Who Takes Up, Who Gains, Who Profits: Evidence from Spain's 'Kit Digital' Digitalisation Vouchers*

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Abstract

Governments increasingly pay the full price of digital tools for small firms. We study Kit Digital, a €3 billion Spanish voucher programme for every firm with fewer than 50 employees, linking 1.5 million contracts and a self-diagnosis completed before applying to the balance sheets of firms and providers. Three in four eligible firms never applied. Among applicants, more leveraged firms were more likely to withdraw before signing a contract, which commits them to advancing the VAT: a fully subsidised purchase does not remove financial constraints. What firms purchased is closely associated with the identity of their provider, which explains up to 49 percentage points of the variation in purchases, against at most 3 for the firm's own accounts and the technologies it already used. Adoption raised total factor productivity by 0.9 percent, and the gain does not increase systematically with intangible capital or prior digital maturity. These results suggest that a fully subsidised voucher delivered through private providers shifts the choice of technology towards the provider, and that removing the price of adoption does not remove the need to finance the commitment to purchase.

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1 Introduction

Small firms lag behind larger firms in the adoption of digital technologies (Cirera et al., 2022; Nicoletti et al., 2020), and governments devote substantial resources to closing this gap. In Europe, NextGenerationEU has funded large-scale investments and reforms supporting the digital and green transitions (Bańkowski et al., 2024). Spain's *Kit Digital* is one of the largest programmes of this kind. With a budget of €3.067 billion, it granted more than 860,000 vouchers to small firms and the self-employed (Red.es, 2026). Firms can use the vouchers to purchase standardised digital solutions from accredited private providers, which deliver the services and redeem the vouchers directly with the programme administrator.

The expected effects of such a voucher scheme are not clear ex ante. If the cost of digital technologies is the main barrier to adoption, providing them at little or no cost should generate high take-up. If instead cost is not the relevant constraint, the programme may primarily subsidise investments that would have occurred anyway, generating deadweight. Our results for Kit Digital show that the first mechanism has limited explanatory power, while the second captures only part of the programme's effects. Three quarters of eligible firms did not apply, indicating that eliminating the direct cost of adoption was not sufficient to induce take-up among most firms. Among participating firms, the choice of digital solution and its price were formally determined by the firm, but in practice varied substantially more across providers than across firm characteristics and needs. In addition, providers earned a surplus on programme sales well above their usual margins. We document these patterns by tracking the universe of eligible firms throughout the programme, linking eligibility and take-up to the solutions contracted, the prices paid, subsequent productivity outcomes, and suppliers' revenues.

Several mechanisms may limit the effectiveness of such a programme. First, by reducing the firm's out-of-pocket cost to zero, the voucher may weaken incentives to bargain over prices or to select the most appropriate solution, while providers have incentives to extract as much of the subsidy as possible. Second, there may be substantial scope for deadweight: many eligible firms were already using the subsidised technologies before the programme. Finally, the productivity returns to basic digital technologies may be limited in the absence of the complementary organisational investments required to use them effectively (Brynjolfsson and Hitt, 2000). At the same time, several mechanisms could generate positive effects. The voucher relaxes not only the direct cost of adoption but also the need to finance the initial investment, which may be particularly important for small firms facing financing constraints (Banerjee and Duflo, 2014; Howell, 2017). The programme may also leverage providers to identify and reach potential adopters that would otherwise be costly for the administration to target. Finally, the subsidised technologies can reduce firms' costs of reaching customers and managing customer relationships (Goldfarb and Tucker, 2019). Which of these mechanisms dominates is ultimately an empirical question, with direct implications for the design of future digital-adoption programmes.

Kit Digital provides Spanish SMEs with vouchers ranging from €2,000 to €12,000, depending on firm size, to purchase digital services from a predefined catalogue. Eligible solutions include websites, social media management, e-commerce, customer and process management, electronic invoicing, cybersecurity, and business analytics. Firms select an accredited provider (*agente digitalizador*) and enter into a separate contract for each solution. Upon signing the contract, the firm assigns to the provider the right to redeem the corresponding subsidy from the programme administrator. Up to a solution-specific cap, the voucher covers the full price excluding VAT, which is initially paid by the firm and subsequently recovered through the tax system.

Three features of Kit Digital make it well suited to studying these questions. First, the voucher reduces the firm's out-of-pocket price to zero up to the solution-specific cap, allowing

us to study barriers to adoption other than the direct monetary cost. Second, we observe the programme at each stage of implementation. Red.es, the public agency administering the programme, provided us with the universe of applications, 1.5 million contracts between firms and accredited providers, and a digital-maturity self-diagnosis completed by each applicant prior to applying, which records the technologies already in use. We link these administrative records to balance-sheet data for both firms and providers from the Central de Balances of the Banco de España. Because these data cover both applicants and non-applicants, we can compare firms that participated in the programme with otherwise eligible firms that did not. Third, in July 2024, the voucher available to the smallest firms increased by €1,000. This externally determined increase provides variation in subsidy generosity that was not chosen by providers, allowing us to study how additional subsidy funds are allocated and how providers respond to an exogenous increase in subsidised demand.

We report four main findings. First, the main loss in take-up occurs before firms apply. Three quarters of eligible firms never apply for the voucher, with a pronounced gradient by firm size: only 13% of the smallest firms apply, compared with 52% of firms with 10 to 49 employees. Conditional on receiving the voucher, however, this size gradient disappears: around four in five firms in every size group ultimately have a digital solution in place. At this stage, financial constraints become more salient. More leveraged firms are more likely to withdraw, and withdrawals are concentrated before firms sign a contract with a provider, which commits them to advancing the VAT on the full purchase price within a few months. Thus, while the voucher eliminates the direct cost of the subsidised solution, it does not eliminate the liquidity required to complete the transaction, a friction that is particularly binding for smaller firms.[1]

Second, when the voucher fully covers the firm's expenditure up to a solution-specific

[1]The self-diagnosis required before application, which could also be completed by firms that ultimately did not apply, provides suggestive evidence on the sources of the initial participation gap. Nearly three quarters of the size gradient in applications is already present in completion of the self-diagnosis, while firms that learned about the programme through a recommendation are 6 percentage points more likely to apply than those that reached it through the programme website.

cap, prices bunch at that cap. Nearly three quarters of contracts are priced exactly at the maximum reimbursable amount, despite competition among almost 10,000 accredited providers. The 2024 increase in the voucher provides particularly clear evidence of this pricing response. Among firms that returned to the programme, virtually all of the additional €1,000 was spent.

A similar pattern emerges in the allocation of expenditure across solutions. Formally, firms choose both the provider and the solutions they purchase, and each contract requires the firm's approval. Yet, because solutions are fully subsidised up to their respective caps, firms face little incentive to trade off their costs, while providers may have substantial influence over which solutions are contracted. Consistent with this mechanism, provider identity explains up to 49 percentage points of the variation in the solutions firms purchase. By comparison, firms' balance-sheet characteristics explain at most 3.4 percentage points, and their pre-programme technology use, measured through the self-diagnosis, explains at most 1.5 percentage points. Thus, firms contracting with the same provider tend to purchase similar bundles of solutions, largely irrespective of the technologies they were already using. We refer to this pattern as *the seller's choice*. The term does not imply that providers formally choose on behalf of firms. Rather, it captures the empirical finding that, under a fully subsidised voucher, the technologies firms adopt are much more closely associated with the identity of their provider than with their observable characteristics or prior technology use. This pattern may arise because providers steer firms toward particular solutions or because firms delegate part of the choice to their provider.

Third, adoption increased productivity among adopting firms. Using later adopters as the comparison group, we estimate that firms implementing a solution in 2023 experienced a 0.9% increase in total factor productivity (TFP) over the following two years. The gains are concentrated among technologies that facilitate customer acquisition and market access, including websites, social media, and e-commerce. Firms adopting only these solutions experienced a 1.8% increase in TFP, whereas we find no productivity gains among firms

adopting only cost-reducing technologies. These differences are indicative, as we cannot reject equal effects across bundles.

We find little evidence that productivity gains increase systematically with firms' pre-existing capabilities, as complementarities with organisational capital would predict (Brynjolfsson and Hitt, 2000). Returns are larger for firms with less intangible capital but also for those with greater prior digital maturity, and they are no larger when the adopted technology is new to the firm.

Fourth, part of the subsidy accrues to providers. For providers, the programme generates a demand shock at prices largely determined by the voucher caps. Each euro billed through the programme is associated with €0.76 in additional provider output, suggesting that roughly one quarter of subsidised expenditure displaces sales that would otherwise have occurred. Providers retain €0.22 per euro billed as surplus; relative to the additional output, this is almost three times their pre-programme surplus of €0.098 per euro of output. Combining both sides of the transaction, each euro of voucher expenditure is associated with €1.47 in additional value added for the recipient firm over the following two years, while generating approximately €0.15 in additional surplus for the provider relative to what the same sales would have yielded at its pre-programme margin.

These findings have direct implications for the design of technology-adoption subsidies. First, eliminating the direct cost of adoption is not sufficient to generate broad take-up: most of the participation gap arises before application and is concentrated among the smallest firms. Second, even when the technology itself is fully subsidised, upfront payments can create liquidity constraints at the contracting stage. Third, the design of the subsidy also shapes the incentives of providers. When firms bear none of the cost below the reimbursement cap, they have little incentive to negotiate lower prices, and prices consequently concentrate at the cap. Moreover, the technologies firms adopt are much more strongly associated with provider identity than with firms' observable characteristics or prior technology use. Taken together, these results show that effective subsidy design requires addressing barriers to participation

and liquidity while also accounting for the incentives created on the supply side.

Our estimates are subject to several identification and measurement concerns, which we address in detail throughout the paper. First, selection into programme participation precludes a credible comparison between adopters and non-applicants: firms that adopt are larger and more productive than firms that never apply. We therefore restrict the productivity analysis to firms that eventually adopt and exploit differences in the timing of adoption, comparing earlier with later adopters. Our estimates should consequently be interpreted as the effect of adoption among participating firms, rather than as the effect of the programme on firms that would otherwise not participate.

Second, identification requires that, absent earlier adoption, the productivity of early and later adopters would have followed similar trajectories. Their productivity evolves in parallel for eight years before the programme, providing support for this assumption. Nevertheless, the estimated effect is sensitive to relatively modest deviations from post-2022 trends: allowing for such deviations can render the estimate statistically indistinguishable from zero. We therefore interpret the evidence as supporting a small positive productivity effect, while acknowledging the limits to causal identification.

Third, endogenous provider choice complicates the interpretation of the *seller's choice*. Firms may select providers on the basis of the technologies they intend to purchase, generating an association between provider identity and technology choice even in the absence of provider influence. We address this concern in two ways. Firms with different observable needs, measured using balance-sheet characteristics and technologies in use before application, do not systematically sort into different providers. Moreover, conditional on provider, firms tend to purchase the solutions that the provider is supplying at the time of contracting. These patterns are difficult to reconcile with sorting on observable technology needs alone, although they do not rule out selection on unobserved preferences.

Finally, our measure of TFP is revenue-based. The estimated productivity gains may therefore partly capture changes in demand or output prices rather than improvements in

physical efficiency.

Our paper contributes to three strands of the literature. First, we contribute to the literature on take-up of public programmes. A large body of work shows that information frictions, complexity, and administrative costs reduce take-up among households (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019) and firms (Humphries et al., 2020; Zwick, 2021). Evaluations of business-support and technology-adoption programmes, however, typically focus on firms that apply (Bronzini and Iachini, 2014; Cingano et al., 2025; Cornet et al., 2006; RSM UK Consulting LLP, 2023) or estimate average effects across eligible areas (Criscuolo et al., 2019). By observing the universe of eligible firms, we show that most of the loss in take-up occurs before application, even when the subsidised technology is provided at no direct cost to the firm. We also identify a distinct financial friction later in the process: firms are more likely to withdraw when they must commit to the purchase and advance the VAT. This result complements evidence that financing constraints limit firms' investment and innovation (Howell, 2017; Ottonello and Winberry, 2024) by showing that liquidity can constrain adoption even when the underlying investment is fully subsidised.

Second, we contribute to the literature on how suppliers shape the allocation and incidence of third-party payments. Existing work shows that suppliers can influence what buyers receive when they bear little of the cost (Clemens and Gottlieb, 2014; Foà et al., 2019; Gruber and Owings, 1996; Molitor, 2018) and capture part of subsidies through higher prices (Cellini and Goldin, 2014; Collinson and Ganong, 2018; Galloway and Li, 2023). We study both margins for productive inputs purchased by firms. Despite firms formally choosing what to buy, provider identity explains substantially more of their technology choices than their observable characteristics or prior technology use. Prices also concentrate at the reimbursement cap despite competition among nearly 10,000 providers, consistent with firms having little incentive to seek lower prices when they bear none of the cost below the cap. Linking these transactions to providers' accounts further allows us to quantify how subsidised expenditure translates into provider output and surplus.

Third, we contribute to the literature on technology adoption and its returns. Existing work typically models adoption as a decision of the firm (Atkin et al., 2017; Bloom et al., 2013) and emphasises complementarities between technology and organisational capabilities in determining its returns (Bloom et al., 2012; Bresnahan et al., 2002; Brynjolfsson and Hitt, 2000; DeStefano et al., 2023). Our results introduce the supply side into this margin of technology adoption. Under a voucher delivered through private providers, the technologies firms adopt are more strongly associated with provider identity than with their observable needs or prior technology use. At the same time, the average productivity gains are modest and concentrated among customer-facing technologies, and we find no systematic evidence that returns increase with firms' pre-existing organisational or digital capabilities.

The rest of the paper is organised as follows. Section 2 describes the programme and its institutional features, and Section 3 presents the data. Sections 4–7 examine programme take-up, technology choice, productivity effects, and provider outcomes. Section 8 concludes.

2 The programme

Origin and size. Kit Digital was established under Spain's Plan de Recuperación, Transformación y Resiliencia (PRTR), financed by the European Union's NextGenerationEU instrument. The programme is managed by Red.es, a public entity attached to the Spanish government and responsible for implementing digitalisation policies.

The programme was launched in February 2022 and closed its application phase in October 2025, with a total budget of €3.067 billion. Over its lifetime, the programme processed more than 1.2 million applications and granted over 860,000 digitalisation vouchers to Spanish SMEs and self-employed workers, representing the largest digitalisation initiative ever implemented in Spain. By the end of the programme, more than 500,000 projects had been completed, and coverage reached approximately 7,500 municipalities, accounting for around 92 percent of the national territory.[2]

[2]Programme figures are from the Red.es Kit Digital portal (Red.es, 2026).

Eligibility and vouchers. Eligibility for the programme was open to small and medium-sized enterprises (SMEs), microenterprises, and self-employed workers operating in Spain. Firms were required to have their fiscal domicile in Spain, be economically active and registered in the relevant tax census, and comply with standard administrative conditions for receiving public subsidies. In particular, beneficiaries had to be current with their tax and social security obligations, not be classified as firms in crisis under European state aid rules, and not be subject to recovery orders or other legal exclusions from public support. In addition, firms were typically required to meet a minimum period of economic activity, as specified in each call, and to complete a digital maturity self-diagnosis prior to applying.

Access to the programme was organised through a system of application calls targeting different firm segments defined by employment size. The programme initially covered firms with up to 49 employees and was subsequently expanded to include larger SMEs with up to 249 employees. Firms were classified into five segments based on employment size, each associated with a different maximum voucher amount. Segment III (0–2 employees, including self-employed) was initially eligible for a voucher of up to €2,000, later increased to €3,000. Segment II (3–9 employees) received up to €6,000, while Segment I (10–49 employees) could obtain up to €12,000. In later phases, the programme was extended to larger firms: Segment IV (50–99 employees) was eligible for up to €25,000, and Segment V (100–249 employees) for up to €29,000.

From the voucher to a solution in service. The implementation followed a two-stage design. In the first stage, firms applied online through the Red.es platform, were screened for eligibility, and, if approved, received a digital voucher corresponding to their size segment. In the second stage, beneficiaries selected an accredited digital solution provider (*Agente Digitalizador*) from a publicly maintained catalogue and signed a formal implementation agreement.

The voucher could only be redeemed through these providers for the purchase of predefined

digital solutions. Payments were made directly by the programme administrator to the provider upon verification of implementation, as firms assigned their right to the subsidy to the provider. As a result, firms never directly handled the voucher funds.

This design implies that the decision to apply for and obtain the voucher is distinct from the decision to execute it, which requires entering into an agreement with a provider and completing the implementation process.

The catalogue. The programme defined a fixed catalogue of eligible digitalisation categories that beneficiaries could adopt through accredited providers. In its initial design, the catalogue comprised ten broad solution areas: (i) website development and online presence, (ii) e-commerce, (iii) social media management, (iv) customer relationship management (CRM), (v) business intelligence and analytics, (vi) process management (ERP-type solutions), (vii) electronic invoicing, (viii) virtual office and collaboration tools, (ix) secure communications, and (x) cybersecurity.

All segments had access to this common set of solution categories, but the maximum subsidised amount and scale of implementation varied by firm size. Within each category, the programme established detailed functional and service requirements, including minimum technical specifications, support obligations, and implementation timelines. Solutions were selected from a centralised catalogue of accredited providers (*Agentes Digitalizadores*) and had to meet predefined standards to qualify for subsidy support.

Firms could allocate their voucher across one or multiple categories, subject to the overall ceiling associated with their segment. This design provided firms with flexibility to tailor digital adoption to their specific needs, while maintaining standardisation in the types and quality of solutions subsidised under the programme.

Payments. Signing a contract transfers the firm's right to collect the subsidy to the provider, which is paid by the administrator once each phase is justified. The firm therefore never advances the subsidised amount. The subsidy for each solution is capped by segment,

so the cap is the highest price that the voucher fully covers, and the firm pays any difference above it. Moreover, the firm pays the VAT on the full price when the provider invoices the first phase, within a few months of signing, and recovers it later through its VAT returns (Appendix E).

Providers. Providers are predominantly small, established firms. Among the 9,773 providers that delivered at least one solution, those with pre-programme balance-sheet data had a median of 4.6 employees and 15 years of activity, and around half operated in information and communication services (Appendix Table 40). The market combines a large number of local providers with a smaller group operating nationally. The 15% of providers active in more than ten provinces, primarily telecommunications companies and online re-sellers, accounted for 79% of all deliveries, most of them to self-employed workers. Provision is more local among firms in our estimation sample, which excludes the self-employed and comprises larger firms: half of their vouchers were delivered by a provider located in the same province. Section 7 examines provider characteristics and the effects of the programme on their outcomes.

Incentives and empirical predictions. Before turning to the empirical analysis, we summarise the incentives implied by the programme's payment rules. Below the reimbursement cap, a lower price reduces public expenditure but does not reduce the amount paid by the firm. Signing therefore entails only one direct financial cost for the firm: financing the VAT between the invoice and its recovery. Moreover, conditional on receiving the voucher, application costs are sunk. The simple framework below formalises these incentives and derives predictions for take-up, pricing, and technology choice.

Consider firm i, with R_i remaining on its voucher. The firm can contract with provider a for solution s, with content q and price p, subject to a solution- and segment-specific reimbursement cap $\bar{p}_s$. By signing, the firm commits to paying the VAT, τp, which it recovers subsequently, as well as any expenditure not covered by the voucher, $\max\{0,\, p - \min(\bar{p}_s, R_i)\}$.

The firm contracts if

$$v_i(q) - \max\{0,\, p - \min(\bar{p}_s, R_i)\} - r_i\,\tau p - \kappa_i \geq 0, \tag{1}$$

where $v_i(q)$ denotes the value of the solution to the firm, r_i its cost of financing the VAT until recovery, and κ_i the cost of finding a provider and completing the transaction. Prior to this stage, applying for the voucher entails a fixed cost F_i, which does not vary with voucher size. Provider a earns $p - c_a(q)$, where $c_a(q)$ is the cost of supplying content q.

The framework yields several empirical predictions. First, below the reimbursement cap, price affects the firm only through the financing cost of VAT. Firms therefore have little incentive to seek lower prices within this range, whereas providers benefit from higher prices. We consequently expect prices to bunch at the reimbursement cap or at the firm's remaining voucher balance, and increases in voucher generosity to translate into additional expenditure (Section 5).

Second, full reimbursement also weakens the link between technology choice and price from the firm's perspective. Conditional on satisfying the firm's participation constraint, providers have incentives to supply solutions that generate higher margins. Technology choice may therefore reflect provider-specific incentives as well as firm needs, implying a stronger role for provider identity in explaining what firms purchase (Section 5).

Third, because application costs are incurred before the voucher is granted and need not increase with firm size, while voucher generosity does, fixed participation costs should generate a size gradient in application. Once the voucher has been granted and these costs are sunk, this gradient should weaken substantially at the implementation stage (Section 4).

Fourth, firms with a higher cost of external funds should be more likely to withdraw when the transaction requires them to advance the VAT. This predicts attrition specifically at the contracting stage, with stronger effects when the VAT payment is large relative to the firm's financial resources (Section 4).

Finally, purchases above the reimbursement cap require firms to bear the marginal cost

directly. Co-payment should therefore occur only when the incremental value of the solution exceeds this additional expenditure, making it less common than purchases fully covered by the voucher (Section 5).

These predictions are not unique to the mechanisms captured by the framework. In particular, a size gradient in applications could also arise from differences in information or awareness rather than fixed participation costs. The empirical analysis therefore uses additional evidence to distinguish among these mechanisms.

3 Data

We combine administrative records of the programme with the balance sheets of Spanish firms, linking them through the firm's tax identifier. The programme records come from Red.es, which provided every application and grant, and the 1,523,006 contracts signed in segments I to III. For each contract, we observe the provider, the solution, the price, the subsidy, its status, and the dates of signature and entry into service. Red.es also provided the self-diagnosis of digital maturity that every firm had to complete before applying, 1.8 million tests in all. The test is particularly informative because it records what the firm already had before the programme: whether it used an ERP or a CRM, had a social media profile and used it to advertise, sold online, and had ICT security measures in place. The test also asked how the firm had heard of the programme. Moreover, since the test was open to any firm, it was also completed by some firms that never applied, which allows us to identify firms that were aware of the programme but did not participate. We link the tests to the balance sheets through the tax identifier of every firm, not only of those that signed a contract.

The balance sheets and income statements come from the Central de Balances Integrada of the Banco de España for the period 2014 to 2024. This database covers incorporated firms, but not the self-employed. As a result, we observe the firms that never applied as well as those that did, and we also observe the accounts of the providers that are companies.

Each part of the analysis uses the sample best suited to the question it addresses. To study take-up, we follow the 542,383 companies with accounts in 2021 and up to 49 employees, all of which could apply. Of these, 325,339 also filed accounts in 2019, before COVID, which allows us to relate take-up to the firm's position before the pandemic. To study prices, we use the 999,074 valid contracts for solutions with a single cap. To estimate the effect on productivity, we use a balanced panel of firms that all applied and put a solution into service, observed without gaps from 2014 to 2024: 30,863 firms that first did so in 2023 and 19,124 that did so later. Within this panel, we also study what firms bought, since almost all of their vouchers come with a self-diagnosis.[3] Finally, the providers with accounts from 2019 to 2021 form the panel of Section 7. Throughout, we exclude the sectors in which production functions are unreliable, namely finance, public administration, education, health and social services, and households.

Measuring productivity. We measure total factor productivity as the part of a firm's value added that is not explained by its labour and capital. In particular, we estimate a value-added production function separately for each 2-digit sector,

$$va_{it} = \beta_l \, \ell_{it} + \beta_k \, k_{it} + \omega_{it} + \varepsilon_{it}, \tag{2}$$

where va is log real value added, ℓ the log real wage bill, k log real fixed assets and ω log TFP. We use the control-function approach of Olley and Pakes (1996) and Levinsohn and Petrin (2003), in the one-step GMM implementation of Wooldridge (2009), with intermediate inputs as the proxy. Monetary variables are deflated with sector-level prices rather than the firm's own prices, so TFP is measured from revenue. Price increases, or higher sales with the same inputs, are therefore recorded as higher TFP. In the estimation sample, TFP is set to missing when a firm reports zero employees but positive wages, and it is winsorised at the

[3] Of the 29,892 vouchers with a provider of 20 or more clients, 29,890 have a self-diagnosis.

1st and 99th percentiles by year and 2-digit sector.[4]

4 Take-up

A voucher that covers the full price removes the cost of purchasing the technology, but not the costs of applying for the voucher, choosing a provider and committing to the purchase. This section follows every eligible firm through each of these stages and documents where adoption is lost. We say that a firm adopts, or implements, when it puts at least one solution into service. Our universe is the 542,383 companies with accounts in 2021 and up to 49 employees, all of which could apply. The self-employed, who received most of the vouchers put into service, are not covered by these data.

Take-up by stage. Almost all of the loss in take-up occurs before application (Figure 1). Three in four eligible firms (74.3%) never apply. Subsequent stages account for much smaller losses: about four in five applicants are granted the voucher, and about four in five of those granted put at least one solution into service (Table **??**). As a result, only 16.9% of all eligible firms end up adopting.

These figures can be compared with those of a programme in which firms paid part of the price. Help to Grow: Digital, the British voucher that covered half the price of software, received 1,507 applications against a target of 100,000, and only 60% of its vouchers were redeemed (RSM UK Consulting LLP, 2023). Although the two programmes differ in several respects, the comparison suggests that covering the full price is associated with much higher redemption, but not with application by most eligible firms.

Application rates increase steeply with firm size, but implementation rates do not. Among firms with up to two employees, only 13.0% apply, against 51.8% of those with 10 to 49 employees. Once the voucher is granted, however, firms of all sizes put solutions into service

[4]The percentiles are computed on the cohorts of 2023, 2024, and 2025 and 2026.

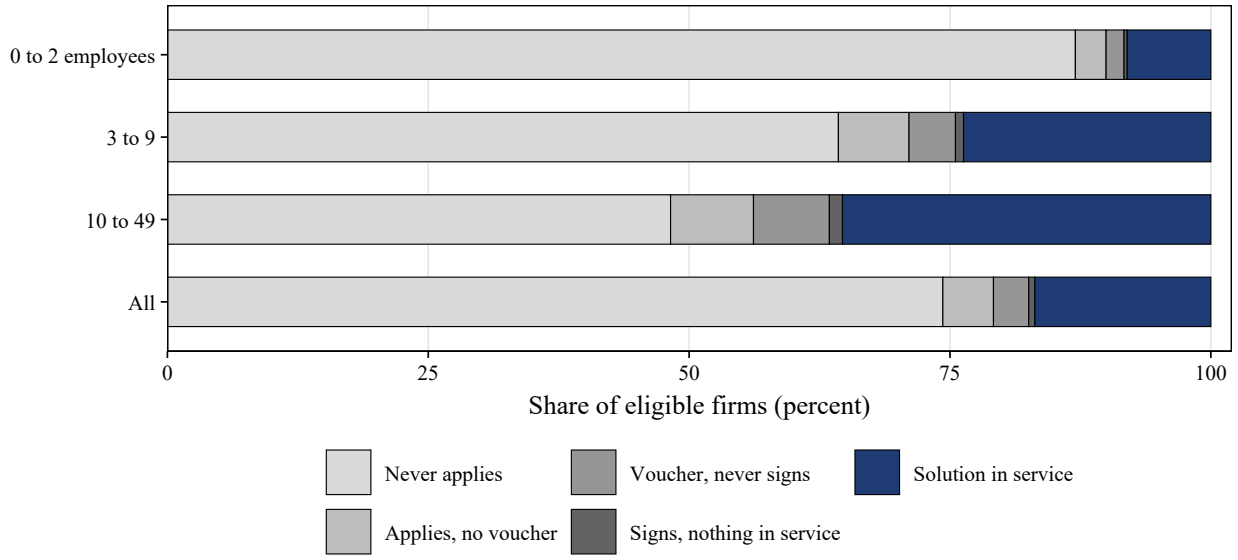


Figure 1: Take up

Notes: Corporations with accounts in 2021 (positive assets) and 0 to 49 employees in 2021, by employment in 2021. Each bar splits firms by the last step they reach: never apply; apply and are not granted the voucher; are granted it and never sign a contract with a provider; sign and put nothing into service; put at least one solution into service. Rates in Table **??**.

at almost the same rate, around 80%.[5] This pattern is consistent with a fixed cost of applying (Section 2), which deters small firms, for which the voucher is smaller, but no longer matters once it has been incurred. Moreover, most granted firms that do not implement drop out at an early stage. Of the 4.0% of eligible firms that obtain a voucher and implement nothing, 3.4 points never sign a contract with a provider.

Firms that knew of the programme. The self-diagnosis provides initial evidence on why so many firms never apply. Every applicant had to complete it, but the test was open to any firm, and 14.3% of the firms that never applied also took it. These firms were aware of the programme but did not apply. The test therefore splits the first step in two: reaching the programme, which we measure by taking the test, and applying once there. Most of the size gradient arises at the first stage. Only 22.8% of the smallest firms took the test, against 63.0% of those with 10 to 49 employees. Among firms that took it, the gap is much

[5]The rates are 79.9% for firms with up to two employees, 82.0% for those with three to nine and 80.5% for those with 10 to 49.

Table 1: The adoption funnel

	0 to 2 employees	3 to 9	10 to 49	All
Firms	297,494	162,413	82,476	542,383
Apply (%)	13.0	35.7	51.8	25.7
Granted, if applied (%)	77.3	81.0	84.7	81.1
Sign with a provider, if granted (%)	82.9	84.6	83.4	83.8
In service, if granted (%)	79.9	82.0	80.5	81.0
Never apply (% of firms)	87.0	64.3	48.2	74.3
Solution in service (% of firms)	8.0	23.7	35.3	16.9

Notes: Corporations with accounts in 2021 (positive assets) and 0 to 49 employees in 2021, by employment in 2021. Each rate is conditional on the previous step. Granted: the voucher is not withdrawn by the applicant (it may later expire or be renounced). Sign: at least one contract with a provider in segments I to III, in any state. In service: at least one contract justified in phase one or two.

narrower: 56.7% of the smallest applied, against 82.1% of the largest (Appendix Table 31). Almost three quarters of the size gradient in applications is thus already present at the stage of completing the self-diagnosis. This suggests that the smallest firms are lost mainly because the programme does not reach them, rather than because applying is too costly once it does.[6]

The channel through which firms learned about the programme points in the same direction. The test asked each firm how it had learned about the programme, and the most common answer, given by a third of the firms that took it, is that someone recommended it. These firms were also among the most likely to apply. Of those recommended by a third party, 75.8% applied, against 67.6% of those that found the programme's website, a gap of 6 points among comparable firms (Appendix Table 32). The channel, however, matters little once the voucher is granted, since recommended firms put a solution into service only 1.8 points more often. As with size, the differences arise in whether the programme reaches the firm rather than after the grant. We do not know who the third party was: a bank, an accountant or a provider looking for clients. Firms with no debt, which apply 8.9 points less than comparable firms (Table 2), are not less likely to report a recommendation, so their lower take-up does not run through who told them about the programme; the identity of the

[6]The test was also the entry point to other services of the AceleraPyme platform, so some of the non-applicants that took it may not have been considering the voucher. Most of them took it in 2024 and 2025, when the programme was still open (Appendix Table 31).

referrer would separate the intermediaries.

Determinants of applying and implementing. We now ask which characteristics of the firm predict each step. To avoid capturing the effects of the pandemic or of the programme itself, we measure these characteristics in 2019, for the 325,339 firms that filed accounts in that year. In particular, we estimate

$$D_i = Z'_{i,2019}\beta + W'_i\gamma + \delta_{j(i),g(i)} + \varepsilon_i, \tag{3}$$

where D_i equals 100 if firm i applies (or, among applicants, implements), $Z_{i,2019}$ contains its leverage, the short-term share of its debt, an indicator of having no debt, its intangible share of assets and its relative wage, all standardised, W_i controls for size, age and TFP, and $\delta_{j,g}$ are fixed effects of sector by voucher segment. Table 2 reports the results.

More productive firms are more likely both to apply and to implement. One standard deviation of TFP raises the probability of applying by 1.9 points, on an average of 34.0, and the probability that an applicant implements by 4.2 points, on an average of 67.9.

The firm's financial position, by contrast, matters only at the implementation stage. Leverage is not associated with a lower probability of applying; if anything, more leveraged firms apply slightly more. Among applicants, however, it is associated with a lower probability of implementing: one standard deviation of it lowers the probability of putting a solution into service by 2.65 points, and a higher short-term share of debt lowers it further.[7] This raises the question of why a leveraged firm would forgo a purchase that is fully subsidised.

Our interpretation is that, while the voucher covers the purchase, it does not finance the commitment that the purchase entails. Signing a contract obliges the firm to pay the VAT on the full price within a few months, before it can recover it, and a firm with limited financial slack may choose not to sign. If this is the relevant friction, two things should

[7] One standard deviation of leverage raises the probability of applying by 0.8 points. One standard deviation of the short-term share of debt lowers the probability of implementing by 1.28 points.

Table 2: Determinants

	Applies	Implements | applies	Co-pays | contracts
Leverage	0.79***	-2.65***	-0.21
Short-term share of debt	0.10	-1.28***	0.15
No debt (0/1)	-8.90***	-5.78***	3.20***
Intangible share	0.62***	-0.50***	0.47***
Relative wage	1.00***	0.55**	1.28***
TFP 2021	1.93***	4.20***	1.35***
Mean of outcome (pp)	34.0	67.9	14.4
Firms	325,339	110,634	75,202

Notes: Linear probability models in% points. Corporations with accounts in 2019 and 2021 and 0 to 49 employees in 2021. Column 1: all firms; column 2: applicants; column 3: firms with valid contracts (co-pays: pays something above the subsidy in some contract). Indicators measured in 2019, all at once, standardised (effect of one standard deviation; no debt is a dummy); missing indicators set to the mean with a missing dummy. Controls: log assets and log employment in 2019 and age in 2019; TFP in 2021, reported in the last row, also enters; fixed effects of 2-digit sector by voucher segment. Stars from standard errors clustered by sector: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

hold (Section 2). First, leverage should stop firms before they sign the contract and not after. Second, its effect should be larger where the VAT weighs more relative to the size of the firm. Both predictions are supported by the data (Appendix Table 33). One standard deviation of leverage raises the probability of failing before signing by 2.64 points and leaves the probability of failing after signing unchanged. Moreover, the effect more than doubles from the largest firms in segment I (1.62 points) to the smallest firms in segment III (3.59 points).

Two alternative explanations could produce a similar pattern. The first is that leveraged firms were about to shrink or close, and failed to implement for that reason. However, the effect barely changes when we control for the growth of TFP and sales between 2019 and 2021, or when we keep only the firms that survived and still filed accounts in 2023 and 2024.[8] A related concern is that leveraged firms are simply less prepared to use digital tools, and drop out for that reason. The self-diagnosis, completed before the grant, allows us to address this concern: adding its score to the regression leaves the effect of leverage unchanged at 2.6 points. The second alternative is that leverage reflects scarce managerial time rather than

[8]The effect is 2.62 points with the growth controls and 2.43 points among surviving firms.

a lack of liquidity; credit data at the firm level, from the Central Credit Register, would allow us to separate the two. With this caveat in mind, the evidence suggests that a financial friction binds at the moment of signing, even when the purchase itself is paid for. This is in line with the evidence that financing constraints hold back investment in innovation (Howell, 2017; Ottonello and Winberry, 2024).

Withdrawals by provider. Once the voucher is granted, implementation also depends on the provider. To identify providers that performed unusually badly, we compare the share of each provider's contracts that were withdrawn with the share we would expect given the solutions, segments, provinces and quarters it served, measured on all other providers (Appendix Table 34). Twenty-two providers, with 15,486 clients, stand out. On average, 32.0% of their contracts were withdrawn, against 6.9% expected.

Clients of these providers rarely turned to another provider. Of those whose contract was withdrawn, 88.4% never signed the same solution with a different provider. This share is close to the 92.6% observed among clients of other providers who withdrew a contract, so a withdrawn solution is rarely replaced, regardless of the provider involved. The loss is nonetheless partial: 28.6% of these clients ended up with no solution in service, against 46.8% of the clients who withdrew a contract with any other provider. These providers hold 1.8% of all contracts; the exit of providers from the programme would extend the test to a larger set of failures.

In sum, most of the loss in take-up occurs before application, and among applicants, more indebted firms are more likely to withdraw before signing. Section 5 examines what the firms that do sign purchase, and at what price.

5 Prices and choice of solutions

Once a firm has signed, it purchases from a provider that is paid by the administrator, and below the cap the firm bears none of the price. The framework in Section 2 yields two

predictions for this setting. First, prices should concentrate at the cap. Second, the identity of the provider should play a large role in what is purchased.

Prices. A firm that pays nothing below the cap has no reason to bargain over the price, since any discount would save money for the administrator and not for the firm. If providers anticipate this, prices should cluster exactly at the cap, or at whatever the firm has left of its voucher when that is lower. Figure 2 shows that this is the case. Of the 999,074 valid contracts we analyse, almost three in four (72.1%) are priced exactly at the cap for their solution and segment, and another 8.2% exactly at what the firm had left. Only 3.1% are priced above the cap, which is the only case in which the firm pays something out of its own pocket (Table 3). Firms thus treat the voucher as a budget to be exhausted rather than as a discount on a price: seven in ten vouchers end up spent in full. The pattern is strongest among the smallest firms, and it also holds among the firms used to estimate productivity effects in Section 6.[9]

Table 3: Contract Prices and the Voucher Cap

	Segment I (10 to 49)	Segment II (3 to 9)	Segment III (0 to 2)	All
At the cap (%)	64.6	64.1	76.2	72.1
At the remaining voucher (%)	3.4	7.2	9.4	8.2
Below both (%)	23.3	24.4	12.7	16.6
Above (firm pays) (%)	8.7	4.2	1.6	3.1
Contracts	123,936	222,165	652,973	999,074

Notes: Valid contracts of segments I to III for solutions with a single cap per contract (excluding solutions paid per user or device and the secure workstation). At the cap: price within one euro of the maximum subsidy for the solution and segment, when the remaining voucher covers it. At the remaining voucher: price within one euro of what the firm had left, when that is below the cap. Remaining voucher: the voucher minus the subsidy of the firm's earlier valid contracts.

There is, however, an alternative reading of this evidence. A price at the cap could simply reflect what the solution costs to deliver, either because the cap was set at cost or because providers fill the voucher with more content rather than charging more for the same content.

[9] In segment III, 76.2% of contracts are priced at the cap. Among the treated firms of Section 6, 62.1% are.

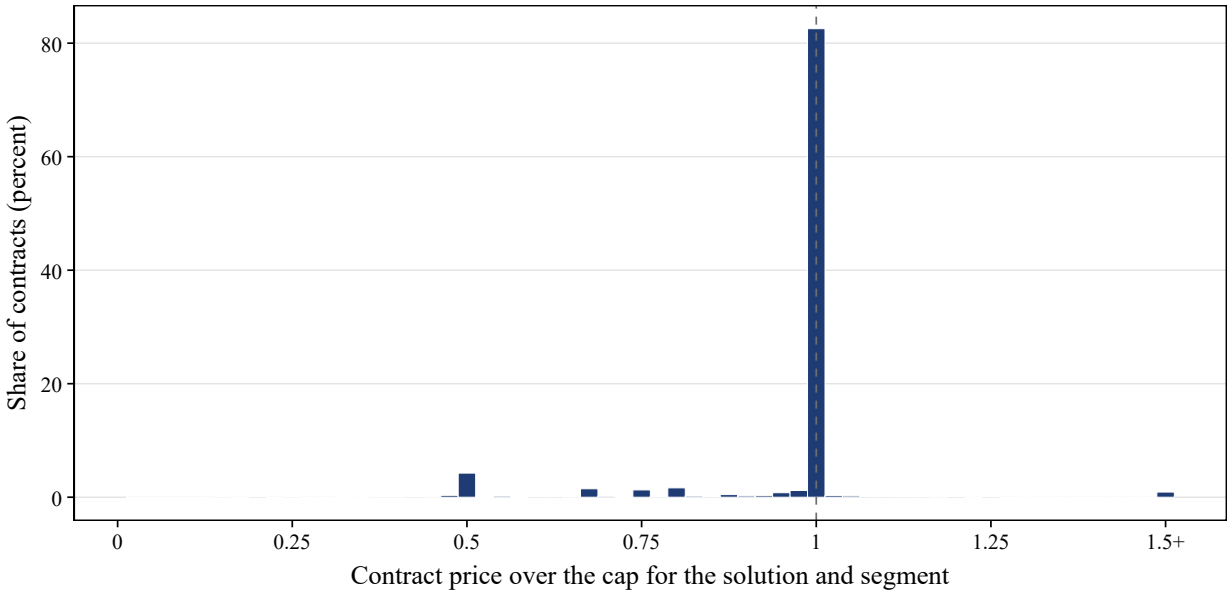


Figure 2: Contract prices bunch exactly at the cap

Notes: Valid contracts of segments I to III for solutions with a single cap per contract, in which the cap and not the remaining voucher is the binding limit. The spike at 1 is the contracts priced exactly at the cap; Table 3 gives the shares. Price over the maximum subsidy for the solution and segment (Orden ETD/1498/2021 and its amendments). Bins of 0.025; the last bin collects prices 1.5 times the cap or more.

We address this concern in three ways.

First, bunching at the cap does not reflect a preference for round numbers. Where the cap is €2,000, 84.2% of contracts cost exactly €2,000; where the cap is higher, only 14.2% do. Second, we compare the same provider selling the same solution in segments with different caps,

$$\ln p_c = \beta \ln \bar{p}_{s(c),\, g(c)} + \mu_{a(c),\, s(c)} + \varepsilon_c, \tag{4}$$

where p_c is the price of contract c, $\bar{p}_{s,g}$ the cap for solution s in segment g, and $\mu_{a,s}$ a fixed effect for each provider and solution. If the same provider sells the same solution at a higher price wherever the cap is higher, the price follows the cap and not the cost of delivery. We find that the price rises by 0.92% for each 1% increase in the cap, and by 0.83% for each 1% increase in the voucher left when this is what binds (Appendix Table 35).

Third, and most directly, the 2024 rise in the voucher of the smallest firms changed what firms had left to spend without changing what providers offered. In July 2024, the voucher of

segment III went from €2,000 to €3,000. Of the firms that had already spent their €2,000, 39.9% came back, and nearly all of them (99.5%) spent exactly the €1,000 they had left (Table 4). Three in four spent it only on the secure workstation, a device introduced together with the rise and capped at exactly €1,000. Moreover, returning firms paid considerably less than new clients for the same solution. For the same solution, sold by the same provider in the same month, the median returning firm paid €1,000 and the median new firm €2,000. Returning firms almost never added money of their own to get the solution at the price new firms paid.[10] The same provider thus charged half the price for the same solution in the same month, to clients that differed only in their remaining voucher, which is difficult to reconcile with prices that reflect content.

Table 4: Change in the Voucher Cap and Returning Firms

	Returning firms (1,000 euros left)	New firms (3,000 euros)
A. Per voucher, contracts signed from July 2024		
Vouchers	59,580	422,196
Spend exactly 1,000 euros (%)	99.5	1.1
Spend exactly 3,000 euros (%)	0.0	67.2
Buy only the secure workstation (%)	77.3	0.5
Pay above the subsidy in some contract (%)	12.2	14.3
B. Same provider, solution and month (solutions capped above 1,000 euros)		
Contracts	9,338	46,440
Median price (euros)	1,000	2,000
Pay the difference above the subsidy (%)	1.3	0.4
C. Same provider and month: returning minus new (pp)		
Solution that fits in 1,000 euros	20.8***	
Secure workstation	30.9***	

Notes: Valid contracts of segment III signed from 1 July 2024. Returning: the voucher had spent €2,000 before July 2024, so €1,000 were left. New: nothing spent before. Panel B: provider, solution and month cells with both types, solutions with a cap above €1,000. Panel C: linear probability models in percentage points with provider by month fixed effects. Fits in €1,000: cap of €1,000 or less, or a solution paid per user whose official total for segment III is below €1,000. Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

These results contrast with the existing evidence on who captures subsidies. The pass-

[10] Returning firms paid the difference in only 1.3% of their contracts. Within the same provider and month, they were 21 points more likely than new firms to choose a solution that fit in what they had left.

through of a subsidy to prices is usually incomplete, and it is the sellers' market power that allows them to keep part of it. For instance, retail prices fell by 67 cents for each dollar of food subsidy in remote Canadian communities (Galloway and Li, 2023), and for-profit colleges eligible for federal aid charged 78% more than comparable colleges that were not, which is about the value of the aid (Cellini and Goldin, 2014). In Kit Digital, the cap passes almost fully into the price, even with almost 10,000 competing providers. This outcome does not require market power; it only requires buyers who, bearing none of the price below the cap, have little reason to seek a lower one. Consistent with this, firms rarely add money of their own: they pay something above the subsidy in 8.5% of contracts in segment I and 2.1% in segment III. We do not observe what each contract contains, so a price at the cap may still reflect more content for some contracts; a sample of contract specifications would settle it.

Choice of solutions. The second prediction concerns the role of the provider in what is purchased. The firm formally makes every purchase decision: it picks a provider and a contract for each solution, and nothing is delivered without its signature. The question is what explains the content of those contracts. If firms bought what they needed, their characteristics and what they already used should predict what they buy. If instead the firm, paying nothing below the cap, takes what its provider offers, the identity of the provider should predict it. To distinguish between the two, we measure how much of what a firm buys can be explained by the firm itself and how much by its provider. We take the 29,892 vouchers of the firms in our estimation sample whose main provider has at least 20 clients, 2,433 providers in all. For each voucher v of firm i, signed with provider a and granted in cell c of province, segment and month, we estimate

$$y_v = \delta_c + X_i'\gamma + \mu_a + u_v, \tag{5}$$

where y_v is the share of spending on cost tools or on demand tools, the number of channels bought, or whether the voucher is spent in full. We then compare how much the explained

variance increases when we add to the cell effects δ_c either the firm's 2021 accounts and sector, X_i, or the provider's fixed effect, μ_a.

The identity of the provider explains substantially more of what a firm purchases than the firm's own characteristics. The firm's accounts (TFP, assets, debt, age, intangible capital, employment and sector) add at most 3.4 percentage points to the adjusted R^2. The provider adds between 22.6 and 49.1 points (Figure 3 and Appendix Table 36). Moreover, the provider explains almost as much once the firm's accounts are already included, around 46 points for the share of spending on cost or demand tools.[11]

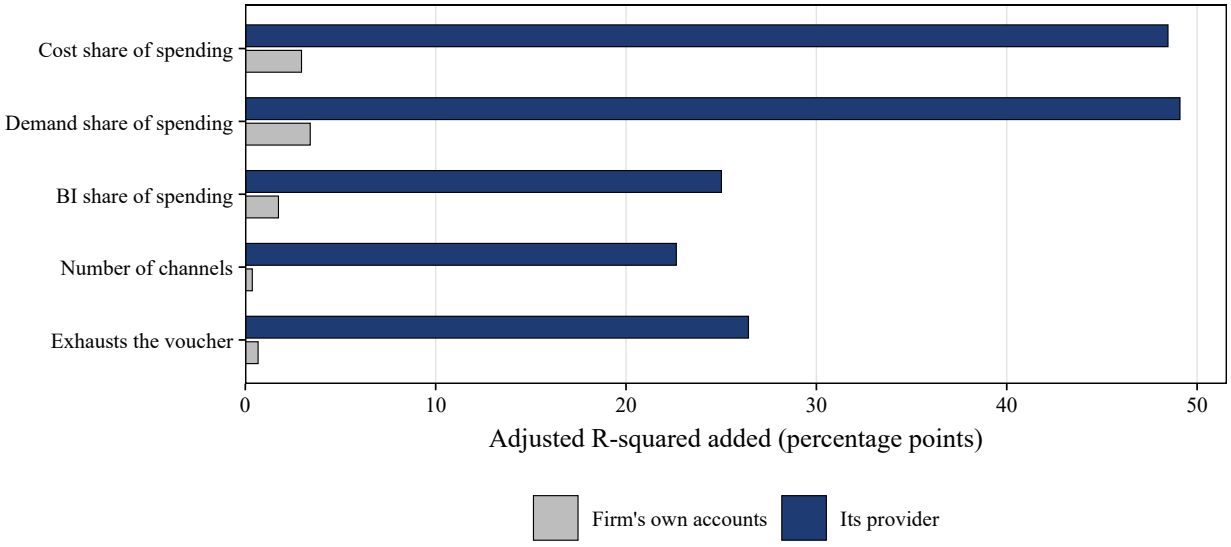


Figure 3: Explaining what Firms Buy

Notes: Vouchers with a solution in service signed before July 2024, of firms in the estimation sample of Section 6, whose main provider (the one paid most) has 20 or more clients. Increase in the adjusted R^2 over fixed effects of province by segment by month of grant, when adding the firm's 2021 accounts and sector, or the provider's fixed effect. Values in Appendix Table 36.

The main concern with this reading is sorting. The provider's fixed effect also captures the kind of firms that choose that provider. If firms with similar needs went to the same specialist, for instance firms that want an online shop going to a provider that specialises in online shops, we would observe the same pattern even if providers played no role in the choice. We use three tests to address this concern.

[11]The accounts add between 0.4 and 3.4 points. After the accounts, the provider adds 45.8 points for the cost share of spending and 46.1 for the demand share.

The first test asks whether firms sort on what their accounts reveal. We replace the provider's fixed effect with what the provider sells to its other clients, $\bar{y}_{a(-v)}$, the average outcome of its clients leaving out firm i:

$$y_v = \beta\, \bar{y}_{a(-v)} + \delta_c + X_i'\gamma + u_v. \tag{6}$$

A provider whose propensity to sell cost tools is one standard deviation higher (29 points) raises the firm's cost share by 26 points, with or without the firm's accounts (Appendix Table 37). If firms sorted to providers according to their characteristics, the cost share that their accounts predict should also rise with the provider's propensity. It rises by only 1.5 points for the same standard deviation.

The second test uses what the firm already had before applying. Accounts may miss a firm's needs, but the self-diagnosis records them directly: whether the firm already used an ERP system, a customer management tool, social media or online sales. Practically all firms in this sample (99.9%) completed it before the voucher was granted. If firms looked for a provider that sells what they lack, their answers should predict what they buy, and the provider's propensity should lose importance once the answers are taken into account. We find neither pattern (Appendix Table 39). The answers add at most 1.5 points to the explained variance of each purchase, against at least 18.9 points for the provider. For example, a firm that already used an ERP system is only 2.2 points less likely to buy process management, whereas one standard deviation of its provider's propensity raises that probability by 34 points. Moreover, the coefficient on the provider's propensity is unchanged when the answers are included.

The third test compares firms that sign with the same provider. Within provider, firms tend to purchase the solutions that the provider is selling to other clients in the month they sign. A provider that sells 10 points more cost tools to its other clients in a given month raises the firm's cost share by 3.7 points, and these monthly swings are unrelated to the firm's

assets or TFP (Appendix Table 38). Since a firm's needs are unlikely to vary with the month in which its provider promotes a product, this pattern is difficult to explain by sorting.

Taken together, these tests indicate that, under a fully funded voucher delivered by private providers, what firms purchase is closely associated with the identity of their provider. This contrasts with the usual view in adoption studies, which treat what to adopt as the firm's decision (Atkin et al., 2017; Bloom et al., 2013). We cannot tell whether providers steer their clients or clients delegate the choice to their providers. In either case, the firm's observable needs are only weakly reflected in what it purchases. Section 6 shows that the bundle a firm buys because of its provider is associated with gains similar to those of the bundles that firms choose, although with less precision.

6 Effect on productivity

Whether adoption raised productivity is a separate question from whether the voucher raised adoption. The previous sections give several reasons for caution: purchases were closely associated with the provider, many firms already used similar tools, and the tools themselves are basic. This section estimates the effect on TFP of putting a first solution into service and examines its robustness to the main threats to the design. It then exploits the differences in what firms bought, and in their finances before the programme, to ask where the gain comes from.

Estimand and identifying assumption. A naive comparison of adopters with firms that never applied would be misleading. As shown in Section 4, more productive firms were more likely to apply, so such a comparison would mix the effect of the technology with the selection of better firms into the programme. For this reason, we only compare firms that all applied, were granted the voucher and put a solution into service, and that differ only in when they did so. If the firms that adopted in 2023 would have followed the same path as those that adopted later, the difference that emerges between the two groups from 2023 onwards is the

effect of the programme.

In particular, the treated group consists of the 30,863 firms whose first solution entered into service in 2023. The control group consists of the 13,575 firms of the 2024 cohort, used only until 2023, and the 5,549 granted firms whose first solution entered into service in 2025 or 2026 (Appendix Table 7). Our target is the average effect on the firms whose first solution entered into service in 2023,

$$\theta = \frac{1}{2} \sum_{t \in \{2023,\, 2024\}} ATT(2023, t), \qquad ATT(2023, t) = E\left[\, Y_{it}(1) - Y_{it}(0) \mid G_i = 2023 \,\right], \quad (7)$$

where Y_{it} is the log TFP of firm i in year t, $Y_{it}(1)$ and $Y_{it}(0)$ are its values with and without the solution in service, and G_i is the year of the firm's first solution in service. In each year, the comparison group is made of the firms not yet treated in that year: the 2024 cohort and the firms treated in 2025 or 2026 for 2023, and only the latter for 2024. This estimand measures the effect on early adopters relative to later ones, and it says nothing about the returns that firms that never applied would have obtained.

The identifying assumption is that, without the programme, early adopters would have followed the same path of TFP from 2022 onwards as later adopters with the same size, age, intangible capital and sector:

$$E\left[\, Y_{it}(0) - Y_{i,2022}(0) \mid X_i,\ G_i = 2023 \,\right] = E\left[\, Y_{it}(0) - Y_{i,2022}(0) \mid X_i,\ G_i > t \,\right], \quad (8)$$

where X_i contains the firm's asset quartile, age, intangible share of assets and sector, all measured in 2022. We need to condition on these characteristics because the timing of adoption is not random. Larger firms adopted earlier: 31 % of treated firms are in the top quartile of assets, against 23 % of controls. The intangible share captures the intangible capital that the firm had already accumulated, and sector absorbs the different paths of TFP across industries after 2022. All these variables are measured in 2022, the year before treatment, so that the programme cannot have affected them.

We estimate each $ATT(2023, t)$ with the doubly robust estimator of Sant'Anna and Zhao (2020). This estimator combines two adjustments: it reweights the controls so that they resemble the treated in their characteristics, and it models the TFP growth of each firm from those characteristics. The estimate is consistent if either of the two models is right. We aggregate the yearly comparisons as in Callaway and Sant'Anna (2021), comparing each year from 2014 to 2024 with 2022. Treated and control firms overlap over the whole range of the propensity score, so no firm needs to be dropped. The years before 2022 allow us to test equation (8) where it can be tested. The assumption would fail if early adopters were on a steeper path after 2022 for reasons other than the programme, for instance if larger firms grew faster after 2022 whatever they bought. Appendix A gives the details of the estimation and the simple comparisons behind the estimator.

Average effect and robustness. Figure 4 summarises the main result of the section. Before the programme, the productivity of early and late adopters moved in parallel for eight years, including through the sharp fall of 2020; taken together, the differences in those years cannot be told apart from zero. After their first solution entered into service, the TFP of early adopters began to rise relative to that of later adopters. Their TFP grew 0.5 % more than that of late adopters in the first year, and 1.3 % more in the second. On average, adoption thus raised productivity by 0.9 % over two years. The effect is also visible in the simple two-period comparison: from 2022 to 2023, the TFP of treated firms grew 0.54 percentage points more than that of controls (Appendix Table 8).

The results are not sensitive to the way in which the controls are adjusted. Without covariates, the estimate is 0.88 %, and the alternative estimators trace the same path year by year (Appendix Figure 10). When firms are weighted by their value added in 2022, the first-year effect rises from 0.5 to 0.95 %, which suggests that the gain is somewhat larger among the firms that produce most.

Eight years of parallel pre-programme trends are reassuring, but they do not rule out a

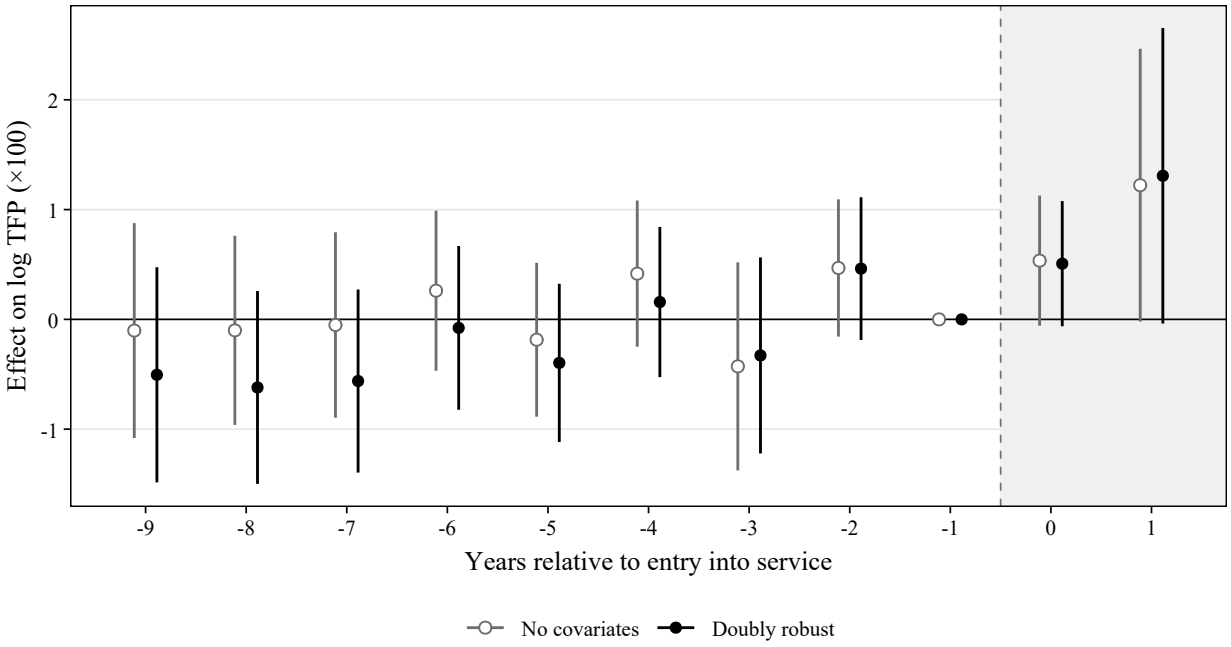


Figure 4: TFP rises after entry into service, with no difference before 2022

Notes: Effect on log TFP (×100) of the 2023 cohort relative to firms not yet treated, by year relative to the first solution in service; base year 2022. Hollow: no covariates. Filled: doubly robust, with asset quartile, age, intangible share and sector in 2022. Bars: 95 % confidence intervals, wild bootstrap clustered by firm. Shaded area: years after entry into service. Estimates in Appendix Table 14.

break after 2022, and the estimate is sensitive to such a break. Following Rambachan and Roth (2023), we allow the deviation from parallel trends after 2022 to be a multiple of the largest deviation observed between consecutive years before 2022. If no deviation is allowed, the effect is clearly positive. If the deviation can reach half the largest one observed before 2022, the effect can no longer be distinguished from zero (Appendix Table 15). Since the years before 2022 cannot rule out a break of that size, the data support a small positive effect.

One plausible source of such a break is debt. As shown in Section 4, leverage delays implementation, so the controls, which implemented later, could differ from the treated in their debt and in what debt does to their growth. In 2022, they were in fact slightly less indebted than the treated firms.[12] When we also balance the two groups on their 2022 debt ratio, the estimate falls to 0.67 %, with the same path before 2022 and less precision (Appendix Table 18). We therefore report both figures: 0.9 % under the main specification

[12]The debt ratio over assets was 0.156 for the controls and 0.177 for the treated.

and 0.7 % when the groups are also balanced on debt.

A second candidate is digital maturity. Treated firms were more digital before the programme: they scored 5.3 out of 12 in the self-diagnosis, against 4.7 for the controls. If firms that were already more digital grew faster after 2022 for that reason alone, the estimate would be biased upwards. Balancing the two groups on the score, however, leaves the estimate at 1.0 %, with no sign of different paths before 2022 (Appendix Table 19). One caveat is that most controls took the test when they applied, after 2022, so their score may already reflect part of what happened in 2023. Treated firms are larger, so part of the effect could still reflect faster growth of larger firms after 2022; comparing firms only within their voucher segment would bound that part.

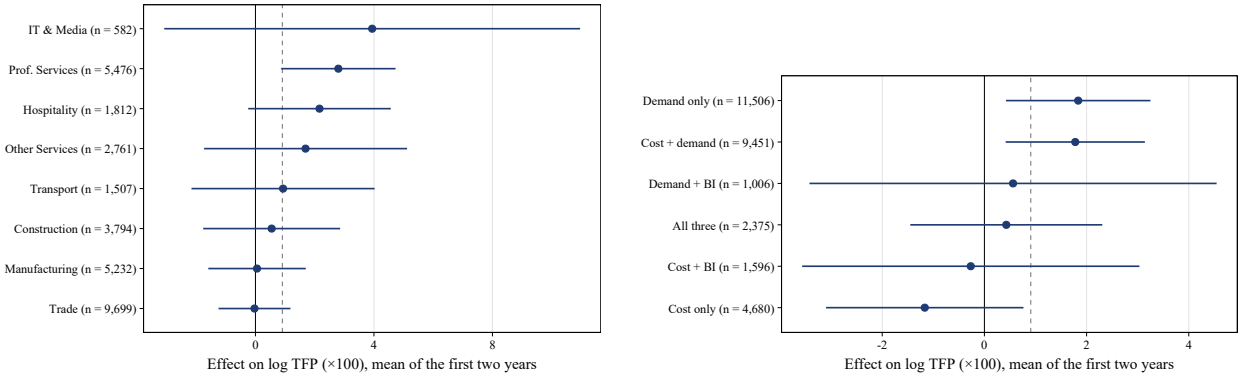
A last caveat concerns what our measure of productivity captures. TFP is computed from value added deflated with sector prices, not with the firm's own prices. As a result, part of a gain that comes with tools to reach customers may reflect higher demand or higher prices rather than higher efficiency. Output-based measures cannot separate the two in our setting, because treated and control firms did not follow parallel paths in output before 2022, and neither did they in value added (Appendix Table 18). In the opposite direction, revenue misses part of what these tools deliver to customers, such as speed and convenience of service (Brynjolfsson and Hitt, 2000).

Overall, the effect is modest. By way of comparison, a fully funded package of digital tools raised TFP by less than 1 % after two years, whereas the first year of intensive management consulting in Bloom et al. (2013) raised it by 17 %. However, returns to information technology often take time to show, because the complementary changes in organisation take time (Brynjolfsson and Hitt, 2003; Brynjolfsson et al., 2021), and two years is all the balance sheets allow so far. The 2025 accounts will add a third year after entry into service.

Heterogeneity by sector and type of solution. The average effect masks heterogeneity along two dimensions, sector and type of tool (Figure 5). A key question is whether these

differences reflect the tools themselves or the firms that purchased them.

By sector, the gain is concentrated in professional services (2.8 %) and hospitality (2.2 %). In contrast, trade and manufacturing, which together account for almost half of treated firms, show no effect. Professional services alone account for more than half of the average effect.[13] By type of tool, the gain is associated with demand tools, those that help the firm reach customers: websites, social media, online sales and customer management. Firms that put only such tools into service gain 1.8 %, as do firms that combine them with cost tools. In contrast, firms that bought only cost tools (process management, electronic invoicing or cybersecurity) show a loss of 1.2 %. These differences should be read as indicative, since neither split rejects equal effects across groups.



(a) Sector (2022)

(b) Bundle of channels put into service

Figure 5: Sector and Bundle Heterogeneity

Notes: Main specification estimated separately in each group, with treated and control firms of the same group. Effect on log TFP (×100), mean of 2023 and 2024; 95 % confidence intervals (analytical). n: treated firms. Dashed line: average effect. Demand: website, online presence, online shop, marketplace, social media, customer management. Cost: process management, electronic invoicing, virtual office, secure communications, cybersecurity. BI: business intelligence and analytics. Estimates in Appendix Tables 20 and 21.

Since each firm chose its bundle, the estimated effect of a bundle is the effect on the firms that chose it. We use two tests to ask whether the gap belongs to the tools or to the firms.

The first test looks at size. If small firms gain more for other reasons and also buy different bundles, the gap between bundles could simply be a gap between small and large firms. However, the gap survives when we compare firms within the same voucher segment

[13] They contribute 0.50 of the 0.91 points of the average effect.

(Table 5). Of the 2.7 point gap between firms that bought only demand tools and those that bought only cost tools, only 0.6 points are explained by their different size mix, and 2.1 points remain within segments. Moreover, the estimate for cost tools is negative in all three segments. One caveat is that the gap within segments comes almost entirely from the smallest firms, where demand tools yield 4.4 %, against 0.9 % among firms with three to nine employees. In addition, the smallest segment is the only one where the years before 2022 reject parallel trends.

Table 5: The gap between demand and cost bundles

	Segment I (10–49)	Segment II (3–9)	Segment III (0–2)	Bundle (main)	Bundle, common segment mix
Demand only	-0.0168	0.0085	0.0438***	0.0184**	0.0082
Cost + demand	-0.0002	0.0116	0.0326**	0.0178**	0.0124**
Cost only	-0.0149	-0.0038	-0.0226	-0.0117	-0.0110
All three	0.0093	0.0000	–	0.0043	0.0035
Cost + BI	0.0313*	-0.0426	–	-0.0027	-0.0149
Demand + BI	0.0128	0.0235	–	0.0056	0.0195
Treated in segment	9,207	15,328	6,328		
Demand only − cost only: 0.0267 = segment composition 0.0057 + within segment 0.0210.					

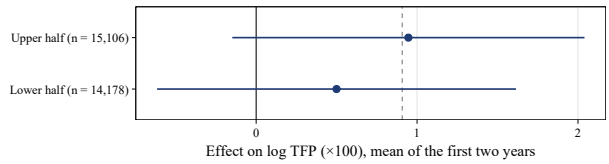
Notes: Main specification estimated in each bundle and segment cell, with treated and control firms of the same cell; mean of the 2023 and 2024 effects on log TFP, stars from analytical standard errors, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Missing cells: fewer than 100 treated or control firms. Bundle (main): estimate for the whole bundle (Table 21). Common segment mix: cell estimates weighted by the segment shares of all treated firms. Last row: Oaxaca decomposition of the gap between demand-only and cost-only bundles into segment composition and differences within segment.

The second test uses the part of the bundle that the firm does not choose. As shown in Section 5, the provider moves what the firm buys almost one for one: from the lowest to the highest tercile of the provider's propensity to sell demand tools to its other clients, the demand share of the firm's own spending rises from 22 to 92 %. If the gap reflected which firms choose each bundle rather than what the tools do, the bundle that a firm buys because of its provider should not predict its gain. The point estimates suggest that it does: the productivity gain rises from 0.3 % in the lowest tercile to 1.0 and 1.4 % in the other two (Appendix Table 30). The terciles cannot be told apart statistically, so this test points the same way as the bundles but does not settle the question. Taken together, the two tests favour an explanation based on the tools rather than on the firms that chose them, although

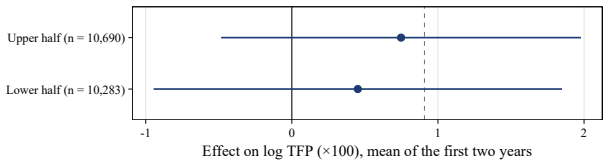
they lack the precision to rule out the latter.

Heterogeneity by financial position and capabilities. Section 4 showed that leverage is associated with lower implementation among applicants. Whether it also limits what adoption returns is a different question, and the two standard explanations for the low digital adoption of small firms give opposite answers. On the one hand, if credit constraints limit adoption, constrained firms should have the largest unexploited returns (Banerjee and Duflo, 2014; de Mel et al., 2008), and a voucher that pays the full price should raise their productivity the most (Howell, 2017). On the other hand, if organisational capabilities are the binding constraint, the gain should be largest where intangible capital and skilled workers were already in place (Bresnahan et al., 2002; Brynjolfsson and Hitt, 2000).

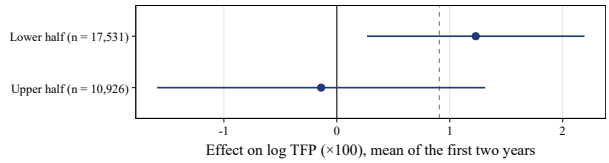
To distinguish between them, we measure each with two indicators from 2019, before COVID and the programme. Financial position is measured by debt over assets and the short-term share of debt, and capabilities by the intangible share of assets and the average wage. Each indicator splits firms at the median of their sector, and we estimate the main specification separately in each half, with treated and control firms of that half (Figure 6).



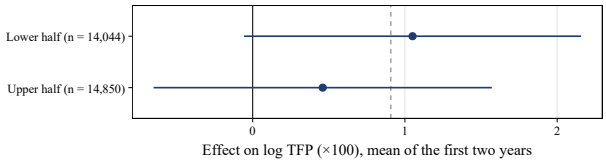
(a) Debt over assets



(b) Short-term share of debt



(c) Intangible share of assets



(d) Average wage

Figure 6: Financial Constraint and Organisational Capabilities Heterogeneity

Notes: Main specification estimated separately in the lower and upper half of each indicator, with the median taken within the 2022 sector. Effect on log TFP (×100), mean of 2023 and 2024; 95 % confidence intervals (analytical). Dashed line: average effect. n: treated firms. Estimates in Appendix Tables 26 to 29.

Neither prediction finds clear support. By financial position, returns barely differ and, if

anything, favour the constrained firms: 0.50 % in the less leveraged half against 0.95 % in the more leveraged one, with the same ordering by the short-term share of debt. By capabilities, returns move in the opposite direction to that predicted by complementarity. Firms with a low intangible share gain 1.23 %, while those with a high share gain nothing, and firms with low wages also gain more than those with high wages. However, none of these differences is large relative to its sampling error.

The self-diagnosis gives a more direct measure of digital capabilities, and it yields a different pattern. Firms in the upper half of the score within their sector gain 1.4 %, against 0.7 % in the lower half (Appendix Table 19). By this measure, the return is, if anything, larger where the firm was already more digital, as complementarity would predict, although the two halves cannot be told apart statistically. The test also allows us to ask whether the gain comes from tools that were new to the firm. A third of treated firms put into service only tools that, according to their own answers, they already used, and another third only tools they lacked. Their gains are similar, 0.6 and 0.5 %, which suggests that the return does not stem from adopting new technologies.[14] One reason may be that the answers are coarse: a firm with an old ERP may still gain from a better one.

These results suggest that the two margins are distinct. Financial position is associated with whether an applicant adopts, but conditional on adoption, leveraged firms gain at least as much as firms with little debt. In turn, the return does not vary in a clear way with the firm's capabilities. It is, if anything, larger where intangible capital was scarce but also where the firm was already more digital, and it is no larger when the tools are new to the firm. From a policy perspective, this suggests that the financial friction operates at the moment of commitment, where covering the remaining payment at signature could be effective, and that there is no evidence of a gap in returns that a larger voucher for constrained firms would close.

[14]For instance, 67.3 % of the treated firms that bought social media management already had a social media profile, and 53.4 % of those that bought process management already used an ERP. Solutions with no matching question in the test, such as the website, do not count.

Value added per euro of voucher. The effect can also be expressed in euros. At the point estimate, the gain exceeds the subsidy. The average treated firm produced about €444,000 of value added in 2022. At that level, the effects of each year imply that a treated firm produced €8,064 more value added over 2023 and 2024 with the same inputs, against an average subsidy of €5,482. Each euro of voucher thus returned 1.47 euros of value added over two years (Appendix Table 16). We cannot do the same calculation with value added directly, because treated and control firms did not follow parallel paths in value added before 2022. The figure ignores any year after 2024 and applies an average effect across firms to a mean dominated by larger ones; the comparison weighted by value added gives a larger first-year effect (Appendix Table 13), so the euro figure is more likely to understate than to overstate.

Section 7 turns to the other side of the transaction: the provider that billed the voucher.

7 Effects on providers

For providers, Kit Digital represented a sudden increase in demand at prices set by the voucher. Between 2022 and 2024, 9,972 providers billed the programme €1,199 million, a figure that reaches €2,290 million once 2025 and 2026 are included (Appendix Table 41). Billing was highly concentrated: the top 1% of providers accounted for a third of the total. For the providers involved, the shock was large. Among the 3,244 providers whose accounts we observe before the programme, the median provider billed the equivalent of 8% of a year's output, and one in ten billed more than a whole year's output. This section examines which firms became providers and how the revenue they billed was allocated.

Selection into the programme. The firms that became providers were already growing. To be accredited, a firm needed a minimum turnover in similar projects before joining, but this requirement was rarely binding.[15] We therefore look at the 13,427 firms that met the

[15]The requirement was €50,000 of turnover in similar projects in the year before joining, or €100,000 in the two years before. Of the providers with accounts in the year before joining, 98% met it with that year's output alone.

threshold in the 17 industries where providers concentrate, of which 18.1% joined, and ask what distinguished them (Appendix Table 42). The firms that joined had grown faster from 2016 to 2019 and again from 2019 to 2021, were more labour intensive and more leveraged, and were, if anything, less productive. Location, however, was the strongest predictor. A province whose expected demand for vouchers was one standard deviation higher, relative to its local digital output, raised the probability of joining by 4.4 points, on a base of 18.1%. Firms thus joined where demand was expected and when they were already expanding. As discussed below, this selection is likely to bias the subsequent comparisons upwards.

Effect on providers' accounts. To measure how providers' accounts responded to the programme, we follow the 3,211 providers with accounts from 2015 to 2024 and relate each outcome to what the provider billed the programme. In particular, we estimate

$$\frac{Y_{it}}{\bar{Y}_i} = \beta \frac{B_{it}}{\bar{Y}_i} + \lambda \frac{B_{i,t-1}}{\bar{Y}_i} + \gamma_1 \frac{B_{i,t+1}}{\bar{Y}_i} + \gamma_2 \frac{B_{i,t+2}}{\bar{Y}_i} + \alpha_i + \delta_{j(i)\,t} + \eta_{s(i)\,t} + \varepsilon_{it}, \tag{9}$$

where Y_{it} is output, inputs, wages or surplus (value added minus wages), B_{it} is what the provider billed the programme in year t, $\bar{Y}_i$ its average output in 2019 to 2021, and the fixed effects are those of the provider, its 4-digit industry by year and its size decile by year. Dividing everything by $\bar{Y}_i$ has a convenient implication: each coefficient reads as euros of outcome per euro billed. The effect of a euro billed is $\beta + \lambda$. The leads γ_1 and γ_2 ask whether what the provider would bill in the future was already moving its output, which would signal that billing picks up a previous trend. The identifying assumption is that, within industry and size class and relative to its own average, a provider's billing is unrelated to other shocks to its output.

Like firms that win public procurement contracts (Ferraz et al., 2015), providers grew with the demand created by the programme (Table 6). Each euro billed raised a provider's output by 0.76 euros. Since a euro billed is a euro of sales, this suggests that about a quarter of the money replaced sales the provider would have made anyway. Of each euro billed, 0.30 went

Table 6: Effect on Providers

	Euros per euro billed				Workers per
	Output	Inputs	Wages	Surplus	€100,000
Billed this year, β	0.664***	0.235***	0.112***	0.260***	0.378**
Billed last year, λ	0.096**	0.061**	0.102***	-0.040*	0.247*
Total, $\beta + \lambda$	0.760***	0.297***	0.214***	0.220***	0.624***
Billed next year, γ_1	0.054**	0.055***	0.012	-0.004	-0.028
Billed in two years, γ_2	0.041**	0.029***	0.011	0.013*	-0.014
Share of output, 2019 to 2021	1	0.561	0.340	0.098	
Providers			3,211		

Notes: Providers that are companies with positive output in 2019, 2020 and 2021, panel 2015 to 2024, without the 1% most exposed. Each outcome and each amount billed are divided by the provider's mean output in 2019 to 2021; billing by the year of entry into service of each contract. Inputs, wages and surplus (value added minus wages) in current euros; winsorised at the 1st and 99th%iles. Each column is a separate regression, so inputs, wages and surplus need not add up to output. Fixed effects of provider, 4-digit industry by year and decile of 2019 to 2021 output by year. Last column: employees on billing in hundreds of thousands of euros. Share of output: aggregate in 2019 to 2021. Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to inputs, 0.21 to wages and 0.22 stayed as surplus. As a benchmark, before the programme surplus represented 0.098 of each euro of output. Relative to the 0.76 euros of additional output, the surplus of 0.22 is almost three times that usual share, so the programme's sales were considerably more profitable than the provider's usual business. Providers also hired, about 0.6 workers for each €100,000 billed.

The main threat to this interpretation is the selection we have just documented. The leads are positive, 0.05 and 0.04. They are small relative to the 0.76 but not zero, indicating that the providers that would bill more were already growing somewhat faster. The event study in Figure 7 is consistent with this. The output of the most exposed providers was rising relative to the others before 2022, and then jumped in 2023. Their surplus, in contrast, shows no trend before the programme and jumps in the same year. Since the providers that joined were already growing, the pass-through to output is more likely to overstate than understate what the programme caused.

The 2024 rise in the voucher. Since what a provider bills also reflects its own effort and growth, we check the previous results against demand that providers did not create. The

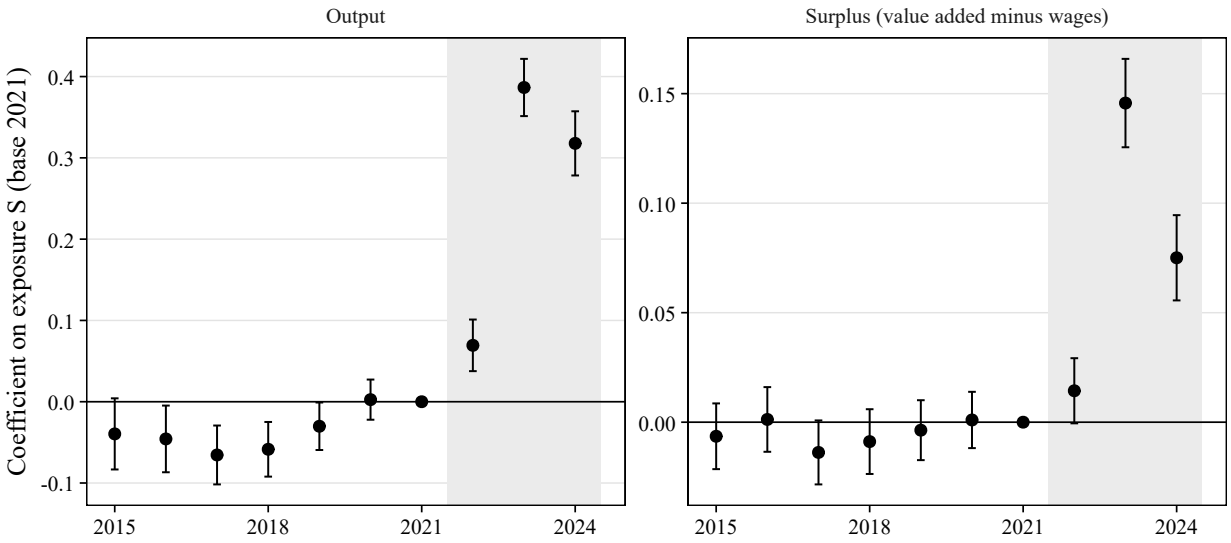


Figure 7: The output of exposed providers jumps in 2023, and their surplus had no trend before

Notes: Coefficients on exposure (billing in 2022 to 2024 over mean output in 2019 to 2021) interacted with year, base 2021, with the fixed effects of Table 6. Outcomes relative to mean output in 2019 to 2021. Bars: 95% confidence intervals, clustered by provider. Shaded: programme years.

2024 rise in the voucher of the smallest firms offers such a shock. When the segment III voucher rose by €1,000 in July 2024, each provider faced €1,000 of potential new demand for every segment III client that had already spent its voucher. In total, this amounted to €149 million, of which returning firms signed €59.5 million, half of it with a provider they already had. We use this potential demand as an instrument for the change in billing from 2023 to 2024, comparing 2,496 providers within the same industry and size class and with the same billing in 2022 and 2023 (Appendix Table 43).

The first stage is strong, and the instrument passes a placebo test. Each euro of potential demand raises billing in 2024 by 1.23 euros, and it does not predict output growth from 2022 to 2023, before the rise took place. Using this variation, each euro billed yields 0.73 euros of output, very close to the 0.76 obtained with the panel. The estimate for surplus is less precise: 0.14 euros, below the 0.22 of the panel but not statistically distinguishable from it.

This exercise rests on the assumption that a provider's book of small clients that had already spent their voucher affects its output in 2024 only through what it billed; the placebo

before the rise tests this assumption where it can be tested. Two further caveats apply. First, the estimate refers to the providers whose billing responded to returning small clients, which are not all providers. Second, part of the first stage comes from contracts signed before July and billed in 2024, so the potential also measures the provider's book of small clients and not only the rise in the voucher.

Both sides of the contract. We now combine the two sides of the contract, in the spirit of the welfare accounting of Hendren and Sprung-Keyser (2020). At the point estimate, each euro of voucher was associated with 1.47 euros of value added for its client over two years (Section 6). On the provider's side, about 0.24 euros of that euro appear to have displaced sales the provider would have made anyway, and 0.22 euros stayed as surplus. That is about 0.15 euros more than the same sales would have earned at the provider's usual margin. Value added is not, however, the same as what the firm would be willing to pay for the solution, which a zero price hides, so these figures are not a marginal value of public funds. They describe how the euro was divided. As with federal aid to for-profit colleges (Cellini and Goldin, 2014), a voucher that sets the price allows providers to retain part of the subsidy. In Kit Digital, that part is about a seventh of each euro, and it went to providers that were already growing. The accounts of the companies outside our data and of the self-employed, 53% of the money, and the applications to become a provider that Red.es rejected, would extend these comparisons to all providers.

8 Conclusion

Small firms adopt fewer digital technologies than large ones, and a growing number of governments try to close this gap by paying the full price of the technology. This paper asks whether removing the price of adoption is sufficient to induce small firms to participate, and what they gain when they adopt. To answer these questions, we study Kit Digital, a Spanish programme that offered every small firm a package of digital tools at zero price, delivered

by private providers, and we follow every eligible firm from eligibility to the productivity outcomes of adoption.

Our main conclusion is that removing the price did not remove the barriers to adoption, and that it shifted towards the provider decisions that prices would otherwise have guided. Three in four eligible companies never applied, and among those with up to two employees, 87 percent never did. Firm size was strongly associated with applying but not with implementing: once the voucher was granted, four in five firms of every size put a solution into service, and more leveraged firms were more likely to withdraw before signing, which would have committed them to advancing the VAT.

Although the firm signed every contract, its provider was closely associated with what it signed. Prices sat exactly at the cap in 72.1 percent of contracts, and the 2024 rise in the voucher of the smallest firms was spent almost to the euro. The identity of the provider explains up to 49.1 percentage points of the variation in each purchase, against at most 3.4 for the firm's own accounts and 1.5 for what the firm said it had before applying. The firms that adopted gained 0.9 percent of TFP over two years, mostly through tools that reach customers. At the point estimate, each euro of voucher returned 1.47 euros of value added to its client, while each euro billed left 0.22 euros of surplus with the provider.

These findings have three implications for subsidies to technology adoption, including those now being proposed for artificial intelligence. First, a zero price does not attract the firms that remain outside the programme. Most adoption is lost before firms apply, among the smallest firms, which points to the cost of applying rather than to the price. We cannot say, however, which part of that cost matters, nor what those firms would have gained. Second, a voucher that covers the full price still requires the firm to commit to the purchase, and it is at this stage that financial position matters. Covering what is left to pay at signature, such as the VAT (about €420 on a €2,000 voucher), would target this friction directly, provided that leverage reflects a lack of liquidity rather than scarce managerial time. By contrast, the return to adoption does not differ with leverage, so there is no case for a larger voucher for

leveraged firms. Third, when private providers deliver the subsidy, their incentives become part of the policy design. The cap operates as a price and the voucher as a budget, leaving the buyer little reason to bargain over either.

The second and third implications point in opposite directions, which constitutes the main trade-off in the design of such programmes. Leaving a small part of the cost with the firm would give it a reason to choose what it buys and at what price. But it would also add a cost at signature, which is the cost that the second implication suggests removing.

Our evidence is subject to several limitations. We observe only two years after adoption, and returns to information technology often take longer to show (Brynjolfsson and Hitt, 2003; Brynjolfsson et al., 2021); the 2025 accounts will add a third year. The estimated effect does not survive a moderate break in trends after 2022, and it falls to 0.7 percent when treated and control firms are also balanced on their debt. TFP is measured from revenue, so part of the gain may reflect demand or prices rather than efficiency. We do not observe what each contract contains, although the same provider charging half the price for the same solution, depending only on the voucher left, points to the price rather than to the content. Finally, 53 percent of the money went to providers whose accounts we do not observe.

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Appendix A The effect on productivity: design and additional results

Sample and comparison groups. The estimation sample contains 483,523 observations of 49,987 firms between 2014 and 2024 (Table 7). A firm enters the sample if its TFP, its covariates, its debt ratio and its voucher segment are observed in every year between its first and its last year with data. We exclude the firms of the 2024 cohort that signed a contract in 2023, since they may already have been treated in 2023.

Figure 8 shows the average log TFP of each group. Two features stand out. First, the three groups move together from 2014 to 2022, including the fall of 2020 and the recovery of 2021. Second, the treated firms are more productive than the controls throughout the period. This is why we compare changes rather than levels.

Table 7: Treated and control groups

Group	Firms	Share of firms	Share of value added (2022)
Cohort 2023	30,863	0.62	0.70
Cohort 2024 (control until 2023)	13,575	0.27	0.23
Not yet treated (2025–26)	5,549	0.11	0.07

Notes: Estimation sample. Cohort: year in which the firm's first solution entered into service (phase one invoice). Share of value added: share of the 2022 value added of the sample.

Two-period comparison. The main estimator aggregates comparisons of each year with 2022. Before turning to it, it is useful to look at the first of these comparisons, 2023 against 2022, which can be read directly from the group means. Among the 28,677 treated and 17,075 control firms observed in both years, the TFP of the treated grows 0.54 percentage points more than that of the controls, and 0.98 points more when firms are weighted by their 2022 value added (Table 8). The same comparison by regression, with firm and year fixed effects or in first differences, gives the same figures (Table 9).

Treated firms are larger than controls (Table 10). In particular, 31 percent of them are in the top asset quartile, against 23 percent of controls, and 31 percent are in the largest

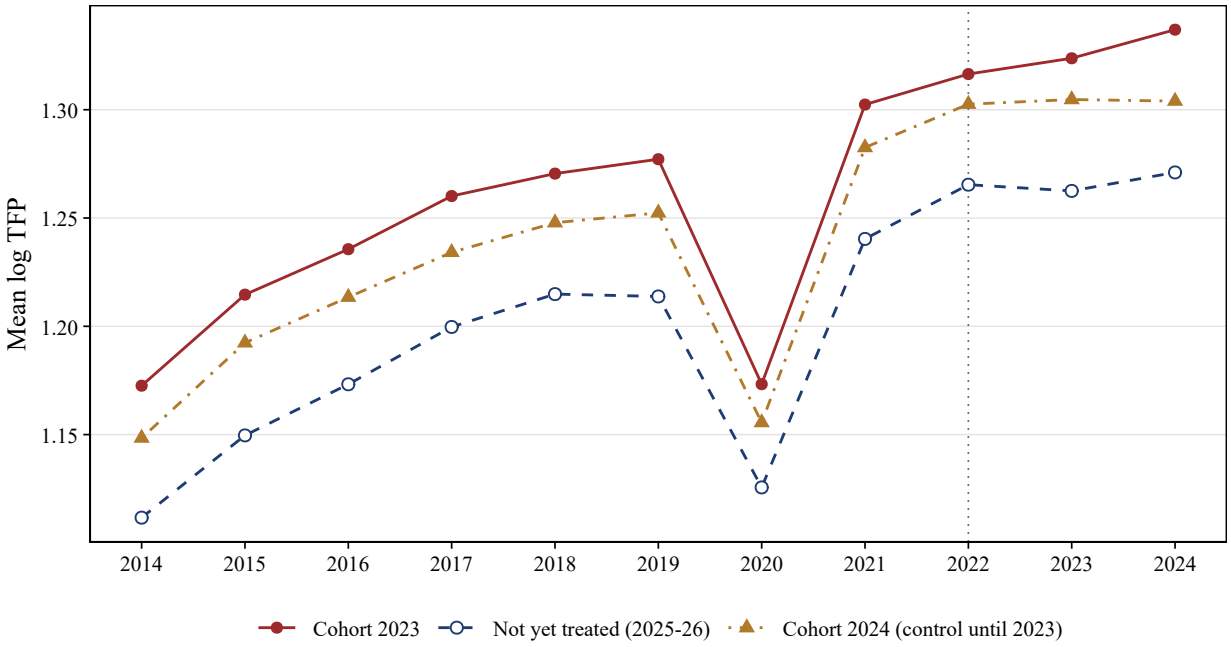


Figure 8: Mean log TFP by group

Notes: Estimation sample, unweighted means. The 2024 cohort is a control only until 2023. Dotted line: 2022, the base year.

voucher segment, against 20 percent. Conversely, only 18 percent are in the smallest segment, against 38 percent. These differences, however, barely affect the estimate: adding the 2022 covariates to the first difference leaves it almost unchanged (Table 11).

Table 12 reports the two pieces of the doubly robust estimator, namely the outcome regression on controls and the propensity score. The propensity scores of treated and controls overlap over their whole range, with a maximum of 0.87, so no observation needs to be trimmed (Figure 9). The three estimators, outcome regression, inverse probability weighting and doubly robust, give very similar results: 0.47 to 0.52 percent without weights and 0.93 to 0.96 percent when firms are weighted by value added (Table 13).

Event study. Table 14 reports the estimates behind Figure 4. Bootstrap and analytical standard errors are close. The joint test that the eight coefficients before 2022 are zero does not reject, with or without covariates, and the three estimators give the same path (Figure 10).

Table 8: Simple difference in differences, 2022 against 2023 and 2024

	Unweighted			Weighted by 2022 value added		
	Cohort 2023	Control	Gap/DiD	Cohort 2023	Control	Gap/DiD
2022 vs. 2023 ($e = 0$). Control: not yet treated 2025–26 and cohort 2024						
2022	1.3196	1.2920	0.0276	1.5772	1.5649	0.0123
2023	1.3239	1.2910	0.0329	1.5636	1.5416	0.0221
Trend/DiD	0.0043	-0.0010	**0.0054**	-0.0135	-0.0233	**0.0098**
2022 vs. 2024 ($e = 1$). Control: not yet treated 2025–26 only						
2022	1.3210	1.2633	0.0578	1.5715	1.5442	0.0273
2024	1.3370	1.2670	0.0700	1.5624	1.5097	0.0527
Trend/DiD	0.0159	0.0037	**0.0122**	-0.0091	-0.0345	**0.0254**

Notes: Mean log TFP of the 2023 cohort and of the controls observed in both years. Upper panel: controls are firms not yet treated in 2025 or 2026 and the 2024 cohort. Lower panel: in 2024 the 2024 cohort is already treated, so controls are firms not yet treated only. Last row of each panel: change over the two years and its difference between groups.

Table 9: Simple difference in differences by regression

Dependent Variables:	log TFP		Δ	log TFP		Δ
	Unweighted			Weighted by 2022 value added		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Constant	1.2920***	1.3093***	-0.0010	1.5649***	1.5735***	-0.0233***
Kit Digital	0.0276***		0.0054*	0.0123		0.0098***
Post	-0.0010			-0.0233***		
Kit Digital × Post	0.0054*	0.0054*		0.0098***	0.0098***	
Firm and year FE	No	Yes	No	No	Yes	No
Fit statistics						
Observations	91,504	91,504	45,752	91,504	91,504	45,752

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Firms observed in 2022 and 2023. Columns 1 and 4: pooled regression with treatment, post and their interaction. Columns 2 and 5: firm and year fixed effects. Columns 3 and 6: change in log TFP from 2022 to 2023 on the treatment dummy. Stars from standard errors clustered by firm: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Covariate balance in 2022

	Unweighted			Weighted by 2022 value added		
Variable	Control	Treated	Norm. diff.	Control	Treated	Norm. diff.
			Levels in 2022			
Assets: 2nd quartile	0.27	0.24	-0.07	0.14	0.11	-0.11
Assets: 3rd quartile	0.26	0.28	0.06	0.25	0.23	-0.05
Assets: 4th quartile	0.23	0.31	0.19	0.54	0.63	0.18
Age	20.42	21.72	0.13	22.92	24.04	0.10
Intangibles / assets	0.01	0.01	0.02	0.01	0.01	0.05
Segment I (10–49)	0.20	0.31	0.25	0.55	0.66	0.24
Segment III (0–2)	0.38	0.18	-0.46	0.12	0.04	-0.32
			Changes 2022–2023			
Intangibles / assets (Δ)	-0.00	0.00	0.15	-0.00	0.00	0.09

Notes: Means of the 2023 cohort and the controls observed in 2022 and 2023. Normalised difference: difference in means over the square root of the average of the two variances. The segment is reported for reference; it is not in the covariates of the main specification.

Table 11: First difference with covariates

Dependent Variable:	dy					
	Unweighted			Weighted by 2022 value added		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Kit Digital	0.0054*	0.0049	0.0061**	0.0098***	0.0093**	0.0105***
Fit statistics						
Observations	45,752	45,752	45,752	45,752	45,752	45,752

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Cols. 2 and 5: covariates in 2022 (including sector). Cols. 3 and 6: 2022–2023 change in the intangible share (may be an effect of the programme).

Notes: Change in log TFP from 2022 to 2023. Columns 2 and 5 add the 2022 covariates (asset quartile, age, intangible share, sector). Columns 3 and 6 add the change in the intangible share, which may itself respond to the programme. Stars from standard errors clustered by firm: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: The two pieces of the doubly robust estimator

Dependent Variables:	Outcome regression	Propensity score	Outcome regression	Propensity score
	Unweighted		Weighted by 2022 value added	
Model:	(1)	(2)	(3)	(4)
	OLS	Logit	OLS	Logit
Variables				
Constant	0.013	-0.023	-0.016	0.179***
Assets: 2nd quartile	-0.016**	0.254***	-0.011	0.251***
Assets: 3rd quartile	-0.008	0.475***	-0.004	0.463***
Assets: 4th quartile	-0.006	0.683***	-0.020**	0.694***
Age	0.000	0.006***	0.001***	0.005***
Intangibles / assets	-0.043	0.671***	-0.004	1.016***
s_2	-0.003	0.187***	0.011	0.188**
s_3	-0.043*	-0.035	-0.081***	0.104
s_4	0.000	0.053	0.005	-0.039
s_5	-0.019	-0.233***	-0.047***	-0.245***
s_6	-0.039***	0.155***	-0.047***	0.112*
s_7	0.008	0.047	-0.003	0.029
s_8	-0.039***	0.018	-0.032**	0.031
Fit statistics				
Observations	17,075	45,752	17,075	45,752

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Outcome regression: change in log TFP from 2022 to 2023 on the 2022 covariates, control firms only. Propensity score: logit of being in the 2023 cohort on the same covariates. Stars from robust standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Simple difference in differences with covariates

	Unweighted			Weighted by 2022 value added		
	Regression	IPW	Doubly robust	Regression	IPW	Doubly robust
Kit Digital	0.0047	0.0052*	0.0051*	0.0093**	0.0096***	0.0095***

Notes: 2023 cohort against controls, 2022 against 2023, estimators of Sant'Anna and Zhao (2020). Covariates in 2022: asset quartile, age, intangible share and sector. Stars from wild bootstrap standard errors (Rademacher weights, 999 replications, clustered by firm): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

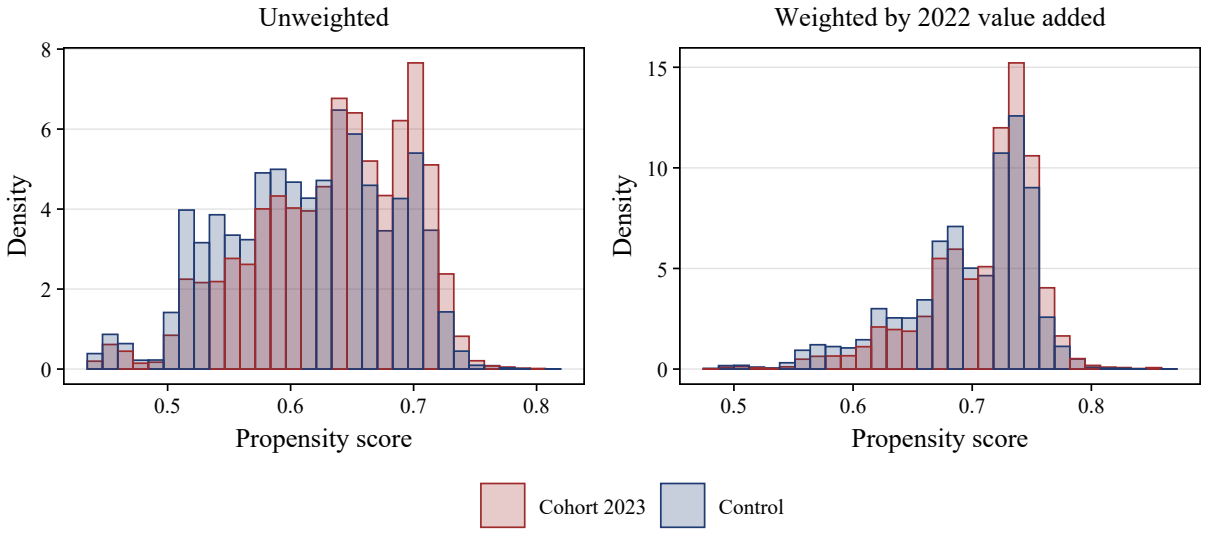


Figure 9: Overlap of the propensity score

Notes: Distribution of the propensity score of Table 12 for the 2023 cohort and the controls. Right panel: densities weighted by 2022 value added.

Table 15 examines how sensitive the average effect of 2023 and 2024 is to a break in trends after 2022. Following Rambachan and Roth (2023), it reports confidence sets that allow the deviation from parallel trends after 2022 to be up to $\bar{M}$ times the largest deviation between consecutive years before 2022. With $\bar{M} = 0$ the set excludes zero, whereas with $\bar{M} = 0.5$ it already includes it.

Robustness. We check the robustness of the main estimate in four directions. First, the main sample drops a firm whenever its TFP, any covariate, its debt ratio or its segment has a gap between its first and last year with data. If we only require TFP to be observed without gaps, the sample gains 33,014 observations and the doubly robust estimate falls slightly, from 0.91 to 0.78 percent (Table 17).

Second, four firms of the sample are themselves providers, so they received a demand shock in addition to the solutions. Excluding them leaves the estimate at 0.91 percent. Moreover, the effect shows no clear pattern by firm age or region (Appendix Tables 24 and 25).

Third, since leverage delays implementation (Section 4), the controls, which implemented later, could differ from the treated in their debt. Among the firms of the 2023 comparison,

Table 14: Event study

e	No covariates	Doubly robust
-9	-0.0010	-0.0050
-8	-0.0010	-0.0062
-7	-0.0005	-0.0056
-6	0.0026	-0.0008
-5	-0.0019	-0.0040
-4	0.0042	0.0016
-3	-0.0043	-0.0033
-2	0.0047	0.0046
0	0.0054*	0.0051*
1	0.0122*	0.0131*
Mean $e \geq 0$	0.0088**	0.0091**
Joint pre-trend p	0.243	0.190

Notes: Effect on log TFP of the 2023 cohort, by year relative to entry into service (e), against firms not yet treated; common base year 2022 ($e = -1$). Doubly robust: covariates in 2022 (asset quartile, age, intangible share, sector). Stars use the wild bootstrap standard error (Rademacher weights, 999 replications, clustered by firm), except for the mean of $e \geq 0$, which uses the analytical one from the influence function. Joint test of the eight coefficients before 2022 with the full analytical covariance matrix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

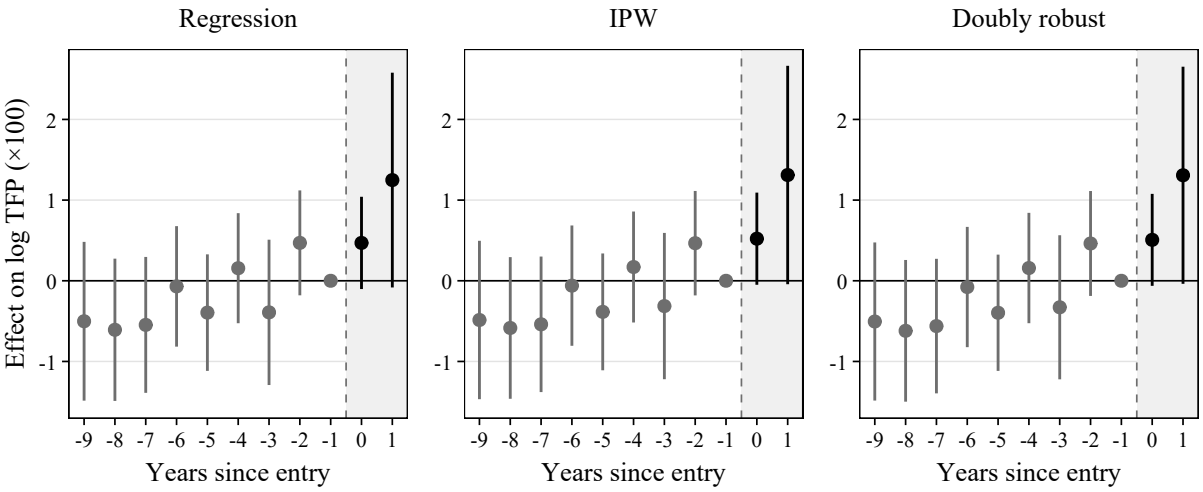


Figure 10: Event study with covariates: three estimators

Notes: As Figure 4, with covariates in 2022, by outcome regression, inverse probability weighting (IPW) and the doubly robust estimator. 95 percent confidence intervals, wild bootstrap clustered by firm.

Table 15: Sensitivity to deviations from parallel trends

	No covariates	Doubly robust
Mean of $e = 0$ and $e = 1$	0.0088	0.0091
95 percent confidence set		
Original	[0.0013, 0.0163]	[0.0013, 0.0169]
$\bar{M} = 0$	[0.0013, 0.0162]	[0.0014, 0.0168]
$\bar{M} = 0.5$	[−0.0067, 0.0239]	[−0.0056, 0.0234]
$\bar{M} = 1$	[−0.0179, 0.0352]	[−0.0160, 0.0338]

Notes: Relative magnitudes restriction of Rambachan and Roth (2023): the deviation from parallel trends between consecutive years after 2022 is at most $\bar{M}$ times the largest such deviation from 2014 to 2022. All eight coefficients before 2022 and the full analytical covariance matrix; C-LF confidence sets for the mean of the 2023 and 2024 effects. Original: confidence interval under parallel trends.

Table 16: Value added per euro of subsidy

	Effect on logs			Value added gained (euros)		Euros per euro
	2023	2024	Pre-trend p	2023	2024	of subsidy, 2023–24
TFP (same inputs)	0.0051*	0.0131**	0.190	2,254	5,810	1.47
Value added	0.0067*	0.0176**	0.000	2,966	7,797	1.96

Treated firms: 29,821. Mean value added in 2022: 444,245 euros. Mean subsidy in service: 5,482 euros.

Notes: Main specification (2023 cohort against firms not yet treated, base year 2022, doubly robust with covariates in 2022) on log TFP and on log value added. Value added gained: the effect of each year times the mean 2022 value added of treated firms (deflated, from the Central de Balances). Euros per euro: value added gained in 2023 and 2024 over the mean subsidy of the firm's contracts in service. No discounting and no years after 2024. The pre-trend test rejects for value added, so only the TFP row is used in the text. Stars from analytical standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 17: Robustness: sample defined by gaps in TFP only

	No covariates			Doubly robust		
Internal gaps	Mean $e \geq 0$	$e = 0$	$e = 1$	Mean $e \geq 0$	$e = 0$	$e = 1$
TFP, covariates and debt (main; 483,523 rows)	0.0088**	0.0054	0.0122	0.0091**	0.0051	0.0131
TFP only (516,537 rows)	0.0075**	0.0041	0.0109	0.0078**	0.0039	0.0117

Notes: Main specification in the main sample (no internal gaps in TFP, covariates, debt ratio and segment) and in a sample that drops firms only for internal gaps in TFP. Stars from analytical standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Joint test of the coefficients before 2022, sample defined by TFP only: $p = 0.247$ without covariates and $p = 0.191$ doubly robust.

the treated had a debt ratio of 0.177 in 2022 and the controls 0.156, a normalised difference of 0.11. Adding the 2022 debt ratio to the covariates lowers the estimate from 0.91 to 0.67 percent, and the joint test of the years before 2022 still does not reject (Table 18). Finally, the same specification on log output, like the one on value added, fails that test. Therefore, output cannot be used to tell a gain in efficiency from a gain in demand or prices.

Table 18: Robustness: debt of the controls and output

	Mean $e \geq 0$	$e = 0$	$e = 1$	Pre-trend p
log TFP, main specification	0.0091**	0.0051	0.0131	0.190
log TFP, adding the 2022 debt ratio	0.0067*	0.0033	0.0100	0.164
log output, main specification	0.0188***	0.0114	0.0263	0.000
Debt ratio in 2022: treated 0.177, controls 0.156 (normalised difference 0.11).				

Notes: Main specification (2023 cohort against firms not yet treated, base year 2022, doubly robust with covariates in 2022, estimator of 01_efecto copied in 08_robustez_efecto). Second row: the 2022 debt ratio added to the covariates. Third row: log output (production) in place of log TFP. Debt ratio: firms of the 2022 against 2023 comparison. Stars from analytical standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Fourth, treated firms were more digital before the programme than the controls. In the self-diagnosis, they scored 5.26 out of 12 and the controls 4.70, a normalised difference of 0.22. Adding the score to the covariates raises the estimate slightly, from 0.91 to 0.98 percent, and the joint test of the years before 2022 does not reject (Table 19). Note that 80.6 percent of treated firms took the test before 2023, against 27.0 percent of the controls, which took it when they applied. The same table splits the sample by the score and by whether the tools put into service were new to the firm according to its answers. Neither split rejects equal effects across groups.

Appendix B Heterogeneity of the effect on productivity

The tables in this appendix estimate the main specification separately in each group, using treated and control firms of the same group, so that each group has its own counterfactual.

Table 19: The effect on productivity and the self-diagnosis

	Mean $e \geq 0$	$e = 0$	$e = 1$	Pre-trend p	Treated	Controls
A. Covariates						
Main specification	0.0091**	0.0051	0.0131	0.190	30,863	19,124
Adding the test score	0.0098**	0.0051	0.0144	0.369	30,863	19,124
B. Test score, within the 2022 sector						
Score: lower half	0.0069	0.0042	0.0097	0.373	16,362	11,664
Score: upper half	0.0139**	0.0074	0.0205	0.744	14,500	7,460
Equality across groups: $p = 0.388$						
C. What the firm put into service, against what it already had						
Only tools it lacked	0.0050	0.0015	0.0084	0.394	10,228	7,694
Both	0.0119	0.0083	0.0156	0.093	5,446	2,129
Only tools it already had	0.0063	-0.0006	0.0133	0.111	10,959	5,503
Equality across groups: $p = 0.836$						
Test score (0 to 12): treated 5.26, controls 4.70 (normalised difference 0.22).						

Notes: Main specification (2023 cohort against firms not yet treated, base year 2022, doubly robust with covariates in 2022, estimator of 01_efecto copied in 09_autodiagnostico). Test score: total of the first AceleraPyme test of each firm, from 0 to 12. Panel A: the score added to the covariates. Panel B: halves of the score within the 2022 sector. Panel C: solutions put into service in segments I to III, at any date, matched to the answer that covers them (ERP for process management and electronic invoicing, CRM for customer management, social media profile for social media management and advanced online presence, online sales of 1 percent of turnover or more for the online shop and marketplace, ICT security measures for cybersecurity); solutions with no matching answer do not count, and firms with none of the matched solutions are left out. In panels B and C, treated and control firms belong to the same group. Equality across groups: Cochran's Q test. Stars from analytical standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All groups are defined before treatment: the voucher segment declared at application, the sector, the employment class and the region, age in 2022, financial and capability indicators in 2019 split at the sector median, and the composition of the main provider's sales to its other clients. The only exception is the bundle of tools. Since the bundle is what the firm put into service, its effect is the effect on the firms that chose it.

Size on its own does not order the returns in a clear way. By voucher segment, the effect is 1.7 percent among the smallest firms, 0.6 percent among firms with three to nine employees and zero among those with ten to 49 (Table 22). By employment in 2019, however, the ordering changes: 1.0, 0.5 and 1.1 percent (Table 23). In both cases the differences are small relative to their sampling error. Moreover, the smallest segment is the only one in which the years before 2022 reject parallel trends, so its larger effect, and the effect of demand tools within it, should be read with care.

Table 20: Effect by sector (2022)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
IT & Media	582	0.019	0.0258	0.0394	0.0317	0.0471	0.419	0.0007
Prof. Services	5,476	0.177	0.0259***	0.0280***	0.0265	0.0294	0.327	0.0050
Hospitality	1,812	0.059	0.0236*	0.0216*	0.0066	0.0366	0.292	0.0013
Other Services	2,761	0.089	0.0186	0.0169	0.0019	0.0319	0.111	0.0015
Transport	1,507	0.049	0.0100	0.0093	0.0030	0.0156	0.948	0.0005
Construction	3,794	0.123	0.0058	0.0055	0.0076	0.0034	0.325	0.0007
Manufacturing	5,232	0.170	-0.0023	0.0005	-0.0058	0.0068	0.948	0.0001
Trade	9,699	0.314	0.0000	-0.0003	-0.0009	0.0002	0.669	-0.0001
Equality across groups (with covariates): Q = 9.02, d.f. = 7, p = 0.251								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 21: Effect by bundle of channels put into service

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Demand only	11,506	0.373	0.0194***	0.0184**	0.0037	0.0330	0.251	0.0068
Cost + demand	9,451	0.306	0.0170**	0.0178**	0.0115	0.0241	0.798	0.0054
Demand + BI	1,006	0.033	-0.0172	0.0056	0.0060	0.0052	0.014	0.0002
All three	2,375	0.077	0.0043	0.0043	0.0102	-0.0016	0.493	0.0003
Cost + BI	1,596	0.052	0.0027	-0.0027	-0.0208	0.0154	0.680	-0.0001
Cost only	4,680	0.152	-0.0120	-0.0117	-0.0040	-0.0194	0.082	-0.0018
Equality across groups (with covariates): Q = 8.39, d.f. = 5, p = 0.136								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 22: Effect by voucher segment (declared at application)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Segment I (10-49)	9,207	0.298	-0.0043	-0.0001	-0.0119	0.0118	0.977	-0.0000
Segment II (3-9)	15,328	0.497	0.0038	0.0056	0.0009	0.0103	0.194	0.0028
Segment III (0-2)	6,328	0.205	0.0162*	0.0165*	0.0163	0.0167	0.010	0.0034
Equality across groups (with covariates): Q = 2.50, d.f. = 2, p = 0.286								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 23: Effect by employment class in 2019

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
0-2 employees	4,831	0.157	0.0088	0.0095	0.0010	0.0180	0.639	0.0015
3-9 employees	14,732	0.477	0.0030	0.0046	0.0065	0.0028	0.454	0.0022
10+ employees	9,849	0.319	0.0071	0.0107	-0.0045	0.0259	1.000	0.0034
Equality across groups (with covariates): Q = 0.60, d.f. = 2, p = 0.742								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 24: Effect by firm age in 2022

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
0-5 years	1,017	0.033	0.0293	0.0302	-0.0043	0.0648	0.709	0.0010
6-10 years	3,189	0.103	-0.0043	-0.0048	0.0019	-0.0115	0.459	-0.0005
11-20 years	10,158	0.329	0.0180***	0.0191***	0.0092	0.0291	0.068	0.0063
21+ years	15,457	0.501	0.0028	0.0042	0.0036	0.0048	0.771	0.0021
Equality across groups (with covariates): Q = 4.90, d.f. = 3, p = 0.179								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 25: Effect by region

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Navarre	505	0.016	0.0521**	0.0826***	-0.0132	0.1784	0.394	0.0014
Canary Islands	459	0.015	0.0772**	0.0679**	0.0783	0.0575	0.091	0.0010
Castilla y León	2,024	0.066	0.0297**	0.0362**	0.0305	0.0418	0.368	0.0024
Andalusia	3,792	0.123	0.0222*	0.0248*	-0.0005	0.0501	0.400	0.0030
La Rioja	329	0.011	-0.0478	0.0174	0.0140	0.0207	0.225	0.0002
Madrid	3,864	0.125	0.0260*	0.0150	0.0139	0.0160	0.370	0.0019
Extremadura	458	0.015	0.0048	0.0139	0.0093	0.0185	0.032	0.0002
Galicia	2,326	0.075	0.0106	0.0084	0.0004	0.0165	0.866	0.0006
Murcia	1,071	0.035	0.0110	0.0083	-0.0012	0.0177	0.162	0.0003
Basque Country	1,578	0.051	0.0019	0.0076	0.0131	0.0022	0.115	0.0004
Aragon	1,443	0.047	0.0071	0.0061	0.0030	0.0092	0.621	0.0003
Catalonia	6,149	0.199	0.0019	-0.0002	0.0029	-0.0032	0.320	-0.0000
Valencia	4,051	0.131	-0.0026	-0.0014	-0.0075	0.0048	0.781	-0.0002
Cantabria	374	0.012	-0.0302	-0.0159	0.0069	-0.0387	0.237	-0.0002
Asturias	676	0.022	-0.0187	-0.0160	0.0140	-0.0460	0.179	-0.0004
Castilla-La Mancha	1,139	0.037	-0.0300	-0.0295	-0.0202	-0.0387	0.613	-0.0011
Balearic Islands	598	0.019	-0.0191	-0.0307	-0.0099	-0.0514	0.709	-0.0006

Equality across groups (with covariates): Q = 24.48, d.f. = 16, p = 0.080

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 26: Effect by 2019 debt over assets (halves within sector)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Lower half	14,178	0.459	0.0029	0.0050	0.0026	0.0074	0.264	0.0023
Upper half	15,106	0.489	0.0112**	0.0095*	0.0053	0.0137	0.475	0.0046

Equality across groups (with covariates): Q = 0.31, d.f. = 1, p = 0.575; upper - lower = 0.0045

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 27: Effect by 2019 short-term share of debt (halves within sector)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Lower half	10,283	0.333	0.0072	0.0045	0.0040	0.0050	0.702	0.0015
Upper half	10,690	0.346	0.0053	0.0075	0.0041	0.0108	0.176	0.0026
Equality across groups (with covariates): Q = 0.10, d.f. = 1, p = 0.755; upper - lower = 0.0030								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 28: Effect by 2019 intangible share of assets (halves within sector)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Lower half	17,531	0.568	0.0104**	0.0123**	0.0080	0.0166	0.330	0.0070
Upper half	10,926	0.354	0.0002	-0.0014	0.0003	-0.0031	0.852	-0.0005
Equality across groups (with covariates): Q = 2.37, d.f. = 1, p = 0.124; upper - lower = -0.0137								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 29: Effect by 2019 average wage (halves within sector)

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Lower half	14,044	0.455	0.0086	0.0105*	0.0024	0.0186	0.326	0.0048
Upper half	14,850	0.481	0.0053	0.0046	0.0054	0.0038	0.142	0.0022
Equality across groups (with covariates): Q = 0.54, d.f. = 1, p = 0.461; upper - lower = -0.0059								

Notes: Main specification (2023 cohort, controls not yet treated, base year 2022, doubly robust with covariates in 2022) estimated separately in each group; treated and control firms belong to the same group. ATT: mean of the 2023 and 2024 effects on log TFP; stars from analytical standard errors. Share: group's share of treated firms. Contrib.: ATT times share. Pre p: joint test of the coefficients before 2022 that the group estimates. Last row: Cochran's Q test of equal effects across groups, with covariates. Groups with fewer than 100 treated or control firms are not estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 30: Effect by what the firm's provider sells to other clients

			ATT, mean $e \geq 0$		With covariates			
Group	Treated	Share	No cov.	With cov.	$e = 0$	$e = 1$	Pre p	Contrib.
Low demand propensity	10,218	0.331	0.0028	0.0028	0.0021	0.0035	0.133	0.0009
Intermediate	10,206	0.331	0.0081	0.0096	0.0077	0.0115	0.723	0.0032
High demand propensity	10,212	0.331	0.0152**	0.0136**	0.0053	0.0219	0.898	0.0045
Equality across groups (with covariates): Q = 1.25, d.f. = 2, p = 0.535								

Notes: Main specification estimated separately by tercile, among treated firms, of the demand share of the sales of the firm's main provider to its other clients (all valid contracts of segments I to III). The tercile is set by the provider, not by the firm. Demand share of the firm's own spending: 0.22, 0.64 and 0.92 from the low to the high tercile (provider propensity 0.21, 0.65 and 0.93). Control firms are classified by the provider they later contract. Other details as in Table 20.

Appendix C Take-up, prices and providers: additional tables

This appendix reports the tables behind Sections 4 and 5: who took the self-diagnosis and how firms heard of the programme, how leveraged applicants fail, which providers have abnormal withdrawal rates, how prices respond to the cap and to the voucher left, and the tests of whether what is bought follows the firm or its provider.

Table 31: Most of the size gradient in applying is already present in who takes the self-diagnosis

	0 to 2 employees	3 to 9	10 to 49	All
Firms	297,494	162,413	82,476	542,383
Apply (%)	13.0	35.7	51.8	25.7
Take the self-diagnosis (%)	22.8	47.4	63.0	36.3
Apply, if took it (%)	56.7	75.2	82.1	70.7
Took it, among applicants (%)	99.8	99.9	99.9	99.9
Took it, among non-applicants (%)	11.4	18.3	23.4	14.3
before 2024 (%)	2.0	5.9	10.5	3.8
Non-applicants that took it	29,413	19,078	9,290	57,781

Notes: Corporations of Table **??**, by employment in 2021. Take the self-diagnosis: the firm completed the AceleraPyme test at least once (linked by the tax identifier of the firm in the Central de Balances, first test of each firm). Applicants had to complete it, so the share among applicants measures the quality of the link. Before 2024: non-applicants whose first test is dated before 1 January 2024.

Table 32: Firms recommended by a third party apply more, but implement at almost the same rate

How the firm heard of the programme	Share of test takers (%)	Raw rate (%)		Adjusted difference (pp)	
		Apply	In service | granted	Apply	In service | granted
Recommended by a third party	34.9	75.8	82.6	6.0***	1.8***
Programme website	7.7	67.6	80.7	(ref.)	(ref.)
Press	8.0	69.5	83.0	1.8***	2.3***
Social media	3.7	62.3	81.2	-4.6***	0.5
E-mail	4.2	77.4	82.8	6.3***	1.8**
Events	1.8	69.2	83.6	0.8	3.0***
Other	24.8	73.4	79.7	4.8***	-1.0**
No answer	14.9	56.5	76.2	-8.0***	-4.3***
Firms	196,896			196,896	112,912

Notes: Corporations of Table **??** that completed the self-diagnosis, by their answer to how they heard of the programme in their first test. Raw rates in percent; in service among firms granted the voucher. Adjusted difference: linear probability models in percentage points against firms that found the programme's website, with log assets, log employment and age in 2021 and fixed effects of 2-digit sector by voucher segment. Stars from standard errors clustered by sector: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 33: How leveraged applicants fail

Outcome (applicants)	Effect of 1 s.d. of leverage (pp)	Mean (pp)	Firms
Implements	-2.65***	67.9	110,634
Fails before signing	2.64***	30.1	110,634
Fails after signing	0.01	2.1	110,634
Implements, 2019–21 growth	-2.62***	67.9	110,634
Implements, survivors	-2.43***	69.2	92,302
Segment I	-1.62***	69.3	37,867
Segment II	-2.52***	68.1	48,894
Segment III	-3.59***	65.1	23,873

Notes: Applicants with accounts in 2019 and 2021 and 0 to 49 employees in 2021. Each row is a linear probability model in percentage points with the specification of Table 2; the table reports the effect of one standard deviation of 2019 leverage. Fails before signing: does not implement and never signed a contract with a provider. Fails after signing: does not implement after signing. 2019 to 2021 growth: adds the change in TFP and in log sales between 2019 and 2021. Survivors: firms that file accounts in 2023 and 2024. Segments by employment in 2021. Stars from standard errors clustered by sector: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 34: Providers with abnormal withdrawal rates

	Abnormal withdrawal rate	Other providers
Providers	22	3,244
Contracts	26,703	1,401,509
Vouchers (clients)	15,486	909,535
Contracts withdrawn (%)	32.0	3.4
Expected from solution, segment, province and quarter (%)	6.9	5.4
Withdrawn clients who sign the same solution elsewhere (%)	11.6	7.4

Notes: All contracts of segments I to III with a signature date. Expected rate: for each contract, the withdrawal rate of all other providers in the same cell of solution, segment, client's province and quarter of signature, averaged over the provider's contracts. Abnormal: 50 or more contracts, an excess of 15 points or more over the expected rate and a binomial z of 5 or more. Rates are simple means across providers. Clients who sign elsewhere: withdrawn contracts whose voucher signs the same solution with a different provider.

Table 35: Price, cap and remaining voucher within provider and solution

	Only the cap binds	The remaining voucher binds
log cap	0.92^{***}	
log remaining voucher		0.83^{***}
Provider × solution fixed effects	Yes	Yes
Contracts	898,263	88,874

Notes: Log contract price on the log cap (contracts in which only the cap binds) and on the log remaining voucher (contracts in which the remaining voucher binds), with provider by solution fixed effects; cells with a single contract dropped. Sample of Table 3. Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 36: Explained variance of what firms buy

	Location × segment × month	Firm's accounts add	Provider adds	Provider, after the firm's accounts
Cost share of spending	7.5	3.0	48.5	45.8
Demand share of spending	11.9	3.4	49.1	46.1
BI share of spending	8.5	1.7	25.0	23.7
Number of channels	19.9	0.4	22.6	22.3
Exhausts the voucher	17.2	0.7	26.4	25.9

Notes: Adjusted R^2 in percent. Vouchers of Figure 3. Column 1: fixed effects of province by segment by month of grant. Columns 2 and 3: increase when adding the firm's 2021 accounts (TFP, log assets, debt ratio, age, intangible share, log employment) and its 2-digit sector, or the main provider's fixed effect. Column 4: increase when adding the provider after the firm's accounts.

Table 37: What the provider sells to other clients and what the firm buys

	Outcome on provider's propensity		Firm's predicted outcome	Mean
	Without accounts	With accounts	on provider's propensity	
Cost share of spending	0.899***	0.892***	0.051***	0.34
Demand share of spending	0.888***	0.879***	0.057***	0.60
BI share of spending	0.877***	0.861***	0.049***	0.06
Number of channels	0.822***	0.826***	0.046***	1.51
Exhausts the voucher	0.757***	0.757***	0.028***	0.48
Vouchers	29,892			

Notes: Vouchers of Figure 3. Provider's propensity: mean outcome of the provider's other clients in the whole programme, leaving out the firm. Columns 1 and 2: firm's outcome on the propensity, with fixed effects of province by segment by month, without and with the firm's 2021 accounts and sector. Column 3: the outcome predicted by the firm's 2021 accounts and sector, on the propensity (a test of sorting). Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 38: Monthly sales of the provider and what the firm buys

	Firm's share on provider's sales that month		Balance, provider FE	
	Without provider FE	With provider FE	log assets 2021	TFP 2021
Cost share	0.94***	0.37***	0.10	-0.00
Demand share	0.91***	0.32***	-0.15	-0.04
BI share	0.76***	0.22***	0.27	0.17
Vouchers	19,249			

Notes: Valid contracts signed before July 2024, without the secure workstation, of firms in the estimation sample of Section 6. For each voucher, its main provider and the month of its first contract with it. Provider's sales that month: spending share of the channel among the provider's other clients who signed that month. Providers with 50 or more clients and 5 or more other clients that month. Fixed effects of province by segment by month. Balance: 2021 log assets and TFP on the provider's monthly share, with provider fixed effects. Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 39: What the firm had before applying and what it bought

		Adjusted R^2 added		Already had it	Provider's propensity, +1 s.d.	
	Mean	Self diagnosis	Provider	(self diagnosis)	Without test	With test
Cost share of spending	33.5	1.1	48.5		26.1***	26.1***
Demand share of spending	60.4	1.2	49.1		28.5***	28.4***
Buys process management	40.5	0.6	48.9	-2.2***	34.2***	34.3***
Buys customer management	21.3	0.2	36.2	-1.8**	24.1***	24.1***
Buys social media management	31.0	1.5	30.6	4.8***	23.0***	22.7***
Buys a website	47.8	1.0	26.4	-4.9***	24.7***	24.9***
Buys an online shop	12.2	1.0	18.9	2.9***	11.5***	11.3***
Buys electronic invoicing	12.9	0.2	31.0	-1.5**	19.2***	19.2***
Vouchers				29,890		

Notes: Vouchers of Figure 3 whose firm completed the self-diagnosis of AceleraPyme (Red.es), first test of each firm. All values in percentage points. Mean: share of vouchers that buy the solution, or mean spending share. Adjusted R^2 added over fixed effects of province by segment by month of grant: by the seven yes or no answers used (training and measures in ICT security, ERP, social media profile, social media for advertising, CRM, online sales of 1 percent of turnover or more) and the total score, or by the provider's fixed effect. Already had it: coefficient of the related answer (ERP for process management and electronic invoicing, CRM for customer management, social media profile for social media and website, online sales for the online shop), with the provider's propensity, all answers and the fixed effects. Provider's propensity: share of the provider's other clients who buy the solution, times its standard deviation, without and with all answers. Stars from standard errors clustered by provider: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D Providers

This appendix describes the providers and reports the tables behind Section 7: the structure of the market, how much the programme weighed for each type of provider, which firms became providers, and the 2024 rise in the voucher as a demand shock.

Table 40: Digital solution providers (*agentes digitalizadores*)

	All executed vouchers	Treated firms, cohort 2023
A. Market structure		
Unique providers	9,773	5,551
Executed vouchers	692,636	30,774
Provider–voucher deliveries	857,512	38,536
Vouchers per provider, median	13.0	3.0
Vouchers per provider, mean	87.7	6.9
Providers serving 1 voucher (%)	11.5	30.9
Providers serving 100+ vouchers (%)	11.4	0.4
Top-10 share of deliveries (%)	26.3	15.8
Top-100 share of deliveries (%)	53.4	32.5
Herfindahl index	89	55
Deliveries by a provider in the firm's province (%)	35.9	52.3
Providers in more than 10 provinces (%)	15.1	2.3
Their share of deliveries (%)	78.6	31.5
B. Providers' accounts in 2021 or earlier		
Providers with 2021 accounts (%)	38.7	48.0
Employees (median)	4.6	5.0
Total assets, thousand euros (median)	262	271
Age, years (median)	15.0	15.0
Share in ICT, NACE J (%)	50.1	51.5
Share in professional services, NACE M (%)	24.1	23.0
Share in trade, NACE G (%)	17.1	17.9

Notes: Executed voucher: an application (segments I to III, including the self-employed) with at least one solution in service. Delivery: a voucher × provider pair with at least one solution in service. Right column: the executed vouchers (30,774) of the 30,863 treated firms of the estimation sample (first solution in service in 2023). Herfindahl index of delivery shares in percent (from 0 to 10,000). Panel B: last balance sheet of 2021 or earlier in the Central de Balances, for providers that are incorporated and file accounts; sector shares among those with a known industry code.

Table 41: Who the providers are and how much the programme weighs for them

		Share of	Exposure S		
	Providers	billing (%)	Median	90th pct.	$S > 1$ (%)
Company outside the panel	3,522	44.8			
Programming and IT consulting (62)	1,889	22.3	0.09	1.03	10.5
Wholesale and retail trade (45 to 47)	744	8.5	0.06	0.86	8.5
Sole proprietor	2,007	8.4			
Other industries	826	7.2	0.09	0.95	9.0
Advertising and marketing (73)	549	3.4	0.06	0.74	7.7
Information services and hosting (63)	137	2.8	0.17	1.37	13.7
Management consulting (70)	189	1.9	0.11	0.93	9.3
Telecommunications (61)	60	0.3	0.03	0.51	4.9
Accounting and tax advice (69)	49	0.3	0.04	0.81	10.3
All providers	9,972	100.0	0.08	0.95	9.5

Notes: Providers with at least one valid contract. Share of billing: subsidy of valid contracts by year of entry into service, 2022 to 2024. Exposure S: billing in 2022 to 2024 over mean output in 2019 to 2021, for the 3,244 providers with positive output in each of those years. Type: 2-digit industry of 2021 (or the latest year before). Companies outside the panel: not in the Central de Balances Integrada with fewer than 55 employees before the programme.

Table 42: Who became a provider

	Mean		Becomes a provider
	Providers	Others	(pp per s.d.)
Log output, 2021	6.08	5.73	-2.93***
Log(1 + employees), 2021	1.84	1.33	8.81***
Output growth, 2016 to 2019	0.28	0.17	1.67***
Output growth, 2019 to 2021	0.12	0.00	2.74***
Age	17.14	16.88	0.38
Intangible share of assets	0.06	0.03	0.77**
Average wage	35.04	37.62	-1.08**
Debt ratio	0.17	0.13	2.05***
Log TFP, 2021	1.07	1.11	-0.51*
Province exposure to demand	-1.61	-1.91	4.37***
Firms	2,437	10,990	13,427
Share that becomes a provider (%)			18.1

Notes: Companies in the 4-digit industries where at least 5 percent of firms with output in 2021, and at least 10 firms, became providers; meeting the turnover requirement with their 2020 and 2021 output; with positive output in 2019, 2020 and 2021. Linear probability model of becoming a provider (0 or 100) on all characteristics jointly, each standardised, with industry fixed effects. Characteristics of 2021 or before, winsorised at the 1st and 99th percentiles; output and average wage in thousands of euros; missing values set to the median, with an indicator. Province exposure: log of the voucher money expected from the province's firms in 2021 (firms in each voucher segment times the voucher) over the 2021 output of the province's firms in these industries. Stars from standard errors clustered by province: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 43: The increase in the segment III voucher as a demand shock to providers

A. On the potential from returning segment III clients	
First stage: change in billing, 2023 to 2024	1.23***
billing in 2024 of contracts signed before July 2024	0.89***
billing in 2024 of contracts signed from July 2024	0.78***
Reduced form: change in output, 2023 to 2024	0.90***
Placebo: change in output, 2022 to 2023	-0.03
Placebo: change in surplus, 2022 to 2023	-0.12
B. Euros per euro billed, instrumented with the potential	
Output	0.73***
Inputs	0.35**
Wages	0.18**
Surplus	0.14
Providers	2,496

Notes: Providers that are companies with accounts in 2022, 2023 and 2024 and positive output in 2019 to 2021, without the 1 percent with the largest potential or the largest billing in 2023. Potential: €1,000 for each segment III client that had spent its €2,000 voucher before July 2024, shared among its providers by what it spent with each. All changes and the potential divided by mean output in 2019 to 2021, winsorised at the 1st and 99th percentiles. Controls: billing in 2022 and 2023, share of segment III in what the provider signed before July 2024, and fixed effects of industry and size decile. Panel B: change in each outcome from 2023 to 2024 on the change in billing, instrumented with the potential. Stars from robust standard errors: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix E Regulatory design of the payment

The payment rules summarised in Section 2 come from Orden ETD/1498/2021 (BOE-A-2021-21873) and its amendments. Four of them are relevant for this paper.

- *Assignment of the payment right.* When the firm signs the implementation agreement with an accredited provider, it assigns to the provider its right to collect the subsidy (arts. 18.4, 21.7 and 22.3). The administrator pays the provider once each implementation phase has been justified (art. 23), and the first payment is made on account (art. 23.3). As a result, the firm never advances the subsidised amount.

- *What the firm pays.* VAT is not eligible for the subsidy (art. 19.2), and the firm pays the provider any difference between the price of the solution and the subsidy (art. 21.4). These are the only payments the firm makes out of its own pocket.

- *Caps per solution.* The subsidy for each solution is capped by segment (art. 18.2 and

annex). The cap is therefore the highest price that the voucher fully covers.

- *Timing.* Implementation has two phases: installation, within three months of the agreement, and twelve months of service (art. 22.2). Annex IV assigns 70% of each solution to the first phase and 30% to the second (40% and 60% for social media management). After the first phase, the provider issues a single invoice for the full contracted amount, showing the subsidy as a deduction (art. 22.3). To collect the subsidy, it must justify this invoice together with proof that the firm has paid its part (art. 31.6.c). The agreement signed with the provider states in advance "the amount that the beneficiary must pay with own funds, including applicable taxes" (Annex I), so the firm knows what it will pay when it signs.