Technoparks and Firm Upgrading*

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July 2026

Abstract

We study the effects of place-based innovation policy on incumbent firms, using the staggered opening of technoparks in Türkiye and administrative data covering the near-universe of non-financial firms from 2009 to 2023. Technopark openings lead firms in treated districts to upgrade technologically. Firms become more likely to buy and sell high-technology goods, while exports of high-R&D and high-technology products rise by around 4 per cent. These changes are accompanied by increases in total factor productivity over the medium term. The mechanism results show that firms import more automation-related capital goods, employ more high-technology workers, and form new relationships with technology-intensive suppliers and customers. The gains are larger among bigger firms, which are better placed to absorb new technologies and reorganise production. The effects also spread beyond treated districts through supply chains: firms upgrade when their customers become exposed to technoparks, while exposure through suppliers has little effect.

*We thank Philippe Aghion, Ufuk Akçigit, Pauline Castaing (discussant), Banu Demir, Joseph Kaboski, Guzmán Ourens (discussant), Tristan Reed, John Van Reenen, and seminar and conference participants at the Central Bank of the Republic of Türkiye (CBRT), European Bank for Reconstruction and Development (EBRD), King's College London, Özyeğin University, the IAB-IBS-LISER Conference on "Jobs, Skills, and Productivity in Structural Transformations," the Columbia University Conference on "New Thinking in Industrial, Innovation & Technology Policy," the CEPR Conference on Growth, Innovation and the Social Model in Europe, the 10th World Bank Conference on Urbanization and Development, the Barcelona Summer Forum, the Tilburg Macroeconomics Conference, and the NOVAFRICA Conference on Economic Development for useful comments. Anton Grahed and Ubeyd Öktem provided excellent research assistance. We are also grateful to the Turkish Technology Development Zones Association for its collaboration on the technopark survey. The views expressed in this paper are those of the authors and do not necessarily represent the views of the EBRD, the CBRT, or their respective Boards of Directors. Any errors are our own.

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1 Introduction

Technoparks, geographically designated areas that attract technology-oriented firms through preferential fiscal treatment, shared infrastructure, and organised links to research institutions, have become one of the most widely deployed instruments of industrial policy over the past three decades. Over two-thirds of all science and technology parks worldwide have been established since the 2000s, and as of 2024, 534 parks operate across 78 countries (IASP, 2024).[1] Yet rigorous evidence on whether technoparks deliver on their core promise remains scarce. Do they generate genuine technological upgrading among nearby firms, shifting them toward more sophisticated products, more technology-intensive supply chains, and higher productivity? Where upgrading does occur, through which channels does it reach firms that never enter the park, and does it strengthen aggregate capabilities rather than simply reshuffling activity across space? The answers matter not just for the evaluation of a single policy but for the design of industrial strategy across the many middle-income economies that have made technoparks a centrepiece of their development programmes.

We study Türkiye's Technology Development Zones programme, known as technoparks, one of the most ambitious such initiatives among middle-income economies. Three features make it a compelling setting. First, the need for technological upgrading is well documented and severe. Türkiye ranks last among all OECD economies in high-technology exports as a share of manufactured exports, and employment in high- and medium-high-technology manufacturing sits well below the European Union average (Appendix Figure A1), a pattern shared by many middle-income economies that remain concentrated in low-value-added production. Closing this gap matters. Technological upgrading raises firm productivity, broadens access to export markets and global value chains, and drives the structural transformation central to long-run growth. Law No. 4691 of July 2001 established a national technopark framework to this end, creating designated zones that cluster innovative firms and support technology-intensive production. Sec-

[1] The first technopark was the Stanford Industrial Park, established in the United States in 1951, which inspired early international models such as the United Kingdom's Cambridge Science Park in 1970 and France's Sophia Antipolis in 1969. During the 1980s and 1990s, Japan, China, and South Korea expanded this framework as a tool for technological upgrading and structural transformation. Today, hundreds of parks operate globally, with the model having spread across a wide range of economies, including Brazil, India, Malaysia, and Taiwan (IASP, 2024).

ond, the programme expanded in a staggered fashion across districts over more than two decades, reaching over 110 districts by 2026 and generating the timing variation we exploit.[2] Third, we draw on administrative microdata that allow us to observe firm-level technological upgrading directly, rather than through productivity residuals. We link balance sheets, employment and occupational records, transaction-level customs data at the 6-digit product level, and a complete census of domestic business-to-business invoices, covering the near-universe of non-financial firms from 2009 to 2023. This combination of sources makes it possible to trace how firms' input sourcing, export composition, technology adoption, and workforce composition respond to a nearby park opening.

To characterise the treatment itself, we further draw on our own survey of operational technoparks conducted in partnership with the Technology Development Zones Association, documenting park objectives, tenant selection criteria, and the deliberate promotion of linkages with surrounding firms. Our analysis focuses throughout on incumbent firms already present in a district before its technopark opened, which are by construction not the park's original tenants, so any effect we estimate is a spillover to the surrounding economy.

The identification challenge is that technoparks are not allocated randomly: districts that receive a park tend to be denser, more economically active, and closer to universities than those that do not. Our preferred design addresses this by combining propensity-score matching with difference-in-differences, pairing each of the 40 treated districts with the most similar untreated district in the same NUTS-2 region and comparing within-firm changes before and after the opening across 245,000 firms and approximately 2.37 million firm-year observations. A complementary all-district specification retains the full national rollout, with identification coming from within-NUTS-2-region-year variation conditional on baseline district characteristics interacted with year dummies, and draws on over 8 million firm-year observations. Both designs produce flat pre-trends and closely similar post-treatment estimates under both conventional two-way fixed effects and the imputation estimator of Borusyak et al. (2024), which is robust to heterogeneous treatment effects in staggered settings.

[2]Early parks were concentrated in major cities such as Ankara, Istanbul, and Izmir, before the programme spread progressively to smaller cities and more peripheral districts.

We find that technopark establishment generates broad-based technological upgrading among incumbent firms in the host district. Following Verhoogen (2023), we organise our main outcomes around three observable dimensions of upgrading: entry into technology-intensive domestic trade, entry into technology-intensive export markets, and firm productivity.[3] On the domestic trade side, treated firms become significantly more likely to buy from and sell to high-technology counterparties, a joint movement on both the input and output sides consistent with input–output quality complementarity (Kugler and Verhoogen, 2012). On the international trade side, the probability of exporting high-R&D and high-technology goods rises by around 4 per cent relative to matched controls. These changes are accompanied by measurable performance gains. Total factor productivity (TFP) rises by 2.9 per cent and revenue per employee by 2.3 per cent, with both effects building gradually over the first three to four years after opening, consistent with the time required for knowledge spillovers to diffuse and firms to reorganise production.

To understand how upgrading occurs, we examine three channels. Automation adoption, measured through imports of industrial robots, numerically controlled machines, and related capital goods, rises by 8.1 per cent. The high-technology share of firms' workforces rises by 3.8 per cent, indicating that firms simultaneously upgrade the technology they use and the workers they employ. Firms also reorient their domestic production networks toward more technology-intensive suppliers and customers, accumulating significantly more high-technology trading relationships both within and across districts. The gains are concentrated among larger firms, consistent with absorptive capacity arguments (Cohen and Levinthal, 1990).

These gains are not confined to the treated districts. Because firms trade across district lines, we test whether a technopark's effects travel along supply chains, using a buyer–supplier exposure design in which a firm's exposure is the share of its sales to, or purchases from, treated districts, with the trading network held fixed at its pre-treatment composition. Production-network theory predicts that a demand shock of this kind travels upstream, so the effect should reach firms that supply treated-district customers and not those that buy from treated-district suppliers. This asymmetry makes supplier exposure

[3]We also study the intensive margins of both domestic and international trade, finding results consistent in sign and significance, which we report in the Appendix.

a placebo within the same regression, turning the design into a falsification test. The prediction holds. Firms more exposed to treated customers raise their own high-technology trade, productivity, and automation, while exposure on the supplier side shows no systematic effect. The spillover onto any single firm is a fraction of the direct effect, yet it reaches firms the policy never targeted.

We then ask whether the gains justify the cost. We compile the revenue forgone through the tax exemptions of Law No. 4691 from the annual reports of the Revenue Administration, and weigh it against the value of the incumbent productivity gain in a break-even calculation of the kind used to evaluate place-based and innovation policy. The test is deliberately demanding, crediting only the spillover onto incumbent firms outside the zone and none of the gains to the tenants inside it. Even so, the incumbent productivity gain is of the same order as the entire fiscal cost of the programme, with the point estimates placing benefits modestly above it, central benefit-cost ratios of roughly 1.2 to 1.5 across our two productivity measures, comparable to the ratios found in the leading place-based evaluations.

Related literature and contributions. Our paper connects to several strands of the literature. First, we contribute to the growing body of work on place-based policies (Juhász et al., 2024). A large literature has examined spatially targeted programmes in high-income economies with mixed conclusions (Glaeser and Gottlieb, 2008; Neumark and Kolko, 2010; Ham et al., 2011; Neumark and Simpson, 2015). More recent work has found positive effects across a range of settings: employment and wage gains from US Empowerment Zones (Busso et al., 2013); small-firm employment increases from UK regional investment subsidies (Criscuolo et al., 2019); output and productivity gains from China's Special Economic Zones (SEZs) (Wang, 2013; Alder et al., 2016; Lu et al., 2019); employment and firm-count increases from Indian location-based tax incentives (Chaurey, 2017); and job creation with positive local spillovers from Indian SEZs (Gallé et al., 2024).[4] Closer to our setting, Görg and Mulyukova (2024) use firm-level data from India to study SEZ effects on productivity and find null average results, with positive effects only for

[4]Evidence on long-run effects is more mixed. Von Ehrlich and Seidel (2018) find that a temporary place-based subsidy in West Germany raised income per square kilometre in targeted areas, but that this reflected spatial redistribution from within a 20 to 30 kilometre radius rather than new activity. Kline and Moretti (2014a) find persistent output growth from the Tennessee Valley Authority, though with some offsetting losses elsewhere.

privately owned zones. We move the focus from employment and investment responses to technological upgrading specifically, showing that a carefully designed innovation zone can lift TFP, automation adoption, and the sophistication of both exports and supply chains among incumbent firms, outcomes prior evaluations have not been able to examine.

Second, our paper is related to the agglomeration and local spillovers literature. Meta-analyses show that doubling employment density generates productivity gains of 3 to 8 per cent (Rosenthal and Strange, 2004; Combes et al., 2012), and matched-plant studies find that spillovers from large plant openings concentrate among local incumbents (Greenstone et al., 2010). Causal evidence on policy-induced spillovers to incumbent firms in middle-income economies remains limited. Our productivity and automation estimates are consistent with technoparks generating local innovation multipliers analogous to the employment multipliers identified by Moretti (2010), and our survey evidence shows that a majority of technopark managers deliberately promote linkages between tenant firms and local suppliers while reporting frequent collaboration between tenants and firms in neighbouring districts.

Third, we contribute to the literature on the geography of innovation and knowledge spillovers (Jaffe et al., 1993; Audretsch and Feldman, 1996). The efficacy of formally designated innovation clusters in middle-income economies remains largely untested. A small body of work points to the lasting importance of inherited knowledge geography: Schweiger et al. (2022) show that former Soviet Science Cities still host more innovative, higher-skilled, and higher-paid activity decades later, while Bircan and De Haas (2020) find that Russian firms' adoption of new technologies is strongest near historical centres of R&D. Both suggest that where knowledge infrastructure already exists, firms upgrade more. We provide direct evidence that place-based policy can create this infrastructure where it is absent, generating the same proximity-driven upgrading among incumbent firms, with effects on export sophistication and productivity that build over time, consistent with the gradual diffusion of localised knowledge spillovers. These findings also connect to theories of endogenous technological change and the middle-income trap (Aghion and Howitt, 1992; Acemoglu et al., 2006; Rodrik, 2016), providing micro-level evidence that place-based innovation policy can address market failures in R&D and firm upgrading in middle-income settings (Akcigit et al., 2016, 2021; Rodrik, 2007a).

Finally, our paper contributes to the firm-level upgrading literature. As highlighted in Verhoogen (2023), upgrading evidence should ideally be grounded in directly observable firm behaviour rather than residual productivity, yet most prior evaluations of place-based innovation policy have had to rely on aggregate employment and output measures for want of more granular data. Our measures of technology-intensive trade via firm-to-firm invoices, export composition via 6-digit customs records, firm productivity from administrative balance sheets, automation adoption via import codes at the 6-digit Harmonized System (HS) level, and workforce composition via occupational registers address this gap directly.[5] We also document important input-side effects, as firms shift toward procuring more technology-intensive intermediate inputs from domestic suppliers, a channel that is visible only because of the firm-to-firm invoice data.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and the staggered rollout of technoparks. Section 3 presents the data and sample construction. Section 4 sets out the estimation strategy and identification assumptions. Section 5 reports the main results on upgrading, mechanisms, robustness and heterogeneity, what distinguishes the most successful technoparks, and district-level firm entry. Section 6 documents the spillover effects through buyer–supplier networks. Section 8 presents cost-benefit calculation. Section 9 concludes.

2 Institutional Background and the Treatment

2.1 Technoparks in Türkiye

Technoparks are specialised sites that bring together firms, start-ups, universities, and research institutions. As a form of place-based industrial policy, they aim to stimulate R&D, facilitate knowledge spillovers, and support firm-level technological upgrading, and they have been widely adopted across low- and middle-income economies seeking to build high-tech industries and regional innovation capacity (IASP, 2024). Yet credible evidence on whether technoparks affect firms *outside* their gates remains scarce. This section describes the Turkish programme, the staggered rollout that provides our identifying

[5] In the taxonomy of Verhoogen (2023), technoparks operate as drivers of know-how, expanding the set of technologies and practices nearby firms can implement, and our data make it possible to observe this process at the level of individual firm decisions.

variation, and, drawing on our own survey of technopark managers, the content of the treatment itself.

Türkiye launched its technopark programme with the Technology Development Zones Law (Law No. 4691) of July 2001. The law established a single national governance framework and defined the incentives available to tenant firms, namely corporate tax exemptions on income generated from software and R&D activities, exemptions from VAT and customs duties, and reduced income tax for R&D personnel. These fiscal incentives are designed to concentrate innovation-oriented activity in close physical proximity to universities, research institutes, and other technology-intensive firms.

The fiscal incentives are only one element of a broader bundle. Technoparks also provide tenant firms with business support services, including patent assistance, incubation, grant-application support, and networking opportunities. Tenant firms themselves typically operate in technology-intensive sectors such as software, information and communication technologies (ICT), advanced engineering, biotechnology, and e-commerce.

Two features of this design matter for our analysis. First, both the incentives and the services are formally available only to tenants, so any effect on non-tenant firms must operate through indirect channels, namely demand for local inputs, labour mobility, knowledge diffusion, or demonstration effects. Second, the common legal framework means that every district receives a broadly comparable bundle, regardless of when its technopark opens.

2.2 The Staggered Rollout of Technoparks

Türkiye's technopark initiative expanded gradually over more than two decades, and this staggered rollout provides the variation underlying our identification strategy. In the early 2000s, only a handful of technoparks existed, mainly in major cities such as Ankara, Istanbul, and Izmir; by 2026, there were more than 110, spanning provinces across the country. Appendix Figure A2 shows this expansion: the bars report new openings each year, while the line traces the cumulative total. Our analysis exploits this timing.[6] We

[6]Throughout the paper, we define a technopark as operational in the year it begins hosting tenant firms and providing services, rather than the earlier year in which the zone received legal designation under Law No. 4691. Legal approval and operational launch can be separated by several years in this programme; using the operational date ensures that treatment is assigned at the point when the park can plausibly begin generating spillovers for surrounding firms.

follow firms that were already operating in a district when its technopark opened, and compare them with similar incumbent firms in matched districts that did not receive one.[7] Because these firms predate the park, they are by construction not its original tenants. Any effect we estimate is a spillover, not a composition effect driven by entry.

The local economies receiving the treatment are well positioned to absorb it. Table A1 reports the sectoral composition of firms located in treated districts, and this composition maps closely onto the upgrading margins we study. Manufacturing accounts for 20.5 per cent of firms (the natural margin for automation adoption, the use of technology-intensive inputs, and export upgrading), while wholesale and retail trade (35.1 per cent) places a large share of firms inside the domestic supply chains through which high-tech products are bought and sold. Knowledge-intensive services add a meaningful further presence: professional, scientific, and technical activities comprise 7.3 per cent of firms, and information and communication a further 2.8 per cent. Treated districts therefore host firms spanning precisely the activities through which technopark spillovers, demand for technology-intensive inputs, diffusion of automation, and trade in high-tech products, can materialise.

2.3 The Treatment

What changes when a district gets a technopark? The treatment is a bundle of fiscal incentives combined with an institutional platform for innovation, physical infrastructure, innovation services, and organised linkages to universities, local businesses, and suppliers. Düzce Teknopark, operational since 2011, illustrates this combination (Appendix Figure A3). The park provides shared workspaces, conference and training facilities, and meeting and collaboration spaces, and it organises local and international business-to-business matchmaking, entrepreneurship events, grant calls, training programmes, and technology workshops. Tenant firms thus gain not only tax advantages but also services and connections that lower the cost of innovating, finding partners, and reaching new markets.

[7] Türkiye's administrative structure consists of 12 regions (NUTS-1), 26 subregions (NUTS-2), 81 provinces (NUTS-3), and 973 districts (LAU-1). The Nomenclature of Territorial Units for Statistics (NUTS) is the European Union's system for standardising territorial divisions into three levels, with Local Administrative Units (LAUs) below these levels. A Turkish district (ilçe) functions similarly to a US county but has less local autonomy and is typically smaller in geographic and population size.

Düzce is illustrative, not exceptional. To characterise the treatment systematically, we surveyed technoparks in March–April 2026 in partnership with the Technology Development Zones Association of Türkiye. The survey asked management teams about tenant-sector composition, priority objectives, tenant selection criteria, university and R&D linkages, infrastructure and support services, networking activities, local supplier relationships, export support, innovation outputs, financing constraints, and the main reasons firms choose to locate in technoparks. An English version of the survey instrument is available here.

The survey covers 109 of the 114 Technology Development Zones in existence as of May 2026, of which 91 are fully operational, 14 in the early stages of operation, and 4 still under development. It thus spans nearly the entire national system, from long-established parks to zones only beginning to host tenant firms.

The responses show that technoparks are designed and run as targeted innovation policy rather than as generic business parks or local employment programmes. Two patterns define the treatment. First, technoparks prioritise innovation over employment objectives. In Appendix Figure A4, Panel (A), 67.9 per cent of technoparks report R&D and innovation among their top three objectives, followed by technological upgrading (57.8 per cent) and productivity growth (55.0 per cent). Traditional regional-policy objectives rank well below: regional development is cited by 34.9 per cent of technoparks, job creation by 28.4 per cent, and attracting foreign firms by 11.9 per cent. Second, tenant selection is closely aligned with this mission. Panel (B) shows that the three most frequently reported selection criteria are technological intensity or R&D focus (57.8 per cent), sectoral fit with the park's specialisation (52.3 per cent), and growth potential or scalability (43.1 per cent). By contrast, space availability (11.9 per cent) and financial stability (11.0 per cent) rank near the bottom. Technoparks select tenants on the basis of their technological orientation, not to fill available space.

The survey also reveals how the treatment can reach firms outside the park. Appendix Figure A5, Panel (A), shows that university–firm linkages are operationally pervasive, even though few technoparks list university linkage as a headline objective: 59.6 per cent facilitate student internships and an equal share host faculty or laboratory presence in the park, 52.3 per cent report joint R&D projects, 51.4 per cent place graduates at

tenant firms, and 45.9 per cent provide shared equipment or facilities. Panel (B) shows that technoparks deliberately build connections beyond their own tenants. Fully 89.0 per cent report intentionally promoting linkages between tenant firms and local suppliers, and the same share report product-development collaboration across technoparks; 85.3 per cent report such collaboration within the park. In addition, 59.0 per cent report that tenant firms frequently collaborate with firms in nearby districts, and 53.5 per cent report frequent collaboration among tenants themselves. These figures are managers' self-reports and may present the parks favourably; we therefore use the survey only to describe what the treatment involves, and base all causal estimates on the administrative data.

The treatment we study is therefore the arrival of this bundle, delivered by a selective, innovation-focused institution that subsidises R&D, provides shared infrastructure and services, and organises linkages between tenants, universities, local suppliers, and surrounding firms. Our question is whether its arrival changes the behaviour of firms already located in the district when the park opens. If technoparks work as designed, their footprint should appear not only in firm entry or employment but in the technological upgrading of these incumbents, visible in high-tech domestic purchases and sales, export sophistication, automation-related imports, productivity, and local firm networks. These are precisely the outcomes we take to the administrative data.

3 Data

We use a unique firm-level panel covering the near-universe of registered non-financial firms in Türkiye over 2009–2023, drawn from administrative records linked at the Central Bank of the Republic of Türkiye (CBRT) through anonymised identifiers.[8] The data bring together four sources: annual balance sheets and income statements from the Tax Authority, the universe of domestic firm-to-firm invoices, transaction-level customs records from the Ministry of Trade, and employment registers from the Social Security Institution (SGK). Linking them lets us follow the same firm over time across its production accounts, its domestic trading partners, its international transactions, and its workforce. A defining feature of these data is that they record the concrete choices firms make. This lets us

[8]We exclude financial-sector firms (NACE K) and public administration (NACE O), as our focus is on non-financial corporations.

measure firm-level upgrading from directly observed outcomes, namely the inputs firms buy, the products they export, and the technologies they adopt, rather than from the productivity residuals or survey responses on which much of the upgrading literature has had to rely (Verhoogen, 2023).

3.1 Estimation Sample

We restrict the sample to pre-existing firms, defined as firms observed at least once before a technopark opens in their district. The restriction applies symmetrically: firms in treated districts must appear before the technopark's opening year, and firms in control districts before the opening year of their matched treated district. This yields a comparable incumbent population on both sides of the comparison and serves three purposes for identification. It guarantees pre-treatment observations, which we need to test for parallel trends and trace dynamic effects. It rules out selection through entry, so post-treatment changes cannot reflect new firms drawn in by the policy. And because incumbents cannot be a technopark's original tenants, it ensures that our estimates capture spillovers to the surrounding economy rather than direct effects on park occupants.

Beyond this requirement, the estimation sample reflects one design choice and three standard screens. The design choice is to keep only firms in treated districts and their matched control districts (see Section 4), so that every comparison is made between observably similar local economies. The screens then ensure data quality. We drop firm-year observations with zero or missing total assets or gross sales, which signal economic inactivity, and a small number of firm-years whose administrative records lack a valid district identifier.[9] Finally, we require each firm to be observed for at least three years, so that every firm contributes meaningful within-firm variation.[10] The sample window runs from 2009, when the financial and transaction records become consistently available and the main phase of technopark expansion begins, through 2023, with districts whose technoparks opened before 2009 contributing no identifying variation in the baseline de-

[9] A firm reporting no assets and no sales in a given year is not meaningfully operating, so these firm-years carry no economic information for our outcomes; in practice they cluster at the start or end of firms' observed spells, consistent with entry and exit rather than with the removal of active observations. Our results are robust to retaining them.

[10] Our results are robust to tightening this requirement to five or six years and to dropping it altogether (Section 5.3).

sign. After these restrictions, the panel comprises roughly 245,000 unique firms and 2.37 million firm-year observations.

Our estimation sample is unbalanced, since we follow each firm for as long as it appears in the data rather than keeping only firms present throughout. This is deliberate as it preserves the entry and exit dynamics that are themselves of interest. It does, however, raise the concern that selective exit could shape our estimates, which we address by confirming that our main results hold on a balanced panel of firms observed in every year (Section 5.3).

3.2 Firm-Level Outcomes

We measure technological upgrading using outcomes drawn from across these linked sources. We assign each firm to a district using its registered headquarters.[11]

Main upgrading outcomes. Our headline analysis centres on two observable signatures of upgrading: what firms trade and how productively they operate. We begin with trade. The firm-to-firm invoice database records all business-to-business transactions above TL 5,000, giving a near-complete map of the domestic supply chain at a granularity rarely available. From these records we build two indicators. *High-Tech Domestic Purchases* equals one if a firm sources inputs from at least one technology-intensive domestic supplier (for example, a machinery manufacturer, a software firm, or an electronics producer) in a given year. *High-Tech Domestic Sales* equals one if a firm sells to at least one such counterparty. Together these indicators capture whether incumbent firms become embedded in technology-intensive domestic value chains, consistent with input–output quality complementarity whereby firms producing higher-quality goods simultaneously demand higher-quality inputs (Kugler and Verhoogen, 2012).[12]

[11]For multi-establishment firms, all financial outcomes (sales, assets, value added, TFP) are recorded at the firm level and attributed to the headquarters district using Tax Authority location records. We do not allocate balance-sheet or income-statement variables to individual establishments; our estimates should be interpreted as effects on incumbent firms *headquartered* in technopark districts, not as establishment-level outcomes. This distinction matters primarily for large multi-site firms; for the small and medium-sized firms that predominate in our sample, headquarters location is typically the sole or principal operating location. We partially address this through a robustness check, which assigns each firm to the district in which it employs the largest share of its workforce in a given year; results are qualitatively identical under this assignment.

[12]The high-technology classification uses the 2-digit NACE sector of the counterparty firm, following Eurostat's technology-intensity taxonomy, rather than the product code of the specific invoice; we there-

The customs records, covering the universe of exports and imports at the 6-digit HS level, yield two analogous export measures, *High-R&D Exports* and *High-Tech Exports*, which capture whether firms shift their export baskets toward more sophisticated products.[13] On the productivity side, we estimate firm-level total factor productivity from the financial statements following Olley and Pakes (1996), and we measure revenue per employee as gross revenue relative to baseline (2009) employment; together these reveal whether upgrading translates into efficiency gains rather than mere expansion. These six outcomes are the focus of our main analysis and event studies.

For the trade outcomes we focus on the extensive margin, namely whether a firm participates in technology-intensive trade at all, for three reasons. First, the extensive margin is where we expect technopark spillovers to bite. Knowledge diffusion, supplier introductions, and demonstration effects should first move incumbents into new technology-intensive relationships and products, before affecting how much they transact within them. Second, the extensive margin is defined for every firm in every year, whereas transaction values are observed only conditional on participation, so intensive-margin estimates condition on an endogenously selected sample. Third, participation indicators are far less sensitive than values to the lumpiness of individual contracts and shipments, which makes them more robust measures of a genuine shift in firm behaviour. Interference between units points the same way: a violation of the stable-unit-treatment-value assumption, in which spillovers reach control-group firms, would contaminate transaction values more than participation indicators, biasing intensive-margin estimates more than the extensive margin we emphasise.

We report the corresponding intensive-margin results, which are consistent in sign and significance, in the Appendix. Intensive-margin outcomes are defined as the value of

fore interpret these indicators as measuring technology-intensive linkage formation rather than product-level upgrading.

[13] *High-R&D Exports* follows the taxonomy of Galindo-Rueda and Verger (2016), which classifies HS-6 products by the R&D intensity of the industry in which they are predominantly produced; high-R&D products include categories such as pharmaceuticals, aerospace equipment, and scientific instruments. *High-Tech Exports* follows the OECD/Eurostat technology-intensity classification mapped to HS-6 codes, covering sectors such as computers and electronics, electrical machinery, and chemicals. The two classifications are related but not identical: the R&D-intensity taxonomy ranks products by the R&D expenditure of their producing industry, while the OECD/Eurostat classification groups industries by broader technology intensity; using both provides reassurance that the export upgrading result is not sensitive to the choice of classification.

domestic transactions or exports in the corresponding technology-intensive category and estimated using Poisson pseudo-maximum-likelihood (PPML), which accommodates the large share of zeros in trade values and yields coefficients interpretable as semi-elasticities.

Mechanisms and other outcomes. To understand how upgrading occurs, we use the same data for a set of complementary outcomes. From the customs records we measure *automation adoption*, an indicator for whether a firm imports automation-related capital goods such as industrial robots, computer numerically controlled (CNC) machines, and automated guided vehicles, following Acemoglu and Restrepo (2022). From the SGK registers, which report employment and the wage bill for every firm and, from 2012 onward, occupational detail, we measure high-tech employment and its workforce share alongside total employment and wages, testing whether upgrading reshapes labour demand and skill composition. [14] Finally, the invoice data let us trace firms' network position, counting distinct suppliers and customers within and across districts and by technological intensity, to assess whether technoparks reposition incumbents within domestic production networks. Together with district-level firm entry (Section 5.5), these outcomes help distinguish the channels through which technopark spillovers reach incumbent firms.

3.3 Regional Data

We compile a set of 2009 district covariates for the matching and regression analyses. Official records from the Ministry of Industry and Technology give the operational year and district of each technopark; we use only those opening after 2009.[15] From TurkStat we obtain baseline demographics, namely population density, population growth, average household size, the male-to-female sex ratio, and the youth share. Aggregating our firm-level data to the district level, we measure economic activity through the number of active firms, total firm sales, and total employment in 2009. We proxy local financial development with the number of active bank branches in 2009, from the CBRT credit registry, capture political alignment through the ruling party's margin of victory in the 2009 local elections, from the Supreme Electoral Board, and include district nighttime

[14] The SGK registers cover formal employment; workers in informal arrangements are not observed, so these results are best interpreted as effects on formal high-tech employment.

[15] In three districts a second technopark opened after 2009; we retain the earliest opening.

light intensity from satellite imagery as a further proxy for economic activity.

We use these covariates in both designs. In the matched design they select, for each treated district, a comparable untreated district and provide the basis for assessing balance. In the all-district design we interact them with year dummies, allowing districts with different 2009 characteristics to follow different trajectories. Monetary and count variables are log-transformed to reduce skewness, and all covariates are fixed at their 2009 pre-treatment values.[16] Appendix Table A2 reports all variables with their definitions and sources.

4 Estimation Strategy

Technoparks are not allocated randomly across districts. Districts that receive a technopark tend to be more urban, denser, and more economically active than those that do not, so a naive comparison of firms in technopark and non-technopark districts would confound the effect of the policy with pre-existing differences and divergent trajectories. We address this with two complementary designs. Our preferred design combines propensity-score matching with a difference-in-differences (DiD) estimator, comparing each treated district only to an observably similar control district in the same region. As a complement, we estimate an all-district specification that exploits the full national rollout of technoparks while flexibly controlling for differential trends (and therefore uses the full firm panel of more than 8 million firm-year observations, against the 2.37 million in the matched sample). The two designs rest on different identifying assumptions and use different comparison groups, and finding similar effects across both is reassuring. We explain the details of each design below and discuss the identifying assumptions they require.

4.1 Matched Difference-in-Differences

Constructing the matched sample. Using the 2009 district covariates described in Section 3.3, which capture demographic, economic, financial, and political conditions, we implement one-to-one nearest-neighbour propensity-score matching without replace-

[16] Population density, active firms, total sales, total employment, bank branches, and nighttime light intensity are log-transformed, with a small constant added where needed to handle zeros.

ment.[17] For each treated district we select the control district with the closest propensity score *within the same NUTS-2 region*, so that treated and control districts are matched both on observables and on broad geography.[18] This within-region matching absorbs regional confounders that vary across, but not within, NUTS-2 areas. We restrict attention to technoparks that became operational after 2009, the first year of our panel, so that treatment status changes during the sample period.[19] The procedure yields 40 treated districts paired with 40 control districts. Our estimates therefore speak to the effect of technopark entry for the kinds of districts treated after 2009, not to the first-generation parks established earlier in Ankara, Istanbul, and Izmir.

As mentioned above, our analysis focuses on incumbent firms located in the broader treated *district*, not inside the technopark itself. The technopark occupies a small designated site within the district; the surrounding district is where the spillover effects we estimate accrue. Every firm in our estimation sample predates the park opening and has no entitlement to the fiscal incentives available only to park tenants. Figure 1 maps the matched sample, with treated districts in red and control districts in blue, and confirms broad and balanced spatial coverage across Türkiye.

Table A3 reports covariate balance before and after matching. Before matching, treated districts differ sharply from the average untreated district, with significantly higher population density, population growth, firm activity, employment, bank presence, and nighttime light intensity. After matching, none of these differences remains statistically significant, and the normalised differences do not exceed the conventional 0.25 threshold for any covariate.

[17] We match at the district level because technopark openings are district-level policy shocks and identifying variation comes from the timing of treatment across districts. Matching at the level of treatment assignment also aligns the design with the level at which spillovers and inference are defined. Firm-level observables are used separately to assess within-pair balance, as reported below.

[18] For districts treated in the late 2010s or early 2020s, baseline characteristics measured in 2009 may imperfectly capture the local economy immediately before treatment. We partly mitigate this through matched-pair-by-year fixed effects, which absorb year-specific shocks common to each pair regardless of timing, and through the pre-trend tests that directly assess whether treated and control districts were diverging before treatment. Matching on characteristics measured closer to the treatment year would provide a useful robustness check. However, implementation is complicated by the staggered timing of treatment: different cohorts have different pre-treatment windows, so defining a common pre-treatment baseline year across all cohorts is not feasible without additional assumptions.

[19] Districts hosting a technopark before 2009 do not provide timing-based identifying variation and are excluded from the matched control pool.

Balance also holds at the firm level. Table A4 regresses an indicator for eventual technopark location on a firm's 2009 characteristics, chosen to span the dimensions along which firms most plausibly differ in ways that could shape their later upgrading: scale (firm size and employment), capitalisation and leverage (equity and current liabilities), liquidity (current assets), and market position (sales). If technoparks were placed where firms were already larger, better capitalised, or more liquid, these characteristics would predict treated-district location and threaten our comparison. They do not. The coefficients are individually small and statistically insignificant, and jointly insignificant ($p = 0.64$), confirming that, within matched districts, treated and control firms were comparable before technoparks opened.

Specification. At the firm level, we estimate the following specification using the matched sample:

$$Y_{idt} = \beta \, Technopark_{dt} + \delta_i + \delta_{pt} + \varepsilon_{idt}, \tag{1}$$

where Y_{idt} is the outcome for incumbent firm i in district d and year t. As discussed above, Y_{idt} ranges over our main upgrading outcomes, namely firms' participation in technology-intensive domestic trade and exports and their productivity, as well as the mechanism outcomes introduced earlier. The treatment $Technopark_{dt}$ equals one once a technopark is operational in district d by year t and zero otherwise. The coefficient of interest, β, is the average effect of technopark openings on incumbent firms in treated relative to matched control districts. Standard errors are clustered at the district level.

Two sets of fixed effects are central to the design. Firm fixed effects, δ_i, absorb all characteristics that do not change over time within a firm, including its initial size, sector, location, and persistent differences in management or productivity. This means that the estimates are not driven by permanent differences between firms in treated and control districts. Instead, identification comes from changes in the same firm's outcomes over time.

Matched-pair-by-year fixed effects, δ_{pt}, absorb shocks that affect both districts in a matched pair in the same year. These may include regional demand conditions, changes in infrastructure, local labour-market shocks, or other policies introduced at the regional level. This is important because treated and control districts may be exposed to common

shocks even after matching. Since each pair-year cell contains one treated district and its matched control, treatment status still varies within the cell. The coefficient β is therefore identified by comparing how outcomes change after the technopark opens in the treated district relative to the change over the same period in its matched control.

This structure has two main advantages. First, each treated district is compared only with a similar control district from the same region. Comparability is therefore built into the research design, rather than relying on a regression to adjust for differences between districts that may have little in common. Second, districts that have already opened a technopark are never used as controls for districts treated later. This rules out the "forbidden comparisons" that can arise in pooled two-way fixed-effects models, where already-treated units may serve as controls even though their outcomes are already affected by the policy. Such comparisons can bias the estimates when treatment effects differ across districts or evolve over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Overall, our approach therefore combines two features of the existing literature. We first match each treated district to a comparable control district, following Card et al. (2008), and then combine a set of clean, pair-specific difference-in-differences comparisons, as in Cengiz et al. (2019) and Deshpande and Li (2019).

Event-study estimates. Identification requires that treated districts and their matched controls would have followed similar trends without a technopark opening. Matching makes this more plausible by pairing districts with similar pre-treatment characteristics. We assess this using the following event-study specification:

$$Y_{idt} = \sum_{\ell \neq -1} \beta_\ell \mathbf{1}[t - T_d = \ell] + \delta_d + \lambda_t + \epsilon_{idt} \tag{2}$$

where Y_{idt} is the outcome for firm i in district d and year t, T_d is the year in which a technopark opens in district d, and ℓ denotes event time. The year before opening, $\ell = -1$, is the omitted category. The specification includes district fixed effects, δ_d, which absorb time-invariant differences across districts, and year fixed effects, λ_t, which capture shocks common to all districts in a given year. We bin the endpoints because the number of observations becomes smaller at longer leads and lags.

The pre-treatment coefficients, $\beta_{\ell \ell < -1}$, show whether treated and control districts fol-

lowed similar trends before a technopark opened. The post-treatment coefficients trace how the effects develop over time. We expect the response to emerge gradually as firms adopt new technologies, reorganise production, and establish new relationships with suppliers and customers.

We estimate equation (2) using two approaches. The first is the conventional two-way fixed-effects estimator. In settings with staggered treatment timing, this estimator can produce misleading estimates when treatment effects differ across cohorts or change over time. We therefore also use the imputation estimator of Borusyak et al. (2024), referred to as BJS, which estimates counterfactual outcomes using untreated observations and then aggregates the resulting cohort-year treatment effects. The two estimators produce similar results across our main outcomes. The pre-treatment coefficients are generally small and statistically insignificant, while the post-treatment estimates are similar in both timing and magnitude. We discuss these results in greater detail below.

4.2 All-District Specification

By restricting the comparison to matched pairs, the matched design achieves close comparability but uses only a small number of districts. We complement this with an all-district specification in the spirit of Lu et al. (2019) that retains every eligible district and instead absorbs baseline differences through flexible controls,

$$Y_{idt} = \beta \, Technopark_{dt} + \delta_i + \delta_{rt} + (X_{d,2009} \times \lambda_t)'\theta + \varepsilon_{idt}, \tag{3}$$

where δ_i are firm fixed effects, δ_{rt} are NUTS-2-region-by-year fixed effects, and $X_{d,2009} \times \lambda_t$ interacts the vector of 2009 district covariates from Section 3.3 with year fixed effects. The region-by-year effects absorb shocks common to a NUTS-2 region in a given year, such as regional demand, credit, or infrastructure changes, and the covariate-by-year interactions let districts that were initially larger, denser, faster-growing, more financially developed, or more politically aligned follow their own trajectories. Identifying variation comes from firms in districts that switch from untreated to treated status, relative to firms in not-yet-treated and never-treated districts. Standard errors are clustered at the district level.[20]

[20] A small number of districts hosted a technopark before 2009. Their treatment status does not change within the sample and so contributes no identifying variation, but we retain them in the estimation sample.

The event-study analogue replaces $\beta\, Technopark_{dt}$ in equation (3) with the event-time indicators of equation (2).

The identifying assumption here is conditional parallel trends, namely that given firm fixed effects, region-by-year shocks, and flexible time paths by baseline characteristics, treated-district firms would have evolved like untreated-district firms absent the opening. This differs from the matched design in what it asks of the data. Its advantage is that it does not depend on the particular matched controls and uses substantially more variation in the timing and geography of adoption, so it speaks to whether the effects reflect the national programme rather than one selected sample. Its trade-off is that the comparison group is more heterogeneous, so it places greater weight on the baseline controls in capturing differential trends. The two designs are therefore complementary.

5 Results

We organise our results in five steps. We first present our main findings on technological upgrading and productivity, reporting average effects and corresponding event studies side by side from the matched and all-district designs, with extensive-margin trade results in the main text and intensive-margin estimates in the Appendix. We then turn to the mechanisms behind these effects, examining automation adoption, the skill composition of firms' workforces, and their position in domestic production networks. Third, we assess the robustness of our findings and document how effects vary across firms. Fourth, we ask what distinguishes the technoparks that drive the average effect, relating each park's estimated effect to its management, geography, scale, and other characteristics. Fifth, we examine district-level firm entry, asking whether technoparks are followed by greater local firm formation. A separate section then asks whether the effects travel beyond the treated district, to the out-of-district trading partners of treated firms.

5.1 Firm Upgrading

High-tech domestic value chains. We first ask whether technopark openings draw incumbent firms into more technology-intensive domestic value chains. Panel A of Table 1 reports the extensive-margin estimates. Treated firms become 1.0 percentage point more likely to purchase high-tech inputs (a 3.5 per cent increase relative to the control mean

of 28.8 per cent), and equally more likely to sell into high-tech markets (a 3.8 per cent increase relative to a control mean of 26.1 per cent). This joint movement on the input and output sides is consistent with input–output quality complementarity, whereby firms producing higher-quality output simultaneously source higher-quality inputs (Kugler and Verhoogen, 2012).[21] The event study in Figure 2 shows flat pre-trends under both the TWFE and BJS estimators, with effects emerging only after the opening and building steadily thereafter.[22] The intensive margin tells the same story. The value of high-tech purchases and sales rises by 3.5 per cent and 2.4 per cent respectively (Table A5, Panel A).

Technology-intensive exports. We next turn from domestic linkages to international markets, asking whether firms also upgrade what they export. Panel B of Table 1 shows that technopark establishment raises firms' propensity to export high-R&D and high-tech products by 0.3 percentage points each, increases of 4.1 per cent and 4.3 per cent relative to their control means. The event study in Figure 3 again shows flat pre-trends under both estimators and gradual post-treatment growth, consistent with the time firms need to develop new products and establish foreign market connections. Confidence intervals widen at the longest horizons, reflecting the smaller sample that identifies these coefficients rather than any instability in the point estimates.[23] The intensive margin confirms the pattern, with high-R&D and high-tech export values rising by 5.3 per cent and 4.1 per cent (Table A5, Panel B).

Productivity. If these changes in trade reflect genuine upgrading rather than relabelling, they should ultimately register in firm performance. We therefore examine total factor

[21] One might ask whether the purchase and sale indicators double-count the same underlying transaction. They do not. Each observation is a firm-year, and we estimate whether a given incumbent becomes more likely to make any high-tech domestic purchase or sale after treatment. Firm fixed effects ensure identification comes from within-firm changes over time, so any mechanical correlation between a firm's purchase and sale indicators is absorbed.

[22] As a complement, Appendix Figures A6–A8 report imputation-estimator event studies on the full sample of districts, with district and NUTS-2-region-by-year fixed effects, no baseline covariates, and weighting by 2009 gross sales; they display the same flat pre-trends and post-opening gains across the domestic-trade, export, and productivity outcomes.

[23] Because adoption is staggered and the sample window ends in 2023, only firms in early-opening districts are observed three and four years after technopark opening. The long-horizon coefficients are therefore identified off these early cohorts alone, leaving fewer treated firm-years and fewer contributing districts than at shorter horizons and inflating the cluster-robust standard errors. For the imputation estimator the effect is more pronounced, since its +4 coefficient is unbinned and its imputed counterfactual relies on the not-yet-treated and never-treated pool in later calendar years, which shrinks as more districts adopt.

productivity and revenue per employee, measured from firms' financial statements for the same pre-existing firms in matched treated and control districts.

Table 2 reports the results. Column (1) shows a 2.9 per cent increase in total factor productivity.[24] Two considerations bear on the TFP estimate. First, the Olley–Pakes estimator requires non-missing capital and investment data, producing a smaller and potentially selected sample (2,088,303 observations versus 2,377,439 in the trade regressions). If technopark exposure affects firm survival or financial-statement reporting in ways correlated with productivity, the TFP estimate could partly reflect selection as well as genuine upgrading. The Olley and Pakes (1996) estimator explicitly models the survival decision in the first stage, which partially addresses this concern; nevertheless, we treat the TFP result as corroborative evidence rather than the primary productivity measure. Second, and reassuringly, the revenue-per-employee estimate, which is free of production-function assumptions, points in the same direction with a similar magnitude (2.3 per cent versus 2.9 per cent), suggesting the productivity pattern is not an artefact of the sample restriction. Panel B of Figure 4 traces the dynamics. The effect builds over the first three post-opening years before stabilising, consistent with the gradual diffusion of knowledge spillovers and the time required for organisational adaptation.

These are economically meaningful magnitudes. The literature on exporting firms finds that firms raise their own productivity by roughly five to ten per cent when they begin to export (Van Biesebroeck, 2005; De Loecker, 2007). Our estimates instead capture an indirect channel, the productivity spillover from a nearby technopark onto local firms, and a spillover of around three per cent is considerable for an effect that operates at arm's length rather than within the firm itself.

All-district design. Our findings do not depend on the matched sample. Table A6 re-estimates the main upgrading results on the full national panel of more than 8 million firm-year observations, replacing matched-pair-by-year fixed effects with NUTS-2-region-by-year fixed effects and interactions between year fixed effects and the 2009 district covariates of Table A2 (see equation (3)). The coefficients are of similar magnitude and remain statistically significant across all outcomes. We treat the matched design as our

[24] TFP is estimated using gross sales as output, with long-term tangible fixed assets, inventories, and the number of employees as inputs. All monetary values are deflated using Türkiye's Producer Price Index, and TFP estimates are winsorised at the 2nd and 98th percentiles.

primary specification, because it delivers the most credible counterfactual by construction and avoids forbidden comparisons, while the all-district design trades comparability for statistical power; and the two designs agree closely. We therefore use the matched sample for the remaining mechanism and robustness analyses.

5.2 Mechanisms

The trade and productivity results show that incumbent firms in treated districts upgrade. We now ask how. If this upgrading is real, it should be visible in the way firms produce, in whom they employ, and in whom they trade with. We examine these channels in turn, looking first at the technology firms adopt and the workers they hire, and then at how they reposition themselves within domestic production networks.

Automation and workforce composition. A natural first place to look is firms' production technology and their workforce, since adopting more advanced technology and hiring the workers to operate it are the most direct expressions of upgrading. We measure technology adoption through firms' imports of automation-related capital goods. Following the taxonomy of Acemoglu and Restrepo (2022), extended to include 3-D printers as in Domini et al. (2022), we classify a firm as adopting automation in a year if it imports capital goods such as industrial robots, numerically controlled machines, or automated welding and conveyor equipment.[25] Column (1) of Table 3 shows that technopark establishment raises the probability of importing such goods by 0.13 percentage points. Against a control mean of 1.6 per cent, this is an 8.1 per cent increase, the largest proportional effect we estimate.

The low base rate reflects the selectivity of the automation-import margin. Importing industrial robots, CNC machinery, or automated capital goods is a substantial investment decision that only a small fraction of firms undertake in any given year. The 8.1 per cent proportional increase therefore represents a meaningful expansion of the technology frontier accessible to incumbent firms. This is important in the Turkish context, where many incumbent SMEs are unlikely to adopt classic industrial robots but may realistically

[25]The 6-digit HS codes used include industrial robots (847950); dedicated machinery (847989); numerically controlled machines (845600–846699, 846820–846899, 851511–851519); automatic welding machines (851521, 851531, 851580, 851590); weaving and knitting machines (844600–844699, 844700–844799); other textile-dedicated machinery (844400–844590); automatic conveyors (842831–842839); automatic regulating instruments (903200–903299); and 3-D printers (847780).

invest in CNC tools or other flexible automation equipment.

Upgrading production should, in turn, change whom firms hire. We measure the technological intensity of a firm's workforce using occupational records from the SGK, which identify each worker's occupation from 2012 onward.[26] Columns (2) and (3) of Table 3 show that technopark establishment raises the probability of employing at least one high-tech worker by 0.8 percentage points (2.9 per cent relative to the control mean) and the high-tech share of the workforce by 0.29 percentage points (3.8 per cent relative to the control mean). The simultaneous increase in automation and skilled employment is consistent with technology–skill complementarity in production, and indicates that technoparks induce broad organisational change rather than a narrow substitution of capital for labour. Because the incumbent firms we study are not technopark tenants, they receive none of the tax exemptions granted inside the park. Importing capital goods and hiring skilled workers are real resource commitments, difficult to explain as fiscal optimisation or as transactions relabelled through park tenants for tax reasons; and the export gains, measured from product-level customs codes, are not driven by domestic tax routing at all.

Domestic production networks. A second channel concerns the technological profile of firms' trading partners. If technoparks diffuse knowledge and raise firm capabilities, treated firms should form relationships with more technologically sophisticated counterparties, not merely transact more in aggregate. To test this, we use the universe of firm-to-firm invoice records to construct, for each firm and year, the count of distinct customers and suppliers, decomposed by the technological intensity of the counterparty (high- versus low-tech, following Eurostat's 2-digit NACE classification) and by the geographic scope of the relationship (within versus across district). The geographic decomposition is motivated by the possibility that technoparks operate through distinct mechanisms at different spatial scales: within-district linkages capture local supply-chain deepening and proximity-based knowledge spillovers, while across-district linkages reflect whether treated firms become attractive counterparties to high-tech partners in broader markets, a pattern more consistent with capability upgrading than with agglomeration alone.

[26] Technology-intensive occupations follow the CEDEFOP taxonomy at the 2-digit ISCO level: science and engineering professionals (ISCO 21), ICT professionals (ISCO 25), science and engineering associate professionals (ISCO 31), and ICT technicians (ISCO 35).

Table 4 shows that technopark establishment generates a statistically significant expansion in high-tech business linkages on both the upstream and downstream sides. On the supplier side (Panel A), treated firms accumulate approximately 3.1 per cent more distinct high-tech suppliers overall, with effects of similar magnitude both within district (2.2 per cent) and across district (3.1 per cent), each statistically significant. On the customer side (Panel B), the effects are somewhat larger: treated firms gain approximately 3.6 per cent more distinct high-tech customers overall, with comparable contributions from within-district (3.5 per cent) and across-district (2.8 per cent) relationships. The response at both margins is consistent with capability upgrading that extends firms' reach beyond the local market, rather than agglomeration alone.

The low-tech results serve as an important check on this interpretation. Under a pure scale story, in which technoparks simply increase firm size or output, linkages should expand uniformly across all partner types. Table A7 shows that this is not the case. Effects on low-tech customer linkages are small and statistically insignificant, and while low-tech supplier linkages also rise, the asymmetry is clearest on the customer side. This asymmetry therefore points to a selective reorientation of production networks toward technology-intensive partners, rather than a mechanical expansion driven by firm growth alone. Taken together, the results in this section indicate that treated firms restructure their value chains toward technology-intensive partners, a pattern that complements the trade and productivity results of Section 5.1 and echoes the survey evidence that technoparks deliberately foster linkages between tenants and local suppliers (Figure A5).

5.3 Robustness and Heterogeneity

Robustness. Our main estimates are robust to a battery of alternative specifications. We implement eleven checks across all main outcomes: (i) varying the minimum observation window to four, five, or six years; (ii) adding 1-digit sector-by-year fixed effects; (iii) replacing matched-pair-by-year with province-by-year fixed effects; (iv) restricting the sample to manufacturing firms only; (v) using a balanced firm panel; (vi) dropping sole proprietorships; (vii) assigning firms to districts using an employment-based location

proxy;[27] (viii) controlling for the contemporaneous rollout of Organised Industrial Zones; (ix) controlling for other district-level industrial policy exposure; (x) excluding the pandemic and immediate post-pandemic years (2020–2022); and (xi) restricting the sample to single-establishment firms, for which the district assignment is unambiguous.

Figures A9 and A10 display the resulting coefficients for the domestic and export outcomes; across all specifications, estimates remain positive, statistically significant, and close to the baseline, confirming that our findings are not driven by sample composition, fixed-effect choice, firm-location measurement, or confounding policy rollouts.

To assess the sensitivity of our results to the choice of matched sample, we implement a leave-one-pair-out exercise in which we iteratively exclude each of the 40 matched district pairs and re-estimate the main specification on the remaining 39 pairs. Figures A11 and A12 display the resulting coefficient distributions for the domestic and export outcomes. In every one of these 40 iterations the estimates stay positive, statistically significant, and close to the baseline, so no single matched pair drives our findings.

Finally, as a falsification check, we verify that the export effects are confined to technology-intensive categories. Table A8 shows that technopark establishment has no statistically significant effect on firms' probability of exporting low-R&D or low-tech products, nor on the value exported conditional on doing so, with effect sizes below 2 per cent in all columns. This rules out the interpretation that our export findings reflect a generic boost to firm export capacity rather than genuine technological upgrading.

Heterogeneity by firm size. Not all incumbent firms are equally positioned to benefit from proximity to a technopark. Absorbing knowledge spillovers, adopting new technologies, and reorganising production all require organisational capacity and financial slack that smaller firms may lack (Cohen and Levinthal, 1990). Firm size is a natural proxy for these capabilities: larger firms tend to employ more skilled workers, invest more in R&D, and have the managerial bandwidth to act on new opportunities (Idson and Oi, 1999). We therefore interact the technopark indicator with an indicator for firms above the 75th percentile of the baseline employment distribution (more than 10 employees) to test whether the aggregate effects mask meaningful heterogeneity along this dimension.

[27] We assign each firm to the district holding the largest share of its workforce in each year, using SGK records. This alternative yields qualitatively identical results.

The gains are broad-based but stronger among larger firms. Table A9 shows that smaller firms experience positive effects on domestic high-tech trade and technology-intensive exports, statistically significant except for the high-tech export margin, but the interaction terms reveal that larger firms benefit substantially more across all outcomes. On the domestic trade margins, the additional effect for larger firms is approximately 2.1 percentage points for high-tech purchases and 2.3 percentage points for high-tech sales, roughly four to five times the baseline effect for smaller firms. The export gradient is similarly pronounced, with larger firms gaining an additional 0.6 percentage points on both high-R&D and high-tech export margins, roughly three to four times the effect estimated for smaller firms. The productivity gradient is particularly stark. We find that TFP gains are statistically insignificant for smaller firms but are 0.136 log points higher for larger ones, while revenue per employee rises by an additional 4.5 per cent for larger firms relative to their smaller counterparts. This pattern is consistent with absorptive capacity, as larger firms are better placed to translate proximity to the park into investment and organisational change, and it mirrors Verhoogen (2008), who finds trade-induced quality upgrading concentrated in larger, more capable plants.

5.4 Characteristics of Successful Technoparks

The average treatment effect masks substantial variation across technoparks. Estimating a separate effect for each park is difficult, however, because parks observed for only a few post-opening years have less precise estimates. We therefore rank parks using empirical-Bayes-adjusted effects, which shrink less precise estimates toward the overall mean.[28]

Figure 5 shows the resulting distribution of park-level effects on a composite index of local technological upgrading.[29] We can estimate separate effects for the 37 of our 40 matched parks that are observed both before and after opening. The adjusted effects range from approximately -0.30 to 0.51 standard deviations, compared with an average of 0.14. Around one-fifth are negative, while the upper quarter forms a clear right tail.

[28]The adjustment shrinks each park's raw treated-minus-control effect toward the grand mean in proportion to its estimation uncertainty, preventing a park from ranking highly because of a noisy short-run estimate. The adjusted and raw rankings are nevertheless very similar.

[29]The index is an inverse-covariance-weighted composite (Anderson, 2008) of the district-level outcomes studied above, covering domestic trade, exports, and productivity. Each park-level estimate measures the treated-minus-control change in the index from before to after opening, expressed in standard deviations.

The weaker-performing parks also tend to be smaller: 89 per cent of firms are located in parks with non-negative estimated effects. The average result therefore reflects broad gains alongside meaningful differences in performance across parks.

We next examine which park characteristics are associated with these differences. We group the available measures into six blocks: management practices, geography, the baseline local economy, scale and infrastructure, finance, and export orientation. The measures combine our survey of park managers with administrative and geographic data.[30]

Figure 6 first relates the park-level effect to each characteristic separately. The largest associations are for service breadth, governance, park scale, and networking. Their coefficients range from 0.073 to 0.089 standard deviations and are statistically significant at the 10 per cent level. By contrast, the predetermined measures of local economic conditions and geography show little systematic relationship with park performance.

Management and scale are correlated, so we also examine them jointly. A one-standard-deviation increase in the management index is associated with a 17.5-percentage-point higher probability of being in the top-performing group (permutation $p = 0.028$). Park scale has little additional explanatory power once management is included. Location is weakly negatively associated with top performance, but this estimate is imprecise and depends on a small number of parks, so we do not place weight on it.[31]

Because the characteristics overlap, we next assess their joint explanatory power. We enter the six blocks sequentially into a regression of the park-level effects and complement this exercise with an order-independent Shapley decomposition (Figure 7). Together, the observed characteristics explain approximately one-quarter of the raw variation across parks, with an adjusted R^2 of about 0.10. Since estimation error accounts for part of the observed dispersion, we estimate that only around two-thirds of the raw variation represents underlying differences across parks. On this basis, the characteristics explain approximately 37 per cent of the variation that is potentially explainable.

Management accounts for the largest share of this explanatory power. Adding the

[30]The management block covers the range of services provided to tenants, governance practices, and networking activity. The other blocks are constructed from administrative and geographic records. Full definitions appear in the Appendix.

[31]One possible interpretation is that parks outside the main economic centres offered firms greater scope to improve, but the evidence is not strong enough to distinguish this explanation from other regional differences.

management block raises the regression R^2 by 11 percentage points, and it is the only block that is statistically significant when entered on its own (coefficient 0.104; pair-clustered $p = 0.017$). The Shapley decomposition assigns management 59 per cent of the explained variation, followed by scale and infrastructure at 18 per cent, export orientation at 11 per cent, finance at 6 per cent, geography at 5 per cent, and the baseline economy at 1 per cent. Adding NUTS-2 region fixed effects raises the unadjusted R^2 to approximately 0.78 but the adjusted R^2 only to 0.29. Regional location therefore captures some remaining variation, although much of the increase in in-sample fit reflects the large number of regional indicators.

Finally, Figure 8 examines the timing of these differences. We divide parks at the median of the management index and plot the treated-minus-control gap for each group over event time, normalised to the year before opening. The two groups follow similar paths before opening but diverge afterward: outcomes improve in the better-managed parks, while they remain flat or decline in the other group. We also find no systematic relationship between the observed characteristics, including management, and pre-opening trends in the placebo analysis.

These results point to management as the park characteristic most consistently associated with stronger performance, but they should not be interpreted as causal estimates of management quality. The manager survey was conducted after the parks opened, so management practices may partly reflect earlier park performance or other unobserved local conditions. The absence of differential pre-trends reduces this concern but cannot eliminate it. The Appendix reports the individual management measures, park-level scatterplots, and robustness across alternative specifications.

5.5 District-Level Firm Entry

Our main analysis deliberately holds the firm population fixed, studying only incumbents present before the opening. As a complement, we now widen the lens to ask whether technoparks also reshape the district-level firm population by attracting new entrants. This question matters both for the external validity of our incumbent results and for assessing the aggregate local effects of technopark policy.

Using Revenue Administration records, we estimate

$$FirmEntry_{dt} = \gamma\ Technopark_{dt} + \delta_d + \delta_{rt} + \varepsilon_{dt}, \tag{4}$$

with district fixed effects δ_d, NUTS-2-region-by-year fixed effects δ_{rt}, and standard errors clustered at the district level. Because our incumbent sample is defined as firms present before the opening year, any entry response leaves $\hat{\beta}$ in equation (1) unaffected by construction, so the two analyses are complementary rather than overlapping.

Appendix Figure A13 traces the dynamics of equation (4) using the full national panel of districts. Under both the conventional two-way fixed-effects and imputation estimators, the pre-opening coefficients are statistically indistinguishable from zero, while the post-opening estimates rise steadily. The timing provides evidence that the entry response emerges only after the technopark becomes operational.

6 Spillovers Through Buyer–Supplier Networks

The analysis so far has treated each district as a self-contained unit, asking how a technopark changes the firms located around it. Firms trade far beyond their own district, so if a technopark raises the capabilities of the firms inside it, those gains need not stop at the district boundary. They can travel along supply chains, from upgraded firms to the partners that buy from and sell to them. This section asks whether a firm's own upgrading responds to how much of its trading network becomes technopark-treated. The question matters for two reasons. It tells us whether the benefits of the policy reach beyond the treated districts through ordinary market linkages, and it bears on how we read the main estimates, because a control-district firm that trades with a treated district is itself partly exposed to the treatment.

6.1 The Direction of the Spillover

Previous work on production networks provides clear guidance on the direction in which these spillovers should propagate. When a shock increases a firm's demand for inputs, its effects travel *upstream* to suppliers, which gain a larger and potentially more sophisticated customer. By contrast, shocks that directly raise a firm's productive capacity may propagate *downstream* to its customers (Acemoglu et al., 2016). A technopark that induces

firms in its district to expand and upgrade therefore represents a demand shock from the perspective of their suppliers. The resulting spillovers should reach firms that *supply* treated-district firms, rather than firms that purchase inputs *from* them. Firm-level evidence showing that supplying more sophisticated buyers can raise a firm's productivity and technology adoption points in the same direction (Alfaro-Ureña et al., 2022; Javorcik, 2004).

Motivated by this literature, we use the data to examine whether the estimated effects follow the direction of propagation predicted by production-network theory. In particular, we distinguish a firm's exposure through its customers from its exposure through its suppliers. If technoparks generate upstream spillovers by increasing the scale and technological sophistication of firms in treated districts, the effects should be concentrated in customer exposure: firms should benefit when a larger share of their sales is directed to treated-district buyers. By contrast, exposure through suppliers should generate weaker or no comparable effects.

This distinction is also useful for interpreting the estimates. Alternative explanations, such as common industry shocks, regional demand growth, mean reversion, or proximity to economically expanding districts, need not systematically follow the direction in which goods flow through the production network. Such forces could therefore generate similar associations through both customer and supplier exposure. Finding that the results are concentrated in exposure to treated customers, while the estimated effect of exposure to treated suppliers is small and statistically insignificant, is consistent with the upstream propagation mechanism highlighted by the previous literature.

6.2 Measuring Exposure

To measure exposure we adapt the buyer–supplier design of Atalay et al. (2023). For each firm i and year t we build two shares from the firm-to-firm invoice records, excluding the firm's own district. *Customer exposure* s_{it}^{cus} is the share of firm i's sales made to customers located in treated districts, and *supplier exposure* s_{it}^{sup} is the share of its input purchases

sourced from suppliers located in treated districts.[32] We then estimate:

$$y_{it} = \beta\, \textit{Technopark}_{d(i),t} + \pi^{\text{cus}}\, s_{it}^{\text{cus}} + \pi^{\text{sup}}\, s_{it}^{\text{sup}} + \alpha_i + \delta_{p(i),t} + \varepsilon_{it}, \tag{5}$$

where $\textit{Technopark}_{d(i),t}$ is the own-district treatment indicator, α_i are firm fixed effects, $\delta_{p(i),t}$ are matched-pair-by-year fixed effects, and standard errors are clustered at the district level. The coefficients of interest are π^{cus} and π^{sup}, the spillovers that reach a firm through its customers and through its suppliers.

Three features distinguish these exposure measures from a firm's own treatment status. First, we exclude the firm's own district from both measures. Exposure therefore does not simply reproduce the direct effect of being located in a technopark district, which is captured separately by *Technopark*. Instead, it isolates spillovers transmitted through trading relationships with firms in other districts. Second, we calculate exposure using transaction shares fixed in each firm's baseline year. A firm's exposure can therefore increase only when the district of one of its existing trading partners becomes treated, not because the firm subsequently redirects its purchases or sales toward treated districts. By construction, the estimates exclude spillovers arising from endogenous network formation or partner switching and capture only effects transmitted through relationships that already existed before treatment. This gives the coefficients a deliberately conservative interpretation. Third, exposure is defined with respect to all firms located in treated districts, rather than technopark tenants alone. The measures therefore capture linkages to the broader local economy surrounding a technopark, including any effects that spread from tenant firms to other firms within the same district.

6.3 Results

Table 5 shows a clear difference between the two exposure measures. Greater exposure to treated customers is associated with improvements across most outcomes, whereas exposure to treated suppliers is generally small and statistically insignificant.

The pattern is strongest for technology-intensive trade. Firms whose existing customers

[32] Formally, $s_{it}^{\text{cus}} = \big(\sum_{j\in \text{Cus}(i)} \textit{Technopark}_{d(j),t}\, w_{ij}\big) \big/ \sum_{j\in \text{Cus}(i)} w_{ij}$, and symmetrically over the firm's suppliers for s_{it}^{sup}, where the sums run over counterparties j located in districts other than i's own and w_{ij} is the value of the i–j transaction in i's baseline year. The sample is restricted to firms in matched pairs containing more than three firms.

become exposed to a technopark increase their high-tech domestic purchases and sales, and export more high-R&D and high-tech products. This extends the upgrading documented within treated districts in Section 5.1 to suppliers located elsewhere. Some of the increase in high-tech sales may be compositional: when treated customers buy more sophisticated inputs, their suppliers mechanically record more sales of such products.

The results for production outcomes are therefore especially informative. Firms with greater customer exposure have higher total factor productivity and are more likely to import automation capital. The coefficient on the high-technology employment share is also positive, although not statistically significant. These outcomes reflect changes in how firms produce, not simply what they sell, and are consistent with suppliers upgrading in response to demand from treated customers.

Exposure to treated suppliers, by contrast, is not systematically related to the outcomes. The direct technopark coefficient also remains positive for most outcomes, indicating that the customer-exposure estimates capture an additional cross-district channel. Adding 1-digit NACE-sector-by-year fixed effects leaves these estimates largely unchanged (Appendix Table A10). The few statistically significant supplier-side coefficients are isolated and do not form a consistent pattern.[33]

6.4 Magnitude and Interpretation

The spillovers are smaller than the direct technopark effects but remain economically meaningful. A ten-percentage-point increase in customer exposure raises the probability of high-tech domestic sales by about 0.16 percentage points, compared with the 1.1-percentage-point direct effect of a technopark opening in the firm's own district, estimated in the same specification.

A ten-percentage-point increase in customer exposure also raises total factor productivity by approximately 0.5 per cent. For firms near the upper end of the exposure distribution, the implied effect is comparable to the five to ten per cent productivity premium associated with entering export markets (Van Biesebroeck, 2005; De Loecker,

[33] Appendix Table A13 re-estimates the exposure specification on the full sample of districts rather than the matched sample; the customer-exposure estimates are smaller and mostly statistically insignificant there, reflecting the weaker comparability of the all-district comparison group, so we rely on the matched sample for the spillover analysis.

2007). The effect on a typical firm is smaller because most firms have limited exposure to treated districts, but the results show that the policy reaches firms outside the districts it directly targets.

These spillovers may also make the main matched-pair estimates conservative, since some control-district firms sell to treated districts and therefore receive indirect exposure. We nevertheless interpret the exposure coefficients as reduced-form evidence rather than fully exogenous causal estimates. Fixing transaction weights at baseline rules out endogenous rewiring, but baseline exposure may still correlate with unobserved shocks shared with customers.

Finally, customer and supplier exposure should be estimated separately. The customer-side estimates are consistently positive, whereas the supplier-side estimates are generally close to zero. A pooled specification averages across these distinct relationships and weakens the customer-side estimate, so we focus on the separate specification and report the pooled results in Appendix Table A11.

7 Spatial Spillovers Across District Borders

Next, we examine whether technoparks generate spatial spillovers across district borders. If nearby untreated districts are affected by a park, they no longer provide a valid comparison group, and the estimated treatment effect will be measured relative to partially exposed controls. Following Butts (2021), we divide untreated districts into mutually exclusive distance rings. The ring coefficients capture spillovers at different distances, while the treatment effect is estimated relative to districts outside all rings, which are assumed to be unaffected.

We construct a contiguity graph using Türkiye's district boundaries, linking districts that share a border, and define $\rho(d, j)$ as the minimum number of district borders separating districts d and j.[34] Let $\mathcal{T}$ denote the set of districts in which a technopark opens during the sample period, and let T_j denote the opening year in district $j \in \mathcal{T}$. The set

[34]District boundaries are taken from the national shapefile and projected to EPSG:5637. Districts are treated as adjacent if their polygons share a boundary, including a single point.

of districts with an operating technopark in year t is

$$\mathcal{T}_t \equiv \{\, j \in \mathcal{T} : T_j \leq t \,\}.$$

We then define each district's distance from the nearest operating technopark as

$$k_{dt} \equiv \min_{j \in \mathcal{T}_t} \rho(d, j),$$

with $k_{dt} \equiv \infty$ before the first technopark opens. Thus, $k_{dt} = 0$ for a district with an operating technopark, $k_{dt} = 1$ for an adjacent district, and $k_{dt} = 2$ for a district two borders away. The spillover indicators are

$$Ring^k_{dt} \equiv \mathbf{1}\{k_{dt} = k\}, \qquad k = 1, 2, \tag{6}$$

and $S_{dt} \equiv \sum_{k=1}^{2} Ring^k_{dt}$ indicates whether district d is within two borders of an operating technopark in year t.

We measure distance using district borders rather than kilometres because the most plausible spillover channels, including commuting, face-to-face interactions and local supplier relationships, are closely tied to administrative geography. Districts also vary considerably in size, so a fixed geographical radius would capture very different economic areas across the country. Border distance provides a more flexible measure of local exposure, although the resulting rings are not directly comparable in physical size across regions.

Figure A14 shows the spatial distribution of the rings around the treated districts.

7.1 Specification

We estimate the following specification on the district-year panel:

$$Y_{dt} = \tau \cdot Technopark_{dt} + \sum_{k=1}^{2} \gamma_k \cdot Ring^k_{dt} + \delta_d + \delta_{rt} + \varepsilon_{dt}, \tag{7}$$

where district fixed effects, δ_d, absorb time-invariant differences across districts, while NUTS-2-region-by-year fixed effects, δ_{rt}, absorb shocks common to districts in the same region and year. The coefficients are therefore identified by comparing districts within the same region-year. The omitted category consists of district-years located more than two borders from any operating technopark. All specifications are unweighted. We report

standard errors clustered at the province level and Conley standard errors using a 100-kilometre cutoff.

We use two estimators. The first estimates equation (7) by OLS. With staggered treatment, this estimator may be affected by the negative-weighting problem described by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020), as already-treated districts can serve as controls for districts treated later. It may also be subject to the contamination bias identified by Goldsmith-Pinkham et al. (2024) when several mutually exclusive treatment indicators enter the same regression.

Our preferred estimates use the two-stage procedure of Gardner (2022). In the first stage, we estimate the fixed effects using only district-years that are neither treated nor exposed to spillovers. In the second stage, we regress the resulting residuals on the technopark and ring indicators. Because the treatment variables are not themselves residualised, this procedure avoids contamination across the treatment and spillover coefficients. Excluding exposed districts from the first stage is important: otherwise, the counterfactual would be estimated partly from districts that have already been affected by a nearby technopark.

7.2 Results

Table 6 reports the estimates from equation (7). Panel A presents the OLS results and Panel B the two-stage estimates. The outcomes are the total number of newly registered firms in each district-year and the corresponding counts by firm category. For each coefficient, the table reports both province-clustered and Conley standard errors.

Figure A15 presents the same coefficients after dividing them by the standard deviation of the corresponding outcome. This makes it easier to compare effects across outcomes with substantially different dispersion.

Three findings stand out. First, technoparks generate a large increase in firm entry in their own districts. The estimated effect on total entry ranges from 108 to 111 additional firms per year, depending on the estimator, and is significant at the one per cent level with both sets of standard errors. Entry also rises among technologically intensive firms. Technoparks generate between 27 and 29 additional entrants purchasing high-R&D goods and between 20 and 22 additional entrants selling high-R&D goods. These effects remain

strong after accounting for spatial spillovers.

Second, spillovers are concentrated among districts that share a border with a technopark district. The first-ring coefficient for total entry is 13.2 under OLS and 12.9 under the two-stage estimator, around one-eighth of the corresponding direct effect. It is significant at the five per cent level with province-clustered standard errors and at the ten per cent level with Conley standard errors.

The category-specific estimates provide weaker evidence of similar spillovers. The first-ring coefficients for firms purchasing and selling high-R&D goods are significant with province-clustered standard errors in both panels, although generally only at the ten per cent level. With Conley standard errors, the OLS estimates become insignificant, while the two-stage estimates remain significant at the ten per cent level. By contrast, the second-ring coefficients are small and statistically insignificant for every outcome. We therefore view the effect on total entry in neighbouring districts as robust, while the category-specific effects are more suggestive. Although the estimates cannot rule out small effects two borders away, there is little evidence that spillovers extend beyond immediately adjacent districts.

Third, accounting for these spillovers increases rather than reduces the estimated effect on treated districts. In specifications without the ring indicators, the coefficient on total entry is 104.3 under OLS and 101.9 under the two-stage estimator. Adding the rings increases these estimates to 108.1 and 110.9, respectively. Because neighbouring districts also experience an increase in entry, including them in the comparison group raises the counterfactual and biases the treatment effect downwards.

The two estimators produce similar results despite the staggered timing of technopark openings. Overall, the findings point to local diffusion rather than spatial displacement: technoparks increase firm entry both in their own districts and, to a smaller extent, in immediately neighbouring districts.

8 Cost-Benefit Analysis

A place-based subsidy is justified only if the value it creates exceeds its fiscal cost. We assess whether this condition holds for the technopark programme using a break-even calculation similar to those used in evaluations of innovation and place-based policies

(Bloom et al., 2019; Kline and Moretti, 2014b; Busso et al., 2013; Criscuolo et al., 2019). Our calculation is deliberately conservative. We ask whether the productivity gain among incumbent firms in treated districts, reported in Table 2, is sufficient on its own to cover the programme's entire fiscal cost. We do not include any benefits accruing to firms located inside the technoparks, increases in technology-intensive trade, employment or automation, or broader agglomeration effects.

The fiscal cost. The programme's direct fiscal cost is the tax revenue forgone through exemptions granted under Law No. 4691. We calculate this cost using the annual tax-expenditure reports published by the Revenue Administration.[35] These exemptions amounted to 0.72 billion US dollars in 2022 and 1.32 billion US dollars in 2023. The increase reflects real growth in the programme rather than inflation alone (Figure 9).[36] These figures remain lower bounds because they exclude employer social-security premium support for exempt personnel and infrastructure spending provided to zone-managing companies. We compare costs and benefits within the same year and focus on 2022, for which we can construct the corresponding value-added base.

The break-even condition. We value the estimated productivity gain as an equivalent proportional increase in value added. The main specification estimates a 2.9 per cent increase in total factor productivity. The revenue-per-employee specification, which does not rely on production-function assumptions and provides a more transparent measure of firm performance, yields an increase of 2.3 per cent (Table 2). We use both estimates in the calculation.

Using value added rather than gross output provides a conservative benchmark. A calculation based on gross output would produce a larger estimated benefit because gross output exceeds value added. Value added also has the advantage of being reported directly in firms' accounts. Let g denote the estimated productivity effect, VA the annual value

[35] Gelir .Idaresi Başkanlığı, *Vergi Harcamaları Raporu*, annual editions for 2020–2024, cross-checked against the tax-expenditure lists of the Ministry of Treasury and Finance and the performance reports of the Ministry of Industry and Technology (`https://www.gib.gov.tr`). The Law No. 4691 items include the corporate-income-tax exemption, the income-tax withholding exemption for research personnel, the value-added-tax exemption under provisional Article 20, and a smaller income-tax exemption on profits.

[36] We convert each year's nominal value using the corresponding annual average market exchange rate, 16.55 lira per dollar in 2022 and 23.75 in 2023, reported by the Central Bank of the Republic of Türkiye. This keeps both costs and benefits in the prices of the same year. Because the calculation compares same-year flows, the benefit-cost ratio does not depend on the currency used. Appendix Table A12 reports the underlying values in Turkish lira.

added of incumbent firms in treated districts, and C the programme's annual fiscal cost. The programme breaks even when

$$\theta, g, VA; \geq; \lambda, C, \tag{8}$$

where $\theta \in [0, 1]$ is the share of the estimated local gain that represents a net increase in national output, after allowing for displacement from other locations, and λ is the marginal cost of public funds. We report results for $\theta \in 0.5, 1$ and values of λ between 1.0 and 2.0, with 1.3 as our central assumption. This range is consistent with estimates for economies with large informal sectors and relatively narrow formal tax bases (Auriol and Warlters, 2012). Setting λ above one recognises that forgone tax revenue must ultimately be replaced by other, potentially distortionary, forms of taxation.

The incumbent firms in treated districts generated approximately 48 billion US dollars in value added in 2022. This total is calculated over the same set of firms used to estimate the productivity effects and is reported in Appendix Table A12. We summarise the comparison using the break-even productivity effect, [$g^* = \frac{\lambda C}{VA}$,] which is the productivity gain required for benefits to equal costs. The programme breaks even at an effect of 1.5 per cent when $\lambda = 1$ and 1.9 per cent under our central assumption of $\lambda = 1.3$. The point estimates from both productivity measures exceed the central break-even threshold.

There are two important qualifications. First, the productivity estimates remain uncertain. The standard error of the total-factor-productivity estimate is approximately one percentage point, producing a confidence interval of roughly one to five per cent. This interval includes the break-even threshold, meaning that we cannot reject exact break-even. Second, the productivity effects are larger among larger firms, which also account for a disproportionate share of value added. Applying the average treatment effect to the aggregate value-added base is therefore likely to understate the value-added-weighted productivity gain.

Magnitudes. Figure 10 reports the implied benefit-cost ratios. Without adjusting for the marginal cost of public funds, the 2.9 per cent total-factor-productivity estimate implies approximately 1.9 dollars of additional value added for each dollar of forgone tax revenue. Under our central assumptions of $\theta = 1$ and $\lambda = 1.3$, the benefit-cost ratio is approximately 1.5. The corresponding ratio based on the 2.3 per cent revenue-per-

employee estimate is approximately 1.2.

We also consider less and more favourable scenarios. In the conservative scenario, we set $\theta = 0.5$, $\lambda = 1.5$, and reduce the value-added base by 20 per cent. Under these assumptions, the estimated productivity gain covers approximately half of the programme's cost. In the favourable scenario, we set $\theta = 1$, $\lambda = 1$, and increase the value-added base by 20 per cent. The estimated benefit then exceeds twice the programme's cost.

The benefit-cost ratio is at or above one under most combinations of assumptions. It falls below one mainly when the marginal cost of public funds is set to 2.0 or when only half of the estimated local gain is treated as a net national gain (Appendix Figure A16). This is a demanding comparison because it assigns the programme's entire national tax expenditure to the productivity gains observed in only the 40 treated districts in our sample. Allocating only the share of national expenditure associated with the sampled technoparks would produce a higher benefit-cost ratio.

Displacement. The displacement parameter θ is the most important source of uncertainty. It affects not only the valuation of the estimated productivity gain but also its interpretation. Because the control firms are located in the same regions as treated firms, the estimated relative effect could overstate the national gain if treated firms expand by drawing activity away from control firms. Conversely, it could understate the effect if control firms are also indirectly exposed to the technoparks and therefore experience some productivity gains themselves, as suggested by the network results in Section 6. The direction of the resulting bias is therefore ambiguous.

The district-level event study shows that firm entry increases following the opening of a technopark, but this evidence cannot establish whether the additional activity was displaced from other locations. We therefore report $\theta = 0.5$ as a conservative scenario rather than attempting to infer displacement from changes in the number of local firms. This adjustment should also be interpreted carefully. Displacement large enough to halve the programme's national benefit would also affect the interpretation of the headline treatment effect, so estimation and valuation cannot be treated as entirely separate issues.

Comparison with the literature. A benefit-cost ratio close to one is broadly consistent with existing evaluations of place-based policies. Kline and Moretti (2014a) conclude that the agglomeration benefits generated by the Tennessee Valley Authority were ap-

proximately sufficient to offset its cost. Our central estimate is of a similar order of magnitude and lies somewhat above one. Greenstone et al. (2010) find substantial productivity spillovers among incumbent plants located near a large new establishment. Our estimated effect is smaller, as might be expected for a technology-zone programme rather than the arrival of a large anchor plant, but points in the same direction. The finding that local economic benefits can approach or exceed programme costs is also consistent with the evidence on US Empowerment Zones in Busso et al. (2013).

Our calculation differs from claims that place-based subsidies are fiscally self-financing, which Slattery and Zidar (2020) find little support for. We do not claim that the programme recovers its cost through higher tax revenues. Instead, we ask whether the additional output generated by more productive firms is large enough to offset the fiscal transfer in real-resource terms. The resulting ratio is therefore not a complete welfare calculation.

Two further qualifications apply. First, the productivity outcome is based on deflated sales and therefore measures revenue productivity rather than physical productivity. The direction of any bias is unclear. A shift towards higher-value products could increase measured markups, while stronger local competition could reduce them (De Loecker and Warzynski, 2012). Second, the calculation compares contemporaneous annual flows. The estimated productivity effect represents a persistent increase in firms' level of output, so the cumulative benefit over several years would exceed the single-year benefit reported here.

Overall, the estimated productivity gain among incumbent firms is of the same order of magnitude as the programme's fiscal cost and exceeds it under our central assumptions. This calculation excludes the other benefits documented in the paper, including gains to technopark tenants, increased technology-intensive trade, greater use of automation, employment effects, and wider spillovers.

9 Conclusions

This paper studies how place-based innovation policy affects incumbent firms by exploiting the staggered opening of technoparks across Türkiye. Using administrative data covering the near-universe of non-financial firms from 2009 to 2023 and a matched difference-in-

differences design, we find that firms in treated districts upgrade technologically. They become more active in technology-intensive domestic trade and exports, invest more in automation, employ a more technology-intensive workforce, and experience meaningful productivity gains. We find no comparable increase in low-technology exports, which indicates that these results reflect technological upgrading rather than a general expansion in firm activity.

The evidence points to several complementary channels. Firms in treated districts import more automation-related capital goods, increase the high-technology share of their workforce, and form new relationships with technology-intensive suppliers and customers. The absence of similar growth in low-technology linkages is particularly clear for customer relationships. The effects also extend beyond the districts that host technoparks. Firms upgrade when their customers become exposed to the policy, showing that the benefits spread through supply chains to firms that are not directly targeted.

The gains are widespread but larger among bigger firms, consistent with the importance of absorptive capacity (Cohen and Levinthal, 1990). Larger firms are better able to finance investment, recruit specialised workers, and reorganise their operations in response to new technological opportunities. At the district level, firm entry rises progressively after a technopark opens, suggesting that the programme also encourages local firm formation.

These findings have several implications for innovation policy. First, place-based innovation programmes can generate productivity gains and technological upgrading among existing firms, extending earlier evidence on the labour-market and investment effects of spatial subsidies (Kline and Moretti, 2014b; Neumark and Simpson, 2015; Criscuolo et al., 2019). Second, the firm-size gradient shows that access to a technopark is not sufficient on its own. The returns depend on whether firms have the managerial, financial, and human-capital capacity to adopt new technologies. Policies that help smaller firms build these capabilities could therefore increase the programme's aggregate impact. Third, the results show that technological upgrading can operate through domestic supply chains, rather than only through exports or foreign direct investment. This gives governments a route to support technological catch-up even when international integration remains limited (Rodrik, 2007b). Finally, our break-even calculation shows that the productivity gains among incumbent firms alone are of the same order of magnitude as the programme's

fiscal cost. This comparison does not include gains to firms located inside the technoparks or the other benefits documented in the paper.

Taken together, the findings show that place-based innovation policy can support structural transformation when it helps firms build and adopt technological capabilities, rather than simply offering incentives to attract investment. The design and management of the local innovation ecosystem are therefore matter as much as the fiscal support itself.

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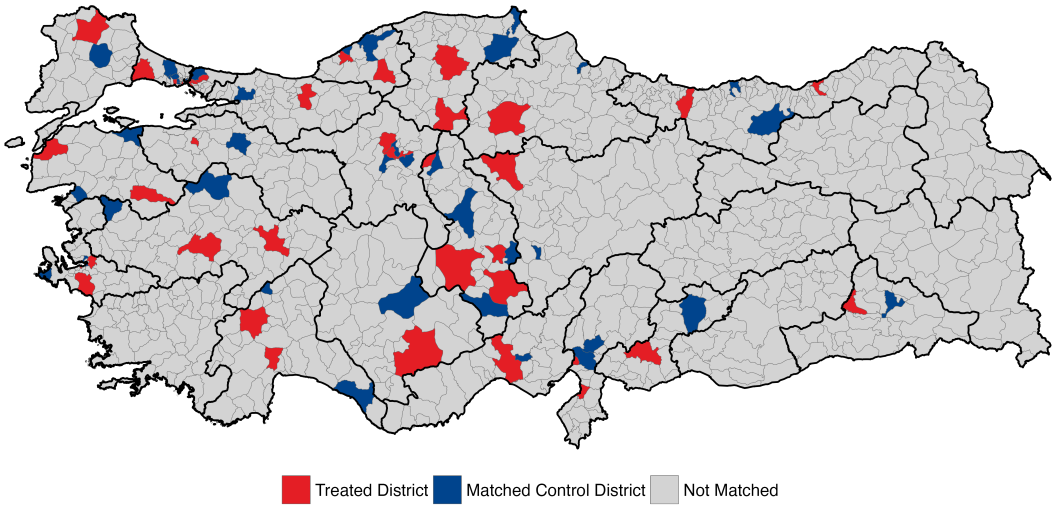
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Figures and Tables

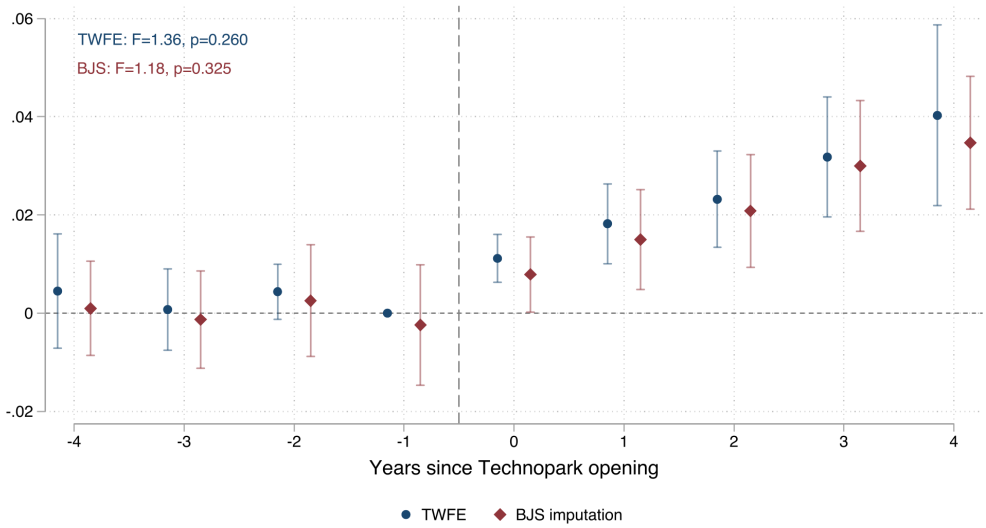
Figure 1: Treated and Matched Control Districts



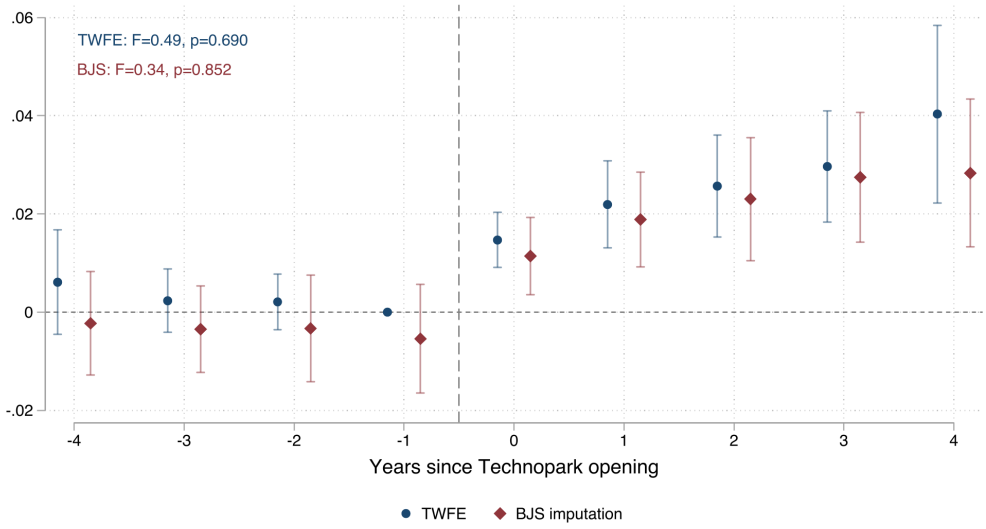
Note: The figure shows treated districts, matched control districts, and unmatched districts in Türkiye. Treated districts are districts that receive a technopark. Each of the 40 treated districts is matched to one never-treated control district within the same NUTS-2 region using propensity-score matching on 2009 district characteristics (see Table A2). Dark solid borders indicate NUTS-2 regions in Türkiye.

Figure 2: Event Study, High-Tech Domestic Purchases and Sales

(A.) High-tech domestic purchases



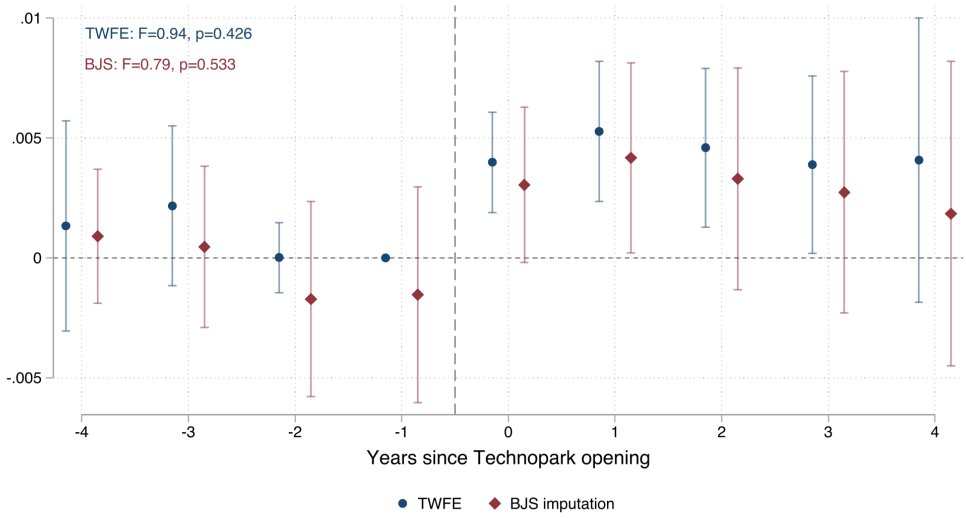
(B.) High-tech domestic sales



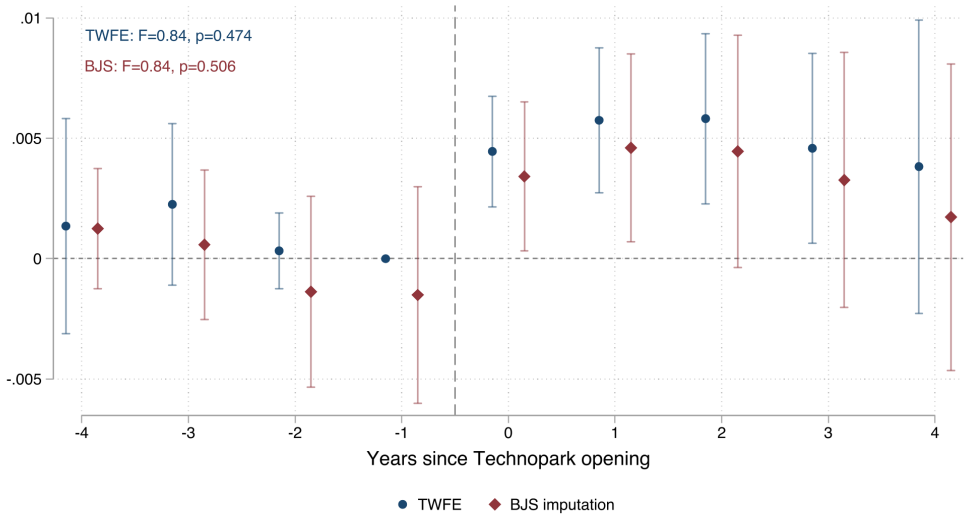
Note: Outcomes equal one if the firm makes any high-tech domestic purchases or high-tech domestic sales in a given year, constructed from firm-to-firm invoice records. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. For the two-way fixed-effects estimator the year before opening is the omitted period, plotted at zero; the imputation estimator estimates every pre-opening coefficient, including the year before opening, as a placebo. Coefficients report percentage-point changes in the probability of each outcome. The figure reports coefficients from conventional two-way fixed effects (TWFE) and Borusyak, Jaravel, and Spiess (2024) (BJS) event-study estimators. The reported F-statistics and p-values test whether the pre-treatment coefficients are jointly different from zero; higher p-values indicate no statistically significant evidence of differential pre-trends. The +4 coefficient is binned to absorb all longer horizons under TWFE; the imputation estimator identifies each horizon separately, so its +4 coefficient is unbinned. Whiskers indicate 95% confidence intervals. Standard errors are clustered at the district level.

Figure 3: Event Study, Technology-Intensive Exports

(A.) High-R&D exports



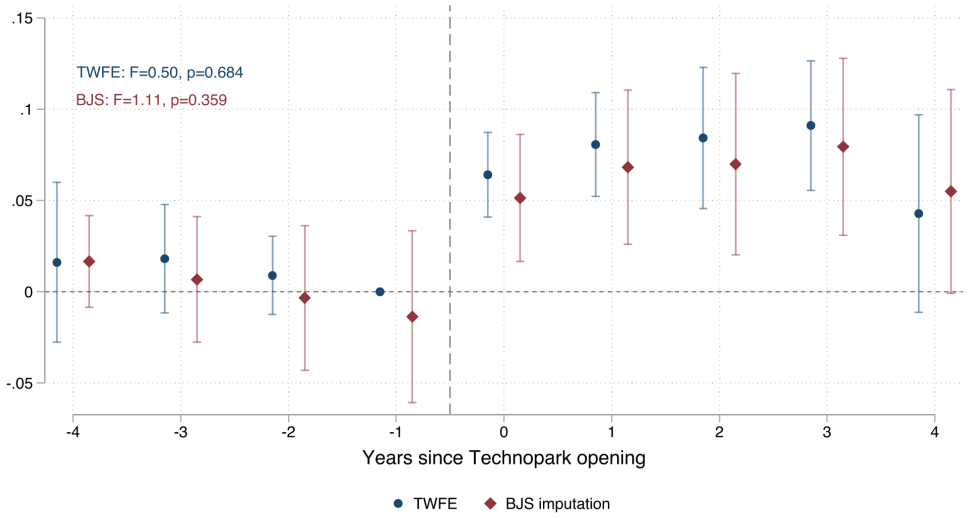
(B.) High-tech exports



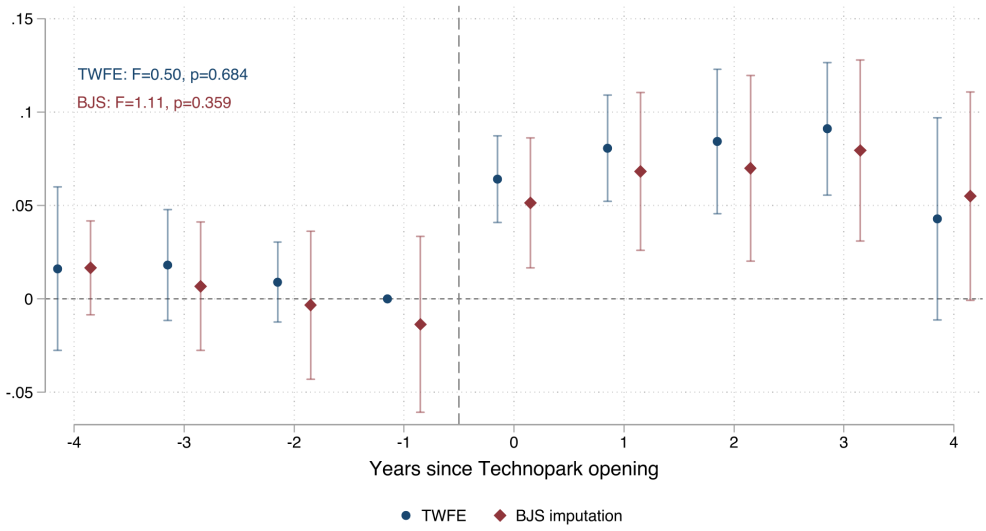
Note: Outcomes equal one if the firm exports any high-R&D or high-tech products in a given year, constructed from transaction-level customs records at the 6-digit HS product level. High-R&D exports are defined using the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes; high-tech exports are defined using the OECD/Eurostat technology-intensity classification mapped to HS product codes. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. For the two-way fixed-effects estimator the year before opening is the omitted period, plotted at zero; the imputation estimator estimates every pre-opening coefficient, including the year before opening, as a placebo. Coefficients report percentage-point changes in the probability of each outcome. The figure reports coefficients from conventional two-way fixed effects (TWFE) and Borusyak, Jaravel, and Spiess (2024) (BJS) event-study estimators. The reported F-statistics and p-values test whether the pre-treatment coefficients are jointly different from zero; higher p-values indicate no statistically significant evidence of differential pre-trends. The +4 coefficient is binned to absorb all longer horizons under TWFE; the imputation estimator identifies each horizon separately, so its +4 coefficient is unbinned. Whiskers indicate 95% confidence intervals. Standard errors are clustered at the district level.

Figure 4: Event Study, TFP and Revenue per Employee

(A.) Total factor productivity

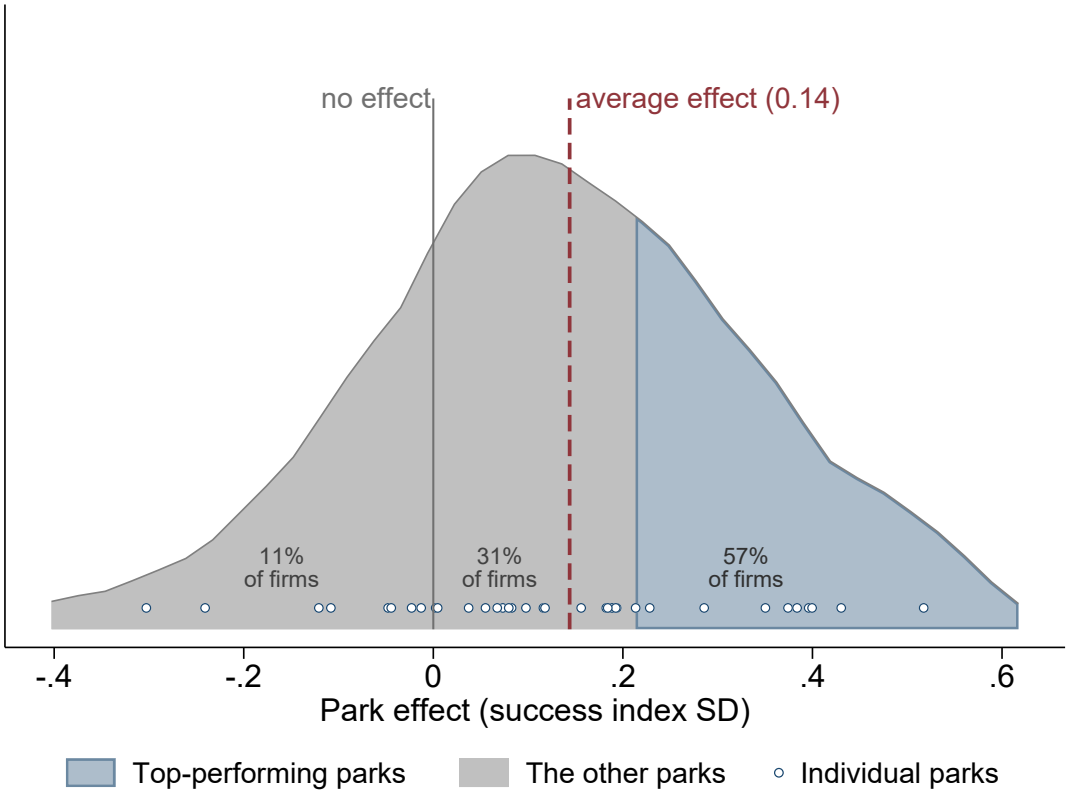


(B.) Revenue per employee (log)



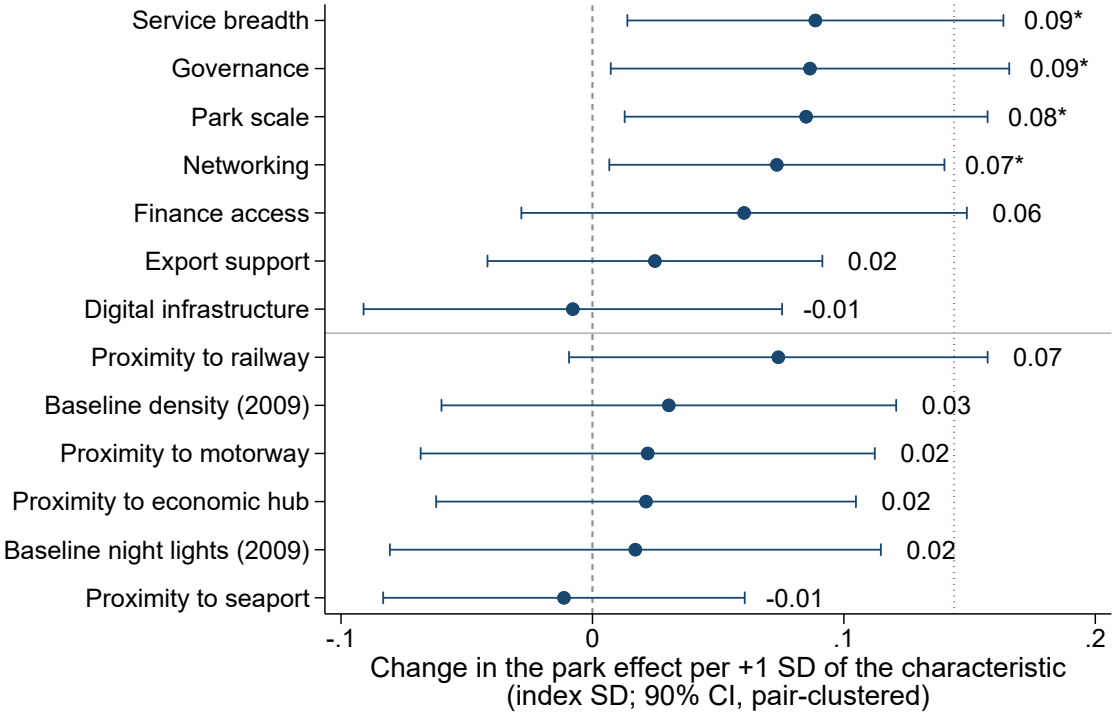
Note: Outcomes are total factor productivity and revenue per employee. Total factor productivity is estimated from firms' administrative financial statements using the semiparametric method of Olley and Pakes (1996). Revenue per employee is defined as the log of gross revenue divided by the firm's 2009 employee count. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023; Panel A is estimated on the restricted total-factor-productivity sample and Panel B on the restricted revenue-per-employee sample described in Table 2. For the two-way fixed-effects estimator the year before opening is the omitted period, plotted at zero; the imputation estimator estimates every pre-opening coefficient, including the year before opening, as a placebo. Coefficients report approximate percentage changes in total factor productivity and revenue per employee. The figure reports coefficients from conventional two-way fixed effects (TWFE) and Borusyak, Jaravel, and Spiess (2024) (BJS) event-study estimators. The reported F-statistics and p-values test whether the pre-treatment coefficients are jointly different from zero; higher p-values indicate no statistically significant evidence of differential pre-trends. The +4 coefficient is binned to absorb all longer horizons under TWFE; the imputation estimator identifies each horizon separately, so its +4 coefficient is unbinned. Whiskers indicate 95% confidence intervals. Standard errors are clustered at the district level.

Figure 5: How Widely Technopark Effects Vary Across Parks



Note: The figure shows the distribution of the technopark effects across the parks observed both before and after opening, as a single kernel density. Each park's effect is its treated-minus-control change in a composite index of local technological upgrading from before to after opening, measured in standard deviations; the index is an inverse-covariance-weighted composite (Anderson, 2008) of the district-level upgrading outcomes. The raw park effects are noise-adjusted by empirical Bayes, which shrinks each estimate toward the mean in proportion to its imprecision, so that parks observed for only a few post-opening years are pulled in and the estimation noise that inflates the raw spread is removed. The tick marks along the base are the individual parks. The top-quarter parks, ranked by their noise-adjusted effect, are the top performers and are shaded; the rest are grey. The solid vertical line is zero, the no-effect benchmark, and the dashed line is the average effect (0.14). Weighting parks by their number of firms (from the manager survey), 11 per cent of firms sit in parks with an effect below zero, 31 per cent in parks between zero and the average, and 58 per cent in parks above the average, so the gains are broad-based rather than confined to a few parks.

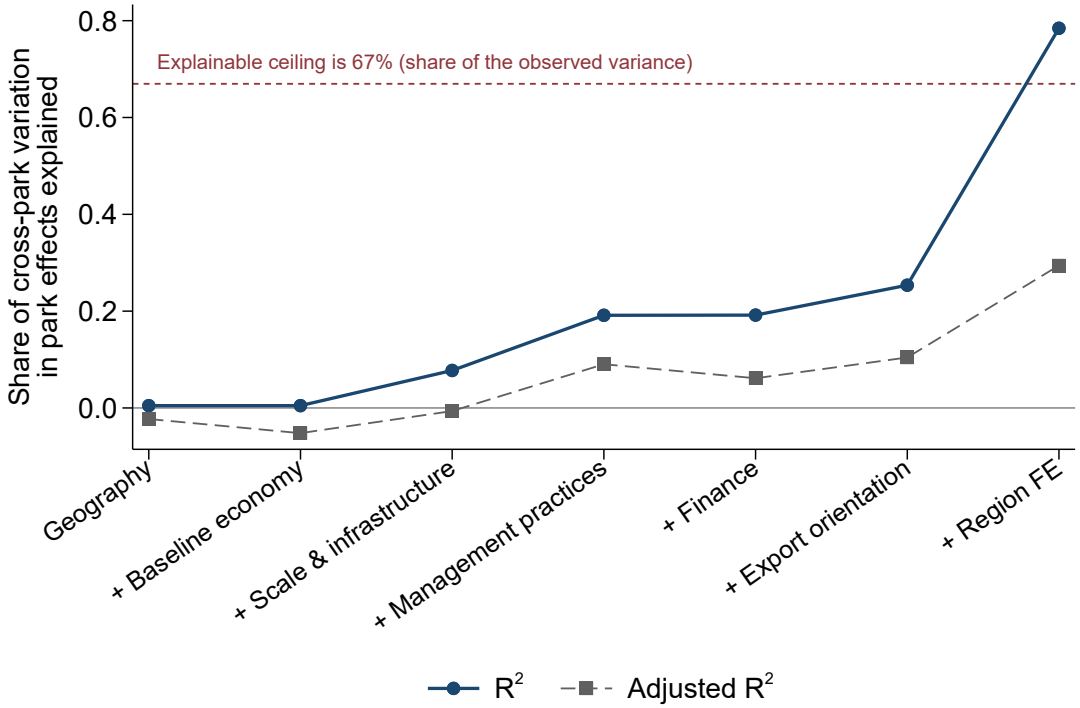
Figure 6: Which Park Characteristics Are Linked to a Larger Effect



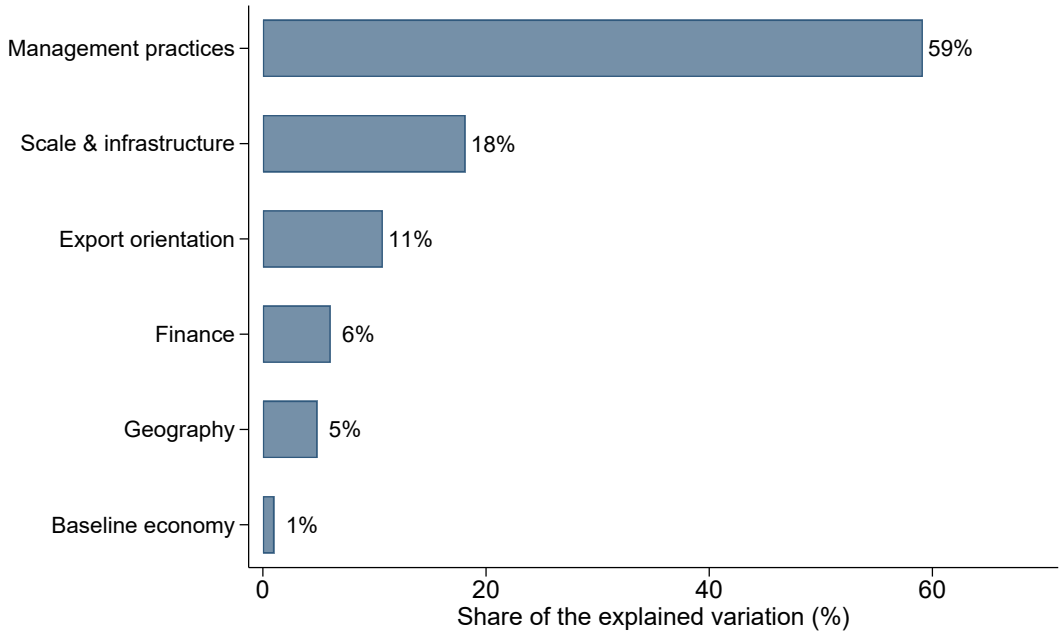
Note: The figure shows how a park's effect varies with each characteristic, estimated across all 37 parks one characteristic at a time. Each point is the change in the park-level effect, in standard deviations of the composite upgrading index, for a one-standard-deviation increase in that characteristic. The survey-based management characteristics (top panel) carry the largest associations, led by service breadth, governance, park scale, and networking; the predetermined locational measures (bottom panel) are close to zero. The dotted vertical line marks the average technopark effect (0.14). Whiskers are 90% confidence intervals with standard errors clustered at the matched-pair level; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Figure 7: What Explains the Differences in Effects Across Parks

(A.) Cumulative fit as characteristics and region fixed effects are added



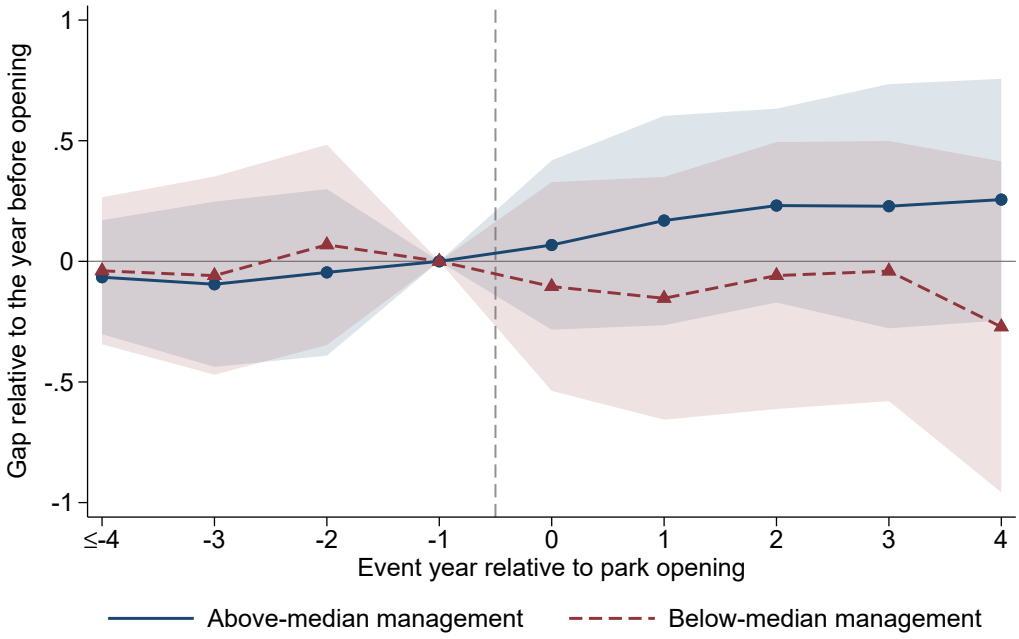
(B.) Composition of the explained variation (order-free Shapley shares)



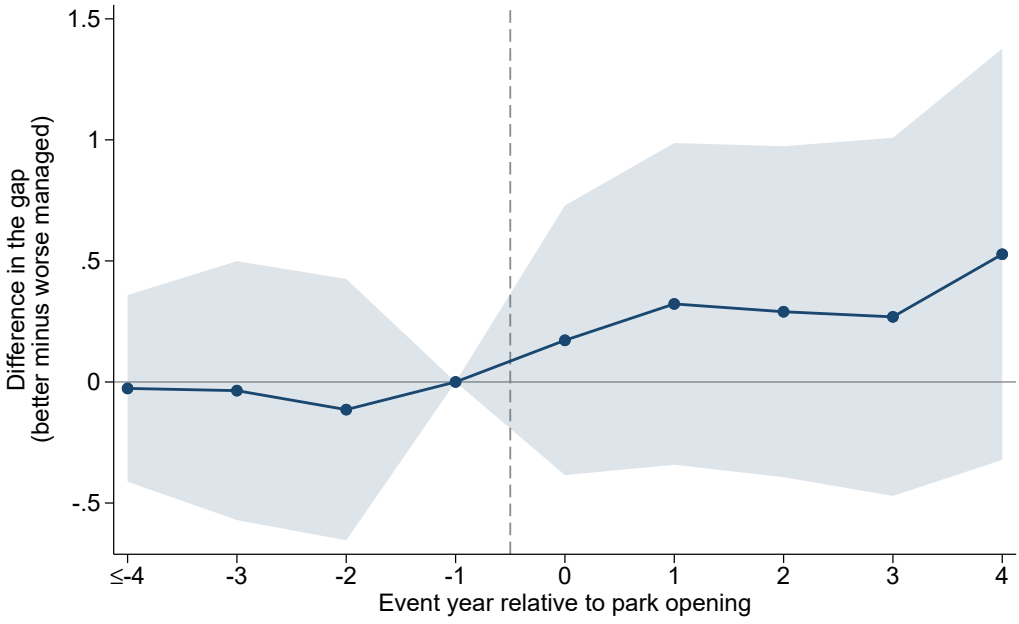
Note: The figure summarises what explains the differences in the technopark effect across parks. Panel A adds the six blocks of characteristics one at a time, in a pre-specified order, to a regression of the park-level effect, and then adds NUTS-2 region fixed effects; it plots the R^2 (solid) and the adjusted R^2 (dashed), which penalises the fit for the number of terms. The characteristics reach an R^2 of about 0.25, roughly 37 per cent of the explainable variation. The explainable ceiling (the dashed maroon line) is 67 per cent, not 100 per cent, because each park's effect is itself estimated with error, so the spread of the estimated effects overstates the true spread across parks. The ceiling is the share of the observed variance that reflects genuine differences between parks rather than this estimation error, obtained by subtracting the average sampling variance of the estimates from their total variance; the remaining third is estimation noise, unrelated to any park characteristic, so no model, however rich, could explain it. Panel B reports order-free Shapley shares, each block's average contribution across all entry orders, expressed as a share of the explained variation so the blocks sum to 100 per cent.

Figure 8: Better-Managed Parks Pull Ahead After Opening

(A.) By management group

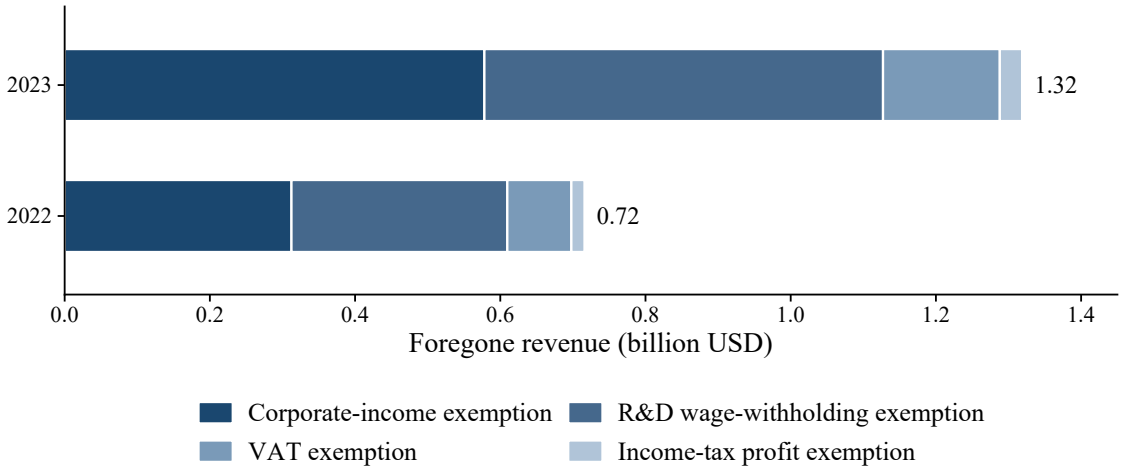


(B.) Difference: better minus worse managed



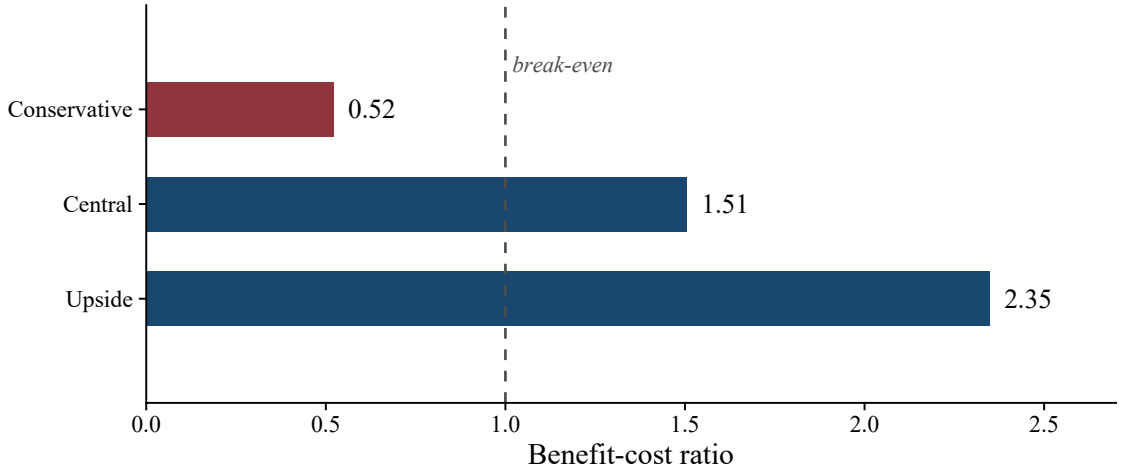
Note: The figure shows the technopark effect over event time, with parks split at the median of the management index, using all 37 parks. Panel A plots each group's mean treated-minus-control gap in the composite upgrading index by year relative to opening, for parks above and below the median. Panel B plots the difference between the two groups, the standard single-line event study. Each series is normalised so that the year before opening ($t = -1$) is zero; the shaded bands are 90% confidence intervals, and the reference year has none by construction. Both groups are flat before opening, which checks for parallel pre-trends, and the difference jumps after opening. The first point ("≤ -4") pools event years -4 and earlier, and the window ends at $+4$.

Figure 9: The Fiscal Cost of the Technopark Programme



Note: Each bar shows the total annual revenue the government forgoes through the tax exemptions granted under Law No. 4691, which is the direct fiscal cost of the programme, split into its four components. The cost rose from 0.72 billion US dollars in 2022 to 1.32 billion in 2023, with the corporate-income exemption and the income-tax withholding exemption on research personnel wages together accounting for most of the total, followed by the value-added-tax exemption and a small income-tax profit exemption. The rise reflects real growth in the programme, since each year's nominal lira figure is converted at that year's average market exchange rate from the Central Bank of the Republic of Türkiye. The series is compiled by hand from five annual editions of the *Vergi Harcamaları Raporu* of the Revenue Administration, 2020–2024. The totals exclude the employer social-security premium support for exempt personnel and Ministry infrastructure outlays to zone-managing companies, and so understate the full fiscal cost. Appendix Table A12 reports the underlying lira values.

Figure 10: Benefit-Cost Ratio of the Incumbent Productivity Spillover



Note: Each bar shows the benefit-cost ratio of the programme under a different set of assumptions, that is, the value of the productivity gain to incumbent firms outside the technopark divided by the fiscal cost. A ratio above one, to the right of the dashed break-even line, means the gain is worth more than the cost; a ratio below one means it falls short. In the central case the ratio is about 1.5: a dollar of forgone revenue returns about 1.9 dollars of value added, or 1.5 once the cost is scaled by the marginal cost of public funds, the conservative case recovers about half of the cost, and the upside case exceeds two. Formally the ratio is $\theta\, g\, VA/(\lambda C)$, where the benefit is the 2.9 per cent productivity gain valued as an equal proportional gain in value added VA, θ is the fraction of the local gain that is a net national addition, λ is the marginal cost of public funds, and C is the fiscal cost of Figure 9. The central case sets $\theta = 1$ and $\lambda = 1.3$; the conservative case combines $\theta = 0.5$, $\lambda = 1.5$, and a value-added base a fifth below the central figure; the upside case sets $\theta = 1$, $\lambda = 1$, and a base a fifth above it. Only incumbent firms outside the zone are counted, so the ratio excludes the gains to tenants, the additional trade and employment documented above, and any persistence of the effect beyond a single year.

Table 1: Technoparks and Firm Upgrading

Panel A. Probability of high-tech domestic purchases and sales		
	(1)	(2)
	High-tech domestic purchases	**High-tech domestic sales**
Technopark	0.010***	0.010***
	(0.003)	(0.002)
Observations	2,377,439	2,377,439
Control mean	0.288	0.261
Effect size	3.5%	3.8%
Panel B. Probability of exporting technology-intensive products		
	(1)	(2)
	High-R&D exports	**High-tech exports**
Technopark	0.003***	0.003***
	(0.001)	(0.001)
Observations	2,377,439	2,377,439
Control mean	0.073	0.070
Effect size	4.1%	4.3%
Firm FE	✓	✓
Pair × Year FE	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on extensive-margin measures of firm upgrading. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Panel A reports indicators equal to one if the firm makes any high-tech domestic purchases or any high-tech domestic sales in a given year, constructed from firm-to-firm invoice records. Panel B reports indicators equal to one if the firm exports any high-R&D or high-tech products in a given year, constructed from transaction-level customs records at the 6-digit HS product level. High-R&D exports are defined using the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes; high-tech exports are defined using the OECD/Eurostat technology-intensity classification mapped to HS product codes. *Technopark* equals one after a technopark becomes operational in the district. Coefficients report percentage-point changes in the probability of each outcome after technopark opening. Effect sizes express the coefficient relative to the control mean. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 2: Technoparks and Firm Productivity

	(1) **Total factor productivity**	(2) **Revenue per employee (log)**
Technopark	0.029***	0.023***
	(0.010)	(0.009)
Observations	2,088,303	1,145,078
Control mean	5.475	11.800
Approx. effect size	2.9%	2.3%
Firm FE	✓	✓
Pair × Year FE	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on firm productivity and revenue per employee. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Column (1) reports firm-level total factor productivity, constructed from administrative financial statements using the semiparametric method introduced by Olley and Pakes (1996). Olley–Pakes estimation requires non-missing balance-sheet variables, especially capital and investment; firms with missing or zero investment are dropped from the production-function estimation. Column (2) reports revenue per employee, defined as the natural logarithm of gross revenue divided by the firm's 2009 employee count, i.e. $\log(\text{gross revenue}_{it}/\text{employment}_{i,2009})$. This measure captures whether firm expansion translates into higher revenue relative to the firm's baseline workforce size, while avoiding mechanical changes driven by contemporaneous hiring after technopark exposure. Baseline employment is taken from administrative financial-statement records; firm-years with missing or zero 2009 employment are excluded from column (2), which accounts for the smaller sample. *Technopark* equals one after a technopark becomes operational in the district. Coefficients for both outcomes can be interpreted approximately as percentage changes; approximate effect sizes interpret the coefficient as a semi-elasticity. Control means are reported in the units of the dependent variables; for revenue per employee, the control mean is the mean log revenue per baseline employee. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 3: Technoparks, Automation Adoption, and High-Tech Employment

	(1) **Automation adoption**	(2) **High-tech employment**	(3) **High-tech employment share**
Technopark	0.0013**	0.008***	0.287***
	(0.0005)	(0.002)	(0.059)
Observations	2,377,439	1,424,174	1,424,174
Control mean	0.016	0.272	7.604
Effect size	8.1%	2.9%	3.8%
Firm FE	✓	✓	✓
Pair × Year FE	✓	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on automation adoption and high-tech employment. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Column (1) is an indicator equal to one if the firm imports automation-related capital goods in a given year, constructed from transaction-level customs records. Column (2) is an indicator equal to one if the firm employs at least one high-tech worker. Column (3) reports the share of high-tech workers in total firm employment, expressed in per cent. Total firm employment is observed from 2009 in administrative records, but occupation-level data identifying high-tech workers are available only from 2012 onward; columns (2) and (3) are therefore estimated on 2012–2023 for firms with available occupation-level records, which accounts for the smaller sample. *Technopark* equals one after a technopark becomes operational in the district. Coefficients in columns (1) and (2) report percentage-point changes in the probability of the corresponding outcome, and the coefficient in column (3) reports the percentage-point change in the employment share, after technopark opening. Effect sizes express the coefficient relative to the control mean. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 4: Technoparks and High-Tech Business Linkages

Panel A. High-tech supplier linkages

	(1) **All high-tech suppliers**	(2) **Within-district high-tech suppliers**	(3) **Across-district high-tech suppliers**
Technopark	0.031*	0.022**	0.031*
	(0.018)	(0.010)	(0.015)
Observations	2,377,439	2,377,439	2,377,439
Control mean	0.379	0.085	0.340
Approx. effect size	3.1%	2.2%	3.1%

Panel B. High-tech customer linkages

	(1) **All high-tech customers**	(2) **Within-district high-tech customers**	(3) **Across-district high-tech customers**
Technopark	0.036**	0.035***	0.028*
	(0.017)	(0.012)	(0.014)
Observations	2,377,439	2,377,439	2,377,439
Control mean	0.378	0.100	0.336
Approx. effect size	3.6%	3.5%	2.8%
Firm FE	✓	✓	✓
Pair × Year FE	✓	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on high-tech business linkages. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Outcomes are log one plus the number of distinct high-tech supplier or customer firms in a firm-year, constructed from firm-to-firm invoice records. High-tech counterparties are firms operating in high- or medium-high-technology 2-digit NACE sectors, following Eurostat's technology-intensity classification. Control means are means of the log-transformed outcome. *Technopark* equals one after a technopark becomes operational in the district. Coefficients are semi-elasticities, and approximate effect sizes interpret the coefficient as an approximate percentage change rather than as the ratio of the coefficient to the control mean. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 5: Network Spillovers: Buyer–Supplier Exposure to Treated Districts

	Technopark (own district) $\hat{\beta}$	**Supplier exposure** $\hat{\pi}^{\text{sup}}$	**Customer exposure** $\hat{\pi}^{\text{cus}}$	N
Technology-intensive trade				
High-tech domestic purchases	0.016***	−0.001	0.016***	1,415,231
	(0.006)	(0.006)	(0.005)	
High-tech domestic sales	0.011**	0.017***	0.016***	1,415,231
	(0.006)	(0.006)	(0.006)	
High-R&D exports	0.005**	0.002	0.005**	1,415,231
	(0.002)	(0.003)	(0.002)	
High-tech exports	0.005**	0.002	0.005**	1,415,231
	(0.002)	(0.003)	(0.002)	
Productivity and technology adoption				
Log TFP	0.027	−0.028	0.050***	1,332,747
	(0.020)	(0.018)	(0.017)	
Automation adoption	0.002*	0.000	0.004***	1,415,231
	(0.001)	(0.001)	(0.002)	
High-tech employment share	0.487***	0.516**	0.454	1,011,357
	(0.109)	(0.220)	(0.309)	
Firm FE	✓	✓	✓	
Pair × year FE	✓	✓	✓	

Note: This table reports firm-level estimates of network spillovers from technopark treatment, adapting the buyer–supplier exposure design of Atalay et al. (2023). Each row is a single regression of the row outcome on the own-district technopark indicator and the two exposure shares, following equation (5). *Supplier exposure* is the share of the firm's input purchases sourced from suppliers located in treated districts, and *Customer exposure* is the share of its sales made to customers located in treated districts; both are constructed from firm-to-firm invoice records, exclude the firm's own district, and hold the transaction weights fixed at each firm's baseline year, so exposure varies over time only as a firm's pre-existing partners become treated. The *Technopark* column reports the direct own-district effect $\hat{\beta}$, and the *Supplier* and *Customer* columns report the network-spillover coefficients $\hat{\pi}^{\text{sup}}$ and $\hat{\pi}^{\text{cus}}$; the spillover operates through a firm's customers rather than its suppliers. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023, restricted to firms in matched pairs containing more than three firms. All columns absorb firm and matched-pair-by-year fixed effects; Appendix Table A10 adds 1-digit-NACE-sector-by-year fixed effects. Standard errors, clustered at the district level, are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 6: Technopark and Contiguity-Ring Spillover Effects on Firm Entry

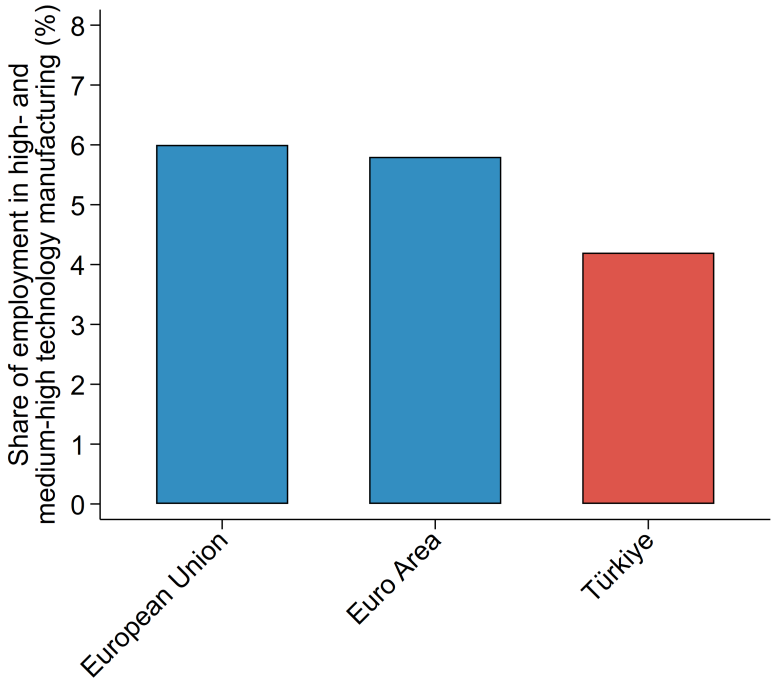
	New firms					
	All	High-R&D purchases	High-R&D sales	High-R&D exports	High-tech exports	High-skill exports
Panel A: *OLS (two-way fixed effects)*						
Technopark (own district)	108.14	28.65	21.76	4.05	4.08	2.54
Province	(27.47)***	(8.53)***	(7.87)***	(1.46)***	(1.50)***	(1.11)**
Conley 100 km	(27.38)***	(8.21)***	(7.51)***	(1.42)***	(1.45)***	(1.10)**
Round-1 spillover (first ring)	13.16	3.02	2.08	0.46	0.46	0.35
Province	(6.38)**	(1.58)*	(1.18)*	(0.31)	(0.31)	(0.26)
Conley 100 km	(6.88)*	(1.84)	(1.52)	(0.37)	(0.38)	(0.29)
Round-2 spillover (second ring)	−2.66	0.51	0.67	0.10	0.09	0.06
Province	(3.51)	(1.42)	(1.22)	(0.30)	(0.28)	(0.16)
Conley 100 km	(3.91)	(1.50)	(1.35)	(0.31)	(0.30)	(0.19)
Panel B: *Two-stage difference-in-differences*						
Technopark (own district)	110.87	26.92	20.32	4.10	4.06	2.50
Province	(28.81)***	(8.02)***	(7.61)***	(1.44)***	(1.46)***	(1.01)**
Conley 100 km	(27.32)***	(7.39)***	(7.01)***	(1.33)***	(1.34)***	(0.95)***
Round-1 spillover (first ring)	12.89	2.95	2.22	0.51	0.48	0.30
Province	(6.32)**	(1.45)**	(1.22)*	(0.30)*	(0.30)	(0.24)
Conley 100 km	(6.60)*	(1.56)*	(1.35)*	(0.34)	(0.35)	(0.25)
Round-2 spillover (second ring)	−0.87	0.98	1.06	0.17	0.16	0.07
Province	(2.94)	(1.11)	(0.95)	(0.24)	(0.23)	(0.13)
Conley 100 km	(3.38)	(1.23)	(1.04)	(0.25)	(0.24)	(0.14)
Observations	13,395	13,395	13,395	13,395	13,395	13,395
District FE	✓	✓	✓	✓	✓	✓
NUTS2 × Year FE	✓	✓	✓	✓	✓	✓

Notes: **Counts of newly created firms in the district-year. Each coefficient is reported once, followed by two standard errors for that same estimate: clustered on province, then the Conley (1999) spatial-HAC form with a uniform kernel and a 100 km cutoff, roughly the reach of the two rings. Stars attach to the standard error they sit on, so a coefficient can be significant under one and not the other. The Technopark coefficient is a total effect on the treated in the sense of Butts (2021): most treated districts are themselves within reach of another park. The omitted comparison group is district-years more than two borders from every operating park. Both panels use the same sample of 893 districts over 2009–2023 and differ only in estimator: Panel A is OLS with the fixed effects absorbed, Panel B the two-stage estimator of Gardner (2022), whose first stage is fit only on district-years that are neither treated nor in any ring.** * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

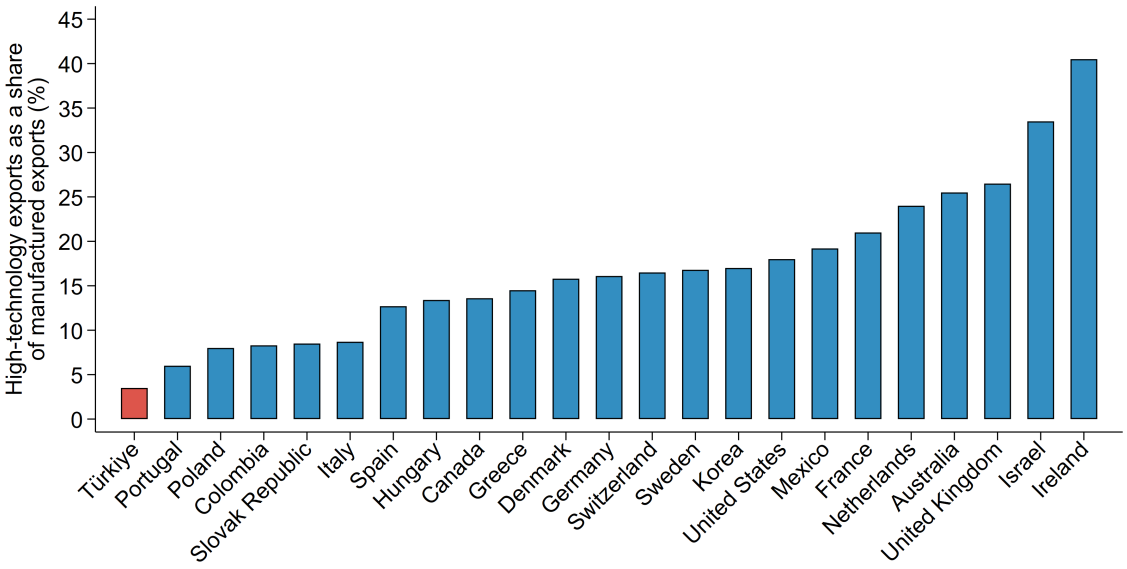
Appendix Figures and Tables

Figure A1: Technology-Intensive Employment and High-Tech Exports in Türkiye

(A.) Tech-intensive manufacturing employment

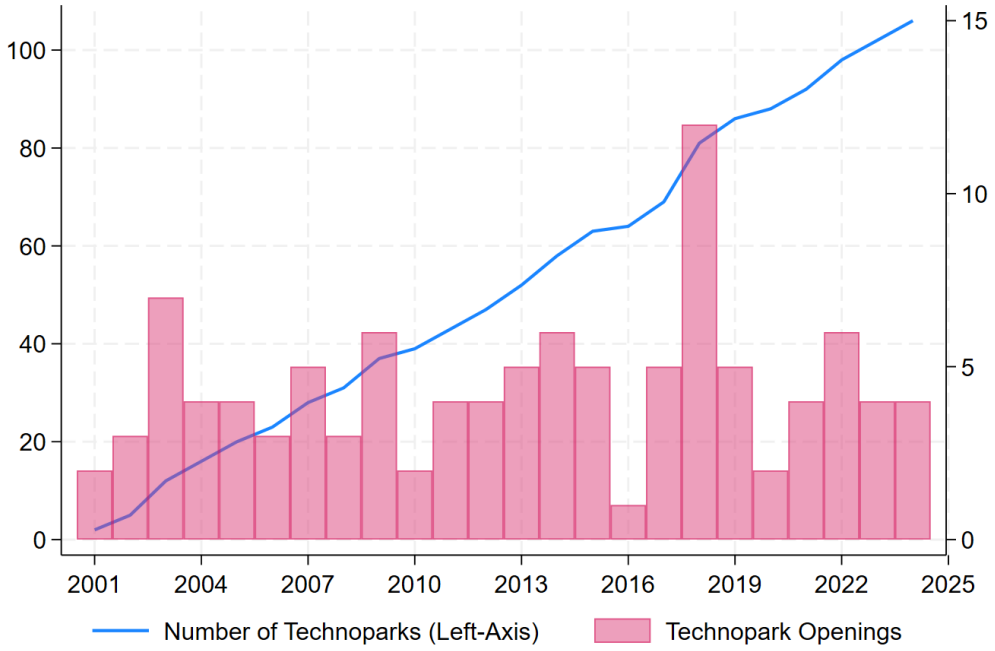


(B.) High-technology exports as a share of manufactured exports



Note: Panel A reports the share of total employment in high- and medium-high-technology manufacturing in 2023. Panel B reports high-technology exports as a share of manufactured exports in 2022. Türkiye is highlighted in red. *Source:* Eurostat; World Bank World Development Indicators.

Figure A2: Türkiye's Technopark Rollout



Note: The figure shows the annual number of technopark openings and the cumulative number of technoparks in Türkiye over time. Pink bars report annual openings using the right axis, while the blue line reports the cumulative number of technoparks using the left axis. The empirical analysis covers technopark openings over 2009–2023.

Figure A3: Technopark Facilities and Support Activities

(A.) Technopark building



(B.) Training hall



(C.) Office space



(D.) Local B2B matching



(E.) Global B2B matching



(F.) Startup events



(G.) Grant calls



(H.) Training programs

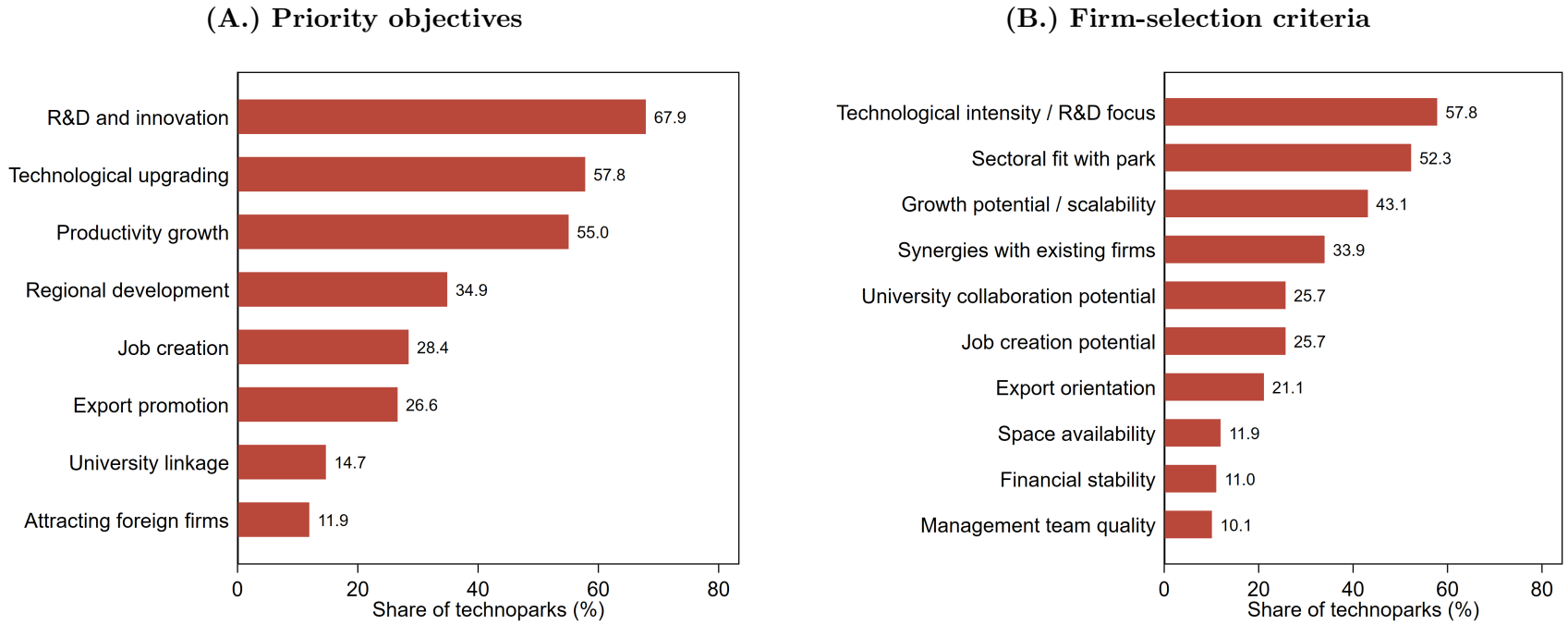


(I.) Tech workshops



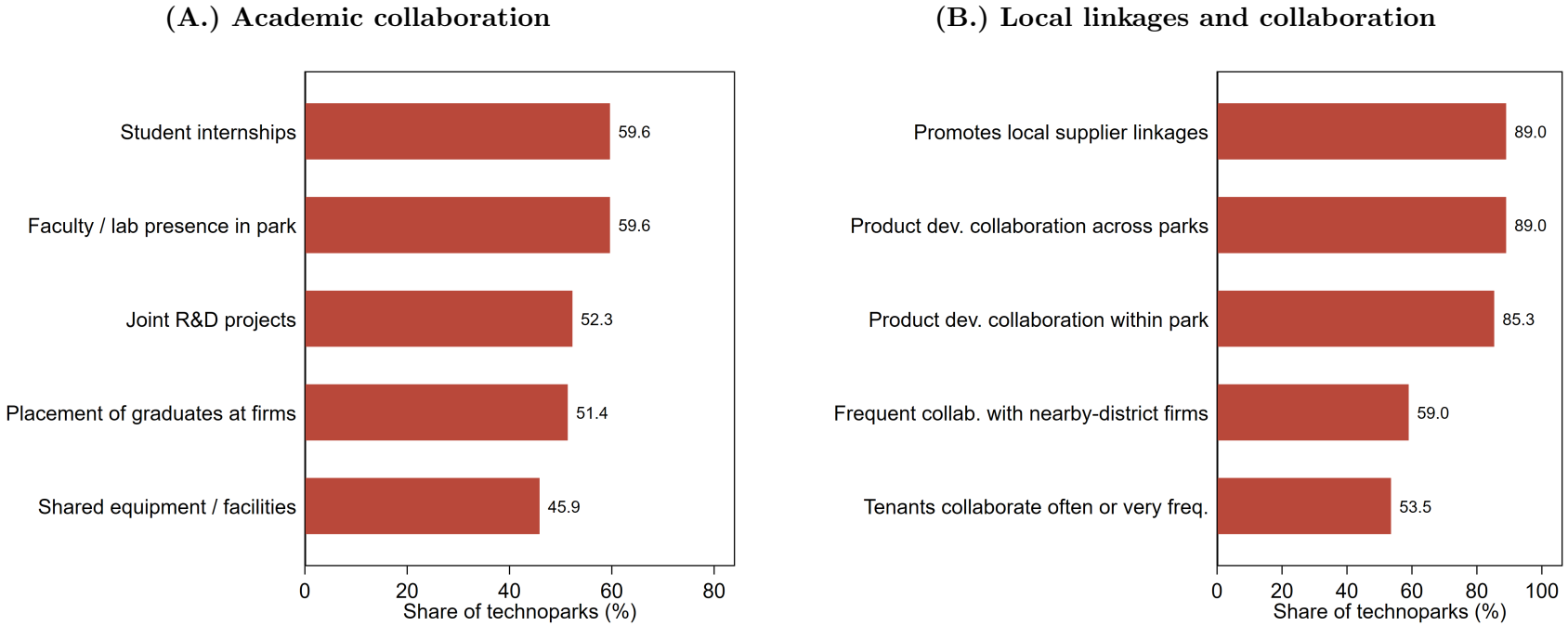
Note: The figure shows Düzce Teknopark as an illustrative example of a fully operational technopark. It began operations in 2011 and combines physical infrastructure, including workspaces and training facilities, with support activities such as business matching, entrepreneurship events, grant calls, training programs, and technology workshops.

Figure A4: Technoparks' Objectives and Selection Criteria



Note: The figure reports results from our survey of 109 technopark managers in Türkiye. Panel A reports responses to the question: "What are the technopark's main objectives?" Each technopark selected its top three priorities, and each bar shows the share of technoparks selecting that objective. Panel B reports the criteria used when selecting firms for admission to the technopark. Each technopark selected its top three criteria, and each bar shows the share of technoparks selecting that criterion. Shares therefore need not sum to 100 per cent within a panel.

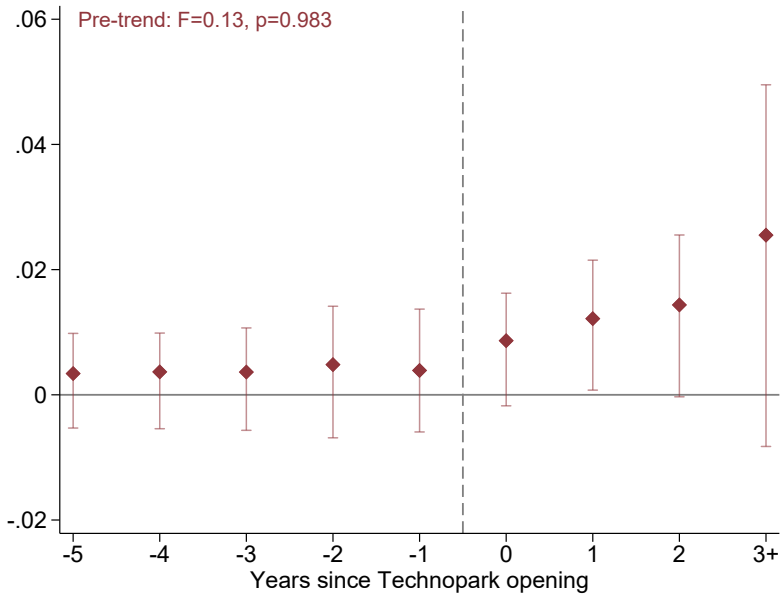
Figure A5: Collaboration and Linkages in Technoparks



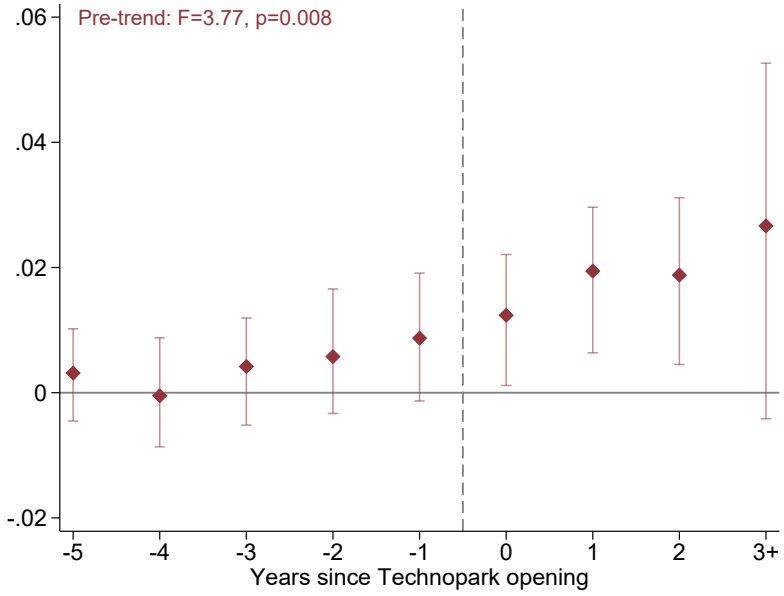
Note: The figure reports results from our survey of 109 technopark managers in Türkiye. Panel A summarises the main forms of academic collaboration between technoparks and universities or research institutions. Each bar shows the share of technoparks reporting the corresponding collaboration channel, including student internships, faculty or laboratory presence inside the park, joint R&D projects, placement of graduates at tenant firms, and shared equipment or facilities. Panel B reports whether technoparks create links among firms and with local suppliers. We measure tenant collaboration, collaboration with nearby firms outside the park, supplier linkages, and product-development collaboration within and across technoparks. Each bar shows the share answering yes or reporting frequent collaboration.

Figure A6: Full-Sample Event Study, High-Tech Domestic Purchases and Sales

(A.) High-tech domestic purchases



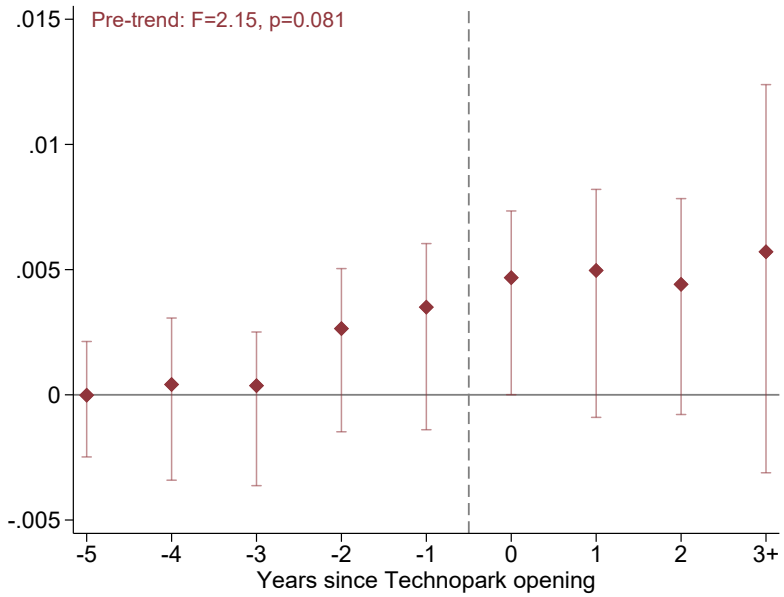
(B.) High-tech domestic sales



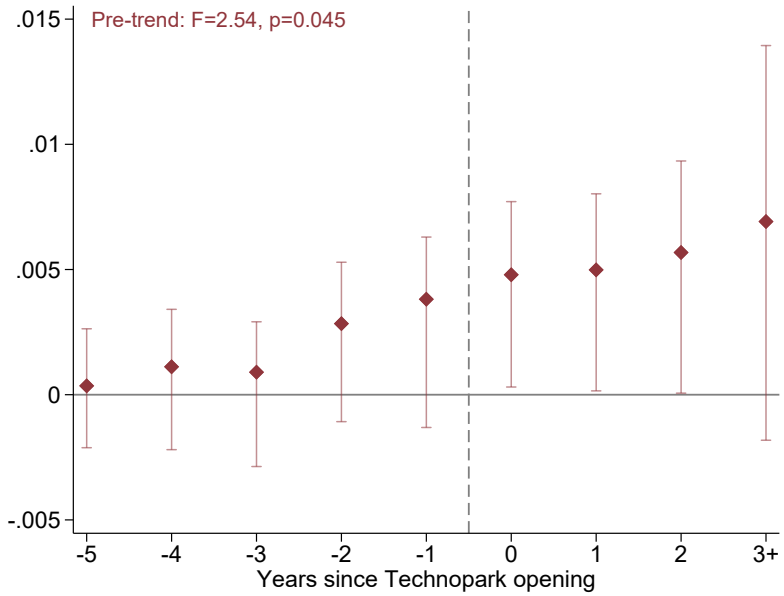
Note: The figure plots event-study estimates from the imputation estimator of Borusyak et al. (2024), complementing the matched event study in Figure 2. The estimates are computed on a district-year panel covering all districts, with district and NUTS-2-region-by-year fixed effects and no baseline covariates; districts are weighted by their 2009 gross sales, and the fixed effects of treated districts are anchored on the years immediately preceding the estimation window. Consistent with the imputation estimator, no period is normalised to zero: every pre-opening coefficient, including that for the year immediately before opening, is estimated as a placebo and plotted with its confidence interval. The final coefficient bins all horizons three or more years after opening ("3+"). Whiskers are 90 per cent cluster-bootstrap confidence intervals from resampling districts (300 replications). The annotation reports the joint test that the pre-opening coefficients are jointly zero.

Figure A7: Full-Sample Event Study, Technology-Intensive Exports

(A.) High-R&D exports



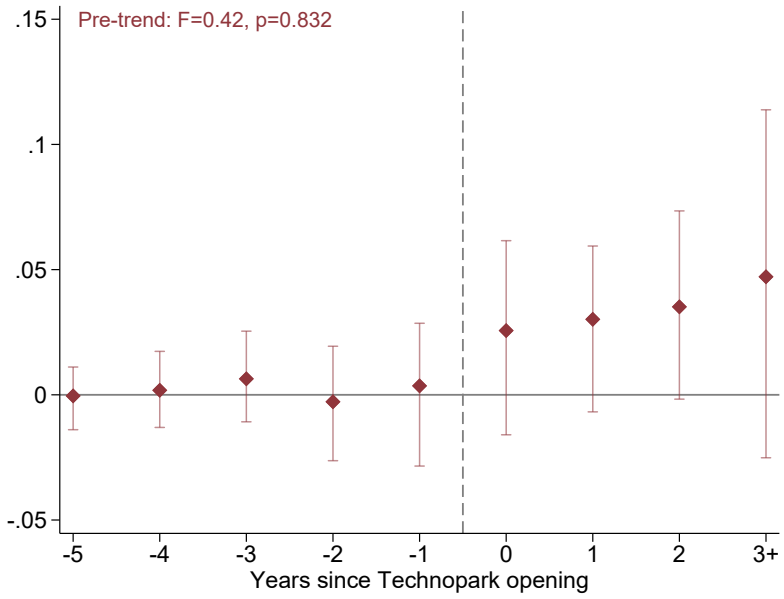
(B.) High-tech exports



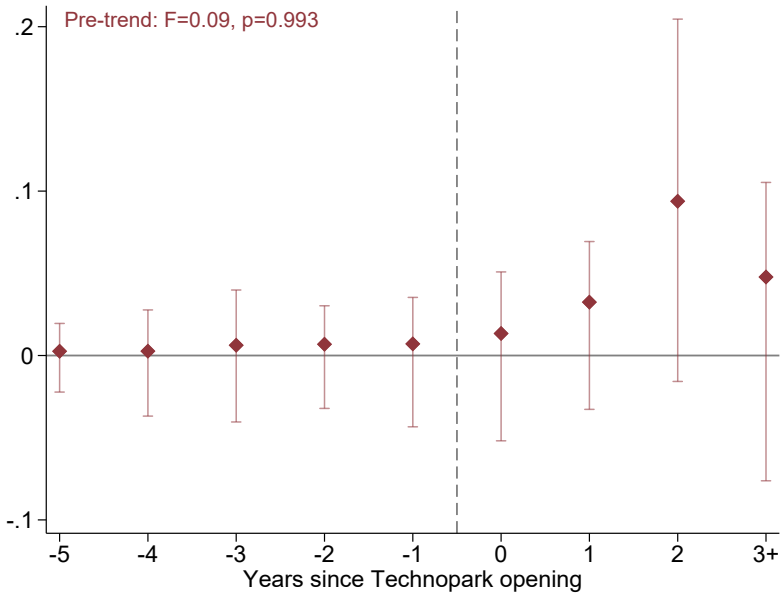
Note: The figure plots event-study estimates from the imputation estimator of Borusyak et al. (2024), complementing the matched event study in Figure 3. The estimates are computed on a district-year panel covering all districts, with district and NUTS-2-region-by-year fixed effects and no baseline covariates; districts are weighted by their 2009 gross sales, and the fixed effects of treated districts are anchored on the years immediately preceding the estimation window. Consistent with the imputation estimator, no period is normalised to zero: every pre-opening coefficient, including that for the year immediately before opening, is estimated as a placebo and plotted with its confidence interval. The final coefficient bins all horizons three or more years after opening ("3+"). Whiskers are 90 per cent cluster-bootstrap confidence intervals from resampling districts (300 replications). The annotation reports the joint test that the pre-opening coefficients are jointly zero.

Figure A8: Full-Sample Event Study, TFP and Revenue per Employee

(A.) Total factor productivity

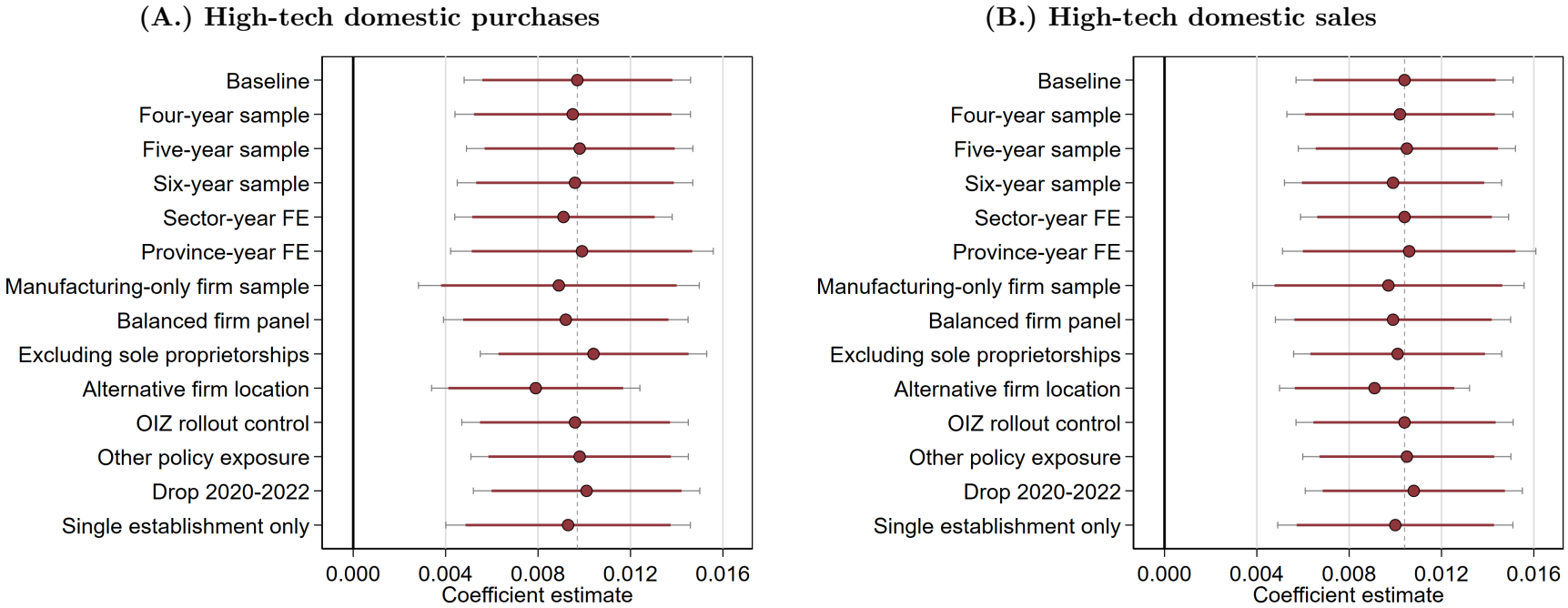


(B.) Revenue per employee (log)



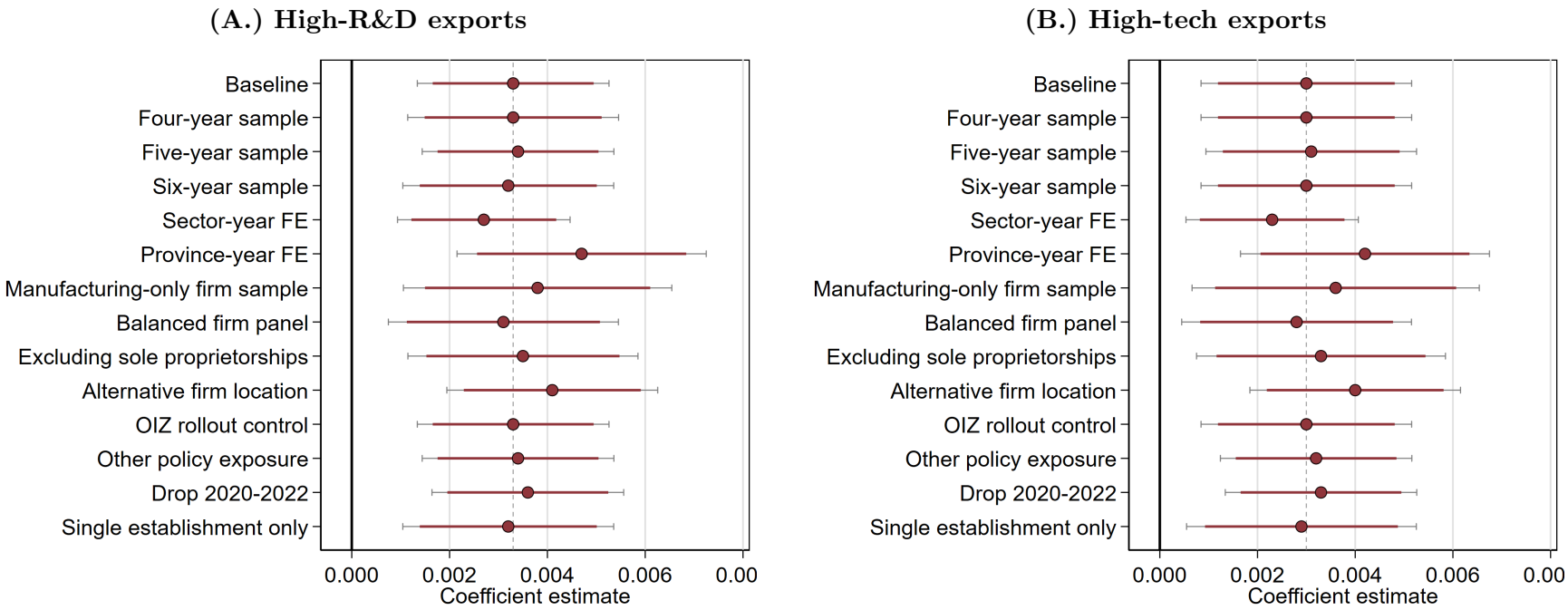
Note: The figure plots event-study estimates from the imputation estimator of Borusyak et al. (2024), complementing the matched event study in Figure 4. The estimates are computed on a district-year panel covering all districts, with district and NUTS-2-region-by-year fixed effects and no baseline covariates; districts are weighted by their 2009 gross sales, and the fixed effects of treated districts are anchored on the years immediately preceding the estimation window. Consistent with the imputation estimator, no period is normalised to zero: every pre-opening coefficient, including that for the year immediately before opening, is estimated as a placebo and plotted with its confidence interval. The final coefficient bins all horizons three or more years after opening ("3+"). Whiskers are 90 per cent cluster-bootstrap confidence intervals from resampling districts (300 replications). The annotation reports the joint test that the pre-opening coefficients are jointly zero.

Figure A9: Robustness of Technopark Effects on High-Tech Domestic Purchases and Sales



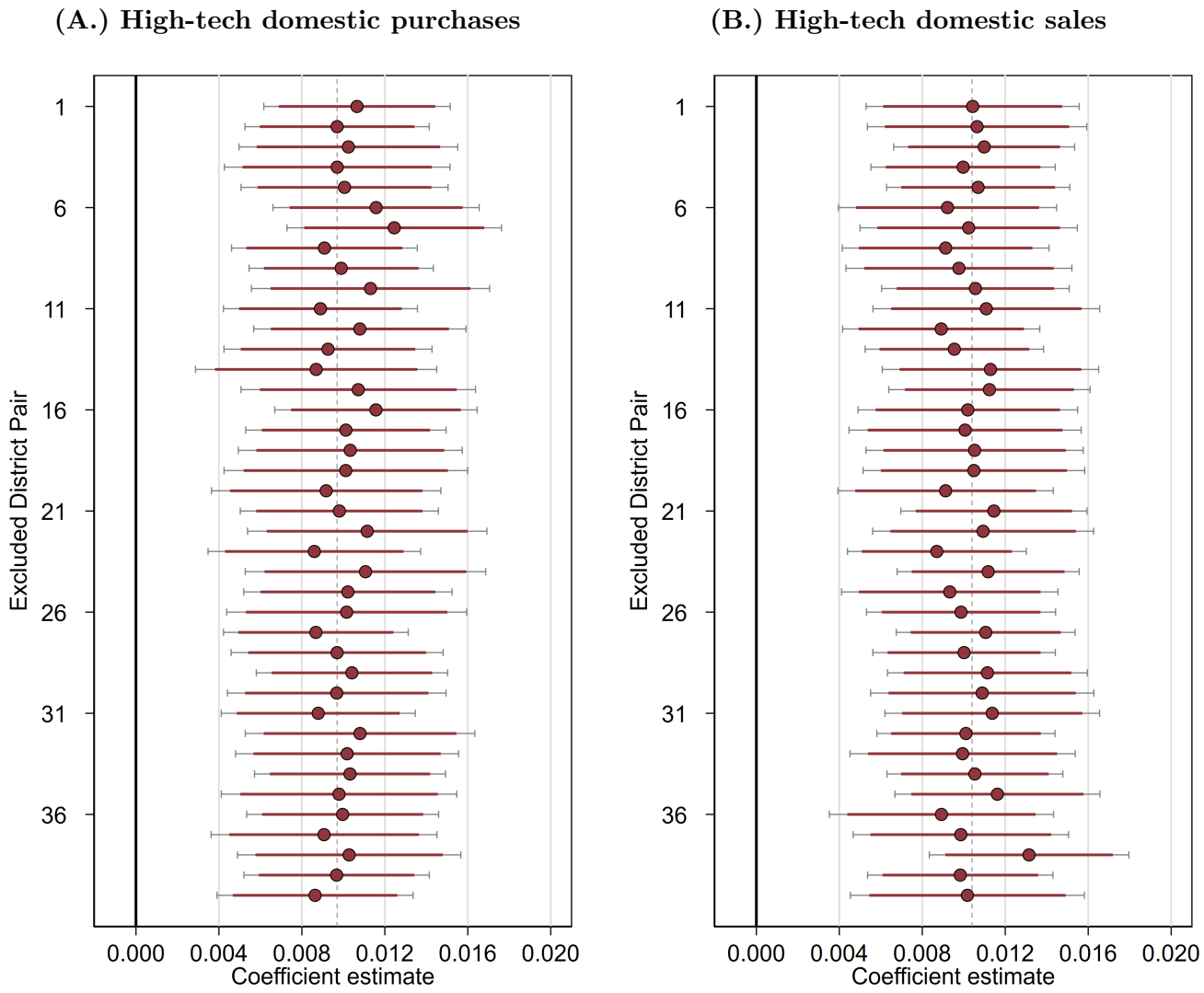
Note: Outcomes are indicators equal to one if a firm makes any high-tech domestic purchases or any high-tech domestic sales in a given year. Each dot reports the estimated coefficient on *Technopark* from a separate regression for the outcome shown in the relevant panel. The *Baseline* row reports the main estimate from Table 1. The *Four-year sample*, *Five-year sample*, and *Six-year sample* rows vary the minimum number of years a firm must be observed in the panel. *Sector-year FE* adds 1-digit sector-by-year fixed effects, while *Province-year FE* replaces the baseline matched-pair-by-year fixed effects with province-by-year fixed effects. *Manufacturing-only firm sample* restricts the sample to manufacturing firms, and *Balanced firm panel* restricts the sample to firms observed in all relevant years. *Excluding sole proprietorships* drops sole proprietors from the sample. *Alternative firm location* assigns firms to districts using an alternative location measure based on employment records, where a firm's district in each year is defined as the district in which it has the largest share of its workforce. *OIZ rollout control* controls for Organised Industrial Zone rollouts, *Other policy exposure* controls for other district-level industrial-policy exposure, *Drop 2020–2022* excludes the pandemic and immediate post-pandemic years, and *Single establishment only* restricts the sample to single-establishment firms, for which the district assignment is unambiguous. Coefficients are interpreted as percentage-point changes in the probability of the corresponding outcome after technopark opening. The thick dark-red horizontal bars report 90% confidence intervals, while the thinner grey capped whiskers report 95% confidence intervals, based on standard errors clustered at the district level. The solid vertical line marks zero. The dashed vertical line marks the baseline coefficient estimate for the corresponding outcome in that panel, providing a benchmark for comparing each robustness estimate with the main specification.

Figure A10: Robustness of Technopark Effects on Technology-Intensive Exports



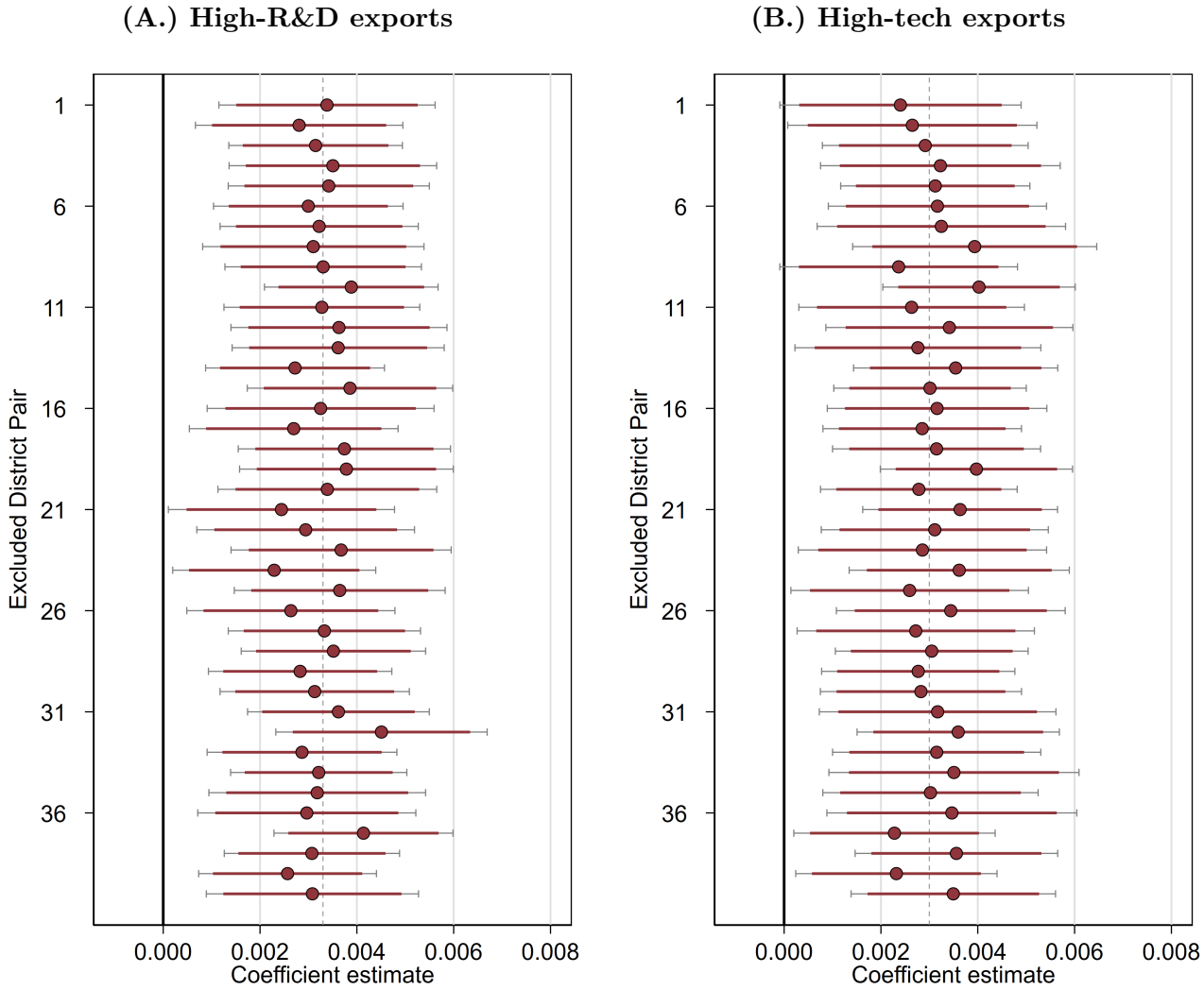
Note: Outcomes are indicators equal to one if a firm exports any product in the corresponding technology-intensive category in a given year. High-R&D exports are defined using the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes; high-tech exports are defined using the OECD/Eurostat technology-intensity classification mapped to HS product codes. Each dot reports the estimated coefficient on *Technopark* from a separate regression for the outcome shown in the relevant panel. The *Baseline* row reports the main estimate from Table 1. The *Four-year sample*, *Five-year sample*, and *Six-year sample* rows vary the minimum number of years a firm must be observed in the panel. *Sector-year FE* adds 1-digit sector-by-year fixed effects, while *Province-year FE* replaces the baseline matched-pair-by-year fixed effects with province-by-year fixed effects. *Manufacturing-only firm sample* restricts the sample to manufacturing firms, and *Balanced firm panel* restricts the sample to firms observed in all relevant years. *Excluding sole proprietorships* drops sole proprietors from the sample. *Alternative firm location* assigns firms to districts using an alternative location measure based on employment records, where a firm's district in each year is defined as the district in which it has the largest share of its workforce. *OIZ rollout control* controls for Organised Industrial Zone rollouts, *Other policy exposure* controls for other district-level industrial-policy exposure, *Drop 2020–2022* excludes the pandemic and immediate post-pandemic years, and *Single establishment only* restricts the sample to single-establishment firms, for which the district assignment is unambiguous. Coefficients are interpreted as percentage-point changes in the probability of the corresponding export outcome after technopark opening. The thick dark-red horizontal bars report 90% confidence intervals, while the thinner grey capped whiskers report 95% confidence intervals, based on standard errors clustered at the district level. The solid vertical line marks zero. The dashed vertical line marks the baseline coefficient estimate for the corresponding outcome in that panel, providing a benchmark for comparing each robustness estimate with the main specification.

Figure A11: Technopark Effects on High-Tech Domestic Trade, Leave-One-Pair-Out Sensitivity Estimates



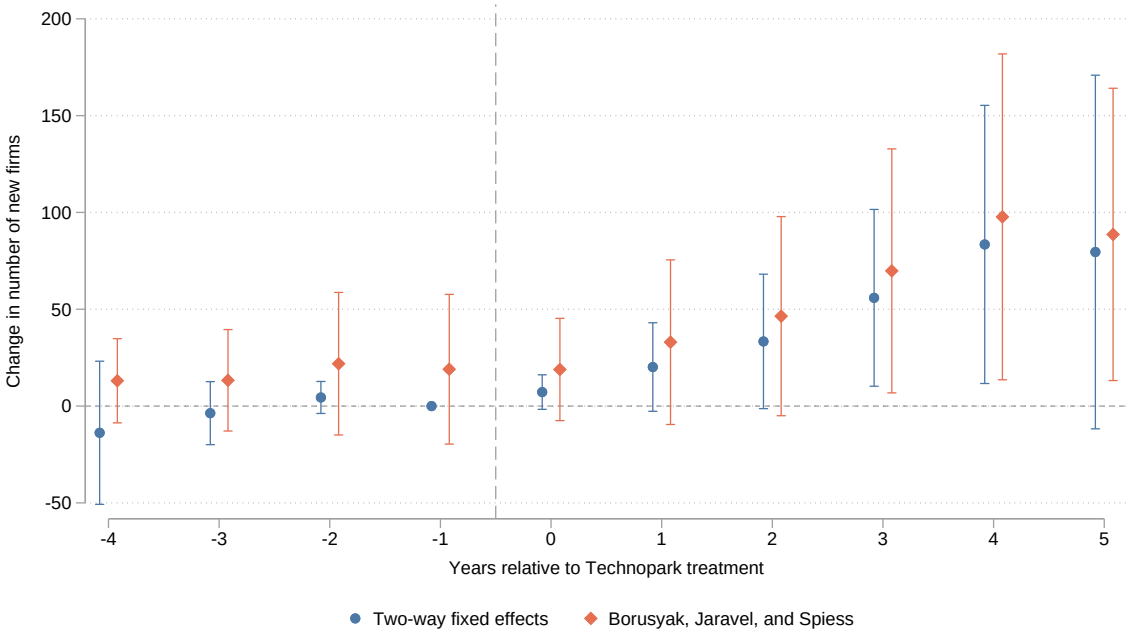
Note: Outcomes are indicators equal to one if a firm makes any high-tech domestic purchases or any high-tech domestic sales in a given year. Each dot reports the estimated coefficient on *Technopark* from a separate regression in which one matched district pair is excluded, iterated across all 40 pairs. The dashed vertical line marks the baseline coefficient estimate for the corresponding outcome in that panel, obtained using the full matched sample, providing a benchmark for comparing each leave-one-out estimate with the main specification. The thick dark-red horizontal bars report 90% confidence intervals, while the thinner grey capped whiskers report 95% confidence intervals, based on standard errors clustered at the district level. The solid vertical line marks zero. Coefficients are interpreted as percentage-point changes in the probability of the corresponding outcome after technopark opening.

Figure A12: Technopark Effects on Technology-Intensive Exports, Leave-One-Pair-Out Sensitivity Estimates



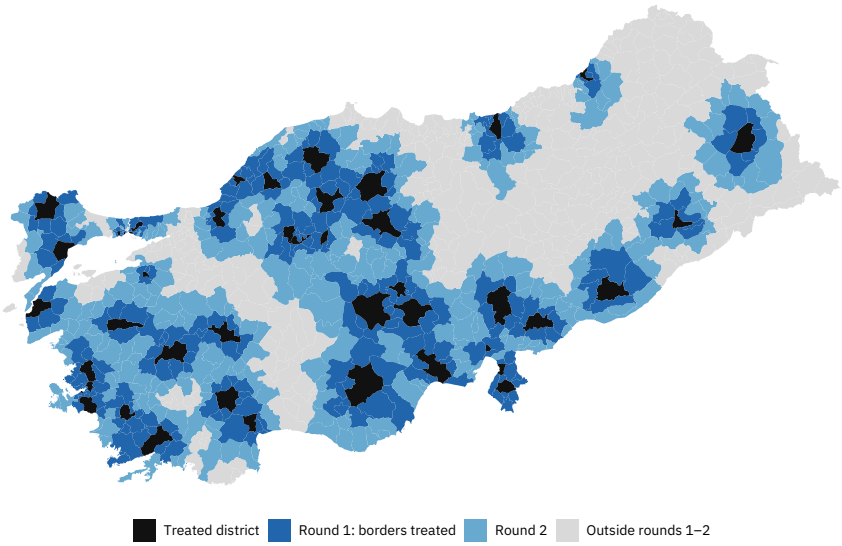
Note: Outcomes are indicators equal to one if a firm exports any product in the corresponding technology-intensive category in a given year. Each dot reports the estimated coefficient on *Technopark* from a separate regression in which one matched district pair is excluded, iterated across all 40 pairs. The dashed vertical line marks the baseline coefficient estimate for the corresponding outcome in that panel, obtained using the full matched sample, providing a benchmark for comparing each leave-one-out estimate with the main specification. The thick dark-red horizontal bars report 90% confidence intervals, while the thinner grey capped whiskers report 95% confidence intervals, based on standard errors clustered at the district level. The solid vertical line marks zero. Coefficients are interpreted as percentage-point changes in the probability of the corresponding export outcome after technopark opening.

Figure A13: Full-Sample Event Study of District-Level Firm Entry



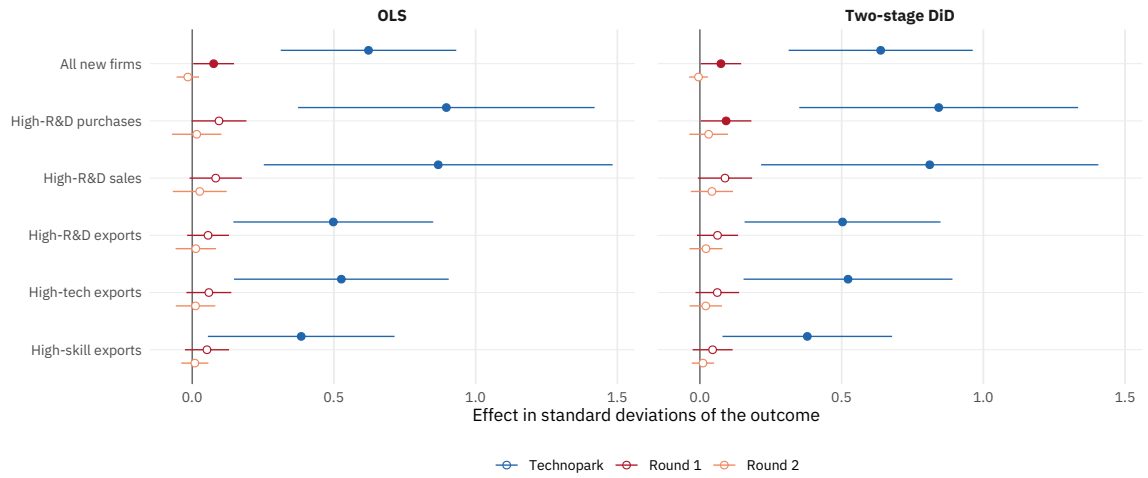
Note: The figure reports dynamic estimates of the effect of technopark establishment on the number of firms entering a district in a given year. The full national panel covers 2009–2023 and contains 908 districts with complete 2009 baseline covariates. Blue circles report conventional two-way fixed effects estimates, while orange diamonds report estimates from the imputation estimator of Borusyak et al. (2024). Both specifications include district fixed effects, NUTS-2-region-by-year fixed effects, and interactions between year indicators and the full set of 2009 district characteristics used for matching. For the two-way fixed effects specification, the year before opening is the omitted period and is plotted at zero; event times four or more years before opening and five or more years after opening are binned at the endpoints. The imputation estimator reports exact horizons and estimates the pre-opening coefficients, including the year immediately before opening, as placebo tests. Whiskers indicate 95% confidence intervals based on standard errors clustered at the district level. *Source:* Authors' calculations using Revenue Administration records.

Figure A14: Treated Districts and Contiguity Rings



Note: District boundaries are from the national *ilçe* shapefile, projected to EPSG:5637. Shading gives k_{dt}, the minimum number of district borders separating a district from the nearest operating technopark, evaluated in 2023, the last year of the panel. Districts more than two borders from every park form the comparison group in equation (7). Because the rings are recomputed each year against the parks operating in that year, a district shown here in an inner ring may have belonged to an outer one earlier in the sample.

Figure A15: Technopark and Contiguity-Ring Effects on Firm Entry: Standardised Coefficients



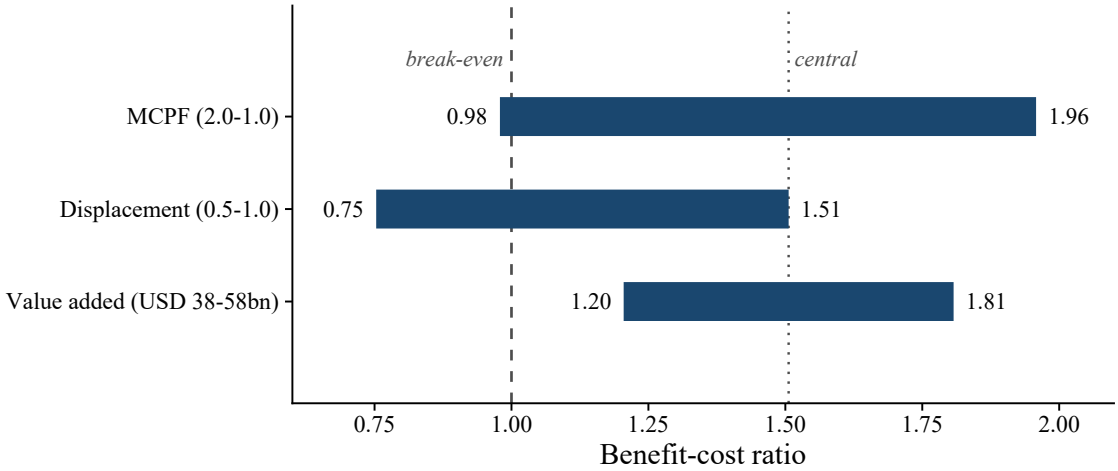
Note: The coefficients of Table 6, each divided by the standard deviation of its outcome among untreated district-years from 2012 on, so that the six counts share one axis. Bars are 95% confidence intervals from standard errors clustered on province; solid points are significant at the 5% level. The left panel is OLS and the right the two-stage estimator, corresponding to Panels A and B of the table. The intervals drawn here use the province-clustered standard errors only; the table reports the Conley standard errors alongside them.

Table A1: Sectoral Breakdown of Firms in Treated Districts

NACE	Sector	Share (%)
G	Wholesale and retail trade	35.1
C	Manufacturing	20.5
F	Construction	12.9
M	Professional, scientific and technical activities	7.3
H	Transportation and storage	5.2
I	Accommodation and food service activities	4.2
N	Administrative and support service activities	3.6
J	Information and communication	2.8
Q	Human health and social work activities	2.3
P	Education	1.5
L	Real estate activities	1.2
S	Other service activities	1.1
A	Agriculture, forestry and fishing	0.6
B	Mining and quarrying	0.5
R	Arts, entertainment and recreation	0.5
D	Electricity, gas, steam and air conditioning supply	0.4
E	Water supply; sewerage and waste management activities	0.3

Note: This table reports the sectoral composition of firms located in treated districts. Sectors are defined using 1-digit NACE classifications. Shares are expressed as percentages of all firms in treated districts.

Figure A16: One-Way Sensitivity of the Benefit-Cost Ratio



Note: Each bar shows how far the benefit-cost ratio moves as one assumption is varied across its plausible range while the others are held at their central values; a wider bar means the ratio depends more on that assumption. The marginal cost of public funds and the displacement adjustment matter most, while the value-added base, which is measured directly rather than assumed, matters least. The ratio stays above the break-even line of one across most of each range and falls below it only at the most adverse end of the two behavioural parameters, a marginal cost of public funds of 2.0 or a displacement adjustment of 0.5, so the conclusion does not hinge on any single assumption. The central case, shown by the dotted line, sets $\theta = 1$, $\lambda = 1.3$, and the value added of Appendix Table A12, and uses the 2022 fiscal cost; the dashed line marks break-even.

Table A2: District-Level Variables Used in Matching

Variable	Definition	Data source
Population density (log)	Log population per square kilometre	TurkStat
Population growth (%)	Annual district-level population growth rate	TurkStat
Household size	Average number of individuals per household	TurkStat
Men per 100 women	Number of men per 100 women in the district	TurkStat
Youth share, age 0–24 (%)	Share of district population aged 0–24	TurkStat
Active firms (log)	Log number of active firms in the district	Revenue Admin., CBRT
Total sales (log)	Log total sales of firms in the district	Revenue Admin., CBRT
Total employment (log)	Log total registered employment in the district	Revenue Admin., CBRT
Bank branches (log)	Log number of bank branches in the district	Credit Registry, CBRT
Ruling-party margin (pp)	Vote-share margin of the ruling party in the 2009 local elections	Supreme Electoral Board
Nighttime lights (log)	Log nighttime light intensity in the district	NOAA/NCEI DMSP-OLS

Note: This table lists the 2009 district-level covariates used to construct the matched control group. Each treated district is matched to one never-treated district in the same NUTS-2 region using propensity scores based on these covariates. Never-treated districts are districts that do not host a technopark at any point over the sample period; districts with a technopark established before 2009 are excluded from the control pool. Matching is implemented one-to-one without replacement and requires matches to fall within a caliper of 0.2 standard deviations of the estimated propensity score. All covariates refer to 2009 values.

Table A3: Balance of District-Level Characteristics Before and After Matching

Variable	(1) Treated mean (Raw, $n = 40$)	(2) Control mean (Raw, $n = 880$)	(3) Diff.	(4) Matched treated ($n = 40$)	(5) Matched control ($n = 40$)	(6) Diff.	(7) Norm. diff.
Population density (log)	5.92	3.97	1.95***	5.92	6.02	-0.10	-0.03
Population growth (%)	2.64	-0.86	3.50***	2.64	2.71	-0.07	-0.01
Household size	3.82	4.09	-0.27	3.82	3.76	0.06	0.06
Men per 100 women	100.99	100.62	0.37	100.99	101.10	-0.11	-0.02
Youth share, age 0–24 (%)	25.27	24.27	1.00	25.27	24.71	0.56	0.10
Active firms (log)	6.90	3.86	3.04***	6.90	6.50	0.40	0.20
Total sales (log)	21.63	17.47	4.16***	21.63	21.09	0.54	0.22
Total employment (log)	9.57	5.75	3.82***	9.57	9.19	0.38	0.19
Bank branches (log)	2.96	1.25	1.71***	2.96	2.64	0.32	0.18
Ruling-party margin (pp)	40.45	38.33	2.12	40.45	38.43	2.02	0.14
Nighttime lights (log)	8.75	7.31	1.44***	8.75	8.47	0.28	0.25

Note: This table reports the balance of district-level covariates before and after propensity score matching. Columns (1)–(3) show means for the 40 treated districts and the 880 never-treated candidate control districts, together with their differences. Columns (4)–(6) report the same statistics for the matched treated and control districts ($n = 40$ each). Column (7) reports normalised differences for the matched sample, calculated as the difference in means divided by the square root of the average of the treated and control variances; normalised differences below 0.25 in absolute value are conventionally taken to indicate adequate balance. Differences in columns (3) and (6) are tested using two-sample t-tests; no difference in column (6) is statistically significant at conventional levels. Variable definitions and data sources are reported in Table A2. All variables refer to 2009 values. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A4: Firm-Level Balance in Treated and Control Districts

Variable	Technopark district
Firm Size	-0.214
	(0.591)
Equity	0.012
	(0.034)
Sales	-0.021
	(0.045)
Current Assets	0.017
	(0.041)
Current Liabilities	-0.009
	(0.037)
Employees	0.003
	(0.019)
Joint significance (p-value)	0.641
Observations	125,109
Sector Fixed Effects	✓

Note: This table reports OLS estimates of the association between 2009 firm characteristics and location in a treated district, using the 2009 cross-section of firms in matched treated and control districts. The dependent variable, *Technopark district*, equals one if the firm is located in a district that eventually hosts a technopark over the sample period, and zero otherwise. Explanatory variables are: *Firm Size* (log of total assets), *Equity* (total equity over total assets), *Sales* (total sales over total assets), *Current Assets* (short-term assets over total assets), *Current Liabilities* (short-term liabilities over total assets), and *Employees* (number of employees). Firms with incomplete 2009 financial-statement information are excluded, which accounts for the difference between the number of observations and the total number of sample firms. All variables except *Employees* are winsorised at the 2nd and 98th percentiles to reduce the influence of outliers. The model includes 2-digit NACE sector fixed effects. *Joint significance* reports the p-value from an F-test of the null hypothesis that all firm characteristics are jointly unrelated to treated-district location. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A5: Technoparks and Intensive-Margin Firm Upgrading

Panel A. Intensive margin of high-tech domestic purchases and sales

	(1) **High-tech domestic purchases**	(2) **High-tech domestic sales**
Technopark	0.035***	0.024**
	(0.012)	(0.011)
Observations	1,518,853	1,367,056
Control mean	1,942.8	1,803.5
Approx. effect size	3.5%	2.4%

Panel B. Intensive margin of technology-intensive exports

	(1) **High-R&D exports**	(2) **High-tech exports**
Technopark	0.053***	0.041***
	(0.014)	(0.014)
Observations	421,800	410,682
Control mean	412.7	396.5
Approx. effect size	5.3%	4.1%
Firm FE	✓	✓
Pair × Year FE	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on intensive-margin measures of technology-intensive market activity. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Outcomes are values of domestic transactions or exports in the corresponding category. Domestic purchases and sales are constructed from firm-to-firm invoice records. Export outcomes are constructed from transaction-level customs records at the 6-digit HS product level. High-R&D exports are defined using the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes; high-tech exports are defined using the OECD/Eurostat technology-intensity classification mapped to HS product codes. *Technopark* equals one after a technopark becomes operational in the district. Estimates are obtained using Poisson pseudo-maximum-likelihood (PPML) regressions; coefficients are semi-elasticities, and approximate effect sizes interpret the coefficient as an approximate percentage change rather than as the ratio of the coefficient to the control mean. PPML estimation drops firms with no positive outcome over the sample period, which accounts for the smaller estimation samples. Control means for domestic outcomes are expressed in TRY thousands and for export outcomes in USD thousands. We interpret these value-based outcomes as complementary evidence because transaction and export values are skewed and may reflect lumpy contracts or shipment timing. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A6: Technoparks and Firm Upgrading (Full-Sample Estimates)

Panel A. Extensive margin

	(1) High-tech domestic purchases	(2) High-tech domestic sales	(3) High-R&D exports	(4) High-tech exports
Technopark	0.011***	0.010***	0.004***	0.004***
	(0.003)	(0.003)	(0.001)	(0.001)
Observations	8,046,965	8,046,965	8,046,965	8,046,965
Mean dep. variable	0.281	0.239	0.061	0.060
Effect size	3.9%	4.2%	6.6%	6.7%

Panel B. Intensive margin

	(1) High-tech domestic purchases	(2) High-tech domestic sales	(3) High-R&D exports	(4) High-tech exports
Technopark	0.074***	0.076***	0.022***	0.021***
	(0.023)	(0.024)	(0.006)	(0.007)
Observations	5,101,326	4,603,737	1,400,534	1,362,090
Mean dep. variable	1.611	1.401	0.310	0.300
Approx. effect size	7.4%	7.6%	2.2%	2.1%
Firm FE	✓	✓	✓	✓
NUTS-2 × Year FE	✓	✓	✓	✓
Baseline covariates × Year FE	✓	✓	✓	✓

Note: This table reports full-sample estimates of the effect of technopark establishment on firm upgrading outcomes, using the universe of Turkish non-financial firms observed over 2009–2023. Panel A reports extensive-margin outcomes, defined as indicators equal to one if the firm makes any high-tech domestic purchases, any high-tech domestic sales, or exports any product in the corresponding technology-intensive category in a given year. Domestic outcomes are constructed from firm-to-firm invoice records; export outcomes are constructed from transaction-level customs records at the 6-digit HS product level. High-R&D exports are defined using the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes; high-tech exports are defined using the OECD/Eurostat technology-intensity classification mapped to HS product codes. *Technopark* equals one after a technopark becomes operational in the district. Coefficients in Panel A are interpreted as percentage-point changes in the probability of the corresponding outcome; effect sizes express the coefficient relative to the mean dependent variable. Because the full sample is not restricted to matched pairs, we report the mean of the dependent variable rather than a control-group mean. Panel B reports intensive-margin outcomes, defined as firm-year values of domestic transactions or exports in the corresponding technology-intensive category. Intensive-margin outcomes are estimated using Poisson pseudo-maximum-likelihood (PPML) regressions; coefficients are semi-elasticities, and approximate effect sizes interpret the coefficient as an approximate percentage change. PPML estimation drops firms with no positive outcome over the sample period, so the Panel B observation counts are smaller than the extensive-margin sample. Mean dependent variables for domestic outcomes are expressed in TRY millions, while mean dependent variables for export outcomes are expressed in USD millions. All specifications in both panels include firm fixed effects, NUTS-2-region-by-year fixed effects, and interactions between year fixed effects and the 2009 district covariates listed in Table A2. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A7: Technoparks and Low-Tech Business Linkages

Panel A. Low-tech supplier linkages

	(1) **All low-tech suppliers**	(2) **Within-district low-tech suppliers**	(3) **Across-district low-tech suppliers**
Technopark	0.041**	0.056*	0.034*
	(0.020)	(0.030)	(0.018)
Observations	2,377,439	2,377,439	2,377,439
Control mean	2.139	1.001	1.903
Approx. effect size	4.1%	5.6%	3.4%

Panel B. Low-tech customer linkages

	(1) **All low-tech customers**	(2) **Within-district low-tech customers**	(3) **Across-district low-tech customers**
Technopark	0.013	0.019	0.012
	(0.014)	(0.019)	(0.014)
Observations	2,377,439	2,377,439	2,377,439
Control mean	2.034	0.992	1.773
Approx. effect size	1.3%	1.9%	1.2%
Firm FE	✓	✓	✓
Pair × Year FE	✓	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on low-tech business linkages. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Outcomes are log one plus the number of distinct low-tech supplier or customer firms in a firm-year, constructed from firm-to-firm invoice records. Control means are means of the log-transformed outcome. *Technopark* equals one after a technopark becomes operational in the district. Coefficients are semi-elasticities, and approximate effect sizes interpret the coefficient as an approximate percentage change rather than as the ratio of the coefficient to the control mean. Effect sizes for statistically insignificant coefficients are reported for completeness. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A8: Technoparks and Low-Sophistication Export Margins

	Low-R&D exports		Low-tech exports	
	(1)	(2)	(3)	(4)
	Extensive margin	**Intensive margin**	**Extensive margin**	**Intensive margin**
Technopark	0.0007	-0.003	0.0010	-0.002
	(0.0007)	(0.018)	(0.0008)	(0.016)
Observations	2,377,439	360,884	2,377,439	371,264
Control mean	0.054	33.0	0.057	37.0
Effect size / Approx. effect size	1.3%	-0.3%	1.8%	-0.2%
Firm FE	✓	✓	✓	✓
Pair × Year FE	✓	✓	✓	✓

Note: This table reports firm-level estimates of the effect of technopark establishment on low-sophistication export margins. The sample consists of pre-existing firms in matched treated and control districts over 2009–2023. Export outcomes are constructed from transaction-level customs records at the 6-digit HS product level. Low-R&D and low-tech exports are defined as exports outside the high-R&D and high-tech categories, respectively, where the high-R&D category follows the Galindo-Rueda and Verger (2016) R&D-intensity taxonomy mapped to HS product codes and the high-tech category follows the OECD/Eurostat technology-intensity classification mapped to HS product codes. Extensive-margin outcomes in columns (1) and (3) are indicators for whether a firm exports products in the corresponding low-sophistication category in a given year; coefficients report percentage-point changes in the corresponding probability, and effect sizes express the coefficient relative to the control mean. Intensive-margin outcomes in columns (2) and (4) are export values, estimated using Poisson pseudo-maximum-likelihood (PPML) regressions; coefficients are semi-elasticities, and approximate effect sizes interpret the coefficient as an approximate percentage change rather than as the ratio of the coefficient to the control mean. The *Effect size / Approx. effect size* row accordingly reports effect sizes for the extensive-margin columns and approximate effect sizes for the intensive-margin columns. PPML estimation drops firms with no positive outcome over the sample period, which accounts for the smaller estimation samples. Control means for intensive-margin outcomes are expressed in USD thousands. Effect sizes for statistically insignificant coefficients are reported for completeness. *Technopark* equals one after a technopark becomes operational in the district. All columns include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. A matched pair consists of one treated district and its matched control district, selected using pre-treatment district characteristics within the same NUTS-2 region. Pair-by-year fixed effects absorb shocks common to the treated and control district within each matched pair in each year. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A9: Heterogeneous Effects of Technoparks by Firm Size

Panel A. High-tech domestic purchases and sales

	(1) **High-tech domestic purchases**	(2) **High-tech domestic sales**
Technopark	0.0045*	0.0047*
	(0.0024)	(0.0025)
Technopark × Larger firm	0.0209***	0.0228***
	(0.0025)	(0.0038)
Observations	2,377,439	2,377,439

Panel B. Technology-intensive exports

	(1) **High-R&D exports**	(2) **High-tech exports**
Technopark	0.0019*	0.0015
	(0.0010)	(0.0011)
Technopark × Larger firm	0.0057***	0.0059***
	(0.0011)	(0.0015)
Observations	2,377,439	2,377,439

Panel C. Productivity and revenue per employee

	(1) **Total factor productivity**	(2) **Revenue per employee (log)**
Technopark	0.0054	0.0120***
	(0.0098)	(0.0030)
Technopark × Larger firm	0.1360***	0.0445***
	(0.0028)	(0.0024)
Observations	2,088,303	1,145,078
Firm FE	✓	✓
Pair × Year FE	✓	✓

Note: This table reports heterogeneity in the effect of technopark establishment by firm size among pre-existing firms in matched treated and control districts over 2009–2023. *Technopark* equals one after a technopark becomes operational in the firm's district. *Larger firm* is an indicator for firms above the 75th percentile of the baseline employment distribution, corresponding to more than 10 employees. Because this firm-size indicator is time-invariant, its standalone coefficient is absorbed by firm fixed effects and is therefore not reported. The coefficient on *Technopark* captures the effect for smaller firms. The coefficient on *Technopark* × *Larger firm* captures the additional effect for larger firms relative to smaller firms. The total effect for larger firms is given by the sum of these two coefficients, but is not reported separately in the table. Panel A reports high-tech domestic value-chain outcomes. Panel B reports technology-intensive export outcomes. Panel C reports firm productivity and revenue per employee. Total factor productivity is constructed from administrative financial statements using the Olley and Pakes (1996) method, as in Table 2. Revenue per employee is defined as the log of gross revenue divided by the firm's 2009 employee count, capturing changes in revenue relative to the firm's baseline workforce size while avoiding mechanical changes from contemporaneous hiring after technopark exposure. All specifications include firm fixed effects and matched-pair-by-year fixed effects, as indicated at the bottom of the table. Standard errors, clustered at the district level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A10: Network Spillovers: Robustness to Industry-by-Year Shocks

	Technopark (own district) $\hat{\beta}$	**Supplier exposure** $\hat{\pi}^{\text{sup}}$	**Customer exposure** $\hat{\pi}^{\text{cus}}$	N
Technology-intensive trade				
High-tech domestic purchases	0.016***	0.001	0.015***	1,415,231
	(0.005)	(0.006)	(0.005)	
High-tech domestic sales	0.012**	0.017***	0.017***	1,415,231
	(0.005)	(0.006)	(0.006)	
High-R&D exports	0.004**	0.002	0.004*	1,415,231
	(0.002)	(0.003)	(0.002)	
High-tech exports	0.003**	0.002	0.004*	1,415,231
	(0.002)	(0.003)	(0.002)	
Productivity and technology adoption				
Log TFP	0.019	−0.022	0.030*	1,332,747
	(0.017)	(0.018)	(0.017)	
Automation adoption	0.002*	0.000	0.004**	1,415,231
	(0.001)	(0.001)	(0.001)	
High-tech employment share	0.420***	0.568**	0.345	1,011,357
	(0.110)	(0.223)	(0.308)	
Firm FE	✓	✓	✓	
Pair × year FE	✓	✓	✓	
NACE-1 × year FE	✓	✓	✓	

Note: This table repeats the network-spillover regressions of Table 5 with 1-digit-NACE-sector-by-year fixed effects added, which absorb any shock common to a sector in a given year and address the concern that customer exposure proxies for an industry-wide demand shock rather than a technopark spillover. Exposure shares, sample, and clustering are as in Table 5. The customer-exposure coefficients are essentially unchanged from the baseline specification, confirming that the downstream spillover does not reflect industry-wide shocks. Standard errors, clustered at the district level, are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A11: Network Spillovers: Joint Buyer–Supplier Exposure

	Baseline fixed effects		+ NACE-1 × year FE		
	Technopark	Joint exp.	Technopark	Joint exp.	N
Technology-intensive trade					
High-tech domestic purchases	0.016***	0.006	0.016***	0.007	1,415,231
	(0.006)	(0.004)	(0.005)	(0.004)	
High-tech domestic sales	0.011**	0.016***	0.012**	0.017***	1,415,231
	(0.006)	(0.004)	(0.005)	(0.004)	
High-R&D exports	0.005**	0.004*	0.004**	0.003	1,415,231
	(0.002)	(0.002)	(0.002)	(0.002)	
High-tech exports	0.005**	0.003	0.003**	0.003	1,415,231
	(0.002)	(0.002)	(0.002)	(0.002)	
Productivity and technology adoption					
Log TFP	0.027	0.005	0.019	0.000	1,332,747
	(0.020)	(0.012)	(0.017)	(0.012)	
Automation adoption	0.002*	0.002***	0.002*	0.002**	1,415,231
	(0.001)	(0.001)	(0.001)	(0.001)	
High-tech employment share	0.487***	0.490***	0.420***	0.474***	1,011,357
	(0.109)	(0.159)	(0.110)	(0.159)	
Firm FE	✓	✓	✓	✓	
Pair × year FE	✓	✓	✓	✓	
NACE-1 × year FE			✓	✓	

Note: This table repeats the network-spillover regressions of Table 5 under the restriction that upstream and downstream exposure carry the same coefficient. The *Joint exp.* column reports the single coefficient on the sum of the supplier and customer exposure shares, $s_{it}^{\text{sup}}+s_{it}^{\text{cus}}$, imposing $\pi^{\text{sup}} = \pi^{\text{cus}}$; the *Technopark* column is the direct own-district effect. Exposure shares, sample, fixed effects, and clustering are as in Table 5. Because supplier exposure is close to zero, the pooled coefficient dilutes the customer-side signal and is weaker than the customer-only estimate for every export and productivity outcome, which is why the separate specification in Table 5 is the preferred reading. Standard errors, clustered at the district level, are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A12: Fiscal Cost of the Technopark Programme, by Exemption

	2022		2023	
Exemption (Law No. 4691)	TL bn	USD bn	TL bn	USD bn
Corporate-income exemption	5.17	0.31	13.73	0.58
R&D wage-withholding exemption	4.92	0.30	13.04	0.55
Value-added-tax exemption	1.46	0.09	3.82	0.16
Income-tax profit exemption	0.30	0.02	0.73	0.03
Total	**11.85**	**0.72**	**31.32**	**1.32**

Note: This table reports the annual revenue forgone under Law No. 4691, compiled by the authors from five annual editions of the *Vergi Harcamaları Raporu* of the Revenue Administration (2020–2024, together 1,927 pages), cross-checked against the Ministry of Treasury and Finance tax-expenditure lists. Nominal lira are converted at annual-average market exchange rates of 16.55 in 2022 and 23.75 in 2023 (Central Bank of the Republic of Türkiye). The totals exclude the employer social-security premium support for exempt research personnel and the Ministry's infrastructure outlays to zone-managing companies, so they understate the full fiscal cost. On the benefit side we value the productivity effect as $\theta\, g\, VA$, where g is the effect, θ is the net-of-displacement share, and VA is the value added of the treated-district incumbent sample, and compare it to λC, where λ is the marginal cost of public funds and C is the cost above. The value-added base of about 48 billion dollars is the sum of 2022 value added across the same firms used to estimate the effect. The central case sets $\theta = 1$ and $\lambda = 1.3$; Figure A16 reports one-way sensitivity of the benefit-cost ratio across the ranges we consider.

Table A13: Network Spillovers: Full-Sample Estimates

	Technopark (own district) $\hat{\beta}$	**Supplier exposure** $\hat{\pi}^{\text{sup}}$	**Customer exposure** $\hat{\pi}^{\text{cus}}$	N
Technology-intensive trade				
High-tech domestic purchases	0.013***	−0.001	−0.004	2,993,061
	(0.004)	(0.003)	(0.003)	
High-tech domestic sales	0.011***	0.006**	0.004*	2,993,061
	(0.004)	(0.003)	(0.003)	
High-R&D exports	0.004***	0.002	0.000	2,993,061
	(0.001)	(0.001)	(0.001)	
High-tech exports	0.004**	0.001	0.000	2,993,061
	(0.001)	(0.001)	(0.001)	
Productivity and technology adoption				
Log TFP	0.043***	−0.012	0.009	2,874,460
	(0.014)	(0.009)	(0.008)	
Automation adoption	0.001*	0.003***	0.000	2,993,061
	(0.001)	(0.001)	(0.001)	
High-tech employment share	0.281***	0.192	0.149	1,958,155
	(0.082)	(0.132)	(0.118)	
Firm FE	✓	✓	✓	
NUTS-2 × year FE	✓	✓	✓	
Baseline covariates × year FE	✓	✓	✓	

Note: This table extends the network-spillover specification in Table 5 to the full national firm panel, including districts that are never treated. Each row is a single regression of the row outcome on the own-district technopark indicator and the two exposure shares. *Supplier exposure* is the share of the firm's input purchases sourced from suppliers located in treated districts, and *Customer exposure* is the share of its sales made to customers located in treated districts. Both measures are constructed from firm-to-firm invoice records, exclude counterparties located in the firm's own district, and hold transaction weights fixed in 2011, the year before the first technopark opening in the estimation window. Exposure therefore varies over time only as a firm's pre-existing trading partners become treated. The sample covers 2009–2023 and is restricted to firms observed in at least four years and to observations with non-missing 2011 exposure shares and baseline district covariates. All regressions absorb firm and NUTS-2-by-year fixed effects, together with interactions between year indicators and the 2009 district characteristics listed in Table A2. Standard errors, clustered at the district level, are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.