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The Economic Impact of EU Guarantees on Credit to SMEs

Evidence from CESEE Countries

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Abstract[1]

This paper estimates the economic impact at final beneficiary level of the Multi-Annual Programme for enterprises and entrepreneurship EU SME Guarantee Facility in Central, Eastern and South-Eastern European (CESEE) Countries in the period 2005-2012. Data on SME beneficiaries has been collected from administrative records and enriched with information on firms' financial accounts taken from the Orbis database. The paper combines propensity scores and difference-in-differences estimation in order to evaluate the effect of having received a MAP-guaranteed SME loan on firm performance (employment, production, profitability and factor productivity) against a control group of comparable firms. Our results offer several insights. We find that the EU SME Guarantee Facility in the CESEE region had, on average, a significant positive effect on firms' employment: beneficiary firms were able to increase their workforce by 17.3%, compared to the control groups, within the first 5 years following the issuance of the guaranteed loan. Moreover, by the fifth year after the signature date, the turnover of MAP beneficiaries had increased by 19.6%, compared to non-beneficiary companies. However, MAP beneficiaries faced a temporary setback in productivity, with respect to their peers, an effect that could be due to allocative inefficiencies following the MAP-induced increase in their production factors. Such gap was, however, partially absorbed over the medium run. By breaking down our sample by country, signature year, size and age classes, we observe that *micro* and *young* SMEs have benefited the most from MAP-guaranteed loans in terms of economic additionality. Overall, our findings suggest that the EU SME Guarantee Facility has been successful in bringing significant positive effects on beneficiary firms in CESEE Countries.

[1] This paper benefited from comments and inputs by many colleagues, for which we are very grateful. We would like to particularly acknowledge help by Dario Prencipe and Matteo Pallini, who collaborated in the model design and early implementation. Moreover, we would like to thank EIF colleagues Helmut Kraemer-Eis, Frank Lang, Salome Gvetadze, Luis Broegas Amaro and Gunnar Mai for their valuable contributions. We are also grateful to Filippo Teoldi, Katharina Ehrhart, Martina Tornari and Valeria Salituro who, from the EC side, provided valuable comments and great support in the publication process. Finally, we thank the EC management for having initiated research work on this topic and – together with the EIF management – encouraged and supported us throughout.

Non-technical Summary

Small and Medium-sized Enterprises (SMEs) are the backbone of the EU economy: they represent 99.8% of EU companies, almost 60% of GDP (total value added) and near 70% of the total workforce. Yet despite their economic importance, even financially viable SMEs face greater problems than larger firms in several respects, including access to finance: their already limited sources of funds are typically further constrained by higher credit rationing and more penalising credit conditions.

To address this market failure, national and supranational governments and organisations in the EU have long adopted a variety of financial measures aimed at supporting SME finance including notably grants, direct lending, guarantee and counter-guarantee schemes, equity financing and support to securitisation of SME loans. The use of Public Credit Guarantee Schemes (PCGSs) is particularly widespread, across both OECD and non-OECD economies, as a direct policy tool to alleviate SMEs' financial distress, and has recently intensified to address the repercussions of the financial crisis on financial and product markets.

Yet despite its policy relevance, SME financial support in general, and credit guarantee schemes in particular, have hardly been the subject of rigorous academic research, partly due to data unavailability; consequently, policy makers across the EU lack a reliable impact assessment of these programmes on final SME beneficiaries.

This paper aims to fill this research void. We carry out an impact assessment of the EU *SME Guarantee Facility* (SMEG)'s loan window under the *Multi-Annual Programme for Enterprise and Entrepreneurship, and in particular for Small and Medium-sized Enterprises* (MAP), focussing our attention on CESEE countries' beneficiaries.[2]

The loan window of the MAP guarantee facility for SMEs provided guarantees on loans to borrowers by covering a share of the default risk of the loan. The MAP guarantee facility for SMEs was managed by the European Investment Fund (EIF) that, under the mandate of the European Commission, extended credit guarantees to financial intermediaries. The financial intermediaries participating in the SMEG MAP facility were public and mutual guarantee institutions, as well as microfinance institutions and commercial or publicly-owned or controlled banks. Loan granting under the MAP loan window in the CESEE region took place from 2003 to 2010, where around 16,000 loans to more than 14,000 SMEs were supplied.

We have at our disposal an administrative dataset – managed by the EIF and never before exploited for research purposes – containing information at the level of both the financial intermediary benefiting from the guarantee and the single final beneficiary (SME) engaging in the loan transaction. Since we intend to focus on the *economic additionality* of the MAP programme – that is its impact on firm performance – we merge the MAP SMEG dataset information on SME loan recipients with the Bureau van Dijk's *Orbis* data on their balance sheets and profit/loss accounts, obtaining a data-rich treatment group, containing information on both loan transactions and firm performance.

[2] CESEE countries are on EU policy makers' radar screens due to the particularly acute financial distress brought about by the crisis; hence they represent an interesting case to test the effects of financial support programmes.

Descriptive statistics of this treatment group can go some way towards an assessment of the MAP economic additionality: indeed, a *Before-after analysis* can show that different measures of beneficiary SMEs' performance have improved in a statistically significant way in the years after the obtention of a MAP-guaranteed loan. However, such analysis lacks an observed counterfactual for every one of the treated individuals. Obviously it is not possible to observe at the same time the effects over the same individual that follow from receiving and not receiving the treatment, given that only one of those two states of the world may occur at the same time. Given this situation, in order to assess the impact of the MAP-guaranteed loan on the treatment group it is necessary to find a comparable *control group*, whose firms are then to be matched with the "closest" (most similar) SMEs in the treatment group.

To construct an appropriate control group, we address several issues, arising, *inter alia,* from the non-random and staggered nature of loan granting (which may generate biased impact estimates) and from the imperfect comparability of the treatment and control groups. To this purpose, we use a propensity score mechanism which associates MAP CESEE beneficiaries with the most similar non-beneficiary firms, before the actual obtention of a MAP-guaranteed loan.

Having matched each treated firm with the appropriate control, we proceed to estimate the impact of the MAP-guaranteed loan on treated SMEs' performance. To this purpose, we use a *Difference-in-Differences* (DID) estimator, which compares the before-after performance of treated firms with the before-after performance of control firms, under the (validated) assumption that the two groups follow a common trend. Note that while the matching controls for the influence of *observable* firm characteristics on firm performance, the DID estimator controls for the influence of *unobservable*, time-invariant factors.

Our results show that, on average, MAP CESEE beneficiaries have experienced a significant increase in *employment* in the order of 14% to 18%, compared to their counterfactuals. A similar result, albeit slightly less significant, is the rise in *turnover* up to 19% within the first five years after signature date. While the overall effect on turnover is mirrored at country level – although with different degrees of robustness – the impact on employment is driven essentially by a positive and significant effect in Romania and a weaker positive effect observable in the medium run for Czech MAP beneficiaries.

In general, the companies benefiting the most from the size (e.g. employment) effects of the MAP facility belong to the category of Micro and Small enterprises, and are typically young companies. From the decomposition by signature year, we note that MAP beneficiaries from signature years 2005 and 2006 have benefitted significantly from the programme in terms of employment growth, while for those companies receiving a loan in 2007 the MAP does not seem to have brought any significant effect. Interestingly, those cohorts featuring a steeper increase in employment also exhibit a decrease in productivity, probably due to temporary allocative inefficiencies following a MAP-guaranteed loan. While these firms belong solely to Romania, their productivity behavior is reflected in the overall estimation, which also features an immediate drop in productivity, typically reabsorbed in the fourth year after loan obtention.

Overall, our findings suggest that the EU SME Guarantee Facility has been successful in bringing significant positive effects on beneficiary firms in CESEE Countries, both in terms of size (represented by employment) and in terms of sales (represented by turnover).

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1 Introduction

Small and Medium-sized Enterprises (SMEs) constitute the connective tissue of the EU productive fabric: they represent 99.8% of EU companies, almost 60% of GDP (total value added) and near 70% of the total workforce.

Yet despite their economic importance, SMEs typically face greater problems than larger firms in several aspects, including access to finance, especially during financial crises.[3] Evidence shows that they face a wider wedge between the cost of internal and external finance:[4] SMEs encounter more difficulties in diversifying financing, both cross-border and by source, and have to rely mostly on local bank credit;[5] in addition, SMEs' limited sources of funds are further constrained by higher credit rationing and more penalising credit conditions (European Central Bank, 2014). Notably, also financially viable SMEs run into access to finance issues, with dire consequences in terms of allocative efficiency and growth.[6]

Economists broadly agree that the market failure underlying such inefficiencies lies in the lender's information and monitoring costs stemming from asymmetric information,[7] which are typically exacerbated by the lack of adequate collateral vexing many SMEs.[8]

To address this market failure, national and supranational governments and organizations in the EU have long adopted a variety of financial measures aimed at supporting SME finance, ranging from grants to financial instruments (direct lending, credit guarantee and counter-guarantee schemes, equity financing, securitisation of SME loans). Non-financial measures have also been enacted, including regulatory discipline privileging SMEs and the setup of promotional banks supporting SME finance.

Through these channels, trillions of euros of SME financing have been supported over the years; only in 2007-2013, at least 150 billion SME financing has been supported thanks to EU public financial measures (through grants, loans and equity).[9]

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[3] On the severity of the access to finance issue for EU SMEs, see Wehinger (2013), European Commission (2014a), Kraemer-Eis, Lang, and Gvetadze (2014), and European Commission and European Central Bank (2014); for CESEE countries in particular, see Pissarides (1999) and the EBRD reports cited therein, as well as European Investment Bank (2014).

[4] See Hubbard (1998), Lerner (1999), Carpenter and Petersen (2002).

[5] Hoffmann and Sørensen (2015) argue that SMEs are local in at least three dimensions: their output and input markets, their ownership structure, their access to finance; this makes them particularly exposed to local shocks, which they have difficulties in diversifying away.

[6] A recent study by the European Commission (European Commission, 2013a) was able to conclude that, in the 2009-2012 period, up to 860,000 financially viable SMEs in the EU were unsuccessful in obtaining a bank loan, thus suffering from a financing gap estimated at up to €112 bn.

[7] Jaffee and Russell (1976), and Stiglitz and Weiss (1981) pioneered the analysis of credit rationing due to asymmetric information, resulting in adverse selection and moral hazard.

[8] On the effect of collateral on loan amounts and conditions, see Bester (1985) and Besanko and Thakor (1987).

[9] This calculation is based on rough estimates of grants and financial instruments (direct loans, credit guarantees, equity financing, securitisation) provided by the EU – both centrally and through shared

The use of Public Credit Guarantee Schemes (PCGSs) is particularly widespread across OECD and non-OECD economies, as a direct policy tool to alleviate SMEs' financial distress (OECD, 2013).[10] PCGSs play a prominent role in the transmission channel of EU support to SMEs in the form of guarantees for loans. Estimates by OECD (2015) on 14 EU members indicate an amount – certainly underestimated[11] – of around 200 billion provided to SMEs in 2007-2013, capable of generating a tenfold amount of financing. For example, in 2007-2013 the Spanish government provided over 70 billion euros in loan guarantees for SMEs.

Despite its policy relevance, SME financial support in general, and credit guarantee schemes in particular, have hardly been the subject of rigorous academic research, partly due to data unavailability; consequently, policy makers across the EU lack a reliable impact assessment of these programmes on final SME beneficiaries. This problem has long been identified (Vogel and Adams, 1997) and has recently been forcefully put in the spotlight by the European Court of Auditors (2012), where the Court recommends that:

> *"The Commission should provide a reliable and technically robust monitoring and evaluation system specific to financial instruments. As a result, financial instruments should be segregated from pure grants in the Commission's monitoring, reporting and auditing processes and the amount of money actually paid to the SMEs should be transparent. In particular, the Commission and the Member States should agree on a small number of measurable, relevant, specific and uniform result indicators for financial instruments"*.

The problem is not peregrine: the credit guarantee policy has been criticised for failing to reach its target groups (SMEs) and impairing the development of an innovative private financial sector by making SMEs highly dependent on government policy (Oh et al., 2009). In addition, particularly in a slow growth period in the EU, national and European institutions bear the responsibility of

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management – during the 2007-2013 Multiannual Financial Framework, multiplied by estimates of "financial leverage" to arrive at an appraisal of the amount of financing supported by the EU. More specifically, figures for centrally-managed grants have been drawn from European Commission (2012), and for centrally-managed financial instruments from European Commission (2014b); grants provided through structural funds are indicated in European Commission (2013b), whereas financial instruments delivered through structural funds are computed based on European Commission (2014c). The leverage ratio adopted as a multiplier of the committed funds to obtain total SME financing has been computed as a weighted average of the grant leverage and the financial instruments leverage. Note that the figures refer only to EU budget commitments, thus excluding financing support by the EIB, EIF or other EU-related institutions.

[10] In the case of firms unable to meet the collateral requirements of the bank, a PCGS can lead to more credit being granted to the firm. Moreover, by reducing the informational asymmetries between a firm and a bank, the presence of a guarantee can lead to lower interest rates being paid by the borrower, hence reducing moral hazard and adverse selection problems. Meyer & Nagarajan (1996) have argued that credit guarantees can lead to a learning process, where banks discover that borrowers benefiting from the guarantee are not as risky and unprofitable as initially expected and become willing to provide loans to them in the future without a guarantee. On the other hand, a PCGS might equally lead to riskier behaviour by both the entrepreneur and the bank (D'Ignazio & Menon, 2013).

[11] The figure excludes PCGSs from international institutions as well as from some EU country's promotional banks.

focusing on the most effective support measures to ensure tangible added value and efficient use of taxpayers money.[12] It is therefore crucial to establish the pure benefits of credit guarantees, as compared to an appropriate control group of SMEs not benefitting from the guaranteed loan.

This paper aims to fill this gap. We carry out an impact assessment of the EU *SME Guarantee Facility's* (SMEG) loan window under the *Multi-Annual Programme for Enterprise and Entrepreneurship, and in particular for Small and Medium-sized Enterprises* (MAP),[13] focussing our attention on CESEE countries' beneficiaries. With the financial crisis, CESEE economies have suffered from a constrained supply of credit to SMEs as deleveraging, low profitability and rising levels of non-performing loans limited banks' risk-taking capacity. This makes these countries a preferred target for intervention by international institutions (*e.g.,* through the so-called *Vienna Initiative*) and an interesting case study for the effectiveness of such interventions.

We make novel use of an administrative dataset – managed by the European Investment Fund (EIF) – containing information at the level of both the financial intermediary benefiting from the guarantee and of the single final beneficiary (SME) engaging in the loan transaction.[14] By merging the MAP SMEG[15] dataset information on SME loan recipients with the Bureau van Dijk's *Orbis* data on their balance sheets and profit/loss accounts, we are able to construct a data-rich treatment group, containing information on both loan transactions and firm performance. The broad range of data on treated firms' characteristics allows us to extract from the *Orbis* database an appropriate control group of SMEs, which constitutes our counterfactual experiment. In order to guard against selection bias stemming from potential endogeneity of the beneficiary choice (Heckman and Hotz, 1989, and Heckman et al., 1998), we employ a Propensity Score Matching (PSM) technique to associate each treated SME with its controls, and then run a Difference-In-Differences (DID) regression to estimate the impact on firm performance of having received a MAP-guaranteed loan. Our approach builds and expands on the methodology used in similar impact evaluation studies, most notably Arraiz et al. (2011).

Our focus on firm performance differs from much of the scholarly literature on public credit guarantee schemes, which tends to concentrate its attention on financial additionality (better access to finance), rather than economic additionality (better economic performance).[16] While testing for financial additionality correctly investigates into the repair of the financial market failure, we believe that analysing the beneficiary SME performance is not only complementary to the financial additionality approach, but it also has two advantages: first, it looks into the ultimate goal of a PCGS, namely the support of firms' employment and output; second, it distinguishes the

[12] Indeed, the European Commission has anchored in the EU Financial Regulation and the Common Provisions Regulation the requirements for ex-ante assessments for all financial instruments.

[13] The programme was established by Decision 2000/819/EC and amended by Decision 2005/1776/EC. For further details, see section 2.1 below.

[14] During the whole process of research, private information on beneficiaries and financial intermediaries have been kept confidential, and all data have been treated anonymously.

[15] As mentioned in Section 3 below, the SMEG facility operated under different SME programmes over time. In order to avoid confusion and repetition, in this work we will use the term "MAP SMEG Facility" or simply "MAP SMEG" as shorthand for the SMEG Facility under MAP.

[16] See the Literature Review section below.

beneficiary firms which are economically and financially viable from the others. Indeed, a better performance after receiving a guaranteed loan testifies to the good use of the easier access to credit provided by the guarantee, and proves that the financial intermediary has selected a creditworthy beneficiary.[17] Since the purpose of PCGS is to support lending to firms who are typically denied credit while being financially and economically viable, focussing on economic additionality allows assessing whether the programme design fulfils its mandate.

The paper proceeds as follows. Section 2 reviews the literature on CGSs; section 3 provides background information on CGSs and the EU MAP; section 4 describes the construction of our novel dataset of MAP beneficiaries and their control group; section 5 illustrates our econometric strategy to estimate the impact of MAP-guaranteed loans on SME performance in the CESEE countries; section 6 presents our results, both for the overall sample and broken down by different criteria; finally, section 7 concludes and provides suggestions for further research.

2 Literature Review

Guarantees in general and public credit guarantee programmes for SMEs in particular are rarely evaluated in peer-reviewed studies with high academic standards.[18] Evaluation reports tend to be qualitative in nature, often involve beneficiary satisfaction surveys, small case studies, or simple regression analysis of outcomes without accounting for major statistical issues like selection bias in program participation (*e.g.* the projects that are the best candidates to be funded – in the sense of maximising the impact of public support – are also the projects that would have the highest expected output in the absence of funding, see Jaffe, 2002).[19]

A major obstacle to in-depth analysis seems to be the data requirements, which include detailed time series on the recipient firms, including information on their performance (turnover, employment, profits…), finances (assets, liabilities, loans, financial dependence…), and other relevant characteristics (age, location, legal form…). Few studies have examined such data, which are usually confidential and expensive to collect (Brown and Earle, 2008 is a noteworthy exception).

A stream of the literature has examined the impact of public credit guarantee schemes on the financial conditions of SMEs (so called financial additionality), rather than on firms' performance (economic additionality). Examples include studies on the Italian Confidi by Busetta and Zazzaro (2006), and Columba, Gambacorta, and Mistrulli (2006). Focusing on Italy, Zecchini and Ventura (2009) adopt a DID technique to find a causal relationship between the public guarantee and the higher debt leverage of guaranteed firms, as well as their lower debt cost. The authors

[17] Stiglitz and Weiss (1981) distinguish between *pure credit rationing* – when the borrower is denied a loan, despite sharing the same characteristics with accepted borrowers – and *redlining* – when borrowers do not receive credit at any interest rate because their projects do not generate a high-enough return to the lender. By focusing on firm performance, our methodology can better isolate the first type of credit rationing.

[18] OECD (2013) also concludes that assessment of CGSs based on evidence is rather scarce.

[19] A selection bias would also arise if candidates to be funded through the public credit guarantee were chosen by financial intermediaries with a view to off-load their riskier portfolio tranches.

conclude that guarantees by Confidis in Italy proved to be an effective instrument in these respects. The authors' result shows that Italy's scheme has reached a measure of effectiveness in reducing SMEs' borrowing cost and easing their financing constraints. The cost reduction is evaluated to be in the range of 16-20%, while the additional supply of credit by banks is estimated at 12.4% at the median. In addition, D'Ignazio and Menon (2013) illustrate that a regional credit guarantee policy in Italy was effective to improve financial conditions for the beneficiary firms. Targeted firms benefited from a substantial decrease in interest rates, but the authors do not find a significant effect on real performance (that is, firm investments). In the same vein, Calcagnini, Farabullini, and Giombini (2014) show that collateral guarantees systematically reduce the interest rate of secured loans, while personal guarantees show no systematic effect on interest rates, but favour firms' access to credit. Castillo Bonilla and Girón (2014) use Stock-Watson dynamic OLS to show that the National Guarantees Fund increases the availability of credit to Colombian SMEs.

Previous academic work on the economic additionality of guaranteed loans, which is mostly extra-European, is generally marred by the lack of either suitable data or shortcomings in the methodology. Brash and Gallagher (2008) analyse SME sales, employment, and survival before and after US Small Business Administration (SBA) financing, using standard multivariate OLS with no control group. Riding and Haines (2001) measured both costs and economic benefits of a public loan guarantee program in Canada, the Small Business Loans Act (SBLA); however, benefits were gauged through a telephone survey on recipients.

High-standard work in developing countries includes Benavente and Crespi (2003) who, using survey data and a "Difference in Differences with Common Support" estimator (based on matching techniques), evaluate Chile's Program of Development (PROFO) and find significant net improvements in TFP growth ranging from 11.7 to 22.9 per cent. Arraiz, Stucchi, and Henriquez (2011) evaluate the impact of a government-subsidised training programme on SME performance in Chile. After identifying a control group through Propensity Score Matching (using nearest-neighbour-matching), they control for the unobservable effect of time-invariant firm characteristics through DID estimation. The programme is found to have increased sales, employment, and the sustainability of small and medium-sized suppliers.

Oh et al. (2009) evaluate the effect of Korean credit guarantee policy in terms of growth of productivity, sales, employment, investment, R&D, wage level of the supported firms and their survival rates. To avoid the selection problem, the authors adopt a kernel-based matching on propensity scores; then they estimate the treatment effect by using difference in means. Results suggest that the guarantee provision helped supported firms to increase or maintain their size in terms of sales and employment and to hire more skilled employees (or it helped to promote welfare of the employees), although credit guarantees did not help firms increase their R&D and investment and hence, growth in productivity. Moreover, the selection of firms to receive guarantee funds was not linked to the productivity of the supported firms.

Evidence from some CESEE countries (Bulgaria, Georgia, Russia and Ukraine) can be found in Cassano, Jõeveer, and Svejnar (2013), who used survey data to focus on European Bank for Reconstruction and Development (EBRD) programmes for lending and performance of SMEs. Their estimates suggest that both EBRD (cashflow-based) and non-EBRD (collateral-based) bank loans have a significant positive effect on most performance indicators of SMEs: they find a positive effect on fixed assets as well as revenues and employment. The two sets of loans differ for the effect on profitability, with firms receiving EBRD loans being more profitable than the corresponding control group of non-EBRD loans' recipients. They also find that none of the effects of non-EBRD loans varies with loan size, while the effect of EBRD loans increases with loan size (relative to revenues) for fixed assets and labour costs, and decreases with loan size for employment. The effects of both types of loans on revenues and profit are invariant with respect to loan size. The study employs a DID regression which controls for the probability of obtaining a cashflow-based or collateral-based loan.

Bah, Brada, and Yigit (2011) look into the effects of USAID's technical and financial assistance on Macedonian SMEs. Controlling for selection bias in programme participation, they use kernel propensity score matching with caliper to estimate the excess growth of employment in assisted firms. They find that assistance programmes raised employment growth by 16-20 percentage points in the first year after assistance and by 26-30 points by the third year. Brown and Earle (2008) also analyse the effect of USAID lending on firm-level employment, sales, and survival for the case of Romanian SMEs. They apply a nearest-neighbour propensity-score matching to survey data, and estimate both difference-in-differences models including matched-pair-specific fixed effects, and models with full dynamics of the effect around treatment date.

The literature has also looked at macroeconomic models to evaluate the effect on SMEs of credit guarantee policies. For instance, Schmidt and van Elkan (2010) developed a macro-economic model to evaluate the effects generated by the activities of the German Guarantee Banks for the forecast period from 2009 to 2015. According to the authors, in all the different specifications GDP increases, the number of employees increases and the number of unemployed falls. In their most realistic specification, Gross Domestic Product increases by an average of EUR 3.4 billion per annum, the number of employees increases by an average of 29,500 per annum and the number of unemployed falls by an average of 23,200 per annum.

3 CGSs and the EU MAP

Credit guarantee schemes (CGSs) provide guarantees on loans to borrowers by covering a share of the default risk of the loan. They are a common feature of financial systems across the world. In many countries, CGSs have existed since the beginning of the 20th century (Beck et al., 2010), but they have experienced unprecedented growth over the last several decades, across OECD and non-OECD countries alike. In particular, CGSs have been an instrument of choice for policy makers to improve access to finance by SMEs and young firms during the recent global financial crisis. Green (2003) calculates that over 2,250 schemes exist in various forms in almost 100 countries and Pombo (2010) estimates that some form of credit guarantee service exists in most regions of the world, although the design and delivery mechanisms of such schemes are rather heterogeneous. The major types of guarantee systems which can be identified are mutual guarantee associations, publicly operated national schemes, corporate associations, schemes arising from bilateral or multilateral cooperation, and schemes operated by NGOs (Green, 2003).

Public CGSs are used in many developed and developing economies to alleviate the constraints facing SMEs in accessing finance. Depending on the scope of the activity, CGS can feature "intermediate" (or "wholesale") guarantees (to facilitate the refinancing of non-bank financial institutions lending to business), "individual" (or "retail") guarantees (on a case-by-case basis, to banks or other financial institutions lending to business) or "portfolio" guarantees (to cover a pre-set volume and type of loans agreed by a lender to his business clients). In addition, direct credit guarantees are granted directly to the lending financial institution; indirect or counter-guarantees protect the main guarantor by sharing participating in his losses.

The EU SME Guarantee (SMEG) facility originated in Council Decision 98/347/EC of 19 May 1998 on measures of financial assistance for innovative and job-creating SMEs — Growth and Employment (G&E), and was continued as part of the subsequent Multi-Annual Programme for Enterprise and Entrepreneurship (MAP), established from 2001 to 2006.[20]

The MAP consists of a set of activities which are designed to improving the overall business environment in Europe. The Programme activities are grouped within three pillars:

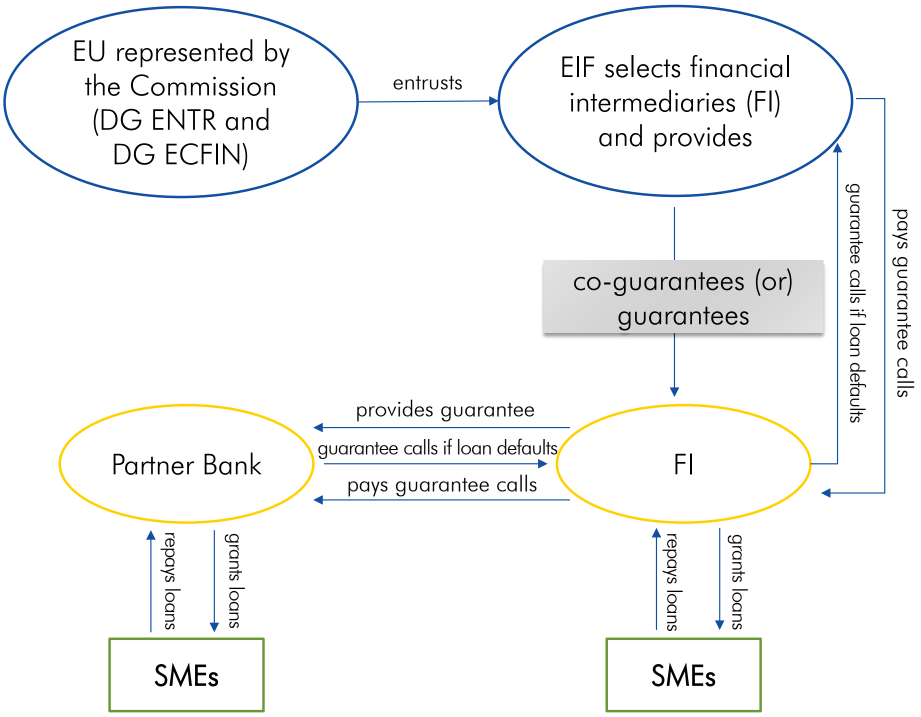
1. Policy Development, where the Commission and Member States study and disseminate policy recommendations designed to ameliorate the overall business environment;
2. Euro Info Centre (EIC) Network, which supports local information centres all over Europe that inform, advise and assist SMEs in EU-related areas;
3. Financial Instruments, which are specifically targeted towards improving the financial environment for businesses, especially SMEs.

In terms of money committed, the third pillar is the most important, and among the Financial Instruments the SMEG facility is the largest. The SMEG operates through 4 windows, guaranteeing loans, micro-credit, equity, ICT loans. Like any Credit Guarantee Scheme, the loan window of the MAP guarantee facility for SMEs provides guarantees on loans to borrowers by covering a share of the default risk of the loan.

Two directorates-general at the European Commission, plus the European Investment Fund (EIF), were involved in the design, management and monitoring of the SMEG facility under MAP. The former Directorate-General for Enterprise and Industry (DG ENTR), now Directorate-General for Internal Market, Industry, Entrepreneurship and SMEs (DG GROW) was the coordinator of the legal basis, prepared the annual work programmes and implementation reports of the various MAP pillars and was responsible for organising the programme evaluations. The Directorate-General for Economic and Financial Affairs (DG ECFIN) had the direct management responsibilities for SMEG, being in charge of negotiating the Fiduciary and Management Agreement (FMA) with the EIF, approving of financial proposals, monitoring cash movements on the trust accounts and ensuring that funds are spent in line with the MAP's objectives. The SMEG process under MAP can be illustrated through a flow diagram as in Figure 1.

[20] The SMEG facility remains an important financial instrument also under the Competitiveness and Innovation Framework Programme (CIP), established in 2007 under the Entrepreneurship and Innovation Programme (EIP), and the Programme for the Competitiveness of Enterprises and Small and Medium-sized Enterprises (COSME), established in 2013.

Figure 1: Overview of the SMEG MAP process flow



Source: Authors, based on European Court of Auditors (2011).

The EIF provided guarantees to financial intermediaries aiming to improve their lending capacity and, therefore, the availability and terms of loans towards SMEs. The EIF signed guarantee agreements in its own name, on behalf of the Commission and at the risk and cost of the Union budget. In order to allow the Commission to monitor the facility, the EIF reported to DG ECFIN on the progress achieved on a quarterly basis. The financial intermediaries participating in the SMEG MAP facility were public and mutual guarantee institutions, as well as microfinance institutions and commercial or publicly-owned or controlled banks. They could be direct lenders providing loans to SMEs or indirect lenders (e.g. public/promotional banks extending (global) loans to other financial intermediaries that then used these funds for providing loans to SMEs) or indirect guarantee organisations (that either co-guaranteed or counter-guaranteed a SME loan portfolio of one or several direct lenders). Each guarantee deal determined *inter alia* specific target volumes for the new dedicated portfolios to be achieved by the financial intermediary. For each defaulted loan, the losses were shared between the EU and the financial intermediary (counter-guarantor, guarantor or bank) on the basis of a portfolio guarantee rate (typically 50%). In addition, portfolio (counter-) guarantees were capped (typically at 20% rate). SMEs had to meet the Commission's SME definition[21] to be eligible for a guarantee under the SMEG facility. [22]

As of 31 December 2014 the MAP SMEG Facility supported 266,501 loans with an outstanding volume of EUR 28,162 million, benefiting 234,413 SMEs and an estimated 940,849 employees.

[21] An SME was defined as an enterprise which employs fewer than 250 employees and which has an annual turnover not exceeding EUR 50 million and/or an annual balance sheet total not exceeding EUR 43 million. See Commission Recommendation of 6 May 2003 (2003/361/EC).

[22] See European Court of Auditors (2011).

These results have been achieved through 51 agreements with 47 intermediaries based in 28 different countries.

4 Data

4.1. EIF MAP Database

The database of the SMEG facility under MAP[23] is managed by EIF and contains information both at the level of the financial intermediary and of the associated transaction. We select data concerning exclusively those transactions issued in CESEE countries within the MAP "loan window",[24] which contains information on 16,051 loans and 14,400 individual beneficiary firms operating in the region.[25] In total 20 CESEE financial intermediaries[26] signed contracts under MAP. The distribution of transactions in CESEE – whether in terms of amounts of loans issued, number of employees supported or a simple count of transaction – is concentrated in four countries (BG, CZ, PL, RO), which alone represent two thirds of all the loans issued under MAP in CESEE.

The MAP Programme was adopted for a period of 5 years starting from January 1, 2001. The actual deployment of the MAP SMEG facility in the CESEE region took place from 2003 to 2010. However, 94% of all issued transactions refer to the triennium 2005-2007, as depicted in Figure 2. Therefore, we focus the analysis on this specific period.

For each MAP-guaranteed loan transaction, the database reports a series of characteristics relating to the transaction itself or the beneficiary firm (*e.g.* guaranteed loan amount,[27] purpose of financing, company name, *etc.*).

The average loan issued in the CESEE region under MAP amounts to 75,000 EUR, to a firm with an average of 18 employees and with a maturity of 5 ½ years. However, the "typical" loan issued under MAP (*i.e.* the mode of the distribution) is much smaller in size and scope: an amount that ranges from 6,000 to 15,000 EUR, a borrower with 1 to 4 employees, and a maturity between 3 and 5 years.

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[23] As mentioned in Section 3, the SMEG facility operated under different SME programmes over time. In order to avoid confusion and repetition, in this work we will use the term "MAP SMEG Facility" or simply "MAP SMEG" as shorthand for the SMEG Facility under MAP.

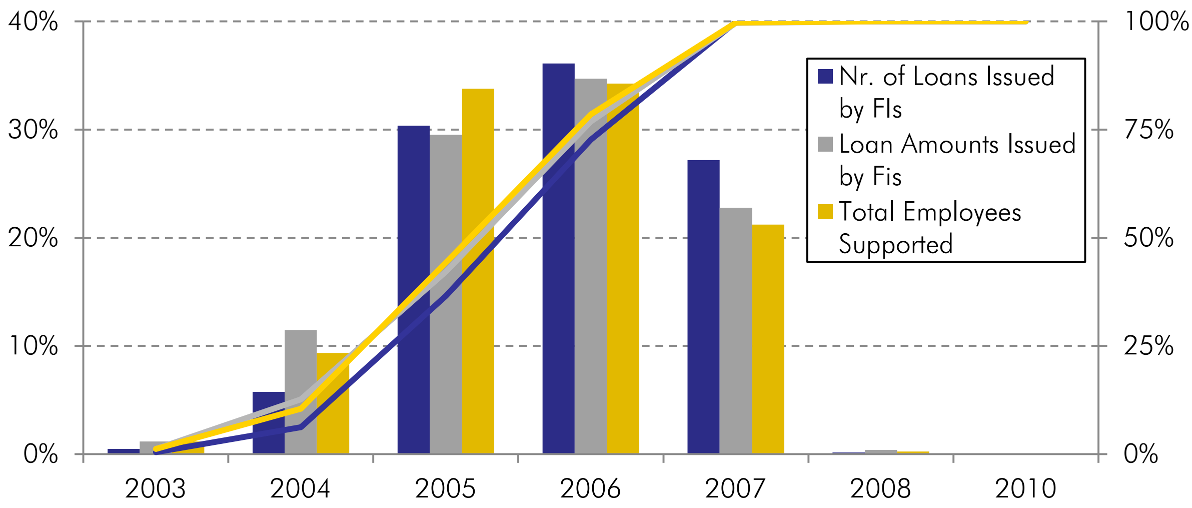
[24] Including only direct or indirect guarantees on loans to SMEs. Alternative active windows were the *micro-loans* and *equity investments* windows (see MAP SMEG's legal base and Fiduciary Management Agreement, European Court of Auditors, 2011. A detailed overview is provided in Section 3 above).

[25] Firms are identified through their company name and an internal ID code collected by the EIF. As such, the actual number of enterprises is lower, due to multiple IDs associated to the same enterprise.

[26] One financial intermediary merged with another financial entity, which then took over the existing portfolio and created a new one.

[27] A significant portion of the financial amounts included in the MAP database are expressed in local currencies. In order to take into account the original value of the variables at the time when each transaction took place, we use the ECB's historical bilateral exchange rate series between the EUR and each specific currency.

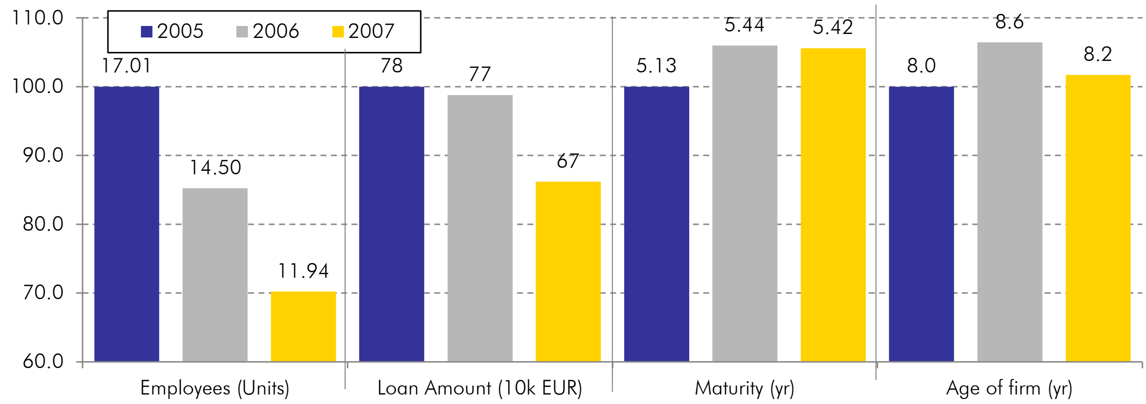
Figure 2: Deployment of the MAP SMEG facility in the CESEE region



Notes: Lines represent the cumulative distribution. Source: Authors

Further decomposition over signature years shows a decline of both the average loan amount and the number of employees in the cohorts from 2005 through 2007 (Figure 3). Similar trends can be appreciated, for most countries, by looking at the loan issuance dynamics within the country itself.

Figure 3: Average values per loan-granting year (2005 = 100)



Note: labels indicate actual values. Source: Authors

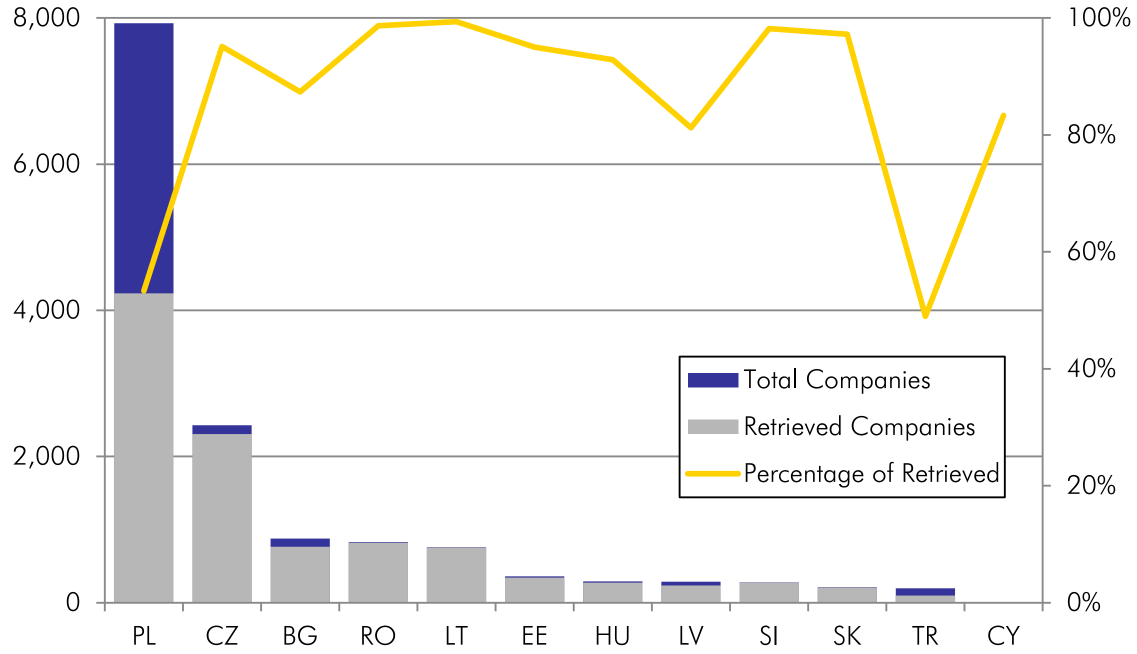
Differences arise not only across cohorts, but also across countries: for instance, the average Romanian borrower features three times as many employees as the Czech. Clearly, the sample characteristics do not simply mirror the respective SME population features, but also depend on the eligibility criteria stipulated in the guarantee agreements between the EIF and each financial intermediary. Indeed the MAP was not a one-size-fits-all programme, but allowed to accommodate specific portfolios of financial intermediaries within the set of predefined eligibility criteria.

4.2. Collection of company-level data

The data included in the MAP database is aimed at satisfying EIF's requirements in terms of sound administration and reporting. In particular, it does not report data at final beneficiary level after the guarantee has been issued.[28] The lack of longitudinal data makes this database not sufficient for a full-fledged econometric analysis. Therefore, in a preliminary step of the analysis we merge information included in the MAP database with company-level data on financial statements, provided by the Bureau Van Dijk's *Orbis* Database (see Appendix I for an overview of the provided financial indicators). The detailed strategy on the pairing of MAP CESEE beneficiaries is described in Appendix II.

Given the small size, young age and/or remoteness of some final beneficiaries, not all companies present in the MAP database could be retrieved in the Orbis Database. Yet a total of 10,043 single companies have been enriched with data contained in Orbis, amounting to approximately 71% of all beneficiaries.[29] A breakdown of successfully retrieved companies per country is depicted in Figure 4. Because of the low share of retrieved information for Turkish and Cypriot firms, we exclude both countries from the analysis.

Figure 4: Number and share of companies retrieved by country



Source: Authors

Using data available in the MAP database pertaining to the characteristics of the beneficiary and the transaction, we observe that the SMEs left unpaired after this process tend to be smaller in size

[28] Except for information concerning the progress of the guarantee itself (i.e. guarantee call, call amount).

[29] This percentage takes into account the double counting of beneficiaries in the MAP database (see Note 25).

than the average (both in terms of number of employees and amount of loan received). Moreover, most of the unpaired enterprises are located in Poland. This creates significant differences between the characteristics observable in the retrieved subset and the overall MAP population, an aspect that could undermine the representativeness of the final sample to be used in the study.

We tackle this particular sample bias by comparing the empirical probability density functions of the indicators available in the original MAP database between the paired and the unpaired firms, also by using a Kolmogorov-Smirnov test Kolmogorov, 1933). The results of this analysis led to the estimation of *ad-hoc* weights based on available indicators included in the MAP database (see Appendix IV).

4.3. Sample selection for descriptive analysis

A final sample of 2,595 MAP beneficiaries[30] was obtained upon removal of outliers and various inconsistencies from the available data on paired companies (see Appendix III). While the size of the final sample, compared with the size of the population, is sufficiently large to allow for a 2.3% margin of error at 99% confidence level, there are some concerns whether such subset can be considered representative of the original group of MAP CESEE beneficiaries. In particular, we remarked in Section 4.2 and Appendix III how both the *merging* procedure with the Orbis database and the incidence of missing values affect the original dataset in a non-random way.

We test this conclusion by using some of the indicators originally included in the MAP database. Both firms' number of employees and loan amount are significantly higher[31] for the final sample with respect to the original population of MAP CESEE beneficiaries. We also notice a significant distribution discrepancy across countries, a feature that was easily identifiable from Figure 4 already but which has been further exacerbated by the selection criterion. On the other hand, the age of the company upon the receipt of the loan is less affected by the selection criterion, leading to a non-significant difference between the population and the matched sample at the 99% confidence level.

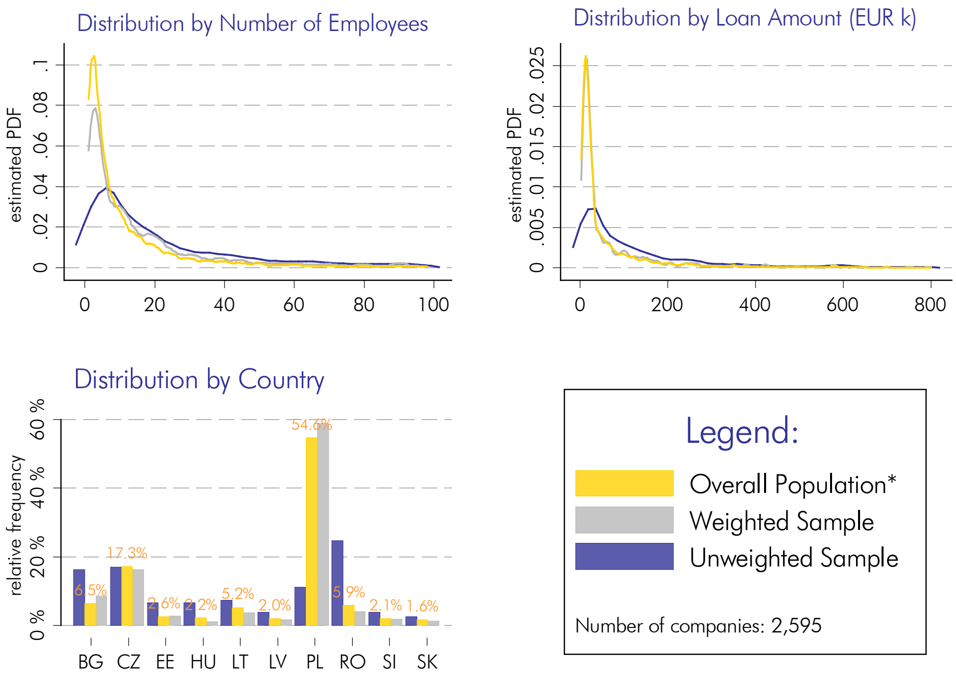
Given the results of these tests, we cannot assume that the final sample guarantees an unbiased portrayal of the original population. We therefore set about reducing the bias of the final sample by re-weighting it based on three different firm's characteristics: the country in which it operates, the registered number of employees at loan issuance, and the economic size of the underlying loan (see Appendix IV). The validity of our re-weighting mechanism is based on the assumption that – were data on companies with missing information to be retrieved – it would have been distributed in the same way as the available company data in the specific weighting cluster.

Various robustness checks have been applied to the final weights in order to test their validity and robustness. An overall assessment of the performance of the estimated sampling weights can be observed in Figure 5.

[30] Due to firm-specific missing values in a given period, the composition of the sample is subject to some (limited) variation over time.

[31] At 99% confidence level.

Figure 5: Distributions of treatment sample after reweighting



**Of MAP CESEE beneficiaries. Source: Authors*

4.4. Analysis of the Panel Dataset

This section concludes our illustration of the data by presenting a number of descriptive statistics related to our final sample. An extended version of this section can be found in EBCI (2014). Table 1 presents the main financial and demographic characteristics of MAP CESEE beneficiaries. In particular, we include a number of financial ratios, such as the return on assets and the return on equity, but also a *financial independence indicator* (defined as working capital and cash flow over total assets) to measure the extent to which firms are independent of external financing. Moreover, we add an indicator of *financial pressure* (interest paid over cash flow, see Nickell and Nicolitsas, 1999) which expresses the ability of firms to repay their debts. Note that all currency values have been previously deflated using country-level price indices for 10 industry branches obtained from Eurostat.[32] Finally, we include the estimated total factor productivity (TFP) for a number of MAP CESEE beneficiaries (see Appendix V for a theoretical reference).

We also compute the average weighted values of three key performance indicators (KPIs), *i.e.* number of employees, turnover and total assets, over the rolling window described in Appendix II. Figure 6 shows the results of this exercise: we observe that, on average, firm's KPIs significantly increased during the 5 years after a MAP-guaranteed loan. However, we also notice a slowdown or a drop in KPI levels in the 3rd and 4th year following the obtention of a loan, a feature that is likely to be a consequence of the economic and financial crisis in the European Union, and in particular in the CESEE region. Indeed, we find a high correlation between the levels in Figure 6 and the GDP of the region, although such correlation progressively loses strength in the second and third cohort.

[32] Missing values for Bulgaria in sector C have been replaced with adjusted data from the National Statistical Institute of Bulgaria. We use 2005 as a baseline year.

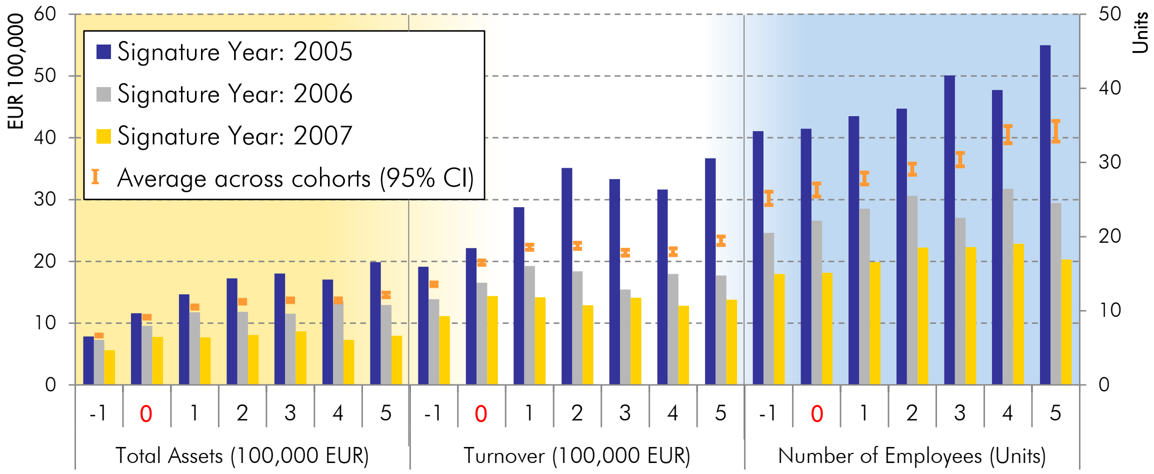
Table 1: Descriptive statistics of MAP CESEE beneficiaries at signature date

Variable	Average	Std. Deviation	Median	Min	Max
Number of employees	24.25	33.279	14	1	250
Firm's age	7.99	5.287	7	0	60
Financial Pressure	0.240	0.548	0.102	0	8.730
Financial Independence	0.320	0.291	0.303	-1.804	1.552
ROE (using P/L before tax %)	0.335	0.625	0.265	-9.232	9.190
ROA (using P/L before tax %)	0.090	0.154	0.060	-0.740	0.973
TFP (e^{θ_i})	1.352	0.706	1.246	0.178	3.994

Note: statistics of each variable were re-weighted to reduce missing values bias, following the methodology of Section 4.3. Source: Authors

Finally, Figure 6 also highlights significant differences in KPI levels across signature years, a finding that is consistent with the analysis of the EIF MAP Database (see Figure 3). It is important to underline that, in the absence of an appropriate control group, this before-after characterisation cannot be used to rigorously evaluate the performance of MAP-guaranteed loans. Indeed, as suggested above, KPI behaviour may well have been driven by macroeconomic forces, which would have operated also in the absence of the programme.

Figure 6: Average performance of MAP CESEE beneficiaries before and after signature date (t=0)



Note: financial values deflated using Eurostat's country-level price indices for 10 industry branches, reference year 2005. Average values re-weighted following the methodology described in Section 4.3. Source: Authors

5 Econometric Strategy

5.1. Theoretical framework

This section builds on the creation of the final sample of firms and introduces our econometric strategy to estimate the economic impact of the MAP in the CESEE region. In order to establish a causal relationship between the obtention of a MAP-guaranteed loan and economic performance, we employ Rubin's Causal Model (Rubin, 1974) based on the concept of *potential outcome*. The main implication of the potential outcome framework is that, conditional on few specific assumptions, the alternative unobservable outcome of treated enterprises, had they not received the treatment (*i.e.* a MAP-guaranteed loan), is replaceable, *on average*, with the outcome of an appropriate control group.

Let us define as potential outcome of a given MAP CESEE beneficiary the expression $Y_i^k|D_i$ with $k \in \{0,1\}$, where Y_i^1 is the *observed* outcome of company *i*, and Y_i^0 represents the *unobservable* outcome of company *i*. The binary variable D defines the firm's treatment status, that equals 1 if treated and 0 otherwise. Therefore, the expression $Y_i^1|D_i = 1$ identifies the *observed* outcome of company *i* when treated, and $Y_i^0|D_i = 1$ the *unobservable* outcome of treated company *i* had it not been treated. In a potential outcome framework, the *Average Treatment Effect (ATE)* can be estimated as follows:

$$\begin{aligned} ATE &= E\left[Y_i^1 | D_i = 1\right] - E\left[Y_i^0 | D_i = 1\right] \\ &= E\left[Y_i | D_i = 1\right] - E\left[Y_i | D_i = 0\right] \end{aligned} \tag{1}$$

i.e. as the average outcome of the treatment group minus the average outcome of the control group. For the ATE to be identifiable (*i.e.* for equation (1) to hold), the model needs to satisfy the following two assumptions:

1. Stable Unit Treatment Value (SUTVA)
2. Unconfoundedness and overlap

SUTVA

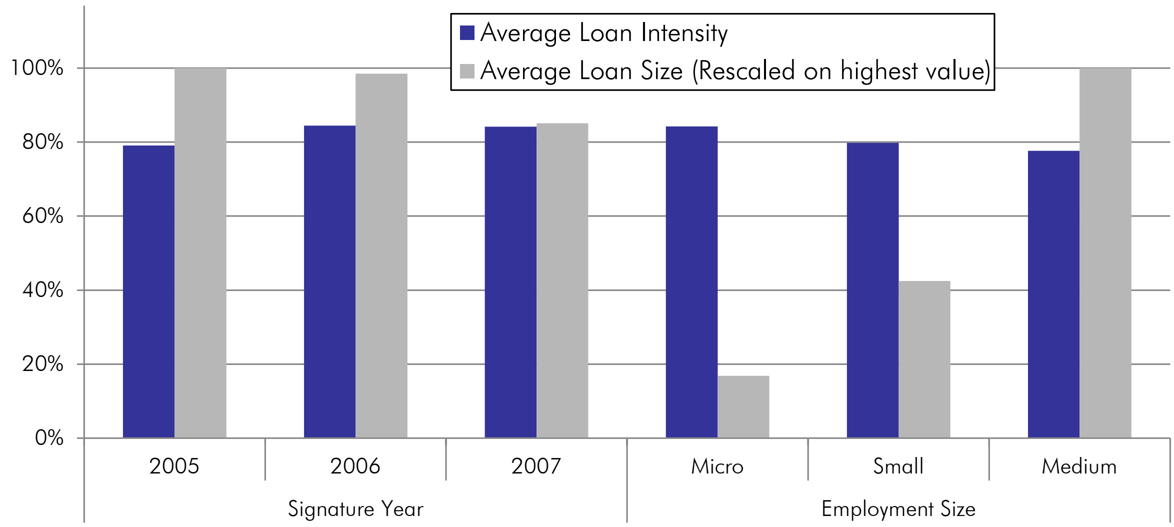
The SUTVA implies that *a)* there is *no interference* between the treatment status of a specific unit and the potential outcome of the other units, and *b)* the treatments for all units are comparable, *i.e.* there is no variation in the *intensity* of the treatment. Concerning point *a)*, this assumption cannot be readily postulated in our sample. Theoretically, there are no observable *direct* effects brought about by a MAP-guaranteed loan,[33] but there could be *indirect* effects, such as *externalities* caused by beneficiaries that could impact the potential outcome of other firms (*spillover* effects).

While it seems reasonable to assume that no spillovers take place either *cross-country* (as financial intermediaries are typically acting at national level) or *cross-sector* (different product lines), there could be in principle a spillover effect at country *and* sector level, for a very limited number of treated companies (*e.g.* if treated company A receives the treatment before treated company B and they both operate in the same product line). In order to verify statement a), we test (and confirm) the absence of such spillovers in our data (see Appendix VI). Overall, our argument is also supported by the fact that the MAP does not constitute a typical change in the regulatory environment, thus only impacting eligible beneficiaries that self-select into the programme.

[33] This is true unless the MAP beneficiary happens to be a company involved in the management of the facility. However, such case was not observed in our CESEE sample.

With respect to point *b)*, we cannot exclude the hypothesis that the treatment's intensity is, in fact, heterogeneous. While this aspect is often overlooked in the literature, we believe this may potentially constitute a source of bias. In order to assess its magnitude, we adopt as a measure of loan intensity the ratio between the loan amount obtained and the envisaged investment amount (both variables included in the EIF MAP database).[34] We observe that treatment intensity is mostly, although not exclusively, explained by differences in country, signature year, firm's size and age (see for example Figure 7). As a result, our final aggregate estimates might suffer from this type of bias. Therefore, as a robustness check, we estimate the average treatment effects within different *clusters*,[35] on the basis that treatment intensity within clusters is more homogeneous. Although we cannot directly test for the validity of our aggregate estimate, we argue that through the results of our robustness tests, the overall estimate properly reflects the findings observed within the analysed clusters.

Figure 7: Observed heterogeneity of treatment



Source: Authors

Unconfoundedness and overlap

We now tackle the notion of unconfoundedness. This assumption essentially means that the assignment of the treatment (the MAP-guaranteed loan) must be independent of the outcomes. This cannot be readily assumed in this study, since *i)* the treatment group is not randomly sampled and *ii)* we do not have a pre-defined control group (*e.g.* a set of companies denied to join the guarantee scheme). In other words, the treatment may not be assumed to be *randomly assigned*, and the estimation of the ATE through equation (1) will lead to biased results. However, we can fulfil Assumption 2 by assuming that the treatment becomes independent of the outcomes once conditioned upon a set of observable firm's characteristics:

[34] We repeat this test using a different measure of loan intensity, that is, the ratio of loan amount and firm's total assets, obtaining similar results.

[35] When sample size permits.

$$\left(Y_i^1, Y_i^0\right) \perp D \mid \mathbf{X} \tag{2}$$

where **X** represents a set of characteristics of the firm. Statement (2) is also known as the *conditional independence assumption* (CIA). Rosenbaum and Rubin (1983) demonstrate that if (2) holds, it is sufficient to control for the *propensity score* (*i.e.* the conditional probability of assignment to a treatment given **X**) in order to satisfy the uncounfoundedness assumption:

$$\left(Y_i^1, Y_i^0\right) \perp D \mid \mathbf{X} \Rightarrow \left(Y_i^1, Y_i^0\right) \perp D \mid p(\mathbf{X}) \tag{3}$$

where $p(\mathbf{X}) = Pr(D=1 \mid \mathbf{X})$ is the *propensity score*. In order to estimate $p(\mathbf{X})$ in a meaningful way, we need to satisfy the *overlap assumption*:

$$0 < Pr(D=1 \mid \mathbf{X}) < 1 \tag{4}$$

i.e. for all levels of **X**, there must be some nonzero probability of being treated as well as of not being treated. This particular assumption is satisfied by our propensity score mechanism, which ensures matching in the *common support* region[36] (see Section 5.2.2 for the implementation of our propensity score model).

However, condition (3) can still be violated in case unobservable characteristics exist and are associated with characteristics that are unbalanced between treatment and control group. In the case of our study one such characteristic could be, for instance, the *private information* held by financial intermediaries with respect to the final beneficiary. This typically unobserved feature could be associated with the selection into the treatment group, thereby leading to biased estimates of the ATE. Following Blundell and Costa Dias (2000), we exclude this possibility by assuming that unobserved characteristics affect participation exclusively by means of individual- and/or time-specific components of the error term.[37] Given such framework, the CIA in Equation (3) can now be replaced with:

$$\left(Y_{t_1}^1 - Y_{t_0}^1, Y_{t_1}^0 - Y_{t_0}^0\right) \perp D \mid p(\mathbf{X}) \tag{5}$$

where t_0 and t_1 are respectively the *before-* and *after-* treatment periods. Condition (5) represents the *common trend* assumption, which grants that the ATE is identifiable if one can assume that, in the absence of the treatment, treated companies would have evolved in the same way as control firms. In this respect, the combination of *propensity score matching* and *difference-in-differences*

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[36] Regions of the density function of X where the support does not overlap for the treatment and control group.

[37] In order to clarify this point, let us assume a general specification of the potential outcome function. To make notation easier, replace $Y_i \mid D_i = 1$ with $Y_{T,it}$ and $Y_i \mid D_i = 0$ with $Y_{C,it}$. We write:

$$\begin{cases} Y_{T,it} = g_{T,t}(\mathbf{X}) + \mu_{T,i} + \lambda_{T,t} + \eta_{it} \\ Y_{C,it} = g_{C,t}(\mathbf{X}) + \mu_{C,i} + \lambda_{C,t} + \eta_{it} \end{cases}$$

i.e. the potential outcome is a function of the firm's observed characteristics **X**, the firm-specific unobservable μ_i, the time-specific unobservable λ_t, and the random error η_{it}.

estimation draws on the approach of Arraiz et al. (2011). Please refer to Section 5.3 for an in-depth explanation of the difference-in-differences estimator implemented in this study.

Aside from our theoretical approach towards the estimation of the ATE, an important *caveat* in our study concerns the notion of potential outcome that we adopt, and the possible limitations that it may entail. As previously stated, we are only able to observe treated enterprises, incurring in the need to generate an *ad-hoc* control group. This does not *per se* constitute a novelty in the literature, but it does nonetheless have an impact on the interpretation of our final results. In particular, while it is certain that an enterprise in our control group has not benefited from a MAP-guaranteed loan, not much is known with respect to its funding sources. For instance, it could be that companies in the control group have obtained a non-guaranteed loan, or alternatively have benefited from other types of subsidised loans or have received no loan at all.[38] Most likely, the control group will contain firms with different financial structures, which leads us to frame in rather restrictive terms the interpretation of our findings: the measured economic impact will refer to the obtention of a MAP-guaranteed loan versus all other possible financing sources, provided each alternative financial structure before each signature year does not prevent a firm from becoming treated in the following period.

As a result, our estimated impact represents the joint economic effect of the MAP guarantee *and* the associated loan, so that the "pure" MAP guarantee effect cannot be disentangled. However, a case could be made that given the programme requirements, were the MAP guarantee not issued, neither would the associated loan. Hence, the "joint" estimated impact is, in fact, to be entirely ascribed to the MAP facility. Further studies should aim to also assess the financial additionality of the MAP guarantee, in order to tackle the identification of the "pure" MAP guarantee effect from a different standpoint.

On a final note, we observe that in an empirical setting, if the assumption of conditional mean independence fails, the propensity score matching estimates of the ATE will be biased, because they will not fully eliminate the effect of the selection process. However, under the assumption of common trend, the employed difference-in-differences estimator will nevertheless be able to provide an unbiased estimate of the *Average Treatment Effect on the Treated (ATT or ATET),* following the proof in Blundell and Costa Dias (2002).

This concludes the description of our theoretical framework. The following sections will expand on the methodological steps that have been adopted to implement it.

5.2. Selection of the control group

The construction of the control group is based on the strategy adopted by Bertoni and Martí (2011) in the context of venture capital financing. However, our study deviates from their method

[38] Note that we also ignore the details of treated firms' financing sources (apart from the MAP-guaranteed loan). As long as they are not time-varying, such financial factors – which may well affect observed firm performance – will be controlled for by means of our estimation technique.

by introducing a *pre-sampling* step which tries to minimise the loss of information due to matching in the *common support region*.

5.2.1. Pre-selection phase

Given that any active CESEE-based SME in the period 2005-2007 is eligible for a MAP-guaranteed loan, our strategy is to build the control group by randomly sampling firms from Orbis in order to ensure a thorough representation of different firm's characteristics. However, these two selection criteria (*i.e.* active status and country of operations) are insufficient to control for the specific sample composition of our treatment group. This could potentially lead to the need of extracting several tens of thousands of firms so as to be able to reconstruct the original sample composition of the treatment group. Instead, we perform a stratified sampling of the control group mirroring different clusters of treated companies, based on classes of signature years, countries, number of employees and age at loan signature (see Table 2).

Table 2: Control group stratification

Signature Year	*{2005, 2006, 2007}*
Country	*{BG, CZ, EE, HU, LT, LV, PL, RO, SI, SK}*
Employees	*{0-10, 11-50, 51-250}*
Age	*{0-2, 3-5, 6-10, 10+}*

Source: Authors

This methodology allows us to identify 360 potential clusters, of which 265 are actually "populated" by firms in the control group. We thus extract, for each treated company in the specific cluster, an arbitrary number k of control firms. We set $k = 11$ which, net of control companies that do not fulfil the sample selection criterion (see Appendix III), yields an approximate 1:9 treatment-to-control ratio. The final pre-matching sample contains 20,585 companies, of which 10.26% are treated.

5.2.2. A parametric model for the estimation of the propensity score

Earlier in this section, we described how the correct estimation of the impact of MAP guarantees needs to satisfy the hypothesis of independence of the treatment from the outcome, conditional on a set of observable characteristics $\mathbf{X}$. In practice, this means that if we are able to select for each treated firm a control firm sharing equivalent characteristics, then the estimated $\widehat{\text{ATE}}$ would be consistent (a procedure called *covariate matching*). However, when multiple characteristics can have an impact on the probability of being treated, providing an exact control to each treated firm becomes practically impossible (a problem that is often referred to as the *curse of dimensionality*). In a setting like ours, where different continuous financial indicators can potentially be used to match treated and control firms, *covariate matching* does not represent a viable solution.

In their seminal work, Rosenbaum and Rubin (1983) show how the use of the *propensity score* $p(\mathbf{X})$ satisfies the CIA, as stated in Equation (3) in Section 5.2. The intuition behind the estimation of the propensity score is rather straightforward: the PS represents an estimated ex-ante probability of obtaining a MAP-guaranteed loan, which is based on a set of firm's characteristics. Therefore, the matching procedure can be implemented in two separate steps: first, we estimate the propensity score through an appropriate probability model; second, we use the model estimates to perform the matching of treatment and control groups. Rosenbaum and Rubin argue that the use of a logit model is to be preferred, and both Månsson and Shukur (2011) and Caliendo and Kopeinig (2008) provide further arguments supporting the logit specification for the probability model. Finally, Zhao (2008) shows in a Monte Carlo setting that while logit and probit specifications are equivalent under unconfoundedness, the logit model is less sensitive in the case such assumption fails.

An important step in the construction of the parametric PS model relates to the choice of covariates. The guiding principle is that the set of $\mathbf{X}$ characteristics must satisfy the CIA, hence only variables that have a simultaneous impact on both the participation decision and the outcome should be included. We build our model choosing from the pool of available KPIs listed in Appendix I, selecting variables according to their significance and the resulting increase in prediction levels (Heckman et al., 1998). The resulting PS model as a whole is subsequently tested using a variety of metrics (see Section 5.2.2.2).

5.2.2.1. Addressing the potential issue of chronology bias

The PS model we implement in our study is peculiar in two key aspects. First, due to the significant differences across cohorts evidenced in Section 4.1 and Section 4.4, we employ a different propensity score model in each signature year, in order to maximise the predictive ability of the PS. Second, we treat the three cohorts independently, and in particular our PS model is implemented in years $t\text{-}1$ and $t\text{-}2$ (with $t \in \{2005, 2006, 2007\}$). While this strategy may in principle collide with the SUTVA (*e.g.* the treated firms entering the programme in 2007 may have already been altered by the programme – started in 2003), we claim that this effect can be assumed as non-significant, and provide statistical proof of this. In fact, our argument is that by not controlling for the closest pre-treatment status, we might introduce a bias known in the epidemiologic literature as *chronology bias*.[39] The principle of *chronology bias* is that, similar to patients participating in a medical trial for a new drug, the farther a pre-treatment status is observed with respect to the treatment date, the higher is the chance to incur in problems generated by a change in the participation process, in the way treatment is administered,[40] *etc.*

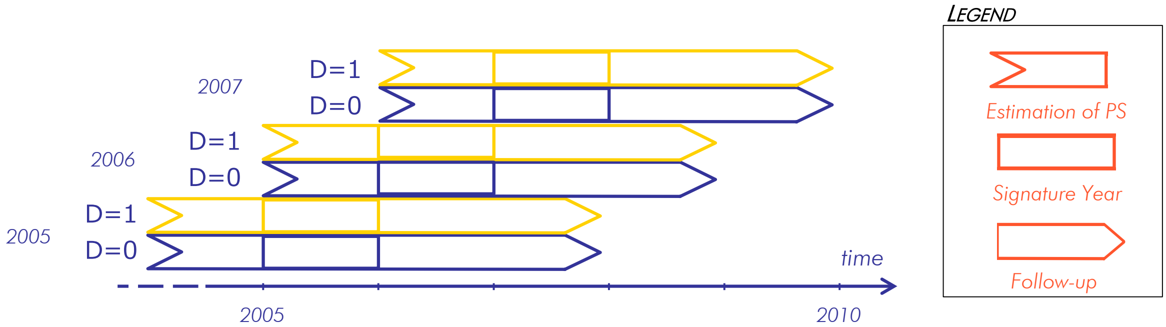
In addition, by performing a PS model on pre-programme data, *i.e.* referring to up to 4 years before the actual obtention of the loan, we would likely incur in an *omitted variable* bias, that is, we would not account for significant information that has a higher likelihood of determining the

[39] Also *channelling bias* (Petri and Urquhart, 1991).

[40] For instance, higher/lower loan intensity attributable to one particular cohort, all other characteristics considered.

participation of the firm, leading to a biased estimation of the treatment effect. Finally, we also note that performing our PS model on pre-programme characteristics, we incur in a significant loss of data, partly explained by the fact that many enterprises did not exist 4 years before their treatment period. We find our approach consistent with the *balanced sequential cohort design* described in Schneeweiss et al. (2011), in which the analysis of subsequent waves of treatment is designed so as to account for the participation period. A graphic interpretation of the balanced sequential cohort design can be found in Figure 8.

Figure 8: Sequential cohort design



Source: Schneeweiss et al. (2011), own elaboration

5.2.2.2. Logit model

This paragraph provides a detailed description of the propensity score model that we estimate. We adopt the principle of Rubin and Thomas (1996) which implies that logit models should be *saturated*[41] in order to i) avoid possible biases in the estimation of the propensity score, and at the same time, ii) avoid incurring in over-fitting. We also follow Caliendo and Kopeinig (2008) in selecting variables "based on economic theory and previous empirical findings". However, it should be noted that our use of the logit model is purely instrumental to the estimation of the propensity score, and as such no space will be given to the interpretation of the estimated coefficients: the main aim of this exercise is to obtain a consistent estimator which features a high discriminatory power between treated and control firms, based on observable characteristics. In other words, we only pursue the best possible estimate $\hat{p}(\mathbf{X})$ of the propensity score which fulfils the CIA. Therefore, we will rather focus on different tests and robustness checks concerning the predictive ability of our PS model.

In order to estimate the propensity score, we follow the baseline specification expressed below:

$$\mathrm{logit}\left(p_{is}(\mathbf{X})\right) = \beta_0 + \alpha C_{is} + \gamma F_{is} + \delta I_{is} \qquad (6)$$

where *i* represents each firm operating in the CESEE region and *s* the signature year. *C* is a set of

[41] See Angrist and Pischke (2008), p.48.

"demographic" characteristics of the firm (country, industry sector,[42] legal form, number of employees, age, *etc.*), F represents variables relating to economic and financial attributes (*e.g.* turnover, assets, shareholder's funds, working capital, *etc.*), and I refers to different potential interactions between all the variables.
The signature year-specific propensity score models are presented in Table 3. We compare the results from the different specifications per signature year with a model pooling all signature years together. Moreover, for all three models we test alternative specifications with a reduced number of variables[43] in order to assess the robustness of the estimated propensity scores and to measure the potential bias introduced by missing values of specific variables.

We also perform a further robustness check by removing outliers from each estimation sample.[44] Not surprisingly, we observe that the predictive ability of all models improves significantly when we exclude the identified outliers. Table 4 summarises the main characteristics of the different specifications of the PS model that have been discussed. We observe that, while the removal of outliers (constituting less than 2.5% of all observations in each signature year) increases significantly the predictive ability of our model, it also causes a drastic reduction in the model's goodness of fit. Conversely, the reduced model scores consistently worse in terms of predictive ability, although it preserves its goodness of fit. Overall, the findings of Table 4 need to be weighed against the *covariate balancing* property of the estimated propensity score, a topic discussed in Section 5.2.3.

The predictive ability of the PS model can also be shown graphically, by comparing the density function of the propensity score of the treated firms vs the control group (Figure 9)). A statistically significant difference in the two distributions[45] confirms that the model is successful in discriminating candidates for a MAP-guaranteed loan.

5.2.3. Propensity score matching

The estimated propensity score $\widehat{p}(\mathbf{X_i})$ can now be used to perform the matching between treatment and control group firms. As anticipated in Section 5, our study builds on the approach of Arraiz et al. (2011) in using propensity score matching (PSM) to remove the selection bias on observable characteristics, and then use a difference-in-differences estimator to quantify the impact of the treatment while eliminating potential biases arising from individual- and/or time-specific effects. In this paragraph we present our PSM strategy, discuss its effectiveness in providing covariate balancing, and describe the final matched sample that will be fed into the difference-in-differences estimator.

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[42] For this, we group company's NACE Rev. 2 main divisions into 6 different macro-categories.

[43] The reduced model produces an increase in the treatment group size of 9.5%.

[44] We define as outlier of the logistic regression any observation with a delta chi-squared influence statistic above 10 (see Hosmer and Lemeshow, 2004).

[45] Assessed with a Kolmogorov-Smirnov test.

Table 3: Propensity Score model

Dependent variable is the obtention of a MAP-guaranteed loan.

		(1)	(2)	(3)	(4)
Variable	Specification type	Signature Year 2005	Signature Year 2006	Signature Year 2007	Pooled Signatures
Number of employees	level	0.0028 (0.00921)	-0.0268** (0.01347)	-0.0054 (0.00542)	-0.0033*** (0.00111)
	logarithm			0.3739*** (0.13803)	
	square	-0.0001 (0.00008)	-0.0002** (0.00008)		
	cube	0.0000 (0.00000)			
Firm's age	level	-0.5094** (0.20452)	0.0858 (0.05373)		0.0051 (0.00786)
	square	0.0126* (0.00696)			
	cube	-0.0003** (0.00011)			
Has patent(s) or trademark(s)	dummy			1.0451*** (0.33185)	
Peer Group Size[46]	level	-0.0019 (0.00249)			-0.0055*** (0.00117)
Turnover	level	-0.3743 (0.27062)	-0.0078 (0.01091)	-0.0184** (0.00737)	0.0019 (0.00172)
	lagged	0.0174 (0.01427)	-0.0188 (0.01179)		
	logarithm	0.9475*** (0.12076)	0.8904*** (0.12433)		
	logarithm of lagged	-0.3656*** (0.09043)			
Total Assets	level			-0.0109 (0.01396)	-0.0106 (0.01044)
	lagged	-0.0422* (0.02162)			
	logarithm		-0.3823*** (0.12407)		
	square				-0.0010*** (0.00019)
	cube				0.0000*** (0.00000)
Tangible Fixed Assets	level	0.2656*** (0.05807)	-0.0091 (0.01777)		0.0450*** (0.01038)
	logarithm		0.4005*** (0.06690)	0.3764*** (0.06725)	
	square	-0.0053*** (0.00140)			
	cube	0.0000*** (0.00000)			
Long term debt	level	-0.1953** (0.08610)	-0.1452* (0.07647)		

[46] BvD Orbis proxy for market size.

Table 3 continued

Variable	Specification type	(1) Signature Year 2005	(2) Signature Year 2006	(3) Signature Year 2007	(4) Pooled Signatures
Current Liabilities	logarithm			-0.3602*** (0.08730)	
Shareholders' Funds	level	-0.1146*** (0.03849)		0.0122 (0.01738)	
Profit/Loss before Tax	level		0.1737 (0.16261)		0.0258** (0.01248)
	square		-0.0492** (0.01922)	-0.0922*** (0.02596)	
	cube		0.0001** (0.00004)	0.0010*** (0.00028)	
	square of lagged	-0.0127 (0.00910)			
Working capital	cube			-0.0001*** (0.00002)	
Solvency Ratio	level	0.0089*** (0.00279)			
Current Ratio	level				-0.0383*** (0.01006)
	square	-0.0060** (0.00263)			
	cube	0.0001** (0.00003)			
Profit Margin	cube			0.0000 (0.00000)	
Cost of Employees	logarithm	-0.2743*** (0.08827)	-0.4563*** (0.09172)		
	square	0.0806*** (0.03006)			
	cube	-0.0080** (0.00331)			
Financial independence	level	0.2627 (0.18740)			
Constant Term	level	-6.7035*** (1.36181)	-2.2368*** (0.83604)	-2.2286*** (0.61298)	-0.8711*** (0.13602)
Obs.		3,736	3,536	3,196	15,835
of which: treated		385	351	224	1,401
Country F-E		Yes	Yes	Yes	Yes
Sector F-E		Yes	No	Yes	Yes
Legal Form F-E		Yes	Yes	No	No
Independence Factor F-E		No	No	Yes	No
Signature Year F-E		No	No	No	Yes
Interactions with age		Yes	Yes	Yes	Yes
Interactions with country		Yes	Yes	Yes	No
Interactions with sectors		Yes	Yes	Yes	No
Additional interactions		Yes (4)	Yes (2)	No	No

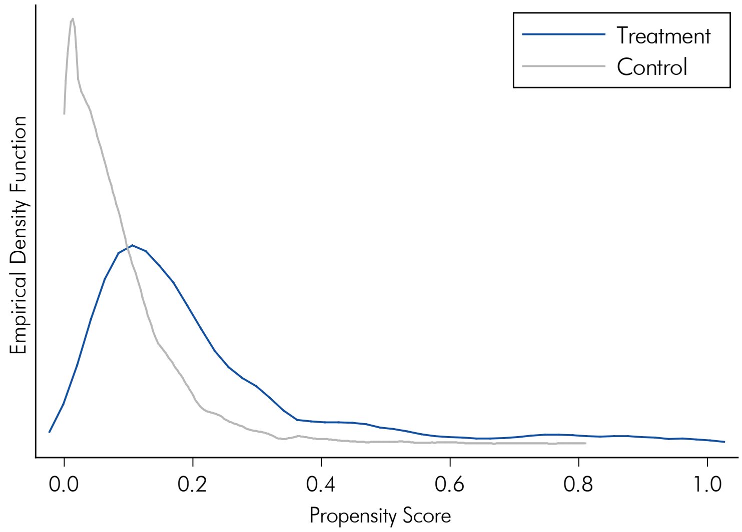
Notes: Financial values expressed in 100,000 EUR. Standard errors are reported in brackets. Cells marked in grey represent variables dropped in the *reduced form* model. P-values scale: * $p<0.10$ ** $p<0.05$ *** $p<0.01$. *Source: Authors*

Table 4: Predictive ability of the propensity score model

	Full Model						Reduced Model			Pooled
	Outliers			No Outliers						
	s=2005	s=2006	s=2007	s=2005	s=2006	s=2007	s=2005	s=2006	s=2007	
Predictive Ability (%)										
Pseudo-R^2	12.56%	20.48%	17.19%	23.90%	33.91%	32.45%	9.67%	15.96%	14.20%	6.36%
Cox-Snell/ML R^2	7.99%	12.41%	8.36%	12.52%	17.30%	12.03%	5.92%	9.63%	6.76%	3.73%
Tjur's D	10.08%	16.76%	10.25%	16.95%	25.95%	18.45%	7.58%	13.33%	8.21%	4.00%
Area under the ROC curve	74.57%	80.98%	80.81%	85.37%	90.24%	91.44%	71.75%	76.81%	78.08%	68.85%
Goodness of Fit (P-values)										
H-L test* (9 groups)	93.74%	85.54%	57.55%	0.00%	0.01%	7.63%	85.00%	22.11%	65.77%	0.00%
H-L test* (11 groups)	94.69%	65.94%	77.75%	0.00%	0.22%	10.09%	86.70%	11.56%	94.53%	0.01%

** Hosmer and Lemeshow (2004). Source: Authors*

Figure 9: A graphical illustration of the PS model's predictive ability



Note: the probability density function of the propensity score was computed using a kernel estimator on pooled signature years. Source: Authors.

Our matching strategy consists of a *nearest-neighbour-matching* (NN-matching) with oversampling and *caliper*, common support condition and replacement.[47] While other studies (see Oh et al., 2009) perform a *kernel matching* in order to minimise the variance of the estimated ATE, our NN-matching technique will instead trade efficiency for a lower estimation bias (see Caliendo and Kopeinig, 2008 and Abadie and Imbens, 2006). With oversampling, we allow k nearest neighbours to match the treatment firm, trading consistency (which can be directly tested)

[47] The matching strategy was implemented using the routine provided in Leuven and Sianesi (2004).

for efficiency (only testable in comparative terms). However, oversampling may facilitate the selection of "bad" matches; we therefore impose two further conditions on the matching technique: the first, *caliper matching*, subjects the selection of the candidate control firm upon the verification that its propensity score lies within a pre-defined range ϕ, centred on the treated firm's PS value; the second, *common support condition*, ensures that matched companies are only selected from the support region in which the propensity scores of treated and controls completely overlap. Finally, the replacement option privileges the reduction in bias (by selecting the closest possible neighbour even if it has been already associated) at the cost of increasing the variance of the estimator (Caliendo and Kopeinig, 2008).

As a result, our PSM procedure involves the definition of two different parameters, k and φ. The choice of k and φ is fundamentally arbitrary, but can be optimised *ex-post* by checking their performance in terms of covariate balancing. Clearly, a higher k will increase bias more than efficiency, while assessing *a priori* which value of the caliper φ is more reasonable is less straightforward (Smith and Todd, 2005). In this respect, we follow the advice of Austin (2011), who suggests linking the choice of the caliper to the standard deviation of the propensity score, and specifically to assign a caliper equal to 20% of the observed standard deviation of the PS.

We thus test various combinations of k and φ, imposing a range rather than specific values for each parameter. In particular, we set $k \in \{3,\cdots,8\}$, $\varphi \in \{0.15, 0.20, 0.25\}$. By comparing the performance of each combination in terms of their covariate balancing ability, we find the optimal setting to have $k=3$ and $\varphi=0.2$. Lower values for k do not enhance significantly the covariate balancing, while fluctuations in φ have virtually no impact. A summary of the covariate balancing checks can be found in Table 5, where for each covariate introduced in model (6) we compute a set of three tests: a two-sample t-test to compare the mean of the treated group against the mean of the control group, a Kolmogorov-Smirnov test for the probability density functions of the two groups, and ψ, the mean difference as a percentage over the standard deviations.

Table 5: Covariate balancing checks

Variable	Reference Period	Mean t-test (P-Value)	K-S test (P-Value)	Ψ (Percentage)
Number of employees	2005	61.5%	5.6%	3.0%
	2006	57.6%	47.6%	3.6%
	2007	70.1%	93.0%	2.9%
	overall	41.0%	12.5%	3.1%
Firm's age	2005	22.0%	75.0%	7.6%
	2006	82.8%	40.7%	1.4%
	2007	30.6%	5.4%	8.0%
	overall	14.5%	3.3%	5.6%
Has patent(s) or trademark(s)*	2005	11.3%	n.a.	9.0%
	2006	84.9%	n.a.	1.2%
	2007	64.2%	n.a.	3.5%
	overall	18.6%	n.a.	4.9%
Peer Group Size	2005	60.8%	58.9%	3.4%
	2006	21.8%	19.8%	-7.3%
	2007	60.3%	96.8%	4.1%
	overall	85.1%	83.6%	-0.7%

Table 5 continued

Variable	Reference Period	Mean t-test (P-Value)	K-S test (P-Value)	Ψ (Percentage)
Operating revenue (Turnover)	2005	46.3%	52.0%	-4.1%
	2006	68.8%	53.8%	-2.5%
	2007	69.5%	31.1%	3.0%
	overall	53.8%	61.0%	-2.2%
Lagged Operating revenue (Turnover)	2005	50.1%	82.4%	-3.8%
	2006	58.5%	41.7%	-3.3%
	2007	36.8%	66.1%	8.3%
	overall	71.9%	77.3%	-1.4%
Total assets	2005	47.0%	89.7%	-4.1%
	2006	92.3%	33.9%	0.6%
	2007	50.5%	64.1%	5.7%
	overall	97.3%	73.1%	-0.1%
Tangible fixed assets	2005	48.6%	16.9%	-4.1%
	2006	79.1%	82.1%	-1.6%
	2007	81.1%	85.9%	2.1%
	overall	75.0%	70.7%	-1.2%
Long term debt	2005	40.1%	100.0%	-4.6%
	2006	88.7%	98.6%	-0.9%
	2007	99.5%	5.2%	-0.1%
	overall	90.2%	14.9%	-0.5%
Current liabilities	2005	47.0%	43.6%	-4.1%
	2006	84.9%	48.8%	1.2%
	2007	53.5%	23.1%	4.9%
	overall	88.0%	22.4%	-0.6%
Shareholders' funds	2005	51.9%	17.1%	-3.8%
	2006	87.8%	52.1%	-1.0%
	2007	50.7%	12.0%	5.5%
	overall	97.1%	1.2%	-0.1%
P/L before tax	2005	74.4%	33.1%	-1.9%
	2006	92.4%	57.8%	0.6%
	2007	74.4%	74.6%	-2.6%
	overall	73.0%	11.6%	-1.3%
Working capital	2005	68.0%	89.9%	-2.4%
	2006	39.1%	22.7%	5.6%
	2007	60.4%	23.2%	4.1%
	overall	74.6%	14.4%	1.2%
Solvency ratio (Asset based) %	2005	60.0%	2.7%	-3.1%
	2006	68.3%	9.9%	-2.5%
	2007	83.0%	6.9%	1.6%
	overall	64.1%	0.1%	-1.7%
Current ratio	2005	66.6%	25.7%	2.8%
	2006	70.0%	45.6%	-2.4%
	2007	78.4%	42.3%	2.1%
	overall	82.5%	9.1%	0.9%

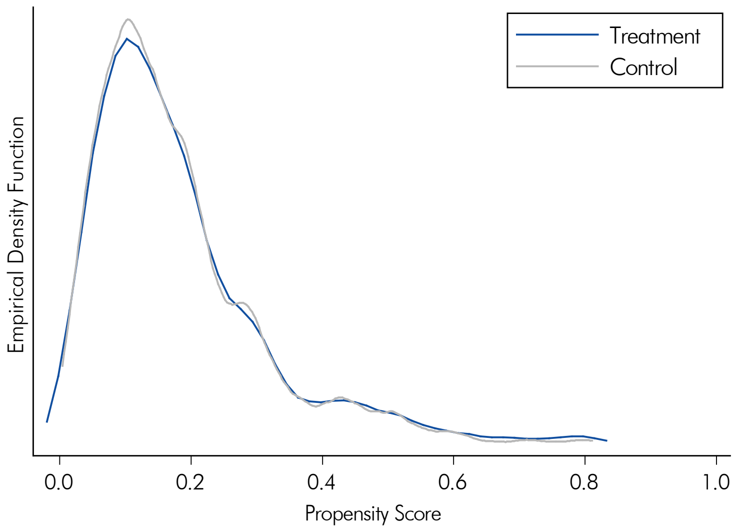
Table 5 continued

Variable	Reference Period	Mean t-test (P-Value)	K-S test (P-Value)	Ψ (Percentage)
Profit margin %	2005	89.5%	24.1%	-0.8%
	2006	95.0%	6.3%	-0.4%
	2007	74.7%	87.1%	-2.5%
	overall	77.0%	6.6%	-1.1%
Costs of employees	2005	74.9%	29.7%	2.0%
	2006	43.0%	80.5%	5.2%
	2007	81.7%	68.2%	-2.0%
	overall	74.0%	16.5%	1.3%

** Test based on $\chi^2(1)$. Source: Authors*

Table 5 above shows that the PSM is successful in removing treatment selection bias at the 95% confidence level. Moreover, a complementary strategy to assess the performance of the PSM procedure is the comparison of the propensity score's empirical density function between the treatment and control group, portrayed in Figure 10.

Figure 10: Empirical density functions of the propensity score after the NN-Matching



Note: the probability density function of the propensity score was computed using a kernel estimator on pooled signature years. Source: Authors.

The latter can be compared to Figure 9, to show that a significant improvement was achieved in the balancing of the propensity score. In particular, the mean test on the propensity score of the treated and control groups does not reject the null hypothesis that both samples are equal at 95% significance level.

5.3. *Difference-in-Differences* framework

To estimate the average treatment effect of a MAP guarantee in the CESEE region, we implement a difference-in-differences (DID) estimation using the following OLS regression:

$$y_{it} = \beta_0 + \delta T_i + \gamma I_t + \theta(T_i \times I_t) + \varepsilon_{it} \tag{7}$$

where T_i is the treatment, I_t is a set of time-specific dummies that indicate the time period (*i.e.* 1 year after the obtention of the guarantee, 2 years.., etc.), $T_i \times I_t$ is their interaction and θ is the average treatment effect that we intend to test. We use different outcome variables y_{it} in order to have a multi-faceted understanding of the MAP effect in the CESEE region. Note that the baseline time period of this model is the year *before* the guaranteed loan was issued (*i.e.* *t-1*).

To estimate the coefficient θ and its standard error, we first use a standard OLS regression under a repeated cross-section framework, as it is customary in the difference-in-differences literature (see Imbens and Wooldridge, 2008 for a comprehensive review of different methods). However, this first method may yield biased estimates for the standard errors in the presence of multiple time periods, as observations concerning single SMEs are likely to be serially correlated. Bertrand et al. (2004) discuss the potential biases in the estimated standard errors occurring when overlooking the auto-correlation structure of the residuals, and suggest using alternative variance estimators of (7) in order to provide further robustness to the OLS results. We follow their approach and, in particular, we compare the initial OLS results with two alternative model specifications (which include firm-specific and country-specific controls respectively) and four alternative estimation methods: firm-level fixed-effects, cluster-robust, block bootstrap,[48] before/after aggregation. Results of these robustness checks can be found in Appendix VII.

Interestingly, we find that for the variables expressing the impact of the MAP facility, our robustness checks yield lower standard errors compared to those computed in the original framework. Although this finding is not confirmed for most other controls in our model (for which our robustness checks indeed show a lower rejection rate of the estimated coefficients), we nevertheless choose to adopt a more conservative approach, by selecting as a benchmark our original OLS estimation which, together with the before/after aggregation is proven to under-reject, while also reporting existing differences with our robustness checks.

With respect to the alternative model specifications, we test the robustness of the estimates in (7) by adding several firm-level and country-level controls. Firm-level data consists of signature year, sector, and country fixed effects, as well as firm's age at loan issuance. Country-level effects are meant to identify macro-financial effects acting at the country level (*e.g.* unemployment rate, public debt), while country-sector effects control for sector-specific differences within each specific country (*e.g.* sectoral gross value added). Country and country-sector controls were extracted from Eurostat. We find that both firm-level and country-level controls increase the predictive ability of our models, thus reducing the potential bias in the estimation of the ATE. Therefore, our final estimates also include such fixed-effects, augmenting the model in (7) as follows:

$$y_{it} = \beta_0 + \delta T_i + \gamma I_t + \theta(T_i \times I_t) + \tau F_i + \eta C_{jt} + \lambda S_{jkt} + \varepsilon_{it} \quad (8)$$

where F_i represents firm i's fixed effects, C_{jt} identifies time-varying characteristics of country j, and S_{jkt} contains time-varying country j and sector k effects. For easiness of the reader, coefficients for

[48] Regarding the block-bootstrap estimation, we note Abadie and Imbens (2008)'s proof of the inefficiency of bootstrapping methods with nearest-neighbour matching, and correct such potential inefficiency by using bootstrap samples with sizes that are smaller than the reference sample sizes. Moreover, the block-bootstrap standard errors are estimated using 500 replications.

these variables are not portrayed among the estimated results of Section 6.

Concluding, we perform a number of tests to validate our key model's assumption thus far. In particular, we implement *a)* an indirect test of the conditional independence assumption based on Chiappori and Salanie (2000), *b)* a test of the non-interference assumption by checking for potential *spillover effects* across cohorts, and *c)* a test of the *common trend assumption,* which constitutes an important requirement for the use of our difference-in-differences approach. The reader is referred to Appendix VI for an overview of these robustness checks.

6 Results

This paper follows the existing literature on the estimation of the economic additionality of CGSs by exploring the different dimensions on which the potential impact of CGSs can be observed. Conceptually, we identify five classes of effects through which the economic additionality of the MAP could be channelled: *size, sales, profit and profitability, productivity,* and *financial resilience*. For each category, we select one or more dependent variables able to identify the causal effect of a MAP-guaranteed loan. For most key financial indicators, we use the logarithm of dependent variables in order to identify the impact in terms of percentage changes, rather than levels. A summary of the dependent variables used in this study can be found in Table 6.

Table 6: Classes of effects and corresponding dependent variables

Effect class	Dependent Variable(s) used
Size	*Number of Employees, Total Assets*
Sales	*Turnover*
Profit	*Profit/Loss before taxes (pbt)*
Productivity	*Estimated TFP, ROA (pbt based)*
Financial Resilience	*Current Ratio, Solvency Ratio*

Source: Authors

6.1. Overall Sample Estimation

A first analysis of the impact of MAP guarantees in the CESEE region is conducted at the aggregate level, using the final matched sample described in Section 5.2.3. For each of the different categories of effects, we select one representative variable in order to illustrate the underlying category effect. We use as a reference the model estimated via OLS, which includes firm-level and country-level controls. Alternative model specifications with different dependent variables, different covariates or further robustness checks are presented in Appendix VI.

Table 7 summarises the results of the estimation on the overall sample. We observe a positive significant *size impact* of MAP-guaranteed loans on CESEE beneficiaries: compared to the control group, benefiting firms were able to increase their number of employees by 17.3% within the first 5 years after the signature date. The effect gains momentum over the course of the analysed period, reaching a peak in the third year after the obtention of the loan.

A similar effect can be observed on other variables related to companies' size, such as total assets and, in particular, fixed assets. However, in this case the uncertainty embedded in the control sample extraction makes it difficult to disentangle the "pure" effect of the MAP guarantee from a more obvious "balance sheet" effect (*i.e.* assets increase simply because a MAP-guaranteed loan preludes to an investment, hence to an increase in assets).

With respect to the impact on sales, we observe a positive and significant[49] effect on turnover, which indicates that MAP beneficiaries have outperformed control group companies by 19.6% in the fifth year after the signature date. Together, the size and sales effects indicate that the MAP facility successfully acted as a countercyclical tool during the financial crisis.

As for profit and profitability, we observe no significant impact brought by the MAP facility on the profits of beneficiaries. This finding, which is quite consistent across alternative specifications and different subsets of companies, is perhaps more related to the human capital and entrepreneurial ability of companies, in the sense that MAP beneficiaries, while on average gaining an advantage in terms of size with respect to their peers, are not in fact able to transform this benefit into an economic gain. However, there could be an alternative, and possibly more plausible explanation for the insignificant effect on profit: firms may decide to trade off their profit advantage with a size advantage, by saving or creating more jobs while foregoing short- to medium-term profits. While it is beyond the scope of this study to assess which of the two conjectures is correct, an analysis of enterprises' costs seems to point to this latter theory, as expenses related to employees appear to increase significantly in the first three years after loan obtention, while the expenditure in materials – a proxy for variable costs – does not follow a similar pattern.

With respect to the impact of the MAP facility on companies' productivity (log-)levels, we notice that the MAP-guaranteed loan has an immediate negative impact on firms' productivity, consisting in a reduction in TFP of 9%-11% in the first three years, compared to the control group. However, the negative impact is then partially absorbed: the years following the obtention of the loan show a less significant (sometimes non-significant) impact, and the magnitude of the effect is reduced. While the observed negative impact on productivity does not constitute a novel finding in the assessment of CGSs (Oh et al., 2009), we should refrain from assigning an unequivocal interpretation to such result: aside from limitations in the estimation of the TFP itself,[50] MAP beneficiaries face a temporary gap in productivity with respect to their peers, that could be due to allocative inefficiencies of beneficiaries after the MAP-induced increase in their production factors or to an "adaptation period" following the obtention of the loan. Such gap is partially absorbed over the medium run.

[49] Significant at 10% and also at 5% level for some periods for the reference model, at 5% and at 1% level for some periods estimators that account for the potential bias in the standard errors. See Appendix VI for additional robustness checks.

[50] See Appendix V.

Table 7: Difference-in-differences estimates. Overall Sample

	(1)	(2)	(3)	(4)	(5)
	Nr. of Employees	Turnover	P/L before tax	Estimated TFP	Current ratio
T_i	-0.0014	0.0598	0.0470	0.0378	0.0001
	(0.041)	(0.058)	(0.075)	(0.028)	(0.002)
I_0	0.0360	0.0886*	0.1830***	0.0095	0.0041**
	(0.034)	(0.048)	(0.063)	(0.026)	(0.002)
I_1	0.0260	0.1362**	0.3774***	-0.0013	0.0063***
	(0.039)	(0.054)	(0.073)	(0.033)	(0.002)
I_2	0.0118	0.0679	0.4386***	-0.0173	0.0095***
	(0.046)	(0.063)	(0.086)	(0.041)	(0.002)
I_3	-0.0601	-0.0054	0.3943***	-0.0471	0.0126***
	(0.055)	(0.074)	(0.103)	(0.048)	(0.003)
I_4	-0.1016*	-0.1560**	0.3481***	-0.1495***	0.0145***
	(0.056)	(0.076)	(0.106)	(0.048)	(0.003)
I_5	-0.1742***	-0.2366***	0.1881*	-0.1730***	0.0169***
	(0.054)	(0.075)	(0.104)	(0.048)	(0.003)
$T_i \times I_0$ (=ATE_{t_0})	0.0505	0.0863	0.0463	-0.1179***	-0.0046*
	(0.059)	(0.082)	(0.107)	(0.040)	(0.003)
$T_i \times I_1$ (=ATE_{t_1})	0.1372**	0.1446*	0.0299	-0.0532	-0.0054*
	(0.060)	(0.083)	(0.109)	(0.042)	(0.003)
$T_i \times I_2$ (=ATE_{t_2})	0.1414**	0.1809**	0.0869	-0.0703*	-0.0077***
	(0.059)	(0.082)	(0.109)	(0.042)	(0.003)
$T_i \times I_3$ (=ATE_{t_3})	0.1875***	0.1522*	0.0282	-0.0894**	-0.0090***
	(0.060)	(0.082)	(0.111)	(0.042)	(0.003)
$T_i \times I_4$ (=ATE_{t_4})	0.1812***	0.1766**	-0.0448	-0.0652	-0.0075***
	(0.060)	(0.083)	(0.113)	(0.043)	(0.003)
$T_i \times I_5$ (=ATE_{t_5})	0.1729***	0.1967**	0.1337	-0.0833*	-0.0080***
	(0.061)	(0.083)	(0.115)	(0.043)	(0.003)
Observations	18,984	19,914	16,192	8,965	19,498
Number of Firms	2,923	2,923	2,899	1,604	2,923
R^2	0.148	0.193	0.060	0.161	0.027

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See Equation (8) for an interpretation of the estimated coefficients. Source: Authors

Furthermore, we observe a negative and significant impact in the medium term on the current ratio, which in comparison to control firms indicates that MAP beneficiaries have, on average, increased their level of current liabilities with respect to their current assets. Although in principle this effect could be indirectly linked to an increased access to finance for MAP beneficiaries, it is also consistent, and perhaps more in agreement, with the alternative theory that MAP beneficiaries increase their current liabilities simply because, as opposed to the majority of their controls, they now have to repay a loan, *i.e.* the MAP-guaranteed loan. A similar conclusion can be reached by looking at the estimated effects on the solvency ratio.[51]

Overall, we note that the MAP facility has generated a significant positive impact on beneficiaries' size and sales, without benefiting nor jeopardising their profits. The observed behaviour of MAP beneficiaries reveals that guaranteed loans are followed by an increase in total assets (a signal that companies are honouring their pledges to invest), as well as in employee costs (because of increasing hiring). Effects on productivity, efficiency and financial sustainability, which appear to be significantly negative in the short term, only fading out in the longer term, suggest that MAP beneficiaries face allocative inefficiencies after the MAP-induced increase in production factors, or experience an "adaptation period" following the obtention of the loan. Although far from confirmatory, we notice that in ***t-1*** the percentage of treated firms with zero reported loan amounts in their balance sheets is approximately 70%, leading to the conjecture that the MAP-guaranteed loan has reached companies not used to obtaining loan financing (which is obviously a political goal of the programme), which thus face increased difficulties in the subsequent financial management of the loan.

As mentioned in Section 5.1, a potential weakness in our approach lies in the theoretical assumption that heterogeneity of treatment intensity is rather limited within our aggregate sample. Moreover, the matching strategy we adopted implicitly assumes that control firms represent appropriate counterfactuals for the analysis we perform, independently of their geographical position, size, age, *etc*. While the methodological rigor should guarantee a sufficient level of confidence with respect to this second argument, both aspects call for a deeper investigation of the robustness of our overall results. We perform such robustness checks by means of an analysis of the impact across different dimensions, provided that the underlying characteristics are unequivocally identifiable for both the treatment and control groups.

In order to preserve the comparability of these robustness checks, for each analysed dimension we employ the same strategy for the estimation of the propensity score and the matching of firms, In all, the results obtained within each dimension support the validity of the overall results, while presenting some additional interesting insights. An overview of the estimations within different dimensions is presented in the sections below.

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[51] Defined as the ratio between shareholder's funds and total assets. See Appendix I.

6.2. Estimation by country

In our second analysis, we divide MAP beneficiaries and control firms by country, and perform the propensity score matching within each country. In order to assess the validity of our overall approach, we maintain the same criteria with respect to both the propensity score model and the matching strategy. Therefore, the propensity scores will now be calculated for each country and signature year, and candidate controls will be matched only within the same country. Unfortunately, this strategy is only feasible for countries with a sufficiently high number of treated companies. Therefore, we are forced to exclude a few countries, in which the final sample size does not allow for the estimation of causal effects. In particular, we concentrate our analysis on 6 countries (BG, CZ, EE, PL, RO, and SI) that exhibit a sample size sufficient to perform the estimation.

We find that the effect on the number of employees reported in Table 7 is mostly due to a positive and significant effect at 95% confidence level in Romania.[52] A weaker positive effect can be also detected in the medium run in the Czech Republic.[53] The results of this analysis are reported in Table 8, where the coefficients of the difference-in-differences estimates are computed for each period.

With respect to sales, our reference model shows positive coefficients for all countries, but we cannot observe a significant effect that is simultaneously verified by all robustness checks. In particular, we observe that the fixed effects, cluster robust and block-bootstrap models identify positive and significant effects in the Czech Republic, Romania and Slovenia, while the pre-post aggregation and the reference model show no significant results. Concerning profits, the estimation within countries follows the trend observed in the overall sample, for which no tangible impact could be observed on profit levels. As for productivity, we observe that the general trend of the overall sample is reproduced solely in Romania, while in other countries no significant reduction in productivity can be detected. Finally, we find that the drivers of the negative impact in the current ratio can be mostly identified within Estonia, Poland and, to a lesser extent, the Czech Republic.

The conclusion of this second exercise is thus twofold: on one hand, the analysis within each country supports the general trend observed in the overall sample, while also highlighting that some of the causal effects of the MAP facility may have played out only in a specific subset of countries. On the other hand, our findings suggest that, at country level, the composition of the treatment group is such that there is a great observed heterogeneity of outcomes, which ultimately affects the significance of the estimated impacts. For this reason, and the others described in Section 6.1, we deepen our analysis further by estimating the impact over three additional criteria: the signature year, the size, and the age of MAP beneficiaries.

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[52] Significant at 99% confidence level in the alternative specifications.

[53] In alternative specifications of our model, the effect on Czech beneficiaries is significant at the 95% confidence level. Estimates of robustness models are not included in this publication, but are available upon request to the authors.

Table 8: Effects on number of employees by country

Country	ATE_{t0}	ATE_{t1}	ATE_{t2}	ATE_{t3}	ATE_{t4}	ATE_{t5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
BG	0.1768 (0.2569)	0.193 (0.2561)	0.1278 (0.2578)	0.1448 (0.2576)	0.1591 (0.2578)	0.0387 (0.2584)	1,143	0.0605	169	0.4780
CZ	0.036 (0.1254)	0.0692 (0.1268)	0.0451 (0.1302)	0.1481 (0.1284)	0.1778 (0.1275)	0.2215* (0.1291)	3,914	0.227	605	0.2426
EE	0.0433 (0.2374)	0.0613 (0.2389)	0.1985 (0.2417)	0.242 (0.2413)	0.2066 (0.2457)	0.1542 (0.2403)	893	0.2091	138	0.3940
PL	0.0627 (0.1642)	0.1218 (0.1633)	0.0297 (0.1567)	0.2518 (0.185)	0.2608 (0.2042)	0.3343 (0.2469)	1,531	0.2109	313	0.2952
RO	0.0627 (0.0886)	0.1551* (0.0899)	0.1775** (0.0884)	0.2024** (0.0884)	0.2009** (0.0893)	0.1858** (0.0898)	8,279	0.1502	1,226	0.7256
SI	-0.0078 (0.272)	0.0545 (0.272)	0.1629 (0.2725)	0.1528 (0.2722)	0.19 (0.2722)	0.2897 (0.2763)	773	0.1517	112	0.3967

*Note: the table only reports those countries for which the sample size allowed performing a propensity score matching. Standard Errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

A number of robustness checks have been used to test for the bias in OLS standard errors in a DID framework. Alternative specifications include: Fixed-Effects regression, Cluster Robust Estimation, Block-Bootstrap estimator, and pre-post aggregation. Each presented model was estimated including firm-level and sector-level controls.
Source: Author

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6.3. Estimations by signature year, size and age classes

We now focus on three potential channels through which the observed heterogeneity of MAP guarantees' causal effect may work. Our starting point is the original propensity score model and matching criteria described in Section 5.2, estimated by signature year and further aggregated into the overall dataset. Instead, in this section we consider the estimated effects in each signature year, in order to highlight potential differences in the channelling of the causal impact. The estimated effects by signature year are reported in Table 9 and Table 10.

The analysis within each signature year, perhaps more than the analysis within each country, is able to identify patterns relating to the various causal effects observed in the overall sample. In particular, we note that MAP beneficiaries from signature years 2005 and 2006 have benefited significantly from the programme in terms of employment growth, while for those companies receiving a loan in 2007 the MAP does not seem to have brought any significant effect. For what concerns the growth in sales, only the 2006 cohort seems to have benefitted from the program, albeit only at 90% confidence level.[54] Once again, no tangible effect is observable in relation to the profitability of MAP CESEE beneficiaries, while productivity levels decrease for those cohorts facing a steeper increase in employment size, providing further support to the theory of temporary allocative inefficiencies following a MAP-guaranteed loan.

We conclude our analysis by identifying the causal effects of the MAP facility within specific types of clusters, which can be correctly identified for both the treatment and control groups. We first focus on the size of firms by classifying companies as Micro, Small and Medium, employing the European Commission definition[55] and taking as reference data the information at signature date. In a further exercise, we categorise companies based on their age at signature date, creating three different clusters: less than 5 years at signature date (*young companies),* between 5 and 10 years (*middle-aged companies*), and above 10 years (*mature companies*). Similarly to the approach followed in Section 6.2, we estimate the propensity score and apply the matching within each of the two classification criteria, and perform the final difference-in-differences regression within each different group. The results of this analysis are presented in Table 11 to Table 14.

We observe that, while the matching performance of companies within the "Micro" and "Small" enterprise clusters is high, the mean test on the propensity score of treated versus controls in the Medium-sized group rejects the null hypothesis that the two samples are similar. This result can be inputted to the use of the caliper in the matching process, which rejects a significant share of matches in this particular cluster. We notice that by reducing the number of nearest-neighbour matches to two, the test on propensity score averages does not reject the null hypothesis, while the difference-in-differences estimates for the Medium size group do not vary significantly. Therefore, in order to preserve the comparability of the results, we maintain the results obtained using the matching specification of the overall sample.

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[54] Fixed-Effect, Cluster Robust and Block-Bootstrap models estimate a significant positive effect at 99% level for cohort 2005 and 2006 (albeit for the former the significance level drops to 95% after the first two years).

[55] See note 21.

Table 9: Effects on number of employees by signature year

Signature Year	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
2005	0.057	0.1565	0.1886**	0.2247**	0.1888**	0.2002**	8132	0.162	1170	0.5020
	(0.0937)	(0.0956)	(0.094)	(0.0949)	(0.095)	(0.096)				
2006	0.0753	0.1732*	0.1628*	0.1957**	0.2324**	0.1865*	6654	0.1709	997	0.0488
	(0.0947)	(0.0948)	(0.0969)	(0.0945)	(0.0959)	(0.0967)				
2007	0.024	0.058	0.0252	0.1158	0.1177	0.1246	4198	0.2046	734	0.1924
	(0.1147)	(0.1152)	(0.1123)	(0.1197)	(0.1219)	(0.126)				

Table 10: Effects on turnover by signature year

Signature Year	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
2005	0.0857	0.1406	0.1655	0.1402	0.1546	0.1648	8308	0.2392	1170	0.5020
	(0.1304)	(0.1325)	(0.1301)	(0.1302)	(0.1312)	(0.1321)				
2006	0.0705	0.1672	0.2331*	0.1859	0.257*	0.2669*	6872	0.1754	997	0.0488
	(0.1371)	(0.1368)	(0.1369)	(0.1366)	(0.1376)	(0.1384)				
2007	0.1262	0.1414	0.1424	0.1341	0.1049	0.1605	4734	0.244	734	0.1924
	(0.1541)	(0.1539)	(0.1538)	(0.1539)	(0.1551)	(0.1588)				

Standard Errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A number of robustness checks have been used to test for the bias in OLS standard errors in a DID framework. Alternative specifications include: Fixed-Effects regression, Cluster Robust Estimation, Block-Bootstrap estimator, and pre-post aggregation. Each model was estimated including firm-level and country-level controls. Source: Authors

Table 11: Effects on number of employees by company size class

SME type	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
Micro	0.1146* (0.0682)	0.1921*** (0.0697)	0.2213*** (0.0695)	0.1785** (0.0694)	0.2058*** (0.0703)	0.1799** (0.0714)	5,570	0.0697	877	0.7207
Small	-0.0052 (0.0572)	0.0983* (0.0577)	0.1103* (0.0574)	0.1553*** (0.058)	0.1806*** (0.0587)	0.1519** (0.0594)	10,527	0.0591	1625	0.8420
Medium	0.2194* (0.1251)	-0.054 (0.1234)	0.0182 (0.1226)	0.1296 (0.123)	0.1087 (0.1232)	0.2072* (0.1249)	2,587	0.0974	399	0.0099

Table 12: Effects on turnover by company size class

SME type	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
Micro	0.2013 (0.1298)	0.2578** (0.1313)	0.3392*** (0.1305)	0.3122** (0.1301)	0.2315* (0.1314)	0.2475* (0.133)	5,823	0.2117	877	0.7207
Small	0.0513 (0.0886)	0.0786 (0.0893)	0.119 (0.0888)	0.1201 (0.0888)	0.1782** (0.0895)	0.1599* (0.0906)	10,887	0.1913	1,625	0.8420
Medium	0.1987 (0.1774)	0.0000 (0.1742)	-0.001 (0.1716)	0.1233 (0.1721)	0.1647 (0.1725)	0.2381 (0.1733)	2,649	0.1461	399	0.0099

Standard Errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A number of robustness checks have been used to test for the bias in OLS standard errors in a DID framework. Alternative specifications include: Fixed-Effects regression, Cluster Robust Estimation, Block-Bootstrap estimator, and pre-post aggregation. Each presented model was estimated including firm-level and country-level controls.
Source: Authors

Table 13: Effects on number of employees by age group

Firm's Age	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
from 0 to 5	0.1059	0.2638*	0.229*	0.3068**	0.3664***	0.3392**	3839	0.2083	597	.3722
	(0.1367)	(0.1373)	(0.1377)	(0.1379)	(0.14)	(0.1407)				
from 5 to 10	0.0441	0.0862	0.1335	0.2205**	0.2867***	0.2677**	6261	0.1561	988	.5180
	(0.1011)	(0.1019)	(0.1019)	(0.1026)	(0.1039)	(0.1055)				
10 or above	0.041	0.1379*	0.1249	0.1993**	0.2007**	0.1952**	9048	0.1327	1387	.4153
	(0.0802)	(0.0811)	(0.0803)	(0.0809)	(0.0815)	(0.0826)				

Table 14: Estimated causal effects on turnover within different age groups

Firm's Age	ATE_{t_0}	ATE_{t_1}	ATE_{t_2}	ATE_{t_3}	ATE_{t_4}	ATE_{t_5}	Obs.	R^2	Nr. Of Firms	P-Value of P-Score Mean Test
from 0 to 5	0.2794	0.2868	0.3353*	0.3622*	0.2285	0.2662	3982	0.2899	597	.3722
	(0.1904)	(0.1907)	(0.1896)	(0.1895)	(0.1919)	(0.1934)				
from 5 to 10	0.0754	0.0871	0.1279	0.1464	0.1371	0.2403*	6602	0.1551	988	.5180
	(0.1347)	(0.1352)	(0.1348)	(0.1344)	(0.1354)	(0.137)				
10 or above	0.0792	0.1485	0.1796	0.1793	0.1933*	0.2133*	9254	0.155	1387	.4153
	(0.1115)	(0.1122)	(0.1111)	(0.1112)	(0.1119)	(0.1131)				

Standard Errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A number of robustness checks have been used to test for the bias in OLS standard errors in a DID framework. Alternative specifications include: Fixed-Effects regression, Cluster Robust Estimation, Block-Bootstrap estimator, and pre-post aggregation. Each presented model was estimated including firm-level and country-level controls.

Source: Authors

Overall, we find that the companies benefiting the most from the *size effect* of the MAP facility belong to the category of Micro and Small enterprises, and are typically young companies. This interesting finding is supported by the significant increase in the number of employees for micro and small enterprises, compared to their peers. Moreover, micro enterprises are among the three size classes featuring the largest net increase in turnover: the estimated impact is significant at the 99% level for our benchmark model in the second and third year after signature date, and amounts to 30% additional sales, compared to the control group, within the first 5 years after the obtention of the loan.

Likewise, young companies are found to experience a boost both in employment and turnover in the years following the obtention of the MAP-guaranteed loan. These findings are significant at the 90% level for our reference model and at 95% in the estimation on pre-post averages.

Micro companies are also found to face a more significant drop in their productivity levels, which is then recovered over the medium term. Interestingly, such effect is dampened in the case of younger companies, for which no tangible drop in productivity is observed in the first two years after the signature date, while the reduction in TFP becomes more apparent after the third year.

Concluding, we note that results by signature year, age and size classes are consistent with the estimates obtained in the aggregate sample, while also identifying typologies of MAP beneficiaries who might have benefitted more from the scheme with respect to other treated firms. In particular, the *size* effect of the MAP program is most noticeable for micro and small enterprises as opposed to medium-sized enterprises, and for young firms as opposed to middle-aged and mature firms. Moreover, there is evidence of a counter-cyclical effect of the MAP programme with respect to signature years 2005 and 2006. The 2007 cohort, which also shows on average lower loan amounts, has instead benefitted less, though not negatively, from such potential counter-cyclical impact.

7 Conclusions and further research

Our study aims to assess the impact of the MAP credit guarantee facility in the CESEE region. Our results show that, on average, MAP CESEE beneficiaries have recorded a significant increase in employment between 14% and 18%, compared to their counterfactuals. A similar result, albeit slightly less significant is related to a rise in the turnover, up to 19% within the first five years after the signature date.

As this positive and significant effect on the size of companies is not followed by an increase in firms' profits, we suspect that companies may choose to substitute their potential profit advantage with a further advantage in size, by preserving or increasing their employment levels. Moreover, we observe that the MAP SMEG causes an immediate drop in productivity, partially reabsorbed over the medium term, which could be explained by allocative inefficiencies linked to factor changes, or to the fact that, since the majority of companies has not benefited from a loan in the two years preceding the MAP-guaranteed transactions, they may face an adjustment period.

Given the significant heterogeneity of the loan intensity across several dimensions, the overall sample estimation needs to be assessed against various cluster decompositions. This study finds heterogeneous patterns in the channelling of the various effects, and in particular it explores differences at country, cohort, size and age level. In this respect, a conclusion that this paper shares with many studies on CGSs is the importance of a careful design and correct implementation to maximise their programmes' effectiveness. Future research might explore to what extent the results obtained on the MAP impact in CESEE countries can be generalised beyond the geographical region involved.

7.1. Further research

As expressed in Section 5, the main theoretical limit of our empirical strategy lies in the fact that our model estimates the joint economic effect of MAP guarantees and of the underlying loans, without the possibility to disentangle the two effects. In this context, further efforts should focus on assessing the financial additionality of the MAP, by analysing the financial characteristics of each transaction (*e.g.* interest rates, size of loans and collaterals, *etc.*) and comparing these characteristics with an appropriate control group. A study of this sort would improve our understanding of the extent to which the effect of MAP loans is to be attributed to the associated guarantee, and ultimately would allow us to separate these two effects.

Moreover, further studies could focus on the impact of MAP guarantees after, say, 6 to 10 years, thereby eliminating the possible impact on current liabilities brought about directly by the MAP-guaranteed loan. Finally, future work could explore the impact of such schemes on the survival pattern of companies, which typically needs longer time spans, beyond the five-year window used in this study.

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Appendix I: Selected financial indicators extracted from the Orbis Database

Source	Label	Definition	Short name
Balance Sheet	Current liabilities	Current liabilities of the company (Loans + Creditors + Other current liabilities)	*currentliab*
	Current ratio	Current assets / Current liabilities	*curratio*
	Fixed assets	Total amount (after depreciation) of non-current assets (Intangible assets + Tangible assets + Other fixed assets).	*fixass*
	Loans	Short term financial debts (e.g. to credit institutions + part of Long term financial debts payable within the year, bonds, etc.)	*loans*
	Long term debt	Long term financial debts (e.g. to credit institutions (loans and credits), bonds)	*longtermdebt*
	Material costs	Detail of the purchases of goods (raw materials + finished goods). No services.	*material*
	Shareholders' funds	Total equity (Capital + Other shareholders' funds)	*sh_funds*
	Tangible fixed assets	All tangible assets such as buildings, machinery, etc.	*tanfixass*
	Total assets	Total assets (Fixed assets + Current assets)	*totass*
	Working capital	Indicates how much capital is used by day to day activities = Stocks + Debtors-Creditors	*wcapital*
Business characteristics	BvD Independence Indicator	Bureau Van Dijk's Independence indicator, which differentiates companies according to their ownership structure	*bvd_indip*
	Listed/Delisted/Unlisted	Whether the company is listed, unlisted or has been delisted	*listed*
	NACE Rev. 2 division code	NACE Rev. 2 main division code	*nace_div*
	NACE Rev. 2 main section	NACE Rev. 2 main section description	*nace_section*
	Number of employees	Total number of employees included in the company's payroll	*nr_emp*
	Number of patents	Number of patents owned by the company	*patents*
	Number of trademarks	Number of trademarks owned by the company	*trademarks*
	Peer Group Size	Size of the Bureau Van Dijk's standard peer group, *i.e.* companies with similar business characteristics	*peersize*
	Standardised legal form	Standardised legal form	*form*
Income Statement	Added value	Profit for period + Depreciation + Taxation + Interests paid + Cost of employees	*va*
	Cash flow	Profit for period + Depreciation	*cashflow*
	Cost of Employees	Detail of all the employees costs of the company (including pension costs)	*empcost*
	EBITDA	Operating profit + Depreciation	*ebitda*
	Gross profit	Operating revenue - Cost of goods sold	*gprofit*
	Interest paid	Total amount of interest charges paid for shares or loans	*intpaid*
	Operating revenue (Turnover)	Total operating revenues (Net sales + Other operating revenues+ Stock variations)	*turnover*

Appendix I continued

Source	Label	Definition	Short name
Ratios	P/L before tax	Operating profit + financial profit	*pbt*
	Profit margin	(Profit before tax / Operating revenue) * 100	*profmargin*
	Liquidity ratio	(Current assets - Stocks) / Current liabilities	*liquidity*
	ROA using P/L before tax	(Profit before tax / Total assets) * 100	*roa_pbt*
	ROE using P/L before tax	(Profit before tax / Shareholders funds) * 100	*roe_pbt*
	Solvency ratio (Asset based)	(Shareholders' funds / Total assets) * 100	*solvratio*

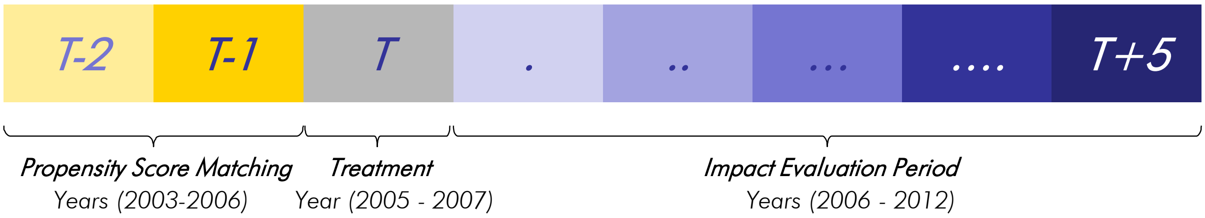
Source: Bureau Van Dijk (2015)

Appendix II: Dataset structure

Prior to the merging process, we scrutinise the original EIF MAP database of CESEE beneficiaries in order to remove a number of potential inconsistencies. In particular, we note that approximately 11.5% of paired beneficiaries receive more than one MAP guaranteed loan within the analysed time frame (*i.e.* up to 5 years after the first loan). As almost half of these *multiple* loans took place within the same quarter, we aggregate these into one single transaction per company (adjusting the characteristics of each transaction – *e.g.* maturity and guarantee rate – accordingly). Companies receiving one or more additional MAP-guaranteed loans outside the same quarter were discarded from the analysis.

In order to augment MAP beneficiaries with data extracted from Orbis, we set up an 8-years rolling window, centred on the *signature year* and focussing on the 2 years before and up to 5 years after the obtention of the underlying loan (see Figure 11). The first two years of the rolling window will be used to perform a *propensity score matching* based on firm's observable characteristics. The remaining years will be used to estimate the impact of the MAP facility, taking the treatment year as a reference point.

Figure 11: The 8-year rolling window



Source: Authors

Appendix III: Removal of outliers and sample selection criterion

In order to remove inconsistencies and outlying values, we further check extracted data from Orbis. This task involves a two-step strategy: first, we remove obvious mistakes in the reporting of data (*e.g.* negative turnover, total assets equalling zero, *etc.*). Second, we test various procedures for outlier detection, such as the use of trimmed means and variances, median absolute deviation, and rolling window median absolute deviation. Privileging the preservation of the sample size rather than the strict identification of each outlier, we set a relatively high threshold for acceptance.[56] This choice is justified by the fact that, at this stage, we only aim to pre-process the data, and that further outlier robustness checks will be used in the estimation phase.

By comparing the three different methodologies, we observe a high correlation between the outliers detected via trimmed mean estimation and median absolute deviation, while the outlier estimation via the rolling-window median absolute deviation identifies a significantly higher share of outliers. Such higher share is not always justified, and the third procedure is discarded as too aggressive. As such, we opt for the selection via the first method, which also entails a simpler implementation.

The final stage of data pre-processing involves the use of a sample selection criterion to identify a subset of MAP beneficiaries that can be successfully employed in order to provide a descriptive analysis of the performance before and after the obtention of a MAP--guaranteed loan. First, we analyse missing values and identify a non-random pattern which is consistent with what has been observed so far, that is, smaller companies tend to show more missing information. Moreover, Table 15 shows that missing entries for most variables are highly correlated with each other. This feature leads us to proceed with the selection of the final sample by focusing exclusively on the variable *Turnover*.

Of course, one strategy would be to simply discard all companies with missing entries in any given year. However, once again the goal was to minimise data loss without hampering the representativeness of the final estimation. Therefore, we apply a "milder" selection criterion which imposes that at least 6 out of 8 years must be available for each firm's time window, and that year *t-1* must always be non-missing. By comparing *ex-post* this criterion with yet more relaxed specifications, we do not observe significant differences. The final selected sample comprises 2,595 MAP beneficiaries, *i.e.* 18.3% of the original population of MAP CESEE beneficiaries.

The reader should note that this sample selection strategy is implemented exclusively to fulfil a descriptive assessment of the MAP CESEE beneficiaries in the analysed time frame. With respect to the econometric analysis, the final number of companies employed reflects the sample size with full data availability on selected financial indicators.

[56] That is, values were discarded if 10 times above or below the trimmed standard deviation in the first case, or 10 times above or below the ratio between the absolute distance from the median and the median absolute deviation in the second and third case.

Table 15: Correlation of missing entries for each variable

Missing values of:	turnover	totass	nr_emp	tanfixass	roe_pbt	cashflow	roa_pbt	pbt
turnover	1							
totass	0.9846	1						
nr_emp	0.8176	0.7075	1					
tanfixass	0.9883	0.9989	0.7372	1				
roe_pbt	0.9807	0.9979	0.6784	0.9944	1			
cashflow	0.9831	0.9961	0.6676	0.9923	0.9694	1		
roa_pbt	0.9862	0.9999	0.7082	0.9985	0.9978	0.9931	1	
pbt	0.9951	0.9966	0.7745	0.998	1	0.9946	1	1

Note: correlation coefficients estimated via tetrachoric correlation. All coefficients are significant at the 99% confidence level. Source: Authors

Appendix IV: Re-weighting implementation

Using consolidated statistical methods particularly common in the area of survey design (Deming and Stephan, 1940), three different dimensions for sample stratification are considered, corresponding to the ones that have shown the most significant differences in terms of distribution (the *strata*):

1. *Country (10 groups)*
2. *Number of Employees (5 groups)*
3. *Loan Amount (5 groups)*

The underlying assumption of this procedure is that, assuming each company to be representative of the specific *cluster* in which it has been assigned, the re-weighting of each cluster will be able to rebalance the original composition of the overall population. Therefore, our assumption is that by dissecting the overall population in 250 different clusters we are able to capture all heterogeneity in the original database. Weights are calculated by approximating to the nearest integer the inverse of the probability of extracting a firm i in cluster h, $Pr_i(I_h^x=1)$, where I_h^x is an indicator variable such that:

$$I_h^x = \begin{cases} 1 & \text{if the firm } i \text{ with non-missing value of } x \text{ in } h \text{ is extracted} \\ 0 & \text{otherwise} \end{cases}$$

where x indicates a reference variable for the calculation of missing variables. Therefore, the weights are computed as the inverse of:

$$Pr_i(I_h^x=1) = \frac{\text{Non-missing}(x)_h}{\text{Size}_h} \tag{9}$$

we estimate these probabilities as the simple proportion of non-missing in terms of x over the entire number of firms in the MAP SMEGF Database that belong to cluster h. As noted in Appendix III, missing values tend to be highly correlated among different variables. Therefore, for the purpose of this analysis we adopt *turnover* as the reference variable, and perform a sensitivity analysis of the resulting weights by testing their appropriateness on alternative variables (*e.g., total assets, number of employees*).

The final weights are calculated as follows: the first series of weights is computed using (9). The algorithm re-calibrates the sample and computes new weights based on the next stratum. The algorithm iterates these steps and converges when the average marginal change is lower than a pre-defined *tolerance level*.

With respect to our use of sampling weights, it is important to note the following two aspects:

A. Sample re-weighting is based on the variables available in the original MAP database. Although we observe that by controlling only for these three strata it is possible to obtain an improvement in population representativeness with respect to all available EIF SMEG variables, it is impossible to assess with certainty that controlling for these three variables will suffice in order to ensure full population representativeness of the treatment group in terms of all observable and unobservable characteristics.

B. The process of re-weighting may negatively affect the regression analysis to be performed on the treatment group. Scholars commonly observe that re-weighting tends to inflate regression's standard errors, which will in turn reduce the significance of the observed regression's coefficients. For such reasons, we refrain from using the estimated weights in our final estimation framework.

Appendix V: Estimation of Total Factor Productivity

Using data from firms' financial statements, we estimate the total factor productivity (TFP) for a subset of all MAP CESEE beneficiaries. The estimation method follows Wooldridge (2009), which builds on the methodology to address the simultaneity problem of TFP estimation set forth by Levinsohn and Petrin (2000), while also taking into account the critique to this latter found in Ackerberg et al. (2006). The estimation of the TFP parameter is based on a log-linear Cobb-Douglas production function of the form:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \theta_{it} + \eta_{it} \tag{10}$$

where y_{it} is the log of value added in a given period, l_{it} the log of number of employees, k_{it} the log of total assets, m_{it} the log expenditure in materials, θ_{it} the firm's log total factor productivity, and η_{it} the error term. All currency values have been deflated using country-level price indices for 10 industry branches obtained from Eurostat.[57]

The estimation of the TFP residual is possible for a restricted set of companies (7.5% of the overall MAP population), namely the ones that have sufficient data for it to be computed. Moreover, we remove the 1% left and right tails of the distribution in order to exclude outlying values. We test the assumption that the group of companies on which the TFP is estimable is not significantly different from the overall population, but the assumption is rejected at 1% significance level. Thus, using the re-weighting mechanism set forth in Appendix IV, we attempt at rebalancing the resulting distribution in order to counter the bias introduced by the estimation procedure. The result of this rebalancing is shown in Figure 12. We find our estimates consistent with Altomonte, Aquilante, and Ottaviano (2012), who include estimated TFPs for companies with 10 employees or more in 7 EU countries[58], once we account for the fact that our sample is composed at 50% by companies with less than 10 employees[59].

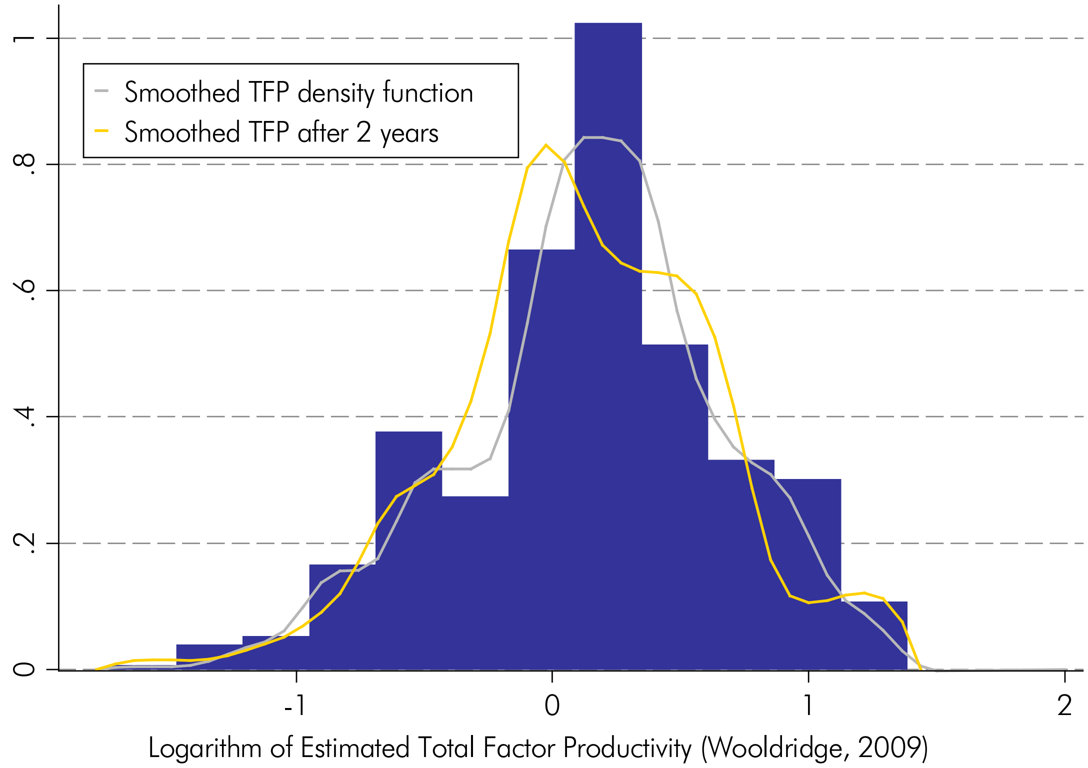
As noted in Gal (2013), the estimation of TFP based on balance sheet data may suffer from several limitations, which may ultimately impact the reliability of the proposed measure. In particular, the author notes that the lack of possible measures concerning the quality and intensity of labour, as well as the impossibility to discern the specific type of firm's capital goods, make the TFP estimate particularly sensitive to changes in capacity utilisation.

[57] Missing values for Bulgaria in sector C have been replaced with adjusted data from the National Statistical Institute of Bulgaria.

[58] Of which only one (Hungary) is part of the CESEE region

[59] Source: EIF MAP Database (see Section 4.1)

Figure 12: TFP distribution of MAP CESEE beneficiaries at signature date



Note: the TFP density distribution is estimated using a standard kernel function over 45 points of the TFP support. Resulting values have been smoothed using a rolling window median smoother. Source: Authors

Appendix VI: Tests on model's assumptions

VI.I Conditional independence assumption

Based on the PS model and the resulting estimated propensity scores, we implement a first test that aims to verify some aspects of the CIA. The procedure follows Chiappori and Salanie (2000) and it is based on the assumption that, once conditioned on an exhaustive set of characteristics, the residuals in Equation (6) should be independent of any omitted covariate. As the observed default can only occur after the obtention of a loan, such information cannot be included into the PS model, since in principle default might be a result of the treatment status and thus endogenous. This offers an ideal setting to test whether the model presented in (6) is, at least partially, robust to omitted variable bias. The test is implemented by estimating the generalised residuals of our propensity score estimation model and the generalised residuals of a logit model with the exact same regressors, but where the dependent variable is the observed default of company i. We estimate the generalised residuals following Gourieroux et al. (1987) and compute the following statistic:

$$W = \frac{Cov(\tilde{\varepsilon}_{is}, \tilde{\eta}_{is})^2}{Cov\left(\tilde{\varepsilon}_{is}{}^2, \tilde{\eta}_{is}{}^2\right)} \tag{11}$$

where $\tilde{\varepsilon}_{is}$ and $\tilde{\eta}_{is}$ are, respectively, the generalised residuals of (6) and of the modified version of (6) with the default indicator as the dependent variable. Under the null hypothesis that $\tilde{\varepsilon}_{is}$ and $\tilde{\eta}_{is}$ are uncorrelated, W is distributed asymptotically as $\chi^2(1)$. Using this framework, we obtain $W=0.015$, which does not reject the hypothesis that $\tilde{\varepsilon}_{is}$ and $\tilde{\eta}_{is}$ are uncorrelated. However, due to the very limited number of defaults within the 5 years after the obtention of the MAP guarantee, the second model is unable to reach maximum-likelihood convergence. In this respect, we further test the independence of the generalised residuals within a broader time window, once again obtaining a non-rejection of the null hypothesis.

VI.II Non-interference assumption

A second exercise tests the *non-interference* assumption of the SUTVA. As expressed in Section 5.1, we claim that, if an interference effect must exist, it must be observable only at the local level, where the probability of having a MAP CESEE beneficiary affecting the product line of another soon-to-be MAP CESEE beneficiary is higher. We thus identify possible *spillover* candidates by creating geographical and industrial clusters, using respectively the first two digits of the company's NACE Rev. 2 primary code and the first two digits of the company's postcode[60]. We also test this against a stricter clustering using the 3rd digit of the aforementioned variables. Using only the first two digits of the postcode and industry code, we find that around 10% of MAP beneficiaries are potential candidates for spillovers, while around 2% are 3rd digit candidates for

[60] Representing a proxy for NUTS 3 level geographical areas.

spillovers. A number of countries are dropped from the analysis because our procedure identifies no potential candidate for spillovers.

Subsequently, we assign a *placebo treatment* variable which is 1 for companies that are potential candidates for spillovers, and 0 otherwise. We use the propensity score estimated in Section 5.2.2 to control for observable characteristics, and employ a difference-in-differences framework that follows the principle set forth in Section 5.3. In particular, we estimate:

$$y_{it} = \beta_0 + \delta s_i + \gamma I_t + \theta(s_i \times I_t) + \lambda \hat{p}(\mathbf{X_i}) + \rho \mathbf{Z_i} + \varepsilon_{it} \tag{12}$$

where s_i is a dummy indicating whether company i is a candidate for spillovers, I_t is a set of time-specific dummies that indicate the time period (*i.e.* 1 year after the obtention of the guarantee, 2 years.., etc.), $s_i \times I_t$ is their interaction and θ is the placebo effect that we intend to test, $\hat{p}(\mathbf{X_i})$ is the estimated propensity score, used as a proxy to control for company's characteristics, and $\boldsymbol{Z_i}$ is a set of additional controls. We use three different dependent variables, that is, the logarithm of number of employees, turnover and total assets.

Using the specified model, we observe that no significant effect at 95% confidence level is estimated for potential spillover candidates.[61] Therefore, no significant difference arises, at least in the relevant attributes analysed, between companies that could have been indirectly impacted by the MAP before obtaining a loan, and companies that have certainly not been impact by the MAP before entering the programme. The same finding is observable when using a stricter clustering, and when using the overall treatment sample (Section 4.3).[62] Finally, our findings are robust to possible distortions in the estimation of DID standard errors (Bertrand et al., 2004).

VI.III Common trend assumption

A final test aims to validate the assumption of common trend in the difference-in-differences estimation framework. The unbiased estimation of θ in Equation (7) relies on the key assumption of *common trend* (Equation (5), that is, the time trends of control firms represent, on average, the path that treated firms would have followed had they not received a MAP-guaranteed loan. In order to validate this assumption, we test for equality of means up to 4 years before the obtention of a loan, obtaining for all key performance indicators a non-rejection of the null hypothesis. Furthermore, we implement a *placebo* test, by estimating potential ATEs in the years before the actual obtention of the loan. The logic of this test consists in estimating the impact of the treatment on an outcome that is certainly not affected by it; should any significant impact emerge, it would be an indication of the inappropriateness of the common trend assumption. In order to avoid incurring in a significant loss of data, which would ultimately affect the reliability of our placebo test, we perform this test by comparing only years *t-2* and *t-1*. The estimated ATE is

[61] Estimates of robustness models are not included in this publication, but are available upon request to the authors.

[62] In this case, we replace $\hat{p}(\mathbf{X_i})$ with a set of relevant control variables.

reported in Table 16, where it can be seen that no significant difference is observable between treatment and control groups before the obtention of a MAP-guaranteed loan.

Table 16: Placebo tests[63]

	(1)	(2)	(3)	(4)	(5)
	Log of Number of employees	Log of Turnover	Log of Total assets	Log of P/L before tax	Current Ratio
Treatment (T_i)	0.0136	0.0164	0.0046	-0.0105	-0.0014
	(0.042)	(0.054)	(0.054)	(0.074)	(0.001)
Period t-1 (I_{t-1})	0.1626***	0.4044***	0.3954***	0.2698***	-0.0010
	(0.043)	(0.057)	(0.057)	(0.079)	(0.001)
ATE ($T_i \times I_{t-1}$)	-0.0282	0.0265	0.0341	0.0458	0.0015
	(0.057)	(0.076)	(0.075)	(0.102)	(0.002)
Obs.	5,584	5,690	5,661	5,003	5,647
R^2	0.173	0.219	0.209	0.064	0.016

All models estimated using firm-level and country-level controls. Standard errors in parentheses. * *$p < 0.10$,* ** *$p < 0.05$,* *** *$p < 0.01$. Source: Authors*

[63] All reported models estimated via OLS, using firm-level and country-level controls. The consistency of standard errors was tested by means of cluster-robust and block-bootstrap estimations.

Appendix VII: Robustness checks on Difference-in-Differences Estimates

Table 17: Estimated ATE on logarithm of number of employees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[A]	OLS[B]	OLS[C]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0148 (0.045)	-0.0034 (0.041)	-0.0014 (0.041)		-0.0014 (0.040)	-0.0014 (0.040)	-0.0044 (0.039)
I_0	0.0990*** (0.035)	0.0716** (0.033)	0.0360 (0.034)	0.0779*** (0.014)	0.0360** (0.014)	0.0360** (0.014)	0.0695 (0.075)
I_1	0.1416*** (0.036)	0.0916*** (0.034)	0.0260 (0.039)	0.0936*** (0.015)	0.0260 (0.025)	0.0260 (0.027)	
I_2	0.1722*** (0.036)	0.0788** (0.034)	0.0118 (0.046)	0.1044*** (0.015)	0.0118 (0.034)	0.0118 (0.035)	
I_3	0.1076*** (0.036)	0.0019 (0.034)	-0.0601 (0.055)	0.0720*** (0.016)	-0.0601 (0.042)	-0.0601 (0.044)	
I_4	0.0796** (0.036)	-0.0561 (0.035)	-0.1016* (0.056)	0.0394** (0.016)	-0.1016** (0.044)	-0.1016** (0.046)	
I_5	0.0486 (0.037)	-0.1100*** (0.036)	-0.1742*** (0.054)		-0.1742*** (0.047)	-0.1742*** (0.047)	
$T_i \times I_0$ (=ATE_{t0})	0.0456 (0.064)	0.0509 (0.059)	0.0505 (0.059)	0.0495* (0.027)	0.0505** (0.023)	0.0505** (0.023)	0.1487*** (0.055)
$T_i \times I_1$ (=ATE_{t1})	0.1386** (0.064)	0.1389** (0.060)	0.1372** (0.060)	0.1292*** (0.027)	0.1372*** (0.030)	0.1372*** (0.030)	
$T_i \times I_2$ (=ATE_{t2})	0.1407** (0.064)	0.1430** (0.059)	0.1414** (0.059)	0.1584*** (0.027)	0.1414*** (0.031)	0.1414*** (0.032)	
$T_i \times I_3$ (=ATE_{t3})	0.1868*** (0.065)	0.1880*** (0.060)	0.1875*** (0.060)	0.1949*** (0.027)	0.1875*** (0.035)	0.1875*** (0.034)	
$T_i \times I_4$ (=ATE_{t4})	0.1809*** (0.065)	0.1838*** (0.060)	0.1812*** (0.060)	0.1975*** (0.028)	0.1812*** (0.038)	0.1812*** (0.038)	
$T_i \times I_5$ (=ATE_{t5})	0.1693** (0.066)	0.1764*** (0.061)	0.1729*** (0.061)	0.1844*** (0.028)	0.1729*** (0.042)	0.1729*** (0.041)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	18,984	18,984	18,984	18,984	18,984	18,984	5,848
Number of firms	2,924	2,924	2,924	2,924	2,924	2,924	2,924
R^2	0.007	0.147	0.148	0.031	0.148	0.148	0.165

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [A] Model estimated without controls, [B] Model estimated with firm-level controls, [C] Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors*

Table 18: Estimated ATE on logarithm of total assets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[(A)]	OLS[(B)]	OLS[(C)]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0973* (0.059)	0.0570 (0.054)	0.0571 (0.054)		0.0571 (0.050)	0.0571 (0.054)	0.0519 (0.052)
I_0	0.2034*** (0.047)	0.1734*** (0.043)	0.1718*** (0.044)	0.1316*** (0.014)	0.1718*** (0.014)	0.1718*** (0.014)	0.3126*** (0.099)
I_1	0.3369*** (0.047)	0.2718*** (0.044)	0.2977*** (0.051)	0.1963*** (0.014)	0.2977*** (0.028)	0.2977*** (0.028)	
I_2	0.3295*** (0.047)	0.2282*** (0.044)	0.3148*** (0.059)	0.1737*** (0.014)	0.3148*** (0.039)	0.3148*** (0.038)	
I_3	0.2911*** (0.047)	0.1636*** (0.044)	0.2952*** (0.070)	0.1283*** (0.015)	0.2952*** (0.049)	0.2952*** (0.047)	
I_4	0.2596*** (0.047)	0.1054** (0.045)	0.2435*** (0.072)	0.0624*** (0.015)	0.2435*** (0.052)	0.2435*** (0.050)	
I_5	0.2814*** (0.047)	0.1097** (0.046)	0.2180*** (0.070)		0.2180*** (0.055)	0.2180*** (0.054)	
$T_i \times I_0$ (=ATE_{t_0})	0.3218*** (0.083)	0.3217*** (0.077)	0.3211*** (0.077)	0.3202*** (0.026)	0.3211*** (0.021)	0.3211*** (0.021)	0.3336*** (0.073)
$T_i \times I_1$ (=ATE_{t_1})	0.3422*** (0.084)	0.3390*** (0.078)	0.3380*** (0.078)	0.3164*** (0.026)	0.3380*** (0.028)	0.3380*** (0.027)	
$T_i \times I_2$ (=ATE_{t_2})	0.3706*** (0.084)	0.3675*** (0.077)	0.3692*** (0.077)	0.3389*** (0.026)	0.3692*** (0.031)	0.3692*** (0.031)	
$T_i \times I_3$ (=ATE_{t_3})	0.3380*** (0.083)	0.3328*** (0.077)	0.3338*** (0.077)	0.3278*** (0.026)	0.3338*** (0.033)	0.3338*** (0.034)	
$T_i \times I_4$ (=ATE_{t_4})	0.3200*** (0.084)	0.3198*** (0.077)	0.3179*** (0.077)	0.3165*** (0.026)	0.3179*** (0.037)	0.3179*** (0.038)	
$T_i \times I_5$ (=ATE_{t_5})	0.3161*** (0.085)	0.3234*** (0.078)	0.3217*** (0.078)	0.3191*** (0.026)	0.3217*** (0.042)	0.3217*** (0.042)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	19,639	19,639	19,639	19,639	19,639	19,639	5,845
Number of firms	2,924	2,924	2,924	2,924	2,924	2,924	2,924
R^2	0.025	0.171	0.172	0.114	0.172	0.172	0.188

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [(A)] Model estimated without controls, [(B)] Model estimated with firm-level controls, [(C)] Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors*

Table 19: Estimated ATE on logarithm of turnover

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[(A)]	OLS[(B)]	OLS[(C)]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.1076*	0.0574	0.0598		0.0598	0.0598	0.0545
	(0.064)	(0.058)	(0.058)		(0.050)	(0.050)	(0.053)
I_0	0.1332***	0.1098**	0.0886*	0.1301***	0.0886***	0.0886***	0.0873
	(0.051)	(0.046)	(0.048)	(0.018)	(0.015)	(0.016)	(0.101)
I_1	0.1920***	0.1441***	0.1362**	0.1958***	0.1362***	0.1362***	
	(0.051)	(0.047)	(0.054)	(0.019)	(0.031)	(0.031)	
I_2	0.0805	0.0011	0.0679	0.1759***	0.0679	0.0679	
	(0.051)	(0.047)	(0.063)	(0.019)	(0.044)	(0.045)	
I_3	-0.0491	-0.1542***	-0.0054	0.1468***	-0.0054	-0.0054	
	(0.051)	(0.047)	(0.074)	(0.020)	(0.055)	(0.057)	
I_4	-0.1948***	-0.3263***	-0.1560**	0.0456**	-0.1560***	-0.1560***	
	(0.051)	(0.048)	(0.076)	(0.020)	(0.058)	(0.060)	
I_5	-0.2016***	-0.3488***	-0.2366***		-0.2366***	-0.2366***	
	(0.051)	(0.049)	(0.075)		(0.061)	(0.063)	
$T_i \times I_0$ (=ATE_{t0})	0.0838	0.0881	0.0863	0.0831**	0.0863***	0.0863***	0.1718**
	(0.091)	(0.082)	(0.082)	(0.035)	(0.019)	(0.018)	(0.075)
$T_i \times I_1$ (=ATE_{t1})	0.1547*	0.1481*	0.1446*	0.1376***	0.1446***	0.1446***	
	(0.091)	(0.083)	(0.083)	(0.035)	(0.027)	(0.026)	
$T_i \times I_2$ (=ATE_{t2})	0.1815**	0.1820**	0.1809**	0.1856***	0.1809***	0.1809***	
	(0.091)	(0.082)	(0.082)	(0.035)	(0.032)	(0.033)	
$T_i \times I_3$ (=ATE_{t3})	0.1463	0.1533*	0.1522*	0.1700***	0.1522***	0.1522***	
	(0.091)	(0.082)	(0.082)	(0.035)	(0.039)	(0.041)	
$T_i \times I_4$ (=ATE_{t4})	0.1749*	0.1823**	0.1766**	0.1952***	0.1766***	0.1766***	
	(0.091)	(0.083)	(0.083)	(0.035)	(0.044)	(0.045)	
$T_i \times I_5$ (=ATE_{t5})	0.1938**	0.2020**	0.1967**	0.2083***	0.1967***	0.1967***	
	(0.092)	(0.083)	(0.083)	(0.035)	(0.051)	(0.054)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	19,914	19,914	19,914	19,914	19,914	19,914	5,848
Number of firms	2,924	2,924	2,924	2,924	2,924	2,924	2,924
R^2	0.012	0.192	0.193	0.074	0.193	0.193	0.205

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (A) Model estimated without controls, (B) Model estimated with firm-level controls, (C) Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors*

Table 20: Estimated ATE on logarithm of P/L before tax

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[(A)]	OLS[(B)]	OLS[(C)]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0733	0.0476	0.0470		0.0470	0.0470	0.0424
	(0.077)	(0.075)	(0.075)		(0.069)	(0.068)	(0.068)
I_0	0.1610**	0.1457**	0.1830***	0.1095***	0.1830***	0.1830***	0.4148***
	(0.063)	(0.061)	(0.063)	(0.037)	(0.038)	(0.037)	(0.130)
I_1	0.2587***	0.2289***	0.3774***	0.2737***	0.3774***	0.3774***	
	(0.064)	(0.063)	(0.073)	(0.038)	(0.056)	(0.060)	
I_2	0.1621**	0.1095*	0.4386***	0.3490***	0.4386***	0.4386***	
	(0.064)	(0.063)	(0.086)	(0.039)	(0.076)	(0.080)	
I_3	-0.0539	-0.1219*	0.3943***	0.3286***	0.3943***	0.3943***	
	(0.065)	(0.065)	(0.103)	(0.043)	(0.092)	(0.095)	
I_4	-0.0998	-0.1956***	0.3481***	0.2326***	0.3481***	0.3481***	
	(0.066)	(0.067)	(0.106)	(0.043)	(0.097)	(0.100)	
I_5	-0.1769***	-0.2819***	0.1881*		0.1881*	0.1881*	
	(0.067)	(0.068)	(0.104)		(0.098)	(0.100)	
$T_i \times I_0$ ($=ATE_{t_0}$)	0.0459	0.0460	0.0463	0.0680	0.0463	0.0463	0.1137
	(0.110)	(0.107)	(0.107)	(0.068)	(0.061)	(0.058)	(0.094)
$T_i \times I_1$ ($=ATE_{t_1}$)	0.0265	0.0278	0.0299	0.0730	0.0299	0.0299	
	(0.112)	(0.109)	(0.109)	(0.069)	(0.072)	(0.071)	
$T_i \times I_2$ ($=ATE_{t_2}$)	0.0773	0.0841	0.0869	0.1249*	0.0869	0.0869	
	(0.112)	(0.109)	(0.109)	(0.069)	(0.077)	(0.075)	
$T_i \times I_3$ ($=ATE_{t_3}$)	0.0334	0.0261	0.0282	0.0537	0.0282	0.0282	
	(0.114)	(0.112)	(0.111)	(0.071)	(0.082)	(0.079)	
$T_i \times I_4$ ($=ATE_{t_4}$)	-0.0541	-0.0416	-0.0448	-0.0013	-0.0448	-0.0448	
	(0.116)	(0.114)	(0.113)	(0.073)	(0.086)	(0.082)	
$T_i \times I_5$ ($=ATE_{t_5}$)	0.1186	0.1350	0.1337	0.1526**	0.1337	0.1337	
	(0.118)	(0.115)	(0.115)	(0.073)	(0.091)	(0.088)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	16,192	16,192	16,192	16,192	16,192	16,192	5,520
Number of firms	2,899	2,899	2,899	2,899	2,899	2,899	2,899
R^2	0.007	0.057	0.060	0.040	0.060	0.060	0.067

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [(A)] Model estimated without controls, [(B)] Model estimated with firm-level controls, [(C)] Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors

Table 21: Estimated ATE on logarithm of TFP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[(A)]	OLS[(B)]	OLS[(C)]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0552*	0.0372	0.0378		0.0378	0.0378	0.0368
	(0.030)	(0.028)	(0.028)		(0.027)	(0.027)	(0.026)
I_0	0.0123	0.0227	0.0095	0.0367**	0.0095	0.0095	0.0398
	(0.026)	(0.024)	(0.026)	(0.016)	(0.018)	(0.019)	(0.077)
I_1	0.0245	0.0346	-0.0013	0.0529***	-0.0013	-0.0013	
	(0.027)	(0.025)	(0.033)	(0.018)	(0.028)	(0.028)	
I_2	0.0207	0.0389	-0.0173	0.0470**	-0.0173	-0.0173	
	(0.028)	(0.026)	(0.041)	(0.020)	(0.039)	(0.039)	
I_3	-0.0254	0.0092	-0.0471	0.0629***	-0.0471	-0.0471	
	(0.028)	(0.026)	(0.048)	(0.022)	(0.046)	(0.049)	
I_4	-0.1293***	-0.1047***	-0.1495***	-0.0096	-0.1495***	-0.1495***	
	(0.028)	(0.027)	(0.048)	(0.021)	(0.046)	(0.048)	
I_5	-0.1675***	-0.1309***	-0.1730***		-0.1730***	-0.1730***	
	(0.029)	(0.028)	(0.048)		(0.046)	(0.047)	
$T_i \times I_0$ (=ATE_{I_0})	-0.1169***	-0.1173***	-0.1179***	-0.1188***	-0.1179***	-0.1179***	-0.1029***
	(0.044)	(0.040)	(0.040)	(0.028)	(0.025)	(0.025)	(0.037)
$T_i \times I_1$ (=ATE_{I_1})	-0.0530	-0.0524	-0.0532	-0.0808***	-0.0532*	-0.0532*	
	(0.045)	(0.042)	(0.042)	(0.029)	(0.030)	(0.028)	
$T_i \times I_2$ (=ATE_{I_2})	-0.0700	-0.0699*	-0.0703*	-0.0751**	-0.0703**	-0.0703**	
	(0.045)	(0.042)	(0.042)	(0.030)	(0.032)	(0.031)	
$T_i \times I_3$ (=ATE_{I_3})	-0.0783*	-0.0894**	-0.0894**	-0.1100***	-0.0894***	-0.0894***	
	(0.045)	(0.042)	(0.042)	(0.030)	(0.034)	(0.035)	
$T_i \times I_4$ (=ATE_{I_4})	-0.0561	-0.0642	-0.0652	-0.0908***	-0.0652*	-0.0652*	
	(0.046)	(0.043)	(0.043)	(0.030)	(0.037)	(0.036)	
$T_i \times I_5$ (=ATE_{I_5})	-0.0739	-0.0818*	-0.0833*	-0.1047***	-0.0833**	-0.0833**	
	(0.047)	(0.043)	(0.043)	(0.031)	(0.037)	(0.036)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	8,965	8,965	8,965	8,965	8,965	8,965	3,164
Number of firms (i)	1,604	1,604	1,604	1,604	1,604	1,604	1,604
R^2	0.016	0.160	0.161	0.045	0.161	0.161	0.190

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [(A)] Model estimated without controls, [(B)] Model estimated with firm-level controls, [(C)] Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors

Table 22: Estimated ATE on ROA (pbt based)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[(A)]	OLS[(B)]	OLS[(C)]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0027 (0.007)	0.0040 (0.007)	0.0043 (0.007)		0.0043 (0.007)	0.0043 (0.007)	0.0050 (0.006)
I_0	-0.0205*** (0.006)	-0.0193*** (0.006)	-0.0170*** (0.006)	-0.0065 (0.004)	-0.0170*** (0.005)	-0.0170*** (0.005)	-0.0056 (0.012)
I_1	-0.0329*** (0.006)	-0.0302*** (0.006)	-0.0183*** (0.007)	0.0016 (0.005)	-0.0183*** (0.006)	-0.0183*** (0.007)	
I_2	-0.0547*** (0.006)	-0.0514*** (0.006)	-0.0215*** (0.008)	0.0081* (0.005)	-0.0215*** (0.008)	-0.0215*** (0.008)	
I_3	-0.0825*** (0.006)	-0.0783*** (0.006)	-0.0251*** (0.009)	0.0151*** (0.005)	-0.0251*** (0.009)	-0.0251*** (0.010)	
I_4	-0.1075*** (0.006)	-0.1020*** (0.006)	-0.0448*** (0.010)	0.0057 (0.005)	-0.0448*** (0.010)	-0.0448*** (0.010)	
I_5	-0.1134*** (0.006)	-0.1071*** (0.006)	-0.0602*** (0.009)		-0.0602*** (0.010)	-0.0602*** (0.010)	
$T_i{\times}I_0\ (=ATE_{t_0})$	-0.0293*** (0.010)	-0.0290*** (0.010)	-0.0293*** (0.010)	-0.0283*** (0.008)	-0.0293*** (0.007)	-0.0293*** (0.007)	-0.0181** (0.009)
$T_i{\times}I_1\ (=ATE_{t_1})$	-0.0214** (0.011)	-0.0217** (0.010)	-0.0218** (0.010)	-0.0214** (0.009)	-0.0218*** (0.008)	-0.0218*** (0.008)	
$T_i{\times}I_2\ (=ATE_{t_2})$	-0.0180* (0.011)	-0.0177* (0.010)	-0.0174* (0.010)	-0.0173** (0.009)	-0.0174** (0.008)	-0.0174** (0.008)	
$T_i{\times}I_3\ (=ATE_{t_3})$	-0.0290*** (0.010)	-0.0286*** (0.010)	-0.0290*** (0.010)	-0.0275*** (0.008)	-0.0290*** (0.009)	-0.0290*** (0.010)	
$T_i{\times}I_4\ (=ATE_{t_4})$	-0.0077 (0.011)	-0.0075 (0.010)	-0.0082 (0.010)	-0.0080 (0.009)	-0.0082 (0.009)	-0.0082 (0.009)	
$T_i{\times}I_5\ (=ATE_{t_5})$	0.0062 (0.011)	0.0070 (0.010)	0.0064 (0.010)	0.0047 (0.009)	0.0064 (0.009)	0.0064 (0.010)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	19,478	19,478	19,478	19,478	19,478	19,478	5,821
Number of firms (i)	2,921	2,921	2,921	2,921	2,921	2,921	2,921
R^2	0.046	0.080	0.085	0.091	0.085	0.085	0.124

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (A) Model estimated without controls, (B) Model estimated with firm-level controls, (C) Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors*

Table 23: Estimated ATE on current ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS(A)	OLS(B)	OLS(C)	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0000	0.0002	0.0001		0.0001	0.0001	0.0001
	(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	(0.002)
I_0	0.0031**	0.0033**	0.0041**	0.0016	0.0041***	0.0041***	0.0149***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)
I_1	0.0043***	0.0046***	0.0063***	0.0016	0.0063***	0.0063***	
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	
I_2	0.0064***	0.0073***	0.0095***	0.0023*	0.0095***	0.0095***	
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	
I_3	0.0091***	0.0101***	0.0126***	0.0020	0.0126***	0.0126***	
	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)	(0.003)	
I_4	0.0112***	0.0122***	0.0145***	0.0008	0.0145***	0.0145***	
	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)	(0.003)	
I_5	0.0134***	0.0145***	0.0169***		0.0169***	0.0169***	
	(0.002)	(0.002)	(0.003)		(0.003)	(0.003)	
$T_i \times I_0$ (=ATE_{t_0})	-0.0047*	-0.0047*	-0.0046*	-0.0047**	-0.0046**	-0.0046**	-0.0078***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
$T_i \times I_1$ (=ATE_{t_1})	-0.0056**	-0.0055**	-0.0054*	-0.0060***	-0.0054***	-0.0054***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	
$T_i \times I_2$ (=ATE_{t_2})	-0.0080***	-0.0078***	-0.0077***	-0.0085***	-0.0077***	-0.0077***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	
$T_i \times I_3$ (=ATE_{t_3})	-0.0090***	-0.0090***	-0.0090***	-0.0093***	-0.0090***	-0.0090***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	
$T_i \times I_4$ (=ATE_{t_4})	-0.0075***	-0.0076***	-0.0075***	-0.0076***	-0.0075***	-0.0075***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	
$T_i \times I_5$ (=ATE_{t_5})	-0.0080***	-0.0081***	-0.0080***	-0.0088***	-0.0080***	-0.0080***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	19,498	19,498	19,498	19,498	19,498	19,498	5,837
Number of firms	2,923	2,923	2,923	2,923	2,923	2,923	2,923
R^2	0.010	0.026	0.027	0.013	0.027	0.027	0.034

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (A) Model estimated without controls, (B) Model estimated with firm-level controls, (C) Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors.*

Table 24: Estimated ATE on solvency ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS[A]	OLS[B]	OLS[C]	F-E	OLS (CLUSTER VAR-COVAR)	OLS (BLOCK BOOTSTRAP)	OLS (PRE-POST)
T_i	0.0044	0.0018	0.0020		0.0020	0.0020	0.0025
	(0.012)	(0.012)	(0.012)		(0.010)	(0.010)	(0.011)
I_0	0.0195**	0.0123	0.0098	0.0077	0.0098**	0.0098**	0.0233
	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)	(0.005)	(0.020)
I_1	0.0380***	0.0221**	0.0181*	0.0134***	0.0181**	0.0181**	
	(0.010)	(0.009)	(0.011)	(0.005)	(0.008)	(0.008)	
I_2	0.0614***	0.0392***	0.0356***	0.0234***	0.0356***	0.0356***	
	(0.010)	(0.009)	(0.013)	(0.005)	(0.011)	(0.011)	
I_3	0.0625***	0.0329***	0.0308**	0.0146***	0.0308**	0.0308**	
	(0.010)	(0.009)	(0.015)	(0.006)	(0.013)	(0.014)	
I_4	0.0694***	0.0327***	0.0326**	0.0079	0.0326**	0.0326**	
	(0.010)	(0.010)	(0.015)	(0.005)	(0.014)	(0.014)	
I_5	0.0746***	0.0321***	0.0322**		0.0322**	0.0322**	
	(0.010)	(0.010)	(0.015)		(0.014)	(0.015)	
$T_i \times I_0$ (=ATE_{t_0})	-0.0646***	-0.0643***	-0.0645***	-0.0635***	-0.0645***	-0.0645***	-0.0476***
	(0.017)	(0.016)	(0.016)	(0.009)	(0.007)	(0.007)	(0.015)
$T_i \times I_1$ (=ATE_{t_1})	-0.0606***	-0.0599***	-0.0602***	-0.0565***	-0.0602***	-0.0602***	
	(0.017)	(0.017)	(0.017)	(0.010)	(0.009)	(0.009)	
$T_i \times I_2$ (=ATE_{t_2})	-0.0588***	-0.0579***	-0.0582***	-0.0524***	-0.0582***	-0.0582***	
	(0.017)	(0.016)	(0.017)	(0.010)	(0.010)	(0.010)	
$T_i \times I_3$ (=ATE_{t_3})	-0.0622***	-0.0610***	-0.0612***	-0.0516***	-0.0612***	-0.0612***	
	(0.017)	(0.016)	(0.016)	(0.010)	(0.012)	(0.011)	
$T_i \times I_4$ (=ATE_{t_4})	-0.0426**	-0.0426**	-0.0429***	-0.0340***	-0.0429***	-0.0429***	
	(0.017)	(0.017)	(0.017)	(0.010)	(0.013)	(0.012)	
$T_i \times I_5$ (=ATE_{t_5})	-0.0235	-0.0223	-0.0227	-0.0185*	-0.0227	-0.0227*	
	(0.017)	(0.017)	(0.017)	(0.010)	(0.014)	(0.013)	
Firm-level Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-level Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	19,411	19,411	19,411	19,411	19,411	19,411	5,829
Number of firms	2,919	2,919	2,919	2,919	2,919	2,919	2,919
R^2	0.012	0.079	0.079	0.021	0.079	0.079	0.094

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [A] Model estimated without controls, [B] Model estimated with firm-level controls, [C] Model estimated with firm- and country-level controls. Standard errors in parentheses. Source: Authors.

Appendix VIII: List of Acronyms

Countries

- BG: Bulgaria
- CY: Cyprus
- CZ: Czech Republic
- EE: Estonia
- HU: Hungary
- LT: Lithuania
- LV: Latvia
- PL: Poland
- RO: Romania
- SI: Slovenia
- SK: Slovakia
- TR: Turkey

Other Acronyms

- ATE: Average Treatment Effect
- CESEE: Central, Eastern and South-Eastern European
- CGS: Credit Guarantee Scheme
- CIA: Conditional Independence Assumption
- CIP: Competitiveness and Innovation Framework Programme
- DG ECFIN: DG for Economic and Financial Affairs
- DG ENTR: DG Enterprise and Industry
- DG GROW: DG for Internal Market, Industry, Entrepreneurship and SMEs
- DG: Directorate General
- DID: Difference-in-Differences
- EBRD: European Bank for Reconstruction and Development
- EC: European Commission
- EIF: European Investment Fund
- EIP: Entrepreneurship and Innovation Programme
- EU: European Union
- FMA: Fiduciary and Management Agreement
- GDP: Gross Domestic Product
- KPI: Key Performance Indicator
- MAP: Multi-Annual Programme
- NACE: General Industrial Classification of Economic Activities within the European Communities
- NGO: Non-Governmental Organization
- NN: Nearest-Neighbour
- OECD: Organisation for Economic Co-operation and Development
- OLS: Ordinary Least Squares
- PBT: Profit and Loss Before Taxes
- PCGS: Public Credit Guarantee Scheme

- PS: Propensity Score
- PSM: Propensity Score Matching
- ROA: Return-on-Assets
- ROE: Return-on-Equity
- SBA: Small Business Administration
- SBLA: Small Business Loans Act
- SME: Small and Medium-sized Enterprise
- SMEGF: SME Guarantee Facility
- SUTVA: Stable Unit Treatment Value Assumption
- TFP: Total Factor Productivity
- US: United States
- USAID: United States Agency for International Development

About …

… the European Investment Fund

The European Investment Fund (EIF) is the European body specialised in small and medium sized enterprise (SME) risk financing. The EIF is part of the European Investment Bank group and has a unique combination of public and private shareholders. It is owned by the EIB (63.7%), the European Union - through the European Commission (24.3%) and a number (26 from 15 countries) of public and private financial institutions (12.0%).

EIF's central mission is to support Europe's SMEs by helping them to access finance. EIF primarily designs and develops venture capital and guarantees instruments which specifically target this market segment. In this role, EIF fosters EU objectives in support of innovation, research and development, entrepreneurship, growth, and employment.

The EIF total net commitments to venture capital and private equity funds amounted to over EUR 8.2bn at end 2014. With investments in over 500 funds, the EIF is the leading player in European venture capital due to the scale and the scope of its investments, especially in the high-tech and early-stage segments. The EIF commitment in guarantees totaled over EUR 5.7bn in over 300 operations at end 2014, positioning it as a major European SME loan guarantees actor and a leading microfinance guarantor.

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