Contagious Deregulation*

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Abstract

Each year, the federal government distributes \$900 billion in grants to nonprofit organizations. Grants exceeding a specified threshold are reviewed by an auditor who is chosen by the recipient. Using a unique grant-level dataset covering \$23.7 trillion in grants, we exploit a major deregulatory reform that raised the threshold and exempted nearly 10% of grants from auditing. In a difference-in-differences framework, we find that treated auditors – who had clients below the new threshold before the reform – nearly ceased to issue negative audits for their remaining clients after the reform. Further tests show that treated auditors struggled to retain clients after the reform and offered more lax supervision to stay competitive. Thus, the deregulation of smaller grants unintentionally weakened the oversight of larger, non-deregulated grants. In a structural model, we identify key cost factors that discourage auditors from issuing negative opinions, especially when the demand for auditors declines. We estimate that the deregulation nearly doubled those costs, prompting auditors to show leniency even when clients potentially mismanage their grants. We evaluate alternative policies, such as subsidizing auditor costs or nationalizing the auditing process, to deregulate markets without compromising the quality of monitoring. Combined, our paper reveals the unintended contagion effect of deregulation, which can impose externalities on non-deregulated firms, service providers, and taxpayers.

Keywords: Economics of regulation, federal grants, nonprofit finance, auditor incentives

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Introduction

Federal grants provide a crucial flow of capital for nonprofit organizations. In a typical year, federal agencies distribute grants worth $900 billion to 22,000 organizations, including 700 private universities ($48 billion), 4,600 private social services organizations ($30 billion), and 150 private hospitals ($5 billion). The government distributes the non-repayable grants generously and monitors their utilization with a gentle touch. Organizations with grants below a designated threshold are exempt from audits. Those exceeding the threshold must pick a private auditor who reviews their compliance with federal regulations. The auditor's report, known as Single Audit, is submitted to the government. If the audit reveals significant issues, it may prompt additional government scrutiny.

We use this setting to uncover the contagion effect of deregulation. To that end, we take advantage of a major deregulatory reform from 2015, which raised the threshold for Single Audits from $500,000 to $750,000. This effectively exempted 10% of nonprofit organizations from auditing. Using a novel grant-level dataset, which covers the years before and after the reform, we document an unexpected outcome: auditors who lost clients with smaller grants (below the new threshold) offered leniency to clients with larger grants (above the new threshold), leading to an overall weaker monitoring of federal grants. Thus, by lowering the demand for auditors, deregulation unintentionally imposed an externality on non-deregulated entities and taxpayers. Using a structural model, we show how deregulation increases auditing costs and motivates auditors to issue positive opinions, even if their clients use the grants improperly. We then propose contagion-free reforms, which combine deregulation with cost subsidies for auditors and reduce their incentive to offer leniency.

Our dataset contains all Single Audit reports from 1997 to 2022. Key variables include the identities of the auditor and the client and the size of the federal grant. Most importantly, we observe the audit outcome and whether it was positive ("unqualified") or negative ("qualified" or "adverse"). The dataset covers 575,855 federal grants worth $23.7 trillion (in 2023 USD), allocated between 51,000 recipients and monitored by 53,000 auditors. The average auditor has 3.8 clients, the average client receives an annual grant worth $9.2 million, and 10.5% of clients receive a negative audit outcome.

We first study the reform in a difference-in-differences framework. We define auditors as treated if they had at least one client below $750,000 before the reform. When those clients were deregulated, they likely ceased to demand auditing services. Equivalently, control auditors had no client below $750,000 before the reform but at least one client below $1,500,000.[1] We find that treated auditors became more reluctant to issue negative audits after the reform. The effect is statistically and economically significant, up to 41% decline relative to the pre-reform mean. We find an even stronger effect on adverse opinions, used to describe severe noncompliance, which treated auditors nearly stopped issuing after the reform (84% decline). Our estimates remain robust across various specifications, including auditor, client, and state×year fixed effects. Event studies demonstrate parallel pre-treatment trends between treated and control groups, and we find similar results in specifications with alternative control group definitions and time windows.

Our results highlight an unintended contagion effect: deregulation of smaller grants (below the threshold) led to fewer negative audits of larger grants (above the threshold). Subsequent tests link this effect to auditor behavior: the lower demand for auditing services motivated auditors to compete for clients by offering leniency.[2] We show that treated auditors lost clients after the reform (1.6-1.9 on average), and the ratio of auditors to clients has increased, suggesting a heightened competition between auditors. We also show that a negative audit is costly to the auditor: the client is 17.1% more likely to lose access to large grants, and even if they receive such a grant, the client is 11.2% more likely to hire a different auditor. Taken together, these findings suggest that the reform motivated auditors to offer leniency to avoid further client losses, resulting in fewer negative audits.

We further examine whether these results are driven by the shift towards large-grant

[1]The second condition ensures that treatment and control auditors are comparable. We obtain similar results using a range of upper bounds for the control group.

[2]The leniency could also be an indirect result of price competition, which would limit auditor resources (as in Hallman, Kartapanis, and Schmidt (2022)).

clients, who may generally be more compliant with regulatory requirements. This explanation appears inconsistent with the evidence. First, the primary specification is difference-in-differences with client fixed effects. This accounts for time-invariant differences between large and small clients and for overall change in compliance after the reform. Instead, we leverage the differential exposure of auditors to reduced client demand. Second, we obtain similar results in a restricted sample of clients that had large grants both before and after the reform. Moreover, we find no change in audit outcomes around the $750,000 threshold during 2004-2012, prior to the baseline estimation window. It suggests that large and small client around the threshold are not fundamentally different, once we control for observable auditor and client characteristics. Thus, it is unlikely that client compliance is a major driver of the contagion effect we document.

Overall, the evidence suggests that deregulation motivated auditors to become more lenient. This is an unexpected contagion effect: partial deregulation of the market led to weaker monitoring of the remaining market participants. To help us quantify the importance of auditor leniency, we develop and estimate a dynamic discrete choice model of the decision to issue a negative audit.[3] In our model, the auditor reviews a client's grant and must decide whether to issue a negative opinion. The monetary and non-monetary costs of a negative audit are captured by three structural parameters. The recurring cost (θ) reflects the auditor's effort and the potential risk of losing the client. The switching cost (η) reflects the additional cost incurred by the auditor when transitioning from a positive to a negative audit. The scale discount (δ) captures economies of scale and affects the switching cost, since auditors with many clients are less sensitive to effort and revenue risk. The auditor also derives utility from a private information shock ε, a state variable observed by the auditor but not by the econometrician. Intuitively, suppose the client mismanaged the federal grant.

[3]In general, formulating and estimating dynamic models of discrete decisions is difficult. Our approach is based on the conditional choice probability (CCP) estimation, which facilitate taking such models to the data. Arcidiacono and Ellickson (2011) provide an overview of applications of such estimations, and some recent examples include failed banks closures (Kang, Lowery, and Wardlaw (2014)), CEO succession planning (He and Schroth (2024)), and under-representation of women CEOs (He and Whited (2023)).

In that case, the auditor will receive an extra utility from a negative audit (good reputation) and a disutility from a positive audit (bad reputation). While we cannot directly study client misconduct in the reduced-form analysis, we can account for that in our model while focusing on the role of auditor incentives.

Estimating the model, we find that the structural parameters are positive and statistically significant. We plot the optimal auditing policies and derive two takeaways. First, audit outcomes are persistent over time. For instance, a "positive" auditor prefers to stay positive over switching to negative. Second, large auditors are more flexible to switch between positive and negative. Intuitively, large auditors enjoy economies of scale, offsetting the switching costs and allowing them more freedom to alternate between audit outcomes. Based on these features, the model successfully predicts audit outcomes in different scenarios that closely match the data-implied outcomes. This bolsters our confidence in the model's ability to capture the decision-making process of auditors.

Armed with these insights, we turn to study the quantitative implications of the deregulation. We estimate the model separately before and after the reform, in 2013-2015 versus 2016-2018.[4] The model successfully generates the data-implied treatment effect, that is, showing that deregulation can reduce negative audit rates by nearly 50%. Moreover, the model attributes this effect to change in auditor incentives: the reform doubled the recurring cost θ and cut the scale discount δ by more than a half. This increased the costs of negative audits overall, and especially limited auditor incentives to break away from their past choices and switch from positive to negative. Consequently, the reform motivated auditors to show leniency toward their remaining clients, regardless of how they mismanage their grants (the unobserved ε).

Our final set of exercises considers counterfactual policies. We first propose contagion-free deregulation reforms, which deregulate one segment of the market without compromising the quality of monitoring in other segments of the market. To achieve that, deregulation

[4] Since our model incorporates the unobservable ε, we compare subsamples following the procedure outlined in Swait and Louviere (1993) and He and Whited (2023).

must be coupled with policies that reduce the costs of audit. For example, subsidize the overtime payments required to conduct a thorough audit, thus lowering the recurring cost θ. We estimate potential combinations of the recurring cost and the switching cost that would eliminate the contagion effect. Indeed, the actual costs we have estimated in the post-reform period are above the curve of the counterfactual policies we propose. This explains why the 2015 reform achieved the surprising outcome: in the absence of targeted cost-reducing policies, auditing costs became too high and motivated auditors to offer leniency.

More speculatively, we consider a set of experiments which shut down the costs of negative audits. One could think of it as nationalizing the Single Audit process, where the government – immune to worries of client defection – directly audits federal grants. We find, for example, that eliminating the recurring cost will double the probability of a negative audit from 8.2% to 19.1%. We translate those probabilities to dollar terms, to capture the expected dollar value of federal grants that will be monitored more rigorously by the zero-cost auditor. From that perspective, shutting down the recurring cost will put $21 billion annually in the "negative audit" column.[5]

In sum, we study the deregulation of the market for federal grants. By raising the minimum threshold that triggers mandatory audit, the reform reduced the demand for auditors. This, in turn, motivated auditors to offer leniency to their remaining clients, resulting in weaker monitoring of large grants. In a structural model, we highlight how this dynamic stems from the cost function of auditors. We propose counterfactual policies that partially deregulate markets while preserving the quality of monitoring in the non-deregulated portions of the market.

Our paper contributes primarily to the literature on the economics of regulation. We show that deregulation can substantially affect firms which have not been deregulated. This unexpected contagion effect differs from typical studies that document direct effects of deregulation on deregulated firms, for instance, how bank deregulation improves the quality of

[5]Of course, one must also account for other costs, such as massive hiring of federal auditors and tackling various issues related to government efficiency.

bank lending (Jayaratne and Strahan (1996)). We highlight two channels that generate the contagion effect: change in auditor incentives and increased compliance by non-deregulated firms. While there is little evidence here for the latter channel, it could be at play in other settings. Using a structural model, we show how auditor incentives contribute to more lax monitoring following a deregulation, regardless of potential contemporaneous changes in compliance by non-deregulated firms.

Our work also contributes to the literature on the efficient design of regulation, where papers typically study the details of industry-specific rules (Gropp et al. (2019); Behn, Haselmann, and Vig (2022)).[6] We uncover the contagion effect, explain its sources, and propose complementary steps to prevent contagion. While we identify the effect in the setting of federal grants, lessons can be applied to other settings. First, many regulations in various domains are triggered by a threshold. A contagion effect can take place whenever such threshold is established or later on updated. Moreover, calls abound to intentionally ease the regulatory burden on small businesses.[7] As we show, deregulating small entities can trigger a contagion effect for large entities. Finally, many firms rely on compliance professionals to navigate the complex regulatory landscape. Deregulation will limit the demand for their services and force them to compete harder, perhaps by offering price concessions or shortcuts to avoid regulatory scrutiny. Thus, deregulation can have a contagion effect on non-deregulated firms through the channel of compliance professionals. We leave those interesting questions to future research.

We also contribute to the literature on nonprofit finance. Studies focus on theoretical models of agency problems associated with nonprofit organizations,[8] or examine investment strategies and performance of university endowments and museums.[9] To our knowledge,

[6]A related literature studies organizational features of regulatory agencies, such as compensation incentives (Kalmenovitz, 2021; Chen, Hajda, and Kalmenovitz, 2024), distribution of field offices (Gopalan, Kalda, and Manela, 2021), and degree of supervision (Hirtle, Kovner, and Plosser, 2020).

[7]See, for instance, a recent Forbes article and studies showing that regulation is particularly burdensome for smaller firms (Kalmenovitz, Lowry, and Volkova (2023); Simkovic, Trebbi, and Zhang (2023)).

[8]Fama and Jensen (1983a); Fama and Jensen (1983b); Hansmann (1987); Rose-Ackerman (1996); Fama and Jensen (1985); Glaeser (2002); Fisman and Hubbard (2003).

[9]Lerner, Schoar, and Wongsunwai (2007); Lerner, Schoar, and Wang (2008); Dimmock (2012); Brown

ours is the first paper to focus on a major source of capital for nonprofit organizations: federal grants. Our analysis provides new stylized facts and insights on the regulation of this important, yet understudied, mode of financing.

Finally, our paper adds to the literature on federal grants, where papers typically study the efficacy of specific grant programs.[10] To our knowledge, we are the first to study economic questions that are relevant for all federal grant programs. Based on a comprehensive dataset, and a combination of reduced-form and structural estimations, we show how a major (but partial) deregulation led to lenient monitoring of the remaining federal grants, and suggest alternative policies to mitigate some of those negative externalities.

1 Institutional setting and data

1.1 Institutional setting

Our paper is focused on Single Audit: the mandatory annual audit of entities receiving federal funds. This is the primary tool for monitoring the use of more than $1.3 trillion in annual federal award expenditures. The goal is to hold recipients accountable and ensure that federal funds are properly obtained and managed. The term "single audit" dates to the Single Audit Act of 1984, which for the first time allowed recipients to conduct a single audit covering all the federal funds it has received during the year.[11]

The Single Audit includes two components.[12] First, it assesses whether the financial statements fairly represent the recipient's financial position, in accordance with the Generally Accepted Accounting Principles (GAAP) or other accounting systems. Additionally, it assesses whether the recipient complies with all federal regulations and policies associated

et al. (2014); Gilbert and Hrdlicka (2015); Yermack (2017).

[10]For example, programs run by the Departments of Energy (Howell (2017); Myers and Lanahan (2022)), Education (Abbott et al. (2019)), and Justice (Chalfin et al. (2022)).

[11]Pub. L. 98-502, October 19, 1984, 98 Stat. 2327.

[12]The details are in Title 31 of the U.S. Code, Chapter 75, and in Title 2 of the Code of Federal Regulations, Part 200.

with the federal awards it has received. The audit is performed by an external auditor who is independent, competent (appropriate education and experience), and licensed (professional certifications).[13] Selection of an auditor is subject to the Federal procurement process, which requires an open and competitive bidding. The client develops a request for proposal, and evaluates the proposals based on factors such as peer reviews and experience.

According to the most recent official burden estimates (Kalmenovitz (2023)), there are approximately 45,000 annual Single Audit submissions. In terms of time burden, the estimated total annual burden is 962,775 hours. This includes the time to review instructions, obtain the required data, and complete and review the information.[14] In terms of dollars, the estimated total annual cost of Single Audits are $27.9 million for clients and $43.3 million for auditors.[15] The former includes the internal costs borne by the recipient's, for instance, staff searching for records. The latter includes the estimated auditor fees, which are paid for through a portion of the grant awards received.

At the conclusion of the Single Audit, the auditor issues separate opinions on the financial statements and on the compliance with federal regulations. For each of these, auditors may issue one of four types of opinions. An *unqualified* ("clean") opinion means that the financial statements are presented fairly and the client has materially complied with Federal program requirements. A *qualified* opinion is similar but highlights specific exceptions. An *adverse* opinion means that the financial records are misrepresented and inaccurate, or that the client did not comply in all material respects with federal regulations. A *disclaimer* of opinion is issued when the auditor is not able to express an opinion, for instance, if the client restricted the auditor's ability to access relevant data. The auditor can also issue one or more findings, pointing to concrete deficiencies. Findings are categorized into four levels, based on their severity: control deficiency, significant deficiency, material weakness,

[13]The requirements are outlined in the Generally Accepted Government Auditing Standards, known as GAGAS or the Yellow Book, which is compiled by the Government Accountability Office.

[14]The burden for a large grantee is estimated at 100 hours, while the total burden for a small grantee is estimated at 21 hours.

[15]Using data on average hourly wage for state, local government, and nonprofit organization ($29), and on average hourly wage for a CPA in the United States ($45).

and material noncompliance. The client prepares a corrective action plan, to respond and address each audit finding. The Single Audit package is then submitted to the Federal Audit Clearinghouse (FAC).

If the Single Audit reveals deficiencies, the entity must take corrective actions to address any compliance issues. Inability to conduct the audit, or to correct compliance deficiencies, directly affects the availability of federal funding. The federal agency can temporarily withhold cash payments, disallow use of funds already disbursed, and initiate suspension or debarment from future awards (§200.339).

The Single Audit requirement does not apply to for-profit organizations. Crucially, it depends on a threshold: the entity must be audited if and only if it expends more than a specified dollar threshold in a given fiscal year. If it expends less than the threshold, no audit is required. Key to our analysis, the threshold has changed over time. It was set at first at $25,000, and raised to $300,000 in June 1996 following the Single Audit Act Amendments of 1996.[16] In June 2003, the threshold was further raised to $500,000. Those revisions became effective for fiscal years ending after December 31, 2003.[17] In December 2013, the OMB published the Uniform Guidance for singe audits, consolidating instructions that were previously dispersed across several OMB guidance documents.[18] The substance of the instructions was not changed, except for raising the threshold once again to $750,000.[19] Those revisions became effective for fiscal years starting after December 26, 2014.

[16]Pub. L. 104-154, July 5, 1996, 110 Stat. 1396. The implementing regulations were published by the Office of Management and Budget (OMB) on April 30, 1996 (61 FR 19134), and the original proposal was released for comments on March 17, 1995 (60 FR 14594). This modification reduced the number of entities subject to the Single Audit requirement from approximately 35,000 to 25,000, according to an ex-post analysis published in the Federal Register in June 1997 (62 FR 35302).

[17]68 FR 38401. The proposal was released for comments on August 2002 (67 FR 52545). OMB estimated that the number of reporting entities will shrink by 18% while still covering 99.5% of federal grants.

[18]Uniform Administrative Requirements, Cost Principles, and Audit Requirements (78 FR 78590), replacing OMB Circulars A-21, A–87, A–110, and A–122, A–89, A–102, A–133, and A–50. The proposal was released for comments on February 28, 2012 (77 FR 11778).

[19]Originally, OMB proposed to raise the threshold to $1,000,000, and to create a middle tier between 1 and 3 million with a more limited Single Audit.

1.2 Data sources

We source a unique dataset from the Federal Audit Clearinghouse (FAC). This is the central repository where all Single Audit reports are collected and disseminated to federal agencies. For each audit, we observe the year (calendar and fiscal) and total expenditure of federal funds covered by the audit. We adjust dollar amounts for inflation and express them in 2023 USD. We further observe full details on the clients – the entity's name, address, and contact person, and equivalent information on the auditing firm. Clients are classified into 10 major categories (such as State, Municipality, and Non-Profit Organizations) and 82 sub-categories. Importantly, we have detailed information on the audit results. Those results are reported in three layers. First, we learn the type of opinion: unqualified ("positive"), qualified, adverse, and disclaimer. This information is available separately for the financial statements and the compliance. Additionally, we learn the level of findings (if there were any): material weakness, material noncompliance, and reportable condition. Finally, we have information on the client's level of risk and whether it is a going concern.

Clients are uniquely identified via their Employer Identification Number (EIN).[20] Auditors are also uniquely identified by their respective EIN but only since 2013. To handle observations prior to 2013, we backfill missing pre-2013 EINs with post-2013 EINs based on unique auditor names.

1.3 Descriptive statistics

The raw data covers all Single Audits conducted from 1997 to 2022. It includes 573,050 reports covering $23.6 trillion over 26 years, expressed in constant 2023 USD.[21] Over the entire sample period, the grants were disbursed among 51,318 unique clients and monitored by 52,870 unique auditors.

[20]Since 2004, some clients are also identified by their Data Universal Numbering System (DUNS) number, but the coverage is not full.

[21]If a recipient appears to have filed more than one audit in a given year, we keep the observation associated with the highest amount. Based on this criteria, we removed 2,805 observations.

Table 1, Panel A, reports key features of our dataset. Our primary sample is a panel of auditor×clients, with 194,846 unique auditor×client pairs. Note that an auditor can have multiple clients in a given year, while a client can only have one auditor in a given year (this is the main rationale of the Single Audit program). Therefore, the auditor×client panel is effectively a client-level panel. The average recipient spends \$41.1 million dollars of federal funds a year (expressed in constant 2023 USD). The distribution is heavily skewed, with the median client spending only \$2.6 million dollars. Pair relations are sticky: conditional on having a relation at time t, the survival rate at time $t+1$ is 60.3% and the switching rate (where both auditor and client continue to time $t+1$ but are no longer connected) is 12.9%. The vast majority of the audits (87.5%) resulted in a positive opinion while 10.5% resulted in a negative opinion.[22] Out of negative audits, 9.1% are qualified and 1.5% are an adverse opinion (more severe offenses). Negative audit rates, either qualified or adverse, are higher in the financial statement part (6.7%) than in the compliance part (4.7%). Focusing on the findings, 11.4% of the audits revealed material weaknesses and 4.6% revealed material non-compliance.

In Panel B of Table 1 we reshape the data at the auditor level, with 151,046 auditor×year pairs. The average auditor audits 58.8 million dollars in annual federal expenditures, whereas the median auditor audits 2.1 million dollars. The average auditor has 3.8 clients in a given year, compared to 1 client for the median auditor. Conditional on having a relation at time t, the survival rate of the average auditor at time $t+1$ is 58.5%.

Figure 1 provides a high-level annual summary of the market for federal grants. In an average year, approximately \$907.3 billion in federal grants are disbursed among 22,000 recipients and monitored by 5,800 auditors. The scope of federal grants has been expanding over time, across all dimensions, except for 2010-2015. Moreover, the reform had a substantial impact on the market. In the 3 years leading up to the reform (2013-2015), grants worth \$2.9 trillion were disbursed among 72,549 recipients and supervised by 4,924 auditors. After

[22] The residual (2%) are either undisclosed or undetermined, and we left those values blank.

the reform (2016-2018), the corresponding numbers are $3.2 trillion, 59,579, and 4,029, respectively. In other words, while the total value of federal grants rose by 6.6%, the number of recipients shrank by 17.9% and the number of auditors declined by 18.1%.

2 Empirical evidence for the contagion effect

2.1 Hypotheses and empirical strategy

The deregulation reform raised the threshold for Single Audits. By raising the threshold, entities spending less than $750,000 of federal funds became exempt from the audit requirement. While they could still voluntarily file Single Audits, they virtually never elect to do so. Indeed, in 2010-2015 there were 11,102 entities spending less than $750,000. In 2016, the number nearly dropped to zero and remained so till the end of the sample period.

Our goal is to identify the contagion effect on non-deregulated entities, which continue to receive large federal grants and undergo Single Audits. In particular, we investigate whether those entities are more likely to successfully pass the audit. Since they continue to abide by the same regulatory requirements, the null hypothesis is that they had no reaction to the reform. However, we hypothesize that the reform could affect their audit outcomes for at least two reasons. First, since fewer recipients are regulated, regulators can closely examine the compliance of the remaining recipients. This would deter misconduct and lead to better audit outcomes. Second, since auditors now compete for fewer clients, they may choose to gain a competitive advantage by offering leniency. Alternatively, they may offer lower price which would inadvertently force them to compromise on audit quality.[23]

To study possible contagion effect, we estimate a difference-in-differences specification:

$$NegativeAudit_{a,c,t} = \alpha + \beta \cdot Treated_a \times Post_t + \lambda_a + \lambda_c + \lambda_t, \tag{1}$$

[23]Note that competition can improve audit quality, if auditors compete by offering more effort. See Hallman, Kartapanis, and Schmidt (2022) for literature review.

where the outcome equals 1 if auditor a issued a negative opinion (qualified or adverse) for client c, and equals 0 for positive opinion.[24] For clarity, we multiply the outcome variable by 100. $Post = 1$ from 2016 onwards, when recipients of small grants became exempt from the Single Audit requirement.[25] We limit the sample to the years 2013-2018, creating a symmetric sample of ± 3 years around the reform. $Treated = 1$ for treated pairs and 0 for control pairs. Note that treatment is denoted with a, that is, treatment is defined by the auditor. Concretely, we look at the auditor's clients in 2015, *before* the reform. If the auditor had at least one client below \$750K, they are assigned to the treatment group ($Treated_a = 1$). Since those clients no longer request audits, their former auditors are treated by the deregulation. The control group includes auditors who had no clients below \$750K in 2015 and had at least one client between \$1,000,000 and \$1,500,000 in 2015. The first condition ensures that control auditors are not directly treated by the reform, since none of their clients were deregulated. The second condition ensures we focus on auditors with clients near the \$750,000 threshold in the pre-period, such that the treatment and control groups are comparable. In this specification, β identifies the reform's effect on clients whose auditor have lost at least one of their (other) clients due to the reform, relative to clients whose auditor was unaffected by the reform. The identification relies on several assumptions which we discuss in the next section. λ_t is year fixed effects, which absorb $Post$ and other macroeconomic conditions, such as economic growth and political climate. λ_c and λ_a are client and auditor fixed effects, the latter of which absorbs $Treated$. We also consider alternative combinations of fixed effects, which we discuss in the next section. Standard errors are double clustered at the auditor and client level.

[24] In subsequent tests we distinguish between levels of negativity and also consider the audit's findings.

[25] The dataset is organized by the audit year which always follows the fiscal year end. As explained in Section 1.1, the reform applied for fiscal years *starting* after December 26, 2014, and thus *ending* after December 26, 2015, which means that the audit will begin after that day. For simplicity, we include in the post-reform period all audits starting in 2016.

2.2 Main result

The results are reported in Table 2. In the baseline specification (column 1), we find that the reform reduced negative audits by 3.0 percentage points among treated auditors, relative to the control group. The effect is statistically significant and economically large: the negative audit rate in 2015, before the reform, was 7.2%. Thus, the effect we document corresponds to 41% of the unconditional mean of the outcome variable in 2015.

In the remaining three columns we tighten the fixed effect specifications. We first replace the auditor and client FE with auditor×client FE (pair), and obtain virtually identical estimates (column 2). This is consistent with the idea that a client-auditor relations are stable and most auditors work with a single client. More importantly, this result specifically identifies the treatment effect within auditor-client pair, suggesting that pair dynamic have changed after the reform and led to fewer negative audits. We then add entity type by year fixed effects (column 3). Recall that clients are classified into 10 major entity type such as State, Municipality, and Non-Profit Organizations. In this specification, we effectively compare clients within the same "industry" and year, some treated by the reform and some not. Here, the coefficient is smaller as we narrow down the identifying variation. Finally, we add state by year fixed effects (column 4). This specification removes state-level trends that could affect audit results, for instance, changing economic conditions or other factors contributing to financial misconduct. The coefficient remains virtually identical.

In the last two columns of the table, we use two granular outcome variables: qualified opinion (less severe offenses) and adverse opinion (more severe offenses). We find that the point estimates are similar across both types of negative opinion. However, they differ substantially in terms of economic magnitude, since the unconditional incidence of qualified opinion is higher than adverse opinion. Taken this into account, we find that the reform decreased adverse (qualified) opinions by 84% (28%) relative to their sample mean. In other words, the reform decreased negative audits in general, and in particular nearly eliminated the likelihood of a severe adverse audit.

Overall, the results document a contagion effect of deregulation. Following the partial deregulation of the federal grants market, treated auditors – who lost clients due to the reform – issue fewer negative opinions for their remaining clients. This is an unexpected outcome of the reform, which aimed to reduce regulatory burden for deregulated recipients but not change the behavior of regulated recipients. Before turning to study the channels leading to this behavior, we report several robustness exercises to support the causal interpretation.

2.3 Additional evidence

The identifying assumption for Equation (1) is that, absent the reform, audit results would trend similarly for treated and control groups. While we cannot definitively prove the parallel trends assumption, we provide suggestive evidence to support it. We estimate a dynamic version of Equation (1):

$$NegativeAudit_{p,t} = \alpha + \sum_{\tau=2013}^{2018} \beta^{\tau} \cdot Treated_p \times \mathbb{1}_{t=\tau} + \overrightarrow{X}_{p,t} + \lambda_t + \lambda_p, \tag{2}$$

This specification compares the audit outcomes of the treated and control groups each year, captured by the coefficient β^{τ}. We include year and pair fixed effects, our preferred specification, and plot the resulting coefficients in Figure 2. Before the treatment (2015 or earlier), the differences between the groups are statistically insignificant and the point estimate is effectively zero. Only after the treatment (2016 or later), the treated group demonstrates substantially lower rates of negative audits. Interestingly, the gap in audit results persists up to 3 years after the reform.

The primary analysis is at the client×auditor level, allowing us to absorb client-level factors. However, the treatment is at the auditor level and does not vary across clients. To address this issue, we also estimate the following specification:

$$y_{a,t} = \alpha + \beta \cdot Treated_a \times Post_t + \lambda_a + \lambda_t, \tag{3}$$

which is similar to Equation (1) except that we study outcomes at the auditor×year level. Consequently, we omit client, entity×year, and state×year fixed effects, which are no longer applicable. The outcome is an indicator for any negative audit issued by the auditor to any of their clients. We also consider two other outcomes: the number of negative audits and the rate of negative audits (number of negative audits divided by number of clients). The results are in Table 3, Panel A. We find that the reform reduced the likelihood of any negative audit by 6.1%, which is 27% of the pre-reform mean. Taking into account the number of clients, the auditor-level negative audit rate has declined by 2.6 percentage points, which is 40% of its pre-reform mean.

Our baseline specification limits the control group to auditors with clients between $1,000,000-1,500,000. This helps ensure that treated and control group are relatively similar. For robustness we consider alternative upper bound, starting from $1,100,000, and gradually expanding it to $1,500,000. The results are in Table 3, Panel B. We find that adjusting the upper bound for the control group makes little difference, and the coefficient is statistically indistinguishable from the one obtained using the baseline definition. In a separate test we examine the time restriction, which in the baseline specification is ±3 years around the reform. A tight window could help identify the causal effect of the reform, but also runs the risk of ignoring some of the long-run shifts in auditor behavior. To examine this possibility we consider alternative windows, starting from ±1 (2015-2016) and gradually expanding it to ±7 (2009-2022). The results are in Table 3, Panel C. We find that our baseline results are robust to alternative windows, with some attrition in statistical significance in the coefficient estimates for the largest windows.

2.4 Channels

In Section 2.2 and Section 2.3 we document a contagion effect, as deregulation of one sector (small recipients) affects outcomes in another sector (large recipients). In this section we investigate the two primary channels: a decline in client misconduct and an increase in auditor leniency. Those channels are not mutually exclusive, and in a reduced-form analysis we cannot definitively disentangle the two: we only observe the final audit outcome, not the client's underlying behavior or the auditor's choice on how to monitor the client. Our structural model (Section 3) can account for those unobserved factors. In this section, however, we will conduct indirect tests to shed some light on those channels.

2.4.1 Auditor leniency

The first channel we consider is centered on the auditor. According to this channel, treated auditors – who have lost their deregulated clients – offer leniency to their remaining clients. This leniency can occur if treated auditors become resource constrained and therefore cannot conduct rigorous audits. Alternatively, treated auditors may exhibit leniency to prevent their remaining clients from leaving. After losing their deregulated clients, the marginal benefit from the remaining clients is higher. If the auditor does not show leniency and issues a negative audit, that could backfire: the client will either lose access to federal funding altogether, or seek a different auditor who will show more leniency.

We offer several pieces of evidence to support this channel. First, we turn to the auditor×year sample and study the treatment effect on the auditor's portfolio. We estimate Equation (3) with year and auditor fixed effects and various outcomes representing the auditor's portfolio. We restrict the sample to include only auditors that were active before and after the reform and present the results in Table 4, Panel A. On the extensive margin, treated auditors became 12.5% more likely to lose at least one client, which is 23% of the pre-treatment probability.[26] Moreover, after losing some clients due to the reform, treated auditors struggled to fill the gap with new clients. They are 15.2% more likely to have fewer clients overall, and have lost 1.6-1.9 clients on average (depending on whether we condition on losing any client).

[26] Note that, by definition, treated auditors had at least one client below the threshold in 2015. However, some of those clients have risen above the threshold in 2016. We estimate that the probability of a client being below the threshold in 2015 and rising above it in 2016 is 17.3%.

Additionally, we study changes in market structure around the reform. To that end, we estimate a version of Equation (3) at the state× year level:

$$Competition_{s,t} = \alpha + \beta \cdot Treated_s \times Post_t + \lambda_s + \lambda_t, \tag{4}$$

where $Competition_{s,t}$ represents competition between auditors in state s at time t. To that end, for each state× year, we compute the ratio between the number of auditors (numerator) and the number of clients (denominator). Higher ratio, driven by more auditors per client, means that auditors must compete harder for a smaller pool of potential clients. The main explanatory variable represents the state's overall exposure to the reform. We compute the number of treated auditors, who had at least one client below the $750,000 in 2015, divided by total number of auditors in the state in 2015. Higher ratio means that the state had greater exposure to the deregulation of federal grants. We add year and state fixed effects and summarize the results in Table 4, Panel B. Overall, the reform increased the number of auditors per clients. Our estimates show that a one-standard-deviation higher exposure is associated with 0.014 increase in state-level competition after the reform. This equals to 6% of the unconditional pre-period mean competition.

Finally, we turn to the full sample (before and after the reform) and estimate the following OLS specification:

$$Survive_{p,t+1} = NegativeAudit_{p,t} + LogAmount_{p,t} + \lambda, \tag{5}$$

where $NegativeAudit_{p,t} = 1$ if the auditor issued a negative opinion to the client at time t, and $Survive_{p,t+1} = 1$ if the auditor retained the client at time $t+1$. As before, outcomes are multiplied by 100 for clarity, and we add a flexible set of fixed effects. The results are reported in Table 5. Across specifications, we find that negative opinion is inversely related to survival. Looking at column (1), the univariate correlation between survival and a negative opinion is significantly negative. We gradually add fixed effects for the entity's type and state, auditor fixed effects, and relationship age fixed effects. In the tightest specification, we find that a negative opinion is associated with 3.8 percentage point decline in survival rate, which is 5.7% of the sample mean.[27] The coefficients remain stable and are all statistically significant. In the last two columns (columns 7-8) we decompose the outcome into two scenarios. In one version, the outcome equals 1 if the client exits the sample altogether, that is, the client has lost access to large federal funds, and 0 otherwise. In another version we focus on clients who stay in the sample, that is, maintain access to large federal funds, and define an outcome variable which equals 1 if the client matched to a different auditor. We find that the probability of exit increases by 17.1% relative to the mean, and the probability of switching auditors – conditional on non-exit – increases by 11.2% relative to the mean. Thus, a negative audit has a substantial negative impact on access to federal funds, and a smaller impact on the likelihood of choosing a new auditor.

Overall, the evidence suggests that issuing a negative audit is a risky choice for the auditor. Their clients are more likely to be denied federal grants, and even if they continue to receive grants, they are more likely to choose a different auditor. This provides an indirect support to the leniency channel, whereby treated auditors have a greater incentive to issue positive audits to avoid losing any more clients.

2.4.2 Client compliance

The second channel we consider is centered on the client. According to this channel, clients who hire treated auditors – those who lost their deregulated clients – are less prone to misconduct. This could happen in several scenarios, which we consider below.

First, the reform could have triggered changes in client composition. Imagine that deregulated clients – who were below the $750,000 threshold before the reform – had a greater tendency to engage in misconduct. Treated auditors lost those clients and were left with clients who are less prone to misconduct. Consequently, we would see lower incidence of

[27] Recall that the dataset starts in 1997 so the age is left-censored.

negative audits among treated auditors. A more nuanced possibility is that low-misconduct clients crossed the threshold at the exact time the reform was implemented. Imagine that in 2015 a group of clients is just below the threshold, and they consider asking for a larger grant in 2016 that would put them above the threshold. Because of the reform, only low-misconduct clients will choose this path, while high-misconduct clients will stay below the threshold to avoid the Single Audit.

To examine this possibility, we shift to the sample of clients in 2004-2012, *before* the pre-treatment period which starts in 2013. We estimate the following regression:

$$NegativeAudit_{c,t} = \beta \cdot \ Above750K + \lambda, \tag{6}$$

where $NegativeAudit_{c,t} = 1$ if client c at time t received a negative audit, and $Above750K$ is equal to one if the client is above the $750,000 threshold. Importantly, since the sample here is limited to 2004-2012, this threshold is merely a placebo: it had no legal implications at that time. λ is a flexible set of controls and fixed effects, to remove other determinants of misconduct. The focus is on β, which summarizes the differences in audit results between clients above and below the threshold. The results are summarized in Table 6, Panel A. We gradually add fixed effects for year, state, type, auditor, and duration of relationship with the current auditor. Overall, the differences between clients above and below the threshold are statistically insignificant and effectively zero. This suggests that clients above the threshold are not fundamentally different from those below it.

From another perspective, we estimate our difference-in-differences specification (Equation (1)) in three samples. The results are summarized in Table 6, Panel B. The first column uses the baseline sample: all clients for all treated and control auditors. The coefficient here equals the coefficient from the main table (Table 2), column (2), with year and pair fixed effects. The second column uses only clients that existed both before and after the reform. This restriction removes clients that no longer participate in the Single Audit process, either because they lost all funding or because they stay below the new threshold. This removes 8,500 observations, nearly 8% of the baseline sample, but we estimate a nearly identical effect. Finally, the third column uses only clients that were above the $750,000 threshold at all times, even before the reform took place. This removes additional 34,800 observations. While the point estimates is smaller, it is highly statistically significant and the economic magnitude is nearly identical to the baseline specification. The last two results suggest that, even if clients below the threshold were generally less compliant (which we do not think is the case), this feature had little or no contribution to the effect we document: our results are nearly identical for clients that were above the threshold before the reform (when it was not binding), and continued to be above it after the reform. For those clients, the only difference is that their auditor was exposed to deregulation via another client who no longer exists.

Finally, even if client composition is unlikely to explain the results, perhaps client incentives shifted after the reform. If there are fewer Single Audits, then recipients may believe that federal regulators will take a closer look at some of those audits, or punish misconduct more severely. Either way, the reform may have deterred clients from engaging in misconduct. We believe that our setting renders this explanation unlikely. The effects we document stem from a difference-in-difference setting. Therefore, even if there were any broad changes after the reform, it is difficult to see why those would take place only among the treated group. Moreover, to our knowledge, there were no systematic changes in how Single Audits are verified and how failed audits are handled.

3 Structural model

In Section 2, we document the contagious effect of deregulation: significant decline in negative audits among regulated entities. We explore two mechanisms, auditor leniency and client compliance. Overall, the evidence is more consistent with the first mechanism. In this section, we develop a structural model that accounts for both mechanisms and investigates their quantitative implications.

3.1 Model setup

We start by presenting a parsimonious model of the auditor's decision to issue a negative opinion. Time is discrete and the time horizon is infinite. A risk-neutral auditor, who discounts cash flows using discount rate $\beta = 0.95$, is engaged by the client to audit a project which was funded by a federal grant. Each period, the auditor collects fees proportional to the project size p_t, which we measure in inflation-adjusted 2023 USD:

$$\pi(p_t) = \phi \cdot p_t$$

The auditor decides whether to issue a positive (unqualified) or negative (qualified or adverse) opinion about the client's use of federal funds. The decision is denoted by d_t, which equals one if the the audit is negative and zero if positive.

Issuing a negative opinion involves a recurring cost, captured by the structural parameter $\theta > 0$. It is a fixed cost, paid each time a negative opinion is issued, irrespective of past audit outcomes or project size.[28] It reflects the required effort and the risk of revenue loss: upon receiving a negative audit, the client may seek a different auditor or lose federal funding, either way terminating the relationship with the auditor. While cannot observe effort, we document the substantial revenue risk in Section 2.4.1 and Table 5.

Asides from the recurring cost, the auditor pays a one-time cost when switching from a positive opinion to a negative opinion. This switching cost is captured by the structural parameter $\eta > 0$. It reflects the additional effort required to install more rigorous auditing practices and the additional risk of losing the client (the net present value of expected future losses). Intuitively, if the auditor already issued a negative opinion in the previous period, then the rigorous practices have already been adopted, and by revealed preference we learn

[28] The results are similar in a model where the recurring cost increases with project size (see Table 7 versus Table A.1).

that the risk of client termination is smaller. It could also reflect the auditor's discomfort from working with a client who switches from compliance to non-compliance. The switching cost is consistent with the patterns we observe in the data. If the auditor issued a positive opinion to the client at time t, the probability of a negative opinion at time $t+1$ is less than 4%. Conversely, if audit outcome at time t was negative, there is more than 50% chance of a negative audit at time $t+1$.[29] This persistence suggests that transitioning from positive to negative triggers a switching cost, on top of the recurring cost.[30]

The third ingredient is a scale discount: the cost of a negative audit decreases with auditor size. The scale discount is represented as $\delta \cdot a_t$, where δ is the structural parameter and a_t is auditor size, measured as the total dollar value of all audited projects.[31] If $\delta < 0$, it implies that larger auditors face lower switching costs. They enjoy economies of scale and can easily switch from lenient to rigorous audit. They are also less concerned about losing a particular client, given that they have many other clients. The inclusion of a scale discount is motivated by the fact that large auditors are more likely to issue a negative audit. We show it in a linear probability model, where the outcome equals one if the auditor had at least one negative audit. The dependent variable is the number of clients or the dollar value of clients in million USD. We include a flexible set of year and auditor fixed effects and report the results in Table A.2. They confirm that large auditors are significantly more likely to issue a negative audit to their clients, which we represent in the model with the scale discount δ.

In sum, we model the auditor incentives as:

$$c(a_t, d_t, d_{t-1}) \quad = \quad \underbrace{\theta \cdot d_t}_{\text{recurring cost}} \quad + \quad \underbrace{\eta \cdot (1 - d_{t-1}) \cdot d_t}_{\text{switching cost}} \quad + \quad \underbrace{\delta \cdot a_t \cdot (1 - d_{t-1}) \cdot d_t}_{\text{scale discount}}.$$

The state variables are auditor size (a_t) and today's audit outcome which was determined

[29] In a formal regression we estimate a substantial autocorrelation of 43%-63% between audit outcomes, depending on which fixed effects are included. See Table A.2, Panel A.

[30] We considered a model with symmetric switching cost, from negative to positive, and the estimates for the key structural parameters are similar.

[31] We obtain similar results when using the number of audited clients.

yesterday (d_{t-1}). There are three cases to consider. If the auditor issues a positive opinion ($d_t = 0$), there is no utility cost. If the auditor issued a negative opinion today ($d_{t-1} = 1$), and will continue to issue a negative opinion tomorrow ($d_t = 1$), only the recurring cost plays a role. If the auditor issued a positive opinion today ($d_{t-1} = 0$) and will issue a negative opinion tomorrow ($d_t = 1$), he must pay the recurring cost ($\theta > 0$) plus a switching cost ($\eta > 0$). Finally, the scale discount ($\delta < 0$) may offset some of the switching costs.[32]

3.2 The auditor's problem

The auditor chooses audit outcome to maximize the intertemporal utility. First, note that the auditor's one-period utility function is:

$$u_t(x_t, d_t) = \pi(p_t) - c(a_t, d_t, d_{t-1}),$$

that is, fees collected minus cost. Here, $x_t \equiv \{p_t, a_t, d_{t-1}\}$ is the vector of state variables observable to the auditor and the econometrician: client size, auditor's portfolio size, and audit outcome from the previous period.

The auditor's intertemporal utility also includes a state variable, $\varepsilon_t(d_t)$. It is observed by the auditor but not by the econometrician, and its value depends on the auditor's opinion. Such specification is a standard feature of dynamic discrete choice models (Rust, 1994). In particular, without the random component, the auditor's decision would be fully characterized by the observable state variables, which is clearly unrealistic. To illustrate the workings of ε, imagine that the client was non-compliant and engaged in misconduct. In this case, the auditor should issue a negative opinion. If he indeed chooses to issue a negative opinion,

[32] The scale discount applies exclusively to the switching cost and not to the recurring cost due to the inherent nature of these expenses. Larger auditors benefit from economies of scale, which enable them to distribute the additional costs associated with switching across a broader client base, resulting in a lower marginal cost. They also possess more sophisticated systems and dedicated resources that facilitate smoother transitions. Finally, the risk of client termination is less significant for large auditors since the loss of any single client has a minimal impact on their overall revenue. In contrast, the recurring cost θ represents the fixed effort and inherent risk associated with issuing a negative opinion. It is the baseline expense essential to maintaining audit quality and integrity.

he gets an extra utility ($\varepsilon_t(1) > 0$). However, if he chooses to issue a positive opinion, he receives a disutility ($\varepsilon_t(0) < 0$). This could be explained as a reputational cost or a risk of being caught by a government watchdog. More generally, one could think of $\varepsilon(d_t)$ as a private information shock, unobserved by the econometrician.

To summarize, the auditor's total utility can be expressed as

$$u_t(x_t, d_t) + \varepsilon_t(d_t) = \begin{cases} \phi p_t - \theta - (\eta + \delta a_t)(1 - d_{t-1}) + \varepsilon_t(1) & \text{if } d_t = 1, \\ \phi p_t + \varepsilon_t(0) & \text{if } d_t = 0. \end{cases}$$

3.3 Model solution

Every period, the auditor decides which opinion to issue, d_t, by maximizing the expected present value of future utility. The Bellman equation for the problem is:

$$V_t(x_t, \varepsilon_t) = \max_{d_t} \left\{ u_t(x_t, d_t) + \varepsilon_t(d_t) + \beta \mathbb{E}\Big[V_{t+1}(x_{t+1}, \varepsilon_{t+1} \mid x_t, \varepsilon_t, d_t) \Big] \right\},$$

where $V_t(x_t, \varepsilon_t)$ is the expected discounted utility of the auditor in state (x_t, ε_t). Note that the problem cannot be solved as-is, given that ε_t is unobservable. Therefore, as in Arcidiacono and Ellickson (2011) or He and Whited (2023), we can integrate out ε to obtain the value function in state x_t before ε_t is revealed:

$$\overline{V}_t(x_t) \equiv \int V_t(x_t, \varepsilon_t) g(\varepsilon_t) d\varepsilon_t.$$

We can now define the conditional value function as the present value of choosing d_t and behaving optimally starting from period $t + 1$:

$$v_t(x_t, d_t) \equiv u_t(x_t, d_t) + \beta \int \overline{V}_{t+1}(x_{t+1}) f(x_{t+1} | x_t, d_t) dx_{t+1}.$$

In this case, the decision rule $\delta_t(x_t, \varepsilon_t)$ solves:

$$\delta_t(x_t, \varepsilon_t) = \arg\max_{d_t} \left[v_t(x_t, d_t) + \varepsilon_t(d_t) \right]. \tag{7}$$

In words, the auditor chooses the audit outcome which yields the highest utility by following the decision rule in Equation (7). The probability that the auditor chooses d_t given state x_t, $\mathbb{P}_t(d_t \mid x_t)$, can be found by integrating the decision rule over the regions of $\varepsilon_t(d_t)$ for which $\delta_t(x_t, \varepsilon_t) = d_t$:

$$\mathbb{P}_t(d_t \mid x_t) = \int \mathbb{I}\{\delta_t(x_t, \varepsilon_t) = d_t\} g(\varepsilon_t) d\varepsilon_t = \int \mathbb{I} \left\{ \arg\max_{d_t} \left[v_t(x_t, d_t) + \varepsilon_t(d_t) \right] = d_t \right\} g(\varepsilon_t) d\varepsilon_t.$$

As common in the literature on discrete dynamic choice models, we assume that ε is independent and identically distributed extreme value type I random variable with constant volatility σ. In this case, the conditional probability function admits a closed-form solution:

$$\mathbb{P}_t(d_t \mid x_t) = \frac{\exp[v_t(x_t, d_t)]}{\exp[v_t(x_t, 0)] + \exp[v_t(x_t, 1)]}, \tag{8}$$

which closely corresponds to logit choice probabilities.

3.4 Model estimation

To estimate our model, we proceed in two steps. First, from the data, we estimate the transition matrix of auditor size a_t. We measure auditor size as the logarithm of the real sum of all amounts audited and winsorize variables at the 1% and 99% percentiles. We estimate the transition matrix of auditor size using a kernel density estimator with 25 evenly-spaced points of support. Second, we estimate the remaining parameters of the model, $\{\sigma, \phi, \theta, \eta, \delta\}$ which determine the auditor's preferences. Those represent the volatility of the unobserved utility (σ), auditor fees (ϕ), recurring cost (θ), switching cost (η), and scale discount (δ). We set the discount rate β to 0.95.

With respect to the structural parameters, note that the audit decision d_t is based on comparing the utility of a positive audit to the utility of a negative audit. This comparison is not affected by rescaling utilities or adding a constant. In our case, this means that we can simplify the estimation: we normalize profits to zero, that is, we set $\phi = 0$. Moreover, we scale the structural parameters by the standard deviation of the private information shock σ.[33] These steps allow us to focus on the objects that are of greater economic interest. It also implies that the parameter estimates capture their importance relative to the dispersion of the unobserved state variable, rather than their objective value. We will return to that point in the next section, when we discuss the quantitative results.

We estimate the remaining parameters $\{\theta, \eta, \delta\}$ using the the nested fixed point (NFXP) method of Rust (1987, 1994) as in Abbring and Klein (2022). That is, we assume that the transition matrices and the discount factor β are given, and use maximum partial likelihood estimation to obtain the estimates of $\{\theta, \eta, \delta\}$. In other words, we solve the decision problem in Equation (7) and use its solution to obtain the conditional choice probabilities in Equation (8). These probabilities are then used to create the likelihood function for estimating the preference parameters.

4 Quantitative implications

4.1 Auditor's incentives

Table 7, Panel A, presents the results of our estimation using the full sample (1999-2022). Several facts emerge. First, the cost parameter θ is positive and statistically significant. We interpret this parameter as a recurring cost from a negative audit, reflecting both effort and the risk of losing the client's business. Second, the cost of switching from a positive to a negative opinion (η) is also positive and significant. In addition to the recurring cost, this

[33] As explained in He and Whited (2023), scaling the parameters by the standard deviation of the private information shock is equivalent to scaling the utility.

switching cost represents the extra effort required to transition to rigorous audits and the heightened risk of losing a client. Third, cost decreases with auditor size (δ), a factor we refer to as a scale discount. Intuitively, larger auditors have a large client base. Therefore, issuing a negative opinion may create less negative externalities for their business. Note that the magnitudes, in themselves, do not have an immediate interpretation. This is because our model accounts for an unobserved information shock, ε, and we estimate the structural parameters relative to that shock. However, it will be useful to compare estimates across different subsamples, a task we will turn to in the next sections.

In Panel B, we analyze the goodness-of-fit of our model. To that end, we compare the data-implied and model-implied probability of issuing a negative opinion in various scenarios.[34] We consider combinations of the two state variables: previous period's opinion and auditor size. For example, in the row "positive opinion and small auditor," we report the incidence of negative audits among small auditors (below median) who issued a positive opinion in the previous period. We find that the model closely matches the outcomes we observe in the data. In particular, the model reproduces the fact that switching costs η are a key force influencing the decision to issue a negative opinion. If the auditor issues a positive opinion today, the probability of a negative opinion tomorrow is 2.6% in the data and in the model. In the data, this probability varies by auditor size, and the model is also able to generate this distinction: large auditors are more likely to switch to a negative opinion than small auditors. In the model, this gap is due to the scale discount δ, whereby a negative audit is less costly for large auditors. Conversely, if the auditor issued a negative opinion today, he is significantly more likely to continue issuing a negative opinion tomorrow: 64.2% in the data and 63.8% in the model. Note that this is not a mechanical result, rather one

[34] We calculate the model-implied probabilities by simulating the data: for each firm, we start from the initial observable states and simulate a history corresponding to this firm's observed sample period. As suggested by Michaelides and Ng (2000), we simulate 10 such panels and the the average across the simulations to mitigate problems related to simulation bias. Note that the data-implied unconditional mean in Table 7 is lower than the one reported in Table 1, since we impose two further restrictions: (1) all variables used in the estimation have non-missing values, and (2) there are at least 15 entities in each entity×state to calculate auditor market concentration. This reduces the current sample to 302,913 observations.

that arises endogenously: while the recurring costs of a negative audit (θ) are substantial, the switching costs (η) are large enough to cause negative audits to persist.

4.2 Optimal audit policy

Having estimated the model, we now turn to understand its implications. As explained in Section 4.1, the magnitudes of the structural parameters from Table 7 do not have an immediate interpretation. However, we can still assess their relative importance by plotting the optimal policies of the auditor. We use the estimated parameters from Table 7 and present the results in Figure 3: the conditional probability of a negative audit outcome in the y-axis, as a function of the state variables $x_t = \{a_t, d_{t-1}\}$.

The graphs reveal several notable findings. First, audit outcomes are persistent over time. In particular, for any given level of auditor size (a_t), the probability of a negative audit today is higher if yesterday's audit was negative than if it was positive. This persistence in audit outcomes results from the substantial switching cost, η, and it is consistent with the patterns we observe in the data. In the graph, we can see that for any given a_t, the level of negative audit is significantly lower in the top panel (where $d_{t-1} = 0$) than in the bottom panel (where $d_{t-1} = 1$).

Second, large auditors are more likely to switch from positive to negative opinion. This heterogeneity across auditors results from the large scale discount δ in our model, which increases with auditor size a_t. This means that large auditors face lower switching costs, and are therefore more likely to change course and issue a negative opinion, relative to small auditors. In the graph, this is captured by the positive slope we observe in the top panel.

Third, large auditors are also more likely to switch from negative to positive opinion. This flexibility stems from the relative magnitudes of the scale discount δ, the switching cost η, and the recurring cost θ. Imagine an auditor who issued a negative opinion today ($d_{t-1} = 1$), and contemplates issuing a positive opinion tomorrow ($d_t = 0$). On one hand, the auditor will no longer need to pay the recurring cost θ. On the other hand, if in the future the auditor will switch back to negative ($d_{t+n} = 1$), he would have to pay a one-time switching cost η minus the scale discount $\delta \cdot a_t$. Since the discount increases with auditor size, the net future cost decreases with auditor size. Therefore, on balance, large auditors are more likely to switch to positive today ($d_t = 0$), as the advantage (saving θ) offsets the risk (paying $\eta - \delta \cdot a_t$). In the graph, this is captured by the negative slope we observe in the bottom panel. Note that the slope here is is less steep than the top panel, since the benefit of δ is uncertain and would only arrive in unknown future period.

4.3 Deregulation

In this section, we turn to the quantitative implications of deregulation. We combine the structural model estimates with the reduced-form estimates from Section 2.

To guide the analysis, we start by studying how changes in each structural parameter $\{\theta, \eta, \delta\}$ affect the likelihood of a negative audit. We do so by calculating a discrete set of counterfactual experiments: starting from the parameter's baseline value,[35] we vary only the respective parameter while keeping all other parameters constant. The results are in the three panels of Figure 4. The x-axis represents the percentage change in the respective structural parameter, relative to its baseline value, and the y-axis represents the resulting negative audit probability.[36] The graphs highlight two key insights. First, negative audits decrease with the recurring cost (θ) and with the switching cost (η). Naturally, higher costs motivate auditors to show leniency toward their clients. Interestingly, the slope of θ is substantially flatter than η, suggesting that they have different quantitative impact on audits. We return to this point in Section 4.4 when estimating the elasticities of both parameters to audits. Second, negative audits increase with the scale discount (δ). Intuitively, a large discount can partially offset the costs of a negative audit. Consequently, the incentive to issue a negative audit improves.

[35] For simplicity, we use the pre-reform estimates (discussed below) as baseline values.

[36] The coordinate (0%, 7.06%) corresponds to the baseline scenario, where the parameter is at its baseline level and the negative audit probability is 7.06%.

With this in mind, we return to the impact of deregulation on negative audits. Recall that we estimate a substantial treatment effect (LATE), around 42% decline in negative audits. Given the results in Figure 4, such a large treatment effect could result from a correspondingly large shift in one or more of the structural parameters. To explore this, we focus on the narrower sample of treated and control auditors between 2013-2018. We split the sample into the pre-treatment period (2013-2015) and the post-treatment period (2016-2018) and re-estimate the model on each subsample. Recall that the parameters are identified only up to a scale factor, which is inversely related to the private information shock σ (Section 3.4). Therefore, when comparing estimates across subsamples, we need to take into account the potential differences in scale. We do so following the procedure for estimating scale factor ratios for pairs of data sets developed by Swait and Louviere (1993), similar to He and Whited (2023). In short, we verify that the scales of two data sets are equal (first stage). If this hypothesis cannot be rejected, we test whether parameters differ after accounting for differences in scale (second stage).

The results are in Panel A of Table 8, showing how all structural parameters changed after the reform: the recurring cost θ more than doubled $((1.017-0.37)/0.37)$, the switching cost η declined by 35% $((4.929-7.635)/7.635)$, and the scale discount δ decreased in absolute value by 63% $((-0.038+0.199)/-0.199)$. Moreover, we find that the model-implied likelihood of negative audit was 7.06% before the reform and 3.25% after the reform. This suggest that the model-implied treatment effect (54%) has an order of magnitude similar to data-implied treatment effect (42%), further confirming the model's success in describing auditor incentives. These results shed more light on the contagious effect of deregulation, by showing that deregulation reduces the incentives to issue a negative audit for the remaining regulated entities. Deregulation increases the overall costs of negative audit and shifts the composition of the cost function: all auditors experience rising recurring costs, and the switching costs rise for large auditors, who no longer enjoy a hefty scale discount. Since auditors now face higher costs for issuing a negative audit, the intended deregulation of small grant recipients led to an unintended deregulation of the remaining grant recipients.[37]

Our model does not take a stand on why the cost parameters have shifted. However, a plausible explanation is that deregulation increased the competition among auditors for the shrinking pool of clients. The competition increased the risk of losing clients and has also limited the scale discount, leading to higher costs of issuing a negative audit. This explanation is consistent with the findings in Section 2.4.1, whereby treated auditors have lost clients and struggled to recruit new ones after the reform was implemented. To further substantiate this explanation, we study how the structural parameters vary by the competitiveness of the auditor markets. We proxy for competition by dividing the number of auditors by the number of clients in each state$\times$year. A higher ratio implies that auditors must compete more intensely for clients, while a lower ratio implies that clients have fewer options, resulting in limited competition among auditors. We split the sample based on the median ratio and present the results in Panel B of Table 8. The results indicate that auditors operating in competitive markets have a higher recurring cost θ, a slightly lower fixed switching cost η, and a substantially weaker scale discount δ. These differences move in the same direction as the changes we report in Panel A, following the deregulation reform. Thus, it provides suggestive evidence that the reform increased auditing costs by increasing the competition for the remaining clients.

4.4 Counterfactual policies

As we explain in Section 4.1, the magnitudes of the structural parameters do not have an immediate interpretation. However, we can still assess their importance via a set of counterfactual experiments. We report by how much will the incidence of negative audits change in each experiment, and compare the effects across different experiments. The results are presented in Table 9.

[37] To be clear, part of the effect may be driven by changes in client misconduct, which is included in the unobserved private information shock ε. However, as shown in Section 2.4, we believe this explanation is inconsistent with the institutional setting and subsequent tests.

In the first set of experiments, we use the full sample (1999-2022) to calculate the elasticity of audits to the structural parameters: by how much would negative audits decline, if θ and η increase by 1% or if δ decreases by 1%. Recall that θ and η increase costs while δ decreases costs. Therefore, we design their counterfactual scenarios in opposite directions. The results are presented in Panel A. We find that a 1% increase in θ results in 0.7% decline in the probability of a negative audit, compared to the baseline model-implied probability of 8.2%. The elasticity of audits to η is substantially higher and equals 2.3%. Thus, our estimates show that switching costs have a larger quantitative impact on the auditor's decision, compared to recurring costs. Finally, the elasticity of audits to δ is 0.9%. This suggests that a large portfolio can offset the switching cost, and motivate auditors to monitor their clients more closely if circumstances call for it.

In the second set of experiments, we shut down each parameter while keeping the other two unchanged. The results are presented in Panel B. We find that with zero recurring cost ($\theta = 0$), the probability of a negative opinion doubles from 8.2% to 19.1%. This reflects the fact that, in our model, a negative opinion is costly for the auditor irrespective of their decision in the previous period. When we turn off η, it means that an auditor who issued a positive opinion faces zero adjustment cost no disincentive to switch to a negative opinion. This results in a large quantitative effect, increasing the likelihood of a negative audit to 40.1%. Finally, when δ is set to 0, auditor size does not influence the decision to issue a negative opinion. In this case, the conditional probability of issuing a negative opinion is roughly halved.

Overall, the results indicate that auditor costs are not only statistically significant but also quantitatively important. To further demonstrate this point we compute the economic magnitude of auditor leniency, driven by the changes in the structural parameters. We calculate the difference between the baseline negative audit probability (8.2%) and its counterfactual value in each scenario, and multiply it by the average annual size of the federal grants market (\$192.6bn). Intuitively, the resulting number reflects the expected dollar value of grants that will be audited differently, solely due to changes in auditor incentives. Concretely, if the counterfactual scenario involves higher (lower) auditor costs, then the number we report reflects the expected value of funds enjoying more (less) auditor leniency. For instance, a 1% increase in recurring cost θ will result in substantial leniency valued at $11m USD.

More concretely, we consider a set of contagion-free deregulation reforms. Those are reforms which aim to deregulate one segment of the market without changing compliance in other segments of the market. In other words, we seek to keep the negative audit rate at the pre-reform level. To that end, the deregulation reform must be coupled with steps that directly target one or more of the structural parameters. For example, reducing θ by launching a government-sponsored insurance program to compensate any auditor who lost clients following a negative audit. For simplicity, we let the scale factor δ reach its post-treatment level (-0.065), and compute combinations of the recurring cost θ and the switching cost η which preserve the pre-treatment negative audit rate of 7.06%. The results in Figure 5, Panel A, highlight three insights. First, to prevent contagion, we must trade off recurring and switching costs. In other words, since both θ and η reduce negative audit, they must move in opposite directions to preserve negative audit rates at pre-treatment levels. Second, the actual post-reform costs – denoted with a blue star – are above the curve of the counterfactual policies we propose. This explains why the deregulation reform had contagious effects: the combined auditing costs were "too high," that is, they incentivized auditors to offer leniency to their remaining clients. Third, the actual pre-reform costs – denoted with a red square – are also above the curve of the counterfactual policies we propose. In theory, this means that we should see fewer negative audits before the reform. Nevertheless, the pre-reform negative audits rate were higher due to the substantially larger scale factor δ, which helped push costs down and thus motivate auditors to issue negative audits more often.

For completion, we propose a set of deregulation reforms which yield similar contagion effect but via a different shift in the cost function. First, we use the estimated post-treatment value of the recurring cost θ (1.017) and compute possible combinations of (η, δ). Those are represented by the black dots. Note that we must achieve similar net switching costs. Therefore, if we look for higher η, the scale discount δ must also be higher; otherwise, the net switching costs will be too high and the contagion will become more severe. Conversely, we hold the recurring cost θ fixed at the pre-treatment level (0.369) and compute possible combinations of (η, δ). Those are represented by the red hollow squares. Note that the actual post-reform costs – denoted with a blue star – are below the curve of the counterfactual policies we propose here. Since we now hold the recurring cost θ at the pre-treatment level, the switching cost must be substantially higher.

5 Conclusions

Each year, the federal government distributes $900 billion in grants to nonprofit organizations. Organizations spending above a specified threshold must undergo a thorough review by a private auditor, a process known as Single Audit. We investigate the impact of a 2015 deregulatory reform, that raised the threshold for Single Audits. By raising the threshold, the reform exempted low-spending organizations from auditing. This reduced the demand for auditors, who then faced increased competition, leading to unintended consequences. Specifically, we find that auditors became more lenient with higher-spending clients, resulting in weaker monitoring of federal grants. Leveraging a unique grant-level dataset spanning 1997-2022, we find that treated auditors – those who lost low-spending clients – were significantly less likely to issue negative or adverse audit opinions post-reform, suggesting that auditors softened their stance to retain remaining clients.

Using a structural model, we identify key cost factors that discourage auditors from issuing negative opinions, especially when client demand declines. Findings indicate that deregulation effectively doubled the recurring cost of negative audits, prompting auditors to avoid issuing negative reports, even when clients potentially mismanaged grants. We then propose alternative policies, such as subsidies covering auditors' costs, to mitigate these contagion effects without weakening oversight on remaining federal grants.

Our paper sheds new light on the the economic impact of deregulation, by showing that even partial deregulation can affect market participants who are not directly deregulated. Our study also offers potential policies to avoid such unintended effects, including nationalizing the Single Audit process or implementing targeted subsidies, which can improve audit rigor and protect taxpayer interests in the federally-funded nonprofit sectors.

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Figure 1: **Market for federal grants**

The first figure shows the total value of federal grants in our sample, adjusted for inflation (2023 USD). The second figure shows the number of grant recipients, and the third figure shows the number of auditors. See Section 1.3.

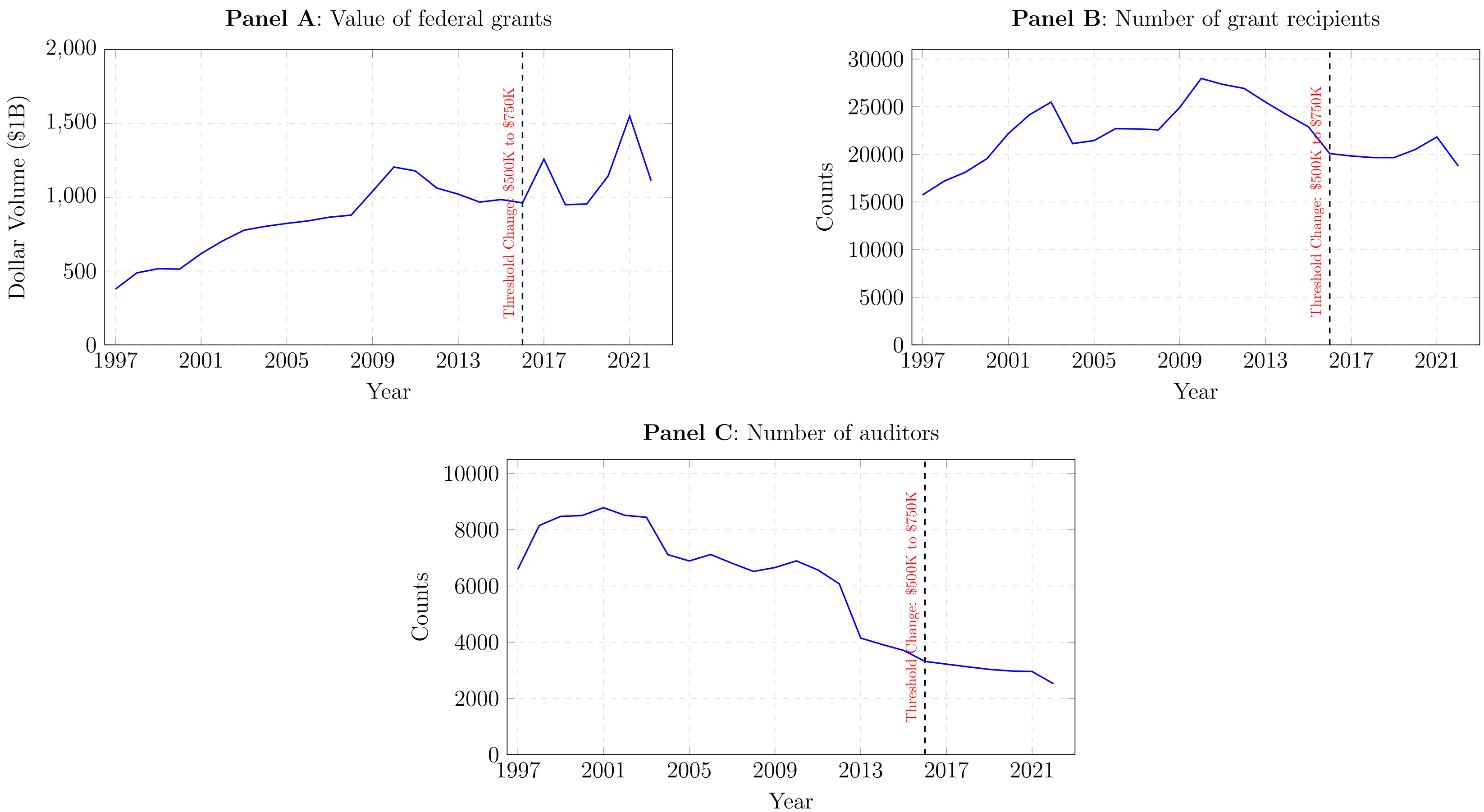


Figure 2: **Dynamic coefficients for difference-in-differences**

Results from estimating Equation (2). Each coefficient represents the estimated gap between the treated and control groups, before and after the reform, and the corresponding 95% confidence intervals. 2015 is the baseline year. See Section 2.3.

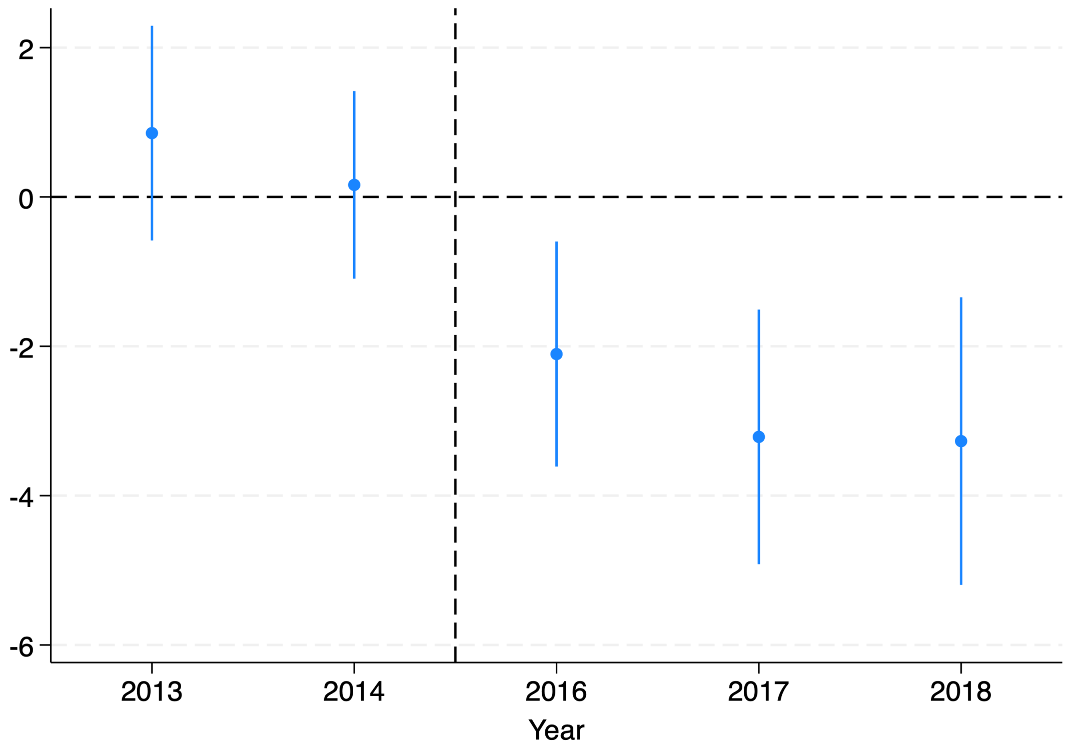


Figure 3: **Policy functions**

The figures show the model-implied probability of issuing a negative opinion tomorrow (d_t), as a function of two state variables: the audit outcome today (d_{t-1}) and the auditor size based on dollar value of clients (a_t). The parameter values used to create the policy functions are from Table 7. Panel A corresponds to the auditor issuing a positive opinion today while Panel B to the auditor issuing a negative opinion today. See Section 4.2.

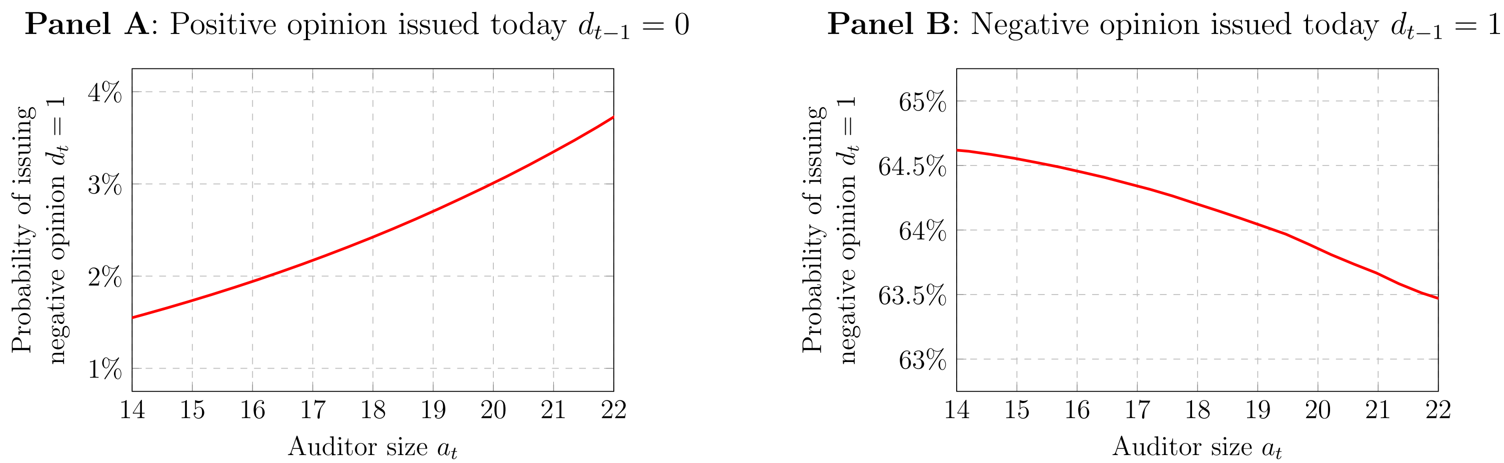


Figure 4: **Comparative statistics**

The figure shows how the probability of a negative audit responds to three structural parameters: the recurring cost θ (Panel A), the switching cost η (Panel B), and the scale discount δ (Panel C). For each parameter, we compute the negative audit probability when the parameter increases. Each curve is calculated based on a discrete set of counterfactual experiments, starting from the baseline values of structural parameters and varying only the respective parameter, while keeping all other parameters constant. See Section 4.3.

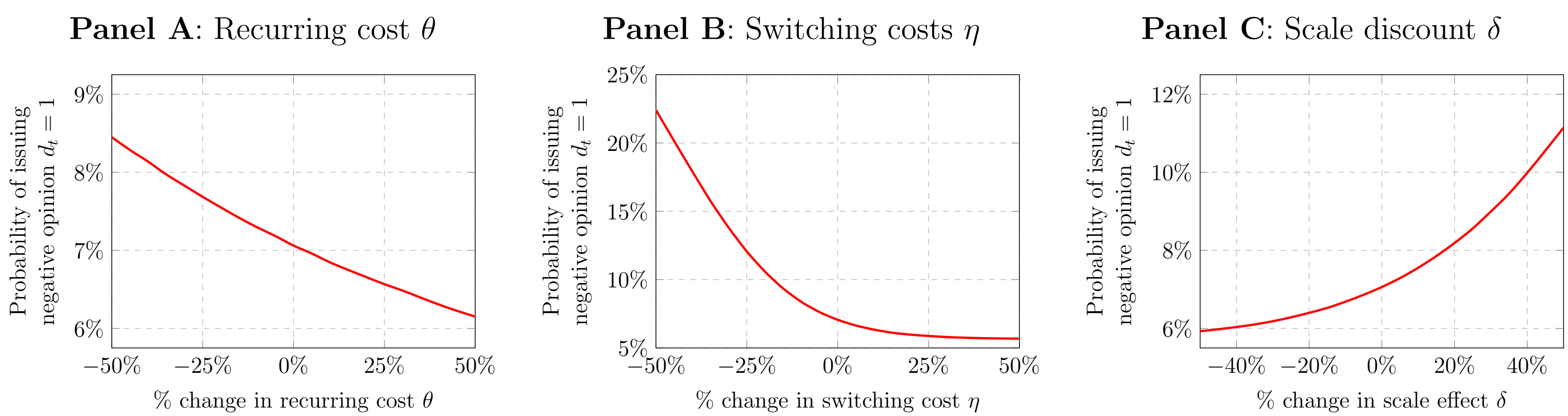
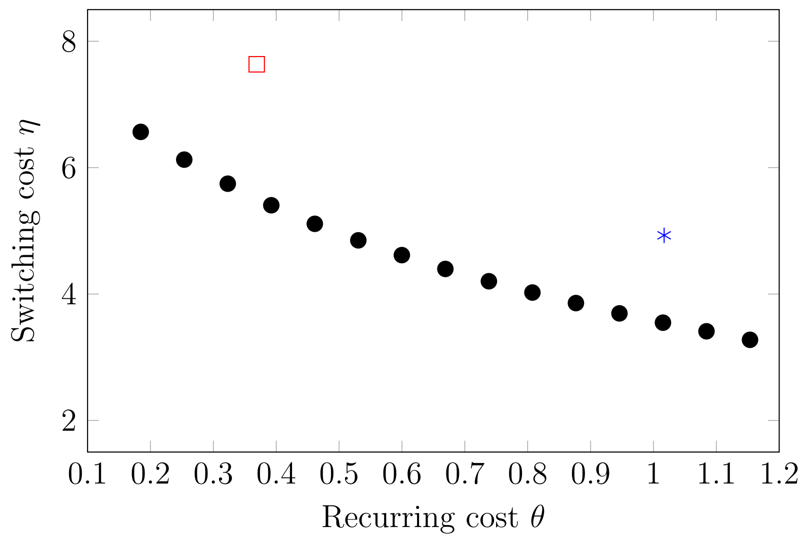


Figure 5: **Alternative deregulation policies**

Panel A. Preventing the contagion effect. The figure presents a range of deregulation reforms which avoid the contagion effect we estimate in Section 2. We hold the scale discount δ fixed at the post-treatment level, and compute combinations of the recurring cost θ and the switching cost η which preserve the pre-treatment negative audit rate of 7.06%. The red square indicates the actual parameter estimates from the *pre*-treatment sample (see Table 8, Panel A): (θ, η)=(0.369,7.635). The blue asterisk indicates the actual parameter estimates from the *post*-treatment sample: $(\theta, \eta) = (1.017, 4.929)$. See Section 4.4.



Panel B. Preserving the contagion effect. The figure presents a range of deregulation reforms which replicate the contagion effect we estimate in Section 2. We hold the recurring cost θ fixed, and compute combinations of the switching cost η and the scale discount δ which achieve the post-treatment 3.25% negative audit rate. The value of θ is X for the black dots (the post-treatment estimate) and X for the red squares (the pre-treatment estimate). The blue asterisk indicates the actual parameter estimates from the post-treatment sample (see Table 8, Panel A): $(\eta, \delta) = (4.929, -0.0651)$. See Section 4.4.

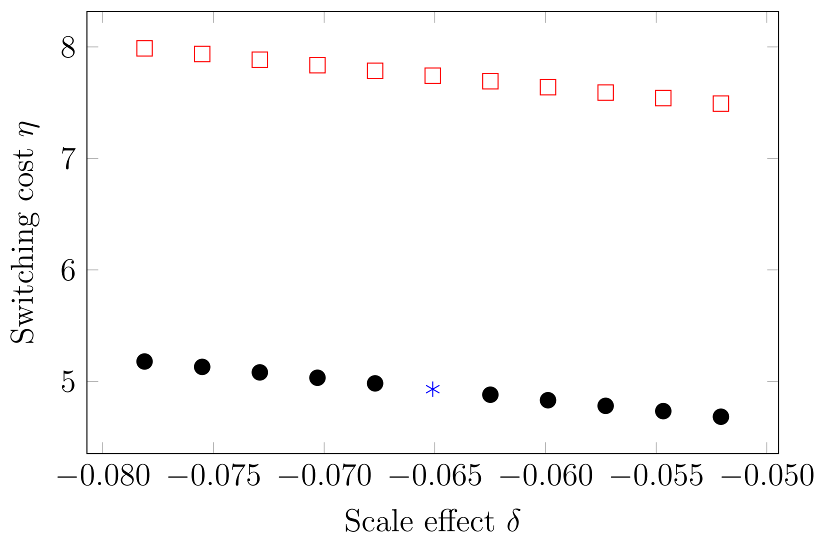


Table 1: **Summary statistics**

Panel A. Clients. Summary statistics at the client level, 1997-2022. *Amount ($1,000)* (*Amount (2023 USD; $1,000)*) is the amount of federal grants spent by the client in thousand USD (adjusted for inflation). *Pair Survival Rate (%)* is the likelihood a client continues to hire the same auditor in the next year. *Auditor Attrition Rate (%)* (*Client Attrition Rate (%)*) is the likelihood the auditor (client) ceases to exist in the sample next year. *Client Switch Rate (%)* is the likelihood a client changes its auditor next year, conditional on both client and auditor existing in the sample next year. *Unqualified Opinion (%)* is the likelihood a client receives a positive audit. *Negative Audit Rate (%)* is the likelihood a client receives at least one qualified or adverse opinion in a given year, and *Negative Audit Rate (FS) (%)* (*Negative Audit Rate (MP) (%)*) is the likelihood of receiving a negative opinion for its financial statements (major programs). *Qualified Opinion (%)* is the likelihood a client receives at least one qualified opinion and no adverse opinions in a given year. *Adverse Opinion (%)* is the likelihood a client receives at least one adverse opinion in a given year. *Going Concern (%)* is the likelihood a client has a going concern finding in a given year. *Material Weakness (%)* is the likelihood a client has a material weakness finding in a given year. *Material Noncompliance (%)* is the likelihood a client has a material noncompliance finding in a given year. *Reportable Condition (%)* is the likelihood a client has a reportable condition finding in a given year. See Section 1.3.

	count	mean	sd	p25	p50	p75
Amount ($1,000)	573,045	6,696	18,733	937	1,866	4,611
Amount (2023 USD; $1,000)	573,045	9,166	24,444	1,328	2,617	6,442
Relations:						
Pair Survival Rate (%)	554,266	60.3	48.9	0.0	100.0	100.0
Client Switch Rate (%)	554,266	13.3	34.0	0.0	0.0	0.0
Auditor Attrition Rate (%)	554,266	12.9	33.6	0.0	0.0	0.0
Client Attrition Rate (%)	554,266	13.5	34.1	0.0	0.0	0.0
Opinions:						
Unqualified Opinion (%)	573,045	87.5	33.1	100.0	100.0	100.0
Negative Audit Rate (%)	573,045	10.5	30.7	0.0	0.0	0.0
Negative Audit Rate (FS) (%)	573,045	6.7	25.0	0.0	0.0	0.0
Negative Audit Rate (MP) (%)	573,045	4.7	21.2	0.0	0.0	0.0
Qualified Opinion (%)	573,045	9.1	28.7	0.0	0.0	0.0
Adverse Opinion (%)	573,045	1.5	12.1	0.0	0.0	0.0
Findings:						
Going Concern (%)	573,045	0.8	9.1	0.0	0.0	0.0
Material Weakness (%)	573,045	11.4	31.8	0.0	0.0	0.0
Material Noncompliance (%)	573,045	4.6	20.8	0.0	0.0	0.0
Reportable Condition (%)	573,045	19.3	39.4	0.0	0.0	0.0

Panel B. Auditors. Summary statistics at the auditor×year level, 1997-2022. *Amount ($1,000)* is the sum of expenditures (in thousands) across all of an auditor's clients in a given year. *Amount (2023 USD; $1,000)* is the sum of client expenditures in inflation-adjusted dollars (2023 base year). *Number of clients* is the number of clients an auditor has in a given year. *Survival rate (%)* is the likelihood an auditor has at least one client next year. See Section 1.3.

	count	mean	sd	p25	p50	p75
Amount ($1,000)	151,046	4,541	13,824	776	1,436	3,397
Amount (2023 USD; $1,000)	151,046	6,582	18,707	1,158	2,138	5,046
Number of Clients	151,046	3.8	12.5	1.0	1.0	3.0
Survival Rate (%)	148,523	58.5	49.3	0.0	100.0	100.0

Table 2: **Main result: contagion effect of deregulation**

We estimate Equation (1) in a sample of client×auditor observations, 2013-2018. $NegativeAudit = 1$ if the auditor provided a negative audit of the client in a given year. $Qualified = 1$ ($Adverse = 1$) if the negative opinion was specifically "qualified" ("adverse"), which represents less (more) severe compliance concerns. $Treated = 1$ if the auditor has at least one client with expenditures below $750K in 2015, and $Treated = 0$ if the auditor did not have any clients under $750K but had at least one client with expenditures between $1 million and $1.5 million in 2015. $Post = 1$ from 2016 onwards. Robust standard errors are adjusted for clustering at the auditor and client level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 2.2.

	Negative Audit (%)				Qualified Opinion (%)	Adverse Opinion (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-3.035***	-3.102***	-1.894**	-2.013***	-1.453**	-1.648**
	(0.919)	(0.948)	(0.811)	(0.759)	(0.701)	(0.833)
Auditor FE	Yes	No	No	No	No	No
Client FE	Yes	No	No	No	No	No
Year FE	Yes	Yes	No	No	Yes	Yes
Pair FE	No	Yes	Yes	Yes	Yes	Yes
Entity Type × Year FE	No	No	Yes	No	No	No
Entity State × Year FE	No	No	No	Yes	No	No
2015 Mean	7.24	7.18	7.18	7.18	5.23	1.95
Effect (%)	-41.91	-43.21	-26.39	-28.04	-27.79	-84.58
Observations	106899	104063	104003	104058	104063	104063

Table 3: **Robustness tests**

Panel A. Alternative sample. Results from estimating Equation (3) at the auditor level, rather than auditor-client level. The outcome is an indicator for any negative audit issued by the auditor to any of their clients (column 1), the number of negative audits (column 2), and the rate of negative audits defined as the number of negative audits divided by number of clients (column 3). See Section 2.3.

	Any Negative Audit (%)	#Negative Audits	Negative Audit Rate (%)
	(1)	(2)	(3)
Treated × Post	-6.110***	-0.665***	-2.594***
	(1.602)	(0.143)	(0.733)
Auditor FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
2015 Mean	22.38	0.71	6.45
Effect (%)	-27.30	-93.86	-40.23
Observations	10786	10786	10786

B. Alternative control group. This table is identical to Table 2, column 2, except for the definition of control auditors. Here, *Treated=1* if an auditor has at least one client with expenditures below $750K in 2015, and *Treated=0* if an auditor does not have any clients under the $750K threshold in 2015 and has at least one client in 2015 with expenditures between $1 million and the specified upper bound ($1.6M to $2.0M). See Section 2.3.

	Negative Audit					
	UB: $1.5M	UB: $1.6M	UB: $1.7M	UB: $1.8M	UB: $1.9M	UB: $2.0M
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-3.102***	-2.879***	-2.865***	-2.782***	-2.645***	-2.735***
	(0.948)	(0.942)	(0.934)	(0.932)	(0.931)	(0.927)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
2015 Mean	7.18	7.21	7.25	7.28	7.30	7.30
Effect (%)	-43.21	-39.91	-39.50	-38.20	-36.24	-37.48
Observations	104063	105768	106843	107850	108723	109279

Panel C. Alternative sample period. This table is identical to Table 2, column 2, except for the sample period. We gradually expand the window, from ±2 years (column 1) to ±14 years (column 6). In the baseline specification, the window is ±3 years. See Section 2.3.

	Negative Audit (%)						
	2015-2016	2014-2017	2013-2018	2012-2019	2011-2020	2010-2021	2009-2020
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	-2.046***	-2.747***	-3.102***	-2.269**	-2.022*	-1.751	-1.588
	(0.764)	(0.849)	(0.948)	(1.017)	(1.144)	(1.238)	(1.307)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2015 Mean	6.54	7.12	7.18	7.19	7.21	7.24	7.24
Effect (%)	-31.30	-38.58	-43.21	-31.54	-28.07	-24.18	-21.94
Observations	29900	67775	104063	135124	163113	189723	211537

Table 4: **Aggregate contagion effect from deregulation**

Panel A. Effect on auditor portfolio. Results from estimating Equation (3) with different outcomes: indicator for losing any client (even if the total number of clients went up or did not change) and for having fewer clients (columns 1-2), dollar value of clients (column 3), number of clients (column 3), and number of clients conditional on losing clients (column 5). See Section 2.4.1.

	Lost Any Client (%)	Fewer Clients (%)	Clientele ($M)	Clients	Clients if Fewer
	(1)	(2)	(3)	(4)	(5)
Treated × Post	12.540***	15.162***	7.518**	-1.696***	-1.946***
	(1.675)	(1.647)	(3.491)	(0.226)	(0.395)
Auditor FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
2015 Mean	54.55	32.26	11.04	103.64	198.62
Effect (%)	22.99	46.99	68.07	-1.64	-0.98
Observations	9491	9841	9841	9841	2896

Panel B. Effect on state-level competition. Results from estimating Equation (4) at the state×year level. Outcome is the number of auditors per clients in the state. *Exposure* is the number of treated auditors in the state. *Post* = 1 after 2016. Robust standard errors are adjusted for clustering at the state level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 2.4.1.

	Ratio of Auditors to Auditees	
	(1)	(2)
Exposure × Post	0.147***	0.147***
	(0.046)	(0.046)
Exposure	-0.274*	
	(0.143)	
Post	-0.218***	
	(0.021)	
Year FE	No	Yes
State FE	No	Yes
2015 Mean	0.25	0.25
Effect (%)	59.29	59.29
Observations	1300	1300

Table 5: **Costs of issuing a negative audit**

Results from estimating Equation (5). The sample covers the period 1997-2021. *Relationship Survived Next Period* is an indicator variable that takes one if the client continues to hire the auditor in the next year, and zero otherwise. *Negative Audit* is an indicator variable that takes one if the auditor provided a negative audit of the client in a given year, and zero otherwise. $Exited = 1$ if the client exits the sample in next period. $Switched = 1$ if the client did not exit but chose a new auditor in the next period. Robust standard errors are adjusted for clustering at the auditor and client level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 2.4.1.

	Relationship Survived Next Period (%)						Exited (%)	Switched (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Negative Audit	-11.215***	-3.529***	-2.885***	-2.564***	-3.428***	-3.815***	2.148***	2.558***
	(0.731)	(0.869)	(0.761)	(0.756)	(0.473)	(0.502)	(0.491)	(0.421)
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Entity Type FE	No	Yes	No	No	No	Yes	Yes	Yes
Entity State FE	No	No	Yes	No	No	Yes	Yes	Yes
Relationship Age FE	No	No	No	Yes	No	Yes	Yes	Yes
Auditor FE	No	No	No	No	Yes	Yes	Yes	Yes
Sample Mean	60.31	64.76	60.31	60.307	63.49	67.39	12.57	22.79
Effect (%)	-18.60	-5.45	-4.78	-4.25	-5.40	-5.66	17.09	11.22
Observations	554266	483649	554266	554266	526374	464625	464625	404470

Table 6: **Evaluating alternative channels**

Panel A. We estimate Equation (6) in the period 2004-2012. *Negative Audit* is an indicator variable that takes one if the auditor provided a negative audit of the client in a given year, and zero otherwise. *Above 750K* is an indicator variable that takes one if a client's expenditure is above $750,000, and zero otherwise. Robust standard errors are adjusted for clustering at the auditor and client level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 2.4.2.

	Negative Audit					
	(1)	(2)	(3)	(4)	(5)	(6)
Above 750K	-0.005	-0.007**	0.000	-0.004	0.002	0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)
Year FE	No	Yes	Yes	Yes	Yes	Yes
Entity Type FE	No	Yes	No	No	No	Yes
Entity State FE	No	No	Yes	No	No	Yes
Relationship Age FE	No	No	No	Yes	No	Yes
Auditor FE	No	No	No	No	Yes	Yes
Sample Mean	0.097	0.097	0.097	0.097	0.097	0.097
Observations	217693	217688	217693	217693	205608	205604

Panel B. We estimate Equation (1) in three different subsamples. Column 1 is the baseline sample and the coefficient is identical to the second column in Table 2. Column 2 uses only clients that existed both before and after the reform. Column 3 uses only clients that were above the $750,000 threshold at all times, even before the reform took place. Robust standard errors are adjusted for clustering at the auditor and client level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 2.4.2.

	Negative Audit (%)		
	Baseline (1)	Auditee exists in pre and post (2)	Auditee above 750K in pre and post (3)
Treated $\times$ Post	-3.102***	-3.102***	-2.587***
	(0.948)	(0.948)	(0.814)
Year FE	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes
2015 Mean	7.18	6.95	6.06
Effect (%)	-43.21	-44.61	-42.66
Observations	104063	95567	60708

Table 7: **Cost of auditing: Structural estimation**

This table presents the estimates of the three structural parameters: θ is the recurring cost of a negative audit, η is the cost of switching from positive to negative audit, and δ is the scale discount. We use the full sample (1999-2022) and the NFXP algorithm of Rust (1987). **Panel A** reports the the estimated parameters and their standard errors. **Panel B** reports the probabilities of issuing a negative opinion in the model and in the data across different subsamples. The subsamples are based off the two state variables: last period's audit outcome (positive or negative), and auditor size, where "Large" and "Small" indicate that the above- and below-median split based on client dollar value. See Section 4.1.

Panel A: Parameter estimates

	θ	η	δ
Estimate	0.368	6.417	-0.119
Std. error	(0.013)	(0.133)	(0.007)

Panel B: Probability of negative opinion vs. state variables

	Simulated	Actual
Full sample	0.081	0.077
Positive opinion	0.026	0.026
... and large auditor	0.031	0.029
... and small auditor	0.021	0.024
Negative opinion	0.638	0.642
... and large auditor	0.636	0.614
... and small auditor	0.640	0.671

Table 8: **Deregulation: quantitative implications**

We estimate the structural parameters $\{\theta, \eta, \delta\}$ on different subsamples. θ is the recurring cost of a negative audit, η is the cost of switching from positive to negative audit, and δ is the scale discount. In Panel A, we split the sample into pre-treatment (2013–2015) and post-treatment (2016–2018). In Panel B, we split the full sample (1999-2022) based on the concentration of the auditor market: we compute the ratio of auditors to clients in each state×year, and markets above (below) median are considered low (high) concentration. We estimate the model using the NFXP algorithm (Rust (1987)) and report the p-values from testing whether parameter estimates across different subsamples are different (Swait and Louviere (1993)). See Section 4.3.

Panel A: Subsample estimates: Pre-treatment vs. Post-treatment				
		θ	η	δ
Pre-treatment	Estimate	0.369	7.635	-0.178
	Std. error	(0.037)	(0.362)	(0.019)
Post-treatment	Estimate	1.017	4.929	-0.065
	Std. error	(0.034)	(0.393)	(0.021)
SL test—p-value	0.000 (1st stage)			

Panel B: Competition in the auditing markets				
		θ	η	δ
Low competition	Estimate	0.326	6.679	-0.144
	Std. error	(0.019)	(0.183)	(0.010)
High competition	Estimate	0.432	6.294	-0.109
	Std. error	(0.021)	(0.223)	(0.012)
SL test—p-value	0.059 (1st stage)			
	0.000 (2nd stage)			

Table 9: **Counterfactual deregulation policies**

We study several counterfactual scenarios. In each scenario, we change the value of one parameter while keeping the remaining two parameters at their estimated values from Table 7. In Panel A, we change each parameter by 1% . In Panel B, we shut down each parameter (setting its value as zero). θ is the recurring cost of a negative audit, η is the cost of switching from positive to negative audit, and δ is the scale discount. For each scenario, we report the new model-implied probability of a negative audit ($\mathbb{P}(d_t|x_t)$); the percent change relative to the baseline probability of 8.2% ($\%\Delta\mathbb{P}(d_t|x_t)$); and the expected dollar value of grants that will be audited more leniently ($ change), calculated as $\%\Delta\mathbb{P}(d_t|x_t)$ times the average annual size of the federal grants market. The model is estimated using the NFXP algorithm of Rust (1987). See Section 4.4.

Panel A: Elasticities	$\mathbb{P}(d_t\|x_t)$	$\%\Delta\mathbb{P}(d_t\|x_t)$	$ change
1% increase in recurring cost θ	8.10%	-0.70%	-$11.0m
1% increase in switching cost η	7.97%	-2.26%	-$35.5m
1% decrease in scale discount δ	8.08%	-0.92%	-$14.4m

Panel B: Turning off parameters	$\mathbb{P}(d_t\|x_t)$	$\%\Delta\mathbb{P}(d_t\|x_t)$	$ change
No recurring cost $\theta = 0$	19.09%	134%	$21.05bn
No switching cost $\eta = 0$	40.09%	392%	$61.49bn
No scale discount $\delta = 0$	5.18%	-37%	-$5.74bn

Internet Appendix

Table A.1: **Estimation results of the extended model**

This table is similar to Table 7, except that we estimate an extended model where the recurring cost increases with project size p_t.

Panel A: Parameter estimates			
	θ	η	δ
Estimate	0.024	6.319	-0.114
Std. error	(0.001)	(0.134)	(0.007)

Panel B: Probability of negative opinion vs. state variables		
	Simulated	Actual
Full sample	0.082	0.077
Positive opinion	0.027	0.026
... and large project	0.027	0.028
... and large auditor	0.030	0.029
... and small auditor	0.022	0.026
... and small project	0.027	0.023
... and large auditor	0.033	0.026
... and small auditor	0.023	0.022
Negative opinion	0.638	0.642
... and large project	0.630	0.618
... and large auditor	0.627	0.598
... and small auditor	0.635	0.643
... and small project	0.655	0.685
... and large auditor	0.651	0.645
... and small auditor	0.658	0.704

Table A.2: **Audit patterns and model parameters**

In Panel A, We estimate the autocorrelation of negative and positive audit rates within client over time. In Panel B, we estimate the correlation between auditor size and the probability of issuing at least one negative audit. Robust standard errors are adjusted for clustering at the auditor and client level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. See Section 3.1.

	Negative Audit (%)			Positive Audit (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Lag Negative Audit	63.388***	62.763***	43.782***			
	(1.007)	(1.039)	(1.191)			
Lag Positive Audit				63.388***	62.763***	43.782***
				(1.007)	(1.039)	(1.191)
Auditee FE	No	No	Yes	No	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Mean	8.40	8.40	8.34	91.60	91.60	91.66
Effect (%)	754.72	747.28	525.09	69.20	68.52	47.76
Observations	334259	334259	328808	334259	334259	328808

	Any Negative Audit (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Clients	0.657***	0.706***	0.459***			
	(0.071)	(0.077)	(0.053)			
Total Clientele ($M)				0.020***	0.021***	0.013***
				(0.004)	(0.004)	(0.003)
Auditor FE	No	No	Yes	No	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Mean	19.15	19.15	20.12	19.15	19.15	20.12
Effect (%)	3.43	3.69	2.28	0.10	0.11	0.06
Observations	151047	151047	120628	151047	151047	120628