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Devolving Skills: The Case of the Apprenticeship Grant for Employers*

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Abstract

One rationale for devolution is that local decision makers may be well placed to adapt national policies to the local context. We test whether such adaptation helps meet programme objectives in the case of the Apprenticeship Grant for Employers. Originally a national programme, aimed at incentivising employers to take on apprentices, reforms a few years into operation gave some Local Authorities negotiated flexibilities in how the scheme operated. We consider the impact of the national scheme and then use a difference-in-differences approach to test whether flexibility led to an increase in the number of apprenticeship starts in devolved areas relative to control groups. We find that flexibility had zero effect. There is suggestive evidence that this is because flexibilities were negotiated on the wrong margins.

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I. Introduction

In recent years, there has been an incremental devolution of responsibilities and powers from UK central government to local government. One rationale for this devolution is that local areas may be better able to judge how to adapt national policies to fit the local context. This devolution, at least so far, has not involved a radical transfer of power. For example, the 'city deals' agreed between 2011 and 2014 did not transfer general powers to Local Authorities (LAs). Instead, they provided some cities, working with their Local Enterprise Partnership (LEP), with a small amount of additional funding or powers to be used flexibly.[1] A few years into operation, the Apprenticeship Grant for Employers (AGE), the focus of this paper, was 'devolved' to 40 LAs as part of the city deals process. Originally a national programme, introduced in 2012, AGE aimed to incentivise employers to take on apprentices. The incentive aimed to encourage small and medium-sized enterprises (SMEs) to hire young apprentices (aged 16–24) and comprised a £1,500 grant per apprentice over and above ongoing subsidies to the cost of training paid independently of the AGE scheme. When the national scheme was reformed a few years after introduction, city deals areas negotiated additional flexibilities implemented in either 2015 or 2016.

Local decision makers may be better placed to introduce flexibilities that fit their context, providing they have good information and are able to balance competing local interests. However, if such conditions are not met, negotiating flexibilities might prove costly, in terms of time and resources, without producing the hoped-for benefits. This paper demonstrates that, in the case of AGE, well-intentioned efforts to negotiate local flexibilities do not appear to have led to better outcomes. Both our own analysis, and results reported in the evaluation of the national scheme,[2] lead us to believe that this may partly be explained by the fact that flexibilities were negotiated on the wrong margins. The main flexibility ensured eligibility for larger firms whereas the national scheme was predominantly used by very small firms. Compounding this, existing evidence suggests that subsidies are not necessarily very effective for increasing the take-up of apprentices. For example, Merrilees (1984) examined a scheme with some similarities in Australia. This affected apprenticeship starts for some trades but not for others, possibly because reducing the cost of apprentices only affected demand where assistant tradespeople were widely used (i.e. apprentices cannot substitute for the work of full tradespeople). Reviews by the What Works Centre for Local Economic Growth (2015, 2017)

[1] In May 2015, legislation to 'provide for the devolution of powers to cities with elected metro mayors, helping to build a Northern Powerhouse' was announced. The government invited areas to submit devolution proposals to be considered during the 2015 Spending Review process. For further details, see https://publications.parliament.uk/pa/cm201516/cmselect/cmcomloc/369/36904.htm#_idTextAnchor005.

[2] BIS, 2013.

and OECD (Kuczera, 2017) find that financial incentives for apprenticeships have modest effects and may involve substantial deadweight. Both recommend careful monitoring and evaluation of the effects of financial incentives as evaluation evidence is relatively thin.

To provide a benchmark for the impact of AGE flexibilities, we assess the effect of the national scheme using administrative data on apprenticeship starts at the LA level. To do this, we use 'difference-in-differences' to examine whether its introduction saw starts increase more in eligible firms than in non-eligible firms. To study the impact of AGE flexibilities, we then evaluate the effect on apprenticeship starts of devolving AGE to 40 LAs (in 2015 or 2016) relative to a control group, again using a difference-in-differences methodology. Our results are robust to using a matched sample of treatment and control areas and to using synthetic controls.

In Section II, we explain AGE in more detail – how the scheme works and how it has evolved. In Section III, we describe the data used for the analysis and explain the methodology. Results for the national scheme and for AGE flexibilities are presented in Section IV. We conclude in Section V.

II. The Apprenticeship Grant for Employers

The AGE was a lump-sum incentive scheme introduced nationally in February 2012. It had two main objectives: to encourage the hiring of young apprentices and to increase the number of apprenticeships in small businesses.[3] The scheme paid a lump-sum incentive of £1,500 per apprentice aged 16–24 to SMEs with 250 or fewer employees, as long as they were 'new to apprenticeships'.[4] The grant was set to £1,500 so that it would be large enough to cover the early costs of the apprenticeship, but not 'too generous' so as to generate the wrong incentives for firms.[5,6] The incentive payment was over and above the subsidies to support the direct cost of off-the-firm training and assessment for apprentices. This subsidy applied to all firms hiring apprentices and was, at the time, 100 per cent for apprentices aged 16–18 and 50 per cent for those aged 19–24. The restriction to firms 'new to apprenticeships' was intended to expand the number of firms offering apprenticeships and to reduce deadweight by avoiding the subsidy of apprenticeships that would have

[3] As discussed by Kuczera (2017), the idea of such lump-sum incentives is to increase the number of apprentices by creating financial incentives for companies that have not provided apprenticeships previously. While it might also be used to improve quality (e.g. by building training capacity within the firm), our data only allow us to focus on the effect on apprenticeship numbers, the main objective of the scheme.

[4] 'New to apprenticeships' was originally defined as never having had an apprentice or having not taken on an apprentice in the last three years. At the end of August 2012, this was changed to 'not in the last year'.

[5] BIS, 2013.

[6] 'The broad consensus was that £1,500 was 'about right': sufficient to cover an apprenticeship's early costs to the employer, which was seen as particularly important for very small businesses, but not such as to constitute the employer's only reason for taking on the young person (BIS, 2013, p.31).

happened anyway. By the end of the first year of the scheme, eligibility was extended to employers with up to 1,000 employees and the maximum number of apprentices who could be taken on was increased from three to ten. The scheme was closed on 31 August 2017.

At the end of 2012, the government commissioned an evaluation of the scheme.[7] The first part of the evaluation consisted of phone and face-to-face interviews with employers, government officials, training providers and other strategic partners (such as the Skill Funding Agency, Chambers of Commerce, Local Authorities, etc.). This survey of recipients found that most employers making use of AGE were small: 80 per cent employed 25 staff or fewer. It also found that most recipients took on only one apprentice. The second part of the evaluation consisted of a cost–benefit analysis, for apprentices, employers and the local economy. The latter exercise is the most closely related to our analysis. It is based on the number of AGE-funded starts, deflated by an assumed deadweight of 22 per cent. The assumption on deadweight was based on the percentage of AGE employers of apprentices aged 16–24 who, when surveyed, said that AGE made no difference to them taking on an apprentice at the point at which they did so.[8,9] We view these methods for 'estimating' deadweight to be highly problematic and we take a more robust approach to estimating the net impact of AGE, as described below. The cost–benefit analysis does provide useful information on costs. As discussed above, off-the-job costs were covered by a subsidy paid independently of AGE. Apprenticeship wages accounted for a large fraction of on-the-job training costs: estimated to be between £11,800 and £25,114 depending on the level, duration and age of the apprentice.[10] Supervisor costs represented another important cost of on-the-job training and were estimated to be between £7,600 and £21,000, again depending on the level and duration. Administrative costs were less important, ranging between £300 and £900 per apprenticeship. This implies that the AGE payment would cover between 3 and 7 per cent of the total costs incurred by firms.[11]

Figure 1 shows the evolution of apprenticeship starts by firm size in our administrative data (discussed below). Consistent with the survey data,

[7]BIS, 2013.

[8]BIS, 2013.

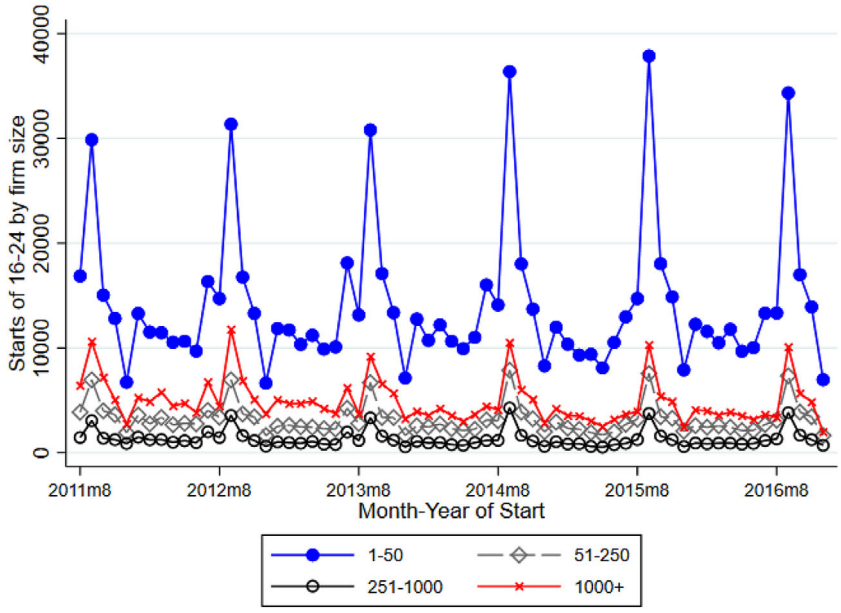
[9]This figure comes from a survey of 500 employers out of a total of 24,000 apprentices. Taken at face value (i.e. assuming that firms can correctly assess the counterfactual), there are still good reasons to think this may underestimate deadweight given that AGE employers had to sign a declaration saying that they could not hire an apprentice without the grant in order to receive payment.

[10]Apprentice age has direct and indirect effects on costs, which work in opposite directions. After the first year of apprenticeship, older apprentices are paid more, increasing the wage cost. Offsetting this, apprenticeships for the young are usually longer, increasing the total wage bill and the cost of supervision.

[11]BIS (2013) estimates an additional benefit to the firm by assuming that when the apprentice is working rather than learning they are as productive as a worker with a qualification below Level 2, but they are paid less. Allowing for this as well means that AGE would cover 12–22 per cent of the net costs.

FIGURE 1

Number of apprenticeship starts for apprentices aged 16–24 by firm size



Source: Authors' own calculations from Individualised Learner Record data.

small firms are more likely to take on apprentices both before and after the introduction of AGE (in 2012, month 2). Based on the figure alone, it is hard to see whether the number of starts increased more for eligible or ineligible firms, but our regression results reported below suggest some differences pre- and post-policy.

In the 2012–13 financial year, firms claimed around 30,000 AGE 16–24 apprenticeship payments (i.e. payments for apprentices aged 16–24).[12] This was substantially below the target of 40,000. However, this did not prevent changes in January 2015 reducing the generosity of the national scheme by restricting it to small firms with up to 50 employees that had hired no apprentices in the previous year.[13] As a result of these changes, several local areas negotiated flexibilities as part of ongoing devolution deals with central government. The 'city deals' involved a small amount of additional funding provided by central government to be used flexibly for specific programmes, the outcomes of which AGE was a relatively small part (although the only part

[12] BIS, 2013.

[13] Firms could now only receive up to five grants rather than up to ten, although in practice very few firms hired more than one apprentice under the scheme.

directly concerned with apprenticeships). For example, some of the deals also provided selective, additional borrowing capacity, retention of some, or all, of any growth in income from business rates and greater influence over a number of programmes formerly delivered by government departments, their agencies and contractors.[14]

AGE flexibilities were agreed with groupings of LAs that formed, or were planning to form, Combined Authorities (CAs). About 20 LAs gained AGE flexibilities in 2015 (all in Greater Manchester CA, West Yorkshire CA and Sheffield City Region) and another 20 gained flexibilities in 2016 (in the West of England, Liverpool City Region, Cambridgeshire and Peterborough and Tees Valley).[15] A full list is provided in online Appendix A.

Table 1 provides an overview of negotiated AGE flexibilities. As is clear from the table, there are aspects of AGE flexibility that are specific to different groups of LAs. Most of them had flexibility in the types of firms that were eligible to claim the subsidy. For example, all but one maintained eligibility for firms with up to 250 employees (at least for a certain number of apprentices; and conditional on not having employed apprentices in the previous year).[16] In contrast, the national scheme restricted eligibility in all other LAs to small firms (with up to 50 employees) from January 2015 onwards. While LAs differ on precise details, most of them diverged from the national scheme in a way to make the local scheme more generous overall by expanding eligibility and increasing the incentive payment in many cases. Given this, and the detailed differences in area-level flexibilities, we focus on evaluating the overall effect of AGE flexibilities.

In the remaining sections, we first consider the impact of the national scheme before investigating whether flexibilities translated into more apprenticeship starts in devolved areas than might have been expected if they had implemented the revised national scheme.

III. Data and methodology

We use Individualised Learner Record (ILR) data – administrative data on all publicly funded apprenticeships in England between 2011 and 2017. This data set provides information on several characteristics of both the apprentice and the apprenticeship but not a detailed measure of employer size. We use the ILR matched to the Employer Data Service (EDS) to give us a better estimate

[14] The fact that AGE flexibilities were part of a broader package limits concerns over the self-selection of LAs into treatment based on pre-existing trends in apprenticeship numbers. It does raise the possibility that other devolution policies might offset any positive effect of AGE flexibilities. We find it hard to think what these might be and, as we report below, pre-policy trends in apprenticeship numbers suggest this is not a major concern.

[15] Suffolk and Norfolk had flexibilities for four months only in 2016. They are excluded from the analysis.

[16] In Sheffield, this was up to 100 employees.

TABLE 1

AGE policy over time and across areas

Time	*AGE*	*Area*	*Eligible firm (number of employees)*	*Number of apprentices*	*Grant amount (in GBP)*
01/02/2012–31/07/2012	AGE 16–24	England	Up to 250	Up to 3	£1,500
01/08/2012–31/12/2014	AGE 16–24	England	Up to 1000	Up to 10	£1,500
01/01/2015–31/07/2017	AGE 16–24	England	Up to 50	Up to 5	£1,500
01/04/2015–31/07/2017	AGE 2015	Greater Manchester CA	Up to 250	Up to 3	£1,500. Additional £1,000 for higher apprenticeship or for providers supporting Trailblazers standards.
01/04/2015–31/07/2017	AGE 2015	Sheffield City Region (GAP)	Up to 100	Up to 4	Different amounts depending on sector. Larger grant for strategic sectors for the region.
01/08/2015–31/07/2017	AGE 2015	West Yorkshire CA	Up to 250	Up to 3	£1,200. Additional £800 for apprenticeships in specific sectors.
01/08/2016–31/07/2017	AGE 2016	The West of England	Up to 250	Up to 5	£1,500. Additional £1,000 for apprenticeships in specific frameworks, for higher level apprenticeships or apprentices from an ethnic minority.
01/08/2016–31/03/2017	AGE 2016	Liverpool City Region	Up to 250	Up to 5	£3,000 for those aged 16–18; £2,500 for those aged 19–24. Additional £1,000 to SMEs for advanced or higher apprentices
01/08/2016–31/03/2017	AGE 2016	Tees Valley (TVAGE)	Up to 250	Up to 3	£1,500. Additional £1,000 for apprenticeships in specific frameworks.
01/08/2016–31/07/2017	AGE 2016	Cambridgeshire and Peterborough CA	Up to 250	Up to 5	£2,000 for those aged 16–18; £1,500 for those aged 19–24.
01/08/2016–31/12/2016	AGE 2016	Suffolk and Norfolk	Up to 250	Up to 5	£2,000 for those aged 16–18; £1,500 for those aged 19–24.

Note: Based on an AGE Devolution Structures document provided by Greater Manchester CA and on information from the website of each CA or city deal.

of firm size in 2017. Given the relatively short time period, and the broad banding for AGE eligibility, mismeasurement of firm size is unlikely to be a major concern.[17] Given our focus on AGE flexibilities at the LA level, we define LAs as our unit of analysis and aggregate data by LA, month and firm size.

We first consider the impact of the national scheme. As this was introduced everywhere at the same time, it is more difficult to estimate a causal effect than when considering the impact of flexibilities. We exploit the fact that eligibility for the national scheme varied by firm size, where firms with over 1,000 employees were never eligible. We estimate the following equation:

$$(1) \qquad \ln(y_{aft}) = \gamma \left(Treat_f \times Post_t\right) + \partial Treat_f + d_t + \mu_a + \epsilon_{aft}.$$

Here, $\ln(y_{aft})$ is the (log + 1) total number of relevant apprenticeship starts in a given area (a) and given time (month and year) in firms of a given size (f).[18] $Treat_f = 1$ for firms with fewer than 1,000 employees. $Post_t = 1$ from the date the national policy was introduced. $Treat_f \times Post_t = 1$ for treated firms after the national policy was introduced, and therefore γ is the coefficient of interest that captures the effect of treatment. d_t are dummies for each month–year combination. We control for area fixed effects (μ_a), which removes the influence of time-invariant factors that might affect the number of apprentices. ϵ_{at} is the error term.[19] We estimate this regression separately comparing number of starts by LA in firms of different sizes (treated firms with 1–50, 1–250 or 251–1,000 employees) to the number of starts by LA for firms with more than 1,000 employees (the control group). In online Appendix B, we also report results from an event study that allows us to consider whether treatment groups look different from the control group in the six months prior to the introduction of the national policy. We estimate these regressions where the dependent variable is the number of apprenticeship starts for those aged 16–24 and the number of apprenticeship starts for those aged over 24. While we might expect the latter not to be affected by the national policy (as there was no explicit incentive to hire older workers), we cannot rule out the possibility of spillover effects due to complementarities or the possibility that the subsidy attached to younger workers relaxed financial constraints on firms of hiring older workers.

[17]For a shorter time period (2011–15), we also have firm data from the Inter-Departmental Business Register (IDBR). Given the available time period, we cannot use these data to evaluate the effects of AGE flexibilities. Estimating the effect of the national policy using both data sources (EDS and IDBR) produces qualitatively similar results.

[18]Given differences in the total number of apprentices across areas, the estimates in logs are easier to interpret (although results when estimating in levels are not very different).

[19]The inclusion of time-varying characteristics of LAs (e.g. such as those included in Table 2) makes no difference to the coefficients of interest.

We can evaluate the effects of flexibilities more rigorously because devolution did not happen everywhere. As with the national policy, we estimate the effect of flexibilities on the number of area-level apprenticeship starts. There are two 'treatment groups' comprised of LAs granted flexibilities in either 2015 (AGE 2015) or 2016 (AGE 2016). The control group comprises the other LAs that were never granted flexibilities (of which there are 270). In a refinement of this approach, we estimate regressions on a subset of LAs that have 'common support'. This is established by estimating the propensity score for being a treatment area on the basis of observable characteristics in the year prior to the start of AGE flexibilities and then trimming the sample such that only treatment and control LAs within the same range are used for the analysis. The procedure is described in online Appendix C. Trimming substantially reduces the number of control areas while only reducing the treatment areas by one or two LAs.

Whether applied to the full sample or the sample of LAs sharing common support, the methodology involves estimating whether apprenticeship starts (aged 16–24) increased in devolved areas relative to a control group, in comparison with previous time periods.[20] This 'difference-in-differences' analysis can be specified as

$$\ln(y_{at}) = \beta\,(Treat_a \times Post_{at}) + d_t + \mu_a + \epsilon_{at}, \quad (2)$$

where $\ln(y_{at})$ is the (log + 1) total number of relevant apprenticeship starts in a given area (a) and given time (defined by month and year).[21] $Treat_a = 1$ for all devolved areas. $Post_{at} = 1$ for the period after the devolution. $Treat_a \times Post_{at} = 1$ for the treated areas post-devolution. Therefore, β is the coefficient of interest that captures the effect of treatment. d_t are dummies for each month–year combination. We control for area fixed effects (μ_a), which removes the influence of time-invariant factors that might affect the number of apprentices. ϵ_{at} is the error term.[22] We estimate this regression separately for all firms and firms of 51–250 employees (i.e. those not eligible for the national scheme at the time of devolution) as well as separately for AGE 2015 and AGE 2016 areas. We also estimate a regression for firms with up to 50 employees, even though they continued to be eligible for the national scheme. This enables us to further evaluate the impact of devolution for firms that continue to be eligible for the nationally determined payment but where devolved LAs may have made the system more generous for those firms within

[20]We exclude those aged 25+ because of the possibility of substitution between younger and older apprentices on account of the incentive scheme.

[21]Given differences in total number of apprentices across areas, the estimates in logs are easier to interpret (although results when estimating in levels are not very different).

[22]Including time-varying characteristics of LAs (e.g. such as those included in Table 2) makes no difference to the coefficients of interest.

a narrower range of parameters (such as the amount of subsidy). We have also estimated an 'event study' where being in a treatment area is interacted with every time period (defined by month and year). This enables us to check for differential trends in treatment and control areas prior to the flexibilities being introduced.

Standard errors are clustered at the LA level. All the regressions are weighted by the annual population of the LA. This is to account for the differing size of each LA (although the unweighted results are not very different).

In addition to estimating on common support, we further check the robustness of our results by using the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010, 2015). The synthetic control group is created as a weighted average of several untreated units. The weights are defined by 'matching' pre-treatment covariates and outcomes such that the synthetic control is as similar as possible to the treated area before the start of the treatment.[23] The method is further explained in online Appendix D.

This uses the idea that in some cases a weighted combination of units may be a better comparison group than any unit on its own. It may be particularly useful for AGE 2015 areas, which are quite different from the other LAs.[24] Table 2 shows that, on average, they are more populous, more likely to be rural and have many more small firms than areas in either the AGE 2016 or never devolved groups. The treatment and control groups already look much more similar when the sample is trimmed based on common support (shown in online Appendix C), as described above. The use of synthetic controls provides an alternative way of achieving the same outcome. In practice, as discussed below, we do not see much difference between results using these two robustness checks.

IV. Results

We start by considering the impact of the national scheme. Results from the event study, reported in online Appendix B, suggest that treated units look similar to the control group in the six months prior to the introduction of

[23] We use as covariates the number of firms in each area by size, the total population, the percentage of the population living in a rural area, the percentage female and the percentage ethnic minority, the percentage of inhabitants aged 16–64, the percentage of those aged 16–64 with higher education (above level 4) and of those with no qualifications, the percentage of employees aged 16–64 receiving work-related training, the percentage of those aged 16–64 who are employed, economically active, and unemployed, and, finally, the percentage of self-employed (for workers aged 16+). The matched outcome variable is the log number of apprenticeships starts.

[24] When estimating the synthetic control method for AGE 2015 areas, we need to estimate the effect for West Yorkshire separately as this had a slightly different start time from other areas.

TABLE 2

LA summary statistics by AGE treatment group

Yearly averages	*Never devolved*	*AGE 2015*	*Difference AGE 2015 and Never devolved*	*AGE 2016*	*Difference AGE 2016 and Never devolved*
Total population, of which	155,635	328,039	172,404***	195,335	39,700
% Female	51.3	51.1	−0.2*	51	−0.0
% Ethnic minority	10.3	11.2	0.9	5	−5.3***
% 16–64 population	62.8	63.9	1.1**	64	0.9
% Population in rural area	32.0	7.5	−24.5***	23	−9.1
For population aged 16–64:					
% No academic qualification	8.2	11.0	2.8***	10	1.7**
% NVQ Level 4 qual. or above	35.7	30.1	−5.6***	33	−3.2
% Economically active	78.8	75.6	−3.2***	77	−2.1**
% Unemployment rate	6.1	7.9	1.8***	8	1.4**
% Employees getting work-related training (last month)	10	10	−0.4	10	−0.4
Number of firms by number of employees (in 2014):					
1–9	5,258	7,748	2,490***	4,803	−455
10–49	549	995	444***	589	40
50–249	96	184	88***	107	11
250+	24	42	18**	25	1
Number of LAs	270	20		20	

Note: The first panel provides statistics for the total population, the second panel for the population aged 16–64 and the third panel for firm size based on the total number of employees. ***, ** and * indicate significance at 1%, 5% and 10%, respectively.
Source: Annual Population Survey (NOMIS), 2011–2016; UK Business Counts (NOMIS), 2014.

the national policy.[25] However, as discussed above, we have very limited pre-treatment data so we focus on the difference-in-difference estimates. These are reported in Table 3 for the number of starts by LA in firms of different sizes (treated firms with 1–50, 1–250 or 251–1,000 employees) compared with the number of starts by LA for firms with more than 1,000 employees (the control group). Panel A shows results where the dependent variable is apprenticeship starts of individuals aged between 16 and 24. Panel B shows results for individuals over the age of 24.

Panel A shows positive treatment effects for all firm size groups. The estimated effects are larger for small firms consistent with BIS (2013), which

[25]The one exception is the number of LA starts in firms with 251–1,000 employees. In this case, apprenticeship starts were lower in treated units than in control units, both before and after the national policy was introduced.

TABLE 3

Difference-in-differences estimates for the national scheme

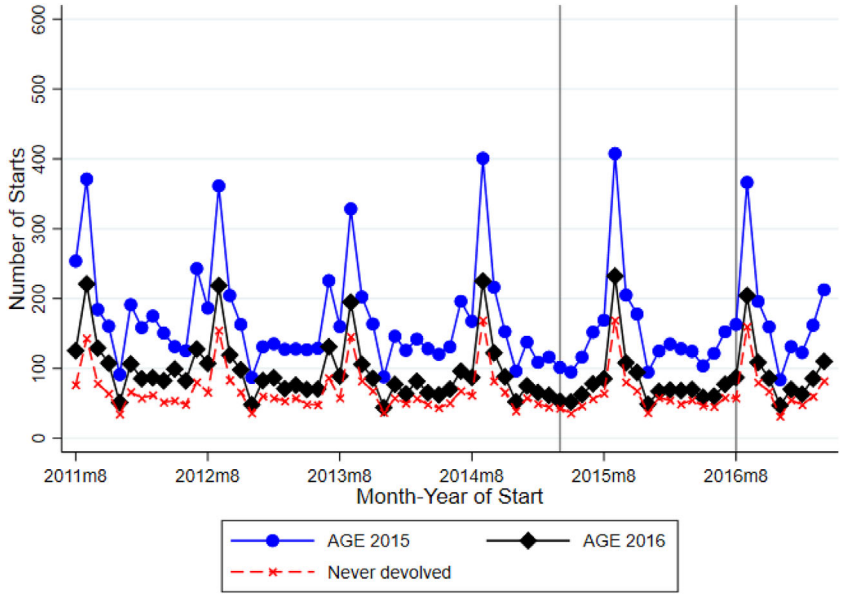
	(1) *≤50 employees versus >1,000 employees*	(2) *≤250 employees versus >1,000 employees*	(3) *251–1000 employees versus >1,000 employees*
Panel A: treatment group versus control group for 16–24 apprenticeship starts			
Treated * Post	0.350***	0.242***	0.170***
	(0.031)	(0.029)	(0.041)
Number of observations	46,688	26,568	26,568
Adjusted R^2	0.814	0.828	0.697
Panel B: treatment group versus control group for 24+ apprenticeship starts			
Treated * Post	0.607***	0.476***	0.147***
	(0.033)	(0.033)	(0.040)
Number of observations	46,688	26,568	26,568
Adjusted R^2	0.724	0.750	0.643

Note: Dependent variable is log (number apprenticeship starts per month + 1). ***, ** and * indicate significance at 1%, 5% and 10%, respectively. Standard errors clustered at LA level reported in parentheses. Observations are LA by month and year. For firms with 50 or fewer employees, we use data from August 2011 to April 2017. For firms up to 250 and firms 251–1,000 employees, we use data from August 2011 to December 2014 because firms with over 50 employees stopped being eligible for AGE from January 2015.

reports that the policy had higher take-up among small firms. These results are consistent with a positive effect of the national policy on apprenticeship starts at eligible firms, with the strongest effects felt for the smallest firms. Note, however, that Panel B also shows significant point estimates for apprenticeship starts for individuals aged over 24 who were ineligible for the policy. Indeed, for the first two columns, the estimated effects are considerably larger. There are at least two explanations. First, it is possible that the national AGE scheme did not have a genuine effect on apprenticeship starts – and that common national factors drove up apprenticeship starts amongst individuals of any age around the time the policy was introduced. Second, there could be complementarities between the hiring of apprentices aged 16–24 (i.e. the target of the AGE policy) and the hiring of older apprentices. Thus, while the evidence is consistent with the national scheme having increased apprenticeship starts, we cannot rule out other causes for this increase.

FIGURE 2

Average number of monthly apprenticeship starts per LA, by AGE treatment group



Note: AGE 2015, AGE 2016 and Never developed are groupings of LAs as defined in the text.
Source: Authors' own calculations from ILR data.

1. Devolution: descriptive statistics

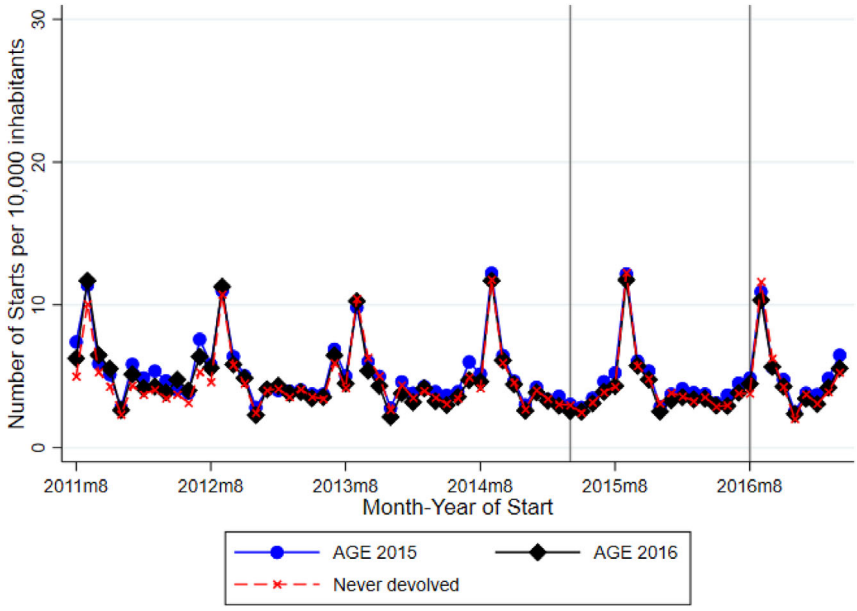
We now turn to the impact of AGE flexibilities. Before reporting regression results, we consider the raw data on the average monthly number of apprenticeship starts by LA for those aged 16–24 in the treatment groups (AGE 2015 and AGE 2016) and the control group for the full sample (the 'Never devolved' areas). Numbers of starts from August 2011 to April 2017 are plotted in Figure 2, and starts per 10,000 inhabitants over the same time period are plotted in Figure 3. The vertical lines indicate when flexibilities were introduced in AGE 2015 and AGE 2016 areas, respectively. Neither plot shows any marked change in the number of apprenticeship starts in treatment areas (relative to control areas) coinciding with policy implementation. Of course, it might be that there are small changes that are not picked up by visual inspection but that can be detected in the regression analysis.

2. Difference-in-differences

Table 4 shows results from the difference-in-differences specification (equation 2), reporting estimates of the coefficient of interest: the effect of

FIGURE 3

Number of monthly apprenticeship starts per LA (per 10,000 inhabitants), by AGE treatment group



Note: AGE 2015, AGE 2016 and Never developed are groupings of LAs as defined in the text.
Source: Authors' own calculations from ILR data.

introducing flexibilities in treatment areas relative to control areas. The upper panel shows results for all LAs and the lower panel shows results for LAs that have 'common support'. Results are estimated separately for AGE 2015 areas (columns 1, 3 and 5) and AGE 2016 areas (columns 2, 4 and 6). They are reported for all firms (columns 1 and 2), the subgroup of firms with 51–250 employees (columns 3 and 4) and for firms with up to 50 employees (columns 5 and 6). Firms of 51–250 employees were eligible to receive the subsidy in all but one of the devolved areas (after the policy was introduced) but not in control areas. Firms with up to 50 employees were eligible for the subsidy both nationally and in the devolved areas. Thus, for those firms with 50 or fewer employees, any 'treatment effect' would be attributable to increased generosity or better targeting in devolved areas. The pattern of results is the same across all specifications. The treatment effect is small, negative and not statistically different from zero in all but two cases.

Figures 4 and 5 show an event study for total number of apprenticeships by LA for the sample with common support for AGE 2015 and AGE 2016 areas,

TABLE 4

Difference-in-differences results for number of apprenticeship starts by LA and firm size

	All firms		*Firms with 51–250 employees*		*Firms with up to 50 employees*	
	AGE 2015 vs. Never devolved *(1)*	AGE 2016 vs. Never devolved *(2)*	AGE 2015 vs. Never devolved *(3)*	AGE 2016 vs. Never devolved *(4)*	AGE 2015 vs. Never devolved *(5)*	AGE 2016 vs. Never devolved *(6)*
All LAs						
Treated * Post	−0.019	−0.026	−0.068	−0.082	−0.021	−0.060**
	(0.016)	(0.026)	(0.044)	(0.064)	(0.027)	(0.024)
Number of obs.	20,010	20,010	19,734	20,010	19,734	20,010
Adjusted R^2	0.909	0.897	0.722	0.697	0.890	0.879
LAs with common support						
Treated * Post	−0.022	−0.051**	0.015	−0.090	−0.028	−0.043
	(0.020)	(0.025)	(0.047)	(0.087)	(0.036)	(0.038)
Number of obs.	3,657	10,833	3,519	10,143	3,519	10,143
Adjusted R^2	0.920	0.920	0.654	0.686	0.873	0.880

Note: Dependent variable is log (number apprenticeship starts per month + 1) as discussed in the text. Estimation is over the time period from August 2011 to April 2017. ***, ** and * indicate significance at 1%, 5% and 10%, respectively. Standard errors clustered at LA level reported in parentheses. The regressions control for LA and month–year dummies. The second panel restricts the analysis to areas with common support on the propensity score.

respectively.[26] The timing of flexibilities is denoted by t and the figures plot coefficients for a set of six-month-period dummies interacted with treatment status from up to 60 months before the policy to up to 24 months afterwards.[27] The coefficients on the interacted dummies are insignificant in all time periods. In other words, consistent with our difference-in-difference results, devolved areas did not have more apprenticeship starts than non-devolved areas, either before or after additional flexibilities were introduced in 2015 and 2016 respectively.[28] The event study for firms with 51–250 employees tells a similar story, though with much wider confidence intervals around estimates (and hence the associated figures are not reported).

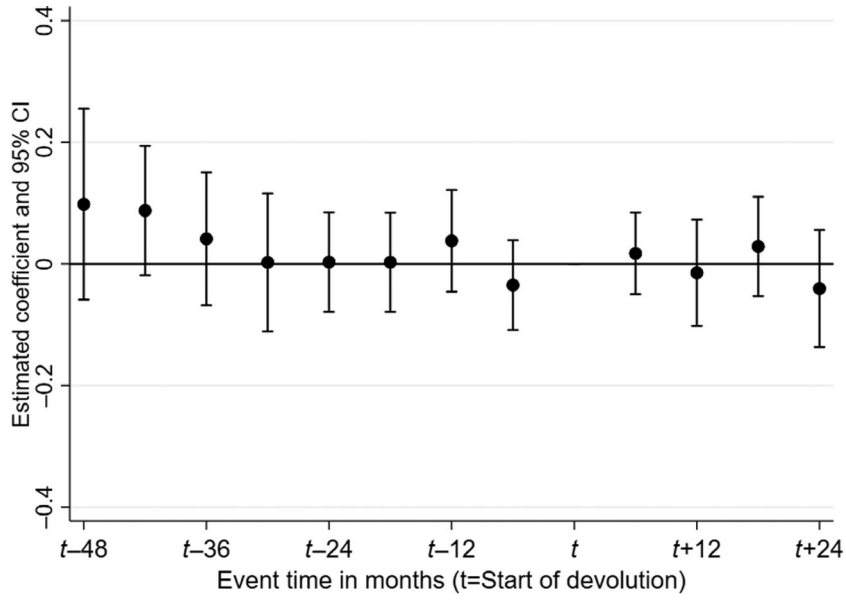
[26]The plot looks very similar for the full sample. However, for the full sample more coefficients are significant in the pre-policy period, suggesting the existence of differential trends in treatment and control areas if we do not restrict the sample to LAs with common support.

[27]The reference period for both Figures 4 and 5 is the same, from August 2011 to April 2017. However, because flexibilities came into effect at different times in different areas the number of lags and leads changes.

[28]There is only one point estimate in the 'pre-policy' period that is statistically different from zero for AGE 2016 areas. But this a long time ($t - 60$) before the policy starts in these areas. All other point estimates are not statistically different from zero either before or after the policy is introduced.

FIGURE 4

Event study for total number of apprenticeship starts by LA for AGE 2015 areas (common support sample)



Note: The figure plots coefficients for a set of six-month-period dummies interacted with treatment status from up to 48 months before the policy (for West Yorkshire CA) to up to 24 months afterwards (for Greater Manchester CA and Sheffield City Region). The devolution started in April 2015 for Greater Manchester CA and Sheffield City Region, and in August 2015 for West Yorkshire CA. Analysis is restricted to areas with common support on the propensity score.

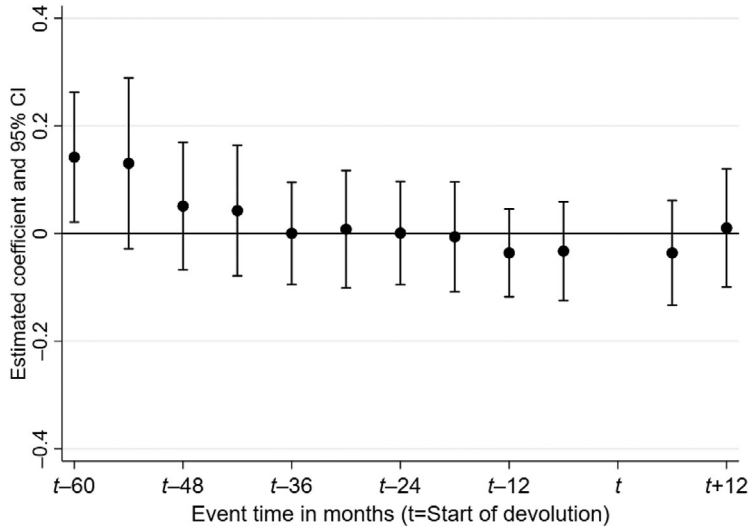
3. Synthetic control method

The results from the synthetic control method are illustrated in Figure 6 for the full sample of firms, with more explanation of results in online Appendix D.[29] Figure 6 illustrates the gap in the number of apprenticeship starts between the devolved areas and the synthetic control group for each time period. A horizontal line at zero in the pre-treatment period would indicate that the synthetic control closely matches the treatment group before the introduction of flexibilities. For AGE 2015, we observe a steady pattern in the pre-treatment period. There is a slight downward-sloping pattern for AGE 2016, which starts in the pre-treatment period and seems to attenuate in the post-intervention period. Overall, however, it fluctuates around zero in both the pre- and

[29]For the synthetic control method, West Yorkshire CA is excluded from the AGE 2015 areas because it implemented the policy slightly later. Results for West Yorkshire CA only are very similar to those reported for other AGE 2015 areas.

FIGURE 5

Event study for total number of apprenticeship starts by LA for AGE 2016 areas (common support sample)



Note: The figure plots coefficients for a set of six-month-period dummies interacted with treatment status from 60 months before the policy to 8 months afterwards. The devolution started in August 2016. Notice that $t + 12$ includes only two months (March and April 2017). Analysis is restricted to areas with common support on the propensity score.

post-treatment periods, and it is usually contained between −0.2 and 0.2 logs. These results confirm those of our main analysis, suggesting no significant effect of the increased flexibility.

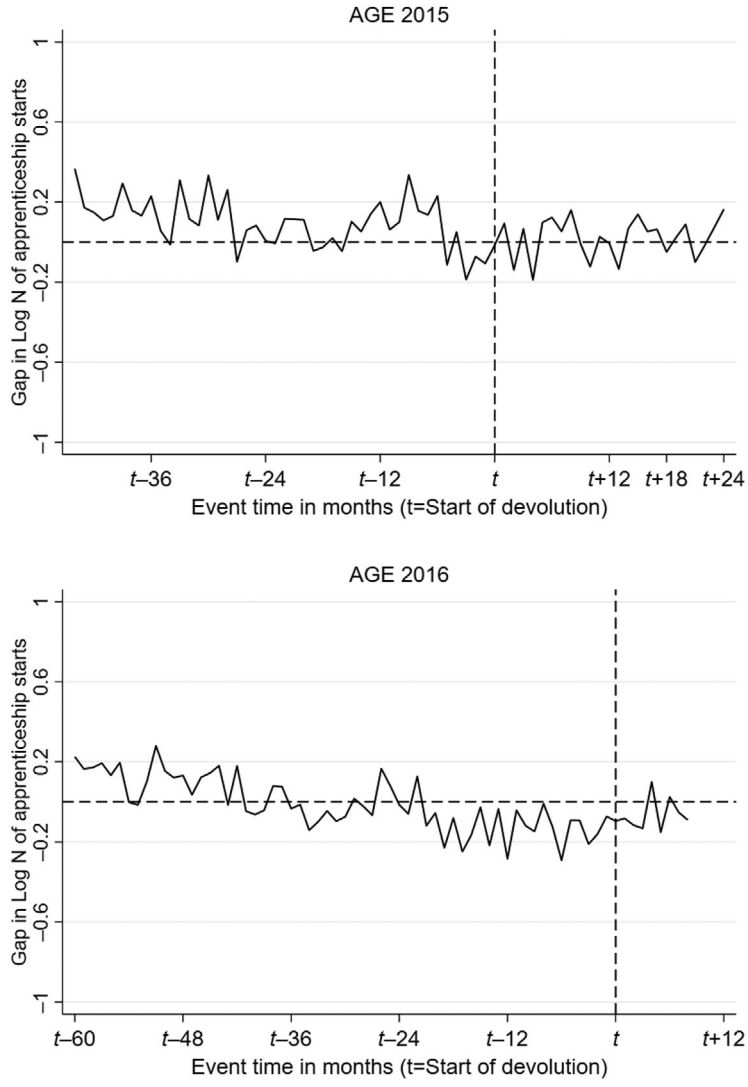
4. Effects on ineligible workers?

As a final exercise we consider whether there are any differences pre- and post- policy in devolved areas for workers who were aged over 24 and thus ineligible for the grant. To do this, Table 5 replicates Table 4 for workers aged 25 and older. As before, we show results from the difference-in-differences specification (equation 2). The upper panel shows results for all LAs and the lower panel shows results for LAs that have common support.

This exercise enables us to comment more closely on whether areas extending the scheme were cutting back on training faster than in other areas. As with Table 4, the treatment effect is small and not statistically different from zero in all but two cases. The exception is for small firms in AGE 2016 areas, which are less likely to hire older apprentices in the post-policy period. We found the same result in Table 4 when considering all LAs (upper panel).

FIGURE 6

Gap in (log) total number of apprenticeship starts between devolved area and the synthetic control group



Note: Results from synthetic control estimated for the full sample of firms. The AGE 2016 group is excluded from estimation for AGE 2015, and vice versa. The devolution started in April 2015 for AGE 2015 areas. Note that as West Yorkshire CA is excluded from the analysis there are fewer lags (pre-policy) for AGE 2015 areas than in Figure 4. The devolution started in August 2016 for AGE 2016 areas.

TABLE 5

Difference-in-differences results for number of apprenticeship starts by LA and firm size (employees aged 25+)

	All firms		*Firms with 51–250 employees*		*Firms with up to 50 employees*	
	AGE 2015 vs. Never devolved *(1)*	AGE 2016 vs. Never devolved *(2)*	AGE 2015 vs. Never devolved *(3)*	AGE 2016 vs. Never devolved *(4)*	AGE 2015 vs. Never devolved *(5)*	AGE 2016 vs. Never devolved *(6)*
All LAs						
Treated * Post	0.029	−0.025	0.064	0.028	0.020	−0.086**
	(0.026)	(0.046)	(0.067)	(0.068)	(0.028)	(0.041)
Number of obs.	20,010	20,010	19,734	20,010	19,734	20,010
Adjusted R^2	0.843	0.826	0.591	0.571	0.801	0.786
LAs with common support						
Treated * Post	0.025	−0.033	0.065	0.031	0.046	−0.107**
	(0.031)	(0.049)	(0.162)	(0.085)	(0.062)	(0.053)
Number of obs.	3,657	10,833	3,519	10,143	3,519	10,143
Adjusted R^2	0.870	0.852	0.548	0.556	0.769	0.786

Note: Dependent variable is log (number apprenticeship starts per month + 1) as discussed in the text. Estimation is over the time period from August 2011 to April 2017. ***, ** and * indicate significance at 1%, 5% and 10%, respectively. Standard errors clustered at LA level reported in parentheses. The regressions control for LA and month–year dummies. The second panel restricts the analysis to areas with common support on the propensity score.

When using common support and eligible apprentices, the negative coefficient appeared for all firms in AGE 2016 areas (Table 4), rather than firms with up to 50 employees (Table 5). Overall, these results do not suggest that devolved policy protected younger apprentices, relative to older apprentices, from changes in economic conditions that might have affected devolved areas differentially over the same period as the policy changes.

V. Conclusion

Devolution of skills policy in England started incrementally but is becoming more important. For example, from 2019, about half of the overall Adult Education Budget will be devolved to mayoral Combined Authorities across England. Many of these areas are the same as those that negotiated flexibilities for AGE. In this paper, we show that all the hard work in negotiating these flexibilities made no measurable difference to the number of apprenticeship starts in devolved areas.

A plausible explanation is that flexibilities were negotiated on the wrong margins. The evaluation of the national scheme[30] suggested that take-up was much more prevalent among very small firms than in any other group. To the extent that the national policy was effective for increasing apprenticeship starts, it was most effective for very small firms. Thus, it is arguable that more effort in devolved areas should have been made to make the system more generous for those firms, rather than expanding subsidies to larger firms where take-up had been poor in the national scheme. This suggests that local decision makers who were negotiating flexibilities either had information constraints (i.e. they did not know about the experience in the national scheme) or were influenced by the wrong stakeholders (e.g. larger local employers). It is also possible that the form of devolution on offer was simply too incremental to be useful at the local level. The more general point is that devolution needs to be accompanied by structures to discern carefully how to use resources effectively in the local context. Otherwise, there is a danger that devolution multiplies bureaucracy (with associated costs) while doing little or nothing for local economic growth.

Supporting information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

• Online Appendix

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