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Abstract

This paper critically evaluates the HESA (Higher Education Statistics Agency) Data Report for the Employer Justified Retirement Age (EJRA) Review Group at the University of Cambridge (Cambridge 2024), identifying significant methodological flaws and misinterpretations. Our analysis reveals issues such as application of data filters only to the Cambridge sample, inconsistent variable treatment, and erroneous statistical conclusions. The Report suggests EJRA increased job creation rates at Cambridge, but we show Cambridge consistently had lower job creation rates for Established Academic Careers compared to other Russell Group universities, both before and after EJRA implementation in 2011. This suggests that EJRA is not a significant factor driving job creation rates. Since other universities without an EJRA exhibit higher job creation rates, this suggests job creation can be sustained without such a policy. We conclude that the EJRA did not achieve its intended goal of increasing opportunities for young academics and may have exacerbated existing disparities compared to other leading universities. We recommend EJRA be abolished at Cambridge since it does not meet its justified aims and could be viewed as unlawful age discrimination.

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Keywords: Discrimination, Job Creation; Retirement; Treatment Effect, Universities
JEL Classification: C12, C21, C23, J21, J26, J63, J83.

1 Introduction

The University of Cambridge in the United Kingdom employs an Employer Justified Retirement Age (EJRA) policy, mandating retirement for all academic and academic-related staff at the end of the academic year in which they turn 67. Following the abolition of the Default Retirement Age (DRA) by the Coalition government in 2011 following on from the Equality Act (2010), employers were permitted to impose mandatory retirement only on the condition that it could be objectively justified as a proportional means of achieving a legitimate social policy aim. Implemented in 2012, after consultation with its staff, the Cambridge EJRA has four stated goals:

- To ensure inter-generational fairness and career progression;
- To enable effective succession-planning;
- To promote innovation in research and knowledge creation; and
- To preserve academic autonomy and freedom.

While the twenty two other Russell Group universities in the U.K. did not choose to introduce an EJRA in 2011, Cambridge and Oxford universities did so, setting them apart from the other Russell Group universities (and indeed other employers).

The effectiveness of the EJRA in achieving its aims is predicated upon its effect upon the rate at which vacancies arise from retirement and other causes. Estimates of the effect of the EJRA on the vacancy creation rate (VCR) may be made using models employing simplifying assumptions. A simple model, presented by Paul Ewart during debates in Congregation at Oxford[1], assumes a fixed number of posts subject to the EJRA and a uniform age distribution i.e. equal numbers of staff in each year group by age. (The Data Report confirms that this reflects the age distribution also at Cambridge.)

Then, initially assuming that vacancies arise *only* by retirements at the EJRA when a cohort reaches the retirement age and are filled by younger academics at the average age of appointment, the process can be viewed as a queue where the numbers joining equal the numbers leaving by retirement. If the EJRA was abolished, then the average age at which people retired would increase, at most, by about 3 years, according to survey data. In the steady state, the length of the queue would increase, but the number in each age group would decrease correspondingly to maintain the same number of staff. Hence the number of vacancies each year would decrease by the same amount.

The effect of an EJRA then is to increase the Vacancy Creation rate (VCR) relative to the situation with no mandatory retirement. It is on this basis that the University seeks to justify its EJRA policy as a means to achieving its stated aims. However, since the policy discriminates on the basis of a protected characteristic, age, it is unlawful unless

[1] See https://tinyurl.com/yc68vksc, page 443.

it can be objectively justified as a **proportionate** means of achieving those aims. Using this simple "queuing" model, it is possible to estimate the effect of the EJRA on the VCR as a measure of its proportionality.

This article critically evaluates the Data Report, identifying significant methodological flaws and misinterpretations. The Data Report suggests that EJRA at the University of Cambridge increased job creation rates. Our analysis reveals issues such as inappropriate data filters, inconsistent variable treatment, and erroneous statistical conclusions. We show that Cambridge consistently had lower job creation rates for Established Academic Careers (EAC) compared to other Russell Group universities, both before and after the EJRA implementation, implying that EJRA is *not* a significant factor driving job creation rates. Since other Russell Group universities, which do not have an EJRA, exhibit higher job creation rates, this suggests that job creation can be sustained or even thrive without such a policy.

2 Qualitative assessment of the EJRA

Using the data quoted in the Data Report, the average age at appointment of 40 and retirement at the EJRA at 67 gives an average career length of 27 years. Assuming, on the basis of survey and other data, that on average people will work for a further 3 years, the average career length will increase by 3/27 i.e. 1/9 or about 11% and the VCR will decrease by a corresponding proportion. Therefore, for a fixed number of posts, the EJRA increases the VCR by about 11%. However, as confirmed by the Data Report, at least 50% of vacancies arise for reasons other than retirement, so the effect of the EJRA on the VCR is reduced to 5.5%. In addition, about 50% of those reaching the EJRA will retire voluntarily and so the relative effect of the EJRA on the VCR is reduced still further to about 2.75%.

The Ewart Employment Tribunal judged that an increase in vacancy creation rate of between 2 – 4% was "trivial" and not a proportionate means of achieving the aims.[2] This finding was upheld by the Employment Appeal Tribunal[3] and was further confirmed in a recent ET cases (2023) involving four claimants at Oxford University.[4] It was acknowledged by Oxford University's statistical analysis in these recent ET tribunals that the modelling method presented by Ewart "gave similar results to the methodology based on Little's Law of queuing, once the correct assumptions were used."[5]

It is worth noting that the recommendation of the Review Report that the EJRA should be changed to set at 69 instead of 67 will result in the policy being even less proportionate. This is because the average extension of 3 years will be to a career of 29

[2]See https://tinyurl.com/2ywaft57

[3]See https://tinyurl.com/3eea3bkm

[4]See https://tinyurl.com/447ecv5a, https://tinyurl.com/5ahknzkk, https://tinyurl.com/5efvjakv, and https://tinyurl.com/3zxt2kd7

[5]Witness statement of Malgorzata Turner in Employment Tribunals referred to in footnote [4] above.

years, rather than 27 years, a percentage reduction of the VCR due to retirements alone from 11% (3/27) to 10% (3/29) and an overall reduction in the VCR of 2.5%. However, if people continue to retire at the current average age, then the extension will be only 1 year past 69, giving a career extension of 1/29 or 3% and an overall reduction of the VCR of 0.75% when vacancies for other reasons and voluntary retirements at the EJRA are taken into account.

The "steady state" conditions of this simple model do not take account of the transient effect induced by a sudden abolition of the EJRA or a change in the age from say 67 to 69. In the latter case, and using the same assumption of a uniform age distribution, there would be no vacancies for two years after which the situation would evolve into the steady state over time. This effect is treated in a dynamic modelling approach using Little's Law that would also have to take account of the vacancies created by reasons other than retirement and the average duration of extensions of employment post-EJRA.

It is clear, however, given the similarity in the situations at Oxford and Cambridge; same age at appointment of 40 and 50% of vacancies arising from reasons other than retirement etc., one expects that the increase the vacancy creation rate by the EJRA at Cambridge will also be in the range 2 – 4%. Any significant deviation from this estimate would suggest that some filtering of the statistics data has been made to change the assumptions built into the model.

We next turn to the specific goals of the EJRA and examine whether the EJRA is a proportionate means of achieving those goals. Recall that the EJRA sets four goals:

Promoting Innovation and Knowledge Creation

The EJRA is predicated on an antiquated university model with a fixed number of professorships, necessitating one professor's departure for another's ascension. This premise aligns with the "lump of labor fallacy" – the erroneous belief that there is a fixed amount of work to be distributed, leading to the misconception that increased productivity causes unemployment. However, this notion is fallacious (Krugman 2024). Arguing against productivity enhancements due to potential job losses is irrational. The creation of new professorships is contingent on funding availability and academic interest, not vacancy generation through retirements. Hiring new faculty members can potentially stimulate novel research avenues and ways of thinking, fostering innovation and knowledge creation, rather than merely replacing outgoing scholars. In addition, the senior outgoing scholars are likely able to generate significant amounts of funds, all of which are lost to the university.

Enabling Effective Succession Planning It is acknowledged that at least 50% of vacancies arise before the mandatory retirement age and these are managed successfully, even if they do not occur at predictable times. It is therefore difficult to imagine that vacancies arising from voluntary retirement could not be equally well managed. Indeed, it

is likely that academics wishing to retire would be willing and capable of giving adequate prior notice of their intention to retire and so mitigate any potential issues related to succession planning.

The EJRA hinders the University's ability to attract senior external candidates, as the prospect of mandatory retirement within a few years can deter potential applicants. Moreover, the policy impedes academic productivity well before retirement, as academics may be unable to supervise doctoral students or apply for grants at least five years prior to the retirement age due to the impending termination of their employment. Consequently, productive senior academics often seek employment elsewhere well before reaching the EJRA, resulting in a detrimental brain drain of highly sought-after and accomplished scholars from the University. This has the impact of reducing predictability since senior academics seeking positions elsewhere will be unlikely to notify the university ahead of time that they are actively looking elsewhere.

Ensuring Intergenerational Fairness and Career Progression

It is improbable that junior academics at Cambridge directly benefit from the forced retirement of their senior colleagues. Senior positions are advertised globally, and the potential for internal promotions is often outweighed by external applicants. Thus, even if the model underpinning the Review Report is sound (which it clearly isn't), the intergenerational fairness argument would not apply within the University context but rather on a global scale, where Cambridge's retirees ostensibly create opportunities for younger academics worldwide. Needless to say, this would only be a drop in the ocean and by no means a proportionate measure. Curiously, this rationale does not extend to administrative positions, which are exempt from the EJRA. Additionally, the policy disproportionately disadvantages women, whose career trajectories are often prolonged due to family obligations, resulting in a shorter timeframe before reaching the retirement age after achieving career milestones. In figure 1, the university argues that the EJRA policy promoted in the HESA review *increases* the lectureship opportunities available to post-doctoral colleagues (University of Cambridge 2024).

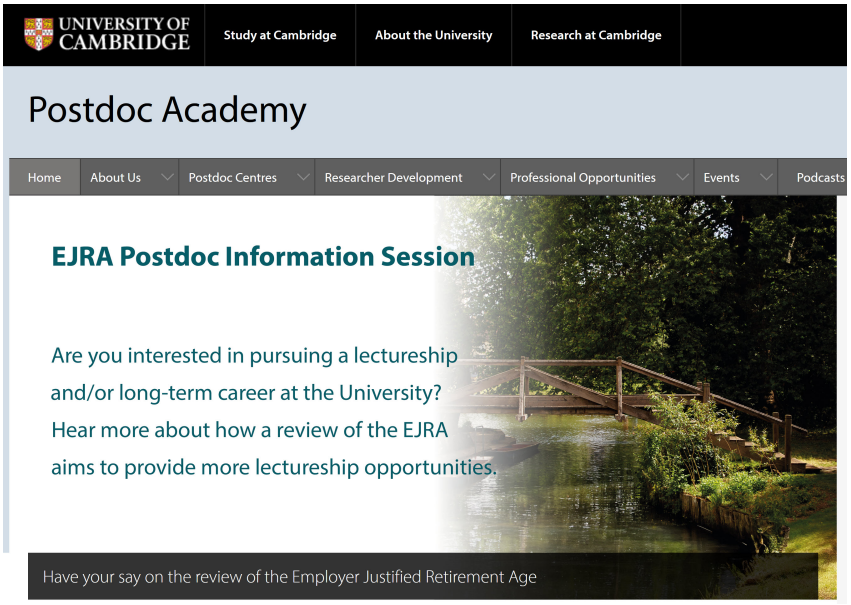

Figure 1: University website for postdoc students at Cambridge: Retrieved 20 May 2024

Since the Data Report suggests *increasing* the retirement age by two years to 69, it is difficult to understand how lectureship opportunities will be increased. One possibility is that the University plans on increasing the number of senior positions such as professorships. Unfortunately, a simple graph of the number of senior professorships at Cambridge, depicted in figure 2 shows a very sharp decline in these positions after the EJRA period in Cambridge.

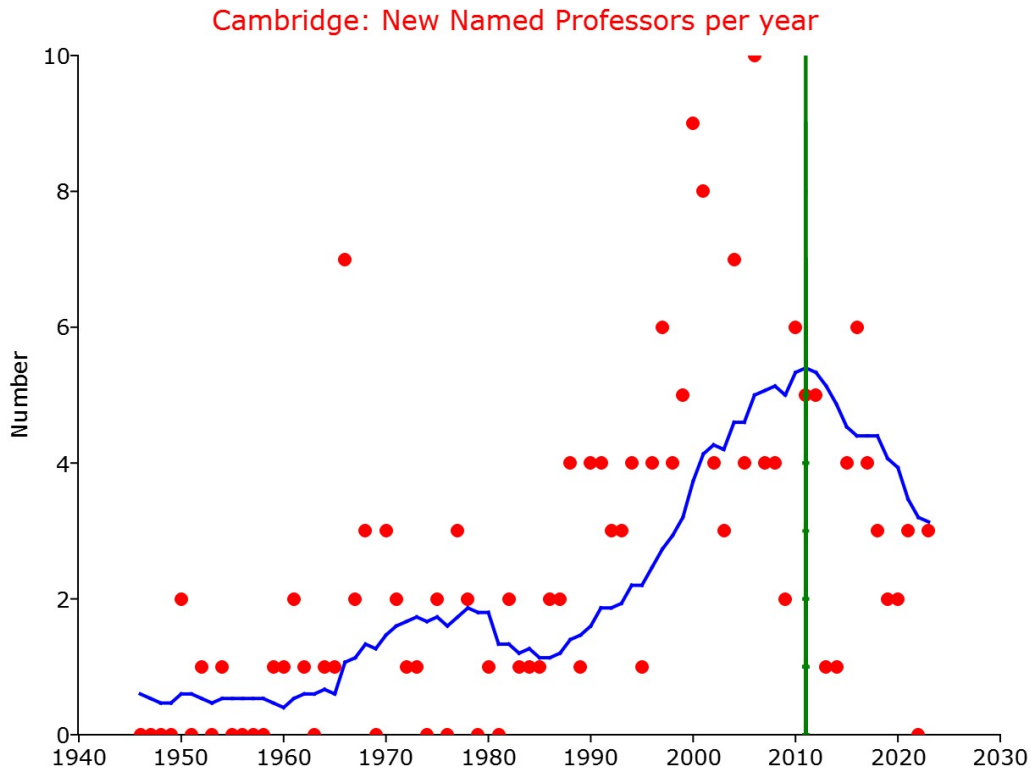

Figure 2: Number of professorships created at Cambridge (15 year trailing average)

Preserving Academic Autonomy and Freedom Finally, the EJRA also undermines academic autonomy and freedom. To secure an extension beyond the retirement age, academics must demonstrate productivity, which may incentivise risk-averse behavior and incremental research rather than pursuing potentially groundbreaking but uncertain avenues. This dynamic has the potential of stifling innovation and knowledge creation, which should be a core objective of a leading research university.

In summary, the arguments highlight the potential adverse impacts of the EJRA on innovation, succession planning, intergenerational fairness, and academic freedom, contending that the policy may be counterproductive to the University's stated aims and could hinder its pursuit of excellence in research and education.

3 Quantitative assessment of the Data Report

The Data Report sought to establish that EJRA has a beneficial effect on vacancy creation based on historical data and statistical techniques. In general terms and when properly applied, the method of analysis (difference-in-differences) used is capable of measuring the impact of a policy by comparing changes, from before to after the policy change, between a group that experienced the policy and a group that did not. In the analysis of whether the removal of EJRA at Cambridge would affect job creation, one group

was the RRG (Remaining Russell Group) where EJRA was removed, and the other group comprised solely of Cambridge where an EJRA was introduced so that mandatory retirement continued, and the difference-in-differences calculation was applied to vacancy creation rates in the RRG and in Cambridge, before and after EJRA removal in the RRG. This section shows that, contrary to sound practice, Cambridge's data was selectively filtered while other universities' data were not treated the same way. The analysis as applied did not adequately control for pre-existing trends, or for differences between universities. In fact, Cambridge consistently had lower vacancy creation rates compared to other universities, both before and after EJRA removal in the RRG. It is shown below that the conclusion that the removal of the EJRA policy would significantly impact the vacancy creation rate in Cambridge simply does not follow from the analysis.

The second part of the Data Report uses a system dynamics model to simulate forward what would be the likely consequences of the abolition of the EJRA at Cambridge in terms of vacancies. In the second part of this section, we point out significant limitations in that analysis as well.

3.1 Effect of EJRA on Job Creation Rate

The authors of the Data Report obtained data from HESA on the number of employees in different categories for each year from 2007-2008 academic year to 2021-2022 academic year for each of the 24 Russell Group universities including Cambridge and Oxford. Oxford is excluded from the analysis with a comparison made only between Cambridge and RRG. The authors describe on page 7 how they applied various "filters" to the HESA data using Cambridge HR data to construct the group that is eligible for EJRA, which was not identified from the HESA data alone. They did not do and could not do such a data editing for the RRG universities and so it is not clear that a fair comparison was made. To characterize this choice, if an RRG university wanted to *introduce* an EJRA, would they also apply it only to a subset of the individuals recorded in the HESA dataset?

The Data Report examines two classes of staff at Cambridge: Established Academic staff (EAC) and Established Academic-related staff (EAR). The EAC group includes academic staff whose primary function is teaching and/or research. Specifically, the Data Report defines EAC as employees with:

- Academic employment function coded as 1 (Teaching Only), 3 (Teaching and Research), or 9 (Neither, e.g. Vice-Chancellor)
- Salary point 38 and above
- Full-time or full-time term-time only employment mode
- Open-ended/permanent terms of employment

The EAR group includes non-academic professional staff supporting teaching and research. The Data Report defines EAR as employees with:

- Academic employment function coded as 4 (Not an academic contract) or X (for data before 2011/12)
- Salary levels point 33 and above
- Wholly or partly financed by the higher education provider
- Open-ended/permanent terms of employment

In effect, the EAC group represents the core group of established academic teaching and research staff, while the EAR group covers the non-academic professional support staff, both at relatively senior levels within the university. The purpose of the Data Report is to show that the university created a significantly higher number of jobs in the Cambridge EAC group than would have arisen if Cambridge did not have an EJRA by comparing with other universities who do not operate an EJRA.

The main variable of interest is defined as

$$r_{it}^{jc} = \frac{N_{it}^{jc}}{N_{it}}, \tag{1}$$

where N_{it} is the total employees in a given group (for example, university i) at year (actually academic year) t, while N_{it}^{jc} is the total number of new appointees at university i in year t (presumably new appointees in the specific group). This is the measure of job creation that is identified as the outcome of the EJRA policy.

This variable is shown in Figure A1 for both the EAC and EAR groups (see figure 3 reproduced below). Figure 3 further breaks this down to EAC STEM and EAC non STEM groups. These figures show the time series of the 22 RRG universities and Cambridge over the period 2007-2008 to 2021-2022 (a period of 15 years) along with the average across RRG and a vertical line indicating 2012, the date from which the abolition of retirement age at RRG is binding. Regardless of the figure examined, the key feature is that Cambridge has had a *lower* job creation rate in comparison with *every* single other RRG university. This is acknowledged on page 8 of the Data Report, "*UoC has a lower job creation rate for EAC compared to most other HEPs* (Higher Education Providers). "*Figure A1 shows the average job creation rate is 3% for UoC between AY 2007/08 and 2021/22, while the average for RRG is 5%.*" In one sense, the reader can stop here since the raw data clearly indicates a result completely divergent from its core message that the EJRA has created significantly more jobs at Cambridge. However, there are potential flaws in this interpretation that suggest we look further.

Specifically, in the Data Report they argue that these numbers are the average over all fifteen years and it is plausible that Cambridge has improved its performance relatively since 2011, post the introduction of EJRA. A simple way of capturing this pattern would

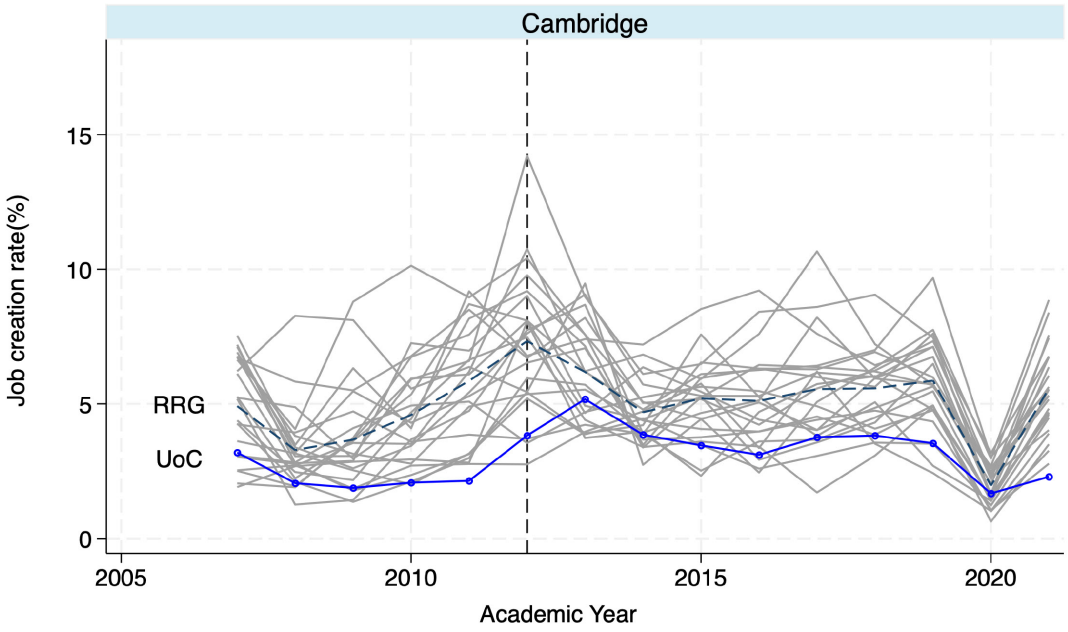

Figure 3: Figure A1 reproduced from the Data Report (Cambridge 2024)

be to compare the average (over time) of Cambridge performance after 2011 with the average of the RRG universities. Unfortunately, looking at Figure 3, it is clear that Cambridge would lose that contest too: In every single year, Cambridge created fewer EAC jobs than the average of the RRG universities and in every year, it has been in the bottom 5.

Yet another possibility is that something else is to blame - perhaps for example, there were other factors, presumably macroeconomic, that affected job creation over this period. In that case, what we need to do is to compare the average performance of Cambridge post 2011 with the average performance of Cambridge pre-2011 and do the same for the RRG universities. This is the heart of why their argument appears to work since one can see that just prior to 2012 the RRG universities created a lot of new positions and their performance subsequent to 2012 was relatively worse. Actually, the key feature of the pre-2012 data was the big dip following the 2008 Global Financial Crisis (GFC) and the strong rebound, especially for RRG universities, just running up to 2012.

The main approach they use is a so-called natural experiment, which is intended to capture how clinical trials are carried out. The treatment or policy variable is the introduction of the EJRA by Cambridge in 2011, and the treatment group is Cambridge with pre-treatment period 2007/2008-2011/2012 and post treatment period 2012/2013 (it takes a year for the abolition of retirement to take effect) to 2021/2022. The control group is the RRG universities. It is important to note that this design addresses the issue about abolition of EJRA as it compares universities without EJRA with Cambridge that does. It does *not* provide any evidence about the effect of raising the age of EJRA to 69 from 67. As outlined in the introduction above, increasing the age of the EJRA results

in its being even less proportionate as a means of achieving the aims.

The main issues for application of this method here are:

1. Treatment is not randomly assigned as in a clinical trial. Cambridge, the authors themselves argue (and the authors of the Oxford EJRA argued before them), is *different* and those differences lead them to choose the EJRA policy in the first place. Fundamentally, one cannot separate out whether the difference between Cambridge and the RRG universities is due to the EJRA or due to the fundamental difference already present.

2. The treatment is really 2012 itself, not the introduction of the EJRA (or rather the division is into the pre- and post-2012 periods). The EJRA was not the only thing that changed after 2012 compared to before 2012 and that could influence vacancy rates.

3. The sample sizes are too small. There is only one treated patient, Cambridge, and only 22 untreated patients, the RRG. Imagine testing a vaccine on one patient and applying a placebo to twenty two. Pfizer's Covid-19 vaccine clinical trial had over 46,000 participants. The statistics here all rely on large sample approximations, and here the smallest group average is made from 5 time series observations.

4. The unit of measurement is at the university level. In a clinical trial, one is looking at individuals and it is reasonable to assume that although they are different in fundamental ways, their health outcomes are essentially independent across individuals and only depends on the treatment. It is hard to believe that is the case for university's economic decisions, which is what is needed here. For example, all the universities are admitting undergraduates through UCAS and they are all trying to recruit from the same pool of academics.

In conclusion, the methods adopted are not appropriate for the data they have. The results they obtained are not credible. The fundamental fact is that Cambridge creates fewer new academic jobs in comparison with RRG whether or not they both had a retirement age. This is clear from Figure A1.

Figure A30 from the Data Report (reproduced in Figure 4) gives their estimates of the treatment effect separately for each year along with confidence bands. This is defined as the difference in job creation between Cambridge and RRG relative to the same difference for 2012 (the zero value for 2012 is suppressed from the figure). According to this graph, the introduction of EJRA had a positive relative effect even five years before it was introduced! In the language of natural experiments, this evidences that the pre treatment parallel trend assumption is violated, which they acknowledge, and which leads them to implement a further couple of inappropriate methods for dealing with this issue. In the Appendix we give a more formal treatment of the statistical issues. Another interesting

aspect of this figure is that there is a declining trend in job creation rates in the post-Brexit period (from 2017 onwards) interrupted by a spike upwards during the Covid year of 2020. Even taking their methods at face value, by 2021, the effect has totally disappeared, so whatever was driving the results has vanished. Another interpretation of this data is just that, as in any time series, there are periods where the series is above trend and periods where it is below trend and these cycles are determined by a complex set of reasons that have not been investigated in this report.

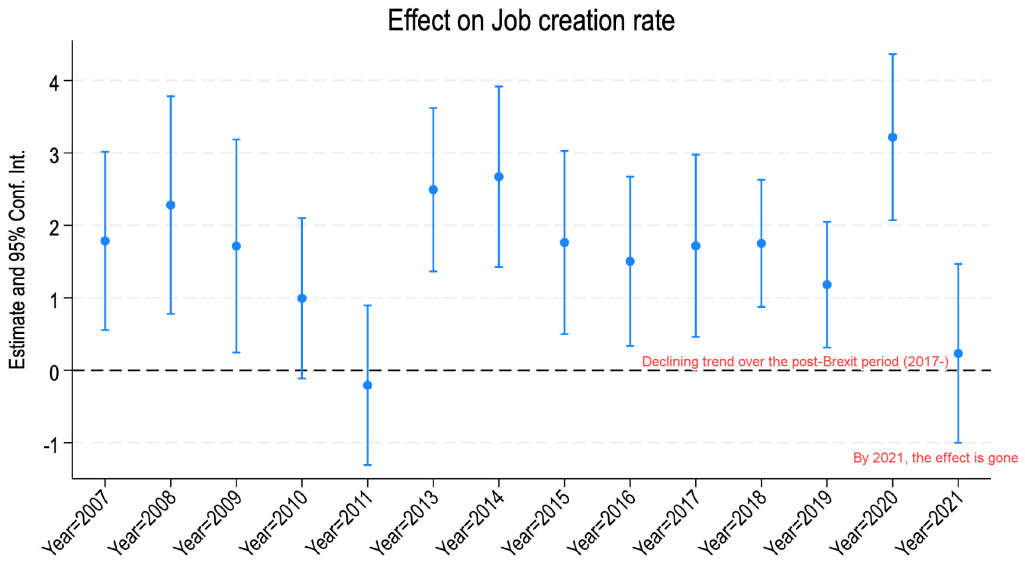

Figure 4: Figure A30 reproduced from HESA Data Report (Cambridge 2024)

3.2 Future Effects of the EJRA Policy

The Data Report forecasts the future effects of a policy change based on Larson and Diaz (2012) that builds on Little's law of queuing (Little (1961)) to study the effect of retirement policy at MIT. This is a more detailed model than that used by Ewart and outlined above and essentially says that in a stationary system, the long run average value of a stock variable (total number of employees) is equal to the product of the long term average net arrival rate to the system and the average residence time in the system. Abolishing the EJRA, for example, would lengthen the steady state time in the system for each employee and would therefore imply, if the total number of employees is held fixed, that the number of vacancies and new hires would fall. The system would take time to adjust to the new long run equilibrium, the so-called steady state. To build this model, one has to estimate the exit rates due to lateral moves, death, and retirement in the absence of a mandate, which is done from the average values observed in RRG over the 15 years of HESA data.

One implication of this model would be that the statistical model in (2) is misspecified, since it implies that there is a constant effect on vacancies from the EJRA whereas the

queuing model says that there is a dynamic response that takes time to take full effect. Secondly, the queuing model is treated as exact, whereas the key parameters are uncertain and estimated from data. In that respect, the estimates reported in the Data Report are subject to wide variability as is any social science phenomenon you are trying to forecast 40 years hence.

The model is also flawed in treating everyone in the queue as the same, failing to account for the differing contributions and income levels generated by faculty at different career stages. Senior faculty who stay on the payroll contribute significantly to the Research Excellence Framework (REF) returns, which in turn bring in pro-rata funding from central sources. Senior faculty tend to attract more PhD students due to their track records and projects. Their research funding attracts overheads, which are typically calculated as a percentage, resulting in more funding for more senior faculty. The Higher Education Quality Enhancement (HEQE) Quality-Related (QR) funding is also based on the actual cost of the faculty member, not a generic FTE. The model errs by taking a "unit person/Full-Time Equivalent" approach. The correct approach would consider an FTE at the typical income level that a faculty member at that career stage generates.

The results are reported in Table 1 of the Data Report. In the first ten years, there are on average 27.7 fewer new appointments. In the decade from 2053-2062 there are 13.3 fewer vacancies; which is apparently the chosen steady state regime. To put this in perspective, consider Table 3.1, which shows the total number of established academic staff at Cambridge over the period 2013-2023. The fourth column shows that in 2023 a total of 33 members of academic staff left office at the end of their EJRA year, ostensibly due to the EJRA itself, rather than to their own personal preferences. However, from 2022 to 2023, the total number of established staff grew from 1642 to 1669, a net addition of 27. In fact, since 2013, the number of established staff has grown by approximately 1% per year. If we extrapolate that trend in establishment size to 2053/2062, the loss in new vacancies would be more than matched by the the number of new positions every year. One final point to bear in mind when looking at the long term steady state, the State pension age is set to increase to 69 in 2046-2048 due to the increase in longevity.

4 Conclusion

This paper critically evaluates the findings of the Data Report for the EJRA Review Group, identifying crucial methodological flaws and misinterpretations. The Data Report suggests that the Employer Justified Retirement Age (EJRA) at the University of Cambridge increased job creation rates and contributed positively to the academic workforce. Our analysis reveals several key issues, including inappropriate filters applied to the data, inconsistent treatment of variables, and erroneous conclusions drawn from statistical models.

The Data Report applies more stringent filters to Cambridge data compared to other

Russell Group universities (RRG). Certain categories of staff are included in the Cambridge analysis but excluded from the RRG analysis, leading to inappropriate comparisons. Relevant data on job creation and retirements are excluded without clear justification, potentially deflating or inflating the observed job creation rates. Furthermore, the job creation rate is defined differently for Cambridge and the RRG, resulting in incommensurate comparisons.

The Data Report also contains inconsistencies in how variables are treated and measured. The time periods analysed for Cambridge were not directly comparable to those for the RRG, with trends and changes post-2011 not uniformly analysed across all institutions. Key variables such as retirement age and job creation rates are defined and calculated differently, failing to account for structural differences between institutions. Additionally, the Data Report does not uniformly adjust for macroeconomic factors and other external influences, leading to an inaccurate representation of EJRA's effect.

Importantly, the Difference-in-Differences model used in the Data Report does not adequately control for pre-existing trends and differences between Cambridge and the RRG, resulting in unjustifiable findings on EJRA's impact on job creation. The Data Report's claim that EJRA led to a 1.6 percentage point increase in job creation does not hold under proper controls and comparisons. The dynamic model used is inconsistent with the static regression approach, assuming a constant effect over time and ignoring potential variable interactions. The Data Report's results lack robustness checks, confidence intervals, and measures of uncertainty, with statistical significance disappearing when alternative methods (such as the wild bootstrap) were applied, indicating unreliable conclusions.

Our analysis shows that Cambridge University consistently had lower job creation rates for Established Academic Careers (EAC) compared to the RRG average, both before and after EJRA implementation in 2011. Cambridge's average job creation rate was approximately 3% per year, while the RRG average was about 5%. There was no relative improvement in job creation rates at Cambridge post-EJRA implementation. Every year since 2011, Cambridge has created fewer EAC jobs than the RRG average, contradicting the Data Report's claim of a positive impact.

Overall, we can conclude that the EJRA policy did not achieve its intended goal of increasing job creation for young academics and may have exacerbated existing disparities in job creation rates compared to other leading universities. Cambridge consistently had lower job creation rates for Established Academic Careers (EAC) compared to other Russell Group universities, both before and after the EJRA implementation, implying that EJRA is *not* a significant factor driving job creation rates. Other Russell Group universities, which do not have an EJRA, exhibit higher job creation rates, suggesting that job creation can be sustained or even thrive without such a policy.

We are grateful to Simon Baron-Cohen for helpful comments.

Dedication. We would like to dedicate this paper to the memory of Ross Anderson who devoted much of his tireless energy to combatting the spurious arguments put forth in favour of the EJRA at Cambridge.

5 Appendix

The main model that is used for establishing a significant effect is presented in Section 3.1 of the Data Report, the first displayed equation. We can rewrite this equation in more standard notation as

$$r_{it}^{jc} = \alpha_i + \gamma_t + \delta D_{it} + \varepsilon_{it}, \tag{2}$$

The variable D_{it} is the treatment dummy, equal to one for Cambridge post-2012 and equal to zero for Cambridge pre-2012, and equal to zero for all the RRG universities from 2007-2008 to 2021-2022. This approach is called a reversal design and it is used since the retirement policy at Cambridge did not change, while it did at the RRG (it was abolished).

The parameter of interest is δ, which captures the effect of EJRA on job creation (which is implicitly constant across time). The statistical null hypothesis is that $\delta = 0$, i.e., that the EJRA had no effect on job creation versus the general alternative. Table A.4 column 1 reports the results of this regression separately for EAC and EAR groups.

The authors compute the so-called difference in differences

$$\widehat{\delta} = \left(\overline{Cam}_{post} - \overline{Cam}_{pre}\right) - \left(\overline{\overline{RRG}}_{post} - \overline{\overline{RRG}}_{pre}\right), \tag{3}$$

where the overbar indicates averages over time and the double bar indicates averages over universities in RRG. It is important to note that there are only five observations in $\overline{Cam}_{pre}$ and ten observations in $\overline{Cam}_{post}$. In Figure A30, they report

$$\widehat{\delta}_t = Cam_t - Cam_{2012} - (\overline{RRG}_t - \overline{RRG}_{2012}). \tag{4}$$

In this case there is only one observation in Cam_t. By comparing with 2012, they carefully use the point of maximum difference to pivot around. A more standard approach would be to compare with the pre-policy average $\overline{Cam}_{pre} - \overline{\overline{RRG}}_{pre}$.

The authors' main estimates in column 1 of Table A.4. indicate that δ is positive and statistically significant under their calculated standard errors. However, when they apply the wild bootstrap standard errors in Section 3.4, they report that the statistical significance vanishes. The authors acknowledge that the usual parallel trends assumption is violated in this data, as is clear from Figure A1 and A30. This would imply the estimates in the first column of Table A.4 are suspect. To address this issue, they do the following described on page 9. They fit the regression model

$$r_{it}^{jc} = a_i + b_i t + e_{it}, \tag{5}$$

for each university using the data before 2012 (5 observations) to obtain $\widehat{a}_i, \widehat{b}_i$ and then calculate the residual $\widehat{r}_{it}^{jc} = r_{it}^{jc} - \widehat{a}_i - \widehat{b}_i t$ for all i, t. Then, they take $\widehat{r}_{it}^{jc}$ as the dependent variable in (2).

This is absurd. To understand why, imagine constructing a linear trend from five observations on any noisy time series and then extrapolating a further ten observations based on that trend. This is extremely bad practice from a time series point of view, and any public body such as the Bank of England could not argue policy on the basis of such evidence. In Figure A3, one can see two RRG universities have rather extreme boosts in job creation followed by a sharp reversal in one case and slower reversal in the other case, all before 2012. So whatever led them to increase job creation and decrease job creation could be repeated at any time after 2012. Logically, if you extrapolated the trend backwards, it appears that for some universities in 2000, the job creation would be negative. Hence the foundation of this pretrend assumption/method is very weak. Mechanically, however, the result of this process is to drag down the RRG values as those fake pretrends did not continue and this gives a boost to the estimated treatment effect. The treatment effect here is to be interpreted relative to a pre-established trend, so the figure of 4.408 in Table A.4 column 2 is not directly comparable with the result from column 1.

The second method they adopt to address the pre 2012 "trend" is based on the regression

$$r_{it}^{jc} = \alpha + \beta s_{it} + u_{it}, \tag{6}$$

where s_{it} is the log of FTE student numbers at university i in year t. In this case, the β is assumed constant over universities and again estimated using the pre-2011 data. They then calculate $\widetilde{r}_{it}^{jc} = r_{it}^{jc} - \widehat{\beta} s_{it}$ for all i, t and then apply the DID to this residual.

In this specification, it is odd to use the log of student numbers to predict the ratio of new hires. If we assume that the staff-student ratio is targeted to be constant we would have (where N is the number faculty and S is student numbers):

$$0 = d\left(\frac{N}{S}\right) = \frac{dN}{S} - \frac{N}{S}\frac{dS}{S} = \frac{N}{S}\left(\frac{dN}{N} - \frac{dS}{S}\right), \tag{7}$$

so that dN/N should be equal to $d \log S$, not $\beta \log S$. Empirically, $s = \log S$ is more related to $\log N$ than $d \log N$. It is also surprising that in this case, they do not allow β to vary with university, perhaps because the result would have been different had they done that. The estimates from this equation are not credible either. Interestingly, in column 3 of Table A.4 they show that for this method the EJRA for EAR staff produces a significant *increase* in vacancies.

The inference methods are "robust standard errors" in the main part and a wild bootstrap method in Section 3.4. The standard errors in Table A.4 effectively assume that ε_{it} is independent across i and t, that is, after accounting for the "main effects" α_i (university specific effect) and γ_t (time specific effect), the remaining movements are

uncorrelated. This is clearly a bad assumption in this case as the universities in the RRG are not separate individuals whose health outcomes do not affect others' outcomes but directly competing institutions who are subject to the same rules and procedures (apart from EJRA). The pre-trend estimation is not taken into account of in the standard errors. Instead, the authors just supply the residuals to STATA and get the usual standard errors that would arise if there were no pre-estimation. Clearly, estimating two parameters from five observations is pushing the boundaries of credibility. Credible standard errors would be much larger than those presented. Indeed, as acknowledged in Section 3.4 (robustness checks), when the wild bootstrap is used (this takes account of heteroscedasticity but not cross university and cross time correlations), the statistical significance disappears. Likewise, when a synthetic control method is used, that is, finding a linear combination of RRG that best matches Cambridge in the pre treatment period with respect to vacancy rate, they report that the statistical significance disappears.

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