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Working paper

Off to the shops? Outside job opportunities and care worker labour supply

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Abstract

Demand for long-term care, and the labour required to provide these services, is rising across the world and shortages of care workers are common in many developed countries. The retail sector, a major employer of workers with similar levels of formal qualification, is often cited as an attractive outside option for care workers and a major drain of workers from the sector. However, empirical evidence for this relationship is scarce. In this paper, we study the impact of supermarket and warehouse openings on the labour supply of care workers in England. Using a difference-in-differences approach, we show that supermarket and warehouse openings are not associated with any local changes in the number of care home workers. These null results are relatively tightly estimated, ruling out meaningful adverse impacts of such openings on care labour supply. This suggests that, despite the prevailing popular narrative, the availability of retail jobs is unlikely to be an important factor in the long-term care sector's ability to attract and retain staff.

Keywords: long-term care; social care; care labour supply

JEL Classification: I11, J24, J45, J60

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1 Introduction

The long-term care (LTC) sector is large, labour-intensive and increasingly economic important in many countries. In England, where LTC is known as social care, the sector employed an estimated 1.6 million people in 2023/24 (Skills for Care, 2024), while 3.4 million people were employed in nursing home and LTC facilities in the US in 2020 (Martinez Hickey et al., 2022). Demand for LTC continues to grow due to demographic pressures, and providers are increasingly struggling to find sufficient numbers of staff: there were 131,000 vacancies in England alone in 2023/24 (Skills for Care, 2024), while employment in this sector fell by more than 10% in the US following the COVID-19 pandemic (Grabowski et al., 2023). Shortages of care staff are likely to have adverse health impacts for care users (Friedrich and Hackmann, 2021; Furtado and Ortega, 2023; Grabowski et al., 2023) and place a burden on the hospital system (Bakx et al., 2020; Crawford et al., 2021). Understanding which factors impact recruitment and retention in the LTC sector is therefore important for maintaining well functioning health and care systems.

One potential source of competition for workers in the LTC sector comes from the retail sector, which employs large numbers of workers with similar levels of formal qualification. In England, a particularly common refrain is the unfavourable comparison between care workers' pay and the wages on offer in supermarkets (Bottery, 2022). One care manager quoted in a recent UK House of Commons Health and Social Care Committee report into the LTC sector stated that *"I dread hearing Aldi opening up nearby, as I know I will lose staff"* (House of Commons Health and Social Care Committee, 2022).¹ The president of the Royal College of Emergency Medicine publicly called for additional funding for long-term care in order to *"Pay people more than stacking shelves in Tesco gives them"* and address staff shortages (Hayward and Willoughby, 2022).² The leader of the Liberal Democrats, Sir Ed Davey MP, argued in the UK Parliament in May 2025 that *"[we] still see people earning more in Amazon warehouses and supermarkets than in care homes, and that will mean our loved ones going without the care they need"* (Davey, 2025).

¹Aldi is a supermarket chain that has expanded substantially in the UK over the past decade.

²Tesco is a large UK supermarket chain.

Even where pay is similar, roles in retail are often perceived as being more attractive: another care employer told an interviewer that *“If you are offered the same amount of money working for the store on the corner or for a supermarket, why would you ever come and work for us?”* (Migration Advisory Committee, 2022), with such concerns frequently cited in the UK media.³ This is also a concern in other countries beyond the UK. For example, in Australia, a trade union leader told a Royal Commission into long-term care that *“the comment I hear when I go and have meetings [with members] is ‘I could get paid more working on the checkout at Aldi’ and it’s technically true”*.⁴ Similar issues have been reported in the United States (Kim, 2024) and Canada (Kokorelias and Sinha, 2023). However, there is currently little credible empirical evidence to validate these concerns.⁵

In this paper, we directly study the impact of increases in the availability of retail and distribution jobs on the labour supply of care home workers in England. Specifically, we exploit openings of large supermarkets and Amazon warehouses to study how the labour supply of care home workers in the same geographic labour market changes in response to these events. Both of these events could expand the local availability of jobs in the retail and distribution sector, and are plausibly exogenous to other factors affecting employment at care homes.

We use a difference-in-differences approach at the care home provider level, comparing changes in the number of staff employed by providers before and after a new supermarket opens in the local area with providers located in areas without new openings. We also examine the impact on vacancies, and on the turnover, new starter and leaver rates at these providers. We then repeat this analysis using openings of Amazon warehouses instead of supermarkets, events which are rarer but represent larger shocks to the local labour market. In all cases, we study impacts up to four years after an opening in the local area. We define provider-specific labour markets based on the distances travelled by staff to the care home over our study period.⁶

³<https://www.theguardian.com/society/2021/sep/04/care-workers-in-england-leaving-for-amazon-and-other-better-paid-jobs>, accessed 25 April 2025

⁴<https://anmj.org.au/aged-care-workers-underpaid-undervalued-and-short-staffed-royal-commission-told/>, accessed 22 April 2025

⁵Previous work examining the destinations of care workers leaving the sector in England shows that around 10% of those moving into the care sector between 2011/12 and 2021/22 previously worked as retail and sales assistants, and around 3% of those leaving the sector went on to work in such roles (Kelly et al., 2022). Similar results were found by Forth and Bryson (2022). These flows will not reflect potential employees who never join the long-term care sector due to alternative opportunities in retail or other industries.

⁶This captures differences in the size of labour market across different areas (e.g. markets are geographically

To avoid the well documented problems with staggered treatment and heterogeneous treatment effects, our baseline analysis uses the estimator proposed by Callaway and Sant’Anna (2021).⁷

For this approach to identify the causal impact of these openings on care labour supply, the identifying assumption is that care labour supply would have evolved in the same way across areas with and without supermarket or warehouse openings had those establishments not opened. New supermarkets and warehouses are, of course, not located at random, and this raises two particular concerns that could violate this identifying assumption. First, that new establishments are located in areas with already strong growth in labour supply. This might be the case if economic conditions were improving quickly, or if the (working age) population was growing strongly for other reasons. This could generate a spurious positive correlation between the supply of care workers and the opening of a new supermarket or warehouse, and lead us to underestimate any negative impact of new openings on care worker labour supply. Second, new establishments may be located in areas with stronger growth in demand for care (and therefore, in turn, demand for care workers). This could be the case if, for example, the local population was becoming older or sicker on average.

We take three main steps to address these concerns. First, we control for changes in population growth and economic conditions in the period prior to openings in our difference-in-differences estimation.⁸ Second, we convincingly demonstrate that treated and untreated long-term care providers were on similar trends prior to supermarket and warehouse openings. Third, we undertake a whole battery of robustness checks, including the use of alternative difference-in-differences estimators, such as OLS and the imputation estimator of Borusyak et al. (2024), where the inclusion of time-varying controls does not meaningfully affect our estimates.

Using this approach, we show that openings of supermarkets and Amazon warehouses had little impact on employment levels, vacancy rates, and staff turnover in nearby care homes. In our baseline specification, the estimates for these outcomes are close to zero, statistically insignificant and precisely estimated. Our primary analysis suggests that, if anything, care home

smaller in large cities and bigger in rural areas where staff routinely travel further distances to work). We show robustness to using alternative labour market definitions in Section 5.3

⁷We also show that our results are robust to using other estimators.

⁸In the traditional OLS approach, we would include time-varying controls for demographic and economic conditions. Using the Callaway and Sant’Anna (2021) estimator, we instead control for the pre-period values of these control variables.

staffing slightly *increased* after a supermarket opening (by 0.1 members of staff per care home). The point estimate at the lower end of the 95% confidence interval is a reduction of 0.4 staff members per care home, equivalent to a reduction of 1.4% of the pre-treatment mean staffing total. In some robustness checks, employing alternative specifications, the estimated effect is negative and statistically significant (though still relatively small in size): the point estimate when using the imputation estimator of Borusyak et al. (2024) suggests a staffing reduction of 1.9% among affected care homes (3.5% at the lower end of the confidence interval). This bounding exercise, and the fact that our primary analysis suggests a much smaller (and statistically insignificant effect), leads us to confidently rule out meaningful adverse impacts of such openings on care labour supply.

We additionally find that supermarket and Amazon warehouse openings had no statistically significant effect on average wages in local care homes. Similarly, we find no changes in training provision or employee characteristics. This suggests that providers do not respond to increased job availability at supermarkets or warehouses by increasing rates of pay to retain staff, by providing more training, or altering the composition of their workforce.

This points instead to two alternative explanations. The first is that these openings do not represent sufficiently large labour market shocks for them to be statistically detectable in our framework. For context, the average supermarket opening that we study – which we would expect to create in the region of 100–150 jobs – affects 15 local care homes in our sample, employing between them an average 472 care workers. The second is that supermarket and Amazon warehouse jobs are not relevant outside options for most care workers, requiring different skills or preferences. In both cases, the key point is that these are potential mechanisms behind our results, and reasons why the policy and media focus on supermarket and Amazon jobs might be misplaced; we provide the first empirical evidence in support of this view.

This paper contributes to the literature on the determinants of labour supply in the LTC sector. Previous work, much of it focused on the US, has examined a number of shocks to, and potential determinants of, LTC labour supply. These include the impact of unemployment in the wider economy (Konetzka et al., 2018; Antwi and Bowblis, 2018), immigration (Furtado and Ortega, 2023; Grabowski et al., 2023), parental leave policies (Friedrich and Hackmann, 2021)

and minimum wages (Ruffini, 2022; Giupponi and Machin, 2018).⁹ We extend the evidence base by examining the impact of clear, well-defined shocks to the outside job opportunities available to care home workers in England. This also speaks to the broader literature on the role of outside pay options in determining public service performance (Propper and Van Reenen, 2010; Britton and Propper, 2016).

We also provide an important contribution to an ongoing policy debate, as UK policymakers consider extending the role of government in pay- and price-setting in the LTC sector. Shortages in LTC staff are often attributed to the relatively favourable conditions in the retail sector. Building upon previous descriptive work studying flows of workers in and out of long-term care in England (Forth and Bryson, 2022; Kelly et al., 2022), our results show that an increase in the number of available local jobs in these sectors has no clear or large impact on the care home labour market. This suggests that policymakers concerned with measuring outside pay options and job opportunities for care workers, and those designing policy aimed at improving retention in the sector, ought to focus their attention on other, more relevant, labour market opportunities.

The rest of the paper is organised as follows. In Section 2, we set out the institutional details of the LTC sector in England. In Section 3 we describe the data used in the analysis. In Section 4 we set out our empirical approach. In Section 5 we show our results, first showing the overall impact on staff numbers before considering other measures of entry and exit of staff, and the mechanisms that might explain our findings. Section 6 concludes.

⁹Konetzka et al. (2018) show that lower levels of local unemployment lead to an outflow of registered nurses from US nursing homes, and an inflow of less qualified licensed practical nurses. Antwi and Bowlis (2018) find that lower unemployment levels increase turnover rates in US nursing homes and reduce care quality. Grabowski et al. (2023) and Furtado and Ortega (2023) show that immigration increases staffing in nursing homes in the US; both studies find positive associated impacts on patient outcomes. Friedrich and Hackmann (2021) exploit a reform to parental leave in Denmark to examine the impact of LTC staff shortages on patient outcomes; they find that the policy reduced nurse labour supply in nursing homes and reduced care quality. Ruffini (2022) and Giupponi and Machin (2018) examine the impact of changes in the minimum wage on staff retention and care quality in care homes in the US and UK, respectively. Ruffini (2022) finds that higher minimum wages improved LTC staff retention and improved care quality in the US. Giupponi and Machin (2018) find that higher wages induced by minimum wage policies increase wages in the LTC sector without corresponding falls in employment, but that care providers lower care quality in response.

2 Background

2.1 Adult social care in England

Long-term care is typically referred to as adult social care (ASC) in England. ASC covers a broad range of non-medical services provided to individuals to help them carry out activities of daily living, such as housework, washing, general mobility and using the toilet. Care can either be provided informally by family, friends and neighbours, or provided formally by trained professionals. Formal care can be provided either in a care recipient's home (referred to as domiciliary care) or in care homes (which are the focus of this study). In 2022/23 there were approximately 370,000 care home residents in England (Office for National Statistics, 2023) living in around 15,000 separate care homes (Skills for Care, 2024).

2.1.1 Funding

Formal care can be either privately or publicly financed. Publicly funded care is subject to a needs test (with would-be recipients needing to have care needs above a certain severity threshold to qualify) and a financial means test (which has both an income and an assets component). Around 37% of care home residents, and 49% of residents of care homes providing care for the elderly, are classified as 'self-funding' their own care, with the remainder being funded by the state (Office for National Statistics, 2023). This is in contrast to publicly funded healthcare, which is provided free at the point of use to all residents in England by the National Health Service (NHS).

Adult social care in England is decentralised and is the responsibility of 152 local governments (known as 'local authorities' or councils). This includes the responsibility for the commissioning of care services from local providers and the setting of the fees paid for such services. Local authority revenues come from a combination of local tax revenues and grants from central government. The latter were cut sharply during the 2010s as part of a broader programme of public spending cuts. As a result, per-person spending on services by English local authorities fell by 24% between 2009/10 and 2019/20 (Ogden et al., 2021). Per-person spending on adult social care was also subject to large cuts over this period (Crawford et al.,

2021). In a context of growing demand for care as the population aged, this led to care rationing and a squeeze on the fees paid to care providers. By 2022, a UK government study found that a significant number of local authorities were paying prices below the cost of providing care (Department of Health and Social Care, 2022). This is important for understanding the pay trends discussed below.

2.1.2 Workforce

Approximately 1.6 million people were employed in the adult social care sector in England in 2023/24 in around 1.3 million full-time equivalent posts (Skills for Care, 2024). Of these, around one-in-seven (14% of posts) represent public sector roles (whether employed by local authorities or the NHS), with the remainder directly employed by care recipients (7%) or, more commonly, employed by independent providers (79%, a combination of private and voluntary sector organisations). In other words, most care workers are employed in the private sector.

Adult social care is officially defined as a low-paying industry by the UK government (Low Pay Commission, 2022). In March 2024 the median hourly rate for care workers was £11.00, 58 pence (5.6%) above the minimum wage. In March 2016, the equivalent gap was also 58 pence, then equivalent to 8.7% more than the minimum wage. Relatedly, pay differentials between more and less experienced care workers have narrowed over time. In March 2016 a care worker with five or more years of experience received an average hourly pay premium of 4.4% (33 pence per hour); by 2024, this had fallen to 0.9% (10 pence) (Skills for Care, 2024).

In the early 2010s, median hourly pay for care workers was higher than for sales and retail assistants (around 2% higher in 2012/13) (Skills for Care, 2021). By 2020/21 this had reversed, with sales and retail assistants receiving median hourly pay around 2% higher. More generally, over that period – the period which we study – pay in the care sector fell relative to pay in competing occupations (Migration Advisory Committee, 2022). In 2023/24, median hourly pay for care workers and for sales and retail assistants was identical (£11.00 per hour). Hourly pay for workers in Amazon distribution centres was considerably higher, with starting pay rates of between £11.80 and £12.50, depending on location, in late 2023.

2.2 Retail jobs and the long-term care policy debate

There is a prevalent narrative in the UK that better-paying jobs in the retail sector, which come with other non-pecuniary benefits, are an important outside option for care workers, and that this contributes to the challenges around recruitment and retention in the long-term care sector. The jobs and pay on offer in supermarkets is a particularly common point of comparison: the president of the Royal College of Emergency Medicine explicitly called for pay for care workers to be more competitive with the pay offered to those stacking shelves, and the Chief Executive of the NHS Confederation pointed to wage inflation in the retail sector as a factor exacerbating staff shortages (Hayward and Willoughby, 2022). Similarly, one care manager bemoaned his inability to compete with pay in retail and hospitality, additionally adding that “We can’t afford to offer £1,000 golden hellos like Amazon” (Ibid.). Such concerns are not restricted to the UK, and have been echoed in countries including the United States (Kim, 2024), Canada (Kokorelias and Sinha, 2023) and Australia (Fedele, 2019).

There are also broader concerns about the impacts of low pay in the sector: the UK government’s independent Migration Advisory Committee concluded that “one cannot seriously address the workforce issues in social care unless pay is improved; this is essential to boosting recruitment and improving retention” (Migration Advisory Committee, 2022).

Although most care workers are employed in the private sector, and not directly by the government, private sector providers are still constrained in what they can afford to pay their staff by the prices paid to them by local authorities. Furthermore, a sizeable proportion of care workers (around one-in-five) are on the state-determined minimum wage and an ever larger group are just above it (Skills for Care, 2024). In other words, even though the government does not directly set the pay for most care workers, it plays an immensely important *indirect* role through setting the price paid for local care packages and through setting the level of the minimum wage.

The degree of state involvement in price- and pay-setting in the sector has increased in recent years. One of the aims of the previous UK government’s 2022/23 ‘Fair Cost of Care’ exercise and Market Sustainability and Improvement Fund was to increase and standardise the fees paid to providers, in order to ensure that local authorities do not pay below the cost of pro-

viding care and undermine the sustainability of the sector.¹⁰ This involvement seems likely to continue. Furthermore, the new government has promised to introduce a ‘Fair Pay Agreement’ for the sector, and recent legislation establishes a framework for pay negotiations between employers and worker representatives in the sector. Whether the state is setting pay directly, or whether decisions are reached through state-facilitated negotiations between employers and employees, it will be important to have an accurate understanding of the relevant outside options for care workers and the impact that these have on workforce outcomes. We now turn to the data and empirical strategy we use to enhance that understanding.

3 Data

3.1 Adult social care data

Our primary data source is the Adult Social Care Workforce Dataset (ASC-WDS) and its predecessor the National Minimum Dataset for Social Care (NMDS-SC). These data are collected by Skills for Care, the strategic workforce development and planning body for adult social care in England. The data are effectively an employer survey: each participating provider is given access to an online platform where they can upload workforce data at both the provider and worker level. Our data are comprised of six-monthly extracts from this system between March 2013 and March 2022. Each extract contains the last updated records for each employer.¹¹

It is mandatory for public sector providers of adult social care to provide data. As a result, our dataset includes all public sector providers. It is not mandatory for private sector providers to provide data, but they are given a number of incentives to do so. This includes access to workforce development funding and access to analytics and insights produced by Skills for Care. The dataset covers just under half of private sector providers.¹²

The data include providers of both residential care (i.e. care homes) and domiciliary care

¹⁰<https://www.gov.uk/government/publications/market-sustainability-and-fair-cost-of-care-fund-2022-to-2023-guidance/market-sustainability-and-fair-cost-of-care-fund-2022-to-2023-guidance>

¹¹All employers can upload data at any point. Employers are encouraged to upload data at least once per year (in September), but a majority choose to do so on a more frequent basis.

¹²<https://www.skillsforcare.org.uk/Adult-Social-Care-Workforce-Data/Workforce-intelligence/about-us/Methodology.aspx>

(i.e. care given to people in their own homes). In our primary analysis we restrict our attention only to providers of residential care because it is easier to identify where staff work. In Section 5.3, we extend the analysis to include providers that also provide domiciliary care.

When using the data, we restrict our sample to observations that were updated within 6 months of the extract date. We also restrict to observations where the provider has valid values for all of our outcome variables of interest. This cleaning is based on the data filters developed by Skills for Care and more detail is contained in Appendix A.

These restrictions mean that our sample may not be fully representative of the Adult Social Care Workforce Dataset. Moreover, because the Adult Social Care Workforce Dataset itself is a non-random sample of social care providers, our sample may not be fully representative of the wider sector. However, by definition, we do not have data on those that are not included in the dataset or have invalid data, and so we are unable to formally test this. Nonetheless, our sample contains a substantial portion of a large sector, and both care providers and the UK government have sufficient confidence in the underlying data to use it to inform decision making.

Our raw data is not balanced across providers for two reasons: some providers open and close over time; and some start and stop reporting (valid) data over time. Restricting our sample to a balanced panel would substantially reduce our sample size. In our primary analysis, we therefore use an unbalanced panel. We think it is unlikely that the opening of a supermarket nearby would affect whether a provider reports (valid) data or lead to a provider closing. For robustness, we repeat our analysis using a balanced and much smaller panel, and report these results in Section 5.3.¹³

We consider six staffing outcome variables of interest. The first is the total number of staff at the workplace. The second is the vacancy rate, which is defined as the number of open vacancies divided by the total number of current staff. The third is the turnover rate, which is defined as the number of staff who left in the last 12 months divided by the total number of staff 12 months ago. This requires a provider to have reported data a year before. The fourth is the starter rate, which is defined as the number of new staff in the last 12 months divided by the total number of current staff. The fifth measure is the leaver rate, which is defined as the

¹³We have checked and find no evidence that provider exit from our sample increases after the opening of a new supermarket. Results are available on request.

number of staff who left in the last 12 months divided by the total number of current staff.¹⁴ Finally, to study some of the potential mechanisms that drive our results, we also consider mean hourly wages of workers.

3.2 Supermarket and warehouse data

To measure the openings of new supermarkets, we use data on UK retail locations collected by Geolytix, a firm that collects and analyses geographic data. These data periodically record the full location of each supermarket in the UK, its brand and its size. We use these data to measure supermarket openings between April 2016 and June 2022.¹⁵

We define a supermarket as opening if it appears in the data for the first time. We only consider major supermarket brands that have been recorded consistently in the dataset over time. We also impose a number of requirements to ensure that we capture genuine openings of new supermarkets, rather than changes in which firm is operating the supermarket. These restrictions and our validation test are described in Appendix A.

In our primary analysis, we restrict our sample to the opening of large supermarkets, defined as those with a footprint greater than 1,400 square metres. This is because these supermarkets are likely to hire the greatest number of workers and therefore have the largest impact on local labour markets. Overall, there are 178 large supermarkets that open during our sample period. Concrete data on the number of jobs created in each opening is difficult to come by. In 2022, there were 50,000 local business units in the UK in the industry code that includes supermarkets (retail sale in non-specialised stores with food; beverages or tobacco predominating). Of these, 1,420 employed 100-249 staff and 875 employed 250 or more. Our supermarket data features 1,439 large supermarkets in 2022, which suggests that large supermarkets employ at least 100 staff (Office for National Statistics, 2024).¹⁶

¹⁴This is similar to our measure of turnover, but does not require a provider to have reported data a year before, and therefore allows us to study a larger sample.

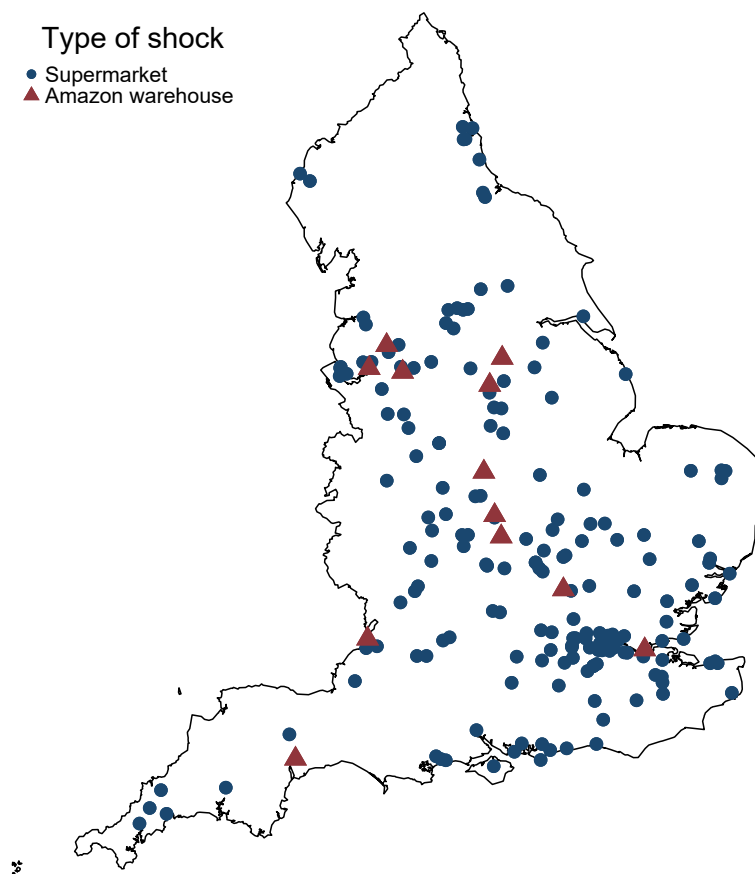
¹⁵Although the data start earlier, in October 2014, we do not use it because it does not include all major UK supermarket brands until the end of 2015.

¹⁶This is consistent with other sources. For example, one major supermarket chain employs around 180 staff per store (<https://corporate.asda.com/newsroom/2023/02/17/asda-to-increase-retail-colleague-pay-rates-by-10>), and news reports that accompany such openings suggest that they can create as many as 150 new jobs (e.g. <https://www.lep.co.uk/news/new-sainsburys-set-to-open-in-seaside-resort-soon-here-is-everything-we-know-4525554>).

We also consider the impact of the opening of Amazon warehouses. We collected data on the location, date of opening and size of new warehouses using Amazon’s own public records and media reports. Appendix A has more details. Between 2015 and 2019 we document twelve new openings, with a reported average employment of 750 at each site. These events are therefore much rarer than supermarket openings, but due to the large number of (higher paying) jobs created at these warehouses, they are likely to have much larger impacts on the local labour market.

Figure 1 shows the geographic distribution of both types of shocks. Both types of shocks are distributed throughout the country.

Figure 1: Geographic distribution of new supermarkets and Amazon warehouses



3.3 Defining an affected provider

For each care home provider, we define a geographic labour market based on the commuting distances of their employees. Specifically, we calculate the 66th percentile distance between

workers' home addresses and the provider location over our whole sample.¹⁷ We then classify a care home provider as affected by a new supermarket or warehouse if a new establishment is opened within this distance.¹⁸ Using the distances travelled by staff employed by each care home allows us to vary the geographic size of the labour market across providers and therefore account for the fact that workers travel different distances in different parts of the country (e.g. those in cities will travel a shorter distance than those living in rural areas). In Section 3.5 we document the size of these labour markets. To test robustness, we vary the cutoff used, employing different distance percentiles and a constant radius (5km).

Using our primary definition, 30% of providers are affected by a new supermarket during our sample period. Because multiple supermarkets can open near a provider over time, 13% of providers receive more than one 'shock'. In our primary analysis, we include these providers and define treatment based on the first shock. We show our results are robust to accounting for multiple openings with OLS and to excluding all providers that experienced more than one shock. 2% of providers in our sample are affected by an Amazon warehouse opening during our sample period, using the same primary definition of a local labour market.

3.4 Local area controls

In our primary analysis, we control for population and earnings growth. This is to account for the systematic choice of opening locations by supermarkets, which we discuss in Section 4. In both cases, these are measured at the upper-tier local authority level using 2021 boundaries.¹⁹ For population growth, we measure the total annual population growth and the annual population growth for those aged 65-74, 75-84 and 85+ using official population estimates from the Office for National Statistics. For earnings growth, we measure the annual growth in median

¹⁷Ideally we would only use pre-treatment data to calculate this distance, in case a new supermarket or warehouse changed commuting patterns. But our definition of treatment requires us to know the 66th percentile of commuting distance (in order to draw a radius and look for supermarket openings). Without knowing the distance, there can be no treatment, and so no pre-treatment period. This makes it impossible to measure pre-treatment commuting distance. We therefore use the average over the whole sample period. For providers with insufficient or missing data we use the local authority average.

¹⁸We calculate the distance as the centroid of the Lower Super Output Area (LSOA) that the provider is located in and the centroid of the LSOA where the opening takes place. The time periods in our two datasets do not always align perfectly. We assign the opening to the six month period in the provider data that most closely matches the opening period in the supermarket data (which are not uniform).

¹⁹Changes in boundaries mean that we do not have median earnings growth for two local authorities for part of our sample period. We exclude affected provider observations from our sample.

real weekly earnings, also based on estimates from the Office for National Statistics.

3.5 Summary statistics

Table 1 summarises our main variables of interest. Column 1 and 2 show the mean and standard deviation of each variable for all providers in our sample calculated at the provider-six month observation level. Column 3 and 4 show this just for providers that experience a supermarket opening during our sample period, and column 5 and 6 show this for providers that did not experience a supermarket opening during our sample period. Column 7 shows the difference in means of the two groups and column 8 shows the p-value for this difference. The final row shows the number of unique providers over our whole sample period.

Table 1: Provider summary statistics

Variable	All		Supermarket opening		No opening		Difference (3 - 5)	
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Mean	(6) SD	(7) Value	(8) p-value
Total staff	35.7	28.3	33.3	29.9	36.7	27.6	-3.39	0.000
Vacancies rate	0.053	0.110	0.062	0.126	0.049	0.103	0.013	0.000
Turnover	0.273	0.289	0.274	0.322	0.272	0.274	0.002	0.498
Starters rate	0.284	0.285	0.282	0.300	0.285	0.278	-0.003	0.207
Leavers rate	0.253	0.262	0.251	0.273	0.254	0.257	-0.003	0.178
Population growth	0.006	0.007	0.007	0.008	0.006	0.007	0.001	0.000
65–74 population growth	0.013	0.015	0.016	0.015	0.012	0.015	0.004	0.000
75–84 population growth	0.021	0.018	0.017	0.018	0.022	0.017	-0.004	0.000
85+ population growth	0.017	0.017	0.018	0.019	0.016	0.016	0.002	0.000
Median weekly earnings growth	0.007	0.033	0.005	0.032	0.008	0.033	-0.003	0.000
Labour market size (km)*	7.03	3.93	8.53	4.83	6.36	3.23	2.18	0.000
Labour market size (ASC staff)*	618	816	1056	1166	376	345	680	0.000
N unique providers	10,130		3,141		6,989			

Note: Means and standard deviations calculated at the observation level. * These are means at the provider level, rather than the observation level. Labour market size (ASC staff) is defined using providers present in the sample in September 2015 excluding their own employment.

The average provider employs 36 staff and has a vacancy rate of 5%. On average, a provider has 28% of its current workforce start and 25% leave in a year. Providers that experience the opening of a new supermarket are slightly smaller on average, have a higher vacancy rate, but have similar turnover, starters and leavers rates to providers that do not experience an opening.

Areas with a new supermarket have a faster growing total population on average, but there is no clear pattern across areas in which age group drives this population growth (which is important, given that we might worry in particular about the additional social care demand from a growing elderly population). Places with a new supermarket have a slower growth in median weekly earnings on average. Any permanent differences between areas with and without an opening are not a threat to our identification, but these differences in population and income growth rates motivate our controls in Section 4.

Table 1 also documents the size of the labour markets we use in our primary analysis. The mean provider has a labour market with a 7km radius and in September 2015, the mean labour market contained 618 other workers in our care home sample. Providers affected by a supermarket opening have substantially larger labour markets than those not affected. This may reflect differences in the types of areas that supermarkets open in, but also mechanically reflects the fact that providers with larger geographic labour markets are more likely to be affected by new openings.

An alternative way to think about the size of local labour markets is to consider the number of providers and staff affected by each opening event. In our sample, a supermarket opening affects a mean of 15 providers (median 12) who were present the period before the opening. Together these affected care providers employed an average of 472 adult social care staff (median 396).

4 Empirical Strategy

To estimate the impact of a supermarket opening on adult social care providers, we take a difference-in-differences approach and compare providers with and without a new supermarket before and after the opening. There are now well known problems with estimating difference-in-differences using a two way fixed effect approach when there are staggered treatments and heterogeneous treatment effects (De Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022). For our primary estimates we therefore use the estimator proposed by Callaway and Sant’Anna (2021). For robustness we repeat

our analysis with the imputation estimator of Borusyak et al. (2024) and the traditional OLS approach.

The key idea of Callaway and Sant’Anna (2021) is to estimate the average treatment effect on the treated (ATT) for each treatment cohort (g) and time period (t). Using the standard potential outcomes framework, these ATTs are defined as

$$ATT(g, t) = E[Y_t(g) - Y_t(0) | G_g = 1] \quad (1)$$

where G_g is an indicator for being treated in period g .

We cannot directly estimate these ATTs because we do not observe the counterfactual outcome for treated providers. We must therefore make two assumptions. First, we assume that there is no anticipation.²⁰ We think it is likely that staff working in care homes are unaware of new supermarkets substantially prior to the opening, and even if they were aware they would not leave their current job before the supermarket started recruiting staff.

Second we assume that there are parallel trends post-treatment between the treated providers and never-treated providers, conditional on observables X one period before treatment. In particular, for all $g \in G, t \in \{g, \dots, T\}$

$$E[Y_t(0) - Y_{t-1}(0) | X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0) | X, C = 1] \quad (2)$$

where $C = 1$ is an indicator for being never treated.

This parallel trends assumption may not hold unconditionally (without the X), because businesses choose the locations in which to open new supermarkets and warehouses in a non-random fashion. We think there are two likely threats. The first is population growth. Supermarkets on average open in areas with faster growing populations, and it may be that the adult social care providers in these areas have a growing workforce, either because of higher demand for social care services from the growing local population, or because the available workforce is larger and they are better able to fill posts. We therefore control for pre-treatment total population growth, as well as population growth for those aged 65-74, 75-84 and 85+ sep-

²⁰This is equivalent to $E[Y_t(g) | X, G_g = 1] = E[Y_t(0) | X, G_g = 1]$ for $t \in \{1, \dots, g-1\}$

arately. The second threat is economic conditions. It may be that supermarkets or warehouses open in areas with better (or worse) economic trends, and this could be correlated with adult social care outcomes (again, either because of changing demand for services or changes in the available workforce). We therefore control for the pre-treatment annual growth in real median gross weekly earnings to capture these differences in local economic conditions.

In all cases, we also test whether outcomes in treated and never treated providers were on different trends prior to a new supermarket opening. Although this is not a strict test of the parallel trends assumption, a lack of divergent trends prior to treatment is still reassuring evidence that we are unlikely to see divergent trends post-treatment. It also points to a lack of anticipatory effects.

One other concern is that there will be spillover effects from areas we define as being treated to nearby areas that we include in our control group. As a robustness check, we therefore repeat our analysis but with the control group defined to exclude providers that were between their 66th percentile and 90th percentile travel distance from the opening.

In the simplest case with no controls, to estimate the $ATT(g, t)$ for each g and t we would estimate the sample equivalent of

$$E[Y_t - Y_{g-1} | G_g = 1] - E[Y_t - Y_{g-1} | C = 1] \quad (3)$$

This is the standard 2x2 difference-in-differences comparison for a specific time period and treatment cohort. In our primary specification, we do control for observables and so we estimate the $ATT(g, t)$ using the doubly robust approach of Sant’Anna and Zhao (2020) within the Callaway and Sant’Anna (2021) estimator.

In this paper, we do not consider the individual $ATT(g, t)$ to be particularly informative. We therefore aggregate these into two sets of more useful treatment estimates. First, we aggregate our estimates into a set of event-study estimates four years around an opening, with the six months before treatment as our reference period. Second, we average our event study estimates over different post-treatment time horizons. In particular, we calculate the average treatment effects on the treated for the one year, two years and four years after a supermarket or warehouse has opened.

Throughout, standard errors are clustered at the Lower Super Output Area (LSOA) level, the small geographical census unit we use to determine whether providers are treated. Many providers are the sole provider within their LSOA, though in other cases there are several providers within an LSOA.²¹

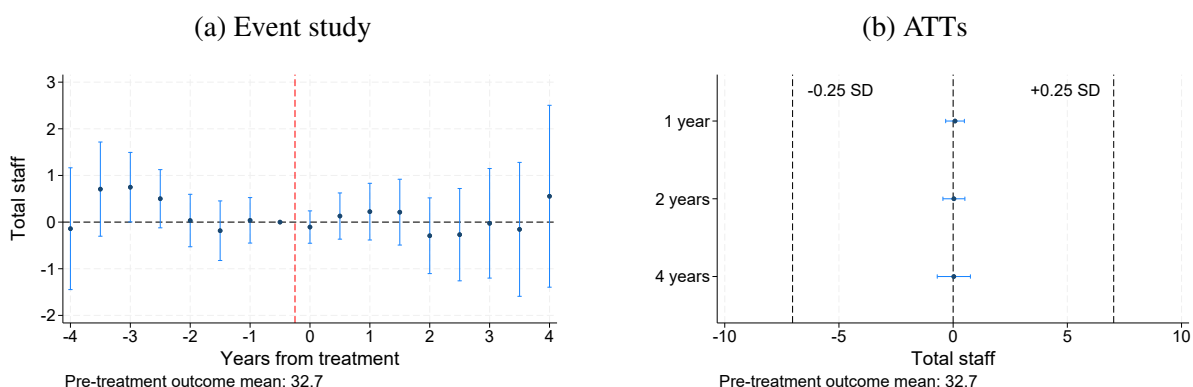
5 Results

We first present our results for the number of total staff employed by care home providers. Next we examine the impact of openings on vacancies, turnover, starter and leaver rates. We then show the robustness of our results to a battery of methodological variations. Finally, we examine and discuss potential mechanisms for our results.

5.1 Total staff

In this section we focus on the total staff employed by care home providers, as the broadest measure of the overall impacts of supermarket and warehouse openings on the care home workforce. Figure 2 shows the impacts of a new large supermarket opening. Panel (a) shows the event study results and Panel (b) shows the average treatment effects over different horizons. Appendix Table B1 gives the estimates of average treatment effects.

Figure 2: Total staffing results, supermarket opening



Note: Error bars show 95% confidence intervals.

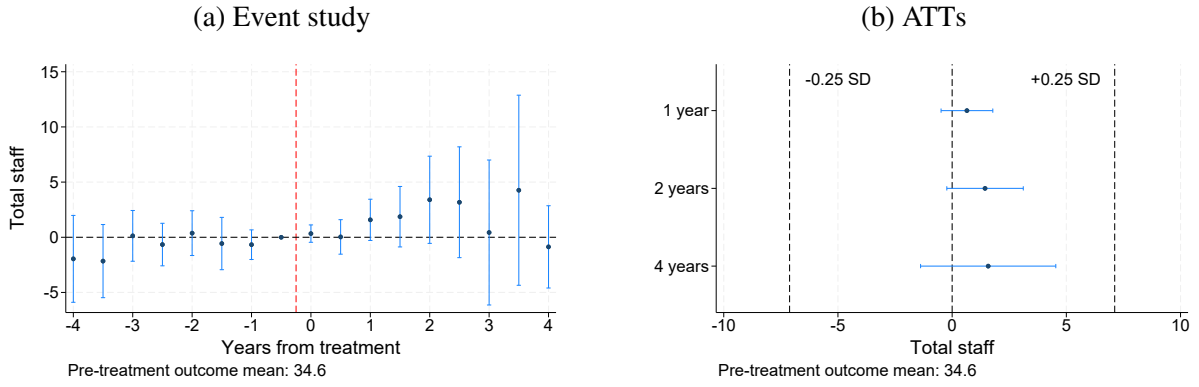
²¹It is not possible to cluster at the level of the labour markets we consider because these vary substantially by care home (even for the same supermarket opening) and overlap. As an alternative, we clustered at a much larger level (local authority) and standard errors for our primary results were only marginally higher. Results available on request.

Our results show there is no statistically significant impact of a new supermarket on the total staff employed by nearby care home providers over a four year period. For example, over the first year after an opening, we estimate there is a increase of 0.1 staff members in affected providers. This is small relative to the average of 32.7 staff six months before the opening. These estimates are also very precise, and so we can rule out economically meaningful changes. For example, our estimated effect over the first year has a 95% confidence interval of -0.3 to 0.5, equivalent to -1.0% to 1.5% of the pre-treatment mean. Contrary to the common narrative, we therefore find that new supermarkets have no meaningful impact on the staffing of care homes in this context.

Panel (a) also shows that there are no meaningful differential trends in outcomes prior to the opening of a new supermarket. This supports our identifying assumption that conditional on our controls, there is no differential trend between treated and never treated providers after treatment (Equation 2).

Figure 3 repeats our analysis of total staffing for the opening of Amazon warehouses. Appendix Table B2 gives the estimates of average treatment effects. Compared with our results for supermarkets, these estimates are less precise, because there are fewer new Amazon warehouses during the our sample period. Nonetheless, they tell the same story: there are no significant changes in nearby care home staffing after the opening of an Amazon warehouse. Again, the point estimates are positive (which may reflect wider impacts of new Amazon warehouses on the local economy and labour market). This means that despite the wider confidence intervals, we can still reject economically meaningful reductions in staff. Over the first year after a new warehouse, we estimate there is an increase of 0.6 staff members, with a 95% confidence interval of -0.5 to 1.8. As with our results for supermarkets, there are also no significant pre-trends in staffing.

Figure 3: Total staffing results, Amazon warehouse opening



Note: Error bars show 95% confidence intervals.

The previous analysis focused on all staff working in care home providers. Most policy discussion has focused on care workers specifically, who make up around half of the workforce. We have focused on all staff in our primary analysis because other low-paid staff at care homes may also be responsive to new supermarkets or Amazon warehouses. But in Figure B1 we repeat our analysis using the total number of care workers as our outcome variable. The results are very similar, with no significant reduction in the number of care workers working in care home providers near new supermarkets or warehouses.

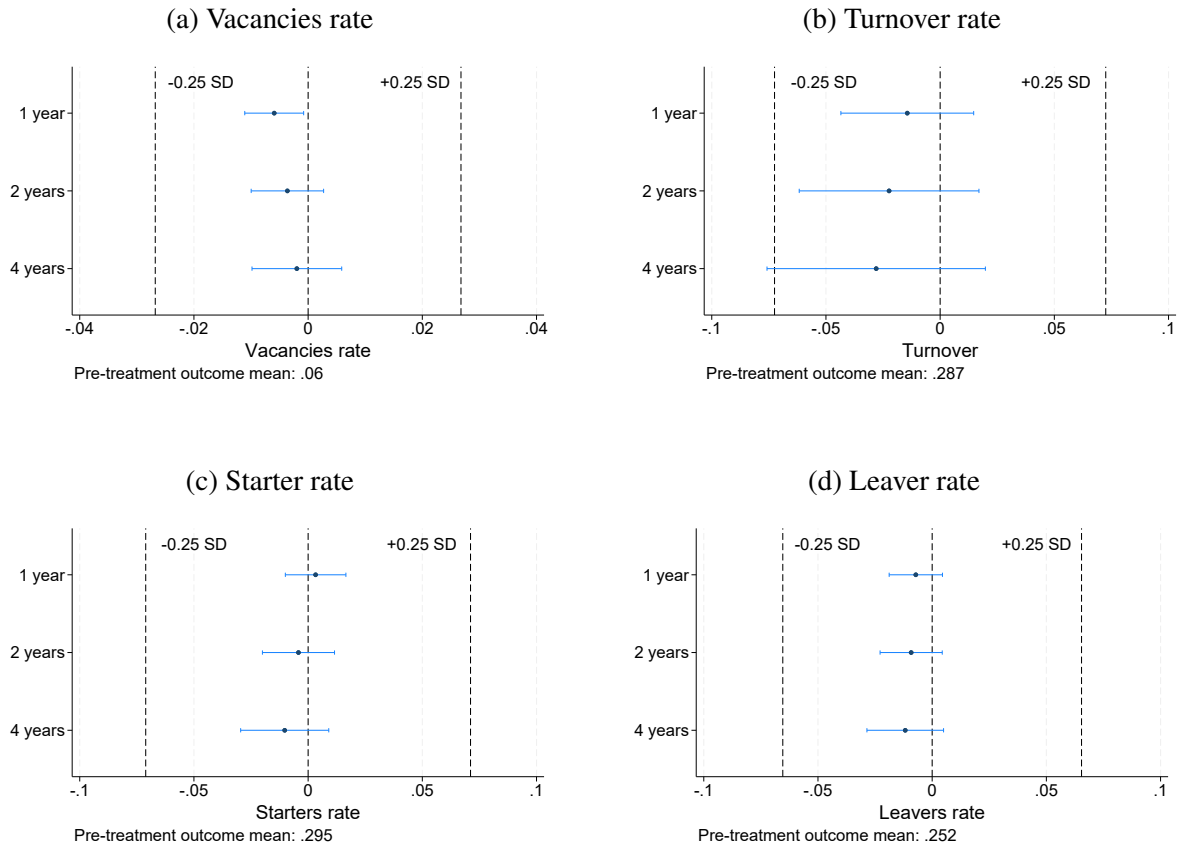
5.2 Vacancies and turnover

Although there is no change in total staffing, it could be that new supermarkets or Amazon warehouses are affecting the workforce in a way that would not be captured by the stock of staff. For example, it could be that it leads to an outflow of experienced staff that is offset by new hires. Although this would not change the total size of the workforce, it could still have negative impacts on care providers and their users. Or, in a more challenging local hiring environment (due to increased availability of supermarket jobs), care homes may post fewer vacancies: the effect could conceivably go in either direction. We now therefore examine four additional staffing outcomes: vacancy, turnover, starter and leaver rates.

Figure 4 shows the average treatment effects of a new supermarket opening for these four outcomes. Appendix Figure B2 shows the corresponding event study estimates and Appendix Table B1 gives the estimates of average treatment effects. Starting first with vacancies, Panel

(a) shows there is a decrease in vacancies within the first year after a new supermarket opens nearby (significant at the 5% level), and no significant change in the vacancies rate of care home providers after that. This means we can rule out any large increases in care home vacancies as a result of a supermarket opening.

Figure 4: Average treatment on treated estimates, supermarket opening



Note: (1) Error bars show 95% confidence intervals. (2) Results for turnover rate are estimated on a smaller sample than other outcomes.

Panel (b) shows the impact of a new supermarket on the turnover rate. These estimates are much less precise, in part because the turnover rate is estimated on a smaller sample than our other outcomes, as we require an observation twelve months before in order to calculate it. Nonetheless, the point estimates are negative and insignificantly different from zero. We can therefore rule out substantial increases in turnover rates as a result of a supermarket opening in the local area.

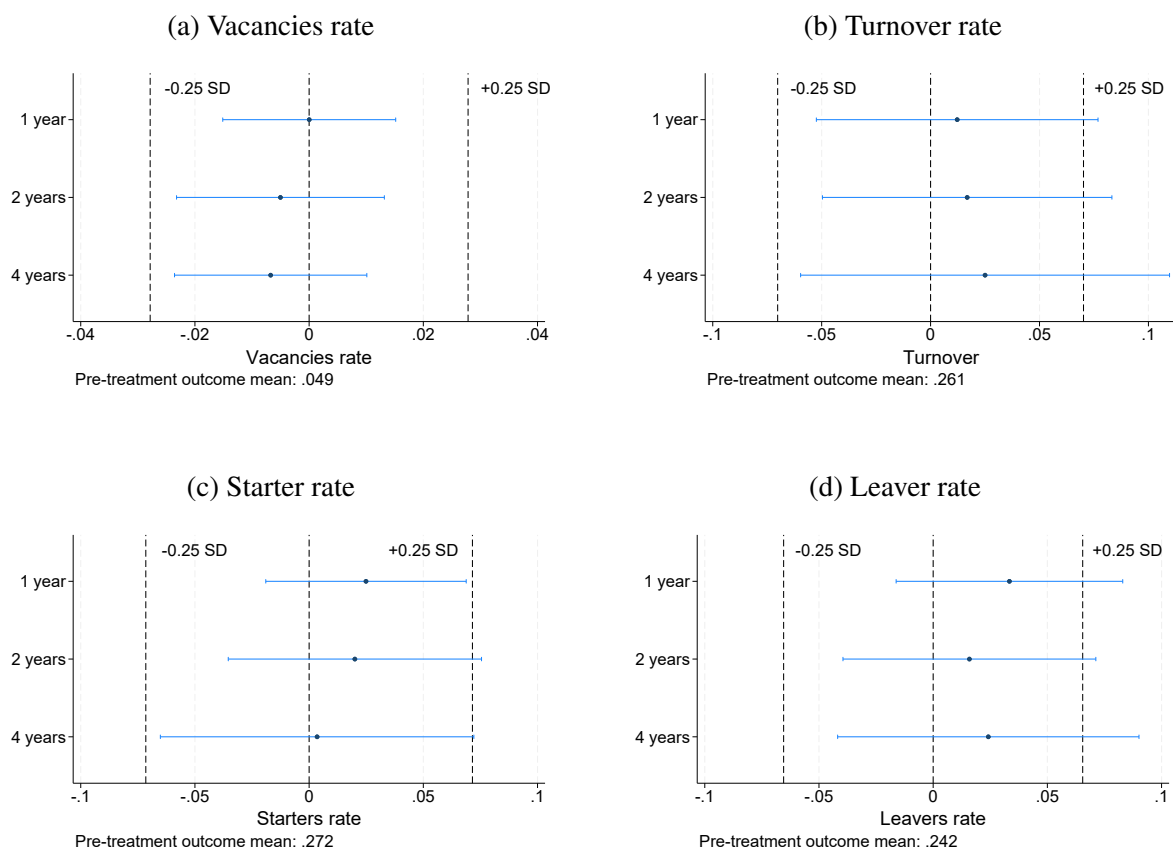
Panels (c) and (d) show the results for starter and leaver rates, respectively. Consistent with our estimates for turnover, the estimated impacts on the starter and leaver rate are mostly nega-

tive and all statistically insignificant. The magnitude of these changes is quite small, although growing modestly over time: the largest point estimate for the starter rate indicates a 1 percentage point fall over the four years after a supermarket opens. This is equivalent to a 3% decrease when compared to the pre-treatment mean starter rate (of 29.5 per cent).

Taken together we interpret these results as being consistent with the absence of an economically meaningful effect on flows of care home staff.

Figure 5 repeats this analysis for the effects of new Amazon warehouses. Appendix Figure B3 shows the corresponding event study estimates and Appendix Table B2 gives the estimates of average treatment effects. As with our total staffing results, these are less precise than our estimates for the effects of a new supermarket. But in all cases, there is no evidence of a statistically significant change in flows of care home staff. The point estimates for the turnover, leaver and starter rates are positive, and the point estimate for the vacancies rate is (slightly) negative.

Figure 5: Average treatment on treated estimates, Amazon opening



Note: (1) Error bars show 95% confidence intervals. (2) Results for turnover rate are estimated on a smaller sample than other outcomes.

5.3 Robustness

In this section we show the robustness of our results to a range of alternative specifications. In particular, we run eight sets of robustness tests for relevant outcomes. First we vary the difference-in-differences estimates we use. We repeat our analysis using OLS and the imputation estimator proposed by Borusyak et al. (2024). As well as calculating and weighting the treatment effects in different ways, both methods allow us to control for time-varying population and income growth, rather than just controlling for pre-treatment values as the Callaway and Sant’Anna (2021) estimator does.

Second, we vary the radius used to define treatment. We repeat our analysis defining affected care home providers using the 50th and 75th percentile of worker travel distances (rather than 66th), as well as a constant 5km radius.

Third, we vary which supermarket shocks we include. We repeat our analysis using only the openings of Aldi and Lidl supermarkets. These German supermarket chains were expanding rapidly across England during this period, and make up 53% of our total opening events. Because they were expanding nationally, their openings may be less correlated with local characteristics than the openings of well established supermarkets. We also repeat our analysis including medium supermarket openings (defined as those between 280 and 1,400 square metres) alongside large supermarket openings.

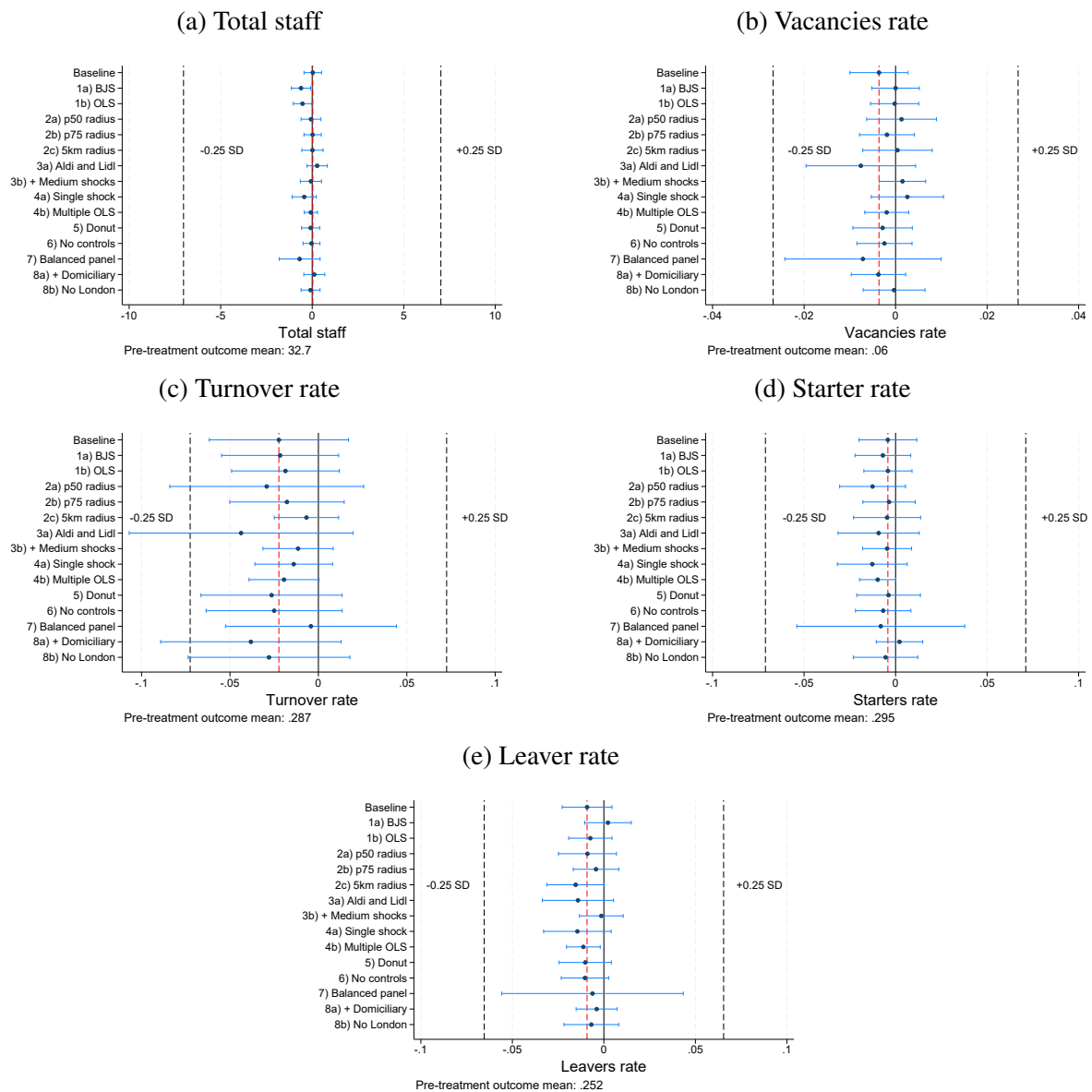
Fourth, we vary how we handle providers that are affected by multiple opening events. We first restrict our treated sample to those that were only affected by a single opening during our sample period. We then account for multiple treatments using OLS. In particular, we run an event study where the coefficients capture being a certain number of periods away from any treatment. For example, in a given period a provider could both be two periods after the opening of one nearby supermarket and three periods before the opening of another.

Fifth, we vary our control group. Because there may be spillovers from treated providers to other nearby providers, we repeat our analysis excluding from the never treated control group providers that were between 75th percentile distance and 90th percentile distance from openings. This is a ‘donut’ approach, in the sense that it leaves providers near and far from openings, but not those in the middle.

Sixth, we repeat our analysis with no population or income growth controls. This shows whether our controls are driving our null results. Seventh, we repeat our analysis using a balanced panel of providers. Finally, we vary our sample. We repeat our analysis including domiciliary care providers. We also repeat our analysis excluding London, to test whether our results are driven by differential patterns in the capital.

Figure 6 shows the robustness of our results for the opening of new supermarkets. For simplicity, we focus on how the two year average treatment effect varies in each case. In almost all cases, the point estimates in our robustness tests are very similar to our primary point estimates and lie within the 95% confidence interval of our primary estimates.

Figure 6: Two year average treatment on treated robustness, supermarket opening

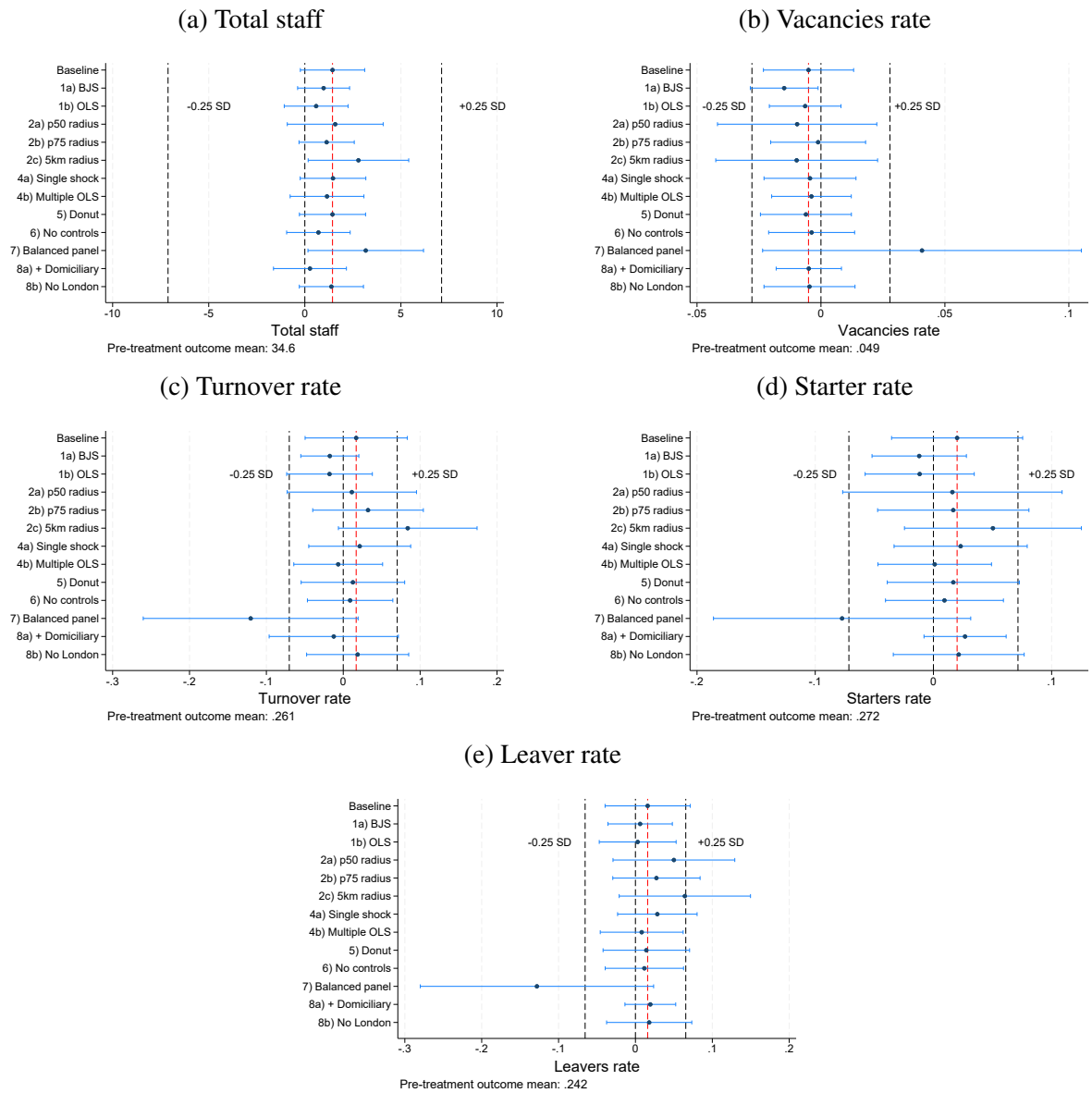


Note: Error bars show 95% confidence intervals. Results for turnover rate are estimated on a smaller sample than other outcomes. The dashed red line shows the baseline coefficient value. Each robustness test changes one aspect of our methodology, leaving all others unchanged. These are numbered according to the eight sets of tests described in the text. '1a) BJS' estimates the equivalent to our primary specification with the imputation estimator proposed by Borusyak et al. (2024). '1b) OLS' estimates the equivalent of our primary specification with OLS. '2a) p50 radius' uses the 50th percentile of worker travel distance to define whether a care home provider is affected by an opening, rather than 66th. '2b) p75 radius' uses the 75th percentile of worker travel distance. '2c) 5k radius' uses a 5km constant radius to define whether a care home provider is affected by an opening. '3a) Aldi and Lidl' only includes new Aldi and Lidl supermarkets. '3b) + Medium shocks' defines treatment using the openings of both medium and large supermarkets. '4a) single shocks' only includes providers that were affected by a single supermarket opening (or none) during our sample period. '4b) multiple OLS' performs a standard OLS event study but allows for multiple treatments, so each event study coefficient captures being a certain number of periods away from any treatment. '5) Donut' excludes from the control group any providers between their 66th and 90th percentile worker travel distance from any treatments (supermarket openings). '6) No controls' excludes our population and earnings controls. '7) Balanced panel' only includes providers that we observe for every period of our sample period. '8a) + Domiciliary' includes domiciliary care providers (those that provide care to people in their own homes) as well as residential care homes. '8b) No London' excludes any providers in London from our analysis.

For total staffing, our primary outcome of interest, all estimates remain very precise and close to zero. All are insignificantly different from zero, apart from our two alternative difference-in-differences estimators. Both OLS and the Borusyak et al. (2024) imputation estimator give reductions in total staffing that are significant at the 5% level. The point estimates imply that a supermarket opening led to a reduction of 0.5 care home staff (OLS) and 0.6 care staff (Borusyak et al., 2024), equivalent to a reduction of 1.6% and 1.9% of the pre-treatment mean staffing total, respectively. We discuss the magnitude and interpretation of these effects in the next section.

Figure 7 repeats this analysis for the opening of new Amazon warehouses. Although less precise, nearly all variants give point estimates similar to our primary point estimates, and lie within the 95% confidence interval of our primary results. The estimates using a balanced panel are presented for consistency with the supermarkets analysis but are particularly imprecisely estimated, as this limits the number of treated providers to just 324, too low for any meaningful degree of statistical precision.

Figure 7: Two year average treatment on treated robustness, Amazon opening



Note: (1) See notes to Figure 6. (2) We exclude robustness tests 3a and 3b because these are not relevant for Amazon warehouses.

5.4 Interpretation and mechanisms

Our baseline results suggest that supermarket and Amazon warehouse openings have no meaningful adverse effect on local care home staffing. If anything, the point estimates in our primary analysis suggests that an opening of either led to a small *increase* in the number of staff employed in nearby care homes. Even if we take the lower end of our 95% confidence interval, our analysis suggests that a supermarket opening led to 0.4 fewer staff per affected care home after two years (1.4% of pre-treatment mean staffing). Some of our specifications used for our robustness checks (OLS and BJS) imply a statistically significant negative effect of care home staffing. The central point estimate in BJS implies a reduction of 0.6 staff (1.9%), and the lower end of the 95% confidence interval implies a reduction of 1.1 staff (3.5%). These are modest effects in all cases, equivalent to only a small fraction of a standard deviation, and a small fraction of the sample pre-treatment mean of 32.7 staff members per care home. Combined with the lack of an effect on vacancies or flows of care home staff (as captured by turnover, leaver, and starter rates), this suggests that – contrary to the prevailing narrative – new supermarkets and Amazon warehouses do not have economically meaningful adverse impacts on social care staffing. There are several potential explanations for this finding.

First, it could be that these openings are insufficiently large shocks to have statistically detectable effects in our framework. In some sense, this is also a mechanism: supermarket openings just aren't big enough events to meaningfully alter local labour market conditions for social care staff. Here, it is useful to consider further the magnitude of our effects. Our analysis finds no statistically significant impact on the number of staff employed *per provider*. If we take the lower end of the 95% confidence interval in our primary analysis, a supermarket opening led to a reduction of 0.4 care home staff per care home after two years; aggregated across the (mean) 15 affected care homes, this suggests an upper estimate of a loss of 6.5 local care home staff in total. That compares to something in the region of 100-150 new jobs at the supermarket, and a mean 472 staff employed by those 15 care providers prior to the opening.²²

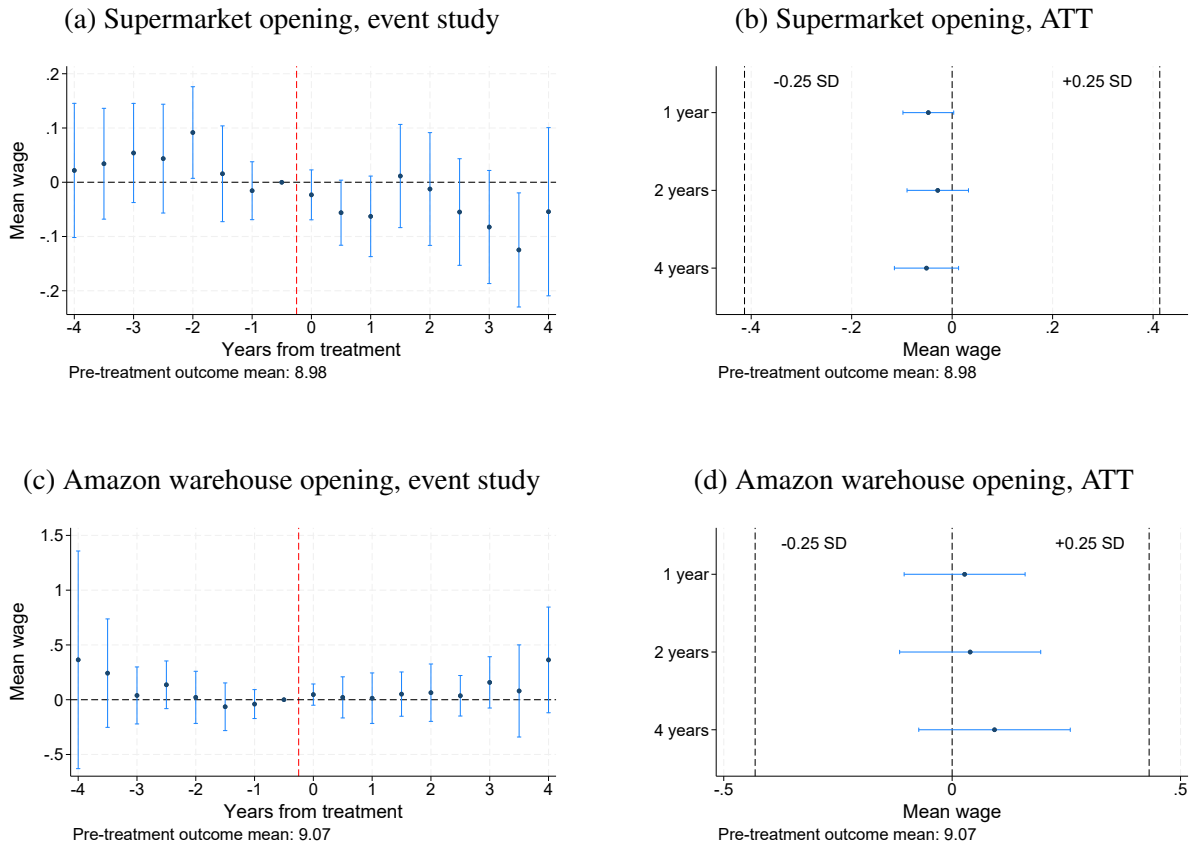
²²Note that this captures only the care homes included in our sample: the actual (mean) number of care homes affected may well be more than 15. This does not affect the interpretation of our results – our estimates suggest a mean reduction of 6.5 staff members from a total of 472 staff among care homes in our sample – but this does mean the 6.5 number should not be compared directly to the 100–150 number (as supermarkets may also have recruited from care homes that are not in our sample).

If we instead take the point estimate from our BJS specification (set out in the robustness tests above), this would suggest 0.6 fewer staff per care home and so 8.9 fewer staff across the 15 affected providers; the lower end of the 95% confidence interval implies 1.1 fewer staff per care home and 16.6 in total. This bounding exercise suggests that, at very most, a supermarket opening could reduce care home staffing by 3.5% (16.6 from 472), with our primary analysis suggesting a much smaller (and statistically insignificant) effect.

Second, contrary to recent discussion, it could be that supermarkets and Amazon warehouses are not relevant outside options for ASC staff, requiring different skills or appealing to a different section of the labour force. This is, however, difficult to formally test with the data available.

Third, it could be that care homes respond to the change in local labour demand (from new supermarkets or warehouses) by raising wages or changing other factors, such as training provision or the workforce composition. A change in wages could explain why total staff numbers remain unchanged, and could even explain the (very small) drop in the leaver rate. To test this, Figure 8 repeats our analysis with our sixth and final outcome variable, the mean wage paid by each provider. For both types of openings, there is no evidence that care home wages significantly increased afterwards. For supermarket openings, point estimates are mostly negative (and statistically significant at the 10% for one year after an opening). This is consistent with the financial constraints on care providers described in Section 2 and means that offsetting wage rises do not appear to be the explanation for why openings have no effect on the workforce. Moreover, we find no impacts when testing for changes in worker characteristics and training. Appendix Table B3 shows there is no significant change to worker age, nationality or tenure after a supermarket opens, and no change in measures of training. This suggests the more likely explanation is that these shocks are too small to be statistically detectable with the data available, or that they are not sufficiently relevant

Figure 8: Effects on mean wages



Note: Error bars show 95% confidence intervals.

6 Conclusion

The LTC sector is an increasingly important part of the economy and the broader health and care system in many countries. This is particularly true of Northern European countries where a greater share of LTC is provided formally, rather than informally (Barczyk and Kredler, 2019), and the sector employs a greater fraction of the overall workforce (OECD, 2023). Demand for workers in this sector is increasing, and there are widespread concerns among policymakers about how to attract and retain staff. In England and elsewhere, there are particular concerns that greater opportunities in the retail sector may reduce labour supply in the LTC sector. Retail may provide alternative employment to those who would otherwise work in the care sector, with higher wages and better working conditions potentially causing existing care workers to shift from their current jobs, and potentially attracting workers who might otherwise have joined the LTC sector. However, these concerns have not been tested empirically before.

In this paper, we study how the labour supply of care workers changed in response to specific changes in the availability of retail jobs in the local area. We focused on new openings of large supermarkets and Amazon warehouses, both of which are events that could plausibly increase the number of retail jobs available in the local labour market, and which are commonly cited by policymakers as events which cause issues for care homes (House of Commons Health and Social Care Committee, 2022; Migration Advisory Committee, 2022).

However, our results suggest that, in practice, these events had little impact on employment at care homes. In our baseline and preferred specification, we find no evidence of any statistically significant effects on total employment at care homes, and neither does there appear to have been any effect on the flows of care home staff, or a significant increase in vacancies. In some robustness checks, employing alternative specifications, the results suggest a negative impact on care home staffing, statistically significant at the 5% level. Even here, the implied effect is relatively small in size. To provide a sense of magnitude, the point estimate at the lower bound of the 95% confidence interval in our primary analysis suggests that the average local care home lost 0.4 staff two years after a supermarket opening; aggregated across the (mean) 15 affected care homes, this suggests that a new supermarket might – at most – have led to 6.5 fewer people working in local care homes in our sample (relative of a mean total of 472 staff prior to the opening). Bearing in mind that this is an upper bound, and that our central estimate is of no resulting reduction in care home staffing, we feel able to confidently rule out an economically meaningful adverse impact of supermarket and Amazon warehouse openings on care home staffing levels.

We also tested whether these events had any impact on wages paid by care homes, in order to examine whether the null effect on employment can be explained by care homes increasing their wages in order to retain staff. We again found no evidence in support of this hypothesis, suggesting that retail opportunities may simply have little impact on the work decisions of the majority of staff in the LTC sector.

Taken together, this suggests that contrary to the popular debate in England and elsewhere, the availability of retail and distribution jobs does not have a major impact on the labour supply of long-term care workers. One possible interpretation of our results is that they simply reflect

the relatively small size of the labour market shocks associated with supermarket or warehouse openings, or that these jobs are irrelevant to and unsuitable for the majority of care home staff. This could be viewed as a weakness of our analysis, but we instead view these as potential mechanisms, and reasons for the policy debate to shift in focus. Government interventions and pay decisions ought not to be motivated by developments in the supermarket sector. Further, in collective pay negotiations between employers and employees (which recent legislation seeks to encourage and facilitate), our analysis suggests that job opportunities within the supermarket sector ought not to be used as a central benchmark for that of care home staff. The drivers of recruitment and retention issues would appear to lie elsewhere.

Of course, our study focuses only on two measures of increased job opportunities in the retail sector, in one country, and may not fully capture the relationship between these industries. For example, it may be that it is the wage level – rather than an increase in the number of jobs available – that matters for attracting staff away from the LTC sector. Broader economic conditions could be more important (Antwi and Bowblis, 2018; Konetzka et al., 2018) than retail jobs specifically. It may also be that it is the availability of roles closer in nature to care roles (such as nursing auxiliary roles in hospitals) that is most important. Further study is required to better understand the determinants of recruitment and retention in the LTC sector, how policies aimed at improving these things ought to be designed, and what impacts they could have on patients.

Our study is also limited by data imperfections. We rely on a dataset that only includes a subset of care home providers from the private sector (albeit a large subset). While this is the best possible data available in this setting at the time of writing, and although we think the research topic to be of sufficient academic and policy interest to be worth studying regardless, this may not provide an entirely representative picture of the entire care home market. Improved and more extensive data collection in the LTC sector would help regulators and researchers alike to better understand the LTC industry, and should be made an important future priority.

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A Additional data details

A.1 Provider data cleaning

We apply the filters developed by Skills for Care and only include observations that have valid values for total staff, vacancies, starters and leavers. This involves removing observations where values are 999, and where levels or rates are implausibly large or small compared with total staff. We also only include an observation if it was updated within the last six months.

Our starting sample of care homes is 184,133 observations (21,203 unique providers). After restricting to those that were updated in the last six months, our sample is 135,459 observations (20,005 unique providers). After eliminating those with invalid data, our sample is 84,807 observations (13,562 unique providers). This sample differs from the sample size of our results and Table 1 because the Callaway and Sant’Anna (2021) estimator does not use all observations.

A.2 Supermarket opening cleaning

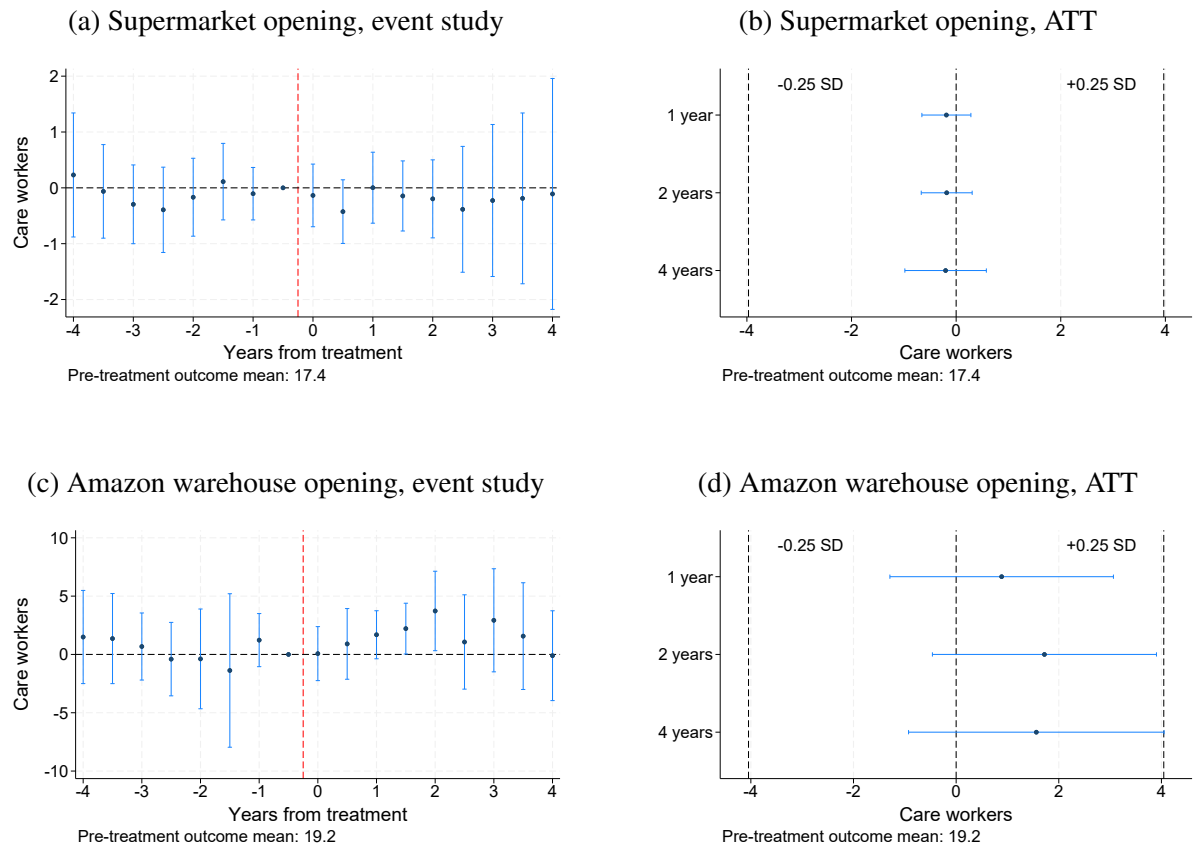
We only include the openings of the following supermarkets: Aldi, Asda, Booths, Budgens, Lidl, Marks and Spencer, Morrisons, Sainsburys, Tesco, Waitrose, Whole Foods, Coop, Costco, Iceland, Makro. We only include openings where the brand is the only brand to have a supermarket in the location, or if there is an existing supermarket but it does not close or change when the new supermarket opens. We tested a random sample of our openings using Google Street View and confirmed these captured genuine supermarket openings at the dates our data suggested.

A.3 Amazon warehouse openings

We recorded Amazon warehouse openings using both Amazon’s public announcements and records, as well as local news stories. We record openings of warehouses in the following locations (with Amazon warehouse codes): Dunstable (LTN4), Manchester (MAN1), Coalville (BHX2), Daventry (BHX3), Doncaster (DDN1), Warrington (MAN2), Tilbury Docks (LCY2), Bolton (MAN3), Bristol (BRS1), Rugby (BHX5), Exeter (DEX2) and Chesterfield (MAN4).

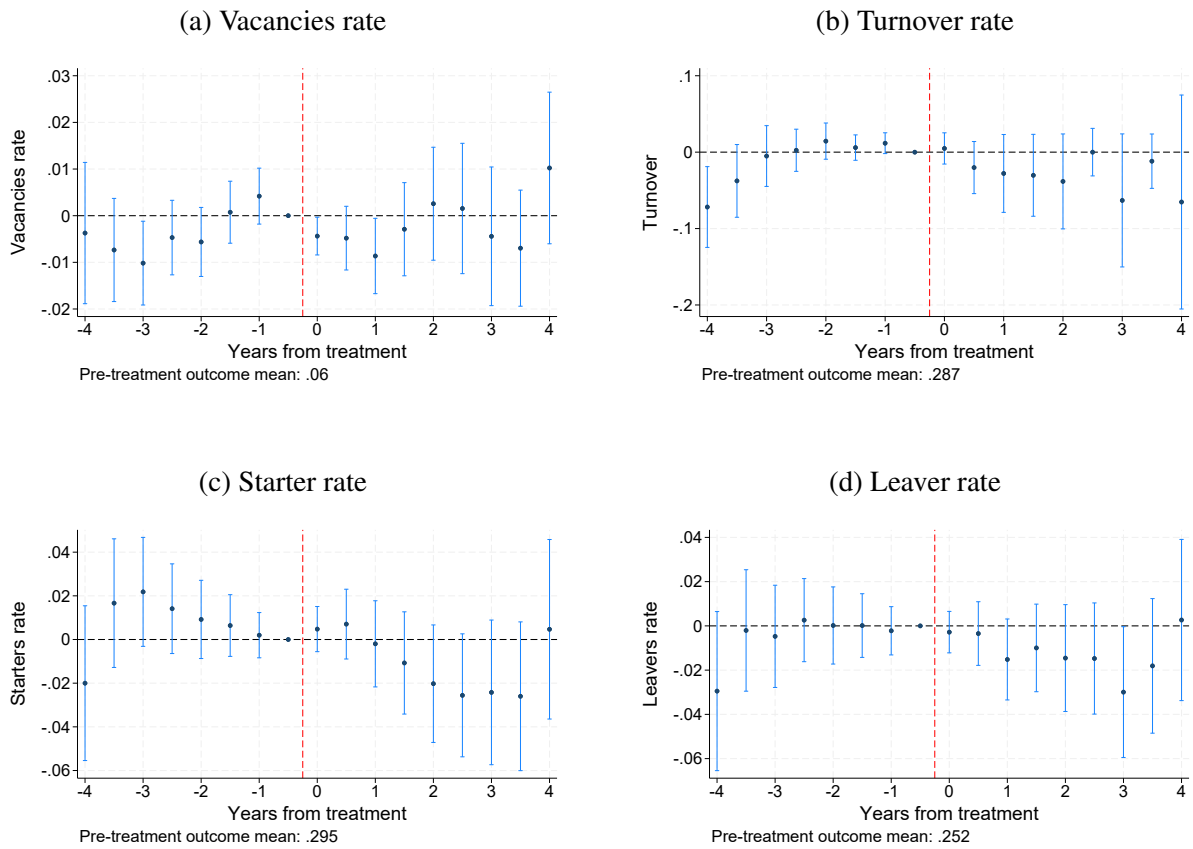
B Additional results

Figure B1: Effects on number of care workers



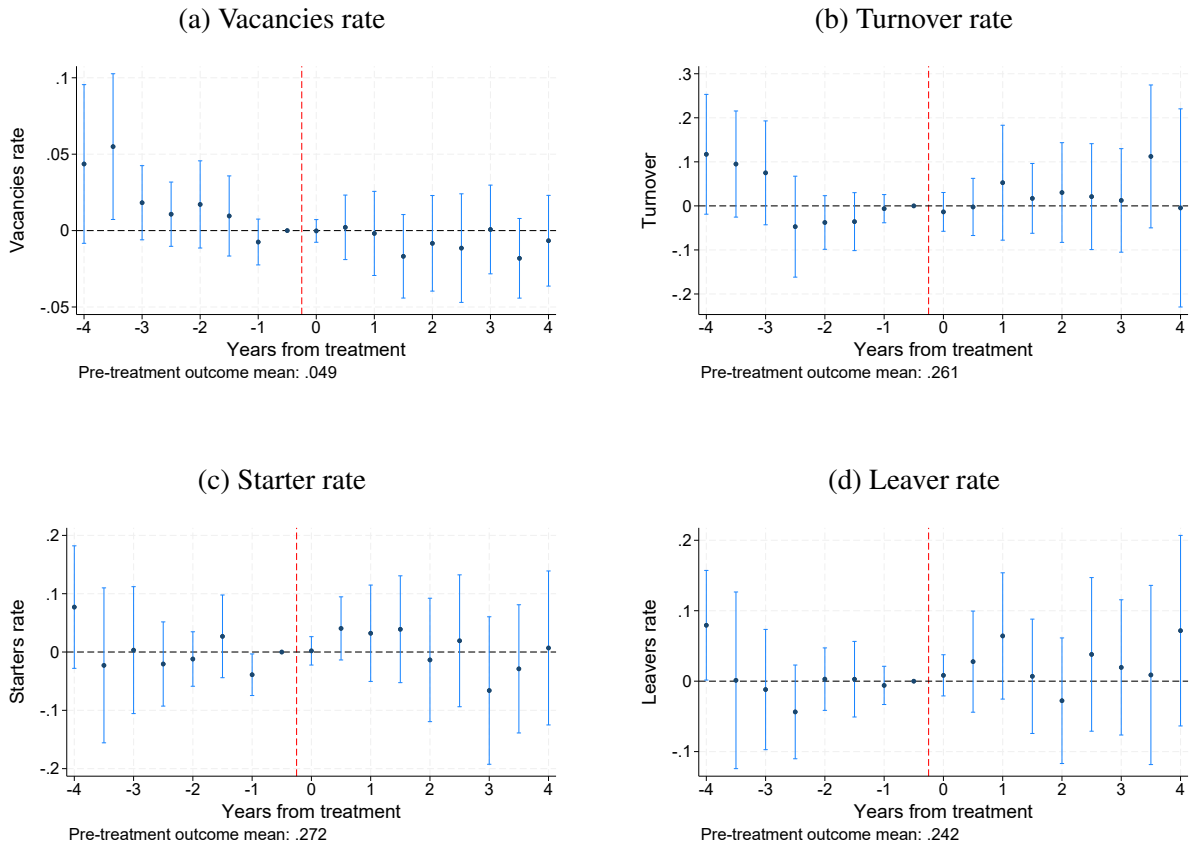
Note: Error bars show 95% confidence intervals.

Figure B2: Event study results, supermarket opening



Note: (1) Error bars show 95% confidence intervals. (2) Results for turnover rate are estimated on a smaller sample than other outcomes.

Figure B3: Event study results, Amazon opening



Note: (1) Error bars show 95% confidence intervals. (2) Results for turnover rate are estimated on a smaller sample than other outcomes.

Table B1: ATTs for supermarket openings

Variable	1 year	2 years	4 years
Total staff	0.0825 (0.209)	0.0337 (0.245)	0.0303 (0.371)
Vacancies rate	-0.00594** (0.00263)	-0.00363 (0.00324)	-0.00197 (0.00401)
Turnover rate	-0.0144 (0.0148)	-0.0224 (0.0201)	-0.028 (0.0244)
Starter rate	0.00328 (0.00676)	-0.00422 (0.00805)	-0.0102 (0.00984)
Leaver rate	-0.00716 (0.00595)	-0.00919 (0.00694)	-0.0118 (0.00856)
Mean wage	-0.0474* (0.0258)	-0.0286 (0.0312)	-0.0511 (0.0326)

Note: (1) Standard errors clustered at the LSOA level in parentheses. (2) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B2: ATTs for Amazon openings

Variable	1 year	2 years	4 years
Total staff	0.648 (0.578)	1.44* (0.854)	1.58 (1.51)
Vacancies rate	0.0000184 (0.00773)	-0.00502 (0.00928)	-0.00673 (0.00859)
Turnover rate	0.0122 (0.033)	0.0168 (0.0339)	0.025 (0.0432)
Starter rate	0.0249 (0.0224)	0.02 (0.0283)	0.0035 (0.035)
Leaver rate	0.0334 (0.0253)	0.0159 (0.0282)	0.0242 (0.0337)
Mean wage	0.0273 (0.0676)	0.0395 (0.0788)	0.0928 (0.0846)

Note: (1) Standard errors clustered at the LSOA level in parentheses. (2) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B3: ATTs for worker characteristics for supermarket openings

Variable	1 year	2 years	4 years
Age	0.00154 (0.075)	0.0562 (0.0937)	0.101 (0.116)
British	-0.00219 (0.00451)	-0.00367 (0.00603)	0.000247 (0.00812)
Years in ASC	-0.171** (0.075)	-0.145 (0.0919)	-0.176 (0.109)
Years in role	-0.0297 (0.041)	0.0047 (0.0514)	-0.0107 (0.0639)
Any training	0.00948 (0.00776)	0.0122 (0.00914)	0.0164 (0.0107)
Num training	0.0141 (0.18)	-0.0749 (0.219)	-0.104 (0.321)

Note: (1) Standard errors clustered at the LSOA level in parentheses. (2) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (3) Any training refers to whether any training instance is recorded in the data. (4) Num training refers to the count of training instances recorded.