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May 2025

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RE-ANALYSIS OF

Ciacci, R. (2024). Banning the purchase of sex increases cases of rape: evidence from Sweden. *Journal of Population Economics*, 37(2), 1-30.

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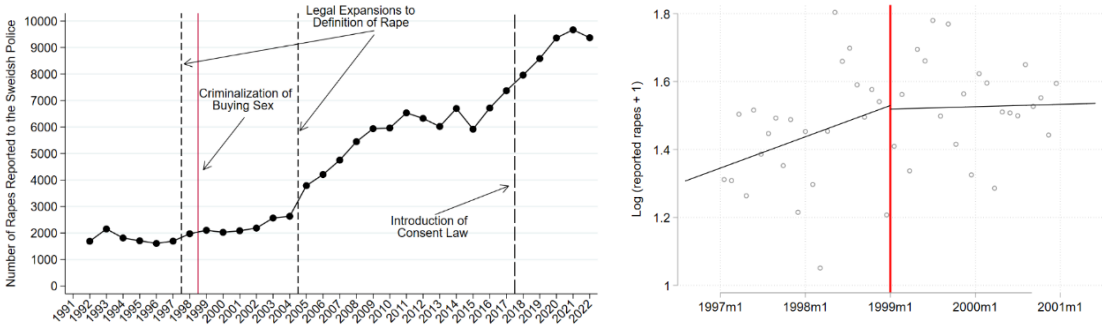
Abstract

The article *Banning the Purchase of Sex Increases Cases of Rape: Evidence from Sweden"* claims that Sweden's 1999 criminalization of buying sex increased rape by 44–62% (Ciacci, 2024). This finding is surprising given the flat time trend in reported rapes around the reform year. We show that errors in the paper's statistical modeling and coding produce the main result. Similar problems exist across four alternative identification strategies. In each case, measurement error or deviations between the implemented and stated methods create the illusion of an effect. Our re-analysis finds no evidence that the policy increased rape. The paper's faulty policy evaluation and top Altmetric score in the *Journal of Population Economics* underscore the importance of retraction.

[*] A note to the reader: Ciacci's original paper was published in March 2024 without replication files. The authors of this report noticed the paper via a viral tweet by John Holbein, which accumulated over 1 million views. A glaring difference between the paper's results and the Swedish time trend in reported rape made us curious about the methods. The author refused to provide replication material and we instead downloaded the raw data from BRÅ (the Swedish National Council for Crime Prevention). Asking the author why the main analysis (RDiT estimate) could not be replicated led him to share a portion of the do-file for that specific analysis. We then discovered that the paper's main result derived entirely from a misspecified regression model estimated with erroneous Stata code. We sent a written report with this information to the author in March 2024, who responded that "I do not agree with the points raised and I will follow the journal procedure for such cases.". We then sent the report to the journal and posted it online. The journal appointed a new editor and three reviewers to evaluate the critique. After 12 months, in March 2025, it published a note from the editor (Zimmerman 2025; doi: /10.1007/s00148-025-01096-1) and new results by the author (with the potentially misleading title "Additional evidence on the effects of banning the purchase of sex on cases of rape in Sweden"; Ciacci 2025). While the editor's note confirmed the existence of the coding error, it omitted the important information that the error produced the paper's main result. The author's comment concluded that the paper's result was "not robust". Our re-analysis was not published and the editor did not respond to an e-mail seeking to confirm that decision. Additional replication data and code for the original paper were posted as an attachment to the editor's note. These files were used for the extended replication provided here, which invalidates the paper's four alternative estimation strategies. The journal's evaluation concluded that academic misconduct did not occur, but did not disclose how it reached this conclusion. The replicators urge the journal to revisit the article in light of this additional evidence.

Introduction: A puzzling claim about a Swedish reform

Ciacci (2024) claims that Sweden's law criminalizing the purchase of sex led to a dramatic increase in rape. This finding is difficult to reconcile with the flat time trend in reported rapes around the reform. Figure 1A shows the absence of any noticeable jump in reported rapes when the new law was implemented in 1999 (red vertical line). Figure 1B shows the same pattern by plotting the paper's outcome variable at the region-month level over a 4-year window around the reform.



A: Number of reported rapes (1991-2022) *B: Log of reported rapes (1997—2001)*

Figure 1. Time trend in reported rapes in Sweden.

Note: Data for plot (A) were downloaded from the Swedish National Council for Crime Prevention. Data for plot (B) are from the replication materials of Ciacci (2024). The regression discontinuity plot was created with the rdplot command in Stata, using a symmetric bandwidth of 24 months. Bins show monthly averages and the first-order polynomials that are fitted using a regression that includes region and month fixed effects. N(B) = 1,008.

A research result usually becomes more credible when it replicates across several identification strategies. Ciacci replicates his main result with no less than four alternative methodologies. A striking figure plots similarly sized and statistically significant reform effects. Our replication documents how each result derives from either erroneous statistical modeling, measurement error, or the use of methods that differ in various ways from those stated in the published paper. Correcting these discrepancies overturns the paper's result in each case. Figure 2 shows the original estimates from Ciacci's (2024) summary figure (red markers) alongside our updated estimates (blue markers). The sections below go through each strategy to explain our re-analysis.

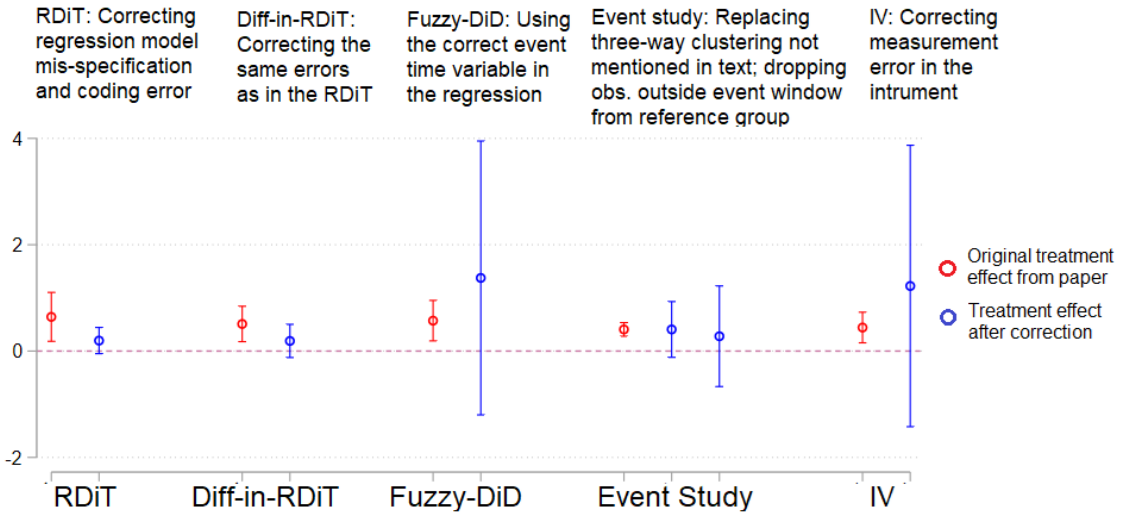


Figure 2. Summary figure of original (red) and corrected (blue) reform effects.

Note: Data from the replication package of Ciacci (2024) were used to produce the estimates in red. Vertical lines are 90% confidence intervals, as in the original figure. The re-estimated reform effects differ from the original estimation as follows. The re-analyzed RDiT and Diff in RDiT estimates drop the year FEs, residualize the outcome with month FEs prior to estimation, and use the "rdrobust command" that produces robust bias-corrected confidence intervals. The re-analyzed fuzzy DiD uses the correctly specified time variable and drops the controls for trends in regions, years and months. The re-analyzed event study clusters standard errors at the region-month level and, in the estimate to the right, excludes observations outside the event window. The re-analyzed IV estimate corrects measurement error by using the same instrument values for Uppsala and Stockholm regions. For the Fuzzy-DiD and event study, Ciacci obtains the number in the summary graph by multiplying the estimated reform effect by 13, with the motivation that "there have been on average 13 fine events for the purchase of sex per month in each region". We do the same but not our inability to understand the motivation for this adjustment or where the number came from, because *fine event* is a binary variable at the month-region level. Ciacci multiplies the IV estimate by 23.5 noting that "there have been on average 23.5 fines for the purchase of sex per month in each region". We again do the same but note that the average number of fines per region and month was 1.47, not 13, between 1999 and 2014.

Main analysis: An RDD impossible to estimate

The paper's main analysis uses a regression discontinuity design with the reform's implementation year as the treatment threshold. The paper estimates

$$log(rape_{rmy}) = \beta_1 \mathbb{I}\{y \geq Jan99\} + \beta_2 F\{y \geq Jan99\} + \gamma officers_{ry} + \alpha_r + \alpha_m + \alpha_y + \epsilon_{rmy} \quad (1)$$

where the dependent variable is the log of reported rapes and the treatment variable is a dummy taking the value 0 for all months before January 1999 and 1 for all months thereafter. The estimate of β_1 captures the treatment effect from the reform, consisting of a potential "jump" in the month-region average of reported rapes at the implementation time point. The second term in the regression is a control function for the running

variable[1], and *officers* is a control for the number of police officers. The specification includes fixed effects for region and month (α_r and α_m). Importantly, it also includes year fixed effects (α_y).

Ciacci's estimates Equation (1) in two samples: the "whole sample" from 1997 to 2014 and the "restricted sample" from 1999 to 2001 (quotations in original). Table 1 shows the estimated reform effects in these windows. The estimated effect in the restricted sample suggests a 65% increase in rape (0.65 log points[2], p-value 0.022), while the whole sample suggests a 55% increase (0.55 log points, p-value 0.075).

A fundamental error renders both these estimates invalid. Equation (1) is impossible to estimate due to perfect multicollinearity between the treatment variable and the fixed effects. Including dummies for each year removes all variation in treatment status conditional on the controls. Using standard Stata commands such as xi:reg, areg, or reghdfe drops the treatment variable and returns an error message, as demonstrated in a log file attached at the end of this report. We make the misspecification explicit in Table 1, denoting with N/A that properly executing the regression does not return any estimated coefficients (columns 2 and 5).

How, then, does Ciacci obtain his estimates? A specific Stata command and variable ordering avoid automatically dropping the treatment variable. Using the reg command with i.s in front of categorical variables but omitting the xi: prefix makes Stata prioritize regressors based on order of appearance. Placing the treatment variable first causes Stata to retain it while dropping later-listed fixed effects. Interestingly, this makes the main result highly sensitive to variable order: simply switching the order of year and month fixed effects eliminates the main effect in the full sample (0.052 log points, p-value 0.896).

Importantly—dropping fixed effects from Equation (1) to obtain an estimate on the treatment variable fundamentally changes its interpretation. Instead of capturing a discontinuity at the threshold, the estimate primarily reflects seasonality in reported rapes between December and January. Rape is a crime known to have strong seasonality, and the lowest number of reports occur in December (see, e.g., Falk 1952, McDowall et al. 2012). This pattern also holds in the Swedish data. Appendix Section A2 discusses which fixed effects drop out of Ciacci's estimation and how this alters the treatment effect's interpretation.

A major obstacle exists for any proper re-analysis of the Swedish reform using regression discontinuity in time. Police bureaucracies under national government control implemented the reform simultaneously across all regions. This results in a single treated

[1] This regression equation from the paper gives the impression of only including a polynomial control function F(.) on the right side of the threshold, but the actual analysis uses a control function on each side.

[2] See Appendix Section A1 for a brief comment on the log transformation of the dependent variable.

cluster and no independent variation to identify a treatment effect of the reform. Implementation was not, as Ciacci appears to claim, under the control of regional elected assemblies and their corresponding regional bureaucracies. This problem alone should have halted the main analysis.[3]

Setting aside the reality of a single treated cluster, we demonstrate the consequences of correcting other errors in Ciacci's RDiT analysis. First, we remove the year fixed effects in Equation (1). Then, we remove monthly seasonality following the best-practice method proposed by Hausman and Rapson (2018), cited by Ciacci as his primary methodological reference. This method involves regressing the outcome on month fixed effects and subsequently using the residuals from that regression as the outcome variable.

A third correction applies the correct optimal bandwidth. Ciacci claims that "[t]he bandwidth used is the optimal bandwidth as defined in Calonico et al. (2014)." His Stata code manually saves this number from the rdwbselect command. It then proceeds to saving and using a different number in the regressions: an alternative bandwidth number provided by this command for calculating "the bias of the RD treatment effect estimator" (rdwbselect help file).

Appendix Table A1 briefly demonstrates the consequences of using the correct bandwidth on the original results. Applying the correct bandwidth renders the estimates implausibly large in the restricted sample and statistically insignificant at conventional levels in the whole sample. The similar estimate sizes and consistent statistical significance across samples clearly hinges on using the wrong bandwidth.

A final correction to the RDiT applies the "robust bias correction" available in the rdrobust package to calculate p-values and confidence intervals. This is necessary because the optimal bandwidth calculation specifically allows for the possibility of bias in the estimate, which means that both the estimates and standard errors need to be corrected (see Cattaneo et al. 2019 for a detailed discussion). Had Ciacci used the rdrobust command rather than the reg command to estimate Equation (1), this correction would have been automatic.

The corrected analysis finds no evidence of a reform effect. The estimated effect sizes are small and statistically insignificant: 0.15 (p-value 0.188) in the restricted sample and 0.066 (p-value 0.260) in the whole sample (columns 3 and 6 in Table 1). We include the corrected estimate for the restricted sample in Figure 2 above. Even in a hypothetical scenario where Swedish regions made autonomous decisions in this policy domain, our re-analysis finds no evidence that criminalizing the client increased reported rapes.

[3] Note also that the lack of variation in the treatment across space precludes improved precision by using more fine-grained data in future studies.

Table 1. Re-analysis of RDiT results in Ciacci (2024).

	Restricted sample			Whole sample		
	Original estimate (Table 3, column 1, row 4; Figure 5)	Re-analysis of column (1) without coding error	Corrected analysis (incorrectly assuming more than one cluster)	Original estimate (Table 3, column 1, row 2)	Re-analysis of columns (4) without coding error	Corrected analysis (incorrectly assuming more than one cluster)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.643**	N/A	0.156	0.548*	N/A	0.066
	(0.280)	N/A	(0.121)	(0.306)	N/A	(0.072)
p-value	*0.022*	*N/A*	*0.188*	*0.075*	*N/A*	*0.260*
Observations	399	N/A	373	1,113	N/A	1,003
Year FE	x	x		X	x	
Region FE	x	x	x	X	x	x
Month FE	x	x	x	X	x	x

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are in parentheses and p-values are in italics. Columns 1 and 4 show the original reform effects from estimating Equation (1) in the restricted and whole data samples. Columns 2 and 5 show that running those regressions without Ciacci's Stata coding error causes the program to drop the treatment dummy and return an error message. Columns 3 and 6 report estimates from a corrected RDiT analysis under the incorrect assumption of more than one cluster of treated units. We correct the original estimation by (a) dropping the year fixed effects, (b) removing monthly seasonality by residualization, (c) applying the correct optimal bandwidth, and (d) using robust bias-corrected p-values.

Difference in RDiT—Failure to remove seasonality and cascading error from the main analysis

Ciacci (2025, page 37) writes that "[t]here might be concerns that the found effect using the RDiT specification is due to seasonal changes in rape due to the fact that the ban went into effect in January. To address this issue, the Diff in RDiT specification solves this problem by differencing the data across time to remove seasonal changes." The paper further describes how "Following Butts (2021) to carry out this estimation it suffices to take the first difference of the dependent variable and time varying controls of regression model (2)" (i.e., regression equation 1 above).

Ciacci's claim that his Diff in RDiT analysis removes seasonality is misleading. The paper cited by Butts (year of published paper is 2023) deals with differences in geographical discontinuities rather than discontinuities in time. This is clear from the abstract that states "*This note extends the difference-in-discontinuities framework discussed in Grembi et al. (2016) to a geographic setting. This paper formalizes the identifying assumptions in this context, which will allow for the removal of time-invariant sorting and multiple treatments similar to the difference-indifferences"*. In other words,

the transformation does not remove seasonal changes. In this case, it even amplifies the December–January difference in the log of reported rapes. Before the transformation, the average difference in levels between December and January during the years used in the RDiT analysis is 0.10 log points. After the transformation, the average difference in the first difference of the outcome variable increases to 0.27 log points—almost three times larger.

Table 2 shows Ciacci's results from estimating Equation (1) after taking first differences. The code for these regressions excludes the month and region fixed effects, although this is not mentioned in the paper. The estimate in the restricted sample now suggests a reform effect of 51% more rapes (0.51 log points, p-value 0.012), and the estimate in the whole sample suggests 57% more rapes (0.57 log points, p-value 0.038).

Table 2. Re-analysis of Difference in RDiT results in Ciacci (2024).

	Restricted sample			Whole sample		
	Original estimate (Table C.1, column 1, row 4; and Figure 5)	Re-analysis of column (1) without coding error	Corrected analysis (incorrectly assuming more than one cluster)	Original estimate (Table C1, column 1, row 2)	Re-analysis of columns (4) without coding error	Corrected analysis (incorrectly assuming more than one cluster)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.510**	N/A	0.138	0.572**	N/A	0.065
	(0.202)	N/A	(0.159)	(0.275)	N/A	(0.082)
p-value	*0.012*	*N/A*	*0.315*	*0.038*	*N/A*	*0.321*
Observations	483	N/A	373	1,113	N/A	1,003
Year FE	X	x		x	x	
Region FE	X	x	x	x	x	x

Note: *** p<0.01, ** p<0.05, * p<0.1 Standard errors in parenthesis and p-values in italics. Columns 1 and 3 show the original reform effect from estimating Equation 1 in the "restricted" and "whole" data samples (quotations in original). Columns 2 and 4 show that running those regressions without the Stata coding error makes the program drop the treatment dummy and return an error message instead of a regression output. Columns 3 and 6 report estimates from a re-analysis under the incorrect assumption of more than one treatment cluster. We correct the original estimation by (a) dropping the year fixed effect, (b) correcting for monthly seasonality by residualization, (c) using the optimal bandwidth rather than a different number, and (d) using robust bias-corrected p-values.

We again show that Ciacci's original regressions are impossible to estimate using a proper Stata command (columns 2 and 5). We then apply a similar set of corrections as in the RDiT analysis: (a) dropping the year fixed effects, (b) replacing the original differenced outcome, which corrects for "time-invariant characteristics", with our seasonality-adjusted outcome by residualization, (c) using the correct optimal bandwidth, and (d) applying robust bias-corrected p-values. The corrected analysis shows no evidence of a reform effect (columns 3 and 6). The estimated reform effect in the

restricted sample is 0.138 (p-value 0.315) and the estimate in the whole sample is 0.065 (p-value 0.321). We include the restricted-sample estimate in our summary Figure 2.

Fuzzy DiD—Using the wrong time variable in the regression

The paper's last three methods examine how police fines given to people who pay for sex can measure the effect of the law. The analysis is based on the idea that men choose between paying for sex and committing rape, depending on the relative prices of these two options. When the cost of paying for sex increases, they switch to rape. Seeing others get fined for buying sex shows signals a price increase for "one unit of prostitution". These assumptions underpin the author's interpretation of a positive relationship between fines and rape as evidence of a reform impact.

The paper uses three different measurements of fines at the region-month level. The Fuzzy DiD uses the cumulative number of *fine events* in a region, where a *fine event* is a dummy variable for at least one fine occurring in a region-month. The event study uses the binary variable for fine events, and the IV analysis uses the monthly number of fines.

Ciacci (2024) applies the Wald-DiD estimator and fuzzydid package from de Chaisemartin and D'Haultfoeuille (2018).[4] This package requires users to define (among other things) the time variable, which in this case should indicate a specific month in a specific year. Ciacci creates this variable in the code but replaces it with the calendar year variable in the Stata command. That command also includes region-, year- and month-specific trends, which are not mentioned in the text.

Table 3 compares the original estimate of the reform effect (column 1) with our adjusted estimates. The statistical significance of the reform effect disappears when we use the correct time variable (column 2), remove the region-, year- and month-level trends to align the estimation with the regression described in the paper (column 3), or do both corrections (column 4, p-value 0.380). We include the original estimate from column 1 and the "doubly corrected" estimate from column 4 in our summary Figure 2. Both are multiplied by 13 to reproduce the author's rescaling (discussed in the figure note). The corrected and rescaled estimate now suggests a (non-significant) 140% increase in rape from the reform.

[4] An important assumption of this estimator is that the treatment effect is stable over time (de Chaisemartin and D'Haultfoeuille 2018). In the case of the cumulative number of fine events, this implies that to satisfy this assumption, a fine event should have the same effect over time relative to treatment, i.e., a fine event should have the same impact on the outcome in the period it was issued as many years later. If this is not satisfied, both the treated and control units may be contaminated with dynamic effects from past fine events and the estimates rendered unreliable. Ciacci mentions that traditional DiD methods are biased, but does not explain what assumptions are needed to interpret the Fuzzy-DiD.

Table 3. Re-analysis of Fuzzy DiD results in Ciacci (2024).

	Original estimate (Table S.5, column 1, row 1)	Estimation with the correct time variable	Removing trend variables not described in the paper's text	Both corrections from columns (2) and (3)
	(1)	(2)	(3)	(4)
Fine events	0.044*	0.017	0.280	0.106
	(0.018)	(0.016)	(2.079)	(0.120)
p-value	*0.014*	*0.310*	*0.893*	*0.380*
Observations	4,536	4,536	4,536	4,536

Note: *** p<0.01, ** p<0.05, * p<0.1 Standard errors in parenthesis and p-values in italics. Results from estimating the Wald-DiD Fuzzy-DiD estimator. Column 1 estimates the same model as in the do-file in the replication package, i.e., using calendar year as the time variable and including region, year-, and month-specific time trends. Column 2 uses the correct year-month variable as a time variable, column 3 drops the trends, which are not described in the text of the paper, and column 4 does both corrections.

Event study—Methodological choices suppress standard errors

The paper's event study tests for the impact of fine events (region-months with at least one fine for sex purchases) on rape in an event window spanning four months. It estimates

$$log(rape_{rmy}) = \sum_{\substack{j=-2 \\ j\neq -1}}^{2} \beta_j fine\ event_{rm-jy} + \alpha_r + \alpha_m + \alpha_y + \gamma\ officers_{ry} + \epsilon_{rmy} \qquad (2)$$

where *fine events* denote dummy variables for each time point in the evaluation window, i.e. from two months before an event (t = -2) to two months thereafter (t = 2). Leaving out the dummy for the month immediately before the event (t = -1) makes this time point the reference category for the point estimates on the other dummy variable. The author includes all observations outside the end points of the window (-2 < t or t > 2) in this control group by assigning them a value 1 on the dummy for t = -1.

The paper finds a non-zero and statistically significant treatment effect of the fine event in the same month as the event occurs, i.e. on the dummy variable for t = 0. The author's interpretation is that a fine event causes an immediate 3.4%[5] increase in rapes (p<0.001). This presumably happens because men who observe another man being fined decide to rape someone instead of buying sex. We show this estimate in column 1 of Table 4. Multiplying it by 13 gives the rescaled original estimate size shown Figure 2.

Examining the do-file for the event study reveals a discrepancy in the method for clustering the standard errors. The code applies a user-written Stata command called reghdfe that allows for multi-way clustering and implements that method based on year, month, and region. This method is not mentioned in the text, which only comments

[5] The estimate in Ciacci's event study is 3.1 and not 3.4

"clustering by region-time" in the note to Table S.10[6]. The chosen method differs from all other analyses in the paper, where clustering is done either at the region or region-month levels.

We analyze if the unusual clustering method affects the results by re-estimating the regression equation with alternative methods. The chosen method gives a small standard error of 0.006 on the treatment effect (p-value < 0.001). Alternative clustering methods return standard errors four times as large. Clustering at the level of the region, region-month, or region-year-month levels (see columns 2—6 in Table 4) gives standard errors on the treatment effect that range in size from 0.023 to 0.026 and results in p-values of around 0.2. We include the estimate from column 4 with region-month clustering in Figure 2 after applying the same rescaling as the author.

Table 4. Re-analysis of event study results in Ciacci (2024); clustering methods.

Clustering method	Three-way Original estimate (Table S.10, column 1	Standard one-way				
Clustering level	Region, year, and month	Region	Region-year	Region-month	Region-year-month	No clustering
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for t = -2	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006
	(0.022)	(0.028)	(0.026)	(0.028)	(0.027)	(0.029)
p-value	*0.779*	*0.825*	*0.809*	*0.822*	*0.816*	*0.829*
Dummy for t = 0	0.031***	0.031	0.031	0.031	0.031	0.031
	(0.006)	(0.023)	(0.024)	(0.025)	(0.023)	(0.026)
p-value	*0.000*	*0.193*	*0.185*	*0.203*	*0.181*	*0.225*
Dummy for t = +1	-0.013	-0.013	-0.013	-0.013	-0.013	-0.013
	(0.048)	(0.043)	(0.039)	(0.040)	(0.038)	(0.037)
p-value	*0.787*	*0.757*	*0.731*	*0.737*	*0.727*	*0.717*
Dummy for t = +2	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.043)	(0.036)	(0.038)	(0.039)	(0.038)	(0.040)
p-value	*0.989*	*0.986*	*0.987*	*0.987*	*0.987*	*0.988*
Observations	4,536	4,536	4,536	4,536	4,536	4,536

Note: *** p<0.01, ** p<0.05, * p<0.1 Standard errors in parenthesis, p-values in italics. Column 1 estimates the same model as in the do-file in the replication package, which uses three-way clustering to calculate the standard errors. The analysis in columns 2 to 5 use the same regression specification (EQ 2) but uses the standard one-way clustering and varies the level of clustering. Column 6 does not cluster the standard errors.

The much smaller standard errors in the three-way clustering likely stem from the small number of clusters. As the asymptotic behavior of multiway clustering is governed by the number of clusters of the dimension with the smallest number of clusters, here the

[6] It is also notable that in other parts of the paper, when Ciacci writes that he clusters by region-month, he uses one-way clustering at the region-month level rather than two-way clustering by region and month.

number of months (12), the number of clusters becomes too small for reliable cluster-robust inference (Cameron and Miller 2015).

Choosing an objectively correct level of clustering is often challenging. The present case does not require a definite answer to this question, because all levels of clustering render the treatment effect statistically insignificant at conventional levels. Because Ciacci (2024) does not mention or motivate his chosen multi-way clustering method in the paper, it is difficult to understand why this method was used in the event-study, where the significant results depend on it, but not in any other analysis.

We next examine the importance of including observations outside the event window in the reference category. Table 5 compares the paper's original estimate (column 1) with an alternative specification that includes a dummy for observations outside the window (column 2). The size of the treatment effects shrinks somewhat and standard errors become about seven times as large in the adjusted specification (p-value 0.541). This drop in precision is perhaps not surprising given that 2,763 of the 2,982 observations with an event time value of t = -1 in the original estimation lie outside the event window. We include this estimate in Figure 2 after applying the same rescaling as the author. We find comparable results if we instead drop observations outside any event window (column 3).

Table 5. Re-analysis of event study results in Ciacci (2024); reference category.

	Original estimate (Table S.10, column 1)	Original estimate after adding an out-of-window dummy	Dropping observations outside any event window
	(1)	(2)	(3)
Dummy for t = -2	-0.006	-0.010	-0.017
	(0.022)	(0.041)	(0.041)
p-value	*0.779*	*0.807*	*0.688*
Dummy for t = 0	0.031***	0.027	0.021
	(0.006)	(0.044)	(0.044)
p-value	*0.000*	*0.541*	*0.637*
Dummy for t = +1	-0.013	-0.017	-0.013
	(0.048)	(0.067)	(0.070)
p-value	*0.787*	*0.801*	*0.852*
Dummy for t = +2	-0.001	-0.005	0.004
	(0.043)	(0.065)	(0.069)
p-value	*0.989*	*0.944*	*0.960*
Observations	4,536	4,536	1,773

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parenthesis, p-values in italics. Column 1 uses the specification from the replication package that produces the estimates in Column 1 in Table S.10. Column 2 uses the same specification but includes a dummy for observations that are outside of the event window, while column 3 drops all observations outside of any event window.

IV Analysis—Measurement error in the instruments

The paper's self-described second main identification strategy uses "instrumental variable techniques that exploit plausibly exogenous variation in the availability of flights (to proxy for access to sex tourism)". The idea is that access to more flights allow men to engage in sex tourism, which lowers the number of fines for sex purchases and obscures the signal to other men that rape has become relatively cheaper. The instrument plausibly meets the exclusion restriction because airlines' operational decisions do not consider specific changes in criminal law.[7]

Measurement error in the instrumental variables causes the IV analysis to replicate the paper's main result. The instruments severely mismeasure their intended variation, which the paper describes as follows: "Intercontinental flights are the main means of transportation for Swedish sex tourists. Plausibly, sex tourists seem more (less) likely to travel in months in which there is a higher (lower) supply of intercontinental (continental) flights. In effect, ideally I would like to exploit monthly increases in the numbers of such flights in the main airport of the region."

The method for identifying a region's main airport introduces measurement error. This method relies on Google Maps queries for "airport" and the name of the region as the point of departure. The airport closest to the region's placement "pin" is assigned as its main airport. Regions without any airports within a 60 km driving distance from that pin are considered as being too distant and have their instruments set to zero.

Ciacci's list of main airports excludes five of Sweden's six largest airports (see list in Appendix Table A2). This happens because Google Maps sets the "pin" for a regional name to an arbitrary location within its borders rather than its main urban hub, erroneously assigning small or no airports to various regions. For example, *Västra Götaland*, which houses Sweden's second-largest city, *Gothenburg*, and its second-largest airport, *Landvetter*, is assigned the tiny *Trollhättan* airport instead. *Landvetter* had 20,813 international flights and 9,341 domestic flights in the measurement year, whereas Trollhättan had just 6 international and 868 domestic flights.[8]

Since we do not have access to complete flight data for each airport, we do not fully correct this analysis. Instead, we focus on specific error apparent to the authors based on personal experience traveling to Arlanda Airport.

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[7] The paper estimates the regression $log(rape_{rmy}) = \beta fines_{rmy} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \epsilon_{rmy}$, where *fines* is the number of fines in a region and month, and $\alpha_r * y$ are a region-year time trends. *Fines* is instrumented by quantitative measurements of international and domestic flights from the region's "main airport".

[8] Likewise, *Malmö* airport (5,048 international flights and 8,486 domestic flights in 2014) has been replaced by the tiny *Kristianstad* airport (54 international and 662 domestic flights) for *Skåne region*, and *Luleå* (355 international and 6,049 domestic flights) has been replaced by *Gällivare* (2 international and 1,372 domestic flights in 2014) as its main airport rather than) for *Norrbotten Region.*

The *Uppsala* region exemplifies the erroneous measure of distance to the main airport. This region houses Sweden's fourth-largest city, *Uppsala*, located just 35 kilometers, a 30-minute drive, from *Arlanda*, the country's largest airport. This distance is almost identical to *Arlanda's* distance from the center of *Stockholm*. However, the random placement of the Google Maps "pin" in *Uppsala's* northern outskirts puts *Arlanda* outside the 60 km radius for inclusion in the instrument (see Appendix Figure A3).

Equipped with an erroneous list of main airports and distances, the paper constructs two instruments for the number of continental (including domestic) and intercontinental flights. The instrument for continental flights is set to zero unless the number of flights in a specific month lies more than one standard deviation above the yearly mean. The instrument for intercontinental flights is set to zero unless the number is more than one standard deviation below the yearly mean.[9] *Arlanda* Airport shows much larger instrument values than other airports due to its size and the omission of most other large airports in the country (see Appendix Figures A4 and A5).

Table 6. Re-analysis of IV results in Ciacci (2024).

	Original estimate (Table S.12, column 1)	Correcting the instrument: Setting Uppsala's instrument values equal to those of Stockholm	Correcting the instrument: Setting Stockholm's instrument values equal to those of Uppsala
	(1)	(2)	(3)
Number of Fines	0.019**	0.052	-0.005
	(0.007)	(0.069)	(0.059)
p-value	*0.011*	*0.447*	*0.932*
Observations	4,416	4,416	4,416
F-stat	77.5	1.4	0.9

Note: *** p<0.01, ** p<0.05, * p<0.1 Clustered standard errors in parenthesis and p-values in italics. Column 1 uses the specification from the replication package that the main IV estimate shown in Figure 5. Column 2 uses the same specification but sets the instruments for Uppsala Region equal to the values for Stockholm Region, with which it shares Arlanda airport. Column 3 sets the instrument to zero in Stockholm.

Our re-analysis shows how measurement error drives the paper's results. Column 1 of Table 6 presents the original estimate of 0.019, rescaled by 23.5 to produce the author's reported 47% increase in rape, shown in Figure 2. Correctly assigning Stockholm's instrument values to Uppsala yields a very weak first stage in the IV analysis and increases the standard error on the treatment effect tenfold, from 0.007 to 0.069 (column 2 of Table 6). The p-value also rises from 0.022 to 0.447. We include the original estimate and the corrected estimate in Figure 2, after applying the same rescaling used by the

[9] These restrictions on the instruments are crucial for finding a treatment effect in the IV analysis.

author. The note to Figure 2 briefly discusses our inability to comprehend where this rescaling number came from.

Given that the population centers of *Stockholm* and *Uppsala* are equidistant from Arlanda, one can also argue that neither *Stockholm* nor *Uppsala* should be defined as close enough for the instrument to be switched on. Setting the instrument to zero for Stockholm also eliminates the significant treatment effect (column 3 of Table 6).

Conclusion

This report re-examines the claim by Ciacci (2024) that Sweden's legislation to criminalize the purchase of sex caused a large increase in rape. Re-analyzing the paper's five identification strategies shows that erroneously implemented statistical methods produce the illusion of reform effects in each case.

The variety of errors in Ciacci (2024) highlight the importance of re-analyses that check whether researchers implement the same methods they describe, and do so correctly. Implemented methods may incorrectly replace variables in regressions, add new ones, or change clustering methods. Commands may also be wrongly implemented. An example of the latter is Ciacci's circumvention of an RDD command's automatic use of an optimal estimation window by manually inserting a different window size.

Most errors in Ciacci's paper would have been difficult to discover in the peer review process. No replication material was posted with the original article, the author refused to share these files, and the editor did not respond to a request for access. We refer the reader to our note on p. 1 for a brief history of our replication efforts.

In conclusion, the research community has a joint responsibility to work to ensure that published research meets high standards of empirical rigor and ethical conduct. The conclusion and policy advice in Ciacci (2024) are erroneous and misleading for policymakers and researchers. We strongly recommend that the journal retract this paper.

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Appendix

Section A1. Log transformation of the outcome variable

A concern not highlighted in our main text stems from using the log-transformed number of region-level rapes as an outcome. This does not identify a policy relevant estimand, but rather an average relative effect at the regional level. As Sweden's counties vary substantially in size and number of crimes, the log-transformed outcome overweighs small counties with small baseline levels of rape. A Swedish policy-maker caring about the country-wide rate of sexual assault would rather be interested in the total effect. An addition concern is that regressions of log+1-transformed outcomes cannot retrieve average relative effects in the presence of zeros in the outcome variable (Chen and Roth, forthcoming).

Reference

Chen, J., & Roth, J. (forthcoming). Logs with zeros? Some problems and solutions. *The Quarterly Journal of Economics*.

Section A2. Interpreting the erroneous regression output in the main analysis

What does the point estimate on the treatment effect capture when partial year and month fixed effects are included? Examining the variation that remains in the data after dropping certain year and/or month fixed effects, we argue that the estimates most likely capture seasonal fluctuations in reported rapes. We begin by discussing what happens in each of the two samples, starting with the full sample.

To the best of our knowledge, the following occurs in the full sample. Stata drops one year dummy among the "treatment years," which ends up being the dummy for the last year (2001). The Treat variable therefore compares 1997 to 2001. To get estimates on the running variables, which are also prioritized by Stata, the program must drop either an additional year dummy or an additional month dummy. The author's code lists the year dummies before the month dummies, causing Stata to drop an additional month dummy (December).

To better understand what is being estimated in this regression, we turn to Figure A1. The left-hand side shows the seasonality in reported rapes in the Swedish data (compare January values shaded in red and December values shaded in green). This seasonality remains when we plot residuals of the paper's outcome variable (log(rapes+1)) from a regression including all the fixed effects not dropped in the original estimation, and excluding the treatment dummy. Residualizing the outcome variable in the same way produces variation that coincides with the seasonality. Figure A1.B compares this

residualized variation in the treatment (solid black line) with the intended variation of a discrete jump at the time threshold (dashed line).

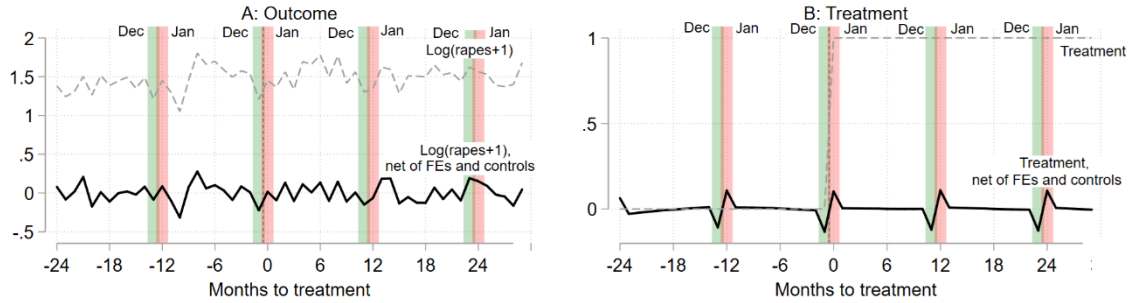

Figure A1. Variation in the outcome and treatment variables used in Ciacci's RDiT analysis in the whole sample.

Note: Plot (A) shows the January-December seasonality in the paper's raw outcome variable (dashed line) and in residuals from regressing that variable on the second order polynomial trends in the running variable and fixed effects that are not dropped in Ciacci's (2024) estimation of Equation 1 in the whole sample (solid line). December is shaded green, January is shaded red. Plot (B) shows the variation in the raw treatment variable (dashed line) and residualized treatment variable (solid line), using the same residualization method as in plot (A).

We can clearly see from Figure A1.B that the treatment is only "switched on" in December and January. In addition, the variation in the rezidualized treatment is independent of the implementation of the reform. This means that the estimates will essentially capture variation between December and January across the full estimation sample. On average, the difference between these two months is about 0.10 log points. How do we when then get to an estimate of 0.5? Turning to the right-hand side (B) we see that the resisualized difference in the treatment variable across the two months is 0.2 and not 1. In other words, when we increase the residualized treatment variable with 0.2, we increase the average outcome with 0.1. The predicted change in the outcome from increasing the treatment by one unit is thus 0.5, which is the estimated treatment effect.

We now repeat the exercise from Figure A1, but for the restricted sample, which only includes 9 months on each side of the threshold after Ciacci's bandwidth restriction. In this analysis, Stata is first forced to drop both year dummies in order to get an estimate on the treatment dummy. To get estimates on all running variables it is also forced to drop the month dummies for November, December, and January. In Figure A2, we illustrate what variation in the treatment and outcome that remains.

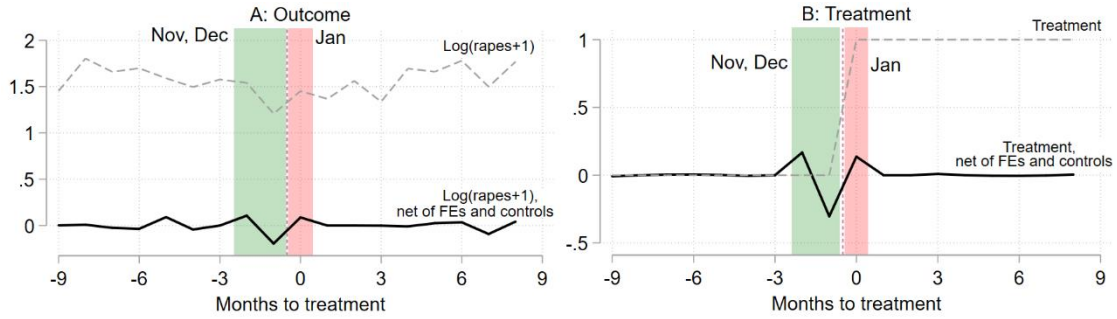

Figure A2. Variation in the outcome and treatment variables used in Ciaccis RDiT analysis in the restricted sample.

Note: Plot (A) shows the January-December seasonality in the paper's raw outcome variable (dashed line) and in residuals from regressing that variable on the second order polynomial trends in the running variable and fixed effects that are not dropped in Ciacci's (2024) estimation of Equation 1 in the restricted sample (solid line). November and December are shaded green, January is shaded red. Plot (B) shows the variation in the raw treatment variable (dashed line) and residualized treatment variable (solid line), using the same residualization method as in plot (A).

We can see that the treatment in Ciacci's analysis is only switched on in the months for which the month dummies are omitted, i.e., November, December, and January (see Figure A2.B). Specifically, the treatment estimate will compare the outcome values at two time points, November prior to the reform and January after the reform, to the outcome in December prior to the reform. Since the outcome is lower in December than in the other two months, Ciacci finds a positive treatment effect, but this estimate no longer captures the intended jump at the time threshold of the reform. We also note that the outcome is marginally higher in November prior to the reform than in January after the reform. This means that pre-reform variation is more important than pre-post variation for producing the large and positive estimate on the treatment variable.

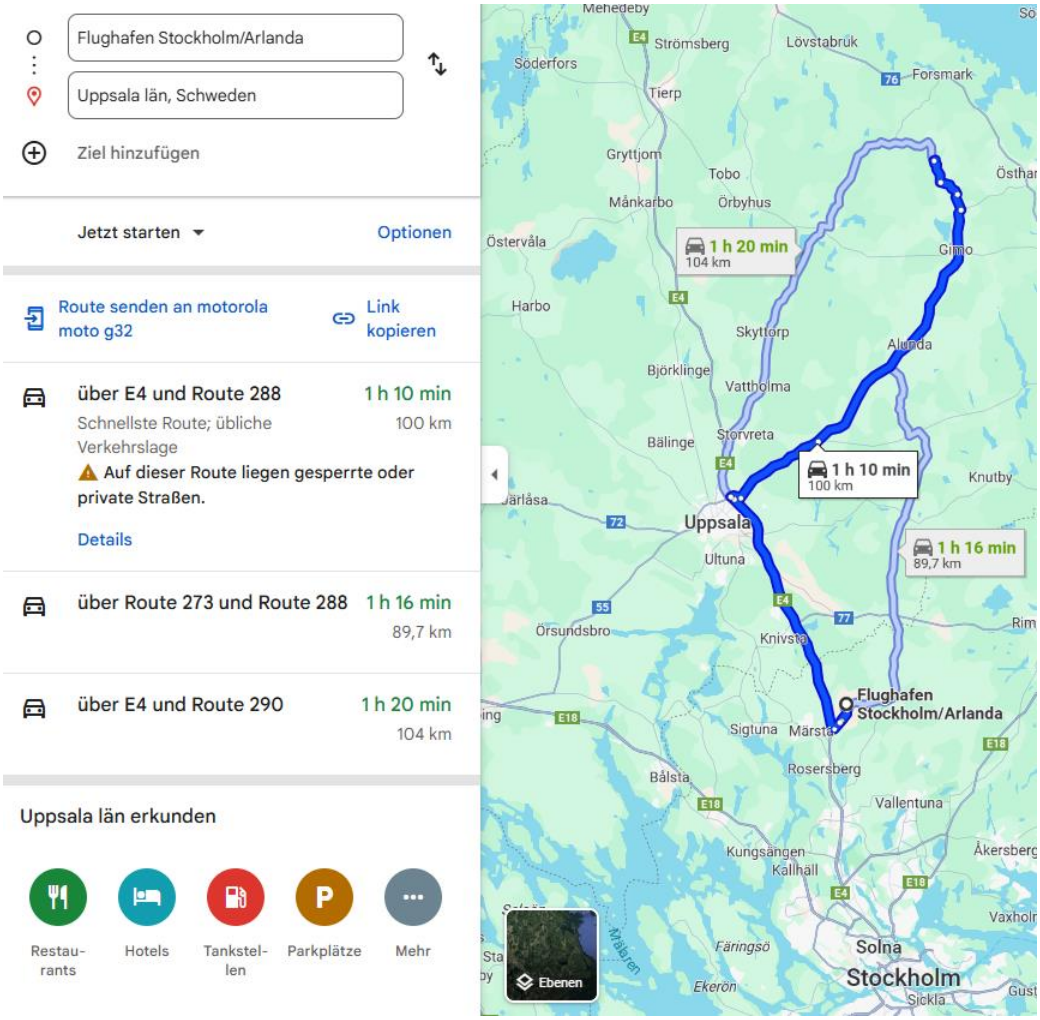

Figure A3. Arbitrary Google Maps pins and the Arlanda-Uppsala distance.

Note: This figure illustrates Ciacci's method for calculating the distance to the main airport for a region and applies it to the region of Uppsala.

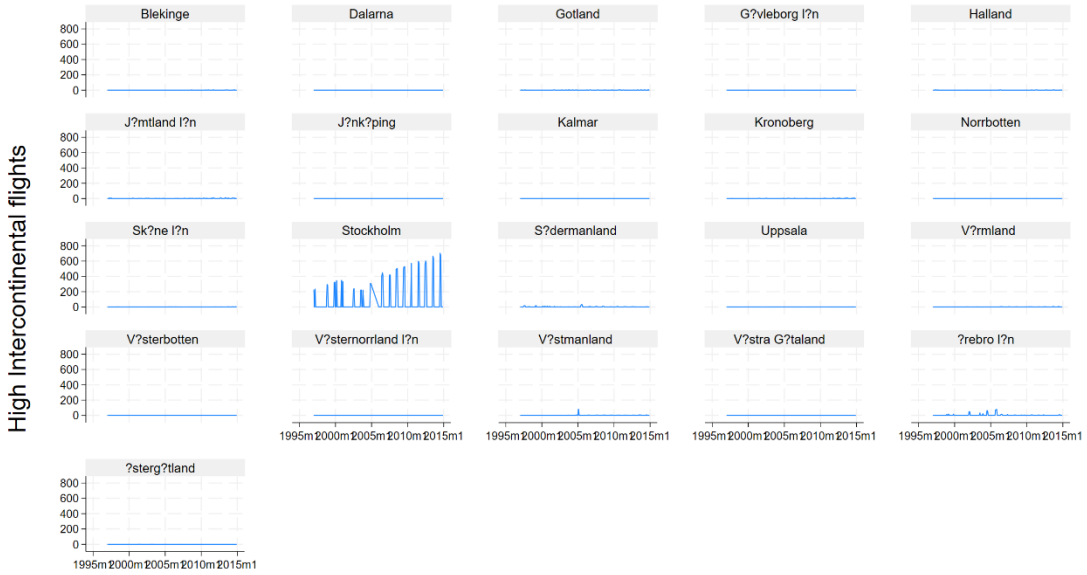

Figure A4. Variation in the instrument "high intercontinental flights" by month and year for each region.

Note: This figure shows the variation in the instrument called "high intercontintal flights" for each of Sweden's 21 regions.

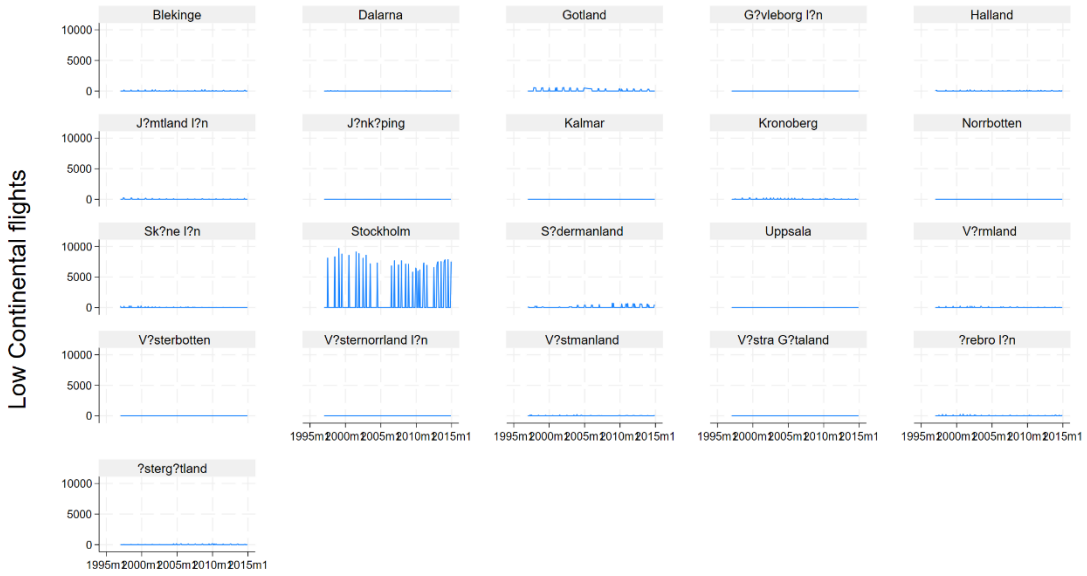

Figure A5. Variation in the instrument "low continental flights" by month and year for each region.

Note: This figure shows the variation in the instrument called "low contintal flights" for each of Sweden's 21 regions.

Table A1. Replication of Table 3 in Ciacci (2024), but with correct optimal bandwidth.

	Log(x+1)	IHS(x)
	(1)	(2)
Panel A: full sample		
First order polynomial		
Treatment	0.284	0.385
	(0.237)	(0.301)
p-value	*0.234*	*0.202*
Observations	987	987
Second order polynomial		
Treatment	0.313	0.422
	(0.245)	(0.311)
p-value	*0.202*	*0.176*
Observations	987	987
Panel B: restricted sample		
First order polynomial		
Treatment	0.600**	0.751**
	(0.265)	(0.338)
p-value	*0.025*	*0.027*
Observations	231	231
Second order polynomial		
Treatment	0.898**	1.132**
	(0.444)	(0.565)
p-value	*0.044*	*0.046*
Observations	231	231

Note: *** p<0.01, ** p<0.05, * p<0.1 Standard errors in parenthesis and p-values in italics. This figure re-estimates the first two columns of Ciacci's Table 3, but replaces the incorrect optimal bandwidths with the correct optimal bandwidths.

Table A2. Ciacci's list of main airports in Swedish regions (original Table B.1).

County	Closest main airport
Blekinge	RNB Ronneby Airport
Dalarna	MXX Mora Siljan Airport
Gotland	VBY Visby Airport
Gävleborg län	MXX Mora Siljan Airport
Halland	HAD Halmstad City Airport
Jämtland län	OSD Åre Östersund Airport
Jönköping	JKG Jönköping Airport
Kalmar	KLR Kalmar Airport
Kronoberg	VXO Växjö Airport
Norrbotten	GEV Gällivare Airport
Skåne län	KID Kristianstad Airport
Stockholm	ARN Stockholm Arlanda Airport
Södermanland	NYO Stockholm Skavsta Airport
Uppsala	ARN Stockholm Arlanda Airport
Värmland	KSD Karlstad Airport
Västerbotten	SQO Storuman Airport closed in June 2010 then HMV Hemavan Airport
Västernorrland län	KRF Höga Kusten Airport
Västmanland	VST Stockholm Västerås Airport
Västra Götaland	THN Trollhättan Vänersborg Airport
Örebro län	ORB Örebro Airport
Östergötland	LPI Linköping City Airport

Statistics/Data analysis

```
      name:  <unnamed>
       log:  C:\Users\c4041171\Dropbox\Projects_joint\Ciacci\replication\output\Ciacci
> _JOPE24_replication_full.smcl
  log type:  smcl
 opened on:  19 Apr 2025, 15:09:27
1 .
2 .
3 .
4 .     gl xvars Treat Treat_running  running  police
5 .
6 .     *original, not dropped.
7 .
8 .     reg lrape   i.regionc i.year i.month if ym>=468-29&ym<=468+28 , cl(regionc_m)

Linear regression                               Number of obs     =      1,113
                                                F(35, 251)        =     196.55
                                                Prob > F          =     0.0000
                                                R-squared         =     0.6892
                                                Root MSE          =     .53773

                                   (Std. err. adjusted for 252 clusters in regionc_m)
------------------------------------------------------------------------------
             |               Robust
       lrape |      Coef.   Std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
     regionc |
      Dalarna |   .5019831   .0909637     5.52   0.000     .3228337    .6811325
      Gotland |  -.1994007   .0812025    -2.46   0.015    -.3593258   -.0394755
  G vleborg l n |    .788728   .1465182     5.38   0.000     .5001661     1.07729
      Halland |   .3649361   .1110164     3.29   0.001     .1462937    .5835785
   J mtland l n |   .1761314   .0792043     2.22   0.027     .0201417     .332121
      J nk ping |   .3825924   .0827832     4.62   0.000     .2195542    .5456306
         Kalmar |   .3748502   .1082171     3.46   0.001     .1617208    .5879795
      Kronoberg |   .0314958    .100936     0.31   0.755    -.1672937    .2302853
      Norrbotten |   .2058376   .0980351     2.10   0.037     .0127613    .3989138
       Sk ne l n |   2.143233   .0745042    28.77   0.000       1.9965    2.289966
      Stockholm |   2.937079    .077698    37.80   0.000     2.784056    3.090102
   S dermanland |   .9527143   .0789177    12.07   0.000      .797289     1.10814
        Uppsala |   .8798043   .0862837    10.20   0.000     .7098719    1.049737
       V rmland |   .4765262   .0804099     5.93   0.000     .3181622    .6348902
   V sterbotten |   .5396367   .0973034     5.55   0.000     .3480015    .7312718
V sternorrland l n |   .3729463   .0851607     4.38   0.000     .2052256     .540667
   V stmanland |   .8319146   .0765542    10.87   0.000     .6811443     .982685
V stra G taland |   2.355823   .0686411    34.32   0.000     2.220637    2.491009
      rebro l n |   .7313701   .0976145     7.49   0.000     .5391222     .923618
    sterg tland |   1.070573   .0893565    11.98   0.000     .8945892    1.246557
                |
        year    |
        1998    |    .102467   .0480924     2.13   0.034     .0077509     .197183
        1999    |   .1504005   .0479105     3.14   0.002     .0560427    .2447583
        2000    |   .1308326   .0456751     2.86   0.005     .0408772    .2207879
        2001    |   .1206302   .0681242     1.77   0.078    -.0135378    .2547981
                |
       month    |
           2    |  -.0279794   .0626324    -0.45   0.655    -.1513314    .0953725
           3    |  -.0590064   .0710753    -0.83   0.407    -.1989863    .0809736
           4    |  -.0490892   .0662219    -0.74   0.459    -.1795105    .0813321
           5    |   .0944874   .0624215     1.51   0.131    -.0284494    .2174241
           6    |   .1508322   .0773633     1.95   0.052    -.0015317     .303196
           7    |   .1557463   .0703628     2.21   0.028     .0171695     .294323
           8    |   .1112775   .0732454     1.52   0.130    -.0329765    .2555314
           9    |   .1358699   .0846584     1.60   0.110    -.0308614    .3026011
          10    |   .0390564   .0729296     0.54   0.593    -.1045755    .1826884
          11    |   .0737118   .0587029     1.26   0.210    -.0419012    .1893248
          12    |  -.0980795   .0713492    -1.37   0.170    -.2385989      .04244
                |
       _cons    |   .5811101   .0869986     6.68   0.000     .4097698    .7524505
------------------------------------------------------------------------------
```

```
9 .
10.     *alternative commands that all drop the treatment variable:
11.
12.     reg lrape i.regionc i.year i.month $xvars if ym>=468-29&ym<=468+28 , cl(regionc_
  > m)
  note: Treat omitted because of collinearity.
  note: running omitted because of collinearity.

  Linear regression                               Number of obs     =      1,113
                                                  F(37, 251)        =     188.56
                                                  Prob > F          =     0.0000
                                                  R-squared         =     0.6893
                                                  Root MSE          =      .5382

                                       (Std. err. adjusted for 252 clusters in regionc_m)
------------------------------------------------------------------------------
             |               Robust
       lrape |  Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
     regionc |
     Dalarna |   .5128953   .1201248     4.27   0.000     .2763143    .7494764
     Gotland |  -.2069927   .0968589    -2.14   0.034    -.3977525   -.0162328
  G vleborg l n |   .7996824   .1639789     4.88   0.000     .4767325    1.122632
     Halland |   .3716924   .1207604     3.08   0.002     .1338595    .6095253
  J mtland l n |   .1738047   .0816017     2.13   0.034     .0130934     .334516
     J nk ping |   .3949132   .1168849     3.38   0.001      .164713    .6251133
        Kalmar |    .380759   .1146699     3.32   0.001     .1549213    .6065967
     Kronoberg |    .031741   .1010594     0.31   0.754    -.1672915    .2307735
    Norrbotten |   .2162319   .1177775     1.84   0.068    -.0157261      .44819
     Sk ne l n |   2.237115   .6386261     3.50   0.001     .9793668    3.494864
     Stockholm |   3.166783   1.575347     2.01   0.045     .0641984    6.269367
  S dermanland |   .9621307   .1009486     9.53   0.000     .7633165    1.160945
     Uppsala |   .8906067   .1117743     7.97   0.000     .6704717    1.110742
      V rmland |   .4867009    .104771     4.65   0.000     .2803586    .6930431
  V sterbotten |   .5458898   .1033929     5.28   0.000     .3422616    .7495179
V sternorrland l n |   .3810199   .0999025     3.81   0.000     .1842658    .5777739
   V stmanland |   .8421589   .1015331     8.29   0.000     .6421935    1.042124
V stra G taland |   2.485975   .8837109     2.81   0.005     .7455419    4.226409
     rebro l n |   .7419214   .1152524     6.44   0.000     .5149364    .9689064
   sterg tland |   1.090303   .1592193     6.85   0.000     .7767266    1.403879
               |
        year   |
        1998   |   .101905   .0484377     2.10   0.036     .0065089    .1973012
        1999   |   .1650084   .0686163     2.40   0.017     .0298713    .3001455
        2000   |   .1792718   .1734371     1.03   0.302    -.1623057    .5208492
        2001   |   .1983432   .2718262     0.73   0.466    -.3370077     .733694
               |
       month   |
           2   |  -.0262692   .0628937    -0.42   0.677    -.1501358    .0975974
           3   |  -.0555858   .0735159    -0.76   0.450    -.2003726    .0892009
           4   |  -.0439585    .068615    -0.64   0.522    -.1790929     .091176
           5   |   .1013284   .0669855     1.51   0.132    -.0305969    .2332538
           6   |   .1585283   .0819687     1.93   0.054    -.0029057    .3199624
           7   |   .1648676   .0772974     2.13   0.034     .0126334    .3171019
           8   |    .121824   .0822337     1.48   0.140     -.040132    .2837801
           9   |   .1478416   .0892957     1.66   0.099    -.0280227     .323706
          10   |   .0524534   .0844133     0.62   0.535    -.1137953    .2187021
          11   |    .088534   .0722777     1.22   0.222     -.053814     .230882
          12   |  -.081832   .0951406    -0.86   0.391    -.2692077    .1055436
               |
        Treat  |          0  (omitted)
Treat_running  |  -.0028504    .009512    -0.30   0.765    -.0215839     .015883
      running  |          0  (omitted)
       police  |   -.000052   .000353    -0.15   0.883    -.0007471    .0006432
        _cons  |   .5854136   .1346259     4.35   0.000     .3202733    .8505539
------------------------------------------------------------------------------
```

```
13.
14.     reghdfe lrape $xvars if ym>=468-29&ym<=468+28 , absorb( i.regionc i.year i.month
  > ) cl(regionc_m)
  (MWFE estimator converged in 3 iterations)
  note: Treat is probably collinear with the fixed effects (all partialled-out values ar
  > e close to zero; tol = 1.0e-09)
  note: running is probably collinear with the fixed effects (all partialled-out values
  > are close to zero; tol = 1.0e-09)

  HDFE Linear regression                            Number of obs   =      1,113
  Absorbing 3 HDFE groups                           F(   2,    251) =       0.06
  Statistics robust to heteroskedasticity           Prob > F        =     0.9398
                                                    R-squared       =     0.6893
                                                    Adj R-squared   =     0.6786
                                                    Within R-sq.    =     0.0001
  Number of clusters (regionc_m) =        252       Root MSE        =     0.5382

                               (Std. err. adjusted for 252 clusters in regionc_m)
------------------------------------------------------------------------------
             |               Robust
       lrape |  Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
       Treat |          0  (omitted)
Treat_running |  -.0028504    .009512    -0.30   0.765    -.0215839     .015883
     running |          0  (omitted)
      police |   -.000052    .000353    -0.15   0.883    -.0007471    .0006432
       _cons |    1.53803   .2723641     5.65   0.000      1.00162    2.074441
------------------------------------------------------------------------------

  Absorbed degrees of freedom:

-----------------------------------------------------+
 Absorbed FE | Categories  - Redundant  = Num. Coefs |
-------------+---------------------------------------|
     regionc |         21           0          21     |
        year |          5           1           4     |
       month |         12           1          11    ? |
-----------------------------------------------------+
  ? = number of redundant parameters may be higher
15.
16.     xi: reg lrape $xvars  i.regionc i.year i.month if ym>=468-29&ym<=468+28 , cl(reg
  > ionc_m)
  i.regionc         _Iregionc_1-21      (naturally coded; _Iregionc_1 omitted)
  i.year            _Iyear_1997-2014    (naturally coded; _Iyear_1997 omitted)
  i.month           _Imonth_1-12        (naturally coded; _Imonth_1 omitted)
  note: Treat omitted because of collinearity.
  note: _Iyear_2001 omitted because of collinearity.
  note: _Iyear_2002 omitted because of collinearity.
  note: _Iyear_2003 omitted because of collinearity.
  note: _Iyear_2004 omitted because of collinearity.
  note: _Iyear_2005 omitted because of collinearity.
  note: _Iyear_2006 omitted because of collinearity.
  note: _Iyear_2007 omitted because of collinearity.
  note: _Iyear_2008 omitted because of collinearity.
  note: _Iyear_2009 omitted because of collinearity.
  note: _Iyear_2010 omitted because of collinearity.
  note: _Iyear_2011 omitted because of collinearity.
  note: _Iyear_2012 omitted because of collinearity.
  note: _Iyear_2013 omitted because of collinearity.
  note: _Iyear_2014 omitted because of collinearity.

  Linear regression                                 Number of obs     =      1,113
                                                    F(37, 251)        =     188.56
                                                    Prob > F          =     0.0000
                                                    R-squared         =     0.6893
                                                    Root MSE          =     .5382
```

(Std. err. adjusted for **252** clusters in **regionc_m**)

lrape	Coefficient	Robust std. err.	t	P>|t|	[95% conf.	interval]
Treat	0	(omitted)				
Treat_running	-.0028504	.009512	-0.30	0.765	-.0215839	.015883
running	.0041321	.005663	0.73	0.466	-.007021	.0152853
police	-.000052	.000353	-0.15	0.883	-.0007471	.0006432
_Iregionc_2	.5128953	.1201248	4.27	0.000	.2763143	.7494764
_Iregionc_3	-.2069927	.0968589	-2.14	0.034	-.3977525	-.0162328
_Iregionc_4	.7996824	.1639789	4.88	0.000	.4767325	1.122632
_Iregionc_5	.3716924	.1207604	3.08	0.002	.1338595	.6095253
_Iregionc_6	.1738047	.0816017	2.13	0.034	.0130934	.334516
_Iregionc_7	.3949132	.1168849	3.38	0.001	.164713	.6251133
_Iregionc_8	.380759	.1146699	3.32	0.001	.1549213	.6065967
_Iregionc_9	.031741	.1010594	0.31	0.754	-.1672915	.2307735
_Iregionc_10	.2162319	.1177775	1.84	0.068	-.0157261	.44819
_Iregionc_11	2.237115	.6386261	3.50	0.001	.9793668	3.494864
_Iregionc_12	3.166783	1.575347	2.01	0.045	.0641984	6.269367
_Iregionc_13	.9621307	.1009486	9.53	0.000	.7633165	1.160945
_Iregionc_14	.8906067	.1117743	7.97	0.000	.6704717	1.110742
_Iregionc_15	.4867009	.104771	4.65	0.000	.2803586	.6930431
_Iregionc_16	.5458898	.1033929	5.28	0.000	.3422616	.7495179
_Iregionc_17	.3810199	.0999025	3.81	0.000	.1842658	.5777739
_Iregionc_18	.8421589	.1015331	8.29	0.000	.6421935	1.042124
_Iregionc_19	2.485975	.8837109	2.81	0.005	.7455419	4.226409
_Iregionc_20	.7419214	.1152524	6.44	0.000	.5149364	.9689064
_Iregionc_21	1.090303	.1592193	6.85	0.000	.7767266	1.403879
_Iyear_1998	.0523192	.0826644	0.63	0.527	-.110485	.2151235
_Iyear_1999	.0658368	.0969917	0.68	0.498	-.1251846	.2568582
_Iyear_2000	.0305144	.0660317	0.46	0.644	-.0995325	.1605612
_Iyear_2001	0	(omitted)				
_Iyear_2002	0	(omitted)				
_Iyear_2003	0	(omitted)				
_Iyear_2004	0	(omitted)				
_Iyear_2005	0	(omitted)				
_Iyear_2006	0	(omitted)				
_Iyear_2007	0	(omitted)				
_Iyear_2008	0	(omitted)				
_Iyear_2009	0	(omitted)				
_Iyear_2010	0	(omitted)				
_Iyear_2011	0	(omitted)				
_Iyear_2012	0	(omitted)				
_Iyear_2013	0	(omitted)				
_Iyear_2014	0	(omitted)				
_Imonth_2	-.0304013	.0625401	-0.49	0.627	-.1535715	.0927688
_Imonth_3	-.0638501	.0712011	-0.90	0.371	-.2040779	.0763776
_Imonth_4	-.0563549	.0672161	-0.84	0.403	-.1887343	.0760245
_Imonth_5	.0847998	.0633324	1.34	0.182	-.0399308	.2095304
_Imonth_6	.1378676	.0767457	1.80	0.074	-.0132801	.2890152
_Imonth_7	.1400747	.0701124	2.00	0.047	.0019912	.2781583
_Imonth_8	.092899	.0725388	1.28	0.201	-.0499634	.2357613
_Imonth_9	.1147845	.0846105	1.36	0.176	-.0518526	.2814215
_Imonth_10	.0152641	.0728012	0.21	0.834	-.1281149	.158643
_Imonth_11	.0472125	.0587203	0.80	0.422	-.0684348	.1628598
_Imonth_12	-.1272857	.0711463	-1.79	0.075	-.2674056	.0128342
_cons	.6845852	.1462179	4.68	0.000	.3966149	.9725555

17.

```
18.     *note: areg only allows for 1 absorb() variable - I put year there and month- an
  > d region- as normal vars
19.
20.     areg lrape $xvars i.month i.regionc if ym>=468-29&ym<=468+28 , absorb( year) cl(
  > regionc_m)
  note: Treat omitted because of collinearity
  note: 12.month omitted because of collinearity

  Linear regression, absorbing indicators                Number of obs     =  1,113
  Absorbed variable: year                                No. of categories =      5
                                                         F(33, 251)        = 205.36
                                                         Prob > F          = 0.0000
                                                         R-squared         = 0.6893
                                                         Adj R-squared     = 0.6786
                                                         Root MSE          = 0.5382

                                        (Std. err. adjusted for 252 clusters in regionc_m)
------------------------------------------------------------------------------
             |               Robust
       lrape |  Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
       Treat |          0  (omitted)
Treat_running |  -.0028504   .009512    -0.30   0.765    -.0215839     .015883
     running |  -.0074393   .0086491   -0.86   0.391    -.0244734    .0095949
      police |   -.000052   .000353    -0.15   0.883    -.0007471    .0006432
             |
       month |
           2 |  -.0188299   .0596914   -0.32   0.753    -.1363896    .0987299
           3 |  -.0407073   .0668919   -0.61   0.543    -.1724483    .0910337
           4 |  -.0216406   .0604618   -0.36   0.721    -.1407178    .0974365
           5 |   .1310855   .0553044    2.37   0.019     .0221656    .2400054
           6 |   .1957247   .0709216    2.76   0.006     .0560475     .335402
           7 |   .2095033   .0636766    3.29   0.001     .0840947    .3349119
           8 |    .173899   .0679378    2.56   0.011     .0400982    .3076997
           9 |   .2073559   .0814703    2.55   0.012     .0469034    .3678083
          10 |   .1194069   .0714907    1.67   0.096    -.0213912     .260205
          11 |   .1629268   .0602789    2.70   0.007     .0442099    .2816437
          12 |          0  (omitted)
             |
     regionc |
     Dalarna |   .5128953   .1201248    4.27   0.000     .2763143    .7494764
     Gotland |  -.2069927   .0968589   -2.14   0.034    -.3977525   -.0162328
G vleborg l n |   .7996824   .1639789    4.88   0.000     .4767325    1.122632
     Halland |   .3716924   .1207604    3.08   0.002     .1338595    .6095253
J mtland l n |   .1738047   .0816017    2.13   0.034     .0130934     .334516
   J nk ping |   .3949132   .1168849    3.38   0.001      .164713    .6251133
      Kalmar |    .380759   .1146699    3.32   0.001     .1549213    .6065967
   Kronoberg |    .031741   .1010594    0.31   0.754    -.1672915    .2307735
   Norrbotten |   .2162319   .1177775    1.84   0.068    -.0157261      .44819
    Sk ne l n |   2.237115   .6386261    3.50   0.001     .9793668    3.494864
    Stockholm |   3.166783   1.575347    2.01   0.045     .0641984    6.269367
S dermanland |   .9621307   .1009486    9.53   0.000     .7633165    1.160945
      Uppsala |   .8906067   .1117743    7.97   0.000     .6704717    1.110742
    V rmland |   .4867009    .104771    4.65   0.000     .2803586    .6930431
 V sterbotten |   .5458898   .1033929    5.28   0.000     .3422616    .7495179
V sternorrland l n |   .3810199   .0999025    3.81   0.000     .1842658    .5777739
   V stmanland |   .8421589   .1015331    8.29   0.000     .6421935    1.042124
V stra G taland |   2.485975   .8837109    2.81   0.005     .7455419    4.226409
      rebro l n |   .7419214   .1152524    6.44   0.000     .5149364    .9689064
   sterg tland |   1.090303   .1592193    6.85   0.000     .7767266    1.403879
             |
         _cons |   .6815672    .120008    5.68   0.000     .4452161    .9179182
------------------------------------------------------------------------------
21.
22.     log close
       name:  <unnamed>
        log:  C:\Users\c4041171\Dropbox\Projects_joint\Ciacci\replication\output\Ciacci
  > _JOPE24_replication_full.smcl
   log type:  smcl
  closed on:  19 Apr 2025, 15:09:28
```