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Conference Paper

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Beiträge zur Jahrestagung des Vereins für Socialpolitik 2020: Gender Economics

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Raffer, Christian (2020) : Accrual Accounting and the Local Government Budget - A Matching Evaluation, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2020: Gender Economics, ZBW - Leibniz Information Centre for Economics, Kiel, Hamburg

This Version is available at:

<https://hdl.handle.net/10419/224630>

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Accrual Accounting and the Local Government Budget – A Matching Evaluation

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Abstract

The transition from cash to accrual accounting is said to change a government's perception of its budget quite fundamentally. Although an exorbitant number of governments have reformed the mode of accounting at high costs in past years, reliable empirical evidence of consequences on their financial situation and decision-making is still scarce. In this paper, budget variables are analysed which are hypothesized to react to the reform: investment expenditure and revenue from asset sales. Microdata from 1,100 local governments in the German state of Baden-Württemberg over the period 2005–2016 is exploited with different matching techniques combined with the conditional DiD estimator. Results imply a robust effect on municipal investment behaviour and indicate an impact on sales revenue. This corroborates the latest empirical results. This not only provides external validation. For the first time a common understanding of the budgetary effects of the accrual accounting reform based on econometric analyses seems to be emerging.

Keywords: Accrual Accounting, Propensity Score Matching, Public Finance

1. Introduction

2 In the mid-2000s, many local governments in Germany faced budgetary
3 hardship due to decreasing financial support from higher levels of government
4 and increasing responsibilities (Ridder et al., 2005). Evaluated on the basis
5 of cash credits as indicator for fiscal pressure, for many of them until today
6 not much of this situation has changed (Boettcher et al., 2019). One reaction
7 was the attempt to increase the efficiency of public service provision by im-
8 plementing modern instruments of government (Naschold and Daley, 1999).

9 These reforms were motivated by the New Public Management movement
10 (Hood, 1991, 1995), which found its major German blueprint for municipal-
11 ities in the so-called New Steering Model (Neues Steuerungsmodell) (KGSt,
12 1993; Christiaens and Van Peteghem, 2007). A central pillar was the exten-
13 sion of traditional cash or cameral accounting by accrual components or even
14 full transition into private sector-style accrual accounting.

15 This accrual accounting reform was – and still is – a global phenomenon
16 promoted by international organisations like the IMF or OECD (Ridder et al.,
17 2005; Cavanagh et al., 2016; OECD and IFAC, 2017). In 2015, 57 countries
18 worldwide had implemented either full or modified accrual accounting at
19 the central government level.¹ Of all the OECD members, 73 percent had
20 completed the reform by 2016. The German central government, however,
21 still relies on a cash-based system. Among German state governments so far
22 only Bremen, Hamburg, Hesse and North Rhine-Wesphalia have reformed
23 (Lampe et al., 2015).² Speaking of local governments, within the European
24 Union only Portugal, Italy, Slovenia, Hungary, Denmark and Germany have
25 either both cash and accrual accounting or a mixed system (Pina et al., 2009;
26 Ernst & Young 2012). In 2017, close to 7,000 out of 11,325 German munic-
27 ipalities (61%) had implemented the reform. Due to the federal structure of
28 Germany, modes of local government transition into accrual accounting are
29 decided by each state separately. This causes heterogeneity in reform details
30 and implementation dates (Hilgers et al., 2018). Whereas, for example, in
31 North Rhine-Westfalia municipalities were obliged to reform by 2009, in the
32 south-western state of Baden-Württemberg they were given until 2020 to re-
33 form. Municipalities in Bavaria, Thuringia and Schleswig-Holstein even have
34 an indefinite right of choice (Hilgers et al., 2018).

35 The European Commission estimated reform costs for a medium-sized
36 member country at EUR 50 million for the central government level only
37 (European Commission, 2013). No aggregate cost information for the lo-
38 cal level is available, but considering average transition costs between EUR
39 100,000 and EUR 200,000 per German local government (Weiss, 2014), bil-
40 lions of tax money must have been devoted to this reform trend since the
41 early 2000s. It is no surprise that private consulting companies are influential

¹Between pure cash and pure accrual accounting there are also hybrid forms like mod-
ified accrual or modified cash accounting. For details see Lande and Rocher (2011).

²In this paper, the 16 German 'Länder' are called 'states'.

42 promoters (Seiwald and Meyer, 2012).

43 Surveys among local politicians and public employees repeatedly revealed
44 a critical but mostly supportive perception of the reform (Weiss, 2014; Hilgers
45 and Burth, 2012; Hilgers et al., 2017, etc.). Academic discussion is contro-
46 versial, too. Several scholars relate major benefits to accrual accounting,
47 which circle around increased effectiveness, efficiency, transparency and ac-
48 countability (see e.g. Budäus, 2009; Burth and Hilgers, 2014; Anessi-Pessina
49 and Steccolini, 2007; Arnaboldi and Lapsley, 2009). Accrual information is
50 said to disclose the financial situation of governments more accurately and to
51 improve managerial and political decision-making. Moreover, it is assumed
52 to allow for better assessment of current and future risks and to increase
53 intergenerational equity as well as transparency for citizens. For the critics,
54 Guthrie (1998) points out that accrual accounting’s view on cost and effi-
55 ciency and its ‘neoclassical’ idea of performance might not be suitable for the
56 public sector. From this perspective, cameralistic cash accounting provides
57 sufficient information whereas accrual accounting is too complex to be used
58 within the political-administrative system (e.g. Brorstroem, 1998; Robinson,
59 1998; Monsen, 2002; Mellett, 1997). Moreover, costs for implementing and
60 running the new system are considerable (Carlin, 2006).

61 With this debate in mind it seems quite astonishing that so far only
62 few researchers have ventured to pin down the impacts of the accrual ac-
63 counting reform by means of modern econometrics. In terms of budgetary
64 impacts, first studies have only recently been published (Dorn et al., 2019;
65 Christofzik, 2019). This analysis adds to the emerging field by evaluating the
66 effect of the accrual accounting reform using budget data from municipalities
67 in the German state of Baden-Württemberg. It is the first paper which im-
68 plements a quasi-experimental approach with different matching techniques
69 (three propensity score matching methods and entropy matching) combined
70 with the conditional DiD estimator. Variables of interest (per capita in-
71 vestment expenditure and sales revenue) have been deducted by a rigorous
72 analysis of reform-related changes in municipal accounting practices. Results
73 imply a robust effect on municipal investment behaviour and point to lower
74 sales revenues after implementation compared to the control group. Both are
75 in line with recent publications and contribute to a just-emerging common
76 understanding of how this large-scale reform alters the financial behaviour of
77 local governments.

78 The article is structured as follows: section 2 summarises the current state
79 of the empirical literature; section 3 introduces the institutional setting for

80 the Baden-Württemberg accruals reform and deduces research hypotheses,
81 section 4 rolls out the empirical strategy and the dataset. In sections 5 and
82 6 results are displayed and discussed; section 7 concludes.

83 **2. Existing Empirical Research**

84 Surveys among practitioners and local politicians about the accruals re-
85 form are abundant. With semi-structured interviews in several German
86 states Jagalla et al. (2011) identified a taxonomy of benefits like enhanced
87 transparency over the long term or increased cost awareness. Burth and
88 Hilgers (2012) surveyed the attitudes of 423 German local politicians and
89 public officials in local finance departments and found support among the
90 majority of respondents. Another survey conducted a few years later by
91 Hilgers et al. (2017) corroborated these initial findings. Similar conclusions
92 are drawn by Riemenschneider (2014) who surveyed 452 local politicians in
93 North Rhine-Westfalia in 2013, four years after implementation. An earlier
94 survey in the same state, conducted only a few months after the final im-
95 plementation deadline, brought about more critical results (Bogumil et al.,
96 2011; Bogumil and Holtkamp, 2012), which may be motivated by missing
97 application experience. The same is true for a survey presented by the
98 Rhineland-Palatinate court of auditors in 2011 (Rechnungshof, 2011) and
99 for a German-wide survey published in 2010 by the municipal association
100 KGSt which reports an on average slightly positive practitioner assessment
101 of reform benefits (KGSt, 2010). In 2010, the German association of cities
102 surveyed its members' evaluations and experiences (Articus and Wagner,
103 2011). Results point to implementation costs as one major obstacle that was
104 predominantly mentioned by those cities which had not yet reformed their
105 accounting. This provides an important insight into the relevant drivers of
106 implementation. As the most important advantage, higher transparency in
107 terms of depreciation and consequences of current fiscal behaviour was men-
108 tioned.

109 Paulsson (2006) surveyed Swedish central government agencies and found
110 that accrual information is most intensely used in times of financial distress,
111 which might point to its corrective role. Andriani et al. (2010) sent out
112 a questionnaire among public officials in Western Australia and identified a
113 perception of higher usefulness of accrual accounting information compared
114 to cash accounting. A large-scale survey among agents at different levels of
115 government conducted by Kober et al. (2010) for the same country provided

116 similar results. The outcome of a rather small survey among local Scottish
117 decision-makers in the late 2000s (Arnaboldi and Lapsley, 2009) was that
118 the implementation of accrual information did not alter interest or improve
119 management decisions. In Greece, where in 2013 local governments operated
120 with both cash and accrual means, Cohen et al. (2013) found that cash-based
121 information was still central in the organisational context of Greek munici-
122 palities but accrual information was also considered in decision-making. In
123 a similar institutional setting, Kobayashi et al. (2016) surveyed the opinion
124 and experience of public employees in financial departments of Japanese mu-
125 nicipalities and found little relevance of accrual information as a supplement
126 to cash accounting. Anessi-Pessina et al. (2008) asked for drivers of imple-
127 mentation among Italian municipalities and identified regional location and
128 preparers' reform perception as relevant.

129 Closely linked to this survey literature is a strand of research that analyses
130 the drivers of benefits by feeding survey responses into structural equation
131 models (Burth and Hilgers, 2014; Hilgers and Burth, 2015). Among local
132 politicians, higher effectiveness and improved management capabilities seem
133 to be most important. In a similar approach, Hirsch et al. (2015) find that
134 a municipal management accounting department has a higher moderating
135 effect on administrative performance if it applies accrual accounting. In
136 a repeated cross-sectional analysis Christiaens (2007) investigated potential
137 drivers of reform implementation at the local level for Flemish municipalities
138 (Christiaens and Van Peteghem, 2007) and found only a limited impact of
139 most analysed variables (e.g. staff training, municipality size, etc.). Even
140 the experience level of local treasurers seems to have had no impact. With
141 cross-sectional regression analysis, mainly US scholars tried to answer the
142 question of whether a municipality that publishes accrual instead of cash
143 or other financial information benefits in terms of lower credit costs, which
144 seems to be the case (Baber and Gore, 2008; Reck and Wilson, 2014), and if
145 it provides an accurate picture of its' default risk (Plummer et al., 2007). In
146 the European context, Bastida et al. (2014) implement a GMM framework
147 in order to evaluate the relation of quality of accounting information and
148 interest rates for municipal bank loans in Spain.

149 For the German local level, two publications have so far analysed the ef-
150 fects of the accruals reform on municipal performance. Lampe et al. (2015)
151 estimate a cost frontier and assess whether accrual accounting improves public
152 sector efficiency among those cities in North Rhine-Westfalia which switched
153 recently. Gärtner (2014) conducts a stochastic frontier analysis of munic-

154 ipalities in Lower Saxony. Both publications present results that indicate
155 an efficiency-enhancing effect. Research interest in the budgetary impact
156 of the accruals reform at the German local level rose lately. Dorn et al.
157 (2019) apply difference-in-differences estimation/event studies for Bavarian
158 county data and find a non-significant reduction of investment expenditure
159 but a significant drop in revenues from non-financial assets after the reform.
160 These findings match those of Christofzik (2019) who estimates difference-
161 in-differences effects with a panel of German-wide local government budget
162 data, aggregated at the state level.

163 To sum, following survey literature, local practitioners and politicians
164 seem to perceive transition into accrual accounting as being useful – at least
165 after having collected some experience with this new mode of accounting.
166 Also, more advanced econometric results indicate positive effects on prac-
167 titioners work, municipal credit costs, and government efficiency. However,
168 even after at least three decades of increased reform activity little is known
169 about its impact on the local financial situation. Only very recently, pub-
170 lications of Dorn et al. (2019) and Christofzik (2019) have started to fill
171 this gap. But still, Carlin’s (2005) and Christensen’s (2009) call for a deeper
172 investigation of the accrual accounting effects on budget outcomes remains
173 valid. This contribution adds to this field.

174 **3. Institutional Setting and Research Hypotheses**

175 In 2003, German state ministers of the interior decided to implement ac-
176 crual accounting at the local level (IMK, 2003). Since this agreement was
177 based on each states’ freedom to develop its own reform, nowadays there is
178 some state-specific heterogeneity of accrual accounting practices at the mu-
179 nicipal level (Hilgers et al., 2018). Consequently, analyses usually concentrate
180 on single states with internally homogeneous institutions. Due to their em-
181 pirically interesting cumulative transition pattern (see Figure 1), this study
182 focusses on municipalities in Baden-Württemberg. The regional system of
183 accrual accounting is called ‘New Local Budgeting, Accounting and Report-
184 ing System’ (Ridder et al., 2005) and the corresponding law passed the state
185 parliament in April 2009. It obliged municipalities to change the mode of
186 accounting from cash to accrual by 2016. Due to considerable inertia another
187 law came into effect in 2013 that prolonged the transition period until 2020.
188 Reasons for this reluctance were hopes to benefit from earlier adopters’ expe-
189 riences, reform complexity and cost intensity in combination with a general

190 lack of qualified personell (Landtag von Baden-Württemberg, 2019; Hilgers
 191 and Burth, 2012). It is the local council which has to take the fundamen-
 192 tal decision to reform and the administration which is responsible for any
 193 subsequent technical implementations.

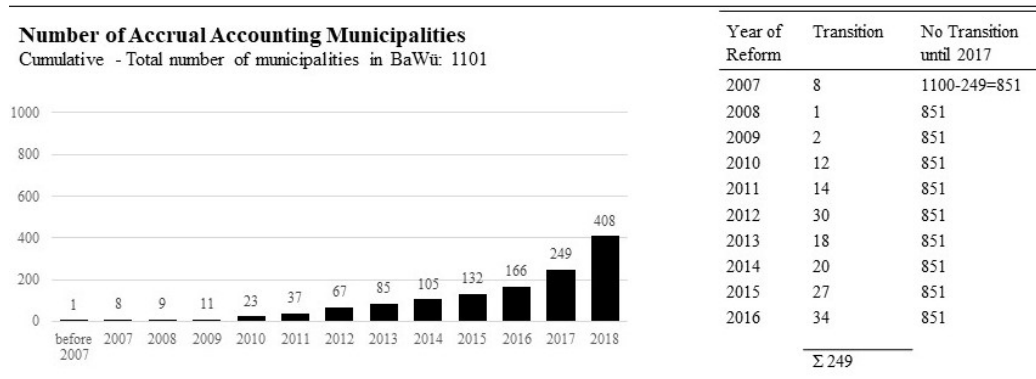


Figure 1: Although Baden-Württemberg has 1,101 municipalities, one was excluded since it only came into existence after 2009. Some pilot municipalities shifted their mode of accounting even before the law came into effect in 2009 (own graph).

194 Following the Baden-Württemberg state ministry of the interior, switch-
 195 ing from cash to accrual is a 'change of paradigms' for municipal budgeting
 196 and accounting (Ministry of the Interior, 2019). Technically speaking, the
 197 traditional cash accounting system consists of an activity side for inflows and
 198 a payment side for outflows, both parts of the capital and the operational
 199 budget respectively (Monsen, 2002).³ Contrary to that, the accrual system
 200 consists of three major components: (1) the balance sheet which covers as-
 201 sets and debts, (2) the financial statement which covers cash flows, and (3)
 202 the profit and loss statement which covers expenditure and revenue (KGSt
 203 and Bertelsmann Foundation, 2010). The results of the financial and the
 204 profit and loss statement contribute to the balance sheet; budget balance is
 205 determined in the profit and loss statement.

206 The reform shifts the focus from simple in- and outflows of money when
 207 transactions are executed to a broader perception of public financial man-

³Whereas the operational budget covers cash flows related to daily operations like receipt of tax revenue or payment of wages, the capital budget covers cash flows related to investment.

208 agement that links those money flows to specific public services/assets and
209 to the appropriate financial year; moreover, it considers depreciation and
210 implicit debt (Hilgers et al., 2018). Whereas, for example, a new municipal
211 building appears in a cash accounting system only once in the capital budget
212 (as outflow of money when the initial investment is made), accrual account-
213 ing additionally considers it an asset with future loss of value. Depreciation,
214 of course, is only possible if the municipality knows about its assets and their
215 value. A purely cash accounting municipality has no detailed valuation of
216 its assets. In case of reform the first step is to take inventory. A further
217 major difference between cash and accrual accounting appears in terms of
218 explicit and implicit debt. Cash accounting only captures current debt that
219 corresponds to an actual inflow of borrowed money (explicit debt). Accrual
220 accounting additionally considers payment obligations that have not yet ma-
221 terialised like future pension payments for current public employees (implicit
222 debts). Under accrual accounting, accruals have to be made for implicit debt
223 (Ridder et al., 2005).

224 Knowledge of explicit and implicit debt plus full information about the
225 future values of today's assets allow for a comprehensive assessment of in-
226 tertemporal financial health. Whereas a cash accounting municipality's bud-
227 get is balanced as soon as all actual outflows of money in a given period are
228 covered by inflows in the same period, an accrual accounting municipality's
229 budget balance requires that available resources in each period fully cover
230 resource consumption, which comprises net outflows of money plus deprecia-
231 tion plus accruals made for future payment obligations (Budäus, 2009). This
232 is represented by a balanced profit and loss statement, which municipalities
233 in Baden-Württemberg have to accomplish in the medium term. Unsur-
234 prisingly, survey research indicates that public financial managers perceive
235 an accrual budget balance as harder to reach (e.g. Riemenschneider, 2014).
236 Considering these systematic differences, many potential research hypothe-
237 ses could be formulated. This analysis focusses on the most straightforward:
238 reform effects on budget indicators.

239 The first hypothesis is linked to depreciation and assumes a change of
240 investment incentives for local decision-makers. Figure two exemplifies the
241 idea with an investment in a long-lasting machine that is bought in year one
242 at a price of 2,000. It is assumed that the machine will depreciate linearly
243 over 10 years. Under cash accounting, only the actual payment of 2,000 in
244 year one appears in the capital budget. Under accrual accounting, however,
245 the picture is more detailed. In year one, the financial budget shortens by

		Cash Accounting		Accrual Accounting		
		Operating Budget (VwH)	Capital Budget (VmH)	Balance Sheet	Profit and Loss Budget	Financial Budget
Acquisition of investment good	Costs of Euro 2000		-2000	+2000 -2000		-2000
Annual	Depreciation				-200	
Sale of investment good after six years	Revenue of current value (800)		+800	-800 +800		+800

Figure 2: Differing systematics of investment expenditure, depreciation and sales revenue in cash versus accrual accounting (Ministry of Interior Saxony, 2010).

246 spending the sum of 2,000 whereas the balance sheet does not change: the
247 increasing asset position is fully counteracted by the decreasing financial
248 position. In subsequent years, however, there is a yearly depreciation of -
249 200, which is an additional annual burden for the profit and loss budget
250 and its balance. From the perspective of cash accounting, the local decision-
251 makers' most likely consideration will be: Is there enough money to afford
252 the machine in year one? From the perspective of accrual accounting, the
253 decision-maker additionally has to ask her-/himself: will there be enough
254 money in the coming 10 years in order to account for annual depreciation
255 under the restriction of a balanced budget? The financial burden induced
256 by the newly acquired machine spreads out over time and complicates the
257 balancing of the budget in the years to come. For local decision-makers – like
258 municipal mayors thinking of their future re-election – this might create an
259 incentive to drop or defer investments since voters tend to punish deficits in
260 election years (Drazen and Eslava, 2010; Brender and Drazen, 2008; Brender,
261 2003). Offsetting those deficits by reducing services or increasing taxes is
262 equally unattractive.

263 **H1:** After the transition from cash to accrual accounting, municipal
264 investments in long-lasting assets like buildings or roads decrease.⁴

⁴Obviously, H1 could also be formulated the opposite way (after the reform, investments in long-lasting assets increase). After all, under accrual accounting the budget balance is not exposed to a one-time high payment any longer as it was under cash accounting but to lower and evenly spread-out burdens in the future – a future in which the decision-maker

265 Hypothesis 2 builds upon the changing budgetary impact of a municipal
266 sale of assets (Ministry of Interior Saxony, 2010). Referring to the example
267 above, our machine is sold after six years for a price reflecting its remaining
268 value of 800. In a cash accounting municipality, this creates a positive in-
269 flow of cash in the capital budget in year six, nothing else. For an accrual
270 accounting municipality, however, the inflow of cash in the financial budget
271 in year six is offset by decreasing assets in the balance sheet. Hence, whereas
272 revenues from asset sales in the cash accounting municipality could be fully
273 used to balance a budget deficit, this option becomes impossible once a mu-
274 nicipality switches to accrual accounting because the transaction does not
275 show up in the profit and loss budget as long as the asset is not sold at a
276 price higher than its current value (in this case, only the margin would con-
277 tribute to budget balancing). Consequently, local politicians may experience
278 a weaker incentive to sell assets after the reform.

279 **H2:** After the transition from cash to accrual accounting, municipal
280 revenues from asset sales decrease.

281 This study aims to investigate these two hypotheses. As outlined above,
282 both are motivated by technical changes of public accounting practices in-
283 duced by the accruals reform. Chapter four provides information about data
284 and methods.

285 4. Data and Method

286 Hypotheses are investigated with a fully balanced panel of annual data
287 (2005 to 2016) from 1,100 municipalities in Baden-Württemberg. It com-
288 prises official municipal budget data but also political and socio-economic
289 variables. Referring to the research question the relevant variables of in-
290 terest⁵ are municipal investments in movable/immovable assets as well as
291 municipal revenues from sales of movable/immovable and financial assets.

may not be held accountable anymore. This relates to the popular idea of a deficit bias as outlined by, e.g., Wyplosz (2013). However, since municipal mayors' incumbencies often last decades and re-election is of high relevance, potential costs of losing voter support are assumed to outweigh the benefits of burden shift.

⁵For the sake of simplicity, "variable of interest" is abbreviated to "VOI" in the remaining text.

292 All data was either provided by the Baden-Württemberg Bureau of Statis-
 293 tics or scrapped from its online data warehouse. Since a municipality starts
 294 to report accrual information once it has transitioned, the Bureau of Statis-
 295 tics re-transforms accrual data into its former cash accounting equivalent.
 296 This allows for comparability over the whole period of interest. Neverthe-
 297 less, considering the error-proneness of municipal reporting immediately after
 298 the implementation of the new practices, data from the first one to two years
 299 after reform needs to be handled with caution.

300 To prevent results from being driven by a few municipalities with extreme
 301 developments in the VOI, outliers were deleted before matching.⁶ This does
 302 not change the results of this analysis, it only compresses estimated effects.
 303 With reference to the standard Rubin Causal Model (Rubin, 1974) the most
 304 important effect of interest is the average treatment effect on the treated
 305 (ATT) which is the average effect of switching to accrual accounting on those
 306 municipalities which effectively reformed. For its estimation the following is
 307 considered:

$$ATT = E[Y_{i1}|D_i = 1] - E[Y_{i0}|D_i = 1] \quad (1)$$

308 where D_i is the reform dummy of municipality i being 1 for reform munici-
 309 palities and 0 otherwise (Caliendo and Kopeinig, 2008; Lin, 2010). $Y_{i0}|D_i = 1$
 310 is the counterfactual value of the VOI that would have been observed if a re-
 311 form municipality had not implemented accrual accounting, and $Y_{i1}|D_i = 1$
 312 the variable actually observed in the very municipality. What makes esti-
 313 mation of ATTs a complicated venture is the unobservability of the coun-
 314 terfactual. Only if a municipality's choice to take on accrual accounting
 315 is completely random one can calculate the ATT by comparing the sample
 316 means of the group of reform with the group of non-reform municipalities.
 317 In the absence of random selection into treatment this, however, would yield
 318 a bias. Especially, if the choice to reform is systematically correlated with
 319 observable variables that also affect the outcome variable.

320 In order to overcome the "selection on observables problem", this paper's
 321 empirical strategy is based upon propensity score matching. The idea is to
 322 use a control group to mimic a randomised experiment. Central for this ap-

⁶A municipality is defined as an outlier in a certain variable if its 2010-2015 change lies beyond 1.5 times the interquantile range of the distribution of changes added at the first and the third quantile (Tukey, 1977).

323 proach is the conditional-independence assumption ($Y_0, Y_1 \perp D|X$) requiring
 324 that the outcome VOI is independent of the reform dummy conditional on
 325 the chosen set of matching covariates.⁷ If so, equation (1) becomes:

$$ATT = E[Y_{i1}|D_i = 1, X_j] - E[Y_{i0}|D_i = 0, X_j] \quad (2)$$

326 in which the second part on the right-hand side is now observable. In
 327 order to avoid the high-dimensionality problem of matching upon a too large
 328 set of covariates X_j it is implemented with the help of propensity scores
 329 (Rosenbaum and Rubin, 1983). These are the probabilities of treatment
 330 assignment conditional on relevant observed characteristics (Austin, 2011)
 331 estimated with the logit specification

$$y_i = \beta_0 + \beta_1 * Z_{ik} + \beta_2 * X_{ij} + \epsilon \quad (3)$$

332 in which y_i is a 0/1 dummy indicating treatment of municipality i and
 333 Z_{ik} is the model-specific VOI in all k available pre-treatment years. Since
 334 2011/12 are the relevant years of reform, the period of pre-treatment years
 335 ends in 2010. Remaining covariates in vector X_{ij} are measured pre-treatment
 336 in 2010, too. Model specification followed the rationale of reaching the high-
 337 est possible level of covariate balance of treated and controls. In the first
 338 model, the VOI is aggregate municipal investment in movable plus immov-
 339 able assets, in the second model it is only investment in immovable assets and
 340 the third model captures movable assets. The fourth model's VOI is aggre-
 341 gate revenue from sales of immovable, movable and financial assets; models
 342 5, 6 and 7 zoom into the three components separately.

343 The choice of covariates X_j , which potentially determine both the like-
 344 lihood of treatment and the outcome variable was based upon existing re-
 345 search. Following survey and qualitative results, reform costs is a decisive

⁷The conditional independence assumption requires that not only those municipalities implement the reform which benefit most from doing so. If, for example, only those municipalities which have fewer investment needs adopt the reform, the comparison of reform and non-reform municipalities in terms of their investment would be biased. The same is true if only the highly indebted municipalities decide to reform since high levels of debt are likely to be correlated with investment behaviour. Therefore, it is necessary to control for those covariates, which in turn requires their observability. By assumption, conditional to all the covariates correlated to selection and outcome, assignment to treatment is random, does not correlate with the outcome anymore and municipalities are equivalent in their remaining characteristics.

346 factor driving (quality of) implementation (Weiss, 2014; Articus and Wagner,
347 2011; Ridder et al., 2005). Keeping this in mind, it seems plausible that the
348 financial situation of a municipality is crucial when it comes to the decision
349 of whether to reform or not. Moreover, the financial situation is crucial for
350 municipal investment and sales behaviour. Included variables proxy not only
351 direct fiscal health of a municipality but also its expenditure and revenue
352 situation:

- 353 • **Primary Balance 2010 p.c.** - direct indicator of fiscal health
- 354 • **Total Municipal Debt 2010 p.c.** - direct indicator of fiscal health
- 355 • **Gross Business Tax Revenue 2010 p.c.** - indirect indicator of fiscal
356 situation (revenue side)
- 357 • **Total Expenditure 2010 p.c.** - indirect indicator of fiscal situation
358 (expenditure side)
- 359 • **Age Ratio** - indirect indicator of fiscal situation (expenditure needs)

360 Debt, expenditure and age ratio⁸ are rather persistent in their develop-
361 ment over time, it is therefore irrelevant if the pre-reform year 2010 or earlier
362 years are considered. In addition, replacing the age ratio with the youth ratio
363 as an indicator of expenditure needs does not change the results. Although
364 the development of the primary balance is more erratic, using 2008 and/or
365 2009 values also did not fundamentally change the results. As for business tax
366 revenue, data restrictions limit the analysis to the pre-treatment year 2010.
367 The covariate also captures the overall economic activity in a municipality.

- 368 • **Inhabitants 2010** - measures municipality size

369 The number of inhabitants is not only relevant for imposing balance in
370 terms of municipality size among treated and controls, it is also indispens-
371 able since it covers the relation of existing human capital and reform imple-
372 mentation. Existing research suggests that larger municipalities have more
373 well-trained employees at their disposal who are capable of implementing

⁸Age/youth ratio: share of inhabitants older than 65/younger than 20 over all inhabi-
tants aged between 20 and 65.

374 accounting innovations (Geißler and Niemann, 2017; Christiaens and Van
375 Peteghem, 2007). Moreover, Shipan and Volden (2008) point to a vital role
376 of city size in local policy diffusion. At the same time, larger sample municipi-
377 palities have, on average, more expenditures per capita (see Table 1), which
378 indicates a more diversified public infrastructure. This relates municipality
379 size to investment expenditure and sales revenue.

- 380 • **Leftist Share** - measures ideology in the municipal council
- 381 • **Voter Turnout** - measures citizens' participation in local politics and
382 proxies the demand for transparent information

383 Finally, two political covariates have been included. Considering the
384 claimed advantage of increasing transparency after the implementation of
385 accrual accounting (Budäus, 2009), in municipalities with higher citizen par-
386 ticipation (proxied by voter turnout in the municipal election previous to the
387 reform) the demand to reform might be higher. Moreover, there is evidence
388 that government ideology has an impact on policy diffusion and budgetary
389 outcomes (Solé-Ollé, 2006; Grossback et al., 2004). Therefore, the seat share
390 of leftist parties within the local government is considered as well.

391 Not considered are neighbouring effects since the geographical distribu-
392 tion of reform municipalities in 2012 does not indicate the like. Moreover,
393 reform perception of local political decision-makers is neglected. The existing
394 literature (Christiaens and Van Peteghem 2007; Kobayashi et al. 2016) and
395 the analysis of actual reform perceptions of responsible decision-makers in
396 Baden-Württemberg municipalities indicate that reform perceptions are no
397 reliable predictor for actual implementation (both variables are discussed in
398 more depth in Appendix 1).

399 Those 44 municipalities which switched from cash to accrual accounting
400 in 2011/12⁹ build the treatment group whereas the 851 municipalities not
401 having implemented the reform until 2017 form the non-treatment group
402 (see Figure 1).¹⁰ Mean comparison of covariates observed in in treatment

⁹The years 2011/12 lie within an institutionally stable period between the implemen-
tation of the law in 2009 and its adaptation in 2013. Moreover, the number of reform
municipalities was higher than in other years within this period (see Figure 1).

¹⁰Except the 44 treatment municipalities of 2011/12, 205 further municipalities imple-
mented the reform between 2007 and 2017. They are dropped from the matching dataset.
Taking into account one more municipality which came into existence only in 2009 after
a municipal merger, Baden-Württemberg had 1,101 municipalities in 2017.

403 and non-treatment group before matching (Table 1) reveals that compared
 404 to non-treatment municipalities the reform municipalities of 2011/12 were on
 405 average larger in population size and had accumulated higher levels of public
 406 debt per capita in 2010. Moreover, higher levels of net business tax revenue
 407 per capita indicates more intense economic activity. Whereas voter turnout
 408 in the local election prior to the transition were lower in reform municipalities,
 409 the share of council seats won by left-wing parties was higher. Summarising
 410 these differences, in 2011/12 the more urban parts of Baden-Württemberg
 411 transitioned to accrual accounting.

	Treated (44)	Untreated (851)
Inhabitants 2010	34012	6237
Age Ratio 2010	32.53	31.55
Voter Turnout	52.08	57.77
Leftist Share 2010	27.87	15.12
Business Tax Revenue 2010 (pc)	361.8	299.2
Municipal Debt 2010 (pc)	1079.64	629.69
Municipal Expenditure 2010 (pc)	2719.59	2585.97
Primary Balance 2010 (pc)	-27.31	-42.57

Table 1: Covariate means of treated and untreated municipalities in Baden-Württemberg before outlier deletion and matching.

412 By matching upon the propensity to reform conditional on this covariate
 413 set, the selection on observables issue should be mitigated. But what about
 414 further potentially observable and not included or even unobservable covari-
 415 ates which impact upon selection into treatment and outcome simultane-
 416 ously? If existent, neglecting those would bias the results. Rosenbaum sensi-
 417 tivity analysis (Rosenbaum, 2002; Keele, 2010) and the conditional DiD esti-
 418 mator (Heckman et al., 1998; Smith and Todd, 2005; Caliendo and Kopeinig,
 419 2008) account for this risk.

420 For robustness reasons, a variety of propensity score matching techniques

421 was implemented: nearest neighbour¹¹ (NN) with and without replacement
422 as well as with a caliper. In NN without replacement, once an untreated
423 municipality has been matched to a given treated municipality because of
424 propensity score proximity, the matched municipality is no longer available
425 as potential match for other treated municipalities (Austin, 2011). This is
426 different in the case of replacement where each non-treated municipality can
427 serve as a matching partner several times. Caliper matching introduces the
428 restriction that the absolute distance of propensity scores of matched mu-
429 nicipalities must be within some pre-specified range. Contrary to the first
430 two algorithms, a treated municipality may drop out because there is no un-
431 treated municipality’s propensity score within the defined caliper distance.
432 The used distance is 0.25 of the standard deviation of the estimated propen-
433 sity scores (Rosenbaum and Rubin, 1985). Whereas the nearest neighbour of
434 a treated municipality may be far away in terms of its propensity score when
435 matching with/without replacement is applied, caliper matching accounts for
436 this risk. Hence, it can be seen as the strictest matching algorithm.

437 Appropriate control units for each treated municipality can only be iden-
438 tified if both groups have similarly distributed propensity scores. Common
439 support was ensured by minima/maxima comparison and visual inspection
440 (Caliendo and Kopeinig, 2008; Lechner, 2001; Heckman et al., 1998).

441 In order to evaluate matching quality, post-matching covariate balance
442 was checked by mean convergence, t-testing the mean difference, analysing
443 the standardised bias improvement¹² following Rosenbaum and Rubin (1985)
444 and comparing the initial pseudo R^2 of each model with its counterpart based
445 on the matched sample (Sianesi, 2004). After successful matching, the ATT
446 was retrieved by simulation (King et al., 2000) and corresponding inference
447 was taken by a paired t -test assuming a lack of independence in the propensity
448 scores of the matched sample (Austin, 2011). The ATT was calculated for
449 the years 2015 and 2016 but not for earlier post-treatment years since, as
450 mentioned above, official budget data is prone to errors in the immediate

¹¹Although the units of analysis are municipalities, the expression "nearest neighbour" has no geographical meaning in this context.

¹² $SB_{before} = 100 * \frac{(\bar{X}_1 - \bar{X}_0)}{\sqrt{0.5 * (V_1(X) + V_0(X))}}$; $SB_{after} = 100 * \frac{(\bar{X}_{1M} - \bar{X}_{0M})}{\sqrt{0.5 * (V_{1M}(X) + V_{0M}(X))}}$ with $X_{1/0}$ and $V_{1/0}$ being before matching mean and variance of the treatment (index 1) and the non-treatment (index 0) group whereas the index M introduces the post-matching state (Caliendo and Kopeinig, 2008).

451 years after the reform. For the conditional DiD estimate the following panel-
452 regression was estimated:

$$y_{it} = \beta_0 + \beta_1 * treat_i + \beta_2 * time_t + \beta_3 * treat_i * time_t + \epsilon_{it} \quad (4)$$

453 The variable $treat_i$ is a 0/1 treatment dummy for each municipality, with
454 1 for treatment and 0 otherwise. The variable $time_t$ is a dummy for each
455 year t indicating years post-treatment with 1 and pre-treatment years with
456 0. Finally, the coefficient β_3 of the multiplicative term $treat_i * time_t$ is the
457 conditional DiD estimator, 'conditional' since sample selection is conditional
458 on the covariate set imposed in the propensity score estimation. As the
459 estimation uses matched data, there is no need for additional covariates. For
460 the conditional DiD estimator the two treatment years 2011 and 2012 were
461 combined to one period which assumes the absence of fundamental structural
462 changes that impacted on the VOIs in these two years. Considering the
463 accrual accounting history of Baden-Württemberg, this seems to be justified.

464 Finally, the matching procedure was repeated with the help of entropy
465 matching as proposed by Hainmüller (2012) (see Appendix 2). With this
466 approach, covariate balance is based on a maximum entropy reweighting
467 scheme for fitting weights which satisfy balance constraints that involve exact
468 balance on the moments of distribution. The estimated ATT then follows
469 the standard Rubin causal model rationale presented above.

470 5. Results

471 5.1. Matching on the Propensity Scores

472 Regression results of propensity score estimation are presented in tables
473 2 and 3.¹³ The only covariate highly significant over all seven models is
474 the number of inhabitants. Its positive sign indicates that the propensity
475 to take on the accruals reform rises with municipality size. This mirrors the
476 eyeball result from the previous chapter that treatment units are, on average,
477 more urban than non-treatment units. Although being close to significance
478 in most models, only in Model 6 does the debt per capita turn significant.
479 This carefully points to the relevance of reform costs. Further covariates

¹³Due to limited space, results for the VOI of 2005 and 2007 are not presented. They are insignificant anyway.

480 seem to be of minor relevance. Nevertheless, they are kept in the models
481 since for matching it is of interest that control and treatment municipalities
482 not only converge in mean size and public debt per capita but also in all
483 other characteristics covered by the covariate set X_j and – more important
484 – in the vector Z_k comprising all pre-treatment years of the respective VOI.
485 Including nonsignificant variables does not bias score estimates or make them
486 inconsistent (Bryson, 2002). However, it may increase the variance. The
487 overall fit of the regressions is reasonable with pseudo- R^2 s varying between
488 0.2 and 0.25. A pseudo- R^2 around 0.2 is comparable to an OLS adjusted R^2
489 of 0.7 (Louviere et al., 2000; Lin, 2010).

490 The distribution of propensity scores, especially in the aggregate munic-
491 ipal investment model one, shows a sufficient degree of overlap (Figure 3).
492 The total number of municipalities behind the overlap analysis of model one
493 (40 treated, 607 untreated) is lower compared to what is presented in the
494 regression results. This is due to the minimum and maximum comparison.
495 As for model one, 184 municipalities were dropped (four treated and 180 un-
496 treated) since there was no score in the other group that allowed for proper
497 matching.¹⁴ Success of the matching was evaluated by the degree of conver-
498 gence of covariate means. Since seven models times three matching methods
499 makes 21 outputs of covariate balance, this paper only provides results for
500 the aggregate models 1 and 4 (tables 4 and 5) gained by NN without re-
501 placement. The balance situation for the remaining 19 matching exercises is
502 comparably sufficient.

503 The most relevant pair of means presented in tables 4 and 5 describes
504 convergence in municipality size. As seen above (Table 1), the raw data
505 before matching revealed an average population of 34,012 for reform munic-
506 ipalities and only 6,237 for non-reform municipalities. After matching with
507 Model 1, the average city size of the 40 remaining treated municipalities lies
508 at 21,222 inhabitants whereas the average in the control group lies at 20,663
509 inhabitants. This balance improvement shows that now – on average – cities
510 of equal size are compared. Moreover, balance improves in total expendi-
511 ture, total debt, business tax revenue (all per capita), age ratio, and the two
512 political variables. The mean comparison in those covariates reveals suffi-
513 cient balance. However, this comes at a cost: especially some pre-treatment

¹⁴The overlap of propensity scores for the disaggregated models 2, 3 and 5 to 7 does not differ fundamentally from the presented aggregate models.

Table 2: Logit model: Estimation of propensity scores

	<i>Dependent variable: Treatment</i>		
	<i>Variable of Interest (VOI): Expenditures for:</i>		
	(Total)	(Immov. Assets)	(Mov. Assets)
VOI 2008 pc	-0.001 (0.001)	-0.001 (0.001)	-0.0001 (0.011)
VOI 2009 pc	-0.0003 (0.001)	-0.0003 (0.001)	-0.002 (0.006)
VOI 2010 pc	0.0001 (0.001)	-0.0001 (0.001)	0.010 (0.007)
Primary Balance 2010 pc	0.0001 (0.001)	0.00002 (0.001)	-0.0002 (0.001)
Total Expenditure 2010 pc	0.0003 (0.0004)	0.0003 (0.0004)	-0.0002 (0.0004)
Total Debt 2010 pc	0.0003 (0.0003)	0.0003 (0.0003)	0.003 (0.003)
Busines Tax Revenue 2010 pc	-0.045 (0.782)	-0.1 (0.782)	0.024 (0.750)
Age Ratio 2010	0.006 (0.033)	0.005 (0.033)	0.008 (0.034)
Inhabitants 2010	0.0001*** (0.00001)	0.00001*** (0.00001)	0.0001*** (0.00001)
Leftist Share	0.368 (1.347)	0.348 (1.347)	1.083 (1.328)
Voter Turnout	0.007 (0.034)	0.008 (0.033)	-0.002 (0.033)
Constant	-4.272* (2.360)	-4.296* (2.357)	-4.064* (2.353)
Observations	831	830	895
Log Likelihood	-134.509	-134.458	-137.188
Akaike Inf. Crit.	299.018	298.916	304.377

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Logit model: Estimation of propensity scores

	<i>Dependent variable: Treatment</i>			
	<i>Variable of Interest (VOI): Revenue from sales of:</i>			
	(Total)	(Immov. Ass.)	(Mov. Ass.)	(Fin. Ass.)
VOI 2008 pc	-0.002 (0.003)	-0.0001 (0.002)	0.045 (0.178)	-0.040 (0.029)
VOI 2009 pc	-0.004 (0.004)	-0.004 (0.004)	-0.660 (0.443)	0.002 (0.009)
VOI 2010 pc	0.002 (0.002)	0.003 (0.002)	0.816*** (0.296)	0.001 (0.006)
Primary Bal. 2010 pc	0.0003 (0.001)	0.0003 (0.001)	-0.001 (0.001)	0.00001 (0.0003)
Total Exp. 2010 pc	-0.00001 (0.0004)	-0.00003 (0.0004)	-0.0003 (0.001)	-0.00002 (0.0003)
Total Debt 2010 pc	0.0002 (.0003)	0.0003 (0.0003)	0.001* (0.0003)	0.0003 (0.0003)
Bus. Tax Rev. 2010 pc	0.059 (0.741)	0.024 (0.741)	0.292 (0.932)	-0.147 (0.747)
Age Ratio 2010	0.024 (0.036)	0.022 (0.036)	0.030 (0.037)	0.011 (0.035)
Inhabitants 2010	0.0001*** (0.00002)	0.0001*** (0.00002)	0.0001*** (0.00002)	0.0001*** (0.00002)
Leftist Share	1.713 (1.435)	1.577 (1.460)	0.137 (1.623)	1.329 (1.327)
Voter Turnout	-0.007 (0.035)	-0.004 (0.035)	-0.034 (0.039)	0.006 (0.034)
Constant	-4.440* (2.473)	-4.476* (2.494)	-2.312 (2.714)	-4.933*** (2.326)
Observations	789	787	604	895
Log Likelihood	-113.897	-114.831	-91.169	-132.055
Akaike Inf. Crit.	257.793	259.661	212.338	294.111

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Distribution of Propensity Scores of Treated and Untreated Municipalities

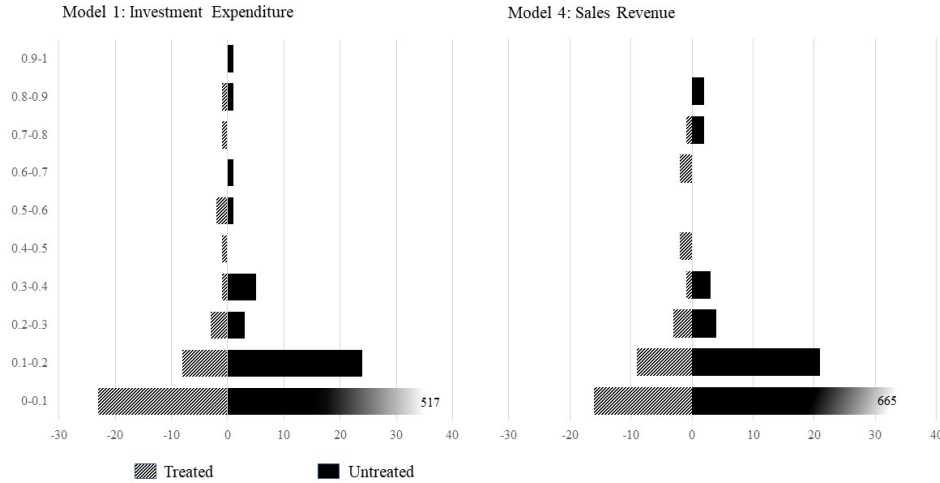


Figure 3: Distribution of propensity scores estimated with model one (aggregate investment) and model four (aggregate revenue from non-financial/financial asset sales).

514 means of the VOI but also the 2010 means of the primary balance did not
 515 converge but drifted further apart. This is not an uncommon phenomenon
 516 in propensity score matching (Hainmueller, 2012). However, balance deteri-
 517 oration in the mentioned covariates is still acceptable. Evaluated with the
 518 standardised bias, which considers not only the first but also the second
 519 moment of distribution, the situation brightens up: the primary balance re-
 520 mains the only covariate with (slightly) deteriorating standardised balance.
 521 This is confirmed by the t-test which is insignificant for all mean differences
 522 and the pseudo- R^2 comparison which shows a level of 0.2185 before match-
 523 ing and decreases to 0.0645 when re-estimating the specification with the
 524 matched sample. To sum up, matching has sufficiently removed the impact
 525 of observable covariates on the propensity to reform.

	Means Treated	Means Control	Bal. Impr.	Std. Bias Reduct.	T-test (p-value)
Investment pc 2005	168.94	181.82	-173.72	25.27	0.579
Investment pc 2006	192.36	189.73	74.55	38.59	0.916
Investment pc 2007	211.95	205.63	-8.19	34.02	0.809
Investment pc 2008	239.91	224.71	-64.08	27.96	0.604
Investment pc 2009	291.54	287.94	87.43	34.15	0.915
Investment pc 2010	283.87	261.47	20.03	14.8	0.508
Primary Bal. 2010	-37.28	-19.14	-694.57	-9.8	0.540
Total Exp. pc 2010	2573.96	2626.99	44.39	15.91	0.689
Total Debt 2010	973.15	1041.48	74.79	59.96	0.709
Bus. Tax Rev. pc 2010	336.6	379.7	4.28	5.96	0.520
Age Ratio 2010	32.53	32.27	34.79	12.41	0.778
Inhabitants 2010	21222.43	20662.7	95.89	70.11	0.903
Leftist Share	26.31	28.11	75.89	62.07	0.623
Voter Turnout	52.96	51.37	56.82	48.38	0.295
pseudo- R^2	McFadden (before)	0.2185	Control	Treated	
	McFadden (after)	0.0645	All	40	
			Matched	40	
			Unmatched	567	0

Table 4: Model 1 - Aggregate investments in movable and immovable assets, per capita: Evaluation of covariate balance after propensity score matching (NN w/o replacement).

	Means Treated	Means Control	Bal. Impr.	Std. Bias Reduct.	T-test (p-value)
Sales 2005	78.62	65.43	-90.39	-11.17	0.422
Sales 2006	81.91	86.44	51.89	7.91	0.813
Sales 2007	88.29	115.79	-508.47	-15.07	0.511
Sales 2008	69.01	83.53	-2832.27	-8.03	0.422
Sales 2009	55.15	64.67	-71.34	-0.18	0.440
Sales 2010	90.32	128.46	-73.57	0.57	0.264
Primary Bal. 2010	-24.28	-11.71	-28.8068	5.33	0.695
Total Exp. pc 2010	2590.49	2704.31	-146.0391	10.66	0.591
Total Debt 2010	1002.47	956.03	86.32	66.76	0.776
Bus. Tax Rev. pc 2010	356.4	410.5	15.22	16.89	0.671
Age Ratio 2010	33.33	32.37	21.1	-1.64	0.261
Inhabitants 2010	21883.48	20175.32	88.8	67.95	0.722
Leftist Share	27.21	29.14	85.21	76.31	0.663
Voter Turnout	52.28	52.69	92.04	82.77	0.823
pseudo- R^2	McFadden (before)	0.25	Control	Treated	
	McFadden (after)	0.12	All	34	
			Matched	34	
			Unmatched	0	

Table 5: Model 4 - Revenues from sales of immovable, movable capital and financial assets, per capita: Evaluation of covariate balance after propensity score matching (NN w/o replacement).

526 *5.2. ATT and Conditional DiD Estimator*

527 This analysis focusses on ATTs for 2015 and 2016 as well as the condi-
528 tional DiD estimator (see tables 6 and 7). Whereas the effects for models 3
529 to 7 remain insignificant or show only tiny and for practical considerations ir-
530 relevant effects, models 1 and 2 provide significant and robust average treat-
531 ment effects on the treated over the whole set of propensity score matching
532 methods. Whereas Model 1 analyses the VOI 'aggregate investment in mov-
533 able and immovable assets', Model 2 captures its component 'investment in
534 immovable assets', which, for the municipal level, is investment in buildings,
535 roads, etc. ATTs for 2015 are rather similar in both models, 2016 ATTs,
536 however, differ to a larger extent. Interpretation of the ATT of -70.54 gener-
537 ated with caliper matching in Model 2 for 2016 is as follows: Due to reforming
538 from cash to accrual accounting in 2011/12, municipalities spent an average
539 of EUR 70.54 less per capita in 2016 on immovable assets (building, roads,
540 etc.) than they would have spent had they not introduced accrual accounting
541 but stayed in the cash accounting mode.

542 Two remarks to these results: First, since effects for the different variables
543 of interest stem from independent matching analyses, the effects of Model 2
544 and Model 3 do not add up to the effects of Model 1. However, the fact that
545 the only significant coefficient of Model 3 (caliper, ATT 2016: -9.53) has the
546 same sign as its Model 2 equivalent indicates the aggregate nature of VOI 1.
547 Second, in order to underpin the assumption that presented ATTs are linked
548 to the accruals reform and do not depend on an unconsidered other event that
549 only happened to the subset of the 2011 or the 2012 reform municipalities,
550 the 2016 ATT for VOI 1 estimated with nearest neighbour matching without
551 replacement (-96.92) was disentangled for both years. This revealed that
552 the effect is based upon a treatment group with 12 municipalities having
553 reformed in 2011 and 28 municipalities in 2012.¹⁵ Whereas the 2016 ATT
554 for the reform municipalities of 2011 is -54.14, the 2016 ATT for the reform
555 municipalities of 2012 lies at -115.92. The weighted average is -97.27, which
556 is close to the presented ATT -96.92. The neglectable difference of 0.35 is due
557 to retrieval by simulation. Disentangling the effect for 2011/2012 reform
558 municipalities strengthens the assumption of the absence of an unobserved

¹⁵The remaining four municipalities in the group of 44 dropped out during data prepara-
tion (outlier deletion, min-max comparison).

559 one-time event.¹⁶

560 Figure 4 provides the graphical representation of municipal investment
561 behaviour for VOIs 1 and 2; the two right panels show the development in
562 treatment and control groups in the $t - 5/t - 6$ pre and $t + 4/t + 5$ post-
563 treatment years for the matched sample. The pre-2011/12 similarity after
564 matching supports the common trend assumption necessary for the DiD-
565 estimator (Angrist and Pischke, 2011). The 2016 gaps between the solid
566 and the dashed line represent the 2016 ATTs. The graphs illustrate nicely
567 that the ATT does not solely consist of decreasing average investment ex-
568 penditures of treatment municipalities after the reform, but to an even larger
569 extent, of increasing average investment expenditures of control group mu-
570 nicipalities. The 2016 ATT of model one retrieved by NN matching without
571 replacement (-96.92), for example, results from a 14.14 percent decline of
572 investment expenditure of the average treatment group municipality from
573 2010 (EUR 283.87 p.c.) to 2016 (EUR 248.68 p.c.) plus a 32.36 percent
574 increase in the average control group municipality. Hence, the ATT does not
575 mean that the reform municipality, on average, spent EUR 96.92 less in 2016
576 compared to 2010. The interpretation is, as already mentioned, that it spent
577 EUR 96.92 less in 2016 than it would have spent had it not transitioned into
578 accrual accounting.

579 Conditional DiD estimates differ slightly from ATTs of 2015 and 2016.
580 Since estimation is based upon the whole panel, the immediate post-treatment
581 years 2013/14 also contribute here. The central implication of the DiD re-
582 sults is that they are within an acceptable range compared to the ATTs
583 and that they show the same sign. That is, once the analysis accounts for
584 time-invariant unobserved covariates, the results hold. Nothing, however,
585 can be said about unobserved time-varying covariates. For this reason, the
586 Rosenbaum sensitivity analysis was implemented (see below).

587 The widely insignificant ATT/DiD results for VOI 3 may be explained
588 by the fact that movable assets are comparably cheaper in price and, con-
589 sequently, a today's investment does not imply a major future burden on

¹⁶Further support comes from the estimation of 2016-ATTs for municipalities which adopted the reform in 2013. Although being insignificant due to the small size of the treatment group, sign and magnitude of estimated ATTs fit to the results presented in Table 6. Nevertheless, explicit results are not part of this paper since in 2013 the institutional setting changed (the transition period was extended), which might have had an impact on the composition of the group of 2013 reform municipalities.

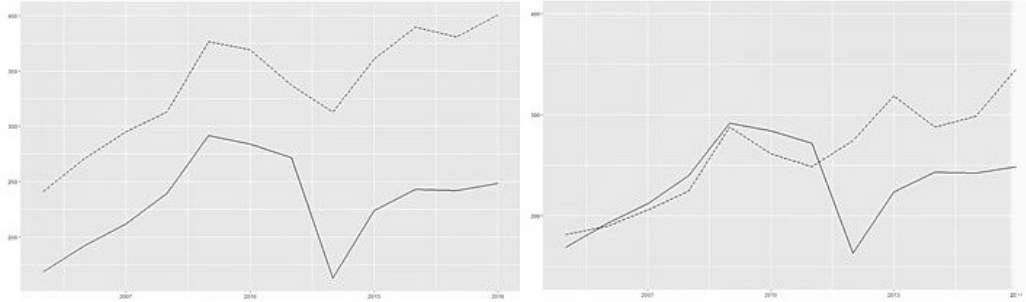
590 the budget. Moreover, movable assets are presumably not as durable as im-
591 movable assets and consequently have a shorter depreciation period. This
592 might weaken the incentive for political decision-makers to set out or defer
593 investments compared to its immovable assets counterpart. Summarising
594 the results for models 1 to 3 one can state that they support Hypothesis 1
595 with regards to aggregate investments and investments in immovable assets.
596 There is only limited support in terms of investment in movable assets.

	Matching Algorithm	ATT (2015)	ATT (2016)	DiD
VOI 1	NN without replacement	-56.65	-96.92**	-73.69***
	NN with replacement	-61.20*	-95.19**	-75.38***
	Caliper	-59.26*	-93.23**	-64.54***
VOI 2	NN without replacement	-56.08*	-59.86*	-55.40***
	NN with replacement	-62.45**	-65.73**	-55.76***
	Caliper	-63.36*	-70.54**	-92.57***
VOI 3	NN without replacement	0.28	-5.84	-1.21
	NN with replacement	0.78	-5.40	-1.26
	Caliper	-0.92	-9.53*	-1.57

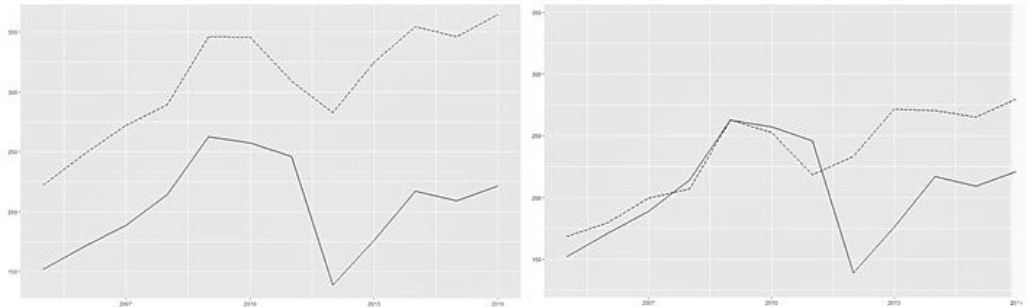
Table 6: Average treatment effects on the treated (ATT) and conditional Difference-in-Differences effects (DiD). VOI 1: Model with total investment p.c. as an dependent variable; VOI 2: Model with investment in immovable assets p.c.; VOI 3: Model with investment in movable assets p.c. ATTs mirror the simulated mean differences in the matched sample; inference was taken by a paired t-test. DiD inference reflects the panel DiD results. Significance levels: $p < 0.1^*$, $p < 0.05^{**}$, $p < 0.01^{***}$

597 When it comes to Hypothesis 2 analysed with models 4 to 7, results are
598 not as clear. Since only a tiny share of municipalities have revenue from sales

Mean municipal investment in movable and immovable assets



Mean municipal investment in immovable assets



Before matching - Solid line: mean investment of the 40 municipalities in the treatment group. Dashed line: mean investment of 851 non-treated municipalities.

After matching - Solid line: mean investment of the 40 municipalities in the treatment group. Dashed line: mean investment of 40 municipalities in the control group.

Figure 4: Development of municipal investment behaviour from 2005 to 2016 for treated and untreated municipalities before matching (left panel) and for treatment and control group after matching (right panel).

599 of movable (VOI 6) and financial assets (VOI 7), the occasionally significant
 600 ATTs or DiDs in models 6 and 7 are hardly interpretable. The insignificant
 601 results of models 4 and 5 point to a drop of revenue from divestment of
 602 immovable assets.

603 Summarising the results, treatment effects on reform municipalities re-
 604 garding investment in immovable assets are significant, negative and of rel-
 605 evant magnitude in years three and four after implementation. Negative,
 606 relevant in size but insignificant is the effect in terms of revenues from sales
 607 of immovable assets.

608 *5.3. Sensitivity Analysis and Further Robustness*

609 To cope with unobserved time-varying covariates which might cause bi-
 610 ased results, significant ATTs of models 1 and 2 were subjected to sensitivity

	Matching Algorithm	ATT (2015)	ATT (2016)	DiD
VOI 4	NN without replacement	-38.43	-30.97	-15.21
	NN with replacement	-35.14	-22.09	-9.42
	Caliper	-26.55	-19.52	-18.37
VOI 5	NN without replacement	-45.79	-21.02	-12.29
	NN with replacement	-45.75	-22.97	-9.19
	Caliper	-28.80	-15.92	-15.31
VOI 6	NN without replacement	-0.18	-0.58*	-0.14
	NN with replacement	-0.14	-0.63	-0.13
	Caliper	-0.07	-0.83	-0.35*
VOI 7	NN without replacement	-3.23**	-0.93*	3.26
	NN with replacement	-3.84**	-1.10*	2.59
	Caliper	-6.40*	-2.28	-5.22

Table 7: Average treatment effects on the treated (ATT) and conditional Difference-in-Differences effects (DiD). VOI 4: Model with total revenue from immovable, movable and financial asset sales p.c. as dependent variable; VOI 5: Model with revenue from immovable asset sales pc; VOI 6: Model with revenue from movable asset sales p.c. VOI 7: Model with revenue from sales of financial assets p.c. ATTs mirror the simulated mean differences in the matched sample; inference was taken by a paired t-test. DiD inference reflects the panel DiD results. Significance levels: $p < 0.1^*$, $p < 0.05^{**}$, $p < 0.01^{***}$

611 analysis following Rosenbaum (2002) (see Table 8). Column four for Model 1
612 shows upper bounds for the p-value of the 2016 ATT given increasing levels

613 of Γ .¹⁷ Results show that Γ may rise until a level of 1.5 and the p-value
614 would still be below the critical 0.1 level. In terms of interpretation, a Γ
615 of 1.5 means that it would be possible to have omitted an unobserved but
616 relevant covariate that quadruples the odds of being treated and doubles the
617 odds of measuring a positive treatment effect. That is: A really important
618 unobserved covariate would be necessary to render the measured effect on
619 investment expenditure insignificant. It seems rather unlikely that such an
620 important driver is unknown and was therefore neglected.

	ATT 2015		ATT 2016	
	Γ	P-Value	Γ	P-Value
Model 1	1	0.062	1	0.012
	1.1	0.096	1.1	0.021
	1.2	0.136	1.2	0.034
			1.3	0.049
			1.4	0.069
			1.5	0.092
Model 2			1.6	0.118
	1	0.027	1	0.042
	1.1	0.046	1.1	0.068
	1.2	0.071	1.2	0.102
		1.3	0.102	

Table 8: Results sensitivity analysis. Γ represents the factor by which two units in the matched sample might differ in treatment probability due to unmeasured covariates.

621 In addition, ATTs were re-estimated with entropy balancing following
622 Hainmüller (2012). By estimating matching weights with a maximum en-
623 tropy scheme, this technique differs quite fundamentally from propensity
624 score matching. As central advantage there is no longer the propensity score
625 matching tradeoff between improving balance for certain covariates at the
626 cost of deteriorating it for others. Entropy balancing improves balance for
627 all included covariates. Regarding the results, relevant ATTs for VOIs one
628 and two range at similar levels as they did under propensity score match-
629 ing and the 2016 ATT for Model 3 turns significant. This strengthens the
630 robustness of the effects presented above.

¹⁷A subject in a matched sample is by the factor $\Gamma > 1$ more likely to select into treatment than another subject because they differ in unobserved factors.

	ATT (2015)	ATT (2016)
VOI 1 (Aggregate investments)	-72.57***	-88.56***
VOI 2 (Investment immov. assets)	-69.19**	-80.66***
VOI 3 (Investment mov. assets)	-2.14	-8.81***
VOI 4 (Aggregate sales rev.)	-18.53	-5.75
VOI 5 (Sales immov. assets)	-10.63	-1.32
VOI 6 (Sales mov. assets)	-0.13	-0.34*
VOI 7 (Sales financial asset)	-6.83*	-1.51**

Table 9: ATT - Entropy Balancing

631 6. Discussion

632 Results indicate robust support for Hypothesis 1: *After the transition*
633 *from cash to accrual accounting, municipal investments in long-lasting as-*
634 *sets like buildings or roads decrease.* The rationale behind this hypothesis
635 is built upon the political economy consideration that a changing over-time
636 distribution of financial burdens after investment due to depreciation changes
637 incentives for political decision-makers at the local level. ATTs for models
638 1 and 2 as well as the conditional DiD coefficients for 2015 and 2016 indi-
639 cate that – in contrast to cash accounting – accrual accounting provides an
640 incentive to drop or defer (less important) investments. Mayors seem to be
641 motivated to avoid future budget deficits (or counteracting adjustments of
642 service levels or local taxes) since this could lead to a loss of voter support
643 (Drazen and Eslava, 2010; Brender and Drazen, 2008; Brender, 2003). Usu-
644 ally, they try to keep their positions as long as possible, sometimes even over
645 decades. Hence, their decision-making is supposed to take the (very) long

646 term into account. Unlike cash accounting, financial information provided by
647 accrual accounting puts more emphasis on future burdens initiated by cur-
648 rent investments in municipal investment plans (Articus and Wagner, 2011).
649 As shown by qualitative research, this does indeed seem to change existing
650 mindsets (Jagalla et al., 2011). Under the assumption that less important
651 investments are skipped first, the indicated behavioural change might be in-
652 terpreted as an increase of fiscal responsibility. At the same time it could
653 be cautiously linked to the widely discussed German local public investment
654 gap (Gornig, 2019; Bach et al., 2013). In addition, since maintenance of as-
655 sets is a central target of the accrual accounting reform (Mord-Wohlgemut
656 et al., 2016; Keller, 2015; Eckstein and Behle, 2015), dropping investment
657 may indicate that it failed in this point. However, the data does not allow
658 for differentiating between investment in new assets and investment for asset
659 maintenance.

660 As an alternative explanation for the identified effect on investment, re-
661 form costs might be considered. Since reform municipalities face high ex-
662 penditures for implementing accrual accounting, scope for investments in
663 subsequent years may be limited. However, this effect should drive down
664 investments in the immediate post-reform years. After five years, which rep-
665 resents one electoral cycle, it should have been phased out and the 2016-ATT
666 is likely to be unaffected. As a further driver, one might consider a lower
667 investment gap in reform compared to control municipalities in the year of re-
668 form. That is, only those municipalities which don not face high investments
669 in the subsequent years and therefore do not expect additional burdens due
670 to depreciation may implement the reform. In this case the ATT would not
671 be linked to the reform but to actual investment needs in 2011/12. How-
672 ever, a smaller investment gap in treated compared to control municipalities
673 would require relatively higher investments in the years prior to the reform.
674 High covariate balance after matching for annual investment since 2005 ren-
675 ders this argument implausible. Considering additionally the broad set of
676 matching covariates, the low relevance of neglected neighbouring effects and
677 reform perceptions of political decision-makers (see Appendix 1), as well as
678 the nature of the conditional DiD estimator, which accounts for unobserved
679 time-consistent covariates, the results presented in this paper are suggested
680 as causal effects of the accrual accounting reform on local government bud-
681 gets and financial behaviour of political decision-makers.

682 Compared to the results regarding investment in immovable assets, the
683 picture for movable assets is less clear. Those are not as durable and are,

684 presumably, cheaper in price. Hence, the incentive to drop or defer invest-
685 ment should be less pronounced and may be stronger for relatively more
686 expensive movable assets like fire engines. However, the analysed VOI does
687 not allow for disentangling different types of movable assets. In general, this
688 may explain the inconclusive ATTs for 2016 when comparing the predomi-
689 nantly insignificant propensity score matching results (Table 6, VOI 3) with
690 the significant entropy balancing results (Table 9).

691 Since most ATTs and DiD estimates for VOI 4 to VOI 7 are insignificant,
692 there is only limited support for Hypothesis 2 (*"After the transition from*
693 *cash to accrual accounting, municipal revenue from asset sales decrease"*).
694 But still, signs of VOIs 4 and 5 indicate a drop in revenue after reform. This
695 supports the idea that implementing accrual accounting spoils any strategy of
696 selling assets so as to balance the budget. The reason for the relatively weak
697 results regarding H2 might lie in the financially sound condition in which
698 municipalities in Baden-Württemberg are due to the decade-long striving
699 economy in the German southwest. There may simply have been no need to
700 sell assets in order to balance budgets during the economic boom years 2015
701 and 2016. As existing empirical research shows, there are similar but more
702 robust results for local governments in other German states which might
703 suffer from more fragile financial conditions (Christofzik, 2019).

704 Identified decreasing aggregate investment expenditure after treatment
705 provides a potential explanation for local government efficiency gains after
706 the reform (Lampe et al., 2015; Gaertner, 2014). Since the stochastic fron-
707 tier analysis usually applied in this literature is based upon expenditure as an
708 input variable in the decision units' provision of public services, decreasing
709 investment at stable levels of output necessarily causes efficiency gains among
710 reform municipalities. Moreover, the findings corroborate recently published
711 work by Dorn et al. (2019) and Christofzik (2019). Based on different Ger-
712 man local government budget data both studies present significant evidence
713 of a post-reform drop in sales revenues from non-financial assets which fits
714 with ATTs of VOI 5. Their estimates for reform-related changes in local
715 investment behaviour point to the same direction as ATTs of VOI 1 and 2.
716 This adds external validity to the results presented in this paper and makes
717 them relevant not only for the treated municipalities in Baden-Württemberg
718 but for all municipalities which replaced cash with accrual accounting in
719 Germany and beyond. Around 17 years after German state ministers of the
720 interior decided to implement accrual accounting at the local level, a com-
721 mon understanding of the reform effects based on empirical analyses seems

722 to be emerging.

723 **7. Conclusion**

724 The local government accrual accounting reform is a prominent topic
725 among public administration and accounting scholars. Although controver-
726 sially discussed since decades, empirical evidence of reform effects on local
727 budgets is still scarce. This paper is one of the first contributions to close
728 this gap. For this purpose, municipal data of the German state of Baden-
729 Württemberg for the years 2005 to 2016 is analysed in an observational study.
730 It exploits the slow but successive transition of municipalities into accrual
731 accounting since 2009. Several propensity score matching methods were used
732 to estimate average treatment effects on the treated for seven fiscal variables.
733 In addition, the conditional difference-in-differences estimator was applied.

734 Subject to sensitivity analysis as well as several robustness checks, results
735 provide support for the hypothesis that the reform changes local decision-
736 makers' investment behaviour and dampens expenditures on immovable as-
737 sets like buildings or roads. One potential explanation is that accrual ac-
738 counting introduces annual depreciation and therefore emphasizes future fi-
739 nancial burdens caused by todays' investments. From the survey literature we
740 know that local decision-makers perceive an accrual budget balance as harder
741 to reach. Combined with the medium-to-long term perspective towards fu-
742 ture local elections this might change mindsets and fiscal decision-making,
743 which – under the assumption of the suspension of less important investments
744 – could be interpreted as being more responsible. On the other hand, one
745 could see the identified investment-lowering effect of the accrual accounting
746 reform as one driver of the German investment gap, which is particularly
747 evident at the local level.

748 Although presented results are based on municipal data from only one
749 German state, the author suggests generalisability. Not only that the find-
750 ings are in line with recently presented evidence for further German states
751 (Christofzik, 2019; Dorn et al., 2019). Moreover, the change from pure cash
752 to pure accrual accounting follows similar patterns in every government at
753 every administrative level all over the world. In Europe alone, several ten
754 thousands of municipalities have implemented the reform so far.

755 Despite similar general patterns, concrete standards of accrual accounting
756 are rather heterogeneous within Germany and beyond. This is a limitation

757 for cross-state and even more cross-country analyses as well as a fundamen-
758 tal problem for Eurostat government finance statistics. Therefore, the Eu-
759 ropean Commission is currently preparing the so-called EPSAS (European
760 Public Sector Accounting Standards) reform, which is nothing less than an
761 approach to fully implement harmonised accrual accounting standards in all
762 governments throughout the European Union. From this perspective, em-
763 pirical evidence on reform effects may provide relevant arguments for the
764 accompanying political discussion.

765 In addition, the results of this paper may motivate more rigorous empiri-
766 cal investigation of the accrual accounting reform in future. Considering the
767 fact that it consumed uncounted billions of public money all over the world
768 which could have been spent for other, maybe more welfare-enhancing pur-
769 poses, it is astonishing that we still cannot sufficiently answer the question
770 of whether the reform lives up to its promises. Although latest empirical
771 results in this and other papers indicate a more conservative financial be-
772 haviour once accrual accounting is implemented, there is still a lot of work
773 to be done.

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1017 **Appendix A. Selection on Unobservables**

1018 This study tries to provide convincing empirical arguments for the cho-
1019 sen set of covariates upon which treated and control units were matched.
1020 Nevertheless, further answers to the central question of time-varying, unob-
1021 servable but influential covariates can be given. From Italian survey literature
1022 (Anessi-Pessina et al., 2008) we know that potential drivers of the decision to
1023 switch from cash to accrual accounting at the local level are geographical lo-
1024 cation, which may in the Italian south-north distinction rather be a question
1025 of municipal financial health, and preparers' perception, which is the attitude
1026 of staff members of the municipal financial department.¹⁸ The relevance of
1027 reform costs was also stressed by the survey findings of Articus and Wagner
1028 (2011) and is comprehensively captured by the fiscal variables of the covariate
1029 vector X_j . In addition, the analysis of geographical patterns among reform
1030 municipalities in 2012 shows no distinct cluster pattern which would point
1031 to neighbouring effects (see Figure 5). Reform perception or openness to-
1032 wards budget innovations, however, is indeed hard to observe but potentially
1033 influential for both the reform probability and the fiscal outcome variables.

1034 Based on existing literature and additional empirical analysis, two ar-
1035 guments challenge this presumed impact. First, one may question that the
1036 innovation openness of local decision-makers shows much variation over time.
1037 Managerial staff in municipal finance departments do not usually hop from
1038 job to job but stay there for many years (Christiaens and Van Peteghem,
1039 2007; Kobayashi et al., 2016). In order to confirm this with data from Baden-
1040 Württemberg, publicly available accrual accounting survey responses from
1041 municipalities in two Baden-Württemberg districts from the year 2012 were
1042 analysed.¹⁹ Among the 31 respondents who in 2012 held relevant positions in
1043 the local finance department, 24 still had their job in 2019 and two of those
1044 who were no longer in their position had left after at least 12 years in office.
1045 Twenty-nine respondents were mayors in 2012 and 18 of them were still in
1046 office in 2019; among the 11 who dropped out eight did so after 12 to 36
1047 years of incumbency. Hence, the conditional DiD estimator should account
1048 for open-minded practitioners.

¹⁸Preparers' perception of the reform may also be called 'openness towards budget innovations'.

¹⁹The survey was conducted as part of a published thesis at the 'University of Applied Sciences Ludwigsburg', Author: Elisabeth Lohr.

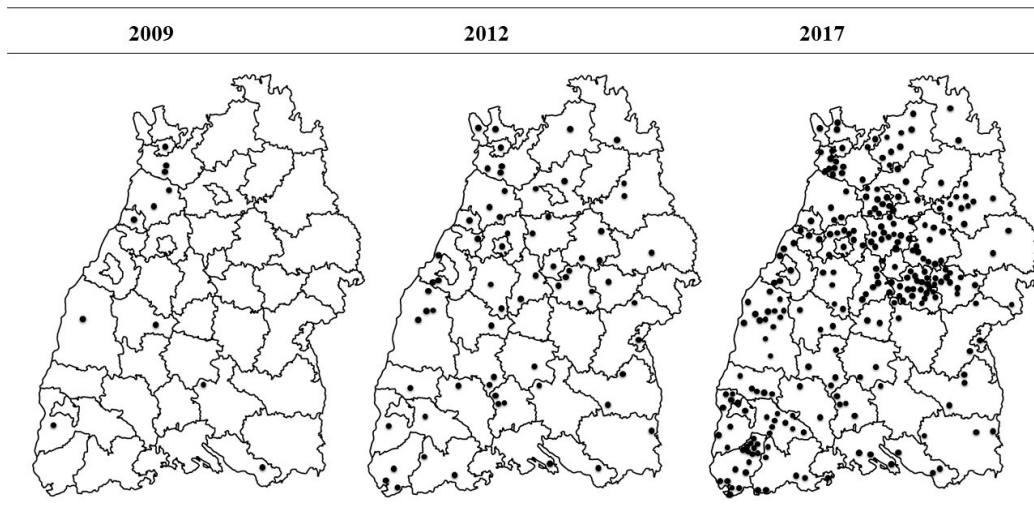


Figure A.5: Geographical distribution of reform municipalities in the years 2009 (year in which the accrual accounting state law came into effect), 2012 and 2017 in Baden-Württemberg. The 2009 transitioners are early adopters/pilot municipalities. The 2012 map shows that in the first years distinct clusters of reform municipalities are widely absent. This indicates the irrelevance of spatial neighbouring effects for matching. The 2017 distribution indicates the relevance of the geographical location around the state capital Stuttgart. This, however, is a time-invariant characteristic for which the conditional DiD-estimator accounts for.

1049 Second, it is far from clear whether positive reform perception is a good
1050 predictor for transition probability. This, for example, is suggested by the
1051 results of Christiaens and Van Peteghem (2007) and Kobayashi et al. (2016)
1052 who find no impact of the local treasurers' education and (business account-
1053 ing) experience on the state of reform implementation/usage of accrual in-
1054 formation in Flemish and Japanese municipalities. Support also comes from
1055 Gärtner (2014) who tried to overcome the issue of selection into the accru-
1056 als reform due to municipal openness towards accounting innovation in the
1057 German state of Lower-Saxony by conducting post-econometric expert in-
1058 terviews. The results provide some additional qualitative indication that
1059 innovation openness is indeed no major reason to self-select into the accrual
1060 accounting reform. To a certain extent, these findings stand in opposition to
1061 the results of Annessi-Pessina (2008). Hence, the picture is at best inconclu-
1062 sive. In the case of Baden-Württemberg a point can be made by analysing

1063 the above-mentioned survey responses from 2012. Municipal decision-makers
 1064 were asked whether – if they had the choice – they would opt for cash or ac-
 1065 crual accounting. The answers are revealed preferences of identifiable local
 1066 decision-makers in the actual sample of this study given in one of the two
 1067 analysed treatment years. These preferences were compared with reform im-
 1068 plementation within the subsequent six years (until 2018). Figure 6 shows
 1069 the results.

Reform perception and subsequent transition

2012 survey results of 57 municipalities compared to actual transition years

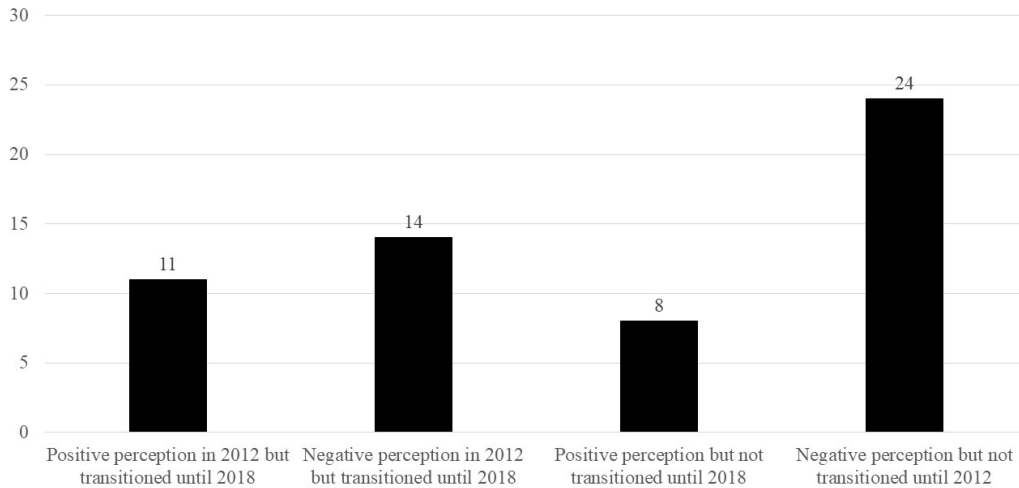


Figure A.6: Revealed reform perception in 2012 compared to transition status in 2018. Total number of surveyed municipalities: 62. Answers of five municipalities were either inconclusive or excluded since they had transitioned before 2012. The survey was conducted as part of a thesis at the 'University of Applied Sciences Ludwigsburg'. Author: Elisabeth Lohr.

1070 Around 61.4 percent (or 35 respondents) show consistent behaviour ac-
 1071 cording to their revealed preference. That is, in 2012 they had either revealed
 1072 a positive perception of the accruals reform and switched in subsequent years
 1073 or they had revealed a negative perception and then did not reform until 2018.
 1074 Around 38.6 percent (22 municipalities), however, behaved opposite to their
 1075 revealed preference. Either they had revealed a positive preference and did
 1076 not reform until 2018 (eight municipalities) or they had revealed a negative
 1077 preference and reformed thereafter (14 municipalities). This indicates that
 1078 the relation of reform perception on transition probability is far from per-

1079 fect and shows that the described 'selection on unobservables problem' based
1080 upon reform perception is a minor concern. But even in the event of being
1081 relevant, the unobserved covariate should be rather time-consistent and the
1082 resulting bias should be removed by the conditional DiD estimator.

1083 **Appendix B. Entropy Balancing**

1084 The implementation of entropy balancing in order to provide alterna-
 1085 tive ATTs for those Baden-Württemberg municipalities which reformed their
 1086 mode of accounting in 2011/12 follows Hainüller’s (2012) baseline scenario.
 1087 Again, the goal was to estimate the simple ATT as presented in equation (1)
 1088 using the difference in mean outcomes between the treatment group and the
 1089 reweighted control group. The counterfactual mean is estimated by:

$$1090 \quad \mathbb{E}[Y(0)|\widehat{D} = 1] = \frac{\sum_{i|D=0} Y_i w_i}{\sum_{i|D=0} w_i}$$

1091 where w_i is a weight chosen for each control unit according to the follow-
 1092 ing reweighting scheme:

$$1094 \quad \min_{w_i} H(w) = \sum_{i|D=0} h(w_i)$$

1095 subject to balance and normalising constraints

$$1096 \quad \sum_{i|D=0} w_i c_{ri}(X_i) = m_r \text{ with } r \in 1, \dots, R \text{ and}$$

$$1097 \quad \sum_{i|D=0} w_i = 1 \text{ and}$$

$$1098 \quad w_i \geq 0 \text{ for all } i \text{ such that } D = 0$$

1099 where $h(\cdot)$ is a distance metric and $c_{ri}(X_i) = m_r$ describes a set of R
 1100 balance constraints imposed on the covariate moments of the reweighted
 1101 control group.

1102 The loss function $h(\cdot)$ uses an entropy divergence defined by $h(w_i) =$
 1103 $w_i \log(w_i/q_i)$ with estimated weight w_i and base weight q_i (with q_i being
 1104 uniform weights). The loss function measures the distance between the dis-
 1105 tribution of estimated w_i and predetermined q_i . Since this distance is min-
 1106 imised, estimated weights remain as close as possible to the uniform base
 1107 weights and relevant information of the original covariate distribution is pre-
 1108 served. On the other hand, the balance constraints are imposed to equalise
 1109 the moments of the covariate distributions between the treatment and the
 1110 reweighted control group.

1111 Following Hainmüller (2012: 31) the 'balancing scheme can be understood
1112 as a generalisation of the conventional propensity score weighting approach
1113 where the researcher first estimates the unit weights with a logistic regres-
1114 sion and then computes balance checks to see if the estimated weights indeed
1115 equalise the covariate distributions'. Conceptually, propensity score weight-
1116 ing and propensity score balancing are rather close. In entropy balancing,
1117 the adjustment problem is tackled from the reverse: weights are directly
1118 estimated from the imposed balance constraints. For more details and appli-
1119 cations, see Hainmüller (2012).