Journal of Public Economics 182 (2020) 104112

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Employment behavior and the economic crisis: Evidence from winners and runners-up in procurement auctions ☆

Klaus Gugler[a], Michael Weichselbaumer[a,*], Christine Zulehner[b,c,d,e]

[a] WU Vienna University of Economics and Business, Welthandelsplatz 1, A-1020 Vienna, Austria
[b] University of Vienna, Oskar-Morgenstern-Platz 1, Vienna A-1090, Austria
[c] Telecom Paris, France
[d] WIFO Vienna, Austria
[e] CEPR, United Kingdom

ARTICLE INFO

Article history:
Received 18 June 2018
Received in revised form 6 November 2019
Accepted 26 November 2019
Available online 24 December 2019

JEL classification:
D44
J23
L74

Keywords:
Economic crisis
Labor hoarding
Labor demand
Public procurement auctions
Regression discontinuity design

ABSTRACT

To estimate the differential effects of increased product demand on the demand for labor before and during the recent economic crisis, we use a combination of detailed employment data and the outcomes of public procurement auctions. We compare the employment reactions of the winner of an auction with the employment reactions of the second ranked firm (i.e., the runner-up firm). Assuming similar ex-ante winning probabilities for both firms, we view winning an auction as an exogenous shock to a firm's production and its demand for labor. Detailed daily employment data cover almost 600 construction firms and over 2500 auctions in Austria over the time period 2006 to 2009. Our main results show that the winning firm significantly increases labor demand in the weeks following an auction but only in the years before the recent economic crisis. It employs about 80 workers (around 3% of the workforce) more after the auction than the runner-up firm. Most of the adjustment takes place within one month after the demand shock. Winners predominantly fire fewer workers after winning than runner-up firms. In the crisis, however, firms do not employ more workers than their competitors after winning an auction. There are no effects on wages. We view labor hoarding and productivity improvements induced by the crisis as the most likely explanations for our findings. We also discuss implications for fiscal and stimulus policy in the crisis.

1. Introduction

Does the effect of an increase in demand on employment differ in an economic crisis? What are the main channels of adjustment (if any), more hires or fewer fires? What are the effects on wages? What are the implications for fiscal and stimulus policy to counteract severe contractions? To answer these questions, we estimate the employment effects when a firm acquires a new contract in a public procurement auction before and during the recent economic crisis, and also assess the effects on wages, hires and fires, and tenure separately.

From the daily variations in our data, we can observe that firms' reactions to a new contract are closely tied to the date when they win the contract. Our data stretch over the period 2006 to 2009, and within that period lies the beginning of the recent economic crisis. Winning firms significantly increase labor demand in the weeks following an auction but only in the years before the crisis. They employ about 80 workers (around 3% of the workforce) more after the auction than the runner-up firm. The adjustment is fairly quick since within one month around three fourths of the adjustment process is finished. Most employment effects are due to blue collar workers (85%) as compared to white collar workers (15%). During the crisis, however, winner and runner-up display no employment reaction to a new contract. Wages do not change after the positive demand shock, neither before nor in the economic crisis. Differences between hiring and firing patterns show that winners achieve the adjustment by firing fewer workers after the auction than runners-up. During

☆ We thank Andrea Weber and seminar participants at the NFN Workshop at Vienna University of Economics and Business, University of Cologne and Goethe University Frankfurt for their comments and suggestions. This project received funding from the Austrian Science Fund (National Research Network S103). All errors and opinions are the authors' sole responsibility.

* Corresponding author.
E-mail addresses: klaus.gugler@wu.ac.at (K. Gugler), michael.weichselbaumer@wu.ac.at (M. Weichselbaumer), christine.zulehner@univie.ac.at (C. Zulehner).

https://doi.org/10.1016/j.jpubeco.2019.104112
0047-2727/

the crisis, the differences in hires and fires between winner and runner-up disappear as well.

Our results imply a strong reaction of employment to demand changes before the crisis, and a labor demand reaction with respect to output of essentially zero during the economic downturn. One interpretation could be labor hoarding, since in the crisis winning firms hold their workforces stable. Moreover, we find no effects on wages, and some form of wage stability is a pre-condition for labor hoarding.[1] Sector-wide statistics are also consistent with labor hoarding in the crisis. Output in the Austrian construction sector decreased by around 2% from 2008 to 2009, but worked hours essentially stayed flat in that time period.[2] However, the results in the period before the crisis may not be explained by labor hoarding alone. An additional explanation could be a change in productivity due to the increased competition in the crisis.[3] Thus, labor markets appear to be working differently in downturns as compared to "normal" times.

For our empirical analysis, we address two concerns inherent in empirical research on labor demand (see for example, Addison et al., 2014). The first concern is potential endogeneity. When a firm adjusts its workforce, it may be due to an exogenous demand shock or it may be due to some other factors such as positive or negative productivity shocks or simply due to unobserved firm heterogeneity. The second concern is related to the frequency of observations which are often only available on a yearly or quarterly basis. This might be too long for an analysis of the adjustment processes taking place within the firm after a shock. This paper tries to tackle both concerns.

We address the first concern with an innovative research set-up. We analyze public procurement auctions in the Austrian construction sector and determine winning bids and runner-up bids. The empirical design rests on the assumption that the winning bidder and its runner-up have similar winning probabilities. When the winning probabilities are close enough to warrant similar expectations of winning (which we show is indeed the case), then the difference in employment between winner and runner-up measures the employment effect of the new contract. Thus, the realization of the positive demand shock due to winning an auction can be regarded as a surprise, and we are able to measure the exogenous element of the demand shock applying similar methods as Greenstone et al. (2010) and Malmendier et al. (2018). Indication of comparability between the winner and the runner-up can be given by comparing their characteristics ex-ante. We find that they are similar in observables such as employment, wage level and distance between firm and project site — all of which are different for the other bidders with ranks three or higher in the auction. Thus, our empirical design provides quasi-experimental evidence on the link between demand for a firm's product and its labor input demand.

Our data set also permits us to exploit daily information on auction outcomes and firm employment. We are thus able to address the second concern by using daily data on the demand shock as well as the subsequent evolution of employment. For our empirical analysis, we use data on Austrian procurement auctions in the construction sector during the period beginning of 2006 to end of 2009 and information on firms' employment coming from the Austrian Social Security Database (ASSD). For all auctions, we know the identity of the winner and the runner-up, their bids and auction characteristics. The ASSD covers private sector workers on a daily basis, and includes a large sample of firms from administrative matched employer-employee data in Austria (Zweimüller et al., 2009). The data allow us on the one hand to pin down demand shocks at a daily basis (winning an auction in contrast to the runner-up) and on the other hand to trace the daily evolution of employees of these firms as well as all hires and fires that take place before and thereafter. We also distinguish between blue and white collar workers, and investigate the channels how winning a contract influences firms' employment by assessing the effect on hires and fires, separately. Given this set-up, we argue that we are able to tackle two important problems in labor demand research.

Based on our empirical approach, we argue that we estimate causal effects of the treatment, i.e., winning the auction, because the only significant observable difference between the winning firm and runner-up firm is the fact that the former won the auction and the latter lost the auction by a small margin. Robustness checks support our findings further. First, the inclusion of dummies for each sample month rules out seasonal effects. Labor demand in the construction sector is determined by weather conditions that vary over the year and the additional controls account for these. Second, we check the results relative to higher-ranked bidders who serve as an additional control group. Third, we explore the similarity of the winner and the runner-up in more detail. Although first and second in the auction, the winner's and the runner-up's winning probabilities may be different. We constrain the relative difference in bids between winner and runner-up. This relative difference is the money left on the table in the auction, and if bidders bid optimally in the auction, it may serve as a proxy for differences in bidders' cost valuations for the project. This makes it possible to rule out differences potentially indicated by large margins or differences in bidders' valuations. Finally, we check for any contamination due to additional auctions in the evaluation period.

Since our auctions consist of public procurement auctions, we can also shed some light on the implications of our results for fiscal and stimulus policy to counteract severe contractions. We find that the employment effect of an exogenous increase in demand was essentially zero in the crisis period of our sample (we determine October 2008 as the beginning of the crisis in Austria). That is, the winner of the auction did not hire more nor fire fewer workers than the runner-up. This implies that second round effects of the fiscal stimulus measures were absent in Austria in the first year after the crisis or could not yet unfold.

The remainder of the paper is organized as follows. In the next section, we relate our approach to the literature. Section 3 describes our research design and sets up the empirical model. We describe our data, give institutional details, and provide evidence on the similarity of the winners and runners-up justifying our research design. Section 4 describes the empirical results graphically and with regression analysis. Further robustness checks are given in Section 5. Section 6 provides a discussion on the possible reasons for the observed pattern of employment responses and Section 7 concludes.

2. Related literature

From a production function logic, one expects a non-negative effect of an increase in the firm's output on its level of employment (see Nickell, 1984 or Bresson et al., 1996).[4] The questions are how

[1] If wages were perfectly flexible, the marginal revenue product of labor and wages plus adjustment costs (if any) would always align.

[2] See http://www.statistik.at/web_de/statistiken/wirtschaft/produktion_und_bauwesen/konjunkturdaten/index.html. See also Huber et al. (2017) for corroborating macroeconomic evidence of relatively stable employment compared to output in the recent economic crisis in Austria.

[3] In Gugler et al. (2015b), using a similar, but less detailed data set, we investigate the effects of the recent economic crisis on firms' bidding behavior and markups in first price sealed bid auctions. Applying a private value auction model, we find procyclical markups.

[4] A rather small literature addresses the estimation of labor demand from an industrial organization perspective. It tackles particular aspects of labor demand in combination with questions on turnover of businesses (see e.g., the survey by Caves, 1998), innovation and productivity (see e.g., Bresnahan et al., 2002; Lachenmaier and Rottmann, 2011), and the effects of mergers on labor demand (see e.g., Gugler and Yurtoglu, 2004).

large the response in labor demand is, which factors determine the amount of the response, e.g., is the response different in an economic crisis as opposed to "normal" times, and which mode of adjustment the firm takes, i.e., does the firm hire more workers, fire fewer workers or a combination of the two? An early examination of labor as an input factor that faces fixed costs of adjustment is by Oi (1962). Given the presence of hiring and training costs, the author argues that, "Even under perfect competition wages would be equated to marginal products if and only if labor is a completely variable factor". Hamermesh (1989) gives plant-level evidence, using monthly data, on the relevance of fixed costs, leading to labor adjustment that displays jumps which are not coincidental with changes in production. The resulting "labor hoarding" was also used to explain the existence of pro-cyclical productivity (see Biddle, 2014), although this pro-cyclicality seems to have disappeared in the recent crisis at least in the US (see Galì and van Rens, 2015).

The main focus of this article is to analyze whether firms react differently in terms of labor demand depending on whether the demand shock is realized in "normal" times or whether it is realized in times of an economic crisis. Nearly 30 years ago, Bentolila and Bertola (1990) explained the employment dynamics in the largest European countries after the first oil shock by firing costs, slower and more uncertain growth, and lower attrition rates. Related to the labor hoarding hypothesis, the authors derive the optimal policy of the firm depending on inherited labor and the strength of demand (i.e., "business conditions" in the real world). In the presence of labor adjustment costs, output demand can fluctuate a lot inside a "corridor" or "range of inaction" without affecting employment. If firms are far away from the "firing line" (e.g., because past demand was high), even a large negative demand shock would not result in immediate lay-offs, but would move firms more toward the "firing barrier". Given the negative demand shock (e.g., the crisis), however, increasing the degree of uncertainty about the future, a positive demand shock (e.g., winning an auction) may not be large enough to induce hiring by firms again (which are farther away from the "hiring barrier" than before the negative shock). We will return to the insights of Bentolila and Bertola (1990), who importantly for our study distinguish between hiring and firing, when we interpret our results.

Recent contributions to the labor hoarding hypothesis are Giroud and Mueller (2017), Dietz et al. (2010), and Giupponi and Landais (2018). Giroud and Mueller (2017) utilize a particular aspect of the labor hoarding hypothesis, namely that firms with little financial slack but not financially unconstrained firms face a trade-off between long-run optimization (saving on the costs of firing, hiring and training workers) and short-run liquidity (subsidizing temporarily wages) needs to identify the effects of labor hoarding. They use micro-level data from the U.S. Census Bureau, and find significantly larger employment losses in response to declines in local consumer demand in establishments of more highly levered firms. According to the authors, the results suggest a possible role for employment policies targeting firms directly besides conventional stimulus. We will also estimate our basic coefficients differentiating between profitable/unprofitable and highly levered/not highly levered firms, and analyze whether labor hoarding is a more likely explanation for profitable and not highly levered firms. These firms could "afford" to hoard labor in the crisis, whereas other firms may not have the resources to invest in labor hoarding.

Dietz et al. (2010) attribute the relatively stable development of employment in the recent crisis at least partly to labor hoarding. Aggregate and firm-level data indicate that firms do not immediately adjust labor input in line with demand for their products. Moreover, subsidized types of labor hoarding, such as short-time work (STW; subsidies for hours reductions to workers in firms experiencing temporary shocks), facilitated the retention of workers by employers, however the authors also find windfall gains due to these policies. Most recently, Giupponi and Landais (2018) use unique administrative social security data for Italy (as we do for Austria), and they find large and significant negative effects of STW treatment on hours, but large and positive effects on headcount employment. This is consistent with labor hoarding. However, employment effects only last until the program stops, and STW offers no long term insurance to workers. Finally, there are significant negative reallocation effects of STW on employment growth of untreated firms in the same local labor market. Their counterfactual policy analysis shows that STW stabilized employment during the Great Recession in Italy, and that it brought (small) positive welfare gains. We show in our empirical analysis that firms in the Austrian construction sector also engaged in labor hoarding, despite the fact that no STW programs were implemented during the crisis.

Our setting studies the effect of additional output contracted by a firm on labor demand and the adjustment process that consequently takes place. Adjustment of labor has been studied extensively, and faces two important challenges. First, potential endogeneity problems must be tackled (e.g., firm heterogeneity or productivity differences; see Bond and Van Reenen (2007) for major studies of estimating labor adjustment). Second, observations are often available only on a yearly or quarterly frequency, which can be too low for some forms of interesting adjustment processes (for a recent overview, see Addison et al., 2014; Kramarz, 2014). Our research design addresses both concerns raised in empirical research on labor demand. The first concern might be considered as minor in our analysis, since the winner and the runner-up are similar in their main characteristics and, in particular, their winning probabilities are similar. Thus, the realization of the positive demand shock due to winning an auction can be regarded as a surprise, and we are able to measure the exogenous element of the demand shock.[5] We address the second concern by using daily data on the demand shock as well as the subsequent evolution of employment, hires and fires.

The research design applied here has some similarities to other studies. Although questions unrelated to this study are at the center of the following papers, all of them exploit a sample of winners and losers in contests to construct a counterfactual outcome of the winner had she not won the contest. While we assess labor demand and use an auction as the contest, Greenstone et al. (2010) quantify agglomeration spillovers and compare changes in total factor productivity (TFP) among incumbent plants in "winning" counties that attracted a large manufacturing plant and "losing" counties that were the new plant's runner-up choice. Prior to the new plant opening, the winning and losing counties had similar trends in TFP. The empirical results show that incumbent plants' TFP is 12% higher in winning counties five years after the opening. They also find that productivity spillovers are larger for plants sharing similar labor and technology pools with the new plant. Malmendier et al. (2018) analyze whether acquiring firms profit from their acquisitions or whether acquiring CEOs overbid and destroy shareholder value. They estimate the long-run abnormal returns to mergers exploiting detailed data on merger contests. Their results show that bidder returns are closely aligned in the years before the contest, but winners underperform losers by 50% over the following three years. Lee and Mas (2012) estimate an average effect of unionization in public firms on cumulative abnormal returns. They find close wins in votes for new unionization are associated with a reduction in the firm's market value. Recently, Ferraz et al. (2015) use a similar strategy to ours in Brazilian government procurement contracts when analyzing the effects of winning on subsequent firm growth.

[5] Of course, the firms taking part in an auction are selected in the sense that they must hold excess capacity and they must look to expand their supply. Given this selection, actually winning a specific auction comes as a relative surprise, i.e., an ex-ante conditional probability is realized with certainty ex post.

Finally, this paper relates to the literature on stimulus and (local) multiplier effects. Ramey (2011a) and Parker (2011) discuss the difficulties in the literature to evaluate stimulus packages. Nekarda and Ramey (2011) test the predictions of neo-classical and neo-Keynesian models aggregated on the level of industries. Ramey (2011a) additionally surveys the literature on stimulus spending and concludes that the multiplier can take on a large range of values. While many studies find positive employment effects, some studies even find negative effects. Somewhat in contrast to us, several studies find larger multipliers during times of higher slack. Interestingly, by many studies the implied costs per job created is estimated to be around 35,000 USD. Our estimates imply smaller costs per job created before the crisis (around 15,000 Euro) but higher costs in the crisis (see below). Papers that report negative effects for fiscal intervention are Knight (2002) and Faggio and Overman (2014). Knight (2002) finds that once one controls for the endogeneity between grant receipts and preferences for public goods, federal highway grants crowd out state highway spending, leading to little or no increase in net spending in the US. These results are consistent with our results that stimulus measures in the recent crisis in the Austrian construction sector had no significant effects on employment. Faggio and Overman (2014) use English data at the Local Authority level for 2003 until 2007 and find no identifiable effect of public sector employment on total private sector employment.

Mixed results are reported by Feyrer and Sacerdote (2011). The authors analyze the American Recovery and Reinvestment Act (ARRA) and report significant heterogeneity by type of spending. While transfers to local governments and school districts to support teachers and police had no positive effect on employment, programs funding support for low income households generated the largest employment response. Cross state regressions reveal costs per job of around 170,000 USD, which is at the upper bounds of estimates in studies cited by Ramey (2011a).

Positive effects of fiscal intervention are reported in the following studies. Chodorow-Reich et al. (2012) find that ARRA transfers to states had an economically and statistically robust positive effect on employment. Using Swedish data, Moretti and Thulin (2013) show that when a local economy generates a new job by attracting a new business in the traded sector, a significant number of additional jobs are created in the non-traded sector.[6] While the authors concentrate on the effect of local development policies and which jobs are particularly valuable, we focus on the direct effect of government spending on job creation and whether this direct effect differs according to the general state of the economy.

3. Empirical approach

This section introduces our empirical approach. We start by describing the econometric model, and outline our specification that compares the effect of winning a contract before and in the recent economic crisis. After a description of the data we use for our empirical analysis, we give some background information on the Austrian labor market and the organization of procurement auctions. Summary statistics are given and extended to a comparison of winners and runners-up. Finally, we discuss the start of the crisis.

3.1. Econometric model

Estimation of the effect of winning an auction on any outcome relies on a valid counterfactual. We argue that a comparison between winner and runner-up provides such a valid counterfactual. Similarity before the event between the two groups of companies in the relevant covariates as well as the outcome measure itself support the view that the difference after winning can identify the auction effect on, e.g., employment. In the first part of our empirical results, we address the comparability of the pre-event variables between the two groups, both for the outcome variables and for relevant characteristics. After establishing the similarity of winners and runners-up, we estimate the average effect of winning an auction on the winner before and in the recent economic crisis. The regression sample consists of winners and runners-up — that is, the bidders ranked first and second from each auction. Each auction has an exact date when the contract is awarded to one firm. Employment records in our data source are on a daily basis, which allows us to measure the event precisely starting after the day the contract was awarded.

Auctions are indexed with subscript j. The outcome is observed on the level of firm i. We aggregate the daily data to fortnights and an observation is either a fortnight before the auction announcement data, $t < 0$, or after the announcement, $t > 0$. To assess the effect of an additional construction project on the number of employees, hires, fires, wages and tenure, we estimate the following equation:

$$y_{ijt}^{h} = \alpha^{h} + \gamma^{h} \times 1(\texttt{Winner})_{ij} + \sum_{t=-8}^{t=9} \tau_{t}^{h} + \sum_{t=-8}^{t=9} \tau_{t}^{h} \times 1(\texttt{Winner})_{ij} + \beta^{h} X + \varepsilon_{ijt}^{h}, \tag{1}$$

where h = `pre-crisis, crisis` and $\tau_t^h \times 1(\texttt{Winner})_{ij}, t = 1, \ldots, t = 9$ measures the effect of winning a contract on the winner's employment. Each τ_t^h stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9 and are allowed to differ before the crisis and in the crisis. The vector X includes lagged employment and firm's backlog. Backlog after the event, i.e.,$t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Our specifications assume that pre-existing trends in employment are similar for winners and runners-up. We test this assumption with pre-event tests on means in Section 3.3, with a graphical analysis in Section 4.1 and with help of the above regression equation by looking at $\tau_t^h \times 1(\texttt{Winner})_{ij}, t = -8, \ldots, -1$.[7] To assess the effect before and in the crisis, we run separate regressions.

3.2. Data and background

For our empirical analysis, we draw on three data sources.[8] The first provides data on Austrian procurement auctions in the construction sector during the period 2006–2009. The data come from an Austrian construction company. For all auctions in both building and heavy construction where this company took part either as the parent company itself or through a subsidiary, we obtained own and competitors' bids, and bidders' and auction characteristics. The data reflect on average 14% of Austria's total construction sector over our sample period. Two thirds of these auctions end up in the estimation sample, because the necessary employment data is available. This amounts to 5.2 billion Euro (CPI-deflated, 2006) or more than 2500 auctions. Each auction record gives information on the number and identity of all bidders, a short description of the project, the bids and

[6] See also Moretti (2010) for estimates on the US.

[7] See Wolfers (2006) for a discussion on time trends in a difference-in-difference context.

[8] Additional information on the industry and data is available in Gugler et al. (2015b) and Gugler et al. (2015a).

the day the project was awarded to the winning bidder. According to the data providing company, the database covers more than 80% of all auctions which must be conducted according to the Austrian Public Procurement Law (PPL, Bundesvergabegesetz).

All Austrian public authorities such as federal and regional governments, government controlled companies (e.g., the Austrian railways company OeBB) and federal agencies (e.g., ASFINAG, organizing highway construction and maintenance) are subject to the PPL. The main purposes of the PPL are to provide for a transparent procurement process, stimulate competition, and safeguard equal treatment of all potential bidders. While there are some restrictions on bidders in place (e.g., proof of commercial and professional abilities; requirement of a 5% bid bond; availability of certain necessary machinery or qualified personnel etc.), these are intended to forestall opportunistic behavior and do not favor one company over the other. In general, the public authority must choose between an "open procedure" or a "restricted procedure with publication of a contract notice". Most projects are "open" (nearly 90%). An open procedure means that the auctions are open to all firms, while in other auctions, the seller invited a restricted sample of firms only. All contracts are awarded by first-price sealed-bid auctions. While there is no explicit reserve price, the seller has the right to withdraw the auction when the winning bid is "contra bonos mores" (offending against good morals). Each project is described in a published procurement catalogue, which is made public several weeks before the letting day. On the letting day, all bids are unsealed, ranked, and the lowest bidder wins the auction. After the lowest bid has been identified, all bidders are informed. If no bidder objects the decision within 10 days, it is final. In principle, therefore, the Austrian procurement process is very transparent and competitive, allowing for perceived probabilities to be similar for a range of competing companies.

Second, for a large subsample of bidders, we have information on their employee structure and wages, based on the records of the Austrian Social Security Database (ASSD). This database covers the universe of private sector workers in Austria since the year 1972 (Zweimüller et al., 2009). All employees in Austria are covered by mandatory social security and therefore included in the database. As payment for social insurance benefits draws on insurance status on a daily basis, data on employment is available on that level. We determine a window of 90 days (working days, Monday to Friday) before and 90 days after the auction and for each day in this window, calculate the number of employees in the firm and their individual wage from the ASSD. We distinguish between employees, hires and fires as well as blue and white collar workers. For employment, the total number of employees is the daily count of employees that have an active ASSD-registration in that firm. (Later, we will aggregate the daily data to fortnights to run the estimations and present the results.) Hires on a given day are calculated as the number of workers that start their employment in that firm on that day, which means that their ASSD-registration becomes active on that day; fires are the number of employees that have the end date of their employment in that firm on a given day. We stick to the raw data on employment, hires and fires, wages and tenure closely: we do not impose ad-hoc restrictions on the length of lay-offs between two employment intervals of the same worker in the same firm. Thus, fires also include temporary lay-offs, and hires measure total inflows and fires measure total outflows. Our auction data end in 2009, but we have employment data until 2010 to construct a symmetrically truncated tenure variable that captures length of employment within 365 days after an employee is hired.

We further calculated backlog of a firm at a point in time as in Jofre-Bonet and Pesendorfer (2003). Each time a firm wins, backlog of this firm increases by the value of the project. Projects are assumed to be worked off linearly over their construction period, thereby releasing capacity. Each firm's backlog obtained is standardized by subtracting its mean and dividing by its standard deviation.[9] Transportation costs are measured by the driving distance of a firm to a construction site.[10]

To obtain our estimation sample, we drop auctions in which some bidders are accused of collusive behavior. Our identification strategy relies on the idea that winners and runners-up have a similar probability of winning an auction and thus assumes that firms participating in the auctions act competitively. Otherwise, the event of winning an auction might be known ahead of time and cannot serve as a natural experiment. From newspaper reports,[11] we know that auctions in highway and heavy construction in the Southern part of Austria (Carinthia and Styria) are part of an antitrust investigation by the Austrian Competition Authority. To be on the safe side, we drop all auctions in these segments and parts of Austria from our analysis, which amounts to 16% of the auctions.

2589 auctions with 2361 winners and 1880 runners-up form our estimation sample. Table 1 summarizes the bidder characteristics.[12] As expected, the average bid of the winners is lower than that of the runners-up. While winners receive on average 2.044 million Euro for a construction contract, a firm coming in second expected 2.132 million Euro in payments for the contract. The number of bids per case (auction) is equal to 7.13. The average firm in our sample employs 2310 workers, of which 1556 are blue collar workers and 668 are white collar workers.

Finally, we match additional firm information from the Bureau van Dijk's Amadeus database, which contains balance sheet information and income statements of companies in Austria. We are able to match firms using their company names and postal codes. A substantial number of observations has missing data in the balance sheet and income statements provided. Thus, we use balance sheet and income data as additional evidence comparing winners and runners-up and for additional robustness checks only.

3.3. Comparison of winners and runners-up

We rely on the runner-up in first-price auctions to identify a control group. The runner-up has the second-lowest bid, often relatively close to the winner of the auction. Similarity of the groups of winners and runners-up with respect to the outcome variable (employees), among others, before the event of winning is crucial to the research design. We compare the pre-event variables between winners and runners-up, and the winners to the other bidders in each of the auctions, selecting ranks 3, 4 and 5. The choice of ranks 3, 4 and 5 is a pragmatic compromise between adding more than only rank number three, but not adding too many ranks which would cause an imbalance by overweighting (in terms of adding numbers of observations) of some auctions which have many bidders compared to auctions that have fewer bidders. Most auctions (about 4 out of 5) have 5 bidders or more. Table 2 demonstrates the similarity of winners and runners-up before the event.

A comparison of column (1) with columns (2) and (4) shows that the rank closest to the winner produces a good comparison group, but not the other firms that participate in the auctions. In particular, the number of employees, hires, fires, wages and tenure of

[9] We use planned construction duration, instead of actual, and estimate construction duration, when it is not available, using a log-log regression of contract duration on project size.

[10] For firms with multiple plants, we use the distance between the headquarter and the construction site as plant locations are not available. However, different locations of a firm are often operated as subsidiaries and distances are then calculated accordingly.

[11] See e.g., Der Standard, 25 March 2019, "Mit welchen Tricks Preisabsprachen am Bau durchgezogen wurden".

[12] For an exact definition of all variables, see Appendix A.

Table 1
Summary statistics.

Variable	Unit	Mean	Std. dev.	10th	50th	90th	Obs.
		(1)	(2)	(3)	(4)	(5)	(6)
Bid of winner	(1000 Euro)	2043.6	6801.0	146.6	753.7	3726.8	2361
Bid of runner-up		2131.5	7021.9	163.3	793.0	3942.2	1880
Number of bids		7.13	2.96	4.0	7.0	11.0	2361
Employees		2310.0	3021.3	47.4	447.0	8020.2	8482
Employees, blue		1555.7	2023.9	26.7	316.8	5261.9	8482
Employees, white		668.1	907.8	8.0	140.4	2473.9	8482
Hires	(per day)	6.05	9.86	0.1	1.0	18.3	8482
Hires, blue		4.99	8.77	0.0	0.8	16.3	8482
Hires, white		0.88	1.40	0.0	0.1	3.0	8482
Fires	(per day)	5.37	9.15	0.1	0.9	15.8	8482
Fires, blue		4.46	8.35	0.0	0.7	14.7	8482
Fires, white		0.75	1.21	0.0	0.1	2.6	8482
Wage	(Euro per day)	93.3	10.8	77.6	96.0	105.0	8482
Wage, blue		85.3	8.8	74.7	87.4	93.4	8482
Wage, white		115.5	13.0	100.6	120.0	125.6	8482
Tenure	(days in 1st year)	331.7	16.6	313.4	332.4	352.8	8482
Backlog	(std. dev.)	0.19	0.84	−0.83	0.10	1.35	8482
Distance firm to project	(km)	138.4	133.8	9.6	86.5	351.6	4231

Notes: Summary statistics of the winners and runners-up appearing as cases in our sample. Bid appears only once for each case and rank, the number of bids only once per case. The other variables' statistics contain data before and after winning the contract. Backlog is standardized by subtraction of the mean and division by the standard deviation on the firm level. Wages are measured per day. All monetary values are in 2006 Euro.

winners and runners-up are similar, but larger than that for other firms. This is also true, if we look at blue collar and white workers separately. While the backlog of winners is statistically significantly smaller than that for runners-up, there is no big difference economically (0.08 standard deviations). We also observe no differences between winners and runners-up concerning their balance sheet data, i.e., assets, cash and liabilities, and income statements, i.e., revenue, EBITDA, profits after taxes, depreciation, interest paid and cost of employees. Again, there are differences between these two types of firms and the group of firms ranked 3–5 in the auctions.

Table 2
Winners, runners-up and ranks 3–5.

Category	Variable	Winners	Runners-up	Diff.	Others	Diff.
		(1)	(2)	(3)	(4)	(5)
Employment	Observations	2361	2351		11,694	
	Employees	2635.2	2597.8	−37.5	1925.6	−709.7**
	Employees, blue	1762.7	1735.8	−26.9	1291.0	−471.6**
	Employees, white	774.1	765.6	−8.5	561.4	−212.7**
	Hires	8.10	8.22	0.13	5.68	−2.42**
	Hires, blue	6.90	7.03	0.13	4.82	−2.08**
	Hires, white	1.00	1.00	0.00	0.71	−0.28**
	Fires	7.01	7.04	0.03	5.38	−1.62**
	Fires, blue	6.04	6.08	0.04	4.66	−1.38**
	Fires, white	0.77	0.76	−0.00	0.57	−0.20**
	Wage	93.8	94.1	0.4	92.6	−1.2**
	Wage, blue	85.8	85.7	−0.2	84.8	−1.0**
	Wage, white	115.9	116.2	0.3	114.7	−1.2**
	Tenure	333.2	333.2	0.0	331.9	−1.36**
Projects	Backlog	−0.08	−0.00	0.08**	0.02	0.10**
	Distance	149.0	145.8	−3.2	141.0	−8.0**
Balance sheet	Observations	1820	1764		4594	
	Total assets	508.7	512.4	3.7	461.9	−46.8**
	Current assets	313.9	313.9	0.0	283.3	−30.6**
	Cash & cash equivalent	27.9	26.9	−1.0	24.5	−3.4**
	Fixed assets	188.8	192.6	3.8	173.7	−15.0**
	Total liabilities	402.3	403.2	0.9	365.1	−37.2**
	Non-current liabilities	89.0	89.8	0.8	79.2	−9.8**
Income statement	Revenue	792.6	791.3	−1.3	713.0	−79.7**
	EBITDA	10.7	11.1	0.4	10.1	−0.6**
	Profit after tax	−3.6	−3.3	0.2	−3.1	0.5**
	Depreciation	10.0	9.8	−0.2	9.2	−0.8**
	Interest paid	10.9	11.2	0.3	9.9	−1.0**
	Costs of employees	195.9	196.2	0.3	177.7	−18.2**
Financial ratios	EBITDA/total assets (in %)	5.0	4.7	−0.3	5.1	0.0
	Profit after tax/total assets (in %)	−1.5	−1.7	−0.1	−1.5	0.0
	Debt ratio (in %)	78.5	78.1	−0.5	78.5	−0.1
	Non-current liabilities/total assets (in %)	18.3	18.3	0.0	18.0	−0.3

Notes: Comparison uses observations for each auction and each firm before the event. All monetary values are in 2006 Euro. Significance levels: $*p < 0.05$, $**p < 0.01$.

Judging from observables, therefore, runners-up appear to be a valid control group.

3.4. Economic crisis

Our empirical model takes into account the recent economic crisis by estimating differential treatment effects for the crisis period. The crisis dummies are intended to capture the effect of a change in firms' employment, hiring and firing patterns caused by the economic crisis. It is therefore essential to determine when the crisis started. To make this judgement, we look at the stock and inflow of construction works and at indicators that address expectations in the sector. Relating the crisis start to macro data on stock and new orders follows our approach in Gugler et al. (2015a). Data on new orders already incorporate information about the future of the sector. To put further weight on the relevance of expectations on the economic development similar to the approach in Ramey (2011b), we extend the evidence to indicators on expectations and some narrative evidence on the expectations of increased government spending.

Economy-wide records on the stock of contracts outstanding and the new orders in the construction sector are shown in Fig. 1. The left graph of Fig. 1 provides the raw series and exhibits strong seasonal fluctuations, which make it hard to discern changing trends. For that reason, the right graph of Fig. 1 is based on a 12-month trailing moving average for each series. After a long period of increasing contract amounts for both series (more than six years), the inflow of contracts shows a decrease around March 2009, going below the value of the dip in August 2008. The stock of contracts appears to be negatively affected as of September 2009, when it falls below the dip in the beginning of 2008. Both series are trailing moving averages and include the effects of the month in question and the 11 months before. Therefore, for each month in the moving average, we subtract 11 months for the earliest month that affects changes in the series. The reduction of aggregate contract amounts therefore seems to have started in April 2008 for new contracts, and October 2008 for the stock of contracts.

Besides the likely importance of the stock and new orders of contracts as measures for the realized path of future activity, they may not contain all information about the expectations that firms' managers have. We use two available sources to obtain measures for expectations of construction firms, potentially affecting employment behavior. First, the Economic Sentiment Indicator (ESI), published by the European Union, from which we use the country-specific subseries for the construction sector. Two questions are considered in the data for the ESI: Is the stock of orders sufficient? How will total employment develop over the next 3 months? For each question, the difference of positive and negative answers relative to all answers is calculated. The series is the average of these two numbers. Second, the Business Cycle Survey by the Austrian Economic Chamber, which publishes the "new orders indicator" (NOI), where firms answer the question: Do you expect new orders for the current and subsequent quarter to be higher compared to the same quarter in the previous year? The NOI is the difference in percentage points between positive and negative answers.

Both indicators reveal a very close relationship in Fig. 2. Both indicators suggest that expectations of the respondents have substantially deteriorated by October 2008. Similar to the pattern of stock of contracts and new orders (Fig. 1), the ESI – containing judgement on the stock of contracts, too – resembles the inertia of the stock of contracts series in Fig. 1, while the NOI – where the question relates to new orders – falls earlier. The sentiment indicators do not support the early date of April 2008 which we drew from inflow of new contracts, despite appearing to be much more directed toward expectations. We see the two indicators as support for choosing October 2008 as a reasonable, rough approximation of expected changes in employment policies by firms, and use the date to define our dummy for the start of the crisis.

Can we observe changes in employment patterns by looking at employment directly? As the economy-wide contract variables in Fig. 1, the number of employees in construction firms display strong seasonal variation, shown on the left side of Fig. 3. The right side of the figure uses the trailing moving averages again, and compares the evolution of employment to the stock of contracts. Employment data count all employees of the winning and runner-up firms in our sample. Both moving averages turn around in January 2009; but while the stock of contracts maintains the downward trend, the employment drop comes to a halt in April 2009 and simply remains flat in the 12-month average, again indicating that some change of employment behavior could have taken place in the crisis.

3.4.1. Government intervention

A stimulus package, containing funds for infrastructure investment of 900 million Euro (railroads: 700, roads: 200), was passed by the parliament on 28 October 2008, a second package contained 875 million Euro for infrastructure investment (public buildings) passed

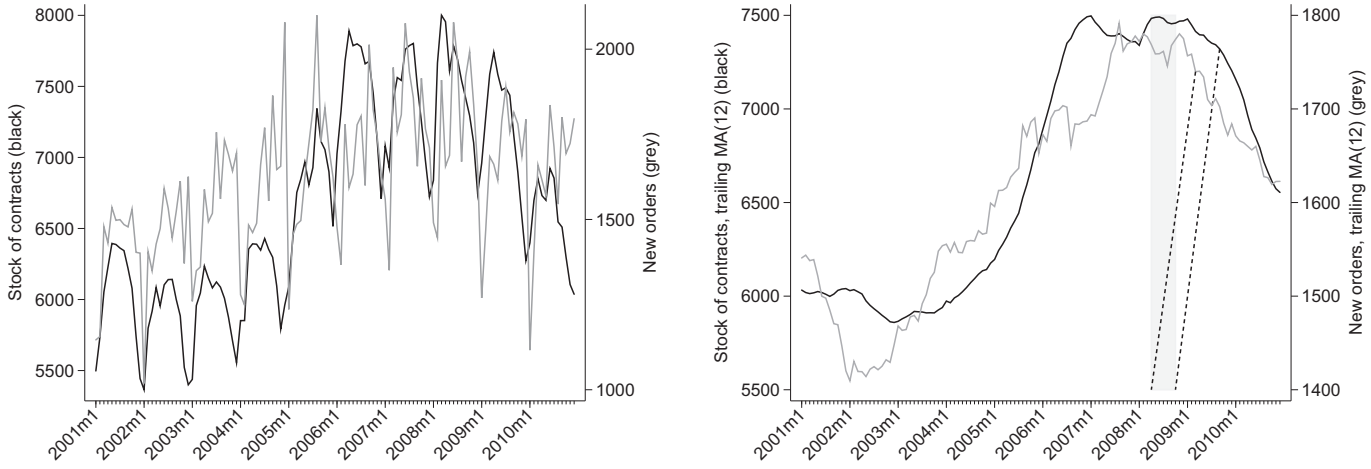

Fig. 1. Stock of contracts and new orders. Notes: The shaded area encompasses the period of the potential crisis start, derived from the stock of orders and new orders. Values are in million 2005 Euro.
Source: Data from Statistik Austria.

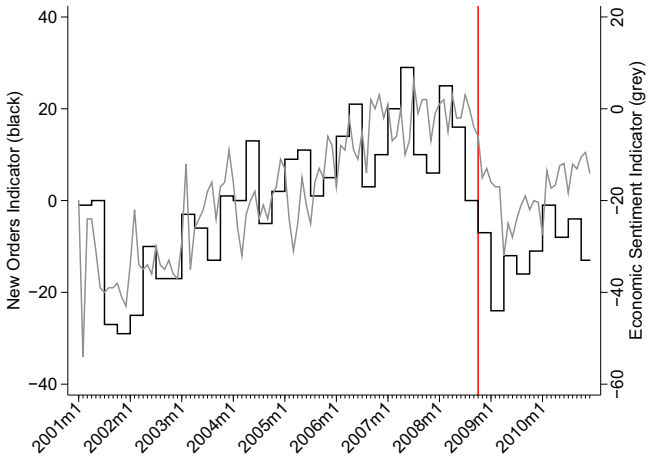

Fig. 2. Measuring expectations in the construction sector. Notes: The Economic Sentiment Indicator is published by the European Union. The graph shows the series for the construction sector. Seasonally adjusted by Eurostat. Sample size for Austria is about 10% of the 4134 (value for 2010) construction firms with 15 employees or more, with a response rate of about 50%. Construction firms covered correspond to about 15% of employment in the sector. (Source: European Commission, Business and Consumer Surveys.)
Business Cycle Survey: The Austrian Economic Chamber publishes the new orders indicator. The index covers some 430 firms, employing about 13,400 workers, on average.
Source: Austrian Economic Chamber, Quarterly Reports on the Business Cycle Inquiry of KMU-Forschung Austria.

on 23 December 2008. The total of 1.775 billion Euro (about 11% of total value added in the construction sector in 2008) was targeted at the construction sector and scheduled for the years 2009 to 2012. Tracing back in time the information about plans to pass stimulus packages, one does not find early positive indications. Mid of September 2008, the chairman of EU finance ministers stated that stimulus packages are not reasonable, after a meeting of EU finance ministers — a position that the Austrian minister of economic affairs also stated publicly at the end of July 2008. On 28 September 2009, national elections took place. On the day before the election, the minister of economic affairs opined that a stimulus package may not be necessary; the finance minister did not address the issue; the frontrunner of the largest party – then minister of transport – was in favor of stimulus package, without mention of size and targets. That the government would vote for stimulus was – in our reading of past media reports – not predictable until a few weeks before the binding decision.[13]

4. Results of empirical analysis

The presentation of the empirical results of the effect of winning has two parts. The first part, a graphical analysis, is based on disaggregated, daily data. We investigate the common trend assumption and the effect of winning a contract. In the second part, we present regression results. These are based on aggregated data and differentiate the effect of winning a contract before the crisis and in the crisis. We reduce the time dimension for the winners versus the runners-up and show the overall dynamic in two-week periods.

4.1. Common trends and effect of winning a contract

Before we investigate the effect of winning a contract before and in the crisis, we have a look whether the common trend assumption is likely to hold. To that end, we analyze our data disaggregated on the daily level and plot the development of employment, hires and fires before and after the event of the winners and the runners-up.

The left graph in Fig. 4 illustrates the daily effects for employment by comparing the employees of winners and runners-up. Each curve shows the normalized number of employees per working day (Monday to Friday, excluding days of the weekend) over a symmetric window before and after the event. Each auction- and firm-specific time series is normalized by the average of the pre-event number of employees of a firm in an auction, before averaging these normalized time series within the two groups. Winners and runners-up develop similarly before the event, but differently afterwards. While both types of firms increase their employment, the winning firms do so to a much larger extent after winning. Employment begins to trend upwards already in the month before the auction for both winner and runner-up firms, that is pre-existing trends in employment are similar for winners and runners-up.

On the right-hand side of Fig. 4, we show the difference between the number of employees of the winner and runner-up. This is the vertical difference between the two curves of Fig. 4's left-hand side series. While there is no discernible difference before the event, a significant effect emerges after the event. After 90 days, the winner employs about 1% additional employees, relative to the runner-up.

Fig. 5 shows the two curves of Fig. 4 as well as the curve for the group of bidders formed by ranks 3, 4 and 5 on the left. Next to it, we show the difference for the winner (black) and runner-up (grey) from the additional group of ranks 3–5. Before the event, employment patterns for winners and runners-up are quite similar to ranks 3–5, although the latter start to diverge from the former about 25 days before the event. Afterwards, the runner-up appears to become more similar to the lower-ranked bidders the more time passes. The gap of about 1% more workers between winner and runner-up therefore remains.

Fig. 5 shows closely related developments for winners and runners-up before the event, and both developments differ from ranks 3–5. After about 2 weeks after the event, the patterns diverge markedly. Thus, the graphical evidence supports similar pre-trends, as found in the comparisons in Table 2. Magnitudes and statistical significance are addressed by the estimations in the next section.

Fig. 6 shows the same daily effects for hires and fires of winners and runners-up as in the above figure. The time series of hires and fires oscillate much more than the employment series. Before the event, both winners and runners-up exhibit a similar pattern of hiring and firing. After the event, the series show that the effect of winning an auction on employment is due to fires and not hires. While winners and runners-up continue to hire similarly after the event, winners fire fewer workers than runners-up.

4.2. Effect of winning a contract before and in the crisis

For the estimation of the differential effect of the crisis, we aggregate our data to two-week periods as described by Eq. (1). Table 3 shows the regression results by fortnights for the pooled sample, pre-crisis and in the crisis.

Before turning to the main results, a few words are in order on pre-existing trends. None of the fortnight dummies interacted with the winner before the auction is significantly different from zero. This confirms again that pre-existing trends in employment are similar

[13] A short-time program was in place in Austria in some sectors, but not in the construction sector. See for example, Bock-Schappelwein et al. (2011). There was, however, a stimulus package directed at the construction sector, which we analyze in Gugler et al. (2015). In the construction sector, there is the widespread practice to send employees into unemployment in the winter months (predominantly December, January, and February), where demand is lower than in the rest of the year. This practice is, however, not crisis-specific and it was common practice also before the crisis.

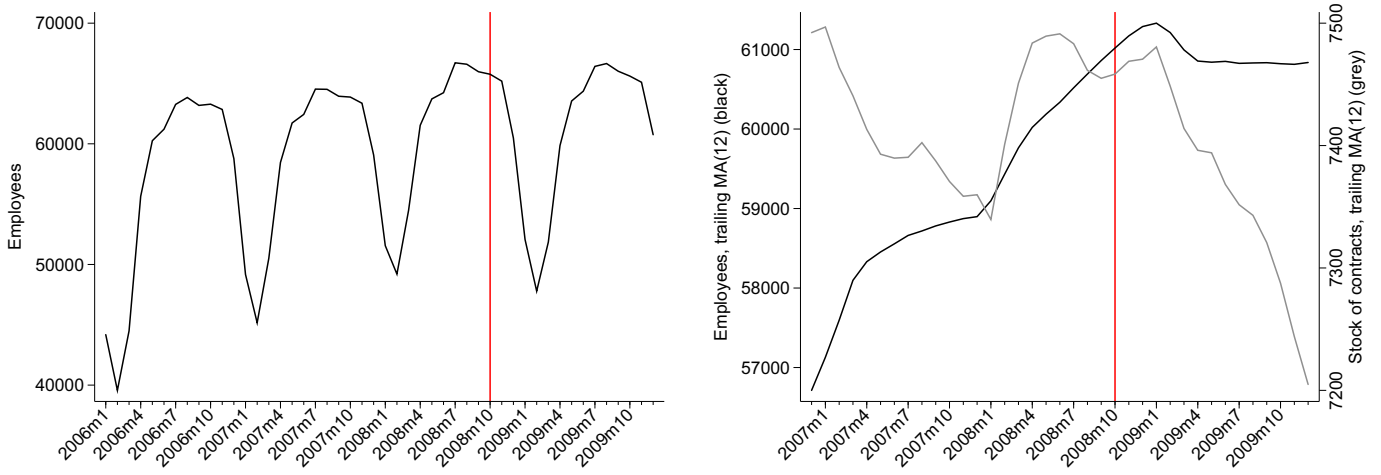

Fig. 3. Employees of the sample firms. Notes: The left graph in this figure shows the number of employees in construction firms; the right graph shows trailing moving averages, and compares the evolution of employment to the stock of contracts.

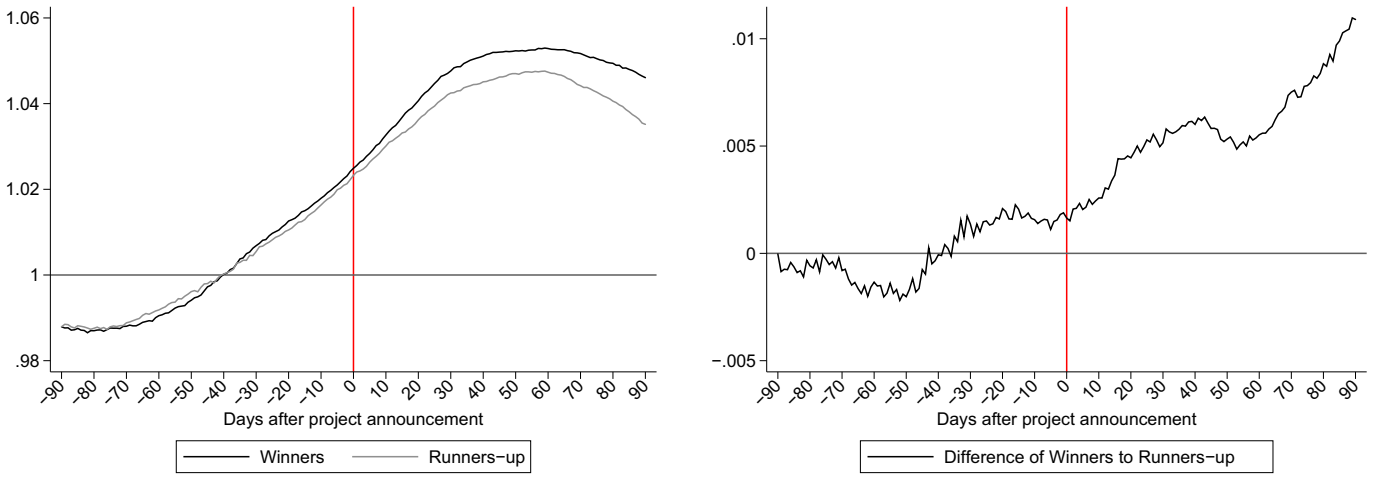

Fig. 4. Normalized number of employees of winner and runner-up. Notes: For each firm in each auction, the 180 working days time series was divided (normalized) by the average over the 90 working days before the event. The left graph displays the series of employees of the winner (black) and the runner-up (grey), which are the group-averages of all the normalized individual, i.e., firm and auction specific, time series. The right graph shows the day-by-day difference between winner and runner-up.

for winners and runners-up. Both types of firms start adding employees before the auction, and while winning firms add more workers than runner-up firms before the auction, they do so insignificantly.

In the pooled sample, winner and runner-up start to differ significantly in fortnight two after the event (+51 employees) and this difference widens around 65 employees after fortnight 4 (two months).

In the pre-crisis sample, winner and runner-up start to differ immediately in fortnight one. The maximum difference is reached in fortnight four with 83 employees. Evaluated at the mean number of employees, this amounts to around 3% of the workforce. The adjustment of the labor force after winning a contract is fairly fast, and around 80% of the total effect is achieved within one month (after two fortnights).[14]

In stark contrast, there is no significant effect in the crisis. Firms do not significantly add additional workers as a reaction to winning a public procurement auction in the crisis. In particular, the difference to the winner in the crisis-sample goes down from fortnight one to fortnight two, which differs from the pattern in the pre-crisis results. Pre-crisis, a strong and significant effect sets in fortnight one and peaks in fortnight four; the gap between the two firms starts to narrow afterwards. In the crisis, there is only one positive reaction at the end of the event window of around 70 employees in fortnight nine after winning, but the comparison to the pre-crisis effects shows that one should not put too much weight on this coefficient, because after more than four months other confounding events may be responsible for it.

In earlier work, we showed that firms reacted to the crisis by adjusting their prices and markups.[15] When it comes to their workforce, however, firms reacted to the crisis by delivering new projects without adjusting their workforce. Thus, the short-run reaction of

[14] Table B.1 in Appendix B describes the results for blue collar and white collar workers. Firms add to their workforce mainly blue collar workers to be able to fulfill their won contracts. Winning firms add about 70 blue collar and 12 white collar workers to their workforce before the crisis.

[15] Gugler et al. (2015b) found that the crisis had severe effects on competition in that the negative demand shock led to more bidders and these bid more aggressively. They found a significant decrease of the winning markup in the crisis period of about 4 percentage points relative to a pre-crisis mean winning markup of 22.9%.

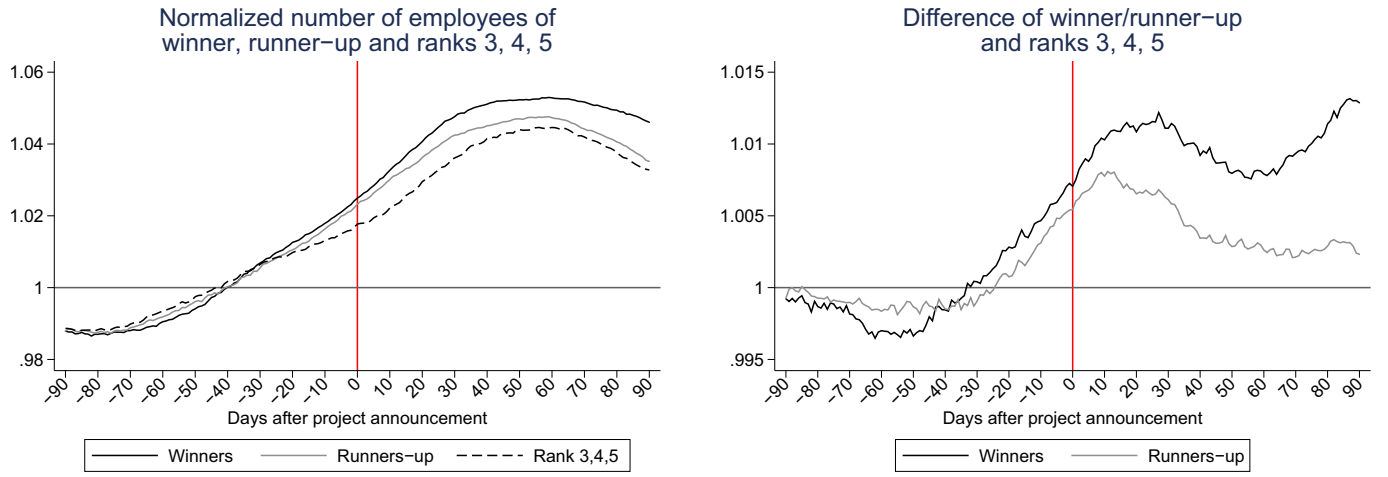

Fig. 5. Comparison of winner, runner-up and ranks 3, 4, and 5. Notes: For each firm in each auction, the 180 working days time series was divided (normalized) by the average over the 90 working days before the event. The left graph displays the series of employees of the winner (black), the runner-up (grey) and the three bidders ranking 3rd, 4th and 5th (dashed black), which are the group-averages of all the normalized individual, i.e., firm and auction specific, time series. The right graph shows the difference between the normalized number of employees for the winner (black) and runner-up (grey), to the time series of the bidders ranked 3, 4 and 5.

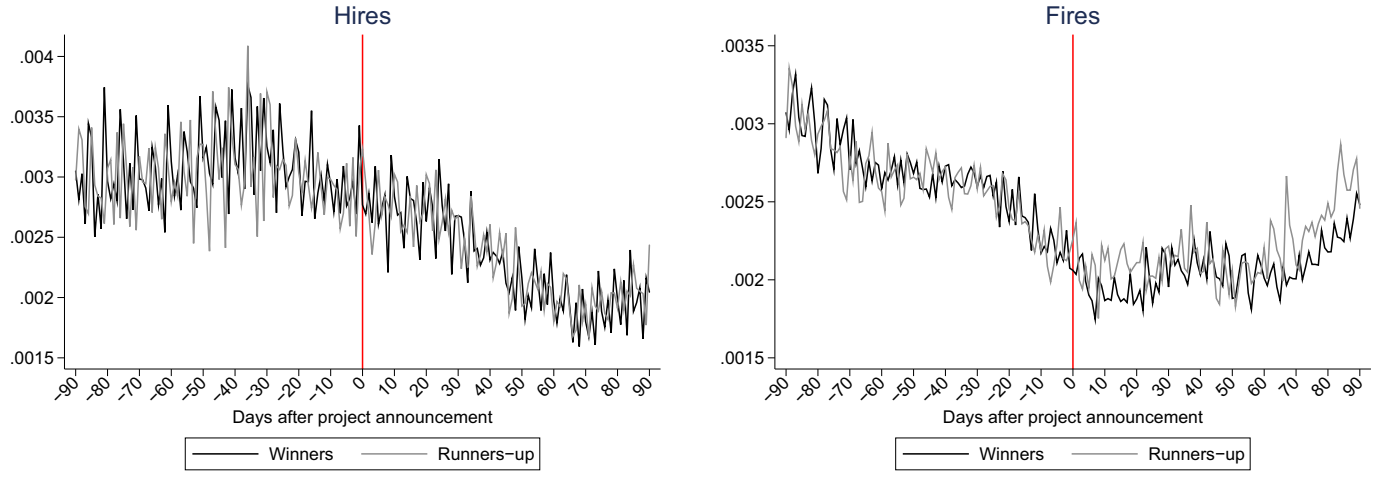

Fig. 6. Number of hires and fires of winner and runner-up. Notes: For each firm in each auction, the 180 working days time series was divided (normalized) by the average number of employees over the 90 working days before the event. The displayed series of hires (left) and fires (right) of the winner (black) and the runner-up (grey) are the group-averages of all the normalized individual, i.e., firm and auction specific, time series.

labor demand with respect to output is essentially zero in the crisis. Possible explanations include that firms may have started to hoard labor, and/or have used the crisis to remove inefficiencies in the production process (see the discussion in Section 6 later).

Back-of-the-envelope calculations suggest that the share of labor costs as a percentage of total contract value is around 60% and that average costs per job created by government intervention are around 15,000 Euro before the crisis.[16] In the crisis, this effect diminishes to essentially zero. Thus, while firms additionally employ people after a positive demand shock elastically in "normal" times, they do not do so in a period of a severe crisis.

[16] Labor costs are equal to: additional number of workers (73, average value of the 9 fortnights) times the daily wage for workers (93 Euro); times 1.31 (31% associated employer outlays); times 90 / 5 * 7(90 working days after winning the contract with the daily wage scaled to 7 days a week, that is 73 * 93 * 1.31 * 90 / 5 * 7 = 1,120,592 Euro. Labor costs divided by the average winning bid before the crisis gives 1,120,592 / 2,043,600 = 0.548. Average costs per job created are 1,120,592 / 73 = 15,351 Euro.

4.3. Mechanism: hires or fires

In the pooled sample, the winner of a procurement contract increases its workforce by around 65 workers. This effect depends on the general state of the economy. Before the recent economic crisis, the winning firm added about 80 workers to its workforce. In the crisis, firms essentially did not add workers. In a further step, we investigate the channels of how firms adjust their workforce. Winning firms could hire more workers, or fire fewer workers, or do both, compared to the firm that has marginally not won the contract. Table 4 shows estimation results distinguishing between hires and fires by fortnights for the whole time period as well as for the two sub-periods before and during the crisis.

We concentrate on a comparison pre-crisis and in the crisis. Pre-crisis, a firm winning a procurement contract reacts to this shock by significantly firing fewer workers than the runner-up immediately after the event. They do not hire more workers, however. The cumulative sum over hires and fires explains the net employment effects from above. In the crisis, winning firms keep their workforces stable. They do not hire nor fire significantly different from the runner-up

Table 3
Effect of winning on employment by fortnight.

	Pooled		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)
−8	−0.6	0.2	3.3	−1.5	−12.1	3.9
−7	3.3	−0.3	11.8	−5.5	−21.5	11.6
−6	8.8	2.1	23.1*	−8.1	−33.8*	27.5
−5	15.3	7.6	35.8**	−6.8	−45.4*	43.8
−4	23.3	15.8	48.7**	1.4	−51.6	51.9
−3	32.6*	22.9	58.6**	13.2	−45.7	48.8
−2	41.0**	28.2	64.4**	24.8	−33.0	38.6
−1	51.6**	32.8	70.8**	38.2	−14.6	21.9
1	64.9**	40.8	80.3**	53.4*	8.2	11.9
2	76.7**	50.5*	88.7**	68.0**	28.7	9.9
3	88.3**	59.5**	97.8**	78.8**	46.1	14.3
4	95.6**	65.1**	104.3**	82.8**	55.6*	23.8
5	99.6**	67.1**	109.8**	81.0**	55.9*	35.2
6	101.6**	66.7**	114.2**	76.5**	52.1	45.0
7	99.0**	68.7**	112.7**	74.0**	46.5	57.8
8	92.6**	69.8**	104.9**	73.5**	43.7	62.8
9	84.3**	71.1**	93.1**	72.5**	43.9	69.8*
Lagged employment	−0.06**		−0.06**		−0.07**	
Winner	−35.4*		−33.1		−33.3	
Backlog	32.1**		70.1**		47.4**	
Constant	2385.1**		2417.1**		2380.0**	
Observations	76,338		54,234		22,104	
R-squared	0.980		0.980		0.980	
Firm FE	Yes		Yes		Yes	

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Column (1) shows the fortnight estimate, column (2) the fortnight interacted with the dummy for the winner, using the pooled sample. Columns (3)–(6) repeat the estimates for the pre-crisis sample and for the crisis sample. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $_{*}p < 0.05$.

immediately after the event until fortnight 6. There are significantly negative coefficients for fires from fortnight 7 onwards. In our interpretation, we put again more weight on the coefficients immediately following the event. These results are consistent with our results from before using net additions to employment.

4.4. Mechanism: tenure

Another important aspect of employment contracts is tenure, the total number of days that a person is employed in the current employment. Stability of employment would imply a large metric

Table 4
Effect of winning on hires and fires by fortnight.

	$\tau\times$ winner					
	Pooled		Pre-crisis		Crisis	
	Hires	Fires	Hires	Fires	Hires	Fires
Variable	(1)	(2)	(3)	(4)	(5)	(6)
−8	1.2	−0.9	0.7	1.7	2.5	−7.2
−7	−1.4	−3.0	−2.0	0.2	−0.1	−11.1
−6	0.8	−3.3	−0.6	0.8	4.2	−13.5
−5	3.5	−6.2	6.6	−0.2	−4.3	−21.0*
−4	6.7	−3.8	10.2	−3.0	−2.0	−5.7
−3	1.2	−5.0	4.5	−6.4	−6.9	−1.6
−2	0.1	−5.4	4.5	−9.0	−10.9	3.3
−1	−0.1	−8.2	3.2	−12.3	−8.6	1.6
1	−2.2	−12.2*	0.0	−15.4*	−8.1	−4.7
2	−3.3	−14.7**	−2.8	−17.5**	−5.1	−8.0
3	−3.1	−11.7*	−5.6	−14.7*	2.5	−4.5
4	−5.3	−10.8*	−9.0	−10.8	3.5	−10.9
5	−7.4	−9.2*	−10.0	−8.3	−1.3	−11.8
6	−8.0	−10.4*	−11.2*	−8.2	−0.6	−15.8
7	−10.8*	−14.5**	−11.2*	−12.0*	−10.1	−20.8**
8	−10.4*	−12.3*	−10.3	−10.7	−10.9	−16.2*
9	−10.7*	−12.3*	−9.7	−7.1	−13.6	−25.0**
Observations	76,338	76,338	54,234	54,234	22,104	22,104
R-squared	0.370	0.295	0.370	0.284	0.374	0.333
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each numbered line stands for the fortnight relative to the event date. Column (1) shows the fortnight estimate interacted with the dummy for the winner for hires and column (2) for fires. Columns (3)–(6) repeat the estimates for pre-crisis and crisis samples. All regressions include the same control variables as in Table 3 as well as fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $_{*}p < 0.05$.

for tenure. If firms aim to hold their workforce more stable in the crisis, one would expect positive effects of the crisis on tenure. To deal with truncation bias toward later periods in the data, tenure is truncated symmetrically for all workers at 365 days. Our employment data goes on for the time needed after the last auction took place in our sample, which is the end of August 2009, plus 90 days after the event, plus 365 days. Therefore, for employment variables, tenure tells what is the average tenure of all employees active in the fortnight observation, given a maximum amount of tenure of a full year.

Table 5 shows estimation results for tenure by fortnights for the whole time period as well as for the two sub-periods before and during the crisis. Again, we concentrate on a comparison of pre-crisis and crisis periods. Pre-crisis employees of winning firms display lower tenure than employees in runner-up firms by around 1.5 days. Since mean tenure is 332 days, this amounts to 0.5%. Thus, before the recent economic crisis, the workforce is a little less stable in winning firms. In the crisis, in contrast, winning firms and runner-up firms do not display different tenure measures of their workforces. Thus, winning firm workforces become relatively more stable in the crisis.

4.5. Wages

Besides the above employment aspects, we also take a look at wages and distinguish between wages of all workers and the wages of new workers. Labor hoarding demands some kind of wage rigidity. If wages would adjust instantaneously to the decreased marginal product of labor after a negative demand shock, for example, employment could be held constant without the need to hoard labor. Due to collective bargaining agreements, wages are rather sticky in Austria, however, wages of new workers may be more flexible. Thus, we look at both wages for all workers of the firm and at wages of new workers. Table 6 describes these results. We observe that there is no reaction of wages – neither for all workers, nor for new workers, neither pre-crisis nor in the crisis – to demand shocks in the short run. We will return to these results when we discuss possible explanations of our findings.

4.6. Description of auctions and firms before and in the crisis

Our results show a strong and significant difference in the employment change between winning and losing an auction before the recent economic crisis compared to the effects during the crisis. One may suspect that there is not only a difference in employment change, but that there are also systematic differences in the auctions and bidders that participate in the auctions before the crisis compared to in the crisis. To rule out these competing explanations, we thus show summary statistics for the main variables describing auctions, firms and bidders in our sample across time.

Table 7 provides the numbers. The winning bid is not significantly different before and in the crisis, but the difference is large economically. It turns out, though, that only two large crisis-projects are responsible for the size of the difference. In the second line of Table 7 provided for the winning bid, the two largest projects are excluded from the comparison; the difference becomes also economically small. Because of only two projects causing this difference and because the empirical specification uses the discrete addition of a project as the basis of the event, it is not surprising that our findings remain unaffected when we drop these two crisis projects. A significant increase happens in the number of bidders participating in the auctions in the crisis. The average number of bidders increased from 6.9 to 7.8, which represents a 13% increase. While the total value from contracts in the private sector fell (see "Order flows", a macro-variable that varies by month), firms entered procurement auctions more frequently. All other auction characteristics do not significantly change across time: the percentage of auctions in heavy construction

Table 5
Effect of winning on tenure by fortnight.

	Pooled		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)
−8	0.1**	−0.0**	0.0**	−0.0**	0.3**	0.1**
−7	0.1**	0.1**	−0.2**	0.2**	0.7**	−0.1**
−6	−0.1**	0.2**	−0.6**	0.3**	1.**	−0.1**
−5	−0.3**	0.2**	−1.0**	0.4**	1.6**	−0.3**
−4	−0.4**	−0.1**	−1.3**	0.0**	1.7**	−0.4**
−3	−0.8**	−0.3**	−1.7**	−0.3**	1.2**	−0.3**
−2	−1.2**	−0.5**	−1.9**	−0.5**	0.6**	−0.4**
−1	−1.7**	−0.5**	−2.3**	−0.6**	−0.3**	−0.2**
1	−2.2**	−0.5**	−2.6**	−0.6**	−1.3**	−0.3**
2	−2.6**	−0.6**	−3.0**	−0.6**	−1.8**	−0.5**
3	−3.0**	−0.7**	−3.2**	−0.8**	−2.6**	−0.5**
4	−3.2**	−1.0**	−3.3**	−1.1**	−3.2**	−0.9**
5	−3.3**	−1.3**	−3.3**	−1.4**	−3.6**	−1.0**
6	−3.5**	−1.2**	−3.1**	−1.6**	−4.5**	−0.4**
7	−3.3**	−1.2**	−2.7**	−1.7**	−4.9**	−0.1**
8	−3.1**	−1.3**	−2.2**	−1.7**	−5.2**	−0.2**
9	−2.8**	−1.2**	−1.6**	−1.7**	−5.8**	0.0**
Winner	0.5**		0.7**		0.1**	
Backlog	−1.1**		−0.9**		−2.3**	
Constant	333.5**		333.3**		334.4**	
Observations	76,338		54,234		22,104	
R-squared	0.635		0.638		0.716	
Firm FE	Yes		Yes		Yes	

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Column (1) shows the fortnight estimate, column (2) the fortnight interacted with the dummy for the winner, using the pooled sample. Columns (3)–(6) repeat the estimates for the pre-crisis sample and for the crisis sample. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 6
Effect of winning on wages by fortnight.

	Wages of all workers				Wages of new workers			
	Pre-crisis		Crisis		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
−8	0.10*	0.00	0.18**	0.05	0.07	−0.42	−1.69	2.47
−7	0.15*	0.05	0.48**	−0.01	0.07	−0.00	0.76	0.63
−6	0.18*	0.13	0.91**	−0.22	−1.11	0.85	1.03	0.64
−5	0.25*	0.14	1.22**	−0.21	−0.73	0.28	2.17	−0.33
−4	0.30**	0.11	1.57**	−0.26	−1.35	0.21	2.06	−0.39
−3	0.29*	0.10	1.74**	−0.31	−2.50**	1.16	0.11	1.19
−2	0.31*	0.07	1.91**	−0.28	−2.32**	0.73	1.29	−1.28
−1	0.31*	0.02	1.89**	−0.19	−2.54**	0.67	0.38	0.27
1	0.23	−0.03	1.74**	−0.25	−2.59**	0.58	−1.38	0.83
2	0.30*	−0.10	1.72**	−0.28	−2.36**	−0.14	−0.35	−0.67
3	0.33*	−0.14	1.69**	−0.20	−2.82**	−0.36	−1.40	0.24
4	0.35*	−0.17	1.67**	−0.19	−3.41**	1.62	−1.57	−1.21
5	0.36**	−0.17	1.75**	−0.28	−2.95**	0.15	−2.02	−1.36
6	0.41**	−0.17	1.76**	−0.26	−2.47**	0.54	−2.08	−1.53
7	0.54**	−0.18	1.72**	−0.22	−0.93	−1.29	−3.22*	0.51
8	0.78**	−0.22	1.62**	−0.23	−1.45	0.52	−3.21*	−0.19
9	1.01**	−0.19	1.54**	−0.28	−0.46	−0.75	−2.55	−0.99
Winner	0.09		0.19		−0.31		0.35	
Backlog	−0.19**		−0.43**		0.29*		−2.35**	
Constant	92.1**		94.2**		83.1**		86.1**	
Observations	54,234		22,104		43,354		17,030	
R-squared	0.933		0.935		0.258		0.300	
Firm FE	Yes		Yes		Yes		Yes	

Notes: The left part, columns (1)–(4), show the results for average wages of all workers. Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Column (1) shows the fortnight estimate, column (2) the fortnight interacted with the dummy for the winner, using the pre-crisis sample. Columns (3)–(4) repeat the estimates for the crisis sample. Columns (5)–(8) repeat the pre-crisis and crisis results for the average wages of new workers. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $_{*}p < 0.05$.

opposed to building, contracts for which a general constructor was demanded and auctions with an open procedure.

For the information for firms' employment and financial statements, each firm is represented once before and once in the crisis. All firms for which we use employment data could also be matched to Bureau van Dijk's financial statement database, but not all have data available, which is responsible for the reduction of the firm sample from "Firms" to the number of firms in "Balance sheet". We observe that wages increased significantly in the crisis. The reason for this increase is that collective bargaining agreements that took place immediately before the crisis were based on biased estimates for the economic development in Austria. Accompanied with a significant slowdown in the inflation rate, real wages then increased substantially in the crisis (Scheiblecker et al., 2010; Leoni and Pollan, 2011). While indebtedness and profitability are higher and interest payments are lower in the crisis, we see no differences in firm size. Thus, it appears that firms economize on interest payments and possibly other resources, in order to increase liquidity in the crisis. In the crisis, they also have a higher backlog, which is a sign of participating and consequently winning more auctions in public procurement auctions since our measure of backlog does only account for public procurement contracts and not contracts from the private sector. Together with the findings that there are more participating firms and that average mark up declined (see Gugler et al., 2015a), we conclude that while characteristics of auctions and firms did not change significantly, the behavior of firms changed in that more firms submit bids in the crisis. Moreover, it appears that firms economize on their resources presumably to increase liquidity during the crisis.

Next, we assess whether firms are different before and in the crisis by comparing the share of won auctions of the 100 most frequent bidders over time. Fig. 7 shows that the share of won contracts before the crisis is positively correlated with the share of won contracts during the crisis. We observe some firms that won contracts before, but none afterwards. In general, however, it seems that the same firms participated and won before and in the crisis.

Fig. 8 graphically reveals more detail about the auctions' sub-industries and regional distribution before and in the crisis. Our data source provides a classification of projects according to about 50 different main and sub-industries. Shown on the left of Fig. 8 are the categories before (horizontal) versus in the crisis (vertical). The grey line depicts the expected number of projects according to the relation of the 765 crisis-auctions in the sample to the 1908 pre-crisis auctions ($765/1908 = 0.40$). There are no strong deviations from the expected relation. The two top-level categories *Heavy construction general* and *Building construction general* have been assigned somewhat less in the crisis, and lie below the 0.40-share-line. Not all projects have been assigned a lower-level industry, but only the top-level category. The difference for the two top-level categories must be an artefact of increased detail of classification by the company, because heavy and building together comprise 95% of all categories, a relation which did not change in the crisis. The right graph of Fig. 8 shows the distribution of regions according to the NUTS-3 classification before and in the crisis. Some variation around the 40-percent-share-line for the crisis can be seen, but is not conspicuous for particular regions.

Finally, we estimate our basic regressions for net employment before and in the crisis additionally accounting for heterogenous effects according to the observables profitability and non-current liabilities to total assets ratios. In Table 8, we interact bidders with a dummy variable indicating whether the firm has above median profitability (EBITDA/total assets) or above median leverage (non-current

Table 7
Difference before and in the crisis across auctions and firms.

		Before the crisis		In the crisis		Difference
		Mean	Std. err.	Mean	Std. err.	
Category	Variable	(1)	(2)	(3)	(4)	(5)
Auctions	Number of auctions	1908		765		
	Winning bid (1000 Euro)	1842.4	101.9	2351.4	367.6	509.0
	Winning bid (1000 Euro), excl. 2 outliers	1842.4	101.9	1920.6	174.8	78.2
	Open procedure	0.85	0.01	0.83	0.01	−0.03
	Number of bidders	6.90	0.06	7.83	0.12	0.93**
	General constructor	0.06	0.01	0.06	0.01	0.00
	Heavy construction	0.45	0.01	0.42	0.02	−0.04
	Order flows (mill. Euro)	1769.2	4.1	1679.1	6.5	−90.1**
Firms	Number of firms	476		325		
	Employees	654.3	56.7	573.6	64.8	−80.68
	Hires	2.07	0.20	1.67	0.22	−0.40
	Fires	1.73	0.18	1.77	0.23	0.03
	Wage	86.3	0.5	88.9	0.7	2.66**
	Wage new	75.5	0.6	79.6	0.8	4.06**
	Backlog	−0.12	0.02	−0.02	0.03	0.10**
	Distance	81.2	4.0	90.7	5.2	9.51
Balance sheet	Number of firms	282		180		
	Total assets	182.8	15.2	204.1	29.1	21.30
	Current assets	117.8	9.9	110.8	14.9	−7.06
	Cash & cash equivalent	10.3	1.0	12.6	1.9	2.24
	Fixed assets	64.9	5.7	88.9	13.9	24.03
	Total liabilities	145.5	12.3	159.7	23.0	14.25
	Non-current liabilities	29.6	2.5	52.8	8.3	23.25**
Income statement	Revenue	270.7	25.1	312.2	38.9	41.59
	EBITDA	4.9	0.5	6.5	1.0	1.60
	Profit after tax	−3.2	0.6	1.1	0.8	4.22**
	Depreciation	4.2	0.3	5.0	0.6	0.80
	Interest paid	5.0	0.5	3.1	0.5	−1.93**
	Costs of employees	71.0	6.2	77.8	9.2	6.84
Financial ratios	EBITDA/total assets (in %)	6.3	0.4	8.1	0.6	1.86**
	Profit after tax/total assets (in %)	−4.2	0.6	0.3	0.8	4.50**
	Debt ratio (in %)	79.1	0.5	78.3	0.7	−0.72
	Non-current liabilities/total assets (in %)	16.9	0.5	25.5	1.5	8.69**

Notes: Mean and standard errors. Monetary values in million 2006 Euro. Employment data for firms and their financial statement data are based on each firm's average before the crisis, and in the crisis; i.e., each firm occurs only once per subsample. The number of firms in the line "balance sheet" represents those firms for which data was available from Bureau van Dijk's database. Significance levels: $^{**}p < 0.01$, $_{*}p < 0.05$.

liabilities/total assets) ratios. Moreover, we include the full set of interaction variables (firm times above median dummy, winner times above median dummy, and firm times post dummy times above median dummy), leaving the winner times post dummy times above median dummy to capture the (differential) treatment effect. As can be seen, more profitable firms react less and more highly indebted companies react more to demand shocks in terms of labor demand, before and in the crisis. Albeit, these effects are not significant.

Summarizing, systematic observable differences in auctions, firms or bidders cannot explain our main findings. However, there appears to be a change in behavior of bidders in that they bid more aggressively and build up liquidity during the crisis.

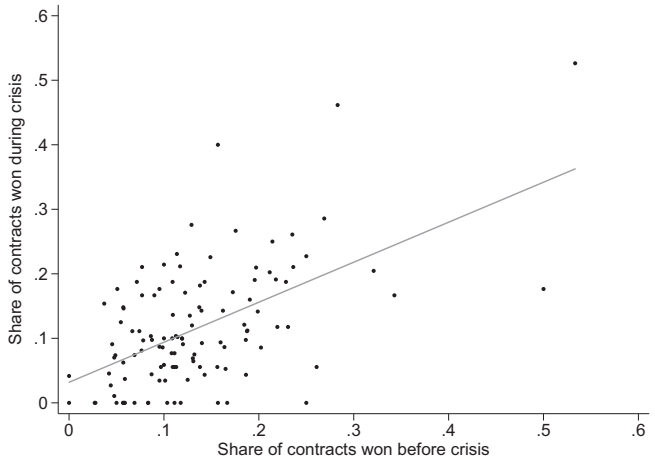

Fig. 7. Share of won auctions before and in the crisis. Notes: Firms that are among the 100 most frequent bidders either before or in the crisis. This union of firms yields 116 observations. The 116 firms have submitted 75.9% of all bids. The grey line shows the linear regression line (slope = 0.620).

5. Robustness checks

To substantiate our results, we present robustness checks. First, we add monthly dummy variables that account for seasonal effects. Second, we include an additional comparison group into our regressions. Third, we account for money left on the table, a measure to assess the differences between the bid of the lowest and the second lowest bidder. We then check whether additional auctions in the evaluation period would contaminate our comparison of winner and runner-up. Finally, we use a sample that also includes auctions where some bidders allegedly collude. For all robustness checks, we repeat the basic regressions in fortnights pre-crisis and in the crisis. We show our results in Tables 9 and 10 and will discuss them in Sections 5.1–5.5.

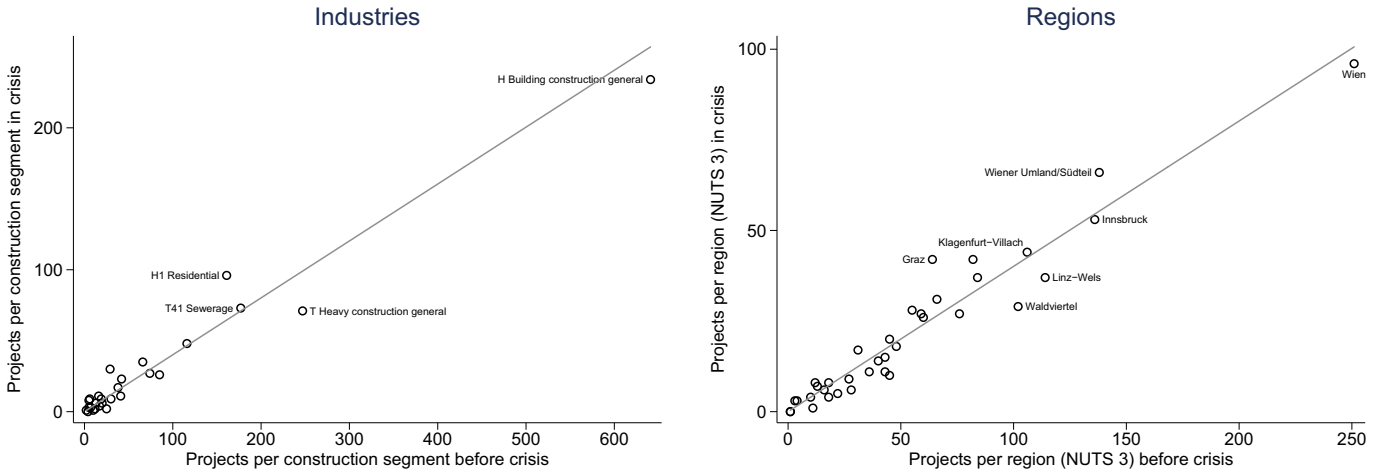

Fig. 8. Industries and regions of auctions before and during crisis. Notes: The left graph shows the count of projects by construction segment before the crisis (horizontal) vs. in the crisis (vertical) in our sample. Our data source provides a classification of projects, assigning about 50 different sub-industries. The graph on the right shows the regional distribution before and in the crisis, according to the NUTS-3-classification. The grey line depicts the expected number of crisis-projects based on the overall relation of projects, which is 765 in crisis compared to 1908 before the crisis, giving a slope of 0.4.

5.1. Adding monthly dummy variables

Our first robustness check estimates our regression model also including monthly dummy variables. Herewith, we want to accommodate potential seasonal effects that may correlate with the treatment effect. The construction sector is characterized by seasonal fluctuations that reflect weather conditions. During winter, construction work usually rests. Column 1 in Tables 9 and 10 shows that our main results do not change when we include monthly dummy variables.

Table 8
Effect of winning on employment with median dummies.

	EBITDA/total assets				Non-curr./total assets			
	Pre-crisis		Crisis		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
−8	3.5**	−1.9**	−13.3**	8.1**	2.6**	−1.8**	−13.3**	8.1**
−7	13.4**	−6.5**	−28.5**	22.6**	11.7**	−6.4**	−28.5**	22.6**
−6	27.6**	−10.3**	−51.4**	50.9**	24.8**	−9.8**	−51.4**	50.9**
−5	44.0**	−10.0**	−78.1**	84.6**	40.4**	−9.3**	−78.1**	84.6**
−4	61.0**	−1.4**	−96.4**	106.8**	56.8**	−0.8**	−96.4**	106.8**
−3	74.2**	11.2**	−90.8**	105.5**	69.5**	11.9**	−90.8**	105.5**
−2	81.6**	24.5**	−74.9**	93.4**	76.3**	25.1**	−74.9**	93.4**
−1	90.5**	39.3**	−47.9**	66.0**	84.6**	39.7**	−47.9**	66.0**
1	132.9**	94.0**	−20.7**	67.7**	73.3**	49.1**	−38.3**	−47.8**
2	143.7**	111.3**	7.0**	54.2**	84.2**	66.3**	−10.6**	−61.4**
3	155.8**	123.9**	25.4**	52.0**	96.2**	79.0**	7.9**	−63.5**
4	164.3**	128.5**	29.7**	60.0**	104.8**	83.6**	12.2**	−55.6**
5	172.1**	125.3**	21.3**	75.4**	112.5**	80.4**	3.8**	−40.1**
6	177.7**	119.7**	10.1**	90.6**	118.1**	74.7**	−7.4**	−25.0**
7	175.6**	116.9**	−1.3**	109.3**	116.0**	71.9**	−18.8**	−6.2**
8	165.3**	116.8**	−5.3**	116.1**	105.7**	71.9**	−22.9**	0.6**
9	149.2**	117.1**	−3.6**	125.3**	89.6**	72.1**	−21.1**	9.8**
Firm > median	−97.6**		−21.8**		−324.7**		−26.4**	
Winner > median	64.1**		62.3**		5.4**		−69.2**	
Firm > median × post	−59.4**		9.1**		57.5**		37.5**	
Winner > median × post		−89.9**		−44.7**		11.8**		126.3**
Lagged employment	−0.06**		−0.07**		−0.05**		−0.07**	
Winner	−66.5**		−98.3**		−35.5**		−14.1**	
Backlog	82.9**		80.8**		106.5**		81.0**	
Constant	3104.3**		2818.2**		3177.6**		2818.8**	
Observations	42,210		14,400		42,210		14,400	
R-squared	0.977		0.977		0.977		0.977	
Firm FE	Yes		Yes		Yes		Yes	

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Columns (1)–(2) show the results in the pre-crisis period when controlling for the dummy "Firm > median", capturing firms above the median size of *EBITDA/total assets* in the subsample. The coefficients below the fortnight lines show the dummy, and interactions of the dummy with the winner, and each interacted with a "post"-dummy for fortnights after the event. Column (1) shows the fortnight estimate, column (2) the fortnight interacted with the dummy for the winner — both for the pre-crisis period. Columns (3)–(4) repeat the estimates for the crisis sample. Columns (5)–(8) repeat the estimates with a median-dummy for *Non-current liabilities/total assets*. Backlog after the event, i.e., $\tau > 0$, remains constant at the level of $\tau = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: ** $p < 0.01$, * $p < 0.05$.

Table 9
Robustness checks pre-crisis.

	Month dummies	Compared to rank > 2		Similar MLOTT	Won after event	All auctions
	$\tau\times$ winner	$\tau\times$ winner	$\tau\times$ rank > 2	$\tau\times$ winner	$\tau\times$ winner	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)
−8	−1.5**	−1.5	0.5	0.9	−1.5	−3.8
−7	−5.4	−5.4	−0.4	−1.3	−5.5	−10.3
−6	−8.2	−8.1	−1.8	−1.9	−8.1	−14.2
−5	−6.8	−6.8	−4.5	1.1	−6.8	−12.7
−4	1.4	1.4	−7.5	9.7	1.4	−4.1
−3	13.1	13.1	−9.6	21.6	13.2	8.4
−2	24.7	24.8	−10.9	32.4	24.8	21.4
−1	38.1	38.1	−13.4	45.4	38.2	38.5
1	53.3*	53.4*	−17.1	61.0*	45.9	59.3**
2	67.9**	67.9**	−17.9	76.6**	54.0*	76.8**
3	78.6**	78.7**	−17.7	88.0**	56.8*	88.0**
4	82.7**	82.8**	−16.0	91.9**	55.0*	91.4**
5	80.9**	80.9**	−16.9	90.3**	47.3*	88.8**
6	76.4**	76.4**	−18.7	85.5**	36.7	83.8**
7	73.9**	74.0**	−19.3	82.4**	28.1	82.1**
8	73.4**	73.4**	−20.0	80.5**	20.8	84.8**
9	72.4**	72.4**	−20.1	77.1**	13.7	84.9**
Observations	54,234	119,358		51,480	54,234	64,422
R-squared	0.981	0.981		0.980	0.981	0.980
Firm FE	Yes	Yes		Yes	Yes	Yes

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Each column shows a different robustness check for the pre-crisis sample. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 10
Robustness checks in crisis.

	Month dummies	Compared to rank > 2		Similar MLOTT	Won after event	All auctions
	$\tau\times$ winner	$\tau\times$ winner	$\tau\times$ rank > 2	$\tau\times$ winner	$\tau\times$ winner	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)
−8	4.3	4.0	1.7	5.2	3.9	11.4
−7	11.8	11.7	2.8	13.0	11.6	23.9
−6	27.5	27.5	6.0	26.2	27.5	37.1
−5	43.7	43.8	10.1	39.3	43.9	47.8
−4	51.7	51.9	13.9	43.0	52.0	51.4
−3	48.2	48.6	8.0	35.3	48.9	45.9
−2	37.8	38.4	−1.0	22.7	38.7	35.9
−1	21.0	21.6	−12.3	4.3	22.0	24.7
1	11.1	11.7	−23.9	−6.0	6.5	21.0
2	9.0	9.6	−29.4	−6.2	−2.0	21.4
3	13.5	14.1	−31.2	1.0	−3.7	25.4
4	23.0	23.6	−28.4	14.6	−1.1	33.0
5	34.3	34.9	−20.4	29.6	4.1	40.9
6	44.2	44.7	−14.5	43.1	6.2	46.0
7	56.9	57.5	−8.8	58.9	13.4	54.3
8	61.9	62.5	−4.9	63.8	13.9	56.7
9	68.9*	69.5*	−3.6	69.4*	14.8	60.1
Observations	22,104	49,572		21,132	22,104	26,568
R-squared	0.980	0.981		0.980	0.980	0.979
Firm FE	Yes	Yes		Yes	Yes	Yes

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Each column shows a different robustness check for the crisis sample. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $^{*}p < 0.05$.

5.2. Ranks 3–5

In the results presented next, we include a further comparison group: the bidders that rank 3rd, 4th and 5th in each auction. Adding these results serves two purposes. First, it shows that higher ranks do not dissipate the effect of winning an auction. Second, it shows that higher ranks are different from the runner-up, too. Columns 2 and 3 of Tables 9 and 10 present the results. Our main results concerning the treatment effect of winning are not affected by the inclusion of firms ranked 3–5 in the auctions.

5.3. Money left on the table

Money left on the table (MLOTT) is calculated as the difference between the lowest and the second lowest bid over the lowest bid. Our aim in this robustness check is to use MLOTT as a measure of the proximity of the bids and bidders' cost valuations[17] of winner

[17] If bidders behave optimally in the auction, there is a monotone relation between bids and valuations. For a detailed theoretical analysis of auctions, see for example, Krishna (2009).

Table 11
Money left on the table.

Bidders	MLOTT	Standard error	Observations
	(1)	(2)	(3)
2	0.1572	0.0205	52
3	0.1036	0.0108	153
4	0.0864	0.0057	268
5	0.0822	0.0051	337
6–10	0.0691	0.0020	1435
11–22	0.0574	0.0044	344

Note: MLOTT in column (1) is defined as the difference between the bids of rank 1 and rank 2, relative to the bid of rank 1. The table shows the relationship of MLOTT to the number of bidders.

and runner-up, and exclude auctions where winner and runner-up are far from each other in terms of MLOTT. MLOTT is, however, systematically affected by the number of bidders in the auction. Table 11 illustrates the relationship between MLOTT and the number of bidders. Since we want to account for bidding differences between winner and runner-up, we need to account for the effect of the number of bidders on MLOTT. We adjust MLOTT via an OLS-dummy-regression on the number of bidders and use the absolute value of the residuals as the measure for proximity.[18] The residuals from this regression are simply the difference of an MLOTT-value to its group mean, where a group is defined by the number of bidders in the auction. Auctions with absolute values of the residuals above their 95-percentile-value are excluded from the regressions in this robustness check, to select for increased comparability of winner and runner-up.[19] Column 4 of Tables 9 and 10 presents the results. Results remain essentially the same as the main results.

5.4. Contamination of evaluation period

Our empirical model may suffer from contamination of the evaluation period. This could be the case when the winning firm in period t wins additional auctions systematically more often in the evaluation period than the runner-up. While we do not think that this is a large problem in our set-up, we performed a robustness check and include in our empirical model a count variable measuring additional auctions won by the winner and the runner-up during the evaluation period, respectively. The results in column 5 of Tables 9 and 10 show that our main conclusions are unaffected, if we control for potentially contaminating (additional) public auctions during the evaluation period. Because of a lack of data from the private sector, we however cannot exclude differential results due to private construction demand.

5.5. Sample of auctions

As our identification strategy relies on the idea that firms behave competitively, we dropped for our main analysis auctions in which some bidders are accused of collusive behavior. These are auctions in highway and heavy construction in the Southern part of Austria (Carinthia and Styria). For comparison, we run our regression model for all auctions in our sample, also including these auctions that might be affected. We show the results in Column 6 of Tables 9 and 10. Our main conclusions do not change, but pre-trends may be affected by collusion.

6. Discussion

Our results imply a strong reaction of employment to output before the crisis, and a labor demand reaction with respect to output of essentially zero during the economic downturn. Winning firms fire fewer workers but do not hire more workers before the crisis. Average tenure is lower for winning firms. In the crisis, we see neither more hires nor fewer fires nor shorter tenure after winning an auction. Wages do not change after winning an auction — neither before nor in the crisis. We next discuss different interpretations and their consistency with this pattern of results.

6.1. Labor hoarding

Labor hoarding is the practice of firms to retain workers released from finished projects to save, in an effort to maximize profits, on training, hiring, firing and other (quasi-)fixed costs. One implication of labor hoarding is pro-cyclical labor productivity, since employment would only be reduced if the value marginal product fell below the wage, not accounting for the sunk costs of hiring and training. One explanation for the pattern in our results is thus that firms change their labor hoarding behavior during the crisis.

The extent of build-up of labor after the event by the winner seems large enough to warrant the view that most of the necessary workforce for the (average) project won is employed after the announcement. Moreover, pre-crisis, winning firms' employees display lower average tenure. While wages are sticky relative to the runner-up also pre-crisis, there is no clear evidence for labor hoarding before the crisis. In the crisis, however, the winner may deploy hoarded workers to the new project, since he does not add workers after winning (but does also not fire workers). Tenure goes up in the crisis after winning a contract compared to pre-crisis. The fact that winners hold their workforces more stable compared to runners-up is consistent with (efficient) labor hoarding. The necessary condition for labor hoarding – some kind of wage rigidity – is also fulfilled in the crisis. The question of course then is why firms react so strongly and timely to the new project before the crisis, but hoard labor during the crisis?

One consistent explanation for our results is the version of the labor hoarding model of Bentolila and Bertola (1990), according to which the optimal policy of the firm is a "range of inaction" for a wide range of fluctuating demand. Above, we showed that the Austrian construction sector experienced a prolonged period of expansion before the economic crisis, e.g., Fig. 1 displayed a period of more than six years of increasing contract amounts before the crisis. It appears therefore likely that firms were near the "hiring barrier" before the crisis, i.e., the threshold where the discounted expected marginal revenue product of labor (MRPL) becomes larger than the discounted wage costs (plus the hiring costs). Accordingly, an additional positive demand shock (winning an auction) leads to a larger employment in the firm before the crisis.[20]

The negative demand shock of the economic crisis, however, is likely to have moved firms more toward the "firing line", i.e., the threshold where the expected MRPL given up by firing the worker becomes lower than the discounted wage costs saved by firing (minus dismissal costs). In the crisis, therefore, the positive demand shock of winning an auction may not be large enough to induce hiring by firms. Thus, in the crisis winning firms neither hire workers nor fire workers after experiencing a positive demand shock, equivalent to the results we find in the data. The demand shock is

[18] Table C.1 in the Appendix C shows the coefficients of the regression. The regression does not include a constant, which makes the coefficients equal to the means of each group.
[19] Taking a 90-percent-threshold does not change the results, either.
[20] One may view the fact that pre-crisis firms did not hire more workers but fired fewer workers after winning as being inconsistent with this explanation. However, as we have shown in Fig. 4, both types of firms, winning and runner-up firms, increase their employment in the month before the event.

not large enough – given the position of the firm inside the "range of inaction" and the heightened uncertainty surrounding the future in the crisis – to hire workers. As the build-up before auctions in the crisis is lower than for auctions before the crisis, they do not fire fewer workers after winning (see Table 3). They hold their workforces stable. Thus, the labor hoarding model of Bentolila and Bertola (1990) could explain both the significant reaction to additional demand before the crisis as well as the muted reaction in the crisis.

Summarizing, labor hoarding is consistent with a pattern where the difference between winner and runner-up workforce is not affected by winning a construction contract as we find in the economic crisis. We, however, have to speculate why there is a change in the winner-effect in the crisis. We view the version of the labor hoarding model of Bentolila and Bertola (1990) as one explanation fitting to what happened in the Austrian construction sector.

6.2. Collusion

Collusion may be a competing hypothesis to labor hoarding. In a collusive environment, employment decisions may be disconnected from winning an auction. For example, if they collude firms may know in advance the auctions they would win and adjust their labor demand decisions accordingly. Our identification strategy of the employment effects relying on the close daily correspondence between demand increase and employment decision would then break down. In order to explain the results before the crisis, however, there must not be collusion before the crisis. Thus, for collusion to explain our results there must be a switch from competition to collusion in the crisis.

With varying assumptions on the characteristics of the demand process and the existence of capacity constraints, models have generated a relationship to prices that is countercyclical (Rotemberg and Saloner, 1986), pro-cyclical (Haltiwanger and Harrington, 1991; Staiger and Wolak, 1992), or have the ability to predict both patterns, depending on the concrete circumstances (Bagwell and Staiger, 1997; Fabra, 2006; Knittel and Lepore, 2010). The assumptions on the nature of the business cycles in the studies are a matter of debate, or taste, and empirical models of the construction sector suggest that neither completely unconstrained nor strictly constrained (exogenous or endogenous) capacities are appropriate, but rather an intermediate case between the two (e.g., Jofre-Bonet and Pesendorfer, 2003; Gugler et al., 2015b). None of the models therefore seems to be a strong indicator that a regime switch from competition to collusion causes the difference in our employment results. Additionally, it is unclear if firms would be able to break the association between employment behavior and collusion if they think that it could be used as a "test on collusion", e.g., by a competition authority. We acknowledge, though, the possibility that a regime change could produce the changing pattern in our results. To counter this explanation, we did drop all procurement auctions in alleged segments and parts of Austria which are subject to bid rigging investigations, according to public information (around 16% of auctions in our sample).

6.3. Productivity

Another explanation why firms do not employ more people in the crisis after winning an auction could be increased productivity of firms. An important effect of the crisis was an increase in competition as measured by the number of bidders. Indeed, we do observe an increase in the average number of bidders during the crisis of about 13%, from 6.9 to 7.8 bidders (see Table 7) in public procurement auctions. Moreover, in Gugler et al. (2015a), we estimate significant declines in markups for bidders in the crisis. More competition can lead to a lower labor response after an increase in output demand: firms in more competitive environments have to be more efficient. Competition moves market share toward more efficient producers, shrinking relatively high-cost firms, sometimes forcing their exit. In our auction context, an increase in competition leads to less shading of the bids implying that the more efficient firms win more often. One reason why they are more efficient is that they may be able to manage the additional project employing fewer additional employees than other firms. This would be consistent with a small but growing literature in industrial organization analyzing the effects of competition on productivity, see e.g., Buccirossi et al. (2013) and De Loecker (2011).

6.4. Stimulus multipliers

While we cannot estimate fiscal multipliers since we only observe direct (i.e., first round) effects of government procurement projects on employment, we can give a tentative assessment of the likely effectiveness of fiscal stimulus in the crisis in the short run. The employment effect of an exogenous increase in demand was essentially zero in the crisis period of our sample. The winner of the auction did not hire more nor fire fewer workers than the runner-up. This implies that first round effects of the fiscal stimulus measures were essentially absent in Austria during that time period and that the effects of fiscal stimulus were minor. However, we have to mention at least two caveats to this (negative) conclusion on multipliers. First, as stated above, labor hoarding essentially implies subsidizing wages by sacrificing short-run profits. Since the marginal propensity to consume is larger for employees than for employers, there may have been a positive multiplier in Austria. Second, the recession in the macroeconomy only lasted for four quarters and amounted to minus 3.8% overall. Growth – albeit muted – resumed in the second half of 2009. Arguably, while significant, this drop in economic activity did not appear to be sufficient to change firms' expectations on future demand fundamentally. Thus, firms engaged in labor hoarding, since they believed that the economic crisis would be only temporary. We do not know what would have happened to labor hoarding incentives if the economic crisis would have dragged on for longer than just for one year.

7. Summary and conclusions

We show that winning of a procurement contract increases the workforce of the winner by around 80 workers before the crisis or about 3% of its workforce. Winning firms, however, do not hire more workers after winning an auction but fire fewer workers than the runner-up thereafter. The effects of the crisis are strong. Winning firms do not increase their number of employees after winning an auction compared to runner-up firms during the crisis. Thus, while we estimate a significant reaction of employment to output before the crisis, we estimate a labor demand reaction with respect to output of essentially zero in the recent economic crisis. We do not disentangle significant wage effects of demand shocks, that is, wages appear to be sticky before as well as in the crisis. We view labor hoarding and productivity increases due to increased competition in the crisis as the most likely explanations.

The results of our approach have strengths, but also some limitations. Runners-up bidding for the same auctions as the winners and losing only by a small margin seem to form a good control group, according to our assessment of relevant variables. Thus, our regression discontinuity design appears to be valid. We can trace employment effects on a daily basis from before the auction to several months after the auction. Through daily information on employment, we gain an unusually precise measure for the impact of winning. Moreover, we can disentangle hires and fires. We are not aware of a study that documents evidence on labor demand, hoarding and the recent crisis in such detail. However, our approach allows only a tentative assessment of the likely economic explanations of the observed empirical patterns.

Appendix A. Variable definitions

Table A.1
Description of variables.

Category	Variable	Description
Employment	*Employees*	Number of employees in a firm
	Employees, blue	Number of blue collar workers in a firm
	Employees, white	Number of white collar workers in a firm
	Hires	Number of hires in a firm
	Hires, blue	Number of blue collar hires in a firm
	Hires, white	Number of white collar hires in a firm
	Fires	Number of fires in a firm
	Fires, blue	Number of blue collar fires in a firm
	Fires, white	Number of white collar fires in a firm
	Wage	Average wage of a worker in a firm
	Wage, blue	Average wage of a blue collar worker in a firm
	Wage, white	Average wage of a white collar worker in a firm
	Tenure	Average tenure of workers employed in a firm, based on individual tenure, which is the total number of days that a person is employed in the current employment. Individual tenure is truncated for all workers at 365 days to avoid truncation bias.
Projects	*Backlog*	Each time a firm wins an auction, backlog of this firm increases by the value of the project. Projects are assumed to be worked off linearly over their construction period, thereby releasing capacity. Each firm's backlog obtained is standardized by subtracting its mean and dividing by its standard deviation.
	Distance	Driving distance of a firm to a construction site
	Number of bidders	Number of bidders in the auction
	Orders flows	Gross inflow of new contracts, countrywide, per month (mill. 2006 Euro)
	Heavy construction	Dummy for auctions in heavy construction sector
	General contractor	Dummy for bidders serving as "general contractors"
	Open procedure	Dummy for auctions following the "open procedure"
Balance sheet	*Total assets*	Total liabilities plus shareholders equity and consist of current assets and non-current assets
	Current assets	Cash, cash equivalent, and others
	Non-current assets	Fixed assets and other
	Total liabilities	Current and non-current liabilities
Income statement	*Revenue*	Revenue of a firm
	EBITDA	Earnings before interest, tax, depreciation and amortization of a firm
	Profit after tax	Profit after taxes of a firm
	Depreciation	Depreciation of a firm
	Interest paid	Interest paid by a firm
	Costs of employees	Cost of employees
Financial ratios	*EBITDA/total assets (in %)*	EBITDA over total assets
	Profit after tax/total assets (in %)	Profit after tax over total assets
	Debt ratio (in %)	Total liabilities over total assets
	Non-current liabilities/total assets (in %)	Non-current liabilities over total assets

Appendix B. Additional estimation results

Table B.1
Effect of winning on blue and white collar workers by fortnight.

	Blue collar workers				White collar workers			
	Pre-crisis		Crisis		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
−8	3.4	−2.3	−12.0	3.7	0.5	0.8	0.1	0.3
−7	11.5	−6.7	−21.9	12.3	1.5**	1.5	0.6	−0.5
−6	20.9	−8.8	−33.8*	28.8	4.0**	1.0	0.5	−1.1
−5	31.6*	−7.8	−43.8*	45.0	6.7**	1.5	−0.9	−0.9
−4	42.7**	−2.7	−48.9	52.8	8.8**	4.6**	−1.7	−0.5
−3	50.8**	6.4	−43.9	48.8	10.7**	6.9**	−0.6	0.6
−2	55.4**	16.8	−33.2	39.1	12.1**	7.9**	1.6	0.0
−1	60.8**	29.1	−17.1	23.2	13.2**	9.0**	3.9*	−0.8
1	68.5**	43.3	4.2	11.9	14.6**	9.8**	5.2**	0.5
2	75.8**	56.6*	24.9	7.5	15.4**	10.8**	4.6**	2.6
3	83.2**	66.6**	41.6	10.5	16.9**	11.6**	5.1**	3.7
4	87.8**	69.9**	49.8*	20.3	18.4**	12.2**	6.1**	3.0
5	91.6**	67.3**	50.0*	32.9	19.9**	12.9**	5.6**	1.5
6	95.1**	61.7**	47.0	43.4	20.3**	13.9**	4.2*	0.9
7	93.7**	57.9**	42.3	55.8	20.0**	15.1**	2.8	1.0
8	86.3**	56.4**	39.0	63.1	19.6**	15.9**	2.8	−1.4

(*continued on next page*)

Table B.1 (*continued*)

	Blue collar workers				White collar workers			
	Pre-crisis		Crisis		Pre-crisis		Crisis	
	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner	τ	$\tau\times$ winner
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
9	74.1**	54.8*	38.5	71.1*	20.1**	16.3**	3.0	−2.5
Lagged employment	−0.07**		−0.07**		0.58**		−0.05**	
Winner	−29.7		−32.0		−6.0**		−1.5	
Backlog	33.1**		44.8**		16.0**		0.7*	
Constant	1644.3**		1537.9**		276.4**		724.3**	
Observations	54,234		22,104		54,234		22,104	
R-squared	0.963		0.956		0.997		0.999	
Firm FE	Yes		Yes		Yes		Yes	

Notes: Each numbered line stands for the fortnight relative to the event date, the coefficients measure the effect relative to fortnight −9. Columns (1)–(4) show results for blue collar workers. Column (1) is the fortnight estimate, column (2) the fortnight interacted with the dummy for the winner, using the pre-crisis sample. Columns (3) and (4) repeat the estimates for the crisis sample. Columns (5)–(8) repeat the estimates for white collar workers. Backlog after the event, i.e., $t > 0$, remains constant at the level of $t = -1$. Lagged employment is the value of the dependent variable 90 days before the event. All regressions include firm fixed effects, and standard errors are clustered at the case level. Significance levels: $^{**}p < 0.01$, $^{*}p < 0.05$.

Appendix C. Auxiliary estimation results

Table C.1
OLS-regression of money left on the table on the number of bidders.

Number of bidders	Coeff.	Std. err.
2	0.157**	(0.012)
3	0.104**	(0.007)
4	0.086**	(0.005)
5	0.082**	(0.005)
6	0.079**	(0.004)
7	0.066**	(0.005)
8	0.063**	(0.005)
9	0.066**	(0.006)
10	0.066**	(0.007)
11	0.055**	(0.008)
12	0.075**	(0.009)
13	0.046**	(0.012)
14	0.056**	(0.015)
15	0.027	(0.020)
16	0.080**	(0.029)
17	0.037	(0.036)
18	0.039	(0.033)
19	0.088	(0.062)
20	0.043	(0.087)
22	0.056	(0.062)
Observations		2589
R-squared		0.439

Notes: Results of an OLS-dummy-regression of money left on the table on dummy variables for the number of bidders in each auction. The regression does not include a constant, which makes the coefficients equal to the means of each group. Standard errors are given in parentheses. Significance levels: $^{**}p < 0.01$, $^{*}p < 0.05$.

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