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Nearest and Dearest? Local Firm Bias in Public Procurement in Switzerland

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Abstract

Preferential treatment of domestic firms is a pervasive phenomenon in international public procurement. At more regional levels, especially in low-corruption contexts, it remains poorly researched. I perform the first study of home bias in municipal procurement in Switzerland using a novel tender-level data set. I show that local agencies award contracts within their own municipality disproportionately often. A reform of regional procurement law did little to curb this behaviour, and may have even fostered it. This result is in line with previous work, which finds that policy interventions are rarely successful at tackling home bias. While I am unable to pinpoint the exact channel of impact, my study calls attention to the persistent lack of transparency in Swiss procurement and underlines the importance of rigorous guidelines.

Keywords: Home bias, local government, procurement, Switzerland

JEL Codes: D04, D73, H57, H76

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1 Introduction

“It is clear that we would like VWP to work with enterprises from Valais as often as possible. [...] Since four members of local umbrella organisations and four other exponents of the Valais industry are on the board of VWP, it is ensured that contracts are awarded to Valaisan companies if possible.”

Answer of the State Council of Valais to a postulate asking why Valais Tourism (VWP) awarded contracts to out-of-canton firms

“In particular, its objectives are: [...] Ensuring equal treatment of all bidders and impartial award procedures.”

Article 1 of the first Intercantonal Agreement on Public Procurement, adopted by Valais in 2003

Despite free market access having been enshrined in Swiss procurement law for over twenty years, the highest political body of the canton of Valais recently endorsed protectionist award practices in a publicly available statement. The Swiss militia system, in which political offices are often held part-time, and the small geographical scale lead to close ties between government agents and civil society: In Valais, a motorway intended to span its main valley only recently surpassed the halfway mark, after a construction period of over forty years and investments of two billion Swiss Francs. Countless contracts had been awarded above market price based on party ties or personal relationships (Pinto, 2013; Venetz, 2023). Likewise, in Graubünden, where a cartel of construction firms strategically coordinated tender submissions in several hundred cases with the help of government employees, a member of the cantonal parliament observed that agents of the civil construction authority saw themselves as patrons of the local industry rather than its overseers (Schmid, 2021).

Even though favouritism in regional procurement is a salient topic in the media and in public debate, little academic attention has been devoted to the topic, with Switzerland as a whole remaining a blind spot in the literature. Identifying partiality in public procurement poses significant challenges, with numerous benign reasons to award contracts to local firms coexisting with

protectionist motives. In this study, I therefore ask whether local firm bias, defined as excess preference for firms located within the same region as the awarding authority, can be identified in municipal procurement in Switzerland, and whether it responded to a recent policy change.

To this end, I gather descriptive evidence towards the manifestation of home bias in municipal procurement based on a novel tender-level data set. By exploiting a staggered reform of regional procurement law, I quantify the impact of complex legislative changes on the share of contracts which are awarded to local firms. Contrary to expectations, I find a null result which masks considerable heterogeneity across time and across reform waves, with significant increases in the local award rate for early adopters and in the service sector. Neither compositional effects nor changes in sustainability preferences offer compelling explanations. Although I cannot identify a single impact channel, with procurement agents, bidders, and local actors all being possible driving forces, this study emphasises the importance of transparent procurement processes. It adds to a growing literature on favouritism in low-corruption contexts and its response to policy instruments, and it opens up new avenues of research which could help disentangle the central puzzle of why the Swiss procurement reform seems to have been ineffective at curbing home bias.

Given the public interest in the topic, home bias is a flourishing research area in the economic literature, with much attention devoted to regionally biased allotment of government grants or investment in local infrastructure.³ In public procurement, too, home bias in the sense of preferential treatment for local firms is a well-documented phenomenon, especially at the national level. Trionfetti (2000) demonstrates a consistent preference for domestic firms in Europe, the US, Japan and South Korea, which can lead to significant distortions of industrial specialisation (Brühlhart & Trionfetti, 2001). In a 2011 EU study, nearly half of the surveyed firms thought that home bias influenced public procurement outcomes to a high degree, whereas only 14.5% did not consider it an issue (European Commission, 2011). Expunging home bias is no mean task, and policy reforms may even backfire. In an analysis of 43 international treaties aimed at liberalising public procurement, Rickard and Kono (2014) show that these agreements failed to combat home bias. At the national level, Fazekas and Cingolani (2017) find that reforms in European countries did not reduce

³Compare, for example, Hodler and Raschky (2014) for a systematic overview. Context-specific evaluations have been performed in Germany (Baskaran & Lopes da Fonseca, 2021), Norway (Fiva & Halse, 2016), Indonesia (Gonschorek, 2021), and Vietnam (Do, Nguyen & Tran, 2017), among others.

favouritism in public procurement; if anything, their results point towards an uptick in biased awards. Neither do government changes prove effective in weakening home bias: Power transitions in Mexico, the UK and Hungary brought little change in award practices (David-Barrett & Fazekas, 2020; Falcón-Cortés, Aldana & Larralde, 2022). Only the urgency created by the Covid-19 crisis diminished domestic firm preference somewhat, but levels quickly bounced back (Hanspach, 2023).

A more recent literature has demonstrated that favouritism is also present at subnational levels (García-Santana & Santamaría, 2023; Herz & Varela-Irimia, 2020), and that its consequences are likely not benign. An EU anti-corruption project uses the term *particularism*, that is, the allocation of public resources which unduly favours certain parties (Mungiu-Pippidi & Kukutschka, 2015). While not necessarily illegal, it results in inefficient outcomes which are often perceived as unfair. Dimitri, Dini and Piga (2006) argue that the strong relations between local procurement agencies and local suppliers can negatively impact procurement efficiency, for example in connection to lobbying and other rent-seeking behaviour. For Hungary and the Czech Republic, Fazekas and Wachs (2020, p. 163) find that “at the micro-level, [...] corruption in public procurement is predominantly about the exclusion of non-favoured suppliers”. In two studies of Italian municipal procurement, policy changes which induced worse procurement outcomes also led to a higher share of awards to local firms (Baltrunaite, Giorgiantonio, Mocetti & Orlando, 2021; Coviello & Gagliarducci, 2017).

Local firm bias at the municipal level has also been documented in low-corruption contexts. Most studies focus on Sweden, which, like Switzerland, is a mature democracy whose municipalities enjoy a relatively high degree of autonomy and considerable financial resources. Erlingsson, Bergh and Sjölin (2008) argue that particularism, especially in the municipal allocation of public funds, is highly understudied and may indeed be on the rise. Hansson and Holmgren (2011) document qualitatively how local interest groups pressured municipal officials to deviate from the law after a regime change in transport service procurement, leading to favourable treatment of local firms. Three recent empirical studies show that Swedish municipalities exploited a discretionary regime to unduly favour local publicly-owned cleaning firms (Hyytinen, Lundberg & Toivanen, 2018), that low political competition at the municipal level is associated with less competitive procurement processes which exhibit signs of manipulation (Broms, Dahlström & Fazekas, 2019), and that firms which win non-competitive municipal tenders are usually local (Wittberg & Fazekas, 2023).

Turning to Switzerland, the literature becomes more sparse: The only econometric evaluations of bias in public procurement were performed by Felder, who estimated the effects of protectionist award practices on price levels both prior to and following a wave of reforms in the late 20th century (Felder, 1994; Felder & Podgorski, 2009). He finds that non-competitive cantonal legislation is associated with construction sector prices that are elevated by 6.3%. This difference persists into the early 21st century despite sweeping reforms, although at a slightly lower level. A recent qualitative analysis of Swiss procurement practices finds that particularism is still an issue, citing language barriers, biased award criteria, and undisclosed scoring procedures as the main impediments to transparency (Mabillard & Zumofen, 2021).

Research on particularism in procurement has in the past been hampered by poor data availability, with data having been notoriously hard to access in the EU until 2015 (Duguay, Rauter & Samuels, 2023). Switzerland is an exception insofar as a publicly accessible e-procurement platform has been in continuous operation since 2010. However, Swiss tender-level data has so far only been exploited by one author, mainly in the context of sustainable IT procurement (Krancher & Stürmer, 2018a, 2018b; Stürmer, Krancher & Myrach, 2017). In their most recent work, Welz and Stürmer (2021) note that sustainability criteria often lack specificity, and that municipal agencies in particular perform worst in terms of sustainability know-how. I have not been able to identify other authors who have performed economic analyses using Swiss tender-level data, indicating that this data source remains underutilised in the literature. Furthermore, the results by Felder exclusively concern the construction sector at the cantonal level. To the best of my knowledge, this study is thus the first in-depth econometric analysis of municipal procurement in Switzerland, and of municipality-level home bias in particular. As such, I add to the literature which analyses favouritism in low-corruption contexts. I also expand the body of evidence on the difficulty of controlling home bias in public procurement through policy reforms. A final key contribution is the novel geocoded tender-level data set, in which contracted firms are linked to their commercial register entries. Thanks to several API interfaces and free-text parsing routines, it can be kept up to date and expanded, allowing for follow-up studies or analyses of other research questions.

In the remainder of this paper, I first summarise key features of the institutional context and the recent policy reform. In Section 3, I then present the hypotheses which form the basis of my analysis. Section 4 introduces the data set and offers a first set of descriptive results. In Section 5, I discuss the empirical

strategy behind the causal analysis presented in Section 6. Section 7 examines the validity of my approach, alternative mechanisms which could be driving my results, and avenues for further research. Section 8 concludes.

2 Institutional Context

Switzerland is organised at three administrative levels: At the nation-wide or federal level, the region-wide or cantonal level, and the local or municipal level. Each of the 26 cantons has its own constitution, parliament, and government, and as such enjoys sovereignty in all areas not explicitly delegated to the federal government. The 2,131 municipalities of Switzerland (as of 01.01.2024) are subject to cantonal law and are thus strictly speaking not sovereign, although roughly one fifth have their own parliament, while the remainder reach decisions and elect executives through direct democracy at communal assemblies (Federal Chancellery, 2024). All three administrative levels levy taxes on both individuals and legal entities, with roughly 50% of all tax revenue being raised at the federal level, 30% at the cantonal level, and 20% at the communal level (Federal Department of Finance, 2024).

In the following, I first discuss the nature and peculiarities of Swiss public procurement which are relevant to this study, especially at the municipal level. I then briefly describe the reform of cantonal procurement law which is exploited in the empirical part of this paper.

2.1 Municipal Procurement in Switzerland

Across all administrative levels, Swiss procurement agencies purchase goods and services amounting to roughly 42 billion Swiss Francs (about 43.5b Euro) per year, of which 20% is attributed to the federal government and 40% each to cantonal and municipal agencies (TRIAS, 2022). Federal, cantonal and municipal procurement agencies, third parties authorised to procure on behalf of a governmental agency, non-commercial entities which fulfill public functions (such as public transport, water or electricity distribution), and any object or service which is subsidised at a rate exceeding 50% are all subject to Swiss procurement law. Since each canton has its own legislation, cantonal and municipal procurement officers must act in accordance with local law (see e.g. State Council of Zurich (2023) for the canton of Zurich), whereas federal agencies are governed by a single nation-wide law, the Federal Act on Public Procurement (PPA). Procure-

ment procedures can be discretionary, in which a contract is awarded directly without a tendering process; selective, in which a contract is published, but the set of tenderers is restricted; or open, in which a tender notice is published and any interested firm may submit a tender offer. All legal texts on procurement furthermore differentiate between three contract types: Construction works, supplies of goods, and services, with different thresholds for award procedures. Table B2 in the Appendix provides further details.

Since 2010, all federal tender notices and awards from selective and open procedures are published on the e-procurement platform Simap.ch (Beschaffungskonferenz des Bundes, 2024). A majority of cantons concurrently adopted the website as their official publication medium (Felder & Podgorski, 2009), although some continue to also publish awards in local gazettes. Municipal agencies joined more gradually over time. Today, the number of procurement agencies registered on the website exceeds 3,000, and the published awards sum to a volume of 17.5b Swiss Francs (CHF), or about 18b Euro (KISSimap, 2024).

Turning to procurement at the municipal level, the main tasks of procurement agencies relate to the provision of education, welfare, transport, and utilities at a communal scale (Federal Chancellery, 2024). These areas often overlap significantly with cantonal and federal scopes of duty: A bus passenger in the canton of Zurich commuting from a peripheral region to the centre may well ride a bus line run by the communal *Stadtbuss Winterthur*, a bus service provided by the cantonal *Zürcher Verkehrsverbund*, and a bus belonging to the federal *Postauto* service, all on the same journey.

The comparatively low size of Swiss municipalities is an important feature. With an average population size of 4,100 and a median of 1,670 inhabitants (as of 01.01.2024), Switzerland is at the lower end of the spectrum in terms of municipality size in Western Europe and near the very bottom for all OECD countries (OECD, 2018a, 2018b). In an analysis of municipal autonomy in Switzerland, Fiechter (2010) notes that this low size often leads to a lack of professionalism and a concentration of administrative functions on relatively few officials, which in turn creates mutual dependencies, fosters conflicts of interest, and limits monitoring activities. This is exacerbated by the fact that state-employed procurement agents have the option to delegate procurement contracts to third parties, such as industry specialists, in which case conflicts of interest may be heightened while administrative supervision is reduced further.⁴

⁴Although comparing procurement outcomes between contracts issued directly by government agencies and those tendered by third parties would be a worthwhile pursuit both from an

At the same time, Fiechter also attests to greater efficiency in the provision of public goods and services and better knowledge of local needs at the municipal level.

2.2 Reforms to Swiss Procurement Law

Until the 1980s, cantonal procurement law was characterised by a strong sense of territorial protectionism; certain cantons discriminated heavily against firms which were not headquartered within their borders, or even excluded them from submitting an offer entirely (Felder & Finsinger, 1987). In the late 20th and early 21st century, a concerted effort at modernisation led to the adoption of World Trade Organisation guidelines for federal and cantonal procurement (WTO, 1994), a new nation-wide law which aimed to curb cantonal protectionism and foster more competitive procurement practices (BGBM, 1996), and the harmonisation of cantonal procurement law with the first Intercantonal Agreement on Public Procurement, which was drafted in 1994 and revised in 2001 (Felder & Podgorski, 2009). As of 2010, which marks the beginning of the study period in this paper, the philosophy underlying public procurement regulation in Switzerland was exemplified by the principles of free market access, competition, and price as the dominant award criterion (Lang & Steiner, 2018).

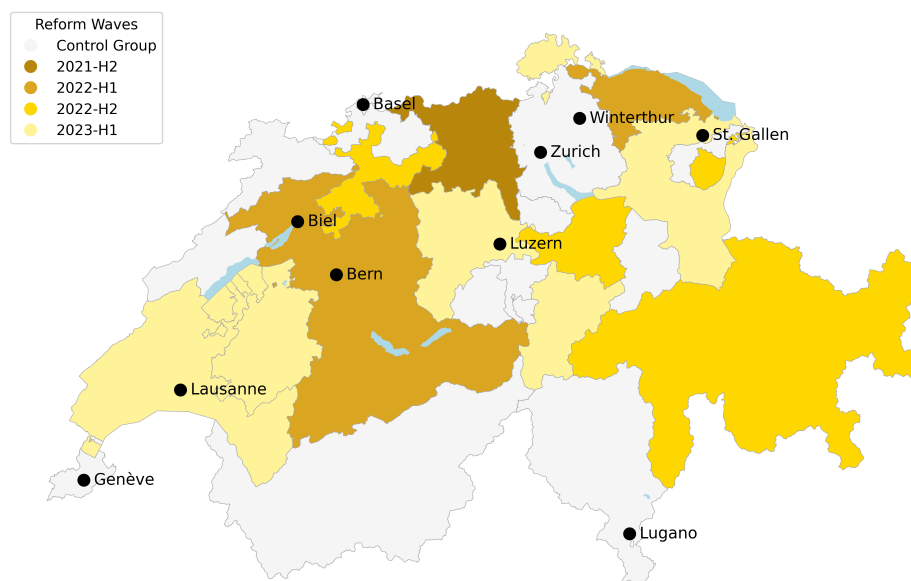
Within the period under observation, a paradigm shift occurred – first at the federal level, then gradually in cantonal law as well. Federal procurement regulation was reformed in 2019 and entered into force on 01 January 2021. Federal and cantonal procurement specialists formulated the revised Intercantonal Agreement on Public Procurement (henceforth abbreviated as *2019 IAPP* or simply *IAPP*), which cantons could choose to adopt into their own legislation. Figure 1 shows the status of each canton as of the end of the study period (31.12.2023). Steiner and Klingler (2023) provide a summary of the reform in a historical context. Two aspects are especially relevant to this paper:

- (i) Where previously, the overarching goal of public procurement had been to “support the economical use⁵ of public funds” (Art. 1(c), PPA, 1996),

academic and a policy standpoint, the structure of my data set does not allow me to differentiate between them. While Simap forms contain separate fields for the demanding authority and the procuring entity, agents unfortunately do not distinguish between the two in practice. As such, the term *municipal tenders* henceforth refers to all contracts issued by municipal authorities, whether directly or via a third party.

⁵Translation by the author. The German, French and Italian wording specify *den wirtschaftlichen Einsatz*, *l’utilisation économique*, and *l’impiego economico*, respectively, all of which refer to economical in the sense of efficient or profitable.

Figure 1: IAPP Reform Waves



Notes: This figure visualises the reform status and length of exposure for each canton as of 31.12.2023. The black dots show the ten largest Swiss cities. Darker cantons adopted the reform at earlier points in time. The term *reform wave* refers to the semester in which the IAPP reform entered into force in a given canton. Appendix Table B1 provides exact reform dates.

the revised law instead specifies the “cost-efficient use of public funds in a manner that is economically, ecologically and socially sustainable” as well as “transparency”, “non-discrimination of tenderers”, and “promotion of effective, fair competition [...] by means of measures against unlawful agreements [...] and corruption” (Art. 2(a)-(d), PPA, 2021). Accordingly, the revised PPA and IAPP allow for a more liberal use of award criteria, especially relating to sustainability. What is more, a contract should not necessarily be awarded to the most economically advantageous tender, but rather to the most advantageous tender, which Steiner and Klingler (2023) argues should foster competition in quality as well as in price.

- (ii) The reform introduces much more stringent regulation on transparency. Tender notices and awards from open and selective procedures must necessarily be published online. Measures against corruption are introduced both for cases which are covered under criminal law as well as for more diffuse forms, such as favouritism, nepotism, and other conflicts of interest.

The IAPP reform represents a relatively complex change to procurement legislation, and it is not immediately clear whether and how local firm bias in

municipal procurement could respond. The next section therefore summarises the main hypotheses concerning the manifestation and evolution of home bias in public procurement in Switzerland.

3 Hypotheses

Home bias can arise in different scopes depending on the level of government, as García-Santana and Santamaría (2023) document: While national agencies prefer domestic to foreign firms, regional authorities favour companies from their own region over other domestic firms. Conversely, a national agent should not differentiate between domestic firms on the basis of their domicile within the country. As such, I posit that municipal procurement officers exhibit a preference for contractors from their own municipality which exceeds the factors cantonal and federal agents take into account, such as distance to the execution site and knowledge of the local context. The literature offers numerous justifications for such a preference, both of general nature and specific to the Swiss context.

In a discussion of regional protectionism in the Swiss canton of Basel-Stadt, Pérez (2004) cites protecting local jobs and thus local tax substrate, diminishing the threat of bankruptcy for local firms, and guaranteeing the local *Arbeitsfrieden* (industrial peace) as the main motivations; principles which can easily be extended to the municipal level. An additional cultural dimension stems from the fundamentally Swiss idea of *Kantönligeist* (cantonal spirit), which Bär (2018, p. 260) describes as “the various differences [...] and resulting tensions between the Swiss cantons” as well as “the strong affection and exaggerated devotion to one’s native town”. Coupled with the militia system implying that procurement officials are often embedded in the local economy and thus potentially dealing not with business partners, but personal acquaintances and friends (Lang & Steiner, 2018), high scrutiny and social control on part of the local constituents (Fiechter, 2010), and the strong emphasis placed on local autonomy even in the broad population (Ladner & Bühlmann, 2007), regional protectionism is likely to be present even more strongly at the municipal than at the cantonal level, and to have survived the reform wave of the early 21st century. As such, in addition to universal reasons for preferring local firms in award procedures, municipal procurement officers may experience stronger incentives to award locally than agents at other administrative levels. The first hypothesis postulated in this paper encapsulates this idea in economic terms:

Hypothesis 1 *Municipal procurement officers have a stronger preference for awarding tenders to local firms than cantonal or federal procurement officers.*

Assuming that procurement officers have such a preference and that it is at odds with the principles of non-discrimination laid out in the relevant regulation, it follows that the likelihood of observing an elevated share of local awards is higher in areas in which officers enjoy more discretion – both in the sense of their freedom of action as well as in terms of publicity. The former has been shown to correlate with favouritism in several studies in the European context (Baltrunaite et al., 2021; Celis Galvez, Titl & Schotanus, 2023; Szucs, 2023). Concerning media attention, Flynn and Harris (2022) show that UK press coverage of public procurement is overwhelmingly negative in tone, with a bias towards the theme of governance failure, while Duguay et al. (2023) find that easier access to EU procurement data is associated with more competitive outcomes. In Switzerland, the foremost candidate which offers high discretion are discretionary awards. Unfortunately, these awards do not enter my data set since they do not have the same publication requirements as selective and open procedures; however, even the latter are likely to be less monitored the lower the contracted sum. Similarly, with construction contracts having received significant media attention after the scandals cited in the introduction, and moreover being a particularly visible allocation of public funds, it stands to reason that a potential home bias would be made more plain in other contract types.

Hypothesis 1a *This preference manifests more readily in low-price contracts and in contract types which are subject to less public scrutiny.*

Given that the regulation of the early 2000s focused on combating territorial protectionism by promoting price as the main – or even sole – award criterion, it is further possible that agencies wishing to award locally do so by citing different criteria, for instance quality, experience, or professional references. In a set of recommendations to the EU, Saussier and Tirole (2015) explicitly warn against entrusting procurement agencies with social or environmental goals, arguing that it increases “the (ever-present) risk of favouritism”.

Hypothesis 1b *This preference becomes more apparent the lower the weight on price as an award criterion.*

The revised IAPP introduces a variety of new publication requirements and anti-corruption legislation. Steiner and Klingler (2023) hail the new framework

as a paradigm shift, provided that it is implemented and enforced properly. As such, excess preference for local firms should be curtailed in cantons where the reform is adopted. As a consequence, the share of local firm awards should fall, especially among the contract types discussed in Hypothesis 1a.

Hypothesis 2 *A canton joining the Intercantonal Agreement on Public Procurement reduces the share of contracts awarded to local firms at the municipal level.*

Since the reform includes more stringent reporting requirements for municipalities, tender awards from previously unobserved procurement agencies should enter the sample after a reform has been implemented. If these municipalities are represented by particularly traditionally-minded procurement officers, and their previous reluctance to report online was thus coupled with a higher preference for local firms, the resulting composition effects may distort the effect postulated in the previous hypothesis.

Hypothesis 2a *Composition effects in which previously unobserved municipalities join the data set post-reform may introduce upward bias on impact estimates.*

Finally, upward pressure on the share of local firm awards may also stem from the reform itself: As feared by Saussier and Tirole (2015), the new focus on sustainability and quality over price could widen the channel described in Hypothesis 1b, or even increase the preference for local firms itself.

Hypothesis 2b *Changes in criterion weighting may increase the share of local firm awards.*

To summarise, I expect to find home bias among municipal procurement officers, which should manifest in particular in low-price contracts, in sectors subject to low public scrutiny, and through contracts which are not (solely) awarded based on price. I predict that the IAPP reform will reduce this bias once it comes into effect, although this may be counteracted by compositional effects and changes in sustainability preferences.

4 Data

In the following, I briefly summarise the structure of my data set, provide summary statistics and graphics, and discuss the limitations inherent to my sources. I then present descriptive evidence for Hypothesis 1 as well as its two subhypotheses. A detailed explanation of the compilation process, including a comprehensive list of data sources, is provided in Appendix A.1.

4.1 Key Variables and Outcomes

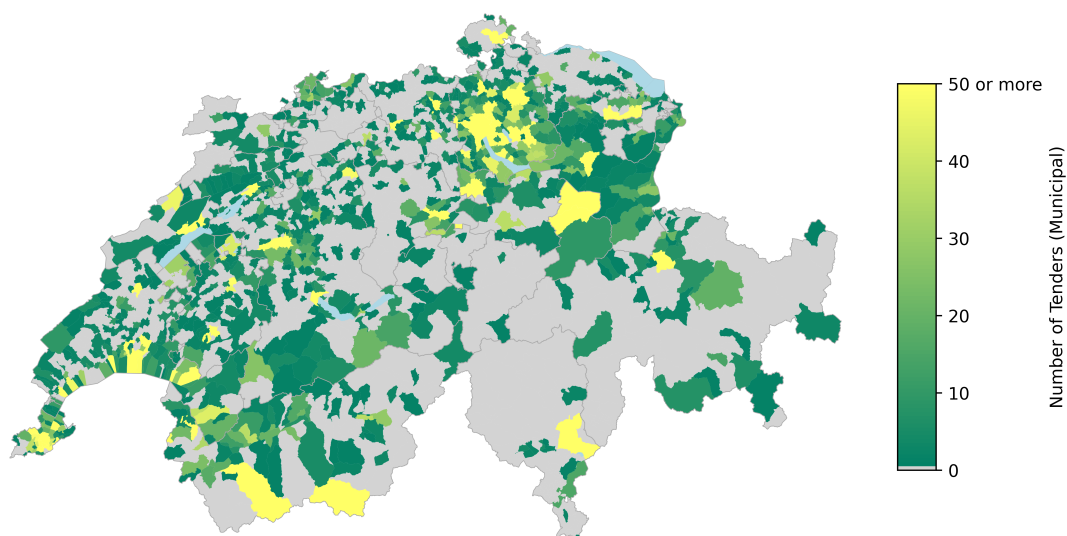
In order to analyse local firm bias in Swiss municipal procurement, I construct a novel dataset comprising 23,678 municipal, 22,995 cantonal and 5,072 federal tender awards from 2010 to 2023. The source data represents the complete set of electronically published public tenders in Switzerland and corresponds to roughly 25% of the estimated total governmental tender volume (TRIAS, 2022). It is retrieved from Simap (2024a, 2024b) and combined with detailed geographical and firm-level data from various governmental sources. Observations are at the firm-by-tender level, meaning, for example, that tenders subdivided into lots are counted separately if the lots are awarded to different firms. For each award, I observe the name and address of both the contracted firm and the awarding agency, the price or price range offered by the contractor, the contract type (construction, supply or service), the location of the execution site, and the relevant Common Procurement Vocabulary (CPV) codes. Further covariates are only available for a subset of all observations: These include the criteria underlying the award decision as well as firm characteristics such as size or capital. Since they may be missing non-randomly, they do not enter the main analyses.

The outcome variable describes whether a contracted firm is *local*, that is, whether its domicile is located within the awarding municipality. To provide a basis for comparison, I construct a similar variable for cantonal and federal tenders, in which a contract is classified as locally awarded if the execution site and the firm are located in the same municipality. Since execution sites are not available as addresses, but must instead be parsed from free text entry fields, comparisons across administrative levels are largely illustrative.

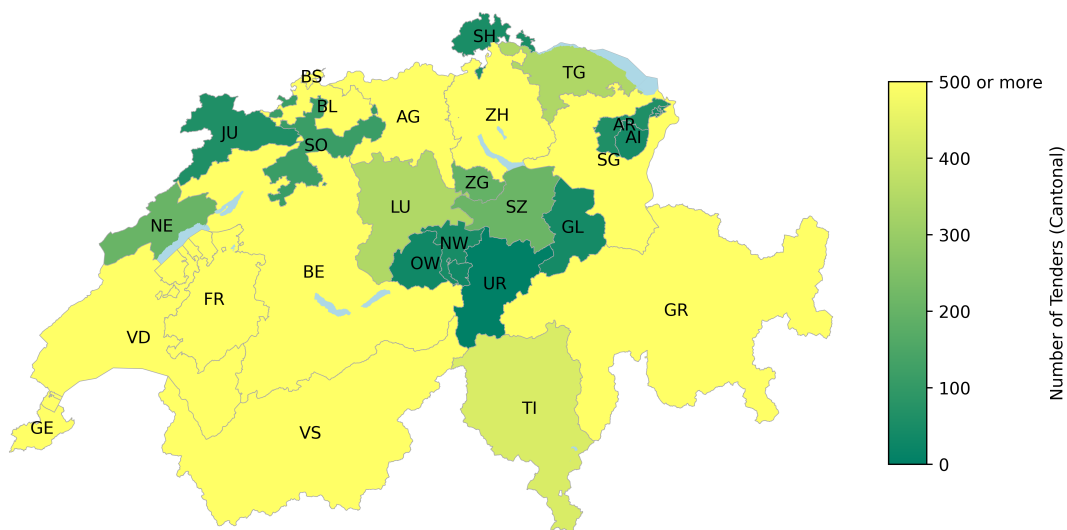
As discussed in Section 2, municipalities had no obligation to publish tender awards online for most of the period under observation. Panel A of Figure 2 shows that as a result, some municipalities do not enter the data set at all. However, with many of the unobserved municipalities being located in the mountainous south-eastern part of Switzerland, the observed regions cover 80% of the Swiss population and each of the 26 cantons. In terms of the number of contracts published during the observation period, large cities such as Zurich, Bern and Geneva dominate the sample; however, medium-size municipalities with 10,000 to 20,000 inhabitants also contribute up to 100 tenders each. Small municipalities with up to 5,000 inhabitants are by and large located at the bottom of the distribution, with observation numbers in the single to low double digits.

Figure 2: Geographical Distribution of Cantonal and Municipal Tenders

Panel A: Municipal Tenders



Panel B: Cantonal Tenders



Notes: This figure visualises the number of tenders published by each municipality or canton over the entire period under observation (01.01.2010–31.12.2023). Regions marked in grey have not published any tenders. Note that the colour palette in Panel B is scaled by a factor of ten compared to Panel A.

At the cantonal level, all cantons are represented in the sample, as shown in Panel B. The cantons of central, north-western and north-eastern Switzerland have the fewest observations. This is due in part to their smaller size, but also to the fact that certain cantons, such as Obwalden (OW) or the two Appenzells (AI, AR), still publish smaller tenders in local publications.

4.2 Descriptive Results

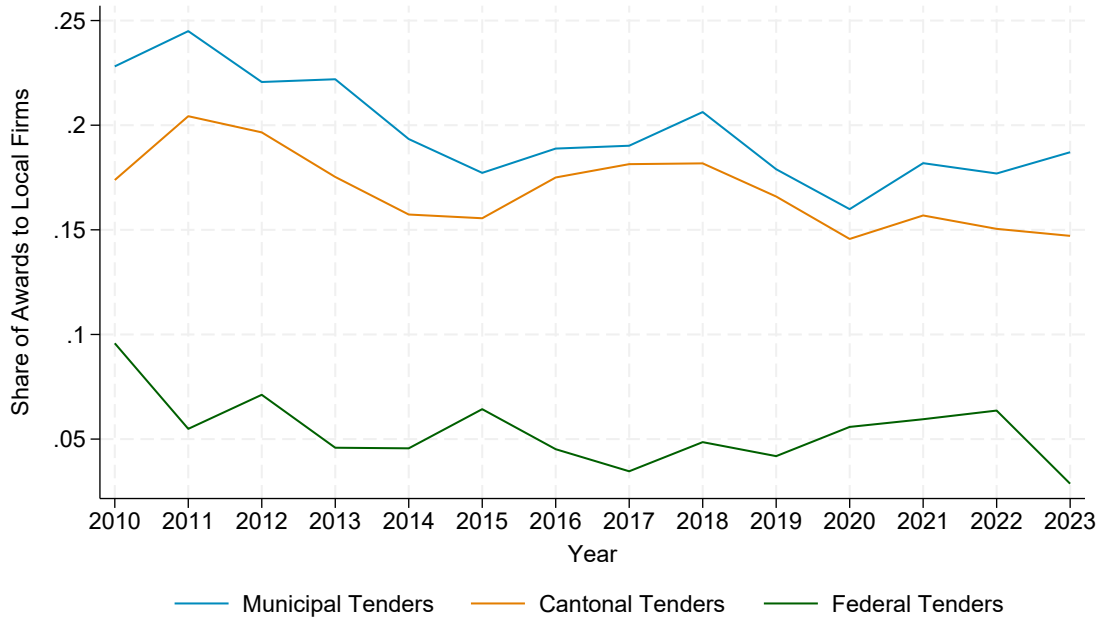
If municipal procurement officers have a higher preference for local firms than agents at other administrative levels, then they should – all else held equal – have a greater likelihood to award a contract within their own municipality. A simple comparison of means can serve as illustrative evidence, provided that it does not mask differences in underlying tender characteristics. As shown in Appendix Figure C1, municipal and cantonal tenders have an extremely similar distribution, save for a spike at the very low end of the spectrum for municipal tenders. Federal tenders cover the same range of prices, but their distribution has a thicker right tail, meaning that they are on average more costly. In terms of sectors, Appendix Table B3 shows that again, cantonal and municipal tenders are almost identically distributed across sectors; although the differences are statistically significant, their magnitudes range between zero and four percentage points, which should not represent a concerning disparity. Federal contracts, on the other hand, differ heavily in two sectors: There are much fewer construction tenders, but significantly more unclassified service contracts. As such, cantonal tenders will serve as the main basis of comparison, with federal tenders largely present for illustration.

Panel A of Figure 3 presents the raw percentage of locally awarded contracts over time and across the three administrative levels. There is a general downwards trend, with about 5 p.p. fewer contracts being awarded locally in 2023 than in 2010 across all administrative levels. Municipalities consistently exhibit the highest share of local awards, exceeding the federal level by about 12 p.p. and the cantonal level by two to five p.p.. Panel B reveals significant heterogeneity across sectors: While the mean and trend are almost identical for cantons and municipalities when it comes to construction tenders, municipalities have a somewhat higher propensity to award supply contracts locally, and a significantly higher likelihood in the service sector.

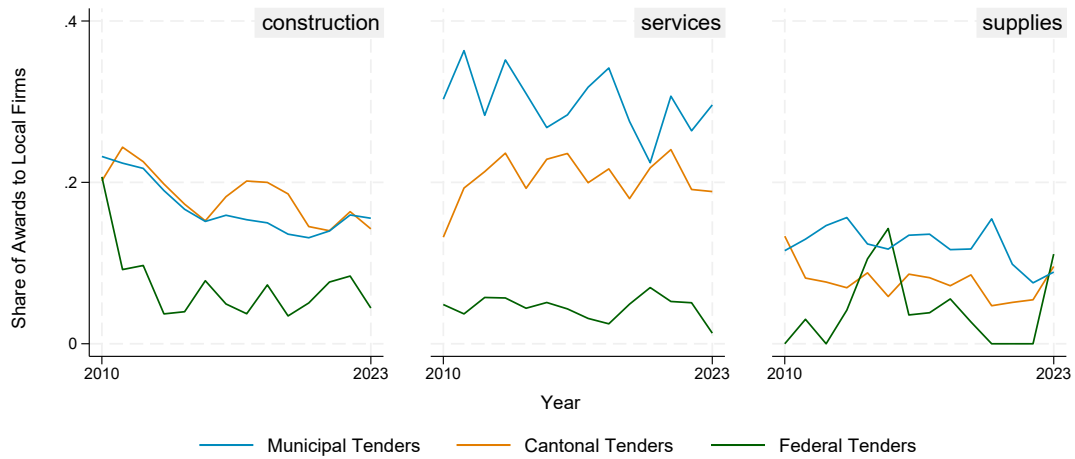
Figure 4 further disentangles the distribution of local firm awards at the municipal level across price categories and industrial sectors. For each price-by-

Figure 3: Local Firm Awards Across Administrative Levels

Panel A: All Tenders



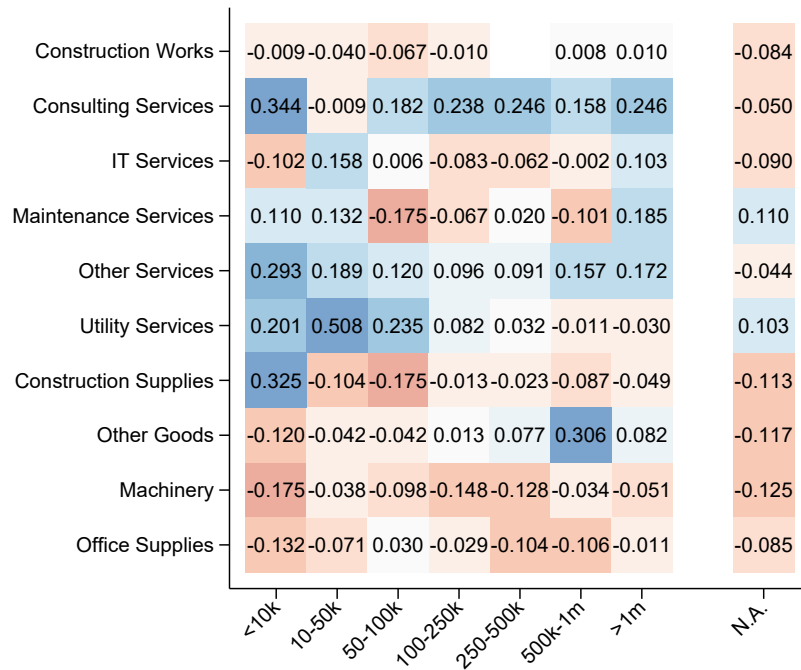
Panel B: By Contract Type



Notes: This figure plots the share of tenders awarded to local firms over the entire period under observation. Panel A shows the share by administrative level, while Panel B further distinguishes between contract types as defined in Swiss procurement law (compare Section 2.1). Note that a firm is classified as local if it is located in the same municipality as the awarding agency in the case of municipal contracts, whereas for cantonal or federal contracts, it must be located in the same municipality as the execution site.

sector cell, the value and colour show how frequently (in percentage points) a contract is awarded to a local firm, relative to the base category. As opposed to Figure 3, Panel B, which was based on the contract types defined in Swiss law, the more granular sectors shown in this figure are based on Common Procurement Vocabulary (CPV) codes, a nomenclature established by the European Union to facilitate and standardise project description in public procurement. The base category is given by medium-sized construction work contracts, which represent the most frequent category overall, and in which 17.5% of awards go to local firms. In order to avoid confounding effects of the IAPP reform, the sample is restricted to contracts which were awarded before the end of 2020, that is, at least six months before the first canton adopted the new IAPP.

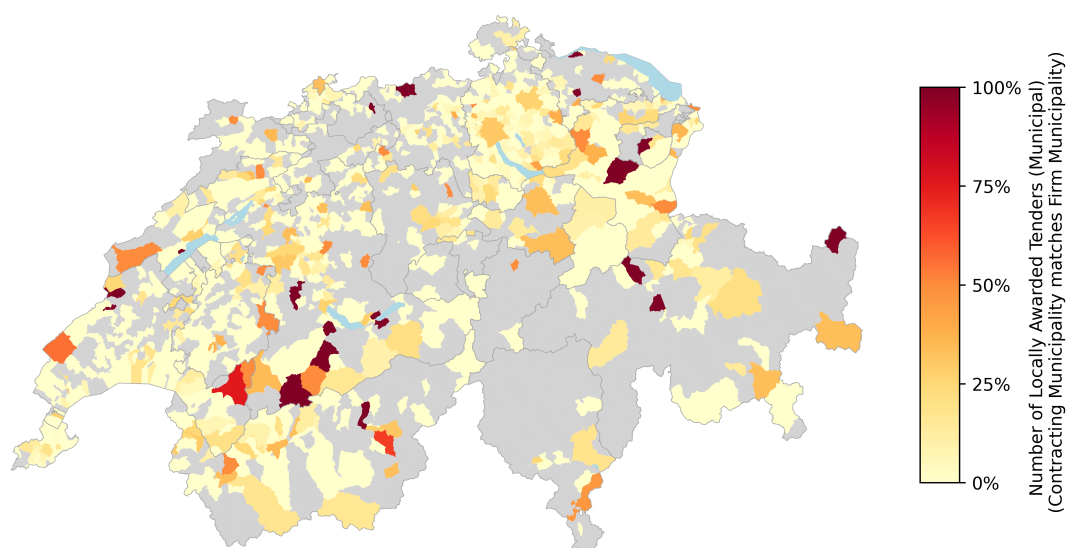
Figure 4: Price-by-Sector Disaggregation for Locally Awarded Municipal Firm Contracts (Pre-Reform Only)



Notes: This matrix shows the coefficients of a regression of the form $local_{ps} = \alpha + P_p \times S_s + \varepsilon_{ps}$, where $local$ takes a value of 1 if a municipal contract in price category p and sector s was awarded locally and 0 otherwise, P is a dummy for the price category and S for the sector, with the levels specified as in the matrix, and ε describes the error term. The base category is a construction contract in the 250,000–500,000 CHF category, which represents the mode and average in terms of sector and price. To avoid confounding effects of the IAPP reform, only contracts up to 31 December 2020 enter the sample. Each cell describes how much more or less likely (in percentage points relative to the base category) a contract of this type is to be awarded locally. Blue represents higher likelihoods and red lower likelihoods, with the colour intensity indicating the magnitude of the coefficient. Note that sectors are not exclusive, meaning that a single contract may have an arbitrary number of sectors.

Two notable patterns emerge: First, low-price contracts are more likely to be awarded locally than contracts in higher price categories. Secondly, consulting and other services (mainly architectural and planning services, financial and administrative services, and a smaller number relating to various social services) are almost universally more likely to be awarded to local firms. Other service types along with physical supplies show a more mixed picture: Utility and maintenance services are awarded locally more often if they are the lower end of the price spectrum or if no price is given, whereas unclassified goods (labelled *Other Goods*) seem to be of local origin at high total sums. Construction works do not appear to be sensitive to price, and neither do machinery and office supply deliveries, although the latter are awarded locally much more rarely than construction contracts.

Figure 5: Share of Local Firm Awards by Municipality



Notes: This map shows the share of municipal contracts which were awarded to local firms for each Swiss municipality as of 2023. If a municipality does not exist anymore at this point in time, for example because it fused with a neighbouring municipality, its tender awards are aggregated into the resulting municipality.

Plotting the share of local firm awards geographically reveals no immediate distinctions by area, although some careful qualitative statements can be ventured. As shown in Figure 5, and bearing the number of observations from Figure 2 in mind, many of the extremely rural municipalities which reported only a handful of contracts each have a local award rate of 0%; similarly, most of the municipalities which are dark red, implying a share of 100%, only con-

tributed one or two contracts in total. The more well-represented municipalities allow for more informative statements. Out of the major cities, most appear to award locally at a rate of 25–30%, somewhat above the 2010–2020 average of about 20%. Among the more rural municipalities, the midlands generally exhibit lower local award rates, whereas the mountainous regions of the Alps and the Jura mountains record higher shares.

Analysing tender-level characteristics reveals interesting disparities, although it is important to note that data points are missing in a non-random fashion, with firm statistics in particular being skewed towards larger firms.⁶ Bearing this in mind, Panel A and B of Table 1 below show that although there are statistically significant differences in pre-award characteristics, the magnitudes are likely not economically impactful: Tenders which are awarded to local firms are of similar size, are about 5 p.p. less likely to be awarded through an open procedure, receive 0.9 additional offers from competing firms, and are awarded slightly less often on the basis of non-price criteria, but a little more often citing sustainability criteria. It therefore seems unlikely that these characteristics reliably indicate whether a tender is awarded locally. Panel C shows that contract winners which are local firms appear to be somewhat smaller than their non-local counterparts, as measured by the number of employees. There is no significant difference in turnover or capital, but this could be owed to the fact that these statistics are only available for 83% and 89% of firms which have a commercial register entry, respectively, with the missing observations generally representing the smallest firms. Finally, there are also different patterns concerning the legal form of the contracted firm, with limited companies (LCs) and limited liability companies (LLCs) being underrepresented among local firms. In turn, although the percentage point increase is modest at 1.5 p.p., a local firm is about 63% more likely to be a sole proprietorship or a partnership. Other legal forms such as cooperatives and associations are also slightly more common in local awards.

⁶Firm characteristics are based on commercial register data, which necessitates matching firm names to their registry entries. This process automatically excludes foreign firms if they do not have a registered Swiss branch. While a company's legal form is always available, firm size indicators are more likely to be recorded for larger firms. They furthermore represent the latest-available statistic rather than a figure dating to the year of the award, and in the case of turnover and the number of employees, they may be based on a range (e.g. '1–5 employees') for data protection reasons. Award criteria are parsed from free text entry fields using a string-matching algorithm, with the criteria themselves determined by a manual survey of a random subsample. I find classifiable criteria for slightly more than half of the contracts which enter the analysis; manual checks reveal that the remainder either does not list criteria at all or refers the reader to an external document. Out of the contracts which have criteria, 86% have retrievable weights attached to each criterion, with the remainder providing either no weighting scheme or unparsable free text (such as point-based weighting systems).

Table 1: Characteristics of Local Firm Awards at the Municipal Level

	(1) Non-Local Firm	(2) Local Firm	(3) Difference	(4) N
<i>Panel A: Tender Characteristics</i>				
Contracted Sum (mCHF)	1.287 [5.853]	2.080 [31.030]	0.793 (0.476)	21,561
Open Procedure	0.886 [0.318]	0.839 [0.368]	-0.047** (0.006)	23,678
Number of Offers	5.774 [5.764]	6.649 [6.420]	0.875** (0.123)	18,161
<i>Panel B: Award Criteria</i>				
Any Criteria	0.556 [0.497]	0.519 [0.500]	-0.037** (0.008)	23,678
Any Non-Price Criteria [†]	0.505 [0.500]	0.463 [0.499]	-0.042** (0.008)	23,678
Sustainability Criteria	0.053 [0.224]	0.070 [0.256]	0.017** (0.004)	23,678
Weight on Non-Price Criteria (%) [‡]	55.842 [32.152]	53.078 [31.737]	-2.764** (0.802)	11,027
Weight on Sust. Criteria (%) [‡]	16.398 [12.668]	13.738 [9.188]	-2.661** (0.757)	961
<i>Panel C: Firm Characteristics</i>				
Capital (mCHF, indicative)	5.445 [46.195]	6.495 [66.162]	1.050 (1.178)	18,402
Turnover (mCHF, indicative)	401.289 [3,759.108]	428.579 [2,673.130]	27.290 (56.305)	17,158
Employees (indicative)	679.865 [7,492.849]	507.815 [2,732.197]	-172.050* (74.807)	19,288
Sole Prop. or Partnership	0.024 [0.153]	0.039 [0.192]	0.015** (0.003)	20,740
LC or LLC	0.895 [0.307]	0.860 [0.347]	-0.035** (0.006)	20,740
Other Legal Form	0.081 [0.273]	0.102 [0.302]	0.020** (0.005)	20,740
Observations	19,148	4,530	23,678	

Notes: This table shows the incidence of selected tender characteristics, award criteria and firm characteristics for all municipal awards in the sample. Column (1) shows the mean incidence if a tender was awarded to a non-local firm, while Column (2) shows the same for contracts which went to local firms. In Panel A, the contracted sum refers to the exact price or midpoint of the price range given in the award notice. The first three rows of Panel B refer to the fraction of the sample for which keywords matching the given criteria could be extracted from the data. [†]Non-price criteria refer to quality, sustainability, time to completion, professional references, experience, and whether the contracted firm trains apprentices. [‡]Weights are only available for 86% of the contracts which have any criteria, or 47% of the total sample; for the remainder, weights were either not provided or impossible to parse. In Panel C, the first three rows provide measures of firm size. Since the commercial register only records the latest entry, these are purely indicative. They are furthermore more likely to be available for larger firms, and in the case of turnover and the number of employees possibly based on ranges. The final three rows summarise the share of different legal forms: Sole proprietorships and partnerships, limited and limited liability companies, and other legal forms. Standard deviations are given in square brackets, robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

5 Empirical Strategy

Hypothesis 2 and its two subhypotheses allow for a more causal analysis. In the following, I introduce the difference-in-differences framework proposed by Callaway and Sant’Anna (2021) and discuss its applicability to my research question.

The structure of the IAPP reform, which cantons adopted at different points in time or not at all, makes it suitable for a staggered difference-in-differences specification with two-way fixed effects at the canton and period level. Due to the issues discussed in Hypothesis 1a, controls for price category and industrial sector appear sensible, as do covariates for structural factors which influence the possibility to award contracts locally, such as relative remoteness and the number of workplaces in a given municipality. As such, the canonical two-way fixed effect model following Baker, Larcker and Wang (2022) takes the following form:

$$local_{ictps} = \alpha_c + \lambda_t + \delta D_{ct} + P_p + S_s + \beta X_i + \varepsilon_{ictps}, \quad (1)$$

where *local* describes whether a municipal contract *i* in canton *c* and period *t* was awarded to a local firm. α_c and λ_t represent canton and period fixed effects. D_{ct} takes a value of 1 if the IAPP reform has entered into force in canton *c* at time *t*, and 0 otherwise. P_p and S_s stand for price and sector fixed effects. X_i describes a vector of three tender-specific covariates: The quarter of the year in which the tender was awarded, which should pick up seasonal variation, the number of workplaces in the awarding municipality, and its relative remoteness as measured by travel time to the nearest major city. Since a high share of local firm awards could influence the number of firms in a municipality, letting the number of workplaces vary over time may lead to a bad control problem; I therefore choose to use the number of workplaces in 2019, which is the last year prior to the period under consideration. Finally, the parameter of interest is δ , which should, if all necessary assumptions hold, quantify the average impact of the reform on the share of locally awarded contracts.

Since the effects of a reform are likely to appear gradually, especially considering the timespan of several months which may elapse between the publication of a tender notice and the final award decision, I also consider a dynamic extension of the static model:

$$local_{ictps} = \alpha_c + \lambda_t + \sum_{\tau=-T}^{-2} \beta_{\tau} D_{ct}^{\tau} + \sum_{\tau=0}^T \delta_{\tau} D_{ct}^{\tau} + P_p + S_s + \beta X_i + \varepsilon_{ictps}, \quad (2)$$

in which D_{ct}^{τ} takes a value of 1 if canton c at time t is τ periods away from the reform coming into force, and 0 otherwise. The β_{τ} coefficients then describe anticipatory effects, which, under standard difference-in-differences assumptions, should be zero. δ_{τ} represent the new coefficients of interest. Given that all relevant assumptions hold, they quantify the impact of the reform after τ periods of exposure, up to a maximum of T .

A significant body of literature has begun to point out flaws in this traditional model. Goodman-Bacon (2021) shows that the resulting estimate is biased under heterogeneous or time-varying treatment effects. Rather than identifying a policy-relevant parameter, δ represents a weighted average of all possible two-by-two difference-in-differences designs in which one group changes treatment status. This includes so-called forbidden comparisons, in which later-treated observations are compared to earlier-treated observations. The weights placed on these individual comparisons can even be negative, leading to estimates which have an opposing sign to the true effect (De Chaisemartin & D'Haultfoeuille, 2022).

Roth, Sant'Anna, Bilinski and Poe (2023) describe several alternative estimation methods, of which Callaway and Sant'Anna (2021) is the most applicable to my setting. Their implementation estimates the overall treatment parameter δ and the time-dependent impact parameters δ_{τ} as aggregates of all clean two-by-two designs, that is, of all comparisons of treated against never-treated units and of treated against not-yet-treated units. In addition, it allows for the decomposition into group-specific treatment effects, which gives insight into treatment heterogeneity. The dynamic TWFE specification outlined in Equation (2) is thus adapted to

$$local_{irtps} = \alpha_r + \lambda_t + \sum_{\tau=-T}^{-2} \beta_{\tau}^r D_{rt}^{\tau} + \sum_{\tau=0}^T \delta_{\tau}^r D_{rt}^{\tau} + P_p + S_s + \beta X_i + \varepsilon_{irtps}, \quad (3)$$

which is estimated separately for each reform wave r in the set of all reform waves $\mathcal{R}$, with fixed effects α_r now at the reform wave level, and cantonal fixed effects omitted due to perfect collinearity. Observations from already-treated

reform groups are excluded from the sample. Time t is measured in semesters.⁷

Letting the index r correspond to the time period in which the given reform wave occurred, and letting ρ denote the time period in which unit i is first treated, a measure for the average effect of being exposed to the reform for τ periods is then given by

$$\delta_{\tau}^{agg} = \sum_{r \in \mathcal{R}} \mathbb{1}\{r + \tau \leq T\} P(\rho = r \mid \rho + \tau \leq T) \delta_{\tau}^r, \quad (4)$$

where δ_{τ}^r refers to the ATT estimate from Equation (4). The indicator function $\mathbb{1}\{r + \tau \leq T\}$ ensures that only periods within the relevant time horizon T are taken into account. $P(\rho = r \mid \rho + \tau \leq T)$ describes the weight placed on each δ_{τ}^r , which corresponds to the size of group r relative to all observations for which period $\rho + \tau$ is still within the time horizon.

Instead of aggregating across reform waves, I also consider measures for the average effect of being exposed to the treatment across time for each wave, which is described by

$$\delta_{agg}^r = \frac{1}{T - r + 1} \sum_{\tau=r}^T \delta_{\tau}^r, \quad (5)$$

thus giving the average of the post-treatment coefficients δ_{τ}^r across all periods from r to T . Finally, I provide an aggregate estimate across time and reform waves, which is specified as

$$\delta_{agg} = \sum_{r \in \mathcal{R}} \delta_{agg}^r P(\rho = r \mid \rho \leq T), \quad (6)$$

where δ_{agg}^r refers to the reform wave ATTs described by Equation (5), and $P(\rho = r \mid \rho \leq T)$ weights the coefficient of each reform wave by its share among all observations. This aggregation method places equal weight on equally-sized groups, whereas averaging over all group-time ATTs would lead to a larger weight being placed on earlier reform waves. Equations (6) and (4) therefore provide estimates for the coefficients of interest δ and δ_{τ} from Equations (1) and (2), respectively, while avoiding forbidden comparisons.

A causal interpretation of these estimates is possible under standard difference-

⁷This allows me to avoid lumping too many cantons together into the same reform wave, but also leaves a sufficient number of observations per group-period cell. I have also performed the main analyses shown in later chapters at the quarterly and yearly level. The results are qualitatively similar, although the coefficients from event study specifications are more erratic at the quarterly level owing to low observation numbers.

in-differences assumptions. Based on the notation in Sant’Anna and Zhao (2020) and Callaway and Sant’Anna (2021), I let R_r and C denote binary variables which take a value of 1 if a unit is first treated in period r or if a unit is never treated, respectively. I define $D_{i,t}$ as a binary treatment indicator for unit i in time t . Given no anticipation effects, the conditional parallel trend assumption can then be summarised as

Assumption 1 *For each $r \in \mathcal{R}$ and each $\{s, t\} \in \{2, \dots, T\} \times \{2, \dots, T\}$ such that $r \leq t \leq s$, we almost surely (a.s.) have*

$$\begin{aligned}\mathbb{E}[Y_t(0) - Y_{t-1}(0) \mid X, R_r = 1] &= \mathbb{E}[Y_t(0) - Y_{t-1}(0) \mid X, R_r = 0, D_s = 0] \\ &= \mathbb{E}[Y_t(0) - Y_{t-1}(0) \mid X, C = 1],\end{aligned}$$

which states that in the absence of treatment, the outcomes of currently treated, currently not-yet-treated, and never-treated units would have evolved in parallel, conditional on all covariates. In my setting, this translates to the assumption that the share of local awards in similar price ranges and industries issued by agencies in comparably remote and economically developed municipalities would have evolved along similar trajectories in absence of the IAPP reform. I choose to compare municipalities in reform cantons to both those in not-yet and in never-reformed cantons since almost all of the never-treated cantons either adopted the reform after the end of the observation period or are currently debating adoption. Challenges to the parallel trend assumption are discussed in Section 7.

The second key assumption concerns an overlap condition, which can be stated as

Assumption 2 *For each $r \in \mathcal{R}$ and each $t \in \{2, \dots, T\}$, there exists some $\epsilon > 0$ such that $P(R_r = 1) > \epsilon$ and $P(R_r = 1 \mid X, R_r + (1 - D_t)(1 - R_r) = 1) < 1 - \epsilon$ (a.s.).*

The first condition states that at least some fraction of the population is treated in each reform wave r , whereas the second specifies that for each possible set of covariates X , there is a positive probability that the unit is not treated in time t , which ensures common support between treatment and control groups in order to prevent irregular identification (Khan & Tamer, 2010).

A key shortcoming of the Callaway-Sant’Anna methodology is that its current implementation does not allow for clustered standard errors if the number of clusters is low. However, the authors note that “the choice of whether to cluster or not is usually not obvious” (Callaway & Sant’Anna, 2021, p. 25). Given that

my sample represents the entire population of digitally published Swiss tenders, there is no sampling-based uncertainty as discussed in Abadie, Athey, Imbens and Wooldridge (2020), and the assumption of an infinite superpopulation that underpins the common theoretical framework for clustered errors does not hold in this setting. Although the reform took place at the cantonal rather than the municipal level, which would warrant clustering from a design-based perspective, robust standard errors should still be relatively conservative in my setting. Following the mathematical reasoning in Abadie, Athey, Imbens and Wooldridge (2023, p. 15–16), robust estimators generally overestimate the true variance under present conditions unless “[...] clusters explain much of the variation in treatment effects”; however, with the Callaway and Sant’Anna (2021) estimator running all admissible two-by-two specifications and then aggregating the different treatment effects ex-post, this variation should be accounted for by design.

6 Difference-in-Differences Analysis

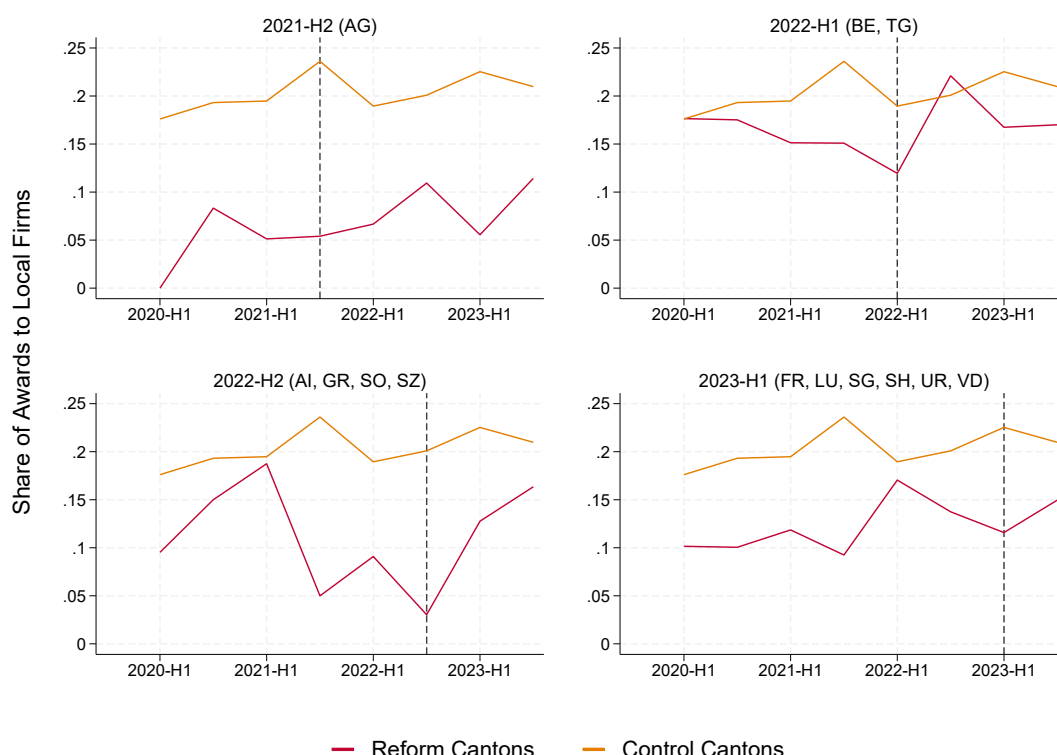
In this chapter, I analyse Hypothesis 2 within the Callaway-Sant’Anna framework described above. The subsequent sections are devoted to investigating potential composition effects as outlined in Hypotheses 2a and 2b, providing robustness tests, and finding tentative explanations for my results.

6.1 Main Results

In order to get a first sense of the potential direction and magnitude of the resulting coefficients, Figure 6 plots the raw percentages of tenders which went to local firms for the four reform waves. As a base of comparison, the average for the cantons which did not undergo a reform is also plotted in each panel. A pre-post comparison shows that for each group, the share of local firm awards picked up relatively sharply after the reform came into effect. However, given that the raw pre-trends look very different, this could represent a number of underlying differences in composition.

Turning to the difference-in-differences specification, the approach by Callaway and Sant’Anna (2021) yields a marginally positive, but statistically insignificant average treatment effect on the treated (ATT) of 2.0 percentage points ($p = 0.435$) in the share of local awards following the reform period under matching pre-trends. The dynamic-effect event study estimation shown in Panel A of Figure

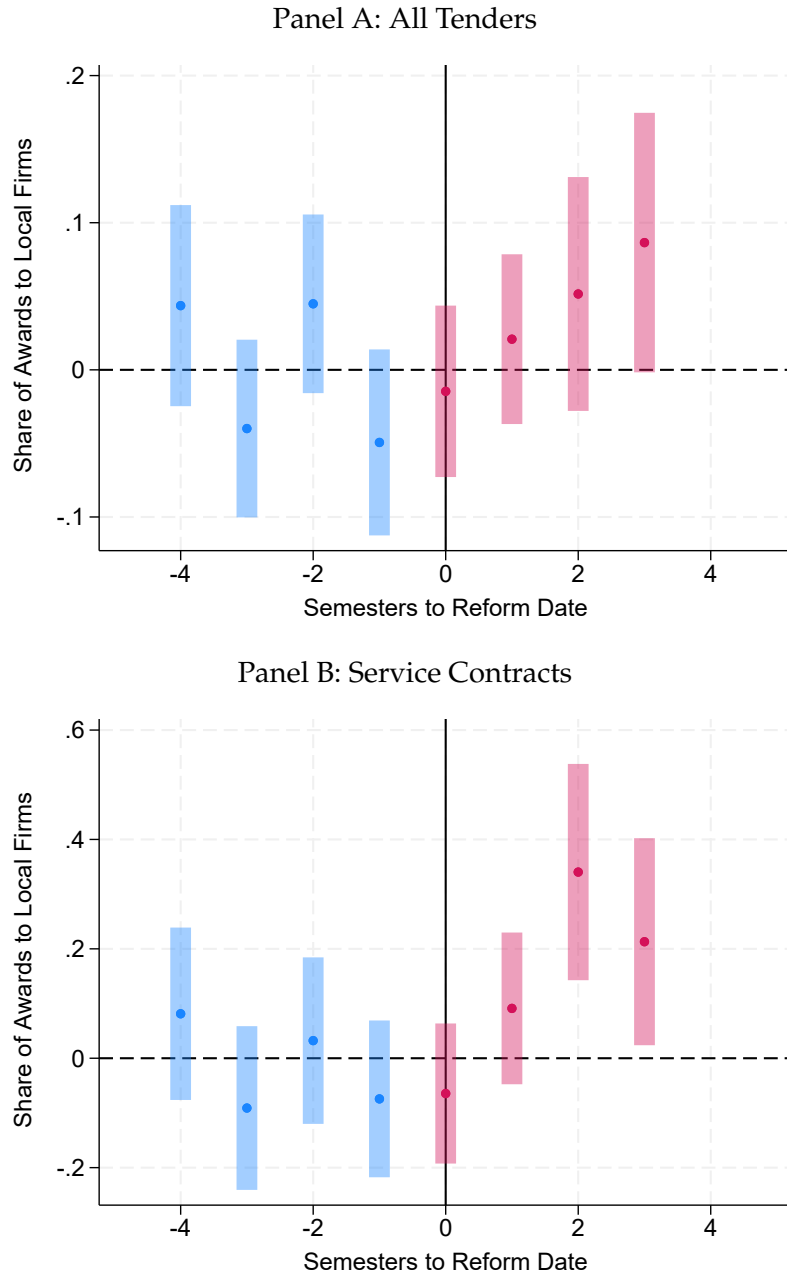
Figure 6: Share of Local Firm Awards Pre- and Post-IAPP



Notes: This figure shows the share of local firm awards for each of the four reform waves as well as for the group which did not undergo a reform. The reform waves are determined by the semester in which the reform occurred, with a dashed line marking the relevant period. H1 refers to the first six months of the year, while H2 refers to the latter half.

7 reveals a weakly increasing trend over time, although none of the estimates are statistically significant. Separate estimations for each contract type show that both construction and supply contracts have stable shares of local firm awards relative to the control group. A different picture presents itself when only considering service contracts, as shown in Panel B of Figure 7: The overall ATT estimate is significant at the 5%-level ($p = 0.046$) and has a considerable magnitude of 11.6 percentage points. Pre-trends appear to match: I fail to reject the pre-trend null hypothesis that all pre-reform coefficients are equal to 0 ($p = 0.408$). As Table 2 and the event graphs by reform wave in Appendix Figures C2 and C3 show, this positive effect is largely driven by the cantons of Aargau, Thurgau and Bern, with the remaining reform cantons having negative (although statistically insignificant) coefficients across the board.

Figure 7: Difference-in-Differences Analysis of the IAPP Reform



Notes: This figure shows the result of a two-way fixed effect staggered difference-in-differences event study specification as in Callaway and Sant’Anna (2021), using the regression specified in Equation (3) and the aggregation method given in Equation (4). Semester 0 refers to the six-month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. Since only a single canton experiences four post-reform semesters, coefficients are only reported for periods in which at least three cantons enter the estimation. The results for the individual reform waves are represented in Appendix Figure C2. All confidence intervals are at the 95%-level.

Table 2: Callaway and Sant’Anna (2021) Difference-in-Differences Analysis for Municipal Tenders

		By contract type		
	(1) All	(2) Construction	(3) Services	(4) Supplies
<i>Cross-Group Aggregate</i>				
ATT	0.028 (0.025)	0.006 (0.032)	0.116* (0.058)	−0.028 (0.051)
<i>By Reform Wave</i>				
2021–H2	0.065 (0.050)	0.003 (0.054)	0.283 (0.161)	−0.005 (0.063)
2022–H1	0.099* (0.042)	0.091 (0.054)	0.205* (0.085)	−0.013 (0.073)
2022–H2	−0.061 (0.084)	−0.062 (0.126)	−0.087 (0.171)	−0.025 (0.088)
2023–H1	−0.054 (0.036)	−0.047 (0.045)	−0.116 (0.087)	−0.059 (0.099)
<i>Pre-Trend Test</i>				
χ^2 (dof = 18)	18.75	17.29	14.69	14.05
<i>p</i> -value	0.408	0.503	0.683	0.726
Observations	10,429	5,688	3,081	1,660

Notes: This table shows the results of performing the analysis outlined in Section 5. In all columns, the outcome variable is the share of tenders which was awarded locally, meaning that the contracted firm is located in the awarding municipality. The reform waves refer to all cantons in which the IAPP reform came into force in a given semester, regardless of whether it occurred at the beginning or end of the period. *2021–H2* refers to the canton of Aargau (AG), *2022–H1* to Bern (BE) and Thurgau (TG), *2022–H2* to Appenzell I.Rh. (AI), Graubünden (GR), Solothurn (SO) and Schwyz (SZ), and *2023–H1* to Fribourg (FR), Luzern (LU), St. Gallen (SG), Schaffhausen (SH), Uri (UR) and Vaud (VD). Please refer to Table B1 for detailed information on reform dates. The null hypothesis in the pre-trend test states that all pre-treatment coefficients for each group are equal to 0. Robust standard errors are shown in round brackets. * $p < 0.05$, ** $p < 0.01$.

6.2 Composition Effects

A potential driver of these results lies in compositional effects. Since the reform requires all administrative levels to publish the results of selective and open tender procedures online, as discussed in Section 2, previously unobserved municipalities may have entered the sample. It seems plausible that such mu-

municipalities could have a higher-than-average share of local firm awards, for example because they strategically published their tender notices in local, perhaps even physical, publications in order to cater to local firms, or if they exhibit a lower degree of professionalism and thus both rely more heavily on their personal networks and are less comfortable with online tools. The number of published tender awards did indeed increase disproportionately in reform cantons during the relevant time window, as Appendix Figure C4 shows. However, contrary to prior reasoning, Appendix Figure C5 implies that the municipalities which newly entered the sample after a reform have a lower propensity to award locally. The same holds true for municipalities which only published awards prior to the reform. Figure C6 thus reproduces the previous analysis using a consistent sample, that is, awards published by municipalities which report both prior to and after the reform. I find virtually unchanged results with slightly narrower confidence intervals. Appendix Table B4 provides ATT estimates and pre-trend tests. The slightly larger ATT estimate across all contracts still appears to be driven by service contracts, for which the coefficient has increased both in magnitude and statistical significance, with a new estimate of 14.9 percentage points ($p = 0.014$).

A second potential confounding effect lies in the price-by-sector composition. While the price and sector fixed effects in Equation (3) should account for shifts along either dimension – that is, horizontal and vertical movements across the matrix shown in Figure 4 –, differential shifts across the categories pose a threat to the parallel trend assumption. Consider, for example, a scenario in which control cantons continue on with the same distribution, but reform cantons experience a shift in the price-by-sector distribution for unrelated reasons. Let this shift be heterogeneous, with the average price of a service contract falling and the average price of a supply contract increasing. There would then be no change in the fixed effects since the overall price and sector distribution remains the same; the share of local firm awards, however, would increase, since both low-price service contracts and high-price supplies contracts are awarded locally more often than their counterparts. Table 3 shows the result of a two-sided Kolmogorov-Smirnov test for reform and non-reform cantons, respectively. The null hypothesis states that both the pre-reform and the post-reform samples are drawn from the same price distribution. Since the null hypothesis is rejected at the 5% level or below for two contract types, there indeed seems to be cause for concern regarding price-by-sector shifts.

Appendix Figure C9 illustrates that controlling for price-by-sector interac-

Table 3: Two-Sample Kolmogorov-Smirnov Test: Pre-Reform vs. Post-Reform Price Distribution

	Reform		No Reform	
	<i>D</i>	<i>p</i> -value	<i>D</i>	<i>p</i> -value
<i>Two-Sample Combined K-S Test</i>				
Construction Works	0.084*	(0.013)	0.034	(0.591)
Service Contracts	0.077	(0.341)	0.120**	(0.000)
Supply Contracts	0.088	(0.503)	0.089	(0.100)

Notes: This table shows the results of a two-sample Kolmogorov-Smirnov test performed separately for each contract type. The null hypothesis states that the pre- and the post-reform samples are drawn from the same underlying distribution. The tests are performed separately for the reform and non-reform cantons and provide a formal test for differences in the distributions shown in Appendix Figure C8. *Pre-reform* refers to all observations from 01 January 2021 up to the reform date for reform cantons, and up to 01 July 2022 (the mid-point of all reforms) for non-reform cantons. The first and third columns, labelled *D*, show the largest absolute difference between the distribution functions. The second and fourth columns report *p*-values based on a five-term approximation, since exact *p*-values proved too computationally heavy to calculate. * $p < 0.05$, ** $p < 0.01$.

tions in addition to the price and sector fixed effects leads to insignificant ATTs, both for the whole sample and by contract type. However, this is largely due to wider confidence intervals as a consequence of introducing 80 additional parameters into an estimation which already suffers from a relatively low number of observations per semester-by-reform wave cell. The qualitative results remain the same as before, albeit with slightly reduced magnitudes.

As such, even though I do not find any statistically significant increases in the share of local firm awards when accounting for differential price-by-sector shifts, I argue that it does not provide a satisfactory explanation for the positive and significant estimates from earlier specifications. To this end, I offer descriptive evidence using a condensed version of the price-by-sector matrix, shown in Appendix Figure C7, and the price-by-sector distribution presented in Appendix Figure C8. The distribution of service contracts in Panel A is of the greatest interest, since service contracts seem to be the driving force behind the positive estimates found earlier. According to the Kolmogorov-Smirnov test in Table 3, only non-reform cantons experienced a significant shift in the price distribution; indeed, there appears to be more density on the far left of the distribution, that is, in the price categories which are the most likely to be awarded locally. This,

however, should exert downward rather than upward pressure on my impact estimate. Although the formal test finds no significant change in the distribution for reform cantons, visual inspection indicates a moderate shift to the right for reform cantons, that is, towards price categories which are less likely to be awarded locally than the lower-price categories, again implying a downward bias. Within supply contracts, there is a statistically insignificant shift towards high-price contracts in non-reform cantons, which are more likely to be awarded locally than their low-price counterparts, thus again creating downward pressure on the estimates. The only shift which would lead to upward bias is the shift towards construction contracts above CHF 1m in reform cantons; however, this fails to explain the positive estimates for services contracts.

6.3 Further Analysis

Having argued against two possible composition effects as drivers of the results found in Section 6.1, I present further exploratory analyses in the following chapter. I first show that the behaviour of cantonal procurement officers did not respond to the IAPP reform in the same way as at the municipal level, which I interpret as evidence against a universal change in sustainability preferences. I then consider changes in award criteria and firm characteristics as possible explanatory factors.

As postulated in Hypothesis 2b, the IAPP reform may have induced an increased preference for local firms on the basis of sustainability considerations. Given that the reform also affected cantonal procurement, and assuming that cantonal officers have little reason to engage in municipal protectionism, an increase in the share of local awards at the cantonal level would point towards this mechanism driving the municipal results. However, as Table 4 shows, the aggregate ATT estimates for cantonal tenders are insignificant across the board. Interestingly, one of the groups which previously drove the positive results for service contracts – the 2022–H1 reform wave consisting of the cantons of Thurgau and Bern – now shows a relatively drastic reduction in the share of local awards, although the estimate narrowly fails to reach statistical significance. In construction contracts, on the other hand, there is a significant increase for this group; the other three reform waves, too, have weakly positive estimates, although none reach statistical significance. Finally, in supplies contracts, only the four cantons belonging to the 2022–H2 wave experience a sharp and significant decline in the share of local awards. Two other groups also see negative

coefficients, while the last is marginally positive, with all three being statistically insignificant. The opposing effects on the different contract types cancel out when looking at the full sample, with estimates being insignificant and close to zero for all groups.

Table 4: Callaway and Sant’Anna (2021) Difference-in-Differences Analysis for Cantonal Tenders

	By contract type			
	(1) All	(2) Construction	(3) Services	(4) Supplies
<i>Cross-Group Aggregate</i>				
ATT	0.002 (0.030)	0.047 (0.038)	−0.025 (0.056)	−0.084 (0.056)
<i>By Reform Wave</i>				
2021–H2	0.010 (0.043)	0.075 (0.043)	−0.031 (0.116)	−0.074 (0.056)
2022–H1	−0.041 (0.069)	0.127** (0.036)	−0.300 (0.160)	−0.169 (0.096)
2022–H2	−0.021 (0.069)	0.012 (0.092)	0.053 (0.157)	−0.337* (0.139)
2023–H1	0.034 (0.052)	0.017 (0.052)	0.025 (0.064)	0.036 (0.089)
<i>Pre-Trend Test</i>				
χ^2 (dof = 14)	17.11	15.33	18.61	12.35
<i>p</i> -value	0.251	0.356	0.180	0.578
Observations	9,822	5,167	2,846	1,809

Notes: This table shows the results of performing the analysis outlined in Section 5 for cantonal rather than municipal tenders. In all columns, the outcome variable is the share of tenders which was awarded locally, meaning that the contracted firm is located in the same municipality as the execution site. The empirical specification is as in Equation (3) with additional tender-level controls, namely language, the activity of the awarding authority, whether the procedure was open, and whether it was subject to WTO regulation. The null hypothesis in the pre-trend test states that all pre-treatment coefficients for each group are equal to 0. Robust standard errors are shown in round brackets. * $p < 0.05$, ** $p < 0.01$.

I next attempt to analyse whether the increase in local awards after the IAPP reform is accompanied by shifts in award criteria or firm characteristics – that is, whether the newly locally awarded contracts differ from the kind of contract which would have previously been awarded to a local firm. Before turning to the

results, it is important to note that the following analysis is purely descriptive. On the one hand, restricting the sample to local awards reduces the number of observations to such a degree that the Callaway-Sant’Anna estimator does not converge anymore. On the other hand, as noted in Section 4.2, the outcomes I consider are not present for all observations, and they are missing non-randomly, with firm size indicators in particular being biased towards larger firms. As such, I also refrain from estimating a static difference-in-differences design as in Equation (1), and instead present within-group differences in means only.

Keeping in mind that differences in characteristics may also be driven by variation in underlying tender features, Table 5 below summarises tender characteristics for non-reform and reform cantons in the periods prior and posterior to the IAPP reform, using only locally awarded tenders from the difference-in-differences sample. As in previous tables, the cut-off for control cantons is the midpoint of all reforms. Tender characteristics in non-reform cantons remain remarkably stable across the two periods, with the only significant differences being an increase in the number of submitted offers and a lower share of tenders for which criteria could be extracted. In reform cantons, the share of awards with any criteria also fell by a comparable percentage. The more striking disparity lies in the average firm size, which is markedly smaller by all measures, although only the differences in turnover and the number of employees are significant.

7 Discussion

In Section 3, I conjectured that home bias is evident in municipal procurement outcomes, and that the IAPP reform reduced its prevalence. In the following, I examine the first hypothesis in light of the descriptive statistics presented in Section 4.2. I then analyse the second statement using the causal results from the previous chapter. My conclusions support the notion that home bias may be present in municipal procurement. Contrary to expectations, the IAPP reform seems to have done little to curb this bias; instead, it may have widened the channels through which local firm preference manifests. Although these findings could have come about through different channels than the one I propose, they retain their relevance to political actors and the general public. I conclude with a summary of additional avenues which should allow for more nuanced policy recommendations in future work.

Using the full 2010–2023 sample, I indeed find a higher share of local awards in municipalities compared to cantonal procurement, although the gap does not

Table 5: Local Award Characteristics Pre- and Post-Reform

	Non-Reform Cantons			Reform Cantons		
	(1) Pre	(2) Post	(3) Difference	(4) Pre	(5) Post	(6) Difference
<i>Panel A: Tender Characteristics</i>						
Contracted Sum (mCHF)	3.808 [67.985]	1.327 [4.406]	-2.480 (2.466)	1.153 [1.799]	1.061 [1.668]	-0.092 (0.159)
Open Procedure	0.811 [0.391]	0.821 [0.384]	0.010 (0.022)	0.879 [0.327]	0.909 [0.288]	0.031 (0.026)
Number of Offers	6.278 [4.548]	7.839 [11.366]	1.561** (0.545)	5.657 [4.736]	4.841 [4.875]	-0.816 (0.433)
<i>Panel B: Award Criteria</i>						
Any Criteria	0.767 [0.423]	0.695 [0.461]	-0.072** (0.025)	0.801 [0.400]	0.712 [0.454]	-0.089* (0.037)
Any Non-Price Criteria [†]	0.701 [0.458]	0.659 [0.474]	-0.041 (0.026)	0.697 [0.460]	0.642 [0.480]	-0.055 (0.041)
Sustainability Criteria	0.082 [0.274]	0.065 [0.247]	-0.016 (0.014)	0.101 [0.302]	0.119 [0.325]	0.018 (0.027)
Weight Non-Price Crit. (%) [‡]	59.161 [33.629]	55.370 [28.220]	-3.791 (2.424)	45.257 [27.770]	46.860 [24.927]	1.604 (2.662)
Weight Sust. Crit. (%) [‡]	13.226 [6.408]	16.591 [12.090]	3.365 (2.677)	16.200 [8.694]	18.571 [10.505]	2.371 (2.875)
Observations	785	537	1,322	297	243	540
<i>Panel C: Firm Characteristics</i>						
Capital (mCHF, indicative)	5.513 [20.220]	4.162 [17.582]	-1.351 (1.151)	5.754 [26.039]	3.295 [17.867]	-2.459 (2.078)
Turnover (mCHF, indicative)	299.988 [1,312.528]	439.792 [1,971.426]	139.804 (109.454)	728.753 [3,112.284]	208.798 [1,125.055]	-519.955* (225.040)
Employees (indicative)	425.718 [1,547.521]	410.243 [1,644.024]	-15.474 (94.067)	716.183 [2,194.106]	286.304 [1,117.419]	-429.879** (156.866)
Sole Prop. or Partnership	0.032 [0.176]	0.045 [0.207]	0.013 (0.011)	0.033 [0.180]	0.038 [0.192]	0.005 (0.017)
LC or LLC	0.861 [0.346]	0.879 [0.327]	0.018 (0.019)	0.893 [0.310]	0.911 [0.286]	0.018 (0.027)
Other Legal Form	0.107 [0.310]	0.076 [0.266]	-0.031 (0.016)	0.074 [0.262]	0.051 [0.221]	-0.023 (0.021)
Observations	754	512	1,266	270	235	505

Notes: This table shows the evolution of award criteria and firm characteristics of contractors for the sample of locally awarded municipal contracts from previous analyses. In Panel A, the contracted sum refers to the exact price or midpoint of the price range given in the award notice. The first three rows of Panel B show the fraction of the sample for which keywords matching the given criteria could be extracted from the data. [†]Non-price criteria refer to quality, sustainability, time to completion, professional references, experience, and whether the contracted firm trains apprentices. [‡]Weights are only available for 1,072 observations, or 77% of local contracts which have any criteria. In Panel C, the first three rows provide measures of firm size. Since the commercial register only records the latest entry, they are purely indicative. They are furthermore more likely to be available for larger firms. Turnover and the number of employees may be based on ranges. The final three rows summarise the share of different legal forms: Sole proprietorships and partnerships, limited and limited liability companies, and other legal forms. In total, 1,771 observations (95% of local firms in the difference-in-differences sample) could be matched to commercial register entries. Standard deviations are given in square brackets, robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

appear to be of concerning magnitude. A disaggregated analysis reveals large disparities especially in low-price contracts and in the service sector, lending support to Hypothesis 1a. Tender-level statistics for municipal awards speak against Hypothesis 1b: Tenders which go to local firms are, in fact, slightly less likely to cite non-price criteria, and even when they do so, they place somewhat lower weight on them. The incidence of sustainability criteria is somewhat higher, although not to a degree which would lend credibility to the idea that non-price criteria are used to enable favouritism as suggested by Saussier and Tirole (2015). In line with previous literature, I find that local firm awards are less likely to occur in open procedures and that they publish criteria more seldomly, both of which point to lower transparency. The finding that local contract winners appear to be smaller firms with less transparent legal forms could be due to the fact that local awards are more likely to occur in the service industry, where economies of scale may matter less. At the same time, it fits into the conclusion drawn by Wittberg and Fazekas (2023, p. 243), who argue that particularism benefits “local firms rather than the leading firms in the market”. A somewhat surprising finding arises in connection with the number of competing offers, which is higher when a tender is awarded locally. This would suggest that, if explicit favouritism is indeed at play in the tendering process, it manifests in the latter stages of the award process. Fazekas, Tóth and King (2013) list over twenty approaches which allow agents to tailor tender outcomes to their preferences, with techniques such as adjusted assessment criteria, strategic information dissemination, and intransparent scoring rules all still being feasible once bids have been submitted. The high number of offers would also fit under the assumption of implicit or subconscious favouritism, in which offers made by local firms are treated preferentially without express manipulations of the tendering process. All in all, the patterns identified across and within municipal procurement awards are consistent with municipal home bias as laid out in Hypothesis 1, although they cannot be causally attributed due to the purely descriptive nature of the underlying analysis.

More causal evidence is given by the difference-in-differences specification which exploits the variation in procurement regulation generated by the adoption of the IAPP reform. Contrary to Hypothesis 2, I do not find a decrease in the share of local awards; indeed, I find an overall null result which masks significant heterogeneity across reform waves and across contract types. The early reform cantons of Aargau, Bern and Thurgau show increases in their local award rate which manifest most strongly in service contracts – the sector previ-

ously identified to be the most prone to local awards – and intensify over time. The two moderating subhypotheses 2a and 2b do not find support in the data: Cross-sectional composition effects, price-by-sector shifts, or changes in award criteria do not seem to be the driving forces behind these results. Neither are the findings replicated at the cantonal level. What is more, with submissions often taking months to evaluate, the null result for later reform waves may be owed to the circumstance that I observe them for at most two post-reform periods. The positive coefficients found for early reform cantons may simply not have manifested yet. An exploratory descriptive analysis further points to a marked decrease in the size of local contract winners and a decrease in the number of competing offers relative to non-reform cantons. Even though one goal of the IAPP reform had been to foster the use of sustainability and other non-price criteria, I find no significant increase in their use, and indeed even a universal decrease in the incidence of any identifiable award criteria. Although I cannot rule out that the keywords in my parsing algorithm are misspecified, this observation is in line with the previous finding that Swiss municipalities perform worse than cantonal or federal procurement agencies when it comes to specifying sustainability criteria (Welz & Stürmer, 2021).

As such, the evidence presented in this paper suggests that municipal agencies award service contracts locally more frequently, with the winners being smaller firms on average, whereas cantonal officers do not change their awarding behaviour as a consequence of the IAPP reform. The descriptive findings allows for some speculation as to the underlying reasons. On the one hand, it is possible that the reform induced a new focus on regional procurement stemming from environmental considerations, although this impact channel is at odds with the lack of an increase in sustainability criteria and the null results at the cantonal level. Instead, it is likely that the more stringent publication requirements are not implemented fully at the municipal level. My findings give no indication that the lack of transparency identified by Mabillard and Zumofen (2021) has abated, whereas the increased discretion stemming from the more generous criterion regime may make it easier for procurement agencies to tailor their criteria to their favoured firm, as criticised by several of Mabillard and Zumofen’s respondents. My results thus echo the finding that policy reforms are rarely effective in reducing home bias, and may even end up fostering it (Fazekas & Cingolani, 2017; Rickard & Kono, 2014). They further support the notion that particularism is not limited to settings characterised by institutionalised corruption, but can also arise in mature democracies, as already shown for the Swedish context by

Broms et al. (2019), Hyytinen et al. (2018), and Wittberg and Fazekas (2023).

7.1 Internal and External Validity

Before reviewing alternative mechanisms and the resulting policy implications, the validity of the findings presented above bears discussion. As laid out in Section 5, a staggered difference-in-differences approach requires two central assumptions to yield valid estimates: The conditional parallel trend assumption as well as the overlap condition.

Since municipal procurement is exclusively governed by the legislation of the canton it is located in, there were no concurrent policy interventions which could have influenced the outcome. All cantons which adopted the IAPP reform into their cantonal constitution did so almost verbatim. Considering the staggered adoption, and with the Swiss economy being highly interconnected across regions, it further seems unlikely that there were other shocks which occurred at the same time as one of the reforms and only influenced either the reform or the control cantons. The main threat to the conditional parallel trend assumption lies in endogenous adoption: If choosing to implement the reform is correlated with the main outcome, it does not seem reasonable that all groups would have evolved in parallel in absence of the reform. However, even though cantonal parliaments had to vote to adopt the IAPP reform, meaning that it was strictly speaking not imposed exogenously, I argue that there should be no connection to home bias at the municipal level. First of all, the relevant agents differ, with municipally-employed procurement agents being relatively far removed – both geographically and in terms of hierarchy – from elected members of cantonal parliaments. Secondly, almost all cantons in the control group either adopted the reform after the end of the period under observation or are currently in the process of discussing it in parliament; only the canton of Ticino has not begun its adoption procedure. The timing of the reform is thus likely owed more to the relative tightness of each parliament’s agenda than its political leaning. Indeed, the reform waves do not appear to correlate with the degree of urbanisation, language, geography, or culture. The overlap condition, too, is justified by the circumstance and timing of cantonal IAPP adoption: Five of the ten largest cities are located in control cantons, the other five in reform cantons; both groups furthermore contain lowland and mountain regions as well as both highly urban and very remote municipalities. As such, the covariates never perfectly predict treatment status, as demanded by the common support condition. The

assumptions which underpin the internal validity of my approach should thus be justified.

Regarding the external validity of my findings, I consider two questions. The first asks whether the conclusions I draw extend to all tenders in Switzerland, namely the awards which are not published electronically. I argue that they do not. Tenders go unpublished because they are confidential, such as in matters of national security, or because the tendered sum is too small, which is the case for almost all discretionary procedures. The former are unlikely to be procured by municipal agencies since defence is a federal responsibility. The latter, on the other hand, give agents complete freedom of action coupled with a total lack of disclosure, making them particularly attractive for realising a potential home bias. Even at the federal level, discretionary awards make up at least 20% of annual spending, which represents a significant obstruction to transparency (Mabillard & Zumofen, 2021). The share of discretionary awards is likely at the same level or higher in municipal procurement. Assuming the impact channel described previously, my estimates thus provide a lower bound on the true incidence rate of local awards.

The second question relates to the relevance of my results in other contexts. While the institutional context of 26 separate procurement regimes on such a small geographical scale is likely unique, I argue that my findings should be of concern to any setting in which local authorities allocate significant public funds under conditions of high autonomy and low publicity. Coupled with the results from Sweden, they indicate that favouritism may be a greater concern than previously thought. Before discussing the policy implications of this statement, however, an examination of challenges to the proposed channel of impact is warranted.

7.2 Alternative Mechanisms

Apart from the channels proposed above, there are several alternative mechanisms which could drive the results found in this study. In the following, I present a non-exhaustive selection.

Compositional Shifts of Award Procedures. As previously noted, my data set only contains selective and open award procedures. Discretionary procedures, which can be employed if the contracted sum is low (compare Appendix Table B2 for exact thresholds) or under certain exceptional circumstances, are not subject to any publication requirements and are thus unobserved. However, as noted

in Section 3, increased officer discretion and reduced observability are linked with a higher propensity towards particularism, meaning that discretionary awards could well be the preferred vehicle to realise local firm bias. Although the IAPP reform weakly facilitated the use of discretionary procedures by raising the relevant threshold for supplies contracts by CHF 50,000, it is still possible that it induced an overall shift away from discretionary into selective and open procedures, for example out of fear of more intensive monitoring following the increased stringency in anti-corruption measures. If officers used the techniques laid out by Fazekas et al. (2013), they could still tailor more competitive tenders to their favoured firm. Under such a scenario, the overall local award rate could be similar across all cantons, and the reform would have merely shifted some local awards into the observed part of the distribution.

Changes in Submission Behaviour on Behalf of Firms. Herz and Varela-Irimia (2020) document that firms' decisions to bid on tenders are influenced by home bias on part of European procurement agencies. In light of this, it is possible that firms anticipated a change in procurement patterns, such as an increased focus on local sourcing due to sustainability concerns, and adapted their submission behaviour, perhaps by focusing on tenders in non-reform cantons. Such a channel would be consistent with the finding that the number of offers per tender significantly increased in control cantons, as well as with the fact that the positive coefficients in the difference-in-differences analysis increase over time, since the submission phase for tenders published in the first or second post-reform period would have begun well before the IAPP entered cantonal law. However, the number of offers per tender in reform cantons remained stable and relatively high at a mean of 4.8, meaning that procurement officers should have still had ample non-local alternatives to choose from. This explanation also fails to account for the patterns observed across contract types.

Pressure Exerted by Local Actors. Finally, the reform may not have influenced the manifestation of a potential home bias on part of municipal officers, but rather could have encouraged local actors to exert pressure on procurement agents, as documented in Hansson and Holmgren (2011) for a Swedish municipality. This channel is consistent with the observation that ex-ante tender characteristics are largely unchanged compared to the pre-reform period, whereas contracted firms are much smaller post-reform.

Under any of these scenarios, competitiveness in the public procurement sector is hampered, although the responsible actors differ. While the results thus hold unmitigated relevance to policymakers, this study cannot provide

an immediate panacea. However, my results underline the need for more binding reporting requirements coupled with stringent enforcement, especially regarding discretionary award procedures. Following the recommendations made by Mabillard and Zumofen (2021), such measures could include reducing language barriers, ensuring full transparency regarding tender documentation and award criteria, and disclosing as much information about the award decision – for example the scores attained by all competing firms – as possible without violating a firm’s right to confidentiality.

7.3 Further Research

Finding a satisfying answer as to why the IAPP seemingly failed to reduce the share of local awards warrants further research, both from an academic and a policy standpoint. The recent literature around bias in public procurement offers several interesting avenues, which unfortunately could not be implemented in this paper due to time limitations or data scarcity.

Fazekas et al. (2013) cite tailoring contract contents, eligibility conditions, assessment grids or award criteria as techniques which allow procurement officers to favour certain firms within the scope of the law. Given the relatively rich project descriptions in the Simap (2024a) data, natural language processing could be employed to identify patterns which predict local awards in contracts from the pre-reform era and determine whether the IAPP reform led to a significant increase in such indicators. The feasibility of applying such technology to tender data has been demonstrated by Torres-Berru, Lopez-Batista and Zhingre (2023), who use a natural language model to detect gender bias in Ecuadorian procurement data.

To combat the difficulties relating to the unobserved discretionary awards, a threshold change introduced in the IAPP reform could be exploited in a regression discontinuity or missing density design. In cantons which adopted the reform, supplies contracts can now be awarded through discretionary procedures below a contract value of CHF 150,000, rather than CHF 100,000. If the density of open and selective awards in the CHF 100,000–150,000 range decreases drastically in municipal awards, but not in cantonal awards, and in reform, but not in control cantons, this could be read as evidence towards strategic behaviour on part of municipal procurement officers. A number of recent studies have employed similar strategies (Carril, 2021; Celis Galvez et al., 2023; Szucs, 2023; Tas, 2023). In my setting, this approach is currently hampered by

low data availability, with only a handful of contracts in the relevant price range having been published in the post-reform window. However, with the number of observations growing and more cantons adopting the reform, a missing density strategy should become feasible in the future.

Roman (2015) furthermore points out that the procurement literature has largely neglected procurement officers and their personal preferences. Given that my data set contains the identity of the responsible agent, a network analysis approach similar to Fazekas and Wachs (2020) would allow me to investigate award patterns within the web of agent-firm relationships. Moreover, with the commercial register listing all powers of attorney given to firm representatives, which encompasses founders, board members, and high-ranking managers, I could expand the scope of this study from local proximity to interpersonal relationships. However, even though all data sources are public, connecting individuals in such a manner – especially with the aim of identifying legally grey behaviour – could pose significant data protection issues.

More generally, the question of why legislation has often proven unable to curb home bias remains unanswered to date. With tender-level data availability growing, research into the determinants of policy failure is of vital interest to policymakers and the wider public, given the large share of public procurement in government spending. With the exception of Sweden, particularism also remains severely understudied in low-corruption contexts, even though all results – including the present analysis – point towards favouritism being at play in regional public procurement.

8 Conclusion

Home bias in procurement and its ramifications for free competition have been the centre of much academic attention to date. Its presence at more regional levels, especially in low-corruption contexts, has only come into focus more recently. Even less is known about the response of small-scale home bias to policy interventions. I attempt to close this research gap for the Swiss context, where municipal procurement outcomes have never been analysed in an econometric study to date, despite the availability of relatively detailed tender-level data.

By comparing municipal to cantonal tenders and local to non-local awards, I find patterns consistent with local firm preference in municipal procurement. Local firm awards, in which the contract winner is domiciled in the awarding municipality, are more prevalent in low-price contracts and in the service sector –

contract types which are arguably subject to the least scrutiny. Local contractors are furthermore smaller than their non-local counterparts and less likely to be limited or limited liability companies, which are the legal forms with the most stringent reporting requirements by Swiss law.

I then exploit a reform to procurement law which introduced more stringent transparency and anti-corruption measures and placed higher weight on sustainability. Since it was adopted in a staggered manner, I employ the two-way fixed effect difference-in-differences approach developed by Callaway and Sant'Anna (2021), which allows for time-varying and group-dependent heterogeneity while avoiding the forbidden comparison problem. I find that the reform did not reduce the share of local firm awards. Indeed, for early reform waves and in the service sector, it led to a significant increase instead. Neither composition effects nor price-by-sector shifts give satisfactory explanations for this result. Instead, further descriptive evidence is consistent with an increased manifestation of conscious or subconscious home bias on part of procurement agents.

My findings could also be driven by other actors: Changes in the submission behaviour of competing firms, pressure exerted by local actors, or shifts between the observed and the unobserved part of the award procedure distribution could have generated similar patterns in the data. As such, I cannot pinpoint a universal policy lever to provide a remedy. Still, my study remains informative to policymakers and the wider public. It highlights the prevalence of home bias even in mature democracies and stresses the importance of transparency in the procurement process. Besides its contributions to the academic literature and to public debate, this study provides a novel tender-level data set with linkages to person and firm records. With increasing data availability and novel research methods, the puzzle around the driving forces behind local firm bias and the most effective means to curb them could be solved in future research.

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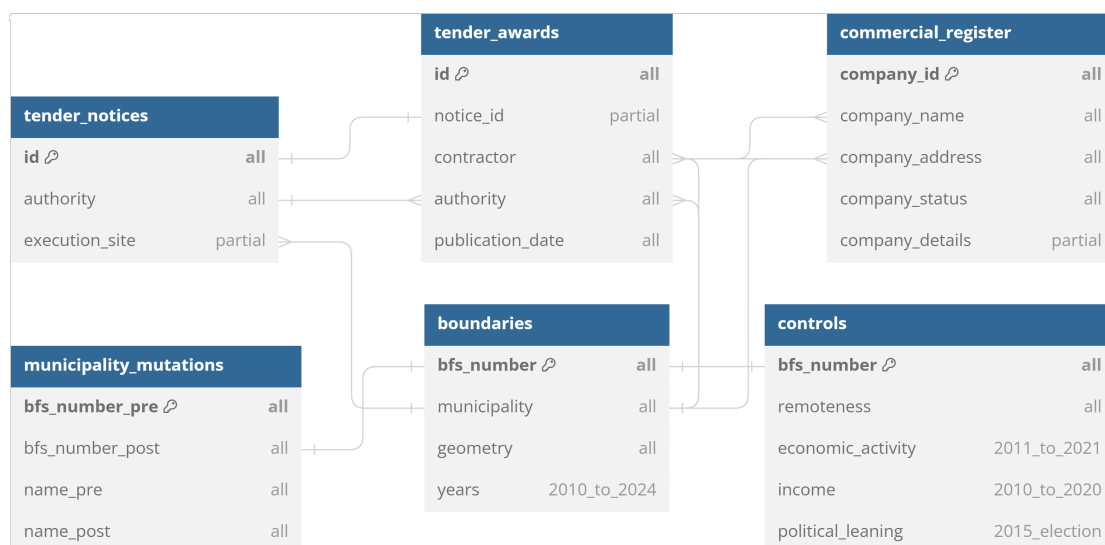
Appendices

A Supplementary Information

A.1 Data Structure

The data set analysed in this paper is assembled from various source data sets. Figure A1 provides a visual aid for the relationships between the different sources. `tender_notices` and `tender_awards` refer to the raw tender data scraped from the Swiss e-procurement website Simap (2024a, 2024b), which were obtained through its public API using an open-source program by Hess (2024). `tender_notices` records announcements of public procurement projects, including a description and, if applicable, the address of the execution site. `tender_awards` is the main data set of interest as it lists successfully awarded projects and thus contains data on the contracted firm and the contracting authority.

Figure A1: Relationships between Source Data Sets



Notes: This figure illustrates the relationships between the source data sets underlying the main data set. Each unique or quasi-unique identifier is marked with a key symbol. Attributes which are present for all observations are labelled *all*, while attributes which are only observed for a subset are labelled *partial*. Time-dependent attributes are labelled with the time span for which they are available. Linkages with two vertical ticks indicate one-to-one relationships, whereas linkages with one vertical tick and one branched end indicate one-to-many relationships. The diagram is simplified for better readability, with many individual attributes being aggregated into broader categories, and certain linkages omitted.

Other sources are the commercial register of Switzerland, which lists all

registered companies and was provided by CRIF AG (2024); additional controls, namely municipality remoteness (Federal Office for Spatial Development, 2017a, 2017b), economic activity (Federal Statistical Office, 2023), average taxable income (Federal Tax Administration, 2024), and political leaning (Federal Statistical Office, 2015); and geographical data consisting of detailed municipality and canton boundaries for the entire period under consideration (Swisstopo, 2024b) in addition to lists of all named localities, municipalities, and municipality mutations (Federal Statistical Office, 2024a, 2024b; Swisstopo, 2024a).

In the following, I outline the process which transforms the source data sets into my final data set. Figure A2 details the exact number of observations in the initial tender award data and in the final data set and shows the losses at each step of the way.

Free Text Parsing. Simap forms consist largely of free text entry fields. As such, it is necessary to parse several key variables – among them the name of the awarding municipality and the execution site of the contract – from the raw data. I use lists of past and present municipalities as well as of all named towns, villages and hamlets in Switzerland to extract matching strings, accounting for possible spelling variants in all national languages (e.g. *St. Gallen* and *Sankt Gallen*, or *Saint-Etienne* and *St-Etienne*), double names in various languages (e.g. *Biel/Bienne*), and shortened versions (e.g. *Muri bei Bern*, *Muri b. Bern*, and *Muri*). Having parsed these names, I can then merge in the respective unique identifier: In the case of municipalities, the number assigned by the Federal Statistical Office BFS (henceforth ‘BFS number’), and in the case of execution sites, a set of coordinates which represent the centroid of the ZIP code area corresponding to a given locality.

Matching Notices to Awards. Since the execution site variable is derived from the tender notice database, but all other information is retrieved from the set of tender awards, it is necessary to merge the two. Only about 60% of awards list the identifier of the associated notice and can thus be matched one-to-one. Fortunately, the publication date of the notice is a required field. As such, awards can be matched to their notices based on the notice publication date together with the project name, the demanding authority, the contracting officer, or any combination of these. Manual checks show that for awards which cannot be matched, the corresponding notice usually appeared in a physical publication, such as a local gazette. Finally, it is possible that my parsing algorithm extracts multiple execution sites (for example in the case of roads which run from one village to another). Since cantonal and federal tenders serve as the control group,

I would like to over- rather than underestimate the share of local awards for these groups to ensure that I do not exaggerate any findings at the municipal level. I therefore identify a firm which received a cantonal or federal contract as *local* if it is located in the same municipality as any one of the execution sites.

Geocoding Firm Addresses. Both in the tender award data and in the commercial register, firm addresses are given in a regular format. Using a governmental API (Swiss Confederation, 2024), I retrieve the coordinates associated with these addresses in the Swiss coordinate system LV95. I find the corresponding municipality by overlaying the municipality boundaries valid on 01 January of the year in which the award was published, adjusting for mutations in municipality borders which occur within that year.

Adding Municipality-Level Controls. The majority of the covariates I employ are at the municipality level and based on BFS numbers; as such, they can be merged in directly. Since some covariates, such as political leaning, date to a single year, I account for municipality mutations in the following way: Municipalities which do not exist anymore are assigned the value of the municipality they fused into. Municipalities which do not exist yet are assigned the average of the municipalities they resulted from.

Adding Firm-Level Controls. The commercial register contains information on firm age and legal form, the identity of its founders and current signatories, and additional variables which allow for an estimate of firm size. The award data only contains the name and address of the contracted firm, but no official identification number. Fortunately, Swiss law requires firm names to be “clearly distinct from every other business name of businesses [...] already registered in Switzerland” (Art. 951, Federal Code of Obligations, 1912); as such, matching on names represents a valid strategy. Since contracting officers are prone to altering company names (for example, ‘Ravel et Fils Ingénierie Lausanne S.A.’ might be shortened to ‘Ravel & Fils SA’, whereas a firm registered as ‘Implenia AG’ might be referred to as its branch office ‘Implenia AG, Zweigstelle Dübendorf’), I employ a fuzzy matching strategy within a limited geographic radius of the contractor’s address. A contracted firm is first compared to all other firms within the same city and then within the same canton. For each commercial register firm the contractor is compared against, the algorithm selects a number of substrings from the longer name equal to the length of the shorter name, and then calculates the Levenshtein distance – that is, the number of character changes necessary to get from one to the other. With a threshold of 95 (where a score of 100 represents a perfect match), I allow only relatively small differences to the best-matching

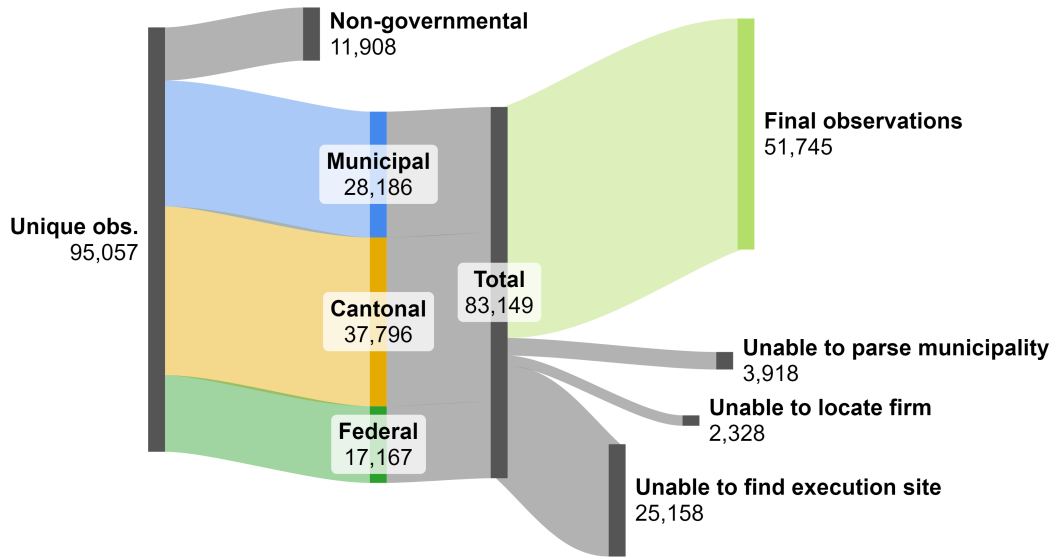
subset of the commercial register name, leading to a match rate of 90.52% among Swiss contractors. Closer investigation shows that unmatched observations (i) belong to a firm which is headquartered in a different canton (since there are 1.2m entities registered in Switzerland, comparing unmatched contractors to all Swiss firms is computationally infeasible); (ii) are not listed in the commercial register at all, presumably because they do not meet the threshold values for mandatory registration or because they are a subsidiary of another firm; (iii) are abbreviated or misspelt to such a degree that the true commercial register entry does not meet the fuzzy match threshold. The final point could be combatted through a lower threshold; trial and error indicates, however, that this yields only modestly higher match rates at the price of much lower accuracy.

Constructing the Outcome Variable. A contracted firm is classified as *local* if the address provided in the award data is located within the awarding municipality at the time of publication. When using cantonal or federal tenders as a comparison group, the municipality of the execution site is relevant instead. To ensure that I do not introduce distortions by constructing the outcome variable in this way, I also parse execution sites for municipal contracts and construct the local firm variable using the execution site municipality rather than the awarding municipality; this approach leads to virtually the same outcome, with differing results in only 0.88% of cases.

As shown in Figure A2, parsing execution sites from the tender notice data incurs the heaviest losses in terms of observations. As discussed above, some awards cannot be matched to a tender notice because the corresponding notice was published elsewhere. For the vast majority, however, a notice is present, but no execution site can be parsed from it. This may be because there is no fixed execution site; because the execution site is named in an external document rather than in the notice itself; or because the tender description is extremely sparse, with the execution site either left implicit or available upon personal inquiry. Since the contracting municipality rather than the execution site is used to construct the outcome variable for municipal contracts, only cantonal and federal observations are lost in this step.

Firm addresses which do not yield valid coordinates are another reason for data losses. They are mainly due to the governmental API I used for this task, which is relatively intolerant towards alternative spellings or mistyped names. This, too, mainly affects cantonal and federal contracts, with only 590 municipal tenders lost to a missing firm site. The main source of lost municipal contracts stems from the 3,918 observations which are classified as municipal in the raw

Figure A2: Number of Observations and Losses in the Generation Process



Notes: This figure illustrates the number of observations in the raw and the final data sets. The number of unique observations on the far left refers to the number of award notices available on Simap.ch for the period of 01.01.2010–31.12.2023 after sorting out tenders published multiple times in different languages. The number of final observations refers to the size of the data set used in all analyses in this paper, comprising 23,678 municipal, 22,995 cantonal, and 5,072 federal tender awards.

data, but for which I am unable to parse a valid municipality name. Because the name of the contracting authority is a free text entry field, some observations are lost to misspellings; the majority, however, is due to contracts which are issued by intermunicipal associations, such as regional power or waste utilities. Since there is no comprehensive list of such organisations, I elected to remove these observations from the data set.

In total, the final data set contains 23,678 municipal, 22,995 cantonal, and 5,072 federal tender awards. For the main category of interest, the municipal tenders, this means that 84.01% of all original observations enter my analysis.

B Supplementary Tables

Table B1: Adoption of the Intercantonal Agreement on Public Procurement by Reform Date

Canton	Abbreviation	Reform Date	Source
Aargau	AG	01.07.2021	Source [DE]
Bern	BE	01.02.2022	Source [DE]
Thurgau	TG	01.04.2022	Source [DE]
Solothurn	SO	01.07.2022	Source [DE]
Schwyz	SZ	01.09.2022	Source [DE]
Graubünden	GR	01.10.2022	Source [DE]
Appenzell Innerrhoden	AI	01.11.2022	Source [DE]
Fribourg	FR	01.01.2023	Source [FR]
Luzern	LU	01.01.2023	Source [DE]
Schaffhausen	SH	01.01.2023	Source [DE]
Vaud	VD	01.01.2023	Source [FR]
Uri	UR	01.06.2023	Source [DE]
Sankt Gallen	SG	01.06.2023	Source [DE]
Zürich (*)	ZH	01.10.2023	Source [DE]
Basel-Landschaft (*)	BL	01.01.2024	Source [DE]
Neuchâtel (*)	NE	01.01.2024	Source [FR]
Valais (*)	VS	01.01.2024	Source [FR]
Basel-Stadt (*)	BS	01.02.2024	Source [DE]
Glarus (*)	GL	01.03.2024	Source [DE]
Zug (*)	ZG	01.03.2024	Source [DE]
Jura (*)	JU	01.05.2024	Source [FR]
Nidwalden (*)	NW	01.05.2024	Source [DE]
Appenzell Ausserrhoden (*)	AR	—	Source [DE]
Genève (*)	GE	—	Source [FR]
Obwalden (*)	OW	—	Source [DE]
Ticino (*)	TI	—	Source [DE]
Switzerland	CH	01.01.2021	Source [EN]

Notes: For each Swiss canton, this table shows whether and when the 2019 Intercantonal Agreement on Public Procurement was adopted into cantonal law. At the federal level, the relevant law is the Federal Act on Public Procurement. Each date refers to the time of entry into force. If no date is provided, a given canton has not adopted the agreement at the time of writing. An adoption date of 01.01.2024 or later means that a canton is treated as a non-signatory in this study since the period under observation ends on 31.12.2023. Furthermore, since the main analysis is performed at the semester level, cantons with reform dates in the second half of 2023 do not have any post-treatment periods and as such also enter the control group. For easier readability, all control group cantons are marked with an asterisk (*).

Table B2: Thresholds for Award Procedures in Cantonal and Municipal Procurement

Award Procedure	Supplies	Services	Construction	
			Ancillary	Main
Discretionary	< 100,000 / < 150,000 [†]	< 150,000	< 150,000	< 300,000
Invitation	< 250,000	< 250,000	< 250,000	< 500,000
Open / Selective	> 250,000	> 250,000	> 250,000	> 500,000

Notes: This table is based on TRIAS (2022). It shows the thresholds (in CHF, referring to the estimated contracted sum) below or above which a given award procedure may or must be applied. For construction contracts, legislation differentiates between the ancillary construction industry (*Baunebengewerbe*) and the main construction industry (*Bauhauptgewerbe*), although the distinction is not relevant to this study. Discretionary awards, in which a procurement agent freely chooses a contractor, and invitations to tender, in which a set of potential contractors is invited to submit an offer, are permitted below the threshold shown in the table. Additionally, they can be employed under special circumstances even when the estimated contracted sum exceeds the relevant threshold; these circumstances include a previous unsuccessful call for tenders, extreme urgency, or follow-up contracts. [†]The 2019 IAPP reform increased this threshold from CHF 100,000 to CHF 150,000 in order to harmonise it with the threshold for services. As such, the lower value applies to non-reform cantons, while the higher value applies to reform cantons once the IAPP has entered into force.

Table B3: Distribution of Sectors Across Administrative Levels

	<i>Means</i>			<i>Differences</i>	
	Municipal	Cantonal	Federal	M vs. C	M vs. F
Office Supplies	0.042 [0.201]	0.056 [0.230]	0.040 [0.196]	0.014** (0.002)	-0.002 (0.003)
Machinery	0.104 [0.305]	0.146 [0.353]	0.082 [0.275]	0.042** (0.003)	-0.022** (0.004)
Other Goods	0.048 [0.215]	0.032 [0.177]	0.039 [0.193]	-0.016** (0.002)	-0.010** (0.003)
Construction Supplies	0.035 [0.184]	0.034 [0.182]	0.009 [0.094]	-0.001 (0.002)	-0.026** (0.002)
Construction Works	0.553 [0.497]	0.525 [0.499]	0.377 [0.485]	-0.028** (0.005)	-0.176** (0.008)
IT Services	0.055 [0.227]	0.079 [0.270]	0.109 [0.312]	0.025** (0.002)	0.055** (0.005)
Consulting Services	0.021 [0.143]	0.039 [0.194]	0.045 [0.206]	0.018** (0.002)	0.024** (0.003)
Maintenance Services	0.018 [0.132]	0.018 [0.134]	0.006 [0.074]	0.000 (0.001)	-0.012** (0.001)
Utilities Services	0.059 [0.236]	0.033 [0.179]	0.021 [0.143]	-0.026** (0.002)	-0.038** (0.003)
Other Services	0.171 [0.377]	0.134 [0.340]	0.381 [0.486]	-0.038** (0.003)	0.209** (0.007)
Observations	23,678	22,995	5,072	46,673	28,750

Notes: This table shows the distribution of sectors across the entire sample for each administrative level. Since the sector classification is based on Common Procurement Vocabulary (CPV) codes, a given contract can belong to multiple sectors, meaning that the sum of the individual percentages exceeds one. Standard deviations are given in square brackets, robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

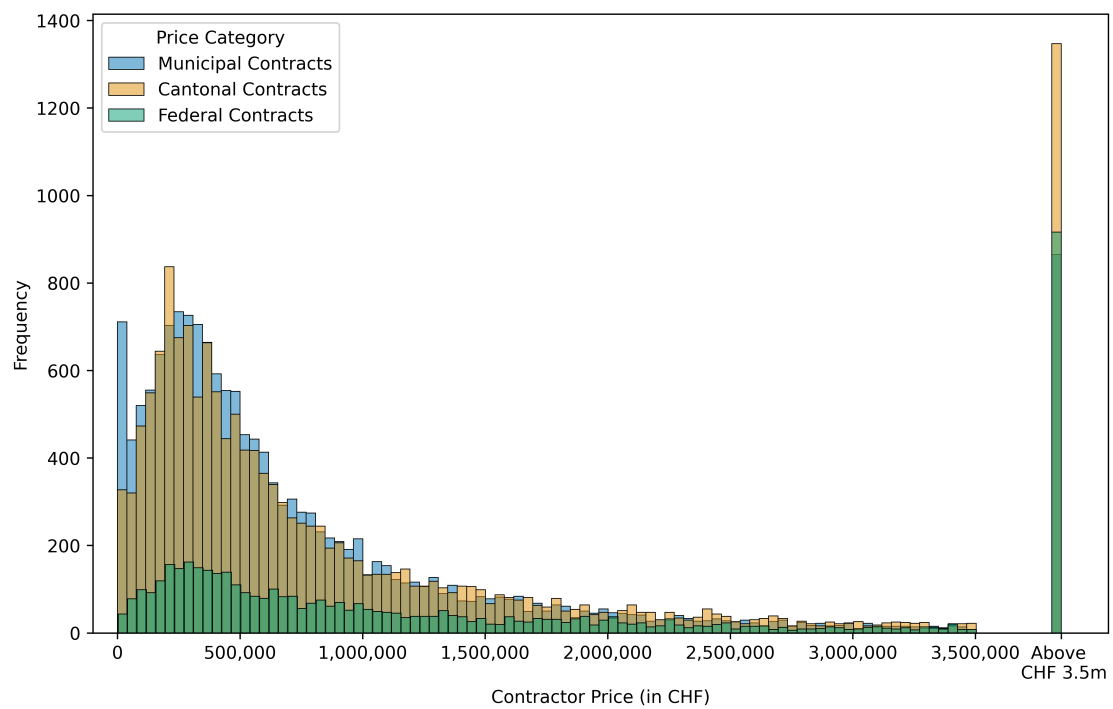
Table B4: Callaway and Sant’Anna (2021) Difference-in-Differences Analysis with Consistent Sample

		By contract type		
	(1) All	(2) Construction	(3) Services	(4) Supplies
<i>Cross-Group Aggregate</i>				
ATT	0.033 (0.027)	0.009 (0.033)	0.149* (0.061)	−0.037 (0.056)
<i>By Reform Wave</i>				
2021–H2	0.084 (0.046)	0.006 (0.056)	0.310* (0.147)	−0.012 (0.087)
2022–H1	0.114* (0.046)	0.110* (0.055)	0.249** (0.091)	−0.015 (0.079)
2022–H2	−0.089 (0.094)	−0.081 (0.126)	−0.014 (0.151)	−0.031 (0.107)
2023–H1	−0.064 (0.037)	−0.055 (0.047)	−0.122 (0.089)	−0.079 (0.096)
<i>Pre-Trend Test</i>				
χ^2 (dof = 17)	20.88	19.38	23.46	11.25
<i>p</i> -value	0.285	0.369	0.173	0.843
Observations	9,709	5,193	2,935	1,581

Notes: This table shows the results of re-estimating Equation (3) using a consistent sample, meaning that municipalities in reform cantons only enter the sample if the report at least one award prior to and at least one award after the reform has come into effect. In all columns, the outcome variable is the share of tenders which was awarded locally. The null hypothesis in the pre-trend test states that all pre-treatment coefficients for each group are equal to 0. Standard errors are shown in round brackets. * $p < 0.05$, ** $p < 0.01$.

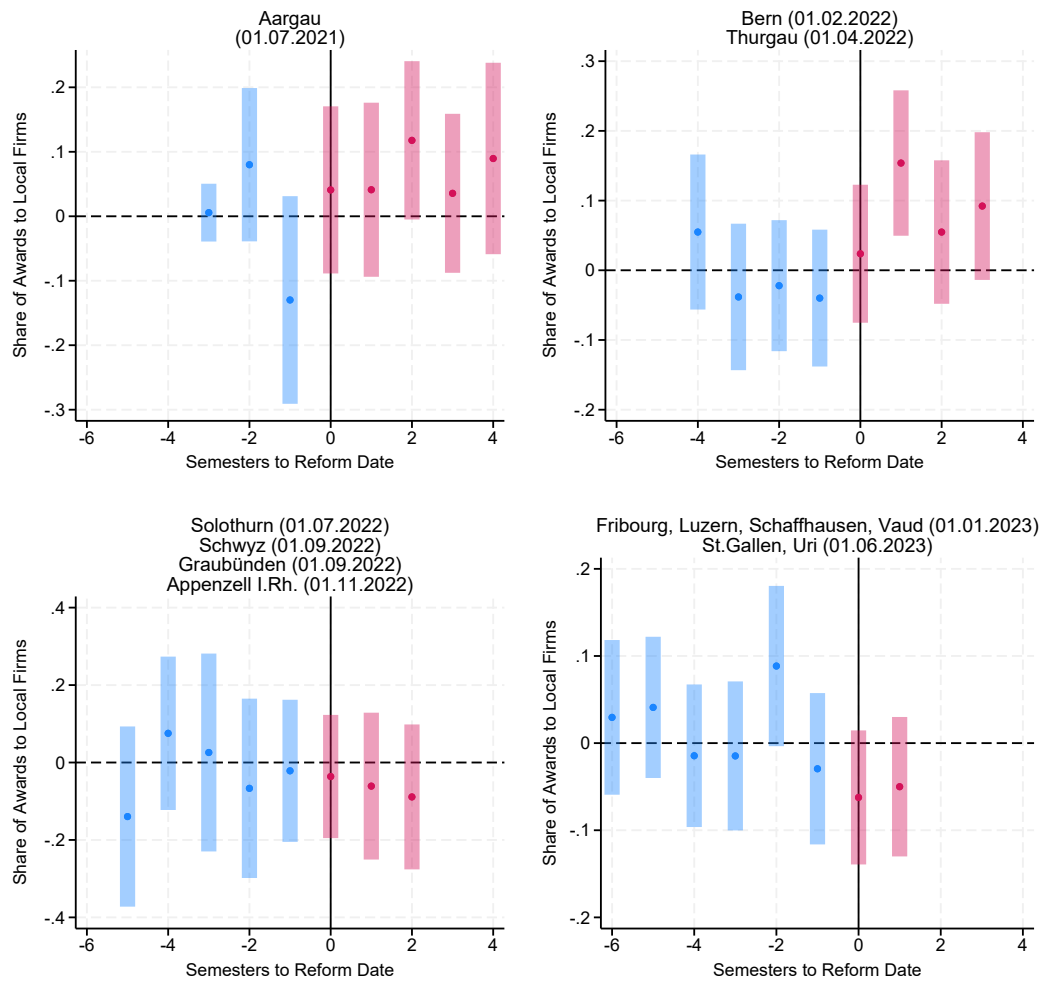
C Supplementary Figures

Figure C1: Distribution of Tender Sizes Across Administrative Levels



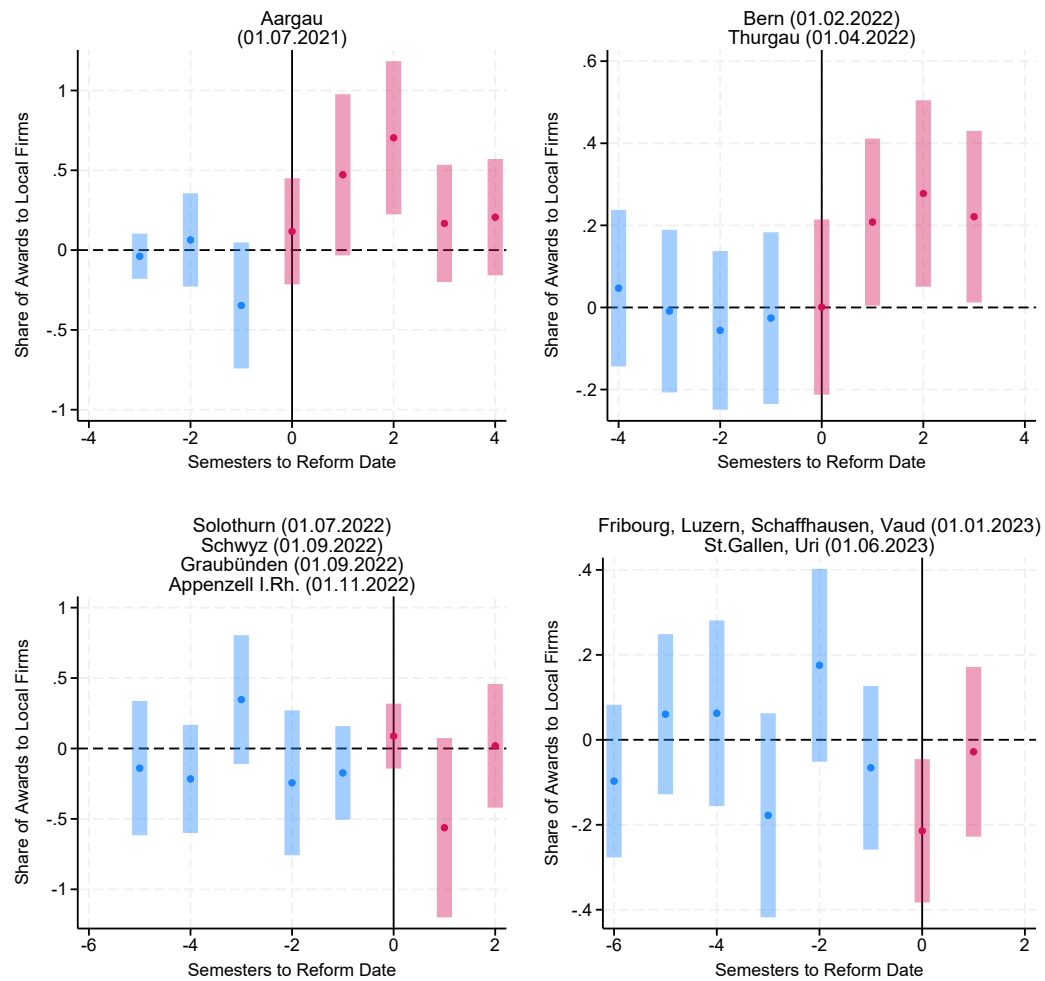
Notes: This figure plots the distribution of contract sizes by administrative level using the price provided in the award notice in Swiss Francs (CHF). In case a price range is given in the tender data, the mean between the lower and upper bound is used.

Figure C2: Difference-in-Differences Analysis of the IAPP Reform by Reform Wave



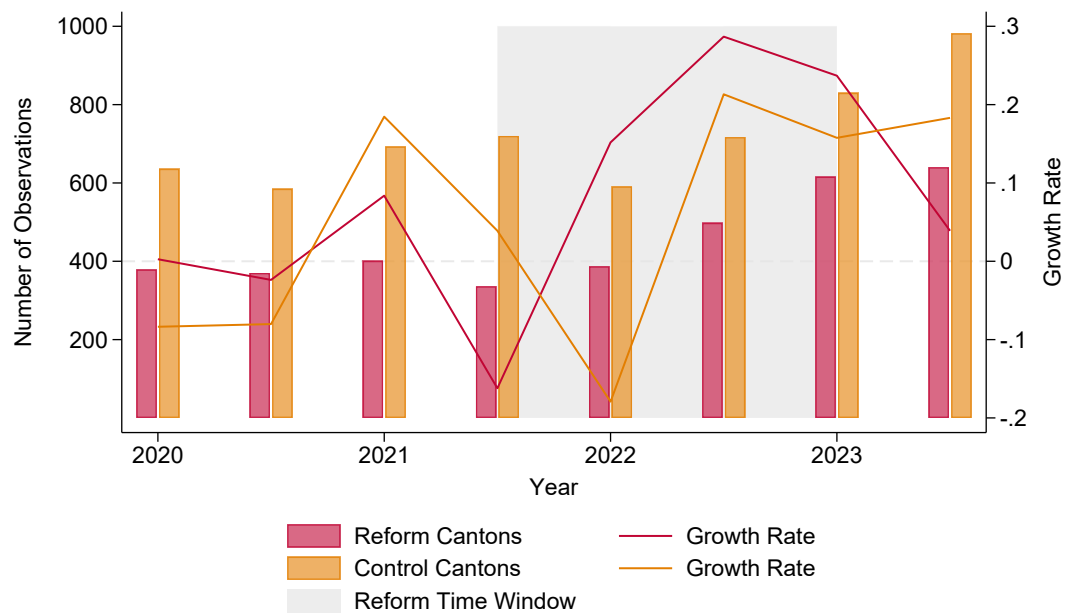
Notes: This figure shows the result of the analysis presented in Panel A of Figure 7 disaggregated by reform wave. Semester 0 refers to the six-month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. All confidence intervals are at the 95%-level.

Figure C3: Difference-in-Differences Analysis of the IAPP Reform by Reform Wave, Service Contracts Only



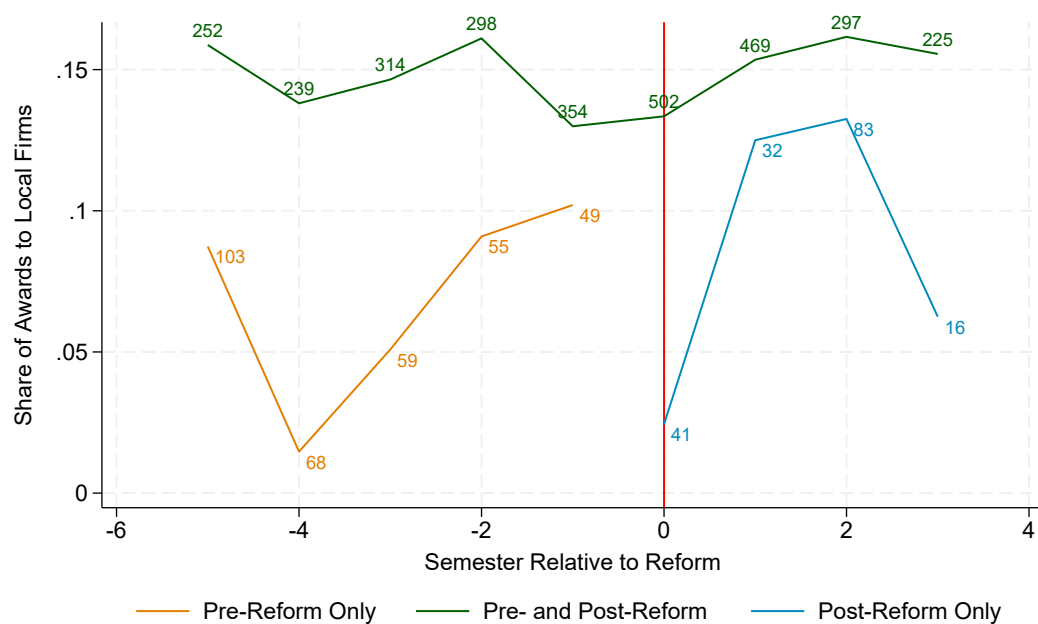
Notes: This figure shows the result of the analysis presented in Panel B of Figure 7 disaggregated by reform wave. Semester 0 refers to the six month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. All confidence intervals are at the 95%-level.

Figure C4: Number of Observations and Observation Growth Rates over Time



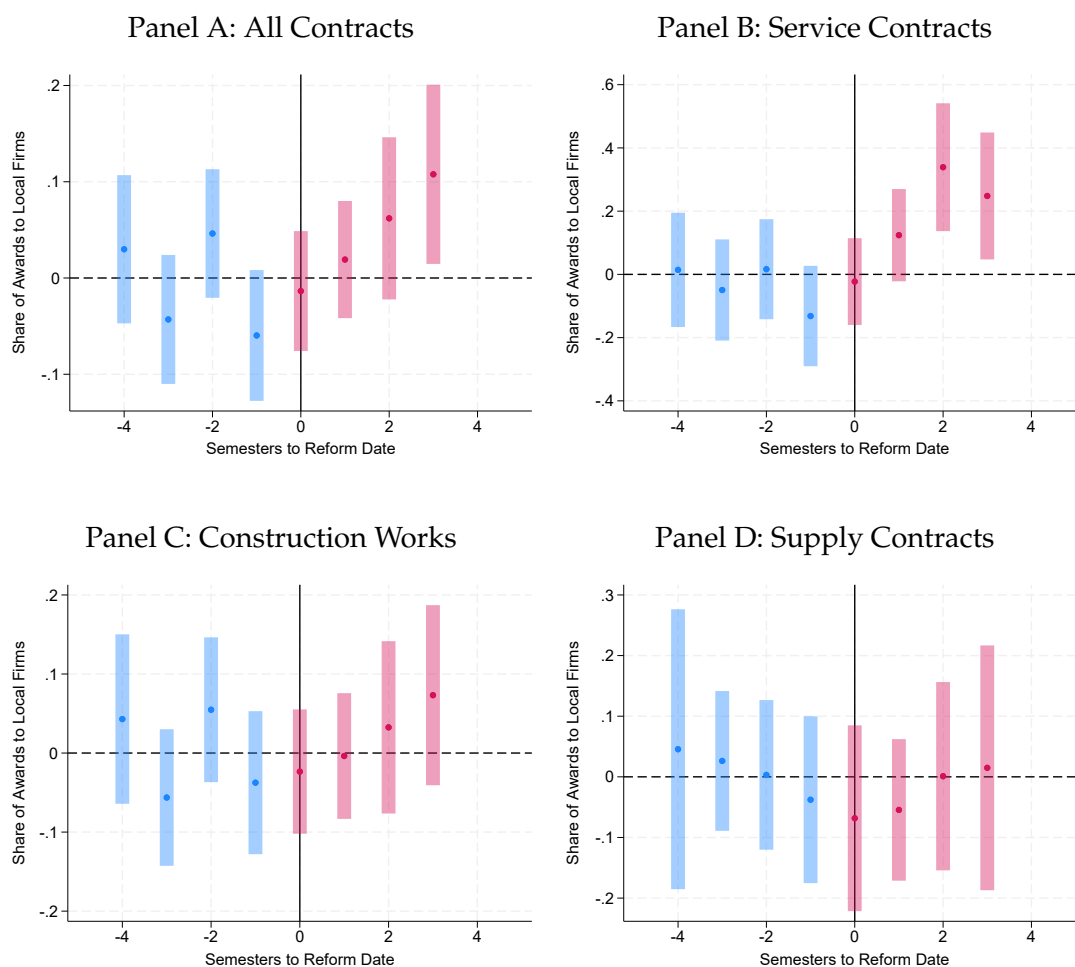
Notes: This figure shows the number of observations per semester (left axis) and the semester-on-semester growth rate (right axis) for reform and non-reform cantons. The period during which IAPP reforms came into force is highlighted in grey.

Figure C5: Share of Local Firm Awards and Observation Numbers for Reform Cantons by Earliest and Latest Reporting Date



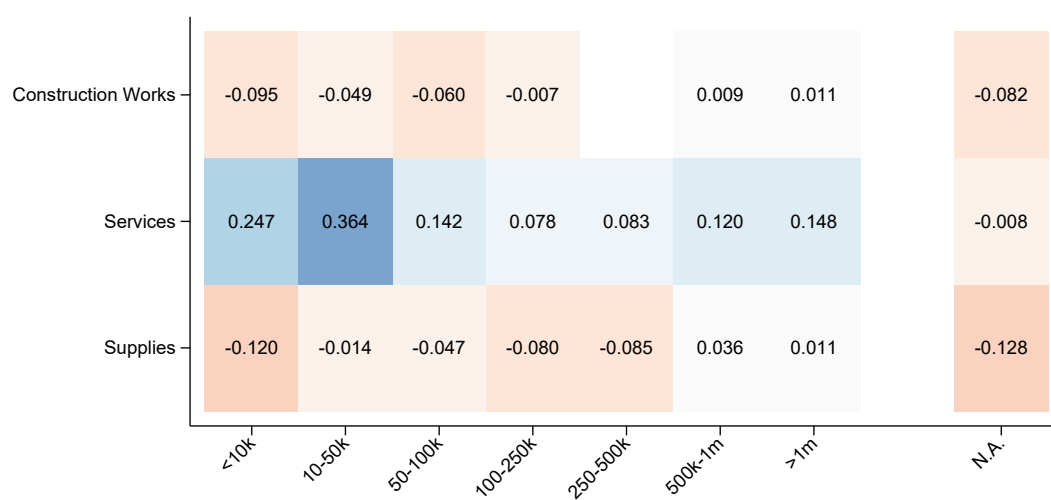
Notes: This figure shows the share of local firm awards in reform cantons for three distinct groups. *Pre-Reform Only* refers to municipalities whose latest tender award was published before the revised IAPP entered into force in their canton. *Post-Reform Only* refers to municipalities whose earliest tender award was published after the relevant reform. Finally, *Pre- and Post-Reform* describes municipalities which published awards both before and after the relevant reform. In order to provide a sense of the weight the three categories have on the final estimates, the number of observations per group and period are printed in the corresponding colour.

Figure C6: Difference-in-Differences Analysis with Consistent Sample



Notes: This figure plots the results of re-estimating the specifications plotted in Figure 7 using a consistent sample, meaning that only municipalities which reported at least once before and once after the relevant reform enter the sample. As before, Semester 0 refers to the six-month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. All confidence intervals are at the 95%-level.

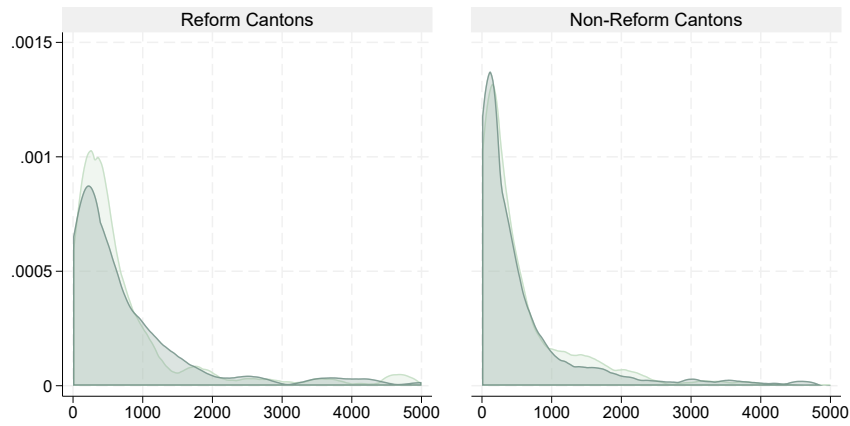
Figure C7: Price-by-Contract Type Disaggregation (Pre-Reform Only)



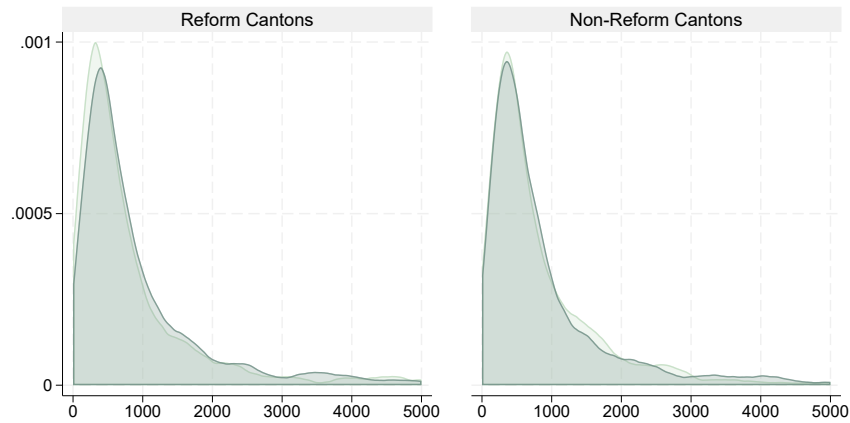
Notes: This matrix reproduces Figure 4 using contract types rather than industrial sectors. As before, the sample is restricted to pre-reform observations, although the results are qualitatively similar regardless of sample choice.

Figure C8: Price Distribution by Contract Type

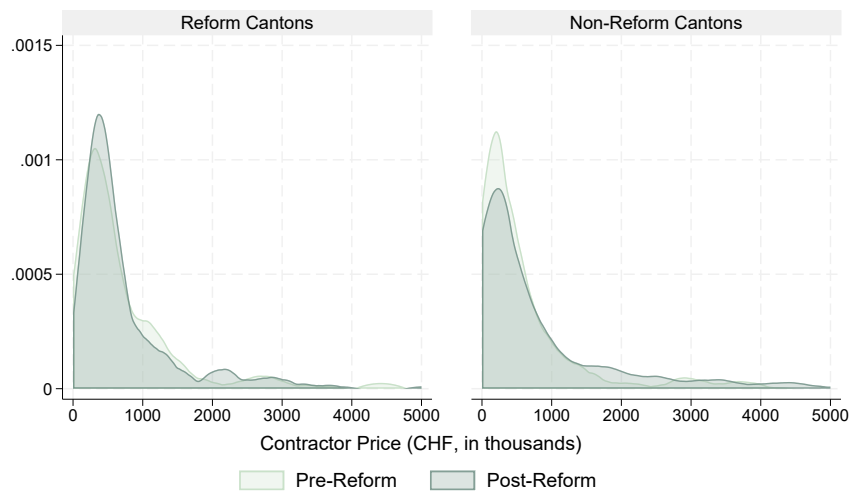
Panel A: Service Contracts



Panel B: Construction Works

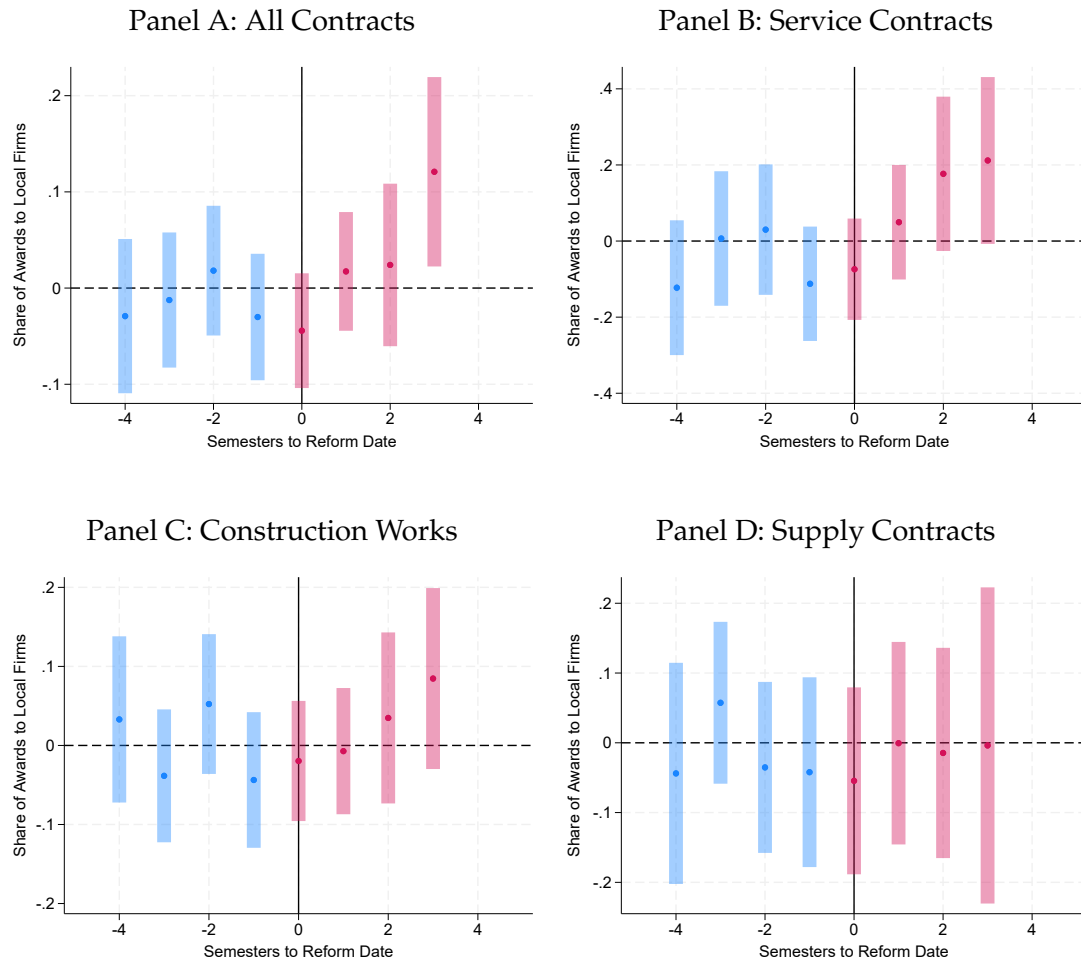


Panel C: Supply Contracts



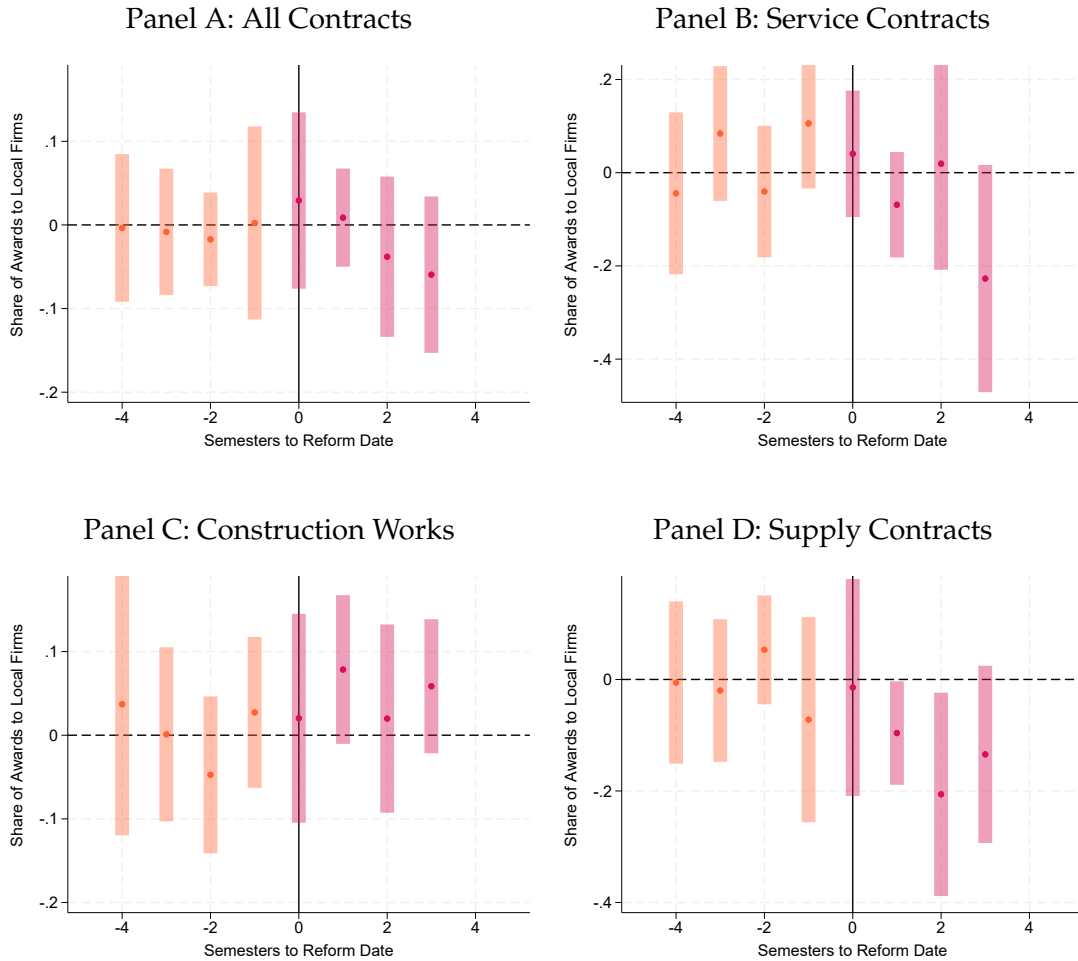
Notes: This figure shows the distribution of prices by contract type. The left axis indicates kernel density. *Pre-reform* refers to all observations from 01 January 2021 until the reform date for reform cantons, or until 01 July 2022 (the mid-point of all reforms) for non-reform cantons.

Figure C9: Difference-in-Differences Analysis with Consistent Sample and Price $\times$ Sector Interaction Terms



Notes: This figure plots the results of re-estimating the specifications plotted in Figure C6 with the addition of price-by-sector terms, that is, adding $P_p \times S_s$ to Equation (3). As before, Semester 0 refers to the six-month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. All confidence intervals are at the 95%-level.

Figure C10: Difference-in-Differences Analysis for Cantonal Tenders



Notes: This figure plots the results of re-estimating the specifications plotted in Figure 7 for cantonal rather than for municipal tenders. The outcome variable is the share of tenders which was awarded locally, meaning that the contracted firm is located in the same municipality as the execution site. The empirical specification is as in Equation (3) with additional tender-level controls, namely language, the activity of the awarding authority, the quarter of the year, whether the procedure was open, and whether it was subject to WTO regulation. As before, Semester 0 refers to the six-month period in which the reform came into force in a given canton, regardless of whether the reform occurred at the beginning or end of the semester. All confidence intervals are at the 95%-level.