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

Guillaume M. A. Morlet[1]

Abstract

The United States Department of Labor facilitates non-formal training through Registered Apprenticeship and has distributed subsidies to incentivise employer participation. The first such subsidy was the American Apprenticeship Initiative (AAI). I analyse the AAI's causal effect on the number of new Registered Apprentices using federal Department of Labor administrative data. I exploit state, time, and industry variation in AAI treatment eligibility to conduct triple-difference estimation. I additionally leverage variation across states and time periods to perform spatial difference-in-discontinuity regressions. Results indicate that the AAI has not led to statistically significantly stronger growth in the number of new Registered Apprentices.

Keywords: Labour Demand; Non-Formal Training; Triple Difference; Spatial Difference-in-Discontinuity; Subsidies.

JEL Codes: J01; J08; J23

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[1] ETH Zurich, Stampfenbachstrasse 69, 8092 Zurich. Corresponding author: guillaume.morlet@mtec.ethz.ch.

1. Introduction

Labour demand is rapidly evolving due to fast-paced technological change (Lightcast, 2022). The speed of this evolution causes the demand for skilled workers to outpace supply in such sectors (Feng, 2021). Consequently, employers in the United States lack qualified workforce in multiple industries (Lerman et al., 2019, Magnini et al., 2024). One solution to mitigate labour market shortages is non-formal Registered Apprenticeship programmes, which quickly and adaptably impart work-relevant skills to the workforce (Lightcast, 2022, OECD/ILO, 2017, Kuczera, 2017). Participation in non-formal training programmes among 25–64-year-olds in OECD countries stands at 40% (Denzler et al., 2025). Widespread participation in such programmes prevents human capital depreciation and furthers lifelong learning, which is particularly important in aging OECD economies. Amid rising automation of routine tasks, participation in non-formal training programmes also allows workers to subsequently be more involved in non-routine tasks (Tamm, 2018), relatively more shielded from automation. Furthermore, non-formal training is essential to prepare the United States workforce for the implementation of large-scale investments in cutting-edge technologies and infrastructure investments, such as the CHIPS and Science Act, or the Infrastructure Investment and Jobs Act (United States Government Accountability Office, 2025).

In consequence, the 47th President of the United States, in line with predecessors, issued an executive order on April 23rd, 2025, aiming to add one million new active Registered Apprentices (White House, 2025). A Registered Apprenticeship is a non-formal industry-driven training programme that combines structured on-the-job learning, classroom instruction, and mentorship, leading to a nation-wide and industry-wide recognised credential. It allows employers to build a skilled, experienced workforce aligned with industry standards. Registered Apprenticeship contains two components: related technical instruction (RTI) and on the job training (OJT) (Gardiner et al., 2021, Lerman and Rauner, 2011). Firms decide on how many Registered Apprenticeship positions they offer. OJT must last at least 2,000 hours (Fumia et al., 2022, Lerman and Rauner, 2011). RTI is often dispensed by community colleges (Gardiner et al., 2021) and entails a minimum of 144 hours. Starting wages of Registered Apprentices are on average $18 per hour (Bruno and Manzo, 2025).

To achieve the goal of expanding Registered Apprenticeship, the United States Department of Labor distributed over $1B of training subsidies between 2015 and 2022 to maximise employer engagement in Registered Apprenticeship (Butrica et al., 2023). In 2022, the Employment and Training Administration of the Department of Labor spent $285M on Registered Apprenticeship training, circa 8% of the federal employment and training allocation (Belman, 2022). Increased subsidisation of Registered Apprenticeship in the United States occurs amidst an international trend in training subsidisation (Employment and Social Development Canada, 2019, République Française, 2025, see Corseuil et al., 2019, for Brazil and Fenizia, Li and Citino, 2024, for Italy). Investigating whether subsidies are an effective way to raise training prevalence is thus of current and international relevance.

The subsidisation policy evaluated in this paper is the American Apprenticeship Initiative (AAI). AAI subsidies totalled $154M (USA Spending, 2015, see Appendix Table 1). These $154M are part of the $1B spent by the federal Department of Labor between 2015 and 2022 as part of vocational training subsidies (Butrica et al., 2023). The AAI aims to increase relatively low employer engagement and upskill the United States workforce through Registered Apprenticeship (United States Department of Labor, 2015, OECD/ILO, 2017, Lerman et al., 2019). The AAI grant period lasted five years; October 1st, 2015, to September 30th, 2020 (United States Department of Labor, 2015). AAI grant funds were distributed to grantees operating in 25 states and Washington D.C. Grantees were either state governments, nonprofit organisations, public/state-controlled higher education institutions (e.g. technical or community colleges), city or township governments, or workforce development agencies (USA Spending, 2015). Grantees are listed in Appendix Table 1. Within their respective operating state, i.e. their place of performance, AAI grantees were to fund Registered Apprenticeships in one of the four manners elicited in Appendix Table 2. Chiefly, these channels were reimbursing employers for OJT costs, and RTI providers for Registered Apprentice's tuition fees (United States Department of Labor, 2015). I provide more details on the AAI's implementation in the background section of this paper (Section 2). The objective of the AAI was to fund the registration of 34,000 new Registered Apprentices over five years (National Governor's Association, 2020). This is 8% of active Registered Apprentices in 2015 (ApprenticeshipUSA, 2024).

In addition, federal Department of Labor guidelines dictated that AAI grantees must channel funding towards industries that were most reliant on H1B visas, i.e. foreign workforce. The AAI was thus to develop the United States' workforce skills within these critical industries to lessen the dependent on foreign labour (United States Department of Labor, 2015). The United States Citizenship and Immigration Services (2025) indicate that on average, between 2009 and 2015, NAICS industries in which employers used the largest fraction of total H1B visas were Professional and Scientific Services (58% of total initial approvals of H1B visas), Educational Services (12%), Advanced Manufacturing (7%), Healthcare and Social Assistance (5%), and Information (4%). The United States Department of Labor (2015) required AAI grantees to, within the states they operate, finance Registered Apprentices and their programmes active in these industries.

Registered Apprentices are on average 28 years of age (Rolland, 2015). Slightly over a third over Registered Apprentices are nonetheless younger than 24. Over 50% of Registered Apprentices are white, circa 88% are male. Joint labour-management programmes, affiliated with labour unions, trained approximately 59% of all Registered Apprenticeship programme participants. 58.6% of Registered Apprentices eventually complete their programme and obtained on average exit hourly wages of $31.81 (Bruno and Manzo, 2015). 88.5% of Registered Apprentices whose programme was supported using AAI funds had a professional activity prior to commencing the programme. They earned more than federal minimum wage, however less than the US mean hourly wage (Walton et al., 2022). 39.4% of these Registered Apprentices had worked in a job similar to the occupation in which they were pursuing the programme. Finally, Registered Apprentices supported by AAI funds enrolled in this programme mainly to train for an entire career, not solely one job, and to gain labour-market employer-valued skills (Walton et al., 2022).

Only few studies provide causal evidence on the impact of subsidies on vocational training, leaving a gap in the literature (Mueller and Behringer, 2012, Kuczera, 2017). Reports evaluating the AAI exist (see e.g. Fumia et al., 2022, Copson et al., 2021, National Governor's Association, 2020). Nevertheless, they are descriptive rather than causal. As measure of AAI effectiveness, such reports employ the number of new Registered Apprentices to directly receive AAI funding (see e.g. Walton et al., 2022). This is however not the causal effect of the AAI on the number of new Registered Apprentices; Registered Apprenticeship positions directly funded by the AAI could have been created even without subsidies. A counterfactual scenario is not considered. The original contribution of this paper is therefore to investigate the causal effect of the AAI on the number of new Registered Apprentices.

The closest paper to this study is Fenizia, Li and Citino (2024). The authors evaluate a programme in Italy in 2007, through which small firms could apply for reductions in social security contributions for apprenticeship contracts. The magnitude of this reduction equates to 8% of the earnings of a 20-month apprenticeship. Fenizia, Li and Citino (2024) do not find that this policy has led to a significance rise in apprenticeship contracts. Firm's demand for apprentices is relatively inelastic. In Italy, apprenticeship training is driven by the need to hire a new employee. Italian firms thus train to retain and secure a pipeline of permanent skilled labour. Subsidies, or payroll tax reductions, affect training costs. The latter are not a primary consideration in firms' training decisions (Fenizia, Li and Citino, 2024).

This paper's empirical analysis uses federal administrative data on start date, state, county, occupation 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, which is then recorded into the nationally representative RAPIDS database (Bruno and Manzo, 2025).

I do not observe individual firms due to dataset limitations. Consequently, I cannot identify which firms received and eventually spent AAI funds. Lerman et al. (2022) however show that approximately 90% of AAI subsidies were eventually spent by grantees, in line with other subsidisation policies (Brebion, 2020). The treatment effects I uncover are therefore intention to treat (ITT). This corresponds to the impact on the number of new Registered Apprentices of making subsidies available to employers in given states and industries. This is a "real world effect". It is of interest to policymakers because take-up rate and treatment participation cannot always be controlled (Albanese et al., 2024). Given that almost all funds were eventually spent, if treatment effects of subsidies across (unobserved) firms were homogenous, this ITT effect would be close in magnitude and sign to the average treatment effect on the treated firms (Battistin and Sianesi, 2011). Moreover, while the identification of firms and AAI fund end recipients would allow the pursuit of a fuzzy RDD via TSLS estimation, this is not advisable due to a power asymmetry problem of the latter methodology. Kaliski et al. (2025) indeed recommend reporting ITT effects in such contexts, which does not suffer from this power asymmetry problem.

This paper uses two identification strategies. In the empirical methodology section, I detail how each identification strategy overcomes selection into treatment; AAI funds were not distributed randomly. The first is triple difference. It considers state, time and industry variation in treatment. The second leverages spatial and time variation in treatment in the form of a spatial difference-in-discontinuity. I use variation across counties located in states of opposite treatment status and variation in AAI treatment timing. The use of the latter identification strategy follows its reliance on alternative identification assumptions, and the findings of Giroud et al. (2024) and Belenzon and Schankerman (2013). Spread of information and knowledge-sharing are very localised, and decays with distance rapidly. Furthermore, knowledge-sharing is stronger within same-industry geography clusters, which may occur within narrow bandwidths about the border (Kim, 2023). Consequently, awareness about the AAI and familiarity with Registered Apprenticeship programmes likely negatively depends on distance and is more similar within a narrow bandwidth about state borders. It thus follows to model the outcome variable as a function of distance to the border of a state of opposite treatment status.

Both identification strategies yield aligned results. The AAI has not significantly increased the number of new Registered Apprentices. Robustness checks corroborate these baseline results. Additional difference-in-discontinuity estimates indicate that the effect of the AAI is not significantly stronger for treated industries located within treated counties in a narrow bandwidth about the state border.

If the triple difference estimates were significant, I would find that \$1M spent through the AAI led to 22 Registered Apprentices hired in treated states, industries, and over the first year of the AAI-treatment period. This equates to \$45,454 per job-year in treated states and industries. Hiring 22 Registered Apprentices at the prevailing competitive wage rate for Registered Apprentices in the relevant industries, states and time period (\$40,114) would however cost \$882,508 annually, therefore would be more economical. Nevertheless, this cost-benefit analysis has a caveat. Washington D.C., New York, Massachusetts, Oregon, Washington, and Wisconsin do not make data available in RAPIDS relative to industry or occupation. These states were thus excluded from triple difference analysis and consequently omitted from this cost-benefit analysis.

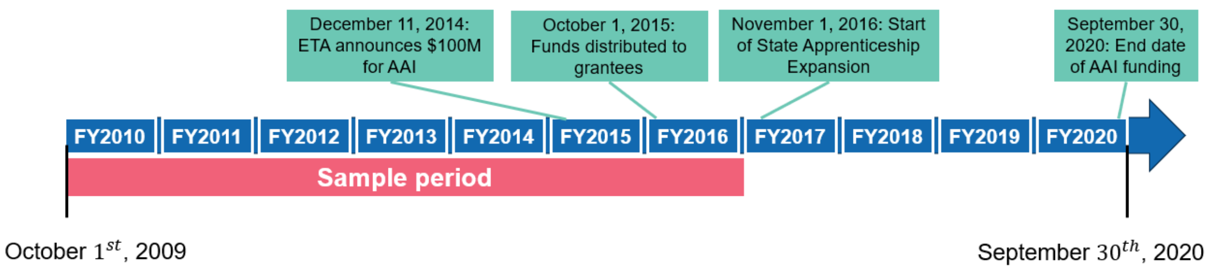
The remainder of this paper is structured as follows. Section 2 discusses background information and implementation of the AAI. Section 3 discusses the econometric methodology. Section 4 represents the data section. Section 5 presents results. Section 6 discusses the results. Section 7 concludes and offers policy recommendations.

2. The American Apprenticeship Initiative

2.1. American Apprenticeship Initiative Background Information

The AAI was announced by the United States Department of Labor on December 11th, 2014. Funds were distributed on October 1st, 2015 (Fumia et al., 2022), thus *not* in a staggered manner. The grant period lasted five years until September 30th, 2020, excluding continuation funding (USA Spending, 2015). Figure 1 shows the timeline of the AAI. On November 1st, 2016, the State Apprenticeship Expansion grant commenced (United States Department of Labor, 2016). This was the first of numerous other federal grants aimed at developing Registered Apprenticeship. However, this paper solely concerns itself with the causal effect of the AAI. Continuing the estimation period post November 1st, 2016, would thus encompass confounding effects of other policies (Bertrand et al., 2004), whose industrial and geographical scope is relatively vaguely defined. Additionally, considering a staggered adoption model would preclude the use of state-by-time or industry-by-time fixed effects, which are however fundamental for causal identification. I thus restrict the baseline sample to end on October 1st, 2016. In robustness checks, I do consider the full AAI grant period and find aligned results.

Figure 1: American Apprenticeship Initiative Timeline

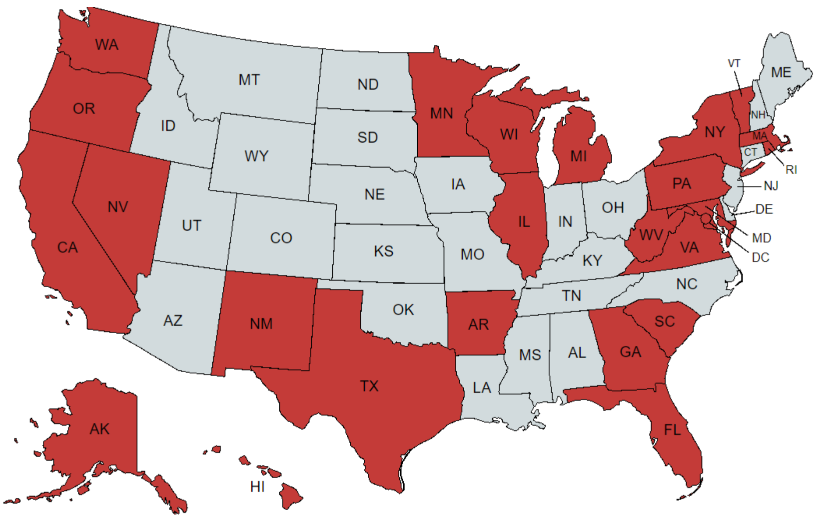


Note: Author's Own Elaboration using Information from National Governor's Association (2020) and United States Department of Labor (2015). FY= fiscal year. AAI = American Apprenticeship Initiative. Sample period refers to the period considered in econometric analysis main specifications, discussed in 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.

Red states in Figure 2 depict the states in which AAI grantees operate. Control states are in grey. States are not treated *directly*. AAI funds were given to "grantees" by the federal Department of Labor. These were assigned a place of performance scope for the grant. This geographic area corresponds to the state in which the grantees operate (USA Spending, 2015). Grantees were to spend AAI funds within their respective states to reinforce the prevalence of Registered Apprenticeship, funding programmes located in industries heavily reliant on H1B visas for their workforce.

These federal guidelines were common to all AAI grantees, as one aim of the AAI was to lessen the dependence on foreign labour (United States Department of Labor, 2015). The funding opportunity announcement for the AAI indeed clearly states: “Applicants should commit, on behalf of the partnership, to (1) work with DOL (or DOL-recognized State Apprenticeship Agencies) to register new apprenticeship programs in H-1B related industries […] such as, Information Technology, and/or other high-growth industries including but not limited to Advanced Manufacturing, Business Services, and Healthcare” (United States Department of Labor, p.2). Data from the United States Citizenship and Immigration Services (2025) indicate that the top five industries most reliant on H1B visas between 2009 and 2015 were Professional and Scientific Services, Educational Services, Advanced Manufacturing, Healthcare and Social Assistance, and Information. These industries will thus be considered as “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.

Grantees are thus intermediary organisations between the federal Department of Labor and “end recipients”. These are typically firms or public colleges who will eventually receive AAI funds. The chief use of these funds is the reimbursement of RTI tuition fees and defrayment of on-the-job training costs (Copson et al., 2021) (described in much more detail in subsection 2.3). Grantees receive AAI funds from the Department of Labor and subsequently distribute them in subawards, vendor contracts, employer reimbursement agreements, or grants to directly reimburse approved RTI costs to the abovementioned end recipients (USA Spending, 2015, United States Department of Labor, 2015).

The 47 grantees of the AAI are listed in Appendix Table 1. They are either state governments, city or township government, nonprofit organisations, labour unions, or state-controlled institutions of higher education (e.g. community and technical college systems). Appendix Table 1 also states grantees’ respective AAI funding amounts, as well as their respective state of performance for the grant. Some states, e.g. California, may contain multiple grantees. Appendix Figure 1 displays a heat map of the United States, summing by state the total amount of funds that all AAI grantees located in that given state have received. For instance, six AAI grantees were in the state of California. Summing all their AAI grants results in a total of circa $19.3M. Appendix Figure 1 shows that the total amount of subsidies by state is positively correlated with a state’s population, as could be expected.

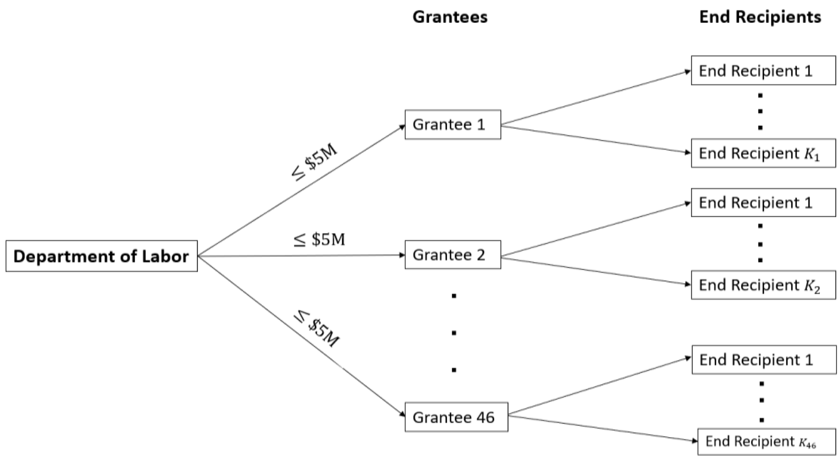
AAI funding amount totalled $154M. The scale of the AAI is relatively small, notably compared to foreign apprenticeship subsidisation policies (see e.g. Brebion, 2020, for France). The AAI aimed to enrol 34,000 new Registered Apprentices in advanced manufacturing, information and healthcare, within targeted states over five years (National Governor's Association, 2020). This represents 8% of all active Registered Apprentices in 2015 (ApprenticeshipUSA, 2024). Per Registered Apprentice, AAI funds amount to $4,526 *on average*, and $1,132 per Registered Apprentice per year *on average* (for a four year programme). $1,132 per Registered Apprentice per year corresponds to circa 2.82% of Registered Apprentice's average starting wage in treated industries and states in 2015 (circa $40,114 per year), according to author's own calculations based on RAPIDS data. The size of per year per Registered Apprentice subsidy, relative to average starting wages in treated industries and states, is comparable to the hiring credits studied by Cahuc et al. (2019) of 4%, but lower than the targeted payroll tax reductions evaluated by Fenizia, Li and Citino (2024) of 8%. Consequently, the scale of the AAI may be considered as relatively small. In comparison, Swiss public authorities spent circa CHF 18,097 per apprentice in 2023, mainly through the funding of vocational schools (SERI, 2024, Federal Statistical Office, 2024).

2.2. Eligibility and Applications for AAI Funding

AAI funding is competitive. It is distributed in a two-stage process. First, applicants (future "grantees" if successful) apply to the Department of Labor for funding. Applications were assessed and scored by a federal Department of Labor grant officer, considered against a list of pre-defined criteria, including applicant's needs, expected outcomes, project design, capacity, and budgeting (United States Department of Labor, 2015). Second, successful applicants were then to use the funding to expand Registered Apprenticeship in industries heavily reliant on H1B visas (listed in subsection 2.1), within their place of performance, i.e. their state (United States Department of Labor, 2015). 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. The reception of these funds is symbolised by the first set of arrows, linking the Department of Labor to AAI grantees. Subsequently, grantees, whose respective profiles are listed in Appendix Table 1, distributed these funds within their respective states to "end recipients", i.e. the last entities to receive funds. "End recipients" principally comprise employers, or e.g. technical colleges directly, paying for Registered Apprentices' tuition fees upfront (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. "End recipients" are not observed in the RAPIDS dataset used in this paper, however.

Figure 3: Distribution of American Apprenticeship Initiative Funds



Note: K denotes the k^th 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 new Registered Apprentices. K_1 refers to the number of end recipients of grantee 1.

2.3. Fund Use

The Department of Labor permitted grantees to use their AAI funds for various purposes, listed in Appendix Table 2. However, the common objective of all grantees 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 (Walton et al., 2022), further diluting the $1,029 per Registered Apprentice per year mentioned above. Two main regulations governed the use of funds. First, at most $10,000 can be spent per Registered Apprentice. Second, AAI funds cannot reimburse apprentice wages (United States Department of Labor, 2015).

As exemplified and discussed above, AAI funds were first received by AAI grantees, who further distributed these funds to end recipients through 1) vendor contracts 2) subawards 3) employer reimbursement agreements and 4) grants reimbursing approved RTI costs. Two-thirds of AAI 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 RTI costs of Registered Apprentices. Grantees do this by paying tuition to the RTI provider directly or by reimbursing the employer ex-post for Registered Apprentices' RTI. 31% of grantees offer employers incentives for OJT, for instance by defraying mentor wage costs (Gardiner et al., 2021, Copson et al., 2021, United States Department of Labor, 2015)[2].

Copson et al. (2021) provide an overview of certain grantees' spending. One such case study is that of the South Carolina Technical College System, which corresponds to points 1) and 4) made above. The South Carolina Technical College System used 84% of its subsidy to fund RTI costs of Registered Apprentices. South Carolina-based Registered Apprentice employers sent applications to the South Carolina Technical College System to receive subsidies to finance RTI of their Registered Apprentices. Subsidies were then allocated directly to the relevant technical colleges providing RTI. These covered the tuition fees of Registered Apprentices whose employers had qualified to obtain subsidies. As at the

[2] Unfortunately, the exact information for each grantees regarding the end distribution of these funds and end recipients is not identifiable. Information regarding AAI applications is also unavailable.

2017/18 fiscal year, 138 company locations in South Carolina had the tuition fees of at least one apprentice covered by the AAI (State Board for Technical and Comprehensive Education, 2018).

Managed Career Solutions, in California, used its AAI grant notably for the second purpose mentioned above (contracts with vendors). It bulk-purchased courses and course subscriptions from institutions such as Udemy and Career Academy to contribute towards the RTI for Registered Apprentices (Copson et al., 2021). Managed Career Solutions also used its AAI grant simultaneously towards the third purpose mentioned above (employer reimbursement agreements for OJT). The latter reimbursed Registered Apprentice employers up to $3K per Registered Apprentice to defray OJT costs, such as costs related to mentoring (Copson et al., 2021).

2.4. How Might AAI Fund Use Affect Training Incentives?

One can model this most prevalent type of fund use (financial incentives to employers) theoretically, through a highly simplified model, borrowing from Morlet and Bolli (2025). Most AAI subsidies were employed to reduce training costs, as reviewed in subsection 2.3. I therefore define the following function, which represents firms' net benefits from Registered Apprentice training. Net benefits from training depend on the number of Registered Apprentices, A, and subsidies, S.

$$NetBenefits(A,S) = A * (ProdValue\ + SavedHire) - CostTrain(A,S) \tag{1}$$

Firms will train if $NetBenefits(A,S) > 0$. $ProdValue$ denotes the productive value, in USD, of a Registered Apprentice's work to their training firm. $SavedHire$ are saved hiring costs, which arise if training firms retain their Registered Apprentices after their graduation. Finally, $CostTrain$ are training costs borne by the training firm. In equation (1), they can be modelled as:

$$CostTrain(A,S) = A * [w_a + \frac{1}{2}C(S) * A] \tag{2}$$

w_a is the wage of Registered Apprentices. C(S) denotes the sum of tuition fees for RTI, mentor wage and opportunity costs, and other miscellaneous training costs. C(S) depends negatively on subsidies, as described in subsection 2.3 and in line with Brebion (2020): $\frac{\partial C(S)}{\partial S} < 0$. In turn, therefore, $\frac{\partial CostTrain(A,S)}{\partial S} < 0$ and $\frac{\partial NetBenefits(A,S)}{\partial S} > 0$. Consequently, from a simplified theoretical perspective, by reducing training costs, subsidies should increase training because the elasticity of training provision with respect to training costs is negative (Brebion, 2020).

3. Econometric Methods

3.1. Triple Difference

The AAI has a three sources of variation: industry, state and time. Treated industries are advanced manufacturing (NAICS code 33), healthcare and social assistance (NAICS code 62), information technology (NAICS code 51), professional and technical services (NAICS code 54), and Educational Services (NAICS code 61), as reviewed in Section 2.1 (United States Department of Labor, 2015, United States Citizenship and Immigration Services, 2025). The remaining industries serve as control.

A triple difference methodology, leveraging these three sources of treatment variation, may be preferable to difference-in-difference for three reasons. First, triple difference eliminates state-time, state-industry, and industry-time varying confounders. All parallel trend violations based on the abovementioned double-differences (e.g. state-by-year) are addressed by these fixed effects. Additionally, these fixed effect vectors address selection into treatment based on these characteristics. For example, if a grantee applies for AAI funds, and sees their application approved, based on a certain industry prevalence in their state, this potential source of selection bias would be captured by state-by-industry fixed effects. Additionally, should a grantee see their application approved because of a recent positive trend in employment growth of a key industry, or because of pre-existing favourable trends in labour market factors, industry-by-year and state-by-year fixed effects in equation (3) would capture the confounding effects of such trends. Additionally, these comprehensive fixed effect vectors address any potential confounding effect of local labour market characteristics (e.g. workforce size or number of firms) that does not vary on a year-by-state-by-industry basis.

Second, Berck and Villas-Boas (2016) 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 is large, and if the outcome variable in treated and untreated industries has 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).

The triple difference specification is presented in equation (3). The dependent variable is the natural logarithm of the number of new 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 the logarithm of $NumberApprentices_{syi}$, I replace all zero values in the dependent variable with one[3]. Employing Poisson pseudo maximum likelihood for equation (3) yields qualitatively aligned results. Results are producible upon request.

$$Log(NumberApprentices_{syi}) = \gamma_0 + \gamma_1 TreatedIndustry_i * TreatedState_s * Post_y + \delta_{sy} + \varphi_{yi} + \theta_{si} + \varepsilon_{syi} \qquad (3)$$

The coefficient of interest in equation (3) is the triple difference coefficient γ_1. $100 * (e^{\gamma_1} - 1)$ measures, in percentage, the growth rate in the number of new 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 (3) absorb all difference-in-differences, and therefore all non-parallel confounding trends in these difference-in-differences. ε_{syi} is the disturbance term. I cluster standard errors by state. γ_1 is thus the *additional* effect of the AAI in treated *industries* located in treated states and during the treatment period, relative to *control industries* in treated states during the treatment period.

I now turn to the *relative* parallel trends assumption required in triple difference estimation. 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 new 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. McConnell (2023) states that parallel trends may hold in levels (of the outcome), i.e. the number of new Registered Apprentices, or in proportions, i.e. the natural logarithm of the number of new Registered

[3]Our panel is strongly balanced. The model in equation (3) is saturated. It yields the identical triple difference coefficient to the saturated equation $Log(NumberApprentices_{syi}) = \gamma_0 + \gamma_1 TreatedIndustry_i * TreatedState_s * Post_y + \gamma_2 TreatedIndustry_i + \gamma_3 TreatedState_s + \gamma_4 Post_t + \gamma_5 Post_t * TreatedIndustry_i + \gamma_6 Post_t * TreatedState_s + \gamma_7 TreatedState_s * TreatedIndustry_i + \varepsilon_{syi}$. Additionally, estimation does not rely on covariates for identification, i.e. it does not rely on a *conditional* relative parallel trends assumption. The triple difference coefficient may thus be viewed as the difference between two difference-in-difference coefficients (Ortiz-Villavicencio and Sant'Anna, 2025, Olden and Moen, 2022).

Apprentices, but not both. I assume the latter, in line with Finkelstein (2007). The dependent variable in the empirical analysis is thus in logarithmic form. Certain states (industries) are much larger than others in terms of population (workforce) and number of new Registered Apprentices, such as California (construction). 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. However, proportional differences in growth rates of the number of new 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 new Registered Apprentices would have remained parallel in the absence of the AAI.

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 new Registered Apprentices in AAI-targeted and non-AAI-targeted industries (Olden and Moen, 2022). Over the post-treatment period, 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 subsections 2.1 and 4.2. In the pre-treatment period, Figure 4 demonstrates trends in the outcome variable of triple 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. If parallel trends in the pre-treatment period hold for a longer period, this reinforces the hypothesis of random treatment timing (Angrist and Pischke, 2009).

The graph was constructed in the following manner. The number of new 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-industry-status combinations. This occurs because there are two statuses for both states and industries: treated or untreated. Furthermore, there are seven fiscal year in the 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 was logged before being then split by industry treatment status:

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

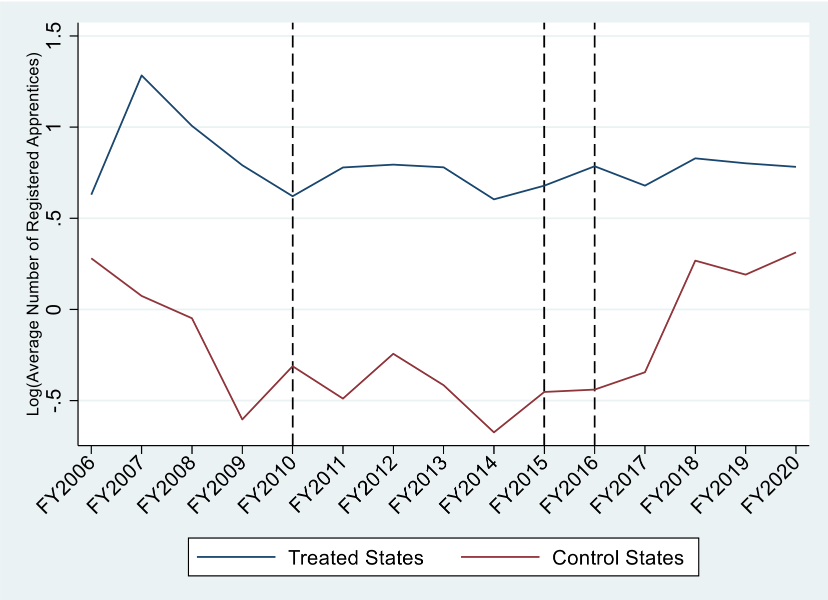
I form coarser groups of observations, by year and treated state status. This yields 14 values. I subtract $Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=0})$ from $Log(\overline{NumberApprentices}_{TS_s,Year,TI_i=1})$:

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

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

These values are plotted in Figure 4. The estimation sample remains October 1st, 2009, to October 1st, 2016. Trends are parallel between treated and control states from fiscal year 2013 until the treatment introduction. After the end of fiscal year 2016, there has been a surge in the growth of healthcare Registered Apprenticeships in control states. Prior to fiscal year 2013, trends are parallel between fiscal years 2007 and 2008. Nevertheless, between fiscal years 2006 and 2007, and fiscal years 2008 to 2012 inclusive, trends do not seem parallel. To investigate this issue more precisely, I execute event-study equation (4). I then test for the joint statistical of coefficients on fiscal years 2006 and 2007, and 2008 to 2012 inclusive, respectively. Both sets of coefficients are jointly insignificantly different from 0, respectively. Moreover, in a robustness check, I restrict the analysis window of triple difference estimation to fiscal years 2015 and 2016. Inference remains qualitatively aligned to baseline inference. I conduct these supplementary tests in line with the limitations highlighted by Roth (2022) relative to simply relying on a visual inspection of parallel trends and overall results from event studies.

Figure 4: Outcome Trends in Triple Difference Estimation

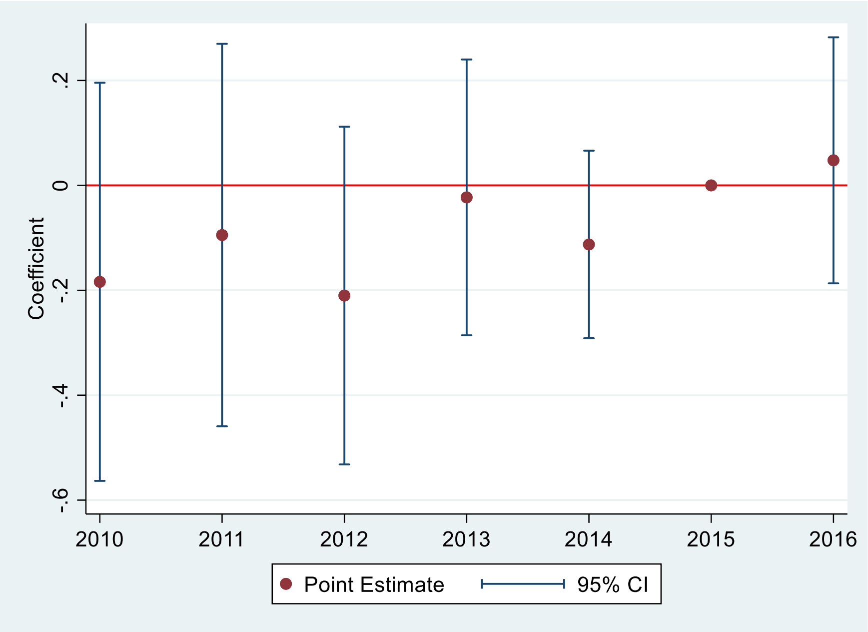


Notes: FY = Fiscal Year. This Figure depicts, by year, and within control and treated states respectively, the proportional difference in the growth rate between the average number of new Registered Apprentices in AAI-targeted and non-AAI-targeted industries, respectively (McConnell, 2023). Because treatment 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 (4). It is shown in Figure 5. 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 (4). 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 (4) 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.

$$Log(NumberApprentices_{syi}) = \delta_{sy} + \varphi_{yi} + \theta_{si} + \sum_{z=-6,z\neq-1}^{-2} \alpha_z * TreatedIndustry_i * TreatedState_s * \mu_z + \alpha_0 * D_{si} * \mu_0 + \varepsilon_{syi} \tag{4}$$

Figure 5: Triple Difference Event Study



Notes: The figure shows coefficients of triple difference coefficient estimates from equation (4) 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 new Registered Apprentices by state, year, and industry cell (the outcome variable of equation (4)). 95% confidence intervals use standard errors clustered by state.

3.3. 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 outcome variable, a log-transformation of the number of new Registered Apprentices, is now modelled as a function of distance to the state border in this methodology.

Counties located closer to a state's border within the control group are better counterfactuals for treated counties for three reasons. First, they may share identical cross-state labour markets (Grant, 1955). Second, knowledge-sharing rapidly decays with distance (Giroud et al., 2024, Belenzon and Schankerman, 2013). Consequently, firms located in control counties situated near the state border are more likely to be aware of Registered Apprenticeship programmes, their benefits, and may have more comparable employer engagement in these programmes. Third, firms located in counties near the state border are more likely to be part of cross-state border industry clusters, in which knowledge-sharing is stronger (Giroud et al., 2024).

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 through the threshold. Following Wagner and Portillo (2024) and Wang et al. (2023), the outcome variable of difference-in-discontinuity estimation is differenced. The identification assumption specific to this case thus refers to the conditional expectation function of the *differenced* counterfactual outcome. 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, and cannot be correlated with actual treatment (Butts, 2023). The effect of this time-invariant selection into treatment can then be fully differenced out.

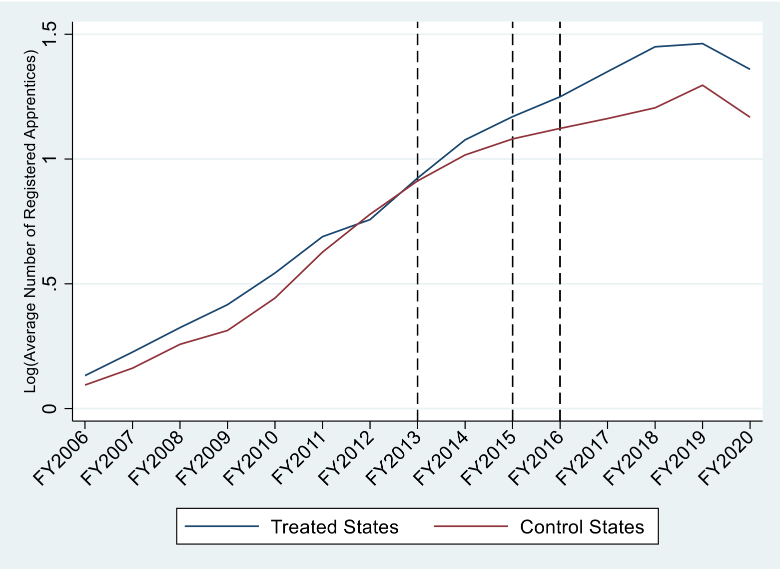
Bennedsen et al. (2022) liken this second assumption to parallel trends. Within a narrow bandwidth about the threshold, the trends of the conditional expectation function of the outcome for treated and control groups would have remained parallel between pre and post periods, had treatment not been implemented. Here, 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. On the other hand, difference-in-difference estimation applies this assumption to the entire estimation sample, not simply a bandwidth about the state borders.

Consequently, difference-in-discontinuity allows selection into treatment to occur on a time-invariant basis. Difference-in-discontinuity thus assumes that county-specific time-varying trends do not correlate with selection into treatment. Given that I restrict the bandwidth about state borders, this assumption becomes more plausible, as counties in different states but very close to each other are likely to face similar labour market fluctuations, weather conditions, industry compositions, Registered Apprenticeship participation, etc. In line with this assumption, difference-in-discontinuity identifies the local average treatment effect for treated units at the state border.

In line with Grembi et al. (2016), the difference-in-discontinuity estimation I pursue seeks to difference out the effects of confounding policies, such as state-specific tax credits for Registered Apprenticeship (United States Department of Labor, 2025). The effects of these confounding policies that also turn on at the threshold (state border) of the running variable were already observed in the pre-treatment period as the policies were implemented prior to the AAI. Essentially, the difference-in-discontinuity methodology is akin a difference-in-difference model in which the treated variable is being located within a treated state and executed non-parametrically within a narrow bandwidth about the state border, for increased comparability.

Figure 6 shows, within this bandwidth, the evolution of the proportional rate of change in the outcome variable between counties located in treated and control states, respectively[4]. Figure 6 considers the period October 1st, 2005, to September 30th, 2020, to show parallel trends for a longer period, reinforcing internal validity (Angrist and Pischke, 2009).

Figure 6: 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 new 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 exposition.

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, 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 6, the parallel trends assumption within the analysis bandwidth largely holds for a long period before the introduction of the AAI. There is nonetheless a divergence in fiscal year 2012.

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 new Registered Apprentices in a given county and year. October 1st, 2013, to September 30th, 2015, is the pre-treatment period. October 1st,

[4] The logarithm of the number of new 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 new 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.

2015, to September 30th, 2016, is the post-treatment period. For each county, I first take the natural logarithm of the number of new 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 new Registered Apprentices, by county, only within the post-treatment period. Subsequently, within counties, I average the logarithm of the number of new Registered Apprentices over the two pre-treatment periods[5]. Finally, I subtract the latter from the former, in line with the difference-in-discontinuity methodology employed by Wang et al. (2023), Lemieux and Milligan (2008), Koster and Van Ommeren (2019) and Wagner and Portillo (2024). This yields $\Delta\overline{\text{NumberApprentices}}_c$.

If $\Delta\overline{\text{NumberApprentices}}_c$ is positive (negative), the number of new Registered Apprentices by county in the post-treatment period is superior (inferior) to the geometric mean of the number of new Registered Apprentices by county in the two pre-treatment periods considered. It thus measures proportional growth in the number of new Registered Apprentices by county. Exponentiating this variable yields a ratio. This ratio indicates how many times larger the number of new 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 3, in addition to state-pair fixed effects. Following the methodology of Calonico et al. (2017):

$$\hat{\tau}(h) = e_0'\hat{\beta}_+(h) - e_0'\hat{\beta}_-(h) \tag{5}$$

$\hat{\tau}(h)$ assesses whether there is a discontinuity in the $\Delta\overline{\text{NumberApprentices}}_c$ at the threshold distance of 0 (Wang et al., 2023). h is the mean-squared error minimising bandwidth (Calonico et al., 2017).

Consider equations (6) to (8). $1(\blacksquare)$ 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, whose descriptive statistics are listed in Appendix Table 3. Z_c' also comprises counties' respective flexible latitude and longitude controls (Keele and Titiunik, 2015).

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 whose border 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 e.g. Utah, a control state, to e.g. Oklahoma, another control state. 51 state-pairs figure in the optimal bandwidth. Within these 51 state-pairs, pairs such as California-Arizona *and* Arizona-California are considered as one unique pair. 1,471 distinct counties figure in the optimal

[5] This arithmetic mean is equivalent to the logarithm of the geometric mean of the number of new 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.

bandwidth of the preferred difference-in-discontinuity specification. This equates to the effective number of observations employed in the preferred difference-in-discontinuity specification. Figure 7 shows the counties which are employed in the estimation bandwidth.

Appendix Table 3 also conducts mean-comparison tests of covariates across treated and control counties, within a narrow bandwidth about the border. Within a narrow bandwidth about the state border, as at 2010, treated counties have a significantly larger civilian labour force, are more inclined to vote for Democrats, and have a lower proportion of non-Hispanic whites. These significant differences may be problematic in "classical" regression discontinuity, or in local-randomisation-based regression discontinuity. Nevertheless, in this difference in discontinuity approach, these pre-treatment differences are removed through a differencing procedure. Furthermore, in Appendix Table 4, we replace the dependent variable of difference in discontinuity with each covariate, sequentially, and verify whether the difference in discontinuity estimate is statistically significant. None of these estimates are significant. Given that observables are smooth through the threshold (state border), this lends credence to the hypothesis that unobservables are smooth through the threshold as well. Finally, Appendix Figure 2 suggests no significant bunching in the running variable through the threshold.

$\hat{\beta}_{+}(h)$ and $\hat{\beta}_{-}(h)$ are calculated in equations (6) and (7) respectively (following Picchetti et al., 2024). The "+" subscript highlights focus on the bandwidth above the threshold, i.e. in treated states. The "-" subscript highlights focus on the bandwidth below the threshold, i.e. in control states. 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.

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

$$\hat{\beta}_{-}(h) = argmin_{\beta} \sum_{c=1}^{n} \left(\Delta\overline{\text{NumberApprentices}}_c - \tau_{s,s'} - Z_c'\gamma - 1(X_c < 0)(1\, X_c)(X_c - \bar{x})'\beta\right)^2 K_h(X_c - \bar{x}) \quad (7)$$

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

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

Where $\text{Log}(\text{NumberApprentices}_{\text{c,post}})$ is the natural logarithm of the number of new Registered Apprentices in county c in the post-treatment period, October 1st, 2015, to September 30th, 2016. π is the coefficient of interest in both equations (9) and (10). It denotes, under identification assumptions, the local intention to treat effect of the AAI, within the respective optimal bandwidth about the border.

To lend credence to the validity of spatial difference-in-discontinuity assumptions, I conduct tests for manipulation of the running variable, and smoothness of observable covariates through the threshold in preferred difference-in-discontinuity specifications. Appendix Figure 2 displays the manipulation test in the estimation sample of the preferred spatial difference-in-discontinuity specification (Cattaneo et al., 2018). The p-value of this test is 0.25. I fail to reject the null hypothesis of no manipulation of the running 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 4 serves this purpose. I obtain the estimate (5) sequentially using each covariate as a dependent variable. All regressions are run within the optimal bandwidth used in baseline specifications. No estimate shown in Appendix Table 4 is statistically significant at any conventional level. Observable characteristics are smooth through the threshold, lending credence to the hypothesis that the counterfactual conditional expectation function is continuous at the threshold. Confidence intervals of estimates in Appendix Table 4 are relatively large, potentially suggesting a lack of power to detect a discontinuity in the covariates at the threshold. Nevertheless, tests for manipulation, as well as a visual inspection of Appendix Figure 2 fails to reject the null hypothesis of no manipulation.

3.4. 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 consider 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, $\Delta\overline{\mathrm{NumberApprentices}_c}$, to be county-industry-treatment specific (treated and control industries), instead of only county-specific: $\Delta\overline{\mathrm{NumberApprentices}_{c,TI}}$.

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 new 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 new Registered Apprentices in the pre-treatment period from that in the post-treatment period. This yields $\Delta\overline{\mathrm{NumberApprentices}_{c,TI}}$.

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%). This reduces noise caused by outlier counties and increases power.

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

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.

4. Data

4.1. Data Source

I employ the administrative dataset "Registered Apprenticeship Partners Information Database System" (RAPIDS) from the United States Department of Labor. This repeated cross-sectional dataset covers all new Registered Apprentices in the United States (ApprenticeshipUSA, 2024). It is at the individual level. It is compulsory for all training establishments with Registered Apprentices in the United States to track and report Registered Apprentice's data to the Department of Labor's Office of Apprenticeship. However, not all states use RAPIDS directly for data reporting.

States are either under the purview of the Department of Labor's Office of Apprenticeship or of their state-level State Apprenticeship Agencies (United States Department of Labor, 2024). All states under the purview of the Department of Labor's Office of Apprenticeship employ the RAPIDS system. 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 excluded from triple difference estimation. These states are Connecticut, New York, Washington, Oregon, Massachusetts, Wisconsin. Washington D.C. is also concerned. I define the year of observation as the year of the Registered Apprentice's start date. Therefore, this paper focuses on the effect of the AAI on the number of *new* Registered Apprentices.

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

The object of interest is the number of new Registered Apprentices per firm. However, firms are not observed in the dataset. I therefore collapse the Registered Apprentice-specific dataset to state-year-industry specific cells. The number of new Registered Apprentices is summed within state-year-industry cells. 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 new Registered Apprentices in empty cells, i.e. state-year-industry cells missing from the RAPIDS dataset. The rationale for doing this is that a cell without any new Registered Apprentices *in fact has 0 new Registered Apprentices*. Finally, before taking the logarithm of the number of new Registered Apprentices per state-year-industry cell, I replace zeros by one.

Appendix Table 5 displays descriptive statistics of the dependent variable in level form. There are on average 58 new 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 estimator to 0 depends on the number of observations within cells (Antman and McKenzie, 2007).

In treated (control) states, there are on average 81 (41) new Registered Apprentices per state, year and two-digit NAICS industry cell. This difference is significant at the 1% level, reflecting the fact that treated states are on average more populous, and may reflect stronger economic activity or better awareness of Registered Apprenticeship programmes in treated states. Nevertheless, the number of new Registered Apprentices per state, year and industry cells is not statistically significantly different across treated and control industries.

Out of 7,392 cells, 4,970 cells (67%) contain 0 new Registered Apprentices. The states in which, throughout the sample, these cells are most frequently located are Rhode Island (treated, 152 cells with 0 Registered Apprentices), South Dakota (control, 147 cells) and Delaware (control, 146 cells). The fiscal years during which these cells most frequently occur are 2010 (805 cells with 0 Registered

Apprentices), 2011 (757 cells), and 2012 (717 cells). Finally, the two-digit NAICS industries containing the most cells with 0 Registered Apprentices are Transportation and Warehousing (301 cells), Management of Companies and Enterprises (298 cells), and Finance and Insurance (294)[6].

25 states, in addition to Washington D.C., are treated. They thus constitute the performance scope of AAI grantees. 19 treated states only are in the triple difference estimation dataset however, due to the abovementioned missing industry data. 25 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 NAICS codes 51 and 62 respectively. Advanced Manufacturing is NAICS 33 (Muro et al., 2015, Conexus Indiana, 2016). Educational Services bears code 61, while Professional and Technical Services industry bears code 54.

The triple difference estimation sample 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) as shown in Figure 1. 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) or the Youth Apprenticeship Readiness Grant. I am however solely interested in the causal effect of the American Apprenticeship initiative. I therefore restrict the analysis window around the treatment period to not capture confounding policies' effects (Bertrand et al., 2004).

4.3. 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 and sum the number of new Registered Apprentices by county-year cell. Counties are nested within states, which are either treated or control. I only consider continental United States. I again assign a value of zero new Registered Apprentices for county-year cells that are missing in the RAPIDS data, following the same logic as above. Again, 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 within control states, whilst it is 117km above the threshold, within treated states. This excludes the states of Wyoming, Montana, Alaska and Hawaii. This bandwidth is calculated using Calonico et al. (2017). 1,471 counties are present in the optimal bandwidth, shown in Figure 7.

Distance, the running variable, is measured as the shortest distance as the crow flies between the centroid of a Registered Apprentice's county and the nearest state border of opposite treatment

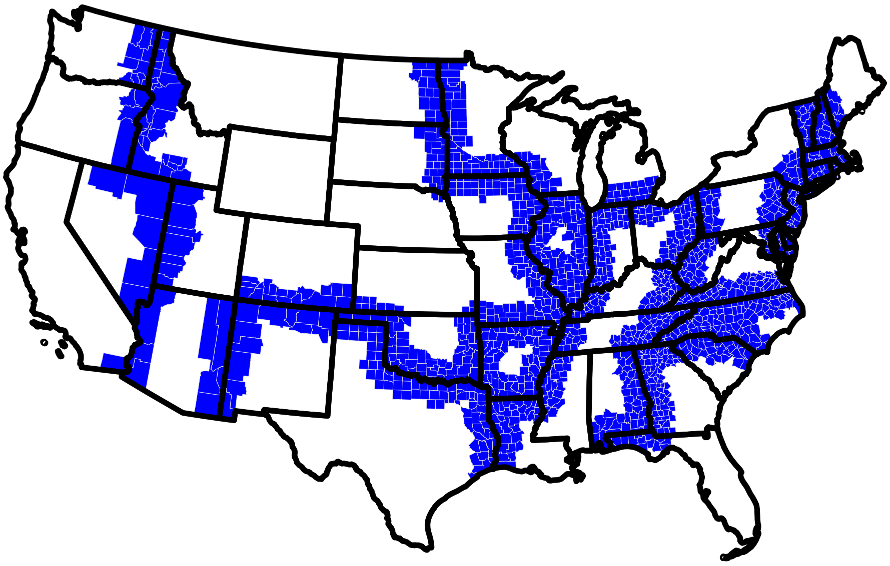
[6]Removing these states, industries, or both simultaneously from the triple difference estimation yields qualitatively aligned results, omitted for brevity but producible upon request.

status. 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. I use geodetic distance to account for the Earth's curvature (Banerjee, 2005).

The running variable is, by county, the shortest distance to another state border of opposite AAI treatment status. Figure 7 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.

Appendix Table 3 shows descriptive statistics of variables employed in difference-in-discontinuity estimations. All descriptive statistics are shown within the optimal bandwidth for the preferred difference-in-discontinuity specification. Variables $\Delta\overline{\text{NumberApprentices}_c}$, 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 new Registered Apprentices was positive between the pre- and post-treatment periods. The number of new Registered Apprentices in the post-treatment period is $e^{0.1} = 1.11$ 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 new Registered Apprentices by county, in the post-treatment period. In the post-treatment period (October 1st, 2015 - September 30th, 2016), the mean number of new Registered Apprentices within a county was 32.09.

Figure 7: 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).

5. Results

5.1. Triple Difference Results

I now turn to triple difference. This subsection considers results of equation (3). Specifically, column (3) of Table 1 displays results from equation (3). Table 1 suggests that the AAI has not significantly affected the number of new 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 0.152. This coefficient is statistically insignificant. Over the first treatment year, the AAI caused a statistically *insignificant* increase of $100(e^{0.152} - 1) = 16.4\%$ Registered Apprentices in each treated state-year-industry cell. The difference in magnitude and significance between the triple difference estimates shown in columns (1), (2), and (3) respectively highlight the importance of state-by-year and industry-by-year fixed effects.

Table 2: Results from Triple Difference Estimations

	(1)	(2)	(3)
Post * Treated Industry * Treated State	0.445***	0.368***	0.152
	(0.124)	(0.106)	(0.142)
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 new Registered Apprentices by state, year, and industry cell. 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), Professional, Scientific, and Technical Services (54), Educational Services (61). The mean of the dependent variable in this estimation sample is 0.98.

Table 6 of the Appendix contains a set of triple difference robustness tests. I first conduct a test for treatment anticipation effect (policy announcement effect) in column (2). I recode treatment as starting not when funds were effectively disbursed by the United States Department of Labor, but when the AAI was first announced (December 11th, 2014). The magnitude of the triple difference coefficient increases but remains insignificant. I do not detect an announcement effect.

In Appendix Table 6, I also extend the treatment period to the full AAI treatment period in column (3), so that the treatment period now is October 1st, 2015, to September 30th, 2020. The triple difference coefficient remains insignificant. Even when considering the full AAI treatment period, potentially confounded by other subsidisation policies, I do not find that the AAI significantly affected the outcome.

Appendix Table 6 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, 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.

Appendix Figure 3 is an event study. It is identical to Figure 5, with the same fixed effect vectors, however it considers the entire period of October 1st, 2005, to September 30th, 2020. Appendix Figure 3 lends credence to the main finding, and corroborates results shown in Figure 5. Appendix Figure 3 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.

Finally, Appendix Table 7 verifies whether certain industries drive baseline triple differences. Appendix Table 7 shows that removing the (treated) professional and technical services industry from the estimation sample lowers the magnitude of the triple difference coefficient by circa one third. Removing the (treated) advanced manufacturing industry in column (4) approximately halves the magnitude of the triple difference coefficient. This may suggest that the AAI may have been more effective in the professional and technical services advanced manufacturing industries. Nevertheless, no triple difference coefficient in Appendix Table 7 is statistically significant. On the other hand, removing both information and healthcare industries in fact largely increases the magnitude of the triple difference coefficient.

Reinforcement of Registered Apprenticeship in IT and healthcare, two treated industries, was particularly difficult; only a small fraction of Registered Apprentices supported by AAI funds was hired in these industries. Copson et al. (2021) suggest three reasons for this. The first reason is that despite efforts, awareness of Registered Apprenticeship among employers in these “non-traditional” industries is relatively low, making firms reluctant to engage in this largely unknown form of training. The second reason is specific to the healthcare industry and concerns a mismatch in state accreditation standards. In many cases, training dispensed by technical colleges, the main providers of RTI in this industry, did not match the structure of OJT certification, provided by training firms. The third reason Copson et al. (2021) set forward is specific to the IT industry, in which employers were concerned that the RTI curricula were not flexible and adaptable enough to reflect rapidly evolving industry needs. Butrica et al. (2023) share an example substantiating the latter concern. 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. Jansen et al. (2017) indeed find that curriculum modernisation increases firms’ demand for apprentices, if the modernisation allows firms to tailor imparted skills to their needs. Thus, curriculum rigidity may be detrimental to employer participation.

Appendix Table 8 contains an additional set of robustness checks pertaining to the triple difference estimation. All are variants of equation (3). In Appendix Table 8, I 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, therefore not concentrated 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 eligible to 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.

The dependent variable in Appendix Table 8 is the number of new Registered Apprentices by state, year, NAICS two-digit industry and O*NET two-digit occupation group. Preferred specifications in Appendix Table 8 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. Even when considering occupational variation in treatment, either within or across industries, I fail to find a statistically significant effect of the AAI.

5.3. Difference-in-Discontinuity Results

I now turn to difference-in-discontinuity and regression discontinuity design. Table 2 displays results from difference-in-discontinuity estimation, following Wang et al. (2023) and Butts (2023). Column (2) of Table 3 depicts results from equation (8). Column (2) of Table 2 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 new Registered Apprentices between the pre- and post-treatment periods within a narrow bandwidth about state borders. When comparing columns (3) and (4) of Table 2, one can see that the inclusion of covariates does not alter inference.

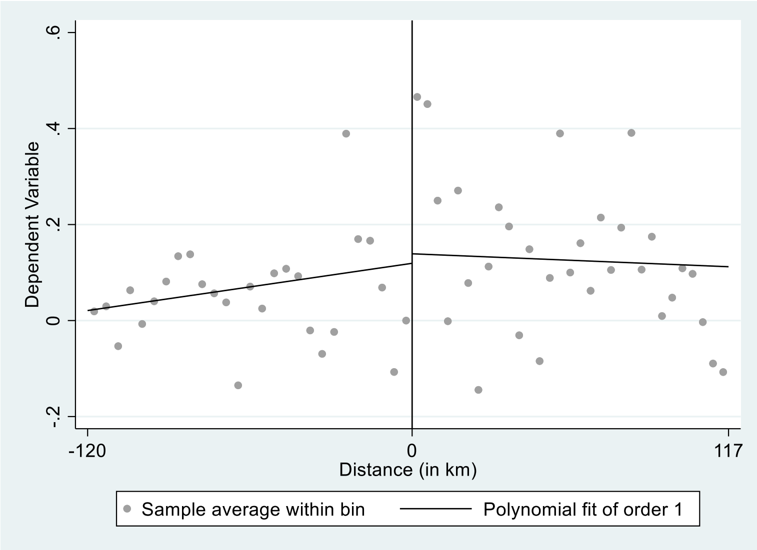
Table 2: Baseline Difference-in-Discontinuity Results

	(1)	(2)
Treated	0.039	0.020
	(0.056)	(0.066)
Covariates	No	Yes
Number of Observations in Optimal Bandwidth	1,864	1,471
Mean Dependent Variable in Optimal Bandwidth	0.0961	0.0964

Notes: Treated denotes an indicator variable assuming the value of one if county c is in a treated state, and 0 else. It corresponds to π in equation (8). 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 $\Delta\overline{\mathrm{NumberApprentices}}_c$. Its construction is detailed in subsection 4. Columns (1) and (2) 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. 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 (2), the mean squared error-minimising bandwidth is 120km in control states, and 117km in treated states.

Figure 8 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.

Figure 8: 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.

Appendix Table 9 contains robustness checks pertaining to difference-in-discontinuity estimation. Column (1) reiterates the baseline difference-in-discontinuity estimate for reference. Appendix Table 7 shares results from a donut regression in column (2). 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.

Appendix 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 Appendix Figure 4, listed in Appendix Table 10). 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, Appendix 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, suggests that over its full period, the AAI has had an economically insignificant effect on the proportional growth in the number of new Registered Apprentices. The mean of the dependent variable, $\Delta\overline{\mathrm{NumberApprentices}_c}$, over the whole period, rises to 0.44. This rise is consistent with the observation that the number of new 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 new Registered Apprentices was 1.55 times larger relative to geometric mean of the number of new Registered Apprentices in the pre-treatment period.

Appendix Table 9 considers treatment falsification as well. The treatment threshold is shifted by 20km into control and then treated states in columns (5) and (6), 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). Appendix Table 9 does not undersmooth (evaluate a bandwidth lower than the MSE-optimal bandwidth), in line with recommendations of Kaliski et al. (2025).

Table 3 depicts results from equation (9). Table 4 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. Columns (1) and (3) quantify a "placebo" effect. This effect is not estimated in baseline difference-in-discontinuity estimation. Within counties located in treated states, it yields the impact of the AAI on control industries. This coefficient will assess compliance of grantees with respect to AAI guidelines concerning industry. Under perfect compliance to AAI guidelines, this effect should be zero. If the "Treated County" coefficient shown in Table 3 is positive and significant, within treated counties, the AAI would have significantly increased the proportional growth in the number of new Registered Apprentices in *non-targeted* industries. This may be caused by imperfect compliance or represent a spillover effect (see Feldman, 1994).

Column (2) of Table 3 complements column (1) but focuses on treated rather than control industries. I compare the magnitude and significance of the coefficients in columns (1) and (2), notably through seemingly unrelated estimation. Second, column (3) of Table 4 gains in efficiency relative to baseline difference-in-discontinuity by taking out an additional difference between treated and control industries *within* treated counties.

In the difference-in-discontinuity specification, with dependent variable $\Delta\overline{\mathrm{NumberApprentices}}_{\mathrm{c,TI}}$ (see subsection 3.4 for its construction), the estimated heterogeneity coefficient shown in Table 3 column (3), is statistically insignificant, with a t-statistic value inferior to 1. I do not find 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. 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 significantly differ from each other. 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 3 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 new Registered Apprentices in control industries, in treated states. The latter denotes, within the optimal bandwidth, the effect of the AAI on treated industries in counties located in treated states, within the optimal bandwidth. 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, within treated counties and across industries, may have been low.

Table 3: 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

Results presented in Section 5 do not indicate that the AAI has had a statistically significant effect on growth in Registered Apprenticeship. This may be because the AAI may have, in truth, not had a significant effect, for reasons discussed in Section 6. Literature has indeed shown that many important non-financial factors affect firms' demand for apprentices, mainly demographics (Muehlemann et al., 2022), or apprenticeship programme duration (Nafilyan and Speckesser, 2019).

The lack of statistically significant discoveries may also arise due to a type 2 error caused by low power. Eight factors may cause low power in the estimations. First, estimation precision may have been hampered by a low number of observations per cell, notably in triple difference regressions (Verbeek and Nijman, 1992), preventing cell means from converging to true population values and lowering efficiency. Second, the process of aggregation pursued over state-year-industry cells induces sampling error. Many observations within the cell are needed to reduce the variance of the sampling error, else imprecise estimates may arise. Third, in triple difference estimation, including three different vectors of interacted fixed effects addresses vast amounts of unobserved heterogeneity, but consumes degrees of freedom and causes the triple difference estimator to rely on solely "residual" variation for identification (Verbeek and Nijman, 1993).

Fourth, low magnitudes uncovered in the results may be due to the relatively small scale of the AAI, discussed in Section 2. The true treatment effect is likely relatively low, requiring high power to detect it. Unfortunately, low power is inversely related to the minimum detectable effect size. In an application to the effect of Medicaid on mortality, Black et al. (2022) show that, through triple difference methodology, with a population treatment effect of 1.53%, statistical power at the 5% significance level is approximately only 51%. Nevertheless, to have 80% power at the 5% level, Black et al. (2022) find that the minimum detectable effect size would have to be 2%.

Fifth, Black et al. (2022) also demonstrate that higher variance in the outcome variable is associated with a higher minimum detectable effect size, for a given level of power. In level form, the outcome variable for triple difference regressions in this paper – the number of new Registered Apprentices by state, industry and year cell – has a variance of 370. This is a relatively high variance, which may contribute to reducing power.

Sixth, 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 because firms and their demand for Registered Apprentices is unobserved.

Seventh, this paper estimates ITT effects and does not observe exactly which firms take-up treatment. Schuenemann et al. (2015) indeed note that statistical power may lack in the settings of certain studies uncovering ITT effects. Eighth, Olden and Moen (2022) show that in triple difference, power increases with the number and especially the proportion of treated clusters, as the proportion of treated clusters approaches half. In this paper however, under the triple difference estimation sample, only 5.4% of state-by-industry clusters are treated. This low proportion arises because only four two-digit NAICS industries are treated, and Connecticut, New York, Washington, Oregon, Massachusetts, Wisconsin, and Washington D.C (all treated) are all missing from triple difference estimation due to missing industry data.

6. Discussion of Results

The AAI may not have in truth statistically significantly affected the number of new Registered Apprentices for at least five reasons. First, AAI grantees did not discriminate between firms according to firm size. Small firms are limited in the number of new 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 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. In addition, firms face substantial training set-up costs. AAI subsidies may have been insufficient to compensate employers *a posteriori* for incurring these costs and offering training (Lerman et al., 2022). This limits subsidies’ effectiveness on extensive training margins.

Second, AAI grantees did not discriminate according to firms’ prior training status. The AAI allowed funds to 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 an upper bound on the number of new Registered Apprentices they can train, for the reasons mentioned above.

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. Copson et al. (2021) confirm findings of the Office of

Inspector General (2021). The former authors, through interviews with a select number of AAI grantees, confirm that AAI funds were spent according to local industry needs and prevalence rather than federal Department of Labor guidelines.

Appendix Table 11 exemplifies the lack of compliance on the industry level. It shows results of triple difference equation (4), with indicators for groups instead of interacted fixed effects. The coefficient on "Treated State * Post" denotes the effect of the AAI on the log of new Registered Apprentices in treated states, in the post treatment period, in *control* industries. The coefficient is positive and statistically significant, suggesting that the AAI has had a positive effect on the growth in the number of new Registered Apprentices in control industries. This suggest that AAI fund distribution did not comply with federal Department of Labor guidelines regarding industry-targeting, as suggested by the findings of Copson et al. (2021) and Office of Inspector General (2021).

Fourth, under high worker mobility, firms have an incentive to externally hire skilled job changers instead of training its own labour (Chang and Wang, 1996). 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 18.4% of Registered Apprentices completed their Registered Apprenticeship in 2021 (Madjd-Sadjadi and Slater, 2025). 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.

Fifth, training subsidies' aim is to depress training costs. Since the elasticity of propensity to train with respect to training costs is negative (Brebion, 2020), lowering training costs should increase the provision of Registered Apprenticeship. Nevertheless, in line with the findings of Fenizia, Li and Citino (2024) for Italy, when asked what the largest barriers to Registered Apprenticeship participation are, United States employers do not state training costs. The top three major barriers, in descending order, are lack of awareness amongst employers about Registered Apprenticeship, lack of the public's understanding regarding how to enter the Registered Apprenticeship system, and employer concerns about Registered Apprenticeship (e.g. quality) (Rosenberg and Dunn, 2020).

7. Concluding Remarks and Policy Recommendations

This paper analyses the causal impact of training subsidies on the number of new 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, which was highlighted by Kuczera (2017) and Mueller and Behringer (2012). Descriptive reports evaluating the AAI's effectiveness exist (National Governor's Association, 2020, Copson et al., 2021, Fumia et al., 2022). Such reports however measure the effectiveness of the AAI using the number of new Registered Apprentices whose training was partially financed with AAI funds. This neglects the counterfactual scenario, i.e. the *additional* number of new Registered Apprentices added *because of* AAI subsidies. Addressing this gap is this paper's original contribution.

I find that the American Apprenticeship Initiative has not led to a statistically significant increase in the growth of the number of new Registered Apprentices. Difference-in-difference, triple difference, regression discontinuity design and spatial difference-in-discontinuity yield aligned results. Through difference-in-discontinuity, I also find that within counties located in treated states, within a narrow bandwidth about the border, the AAI did not have a significantly stronger effect on the number of new Registered Apprentices in treated industries.

This study has four 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. I discuss potential reasons for this low power in Section 5. Second, the RAPIDS dataset does not allow the observation of individual firms. I cannot identify which firms eventually received and spent AAI funds. Effects estimated are ITT effects, rather than the average treatment effect on the treated (ATT) of subsidies on treated firms. ITT effects are nonetheless of interest to policymakers, as take-up and treatment participation of given firms cannot be perfectly enforced (Albanese et al., 2024).

Third, subsidies were not allocated randomly. Causal inference methods are used in this paper to address potential non-random selection into treatment, unobserved confounders and omitted variables. However, I cannot exclude potential bias. Fourth, and related to the third point, the presence of confounding policies, such as the State Apprenticeship Expansion, shortly after the start of the AAI is a limitation. Following Bertrand et al. (2004), I thus reduce the length of the analysis window's length around the treatment to avoid capturing effects of confounding policies. While I do consider the full AAI treatment period in robustness checks and obtain aligned results, these estimates may encompass effects of the State Apprenticeship Expansion Initiative or the Youth Apprenticeship Readiness Act (United States Department of Labor, 2019). The scope of these policies is relatively ill-defined. One may consider a staggered model across states and time, nevertheless this would preclude the use of state-by-time fixed effects, which are crucial to capture unobserved confounders.

Despite these limitations, results uncovered in this paper have key policy implications. First, centralising Registered Apprenticeship standards under one federal authority would permit regulatory alignment and lift an impediment to multi-state Registered Apprentice sponsors. This may in turn alleviate bureaucratic costs, which many employers state as a barrier to offering training programmes (OECD/ILO, 2017, Gardiner et al., 2021, Copson et al., 2021). Second, the establishment of occupation or industry-wide professional organisations can strengthen the Registered Apprenticeship ecosystem by leveraging pooled occupational expertise to advise employers on standardised curricula and by organising intercompany courses for specialised competencies that individual firms cannot deliver in-house. Resource-pooling also creates economies of scale. Third, making training firms more visible may increase apprenticeship positions by improving the reputation of training firms with clients and potential workforce. 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

More research is needed on the effect of subsidies on training. To surmount limitations faced in this paper, future research may employ firm-level administrative panel data. If subsidies are not distributed randomly, then future research should compare outcomes in firms who applied to receive subsidies, and were successful (treatment group), to firms who also applied to receive subsidies, but were unsuccessful (control group). Having firm-level data would also allow to identify treatment effect heterogeneity of subsidies as a function of various firm characteristics. Researchers could also distinguish between the effects of subsidies on intensive and extensive margins, respectively.

Firm-level panel data would enable researchers to leverage within-firm variation, eliminating firm-level confounders, greatly reducing residual variation and increasing statistical power. The average treatment effect of subsidies on treated firms could additionally be uncovered. Furthermore, future research should consider demand for Registered Apprentices as dependent variable, i.e. the sum of filled and vacant positions. Researchers could distinguish between the effect of subsidies on intensive and extensive training margins. Finally, future research may evaluate the effect of the AAI on other outcomes, such as Registered Apprentice completion rates for instance.

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Appendix: Tables

Table 1: AAI Grantees List and Information

Recipient name	Business type description	Total funding amount ($)	State of performance
Arkansas Division Of Workforce Services	State Government	637220.24*	Arkansas
Workforce Development Board Of Herkimer, Madison And Oneida Counties, Inc	Nonprofit With 501C3 Irs Status (Other Than An Institution Of Higher Education)	2497530.74	New York

William Rainey Harper College	Public/State Controlled Institution Of Higher Education	2486361.76	Illinois
West Central Job Partnership, Inc.	Nonprofit With 501C3 Irs Status (Other Then Institution Of Higher Education)	2998625.0	Pennsylvania
Vermont Department Of Labor	State Government	2956897.85	Vermont
State Of Oregon Employment Department	State Government	2116618.28	Oregon
State Of Hawaii Department Of Labor And Industrial Relations	State Government	1644224.31	Hawaii
Southeast Michigan Community Alliance (Semca)	Nonprofit Without 501C3 Irs Status (Other Than An Institution Of Higher Education)	3989243.49	Michigan
Shenandoah Valley Workforce Investment Board, Inc.	Nonprofit With 501C3 Irs Status (Other Than An Institution Of Higher Education)	3962974.73	Virginia
San Francisco, City And County Of	County Government	2994268.7	California
Philadelphia Works	Nonprofit With 501C3 Irs Status (Other Than An Institution Of Higher Education)	2883555.94	Pennsylvania
Milwaukee Area Workforce Investment Board	Nonprofit With 501C3 Irs Status (Other Then Institution Of Higher Education)	2999591.0	Wisconsin
Managed Career Solutions, Inc.	Other	2867956.35	California
Macomb Community College	Public/State Controlled Institution Of Higher Education	3728731.53	Michigan
Homework Hangout Club, Inc.	Nonprofit With 501C3 IR Status (Other Then Institution Of Higher Education)	2989850.0	Illinois
Technical College System Of Georgia	State Government	2866709.8	Georgia
Focus: Hope	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	2968708.15	Michigan
Executive Office Of Labor And Workforce Development	State Government	2691172.07	Massachusetts
Economic Development And Industrial Corporati	City Or Township Government	2999999.0	Massachusetts
Board Of Regents, Nshe, Obo Truckee Meadows Community College	Public/State Controlled Institution Of Higher Education	2629487.96	Nevada
Alaska Department Of Labor And Workforce Development	State Government	2749897.14	Alaska
Mission College	Public/State Controlled Institution Of Higher Education	2871717.83	California
Illinois Manufacturers' Association Education Foundation	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	2134589.11	Illinois
International Brotherhood Of Teamsters	Labor Union	3702920.68	District Of Columbia
J. Sargeant Reynolds Community College	Public/State Controlled Institution Of Higher Education	2862473.29	Virginia
Npower, Inc.	Nonprofit With 501C3 IRS Status (Other Then Institution Of Higher Education)	3668460.0	New York
United Way Of Buffalo & Erie County	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	2313573.32	New York
Milwaukee Institute Of Art & Design	Private Institution of Higher Education	1408452.15	Wisconsin
Central New Mexico Community College	Public/State Controlled Institution Of Higher Education	2992838.31	New Mexico
National Governors Association	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	2468225.0	District Of Columbia
Able-Disabled Advocacy, Inc.	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	3200226.7	California
Wisconsin Department Of Workforce Development	State Government	4684997.7	Wisconsin
Washington State Department Of Labor And Industries	State Government	3611070.67	Washington
Uaw-Labor Employment And Training Corporation	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	2376190.04	California
The Providence Plan	Nonprofit With 501C3 IRS Status (Other Then Institution Of Higher Education)	764006.43	Rhode Island
South Seattle College	Public/State Controlled Institution Of Higher Education	4684643.87	Washington

Sc State Board For Technical And Comprehensive Education	Public/State Controlled Institution Of Higher Education	4947787.88	South Carolina
Electrical Training Alliance	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	4523605.07	Maryland
Minnesota Department Of Employment And Economic Development	State Government	4098141.7	Minnesota
Marshall University Research Corporation	Public/State Controlled Institution Of Higher Education	4939584.21	West Virginia
Los Rios Community College District	Public/State Controlled Institution Of Higher Education	4999999.99	California
Jobs For The Future	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	4999999.9	Massachusetts
International Transportation Learning Center	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	3943615.57	Maryland
Houston Community College	Public/State Controlled Institution Of Higher Education	4254067.6	Texas
Connecticut Department Of Labor	State Government	5000000.0	Connecticut
Florida State College At Jacksonville	Public/State Controlled Institution Of Higher Education	4975838.98	Florida
Ahima Foundation	Nonprofit With 501C3 IRS Status (Other Than An Institution Of Higher Education)	4819244.57	Illinois
Total Amount		**$153,905,895**	

Notes: *: For Arkansas, the amount shown is the total outlayed amount. Source: USA Spending (2015).

Table 2: Allowable Use of American Apprenticeship Initiative Grant Funds

Activity	**Allowable Use of American Apprenticeship Initiative Grant Funds**
On the job Training	Reimburse overhead costs associated with training provision, shadowing, mentoring and supervision
RTI	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	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 3: Descriptive Statistics –Spatial Difference-in-Discontinuity in Optimal Bandwidth of Difference-in-Discontinuity

Variable	Variable Explanation	Mean (Pooled Sample)	Mean (Treated Counties)	Mean (Control Counties)	Difference (Control - Treated)
Dependent Variable					
$\Delta\overline{\text{NumberApprentices}_c}$	Construction of Variable Described in Subsection 4.3.	0.100	0.120	0.070	-0.050
Covariates					
Share of Women	County-Level Average Share of Women (%) in 2010	50.32	50.32	50.32	0.000
Share of High School Graduates	County-Level Average Share of High School Graduates (%) in 2010	82.19	82.72	81.70	-1.020***
Share of Bachelor Degree Holders	County-Level Average Share of bachelor's degree Holders (%) in 2010	18.45	19.32	17.64	-1.680***
Mean Travel Time to Work	County-Level Average Travel Time to Work, in minutes, in 2010	23.34	23.82	22.91	-0.910***
Democrat Vote Share	County Democrat Vote Share in 2012 Presidential Election (%)	39.94	41.07	38.89	-2.180***
Log(Per-Capita Income)	County Log(Per-Capita Income) in 2010	9.980	10.01	9.950	-0.060***
Log(Civilian Labour Force)	County Log(Civilian Labour Force) in 2010	9.690	9.790	9.600	-0.190***
Log(Employed)	County Log(Employed) in 2010	9.590	9.690	9.500	-0.190***
Share of Non-Hispanic Whites	County Share of Non-Hispanic Whites in 2010 (%)	79.21	77.50	80.80	3.300***

Notes: The number of observations in the difference-in-discontinuity sample, within the bandwidth used, is 1,471. In the treated counties column, the number of observations is 709. In the control counties column, the number of observations is 762. The optimal bandwidth is 120km in control states, and 117km in treated states. All variables shown in Table 6 are sourced from ApprenticeshipUSA (2024), United States Department of Agriculture (2024). Section 3.3 describes the construction of the dependent variables in the difference in discontinuity ($\Delta\overline{\text{NumberApprentices}_c}$) specifications. The share of bachelor's degree holders closely resembles that of McElrath and Martin (2021) in 2005-2009. Standard deviation is in parentheses below the mean. The last column conducts a mean-comparison t-test between treated and control counties. The last column displays results from a mean comparison test using a t-test assuming unequal variances. *p<0.1, **p<0.05, ***p<0.01. The last column contains results of a two-sided mean comparison test, assuming unequal variances.

Table 4: 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 Democrat 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 difference-in-discontinuity estimates. All estimates test the balancedness of county-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. Column titles correspond to the covariate being used as dependent variable in the corresponding specification. Each covariate is described in Appendix Table 3, 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.

Table 5: Descriptive Statistics in Triple-Difference Estimation Sample

Variable Name	Variable Explanation	Mean Pooled Sample (Std. Dev. in parentheses)	Mean Treated States (Std. Dev. in parentheses)	Mean Control States (Std. Dev. in parentheses)	Difference (Control Mean – Treated Mean)
Dependent Variable					
NumberApprentices	Number of new Registered Apprentices by state, year and NAICS industry	58.3	80.9	41.1	-39.8***
		(370)	(503)	(219)	

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. ***p<0.01, **p<0.05, *p<0.1. The last column contains results of a mean comparison test, assuming unequal variances.

Table 6: 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	0.152	0.177	0.135	0.048	0.170
	(0.142)	(0.128)			
			(0.134)	(0.116)	(0.143)
Observations	7,392	7,392	11,616	2,112	15,840
Mean of Dependent Variable	0.98	0.97	1.22	1.26	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 new 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 reference. 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.

Table 7: Sequentially Omitting Industries in Triple Difference Estimation

	(1)	(2)	(3)	(4)	(5)	(6)
Treated State * Treated Industry * Post	0.152	0.106	0.139	0.0695	0.216	0.229
	(0.142)	(0.159)	(0.140)	(0.163)	(0.158)	(0.145)
Mean of Dependent Variable in Estimation Sample	0.980	1.012	0.927	0.901	1.014	0.996

Notes: Standard errors clustered by state in parentheses. This Table shows triple difference regressions, sequentially omitting AAI-treated industries (the last column omits a control industry). N=7,084. ***p<0.01, **p<0.05, *p<0.1. The first column reiterates the baseline estimate. Column (2) removes the professional and technical services industry (NAICS code 54). Column (3) removes educational services. Column (4) removes advanced manufacturing (NAICS code 33). Column (5) removes the information industry. Column (6) removes the healthcare and social assistance industry.

Table 8: 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)	0.058		0.049		0.038		0.059*		0.048	
	(0.037)		(0.034)		(0.033)		(0.033)		(0.032)	
Triple Difference (Treated Occupation *or* Industry)		0.022*		0.014		0.005		0.020*		0.011
		(0.012)		(0.011)		(0.011)		(0.011)		(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 new 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 is 0.08. "Triple Difference (Treated Occupation and Industry)" refers to the triple difference coefficient γ_1 in equation (2). However, in the first row, 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 AAI-treated industries of healthcare (NAICS code 62), information (NAICS code 51), professional and technical services (54), educational services (61), and advanced manufacturing (NAICS code 33).

Table 9: Difference-in-Discontinuity Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Donut Difference in Discontinuity	Metropolitan area Difference-in-Discontinuity	Extending treatment window until 2020	**Placebo:** Shifted Distance Threshold in Control States	**Placebo:** Shifted Distance Threshold in Treated States
Treated	0.020	0.700	0.850	0.007	-0.044	-0.041
	(0.07)	(0.74)	(1.29)	(0.06)	(0.063)	(0.060)
Mean Dep. Var. in optimal bandwidth	0.096	0.089	0.219	0.445	0.102	0.095
Observations within optimal bandwidth	1,471	494	41	1,548	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 $\Delta\overline{\text{NumberApprentices}}_c$. 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 6. 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 10). In column (4), I employ the full sample of data, i.e. October 1st, 2009, until September 30th, 2020. Columns (5) and (6) are placebo regressions. In column (5), the threshold for treatment is shifted by 20km inside control states. In column (6), the threshold for treatment is shifted by 20km inside treated states.

Table 10: 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).

Table 11: Triple Difference Regression – Spillover Effects Estimated

	(1)
Post	0.241***
	(0.0314)
Treated Industry	0.223**
	(0.0977)
Treated State	0.130
	(0.144)
Treated State * Post	0.168**
	(0.0761)
Treated Industry * Post	0.216**
	(0.0908)
Treated Industry * Treated State	0.247
	(0.172)
Treated Industry * Treated State * Post	0.152
	(0.140)
Observations	7,392

Notes: Standard errors clustered by state. *** p<0.01, ** p<0.05, * p<0.1. This Table displays results from equation (4), nevertheless I specify now equation (4) as a saturated model with a dummy for each group instead of interacted fixed effects, as highlighted in footnote 3.

Appendix: Figures

Figure 1: Heat Map of State-Level AAI Grants (Individual AAI Grantee Funds Summed by State)

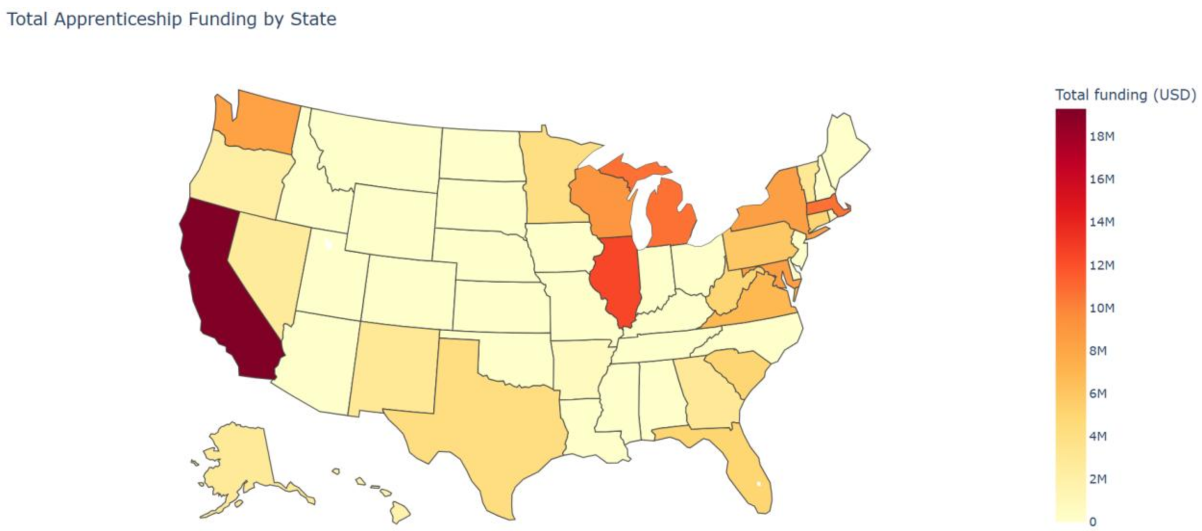
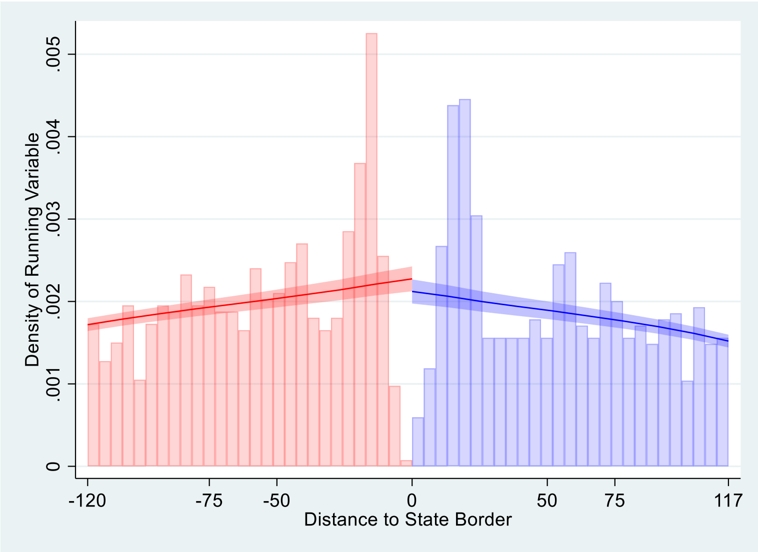
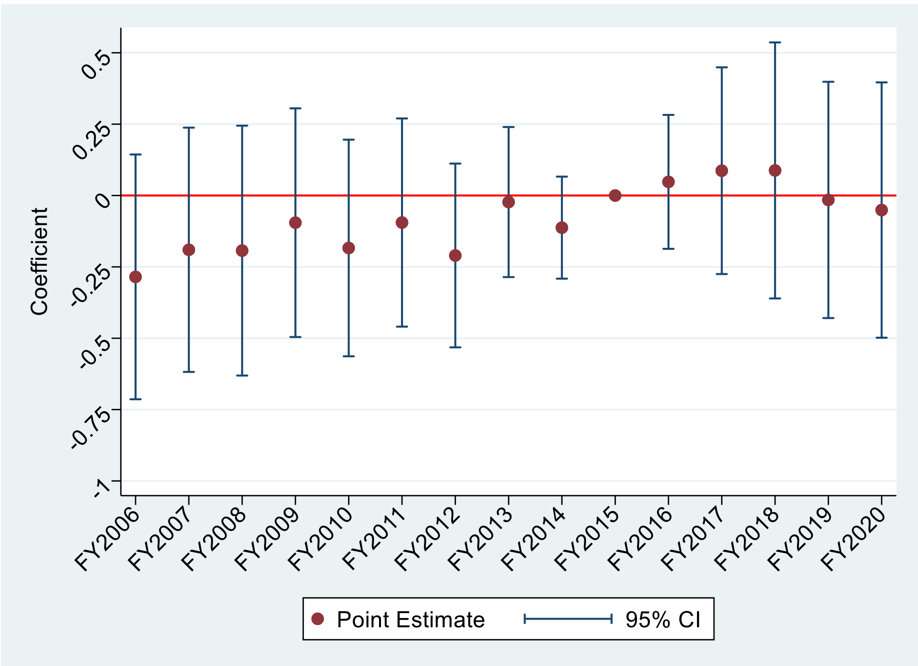


Figure 2: Manipulation Test in the Running Variable



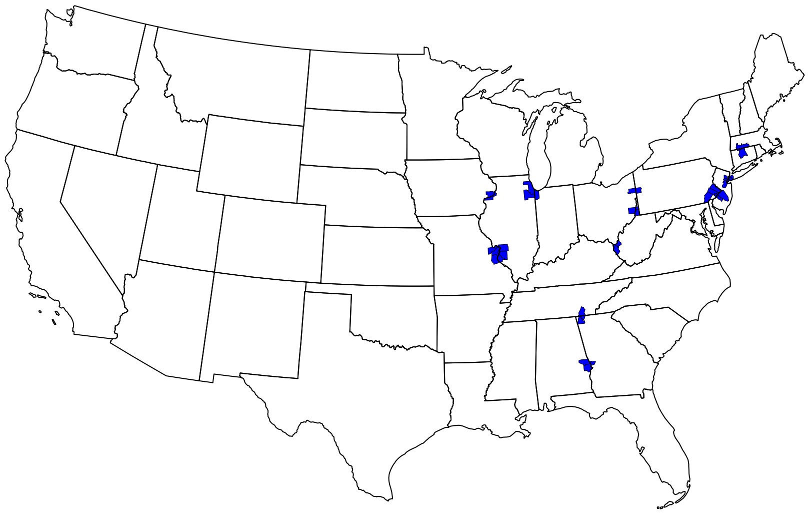
Notes: This Figure tests the hypothesis of no manipulation or bunching in the running variable at the threshold. The running variable is shown on the X-axis. Using methodology of Calonico et al. (2017), this graph tests the null hypothesis that the density of the running variable is continuous at the 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 3: 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 new Registered Apprentices by state-year-industry cell with respect to the AAI. Blue bands denote 95% confidence intervals. Standard errors are clustered by state.

Figure 4: 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 12. These cross-state metropolitan areas are sourced from Grant (1955). The are listed in Table 12.