax rates among their industry peers just before an alliance is initiated). We then allocate this adjusted *pre cash ETR3* into four bins according to the quartiles of its distribution. Industry adjustment (Brown and Drake 2014) and a multiperiod measure (Dyreng, Hanlon, and Maydew 2008; Dyreng et al. 2017) help us to validate the identification of undersheltered firms and more aggressive tax planners. A partner is treated as a low-tax firm in a strategic alliance when its adjusted *pre cash ETR3* is in the first bin (i.e., lowest quartile). Conversely, firms that do not qualify as low-tax firms are classified as high-tax firms.

Since we are interested in changes to firms' tax planning and our sample consists of one observation per firm in an alliance (i.e., our sample mirrors a pooled cross section and not a panel), we utilize the first difference as outcome variable of interest:

$$change\ cash\ ETR3_{i,t=1} = cash\ ETR3_{[t_1;t_3]} - pre\ cash\ ETR3_{[t_{-2};t_0]} \tag{3}$$

Values of > 0 for *change cash ETR3* from equation (3) indicate increases in cash

[3] We also refer to *cash ETR3* and *pre cash ETR3* as *cash ETR3*$_{[t_1;t_3]}$ and *pre cash ETR3*$_{[t_{-2};t_0]}$ to highlight the respective timing around the initiation of a strategic alliance in t_1. Our analyses, thus, consider strategic alliances between 1997 (1994-1996 for *pre cash ETR3*) and 2019 (2019-2021 for *cash ETR3*).

effective tax rates, whereas values of < 0 indicate decreases in cash effective tax rates which would be consistent with more tax planning.

3.3 Focal Independent Variable: *hightolow*

Strategic alliances may be composed of low-tax firms only, high-tax firms only, or a combination of high-tax and low-tax firms (see Section 3.2). In our analyses, we focus on high-tax firms as potential beneficiaries of tax knowledge diffusion ("work with superwoman") and discriminate between high-tax firms that initiate strategic alliances with low-tax firms ($hightolow = 1$) and high-tax firms that initiate strategic alliances with other high-tax firms ($hightolow = 0$):

$$\boldsymbol{hightolow} = \begin{cases} 1, & \textbf{high-tax firm in alliance to low-tax firm(s),} \\ 0, & \textbf{high-tax firm in alliance to high-tax firm(s).} \end{cases} \tag{4}$$

Applying the above described identification strategy leads us to 284 observations of $hightolow = 1$ and 965 observations of $hightolow = 0$. Overall, our sample selection and identification strategy ensures that high-tax firms are in very similar situations except for potentially experiencing tax knowledge diffusion. This implies that mechanical tax effects associated with strategic alliances would affect both groups of *hightolow* similarly (the same would hold for mean-reversion). Therefore, high-tax firms establish the treatment group and the control group for our analyses whereas their allocation to the treatment group exogenously depends on a partner's tax knowledge (see also Section 4.2). Our control-group, thus, represents pseudo treated high-tax firms with on average identical chances and challenges after initiating a strategic alliance as the treated high-tax firms except for the fact that their alliance has no low-tax partner.

3.4 Regression Design

We estimate the following linear regression model by OLS:

$$\begin{aligned} \textit{change cash ETR3}_{i,t=1} = \beta_0 \; + \; \beta_1 \textit{hightolow}_{i,t=1} \; + \; \Sigma_n \, \beta_n \; \textit{partner controls}^n_{i,t=1} \\ + \; \Sigma_l \, \beta_l \; \textit{alliance controls}^l_{i,t=1} \; + \; \Sigma_k \, \beta_k \; \textit{firm controls}^k_{i,t=1} \\ + \; \delta_{ind} \; + \; \tau_t \; + \; \varepsilon_{i,t} \end{aligned} \tag{5}$$

The outcome variable of interest in equation (5) is *change cash ETR3* which is the first difference estimator *cash ETR3* minus *pre cash ETR3* (see also equation (3)). It captures changes in (high-tax) firms' tax planning from before to after the initiation of a strategic alliance in t_1. The independent variable of interest in equation (5) is *hightolow*, which is constructed as an indicator variable to distinguish between high-tax firms that invest in strategic alliances with low-tax firms (*hightolow* $= 1$) and high-tax firms that invest in strategic alliances with other high-tax firms (*hightolow* $= 0$). In essence, *hightolow* captures eventual differences in *change cash ETR3* between treatment and control observations that stem from the treatment. A negative coefficient for *hightolow* would suggest that effective tax rates of treated firms in strategic alliances decrease relative to the effective tax rates of control firms. Our conceptual framework indicates that this finding would identify tax knowledge diffusion via strategic alliances.

Additionally, we include multiple vectors of control variables that are constructed at the partner-, alliance-, and firm-level. The *partner controls* capture how the partners' organizational structures and their operational environments relate to each other. From Compustat data, we infer whether the partners in an alliance share the audit firm and/or industry affiliation in the year of alliance initiation (i.e., we construct the indicator variables *PartSameAuditor* and *PartSameInd*). We also observe whether their headquarters are located in the same region as defined by the Bureau of Economic Analysis (*PartSameBEARegion*).[4] In the main analysis, we substitute *PartSameBEARegion* with a manually collected measure of the geographical distance (as the crow flies) between the zip codes of the partners' headquarters (*HQDistance*) to control for the potential impact of geographical proximity in tax knowledge diffusion.

[4]The respective BEA regions are Far West, Great Lakes, Mideast, New England, Plains, Rocky Mountains, Southeast and Southwest.

[Figure 1 about here.]

Furthermore, characteristics of a strategic alliance related to its business purpose might facilitate or impede tax knowledge diffusion. SDC provides information on an alliance's business purpose with a description of every strategic alliance. We apply textual analysis to these deal descriptions to derive the main business purposes of the strategic alliances in our sample (*alliance controls*). The word cloud depicted in Figure 1 shows the 40 most common words used in the descriptions. We base regular expressions on selected features and use pattern matching to identify *Purpose Develop*, *Purpose Licensing*, *Purpose Marketing*, *Purpose Manufacturing*, *Purpose Service*, *Purpose Supply*, *Purpose Tech*, and *Purpose Wholesale* as main activities. We construct indicator variables for each of these activities and include them in equation (5).

Finally, we control for within-firm determinants of tax planning by including a vector of *firm controls*. We basically follow Dyreng, Hanlon, and Maydew (2010) and consider *AdExp3*, *CapEx3*, *Cash3*, *EBITDA3*, *GrowthSale3*, *Intangibles3*, *Leverage3*, *MNE3* (indicator), *NOL3* (indicator), *NOL3*, *PPE3*, *RnDExp3*, *SGA3*, and *Size3*, whereas continuous *firm controls* are included as first differences ($[t_1; t_3] - [t_{-2}; t_0]$). Additionally, we employ data by Gentry et al. (2021) on CEO turnover, to control for the tension between tax knowledge transfers via labor markets and top management continuity as eventual facilitator of knowledge diffusion. We construct the indicator variable *CEOTurnover3*, which equals one when a firm experiences a CEO turnover between t_1 and t_3. We include year τ_t and industry δ_{ind} fixed effects in equation (5) and cluster robust standard errors at the firm level (Petersen 2009).

3.5 Descriptive Statistics

[Table 2 about here.]

Table 2 contains descriptive statistics for the outcome variables of interest (e.g., *change cash ETR3*) and the *firm controls* (see Section 3.4) conditional on the classifications by *hightolow* and *lowtohigh*, whereas *lowtohigh* is an indicator variable that is constructed inversely to *hightolow* by classifying whether low-tax firms are in strategic alliances with other low-tax firms or with high-tax firms (for an analysis, see Section 5). Conditioning on *hightolow*, we do not observe economically significant differences in the *firm controls* between the treatment group and the control group. In particular, only few differences in the averages of the *firm controls* are statistically significant at the 10% level (differences in *change Cash3*, *change Leverage3*, and *NOL3* are statistically different and lower between treatment and control group with p-values of 0.0786, 0.0923, and 0.0274). We conclude that our control-group represents pseudo treated firms with on average identical chances and challenges after initiating a strategic alliance as the treated firms except for the fact that their alliance has no low-tax partner. Consequently, these observations lend additional credibility to our control group identification.

4 Results

4.1 Main Analysis

[Figure 2 about here.]

Before we turn to the multivariate analysis, we descriptively analyze changes in the tax planning behavior of high-tax firms in strategic alliances with low-tax firms in comparison to high-tax firms in strategic alliances with high-tax firms. In Figure 2, we depict density plots and box plots of *change cash ETR3* conditional on *hightolow*. Generally, we observe reductions in cash effective tax rates for treatment and control groups and test for the difference between groups (and between periods). The right edge of the figure depicts the difference in the averages of *change cash ETR3* between the *hightolow* conditions (including the 95% error bar). The difference is negative and economically and statistically significant (difference of -0.0298 and p-value of 0.01). We interpret this finding as a first indication of the existence of tax knowledge diffusion via strategic alliances. In particular, we conclude that this and subsequent results do not reflect unexpected variation or increases in effective tax rates among the control observations.

[Table 3 about here.]

The main variable of interest in our regression analysis is *hightolow* because it isolates the incremental effect a low-tax partner exerts on a high-tax firm's tax knowledge and eventually on its tax planning behavior. In Table 3, we show the results for estimating equation (5) with *change cash ETR3* (columns (1) and (2)), *delta cash ETR3* (column (3)), and *cash ETR3* (column (4)) as dependent variables, whereas *delta cash ETR3* is constructed as *cash ETR3* scaled by *pre cash ETR3* minus one. The estimates for *hightolow* are negative and significant in all specifications. In the specification that includes all control variables and has *change cash ETR3* as the dependent variable (column (2), preferred specification), the estimate for *hightolow* has a magnitude of -0.0302 (p-value 0.018). Economically, this and the other results in Table 3 are consistent with our descriptive inferences in terms of direction and magnitude for both post-levels of and changes in tax planning behavior. Notably, we test for differences in the development of effective tax rates between high-tax firms conditional on their treatment status. If reversion to the mean influenced high-tax firms' post-initiation effective tax rates, our research design would account for this factor and persist in identifying an incremental treatment effect because mean reversion should occur irrespective of the treatment status. Because the covariates of partner, alliance, and firm controls additionally account for a broad range of alternative explanations, we conclude that the (relative) increase in tax planning for high-tax firms in alliances to low-tax partners stems from tax knowledge diffusion altering the equilibrium of expected costs and benefits of tax planning.

Furthermore, the estimates for the control variables are consistent between the specifications. In comparison to the estimates for *hightolow*, however, these estimates are either economically marginally influencing the outcome variables of interest or are statistically not significant at conventional levels. For instance, the estimate for *Purpose Develop* loads negative but is insignificant in the preferred specification (p-value 0.209). We cautiously interpret the coefficient to be consistent with research that shows that strategic alliances in R&D lead to higher patent output (K. Li, Qiu, and Wang 2019) and that patents have a causal effect on corporate tax planning that is incremental to the effect of R&D expenses on tax planning (Cheng et al. 2021). If strategic alliances in R&D, however, allowed firms to employ specific tax credits (as broadly suggested by Demirkan, Olson, and Zhou (2024)), this effect would be unconditional to our classification of alliances/observations as of the *hightolow* type. In several cross-sectional analyses (see Section 4.3), we additionally focus on interactions of *hightolow* and control variables to investigate whether these interactions eventually remove or complement the identified effects from the preferred specification.[5]

[Table 4 about here.]

Thus far, the data indicate that the high-tax firms in our sample are in very similar situations except for potentially experiencing tax knowledge diffusion. In the interest of caution and methodological thoroughness, we employ entropy-balancing weighting (Hainmueller 2012;

[5] An alliance's business purpose is selected by the partners. Therefore, we refrain from interacting *hightolow* with the *alliance controls* in additional analyses.

Hainmueller and Xu 2013) and use the entropy weights to re-estimate equation (5). These weights approach to control for any characteristics of high-tax firms investing into low-tax alliances that could differ from the characteristics of high-tax firms investing in high-tax alliances and drive differences in the outcome variable. Specifically, Table 4 presents results for multiple variations of our preferred specification which employ entropy weights that are constructed over different moments and variables. The first specification uses the continuous *firm controls* over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$ to calculate the entropy weights so that control observations are reweighted to satisfy the balance constraint that the averages and variances (i.e., two moments) match the corresponding moments of the treated units. The entropy weights in the second specification are constructed identically but respectively use the first moment as balance constraint. We use the singleperiod firm controls in the year of the treatment (t_1) (as, for instance, in Gallemore, Gipper, and Maydew (2019)) to calculate the entropy weights in the third specification. The fourth specification uses the singleperiod observations of *cash ETR* in the preperiod $[t_{-2}; t_0]$ to calculate the weights. Throughout all specifications, we observe estimates for *hightolow* that closely mirror the estimates from our preferred specification in Table 3, both in terms of magnitude and significance, reducing concerns about unobserved characteristics among treated and control observations explaining our results.

Overall, our main analysis reveals that high-tax firms increase their tax planning after initiating strategic alliances with low-tax firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. These insights indicate that tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010). Importantly, the identified average treatment effect on the treated is neither surprisingly large nor negligibly small. Our estimates indicate, consistent across a broad set of specifications, a reduction of 2.5 to 3 percentage points in a treated firm's effective tax rate, supported by a 95% confidence interval of $[-0.0552, -0.0052]$ in the preferred specification. Considering a sample average of ~29% pretreatment, the identified decreases are both statistically and economically significant.

4.2 Anticipation?

In a strategic alliance, partner choice is evidently driven by the alliance's scope. Our identification strategy, however, is agnostic about the scope of a strategic alliance and respectively considers partners' classification as low-tax or high-tax firms to determine treatment status. Therefore, we argue that firms are plausibly exogenously treated. However, if high-tax firms anticipated the beneficial diffusion of tax knowledge and selected low-tax firms as partners because of their sophisticated tax knowledge, endogenous treatment assignment would affect the inferences from OLS estimators (Lennox, Francis, and Wang 2012). Consequently, we are interested in whether treatment indeed is exogenous.

[Figure 3 about here.]

Generally, any intention to benefit from tax knowledge diffusion via an investment in a strategic alliance must be a byproduct of other main incentives. When investing in strategic alliances, firms pool their resources to achieve strategic objectives (Meier et al. 2016). Thus, it would be an economic pitfall if firms weighted the potential diffusion of tax knowledge over the selection of a partner that best suits the network's main business purpose. Furthermore, Baxamusa, Jalal, and Jha (2018) emphasize that there are considerably less due diligence analyses when investing in strategic alliances than when investing in M&As. As effective tax rates, unlike firms' underlying tax planning strategies, are publicly available, we take these arguments to an empirical analysis. Specifically, we analyze abnormal returns at the announcement dates of the strategic alliances in our sample. The idea is that if tax knowledge diffusion could be anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study in WRDS using a market adjusted model with 100 days as estimation window and a $[-1; 1]$ day event window. Generally, we find, consistent with the findings by Chan et al. (1997), that cumulative abnormal returns (CAR) are on average positive across all announcements in our sample (average CAR of 0.007 with a t-statistic 6.2). Next, we depict density plots and averages of CAR by *hightolow* in Figure 3. We observe, counter to the idea of anticipation of tax knowledge diffusion, that abnormal returns of control firms' announcements exceed the abnormal returns for the treated firms (p-value 0.0313).[6] We conclude that tax knowledge diffusion via strategic alliances is not

[6] In an untabulated analysis, we run a regression that includes all controls from the preferred specification (incl. the

anticipated, reducing remaining concerns about endogenous treatment assignment.

4.3 Mechanisms

Next, we are interested in facilitators and inhibitors of tax knowledge diffusion via strategic alliances. In particular, we focus on elapsed time, CEO turnover, geographical proximity, industry affiliation, and market power.

Elapsed Time

[Table 5 about here.]

Knowledge diffusion is a gradual, multi-stage process which requires continuous exchange (Bresman, Birkinshaw, and Nobel 2010; Inkpen 2000; Szulanski 1996) and elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv 2003; Isaksson, Simeth, and Seifert 2016). To test whether elapsed time facilitates tax knowledge diffusion via strategic alliances, we estimate three specifications of equation (5) and present the results in Table 5. We extend the posttreatment period (keeping the preperiod $[t_{-2}; t_0]$ constant) by one year with each specification. For the construction of the dependent variables (*change cash ETR*), we use the singleperiod *cash ETRs* at t_1, t_2, and t_3, calculate first differences against *pre cash ETR3*, and then calculate the average of the sum of these calculations over the number of periods since treatment.[7] The results indicate that the treatment effect particularly increases in magnitude with elapsed time since the initiation of an alliance. Although differences for the estimates of *hightolow* between the specifications are not statistically significant at conventional levels, the estimates within the specifications themselves turn significant with elapsed time. Our findings, thus, are consistent with the evidence by Kim et al. (2019), who suggest that firms are generally able to adjust their tax planning behavior within three years and that high-tax firms may increase their tax planning behavior faster. Furthermore, and consistent with our conceptual expectations, we conclude that elapsed time facilitates tax knowledge diffusion via strategic alliances.

fixed effects) and *CAR* as dependent variable. We find that the difference from the univariate test (presented in Figure 3) prevails economically and statistically.

[7] For instance, the dependent variable for the second specification is calculated as $[(cash\ ETR_{t_1} - pre\ cash\ ETR3) + (cash\ ETR_{t_2} - pre\ cash\ ETR3)]/2$.

CEO Turnover

[Table 6 about here.]

Next, we turn to cross sectional analyses. We estimate specifications of equation (5) with *change cash ETR3* as dependent variable and include interaction terms of *hightolow* with the cross-sectional variable of interest (X, see column headers). First, we employ data by Gentry et al. (2021) on CEO turnover and construct the indicator variable *CEOTurnover3*, which equals one when a firm experiences a CEO turnover between t_1 and t_3. In particular, we are interested in top management continuity as eventual facilitator of knowledge diffusion since management research suggests an impact for a multitude of soft factors. Prominent examples are communication (Bresman, Birkinshaw, and Nobel 2010; Bushee, Kim-Gina, and Leung 2020), partner trustworthiness (Jiang et al. 2016), commitment (Bushee, Kim-Gina, and Leung 2020), managerial flexibility (Chan et al. 1997; Chen, King, and Wen 2015), partnering mindset (Kale, Singh, and Bell 2009), and learning intent (Hamel 1991; Mazloomi Khamseh, Jolly, and Morel 2017). Frank et al. (2021) focus on knowledge in the relationship between third-party insurers and audit firms and present interview evidence that "…one-on-one consultations tend to be most effective because they can make the necessary reductions in tacitness, ambiguity, and complexity of knowledge during the process…" (p. 38). A CEO turnover could, thus, inhibit tax knowledge diffusion. The results are depicted in Table 6 and indicate that the baseline effect for *hightolow* prevails economically and statistically. Consistent with our expectations, the interaction of *hightolow* with *CEOTurnover3* loads positive but falls short of conventional levels of statistical significance, indicating that tax planning responses when there are fewer changes in a high-tax firm's management. This result is broadly consistent with the notion that top management continuity enables the building of trust and supports exchange in the facilitation of knowledge diffusion.

Distance

Brown (2011) hypothesized that tax shelter adoption may spread regionally because "local business elites are connected through a range of formal and informal institutions that facilitate communication, from the country club to local charity organizations" (p. 34). Therefore, we are interested in whether geographical distance between the partners' headquarters removes (i.e., distance as alternative channel) or facilitates/inhibits (i.e., distance and treatment as two complementary diffusion mechanisms) tax knowledge diffusion via strategic alliance. We manually collect the geographical distance (as the crow flies in miles) between the zip codes of the partners' headquarters (*HQDistance*) and interact the standardized values with *hightolow* in the second specification of Table 6. The baseline and interaction estimates for *zHQDistance* indicate that increasing geographical distance between the partners' headquarters mitigates the observed decreases in *change cash ETR3*.[8] The interaction term, however, is respectively marginally different from zero and beyond common levels of statistical significance. The baseline effect for *hightolow* is economically and statistically similar to the results from the main analysis. These findings, generally consistent with the inferences by Brown (2011), suggest that geographical distance between firms can affect tax planning but particularly underscore our treatment indication as unique channel for tax knowledge diffusion between firms.

Same Industry

Next, we focus on partners that are in the same industry because industry peers can influence tax planning (Bird, Edwards, and Ruchti 2018) and a shared industry affiliation could speak to partner similarity. However, eventual effects of shared industry affiliation could be moderated by competition (e.g., see Bourveau, She, and Žaldkokas (2020) and J. Cai and Szeidl (2018) on the opposing effects of competition on collusion and diffusion of information; and Lavie, Lunnan, and Truong (2022) on restrictions in alliances from business similarity). We construct the indicator variable *PartSameInd* which equals one when the partners of an alliance belong to the same industry.[9] The results in column (3) of Table 6 indicate, consistent with the other cross sectional analyses, that the baseline effect for *hightolow* prevails economically and statistically. The baseline coefficient for *PartSameInd* (negative) and the estimate for *hightolow* × *PartSameInd* (positive) show different signs, are economically meaningful but fall short of statistical significance. These results suggest that a shared industry affiliation rather serves as substitute to our treatment indication and subsumes part of the treatment effect identified in the main analysis. Consistent with the inferences by Brown and Drake (2014), Brown (2011), and (research on) the effects of competition (see above), we find that *PartSameInd* does not facilitate but moderates tax knowledge diffusion for the treated.

[8] z indicates standardization at mean zero and a standard deviation of one. We find consistent evidence when we replace *HQDistance* with the indicator variable *PartSameBEARegion* (untabulated).

[9] When we exclude firms that belong to the Fama-French-12 industry classification "other" in this analysis (untabulated), we find consistent evidence to the results presented in Table 6.

Market Shares

Finally, we focus on differences in the market shares of the partners in strategic alliances as these differences could reflect relative power of the partners vis-à-vis each other. In particular, analyzing differences in market shares allows us to determine whether tax knowledge diffusion is driven by power dynamics or unintended. If a firm with a substantial market share could influence its alliance partner to share and transfer tax knowledge or engage in tax planning, it would suggest a power-induced mechanism, indicating intended transfers of tax knowledge. Conversely, if differences in market shares do not significantly influence (i.e., decrease) *change cash ETR3*, it would indicate that tax knowledge diffusion is indeed unintended.

We construct *DiffPartMarketShare* which is the difference between a high-tax firm's minus its alliance partner's *MarketShare*. The variable *MarketShare*, thereby, is constructed consistent with the market share calculations of the *HHI* (i.e., the percentage of a firm's sales within industry and year). Thus, *DiffPartMarketShare* increases with a firm's market share and decreases when the partner's market share increases. For the analysis, we standardize *DiffPartMarketShare* at mean zero and standard deviation one and include the baselines and an interaction of *hightolow* and *zDiffPartMarketShare*.[10] The results are depicted in column (4) of Table 6. The estimate for *hightolow* persists in direction, magnitude, and statistical significance. Interestingly, the interaction is positive and statistically significant (p-value 0.076). This finding indicates that with a one standard deviation increase in *DiffPartMarketShare* the baseline effect of *hightolow* on *change cash ETR3* is weakened by an economically meaningful 1.5 percentage points. Thus, we find no evidence for a power-induced mechanism but, consistent with trust and mutual commitment, that tax knowledge diffusion via strategic alliances is unintended (see also Section 4.2).

Overall, the evidence in Section 4.3 suggests that mechanisms are neither mutually exclusive nor reinforcing. In particular, we find evidence that elapsed time, consistent with an increased probability of information exchange due to trust and mutual commitment, facilitates tax knowledge diffusion via strategic alliances. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. We also find that a shared industry affiliation rather inhibits tax knowledge diffusion via strategic alliances. Results from

[10] z indicates standardization at mean zero and a standard deviation of one. Market shares are calculated within the Compustat universe (i.e., before merging Compustat to SDC data). We neither square firms' market shares nor include the Fama-French-12 industry classification "other" in this analysis.

analyzing differences in firms' market shares further suggest that tax knowledge diffusion is unintended and not power-induced. Throughout all cross sectional analyses the baseline estimate for *hightolow* persists in direction, magnitude, and statistical significance.

4.4 Alternative Channels

[Table 7 about here.]

Same Audit Firm

Next, we examine how tax knowledge diffusion via strategic alliances is impacted when partners share an audit firm (*PartSameAuditor*) because we are interested in whether the identified effects are robust to alternative channels of intentional tax knowledge transfers. Generally, evidence on the impact of audit firms on tax planning outcomes is mixed.[11] Brown (2011) does not find significant tax shelter adoption via shared audit firms, and Klassen, Lisowsky, and Mescall (2016) show that less tax aggressiveness in the past is associated with the auditor preparing a firm's tax return. In contrast, Lim et al. (2018) and Cen et al. (2020) suggest that shared auditors facilitate tax planning. Consistent with the mixed evidence from prior literature, Nesbitt, Persson, and Shaw (2020) suggest that there are limits to the relation between auditor-provided tax services and clients' tax planning.

We construct the indicator variable *PartSameAuditor* that equals one when the partners in an alliance share an audit firm. We estimate equation (5) with *change cash ETR3* as dependent variable and include the interaction of *hightolow* with *PartSameAuditor*. Column (1) of Table 7 depicts the results. We observe that the baseline effect for *hightolow* is negative and economically meaningful (p-value 0.096). The negative estimate for the interaction term for *hightolow* $\times$ *PartSameAuditor* does not surpass common levels of statistical significance but is economically particularly sizable. We conclude that the results from our main analyses persist and that we do not spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances.

[11] E.g., see Aobdia (2015); Y. Cai et al. (2016); Dhaliwal et al. (2016); McGuire, Omer, and Wang (2012); Klassen, Lisowsky, and Mescall (2016); Lim et al. (2018); Frey (2018); Bianchi et al. (2018); Nesbitt, Persson, and Shaw (2020); Hux, Bedard, and Noga (2022).

Board Ties

Since Brown and Drake (2014) indicate that board ties can impact tax planning of connected firms, we consider board ties among the partners in the alliances of our sample. We use data from ISS to construct the indicator variable *BoardTie3* that equals one when the partners have at least one common member among their board of directors (at any point over the $[t_1; t_3]$ period). First, we note that board ties are rare among the observations in our sample. This is particularly true for the treated observations (we observe an overlap of seven board ties among the 284 $hightolow = 1$ observations). Therefore, we do not estimate a specification that includes an interaction but respectively add *BoardTie3* as additional control variable to the analysis. Results for this specification are depicted in column (2) of Table 7. We observe that the inferences from our main analysis effectively remain unchanged because the coefficient for *hightolow* is estimated as $-\ 0.0303$ (main analysis $-\ 0.0302$). In seemingly unrelated regressions (untabulated), we additionally analyze whether our inferences change when we remove observations with board ties from the sample (both for control and treated observations). We find that the estimated coefficient for *hightolow* remains effectively unchanged (difference $-\ 0.001$). Therefore, we conclude that the results from our main analyses persist and that we do not spuriously pick up an alternative channel (here board ties) when identifying tax knowledge diffusion via strategic alliances.

5 Robustness Checks

[Table 8 about here.]

Alternative Tax Planning Measures

We turn to the robustness of our results by employing alternative tax planning measures. Table 8 depicts results for specifications of equation (5) with *change CTD3* (column (1)) and *change GAAP ETR3* (column (2)) as dependent variables. We utilize the cash tax differential (*CTD*) developed by Henry and Sansing (2018) which allows us to identify whether high-tax firms become rather tax-favored relative to the control observations. *GAAP ETR3* utilizes a GAAP measure of taxes paid (i.e., total income tax expense) instead of cash taxes paid in the numerator of the effective tax rate. For both *change CTD3* and *change GAAP ETR3*, a negative estimate for *hightolow* would be consistent with our main analysis. We find consistent and economically meaningful evidence. For instance, the estimate for *change GAAP ETR3* is just marginally shy to the estimate in our preferred specification with *change cash ETR3* as dependent variable.

Pretreatment ETR Volatility

Next, we focus on volatility in the singleperiod *cash ETR* observations in the pretreatment period because we want to ensure that we do not interpret volatility in high-tax firms' tax planning measures as eventual treatment effect (see also the fourth specification in Table 4, which uses pretreatment singleperiod *cash ETRs* to calculate the entropy weights). Initially, we construct a volatility measure σ that captures the volatility in the cash effective tax rates in the pretreatment period:

$$\sigma = \sqrt{\frac{1}{3}\sum_{i=-2}^{0}(cash\ ETR_i - pre\ cash\ ETR3)^2} \tag{6}$$

We then test (untabulated) whether the measure from equation (6) differs between treatment and control group and find no statistically significant difference. Finally, we exclude all observations that belong to the top-quintile of the volatility measure from the analysis and re-estimate equation (5) (column (3) of Table 8). We find that the identified effect from our main analysis prevails and conclude that our findings do not reflect eventual pretrends in effective tax rates of high-tax firms.

Low-Tax Firms

In our main analysis, we do not consider low-tax firms because we are interested in whether high-tax firms' tax planning responses to "working with superwoman". Furthermore, our conceptual framework of (unintended) knowledge diffusion suggests that there is little reason to expect a tax planning response for low-tax firms. However, if tax knowledge is transferred intentionally for joint tax planning (e.g., see Cen et al. (2017); Cen et al. (2020)), low-tax firms' effective tax rates could also change. To empirically control for this notion, we construct *lowtohigh*, which is an indicator that equals one for low-tax firms in alliances with high-tax firms and zero for low-tax firms in alliances with low-tax firms. We then re-estimate equation (5) (column (4) of Table 8). We find that the coefficient estimate for *lowtohigh* is (i) beyond common levels of significance (p-value 0.753) and (ii) economically not meaningful different from zero.

Valuation Allowance Releases

[Table 9 about here.]

Table 9 depicts results of specifications that consider advances by Drake, Hamilton, and Lusch (2020) on the effect of valuation allowance releases on effective tax rates. The authors document how effective tax rates decrease when valuation allowances are released and conclude that this effect challenges the assumption that lower effective tax rates indicate tax planning. Therefore, we follow the insights in Drake, Hamilton, and Lusch (2020) and calculate the probability for such events from Compustat data. We then construct first differences between the $[t_1; t_3]$ and $[t_{-2}; t_0]$ periods (*change Val. All. Release3*) and test whether our treatment indication increases the probability for a valuation allowance release (column (1) of Table 9).

We find no evidence for such an increase. Rather, the estimate for *hightolow* is negative and beyond common levels of statistical significance. Next, we include *change Val. All. Release3* as additional control variable, interact it with *hightolow*, and regress these and the control variables from equation (5) on *change cash ETR3*. If releases of valuation allowances confoundingly captured decrease in effective tax rates, we would expect that the baseline effect for *hightolow* diminishes. Again, we find no evidence for this influence. Instead, the estimate is respectively marginally different from our main analysis. Finally, we are interested in whether our classification of low-tax firms (as an prerequisite for the treatment assignment of high-tax firms) captured firms that show low effective tax rates due to valuation allowance releases. Therefore, we capture not the own firm's but the partner's probability for a valuation allowance release over the $[t_{-2}; t_0]$ period (*Part Pre Val. All. Release3*) and include and interact this variable with *hightolow* (column (3)). We find that the inferences from our analyses remain unchanged when controlling for this variable. In essence, these findings support our identification strategy. We conclude that our results are robust to the influence of valuation allowance release and indeed capture tax planning responses by the treated high-tax firms due to tax knowledge diffusion.

6 Conclusion

The purpose of this study is to shed light on whether "working with superwoman" triggers tax planning responses suggesting a novel perspective on the puzzle on firms' (dis)engagement from tax planning. Utilizing data on strategic alliances between publicly traded U.S. firms allows us to distinguish between alliances that bring together high-tax and low-tax firms. We empirically identify tax knowledge diffusion via strategic alliances by robustly documenting an economically meaningful decrease in cash effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to the effects on high-tax firms in strategic alliances with other high-tax firms.

Building on our conceptual framework we investigate several mechanisms which may facilitate tax knowledge diffusion via close cooperation and continued exchange between alliance partners. We observe that elapsed time, likely indicative of enhanced information exchange facilitated by trust and mutual commitment, promotes the diffusion of tax knowledge through strategic alliances. While there are indications supporting this assertion, albeit weaker, regarding CEO continuity and spatial proximity between partners, the evidence suggests that shared industry affiliation inhibits the diffusion of tax knowledge through such alliances.

Our findings suggest that informal private exchange may reduce the expected costs of tax planning. This effect seems to be unforeseen prior to the establishment of a strategic and on average it only benefits high-tax firms. Hence, our research underscores the significance of integrating tax considerations into the managerial frameworks governing strategic alliances. Additionally, it provides insights for policymakers regarding how promoting collaboration through strategic alliances can impact firms' decisions regarding tax planning.

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Appendix

Table A1: Variable Definitions

Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
Sampling Unit	
Strategic Alliance	Contract based cooperation between publicly traded U.S. firms in the sample period 1994 to 2021; data are from SDC Platinum (STRATEGICALLIANCE/SAF); for analyses, data are considered at the firm × alliance level.
Main Outcome Variables	
change cash ETR3	First difference between *cash ETR3* and *pre cash ETR3*, calculated as: *change cash ETR3* = *cash ETR3* - pre *cash ETR3*
pre cash ETR3	See *cash ETR3* for construction; difference: numerator and denominator are calculated over three preceding periods to the initiation of an alliance.
cash ETR3	Multiperiod cash effective tax rate: $cash\ ETR3_{i,t=1} = \frac{\Sigma_{t=1}^{3}(txpd_{i,t})}{\Sigma_{t=1}^{3}(pi_{i,t} - spi_{i,t})}$ Defined as cash taxes paid (txpd) divided by pre-tax income (pi) before special items (spi); special items are reset to 0 when missing; numerator and denominator are constructed as the sum of the current and two subsequent fiscal years (with alliance initiation in t_1); observations with a negative denominator are reset to missing; winsorized at 0 and 1.
delta cash ETR3	Alternative to *change cash ETR3*; constructed as *cash ETR3* scaled by *pre cash ETR3* minus one; reset to missing when numerator or denominator equal 0; winsorized at p1 and p99.
Main Variables of Interest	
high-tax firm	Inverse to *low-tax firm*; indicator variable; constructed at alliance initiation t_1; equals 1 if the firm's industry adjusted *pre cash ETR3* does not belong to the lowest quartile; 0 for low-tax firms.
hightolow	Treatment indicator; indicator variable; equals 1 for high-tax firms in strategic alliance with low-tax firms; equals 0 for high-tax firms in strategic alliance with high-tax firms; see *high-tax firm* and *low-tax firm* for details.
low-tax firm	Indicator variable; constructed at alliance initiation t_1; equals 1 if the firm's industry adjusted *pre cash ETR3* does belong to the lowest quartile; 0 for high-tax firms.
lowtohigh	Indicator variable; equals 1 for low-tax firms in strategic alliance with high-tax firm; equals 0 for low-tax firms in strategic alliance with low-tax firm; see *high-tax firm* and *low-tax firm* for details.
Partner Controls	
HQDistance	Distance (in miles as the crow flies) between the partners of an alliance according to the zip code of the partners' historical headquarters (addzip) at t_1; collected from freemaptools.com; standardized for regressions.
PartSameAuditor	Indicator variable; equals 1 when all partners in an alliance share the same audit firm (au) in t_1; 0 otherwise.
PartSameInd	Constructed identically to *PartSameAuditor* but for industry affiliation; industry is classified using Fama French 12 industries (sic).

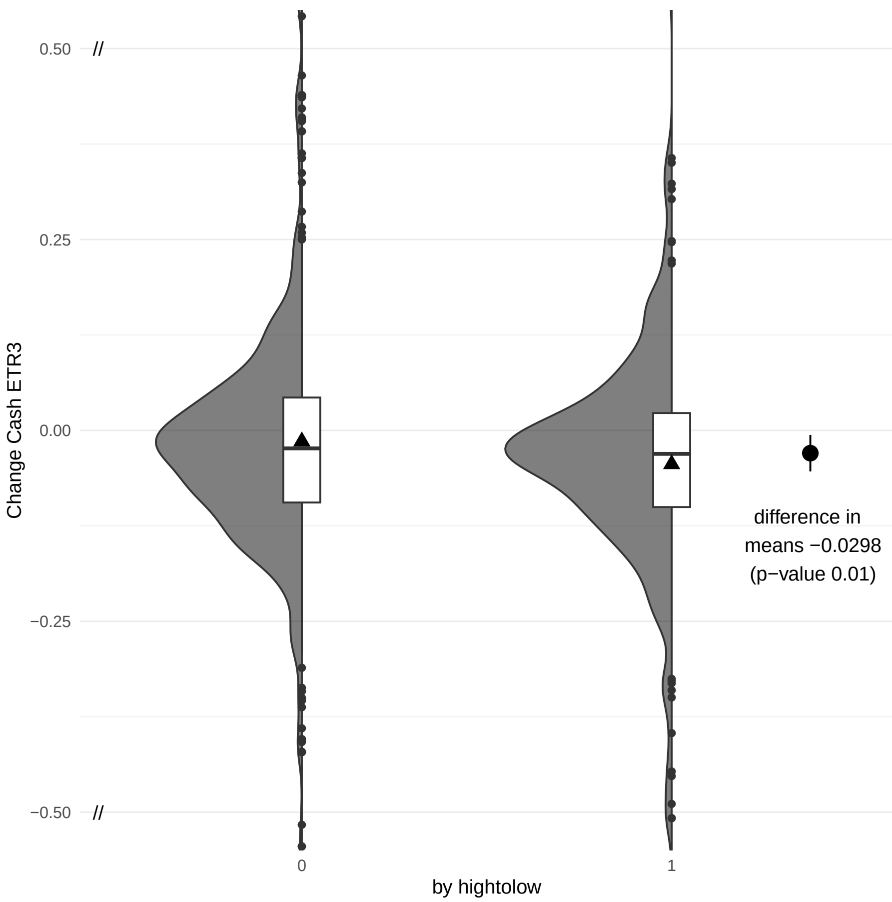
Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
PartSameBEARegion	Substitute to *HQDistance*; constructed identically to *PartSameAuditor* but for HQ-locations; equals 1 when all partners in an alliance are located in the same BEA region in t_1; 0 otherwise; the respective regions, as defined by the Bureau of Economic Analysis, are Far West, Great Lakes, Mideast, New England, Plains, Rocky Mountains, Southeast and Southwest.
Alliance Controls	
Σ alliance controls	Indicator variables; indicative of the main business purpose of a strategic alliance; derived from an alliance's deal description (DEALTEXT) in SDC; comprise *PurposeDevelop, PurposeLicense , PurposeManufacture, PurposeMarketing, PurposeService, PurposeSupply, PurposeTech, and PurposeWholesale.*
Firm Controls (Indicators)	
CEOTurnover3	Indicator variable, equals 1 when a firm experiences a CEO turnover in the current or subsequent two firm-years $[t_1; t_3]$, 0 otherwise; data from Gentry et al. (2021).
MNE3	Indicator variable; equals 1 for nonmissing, nonzero sum of pre-tax income from foreign operations (pifo) over $[t_1; t_3]$; 0 otherwise.
NOL3	Indicator variable equals 1 for nonmissing, nonzero sum of tax loss carry forwards (tlcf) over $[t_1; t_3]$; 0 otherwise.
Firm Controls (Continuous)	
AdExp3	Advertising expense (xad) divided by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
CapEx3	Reported capital expenditures (capx) divided by gross property, plant, and equipment (ppegt); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Cash3	Cash and cash equivalents (che) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
GrowthSale3	The annual average growth rate (geometric mean) of net sales (sale) over three years; when missing reset to annual growth rate, thereafter reset to 0.
EBITDA3	Earnings before interest, taxes, depreciation and amortization (ebitda) scaled by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Intangibles3	The ratio of intangible assets (intan) to total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Leverage3	The sum of long-term debt (dltt) and long-term debt in current liabilities (dlc) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.

Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
NOL3	The sum of tax loss carry forwards (tlcf) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0; this variable is included as *change NOL3* in analyses (see below) and *NOL3* is included as indicator (see above).
PPE3	Gross property, plant, and equipment (ppegt) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
RnDExp3	Research and development expenses (xrd) scaled by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0
SGA3	Selling, general, and administrative expense (xsga); divided by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Size3	The natural log of total assets (at) for the respective and two subsequent periods; when missing reset to annual measure, thereafter reset to 0. **Note**: continuous *firm controls* are constructed as first differences for the analyses (e.g., see *change cash ETR3*).
Other Variables	
BoardTie3	Indicator variable; we use identifiers of the members of firms' boards of directors from ISS (formerly Riskmetrics) to construct a variable that equals one when the partners in an alliance have at least one common board member (i.e., a board tie); zero otherwise.
CAR	Cumulative abnormal return; we run an event study (to the announcements of the strategic alliances on our sample) in WRDS using a market adjusted model with 100 days as estimation window and a $[-1; 1]$ day event window.
CTD	Cash tax differential; calculated following Henry and Sansing (2018); captures the extent to which a firm is tax-favored (< 0) or tax-disfavored (> 0).
DiffPartMarketShare	Difference between a high-tax firm's and its alliance partner's *MarketShare*. *MarketShare* is constructed consistent with the market share calculations of the HHI (i.e., the percentage of a firm's sales (sale) within industry and year (fyear)); *MarketShare* (which is not squared) is constructed within the Compustat universe; industry "other" is neglected; standardized for regressions.
GAAP ETR	see *cash ETR3*; the *GAAP ETR* utilizes a GAAP measure of taxes paid (i.e., total income tax expense, txt) instead of cash taxes paid (txpd) in the numerator of the effective tax rate.
Valuation Allowance Release	Probabilities for valuation allowance releases are calculated following Drake et al. (2020); linear combination that considers previous loss years (pi), tax loss carry forwards (tlcf, both as indicator and continuous change variable), and free cash flows (oancf, capx).

Figure 1: Main Business Purposes of Strategic Alliances

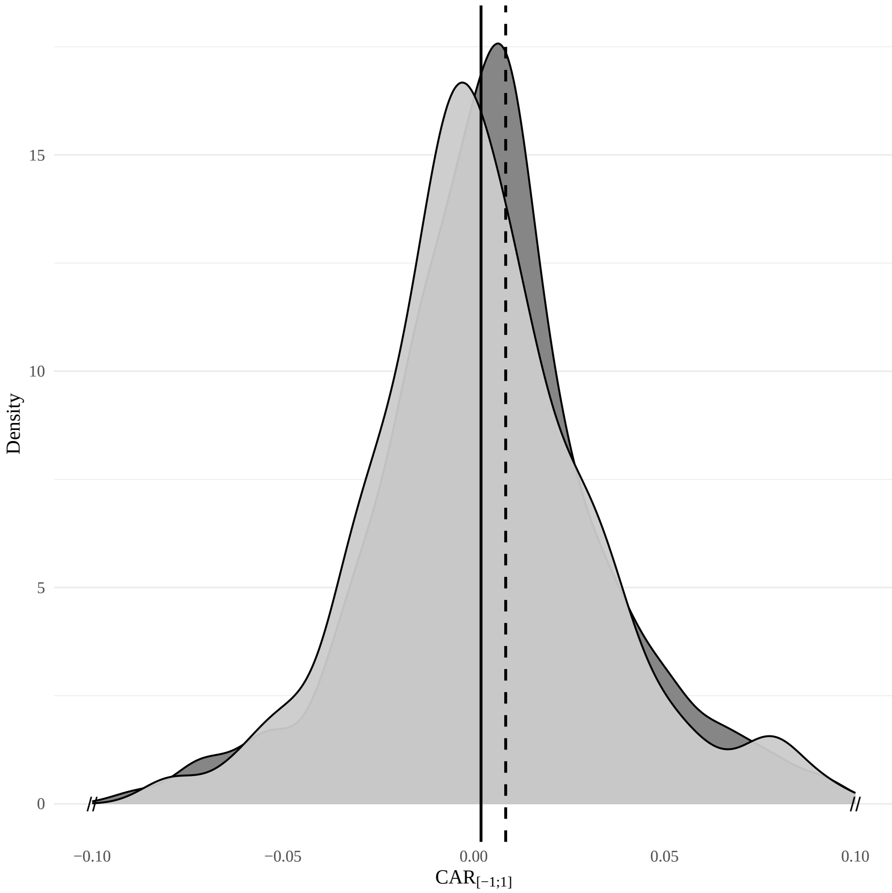
The word cloud depicted in this figure shows the 40 most common words used in the deal description of the alliances in our sample (as provided by SDC). By systematically searching through the deal descriptions (using regular expressions based on the above presented terms), we identify developing, licensing, manufacturing, marketing, services, supply, technology, and wholesale (in alphabetical order) as main business purposes of the alliances in our sample. We construct indicator variables accordingly and include them in our regression analyses (*alliance controls*). All variables are defined in detail in the Appendix.

Figure 2: Changes in Tax Planning - Treatment & Control



This figure depicts density plots and box plots (triangles mark the averages) of *change cash ETR3* conditional on *hightolow* (the depicted distributions of *change cash ETR3* in this figure include the $[-0.5; 0.5]$ range). Generally, we observe reductions in cash effective tax rates for treatment and control groups and test for the difference between groups (and between periods). The right edge of the figure depicts the difference in the averages of *change cash ETR3* between the *hightolow* conditions (including the 95% error bar). All variables are defined in detail in the Appendix.

Figure 3: Abnormal Returns at Announcement



This figure depicts density plots and averages of cumulative abnormal returns (CAR) by *hightolow*: light-gray fill and solid vertical line if *hightolow* equals 1; dark-gray fill and dashed vertical line if *hightolow* equals 0. If tax knowledge diffusion was anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study in WRDS using a market adjusted model with 100 days as estimation window and a $[-1;1]$ day event window. We find (i) that $CARs$ are on average positive across all announcements in our sample, and (ii) that abnormal returns for treated observations are on average lower, reducing remaining concerns about endogenous treatment assignment. The depicted distributions of $CARs$ in this figure include the $[-0.1;0.1]$ range. All variables are defined in detail in the Appendix.

Table 1: Sample Selection

Panel A: Strategic Alliances

Selection Step SDC Platinum (UPPER) and Compustat (lower) data items in parentheses	Alliances	Firms	Firm $\times$ Alliance
Compustat (cusip) and SDC Platinum (ULTPARENTCUSIP) data merged according to year of alliance initiation (DATEEFFECTIVE / fyear) & strategic alliances between U.S. firms (loc, fic, curcd, cik) & sample period 1994 - 2021	$26,148$	$6,667$	$31,418$
./. Identify all firms in a strategic alliance in Compustat data (NUMBEROFPARTICIPANTS)	$4,654$	$3,676$	$9,425$
./. Identify *pre cash ETR3* and *cash ETR3* (txpd, pi, spi) of all firms in a strategic alliance	808	845	**1,629**

Panel B: Classification

High-Tax Firms	
high-tax firm in alliance to low-tax firm(s) = treated	**284**
high-tax firm in alliance to high-tax firm(s) = control	**965**
Treated + control = number of obs. in main analysis	**1,249**
Low-Tax Firms	
low-tax firm in alliance to high-tax firm(s)	285
low-tax firm in alliance to low-tax firm(s)	95

We exploit data on strategic alliances from Refinitiv's SDC Platinum (SDC) database on strategic alliances over the 1994-2021 period (see Panel A). Initially, we deflate our sample to observations that are flagged as strategic alliances by excluding equity joint ventures from the data. We then reshape data from the alliance to the partner (i.e., firm $\times$ alliance) level. To illustrate: a strategic alliance between two partners translates to one observation for each of the two partners (i.e., two observations at the firm $\times$ alliance level.) Compustat data (via Wharton Research Data Services) provide firm-year-level accounting information, and we merge SDC and Compustat data by using a firm's historical six-digit CUSIP number (at the level of the ultimate parent of the participant). Next, we require to observe *cash ETR3* and *pre cash ETR3* of all partners for an alliance to be considered in our analysis. We classify (see Panel B) firms as high-tax or low-tax based on their *pre cash ETR3* which we industry-year-mean adjust for this purpose (i.e., we are interested in firms that show low/high multiperiod effective tax rates among their industry peers just before an alliance is initiated). We then allocate this adjusted *pre cash ETR3* into four bins according to the quartiles of its distribution. Consequently, strategic alliances may be composed of low-tax firms only, high-tax firms only, or a combination of high-tax and low-tax firms. In our analyses, we focus on high-tax firms as potential beneficiaries of tax knowledge diffusion ("work with superwoman") and discriminate between high-tax firms that invest in strategic alliances with low-tax firms ($hightolow = 1$, treated) and high-tax firms that invest in strategic alliances with other high-tax firms ($hightolow = 0$, control). All variables are defined in detail in the Appendix.

Table 2: Firm $\times$Alliance Observations

N (1,629)		284			965			285			95	
hightolow		=1			=0							
lowtohigh								=1			=0	
	mean	p50	SD	mean	p50	SD	mean	p50	SD	mean	p50	SD
change cash ETR3	−0.0442	−0.0308	0.1783	−0.0144	−0.0235	0.1856	0.0736	0.0430	0.1455	0.0851	0.0721	0.1611
pre cash $ETR3_{[t_{-2};t_0]}$	0.2917	0.2778	0.1373	0.2804	0.2643	0.1296	0.0934	0.0932	0.0578	0.0947	0.0894	0.0610
cash $ETR3_{[t_1;t_3]}$	0.2475	0.2249	0.1449	0.2660	0.2344	0.1755	0.1670	0.1427	0.1501	0.1798	0.1571	0.1676
delta cash ETR3	−0.0377	−0.1151	0.6189	0.0547	−0.0857	0.7829	1.5080	0.4804	3.0103	1.7000	0.6339	3.2187
Partner Controls												
z HQ Distance	−0.0195	−0.3071	1.0534	0.0058	−0.1881	0.9842	.	.	.	.	.	.
PartSameAuditor	0.2183	0.0000	0.4138	0.2010	0.0000	0.4010	0.2175	0.0000	0.4133	0.2526	0.0000	0.4368
PartSameInd	0.4366	0.0000	0.4968	0.4767	0.0000	0.4997	0.4386	0.0000	0.4971	0.6000	1.0000	0.4925
Alliance Controls												
Purpose Develop	0.2606	0.0000	0.4397	0.2705	0.0000	0.4444	0.2632	0.0000	0.4411	0.3053	0.0000	0.4630
Purpose Licensing	0.1725	0.0000	0.3785	0.1679	0.0000	0.3739	0.1719	0.0000	0.3780	0.1263	0.0000	0.3340
Purpose Manufacturing	0.2570	0.0000	0.4378	0.2394	0.0000	0.4269	0.2561	0.0000	0.4373	0.2526	0.0000	0.4368
Purpose Marketing	0.1268	0.0000	0.3333	0.0705	0.0000	0.2561	0.1263	0.0000	0.3328	0.0842	0.0000	0.2792
Purpose Service	0.0387	0.0000	0.1933	0.0249	0.0000	0.1558	0.0386	0.0000	0.1930	0.0000	0.0000	0.0000
Purpose Supply	0.0634	0.0000	0.2441	0.0466	0.0000	0.2110	0.0632	0.0000	0.2437	0.0947	0.0000	0.2944
Purpose Tech	0.3838	0.0000	0.4872	0.4321	0.0000	0.4956	0.3860	0.0000	0.4877	0.3684	0.0000	0.4849
Purpose Wholesale	0.0880	0.0000	0.2838	0.0456	0.0000	0.2087	0.0877	0.0000	0.2834	0.1263	0.0000	0.3340
Firm Controls												
CEO Turnover3	0.3063	0.0000	0.4618	0.3523	0.0000	0.4779	0.3509	0.0000	0.4781	0.2947	0.0000	0.4583
change AdExp3	−0.0010	0.0000	0.0067	−0.0001	0.0000	0.0099	−0.0008	0.0000	0.0089	−0.0010	0.0000	0.0120
change CapEx3	−0.0162	−0.0055	0.0579	−0.0126	−0.0051	0.0577	−0.0120	−0.0042	0.0756	−0.0317	−0.0179	0.0748
change Cash3	−0.0084	0.0011	0.0837	−0.0183	−0.0033	0.0841	−0.0086	−0.0028	0.0856	−0.0103	−0.0064	0.0872
change EBITDA3	−0.0256	−0.0175	0.0591	−0.0303	−0.0207	0.0623	−0.0265	−0.0132	0.0634	−0.0285	−0.0238	0.0567
change GrowthSale3	−0.0484	−0.0309	0.1443	−0.0385	−0.0257	0.1398	−0.0556	−0.0339	0.1503	−0.0356	−0.0376	0.1398
change Intangibles3	0.0266	0.0010	0.0806	0.0258	0.0018	0.0803	0.0326	0.0081	0.0910	0.0292	0.0064	0.0699
change Leverage3	0.0096	0.0008	0.0833	0.0187	0.0037	0.0793	0.0016	0.0000	0.1149	0.0170	0.0000	0.0940
MNE3	0.7148	1.0000	0.4523	0.7492	1.0000	0.4337	0.7684	1.0000	0.4226	0.8000	1.0000	0.4021
NOL3	0.2958	0.0000	0.4572	0.3668	0.0000	0.4822	0.4175	0.0000	0.4940	0.4000	0.0000	0.4925
change NOL3	0.5794	0.0293	3.9612	0.4683	0.0136	3.6939	0.0136	0.0153	4.0831	0.3067	0.0057	4.1486
change PPE3	−0.0284	−0.0180	0.0996	−0.0188	−0.0065	0.0955	−0.0140	−0.0040	0.1015	−0.0081	−0.0112	0.1011
change RnDExp3	0.0015	0.0000	0.0245	−0.0007	0.0000	0.0337	−0.0044	0.0000	0.0357	−0.0005	0.0000	0.0309
change SGA3	−0.0009	0.0000	0.0440	−0.0011	0.0000	0.0475	0.0046	0.0000	0.0470	0.0075	0.0000	0.0500
change Size3	0.3172	0.2248	0.3605	0.3490	0.2169	0.4099	0.3830	0.2751	0.4240	0.3209	0.3025	0.3627

This table shows descriptive statistics for our main variables of interest (e.g., *change cash ETR3*), *partner controls*, *alliance controls*, and *firm controls*. Statistics are presented by conditioning the observations on *hightolow* and *lowtohigh*. All variables are defined in detail in the Appendix.

Table 3: Main Analysis

	Dependent Variable:			
	change cash ETR3	*change cash ETR3*	*delta cash ETR3*	*cash ETR3*
hightolow	−0.0250	−0.0302	−0.1274	−0.0253
	(0.0136)	(0.0127)	(0.0478)	(0.0111)
Partner Controls				
zHQDistance		0.0099	0.0073	−0.0021
		(0.0049)	(0.0207)	(0.0047)
PartSameAuditor		−0.0183	−0.0296	−0.0105
		(0.0139)	(0.0437)	(0.0104)
PartSameInd		−0.0181	−0.0792	−0.0157
		(0.0123)	(0.0473)	(0.0112)
Alliance Controls				
PurposeDevelop		−0.0167	−0.0987	−0.0193
		(0.0133)	(0.0558)	(0.0113)
PurposeLicensing		−0.0266	−0.0674	−0.0139
		(0.0168)	(0.0683)	(0.0135)
PurposeManufacturing		−0.0218	0.0012	0.0277
		(0.0255)	(0.1027)	(0.0184)
PurposeMarketing		0.0083	0.0782	0.0028
		(0.0132)	(0.0551)	(0.0110)
PurposeService		−0.0286	−0.0772	−0.0267
		(0.0232)	(0.0952)	(0.0323)
PurposeSupply		−0.0135	−0.0069	0.0204
		(0.0249)	(0.0736)	(0.0229)
PurposeTech		−0.0206	−0.0649	−0.0138
		(0.0130)	(0.0485)	(0.0119)
PurposeWholesale		−0.0171	−0.0010	0.0180
		(0.0264)	(0.1043)	(0.0195)
Firm Controls				
CEOTurnover3		0.0062	−0.0004	0.0050
		(0.0152)	(0.0628)	(0.0160)
All Firm Controls	No	First Diff.	First Diff.	Levels
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1228	1249
Adjusted R^2	0.0365	0.1434	0.1690	0.1430

This table show the results for estimating equation (5) with *change cash ETR3* (columns (1) and (2): column (2) is the preferred specification), *delta cash ETR3* (column (3)), and *cash ETR3* (column (4)) as dependent variables, whereas *change cash ETR3* is constructed as first difference *cash ETR3* minus *pre cash ETR3*, and *delta cash ETR3* is constructed as *cash ETR3* scaled by *pre cash ETR3* minus one. Our focal variable of interest is *hightolow*. In specifications with *change cash ETR3* and *delta cash ETR3* as the dependent variables, first differences of continuous firm controls (if included) are applied. *z* indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 4: Entropy Weights

	Dependent Variable:			
	change cash ETR3	*change cash ETR3*	*change cash ETR3*	*change cash ETR3*
hightolow	−0.0294	−0.0273	−0.0300	−0.0206
	(0.0121)	(0.0120)	(0.0123)	(0.0114)
Entropy Weights	Two Moments	One Moment	Treatment Year	Pre cash ETRs
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1249	1205
Adjusted R^2	0.1854	0.1765	0.1849	0.2060

This table depicts results for multiple variations of our preferred specification which employ entropy weights that are constructed over different moments and variables. The first specification uses the continuous *firm controls* over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$ to calculate the entropy weights so that control observations are reweighted to satisfy the balance constraint that the averages and variances (i.e., two moments) match the corresponding moments of the treated units. The entropy weights in the second specification are constructed identically but respectively use the first moment as balance constraint. We use the singleperiod firm controls in the year of the treatment (t_1) to calculate the entropy weights in the third specification. The fourth specification uses the singleperiod observations of *cash ETR* in the preperiod $[t_{-2}; t_0]$ to calculate the weights. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 5: Elapsed Time

	Dependent Variable:		
	change cash ETR	*change cash ETR*	*change cash ETR*
Elapsed Time	from $t_{[-2;0]}$ to t_1	from $t_{[-2;0]}$ to $t_{[1;2]}$	from $t_{[-2;0]}$ to $t_{[1;3]}$
hightolow	−0.0178	−0.0215	−0.0223
	(0.0132)	(0.0118)	(0.0108)
Partner Controls	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm
Observations	1227	1200	1158
Adjusted R^2	0.1128	0.1143	0.1254

This table depicts results for three specifications of equation (5). We extend the posttreatment period (keeping the preperiod $[t_{-2}; t_0]$ constant) by one year with each specification. For the construction of the dependent variables (*change cash ETR*), we use the singleperiod *cash ETRs* at t_1, t_2, and t_3, calculate first differences against *pre cash ETR3*, and then calculate the average of the sum of these calculations over the number of periods since treatment (for instance, the dependent variable for the second specification is calculated as [(*cash ETR*$_{t_1}$ - *pre cash ETR3*) + (*cash ETR*$_{t_2}$ - *pre cash ETR3*)]/2). Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 6: Mechanisms

	Dependent Variable:			
	change cash $ETR3$	*change cash $ETR3$*	*change cash $ETR3$*	*change cash $ETR3$*
X equals	*CEOTurnover3*	*zHQdistance*	*PartSameInd*	*zDiffPartMktShare*
hightolow	−0.0331	−0.0303	−0.0389	−0.0256
	(0.0147)	(0.0127)	(0.0170)	(0.0126)
X	0.0042	0.0096	−0.0229	0.0037
	(0.0169)	(0.0058)	(0.0140)	(0.0067)
hightolow $\times X$	0.0091	0.0012	0.0197	0.0156
	(0.0238)	(0.0124)	(0.0247)	(0.0088)
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1249	1167
Adjusted R^2	0.1428	0.1427	0.1432	0.1161
β_1 vs. β_3 [p-value]	[0.0562]	[0.0515]	[0.0454]	[0.0669]

We estimate specifications of equation (5) and include interaction terms of *hightolow* with the cross-sectional variable of interest (X). Our focus is on CEO turnover, geographical proximity, market power, and industry affiliation. *CEOTurnover3* is an indicator variable which equals one when a firm experiences a CEO turnover between t_1 and t_3. *HQDistance* is the handcollected geographical distance (as the crow flies in miles) between the zip codes of the partners' headquarters. *PartSameInd* is an indicator variable that equals one when the partners of an alliance belong to the same industry. *DiffPartMarketShare* is the difference between a high-tax firm's and its alliance partner's *MarketShare*. *MarketShare* is constructed consistent with the market share calculations of the *HHI* (i.e., the percentage of a firm's sales within industry and year). Column (4) neglects firms from the "other" industry. z indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 7: Alternative Channels

	Dependent Variable:	
	*change cash ETR*3	*change cash ETR*3
Channel	*PartSameAuditor*	*BoardTie*3
hightolow	−0.0209	−0.0303
	(0.0125)	(0.0127)
PartSameAuditor	−0.0077	−0.0185
	(0.0138)	(0.0140)
hightolow × *PartSameAuditor*	−0.0433	
	(0.0375)	
*BoardTie*3		−0.0126
		(0.0300)
Partner Controls	Yes	Yes
Alliance Controls	Yes	Yes
Firm Controls	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
SE Cluster	Firm	Firm
Observations	1249	1249
Adjusted R^2	0.1443	0.1428

We focus on channels of intentional tax knowledge transfers to analyze whether we spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances. In column (1), we focus on partners that share an audit firm. The indicator variable *PartSameAuditor* equals one when the partners in an alliance share an audit firm in t_1. In column (2) we consider board ties among the partners in the alliances of our sample. We use data from ISS to construct the indicator variable *BoardTie3* that equals one when the partners have at least one common member among their board of directors (at any point over the $[t_1; t_3]$ period). Note that board ties are rare among the observations in our sample (we observe an overlap of seven board ties among the 284 *hightolow* = 0 observations). Therefore, we do not estimate a specification that includes an interaction but respectively add *BoardTie3* as additional control variable to the analysis. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 8: Robustness Checks

	Dependent Variable:			
	change $CTD3$	*change* $GAAP\ ETR3$	*change* $cash\ ETR3$	*change* $cash\ ETR3$
Specification	Alt. Measure	Alt. Measure	Pre-Volatility	Low-Tax Firms
hightolow	−0.0018	−0.0215	−0.0258	
	(0.0010)	(0.0103)	(0.0119)	
lowtohigh				−0.0058
				(0.0184)
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	999	380
Adjusted R^2	0.2112	0.0978	0.1475	0.0771

This table depicts results for specifications of equation (5) with *change CTD3* (column (1)) and *change GAAP ETR3* (column (2)) as dependent variables. *CTD* is constructed by following by Henry and Sansing (2018). *GAAP ETR3* utilizes a GAAP measure of taxes paid (i.e., total income tax expense) instead of cash taxes paid in the numerator of the effective tax rate. The directional interpretation of these measures is identical to our main outcome variable of interest. In column (3), we focus on volatility in the singleperiod *cash ETR* observations in the pretreatment period (i.e., we calculate a volatility measure σ that captures the volatility between the singleperiod cash effective tax rates and *pre cash ETR3* in the pretreatment period) because we want to ensure that we do not interpret volatility in high-tax firms' tax planning measures as eventual treatment effect (see also the fourth specification in Table 4, which uses pretreatment singleperiod *cash ETRs* to calculate the entropy weights). Therefore, we exclude observations that belong to the top-quintile of this volatility measure from the analysis. The fourth specification focuses on low-tax firms. We construct *lowtohigh*, which is an indicator that equals one for low-tax firms in alliances with high-tax firms and zero for low-tax firms in alliances with low-tax firms and estimate its impact on *change cash ETR3*. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 9: Valuation Allowance Releases

	Dependent Variable:		
	*change Val. All. Release*3	*change cash ETR*3	*change cash ETR*3
X equals		*z change Val. All. Release*3	*z Part Pre Val. All. Release*3
hightolow	−0.0283	−0.0297	−0.0280
	(0.0353)	(0.0128)	(0.0132)
X		−0.0009	−0.0025
		(0.0093)	(0.0070)
hightolow $\times X$		0.0030	−0.0021
		(0.0139)	(0.0100)
Partner Controls	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm
Observations	1249	1249	1249
Adjusted R^2	0.1246	0.1430	0.1432

We follow the insights by Drake, Hamilton, and Lusch (2020) and calculate the probability for valuation allowance releases from Compustat data. We then construct first differences between the $[t_1; t_3]$ and $[t_{-2}; t_0]$ periods (*change Val. All. Release3*) and test whether our treatment indication increases the probability for a valuation allowance release (column (1)). In column (2), we include *z change Val. All. Release3* as additional control variable, interact it with *hightolow*, and regress these and the control variables from equation (5) on *change cash ETR3* (note that we exclude *MNE3*, *NOL3*, and *change NOL3* from the firm controls because loss information enter the construction of *change Val. All. Release3*). In column (3), we capture not the own firm's but the partner's probability for a valuation allowance release over the $[t_{-2}; t_0]$ period (*Part Pre Val. All. Release3*) and include and interact this variable with *hightolow*. *z* indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.