Wage subsidies and the reliance of youth on welfare payments

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Abstract

I estimate the treatment effects of Youth Jobs PaTH (PaTH) and Youth Wage Subsidy (YWS) programs in Australia. The goal of these programs is to improve the employment prospects of unemployed youth. PaTH has three components: Employability Skills Training (EST), Internship, and Youth Bonus Wage Subsidy (YBWS) of up to A$10,000. It is not necessary to complete all three components. Max subsidy for YWS is A$6,500. The subsidies are provided to the firms who hire unemployed youth. The original contribution of the paper is the comparison of treatment effects of these two programs and this comparison can help policy makers with the design of wage subsidies. The treatment effects of 'eligibility for' and 'participation in' YWS (versus no intervention) on the probability of unemployed youth exiting welfare system are 0.5 pp and 3.0 pp respectively (for up to 23 months in the future). The effects for PaTH are 2.0 pp and 9.4 pp respectively (about 3 to 4 times larger than YWS). The effect sizes are in line with the literature and are economically meaningful, but many estimates are statistically insignificant at the usual levels.

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1 Introduction

Wage subsidies are financial incentives provided to firms for hiring unemployed jobseekers or to help firms cover part of the cost of existing employees. These subsidies are widely used to stimulate the economy during downturns[1], can increase efficiency in economy under certain circumstances (such as institutional and information frictions)[2], and can be targeted at vulnerable workers (such as youth, parents returning to work, long term unemployed)[3] to reduce inequality in employment. Wage subsidies targeted to unemployed youth deserve a closer attention because the labor market outcomes of youth in Australia have been deteriorating after the 2008 financial crisis and youth have also been disproportionally affected by COVID[4]. Poor labor market outcomes for youth due to these adverse macroeconomic conditions can have long term negative impacts on youth (scarring effects (Borland, 2020)) and youth wage subsidies can potentially alleviate these long term problems by improving the current employment outcomes for youth. This paper investigates whether wage subsidies can improve outcomes for youth by estimating treatment effects of two recent programs in Australia: Youth Jobs PaTH (PaTH) and Youth Wage Subsidy (YWS) programs.

The aim of PaTH and YWS is to improve the employment prospects of unemployed young jobseekers. PaTH started in Jan 2017 and has three components: Employability Skills Training (EST), Internships, and Youth Bonus Wage Subsidy (YBWS). EST pro-

[1]Recent Australian examples: The Jobkeeper payment was provided to firms during COVID to help firms retain existing employees. As a followup to Jobkeeper payment, the JobMaker Hiring Credit provides A$200 (100) per week for upto a year when firm hires and retains an unemployed person under 30 (35) years of age. The Victorian state government introduced New Jobs Tax Credit in late 2020. By reducing the payroll tax liability of firms, the tax credit helps firms with hiring new workers, retaining existing workers, and restoring usual worker hours. A good sample of recent subsidy programs in other countries can be found in the list of papers used by Card et al. (2017) in their meta analysis.

[2]See Bell et al. (1999) and Borland (2016) for theoretical details.

[3]At the time of writing this paper, Australia have wage subsidies for each of these three groups as well as for other vulnerable groups such as seniors (age 50+), people with disability, and indigenous people. The author thanks Carmel Regan at the Department of Employment for the list of current wage subsidies in Australia.

[4]From 2008 to 2019, the employment rate of youth (those aged 15 to 24) has decreased by 4.3 percentage points whereas the employment rate of those aged 25 or above has increased by 1 percentage point (Borland & Coelli, 2020). During the same period the average weekly hours worked, conditional on being employed, by 15-24 and 25-35 age groups have decreased by 2.5 and 2.0 hours respectively whereas it has remained the same for those aged 35 or above; this decrease is even more problematic considering that youth (age 15 to 24) initially worked about 10 hours / week less than other age groups (authors own calculation using the HILDA data). During the COVID crisis, the initial fall in employment was four times larger for youth as compared to the overall working age population (Kabátek, 2020) although the speed of recovery of employment rate for youth was much faster once the COVID restrictions started easing (Borland & Charlton, 2020).

vides soft skills valued by employers such as communication, team work, and planning and executing tasks. It also prepares jobseekers for recruitment by improving their interview skills and CVs. In addition, EST may include visits by employers and group visits to employers which can help jobseekers choose a career path that suits their skills and interests. Internships provide an opportunity to gain work experience. PaTH internships provide a one time payment of upto A$1000 to incentivize firms to host interns and A$200 per fortnight to jobseekers on top of their regular fortnightly welfare payments while the internship lasts. The internship lasts between 4 to 12 weeks. YBWS provides firms upto A$10,000 over a six months periods when firms hire an eligible unemployed youth under 25 years of age. Note that YBWS provides 'firms' an option to get the subsidies when they hire the eligible unemployed youth; the subsidies are provided to firms and not to the unemployed jobseekers. YWS targets a slightly higher age group (ages 25 to 29) than YBWS and gives firms a subsidy of upto A$6,500 when firms hire an unemployed youth in this slightly higher age group.

PaTH 'treatment' is making unemployed young jobseekers eligible for the three components of PaTH. But it is not necessary for jobseekers to complete all three components of PaTH; jobseekers may not commence any component or they may commence different combinations for the three components such as EST followed by YBWS. Since its start till 28 February 2019, about 66800 individuals have participated in at least one component of PaTH (20% participation rate) with approximately 52000, 9200, and 38000 participants in EST, Internships, and YBWS respectively (DESE, 2020). So many PaTH participants complete both EST and YBWS. The YWS 'treatment' is making unemployed youth eligible for subsidies i.e. firms have the option to obtain subsidies when they hire these unemployed youth. From Jan 2017 to Dec 2018 about 15000 individuals participated in YWS - 14% participation rate (Senate Estimates, 2019a).

The goal of the paper is to estimate the treatment effects of the two programs and compare the treatment effects. I exploit eligibility rules of the two programs to estimate the treatment effects of the programs. Figure 1 shows that age determines eligibility: individuals age 15 to 24 (inclusive) are eligible for PaTH, individuals age 25 to 29 (inclusive) are eligible for YWS, and individuals age 30 or above are ineligible for both PaTH and YWS. Eligibility based on age is only true in the analysis sample and the data section provides details of how this analysis sample is constructed.

Table 1 summarizes the treatment effects estimated in this paper. It tells us the treatment effect being estimated, the type of treatment effect, age groups being compared for estimation, and the methodology used for estimation. For example, row 2 asks what is the treatment effect of being 'eligible' for YWS versus no intervention. A person eligible for YWS may not necessarily commence a subsidized job (i.e. may not participate) and so the treatment effect in row 2 is an Intention to Treat (ITT) effect. The treatment effect in row 2 is estimated by comparing individuals above and below the cutoff age 30 using sharp Regression Discontinuity and Diff-in-Diff research designs. Similarly, row 3 asks what is the ITT effect of being eligible for PaTH vs no intervention. Comparing the treatment effects in rows 2 and 3 gives us an idea of the relative effectiveness of the two programs. Instead of indirect comparisons using rows 2 and 3, row 1 directly compares age groups eligible for PaTH and YWS and has the advantage of having standard errors for the comparison. Rows 4 and 5 ask what are the treatment effects on the participants of these two programs i.e. what is the Average Treatment Effect on the Treated (ATET). The ATET estimates are only an approximation because the data does not contain individual level participation information. Aggregate participation rates from official reports are used to scale up the ITT estimates in rows 2 and 3 to provide approximate ATET effects in rows 5 and 4 respectively. Comparing the treatment effects in rows 4 and 5 gives us an idea of the relative effectiveness of the two programs for participants.

ITT is important to estimate for four reasons. First, ITT is relevant to policy. ITT gives the effect on 'average' individual made eligible; those who participate may have very different characteristics than the 'average' individual. Also ITT is a relevant quantity to measure if government can only provide option of subsidies to firm and cannot force firm to get the subsidies. Second, past studies mostly use matching techniques to estimate the ATET of participation in wage subsidies and must assume that the techniques address self selection into programs. ITT estimates the effect of eligibility and the eligibility rules of PaTH and YWS prevent individuals to self select into eligibility. So, the ITT estimates are much more robust to selection issues. Third, I lack individual level participation information and can only provide approximate ATET effects. To provide these approximate ATET effects, I first need to estimate ITT. Fourth, it is consistent with recent wage subsidies literature to estimate ITT rather than ATET[5].

[5]Boockmann et al. (2012), Huttunen et al. (2013), Schünemann et al. (2013), Sjögren & Vikström (2015), Pasquini et al. (2019) all estimate ITT effect of wage subsidy programs.

Table 2 provides a summary of main results. The outcome variable is a dummy equal to 1 if individual is on income support s months after becoming eligible s = {1,2,...,23}. The treatment effects are estimated for each s and the table shows the average effect across 23 months. For comparison, 7% of control group in rows 2 to 5 are 'off' income support 1 month after eligibility and 40% after 23 months. So a 2 pp ITT effect in row 3 is large compared to 7% but small relative to 40%. The ITT effect of YWS in row 2 is both small in magnitude and statistically insignificant at 5% level. The ITT effect of PaTH is 4 (using rows 2 and 3) to 3 (using rows 1 and 2) times larger than the ITT effect of YWS but this difference is only marginally statistically significant at 5% level. The ATET effect of YWS in row 5 is statistically insignificant at 5% level. The 9.4 pp ATET effect of PaTH in row 4 is statistically significant. The difference in employment rates of a person with high school vs a certificate level education in Australia is about 10 pp (Borland & Coelli, 2016) so a 9.4 pp ATET is economically meaningful effect. The ATET estimates are comparable to international estimates of about 6 pp in the medium run (12 to 24 months) (Card et al. (2017)) and the small ITT effects of wage subsidies are also common in the literature[6].

The first contribution of the paper is comparison of two different programs which may help policy makers when they are designing wage subsidy programs. This is the first paper estimating the effect of package (training, internship, higher subsidy amount) vs lower subsidy amount. Existing literature has compared the effect of packaged program (jobsearch assistance, training, work experience, and wage subsidies) vs control (Blundell et al. (2004), De Giorgi (2005)) but not the effect of package vs subsidy. Jaenichen et al. (2011) estimates the effect of package (training + subsidy) vs training but not the effect of package vs subsidy. Sjögren & Vikström (2015) estimates the effect of doubling the subsidy amount and the effect of doubling the subsidy duration but it does not answer what happens when we increase the subsidy amount and also provide trainings and internships. Policy makers now have evidence on the effectiveness of different design options which can help them decide how to design wage subsidy programs.

The second contribution of the paper is that it estimates the ITT effects at short- (less than 1 year), medium- (between 1 and 2 years), and long- (more than 2 years) run

[6]Boockmann et al. (2012), Huttunen et al. (2013), Schünemann et al. (2013), Sjögren & Vikström (2015), Pasquini et al. (2019) find ITT effects of wage subsidies to be between 0 and 2 percentage points.

horizons whereas most other ITT studies of wage subsidies only consider short run effects[7]. This study also estimates the effects at very high frequency i.e. in each month after a person becomes eligible for programs. This has advantage over studies[8] which estimate effects at one or two points because it is possible that the estimates at a single point in time are large by chance; having estimates at high frequency for a long horizon can help discard this possibility.

The third contribution is Australian specific. DESSFB (2019, page 1) reports that "More analysis is required to determine the causal impact of PaTH.". This paper is a first systematic effort in estimating the causal impact of PaTH and is hence useful in evaluating PaTH if policy makers are hesitant to use evidence from other countries because of concerns about external validity. This paper is also estimating the effect of a recent wage subsidy program whereas past Australian studies have focused on programs before 1998 (Richardson (1998), Knight (2002), Johnston (2007)).

The rest of the paper is organized as follows. The next section provides institutional details and discusses how the eligibility rules for PaTH and YWS programs can be used for identification of causal effects. Section 3 introduces the data, explains how the analysis sample and outcome variable is constructed, provides descriptive evidence for difference in outcome variable across groups, and talks about the strengths and weaknesses of the data. Section 4 provides details of the methods used to answer the research questions. Main results are shown and discussed in section 5. Identification assumptions of the methods are tested in section 6. Section 7 performs various robustness checks. The paper concludes by discussing how well the paper has answered the research questions posed in the introduction and then provides some policy implications of the results and directions for future research.

2 Insitutional Setup

The Youth Jobs PaTH program was announced in the 2016-17 Australian Budget. The goal of the PaTH program is to improve the employment prospects of youth via its three

[7]Boockmann et al. (2012), Schünemann et al. (2013), Huttunen et al. (2013), Pasquini et al. (2019).

[8]Examples: Richardson (1998) and Knight (2002) use survey data which asks questions at specific points in time many months after program participation. Schünemann et al. (2013) estimates the effect either almost immediately after eligibility or 12 months after eligibility. Pasquini et al. (2019) only estimates effect immediately after a person becomes eligible. Huttunen et al. (2013) measures outcome based on end of year employment status.

components: (1) Employability Skills Training (EST) (2) Internships (3) Youth Bonus Wage Subsidy (YBWS). EST provides youth the skills valued by employers, Internship provides youth work experience, and YBWS provides financial incentives to *firms* to hire unemployed youth. YBWS started on 1 Jan 2017, and the other two components on 1 April 2017.

An unemployed jobseeker is eligible for YBWS (i.e. employer will get subsidies for hiring the jobseeker) if, at the time of hire, the person

1. is aged 15 to 24 (inclusive) and

2. has been continuously receiving assistance from employment services providers for at least the previous six months.

When we restrict our attention to those unemployed jobseekers who meet the second eligibility rule i.e. the rule (2) regarding duration of assistance, then age (at the time of meeting the second eligibility rule) completely determines who is eligible for YBWS[9]. Unless otherwise specified, the second eligibility rule mentioned in this paper always refer to the eligibility rule (2) mentioned above. Virtually everyone eligible for YBWS is eligible for the other two components of PaTH i.e. EST and PaTH Internships[10].

Youth Wage Subsidy (YWS) started on 1 July 2015 (notice the missing word 'Bonus') and have the same second eligibility rule as YBWS. From 1 July 2015 to 31 Dec 2016, those aged 15 to 29 (inclusive) were eligible for YWS. Since the start of YBWS on 1 Jan 2017, those aged 25 to 29 (inclusive) are eligible for YWS. This means that once we restrict our sample to jobseekers meeting the second rule on or after 1 Jan 2017, then those aged 15 to 24 (inclusive) are eligible for PaTH package (EST, internships, YBWS), those aged 25 to 29 (inclusive) are eligible for YWS only, and those aged 30 or above are ineligible for both PaTH package and YWS (age is calculated at the time a person meets the second rule). Figure 1 summarizes this point visually. I estimate different treatment

[9]De Giorgi (2005) uses a similar approach to define eligible and ineligible groups.

[10]Age requirement for PaTH internship is 17 to 24 (inclusive), so those aged 15 and 16 and eligible for YBWS are not eligible for internships. This is not a problem is my analysis because I use observations close to age 25 while estimating my models (i.e. observations aged 15 and 16 are not used). Also, YBWS is available for jobseekers receiving help from jobactive and Transition to Work (TtW) employment services providers but EST is only available to those receiving assistance from jobactive. This is a minor issue because the caseload of TtW is very small as compared to jobactive; approximately 95% of my sample consist of individuals receiving help from jobactive and only 5% from TtW. In addition, those receiving help from TtW get similar training as EST. Jobactive is a general service provider whereas TtW provides specialized service to some youth.

effects by comparing the three groups in figure 1[11]. I now provide extra details of these programs. The details are based on DESSFB (2019) and DESE (2020).

EST makes sure jobseekers are ready for work by providing them with some basic skills required by employers. Note that the training is provided by a network of education providers and not by employers themselves. The training consists of two 75 hours blocks. Block 1 provides soft skills such as communication skills, working in a group, and planning. Block 2 helps with job hunting and career development; it may also include visits from employers and group visits to employers in the industries and occupations that jobseekers are interested in. Each block is delivered face-to-face for 25 hours/week over three weeks for full time participants and 15 hours/week over five weeks for part time participants. A person is eligible for EST if he or she is aged 15 to 24 (inclusive), receives assistance from a jobactive employment services provider[12], and gets an income support payments which has a mutual obligation requirement. It is compulsory for jobseeker to participate in EST once he or she has received five months of help from jobactive. But the jobactive service provider can waive this requirement if it believes the jobseeker (i) must solve other barriers first (examples: language skills, health problems), (ii) has already completed a similar training (iii) already has employability skills based on past job experience. From 1 April 2017 to 28 February 2019, around 36,000 jobseekers have started at least one of the EST training blocks (DESSFB, 2019, page 22).

PaTH internships provide jobseekers with work experience and, by doing so, breaks the vicious cycle of needing experience to get a job and needing a job to get experience. The internship lasts between 4 to 12 weeks, provides a one time payment of up to A$1000 to employers for hosting the intern, and provides A$200 per fortnight to participating jobseekers (in addition to jobseekers' regular fortnightly welfare) payments. From 1 April 2017 to 31 June 2019[13], a jobseeker is eligible for PaTH internships, if he or she (i) is aged 17 to 24 (inclusive) (ii) has been receiving continuous assistance from jobactive,

[11]There are two issues to note here. First, it is possible that 'some' of those aged 25 to 29 were eligible for EST in the 'past'. Second, it is possible that 'some' of those aged 30 or above were eligible for YWS in the 'past'. The first issue can create problems when I compare the effect of PaTH package vs YWS only. The second issue can create problems when I compare the effect of PaTH package vs control (aged 30 and above) and the effect of YWS vs control (aged 30 and above). I have solutions for both issues and work is in progress to check whether results are robust to these solutions.

[12]Jobactive is a network of employment services providers and assist unemployed jobseekers in their job search.

[13]Eligibility rules have changed slightly starting 1 July 2019

TtW, and Disability Employment Services (DES) providers[14] for at least six months (iii) is receiving payment which has mutual obligation requirement. An employer is eligible for hosting the intern if there is a decent chance of employment for the intern with the employer once the internship ends. Interns who do not get permanent positions with the employer at the end of the internships still find the internships beneficial; these interns (and their employment services providers) report that by participating in internships they have gained work experience, learned new skills, and have seen improvement in their self confidence. Employers report that they liked using internships as part of their recruitment process. Since the start of the internship program (1 April 2017) to 28 Feb 2019, around 8,500 individuals have participated in PaTH internships (DESSFB, 2019, page 23).

YBWS provides up to A$10,000 to employers when employers hire an eligible unemployed jobseeker. A jobseeker is eligible if, at the time of hire, he or she is aged 15 to 24 and has been receiving assistance from jobactive or TtW employment services providers continuously for the past 6 months[15]. I want to reiterate that the subsidy is provided to the employer and not to the jobseeker. The payment to the employer is made over a maximum of six months. The payment is flexible and is negotiable. For example: an employer may get A$4,000 upfront and A$1000 per month over six months (for a total of A$10,000). The employer must document that the jobseeker is hired for an ongoing position (position can be full-time, part-time, or casual) and does not displace an existing employee. The employer must also make sure the jobseeker works at least 20 hours per week on average during the subsidy period. From 1 Jan 2017 to 28 Feb 2019 around 34,500 jobseekers have been placed into employments that provide YBWS to employers (DESSFB, 2019, page 24).

YWS is very similar to YBWS. It provides a smaller subsidy of up to 6,500 AUD to employers over a six months period when firms hire an eligible unemployed youth. Since 1 Jan 2017, a eligible youth is someone who, at the time of hire, is aged 25 to 29 (inclusive) and have been receiving assistance from jobactive employment services provider for the past 6 months. Payment method and employer eligibility is similar to YBWS. From 1 July 2015 to 28 December 2018 around 24,750 jobseekers have been placed into employments that provide YWS to employers (Senate Estimates, 2019a).

[14]Jobactive is the main network of employment services providers. TtW provides specialized services to some youth. DES providers services to people with disability.

[15]Eligibility also requires that the person is receiving an income support payment that has an mutual obligation requirement

3 Data

This study uses the Research and Evaluation Database (RED). RED is an administrative database of the Australian Government income support payment recipients. It is a longitudinal data and contains information on all the income support payment episodes of a person who has had a payment episode since 1 July 1998. RED is continuously updated to include new episodes. The last date of observation available in my data is 2 Jan 2021. The Department of Education, Skills and Employment has provided the data to the author for this research project. Personal identifiers in RED have been removed to preserve the privacy of income support payment recipients. The following are the three main categories of information in RED I use in this paper:

- benefit history: starting and ending dates of each payment episode, the type of payment in each episode (examples: Newstart Allowance, Youth Allowance)[16].
- activity performed: starting and ending dates of activities performed by income support recipients, the type of activity (examples: jobsearch, training). These activities can be required to be completed by recipients as part of their mutual obligation requirements.
- demographics: date of birth, gender, country of birth, preferred language, number of children, marital status, education level.

I use official reports for the cumulative number of PaTH and YWS participants (DESE (2020, pages 8-10), DESSFB (2019, pages 1, 22-24), Senate Estimates (a)).

The analysis sample for estimating the ITT effect of PaTH package vs YWS (question 1) consists of individuals who meet the second eligibility rule between Jan 2017 and Feb 2019[17]. Figure 2 shows an example of an unemployed jobseeker in the analysis sample. The jobseeker enters the welfare system in May 2017 and starts receiving income payments and support from employment services system in the same month. The jobseeker continuous receiving help from employment service providers and in October 2017

[16] Newstart Allowance is provided to unemployed jobseekers with age above 22 and below the Age Pension age. It was renamed to Jobseeker Payment on 20 March 2020. Youth Allowance is provided to young students, apprentices, and jobseekers (jobseekers aged 16 to 21).

[17] Jan 2017 is chosen as a start month because YBWS component of PaTH started in this month which. Feb 2019 is chosen is the end month because I know the cumulative number of participants up to Feb 2019 using the DESSFB (2019) official report. The number of participants up to Feb 2019 will be needed to estimate the approximate ATET of PaTH.

he or she meets the second eligibility rule of YBWS (i.e. has received assistance from employment services providers for 6 months). The age of the jobseeker is calculated on October 2017; if the person is aged below 25, he or she is eligible for YBWS and if aged 25 or above, he or she is ineligible. I use the earliest month a jobseeker meets the second eligibility rule so that I do not classify a person ineligible if he or she had been eligible for YBWS in the past. The analysis sample consists of individuals aged 15 to 35 and the total number of individuals in the sample is 466,700. The analysis samples for ITT effect of YWS vs Control (question 2) is constructed in a similar manner but now the sample is restricted to those aged 25 and above and the eligible group are those aged less than 30. The analysis samples mentioned above are for the RDD methodology. I also use Diff-in-Diff to answer questions 1 and 2. For the Diff-in-Diff approach, the above samples are *post* samples. I construct *pre* samples by selecting individuals who meet the second eligibility rule sometime before the start of the programs (i.e. between Dec 2012 to Jan 2015). For the ITT effect of PaTH vs control (question 3), I use Diff-in-Diff and construct pre and post samples as described above. The eligible group for question 3 are those aged less than 25 and the ineligible are those aged 30 and above. Question 4 and 5 are based on the Diff-in-Diff analysis samples of questions 3 and 2 respectively.

The outcome variable, y[s], is a dummy variable equal to 1 if the person is on income support payments s months after meeting the second eligibility rule (s = 1,2,...,23) and 0 otherwise. Figure 2 shows the outcome variable for an individual in the analysis sample who meets the second eligibility rule in October 2017. For this individual y[1] is equal to 1 if he or she is receiving income support payments in November 2017 and 0 otherwise, y[2] is equal to 1 if he or she is receiving income support payments in December 2017 and 0 otherwise, ... , y[23] is equal to 1 if he or she is receiving income support payments in September 2019 and 0 otherwise. A value of 0 for the variable implies that the person is less reliant on income support payments. I stop at y[23] because my version of RED data stops on Jan 2021 and a person who meets the second eligibility rule in Feb 2019 can be followed for no more than 23 months. To ensure a similar length of time path for each individual in the sample, I decided to follow each individual up to 23 months after he or she meets the second rule.

Figure 3[18] shows the outcome variable y[23] against age. The dots are averages for

[18]The graph is created using the rdplot command in Stata (Calonico et al. (2014b), Calonico et al. (2017))

one monthly age bins and the the fitted line is from a regression of the outcome variable on age, age squared, and a dummy variable equal to one if individual is below 25 and 0 otherwise. This is an RDD graph of the outcome vs running variable and provides some descriptive evidence that being eligible for PaTH package vs YWS results in a 1.5 percentage points less reliance on welfare payments 23 months after meeting the second eligibility rule. Figure 4 shows the difference in mean of y[s] for PaTH eligible group (aged 23,24) and YWS eligible group (aged 25,26) along with the estimated standard error of the difference (s = 1,2,...,23). This difference can be considered a fully non-parametric RDD estimate of the ITT effect of PaTH package vs YWS and it provides some descriptive evidence that being eligible for PaTH vs YWS makes a person consistently less reliant on welfare payments for up to 2 years after becoming eligible (i.e. after meeting the second rule). However, the fully non-parametric estimates can be biased because it puts equal weights to all observations and fails to account for any systematic relationship between age and the outcome variable. In the RDD analysis that will follow, I will check if the descriptive findings of these two graphs persist once a more systematic evaluation of the ITT effect of PaTH vs YWS is conducted.

There are many strengths of using RED for this paper. First, it contains the *population* of interest and not just a small *sample* of interest. In other words, it contains all the people eligible for different programs shown in figure 1. This has advantage over studies[19] using small samples to generalize results for the whole population in a country. Second, many papers[20] on wage studies use large administrative databases on income support recipients (sometimes combining it with other databases to construct, for example, linked employer-employee database) which makes this paper consistent with the wage subsidies literature.

The third strength of RED is that it allows to consider outcomes at short- (less than 1 year), medium- (between 1 and 2 years), and long- (more than 2 years) run

[19]Richardson (1998), Knight (2002) use Australian Youth Survey which contains a small sample of administrative data on individuals on income support. Schunemann et al. (2013) uses 2% sample of administrative data of income support recipients.

[20]De Giorgi (2005), Jaenichen et al. (2011), Boockmann et al. (2012), Schunemann et al. (2013), Huttunen et al. (2013), Sjögren & Vikström (2015), Pasquini et al. (2019), Saez et al. (2019), van den Berg et al. (2020). Two of these papers (Huttunen et al., 2013, Saez et al., 2019) study the cuts in employer-borne in payroll taxes which can be considered as a type of wage subsidy applied to both existing and new hires at a firm; this differs from the subsidy studied in my paper where subsidy is provided to firms for hiring unemployed jobseekers.

horizons whereas most other studies of wage subsidies only consider short run effects[21]. The fourth strenght of RED is that it allows outcomes to be measured at high frequency i.e. I measure the effect at each month after a person becomes eligible for subsidy[22]. Some other studies measure outcome at very low frequency[23] which may fail to provide a complete picture of the treatment effect over time. The fifth strength of RED is that it has exact information on date of birth, date of entry and exit from payments and activities, and type of welfare payments. This information is used to construct the analysis sample and the outcome variable observed at monthly frequency which has minimal measurement error as compared to outcomes measured at monthly frequency based on survey data[24].

There are three main weaknesses of using RED in this paper. First, it does not have individual level information on participation in labor market programs (such as YBWS, YWS, and EST). Information on participation in these programs is available in other administrative data (such as the Employment Services System data which is not available to external researchers)[25]. So to provide an approximate estimate of the treatment effect of participation (ATET), I use 'aggregate' participation data from official reports (DESSFB (2019), DESE (2020)) to obtain participation rates in PaTH and YWS. These participation rates are used to scale up ITT estimates to provide an approximate ATET. Schunemann et al. (2013) uses a similar method to estimate ATET.

The second weakness of RED for my paper is that it lacks employment services provider information; I do not know for sure whether a person is receiving assistance from employment services providers and I also do not the duration of such assistance and also the type of service provider (jobactive, TtW, ParentsNext, DES, etc.). These pieces of information are needed to restrict the sample to those meeting the second eligibility rule of YBWS. My solution is to restrict sample to those who are receiving certain types of pay-

[21] Boockmann et al. (2012), Schunemann et al. (2013), Huttunen et al. (2013), Pasquini et al. (2019)

[22] Upto 23 months in my main specification and upto 34 months in a robustness check

[23] De Giorgi (2005) measures outcome 18 month after a person enters in the program; Richardon (1998) and Knight (2002) use responses from future survey waves conducted one or two years after program participation; Huttunen et al. (2013) only considers end of year employment status; Schunemann et al. (2013) measures outcome once at the beginning of eligibility for subsidy and once after 12 months; Pasquini et al. (2019) only considers outcome at the beginning of eligibility.

[24] For example: HILDA interviews an individual once per year and asks the individual to *recall* his or her employment status in the past year. This method can lead to large discrepancies in the responses of people between survey years for the employment status of the same month.

[25] Out of EST, PaTH internships, YBWS, and YWS, I only have information on those who participates in PaTH internships. If one have participation data (by linking RED with Employment Services System data) then one could use, for example, 'matching' techniques to estimate ATET (as done in many previous studies).

ments (NSA, Youth Allowance (for jobseekers)) and then proxy for duration of assistance received by the duration of two types of activities (jobsearch and TtW activities). Three pieces of evidence support this proxy. First, I have data on who participates in PaTH Internships. The proxy predicts that 97% of PaTH Internship participants are eligible for PaTH Internships. Second, the proxy does a good job of matching the actual number of job seekers eligible for YBWS at two points in time: 31 March 2018 and 28 Feb 2019. The eligible samples (for YBWS) at these two points according to official reports are 84,968 and 74,291 respectively (DESE(2020, page 105), DESSFB (2019, page 24)). The proxy gives eligible samples of 79,108 and 73,273 respectively. Third, the official youth jobactive caseload was 144,822 on 31 Dec 2016 (Labor Market Information Portal) and the proxy gives a caseload of 140,310 on 1 Jan 2017.

The third weakness of RED in my paper is that I cannot follow a person once he or she leaves the welfare system. Ideally, one would like to know the destination of the person who leaves the welfare system. For example, we would like to know whether the person has obtained employment, whether it is a full-time or part-time employment, what is the wage income from this employment, the industry and occupation type of employment, the duration a person remains employed. De Giorgi (2005) for example complements the administrative data of welfare payment recipients in the UK with another administrative data that follows the labor market program participants once they leave the welfare payment system and is thus able to measure employment status 18 months after the starting the program[26]. But from a policy perspective it is still interesting to see the effect of programs on the probability that a person leaves the welfare system; the estimated effect can tell us how much the government can save on welfare payments as a result of these programs.

4 Methodology

The analysis sample consists of individuals meeting the second eligibility rule; in this sample age completely determines who is eligible for PaTH, who is eligible for YWS, and who is ineligible for both PaTH and YWS. This age threshold for eligibility for different programs is exploited in the RD and Diff-in-Diff research designs to estimate different

[26]In Australia there is the Post-Program Monitoring Survey which measures employment and education outcomes of participants 3 months after the completion of programs.

treatment effects.

4.1 Regression Discontinuity Design

I use a sharp RD design for questions 1 and 2. In the analysis sample, age determines eligibility: those aged below 25 are eligible for PaTH and those aged 25 or above but below 30 are eligible for YWS. This means that we can use a sharp RD model with age as the running variable to estimate the ITT effect of eligibility for PaTH vs eligibility for YWS at the threshold age 25. Formally, I estimate the following sharp RD model:

$$y_i[s] = f(age_i) + \beta_s Eligible_i + \epsilon_{is} \tag{1}$$

where age_i is the age of individuals i at the time he or she meets the second eligibility rule (measured in years and months, example: 22 years 3 months old); $f(age_i)$ is a continuous function of age_i; $Eligible_i$ is a dummy variable equal to 1 if $age_i \in (15, 25)$ and equal to 0 if $age_i \in [25, 30)$; $y_i[s]$ is equal to 1 if individual i is on income support s months after meeting the second eligibility rule and 0 if he or she is off income support (s = 1, 2, ... , 23); and ϵ_{is} is an i.i.d. error term[27]. The parameter of interest is β_s which measures the ITT effect of eligibility for PaTH package vs eligibility for YWS s months after meeting the second eligibility rule. For the ITT effect of eligibility for YWS vs control (question 2), I estimate a similar sharp RD model with threshold age 30 where $Eligible_i = 1$ if $age_i \in [25, 30)$ and $= 0$ if $age_i \in [30, 35)$. De Giorgi (2005) uses a similar sharp RD model with age as the running variable for estimating the effect of The New Deal for Young People - a program in the UK providing jobsearch assistance, training and education, wage subsidies, and work experience to unemployed youth[28].

I estimate β_s in equation 1 using the 'rdrobust' Stata package developed by Calonico et al. (2014b, 2017). The package is based on local polynomial regression procedures developed by the same authors in their Calonico et al. (2014a) paper. Hyytinen et al. (2018) shows that the correct application of this procedure can yield estimates very

[27] We can think of the sharp RD as an attempt to estimate the conditional expectation function of y with respect to age, $E(y|age)$, on both sides of the threshold. We can write the conditional expectation function as a sum of continuous and discontinuous function of age: $E(y|age) = f(age) + \beta Eligible$. In this case we can write $y = E(y|age) + \epsilon = f(age) + \beta Eligible + \epsilon$ where by the property of conditional expectation ϵ is iid.

[28] In the UK context, participation in the program is mandatory and hence the RD model in De Giorgi (2005) estimates ATET.

close to the estimates obtained from a randomized experiment. Following the suggestions of Gelman and Imbens (2018), I estimate local linear and local polynomial regression models (i.e. use linear and quadratic polynomials for age in equation 1) and do not use higher order polynomials of age to approximate for f(age). In the main results I report estimates of β_s using the default rdrobust specification: automatic bandwidth selection[29], triangular kernel[30], and linear polynomial for age. As a robustness, I also estimate 19 other specifications i.e. I estimate a total of 20 specifications [five bandwidth choices (automatic, 1, 1.5, 2, 2.5) $\times$ two polynomial orders (linear and quadratic) $\times$ two kernel types (triangular and uniform)].

4.2 Difference-in-Differences

In addition to the RD method, I also use Diff-in-Diff method to answer questions 1 and 2 and check if the two methods yield similar results. I use Diff-in-Diff to answer question 3 as well. I use the canonical 2 $\times$ 2 model consisting of two time periods (pre- and post- treatment samples) and two groups (eligible and ineligible). I now define the two time periods and the two groups for question 1. The pre-treatment sample consists of individuals who meet the second eligibility rule sometime before the program starts (Dec 2012 to Jan 2015) and the post-treatment sample consists of those who meet the second rule after the program starts (Jan 2017 to Feb 2019). In each of these samples, there is an eligible group (those aged below 25 at the time they meet the second rule) and ineligible group (those whose age $\in [25, 30)$). I follow each individual in these two samples for 23 months to check whether the person is on or off income support. Formally, I estimate the following model:

$$y_{iT}[s] = \alpha_s T + \gamma_s Eligible_i + \beta_s (T * Eligible_i) + \epsilon_{iTs} \tag{2}$$

where $T = 0$ for observations in pre-treatment sample and $T = 1$ for observations in post-treatment sample; $Eligible_i = 1$ if $age_i \in (15, 25)$ and $= 0$ if $age_i \in [25, 30)$ where age_i is age at the time a person meets the second eligibility rule; $T * Eligible_i$ is interaction of the two dummy variables T and $Eligible_i$; and ϵ_{iTs} is an i.i.d. error term. The parameter of

[29] Selects bandwidth around the threshold age 25 (for question 1) and 30 (for question 2) by minimizing MSE. The analysis is conducted using observations within this bandwidth.

[30] This gives more importance to observations closer to the age threshold.

interest is β_s; this parameter captures the ITT effect of eligibility for PaTH vs eligibility for YWS. The Diff-in-Diff method employed in this paper is similar to Katz (1996) and Boockmann et al. (2012); these two papers also have pre and post periods and eligible and ineligible age groups and use the Diff-in-Diff framework to estimate the ITT effect of wage subsidies in the US and Germany.

For question 2 the pre-treatment sample consists of those meeting the second eligibility rule between Jan 2013 and Dec 2014 and the post-treatment sample of those meeting the second rule between Jan 2017 and Dec 2018. Each sample consists of eligible (age $\in [25, 30)$) and ineligible (age $\in [30, 35)$) group where age of the individual is calculated at the time he or she meets the second rule. The β_s in equation 2 then captures the ITT effect of eligibility for YWS vs Control. Recall that control group is not eligible for YWS nor for PaTH package.

For question 3 the pre-treatment sample consists of those meeting the second eligibility rule between Dec 2012 and Jan 2015 and the post-treatment sample of those meeting the second rule between Jan 2017 and Feb 2019. Each sample consists of eligible (age $\in (15, 25)$) and ineligible (age $\in [30, 35)$) group where age of the individual is calculated at the time he or she meets the second rule. I am excluding individuals aged [25,30) from control group because these individuals are eligible for YWS and I do not want the control to be contaminated by the effect of YWS. The β_s in equation 2 then captures the ITT effect of eligibility for PaTH package vs Control.

We can consider YWS as treatment group 1 and PaTH package as treatment group 2. Question 2 estimates ITT effect of YWS relative to control group and Question 3 estimates ITT effect of PaTH relative to control group. Comparing these two ITT effects can give us an idea of the relative impact of YWS and PaTH (relative to control group). Question 1 compares the effect of YWS and PaTH directly rather than relative to a control group. Direct comparison of PaTH vs YWS (question 1) and indirect comparison (question 2 and 3) do not necessarily have to yield the *same* difference in effects of PaTH vs YWS but they should still give reasonably similar differences in terms of sign and magnitude.

I estimate equation 2 by OLS. For completeness, the equation is estimated five times by manually choosing observations in five different bandwidths around the threshold age: 0.5, 1, 1.5, 2, 2.5. For example: a bandwidth of 2 for question 1 would mean that equation

2 is estimated using OLS by restricting sample to those age 23 to 27. I report results for bandwidth size 2.5. There is a concern that those just below the threshold age 25 are eligible for PaTH package for a short time period and hence have less time to avail opportunities provided by PaTH. For robustness, those aged 24 are removed from the analysis sample and equation 2 is restimated (I call this exercise 'donut' Diff-in-Diff). As an another robustness exercise, I include calender month dummies instead of a single time dummy T in equation 2 to control flexibly for business cycle effects.

4.3 Approximate Average Treatment Effect on the Treated (ATET)

I use a two sample IV approach to provide an approximate ATET estimate for PaTH and YWS. We are interested in the effect of participation dummy (p) on the outcome variable (y):

$$y = \alpha + \beta p + \epsilon. \tag{3}$$

The reduced form regresses the outcome variable (y) on the eligibility dummy (instrument z):

$$y = \gamma + \delta z + \mu. \tag{4}$$

The first stage regresses the participation dummy (p) on the eligibility dummy (instrument z):

$$p = \rho + \pi z + \eta. \tag{5}$$

The IV estimator for the parameter of interest β is the ratio of reduced form to first stage:

$$\beta = \frac{\delta}{\pi}. \tag{6}$$

The numerator in 6 can be considered an ITT effect. And since both p and z are dummies, the denominator in 6 is equal to the difference in participation rates of eligible and ineligible groups. Now because of eligibility rules, the participation rate for ineligible groups in 0 which lets us write the parameter of interest as follows:

$$\beta = \frac{ITT}{participation\ rate\ in\ the\ eligible\ group} \tag{7}$$

I call 7 a two sample IV approach because the numerator is obtained from RED data and the denominator from official reports. The standard errors for β are obtained by bootstrap method. There is a concern that participants are very different from an average member of the eligible group. For example, those who participate may very well be those who are more likely to leave the welfare system. This implies that 7 may not give us the treatment effect of participation for the entire population. So I consider a heterogeneous effects model where the IV estimate gives us Local Average Treatment Effects (LATE) and invoke the LATE theorem under one-sided non-compliance (Angrist & Picschke, 2008, chapter 4, Theorem 4.4.2, page 164). The theorem operates in cases where the control group cannot get treated (i.e. cannot participate). In these cases we can have at most one sided non-compliance: only people assigned to the treatment group can deviate from their assigned treatment status i.e. do not take up treatment. Under such conditions, the theorem establishes the following:

$$\beta = LATE = ATET = ITT/(ParticipationRate) \tag{8}$$

and so the ratio of ITT estimates by participation rate gives us an approximate ATET. Because of the eligibility rules for PaTH and YWS individuals in control group cannot participate in programs. That is, we have one sided non-compliance and can thus use equation 8 to obtain ATET for PaTH and YWS. An evidence of one-sided non-compliance is that out 13425 individuals who participated in PaTH internships, 13412 were aged less than 25 at the time they started the internship. For the 13 individuals who were 25 or above, the maximum age at the time the internship started was 25 years and one month. It is possible that paperwork or application for internship started much earlier than the actual starting date of internship and age at the time of paperwork or submission of application is considered by employment services providers rather than age at the time of internship.

I assume that the ITT effect of eligibility for PaTH vs control obtained via Diff-in-Diff (question 3) is a good approximation of ITT effect obtained via randomized controlled experiment. The similarity between RD and Diff-in-Diff estimates of questions 1 and 2 in the results section will provide some backing to this assumption. Using the cumulative number of participants in PaTH till 28 Feb 2019 (obtained via official reports DESE (2020), DESSFB (2019)) and the cumulative number of eligible individuals for PaTH till

28 Feb 2019 (obtained via RED), I have estimated the participation rate for PaTH to be about 20%. So, I obtain approximate ATET for PaTH by dividing ITT (from question 3) by 0.2.

An approximate ATET for participants of YWS vs control (question 5) is calculated in the same manner. There is one sided non-compliance because of age threshold eligibility rule for YWS which implies that we can use LATE theorem. ITT is approximated by Diff-in-Diff estimates used for answering question 2. Participation rate of 14% is calculated using cumulative number of YWS eligible population till 31 Dec 2018 (RED) and the cumulative number of YWS participants till 31 Dec 2018 (Senate Estimates (2019a)). Schünemann et al. (2013) uses a similar approach to provide approximate point estimate for ATET of participating in wage subsidies; these authors use bootstrap standard errors to obtain standard errors for the point estimate.

5 Results

5.1 ITT effect of eligibility for PaTH vs YWS (Question 1)

Figure 5 shows the RD estimate of β_s in equation 1 for s = 1, 2, ..., 23. The estimates provide ITT effect of eligibility for PaTH vs YWS for different points in time after the person becomes eligible. For the first 19 months, a person eligible for PaTH is about 1 percentage points less likely to be receiving income support payments as compared to the case when the person is eligible for YWS. For 20 to 23 months after becoming eligible for PaTH, a person is 2 percentage points less likely to be receiving income support payments as compared to a scenario where he is eligible for YWS instead of PaTH. The estimates for the last four months are statistically significant at 5% significance level.

Figure 6 shows the Diff-in-Diff estimate of β_s in equation 2 for s = 1, 2, ..., 23. As was with the RD case above, the Diff-in-Diff estimates provide ITT effect of eligibility for PaTH vs YWS for different points in time in the future since meeting the second eligibility rule. The Diff-in-Diff estimates show that for 4 to 20 months after meeting the second eligibility rule, a person eligible for PaTH is about 1 percentage points less likely to be receiving income support as compared to a scenario where the person is eligible for YWS. The estimates are smaller at the beginning and end of the time horizon. Overall, the RD and Diff-in-Diff estimates are very similar which provides some confidence in the

ITT estimates for question 1. There is a slight departure between the two methods at the end of the time horizon but confidence intervals of estimates from both methods still overlap with each other at the end horizon which tells us that the two methods yield reasonably similar estimates. One explanation for he small difference between RD and Diff-in-Diff estimates is that RD gives effect at the threshold age whereas Diff-in-Diff gives effect averaged across a wider range of age.

5.2 ITT effect of eligibility for YWS vs Control (Question 2)

Figure 7 shows the estimate of β_s in equation 1 for the case when threshold age is 30 and we are estimating the ITT effect of being eligible for YWS as compared to not being eligible for YWS. On average, a person eligible for YWS is equally likely to be on income support for the first twelve months after becoming eligible, as compared to the case where the person is not eligible. For the last eleven months, a person eligible for YWS is 1 percentage points less likely to be on income support as compared the case when he or she is ineligible. All the estimates are statistically insignificant at the 5% level. The Diff-in-Diff estimate of β_s in equation 2 is shown in figure 8. The estimates tell a similar story as in the RD case: The eligibility effect of YWS vs control is close to zero and statistically insignificant. One explanation for small effects for YWS is its low participation rates.

5.3 ITT effect of eligibility for PaTH vs Control (Question 3)

Figure 9 shows the Diff-in-Diff estimates for question 3. These are parameter estimates for β_s in equation 2 where the eligible group are those whose age is between 15 and 25 and the ineligible group those whose age is between 30 and 35. On average[31], a person eligible for PaTH is two percentage points less likely to be receiving income support for up to 23 months after becoming eligible, as compared to the case where the person is not eligible for PaTH. The estimates are statistically significant at the 5% level. The effect size is bigger in the middle of time horizon as compared to the end points of the time horizon. A possible explanation for the U-shaped time profile of ITT effects is that 70% of YBWS participants are unemployed for more than 12 months and 37% of participants are unemployed for more than 24 months (DESE, 2020, Table 7, page 105). So most of the YBWS participants enter subsidized employment 6 to 18 months after meeting the

[31] Average is calculated using the 23 estimates over the time horizon.

second eligibility rule and during their subsidized employment they could be off income support. This may explain the larger ITT effects observed in the middle of time horizon.

To provide an idea of the size of the ITT estimates, Figure A1 shows the Diff-in-Diff ITT estimates for questions 2 and 3 as percentage of control mean. Control mean refers to the proportion of control group who are OFF income support s months after becoming eligible i.e. I find mean of 1 - outcome variable. The absolute values of ITT are used to compute the percentages. We can see that on average the ITT effect of PaTH is about 6 percent of control mean and the ITT effect of YWS is 1.4 percent of control mean. As a concrete example, the ITT effect at s = 8 is -0.024 and about 30 percent of control group are OFF income support 8 months after meeting second eligibility rule. This means that the ITT effect is 8 percent of control mean ($\frac{0.024}{0.30} \times 100$).

In the literature, it is common to find ITT effects of eligibility for wage subsidies that are small in magnitude or statistically insignificant or both. The point estimates for ITT effects in Boockmann et al. (2012), Schünemann et al. (2013), and Huttunen et al. (2013) are either of similar or smaller magnitudes as the ones found in questions 1,2, and 3 above. The standard errors of point estimates are also quite large in these three papers which makes the estimates statistically indistinguishable from 0. Sjögren & Vikström (2015) and Pasquini el al. (2019) report estimates that are statistically significant at 1% level and may appear to be much larger than the estimates obtained in questions 1,2, and 3 above if one is not careful in observing how these two papers measure their outcome variable. These two papers report effects relative to control group mean but when we consider the percentage point difference in outcomes of eligible and ineligible groups (as is done in my paper) then the magnitude of ITT effects are of similar magnitude as in questions 1,2, and 3. Therefore, the magnitude and standard errors of the ITT effects in the current paper is consistent with the literature.

5.4 ATET estimates for questions 4 and 5

Using equation 8, I provide approximate estimates for the Average Treatment Effect on the Treated (ATET) of participating in PaTH vs control (question 4) and ATET of participating in YWS vs control (question 5). For the numerator term (ITT) in equation 8, I use Diff-in-Diff estimates of question 3 and question 2 for questions 4 and 5 respectively. The denominator (participation rate) is 0.2 for PaTH and 0.14 for YWS. Figure 10 shows

the ATET at different points in time after a person becomes eligible for PaTH and YWS[32]. On average[33], a YWS participant is 3 percentage points less likely to be receiving income support payment as compared to a scenario where he or she does not participate. Also, on average[34], a person who participates in at least one component of PaTH is 9.4 percentage points less likely to be receiving income support payments. As a comparison, the difference in the employment rate of a person with high school degree and a 2 or 3 year community college degree is 10 percentage points (Card et al., 2017, page 896). So the ATET of PaTH and YWS are economically meaningful impacts. ATET of YWS is not statistically significant at 5% level, ATET of PaTH is statistically significant at 5% level, and there is overlap in the confidence intervals of the ATET of the two programs which suggests that the difference in ATET of the two programs may not be statistically significant at 5% level (see figure A3).

To provide an idea of the size of the ATET estimates, Figure A2 shows the ATET estimates for questions 4 and 5 as percentage of control mean. Control mean refers to the proportion of control group who are OFF income support s months after becoming eligible i.e. I find mean of 1 - outcome variable. The absolute values of ATET are used to compute the percentages. We can see that on average the ATET effect of PaTH is about 30 percent of control mean and the ITT effect of YWS is about 10 percent of control mean. So the ATET estimates are quite large when compared to the proportion of control group who are off income support.

Card et al. (2017) conducts a meta analysis of 207 recent active labor market programs, including wage subsidy programs, and provides good benchmarks for the ATET for PaTH and YWS participants. Across all the studies in their meta analysis, the mean short term (less than 1 year), medium term (between 1 and 2 years), and long term (more than 2 years) effects of program participation, on the probability of finding employment, is 1.6, 5.4, and 8.7 respectively (Card et al., 2017, Table 2). Once we consider studies on wage subsidies only then the mean short term, medium term, and long term effects, on the probability of finding employment, is 1.1, 6.2, 21.1 (Card et al. , 2017, Table 3(a)). The short term and medium term effect of participating in YWS, on the probability of exiting

[32]The effect is measured after a person becomes eligible and not from the start or end of the program participation.
[33]Average is calculated using the 23 estimates over the time horizon.
[34]Average is calculated using the 23 estimates over the time horizon.

welfare[35], is 2.5 and 3.5: YWS has larger short term effect than an average international wage subsidy program but a smaller medium term effect. And the short and medium term effect of participating in at least one component of PaTH, on the probability of existing welfare system, is 9.2 and 9.6 respectively: PaTH has a much larger short term effect and almost double the medium term impact than a typical international active labor market program. However, the ATET effect of YWS and PaTH are small when compared to past Australian studies which find an ATET of about 20 percentage points (Richardson (1998), Knight (2002), Johnston (2007)).

5.5 Summary of the Results section

Table 2 summarizes the results. For each of the five questions I have asked in this paper, the table provides description of the treatment effect being estimated, type of treatment effect, methodology used to answer the question, and the average estimates (where the average is calculated across time periods). Three points summarizes the results. First, the ITT estimates are small compared to ATET effects. This is not surprising because participation rates are far from 100%. Second, the ITT effects are small in magnitude but still consistent with estimates found in the literature. The ATET effects are economically significant and the magnitude of the estimates are in line with the international literature but smaller than the past Australian studies. Third, the ITT effect of PaTH is three (using question 1 and 2[36]) to four (using question 2 and 3) times larger than the ITT effect of YWS. The ATET effect of PaTH is more than three times larger than the ATET effect of YWS.

6 Identification Tests

6.1 Identification tests for RDD

In this section I test the identifying assumptions of the RD model in equation 1. The tests conducted are suggested by Imbens & Lemieux (2008) and Lee & Lemieux (2010).

[35]The individuals in my analysis sample are unemployed jobseekers and the most likely reason such individuals will leave income support system is when they get employed. This makes comparison with the mean effects reported in Card el al. (2017) more consistent.

[36]ITT effect of YWS vs control = -0.5. ITT effect of PaTH vs control = ITT effect of PaTH vs YWS + ITT effect of YWS vs control = -1.1 - 0.5 = -1.6.

I do the tests only for the RD model used for question 1. They main assumption of the model is that potential outcomes are continuous at the threshold age 25. This assumption cannot be tested directly but there are three indirect ways to check this: placebo analysis sample, placebo age thresholds, and placebo outcome variables (also known as balance test). Another assumption in an RD model is that people do not manipulate the running variable. Density test directly checks this assumption.

6.1.1 Density (or Manipulation) Test

To check whether people misreport their age to become eligible for PaTH programs, we can estimate the density of age and test if their is a discontinuity in the density of age at the threshold age 25. The null hypothesis is that there is no manipulation. I conduct this test using the rddensity command in Stata (Cattaneo et al., 2018) and obtain a p-value of 0.46 i.e. we cannot reject the hypothesis that people do not manipulate their age. Figure 11 shows that the density curve is continuous at the threshold age 25 because the confidence intervals of the estimated density curve overlaps at the threshold. Therefore, there is no evidence that people are misreporting their age to become eligible for PaTH programs.

6.1.2 Placebo Sample

If the identifying assumption of the RD model is true then there should not be any ITT effects of PaTH vs YWS for a sample of individuals before the start of the PaTH program. I construct a placebo sample consisting of individuals meeting the second eligibility rule sometime between Jan 2013 and Dec 2014 and estimate equation 1 for this sample. Figure 12 shows the estimates for β_s in equation 1 for this sample, where s = (1, 2, ..., 23)[37]. The estimates are centered around 0 and statistically insignificant at 5% level which means that we cannot reject the validity of the main RD identifying assumption based on this placebo test.

[37]The graph if for the default RD specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel. The graphs for 19 other specifications are similar to the default case and are available upon request.

6.1.3 Placebo Threshold Age

If the identifying assumption of the RD model is true then there should not be any ITT effects of PaTH vs YWS at non-threshold ages. The appendix figure A4 shows the ITT effects of PaTH vs YWS when it is assumed that the threshold ages are 20, 21, 22, 23, 24, 26, 27, 28, and 29. The ITT effects for these non-threshold ares are either centered around zero or slightly positive. This lends support to the claim that the persistent negative estimates over the whole horizon observed at age 25 in figure 5 is very unique and not generally observable at non-threshold ages. So we cannot reject the validity of the main identifying assumption of RD based on this placebo test.

The exception is age 22 where we observe negative estimates for the entire horizon. However, age 22 is very special in the context of this paper. It is the age where eligibility for Youth Allowance (for jobseekers) ends and eligibility for Newstart Allowance[38] starts and the RD is most likely picking the effect of this change in eligibility at age 22. Figure A5 in the appendix shows the histogram of age for my analysis sample. We can observe a large spike between ages 22 and 23. Another way to look at the spike is in appendix figure A6 which shows discontinuity in the density of age at age 22. The discontinuity itself may not be a big problem here, the main problem is that there is difference in a type of individual above and below the threshold age 22 - those aged below 22 will need to reapply to continue receiving welfare payments and while those aged above 22 are either new entrants in the system or members who continue receiving payments by submitting new application. Mechanically, those below 22 will drop out of the system if they do not reapply and this could be the main reason why we observe negative effects at the threshold age 22.

6.1.4 Placebo outcome variables

If the identifying assumption of RD is valid then there should not be a jump, at threshold age 25, in variables not affected by the program. I call these variables placebo outcome variables and estimate the RD model in equation 1 using these outcome variables. This exercise is also called the balance test for the equality of baseline covariates, a test conducted in randomized controlled experiments to check if randomization has been done

[38]Both of these payments are made to unemployed jobseekers.

correctly. There are six such variables in this paper: gender[39], immigrant status[40], language[41], marital status[42], children status[43], education level[44]. These outcome variables are observed at one point in time (as opposed to the true outcome variable which is observed over a 23 months horizon).

Figure A7 in the appendix shows estimates of β_s in equation 1 when we use these six placebo outcome variables. Each point shows the estimates from a different RD specification and there are twenty such specifications (five bandwidth choices $\times$ two polynomial orders $\times$ two kernel types). Overall, the estimates are statistically insignificant at 5% level which provides some support to the identifying assumption of RD.

The exception is immigrant status which is statistically significant for some specifications but there are two reasons this should not be a cause of concern. First, Lee & Lemeiux (2010) argue that it is possible that some of these placebo outcome variables (they call them covariates) may be statistically significant due to random chance. They suggest estimating the RD equation (such as equation 1) via Seemingly Unrelated Regression (SUR) Model. In the SUR method we estimate the six equations (for six placebo outcome variables) together and test for the joint significance of the main RD parameters. I estimate ten SUR models (five bandwidths $\times$ two polynomial orders) and in 8 out of 10 of these models, the main RD parameters are jointly statistically insignificant. Second, figure A8 plots the immigrant status variable against the running variable age. We can observe a large outlier for the age bin just above the age 25 and the RD estimate gives a lot of importance to this age bin which results in the negative RD estimates observed in figure A7 for immigrant status. There are are couple of more such random outliers suggesting that random chance may have resulted in the negative RD estimates for immigrant status. I remove observations aged two months below and two months above age 25 and re-estimate the RD models for immigrant status placebo outcome variable. Figure A9 shows that in this 'donut RD' approach, the ITT effect at threshold age 25 is now zero.

[39]Dummy equal to 1 if individual is female and 0 otherwise
[40]Dummy equal to 1 if country of birth is not Australia and equal to 0 if country of birth in Australia
[41]Dummy equal to 1 if preferred language is English and 0 otherwise
[42]Dummy equal to 1 if individual is single and 0 otherwise
[43]Dummy equal to 1 if individual has a child below age 16 and 0 otherwise
[44]Dummy equal to 1 if he higher than high school school qualification

6.2 Identification test for Diff-in-Diff

Parallel trends is one of the main assumptions of Diff-in-Diff. I check for this assumption by doing a placebo test where both the pre- and post-treatment samples are obtained during time periods when the PaTH and YWS did not exist[45]. If the assumption is valid then we should not observe any effects for these placebo samples. Boockmann et al. (2012) uses a similar placebo test to check for parallel trends. Figure 13 shows the ITT estimates for Diff-in-Diff when placebo samples are used[46]. For the ITT effects of PaTH vs YWS (top figure) and the ITT effects of YWS vs control (middle figure), we obtain effects that are centered around 0 and are statistically insignificant at 5% level. This gives us confidence that the parallel trends assumption is valid in these two cases. The bottom figure in figure 13 shows ITT effects of PaTH vs control in the placebo sample. We observe negative and statistically significant estimates, especially for effects at long horizons which casts doubt on the validity of parallel trends assumption when comparing PaTH vs control. One explanation behind these results is that the age groups used for comparing PaTH vs YWS and for comparing YWS vs control are not too different from each other but the age groups used for comparing PaTH vs control are meaningfully different because the age groups are far from each other. Figure 13 tells us that most likely we will overestimate the negative effects when estimating the ITT effect of PaTH vs control; to address this concern I add extra controls in equation 2 which slightly reduces the magnitude of the estimates (see figure A26).

7 Robustness Checks

7.1 Robustness across model specifications

7.1.1 RD model specifications

In the results section for question 1, the RD estimates from the default specification (automatic bandwidth selection, linear polynomial for age, and triangular kernel) are

[45] For PaTH vs YWS: the pre- and post-treatment samples include individuals who meet the second rule in 2011 and 2014 respectively. For PaTH vs control and YWs vs control, the pre- and post-treatment samples consist of those who meet the second eligibility rule in time periods July 2009-July 2010 and July 2012-July 2013 respectively.

[46] See figures A10, A11, A12 for the tests at different bandwidth sizes. The key message remains the same i.e. placebo tests work for PaTH vs YWS and YWS vs control but not for PaTH vs control.

shown figure 5. As a robustness, I estimate the RD model in equation 1 for question 1 using 19 other specifications. Figure A13 in the appendix shows the estimates for the default and 19 other specifications (5 bandwidth choices (automatic, 1, 1.5, 2, 2.5) $\times$ 2 polynomial orders for age (linear and quadratic) $\times$ 2 kernel types). We can see that the estimates are quite similar across different specifications; for the first 19 months, the estimates are about -1 percentage points and for the last 4 months about -2 percentage points.

Figure A14 in the appendix shows the RD estimates for the same 20 different specifications for question 2. Overall, we can see that the estimates are fairly similar to the estimates of the default specification shown in 7: the estimates are centered around 0 for the first 12 months after a person becomes eligible for YWS and around -0.01 (a 1 percentage point difference) for the last 11 months.

7.1.2 Diff-in-Diff Model Specifications

The Diff-in-Diff estimates in the results section for question 1 uses observations between ages 22.5 and 27.5 i.e. a bandwidth of size 2.5. Figure A15 in the appendix shows the Diff-in-Diff estimates for bandwidth sizes 0.5, 1, 1.5, 2, and 2.5. All five specifications show negative estimates but there is a slight variation in magnitude across specifications. The average effect, across horizons and specifications, is -0.8 percentage points which is close to the average effect of -1.1 percentage points, across horizons, from the default RD specification. My preferred Diff-in-Diff specification for question 1 is the one with bandwidth size 2.5. There are two reasons for this. First, the bandwidth size 2.5 present estimates that are not too high nor too low as compared to other specifications. Second, RD estimates are valid just at the threshold and so I want the Diff-in-Diff to give estimates for a much larger age interval.

Similarly, the Diff-in-Diff estimates in the results section for question 2 uses bandwidth of size 2.5. Figure A16 in the appendix shows the Diff-in-Diff estimates for bandwidth sizes 0.5, 1, 1.5, 2, and 2.5. The estimates are fairly similar across specifications. The mean effect, across horizons and specifications, is -0.46 percentage points which is very close to the mean effect, across horizons, of -0.45 percentage points obtained from the default RD specification.

As in questions 1 and 2, the Diff-in-Diff estimates in the results section for question

3 uses bandwidth of size 2.5. Figure A17 shows the Diff-in-Diff estimates for bandwidth sizes 0.5, 1, 1.5, 2, and 2.5. The estimates are fairly similar across specifications though the magnitude of bandwidth size 2.5 seems to be larger than the other specifications. The mean effect, across horizons and specifications, is -1.3 and the mean effect, across horizons, for specification with bandwidth size 2.5 is -1.8.

There is a concern that those just below the threshold age are eligible for programs for only a short time period and are thus less likely to benefit from the programs. To address this issue I remove from sample those aged 24 or above and below 25 for question 1, those aged 29 or above and below 30 for question 2, and those aged 24 or above and below 25 for question 3 and re-estimate the Diff-in-Diff models. This (semi[47]) donut approach yields estimates which are very close to non donut approach estimates (appendix figures A18, A19, A20) especially for my preferred Diff-in-Diff specification that use bandwidth size 2.5. But overall, the donut approach yields slightly larger (in magnitude) estimates than the non-donut approach; validating the concern that those just below the threshold age have a limited time to avail the opportunities provided by PaTH and YWS.

The time dummy, T, in equation 2 may not capture the business cycle effects; these effects are likely to be present because the pre-treatment and post-treatment samples are between 2 to 6 years apart. To address this concern, I use monthly dummies in 2 instead of a single dummy T. These monthly dummies should flexibly control for any business cycle effects. Figures A21, A22, and A23 shows the Diff-in-Diff ITT estimates for questions 1,2, and 3 respectively. The left subfigures show estimates using a single time dummy whereas the right subfigures show estimates using a set of monthly dummies. Overall, the estimates are robust when month dummies are used instead of a single time dummy.

There may be concerns that the age groups used for comparison are too different from each other. This is especially concerning for PaTH vs control comparison because it was shown in figure 13 (bottom panel) that parallel trends do not hold for this comparison. To address this concern, and hopefully to increase precision of estimates as well, I includes 7 other control variables: dummies for gender, country of birth, preferred language, marital status, state of residence, education level, number of children. Figures A24, A25, and A26 shows the Diff-in-Diff estimates for PaTH vs YWS, YWS vs control, and PaTH vs control respectively once we add the extra control variables in equation 2. The estimates

[47]'Semi' because in the RD literature observations close to threshold from both sides are removed while in here I am removing observations from one side only.

for PaTH vs YWS are robust to the inclusion of extra covariates but the estimates of YWS vs control and the estimates for PaTH vs control become smaller in magnitude (especially for PaTH vs control). This result suggests that the estimates without controls are slightly overestimating the effects of the programs and that it is important to account for this overestimation by adding controls.

7.2 Robustness checks for question 1

7.2.1 RD model with extra covariates

In the placebo test that used covariates as outcome variables, we saw that the estimates were statistically insignificant individually (across 20 RD specifications) and jointly (across 10 SUR specifications). But there may still be a concern about the magnitude of the point estimates for placebo outcome variables. To address this concern, I re-estimate the RD model in equation 1 by including the three out of six control variables used in the placebo outcome variable test (dummies for gender, immigrant status, language)[48]. Figure 14 shows that the estimates remain virtually the same when the covariates are included in the RD model. This is true for the 19 other RD specifications as well (appendix figure A27).

7.2.2 RD model with bias-correction

The rdrobust stata package also implements another procedure that corrects for the possible bias in the classical RD estimates shown in the results section. Hyytinen et al. (2018) suggests that it is important to correct this bias. Figure 15 shows that the estimates in the default rdrobust specification remain very robust to the bias-correction procedure. This is true for other RD specifications as well, especially when one uses automatic bandwidth selection. For some specifications, the bias-correction yields slightly larger (in magnitude) estimates at the end of the time horizon but overall across all specifications, the general time profile of estimates is similar with and without bias-correction (appendix figure A28).

[48]The choice of these three variables is based on the fact that all observations have information for these three variables whereas if I use all six covariates then my sample size drops by almost 25,000 because not all observations have information on the remaining three covariates (marital status, children, and education level). Also, the biggest concern in the placebo test was about immigrant status. In addition, immigrant status and language dummy have the largest point estimates which tells us that it is more important to control for these. The RD estimates in the smaller sample (25000 less observations) with and without all six covariates are very similar (graphs available on request)

7.2.3 RD model for short-term unemployed

I remove the observations who have been receiving assistance with jobsearch from employment services providers for more than 12 months at the start of the PaTH program (1 Jan 2017) and thus, among those who were eligible at the beginning of the program, restricting sample to relatively short term unemployed (less than 12 months). Figure 16 shows that the estimates remain reasonably robust when longterm unemployed are removed from the sample. This is true for the 19 other specifications as well (appendix figure A29).

7.2.4 RD model with longer time horizon

I construct another analysis sample which consisted of individuals meeting the second eligibility rule sometimes between 1 Jan 2017 and 31 March 2018 (the original sample's time period was 1 Jan 2017 to 28 February 2019). I call this the March sample. In this sample, I follow an individual up to 34 months to see whether a person is off or on income support in these months[49]. Figure 17 shows that the time profile of ITT effects till month 23 are very similar to the estimates from original sample. It also shows that the higher estimates observed at the end of the time horizon for original sample is a temporary phenomenon as the estimates in the March sample seems to be approaching -1 percentage point again by the end of the 3rd year after a persons becomes eligible. The time profile of estimates are reasonably similar across 19 other RD specifications (appendix figure A30).

7.3 Robustness checks for question 2

In the main RD analysis for question 2, the analysis sample consists of individuals who meet the second eligibility rule sometimes between Jan 2017 and Dec 2018. An issue with this sample is that it allows for the possibility that some individuals in the control group may have been eligible for YWS in the past which may bias the RD estimates. To address this issue, I construct another analysis sample consisting of individuals who meet the second eligibility rule between July 2015 (when the program started) and Dec 2018. In this new sample, it is not possible for a member of control group to be eligible for YWS in the past. I call the original sample the January 2017 sample and the new one July

[49] 34 is the limit because those eligible in April 2018 can be followed for maximum of 34 months - end of time in RED data is Jan 2021 i.e. 34 months between March 2018 and Jan 2021

2015 sample. Figure 18 shows that the overall pattern of ITT estimates does not change; the effect size is centered around zero for the first 12 months and then they are centered around -0.01. This is true across other specifications as well (appendix figure A31).

7.4 Alternate outcome variable: total months on income support

I estimate the treatment effects using alternate outcome variable: total months on income support (out of 23 months after meeting the second eligibility rule). This variable may be of more interest to the policy makers than the one used for the main analysis. Table 3 shows the treatment effect estimates for this outcome variable. Column 1 shows the treatment effect being estimated and column 2 presents the point estimates and their standard errors. The estimates are from Diff-in-Diff specification with bandwidth size 2.5. Column 3 shows the number of months an average member of a control group is OFF income support (this is equal to 23 - outcome variable). Column 4 presents the point estimates as percent of values in column 3. The ITT and ATET effects of YWS are statistically insignificant at 5% level (rows 2 and 5). The ITT effect of PaTH is -0.43 (row 3). Column 3 and row 3 show that an ineligible person spends about 7 months and 1 week off income support (out of 23 months after becoming eligible) and so the ITT effect of PaTH is about 6% of control mean. The ATET effect of PaTH is -2.16 (row 4) which means that, comparing to counterfactual of no participation, a person participating in a component of PaTH spends 2 months less on income support (in the 23 months after becoming eligible) or alternatively, spends 2 more months OFF income support; this is a large effect considering that a member of control group spends 7 month off income support. Direct comparison of PaTH and YWS in row 1 shows that there is a statistically significant ITT effect of PaTH vs YWS with magnitude of about 1 week.

Figures A32, A33, A34, A35, and A36 show estimates for the treatment effects from different RD and Diff-in-Diff specifications. The estimates are of a similar magnitude as in table 3. In figure A32 the mean ITT effect of PaTH vs YWS across RD specifications is -0.27 and across Diff-in-Diff specifications is -0.19 whereas in row 1 of table 3 the effect size is -0.23. In figure A33 the mean ITT effect of YWS vs control across RD specifications is -0.10 and across Diff-in-Diff specifications is -0.11 whereas in row 2 of table 3 the effect size is -0.09. In figure A34 the mean ITT effect of PaTH vs YWS across Diff-in-Diff

specifications is -0.3 whereas in row 3 of table 3 the effect size is -0.43. So there is some discrepancy across specifications for ITT effect of PaTH vs control. Since ATET estimates are obtained from ITT estimates so similarity across specifications for ATET follows from similarity in ITT estimates.

7.5 Donut RD: Sensitivity to observations near the cutoff

Cattaneo et al. (2019) suggests that the sensitivity of results to observations near the cutoff should be tested. It is important to test for this sensitivity when there is a possibility of manipulation of running variable. In this paper it is also important to test for this sensitivity because of the possibility of measurement error in the constructions of analysis sample and the running variable. A 'donut hole' approach is used for this sensitivity test. In this approach observations in some radius around the cutoff point are excluded from the analysis. Figure A37 shows donut RD estimates for the ITT effect of PaTH vs YWS for different radius sizes. The estimates are for the default rdrobust specification. The top panel shows estimates for the original outcome variable (to avoid cluttering, estimates at four points in time are shown) and the bottom panel for the alternate outcome variable. The estimates are fairly robust for different radius sizes: the sign of the estimates are mostly consistent and the confidence intervals overlap a lot. One important observation is that the estimates for month 23 become smaller in magnitude and are statistically insignificant when radius is greater than 0; this increases the similarity between RD and Diff-in-Diff estimates discussed in the results section. Similar estimates showing robustness to different radius sizes are obtained for the ITT effect of YWS vs control (figure A38).

8 Conclusion

Can wage subsidies can help unemployed youth rely less on welfare payments? If yes, then will youth's reliance on welfare payments decrease even further when we increase the subsidy amount and also provide training and internship opportunities? The paper finds that low wage subsidy incentives (YWS) have statistically insignificant effect on the welfare exit rates of youth. But the effect size is three times larger and statistically significant at usual levels when the subsidy amount is increased and training and internships

are provided as well. The magnitude of the effects are economically meaningful, in line with international literature but smaller than past Australian studies. Identification tests, robustness checks, and similarity between estimates from two different methodologies increase our confidence that the two research questions have been answered carefully. The ATET estimates be considered with caution as they are only an approximation.

The results have two policy implications. First, based on back of the envelop calculations, the government recovers the cost of the PaTH program within two and a half years[50]. The cost of PaTH program from Jan 2017 (start of PaTH) to Jan 2019 was A$226 millions (Senate Estimates (b)). The estimates in the paper implies that PaTH participant is 9.4 percentage points less likely to be on income support. Thus the benefit of PaTH = (number of PaTH participants kept off income support) × (welfare payments per fortnight that unemployed jobseekers receive, A$) × (number of fortnights a PaTH participant is kept off income support payments[51]) = (6276[52]) × (600) × (60) = A$226 million. If these people not on income support are employed, there may also be income tax receipts on top of the saving in income support payments.

The second policy implications is that the benefit of PaTH package is three times larger (or 200% more) than the benefit of YWS[53] but the maximum per participant cost of PaTH is at most two times larger (or 100% more) than the maximum per participant cost of YWS[54]. This assumes that the PaTH participant completes all three elements of PaTH. But in reality, about 75% of PaTH participants complete only one component (DESE, 2020). Using the true per participant cost, the cost of PaTH is only 13% larger than the cost of YWS[55]. Thus, providing training and internship opportunities alongside

[50] More precisely, within 60 fortnights.

[51] Based on ITT estimates in robustness checks we know that effects can persist for at least 34 months (or about 72 fortnights).

[52] Number of individuals who participate in at least one component of PaTH = 66770 (DESSFB, 2019, page 1). Number of PaTH participants kept off income support = 66770 * ATET = 66770 * 0.094 = 6276.

[53] This is another way of saying that the ITT or ATET estimates of PaTH are at least three times as large as the ITT or ATET estimates of YWS.

[54] Max per participant cost of PaTH = A$10000 (YBWS) + A$2200 (Internships) + A$1000 (EST) = A$13200. Max per participant cost of YWS = A$6500. We know the max subsidy amounts from institutional rules. We also know that internships provide firms a one time payment of up to $1000 for hosting the intern and provides the interns A$200 per fortnight. The max duration of internship is 12 weeks. EST costs are calculated using the number of EST participants from DESSFB (2019, page 1) and Senate Estimates (b).

[55] Cost per YBWS participant is A$4500 (using Senate Estimates (b) and DESSFB (2019)). Since max YWS is two thirds of max YBWS so I assume cost per participant of YWS is A$3000. Total cost of PaTH is A$226 million (Senate Estimates (b)). Total number of individuals who participate in at least one component of PaTH = 66771. Thus true cost per PaTH participant is A$3385 (= 226000000/66771) which is close to per participant cost of A$3000 for YWS.

wage subsidies is a cost-effective way to increase the usefulness of wage subsidies.

There are two avenues for future research. The first avenue will depend on the type of data sources available to researchers. I will give examples of Australian data sources:

- Employment Services System (ESS) administrative database has information on participation in active labor market programs. Linking ESS with RED will improve the current paper in two ways. Firstly, one no longer needs to use proxy variables to obtain eligible population. Secondly, direct ATET estimates can be calculated using matching. Other approaches for estimating ATET may also be used. For example, one could use the random allocation of welfare recipients to employment services providers (and random allocation of recipients to case workers within each employment service provider) and the variation in the success rate of service providers (or case workers) in finding subsidized jobs or internships to estimate ATET. Weatherburn & Williams (2020) uses a similar approach to answer whether electronic monitoring reduce reoffending in Australia.

- RED + ESS + ALife (tax data): Combining these three admin data sources one can estimate the effect of PaTH and YWS on labor market outcomes (employment status, wages, occupational structure, retirement savings).

- RED + ESS + ALife + BLADE (firm data): A linked employer employee data can help look at both firm and worker level outcomes. Saez et al. (2019) is a recent study that that looks at both firm and worker level outcomes associated with employer level tax credit for young employees. de Mel et al. (2019) is another paper that looks at the effect of wage subsidies on small firms in a developing country.

The second avenue for future research has to do with studying macro level or general equilibrium effects of PaTH and YWS. Will ineligible group suffer because now firms will hire eligible group members whereas in the absence of programs firms would have hired members of ineligible group (substitution effect)? Will new subsidized hires replace existing employees or reduce their working hours? Will firms hire individuals even without subsidies (deadweight loss)? Does overall welfare increase because of the program? One can use structural methods (Example: Bucher (2010)) or use program evaluation techniques and certain institutional features (Examples: Blundell et al. (2004), Boockmann et al. (2013), and Pasquini et al. (2019)) to answer these questions.

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Tables

Table 1: Treatment Effects Estimated

Qts No.	Treatment Effect	Type of Treatment Effect	Age Groups Compared	Methodology
1	Eligibility for PaTH (subsidy = A$10000, internship, EST) versus Eligibility for YWS (subsidy = A$6500)	ITT	[15, 25) vs [25, 30)	RD and Diff-in-Diff
2	Eligibility for YWS (subsidy = A$6500) versus no intervention	ITT	[25, 30) vs [30, 35)	RD and Diff-in-Diff
3	Eligibility for PaTH (subsidy = A$10000, internship, EST) versus no intervention	ITT	[15, 25) vs [30 to 35)	Diff-in-Diff
4	Participation in one element of PaTH versus no intervention	ATET	[15, 25) versus [30, 35)	Two Sample IV
5	Participation in YWS versus no intervention	ATET	[25, 30) versus [30, 35)	Two Sample IV

Table 2: Summary of Results

Qts No.	Treatment Effect	Type of Treatment Effect	Methodology	Estimates (average over time)
1	Effect of eligibility for PaTH vs YWS	ITT	RD and Diff-in-Diff	-1.1 pp
2	Effect of eligibility for YWS vs Control	ITT	RD and Diff-in-Diff	-0.5 pp
3	Effect of eligibility for PaTH vs Control	ITT	Diff-in-Diff	-2.0 pp
4	Effect of participating in PaTH vs Control	ATET	Two Sample IV	-9.4 pp
5	Effect of participating in YWS vs Control	ATET	Two Sample IV	-3.0 pp

Table 3: Summary of results with new outcome variable: total months on income support

	(1) Treatment effect	(2) Effect size	(3) Control mean	(4) Effect size as percent of control mean
1	PaTH vs YWS (ITT)	-0.23***	8.26	2.78
		(0.06)		
2	YWS vs control (ITT)	-0.09	7.25	1.24
		(0.08)		
3	PaTH vs control (ITT)	-0.43***	7.24	5.94
		(0.07)		
4	PaTH vs control (ATET)	-2.16***	7.24	29.83
		(0.30)		
5	YWS vs control (ATET)	-0.68	7.25	9.34
		(0.59)		

Notes: Outcome variable: total number of months on income support (out of 23 months after becoming eligible). Coefficients are from Diff-in-Diff model with 2.5 bandwidth size. Standard errors in parenthesis. For ATET estimates, the standard errors are calculated using bootstrap method. Values in column (3) are equal to 23 – outcome variable i.e. the values are the mean number of months OFF income support. Values in column (4) are obtained by multiplying column (2) by -1 and dividing it by column (3) and multiplying the ratio by 100. ***: $p<0.01$; **: $p<0.05$; *:$p<0.10$.

Figures

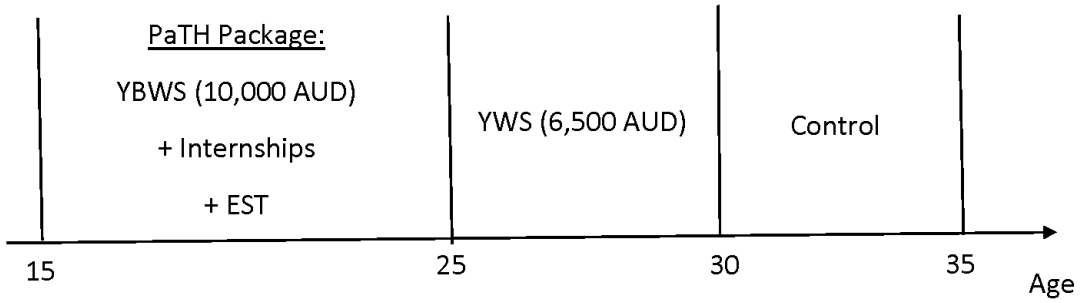

Figure 1: Age thresholds for program eligibility

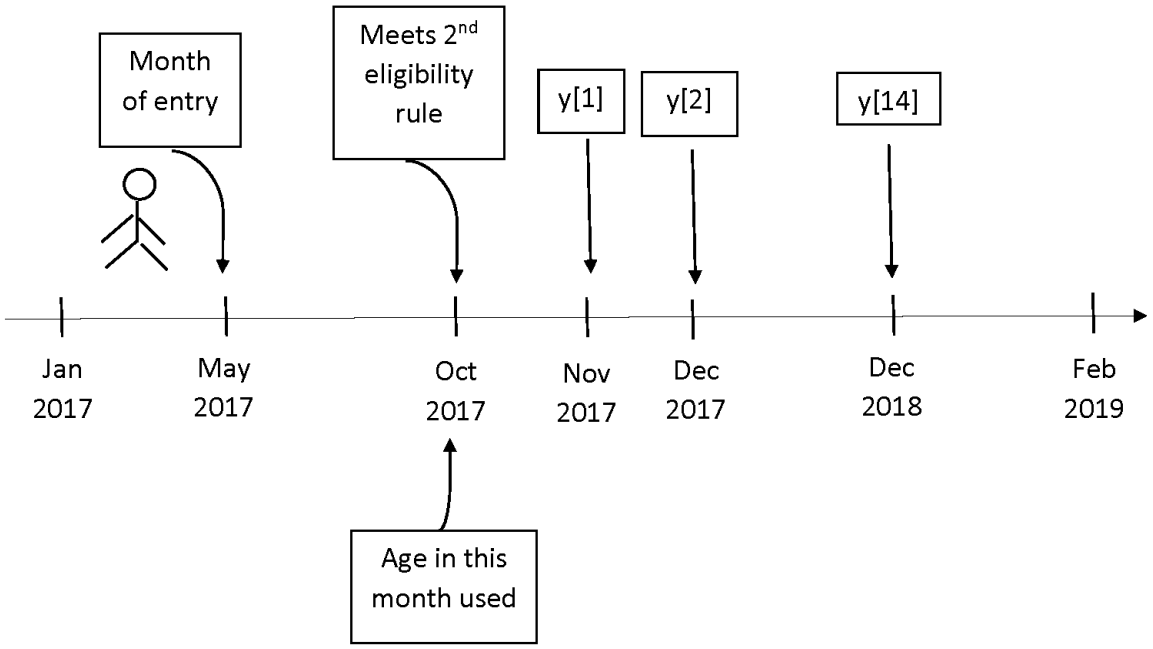

Figure 2: Example of individual in the analysis sample

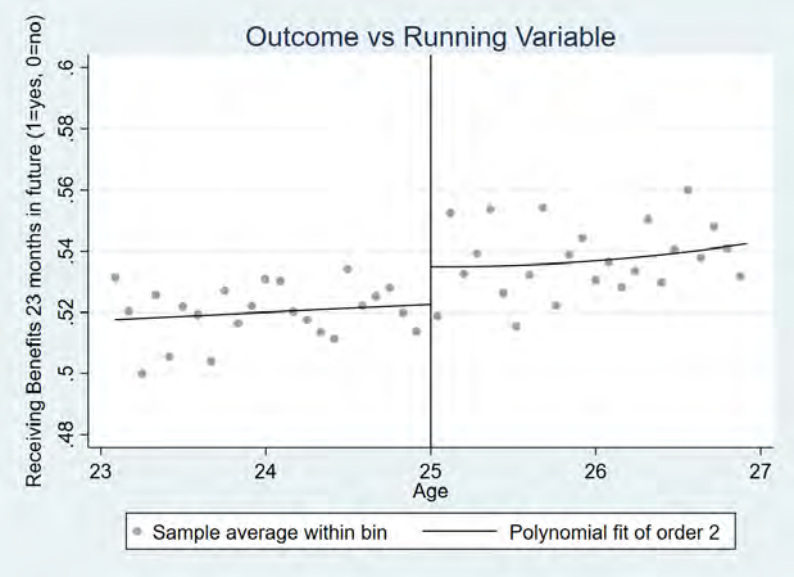

Figure 3: Outcome, y[23], vs running variable

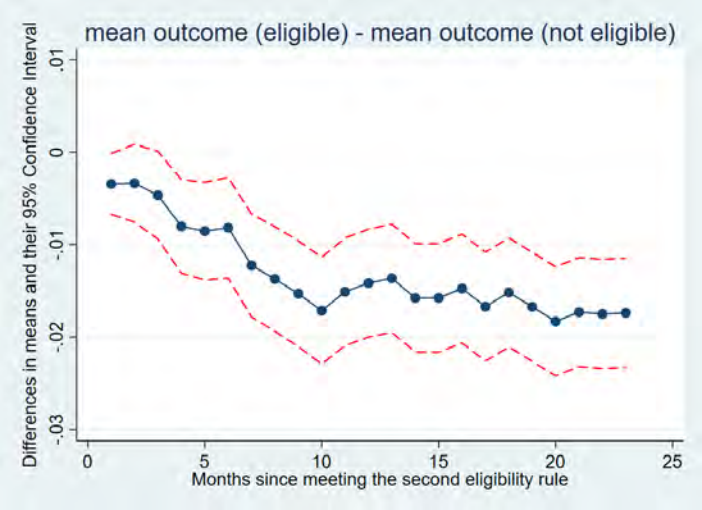

Figure 4: eligible group mean - ineligible group mean

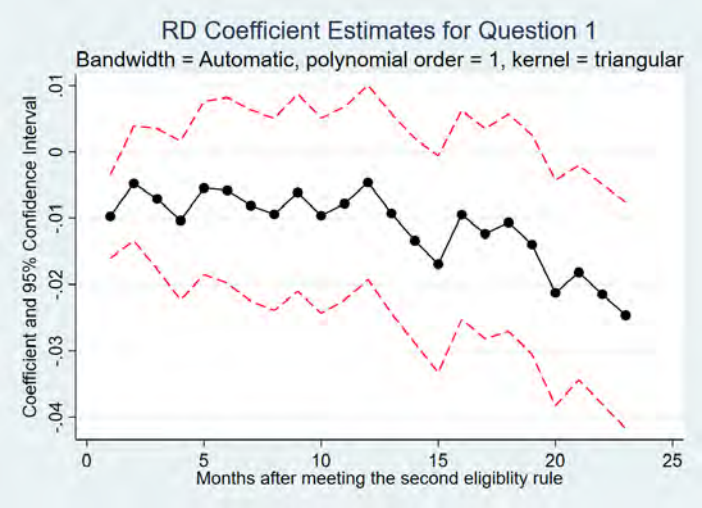

Figure 5: RD estimates for ITT effect of PaTH vs YWS

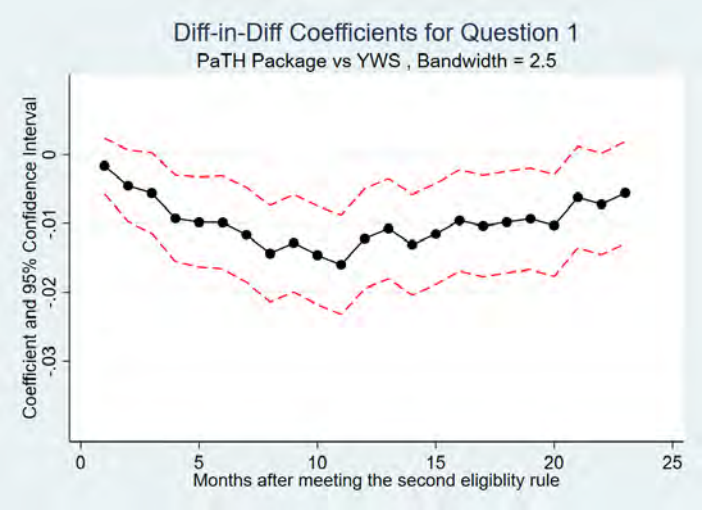

Figure 6: Diff-in-Diff estimates for ITT effect of PaTH vs YWS

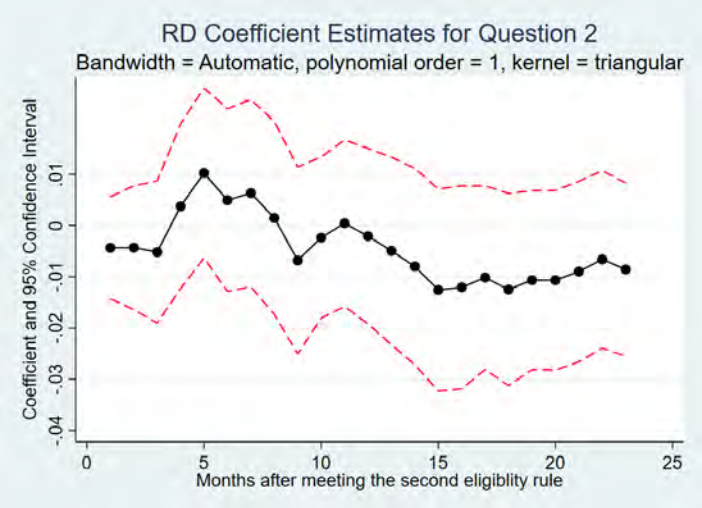

Figure 7: RD estimates for ITT effect of YWS vs Control

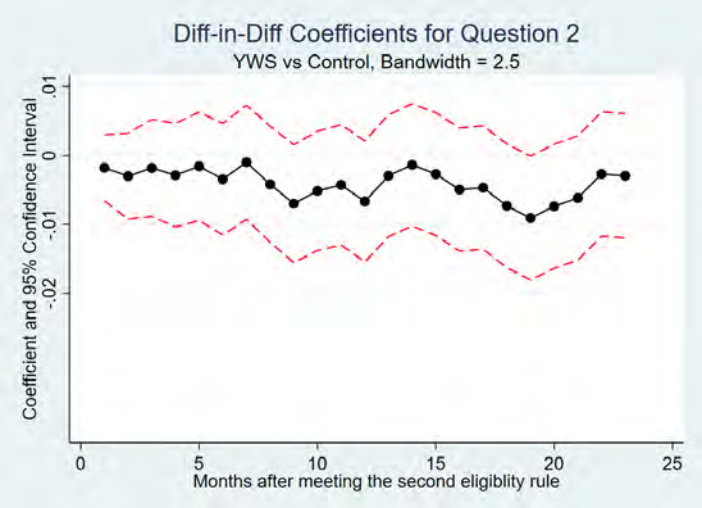

Figure 8: Diff-in-Diff estimates for ITT effect of YWS vs Control

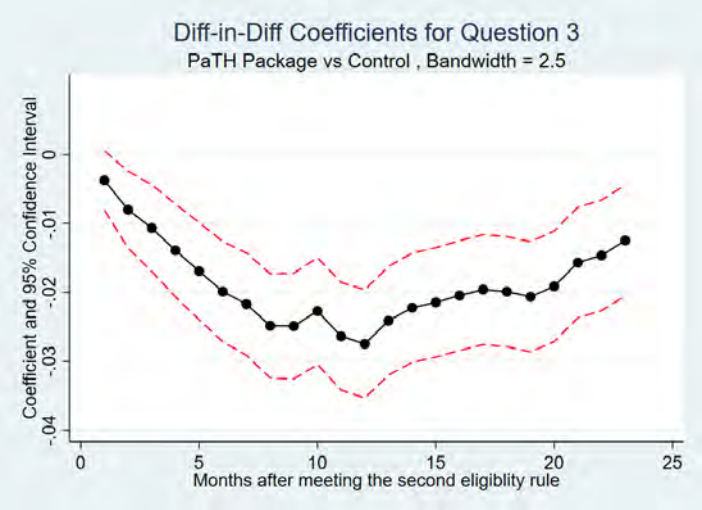

Figure 9: Diff-in-Diff estimates for ITT effect of PaTH vs Control

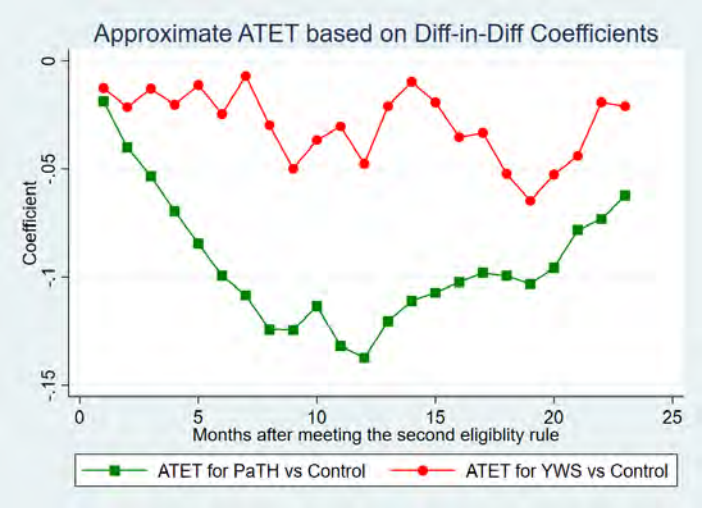

Figure 10: Estimates for ATET effect of PaTH vs Control and of YWS vs Control

Figure 11: Testing continuity of density curve of age at age 25

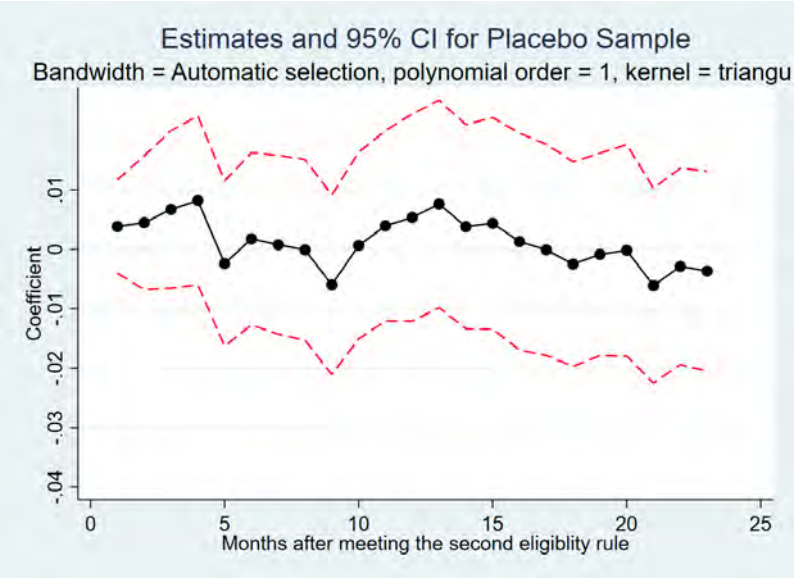

Figure 12: ITT effect of eligibility for PaTH vs YWS for Placebo Sample

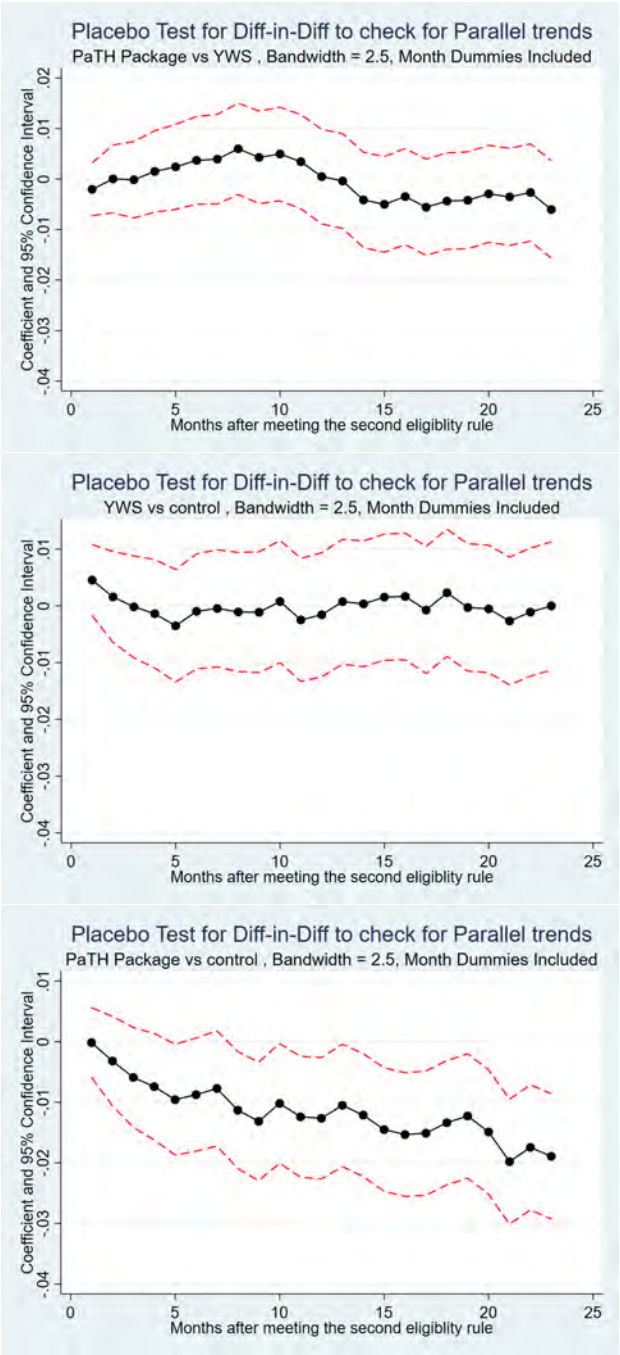

Figure 13: Placebo test for Diff-in-Diff.
The top figure is for ITT effect of PaTH vs YWS, the middle figure for ITT effect of YWS vs control, and the bottom figure is for the ITT effect of PaTH vs control.

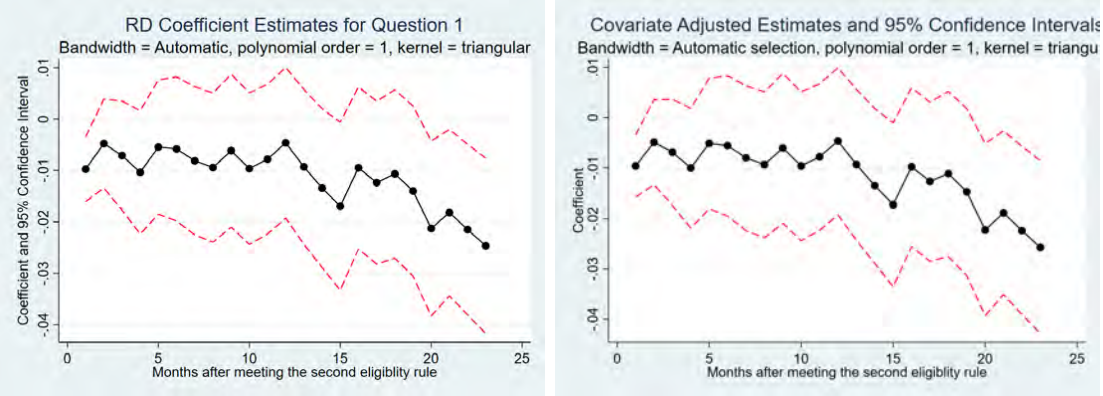

Figure 14: ITT effect of Eligibility for PaTH vs YWS with and without covariates.
Note: The left figure shows estimates without covariate adjustment and the right figure with covariate adjustment. The RD estimates are from the default specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel. The covariates included are: dummies for gender, immigrant status, and language.

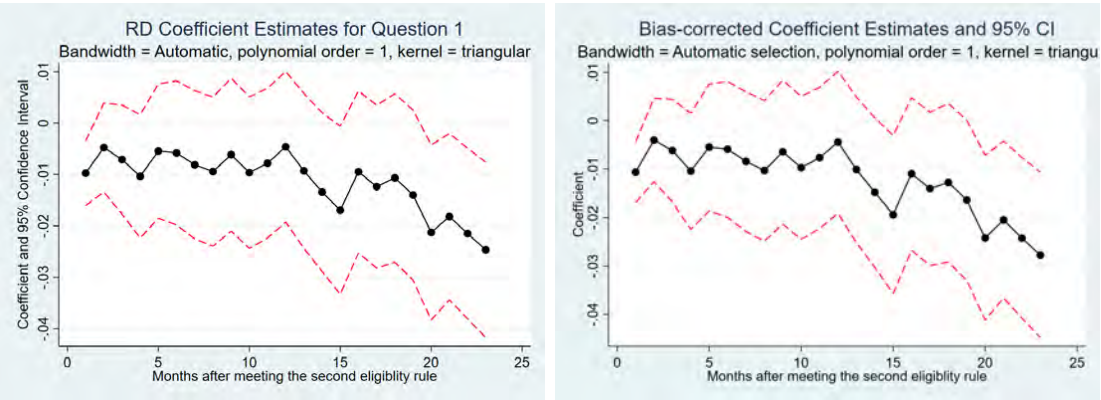

Figure 15: ITT effect of Eligibility for PaTH vs YWS with and without bias-correction.
Note: The left figure shows estimates without bias-correction and the right figure with bias-correction. The RD estimates are from the default specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel.

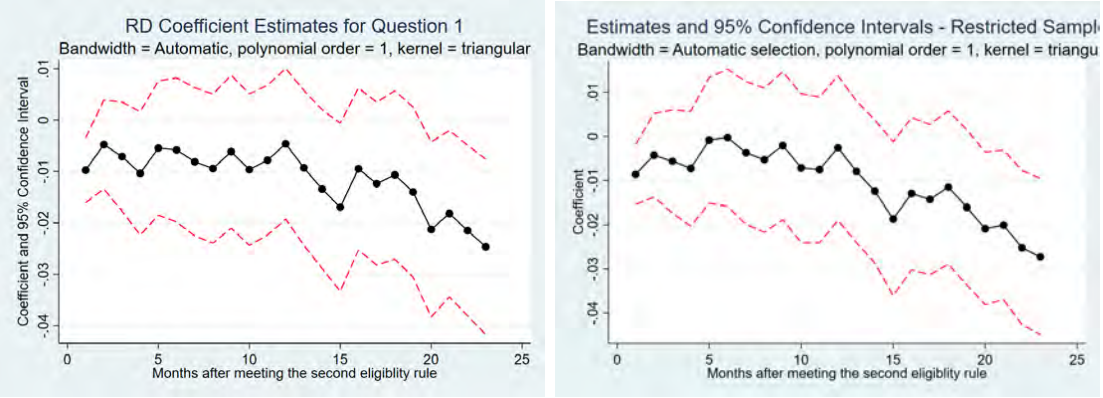

Figure 16: RD ITT effect of Eligibility for PaTH vs YWS for full and restricted sample.
Note: The left figure shows estimates for full sample and the right figure for the restricted sample where longterm unemployed eligible at the start of the program (1 Jan 2017) are removed from the sample. The RD estimates are from the default specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel.

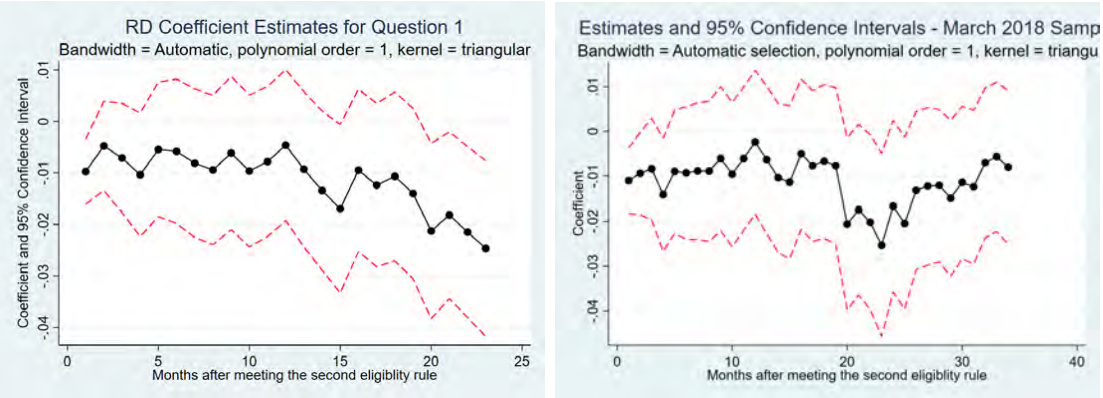

Figure 17: RD ITT effect of Eligibility for PaTH vs YWS for full and March sample.
Note: The left figure shows estimates for full sample and the right figure for the March sample. The RD estimates are from the default specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel.

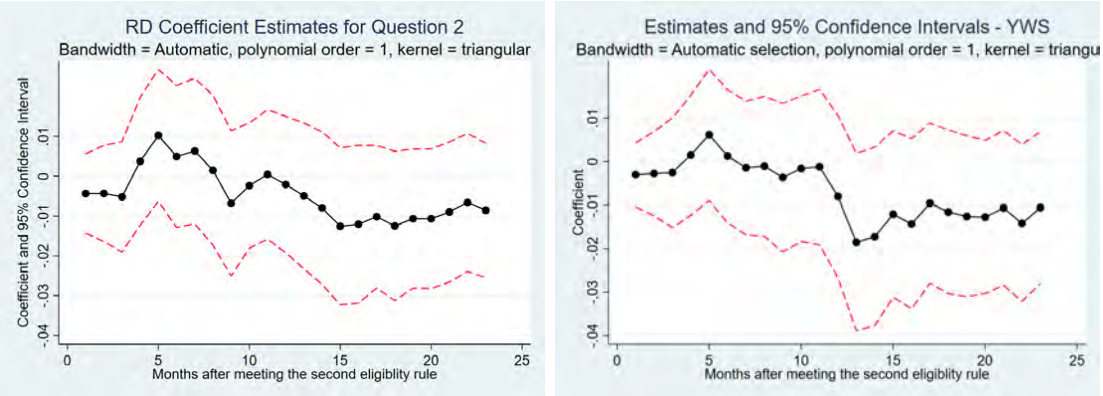

Figure 18: RD ITT effect of Eligibility for YWS vs Control for January and July samples.
Note: The left figure shows estimates for January 2017 sample and the right figure for the July 2015 sample. The RD estimates are from the default specification: automatic bandwidth selection, linear polynomial for age, and triangular kernel.

Appendix

A.1 Estimates relative to control mean

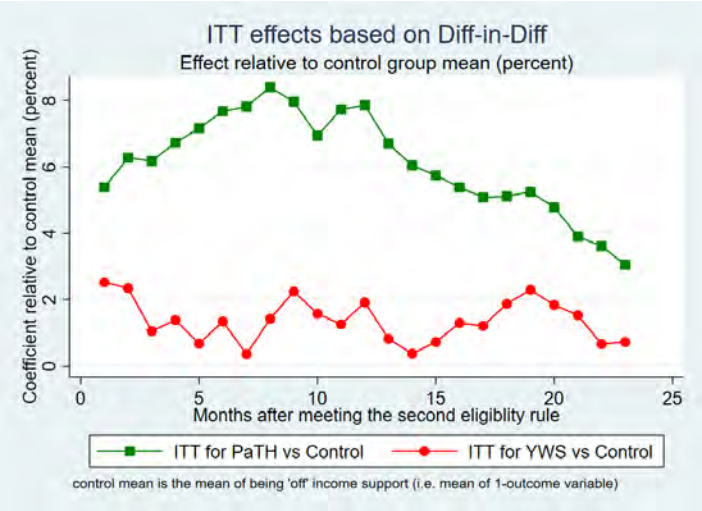

Figure A1: ITT estimates relative to control mean.
Notes: The ITT estimates are from Diff-in-Diff specification with bandwidth size 2.5. Control mean refers to the mean of (1-outcome variable) i.e it is the mean probability a member of control group is OFF income support s months after eligibility. I use absolute values of the ITT estimates to write ITT as percent of control mean.

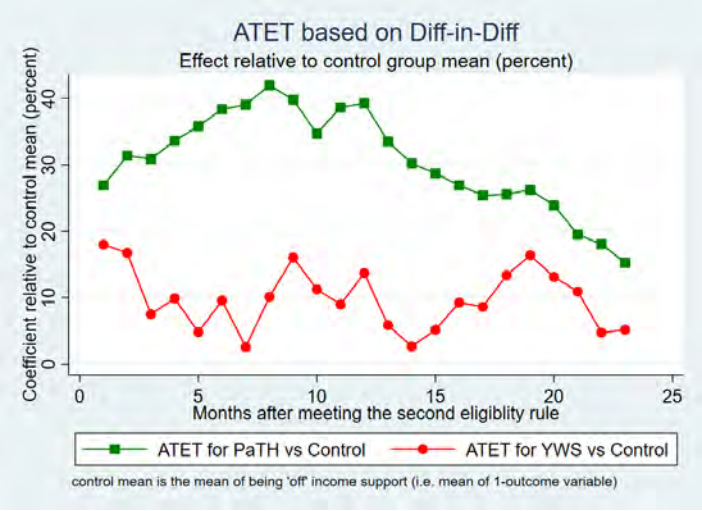

Figure A2: ATET estimates relative to control mean.
Notes: The ATET estimates are from Diff-in-Diff specification with bandwidth size 2.5. Control mean refers to the mean of (1-outcome variable) i.e it is the mean probability a member of control group is OFF income support s months after eligibility. I use absolute values of the ITT estimates to write ITT as percent of control mean.

A.2 ATET with bootstrap standard errors

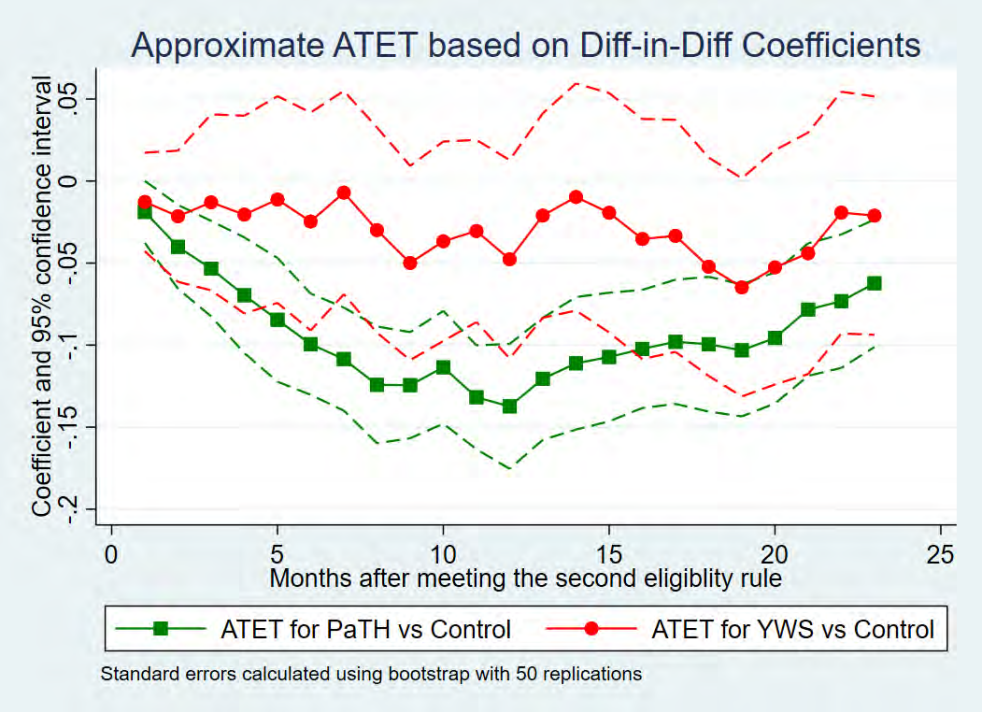

Figure A3: ATET estimates with bootstrap confidence intervals.
Notes: Standard errors calculated using bootstrap with 50 replications and the confidence intervals are normal-based i.e. it assumes the atet estimate is normally distributed. The lincom command in stata yields similar standard errors.

A.3 Placebo Threshold Age

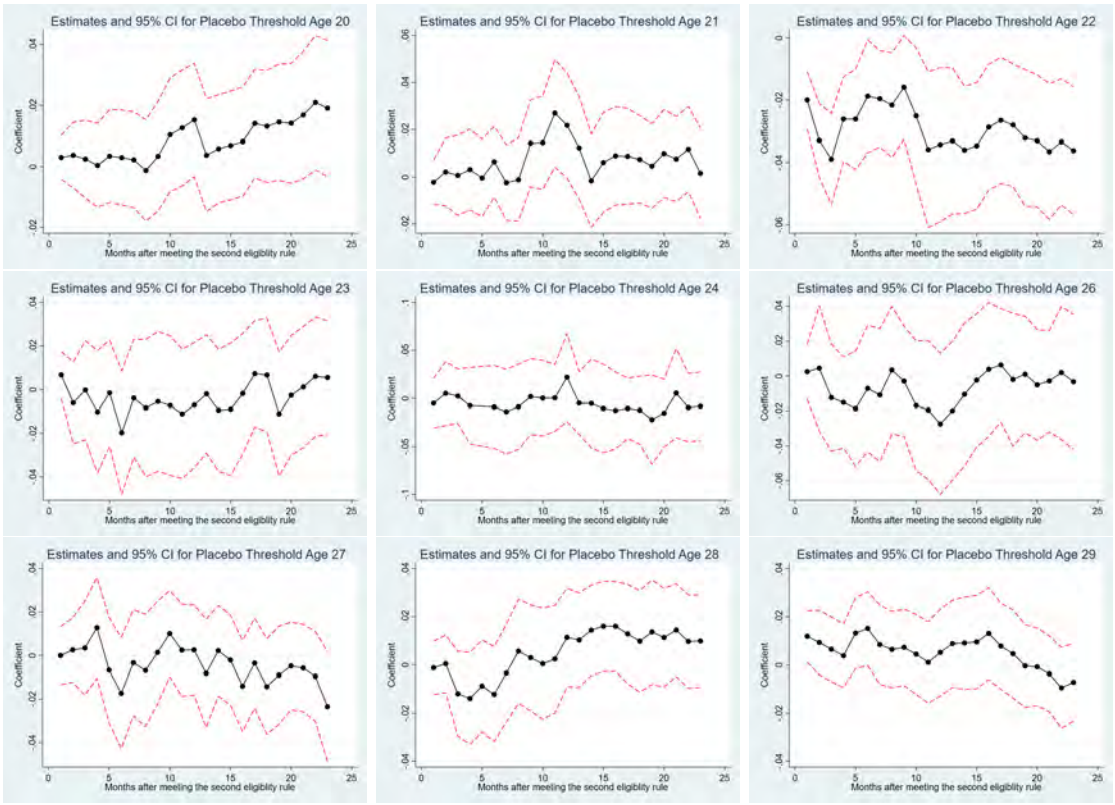

Figure A4: ITT effect of Eligibility for PaTH vs YWS for Placebo Threshold Ages.
Note: Estimates are obtained using the default RD specification of rdrobust stata command: automatic bandwidth selection, linear polynomial for age, and triangular kernel.

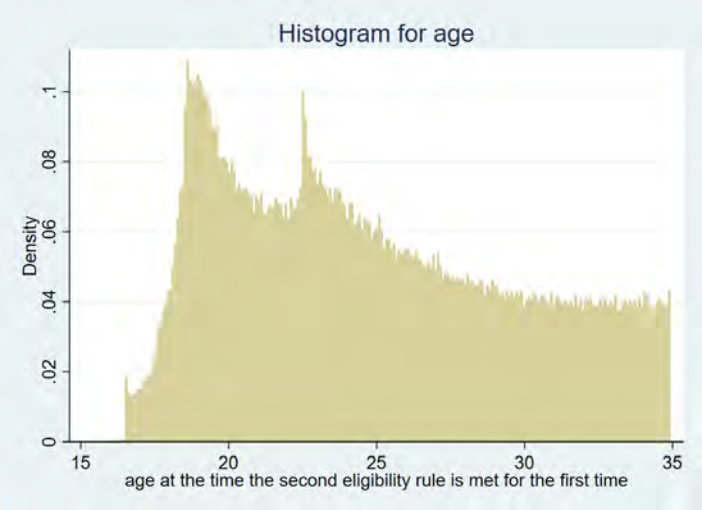

Figure A5: Histogram for age

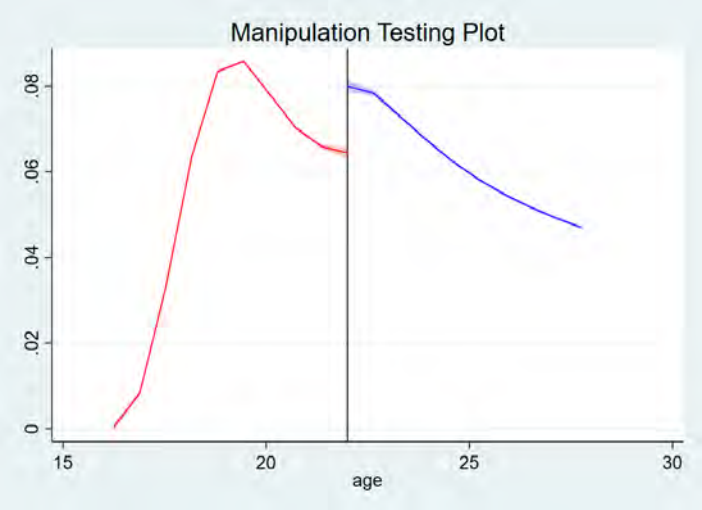

Figure A6: Density test at age 22

A.4 Placebo Outcome Variable

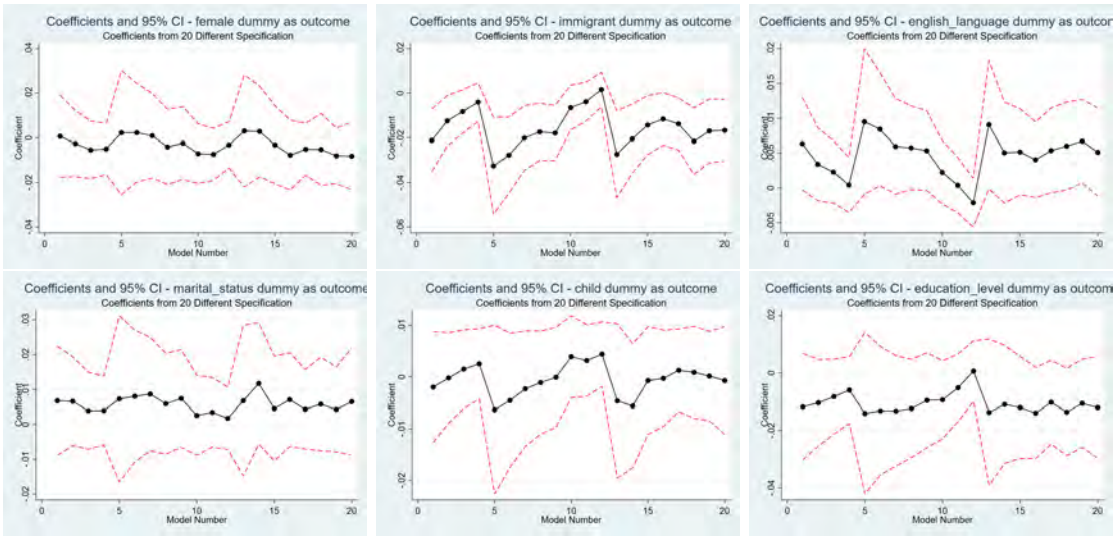

Figure A7: ITT effect of Eligibility for PaTH vs YWS for Placebo Outcome Variables.
Note: Each model referes to a different RD specification. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used.

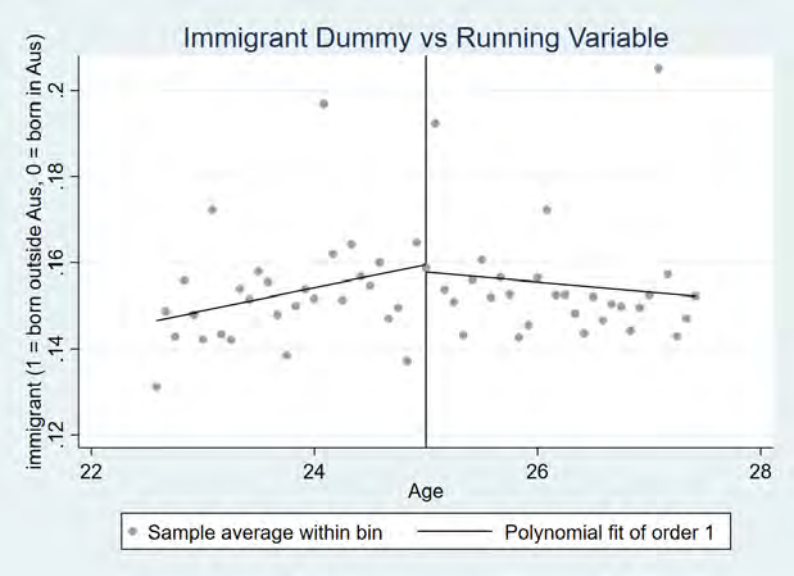

Figure A8: Immigrant dummy vs running variable.
Note: Each age bin shows the mean value of covariate for individuals within that one monthly age bin.

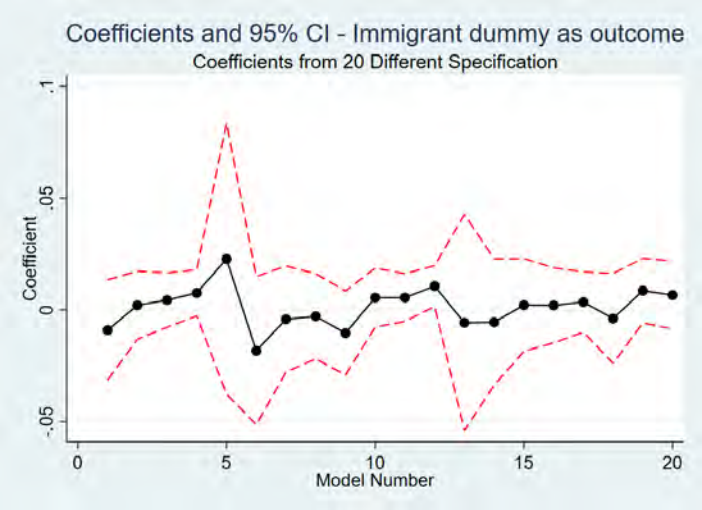

Figure A9: ITT effect of PaTH vs YWS for Immigrant Status - donut approach.
Note: Each model referes to a different RD specification. A total of twenty (5 bandwidth sizes $\times$ 2 polynomial order $\times$ 2 kernel types) specifications are used.

A.5 Placebo test for Diff-in-Diff - question 1

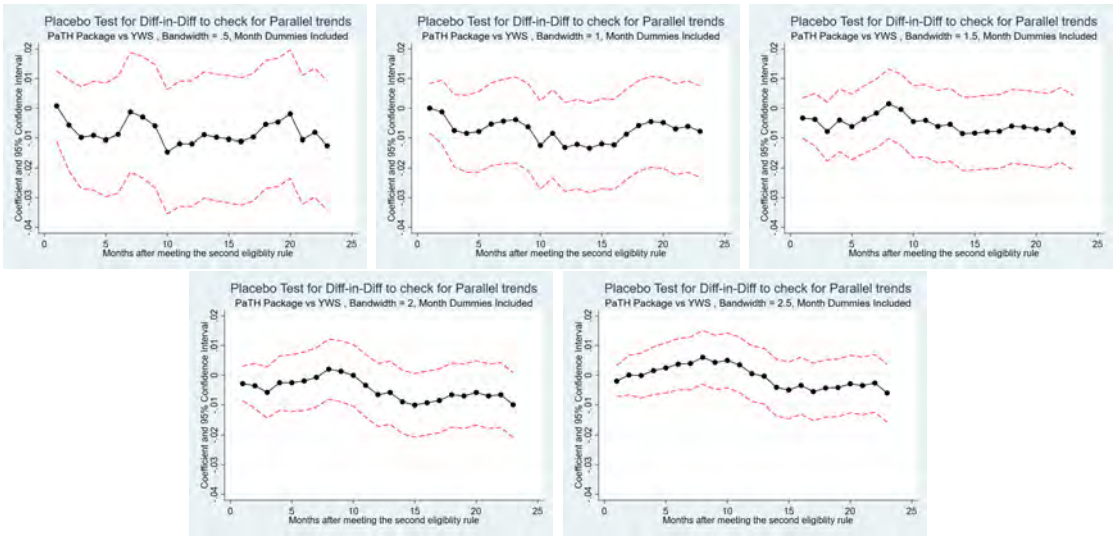

Figure A10: ITT effect of Eligibility for PaTH vs YWS for Placebo Sample.
Note: Each specification uses a different bandwidth around threshold age 25. The placebo test checks for parallel trends by estimating equation 2 using pre- and post- treatment samples before the start of the PaTH program.

A.6 Placebo test for Diff-in-Diff - question 2

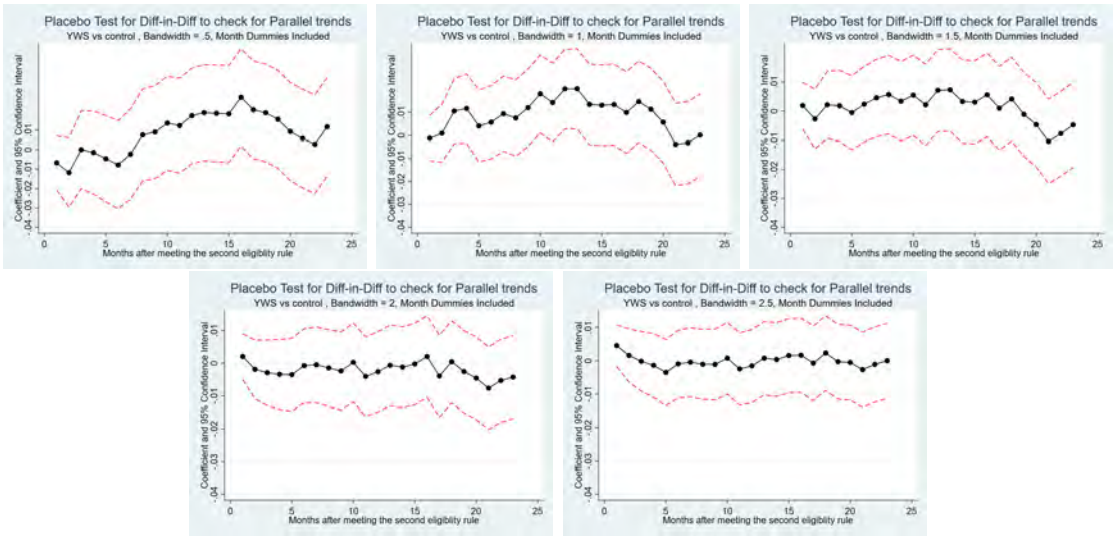

Figure A11: ITT effect of Eligibility for YWS vs control for Placebo Sample.
Note: Each specification uses a different bandwidth around threshold age 30. The placebo test checks for parallel trends by estimating equation 2 using pre- and post- treatment samples before the start of the YWS program.

A.7 Placebo test for Diff-in-Diff - question 3

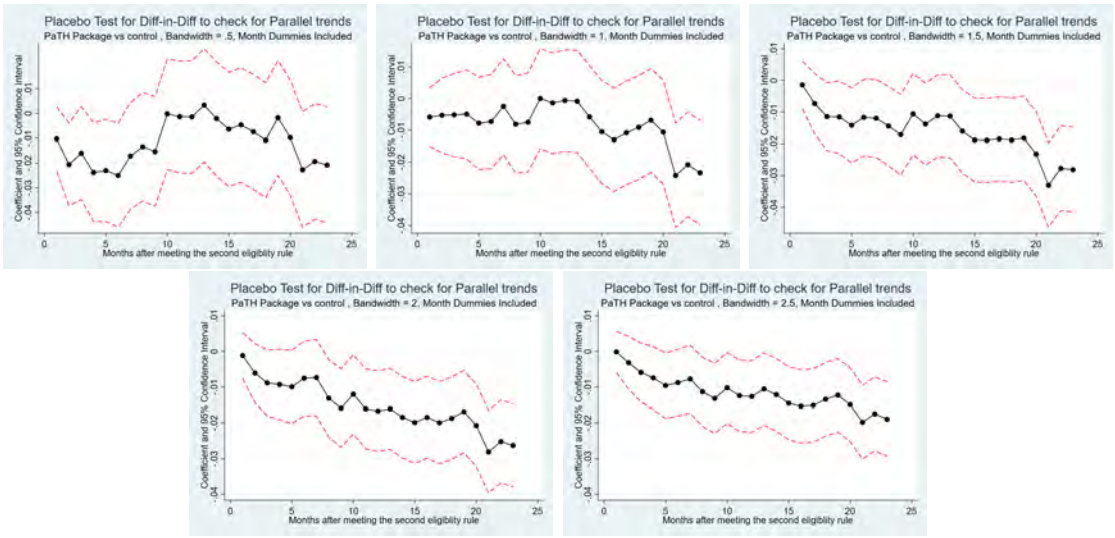

Figure A12: ITT effect of Eligibility for PaTH vs control for Placebo Sample.
Note: Each specification uses a different bandwidth. The placebo test checks for parallel trends by estimating equation 2 using pre- and post- treatment samples before the start of the PaTh and YWS programs.

A.8 RD model specifications for question 1

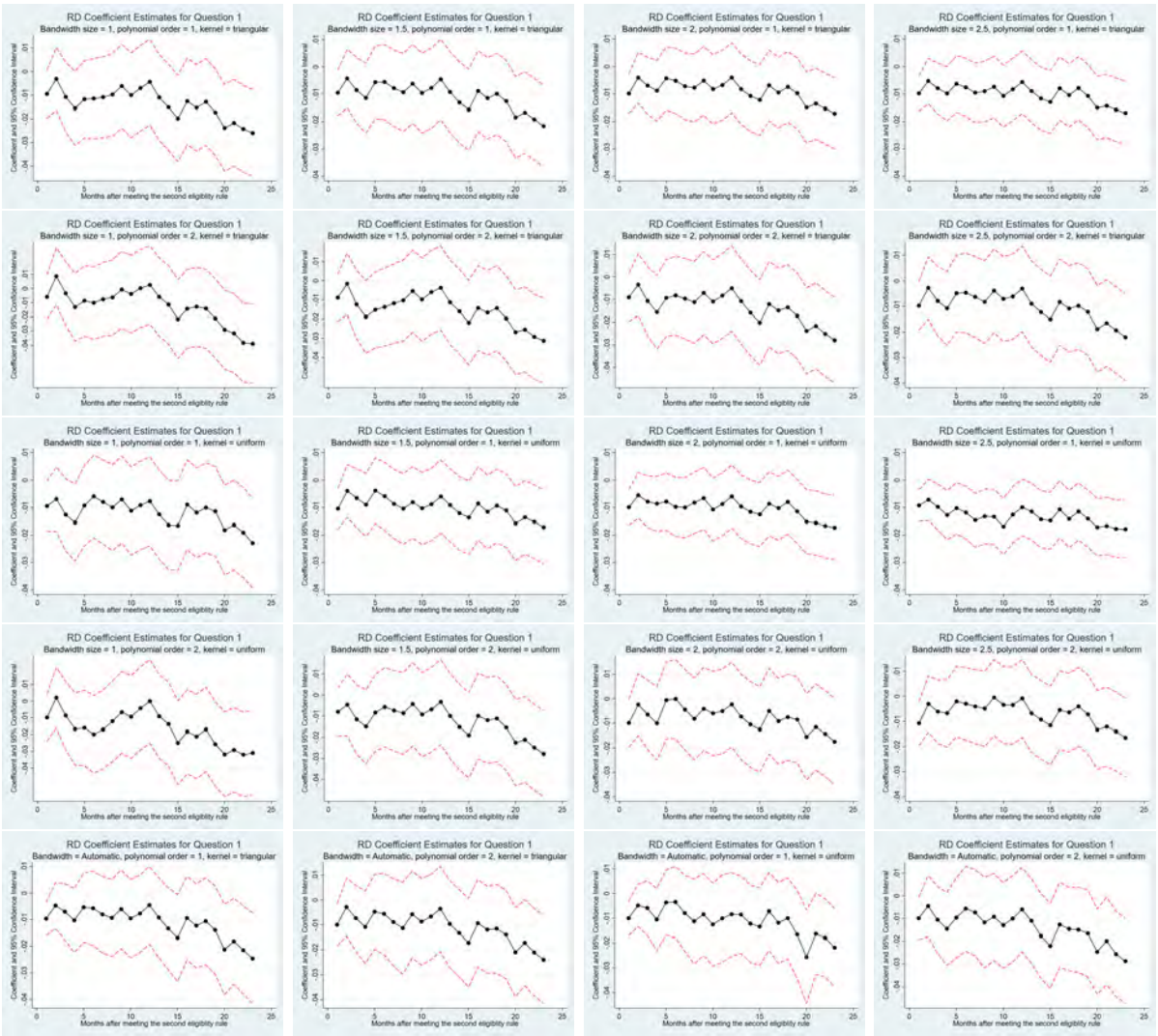

Figure A13: ITT effect of Eligibility for PaTH vs YWS for different RD specifications.
Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.9 RD model specifications for question 2

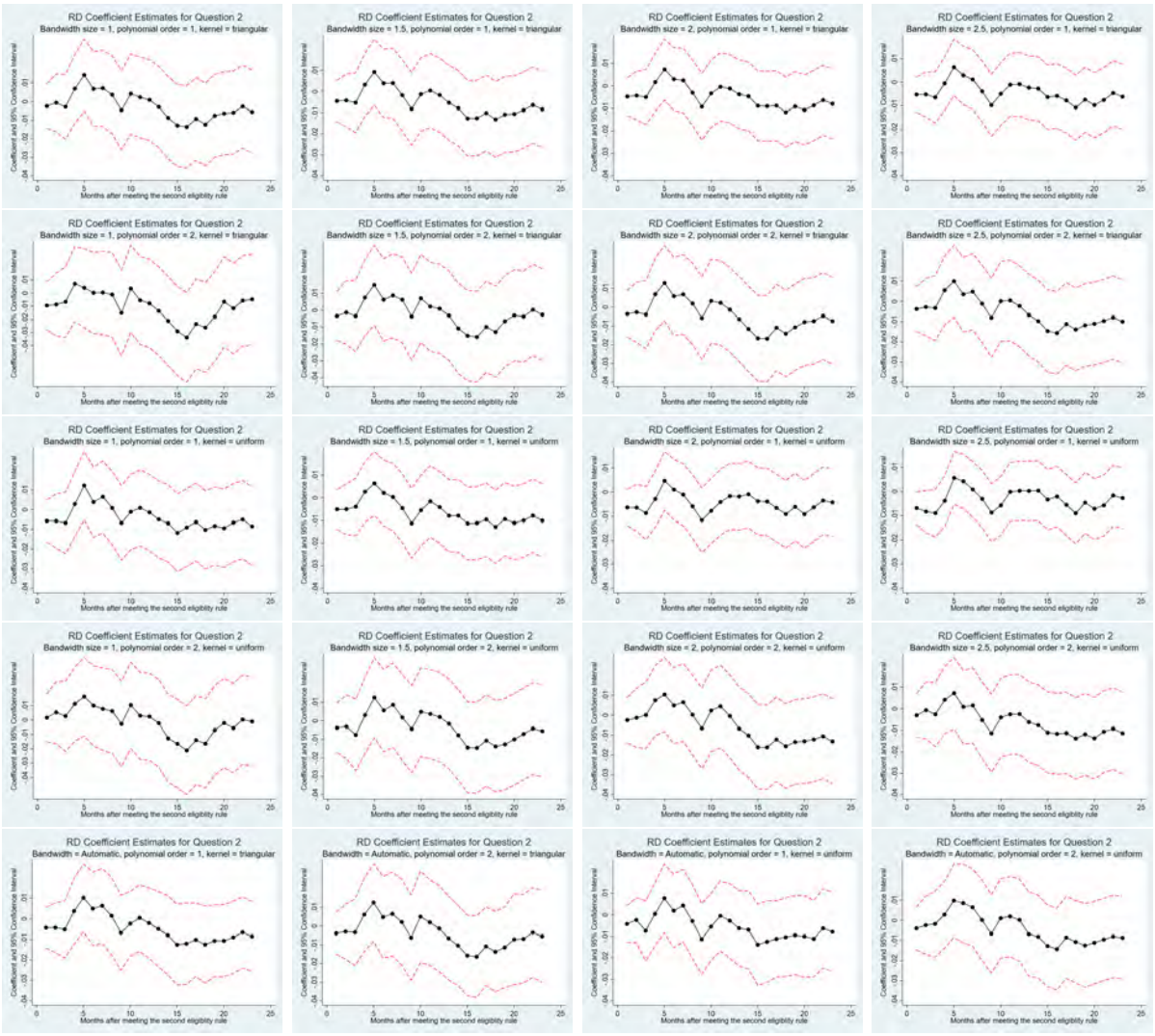

Figure A14: ITT effect of Eligibility for YWS vs Control for different RD specifications. Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.10 Diff-in-Diff model specifications for Question 1

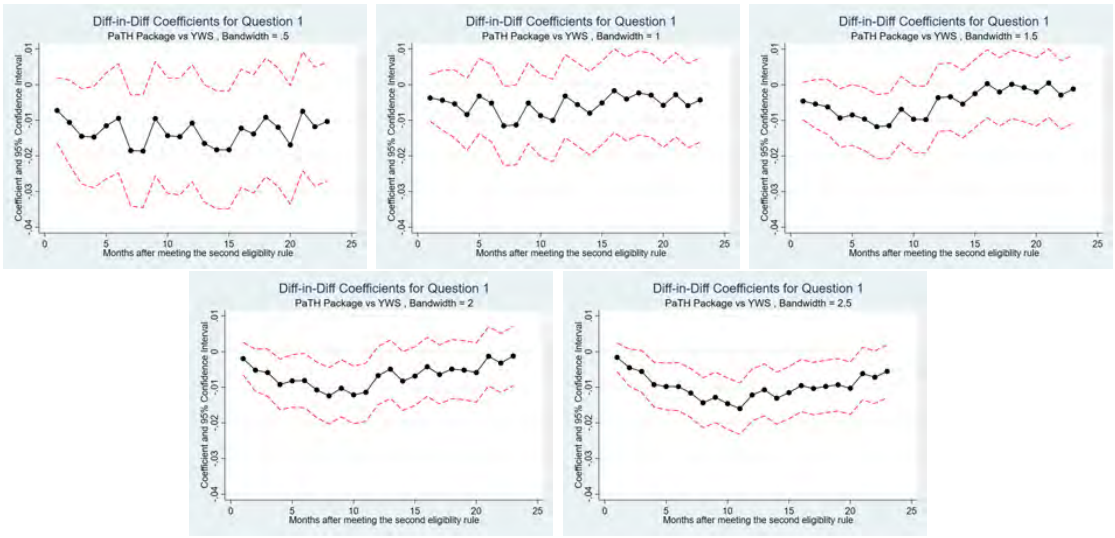

Figure A15: ITT effect of Eligibility for PaTH vs YWS for different Diff-in-Diff specifications.
Note: Each specification uses a different bandwidth around threshold age 25.

A.11 Diff-in-Diff model specifications for Question 2

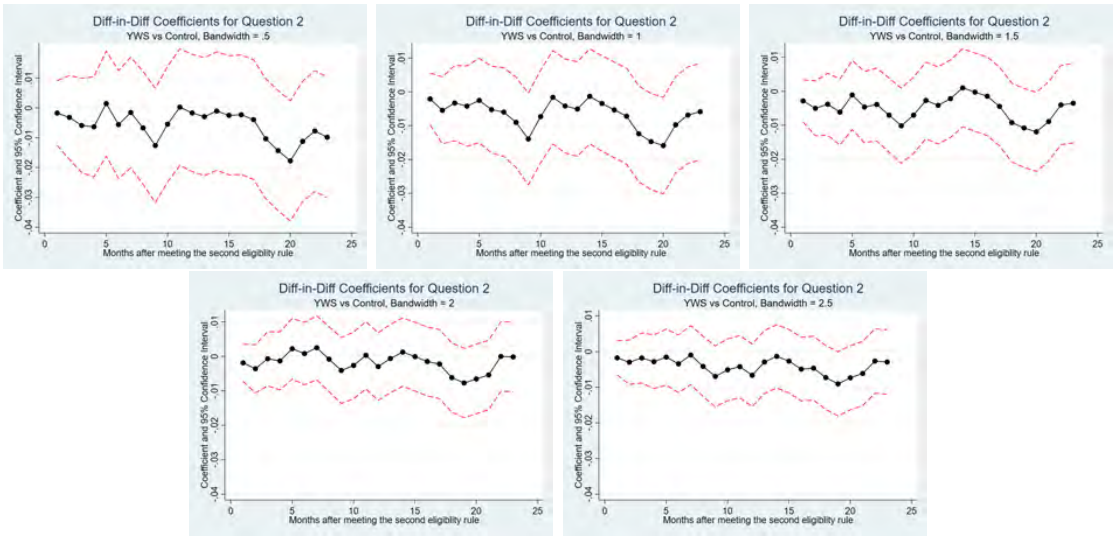

Figure A16: ITT effect of Eligibility for YWS vs Control for different Diff-in-Diff specifications.
Note: Each specification uses a different bandwidth around threshold age 25.

A.12 Diff-in-Diff model specifications for Question 3

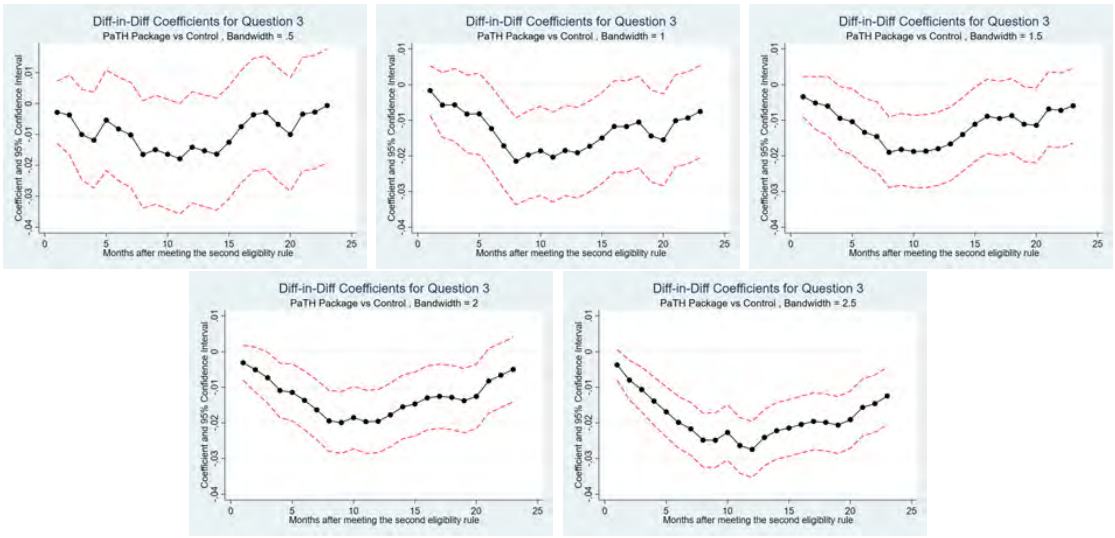

Figure A17: ITT effect of Eligibility for PaTH vs Control for different Diff-in-Diff specifications.
Note: Each specification uses a different bandwidth around threshold age 25.

A.13 Donut Diff-in-Diff model specifications for Question 1

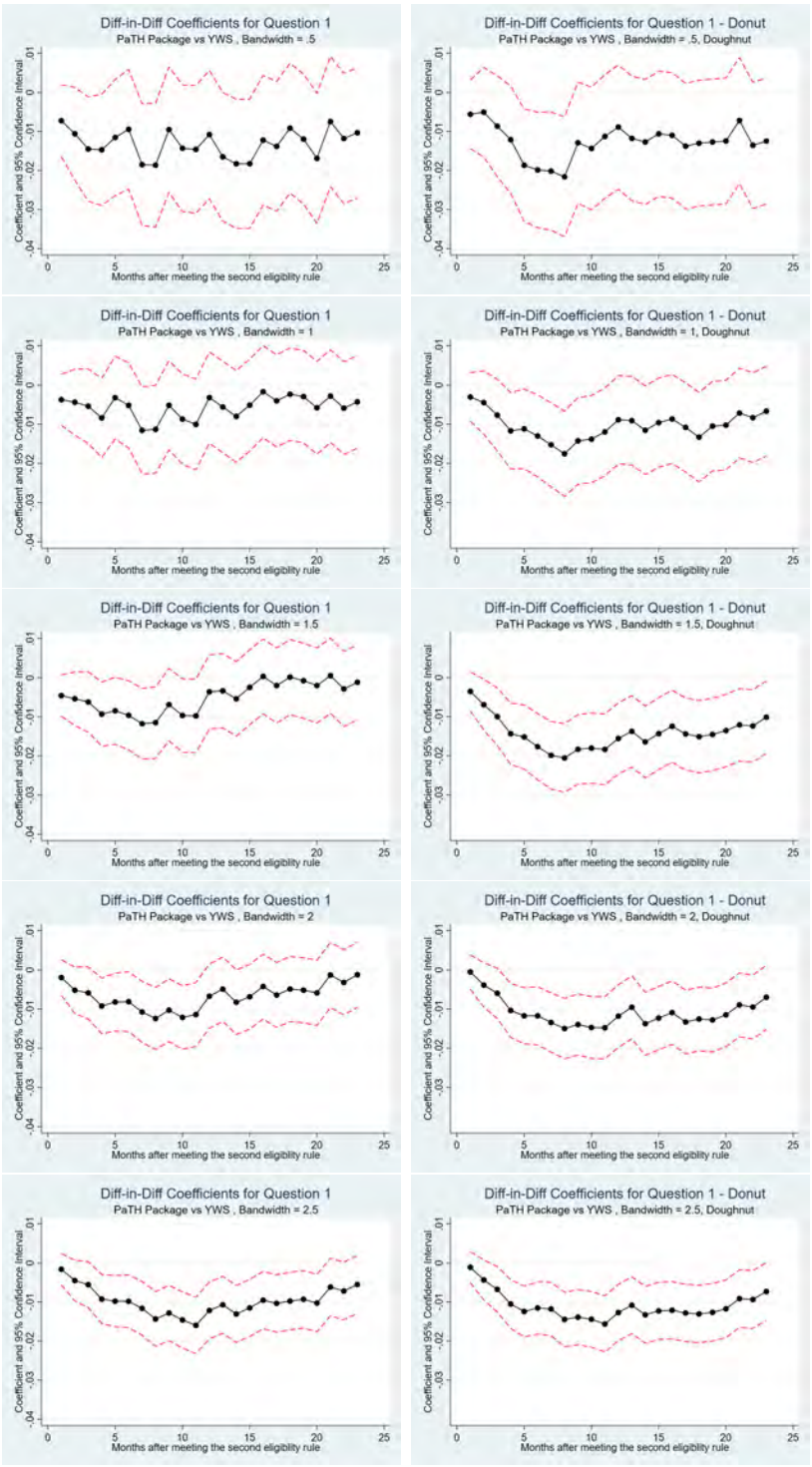

Figure A18: ITT effect of Eligibility for PaTH vs Control for different Donut Diff-in-Diff specifications.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from donut approach. The donut approach removes observations aged above 24 or above and below 25. Specification with bandwidth of size x uses observations whose age $\in [24 - x, 24)$ and $\in [25, 25 + x)$.

A.14 Donut Diff-in-Diff model specifications for Question 2

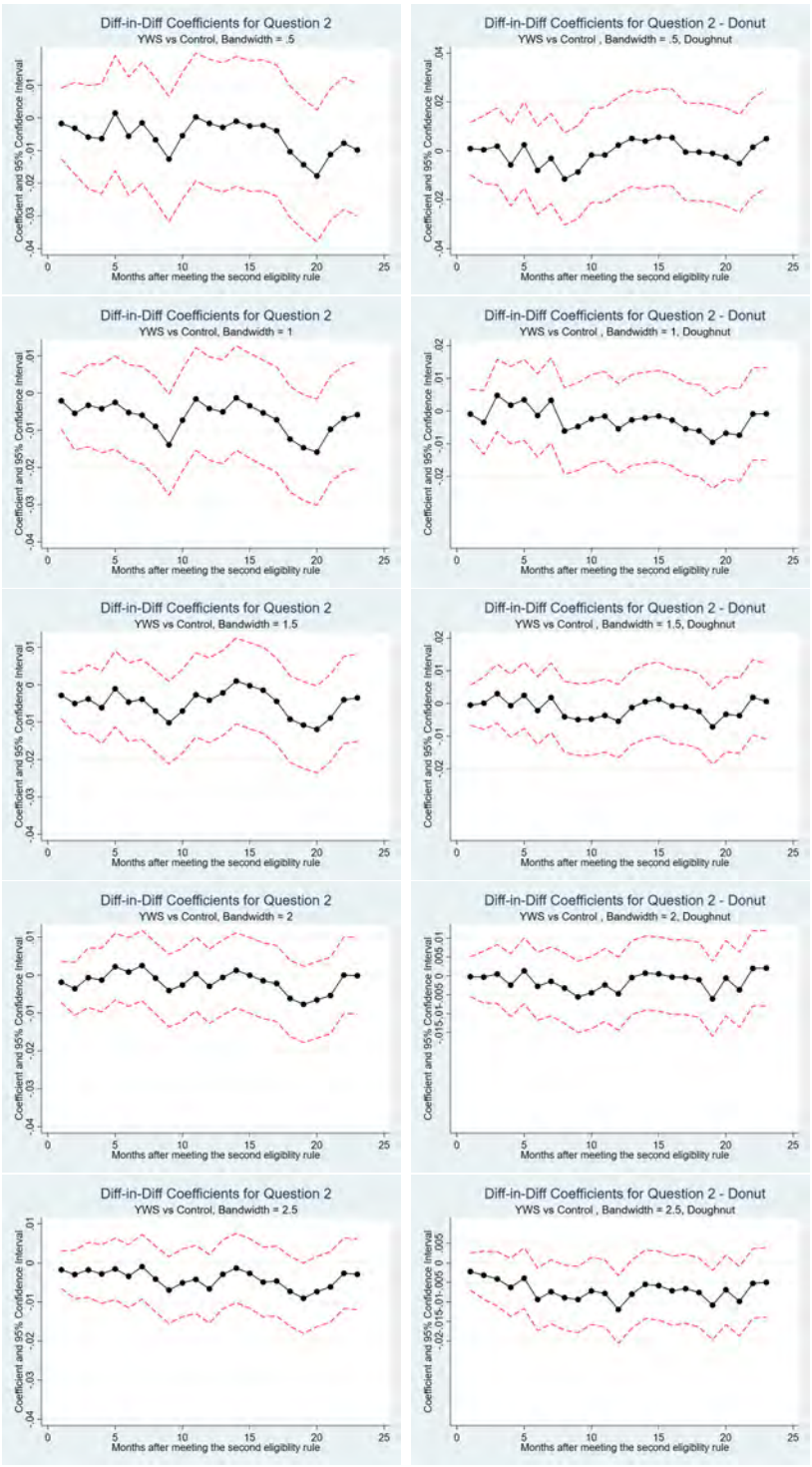

Figure A19: ITT effect of Eligibility for YWS vs Control for different Donut Diff-in-Diff specifications.
Note: The donut approach removes observations aged above 29 or above and below 30. Specification with bandwidth of size x uses observations whose age $\in [29-x, 29)$ and $\in [30, 30+x)$.

A.15 Donut Diff-in-Diff model specifications for Question 3

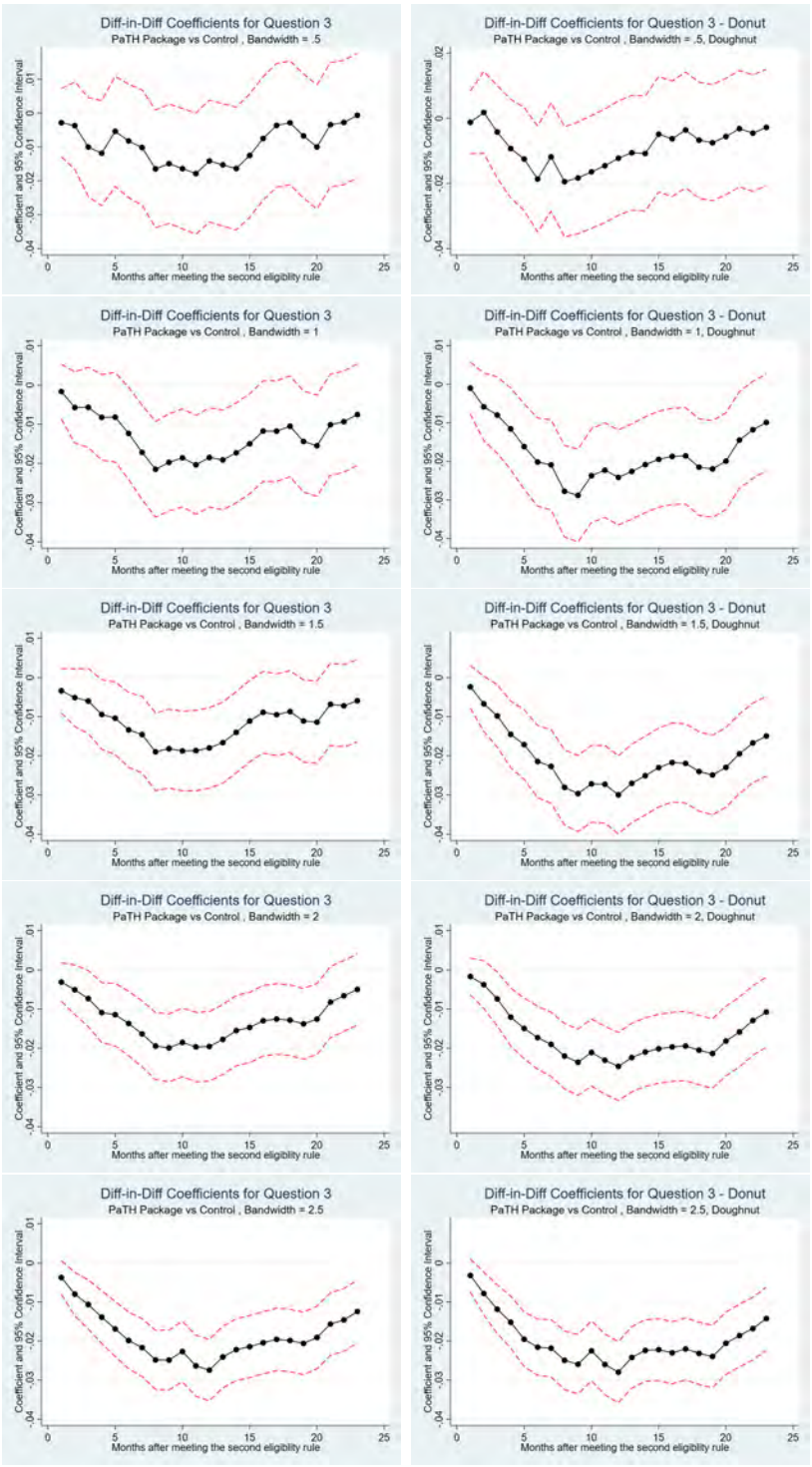

Figure A20: ITT effect of Eligibility for YWS vs Control for different Donut Diff-in-Diff specifications.

Note: The donut approach removes observations aged above 24 or above and below 30. Specification with bandwidth of size x uses observations whose age $\in [24 - x, 24)$ and $\in [30, 30 + x)$.

A.16 Diff-in-Diff with month dummies for question 1

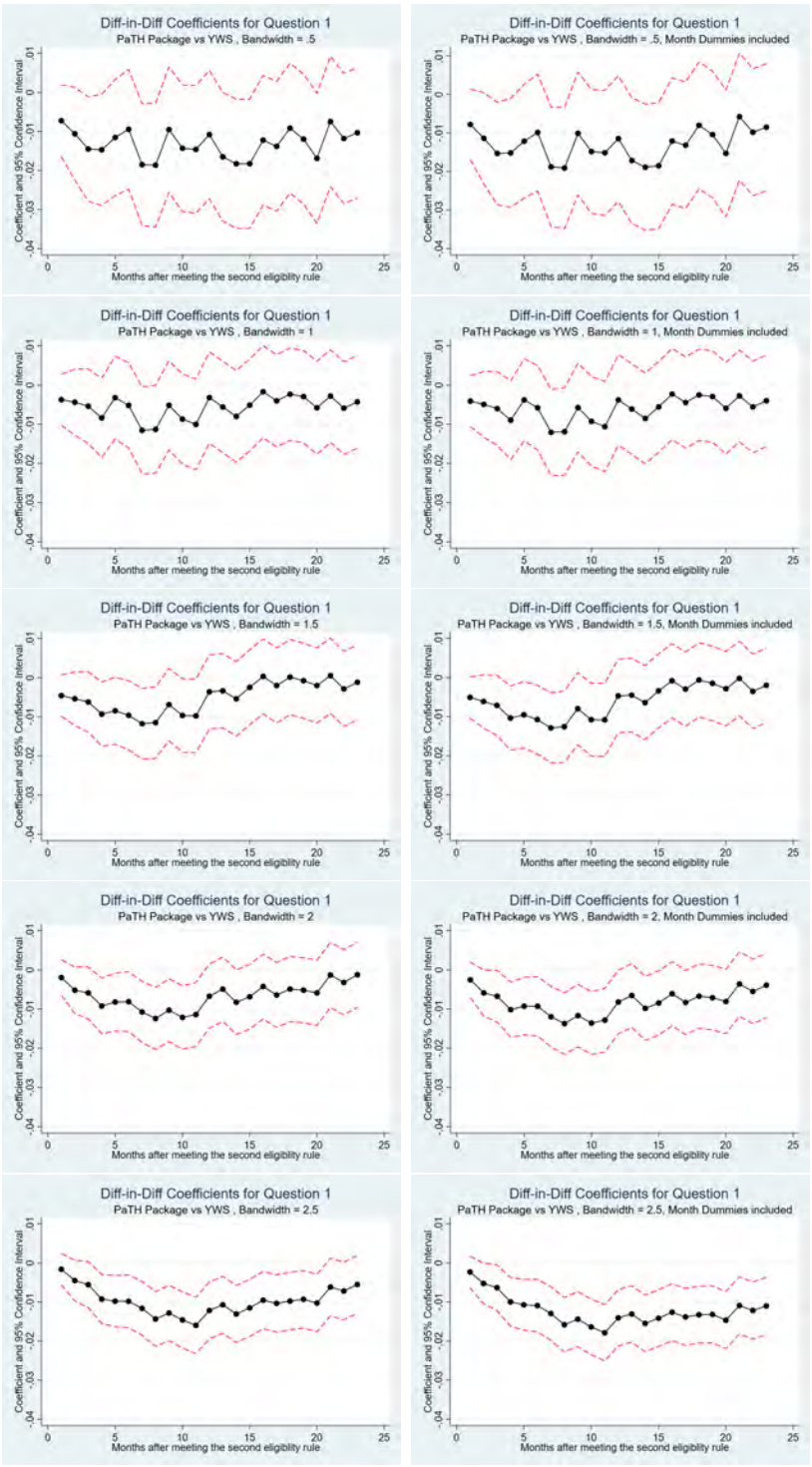

Figure A21: ITT effect of Eligibility for PaTH vs YWS when month dummies included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes month dummies.

A.17 Diff-in-Diff with month dummies for question 2

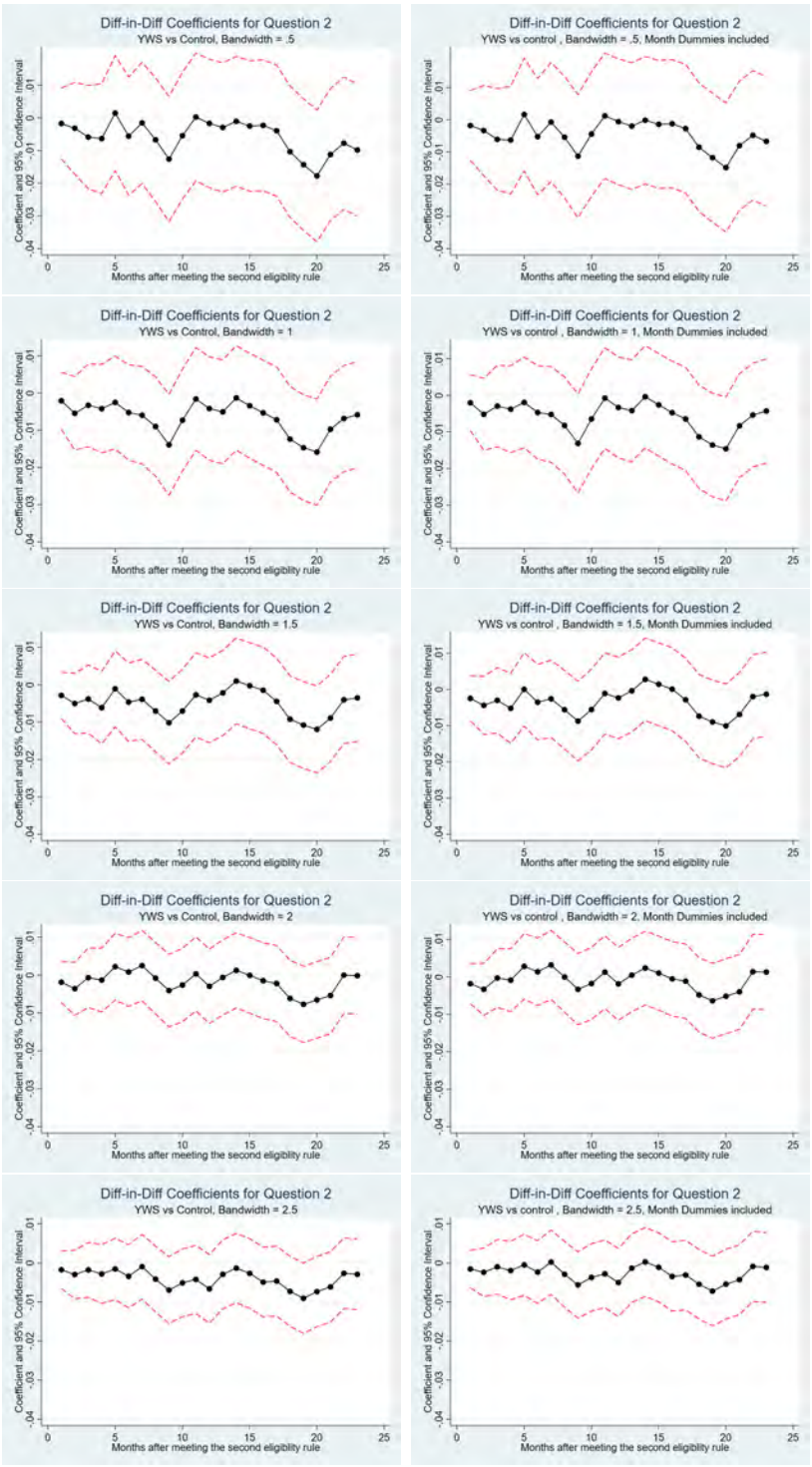

Figure A22: ITT effect of Eligibility for YWS vs Control when month dummies included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes month dummies.

A.18 Diff-in-Diff with month dummies for question 3

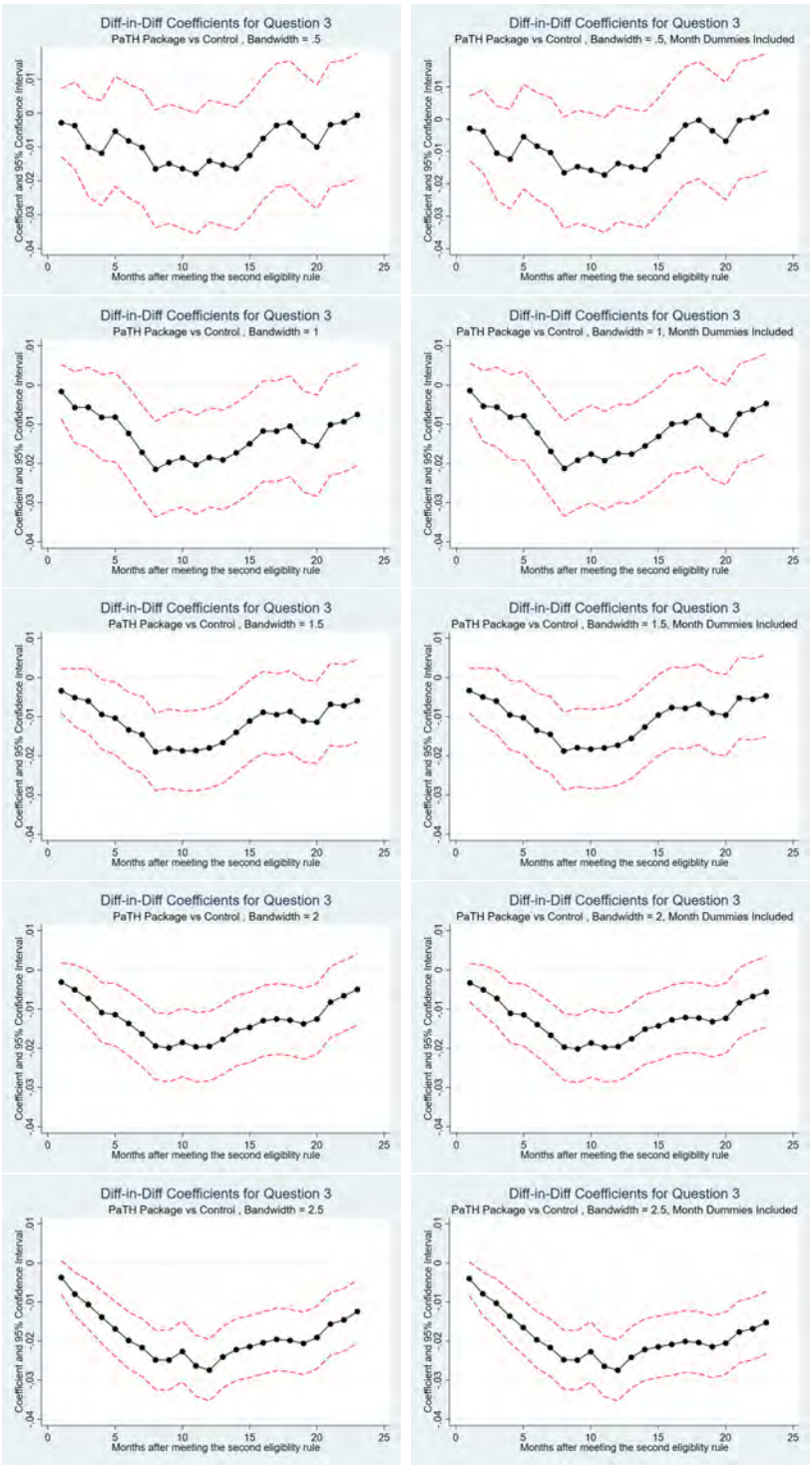

Figure A23: ITT effect of Eligibility for PaTH vs Control when month dummies included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes month dummies.

A.19 Diff-in-Diff with extra covariates for question 1

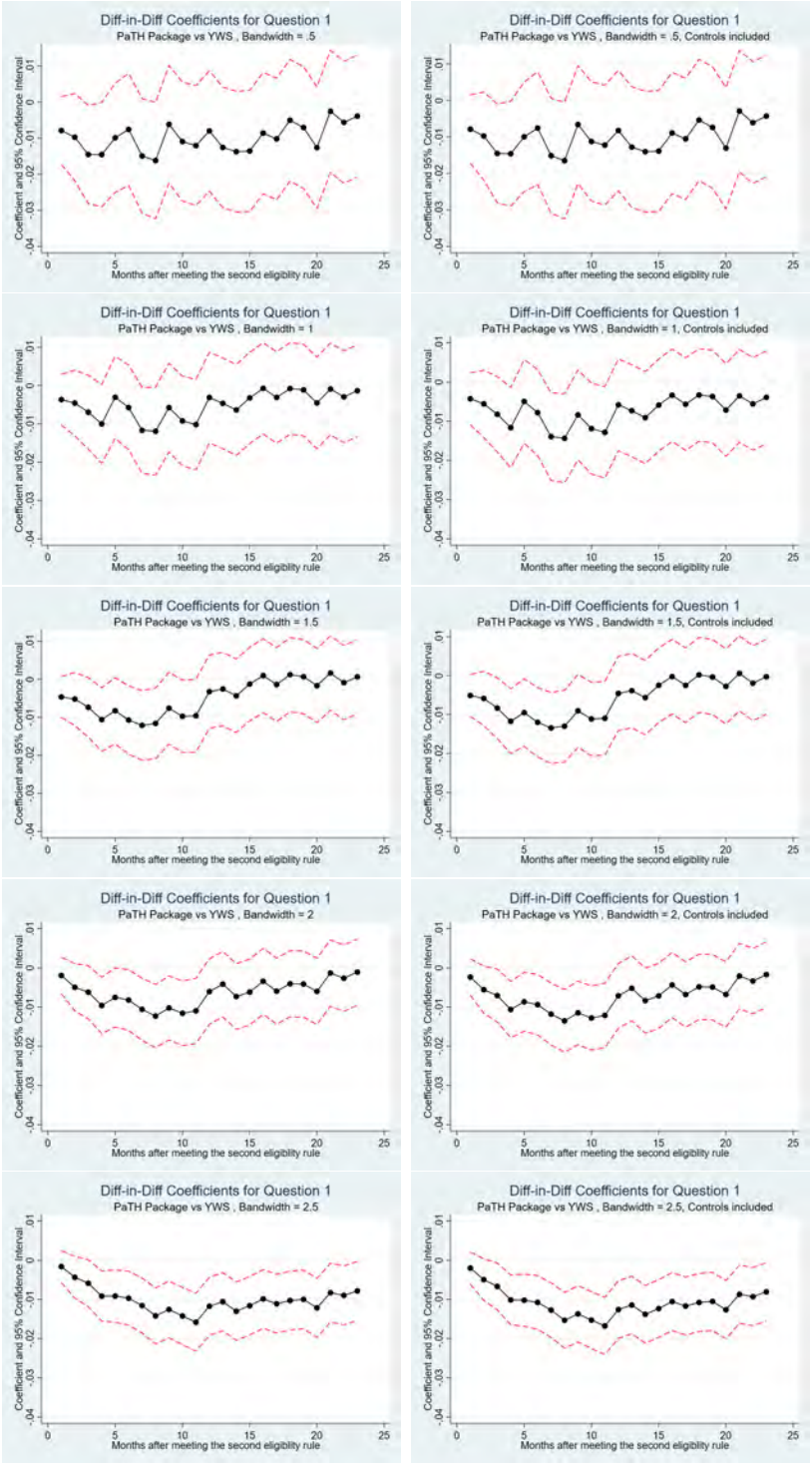

Figure A24: ITT effect of Eligibility for PaTH vs YWS when extra covariates are included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes extra covariates. Both the left and right subfigures are obtained from the sample which includes extra covariates; the size of this sample is reduced because some individuals are missing the values for covariates. The covariates are dummies for gender, country of birth, language, state of residence, education level, children status, marital status. Month dummies are used to control flexibly for time effects.

A.20 Diff-in-Diff with extra covariates for question 2

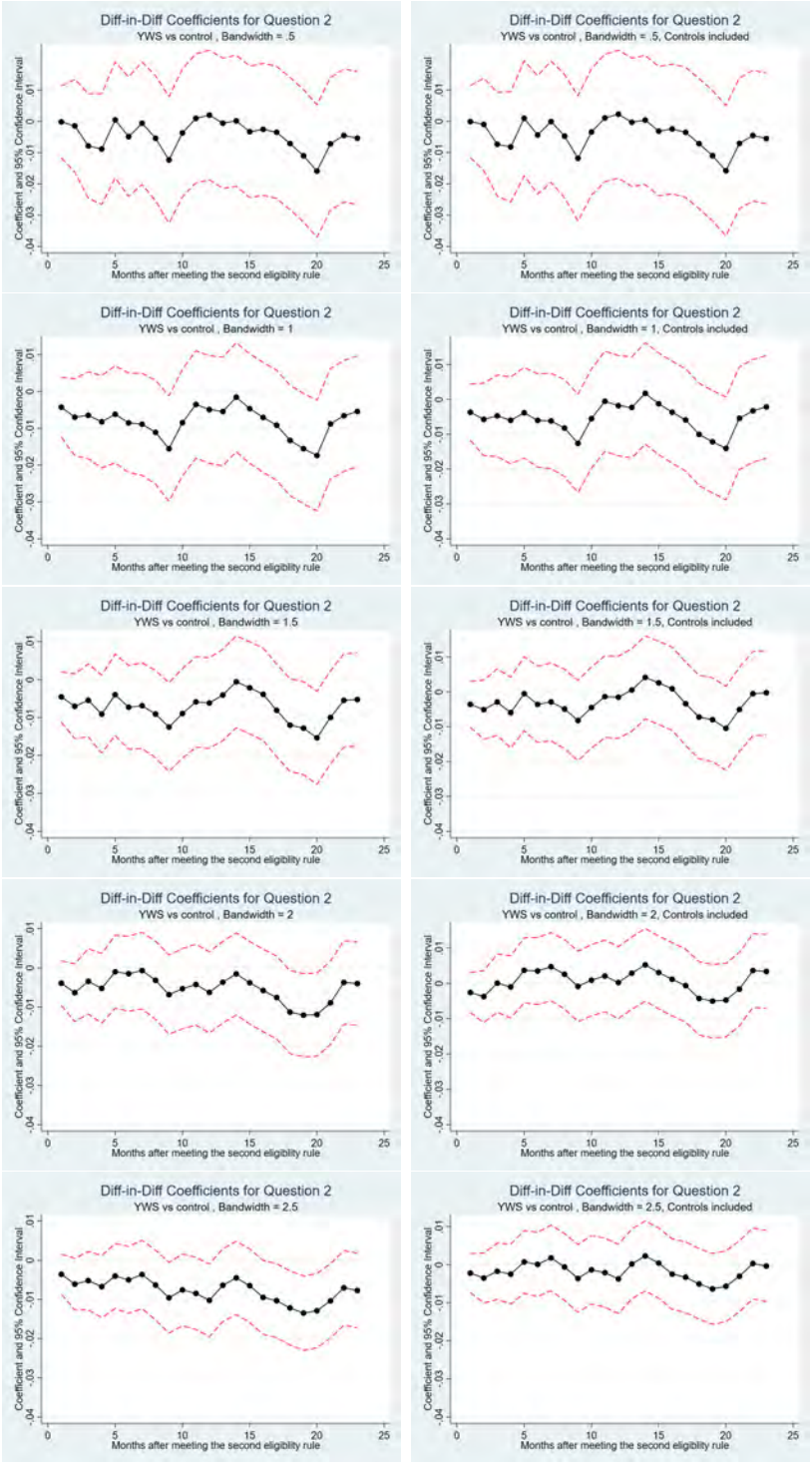

Figure A25: ITT effect of Eligibility for YWS vs control when extra covariates are included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes extra covariates. Both the left and right subfigures are obtained from the sample which includes extra covariates; the size of this sample is reduced because some individuals are missing the values for covariates. The covariates are dummies for gender, country of birth, language, state of residence, education level, children status, marital status. Month dummies are used to control flexibly for time effects.

A.21 Diff-in-Diff with extra covariates for question 3

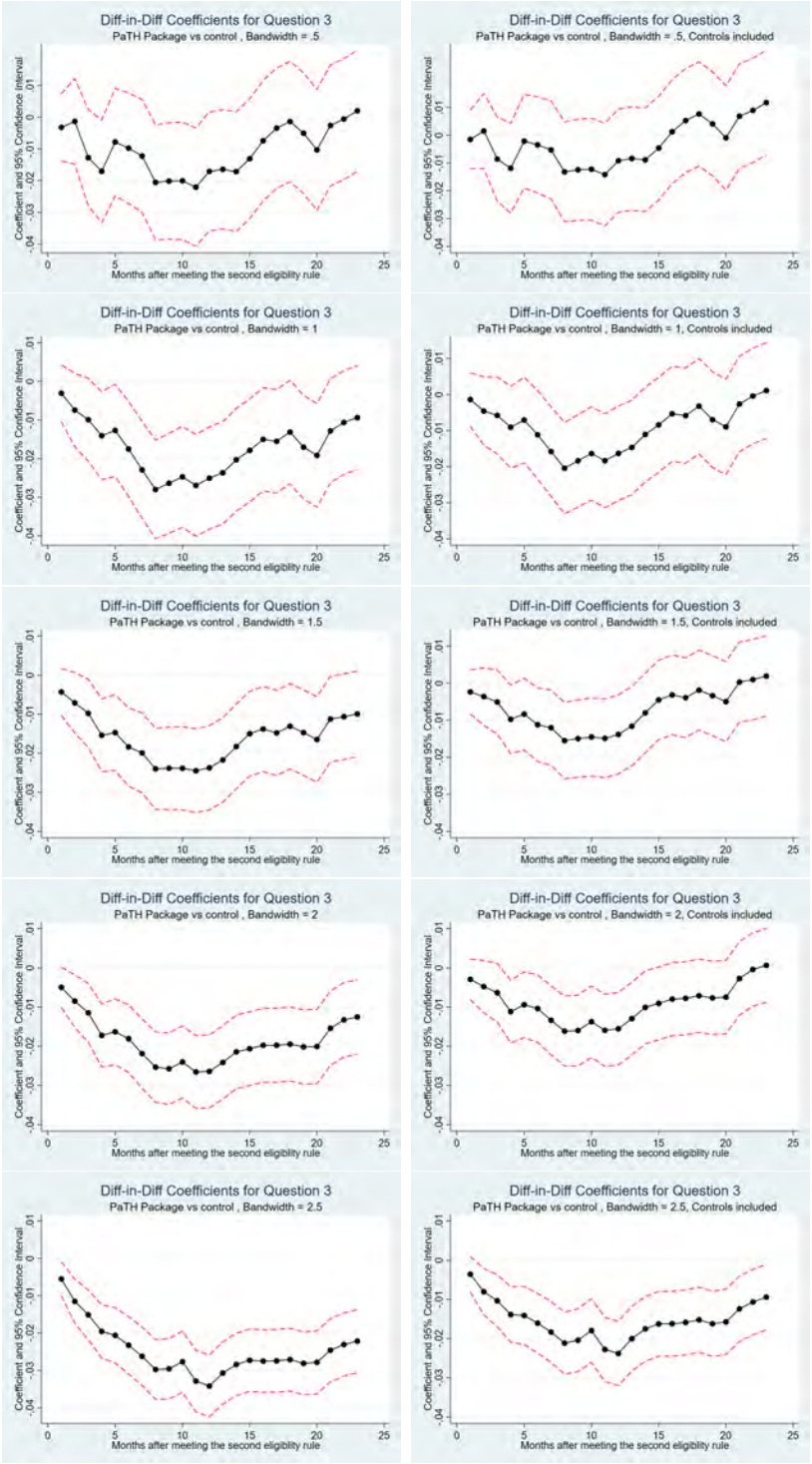

Figure A26: ITT effect of Eligibility for PaTH vs control when extra covariates are included in the model.

Note: The left subfigure in each row is the regular Diff-in-Diff approach and the right subfigure is from the specification which includes extra covariates. Both the left and right subfigures are obtained from the sample which includes extra covariates; the size of this sample is reduced because some individuals are missing the values for covariates. The covariates are dummies for gender, country of birth, language, state of residence, education level, children status, marital status. Month dummies are used to control flexibly for time effects.

A.22 RD model with Extra Covariates

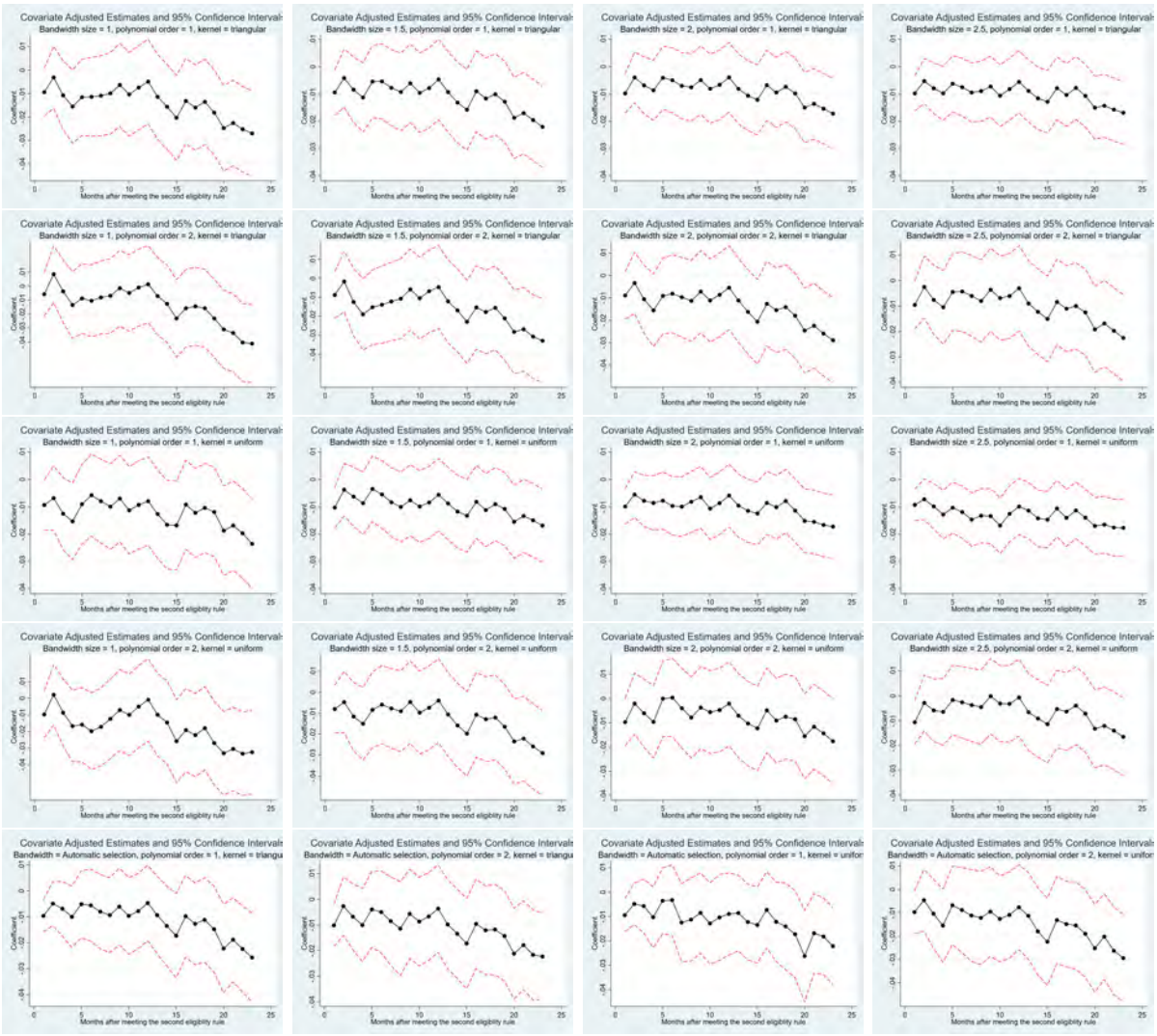

Figure A27: ITT effect of Eligibility for PaTH vs YWS for different RD specifications with extra covariates.

Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column). The extra covariates are dummies for gender, immigrant status, and language.

A.23 RD model with Bias Correction

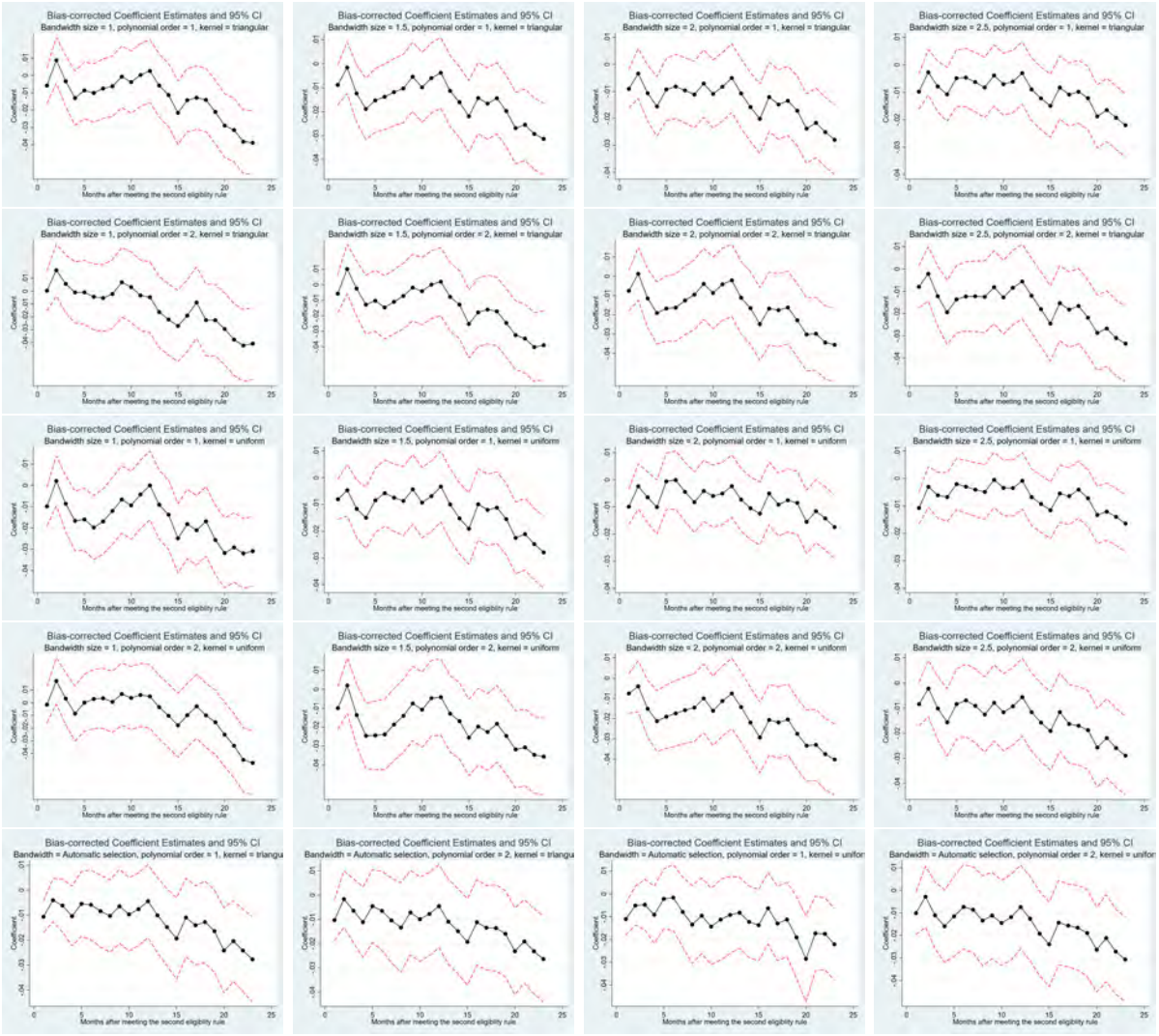

Figure A28: ITT effect of Eligibility for PaTH vs YWS for different RD specifications with bias correction.

Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.24 RD model for short term unemployed

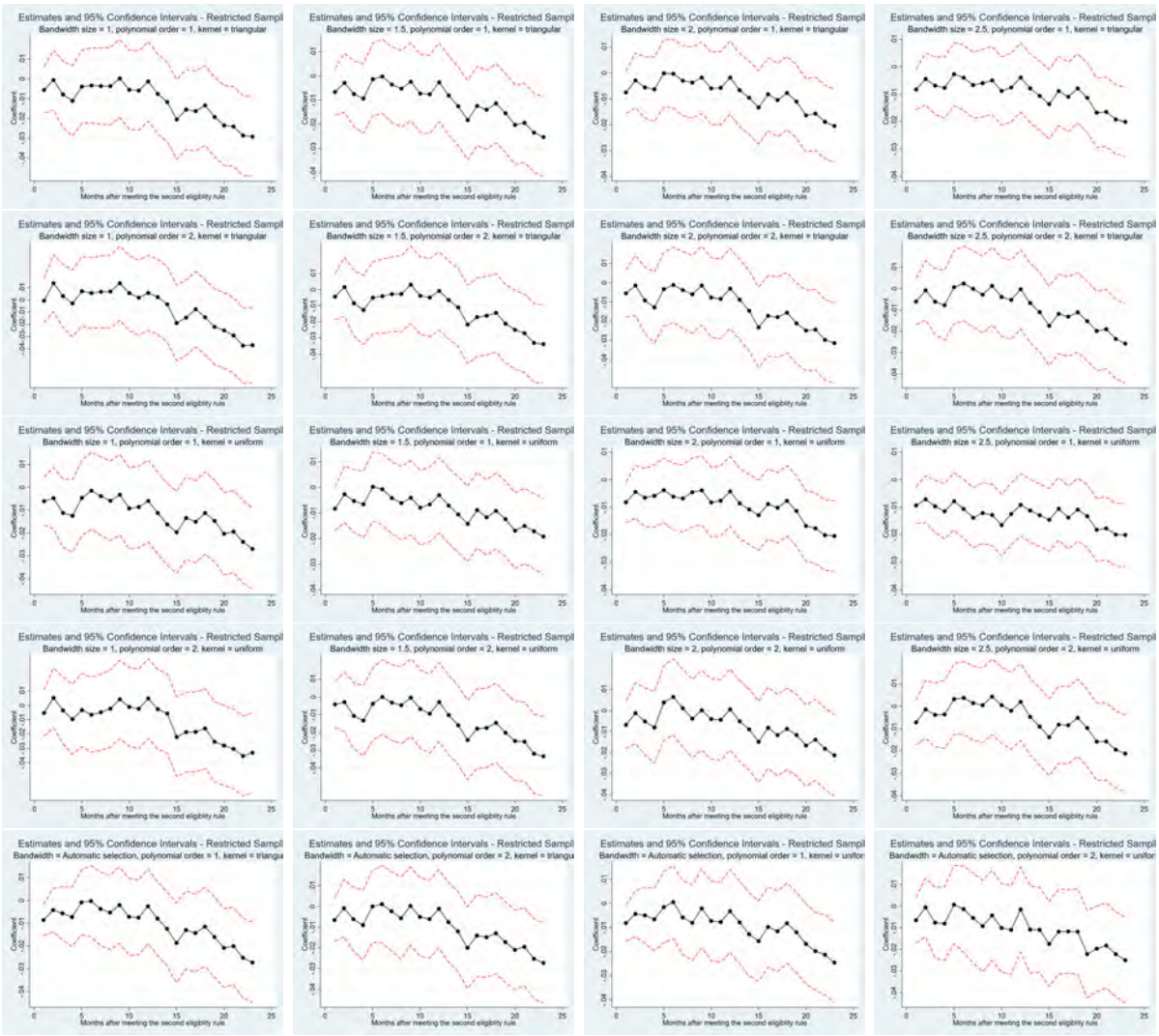

Figure A29: ITT effect of Eligibility for PaTH vs YWS for different RD specifications for sample restricted to short-term unemployed.

Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.25 RD model for March Sample

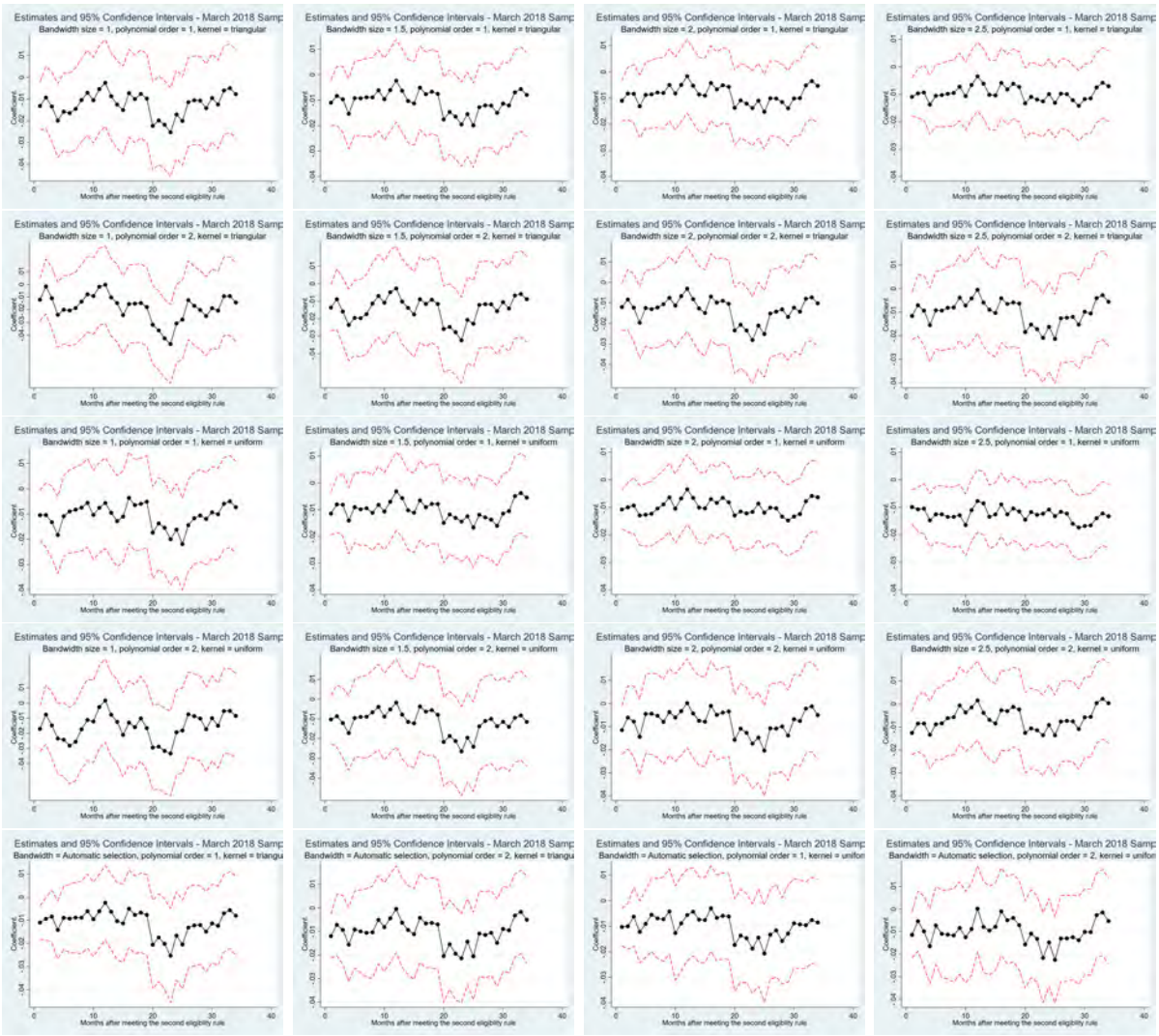

Figure A30: ITT effect of Eligibility for PaTH vs YWS for different RD specifications for March sample.

Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.26 RD model for July 2015 sample (for question 2)

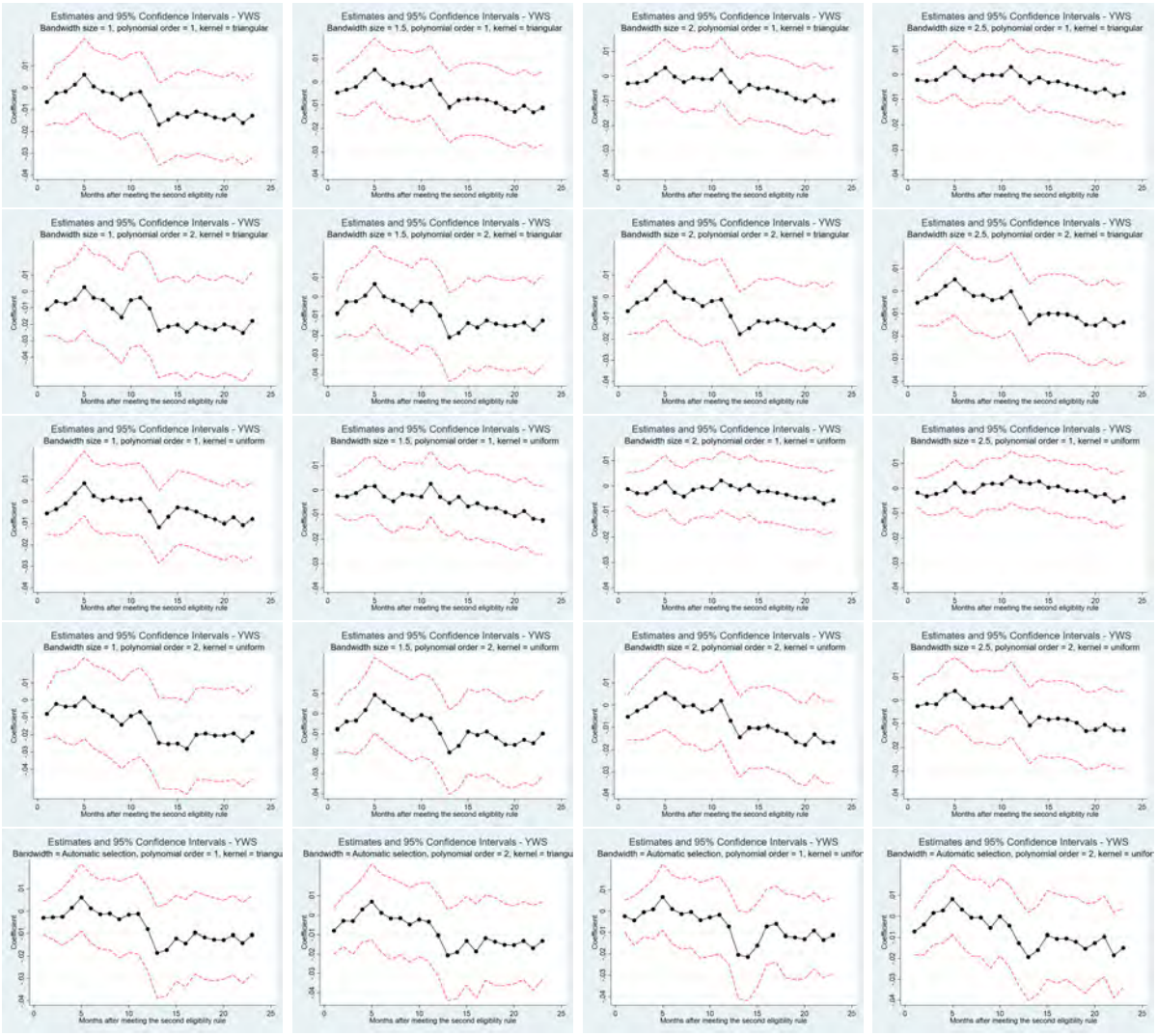

Figure A31: ITT effect of Eligibility for YWS vs Control for different RD specifications for July 2015 sample.

Note: Each subfigure shows estimates from a different specification of rdrobust command. A total of twenty (5 bandwidth sizes × 2 polynomial order × 2 kernel types) specifications are used. The default specification is automatic bandwidth selection, linear polynomial for age, and triangular kernel (the subfigure in the last row and first column).

A.27 Alternate outcome variable: total months on income support (question 1)

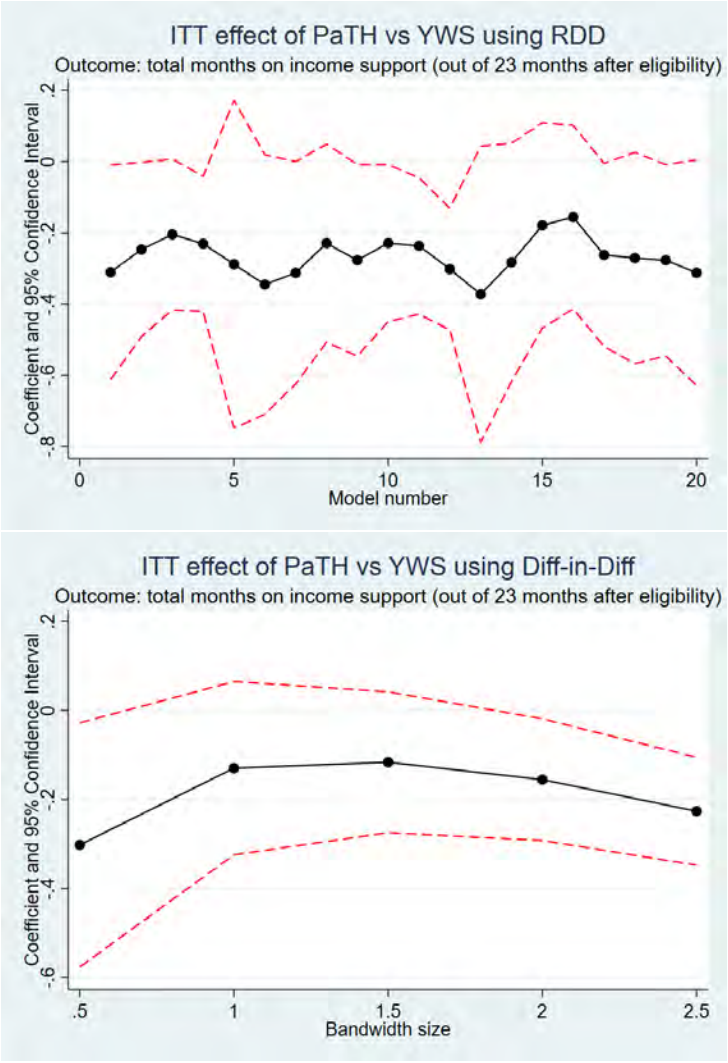

Figure A32: ITT effect of Eligibility for PaTH vs YWS for alternate outcome variable: total months on income support.

Notes: The top figure is for RD and the bottom for Diff-in-Diff. The black dots in the top figure show estimates from 20 different RD specification, with specification 17 being the default one used in table **??**. The specifications differ in the bandwidth size, polynomial order for age, and kernel type. The black dots in the bottom figure show estimates from 5 different Diff-in-Diff specifications where each specification uses different bandwidth around threshold age 25 for constructing treatment and control samples; the specification used in table 3 used bandwidth size 2.5.

A.28 Alternate outcome variable: total months on income support (question 2)

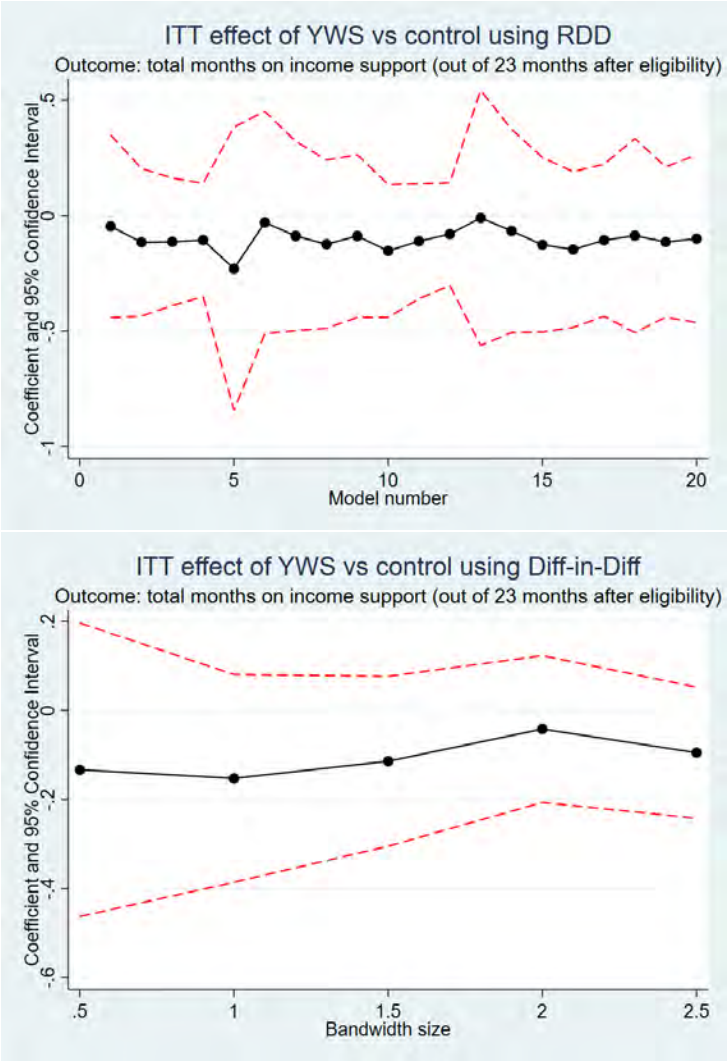

Figure A33: ITT effect of Eligibility for YWS vs control for alternate outcome variable: total months on income support.

Notes: The top figure is for RD and the bottom for Diff-in-Diff. The black dots in the top figure show estimates from 20 different RD specification, with specification 17 being the default one used in table **??**. The specifications differ in the bandwidth size, polynomial order for age, and kernel type. The black dots in the bottom figure show estimates from 5 different Diff-in-Diff specifications where each specification uses different bandwidth around threshold age 30 for constructing treatment and control samples; the specification used in table 3 used bandwidth size 2.5.

A.29 Alternate outcome variable: total months on income support (question 3)

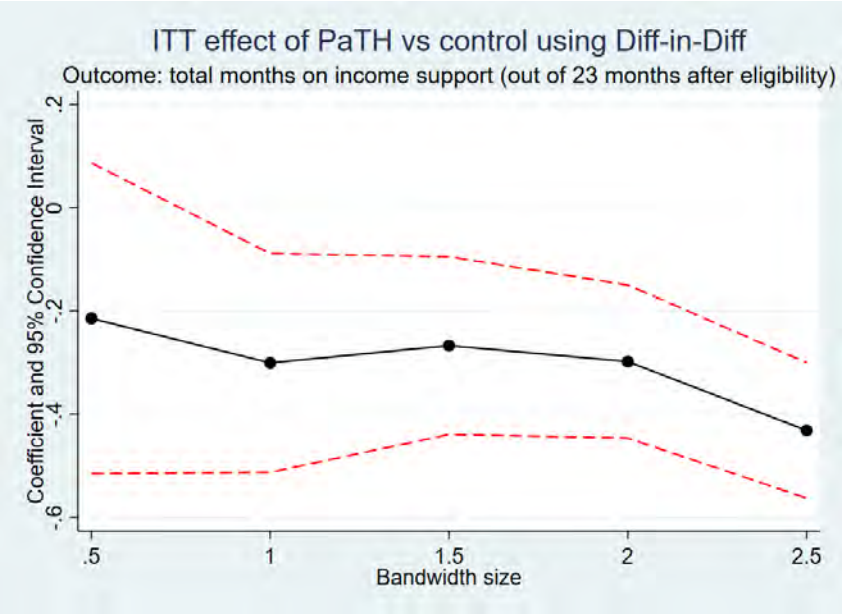

Figure A34: ITT effect of Eligibility for YWS vs control for alternate outcome variable: total months on income support.

Notes: The black dots show estimates from 5 different Diff-in-Diff specifications where each specification uses different bandwidth for constructing treatment and control samples; the specification used in table 3 used bandwidth size 2.5.

A.30 Alternate outcome variable: total months on income support (question 4)

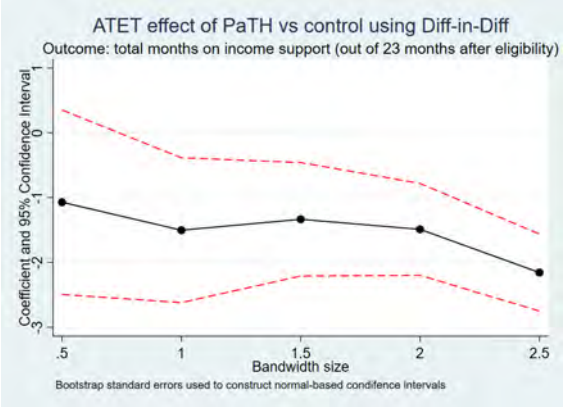

Figure A35: ATET effect of participating in a component of PaTH vs no participation. An alternate outcome variable is used: total months on income support.
Notes: The black dots show estimates from 5 different Diff-in-Diff specifications where each specification uses different bandwidth for constructing treatment and control samples; the specification used in table 3 used bandwidth size 2.5. The ATET estimates are obtained by dividing the ITT estimates in A34 by 0.2 (participation rate) and the standard error of these estimates are obtained by bootstrap method.

A.31 Alternate outcome variable: total months on income support (question 5)

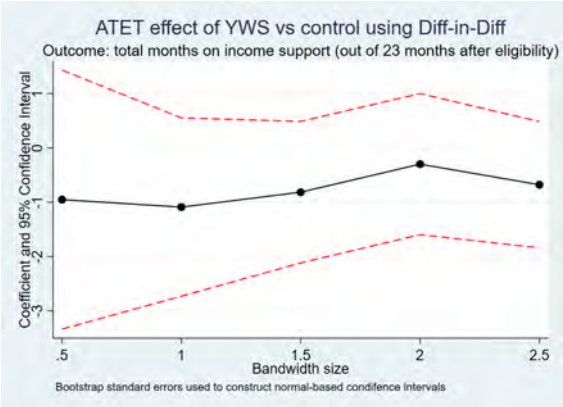

Figure A36: ATET effect of participating in a component of YWS vs no participation. An alternate outcome variable is used: total months on income support.
Notes: The black dots show estimates from 5 different Diff-in-Diff specifications where each specification uses different bandwidth for constructing treatment and control samples; the specification used in table 3 used bandwidth size 2.5. The ATET estimates are obtained by dividing the ITT estimates in A33 by 0.14 (participation rate) and the standard error of these estimates are obtained by bootstrap method.

A.32 Donut RD: Sensitivity to observations near the cutoff

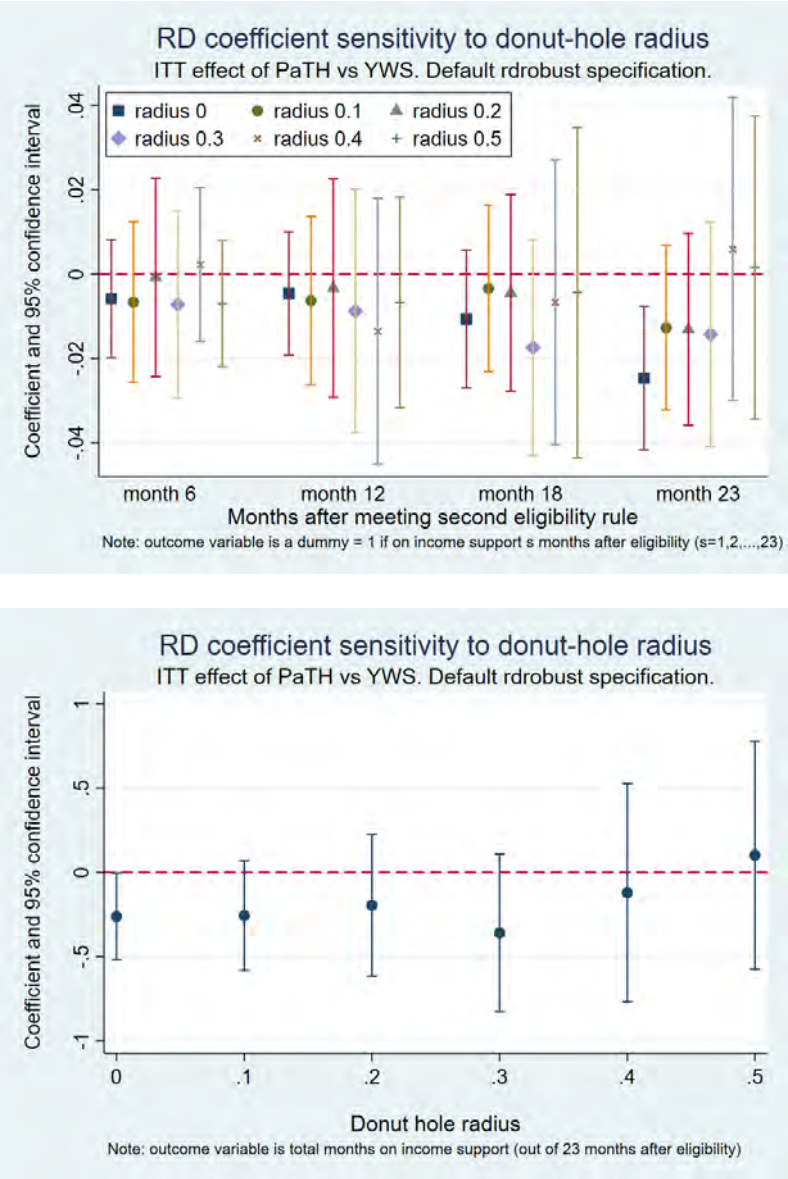

Figure A37: ITT effect of Eligibility for PaTH vs YWS for Donut RD.
Notes: Donut RD removes observations near the cutoff. Donut hole radius refers to the radius around the cutoff where observations are excluded. The top panel reports estimates for the original outcome variable for s = 6, 12, 18, and 23. The bottom panel reports estimates for alternate outcome variable: total months on income support (out of 23 months after eligibility).

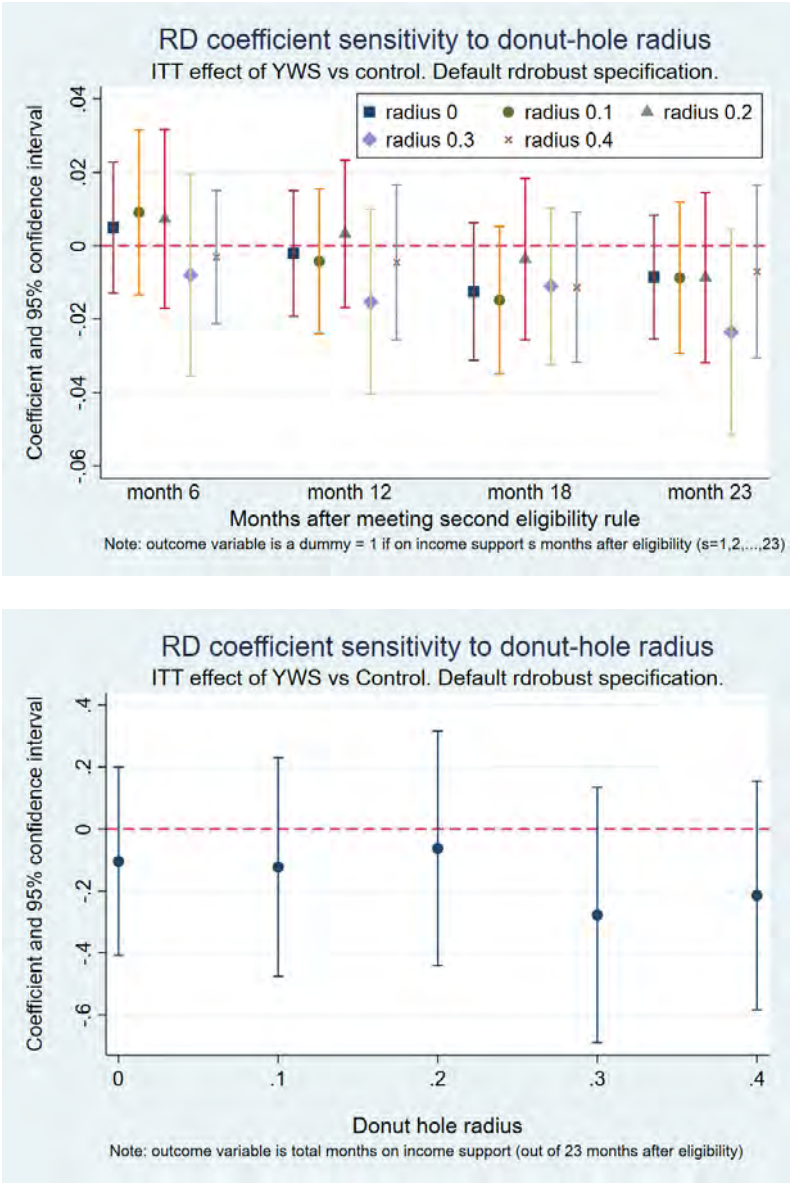

Figure A38: ITT effect of Eligibility for YWS vs control for Donut RD.
Notes: Donut RD removes observations near the cutoff. Donut hole radius refers to the radius around the cutoff where observations are excluded. The top panel reports estimates for the original outcome variable for s = 6, 12, 18, and 23. The bottom panel reports estimates for alternate outcome variable: total months on income support (out of 23 months after eligibility).