The Impact of Subsidies on Training: Evidence from the United States

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The Impact of Subsidies on Training: Evidence from the United States

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Abstract

Employer-provided training facilitates labour market integration, rapidly adapts to evolving labour market trends and imparts work-relevant skills. The United States Department of Labor facilitates training through Registered Apprenticeship. The United States Department of Labor distributes subsidies to incentivise employer participation, in amounts exceeding one billion USD since 2015. The first such subsidy was the American Apprenticeship Initiative (AAI). The AAI's goal was to increase the number of Registered Apprenticeship contracts in eligible states in the advanced manufacturing, information, and healthcare industries. I investigate the AAI's causal effect on the number of Registered Apprenticeship contracts using United States Department of Labor administrative data. I exploit state, time, and industry variation in AAI treatment eligibility to conduct difference-in-difference and triple-difference methodologies. To reinforce internal validity, I leverage spatial variation to perform spatial regression discontinuity and spatial difference-in-discontinuity estimations. Results indicate that the AAI has not caused growth in the number of Registered Apprenticeship contracts. I find no statistically significant heterogeneity in the effect of subsidies on Registered Apprenticeship contracts between states with high and low credit constraint index averages. Similarly, the prevalence of small firms does not drive treatment effect heterogeneity.

Keywords: Labour Demand; Triple Difference; Spatial Difference-in-Discontinuity; Subsidies; Public Policy Evaluation.

JEL Codes: J01; J08; J23

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1. Introduction

Labour demand, particularly in innovative sectors, is rapidly evolving due to fast-paced technological change (Lightcast, 2022, Wang, 2012). The speed of this evolution causes the demand for skilled workers to outpace supply in such sectors (Feng, 2021, Wang, 2012). Consequently, employers in the United States lack a qualified workforce in multiple industries (Lerman et al., 2019 Kalejaiye, 2023, Magnini et al., 2024). Adaptable solutions to mitigate labour market shortages are dual vocational education and training (VET) in Europe, and Registered Apprenticeship in the United States separately. They permit to quickly impart work-relevant skills to the workforce (Lightcast, 2022, Feng, 2021, Bolli et al., 2021, Gallup, 2024, OECD/ILO, 2017, Burlat, 2024).

A well-established Registered Apprenticeship system is associated with a reduction of skill shortages and youth unemployment rates (Lou and Hawley, 2019, Reed et al., 2012, Kuczera, 2017, OECD/ILO, 2017). To achieve these benefits, the United States Department of Labor has distributed over $1B of training subsidies to maximise employer engagement in Registered Apprenticeship between 2015 and 2022 (Butrica et al., 2023, Gardiner et al., 2021). Increased subsidisation of Registered Apprenticeship in the United States occurs amidst an international trend in public subsidisation of training (United Kingdom Department of Business, Innovation and Skills, 2013, Employment and Social Development Canada, 2019, République Française, 2025, Australian Government, 2014, Sweden Ministry of Employment, 2014, see Corseuil et al., 2019, for Brazil). Investigating whether subsidies are an effective way to raise training prevalence is thus of current and widespread relevance.

Registered Apprenticeship in the United States contains two components: related technical instruction and on the job training (Gardiner et al., 2021, Lerman and Rauner, 2011). First, on-the-job training entails competence acquisition relevant to tasks performed. This comprises learning regarding processes, physical and social work environments (De Jong, 1996). It imparts both firm-specific and general skills (Lerman, 2016, Lerman and Rauner, 2011). Within the framework of Registered Apprenticeship, on the job training is supervised by an on-site mentor. It must last at least 2,000 hours (Fumia et al., 2022, United States Department of Labor, 2008, Lerman and Rauner, 2011). Second, related technical instruction is dispensed by community colleges, professional associations, labour unions, or by employers themselves (Gardiner et al., 2021, Webster et al., 2022). Related technical instruction entails a minimum of 144 hours of instruction. It may be conducted in an in-person or virtual environment (United States Department of Labor, 2008, Lerman and Rauner, 2011).

Registered Apprenticeship belongs to non-formal education. Eurostat (2016) conveys that both formal and non-formal education are institutionalised, intentional, and contain predetermined teaching methods. However, unlike non-formal education, formal education leads to a certification recognised by national education authorities and is classified in UNESCO’s (2011) International Standard Classification of Education (ISCED). It is organised and systematically planned (Coombs and Ahmed, 1974, Johnson and Majewska, 2022). Registered Apprentices do receive an industry-recognised and nationally recognised certificate upon completion, recognised by the United States Department of Labor (Gardiner et al., 2021, Lerman and Rauner, 2011). However, this certificate is not recognised by the Department of Education of the United States and is not listed in the United States’ ISCED.

The relevant literature, reviewed in this paper, entails studies discussing subsidies and training funds pertaining to the Danish (Westergaard-Nielsen and Rasmussen, 2000), German (Schuss, 2023), Dutch (Kamphuis et al., 2010), Swiss (Muehlemann and Wolter, 2014, Muehlemann et al., 2005) and French (Brebion, 2020) dual VET systems. These studies analyse formal education. They are relevant here for two reasons. First, literature on the causal impact of subsidies on non-formal training provision by firms, especially in the United States, is scant (Kuczera, 2017, Mueller and Behringer, 2012). Second, the average duration of Registered Apprenticeship is comparable to that of most European dual VET programmes. The most frequent duration of Registered Apprenticeship is four years (Collins, 2016), versus three years in Germany and Switzerland (Muehlemann et al., 2010). Similar duration of training is important because it entails similar investment horizons for training firms. Duration affects the decision to invest in training, and the period of occurrence of cost and benefits from training. Firms with similar investment horizons will face analogous economic cycle fluctuations over time (Malcolmson et al., 2003). This reinforces comparability between training firms regarding economic conditions.

The subsidisation policy evaluated in this paper is the American Apprenticeship Initiative (AAI). Its objective was to lessen the dependence on foreign labour in key industries. It aims to do so by maximising employer engagement and upskilling the United States workforce through Registered Apprenticeships (United States Department of Labor, 2015). Employer engagement in Registered Apprenticeship is indeed low in the United States (Lerman, 2016, Lerman et al., 2019, United States Department of Labor, 2015, OECD/ILO, 2017). The AAI grant period lasted five years from October 1st, 2015, to September 30th, 2020 (USA Grants, 2015). These funds were distributed in 24 states. The AAI targeted the key industries of advanced manufacturing, healthcare, and social assistance, and information (United States Department of Labor, 2015).

The original contribution of this paper is therefore to investigate the causal effect of the AAI on the number of Registered Apprentices in the United States. There is a lack of causal evidence on the impact of subsidies on training (Mueller and Behringer, 2012). This lack of evidence is particularly acute regarding Registered Apprenticeship positions (Kuczera, 2017).

This paper's empirical analysis uses administrative data on start date, state, county, and industry of Registered Apprentices from RAPIDS (Registered Apprenticeship Partners Information Database System, ApprenticeshipUSA, 2024). Employers across the United States must submit individual Registered Apprentice record data to the Department of Labor's Office of Apprenticeship. These data are then recorded into the RAPIDS database.

This paper uses four identification strategies. The first is difference-in-difference estimation across treated states and time. The second identification strategy additionally leverages industry variation in treatment eligibility in triple difference estimations. The third identification strategy leverages spatial variation in treatment in the form of a spatial regression discontinuity design. Finally, the fourth identification strategy is spatial difference-in-discontinuity estimation, leveraging spatial and time variation. All four identification strategies lead to identical inference. The AAI has not statistically significantly increased the number of Registered Apprentices. All robustness checks corroborate these baseline results. Triple difference and difference-in-discontinuity coefficients additionally convey a small treatment effect magnitude. This suggests that the AAI has had a statistically and economically insignificant effect on the growth rate of the number of Registered Apprentices.

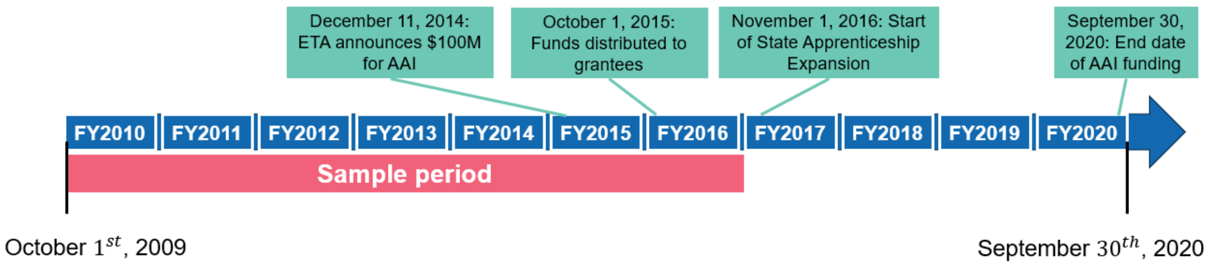
The remainder of this paper is structured as follows. Section 2 discusses background information and implementation of the AAI. Section 3 reviews extant literature on the effects of subsidies, as well as training funds, on training provision. Section 4 discusses the econometric methodology. Section 5 represents the data section. Section 6 analyses results. Section 7 discusses the results. Section 8 concludes and offers policy recommendations.

2. The American Apprenticeship Initiative

2.1. Background Information

The AAI was announced by the United States Department of Labor on December 11th, 2014. Funds were distributed starting on October 1st, 2015 (Fumia et al., 2022) and lasted five years till September 30th, 2020 (USA Spending, 2015). Figure 1 shows the timeline of the AAI. It also indicates the start of the State Apprenticeship Expansion. This policy occurred after the start of the AAI. It is a potential confounder (United States Department of Labor, 2016).

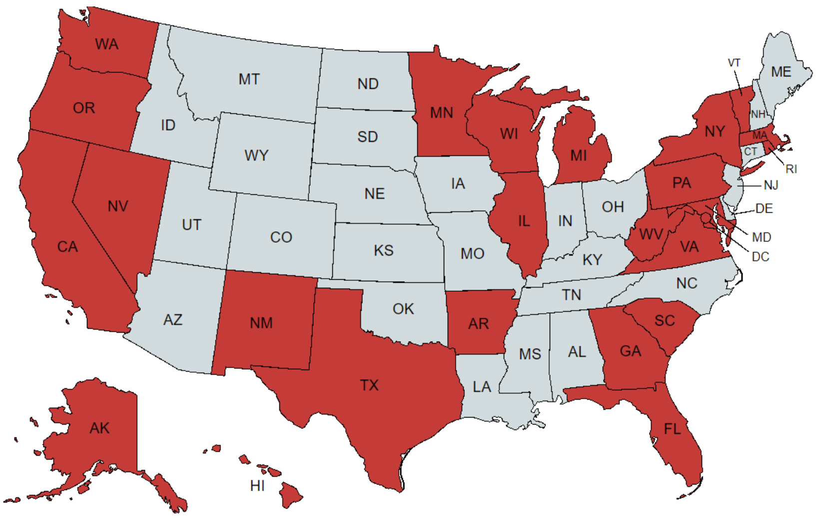
Figure 1: American Apprenticeship Initiative Timeline



Note: Author's Own Elaboration using Information from National Governor's Association (2020) and United States Department of Labor (2015). FY= fiscal year. AAI = American Apprenticeship Initiative. Sample period refers to the period considered in our econometric analysis main specifications, discussed in Section 4. Years indicated on the horizontal axis indicate United States fiscal years. For instance, the 2015 fiscal year runs from October 1st, 2015, to September 30th, 2016.

Figure 2 displays AAI treated states, receiving AAI funds, across the continental United States in red. Alaska and Hawaii are also treated. They are mainly located on the Atlantic and Pacific Coasts, as well as within states bordering the Great Lakes. Both Alaska and Hawaii are also treated by the AAI.

Figure 2: Treated States in the United States

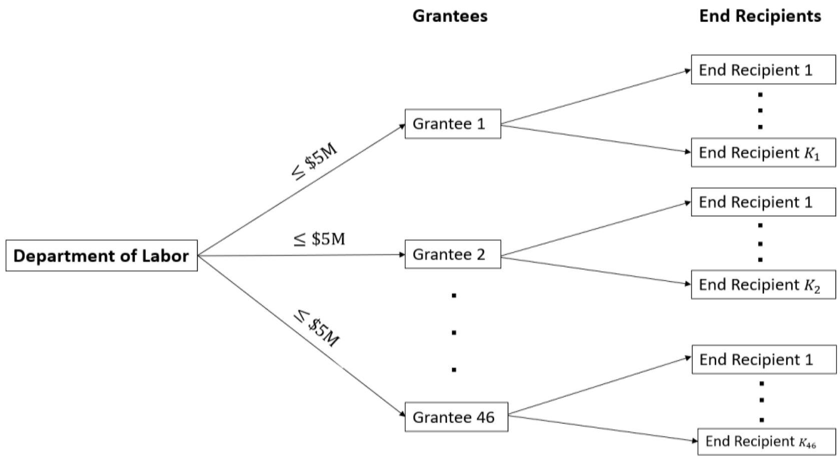


Note: States coloured in red are treated by the American Apprenticeship Initiative. Grey states are considered control, as they were not treated by the AAI. Figure is author's own elaboration using USA Spending (2015) data.

2.2. Eligibility and Applications for AAI Funding

AAI funding is distributed in a two-stage process. First, applicants apply at the Department of Labor for funding. Second, successful applicants then use the funding to expand Registered Apprenticeship in the advanced manufacturing, information, and healthcare industries, within their place of performance, i.e. their state. The specific use of funds by grantees is discussed in subsection 2.3. Figure 3 illustrates the mechanisms of AAI fund attributions. Each grantee first received a subsidy of at most $5M on October 1st, 2015, the effective start date of the grant. The reception of these funds is symbolised by the first set of arrows, linking the Department of Labor to AAI grantees. Subsequently, grantees distributed these funds within their respective states to "end recipients", i.e. the last entities to receive funds. "End recipients" include community colleges or employers (Gardiner et al., 2021, Copson et al., 2021, Kuehn et al., 2022). This distribution of funds is symbolised by the second set of arrows in Figure 3, linking grantees to end recipients.

Figure 3: Distribution of American Apprenticeship Initiative Funds



Note: K denotes the kth end recipient. “End recipient” denotes the last entity to receive subsidies. These entities principally included community colleges or Registered Apprentice employers. (Gardiner et al., 2021). Each arrow going from “Grantee” to “End Recipient” represent a channel through which AAI funds may affect the number of Registered Apprentices. K_1 refers to the number of end recipients of grantee 1.

AAI applicants must consist of public-private partnerships of multiple entities. Public sector entities may include but are not limited to a representative of the workforce investment system, a public education or training provider, e.g. a community college system or technical college systems, or a State Apprenticeship Agency. Private sector entities may include but are not limited to a consortium of business or a single business, a nonprofit organisation, e.g. a chamber of commerce, or a workforce intermediary, for instance a labour union or industrial association (United States Department of Labor, 2015). For example, Los Rios Community College District, a grantee in California, partnered with IBM, California Manufacturers & Technology Association, among others. These partnerships then conducted outreach to local employers (e.g. LinkedIn, Zendesk or Pinterest in California). The latter eventually receive AAI funding (National Governor’s Association, 2020).

AAI applicants must show the ability and commitment to expand Registered Apprenticeship within the industries of healthcare and social assistance, advanced manufacturing, or information technology (United States Department of Labor, 2015). Each submitted application was assessed and scored against a set of predefined criteria. Scoring is points-based. The maximum number of points awarded to an applicant is 100. A grant officer of the Department of Labor decides whether to grant funds.

2.3. Fund Use

The Department of Labor permitted grantees to use their AAI funds for various purposes, listed in Table A1. However, one common goal was to be pursued according to AAI guidelines. This objective is to increase the prevalence of Registered Apprenticeships in the grantees’ respective states and in the industries of healthcare, advanced manufacturing, and information (United States Department of Labor, 2015). Funds could also be used on Registered Apprentices *already* working at the training firm. Two main regulations governed the use of funds. First, at most $10,000 can be spent per apprentice. Second, AAI funds cannot be used to reimburse apprentice wages (United States Department of Labor, 2015).

Two-thirds of grantees used financial support to provide incentives to employers to supply Registered Apprenticeship positions (Gardiner et al., 2021, Copson et al., 2021, United States Department of Labor, 2015). 38% of grantees cover related technical instruction costs of Registered Apprentices. Grantees do this either by paying tuition to the related technical instruction provider directly or by reimbursing the employer ex-post for Registered Apprentices' related technical instruction. 31% of grantees offer employers incentives for on-the-job training, for instance by defraying mentor wage costs (Gardiner et al., 2021, Copson et al., 2021, United States Department of Labor, 2015)[1].

Grantees were not obligated to further distribute all received AAI funds to "end recipients". For instance, most of Wisconsin Department for Workforce Development's AAI subsidy was used to directly hire "apprenticeship liaison" or "apprenticeship coordinator" staff (Gardiner et al., 2021). Liaison staff coordinate local training activities and conduct *localised* outreach towards employers, regarding the promotion and development of Registered Apprenticeship programmes (Copson et al., 2021). Liaison staff possess expertise in local labour market needs. Liaison staff thus act as intermediaries between companies and governing authorities. They help employers navigate federal and state regulations. This alleviates bureaucratic costs. The overarching goal is to maximise employer engagement in Registered Apprenticeship (Webster et al., 2022).

Copson et al. (2021) provide an overview of certain grantees' spending. One such case study is that of the South Carolina Technical College System. The South Carolina Technical College System used 84% of its subsidy to fund related technical instruction costs of Registered Apprentices. South Carolina-based Registered Apprentice employers sent applications to the South Carolina Technical College System to receive subsidies to help finance related technical instruction of their Registered Apprentices. Subsidies were then allocated directly to the relevant technical colleges providing related technical instruction. These covered the tuition fees of Registered Apprentices whose employers had qualified to obtain subsidies. As at the 2017/18 fiscal year, 138 company locations had the tuition fees of at least one apprentice covered by the AAI (State Board for Technical and Comprehensive Education, 2018).

3. Relation to the Literature

This section places this paper into the broader context of studies evaluating the impact of policy interventions on training provision. Two types of policy interventions aimed at raising training provision exist. The first is subsidisation. Subsidisation involves a payment or tax concession from public authorities (Steenblick, 1995). Subsidies aim to alleviate the cost burden of training to employers (Gardiner et al., 2021, Rosenberg and Dunn, 2010, Webster et al., 2022). The second type of policy intervention is training funds. Training funds are stocks or flows of financing serving the purpose of developing work-relevant productive skills (Johanson, 2009). Although often established by public authorities, they are typically not financed by governments, but through payroll (equivalently wage bill) taxes. This financing method is also referred to as levy system (Schuss, 2023). Firms make voluntary or compulsory contributions to the training fund through this levy system, depending on their payroll. Subsequently, they may apply for the reimbursement of their training activities (Johanson, 2009).

Two factors, which disincentivise firms from offering training, may prompt policymakers to intervene in training provision. The first is difficulty accessing bank financing to fund training. This situation defines

[1] Unfortunately, the exact information for each grantees regarding the end distribution of these funds and end recipients is not identifiable.

credit constraint. It may occur because training has no collateral (Stevens, 1999). Financial institutions thus demand a higher interest rate to training providers to compensate for this lack of collateral. Firms' credit constraints might disincentivise firm investment in apprenticeship, leading to less apprenticeship positions overall. The second factor is that the provision of training by individual firms is relatively costly. This follows from the absence of economies of scale (Kuku et al., 2016). To alleviate such costs, policymakers may establish training funds, pooling training investments to create economies of scale.

This section first reviews the strand of literature discussing the effect of subsidies on formal apprenticeships. In the second subsection, I review the impact of subsidies on firm-provided training. Firm-provided training is mostly non-formal (Fialho et al., 2019). In the third subsection, I review the effect of training funds on formal apprenticeships and firm-provided training. The final subsection postulates that credit constraints may moderate the effect of subsidies on training.

3.1. Effect of Training Subsidies on Apprenticeship Positions

Policymakers' overall rationale for subsidising training is to increase provision of training by lowering its cost (Westergaard-Nielsen and Rasmussen, 2000, Brebion, 2020, Muehlemann et al., 2005, Mueller and Behringer, 2012, OECD/ILO, 2017). However, there is only limited empirical evidence regarding the effect of subsidies on the supply of apprenticeship positions.

Westergaard-Nielsen and Rasmussen (2000) evaluate the effect of subsidies in Denmark on the supply of apprenticeship positions. Public authorities offer subsidies to incentivise firms to retain their supply of apprenticeship positions in response to adverse market conditions. Using a random-effects Poisson model, Westergaard-Nielsen and Rasmussen (2000) find that a 50% increase in subsidisation levels results in an overall increase in the number of apprentices by approximately 5%.

Brebion (2020) evaluates the impact of a subsidisation reform on apprenticeship training in France. French authorities subsidised apprenticeships to combat high rates of youth unemployment. Through triple difference estimations, Brebion (2020) finds that subsidies increase the number of apprentices within training firms. However, subsidies did not increase the share of firms that train apprentices. Consequently, Brebion (2020) finds that subsidies were effective at raising intensive training margins, but not extensive training margins.

3.2. Effect of Subsidies on Firm-Provided Training

Georg and Strobl (2006) evaluate the effect of subsidies on firm-provided training in Ireland. They distinguish between domestically owned and foreign-owned firms. The authors underline that subsidies are implemented when training is low because of firms' resource-constraints. Skill shortages ensue, as training falls below its socially optimal level. Georg and Strobl (2006) use difference-in-difference estimation with propensity score matching. Authors find that subsidies were effective at bolstering training expenditure in production plants belonging to domestic firms. However, subsidies did not affect training expenditure in production plants of foreign, multinational firms, who are less credit constrained.

Leuven and Oosterbeek (2004) evaluate the effect of a tax deduction on training for workers above 40 years of age in the Netherlands. Rationale for this training subsidisation policy was the importance of the pursuit of lifelong learning. This will continuously upskill the workforce and foster economic prosperity. Leuven and Oosterbeek (2004) exploit a discontinuity in the eligibility of workers above 40 years in a fuzzy regression discontinuity framework. The authors find that this policy results in training participation for workers slightly over 40 being 15 to 20% higher than those for workers just below forty years of age. However, authors show this is due to training postponement rather than a genuine training stimulation.

Tian et al. (2022) analyse the impact of a rise in the tax credit rate of firms for on-the-job training of employees in China. The aim of this policy was to remediate insufficient funding of employees' on-the-job training by firms. The authors employ difference-in-difference methodology. Tian et al. (2022) find that this increase in the tax deduction rate significantly boosted firms' expenses on employee on-the-job training. The effect was the strongest for privately-owned firms, and small firms, who were more likely to be credit constrained.

Martins (2021) investigates the effect of a policy entitled "Training for Innovation and Management" on training expenditure in Portugal. It aims to correct underinvestment in training caused by two market imperfections: poaching and credit constraints. The importance of training investments is emphasised by rapidly evolving technology, requiring up-to-date human capital. The author finds, through difference-in-difference methodology, that the subsidies increased training expenditures of firms and the number of training hours significantly.

Goerlitz (2010) evaluates the effect of a publicly financed voucher policy on training participation in Germany. This voucher's objective was to increase the training participation of low-skill workers amongst firms with less than 250 employees. Employee participation in training diminishes the latter's risk of dismissal and increases their respective firms' competitiveness (Goerlitz, 2010). The author finds, through triple difference methodology, that the share of establishments investing in training rose. On the other hand, among firms already providing training, the number of training hours provided was not significantly affected by the policy.

Holzer et al. (1993) investigate the effect of a Michigan state-financed training subsidy on firm-provided training hours. The subsidy aimed to bolster training to upskill the local workforce amidst rising international competition. In turn, the objective is to prevent employment and wage loss. Subsidies were thus targeted at financing the training of workers to learn the usage of modern, innovative technologies. Authors, through first-difference equations, find that this programme was associated with a two to three-fold increase in training hours provided by recipient employers.

Abramovsky et al. (2011) evaluate the impact of an employer training pilot scheme for the "Train to Gain" policy implemented in Great Britain. Policymakers strived to increase long-run productivity performance of the workforce by increasing the comparatively low national skill level. Using difference-in-difference methodology, Abramovsky et al. (2011) find that, among employers eligible to subsidies, there was no statistically significant increase in the share of employers providing training. Employees' take-up of training also was not significantly affected by the employer training pilot scheme.

3.3. Effect of Training Funds on Apprenticeship Positions and Firm-Provided Training

Training funds are a different policy instrument to subsidies. Nonetheless, they share the same goal: correcting market imperfections that result in the level of training being suboptimal (Stevens, 1999, Kamphuis et al., 2010). Training funds are particularly aimed at mitigating poaching and at creating economies of scale for training investments.

Typically, training funds are financed by levy contributions from firms, which increase with firms' respective wage bills (Schuss, 2023). Subsequently, training funds reimburse or directly finance pre-defined training activities. Shared investments in training funds make all member firms invest in the skill development of the workforce. This reinforces the incentive for a firm to avail from the training fund into which it pays and train its own labour, instead of poaching skilled labour. Training funds are also referred to as levy systems. They can be state-specific, industry-specific, or both (Schuss, 2023).

Schuss (2023) analyses the effect of the introduction of a levy scheme in Germany in the geriatric nursing sector on apprenticeship training. This specific sector was targeted due to labour shortages and population ageing. Moreover, regulation of long-term care insurance in Germany prevents training firms active in this sector from adjusting prices in response to apprenticeship costs. This disincentivises the training of apprentices. The overarching goal of this levy scheme was thus to remedy this market imperfection and provide a financial incentive to apprenticeship training. Schuss (2023) uses staggered difference-in-difference methodology to find that the levy scheme has a positive effect on the extensive margin of training in ambulatory care. It also has a positive effect on intensive training margins in inpatient care.

Kamphuis et al. (2010) evaluate the impact of sectoral training and development funds on apprenticeship training in the Netherlands. The aim of these funds is to decrease the marginal cost of training, create economies of scale and combat poaching (Kamphuis et al., 2010). Through multilevel modelling combined with propensity score matching, Kamphuis et al. (2010) find that sectoral training funds did not statistically significantly cause firms in these sectors to invest more in training.

Kuku et al. (2016) evaluate the impact of training funds on the number of training hours provided by firms in Mauritius. The rationale for the training fund's implementation was to bolster lifelong learning to enhance human capital. This would help shift Mauritius' economy's paradigm towards a knowledge economy, based on human capital and innovation. Kuku et al. (2016) use multivariate OLS and probit regressions. The authors demonstrate that training funds weigh on the finances of medium and large firms. These firms tend to pay more in payroll taxes than they receive in subsidies. Medium and large firms generally provide most of the training in emerging economies. Adding to these firms' financial burden may cause them to cut back on training expenditures. Overall, this reduces the level of training in the economy.

Appendix Table A2 summarises literature reviewed in subsections 3.1, 3.2 and 3.3. In view of extant literature, I posit the following hypothesis regarding the effect of the AAI on the number of Registered Apprentices. It will be empirically tested in this paper:

H1: Subsidies granted through the American Apprenticeship Initiative have increased the number of Registered Apprentices.

3.4. Heterogeneity in the Effect of Subsidies

I now turn to the moderating effect of credit constraints on the impact of subsidies on training. Credit constraints impede training (Stevens, 1999). Consequently, subsidies to mitigate these credit market imperfections should be more effective at raising training in case of higher credit constraints (Stevens, 1999, Kuczera, 2017).

Popov (2014) shows that credit constraints reduce investment in on-the-job training. Georg and Strobl (2006) further show that subsidies are more effective at raising the training supply of credit constrained firms, relative to firms that do not face credit constraints. Brunello et al. (2020) find that firms facing financing constraints significantly reduces investment in training per employee. Perez-Orive (2016) notes that credit constrained firms prefer liquid, short-term investments. This is because such investments loosen current and future credit constraints, notably through the possession of higher-quality collateral. On the other hand, human capital has no collateral (Stevens, 1999). Human capital investments are not short-term. Benefits associated with higher human capital accrue on the longer-term.

Firm size is another proxy for credit constraint. Mueller and Behringer (2012) highlight that major disparities exist in training incidence between small and large firms because of credit constraints. Wang et al. (2022) confirm that small firms smaller firms are more adversely affected by credit constraints. Consequently, based on the existing literature, the following hypothesis concerning heterogeneity in the impact of the AAI on the number of Registered Apprentices emerges:

H2: Restricted access to borrowing to finance firms' expenses, i.e. credit constraints, exacerbate the effect of the American Apprenticeship Initiative.

4. Econometric Methods

4.1. Difference-in-Difference

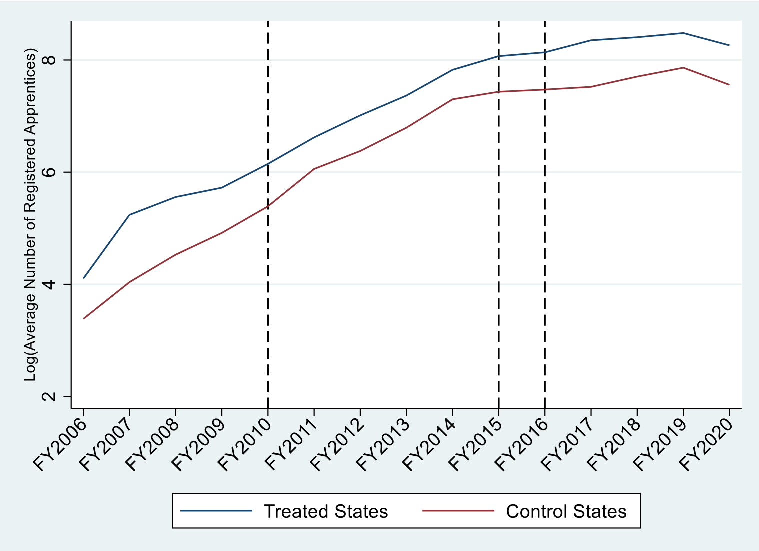
The introduction of the AAI in 24 states on October 1st, 2015, enables a difference-in-difference specification. States that are within the scope of the AAI are referred to as treated, while the remaining states are controls. For this difference-in-difference methodology, I use a balanced panel of 50 states added to Washington D.C. The sample period comprises fiscal years 2010 to 2016. This yields 357 observations.

The identification assumption of difference-in-difference is parallel trends. McConnell (2023) states that parallel trends may hold in levels (of the outcome), i.e. the number of Registered Apprentices, or in proportions, i.e. the natural logarithm of the number of Registered Apprentices. I assume the latter. Certain states are much larger than others in terms of population and number of Registered Apprentices, such as California. The evolution of their Registered Apprentice population over time, and other factors, may heavily differ due to e.g. selective migration. Parallel trends thus may not hold in levels. The number of Registered Apprentices, in absolute value, may substantially diverge over time. However, proportional differences in growth rates of the number of Registered Apprentices are much more likely to evolve in parallel. Here, the parallel trends considering proportions implies that the proportional difference in the growth rates in the number of Registered Apprentices would have remained parallel in the absence of the AAI. I choose to present parallel trends considering proportional differences in growth rates for the above reasons, in line with Finkelstein (2007).

Figure 4 displays trends in the outcome variable of difference-in-difference during the entire AAI treatment period, for descriptive purposes: October 1st, 2015, to September 30th, 2020. Nonetheless, the estimation sample ends on September 30th, 2016. Reasons for this are discussed in subsection 5.2. Similarly, in the pre-treatment period, Figure 4 demonstrates trends in the outcome variable of difference-in-difference estimation between October 1st, 2005, and September 30th, 2015. The estimation sample however only starts on October 1st, 2009. Trends in the outcome before this are shown for descriptive purposes. Parallel trends in the pre-treatment period hold for a longer period, reinforcing the internal validity of difference-in-difference (Angrist and Pischke, 2009).

Figure 4 shows parallel trends in the proportional difference in the growth rates in the number of Registered Apprentices. A slight inflexion in the growth rate of the number of Registered Apprentices in control states in fiscal year 2014 is however notable. On the other hand, the growth rate remains stable in treated states, other than an inflexion in fiscal year 2007 (which does not figure in the estimation sample).

Figure 4: Parallel Trends in the Natural Logarithm of Number of Registered Apprentices by State and Year

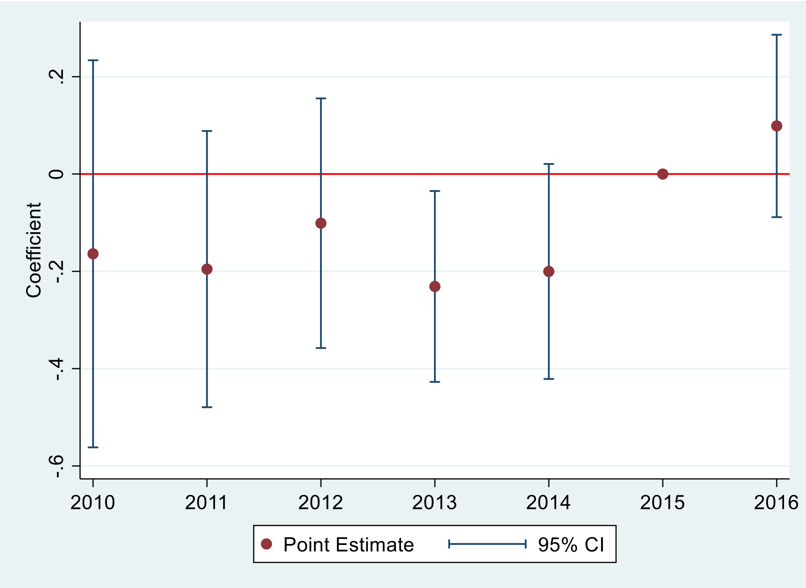


Notes: FY = Fiscal Year. This Figure depicts, by year, the proportional difference in the growth rate between the average number of Registered Apprentices. Because treatment was implemented on October 1st, the start of a fiscal year, the x-axis was adapted accordingly. Each value of the x-axis represents a United States fiscal year, i.e. from October 1st to September 30th. For example, the value 2010 indicates the 2010 fiscal year, i.e. October 1st, 2009, to September 30th, 2010. The first vertical dashed line marks the start of the estimation sample, on October 1st, 2009. The second vertical dotted black line marks the introduction of the AAI on the October 1st, 2015. The third vertical dotted line marks the end of the estimation sample, i.e. September 30th, 2016. The entire AAI treatment period lasted from October 1st, 2015, to September 30th, 2020. The average number of Registered Apprentices by state and year is 1,426, with a standard deviation of 2,318.94.

Figure 5 is an event study. It plots the interaction, for each year in the sample, between a fiscal year dummy and a dummy for treated state. Coefficients are relative to the last year preceding treatment.

The magnitude of coefficients pre-treatment is relatively stable across time, pre-treatment. All are negative and insignificant. Nonetheless, in the 2013 fiscal year, an interaction is negative and statistically significant at the 5% level. In the 2014 fiscal year, the interaction is negative and close to significant at the 5% level. Two additional F-tests are conducted. The first F-test fails to reject the hypothesis that all coefficients depicted in Figure 5 are jointly equal to 0. The second F-test fails to reject the null hypothesis that all coefficients depicted in Figure 5 are jointly equal to 0.

Figure 5: Difference-in-Difference Event Study



Notes: The outcome is the logarithm of the number of Registered Apprentices by state and year. Treated and control states are shown in Figure 2. Coefficients are relative to the last year preceding treatment. Treatment commences on October 1st, 2015. The interaction, for each year in the sample, between a fiscal year dummy and a dummy for treated state is plotted, along with its 95% confidence intervals. Standard errors are clustered by state.

While the pre-treatment coefficients are jointly statistically insignificant, the event study may have committed a type 2 error and lack the power to detect a significant divergence in trends (Roth, 2022). I address this limitation in four ways. First, I add state and time-varying covariates in equation (1). I aim to address any divergence in trends between treated and control states caused by these variables. Second, I conduct triple difference estimation, discussed in subsection 4.2. Third, I also execute synthetic difference-in-difference methodology (Arkhangelsky et al., 2021). This methodology partially addresses the issue underlined by Roth (2022). While Figure 5 solely considers past data to verify parallel trends, synthetic difference-in-difference methodology uses this past data to reweight control units and pre-treatment time periods. This weighting procedure makes pre-treatment trends in the outcome parallel between treated and control. Fourth, I execute equation (1) after propensity score matching. I match on pre-treatment outcomes in addition to covariates. This may reinforce the validity of the parallel trends assumption and reduce estimation bias. The hypothesis is that the amount of unobserved heterogeneity captured exceeds a regression to the mean effect in the final difference-in-difference estimation (Ham and Miratrix, 2024).

Equation (1) displays the difference-in-difference equation. Difference-in-difference relies on the following equation:

$$\text{Log}(NumberApprentices_{sy}) = \alpha_s + \delta_y + \gamma TreatedState_s * Post_y + X'_{sy}\varphi + \varepsilon_{sy} \quad (1)$$

α_s are state fixed effects. δ_y are year fixed effects. $100 * (e^{\gamma} - 1)$ denotes, in percentage, the proportional difference in growth rates between treated and control states during the AAI treatment period (McConnell, 2023). Treated states are shown in Figure 2. $Post_y$ is a dummy equal to 1 for the period October 1st, 2015, to September 30th, 2016, 0 otherwise. ε_{sy} is the state-by-year error term. $NumberApprentices_{sy}$ denotes the number of Registered Apprentices by state and year. Nevertheless, I do not solely rely on difference-in-difference estimation. Furthermore, there exist state-by-time varying confounders may bias the difference-in-difference estimator. Triple difference methodology may eliminate these confounders. I expand on triple difference estimation in the next subsection. X_{sy} is a vector containing two covariates, sourced from the Bureau of Economic Analysis (2024). They are the natural logarithm of a state's population by year, and the natural logarithm of annual personal income, by state and year (in USD millions)[2].

4.2. Triple Difference

The AAI has a third source of variation: industry. AAI guidelines state that grantees should target industries in which employers are most reliant on foreign labour: advanced manufacturing (NAICS code 33), healthcare and social assistance (NAICS code 62), and information technology (NAICS code 51) (United States Department of Labor, 2015). These industries are treated. The remaining two-digit NAICS industries are control. This third source of treatment variation allows taking a third difference, resulting in a triple difference estimation. Olden and Moen (2022) state the triple difference estimator consists of the difference of two difference-in-differences. Here, the first difference-in-difference is conducted within treated and control states respectively, between treated and control industries. The second difference-in-difference is conducted between treated and control states. The second difference is thus the difference in the difference-in-difference estimates obtained in the first step. This result of the difference in these two difference-in-differences is the triple difference estimate.

A triple difference methodology may be preferable to difference-in-difference analysis for three reasons. First, it eliminates state-time, state-industry, and industry-time varying confounders (Paik et al., 2016, Berck and Villas-Boas, 2016). Second, Berck and Villas-Boas (2016) also highlight that triple difference estimation yields lower bias than difference-in-difference estimation in the presence of a confounder. This is the case if the effect of the confounder must be large, and treated and untreated industries must have a similar response to state-industry-year-varying confounders. Third, the triple difference estimator has a lower type 1 and type 2 error risk (Olden and Moen, 2022).

Triple difference methodology makes the identifying assumption of *relative* parallel trends (Olden and Moen, 2022). This assumption implies that the proportional growth rate of the difference in the number of Registered Apprentices between treated and non-treated industries *within* treated and control states *respectively* would have continued to move in parallel in the absence of treatment.

The triple difference specification is presented in equation (2). The dependent variable is the natural logarithm of the number of Registered Apprentices in state s, industry i, in year y. δ_{sy} are state-by-year fixed effects. θ_{si} are state-by-industry fixed effects. φ_{yi} are year-by-industry fixed effects. Before taking

[2] Their descriptive statistics are available upon request. They were omitted for brevity.

the logarithm of $NumberApprentices_{syi}$, I replace all zero values in the dependent variable with one. The vector of covariates X_{sy} included in equation (1) is subsumed by state-by-year fixed effects[3].

$$Log(NumberApprentices_{syi}) = \gamma_0 + \gamma_1 TreatedIndustry_i * TreatedState_s * Post_y + \delta_{sy} + \varphi_{yi} + \theta_{si} + \varepsilon_{syi} \qquad (2)$$

The coefficient of interest in equation (2) is the triple difference coefficient γ_1. $100 * (e^{\gamma_1} - 1)$ measures, in percentage, the growth rate in the number of Registered Apprentices in treated states, treated industries and during the treatment period induced by the AAI. The effect is therefore in proportional terms. The fixed effects in equation (2) absorb all difference-in-differences, and therefore all non-parallel confounding trends in these difference-in-differences (Paik et al., 2016). ε_{syi} is the disturbance term.

In line with Paik et al. (2016), I cluster standard errors by state. I also test other standard error estimation methods for robustness. I first pursue the two-way clustering method of Cameron et al. (2011), clustering by state and industry. Subsequently, I estimate standard errors using one-way clustering by state-year cells. Inference remains qualitatively aligned. Estimations using the latter standard error calculation methods are producible upon request.

I now turn to the *relative* parallel trends assumption required in triple difference estimation. Figure 4 is the analogue of a "parallel trends graph" for the triple difference setting. It depicts, by year, within control and treated states respectively, the *proportional* difference between the number of Registered Apprentices in AAI-targeted and non-AAI-targeted industries (Olden and Moen, 2022). The graph was constructed in the following manner. The number of Registered Apprentices is averaged in groups according to the year, treated state status, and treated industry status:

$$\overline{NumberApprentices}_{TS_s,Year,TI_i} = \frac{1}{N_{States\ in\ TS_s}} \sum_{TS_s} \frac{1}{N_{Industries\ in\ TI_i}} \sum_{TI_i} NumberApprentices_{syi}$$

TS_s refers to state s' treatment status (binary, i.e. treated or control). $TS_s = 1$ if state s is treated, 0 else. Notation is analogous for industry treatment status. TI_i refers to industry i's treatment status. $TI_i = 1$ if industry i is treated, 0 else. $N_{States\ in\ TS_s}$ is the number of states in state treatment status s (i.e. treated or control). $N_{Industries\ in\ TI_i}$ is the number of industries in industry treatment status i (i.e. treated or control). This results in 28 different values: all year-treated-state-status combinations. This occurs because there are two statuses for both states and industries: treated or untreated. Furthermore, there are seven fiscal year in our data: fiscal year 2010 to fiscal year 2016. In the United States, fiscal years run from October 1st in one year, to September 30th in the next calendar year. I then take the logarithm of these 28 values.

The resulting average, shown above, was thus logged before being then split in two, by industry treatment status:

$Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=1})$ and $Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=0})$

I form coarser groups of observations, by year and treated state status. This yields 14 values. I subtract $Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=0})$ from $Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=1})$:

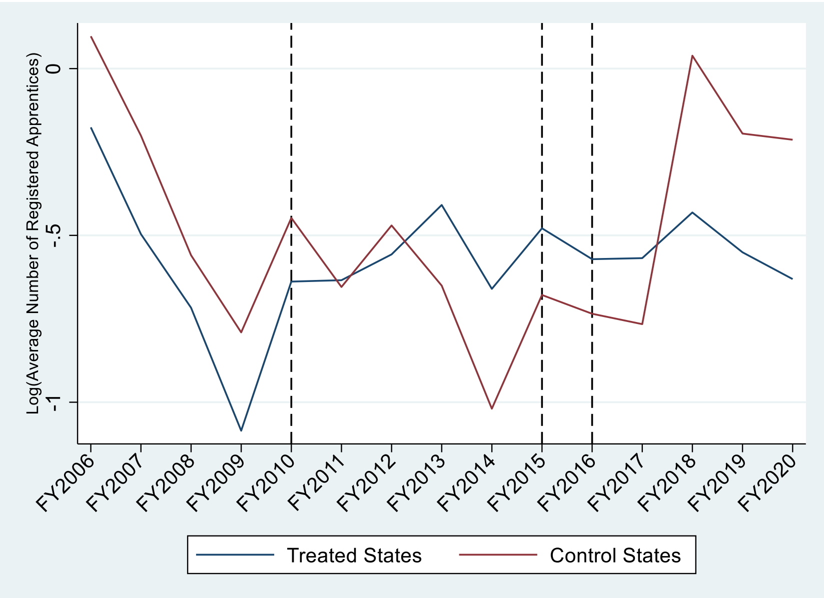
[3] Employing Poisson pseudo maximum likelihood for equation (2) yields qualitatively aligned results. Results are producible upon request. They were omitted for brevity.

$$\overline{NumberApprentices}_{TS_s=1,Year} = \sum_{TI_i=1} Log\left(\overline{NumberApprentices}_{TS_s=1,Year,TI_i=1}\right) - \sum_{TI_i=0} Log\left(\overline{NumberApprentices}_{TS_s=1,Year,TI_i=0}\right)$$

$$\overline{NumberApprentices}_{TS_s=0,Year} = \sum_{TI_i=1} Log\left(\overline{NumberApprentices}_{TS_s=0,Year,TI_i=1}\right) - \sum_{TI_i=0} Log\left(\overline{NumberApprentices}_{TS_s=0,Year,TI_i=0}\right)$$

These values are plotted in Figure 6. Figure 6 considers the same period as that shown in Figure 4. The estimation sample remains October 1st, 2009, to October 1st, 2016, in line with difference-in-difference estimation. Trends are parallel between treated and control states from fiscal year 2013 until the treatment introduction. Prior to this, from fiscal year 2006 to the start of the estimation sample (fiscal year 2010), trends are also parallel between treated and control states. Overall, Figure 6 demonstrates parallel trends between control and treated states hold in the pre-treatment period, apart from a divergence between fiscal years 2010 and fiscal year 2012, inclusive.

Figure 6: Parallel Trends in Triple Difference Estimation

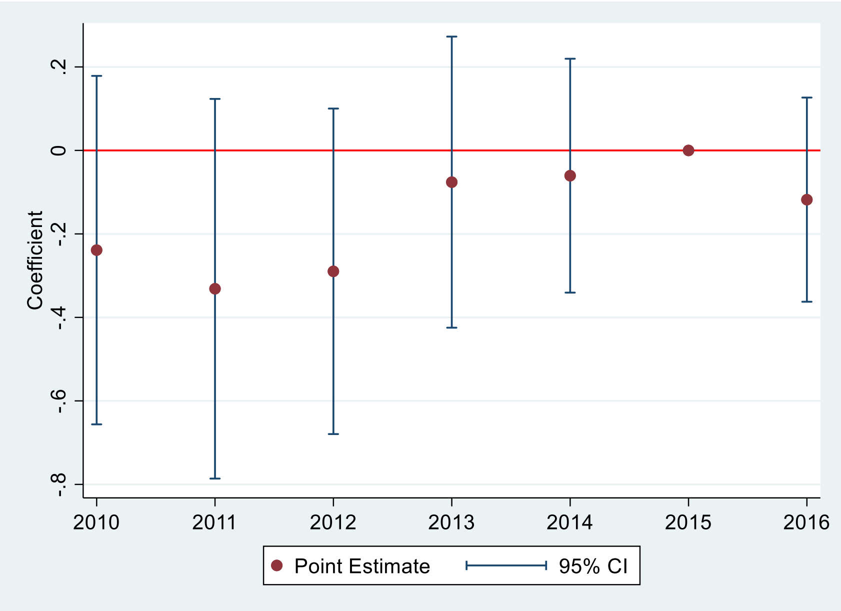


Notes: FY = Fiscal Year. This Figure depicts, by year, and within control and treated states respectively, the proportional difference in the growth rate between the average number of Registered Apprentices in AAI-targeted and non-AAI-targeted industries, respectively (McConnell, 2023). Because treatment was implemented on October 1st, the start of a fiscal year, the x-axis was adapted accordingly. Each value of the x-axis represents a United States fiscal year, i.e. from October 1st to September 30th. For example, the value 2010 indicates the 2010 fiscal year, i.e. October 1st, 2009, to September 30th, 2010. The first vertical dashed line marks the start of the estimation sample, on October 1st, 2009. The second vertical dashed black line marks the introduction of the AAI on the October 1st, 2015. The third vertical dashed line marks the end of the estimation sample, i.e. September 30th, 2016. The entire AAI treatment period lasted from October 1st, 2015, to September 30th, 2020.

I now conduct an event-study in equation (3), in line with Paik et al. (2016). This is a further test for the plausibility of parallel trends. The fiscal year before the AAI, year -1 (October 1st, 2014, to September 30th, 2015), is the comparison year. I thus set $\alpha_{-1} = 0$ in equation (3). All other α coefficients are to be interpreted relative to the period before treatment and relative to control group states. α_0 is therefore the triple difference coefficient for the year October 1st, 2015, to September 30th, 2016, relative to the last pre-treatment year October 1st, 2014, to September 30th, 2015. Equation (3) also serves as a parallel trends test. Namely, I test for the individual and joint significance of coefficients α_{-6} to α_{-2}. D_{si} is a binary variable, assuming the value of 1 if an observation in state s, industry, i, is treated, 0 else. μ_z are year dummies, for example μ_0 is a dummy indicating the year is October 1st, 2015, to September 30th, 2016. Coefficients α_{-6} to α_{-2} are individually and jointly statistically insignificant. I thus do not detect significant pre-trend divergence.

$$Log(NumberApprentices_{syi}) = \delta_{sy} + \varphi_{yi} + \theta_{si} + \sum_{z=-6,z\neq-1}^{-2} \alpha_z * TreatedIndustry_i * TreatedState_s * \mu_z + \alpha_0 * D_{si} * \mu_0 + \varepsilon_{syi} \quad (3)$$

Figure 7: Triple Difference Event Study



Notes: The figure shows coefficients of triple difference coefficient estimates from equation (3) that are to be interpreted relative to the year before the treatment (2015 on the x-axis). The y-axis is to be interpreted as the effect on the logarithm of the number of Registered Apprentices by state, year, and industry cell. 95% confidence intervals use standard errors clustered by state. Figure 2 shows treated states. Treated NAICS industries are Healthcare and Social Assistance (62), Information (51) and Advanced Manufacturing (33).

4.3. Difference-in-Discontinuity

I complement triple difference estimation with the alternate identification strategy of difference-in-discontinuity estimations (Butts, 2023, Wang et al., 2023, Garg and Shenoy, 2021). Thereby, I compare counties that are neighbouring each other but that differ in treatment status because they belong to different states. The identification assumptions of difference-in-discontinuity are two-fold (Grembi et al., 2016). First, the conditional expectation function of the counterfactual outcome must be continuous at the threshold. This guarantees that any discontinuity at the threshold is imputable to the causal effect of the treatment. Second, the effect of any confounder must be fully observed in the pre-treatment period. In other words, sorting cannot occur on a time-varying basis, between the pre-treatment and treatment periods. It cannot be correlated with actual treatment (Butts, 2023). The effect of this confounder can then be fully differenced out in difference-in-discontinuity estimation.

Bennedsen et al. (2022) liken this second assumption to parallel trends. Within a narrow bandwidth about the threshold, the trends of the conditional expectation function of the outcome for treated and control groups would have remained parallel between pre and post periods, had treatment not been implemented. This entails that sorting cannot be correlated to treatment, nor can it occur between pre and post treatment periods. On the other hand, in triple difference estimation, the identification assumption states that the trends of the conditional expectation function of the outcome for treated and control groups – conditional on state-by-year, industry-by-year, and state-by-industry fixed effects, would have remained parallel between pre and post periods, had treatment not been implemented.

Because in difference-in-discontinuity estimation, the parallel trends assumption only applies to observations within the threshold, the parallel trends assumption is weaker than in the triple difference estimation. The parallel trends assumption in triple difference applies to all observations, not restricted within a bandwidth (Bennedsen et al., 2022, Picchetti et al., 2024). In this context, the optimal mean-square error minimising bandwidth includes counties whose centroids are within 120km of the state border in control states, and 117km in treated states.

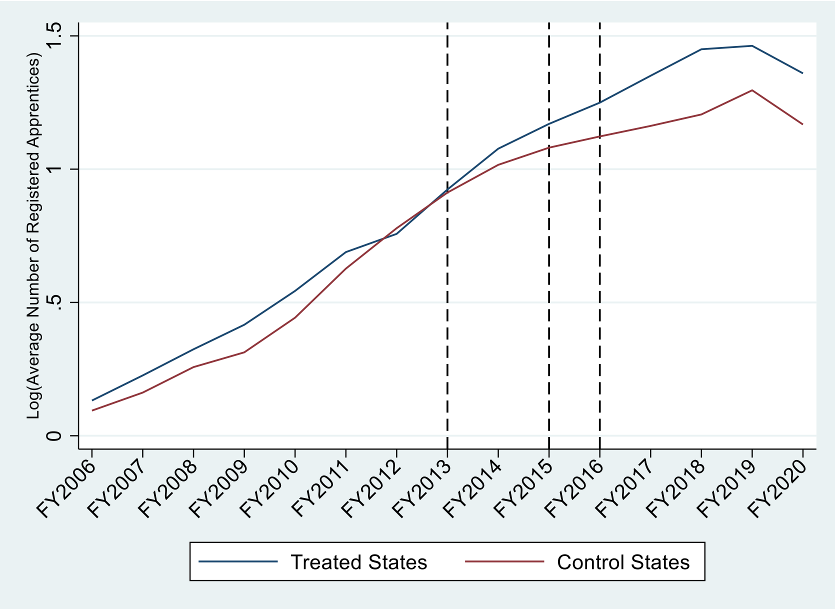
Finally, Grembi et al. (2016) show that difference-in-discontinuity eliminates the effect of confounding time-invariant treatments also turning on at the threshold. This is particularly important in spatial analysis (Keele and Titiunik, 2015). When crossing a state border, laws, fiscal rules, employment legislation and training incentives may change. Their change could cause a discontinuity at the threshold in the conditional expectation function of the outcome not due to the treatment. Difference-in-discontinuity thus eliminates the effects of such time-invariant confounding treatments.

Figure 8 shows, within this bandwidth, the evolution of the logarithm of the number of Registered Apprentices by county-year, averaged by year and states' treatment status[4]. In line with McConnell (2023), this corresponds to the evolution of the proportional rate of change in the outcome variable between treated and control counties (i.e. counties located in treated and control states, respectively) over time. Figure 8 considers the period October 1st, 2005, to September 30th, 2020. This is for descriptive and exposition purposes.

[4] The logarithm of the number of Registered Apprentices by county and year is not the dependent variable in difference-in-discontinuity estimation. The dependent variable in difference-in-discontinuity estimation refers to the difference between the average of the logarithm of the number of Registered Apprentices over the pre and post treatment periods. One cannot show trends over time in the latter variable, as it is county-specific and time-invariant. The construction of the difference-in-discontinuity dependent variable is discussed in this subsection.

The construction of the dependent variable in difference-in-discontinuity estimation involves an averaging procedure. Therefore, difference-in-discontinuity estimation excludes all observations prior to October 1st, 2013. This exclusion is thus to prevent the influence of pre-treatment confounders on the dependent variable. Thus, in the difference-in-discontinuity estimation sample, the pre-treatment period spans from October 1st, 2013, to September 30th, 2015, and the post-treatment period from October 1st, 2015, to September 30th, 2016. Examining Figure 8, one may notice that the parallel trends assumption largely holds for a long period before the introduction of the AAI. There is nonetheless a slight divergence in fiscal year 2012. This lends credence to the validity of the identifying assumption (Bennedsen et al., 2022).

Figure 8: Parallel Trends within Optimal Bandwidth



Notes: FY = Fiscal Year. This Figure depicts, by year, the proportional difference in the growth rate between the average number of Registered Apprentices in treated and control counties. This graph only considers counties located within the optimal mean-squared-error minimising bandwidth, calculated using Calonico et al. (2017). The optimal bandwidth is 120km in control states, and 117km in treated states. Because treatment was implemented on October 1st, the start of a fiscal year, the x-axis was adapted accordingly. Each value of the x-axis represents a United States fiscal year, i.e. from October 1st to September 30th. For example, the value 2010 indicates the 2010 fiscal year, i.e. October 1st, 2009, to September 30th, 2010. The first vertical dashed line marks the start of the estimation sample of difference-in-discontinuity and regression discontinuity design, on October 1st, 2013. The second vertical dashed black line marks the introduction of the AAI on the October 1st, 2015. The third vertical dashed line marks the end of the estimation sample, i.e. September 30th, 2016. The AAI treatment period lasted from October 1st, 2015, to September 30th, 2020. In difference-in-discontinuity estimation, I only consider fiscal years 2014 to 2016. In this figure, I extend the pre-treatment period to show longer trends, for the purpose of exposition.

To perform difference-in-discontinuity estimation, I collapse the original Registered Apprentice-level dataset to a county-year level. This results in the number of Registered Apprentices in a given county, each year. I discard all observations before October 1st, 2013, in the spatial difference in discontinuity. This avoids picking up, through an averaging procedure, pre-treatment confounding factors which could affect the results. October 1st, 2013, to September 30th, 2015, is thus the pre-treatment period. October 1st, 2015, to September 30th, 2016, is the post-treatment period.

For each county, I first take the natural logarithm of the number of Registered Apprentices. I replace zeros in the latter variable with 1 before taking the logarithm. I then create a variable equal to the natural logarithm of the number of Registered Apprentices, by county, only within the post-treatment period. Subsequently, within counties, I average the logarithm of the number of Registered Apprentices over the two pre-treatment periods[5]. Finally, I subtract the latter from the former. This procedure results in the following variable: $\Delta\overline{\text{NumberApprentices}}_c$. If $\Delta\overline{\text{NumberApprentices}}_c$ is positive (negative), the number of Registered Apprentices by county in the post-treatment period is superior (inferior) to the geometric mean of the number of Registered Apprentices by county in the two pre-treatment periods considered. It thus measures proportional growth rate in the number of Registered Apprentices by county. Exponentiating this variable yields a ratio interpretation. This ratio indicates how many times larger (or smaller) the number of Registered Apprentices by county was in the post-treatment period relative to the pre-treatment geometric mean.

I then use regression discontinuity methodology outlined in Calonico et al. (2017) on the outcome, $\Delta\overline{\text{NumberApprentices}}_c$. This follows Garg and Shenoy (2021) and Wang et al. (2023). In the preferred difference-in-discontinuity specification, I include all covariates listed in Appendix Table A3, in addition to state-pair fixed effects. Following the methodology of Calonico et al. (2017), the estimate is derived as:

$$\hat{\tau}(h) = e_0'\hat{\beta}_+(h) - e_0'\hat{\beta}_-(h) \tag{4}$$

$\hat{\tau}(h)$ assesses whether there is a discontinuity in the $\Delta\overline{\text{NumberApprentices}}_c$ at the threshold distance of 0 (Wang et al., 2023). h denotes the optimal bandwidth. It is calculated to minimise mean square error (Calonico et al., 2017). Now consider equations (5) to (8). $1(\cdot)$ is the indicator function. Standard errors are calculated using heteroscedasticity consistent “HC3” weights. Because distance is recoded to be negative in control states, $1(X_c \geq 0)$ is an indicator assuming the value of one if county c is located within a treated state, and zero otherwise. State-pair fixed effects, $\tau_{s,s'}$, $\forall s \neq s'$, are included in all specifications.

In line with Gelman and Imbens (2019), I specify a local linear estimator. Z_c' are county-level covariates, listed in Appendix Table A3. Furthermore, in line with (Keele and Titiunik, 2015), Z_c' also comprises flexible controls of counties’ respective latitudes and longitudes. Specifically, it comprises a county’s latitude, longitude, an interaction between latitude and longitude, the squared value of latitude, and the squared value of longitude.

Each state-pair includes counties located within two distinct states. They are defined by counties located in a state of origin, and the destination state. The destination state is the state towards which the distance is calculated. State-pair fixed effects assume the value of 1 for a specific combination of two states (one state of origin and one destination state), and zero otherwise. I use state-pairs to eliminate all combinations of states that do not include exactly one control state, together with one treated state. For instance, I do not use distances from counties in California (treated state), to the border with Oregon, another treated state. Analogously, I remove from the dataset distances from counties in Utah, a control state, to e.g. Oklahoma, another control state. 51 state-pairs figure in the optimal bandwidth. One must note that within these state-pairs, pairs such as California-Arizona *and* Arizona-California are considered as one unique pair. 1,471 distinct counties figure in the optimal bandwidth of the preferred difference-

[5] This arithmetic mean is equivalent to the logarithm of the geometric mean of the number of Registered Apprentices in the pre-treatment period. The average of a logarithm is inferior or equal to the logarithm of the average of the corresponding value by Jensen’s inequality. When I construct the variable by taking the logarithm of the average instead of the average (arithmetic mean) of logarithms, I obtain qualitatively aligned results, producible upon request.

in-discontinuity specification. This equates to the effective number of observations employed in the preferred difference-in-discontinuity specification.

$\hat{\beta}_{+}(h)$ and $\hat{\beta}_{-}(h)$ are calculated in equations (5) and (6) respectively (following Pichetti et al., 2024). The "+" subscript highlights focus on the bandwidth above the threshold. The "-" subscript highlights focus on the bandwidth below the threshold. e_0' is the row vector containing the intercept as first element, 0 as other elements. I use a triangular kernel, linearly down-weighting observations further away from the threshold. I employ $K_h(X_c - \bar{x})$. X_c denotes the running variable, distance from county centroid to the nearest state border with a different treatment status. Distance is negative for counties located in control states. $\bar{x}$ is the threshold distance, i.e. 0 kilometres.

$$\hat{\beta}_{+}(h) = argmin_{\beta} \sum_{c=1}^{n} \left(\Delta\overline{\text{NumberApprentices}}_c - \tau_{s,s'} - Z_c'\gamma - 1(X_c \geq 0)(1\ X_c)(X_c - \bar{x})'\beta\right)^2 K_h(X_c - \bar{x}) \quad (5)$$

$$\hat{\beta}_{-}(h) = argmin_{\beta} \sum_{c=1}^{n} \left(\Delta\overline{\text{NumberApprentices}}_c - \tau_{s,s'} - Z_c'\gamma - 1(X_c < 0)(1\ X_c)(X_c - \bar{x})'\beta\right)^2 K_h(X_c - \bar{x}) \quad (6)$$

The corresponding difference-in-discontinuity regression is as follows:

$$\Delta\overline{\text{NumberApprentices}}_c = \alpha + \pi * 1(X_c \geq 0) + \delta * X_c + \theta * X_c * 1(X_c \geq 0) + Z_c'\gamma + \tau_{s,s'} + \varepsilon_c \quad (7)$$

I also execute the following regression discontinuity design estimation:

$$\text{Log}(\text{NumberApprentices}_{\text{c,post}}) = \alpha + \pi * 1(X_c \geq 0) + \delta * X_c + \theta * X_c * 1(X_c \geq 0) + Z_c'\gamma + \tau_{s,s'} + \varepsilon_c \quad (8)$$

Where $\text{Log}(\text{NumberApprentices}_{\text{c,post}})$ is the natural logarithm of the number of Registered Apprentices in county c in the post-treatment period, October 1st, 2015, to September 30th, 2016. π is the coefficient of interest. It denotes, under identification assumptions, the local average treatment effect of the AAI, within the respective optimal bandwidth about the border.

I conduct tests for manipulation, smoothness of observable covariates through the threshold, and continuity in the density of the running variable of the preferred difference-in-discontinuity specification. Appendix Figure B1 displays the manipulation test in the estimation sample of the preferred spatial difference-in-discontinuity specification (Cattaneo et al., 2018). This figure serves to test the null hypothesis of no manipulation of the running variable. The test aims to detect bunching in the dependent variable. The attached p-value of this test is 0.25. I fail to reject the null hypothesis of no manipulation of the dependent variable.

A corollary of the identifying assumption in regression discontinuity is that unobservable variables, in addition to the running variable and observable variables, are continuous at the threshold. This allows the interpretation of any jump in the outcome variable at the threshold as the treatment effect. I cannot directly test this corollary. However, I can test whether there is a discontinuous jump in observed covariates at the border (Keele and Titiunik, 2015). Appendix Table A4 serves this purpose. I obtain the estimate (4) sequentially using each covariate as a dependent variable. All regressions are run within the optimal bandwidth used in baseline specifications. If a statistically significant estimate is obtained, then the covariate is discontinuous at the threshold. This may be cause for concern. No estimate shown in Table A4 is statistically significant at any conventional level. Observable characteristics are smooth through the threshold.

4.4. Difference-in-Difference-in-Discontinuity: Industry Variation

In baseline difference-in-discontinuity estimation, I ignore industry variation in treatment, and solely leverage geographic variation. I now conduct “difference-in-difference-in-discontinuity” estimation, considering the industrial treatment facet of the AAI. Industries are again defined as two-digit NAICS industries. To this end, I recreate the dependent variable used in difference-in-discontinuity estimation, $\Delta\overline{\text{NumberApprentices}}_c$, to be county-industry-treatment specific (treated and control industries), instead of only county-specific: $\Delta\overline{\text{NumberApprentices}}_{c,TI}$.

While the dataset used for baseline difference-in-discontinuity estimation is county-year specific, I now create a county-year-industry-treatment-status-specific dataset. I then average the logarithm of the number of Registered Apprentices over time, within county-industry-treatment-status cells for the post-treatment and pre-treatment periods, respectively. Finally, within county-industry cells, I subtract the average of the logarithm of the number of Registered Apprentices in the pre-treatment period from that in the post-treatment period. This yields the new dependent variable $\Delta\overline{\text{NumberApprentices}}_{c,TI}$.

Equation (9) investigates whether, within a bandwidth around the border, the treatment effect of the AAI is significantly stronger in counties located in treated states, in treated industries (healthcare, information, and advanced manufacturing). This equation employs a triangular kernel and is executed within the same bandwidth as baseline difference-in-discontinuity: 120km in control states and 117km in treated states. The dependent variable, as well as distance X_c, are both winsorized (top and bottom 1%). In equation (9), standard errors are calculated using “HC3” heteroscedasticity-consistent weights.

$$\Delta\overline{\text{NumberApprentices}}_{c,TI} = \mu_0 + \mu_1 X_c + \mu_2 1(X_c \geq 0) + \mu_3 1(X_c \geq 0) * X_c + \mu_4 X_c * TreatedIndustry_i + \mu_5 TreatedIndustry_i * 1(X_c \geq 0) + \mu_6 TreatedIndustry_i * 1(X_c \geq 0) + \tau_{s,s'} + Z_c' \mu_7 + \varepsilon_{c,TI} \quad (9)$$

μ_6 is the coefficient of interest. If it is positive and significant, the AAI has a significantly stronger and more positive impact on treated industries located in treated counties. Equation (9) nonetheless has a shortcoming. Because distance is county-specific, but the dependent variable is county-industry specific, mass points in the running variable, distance, may occur. This may impede the performance of the estimator (Calonico et al., 2017). For this reason, I also resort to sample split regressions to investigate industrial variation in the treatment effect of the AAI.

5. Data

5.1. Data Source

I employ the administrative dataset "Registered Apprenticeship Partners Information Database System" (RAPIDS) from the United States Department of Labor. This dataset covers all Registered Apprentices in the United States (ApprenticeshipUSA, 2024). It is indeed compulsory for all training establishments with Registered Apprentices in the United States to track and report Registered Apprentices data at the individual level to the Department of Labor's Office of Apprenticeship. However, not all states use RAPIDS. States are either under the purview of the Department of Labor's Office of Apprenticeship or of their state-level State Apprenticeship Agencies (see United States Department of Labor, 2024a, for a map depicting the purview of the Department of Labor's Office of Apprenticeship and State Apprenticeship Agencies as at 2024).

All states under the purview of the Department of Labor's Office of Apprenticeship employ the RAPIDS system. On the other hand, states under the purview of State Apprenticeship Agencies are free to employ their own proprietary database for data. They must still submit individual-level data to the Department of Labor's Office of Apprenticeship to be integrated in the RAPIDS database. Employers located in states using their own proprietary database do not collect information on NAICS industry, nor occupation. These states are therefore excluded from triple difference estimation. These states are Connecticut, New York, Washington, Oregon, Massachusetts, Wisconsin. Washington D.C, not a state but a district, also does not collect information regarding Registered Apprentices' industry. 44 states are included in triple difference estimation.

I define the year of observation as the year of the Registered Apprentice's start date. RAPIDS further entails information on the state, industry, and county of each registered apprentice's programme. I remove from the estimation dataset all Registered Apprentices part of national programmes, and the United States Military Apprenticeship Programme. The principal reason is that no geographical information (neither state nor county) is attached to these programmes. Furthermore, in this case, the employer is mainly the United States Department of Defence, which would serve as a bad counterfactual for private employers.

5.2. Estimation Sample: State-Year-Industry Level Dataset – Triple Difference

The object of interest is the number of Registered Apprentices in a firm. However, firms are not observed in the dataset. I therefore collapse the Registered Apprentice-specific dataset to state-year-industry specific cells to create a "pseudo-panel" dataset (Guillerm, 2017). There are 44 states, seven years, and 24 two-digit NAICS industries. There are 7,392 distinct state-year-industry cells, which uniquely define observations. In baseline estimations, I assign a value of 0 for the number of Registered Apprentices in empty cells, i.e. state-year-industry cells missing from the RAPIDS dataset. The rationale for doing this is that a firm (proxied by state-year-industry cells in this dataset) that does not train Registered Apprentices *in fact trains 0 Registered Apprentices*. Finally, in all triple difference estimations, before taking the logarithm of the number of Registered Apprentices per state-year-industry cell, I replace zeros by one.

The pseudo-panel has 58 observations per cell; there are on average 58 Registered Apprentices per state, year and two-digit NAICS industry cell. This value falls short of the optimal number of observations per cell to minimise intra-cell measurement error, which is around 100 observations (Verbeek and Nijman, 1992). This may cause relatively large standard errors, as the speed of convergence of the average of the residuals in the pseudo-panel estimator to 0 depends on the number of observations within cells (Antman and McKenzie, 2007).

24 states, in addition to Washington D.C., are treated. They are within the performance scope of AAI grantees. 17 states only are in the triple difference estimation dataset however, due to the abovementioned missing industry data. 26 other states are referred to as the control states. To define treated industries, I use the North American Industry Classification System (NAICS) of 2012. Information and Healthcare industries are defined as those industries commencing with an NAICS code of 51 and 62 respectively. I define Advanced Manufacturing as NAICS 33 (Muro et al., 2015, Conexus Indiana, 2016).

Table 1 displays descriptive statistics of the dependent variable in level form. The dependent variable in the estimation sample is the natural logarithm of the number of Registered Apprentices in state s, year y and industry i. There are on average 58 Registered Apprentices in each state s, year y and NAICS two-digit industry i. The number of Registered Apprentices per state-year-industry cell displays great variability in the estimation sample, with a standard deviation of 370. Treated states are marginally but significantly less credit constrained and have a lower proportion of firms with less than twenty employees, i.e. a lower fraction of smaller firms.

Triple difference estimations include all Registered Apprentices whose start dates are between October 1st,2009 to September 30th, 2016. In all empirical estimations, year is derived from the year of start of a Registered Apprentice. Funding information was retrieved from USA Spending (2015). The complete AAI performance period lasted from October 1st, 2015, to September 30th, 2020, inclusive (United States Department of Labor, 2015). However, as baseline estimation, I consider October 1st, 2015, until September 30th, 2016, as treatment period. Funds from the State Apprenticeship Expansion programme, a subsequent federal programme subsidising Registered Apprenticeships, were distributed in November 2016 (United States Department of Labor, 2016). From the end of 2016 onwards, many successive and overlapping confounding subsidisation programmes occurred, such as the Apprenticeship State Expansion grant in 2019 for example (United States Department of Labor, 2019). I am however solely interested in the causal effect of the American Apprenticeship initiative. Therefore, I follow Bertrand et al. (2004) and restrict the analysis window around the treatment period to avoid picking up effects of confounding policies or events. In a robustness check, I extend the period of AAI treatment under consideration to verify that results are qualitatively aligned with those obtained in the baseline specification, which is the case.

Table 1: Descriptive Statistics in Triple-Difference Estimation Sample

Variable Name	Variable Explanation	Mean (Std. Dev. in parentheses)	Min.	Max.
Dependent Variable				
NumberApprentices	Number of Registered Apprentices by state, year and two-digit NAICS industry code	58.3 (370)	0	13,501
Heterogeneity Variables				
Percentage of Firms with Less Than 20 Employees	State-level average percentage of firms with less than 20 employees in 2013	85.5 (1.84)	80.2	90.99
Credit Constraint Index	State-level average credit constraint index in 2012	26.7 (4.42)	18.2	35.6

Notes: There are 7,392 observations in the triple-difference estimation sample. This total number of observations is composed of 24 distinct NAICS industries, 44 states, and seven years. NAICS = North American Industry Classification System. Data on firm size by state are sourced from the United States Census Bureau (2013). The 2012 credit constraint index is sourced from the Federal Reserve Bank of New York (Federal Reserve Bank of New York, 2021).

I use two proxies for the credit constraints of firms (see subsection 3.4). First, I use the credit constraint index of the Federal Reserve Bank of New York in 2012 (Federal Reserve Bank of New York, 2021). The credit constraint index combines the share of residents in each community without a credit file and those with credit-limiting factors, such as high credit utilization and low credit scores, into a single score. Higher scores indicate greater credit constraint. The index is then averaged over county and subsequently state level. Second, I proxy credit constraints by the percentage of firms that have less than 20 employees in 2013. Data on firm size are sourced from the United States Census Bureau (2013). I aggregate the credit constraint proxies to a state level in heterogeneity triple difference estimations, and to a county level in difference-in-discontinuity heterogeneity estimations. I then split the sample at the median value and execute estimations on both subsamples. Table 1 shows that within states, 85.5% of firms present in the states analysed had less than 20 employees in 2013. The median share of firms with less than 20 employees in 2013 is 85.5%. The median value of the credit constraint index in the estimation sample is 26.2.

5.3. Descriptive Statistics: Difference-in-Discontinuity Estimation Dataset

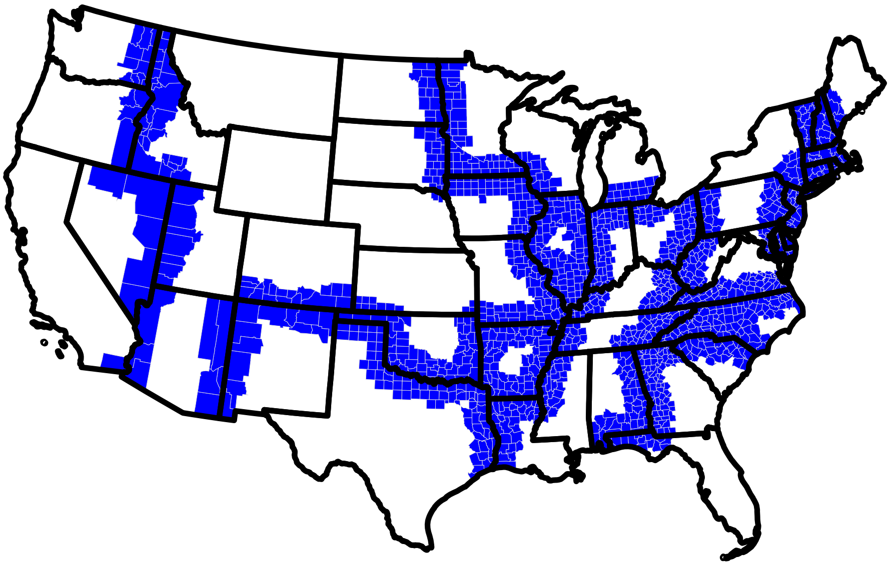
The difference-in-discontinuity estimation exploits variation in one-dimensional space (Keele and Titiunik, 2015) and time. Therefore, I no longer leverage industry variation and no longer eliminate the states that do not entail NAICS industry information. I now consider county-by-year level data. Counties are of course nested within states, which are either treated or control. I only consider continental United States. As I do for triple difference estimation, I assign a value of zero Registered Apprentices for county-year cells that are missing in the RAPIDS data. Again, the rationale is that counties missing from RAPIDS are counties in which zero Registered Apprentices are trained. Before taking the natural logarithm of the number of Registered Apprentices by county and year, I replace the value of zero with one.

In baseline estimations shown, I allow the bandwidth to differ on either side of the cutoff. In the preferred difference in discontinuity specification, the optimal bandwidth is 120km below the threshold, whilst it is 117km above the threshold. This excludes the state of Wyoming, as well as Alaska and Hawaii, which are not part of the continental United States. This bandwidth is calculated using Calonico et al. (2017). 1,471 counties are present in the optimal bandwidth. They are shown in the map depicted in Figure 9.

Distance, the running variable, is measured as the shortest distance between the centroid of a Registered Apprentice's county and the nearest state border of opposite treatment status as the crow flies. For a county located in a treated state, e.g. California, it is the shortest distance to the border with a control state. In this example, the distance will be from the county centroid located in California, to the border with Arizona, a control state. For a county located in a control state, it is the shortest distance to treatment. In line with Banerjee (2005), I use geodetic distance to account for the curvature of the Earth.

Originally, multiple distances are computed within each county. However, I only retain one distance per county. This is the shortest distance, within this county, to another state border of opposite AAI treatment status. For instance, Nevada is a treated state. It borders three control states: Idaho, Utah, and Arizona. Therefore, for each county in Nevada, three distances are originally calculated. Then, the smallest distance is retained. Consider White Pine County in Nevada. It is closer in distance, as the crow flies, to Utah than it is to Idaho or Arizona, the two other states bordering Nevada that are not treated by the AAI. Therefore, for White Pine County, only the distance to Utah is retained. As a rule, for each county located in a treated state, I retain only the shortest distance to control states, as discussed in the previous example. For each county located in a control state, I retain only the shortest distance to a treated state. Figure 9 shows the counties used in difference-in-discontinuity estimation that are within the optimal mean squared-error minimising bandwidth. Wyoming and Montana are the only states in the continental United States not included in the optimal bandwidth of the preferred difference-in-discontinuity specification. They are located too far away, in terms of geodetic distance, from treated states.

Figure 9: Counties Employed in Difference in Discontinuity within Optimal Bandwidth



Notes: Counties highlighted in blue are located within the optimal mean-square error minimising bandwidth: 120km in control states and 117km in treated states. The optimal mean-square error minimising bandwidth is computed using methodology of Calonico et al. (2017).

Table A3 shows descriptive statistics of variables employed in difference-in-discontinuity estimations. Table A3 also displays the covariates employed in regression discontinuity and difference-in-discontinuity estimation, as well as their descriptive statistics. Finally, Table A3 contains the descriptive statistics of variables serving to test hypothesis H2 of treatment effect heterogeneity according to credit constraint. All descriptive statistics are shown within the optimal bandwidth for the preferred difference-in-discontinuity specification: 120km in control states and 117km within treated states.

Variables $\Delta\overline{\text{NumberApprentices}}_c$, $Log(\text{NumberApprentices}_{c,post})$, and distance are winsorized (top 1% and bottom 1%). A value of 0.10 for the dependent variable in the difference-in-discontinuity estimation, $\Delta\overline{\text{NumberApprentices}}_c$, means that within county, the proportional change in the number of Registered Apprentices was positive between the pre- and post-treatment periods. The number of Registered Apprentices in the post-treatment period is 1.11 times larger than its geometric mean in the two pre-treatment periods considered. In "classical" regression discontinuity design, the dependent variable is the logarithm of the number of Registered Apprentices by county, in the post-treatment period. The average (arithmetic mean) value of this variable in level form is 32.09. This means that in the post-treatment period (October 1st, 2015 - September 30th, 2016), the number of Registered Apprentices within county was on average 32.09.

6. Results

This section serves to present estimation results. First, I present results from state-by-year difference-in-difference estimation. Second, I show triple difference results. Third, I show spatial regression discontinuity design and spatial difference-in-discontinuity results. Fourth, I show results from the heterogeneity analysis set forth in subsection 3.4.

6.1. Difference-in-Difference Results

This subsection presents results from difference-in-difference estimation, discussed in subsection 4.1. Table 2 displays the results. Column (3) shows results from equation (1). The difference-in-difference coefficient is positive. However, this coefficient is statistically insignificant at all conventional significance levels. I therefore fail to find that the AAI has affected the number of Registered Apprentices in treated states over the first year of the treatment period. This is after having addressed state-specific time-invariant unobserved heterogeneity, as well as the effects of time shocks. In columns (2) and (3), I also control for the natural logarithm of a state's population in a given year, and the natural logarithm of annual personal income, by state and year (in USD millions).

Table 2: Results from Difference-in-Difference Estimations

	(1)	(2)	(3)
Treated State * AAI Period	0.650*	0.304	0.208
	(0.369)	(0.212)	(0.148)
State FE	No	No	Yes
Year FE	Yes	Yes	Yes
Covariates	No	Yes	Yes

Notes: N = 357. *p<0.1, **p<0.05, ***p<0.01. The dependent variable is the natural logarithm of the number of Registered Apprentices in each state and year. The mean of the dependent variable, in levels, is 1,426.29. Treated states are mapped in Figure 2. The AAI period corresponds to the post-treatment period, which is October 1st, 2015, to September 30th, 2016. Covariates are the natural logarithm of a state's population in a given year, and the natural logarithm of annual personal income, by state and year (in USD millions).

Table 3 displays results of difference-in-difference robustness checks. Column (1) reiterates the baseline difference-in-difference estimate. Column (2) displays results of synthetic difference-in-difference, following Arkhangelsky et al. (2021). Column (3) displays results of equation (1), weighted with weights generated through propensity score matching. Propensity score matching is conducted on covariates specified in equation (1), and pre-treatment outcome variable values (Ham and Miratrix, 2024). The result shown in column (2) suggests the AAI has statistically insignificantly increased the number of Registered Apprentices by 7.47% in treated states during the treatment period. The result shown in column (3) suggests the AAI has statistically insignificantly increased the number of Registered Apprentices by 6.61% in treated states during the treatment period. These robustness checks lead to the same inference as do baseline results. The AAI has not led to statistically significant growth in the number of Registered Apprentices. The aim of these robustness checks was to reduce unobserved heterogeneity causing diverging trends in the outcome between treated and control groups. The magnitude of results derived from these robustness checks is substantially lower than baseline results.

Table 3: Difference-in-Difference Robustness Checks

	(1)	(2)	(3)
	Baseline Estimate	Synthetic Difference-in-Difference	Matching on Covariates and Pre-Treatment Outcome Variable Values
Treatment Period * Treated States	0.208	0.072	0.064
	(0.148)	(0.089)	(0.188)
Observations	357	357	252
Mean of Dependent Variable	1,426	1,426	1,433

Notes: *p<0.1, **p<0.05, ***p<0.01. The dependent variable is the natural logarithm of the number of Registered Apprentices in each state and year. Treated states are mapped in Figure 2. The AAI period corresponds to the post-treatment period, which is October 1st, 2015, to September 30th, 2016. Covariates are the natural logarithm of a state's population in a given year, and the natural logarithm of annual personal income, by state and year (in USD millions). Column (1) re-iterates the baseline difference-in-difference coefficient. Column (2) implements synthetic difference-in-difference methodology of Arkhangelsky et al. (2021), discussed in subsection 4.1. Column (3) weights equation (1) using weights derived from propensity score matching on covariates listed above, as well as on pre-treatment outcome values. The number of observations declines because certain are off common support and are thus not included in the estimation.

6.2. Triple Difference Results

I now turn to triple difference. This subsection considers results of equation (2). Table 4 displays results from triple difference estimation. Specifically, column (3) displays results from equation (2). Table 2 suggests that the AAI has not significantly affected the number of Registered Apprentices. In column (3), the preferred specification, the coefficient on the triple interaction term of interest "Treatment Period * Treated States * Treated NAICS Industry" is 0.048. This coefficient is statistically insignificant. Over the first treatment year, the AAI caused a statistically *insignificant* increase of $100(e^{0.048} - 1) = 4.92\%$ Registered Apprentices in each treated state-year-industry cell. This represents almost 3 Registered Apprentices per treated state-year-industry cell. The difference in magnitude and significance between the triple difference estimates shown in columns (1) and (3) respectively highlight the importance of state-by-year and industry-by-year fixed effects. The magnitude of the triple difference estimate in column (3) of Table 4 is substantially lower than that of column (3) in Table 2, which is a difference-in-difference estimate. This highlights that addressing state-by-year unobserved heterogeneity, as well as state-by-industry and industry-specific time-varying shocks diminishes the estimate of the treatment effect.

Table 4: Results from Triple Difference Estimations

	(1)	(2)	(3)
Post * Treated Industry * Treated State	0.302**	0.197*	0.048
	(0.124)	(0.104)	(0.158)
State-by-Year Fixed Effects	No	Yes	Yes
Industry-by-Year Fixed Effects	No	No	Yes

Notes: N = 7,392. In all columns the dependent variable is the logarithm of the number of Registered Apprentices by state, year, and industry cell. Values of zero in the level dependent variable were replaced with 1 before taking the logarithm. Standard errors are in parentheses clustered by state. All estimations contain state-by-industry and year fixed effects. ***p<0.01, **p<0.05, *p<0.1. This table shows the triple difference coefficient. Treatment period denotes the period October 1st, 2015, to September 30th, 2016. Figure 2 shows treated states. Treated NAICS industries are Healthcare and Social Assistance (62), Information (51), Advanced Manufacturing (33). The mean of the dependent variable, in level, is 58 Registered Apprentices by state, year and industry cell.

Table 5 contains a set of triple difference robustness tests. I first conduct a test for treatment anticipation effect (policy announcement effect). This is done by recoding treatment as starting not when funds were effectively disbursed by the United States Department of Labor, but as starting when the AAI was first announced (December 11th, 2014). The magnitude of the triple difference coefficient increases but remains statistically insignificant. I thus fail to detect a significant announcement effect.

In Table 5, I also extend the treatment period to the full AAI treatment period, so that the treatment period now is October 1st, 2015, to September 30th, 2020. The triple difference coefficient remains statistically insignificant. However, the magnitude of the negative point estimate decreases substantially. The semi-elasticity is very close to 0 (-0.6%). Even when considering the full AAI treatment period, I fail to find that the AAI has significantly affected the number of Registered Apprentices.

Table 5 column (4) also restricts the window of analysis to September 30th, 2014, to October 1st, 2016. While the magnitude of the coefficient grows and its sign reverses, the coefficient remains statistically insignificant. Now, going in the opposite direction, in column (5) I execute equation (2) on the estimation period October 1st, 2005, until September 30th, 2020. Results are again highly qualitatively aligned to baseline, although the triple difference coefficient's magnitude is substantially lower.

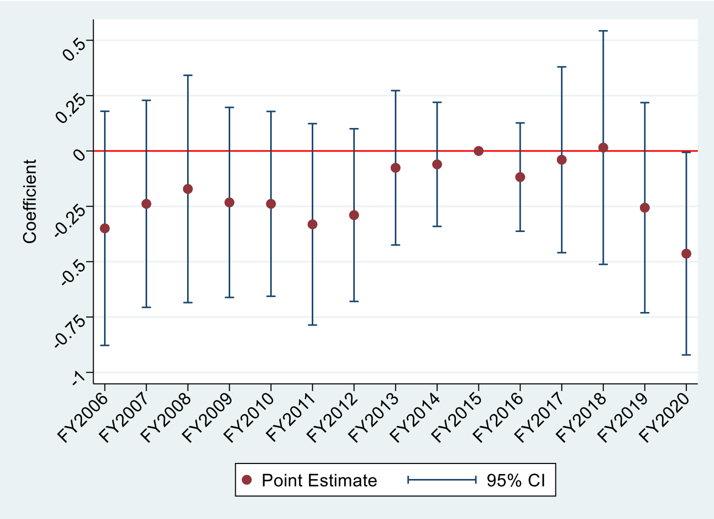
Table 5: Triple Difference Robustness Checks

	(1)	(2)	(3)	(4)	(5)
	Baseline Estimate	Including anticipation effect	Extending treatment window until Fiscal Year 2020	Only considering last year of pre-treatment period	Estimation Period Fiscal Year 2005 – Fiscal Year 2020
Treatment Period * Treated States * Treated Industry	0.048	0.159	-0.006	-0.118	0.026
	(0.158)	(0.162)	(0.147)	(0.121)	(0.163)
Observations	7,392	7,392	11,616	2,112	15,840
Mean of Dependent Variable	0.983	0.983	1.220	1.260	0.980

Notes: All estimations contain state-by-year, industry-by-year, and state-by-industry fixed effects. The dependent variable, in all specifications, is the natural logarithm of the number of Registered Apprentices in state s, year y and industry i. All columns show variants of triple difference equation (2). Column (1) reiterates the baseline triple difference treatment effect for references. In column (2) I conduct a test for treatment anticipation effect (here same as policy announcement effect). The treatment period is recoded to capture December 11th, 2014, to September 30th, 2016. The AAI was announced on the December 11th 2014 (United States Department of Labor, 2015). In column (3), I extend the treatment period to the full AAI treatment period, so that the treatment period now is October 1st, 2015, to September 30th, 2020. In column (4) I further restrict the window of analysis around the time of treatment. I now only consider fiscal years 2015 and 2016 (October 1st, 2014, to September 30th, 2015, and October 1st, 2015, to September 30th, 2016). In column (5), I use an extended estimation period: October 1st, 2005, to September 30th, 2020.

Figure 10 is an event study. It is identical to Figure 7, however it considers the entire period of October 1st, 2005, to September 30th, 2020. Figure 10 lends credence to the main finding, and corroborates results shown in Figure 7. The AAI has had a statistically insignificant effect on the number of Registered Apprentices in treated states and treated industries. In line with Figure 6, Figure 10 also shows that in triple difference estimation, parallel trends hold for a long period pre-treatment. Pre-treatment interaction terms plotted are individually and jointly statistically insignificant.

Figure 10: Event Study on Full American Apprenticeship Initiative Treatment Period



Notes: Triple difference coefficient estimates from equation (2), however modified to include the whole AAI treatment period (October 1st, 2015, to September 30th, 2020) are to be interpreted relative to the year before the treatment (2015 on the graph's x-axis). The y-axis is to be interpreted as the semi-elasticity of the number of Registered Apprentices by state-year-industry cell with respect to the AAI. Blue bands denote 95% confidence intervals. Standard errors are clustered by state, in line with Paik et al. (2016).

Table 6 contains an additional set of robustness checks pertaining to the triple difference estimation. All are variants of equation (2). In Table 6, I also consider occupational variation in the AAI treatment. I conduct this robustness check for two reasons. First, occupations located within the Computer and Mathematical occupation group, in the United States, have the lowest industry quotient and concentration index within industries (Watson, 2014). This signifies that these occupations are dispersed across industries, therefore not concentrated exclusively in the NAICS Information industry. Consequently, by considering only the information (NAICS code 51), I potentially did not capture Registered Apprenticeships in computer-related occupations that may have received AAI funding but were not in the information industry.

Second, in the United States, Computer and Mathematical Occupations constituted 77% of the occupations meeting the H-1B Specialty Occupations Labor Condition Programme in the 2013 fiscal year (United States Department of Labor, 2015). Registered Apprentices in these occupations were thus prime candidates to receive AAI funding. Amongst the remaining 23% figured numerous occupations such as Engineers and Health Diagnosing and Treating Practitioners. These occupations are respectively primarily located within the occupation groups Architecture and Engineering (O*NET SOC Code 17), Healthcare Practitioners and Technical (O*NET SOC Code 29), and Healthcare Support (O*NET SOC Code 31). Estimates' respective magnitudes are smaller relative to baseline and remain insignificant. Overall, even when considering occupational variation in treatment, either within or across industries, I fail to find a statistically significant effect of the AAI. The magnitude of the estimated effect is also very close to zero.

The dependent variable in Table 6 is the number of Registered Apprentices by state, year, NAICS two-digit industry and O*NET two-digit occupation group. Preferred specifications in Table A6 are shown in columns (9) and (10), in which all cross-sectional heterogeneity is addressed. Additionally, all time-varying occupation, industry and state unobserved heterogeneity is also captured by the fixed effects. Estimates' respective magnitudes are smaller relative to baseline and remain insignificant. Columns (9) and (10) of Table A6 indicate that even when considering occupational variation in treatment, either within or across industries, I fail to find a statistically significant effect of the AAI. The magnitude of the estimated effect is also very close to zero.

The final triple difference robustness check is Table 7. It contains a set of 44 "leave-one-out" regressions. Each row corresponds to equation (2), in which I sequentially omit one state. All results are statistically insignificant. Results are aligned to baseline and homogenous. No particular state is driving the results.

Table 6: Triple Difference Robustness Tests – Occupational and Industrial Variation in Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Triple Difference (Treated Occupation and Industry)	0.049		0.040		0.031		0.048		0.039	
	(0.034)		(0.031)		(0.032)		(0.030)		(0.031)	
Triple Difference (Treated Occupation or Industry)		0.017		0.008		0.003		0.015		0.010
		(0.012)		(0.010)		(0.011)		(0.010)		(0.011)
State-by-Year FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-Year FE	No	No	No	No	Yes	Yes	No	No	Yes	Yes
Occupation-by-Year FE	No	No	No	No	No	No	Yes	Yes	Yes	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. N = 170,016. All estimations include state-by-industry-by-occupation and year fixed effects. Standard errors clustered by state. In all specifications the dependent variable is the natural logarithm of the number of Registered Apprentices by state, industry, year, and occupation. Occupation is defined by 2-Digit O*Net Codes. Industries are defined by NAICS 2-digit codes. The mean of the dependent variable, in level form, is 2.07. "Triple Difference (Treated Occupation and Industry)" refers to the triple difference coefficient γ_1 in equation (2). However, it now captures Computer and Mathematical occupations (O*NET code 15), Architecture and Engineering Occupations (O*NET code 17), Healthcare Practitioners and Technical Occupations (O*NET code 29), and Healthcare Support occupations (O*NET code 31), that are carried out *within* the three AAI-treated industries of healthcare (NAICS code 62), information (NAICS code 51), and advanced manufacturing (NAICS code 33).

Table 7: “Leave one out” State Triple Difference Regressions

State	Triple Difference Coefficient	Standard Error	Mean of Dependent Variable
Alaska	0.068	(0.161)	0.984
Alabama	0.031	(0.161)	0.987
Arkansas	0.078	(0.159)	0.995
Arizona	0.103	(0.151)	0.98
California	-0.011	(0.150)	0.95
Colorado	0.065	(0.161)	0.974
Delaware	0.048	(0.162)	1.002
Florida	0.025	(0.160)	0.967
Georgia	0.062	(0.162)	0.985
Hawaii	0.057	(0.162)	0.988
Iowa	0.020	(0.159)	0.975
Idaho	0.014	(0.158)	0.989
Illinois	0.043	(0.162)	0.976
Indiana	0.039	(0.162)	0.974
Kansas	0.058	(0.162)	0.992
Kentucky	0.051	(0.162)	0.983
Louisiana	0.038	(0.162)	0.983
Maryland	0.056	(0.162)	0.986
Maine	0.092	(0.155)	0.992
Michigan	0.080	(0.159)	0.96
Minnesota	0.063	(0.162)	0.99
Missouri	0.051	(0.162)	0.972
Mississippi	0.056	(0.162)	0.994
Montana	0.071	(0.160)	0.997
North Carolina	0.064	(0.161)	0.965
North Dakota	0.043	(0.162)	0.994
Nebraska	0.028	(0.161)	0.992
New Hampshire	0.086	(0.157)	0.993
New Jersey	0.060	(0.161)	0.975
New Mexico	0.045	(0.162)	0.991
Nevada	0.050	(0.162)	0.993
Ohio	0.028	(0.161)	0.969
Oklahoma	-0.005	(0.152)	0.986
Pennsylvania	0.052	(0.162)	0.976
Rhode Island	0.073	(0.160)	1.002
South Carolina	0.078	(0.159)	0.954
South Dakota	0.042	(0.162)	0.997
Tennessee	0.049	(0.162)	0.977
Texas	0.016	(0.159)	0.966
Utah	0.045	(0.162)	0.982
Virginia	0.009	(0.157)	0.988
Vermont	0.052	(0.162)	0.987
West Virginia	0.020	(0.159)	0.979
Wyoming	0.027	(0.160)	0.997

Notes: N = 7,224. Standard errors in parentheses, clustered by state. *p<0.1, **p<0.05, ***p<0.01. The state mentioned in the leftmost column is the state that is left out of the estimation in the corresponding specification. States in bold are treated. In all specifications, the dependent variable is the natural logarithm of the number of Registered Apprentices by state, year and industry. This Table displays results from equation (2).

6.3. Difference-in-Discontinuity

I now turn to difference-in-discontinuity, and regression discontinuity design methodology. Table 8 displays results from difference-in-discontinuity estimation, following Wang et al. (2023) and Butts (2023). In columns (1) and (2), the dependent variable is the number of Registered Apprentices by county in the post-treatment period (October 1st, 2015 – September 30th, 2016). The columns thus show "classical" regression discontinuity design estimations. Columns (3) and (4) display results from difference-in-discontinuity specifications. The construction of the dependent variable is described in Section 5. Column (2) of Table 8 depicts results from equation (8). Column (4) depicts results from equation (7).

In both columns (3) and (4), Column (4) of Table 8 suggests that the AAI has insignificantly increased the dependent variable, $\Delta\overline{\mathrm{NumberApprentices}}_c$, by approximately 2% within the optimal bandwidth about the threshold. In other words, the AAI did not statistically significantly affect the proportional growth rate of the number of Registered Apprentices between the pre- and post-treatment periods within a narrow bandwidth about state borders.

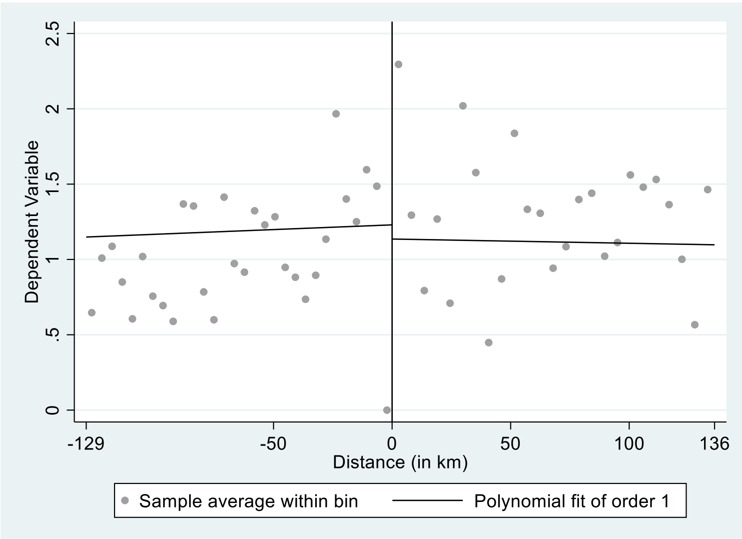
Table 8: Baseline Regression Discontinuity and Difference-in-Discontinuity Results

	(1)	(2)	(3)	(4)
	Regression Discontinuity Design	Regression Discontinuity Design	Difference-in-Discontinuity Design	Difference-in-Discontinuity Design
Treated	-0.307*	-0.094	0.039	0.020
	(0.181)	(0.120)	(0.0562)	(0.066)
Covariates	No	Yes	No	Yes
Number of Observations in Optimal Bandwidth	1,367	1,599	1,864	1,471
Mean Dependent Variable in Optimal Bandwidth	1.163	1.152	0.0961	0.0964

Notes: Treated denotes an indicator variable assuming the value of one if county c is located in a treated state, and 0 else. It corresponds to "$1(X_c \geq 0)$" in equations (5)-(9). Coefficients shown in the corresponding row are the regression discontinuity and difference-in-discontinuity estimates, respectively. All estimates are produced using the regression discontinuity design methodology of Calonico et al. (2017). In columns (1) and (2), the dependent variable is the natural logarithm of the number of Registered Apprentices by county in the post-treatment period (October 1st, 2015 – September 30th, 2016). The methodology is regression discontinuity design. In columns (3) and (4), the dependent variable is $\Delta\overline{\mathrm{NumberApprentices}}_c$. Its construction is detailed in subsection 4.3. Columns (3) and (4) display results from difference-in-discontinuity specifications (Butts, 2023, Wang et al., 2023, Picchetti et al., 2024). Standard errors are calculated using Heteroscedasticity Consistent HC3 weights. In line with Gelman and Imbens (2019), I run local linear regressions (polynomial order is 1). Table A3 lists the covariates. All columns contain state-pair fixed effects. Optimal bandwidth minimises mean square error based on Calonico et al. (2017). Threshold signifies a 0km distance from a given county's centroid to the state border of opposite treatment status. In column (4), the mean squared error-minimising bandwidth is 120km in control states, and 117km in treated states.

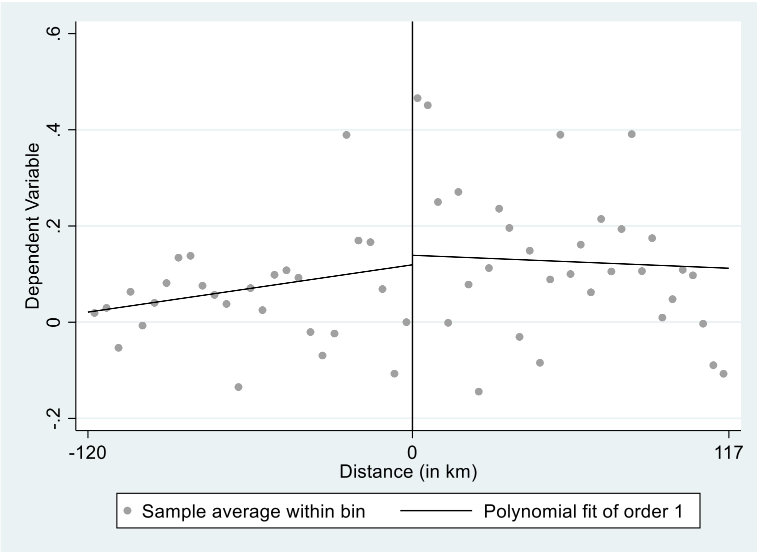
Figure 11 depicts a regression discontinuity plot for the regression discontinuity specification with covariates. The jump in the outcome at the threshold, $\mathrm{Log}(\mathrm{NumberApprentices}_{\mathrm{c,post}})$, is small and statistically insignificant. This insignificantly negative effect is shown in column (2) of Table 8. Figure 12 depicts a regression discontinuity plot for the preferred difference-in-discontinuity specification with covariates. The jump in the outcome $\Delta\overline{\mathrm{NumberApprentices}_c}$ at the threshold is small and statistically insignificant. When comparing columns (3) and (4) of Table 8, one can see that the inclusion of covariates does not alter inference. It does not sway statistical significance, nor does it reverse the sign of the estimate.

Figure 11: Regression Discontinuity Plot for Regression Discontinuity Design Specification



Note: Triangular kernel used for weighting. Plot produced using methodology of Calonico et al. (2017). The dependent variable, described in Appendix Table A3, is $\Delta\overline{\mathrm{NumberApprentices}_c}$. Distance is the running variable. It is negative for counties in control states. Distance is measured as the shortest distance between the centroid of a Registered Apprentice County and the nearest state border of a state with differing treatment status as the crow flies. It is measured in kilometres. 30 bins were selected below the threshold, with an average length of 4.294. 25 bins were selected above the threshold, with an average length of 5.433.

Figure 12: Regression Discontinuity Plot for Difference in Discontinuity Specification



Note: Triangular kernel used for weighting. Plot produced using methodology of Calonico et al. (2017). The dependent variable is $\mathrm{Log}(\mathrm{NumberApprentices}_{c,post})$. Distance is the running variable. It is negative for counties in control states. Distance is measured as the shortest distance between the centroid of a Registered Apprentice County and the nearest state border of opposite treatment status as the crow flies. It is measured in kilometres. The specification plot is shown within the optimal bandwidth for the preferred regression discontinuity design specification. 27 bins were selected below the threshold, with an average length of 4.436. 31 bins were selected above the threshold, with an average length of 3.771.

Table 9 contains robustness checks pertaining to difference-in-discontinuity estimation. Column (1) reiterates the baseline difference-in-discontinuity estimate. Table 9 shares results from donut regressions in its second column. Donut regression results indicate that omitting the counties most at risk of non-random, time-varying sorting of employers across the border does not qualitatively alter inference.

Table 9 column (3) also displays results from the difference-in-discontinuity preferred specification, run only in cross-state metropolitan areas, as defined by Grant (1955) (mapped in Figure B2, listed in Table A5). Focusing on counties within a metropolitan area makes them more comparable to each other, as they share many characteristics, both unobservable and observable. Only considering these 41 counties does not change baseline inference: I fail to find a statistically significant effect of the AAI in cross-state metropolitan areas.

In addition, Table 9 demonstrates results from the full AAI treatment period in column (4). The point estimate is 0.007, very close to 0. The estimate is statistically insignificant. This insignificance, combined with the low magnitude of the difference-in-discontinuity estimate, suggest that over its full period, the AAI has had an economically insignificant effect on the proportional growth in the number of Registered Apprentices.

The mean of the dependent variable, $\overline{\Delta \mathrm{NumberApprentices}_c}$, over the whole period, rises to 0.44. This rise is consistent with the observation that the number of Registered Apprentices in the United States is rising over time (ApprenticeshipUSA, 2024). $e^{0.44} = 1.55$, suggesting that over the full post-treatment period of the AAI, the number of Registered Apprentices was 1.55 times larger relative to the pre-treatment period.

In column (5) of Table 9, I restrict the bandwidth on both sides of the threshold to 80km. Observations located at extremities of the optimal bandwidth, calculated using methodology of Calonico et al. (2017), may not be comparable to each other based on unobservables. This increases bias. By arbitrarily reducing the bandwidth, I “tip the scales” in favour of bias reduction, against an increase in variance. In column (5) of Table 9, the standard errors are inflated relative to baseline. This imprecision in estimation yields a statistically insignificant estimate. All results of Table 9 consequently show that within the respective bandwidths about the state borders, the AAI has not statistically significantly affected the proportional growth in the number of Registered Apprentices.

Table 9 considers treatment falsification as well. The treatment threshold is shifted by 20km into control, and then treated states in columns (6) and (7), respectively. These placebo tests yield statistically insignificant results. This demonstrates that results are robust to shifting the treatment threshold, reinforcing their stability (Wang et al., 2023).

Table 9: Difference-in-Discontinuity Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Donut Diff-in-Disc	Metropolitan area Difference-in-Discontinuity	Extending treatment window until 2020	Restricting bandwidth on both sides of threshold to 80km	**Placebo:** Shifted Distance Threshold in Control States	**Placebo:** Shifted Distance Threshold in Treated States
Treated	0.02	0.70	0.85	0.007	0.03	-0.044	-0.041
	(0.07)	(0.74)	(1.29)	(0.06)	(0.08)	(0.063)	(0.060)
Mean Dep. Var. in optimal bandwidth	0.096	0.089	0.219	0.445	0.108	0.102	0.095
Observations within optimal bandwidth	1,471	494	41	1,548	1,078	1,432	1,635

Notes: Diff in Disc = Difference in Discontinuity. Treated denotes an indicator variable assuming the value of one if county c is located in a treated state, and 0 else. Coefficients shown in the corresponding row are the regression discontinuity and difference-in-discontinuity estimates, respectively. All estimates are produced using regression discontinuity design methodology of Calonico et al. (2017). In all columns, the dependent variable is $\Delta\overline{\mathrm{NumberApprentices}}_c$. All columns contain state-pair fixed effects and covariates. Optimal bandwidth minimises mean square error and is also calculated using methodology of Calonico et al. (2017). Threshold signifies 0km distance to the state border of opposite treatment status. Distances in control states are recoded to be negative. Standard errors are calculated using “HC3” heteroscedasticity-consistent weights. Covariates are listed and described in Table A3. Column (1) is the baseline difference-in-discontinuity estimate. It is here for reference. Column (2) is a “doughnut” regression. It omits counties located less than 50km from the state border of opposite treatment status. In column (3), I restrict the analysis to counties that are part of cross-state metropolitan areas (listed in Appendix Table A3). In column (4), I employ the full sample of data, i.e. October 1st, 2009, until September 30th, 2020. In column (5), I restrict the bandwidth on both sides of the threshold to 80km. Columns (6) and (7) are placebo regressions. In column (6), the threshold for treatment is shifted by 20km inside control states. In column (7), the threshold for treatment is shifted by 20km inside treated states.

Table 10 depicts results from equation (9). Table 10 investigates whether the AAI has had a significantly more positive on treated industries, i.e. healthcare, information, and advanced manufacturing, when considering only counties within a bandwidth around state borders of 120km in control states and 117km in treated states. More specifically, Table 10 serves two purposes. First, columns (1) and (3) quantify a "placebo" effect. This effect is not quantified in baseline difference-in-discontinuity estimation. Within counties located in treated states, it yields the impact of the AAI on control industries. To a certain extent, this coefficient will, at least partially, assess compliance of grantees with respect to AAI guidelines concerning industry. Under perfect compliance, this effect should be zero. Therefore, if the coefficient shown in Table 5 is positive and significant, within treated counties, the AAI would have significantly increased the proportional growth in the number of Registered Apprentices in *non-targeted* industries. This may be caused by imperfect compliance or may represent a spillover effect (see Feldman, 1994 and Scherer, 1982). A change in the growth rate of Registered Apprentices, within a treated county and industry, may indeed affect the number of Registered Apprentices in other industries. This intertwinement would occur through the interconnectedness of firms located in proximity to each other.

Column (2) of Table 10 complements column (1) but focuses on treated rather than control industries. I will compare the magnitude and significance of the coefficients shown in columns (1) and (2), notably through seemingly unrelated regression. Second, column (3) of Table 10 gains in efficiency relative to baseline difference-in-discontinuity by taking out an additional difference between treated and control industries *within* treated counties. Therefore, type 2 error risk is also reduced (Egerod and Hollenbach, 2024). This procedure is analogous to triple difference estimation, relative to difference-in-difference estimation.

In the difference-in-discontinuity specification, with dependent variable $\Delta\overline{\mathrm{NumberApprentices}}_{c,TI}$ (see subsection 4.4 for its construction), the estimated heterogeneity coefficient shown in Table 10 column (3), is statistically insignificant, with a t-statistic value inferior to 1. I fail to find sufficient evidence to support the hypothesis that the AAI was significantly more effective in treated industries in counties located in treated states.

However, comparing columns (1) and (2), the sign of the difference-in-discontinuity coefficient in equation (9) changes sign. It is negative in control industries, whilst it is positive in treated industries. This may suggest that the AAI has had a stronger effect on treated industries located in counties within treated states. To further investigate this, I execute seemingly unrelated estimation to compare the coefficients shown in the two subsamples of columns (1) and (2). The coefficients do not differ significantly at the 10% level. Consequently, I fail to find evidence supporting the hypothesis that the AAI has had a significantly stronger effect on treated industries, located in counties within treated states.

In addition, the "Treated County" coefficient in Table 10 column (3), although insignificant, is negative. On the other hand, the coefficient on the "Treated County * Treated Industry" is positive, although statistically insignificant. The former coefficient denotes, within the optimal bandwidth, the effect of the AAI on the proportional growth in the number of Registered Apprentices in control industries, in treated states. The latter denotes, within the optimal bandwidth, the effect of the AAI on treated industries in treated states. The latter coefficient also exceeds the former in magnitude. Consequently, this may suggest that the AAI may have had a positive effect on treated industries within treated counties, while having no effect on control industries in treated counties. Spillover effects within the optimal bandwidth, in treated counties, may have been low. Nonetheless, these are mere suggestions; I cannot make this assertion with 95% confidence.

Table 10: Regression Discontinuity and Difference-in-Discontinuity Results – Industry Heterogeneity

	(1)	(2)	(3)
	Control Industry Subsample	Treated Industry Subsample	Full Sample
Treated County	-0.079	0.008	-0.0890
	(0.054)	(0.047)	(0.0549)
Treated County * Treated Industry			0.107
			(0.071)
Number of Observations in Optimal Bandwidth	1,473	1,473	2,946
Mean Dependent Variable in Optimal Bandwidth	0.0678	0.0238	0.0458

Notes: Treated County denotes an indicator variable assuming the value of one if county c is located in a treated state, and 0 else. Coefficients shown in the corresponding row are the regression discontinuity and difference-in-discontinuity estimates, respectively. All estimates are produced using the regression discontinuity design methodology of Calonico et al. (2017). In column (1), the dependent variable is the natural logarithm of the number of Registered Apprentices by county in the post-treatment period (October 1st, 2015 – September 30th, 2016). The methodology is regression discontinuity design. In column (2), the dependent variable is $\Delta\overline{\mathrm{NumberApprentices}}_{c,TreatedIndustry}$. Its construction is detailed in subsection 4.4. Column (2) displays results from difference-in-discontinuity specifications (Butts, 2023, Wang et al., 2023, Picchetti et al., 2024). Standard errors are calculated using "HC3" heteroscedasticity-consistent weights. In line with Gelman and Imbens (2019), in all specifications the order of the polynomial of the running variable is 1. Table A3 lists the covariates. All columns contain state-pair fixed effects. Optimal bandwidth minimises mean square error based on Calonico et al. (2017). Threshold signifies a 0km distance from a given county's centroid to the state border of opposite treatment status. In all columns, the mean squared error-minimising bandwidth is 120km in control states, and 117km in treated states. Treated industries comprise healthcare (NAICS 62), information (NAICS 51), and advanced manufacturing (NAICS 33).

6.4. Treatment Effect Heterogeneity: Triple Difference and Difference-in-Discontinuity

As stated in Subsection 3.4, I now analyse treatment effect heterogeneity using credit constraint (Popov, 2014). I dichotomise the credit constraint index at its median value and generate two samples: above and below median credit constraint, respectively. The percentage of firms with 20 employees or less is also split at its median for heterogeneity analysis. Table 11 presents sample split regressions of equation (2), i.e. the triple difference methodology. Columns (1) and (2) only consider states with above and below or equal to median credit constraint index, respectively. Columns (3) and (4) only consider states with above or below or equal to median percentage of firms with 20 employees or less. I refer to these states as high and low share of small firms states, respectively.

Fafchamps and Labonne (2017) highlight that in most empirical settings, sample splits are an adequate strategy to investigate heterogeneity. With more than 3,000 observations, economically significant effect sizes can be discovered at statistical power exceeding the conventional 80% level.

No estimate shown in Table 11 is statistically significant at conventional levels. There does not seem to be a significantly heterogeneous effect of the AAI according to the level of a state's credit constraint. Nonetheless, although statistically insignificant, coefficient signs in Table 11 are aligned with the signs predicted by H2. Hypothesis H2 predicts that the AAI has a positive and relatively stronger treatment effect for states bearing high credit constraint index averages, and states with a larger share of small firms, more likely to face credit constraints than large firms. Table 11 indeed indicates that the AAI has an insignificant but positive treatment effect in states bearing high credit constraint index averages, and states with a larger share of small firms. Table 11 also indicates that the AAI has an insignificant but negative treatment effect in states bearing low credit constraint index averages, and states with a lower share of small firms.

Table 11: Heterogeneity of Triple Difference Results

	(1)	(2)	(3)	(4)
	High Credit Constraint	High share of small firms	Low Credit Constraint	Low share of small firms
Treatment Period * Treated States * Treated Industries	0.149	0.039	-0.085	-0.013
	(0.242)	(0.279)	(0.206)	(0.162)
Observations	3,696	3,696	3,696	3,696
Mean of Dependent Variable	1.128	1.142	0.838	0.824

Notes: In all columns, the dependent variable is the natural logarithm of the number of Registered Apprentices by State, year, and industry cell. Standard errors are in parentheses, clustered by State-industry cells. All specifications include State-by-year, State-by-NAICS 2-digit industry, and industry-by-year fixed effects. In column (1), only the subsample of states with above median credit constraint index is considered. In column (2), only the subsample of states with above median percentage of firms with 20 employees or less is considered. In column (3), only the subsample of states with below or equal to median percentage of firms with 20 employees or less is considered. In column (4), only the subsample of states with below or equal to median percentage of firms with 20 employees or less is considered.

Furthermore, a quadruple difference specification does not yield a significant interaction term between the triple difference regression and indicators for above median credit constraint and above median percentage of firms with twenty employees or less, respectively. This specification is equation (10). I again fail to find significant treatment effect heterogeneity. γ_2 is nonetheless positive, in line with the prediction set forth in hypothesis H2.

$$Log(NumberApprentices_{syi}) = \gamma_0 + \gamma_1 TreatedIndustry_i * TreatedState_s * Post_y + \gamma_2 TreatedIndustry_i * TreatedState_s * Post_y * AboveMedianCreditConstraint_s + \delta_{sy} + \varphi_{yi} + \theta_{si} + \varepsilon_{syi} \quad (10)$$

I now analyse heterogeneity in the treatment effect of the AAI using difference-in-discontinuity methodology in two ways. First, according to equation (7), I perform sample splits. The sample is first split at the median level of the credit constraint index. Then, it is split at the median of the percentage of firms with less than 20 employees. Second, I run equation (11):

$$\Delta\overline{\text{NumberApprentices}}_c = \mu_0 + \mu_1 X_c + \mu_2 1(X_c \geq 0) + \mu_3 1(X_c \geq 0) * X_c + \mu_4 AboveMedianCreditConstraint_c + \mu_5 X_c * AboveMedianCreditConstraint_c + \mu_6 X_c * AboveMedianCreditConstraint_c * 1(X_c \geq 0) + \mu_7 AboveMedianCreditConstraint_c * 1(X_c \geq 0) + \tau_{s,s'} + Z_c' \mu_8 + \varepsilon_c$$
(11)

I first select the optimal bandwidth for estimation in each of the below and above median credit constraint subsample using methodology developed by Calonico et al. (2017). A triangular kernel is used for weighting. Subsequently, I run equation (11) within the optimal bandwidth of baseline estimation. $AboveMedianCreditConstraint_c$ is an indicator variable, equal to 1 if county c has a credit constraint index above median, 0 else. μ_7 is the coefficient of interest here. If it is positive and significant, the AAI has a significantly more positive effect on relatively more credit-constrained counties. μ_7 is statistically insignificant, but negative. Again, this is the opposite sign of what was predicted by hypothesis H2.

I consequently fail to find that the AAI was significantly more or less effective in affecting the proportional growth rate in the number of Registered Apprentices in below or above median credit constraint counties. I also fail to find that the AAI was significantly more or less effective in affecting Registered Apprenticeship in counties located in states with a higher or lower share of small firms. Estimates remain noisily estimated. Noise is reinforced by the lower number of observations included in estimation.

Nonetheless, although insignificant, coefficient signs in Table 12 partially support hypothesis H2. In counties bearing higher credit constraint index averages, and a high share of small firms, the AAI has had an insignificantly positive effect on the number of Registered Apprentices. These signs are aligned with hypothesis H2. In counties bearing lower credit constraint index averages, and lower shares of small firms, the AAI has however also had an insignificantly negative effect on the proportional growth in the number of Registered Apprentices. These signs are thus in contradiction with hypothesis H2.

Table 12: Heterogeneity of Difference-in-Discontinuity Results

	(1)	(2)	(3)	(4)
	High Credit Constraint	High share of small firms	Low Credit Constraint	Low share of small firms
Treated	0.0028	0.025	0.059	0.061
	(0.084)	(0.078)	(0.092)	(0.094)
Observations in Optimal Bandwidth	770	794	663	768
Mean Dependent Variable in Optimal Bandwidth	0.07	0.10	0.14	0.09

Notes: Treated denotes an indicator variable assuming the value of one if county c is located in a treated state, and 0 else. Coefficients shown in the corresponding row are the regression discontinuity and difference-in-discontinuity estimates, respectively. In all columns, the dependent variable is $\Delta\overline{\mathrm{NumberApprentices}}_c$. Methodology is difference-in-discontinuity. In columns (1) and (2), the subsample is only counties above median credit constraint (as defined by the 2012 Credit constraint Index of the Federal Reserve Bank of New York, 2021). In columns (3) and (4), the subsample is only counties with below or median credit constraint (as defined by the 2012 Credit constraint Index of the Federal Reserve Bank of New York, 2021). Covariates are included in all specifications shown. They are listed and described in Table A3. Threshold signifies 0km distance to the state border of differing treatment status. Standard errors are calculated with "HC3" heteroscedasticity-consistent weights.

Results presented in Section 6 do not indicate that the AAI has had a statistically significant effect on growth in Registered Apprenticeship. Many estimates suggest the effect was also economically insignificant, after addressing as much unobserved heterogeneity as possible. This may be due to three reasons. First, the AAI may have, in truth, not had a significant effect. A type 2 error may have occurred due low power, caused by relatively noisy estimation (Egerod and Hollenbach, 2024). Third, in the RAPIDS dataset, only filled Registered Apprenticeship positions are observed. The AAI may have increased the supply of Registered Apprenticeship positions, which would have not been met by an increase in the demand for Registered Apprenticeship positions. The result is vacant positions, that do not appear in RAPIDS. In Section 7, I will expand on the first reason above, discussing why the AAI may have, in truth, not had a significant impact.

7. Discussion of Results

The AAI may have not statistically significantly affected the number of Registered Apprentices for at least six reasons, discussed in this section. First, AAI grantees did not discriminate firms according to firm size. Small firms are limited in the number of Registered Apprentices they can retain as skilled workers (Gunn and Da Silva, 2008). They may not have the personnel, large enough facilities or simply resources necessary to retain and thus expand their workforce, irrespective of subsidies they receive. A one-off subsidy granted to a small firm may thus be effective at inducing the temporary hiring of Registered Apprentices. However, this effectiveness is likely to be limited and have highly diminishing to null marginal returns. Public authorities wishing to grant subsidies must thus avoid a "one size fits all" strategy if they wish to minimise windfall gains and deadweight loss, while maximising effectiveness.

Second, AAI funds were not exclusively aimed at firms that did not already train. Funds could also be used on Registered Apprentices already working in a firm (United States Department of Labor, 2015). However, Muehlemann et al. (2005) argue that subsidies should exclusively target extensive margins. Once a firm has decided to train, variations in marginal costs in absolute terms no longer affect their demand for Registered Apprentices. This is because conditional on offering Registered Apprenticeship positions, firms face upper bounds on the number of Registered Apprentices they can train, for the reasons mentioned above. However, firms face substantial fixed costs when setting up training facilities, which may prevent them from offering Registered Apprenticeship positions (Muehlemann et al., 2005)[6].

Third, compliance of the grantees to AAI industry guidelines set out by the United States Department of Labor (2015) was low. The Office of Inspector General (2021) concluded that 88.5% of Registered Apprenticeship positions that were tied to AAI funds did not meet the key industry (nor occupation) criterion for H-1B visas. This key industry criterion required the expansion of Registered Apprenticeship in the industries of healthcare, information, and advanced manufacturing (United States Department of Labor, 2015). Consequently, AAI grantees, in large part, did not comply with this aspect of AAI guidelines. They funded firms who, for the most part, did not reinforce their engagement in Registered Apprenticeship within the targeted industries. The Department of Labor did not strictly enforce the industry-facet of the AAI among end recipients. This leaves the latter leeway in their decision of fund use (Office of the Inspector General, 2021). Stricter monitoring and control over grantee spending could have possibly mitigated this problem.

Fourth, certain attributes of Registered Apprenticeships may represent an impediment to their expansion. Kamphuis et al. (2010, p.287) indeed highlight that "financial stimulus may not be enough to trigger an investment decision on behalf of the firm". This may especially hold true in certain regulatory environments. Registered Apprenticeship curricula elaboration and adaptation processes may for instance be such an impediment (National Governor's Association, 2020). In New York state, Registered Apprenticeship curricula cannot be modified within the two years following their launch (Butrica et al., 2023). This rigidity may prevent employers from ensuring programmes reflect technological advancements. This is especially true in the information industry, one of the AAI's targeted industries. On the other hand, Schulteiss and Backes-Gellner (2022), referring to the Swiss dual VET context, demonstrate that curricula updates may yield substantial benefits to employers. Curricula updates diminish the time to adoption of new technologies in firms' workplaces. Additionally, apprentices having undergone training under updated curricula, reflecting technological advancements, represent a pipeline

[6] Findings and remarks of Muehlemann et al. (2005) apply to Switzerland. No such evaluation has been conducted in the context of Registered Apprenticeship in the United States. The apprenticeship ecosystem is very different in Switzerland than in the United States. Consequently, the findings of Muehlemann et al. (2005) may not exactly carry over to the United States' context. These findings are nonetheless indicative, and the most pertinent results on the subject thus far.

of labour supply with future-oriented skills. This is desirable to employers (Schulteiss and Backes-Gellner, 2022).

Fifth, under high worker mobility, firms have an incentive to externally hire skilled job changers instead of training its own labour (Acemoglu, 1997, Chang and Wang, 1996, Martins, 2021). In addition, the probability of retaining workers after training declines. Therefore, total expected net benefits from training decline (Chang and Wang, 1996). Relative to e.g. Germany, OECD (2023) data show that over the 2017/9 period, average labour market transition rates were higher in the United States. Moreover, between 2012/4 and 2017/9, the ratio of job-to-job transition increased by circa 4.5% in the United States, versus e.g. 2% in Germany. The comparatively high rate of worker mobility in the United States, combined with the presence of poaching, decreases the incentive to recruit Registered Apprentices (Chang and Wang, 1996). Further, under generally high worker mobility, only about 35% of Registered Apprentices complete their Registered Apprenticeship (Jones et al., 2021, Glover and Bilginsoy, 2005). Due to such low completion rates, the probability of employers being able to realise net benefits after training through retention is low. Analogously, the probability that Registered Apprentices remain with their training firm until the period during which their productivity exceeds their training costs is also low (Malcolmson et al., 2003). Subsidies may not be sufficient to compensate for this.

Sixth, Gardiner et al. (2021) survey collected information on United States employers' greatest barrier to expanding Registered Apprenticeship. The most cited reason was that (potential) employers lack resources and workers available to conduct on-the-job training. Such employers face a ceiling on their demand for Registered Apprentices: the number of skilled workers available to conduct training. This ceiling further limits the effectiveness of subsidies.

8. Conclusion

This paper analyses the causal impact of training subsidies on the number of Registered Apprentices in the United States. This paper fills an important gap in the literature, as limited empirical evidence exists on the causal impact of subsidies on the incidence of training (Muehlemann and Wolter, 2014, Kuczera, 2017). I find that the American Apprenticeship Initiative has not led to a statistically significant increase in the growth of the number of Registered Apprentices. The effect of the AAI is not significantly different in areas facing high liquidity constraints, and in areas with a high prevalence of small enterprises. Difference-in-difference, triple difference, regression discontinuity design and spatial difference-in-discontinuity estimations all yield aligned inference.

This study has three main limitations. First, absence of evidence of an effect is not evidence of absence. Although many point estimates indicate a near 0 effect, most confidence intervals are relatively noisy, increasing the risk of Type 2 error (Egerod and Hollenbach, 2024). Second, subsidies were not allocated randomly. Multiple causal inference methods are used in this paper to address unobserved confounders and omitted variables. However, I cannot fully exclude potentially biased results. Third, the RAPIDS dataset does not contain industry data for the states of Connecticut, Massachusetts, New York, Oregon, Washington, nor Wisconsin. Data for Washington D.C. are absent as well. This is because they employ their own proprietary state data reporting system. Their proprietary system requires the submission of Registered Apprentice-level information, but not of occupation nor industry-level information (Bilginsoy et al., 2022). This does not impede difference-in-difference, regression discontinuity design and difference-in-discontinuity analysis. Nonetheless, the absence of NAICS industry information for the abovementioned states has excluded them from triple difference analysis, which leverages information on an industry level. Consequently, the scope of the triple difference estimation results is not nation-wide. External validity of triple difference results is thereby limited.

Employer engagement in the United States in Registered Apprenticeship is relatively low (OECD/ILO, 2017). The breadth of Registered Apprenticeship's scope is thus relatively narrow, in the framework of Rageth et al. (2021). I now discuss five non-financial policy recommendations to increase the supply of Registered Apprenticeship positions. First, the Department of Labor could reinforce regulatory alignment across states. Regulation governing Registered Apprenticeship curricula, employment and standards indeed differs across states, because governing agencies also differ (United States Department of Labor, 2024). Among the fifty US states, there exist thirty-one distinct State Apprenticeship Agencies (United States Department of Labor, 2024), in addition to the Department of Labor's Office of Apprenticeship. The latter oversees Registered Apprenticeship training in twenty states. Each State apprenticeship agency has discretion over whether to register a Registered Apprenticeship programme. State apprenticeship agencies also decide over standards of compliance for Registered Apprenticeships and regulations employers must abide by within their state. By centralising regulation and the ability to register programmes within one federal entity, one could mitigate dissonance between states. Aligning regulations on a federal level would also simplify the engagement in Registered Apprenticeship of employers operating in multiple states (OECD/ILO, 2017). The Department of Labor's Office of Apprenticeship would consequently oversee quality control, and recognition of Registered Apprenticeship completion credentials (Embassy of Switzerland to the United States, 2022).

Second, contracts bespoke to Registered Apprenticeship training may be established between training firms and Registered Apprentices. Contracts should set wages of Registered Apprentices and the duration of Registered Apprenticeships (Malcolmson et al., 2003). Ensuring a minimum duration of Registered Apprenticeship ensures a period of training, towards the end, during which a Registered Apprentice's productivity exceeds their training cost. Contracts may stipulate that Registered Apprentice wages during training be low compared to that of a skilled worker. This increases the capacity of employers to realise net benefits during training, incentivising their engagement in Registered Apprenticeship training (Malcolmson et al., 2003).

Third, the establishment and strengthening of industry or occupation-wide professional organisations would be beneficial. In turn, it would create a training ecosystem favourable to the engagement of employers in Registered Apprenticeship. Well-established professional organisations may serve two key roles. First, they may advise individual employers as to the training content of Registered Apprentices operating in a specific occupation, thanks to their occupation-wide pooled expertise. This advisory function may take the form of pre-defined training plans (Embassy of Switzerland in the United States, 2022). Second, professional organisations may conduct intercompany courses for Registered Apprentices. Certain small firms may lack the in-house resources or expertise to teach a specific important facet of an occupation. If this is the case, such firms may send Registered Apprentices to intercompany courses. These facets would be taught through the latter courses. Economies of scale would then result, through the pooling of resources. In turn, Registered Apprentices having acquired such training may return to their sponsoring firm and diffuse their acquired knowledge.

Fourth, making training firms more visible may increase apprenticeship positions by improving the reputation of training firms with clients and potential workforce (Card et al., 2016). Accreditations such as the "Training Firm" vignette in Switzerland may therefore be implemented in the United States. It may be granted to firms training Registered Apprentices (see Swiss Secretariat for Education, Research, and Innovation, 2024, for more details regarding the training firm accreditation).

Fifth, reducing bureaucracy might increase Registered Apprenticeship positions. Employers view bureaucracy as a major hurdle to Registered Apprenticeship participation (Gardiner et al., 2021, Lerman, 2012, Copson et al., 2021, CEDEFOP, 2015).

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Appendix A: Tables

Table A1: Allowable Use of American Apprenticeship Initiative Grant Funds

Activity	**Allowable Use of American Apprenticeship Initiative Grant Funds**
On-the-Job Learning	Reimburse overhead costs associated with training provision, shadowing, mentoring and supervision
Related Technical Instruction	Development of courses, educational fees and tuition, training facility costs, instruction delivery costs (e.g. classroom instruction, virtual learning technology)
“Pre-apprenticeship” Activities	Provide preparatory skills for future Registered Apprentices, streamline the recruitment process, and help move job-ready Registered Apprentices into Registered Apprenticeship
Miscellaneous Activities Supporting Registered Apprenticeship	These activities include programme oversight and management costs, grant reporting costs, and other administrative functional costs, development costs of outreach and promotion to support increased awareness of Registered Apprenticeship for employers, etc.

Notes: Author’s own elaboration, using information from United States Department of Labor (2015).

Table A2: Extant Literature Summary

Authors	Country	Type of Training	Instrument	Motivation	Methodology	Main Finding
Westergaard-Nielsen and Rasmussen (2000)	Denmark	Formal, Apprenticeship	Subsidy	Incentivise firms to retain their supply of apprenticeship positions in response to adverse market conditions	Random-Effects Poisson	A 50% increase in subsidisation levels results in an overall increase in the number of apprentices by approximately 5%.
Brebion (2020)	France	Formal, Apprenticeship	Subsidy	Reduction of high youth unemployment rates through higher apprenticeship prevalence	Triple Difference	Subsidies increase extensive but do not affect intensive training margins.
Georg and Strobl (2006)	Ireland	Non-formal, On-the-job Training	Subsidy	Remedy skill shortages and market imperfections hindering training investment	Difference-in-difference	Subsidies increase training in domestic plants, not in foreign-owned plants
Leuven and Oosterbeek (2004)	Netherlands	Non-formal, On-the-job Training	Tax deduction	Increase the prevalence of lifelong learning	Regression Discontinuity Design	Authors detect a policy displacement effect. Subsidies did not stimulate overall training incidence.
Tian et al. (2022)	China	Non-formal, On-the-job Training	Tax deduction	Remedy insufficient investment of firms in on-the-job training	Difference-in-Difference	Authors find a positive effect of tax deduction on training expenses of privately-owned, small firms.
Martins (2021)	Portugal	Non-formal, On-the-job Training	Subsidy	Reinforce human capital amidst technological change	Difference-in-Difference	Subsidies increased the number of training hours and training expenditures.
Goerlitz (2010)	Germany	Non-formal, On-the-job Training	Subsidy	Increase participation of low-skilled workers in training	Triple Difference	Subsidies increase extensive training margins, but do not affect intensive training margins
Holzer et al. (1993)	United States (Michigan)	Non-formal, On-the-job Training	Subsidy	Upskill local workforce amidst rising international competition in manufacturing sector	First-Difference Multivariate OLS	Subsidies increase training hours two to three-fold.
Abramovsky et al. (2011)	United Kingdom	Non-formal, On-the-job Training	Subsidy	Increase participation of low-skilled workers in training, and increase overall national skill level	Difference-in-Difference	In the short-term, there was no significant increase in the share of employers providing training.
Schuss (2023)	Germany	Formal, Apprenticeship	Training Fund	Remedy labour shortages in key healthcare sector, amidst ageing population	Staggered Difference-in-Difference	Training fund has a positive effect on extensive training margin in ambulatory care, and positive effect on intensive training margin in inpatient care.
Kamphuis et al. (2010)	Netherlands	Formal, Apprenticeship	Training Fund	Achieve economies of scale to decrease marginal cost of training, combat poaching	Multilevel Regression with Propensity Score Matching	Sectoral training funds did not cause firms in these sectors to invest more in training.
Kuku et al. (2016)	Mauritius	Non-formal, On-the-job Training	Training Fund	Overcome credit and labour market imperfections causing suboptimal training investment	Multivariate Probit	Overall, training funds represent a financial burden for medium and large firms, who provide most of the training. These firms respond by reducing training investment.

Table A3: Descriptive Statistics – Regression Discontinuity Design and Spatial Difference-in-Discontinuity in Optimal Bandwidth of Difference-in-Discontinuity

Variable	Variable Explanation	Mean	Standard Deviation	Min.	Max.
Dependent Variables					
$\text{Log}(\text{NumberApprentices}_{c,post})$	Dependent Variable of Regression Discontinuity Design: The natural logarithm of the number of Registered Apprentices by county, in the post-treatment period.	1.18	1.82	0	6.70
$\Delta\overline{\text{NumberApprentices}_c}$	Dependent Variable of Difference in Discontinuity Estimation: Construction Described in Subsection 4.3.	0.10	0.60	-1.67	2.30
Covariates					
Share of Women	County-Level Average Share of Women (%) in 2010	50.33	2.02	33.10	55.50
Share of High School Graduates	County-Level Average Share of High School Graduates (%) in 2010	82.20	7.09	56.30	97.40
Share of Bachelor Degree Holders	County-Level Average Share of Bachelor Degree Holders (%) in 2010	18.45	8.58	4.30	58.30
Log(Per-Capita Income)	Natural Logarithm of County-Level Average Per-Capita Income in 2010 (USD)	9.98	0.22	9.28	10.99
Log(Civilian Labour Force)	Natural Logarithm of Civilian Labour Force in 2010	9.69	1.37	5.63	14.79
Log(Employed)	Natural Logarithm of Number of Employed Individuals in 2010	9.59	1.37	5.58	14.67
Share of Non-Hispanic Whites	County-Level Share of Non-Hispanic Whites in 2010	79.21	18.67	10.30	98.70
Democrat Vote Share	County-Level Share of Democrat Votes in the 2012 Presidential Election	39.94	14.77	3.45	91.25
Mean Travel Time to Work	County-Level Average Travel Time to Work, in minutes, in 2010	23.34	5.13	4.50	42.10
Heterogeneity	*County-Level Variables Showcasing Treatment Effect Heterogeneity*				
Credit Constraint	County-Level Credit Constraint Index in 2012	29.69	9.29	6.60	64.30
Share of Small Firms	Share of Firms with 20 Employees or Less in 2013 (%)	76.87	5.60	28.05	96.00

Notes: The number of observations in the difference-in-discontinuity sample, within the bandwidth used, is 1,471. The optimal bandwidth is 120km in control states, and 117km in treated states. All variables shown in Table A3 are sourced from ApprenticeshipUSA (2024), United States Department of Agriculture (2024), United States Census Bureau (2013) and Federal Reserve Bank of New York (2021). Section 4.3 describes the construction of the dependent variables in the difference in discontinuity ($\Delta\overline{\text{NumberApprentices}_c}$) and regression discontinuity design ($\text{NumberApprentices}_{c,post}$) specifications.

Table A4: Covariate Balance at the Threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	County Share of Women in 2010	County Share of Non-Hispanic Whites in 2010	County Share of High-School Graduates in 2010	County Democratic Vote Share in 2012 Presidential Election	County Share of Population with Bachelor Degrees in 2010	County Mean Work-Travel Time in 2010	County Log(Per-Capita Income) in 2010	County Log(Civilian Labour Force) in 2010	County Log(Employed) in 2010
Treated	0.0641	-0.0641	0.195	0.0712	-0.0529	0.0154	0.00138	-0.00139	0.00128
	(0.194)	(0.706)	(0.349)	(0.624)	(0.368)	(0.410)	(0.00923)	(0.00192)	(0.00192)
Mean of Dep. Var. in optimal bandwidth	50.33	79.22	82.20	39.94	18.45	23.34	9.978	9.694	9.590

Notes: Treated denotes an indicator variable assuming the value of one if county c is located in a treated state, and 0 else. Coefficients shown in the corresponding row are the regression discontinuity and difference-in-discontinuity estimates, respectively. All estimates test the balancedness of county- and state-specific covariates, following the regression discontinuity design methodology of Calonico et al. (2017). Covariates are used as dependent variables sequentially. All regressions are run within the optimal bandwidth of difference-in-discontinuity estimation: 120km in control states and 117km in treated states. They are variants of equation (7). Column titles correspond to the covariate being used as dependent variable in the corresponding specification. Each covariate is described in Table A3, along with its descriptive statistics. Threshold signifies 0km distance to the state border of opposite treatment status. Distance is the running variable. Standard errors are calculated using "HC3" heteroscedasticity-consistent weights. All estimates include a first order polynomial of distance (Gelman and Imbens, 2019). *p<0.1, **p<0.05, ***p<0.01. In all columns, all other covariates are used in the estimation, as well as state-pair fixed effects and flexible controls for county centroids' respective latitudes and longitudes. The number of observations within the optimal bandwidth, in all columns, is 1,475.

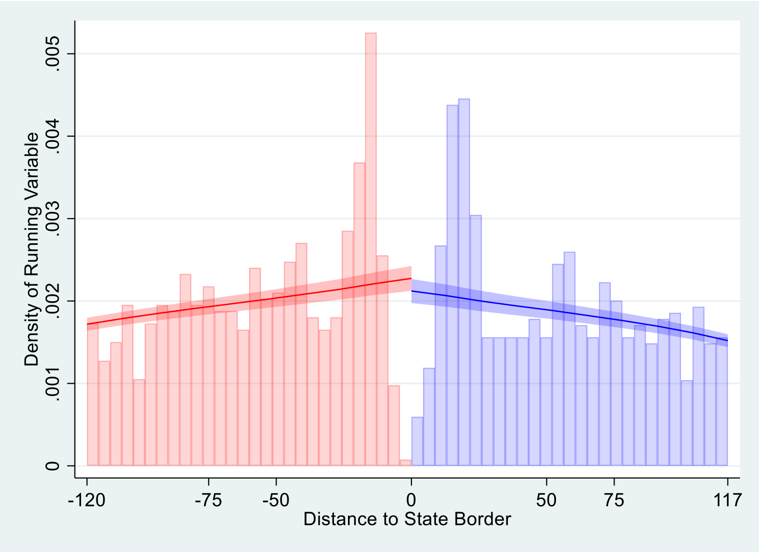
Table A5: Table of Counties in Metropolitan Areas with Variation in Treatment Status

Metropolitan Area	States	County Federal Information Processing System (FIPS) Codes in the Cross-State Metropolitan Area
Chicago	Illinois (treated), Indiana (control)	18089, 17031, 17197
Chattanooga	Georgia (Treated), Tennessee (TN)	47065, 13295, 13047
Columbus	Georgia (Treated), Alabama (AL)	13215, 01081, 01113
Philadelphia	Pennsylvania (Treated), New Jersey (Control)	42101, 42017, 42091, 42029, 34005, 34007
New York	New York (Treated), New Jersey (Control)	36081, 36047, 34039, 36061, 36085, 34013
Davenport	Illinois (Treated), Iowa (Control)	19163, 17161
Huntington	West Virginia (Treated), Kentucky (Control)	34017, 54011, 54099, 34023
Saint Louis	Illinois (Treated), Missouri (Control)	29510, 29189, 29099, 29183, 17119, 17163, 17133
Springfield	Massachusetts (Treated), Connecticut (Control)	25013, 09003
Wheeling	West Virginia (Treated), Ohio (Control)	54069, 54051, 39013
Youngstown	Pennsylvania (Treated), Ohio (Control)	39099, 42073

Notes: These metropolitan areas follow the classification of cross-state metropolitan areas of Grant (1955).

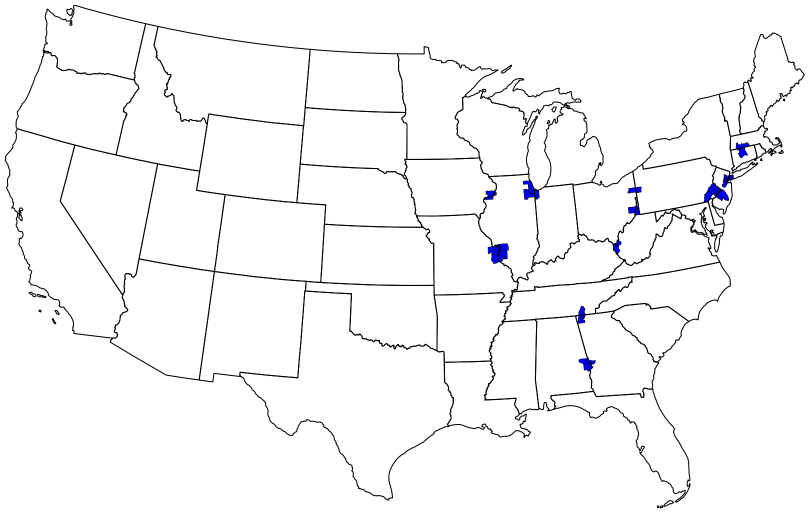
Appendix B: Figures

Figure B1: Manipulation Test in the Running Variable



Notes: This Figure tests the hypothesis of no manipulation or bunching in the running variable at the threshold. The running variable is shown on the X-axis. Using methodology of Calonico et al. (2017), this graph tests the null hypothesis that the density of the running variable is continuous at the threshold distance of 0. The associated p-value with the test of this null hypothesis is 0.25. I fail to reject the null hypothesis that the density of the running variable is continuous at the threshold distance of 0. The dip in the density near 0km signifies that very few county centroids are located within 5km to 10km of the nearest state border of opposite treatment status.

Figure B2: Map of Counties in Metropolitan Areas with Variation in Treatment Status



Note: This Figure depicts on a map the cross-state metropolitan areas shown in Table A5. These cross-state metropolitan areas are sourced from Grant (1955). The are listed in Table A5.

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