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Labour market power: New evidence on Non-Compete Agreements and the effects of M&A in the UK

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Abstract

Monopsony power is an important feature of modern labour markets. We examine its impact on workers. We report the first representative survey of Non-Compete-Agreements (NCA) in the UK and find that about 26% of workers appear to be covered, a higher fraction than in comparable surveys in the US (18%) and Italy (16%). Although NCAs are more prevalent for skilled workers, a large number of low skilled workers are also subject to NCAs (e.g. over a fifth of plant operators). Moreover, although NCAs are associated with higher training (conditional on other measures of skills), we argue that such benefits are unlikely to justify their high prevalence. Finally, we examine the impact of over 2,000 M&A UK panel data between 1997 and 2022 (over 900,000 observations). The data suggests that M&A tends to reduce employment growth in the merged entity (from 3% a year prior to the merger to about zero in the subsequent five years), particularly in target firms. However, there is no evidence of any falls in average wage growth (in acquirer or target) as monopsony would predict – if anything, average wages are higher. Nor does profitability or productivity change post-merger.

Keywords: management practices, productivity, competition

JEL Codes: L2; M2; O32; O33

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1. Introduction

There is growing global concern about reduced firm competition, both in terms of monopoly and monopsony (de Loecker, Obermeier and Van Reenen, 2022), fuelled by the increasing size of large companies, the rise of firm concentration, and higher aggregate profit mark-ups. This paper examines whether there is evidence of firm labour market power affecting wages and jobs in the UK.

On July 9, 2021, United States' President Biden signed an Executive Order (EO 14036) (The White House, 2021) to promote competition in the U.S. This was not confined to labour market power, but it was clearly an important focus. Section 1 of the Order states “Consolidation has increased the power of corporate employers, making it harder for workers to bargain for higher wages and better work conditions”. Following this, the US Federal Trade Commission (FTC, 2023) proposed banning non-compete agreements (NCAs), claiming that “non-competes block workers from freely switching jobs, depriving them of higher wages and better working conditions, and depriving businesses of a talent pool that they need to build and expand.” Concurrently, the last decade has seen a rising tide of labour market concerns from competition authorities globally, and an increased focus on no poach and wage fixing agreements, with authorities such as the Brazilian Conselho Administrativo de Defesa Econômica, Romanian Consiliul Concurenței, Portuguese Autoridade da Concorrência and a variety of other authorities launching different investigations (see Appendix A).

We address this issue in four ways. First, in Section 2 we present the institutional background on non-competes and no poach agreements which involved interviews with leading experts in the field. Second, in Section 3, we present a review of the theoretical and empirical literature on labour market power. Our main contributions are to provide new empirical evidence. Boeri, Garnero, and Luisetto (2022) surprised many by presenting new survey evidence that two million Italian private sector employees were under NCAs. We designed and implemented a very similar survey on a representative sample of almost 2,000 UK workers. In Section 4, our analysis of this data finds an even higher prevalence than in Italy

(and the US) of around 26% of UK employees being likely covered by an NCA. We describe who is covered by such agreements. More skilled workers, whether measured by education, occupation, industry or income, are more likely to be covered. But large fractions of unskilled workers are also covered, which is harder to understand. We find that employer-provided training is more likely to be provided when an employee is covered by an NCA (even controlling for skills), which may be a partial explanation. In Section 5, we provide new econometric analysis on the effects of M&A on wages and jobs. We examine over 2,000 UK merger and acquisition (M&A) events between 1997-2021 on over 900,000 observations. Using an event study analysis, we find that employment growth slows from about 3% a year before the merger to essentially zero five years after the merger. This was particularly strong in the target firms. By contrast, average wage growth is as strong (or stronger) before as it is after the merger. Section 6 offers some concluding comments.

2. Some Institutional Background

The terms of employment contracts include not only clauses defining an employer's remuneration and employee's contractual job duties, but also various clauses restricting employee behaviour and options after leaving the firm (USDT, 2022). There can be efficiency arguments for these covenants and in a perfectly competitive labour market jobseekers make free choices to accept or reject offers. Moreover, since workers dislike restrictions on their behaviour, they will have to be compensated in other ways to induce them to accept such a job offer (e.g. higher remuneration). On the other hand, if there are imperfections in the labour market, for example due to search and matching frictions, these covenants can be a way of exercising employer power. Table 1 shows six types of restrictive employment agreements, but the paper will focus on NCAs and no poach clauses.

2.1 Non-Compete Agreements

NCAs can encourage companies to invest in employee development by mitigating the 'hold-up' effect. This effect refers to situations where firms are reluctant to invest in their

employees (such as through training) because they fear that employees may leave the firm and take the value of the investment with them before the firm can derive a direct benefit from it (Rubin & Shedd, 1981). They are often used in high-tech industries to protect intellectual property, which can potentially increase innovation incentives.

Table 1: Type of restrictive employment agreement

<i>Types</i>	<i>Details</i>
<i>Non-compete agreements</i>	An ex-staff member is prohibited from being employed by competitors after their departure. This restriction usually has a defined duration, geographic scope, and pertains to a particular sector.
<i>No poach agreements</i>	Multiple employers come to an agreement not to recruit or employ each other's present or past staff members.
<i>Non-disclosure agreements</i>	Prevents current or former employees from revealing information that is considered both confidential and valuable in order to protect it.
<i>Training repayment agreements</i>	If an employee leaves their job before a certain time period, they are obligated to reimburse the employer for the cost of any training they received. This reimbursement agreement is usually calculated based on the duration of the employee's tenure after completing the training.
<i>Non-recruitment agreements</i>	An employee or former employee is prohibited from enticing or hiring the employees of their employer away from the employer for a certain period.
<i>Non-solicitation agreements</i>	After leaving a company, an employee agrees not to contact or attract the clients or customers of that company for their own or a competitor's advantage.

Notes: The highlighted restrictive agreements are those of focus in this paper.
Source: (USDT, 2022)

A cost of these agreements is that they limit available options for alternative employment. Typically, these restrictions are imposed within a similar set of industries or occupations in

a specific geographic location for a defined period, normally between six to twelve months in the UK. However, they can lead to negative outcomes. Balasubramanian et al. (2017) argue that the narrowing of outside options through NCAs weakens employee's bargaining power in relation to their employers which creates a 'lock-in' effect, leading to reduced worker mobility, longer job tenure, and stagnant or declining wages. NCAs can therefore encourage companies to invest in their employees while also creating frictions in the labour market (McAdams, 2019). In addition, although innovation incentives may be enhanced for a given firm, the presence of NCAs slows the diffusion of innovation across an economy, which may in fact reduce aggregate innovation.

Non-compete contract enforceability in the UK is a relatively grey area. To make an individual follow a non-compete the employer would need to take them to court, where they would need to prove that the non-compete is "reasonable". Some justifications, for example approaching former customers, confidentiality and starting a competing business are often seen as legitimate, but the scope of these contracts can extend to not even working in the same region, in the same industry or for any competitor (Citizens Advice, 2023).

The debate surrounding the regulation of non-competes has gained traction over recent years with regulatory bodies in several countries actively tackling the matter. Most notably, the United States' Federal Trade Commission proposed banning non-competes for millions of Americans in January 2023 (FTC, 2023). The ban is intended to apply to anyone working for an employer, paid or unpaid, as well as to independent contractors. The FTC is currently seeking public comment on its suggestion of a general ban with its implementation pending.

The UK's Department of Business and Trade is working on limiting the maximum length of non-competes to three months. It started seeking consultation on possible regulation of NCAs in late 2020. In May 2023 it presented policy measures to strengthen employment standards (UK Department of Business and Trade, 2023), And after considering mandatory compensation and a complete ban, the proposal now intends to limit the maximum duration of non-compete clauses to three months. This limit was chosen to enhance flexibility and innovation on the one hand while preserving employer freedom to negotiate such clauses on the other.

2.2 No poach Clauses

No poach agreements are in principle illegal in the UK, but no poach agreements between franchisors and franchisees are allowed according to the British Franchise Association (BFA). This prevents one franchise from hiring another franchise's employees within the same brand, which potentially reduces wages by prohibiting franchises from competing for workers. These situations may create a vicious cycle: workers are trained in the specific context for learning the nuances of working in a specific brand environment, for example via McDonalds' "Hamburger University". But these workers are restricted from moving to another McDonalds location, and their training may be inappropriate for moving to another competitor, such as Burger King. Since the franchisor knows that their workers cannot easily move to another franchise, they may offer lower pay.

Employees are not privy to these agreements and are often unaware of their existence despite the role they play in narrowing current and future employment opportunities. Tracking their prevalence and impact on a market remains a challenge for another reason: if evidence of collusion is found between employers regarding hiring practices, it could be unlawful under the Sherman Antitrust Act in the United States and the Competition Act (1998) in the UK.¹ Wage fixing between firms has been investigated by the CMA as recently as July 2022. While it is difficult to discern the scope of collusion, the high-profile anti-trust case in Silicon Valley which included Apple and Google has affirmed that it is a pertinent regulatory concern (Krueger and Ashenfelter, 2018).

The ubiquity of no poach agreements is central to their impact. In a study analysing 2016 Financial Disclosure Documents from 156 of the largest franchise chains in the United States, 58% of major franchise contracts contained a no poach agreement, including McDonald's, Burger King, Jiffy Lube, and H&R Block (Krueger and Ashenfelter, 2018). These contracts placed limits on the ability of franchises within the same chain to employ workers from each other. Until the Assurance of Discontinuance in the US, no poach clauses were also becoming

¹ Non-competes are agreements between and employer and employee and so generally out of scope for competition authorities, whereas no-poach are agreements between employers so firmly in-scope.

more common over time; one may expect that other markets that have franchise no poach clauses but have not had a similar agreement would see saturation of these clauses. Evaluating data for the 45 largest US franchisors in 1996 reveals that 35.6% of franchisors had a no poach clause, compared with 53.3.% in 2016 (Krueger and Ashenfelter, 2018).

3. Literature Review

3.1 Theory

The labour market matches workers to jobs with firms providing pay and other benefits in return for labour time. We would like a well-functioning employment market to deliver good-quality jobs with low frictions to job mobility, thus enhancing aggregate productivity. Companies try to recruit employees who are best suited to their needs and job seekers try to find jobs that satisfy their desires. In a perfectly competitive (frictionless) labour market workers are paid the value of their marginal revenue product of labour (MPL), i.e., the market value of their contribution to production, subject to any compensating differentials. While this is a useful benchmark, labour markets rarely function like this. Various frictions such as job search costs over time, commuting costs and limited information of better outside options create frictions in the matching process between employees and firms. These frictions can enable firms to exert wage-setting power in the labour market, and they may also seek to increase these frictions in order to enhance their market power.

Robinson (1933) coined the term “monopsony power” to indicate the market power for firms over inputs like labour, just as monopoly power gives firms power over consumers in the product market. Absent differential job amenities, a perfectly competitive labour market delivers a uniform wage for a worker of a given skill type, so any attempt to cut the wage below this competitive wage will lead to all workers leaving the firm immediately. In a monopsonistic labour market this is no longer the case. The firm faces an upward sloping labour supply curve, so that cutting the wage below the competitive level will lead to the loss of some, but not all workers. The less sensitive workers are to wage changes, the more

monopsony power a firm will enjoy (and the greater the wage mark-down with respect to the MPL).

Wage setting power in labour markets is analogous to price setting power under imperfect competition in product markets. Here, a firm with a degree of market power faces a downward sloping demand curve, so that a price increase leads to the loss of some, but not all consumers. In the context of monopsony, the losses to workers in terms of wages can include other forms of compensation such as working conditions, benefits, and job quality.

There are several approaches to examining monopsony power. In the classical case, there is only one viable employer for a worker, such as in the case of a “company town” where workers are tied to a specific location (due to high geographic mobility costs or having very specific skills). This is the case of a single-firm monopsonist. More generally, a large share of jobs in a small number of firms (high labour market concentration) may also cause monopsonistic power as it limits the choices a worker has over where to work. This motivates the concern arising from “superstar” firms expanding their market share and industries becoming more concentrated (e.g. Autor et al., 2020). Traditionally, competition authorities have been relaxed about this issue as the consensus was that there were sufficient alternative firms to work for. However, if workers cannot easily move across areas or have skills that are not easily transferable between industries and firms, then the effective number of alternative employers may be small. Additionally, it might be easier for employers to collude (explicitly or tacitly) to keep wages low (e.g., through “no poach clauses” discussed below) when labour markets are more concentrated as coordination between them is simpler.

More recently, there have been two approaches to understanding monopsonies. The first class of models (Card, Heining and Kline, 2018) emphasises that workers are not only concerned about pay, but they also have preferences regarding other factors such as the nature of the work, company culture, co-workers, managers, commute times. Different workers may value these aspects differently. If a worker highly values a particular aspect of a job, like commuting time to the workplace or an inclusive company culture, they may accept a lower wage than other workers for the same position. This allows the employer to reduce compensation and still retain workers. These often generate “monopsonistic competition”

models where there are a large number of firms competing, but each has a local monopsony over particular groups of workers due to jobs being differentiated. Similar to the classical approach, this leads to firms facing upward sloping labour supply curves rather than constant ones.

The second approach comprises the “search and matching” models of labour markets (Burdett & Mortensen, 1998; Manning, 2003). These types of models incorporate the notion that there are search frictions and opportunity costs associated with job searches from both the perspectives of workers and firms. Some frictions that job seekers face occur naturally, but employers may intentionally amplify these frictions in order to increase their bargaining power. "Natural" frictions refer to those that are not intentionally created by employers. For instance, the high financial and interpersonal expenses associated with relocating may cause an individual to remain in their current job despite having better options elsewhere. A major friction is information asymmetry about wages. Systematic underestimation of wages paid by competitors would make workers less likely to switch jobs due to lack of awareness about external options. These information asymmetries are more pronounced for workers than they are for firms, with (Jäger, Roth, Roussille, & Schoefer, 2021) finding that 10% of jobs in Germany could not continue paying their current wages if employees had a proper understanding of their outside options. The authors suggest that lower-paid jobs are especially vulnerable to this.

As a result, employers who are aware of these costs can set wages below the competitive equilibrium. Hence, when firms post a wage below the competitive level, many workers are unsure whether they can find a better option and are unprepared to pay search costs to find out. This enables low wage firms to co-exist with high paying ones.

Both these classes of modern monopsony models do not rely on small numbers of firms in the labour market, as traditional models do.

3.2 Empirics

There are four broad empirical approaches to looking at the issue of monopsony: (i) estimating wage mark-downs; (ii) analysing how concentration (often induced by mergers) affects wages; (iii) looking at the impact of non-competes and (iv) examining no poaching agreements. For an excellent overview of the evidence in the UK, see Competition and Markets Authority (2024).

Mark-Downs

Card (2022) summarizes the history of economic literature on monopsony and labour market frictions. A key measure of monopsony is the mark-down of wages under MPL, which will in general be related to the elasticity of the firm-specific labour supply curve. The more inelastic this is (i.e. the less responsive workers are to changes in their offer wages), the larger will be the mark-down. How can such mark-downs be estimated? Manning (2011) discusses many methods, perhaps the most common being the examination of the relationship of quit, recruiting and application elasticities to the wage. A recent example in the UK is Datta (2023) who uses human resources data for a large multi-establishment company in the UK. He generates exogenous variation in wages to different establishments through two methods: (i) a public sector wage floor that affects outlets supplying the government, but not the private sector and (ii) idiosyncratic “mistakes” in the advertised wage. The recruitment elasticity is actually larger than the separation elasticity and suggests a mark-down of around 18%.

A recent US example in this spirit is Azar, Berry and Marinescu (2022), which examine data on job seekers, vacancies, posted wages and job boards in the US. Using the application elasticities they find mark-downs of 21%. The authors further show how this mark-down holds even in the largest industries, which should have the most competitive labour markets.

There are several other methodologies. Some papers have looked directly at the way wages change to shocks in the employment (or size) of firms (e.g. Lamadon et al., 2022; Kroft et al., 2022). Another strategy (e.g. Yeh et al, 2022) is to use a production function approach, comparing the estimated output elasticity of labour (from the production function) to the labour share of revenue (from the data), which should be equal under perfect competition. This has to be adjusted to consider markups, which can be done by comparing the elasticities and shares of a factor assumed to be supplied competitively (e.g. intermediates). A third approach is to use a more structural model of the labour market such as Berger et al. (2022), which allows the local market shares of establishments to matter (as would be suggested by more general oligopsony models that generalize monopolistic competition). All three methods find evidence of substantial mark-downs.

Looking across the mark-down literature Naidu, Posner and Weyl (2018) find wage mark-downs in the range of 20-25%, which is consistent with Card (2022).

Labour Market Concentration and Mergers

An older literature analyses the concentration-wage relationship at the industry level (e.g. Weiss, 1966). Modern studies focus on panel data of individuals and firms. For example, Azar, Marinescu and Steinbaum (2022) find lower wages in concentrated markets using evidence from job postings in the US. Moving from the 25th percentile of concentration (measured by HHI) to the 75th is associated with between a 5% to 17% decline in posted wages. Benmelech, Bergman and Kim (2018) examine plant-level manufacturing data. They find an overall negative correlation between wages and local HHI measures of employer concentration, which increases over time. They also find that this negative correlation is weaker in highly unionized industries. In the UK, Abel, Tenreyro and Thwaites (2018) look at wages and concentration and find a negative association. Like Benmelech et al. (2018), they also find that the effect differs with levels of unionisation: higher levels of labour market concentration do not affect unionised workers. But for a worker not covered by a collective

bargaining agreement, moving from the 25th percentile of concentration to the 75th is associated with a pay decrease of 1.1%.

Overall, this literature suggests that labour market concentration is associated with lower wages, consistent with a monopsonistic view of the labour market.² An important issue, of course, are concerns over endogeneity as there is no clear explanation of what drives concentration and whether these drivers could be independently correlated with wage changes.

A more obvious candidate for big concentration changes is M&A activity. Prager and Schmitt (2021) study US hospital mergers and find reduced wage growth where increasing concentration induced by a merger is large and work skills are industry specific. The authors estimate that for the top quartile of concentration increasing mergers, wages are 4% lower for skilled non-health professionals and almost 7% lower for nursing and pharmacy workers.

Also in the healthcare sector, Guanzioli (2023) looks at a merger between two Brazilian retail pharmacy chains using matched employer-employee data. Unlike Prager and Schmitt (2021), he finds no negative effects on skilled workers after controlling for individual fixed effects but *does* find negative wage effects on relatively unskilled workers (salespeople). He argues that strong unions for the pharmacists prevented wage cuts, whereas uncovered salespeople still search disproportionately in monopsonistic pharmacy labour markets. Thoreson (2023) also examines the pharmacy industry but looks at deregulation in Sweden which led to a rapid increase in entry and therefore a decrease in concentration. Wages rose in the industry, especially for more mobile workers, as well as younger and foreign workers (consistent with a monopsony explanation). This is a nice alternative to M&A studies, exploiting an alternative source of quasi-experimental variation in concentration.

These studies are for specific industries so have the advantages of controlling for many unobservables but may be hard to generalize. Arnold (2020) estimates the impact of mergers on wages in the US across a large number of sectors. Using a difference-in-differences

² For other recent examples see Amodio et al (2023), Bassanini et al. (2023) and Lagaras (2023).

design, he finds that when mergers have a substantial increase on local concentration, wages fall by 2%, with the largest impacts being in already highly concentrated markets.

There is a large literature on the effect of M&A on workers, focusing on the impacts within the merging parties themselves rather than on the wider effects on local labor markets. Arnold (2020) argues that for most mergers there is minimal impact on labour market concentration. Consequently, he does not find negative effects for most mergers. Using German employer-employee data, Gehrke et al. (2021) present evidence of employment falls following mergers (especially in targets) and substantial changes in composition of the workforce (generally towards younger and less experienced workers). They also present a survey of 31 econometric studies of the effects of mergers on jobs and or wages. The preponderance of studies find negative effects on employment, whereas the wage evidence is more ambiguous.

Non-Compete Agreements (NCAs)

The literature on non-competes and wages is far smaller. A nice natural experiment was the ban on NCAs in 2008 for hourly paid workers in Oregon. Lipsitz & Starr (2021) found that hourly worker wages increased by 2-3% on average following this change and the within industry mobility of hourly workers increased by 12-18%. Workers were therefore able to move to better jobs, suggesting that the ban improved the pairing of workers and firms, as well as productivity.

One of the central arguments that firms make in support of NCAs is the defence of their investment in worker training, and the necessity of NCAs to retain workers who would otherwise utilise the firm-sponsored upgrade in skills and certification to find a new job. NCAs raise the cost to the worker of moving jobs and protects the firm from the loss of a worker it has invested in. Starr (2019) examines the Survey of Income and Program Participation and presents a more nuanced picture of noncompete agreement enforceability and the impact on worker wages and training. He found that a shift from no enforceability to mean enforceability increased firm sponsored training by 14%, with no such correlation between mean enforceability and self-sponsored training. NCAs, even when proved to be

unenforceable remained a factor for a considerable number of employees, suggesting that the influence of NCAs extends beyond their actual legal standing. Starr et al. (2021) found that 70% of US employees with unenforceable NCAs understood them to be enforceable.

No poach Agreements

We know of no econometric study that has looked at the impact of no poach agreements. They are illegal in most jurisdictions (including the UK and the US) There is, however, ambiguity over whether they can be imposed in franchise agreements between different branches of a single franchiser. There are currently legal challenges in the US, particularly in the case of McDonalds.³

The Wisconsin Department of Financial Institutions (WDFI) holds US Franchise Disclosure Documents (FDD), and all franchises in the US must submit their FDD to the WDFI. This makes it straightforward to check whether US franchises have a no poach clause in the FDD. Callaci et al. (2022) matched Franchise Disclosure Documents with job adverts using the Burning Glass Technologies database. The findings showed that the top occupations with no poach provisions were in the food service industry, drivers, customer service representatives, retail sales and hospitality. These individuals all earned on average below half of the median earnings in the US. They found that 56% of franchises with Franchise Disclosure Documents in the database had a no poach agreement in place. The industries with the highest incidence of no poach agreements were restaurants, accommodation, personal care services, and individual and family services.

By contrast, no such database exists in the UK, and therefore there is no straightforward way to determine whether franchises have a no poach clause. Through some desk research, phone calls and investigations, we were able to obtain one Franchise Agreement (equivalent to the

³ <https://www.hcamag.com/us/specialization/employment-law/lessons-for-hr-after-mcdonalds-no-poach-court-decision/460048>

US FDD) and were able to locate a no poach provision within the document. The phrasing of the no poach is as follows:

“You shall not during the Term, except with Our prior written consent: (b) at any time, employ or engage or seek to employ or engage, any person then employed by Us or by any of Our franchisees or licensees, nor shall You directly or indirectly induce any such person to leave their employment without the previous written consent of such person’s employer, nor will You employ such person without Our consent within 6 months after the termination of their employment.”

This fragment of the Franchise Agreement was obtained under conditions of anonymity of both the franchise to which it belongs, as well as the individual who signed it.

Many of the franchises with a no poach clause in Callaci et al. (2022) are firms with a significant presence in the UK, including Pizza Hut, Five Guys, Hertz, UPS and Domino’s Pizza (among others). Given the existence of no poach agreements in the US, it is quite possible that some of them have no poach clauses in the UK. The British Franchising Association estimates that 710,000 people work for franchises in the UK (BFA, 2021).⁴

Summary on empirical evidence

In summary, the existing literature suggests that there is evidence for monopsony from the empirical literature on wage mark-downs and concentration (especially in a non-union environment). The literature on non-competes and no poaches is very small, but it does suggest they cause a limitation on labour market mobility.

⁴ One franchise owner we interviewed mentioned that their franchise lawyer had seen “hundreds” of no poach provisions in UK Franchise Agreements, and that they were included as standard in the contracts.

4. Non-Compete Agreements (NCA)

This section explores the prevalence of non-compete agreements in the UK, based on the results of the first reported employee survey covering this topic in the UK. The results point to one fourth of the UK workforce likely being under a non-compete in their current job, in line with results from other countries. Despite these agreements being more prevalent for high-income and high-skilled workers, they still affect a significant proportion of low earners, for which it is harder to find justification from an economics perspective. Workers that receive training from their employer are more likely to be under a non-compete, but we are unable to conduct a cost-benefit analysis on the value of the training to ascertain if the worker is adequately compensated for the resulting reduced mobility.

4.1 Data and Methodology

High quality NCA surveys exist in the US (Starr et al., 2021) and Italy (Boeri et al., 2022) among others, but not in the UK. We commissioned Ipsos to implement a survey of 2,713 employees (81% full-timers and 19% part-timers) which went out to field from January 6th to 13th 2023. The survey was funded by the Competition and Markets Authority (CMA) as part of an LSE Capstone project. The questions were designed to be as close as possible in phrasing, options and style to the questions used in the US and Italian contexts. The details of the questionnaire can be found in Appendix D.

In addition to some questions about employment, the questionnaire asked about whether respondents had an NCA in place, if they had received training and whether they had had an NCA in the past. In some places the phrasing differed from either the US or the Italian questionnaires: for example, the US survey only gave “Yes”, “No” and “I don’t know” as answers to whether a respondent had a non-compete in place. Our survey, following the Italian questionnaire, allowed for “Yes”, “Probably Yes”, “Probably No”, “No” and “I don’t know”. This allowed for more granular analysis of the results. In addition, both the Italian and US surveys had many more questions than our own survey. These questions were about the “signing behaviours” of the respondents: for example, whether they had a lawyer present when they signed; if they read the contract fully, or whether they signed without reading it.

Other questions involved other restrictive covenants, such as non-disclosure agreements (NDAs).

4.2 Descriptive Analysis

One headline result is that 10% of workers in the UK definitely have an NCA in place, with a further 16% of employees claiming that they “probably” have one in place. Of those who have a non-compete, 51% claim they have “not considered applying for a role with a competing organisation, but if I did then the agreement would put me off applying” or “I have considered applying for a role with a competing organisation, but the agreement put me off applying.” This points to NCAs not only being prevalent in the UK, but also causing significant disruption to labour mobility in the country.

Table 2 displays the most relevant studies on NCAs which are comparable with the UK one. As noted above, the US and Italy have the most comparable surveys. Both countries have many non-competes, but with lower prevalence than the UK: 16% in Italy and 18% in the US. These are major shares of the working force and go beyond what is regarded as the high tech/high skill sectors. The Austrian (33%), Finnish (45%), and one of the Dutch surveys (37%) show higher incidence of NCAs, but these surveys are not directly comparable to the UK, US and Italian due to methodological differences (they cover different sectors of the labour market).

We begin by breaking down the prevalence of NCAs by three measures of skill: education, wages and occupation. All show that NCAs are more common for higher skilled groups, but also show a surprisingly high prevalence level amongst lower skilled groups.

Figure 1 breaks down NCAs by education group. Workers with a postgraduate degree show the highest prevalence (36%). However, NCAs are also common among employees with lower education levels: 22% of those with only primary secondary education are also covered by an NCA. Figure 2 shows higher skilled occupations have a higher NCA prevalence. About 45% of entrepreneurs and executives are covered compared to 23% of plant operators. Finally, Figure 3 shows that high income groups are generally more likely to be covered (although the relationship is weaker than for education and occupation). The highest coverage is for

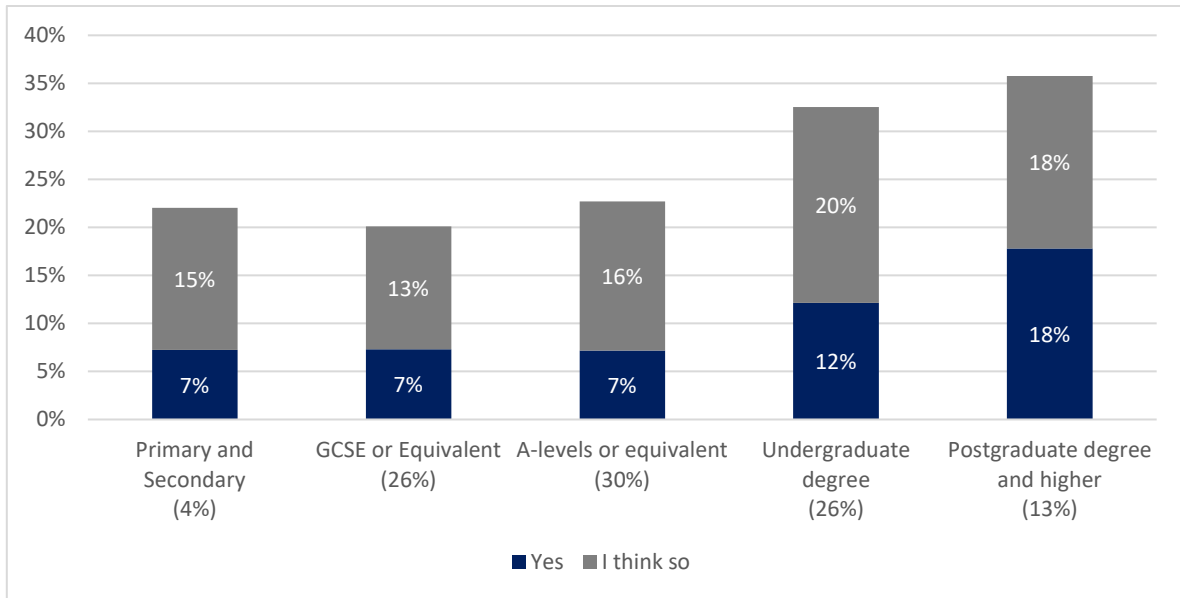
workers earning £100,000 or more. Appendix D contains more descriptives showing that those in London and from higher socio-economic groups are also more likely to be covered.

Table 2: Overview of extant surveys of NCAs across countries

Country	Year	Prevalence	Comments	Source
UK	2023	26%	Includes NET of “Yes” and “Probably Yes” responses	Starr et al. (2021)
USA	2021	18%	Includes only “Yes” as survey only asked for “Yes”, “No” or “Don’t know”	
Italy	2022	16%	Includes NET of “Yes” and “Probably Yes” responses	
Netherlands	2015 / 2021	12% / 37%	Employee Survey (covers employees who changed jobs in the past 12 months) / Employer Survey	Streefkerk, Elshout, & Cuelenaere (2015) & Panteia (2021)
Finland	2017	45%	Survey covers members of Finnish Trade Union Akava only	Vuorenkoski (2018)
Austria	2021	33%	Private sector low earning workers. Survey conducted in 2005/06.	Young (2021)
Denmark	2012	20%	Survey covers Business Denmark, a Danish Trade Union for Sales and Marketing employees.	Dahl & Stamhus (2013)
Australia	2023	22%	Covers employees who changed jobs in the past 12 months	Andrews & Jarvis (2023)

Notes: This table uses a variety of sources, some of which may not be directly comparable. Surveys for the UK, USA, and Italy are most comparable in terms of methodology and the UK survey methodology borrows from both Boeri and Starr to make results comparable.

Figure 1: Non-compete prevalence by education level

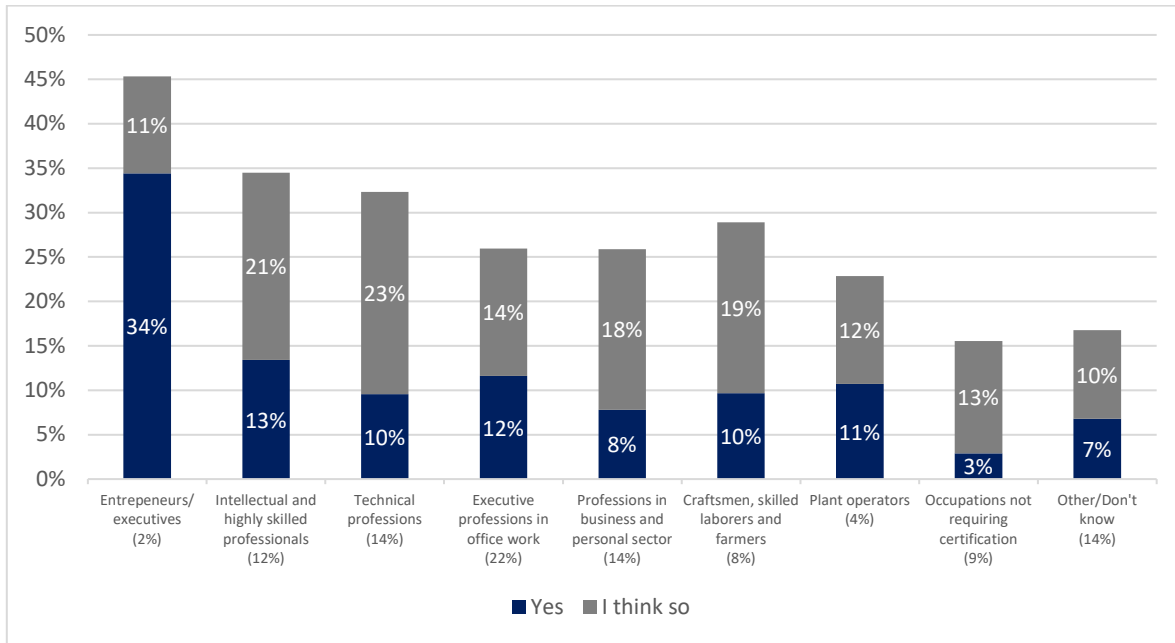


Notes: “Yes” includes respondents who say they definitely are under an NCA. “I think so” includes those who say they probably are under an NCA. Results are weighted using survey weights in order to make them more representative of the overall population. Numbers in brackets indicate the share of the overall population that fall within each category.

Source: Authors’ computations based on survey results.

The finding that high skilled workers are more likely to be covered by an NCA is consistent with Starr (2019), Boeri et al. (2022) and the other surveys. It also chimes with the idea that these workers will have more access to confidential information, sales lists, as well as business costs, goals or plans. However, our survey shows that there are also significant numbers of those who are unlikely to have access to any secret or specialised information that are covered by NCAs. One area that the survey was not able to cover but is further explored in other literature on non-compete prevalence is the “access to confidential information”. But even amongst those skilled groups with access to trade secrets, it is unclear whether NCAs are the appropriate instrument. Such information could be protected through intellectual property and trade secret laws, rather than the coarse and blanket NCAs.

Figure 2: Non-compete prevalence by occupation

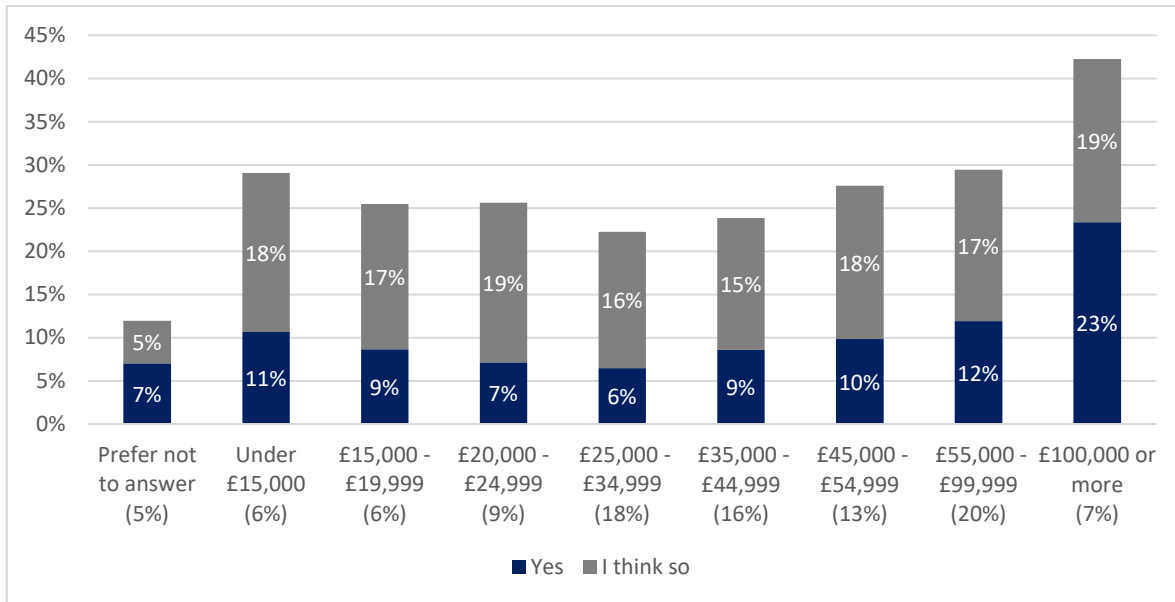


Notes: “Yes” includes respondents who say they definitely are under an NCA. “I think so” includes those who say they probably are under an NCA. Results are weighted using survey weights in order to make them more representative of the overall population. Numbers in brackets indicate the share of the overall population that fall within each category.

Source: Authors’ computations based on survey results.

Figure 4 shows that those workers receiving training in the workplace are much more likely to be under a non-compete than those who do not. Training can be a legitimate reason for the use of NCAs in order to prevent recently trained workers from leaving. However, agreements to repay training costs in case the employee leaves the company before a certain period can establish the same outcome without the need for a non-compete, which further highlights the use of NCAs as catch-all tools which employers routinely adopt for their convenience. Moreover, some training may not be compensation enough for the reduced mobility caused by NCAs.

Figure 3: Non-compete prevalence by income bracket



Notes: “Yes” includes respondents who say they definitely are under an NCA. “I think so” includes those who say they probably are under an NCA. Results are weighted using survey weights in order to make them more representative of the overall population. Numbers in brackets indicate the share of the overall population that fall within each category.

Source: Authors’ computations based on survey results.

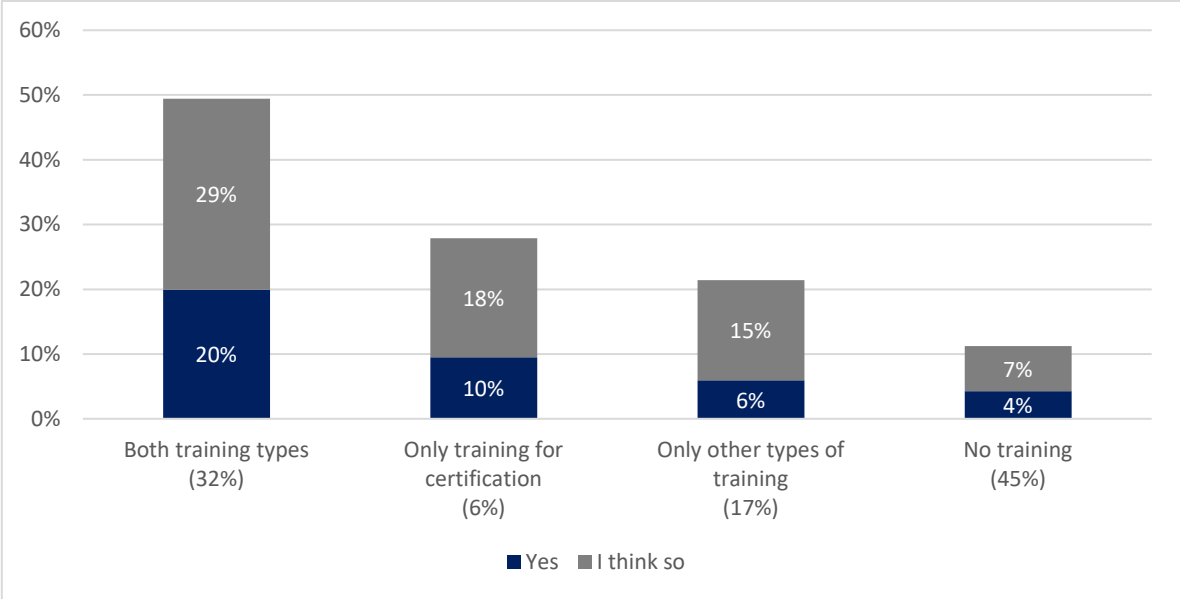
4.3 Regression Analysis of Non-Competes

To further investigate the observed differences in NCA prevalence we employ regression analysis where the outcome is a dummy equal to one if a worker is covered by a noncompete. We code “yes” and “probably yes” together as one and “no” and “probably no” as zero, and drop those replying “don’t know” from the sample. We present linear probability models for worker i :

$$NCA_i = \alpha + \beta X_i + \epsilon_i \tag{4}$$

Where NCA_i is the dummy for being covered, α is a constant, X_i is a vector of individual i 's characteristics and ϵ_i is the error term. We also estimate probits, but the marginal effects at the mean were near identical, so we only present OLS results to make interpretation easier.

Figure 4: Non-compete prevalence by training type



Notes: “Yes” includes respondents who say they definitely are under an NCA. “I think so” includes those who say they probably are under an NCA. Results are weighted using survey weights in order to make them more representative of the overall population. Numbers in brackets indicate the share of the overall population that fall within each category. Training for certification refers to training paid for by the employer that leads to a certificate or diploma, while other types of training refers to training paid for by the employer that does not lead to any certification or diploma.

Source: Authors’ computations based on survey results.

We focus on training as it could be correlated with skill and split it into formal (resulting in a certification or degree) and informal (not leading to a certification or degree). Column (1) of Table 3 includes just the two training variables. Column (2) adds gender, job tenure in years and industry. Column (3) includes tenure and income. Column (4) includes income and education, while column (5) includes all controls. Throughout the table, both types of training are positive and significant with larger coefficients for formal than informal training, and the point estimates not changing much across columns. For example, in column (5) formal

training is associated with an increase in the probability of the worker having an NCA by 22 percentage points, and informal training with an increase of 14 percentage points.

Women are less likely to be under an NCA than men, but this effect is insignificant in the final column when we control for other variables, particularly industry and occupation.

4.4 Discussion

Our survey shows that about one quarter of UK workers appear to be under an NCA, a higher proportion than in the US and Italy where similar surveys have been run. As shown in both the literature review and survey analysis, this can have severe labour mobility restrictions, which can depress wages and make the labour market more inefficient.

The incidence of NCAs is strongest for higher skilled workers, which could be rationalised by the need to keep trade secrets. But it is also high for unskilled workers, where the rationale is less clear. There is a strong correlation with training (even controlling for skill as measured by education, occupation or income). This gives credence to the argument that employers are trying to recoup some of the rents from training which would be lost if workers left for another employer, although without knowing the value of the training provided it is impossible to know if this is a genuine concern or just an excuse for the use of NCAs. Even when the training provided justifies the employer's concerns, there may be other ways to ensure proper investment in the workers rather than a blanket NCA.

Table 3: Training and Non-Compete Agreements

Dependent Variable: Prob (NCA = 1)	(1) Baseline	(2) Baseline plus industry	(3) Baseline plus income	(4) Baseline plus income & education	(5) Baseline plus all controls
Formal training	0.276*** (0.0290)	0.265*** (0.0283)	0.268*** (0.0283)	0.269*** (0.0283)	0.225*** (0.0278)
Informal training	0.156*** (0.0270)	0.160*** (0.0267)	0.156*** (0.0264)	0.154*** (0.0267)	0.145*** (0.0258)
Female		-0.0221 (0.0227)	-0.0535** (0.0219)	-0.0517** (0.0220)	-0.0342 (0.0228)
Tenure	No	Yes	Yes	Yes	Yes
Income	No	No	Yes	Yes	Yes
Industry	No	Yes	No	No	Yes
Education attainment	No	No	No	Yes	Yes
Occupation	No	No	No	No	Yes
Age	No	No	No	No	Yes
Region	No	No	No	No	Yes
<i>Observations</i>	<i>1,972</i>	<i>1,972</i>	<i>1,972</i>	<i>1,972</i>	<i>1,972</i>
<i>R-squared</i>	<i>0.162</i>	<i>0.210</i>	<i>0.188</i>	<i>0.192</i>	<i>0.262</i>

Notes: Regressions conducted using OLS with survey weights. Robust standard errors in parentheses. Tenure is a continuous variable. Income is a categorical variable with 9 bins. Industry is defined according to the SIC codes at the Section level and has 24 categories. Education attainment is a categorical value with 5 bins. Occupation refers to the specific job role the respondent has, and it is composed of 10 categories. Formal training refers to training paid for by the employer that leads to a certificate or diploma, while informal training refers to training paid for by the employer that does not lead to any certification or diploma.

5. The Impact of Mergers and Acquisitions on jobs and wages

We now examine the impact of mergers and acquisitions (M&A) on firm-level employment and wages. Our analysis focuses on M&A events in which both the acquiror and the target companies are located in the UK. The control group are other UK based companies not engaging in M&A activity. We use event studies with two-way fixed effects with the year prior to the merger as the baseline. This way we can study pre-trends as well as the dynamic development of the treatment over time following the merger or acquisition.

In extensions, we refine the event studies by looking at M&A events within the same industry and size group and we also disaggregate the effects in acquiror vs. target as generally both firms continue to provide data after the event (although this must be taken with particular caution as firms are restructuring their accounts). A downside of the analysis is that we do not have individual worker data, so the wages are average wages (wage bill divided by employment), meaning we cannot disentangle changing compositional effects.

5.1 Data and Methods

We combine data from two main sources: Orbis M&A and Historical Orbis. From Orbis M&A we extract information about M&A events occurring between 2002 and 2017 in which both the targets and acquirors were UK-based companies. We then merge these with annual company accounts from Historical Orbis. We look at firm outcomes from 1997 to 2022.

Historical Orbis often contains duplicates as firms routinely file multiple reports within the same year (e.g. a consolidated and unconsolidated account). We follow the data cleaning process used in De Loecker et al. (2022), which we describe in detail in Appendix B, to remove those duplicates. All financial information is deflated by the CPI to be in constant prices. Many companies which engage in more than one acquisition within our time period⁵ appear in our sample, so we restrict the sample to events in which there is one acquiror and

⁵ Multiple acquisitions range from two to 72. Companies which acquire several targets are commonly holding companies.

one target to simplify our data analysis. We also restrict the sample to companies for which there is both wage and employment data for at least five years.

The final analysis sample contains 2,060 M&A events, for a total of 116,925 observations in the treatment group, and 906,297 observations in the control group.

To have a measure that captures the effect of the M&A event in both target and acquiror, we create a measure of “synthetic” employment and “synthetic” wages, which we define as the number of employees and hypothetical average wage of the two companies were they one firm even before the M&A events. Synthetic employment is the sum of total employees of the two companies for each year, while synthetic wage is average wage of each company weighted by the respective number of employees. So considering two firms (denoted with sub-scripts “a” and “b” who were involved in an M&A event, we define:

$$\text{Synthetic employment}_{ab} = \text{employment}_a + \text{employment}_b \quad (1)$$

$$\begin{aligned} \text{Synthetic wage}_{ab} = & \text{average wage}_a \left(\frac{\text{employment}_a}{\text{synthetic employment}_{ab}} \right) + \\ & \text{average wage}_b \left(\frac{\text{employment}_b}{\text{synthetic employment}_{ab}} \right) \end{aligned} \quad (2)$$

The logarithm of these measures are our main dependent variables. For all specifications we include year and firm fixed effects and cluster our standard errors at the firm level. The baseline specification is:

$$y_{it} = \sum_{j=-5}^{-2} \beta_j D_{it+j} + \sum_{j=0}^5 \beta_j D_{it+j} + \gamma_i + \theta_t + \epsilon_{it} \quad (3)$$

Where y_{it} represents the dependent variable (log of synthetic wage or employment), γ_i are firm fixed effects, θ_t are time fixed effects, D_{it+j} is the treatment indicator, which we

normalize to be relative to the period in the year before the merger. We also examine other outcomes such as profits (EBITDA) and productivity (sales per employee).

We look at the heterogeneity of the treatment effect on various dimensions. In particular, we distinguish between target and acquirer as the target continues to produce accounts five years after the M&A event in almost all cases. We also look at differences by size, whether the margining parties are in the same industry, same location, etc.

Different theories give different predictions on outcomes. If the merger creates employer monopoly power or monopsony power, employment should fall - in the former case to raise prices (markups) and in the latter case to reduce wages (mark-downs). In contrast, there should be no effect on wages in the case of monopoly and a negative effect on wages in the case of monopsony. If the merger raises efficiency, then in principle employment could rise as the firm may lower prices and grow. This depends on the demand curve, elasticities of substitution, etc. If there are only small effects on output, greater efficiency could also mean a fall in employment.

5.2 Descriptives

Table 4 shows the summary statistics for our sample, broken down by control and treatment groups, as well as by target and acquirer. Table 4 shows that those companies who are involved in an M&A event tend to be larger. The control group has an average employment of 308, whereas the treatment acquirer has on average over a thousand workers. The treatment target are more comparable, however, with an employment level of 344. As can be seen by the difference between the mean and medians, as well as by the standard deviation, there are many outliers in our sample, particularly in the control group. This issue is mitigated by taking the logs of the outcome variables for the regression analysis.

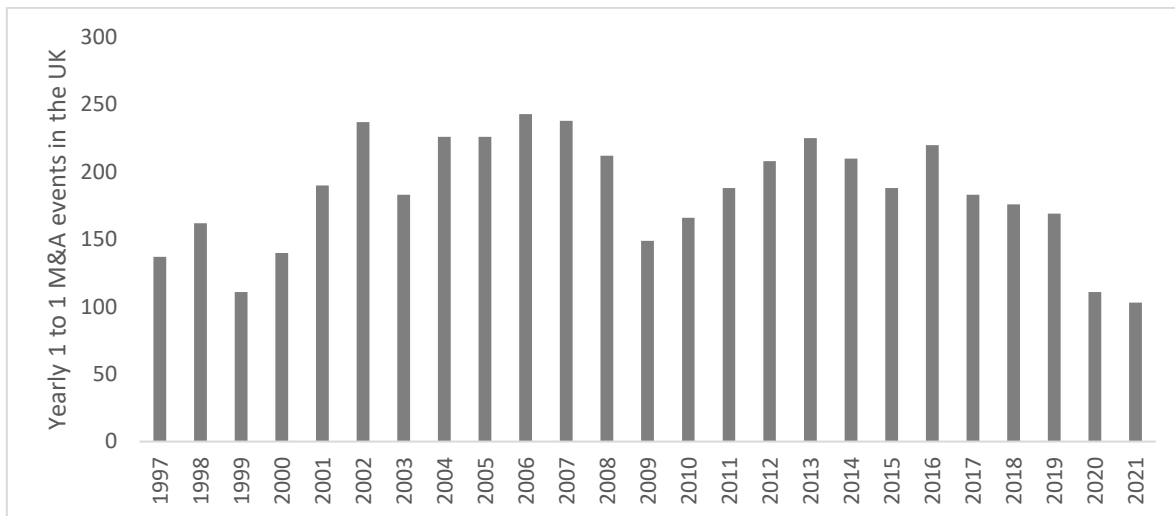
Figure 5 shows the yearly M&A activity for our sample. The yearly events are relatively constant, with a dip in activity following the 2008 crisis, and a slowdown after 2016, following the Brexit referendum.

Table 4: Descriptive Statistics

		Mean	Median	Standard Deviation	Observations
Control Group:	Employment	308	47	4,045.48	906,297
	Wage	76,212.79	35,725.34	3,632,200	906,226
	EBITDA	8,779,194	383,465.2	333,000,000	813,975
Treatment: Synthetic	Employment	1,587	301	6846.52	12,520
	Wage	51,741.53	41,123.08	44,439.61	11,785
	EBITDA	44,100,000	3,576,203	394,000,000	11,876
Treatment: Acquiror	Employment	1,092	170	4,950.84	21,300
	Wage	60,834.06	42,077.57	191,782.6	16,772
	EBITDA	34,400,000	2,258,036	337,000,000	16,734
Treatment: Target	Employment	344	69	2,543.54	21,300
	Wage	52,023.99	38,975.59	68,571.56	13,862
	EBITDA	8,716,464	686,909	81,100,000	14,134

Notes: The control group includes all companies based in the UK available in ORBIS. Treatment refers to companies involved in an M&A event in the period 1997-2021. Wage and EBITDA are in pounds sterling.

Figure 5: Evolution of M&A activity: Yearly M&A events



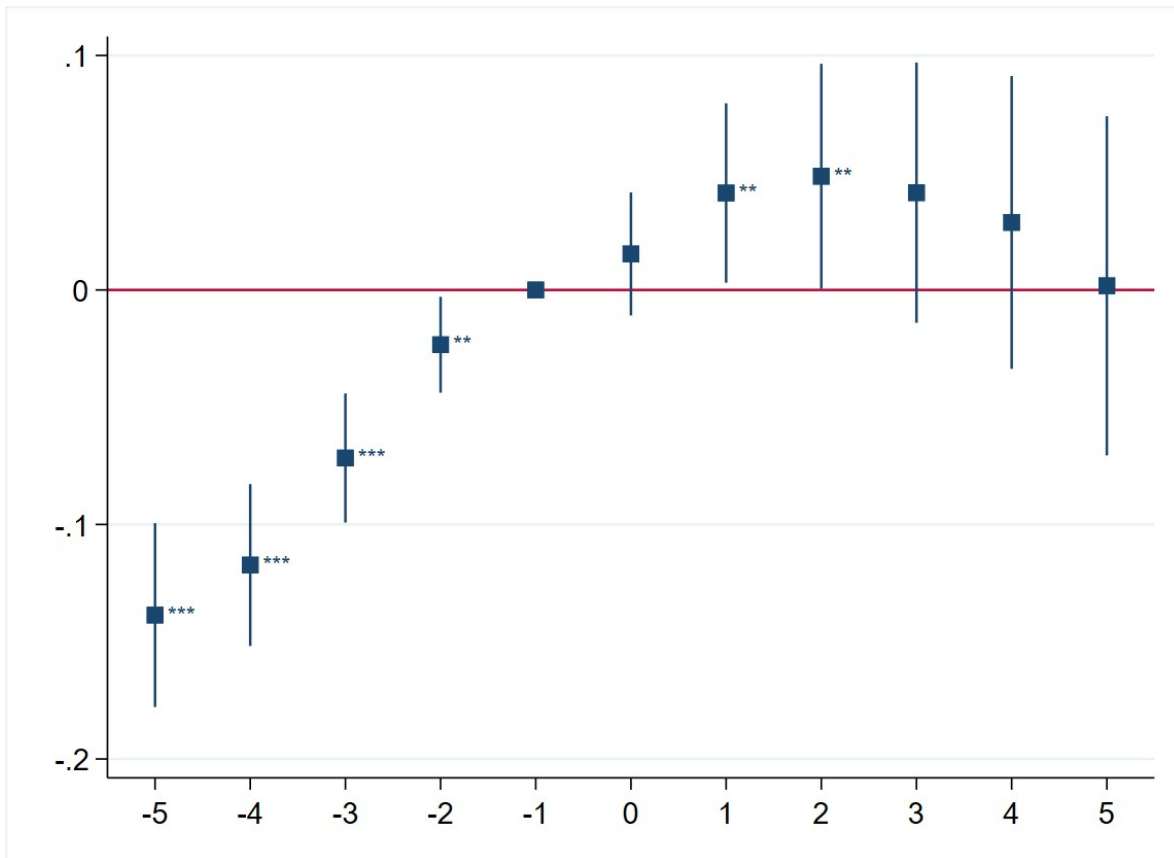
Notes: One to one M&A events refer to those in which only one acquiror acquires one target, or when two firms merge. Sample only includes firms involved in only one operation between 1997 and 2021.

5.3 Results

Basic Results

Figure 6 shows the event study results for employment. The period prior to the M&A is used as baseline. The graph displays clear pre-trends with employment growing in the years leading up to the merger and then stagnating two years after the merger before falling again to its pre-merger level. The firm grows by about 15% over the five years leading up to the merger. The simple interpretation of this graph is that growing firms are more likely to merge, but after a merger this growth ceases. So we could interpret the M&A event as having a negative effect on firm employment growth.

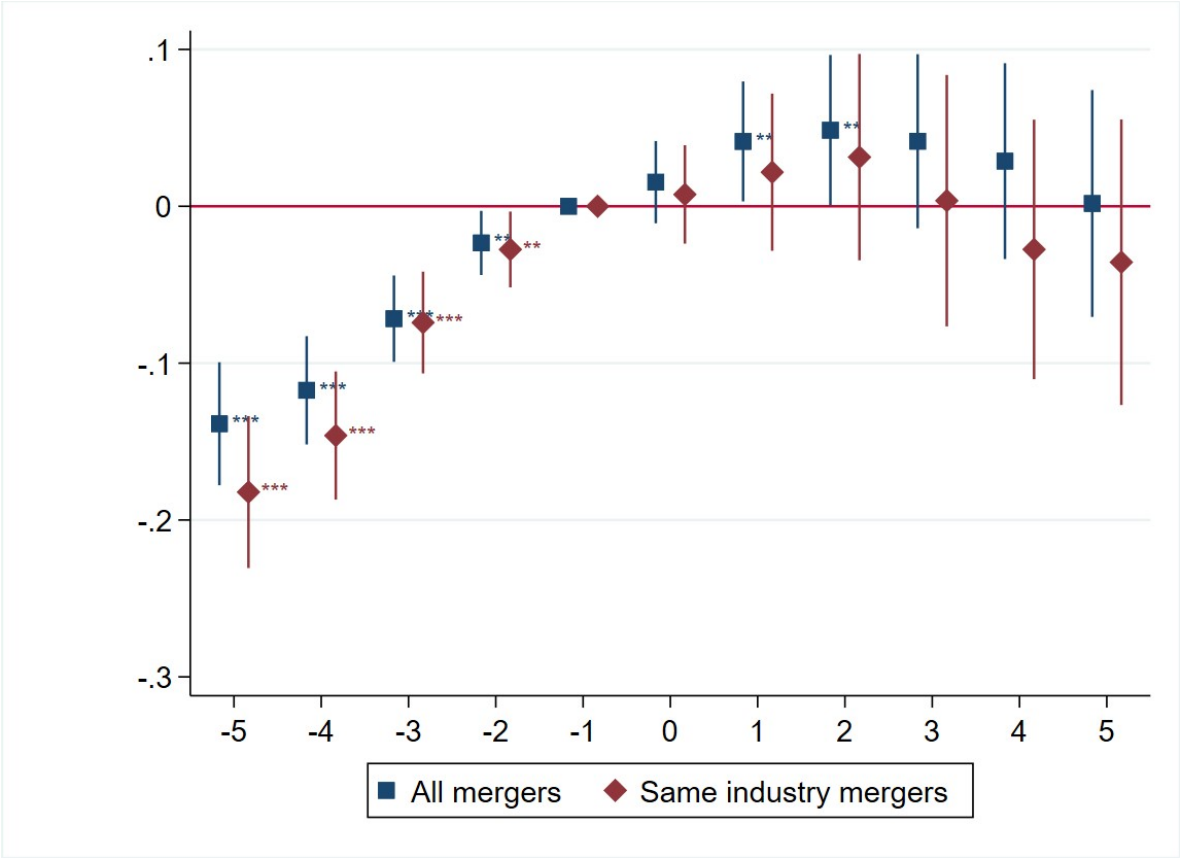
Figure 6: Event study – Employment (all firms)



Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at the 10% level, ** significant at 5%, *** significant at 1%.

Of course, the presence of pre-trends makes the interpretation of the treatment effects challenging, because there are clearly selection effects regarding the firms who engage in M&A activity compared to those who do not.

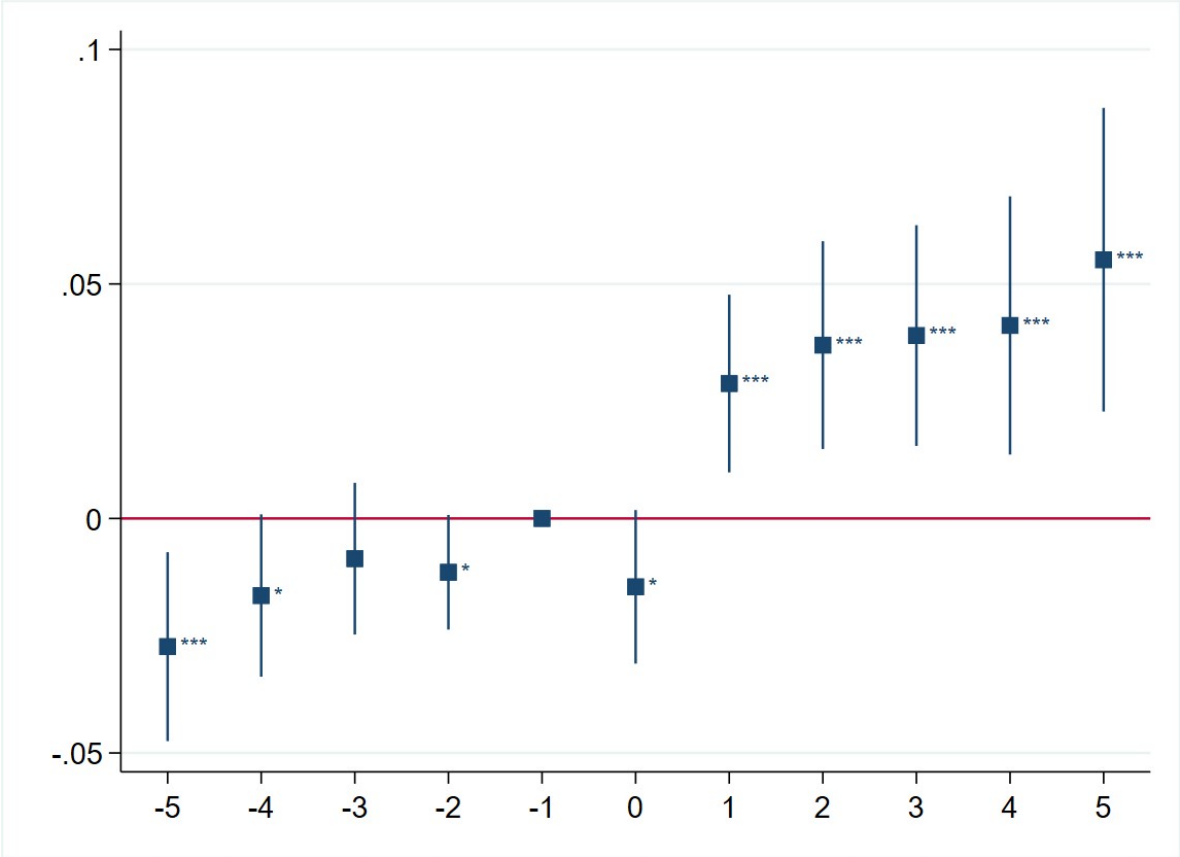
Figure 7: Employment (all firms), baseline and same industry M&A



Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Firms are considered to be in the same industry if they share the first 2 digits of their main UKSIC code. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at 10%, ** significant at 5%, *** significant at 1%

Figure 7 looks at the sub-sample of mergers that occur only in the same two-digit industry, (using all other untreated companies as the control), showing very similar patterns, but with a slightly larger pre-trend.⁶

Figure 8: Event study – Wage (all firms)



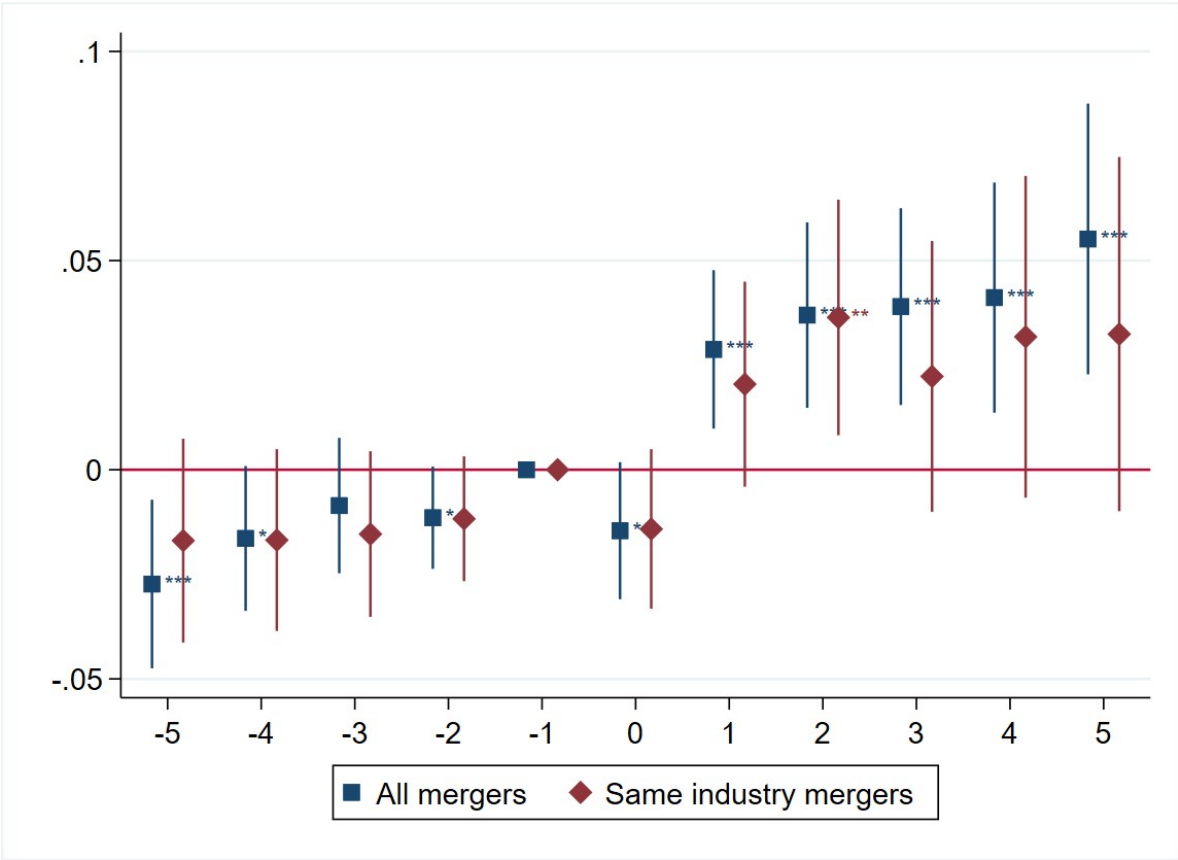
Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 8 pictures the results for wages. In contrast to employment, we do not see such strong pre-trends, only a slight growth in average wages of a percent or two leading up to the M&A

⁶ The analysis by acquirer size shows that large companies have stronger pre-trends and decline after the M&A event, followed by small companies. Despite this, the differences in coefficients are not statistically significant.

event, but these are only weakly significant. Post merger, there appears to be a jump *up* in wages, rising to about 5% five years after the merger. When including the point estimates of the same industry mergers specification into the graph next to the baseline estimates no clear picture emerges (Figure 9), as things look broadly similar.⁷

Figure 9: Event study – Wage (all firms), base and same industry M&A



Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Firms are considered to be in the same industry if they share the first 2 digits of their main UKSIC code. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at 10%, ** significant at 5%, *** significant at 1%.

⁷ Digging into the type of merger, the positive effect on wages seems to be driven by SMEs. Unfortunately, each size group shows different pre-trends

The apparent increase in wages could be due to the merger creating some firm monopoly power and workers sharing in these rents. But it could also be a compositional effect, with firms get rid of low-wage workers. Unfortunately, we cannot be sure which mechanism is at play without more granular data on wages.

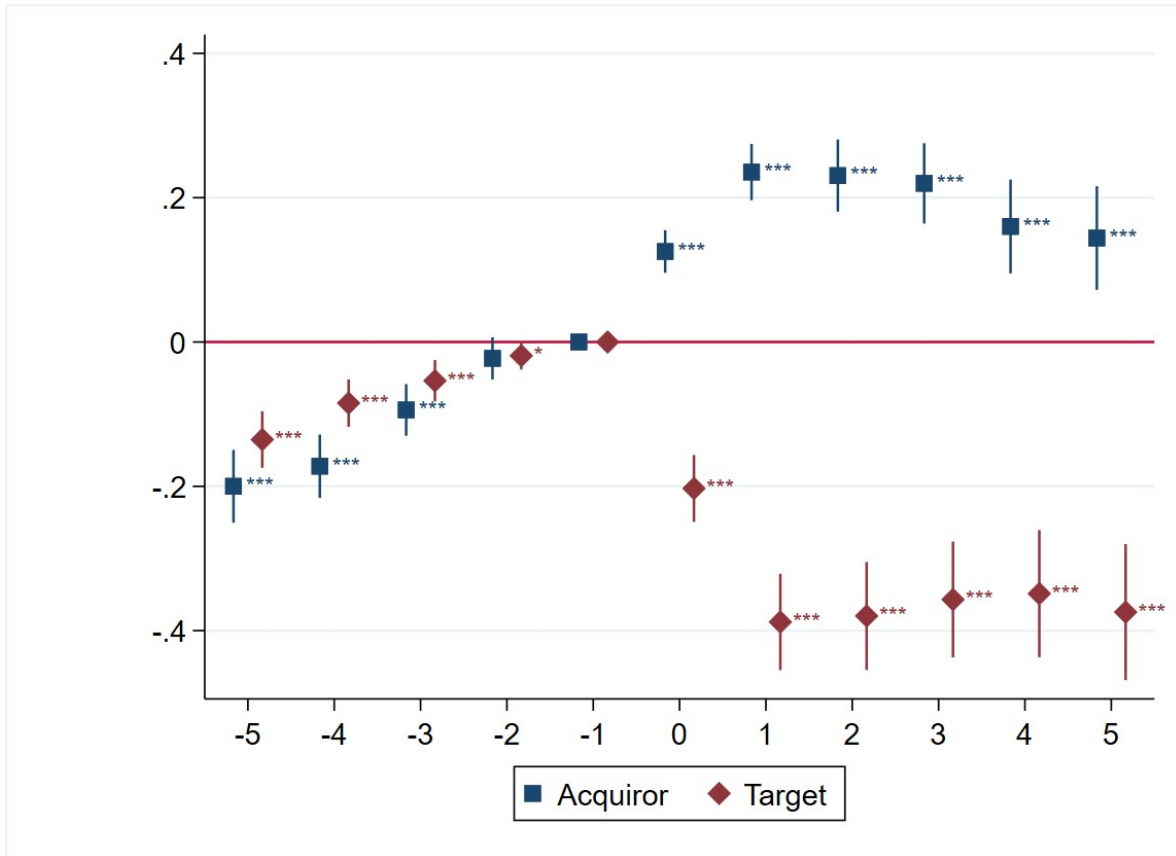
One way of trying to distinguish between different theories of what might be driving these changes is to examine other outcomes. The overall profitability of firms looks quite similar to employment, with positive pre-trends, but then a levelling off after the merger event. Looking at profits per head or per unit of sales, did not reveal any significant effects, nor on productivity (sales per worker). This suggests that there were no major improvements in profitability or efficiency following the M&A. This is consistent with no increases in efficiency or growth in market power following the M&A event. Hence, this suggests the wage effects may more likely be due to compositional effects, rather than a more interesting market power impact explanation.

5.4 Target and acquiror breakdown analysis

Given that more than 95% of the operations in the data are acquisitions and not mergers, there is reason to believe there might be differences in how acquiror and target companies are affected by these operations. The median number of workers in acquiring companies is 170, while it is 69 for target companies, less than half. Acquiring companies also have higher median wages than target companies.

Figures 10 and 11 show the results for employment and wages. There are starkly different effects for the acquiror and target. Before the M&A event, both series show somewhat similar pre-trends, albeit the acquiror grows slightly faster than the target. After the operation employment plummets in the target company (-20% the year of the operation, stabilising between -35% and -39% the years after). Meanwhile, employment continues to grow for the acquiror, then stagnates.

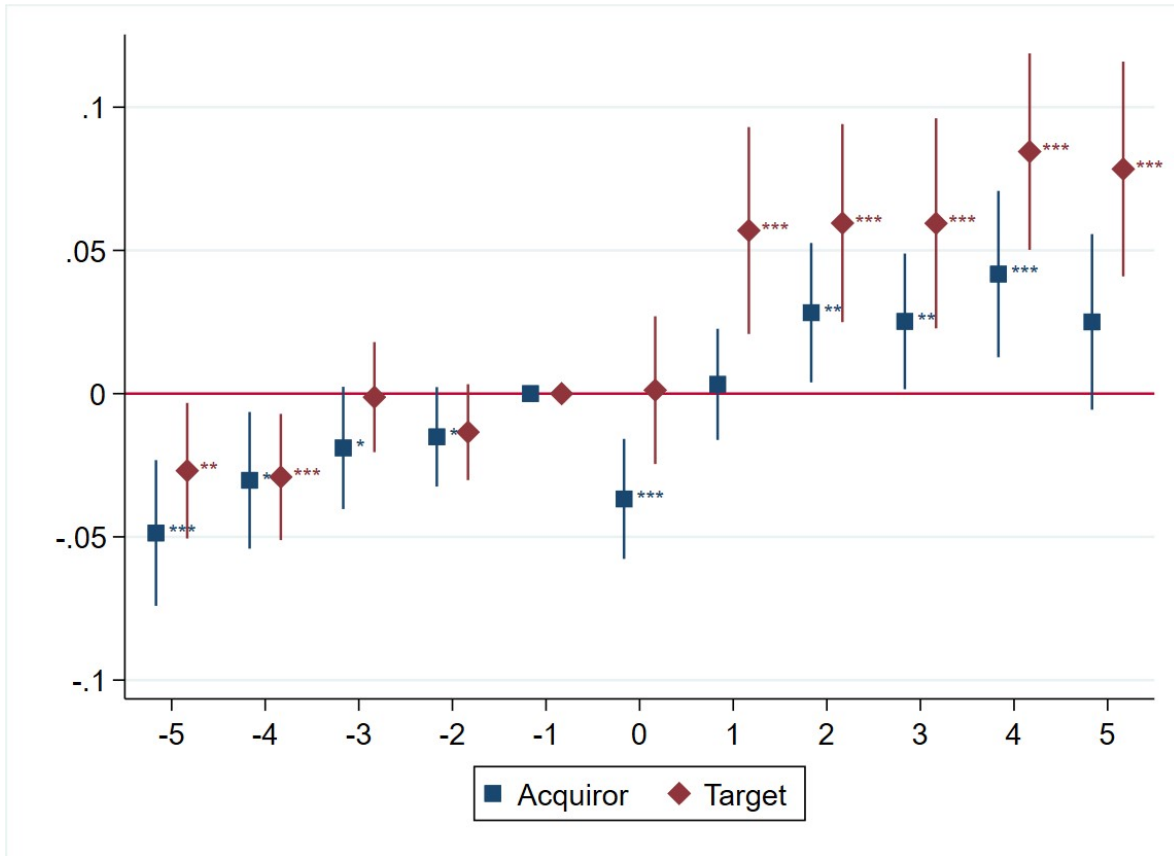
Figure 10: Event study Employment: acquiror and target companies



Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at 10%, ** significant at 5%, *** significant at 1%.

These results seem to point to a reallocation of workers from target companies to acquiror companies, followed by a slow decline in employment in the acquiror company, and thus the overall negative effect on employment growth seen above. This effect increases with the size of the acquiror, especially the reduction in employment in the target company.

Figure 11: Event study of Wage: Acquiror and target companies



Notes: The points represent the estimated coefficients, while the lines are the 95% confidence intervals. The year of the M&A operation is 0, all other years are relative to it. This analysis contains firms of all sizes. Only firms with five or more observations for both employment and wage are included in the analysis; * significant at 10%, ** significant at 5%, *** significant at 1%

Figure 11 shows the results on wages split between acquiror and target. As with employment, both show similar trends pre-merger. After the M&A event, wages grew in both target and acquiror (as in the synthetic wage effects), but slightly more so in the target.

The negative employment growth effects and positive wage effects in target companies are consistent with the evidence from Todd and Heining (2022) on Germany. Their data is richer as they have employer-employee panel data, so can follow the same workers before and after a merger, but the similar results are reassuring that the broad trends we see here on wages may generalize.

5.5 Discussion and Interpretation

We have found generally negative effects on jobs (slower employment growth), which are strongest on the target firms, but a small, positive effect on wages. The results are not straightforward to theoretically interpret. Certainly, they are inconsistent with a simple monopsony story as average wages *rise* rather than fall post-merger. Although the fall in employment growth could be consistent with monopsony, the wage effects do not allow for this simple explanation. But nor can the monopoly story easily rationalize the results. The fall in employment growth would be consistent with enhanced product market power, but the increase in wages is not. One could tell a bargaining story as in Van Reenen (1996) where the product market power created by the merger gets shared with workers in the form of higher wage rents. But the lack of effects on profits (or productivity) post-merger suggests that this is unlikely – increased monopoly power would imply higher profit margins and we do not see much evidence of this.

One alternative story is that the slowing employment growth is because the merging firms are disproportionately shedding less skilled workers. This would mean a changing composition, so that the apparent increase in wages does not reflect individual workers earning more, but a change in the skill mix. The bigger employment drops and slightly higher wage increases in the target firms would be broadly consistent with this hypothesis.

There are at least three areas for improving the analysis. First, using individual worker panel data so that the same individuals could be followed pre and post-merger would help address compositional concerns when examining wages. Second, trying to understand the reasons for pre-trends and constructing a better comparison group would help credibility, and requires a better model of who merges and why (a large challenge). Third, distinguishing between mergers which create greater concentration in the labour market would be potentially very useful, as this is where we would expect to see the most negative effects if monopsony is at play (cf. Arnold, 2020, and Prager and Schmitt, 2021). This requires defining the relevant market, which is tricky.

6. Conclusions

We have presented some evidence of the role of labour market power on workers. The results from our new non-compete agreement (NCA) survey of a representative sample of almost 2,000 workers indicates that around a *quarter of UK workers are likely to be under an NCA*. We find this surprisingly high, and the incidence is larger than suggested by comparable surveys in the US and Italy where the incidence is 18% and 16% respectively. Although the prevalence of these agreements is greater for more skilled workers (whether measured by education, occupation, industry or wage), NCAs affect all groups to some extent (for example, 23% of plant operators are covered). This is worrying for those at the bottom of the wage distribution, who are likely the most affected by the reduced labour mobility that NCAs induce. On the positive side, NCA prevalence is strongly correlated with the provision of employer-paid training (even after controlling for other measures of skill). This is consistent with a potential benefit of NCAs, as employers may under-provide training if they fear workers will be poached by other firms. The question is whether such putative benefits are sufficiently large to justify the harms of NCAs in reducing labour mobility. The UK government has recently proposed a cap on the duration of NCAs to three months. A more radical option would be to ban these agreements completely for low wage workers, who are very unlikely to hold valuable company information and have less bargaining power.

On no poach, we recommend a comprehensive analysis of the prevalence of these clauses should be undertaken, as US evidence suggests they are common in franchises. There is a strong argument for a database of Franchise Agreements to be created, where franchisors would have to submit their documents, following the US model of the Wisconsin Department of Financial Institutions (WDFI).

Finally, we present an econometric analysis of a firm-level panel of over 2,000 UK M&A events. We use a new database with over 900,000 observations between 1997 and 2021. We found evidence of a significant decrease in employment growth following M&A activity, especially for target firms. The total employment of the merger entity grew by about 15% in the five years prior to the merger (so about 3% a year) but stopped growing in the five years

afterwards. Average wage growth did not fall, however, and if anything increased. The lack of negative wage effects and no increases in profit or productivity do not point to monopoly nor monopsony being a major issue following an “average merger”. However, the absence of employer-employee panel data is a major caveat as we cannot control for compositional effects, so it is possible that the increase in average wages could be due to low earning workers leaving the firm, driving up average wages. We leave this as an important avenue for future work.

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APPENDIX A: HIGH PROFILE COLLUSION CASES

Date	Actors	Details
7 January 2021	Hungarian competition authority	The Hungarian competition authority fined the Association of Hungarian Human Resource Consulting Agencies €2.8 million for imposing rules on its members that set minimum prices for their services and prevented them from recruiting each other's employees.
16 March 2021	Brazilian competition authority	The Brazilian competition authority, CADE, opened an investigation into three dozen companies, including the Brazilian subsidiaries of Abbott, Acelity LP, Baxter, Bayer and Siemens Healthcare —as well as 108 individuals linked to them —for allegedly engaging in wage-fixing labour agreements in the healthcare market.
23 September 2021	Mexican antitrust authority	Mexico's antitrust authority fined more than a dozen football clubs and the Mexican Football Federation approximately €7.5 million over salary caps for female players and labour movement restrictions.
1 February 2022	Romanian antitrust watchdog	Romania's antitrust watchdog announced it was investigating seven automotive engineering and technology providers for alleged no poach and wage-fixing agreements, in the agency's first probe into labour markets.
14 February 2022	Peruvian competition authority	Peru's competition authority launched an investigation into six construction companies after they allegedly agreed not to hire each other's employees, marking what appears to be the agency's first-ever probe into alleged no poach agreements.
7 March 2022	Greek antitrust authority	Greece's antitrust authority imposed behavioural remedies on an elevator maintenance and installation trade association for setting minimum wages, six weeks after fining another association in the sector for the same conduct.

3 May 2022	Portuguese competition authority	The Portuguese competition authority fined 31 football clubs and Portugal's Football League €11.3 million for implementing an anticompetitive no poach agreement, which agreed not to hire players who unilaterally terminated their employment contracts between 2019 and 2020 due to the covid-19 pandemic.
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Source: Stephenson Harwood. (2022). Competition and markets authority wage-fixing cartel probe. Retrieved October/November 2022, from [https://www.shlegal.com/docs/default-source/news-insights/documents/2022/1659097964-52296competition-and-markets-authority-wage-fixing-cartel-probe-\(104432784-1\).pdf?sfvrsn=3cfd5b_022](https://www.shlegal.com/docs/default-source/news-insights/documents/2022/1659097964-52296competition-and-markets-authority-wage-fixing-cartel-probe-(104432784-1).pdf?sfvrsn=3cfd5b_022)

APPENDIX B: DATA CLEANING PROCESS

All data analysis was conducted in STATA. The following section describes the steps we followed and their underlying rationale.

First we download from the Orbis M&A database all available M&A events in which either the acquiror or the target are located in the UK. Given the way the database is structured we have to make some adjustments in order for STATA to be able to interpret them. The main issue is that for instances in which an acquiror buys several targets in the same operation, some variables seem to be missing, but in reality they take the value of the first row of that deal, so we replace them with the value for the observation above them if they belong to the same operation. After this we only keep events in which both acquirors and targets are based in the UK, dropping all other observations. Given that we have many possible dates we could use to define when an M&A event takes place (date of announcement, date of completion, etc...), and some of them are incomplete, we decide to use date of completion (`complete_date`), and substitute it with date of announcement (`announce_date`) when it is missing. The rationale behind this is that the date of completion appears to be the most precise way of determining when the event factually takes place; further both date of completion and date of announcement are the date entries with the lowest number of missing values. Additionally, that many times both these dates coincide. The last two steps are to merge this dataset with the deflator series from the UK government (<https://www.gov.uk/government/collections/gdp-deflators-at-market-prices-and-money-gdp>) to be able to generate constant and therefore comparable financial values over time, and after we drop observations for which we do not have a Bvid number (the firm identifier for all Bureau van Dijck databases) for either the acquiror or target, as we need this unique identifier to merge in financial firm-level information from the Orbis database.

We acquire financial information for all UK companies for the relevant time period from the Historical Orbis database. We follow the same cleaning process as before. Subsequently we handle duplicate reports as for some firms there are multiple filings for the same year. We follow the approach detailed in De Loecker, Obermeier and Van Reenen (2022): Conditional on firm identifier, year and consolidation code, we i) take annual reports over local registry

filings, then ii) the observation with the fewer missing values, then iii) the first observation in the dataset. For firms in which the consolidation code varies between observations we choose consolidated reports (C#), over unconsolidated ones (U#).

Full Database:

Once we have constructed the main database we create average wage for both target and acquiror firms by dividing cost of employees between number of employees to then construct the synthetic variables and their log transformations as explained in the main body of the report by summing up employment in both firms and creating a synthetic wage based on the weighted average by employees of the average wages of both firms. We then drop the observations for which there is no wage or employment data in any period and create the treatment variables.

For more detailed information please see the do-files used for the process on request.

APPENDIX C: REGRESSION TABLES

Table C.1: Regression results – M&A activities on employment and wage

Dependent Variable	(1) Employment synthetic (ln)	(2) Employment synthetic (ln); same industry	(3) Wage synthetic (ln)	(4) Wage synthetic (ln); same industry	(5) Synthetic EBITDA (ln)
5 years before M&A	-0.139*** (0.0200)	-0.182*** (0.0247)	-0.0273*** (0.0103)	-0.0169 (0.0124)	-0.206*** (0.0347)
4 years before M&A	-0.117*** (0.0176)	-0.146*** (0.0208)	-0.0164* (0.00883)	-0.0168 (0.0111)	-0.159*** (0.0301)
3 years before M&A	-0.0717*** (0.0140)	-0.0741*** (0.0165)	-0.00858 (0.00825)	-0.0154 (0.0101)	-0.105*** (0.0254)
2 years before M&A	-0.0234** (0.0104)	-0.0275** (0.0123)	-0.0115* (0.00624)	-0.0117 (0.00761)	-0.0338 (0.0224)
Year of M&A	0.0154 (0.0134)	0.00754 (0.0160)	-0.0146* (0.00835)	-0.0142 (0.00973)	0.00929 (0.0259)
1 year after M&A	0.0413** (0.0195)	0.0217 (0.0256)	0.0288*** (0.00967)	0.0205 (0.0125)	0.0847** * (0.0307)
2 years after M&A	0.0485** (0.0245)	0.0313 (0.0336)	0.0370*** (0.0113)	0.0364** (0.0144)	0.124*** (0.0347)
3 years after M&A	0.0415 (0.0283)	0.00359 (0.0409)	0.0390*** (0.0120)	0.0223 (0.0165)	0.112*** (0.0402)
4 years after M&A	0.0288 (0.0318)	-0.0275 (0.0422)	0.0411*** (0.0140)	0.0318 (0.0196)	0.130*** (0.0430)
5 years after M&A	0.00177 (0.0369)	-0.0356 (0.0464)	0.0552*** (0.0165)	0.0324 (0.0216)	0.148*** (0.0496)
Observations	918,816	913,637	917,970	913,085	836,687
R-squared	0.016	0.016	0.004	0.004	0.007
Number of Firms (Clusters)	114,792	113,958	114,752	113,940	219,994

Notes: Robust standard errors in parentheses. All treatment coefficients measure the difference in the outcome variable with respect to the year before the M&A event. All specifications include time and firm fixed effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Source: Authors computations based on Orbis data

APPENDIX D: ADDITIONAL SURVEY INFORMATION

Table D.2: Survey Questions

“Which one of the following best describes your main job/role?”

“Which SIC code best describes your employer?”⁸

“How long have you been working for your current employer?”

“Regardless of whether you have requested any or not, has your current employer ever paid for (either fully or in-part) any training for you that is for the purpose of obtaining professional qualifications?”

“Excluding any training you have had that is designed just to help you perform your usual day-to-day role, have you received any training or coaching from your current employer that is designed to further your general personal or professional development?”

“Thinking about your current main employer, does this employer currently have an NCA in place with you?”

What was your employment status 5 years ago (i.e. in January 2018)?”

“Thinking back to 5 years ago, regardless of whether you were with the same employer or not, did that employer have an NCA in place with you at that time?”

Finally, questions examining whether an employee was dissuaded from applying to a competitor organisation depending on whether they had an NCA in place or not.

Notes: Survey included 9 questions beyond the demographic breaks (Gender, age, number of children, education attainment, yearly income, employment status, ethnicity, household size, marital status, region, and social grade).

⁸ SIC Codes are Standard Industry Codes, used to delineate the types of industry that exist. Industries such as “Financial Services” or “Real Estate” or “Construction” are listed in the survey itself.

Table D.2: Survey Descriptives

	Overall	With Non-competent Agreement	Without Non-competent Agreement	Difference
Gender				
Male	0.523	0.595	0.507	0.088
Age group				
Age: 16-24	0.101	0.192	0.072	0.12
Age: 25-34	0.254	0.346	0.211	0.135
Age: 35-44	0.229	0.223	0.221	0.002
Age: 45-54	0.237	0.155	0.276	-0.12
Age: 55-75	0.18	0.084	0.221	-0.137
Education Level				
Education: No formal qualifications	0.039	0.032	0.037	-0.005
Education: GCSE/O Level/NVQ12	0.264	0.202	0.269	-0.068
Education: A Level or equivalent	0.3	0.259	0.317	-0.057
Education: Degree/Masters/Ph D	0.397	0.507	0.378	0.129
Training				
Both Types of Training	0.323	0.606	0.222	0.384
Only Informal Training	0.063	0.067	0.062	0.005
Only Formal Training	0.168	0.137	0.180	-0.043
No Training	0.446	0.190	0.537	-0.347
Occupation				
Entrepreneurs/executives	0.023	0.043	0.024	0.019

Intellectual and highly skilled professionals	0.106	0.163	0.125	0.038
Technical professions	0.116	0.177	0.138	0.039
Executive professions in office work	0.163	0.215	0.211	0.004
Professions in business and personal sector	0.111	0.135	0.148	-0.013
Craftsmen, skilled laborers and farmers	0.061	0.091	0.072	0.018
Plant operators	0.033	0.036	0.046	-0.009
Occupations not requiring certification	0.068	0.053	0.106	-0.053
Other	0.078	0.070	0.117	-0.046
Social grade				
Social grade: AB	0.307	0.405	0.291	0.114
Social grade: C1	0.322	0.262	0.342	-0.08
Social grade: C2	0.205	0.231	0.194	0.037
Social grade: DE	0.166	0.102	0.174	-0.072
Employment status				
Employment: full-time	0.814	0.887	0.803	0.083
Employment: part-time	0.186	0.113	0.197	-0.083
Regions				
Region: Northeast	0.04	0.045	0.031	0.014
Region: Northwest	0.112	0.101	0.112	-0.011

Region: Yorkshire and Humberside	0.077	0.062	0.081	-0.019
Region: West Midlands	0.086	0.092	0.086	0.007
Region: East Midlands	0.078	0.08	0.068	0.011
Region: East of England	0.091	0.104	0.086	0.018
Region: Southwest	0.081	0.056	0.091	-0.035
Region: Southeast	0.139	0.112	0.163	-0.051
Region: Greater London	0.146	0.214	0.119	0.095
Region: Wales	0.04	0.048	0.041	0.007
Region: Scotland	0.081	0.069	0.087	-0.019
Region: Northern Ireland	0.029	0.018	0.036	-0.017

Income group

Income: up to £19,999	0.117	0.122	0.114	0.008
Income: £20,000-£34,999	0.269	0.239	0.269	-0.03
Income: £35,000 - £54,999	0.294	0.284	0.298	-0.014
Income: £55,000+	0.267	0.331	0.266	0.064
Income: refused	0.053	0.024	0.052	-0.029

Marital status

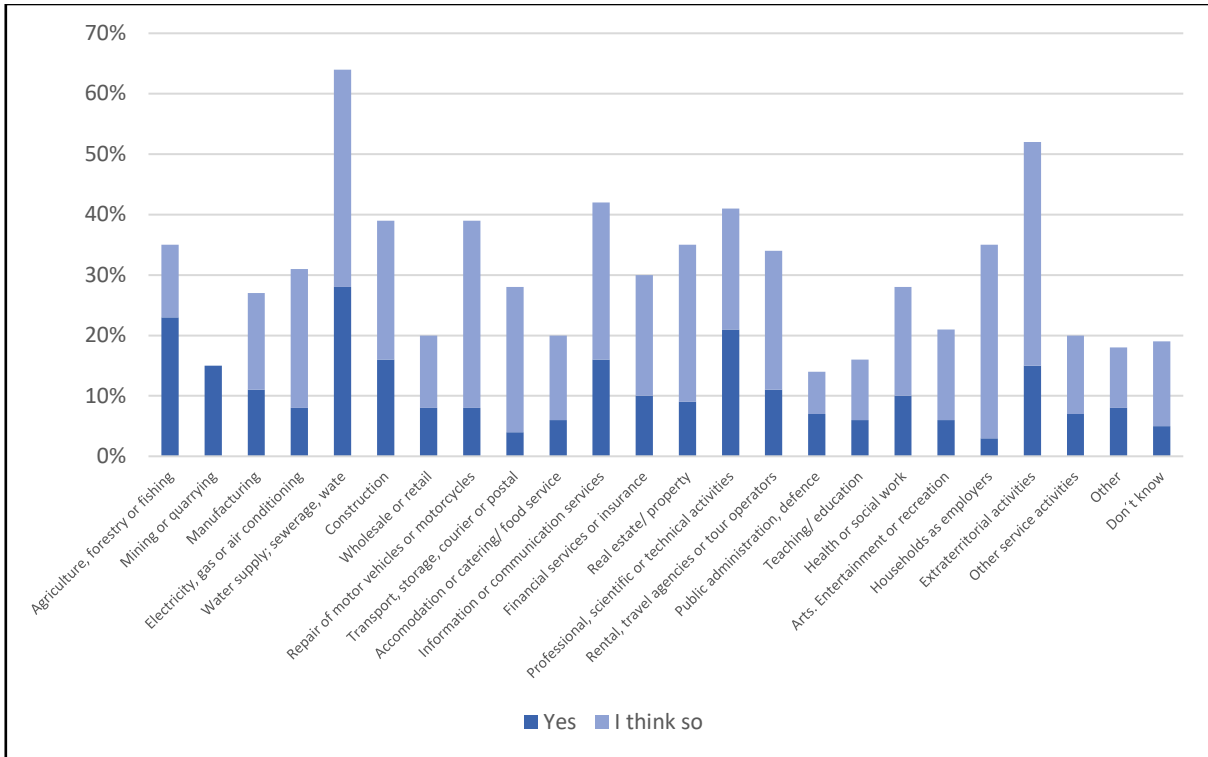
Marital status: married	0.643	0.695	0.626	0.069
Marital status: single	0.274	0.256	0.277	-0.02

Marital status: widowed/divorced/ separated	0.083	0.049	0.098	-0.049
Household size				
Household size: 1	0.15	0.108	0.172	-0.065
Household size: 2	0.322	0.254	0.352	-0.098
Household size: 3	0.246	0.304	0.221	0.083
Household size: 4+	0.282	0.335	0.255	0.08
Children HH				
Children HH (17 or under): none	0.429	0.567	0.368	0.2
Children HH (17 or under): any	0.571	0.433	0.632	-0.2

Notes: Averages are calculated by category, e.g.: 52.3% of all survey participants are male while 59.5% of individuals with non-competent are male and 50.7% of those without non-competent are male. This logic extends to other categories; “With NCA” is computed based on the NET of “yes” and “probably yes”, likewise “without NCA” is computed based on the NET of “no” and “probably no”; whenever the “Overall” fraction is not in-between the “with NCA” or “without NC” fractions, this has to do with the “Don’t know” respondents in the data rather than computing mistakes; *This table mirrors the sample means table that Boeri et al. (2022) displayed in their own analysis.*

Source: Authors’ computations based on survey results.

Figure D.1: Non-compete prevalence by industry



Notes: “Yes” includes respondents who say they definitely are under an NCA. “I think so” includes those who say they probably are under an NCA. Results are weighted using survey weights in order to make them more representative of the overall population. Numbers in brackets indicate the share of the overall population that fall within each category.

Source: Authors’ computations based on survey results.

Table D.3: Results of adjusted Wald-tests based on regression results

Model	Wald-test on	F-value	p-value
Specification (2)	Industry	3.19	0.00***
Specification (3)	Income brackets	1.69	0.096*
Specification (4)	Income brackets	1.61	0.115
	Education	2.01	0.091*
Specification (5)	Income brackets	1.68	0.099*
	Industry	3.18	0.00***
	Education	1.13	0.339
	Occupation	2.28	0.015**

Notes: Adjusted Wald-test checks joint statistical significance and results in F-value; * significant at 10%; ** significant at 5%; *** significant at 1%
Source: Authors' computations based on survey results.

APPENDIX E: EXAMPLES OF NCA REGULATION IN US STATES

Regions	Details
Alabama	NCA's are enforceable, but can only be agreed to after employment has started. Groups which cannot have NCA's: doctors, veterinarians, lawyers and other high skilled professions.
Arizona	NCA are enforceable except for: Broadcasters, physicians
California	NCA's are unenforceable. Non-solicitation enforceable under certain circumstances.
Colorado	NCA's generally not enforceable, except for purchase and sale of businesses
Connecticut	NCA are enforceable except for broadcasters and security guards
Delaware	NCA's are enforceable except for physicians
D.C.	NCA's are unenforceable, except for sale of business and for confidential information.
Florida	NCA's are enforceable except for mediators
Illinois	NCA's are enforceable except for broadcasters, government contractors, and physicians
Iowa	NCA's are enforceable except for franchisees that do not renew
Kansas	NCA's are enforceable except for accountants
Louisiana	NCA's are enforceable except for auto salesmen and real estate broker licensees (with special requirements), with special circumstances.
Maine	NCA's are enforceable except for broadcast industry professionals
Maryland	NCA's are enforceable except for broadcasters, physicians, nurses, social workers, and psychologists
Missouri	NCA's are enforceable except for secretaries and clerks
Montana	NCA's unenforceable except for the sale of a business or dissolution of partnership.

New Jersey	NCA's are enforceable except for in-house counsels and psychologists
North Dakota	Generally unenforceable, with narrow exceptions.
Oklahoma	NCA's unenforceable.
Oregon	NCA's enforceable only to those engaged in administrative, executive, or professional work who earn a salary and are paid on a salary basis (see ORS 653.020). NCA unenforceable against an employee whose total gross income is less than the median income of a family of four in Oregon as determined by the latest statistics from the U.S. Census Bureau (\$67,315 for 2013 in Oregon) (see ORS 653.295(1)(d)).
Tennessee	NCA's are enforceable except for physicians (in some circumstances)
Texas	NCA's are enforceable except for physicians (in some circumstances)
Vermont	NCA's are enforceable except for beauticians and cosmetologists (by their schools)
Washington	NCA's are enforceable except for broadcasters (in some circumstances)

Source: Legal Nature. Are non-compete agreements enforceable in my state? Retrieved March 21, 2023, from <https://www.legalnature.com/guides/are-non-compete-agreements-enforceable-in-my-state>

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