The London School of Economics and Political Science

Essays in Empirical Corporate Finance

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Thesis submitted to the Department of Finance

of the London School of Economics and Political Science

for the degree of

DOCTOR OF PHILOSOPHY

London, United Kingdom

July 2019

Declaration

I certify that the thesis I have presented for examination for the Ph.D. degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 39,656 words.

Statement of conjoint work

I confirm that Chapter 2 is jointly co-authored with Krzysztof Kalisiak. I contributed 50% of the work for Chapter 2.

Abstract

This thesis contains three essays in empirical corporate finance. In the first chapter I show how cash constrained firms take actions that improve their short-term liquidity even if they hurt long-term profitability. In government procurement auctions, constrained firms have incentives to overbid because winning a contract improves their short-term cash flows. However, overbidding results in lower long-term profits. I provide an unbiased estimate of the drop in performance of winners following an award by measuring performance relative to companies which placed second in the auction. I show that financial constraints predict aggressive bidding, that firms overbid less in auctions that require larger deposits, and that winning long-term contracts causes a short-term increase and subsequent decline in profitability. My results offer a non-behavioural explanation for the *winner's curse*.

In the second chapter, co-authored with Krzysztof Kalisiak, we investigate how access to local financing supports economic development. Firms with easier access to financing respond to a future improvement in investment opportunities at the time the improvement is announced. Other firms catch up only after the improvement and associated cash flows are realized. We exploit variation caused by infrastructure development in the oil industry that exogenously affects firms in only one region and use nearby regions as control. The event creates a gap between announcement and realization dates, which eliminates the problem of reverse causality and highlights the role of financial constraints.

In the third chapter, I examine firm behaviour after major R&D breakthroughs. I use the example of pharmaceutical companies that carry out last-stage clinical trials for new oncology drugs. *Success* is defined as Food and Drug Administration approval to market new drugs. Companies that obtain approval increase capital expenditure. However, there is no change to their research and development expense, cash holdings, or short-term investments. This supports the hypothesis that innovative firms follow long-term strategies and finalising drug development, even though infrequent, does not radically change their behaviour.

Acknowledgement

I would like to express my deep gratitude to Vicente Cuñat and Dirk Jenter, my supervisors, for their guidance, patience and encouragement. I could not have imagined having better academic mentors.

I want to give special thanks to Christian Julliard, Moqi Groen-Xu and Daniel Paravisini who advised me at various stages of my Ph.D. studies. They helped me both with useful critiques and with encouragement. My grateful thanks are extended to the whole faculty in the Department of Finance for their insightful comments during presentations. I am also indebted to the Systemic Risk Centre at LSE for providing a great environment to work.

I would like to thank my co-author and friend, Krzysztof Kalisiak, and all my colleagues at LSE. We have gone through the Ph.D. together, discussing ideas, sharing experiences and supporting each other in the most challenging times.

Last, but not least, I would like to thank my family. I am very grateful for their unconditional support that helped me to grow as a researcher and as a person. Most of all, I would like to thank my beloved husband Bartosz. He has been with me from the very beginning of my academic career and I am grateful that we have made this exciting journey together. I would not have been where I am without him.

I dedicate this work to my son Leonard. He is my greatest inspiration.

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Chapter 1

Strategic overbidding in procurement auctions?

1.1 Introduction

The impact of financial constraints on firm behaviour is a central question in corporate finance. Most studies focus on the effects of liquidity constraints on total investment (for example, Fazzari, Hubbard, and Petersen (1988); Kaplan and Zingales (1997); Whited (1992)). However, financial constraints might also distort firms' objectives from long-term value maximization to short-term survival. There is little evidence on how far constrained firms are ready to go compromising future payoffs to secure short-term liquidity.

In this paper I document that winners of government procurement auctions perform poorly and propose a novel explanation that is based on this intertemporal trade-off. I test the hypothesis that cash constrained firms strategically overbid to improve short-term liquidity. Winning a procurement contract ensures short-term cash inflows, and submitting a low-price offer increases the chance of winning.[1] Overbidding, however, creates losses in the longer term.

I test this hypothesis against two alternative explanations. Firstly, firms might

[1] Regular payments for the work performed are standard for government contracts in many countries (for example, U.S., Germany, Poland), including my setting.

bid optimally ex ante but perform poorly ex post because of unexpected negative shocks that hit the economy after the award. I refer to this as a *bad luck* story. Secondly, common value auctions might suffer from the *winner's curse*. The winner's curse explanation posits that firms do not bid optimally because they do not account for the fact that the most optimistic agent wins the auction.

I start by measuring the change in performance of firms winning government contracts. Using data on procurement auctions for road construction in Poland from 2010 to 2014, I compare firms that ranked first (winners) with firms that ranked second (runners-up) in a competitive auction. Looking at firms that chose to participate in the same auction and were similarly ranked reduces selection bias, as these companies should be comparable in terms of their abilities and availability to carry out the project. Expertise, cost efficiency and spare capacity impact both the offer price and subsequent performance, but are otherwise unobservable. Closely-ranked firms are more likely to be similar when the auction is more competitive, that is, when there are many participants. I support my identification assumption by comparing the observable characteristics of winners and runners-up before the auction. I do not find significant differences in terms of profits, assets, liabilities, solvency, current ratio, profit margins and other performance measures. This holds even when I account for the fact that some firms both won and placed second in an auction in the same year and exclude such observations from the control sample.

I show that the difference in the performance change between winners and runners-up in the two years following an auction is negative and amounts to a drop of 2.30% in the profit margin. This decline in performance of winners and its significance is stable across different subsamples, when data are collapsed at the firm-year level, and when observations are matched on firm size, auction impact, and award year.[2] Deteriorating performance after winning an auction suggests that companies overbid by submitting underpriced offers.

In the second part of the paper, I investigate the cause of this behaviour. First, I try to understand selection into aggressive bidding. I compare the pre-award char-

[2]The auction impact is measured as auction size relative to firm size.

acteristics of aggressive bidders (winners and runners-up) with a matched sample of other construction companies. I find that low cash holdings, low cash flow growth, and several other measures, which might be associated with financial constraints, predict aggressive bidding. These results suggest that firms ready to sign underpriced contracts are financially constrained.

Next, I examine how the profitability of auction winners and runners-up evolves over time. In my setting, government contracts improve short-term liquidity because they ensure regular (usually monthly) payments for the work performed. If liquidity constrained firms aim for short-term gains, they should perform well shortly after the award. I demonstrate that firms winning long-term contracts experience a temporary increase in both profits and cash flows in the award year. With annual data, I cannot exclude the possibility that pre-award events underlie this positive change. However, for contracts signed at the end of a year, the positive impact extends to the year following the award. This is not the case for contracts signed at the beginning of a year and suggests that there is a link between the temporary improvement in firm performance and the auction resolution. This pattern confirms that firms have incentives to overbid and sign new projects. By doing so, companies front-load cash flows because early payments are received at the cost of profits made in the future.

I further investigate the heterogeneity in bidding behaviour caused by the differences in bid guarantees required in auctions. A bid guarantee is a deposit that a firm has to pay in order to participate in the bidding. It is re-paid unless a firm wins but refuses to sign the contract. Upfront deposits may discourage companies facing liquidity issues. I find that the decline in winners' performance is driven mostly by auctions with low deposit requirements. This is consistent with the idea that cash constraints underlie aggressive bidding and the subsequent drop in profitability.

The phenomenon of underperforming after auction wins held during the period from 2010 to 2014. Winners of auctions resolved in 2008 and 2009 perform better. This aligns with the intuition that auction winners should not consistently overbid in equilibrium. The 2008–2009 period is the end of the rapid development of the Polish construction market following Poland's accession to the European Union. These years are characterised by an ample supply of government procurement contracts, while the period from 2010 to 2014 is marked by lower demand for construction services. It is, therefore, likely that many more construction companies found themselves financially constrained in the latter period, which can explain their more aggressive bidding behaviour in procurement auctions. This suggests that how aggressively firms bid and to what extent they sacrifice long-term outcomes depends on external economic conditions that vary over time.

I also test alternative hypotheses that could explain the poor performance of auction winners. Firms' profits might deteriorate due to *bad luck*, that is, firms might bid optimally but are hit by a negative post-award shock. I consider two potential shocks: the financial crisis and a local cost shock. Neither can explain the results. The bad performance of auction winners from 2010 to 2014 is unlikely to be related to the financial crisis in the EU in 2009 as Poland experienced only a mild slowdown around that time. Aggregate demand was additionally boosted by increased infrastructure investments for the upcoming European Football Championships in 2012. I also do not find evidence of survivorship bias, which would have to be present in this scenario because a macroeconomic shock inevitably affects both winners and runners-up. To assess whether my results might be due to a local cost shock, I examine the developments on the natural aggregates (crushed rock, sand and gravel) market in Poland. The biggest increase in production was recorded in 2011, which suggests that there was high demand for building materials and possibly an increase in prices of natural aggregates in previous years. This is inconsistent with the fact that the least profitable government contracts were signed in 2012.

Finally, I examine whether the underperformance of auction winners might reflect the winner's curse. The winner's curse hypothesis is a standard explanation of negative payoffs in common value auctions. According to this hypothesis, agents do not account for the fact that the most optimistic bidder wins. In the context of government procurement, the cheapest offer gets the award but the price is on average below the project cost and the winning firm incurs losses. If the winner's curse explains my results, then firm's ability to bid optimally must be time-varying, as the decline in winners' performance is not observed during 2008–2009. This might be the case if, for example, the optimal bidding strategies depend on experience and a changing economic environment, as proposed by Dyer, Kagel, and Levin (1989). They claim that firms' bidding improves with experience, but changes in external conditions may distort the learning process and create a need for re-adjustment. Their story implies that more experienced companies should perform better. It is, however, inconsistent with my results because my sample comprises of mature firms, with the majority over 10 years old. I also do not find a link between firm's bidding experience and performance after winning.

My paper adds to the large literature on firms' financial constraints. The analysis of behaviour of constrained firms started with the work of Fazzari, Hubbard, and Peteresen (1988) and has been continued in many studies (for example, Hoshi, Kashyap, and Scharfstein (1991); Kaplan and Zingales (1997); Lamont (1997)). Liquidity demand is directly examined by Almeida, Campello, and Weisbach (2004) who find that financially constrained firms have a higher propensity to save cash. These studies, however, do not emphasize that projects have different impacts on short-term and long-term profits.

Busse (2002) describes behaviour similar to overbidding in the context of airlines. She finds that the poor financial condition of an airline increases the probability of starting a price war. Phillips and Sertsios (2013) reach the same conclusions and additionally show that airlines in financial distress are ready to compromise on quality. The drop in quality induced by the willingness to secure cash flows for debt repayments is also documented by Matsa (2010). All these findings are consistent with the strategic overbidding hypothesis I propose. My results additionally imply that constrained firms aim to improve short-term cash flows even when it hurts long-term payoffs. I also directly measure the drop in profits accepted as a trade-off.

The idea of the liquidity-profitability trade-off also appears in the theoretical predictions of Garicano and Steinwender (2016). Based on the work of Aghion et al. (2010) they build a model in which liquidity constrained firms give up future expected payoffs to increase the probability of surviving. The difference with my setting is that firms do not substitute long-term investment with short-term investment, but sign underpriced contracts and front-load cash flows from new projects. Also, it is not necessarily in response to a negative shock. Firms aim to improve liquidity even when they are not faced with the risk of immediate bankruptcy. This last feature is in line with the model of Maksimovic and Titman (1991).

The idea of short-termism, which is inherent in the trade-off I discuss, puts my paper close to studies of managerial myopia. Optimal contracts with managerial short-termism are examined in several articles. Varas (2017) and Edmans et al. (2012) provide examples of models in which managers improve today's performance by compromising future outcomes. I show similar myopia at a different level because my sample consists of medium-sized, unlisted companies that do not face open manager-shareholder conflicts. It proves that short-termism pressure exists in private firms, even though it may be weaker than that experienced by listed companies (Jensen and Meckling (1976)).

My analysis also relates to the empirical auctions literature which documents that winners incur losses following an award. Such underperformance was interpreted as a sign of the winner's curse in the context of oil leases (Capen, Clapp, and Campbell (1971)), baseball players' market (Blecherman and Camerer (1998)), real estate (Ashenfelter and Genesove (1992)) or initial public offering underpricing (Rock (1986)).[3] I also document the poor performance of auction winners, but I propose that flawed bidding strategies are not the only plausible explanation. My hypothesis, therefore, provides a new perspective on a well-known result. It might also explain why some existing empirical studies do not find evidence of the winner's curse (for example, Thiel (1988)).

My paper additionally makes a methodological contribution. The theory distinguishes common value auctions, in which an auctioned object has the same value for all bidders, and private value auctions, in which each bidder has his own valuation of the object. In reality, auctions contain both *common* and *private* value elements.

[3]Kagel and Levin (2002, chap. 1 and 2) provide a comprehensive summary of relevant empirical work on the winner's curse.

In the case of government contracts, a common value element depends on, for example, the type and scope of the project, while an individual value element reflects each firm's ability. Modelling both components significantly complicates theoretical considerations.[4] By looking at closely-ranked firms, I aim to eliminate the *private* factor. This provides a simple approach to testing hypotheses regarding common value auctions.

My methodology also adds to the mergers and acquisitions literature where finding an appropriate benchmark for the evaluation of acquirers has proved challenging. Some authors consider failed mergers (for example, Savor and Lu (2009); Seru (2014)). My approach is similar to the idea of Malmendier, Moretti, and Peters (2018). They compare winners of merger contests with other contest participants. My setting has several advantages. First, I can clearly define similar firms and competitive contests because I have an official firm ranking. Second, I look only at closely ranked firms. Third, I exploit a direct link between winning and profitability that relies on the offer price.

The paper is structured as follows: in Section 1.2 I present the setting and datasets. Section 1.3 describes the methodology and reviews identification assumptions. Section 1.4 documents the drop in winners' performance. In section 1.5 I test the strategic overbidding hypothesis. Sections 1.6 and 1.7 discuss alternative explanations. In section 1.8 I present several robustness checks. Section 1.9 concludes.

1.2 Setting and data

1.2.1 Road construction in Poland

The Polish economy is ranked eight in the European Union by size[5] and is the largest one among countries from the former Soviet bloc. Construction accounts for

[4]A model of an auction with both common and individual components appears in, for example, Hong and Shum (2002).

[5]Source: World Bank (*https://data.worldbank.org/indicator/NY.GDP.MKTP.CD?locations=PL*)

9.5% of gross domestic product.[6] Contracts for road construction are mostly provided by the government. The institution responsible for procurement is called the General Directorate for National Roads and Highways (GDDKiA). Poland provides a good setting for the analysis based on recent data because it was not severely affected by the crisis spreading from the United States into Europe in 2008 and 2009. Apart from a strong internal market, the upcoming European Football Championships in 2012 helped the country to navigate away from the recession because the preparations involved many infrastructural investments.

Government procurement for road construction in Poland is regulated by Public Procurement Law as of 29 January 2004. The law ensures equal treatment of all bidders and transparency of the whole process. These rules provide a foundation for the whole analysis by ensuring there is no obvious selection bias in my sample. In the most popular auction modes firms submit sealed offers after the auction announcement.[7] Offers are confidential until the resolution day when they are publicly opened and undergo evaluation. It eliminates collusion and corruption. Points are awarded according to the clear criteria that are announced in advance. The offer with the highest cumulative score wins the auction.[8] Deadlines for submission, resolution, and signing the contract are fixed and also available beforehand. Any company may participate in a government procurement auction.[9] A deposit (bid guarantee) of 0.5%–3% of the estimated contract size may be required to guarantee an offer is serious and binding.

Criteria for awarding points can include price, promised deadlines or length of a granted guarantee. I analyse only procurement for *construction* because price is the main determinant of the winner in these auctions. Even with other criteria, the

[6]Source: My own calculation based on 2017 data from the Central Statistical Office, quarterly national accounts of gross domestic product 2013–2017, pp. 37, Table 2; output in 2013–2017, current prices.

[7]These modes are called *limited* and *unlimited* auctions.

[8]Sometimes auctions comprise of several tasks, and firms may submit only partial offers for some of the tasks. Since winners and runners-up are not clearly defined in such auctions I do not include them in my analysis.

[9]In a *limited* mode, the authorities may pre-select suppliers to limit the number of bidders. Pre-selection criteria are also known in advance, so this two-step procedure does not affect the competitiveness of a process or the equality of potential participants.

weight imposed on price is such that it outweighs everything else. Also, offers vary little in other dimensions. Such a setting provides a one-to-one link between price, ranking place, and winning.

Each offer contains price quotes for the whole work itemized in the specification. Only final price matters for the offer evaluation, but quotes for different work stages are relevant for payments. Once a company begins construction, it regularly submits (usually at the end of a month) payment claims for the work performed and receives payments according to how it is priced in the offer. What is important, Polish government does not delay payments. These rules have two important implications for my analysis. First, firms that sign government contracts receive almost immediate cash inflows. Second, different work stages can be differently priced. Specifically, firms can impose high prices on the initial work so that early payments are relatively higher than payments received in the end.[10]

While it is possible to front-load future payments, it is not possible to renegotiate the overall offer price because The Public Procurement Law does not allow any significant changes to the contract signed between authorities and a firm. Submitted offers are binding, and firms cannot expect to recoup losses during negotiations later on.

1.2.2 Auctions data

The data about available and already assigned government contracts for road construction in Poland are published on the website of the General Directorate for National Roads and Highways (GDDKiA). These data document the whole bidding process, ensuring transparency and preventing corruption in public auctions. The dataset is published in Polish and contains detailed information about contractual requirements and obligations, winning criteria, participants, and the value of the offers and deadlines. The first entries come from the end of 2005.

The minimum set of information I require to insert an auction in the sample

[10]According to the practitioners I talked to, it is common to put high margins on tasks that are done early to ensure quick cash inflow to the firm.

includes the year of the award, the name of the winning firm and the runner-up, the prices of the top two offers, and the number of auction participants. These data are rarely missing, so my sample covers almost all contracts provided by GDDKiA. Appendix 1.12.1 describes the data collection process in details.

Table 1.1 presents the summary statistics of auctions resolved from 2008 to 2014. The summary includes both competitive and non-competitive auctions to provide a background description of the investments available to construction firms in these years. During that time, over 3,000 public auctions took place for infrastructure construction work in that period. The median winning offer amounts to PLN 0.71 million and lasts a little over 7 months. However, contract sizes and lengths are widely variable. On average, four competitors bid for each contract.

Figure 1.1 presents characteristics of auctions by year. The number of auctions initiated by the government is relatively stable until 2013, at which point, the number drops. This time period coincides with the completion of preparations for the European Football Championships that took place in 2012. However, the overall amount of work offered does not drop drastically but falls steadily over years. Competition increases over time and in 2014 on average there is one more participant in each auction compared to 2008. Mean price varies a lot over the years but this measure is sensitive to the presence of big infrastructure investment projects. Ventures like building the motorway *Autostrada A4* are not undertaken every year and are also likely to crowd out smaller investments.

1.2.3 Financial data and merged sample

I use financial data on Polish firms included in the international database of Bureau van Dijk *Orbis*, which provides extensive data on firms' financials and history. I evaluate the coverage of Polish firms by looking at the statistics from the Polish Central Statistical Office (GUS). For 2015, Orbis lists 1.539 million Polish firms, whereas the Central Statistical Office records 1.914 million firms. At the end of the sample, around 80% of Polish firms are covered in the Orbis database. This percentage is smaller in the beginning of the sample and amounts to 55%. The coverage is better for bigger companies, which are more likely to compete for government contracts, especially in bigger auctions with more participants.

The accounting data in Orbis are available for the 10 most recent years and cover the period from 2006 to 2016. However, for years 2006–2007 I have only a limited set of variables. My sample assumes a five-year observation window centred on auction awards. This allows me to study auctions resolved from 2010 to 2014 using a wider set of variables or auctions resolved from 2008 to 2014 based on a limited set of variables. I focus on public auctions resolved from 2010 to 2014 because it was a period of relative stability in the construction industry.

I merge the data sources based on firm's name, city, and legal form. The merging is successful for 88.7% of the observations and 79.8% of firms and does not vary depending on the auction award year. Appendix 1.12.1 provides more details about the merged sample.

1.3 Empirical strategy

1.3.1 Identification

The main idea underlying my empirical analysis is to compare performance of procurement auction winners and runners-up around the award year. Firms that participate in the same auction and are ranked at the top close to each other are likely to be similar in terms of abilities, investment opportunities, capacity and bidding strategy. The auction award exposes some firms to treatment. I employ a difference-in-differences regression at the firm-auction level centred on the award year to identify the causal effect of winning a procurement auction. The baseline specification is:

$$\begin{aligned} y_{ijt} &= \beta_{Post} Post_t + \beta_{Win} Win_{ij} + \beta_{dd} Post_t \times Win_{ij} + AuctionGroupFE_{g(j)} + \\ &\quad + YearFE_{f(j,t)} + \epsilon_{ijt}, \end{aligned} \tag{1.1}$$

where y_{ijt} is performance of firm i that participates in auction j. It is measured at time t relative to the award year, where t runs from -2 to 2. The parameter of interest is denoted as β_{dd} and captures the causal effect of winning an auction on firm performance after the award compared with performance of runners-up. The dummy Win_{ij} identifies auction winners. Companies that both won and placed second in an auction in a given year are treated because winning any contract affects firm performance. The variable $Post_t$ distinguishes the pre-award period from the post-award period. Ideally, I would introduce auction fixed effects and analyse the within-auction variation between winners and runners-up following the award. In practice, however, not every auction has a winner - runner-up pair because of missing financial data or because the runner-up won another auction and became treated. Therefore I combine auctions into small groups based on the award year and firm size, and introduce auction group fixed effects $AuctionGroupFE_{g(j)}$. The main specification has 100 auction groups based on the five resolution years and twenty size demi-deciles. I exclude few groups with only winners or only runners-up. In the following paragraph I discuss the impact of this adjustment on identification. Year fixed effects ($YearFE_{f(j,t)}$) depend on the auction award year and the period around the award. They control for the overall industry cyclicality and are included because auctions are resolved in different years. Standard errors are robust and clustered at the auction group level. The results are robust to alternative specifications, which are discussed in detail in Section 1.8.

The identification relies on the assumption that the dummy Win is not correlated with firms' abilities and investment opportunities. To ensure it is satisfied, I consider only *competitive auctions* with at least three participants and focus on the winners and the closest competitors (runners-up). In the competitive environment, firms bidding for the same contract and ranked close to each other should be comparable in terms of the capacity and abilities to fulfil the contract. I measure the treatment effect within a group of similar auctions, so to validate my identification I have to make a stronger claim. I assume that firms bidding for similar contracts and ranking at the top are comparable. To identify *similar* auctions I control for auction size and resolution year. Controlling for size is crucial because firm's ability to carry out a construction project is mainly determined by the size of the contract. In the Appendix (Table A.2, Appendix 1.12.2) I present the results based on winner - runner-up pairs with auction fixed effects.

Another identification challenge arises because some firms participate in several auctions every year. The decision about participation is endogenous, and firms which participate in many auctions are more likely to be treated because of my definition of the control group. The causal interpretation relies on the assumption that participation is not correlated with worse performance. One of the concerns is that bigger firms (with a bigger capacity) participate in auctions more often and hence appear in the treatment group more often. If size affects performance in my observation period, then the documented effect would arise due to selection bias. To address this issue, I match auction participants based on size, auction impact, and auction year.[11] The second concern is that firms become more desperate with each lost auction. It would create selection bias with desperate, worse-performing firms appearing more frequently in the treatment group. However, I do not find evidence that likelihood of winning increases after several lost auctions. Another concern is that firms stop participating when they get better outside options. These firms are more likely to be in the control group because frequent participation increases the likelihood of treatment. In fact, firms which participate in only one auction do not perform better. Table A.3 in Appendix 1.12.2 includes the estimates of a regression controlling for one-time participation. Furthermore, outside options are limited and involve non-competitive (less popular) contracts, local contracts smaller in size, or rare private contracts. Finally, to address any other concerns arising due to endogenous participation I perform two robustness tests in which the link between participation and treatment is broken. I run the regression on firms that participated only once in a given year. I also collapse the data at the firm-year level and introduce non-binary treatment intensity, which depends on sizes of won and lost contracts.

[11]The auction impact is measured as auction size relative to firm size.

The conclusions do not change if not winning any auction during in a given year signals a bad company. Such a story is against the idea of *losing winners*, and if it is true, then my estimates provide a lower bound of the magnitude of the decline in profitability of auction winners.

My primary performance measure is profit margin, which is defined as earnings before tax over sales. This measure has several advantages. Firstly, it is a scaled measure that facilitates a comparison of companies of different sizes. Secondly, it is naturally linked to the bidding process because prices offered by firms are essentially summed cost estimates and margins. Thirdly, and finally, from the practical point of view, profit margin is a relatively well-populated variable in my dataset. Other measures I consider are *return on assets (EBT)*, calculated as earnings before tax over total assets, and *return on assets (NI)*, calculated as net income over total assets. As the award may affect assets, I construct two more measures: *return on assets (EBT, pre-award assets)*, and *return on assets (NI, pre-award assets)*, in which I divide profits by average assets of a firm in two years before the auction award. I rely on earnings before tax as a profit measure because Polish construction firms tend to lease and not buy machinery. Lease payments constitute a predictable cost that should be accounted for in the offer submitted in the bidding process and included later in profit calculations. Table A.4 in Appendix 1.12.2 presents the basic results based on earnings before interest, taxes, depreciation, and amortization (EBITDA).

Running a regression at the firm-auction level has several advantages. Doing so allows a direct comparison of top ranking firms and the inclusion of auction group fixed effects. It also assigns more importance to companies that won several contracts in a given year. Finally, it does not require taking a stand on how important a particular auction for a particular company is, which may be difficult for firms submitting joint offers.

Most firms appear in my sample at most few times. However, a few outliers reappear dozens of times. In order to ensure that my sample of firms is representative and results are not driven by a small pool of outliers, I drop firms that rank at the top of the auction ranking more than 40 times in the relevant period. It coincides with dropping five big companies that also tend to participate in smaller auctions. Figure 1.2 presents how often firms appear at the top of the competitive auction ranking in the refined sample. Analogical figure based on the raw sample is in the Appendix 1.12.2 (Figure A.2). I test alternative refinements regarding firm participation in Section 1.8.3.

1.3.2 Firm characteristics

The comparability of firms that rank first and second in an auction should be reflected in the observables. A significant imbalance would suggest that despite the outlined intuition winners are systematically different. Such differences may raise concerns whether time effects for the treated and control companies are indeed the same. Table 1.2 presents the average firm characteristics and performance measures in the two years before auction awards. To ensure extreme values do not obscure the summary statistics, I winsorize the data at the 0.1% level.

The last two columns present the results of the t-test on differences in means between *winners* and *runners-up*. The *runners-up* sample does not include firms that placed both first and second in an auction in a given year. None of the variables in the t-test yields a significant difference. The variability between firms is, however, huge and the differences between coefficients raise concerns even though they are statistically insignificant. The disparities are mostly observed in the variables expressed in levels, and not in ratios. This finding suggests there may be a weak size effect in my sample, and the control group consists of smaller firms. This possibility was already mentioned in the previous section. The size effect undermines the conclusions if bigger firms on average perform worse after winning a government contract compared to small firms. Then proper identification requires controlling for differences in the firm size. To address this concern, I repeat the analysis on the matched sample. I match observations on firm size, auction impact and auction award year. Accounting for the auction impact is particularly relevant for big firms, for which the influence of small contracts may be blurred by other factors. The timing of the award is important because the construction industry is cyclical.

1.4 Winning by losing

I find that firm performance significantly deteriorates after winning a competitive procurement auction compared with firms ranked right below the winner. Performance is measured by profit margin and return on assets. Such results suggest that firms overbid in government procurement auctions. The conclusions do not change when I analyse the matched sample controlling for firm size, auction impact, and award year.

1.4.1 Performance of winners after auctions

According to the theory of common value auctions, a company which bids optimally will not on average incur losses. The optimal bid has to account for the fact that, given cost efficiency, there is a trade-off between the margins a company can demand and the likelihood of winning. While putting everything at stake of winning may drive profits down until they are equal to profits from alternative investments, under optimal strategy profitability cannot go below that threshold. The threshold may be close to zero, especially in a setting where there is one main supplier of contracts. However, unless companies set more complicated objectives than long-term profit maximization, government contract winners should not do worse than their competitors.

Figure 1.3 presents a simple argument that the above economic intuition does not apply in my setting. The figure shows the average profit margins of winners and runners-up in competitive auctions for road construction. The roughly parallel behaviour of winners and runners-up diverges after the auction award and winners' performance deteriorates. This finding motivates the more rigorous econometric analysis that follows.

The regressions in this section are estimated based on Equation 1.1. Table 1.3 summarises the results. I find a strong, negative effect of winning a competitive government auction on company's performance in the two years following the award. Profit margins drop on average by 2.30% in the post-period relative to profit margins earned by companies that placed second in the bidding process. These results are confirmed by other profit measures. Return on assets based on earnings before tax and net income drop by 4.39% and 4.62%, respectively. The fall in return on assets scaled by pre-award assets is slightly smaller, but also significant.

Poor performance of firms that we describe as *winners* is puzzling. My results indicate that it is winning a new contract that causes the deterioration in profitability. Contracts signed upon winning in a competitive procurement process are underpriced. As the contractual price is directly linked to the bid submitted by a winner, it implies that firms overbid.

There is a concern that my results partially capture the marginal effect. Standard economic theory suggests that a firm doing many projects chooses first the most profitable ones. As my treatment group includes firms winning several auctions in a year, their average profitability could relatively fall due to decreasing productivity of capital. My setting, however, is more complicated because there is only one supplier of contracts, and firms do not have a variety of projects to choose from. Theoretically, to distinguish the average and the marginal effect I would have to look at the return on capital employed with and without the contract, but these values are unobservable. I briefly address this issue in Section 1.8.3, where I focus on the number of auctions in which a company participates each year.

1.4.2 Results in the matched sample

One of the main identification concerns mentioned in Section 1.3.1 refers to the fact that firm performance and treatment status are correlated because of the selection bias related to the firm size. Bigger firms can carry out more projects at the same time and participate in auctions more often. This positively affects their probability of treatment. Firms' characteristics discussed in Section 1.3.2 also suggest that there might be a size effect in my sample. To alleviate the concern that the results from the previous section capture the heterogeneity in firm performance related to the firm size and not the impact of auction winning, I evaluate performance based on the matched sample. I collapse the data at the firm-year level and match firms using the propensity score (Rosenbaum and Rubin (1983)). Variables underlying the conditional probability of treatment include firm size, auction impact, and the auction award year. Collapsing the data helps capturing the overall situation of a firm in a given year and automatically prohibits matching a company with itself.

The exact matching methodology and estimation algorithm I employ were proposed by Ho et al. (2007, 2007b). I measure firm size by average total assets in the two years before an auction award. The auction impact is defined as auction size over firm size one year before the award. The auction size is measured by the price offered by the winner. I enforce exact matching on the auction year. I apply a logit model to estimate the conditional probability of treatment based on the listed covariates. I discard treated observations that fall outside support and match with replacement. Following Austin (2011), I specify the maximum distance between the two observations that could be potential pairs not to exceed 0.2 of a standard deviation of the distance measure (logit of a propensity score). Distribution of the propensity score can be found in the Appendix 1.12.2 (Figure A.3).

The regressions and tests performed on the matched sample have one weakness. As pointed out by Abadie and Imbens (2016), standard errors should account for the fact that the propensity score used in the matching procedure is itself estimated. I do not introduce any correction for propensity score estimation, which may create a bias in the obtained estimates.

In the first test on the matched sample, I compare the average performance of winners and runners-up using the t-test. I average the pre- and post-award values and compare the changes over time. Table 1.4 presents the results of the t-test. The difference between the average performance of winners and runners-up before and after the auction is negative and significant. Profitability of all companies falls but the drop in the case of winners is stronger.

In the second step, I run two regressions on the matched sample. The first specification represents a weighted regression:

$$
\begin{aligned}
y_{ikt} &= \beta_{Post}Post_t + \beta_{Win}Win_{ik} + \beta_{dd}Post_t \times Win_{ik} + FirmFE_i \\
&\quad +YearFE_{f(k,t)} + \epsilon_{ikt}. \qquad (1.2)
\end{aligned}
$$

y_{ikt} is the performance of firm i, which participates in an auction in year k. Like in the baseline specification (Equation 1.1), t measures the periods relative to the award, Win_{ik} identifies winners, and $Post_t$ distinguishes the pre-award years form the post-award years. The parameter of interest is denoted β_{dd}, and captures the causal effect of winning an auction on firm performance after the award compared to the performance of runners-up. I include firm and year fixed effects. Weighting reflects the matching stage and ensures the balance in firm size and auction impact between treatment and control groups. Standard errors are clustered at the firm level.

The second regression includes fixed effects ($PairFE_p$) to identify pairs of matched observations:

$$
\begin{aligned}
y_{ipt} &= \beta_{Post}Post_t + \beta_{Win}Win_{ip} + \beta_{dd}Post_t \times Win_{ip} + PairFE_p + \\
&\quad +YearFE_{f(p,t)} + \epsilon_{ipt}. \qquad (1.3)
\end{aligned}
$$

y_{ipt} is the performance of firm i from pair p. Standard errors are clustered at the pair level.

Table 1.5 summarises the estimation results for both approaches. In line with the t-test results, the impact of an auction award is negative and significant. The average drop in profit margin amounts to -2.5%. This number is very close to the estimates of the main regressions summarised in Table 1.3. Even if there is a size effect, the fact that treated observations are on average bigger does not affect the conclusions about firm performance following an auction win.

1.5 Main hypothesis

Poor performance of auction winners suggests that firms overbid in government procurement auctions. In this section, I propose and test a novel explanation of these results. Financially constrained firms intentionally bid aggressively and submit underpriced offers that generate losses in the long term. They compromise future payoffs aiming to improve their cash flow position in the short term. It is possible because the contracts I consider ensure regular payments. Polish government payments are rarely delayed because in order to start procurement procedures authorities need to arrange in advance resources for the project. It is true for the Polish government and it is also a standard characteristics of procurement in many other countries.[12] In addition, according to the evidence of practitioners, contracts are often more profitable in the beginning because firms can differently price tasks itemized in the specification. Namely, they can set higher margins on the tasks done in the very beginning. Manipulating margins does not lower the chances of signing a contract because offer evaluation depends only on the final price. Regular payments and possibility of cash flow front-loading provide incentives for firms to start new projects even when they are unprofitable overall.

If the hypothesis is correct, firms that overbid are more financially constrained. Overbidding and front-loading cash flows also should be profitable in the very beginning, especially if firms receive first payments calculated based on higher margins. Finally, the decline in performance should be weaker in auctions with high bid guarantees. The requirement to pay an upfront deposit may discourage most constrained firms that have the strongest incentives to overbid. I analyse these three implications to test whether the strategic overbidding hypothesis is a plausible explanation.

1.5.1 Cash constrained firms overbid

In the first test I check whether cash-poor firms are more likely to bid more aggressively. I compare pre-award financial constraints measures of aggressive bidders

[12] However, it is not true everywhere (for example, Brazil).

and other companies. Compared to the first stage of the empirical analysis I shift the focus from the two firms at the top of the procurement auction ranking to the whole population of construction firms in Poland.

I define aggressive bidders as firms which ranked first or second in an auction in a given year. It is natural to assume that aggressive bidders rank at the top in the bidding process. However, aggressive bidding does not necessarily guarantee that a bidder wins because more than one aggressive bidder may be present in an auction. My definition is in line with the previously stated assumption that winners and runners-up are similar (also in their bidding strategies) and their treatment / control status is assigned randomly.

The challenge of the analysis is to construct an appropriate control group. Theoretically, my control group consists of all companies that could have participated in government auctions or participated and did not overbid, that is, ranked lower. Since the intention to participate is not directly observable, I widen this theoretical definition and include all Polish construction firms. More specifically, I pick all Polish construction firms from the Orbis database that have non-missing values of cash flows in the relevant years.[13] The big caveat is that the category *Construction* may also include companies mostly dealing with housing that usually operate in the private sector and less often apply for public contracts. The available data do not allow me to distinguish between different construction branches. However, all branches use the same natural aggregates suppliers, and also the upcoming Football Championships affected the housing industry via hotel infrastructure development, so time effects across branches are likely to be similar. To further refine the sample, I match observations on firm size and long-term debt.[14] By doing so, I intend to exclude small firms ineligible for bidding and big, highly levered companies operating only in the housing sector. My matching methodology mirrors the approach presented in Section 1.4.2. I use the advantage of having a big pool of potential control firms and

[13] Not all auction firms are described as *Construction* firms. Some entities belong to other sectors, for example, *Metals and metal products* or *Machinery, equipment.* However, *Construction* firms account for 94.05% of the sample, so limiting the analysis to this single sector is reasonable.

[14] Observables used for matching are measured as the average value in the two year before the auction award.

match three control observations for each treated one. Within the caliper range, I choose the control observations that are closest in terms of Mahalanobis metric calculated using matching variables (total assets or total assets and long-term debt). This step has not been added section 1.4.2. By matching firms closest in terms of observables I intend to make up for the lack of information about the intention to participate. It was not necessary previously because I matched companies that I knew chose to participate and bid aggressively. In that scenario all treatment and control companies were comparable in terms of the intention to bid by definition, and the main concern was size imbalance.

The regression I run in this part is specified below:

$$AggressiveBidder_{it} = \alpha + \beta Constraints_{it} + \epsilon_{it}. \tag{1.4}$$

$AggressiveBidder_{it}$ is a dummy recording whether firm i in year t is an aggressive bidder, that is, whether it placed at the top (first or second) of the bidding ranking in a given year. Variable $Constraints_{it}$ measures severity of financial constraints of firm i. Since there is no consensus in the literature how to measure financial constraints, I consider several alternatives: cash holdings scaled by fixed assets, cash flow growth,[15] share of current liabilities in total liabilities, sales growth, and current ratio. All variables are fixed one year before the auction award. Standard errors are robust. The parameter of interest, β, measures whether financial constraints predict aggressive bidding.

The estimation results are presented in Table 1.6 for the sample matched based on firm size and in Table 1.7 for the sample matched based on firm size and long-term debt. Low cash holdings, low cash flow growth, low sales growth, high share of current liabilities in total liabilities, and low current ratio before an auction increase the probability that a company places first or second in a government procurement auction. Apart from the cash flow growth, the results are even more pronounced in

[15] Defined as

$$CFgrowth_{it} = \frac{CashFlow_{i,t-1} - CashFlow_{i,t-2}}{|CashFlow_{i,t-2}|}. \tag{1.5}$$

the sample matched both on assets and debt. Since high leverage is typical of the housing industry, the improvement suggests that including companies from other construction branches undermines the effect.

The overall results support the hypothesis that there are differences in cash positions of companies ranking high in the government auctions and other companies. Firms ready to bid aggressively for new contracts are more cash constrained. Even though it is not obvious how to measure firms' financial constraints most accurately, my results are consistent irrespective whether I rely on cash-, sales- or liabilities-based measure.

Because of the limited set of variables, I cannot analyse whether aggressive bidders were more constrained also before 2010 or whether this pattern is specific to my observation window. If the link between cash condition of the firm and winning existed outside my observation window, it would imply that liquidity aspect is always present in the bidding strategy. It would not mean, however, that margins are always driven below zero.

1.5.2 Short-term benefits

The second testable implication of the hypothesis is that firms perform better shortly after signing a contract. They exploit regular payments and front-load cash flows ensured by winning a procurement auction. Annual data do not allow for a very detailed analysis of profits over time. To shed more light on potential short-term benefits, I compare differences in the dynamics of the treatment effect depending on the length of the project. Contract deadlines significantly vary from one month to five years, as shown in Table 1.1. A five year observation window around the award date may not record the whole impact of the longest contracts, but this limit is necessary to avoid capturing other effects. For projects lasting a few months, early benefits would be quickly levelled out when the contract is finalised. In this case, the annual measure should not capture any positive impact. Long-term contracts give more scope for manipulation. It is difficult to predict how long companies will experience relatively high cash inflows, but the overall treatment effect should be at least weaker and could be reversed in the first year following the auction award.

Figure 1.4 presents how performance of winners relative to runners-up changes over time depending on contract lengths. The plot depicts coefficients on interactions between a treatment dummy (Win) and time-around-award dummies. Red-dashed lines separate pre- and post-award periods. The last point in the *pre-* period is the award year. There are no differences in performance of winners and runners-up in the years preceding the award. After the award winners of shorter contracts experience an immediate drop in profit margins. For long-term contracts, on the contrary, profit margins spike in the award year. The evidence is only suggestive because the data at an annual frequency do not allow me to clearly determine whether the observed increase captures an influence of the award or any other event from before contract signing. To bypass this issue, I look only at long-term contracts and compare the impact of auctions resolved in the first half of a year and auctions resolved in the second half of a year. Figure 1.5 depicts the results. For auctions finalised in the beginning of the year, relative performance of the winners drops below zero already in the year after the award. For auctions finalised at the end of the year, the positive spike also appears in the year following the award, and performance deteriorates only in year two.

The temporary increase in performance is consistent with the hypothesis that cash constrained firms strategically overbid in procurement auctions in order to exploit short-term benefits of signing a new contract. I identify the benefits of long-term contracts only, but I presume the benefits of short-term contracts are not captured by the data at annual frequency. The weakness of the evidence is that the samples underlying the plots are small, so the test has little power and standard errors of the coefficients are big. It is reassuring, however, that the same patterns are observed in the analysis of cash flows. Analogical plots based on cash flows can be found in Appendix 1.12.2 (Figures A.4 and A.5).

1.5.3 Disciplining role of deposits

A bid guarantee is a deposit which a firm has to pay in order to participate in an auction. It is set to ensure that firms submit serious offers and do not withdraw when other opportunities arise. If a firm leaves a bidding process after the official offers opening or does not sign the granted contract, it loses the deposit paid as a bid guarantee. Financially constrained firms are most likely to participate in auctions in which a small (or none at all) bid guarantee is required. If financial constraints underlie also aggressive bidding, then the cheapest offers and the worst performance following the award should be observed in auctions with small bid guarantees. I explore whether the variation in bid guarantees induces the suspected heterogeneity in the treatment effect.

The challenge of the test is to construct a meaningful measure of bid guarantees. Bid guarantees are required in almost 86% of auctions in my sample and their value is strongly correlated with auction size.[16] Auction size is correlated with contract length, and they both determine to what extent firms are able to exploit short-term benefits of contract signing. The observed short-term benefits lead to the relatively lower impact of bigger auctions because the subsequent decline is not fully captured in the short observation window. To discover an unbiased measure of the impact of the bid guarantee, I must control for auction size.

I propose two measures of bid guarantees. The first one is calculated within auction groups determined by auction size demi-deciles. For each group I define the deposit measure as a binary variable equal to zero (one) for auctions with a deposit below (above) the median calculated within the group. The second measure is calculated separately in the subsamples of short-term contracts with deadlines less than eight months and long-term contracts with deadlines of eight months or longer. This division is built on the conclusions from the previous section, where I showed that long-term contracts bring visible short-term benefits. I then define a dummy variable equal to one for auctions with deposits above the third quartile

[16]The correlation is not surprising because, according to procurement law, bid guarantees are set at the value of 0.5%–3% of the estimated contract size.

calculated in the subsample and zero otherwise. With these measures at disposal, I estimate the triple difference regression specified as follows:

$$
\begin{aligned}
y_{ijt} = & \beta_{Post} Post_t + \beta_{Win} Win_{ij} + \beta_D Deposit_j + \beta_{dd} Post_t \times Win_{ij} + \\
& + \beta_{PostD} Post_t \times Deposit_j + \beta_{WinD} Win_{ij} \times Deposit_j + \\
& + \beta_{ddd} Post_t \times Win_{ij} \times Deposit_j + AuctionGroupFE_{g(j)} + \\
& + YearFE_{f(j,t)} + \epsilon_{ijt}. \qquad (1.6)
\end{aligned}
$$

The notation is consistent with that used in Equation 1.1. The dummy $Deposit_j$ is a bid guarantee measure. It equals one for auctions with high bid guarantees and zero otherwise. The coefficient of interest, β_{ddd}, captures the impact of winning an auction with a high deposit requirement on the subsequent performance of winners relative to runners-up and auctions with a low deposit requirement.

Table 1.8 presents the regression estimates. The results for both deposit measures are consistent. Similarly to the baseline specification, there is a clear negative effect of winning an auction on post-award performance. It is not true for auctions with high deposits though. The coefficients on the triple interaction term (β_{ddd}) are positive and the magnitudes are big enough to reverse on average the initial drop in profit margin measured by β_{dd}. The positive deposit impact is significant in the case of the first deposit measure and in the subsample of short-term contracts in the second deposit measure. The estimation outcome favours the hypothesis about the disciplining role of bid guarantees that lower the scope of overbidding of cash-poor firms. The results are not very sharp, but the impact of bid guarantees has to be limited because their values are also limited. Too big deposit values could undermine the competition, and bid guarantees are not introduced to create barriers to entry into a bidding process. They are to exclude participants that do not really care about winning.

1.6 Bad luck of overbidding?

Previous section presented the evidence in support of the idea that firms may strategically submit low-price offers. The proposed story explains the documented poor performance of auction winners with firms' tendency to overbid when they face liquidity issues. An alternative hypothesis states that firms bid optimally but perform poorly because of a negative shock that hits the economy or the industry. This story has several testable implications. First, *winning by losing* arises in response to unexpected and unfavourable changes in economic conditions and thus cannot be an equilibrium outcome. Second, an identifiable shock affects winning firms. I consider two potential negative shocks: the financial crisis (global shock) and a local cost shock. Finally, there is a reason why runners-up seem to be unaffected by the shock. In the case of a global shock it could be because of survivorship bias. This concept assumes that all firms are equally affected by the shock, but due to increased bankruptcy rates of runners-up the performance evaluation is inaccurate as the worst-performing runners-up leave the sample. Firms losing government contracts perform better only conditional on survival. The probability of bankruptcy, on the other hand, is lower for winners that are able to continue operations at lower margins. In the case of a local cost shock, it is possible that runners-up are not affected because they stay idle as outside options are limited and mostly comprise of smaller contracts. I discuss each of the three implications in the following sections.

1.6.1 Firm performance in different years

It is not possible for firms to constantly incur losses and still operate on the market. Even making *relative losses* compared to the competitors would be difficult to reconcile with rationality. Especially for companies in the road construction industry, in which the government is basically the sole supplier of contracts. Therefore I suspect that *winning by losing* is a temporary phenomenon and test for its presence outside my observation window. It is not a test that can unequivocally determine whether the *bad luck* story is true because better performance of winners in other years may be consistent with several other hypotheses as well. However, pinning down the timeline of poor performance of public auction winners may help identifying any negative shocks relevant for the *bad luck* hypothesis

To investigate the effect in different years, I extend the sample so that it also covers auctions from 2008 and 2009.[17] In order to understand whether post-award performance of auction winners changed around 2010 I run a triple difference regression specified as follows:

$$\begin{aligned} y_{ijt} &= \beta_{Post} Post_t + \beta_{Win} Win_{ij} + \beta_{dd} Post_t \times Win_{ij} + \\ &+ \beta_{PostT} Post_t \times After'09_j + \beta_{WinT} Win_{ij} \times After'09_j + \\ &+ \beta_{ddd} Post_t \times Win_{ij} \times After'09_j + AuctionGroupFE_{g(j)} + \\ &+ YearFE_{f(j,t)} + \epsilon_{ijt}. \end{aligned} \tag{1.7}$$

It is an extension of Equation 1.1 and the notation is consistent. The dummy $After'09_j$ identifies auctions resolved after 2009. It does not appear separately in the regression because it is absorbed by auction group fixed effects ($AuctionGroupFE_{g(j)}$). My model has two coefficients of interest, β_{dd} and β_{ddd}. β_{ddd} measures the impact of the contract award on winners after the auction resolution, compared with firms that placed second and relative to auctions resolved in 2008 and in 2009. By comparing coefficients β_{dd} and β_{ddd} I intend to determine whether the impact of winning an auction on firm performance changed over time. Specifically, whether it is different in years 2008–2009.

Table 1.9 presents the regression estimates. For all performance measures (profit margin, return on assets and return on assets scaled by pre-award assets) the two coefficients of interest are significant at the 1% level. Winners of government procurement auctions in general perform better than runners-up following the award as indicated by the positive coefficients on $Post \times Win$. The same is not true for auctions resolved after 2009 because the coefficients on the triple interaction term

[17] The extended sample has a limited pool of variables, and not all regressions can be run on the extended sample.

are negative. The effect observed for auctions from 2010 to 2014 is much stronger, leading to the negative impact of winning observed in my initial sample.

I also support the evidence from the table with a graphical illustration. Figure 1.6 plots the treatment effect separately for each award year. The red dashed line separates the initial data from the extended sample. In earlier years, winners' performance compared with that of runners-up was better. The most pronounced positive effect is for auctions resolved in 2008, whereas 2009 and 2010 constitute a transition period in which profit margins of winners drop below the profit margins of runners-up and continue to fall. For auctions resolved in 2012 the difference reaches almost 5% in terms of the profit margin. An even more pronounced pattern is observed for return on assets. Here, the caveat is that my specification does not account for sizes of supplied contracts and may be affected by big infrastructural investments. I have shown that long-term contracts bring short-term benefits which undermine the negative award impact measured in the two-year post-period. Disregarding size composition of contracts may be an issue for the estimate of the 2009 effect because, as presented in Figure 1.1, many very big investment projects were initiated that year. The adjustment could decrease the coefficient.

Figure 1.6 also raises concerns that poor performance of firms winning government contracts is driven purely by auction resolved in year 2012. To test this possibility I run the main regression (Equation 1.1) on the sample excluding auctions resolved in year 2012. The results are presented in Table A.5 in the Appendix 1.12.2. While there is a drop in magnitude of the coefficient measuring the impact of auction winning, the results stay negative and significant. Therefore they are not not driven by one year only.

1.6.2 Negative shock

The previous section confirmed the intuition that *winning by losing* is not valid outside the observation period. It is in line with the *bad luck* story and also, more generally, with the intuition that firms operating on negative margins cannot survive on the market. The magnitude of the decline in winners' performance in different years, however, contradicts the idea that the drop in global demand during the financial crisis in the European Union is behind the observed performance patterns. As shown in Figure 1.6, the main decrease is observed in 2012 and not in 2009, when the bust in Europe was most severe. It gradually arises after 2009, when a slow recovery of the European Union starts.

The *bad luck* story has also another weakness. As already mentioned in Section 1.2.1, the *crisis* in Poland's case was really a slowdown of a continuously growing economy. The Polish economy grew in 2009 by 1.3%[18] compared with a -4.9% aggregated loss of other EU countries.[19] Furthermore, the global downturn is likely to indirectly affect firms through the supply of contracts. It may be difficult to sign a contract, but conditional on signing a firm is in a comfortable position because the government does not withhold or delay payments. Summing up, the financial crisis cannot be responsible for poor performance of auction winners.

Another version of this hypothesis states that the profitability of the contracts won in procurement auctions was affected by a shock that was specific to the Polish construction industry. Preparations for the European Football Championship initiated many infrastructure investments in Poland and increased the demand for natural aggregates. I evaluate the link between the natural aggregates market and profitability of construction contracts by studying the volume of production of the natural aggregates. Production and price should be closely related because Polish firms rely on local suppliers due to high transportation costs. I rely on information about production because I do not have data on historical prices.

As depicted in Figure 1.7, the rapid increase in production in 2011 suggests there was high, possibly unexpected demand for building materials.[20] The supply-demand imbalance, stemming from the oversupply of government contracts, decreased after

[18]Percentage change in the first quarter of 2009 compared to the same quarter of the previous year.

[19]Source: Eurostat, Newsrelease Euroindicators, 22/2010 12 February 2010.

[20]The natural aggregates production reached its high record in 2011. The increase was to a large extent due to developments in construction. The main recipient of aggregates is the housing industry, which accounts for about 60% of demand. Infrastructure construction accounted for about 30% of demand during the boom (Kabzinski (2011)).

2011 when the production went back to its long-term trend. Hence the highest prices were most likely recorded in 2011 or earlier, and the construction industry might have been negatively surprised by the material costs at that time. This surprise would affect contracts signed in 2009 or 2010. However, auctions resolved in 2009 are not yet unprofitable for winners. The main drop in performance is recorded for auctions resolved in 2012, which depend on 2013 and 2014 prices. Therefore it is unlikely that material costs caused a crisis in the road construction industry. Furthermore, the industrial literature perceives 2011 as a highly profitable year for the aggregate production industry. It does not mention the *collapse* or *crisis* in the construction sector and indicates that movements on the materials markets were, to some extent, predictable.

1.6.3 Testing for survivorship bias

The previous subsection already provided evidence against the *bad luck* story. To complete the argument I test also the third implication of the hypothesis, that is, whether survivorship bias is present in my sample. If there is survivorship bias then the observed drop in winners' performance mechanically arises when badly performing runners-up go bankrupt. I check whether winners and runners-up leave my sample in a consistently different manner.

Table 1.10 presents the survivorship of firms in my sample. I measure survivorship with a simple approximation by counting non-missing entries of the profit margin data in my sample. First, I consider all firms with recorded profit margins in the auction award year. Subsequently, I analyse how many firms recorded profit margins in the next year and in the two years after the award. I do it separately for *winners* and *runners-up*. I rely on profit margin because it is a primary dependent variable in my analysis. It underlies the samples used for estimation and is among well-populated accounting variables in the Orbis dataset.

The results show that fractions of companies lost in years following the award do not significantly differ in the subsamples. The fractions of the lost companies vary from 7.14% to 7.2% in the first year and from 9.59% to 11.89% in the second year. In the second year the drop rate is actually lower in the *Runners-up* subsample. My considerations do not include firms for which no entries of profit margin exist. Missing data in the auction award year are unlikely to result from the liquidation of a firm because such firms cannot participate in auctions. Financial information is missing most likely for very small firms, which face no reporting requirements.

Additionally, I consider a possibility that survivorship bias reveals itself at the earlier stage of the data collection process, that is, when I match auction firms with the accounting data. Missing a whole company in the Orbis dataset could be due to the fact that they left the market. The Orbis database does not automatically drop firms that enter bankruptcy, but these firms may be more difficult to match as their name and legal status change during the liquidation. Therefore I check the merging success rates by firm's status. Table 1.11 presents the results.

Matching is successful for over 80% of firms and varies from 80.74% to 81.50% in the subsamples. The differences are very small and, contrary to the survivorship bias prediction, merging is slightly better for the *Runners-up* subsample.

In sum, both coverage of firms in the database and the percentage of missing entries, which I examine to learn whether a firm exists or left the market, do not depend on firm's rank achieved in the bidding process. The hypothesis that winners' performance is worse compared with that of runners-up because the worst runners-up leave the sample after the economy is hit by a negative shock is not supported by the data.

1.7 Winner's curse

The third hypothesis I consider is closely related to the setting I use. The winner's curse is a phenomenon that arises in common value auctions. Firms are not able to form an optimal strategy that aligns with their ex post beliefs about the contract value. They do not take into account that the winning offer is the most optimistic one and can deviate from the unbiased estimate of the auction subject.

In the case of government procurement auctions the winning offer is too cheap and the winner ends up signing an underpriced contract.

The main issue with the winner's curse explanation is that the phenomenon of auction winners being worse off is not permanent. For auctions resolved from 2008 to 2009, winning firms actually gain relative to firms that place second and miss the award. Assuming overbidding is involuntary, it is necessary to explain why the inability to submit an optimal offer changes over time. A fitting scenario is described in the existing literature. Dyer, Kagel, and Levin (1989) prove in an experimental study that executives in the construction industry (who regularly submit offers for projects in common-value-type public auctions) overbid in line with the winner's curse. Their performance improves with experience, and the authors conclude they have to adapt to bidding in real-life auctions.

The story of Dyer, Kagel, and Levin (1989) relies on the link between overbidding and experience.[21] Because experience drives the ability to adjust bidding strategies and submit optimal offers, experienced firms should suffer less from the winner's curse and as a result perform better after the award. I assume that this is true even if all bidders have to adapt their strategies in response to changes in economic conditions.

To examine the impact of experience on bidding ability I look at firm age. As presented in Table 1.12, the median company in my sample is 18 years old and only 5% are younger than five years old. My sample consists of mature firms, so the winner's curse effect should be weak or altogether absent. It is also confirmed by the manager's individual experience measured by age, which is available for a few companies. Information about managers' age is non-missing only for 5% of firms, but no manager is younger than 40 years old.

In a more rigorous test, I run a triple difference regression in which the third difference allows me to evaluate the impact of winning an auction on an experienced

[21]Other works prove, however, that impact of experience may indirectly reflect also other factors. For example, Da Silva, Dunne, and Kosmopoulou (2003) show that new entrants in auctions bid more aggressively and win with much lower prices than incumbent firms because of the higher cost dispersion. It is consistent with the idea that inexperienced bidders are more aggressive, but it abstracts from the winner's curse problem.

and an inexperienced firm. The specification is as follows:

$$
\begin{aligned}
y_{ijt} &= \beta_{Post} Post_t + \beta_{Win} Win_{ij} + \beta_E Exp_{ij} + \beta_{dd} Post_t \times Win_{ij} + \\
&\quad + \beta_{EPost} Post_t \times Exp_{ij} + \beta_{WinE} Win_{ij} \times Exp_{ij} + \\
&\quad + \beta_{ddd} Post_t \times Win_{ij} \times Exp_{ij} + AuctionGroupFE_{g(j)} + \\
&\quad + YearFE_{f(j,t)} + \epsilon_{ijt}. \qquad (1.8)
\end{aligned}
$$

The notation is consistent with that used in Equation 1.1. The dummy Exp_{ij} determines whether firm i in auction j is an experienced bidder. β_{ddd} measures the impact of a contract award on an experienced bidder after the auction resolution, compared with firms that ranked second and relative to inexperienced bidders. By looking at β_{dd} and β_{ddd} I can determine whether the decline in performance of winners depends on how experienced a company is.

I measure experience based on the past bidding record. I construct two alternative dummy measures. The first one is equal to one for firms that bid and ranked at the top two in the past two years. The second measure is equal to one for firms that won in the past two years. The idea behind this is that exposure to either the bidding process or winning a contract in an auction provides additional information about the optimal bidding strategy.

Table 1.13 summarises the estimation results. Experience does not affect performance of winners following the award. The caveat here is that the accuracy of measures is disputable because past participation and winning is correlated with other firm characteristics as, for example, firm size. I cannot exclude the possibility that the impact of experience is weaker than potential heterogeneity in the results coming from other firm characteristics.

Summing up, I do not find evidence in support of the winner's curse. There is no link between performance and experience and my sample consists of relatively mature firms that are less likely to suffer from the winner's curse. Furthermore, the hypothesis cannot explain the correlation between companies' cash flows and aggressive bidding documented in Section 1.5. It is unlikely that the winner's curse is responsible for the observed strong drop in performance of firms that win government contracts.

1.8 Robustness checks

In this section I test the robustness of the documented drop in winners' performance. First, I run the main regression on several subsamples to ensure the results are not driven by a small subset of firms. In Section 1.8.2 I address the identification issue related to firms that both win and rank second in the same year. In Section 1.8.3 I explore alternative restrictions on firms' participation. Finally, in Section 1.8.4 I discuss how accounting rules may affect the documented results.

1.8.1 Robustness in the subsamples

I re-run the main regression specified in Equation 1.1 on the subsamples to ensure the results are not driven by outliers, and I analyse the potential heterogeneity in the treatment effect. Table 1.14 presents the results.

The first column contains the estimates of the regression in which 4% of the smallest and largest auctions are dropped. The regression aims to address the concern that the effect is driven only by very small or very big contracts. It is a valid concern because of the huge variability in auction sizes. The size of an auction is measured in terms of the price offered by a winner. The threshold of 4% is arbitrary, but very similar results are obtained with the 2% and 8% cut-offs. In the second column I present the results when the estimation excludes a few unlimited companies in my sample. The majority of the firms in my sample have limited liability. If the excluded companies created the treatment effect, this would undermine the idea of strategic firm behaviour. Compromising long-term profits increases the probability of bankruptcy and should be less pronounced in behaviour of companies in which an owner is responsible for business debt. The third column presents the results for unlimited auctions only. As mentioned in Section 1.2.1, both limited and unlimited auctions ensure equality of all bidders and specify evaluation criteria in advance. The difference is that limited auctions are organised in two stages, and the first stage provides a signal about the competitiveness of an auction that can affect bidding. Different procedures could also attract a different pool of bidders. The results for these three subsamples do not differ from the main estimates. The impact of auction winning is significant and leads to a drop in the profit margin of around 2.5%.

The last column presents the results for high-impact auctions.[22] If the documented effect measures the auction influence on post-award company performance, then it should be stronger for relatively big auctions. As expected, the treatment effect is significant and bigger in magnitude - the drop in profit margin amounts to over 3%.

1.8.2 Winning and losing in the same year

Firms that participate in several auctions every year and both win and place second raise concerns about the identification validity, which I discussed in Section 1.3.2. To argue in support of the identification strategy, I perform two additional tests.

First, I run a regression on the subsample of firms that ranked first or second in an auction only once per year. It circumvents the problem that participation is a firm's choice that correlates with the treatment status. Table 1.15 presents the estimation results. Conclusions are consistent with the main results. Coefficient on the interaction term is significant and negative. The post-award performance of winners relative to runners-up decreases on average by 2.60%.

In the second approach, I consolidate the data at the firm-year level and define non-binary treatment intensity. The regression is specified as follows:

$$\begin{aligned} y_{ikt} \;=\; & \beta_{Post} Post_t + \beta_{WR} WinningRate_{ik} + \beta_{dd} Post_t \times WinningRate_{ik} + \\ & + FirmFE_i + YearFE_{f(k,t)} + \epsilon_{ikt}. \end{aligned} \tag{1.9}$$

[22] As defined is Section 1.4.2, auction impact is calculated separately for each auction-firm observation as winning offer price over firm's total assets one year before the award.

y_{ikt} is the performance of firm i, which participates in auctions in year k. Variable t measures time relative to the award. Variable $WinningRate_{ik}$ is defined as the cumulative dollar-size of won auctions in a particular year over the cumulative dollar-size of auctions in which a firm ranked first or second. I measure auction size by the price offered by a winner. $WinningRate_{ik}$ is zero for firms that have not won any competitive contract in a particular year. It is equal to one for companies that have not lost any auction conditional on ranking in the top two. For firms that placed both first and second in a particular year, the variable takes a value between zero and one, depending on the sizes of auctions. The observations are collapsed at the firm-year level, so I include firm and year fixed effects. Standard errors are clustered at the firm level. Table 1.16 presents the results. They are consistent with the earlier findings. The impact of an auction award on the subsequent firm performance when treatment is non-binary is negative for the majority of the considered performance measures.

1.8.3 Alternative participation restrictions

The main sample excludes five firms that participated and ranked in the top two of more than 40 auctions during the observation period. The number is set as a compromise between preserving the original sample and firm representativeness. Table 1.17 presents the results for the alternative cut-off definitions.

The first column shows the regression results when no participation restrictions are imposed. The second column presents the estimates for a subsample of firms that ranked in the top two in a maximum of 10 auctions in a year. The third regression excludes firms that won or placed second in more than 10 auctions in total from 2010 to 2014. All coefficients are significant and close to the baseline regression results.

The treatment effect is a little smaller in the absolute terms in the first subsample. It implies that firms that participate in many auctions in a given year perform slightly better afterwards. It may be related to having more opportunities to front-load cash flows after signing many contracts. However, better performance for frequent participants also provides the argument that results do not reflect the decreasing marginal efficiency of capital. If this was the case, then the observed decline in performance would be driven by companies that win many auctions in a given year and receive relatively lower returns due to limited investment opportunities. On the contrary, dropping companies with more than 40 appearances in the sample actually improves significance and increases the magnitude of the effect compared with the raw sample. Similarly, limiting the sample to companies that ranked in the top two in less than 10 auctions within a five-year period increases the magnitude of the drop in profitability without any loss of significance.

In the last three specifications I do not drop firms from the sample but limit firm appearances in the sample. The subsample *First appearance in a year* (the fourth column) includes only the first auction in which a firm participated in a given year. The last two subsamples take into account only one firm-auction observation in the whole observation period. The subsample *First appearance only* includes the very first appearance, while the subsample *One random appearance* picks one random appearance. I document the negative and significant treatment effect of auction winning in all the subsamples.

1.8.4 Accounting rules

Measuring performance using accounting variables poses a few risks. First, there may be delays in reporting or publishing data. Second, shifts caused by changes in accounting rules may occur. In both of these scenarios, worsening performance would not reflect firms' strategic actions. I address these concerns in the following paragraphs.

The first scenario is unlikely because of the system of payment claims described in Section 1.2.1. Companies immediately receive payments for their work, so the impact of the signed contract should be recorded in the considered observation window. Also, in the case of Poland the delays in government payments are unlikely because public authorities need to organise and allocate resources before the auction is resolved. Even an economic downturn does not affect the financing of the ongoing projects but rather the future supply of contracts.

Mechanical changes in the accounting rules are most likely to arise after changes in tax rates. The corporate tax rate in Poland has been stable since 2005 and equals 19%. The sales tax rates changed in 2011 with the general rate increasing from 22% to 23%. This change is not responsible for the relative poor performance of winners for several reasons. First, the reform was announced in the middle of year 2010. To be affected by this change, contracts would have to be signed before July 2010 and last longer than six months. Offers submitted in auctions finalised after that date would take the reform into account. Second, as offers contain net prices, tax rates, and gross prices, any changes arising because of a change in sales tax rate are easy to measure. For the small pool of affected auctions this would be one of the very rare scenarios in which a price could be adjusted after the contract is signed. It could be possible because of the exogenous nature of the change of circumstances and the fact that it would not undermine competitiveness of any firm. Furthermore, an increase in the gross price would not shift the economic balance in favour of the winner as winners' remuneration relies on net prices.[23]

1.9 Conclusions

In this paper I consider the hypothesis that cash constrained firms buy liquidity in exchange for future profits. I test this hypothesis in the context of government procurement auctions for which I document poor performance of winning companies. Profit margins of winners drop on average by 2.30% in the two years following the award. This number is measured relative to the firms that placed second and lost in the bidding process. My methodology provides an unbiased estimate of the

[23] Arguments regarding sales tax rate change rely on the official law interpretation published by The Public Procurement Office. Source: *https://www.uzp.gov.pl/baza-wiedzy/interpretacja-przepisow/opinie-archiwalne/zmiana-umowy-w-sprawie-zamowienia-publicznego-w-zwiazku-z-ustawowa-zmiana-stawki-podatku-od-towarow-i-uslug-vat* (accessed 17.08.2018).

performance drop. The results are robust in various subsamples and when observations are matched on firm size and auction impact. The magnitude of the drop is economically significant as construction companies usually do not operate on margins above 10%. The empirical analysis suggests that firms ranking at the top in procurement auctions overbid.

I find several pieces of evidence in support of the idea that documented overbidding reflects the intertemporal trade-off that liquidity constrained firms face. First, pre-award low cash holdings, low cash flow growth, low sales growth, low current ratio or high share of current liabilities predict the top position in the bidding ranking. It implies that aggressive bidders are financially constrained. Second, in the case of long-term contracts the drop in performance is delayed and I observe a temporary positive impact on profitability shortly after the auction resolution. It confirms the existence of short-term benefits that provide incentives for aggressive bidding. Third, the observed overbidding is driven mostly by auctions which do not require high bid guarantees. Setting an upfront deposit as a participation requirement discourages the most constrained firms from bidding, leading to submission of less underpriced offers and relatively better performance after contract signing.

I also take into account alternative explanations of the deteriorating performance of winners of government contracts. I consider the hypothesis that firms bid optimally ex ante but are hit with a negative shock realization after the auction award. I reject this possibility because the timing of potential negative shocks is inconsistent with the magnitude of the decline in firm performance in different years. Furthermore, I analyse the possibility that results reflect the winner's curse. I do not find evidence in support of this hypothesis because in my setting experience does not significantly improve firm performance. However, I do not have enough evidence to reject it. In reality, both strategic overbidding and the winner's curse may exist and lead to worse performance of auction winners. A natural follow-up step is to measure which effect is stronger. My setting provides more evidence in favour of the strategic firm behaviour.

My results emphasize the importance of the liquidity-profitability trade-off faced by cash constrained firms. The drop in profits does not lead firms to bankruptcy in my sample as I do not find differences in survivorship of winners and runners-up. The overall effect on welfare, however, is ambiguous. The direct, short-term effects are positive: overbidding does not increase bankruptcy rate, government pays lower prices, and firms improve cash flows. However, aggressive bidding also affects the other market participants. It creates a pressure on offered prices, which impacts bidding strategies and potentially cash flows of other companies. Price pressure may also decrease competition because low expected profit margins discourage companies from auction participation or new entrepreneurs from entering the market.

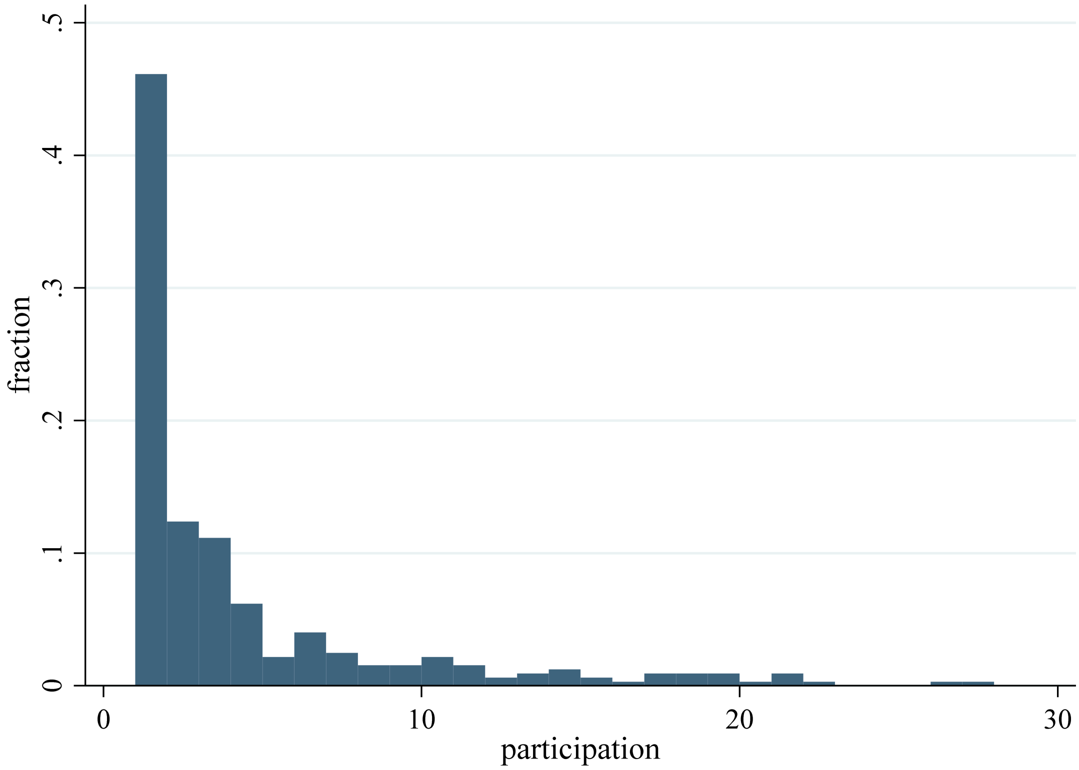
Although my unique dataset allows for a comprehensive analysis of the procurement auction bidding strategies, the dataset of firms' financials does not ensure full coverage of auction firms. Thus my conclusions may not be easily generalised to small firms, which are underrepresented in the final sample compared to how often they participate in public auctions. This limitation also does not allow me to study the role of limited liability, which could add value to considerations about intentional overbidding. On the other hand, the analysis supplies a valid benchmark for evaluation of auction winners' performance and a non-behavioural explanation of the *winner's curse*. The conclusions I draw may be relevant in other settings because they rely on the analysis of company's core activity - the main and direct source of firm's profits and losses.

1.10 Figures

Figure 1.1: Auctions by year.

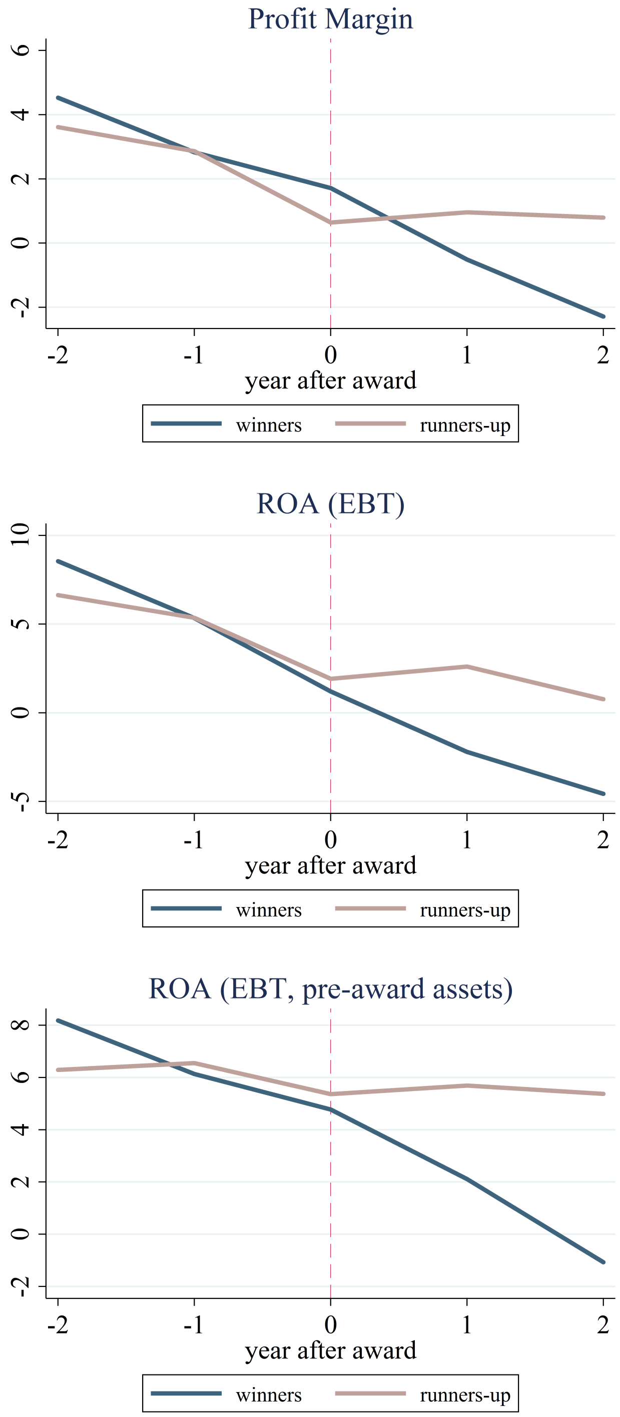
Notes: This figure presents characteristics of procurement auctions by award year. *Offer price* refers to the price offered by the auction winner. Sample: limited and unlimited auctions resolved resolved from 2008 to 2014.

Figure 1.2: Firms' participation in auctions.



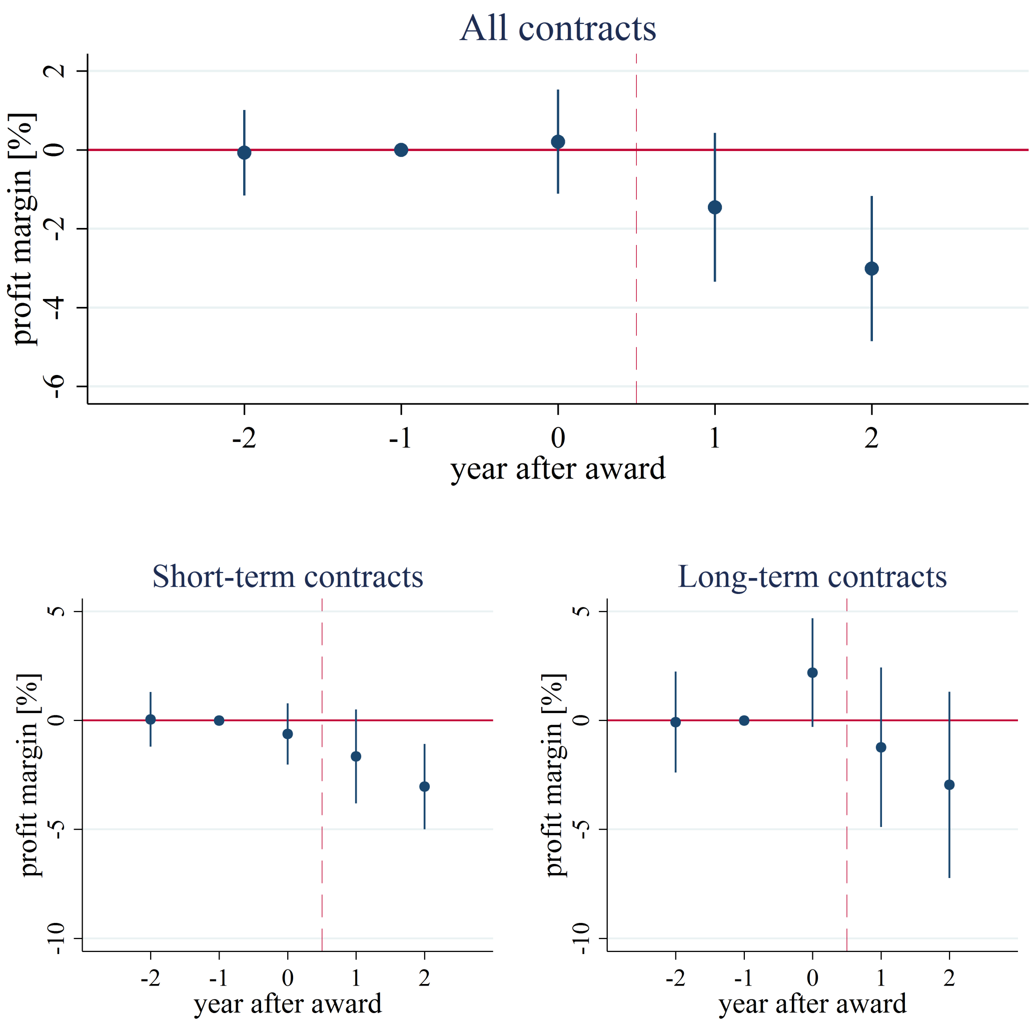
Notes: This figure presents frequencies with which the same firm re-appears in my sample in different auctions. A firm is counted when it ranks in the top two. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014.

Figure 1.3: Firm performance around the award year by rank.



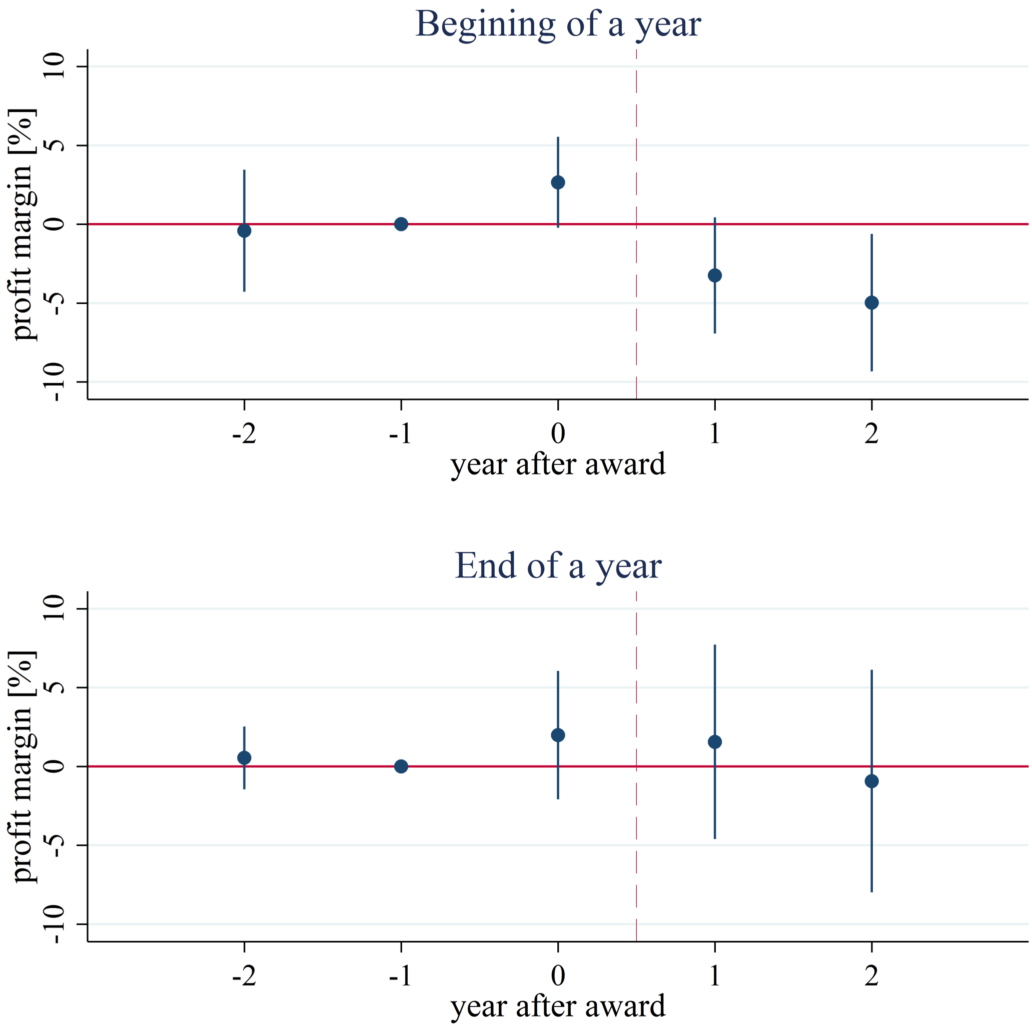
Notes: This figure presents the average performance of firms winning procurement auctions (*winners*) and firms that rank second and do not win other auction (*runners-up*) around the award year. Year 0 is the award year. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure 1.4: Treatment effect around the award year.



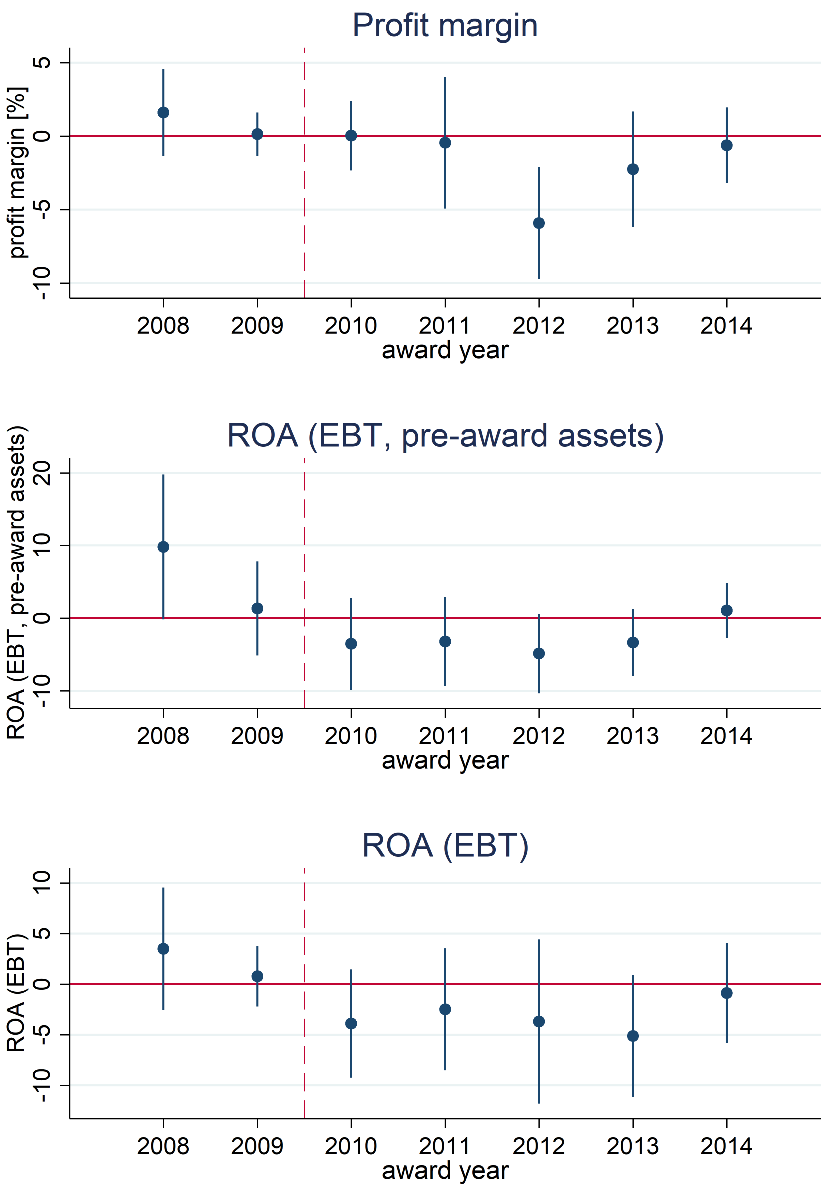
Notes: This figure shows the treatment effect (the impact of winning an auction) separately for each year around the auction award. The dashed line distinguishes the pre-award period from the post-award period. The top panel presents the sample of all contracts; the bottom-left panel presents short-term contracts lasting fewer than eight months; the bottom-right panel presents long-term contracts lasting eight months or more. The dependent variable is defined as: profit margin = EBT/sales. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure 1.5: Treatment effect around the award year: Long-term contracts.



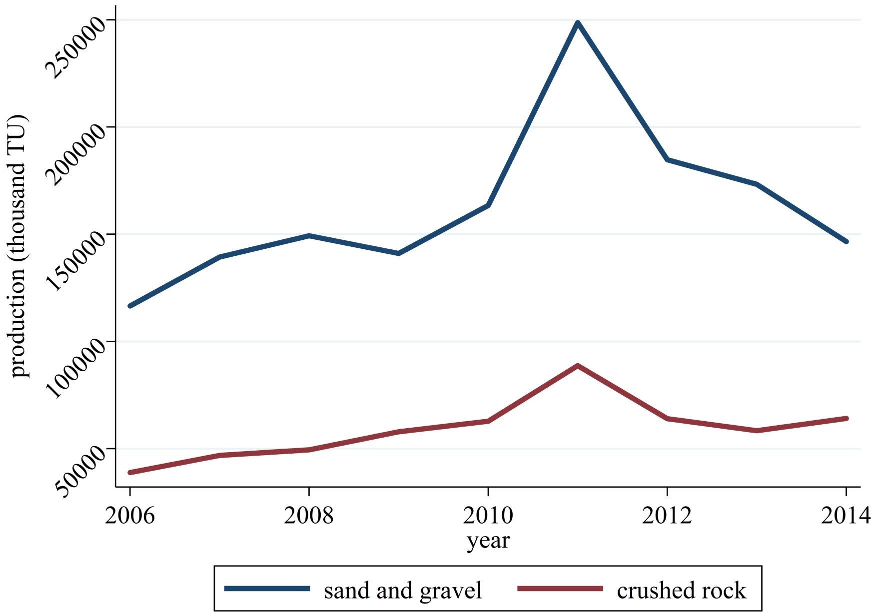
Notes: This figure shows the treatment effect (the impact of winning an auction) separately for each year around the auction award for long-term contracts. The dashed line distinguishes the pre-award period from the post-award period. The top panel presents contracts resolved in the beginning of the year (first half of the year); bottom panel presents contracts resolved in the end of the year (second half of the year). The dependent variable is defined as: profit margin = EBT/sales. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure 1.6: Treatment effect by auction award year: Auctions from 2008 to 2014.



Notes: This figure shows the treatment effect (the impact of winning an auction) separately for each auction resolution year. The dashed line distinguishes the main sample from the additional auction years. The dependent variables are defined as: profit margin = EBT/sales, ROA (EBT) = EBT/total assets, ROA (EBT, lagged assets) = EBT/(average total assets before award). Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure 1.7: Natural aggregates production in Poland.



Notes: This figure presents the production of two main natural aggregates (construction materials) in Poland from 2006 to 2014. The figure is based on Table 1 from Koziol, Cieplinski, Machniak, and Borcz (2015).

1.11 Tables

Table 1.1: Procurement auctions from 2008 to 2014.

	mean	sd	p50	min	max
Price [million PLN]	26.2	133.5	0.71	0.0061	2195.9
Number of bidders	3.92	2.81	3	1	25
Contract length [months]	7.18	9.08	3	1	60
Deposit [million PLN]	0.26	1.28	0.0075	0	15
Observations	3441				

Notes: This table shows summary statistics of procurement auctions. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014.

Table 1.2: Firm characteristics by rank.

	Winners		Runners-up		T-test	
	mean	sd	mean	sd	diff	t-stat
Total assets	73.8	(257.29)	64.5	(297.01)	9.26	(0.40)
Employees	168.5	(258.13)	138.3	(198.01)	30.2	(1.26)
Turnover	120.7	(357.21)	99.0	(381.64)	21.7	(0.70)
EBT	3.95	(22.11)	1.72	(15.43)	2.23	(1.34)
NI	2.87	(18.07)	1.14	(12.05)	1.73	(1.28)
ROA (EBT) [%]	7.12	(11.56)	7.00	(14.11)	0.13	(0.12)
ROA (NI) [%]	5.62	(10.44)	5.57	(12.78)	0.053	(0.056)
Cash flow	6.81	(21.41)	3.56	(18.29)	3.24	(1.64)
Cash flow/turnover	0.060	(0.06)	0.059	(0.08)	0.0016	(0.24)
Equity	24.7	(76.51)	24.7	(105.31)	-0.031	(-0.0041)
Long-term debt	4.97	(31.93)	5.23	(43.59)	-0.25	(-0.075)
Current liabilities	38.5	(138.76)	31.1	(141.11)	7.42	(0.63)
Solvency ratio [%]	42.7	(22.61)	44.1	(23.15)	-1.36	(-0.71)
Current ratio	1.76	(1.20)	1.96	(1.77)	-0.20	(-1.61)
Profit margin [%]	3.64	(6.66)	3.24	(7.93)	0.40	(0.66)
Auctions	916					
Firms	315					

Notes: This table shows summary statistics of firms bidding in competitive auctions. Averages are calculated over the two years before an auction award. Non-ratio variables are expressed in millions of PLN. The columns *diff* and *t-stat* present the results of a t-test of the differences in means between *Winners* and *Runners-up*. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Subsample *Winners*: firms that placed first in an auction in a given year. Subsample *Runners-up*: firms that placed second and did not place first in an auction in a given year. Data are winsorized at the 0.1% level.

Table 1.3: Firm performance after auctions.

	Profit margin	ROA (EBT)	ROA (NI)	ROA (EBT, pre-award assets)	ROA (NI, pre-award assets)
Post	1.14	2.85*	3.11*	1.39	1.16
	(0.79)	(1.57)	(1.59)	(1.62)	(1.39)
Win	0.36	0.30	0.38	0.16	0.34
	(0.49)	(1.07)	(1.09)	(1.30)	(1.30)
Post x Win	-2.30***	-4.39***	-4.62***	-4.16***	-3.58***
	(0.75)	(1.45)	(1.46)	(1.37)	(1.13)
Group FE	X	X	X	X	X
Year FE	X	X	X	X	X
Obs	4310	4435	4430	4435	4435

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance. The specification is presented in Equation 1.1. The dependent variables are defined as: profit margin = EBT/sales, ROA (EBT) = EBT/total assets, ROA (NI) = net income/total assets, ROA (EBT, lagged assets) = EBT/(average total assets before award), ROA (NI, lagged assets) = net income/(average total assets before award). The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.4: Firms' profit margins after auctions - t-test: Matched sample.

Winners	Runners-up	t-stat	p-value
-3.96	-1.27	-2.6834	0.007

Notes: The table shows the t-test of the mean change in performance of *Winners* and *Runners-up* after an auction award. Observations are collapsed at the firm-year level. Firms are matched based on average pre-award *assets*, *auction impact*, and *auction award year*. The matching procedure follows that of Ho et al. (2007). The dependent variable is defined as: profit margin = EBT/sales. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Subsample *Winners*: firms that placed first in an auction in a given year. Subsample *Runners-up*: firms that placed second and did not place first in an auction in a given year. Data are winsorized at the 0.1% level.

Table 1.5: Firms' profit margins after auctions: Matched sample.

	Probability Weights	Pair Fixed Effects
Post	0.93	0.73
	(0.78)	(0.71)
Treat	1.12**	-0.059
	(0.53)	(0.52)
Post x Treat	-2.52**	-2.51***
	(0.99)	(0.78)
Year FE	X	X
Firm FE	X	
Pair FE		X
Obs	2670	3560

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance. The specification of the first model is presented in Equation 1.2. The regression is weighted to ensure covariate balance. Standard errors are clustered at the firm level. The specification of the second model is presented in Equation 1.3. It includes pair fixed effects to identify matched observations. Standard errors are clustered at the pair level. Observations are at the firm-year level and matched based on firms' average pre-award *assets*, *auction impact*, and *auction award year*. The matching procedure follows that of Ho et al. (2007). The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.6: Predictors of aggressive bidding: Sample matched on firm size.

	(1)	(2)	(3)	(4)	(5)
Cash/FixedAssets	-0.011***				
	(0.0041)				
CF growth		-0.025**			
		(0.012)			
SalesGrowth			-0.079***		
			(0.014)		
CurrentLiab./TotalLiab.				0.66***	
				(0.12)	
CurrentRatio					-0.11***
					(0.024)
Obs	2519	1749	2565	2744	2697

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of financial constraints on the probability of aggressive bidding in an auction. The specification is presented in Equation 1.4. Observations are matched based on *year* and firm's average *pre-award assets*. The matching procedure follows that of Ho et al. (2007). The estimations are based on a logistic function. The regressions are weighted with sampling weights obtained during matching. The dependent variable $AggressiveBidder$ is a dummy equal to one for firms that ranked first or second in a relevant auction and zero for all other Polish construction firms. Relevant auctions are defined as limited and unlimited auctions with three or more participants resolved from 2010 to 2014. Firms participating in more than 40 auctions from 2010 to 2014 are excluded from the group of aggressive bidders. Constraints measures are fixed one year before the auction award. Equation 1.5 defines the variable $CFgrowth$. Standard errors are robust. Data are winsorized at the 1% level.

Table 1.7: Predictors of aggressive bidding: Sample matched on firm size and long-term debt.

	(1)	(2)	(3)	(4)	(5)
Cash/FixedAssets	-0.023**				
	(0.0089)				
CF growth		-0.021			
		(0.020)			
SalesGrowth			-0.11***		
			(0.020)		
CurrentLiab./TotalLiab.				0.33**	
				(0.15)	
CurrentRatio					-0.16***
					(0.028)
Obs	2018	1353	2011	2122	2088

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of financial constraints on the probability of aggressive bidding in an auction. The specification is presented in Equation 1.4. Observations are matched based on *year*, firm's average *pre-award assets*, and average *long-term debt*. The matching procedure follows that of Ho et al. (2007). The estimations are based on a logistic function. The regressions are weighted with sampling weights obtained during matching. The dependent variable *AggressiveBidder* is a dummy equal to one for firms that ranked first or second in a relevant auction and zero for all other Polish construction firms. Relevant auctions are defined as limited and unlimited auctions with three or more participants resolved from 2010 to 2014. Firms participating in more than 40 auctions from 2010 to 2014 are excluded from the group of aggressive bidders. Constraints measures are fixed one year before the auction award. Equation 1.5 defines the variable *CFgrowth*. Standard errors are robust. Data are winsorized at the 1% level.

Table 1.8: Disciplining role of deposits: Profit margin.

	Deposit 1 All contracts	Deposit 2 Short-term contracts	Deposit 2 Long-term contracts
Post	1.86*	1.55	2.35
	(1.07)	(1.11)	(2.34)
Win	0.52	-0.16	1.65
	(0.73)	(0.82)	(1.09)
Deposit	0.75	1.48	0.35
	(0.90)	(1.20)	(1.52)
Post x Win	-4.06***	-3.24***	-3.95*
	(1.19)	(1.13)	(2.28)
Post x Deposit	-1.82	-2.39*	-3.98
	(1.26)	(1.40)	(2.55)
Win x Deposit	-0.18	0.23	0.69
	(0.96)	(1.16)	(2.05)
Post x Win x Deposit	3.85**	3.70*	4.76
	(1.60)	(1.84)	(3.23)
Group FE	X	X	X
Year FE	X	X	X
Obs	4485	3155	1330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance for auctions with high and low deposit requirements. The specification is presented in Equation 1.6. The *Deposit 1* measure is a dummy equal to one for auctions with deposits above the median calculated within the auction group based on auction size demi-deciles. The *Deposit 2* measure is a dummy equal to one for auctions with deposits above the third quartile based on the subsample of short-term / long-term contracts. Short-term contracts are defined as contracts with deadlines below 8 months. Long-term contracts defined as contracts with deadlines of 8 months or more. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.9: Firm performance after auctions: Auctions from 2008 to 2014.

	Profit margin	ROA (EBT)	ROA (EBT, pre-award assets)
Post	-2.12**	-3.86**	-6.53**
	(0.91)	(1.91)	(2.94)
Win	-2.28**	-4.77**	-6.19**
	(0.92)	(2.08)	(2.77)
Post x Win	2.61***	5.33***	8.40***
	(0.81)	(1.72)	(2.83)
Post x After'09	3.28***	6.53***	8.45**
	(1.18)	(2.40)	(3.28)
Win x After'09	2.74**	5.36**	6.56**
	(1.05)	(2.35)	(3.05)
Post x Win x After'09	-4.98***	-9.71***	-12.7***
	(1.11)	(2.26)	(3.15)
Group FE	X	X	X
Year FE	X	X	X
Obs	6135	6245	6245

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance for auctions resolved in different years. The specification is presented in Equation 1.7. The dependent variables are defined as: profit margin = EBT/sales, ROA (EBT) = EBT/total assets, ROA (EBT, lagged assets) = EBT/(average total assets before award). The dummy Win identifies firms that won an auction in a given year. The dummy $Post$ distinguishes the pre-award period from the post-award period. The dummy $After'09$ distinguishes auctions resolved in 2008 or 2009 from those resolved after 2009. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2008 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.10: Firm survivorship.

Year after award	Winners		Runners-up	
	No	Drop-rate	No	Drop-rate
Year 0	308	.	236	.
Year 1	286	7.14%	219	7.20%
Year 2	254	11.89%	198	9.59%

Notes: This table shows the percentage of missing entries of profit margin in the financial database in the years following an auction award. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014. Subsample *Winners*: firms that placed first in an auction in a given year. Subsample *Runners-up*: firms that placed second and did not place first in an auction in a given year.

Table 1.11: Merging success rates by rank.

	Winners	Runners-up
Matched	696	569
Unmatched	166	131
Matched (%)	80.74%	81.29%

Notes: This table shows the percentage of merged firms listed in the auction data files with the Orbis financial database. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014. Subsample *Winners*: firms that placed first in an auction in a given year. Subsample *Runners-up*: firms that placed second and did not place first in an auction in a given year.

Table 1.12: Firm age distribution.

	All	Winners	Runners-up
p5	4	5	4
1st quartile	12	12	10
median	18	18	16
3rd quartile	21	21	21
p95	60	59	61

Notes: This table shows firm age distribution. Age is estimated based on the incorporation year from the Orbis financial database. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014.

Table 1.13: Firm performance after auctions: Experience.

	Participated before	Won before
Post	1.97**	1.62**
	(0.91)	(0.80)
Win	0.28	0.20
	(0.70)	(0.63)
Exp	-1.44	-1.67
	(0.95)	(1.05)
Post x Win	-3.24***	-2.51***
	(1.03)	(0.95)
Post x Exp	-1.93	-1.51
	(1.26)	(1.38)
Win x Exp	0.54	0.97
	(1.09)	(1.19)
Post x Win x Exp	2.06	0.99
	(1.59)	(1.75)
Group FE	X	X
Year FE	4330	4330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance for experienced and inexperienced bidders. The specification is presented in Equation 1.8. The dummy *Exp* identifies experienced firms. In the column *Participated before*, the experience measure equals one for firms that ranked first or second in the two years before an auction resolution. In the column *Won before* the experience measure equals one for firms that ranked first (won) in the two years before an auction resolution. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.14: Firms' profit margins after auctions: Subsamples.

	No price outliers	Only limited liability	Unlimited auctions	Auction impact above 20%
Post	1.68**	1.89**	1.63*	2.02
	(0.80)	(0.84)	(0.83)	(1.44)
Win	0.51	0.46	0.48	-0.17
	(0.50)	(0.53)	(0.52)	(0.82)
Post x Win	-2.59***	-2.92***	-2.47***	-3.19**
	(0.78)	(0.78)	(0.80)	(1.36)
Group FE	X	X	X	X
Year FE	X	X	X	X
Obs	3960	3675	3855	1135

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance based on the various subsamples. The specification is presented in Equation 1.1. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Main sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Subsample *No price outliers* excludes 4% of the biggest and smallest auctions measured by auction size. Subsample *Only limited liability* excludes unlimited liability firms. Subsample *Unlimited auctions* excludes *Limited auctions* with a two-stage selection of winners (defined in detail in Section 1.2.1). Subsample *Auction impact above* 20% excludes auction-firm observations in which the auction size is smaller than 20% of the firm size. Data are winsorized at the 0.1% level.

Table 1.15: Firms' profit margins after auctions: Firms participating once per year.

	Profit margin
Post	0.38
	(1.25)
Win	0.77
	(0.99)
Post x Win	-2.60**
	(1.25)
Group FE	X
Year FE	1525

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This regression shows the impact of winning an auction on firms' subsequent performance in the subsample of firms which participate only in one auction per year. The specification is presented in Equation 1.1. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy $Post$ distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in an auction only once per year. *Participation* is defined as firm's appearance in the top two places of the auction ranking. Data are winsorized at the 0.1% level.

Table 1.16: Firm performance after auctions: Consolidated data.

	Profit margin	ROA (EBT)	ROA (NI)	ROA (EBT, lagged assets)	ROA (NI, lagged assets)
Post	0.17	0.16	0.17	1.60	0.76
	(0.61)	(1.22)	(1.19)	(1.39)	(1.08)
WinningRate	0.90	0.78	1.02	1.21	1.05
	(0.59)	(1.09)	(1.05)	(1.04)	(0.87)
Post x WinningRate	-2.14**	-2.98	-3.44	-3.78*	-3.35**
	(1.01)	(2.05)	(2.10)	(2.04)	(1.69)
Year FE	X	X	X	X	X
Firm FE	X	X	X	X	X
Obs	2995	3070	3065	3070	3070

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance. Equation 1.9 presents the specification. Data are consolidated at the firm-year level. $WinningRate$ is between zero and one and measures treatment intensity. It is defined as the dollar size of auctions won in a given year to auctions in which a firm ranked in the top two. The dependent variables are defined as: profit margin = EBT/sales, ROA (EBT) = EBT/total assets, ROA (NI) = net income/total assets, ROA (EBT, lagged assets) = EBT/(average total assets before award), ROA (NI, lagged assets) = net income/(average total assets before award). The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the firm level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table 1.17: Firms' profit margins after auctions: Participation restrictions.

	No restrictions	Less than 10 auctions per year	Less than 10 auctions overall	First appearance in a year	First appearance only	One random appearance
Post	0.88	1.10	0.89	0.46	1.60	1.00
	(0.73)	(0.78)	(0.99)	(0.87)	(1.01)	(1.24)
Win	0.51	0.23	0.43	0.24	0.36	0.14
	(0.47)	(0.49)	(0.60)	(0.58)	(0.77)	(0.98)
Post x Win	-1.82**	-2.14***	-2.60***	-1.72**	-2.33**	-2.03*
	(0.70)	(0.75)	(0.98)	(0.76)	(1.09)	(1.18)
Group FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Obs	5135	4355	2630	3015	1555	1585

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firms' subsequent performance depending on participation restrictions set on the sample. Equation 1.1 presents the specification. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Main sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014. *Participation* is defined as firm appearance in the top two of the auction ranking. Subsample *No restrictions* does not exclude any companies. Subsample *Less than 10 auctions per year* includes firms participating in a maximum of 10 auction per year. Subsample *Less than 10 auctions overall* includes firms participating in a maximum of 10 auctions over the observation period. Subsample *First appearance in a year* includes only the first firm's appearance in a year. Subsample *First appearance only* includes only the first firm's appearance in the sample. Subsample *One random appearance* includes only one random firm's appearance in the sample. Data are winsorized at the 0.1% level.

1.12 Appendix

1.12.1 Data collection

All auction data are collected from the website of the General Directorate for National Roads and Highways (GDDKiA). In November 2017 I downloaded all tender reports available under the link *www.gddkia.gov.pl/ajax/zamowieniePubliczne Szczegoly.php?id=X* by looping over subpages for each auction, where "X" denotes auction ID number. The collection process was performed in two stages. First, I collected all information directly published on the website. Then I extracted information from the downloaded supplementary files.

Website data

Data available on the website contains basic information about the auctions. The information is presented in a semi-structured form, that is, the website layout and row titles are usually the same for all auctions. Downloaded data include: ID number, auction number, the number of submitted offers, the types of works covered, ordering authority, winning firm name and address, resolution date, winning offer price, auction announcement date, deadline, deposit, winning criteria, offer with lowest price, offer with highest price, reasons of cancellation, and auction group. I exclude auctions that were cancelled or that fell outside the relevant observation window. I also try to identify and exclude multitask auctions by searching for words like *part*, *task*, *number* in variables *winner*, *cancel reason*, *price*, *deposit* or *number of participants*. The auction deadline is defined in number of months (weeks) or given as a completion date. In the case of the latter, I approximate the deadline in months by looking at the auction resolution date. In the case of joint offers, I identify the leader and partners and list them separately in my dataset. I focus on *unlimited* and *limited* auctions for *construction*. At this stage I already drop auctions with fewer than three participants. Any missing entries are filled in by hand.

Extraction from files

I begin by downloading all the files entitled *Information about the best offer choice*. These files are usually in the ".pdf" and, occasionally, in the ".doc" or the ".jpg" format. They contain information about all the participants, prices they submit, and score they get. The ".pdf" files are *scanned*, so the data they contain needs to be extracted via Optical Character Recognition (OCR). I use the Adobe Acrobat XI Pro program and transform files into the ".xls" format. Many of these files contain a table with the ranking. I identify whether this is the case by looking for a variable that would denote *Offer number* and extract the whole table, row by row. The column names differ across files but I identify among them *firm name*, *offered price*, and *offer evaluation*. Auctions for which I cannot identify these columns are set aside for a manual check. The same goes for auctions for which most of the row entries are missing. I rank offers according to the points awarded and drop auctions with fewer than three participants.

Final improvements

The file with extracted data is then merged with auction information obtained in the first stage. I use the merged file to compare winners identified from the ".pdf" files and the winners' names published online. I manually correct any discrepancies. I supplement the file with the data typed in by hand.

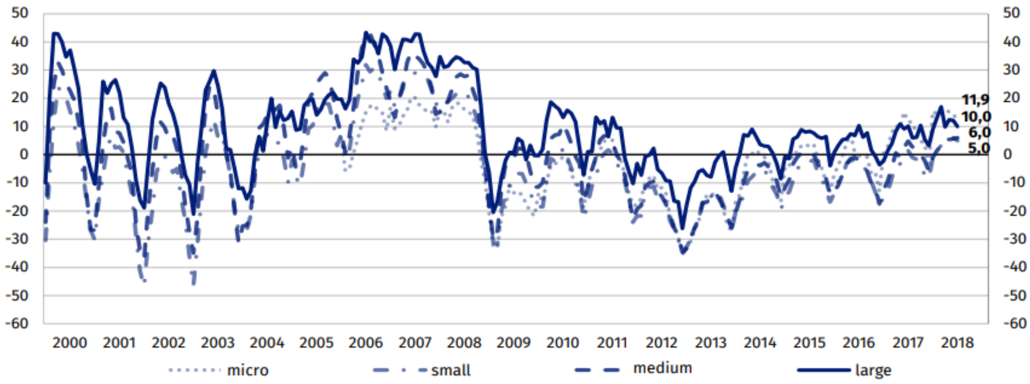
I end up with a file in which the data are at the auction-firm level. Each auction has at least one winner (or more in cases of joint offers) and one runner-up (or more in the case of joint offers or ties). The data about auctions include dates, deadlines, winning offer prices, the number of competitors (initial and after potential exclusions), deposits and point differences between winners and runners-up.

Merged sample

As mentioned in Section 1.2.3, firms' financial data is merged with auction data by their name, city, and legal form. If the last two do not agree, I check firm's history in the Polish National Court Register (KRS), which has an excellent coverage and is available online. The ID number for Polish firms in the Orbis database relies on the official register number from the Polish Central Statistical Office also used by National Court Register. It lowers the risk of confusing two companies with a similar name or a company that changed its name. Table A.1 presents percentages of successfully merged auctions with firm data by auction award year.

I focus on public auctions from 2010 to 2014. The end of the observation period is dictated by the data availability. The beginning is set to 2010 in order to capture the period of relative stability in the construction industry. Figure A.1 depicts the overall situation in the Polish construction industry, including that road construction. It shows that the industry was thriving around 2007. The boom phase lasted a few years and ended around 2009. Section 1.6.1 makes a comparison between auctions by year, including 2008 and 2009. An extended sample has fewer variables available and worse data coverage.

Figure A.1: Boom in general construction industry around 2007.



Notes: This figure shows business tendency in general construction. There was a boom in construction around 2006–2008. Source: Business tendency in industry, construction, trade and services from 2000 to 2018, July 2018, pp.18, Polish Central Statistical Office.

Table A.1: Merging success rates by auction award year.

Award Year	2008	2009	2010	2011	2012	2013	2014
Merged	91.28%	87.17%	86.03%	90.10%	90.23%	87.62%	87.77%

Notes: This table shows the percentage of successfully merged auction observations with the financial data by auction award year.

1.12.2 Other Tables and Figures

Table A.2: Firm performance after auctions: Auction pairs.

	All contracts	Contracts below 10 million PLN
Post	-0.46	0.77
	(1.05)	(1.03)
Win	0.68	-0.12
	(0.74)	(0.74)
Post x Win	-0.80	-2.30*
	(1.14)	(1.26)
Auction FE	X	X
Year FE	X	X
Obs	1675	1295

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of winning an auction on firm's subsequent performance. The specification is presented in Equation 1.1. Group FE are replaced with Auction FE and the sample includes only auctions for which the data on both winner and runner-up are available. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; no restriction on firms' participation to avoid decreasing the sample size. Subsample *Contracts below 10 million PLN* excludes auctions in which winning offer is above 10 million PLN. Data are winsorized at the 0.1% level.

Table A.3: Firm performance after auctions: Controlling for firms participating only once per year.

	Profit margin
Post	1.37
	(1.10)
Win	0.66
	(0.55)
Post x Win	-2.45***
	(0.91)
One	0.80
	(0.51)
One x Post	-0.41
	(0.97)
Group FE	X
Year FE	
Obs	4330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This regression shows the impact of winning an auction on firm's subsequent performance. The specification is presented in Equation 1.1, but additionally includes controls for infrequent participation - *One* and $One \times Post$. The dummy *One* identifies firms that in a given year participate only once. The dependent variable is defined as: profit margin = EBT/sales. The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Table A.4: Firm performance after auctions: EBITDA.

	EBITDA (pre-award assets)
Post	1.62
	(2.19)
Win	-0.081
	(1.70)
Post x Win	-4.04*
	(2.22)
Group FE	X
Year FE	X
Obs	3130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This regression shows the impact of winning an auction on firm's subsequent performance. The specification is presented in Equation 1.1. The dependent variable is defined as: EBITDA scaled by average total assets in two year before award. The dummy Win identifies firms that won an auction in a given year. The dummy $Post$ distinguishes the pre-award period from the post-award period. Errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; no restriction on firms' participation to avoid decreasing the sample size. Data are winsorized at the 0.1% level.

Table A.5: Firm performance after auctions: Without year 2012.

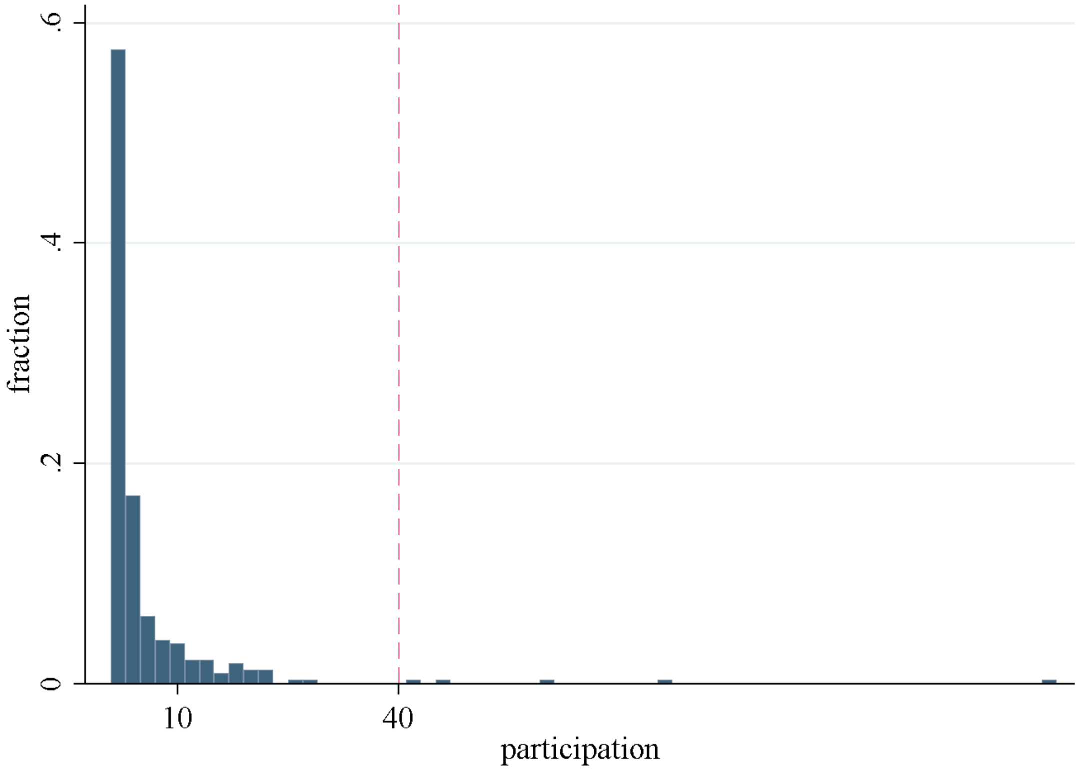
	Profit margin	ROA (EBT)	ROA (NI)	ROA (EBT, pre-award assets)	ROA (NI, pre-award assets)
Post	0.58	3.29*	3.34*	3.29*	2.68*
	(0.85)	(1.72)	(1.71)	(1.87)	(1.60)
Win	0.73	0.68	0.80	1.24	1.33
	(0.58)	(1.24)	(1.25)	(1.52)	(1.55)
Post x Win	-1.78**	-4.79***	-5.15***	-4.81***	-4.00***
	(0.82)	(1.57)	(1.57)	(1.65)	(1.33)
Group FE	X	X	X	X	X
Year FE	X	X	X	X	X
Obs	3280	3360	3355	3360	3360

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

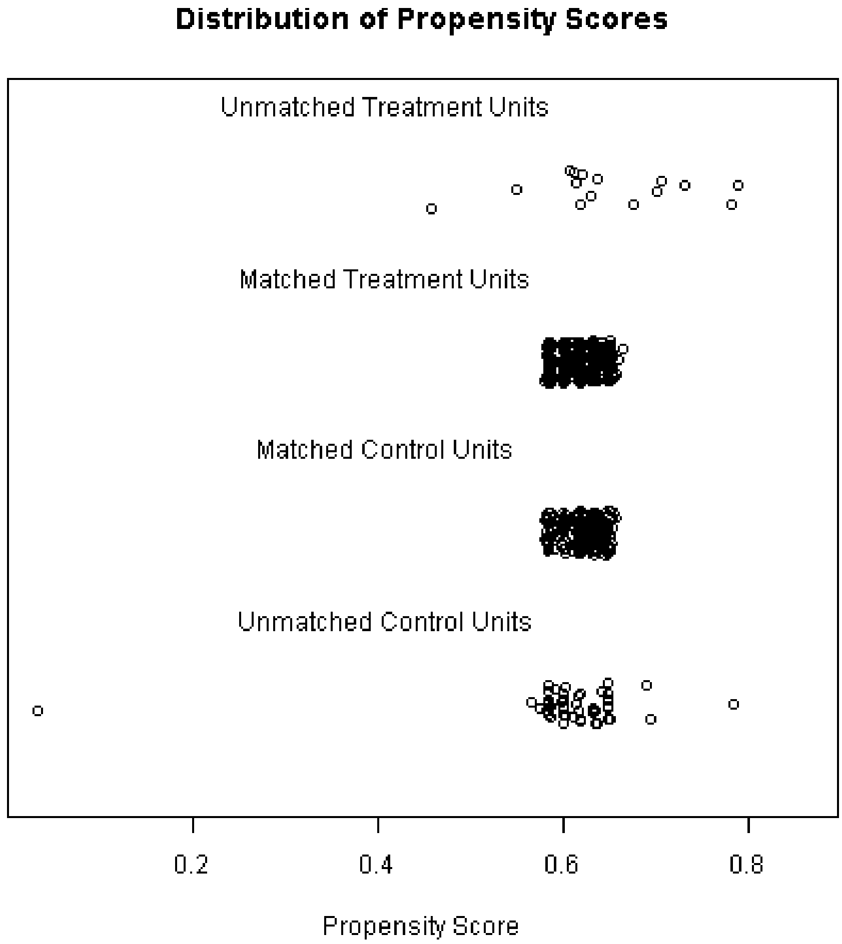
Notes: These regressions show the impact of winning an auction on firm's subsequent performance. The sample excludes auctions resolved in year 2012. The specification is presented in Equation 1.1. The dependent variables are defined as: profit margin = EBT/sales, ROA (EBT) = EBT/total assets, ROA (NI) = net income/total assets, ROA (EBT, lagged assets) = EBT/(average total assets before award), ROA (NI, lagged assets) = net income/(average total assets before award). The dummy Win identifies firms that won an auction in a given year. The dummy *Post* distinguishes the pre-award period from the post-award period. Standard errors are clustered at the auction group level. Sample: limited and unlimited auctions with three or more participants resolved in years 2010, 2011, 2013 or 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure A.2: Firms' participation in auctions: All firms.



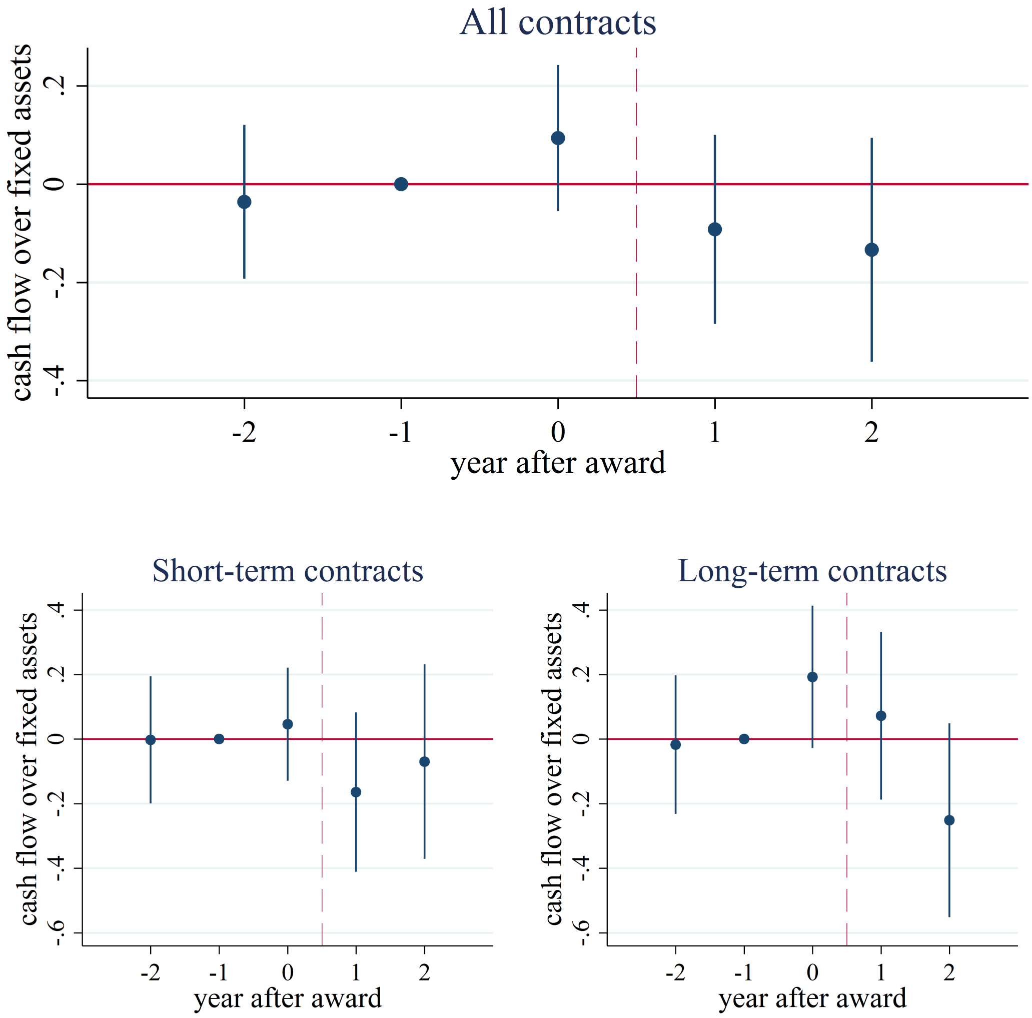
Notes: This figure presents frequencies with which the same firm re-appears in the sample in different years. A firm is counted when it ranks in the top two in an auction. The dashed line reflects the cut-off used in the main sample. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014.

Figure A.3: Propensity score distribution.



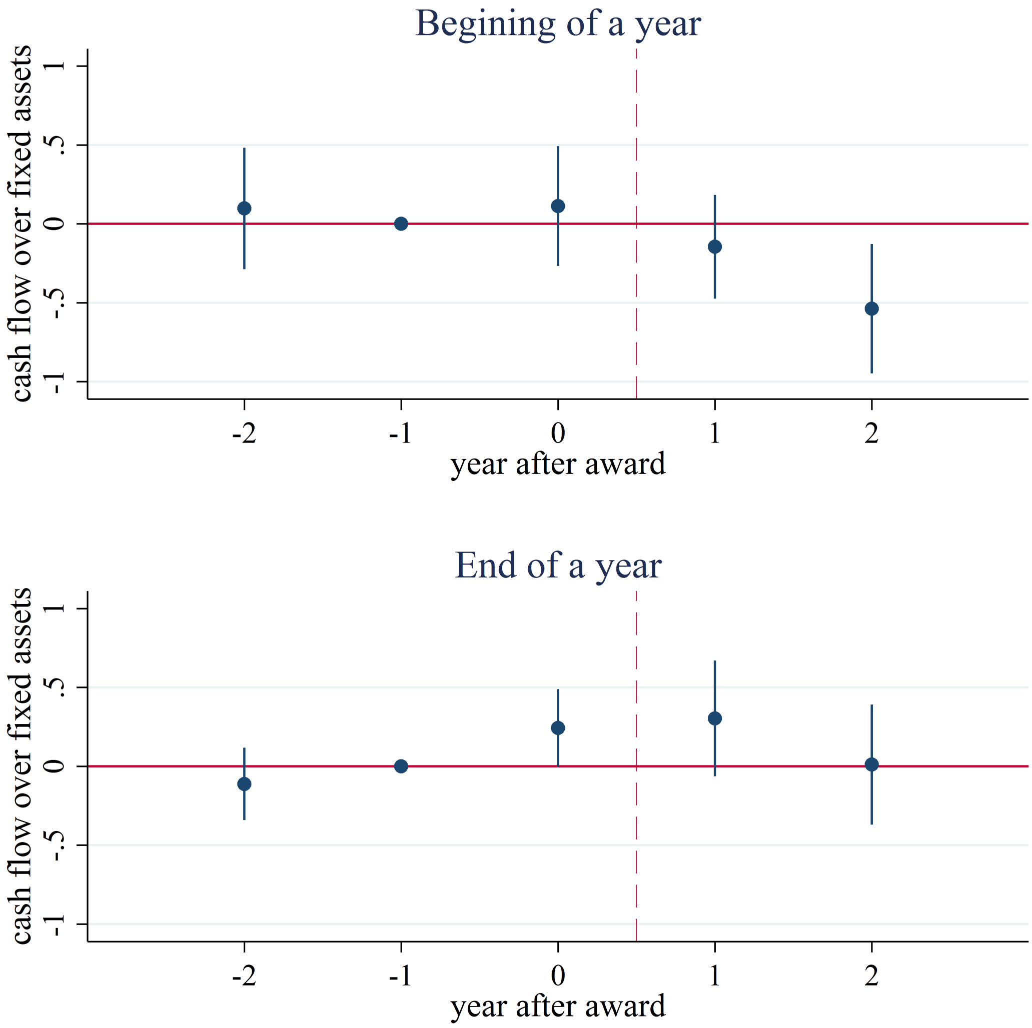
Notes: This figure presents the distribution of the propensity score in the raw and matched sample by rank. The matching procedure follows that of Ho et al. (2007). Firms are matched based on average pre-award *assets*, *auction impact*, and *auction award year*. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014.

Figure A.4: Treatment effect around the award year (Cash flows).



Notes: This figure shows the treatment effect (the impact of winning an auction) separately for each year around the auction award. The dashed line distinguishes the pre-award period from the post-award period. The top panel presents the sample of all contracts; the bottom-left panel presents the short-term contracts lasting fewer than eight months; the bottom-right panel presents long-term contracts lasting eight months or more. The dependent variable is defined as: cash flow (fixed assets) = cash flow/fixed assets. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Figure A.5: Treatment effect around the award year (Cash flows): Long-term contracts.



Notes: This figure shows the treatment effect (the impact of winning an auction) separately for each year around the auction award for long-term contracts. The dashed line distinguishes the pre-award period from the post-award period. The top panel presents contracts resolved in the beginning of the year (first half of the year); bottom panel presents contracts resolved in the end of the year (second half of the year). The dependent variable is defined as: cash flow (fixed assets) = cash flow/fixed assets. Sample: limited and unlimited auctions with three or more participants resolved from 2010 to 2014; and firms participating in 40 or fewer auctions from 2010 to 2014. Data are winsorized at the 0.1% level.

Chapter 2

Financial markets and regional economic development.

2.1 Introduction

In this paper we show that the heterogeneous response of the labour market to cash flow shocks reveals novel information about the credit market in the U.S. at the county level. The uniqueness of the study lies in the identification strategy, which is based on the announcement of reversing the direction of flow in a major crude oil pipeline. Specific structure of the event creates a one-year gap between the news of the shock and its realization. Thanks to that we can provide causal evidence that even in developed countries access to local financing can facilitate economic growth through job creation.

In recent years after the Great Recession more attention is paid to the role of financial intermediaries. It is believed that access to credit facilitated by banks influences firms' behaviour. However, there is little empirical research that would in a causal setup precisely estimate the extent to which those financial constraints are present. It is challenging to measure them directly due to issues with endogeneity, so finding the experiment was essential.

Financial intermediaries can be especially helpful in providing credit after an investment opportunity shock. Insufficient access to credit can lead to underinvestment and low growth. Consequences of inadequate local economic growth are dire and range from unemployment and forced labour mobility, spillovers from one sector to another, or even skewed outcomes of elections. However, it is worth investigating if this type of process is relevant to the U.S., a developed economy with plenty of big financial institutions that have vast resources to invest in firms and presumably face small financial constraints.

The difficulty in answering this question lies in finding a credible quasi-natural experiment that would allow to establish a causal relationship between access to financing and local employment levels. In the paper we exploit variation triggered by infrastructural developments in the oil industry that exogenously affected firms in a region called Bakken Formation, which occupies parts of Montana and North Dakota. The particular event that we use is the reversal of the direction of the flow of Seaway pipeline. It allowed to transport crude from Cushing, OK to refineries in Texas, reducing the oversupply in the storage hub and price disparities between domestic and foreign oil. The advantage of the approach relies on the specific structure of the event that creates a one-year gap between dates of the announcement, so the news of income shock, and the actual realization of the shock. It eliminates the problem of reverse causality and accentuates the role of financial constraints.

The paper shows that the presence of robust financial institutions at a county level in the U.S., which is considered a developed market, has potential to affect the responsiveness of local economic growth[1] to cash flow shocks. Furthermore, our results suggest that geographical closeness of lenders and borrowers that is likely to alleviate asymmetric information issues inherent in corporate financing plays a big role in ability to react to income shocks.

The intuition behind the result is as follows. All oil producers operating in the Bakken Formation learn that they are going to receive a positive cash flow shock in the future because their product will no longer be trapped in the storage hub in Cushing, OK. Those that are located within counties with relatively good access

[1]Due to data availability local development is measured by quarterly employment at a county level.

to finance can borrow funds in order to respond to the news immediately. New investment is accomplished by drilling new wells, which requires an increase in the number of jobs in the oil sector in the quarter of the announcement. Consequentially, we demonstrate that quarterly employment in the oil sector increases substantially more in those counties that have deep financial markets instantly. The value of the response that we find is relatively big which is consistent with the nature of the oil production process because employment is needed mostly during the drilling phase and can be subsequently greatly reduced. However, investment in counties with poor banking is delayed until the realization of the actual income shock, which corresponds to the time when the infrastructure project is completed.

The causal interpretation in the analysis relies on the specific nature of the shock. The gap between the news and the moment cash flows reach the regions creates incentives for firms rather than banks to respond at the time of the announcement. This is crucial in order to claim that the pre-existing banking network is not correlated with the future investment opportunities anticipated after the shock. Any other shock to the oil industry without such a specific timing structure would not allow us to disentangle the response coming from more productive counties from the one originating in less financially constrained counties. It is therefore necessary to control for other oil industry shocks.

Additionally, using constraints in infrastructure to study the employment response in the oil sector by exploiting heterogeneity in access to finance requires making some assumptions about the banking sector. Specifically, that the banking industry did not experience any significant development at the time of the announcement or that bank location and size are not endogenous. Since financial development is heterogeneous rather than exogenous, looking at differential response to the announcement can lead to distorted estimates due to underlying differences behind the development of the financial sector. Finally, even though there is little correlation between financial depth measures that are used in the paper and mean oil production before the gap in oil price indices emerged, one could be concerned that something happened in the banking sector (or the whole economy) at the same time as the reversal was announced, which would further bias the results.

In order to address those concerns, we extend the analysis by including another region, called Niobrara Formation, which was affected by the shale revolution and inadequate infrastructure but could not benefit from the Seaway pipeline reversal. Increased outbound capacity form Cushing, OK could benefit only producers well connected to the hub. Niobrara did not gain immediately after the reversal due to insufficient pipeline infrastructure at the time. Therefore firms operating in this region are expected not to react after the announcement even if they are located in counties with deep financial markets because they cannot benefit from the project.

The increase in employment observed in Bakken region compared to Niobrara is driven by counties with local access to deep financial markets, confirming the baseline results. The interaction term of announcement and banking measure is not significant, which suggests that the effect discovered in original difference-in-difference regression with Bakken region cannot be simply attributed to developments in banking sector.

Our main banking measure relies on the physical existence of commercial banks. However, the results are also consistent if we focus on the amount of commercial loans granted locally. The measures based on the amount of commercial assets available in the county has turned out inconsistent. It seems that it is not the availability of funds that matters but rather a willingness to finance oil projects which is probably best measured by a physical presence of a branch or historical lending activity. Those measures perhaps reflect the presence of bankers who have developed personal relationships with firms and know their businesses and oil industry overall.

Nonetheless, we find that even in developed countries regions with deeper financial markets, so those with more bank branches rather than those with more funds available, react faster to investment opportunity shocks.

Oil industry and in particular the U.S. shale revolution have been recently closely examined in an array of research articles. The scope of these papers ranges from descriptive studies documenting the impact of the boom (for example Decker (2016)) to structural analysis (for example Allcott and Keniston (2014) or Caldara et al.

(2018)) to the ones using oil and natural gas shale discoveries as a quasi-experiment to identify the influence of shocks on economic outcomes (for example Feyrer et al. (2017) who estimate the degree to which economic shocks propagate locally). Our paper adds to this body of work in a number of ways.

First, we complement the literature that studies the infrastructure in the oil sector. Borenstein and Kellogg (2014) as well as Kilian (2016) were among the first to discuss the economic consequences of inadequate infrastructure in the industry. They show that discrepancy between the Midwest and international crude oil prices did not pass through to gasoline prices. We start the analysis by examining the bottlenecks in the pipeline system. However, we differ from other authors by focusing specifically on the Seaway pipeline reversal and developing a quasi-experimental design. To the best of our knowledge we are the first to propose a pipeline reversal as an experiment in order to identify causal relationship in data.

Second, we target the link between the timing of investment in the infrastructure (and therefore employment in the industry) in response to positive income shocks. Kellogg (2014) and Anderson et al. (2018) provide crucial evidence on how drilling activity is connected to changes in employment in the sector. Our paper leverages on their findings by emphasizing the gap between the announcement and realization of the cash flow shock and positing when it is optimal for firms to react. This allows us to investigate whether access to local financing can facilitate economic development.

Our paper also adds to the growing empirical literature on the physical proximity between lenders and borrowers[2] and resulting financial constraints. Early papers, for example Petersen and Rajan (2002) or Berger (2003), based on the idea that innovation in banking and information technology (wide adoption of credit scores etc.) reduces importance of location, argued that proximity between lenders and borrowers is becoming less significant even in small business lending. This can be explained by improvements in lender productivity due to technological change which reduces the need for face-to-face interactions between the lender and borrower. Fur-

[2]For some recent studies see Brevoort et al. (2010) or Agarwal and Hauswald (2010).

thermore, some authors, for example Jayaratne and Strahan (1996), Kroszner and Strahan (1999), Bertrand et al. (2007) or Tewari (2014), have emphasized the effects of ongoing bank deregulation in facilitating growth, entrepreneurship, and lending to consumers.

However, more recent papers have documented significant correlations between distance and credit outcomes. This was largely done by looking at either survey data or data obtained from a single lender. Among papers that are most closely related to ours are Nguyen (2019), who uses exogenous variation in exposure to post-merger branch consolidation as an instrument to show how distance shapes credit allocation, or Bonfim et al. (2018), who use matching estimator to study lending relationships between firms and bank branches. In contrast, we use differences-in-differences approach in a plausible quasi-experimental design.

Finally, our paper relates to a number of recent studies which examined how the shale oil revolution can be used to investigate financial markets. Gilje (2019) shows that external finance dependence and presence of small banks shapes firms' response to shock in funds availability. Plosser (2014) provides evidence on how extra funds are allocated between liquid assets as opposed to loans. Gilje et al. (2016) document that banks exposed to shale booms have greater capacity to originate and hold new loans in non-boom counties. Gilje (2016) analyses how firms engage in risk-shifting when distressed. Our paper adds to this literature by providing completely different quasi-exogenous variation that can be used in future studies.

The rest of the paper is organized as follows: in Section 2.2 we present the setting and characteristics of the pipeline infrastructure in the U.S. as well as discuss the validity of the quasi-experiment used in the identification procedure. Section 2.3 describes datasets and sample selected to be used in the empirical part. Next section, 2.4, discusses methodology, including identification assumptions, empirical specification and controls, and treatment and control group characteristics. Section 2.5 presents the evidence of importance of local financial markets to local economic development by comparing response in employment immediately after the announcement of the shock to the change after the realization of the event. In Section 2.6, we offer several additional robustness checks. Section 2.7 concludes.

2.2 Institutional setting

Since the World War II, the United States is divided into five regions called Petroleum Administration for Defense Districts (PADDs) as presented in Figure 2.1. West Coast (PADD V) and East Coast (PADD I) are significantly disjoint from the other three districts in the central part of the U.S. in terms of oil transportation and trade, which will play a significant role in the identification strategy presented in the paper. In the following section we focus on briefly characterising pipeline infrastructure in the U.S. and the challenges it posed to the oil industry focusing on the Gulf Coast, Midwest and Rocky Mountain (PADDs II to IV).

2.2.1 Infrastructure bottleneck

Before the shale oil revolution started, most oil refined domestically had been imported to the U.S. via Gulf and East Coast ports[3] and then if need be transported further inland. Domestic crude oil production has also been concentrated in that region since the discovery of the Spindletop oilfield that prompted the Texas oil boom in early twentieth century and paved the way for mass consumption of petroleum as fuel. Drilling offshore Texas, Louisiana, Mississippi, and Alabama across the Gulf of Mexico soon became another major source of kerosene.

The emergence of the extraction part of the industry was accompanied by a rapid development of oil refineries in the Gulf Coast area.[4] Only some of the petroleum was transported north to the Midwest through a system of interstate pipelines. One of the most important transshipment points on the way is located in Cushing, OK. The area became a major storage place and a trading hub and is also where the West

[3]For example the Louisiana Offshore Oil Port (*https://www.loopllc.com/About/History*) has been the largest point of entry for waterborne crude oil imported to the U.S. and is the single domestic port suitable to serve biggest crude oil carriers in the world.

[4]The region is responsible for about half of the overall domestic refining capacity.

Texas Intermediate[5] is priced and delivered. Since Cushing, OK, has connected Gulf Cost suppliers of crude oil with the northern parts of the U.S. for years the pipeline infrastructure was oriented northbound.

Surprisingly, basins in the North became significant after advances in technology made hydraulic fracturing and horizontal drilling viable production methods. Large shale oil formations, like the Bakken Formation occupying parts of North Dakota and Montana, as well as the Niobrara formation in Colorado, Utah, and Wyoming rapidly gained on importance as tight oil deposits suddenly became recoverable. Just within years of the start of the shale boom extraction in PADDs II and III tripled in volume.[6]

Existing infrastructure allowed the ever-increasing amounts of shale oil to be transported South, to the hub located in Cushing, OK, but not further down to refineries scattered across the Gulf Coast, where one would naturally want the crude to be. The thick pipeline net was focused on transporting crude and refined products into the interior of the continent, and not to the Gulf Coast. Inadequate pipeline infrastructure oriented northbound and increasing production in the North finally caused an oil glut in Cushing, OK. Surplus of domestic oil was then reinforced by inflow of Canadian crude, production of which was triggered by the same surge in oil price and availability of new technologies that caused the shale oil revolution in the U.S.

Growing imbalance between in- and outbound capacity in Cushing, OK, became visible in prices in the end of 2010. In the next six months the gap between the price of oil in Cushing (measured by the West Texas Intermediate index) and the price in international markets (measured by the Brent index) reached over 20% and persisted for a few years as shown in Figure 2.2. Even though refiners on the Gulf Coast would be willing to buy cheaper domestic oil instead of the imported variety,

[5] West Texas Intermediate is commonly abbreviated as WTI. Alongside Brent, which is priced in London, it is a commonly used benchmark for oil prices. The name itself denotes a grade of the crude oil.

[6] Crude oil production data is recorded by the U.S. Energy Information Administration. For the latest data see: *https://www.eia.gov/dnav/pet/pet_crd_crpdn_adc_mbblpd_a.htm.*

it was largely limited by the lack of efficient means of transportation.[7]

The glut was significantly alleviated when one of the main pipelines connecting Texas with Midlands was reversed.[8] The project initially involved converting a gas pipeline, but was subsequently replaced with the idea to reverse the flow of crude oil in an already existing pipeline. This has been known in the literature and news as the Seaway Reversal and marks a revolution in the crude transportation map of the U.S.[9]

The Seaway Crude Pipeline System, called Seaway for short, initially run from Freeport, TX to Cushing, OK (through the Texas City, Texas Terminal and Distribution System) and carried petroleum to the North consistently since 1996. In May 2012 two midstream companies, Enterprise Product and Enbridge, completed a joint-venture project to reverse the flow direction of the pipeline to transport crude oil from Cushing, OK to Freeport, TX.[10]

Crucially, the intention to resolve the oil glut in Cushing was first publicized in April 2011 - over a year before the project completion date. The gap between the announcement and realization dates is essential for the identification strategy proposed in this paper. Since it was the first viable solution to the pipeline infrastructure problem since the disparity between the WTI and Brent indices emerged, focusing on the announcement helps eliminating the potential reverse causality. What is more, it uniquely allows us to document the presence and investigate the degree of financial constraints in a developed market economy. We return to this point when discussing the optimal time for drilling firms to react to income shocks.

[7]Due to the inadequate pipeline infrastructure producers and refineries were forced to use alternative means of moving oil, primarily rail or boat, despite significantly higher charges per barrel and lower reliability. Continuous productivity improvements in the shale oil extraction process that coincided with infrastructure bottleneck partially lessened the problem.

[8]The layout and direction of oil flow of major interstate pipelines in PADDS II trough IV and Canada is presented in Figure 2.3

[9]For example: "This is the first project announced since we started seeing a big spread between crude in the Midcontinent and on the Gulf Coast, Colson, [...] The first pipeline to solve that problem is going to be valuable." (See: *https://fuelfix.com/blog/2011/04/26/enterprise-energy-transfer-partners-plan-cushing-to-houston-pipeline/*).

[10]Initially, it was operating at a limited capacity of 150b/d. Full utilization of 400b/d has not started until January 2013.

2.2.2 Financing the investment in oil extraction

Domanski et al. (2015) estimate that the total debt of the oil and gas sector globally amounts to $2.5 trillion in 2015. Sure enough the shale oil industry is known for heavy reliance on external capital to develop new projects. According to ThomsonONE oil industry is financed by a combination of equity, project finance, and bonds, however the majority of the capital is provided through bank loans,[11] including so-called reserve-based loans. Small-to-medium-sized borrowers who are typically responsible for oilfield services (like services, seismic interpretations or drilling contracts) particularly rely on traditional sources of finance as they lack direct access to institutional markets.

The crucial decision each drilling firm has to make when facing uncertainty is when it is optimal for it to react to income shocks. It has been shown by Anderson et al. (2018)[12] that oil production from existing wells does not respond to price shocks because it is constrained by reservoir pressure, which decays as oil is extracted. Instead firms adjust their production via decisions about new drilling by either accelerating or postponing investment. Even though Anderson et al. establish this by looking at oil production form existing wells in Texas, it is reasonable to expect that the same pattern occurs in Bakken and Niobrara.

However, drilling a well requires extensive planning in advance (seismological surveys, mineral rights, covering bond, environmental impact assessment, creating infrastructure, mapping etc.). For example, planning and permitting process in Colorado[13] may take as long as a year to complete, before the well can be scheduled for drilling. Furthermore, drilling itself of a typical onshore oil well can take between 1 and 3 months[14] (horizontal drilling requires twice as much time as vertical drilling and additional time is needed to make the well fully operational). This provides some

[11] Source: Brogan (2014).

[12] For additional reference see Kellogg (2014).

[13] For details on the regulatory process for permitting and tracking an oil or gas well (*http://cogcc.state.co.us/documents/about/general/RegProcessPermitandTrackingWell.pdf*).

[14] For example, analysis based on proprietary survey data provided by the Oklahoma Oil and Gas Association can be found at *http://okoga.com/wp-content/uploads/2017/05/OK-Oil-Gas-Well-Drilling-OKOGA.pdf*.

rationale behind conjecture that firms need to react as soon as they learn about the positive news regarding transport opportunities and closure of gap between Brent and WTI instead of waiting until the reversal is complete.

2.2.3 Differences in the North

Finally, it is important to distinguish between two shale oil producing regions in the North, Bakken and Niobrara Formations. The fundamental difference between them lies in how they were affected by the announcement of and the subsequent Seaway Pipeline reversal. Due to extremely limited capacity of interstate pipelines connecting Niobrara and Cushing, OK, the increased outbound capacity from the oil hub benefited only producers operating in the Bakken area, which were connected through the Keystone and Enbridge pipelines. At the time oil fields in Wyoming, Nebraska or Colorado lacked the necessary infrastructure to directly gain from the reversal. This interpretation was confirmed in the following years when additional infrastructure projects were announced that aimed to connect Niobrara to Cushing, OK.

Differences between the two regions are depicted in Figure 2.3. While oil produced in Bakken can be shipped south through the system of major pipelines, Niobrara's output is limited to 175 thousand barrels per day. Historically, relatively small amounts of traditionally extracted oil were refined in local refineries, therefore there was no need for extensive infrastructure connecting to refineries in the south. After the shale boom however, local refineries could no longer meet the market demand.

To further corroborate the differences between Niobrara and Bakken we present Figure 2.4, which highlights the response in pipeline transports of crude oil after the announcement, reversal, and expansion of the project. Shipment from both regions to Cushing, OK, remains unaffected after the news of the reversal becomes public reflecting the already fully utilised pipeline capacities. However, as predicted the volume transported from Bakken to PADD 3 reacts sharply at dates of the actual reversal and expansion, while figures for Niobrara are relatively stable.

In this paper we exploit the fact that the reversal, which reduced the oversupply in Cushing, OK and accommodated the increase in production in the north, differentially affected producers in Bakken (treated region) and Niobrara (control region) plays. Therefore the two formations can potentially serve as quasi-natural treatment and control groups in assessing the impact of financial constraints on investment decisions.

2.3 Data description

All data used in this paper come from publicly available sources. Here we only provide sources and briefly justify choices behind sample and variable selection process.

2.3.1 Employment data

Data about the employment come from the Quarterly Workforce Indicators (QWI) database published by the United States Census Bureau[15] and collected in cooperation with all state agencies. It contains local labour market statistics disaggregated by industry, worker demographics, employer age and size. The QWI is a product of the Longitudinal Employer-Household Dynamics[16] linked employer-employee microdata, which includes 95% of private sector jobs thus providing relatively precise description of the labour market dynamics at the county level.

One of the unique characteristics of the QWI is that they are job-level data that track worker-employer pairs. The association between an individual and a particular firm provides data on worker flows (hires, separations, and turnover). We use this feature by basing our empirical analysis on *end-of-quarter* employment.[17] This

[15] Data are available publicly at *https://qwiexplorer.ces.census.gov*.

[16] For more information see: *https://lehd.ces.census.gov/*.

[17] Census definition: "when an individual receives earnings from the same employer in consecutive quarters, that individual is interpreted as having had an active job on the boundary between the quarters". According to Census the measure provides an "estimate of the number of jobs on the last day of the quarter". Source: Longitudinal Employer-Household Dynamics (*https://lehd.ces.census.gov/doc/QWI_101.pdf*).

approach allows us to pin down the timing of the events more accurately.

Finally, we are able to identify employment broadly associated with the oil sector by restricting data using the North American Industry Classification System (NAICS). Our sample includes industries with codes 21 (Mining, Quarrying, and Oil and Gas Extraction), 22 (Utilities), 23 (Construction[18]). It was not possible to further decompose workers with 4 or 6 digit classification due to limited data availability. However, we believe that the selected sector levels are a good approximation of the petroleum industry in Bakken and Niobrara and provide a good measure of economic activity that could be associated with the news of Seaway Pipeline reversal.

2.3.2 Banking data

Information about physical location of bank branches[19] as well as banks' assets and loans is taken from The Federal Deposit Insurance Corporation (FDIC),[20] which features quarterly financial statistics for the U.S. We use these data to construct various measures of the depth of financial sector at the county level to reflect the ability of oil related companies to borrow money for new investment. As we argued in Section 2.2.2 access to loans is crucial for the oil industry because of its high capital intensity. Therefore we expect that measures of banking development will be informative about the speed with which firms can react to business opportunities.

Furthermore, we take advantage of an indicator variable provided by the FDIC that describes institution's primary specialization in terms of asset concentration.

[18]Including *Utility System Construction*.

[19]Definition of branch/office by FDIC: "A branch/office is any location, or facility, of a financial institution, including its main office, where deposit accounts are opened, deposits are accepted, checks paid, and loans granted. Some branches include, but are not limited to, brick and mortar locations, detached or attached drive-in facilities, seasonal offices, offices on military bases or government installations, paying/receiving stations or units, non-deposit offices, Internet and PhoneBanking locations where a customer can open accounts, make deposits and borrow money. A branch does not include Automated Teller Machines (ATM), Consumer Credit Offices, Contractual Offices, Customer Bank Communication Terminals (CBCT), Electronic Fund Transfer Units (EFTU), and Loan Production Offices Summary of Deposits information is required for each insured office located in any State, the District of Columbia, the Commonwealth of Puerto Rico or any U.S. territory or possession such as Guam or the U.S. Virgin Islands, without regard to the location of the main office."

[20]*https://www.fdic.gov/*

In order to increase precision of estimates by capturing lending to business (rather than households) we focus our empirical analysis on banks that belong to the category: *Commercial Lending Specialization*.[21] According to the definition these are "institutions whose commercial and industrial loans, plus real estate construction and development loans, plus loans secured by commercial real estate properties exceed 25 percent of total assets."

Finally institutions are matched with their locations using a unique number assigned by the FDIC for the purpose of issuance of insurance certificates.

2.3.3 Other data and information sources

We supplement the analysis with various data and information sources that help validating the quasi-experiment, increase the precision of regression estimates and allow to verify sample unbiasedness.

County level data on monthly oil production was hand-collected from state agencies in Bakken and Niobrara regions.[22] Other data on oil sector (such as pipeline transportation between PADDs etc.) as well as maps and diagrams were taken from the U.S. Energy Information Administration.[23] Finally, information on the oil supply chain and pipeline infrastructure is derived from the report *Crude Oil Infrastructure* published by the National Petroleum Council.[24]

County characteristics used as controls in all specifications come from 2010 United States Census and include information on population, area, and density. WTI and Brent crude oil price indices were taken from Bloomberg.

[21] Other categories include: International, Agricultural, Credit-card, Mortgage Lending, Consumer Lending or Other Specializations.

[22] For example: Industrial Commission, Department of Mineral Resources, Oil and Gas Division in North Dakota (*https://www.dmr.nd.gov/oilgas/*) or Oil & Gas Conservation Commission (*http://wogcc.wyo.gov/*) in Wyoming

[23] *https://www.eia.gov/*

[24] The National Petroleum Council (*https://www.npc.org/*) is a federally chartered and privately funded advisory committee, which was established by the Secretary of the Interior in 1946.

2.3.4 Selected sample and summary statistics

Counties were selected based on information provided by state agencies and the EIA to capture the underlying geological landscape of Bakken and Niobrara Formations. We then extended the list by including neighbouring counties with consistent positive oil production as they would likely be interconnected with local pipeline systems and would be therefore potentially impacted by the announcement of the reversal. This was made possible using hand-collected data on monthly oil production in all counties. Our sample consists of 105 counties, 35% of which are in Bakken and 62% have good access to banking. Maps presenting geographical distribution of counties are in the Appendix (Figures B.1, B.2 and B.3). Full list of counties is also included in the Appendix (Table B.1).

We have restricted the time window to a period of relatively stable oil prices to decrease the role of market volatility in addition to using fixed time effects. The main analysis based on level of employment in the oil sector is carried on the sample ranging from 2010q1 to 2013q2. Earlier years have been additionally used to established trends or scaling. We can extend the sample to range from 2004q1 to 2013q2 when we measure changes in employment. Details can be found in Section 2.4.1.

Table 2.1 provides summary statistics for the selected sample and includes counties with deep and shallow banking sectors as well as from Bakken and Niobrara regions. Standard deviations of population, area, and therefore density are relatively large, reflecting differences in administrative divisions in the U.S. However, states like North Dakota and Montana (Bakken) as well as Wyoming, Nebraska, and Kansas (Niobrara) are relatively less populated than the rest of country. Oil sector plays an important role in the economies of selected counties as about 5% of the population is working within broadly defined sector.[25] Even though the U.S. is a financially developed country there are still vast differences in access to physical branch locations as the median (1) substantially differs from the mean (8.25), partially reflecting differences between cities and urban locations. However, two other

[25]For details about the definition of employment and sector see 2.3.1.

measures of bank development, loans and assets per capita, also suggest diversity across regions.

2.4 Empirical strategy

2.4.1 Identification

The goal of this paper is to evaluate the causal impact of local banking on economic development. The usual difficulty with such a task is the possibility of reverse causality. To provide causal interpretation it is necessary to disentangle the impact of banking on local economy from the fact that regions with better investment opportunities may attract more banks.

In this paper we exploit a quasi-natural experiment triggered by the big infrastructure investment in the oil pipeline network in the U.S.[26] The flow reversal in one of the big pipelines facilitates the transport of oil produced in the northern shale oil basins. The new route increases the price received by producers because extracted oil was previously stuck in overfilled storage hubs. The announcement of the project creates a positive intertemporal cash flow shock in regions connected to the discussed pipeline. Since the actual cash flows are not affected until the project is finalised, the shock exposes to treatment only areas with good access to financing. Oil producers have incentives to respond to improvement in investment opportunities immediately because increasing oil production takes time. However, they may not have resources to do so and the access to local financing alleviates this problem. The advantage is temporary because increased cash flows realized after the pipeline launch should reduce firms' financial constraints. Most importantly, the news affects firms' investment opportunities, but is not correlated with the existing banking network. Therefore we can evaluate the causal impact of the pre-existing local banking network on economic development in a difference-in-differences setting. The baseline

[26] Details were described in Section 2.2.

specification is as follows:

$$\begin{aligned} y_{it} &= \alpha_i + \beta_I Interim_t + \beta_{IB} Interim_t \times Bank_i + \beta_P Post_t + \\ &+ \beta_{PB} Post_t \times Bank_i + \epsilon_{it}, \end{aligned} \tag{2.1}$$

where the dependent variable y_{it} is a measure of local development in county i at time t. α_i denotes county fixed effects that account for time-invariant differences between counties. The dummy $Bank_i$ distinguishes between counties with deep and shallow banking sector. It does not appear separately because it is absorbed by county-level fixed effects. The regression is estimated on the sample of counties affected by the new pipeline project (Bakken region).

The dummy $Interim_t$ identifies a period after the announcement and before the pipeline launch. It is equal to one from 2011q2 to 2012q1 (four quarters). The dummy $Post_t$ is equal to one from 2012q2 onwards (five quarters) and identifies the pipeline launch. We include these dummies instead of a full set of time effects because we want to directly observe the impact of the announcement and the launch also on counties without deep financial markets. We adjust the data for quarterly seasonality, and the exact procedure is discussed in the following paragraphs.

Coefficients β_{IB} and β_{PB} estimate the causal impact of local access to financing on local development when future *expected* cash flows increase, and when the *actual* cash flows increase. ϵ_{it} are random disturbances. Standard errors are robust and not clustered at the county level due to relatively small number of different counties in the sample. Therefore the significance of the coefficients needs to be analysed with caution as it may be overestimated due to possible within-county error correlation. The issue is briefly discussed in Section 2.6.

Local development is measured based on county employment in the oil sector. It is a meaningful measure of development and producers' response to the positive intertemporal cash flow shock because increasing future oil production involves investment in new oil wells. Pure maintenance of existing wells does not require many employees, but additional workforce is necessary for drilling. Employment is measured at the end of the quarter either in levels or as a change relative to a previous quarter. In both cases it is scaled with average employment in the oil sector in a county three years before the announcement (2008q2–2011q1). As an alternative, we scale the dependent variable with a county population (published in 2010 U.S. Census). Scaling is necessary due to substantial variability in sizes of populations and oil sectors in the counties.

Employment and oil production are both subject to seasonal fluctuations and require adjustment. Otherwise a seasonal change may be mistakenly interpreted as a consequence of the event. We estimate and subtract quarterly fixed effects for each county separately. The estimation is based on the pre-event sample (2004q1–2010q4) to ensure the event itself does not impact the results. For the development measure based on employment in levels we must also account for the trend. It is not an easy task because there is a trend change around 2010q1. We cut the sample at 2010q1, estimate the trend based on the pre-announcement period (2010q1-2011q1) and continue the analysis on the detrended time series. The weakness of this approach is the short observation window used for trend estimations, but the plots of errors and fitted values do not raise any concerns (Figures B.4 and B.5). It is not necessary to eliminate the trend from the second measure of employment based on quarterly changes. The dependent variables are additionally winsorized to account for outliers.

We consider several binary measures of local banking development. All the measures rely on banking data from one year before the event (2010q2). The measure based on *Branches* is equal to one for counties with at least one bank branch. It reflects the idea that access to financing is limited if a firm is not in a close proximity to the bank branch. It is refined by excluding branches which grant few loans, i.e. for which the overall amount of commercial lending is below the tenth percentile.[27] The second measure, *Loans*, is based on commercial loans granted in a county. It is equal to one if the amount of loans granted in a county is above the sample median or the amount of loans relative to the population is above the sample median. This

[27]The percentile is calculated based on counties in which the lending is positive.

measure captures the idea that access to financing can be measured by the scale of lending. Finally, the third measure, *Assets*, relies on banks' assets. It is equal to one if banks' assets relative to a county population is above the sample median. The scaling is necessary to avoid capturing only bigger or more populated counties.

The main assumption validating our regression (Equation 2.1) is that development of the local banking system is independent of the investment opportunities in the oil industry in the county. It is likely to be satisfied thanks to the specific time structure of the event. First, the information about the future increase in cash flows comes one year in advance. Second, investment in oil industry requires time. Hence by measuring response of employment at the time of the announcement we are likely to capture only the impact of access to local financing on firm's ability to exploit new investment opportunities. Existing banking network may be correlated with the existing quality of oil reserves, but not necessarily with the productivity of new wells since oil reserves are difficult to estimate and often found in completely new areas. Also, banks do not have incentives to respond simultaneously with firms because before the real cash flows are realized the uncertainty regarding quality of resources is high. In this setting it is unlikely that banking development is correlated with the area's post-announcement investment opportunities. This feature alleviates the problem of reverse causality.

2.4.2 Controlling for other oil industry and banking shocks

There is a concern stemming from the fact that oil sector depends on many different economic and political factors. Changes in oil industry may arise because of the other shocks than the new pipeline infrastructure investment. In such a scenario it would not be possible to prove that the effect is causal as the identification assumption relies on the specific nature of the shock. Similarly, there could be a macroeconomic shock to banking that would affect only counties with more developed local banking sector. To address these concerns we extend the setting and compare oil-producing counties from the region that suffers from insufficient pipeline infrastructure, but is not affected by the reversal (Niobrara). Introducing another control region allows us to separate the effect of changes affecting the whole oil industry or banking from the effect of the reversal. The extended specification is a triple difference regression presented below:

$$\begin{aligned} y_{it} &= \alpha_i + \beta_I Interim_t + \beta_{IB} Interim_t \times Bank_i + \beta_{IT} Interim_t \times Treat_i + \\ &+ \beta_{ITB} Interim_t \times Treat_i \times Bank_i + \beta_P Post_t + \beta_{PB} Post_t \times Bank_i + \\ &+ \beta_{PT} Post_t \times Treat_i + \beta_{PTB} Post_t \times Treat_i \times Bank_i + \epsilon_{it}. \end{aligned} \tag{2.2}$$

The notation is consistent with the previous specification (Equation 2.1). The dummy $Treat_i$ identifies regions affected by the reversal project, i.e. regions with a convenient connection to the new pipeline before the announcement (Bakken). Similarly to the dummy $Bank_i$, $Treat_i$ does not appear separately, or as $Treat_i \times Bank_i$, because these dummies are absorbed by county-level fixed effects. The coefficient β_{ITB} captures the influence of the announcement in counties with deep financial markets relative to counties with shallow financial markets and affected by the intertemporal cash flow shock as opposed to the unaffected region. Similarly, the coefficient β_{PTB} captures the influence of the pipeline launch. Standard errors are robust and clustered at the county level.

The dependent variables are adjusted for seasonality and trend in the same way as in the previous specification. We include separate trends for the treated and control counties.

The additional condition needed to justify the causal inference in the above specification regards the independence of oil infrastructure development and investment opportunities in both oil regions. There is a valid concern that pipelines connecting most profitable regions would be built first. However, we consider a reversal of an existing pipeline and not building a new one. The choice which regions become connected depends on historical structure of the oil industry in the U.S. and not on the current investment opportunities in the regions. Therefore the identification assumption is likely to hold.

2.4.3 Treatment and control group characteristics

Table 2.2 characterises counties with better and worse access to local financing. Not surprisingly, counties with more developed banking sectors have more commercial bank branches, and banks there are on average bigger and grant more loans. The difference in median number of bank branches is one and it actually determines whether there is a local bank branch or not. Bank branches in counties with shallow banking sectors are small and the scale of a difference in terms of assets per capita is 8.65 to 7638.6 thousand dollars. Counties with better access to financing are also more populated. Local oil sectors in these counties are bigger both in relative and absolute terms. The share of employment in the oil sector in the population is, however, of the same order and amounts to almost 8% and 5% in counties with deep and shallow banking sectors respectively. Average oil production varies a lot in both groups.

Table 2.3 provides county summary statistics by treatment status (Bakken vs Niobrara). Bakken region, which was affected by the pipeline reversal, consists of less populated counties in which local oil sectors are slightly more important. The difference is not striking, though, since the average share of employment in oil sector in population equals 6.4% and 5.1% in Bakken and Niobrara regions respectively. Local banking sectors are more developed in the control region (Niobrara). This disparity may be naturally related to population sizes since the amount of granted commercial loans per capita is similar in both areas, especially in terms of a median (14.6 vs 16.1). Also, the share of counties with deep financial markets is similar in both regions.

While the methodology accounts for potential pre-event differences between the treated and non-treated counties, the time-effects in the sub-groups are assumed to be the same in the absence of treatment. In our setting it implies that the investment opportunities in the oil sector cannot be correlated with the treatment status and the banking measures. The justification of this assumption has been discussed in Sections 2.4.1 and 2.4.2. It the following paragraphs we support it further with a graphical illustration.

The stability of the differences between the treated and the control counties also implies that economic development measures before the event plotted for both regions should create parallel trajectories. As mentioned in the previous sections, for the measure relying on employment in levels this claim has to hold in terms of deviations from a trend. Figures 2.5 and 2.6 depict employment in the oil sector over time. Each line represents the time series of scaled employment/employment changes averaged across counties by treatment/control status and by deep/shallow banking. Vertical lines indicate the announcement and the launch of the pipeline reversal.

The lines are roughly parallel in the pre-announcement period. In the control region they stay stable in the whole observation window. It confirms the assumption that the control region has not been affected by the pipeline reversal. Treated counties respond, but the timing of the response is different depending on the depth of the local financial markets. Counties with better access to financing (deep financial markets) respond positively already around the announcement date, while employment in counties without good access to financing (shallow financial markets) stays stable around the announcement. These counties, however, react at the end of the observation window after the pipeline launch. It is also time when the increase in employment in the affected counties with deep banking sectors fades away. The gap between treated counties with deep and shallow banking sectors closes at the end of the observation period.

The measure relying on employment changes is much noisier, but the pattern is similar. Treated counties with better access to financing exhibit bigger changes in employment in the period between announcement and launch. After the launch, this behaviour immediately reverses and the affected counties with shallow banking sectors are characterised on average by higher growth rates in employment, while these rates drop for counties with deep banking sectors.

The presented figures confirm several key facts consistent with the hypothesis that local financial markets facilitate local economic growth. Firstly, treated counties respond to the cash flow shock. Secondly, there is a clear reaction to the announcement in the treated counties with deep financial markets. Finally, treated counties with less developed banking sector respond to the cash flow shock with a delay.

2.5 Importance of local financial markets

2.5.1 Response to the project announcement

Access to external financing increases firm's ability to start new investment. While it is not always true that delaying investment is bad, firms expecting higher oil prices and greater transport capacity at a specific point in time in future have additional incentives to produce more oil when the event happens. Increasing production requires drilling new wells, hence firms need to plan in advance production expansion. In our setting there is a four-quarter gap between the announcement and realization of the new pipeline project. In order to increase production at the time of the pipeline launch, firms should respond shortly after they learn about the new opportunities. In this section we analyse the employment response of to the shock at the time of announcement. Therefore the main coefficients of interest are interactions with the dummy $Interim$. The interpretation of coefficients gives insight about the causal role of banking in development because in the interim period between announcement and launch the role of financial constraints is accentuated. The analysis of the employment reaction post-launch is discussed later.

Measuring employment in levels after the project announcement

Table 2.4 shows the results of the baseline specification (Equation 2.1) using the sample of counties affected by the reversal (Bakken). The left panel shows the estimates when the dependent variable is scaled with the mean employment in the oil sector and right panel when the dependent variable is scaled with county's population.[28] Each column corresponds to a different measure of county's banking sector development.

The results in the left panel confirm that financial markets have positive impact on economic development. After the announcement the employment in the oil sector increases in counties with better access to financing. The coefficient on the interaction $Interim \times Bank$ is positive and significant, and amounts to $0.20 - 0.46$. The post-announcement change in employment in the oil sector is on average higher by around two–four people for every 10 people employed in counties with better access to financing relative to counties with worse access to financing. It is a big number, but it is consistent with the characteristics of oil industry. Fluctuations in oil sector employment tend to be high because drilling new wells is a temporary activity that requires much more labour than maintenance of the existing ones. More importantly, we do not find any change in employment in counties with shallow banking sectors. The results imply that, in general, firms without access to external financing are not able to instantly respond to positive shocks to future investment opportunities.

The results in the right panel are consistent for the first two banking measures. The coefficients on the interaction term $Interim \times Bank$ are positive and significant. The magnitudes are different because they are affected by the relative sizes of local oil sectors. The coefficients on the $Interim$ dummy are negative implying that in the interim period the employment in the oil sector was falling.

The results for the banking measure based on assets are weaker in terms of significance in the left panel and inconsistent in the right panel. We suspect that the $Assets$ measure is less informative about the availability of financing at the local level. While assets capture bank's ability to lend money, they may not reflect bank's willingness to engage in lending for oil-related projects. Firms may more easily obtain a loan for drilling of oil wells if banks are smaller and *local* for several reasons. Firstly, geographical closeness alleviates asymmetric information inherent

[28]For clarity, reported coefficients and standard errors based on regressions of employment scaled with county population are multiplied by 100.

in corporate financing. Secondly, local banks are equipped with better expertise about local industry, including oil. Finally, local banks are able to maintain better relationships with entrepreneurs. Close borrower-lender relationships are less likely to arise with banks operating at the bigger scale and at the national level. We drop the asset-based measure of banking from the other reports.

Table 2.5 shows the results of the triple difference specification presented in Equation 2.2. The results are consistent with the previous conclusions that local financing facilitates economic growth through job creation. The post-announcement reaction in the affected region, $Interim \times Treat$, is not significant (or is even negative in the right panel). However, the post-announcement reaction in the affected counties with deep financial markets ($Interim \times Treat \times Bank$) is positive and significant at the 5% level. The coefficients have similar magnitudes as in the basic specification. The drop in significance may result from the small testing power. There are only 35 treated counties and the sample period is relatively short considering the high volatility of employment, oil production and oil prices. However, the loss of significance may also come from more accurately measured standard errors. In the triple difference specifications the errors are clustered at the county level.

Measuring employment in changes after the project announcement

In this section we replicate the previous estimations but measure economic development with employment changes. Starting new drilling projects should increase employment, and the impact is likely to be visible both in levels and changes. Even though the measure relying on quarterly changes exhibits high volatility, it has an advantage that there is no visible trend change over years. Hence by using this measure we are able to extend the observation period.

Table 2.6 presents the baseline estimation results. The sample comprises of counties affected by the new project (Bakken). The response of employment in the oil sector following the project announcement is positive and significant in counties with better access to local financing. It confirms the importance of local financing in exploiting new investment opportunities and stimulating economic growth. Conclusions are the same for both types of scaling of the dependent variable.

Measuring employment in terms of growth rates also gives consistent results in the extended sample. The estimates of the triple difference regressions are presented in Table 2.7. The coefficients on the interaction, $Interim \times Treat \times Bank$, are positive and significant. The values are similar in magnitudes compared to the baseline specification.

Overall, the results suggest that well-developed local banking sector has positive and significant impact on local economic development. The conclusions are the same when we measure development using employment levels and quarterly changes in employment. The depth of the local banking sector can be consistently measured by either the number (existence) of local bank branches or the scale of local lending.

2.5.2 Catching-up

The results from the previous section support the hypothesis that developed local banking sector constitutes a crucial factor in economic development and is not only a by-product in highly productive regions. Firms in counties with better access to external financing are able to respond instantly (which we assume is optimal) to improvement in future investment opportunities. If the response is indeed possible because of the local access to financing then firms in more constrained regions should respond when the cash flows actually increase and improve their borrowing capacity.

In this part we, therefore, examine what happens after the pipeline project is realized. Most importantly, we are interested whether the areas limited by poor access to financing are able to follow the trend or catch-up after the new pipeline is launched. Identifying the response from more financially constrained counties is not only interesting, but provides a relevant argument in support of the hypothesis that the financial markets are behind the patterns in employment observed around the announcement.

The launch of the pipeline increases profitability of companies by increasing prices obtained from selling oil, reducing transportation costs and increasing quantities that firms are able to sell.[29] The intuitive graphical analysis presented in Section 2.4.3 already indicated the possibility of catching-up, i.e. that affected counties without good financing options respond to the discussed shock with a delay after the expected cash flows are realized. The graphs create an impression that the gap in employment between treated counties with deep and shallow banking sectors closes after the pipeline launch. It is consistent with the idea that counties that responded earlier may reduce employment and focus on maintenance of new wells, while counties that respond with a delay aggressively increase employment during the drilling stage.

To draw more rigorous conclusions about the possible catching-up process we analyse the significance of coefficients $Post \times Treat$ and $Post \times Treat \times Bank$ in regressions 2.1 and 2.2.

Measuring employment in levels after the pipeline launch

We go back to the results based on the treated counties (Bakken sample) presented in Table 2.4. First we focus on the estimates in the left panel that use employment scaled with its pre-event mean as a dependent variable. The response after the pipeline launch is different than after the announcement. All counties increase employment as the coefficient on the *Post* dummy is positive and significant. It suggests that financial constraints in counties with limited access to local banking have been alleviated, and firms in these counties are able to respond to opportunities arising from better transportation options.

The role of local banking after actual cash flows increase is ambiguous. It is not obvious whether counties with better access to financing still drill more intensively because the conclusions depend on the adopted banking measure. The coefficient on $Post \times Bank$ dummy is insignificant in the first column, but big and significant

[29]We do not take a stand on the actual size of additional cash flows relative to expectations formed at the announcement, and we assume it is a second-order effect compared to actual money flowing in that would not change the direction of the impact. Theoretically, however, this could be either a positive or a negative surprise, which would reinforce of damped the effect.

at 5% level in the second column. The latter implies that deep financial markets are important even when firms' internal funds increase. Insignificance, on the other hand, does not mean that local banking does not play a role anymore since firms in counties with deep financial markets could simply finalise their projects. We do not aim to make a stand when counties with better access to financing should stop their investment. Both continuous drilling and finalising the investment are possible scenarios and do not contradict the view that local banking impacts development.

The results accounting for other shocks in the triple difference setting are presented in Table 2.5, left panel. The coefficients on the interaction on $Post \times Treat$ and $Post \times Treat \times Bank$ are positive but insignificant. It seems that we lack power for this estimation. When we do not differentiate counties by banking in the post-launch period, the impact of the launch in the treated counties is positive and significant. These estimates are included in the Appendix (Table B.2). It implies that after the launch all affected counties, irrespective of the development of the local banking, increase their employment. Given the results, we cannot, however, determine whether the investment in the post period is mostly driven by counties that could not react earlier due to financial constraints.

There are three factors that may justify low power. Firstly, the post-launch period is very short. This is something we cannot improve on because the employment data are not available for some states after year 2014, and, more importantly, the period of relatively stable and high oil prices finishes in 2014. Secondly, the graphical analysis suggests the reaction of affected counties with shallow banking is slow. Furthermore, the distinction between counties with and without access to external financing may be irrelevant because counties which started investment around the announcement do not have to necessarily finish it completely by the time the pipeline is launched.

The results for the dependent variable scaled with population are presented in the right panels of Tables 2.4 and 2.5. The reported outcomes lead to very different conclusions. The employment in the oil sector on average drops post-launch and increases in counties with better access to financing. This variable, however, has a long right tail and is sensitive to how the data are winsorized. There are several counties in which the oil sector is very important and they may blur the results. For this reason we look at the results in the right panel with caution. Scaling by population does not allow to abstract from the importance of the oil sector in the county. Scaling with the pre-event mean employment is preferred since it allows to compare the condition of the oil sector before and after the event irrespective of its relative size.

Measuring employment in changes after the pipeline launch

Since the launch of the pipeline does not imply that counties with better access to financing need to stop all the investment, the employment patterns in the post-period may be captured more accurately by employment changes. Even if employment in all treated counties increases, it is likely that counties in which there was little action in anticipation of the launch try to make up for the time lost by waiting for financing.

In the region affected by the reversal (Bakken) the initial positive impact of the announcement in counties with deep banking sector seems to be reversed following the launch of the pipeline (Table 2.6). This reversal is not significant though. The results confirm, however, that in the post period the increased investment takes place in counties with shallow banking sectors. The coefficient on the *Post* dummy is positive significant at 5% level. Relative to counties with better financing options that responded at the announcement changes in employment are slightly smaller (0.13 vs 0.081).

The results based on the extended sample (Bakken and Niobrara) are inconclusive (Table 2.7). They are consistent in terms of signs, but insignificant. The coefficient on the interaction $Post \times Treat$ is significant at 10% level in the right panel, where changes are scaled with the county population.

The provided results confirm that the advantage of counties with deep banking sector that can respond instantly to the intertemporal cash flow shock is temporary.

The impact of the launch in affected counties does not differ depending on the local banking. All affected counties increase employment, even though coefficients are not significant in some specifications. We did not manage to clearly discern the reaction in the affected counties with shallow banking sectors which would further support the hypothesis that local banking is crucial for economic development. However, the graphical analysis and the results from the previous subsection provide pieces of suggestive evidence that catching-up indeed takes place, but is more difficult to measure due to possible delays and instability in the oil sector that starts playing a role in the later quarters.

2.6 Robustness checks

In this section we test the robustness of the results. We focus on two dimensions: event time and sample composition. In the first step we want to check that the project announcement and realization indeed determine the periods of major changes in the investment activity of oil producing firms measured by employment. Second, we do a placebo test to evaluate how often random treatment status and deep banking sector assignment produce significant results.

2.6.1 Conditional trends

To analyse the employment in counties over time and pin down the timing of employment reaction we run a series of regressions in which time variables $Interim_t$ and $Post_t$ are replaced with time dummies and plot interaction terms between these dummies, $Treat_i$ and $Bank_i$. For the dependent variable based on employment in levels we use quarterly time dummies. For the dependent variable based on changes in employment we use year dummies to capture the annual averages. The average each year is calculated staring from the second quarter so that the specification fits the event time structure. The sample length is adjusted so that each dummy captures four quarters. Such regressions allow us to track changes in economic development over time and identify when they exactly occur. Most importantly, we expect no significant movements before the event.

The results for the employment measured in levels are presented in Figure 2.7 for the affected counties only (Bakken), and in Figure 2.8 for extended sample (Bakken and Niobrara). Top panels present the effects of common movements of employment in all affected counties. Bottom panels present the additional movements of employment in the affected counties with good access to external financing. The patterns are very similar in both samples. On average, there is no reaction in the affected counties until the project is finalised. Coefficients are oscillating around zero. Shortly after the launch there is an increase and estimates are positive. The plotted errors are big but we attribute this to the fact that we estimate many coefficients based on a relatively small sample. The pattern for affected counties with better access to local banking confirms the previous conclusions. There is an observable increase in employment after the project announcement which fades away shortly after the pipeline launch. The temporary response suggests that firms were able to finalise the investment around the anticipated date of the pipeline flow increase. The response to the announcement may be delayed by one quarter. It is possible that firms do not respond as quickly as in the end of June 2011 to the news that arrived in the end of April 2011.

What raises some concerns is the weak trend in the pre-announcement period observed in the affected counties with access to local banking. It is significant in the triple difference specification. Since we consider it unlikely that the announcement was anticipated, it may suggest that some other shocks affected these counties as well. We think, however, that it is a result of imperfectly measured trends in the pre-announcement period. We discuss this issue further in the following paragraph when we employ our second development measure.

The results for the employment measured in changes are presented in an analogical manner in Figures 2.9 (Bakken) and 2.10 (Bakken and Niobrara). In general the plots confirm that affected counties with access to external financing increase employment after the announcement and before the project launch, and this pattern is reversed in the post-launch period. In the extended sample, the increase after the announcement is very close to zero though. Counties without good financing options respond only in the post-launch period. As opposed to the measure of employment in levels, there are no concerns about the pre-announcement trend. In the two years before the announcement, when a possible pre-trend was observed in the employment measured in levels, the coefficients are stable and close to zero. It confirms our claims that the previously observed trend is not due to possible shock to counties with better developed local banking sectors.

Overall, the movements in employment in the oil sector in response to the announcement of a new pipeline development project and after its realization are consistent with the previous results. However, the analysis suffers from the noisy data and a short sample which is reflected in the low power and changing significance of the coefficients.

2.6.2 Placebo test

We run regressions on the artificial sample where the treatment status and the values of banking measure are assigned randomly. The assignment aims to preserve on average the structure of the original sample in terms of proportions of treated counties and counties with deep financial markets. We run regressions 2.1 and 2.2 on the artificially categorized sample of counties 100 times. We then count how many times the coefficients on the variables measuring the response of employment in the affected counties with deep banking sectors ($Interim \times Bank$ and $Interim \times Treat \times Bank$) are significant at 5% level. If the standard errors are calculated correctly and the documented increase in employment is indeed in response to the project announcement and possible only in counties with access to external financing, then coefficients should not be significant more than 5% of time. The dependent variable is scaled with its pre-event mean. We focus only on the announcement because this is the main event of interest in this analysis that is crucial for the causal inference about banking and economic development.

In the sample that comprises of counties affected by the reversal (Bakken) when the development is measured with employment in levels the coefficient is insignificant in 64% cases. The analogical rejection rate for the extended sample (Bakken and Niobrara) equals 97%. The latter is consistent with the predictions and justifies the adopted division between treated and control counties and counties with deep/shallow local banking sectors. The former number shows the significance in over 30% of cases, which as a stand-alone results raises serious concerns. The most likely reason is that standard errors in this sample are underestimated due to lack of clustering. We did not introduce clustering because with a small number of clusters the intra-group correlation is underestimated. The percentages of insignificant estimates when placebo test uses measure of employment in changes equals 73% and 96% for the difference-in-differences and triple difference regressions respectively. The results of the placebo tests emphasize that caution is needed in evaluation of results based on the affected region only. Still, we consider the baseline results to be informative because they are very consistent with the triple difference specification, which does not suffer from the underestimated standard errors.

2.7 Conclusions

In the paper we employ a unique setting to analyse the role of local banking in economic development. We are able to disentangle the causal impact of the existing banking network from the fact that more productive regions attract banking development. Lack of reverse causality in our setting comes from the specific structure of the studied intertemporal cash flow shock. The news about the shock arrives four quarters before the cash flows increase and the associated uncertainty is resolved. The announcement creates incentives for firms to respond. Banking network, on the other hand, cannot expand in the most productive regions because the productivity is not revealed before the shock is realized.

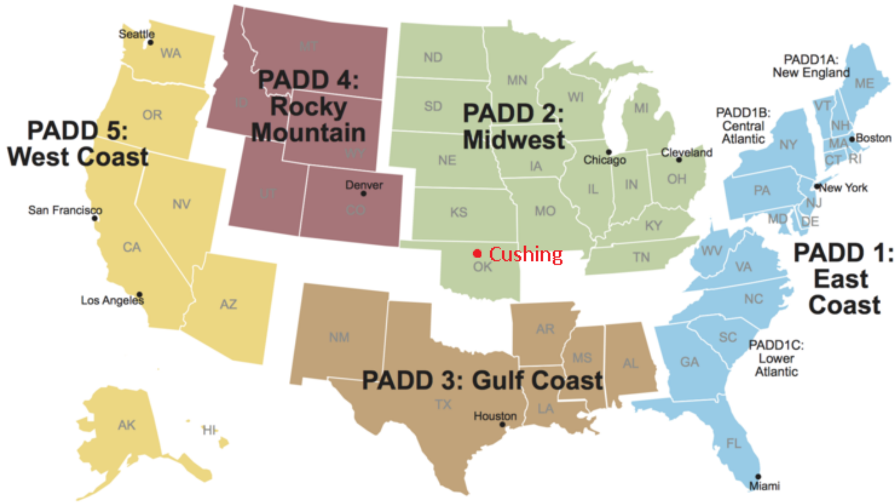
The main finding of the paper confirms that local banking is important for economic growth. Lack of local banking sector increases financial constraints of the firms and delays job creation in the oil sector. Counties hit with the news about future improvement of cash flows respond only if they have easy access to external financing. We believe that in the oil industry employment may proxy for investment because drilling new wells requires more labour input than maintenance of existing wells.

The investment opportunities created by the shock do not disappear if they are not exploited straight away. This feature allow us to study how firms time their investment. We find that firms in counties that are also exposed to the positive shock, but do not have good financing options respond to it with a delay. More specifically, they respond when the anticipated increase in cash flows occurs and reduces financial constraints itself. Therefore local banking facilitates economic growth not only via provision of financing, but also because it helps optimizing the timing of investment.

Geographical closeness in our setting is helpful because investment opportunities in the oil industry are difficult to assess. Local presence may provide additional expertise and increase willingness to lend to oil producers. While this setting is rather specific, geographical proximity may be also useful when information asymmetry between entrepreneurs and lenders is particularly severe. In such situations local banks are more likely to build long-term relationships with borrowers.

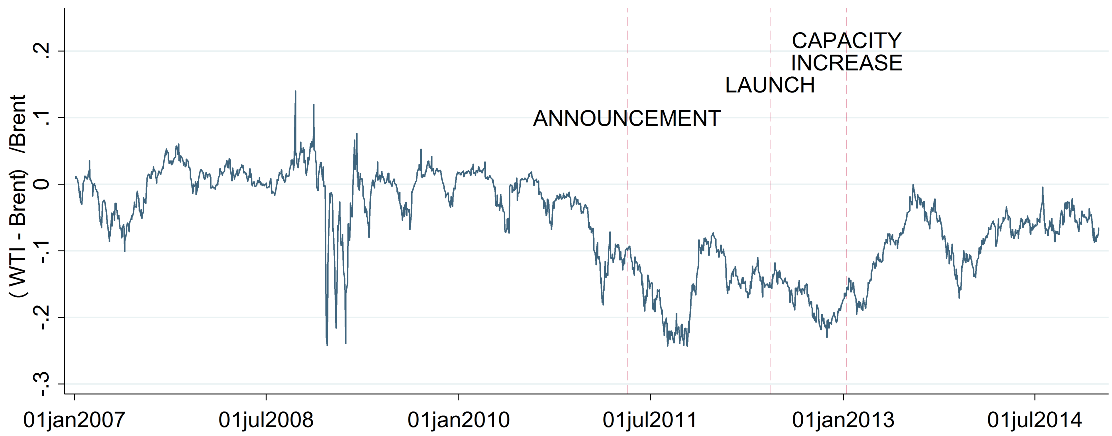
2.8 Figures

Figure 2.1: Division of the U.S. into Petroleum Administration for Defense Districts (PADDs).



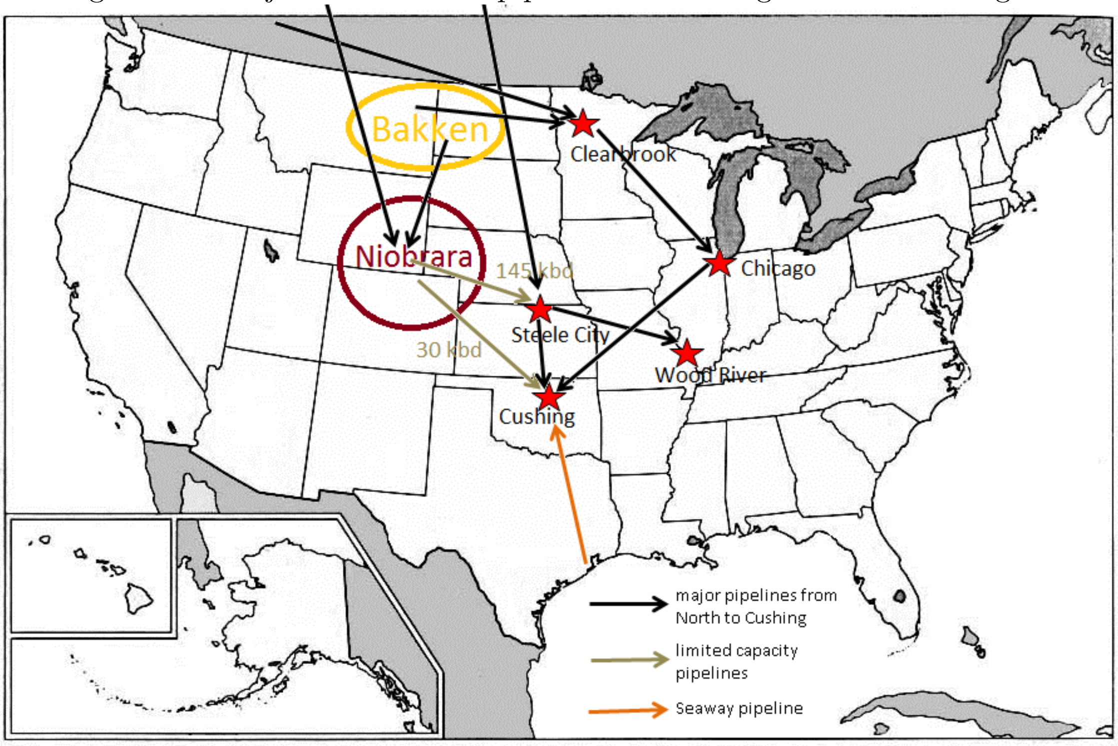
Notes: This figure shows U.S. Petroleum Administration for Defense Districts. Source: EIA.

Figure 2.2: Crude oil price indices gap as percentage of Brent.



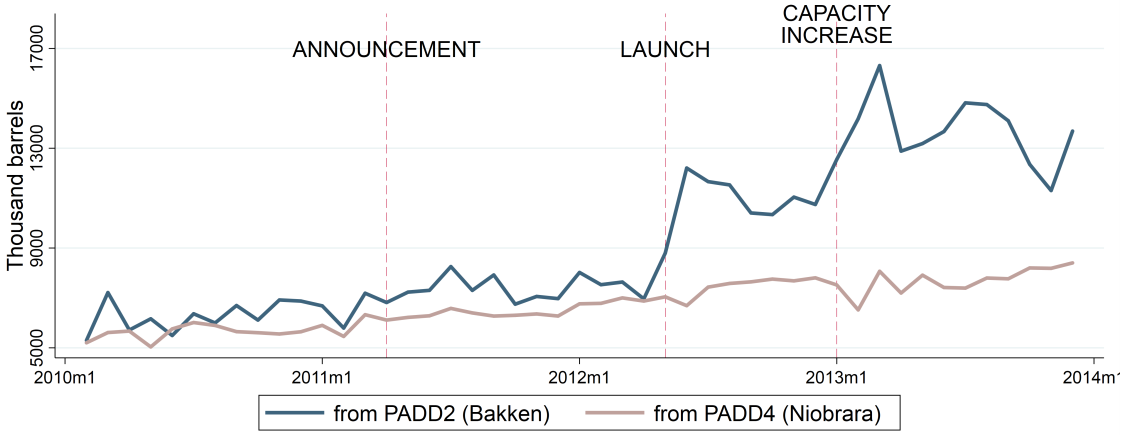
Notes: This figure shows the gap between WTI and Brent oil price indices as a percent of the Brent price. Source: Bloomberg.

Figure 2.3: Major interstate oil pipelines connecting PADDs 2 through 4.



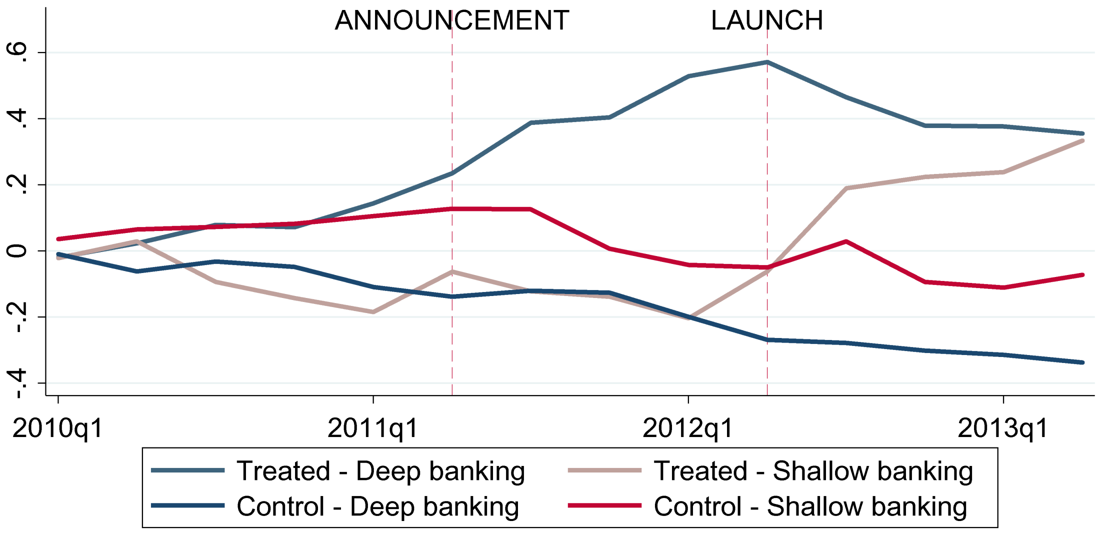
Notes: This figure shows the two main shale oil production regions analysed in the paper - Bakken and Niobrara - and the relevant pipeline structure. Arrows show the direction of the oil flow. Pipelines of significant/limited capacities are shown in black/beige. The pipeline investment project discussed in the paper is shown in orange. Source: Own work based on EIA data sources.

Figure 2.4: The impact of Seaway pipeline reversal on oil transport between PADDs.



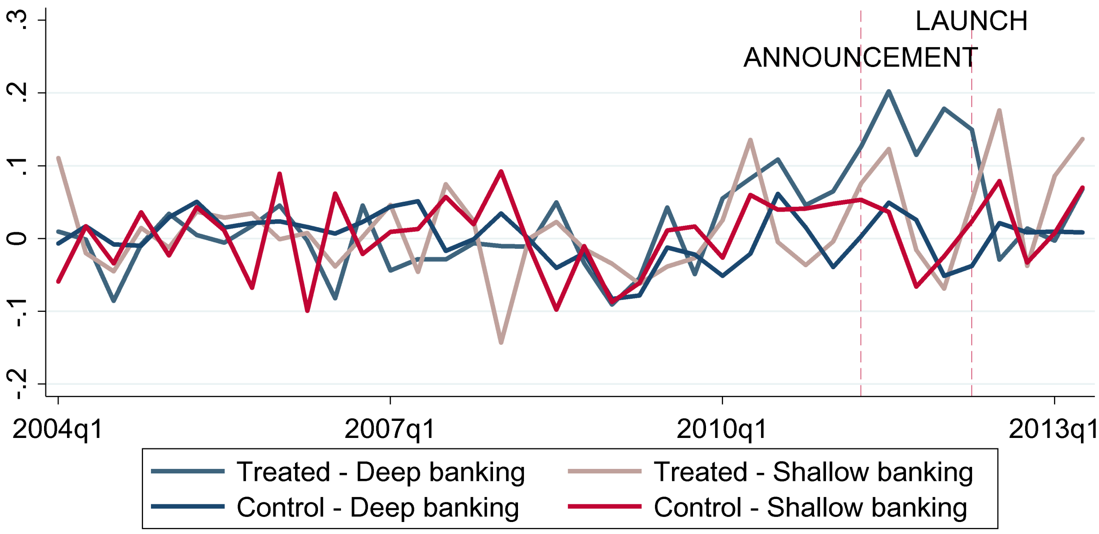
Notes: This figure shows the amounts of oil transported from districts including Bakken (PADD2) and Niobrara (PADD4) over time. Source: EIA.

Figure 2.5: Employment in the oil sector over time.



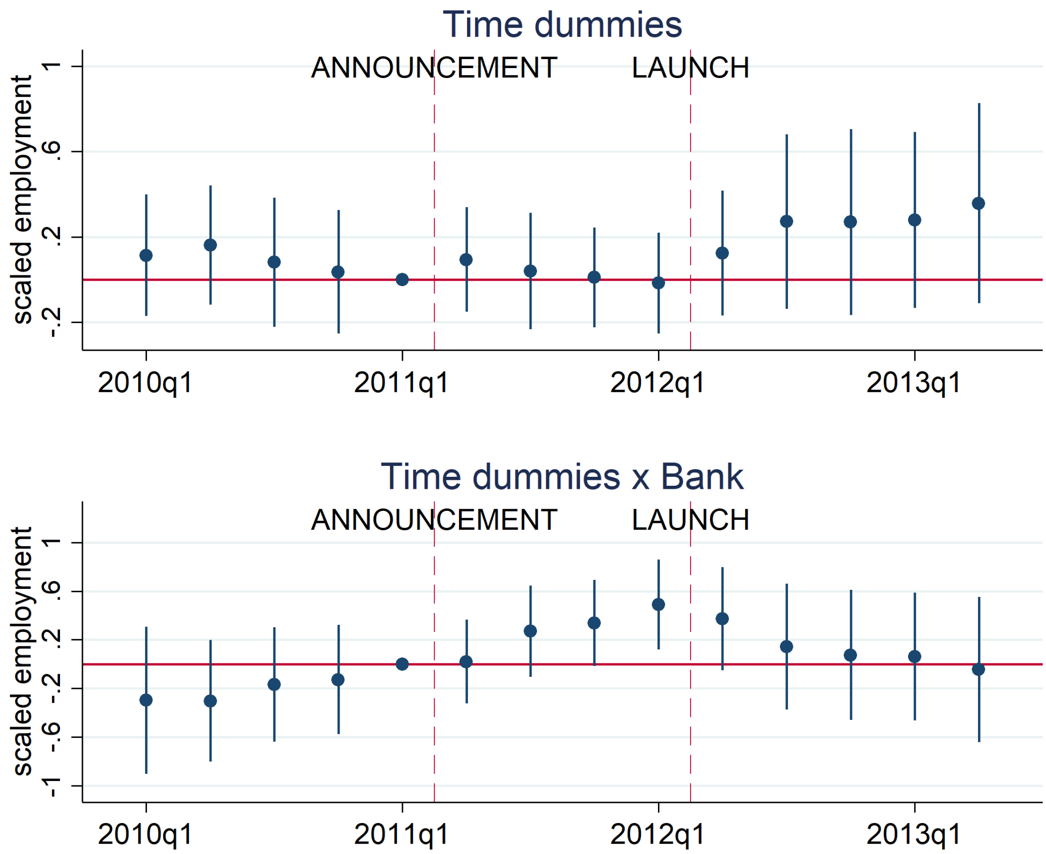
Notes: This figure shows employment in the oil sector by treatment status and banking development over time. *Employment* is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates separate for the treated and control counties.

Figure 2.6: Employment changes in the oil sector over time.



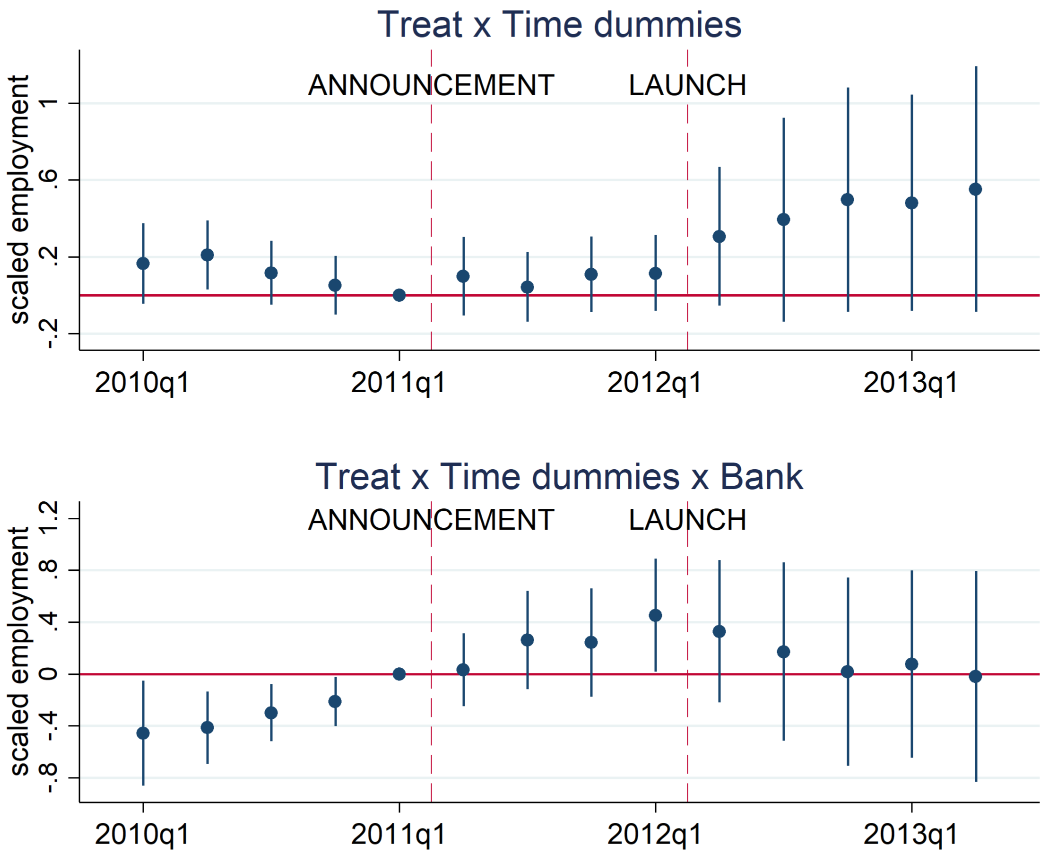
Notes: This figure shows employment in the oil sector by treatment status and banking development over time. *Employment* is defined as the quarterly change in employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality.

Figure 2.7: Time trends in the affected region (Bakken): Employment levels.



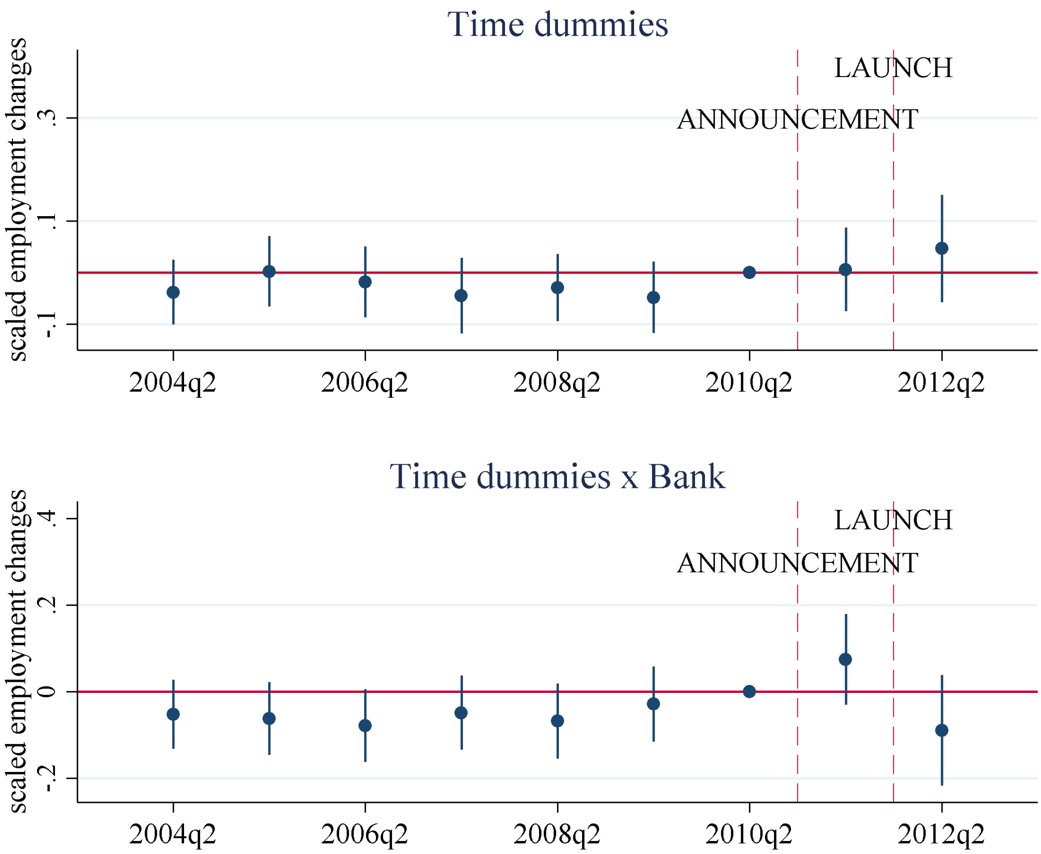
Notes: This figure shows the treatment effect (the impact of local banking) over time. The dependent variable is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates. The banking sector depth is evaluated based on the *Branches* metric (defined in Section 2.4.1). The dashed lines distinguish the pre-announcement period from the post-announcement period and the pre-realization period from the post-realization period. Sample: counties with limited oil transportation options and affected by the new pipeline project (Bakken region); 2010q1–2013q2. Data are winsorized in the right tail at 1% level.

Figure 2.8: Time trends in the extended sample (Bakken and Niobrara): Employment levels.



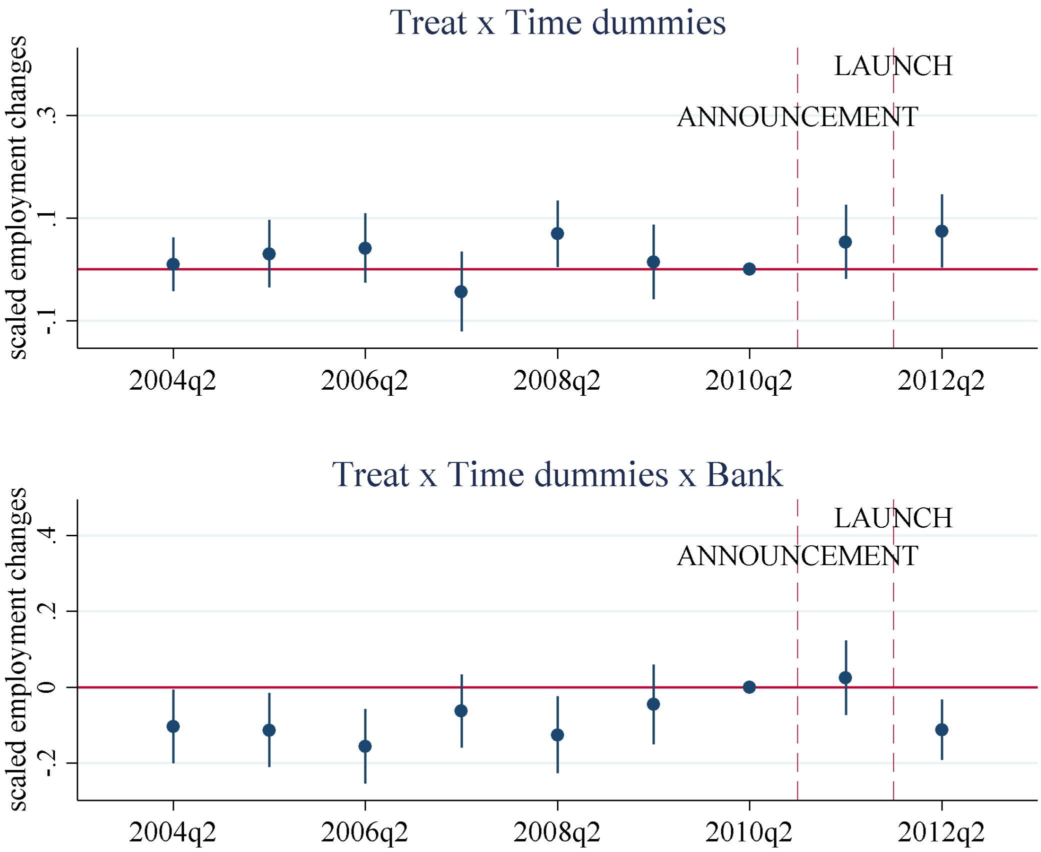
Notes: This figure shows the treatment effect (the impact of local banking) over time. The dependent variable is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates separate for the treated and control counties. The banking sector depth is evaluated based on *Branches* metric (defined in Section 2.4.1). The dashed lines distinguish the pre-announcement period from the post-announcement period and the pre-realization period from the post-realization period. Sample: counties with limited oil transportation options (Bakken and Niobrara regions); 2010q1–2013q2. Data are winsorized in the right tail at 1% level.

Figure 2.9: Time trends in the affected region (Bakken): Employment changes.



Notes: This figure shows the treatment effect (the impact of local banking) over time. The dependent variable is defined as the quarterly change in employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality. The banking sector depth is evaluated based on *Branches* metric (defined in Section 2.4.1). The dashed lines distinguish the pre-announcement period from the post-announcement period and the pre-realization period from the post-realization period. Sample: counties with limited oil transportation options and affected by the new pipeline project (Bakken region); 2004q1–2013q2. Data are winsorized in the right tail at 1% level.

Figure 2.10: Time trends in the extended sample (Bakken and Niobrara): Employment changes.



Notes: This figure shows the treatment effect (the impact of local banking) over time. The dependent variable is defined as the quarterly change in employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality. The banking sector depth is evaluated based on *Branches* metric (defined in Section 2.4.1). The dashed lines distinguish the pre-announcement period from the post-announcement period and the pre-realization period from the post-realization period. Sample: counties with limited oil transportation options (Bakken and Niobrara regions); 2004q1–2013q2. Data are winsorized in the right tail at 1% level.

2.9 Tables

Table 2.1: County summary statistics.

	p50	mean	sd
population	8962	44487.4	111436.7
area [sq mi]	2056.0	2377.3	1760.0
density	4.10	82.9	426.0
employment in oil sector	471.8	1898.8	3896.0
employment in oil sector / population	0.033	0.056	0.057
oil production	49089.3	428812.2	1117295.0
bank branches	1	8.25	19.8
loans (thous)	98696	12831336.4	19024731.9
loans per capita (thous p.c.)	14.6	436.2	1008.5
bank assets (thous)	855917	108205464.0	158603936.2
assets per capita (thous p.c.)	152.6	3759.8	8724.4
counties	105		
treated	37		
deep banking	65		

Notes: This table shows county summary statistics. Employment in the oil sector and oil production are calculated as the averages over the period 2008q2–2011q1. The other time-varying variables are fixed one year before the reversal announcement (2010q2).

Table 2.2: County summary statistics by banking deveopment.

	Deep banking			Shallow banking		
	p50	mean	sd	p50	mean	sd
population	8964	17936.8	31652.4	3384	4527.4	3050.2
area [sq mi]	2059.1	2256.0	1069.6	1870.9	2242.4	1164.9
density	4.05	8.20	12.6	1.80	2.15	1.31
employment in oil sector	488.4	1139.8	1520.9	128.8	201.7	271.3
employment in oil sector / population	0.051	0.077	0.071	0.021	0.046	0.048
oil production	10347.2	835161.7	1936667.9	103.2	291503.2	857393.9
bank branches	1	3.32	5.89	0	0.067	0.26
loans (thous)	539927.5	11973809.9	15890892.8	0	1783.8	6908.6
loans per capita (thous p.c.)	63.5	885.2	1620.3	0	0.53	2.04
bank assets (thous)	4724840.5	103665811.3	137598290.7	0	29282.7	113411.5
assets per capita (thous p.c.)	520.8	7638.6	14014.8	0	8.65	33.5
counties	22			15		

Notes: This table shows county summary statistics by banking development. The banking sector depth is evaluated based on the *Branches* metric (defined in Section 2.4.1). Employment in the oil sector and oil production are calculated as the averages over the period 2008q2–2011q1. The other time-varying variables are fixed one year before the announcement (2010q2).

Table 2.3: County summary statistics by treatment status.

	Treated (Bakken)			Control (Niobrara)		
	p50	mean	sd	p50	mean	sd
population	6429	12500.6	25151.4	12109.5	61891.9	134400.7
area [sq mi]	2056.0	2250.5	1093.2	2047.0	2446.3	2037.7
density	2.80	5.75	10.1	4.40	124.9	525.8
employment in oil sector	277.8	759.5	1263.3	612.9	2518.8	4647.3
employment in oil sector / population	0.048	0.064	0.064	0.029	0.051	0.053
oil production	365.6	614759.6	1595934.0	60714.9	327635.0	734649.7
bank branches	1	2	4.78	3	11.6	23.7
loans (thous)	67485	7120285.8	13520841.9	133449	15938819.8	20875285.6
loans per capita (thous p.c.)	14.6	526.6	1313.6	16.1	387.1	803.0
bank assets (thous)	422433	61651002.4	117069919.6	2447273	133536568.1	172727226.1
assets per capita (thous p.c.)	97.7	4545.4	11357.7	235.6	3332.4	6950.5
counties	37			68		
deep banking	22			43		

Notes: This table shows county summary statistics by treatment status. Employment in the oil sector and oil production are calculated as the averages over the period 2008q2–2011q1. The other time-varying variables are fixed one year before the announcement (2010q2).

Table 2.4: Employment after announcement and realization of the new pipeline project: Affected region (Bakken).

	Scaled with employment			Scaled with population		
	Branches	Loans	Assets	Branches	Loans	Assets
Interim	-0.049	-0.047	0.099	-1.02***	-0.81***	0.61
	(0.073)	(0.061)	(0.067)	(0.23)	(0.25)	(0.48)
Interim x Bank	0.38***	0.46***	0.20*	3.06***	3.32***	0.49
	(0.11)	(0.11)	(0.11)	(0.62)	(0.72)	(0.74)
Post	0.27**	0.18*	0.37***	-1.54***	-1.20***	1.06*
	(0.11)	(0.093)	(0.090)	(0.24)	(0.29)	(0.55)
Post x Bank	0.10	0.30**	-0.11	4.05***	4.24***	-0.49
	(0.14)	(0.13)	(0.13)	(0.69)	(0.81)	(0.82)
County FE	X	X	X	X	X	X
Obs	518	518	518	518	518	518

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of local banking on employment in the oil sector after the announcement and the realization of the new pipeline project. The specification is presented in Equation 2.1. The dependent variable is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean/population. The time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates. The dummy *Bank* identifies counties with better developed local banking sectors. Each column evaluates the local banking sector based on a different metric: *Branches*, *Loans* or *Assets* (defined in Section 2.4.1). The dummy *Interim* identifies the period between announcement and launch. The dummy *Post* distinguishes the pre-realization period from the post-realization period. Standard errors are robust. Sample: counties with limited oil transportation options and affected by the new pipeline project (Bakken region); 2010q1–2013q2. Data are winsorized in the right tail at 1% level. Coefficients and standard errors in the right panel *Scaled with population* are multiplied by 100.

Table 2.5: Employment after announcement and realization of the new pipeline project: Extended sample (Bakken and Niobrara).

	Scaled with employment		Scaled with population	
	Branches	Loans	Branches	Loans
Interim	-0.018	-0.029	-0.056	-0.12
	(0.051)	(0.044)	(0.087)	(0.100)
Interim x Bank	-0.076	-0.065	-0.17	-0.075
	(0.055)	(0.049)	(0.15)	(0.16)
Interim x Treat	-0.031	-0.018	-0.97***	-0.69*
	(0.091)	(0.075)	(0.35)	(0.35)
Interim x Treat x Bank	0.45**	0.52**	3.24**	3.39**
	(0.20)	(0.23)	(1.37)	(1.62)
Post	-0.13**	-0.16***	-0.30**	-0.68**
	(0.060)	(0.056)	(0.13)	(0.29)
Post x Bank	-0.12*	-0.089	-0.50*	0.11
	(0.065)	(0.060)	(0.26)	(0.33)
Post x Treat	0.40	0.34	-1.24**	-0.51
	(0.28)	(0.23)	(0.52)	(0.78)
Post x Treat x Bank	0.22	0.39	4.56**	4.13*
	(0.37)	(0.37)	(2.01)	(2.39)
County FE	X	X	X	X
Obs	1470	1470	1470	1470

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of local banking on employment in the oil sector after the announcement and the realization of the new pipeline project. The specification is presented in Equation 2.2. The dependent variable is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean/population. The underlying time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates separate for the treated and control counties. The dummy *Bank* identifies counties with better developed local banking sectors. Each column evaluates the local banking sector based on a different metric: *Branches* or *Loans* (defined in Section 2.4.1). The dummy $Treat$ identifies counties affected by the new pipeline project (Bakken). The dummy $Interim$ identifies the period between announcement and launch. The dummy $Post$ distinguishes the pre-realization from the post-realization periods. Standard errors are clustered at the county level. Sample: counties with limited oil transportation options (Bakken and Niobrara regions); 2010q1–2013q2. Data are winsorized in the right tail at 1% level. Coefficients and standard errors in the right panel *Scaled with population* are multiplied by 100.

Table 2.6: Employment changes after announcement and realization of the new pipeline project: Affected region (Bakken).

	Scaled with employment		Scaled with population	
	Branches	Loans	Branches	Loans
Interim	0.027	0.026	0.19	0.21
	(0.032)	(0.027)	(0.15)	(0.14)
Interim x Bank	0.13***	0.16***	0.61***	0.71***
	(0.044)	(0.043)	(0.21)	(0.22)
Post	0.081**	0.060*	0.25*	0.19
	(0.040)	(0.033)	(0.13)	(0.13)
Post x Bank	-0.044	-0.010	-0.022	0.094
	(0.049)	(0.047)	(0.22)	(0.24)
County FE	X	X	X	X
Obs	1368	1368	1368	1368

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of local banking on employment in the oil sector after the announcement and the realization of the new pipeline project. The specification is presented in Equation 2.1. The dependent variable is defined as the quarterly change in employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean/population. The underlying time series are adjusted for quarterly seasonality. The dummy *Bank* identifies counties with better developed local banking sectors. Each column evaluates the local banking sector based on a different metric: *Branches* or *Loans* (defined in Section 2.4.1). The dummy *Interim* identifies the period between announcement and launch. The dummy *Post* distinguishes the pre-realization from the post-realization period. Standard errors are robust. Sample: counties with limited oil transportation options and affected by the new pipeline project (Bakken region); 2004q1–2013q2. Data are winsorized in the right tail at 1% level. Coefficients and standard errors in the right panel *Scaled with population* are multiplied by 100.

Table 2.7: Employment changes after announcement and realization of the new pipeline project: Extended sample (Bakken and Niobrara).

	Scaled with employment		Scaled with population	
	Branches	Loans	Branches	Loans
Interim	-0.0026	-0.00090	-0.011	-0.065
	(0.017)	(0.015)	(0.040)	(0.070)
Interim x Bank	0.0093	0.0074	-0.011	0.086
	(0.018)	(0.016)	(0.065)	(0.076)
Interim x Treat	0.030	0.027	0.20*	0.27**
	(0.025)	(0.023)	(0.11)	(0.13)
Interim x Treat x Bank	0.12*	0.15**	0.62**	0.62*
	(0.059)	(0.067)	(0.30)	(0.34)
Post	0.027*	0.019	0.053	-0.045
	(0.015)	(0.014)	(0.036)	(0.065)
Post x Bank	-0.025	-0.014	-0.12*	0.039
	(0.017)	(0.016)	(0.064)	(0.076)
Post x Treat	0.054	0.041	0.20*	0.23**
	(0.045)	(0.038)	(0.11)	(0.11)
Post x Treat x Bank	-0.019	0.0034	0.099	0.055
	(0.055)	(0.053)	(0.19)	(0.21)
County FE	X	X	X	X
Obs	3952	3952	3952	3952

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of local banking on employment in the oil sector after the announcement and the realization of the new pipeline project. The specification is presented in Equation 2.2. The dependent variable is defined as the quarterly change in employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean/population. The underlying time series are adjusted for quarterly seasonality. The dummy $Bank$ identifies counties with better developed local banking sector. Each column evaluates banking sector based on a different metric: *Branches* or *Loans* (defined in Section 2.4.1). The dummy $Treat$ identifies counties affected by the new pipeline project (Bakken). The dummy $Interim$ identifies the period between announcement and launch. The dummy $Post$ distinguishes the pre-realization from the post-realization periods. Standard errors are clustered at the county level. Sample: counties with limited oil transportation options (Bakken and Niobrara regions); 2004q1–2013q2. Data are winsorized in the right tail at 1% level. Coefficients and standard errors in the right panel *Scaled with population* are multiplied by 100.

2.10 Appendix

Table B.1: List of counties.

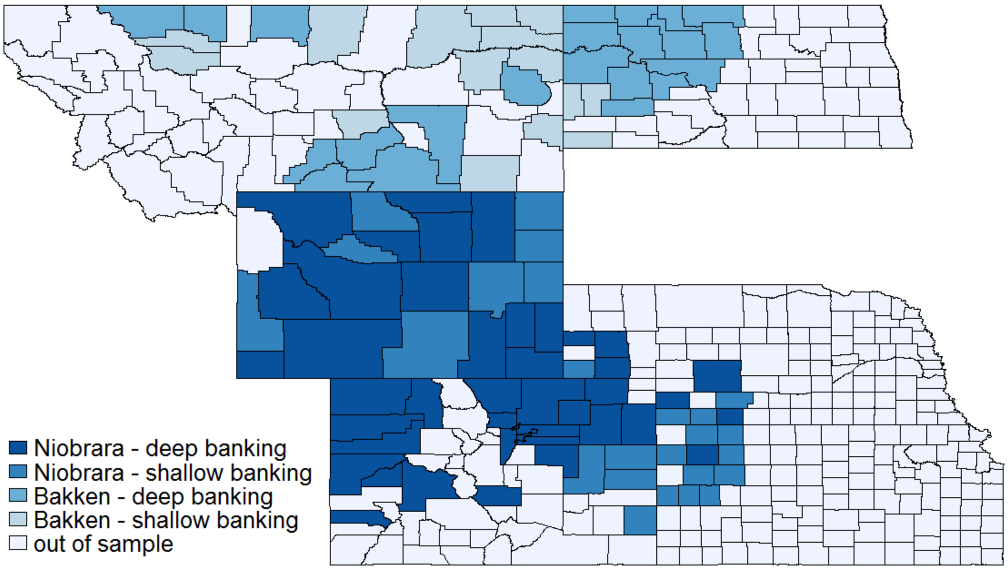
County	State	Treatment status	Banking development
Adams County	CO	0	1
Arapahoe County	CO	0	1
Boulder County	CO	0	1
Broomfield County	CO	0	1
Cheyenne County	CO	0	0
Delta County	CO	0	1
Denver County	CO	0	1
Elbert County	CO	0	1
Fremont County	CO	0	1
Garfield County	CO	0	1
Gunnison County	CO	0	1
Jefferson County	CO	0	1
Kit Carson County	CO	0	0
Larimer County	CO	0	1
Lincoln County	CO	0	0
Logan County	CO	0	1
Mesa County	CO	0	1
Moffat County	CO	0	1
Morgan County	CO	0	1
Prowers County	CO	0	0
Rio Blanco County	CO	0	1
Routt County	CO	0	1
San Miguel County	CO	0	1
Washington County	CO	0	1
Weld County	CO	0	1
Yuma County	CO	0	1
Decatur County	KS	0	0
Gove County	KS	0	0
Greeley County	KS	0	0
Logan County	KS	0	0
Rawlins County	KS	0	0
Scott County	KS	0	0
Sheridan County	KS	0	0
Sherman County	KS	0	0
Thomas County	KS	0	1
Wichita County	KS	0	0
...			

County	State	Treatment status	Banking development
Big Horn County	MT	1	1
Blaine County	MT	1	0
Carbon County	MT	1	1
Daniels County	MT	1	0
Dawson County	MT	1	1
Fallon County	MT	1	0
Glacier County	MT	1	1
Hill County	MT	1	1
McCone County	MT	1	0
Musselshell County	MT	1	0
Pondera County	MT	1	0
Powder River County	MT	1	0
Richland County	MT	1	0
Roosevelt County	MT	1	0
Rosebud County	MT	1	1
Sheridan County	MT	1	0
Stillwater County	MT	1	1
Teton County	MT	1	0
Toole County	MT	1	1
Valley County	MT	1	0
Yellowstone County	MT	1	1
Billings County	ND	1	0
Bottineau County	ND	1	1
Bowman County	ND	1	0
Burke County	ND	1	1
Divide County	ND	1	1
Dunn County	ND	1	1
Golden Valley County	ND	1	0
McHenry County	ND	1	1
McKenzie County	ND	1	1
McLean County	ND	1	1
Mercer County	ND	1	1
Mountrail County	ND	1	1
Renville County	ND	1	1
Stark County	ND	1	1
Ward County	ND	1	1
Williams County	ND	1	1
Chase County	NE	0	1
Cheyenne County	NE	0	1
Dundy County	NE	0	0
...			

County	State	Treatment status	Banking development
Frontier County	NE	0	0
Hitchcock County	NE	0	0
Kimball County	NE	0	0
Lincoln County	NE	0	1
Morrill County	NE	0	1
Red Willow County	NE	0	1
Scotts Bluff County	NE	0	1
Albany County	WY	0	1
Big Horn County	WY	0	0
Campbell County	WY	0	1
Carbon County	WY	0	0
Converse County	WY	0	0
Crook County	WY	0	0
Fremont County	WY	0	1
Goshen County	WY	0	1
Hot Springs County	WY	0	0
Johnson County	WY	0	1
Laramie County	WY	0	1
Lincoln County	WY	0	0
Natrona County	WY	0	1
Niobrara County	WY	0	0
Park County	WY	0	1
Platte County	WY	0	1
Sheridan County	WY	0	1
Sublette County	WY	0	1
Sweetwater County	WY	0	1
Uinta County	WY	0	1
Washakie County	WY	0	1
Weston County	WY	0	0

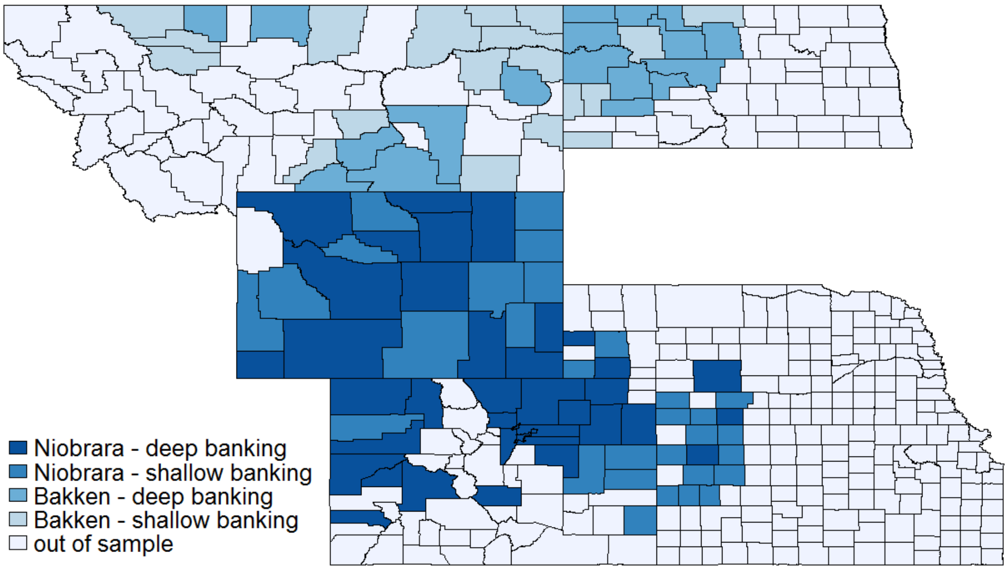
Notes: This table lists counties in the sample by treatment status and by banking development.

Figure B.1: Counties by treatment status and banking development (Branches).



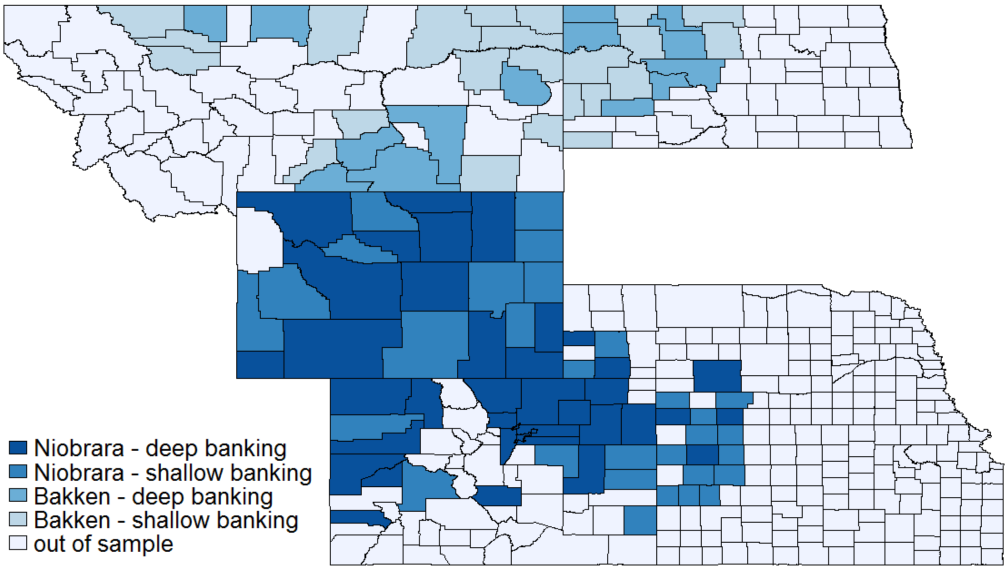
Notes: This figure shows a map of counties categorized by treatment status and banking development. The banking measure is based on the number of branches of commercial banks in a county (details in Section 2.4.1).

Figure B.2: Counties by treatment status and banking development (Loans).



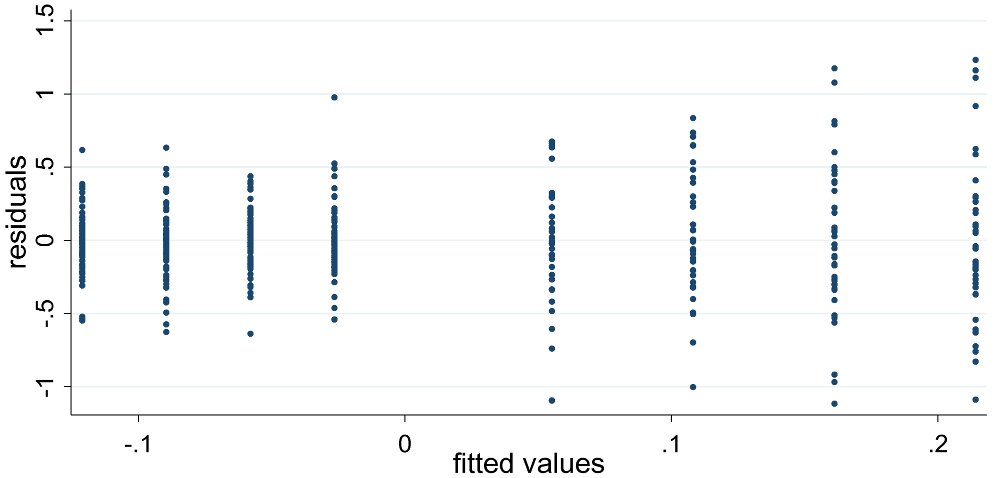
Notes: This figure shows a map of counties categorized by treatment status and banking development. The banking measure based on the amount of commercial loans granted in a county (details in Section 2.4.1).

Figure B.3: Counties by treatment status and banking development (Assets).



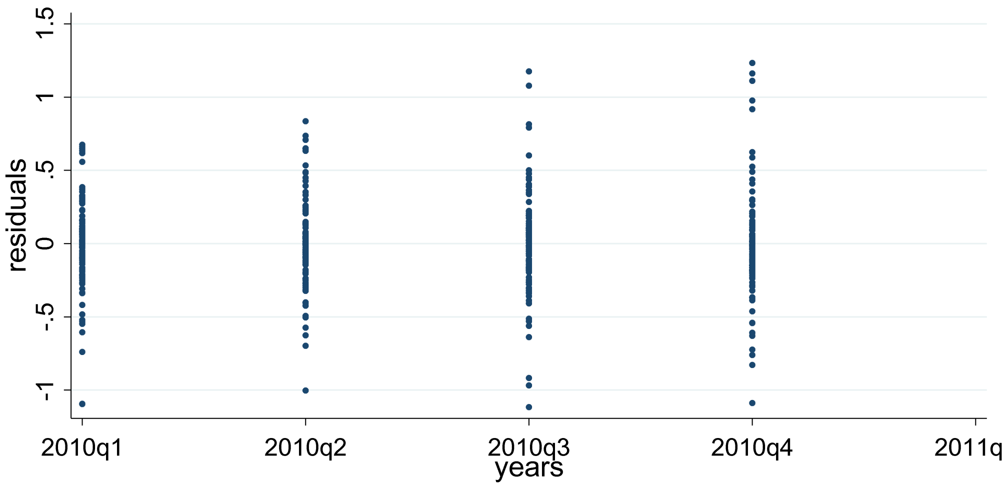
Notes: This figure shows a map of counties categorized by treatment status and banking development. The banking measure based on the assets of commercial banks in a county (details in Section 2.4.1).

Figure B.4: Detrending: Residuals vs Fitted values



Notes: This figure shows residuals and fitted values for the regression $y_{iyq} = \theta_0 Trend_y + \theta_1 Trend_y Treat_i + \eta_{iyq}$. i denotes counties, y years and q quarters. The dependent variable is defined as employment in the oil sector at the end of a quarter scaled with its pre-event mean. The time series do not include county-specific quarterly fixed effects. Sample: 2010q1–2010q4.

Figure B.5: Detrending: Residuals over time



Notes: The figure depicts residuals over time for the regression $y_{iyq} = \theta_0 Trend_y + \theta_1 Trend_y Treat_i + \eta_{iyq}$. i denotes counties, y years and q quarters. The dependent variable is defined as employment in the oil sector at the end of a quarter scaled with its pre-event mean. The time series do not include county-specific quarterly fixed effects. Sample: 2010q1–2010q4.

Table B.2: Employment after announcement and realization of the new pipeline project: Extended sample (Bakken and Niobrara), alternative specification.

	Scaled with employment	
	Branches	Loans
Interim	-0.054	-0.054
	(0.047)	(0.040)
Interim x Bank	-0.018	-0.020
	(0.051)	(0.045)
Interim x Treat	0.036	0.081
	(0.10)	(0.089)
Interim x Treat x Bank	0.35^{***}	0.33^{**}
	(0.13)	(0.13)
Post	-0.21^{***}	-0.21^{***}
	(0.028)	(0.028)
Post x Treat	0.53^{***}	0.53^{***}
	(0.18)	(0.18)
County FE	X	X
Obs	1470	1470

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This regression shows the impact of local banking on employment in the oil sector after the announcement and realization of a new pipeline project. The specification is presented in Equation 2.2 but the impact of banking is measured only in the *Interim* period. The dependent variable is defined as the employment in the oil sector measured at the end of a quarter and scaled with its pre-event mean. The underlying time series are adjusted for quarterly seasonality and detrended using pre-event trend estimates separate for the treated and control counties. The dummy *Bank* identifies counties with better developed local banking sectors. Each column evaluates the local banking sector based on a different metric: *Branches* or *Loans* (defined in Section 2.4.1). The dummy *Interim* identifies the period between announcement and launch. The dummy *Post* distinguishes the pre-realization from the post-realization period. Standard errors are clustered at the county level. Sample: counties with limited oil transportation options (Bakken and Niobrara regions); 2010q1–2013q2. Data are winsorized in the right tail at 1% level.

Chapter 3

Firm behaviour after R&D breakthroughs.

3.1 Introduction

Innovation is the force underlying growth and social welfare. Research and development process is, however, lengthy, costly and risky. The uncertainty is not resolved until the project is finalised, and cash flows are generated only conditional on success. Hence popular corporate finance questions may be answered differently in the context of R&D-intensive firms, and the topic of innovation attracts a lot of literature.

The analysis of unrealized innovation involves uncertainty, agency problems and financing (for example, Holmstrom (1989), Acharya and Subramaniam (2009), Manso (2011)). Realized innovation opens discussion about patenting and protection, which motivate innovators and are relevant for policy makers (for example, Klemperer (1990), Aghion and Tirole (1994), Lerner (1994)). However, to better understand the innovation process it is also interesting to look at the moment when the uncertainty is positively resolved. Successful completion of an R&D project is a one-time positive breakthrough event in a tedious process of developing a new product. An R&D success has the features of a rare random shock - it is uncertain whether and when it will occur. On the other hand, for a firm that continuously engages in research, it is also a necessary condition for survival in the medium horizon. It is not obvious how such an event translates into firm's strategy focused on long-term value maximisation.

In this paper I propose a new measure of an R&D breakthrough and analyse its impact on firm's strategy. An R&D breakthrough creates a windfall of investment opportunities. A company has a wider portfolio of projects to choose from and, more importantly, better access to external finance. The latter improves because innovation success signals profitability and may be used as a form of collateral. I test how successful completion of innovation process affects firm's investment and financing choice.

I base my study on the data from the pharmaceutical industry and define an *R&D breakthrough* as regulator's consent to introduce a new oncology treatment method to the market. The consent is granted by Food and Drug Administration (FDA) in the form of the approval. I study firm's strategy in the four quarters following the approval relative to the four previous quarters. I compare the strategy of a successful firm in a difference-in-differences setting using as a control group other companies in the last stage of the development process. Complicated regulations in the pharmaceutical industry make approvals, and, what is more important, the timing of approvals, uncertain and unpredictable. Therefore companies engaged in the last stages of clinical trials in each quarter are assumed to be randomly sorted into treatment and control groups. To ensure this assumption is likely to hold I control for firms' observable characteristics.

I find that companies that obtain the FDA approval increase their capital expenditure relative to other innovative companies in the last stage of an R&D process. The increase within a year amounts to around 2% in terms of company's assets. There is no evidence that firms change their research and development expense, cash holdings and short-term investments or debt-equity ratio. Lack of response of debt and cash suggests that innovative firms in the last stages of a research and development process do not suffer from financial constraints. Therefore the increase in investment measured by capital expenditure is not likely to result from better financing options. New investment also does not aim to replace the finalised innovation in the production pipeline because research and development expense does not change.

The main contribution of the paper is to evaluate innovativeness with FDA approvals for new oncology treatment. This measure constitutes an alternative to the patent-based measures of innovativeness, which are often applied in the literature (for example, Jaffe, Trajtenberg and Henderson (1993), Lerner, Sorensen and Strömberg (2011), Howell (2017)). There are several reasons why my measure should be more accurate and precise compared to obtained patents and citations. Firstly, approvals identify only successful research and directly translate into real money. Filed patents do not guarantee that the product is marketable in future and are not informative when a patented subject may bring profits. Secondly, patent filings provide different effective marketing rights protection and profits depending on how quickly a product is marketed. Therefore the level of protection they guarantee is not homogeneous across firms and subjects of the patent filings. It is true even for products from the same industry. Last but not least, several patents can be filed on one product. Hence the same innovation may be unnecessarily counted multiple times. Furthermore, even though a measure based on approvals is more closely related to the economic success, it is not affected by external economic conditions as, for example, demand shocks.

Company's strategy after a positive or negative shock has been widely studied in the context of investment. Several authors analyse it in the quasi-natural experiment environment (for example, Blanchard et al. (1994), Lamont (1997), Rauh (2006)) and confirm the hypothesis that external capital markets are imperfect, contrary to the model of Modigliani and Miller (1958).

The above studies, however, do not focus on R&D-intensive companies. Such companies may behave differently because innovation is a lengthy process, marked with high uncertainty, which is not resolved even in the final stage of the development process (Nanda, Younge and Fleming (2013)). In such environment it is particularly difficult to raise debt. Even if companies could convince lenders about the profitability of the project they may not want to do so to avoid information disclosure (Thakor and Lo (2015)). Hence alternative forms of financing play an important role and financing R&D investment constitutes a separate stream of literature. Nanda, Younge and Fleming (2013) investigate the role of venture capital in financing R&D. Howell (2017) analyses the role of grants in financing innovation and finds that financing support increases the probability of success.

In my paper I study the strategy of an R&D-intensive firm after a specific positive shock to investment opportunities, i.e. successful completion of innovation. Similar exercise is done by Phillips and Sertsios (2014) who also examine a positive shock to investment opportunities in the innovative industry. In the context of public and private companies producing medical devices, they document that publicly traded firms after a positive shock to investment opportunities increase their external financing and subsequent product introductions more compared to private firms. My sample consists only of publicly traded firms, but I do not find any change in financing decisions induced by the shock. I follow companies only one year after the R&D breakthrough though, while Phillips and Sertsios rely on a three-year observation window arguing that financing activity can last up to two years.

My study is also related to the literature about predictability of the research potential. Cohen, Diether and Malloy (2012) claim that firm's ability to innovate is predictable but this predictability is ignored by market participants. To measure R&D efforts and abilities they rely on patent database. In my paper I claim that companies cannot predict the timing of the FDA drug approval, but it does not necessarily contradict their findings. Firstly, I account for company's fundamentals and research efforts. Secondly, I take into account specific characteristics of the pharmaceutical market and look at projects close to completion.

The paper is organized as follows. In Section 3.2 I characterise the research and development process in the pharmaceutical industry. I pay attention to complicated regulations that induce even more uncertainty. The data collection process is described in Section 3.3. In Section 3.4 I present the methodology. The empirical results are summarised in Section 3.5. Section 3.6 concludes.

3.2 Innovation in pharmaceutical industry

3.2.1 General overview

Pharmaceutical industry has several specific features. From the social perspective, it is an important area of research that brings significant benefits to public health. On the other hand, it is potentially very profitable. It forms the largest market in the United States reaching around \$300 billion is terms of sales and accounting for about 36% of sales globally as of 2009. What is more, once a product is on the market, the operational risk related to its sale is relatively small. The development process that brings a new treatment to the market is itself crucial. Even though both society and companies are interested in new treatment methods, the market has to be thoroughly regulated to ensure safety of the drugs available to patients. In addition, regulations defining patent protection and market exclusivity rights motivate inventors to engage in the most costly and/or uncertain research studies for the sake of patients.

Out of all medical conditions, the area of oncology gains on relevance most rapidly. As presented in Figure 3.1, cancer is the second leading cause of death as of 2013. The difference between the first and second cause is small (32% to 30%) and has been getting smaller over the years. People have significantly improved prevention and treatment of heart illnesses, while the area of oncology is still a great challenge. Furthermore, oncology treatment becomes more urgent as the number of patients increases. It requires long-term studies as the true treatment effects often can be assessed only after a few years. Despite the intensified research efforts there is no *cure* and the uncertainty about effectiveness of a treatment for an individual patient is very big. Summing up, among various medical research topics oncology is one of the most costly ones, for which successes are rare and which brings massive benefits to society. These are the reasons why getting approval to introduce an oncology treatment can be perceived as an R&D breakthrough for a company engaged in pharmaceutical innovation.

3.2.2 Food and Drug Administration regulations

This section describes procedures that have to be fulfilled before a new drug is made available to patients. The aim of the overview is to explain the difficulties faced by innovators in the pharmaceutical industry, especially in the last stages of an R&D process.

To get approval for a new drug in the United States a company has to go through several stages of studies. Firstly, it has to test the safety of a drug in preclinical trials before it can be used on humans.[1] Once a drug is considered safe enough, a company may start clinical trials in order to assess its safety and efficacy on real patients.

Clinical trials involve at least three phases.[2] Phase III trials are the most crucial studies carried out on the large samples of patients to prove the efficacy.[3] The development process may be also shorter if a company obtains an *accelerated approval* designation.[4] This allows a company to present the results based on surrogate endpoints which are markers able to predict final clinical benefit.[5]

Clinical trials have to be formally registered. For *serious and life-threatening* conditions the requirement to submit basic information about clinical trials was introduced in 1997.[6] In 2007[7] FDA clarified the requirements for registration[8] and also set penalties for non-compliance. This law allows me to identify companies

[1]The information gathered in this phase is summarised in Investigational New Drug (IND) application that is submitted to Food and Drug Administration (FDA).

[2]Probabilities of going through subsequent trials can be found in for example Krieger and Ruback (2001).

[3]Phase I trials are to determine the safety and dosing of a drug on healthy volunteers. Phase II is carried out on sick patients to further learn about the safety of a drug and get first results about its efficacy. The trials are sometimes extended and include phase IV. These are carried when the drug is already approved and marketed to confirm the results from previous trials. Sometimes a company commits to phase IV actions in other circumstances, for example, to compare it with other drugs, to monitor long-term influence or to analyse cost-effectiveness.

[4]They are granted for drugs that "treat serious conditions, and that fill an unmet medical need" (FDA guidelines).

[5]*Accelerated approval* application may be submitted without results from phase III clinical trials. The practice shows that majority of *accelerated approvals* rely on phase II trials only (George (2003)).

[6]FDA Modernization Act.

[7]Food and Drug Administration Amendments Act of 2007 (FDAAA).

[8]Formal registration is necessary for *applicable* clinical trials. Generally, studies that meet the definition of *phase 1* are not applicable drug clinical trials.

aiming to introduce a new oncology drug to the market.

If a drug passes phase III trials, a company can ask for approval by submitting a New Drug Application (NDA) or Biologics License Application (BLA) if a drug is a biologic.[9] FDA does not make public its negative decisions or clinical assessments on which they are based.

Patents and exclusivity marketing rights can be granted to companies in order to protect the innovation. Patents are issued by Patent Trademark Office (PTO) and they exclude others from making, using, or selling an invention. They are granted for 20 years[10] from the issue date. As companies usually apply in the early stages of the development process, the effective patent protection is much shorter. It is possible to apply for the Patent Term Restoration (PTR) that extends the patent life by up to five years. Patents and patent-citations constitute common measures of innovativeness.[11] While they may properly approximate company's research efforts, they do not accurately measure how the innovation translates to company profits. Therefore they also do not capture breakthrough events of the innovation process.

Exclusivity became an important provision in the Hatch-Waxman Act in 1984.[12] Exclusivity marketing rights are granted by FDA after the approval and equal three to seven years[13]. Exclusivity period ensures that for a limited period of time a company that successfully finalised the development of an innovative drug is the drug's sole supplier. They provide a link between successful research process and profits.

The outlined protection is necessary because of competitors producing low-cost

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[9] Biologics are usually made from a variety of natural sources (human, animal or microorganism). Biologics are regulated by Center for Biologic Evaluation and Research (within FDA).

[10] 17 years before 1995.

[11] Examples of papers using patent-based metrics of innovation include, for example, Lerner, Sorensen and Strömberg (2011), Cohen, Diether and Malloy (2013), Chemmanur, Loutskina and Tian (2014), Fang, Tian and Tice (2014), Tian and Wang (2014), Bernstein (2015), Bernstein, Giroud and Townsend (2016), Howell (2017), Balsmeier, Fleming and Manso (2017), and many others.

[12] Drug Price Competition and Patent Term Act of 1984

[13] Five years for New Chemical Entities (NCEs) (New Chemical Entity is a drug that contains no active moiety that has been approved by the FDA in any other application submitted under section 505(b) of the Federal Food, Drug, and Cosmetic Act), three years for other products and seven years is for Orphan Drugs (drugs for so called *rare diseases* for which patient populations are small and one cannot expect that the costs of developing the drug will be recovered within seven years following the approval).

generic drugs.[14] The process of getting approval for a generic equivalent drug is much easier, quicker, less uncertain and cheaper.[15] A company has to prove only that its product is bioequivalent with respect to pharmacokinetic and pharmacodynamic properties, i.e. performs in the same manner. This can be often proved by measuring how much of the active ingredient reaches the bloodstream in a specific time period. In general, generic and branded counterparts are equivalent and product positioning in the case of treatment of serious diseases is limited. Introduction of generic drugs to the market increases the competition. From the social perspective it is beneficial because it speeds up the drug production process and provides low-cost treatment methods. From the perspective of the innovator, however, it once again emphasizes the importance of the FDA approval because this limited period shortly after the approval is the period when the company is rewarded for its innovativeness.

3.2.3 Timing uncertainty

Carrying clinical trials does not guarantee a success. Even if a company relies on promising early results and has secured financing there is always uncertainty if a drug will be marketable. Applications for FDA approvals are made for less than 20% phase III studies (Krieger and Ruback (2001)) and even conditional on filing there is no certainty about the approval. It introduces randomness into the selection with regard to which companies get the approval and related benefits of the monopolistic sales. However, this it not the only source of randomness in the approval process. Conditional on knowing the new drug is marketable there is still uncertainty when it is introduced to the market because of the lengthy, multi-stage approval process.

After promising results of the last-stage clinical trials a company submits an application for the FDA approval. When the application is submitted FDA has 60 days

[14] A *generic* product is "one that is comparable to an innovator drug product in dosage form, strength, route of administration, quality, performance characteristics and intended use" (FDA guidelines).

[15] A generic drug a company has to submit and Abbreviated New Drug Application (ANDA), which usually does not require carrying out any preclinical or clinical trials. As generic drugs can be marketed only after the patent and exclusivity periods expire, FDA may grant tentative approval if the drug is ready earlier. It delays the final approval until exclusivity issues are resolved.

to accept it for review and 74 days to communicate to a company any significant deficiencies of the application. In this initial phase it is also decided whether a review gets *priority review* status and whether fees related to the process are paid, waived or exempted.[16] *Priority* is granted if a drug introduces "significant improvements in the safety or effectiveness of the treatment, diagnosis, or prevention of serious conditions when compared to standard applications."[17] It affects the approval process since FDA dedicates all attention and resources to evaluate such drugs.

After the application is accepted for review, FDA has the goal to review 90% of incoming original NDA and BLA applications within 10 months.[18] The *review clock* can be extended by three months if needed. During the review process FDA may issue letters when it needs assistance and further information to carry out the assessment. These letters may change the review time goal if there is a considerable amount of information that has to be provided by a company.

The review process can be sped up if a company obtains a *fast track*[19] or *breakthrough therapy*[20] status. The *fast track* designation makes the company eligible for more frequent meetings with FDA or sending completed sections of the NDA/BLA application before the whole form is completed (*rolling review*). The *breakthrough therapy* status offers additionally more intensive guidance since the early stages of clinical trials.

The initial application is often rejected. FDA authorities may provide guidelines about additional tests that has to be done which may require additional, costly trials. If a company decides to work further on the drug it may resubmit the application once again or more if necessary. The *review clock* goals for resubmissions are

[16] A *priority* status is considered for each application, but may be also requested by a company.

[17] FDA guidelines.

[18] Source: *https://www.fda.gov/ForIndustry/UserFees/PrescriptionDrugUserFee/ ucm118925* (2007).

[19] The *fast track* approvals are designed to facilitate development of drugs to "treat serious conditions and fill an unmet medical need" (FDA guidelines). The request for the *Fast Track* designation can be done at any stage of the development process and FDA has 60 days to deliver the decision.

[20] *Breakthrough Therapy* designation is designed to expedite the development drugs for a serious condition for which preliminary clinical evidence "indicates that the drug may demonstrate substantial improvement over available therapy on a clinically significant endpoint" (FDA guidelines). To obtain a designation the company has to submit a request before the end-of-phase-II meetings and FDA has 60 days to deliver a decision.

shorter and depending on the type of resubmission equal two or six months.

As outlined above, the timing of the review process is uncertain and difficult to predict. It involves several stages that take various times, can be extended or interrupted. Drug's indication and status also play a significant role. Additionally, the process often involves resubmissions. Based on these characteristics in my analysis I assume the FDA approval cannot be predicted in the medium horizon of one or two years. Therefore an innovative firm does not know when the drug will be introduced to the market and cannot act before the final decision is made. The approval comes as a random windfall of investment opportunities.

3.3 Data sources and main variables

3.3.1 Data

Clinical Trials

I collect the data on clinical trials from the website *ClinicalTrials.gov*,[21] which gathers information about all trials conducted around the world. I focus on clinical trials for oncology drugs carried out during the period from 1997 to 2014.[22] The dataset theoretically includes all research studies conducted in the chosen period because, as described in Section 3.2, trials for treatments of *serious or life-threatening* conditions, including the whole area of oncology, are subject to mandatory registration. I consider only not recruiting, closed, phase III trials because they are the last step before a company can file for the FDA approval.[23]

Each observation in the data set refers to a separate study. I keep only studies sponsored by for-profit companies.[24] I transform the database to get observations

[21] *http://clinicaltrials.gov/*

[22] Strictly speaking, drugs and biologics are different products. To simplify description, however, I use the term *drug* for both drugs and biologics.

[23] The exception here are drugs with the *accelerated approval* status, which do not always require phase III trials. These are rare situations, but theoretically some drugs qualify for the approval application after phase II trials. Since they may be difficult to identify in the trials data, I do not include them in my analysis.

[24] Other sponsor types include academic institutions, National Institutes of Health (NIH) and other government institutions.

at the firm-quarter level. A company is in the sample if it has carried out at least one phase III clinical trial in the past two years. I drop companies after they are acquired, privatised or go bankrupt. For each quarter I calculate firm's research effort which depends on the average number of phase III clinical trials conducted in a quarter relative to firm size.[25]

Compustat, CRSP

Companies' fundamentals for the relevant period are obtained from Compustat North America, Compustat Global and CRSP databases. I restrict the sample to listed companies because of the data availability. I drop joint ventures and subsidiaries for which the fundamentals are available only for a parent company. The rationale behind is that the strategy of a subsidiary in such a situation cannot be tracked, while the strategy of a parent depends on more factors. I do not exclude divisions of the companies that appear under separate names. The distinction whether a company truly is a subsidiary or just a division is made case by case for each company.

The variables of interest include capital expenditure, research and development expense, cash and short-term investments and debt-equity ratio. Flow variables, capital expenditure and research and development expense, are summed over four quarters and scaled with company's assets. The sum is taken to avoid problems with seasonality which appears in the quarterly data of flow variables. I scale stock variables, cash and short-term investments and debt-equity ratio, with company's assets and calculate the average over four quarters.

Other variables included in the analysis, firm size, debt and profitability, are used to match similar companies. Size is calculated as natural logarithm of company's assets in the past four quarters. Debt is measured as the four-quarter average of total liabilities scaled with total assets. Profitability is measured as the four-quarter average of earnings before tax scaled with total assets. I also create a proxy for company's origin. Dummy $foreign$ equals one if a company is listed on the American

[25] Details in the Appendix.

stock exchange, and zero otherwise. Detailed definitions of all relevant variables are included in the Appendix.

I drop firms with negative equity in the past year. Equity is calculated as total assets minus total liabilities. I also drop firms with negative capital expenditure, negative research and development expense and negative cash and short-term investments.

FDA approvals

The list of approved oncology drugs is published on the National Cancer Institute website.[26] The details regarding the approvals can be found in the FDA database (Drugs@FDA database).[27] The database contains the whole history for each drug including the initial approval, approvals of generic alternatives and all supplements that introduce minor or major improvements. I focus on the initial approvals (NDA or BLA) that bring an innovative drug to the market for the first time. These are the approvals that required most research effort, are most uncertain, and in the case of success give market exclusivity rights.

Information in the Drugs@FDA database dates back to 1939. I downloaded the data for the period from 1997 to 2014, but this observation window is determined by the availability of the clinical trials documentation. The main variables include application number, approval date (day-moth-year), name of a company holding the marketing rights, drug name, main active ingredient, marketing status, dosage and strength, chemical type and review classification. For the sake of the analysis I need the approval date and the name of the initial sponsor (applicant). Identifying the applicant is not easy because the database is updated and shows the current holder of marketing rights. This is often a different entity than the originator or an application sponsor because of the frequent mergers and acquisition in the pharmaceutical sector, and because the marketing rights can be sold. Determining the initial sponsor is necessary because it is a company that carries the trials, obtains

[26] *http://www.cancer.gov/about-cancer/treatment/drugs#E*, accessed in December 2014.

[27] The database does not contain entries about combinations of drugs or vaccines. Also, some biologics regulated by the Centre for Biologics Evaluation and Research are not in the database.

the potential approval and benefits immediately after the FDA positive decision. To uncover a company that was granted the approval I look at the original approval letters that are available for almost all NDA applications.

Finally, I merge information about granted approvals with the dataset of companies conducting clinical trials. I am able to match only 87 out of 143 approvals granted in years from 1997 to 2014. This poor matching accuracy is a weakness of the analysis. Firstly, it diminishes the sample of rare events and lowers the power of the statistical tests. Secondly, some of the companies conducting clinical trials may not be properly identified as treated. Such mistakes introduce additional noise. Finally, if the unmatched data are systematically different, then the results would be valid for the sample of the matched approvals only. I have not found confirmation, however, that this could be the case.

One of the possible reasons of poor matching accuracy is that not matched approvals were the ones with a special status and did not require phase III trials. It seems unlikely though that so many approvals would fall into this category. Another possibility is that some applications were filed by a different company than the trials' sponsor. Sponsors of trials usually work with collaborators, hence it is possible that more companies are involved in testing. Unfortunately, the available data do not allow me to determine what role a collaborator has in the trials. Therefore I do not include collaborators in the sample. In general, the above issue is partially alleviated by the matching procedure that allows me to account for observable differences between companies. I continue with the sample of 87 approvals. This number further lowers to 73 due to lack of full financial data.

3.3.2 Sample summary

Subsequent approvals

Even though the data cover several years I look at the approvals through a two-period window, i.e. I compare what happened in the year following and preceding the approval. The quarter of the approval is included in the pre-event period. In general, I do not exclude companies from the sample after they get approval. Pharmaceutical companies need constant innovation in order to grow and stay on the market. Hence each approval is a huge success for a company, and firms cannot stay passive after just one breakthrough. The approval can be perceived as a medium-term goal, and in the long-term firms need to continue costly R&D efforts even when they already have a drug on the market. Therefore as long as a company engages in clinical trials, it is reasonable to assume that the FDA approval has an effect irrespective whether it is a first or a subsequent one. I exclude, however, approvals that occurred within four quarters of the previous approval. I call them *overlapping* approvals. In such cases the pre-event period of the second approval overlaps with the post-event period of the first approval. It is impossible to disentangle the effect of the first approval and accurately measure firm's strategy in the pre-event period of the second approval. In a year before and after the approval the company also cannot serve as a control company. I end up with 60 approvals in the sample.

Summary

The main sample consists of 3,494 firm-quarter observations. It is an unbalanced panel of firms with a chance for approval in a given quarter. Dates span from 1997 to 2014 and and there are 159 distinct companies. The number of industry sponsors of clinical trials was higher and equal to 303 in the clinical trials dataset. The majority (87) of the excluded companies were private firms for which the fundamentals are not available. Other companies were excluded as subsidiaries or due to missing data or unrealistic values reported as described in the previous paragraphs.

Table 3.1 presents the basic characteristics of the firm-quarter observations in the sample.[28] The data are presented at the firm-quarter level because in the subsequent parts of the analysis the observations are matched based on firms characteristics and date. The statistics can be intuitively viewed as weighted values in which bigger weights are assigned to companies carrying out trials for longer.

[28]Analogical table based on the sample in which I do not exclude subsequent approvals is in the Appendix (Table C.1).

Firms in the sample differ a lot in terms of size and their spending. To smooth the cross-sectional variability in firms' assets, in the matching stage I measure the size in terms of the logarithm. There is also variability in the number of trials that companies carry out both in the absolute terms and relative to the firm size. Around a third of companies are foreign (46 out of 159). The companies are highly levered and many of them are unprofitable. This is the standard feature of innovative companies, which have to bear large costs of the R&D process and can realize benefits only upon successful project completion.

Out of 159 companies in the sample only 28 got approval, which constitutes less than 18%. The frequency of the innovation breakthroughs is very small and amounts to around 1.8%. It is much smaller than the percentage of successful companies. It may be justified by a survivorship bias since companies failing the last stage of the innovation process are less likely to continue operations and often go bankrupt or are acquired by another entity.

Figure 3.2 presents the number of trials carried out by the companies in the sample over time. The number of trials grows significantly since 1997 and stabilises around 400 in a quarter. The plot shows only trials for new treatments in the area of oncology. The increase in the number of ongoing studies may represent the growth of the pharmaceutical research, but there are several other factors affecting the observed pattern. Firstly, uncovering sponsors and matching trials with companies for which financial data are available is easier for more recent trials. Secondly, regulations to register studies of life threatening diseases were introduced in 1997, while the requirement to register all studies was established in 2007. Even though trials of oncology treatments theoretically should be registered from 1997 onwards, there could be an adaptation period. Also, before 2007 there were no official penalties for non-compliance. Finally, I focus on trials in which the main sponsor is a for-profit company while the type of entities sponsoring clinical trials may change over time. The same figure presents also the number of approvals granted to companies in the sample. The red dots represent the number of approvals granted in a quarter if the number is non-zero. The values vary from one to four per quarter. Approvals are evenly distributed over time with the exception of the initial period when they are more rare. Overall, the documented pattern confirms that not only drug regulations, but also the whole research environment changes over time.

3.4 Empirical strategy

3.4.1 Identification

The aim of the analysis is to evaluate the impact of an *R&D* breakthrough on company's strategy. It is measured in the sample of companies in the phase III clinical trials for a new oncology treatment. The *R&D* breakthrough is defined as the FDA approval to market a new oncology drug. Conducting last-stage clinical trials exposes some firms to treatment because some of the firms obtain approval and some not. I match observations in order to ensure that treated and non-treated companies are similar. The matching methodology is described in detail in Section 3.4.2. The treatment is assumed to be as good as random because of the uncertainty related to the research process and, most importantly, because of the unknown timing of approval described in Section 3.2.3. The related difference-in-differences regression is specified as follows:

$$y_{iqt} = \alpha_{iq} + \alpha_P \times Post_t + \alpha_{PT} Post_t \times Treat_{iq} + \epsilon_{iqt}. \tag{3.1}$$

y_{iqt} denotes the dependent variable of interest for a company i in period t around year-quarter q, in which a company got/could get approval. A company i is assumed to be a different entity in every year-quarter q. The dummy $Post_t$ identifies pre- and post-approval periods.[29] α_{iq} are firm-year-quarter fixed effects that capture time-invariant effects. The dummy $Treat_{iq}$ is equal to one if a company i obtained the FDA approval in year-quarter q. It is not included separately in the regression

[29] Companies in the control group did not get approval in a given quarter. t therefore denotes *potential approval time* for these companies. In practice control companies are matched with treated companies and I study the strategy of both entities around the time when the treated one got approval. Matching is explained in detail the following paragraphs.

because it is absorbed by α_{iq} fixed effects. The regression is weighted after the matching procedure to ensure covariate balance. The difference in the strategy response between the treated and not treated entities can be interpreted as the causal effect of the event (FDA approval) and is measured by α_{PT}. Standard errors are clustered at firm-year-quarter level.

I also consider an alternative specification where I directly identify matched observations with fixed effects:

$$y_{igt} = \alpha_g + \alpha_P \times Post_t + \alpha_T Treat_{ig} + \alpha_{PT} Post_t \times Treat_{ig} + \epsilon_{igt}. \tag{3.2}$$

g identifies matched pairs. The dummy $Treat_{ig}$ distinguishes between treated and control observations within matched pairs. Standard errors are clustered at the pair level.

The main assumption underlying the validity of my regressions states that time effects in the treatment and control groups are the same. The idea behind is that treated and control entities would have developed in the same way if not for the event that randomly hits one of them. While all companies carrying out clinical trials have chances for approval, the sample is very heterogeneous - firms differ in size, access to financing and research pipelines. There is a concern that these differences between companies are related both to the probability of treatment and to the outcome variables. To ensure the identification assumption holds I match treated and control companies based on several observables. The matching process is described below.

3.4.2 Propensity score matching

The idea of propensity score[30] comes from Rosenbaum and Rubin (1983) who develop standard matching procedures. Propensity score itself is a conditional probability that an observation is going to be treated. It is conditioned on some vector of

[30]Review of matching on propensity score and alternative estimators of average treatment effects can be found in Imbens (2004).

observable variables **X**. The main conclusion regarding propensity score matching is that it is equivalent to matching on the observables **X**. Hence instead of multi-dimensional matching one can balance covariates in treatment and control groups based on the propensity score.

Matching helps reducing bias related to observable characteristics. It does not reduce the imbalance coming from latent covariates. For example, it does not resolve the issue that some initiatives may have more potential than others. In my setting, however, the imbalance in the latent variables is not the main concern because I look at nearly completed studies. The least promising innovations are naturally filtered out when a product passes subsequent development stages. A treatment that reaches phase II or phase III clinical trial consistently confirms its potential for success and it is reasonable to argue that determining whether and when a particular innovation will be profitable is not possible. Even if there is some degree of predictability in the FDA approval based on insider knowledge, the timing is unpredictable. Therefore companies cannot adjust their strategies based on this knowledge.

The assumption justifying propensity score matching is that determinants of the participation are known. I include the following observables as potentially affecting the probability of treatment: being American/non-American company, number of phase III clinical trials relative to firm size, debt, profitability and size.[31] Being a foreign company is likely to affect the treatment probability because FDA approvals regard the U.S. market only. American companies are more focused on the U.S. market which may increase the probability of success. It may also imply that American approvals are of different importance for foreign companies and thus affect the outcome variables as well. The number of phase III clinical trials serves as an approximation of the intensity of research efforts that directly lead to final approval. It can also describe the maturity of company's research pipeline. Debt burden reflects firm's financing situation and is likely to increase if a company has been waiting long for another research success. Therefore it may indirectly measure how close to an end of the development process a company is. The size of the taken

[31]These variables were described in Section 3.3.1. Definitions are included in the Appendix.

debt would also affect company's spending following the approval. Past profitability and size are supposed to capture how experienced and successful a company is. It is likely that bigger, older companies have already obtained approval in the past or carried many other trials and are more likely to successfully finalise their research. To account for time-varying regulations in the pharmaceutical industry and changes in the research environment I enforce exact matching on the year and quarter. The pool of possible matches for a treated company consists of companies that in a given quarter conduct phase III clinical trials or conducted them in the past two years.

There are several ways to estimate the probability of treatment. The significant part of the literature relies on the logit model, and Deheija and Wahba (1999) show that logit estimates are quite accurate. I also employ a logistic regression and match observation using the algorithm of Ho et al. (2007 and 2007b). For each treated observation I search with replacement for two nearest neighbours in terms of the propensity score. The practice shows that forcing either one or two neighbours is optimal and minimises the mean squared error of the treatment effect (Austin (2010)). I match only observations within a certain fixed distance called *caliper* value (Cochrane and Rubin (1973)). Intuitively, it is easier to find matches for treated entities with a wider caliper band. On the other hand, big differences in the propensity score of the matched observations undermine the idea of comparability of the matched observations. Setting the caliper value reflects the trade-off between the bias of estimates, if the matched observations are not similar enough, and the variance, if the number of successful matches is low. Austin (2011) provides the optimal caliper value that takes into account both factors in the following form:

$$caliper = 0.2 \cdot std.dev. \left(\ln \left(\frac{PropensityScore}{1 - PropensityScore} \right) \right) \tag{3.3}$$

I employ this formula in my analysis.

Propensity score matching has been recently criticized by King and Nielsen (2018) who claim it is model dependent and involves a lot of random pruning, which unnecessarily reduces the sample size. Propensity score matching, however, resembles the randomization within observations with a similar propensity score. Therefore if covariates determine treatment, then matching improves the balance in the covariates. This statement reiterates the assumption mentioned in the previous paragraphs necessary to rationalise propensity score matching. The critique is more applicable to the one-to-one matching without replacement which is not the procedure I adopt.

3.4.3 Matching results and validity

This section summarises the outcome of the matching procedure. Table 3.2 shows the number of observations in the treatment and control groups. Out of 60 treated firm-quarter observations one has not been matched. Depending on the outcome variable, some observations may be further dropped if there are missing entries in the financials. 106 out of 3243 control observations were matched. It implies that some treated observations were matched with only one control observation.

Figure 3.3 shows the distribution of the propensity scores based on the treatment status and matching success. The picture confirms that propensity scores in the treatment and control groups are distributed along the supports that significantly overlap. Figure 3.4 depicts histograms of the propensity score distributions. Raw control group is dominated by companies for which the estimated probability of treatment is very small. In the matched sample, both for the treatment and control observations propensity score values are concentrated in the range of $0.04 - 0.08$.

Tables 3.3 and 3.4 show summary statistics of the raw and matched samples by the treatment status. In terms of means, the differences between the treatment and control groups disappear after matching. Comparing the differences in means, however, is not sufficient (Basu, Polsky and Manning (2008)). Imai et al. (2008) show that the more control observations are dropped from the sample, the more likely that results of a t-test would falsely indicate an improvement in the balance of covariates. A more accurate measure of balance is provided by quantile-quantile plots of the treated and control groups. The idea, which comes from Stuart (2010), is that if the distributions of covariates are similar for the treated and non-treated observations, then such a plot should resemble a 45-degree line. Figure 3.5 presents quantile-quantile plots for all the covariates employed in matching. All the plots indicate there is significant imbalance in the analysed observables in the raw sample. It is very pronounced in firm size and number of conducted trials. Matched observations lie much closer to the 45-degree line.

Summing up, the matching improves the balance in terms of five observable characteristics: origin, research effort, debt burden, profitability and size. The improvement is visible in terms of the means and, more importantly, in the distributions presented in the quantile-quantile plots. The obtained sample is used to estimate regressions 3.1 and 3.2. I continue the analysis as if the propensity scores underlying matching were known, not estimated. Since this is not true, it may introduce bias in the standard errors as noted by Abadie and Imbens (2016). While there are guidelines how to correct errors in certain settings, there is no ready solution for every scenario. Therefore I do not introduce any correction for propensity score estimation acknowledging that the reported standard errors may be imperfectly measured.

3.5 Impact of R&D breakthroughs

FDA approval generates an increase in firm's investment opportunities that is not predictable. After a long development process innovators can finally monetise the innovation. Such an event affects firm's situation in several ways. Firstly, a firm has to prepare the introduction of a drug to the market. Secondly, the new drug on the market may trigger several follow-up studies.[32] Thirdly, firm's borrowing capacity increases since selling new drug promises profits in the near future. Finally, firm's pipeline structure changes as one of the late-stage projects is completed. Overall, the approval event changes the structure of the ongoing projects, the portfolio of possible investments and the cost of financing.

[32]New treatments after initial introduction onto the market are often tested for different diseases, doses forms etc. Studies for such follow-up approvals are simpler since at least the safety of the drug is proved.

A wider choice of projects may affect investment. If a company needs to restructure its pipeline and immediately initiate a new research study, there should be a change in research and development expense.[33] Finally, if companies in the last stage of clinical trials are financially constrained, the approval could result in higher debt and lower cash holdings. R&D success lowers the cost of external financing and the need for a safety buffer in the case of liquidity issues.

Capital expenditure

Table 3.5 presents the estimation results for regressions 3.1 and 3.2 with capital expenditure as the outcome variable. The coefficient on the interaction term $Post \times Treat$ measures the causal impact of the FDA approval on firm's capital expenditure in the following four quarters. Companies that successfully finished studies and expect to introduce a drug to the market increase the capital expenditure relative to total assets by around 2%. The increase is significant at the 5% in the regression with pair fixed effects and at 10% in the weighted regression. The magnitude of the approval impact is big since the pre-approval average capital expenditure scaled by assets in the sample amounts to around 6% as presented in Table 3.1.

Not surprisingly, a positive shock to investment opportunities increases capital expenditure. At this stage, however, it is not obvious what the increased capital expenditure is earmarked for. It may be a temporary increase necessary to set up manufacturing of a new drug. Alternatively, it may an investment in the long-term assets to improve the research potential of a company. Finally, it may reflect undertaking projects that were unavailable due to lack of financing.

The results also imply that companies tend to decrease their capital expenditure

[33]Another way of improving the pipeline structure is through acquisition. Pharmaceutical industry is characterised by frequent mergers and acquisitions. They often involve, however, private companies which are not included in my sample. The number of cases when a company from my sample is involved in M&A activity within a few quarters of the approval is very small. Therefore M&A activity has not been explored further in the paper. The topic of incentives for acquisitions in innovative industries has been discussed, for example, in Higgins and Rodriguez (2006) or Phillips, Hoberg and Fresard (2018).

over time. Coefficient on the *Post* is negative and significant. It may be a consequence of the financial burden of the ongoing research trials that limits their other activities.

Research and development expense

Table 3.6 presents the impact of the FDA approval on the subsequent research and development expense. None of the coefficients in the regression is significant. The companies that obtained the FDA approval do not increase their research expenditure. Lack of response suggests that research efforts do not depend on the timing when the research milestones are reached. This supports the hypothesis that the strategy of an R&D-intensive firm is long-term. This is true in the sample of firms that survived until the final stages of the innovation process. Alternatively, new research endeavours may be delayed since the assessment of the available new projects is time-consuming. Companies may also choose to widen the research pipeline by acquiring other entities at early stages of the innovation process.

The results also indicate that there is no difference in the research expense between treated and control firms. Significantly higher research and development expense for treated firms would raise doubts about lack of predictability of the final success. The coefficient on the dummy $Treat$ is insignificant.

Cash and short-term investments

Table 3.7 summarises the impact of the FDA approval on company's cash and short-term investments. The results do not confirm that companies change their cash holdings in the year following the approval. Since a firm after an R&D breakthrough gets better access to external financing and a promise of future profits, its demand for liquidity may go down if a firm was financially constrained. Lack of response of cash and short-term investments implies that firms in the last stages of the research and development process are not necessarily financially constrained.

Debt-equity ratio

Analysing firm's debt gives further insight about firm's financial constraints. Table 3.8 presents the changes in firm's debt-equity ratio after the FDA approval. Approval facilitates external financing because it constitutes a positive signal to potential investors. However, I do not observe any changes in firm's capital structure after an R&D breakthrough. The treatment effect is negative, but insignificant. It does not support the hypothesis that considered firms are financially constrained and benefit greatly after an R&D breakthrough in terms of potential financing. This finding, even though surprising, is consistent with my previous results regarding cash holdings.

While irrelevance of financial constraints is one of the stories, there are other explanations why the approval does not have significant impact on company's capital structure. Firstly, approval as a positive signal may facilitate debt financing and encourage investors at the same time. If both debt and equity change, the overall impact on the ratio is not obvious. Secondly, the effect may be heterogeneous among firms and go in opposite directions depending on other factors. Finally, the impact may be delayed and better financing opportunities are not realized immediately after positive shock.

Overall, the results suggest that R&D breakthroughs measured with the FDA approvals have limited direct impact on company's actions. Even though these are rare events with a potentially huge impact on company's profits within the next few years, they do not define turning points in companies' strategies and innovators seem to follow their long-term plans.

It is possible that successful R&D is not necessarily perceived by companies as a one-time random shock like, for example, rare shocks in macroeconomic models. It may resemble an event recurring with uncertain timing. Hence firms implement long-term strategies knowing the breakthrough has to occur, but preparing for the long and costly waiting for success. While this perception of marketable ideas is not true in general, it may be true conditional on firm's surviving on the market.

Without successful R&D firms need to terminate operations or engage in M&A.

My findings also indicate that innovative companies are not necessarily financially constrained. These results are novel in the innovation literature, but may be specific to my sample. I rely on public firms with research and development projects close to completion after several stages of assessment. While it is early to draw definite conclusions, it would imply that the analysed struggles with financing innovation are relevant only in the initial stages of the process. This may be important for policy makers who design incentives for research going in specific, socially most desirable directions.

3.6 Conclusions

In this paper I analyse the medium-horizon impact of the finalised research and development process based on the example from the pharmaceutical sector. I use a new measure of innovativeness and innovation success that relies on FDA approvals of new oncology drugs. The main advantage of the measure is that it directly translates into real profits. I estimate difference-in-differences regressions on the sample matched using propensity score.

I document significant changes in firm's strategy following an R&D breakthrough reflected in the capital expenditure measured relative to firm size. Companies increase the capital expenditure in the four quarters following the FDA approval. There are no differences in the research and development expense, cash and short term investment and debt-equity ratio between companies that have obtained the approval and other companies carrying out clinical trials.

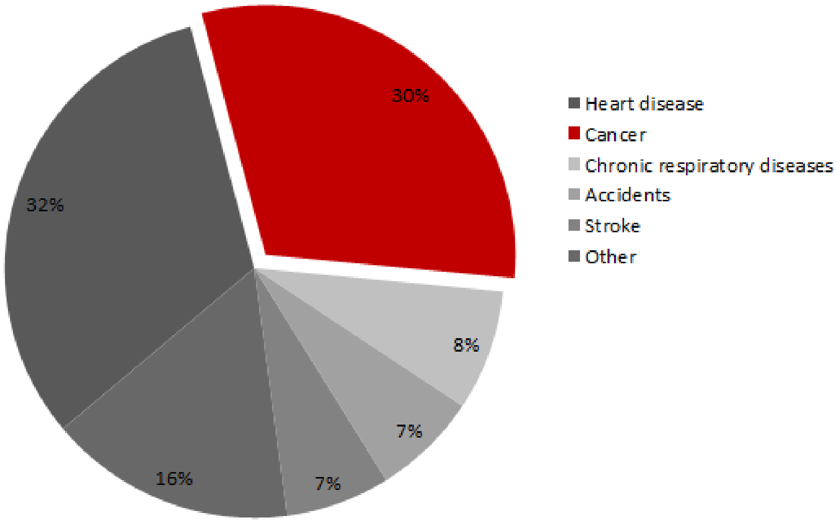
Lack of changes in the capital structure and cash buffer suggest that firms in the last stages of the research and development process are not financially constrained. This may imply that companies that survived until the stage are ready to pursue long-term strategies and continue with research studies until some of them prove successful. The increase in capital expenditure is therefore not resulting from better financing options, but from a wider choice of projects. However, new projects are not supposed to replace the finalised innovation since there is lack of response in terms of research and development expense.

The main weakness of the analysis is the small sample size. Firstly, study considers only listed companies. Secondly, many observations are lost during merging information from different sources. It is a serious drawback because in rare events studies any additional entry adds a lot of value.

Pharmaceutical industry is a broad and relevant environment focused on research and development. Therefore the conclusions drawn in this paper can be extended to other types of innovation-intensive industries.

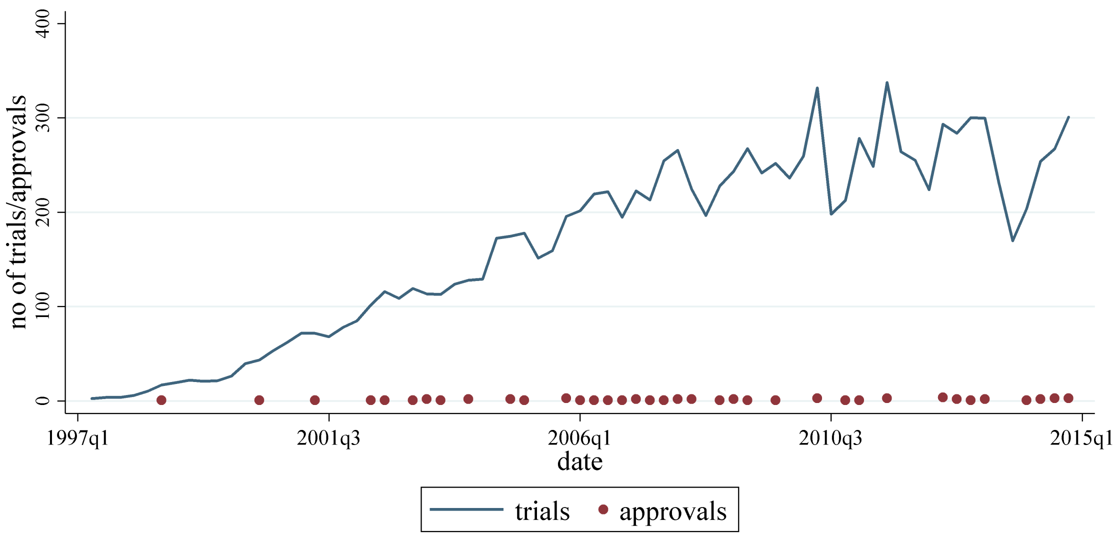
3.7 Figures

Figure 3.1: Leading death causes in 2013.



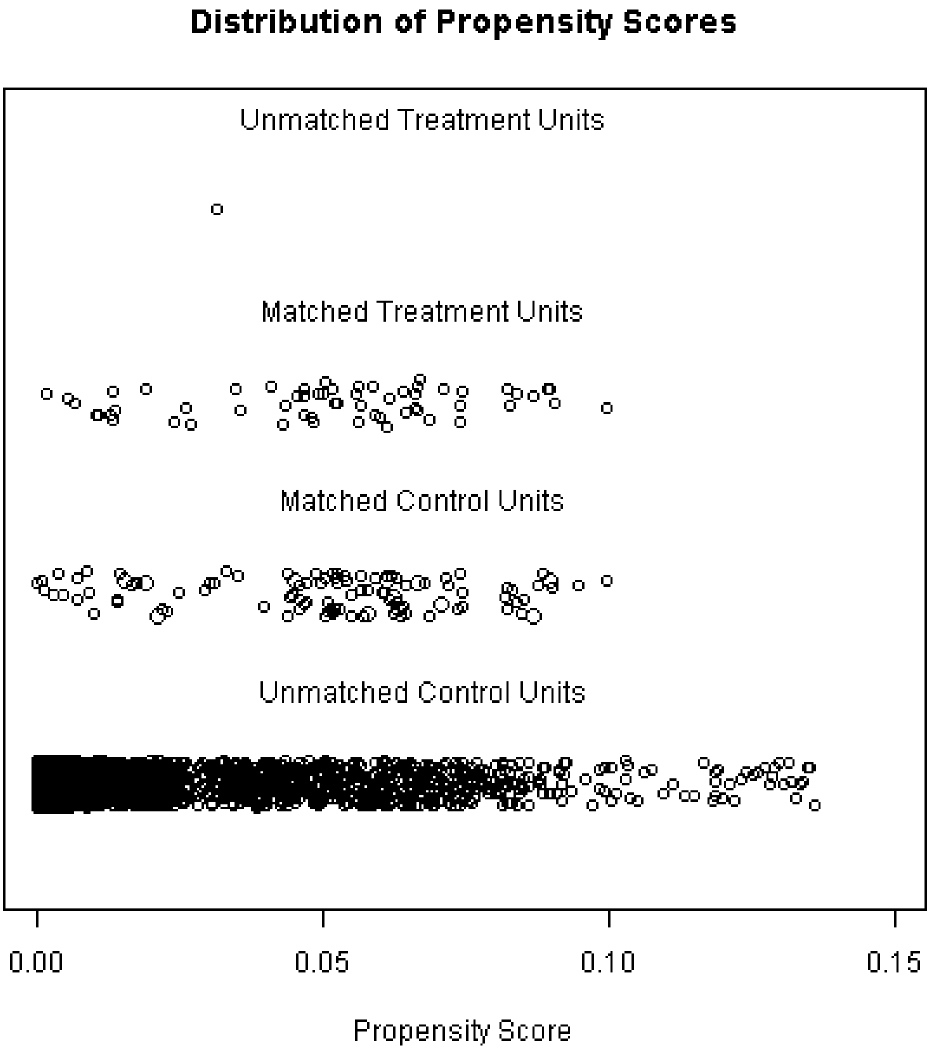
Notes: This figure shows leading death causes in 2013 in the U.S.

Figure 3.2: Phase III clinical trials and FDA approvals over time.



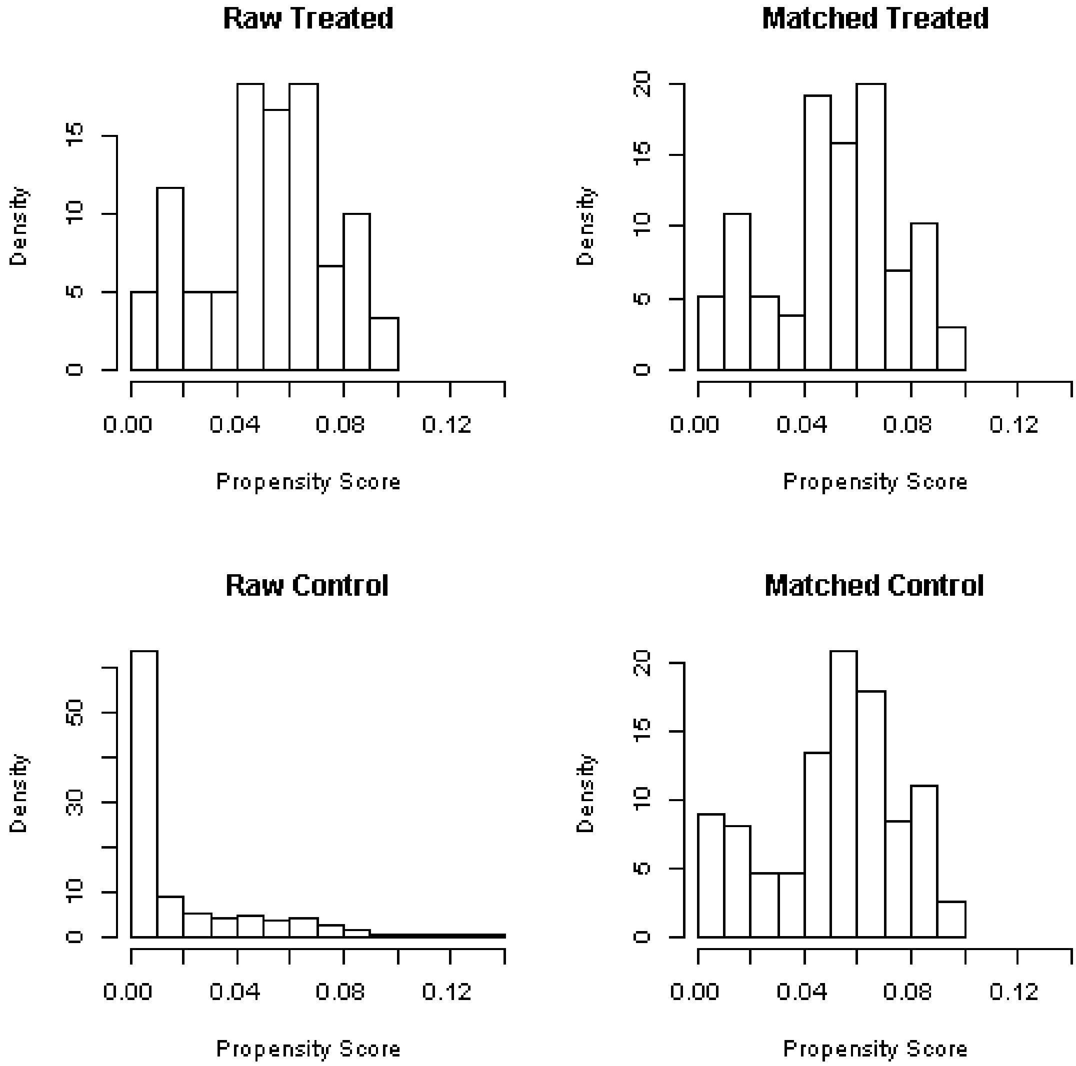
Notes: This figure shows the number of clinical trials (line) and FDA approvals (dot) for oncology drugs conducted/obtained in years 1997–2014 by firms in my sample (for-profit companies with available financial data). The approvals are presented only for non-zero values.

Figure 3.3: Propensity score distribution by treatment and matching status.



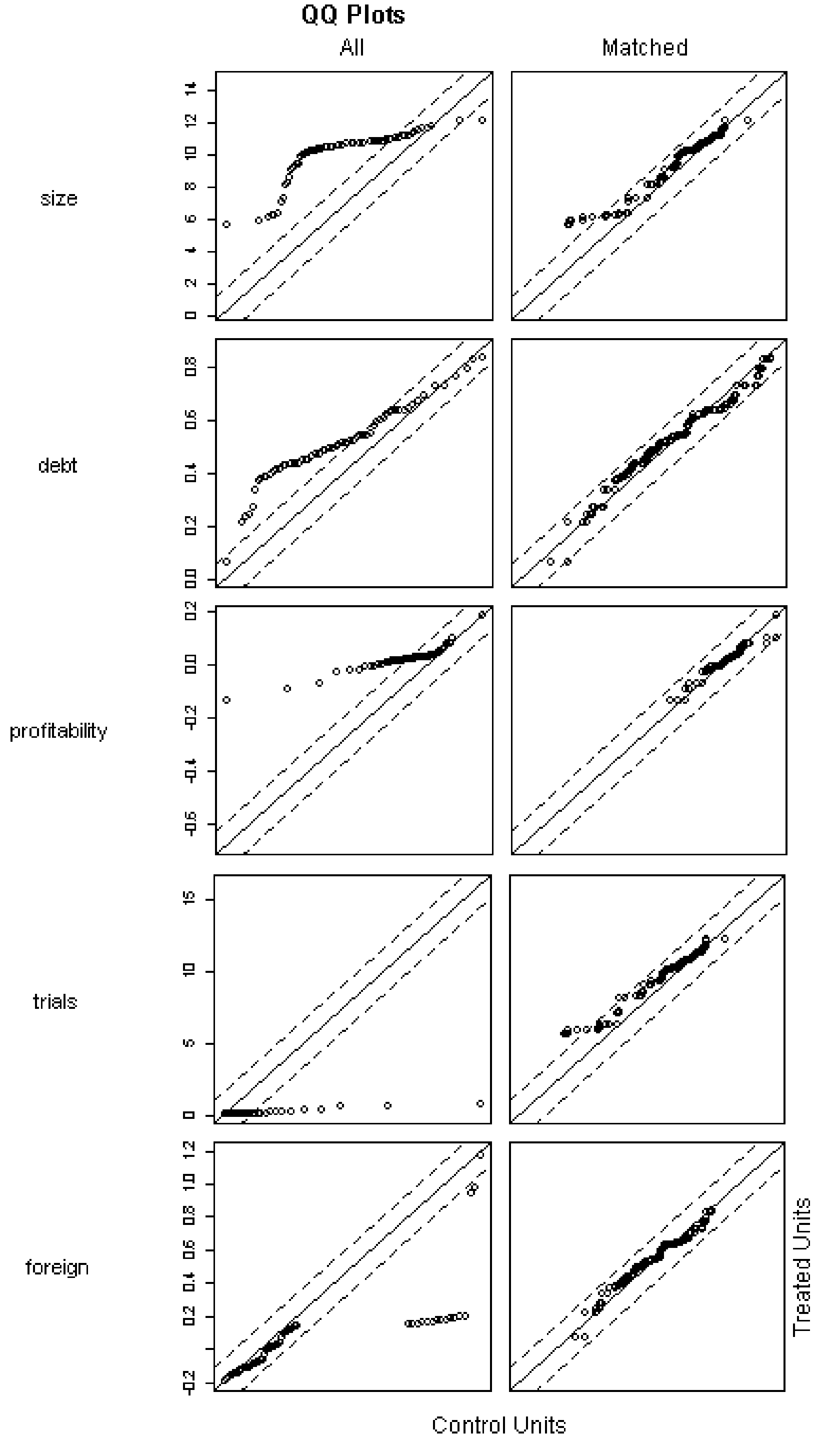
Notes: This figure shows the distribution of the propensity score in the raw and in the matched sample and by treatment status. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Figure 3.4: Propensity score histograms for raw and matched data.



Notes: This figure shows histograms of the distribution of the propensity score in the raw and in the matched sample and by treatment status. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Figure 3.5: Quantile-quantile plots for raw and matched data.



Notes: This figure shows quantile-quantile plots for the raw and the matched sample. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort* (trials), *origin* (foreign) and *date*. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

3.8 Tables

Table 3.1: Summary statistics of companies conducting clinical trials.

	p50	mean	sd
total assets	471.0	82606.2	345902.1
total debt (scaled)	0.38	0.39	0.21
profitability (scaled)	-0.020	-0.058	0.13
no of phase III trials	1	3.35	6.50
trials relative to firm size	0.11	1.18	2.40
capital expenditure (scaled)	0.040	0.060	0.070
r&d expense (scaled)	0.21	0.29	0.27
cash/short-term investment (scaled)	0.41	0.47	0.32
debt/equity	0.67	1.16	1.74
firm-quarter obs	3494		
firms	159		
foreign firms	46		
firms with approval	28		
approvals	60		

Notes: This table shows summary statistics of for-profit companies conducting phase III clinical trials in years 1997–2014. Companies in different quarters are treated as separate entities. Variables of interest are defined as: size = average total assets in the past four quarters; total debt (scaled) = average total liabilities over total assets in the past four quarters; profitability (scaled) = average EBT over total assets in the past four quarters; trials relative to firm size = logarithm of one plus number of trials over total assets; capital expenditure (scaled) = summed capital expenditure over average total assets in the past four quarters; R&D expense = summed R&D expense over average total assets in the past four quarters; cash/short-term investments (scaled) = average cash and short-term investment over total assets in the past four quarters; debt/equity = average total liabilities over equity in the past four quarters. Detailed definitions are in the Appendix. *Overlapping approvals* are excluded as explained in Section 3.3.2.

Table 3.2: Matched observations

	Control	Treated
All	3243	60
Matched	106	59
Unmatched	3137	1
Discarded	0	0

Notes: This table shows the number of matched and not matched observations by the treatment status. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.3: Covariate balance in raw data

	Means Treated	Means Control	SD Control	Mean Diff
distance	0.0509	0.0176	0.0254	0.0333
size	9.9103	6.6482	2.9642	3.2622
debt	0.5141	0.3937	0.2145	0.1204
profitability	0.0245	-0.0591	0.1287	0.0836
foreign	0.0500	0.2584	0.4378	-0.2084
trials	0.0905	1.2525	2.4654	-1.1620

Notes: This table shows summary statistics of companies in the raw sample. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.4: Covariate balance in matched data

	Means Treated	Means Control	SD Control	Mean Diff
distance	0.0512	0.0507	0.0245	0.0005
size	9.9251	9.5176	2.1311	0.4074
debt	0.5188	0.5096	0.1665	0.0092
profitability	0.0244	0.0307	0.0530	-0.0063
foreign	0.0508	0.0424	0.2024	0.0085
trials	0.0918	0.1163	0.4696	-0.0245

Notes: This table shows summary statistics of companies in the matched sample. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort* (trials), *origin* (foreign) and *date*. Variables' definitions are in the Appendix. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.5: Capital expenditure after FDA approval.

	Weighted	Pair FE
Post	-0.014***	-0.014***
	(0.0045)	(0.0036)
Treat		-0.0084
		(0.010)
Post x Treat	0.019*	0.020**
	(0.010)	(0.0075)
Firm x Year-Quarter FE	X	
Pair FE		X
Obs	220	222

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of the FDA approval on the capital expenditure of a firm. The specifications are presented in Equations 3.1 and 3.2. The dependent variable is defined as summed capital expenditure in the four quarters over the average total assets in the period. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. In the column *Weighted* the regression is weighted with the sampling weights obtained during matching and standard errors are clustered at the firm-year-quarter level. In the column *Pair FE* the regression includes fixed effects to identify matched observations and standard errors are clustered at the pair level. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.6: Research and development expense after FDA approval.

	Weighted	Pair FE
Post	-0.0052	-0.0029
	(0.012)	(0.010)
Treat		-0.0019
		(0.014)
Post x Treat	0.00075	-0.0015
	(0.013)	(0.012)
Firm x Year-Quarter FE	X	
Pair FE		X
Obs	210	212

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of the FDA approval on the R&D expenditure of the firm. The specifications are presented in Equations 3.1 and 3.2. The dependent variable is defined as summed R&D expenditure in the four quarters over the average total assets in the period. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. In the column *Weighted* the regression is weighted with the sampling weights obtained during matching and standard errors are clustered at the firm-year-quarter level. In the column *Pair FE* the regression includes fixed effects to identify matched observations and standard errors are clustered at the pair level. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.7: Cash and short-term investments after FDA approval.

	Weighted	Pair FE
Post	-0.0047	-0.0020
	(0.012)	(0.010)
Treat		0.0064
		(0.025)
Post x Treat	-0.0058	-0.0085
	(0.018)	(0.012)
Firm x Year-Quarter FE	X	
Pair FE		X
Obs	318	322

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of the FDA approval on the cash and short-term investments of the firm. The specifications are presented in Equations 3.1 and 3.2. The dependent variable is defined as average cash and short-term investments over total assets in the four quarters. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. In the column *Weighted* the regression is weighted with the sampling weights obtained during matching and standard errors are clustered at the firm-year-quarter level. In the column *Pair FE* the regression includes fixed effects to identify matched observations and standard errors are clustered at the pair level. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

Table 3.8: Debt-equity ratio after FDA approval.

	Weighted	Pair FE
Post	-0.095	-0.086
	(0.13)	(0.10)
Treat		0.23
		(0.35)
Post x Treat	-0.11	-0.12
	(0.28)	(0.16)
Firm x Year-Quarter FE	X	
Pair FE		X
Obs	318	322

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions show the impact of the FDA approval on the debt-equity ratio of the firm. The specifications are presented in Equations 3.1 and 3.2. The dependent variable variable is defined as average debt-equity ratio in the four quarters. The matching methodology follows that of Ho et al. (2007). Firm-quarter observations are matched based on *size*, *debt*, *profitability*, *research effort*, *origin* and *date*. Variables' definitions are in the Appendix. In the column *Weighted* the regression is weighted with the sampling weights obtained during matching and standard errors are clustered at the firm-year-quarter level. In the column *Pair FE* the regression includes fixed effects to identify matched observations and standard errors are clustered at the pair level. Sample: for-profit companies conducting clinical trials for oncology drugs in 1997–2014.

3.9 Appendix

Definitions

The dummy i identifies time around the approval. ($i = 0$ denotes the quarter of approval). The dummy $t = 0$ ($t = 1$) denotes pre- (post-) approval period.

Pre-approval controls:

$$\text{size} = \ln\left(\sum_{i=0}^{3} 0.25 \cdot \text{totals assets}_{-i}\right)$$
$$\text{debt} = \sum_{i=0}^{3} 0.25 \cdot \frac{\text{total liabilities}_{-i}}{\text{total assets}_{-i}}$$
$$\text{profitability} = \sum_{i=0}^{3} 0.25 \cdot \frac{\text{EBT}_{-i}}{\text{total assets}_{-i}}$$
$$\text{trials} = 100 \cdot \ln\left(1 + \frac{\text{number of phase III clinical trials}_0}{\sum_{i=0}^{3} 0.25 \cdot \text{total assets}_{-i}}\right)$$
$$\text{foreign} = \mathbb{1}_{\text{company listed on the U.S. stock exchange}}$$

I winsorize the debt and trials measure in the right-tail at 0.5%. I also winsorize profitability at 0.5%.

Dependent variables:

$$\text{capex}_{t=0} = \frac{\sum_{i=0}^{3} \text{capital expenditure}_{-i}}{\sum_{i=0}^{3} 0.25 \cdot \text{total assets}_{-i}}$$
$$\text{capex}_{t=1} = \frac{\sum_{i=1}^{4} \text{capital expenditure}_{i}}{\sum_{i=1}^{4} 0.25 \cdot \text{total assets}_{i}}$$
$$\text{R\&D}_{t=0} = \frac{\sum_{i=0}^{3} \text{R\&D expenditure}_{-i}}{\sum_{i=0}^{3} 0.25 \cdot \text{total assets}_{-i}}$$
$$\text{R\&D}_{t=1} = \frac{\sum_{i=1}^{4} \text{R\&D expenditure}_{i}}{\sum_{i=1}^{4} 0.25 \cdot \text{total assets}_{i}}$$

$$\text{cash/short-term investments}_{t=0} = \sum_{i=0}^{3} 0.25 \cdot \frac{\text{cash/short-term investments}_{-i}}{\text{total assets}_{-i}}$$

$$\text{cash/short-term investments}_{t=1} = \sum_{i=1}^{4} 0.25 \cdot \frac{\text{cash/short-term investments}_{i}}{\text{total assets}_{i}}$$

$$\text{D/E}_{t=0} = \sum_{i=0}^{3} 0.25 \cdot \frac{\text{total liabilities}_{-i}}{\text{total assets-total liabilities}_{-i}}$$

$$\text{D/E}_{t=1} = \sum_{i=1}^{4} 0.25 \cdot \frac{\text{total liabilities}_{i}}{\text{total assets-total liabilities}_{i}}$$

I winsorize capital expenditure and R&D expenditure values in the right-tail at 0.5%, and debt-equity ratio at 1% level. These refinements do not affect the results.

Table C.1: Summary statistics of companies conducting clinical trials: With overlapping approvals.

	p50	mean	sd
total assets	850.5	77982.3	326290.5
total debt (scaled)	0.41	0.41	0.21
profitability (scaled)	-0.0031	-0.048	0.12
no of phase III trials	1	4.66	8.48
trials relative to firm size	0.067	1.06	2.29
capital expenditure (scaled)	0.044	0.062	0.069
r&d expense (scaled)	0.17	0.27	0.25
cash/short-term investment (scaled)	0.34	0.44	0.32
debt/equity	0.74	1.21	1.79
firm-quarter obs	3936		
firms	159		
foreign firms	46		
firms with approval	28		
approvals	73		

Notes: This table shows summary statistics of for-profit companies conducting phase III clinical trials in years 1997–2014. Companies in different quarters are treated as separate entities. Variables of interest are defined as: size = average total assets in the past four quarters; total debt (scaled) = average total liabilities over total assets in the past four quarters; profitability (scaled) = average EBT over total assets in the past four quarters; trials relative to firm size = logarithm of one plus number of trials over total assets; capital expenditure (scaled) = summed capital expenditure over average total assets in the past four quarters; R&D expense = summed R&D expense over average total assets in the past four quarters; cash/short-term investments (scaled) = average cash and short-term investment over total assets in the past four quarters; debt/equity = average total liabilities over equity in the past four quarters. Detailed definitions are in the Appendix. *Overlapping approvals* are not excluded.

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