Reply to the anonymous reader's comments

"Do Prostitution Laws Turn a John into a Rapist? Evidence from Europe"

By Huasheng Gao and Vanya Petrova June 18, 2023

We thank the anonymous reader for the very careful reading of our paper, which flatters us. In the point-by-point responses below, comments from the reader appear in italics immediately preceding our responses.

Reader's Comment 1.

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 its seems that there are some discrepancies in the official statistics from Eurostat (where they claim the 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 is 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 immedietly 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 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 the 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.*

Response (R1): First, in our data collection process, we have tried to follow a systematic approach to gather comprehensive information on rape rates: Our initial step involved collecting the number of police-recorded rape cases from Eurostat, the EU's statistical office, which is a reliable and authoritative source of statistical data in Europe. To address the issue of missing county-year observations from Eurostat, we have employed a multipronged strategy: We first turned to the UN Office on Drugs and Crime (UNODC), which compiles global crime statistics, including data on sexual offenses. Further, we have incorporated national statistics provided by individual countries (in some cases, we have translated the relevant information from the official country languages). We strived to access and include relevant national data, which enabled us to capture a more comprehensive and nuanced picture of the prevalence and incidence of rape across different jurisdictions. In some cases, when data on rape per 100,000 population was not available, we have calculated it based on the number of reported rape incidents and the country's population. Generally speaking, the variable *raperate* was compiled from the data available from Eurostat and UNODC, while the variable *raperate_2* is compiled from these sources and all additional data (available at this point in time). The whole process is stated on p. 763 of the paper: "*We collect the number of police-recorded rape offences from Eurostat. … In cases where rape rates are missing in Eurostat, we collect such information from the UN Office on Drugs and Crime (UNODC) and national statistics.*"

Second, it is not practical for us (or for any authors in an academic paper) to elaborate on all these technical details in the paper because (1) It is common practice to prioritize the core findings and implications in an academic paper, providing readers with a concise and focused representation of the study, and (2) we have always faced space constraints in the journal.

Third, the data we have used in the paper were the most up-to-date data available at the time we started the empirical work in 2018 (see details in our R3 below). Eurostat is constantly revising its data. It is possible that the data contained in its current version are different from the historical version (see the data manual https://ec.europa.eu/eurostat/documents/64346/2989606/Methodological+guide+for+users/bfd3bb4a-67b7-44de-860e-cb911df9e17a and related screenshot below). A similar practice is likely implemented by other data sources.

Coordination was set-up with the UN Office on Drug and Crime (UNDOC) to collect data through the United Nations Surveys on Crime Trends and the Operations of Criminal Justice Systems (UN-CTS). The year 2014 is an important milestone for this cooperation with the organisation of the first joint Eurostat/UNODC statistical data collection on crime and criminal justice from EU Member States, EFTA countries, Candidate Countries and potential Candidate Countries. In addition to the information required by UNDOC, some data are also collected for specific areas of interest to the European Commission (see Chapter 1.2.1 Administrative sources of crime data).

The UN-CTS collection and consequently also the Eurostat data collection are updated and revised constantly, ensuring consistency of the data over time. Future revisions will bring some changes in the definition and inclusion of offences in the questionnaire due to the alignment of the data collections with the International Classification of Crime for Statistical Purposes (see Chapter 1.3 International Classification of Crime for Statistical Purposes (ICCS)).

Fourth, we re-estimate our baseline regression by removing the four countries mentioned above (i.e., Sweden, Spain, UK, and Germany). As shown in Table R1 below, our main inference is largely unchanged.

Table R1: Effects of Prostitution Laws on the Rape Rate

This table re-estimates our baseline regression (Table 5 in Gao and Petrova 2022*)* by excluding Sweden, Spain, UK, and Germany.

Dependent=raperate_2	(1)	(2)	(3)	(4)
Legal Prostitution	-5.323**	-4.829***		
	[0.020]	[0.002]		
Prostitution Liberalization			-2.225*	
			[0.099]	
Prostitution Prohibition				8.505**
				[0.021]
Ln(GDP Per Capita)		-4.311***	-4.117***	-5.383***
		[0.002]	[0.006]	[0.000]
Ln(Population)		0.732	-1.372	3.596
		[0.815]	[0.708]	[0.371]
Unemployment Rate		-0.168**	-0.125	-0.200**
		[0.043]	[0.136]	[0.033]
Women per 100 Men		-0.344	-0.369	-0.427
		[0.271]	[0.318]	[0.370]
Police Officers		-5.185	-4.652	-1.404
		[0.270]	[0.245]	[0.740]
Immigrants		0.127	-0.011	0.105
		[0.467]	[0.958]	[0.553]

Gender Inequality Index		11.346	9.325	4.322
		[0.349]	[0.562]	[0.769]
Constant	12.684***	77.576	109.388	47.381
	[0.000]	[0.303]	[0.182]	[0.547]
N	745	745	607	577
Adjusted R^2	0.782	0.818	0.794	0.846
Mean of dependent variable	8.19	8.19	6.75	8.62

Fifth, the authors mention some changes in the definition of "rape" in some countries. Since we have a long period in the sample (1990-2017), it is inevitable to have some confounding events. As we stated on p. 754 of the paper, one advantage of our setting is: "*Because multiple shocks affect different countries at different time, we can avoid a common identification challenge faced by studies with a single shock: the potential noise coinciding with the shock that directly affects rape rates*." Further, as we explain our baseline regression on p. 768, the year fixed effects enable us to control for intertemporal trends of sex crimes. The country fixed effects allow us to control for time-invariant differences in sexual crimes across countries. These fixed effects in the regression can largely control for any trend in legal definitions of rapes.

Reader's Comment 2

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 re-estimate 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.

Response (R2): Many confounding events can happen if it is many years before or after the policy change. Our way of testing parallel trends assumption is common in the literature.

Example 1: Below is one of our other papers published in *Review of Accounting Studies* (https://link.springer.com/article/10.1007/s11142-018-9475-x). It uses exactly the same 5 dummies to capture the pre-trend for the sample period of 1987 to 2011.

Table 3 Testing H1

	(1) *DA*	(2) *DA*	(3) *DA*
IDD	−0.008***	−0.009***	
	(−2.907)	(−5.093)	
IDD^{-2}			0.001
			(0.346)
IDD^{-1}			0.007
			(1.290)
IDD^{0}			−0.005
			(−1.293)
IDD^{1}			−0.011*
			(−1.849)
IDD^{2+}			−0.009**
			(−2.591)
Ln (total assets)		−0.021***	−0.021***
		(−8.299)	(−8.111)

Example 2: Below is a classic paper published in *Journal of Political Economy* and cited in our paper (https://www.journals.uchicago.edu/doi/abs/10.1086/376950). It uses almost the same setting (4 dummies) to capture the pre-trend for the sample period of 1975-1995.

TABLE 6
EFFECTS OF BUSINESS COMBINATION LAWS ON PLANT BIRTHS (N=225,231)

	DEPENDENT VARIABLE: BIRTH DUMMY				
	Linear Probability Model				Probit Probability Model
	(1)	(2)	(3)	(4)	(5)
BC	−.019	−.019	−.020	...	−.008
	(.004)	(.004)	(.004)		(.002)
State-year	...	.391	.360	.390	.156
		(.063)	(.059)	(.063)	(.023)
Before^{-1}	...	...	...	.017	...
				(.004)	
Before^{0}	...	...	...	−.014	...
				(.004)	
After^{1}	...	...	...	−.014	...
				(.007)	
After^{2+}	...	...	...	−.015	...
				(.004)	
Firm fixed effects?	yes	yes	yes	yes	yes
Year fixed effects?	yes	yes	yes	yes	yes
Log(base year firm					

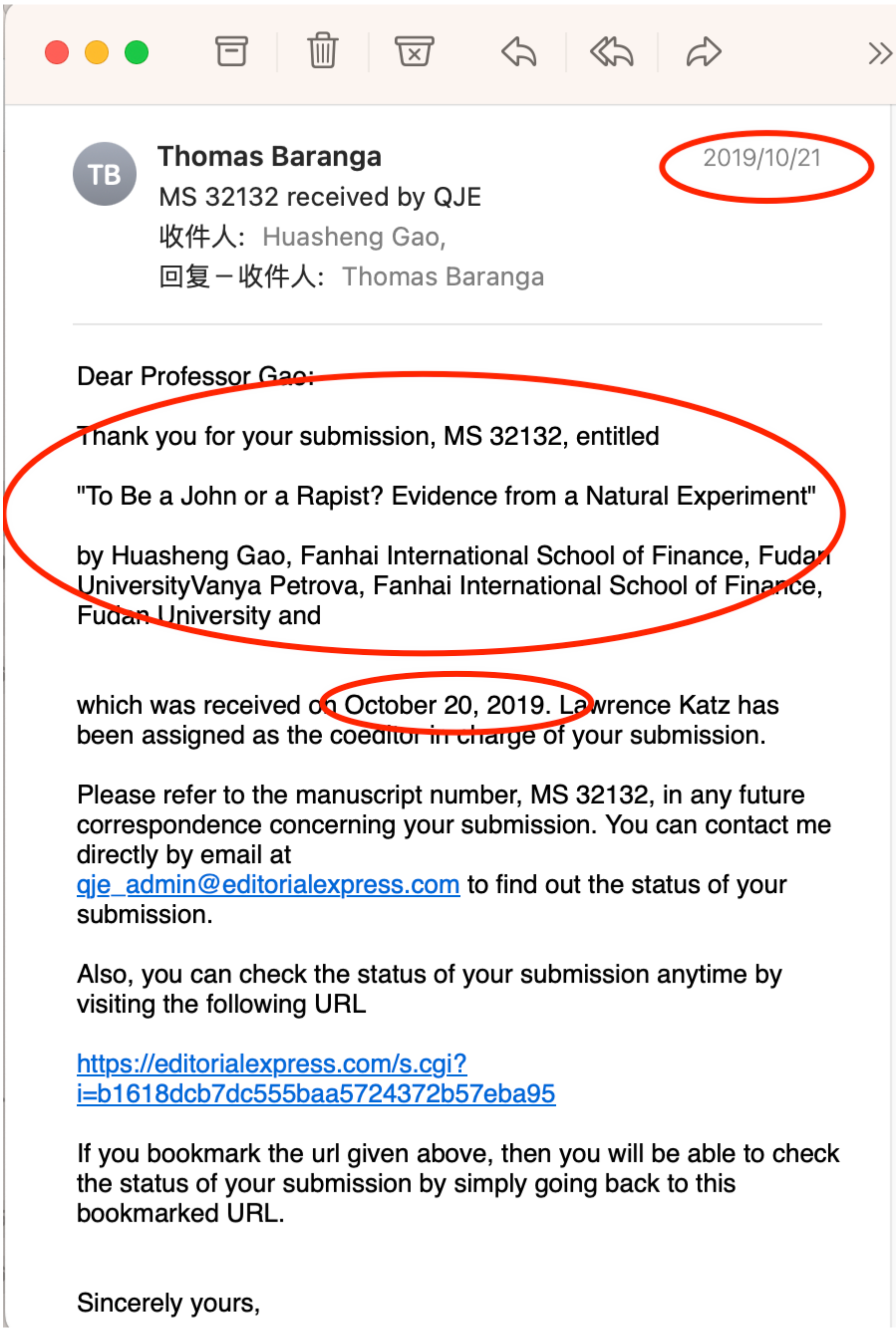
Reader's Comment 3

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.

Response (R3)*:* First, we tried to include as many countries and as many years as possible. Some countries changed the law in earlier years (so that we have fewer pre observations but more post observations) and some did so in later years (so that we have fewer post observations but more pre observations). This is simply the fact.

Second, the reader points out some changes on rape definition in Ireland in 2017. Similar points have been made in comment 1. We have already made the response in our R1.

Third, we end our sample in 2017 because we started to compile data and do the empirical work in 2018. When we started the project in 2018, the best available data was by 2017. We first submitted the completed draft in 2019; after rejections by several journals and a few rounds of revisions, this paper was eventually accepted for publication in JLE in 2022. Below is the email exchange when we first submitted the early version of our paper to *Quarterly Journal of Economics* (QJE) in Oct. 2019.



Thomas Baranga 2019/10/21

MS 32132 received by QJE

收件人： Huasheng Gao,

回复－收件人： Thomas Baranga

Dear Professor Gao:

Thank you for your submission, MS 32132, entitled

"To Be a John or a Rapist? Evidence from a Natural Experiment"

by Huasheng Gao, Fanhai International School of Finance, Fudan UniversityVanya Petrova, Fanhai International School of Finance, Fudan University and

which was received on October 20, 2019. Lawrence Katz has been assigned as the coeditor in charge of your submission.

Please refer to the manuscript number, MS 32132, in any future correspondence concerning your submission. You can contact me directly by email at qje_admin@editorialexpress.com to find out the status of your submission.

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Sincerely yours,

Reader's Comment 4

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.*

Response (R4)*:* As we explained above, we tried to include as many countries as possible in the sample (if we had not, other readers could have also "wondered" why we only picked a particular part of Europe).

The reader is correct that rape rate is influenced by many other factors, such as gender equality and rape disclosure. As a matter of fact, Section 5.4.1 of our paper formally examines the possibility of the rape underreporting problem and shows that such an underreporting problem leads to an underestimate of the effect of prostitution laws (i.e., our results should be stronger if all rapes were reported). Section 5.4.1 also examines the role of gender equality and shows that our results are stronger if gender equality is greater.

5.4. Heterogeneous Treatment Effects

5.4.1. Underreporting Rapes

It is widely documented that reported rapes are likely to be an underestimate of the number of offenses (Cunningham and Shah 2018). Various barriers stop victims from disclosing sex crimes: shame, denial, depression, fear of retaliation,

Reader's Summary

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?

Response to (1)-(3): As detailed in R1, we strived to cover as many countries and as many years as possible by starting with Eurostat first. For any missing value in Eurostat, we collected data from UNODC and various national statistics.

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).

Response to (4): Ireland and France are included simply because they indeed adopted the Nordic model in our sample period 1990-2017. (If we did not include Ireland and France, other readers would probably ask, "Why did not they include Ireland and France?") The year of adopting for each country is clearly presented in Table 1 of the paper.

5) Why did the analysis abruptly end in 2017, when there was a dramatic increase in rape across Europe (especially in the legalization countries).

Response to (5): As detailed in R3, we started our project in 2018 when the best available data was by 2017.

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?

Response to (6): As detailed in R4, we tried to include as many countries as possible in the sample, regardless of whether they are Eastern or Southern European countries. If we had not done so, other readers could have also "wondered" why we only picked a particular part of Europe.

Our Final Words to the Anonymous Reader

The reader seems particularly interested in the Nordic model. It is not surprising to see prostitution prohibition increase sexual violence, as similar conclusions have also been drawn by several studies in different settings, including New York City, California, Netherlands, Rhode Island, Sweden, and Northern Ireland (see p. 755 of the paper for the review).

We understand that the Nordic model is an important legal change globally advanced by many interested groups. But for a policy change as important as the Nordic model, its effect on society should not be simply black and white, but multifaceted: It may have positive effects on aspects A, B, and C and negative effects on aspects X, Y, and Z. For example, one of our recent new works (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4411207) shows that the Nordic model also has important positive effects of increasing marriage and decreasing divorce rates. **A reasonable way to make the Nordic model work better is trying to understand its various impacts and to minimize its negative effect and magnify its positive effect.** Although our paper shows one negative aspect of the Nordic model, it does not claim anything on its overall effect.