

First email:

These authors claim to have identified “a causal effect of the liberalization and prohibition of commercial sex on rape rates” (pp 753) using a difference-in-differences approach, with the criminalization of the purchase of sex directly causing an increase in (forcible) rape in the general public and decriminalization causing a decrease in rape in the general public.

I was able to download the supplementary data, which is kindly available on the journal website, and it seems that there are some discrepancies in the official statistics from [Eurostat](#) (where they claim they obtain the data) and from the rape rates in their dataset. Furthermore, it appears that some of the outcome data may have been imputed by some unknown method, which was not noted in the article.

Obviously, I am not arguing that it is wrong to impute data (under the right circumstances), but what is concerning is that although they use a difference-in-differences model to attribute a causal effect of prostitution regulation on rape rates, the years on and around which the purchase of sex was criminalized in Sweden (using the Nordic Model — a “feminist” prostitution regulatory approach in which sex purchasing is banned and selling is legalized) and the years on and around which that the purchase of sex was legalized in Spain appear to be imputed.

It appears that the data for Sweden in 1998, 1999 and 2000 were missing from the United Nations Surveys of Crime Trends (UN-CTS) on the UNODC website (where they claimed to have gotten historical data). Sweden implemented the Nordic Model in 1999. Although the UN-CTS or Eurostat did not have this data, the official Swedish open data website did have it. (

<https://bra.se/bra-in-english/home/crime-and-statistics/crime-statistics.html#Reportedoffences>). Comparing the data of Gao & Petrova in the year 1999 to the official Swedish statistics, the Gao & Petrova rape data appears to be lower in the years preceding the ban of the purchase of sex (which was implemented in 1999) and higher the year the law was implemented. In the years that were missing (1998, 1999, and 2000) in the Gao and Petrova dataset were “filled-in” with 14 per 100,000, 14 per 100,000, and 25 per 100,000. The official Swedish statistics shows 22 per 100,000, 24 per 100,000, and 23 per 100,000 those same years. The Gao & Petrova dataset made it appear as though Sweden had a 78.6% increase in documented forcible rape the year of the ban of the purchase of sex, which was a sustained effect, while in reality there was only a small increase which immediately dropped the next year. The rape rate in Sweden in fact didn’t increase over 78.6% until 2005 — 6 years after the ban on the purchase of sex — which coincided with an expansion to the legal definition of rape.

Legislative changes which expanded rape definitions occurred often on the same year as regulatory changes of prostitution, or within several years. In multiple reports, we see that these expansions of the definition of rape in Sweden has been strongly associated with an increase in documented rape offenses. The expansion in 1992-1993 (to redefine many

previously considered “sexual assaults” as rape) was reportedly associated with a 25% increase in documented offenses (Von Hofer, H., 2010). The expansion of the legal definition of rape to mean “sex without consent” in 2018 was associated with a 75% increase in documented rape offenses, which The National Council on Crime Prevention (Bra) claims was directly responsible for this increase.

We can see for Spain however, a country which initiated prostitution legalization 1995, the data on the [UNODC \(United Nation office of Drugs and Crimes\) website](#) the rape rate on the years surrounding the legalization of prostitution per 100,000 are 3.08 in 1993, 3.09 in 1994, missing in years 1995 - 1997, 15.07 in 1998 and 14.57 in 1999, 13.99 in 2000 and then missing the next two years. In the Gao and Petrova dataset that was available on the journal website, the column “raperate” is missing these years and the outcome variable, which in their files they claim is called “raperape_2” contains the value 3.8 rather than 14.5 and 4.3 rather than 13.99 that is on the UNODC website. I’m just curious where exactly this data came from and if they imputed it, which method did they use that gave them such small numbers for Spain — a prostitution legalization country — and such high numbers for Sweden, a prostitution criminalization country? Furthermore, if the rate of rape is available in the official United Nations statistics, why would they be omitting it and later imputing it with lower numbers in the legalized group and imputing it with higher numbers in the criminalized group — then claiming that they passively identified a causal relationship between prostitution criminalization and rape?

In addition to data being different in some locations from official sources, it was sometimes completely missing — when the data was in fact available online in the data sources where the researchers claimed to obtain it. For instance, the United Kingdom (a control country) rape rate outcome data is completely omitted from the years 2000, 2002 and 2009 - 2018, which ends up being almost 50% of rape data in the United Kingdom, which was not addressed in the journal. From 2009 to 2017 the rate of rape in the United Kingdom changes from 27.39 to 92.29 (from Eurostat, where they claimed to obtain more recent crime data). In 2018 it increased to 99.48 per 100,000. Germany (a legalized country) in the years 2016 and 2017 are completely omitted. The rate of rape there increases dramatically beginning those years. In their article, the graphs of aggregated treatment/control groups which show no increase in the control and decriminalized groups appear to be created without these observations. Although the paper was published in 2022 and data is readily available on Eurostat for all years 2008-2020, the rape rate in the Gao & Petrova dataset as well as the crime data that they used to insinuate that rape increased while other crime has not increased, only has valid data until 2017 for rape and homicide and 2015 for other crime. Crime data is available on Eurostat until 2020. Data irregularities were also in the homicide data, which diverged significantly from that of the Eurostat database in the Netherlands (2016 and 2017). It obviously wasn't possible for me to go to every single data source so I'm not sure about any of the other variables.

On page 771 they claim to test for the parallel trends assumption by performing an analysis for “pretreatment trends and reversals.” This analysis includes only 2 treatment

years prior to intervention and two years post intervention. (These results are on page 772, Table 6)

From Gao and Petrova:

“...Five dummy variables designate each year relative to the enactment of the prostitution law. In column 1 of Table 6, we reestimate column 3 of Table 5 by replacing Prostitution Liberalization with these five indicator variables. The coefficients on the Year -2 and Year -1 indicators are especially important because their significance and magnitude indicate whether there is any difference between the treatment and the control groups prior to the policy change. The coefficients are close to 0 and not statistically significant, which suggests that the parallel-trends assumption is not violated. Moreover, the impact of prostitution liberalization shows up after the law’s enactment: the coefficient on Year 2+ is significantly negative. In column 2 of Table 6, we focus on the prohibition of commercial sex and reestimate column 4 of Table 5 by replacing Prostitution Prohibition with the five indicator variables. The treated and control groups have similar trends prior to the policy change: The coefficients of Year -2 and Year -1 are not significantly different from 0. The positive effect of prostitution prohibition on a country’s rape rate shows up after the policy change: the coefficients on Year +1 and Year 2+ are significantly positive. Overall, Table 6 confirms that the treated and control groups have a similar trend in rape rates prior to the changes in law, which supports the parallel-trends assumption. Moreover, Table 6 indicates that most of the impact of prostitution laws on rape rates occurs after the laws are enacted, which suggests a causal effect.” -pg. 771

Why have researchers, who have in some cases over 20 years of pretreatment data, only tested 2 observation periods of that pretreatment data and 2 observation periods of post treatment data? I noticed that there were indeed more than just Year 1+ , Year 2+... but relative year variables currently in the dataset that extended 10 years in either direction. When I added all of them in the model I was surprised to see that at year 3+, the coefficients drop significantly in the criminalization model..... while in the legalization group at year 3+ the coefficients increase. With the inclusion of all the relative year variables they provided in the dataset (Year_plus_1 ... Year_plus_10), the coefficients for the criminalized model would be 19, -12, -8.3, -13 for year two+, year 3+ year 4+ and year 5+ respectively. The opposite is seen in the decriminalized model, the coefficients are -2.3, 0.62, 0.30, 0.10 for the years two+, year 3+ year 4+ and year 5+ respectively. Failing to report beyond year 2+ clearly has a critical impact on the interpretation of the models.

Upon checking their coding schemes, it appears that the variable “*year_two_plus*” included in this model did not have the same coding scheme as the rest of their variables. The other variables had a dummy-coded scheme that were coded “1” to indicate the presence of the relative year in question, and 0 on all years otherwise. They however used the variable “*year_two_plus*” (which we can see from the file “JLE_online_appendix.do” which is provided in the supplementary data files), which was

coded 1 on the relative year and 1 all years afterwards. When we replace the inconsistently coded variable with the correctly coded variable (called "year_plus_2") we see that again the results are completely different. Rather than the year 1+ and year 2+ coefficients being 4.6 and 14, they are -1.9 and -1.9 in the criminalization. Rather than the coefficients being -0.79 and -2.9 for time 2 and 3 in the legalization group, they are .80 and .34.

We can see that their conclusions that:

"The positive effect of prostitution prohibition on a country's rape rate shows up after the policy change: the coefficients on Year +1 and Year 2+ are significantly positive....Moreover, the impact of prostitution liberalization shows up after the law's enactment: the coefficient on Year 2+ is significantly negative..." Pp. 771

Are probably not accurate.

They included Ireland and France as Nordic Model countries throughout the entire analysis despite the fact that they only implemented their ban on purchasing sex in 2016 and 2017 respectively and the fact that Ireland dramatically expanded the legal definition of rape that same year. While they briefly mention that their analysis ends in 2017, they don't explain why. This means there were no post observational periods for Ireland at all even though the claims that prostitution criminalization "causes" rape has been generalized to these countries, which were not in the analysis after the implementation of the ban.

The observed data clearly shows all countries other than those in Eastern European some in Southern European increasing dramatically in the documented rape rate. The countries with large commercial sex industries (Germany, Netherlands) have been aggregated with a large number of Southern and Eastern European countries and placed into the "decriminalized" group while the Nordic Model group consists only of Northern European and Western European countries. The criminalized group included the Nordic Model countries plus Croatia. The observed data demonstrates rape had been increasing every year *prior* to the intervention. By aggregating the Eastern and Southern European countries with other countries, it dramatically changed the trend of the data and was quite misleading. From self-reported survey data in the EU, (a survey which the authors know about as they themselves briefly cite it) the self-reported rate of rape in Sweden, the Netherlands, Hungary, Bulgaria, etc., are exactly the same, despite the fact that Sweden has higher documented rape offenses. I assume that this is due to the disparities in rape disclosures due to gender equality and social norms and the expansive rape legislation in countries which have banned the purchase of sex, as this desire for gender equality is the reason they banned the purchase of sex in the first place.

In summary, I am wondering:

- 1) Where exactly did they get this data, and why are there discrepancies from Eurostat and other sources?
- 2) Was there any imputation performed, if so, what method and why?
- 3) Why were some country years (in the UK and Germany) in which there were dramatic increases in the documented rapes in control and legalization countries omitted from the analysis?
- 4) Why did they include Ireland and France as Nordic Model countries and fail to clearly note that Ireland was not in the analysis for post observational periods (and France for only 1 observation).
- 5) Why did the analysis abruptly end in 2017, when there was a dramatic increase in rape across Europe (especially in the legalization countries).
- 6) Why did they omit the fact that disaggregated, all countries increase in rape rate regardless of prostitution regulation status other than Eastern and Southern European countries, despite the fact that most countries they had the same self-reported rape rate as in Sweden?

Second email:

Thank you for your response.

I have just informed you, the editor, that it appears that the authors made an error in at least one of their models that resulted in a substantive difference in the conclusions of the article you edited (page 772, table 6). It appears that they used a variable which was not correctly coded ("year_two_plus") and then attributed a causal relationship to a policy based on this one model alone. When you replace the inconsistently coded variable with the consistently coded variable then it changes both the direction and magnitude of the coefficients for many of the relative year dummies in the liberalization and prohibition models. The claims they made on page 771 may not be true:

"The positive effect of prostitution prohibition on a country's rape rate shows up after the policy change: the coefficients on Year +1 and Year 2+ are significantly positive... Table 6 confirms that the treated and control groups have a similar trend in rape rates prior to the changes in law, which supports the parallel-trends assumption. Moreover, Table 6 indicates that most of the impact of prostitution laws on rape rates occurs after the laws are enacted, which suggests a causal effect."

I have already sent you screenshots of the discrepancy of their data and the official data, but I will now send you a screenshot of the inconsistency in their coding scheme that has led to this possible error in their model. **I am requesting you investigate if these models are correct and if so, at very least issue a correction.** According your COPE publication ethics guidelines, the editor is responsible for investigating such issues when complaints are raised, and if required issue a correction or retraction:

<https://publicationethics.org/sites/default/files/respond-whistleblowers-concerns-cope-flowchart.pdf>

I also reported to you that there were possible discrepancies in the data that the authors used (the data is published on your website) and the data in the public databases where they claimed to obtain this data. As you know, the editor is responsible for everything he prints. If you are unwilling to investigate these issues or unwilling to issue a correction or clarification then please explain your critique process to me and I would be thrilled to publish this in a critique.