

DISCUSSION PAPER SERIES

IZA DP No. 14465

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## ABSTRACT

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### **What Does Codetermination Do?\***

We provide a comprehensive overview of codetermination, i.e., worker representation in firms' governance and management. We cover the institution's history, implementation, and the best available evidence on its economic impacts. We argue that existing quasi-experimental estimates suggest that codetermination has zero or very small positive effects on worker and firm outcomes at the partial-equilibrium firm level. In addition, we test for general-equilibrium effects of codetermination laws using novel cross-country event studies exploiting a series of codetermination reforms between the 1960s and 2010s, and find no evidence that codetermination laws shift aggregate economic outcomes or the quality of industrial relations. We offer three potential explanations of the institution's limited impact. First, existing codetermination laws convey relatively little authority to workers. Second, countries with codetermination laws have high baseline levels of informal worker involvement in decision-making, independently of formal codetermination. Third, codetermination laws may interact with other labor market institutions, such as union representation and collective bargaining. We close by discussing implications of these facts for recent codetermination proposals in the United States.

**JEL Classification:** J08, K31, M1, M5

**Keywords:** codetermination, unions, worker representation

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\* We thank Nelson Mesker for excellent research assistance and Tommy O'Donnell for help with the draft. We thank Markus Helfen and Ewan McGaughey for helpful comments. Jäger thanks the MIT Policy Lab for financial support.

# 1 Introduction

Firms in the United States and other liberal market economies operate under a system of "shareholder primacy." Shareholder primacy is a model of corporate governance under which firms are ultimately controlled by their shareholders or owners, and are governed with the exclusive objective of maximizing the welfare of those shareholders or owners. In the past several years, amid rising inequality, a declining labor share, and growing evidence of the importance of employer power in the labor market, interest in alternative systems of corporate governance has risen rapidly. In particular, the European model of "codetermination," under which ultimate control over firms is shared between shareholders and workers, has received a great deal of attention. Since 2018, the U.S. Democratic Party, the British Labour and Conservative Parties, and the Australian Labour Party have proposed or expressed interest in codetermination legislation; prominent academics have called for a democratization of the workplace; and even the U.S. Business Roundtable and World Economic Forum have publicly distanced themselves from shareholder primacy.<sup>1</sup> This paper contributes to the ongoing debate by providing a comprehensive overview of the European model of codetermination, covering its history, institutional implementation, the best available evidence on its economic impacts, and potential explanations of its apparently limited effects.

European codetermination originated in a series of shared governance agreements signed by German unions and employer associations in 1918 and 1919, following a surge of post-war worker mobilization and culminating in the landmark Works Council Act of 1920 (McGaughey, 2016). In the subsequent decades, labor movements across Europe began advocating for shared governance, and codetermination arrangements were introduced through laws or collective agreements in Austria, Belgium, Denmark, Norway, and Sweden (Adler, 1922; Hurley, 1953; Bjorheim, 1974; Knudsen, 2006; Votinius, 2012). From the 1950s onwards, another series of codetermination laws passed in European countries expanded and codified workers' rights to participate in firm governance, and established the board-level and shop-floor codetermination arrangements that exist today (ETUI, 2020).

Under board-level codetermination, workers are allocated a share of the seats on a company's board, and thereby participate in high-level company decision-making (Jäger,

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<sup>1</sup>In the United States, the *Reward Work Act* and *Accountable Capitalism Act*, proposed by Democratic senators in 2018, would require 33% or 40% of seats on the boards of large companies to be allocated to workers. In the United Kingdom, the Conservative leader Theresa May called for worker representation on company boards during the 2016 election campaign (The Guardian, 11/12/16), and the Labour Party's platform in 2018 included a proposal for 33% worker representation on the boards of companies with more than 250 workers (Reuters, 09/23/2018). In 2019, the Australian Labour Party said it was considering various codetermination proposals (Sydney Morning Herald, 11/07/2019). Academics advocating codetermination include Anderson (2017), Ferreras (2017), Autor, Mindell, and Reynolds (2019), Piketty (2020), and Fraser, Mouffe, Sassen, Müller, Rodrik, Piketty, Zucman, Chang et al. (2020). The Business Roundtable and World Economic Forum issued statements in 2019 expressing support for stakeholder governance (a system of corporate governance in which firms have an obligation to serve non-shareholder stakeholders, such as workers).

Schoefer, and Heining, 2021). Meanwhile, under shop-floor codetermination, workers elect establishment-level representatives who hold information, consultation, and sometimes co-decision-making rights with respect to decisions about employment and working conditions (Addison, 2009). Board-level and shop-floor codetermination laws are widespread in Europe today, but absent in liberal market economies such as the United States, United Kingdom, Australia, and New Zealand (Adams, Bishop, and Deakin, 2016). Yet, surveys suggest that workers in these liberal market economies would prefer a greater voice in workplace governance (Bryson and Freeman, 2013; Kochan, Yang, Kimball, and Kelly, 2019; Hertel-Fernandez, Kimball, and Kochan, 2020; Mazumder and Yan, 2020).

*A priori*, codetermination laws could have dramatic positive or negative effects on both worker and firm outcomes. Opponents of codetermination argue that shared governance leads to "hold-up" problems that deter capital investment and cause production to stagnate, leaving shareholders worse off as profits vanish, workers worse off as wages fall, and consumers worse off as prices rise (Jensen and Meckling, 1979). Proponents of codetermination, on the other hand, emphasize the ability of shared governance to improve worker outcomes by correcting for imbalances of power between workers and employers (Frege and Godard, 2014; Anderson, 2017). Proponents also argue that codetermination improves productivity, firm performance, and worker satisfaction by incentivizing worker investment in firm-specific human capital and facilitating information sharing (Freeman and Lazear, 1995; FitzRoy and Nolan, 2021). In the public discourse, much of the rhetoric from both proponents and opponents of recent codetermination proposals has characterized the institution as radical and transformative. Senator Elizabeth Warren's *Accountable Capitalism Act*, which included moderate codetermination provisions, was hailed by some commentators as a plan aimed at "ending inequality [...] and saving capitalism" (Yglesias, 2018), and decried by others as "utterly bonkers" (Williamson, 2018) and "[a plan to] destroy capitalism" (Shackford, 2018).

The best available evidence, which we review here, indicates that neither of these perspectives accurately reflects the impacts of actually existing codetermination institutions. Modern empirical studies that exploit quasi-random variation in firms' exposure to codetermination conclude that both board-level and shop-floor codetermination have limited economic impacts at the partial-equilibrium level. From workers' perspectives, the introduction of codetermination in an individual firm results in either zero or very small positive effects on wages; it may slightly increase job security, possibly at the cost of reduced hiring; it does not appear to reduce the rate of voluntary separations; and recent evidence suggests that the institution may mildly improve subjective job quality. From the perspective of employers, firm-level introductions of codetermination either do not affect or very slightly increase productivity, capital investment, revenue, and firm survival. In addition, there is little evidence that employers attempt to avoid codetermination requirements, and survey evidence suggests that European managers

and CEOs have generally neutral or positive views of shared governance. These latter pieces of evidence provide further confirmation that codetermination is unlikely to substantially worsen firm performance.

The existing evidence we review consists entirely of microeconomic estimates of the impacts of introducing codetermination in individual firms. These estimates leave open the possibility that codetermination laws have general-equilibrium effects that cannot be picked up by studies exploiting firm-level variation in codetermination. For example, competition in product or factor markets may leave wages or amenities unresponsive to the implementation of codetermination in a specific firm, but overall job quality may rise in all firms if codetermination is imposed at the economy-wide level.

To address this challenge, we complement our review of existing microeconomic studies with novel evidence from country-level event studies, in the spirit of Acemoglu, Naidu, Restrepo, and Robinson's (2019) analysis of the effects of democratization. Drawing on historical datasets covering aggregate economic outcomes and the aggregate quality of industrial relations, we estimate the country-level effects of a series of codetermination reforms in Western European countries between the 1960s and 2010s using a cross-country difference-in-differences framework with synthetic control groups. We find no evidence that the introduction of codetermination laws affects wages, the labor share, productivity, capital formation, GDP, income inequality, or the quality of industrial relations as proxied by strikes, but find suggestive evidence for increases in the share of workers belonging to a union. We therefore conclude that there is no evidence that codetermination's general-equilibrium effects greatly exceed its small partial-equilibrium effects. However, we cannot entirely rule out this possibility, due to the slightly imprecise estimates and inherent limitations of our cross-country event study design, primarily the small sample size and the possible endogeneity of codetermination reforms.

We offer three potential explanations for the apparently limited impacts of actually existing codetermination institutions. First, existing codetermination laws convey relatively little authority to workers. Typically, workers are either granted a minority share of the seats on their company's board, meaning they can always be overruled by unanimous shareholders, or they are given the right to form a shop-floor representative body with few substantive decision-making rights. Surveys and interviews confirm that worker representatives believe they lack influence over most corporate decisions, with the exception of decisions about immediate working conditions. Since codetermination laws do not dramatically reshape existing authority structures, it is unsurprising that their effects are limited. It remains possible that codetermination structures involving a more significant shift of power to labor—like recent proposals for a bicameral model of firm governance where decisions would have to pass through both shareholder-elected and worker-elected bodies (Ferrerias, 2017; Fraser et al.,

2020)—would produce more dramatic effects. With the exception of one limited example we review below (the German coal, steel, and mining industries, where firms with more than 1,000 employees must adopt full parity representation at the board level), these experiments have not been attempted so far in any of the countries with codetermination laws.

Second, European countries with codetermination laws have very high baseline levels of informal worker consultation and involvement in decision-making, stemming from an overall culture of cooperative industrial relations. Surveys suggest that in non-codetermined firms, workers are informally involved in major decisions to almost the same extent as worker representatives are in codetermined firms. This provides another reason why formal codetermination may not meaningfully boost workers' influence, relative to the counterfactual. Based on our analysis of strikes, these cooperative cultures do not appear to be a consequence of the historical introduction of codetermination laws, though our evidence does not conclusively rule out this channel and suggests small aggregate increases in union density as a consequence of codetermination laws. Rather, the qualitative historical evidence suggests that codetermination laws were more likely to arise in countries with pre-existing cultures of industrial democracy and social partnership between workers and businesses. Notably, this explanation of codetermination's limited effects leaves open the possibility that introducing codetermination laws in a context like the United States, with more hostile pre-existing industrial relations, might have different impacts.

Third, codetermination laws coexist with other important pro-worker institutions whose influence may leave little room for codetermination to have an impact. European countries with codetermination laws tend to also have centralized collective bargaining systems, high rates of union representation, and extensive labor market regulations. These institutions may already capture most of the low-hanging fruit when it comes to affecting worker outcomes, leaving limited scope for codetermination to further shape outcomes. Of our three proposed explanations, this is the most speculative, since the few existing empirical tests of this hypothesis fail to corroborate it.<sup>2</sup> Additionally, it is possible that these institutions are instead complementary to codetermination, such that codetermination's effects would be even smaller in their absence; for example, widespread worker organization through union membership may be necessary for workers to fully take advantage of co-decision-making rights. In any case, this explanation once again leaves room for codetermination to have different effects if introduced in less regulated contexts, like the United States.

Overall, we conclude that codetermination as currently instituted is a relatively weak institution that has largely incremental impacts. Notably, codetermination proposals in the United States and elsewhere typically suggest replicating the European arrangements of

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<sup>2</sup>Studies that test for heterogeneity in the impacts of codetermination between contexts with higher versus lower collective bargaining coverage fail to find larger impacts of codetermination in contexts with lower collective bargaining coverage, as we describe in Section 6.3.

minority board-level representation or shop-floor representation. If codetermination's anemic impacts are explained by an inherent lack of power conveyed by these arrangements, then these codetermination proposals are likely to have similarly limited effects if implemented. If, by contrast, institutional features of European labor markets—like highly cooperative industrial relations, or extensive regulation—are responsible for codetermination's limited effects, then recent American codetermination proposals could have more significant impacts, either positive or negative. We close the paper with an extensive discussion of the implications of these considerations for codetermination proposals in the United States.

This paper focuses exclusively on the economic consequences of codetermination laws. There is a long history of advocacy and activism for codetermination from a non-consequentialist perspective, ranging from German invocations of democratic rights to justify workplace representation (Addison, 2009) to modern work in political philosophy condemning the arbitrary domination of workers by employers under systems of shareholder primacy and at-will employment (Anderson, 2017). This paper does not speak to these non-consequentialist justifications, and, if anything, the finding that codetermination has few economic consequences leaves more room for non-consequentialist considerations to influence policy-making.

**Outline** The rest of this paper proceeds as follows. In Section 2, we define codetermination, briefly discuss the key theoretical arguments for and against codetermination, and describe existing codetermination laws and their historical relationship with labor unions. Next, we synthesize the existing empirical evidence on the impacts of codetermination, beginning with worker outcomes in Section 3 and moving on to firm outcomes in Section 4. In Section 5, we address the concern that existing partial-equilibrium estimates ignore potential general-equilibrium impacts of codetermination laws, by contributing novel estimates of the country-level effects of codetermination reforms using a cross-country event study design. In Section 6, we build a comprehensive characterization of codetermination that we argue accounts for its apparently limited effects. In Section 6.1, we consider *power*: we draw on extensive qualitative evidence to argue that existing codetermination laws give workers little power. In Section 6.2, we discuss *the quality of industrial relations*: we point to survey evidence suggesting the existence of a powerful culture of informal workplace involvement that operates independently of formal codetermination. In Section 6.3, we discuss *institutions*: we consider whether the presence of powerful alternative pro-worker institutions explains codetermination's limited impact. The latter two explanations draw on features of European labor markets that are much less present in the United States, so in Section 7 we consider what the European evidence implies for recent American codetermination proposals. Finally, in Section 8, we conclude.

## 2 What is Codetermination?

### 2.1 Shareholder Primacy versus Codetermination

Codetermination is a system of corporate governance that is best defined by contrast with its main contemporary rival, "shareholder primacy." Under shareholder primacy, the day-to-day operations of a firm are controlled by managers, who are appointed by the firm's shareholders or owners and are charged exclusively with maximizing the welfare of those shareholders or owners. Typically, this is understood to mean maximizing long-term profits and ultimately dividend payouts. By contrast, under codetermination, managers and workers share control over the day-to-day governance of a firm, and the ultimate purpose of a firm is to serve the interests of both its shareholders and workers. Figure 1 visualizes the differences between shareholder primacy and codetermination.

Shareholder primacy, understood as the idea that firms should be governed with the exclusive purpose of maximizing the returns of shareholders or owners, is not enshrined in United States law. Rather, it is an academic legal doctrine that has been intellectually and culturally dominant since its popularization in the 1970s and 1980s by Milton Friedman (1970) and others (Posner, 2019). However, United States corporate law *has* allocated exclusive discretion over the governance of firms to managers and shareholders since the early 20th century (Millon, 2013), and workers have been specifically banned from participating cooperatively in corporate governance since the National Labor Relations Act of 1935 (Strine Jr., Kovvali, and Williams, 2021).

**Arguments for Shareholder Primacy** Shareholder primacy's intellectual dominance has been so extreme that in 2000, a pair of Yale and Harvard law professors declared that the view no longer has any "serious competitors" (Hansmann and Kraakman, 2000). This dominance has in part been the result of several influential theoretical arguments for shareholder primacy (originating from the powerful Friedman, 1970, doctrine). Proponents of shareholder primacy argue that, as a firm's "residual claimants," shareholders or owners are the only actors properly incentivized to grow the value of the firm, and therefore giving them control over corporate governance creates the right conditions for economic growth (Hansmann and Kraakman, 2000). In addition, a firm's residual claimants are the only actors whose rights cannot be adequately protected by contract (since they are entitled to the profits that remain when all of a firm's contractual obligations have been discharged), so the only way to protect shareholders' or owners' rights to the fruits of their investment is to give them control of the firm (Hansmann and Kraakman, 2000; Stout, 2002). In particular, supporters of shareholder primacy warn that involving workers in corporate governance leads to "hold-up" problems that reduce capital investment and leave everyone worse off (Jensen and Meckling, 1979). Essentially,

Jensen and Meckling's (1979) argument is that in the presence of codetermination, potential investors in a firm know that once the benefits of their investment are realized, workers can capture a significant share of the increase in surplus for themselves by increasing their own compensation. Knowledge of this fact deters investment, leaving both investors and workers worse off as capital investment stagnates:

[T]he workers will begin 'eating [the firm] up' by transforming the assets of the firm into consumption or personal assets. [...] It will become difficult for the firm to obtain capital in the private capital markets. [...] The result of this process will be a significant reduction in the country's capital stock, increased unemployment, reduced labor income, and an overall reduction in output and welfare. (Jensen and Meckling, 1979, p.504)

**Arguments for Codetermination** In contrast, proponents of codetermination offer a number of theoretical arguments in favor of shared governance. First, codetermination is one possible antidote for power imbalances between workers and employers that arise in imperfectly competitive labor markets. According to this view, empowering workers within firms' governance structures can prevent exploitation, abuse, and underpayment of workers (Frege and Godard, 2014; Anderson, 2017). This argument complements a broader tradition in labor economics and industrial relations that highlights the importance of institutional factors, such as worker representation, in determining wages and other aspects of worker outcomes (Osterman, 2011; Stansbury and Summers, 2020). Second, proponents of codetermination argue that shared governance improves productivity by fixing problems arising from the importance of implicit contracts and investment in firm-specific human capital (Malcomson, 1983; Furubotn, 1988; Smith, 1991; Freeman and Lazear, 1995; Strine Jr., 2002). In an influential class of models, asymmetric information or limitations on contracting force firms to rely on implicit promises of future promotions and pay rises in order to encourage workers to expend effort or develop their firm-specific skills (see, e.g., Lazear, 1979; Carmichael, 1983). Under shareholder primacy, workers have no guarantee that firms will not renege on these implicit promises, leading to a potential hold-up problem that discourages effort and investment in skills, making both sides worse off. Codetermination can help make these implicit promises credible, exactly because workers are involved in decision-making. Third, supporters of codetermination argue that it facilitates and incentivizes information-sharing, which can improve both worker satisfaction and firm performance (Hirschman, 1970; Freeman and Lazear, 1995).

Proponents of codetermination also offer theoretical retorts to a key challenge posed by advocates of the shareholder primacy view: since profit-maximizing firms appear not to voluntarily adopt codetermination in settings where it is not legally mandated, it must

not be a surplus-maximizing institution and can only be implemented by fiat (Jensen and Meckling, 1979). Defenders of codetermination reply that employers would not voluntarily give up power that lets them extract rents from workers even if doing so increased the size of the pie (Freeman and Lazear, 1995), and that "prisoner's dilemma" dynamics can prevent firms from unilaterally adopting codetermination even if an economy-wide imposition of the institution would be socially beneficial.<sup>3</sup> Finally, as we describe in Section 7, the National Labor Relations Act imposes stark limits on codetermination in the United States. The absence of voluntarily adopted codetermination arrangements therefore does not support the Panglossian conclusions by Jensen and Meckling (1979) in favor of shareholder primacy, at least in the United States. (Turning their argument on its head, one might jokingly contend that shareholder primacy can only be implemented by fiat.)

## 2.2 Codetermination Laws

Codetermination is most widely practiced in Europe, where many countries have passed codetermination laws that apply to firms above certain size thresholds. These laws typically require covered firms to adopt, or give their workers the right to demand, board-level worker representation, shop-floor worker representation, or both.

**Board-Level Representation** Under board-level representation, a certain share of seats on a company's board (usually 20-40%) are allocated to worker-elected representatives (Conchon, 2011). These representatives are given the same rights as other members of the board, meaning that workers receive a say on all issues that come before a company's board, with the exception of issues that constitute a direct conflict of interest. For example, in Sweden, board-level representatives must recuse themselves from decisions about collective bargaining or industrial action, as stipulated in Section 14 of the Board Representation (Private Sector Employees) Act.

Board-level representation laws are virtually unique to Europe—as Figure 2 illustrates, using data from the CBR Labor Regulation Index (Adams, Bishop, and Deakin, 2016), only five countries outside of the continent have such laws. Additionally, board-level representation requirements typically apply only to large firms; for example, in Germany, only companies with more than 500 employees must have worker representation on their boards (see Jäger,

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<sup>3</sup>For example, Levine and Tyson (1990) point out that codetermination can lead to within-firm wage compression that causes talented workers to leave for non-codetermined firms that offer higher wages (see also Acemoglu, Aghion, and Violante, 2001, for a related argument regarding unions). Burdín (2016) provides some empirical support for this claim. More broadly, Levine and Tyson argue that markets might have multiple equilibria – codetermined and non-codetermined equilibria – and firms in a non-codetermined equilibrium cannot unilaterally initiate a shift to the codetermined equilibrium. Hayden and Bodie (2021) argue that the adoption of codetermination can send a negative signal to the stock market about the state of a firm's labor relations, causing its stock price to crash. All of these dynamics might prevent unilateral voluntary adoption of codetermination even if codetermination would be socially beneficial when imposed on all firms.

Schoefer, and Heining, 2021, for exceptions where codetermination is mandated in smaller firms).

Almost all board-level representation laws grant workers a minority position on the board. The notable exception is Germany: while German firms with between 500 and 2,000 employees must allocate only 33% of board seats to workers, firms with over 2,000 employees are subject to "quasi-parity" representation, meaning that 50% of seats go to workers, but shareholders receive a tie-breaking vote. For historical reasons going back to the introduction of board-level codetermination in the aftermath of World War II, firms with more than 1,000 employees in the mining, coal, and steel sectors are subject to full parity representation, where workers receive 50% of seats and shareholders are not given a casting vote (Jäger, Schoefer, and Heining, 2021). The introduction of parity codetermination in these sectors has its roots in the desire of the post-war occupying powers to limit the influence of Nazi-affiliated industry leaders by imposing strong power-sharing requirements (Paster, 2012; McGaughey, 2016). Aside from this unique setting, full parity has not been implemented in other sectors; in fact, even quasi-parity for very large firms is unique to Germany.

**Shop-Floor Representation** Under shop-floor representation, elected shop-floor worker representatives are given information, consultation, and sometimes co-decision-making rights over questions of working conditions. Shop-floor representation laws are much more widespread outside of Europe than board-level representation laws, as Figure 3 illustrates, again using CBR Labor Regulation Index data. In addition, shop-floor representation laws usually have lower size thresholds (for example, in Germany, shop-floor representation rights apply to any establishment with 5 or more workers; Addison, 2009).

Shop-floor representation laws vary in the formal authority they give to workers. Employers are usually required to inform and consult shop-floor representatives in advance about decisions regarding working hours, working conditions, or the recruitment, transfer, or dismissal of employees (Aumayr, Stavroula, Foden, Scepanovics, and Wolf, 2011). These information and consultation requirements do not convey any substantive authority to workers, though they may put some implicit pressure on employers to reach a consensus with workers.

Some countries additionally give shop-floor representatives the right to appeal to an employment court if their advice is ignored; the employer's decision can then be overturned by the court, though usually the grounds for overturning a decision are very narrow. For example, Dutch shop-floor representatives have a right to provide advice on all major strategic decisions, and if their advice is ignored they can appeal to the Enterprise Chamber of the Amsterdam Court; however, the Court typically overturns only obviously negligent employer decisions (Eurofound, 1997).

Finally, a small number of countries grant shop-floor representatives co-decision-making powers. For example, in Germany, shop-floor representatives have a right to participate

in decisions about working hours, leave arrangements, the introduction of productivity-monitoring technology, and performance-related pay (Addison, Schnabel, and Wagner, 2001). In addition, German shop-floor representatives can veto "unwarranted" dismissals of staff, in which case the firm must bring a case to a labor court if they wish to override the veto (as determined in the Works Constitution Act). German shop-floor representatives also serve as the bargaining party representing workers in establishment- or firm-level negotiations, including over wages, when opening clauses allow for local deviations from collective bargaining agreements. Austrian shop-floor representatives have co-decision-making rights in the areas of working hours and disciplinary procedures (Aumayr, Stavroula, Foden, Scepanovics, and Wolf, 2011). In the Netherlands, most changes to workplace regulations (for example, changes to pension schemes, or hours/leave arrangements) must be approved by shop-floor representatives (ETUI, 2020).

Figure 4 maps the statutory powers of shop-floor representatives in European countries, based on data from the OECD/AIAS ICTWSS database (Visser, 2021). Only Germany, Austria, the Netherlands, and Sweden give shop-floor representatives co-decision-making powers; the other Western European countries, with the exception of the United Kingdom, give shop-floor representatives information and consultation rights with a possibility of judicial redress. Elsewhere in Europe, shop-floor representatives are limited to just information and consultation rights, or no right to shop-floor representation exists at all.

**Mandates versus Rights** Codetermination laws can be formulated either as a *mandate*, requiring all covered firms to adopt codetermination, or a *right* for workers in covered firms to take up codetermination. For example, board-level representation is a mandate in Germany (Jäger, Schoefer, and Heining, 2021) but a right in Finland (Harju, Jäger, and Schoefer, 2021), and shop-floor representation is a right even in Germany (Addison, 2009).

When codetermination laws are formulated as a right, take-up of codetermination among covered firms is usually widespread but not universal. For example, across Denmark, Norway, and Sweden, workers in about 53% of covered firms take up their right to board-level representation (Gregoric and Rapp, 2019); in Finland, workers in about 15% of covered firms take up formal board-level representation, and in a further 40% of covered firms workers negotiate alternative forms of representation as permitted by Finland's codetermination law (Harju, Jäger, and Schoefer, 2021); finally, in Germany, workers in about 20% of covered establishments set up shop-floor representation (Addison, Schnabel, and Wagner, 1997). Firms that take up codetermination tend to be larger and older than firms that do not (Fitzroy and Kraft, 1987; Addison, Schnabel, and Wagner, 1997; Gregoric and Rapp, 2019). Additionally, workers become more likely to take up codetermination rights following events that threaten their job security, like recessions or changes in ownership (Kraft and Lang, 2008; Jirjahn, 2009; Mohrenweiser, Marginson, and Backes-Gellner, 2011).

## 2.3 Unions and Codetermination

Codetermination is a system of cooperative co-governance between employers and worker representatives that contrasts with adversarial collective bargaining between employers and labor unions. Unions and codetermination have a complex relationship, both presently and historically.

**Positive Correlation Between Union Strength and Codetermination** As Panels (a) and (b) of Figure 5 show, drawing on data from the OECD/IAIS ICTWSS dataset (Visser, 2021), countries with codetermination laws tend also to have high union density and strong collective bargaining frameworks, a point to which we return in Section 6.3. This positive correlation between codetermination and union strength is likely explained by a couple of factors. First, countries with a disposition towards giving workers power may tend to both empower unions and implement codetermination laws. Second, as we now describe, unions have historically played a major role in advocating or bargaining for codetermination.

**Unions and Codetermination: a Fraught Historical Relationship** In Denmark, Sweden, Norway, and Germany, codetermination requirements initially arose not through legislation, but through large-scale collective agreements negotiated between unions and employer associations (Knudsen, 2006; Votinius, 2012; Bjorheim, 1974; McGaughey, 2016). Codetermination in Sweden, for example, originated in the 1938 Saltsjöbaden Agreement, where unions and employer associations agreed to limit their use of adversarial industrial action and instead work cooperatively towards mutually agreeable solutions. This Agreement was followed up by a 1946 Agreement establishing widespread shop-floor worker representation. The 1946 Agreement held until the 1970s, when the social compact between unions and employers began to break down in the face of economic turmoil. At this point, Swedish unions began lobbying the Swedish legislature for formal codetermination laws, efforts which culminated in the 1976 Co-determination Act (Wheeler, 2002; Votinius, 2012).

However, codetermination laws have also at times faced fierce opposition from unions and labor groups, who have sometimes viewed the laws as corporatist compromises or attempts to undermine the labor movement (McGaughey, 2018). For example, throughout the first half of the 20th century, Norwegian unions were internally split on whether to support or oppose codetermination.<sup>4</sup> As Bjorheim (1974) writes:

A committee on socialization set up by the Joint Committee of the Labour Party and Trade Union organization had in 1919 even opposed workers' representation on

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<sup>4</sup>After World War II, Norwegian unions decided to advocate for codetermination, and shop-floor representation requirements were established by a 1966 Co-operation Agreement between unions and employer associations. This was followed up by 1973 legislation introducing board-level worker representation in Norwegian firms with 30 or more employees (Bjorheim, 1974).

the boards of companies, arguing that the aim of the private capitalistic company is to make profit for the owners at the cost of the consumers. In the board of such a company the worker representative will very easily become co-responsible for the system he is fighting against. He will also easily become morally responsible for the company's management, without having a corresponding opportunity to exercise influence over it. (Bjorheim, 1974, p.6)

This aversion to direct cooperation with employers, and preference for adversarial industrial action, was echoed in the 1970s by the prominent American union leader Lane Kirkland, who reacted to codetermination proposals by declaring that:

[The American worker] is smart enough to know, in his bones, that salvation lies—not in reshuffling the chairs in the boardroom or executive suite—but in the growing strength and bargaining power of his own autonomous organizations. (As quoted in Summers, 1982, p.155)

The history of German codetermination exemplifies the oft-complex relationship between codetermination and labor unions. In Germany, the world's first codetermination law arose in 1920 out of industry-level codetermination agreements negotiated between unions and employer associations in 1918 and 1919 (McGaughey, 2016). However, the draft legislation was viewed by revolutionary labor groups as an unacceptably conciliatory compromise between their demands and the interests of employer associations, and the law's introduction was accompanied by a bloody demonstration in front of the Reichstag involving 100,000 workers (Weipert, 2012). Similarly, German labor groups opposed the 1952 law introducing shop-floor representation in its present form, out of fear that it would weaken the labor movement (Thelen, 1991). In 1976, Germany introduced a law increasing the share of worker representatives on the boards of large companies from 33% to 50%. This expansion was advanced and supported by labor groups, but heavily opposed by German employer associations (Höpner, 2004).

The frequent perception among labor groups that codetermination laws are insufficiently radical is rooted in the political history of codetermination legislation. Most existing codetermination laws, rather than being radical leftist policy victories, arose out of negotiated compromises between left-wing and right-wing groups. This is true of the introduction of codetermination laws in Denmark (Rose, 2008), France (Moss, 1988), Finland (Harju, Jäger, and Schoefer, 2021), and Germany (Paster, 2012).

**Conclusion** Overall, despite a fraught historical relationship, virtually all modern codetermination laws exist in countries with strong and widespread union representation. As we discuss in Section 6.3, this may mean that existing evidence on the effects of codetermination cannot be straightforwardly translated to settings like the United States, where private-sector

unions have become weak over time. This context should be kept in mind as we move on to reviewing the empirical evidence on the impacts of codetermination.

### 3 Job Quality: Do Workers Benefit from Codetermination?

Having reviewed the nature and history of codetermination, we now ask whether the available evidence suggests that the institution improves worker welfare. This question is of major interest for two reasons. First, one of the primary motivations for codetermination laws is their purported ability to boost worker power and thereby prevent mistreatment or exploitation of workers. In particular, a longstanding body of work in labor economics emphasizes the importance of institutional factors in shaping worker outcomes (e.g. Osterman, 2011; Stansbury and Summers, 2020). Systems of worker representation—like union representation or codetermination—are a major focus of this body of work. For example, Stansbury and Summers (2020) write that:

[...] eliminating all sources of market power may not be feasible. Instead, if increases in the labor share are to be achieved, institutional changes that enhance workers' countervailing power—such as strengthening labor unions or promoting corporate governance arrangements that increase worker power—may be necessary (but would need to be carefully considered in light of the possible risks of increasing unemployment). (Stansbury and Summers, 2020, p.7)

And Piketty (2020) argues that:

[Codetermination] has been one of the most highly developed and durable means of institutionalizing the new balance of power between workers and capital. (Piketty, 2020, p.500)

Second, understanding codetermination's effects on worker outcomes is key to understanding the institution's effects on firm performance and broader economic outcomes. For example, the "hold-up" hypothesis described in Section 2 claimed that codetermination worsens firm performance and slows economic growth *by* increasing the share of rents captured by workers. Checking whether codetermination indeed increases rent-sharing with workers therefore helps us understand its broader economic effects.

Our overall conclusion in this section is that the existing empirical evidence points to nonexistent or very small positive effects of codetermination on key proxies for worker welfare, including wages, rent-sharing, voluntary and involuntary separations, health, and subjective job quality.

### 3.1 Sources of Evidence

There is a large literature studying the effects of firm-level variation in codetermination on worker outcomes, consisting mostly of studies from the 1980s to 2000s examining German codetermination (see Conchon, 2011, for a summary of studies of German board-level representation and Addison, 2009, for a summary of studies of German shop-floor representation). These studies estimate the impacts of codetermination by comparing the outcomes of workers in firms with versus without codetermination, or in industries subject to more stringent versus less stringent codetermination requirements, controlling for the influence of confounding variables.

In our view, the ideal research design to study the causal effects of codetermination would be an experiment that randomly imposes codetermination in some firms (or some economies) but not in others. We particularly draw on two studies that use "natural experiments" to approximate this ideal design, by exploiting the introduction or repeal of the institution in some firms along with large micro data sets on individual firms and workers. First, Jäger, Schoefer, and Heining (2021) study the 1994 abolition of minority board-level representation requirements in new shareholder corporations in Germany. Second, Harju, Jäger, and Schoefer (2021) estimate the impacts of a 1990 introduction of minority board-level representation rights in Finnish firms with 150 or more employees, and a 2008 expansion of shop-floor representation rights to Finnish firms with between 20 and 30 employees. Both of these studies exploit the selective coverage of the codetermination reforms, and compare the outcomes of firms covered by the reforms to the outcomes of observably similar firms not covered by the reforms in a difference-in-differences framework. In forming our conclusions from the existing body of evidence, we place heavier weight on research designs we judge to more credibly approximate the ideal laboratory experiment, like these reform-based difference-in-differences studies. Additional work aiming to approximate the ideal experiment includes regression discontinuity designs exploiting sharp variation of firms above and below the relevant employment thresholds (see, e.g., Gorton and Schmid, 2004; Kim, Maug, and Schneider, 2019; Blandhol, Mogstad, Nilsson, and Vestad, 2020) and event studies of specific firms adopting shop-floor or board-level codetermination (see, e.g., Mueller and Stegmaier, 2017; Blandhol, Mogstad, Nilsson, and Vestad, 2020). These designs require different (plausibly stronger) identification assumptions compared to reform-based difference-in-differences designs (for a discussion, see Harju, Jäger, and Schoefer, 2021), although we view them as considerably more compelling than the rich literature comparing firms with and without codetermination.<sup>5</sup>

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<sup>5</sup>For instance the regression discontinuity designs require the assumption that firms do not strategically affect their size to evade or seek codetermination (even if codetermination had large effects). The event study designs require the assumption that firms' decision to adopt codetermination is unrelated to panel variation in the outcome variables. The reform-based difference-in-differences designs, by contrast, require the assumption of parallel trends in outcomes of the treatment and control groups absent the reform, and the absence of

The existing literature consists almost entirely of microeconomic studies that examine variation in codetermination at the firm level. With the exception of a few industry-level studies (Svejnar, 1981; Benelli, Loderer, and Lys, 1987), the existing body of evidence cannot speak to the general-equilibrium effects of codetermination laws. To fill this gap, after reviewing the existing microeconomic evidence on codetermination in this section and in Section 4, we contribute novel estimates of the country-level impacts of codetermination laws in Section 5, using cross-country event studies.

## 3.2 Impacts on Worker Outcomes

**Wages and Rent-Sharing** While most codetermination laws do not permit worker representatives to participate directly in wage setting (with wage negotiations being left to unions), codetermination might indirectly increase wages by expanding workers' influence and thereby boosting their bargaining power. It might also decrease wages by worsening firm performance and reducing the size of the pie that is divided between workers and employers. Recent studies find either no effects, or small positive effects, of minority board-level representation and shop-floor representation on wages and the sharing of profits with workers. The evidence on the wage effects of quasi-parity or parity board-level representation is less clear; two studies find moderate effects on wages, but in opposite directions.

In the German context, Jäger, Schoefer, and Heining (2021) estimate that minority board-level representation increases composition-adjusted wages by around 1.1%, and are unable to rule out zero effects (the 95% confidence interval extends from -1.8% to 4%). They also find no effect on the degree to which rents are shared with workers. Evidence from the Nordic countries shows similarly limited effects of minority board-level representation on wages. Harju, Jäger, and Schoefer (2021) find that Finnish board-level representation increases composition-adjusted wages by about 1.6% on average (with a 95% confidence interval from -0.2% to 3.4%). They find some suggestive evidence of pay compression, with small wage gains concentrated among the bottom deciles of workers, and find no evidence of increased rent-sharing with workers. A recent paper by Blandhol, Mogstad, Nilsson, and Vestad (2020) studies the Norwegian context, and finds small or zero wage effects from board-level representation using event study designs and regression discontinuity approaches.

Shop-floor representation appears to have similarly limited wage effects. Keskinen (2017) and Harju, Jäger, and Schoefer (2021) study a 2008 Finnish reform that extended shop-floor representation requirements to firms with between 20 and 30 employees, and find no evidence that the reform affected average wages in those firms. This is broadly consistent with an older literature on German shop-floor representation, which has produced mixed results, with some studies finding no effect on wages (Fitzroy and Kraft, 1985) and some studies finding positive group-specific time-varying shocks confounding the variation induced by the reform.

effects (Addison, Schnabel, and Wagner, 2001; see Addison, Teixeira, and Zwick, 2010, for a full overview of this literature).

An interesting open question is whether quasi-parity and parity board-level representation requirements, which apply to very large German firms and mandate that 50% of board seats be allocated to workers, have more noticeable effects on wages.<sup>6</sup> Kim, Maug, and Schneider (2019) find that, compared to firms with 33% board-level representation, workers in firms subject to quasi-parity representation earn on average 3.3% *lower* wages but are more insulated from layoffs during economic downturns. The authors interpret this as an insurance arrangement where workers in quasi-parity firms agree to earn slightly lower wages in exchange for greater job security. Meanwhile, Svejnar (1981) studies the 1951 introduction of parity codetermination in the German iron, steel, and mining sectors. Compared to other German sectors, he finds wage increases of 5-6% in the iron and steel sectors and no wage effect in the mining sector. Overall, we judge that the evidence is still inconclusive on the wage effects of quasi-parity or parity codetermination, but existing studies leave open the possibility that these institutions have larger effects than minority board-level or shop-floor representation.

**Separations and Job Security** Codetermination could plausibly reduce both voluntary and involuntary separations. On the voluntary side, a key prediction of the "exit-voice" framework developed by Hirschman (1970) is that giving dissatisfied workers an avenue to change their firm from within will reduce their probability of quitting. Additionally, if codetermination increases job quality, then we should observe fewer workers voluntarily leaving codetermined firms. On the involuntary side, worker representatives will plausibly strive to protect incumbent workers by advocating against layoffs.

The existing literature suggests small, if any, effects of codetermination on the rate of voluntary separations. Jäger, Schoefer, and Heining (2021) find no effect of German minority board-level representation on the overall rate of separations, and Harju, Jäger, and Schoefer (2021) find no effect of Finnish minority board-level representation on employment-to-employment separations (which they use as a proxy for voluntary separations). Addison, Schnabel, and Wagner (2001) find, by contrast, that German firms with shop-floor representation have lower rates of voluntary quits.

There is stronger evidence that codetermination reduces the frequency of involuntary separations. Harju, Jäger, and Schoefer (2021) find that Finnish board-level representation causes a 2 percentage point decline in the frequency of separations into nonemployment, which they use as a proxy for involuntary layoffs. In addition, as previously mentioned, Kim, Maug, and Schneider (2019) find that workers in German firms with quasi-parity board-level representation are protected from layoffs in exchange for earning lower wages. Addison,

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<sup>6</sup>Under quasi-parity codetermination, shareholders are given a casting vote, and under parity codetermination they are not.

Schnabel, and Wagner (2001) find that German shop-floor representation is associated with lower rates of dismissals as well as voluntary quits.

One reason why codetermination might reduce involuntary layoffs is that shared governance could enable firms to adjust wages more flexibly in response to crises. In recessions, firms can stabilize employment by lowering wages. However, in reality, firms tend to exhibit downward wage rigidity (an unwillingness to lower wages), so economic downturns cause layoffs and create unemployment (for a review, see Bewley, 2002). There is some evidence that codetermined firms exhibit less downward wage rigidity than non-codetermined firms, and are therefore better able to retain their employees during recessions. For example, Burdín and Dean (2009) show that Uruguayan worker-managed firms are more likely than traditional capitalist firms to adjust wages downwards in the face of macroeconomic shocks, and as a result are less likely to reduce employment; Gregoric and Rapp (2019) find similar results for Nordic firms with board-level worker representatives. Consistent with the "insurance arrangement" results of Kim, Maug, and Schneider (2019), Rehder (2003) provides evidence on employment pacts negotiated between German firms and shop-floor representatives that preserve employment during crises in exchange for reductions in pay or hours. Codetermined firms may be more able to adjust wages downwards because a seat at the table allows workers to verify that the economic conditions genuinely necessitate wage cuts, thereby overcoming concerns that firms may opportunistically restructure or reduce wages.

If codetermination increases the job security of incumbent workers and causes them to become more entrenched, this might reduce the hiring rate of codetermined firms, in a standard insider-outsider mechanism (Lindbeck and Snower, 1989).<sup>7</sup> Indeed, there is evidence that shop-floor representation causes Finnish and German firms to reduce their hiring rates (Keskinen, 2017; Gralla and Kraft, 2018). However, there is also evidence in the opposite direction; Jirjahn (2010) finds that German shop-floor representation is associated with *higher* employment growth, and Burdín and Dean (2012) show that worker-managed firms in Uruguay do tend to prioritize employment growth, rather than merely maximising the utility of incumbent workers (as many models of codetermination, perhaps uncharitably, assume).

**Health, Subjective Wellbeing, and Other Difficult-to-Observe Outcomes** The empirical literature obviously focuses on studying outcomes that are observable in commonly available datasets, like wages or separations. However, proponents of codetermination often argue

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<sup>7</sup>For example, Jensen and Meckling (1979, p.484) argue that labor-managed firms fail to make socially beneficial investments because "New employees acquire the same claims on cash flows as those already employed by the firm. This means that whenever investment is accompanied by additional labor old workers have to share with new workers the anticipated cash flows from past investments. Thus, the expectation that a new project will generate cash flows which have a positive present value to the *firm* will not be sufficient to induce the current workers to undertake that project."

that shared governance is beneficial because it helps prevent abuse and mistreatment of workers and improves workplace health and safety (Anderson, 2017). These outcomes are difficult to measure and observe in standard data, but researchers can proxy for them using data on sickness leaves and subjective job quality. Moreover, voluntary separations, which we discussed above, are considered a standard revealed-preference catch-all measure of the relative attractiveness of an employer.

Harju, Jäger, and Schoefer (2021) find that both Finnish board-level and shop-floor representation do not affect the frequency of sickness leaves, suggesting that codetermination does not noticeably improve worker health and safety. However, they do find positive effects of both types of codetermination on subjective job quality, by merging their sample of firms with a unique Finnish job quality survey, suggesting that codetermination does improve aspects of job quality that may be hard to measure with wage or separations data.

There is also some evidence that codetermination improves the quality of workplace amenities. For example, Heywood and Jirjahn (2009) and Burdín and Pérotin (2019) find that the presence of shop-floor representation in a firm is associated with a greater availability of family-friendly practices like flexible working arrangements, parental leave, and childcare provision.

Overall, there is some evidence that codetermination improves difficult-to-measure aspects of job quality, but the literature in this area is sparse. More evidence is necessary before we can confidently conclude that codetermination improves non-pecuniary aspects of job quality, and to what extent.

**Conclusion** The existing microeconomic evidence suggests that codetermination has zero or small positive effects on proxies for worker welfare. Codetermination has, at most, small positive impacts on wages; it does not reduce the rate of voluntary separations or improve worker health; it does seem to slightly reduce the rate of involuntary dismissals, though this may be accompanied by a reduction in hiring; and some suggestive and limited evidence may point towards increases in subjective job quality.

## 4 Firm Performance: Do Shareholders Suffer (or Benefit) from Codetermination?

We now discuss evidence on the effects of codetermination on firm-level outcomes, like firm survival, productivity, revenue, and investment. This evidence is of interest for two reasons. First, empirical evidence can help us adjudicate between the conflicting theoretical predictions about codetermination's impacts on firm performance. Some of the theoretical arguments discussed in Section 2 emphasized the purported *negative* effects of the institution

on firm performance via hold-up problems or distortion of incentives, while other arguments suggested that codetermination can *improve* firm performance by improving information flows and increasing workers' effort and investment in human capital. Second, in addition to affecting shareholders, codetermination's impacts on firm performance affect workers and consumers (by modifying the size of the pie that is divided between workers and shareholders, and by affecting prices and the broader economy).

Overall, existing microeconomic estimates indicate that codetermination has no effects or slight positive effects on a variety of measures of firm performance. In addition, firms do not try to evade codetermination laws by adjusting their size, providing revealed-preference evidence that codetermination does not substantially harm firm performance. Finally, survey evidence shows that managers and executive directors tend to have neutral or positive views of codetermination.

## 4.1 Impacts on Firm-Level Outcomes

**Microeconomic Evidence** Jäger, Schoefer, and Heining (2021) and Harju, Jäger, and Schoefer (2021) use reform-based difference-in-differences strategies to study the effects of German and Finnish minority board-level representation on firm performance, and find zero or small positive effects. Specifically, Jäger, Schoefer, and Heining (2021) find a slight increase in productivity, slightly larger capital stocks, and no reductions in revenue as a result of minority board-level representation. Harju, Jäger, and Schoefer (2021) study a more comprehensive set of outcomes, and find slight positive effects on firm survival, labor productivity, and capital intensity, and no effects on profitability.

These findings are consistent with the older, mostly correlational, literature on German board-level representation, which produces mixed estimates that are generally close to zero. For example, of the 30 studies of board-level representation surveyed by Conchon (2011), 10 find a positive effect on firm performance, 11 find no effect, and 9 find a negative effect.<sup>8</sup>

Shop-floor representation has similarly small effects on firm performance. Keskinen (2017) and Harju, Jäger, and Schoefer (2021) find no effects on firm performance of Finnish shop-floor representation. Older literature on German shop-floor representation finds mixed results, with some studies suggesting positive effects on firm performance, others suggesting no effects or mixed effects, and others suggesting negative effects; see Addison, Schnabel, and Wagner (2004) and Addison (2009) for reviews of this literature.

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<sup>8</sup>A few of these studies have used event study or regression discontinuity designs to analyze effects, e.g., on stock market valuations. For example, Gorton and Schmid (2004) document a negative effect on stock market valuation comparing firms above and below the 2,000 employee cutoff, i.e. comparing firms with one-third vs. quasi-parity representation. By contrast, Baums and Frick (1998) find no effect on stock market valuation using an event study methodology of court decisions regarding codetermination in individual firms (similarly, Bermig and Frick, 2010, find no effect on firm performance or valuation controlling for firm fixed effects).

**Revealed-Preference Evidence** In many settings, codetermination requirements can be avoided by firms determined to do so. First, codetermination laws typically apply only to firms above a certain size threshold, meaning that firms slightly above the threshold can evade the law by reducing their size or reporting their employment count in a way that puts them just below the threshold. Second, as we discussed in Section 2, many codetermination laws give workers a *right* to codetermination that they can voluntarily take up. Standard Coasean bargaining frameworks predict that if codetermination worsens firm performance and thereby reduces overall surplus, employers will bargain or pressure workers into not taking up their right to codetermine.

Despite firms' ability to avoid codetermination by, e.g., outsourcing employment or shifting their organizational structure, systematic studies have found no empirical evidence that firms manipulate their size in order to avoid codetermination requirements. For example, Lin, Schmid, and Xuan (2018), Kim, Maug, and Schneider (2019), Redeker (2019), Blandhol, Mogstad, Nilsson, and Vestad (2020), Jäger, Schoefer, and Heining (2021), and Harju, Jäger, and Schoefer (2021) plot the distributions of firm sizes in their samples, covering firms in Germany, Norway, and Finland, and find no evidence that firms bunch just below the relevant size thresholds.<sup>9</sup> Given evidence that firm size does respond to other size-dependent policies that impose costs (see, e.g., Garicano, Lelarge, and Van Reenen, 2016), these facts provide revealed-preference evidence that codetermination does not substantially harm firm performance, at least among firms within a narrow range around the size thresholds.

The evidence on take-up of codetermination in contexts where it is a right (rather than a mandate) is less conclusive. As we mentioned in Section 2, usually workers in about 50% of firms subject to codetermination rights end up taking up their statutory rights (Addison, Schnabel, and Wagner, 1997; Gregoric and Rapp, 2019; Harju, Jäger, and Schoefer, 2021). The Coasean bargaining framework suggests that, at least in those firms, codetermination does not substantially harm firm performance. These 50% of covered firms account for a disproportionate share of total employment, since, among firms large enough to be subject to codetermination rights, firm size is positively correlated with take-up of codetermination. For example, Addison, Schnabel, and Wagner (1997) find that although only 20% of affected German firms take up shop-floor representation, those firms account for 75% of employment among affected firms.

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<sup>9</sup>We note a tension between the absence of bunching at the 2,000 employee policy threshold in Germany, the threshold for quasi-parity rather than one-third board-level codetermination, documented in several papers (Lin, Schmid, and Xuan, 2018; Kim, Maug, and Schneider, 2019; Redeker, 2019) and the findings in Gorton and Schmid (2004), who documented sharp declines in stock market valuation comparing firms above and below the policy threshold (but had not implemented a McCrary, 2008, test). That is, if quasi-parity codetermination indeed had sharp negative effects on profitability or stock market valuations, then one would expect significant bunching at the 2,000 employee threshold following the results by Garicano, Lelarge, and Van Reenen (2016). In turn, if there were indeed no bunching at the 2,000 employee threshold, then it would be surprising to see sharp declines in stock market valuations for firms above the threshold.

However, this still leaves about 50% of firms subject to codetermination rights whose workers do not take up their right to codetermination. As reported in Harju, Jäger, and Schoefer (2021), when Finnish workers in above-threshold firms are asked why they have not exercised their statutory right, the most common response is that the employer opposed it. Since Harju, Jäger, and Schoefer (2021) find little evidence of increases in worker compensation in above-threshold firms, it does not seem to be the case that employers bargained workers into not taking up codetermination in exchange for additional compensation. Rather, it seems that many employers possess enough power to block their workers' take-up of their statutory rights, and many choose to exercise this power, perhaps because they are concerned about effects on their firm's performance. Alternatively, perhaps workers do not value the institution enough to demand it (or withstand their employer's opposition). In particular, workers might be reluctant to insist on implementing an institution that they (correctly or incorrectly) fear would threaten their job security (c.f. Friedman, 1951).

There is also some evidence that firms attempt to minimize their exposure to codetermination through mechanisms other than manipulating their size or blocking the institution entirely. For example, Strine Jr., Kovvali, and Williams (2021) describe how German firms sometimes use "the selection of additional vice-chairs" and "equity-controlled subcommittees" in order to "put formal and informal power in the hands of stockholder representatives on the supervisory board [and minimize the power of employee representatives]." Thus, there is *some* suggestive evidence that firms are sometimes concerned about the effects of codetermination and take steps to block worker representation or reduce the power of worker representatives.

**Surveys of Managers' and Directors' Attitudes** If codetermination worsens firm performance, we should expect managers and directors to have negative attitudes towards the institution. However, when surveyed, managers and directors instead mostly express neutral or positive attitudes.

For example, Levinson (2000) reports that 76% of Swedish Corporate Directors hold "positive" or "very positive" views of codetermination, with 19% having a neutral view and only 5% having a negative view. Slovenian managers hold moderately positive views of codetermination, giving it an average score of 3.43 on a 5-point favorability scale (Franca and Pahor, 2014). In the Netherlands, about 80% of managers think shop-floor representation has a neutral impact on efficiency and innovation, with 5-10% estimating a positive effect and 10-15% estimating a negative effect (Van Den Berg, Grift, and Witteloostuijn, 2000). Paster (2012) reports that 71% of German executives oppose the abolition of mandatory board-level representation.

Perceptions of quasi-parity board-level representation are more negative than perceptions of minority board-level representation. Paster (2012) notes that while German firms with one-third board-level representation tend to see codetermination as an advantage (57%) rather

than a disadvantage (19%), more firms subject to quasi-parity board-level representation see it as a disadvantage (38%) than an advantage (34%). This is consistent with anecdotal reports that directors in German industries subject to quasi-parity or parity codetermination dislike the institution (Stettes, 2007).

The positive perceptions of codetermination among individual managers stand in contrast to employer associations' sharp opposition to expansions of codetermination rights in some cases, e.g., in Germany in the 1970s (Höpner, 2004). On the one hand, it is possible that practical experience with the institution may have softened managers' perceptions. Alternatively, the presence of codetermination may have led to a selection of managers less opposed to the institution, or managers might report positive views of codetermination out of social desirability bias (despite the anonymized nature of the surveys). Finally, employer associations may strategically overstate the negative impacts of codetermination (Paster, 2012; Müller and Stegmaier, 2020).

Overall, there is little evidence that managers or directors generally dislike codetermination, except for quasi-parity (and, in very few firms, parity) board-level representation that exists in Germany.

**Conclusion** Overall, the existing literature suggests that codetermination is a benign or at least not substantially harmful institution from the perspective of firm performance. There is no evidence for reductions in investment, profitability, or productivity at the firm level, and there is revealed-preference and survey evidence that would be hard to square with managers and directors strongly objecting to the institution.

The fact that codetermination does not appear to worsen firm performance is less surprising given the evidence in Section 3 that the institution does not increase the share of rents going to workers. As we described in Section 2, one of the major mechanisms through which codetermination is hypothesized to worsen firm performance is that workers' ability to capture increases in surplus for themselves deters capital investment. The fact that codetermination does not increase rent-sharing constitutes direct evidence against the existence of this hold-up mechanism, and therefore reduces the probability that codetermination harms firm performance. That said, there are independent mechanisms through which codetermination could worsen firm performance—for example, by disincentivizing expansions or leading to slow or gridlocked decision-making (Jensen and Meckling, 1979; Hayden and Bodie, 2021)—and the evidence summarized in this section allows us to additionally rule out those mechanisms.

## 5 General-Equilibrium Effects of Codetermination Laws

In this section, we contribute novel evidence on the general-equilibrium, country-level effects of codetermination, complementing our review of the partial-equilibrium firm-level evidence.

### 5.1 Motivation

The microeconomic studies surveyed in the previous two sections produce estimates of the partial-equilibrium effects of introducing codetermination in individual firms. However, codetermination laws might also have—or even primarily work through—general-equilibrium effects that cannot be picked up by comparisons of firms on either side of the margins of coverage of codetermination laws. There are various plausible channels through which this could occur, and we now review two of them.

**Competition** Competitive pressures from non-codetermined firms in product, factor, or financial markets might render an individual codetermined firm unable to significantly revise its practices. By contrast, a broad-based imposition of codetermination on the major players in a market could alter the market's competitive equilibrium. For example, an individual codetermined firm might be unwilling to significantly boost its wages, for fear that its share price will crash as investors flee to its non-codetermined competitors (Hayden and Bodie, 2021). A nation-wide codetermination mandate could assuage fears about capital flight and thereby enable large codetermined firms to change their practices, which might in turn create competitive pressures that shift the behavior of small firms not covered by the codetermination mandate. Levine and Tyson (1990) describe at length how an economy might have multiple potential competitive equilibria, and how a codetermination mandate could trigger a shift to an equilibrium that provides better outcomes for workers.

**Industrial Relations** Codetermination laws might affect the overall nature and quality of a country's industrial relations, and hence influence economic outcomes. For example, formal institutionalization of worker-management cooperation in large firms could have cultural spillover effects that create economy-wide norms of worker participation. If these economy-wide norms increase worker participation in all firms regardless of the presence of formal codetermination, partial-equilibrium estimates comparing firms subject versus not subject to formal codetermination requirements will misleadingly suggest that codetermination does not affect firm behavior, but in fact the codetermination law will have shifted decision-making in all firms. Similarly, by giving workers the ability to participate collaboratively in decision-making, codetermination laws might make industrial relations friendlier overall, and reduce adversarial conflict between employers and workers. Indeed, scholars of industrial relations

have credited the institutions of codetermination in Germany with creating a more cooperative industrial relations climate and enabling peaceful adjustment processes to economic shocks and transitions (Thelen, 1991).

## 5.2 General Equilibrium Effects of Codetermination

To our knowledge, there is no existing empirical evidence on the general-equilibrium impacts of codetermination laws, with the exception of a few studies examining the institution's industry-level effects (Svejnar, 1981; Benelli, Loderer, and Lys, 1987) or analyzing its impact in cross-sectional regressions (Hörisch, 2012). The existing literature's focus on firm-level variation is unsurprising given the advantages of using firm-level microdata, including large sample sizes and the ability to cleanly identify the causal effects of codetermination laws by comparing firms on either side of the eligibility thresholds of codetermination requirements. However, evidence on codetermination's general-equilibrium effects is vital to a full picture of the institution's economic impacts. We therefore contribute novel estimates of the country-level effects of codetermination laws, using a cross-country event study design exploiting major historical codetermination reforms.

**Sample and Codetermination Reforms** In our main sample, we restrict our attention to European countries that were independent and democratic between 1960 and 2019 and for whom we have non-missing data for our main outcome variables. These countries are: Austria, Belgium, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. For right-hand side variation in codetermination, we exploit ten introductions or expansions of codetermination occurring in our sample countries between 1960 and 2019: introductions of minority board-level representation in Austria, Denmark, Finland, France, and Norway; a switch from one-third to quasi-parity board-level representation in large firms in Germany; and expansions of shop-floor representation rights in Finland, the Netherlands, Norway, and Sweden (for Finland and Norway, we study both the board-level and shop-floor reforms as separate reforms). Notably, this list excludes some codetermination "reforms" happening in our sample countries between 1960 and 2019 (e.g., in Italy) that we do not consider to be substantive introductions or extensions of codetermination. A full explanation of how we selected our sample and list of reforms, and a table describing the details and institutional contexts of each reform, are available in Appendix Section B.1.

We use our main sample in all of our analyses except for the ones involving our "income inequality" outcome variable. Data on income inequality is not available back to 1960 for several of the countries in our main sample (Austria, Belgium, Finland, Iceland, Ireland, and Italy). We therefore restrict to the other countries in our main sample, study only the

seven reforms occurring in those remaining countries, and add three additional comparison countries (Australia, Canada, and the United States) to compensate for the loss of Belgium, Iceland, Ireland, and Italy.

**Data and Outcome Variables** We study two classes of outcomes: aggregate economic outcomes and industrial relations outcomes.

Our economic outcome variables are drawn from the European Commission's AMECO Database and the World Inequality Database, and cover wage growth, the labor share, productivity growth, net capital formation, growth in GDP per capita, and the share of national income held by the bottom 90% (which we use as the most extensively available measure of income inequality).<sup>10</sup>

In addition, we draw on three supplementary data sources to examine the effects of codetermination laws on industrial relations. First, the International Labour Organization (ILO) has collected annual data on strike activity at the country level since the 1920s.<sup>11</sup> If codetermination laws improve the quality and cooperativeness of industrial relations, we can naturally expect them to reduce the intensity of destructive industrial conflict such as strikes.<sup>12</sup> We therefore draw on the ILO data, and calculate a measure of strike intensity which represents the number of days lost to strikes in a country in a given year, normalized by the size of the country's working-age population in that year. Since the cardinal values of this variable are extremely volatile, we convert to an ordinal variable that ranges between 0 and 1 and ranks a country's strike intensity in year  $k$  compared to its strike intensity in all of the other years covered by the ILO data. A value of 1 represents the country's highest-ranked year in terms of strike intensity. Our estimates effectively check whether the years following a country's introduction of a codetermination law are ranked lower on average than the pre-reform years in terms of strike intensity.

Strike intensity is admittedly an imperfect proxy for the quality of industrial relations. An increase in strikes may reflect an increase in the robustness of worker organization and worker-management dialogue rather than a breakdown in the "quality" of industrial relations, and conversely a decrease in strikes may reflect suppression of worker organization rather than increased cooperation between workers and employers. In addition, strikes are an extreme event and a very coarse-grained measure of industrial relations; the quality of industrial relations may differ greatly even conditional on a given strike intensity level. We therefore

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<sup>10</sup>We choose to specify our wage/TFP/GDP variables in terms of growth rates rather than levels because specifications using levels are heavily affected by the accumulating effects of the sharp drop in growth rates between 2 years pre-reform and 1 year pre-reform that are observed for each of these outcome variables in Figure 6. Thanks to Ewan McGaughey for suggesting income inequality as an outcome variable.

<sup>11</sup>Thanks to Sjaak van der Velden for sharing compiled and digitized versions of these data with us, and see van der Velden (2018) for a discussion of the strengths and weaknesses of the ILO data.

<sup>12</sup>Indeed, the codetermination provisions established in Sweden's Saltsjöbaden Agreement, described in Section 2.3, were intended to directly replace adversarial industrial action.

supplement our event study analysis of strikes with cross-sectional analysis of a variable that better represents the quality of a country's industrial relations, but for which we lack historical data: a survey item from the World Economic Forum's Executive Opinion Survey that asks managers in different countries to evaluate the "cooperativeness" of labor relations in their country on a 1-7 point scale. The results from this cross-sectional analysis are similar to the results from our event study analysis of strikes, as we describe below.

Finally, we examine the interaction between the two pillars of European industrial relations (union representation and codetermination) by checking whether codetermination reforms affect a country's subsequent union density. To study this, we draw on historical data on union density at the country-year level from the OECD/IAIS ICTWSS database, compiled by Visser (2021). This analysis is motivated by the observation that, as we show in Section 6.3, countries with codetermination laws experienced a much less rapid decline in union density during the 1980s and 1990s compared to countries without codetermination. If this fact is attributable to a causal effect of codetermination on union density, that would be evidence for another channel through which codetermination laws could have general-equilibrium economic effects.

A full description of our data sources and outcome variable definitions is available in Appendix Sections B.2-B.5.

**Methodology** For each country-reform event in our sample, we construct a synthetic control unit out of the other countries in our sample that do not experience a codetermination reform at any point within a 10-year radius around the reform of interest. Synthetic control weights are calculated by matching on pre-reform macroeconomic characteristics (GDP growth, wage growth, TFP growth, the labor share, and net capital formation). Thus, for example, for the 1978 Finnish reform, we construct a "synthetic Finland" consisting of a weighted average of the other countries in our sample that do not experience a codetermination reform between 1968 and 1988, with the weights optimized to make synthetic Finland maximally resemble actual Finland in terms of 1968-1977 macroeconomic outcomes. A full description of the synthetic control procedure is available in Appendix Section B.7.1.

We then pool together all of our country-reforms and synthetic control units, aligned by relative event time, and run difference-in-differences regressions comparing the outcomes of the treated and control groups before and after the codetermination reforms. The regression equation is as follows:

$$y_{ik} = \alpha_i + \beta_k + \theta_{\text{Year}(ik)} + \sum_{s=-10, s \neq -1}^{10} \tau_s^{\text{Treated}} \times \mathbb{1}[k = s] \times \text{Treated}_i + \varepsilon_{ik}, \quad (1)$$

where  $y_{ik}$  denotes the outcome for country-reform  $i$  in year  $k = t - \text{reformyear}_i$  relative to the reform occurring in  $\text{reformyear}_i$  (the reform year for synthetic control units is set equal to

their respective country-reform's reform year). The  $\alpha_i$  are unit (country-reform) fixed effects,  $\beta_k$  are event time fixed effects,  $\theta_{\text{Year}(ik)}$  are calendar year fixed effects (e.g. "1975"),  $\mathbb{1}[k = s]$  is an indicator for being in the  $s$ th year relative to the reform, and  $\text{Treated}_i$  is an indicator for unit  $i$  being a treated country-reform. The  $\tau_s^{\text{Treated}}$  are the coefficients of interest, and represent effects relative to the omitted  $s = -1$ . Standard errors are clustered at the country-reform level.

In addition to plotting the full set of dynamic coefficients  $\tau_s^{\text{Treated}}$ , we also report average pre-reform and post-reform coefficients, which are  $\tau_{\text{Pre}}^{\text{Treated}}$  and  $\tau_{\text{Post}}^{\text{Treated}}$  in the following regression:

$$y_{ik} = \alpha_i + \beta_k + \theta_{\text{Year}(ik)} + \tau_{\text{Pre}}^{\text{Treated}} \times (\mathbb{1}[k < -1] \times \text{Treated}_i) + \tau_{\text{Post}}^{\text{Treated}} \times (\mathbb{1}[k \geq 0] \times \text{Treated}_i) + \varepsilon_{ik}, \quad (2)$$

where notation is the same as above. Due to the small number of clusters, we report p-values for the pre-reform and post-reform coefficients calculated using the wild bootstrap method, following Cameron, Gelbach, and Miller (2008) and using the code developed by Roodman, Nielsen, MacKinnon, and Webb (2019). In addition, we report average post-reform coefficients calculated using the imputation methodology developed by Borusyak, Jaravel, and Spiess (2021), which deals with problems arising when two-way fixed effects models are used in contexts with heterogeneous treatment times, like the context we consider. For an explanation of the Borusyak, Jaravel, and Spiess (2021) methodology, see our full methodological details in Appendix Section B.7.

**Results** Event study results for our economic outcome variables are plotted in Panels (a)-(e) of Figure 6. In addition, Appendix Figure A.1 presents a raw time series plot comparing aggregate outcomes in our treated group to aggregate outcomes in our synthetic control group; Appendix Figure A.2 checks the quality of our synthetic control fits by matching only on a training period ( $k = -10$  to  $k = -6$ ) and plotting the quality of the resulting fit in an evaluation period ( $k = -5$  to  $k = -1$ ); and Appendix Figures A.3-A.12 present time series plots comparing each country-reform to its synthetic control unit.

The results in Figure 6 suggest that codetermination reforms have no noticeable effects on aggregate economic outcomes. Estimates are slightly imprecise, and pre-trends are at times unstable, but the post-reform coefficients for all of the outcome variables cluster around zero and none are statistically significant.

Event study results for our strike intensity and union density outcome variables are plotted in Panels (a) and (b) of Figure 7. The estimates suggest no effect of codetermination reforms on strike intensity or union density, at least for the first 6 years following the reforms. Both plots show suggestive evidence of positive treatment effects beginning 7-8 years post-reform, which could be attributed to a delayed effect of the codetermination reforms on industrial relations.

In the absence of a story for how causal effects of the reforms could take 7-8 years to materialize, these positive coefficients could instead be attributed to global changes in industrial relations during the 1980s that differentially affected codetermined and non-codetermined countries (most of the reforms we study occur in the mid-to-late 1970s, so 7 years post-reform is the mid-1980s).

In any case, the estimates certainly do not suggest that strike intensity *declines* following codetermination reforms, so we fail to find evidence that codetermination laws improve the quality of industrial relations in that dimension. The positive longer-run coefficients for union density leave room for causal general-equilibrium effects on unionization, and are consistent with at least some of the slower decline in unionization in countries with codetermination laws (which we describe in Section 6.3) being causally attributable to the presence of codetermination.

As we have mentioned, strike intensity is not a pure measure of the quality of industrial relations, so we supplement our event study analysis of strikes with cross-sectional analysis of a survey item from the World Economic Forum's Executive Opinion Survey. The survey item asks executives in different countries to assess their country's labor-management relations on a scale from 1 ("Generally confrontational") to 7 ("Generally cooperative"). We have access to country-level average responses to this question from 2007 onwards; Appendix Figure A.16 presents a global map of average responses.

The poster children for codetermination—Germany, Austria, and the Nordic countries—indeed have some of the most cooperative labor relations in the world according to the Executive Opinion Survey metric, ranking in the 94th percentile on average (for comparison, the United States is in the 79th percentile). But a broader analysis, including countries like France with codetermination laws but much less cooperative industrial relations (18th percentile), fails to find a strong relationship between codetermination laws and cooperation in industrial relations. Panel (c) of Figure 7 presents a binned scatterplot showing the correlation between the strength of a country's codetermination laws (as measured by the CBR Labor Regulation Index; Adams, Bishop, and Deakin, 2016) and the cooperativeness of the country's labor relations (as measured by the Executive Opinion Survey item). The results show a weak and statistically insignificant positive correlation between the strength of codetermination laws and the cooperativeness of labor relations (correlation coefficient 0.115, p-value 0.236).

We therefore once again do not find strong evidence for the hypothesis that codetermination laws improve the quality of industrial relations. That said, the Executive Opinion Survey measure of industrial relations has its own weaknesses, primarily the difficulty of comparing responses to ordinal survey scales across countries. In addition, this is a purely cross-sectional descriptive design that inherently cannot capture the causal effect of codetermination on industrial relations due to potential omitted variable bias.

**Conclusion and Caveats** Overall, we find no evidence that codetermination reforms affect aggregate economic outcomes or influence overall industrial relations, with the exception of slight evidence for increases in union density. Three important caveats apply to the results from our cross-country event studies. First, as we have noted, the estimates from these event studies are somewhat imprecise. Second, there are inherent limits on the credibility of cross-country difference-in-differences analyses, since sample sizes are small and the introduction of national legislation may be correlated with unobserved country-specific trends. Third, most of the reforms we study are not wholesale introductions of codetermination in contexts where no codetermination existed beforehand; rather, most reforms we study are codifications and expansions of codetermination rights in countries where a patchwork of informal or negotiated codetermination arrangements already existed (see Appendix Table A.1 for a full description of the details and contexts of the reforms). Hence, our results may not speak to the impact of introducing codetermination in contexts like the United States, where no codetermination arrangements exist at all because of the National Labor Relations Act; we return to this point in Section 7. That said, we do study a pair of reforms that approximate wholesale introductions of codetermination (the 1978 Finnish reform and 1979 Dutch reform), and the impacts of these reforms do not appear to be larger than the impacts of the other reforms we study (see Appendix Figures A.5 and A.9).

Despite these caveats, the almost complete absence of pre-existing evidence on the general-equilibrium impacts of codetermination means we think these estimates constitute a valuable contribution to our knowledge about the economic effects of codetermination. While these results are not definitive proof that codetermination laws lack general-equilibrium impacts, they are reasonably strong evidence against the proposition that the general-equilibrium effects of codetermination substantially exceed its small or nonexistent partial-equilibrium effects.

## 6 What Explains Codetermination's Limited Effects?

The empirical evidence we summarized and presented in Sections 3-5 suggests that codetermination has, on net, no effects, or slight positive effects, on a variety of worker and firm outcomes, including wages, job security, overall job satisfaction, productivity, and investment. These mild or nonexistent effects stand in contrast to the predictions of both proponents and opponents of codetermination, who argue either that codetermination will (beneficially) shift power towards workers and significantly increase rent-sharing, cooperation, and productivity, or that codetermination will (harmfully) cripple firm performance and cause investment to decline. In the remainder of this paper, we draw on qualitative evidence to build a comprehensive description of how codetermination operates in practice, a description which we suggest

might help explain the institution's limited effects.

## 6.1 Does Codetermination Boost Labor's Power?

The first explanation we propose for codetermination's limited impact is that existing codetermination laws do not significantly boost workers' influence. This is firstly because, as we describe in this section, codetermination laws appear to convey relatively little authority to workers; and secondly, as we describe in Section 6.2, because European workers seem to have high baseline levels of influence independently of formal codetermination.

### 6.1.1 Formal Authority

As we described in Section 2.2, codetermination laws rarely give workers much formal authority.<sup>13</sup> Board-level representation laws outside of Germany grant workers a minority of seats on the board, meaning workers can always be overruled by unanimous shareholders. And shop-floor worker representatives are usually limited to information and consultation rights, possibly with very restricted rights to judicial redress; only in a few countries (e.g., Austria and Germany) are they given any co-decision-making powers, and these powers are typically narrowly defined (Aumayr, Stavroula, Foden, Scepanovics, and Wolf, 2011).

These limited formal rights *could* conceivably boost workers' real authority. At the board level, interview evidence suggests that boards with worker representatives strive to reach unanimous decisions, meaning that shareholder representatives try to secure the consent of worker representatives. In addition, worker representatives sometimes hold the decisive votes when shareholder representatives disagree amongst themselves (Gold, Kluge, and Conchon, 2010). At the shop-floor level, as we have mentioned, information and consultation procedures might put pressure on employers to reach a consensus with workers. Understanding whether codetermination laws convey real power therefore requires investigating whether these limited formal rights translate into more substantial real authority.

### 6.1.2 Real Authority

To ascertain whether codetermination increases workers' real authority, we draw on several sources of qualitative evidence: detailed case studies of codetermination in Swedish firms by Sandberg et al. (1992) and Wheeler (2002); interviews of European worker representatives conducted by Gold, Kluge, and Conchon (2010); and surveys of Swedish and Finnish worker representatives and managers discussed by Levinson (2000) and Harju, Jäger, and Schoefer

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<sup>13</sup>Following Aghion and Tirole (1997), we distinguish between formal authority (the right to decide) and real authority (the effective control over decisions).

(2021), respectively. Our synthesis of this qualitative evidence supports four conclusions about the real authority conveyed by codetermination laws.

**Moderate Influence on Working Conditions** First, codetermination grants workers a non-trivial amount of control over decisions about working conditions.

Workers and managers unanimously agree that worker representatives wield more influence over decisions about working conditions than over any other kind of decision (Levinson, 2000; Harju, Jäger, and Schoefer, 2021). Decisions about working conditions are where worker representatives focus most of their attention: for example, Swedish worker representatives interviewed by Wheeler (2002) report spending the majority of their time participating in decisions about working hours, health and safety, and workplace amenities, as well as using their position to help solve problems of sexual harassment and mistreatment of workers by managers.

Exactly how much influence worker representatives wield in this area is ambiguous. Finnish worker representatives are relatively pessimistic, with only 30% claiming an ability to improve working conditions at their company (Harju, Jäger, and Schoefer, 2021). Danish worker representatives are more optimistic, with 55% claiming a "reasonable" or "high" amount of influence in general (Rose, 2008).<sup>14</sup>

Managers also present mixed assessments of worker representatives' influence over working conditions. In the 2019 European Company Survey, just under 50% of managers claim that worker representatives in their establishments wield a "moderate" or "great" amount of influence over decisions about working conditions (see Panel (b) of Figure 8, which we will discuss further in Section 6.2). Meanwhile, a striking 96% of Swedish Managing Directors say that worker representatives exert a "large" or "very large" degree of influence over decisions in this area (Levinson, 2000).

Overall, this mixed evidence suggests that worker representatives wield some amount of control over decisions about working conditions, with the exact extent of this control remaining unclear, especially given the difficulty of interpreting statements like "a great amount of influence."

Worker representatives' ability to improve working conditions might explain the positive impacts of codetermination on subjective job quality documented in Section 3, though the fact that codetermination does not reduce voluntary separations or improve health and safety suggests that worker representatives are not dramatically reshaping the workplace.

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<sup>14</sup>Danish representatives are asked about their influence *in general*, not their influence on decisions of working conditions specifically. Since working conditions appear to be the area where worker representatives are most influential, this number is likely a lower bound on the relevant percentage.

**Small Amount of Influence on Layoffs** Worker representatives state that their highest priority is protecting incumbent workers from layoffs (Harju, Jäger, and Schoefer, 2021). In accordance with information and consultation requirements, worker representatives describe being extensively consulted about planned layoffs, and a few worker representatives even report successfully negotiating the prevention of layoffs. However, worker representatives agree that managers usually override their objections to planned layoffs (Wheeler, 2002; Gold, Kluge, and Conchon, 2010). Managers in the 2019 European Company Survey mostly agree that worker representatives lack influence over layoff decisions (see Panel (a) of Appendix Figure A.13).

Anecdotal evidence suggests that worker representatives may be more able to influence layoff decisions during economic crises. For example, as we mentioned in Section 3, Rehder (2003) describes how German firms experiencing an economic downturn sometimes reach agreements with worker representatives to preserve workers' jobs in exchange for cuts to wages and hours.

Worker representatives' (limited) ability to prevent layoffs may explain the finding described in Section 3 that codetermination sometimes causes small reductions in involuntary separations.

**Small Amount of Influence on Wage Setting** Similarly to layoff decisions, worker representatives describe wielding only a small amount of influence over wage setting decisions. Just 15% of Finnish worker representatives believe they can affect wage levels at their workplace (Harju, Jäger, and Schoefer, 2021), and only 35% of managers in the 2019 European Company Survey claim that worker representatives wield a "moderate" or "great" amount of influence over payment schemes (see Panel (d) of Appendix Figure A.13). Worker representatives' general lack of involvement in wage setting decisions is unsurprising, given that wage negotiations in Europe are typically the domain of unions and sectoral bargaining, as we describe in Section 6.3. (An exception are works councils in Germany, which have codetermination rights over performance pay and can serve as a bargaining party in wage negotiations if an opening clause in the relevant collective bargaining agreement allows it; Addison, Schnabel, and Wagner, 2001). Overall, worker representatives' ability to exert some small amount of control over wage setting may explain the very small positive wage impacts of codetermination documented in Section 3.

**No Influence on Corporate Strategy** There is nearly unanimous agreement that worker representatives have no influence on broad strategic decisions, even when they sit on company boards. Fewer than 5% of Finnish worker representatives believe they can affect strategic decisions (Harju, Jäger, and Schoefer, 2021), and Swedish Managing Directors report that worker representatives are almost completely inactive during strategic discussions in board meetings (Levinson, 2000). The general perception among worker representatives is that

strategic decisions are made out of their view, and presented to them once management's mind is already made up (Wheeler, 2002; Gold, Kluge, and Conchon, 2010). While this survey evidence leaves room for equilibrium or anticipation effects of board-level representation on the decisions of managers, this self-professed near-complete lack of influence on strategic decisions highlights the lack of power conveyed by arrangements like minority board-level representation. Worker representatives' inability to influence strategic decisions in particular may help explain the absence of evidence for negative effects of codetermination on firm performance.

### **6.1.3 Conclusion**

The qualitative evidence paints a picture of an institution that gives workers some control over their immediate working conditions, but grants them negligible authority beyond that. Given this characterization, it is unsurprising that codetermination fails to significantly shift major outcomes like wages or investment. This "limited power" explanation also accounts for the fact that quasi-parity and parity codetermination arrangements (which convey greater formal authority to workers) provoke more opposition from managers and directors, and possibly have larger wage effects, as we described in Sections 3 and 4.

Importantly, recent codetermination proposals in the United States and United Kingdom mostly emulate existing codetermination laws, usually by proposing minority board-level representation. If limited power conveyed by minority board-level representation is key to explaining codetermination's lack of impact, then we should expect the codetermination proposals in the US and UK to have similarly negligible impacts if implemented. We return to this point in Section 7.

## **6.2 Quality of Industrial and Workforce-Management Relations**

The surveys of worker representatives we drew on in Section 6.1 included a minority of respondents who claimed to possess the power to influence important decisions. However, even if these worker representatives are right that they possess this power, this does not imply that codetermination boosts workers' impact on decision-making. First, it is possible that, in the absence of formal codetermination, workers would wield exactly the same power over decisions through informal participation mechanisms. Second, it is possible that worker representatives choose to use their influence in a way that mostly aligns with the desires of managers and directors, meaning that worker involvement does not substantially shift decision-making. As we describe in this section, the qualitative evidence supports both of these hypotheses. European countries appear to have a strong informal culture of worker involvement, worker-management cooperation, and alignment of priorities, which may leave

little room for formal codetermination to make a difference.

**Worker Involvement** Survey evidence indicates that European firms have a robust culture of informal worker participation and information sharing that operates even in firms without formal codetermination. If worker involvement in decision-making would happen regardless of the presence of formal codetermination, it is not surprising that codetermination does not have a noticeable impact.

The evidence for a robust culture of informal worker involvement comes from interviews of European managers conducted as part of the European Company Survey. As illustrated in Figure 8, across both the 2013 and 2019 waves of the survey, managers in establishments without formal worker representation report levels of worker involvement in decision-making comparable to the levels of worker involvement in firms with formal codetermination. For example, in the 2013 survey, just under 50% of managers in firms without formal worker representation reported that workers were directly involved in the firm's most important recent decision, compared to 60% of managers in firms with worker representation who said that worker representatives were involved in the most important recent decision (Panel (a) of Figure 8). In the 2019 survey, about 55% of managers in establishments without worker representation said that workers directly exerted a "moderate" or "great" amount of influence over decisions about working conditions, compared to 45% of managers in firms with worker representation who said the same thing about worker representatives (Panel (b) of Figure 8). Panel (c) of Figure 8 shows that larger European firms (which are more likely to be subject to codetermination requirements) do not involve workers in decision-making more frequently than smaller firms; rather, the nature of the worker involvement simply shifts, from informal direct involvement in smaller firms to formal representation in larger firms. Panel (d) of Figure 8 shows that, in simple cross-country regressions, the strength of a country's codetermination laws is uncorrelated with the percentage of firms in that country who report some kind of worker involvement in decision-making. Overall, this evidence paints a picture of widespread informal worker participation in decision-making even in firms without formal codetermination.

We caution that this survey evidence is far from conclusive. One obvious concern is that managers accustomed to formal codetermination might have higher standards for what constitutes worker involvement in a decision. In that case, although managers in firms with and without formal codetermination might report equal rates of "worker involvement," workers would in fact be much more involved in codetermined firms. In addition, the results presented above are purely descriptive, and although they *suggest* that codetermination laws do not increase the amount or intensity of worker involvement in decision-making, they are far from credible causal evidence for this hypothesis.

Despite these caveats, the results above provide at least suggestive evidence for an

explanation of the limited effects of codetermination: European countries have such a robust culture of worker participation that worker involvement in decision-making happens to an equal extent even in the absence of formal codetermination.

**Harmonious Labor-Management Relations** The relative warmth of labor-management relations in Europe may also help explain codetermination's limited impact. Most of the evidence cited in Sections 3-4 was from studies of German or Nordic codetermination; as we noted in Section 5, while there is no evidence that codetermination laws *in general* are positively correlated with cooperative industrial relations, Germany and the Nordic countries *in particular* have some of the most cooperative industrial relations in the world. Ideals of social partnership between employers and workers, and of "virtuous circles" connecting the interests of the two parties, dominate industrial relations (Kettunen, 2012; Behrens and Helfen, 2016).

In such a cooperative context, worker representatives may decide to exercise their powers in mostly non-disruptive ways that line up with the plans of managers and employers. Indeed, in in-depth interviews, German and Nordic board-level representatives report mostly acquiescing to managers' plans and just contributing information, in recognition of the fact that workers' interests are broadly aligned with the interests of the firm (Gold, Kluge, and Conchon, 2010). This alignment of priorities may be another key element contributing to codetermination's limited impact.

**Impacts of Codetermination Laws on Industrial Relations** One natural hypothesis is that both of the factors outlined above are *consequences of* the historical introduction of codetermination laws. As we described in Section 5, codetermination laws could normalize worker participation in decision-making and thereby create widespread cultures of worker involvement that end up operating independently of formal codetermination arrangements. Similarly, allowing workers to participate cooperatively in decision-making may make the worker-management relationship less adversarial and more friendly.

However, our empirical tests in Section 5 failed to find evidence that codetermination laws improve the quality or friendliness of industrial relations. Strike intensity does not appear to decline following codetermination reforms, and in general there is no correlation between the existence of codetermination laws and the cooperativeness of a country's industrial relations.

Rather, in our view, a reading of the historical evidence supports a different hypothesis: that in countries like Germany, Norway, and Sweden, codetermination laws *arose because of* pre-existing cultures of worker participation, rather than *causing* the development of those cultures. Germany and the Nordic countries have histories of industrial democracy, worker mobilization, and social partnership dating back to at least the early 1900s, with the codetermination reforms we study in the 1960s onwards being codifications and expansions of

pre-existing ad-hoc arrangements (Adler, 1922; BJORHEIM, 1974; Knudsen, 2006; Sippola, 2012; Votinius, 2012; McGaughey, 2016). We remain agnostic about what caused these cultures to develop in the first place, but it is implausible to suggest that, e.g., the high level of cooperation in contemporary Swedish industrial relations is entirely attributable to the introduction of minority board-level representation in the 1970s.

**Conclusion** If codetermination's limited impact is indeed explained by pre-existing cultures of worker-management cooperation or friendly industrial relations, then the effects of codetermination may well be different if implemented in contexts like the United States, where industrial relations are more adversarial. Codetermination might boost workers' influence by more in the absence of a pre-existing culture of informal worker involvement; and codetermination might proceed less harmoniously if managers are negatively predisposed towards worker representation. We return to this point in Section 7.

### 6.3 Other Labor Market Institutions

As we have noted, virtually all of the empirical evidence we drew on in Sections 3 and 4 comes from studies of German or Nordic codetermination. Compared to liberal market economies like the United States, the German and Nordic labor markets feature a range of other powerful pro-labor institutions: highly centralized collective bargaining, powerful unions, and extensive labor market regulations. The presence of these institutions may leave little scope for codetermination to have an impact.

To visualize the institutional differences between European countries with codetermination and comparable countries without codetermination (non-codetermined European countries, and liberal market economies like the United States, Canada, Australia, and New Zealand), we draw on data from the OECD/IAIS ICTWSS database (Visser, 2021) and the CBR Labor Regulation Index (Adams, Bishop, and Deakin, 2016). Panels (a), (b), and (c) of Figure 5 compare the degree of centralization of collective bargaining, rates of union membership, and the intensity of labor market regulation in codetermined versus non-codetermined countries, in 1960 and in 2018.

**Centralized Collective Bargaining** As Panel (a) of Figure 5 shows, countries with codetermination laws tend to have more centralized systems of collective bargaining than comparable countries without codetermination, both presently and historically. In liberal market economies like the United States, collective bargaining proceeds exclusively at the company level and coverage is consequently very spotty (Compa, 2014). By contrast, collective bargaining in Germany and the Nordic countries happens primarily at the sectoral level. Labor unions and employer associations negotiate collective bargaining agreements that cover entire industries

or industry-regions. These agreements specify wage floors and impose other restrictions on working conditions. Employers can deviate upwards (and sometimes even downwards) from these wage floors, and descriptive studies confirm that employers retain plenty of flexibility in wage setting despite the existence of these agreements (Uusitalo and Vartiainen, 2009; Card, Heining, and Kline, 2013). Sectoral bargaining therefore does not necessarily limit the scope for codetermination to improve wages or working conditions. Nevertheless, it is possible that these collective bargaining agreements create significant wage compression (e.g., by affecting pay setting norms; Western and Rosenfeld, 2011), which may dampen the effect of codetermination on wages.<sup>15</sup>

While this is an *a priori* plausible explanation of codetermination's limited effects on wages and job quality, the existing evidence does not strongly support it. For example, Jäger, Schoefer, and Heining (2021) do not find larger wage effects of board-level representation in German industries with lower collective bargaining coverage, suggesting that their finding of zero or small wage effects is not driven by the presence of collective bargaining agreements. Similarly, a large body of studies of German shop-floor representation have not found that shop-floor representation has larger wage effects in establishments not covered by a collective bargaining agreement (Jirjahn, 2017).

Thus, the existence of centralized systems of collective bargaining seems unlikely to explain the limited effects of codetermination. In Section 7, we turn our attention to potential *complementarities* between sectoral collective bargaining and codetermination.

**Union Representation** Countries with codetermination laws have much higher rates of union membership than comparable countries without codetermination. Panel (b) of Figure 5 shows that, in 1960, the average rate of union membership in countries that would go on to adopt codetermination laws was 10 percentage points higher than in comparable countries that would not adopt codetermination; by 2018, the gap between the two groups had grown to about 20 percentage points.<sup>16</sup>

High union density could help explain codetermination's limited impacts because (despite

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<sup>15</sup>In addition, as we discuss in Section 7, sectoral bargaining banishes adversarial negotiations over the distribution of rents to the sectoral level, and may thereby alleviate hold-up problems resulting from boosts to worker bargaining power (Acemoglu, 2001, 2019) and leave more room for cooperative labor-management relations at the firm level.

<sup>16</sup>The fact that countries with higher union density in 1960 were subsequently more likely to adopt codetermination laws may be attributable to the fact that unions have often been the major political force advocating for codetermination laws, as we discussed in Section 2.3. Meanwhile, as Panel (d) of Figure 5 shows, the widening of the gap in union density between codetermined and non-codetermined countries from 1960 to 2018 is attributable to the fact that, while union density in both groups of countries experienced a significant decline in the 1980s and 1990s, the decline was much more rapid in countries without codetermination. As Panel (b) of Figure 7 shows using our cross-country event study design with union density as the outcome variable, this may be attributable to a causal effect of union density, if a persuasive story can be given for how codetermination might have delayed effects on union density.

lacking formal co-decision-making rights) unions might play an important role in advocating for workers, which may render formal codetermination superfluous. For example, the Nordic countries have histories of establishment-level union representation through "shop stewards" that predate the introduction of formal co-decision-making arrangements (Bjorheim, 1974; Knudsen, 2006). If these shop stewards already satisfied Nordic workers' desire for voice, introducing additional codetermination arrangements might not have much of an effect.

On the other hand, high union density and codetermination rights may well be complements, if an organized workforce is necessary to take full advantage of co-decision-making rights.<sup>17</sup> The fact that unions have often expended a great deal of political effort advocating for codetermination rights (as described in Section 2.3) certainly suggests that they do not consider co-decision-making rights to be superfluous. In any case, both of these hypotheses suggest that codetermination might have different effects in countries with lower rates of union representation. Again, we return to this topic in Section 7.

**Labor Market Regulation** The German and Nordic labor markets are tightly regulated by global standards, and especially when compared to countries like the United States. Panel (c) of Figure 5 shows that, on average, countries with codetermination currently sit at the 70th percentile worldwide in terms of the intensity of labor market regulation, while comparable countries without codetermination sit at the 25th percentile on average. German and Nordic labor market regulations—covering everything from flexible hours to overtime to leave entitlements to restrictions on dismissal—may leave little scope for worker representatives to negotiate improvements in working conditions.

**Conclusion** Several important labor market institutions—sectoral collective bargaining, widespread union representation, and extensive regulation—may already capture most of the low-hanging fruit when it comes to affecting worker outcomes, leaving little room for German and Nordic codetermination to make an impact. If this explanation is correct, then codetermination may have larger impacts (either positive or negative) if implemented in contexts like the United States, where these institutions are less powerful, a point to which we return in Section 7.

However, we caution that these hypotheses remain mostly empirically untested, and the small amount of available empirical evidence contradicts the hypothesis that collective bargaining is responsible for codetermination's lack of wage effects.

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<sup>17</sup>For example, Kohler (1986, p.547) writes that "in the presence of a union, worker participation programs can produce important benefits [...] In the absence of a self-organized, autonomous employee association, however, the degree of worker participation in management decisionmaking will remain, at best, superficial." Liebman (2017, p.15) similarly reports that "a European expert who advises companies on their European Works Councils has made the case to me that in workplaces without a trade union the works councils are generally weak and ineffective."

## 7 What Would Codetermination Do in the United States?

The past four years have seen a surge of interest in codetermination in the United States, driven by an increasingly widespread belief among progressive policy-makers, commentators, and academics that recent afflictions suffered by American workers are at least partially attributable to a sharp decline in the power of workers relative to shareholders within corporations (see, e.g., Liebman, 2017; Greenwald, Lettau, and Ludvigson, 2018; Hockett et al., 2018; Yglesias, 2018; Stansbury and Summers, 2020; Strine Jr., Kovvali, and Williams, 2021). This diagnosis has motivated policy proposals aimed at expanding or strengthening worker representation, either by reinvigorating American unions or by introducing shared governance arrangements modeled after European codetermination.

Having surveyed the best available evidence on codetermination in Europe, we now discuss more speculatively what this evidence might imply for a potential introduction of codetermination in the United States. As we have noted, European codetermination arrangements are nested within the broader institutional context of European "coordinated market economies," which have long histories of worker mobilization, highly centralized systems of collective bargaining, and industrial relations systems dominated by ideals of "social partnership" between employers and workers (Hall and Soskice, 2001; Aguilera and Jackson, 2003). By contrast, the United States is a "liberal market economy" where large-scale worker mobilization and collective bargaining are rare (at least in the private sector), and industrial relations are adversarial and often actively hostile. As a result, lessons drawn from studies of European codetermination may not straightforwardly translate to the United States. In this section, we extend our earlier discussion of potential interactions between codetermination and other European institutions, focusing specifically on what these interactions might imply for the effects of codetermination in the United States.

### 7.1 Recent Codetermination Proposals

**Board-Level and Shop-Floor Representation Proposals** Board-level codetermination provisions are a key element of two pieces of legislation introduced by Democratic senators in 2018. The *Reward Work Act*, introduced by Senators Tammy Baldwin, Elizabeth Warren, and Brian Schatz, would require all listed companies in the United States to adopt one-third board-level worker representation, and would also ban stock repurchases. The *Accountable Capitalism Act*, introduced later in the same year by Senator Elizabeth Warren, would require corporations with more than \$1 billion in tax receipts to adopt 40% board-level worker representation, and more broadly would require these corporations to shift away from a governance model based on shareholder primacy to a model rooted in the ideals of "stakeholder governance."

Compared to board-level codetermination, shop-floor codetermination has not received

much attention from American legislators. As Liebman (2017) describes, state-level shop-floor codetermination mandates would arguably be illegal under the 1935 National Labor Relations Act. However, despite this legal barrier, shop-floor codetermination laws have been informally proposed several times in the past few years (Liebman, 2017; Cass, 2020; Strine Jr., Kovvali, and Williams, 2021), and the possibility of establishing a German-style works council was raised repeatedly during unionization drives in Volkswagen's Chattanooga plant in 2014 and 2019 (Silvia, 2018, 2020). Importantly, any attempt at federal shop-floor codetermination legislation would force a much broader conversation about amending the relevant provisions of the National Labor Relations Act, which currently stifles any local experimentation with more cooperative forms of shop-floor representation (even if both workers and the employers would support such experimentation).

Recent codetermination proposals are not being advanced in isolation: they are part of a broader legislative agenda pursued by Democratic representatives that aims to increase the influence of American workers in corporate power structures. Related proposals include the broader stakeholder governance provisions in the *Accountable Capitalism Act*, as well as the *PRO Act*, which would significantly strengthen American unions by allowing them to override right-to-work laws and restricting the ability of employers to interfere with union elections (NPR, 03/09/21).

**Recent Public Debate** Board-level codetermination proposals have triggered mixed reactions in the public discourse. The *Accountable Capitalism Act* was lauded by some commentators as a necessary check on the excesses of American shareholder capitalism (Yglesias, 2018; Vogel, 2019), and decried by others as disastrous for innovation and economic growth (Hammond, 2018; Shackford, 2018; Williamson, 2018). The disagreement has not always fallen along partisan lines; some conservative commentators, such as Oren Cass, have argued for codetermination (Cass, 2020). In addition, perhaps surprisingly, the business community has expressed support for the idea of abandoning shareholder primacy. In 2019, the U.S. Business Roundtable and World Economic Forum released statements endorsing a "stakeholder governance" model that would task corporations with serving the interests of non-shareholder stakeholders, including workers and consumers. However, these announcements have been accused of being purely performative (Cass, 2020); they do not include proposals that would actually shift authority away from directors and managers, but simply suggest expanding the mandate of those directors and managers in vague and unaccountable ways (The Economist, 08/24/2019).

**Implementation of Codetermination Proposals** As Liebman (2017) and Strine Jr., Kovvali, and Williams (2021) emphasize, advocates of recent codetermination proposals must face up to a long list of important practical questions concerning how codetermination would be

implemented in the United States. These questions include: how would elections of worker representatives be organized, and who would be eligible to vote in them? Who would bear the associated costs? How would disputes between worker representatives and firms be resolved? How, if at all, would middle managers be represented in codetermination arrangements?

Additionally, critics of recent codetermination proposals, such as Dammann and Eidenmueller (2021), pose a number of questions that they believe highlight the unsuitability of codetermination arrangements for American firms. These questions include: what procedures would exist for dismissal of problematic board-level worker representatives? How would worker representatives be involved in bankruptcy governance, or negotiations surrounding mergers and hostile takeovers? How could corporate law be designed to prevent avoidance of codetermination requirements?

It is tempting to respond to all of these questions by saying that the United States should simply imitate European arrangements. However, the European solutions to these practical problems draw indispensably on institutional features of European labor markets, including widespread union representation and broader frameworks of social partnership, that are comparatively absent in the United States. This observation leads us naturally into a discussion of interactions between European codetermination and other European labor market institutions, and what these interactions might imply for American codetermination.

## **7.2 What Would American Codetermination Look Like? What Would it Do?**

We concluded in Sections 3-5 that the best available evidence suggests limited effects of codetermination on core economic outcomes. Moreover, we proposed three explanations for the institution's limited impact. First, we suggested that existing codetermination laws convey negligible authority to workers. Recent codetermination proposals like the *Reward Work Act* and *Accountable Capitalism Act* replicate the European board-level codetermination arrangements that we argued barely increase workers' authority, so if this explanation indeed accounts for European codetermination's limited impacts, we should expect American codetermination proposals to have similarly small effects. However, our second and third explanations relied on institutional features of European labor markets that are much less present in the United States: harmonious industrial relations, widespread unionization, centralized collective bargaining, and extensive labor market regulation. If *these* explanations account for European codetermination's limited impacts, there is room for codetermination to have different effects in the United States. We now explore this possibility in more depth. In addition to considering the implications of interactions between codetermination and other institutions for the *causal effects* of codetermination, we also consider implications for the *practical implementation* of codetermination.

**Union Representation** As Panel (b) of Figure 5 illustrates, union representation in countries with codetermination is much more extensive than in the United States. This fact has several immediate practical implications, as Strine Jr., Kovvali, and Williams (2021) describe. European codetermination laws typically set up shared governance procedures that explicitly refer to unions at a number of stages (ETUI, 2020). First, when legislation prescribes a *right* to codetermination that workers can voluntarily take up, company-level union representatives are usually responsible for initiating the proceedings that establish codetermination in a particular workplace. Second, elections of worker representatives are usually organized and monitored by unions, or in some cases worker representatives are directly nominated by unions and selected through internal union procedures. Third, union representatives form a ready pool of able and willing candidates for worker representative positions. Overall, much of the practical infrastructure of codetermination in European countries relies on the near-universal presence and widespread legitimacy of union representatives. There is therefore little precedent for how codetermination might be implemented in the United States, where many workplaces either lack union representatives entirely, or lack union representatives who hold the confidence of workers.

In addition, low union density in the United States may mean that American codetermination would have either *stronger* or *even weaker* impacts than European codetermination. As we described in Section 6.3, union representation could be either a substitute or complement for codetermination. According to the "substitute" theory, union representation already caters to European workers' demands for voice, and codetermination is therefore a superfluous institution that has no marginal effect. This theory is hard to square with the history of vigorous union advocacy for codetermination rights, which presumably reflects unions' belief that codetermination rights are *not* redundant. By contrast, according to the "complement" theory, union representation complements codetermination in a number of important ways. First, worker organization through unions lets workers speak with a unified voice and thereby take full advantage of co-decision-making rights; Liebman (2017) cites anecdotal evidence for this mechanism. Second, unions provide an outlet for workers' adversarial demands or grievances, leaving board-level or shop-floor worker representatives free to cooperate harmoniously with employers. If, in the absence of union representation, American worker representatives would be forced to be a vehicle for workers' adversarial demands, they might find it much harder to form a working relationship with employers.

Overall, there are important interactions between codetermination and union representation arising from practical considerations and from union representation's potential mediation or enhancement of codetermination's causal impacts. This has some important implications for codetermination proposals in the United States. As we have noted, reforms to strengthen unions are a major part of the recent Democratic legislative agenda.

**Sectoral Bargaining** As Panel (a) of Figure 5 shows, European countries with codetermination have collective bargaining systems that operate primarily at the industry level. By contrast, collective bargaining in the United States happens almost exclusively at the company level, if at all. As we noted in Section 6.3, there is no evidence that sectoral bargaining is a *substitute* for codetermination in the sense of creating wage compression that limits codetermination's wage effects (Jäger, Schoefer, and Heining, 2021). However, there are some potentially relevant *complementarities* between sectoral bargaining and codetermination that we now describe.

First, sectoral bargaining creates significant distance between high-stakes adversarial bargaining and individual employers and their employees, which may enable warmer employer-employee relationships and thereby make shared governance proceed more smoothly. Anecdotal evidence suggests that American managers are encouraged to fear and obstruct unionization (Lafer and Loustaunau, 2020), perhaps in part because the costs of unionization are so salient to employers when collective bargaining happens at the company level. When collective bargaining is more centralized and remote from the firm, as it frequently is in Europe, employers may be more amenable to the idea of worker representation. Additionally, the European practice of legislatively "extending" sectoral agreements to cover employers not initially party to the negotiations removes much of the incentive of employers to resist unionization, since they cannot thereby escape collective bargaining. This may again make employers less hostile to worker representation.

Second, at a practical level, sectoral collective agreements usually contain provisions mandating the appointment of establishment-level union representatives who are responsible for locally implementing the sectoral agreements (ETUI, 2020). This is an important mechanism through which European countries achieve near-universal union representation, which matters for the reasons we discussed above.

Thus, there may also be important complementarities between codetermination and industry-level systems of collective bargaining in Europe. Several Democratic candidates in the 2020 presidential primaries included sectoral bargaining proposals in their platforms, including Bernie Sanders, Elizabeth Warren, Pete Buttigieg, and Beto O'Rourke (Marketplace, 11/01/2019).

**Quality of Industrial Relations** Cooperative industrial relations may be a crucial complement to codetermination. A long-standing hypothesis is that the effects of increasing worker power crucially hinge on the pre-existing quality of labor-management relationships (Freeman and Medoff, 1984; Kochan and Kimball, 2019). In a hostile atmosphere, boosting workers' authority might simply intensify negative-sum conflict between workers and managers, consistent with negative effects of unionization on firm performance in the United States, at least on average (Lee and Mas, 2012; Frandsen, forthcoming). By contrast, giving workers co-decision-making rights in a cooperative atmosphere might produce less harmful results.

As we mentioned in Section 5, results from the Executive Opinion Survey indicate that managers consider labor relations in Germany and the Nordic countries to be more harmonious and cooperative than labor relations in the United States. In addition, we described above how other institutional differences between the United States and Europe—the relative absence of union-based outlets for adversarial grievances, and the proximate threat of company-level collective bargaining—may lead to a more hostile relationship between employers and worker representatives if codetermination was introduced in the United States.

**Conclusion** Given the plethora of institutional differences between American and European labor markets, it is hard to draw conclusions from the existing European evidence about the likely effects of American codetermination proposals. American codetermination could have smaller or larger effects than the European variant; if the effects are larger, they might be positive, negative, or a combination of the two. For example, American codetermination could have a larger positive effect on rent-sharing with workers, but this could then cause hold-up problems to kick in and lead to reductions in investment and economic growth.

If there is one conclusion that *is* clear from this discussion, it is that codetermination is not a standalone institution. Rather, it is part of a broader institutional and cultural package whose other elements complement codetermination and supply its practical infrastructure. The recent American policy discourse has seen several proposals to introduce other elements of the European package, including stronger union representation and sectoral collective bargaining.

## 8 Overall Conclusions

The available evidence indicates that the European model of codetermination is neither a panacea for all of the problems faced by 21st-century workers, nor a destructive institution that is dramatically inferior to shareholder primacy. Rather, as currently implemented, it is a moderate institution with, on net, nonexistent or small positive effects. Board-level and shop-floor worker representation cause at most small increases in wages, possibly lead to slight increases in job security and satisfaction, and have largely zero or small positive effects on firm performance. The small size of these effects may reflect limited power conveyed by existing codetermination arrangements, cultures of informal worker-management cooperation, or the influence of other pro-labor institutions. If cultural or institutional features of European labor markets are key to explaining codetermination's limited impacts, codetermination might have more noticeable effects if introduced in the United States. Moreover, there are important practical complementarities between codetermination and other European labor market institutions. Recent codetermination proposals in the United States sit alongside

proposals to adopt other features of the European institutional package, including strong union representation or sectoral bargaining.

There are a number of promising directions for future work on codetermination. First, as we have noted, it remains an open question whether codetermination arrangements that convey greater power to workers, such as quasi-parity or parity board-level representation in Germany, have more substantial positive or negative impacts (Svejnar, 1981; Kim, Maug, and Schneider, 2019). Second, while we provide country-level event study analyses, a promising avenue for future work on general-equilibrium effects of codetermination may be to leverage treatment variation at the level of industries or local labor markets. Third, beyond a few correlational tests or heterogeneity analyses, there is a paucity of evidence speaking to the interaction of codetermination with other labor market institutions (see Jäger, Schoefer, and Heining, 2021, for heterogeneity by collective bargaining prevalence). Fourth, while codetermination appears to have near-zero effects on average, it remains possible that formal codetermination rights are more impactful in particular contexts or during particular periods of time—for example, during economic crises or firm shutdowns (see, e.g., Rehder, 2003). Fifth, due to data limitations, we lack estimates of the effects of shared governance on intangible outcomes like worker alienation or feelings of domination or insecurity (a plausible pathway for this line of work would be merged administrative and individual-level survey data, see Harju, Jäger, and Schoefer, 2021, for effects on subjective measures of job quality). Sixth, we lack a thorough body of evidence on the effects of industrial democracy and worker representation on workers' political engagement and attitudes (for recent work in this area, see Budd, Lamare, and Timming, 2017, Feigenbaum, Hertel-Fernandez, and Williamson, 2018, and Frymer and Grumbach, 2021). Seventh, we lack quantitative evidence on the relationship between codetermination and heterogeneity in management practices (c.f. Bloom et al., 2019, on the relationship between union strength and management quality in the United States). Eighth, further examination of the interaction between worker representation and decisions about outsourcing, nonstandard work arrangements, and the structure of work might yield deeper insights into the institution's impacts (Burdín and Pérotin, 2019; Katz and Krueger, 2019; Drenik, Jäger, Plotkin, and Schoefer, forthcoming). Finally, looking forward, systems of worker representation may interact importantly with technological change, including the spread of automation, workplace monitoring technologies, and algorithmic decision-making (Autor, Mindell, and Reynolds, 2019; Belloc, Burdín, and Landini, 2020; Kasy and Abebe, 2021).

We close by reiterating that our paper focuses solely on the economic consequences of codetermination. Addison (2009) observes that, perhaps surprisingly, economic considerations have not historically been at the forefront of the German public debate about codetermination. Rather, the conversation has been dominated by non-consequentialist justifications of codetermination rooted in principles of economic democracy and the dignity of work (see, e.g., the

1971 *Freiburger Thesen*, the party platform of the classically-liberal FDP, which shaped the 1976 expansion of board-level codetermination during its time as a junior coalition partner of the SPD). In 2001, the German government dismissed concerns that its drafted extension of the Works Council Act would be costly to businesses by declaring:

Democracy is not cost neutral. This principle also applies to democracy at the workplace and to the resulting system of establishment-level codetermination. (As quoted in Addison, 2009, p.22; see Bundestag, 2001)

The idea that democratic political principles should be extended to the realm of private business has recently regained popularity in analytic political philosophy (Anderson, 2017; Frega, Herzog, and Neuhäuser, 2019; Herzog, 2020; Walters, 2021). Elizabeth Anderson writes:

Government is everywhere, not just in the form of the state, but even more pervasively in the workplace. [...] The vast majority [of workers] are subject to private, authoritarian government, not through their own choice, but through laws that have handed nearly all authority to their employers. (Anderson, 2017, p.70-71)

She adds that, once we recognize this fact, we are forced to conclude that:

[...] There is no adequate substitute for recognizing workers' voice in their government. (Anderson, 2017, p.69)

Much of the existing empirical research does not directly speak to these important non-consequentialist questions. However, our conclusion—that codetermination in its current form appears to have limited consequences for core economic outcomes—may shift the focus of the debate to such non-consequentialist arguments.

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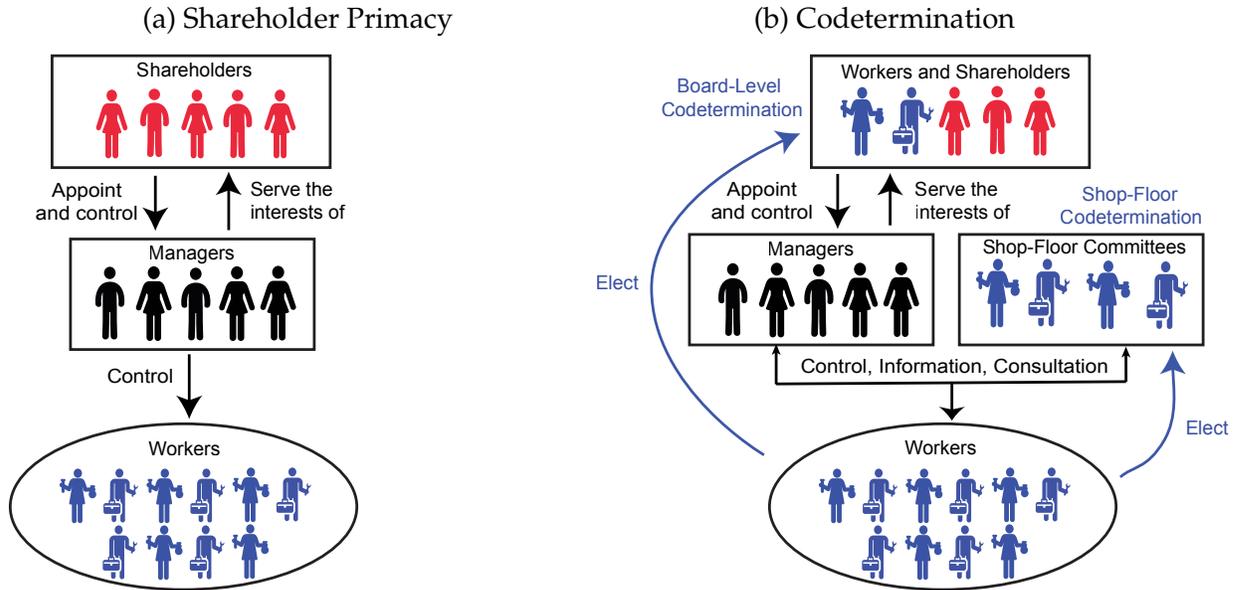
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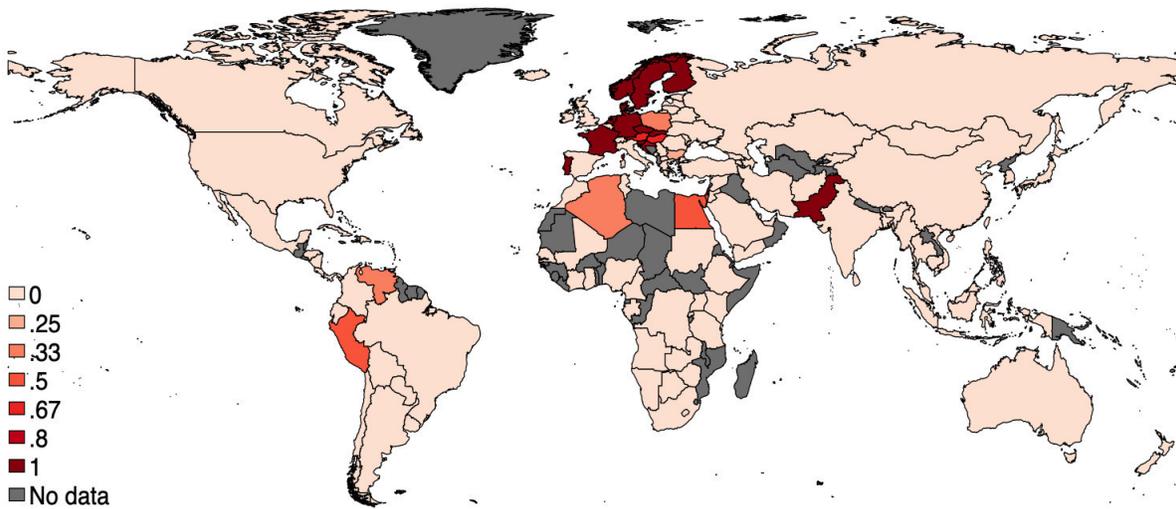
# 9 Figures

Figure 1: Shareholder Primacy versus Codetermination



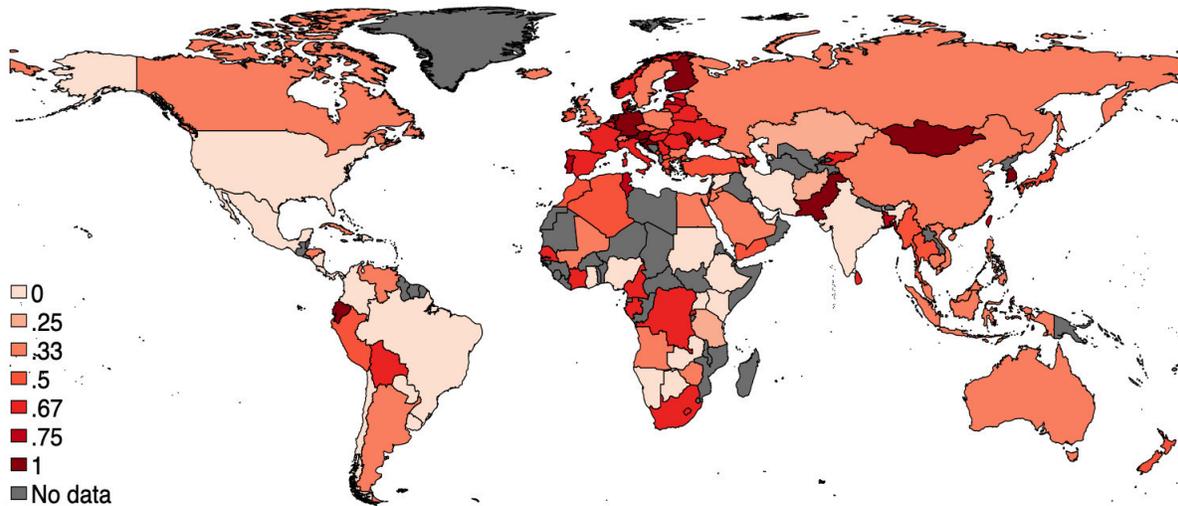
Note: This figure compares the structure of corporate governance under shareholder primacy (Panel (a)) versus codetermination (Panel (b)).

Figure 2: Board-Level Representation Worldwide (2015)



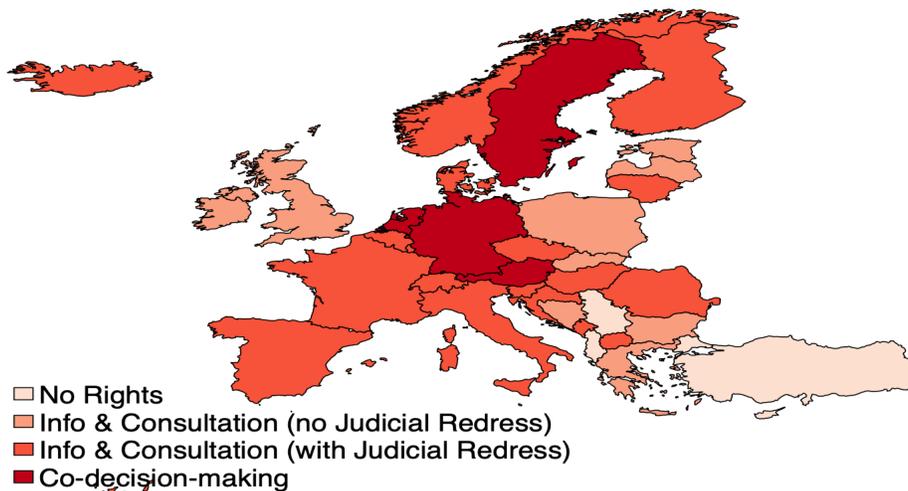
Note: This map displays the existence and strength of laws mandating board-level worker representation, as of 2015. A country is assigned a value of 1 if workers in all companies above some size threshold have a right to board-level representation, and 0 if there is no set of companies where workers have this right. Scores between 0 and 1 reflect weaker laws, for example laws requiring only that formerly state-owned companies have board-level worker representatives. Data are from the "Codetermination: board membership" variable in the CBR Labor Regulation Index dataset; see Adams, Bishop, and Deakin (2016) for a complete description of the dataset.

Figure 3: Shop-Floor Representation Worldwide (2015)



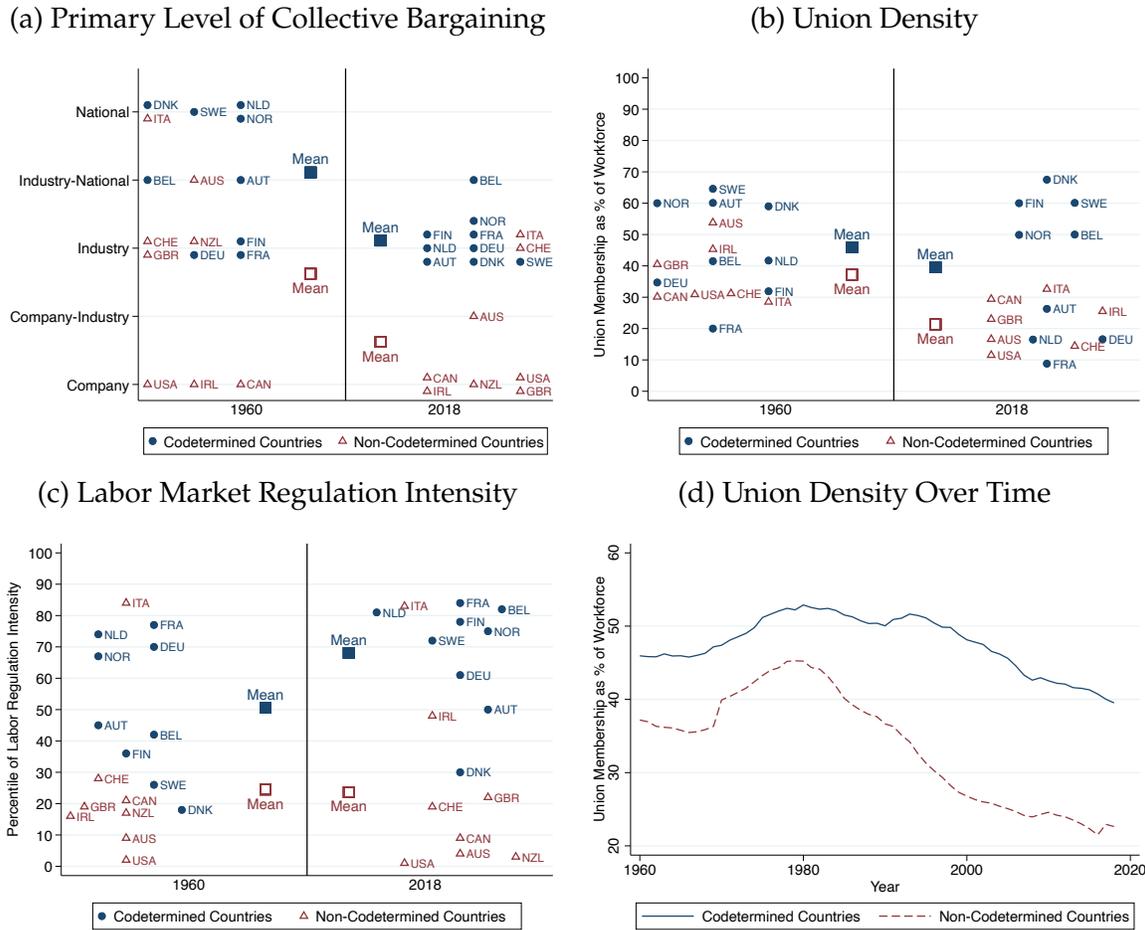
*Note:* This map displays the existence and strength of laws giving workers rights to shop-floor representation, as of 2015. A country is assigned a value of 1 if a right to shop-floor representation exists and the shop-floor representatives are given co-decision-making powers; a value of 0.67 if a right to shop-floor representation exists but councils do not have co-decision-making powers; a value of 0.5 if a right to shop-floor representation exists except in cases where employers can point to pre-existing alternative arrangements; and a value of 0.33 if workers have rights to information and consultation but not rights to shop-floor representation. There is scope for further idiosyncratic variation between 0 and 1 based on the strength of the laws. Data are from the "Codetermination and information and consultation of workers" variable in the CBR Labor Regulation Index dataset; see Adams, Bishop, and Deakin (2016) for a complete description of the dataset.

Figure 4: Powers of Shop-Floor Representatives in Europe (2019)



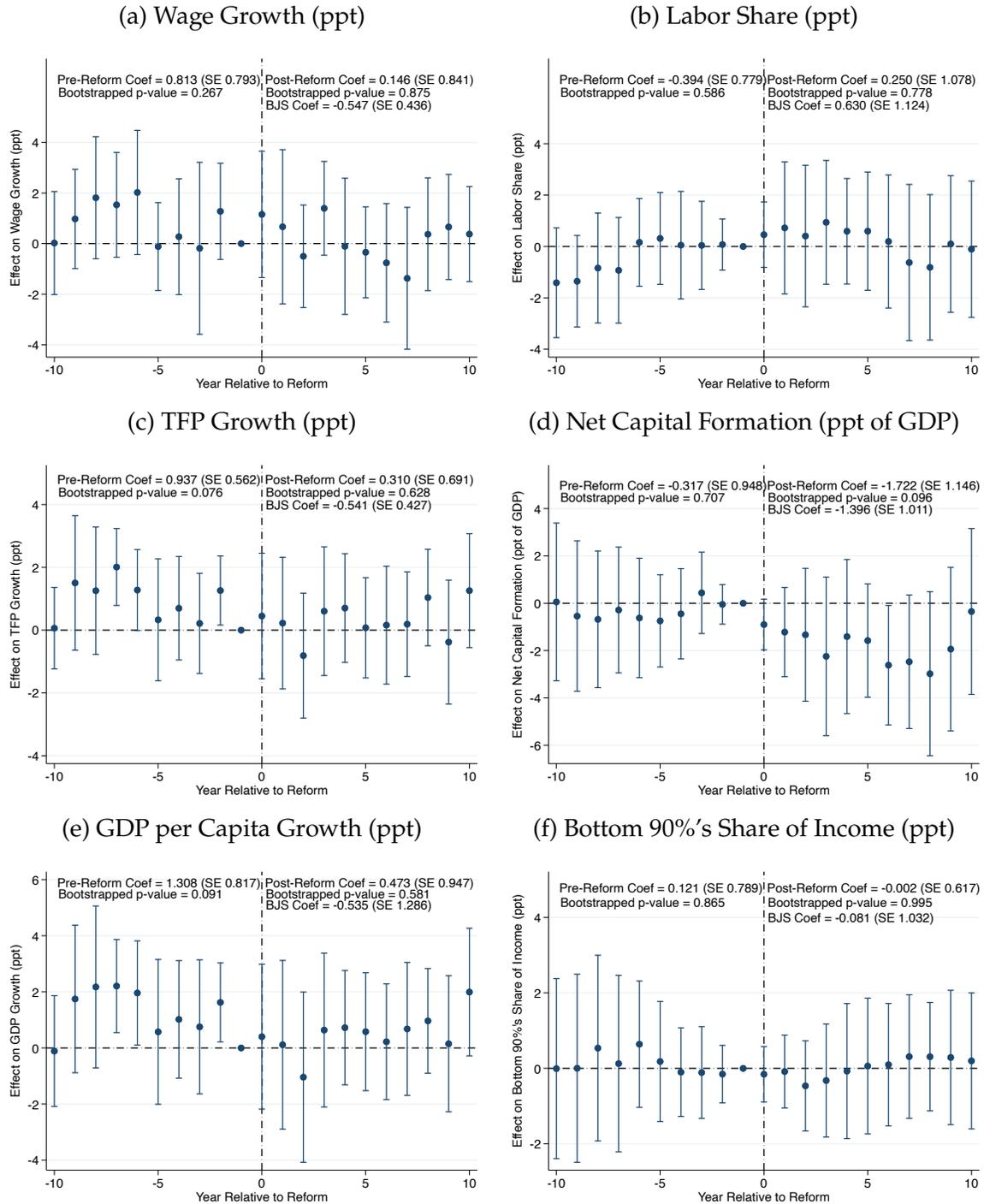
*Note:* This map displays the powers of shop-floor representatives in European countries, as of 2019. A value of 0 indicates that no right to shop-floor representation exists; a value of 0.33 says that a right to shop-floor representation exists, and shop-floor representatives have information and consultation rights, but with no possibility of judicial redress; a value of 0.66 says that shop-floor representatives have a right to information and to provide advice, and can appeal to employment courts if their advice is ignored; and a value of 1 says that shop-floor representatives have co-decision-making powers on certain issues. Data are from the "WC\_rights" variable in the OECD/AIAS ICTWSS database; see Visser (2021) for a full description of the dataset.

Figure 5: Institutional Differences Between Codetermined and Non-Codetermined Countries



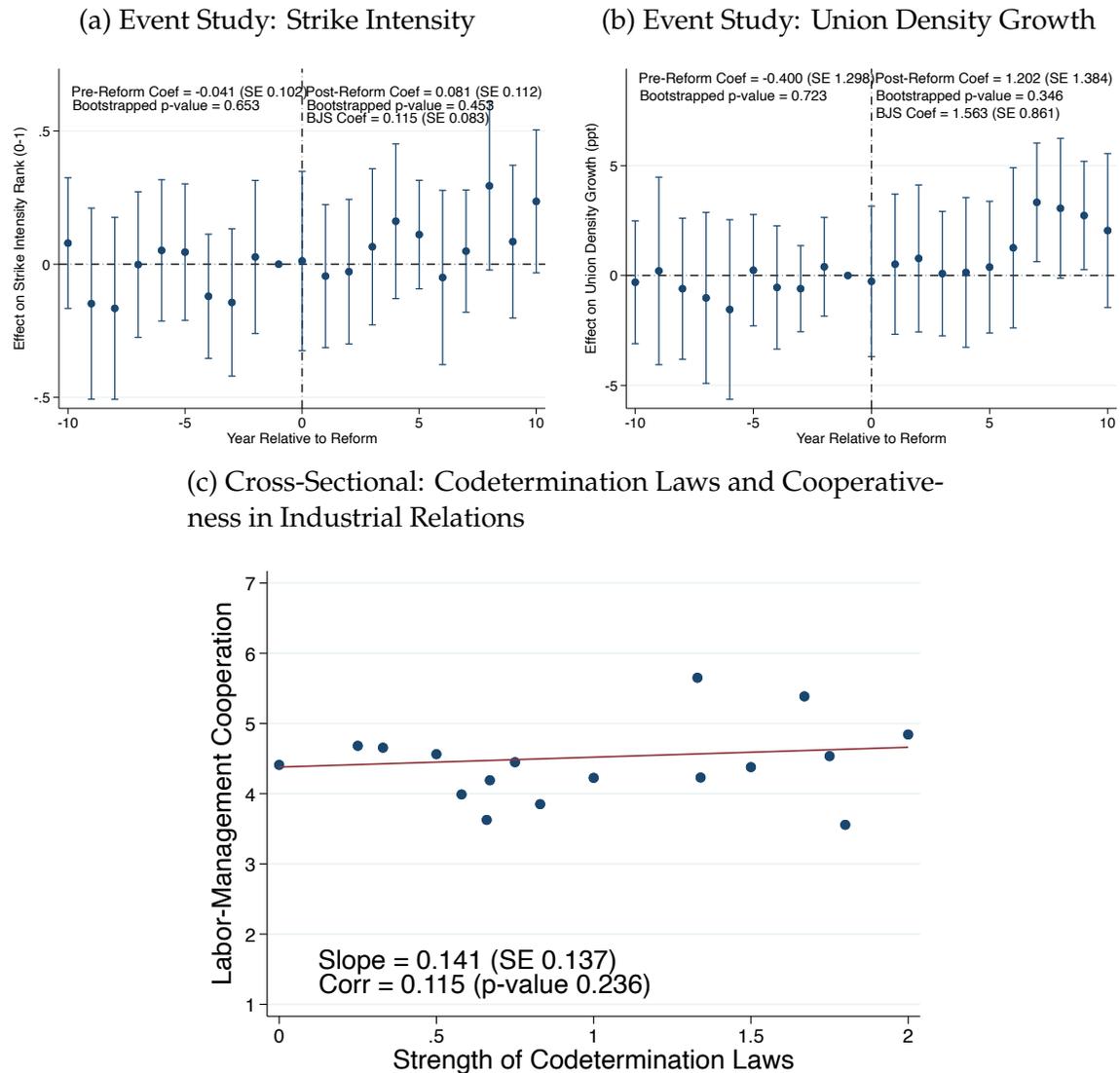
*Note:* This figure displays institutional differences between European countries with codetermination laws and comparable countries without codetermination laws. The sample of countries consists of the European democracies included in our cross-country event studies (as explained in Appendix Section B.1), as well as the liberal market economies of the United States, Canada, Australia, and New Zealand. Panel (a) displays the degree of centralization of collective bargaining in each country in 1960 and in 2018, on a 1-5 point scale. Panel (b) displays union density (the percentage of workers who belong to a union) in each country in 1960 and 2018, and Panel (d) presents a time-series plot of the unweighted mean union density in the codetermined versus non-codetermined groups. Data for Panels (a), (b), and (d) are drawn from the OECD/IAIS ICTWSS dataset (Visser, 2021). Panel (c) presents each country's percentile ranking worldwide in terms of the overall intensity of labor regulations, in 1970 and 2013. The overall intensity of labor regulations is measured by the sum of all the labor regulation variables in the CBR Labor Regulation Index (Adams, Bishop, and Deakin, 2016), excluding the variables about codetermination and unionization. The global sample within which our sample of countries are ranked consists of the 84 countries in the CBR dataset with nonmissing data back to 1970. Across all panels, within the "1960" and "2018" columns, horizontal dispersion in the dots does not signify anything; it is just to make the dot labels readable. The same holds for, in Panel (a), the slight vertical dispersion around the "Industry" and "National" collective bargaining levels.

Figure 6: Effects of Codetermination Reforms on Aggregate Outcomes



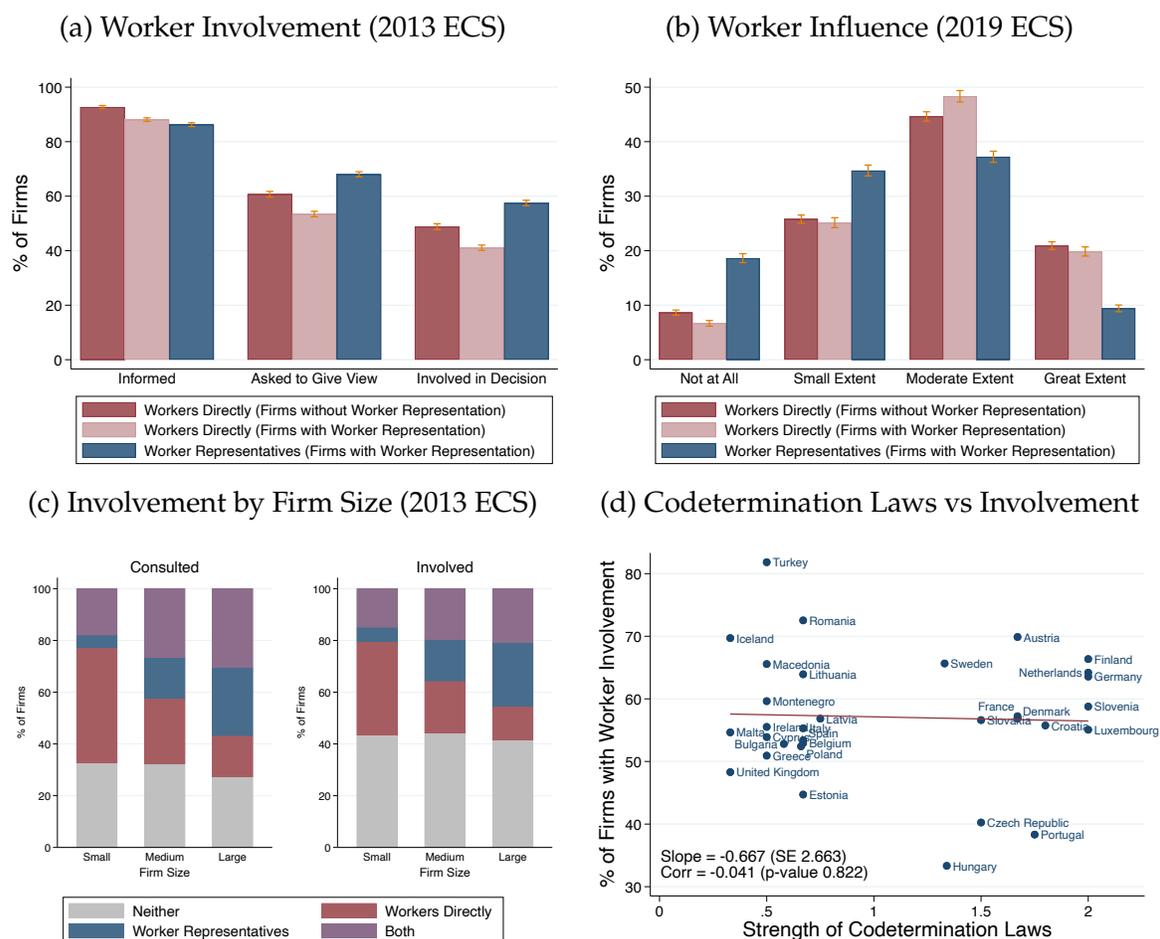
*Note:* These panels present coefficients from difference-in-differences regressions where the outcome variable is an aggregate economic outcome, the coefficients represent the effects of codetermination reforms, and the sample is our full list of country-reforms and their respective synthetic control units (the sample in Panel (f) differs from the sample in the others, for reasons described in Appendix Section B.6). In Panel (a), the dependent variable is wage growth in year  $t$  (compared to year  $t - 1$ , in %); in Panel (b), the dependent variable is labor income as a share of GDP in year  $t$  (in %); in Panel (c), it is total factor productivity growth in year  $t$  (in %); in Panel (d), it is net capital formation in year  $t$  as a % of year  $t$  GDP; in Panel (e), it is growth in GDP per capita in year  $t$  (%); and in Panel (f), it is the share of national income held by the bottom 90%. Standard errors are clustered at the country-reform level. We report average pre-reform and average post-reform coefficients, and report bootstrapped p-values for those coefficients due to the small number of clusters. In addition, we report post-reform coefficients estimated using the imputation methodology developed by Borusyak, Jaravel, and Spiess (2021). See Appendix Sections B.1-B.7 for a full description of the sample, data, and methodology.

Figure 7: Codetermination and Industrial Relations



*Note:* Panels (a) and (b) plot coefficients from our standard cross-country event study specifications (see Note to Figure 6), for industrial relations outcome variables. The dependent variable in Panel (a), strike intensity, is a continuous variable between 0 and 1, with a value of 1 representing the highest-ranked year in that country (the year with the most intense strike activity). A positive coefficient means an increase in strike intensity, and a negative coefficient represents a decrease in strike intensity. The dependent variable in Panel (b) is union density growth in year  $t$  relative to year  $t - 1$ , in %. Panel (c) is a binned scatter plot displaying the cross-sectional correlation between the strength of a country's codetermination laws (as measured by the CBR Labor Regulation Index) and the degree of cooperativeness in the country's industrial relations (as measured by a 1-7 point survey item from the World Economic Forum's Executive Opinion survey, described in Appendix Section B.3). We report the slope of the relationship, a robust standard error, and a correlation coefficient and p-value.

Figure 8: Informal Worker Involvement in the 2013 and 2019 European Company Survey



Note: Panel (a): the 2013 ECS asks managers if, in the past 3 years, their firm underwent a major change in any of a list of areas (remuneration, use of technology, work organization/allocation, recruitment policies, and working time arrangements). The 68% of managers who say "yes" are asked which one was the most important change, and then asked whether workers were directly (1) informed in advance about that change, (2) consulted about that change, or (3) involved in decision-making about that change. Managers in establishments with worker representation are additionally asked whether worker representatives were informed, consulted, or involved in the decision. The dark red bars plot responses about direct involvement from managers in firms *without* formal worker representatives. Light red bars plot responses about direct involvement from firms *with* worker representatives, and blue bars plot responses about worker representative involvement in firms with worker representation. Panel (b): in the 2019 ECS, managers are asked how much influence workers directly exercise over decisions about the organization of work. Managers in firms with worker representation are also asked about the influence of worker representatives. Responses are plotted in dark red, light red, and blue bars analogous to those in Panel (a). The same questions are also asked about decision-making in four other areas; responses in those four areas are plotted in Appendix Figure A.13. Panel (c) plots responses to the same questions as Panel (a), split up by firm size (small firms are those with fewer than 50 workers; medium firms are between 50 and 250; large firms are 250+). Grey bars represent firms where workers were not involved at all in the last major decision, red bars represent firms where workers were directly involved (and worker representatives were not involved), blue bars represent firms where worker representatives were involved (and workers were not directly involved), and purple bars represent firms where both workers directly and worker representatives were involved. All firms, including those without formal worker representation, are included. Panel (d): presents a scatterplot from a regression of the percentage of firms in a country who involved workers or worker representatives in their last major decision (from the same questions as Panels (a) and (c)) on a measure of the strength of board-level and shop-floor representation laws (calculated from the CBR Labor Regulation Index, Adams, Bishop, and Deakin, 2016).

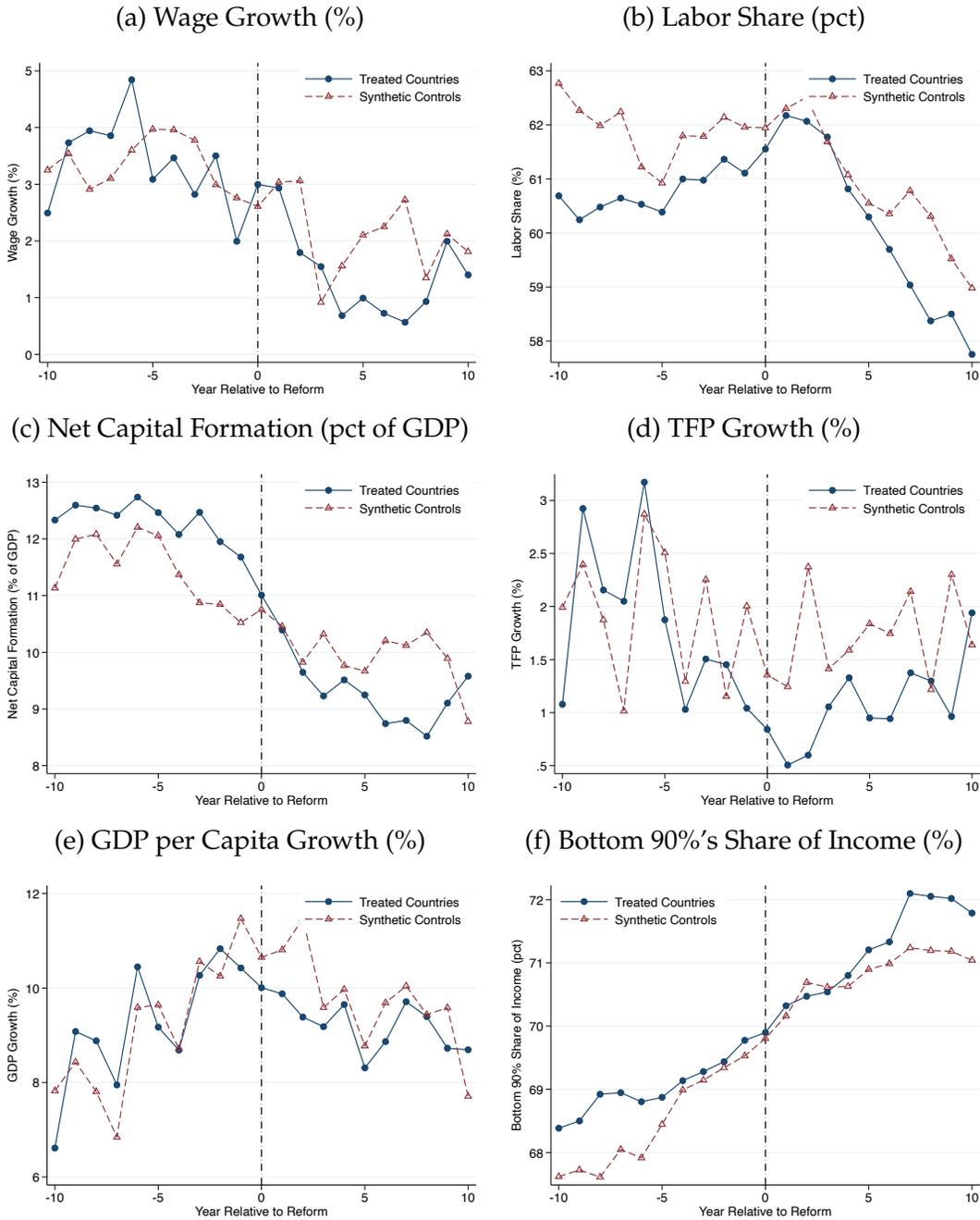
**Online Appendix of:**  
**What Does Codetermination Do?**  
**Simon Jäger, Shakked Noy, Benjamin Schoefer**

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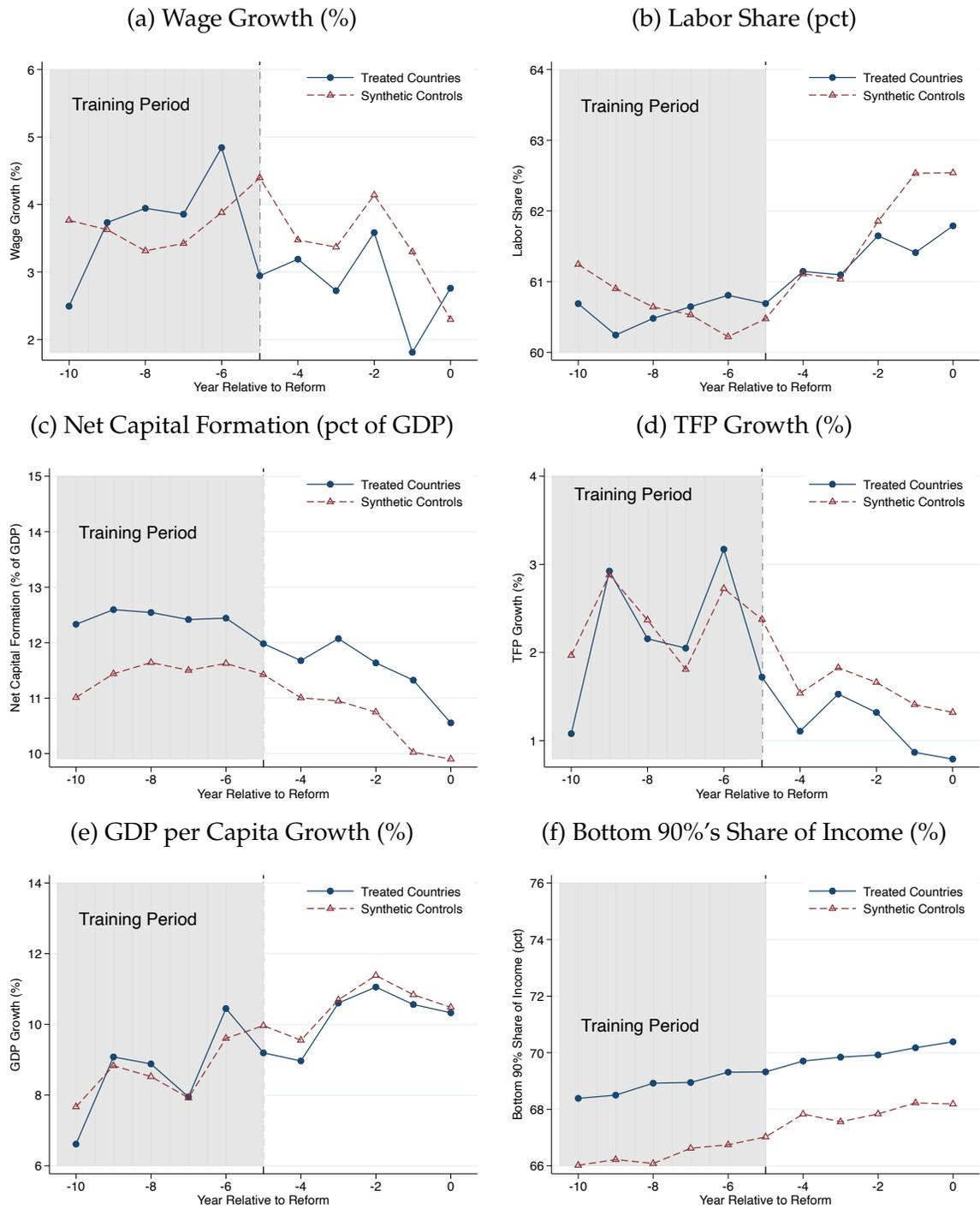
# A Appendix Figures

Figure A.1: Aggregate Synthetic Control Plots



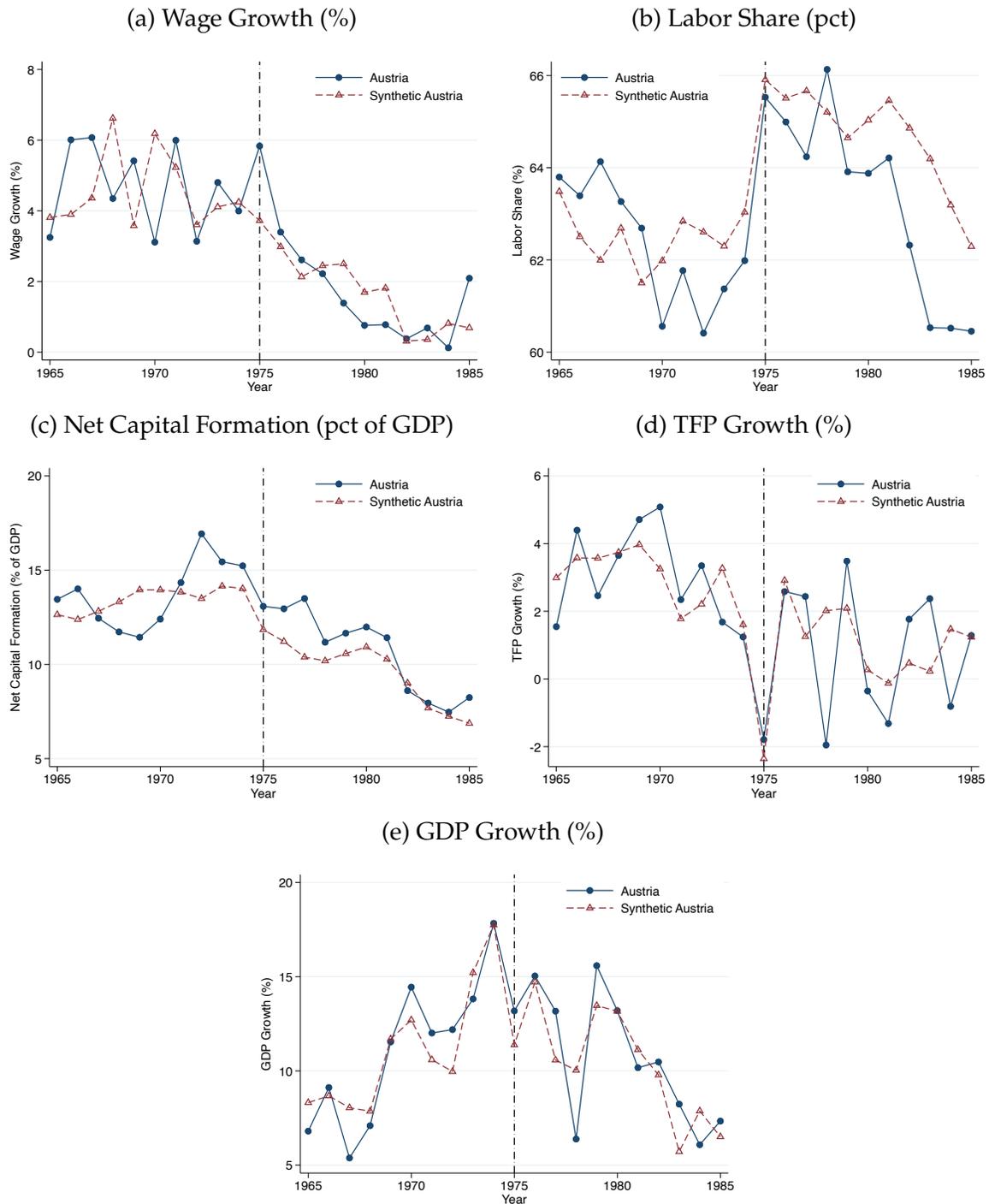
*Note:* For each of our dependent variables, this figure plots the raw time series in our group of treated country-reforms and our group of synthetic control units. The blue (solid circle) line represents the outcome variable averaged across our treated country-reforms, and the red (hollow triangle) line is averaged across our synthetic control units.

Figure A.2: Validation Check for Aggregate Synthetic Control Plots



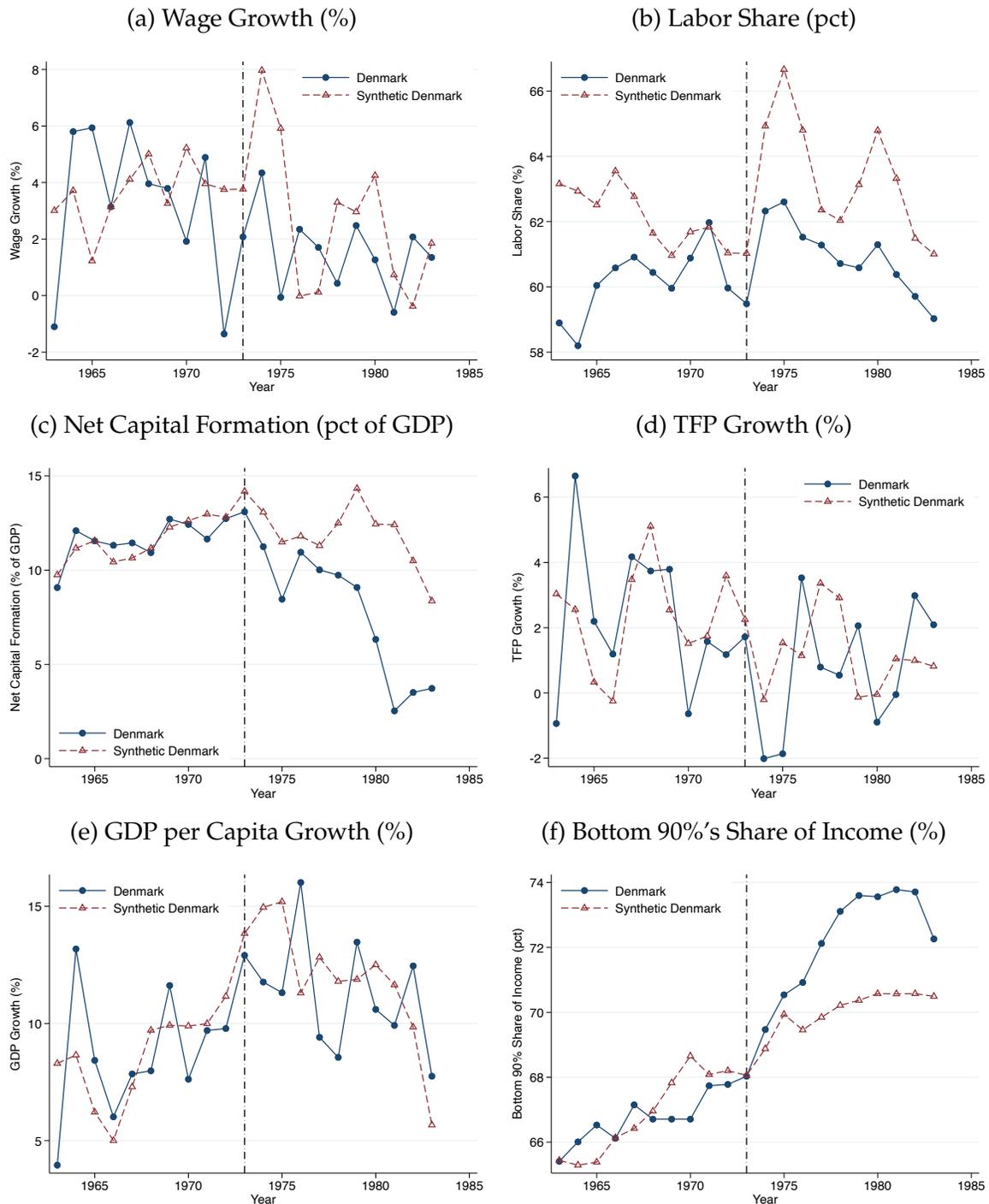
Note: This figure replicates Figure A.1, but matches the synthetic control units only on the first half of the pre-reform period, and plots the results in the second half of the pre-reform period, to check the quality of the synthetic control fits. The 1966 Norwegian reform is excluded from this plot because we only have 6 years of pre-period data for that reform.

Figure A.3: Synthetic Control Plots (Austria 1975)



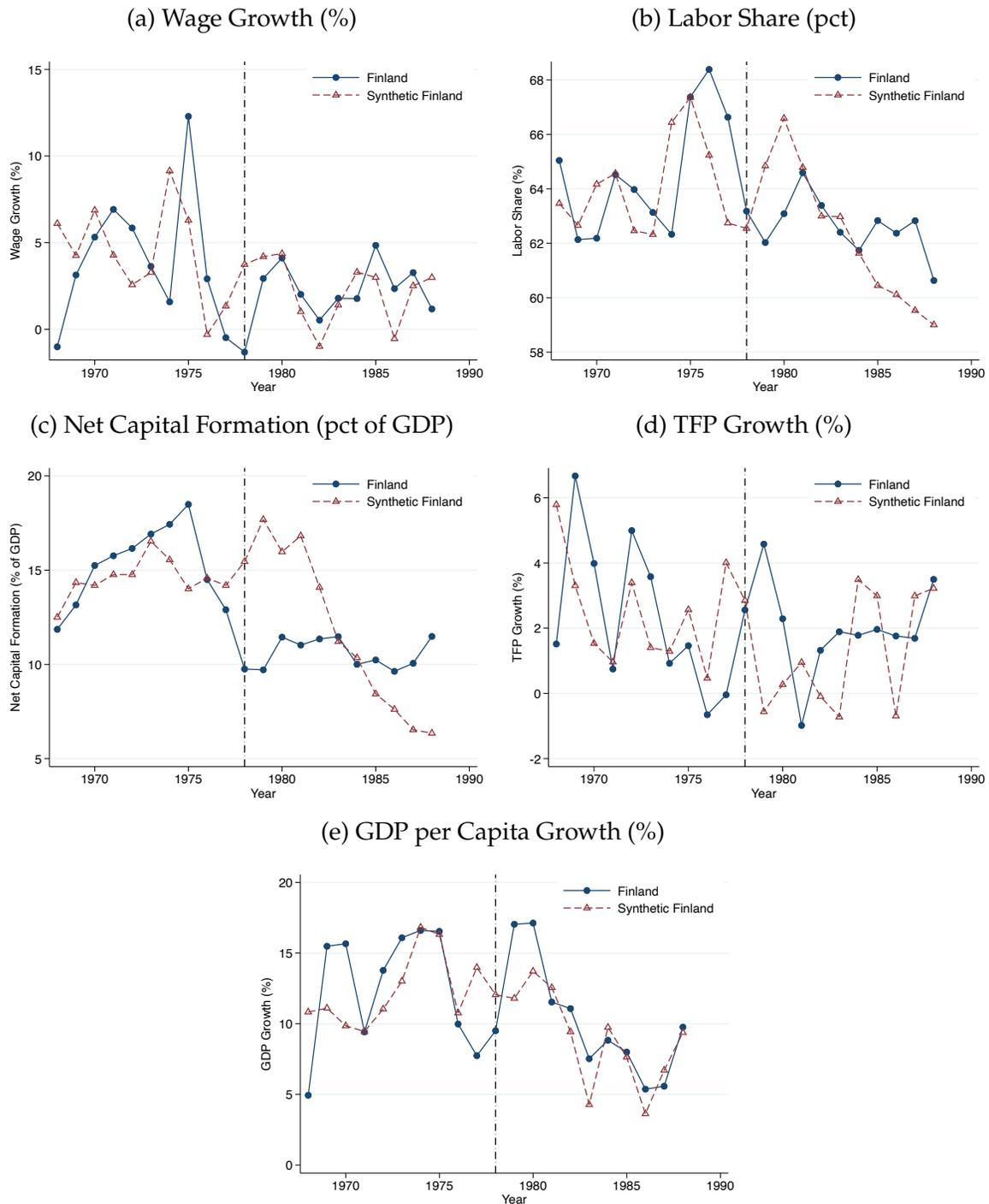
*Note:* For each of our dependent variables, this figure plots the raw time series in Austria (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Austria's 1975 codetermination reform. There is no Panel (f) because Austria is missing data back to 1960 for our "income inequality" outcome variable and is therefore excluded from the income inequality analyses.

Figure A.4: Synthetic Control Plots (Denmark 1973)



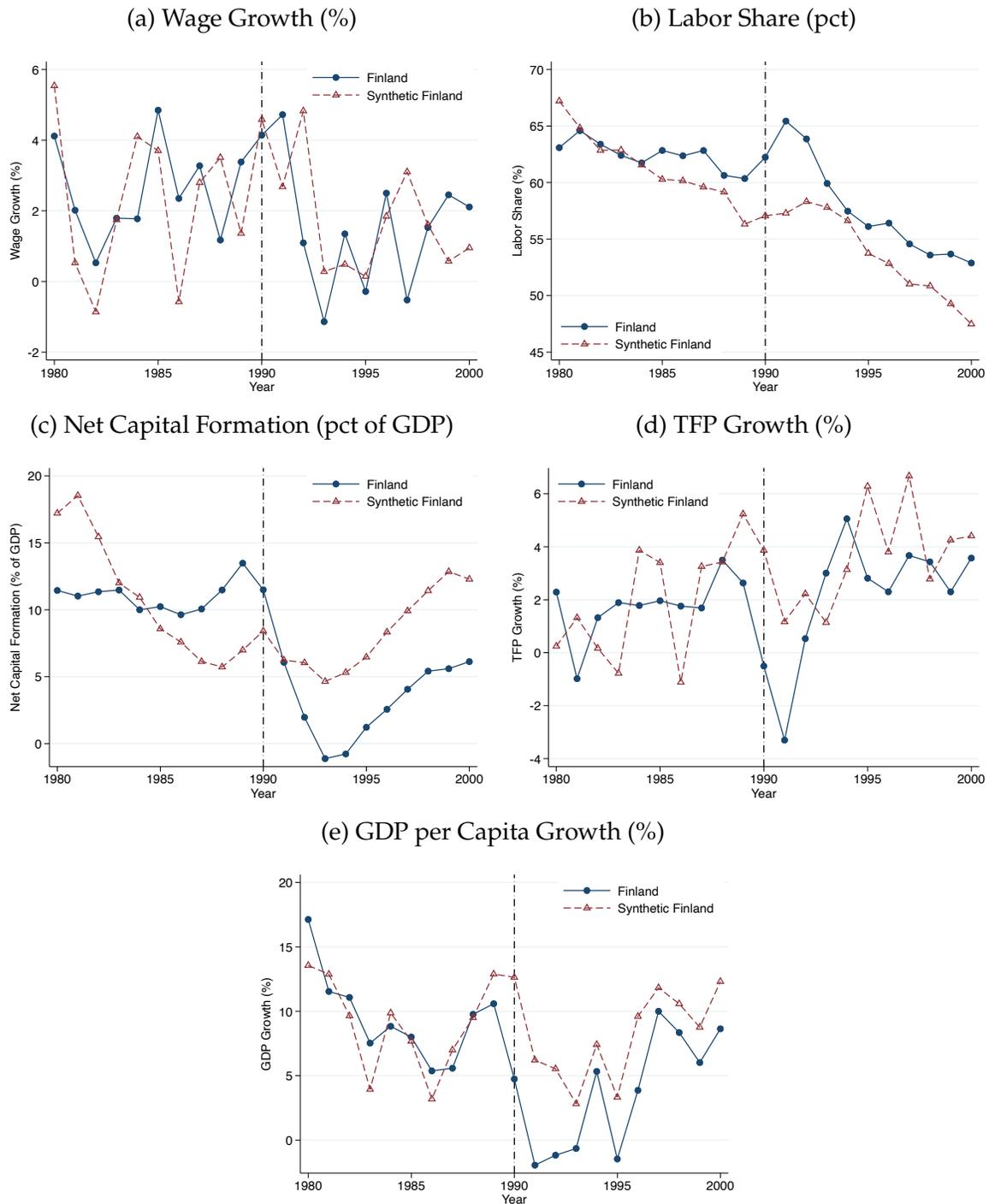
*Note:* For each of our dependent variables, this figure plots the raw time series in Denmark (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Denmark's 1973 codetermination reform.

Figure A.5: Synthetic Control Plots (Finland 1978)



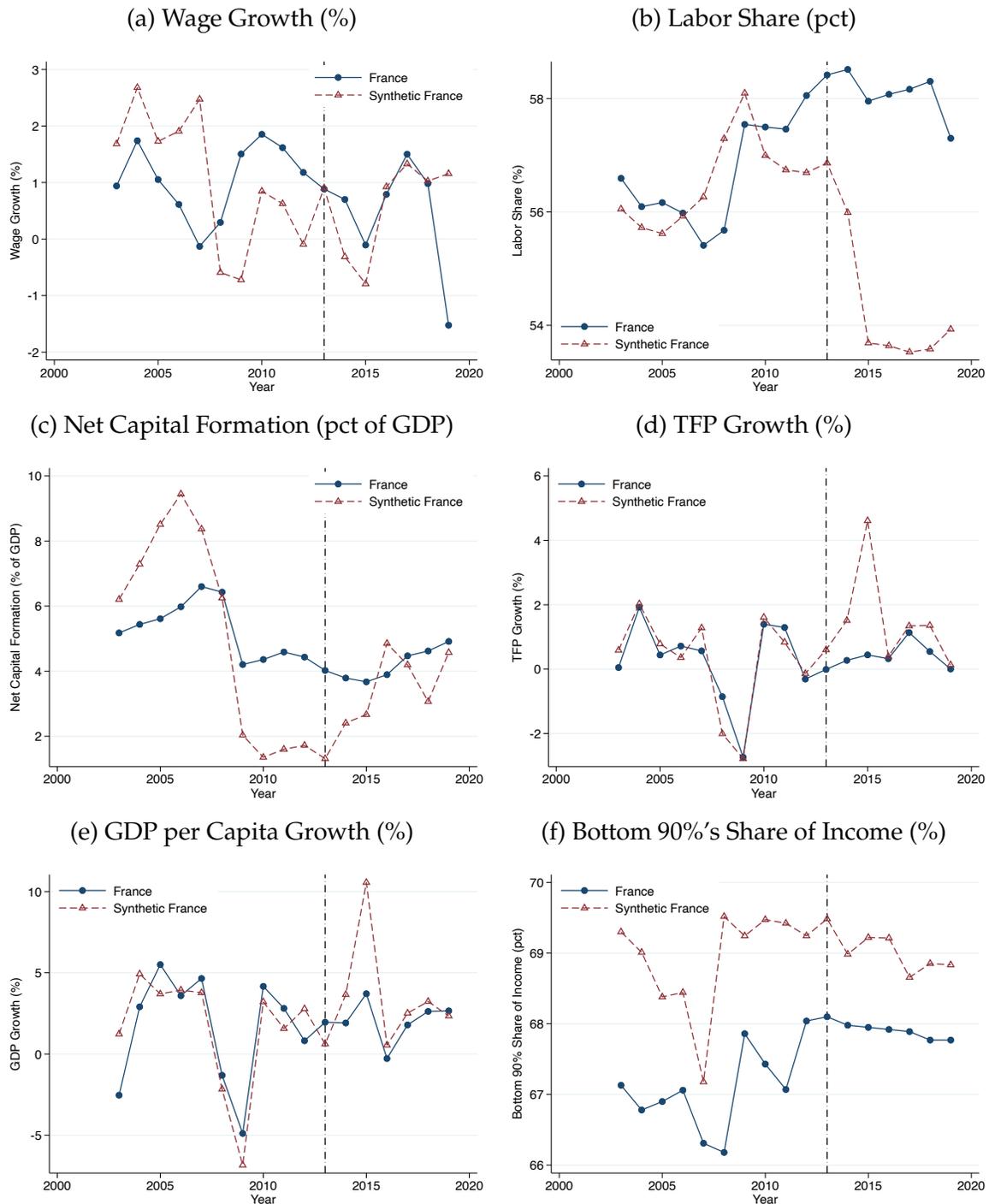
*Note:* For each of our dependent variables, this figure plots the raw time series in Finland (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Finland's 1978 codetermination reform. There is no Panel (f) because Finland is missing data back to 1960 for our "income inequality" outcome variable and is therefore excluded from the income inequality analyses.

Figure A.6: Synthetic Control Plots (Finland 1990)



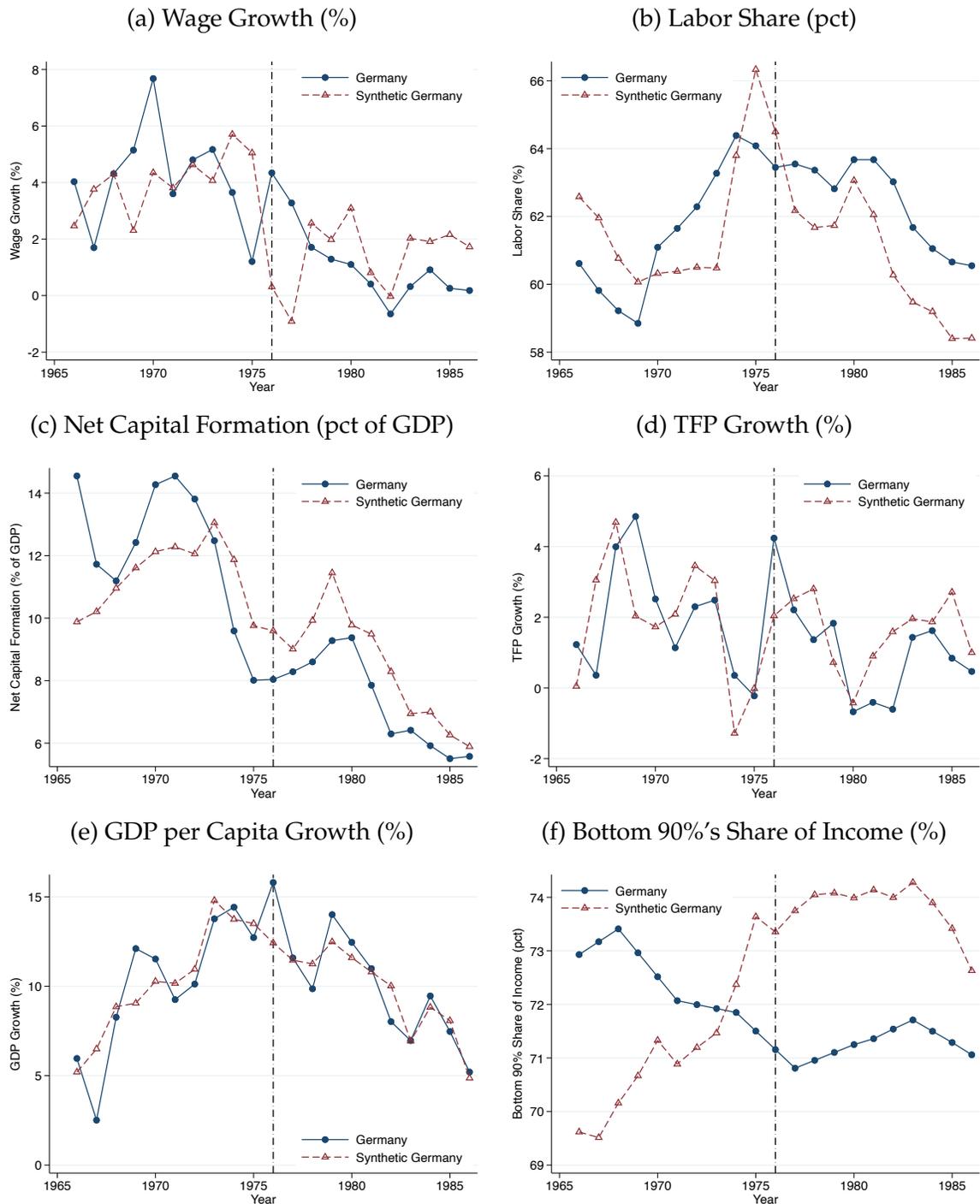
*Note:* For each of our dependent variables, this figure plots the raw time series in Finland (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Finland's 1990 codetermination reform. There is no Panel (f) because Finland is missing data back to 1960 for our "income inequality" outcome variable and is therefore excluded from the income inequality analyses.

Figure A.7: Synthetic Control Plots (France 2013)



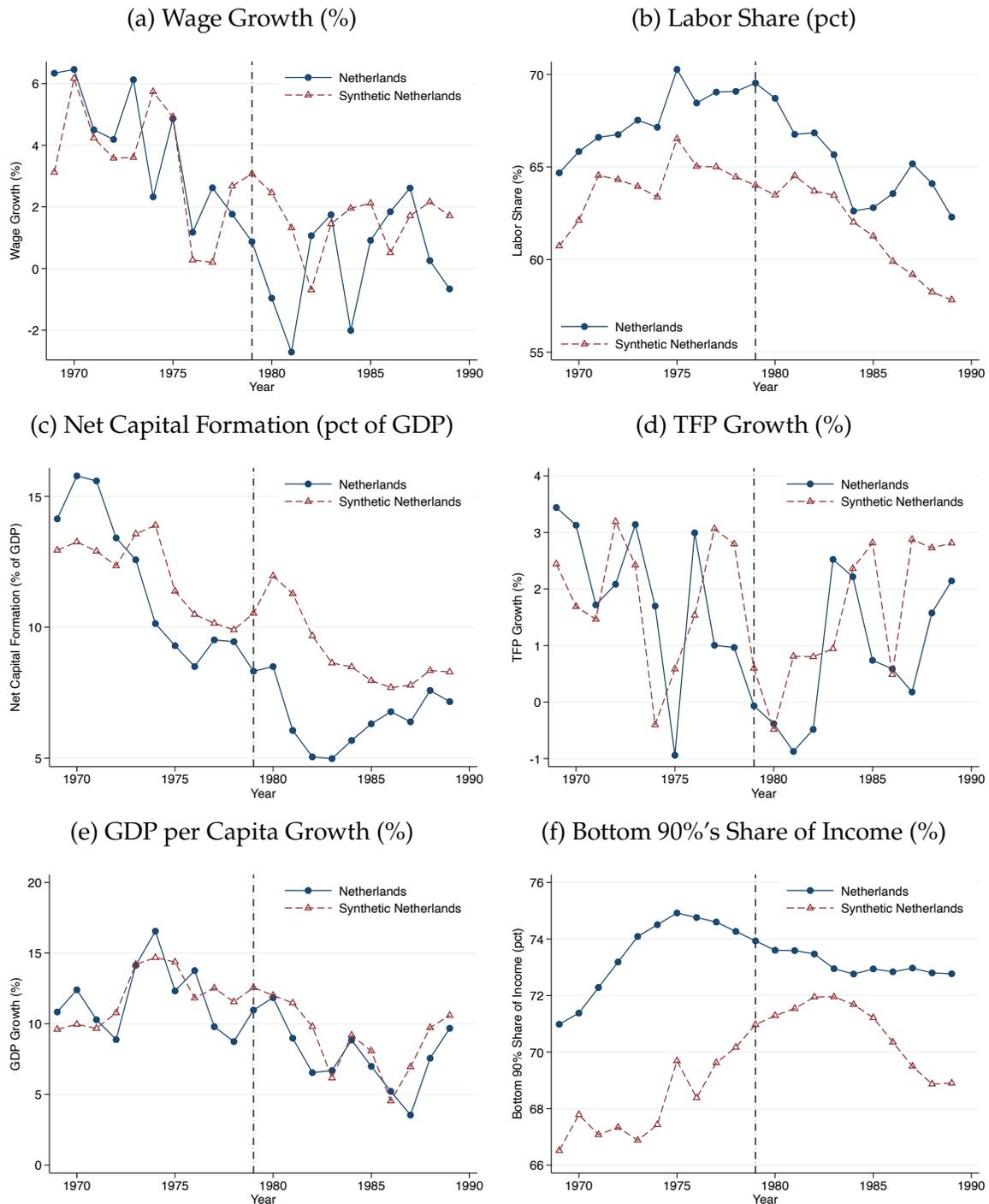
*Note:* For each of our dependent variables, this figure plots the raw time series in France (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the period surrounding France's 2013 codetermination reform.

Figure A.8: Synthetic Control Plots (Germany 1976)



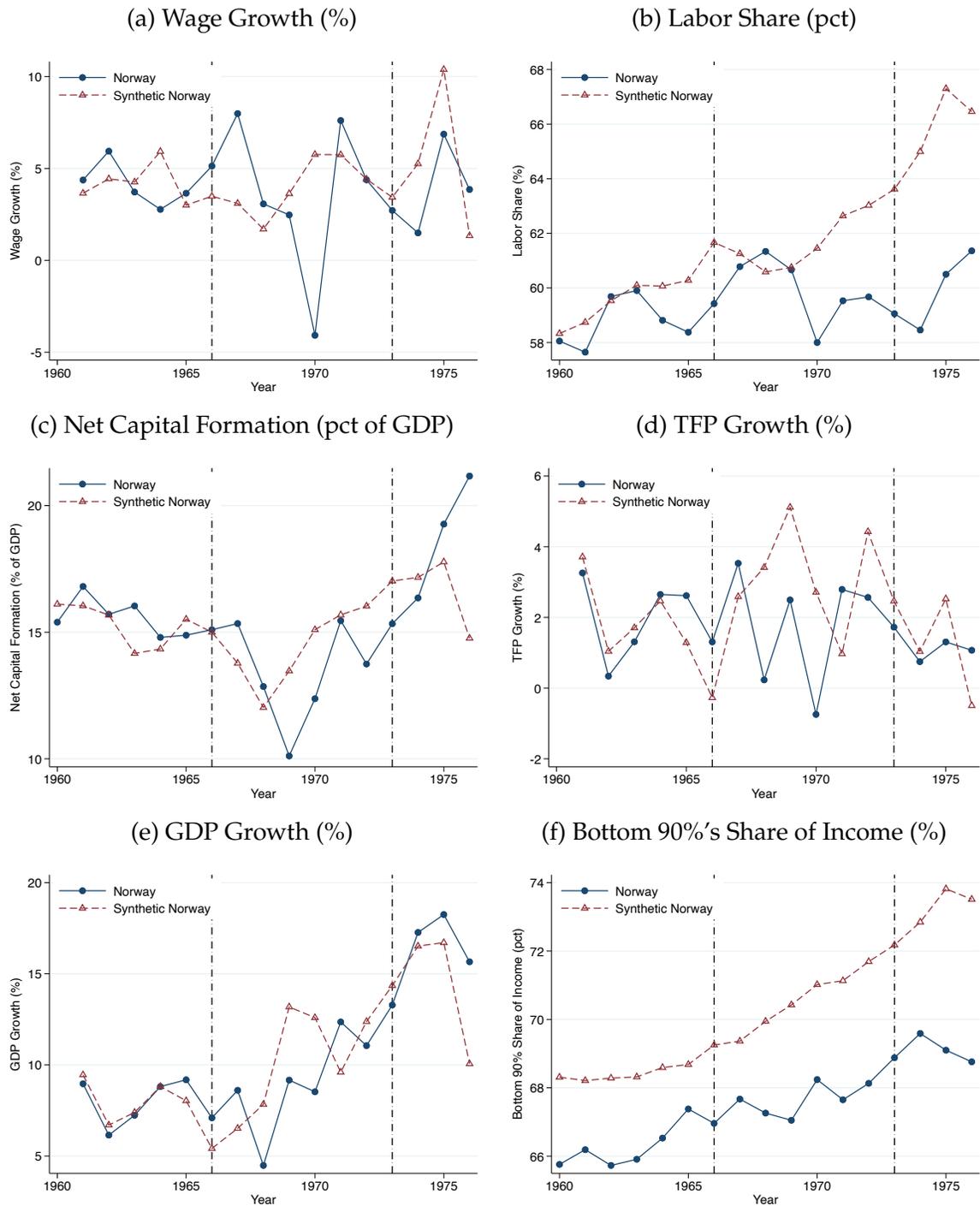
*Note:* For each of our dependent variables, this figure plots the raw time series in Germany (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Germany's 1976 codetermination reform.

Figure A.9: Synthetic Control Plots (Netherlands 1979)



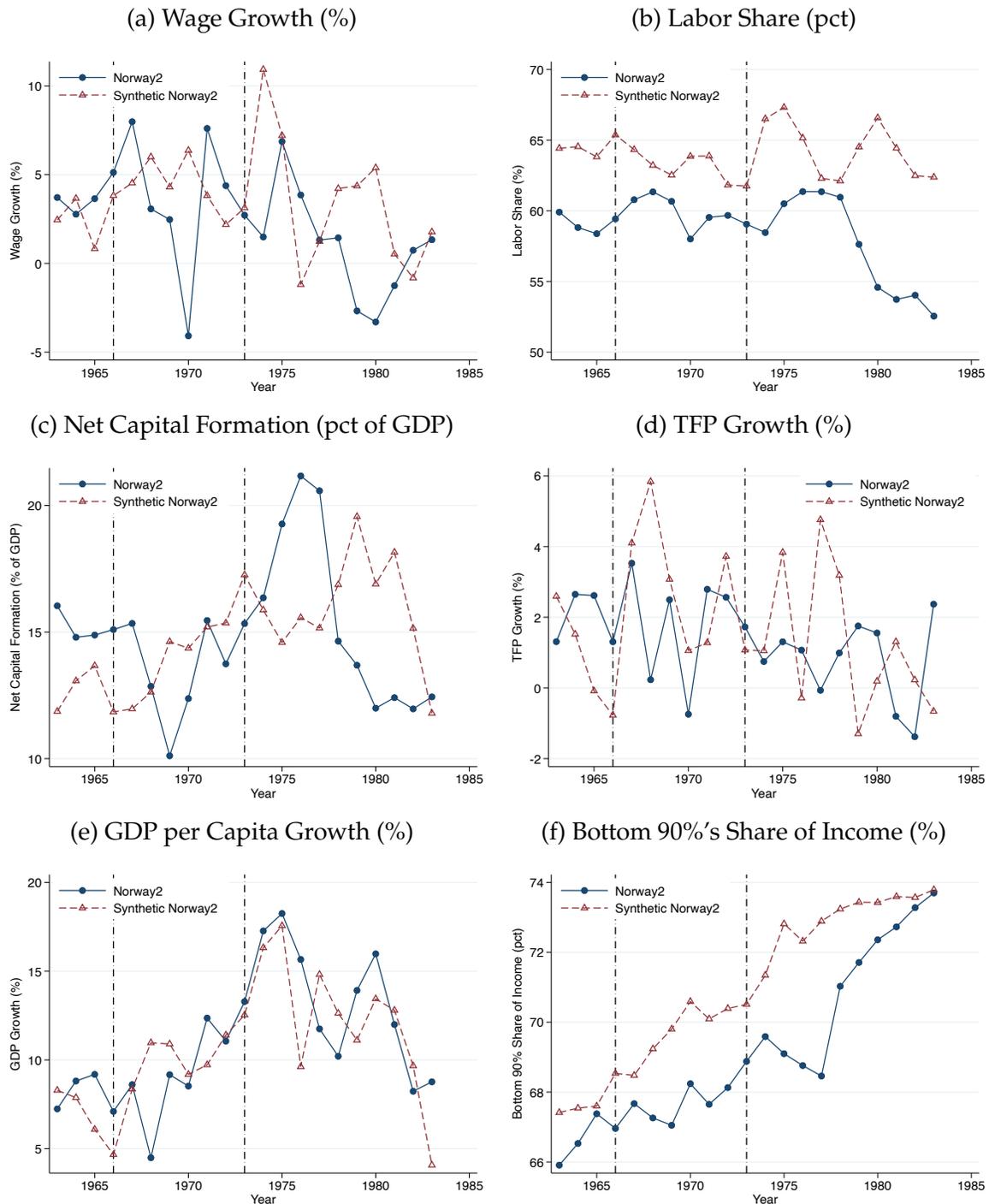
Note: For each of our dependent variables, this figure plots the raw time series in the Netherlands (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding the Netherlands' 1979 codetermination reform.

Figure A.10: Synthetic Control Plots (Norway 1966)



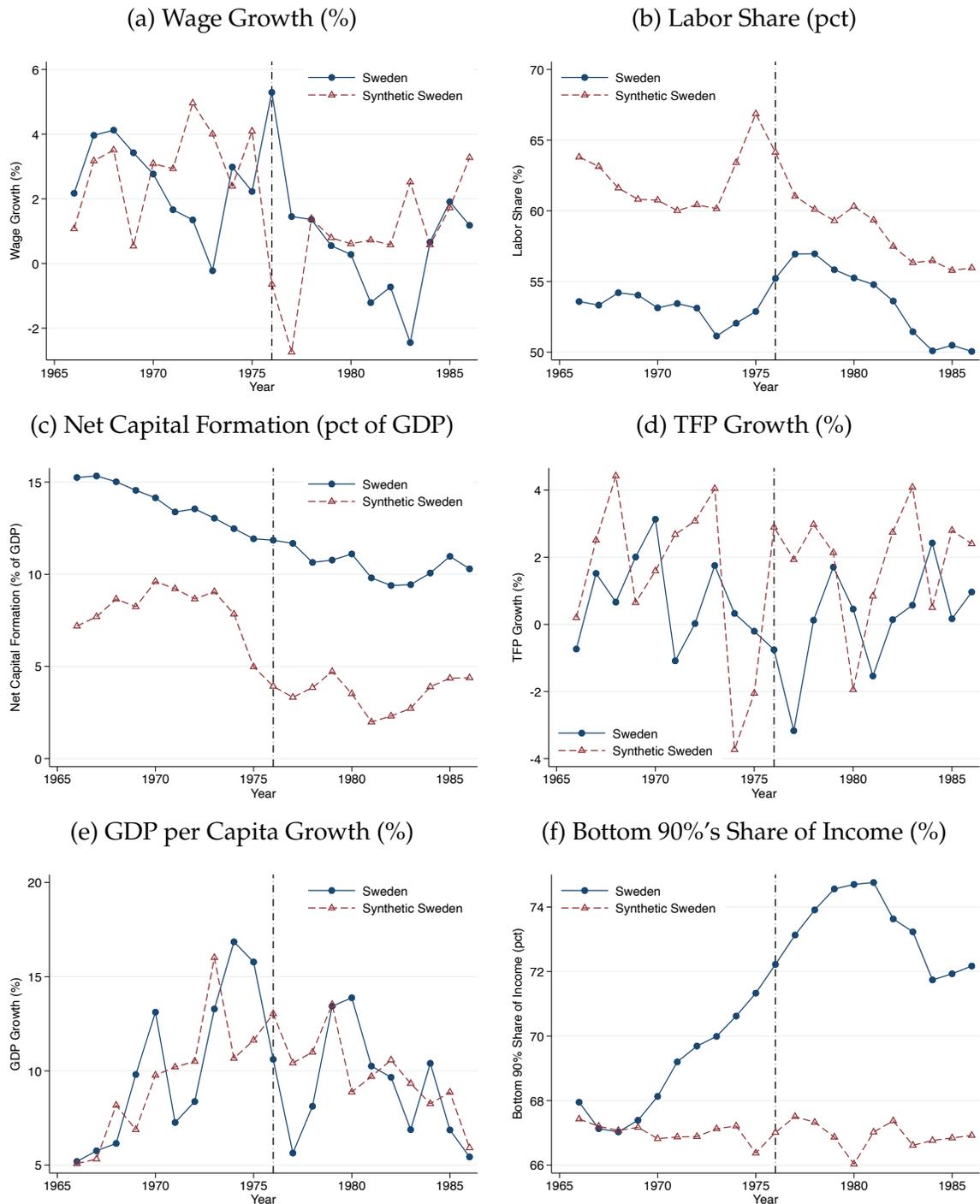
*Note:* For each of our dependent variables, this figure plots the raw time series in Norway (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Norway's 1966 codetermination reform.

Figure A.11: Synthetic Control Plots (Norway 1973)



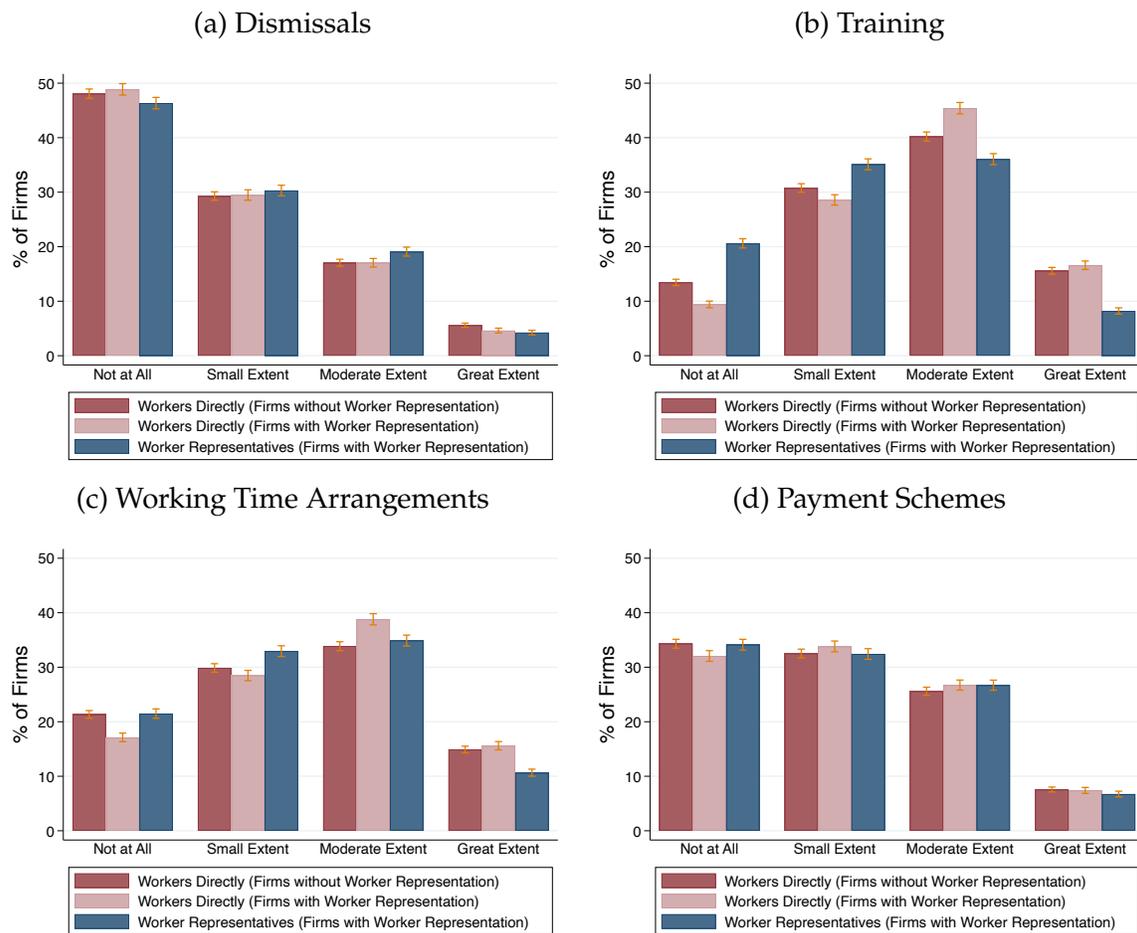
*Note:* For each of our dependent variables, this figure plots the raw time series in Norway (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Norway's 1973 codetermination reform.

Figure A.12: Synthetic Control Plots (Sweden 1976)



*Note:* For each of our dependent variables, this figure plots the raw time series in Sweden (blue, solid circles) compared to its synthetic control unit (red, hollow triangles), in the 21-year period surrounding Sweden's 1976 codetermination reform.

Figure A.13: Worker Influence in Other Areas (2019 ECS)



Note: Panel (b) of Figure 8 plots responses to a 2019 European Company Survey question about workers' influence on decisions regarding *workplace organization*. The same question is also asked about workers' influence in four other areas: dismissals, training, working time arrangements, and payment schemes. The four panels of this Figure plot the responses for those four areas.

## B Event Studies: Data and Empirical Methodology

### B.1 Sample Construction and Identifying Codetermination Reforms

In our main event study analyses, we restrict our attention to countries satisfying the following criteria:

- They are European (since our paper studies *European* codetermination in particular).
- We have non-missing data for at least some of our outcome variables for the period 1960-2019 (notably, this excludes former Soviet Bloc countries, for whom we lack data before the 1990s; it also excludes Cyprus and Malta, whose macroeconomic data series are not available until the 1990s).
- They are democracies across the entire period we study (this excludes Portugal, Spain, Greece, and Turkey).

We also exclude Luxembourg because we consider it to be economically unrepresentative. Imposing these restrictions leaves us with the following countries: Austria, Belgium, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. As we describe in Section B.6, we further narrow down our sample when analyzing our "income inequality" outcome variable.

Next, among these countries, we identify reforms that introduced or extended codetermination (either legislatively or through collective agreements). To search for codetermination reforms, we begin by examining all changes in the "codetermination" variables in the CBR Labor Regulation Index historical dataset (Adams, Bishop, and Deakin, 2016), and supplement this with detailed country-by-country internet searches. Based on this searching and qualitative evaluations of the "substantiveness" of reforms, we narrow down to the list of codetermination reforms visualized in Figure A.14.

Institutional details of these reforms are summarized in Table A.1. We exclude from this list several reforms which we do not consider to be substantive introductions or extensions of codetermination; these excluded reforms, and our reasons for excluding them, are summarized in Table A.2.

Figure A.14: Codetermination Reforms



Table A.1: Institutional Details of Codetermination Reforms

Country	Description of Reform and Institutional Background
Austria (1975)	<p>The Labour Constitution Act 1975 introduced one-third board-level representation in firms with 40 or more workers, and gave works councils the power to nominate these representatives (Adams, Bishop, and Deakin, 2016; ETUI, 2020).</p> <p>Works councils in Austria have existed since the Works Council Act of 1919, and have had extensive shop-floor codetermination powers since then (Adler, 1922). Their powers were slightly extended and clarified in the 1975 Labour Constitution Act, but the introduction of board-level representation was the Act’s primary expansion of codetermination.</p>

Continued on next page

Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
Denmark (1973)	<p>The Danish Companies Act 1973 introduced one-third board-level representation rights in firms with 35 or more employees. Take-up of this right has been moderate-to-high; about 55% of Danish workers are employed in a company with board-level representation (ETUI, 2020).</p> <p>Shop-floor representation has existed in Denmark since the early 1900s, through large-scale collective agreements between unions and employer associations. An agreement in 1900 established official shop stewards, and an agreement in 1947 established co-operation committees (similar to works councils). Denmark had no official legislation on shop-floor representation until 2005, when an EU Directive required the introduction of legislation (Knudsen, 2006).</p>
Finland (1978, 1990)	<p>A 1978 Cooperation Act introduced shop-floor representation rights in firms with 30 or more employees. In addition, a 1990 Act established board-level representation rights in firms with 150 or more employees. These reforms are both substantive, so we study each of them separately. Take-up of shop-floor representation is very high, while take-up of board-level representation has been moderate (Harju, Jäger, and Schoefer, 2021).</p> <p>Prior to 1978, codetermination did not really exist in Finland, though there was high collective bargaining coverage and high union density. Finnish unions were powerful, but favored adversarial bargaining and negotiation over codetermination. During the economic disruptions of the 1960s and 1970s, Finnish unions changed their minds and began advocating for industrial democracy. Their lobbying efforts culminated in the 1978 Cooperation Act (Sippola, 2012).</p>

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Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
France (2013)	<p>A 2013 reform introduced mandatory board-level worker representation in share-based firms with 5,000 or more workers, a threshold that was lowered to 1,000 in 2015. Data from a top group of French companies indicates that the reform had substantial bite. Prior to 2013, only state-owned or formerly state-owned French companies tended to have board-level worker representation (ETUI, 2020).</p> <p>France has had mandatory shop-floor representation in firms with 50 or more workers since 1945. The Auroux Laws of 1982 slightly extended the rights of shop-floor representatives and clarified their ability to coexist with establishment-level union representation, but did not substantially increase the strength of shop-floor representation (Fairris and Ashkenazy, 2010).</p>

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Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
Germany (1976)	<p>The 1976 <i>Mitbestimmungsgesetz</i> increased the board-level representation requirements for firms with more than 2,000 employees not in the mining, coal, or steel sectors from one-third to quasi-parity representation. As we describe in Sections 2.3 and 4, this was a major change that was heavily opposed by employer associations, and the evidence indicates that quasi-parity board-level representation is a significantly stronger institution than one-third board-level representation. In addition, our calculations from Bureau van Dijk data indicate that more than 30% of German workers are employed in firms with more than 2,000 workers, so this reform had substantial coverage.</p> <p>Board-level representation requirements were originally introduced in Germany in 1952, with firms in the mining, coal, and steel sectors required to adopt parity representation, and firms with over 500 employees in other sectors required to adopt one-third representation.</p> <p>Shop-floor representation in Germany has a long history dating back to the early 1900s. The pre-Nazi works council system was re-established in Germany in 1946 by the occupying powers, but works councils were voluntary and had no co-decision-making powers. In 1952, the Works Constitution Act mandated works councils in establishments with 5 or more employees, and gave them co-decision-making rights. This reform is too early for us to study given our data. A 1972 Act "expanded the coverage and duties of works' councils," but was not a particularly substantive extension of shop-floor representation (Havlovic, 1990).</p>

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Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
Netherlands (1979)	<p data-bbox="500 321 1442 405">A 1979 amendment to the Works Councils Act gave previously toothless Dutch works councils much broader scope and powers.</p> <p data-bbox="500 432 1442 989">Prior to the 1950s, the Netherlands had an extensive union presence but no codetermination. In the post-war reconstruction of the 1950s, workers began advocating for co-decision-making rights. In 1950, the Dutch Works Council Act required all establishments with 50 or more workers to set up a works council. However, the works councils were chaired by managers, and had only information and discussion rights, without co-decision-making powers. In 1979, the Act underwent a huge amendment, which removed managers from the works councils, broadened the works councils' scope, gave works councils consultation rights on all major decisions, and gave them a right to appeal to an employment court if the employer did not follow their advice (Eurofound, 1997).</p> <p data-bbox="500 1016 1442 1142">The Netherlands has also technically had one-third board-level representation rights since 2004, but the rights are not substantive. As van het Kaar (2007) writes:</p> <p data-bbox="578 1190 1364 1558">[...] because the individuals nominated by the works council may not be employees or trade union officials dealing with the company, they are in no sense direct representatives of the workforce. Dutch law requires all supervisory board members to act in the interests of the company as a whole; individual supervisory board members may not represent specific interests, such as the workforce or a major shareholder.</p>

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Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
Norway (1966, 1973)	<p data-bbox="500 321 1438 590">A 1966 Co-operation Agreement between union associations and employer associations mandated works councils in establishments with 100 or more employees, and gave those councils information and consultation rights. In addition, the Limited Liability Companies Act 1973 introduced one-third board-level representation rights in firms with 30 or more employees.</p> <p data-bbox="500 621 1438 890">Codetermination in Norway originated with a 1945 collective agreement establishing joint consultation committees; this was followed up by a 1957 agreement strengthening the rights of shop-floor union representatives, and the 1966 agreement, which significantly expanded shop-floor representation by establishing works councils (Bjorheim, 1974).</p>

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Table A.1—continued from previous page

Country	Description of Reform and Institutional Background
Sweden (1976)	<p>The 1976 Co-determination Act gave Swedish unions extensive consultation, negotiation, and co-decision-making powers. Establishment-level union representation is the primary channel for codetermination in Sweden; although the country also has board-level representation (introduced by a 1973 law) and works council representation, these institutions are secondary to the primary institution of codetermination through union representation. As Anders Victorin writes:</p> <p style="padding-left: 40px;">[Neither board-level representation nor works councils] form the focal point of the Swedish system of industrial democracy [...] The Swedish system is rather based on negotiations, information and collective bargaining. [...] [Through the Co-determination Act] the employer has extensive duties to inform the trade unions with which he has a collective relationship [...] it is in such negotiation that important conflicts [...] are to be resolved, not board meetings. (Victorin, 2000, p.4)</p> <p style="padding-left: 40px;">Board representation in Sweden represents a side-track in the development of industrial democracy [...] the unions look upon minority board representation more as a means of gaining information than as a means of exercising substantive influence on the decisions of a company. (Victorin, 1979, p.117)</p>

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In Table A.2, we describe European codetermination "reforms" that occur in our sample countries between 1960 and 2019 and that we exclude because we do not believe they constitute substantive introductions or extensions of codetermination.

Table A.2: Excluded Reforms

Country	Description of Reform and Institutional Background
France (1982)	Excluded for reasons described in the "France" entry of Table A.1.
Germany (1972)	Excluded for reasons described in the "Germany" entry of Table A.1.
Greece (1988, 1990)	Two Greek reforms in 1988 and 1990 established rights to form works councils, and Greece was democratic for a 10-year period preceding the 1988 reform, so we could include Greece as a treated country in our analyses despite excluding undemocratic countries from our sample. However, take-up of the right to form a works council has been virtually nonexistent due to a lack of enthusiasm on the part of both employers and workers—fewer than 2% of workplaces have an established works council (Carley, Baradel, and Welz, 2005). We therefore exclude the Greek reform, since it has not had any bite.
Italy (1970)	The 1970 Workers' Statute gave unions the right to form shop-floor representative bodies; this right was extended by collective agreements in 1993 and 2014 (ETUI, 2020). However, these bodies lack substantive codetermination rights and the real worker power in Italian workplaces lies with unions, through collective bargaining and adversarial negotiations (Degrauwe et al., 2018).
Luxembourg (1974)	Luxembourg introduced board-level and shop-floor representation laws in 1974, but we exclude it from our analysis because of its economic unrepresentativeness.
Netherlands (2004)	Excluded for reasons described in the "Netherlands" entry of Table A.1.

## B.2 Strikes Data

Our data on strikes and work stoppages are drawn from the International Labour Organization's (ILO's) Yearbooks of Labour Statistics, which have been published annually since 1936. We thank Sjaak van der Velden for sharing compiled and digitized versions of these data with us. The data are only available until 2008, so the 2013 French reform is excluded from our analysis of strikes.

The ILO collects data on strikes from national statistical agencies, and strives to make these

data as consistent as possible across countries, though some cross-country inconsistencies remain (for example, there is some variation in the criteria for a strike to be recorded).

The main variable we draw on is the "number of working days lost" to strikes and work stoppages. We apply a pair of transformations to this variable. First, we normalize it by the size of the country's working-age population in that year, using World Bank population data.<sup>18</sup> Next, we transform it into an ordinal variable. The motivation for this is that the cardinal values of the variable are extremely volatile, with massive outliers. For example, consider the raw time series for Germany, plotted in Panel (a) of Figure A.15. The variable is so volatile that even log transformations or winsorizations are insufficient to prevent outlier values from driving results.

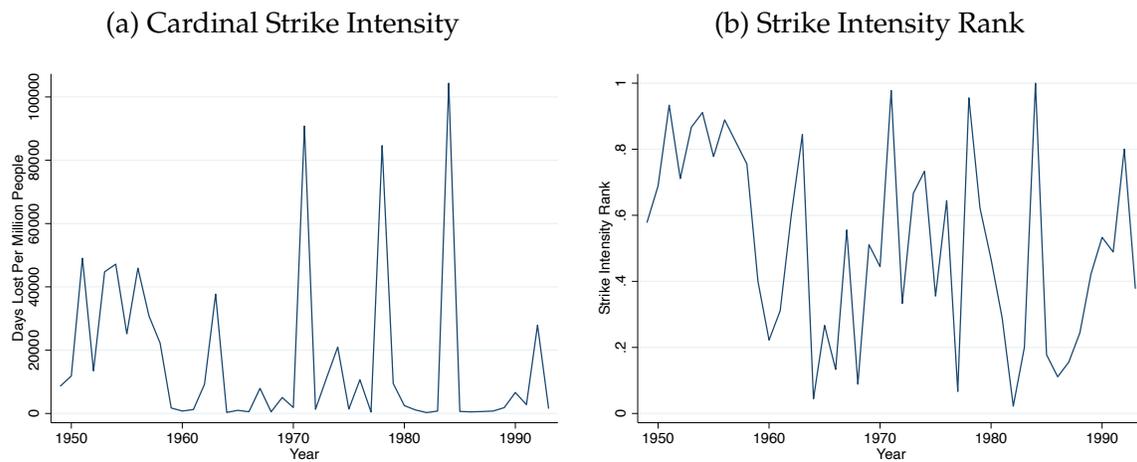
To transform from a cardinal strikes variable to an ordinal one, we use the following procedure. Within each country, we rank all years with nonmissing strikes data according to the cardinal "working days lost divided by working-age population" variable. We divide this rank by the number of years with nonmissing strikes data, to yield a variable that ranges between 0 and 1. A value of 0 represents the year with the lowest strike intensity<sup>19</sup> and a value of 1 represents the year with the highest strike intensity. We use this 0-to-1 rank variable as the outcome variable in our event study specifications, so our specifications effectively test whether the years following a codetermination reform are ranked lower or higher in terms of strike intensity than the years preceding a codetermination reform. As Panel (b) of Figure A.15 illustrates by plotting the time series of Germany's strike intensity rank, the strike rank variable is still fairly volatile but does not have large cardinal outliers that could singlehandedly drive our results.

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<sup>18</sup>World Bank population data is only available back to 1960, so prior to 1960 we use total population data from the ILO, which we have in roughly 10-15 year intervals. We linearly interpolate the ILO variable between the years for which we have values, and multiply it by the percentage of the population who are working-age in 1960 in the World Bank data.

<sup>19</sup>A value of exactly 0 can only be reached as  $t \rightarrow \infty$ .

Figure A.15: Germany Strikes Time Series



### B.3 Cooperative Industrial Relations Data

To measure cross-country differences in the cooperativeness of industrial relations, we draw on an item in the World Economic Forum’s Executive Opinion Survey. Conducted annually in about 140 countries, the Executive Opinion Survey contains the following question, with responses on a 1-7 scale:

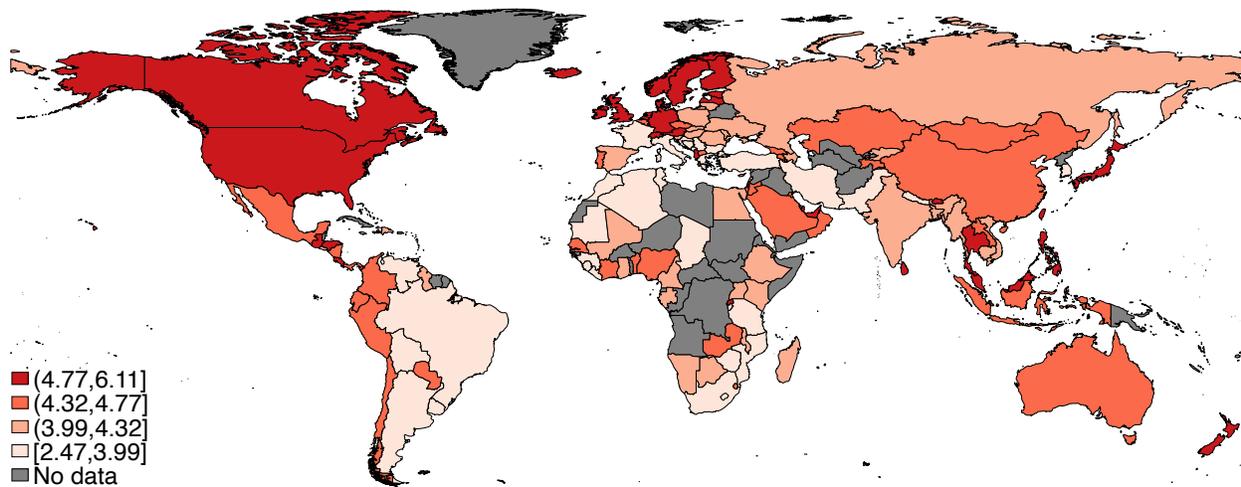
In your country, how do you characterize labor-employer relations?

Generally confrontational  1  2  3  4  5  6  7  Generally cooperative

Country-level averages from the Executive Opinion Survey are published biannually in the World Economic Forum’s Global Competitiveness Report; we have access to this data biannually since the 2007-2008 Report.

Figure A.16 maps the average cooperativeness of labor relations in each country in 2015 (the year we use for calculating the correlation between cooperativeness and codetermination laws, since our data on codetermination laws from the CBR Labor Regulation Index ends in 2015).

Figure A.16: Cooperativeness of Industrial Relations (2015)



## B.4 Union Density Data

Our data on union density are drawn from the OECD/IAIS dataset compiled by Visser (2021), which contains information about union density at the country-year level for all countries in our sample except Iceland extending back to 1960. "Union density" is defined as the percentage of a country's workers who belong to a union. Many of the countries in our sample are missing union density information for a small number of years between 1960 and 2019; we fill in these gaps using linear interpolations of the variable.

## B.5 Macroeconomic Data

Our aggregate economic variables (wage growth, the labor share, net capital formation, and total factor productivity) are drawn from the European Commission's AMECO Database, which contains macroeconomic statistics at the country-year level for European Union and some OECD countries since 1960. All currency amounts in the database are presented in Euros. The variables we use are defined as follows:

- **Wage growth** in year  $t$  is the percentage growth in average real wages in year  $t$  compared to year  $t - 1$ , i.e.  $100 \times (w_t - w_{t-1})/w_{t-1}$  if we let  $w_t$  denote average wages in year  $t$ .
- The **labor share** is total compensation of employees as a percentage of GDP.
- **Net capital formation** is net formation of fixed capital as a percentage of GDP.
- **TFP growth** is the percentage growth in total factor productivity in year  $t$  compared to year  $t - 1$ , defined identically to growth in real wages.

- **GDP growth** is the percentage growth in GDP per capita in year  $t$  compared to year  $t - 1$ , defined identically to growth in real wages.

We choose to specify our wage, TFP, and GDP variables in terms of growth rates rather than levels for reasons explained in Footnote 10.

## B.6 Income Inequality Data

Our data on income inequality are drawn from the World Inequality Database. We draw on the most extensively available measure of income inequality in the database, which is the share of a country's national income held by the bottom 90%, where the population consists of equal-split adults.

Data back to 1960 are available only for some of the countries in our main sample: Denmark, France, Germany, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom, covering 7 of our 10 codetermination reforms. In our analyses of income inequality, we restrict to these countries, and add the Australia, Canada, and the United States as additional comparison countries to compensate for our loss of countries.

## B.7 Empirical Methodology

### B.7.1 Constructing Synthetic Control Groups

For each country-reform and each outcome variable, we construct a synthetic control group using the following procedure. For ease of exposition, suppose we are constructing a synthetic control group for Austria's 1975 reform, for the wage growth variable.

We begin by identifying all other countries in our sample that do not experience a codetermination reform between 1965 (10 years pre-reform) and 1985 (10 years post-reform). Recall that, as described in Appendix Section B.1, our sample consists of European countries that were independent and democratic from 1960-2019 and that have non-missing macroeconomic data for most of that period (Austria, Belgium, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom).

Notably, the pool of comparison countries for Austria includes both countries that never have codetermination laws across this 21-year period, *and* countries that always have codetermination laws over this period but do not experience a reform in this period. In the case of the 1975 Austrian reform, the comparison countries are the United Kingdom, Italy, and Ireland (which never have codetermination), as well as Belgium (which has shop-floor representation from 1948 onwards; Hurley, 1953) and France (which has shop-floor representation from 1945 onwards, and does not have board-level representation until 2013—see Appendix Table A.1). Iceland is not included because its data on wage growth start only in 1970, halfway through the pre-treatment period.

We select synthetic control weights for the comparison countries by matching on the pre-reform levels of the following aggregate outcome variables: wage growth, the labor share, net capital formation, TFP growth, and GDP growth (i.e., exactly the outcome variables we study).<sup>20</sup> This method of selecting weights gives us a synthetic control unit that resembles the treated country in terms of pre-reform growth in GDP, productivity, and wages, and in terms of the pre-reform levels of the labor share and capital formation.

In the case we're considering, the matching procedure assigns a weight of 0.026 to Belgium, a weight of 0.527 to France, a weight of 0.059 to Ireland, a weight of 0.357 to Italy, a weight of 0.031 to the United Kingdom, and weights of zero to the other control countries.

### B.7.2 Difference-in-Differences Specifications

This section reiterates and expands on the explanation of our regression specifications given in Section 5.

After constructing a synthetic control unit for each country-reform, we then pool together all of our country-reforms and synthetic control units, and run difference-in-differences regressions comparing the outcomes of the treated and control groups before and after the codetermination reforms. The regression equation is as follows:

$$y_{ik} = \alpha_i + \beta_k + \theta_{\text{Year}(ik)} + \sum_{s=-10, s \neq -1}^{10} \tau_s^{\text{Treated}} \times \mathbb{1}[k = s] \times \text{Treated}_i + \varepsilon_{ik}, \quad (\text{A.1})$$

where  $y_{ik}$  denotes the outcome for country-reform  $i$  in year  $k = t - \text{reformyear}_i$  relative to the reform occurring in  $\text{reformyear}_i$  (the reform year for synthetic control units is set equal to their respective country-reform's reform year). The  $\alpha_i$  are unit (country-reform) fixed effects,  $\beta_k$  are event time fixed effects,  $\theta_{\text{Year}(ik)}$  are calendar year fixed effects (e.g. "1975"),  $\mathbb{1}[k = s]$  is an indicator for being in the  $s$ th year relative to the reform, and  $\text{Treated}_i$  is an indicator for unit  $i$  being a treated country-reform. The  $\tau_s^{\text{Treated}}$  are the coefficients of interest, and represent effects relative to the omitted  $s = -1$ . Standard errors are clustered at the country-reform level.

In addition to plotting the full set of dynamic coefficients  $\tau_s^{\text{Treated}}$ , we also report average pre-reform and post-reform coefficients, which are  $\tau_{\text{Pre}}^{\text{Treated}}$  and  $\tau_{\text{Post}}^{\text{Treated}}$  in the following regression:

$$y_{ik} = \alpha_i + \beta_k + \theta_{\text{Year}(ik)} + \tau_{\text{Pre}}^{\text{Treated}} \times \left( \mathbb{1}[k < -1] \times \text{Treated}_i \right) + \tau_{\text{Post}}^{\text{Treated}} \times \left( \mathbb{1}[k \geq 0] \times \text{Treated}_i \right) + \varepsilon_{ik}, \quad (\text{A.2})$$

where notation is the same as above. Due to the small number of clusters, we report p-values for the pre-reform and post-reform coefficients calculated using the wild bootstrap method,

<sup>20</sup>Using the `synth` command in Stata (Abadie, Diamond, and Hainmueller, 2011).

following Cameron, Gelbach, and Miller (2008) and using the code developed by Roodman, Nielsen, MacKinnon, and Webb (2019).

In addition, we report average post-reform coefficients calculated using the imputation methodology developed by Borusyak, Jaravel, and Spiess (2021), which deals with problems arising when two-way fixed effects models are used in contexts with heterogeneous treatment times, like the context we consider. The imputation methodology proceeds as follows. First, we obtain estimated unit and time effects  $\hat{\alpha}'_i$  and  $\hat{\beta}'_k$  from a regression of the outcome variable on unit and time fixed effects, for untreated observations only:

$$y_{ik} = \alpha'_i + \beta'_k + \varepsilon_{ik}, \quad (\text{A.3})$$

where notation is as above, and the regression is restricted to observations  $ik$  that satisfy  $\text{Treated}_i = 0$  or  $k < 0$ .

These estimated unit and time effects are used to impute a potential outcome in the absence of treatment for each treated observation  $ik$ , equal to  $\hat{\alpha}'_i + \hat{\beta}'_k$ . This imputed potential outcome is converted into an imputed treatment effect  $y_{ik} - \hat{\alpha}'_i - \hat{\beta}'_k$ , using observation  $ik$ 's actual outcome. An overall treatment effect for each post-reform period is then calculated by taking the average of the unit-specific treatment effects for that period. Standard errors for these coefficients are calculated using a methodology that clusters at the country-reform level. Coefficients and standard errors calculated using this method are printed in the panels of Figure 6.

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