Employment Protection and the Direction of Technology Adoption*

Andrea Manera — MIT

Martina Uccioli — MIT

January 11, 2021

Abstract

We study the impact of employment protection legislation (EPL) on firms' innovation choices, through an event-study analysis of several labor market reforms occurring in Europe over 2000-2016. Data on firms' technology adoption from the Community Innovation Survey reveal that substantial drops in EPL for temporary workers prompt a reallocation of innovation efforts towards the introduction of new products, away from process innovation aimed at cutting labor costs. Among innovative firms, the share of product innovators increases by 15% of the pre-reform value (10pp in absolute terms), while the share of firms specializing in process innovation falls by 35% (also 10pp).

*We thank Amy Finkelstein and Simon Jäger for invaluable advice throughout the project. We also thank Edoardo Acabbi, Daron Acemoglu, Joshua Angrist, David Autor, Martin Beraja, Ludovica Ciasullo, Enrico De Magistris, Michele Fornino, Jonathan Gruber, Ilse Lindenlaub, Ioana Marinescu, Garima Sharma, John Van Reenen and participants at the MIT Economics PF Lunch, Labor Lunch, Third Year Lunch and the Boston University WEorg mentoring workshop for helpful discussions and suggestions. We acknowledge use of microdata from the Community Innovation Survey under RPP 94/2020-CIS-SES-MMD. We specify that the results and conclusions are those of the authors and not those of Eurostat, the European Commission or any of the national statistical authorities whose data have been used.

In recent years, the increase in inequality and the progress of automation have put labor standards back on center stage, and intensified the calls for stricter employment regulation. In the US, the proposed Schedules that Work Act of 2019—"to require employers to provide more predictable and stable schedules for employees"—, and the New Jersey S3170 bill—increasing advance notices and mandating severance payments for workers in mass layoffs—are just two of numerous examples.[1] The proponents of these measures often see them as a way of mitigating the adverse employment effects of technological trends.

However, employment protection measures may *themselves* impact firms' technology choices, as several models predict. For example, in the framework of Saint-Paul (2002), firms can invest in product innovations that create new varieties of a certain good, or process innovations that reduce labor costs. Since new varieties face the risk of being displaced by process innovators, product innovators face higher revenue volatility and a larger risk of laying off employees. As a result, stricter employment protection legislation that increases firing costs can induce firms to reallocate resources from product to process innovation.

We test this hypothesis empirically, analyzing the role of employment protection legislation (EPL) on firms' innovation choices across Europe over the years 2000-2016, when several countries substantially loosened regulations on fixed-term contracts. We combine data from the European Community Innovation Survey with measures of strictness of EPL compiled by the OECD to assess the effects of this deregulation on the direction of innovation and technology adoption. We focus on the choice between process innovation—aimed at modifying production techniques—and product innovation—aimed at the development and introduction of new or improved goods and services. We believe this distinction to be important, since new goods or services can only weakly increase overall welfare, while process innovation may reduce it when coupled with significant workers' displacement.[2] These considerations also highlight that process and

[1] See https://www.congress.gov/bill/116th-congress/house-bill/5004/text and https://www.njleg.state.nj.us/2018/Bills/S3500/3170_I1.HTM

[2] A case in point is provided by automation—a type of process innovation—when its displacement

product innovation entail different distributional effects.

We study manufacturing firms in 18 European countries, adopting an event-study design around large reductions in EPL for temporary workers (the 2.5 percentile of the distribution of EPL changes). Our estimates reveal a sizable—albeit noisy—increase in the share of temporary workers of 30 – 50% of the pre-period value. This suggests that these reforms were successful in increasing the take-up of temporary contracts, reducing firms' firing costs.

Consistent with the theory outlined above, we find that these large EPL reductions increased the share of innovative firms engaging in product innovation by 10pp (15% of the pre-reform share), and reduced the fraction of firms engaged exclusively in process innovation by 10pp (35% of the pre-reform share). At the same time, the share of innovative firms—either product or process innovators—was essentially unchanged by the reforms. These findings suggest that the reduction in average firing costs occasioned by big EPL drops made product innovation relatively more attractive than process innovation. As a result, firms which were not conducting any product innovation started to do so, partially reallocating their efforts away from process innovation.

These results are robust to alternative sample definitions, and to the inclusion of flexible covariate-specific time trends capturing features of the labor market and industry composition. We provide suggestive evidence in support of the identifying assumptions underlying the event-study design through an interaction-weighted estimation of pre-trends (Sun and Abraham, Forthcoming) and a randomization exercise (Hsiang and Jina, 2014).

Related Literature. Our paper contributes to the literature on the effects of EPL on firm and worker outcomes, as well as macroeconomic variables.[3] Recent empirical

effect on workers outweighs its productivity effect (Acemoglu and Restrepo, 2020b). The distinction between process and product innovation may also matter in the presence of differences in knowledge spillovers, arising from differential patenting activity (Hall et al., 2014).

[3] We refer the interested reader to Boeri et al. (2015), Duval and Furceri (2018), Daruich et al. (2019), and García-Pérez et al. (2019) for recent examples of studies in this area and a complete discussions of the related literature.

studies directly identify the effect of labor regulations on specific types of innovation. Griffith and Macartney (2014), Acharya et al. (2014) and Aghion et al. (2019) focus on the distinction between incremental and radical innovation, finding contrasting results. Garcia-Vega et al. (2019) show that a reduction in the strictness of labor laws leads to an increase in product innovation, and Bena et al. (2020) show that process innovation patents increase with EPL strictness. We departs from the bulk of this literature by focusing on the *adoption* of process and product innovation—which includes, but is not limited to, patented inventions (Maclaurin, 1953). Moreover, we conduct a joint analysis of multiple reform episodes across countries, and highlight a pattern of substitution between these two innovation types, which is novel to the literature.

We empirically test predictions from several theoretical papers on the effects of labor market institutions on innovation. This literature has primarily focused on the impact of labor rigidity on overall innovation activity, with varying conclusions (positive according to Acemoglu, 1993; Acemoglu and Pischke, 1998, 1999; Belot et al., 2002; Acharya et al., 2014; negative for Malcomson, 1997; Cuñat and Melitz, 2012; Aghion et al., 2019).[4] Few papers have focused on the *direction* of innovation activity, generally finding that highly regulated labor markets encourage process innovation, which may or may not come at the expense of product innovation (Boone, 2000; Saint-Paul, 2002; Alesina et al., 2018; Fornino and Manera, 2019).[5] In particular, we test predictions from Saint-Paul (2002), who models the tension between labor laws imposing high firing costs and risky investments in product innovation. We therefore emphasize the particular channel linking EPL to innovation through its negative impact on risky economic activities, documented in Samaniego (2006); Bartelsman et al. (2011); Cuñat and Melitz (2012).

The paper is organized as follows. Section 1 briefly sketches the conceptual framework that guides the interpretation of our results; Section 2 describes the data; Section 3 presents our findings, and Section 4 concludes.

[4]See Kleinknecht et al. (2014); Griffith and Macartney (2014) for further references.

[5]Bassanini and Ekkehard (2002) argue instead that high firing costs discourage labor-saving (process) innovations among incumbents.

1 Conceptual Framework

This section derives testable implications for our empirical analysis from the framework proposed by Saint-Paul (2002). In the model, firms can choose between investing in product or process innovation, with product innovation involving higher employment risk in equilibrium. This feature is consistent with results in Bartelsman et al. (2011) and Samaniego (2006), and is reproduced by correlations from CIS microdata, reported in Appendix B.4. We derive three predictions. First, low firing costs are associated with relatively high (low) product (process) innovation. Second, sizable reductions in firing costs generate a reallocation of resources toward product innovation, and away from process innovation. Third, a decrease in firing costs triggers reallocation only if initial firing costs are sufficiently high. Our conceptual framework is deliberately minimal, and we acknowledge that it captures just one of the many channels through which EPL could affect innovation choices. Despite its simplicity, the model reproduces many of the patterns that arise in the data. We provide a simplified exposition of the model, and refer the reader to Saint-Paul (2002) for additional details. Appendix A provides a graphical analysis.

Environment and Equilibrium. The world economy comprises two countries, indexed by $c \in \{1,2\}$, which differ by the size of the firing costs, F_c, paid when employment relations are destroyed.[6] In each country, monopolistically competitive firms produce a continuum of tradable product varieties. Each firm acquires its monopolistic position by introducing a new product (product innovation); or by cutting the unit labor cost of an existing good and replacing the incumbent (process innovation). Both types of firms face an exogenous destruction risk, but product innovators face the additional risk of being displaced by process innovators at an endogenous "product displacement rate," ν. We assume that products can only be imitated once, so process innovators do not get further displaced.[7]

[6] The two-country assumption allows full specialization while maintaining simplicity.

[7] Milder assumptions on decreasing returns to process innovation would preserve the risk ranking between the two activities.

Each country is endowed with a fixed stock of researchers which firms allocate to their preferred innovation type. Product innovation succeeds at an exogenous rate, γ, while process innovation has an endogenous success rate η. This rate depends positively on the amount of new products to imitate, and negatively on the aggregate number of researchers allocated to this innovation type, as each scientist is less likely to be the first to develop a process innovation.[8] More researchers devoted to process innovation also imply a higher product displacement rate, ν. Therefore, the technology "hazard frontier" is a downward-sloping locus in the (ν, η) space.

Labor is the only factor in a linear production function, so the unit cost of each good is a linear decreasing function of country-specific firing costs and the probability of shutdown, which depends on the type of innovation pursued. Therefore, the expected profits, $\Pi_c^{product}(\nu; F_c)$ and $\Pi_c^{process}(F_c)$, depend negatively on their arguments.

The indifference condition,

$$R_c(\nu, \eta; F_c) \equiv \frac{\gamma \Pi_c^{product}(\nu; F_c)}{\eta \Pi_c^{process}(F_c)} = 1 \tag{1}$$

summarizes firm's innovation incentives in country c. When the indifference condition holds, the expected returns from the two innovation activities are equalized, and both product and process innovation are carried out in country c. When this condition is violated, each firm in country c specializes in the same type of innovation (process if $R_c(\cdot) < 1$; product otherwise). Condition (1) defines an implicit function, which gives rise to two downward sloping schedules—one for each country—in the (ν, η) space. The equilibrium of this model is a pair, (ν^*, η^*), obtained combining the two countries' indifference conditions (1) with the hazard frontier. Note that the schedule $R_c(\nu, \eta; F_c)$ shifts down with increases in F_c, as higher firing costs make (riskier) product innovation less profitable relative to process innovation. Since the hazard frontier is also downward-sloping, a reduction in firing costs weakly increases (reduces) equilibrium ν^* (η^*).

[8] See Saint-Paul (2002) for an example of technology production function with these features.

More in general, the model determines two types of equilibria. When the equilibrium rates, ν^*, η^*, fall on the indifference schedule of either country, this country carries out both types of research, while the other specializes. Otherwise, both countries fully specialize, and the world ratio of product-to-process innovators is simply the relative endowment of researchers in the two countries.

Model implications. The model makes three main predictions. First, firing costs are negatively correlated with product innovation across countries. Intuitively, a country with low firing costs is better equipped to face the high uncertainty that product innovation entails. This is consistent with the cross-sectional evidence reported in Appendix B.4. Second, a fall in firing costs makes product innovation relatively more profitable, as firms incur lower expected losses for any given product displacement rate, ν, which may trigger a reallocation away from process innovation. Finally, the model implies that the extent of reallocation from process towards product innovation depends on the initial level of firing costs, as well as on the size of their change. This is apparent when examining indifference condition (1). For fixed (ν, η), a decrease in firing costs, F_c increases the relative return, R_c, as product innovators face a higher destruction probability, making expected profits more responsive to shifts in the firing cost. If in equilibrium country c is already specialized in product innovation, $R_c(\nu^*, \eta^*; F_c) > 1$, this change in firing costs only strengthens the already existing incentives to specialize in product innovation. If instead country c specializes in process innovation, $R_c(\nu^*, \eta^*; F_c) < 1$, a sufficiently large decrease in firing costs can turn the inequality into equality, triggering a reallocation of researchers from process to product innovation.

2 Data and Sample Selection

2.1 Community Innovation Survey (CIS)

The Community Innovation Survey (Eurostat, 2000-2016a) asks a sample of European enterprises about their innovation activities. This survey is particularly suitable for our analysis for several reasons. First, it allows us to distinguish product from process innovation, which is usually unfeasible in administrative data sources, which mainly report overall R&D investments. Second, the survey inquires about both technological innovation and adoption, regardless of whether they required in-house R&D activity, or whether they lead to inventions and patenting. This provides a more complete picture of innovation than balance-sheet and patent data sources. Finally, firms also report the intended objectives of their innovation activities, shedding light on their motives.

The CIS surveys enterprises with 10 employees or more.[9] Coordinated by Eurostat (the EU statistical agency), it is carried out by national statistical agencies but designed to ensure comparability across countries. We restrict our attention to the eight waves carried out between 2000 and 2016, which feature consistent variable definitions.

We focus on two main variables: product innovation and process innovation. A firm is a "product innovator" if it answered affirmatively to the question: "During the period [year of survey minus 2 - year of survey], did your enterprise introduce onto the market any new or significantly improved products (goods or services)?"[10] A firm is instead classified as "a process innovator" based on the answer to the question: "During the period [year of survey minus 2 - year of survey], has your enterprise introduced any new or significantly improved production processes including methods of supplying services and ways of delivering products?" In both cases, the CIS questionnaire specifies that "the innovation (new or improved) must be new to your enterprise, but it does not need to be new to your sector or market. It does not matter if the innovation was originally developed by your enterprise or by other enterprises." Finally, "Innovative" firms are those that fall in either of the above classifications. In our theoretical framework, the relative share of process to product innovators is a sufficient statistic for the allocation of researchers across the two activities.

The CIS refers surveyed firms to the Oslo Manual (OECD, 2005), which reports some clarifying examples of innovation types. Examples of process innovation include: in-

[9] Appendix Table B.1 reports coverage summary statistics.

[10] See e.g. `https://ec.europa.eu/eurostat/documents/203647/203701/CIS_Survey_form_3.pdf`.

stallation of new or improved manufacturing technology, such as automation equipment; new equipment required for new products; new or improved software. Examples of product innovation are: integrating products (e.g. cameras in phones); improvements in energy efficiency of products; new or improved services (e.g. internet banking). Both designations explicitly exclude marginal or purely cosmetic changes.

Table 1 summarizes these key variables for the manufacturing sector across the countries in our sample.[11]

Process innovation as a proxy for labor-cost-cutting technology. The survey also inquires about innovators' motives. One of the questions asks: "How important was [to reduce labour costs per unit output] as objective for your activities to develop product or process innovations during [the last three years]?" [12] Respondents choose between "High," "Medium," "Low," and "Not relevant." Panel (a) of Figure 1 shows the share of process and product innovators grouped by answers to this question. Only 50% of the respondents who consider labor cost reductions irrelevant are process innovators, as opposed to 76% carrying out product innovation (firms can carry out both activities at once). As the objective to reduce labor costs moves from low to high, the fraction of respondents carrying out process innovation increases from 73% to 86%, compared to a slight decrease in product innovators. This pattern emerges starkly in Panel (b), reporting the fraction of firms which conduct only one innovation activity at time. Consistent with our conceptual framework, process innovation is often seen as a way to reduce unit labor costs.

2.2 OECD Indicators of Employment Protection

We obtain our indicator of strictness of Employment Protection Legislation (EPL) from the OECD (OECD, 2000-2018). EPL refers to the body of legislation that regulates procedures and costs involved in hiring and dismissing workers, individually or collectively,

[11] Appendix Table B.2 expands to all sectors.

[12] This question comes from the 2010 survey (`https://ec.europa.eu/eurostat/documents/203647/203701/CIS_Survey_form_2010.pdf`). Other waves ask comparable questions.

and that governs fixed-term and temporary work. This measure maps to the model's firing costs. The OECD has developed three main numerical indicators of EPL that are comparable across countries and over time. Two of these indices refer to legislation regulating open-ended employment contracts, focusing on individual and collective dismissals. The third index measures employment protection for temporary workers. All three indices range from 0 to 6 and aggregate a number of sub-components. We focus on the index of EPL strictness for temporary employment.[13] In what follows, we refer to this indicator as "EPL for temporary workers," "EPL Temp," or simply "EPL." While nothing in the theory indicates that the correct measure to use is EPL for temporary—rather than regular—workers, there were no large changes in EPL for regular workers over the sample period. Thus, large changes in EPL Temp are a good proxy for large changes in the general level of EPL in early-2000 European countries.[14]

The EPL for temporary workers measures the strictness of regulations on fixed-term contracts and temporary work agency employment. This index is constructed as the simple average of two sub-components relating to fixed-term contracts and temporary work agencies. Each of these two sub-components is itself constructed as the weighted average of three indices, measuring: the scope of application of temporary contracts (with weight 1/2); their maximum number of renewals (1/4); and their maximum duration in months (1/4). For example, the sub-component relative to the scope of application of fixed-term contracts takes a value of 6 (strictest EPL) if these contracts can only be used for tasks that require a fixed amount of time to be carried out; a value of 4 if either employers or employees can be exempted from restrictions (2 if both can be exempted); and a value of 0 (lightest EPL) if there are no restrictions. The other two indices are constructed discretizing the underlying quantities.

Big EPL Drops. We consider a country as experiencing a "big EPL drop" in a specific year, if the country-year pair registers a change in EPL for temporary workers smaller

[13]For further details on this measure and its evolution over our sample period, see Appendix B.3.

[14]Appendix C.4 provides further discussion and presents alternative results that use EPL for regular workers to define treatment.

than −20% (2.5 percentile of EPL changes). This procedure singles out five countries as treated: Germany, Greece, Italy, Portugal and Sweden. All these countries implemented measures to extend the duration or scope of fixed-term contracts.[15] By contrast, the majority of countries experienced no or small changes in this measure over the sample period (see the standard deviations in column 5 of Table 1), and provide suitable controls for our analysis. Appendix B.3 presents a detailed description of level and changes in EPL across Europe over the sample years.

We validate the selection of large EPL drops against the database of "major narrative labor market reforms" assembled by Duval et al. (2019). All the drops that we select appear in the database; excluded episodes do not appear, or represent reforms that were subsequently reversed.[16] We set the treatment date to the year in the sample that sees a reduction in EPL, reported in column 6 of Table 1.[17]

2.3 Sample selection

Our raw sample consists of 27 countries appearing in both the CIS and OECD dataset of EPL strictness. We drop Croatia and Slovenia, for which we have less than two matched observations across these two datasets, and seven other countries that see a large increase in EPL for temporary workers over the sample period (20% or more, the 97.5 percentile in the distribution of changes).[18] This leaves 18 countries in the baseline sample. We focus on the manufacturing sector, which is consistently surveyed by all countries for all waves in which they participated. We verify the robustness of our re-

[15] In Italy, for example, Law 368 of 2001 introduced a "generic reason" for the use of temporary contracts to a previously highly restrictive list, which decreased EPL Temp by 26.8%. We provide a short summary of reform episodes in Appendix Table B.3 .

[16] For example, we exclude the EPL drop which occurred in Spain in 2011, when restrictions on temporary work agencies were reduced. The following year, EPL was tightened through a reduction in the maximum duration of temporary contracts.

[17] In all treated countries reforms happened in odd years, except for Germany (2002), and the CIS only reports data biannually. Therefore, we harmonize the treatment variable attributing to Germany the treatment year 2001, the last odd year before the reform.

[18] These countries are: Czechia, Estonia, Hungary, Ireland, Poland, Slovakia and the United Kingdom. Many of these are countries where EPL changes occur around the date of EU accession. Further, when these large increases are studied as events, there are clear pre-trends in all our outcomes of interest.

sults to including all available sectors in Appendix C.

3 Empirical Strategy and Results

3.1 Empirical Strategy

We run the event-study regression

$$Y_{it} = \alpha_i + \delta_t + \sum_{e=m}^{n} \kappa_e \times \mathbb{1}\{t - (\text{Event Year})_i = e\} \times \mathbb{1}\{\text{Treated}\}_i + \epsilon_{it}, \tag{2}$$

where Y_{it} indicates outcome Y for country i in year t, α_i and δ_t are country and time fixed effects respectively, $\mathbb{1}\{t - (\text{Event Year})_i = e\}$ are indicators for year e relative to the "big drop in EPL" event taking place at $t = (\text{Event Year})_i$, and $\mathbb{1}\{\text{Treated}\}_i$ indicates that country i is treated. Thus, coefficients κ_e capture the effect of treatment at event time e (we normalize $\kappa_{-1} = 0$). Due to the biannual nature of the survey, the range $[m, n]$ is composed of odd numbers in $[-7, +15]$ (excluding -5).[19] We report standard errors clustered at the country level, as well as wild-bootstrap confidence intervals (Cameron et al., 2008). We also run the difference-in-differences specification

$$\begin{aligned} Y_{it} = \alpha_i + \delta_t + \beta_{SR} \times \mathbb{1}\{t - (\text{Event Year})_i \leq 6\} \times \mathbb{1}\{\text{Treated}\}_i + \\ + \beta_{LR} \times \mathbb{1}\{t - (\text{Event Year})_i > 6\} \times \mathbb{1}\{\text{Treated}\}_i + \epsilon_{it}, \end{aligned} \tag{3}$$

which splits the post-treatment period into "short run" (up to six years after treatment) and "long run" (years seven and above).

Identification Assumptions. We require three main assumptions for our baseline specification to identify the average treatment-on-the-treated effects of EPL reductions (Sun and Abraham, Forthcoming): parallel trends between treated and never-treated coun-

[19] As the κ_{-7} coefficient is always estimated very imprecisely, we report event-study graphs from lag -3. Full results are available on request.

tries; no anticipation effects; and no selection into early versus late treatment.

We take several steps to mitigate concerns about a violation of the parallel-trends assumption. First, when estimating Equation (2) we test for, and always reject, the presence of significant pre-trends. This is also true when adopting the specification from Sun and Abraham (Forthcoming), which addresses the concern of spurious pre-trend test results.[20] Event-study coefficients could also be biased by the omission of relevant time-varying covariates, like other features of the labor market and industry composition. We tackle this threat by restricting our baseline analysis to firms in the manufacturing sector, and conducting a robustness exercise where we include interactions of time dummies with a rich set of covariates.

The biennial structure of our panel, combined with the absence of pre-trends, comforts us about the absence of substantial anticipation effects on our variables. In addition, these variables seem to react slowly to policy, as suggested by treatment effects manifesting in the long run.

We confirm the robustness of our results to relaxing the assumption of treatment effect homogeneity in Appendix D.1, where interaction-weighted event-study coefficients produce unchanged estimates relative to our baseline (Sun and Abraham, Forthcoming). Appendix D.2 further assesses selection concerns through a randomization inference exercise.

3.2 Results

EPL Temp and Share of Temporary Workers. Panel (a) of Figure 2 summarizes the results of the event study analysis when the dependent variable is the EPL index itself. The figure shows that the event "big EPL drop" reflects a permanent level shift in EPL Temp, allowing us to interpret the event-study estimates as responses to *permanent* changes in EPL for temporary workers. In panel (b), we report event-study coefficients

[20]The authors show that when treatment effects are heterogeneous, each event-study coefficient averages over all leads and lags of cohort-specific treatment effects, invalidating the use of pre-period coefficients to test for pre-trends.

for the share of temporary workers over total employment in manufacturing (Eurostat, 2000-2016b). The midpoints of our estimates reveal a sizable—albeit noisy—increase in the share of temporary workers following these reforms, corresponding to $30-50\%$ of the pre-reform value. This pattern is consistent with micro-level results in Daruich et al. (2019). We interpret the increase in the share of temporary workers as suggestive that the reforms succeded in promoting the take-up of fixed-term contracts, lowering average firing costs for our reference population of firms.

Main Results. Figure 3 displays our main results. We plot the event-study coefficients around large EPL drops together with the confidence bands resulting from cluster-robust standard errors and a wild bootstrap procedure. Common to all panels is the absence of significant pre-trends in the variables of interest.

Panel (a) depicts the path of the share of innovators over the total number of firms surveyed. The event-study coefficients are mostly non-significant, suggesting that labor market reforms did not increase overall innovation activity. The remaining panels present a pattern of reallocation across different types of innovation. In particular, panel (d) displays the significant drop in the ratio of process innovators to product innovators in the years following the event. This ratio falls by an average of 0.25 in the long run (after 7 years from the event), about 25% of the pre-treatment average (1.03). Panels (b) and (c) show that this result is driven by a mild and noisy decrease in process innovation coupled with a sizable and significant increase in product innovation—the long-run increase of 0.1 in the fraction of product innovators corresponds to about 15% of the pre-treatment average (0.67). We believe that effects manifest in the long run for two reasons. First, in view of high firing costs for regular workers, firms might be slow to adjust the composition of their workforce, relying on retirement and voluntary separations to replace regular workers with newly-hired temporary workers. Second, aggregate innovation activity can respond with a lag to reforms, both because of the slow workforce adjustment mentioned above, and because firms might be reluctant to interrupt multi-year innovation projects close to completion.

Panels (e) and (f) extend our main findings to firms that exclusively conduct either process or product innovations.[21] The share of innovative firms that implement only process innovations fell sharply and significantly by about 0.1—more than 35% of the pre-treatment average. Over the same treatment period, the share of firms conducting only product innovations increased by the same amount, resulting in a fall of about 60% of the "process only" to "product only" innovators ratio. Thus, labor market reforms seem to reduce the attractiveness of process innovation when this is the sole activity of the firm. This contrasts with panel (c), which shows that overall process innovation—including firms that also carry out product innovation—is not significantly affected by EPL reductions. These findings suggests that firms cut on innovations aimed exclusively at labor costs reductions, while they keep conducting other process innovations that are needed to support the introduction of new products. These results depose in favor of a general reallocation of innovation activity from process innovation—often motivated by a desire to reduce labor costs—towards product innovation.

Table 2 reports the coefficients from the difference-in-differences model (3). These results highlight that significant effects only emerge in the long run. In particular, columns (2), (4) and (5) confirm the significant long-run increase in product innovation, decrease in exclusive process innovation, and reduction in the ratio of process innovators to product innovators.

Robustness to Additional Covariates. Column 6 of Table 2 assesses the robustness of our results to the inclusion of time dummies interacted with the value in year 2000 of several variables capturing other institutional features of the labor market and sectoral composition. We focus on the ratio of process innovators to product innovators as our outcome, a measure that summarizes the reallocation across different innovation activities, and that fully characterizes the equilibrium of the model when overall innovation activity is fixed. Our controls are: automation potential, an employment-weighted average of adjusted robot penetration, and task offshoring—as measured in Autor and Dorn (2013)—across detailed manufacturing sectors from Acemoglu and Re-

[21] Recall that our measure of product (process) innovators includes any firm that implemented product (process) innovations, regardless of whether said firm has also introduced other types of innovation.

strepo (2020a); EPL for regular workers (both individual and collective dismissal); the manufacturing capital-labor ratio from EU KLEMS (Van Ark and Jäger, 2017); and total labor spending on labor market policies as a percentage of GDP from the OECD, which combines active labor market policies and unemployment benefits. The inclusion of interacted controls naturally results in a substantial degree of freedom reduction, as well as a sample restriction due to data availability (reported in Appendix Table C.3). Estimates for the long-run treatment effect are unchanged relative to the baseline. Appendix C.3 discusses further robustness exercises.

Treatment Effect Heterogeneity. Appendix C.1 discusses three additional sets of results. First, we find that the effect of weakening EPL on the process-product innovators ratio is strongest for small firms (10-49 employees) and decreases with size. This is likely because larger firms have access to less burdensome collective dismissal procedures (Aleksynska and Muller, 2020) or because they have enough funds to pursue multiple projects, which dampens their response on the extensive margin of innovation. Second, we split countries by their initial EPL level, so that we compare high(low)-EPL treated countries to high(low)-EPL control countries, and run separate regressions on both sample partitions. Third, we divide treated countries into two groups according to the size of the EPL drop, and separately compare these groups to all control countries. All these exercises restrict the size of the treatment group, resulting in imprecise estimates. Nevertheless, our results suggest that labor market reforms have a sizable and significant effect on innovative activities only when the starting EPL is high, and that only relatively large EPL reductions trigger the reallocation of innovation from process to product, verifying two other predictions of the model.

Robustness to Alternative Sample Definitions. We consider two alternative sample definitions in Appendix C.2. First, we expand the sample of firms to include all sectors (in addition to manufacturing). Our main results carry over to this setting. Second, we limit the sample to two panels of eleven countries each, balanced around the time of the event. Coincidentally, the latter exercise corresponds to the heterogeneity by size of the EPL drop discussed in the above paragraph.

Robustness to EPL for Regular Workers. Three of the treated countries (Greece, Italy and Portugal) and two control countries (Spain and Denmark) face large relaxations of EPL for regular workers over the sample period. In Appendix C.4, we propose two exercises to account for these episodes. First, we run our baseline specification excluding countries that see large drops in EPL for *regular* workers, which produces broadly similar estimates. Second, we include a set of event-time dummies around big drops in EPL for regular workers, constructed following the same criteria for the EPL Temp measure. This produces estimates that are almost identical to our baseline.[22]

Alternative Estimation Strategies. Appendix D considers two alternative estimation strategies. First, we obtain event-study coefficients as weighted averages of cohort-specific treatment effects at each horizon, following Sun and Abraham (Forthcoming). This set of estimates confirms our baseline results, and allows us to exclude significant pre-trends in our variable of interests. Second, we obtain randomization t-statistic p-values (MacKinnon and Webb, 2020) for the long-run difference-in-differences coefficient, reassigning residualized values of the long-run treatment dummy along three dimensions (Hsiang and Jina, 2014): within countries over time; between countries, leaving time assignments unchanged; and along both dimensions. The resulting p-values from all schemes confirm the significance of our main results, and suggest that they are not driven by systematic bias in time and country assignment to treatment.

4 Conclusion

In this paper, we have investigated the effect of changes in employment protection legislation across European countries on the direction of technology adoption. Our findings show that countries that eased the use of temporary contracts have experienced

[22] Appendix C.4 also reports estimates using drops in EPL for regular workers as the main event. The smaller import of these episodes results in non-significant estimates for the outcomes of interest.

an increase in product innovation activities. Our results also suggest that this increase occurred through a reallocation away from process innovation—which is generally motivated by a desire to cut labor costs. The effects are sizable, implying an increase in the share of innovative firms engaging in product innovation by about 10pp, and a corresponding decrease in innovation activity directed exclusively to production process improvements. We have interpreted our results in light of the model proposed by Saint-Paul (2002), which features an endogenous choice between product and process innovation. In this framework, firing costs directly affect the direction of innovation because introducing new products is riskier than just improving existing ones.

From a policy standpoint, our findings add a new rationale for structural labor market reforms, by highlighting their impact on the direction of innovation activity. Our results suggest that countries with high firing costs naturally direct their research efforts toward process innovations to reduce labor costs, at the detriment of innovations that might expand the range or increase the quality of existing products. Depending on which type of innovation is more relevant for economic growth, the direction of innovation activity towards product or process innovation might be more or less desirable.

We identify two avenues for future research. First, further theoretical work is warranted to draw formal normative conclusions on the effects of EPL on the direction of innovation. Mapping our results to aggregate welfare is not straightforward. Indeed, while spillovers from the two activities could provide a sufficient statistic in a representative agent economy, welfare evaluation in more general settings is complicated by their distributional effects (such as worker displacement from labor-substituting technologies, or product innovation that is biased towards wealthier agents). Second, we would like to expand our research to the analysis of the direction of process innovation towards labor-complementing or labor-substituting technologies, building groups of "complementing" and "substituting" innovators. This could be accomplished through the use of the full CIS firm-level data, beyond the subset currently available to researchers.

References

D. Acemoglu. Labor Market Imperfections and Thick Market Externalities from Innovation. Mimeo, MIT, 1993.

D. Acemoglu and J.-S. Pischke. Why Do Firms Train? Theory and Evidence. *The Quarterly Journal of Economics*, 113(1):79, 1998.

D. Acemoglu and J.-S. Pischke. Beyond Becker: Training in Imperfect Labour Markets. *The economic journal*, 109(453):112–142, 1999.

D. Acemoglu and P. Restrepo. Replication data for: Robots and jobs: Evidence from US labor markets. *Journal of Political Economy*, 128(6):2188–2244, 2020a. doi: https://doi.org/10.1086/705716.

D. Acemoglu and P. Restrepo. Robots and jobs: Evidence from US labor markets. *Journal of Political Economy*, 128(6):2188–2244, 2020b.

V. V. Acharya, R. P. Baghai, and K. V. Subramanian. Wrongful Discharge Laws and Innovation. *Review of Financial Studies*, 27(1):301–346, Jan. 2014.

P. Aghion, A. Bergeaud, and J. Van Reenen. The Impact of Regulation on Innovation. Working Paper, 2019.

M. Aleksynska and A. Muller. The Regulation of Collective Dismissals: Economic Rationale and Legal Practice. ILO Working Paper 4, 2020.

A. Alesina, M. Battisti, and J. Zeira. Technology and Labor Regulations: Theory and Evidence. *Journal of Economic Growth*, 23(1):41–78, Mar. 2018.

D. Autor and D. Dorn. The Growth of Low-Skill Service Jobs and the Polarization of the US Labor Market. *American Economic Review*, 103(5):1553–97, 2013.

E. J. Bartelsman, P. A. Gautier, and J. De Wind. Employment Protection, Technology Choice, and Worker Allocation. De Nederlandsche Bank Working Paper 295, May 2011.

A. Bassanini and E. Ekkehard. Labour Market Regulation, Industrial Relations and Technological Regimes: A Tale of Comparative Advantage. *Industrial and Corporate Change*, 11(3):391–426, June 2002.

M. Belot, J. Boone, and J. C. Van Ours. Welfare effects of employment protection. CEPR Discussion Paper, 2002.

J. Bena, H. Ortiz-Molina, and E. Simintzi. Shielding Firm Value: Employment Protection and Process Innovation. Working paper, 2020.

T. Boeri, P. Cahuc, and A. Zylberberg. The Costs of Flexibility-Enhancing Structural Reforms. OECD Economics Department Working Paper 1264, 2015.

J. Boone. Technological Progress, Downsizing and Unemployment. *The Economic Journal*, 110(465):581–600, July 2000.

A. C. Cameron and D. L. Miller. A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources*, 50(2):317–372, Mar. 2015.

A. C. Cameron, J. B. Gelbach, and D. L. Miller. Bootstrap-Based Improvements for Inference with Clustered Errors. *Review of Economics & Statistics*, 90(3):414–427, Aug. 2008.

A. Cuñat and M. J. Melitz. Volatility, Labor Market Flexibility, and the Pattern of Comparative Advantage. *Journal of the European Economic Association*, 10(2):225–254, Apr. 2012.

D. Daruich, S. Di Addario, and R. Saggio. The Effects of Partial Employment Protection Reforms: Evidence from Italy. Working paper, 2019.

R. Duval and D. Furceri. The Effects of Labor and Product Market Reforms: The Role of Macroeconomic Conditions and Policies. *IMF Economic Review*, 66(1):31–69, 2018.

R. Duval, D. Furceri, B. Hu, J. Tovar Jalles, and H. Nguyen. A Narrative Database of Major Labor and Product Market Reforms in Advanced Economies. IMF Working Paper 18/19, 2019.

Eurostat. Community Innovation Survey: Series inno_cis3; inno_cis4; inno_cis2006; inno_cis2008; inno_cis2010; inno_cis2012; inno_cis2014; inno_cis2016, 2000-2016a. URL `https://ec.europa.eu/eurostat/web/main/data/database`. Accessed on 01.11.2021.

Eurostat. EU Labour Force Survey: Series lfsq_emp; lfsq_egana; lfsq_egan2; lfsq_egana2d; lfsq_etgana; lfsq_etgan2, 2000-2016b. URL `https://ec.europa.eu/eurostat/web/main/data/database`. Accessed on 01.11.2021.

M. Fornino and A. Manera. Automation and the Future of Work: Assessing the Role of Labor Flexibility. Working Paper, 2019.

J. I. García-Pérez, I. Marinescu, and J. Vall Castello. Can Fixed-term Contracts Put Low Skilled Youth on a Better Career Path? Evidence from Spain. *The Economic Journal*, 129(620):1693–1730, 2019.

M. Garcia-Vega, R. Kneller, and J. Stiebale. Labour Market Reform and Innovation: Evidence from Spain. Research Paper 17, University of Nottingham, 2019.

R. Griffith and G. Macartney. Employment Protection Legislation, Multinational Firms, and Innovation. *The Review of Economics and Statistics*, 96(1):135–150, 2014.

B. Hall, C. Helmers, M. Rogers, and V. Sena. The Choice between Formal and Informal Intellectual Property: A Review. *Journal of Economic Literature*, 52(2):375–423, 2014.

S. M. Hsiang and A. S. Jina. The Causal Effect of Environmental Catastrophe on Long-Run Economic Growth: Evidence From 6,700 Cyclones. NBER Working Paper w20352, National Bureau of Economic Research, July 2014.

P. E. Kennedy. Randomization Tests in Econometrics. *Journal of Business & Economic Statistics*, 13:85–94, Jan. 1995.

A. Kleinknecht, F. N. van Schaik, and H. Zhou. Is Flexible Labour Good for Innovation? Evidence From Firm-Level Data. *Cambridge Journal of Economics*, 38(5):1207–1219, Sept. 2014.

J. G. MacKinnon and M. D. Webb. Randomization Inference for Difference-in-Differences with Few Treated Clusters. *Journal of Econometrics*, 218(2):435–450, Oct. 2020.

W. R. Maclaurin. The Sequence from Invention to Innovation and its Relation to Economic Growth. *The Quarterly Journal of Economics*, 67(1):97–111, 1953.

J. M. Malcomson. Contracts, Hold-Up, and Labor Markets. *Journal of Economic Literature*, 35(4):1916–1957, 1997.

OECD. OECD Employment Database. Series: CBC, TUD, LMPEXP, EPL_T, EPL_R, EPL_CD, EPL_OV., 2000-2018. URL `https://www.oecd.org/els/emp/onlineoecdemploymentdatabase.htm`. Accessed on 01.11.2021.

OECD. *Employment Outlook 2004*. 2004.

OECD. *Oslo Manual: Guidelines for Collecting and Interpreting Innovation Data*. The Measurement of Scientific and Technological Activities. OECD, third edition, 2005.

OECD. *Employment Outlook 2014*. 2014.

OECD. *Employment Outlook 2020*. 2020.

J. Rothstein. Teacher Quality in Educational Production: Tracking, Decay, and Student Achievement. *The Quarterly Journal of Economics*, 125(1):175–214, 2010.

G. Saint-Paul. Employment Protection, International Specialization, and Innovation. *European Economic Review*, 46(2):375–395, 2002.

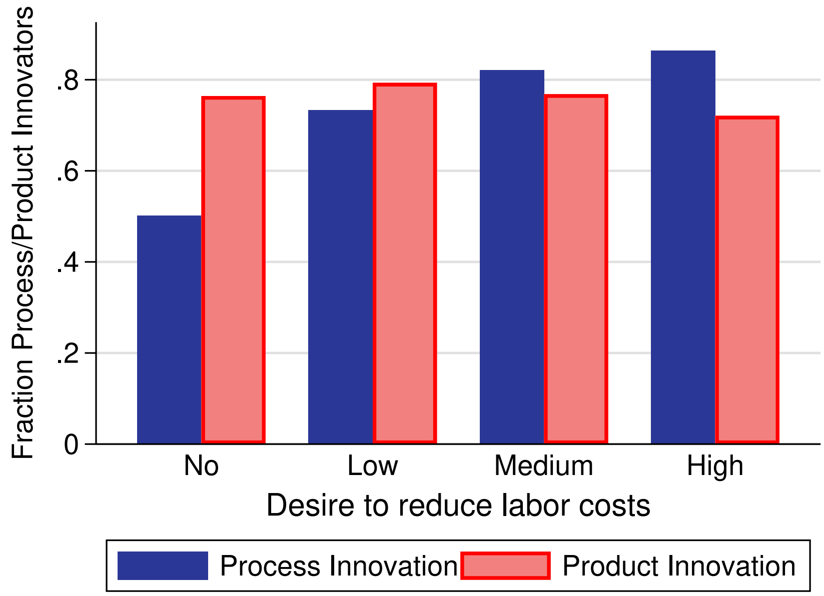
R. M. Samaniego. Employment Protection and High-Tech Aversion. *Review of Economic Dynamics*, 9(2):224–241, 2006.

L. Sun and S. Abraham. Estimating Dynamic Treatment Effects in Event Studies With Heterogeneous Treatment Effects. *Journal of Econometrics,* Forthcoming.

B. Van Ark and K. Jäger. Recent Trends in Europe's Output and Productivity Growth Performance at the Sector Level, 2002-2015. *International Productivity Monitor*, (33): 8–23, 2017.

Figures and Tables

(a)



(b)

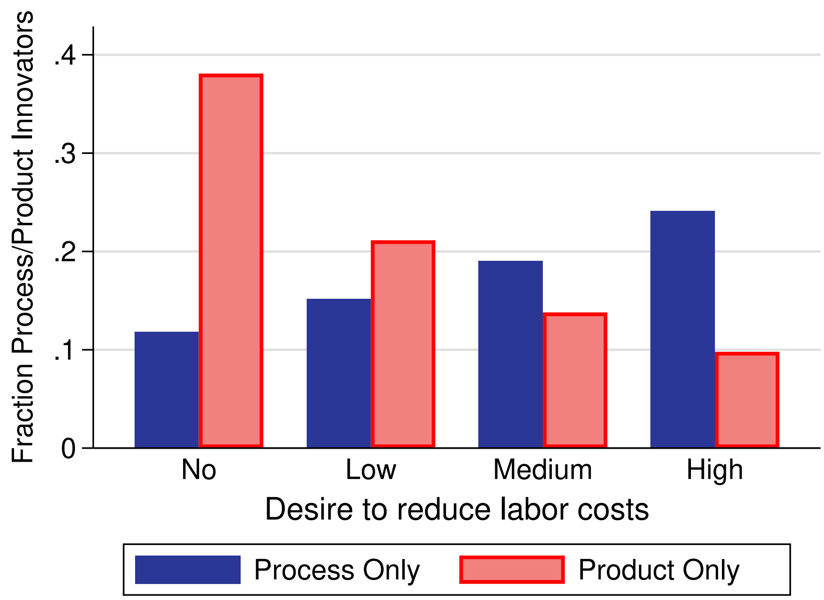
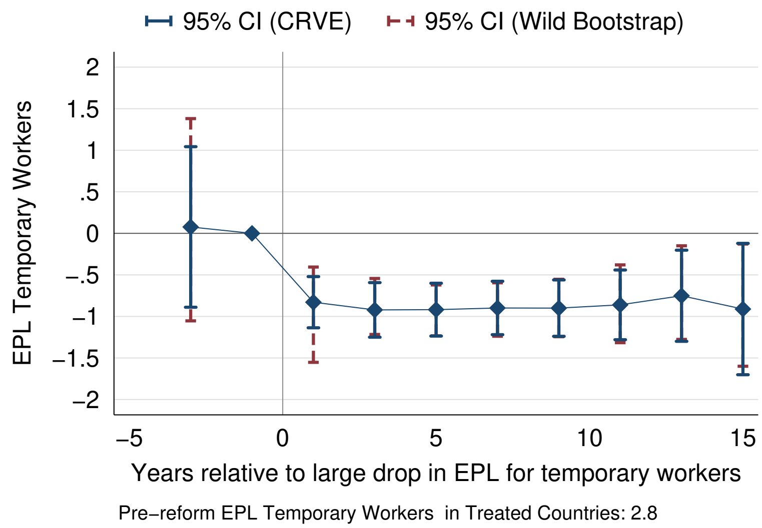


Figure 1: Product and process innovation by desire to cut labor costs

Note: For each possible response to the question of whether the objective/effect of innovation was the reduction in labor costs (how much innovation "reduced labour costs per produced unit"), this figure shows the fraction of respondents who reported doing product/process innovation. Panel (a) does not condition on whether firms carry out the other innovation type, while panel (b) reports statistics for firms that carry out only one type of innovation. More details are in the text. We use CIS firm-level data, which include only a subset of surveyed countries. We restrict the sample to the year 2000 (before EPL changes), include all available industries, and consider only countries that feature also in our baseline regression sample (Belgium, Germany, Greece, Iceland, Latvia, Lithuania, and Spain).



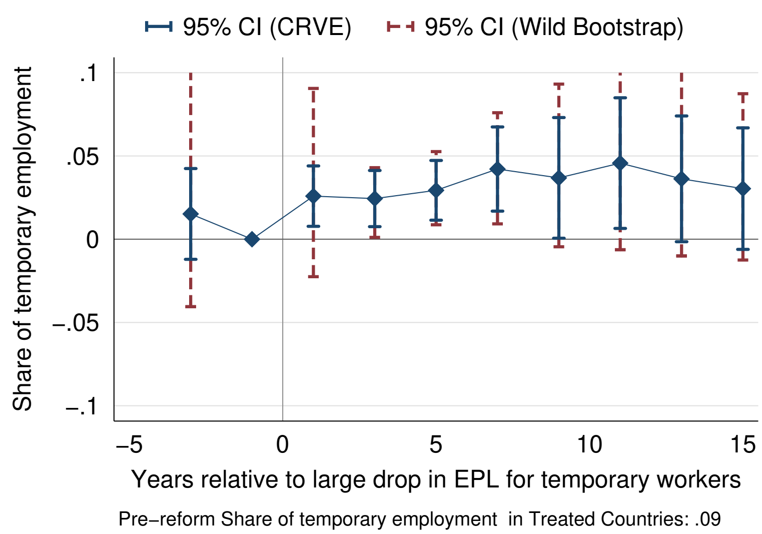


Figure 2: Evolution of EPL for temporary workers and the share of temporary employment around a big EPL drop

Note: Panel (a) shows the path of the index of strictness of Employment Protection Legislation (EPL) for temporary workers around the event (a big drop in EPL for temporary workers). A drop in EPL is considered large if the measure of EPL drops by 20% or more from one year to the next. The figure reports the coefficients κ_e from regression (2) with "EPL for temporary workers" as outcome variable. All countries in the sample are included in the regression. Panel (b) reports the same coefficients for the share of temporary workers over total dependent employment in the manufacturing sector, as computed by Eurostat.

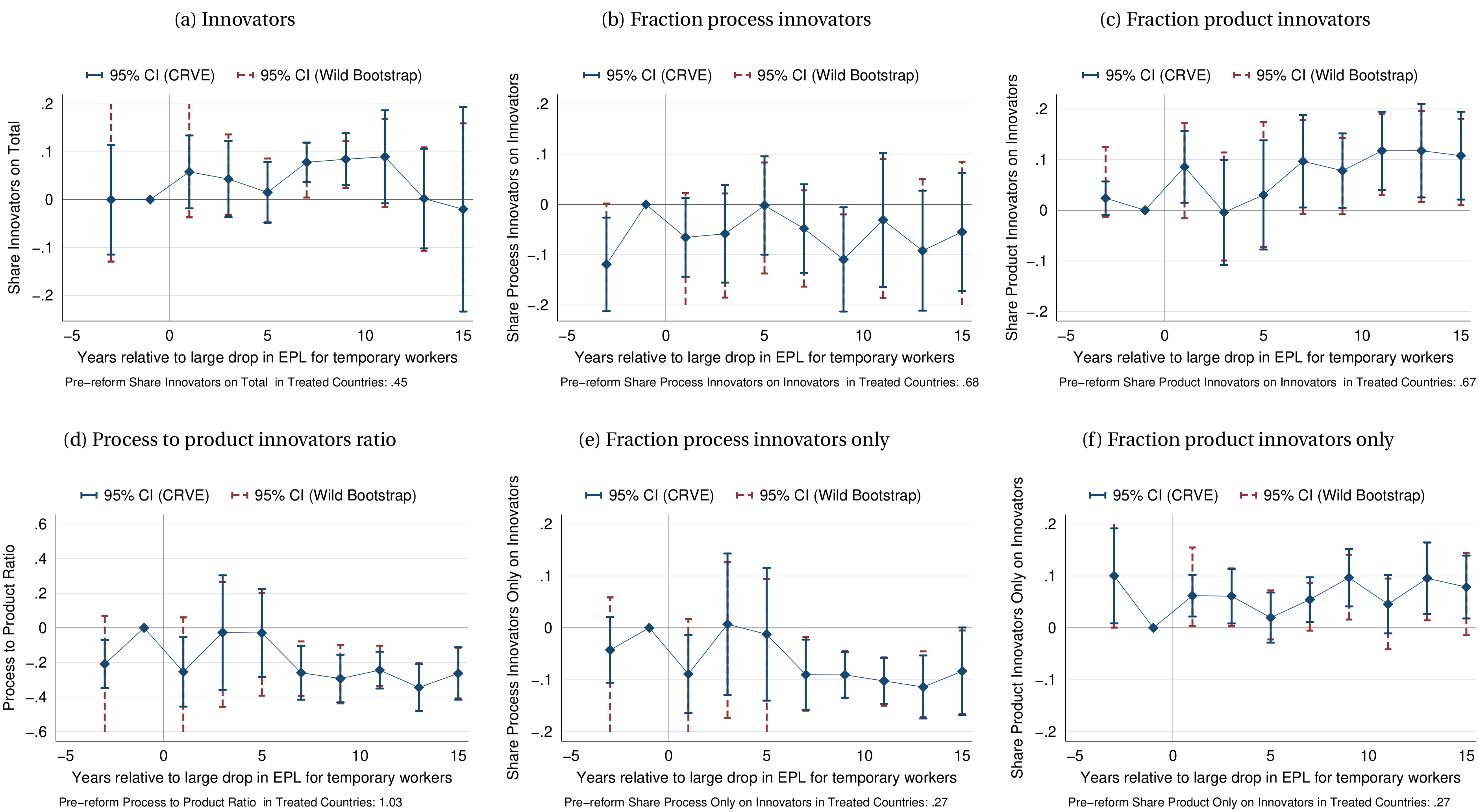


Figure 3: Main Results: effect of large EPL drop on innovators and product/process innovation

Note: This exhibit plots the coefficients κ_e resulting from the estimation of Equation (2) on the sample described in Section B.1. The coefficient κ_{-1} is normalized to zero, and $e = 0$ denotes the event year. The outcomes are: share of innovating firms out of all respondents (panel a), share of process/product innovators over innovators (panels b/c), total number of process innovators over total number of product innovators (panel d), share of innovating firms that carry out exclusively process/product innovation (panel e/f). The solid bars are 95% confidence intervals constructed using a cluster-robust variance estimator, clustering at the country level. Dashed bars are 95% wild cluster bootstrap confidence intervals (Rademacher weights, 999 repetitions). The absence of a horizontal dash at the endpoints indicates that bars have been truncated to fit the graph area.

Table 1: Summary Statistics on main variables - Manufacturing

Country	(1) Innovators	(2) Fraction Process	(3) Fraction Product	(4) Process over Product	(5) EPL Temp	(6) Year EPL Drop
Austria	0.52	0.72	0.72	1.00	1.31	.
	(0.04)	(0.10)	(0.03)	(0.14)	(0)	.
Belgium	0.59	0.71	0.67	1.06	2.21	.
	(0.05)	(0.04)	(0.06)	(0.11)	(0.08)	.
Denmark	0.49	0.63	0.67	0.97	1.43	.
	(0.08)	(0.03)	(0.10)	(0.15)	(0.11)	.
Finland	0.54	0.67	0.70	0.96	1.56	.
	(0.05)	(0.06)	(0.04)	(0.12)	(0)	.
France	0.43	0.68	0.67	1.02	3.11	.
	(0.06)	(0.07)	(0.03)	(0.15)	(0.04)	.
Germany	0.67	0.52	0.69	0.76	1.14	2001
	(0.06)	(0.05)	(0.03)	(0.08)	(0.32)	.
Greece	0.37	0.78	0.65	1.20	2.91	2003
	(0.07)	(0.09)	(0.06)	(0.14)	(1.02)	.
Iceland	0.49	0.69	0.74	0.94	0.63	.
	(0.02)	(0.03)	(0.06)	(0.05)	(0)	.
Italy	0.43	0.75	0.65	1.18	2.14	2001
	(0.05)	(0.04)	(0.08)	(0.22)	(0.50)	.
Latvia	0.21	0.68	0.67	1.02	0.88	.
	(0.04)	(0.01)	(0.00)	(0.01)	(0)	.
Lithuania	0.41	0.83	0.69	1.21	2.38	.
	(0.02)	(0.03)	(0.04)	(0.11)	(0)	.
Luxembourg	0.51	0.73	0.70	1.05	3.75	.
	(0.05)	(0.06)	(0.08)	(0.16)	(0)	.
Netherlands	0.49	0.64	0.73	0.88	0.96	.
	(0.07)	(0.04)	(0.04)	(0.07)	(0.08)	.
Norway	0.44	0.55	0.69	0.79	2.91	.
	(0.08)	(0.11)	(0.07)	(0.09)	(0.18)	.
Portugal	0.45	0.81	0.63	1.28	2.23	2007
	(0.06)	(0.06)	(0.03)	(0.08)	(0.40)	.
Spain	0.34	0.71	0.54	1.31	2.98	.
	(0.04)	(0.05)	(0.06)	(0.16)	(0.32)	.
Sweden	0.50	0.61	0.67	0.90	1.06	2007
	(0.02)	(0.06)	(0.03)	(0.08)	(0.33)	.
Turkey	0.40	0.75	0.67	1.13	4.88	.
	(0.09)	(0.03)	(0.04)	(0.08)	(0)	.

Note: This table reports means and standard deviations (in parentheses) of selected variables in our sample. Variables in columns 1 through 4 come from the CIS (restricting to manufacturing firms). Column 1 reports the share of respondents that report carrying out at least one type of innovation (product and/or process); column 2/3 report the shares of innovating firms that engage in process/product innovation and column 4 reports their ratio. Columns 5 and 6 report the OECD index of EPL strictness for temporary workers, and the year (if any) in which EPL drops by 20% or more.

Table 2: Difference-in-differences results

	(1) Share Innovators on Total	(2) Share Product Innovators on Innovators	(3) Share Process Innovators on Innovators	(4) Share Process Innovators Only on Innovators	(5) Process to Product Ratio	(6) Process to Product Ratio, Controls
Short Run	0.033	0.019	-0.007	-0.005	-0.013	-0.270
	(0.027)	(0.046)	(0.041)	(0.051)	(0.111)	(0.125)
	[−0.027, 0.124]	[−0.095, 0.153]	[−0.141, 0.069]	[−0.194, 0.114]	[−0.449, 0.232]	[−0.812, 0.033]
Long Run	0.061	0.097	-0.051	-0.090	-0.251	-0.248
	(0.033)	(0.039)	(0.045)	(0.022)	(0.059)	(0.055)
	[−0.023, 0.122]	[0.004, 0.172]	[−0.164, 0.047]	[−0.144, −0.044]	[−0.353, −0.074]	[−0.482, −0.090]
Constant	0.546	0.805	0.669	0.163	0.778	0.802
	(0.013)	(0.023)	(0.035)	(0.014)	(0.037)	(0.112)
N	119	119	119	119	119	79
Number of Clusters	18	18	18	18	18	10
Number of Firms	2298051	2298051	2298051	2298051	2298051	1922666

Note: This table reports difference-in-differences coefficients from the estimation of Equation (3) on the main outcomes of interest. “Short-Run” is a dummy equals to 1 if the treatment occurred between 1 and 6 years prior. “Long-Run” is a dummy for the treatment occurring 7 or more years prior. All specifications include country and time fixed-effects. See Section 3.1 for further details.

A Graphical Representation of Saint-Paul (2002)

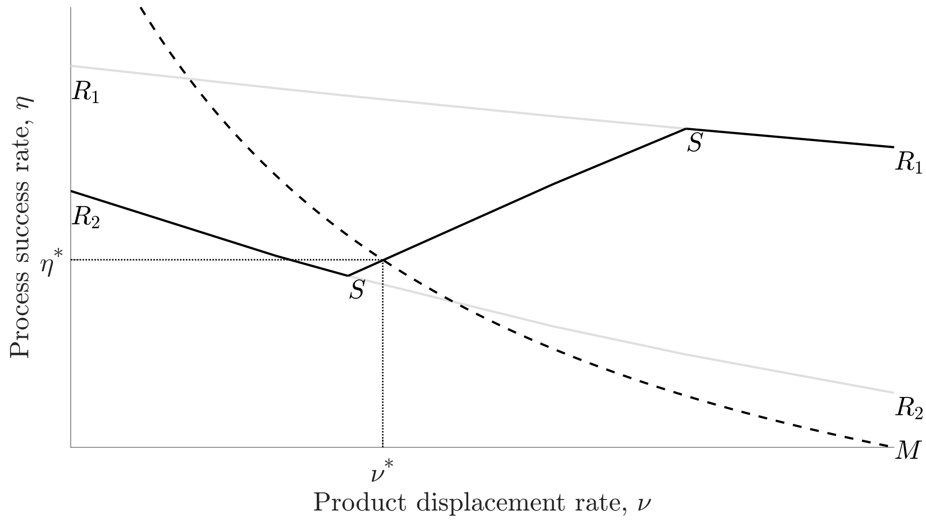


Figure A.1: Equilibrium with full specialization, country 1 has lower firing costs.

Note: this figure depicts the determination of equilibrium product displacement and process innovation success rates. The equilibrium pair, (ν^*, η^*), is determined by combining the hazard frontier condition in locus M with the bold locus R_2SSR_1, which represents the set of possible equilibria for different specifications of the technology production function. The curves R_1R_1 and R_2R_2 represent the indifference conditions between process and product innovation in the two countries, while the locus SS gives the combinations of (ν, η) satisfying the steady state relation when the two countries are fully specialized.

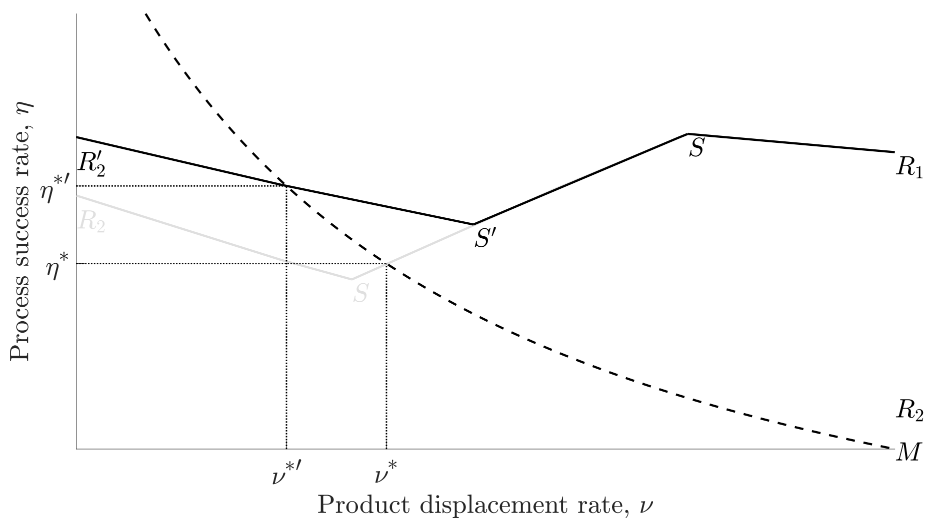


Figure A.2: Impact of a reduction in country 1's firing costs.

Note: this figure depicts the effect of a reduction in country 2's firing costs, F_2, on the equilibrium research allocation. The segment corresponding to imperfect specialization in country 2 shifts upwards from R_2S to $R_2'S'$. This result in a transition from (ν^*, η^*) to the new equilibrium pair, $(\nu^{*\prime}, \eta^{*\prime})$, characterized by higher product innovation and lower process innovation in country 2 and in the world as a whole.

B Data Appendix

B.1 Data Sources

Our data sources are;

- **Innovation data** from the Community Innovation Survey[23]
  - Aggregate data from Eurostat website: `https://ec.europa.eu/eurostat/web/main/data/database`, Database by themes > Science, Technology and Digital Society > Science and technology (scitech) > Community Innovation Survey (inn). Relevant series: CIS3 through CIS2016;
  - Micro-data (Scientific Use Files) for a subset of countries from Eurostat under RPP 94/2020-CIS-SES-MMD;[24]
  - Questionnaires from Eurostat website: link to all; paths are `https://ec.europa.eu/eurostat/documents/203647/203701/CIS_Survey_form_XXX.pdf` where "XXX" are 3, 4, 2006-2016.
- Index of **strictness of Employment Protection Legislation (EPL)** from the OECD. Main source: `https://stats.oecd.org/Index.aspx?DataSetCode=XXX`, where "XXX" is to be replaced by the dataset code; Main references: `https://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection.htm`, OECD (2020) and OECD (2014). Series used:
  - EPL Temp: "Strictness of hiring regulation for workers on temporary contracts" (EPT version 1) [Dataset code: EPL_T];
  - EPL Regular: "Strictness of regulation of individual dismissals of workers on regular contracts" (EPR version 1) [Dataset code: EPL_R];
  - EPL Collective: "Strictness of regulation of collective dismissals of workers on regular contracts" (EPC version 2) [Dataset code: EPL_CD];
  - EPL Total: "Strictness of dismissal regulation for workers on regular contracts (both individual and collective dismissals)" (EPRC version 2). Weighted average of EPL Regular (5/7 of the weight) and of EPL Collective (2/7 of the weight) [Dataset code: EPL_OV].
- **Permanent and temporary employment** (aged 15-64, both sexes) from the Labour Force Survey (Eurostat). Main source: `https://ec.europa.eu/eurostat/web/`

[23] CIS results were collected under European Commission Regulation (EC) No 1450/2004 until 2010 and under regulation EC No 995/2012 starting in 2012.

[24] We specify that the results and conclusions are those of the authors and not those of Eurostat, the European Commission or any of the national statistical authorities whose data have been used.

main/data/database, Database by themes > Population and social conditions > Labour market (labour) > Employment and unemployment (Labour force survey) (employ) > LFS series - detailed quarterly survey results (from 1998 onwards) (lfsq) > Employment - LFS series (lfsq_emp). Series used:

  - Employment by economic activity: lfsq_egana (1998-2008), lfsq_egan2 (from 2008 onwards);
  - Employment by detailed economic activity: lfsq_egana2d (1998-2008);
  - Temporary employees by economic activity: lfsq_etgana (1998-2008), lfsq_etgan2 (from 2008 onwards);
  - Share of temporary workers: ratio of temporary employment over total employment by economic activity.

- **Labor market institutions and policies** from the OECD. Main source: https://stats.oecd.org/Index.aspx?DataSetCode=XXX, where "XXX" is to be replaced by the dataset code. Series used:

  - Collective bargaining coverage [Dataset code: CBC];
  - Trade union density [Dataset code: TUD];
  - Spending on unemployment benefits (% of GDP) [Dataset code: LMPEXP, #80];
  - Spending on active labor market policies (% of GDP) [Dataset code: LMPEXP, #112];
  - Total spending on labor market policies (sum of unemployment benefits and active labor market policies).

- **Labor share and Capital-labor ratio** for the manufacturing sector from EU KLEMS. Main source: https://euklems.eu/. Variables used:

  - Labor share: LAB / (LAB+CAP)
  - Capital-labor ratio: CAP_QI / LAB_QI

- **Automation and offshoring** for detailed manufacturing sectors from the replication package of (Acemoglu and Restrepo, 2020b). Variables used:

  - Automation potential: average robot penetration for detailed manufacturing sectors in the US for 2004 (apr_us_lv_04)
  - Task offshoring: task offshoring at the broad industry level from Autor and Dorn (2013) (task_offshore_manuf)

B.2 Additional Descriptive Statistics

Table B.1 : Number of respondents in CIS

Country	Industry			Services		
	Innovative Firms (1)	Total Respondents (2)	Percentage Innovative (3)	Innovative Firms (4)	Total Respondents (5)	Percentage Innovative (6)
Austria	3,727	7,251	51	3,673	8,754	42
Belgium	3,585	6,126	59	3,646	7,827	46
Denmark	1,931	3,948	47	1,916	4,953	39
Finland	2,133	4,025	53	1,826	4,211	43
France	14,140	33,608	43	11,709	35,685	33
Germany	40,140	61,308	66	32,801	62,752	53
Greece	2,334	6,332	38	2,192	5,684	38
Iceland	213	422	50	223	400	56
Italy	34,635	82,119	42	12,250	38,897	31
Latvia	476	2,266	21	409	2,612	16
Lithuania	920	2,998	31	901	3,474	25
Luxembourg	169	342	49	570	1,223	47
Netherlands	4,803	9,824	49	6,413	16,647	39
Norway	1,677	3,930	43	1,917	5,013	38
Portugal	5,766	12,875	45	3,598	7,405	49
Spain	13,465	40,279	33	8,430	34,439	25
Sweden	3,603	7,278	49	4,271	9,874	44
Turkey	16,476	41,726	39	10,382	32,630	31

Note: This table reports the average number of respondents to the Community Innovation Survey per wave by country and macro-sector. The macro sectors are Industry (sectors C-E in NACE Rev.1) and Services (Innovation core services activities; G51, I, J, K72, K74.2 and K74.3 in NACE Rev.1 for years 2004-2016, sectors G-K in year 2000). Columns 2 and 5 report the average of all respondents, while columns 1 and 4 of the innovative firms, where a firm is innovative if it carries out at least one type of process or product innovation in the three years preceding the survey. Columns 3 and 6 report the average number of innovative firms out of all respondents.

Table B.2 : Summary Statistics on main variables - all sectors

Country	Innovators	Fraction Process Innovators	Fraction Product Innovators	Process to Product Innovators Ratio
	(1)	(2)	(3)	(4)
Austria	0.46	0.73	0.67	1.11
	(0.13)	(0.11)	(0.09)	(0.30)
Belgium	0.48	0.68	0.64	1.09
	(0.14)	(0.09)	(0.11)	(0.33)
Denmark	0.41	0.64	0.62	1.12
	(0.11)	(0.07)	(0.14)	(0.49)
Finland	0.47	0.67	0.66	1.09
	(0.13)	(0.09)	(0.12)	(0.40)
France	0.30	0.73	0.58	1.36
	(0.11)	(0.09)	(0.11)	(0.53)
Germany	0.59	0.54	0.64	0.88
	(0.14)	(0.07)	(0.10)	(0.25)
Greece	0.37	0.79	0.64	1.26
	(0.08)	(0.09)	(0.08)	(0.28)
Iceland	0.48	0.66	0.71	0.96
	(0.10)	(0.08)	(0.11)	(0.22)
Italy	0.33	0.75	0.60	1.35
	(0.11)	(0.08)	(0.12)	(0.58)
Latvia	0.17	0.71	0.52	1.72
	(0.07)	(0.10)	(0.19)	(1.20)
Lithuania	0.34	0.86	0.55	1.71
	(0.09)	(0.05)	(0.15)	(0.59)
Luxembourg	0.45	0.69	0.67	1.08
	(0.11)	(0.10)	(0.13)	(0.32)
Netherlands	0.35	0.64	0.67	0.98
	(0.12)	(0.08)	(0.10)	(0.26)
Norway	0.34	0.55	0.61	0.99
	(0.15)	(0.12)	(0.16)	(0.43)
Portugal	0.45	0.80	0.61	1.35
	(0.07)	(0.07)	(0.08)	(0.76)
Spain	0.23	0.71	0.43	1.88
	(0.10)	(0.09)	(0.14)	(0.82)
Sweden	0.49	0.59	0.69	0.87
	(0.08)	(0.10)	(0.07)	(0.19)
Turkey	0.36	0.73	0.65	1.14
	(0.09)	(0.05)	(0.06)	(0.12)

Note: This table replicates columns 1 through 4 of Table 1 but including all sectors available (that is, without restricting to the manufacturing sector). See note to Table 1 for details.

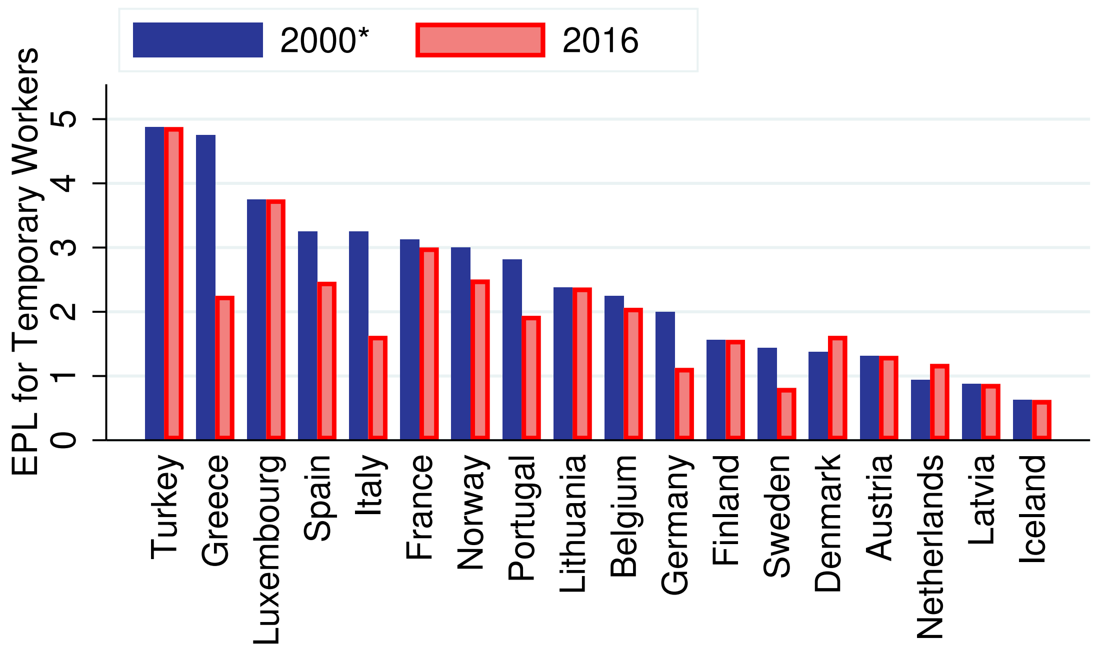
B.3 Details on EPL Reforms

In this subsection, we describe how the index of strictness of Employment Protection Legislation for temporary workers varies across countries and over time, and we provide more details on the identification of the treatment and the selection of controls.

Figure B.1 shows the level of EPL for temporary workers for each country in our sample at the beginning and at the end of the sample period. About a third of the countries in our sample experienced no change at all, while the remaining two thirds introduced at least some change to this measure over the period of interest, with a few presenting substantial variation—induced by large reforms that lifted regulatory burdens on temporary contracts, as described in Table B.3 .

This pattern is apparent in Figure B.2, which depicts the frequency distribution of year-to-year percentage changes in EPL for temporary workers. The figure groups changes into three categories: large drops, small changes, and large increases; we define a change as large if it is smaller than −20% or greater than 20%. These cutoffs correspond to the 2.5 and 97.5 percentiles of EPL changes, respectively. Small or zero changes represent the vast majority of the observations (95%). We use large drops as treatment, while countries with only small or no changes in EPL form our control group. We drop countries with large positive changes as they do not constitute a valid control group. In particular, many of these are eastern European countries, where EPL changes occur around the date of EU accession.[25] Finally, Figure B.3 reports the path of the EPL measure around the time of the event for treated countries. EPL is mostly stable before and after reform episodes, and only Sweden and Portugal experience (minor) increases in EPL in the ten years following the event.

[25] When these large increases are studied as events, there are clear pre-trends in all our outcomes of interest.



*Earliest year available. This is 2000 for the majority of the sample, 2006 for Turkey, 2008 for Luxembourg, 2010 for Iceland, 2012 for Latvia, 2014 for Lithuania.

Figure B.1: EPL for temporary workers

Note: This figure shows the measure of strictness of Employment Protection Legislation (EPL) for temporary workers at the beginning and at the end of the sample period (respectively 2000 and 2016) for the sample of countries in the analysis. For the countries without data in 2000 we use the earliest data point available, as indicated in the figure. The source is the OECD Indicators of Employment Protection database.

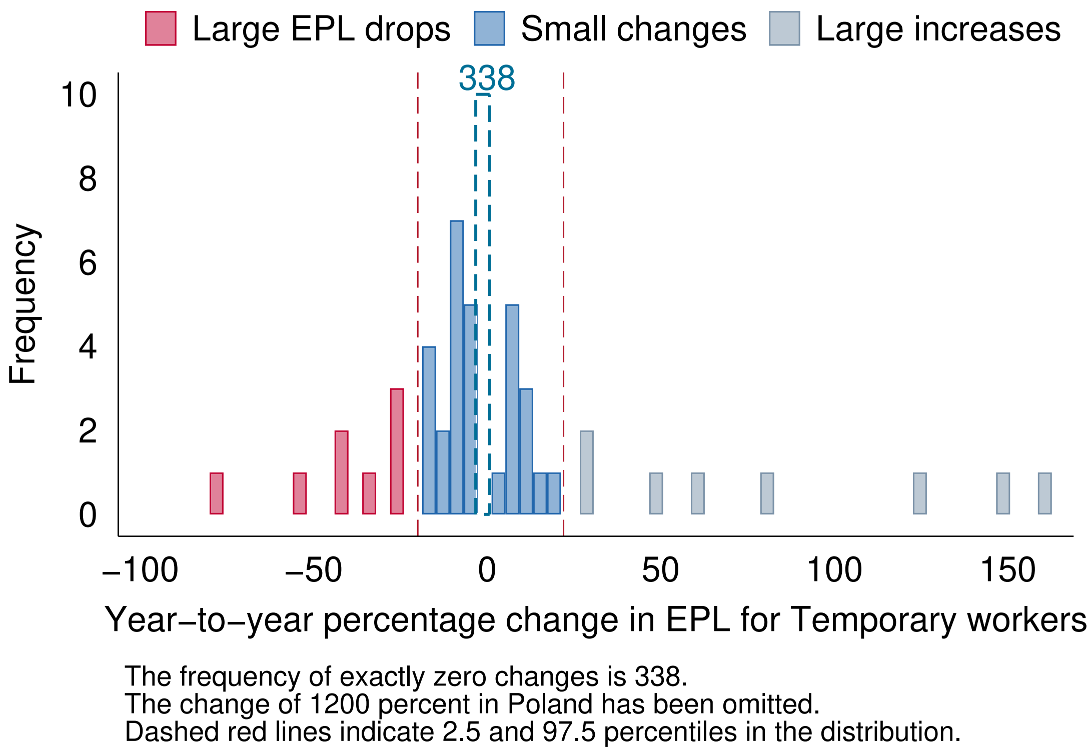


Figure B.2: Frequency distribution of percent EPL changes

Note: The figure shows the frequency of percent yearly changes in the measure of Employment Protection Legislation (EPL) for temporary workers. The frequency of exactly zero changes is represented by the dashed bar. We omitted the 12-fold increase in EPL for Poland in 2003-2004. The graph uses EPL data from 2000 to 2016 for all European countries for which they are available (not just those in the sample). The vertical dashed lines separates small changes (center) from "large EPL drops" (yearly drops in the index larger than 20% of their previous value, on the left) and "large increases" (yearly increases in the index of more than 20%, on the right). Authors' calculations from the OECD Indicators of Employment Protection database.

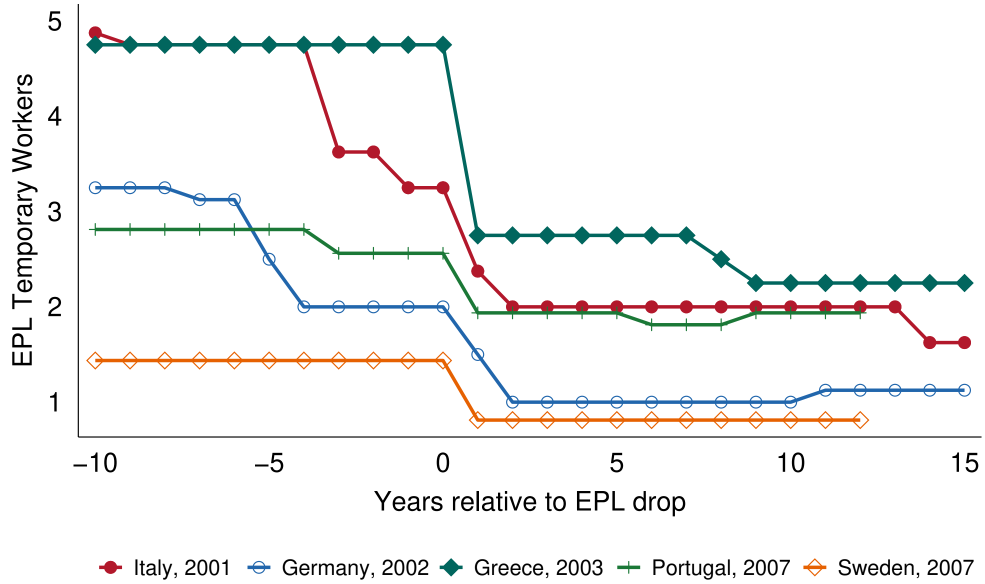


Figure B.3: EPL for temporary workers around Big EPL drop

Note: This figure shows the evolution of the index of strictness of Employment Protection Legislation (EPL) for temporary workers around the event (a large drop in this index). A drop in EPL is considered large if the measure of EPL drops by 20% or more from one year to the next. The figure shows the raw data for the five countries in our sample which experienced a large EPL drop within the sample period (2000-2016). The x-axis is expressed in relative years from the event; the date of the event for each country is indicated in the label.

Table B.3 : Reform episodes corresponding to "large EPL drops"

Country (Year)	Reform	Change in EPL index	Percent change
Italy (2001)	Expanded valid cases for the use of fixed-term contracts (law no. 368/2001).	-0.88	-26.8%
Germany (2002)	Maximum total duration of temporary work agreements was increased to 24 months, any limit to total duration lifted in 2004.	-1	-50%
Greece (2003)	Fixed-term contracts maximum renewals increased.	-2	-42.1%
Portugal (2007)	Maximum permitted assignment to temporary work agencies increased from one to two years.	-0.62	-24.2%
Sweden (2007)	Extension of maximum duration of temporary contracts increased from one to two years.	-0.63	-43.8%

Note: this table reports the reform episodes associated to "large EPL drops" as defined in the text. The sources are OECD (2004) for Italy, Germany and Greece, and Duval et al. (2019) for Portugal and Sweden.

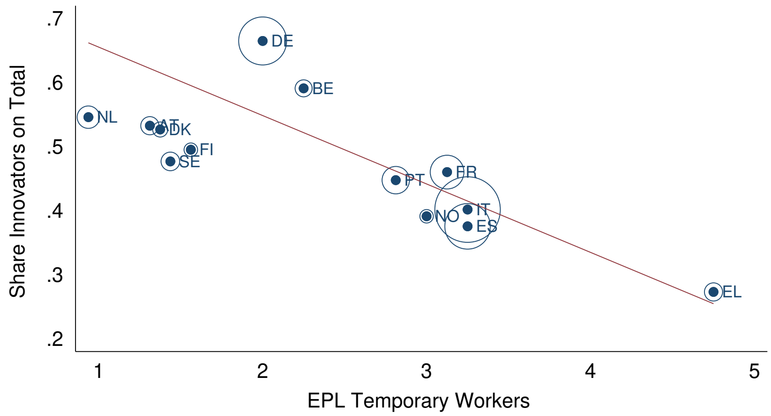
B.4 Descriptive Results

EPL and innovation in the cross-section. Figure B.4 shows how our measure of employment protection correlates with key outcome variables in the first year of our panel (2000). Panel (a) shows a strong negative correlation between employment protection for temporary workers and the share of innovative firms driven by two distinguishable clusters: the northern European countries, such as Netherlands and Germany, with a relatively low level of employment protection and a relatively high share of firms that report carrying out innovation activities; and the southern European countries, with relatively fewer innovators and a higher value for the EPL index.[26] Among firms that engage in any innovation, panels (b) and (c) show that higher employment protection for temporary workers is associated with a relatively low share of firms conducting product innovation and a relatively high share of process innovators, while the converse holds for low-EPL countries. The same patterns are present in all waves of the survey (available upon request), and in the pooled sample, as can be noted from a comparison of columns 1 through 4 with column 5 in Table 1.

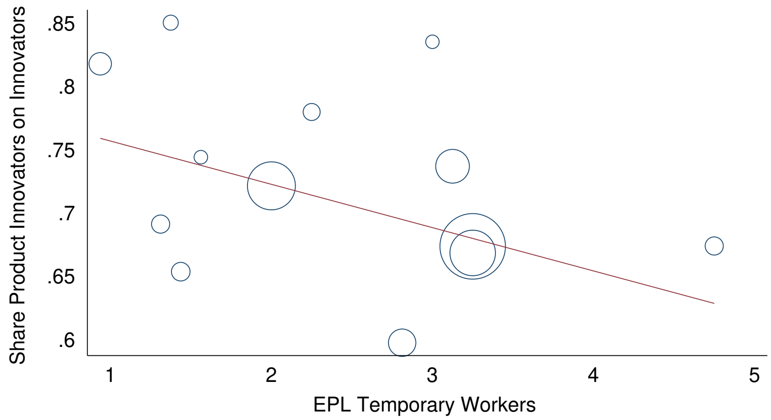
The risk channel. The model provides differential riskiness of process and product innovation as the channel through which EPL affects these two variables. In Table B.4 , we present correlations between risk of downsizing and innovation variables from firm-level data. We run simple regressions of an indicator for whether the firm reduced its employment level on the indicators for process and product innovation. The resulting coefficients highlight that both types of innovation result in a higher probability of having to reduce employment. However, product innovation presents a stronger correlation with downsizing, as apparent when both process and product innovators are included as explanatory variables, or when the sample is restricted to firms that carry out only one type of innovation.

[26] An exception to this geographical pattern is Norway, which appears among southern European countries both in terms of EPL and of share of innovators.

(a) Innovators



(b) Product on innovators



(c) Process on innovators

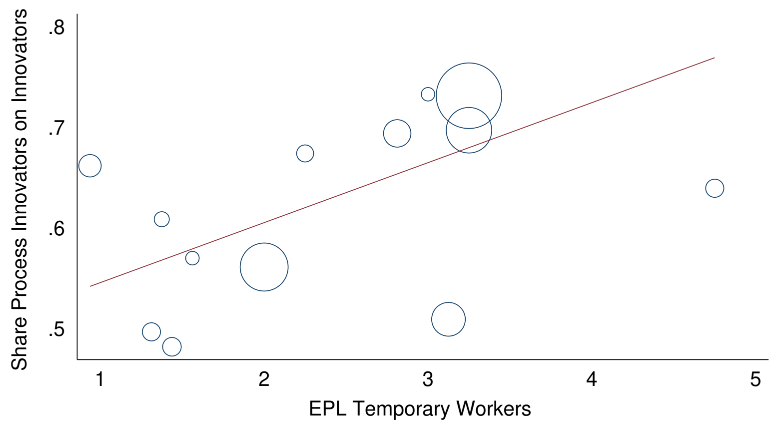


Figure B.4: Correlations between EPL for temporary workers and main CIS variables in 2000

Note: The figure shows the cross-sectional correlations between EPL Temp and the three key outcome variables from the CIS: share of firms that carry out innovation out of all respondents in panel (a), share of innovative firms that engage in product innovation in panel (b), and share of innovative firms that engage in process innovation in panel (c). Data are for the manufacturing sector in the first year of our sample (2000). Observations are weighted by the number of respondent firms in each country.

Table B.4 : Correlations between probability of downsizing and product and process innovation variables

	(1)	(2)	(3)	(4)	(5)
Product Innovation	0.047		0.037		
	(0.013)		(0.015)		
Process Innovation		0.042	0.026		
		(0.014)	(0.016)		
Product Innovation Only				0.040	
				(0.017)	
Process Innovation Only					0.029
					(0.023)
Observations	10286	10282	10281	6861	6612

Standard errors in parentheses.

Note: This table reports the correlation between innovation variables and the probability of a reduction in the size of the firm over the years 1998-2000, corresponding to the first CIS wave. We use firm-level data from CIS Scientific Use Files, and select the subset of countries which also appear in our main regression analysis. The main dependent variable is an indicator for whether the firm has reduced the number of employees between the year 1998 and 2000. The available data only report a recoded variable for the size of the firm. Specifically, size is recoded into four bins: less than 10 employees; between 10 and 49 employees; between 50 and 249 employees; more than 250 employees. We therefore attribute a value of 1 to the indicator variable if the firm has changed its size bin downwards between 1998 and 2000. Columns 1 to 3 in the table report regressions of the downsize indicator on the dummies for product or process innovation as defined in the main text. Columns 4 and 5 repeat the same exercise as columns 1 and 2, dropping the observations for firms which conduct both types.

C Additional Results and Robustness

C.1 Treatment Effect Heterogeneity

In what follows, we focus on the ratio of process innovators to product innovators, a measure that neatly summarizes the reallocation across different innovation activities, and that fully characterizes the equilibrium of the model.

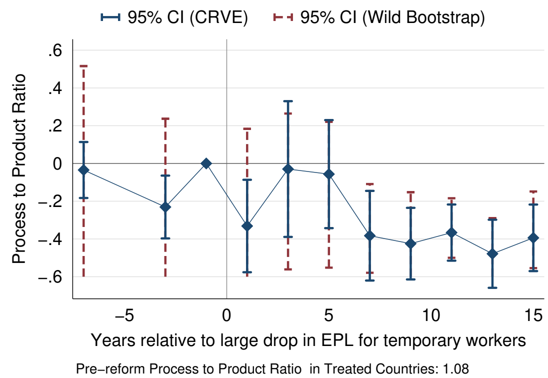
Figure C.1 shows that that the effect of weakening EPL on the process-product innovators ratio is strongest for small firms (10-49 employees), and decreases with size. In particular, our point estimates reveal a sizable and persistent reduction of more than 35% of the pre-period average for small firms. By contrast, medium-sized firms (50-250 employees) only see a 20% decline in the process-product innovators ratio, which is not significant in the long-run. Large firms (250+ employees) see no significant changes in this measure. These findings are consistent with larger firms being less affected by the rigidity of individual employment relation, as they have access to collective dismissals that significantly reduce the costs and burdens on firing firms.[27] Another possible explanation for this heterogeneity stems from the fact that we can only measure the extensive margin of innovation. Larger firms are more likely to have the resources to pursue different types of innovation, so the effects of a reform would predominantly occur along the intensive margin.

Due to the aggregate nature of our data, we are unable to observe changes in individual firm sizes. It is therefore possible that our baseline findings are driven by a general increase in firm size, which is negatively correlated with the process/product innovation ratio. However, Figure C.1 shows that both medium and small firms reduce their efforts in process innovation relative to product innovation, suggesting that this size channel is unlikely to be the sole driver of our results.

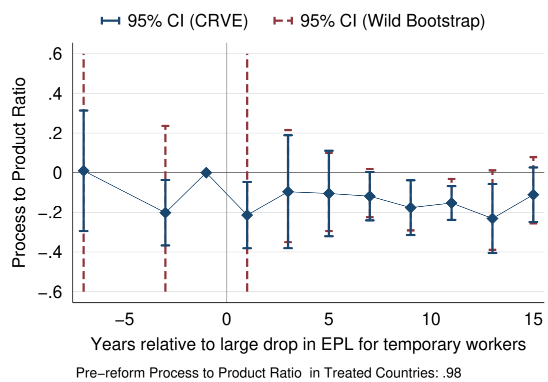
The other two heterogeneity results group treated countries by initial starting EPL (Figure C.2) and size of the drop in EPL (Figure C.3). In the first exercise, we split countries by their initial EPL level, so that we compare high(low)-EPL treated countries to high(low)-EPL control countries. In the second exercise, we split treated countries according to whether the size of the drop is high or low relative to the rest of the treated group, and compare them to all control countries. We then run separate regressions for each of these four groups. Partitioning restricts the size of the treatment group, so the reported coefficients should be interpreted with caution, and are estimated more imprecisely. With this caveat in mind, Figure C.2 suggests that labor market reforms have a sizable and significant effect on innovative activities only when starting EPL is high. Figure C.3 highlights that, in our treatment group, only the relatively large EPL reductions—more drastic reforms—induce a reallocation of innovation.

[27] For a comprehensive review on collective dismissals, see Aleksynska and Muller (2020).

(a) Less than 50 employees



(b) Between 50 and 250 employees



(c) More than 250 employees

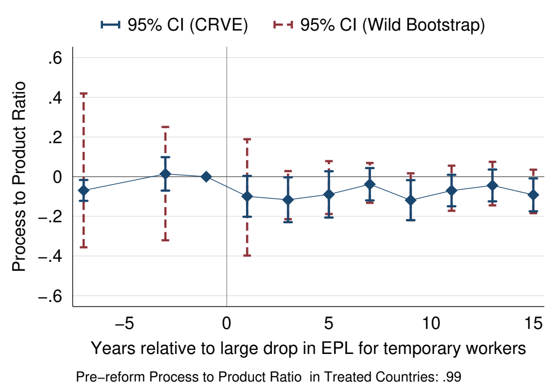


Figure C.1: Treatment heterogeneity by firm size: process to product innovators ratio.

Note: The panels in the figure plot the coefficients κ_e from regression (2). The outcome is the ratio of process innovators to product innovators. The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details. Panel (a) restricts the sample to firms with 10-49 employees at the time of the survey, panel (b) to firms with 50 to 249 employees, panel (c) to firms with more than 250 employees.

(a) High starting EPL

(b) Low starting EPL

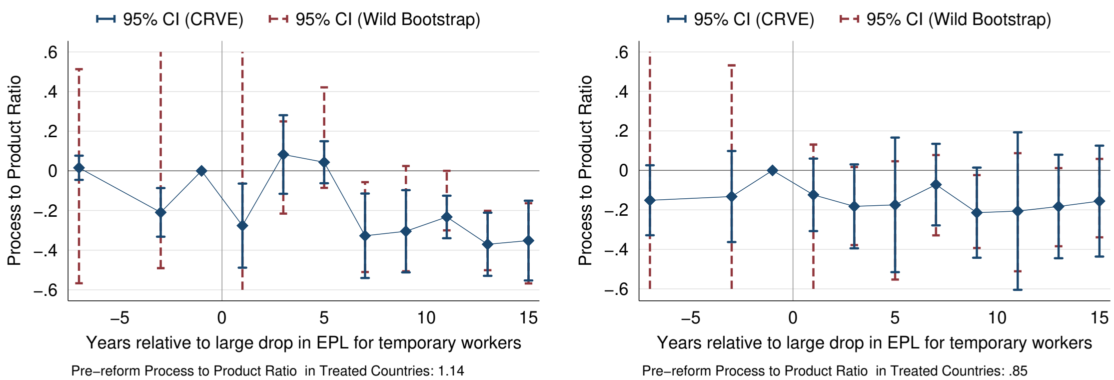


Figure C.2: Treatment heterogeneity by starting EPL level

Note: The panels in the figure plot the coefficients κ_e from regression (2). The outcome is the ratio of process innovators to product innovators. The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details. Both panels only use countries for which data on EPL for temporary workers is available for the year 2000 (all except for Turkey, Luxembourg, Iceland, Latvia, Lithuania). Panel (a) restricts the sample to countries with EPL for temporary workers in 2000 above median, while panel (b) below median. Thus the countries in panel (a) are Greece (EPL Temp in 2000: 4.75), Italy (3.25), Portugal (2.81) as treated and Spain (3.25), France (3.13), Norway (3), Belgium (2.25) as controls. Countries in panel (b) are Germany (2), Sweden (1.44) as treated, and Finland (1.56), Denmark (1.38), Austria (1.31), Netherlands (0.94) as controls.

(a) Large EPL drop

(b) Small EPL drop

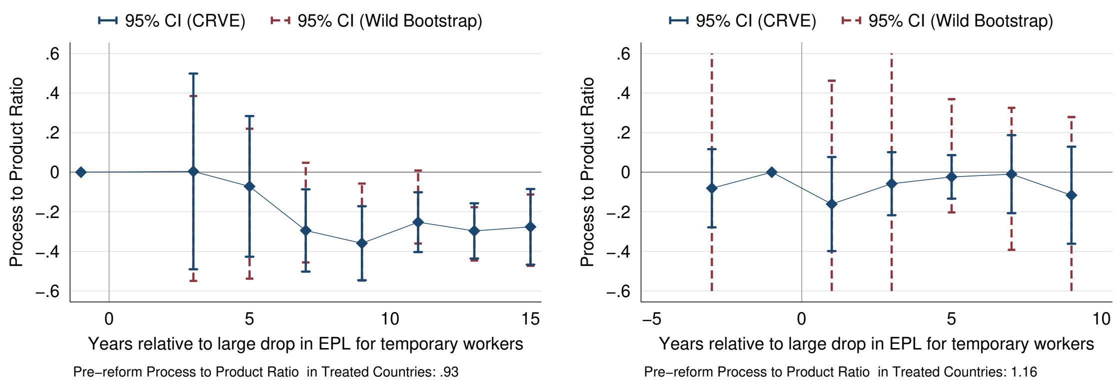


Figure C.3: Treatment heterogeneity by size of EPL drop

Note: The panels in the figure plot the coefficients κ_e from regression (2). The outcome is the ratio of process innovators to product innovators. The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details. Both panels drop countries for which we have some missing observations (Greece, Denmark, Iceland, Latvia, Turkey), but otherwise keep all the control countries. Panel (a) restricts the sample to treated countries where the big drop in EPL is relatively large (Germany, -1.25, and Italy, -1, between 2000 and 2004) while panel (b) to treated countries where the drop in EPL is relatively small (Portugal and Sweden, both -0.63 between 2006 and 2008).

C.2 Different Samples

We modify the baseline sample in two types of robustness exercises. First, we expand the sample of firms to include all sectors (in addition to manufacturing). As shown in Figure C.4, all our results carry over, and are of the same absolute and percentage magnitudes as in our baseline.

Second, we limit the sample to two panels of eleven countries each, balanced around the time of the event. Indeed, Table C.1 shows that we do not observe the same periods around the treatment date for the entire group of treated countries. Motivated by these patterns, we analyze separately the Germany-Italy and the Portugal-Sweden episodes in Figures C.5 and C.6. While the results are qualitatively unaffected by this partition, we can see that the event-study coefficients for the Portugal-Sweden episode are small and non-significant, while those for Italy and Germany are large and statistically significant for the same outcomes as the general results. To understand this result, note that this partition of the sample actually corresponds to the heterogeneity presented in Figure C.3: Germany and Italy were subject to a sizable reduction in EPL, while Portugal and Sweden passed less radical reforms, as is apparent from Figure B.3.

Table C.1 : Observations for treated countries relative to treatment

Relative Year	Germany	Greece	Italy	Portugal	Sweden	Total
-7	0	0	0	1	1	2
-3	0	1	0	1	1	3
-1	1	0	1	1	1	4
1	0	1	0	1	1	3
3	1	1	1	1	1	5
5	1	0	1	1	1	4
7	1	0	1	1	1	4
9	1	1	1	1	1	5
11	1	1	1	0	0	3
13	1	1	1	0	0	3
15	1	0	1	0	0	2
Total	8	6	8	8	8	38

Note: This table reports the years in which we have observations for each of the treated countries relative to the year of the event (big drop in EPL).

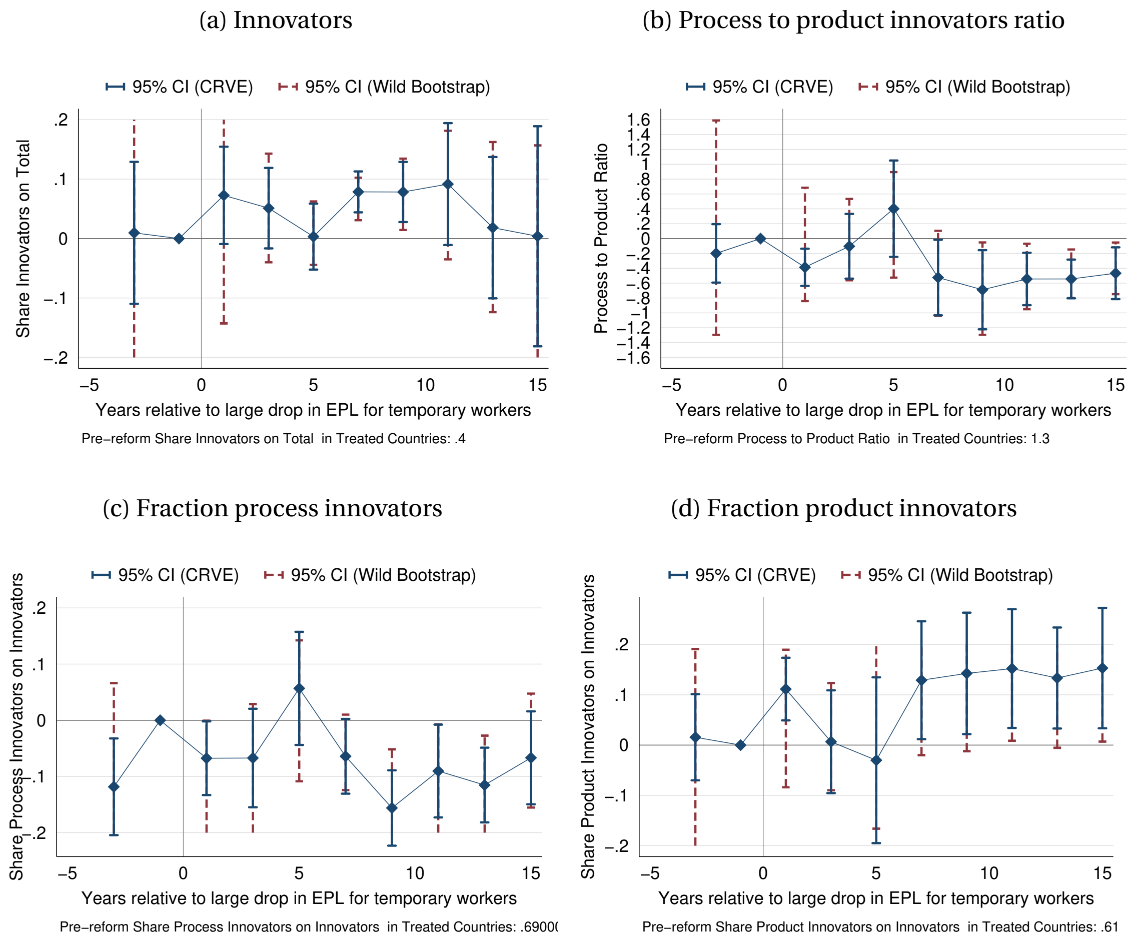
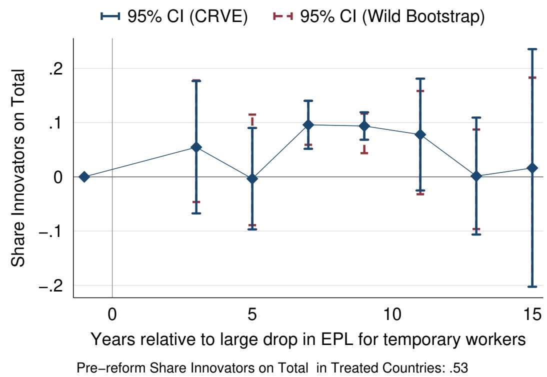


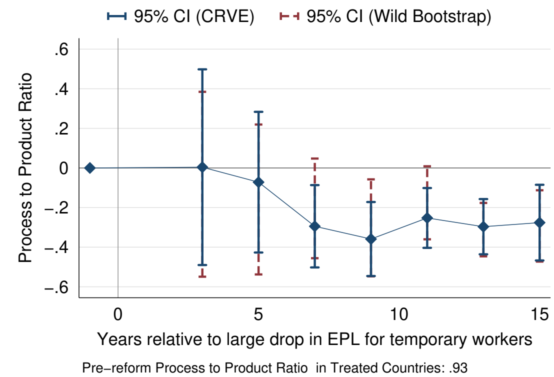
Figure C.4: Robustness to using all sectors

Note: The panels in the figure plot the coefficients κ_e from regression (2), except that country and time fixed effects are replaced by country-by-sector and time-by-sector fixed effects. The sample includes all sectors available. The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details.

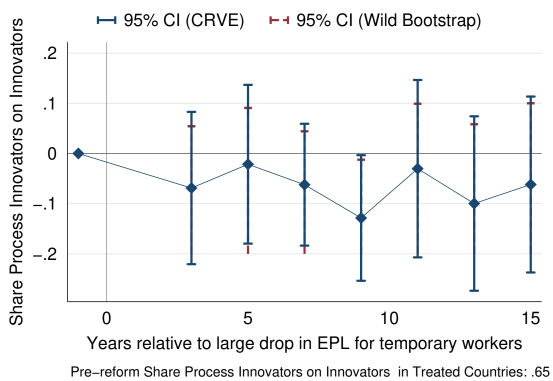
(a) Innovators



(b) Process to product innovators ratio



(c) Fraction process innovators



(d) Fraction product innovators

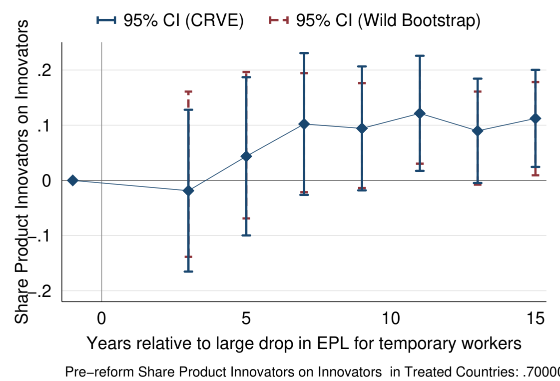
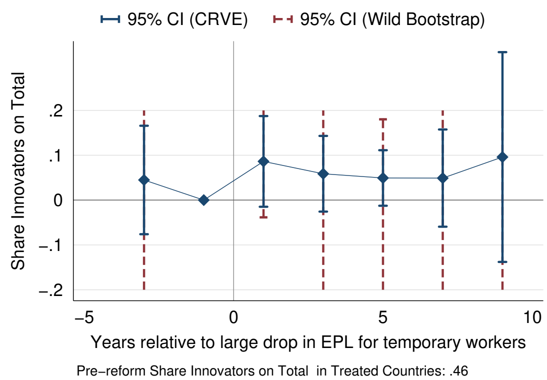


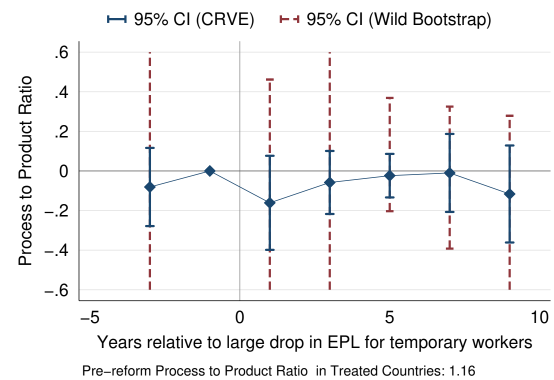
Figure C.5: Robustness to balancing the panel. Germany and Italy as treated countries.

Note: The panels in the figure plot the coefficients κ_e from regression (2). The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details. The sample is restricted to treated countries that we observe in year -1 and then in all odd years from 3 to 15 relative to the treatment year (Italy and Germany). See Table C.1 for availability of observations for the treated countries. These correspond to the countries that experience a large EPL drop (see note to Figure C.3). Among the control countries, we restrict to those that we observe for all eight waves (i.e. we drop Denmark, Turkey, Latvia and Iceland).

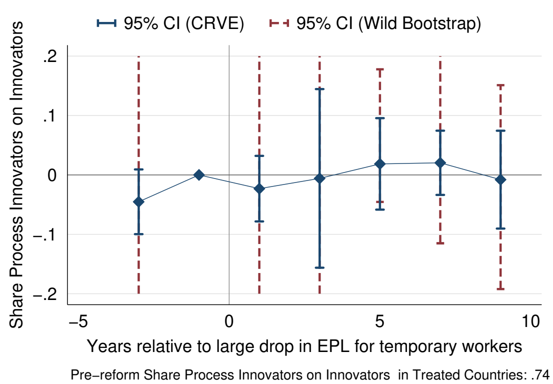
(a) Innovators



(b) Process to product innovators ratio



(c) Fraction process innovators



(d) Fraction product innovators

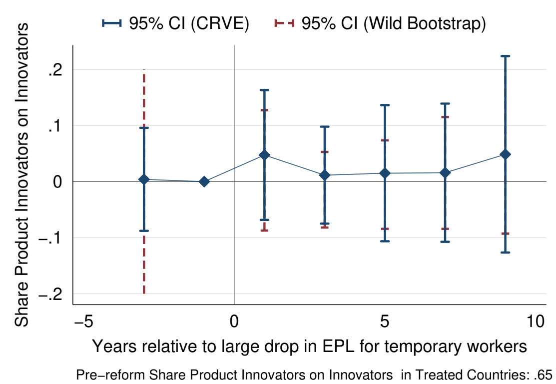


Figure C.6: Robustness to balancing the panel. Portugal and Sweden as treated countries.

Note: The panels in the figure plot the coefficients κ_e from regression (2). The treatment is a big drop in EPL for temporary workers. See note to Figure 3 for details. The sample is restricted to treated countries that we observe in year -7 and then in all odd years from -3 to 9 relative to the treatment year (Portugal and Sweden). See Table C.1 for availability of observations for the treated countries. These correspond to the countries that experience a small EPL drop (see note to Figure C.3). Among the control countries, we restrict to those that we observe for all eight waves (i.e. we drop Denmark, Turkey, Latvia and Iceland).

C.3 Additional Controls

In this appendix, we discuss the addition of flexible covariate-specific time trends to our baseline specification (3), in order to assess the robustness of our findings to the inclusion of further control variables. Namely, we include interactions of year dummies with a set of covariates taken at their value in 2000 (before any of the treatments took place). The covariates we include are:

- *Labor market features* in 2000: EPL for individual dismissals; EPL for collective dismissals; EPL for regular workers (weighted average of EPL for individual and collective dismissals); collective bargaining coverage; trade union density; spending on unemployment benefits (% of GDP); spending on active labor market policies (% of GDP); total spending on labor market policies (sum of unemployment benefits and active labor market policies); labor share for the manufacturing sector.

- *Sectoral composition* in 2000: average potential for automation, constructed as an employment-weighted average (using 2000 employment levels) of the average robot penetration for detailed manufacturing sectors in the US for 2004 (Acemoglu and Restrepo, 2020a);[28] task offshoring at the broad industry level (Autor and Dorn, 2013); average capital-labor ratio for the manufacturing sector.

These controls are not available for all countries in year 2000: Tables C.3 and C.5 indicate the coverage for each variable. Since the sample size does not allow us to include all the covariates at the same time, we proceed as follows.

First, we select a subset of summary variables to include in the diff-in-diff specification (3), chosen to maximize the spectrum of areas and the number of countries covered, while preserving enough degrees of freedom to reliably estimate the parameters of interest. To this end, our first set of specifications includes Automation potential, EPL for regular workers (both individual and collective dismissals), Task offshoring, Total spending on labor market policies, and the Capital-labor ratio. These results are presented in the main text in column 6 of Table 2. The availability of these variables by country is listed in Table C.3 .

Second, we present results for additional variables individually. Availability is summarized in Table C.5 and results reported in Table C.4 .

[28] We chose to use the US APR to avoid any endogeneity issues with the European measure. We choose 2004 as our reference year, as it is the earliest year reported by Acemoglu and Restrepo (2020b) that features non-negligible robot penetration in most of the sectors according to the IFR classification.

Results

Table C.2 reports the coefficients on the dummies "short run" (≤ 6 years after treatment) and long run (> 6) resulting from the estimation of our diff-in-diff specification with covariate-specific trends for automation potential, overall EPL, task offshoring, total spending on labor market policies, and the capital-labor ratio. The outcome of interest is the ratio of process to product innovation, our main summary measure. In the table, we indicate the controls included in each specification, and report the number of clusters together with model and residual degrees of freedom. These statistics reveal that the inclusion of interacted controls naturally results in substantial degree of freedom reductions, as well as a sample restrictions due to data availability (we report variable availability for each country in Table C.2).

Since the estimates are generally noisier, we report 90% wild-bootstrap confidence interval to assess the significance of our estimates. Column 1 of Table C.2 reports the results when all time-varying controls are included. This corresponds to column 6 of Table 2, from which it differs exclusively for the level of bootstrap confidence intervals. Columns 2-6 report the results when variables are included one at a time, which show that our results are qualitatively robust to different sample selections. While the magnitude of the estimated coefficient is reduced, we still see that the ratio of process to product innovators has fallen significantly in the long-run, as evident from the wild-bootstrap 90% confidence interval. The lowest midpoint estimate (in column 3) would still imply that big EPL drops reduce the ratio of process to product innovation by about 15pp (down from our baseline estimate of 25%).

Table C.4 reports the coefficients from the estimation of Equation (3) on the product-to-process ratio, using as controls detailed EPL for regular workers (distinguishing between individual and collective dismissals); detailed spending on labor market policies (unemployment and active labor market policies separately); and collective bargaining coverage and trade union density, which we exclude from the main set of controls due to their limited availability. Indeed, these additional variables are available only for non-overlapping sub-samples of our data (see Table C.5). For this reason, we cannot include all the controls together, and even limited combinations of these variables lead to prohibitive sample restrictions. With these caveats in mind, Table C.4 confirm that our results on the long-run effects of large drops in EPL on the ratio of process to product innovation are qualitatively robust, and 10% significant in almost all cases.

Table C.2 : Difference-in-differences results for the ratio of process to product innovators, added controls

	(1)	(2)	(3)	(4)	(5)	(6)
Short Run	-0.270	-0.025	0.030	0.010	-0.034	0.063
	(0.125)	(0.100)	(0.137)	(0.122)	(0.090)	(0.110)
	[−0.571, −0.008]	[−0.224, 0.154]	[−0.311, 0.281]	[−0.280, 0.247]	[−0.206, 0.156]	[−0.403, 0.278]
Long Run	-0.248	-0.214	-0.159	-0.188	-0.209	-0.249
	(0.055)	(0.074)	(0.062)	(0.078)	(0.080)	(0.082)
	[−0.369, −0.129]	[−0.331, −0.036]	[−0.289, −0.063]	[−0.360, 0.007]	[−0.328, −0.003]	[−0.363, −0.024]
Constant	0.802	0.783	0.806	0.816	0.783	0.773
	(0.112)	(0.056)	(0.048)	(0.061)	(0.058)	(0.038)
Automation potential	✓	✓				
EPL total	✓		✓			
Task offshoring	✓			✓		
Total LMP spending	✓				✓	
Capital-Labor Ratio	✓					✓
Observations	79	114	106	114	101	79
Number of Clusters	10	17	14	17	13	10
Number of Firms	1922666	2099149	2283908	2099149	2085006	1922666
DoF Residual	25	81	76	81	72	53
DoF Model	53	32	29	32	28	25

Note: The dependent variable in all columns is the ratio of process to product innovators. Cluster-robust standard errors in parentheses; wild-boostrap 90% confidence intervals in brackets. This table displays the estimates for our baseline diff-in-diff specification controlling for interactions of year dummies with the listed variables. See text for a description and sources for the control variables. “DoF Residual” and “DoF Model” denote the degrees of freedom of the residuals and the model respectively.

Table C.3 : Variable Availability for Selected Controls

Country	All controls	Automation potential	EPL total	Task offshoring	Total LMP spending	Capital-Labor Ratio
Austria	✓	✓	✓	✓	✓	✓
Belgium	✓	✓	✓	✓	✓	✓
Germany	✓	✓	✓	✓	✓	✓
Denmark	✓	✓	✓	✓	✓	✓
Greece		✓	✓	✓	✓	
Spain	✓	✓	✓	✓	✓	✓
Finland	✓	✓	✓	✓	✓	✓
France	✓	✓	✓	✓	✓	✓
Iceland		✓		✓		
Italy	✓	✓	✓	✓	✓	✓
Lithuania		✓		✓		
Luxembourg		✓		✓		
Latvia		✓		✓		
Netherlands	✓	✓	✓	✓	✓	✓
Norway		✓	✓	✓	✓	
Portugal		✓	✓	✓	✓	
Sweden	✓	✓	✓	✓	✓	✓
Turkey			✓			

Note: This table displays the availability for each variable and country in the benchmark year 2000. “All controls” denotes the joint availability of all the variables in the subsequent columns.

Table C.4 : Labor Market Controls, one by one

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Short Run	-0.120	0.023	-0.037	-0.026	0.105	0.019	0.057
	(0.107)	(0.130)	(0.099)	(0.089)	(0.089)	(0.155)	(0.088)
	[−0.383, 0.114]	[−0.334, 0.232]	[−0.264, 0.164]	[−0.218, 0.143]	[−0.124, 0.297]	[−0.389, 0.255]	[−0.294, 0.317]
Long Run	-0.225	-0.218	-0.235	-0.194	-0.092	-0.254	-0.238
	(0.059)	(0.056)	(0.074)	(0.084)	(0.064)	(0.048)	(0.077)
	[−0.326, −0.090]	[−0.321, −0.120]	[−0.343, −0.075]	[−0.324, 0.055]	[−0.220, −0.005]	[−0.331, −0.157]	[−0.351, −0.010]
Constant	0.772	0.792	0.762	0.798	0.769	0.791	0.777
	(0.036)	(0.043)	(0.062)	(0.055)	(0.041)	(0.038)	(0.050)
EPL collective	✓						
EPL regular		✓					
Active LMP spending			✓				
Unemployment spending				✓			
CB coverage					✓		
Trade union Density						✓	
Labor Share							✓
Observations	106	106	101	106	90	95	103
Number of Clusters	14	14	13	14	13	13	15
Number of Firms	2283908	2283908	2085006	2086551	1719133	2149570	2071147
DoF Residual	76	76	72	76	61	66	72
DoF Model	29	29	28	29	28	28	30

Note: Cluster-robust standard errors in parentheses; wild-boostrap 90% confidence intervals in brackets. This table displays the estimates for our baseline diff-in-diff specification controlling for interactions of year dummies with the listed variables. See text for a description and sources for the control variables. "DoF Residual" and "DoF Model" denote the degrees of freedom of the residuals and the model respectively.

Table C.5 : Labor Market Controls, Variable Availability

Country	All controls	EPL collective	EPL regular	Active LMP spending	Unemployment spending	CB coverage	Trade union Density	Labor Share
Austria	✓	✓	✓	✓	✓	✓	✓	✓
Belgium	✓	✓	✓	✓	✓	✓	✓	✓
Germany	✓	✓	✓	✓	✓	✓	✓	✓
Denmark	✓	✓	✓	✓	✓	✓	✓	✓
Greece		✓	✓	✓	✓	✓		✓
Spain		✓	✓	✓	✓		✓	✓
Finland	✓	✓	✓	✓	✓	✓	✓	✓
France		✓	✓	✓	✓		✓	✓
Iceland						✓	✓	
Italy	✓	✓	✓	✓	✓	✓	✓	✓
Lithuania								✓
Luxembourg					✓	✓		✓
Latvia								✓
Netherlands	✓	✓	✓	✓	✓	✓	✓	✓
Norway		✓	✓	✓	✓		✓	
Portugal		✓	✓	✓	✓	✓		✓
Sweden	✓	✓	✓	✓	✓	✓	✓	✓
Turkey		✓	✓			✓	✓	

Note: This table displays the availability for each variable and country in the benchmark year 2000. “All controls” denotes the joint availability of all the variables in the subsequent columns.

C.4 EPL for Regular Workers

In this appendix, we verify the robustness of our results to controlling for employment protection for regular workers, and discuss the alternative use of EPL for regular workers (individual and collective dismissals) to define treatment. We perform two sets of exercises. First, we run our main specification, which uses a drop in EPL temp for the definition of treatment, dropping countries that experience a large drop in EPL for regular workers over the sample period (Table C.7) or controlling flexibly for event-time dummies around drops in regular EPL (Table C.8). Second, we run our event-study regression using as event in turn a drop in EPL for regular workers (Figure C.7(a)) and any EPL drop (Figure C.7(b)).

Variable Construction

In the main analysis, the measure of employment protection we use (EPL Temp) is the index of "Strictness of hiring regulation for workers on temporary contracts". In the robustness exercises, the index of Employment Protection Legislation for regular workers (henceforth, EPL Total) we use is the index of "Strictness of dismissal regulation for workers on regular contracts (both individual and collective dismissals)". More details can be found in Appendix B.1.

For both EPL Temp and EPL Total, we select as the threshold for "large drops" the 2.5 percentile in the distribution of yearly percentage changes across European countries in the sample period, 2000 to 2016. For EPL Temp, this corresponds to a drop of 20%, while for EPL Total the corresponding figure is a drop of 10%. After excluding countries that experience a large increase in EPL Temp during the sample period (as explained in the sample selection, Section 2.3), we are left with five countries treated according to each of these measures. The countries and the corresponding year of large EPL drop are summarized in Table C.6 . For countries in which the event fell in an even year, we assigned the previous odd year as treatment, in order to conform to the biannual nature of the CIS data. In the main analysis (EPL Temp), this only affects Germany, which experienced a large drop in EPL in 2002 but to which we assign 2001 as the year of treatment. For EPL Total, this shifts the treatment for Greece and Spain.

For positive changes, we utilize a symmetric approach: we use the same threshold in absolute value (a yearly percentage change of 20% for EPL Temp and of 10% for EPL Total) and identify as large positive changes yearly percentage changes greater than that amount. For EPL Temp, this corresponds to the 97.5 percentile in the distribution of yearly percentage changes. As explained in the main body of the paper, these changes belong to countries that we drop from our main specification. By contrast, no change in EPL Total meets the criterion to be identified as large positive change, reflecting the fact that during the sample period changes in EPL for regular workers were rare, small, and relatively large ones were aimed at liberalizing the market rather than constraining it further.

Changes in EPL

In the main text, we argued that EPL for regular workers was mostly stable in Europe during the sample period - the changes were few and small on average. Indeed, when considering the distribution of yearly percentage changes, the mean is 0.5% and the standard deviation is 2.4 pp, while the corresponding figures for the EPL for temporary workers are 4% and 65.5 pp. The greater variability of the latter measure is the main justification for our preference.

Results

In order to show that our results are not driven by underlying changes in EPL for regular workers, we run the diff-in-diff specification (3) excluding the five countries that experienced a large drop in EPL for regular workers during the sample period. These countries are Italy, Greece, and Portugal (treated), and Spain and Denmark (control). The estimated coefficients are reported in Table C.7 . We also run an alternative specification in which, rather than excluding the countries that experienced a large drop in EPL Total, we include as controls event-time dummies relative to the drop in EPL Total, which allows us to flexibly control for the evolution of the outcomes of interest around this alternative treatment. The estimated coefficients are reported in Table C.8 . As can be noted by comparing these tables with Table 2 in the main text, estimates are largely unaffected, both in their magnitude and in their significance.

In analyzing the effect of changes in EPL on innovation activity we chose to use EPL for temporary workers as a proxy for the general level of EPL. While nothing in the theory indicates that the correct measure is EPL for temporary—rather than regular—workers, there were no large changes in EPL Total over the sample period. Thus, large changes in EPL for temporary workers are a good proxy for large changes in the general level of EPL in early-2000 European countries.

Figure C.7 reports the coefficients on the relative-time dummies from specification (2), using the ratio of process to product innovation as outcome. In panel (a) treatment is a large drop in EPL Total, while in panel (b) the treatment year is defined as the earlier between the year of drop in EPL Temp and the year of drop of EPL Total. In panel (a) we see no movement in the process to product innovation ratio around a drop in EPL Total. Note that this is almost identical to panel (b) in Figure C.3, where we had restricted the treated countries to those that had experienced a relatively smaller drop in EPL Temp. We interpret this evidence as suggesting that even relatively large drops in EPL Total were not large enough to trigger the reallocation of innovation activity from process to product innovation observed following the (much larger) drops in EPL Temp. Accordingly, panel (b) shows that, when using both measures, the evolution of the outcome of interest mirrors that of the main specification.

Table C.6 : Big EPL Drop Events

Country	Year drop in EPL Temp	Year drop in EPL Total
Germany	2001	.
Denmark	.	2005
Greece	2003	2009
Spain	.	2011
Italy	2001	2015
Portugal	2007	2011
Sweden	2007	.

Note: Year of large EPL drop by country. A negative change in EPL is considered large if it is in the bottom 2.5% in the distribution of yearly changes in the sample period; this corresponds to negative changes larger than 20% in absolute value for EPL Temp and than 10% for EPL Total. In order to conform to the biannual nature of the CIS data, for countries in which the drop took place in an even year, we assigned the previous odd year.

Table C.7 : Main Results Excluding Countries with Big Drops in EPL Total

	(1)	(2)	(3)	(4)
	Share Innovators on Total	Share Product Innovators on Innovators	Share Process Innovators on Innovators	Process to Product Ratio
Short Run	0.056	0.039	-0.094	-0.179
	(0.042)	(0.029)	(0.058)	(0.103)
	[−0.020, 0.160]	[−0.009, 0.125]	[−0.247, 0.097]	[−0.488, 0.006]
Long Run	-0.029	0.013	-0.129	-0.204
	(0.029)	(0.022)	(0.059)	(0.090)
	[−0.115, 0.034]	[−0.029, 0.071]	[−0.236, 0.018]	[−0.359, 0.024]
Constant	0.523	0.771	0.633	0.813
	(0.019)	(0.019)	(0.048)	(0.064)
N	82	82	82	82
Number of Clusters	13	13	13	13
Number of Firms	1193721	1193721	1193721	1193721

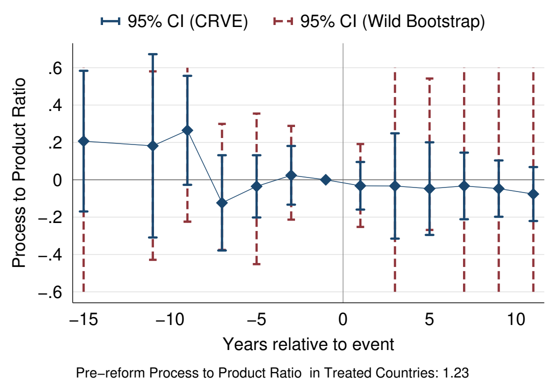
Note: This table reports the estimated coefficients of the diff-in-diff specification (3). Relative to the main specifcation, here we drop countries experiencing large drops in EPL Total during the sample period (Italy, Greece, Portugal, Spain and Denmark). Treated countries are Germany and Sweden.

Table C.8 : Main Results Controlling for Big Drops in EPL Total

	(1) Share Innovators on Total	(2) Share Product Innovators on Innovators	(3) Share Process Innovators on Innovators	(4) Process to Product Ratio
Short Run	0.058	0.072	-0.011	-0.133
	(0.028)	(0.049)	(0.061)	(0.132)
	[−0.010, 0.110]	[−0.141, 0.181]	[−0.138, 0.092]	[−0.452, 0.280]
Long Run	0.047	0.094	-0.044	-0.256
	(0.042)	(0.047)	(0.068)	(0.113)
	[−0.058, 0.129]	[−0.052, 0.184]	[−0.168, 0.087]	[−0.467, −0.007]
Constant	0.561	0.841	0.664	0.705
	(0.025)	(0.042)	(0.042)	(0.069)
N	119	119	119	119
Number of Clusters	18	18	18	18
Number of Firms	2298051	2298051	2298051	2298051

Note: This table reports the estimated coefficients of the diff-in-diff specification (3) including as controls flexible event-time dummies around a large drop in EPL Total.

(a) Event is drop in EPL for regular workers



(b) Event is earliest drop in EPL, either of EPL for temporary or for regular workers

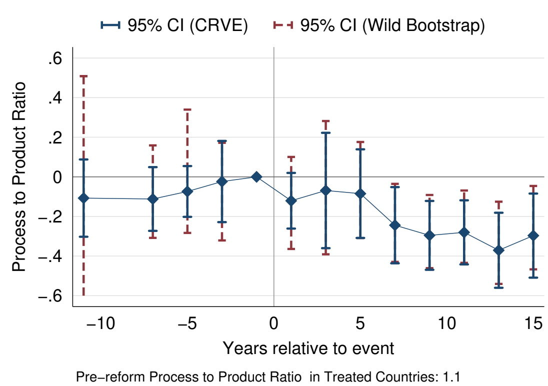


Figure C.7: Event study coefficients for alternative definitions of treatment

Note: The panels in the figure plot the coefficients κ_e from regression (2). The outcome is the ratio of process innovators to product innovators. See note to Figure 3 for details. In panel (a) the treatment is a large drop in EPL for regular workers. In panel (b) the treatment is the first year a country experiences a large EPL drop, either of EPL Temp or of EPL Total.

D Alternative Estimation Strategies

D.1 Interaction-Weighted Event Study

In this appendix, we estimate interaction-weighted (IW) event-study coefficients, and compute their standard errors using the procedure in Sun and Abraham (Forthcoming). We do not adopt this specification as our baseline because cluster-robust standard errors—an input for the computation of the IW variance-covariance—are not reliable with the few clusters we have (Cameron and Miller, 2015). At the same time, the bootstrap properties of the IW estimator with few clusters have not been explored yet. The estimator is constructed via the following steps:

1. Estimate the saturated interaction model:

$$Y_{it} = \alpha_i + \delta_t + \sum_{e \neq -1} \kappa_{e,c} \times \mathbb{1}\{\text{Cohort} = c\}_i \times \mathbb{1}\{t - (\text{Event Year})_i = e\} \times \mathbb{1}\{\text{Treated}\}_i + \varepsilon_{it}, \tag{4}$$

   with the same notation as in the main text, and where $\mathbb{1}\{\text{Cohort} = c\}_i$ is a dummy for whether country i belongs to treatment cohort $c \in \mathscr{C}$, the set of all treated cohorts. In this context, the estimated coefficients $\hat{\kappa}_{e,c}$ are cohort-average treatment-on-the-treated (CATT) effects for relative event time e. In our application, we assign a different cohort to each country to examine country-specific effects. We verify that this procedure results in the same IW estimator and standard errors as assigning a different cohort depending on the treatment year $(\text{Event Year})_i$.

2. Estimate the system of auxiliary regressions:

$$\{\text{Cohort} = c\}_i = \sum_{e \neq -1} \xi_{e,c} \mathbb{1}\{t - (\text{Event Year})_i = e\} \times \mathbb{1}\{\text{Treated}\}_i + \upsilon_{it}, \ \forall c \in \mathscr{C}. \tag{5}$$

   These regressions return an estimate for the share of observations from cohort c that are treated at event time e, $\hat{\xi}_{e,c}$, along with the variance-covariance matrix of the system, needed to compute the standard errors.

3. Finally, obtain the IW estimator for each event period by averaging the coefficients from (4) using the shares in (5):

$$\hat{\beta}_e^{IW} = \sum_{c \in \mathscr{C}} \hat{\xi}_{e,c} \hat{\kappa}_{e,c}. \tag{6}$$

We compute the standard errors for the estimator at each event time e following Proposition 6 in Sun and Abraham (Forthcoming).

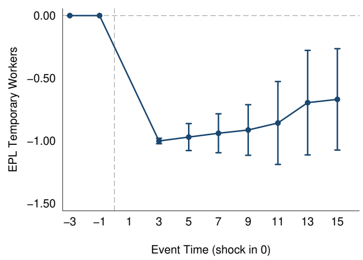
Results

The resulting estimates allow us to rule out the presence of significant pre-trends for all variables of interest. Further, we are able to analyze the effects of a reduction in EPL strictness for the various countries separately. In the bottom panel of the following figures, we plot the coefficients $\hat{\beta}_e^{IW}$. In each figure, the five upper panels report the coefficients $\hat{\kappa}_{e,c}$ for each country c, together with cluster-robust standard errors. The identification for the estimated effects comes from a comparison of each of the treated countries with never-treated, as well as yet-to-be-treated, countries.

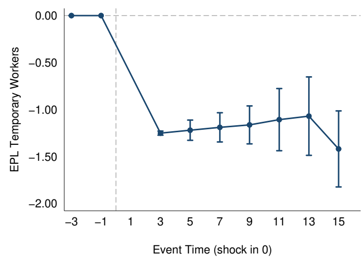
Overall, we find that all the main results presented in the text are robust to this estimation procedure. In particular, Figure D.4 confirms that the share of product innovators increases following a big drop in EPL, leading to an overall reduction in the process/product ratio in Figure D.6. Similarly, the share of firms conducting only process innovation falls (Figure D.7). The country-specific effects underlying the IW estimator also highlight that overall treatment effects are driven by changes in Germany, Italy and Portugal, which are also the countries where the share of temporary workers rose significantly after the labor market reforms (Figure D.2), suggesting that the reforms relaxed a previously binding constraint. These same countries see a significant reduction in the process/product ratio, as displayed in Figure D.6. We interpret these findings as indicating that the effects of the labor market reforms studied in this paper were mediated by an increase in the take-up of more flexible temporary contracts.

Figure D.1: EPL Temp: Country-Specific Coefficients and IW Estimator

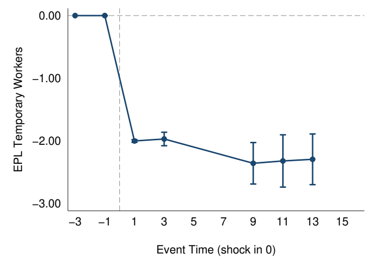
(a) Germany



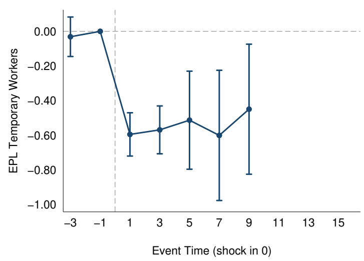
(b) Italy



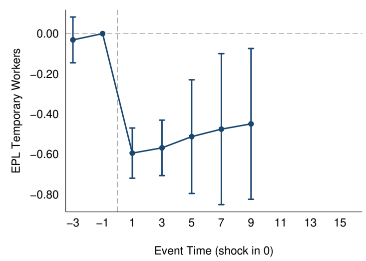
(c) Greece



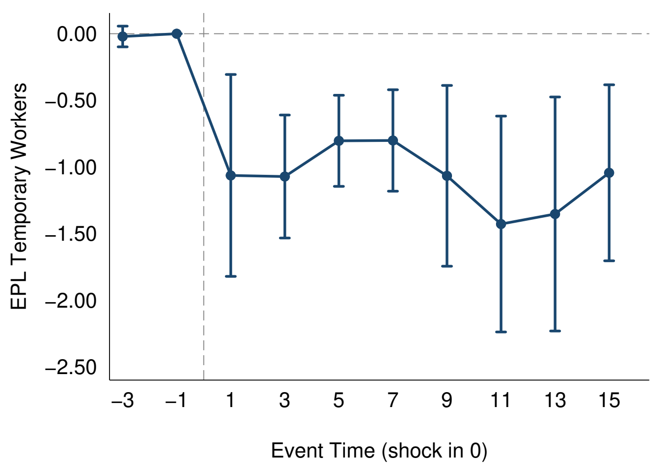
(d) Portugal



(e) Sweden

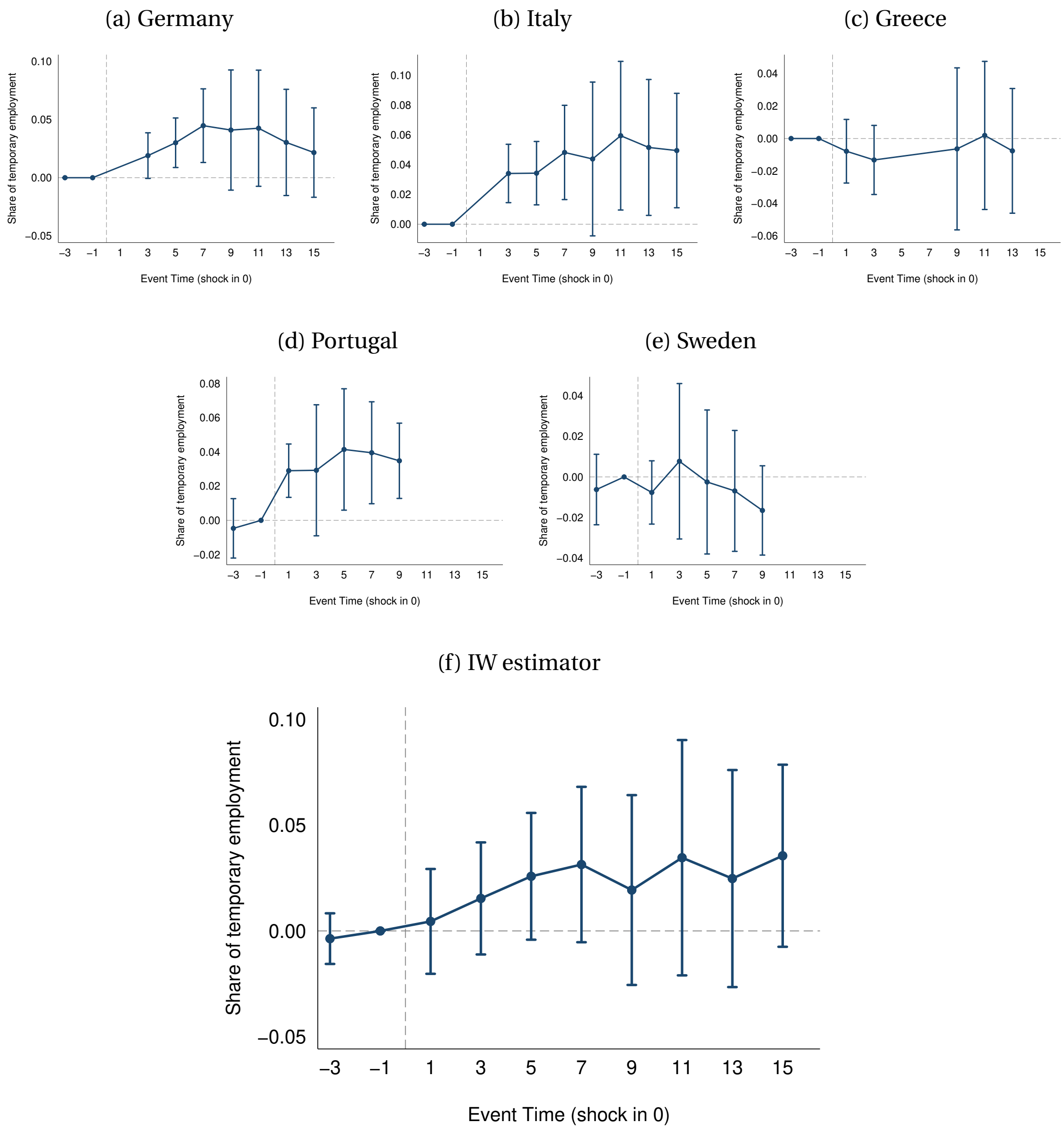


(f) IW estimator



Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

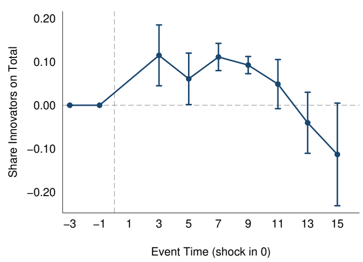
Figure D.2: Share Temp: Country-Specific Coefficients and IW Estimator



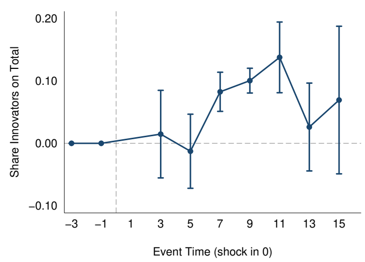
Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

Figure D.3: Innovators on Total: Country-Specific Coefficients and IW Estimator

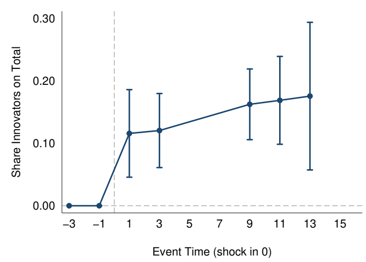
(a) Germany



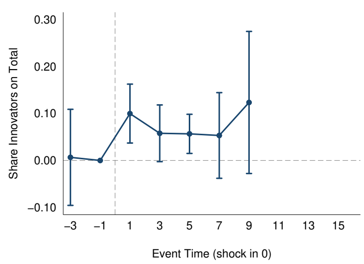
(b) Italy



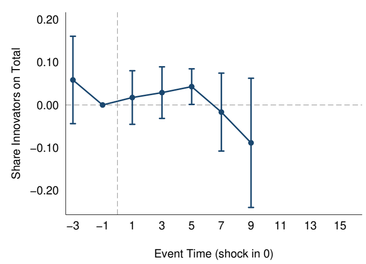
(c) Greece



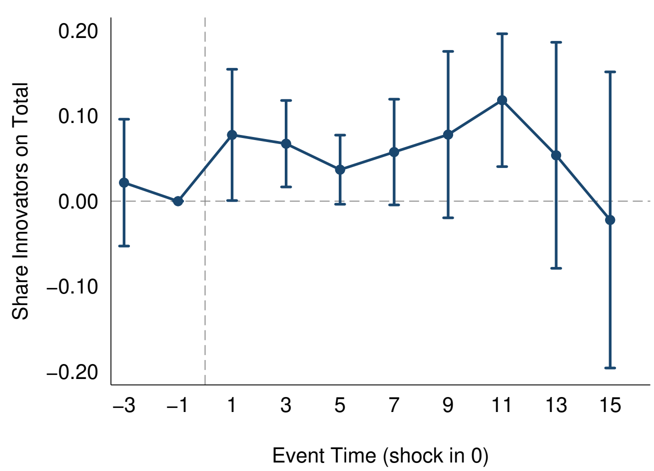
(d) Portugal



(e) Sweden

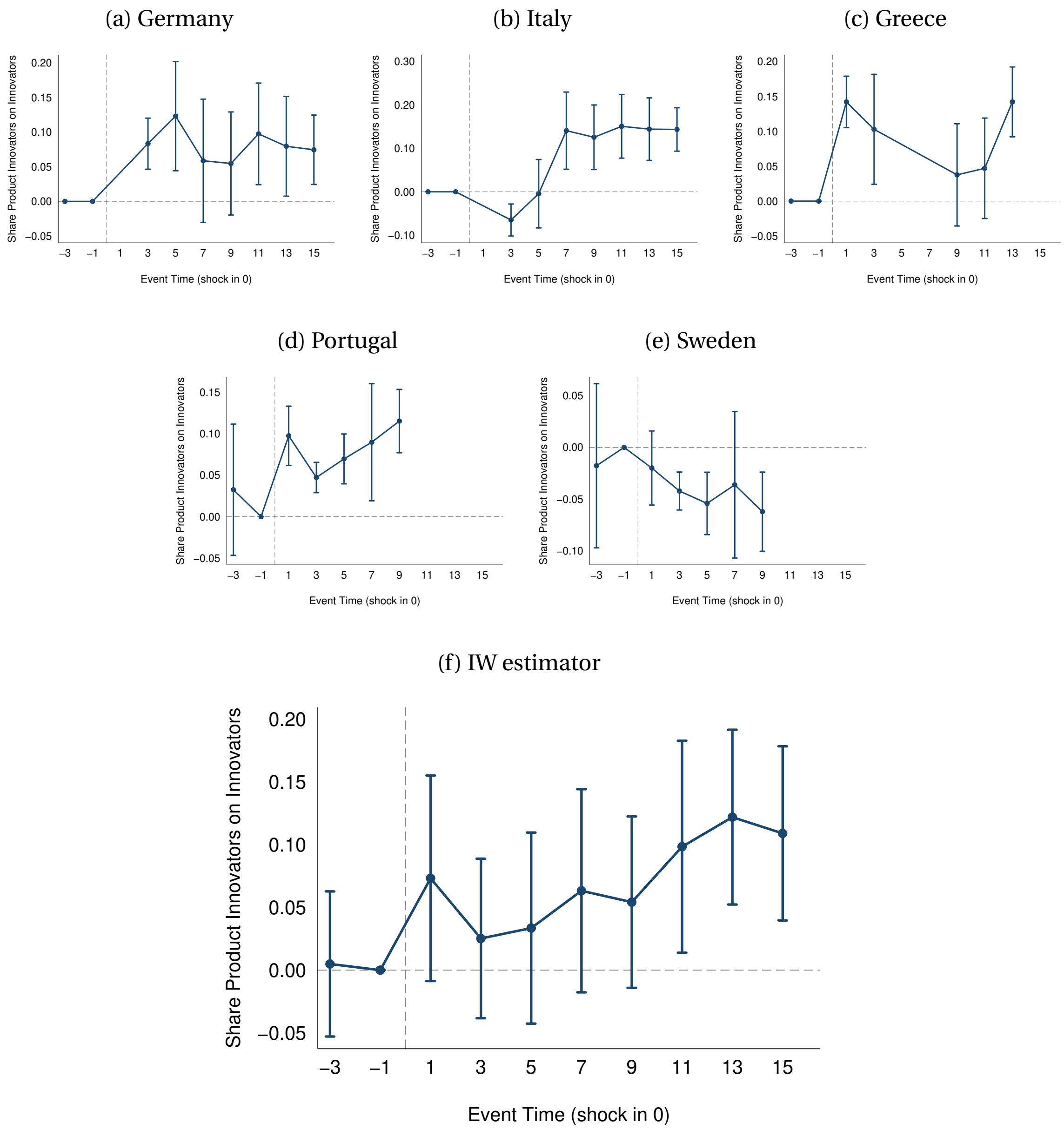


(f) IW estimator



Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

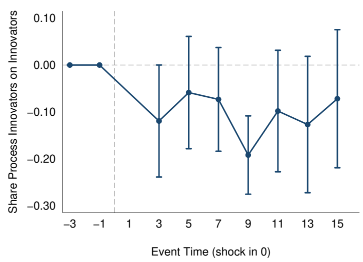
Figure D.4: Product Innovators on Innovators: Country-Specific Coefficients and IW Estimator



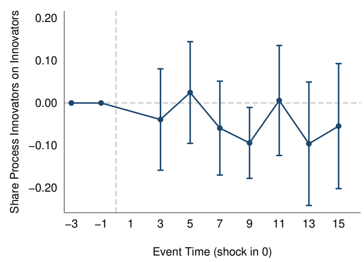
Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

Figure D.5: Process Innovators on Innovators: Country-Specific Coefficients and IW Estimator

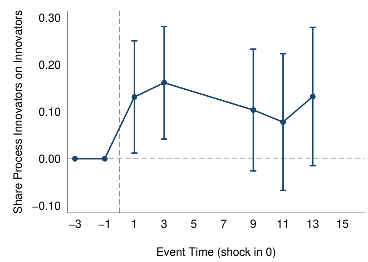
(a) Germany



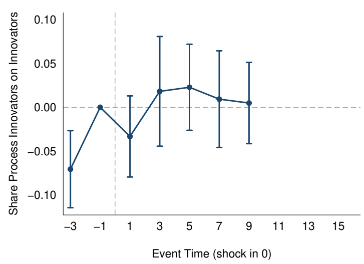
(b) Italy



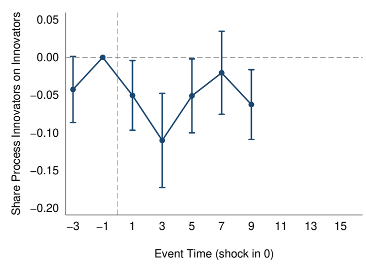
(c) Greece



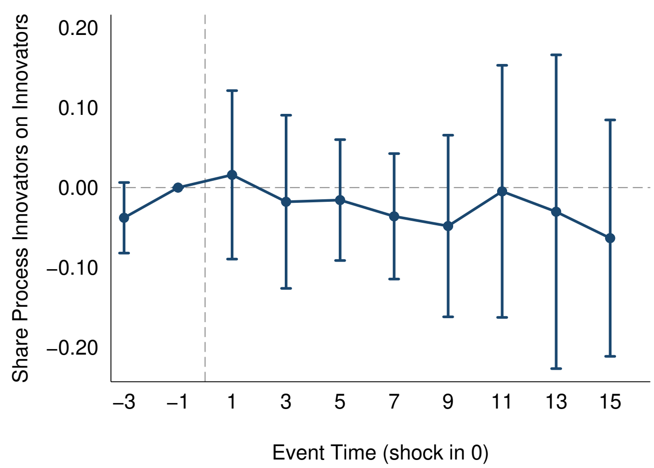
(d) Portugal



(e) Sweden



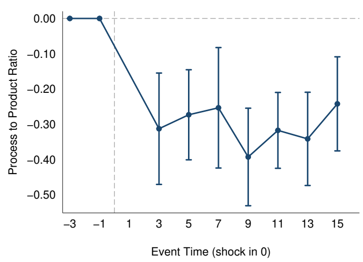
(f) IW estimator



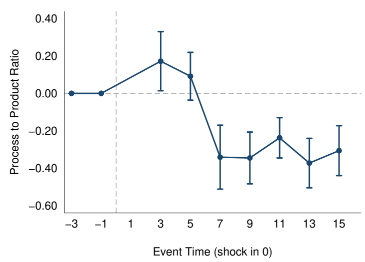
Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

Figure D.6: Process on Product Ratio: Country-Specific Coefficients and IW Estimator

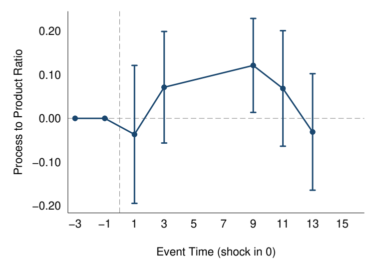
(a) Germany



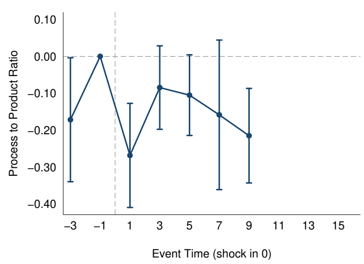
(b) Italy



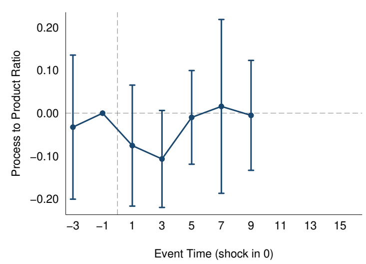
(c) Greece



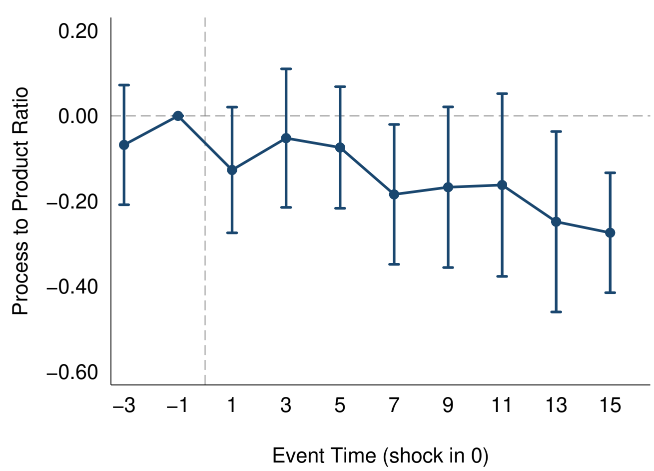
(d) Portugal



(e) Sweden

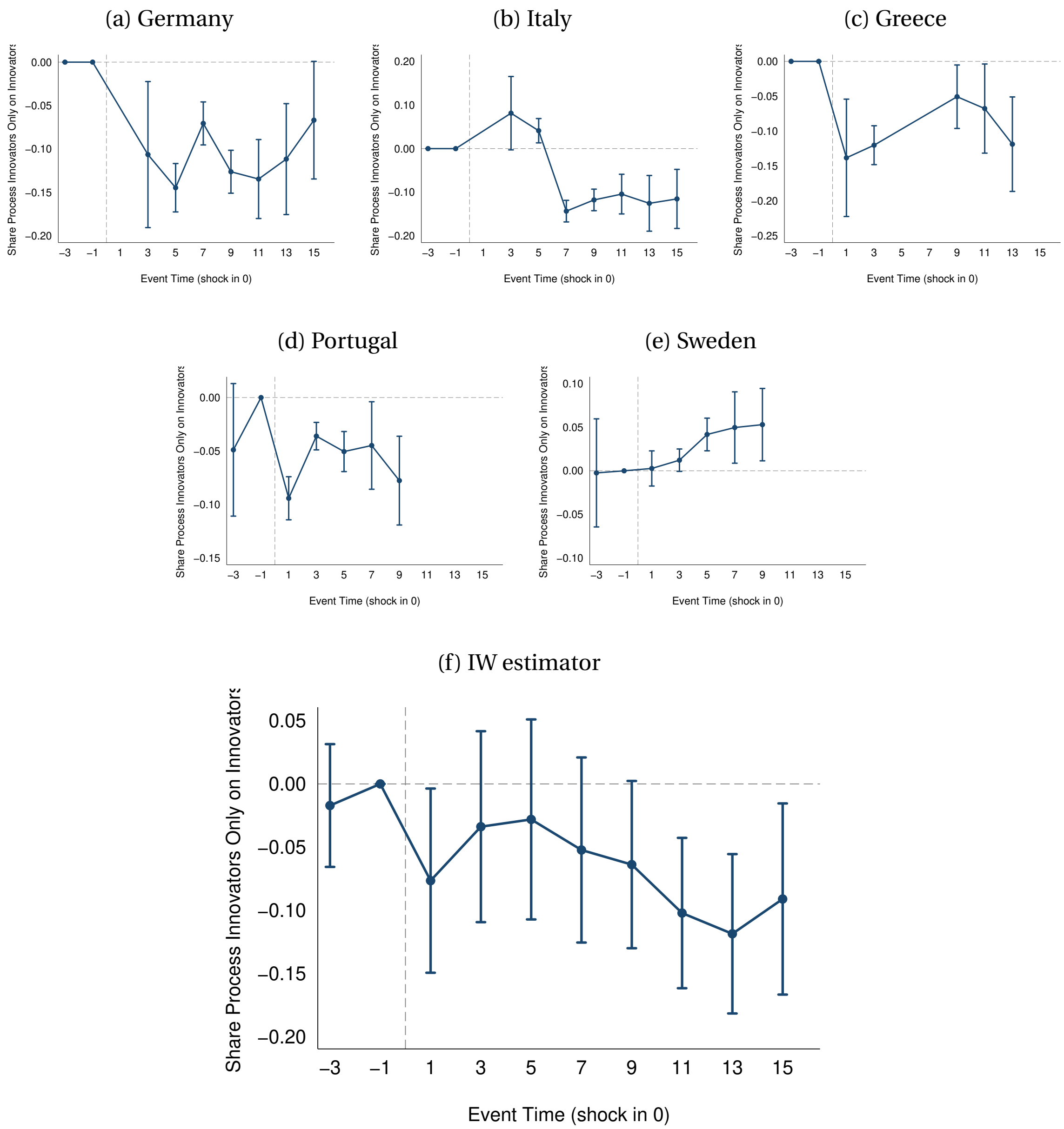


(f) IW estimator



Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

Figure D.7: Process Only on Innovators: Country-Specific Coefficients and IW Estimator



Note: Panels (a) to (e) report country-specific event-study coefficients, $\kappa_{e,c}$, from Equation (4) . Panel (f) displays the interaction-weighted event-study coefficients obtained from their aggregation, $\hat{\beta}_e^{IW}$, from Equations (5) and (6) . Panels (a) to (e) report cluster-robust standard errors, while panel (f) reports IW standard errors constructed following Proposition 6 in Sun and Abraham (Forthcoming). See main text for the dependent variable definition.

D.2 Permutation Tests

In what follows, we use randomization inference to test the significance of our results. We conduct permutation experiments reassigning treatment status and/or year relative to treatment in the following way: (1) across periods within treated countries (within); (2) across countries preserving the treatment periods (between); (3) across both countries and periods.

We run the diff-in-diff specification (3) and focus on the "long run" coefficient. Specifically, we test whether the distribution of estimated long run treatment effects is centered around 0. In particular, if the distribution resulting from (1) is non-centered, baseline treatment effects stem partly from permanent heterogeneity across countries. That is, random assignment across countries is not satisfied. If the distribution resulting from (2) is non-centered, baseline treatment effects stem from differences in the time path of variables. That is, random assignment over time is not satisfied. The distribution from (3) provides a way to compute an overall randomization p-value.

Our main references are: Kennedy (1995) for residualization and general concept; Rothstein (2010) for the placebo test idea; Hsiang and Jina (2014) for the implementation; MacKinnon and Webb (2020) for using t-stat for p-values instead of coefficients.

Methodology

Our main specification (Equation (3) in the main text) can be written compactly as

$$Y_{it} = \alpha_i + \delta_t + \beta_{SR} \cdot D_{it}^{SR} + \beta_{LR} \cdot D_{it}^{LR} + \varepsilon_{ct},$$

where Y_{it} is the outcome of interest, α_i, δ_t are country- and time-effects, and D_{it}^{SR}, D_{it}^{LR} are dummies denoting treatment in the short run (equal to 1 if the country is treated and the years from treatment are between 0 and 6) and the long run (equal to 1 after 6 years from treatment). We conduct randomization inference on the coefficient β_{LR} to assess the potential bias generated by non-random assignment of the treatment across countries or time periods, as well as to generate alternative p-values for our estimates.

Using the Frisch-Waugh-Lowell theorem, we can rewrite our specification as:

$$\tilde{Y}_{it} = \beta_{LR} \cdot \tilde{D}_{it}^{LR} + \varepsilon_{it},$$

where, for $X \in \{Y, D^{LR}\}$, the notation $\tilde{X}$ indicates the residuals from regressing X on α_i, δ_t and D_{it}^{SR}.

We then apply the randomization scheme (4) in Kennedy (1995), which consists in reassigning $\tilde{D}_{it}^{LR}$ randomly N times (sampling uniformly without replacement), and computing the ensuing distribution of estimates $\hat{\beta}_n$, $n \in \{1, \ldots, N\}$, and of the corresponding t-statistics. We can use this distribution of t-statistics to obtain a p-value for the null hypothesis that the $\hat{\beta}_{LR}$ coefficient estimated from the original assignment is equal to 0. Crucially, under the null hypothesis we assume that the errors are exchangeable, that is, any permutation of true errors ε_{it} has the same distribution. In this case, the test delivers exact significance values.

In panel data, we have three ways of permuting the observations, which yield to different sets of coefficient estimates:

1. Permuting *within* countries, where we shuffle the time indexes, t, while keeping countries i fixed. In this case, the null hypothesis is that residuals are exchangeable over time within countries. Thus, the treated countries stay the same, but the treatment periods are shuffled. We call the estimated coefficients from this procedure $\hat{\beta}^w$, where w stands for *within*;

2. Permuting *between* countries, where we shuffle the i indexes, while keeping t fixed. In this case, the null hypothesis is that residuals are exchangeable across countries within time periods. We call the estimated coefficients from this procedure $\hat{\beta}^b$, where b stands for *between*;

3. Permuting both periods and countries, where it indexes are shuffled. We call the estimated coefficients from this procedure $\hat{\beta}^t$, where t stands for *total*.

These three schemes are used by Hsiang and Jina (2014), from which we also borrow parts of the randomization code. The corresponding t-statistics for the three coefficients are denoted by $\hat{T}^w$, $\hat{T}^b$, $\hat{T}^t$. Following MacKinnon and Webb (2020), and given a value for the t-statistic in the original sample, $\hat{T}$, we compute p-values:

$$p^j = \frac{1}{N}\sum_{n=1}^{N} \mathbf{1}\left\{|\hat{T}_n^j| > \hat{T}\right\}, \quad j \in \{w, b, t\}.$$

And use them to assess the significance of estimated coefficients.

The distributions of estimated t-statistics and coefficients can also inform us about the failure of random assignment along various dimensions. A non-centered distribution of coefficients resulting from *within* randomization points to the fact that country differences drive the estimated coefficients (if time is randomly assigned, treatment is just a country indicator). A similar result for *between* randomization points to the fact that estimated treatment effects are driven by time trends independent of treatment (if countries are randomly assigned, treatment is a period indicator).

Results

For each scheme, we report the randomization p-value computed as above, as well as the average of the distribution of the estimated long-run coefficients across all permutations. For all outcomes, the average of estimated coefficients is very close to 0, and at least one order of magnitude smaller than estimated coefficients in the original sample. We obtain p-values below 10% for all randomization schemes when the share of temporary workers is the dependent variable, and below 2% when the share of process innovators only and the ratio of process innovators to product innovators are the dependent variables.

The estimated coefficients from our baseline specification can be interpreted as causal if we have both random assignment of treatment across countries (so that treated countries can be compared to never-treated) and between time periods within each country (so that treated countries can be compared to yet-to-be-treated countries). The randomization schemes that we employ act along these two dimensions separately. The “within” scheme randomizes time periods among treated countries, while the “between” scheme randomizes treatment across all countries. This allows us to provide suggestive evidence in favor, or against, these random assignment hypotheses. In the Figures reported below, we depict the distribution of t-statistics for the coefficients estimated according to each randomization scheme. All distributions have their mean and mode around 0. The “between” scheme produces more skewed distributions. This result can be rationalized by recalling that treated countries constitute a sizable share of the overall sample, so many permutation estimate a non-zero treatment effects due to the inclusion of countries with true non-zero treatment effect in the permutation sample. Also recall that we produce these p-values by reassigning *residualized* treatment dummies, which are different from 0 in most cases. Thus, actually-treated countries often receive non-zero values for the treatment.

Overall, our findings depose against the presence of systematic bias, as evidenced by the centered coefficient distributions, and confirm the significance of our baseline results.

Table D.1 : Baseline estimates and randomization test results, full sample

	(1) Share Temp. Workers	(2) EPL Temp. Workers	(3) Innovators on Total	(4) Product on Innovators	(5) Process on Innovators	(6) Process Only on Innovators	(7) Process on Product
Short Run	0.024	-0.917	0.033	0.019	-0.007	-0.005	-0.013
	(0.000)	(0.086)	(0.028)	(0.023)	(0.023)	(0.023)	(0.055)
	[0.008]	[0.149]	[0.027]	[0.046]	[0.041]	[0.051]	[0.111]
	[0.000, 0.045]	[−1.275, −0.626]	[−0.027, 0.121]	[−0.090, 0.155]	[−0.145, 0.082]	[−0.193, 0.110]	[−0.422, 0.228]
Long Run	0.037	-0.883	0.061	0.097	-0.051	-0.090	-0.251
	(0.000)	(0.091)	(0.030)	(0.025)	(0.024)	(0.024)	(0.057)
	[0.016]	[0.205]	[0.033]	[0.039]	[0.045]	[0.022]	[0.059]
	[0.000, 0.090]	[−1.339, −0.428]	[−0.030, 0.120]	[0.001, 0.176]	[−0.167, 0.049]	[−0.143, −0.042]	[−0.351, −0.074]
N	119	119	119	119	119	119	119
Within p-value	0.0558	0.0040	0.1416	0.0487	0.3186	0.0027	0.0054
Between p-value	0.0333	0.0016	0.2081	0.1138	0.2001	0.0050	0.0162
Total p-value	0.0754	0.0090	0.1744	0.0941	0.3625	0.0118	0.0163
Within mean coefficient	-0.0000	-0.0007	-0.0002	-0.0003	-0.0002	-0.0002	0.0001
Between mean coefficient	0.0001	-0.0083	0.0013	0.0011	-0.0004	-0.0008	-0.0022
Total mean coefficient	0.0000	-0.0007	0.0002	0.0005	-0.0003	-0.0007	-0.0014

Note: parentheses denote OLS standard errors; brackets denote cluster-robust standard errors and wild-boostrap 95% confidence interval. Within, between and total "p-values" denote the p-value for the two-sided randomization test corresponding to the "Long Run" coefficient. The p-value is obtained as the share of absolute value of the t-statistic that are more extreme than the original sample across 10000 permutations. "Within" permutes time within clusters; "Between" permutes cluster assignment; "Total" permutes both time and cluster assignment. "Mean Coefficients" report the average of the estimated "Long Run" coefficients across the 10000 permutations for each randomization scheme.

Figure D.8: Share Temporary Workers

(a) Within Randomization (b) Between Randomization (c) Total Randomization

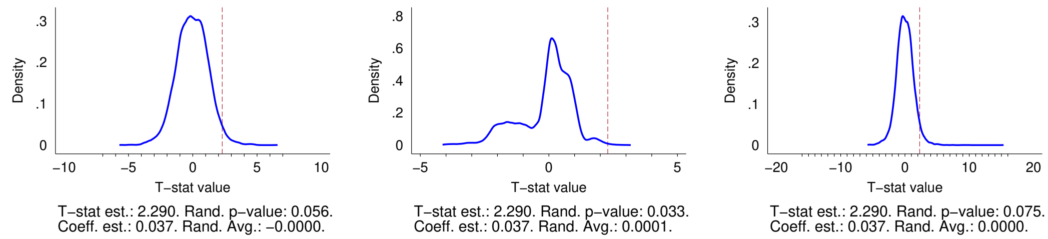


Figure D.9: EPL Temporary Workers

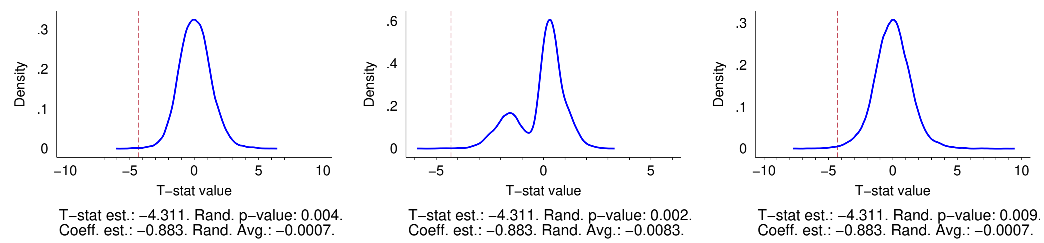


Figure D.10: Innovators on Total

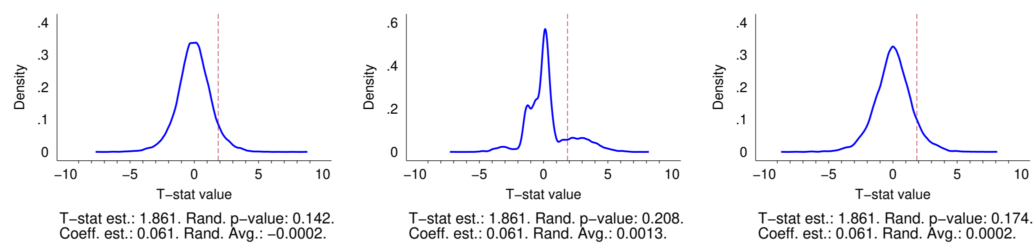


Figure D.11: Product on Innovators

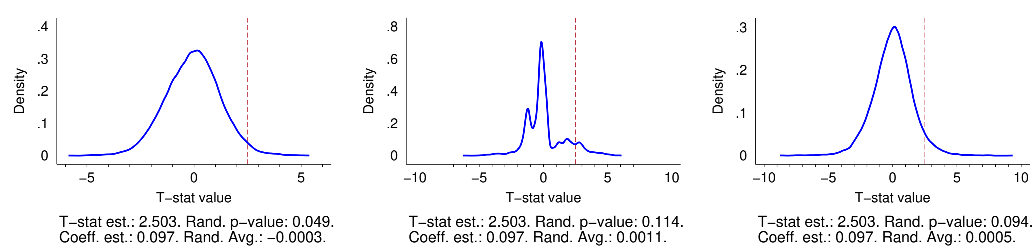


Figure D.12: Process on Innovators

(a) Within Randomization (b) Between Randomization (c) Total Randomization

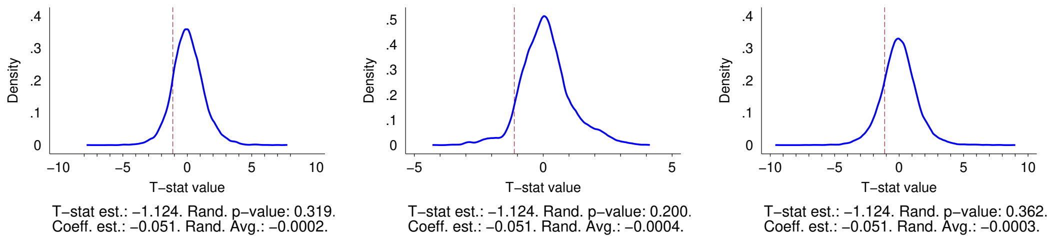


Figure D.13: Process Only on Innovators

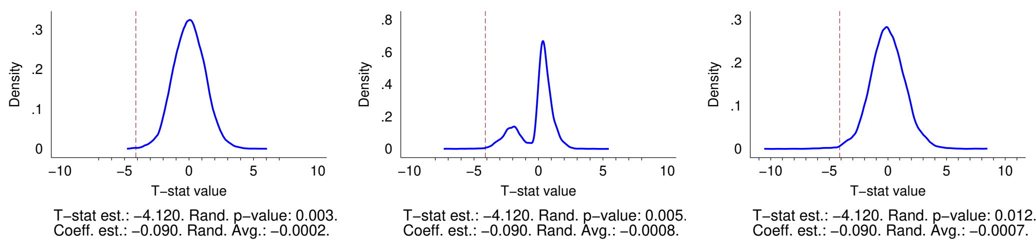


Figure D.14: Process on Product