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Abstract

We evaluate the effect of the 2015 criminalization of the purchasing of sexual services in Northern Ireland on the market of such services, sexually transmitted infections and sexual violence. This sub-national change in sex market regulation gives us an opportunity to estimate the causal effects of this new law which has become increasingly popular with policymakers over the last 20 years. Based on newly constructed data sets, our results indicate that the law reduced, temporarily, the size of the market for sexual services and fees for sexual services, lowered sexually transmitted infection rates among women and increased sexual violence committed against women. These results are largely consistent with recent evidence on the effects of different kinds of sex market regulation.

1 Introduction

We study the 2015 adoption of the so-called 'Sex Buyer Law' (SBL) which criminalized the purchasing, but not the selling, of sexual services in Northern Ireland.[1] Prior to 2015, the general regulatory approach to the sex market was the same throughout the United Kingdom (UK)[2] and the Republic of Ireland, i.e. buying and selling sexual services were legal though solicitation (i.e. street sex work) and third-party activities (e.g. 'pimping,' brothel keeping) were not. On 1 June 2015, a law criminalizing the purchasing of sex came into effect in Northern Ireland. We use this rare sub-national change in sex work regulation to estimate the effect of criminalizing demand for sexual services on the market for sexual services, the rate of sexually transmitted infections and sexual violence.

Two recent papers study the effects of changing sex market regulation. Cunningham and Shah (2018) study the effects of the decriminalization, and subsequent recriminalization, of the sex market in Rhode Island, USA. Cameron, Seager and Shah (2020) study the criminalization of the sex market in East Java, Indonesia. Such changes in sex market regulation are rare. Our contribution to this literature is to study the effects of a different regulatory approach, one that has been introduced in eight countries in Europe and North America since 1999 and has been debated in many others: the criminalization of the demand, but not the supply, side of the sex market.

An important gap in the literature on the SBL is the general absence of credible identification. Of the papers and reports looking at the effects of the SBL on material outcomes, only Ciacci (2020) and Berlin et al. (2020) use methods designed to allow for causal inference. In the absence of a convincing counterfactual, the existing evidence is descriptive and important but reveals little about changes to the market for sexual services, sexual health, or sexual violence caused by the SBL. A difficulty in the study of sex market regulation is that such policy is generally determined at the national level. As such, most attempts to evaluate the impact of changes to the regulations around sex work tend to consider changes within a country following the introduction of the new legislation. Credible control groups are rare and so evidence of the causal impact of changes in the regulation have been rare. We aim to redress this gap in the

[1] This approach is also referred to as the 'Nordic Model' or 'Swedish Model' as Sweden was the first to adopt it followed by other Nordic countries.

[2] The nomenclature of the British Isles can be confusing. *Great Britain* is the island on which Scotland, Wales and England sit. *The United Kingdom of Great Britain and Northern Ireland*, is the country commonly referred to as Britain. Ireland refers to the island on which the Republic of Ireland and Northern Ireland sit.

literature.

Using newly constructed data sets we provide evidence that the adoption of the SBL in Northern Ireland led to:

- a large, though temporary, decrease in the number of reported transactions.
- a temporary decrease in fees.
- a decrease in the number of gonorrhea and chlamydia infections among women.
- an increase in sexual assaults committed against women.

There is significant debate in many countries around the regulatory approach applied to buying and selling sexual services, i.e. prostitution.[3,4] States employ a wide range of 'prostitution regimes,' the laws and enforcement practices pertaining to the buying and selling of sexual services.

In recent decades, debate has increasingly focused on the criminalization of demand, i.e. the SBL (Harcourt and Donovan 2005, Ward and Wylie 2017*b*). This approach is of particular interest as it has been introduced in a number of countries over the last 20 years with a several others currently considering it (Ward and Wylie 2017*a*; The Economist 2019). The SBL has been adopted in Sweden (1999), Norway (2009), Iceland (2009), Canada (2014), Northern Ireland (2015), France (2016), Republic of Ireland (2017), and Israel (2020). In each case, both buying and selling sex had been legal prior to the introduction of the SBL. In 2014 the European Parliament's Women's Rights and Gender Equality Committee published a report recommending the implementation of the SBL across the European Union.[5] In the Spring of 2019 the adoption of the law was debated in the Dutch parliament (Holligan 2019). A cross-party coalition of lawmakers in Germany have recently pressed for the introduction of the SBL there (Engel 2020). In 2019 the Scottish government consulted Sweden on the implementation and effectiveness of

[3] These debates are further complicated where concerns about sex work are amalgamated with concerns about human trafficking (Skilbrei 2019) though the relationship between trafficking and prostitution is complex (Augustín 2007; Sanghera 2012; David and Loopmans 2017).

[4] A note on the use of terminology in this paper. The term 'sex work' and 'sex worker' have themselves become political (Jeffreys 2014; Jenness 1990) and have come to play a major role in feminist discourse around sex work (Sutherland 2017). One's use of the term has come to indicate one's views about the legitimacy of sex work as work as opposed to a form of violence (Bindel 2017). The term 'prostitute' in reference to those who sell sexual services is generally seen to be pejorative (Bernstein 2017). We follow Skilbrei (2019) in using the term 'prostitution' to refer to the act of selling sexual services but the term 'sex worker' in reference to the people involved in selling sexual services.

[5] Though the European Parliament has maintained that laws pertaining to prostitution are the concern of individual countries (Kingston and Thomas 2018).

the law (Stoyle 2019) and the adoption of an SBL remains the policy of the governing Scottish National Party, though not yet made law (Davidson 2019). However, the increasing popularity of the SBL among policymakers 'cannot be accounted for in terms of established facts or proven efficacy' (Ward and Wylie 2017*a*, p. 156).

In general, arguments about the 'right' legislative approach to the sex market rest largely on the degree to which one views the selling of sex as a legitimate occupation or as exploitation and inconsistent with gender equality (Watson 2014; Pitcher and Wijers 2014; HCHAC 2016).[6] Advocates of the SBL argue that its purpose is to make it 'clear what we as a society consider unacceptable and discouraging people from doing those things.'[7] Ekberg (2004) notes that the law was first advocated for in Sweden by those who viewed prostitution as 'another patriarchal tool of oppression that has deleterious effects on the women and girls who are induced and kept in prostitution, as well as an extreme form of male violence used to control female human beings as a class' (p. 1191). She argues that gender equality requires the rejection of the 'idea that women and children, mostly girls, are commodities that can be bought, sold, and sexually exploited by men' (p. 1188).

Alternatively, advocates for legalization recognize a critical difference between forced and voluntary prostitution and see the latter as a legitimate form of work (Outshoorn 2004). With respect to the SBL, a survey suggests that sex workers themselves are almost universally opposed to it (Huschke et al. 2014) and claim it makes their work more dangerous (HCHAC, 2016). While proponents and opponents of the SBL make various claims as to its impact on the market for sexual services and the well-being of sex workers themselves, the effects of the regime are likely to be context specific (Skilbrei 2019) and existing evidence as to the effects of the law is sparse, descriptive, and not always consistent.

The paper proceeds as follows. Section 2 provides some historical context for sex market regulation in the UK including the adoption of the SBL in Northern Ireland. Section 3 presents the data we use in our paper and discusses the methods we apply. Section 4 presents statistical evidence of the effect of the SBL on the UK sex market. Section 5 and 6 are on the impact of the SBL on sexually transmitted infection (STI) rates and sexual violence, respectively. Section 7 concludes.

[6] An alternative framing of this debate is harm reduction versus abolition (Skilbrei 2019; Abel 2010), though arguments of pro-criminalization advocates might fairly be interpreted as harm reduction via abolition.

[7] https://nordicmodelnow.org/what-is-the-nordic-model

2 Regulating the market for sex

In this section we present a brief history of sex market regulation in the UK and discuss the existing evidence on the effects of different regulatory regimes.

Sex market regulation in the United Kingdom

The component nations of the United Kingdom (England, Scotland, Wales, and Northern Ireland) have varying degrees of legislative autonomy from the British parliament though with respect to sex work the general regulatory framework had been largely harmonious between them. The buying and selling of sexual services in private was legal and unregulated, i.e. no standards of practice or licensing were required for sex workers or clients. This remains the case in England, Scotland and Wales today. However, as noted above, in June 2015 Northern Ireland deviated from this approach by criminalizing the purchasing of sex. Other activities related to the sex market such as solicitation in the street or a public place, brothel keeping, and controlling/exploiting prostitution for gain, i.e. 'pimping,'[8] are currently against the law throughout the United Kingdom.

Northern Ireland ultimately adopted the SBL as one part of the Human Trafficking and Exploitation (Criminal Justice and Support for Victims) Act that was passed by the Northern Irish Assembly in October of 2014. The SBL was the only clause in the human trafficking and exploitation bill that the Committee for Justice could not agree on whether or not to include. As it was quite controversial, its inclusion in the final bill was not assured (Ford and Morrow 2014). The proposal was supported by the Catholic Church (Brady 2013), anti-sex work advocates (BBC 2014), and gender equality campaigns (Fawcett Society 2014). The criminalization of the purchasing of sex was opposed publicly by the Minister of Justice at the time (Kilpatrick 2014) and was not supported, though not officially opposed, by the Police Services of Northern Ireland (HCHAC, 2016). In a survey of sex workers in Northern Ireland Huschke et al. (2014) found that 98% opposed the proposed law.

The purchasing of sex, independent of the context of the exchange, became a crime on 1 June 2015 introducing penalties of £1,000 and up to a year in prison for those found guilty of

[8] See Section 53 of the Sexual Offences Act 2003 for formal legal definitions of this activity.

paying for sexual services.[9]

Northern Ireland was not unique in the United Kingdom in its interest in the SBL. In 2014, the All-Party Parliamentary Group on Prostitution and the Global Sex Trade (APPG on Prostitution) published a report on the legal environment around sex work in England and Wales. The final recommendation of the report was that England and Wales ought to introduce a general offense for the purchase of sexual services, i.e. an SBL. And in November 2014, an amendment was added to a larger human trafficking and exploitation bill (the Modern Slavery Bill) which criminalized the purchasing of sex. Though the amendment was ultimately dropped from the final bill, it did enjoy cross-party support when introduced (Adams 2014). A follow-up report submitted to the APPG on Prostitution in 2016 was titled 'How to implement the Sex Buyer Law in the UK.'

In 2016, the Parliament Home Affairs Committee published a comprehensive report on prostitution in the United Kingdom and legislative models of the market for sex. In contrast to the findings of the APPG on Prostitution, the committee laid out proposals for reforms to existing legislation including the decriminalization of solicitation and brothel keeping, both in an effort to improve sex worker safety and to help sex workers to more readily exit the industry if they wish. The report included a review of the SBL as implemented in Northern Ireland though the committee concluded that they were 'not yet convinced that the sex buyer law would be effective in reducing demand or in improving the lives of sex workers' (HCHAC, 2016, p. 38) though they note that, at the time of their report, it was too early to fully assess the impact of the sex buyer law in Northern Ireland. Most recently, a bill criminalizing the purchasing of sex passed first reading, i.e. was formally introduced, in the British Parliament in December 2020 (Oppenheim, 2020).

In Scotland, there were also attempts to introduce the SBL. In 2010, the SBL was voted down 5 votes to 3 in committee (Johnson 2010), and again in 2012.[10] In 2015, the Scottish government commissioned a report to 'investigate the reliability of the evidence available on

[9] 'Payment' and 'sexual services' are defined in Part 2, Section 15 of the legislation as: (3) In paragraph (1), "payment" means any financial advantage to B, or any person other than B, including the discharge of an obligation to pay or the provision of goods or services (other than sexual services) gratuitously or at a discount.

(4) No offence is committed under this Article unless the sexual services that are provided or are to be provided by B to A involve-
(a) B being physically in A's presence, (b) B touching A or A touching B, and (c) the touching being sexual; or (d) B touching B in a sexual manner for the sexual gratification of A, B being physically in A's presence.

[10] Proposed by Rhoda Grant but lacked cross-party support (Scottish Parliament, 2012).

the criminalization of the purchase of sex' (Malloch et al., 2014). In the same year, another bill seeking to change the regulatory framework around sex work in Scotland was introduced, though this bill sought to decriminalize much of the sector including relaxing the restriction on brothel keeping, solicitation and 'living on the avails' of prostitution (Smith 2015). This also failed to become law. The SBL continues to be debated in Scotland and the current policy of the governing Scottish National Party is the adoption of an SBL (Davidson 2019).[11]

We provide this much background on the SBL in the United Kingdom to make clear that the adoption of the SBL in Northern Ireland was not driven by some particularities of it relative to the rest of the UK. The SBL has been debated and voted on across the United Kingdom, but it is only in Northern Ireland that it became law and its introduction there was on a narrow vote in committee. At the same time the SBL has been debated but fallen short of passing in the British and Scottish parliaments at various times. The SBL is therefore not unique to Northern Ireland or driven by some uniquely Northern Irish characteristic. As the other countries in the United Kingdom have all considered and voted on SBLs, we propose to use them as counterfactuals to Northern Ireland. What was unique to Northern Ireland is that it was the only region of the United Kingdom to pass such legislation (and even then only with considerable debate around it) and while very similar legislation was being debated elsewhere in the country. We take this as evidence that the SBL might have been introduced in Scotland, England, or Wales at any time, but in fact was only made law in Northern Ireland.

Evidence on sex market regulation

There has been a great deal of research done on sex work in Criminology and Law (for an overview of UK-related work especially see Sanders 2017), Sociology (for an overview of UK-related work especially see Sanders et al. 2009 and Weitzer 2009) and, to a somewhat lesser extent, Economics (see Cunningham and Shah 2017 for an overview). However, there 'are few examples of in-depth qualitative research on the workings and effectiveness' of sex market regulatory regimes (Skilbrei 2019 p. 495). Both researchers and policymakers have noted the lack of convincing evidence about the effects of different regulatory regimes, the SBL specifically (HCHAC, 2016; Svanström 2017; David and Loopmans 2017).

[11] The SBL was also debated in Republic of Ireland at the time. An Oireachtas Justice Committee published a report recommending the introduction of an SBL in 2013. The SBL was signed into law by the Irish Minister for Justice on 27 March 2017.

A small number of papers (Bisschop et al. 2017; Cunningham and Shah 2018; Cameron et al. 2020) use natural experiments to credibly estimate the causal effects of changing prostitution regimes.[12]

Cunningham and Shah (2018) study the effect of the unexpected judicial decriminalization of the sex market in Rhode Island in 2003 and subsequent legislated recriminalization in 2009. Using difference-in-differences and synthetic control methods, they found that decriminalization lead to large decreases in STI rates (about 40%) and sexual violence (about 30%). They attribute the fall in STI rates to a decrease in the riskiness of the client pool following decriminalization and to a decrease in risky sexual activities undertaken. The authors posit that the fall in sexual violence could be the result of an improved bargaining position of sex workers and the possibility that some men view sexual violence and prostitution as substitutes. The evidence on the effects of the 2009 recriminalization is less clear.

Similar results are found in Bisschop et al. (2017) who use the introduction of 'tipplezones' (designated areas in cities where sex work is legal) in the Netherlands to study the impact of local decriminalization. Using difference-in-differences they also find large negative effects on STI rates as well as some evidence of a negative effect on sexual violence.

Cameron et al. (2020) study the effect of criminalization. In 2014 East Java unexpectedly closed formal worksites where sex workers lived and worked. This action deviated from the rest of Indonesia where sex work is not explicitly referenced in law and is generally allowed to take place. Rather than a new piece of legislation coming into effect, the closure of the worksites was the result of local officials more strictly interpreting morality regulations that did already exist in Indonesian law, effectively criminalizing sex work in East Java. The authors find that the closures lead to large increases, 58 percent, in STI rates among female sex workers, largely the result of decreased condom use. They also find that the sex market did indeed shrink due to the criminalization, but that the effect was temporary. Within five years the market had largely rebounded and the higher rate of STI infections among the sex workers eventually manifested in higher infection rates in the population as a whole.

Della Giusta et al. (2019) consider the effect of tighter regulations around the British sex market, introduced in Scotland in 2007 and the rest of the country in 2009. They find that demand did not change in response to the stricter laws, even though these new regulations did

[12] Theoretical treatments of the sex market have been developed in Edlund and Korn (2002), Immordino and Russo (2015), and Lee and Persson (2016).

not criminalize the buying or selling of sexual services per se. They do find that the composition of clients became riskier.

Studies on the effects of SBL are rare and most of them come in the form of reports on the SBL published or commissioned by the governments that introduced the law (e.g. Statens Offentliga Utredningar 2010 for Sweden, Rasmussen et al. 2014 for Norway (and Bjørndah 2012 for Oslo specifically) and Ellison et al. 2019 for Northern Ireland[13]). Other reports have been produced by Non-Governmental Organizations (e.g. Amnesty International (2016)), sex work advocacy organisations (Ugly Mugs 2019; Fisher 2017; Riise 2014). These reports do not use methods that would allow for a causal interpretations of their analysis.

The evidence on the effects of the SBL in these reports can be inconsistent and a clear and convincing picture of the effects of the SBL has yet to emerge. For example, in their assessment of Ellison et al. (2019), the Northern Irish Department of Justice concluded that 'there is no evidence that the offence of purchasing sexual services has produced a downward pressure on the demand for, or supply of, sexual services' (Department of Justice 2019, p. 5). This conclusion is in contrast to that drawn in the analogous report commissioned by the Swedish government in 2010 (Statens Offentliga Utredningar 2010) which concluded that the SBL there significantly reduced the size of the sex market, saying that it can be 'considered a direct result of the criminalization of sex purchases' (p. 35).[14] Rasmussen et al. (2014) finds that the SBL in Norway also reduced demand as measured by the participation in the street market.

There is limited work on the SBL from economists though these studies endeavour to provide evidence as to the causal effects of regulatory changes. Ciacci (2020) studies the effect of the SBL introduced in Sweden and finds evidence that the law led to an increase in outdoor sexual assaults. Jakobsson and Kotsadam (2011) use Sweden as a counterfactual to show that the introduction of the SBL in Norway lead to more negative attitudes towards sex work. Berlin et al. (2020) also study the Swedish SBL and find evidence that it increased violence perpetrated against women but that it was an increase in domestic violence rather than violence perpetrated against sex workers.

Each of these studies a distinct change to the regulatory framework around the sex market

[13] This report was commissioned by the Northern Irish Department of Justice and carried out by a team of researchers at Queen's University Belfast.

[14] The conclusions in the Swedish report are based largely on changes to the outdoor sex market. The observed changes therein may have been driven more by the increasing prevalence of the Internet and online markets than by the law. Internet penetration rose from 41.4% in 1999 when the SBL was introduced in Sweden to 90.9% by 2010 (Forzati et al. 2010). By the time the SBL was introduced in Northern Ireland in 2015, 82% of households had Internet access (Office for National Statistics 2016).

and in distinct settings. But the preponderance of evidence, limited though it may be, suggests that less permissive regulation can reduce the size of the market for sexual services but may have unintended effects including increased STI rates and increased sexual violence.

3 Data and Methods

We estimate the effect of the SBL on the market for sexual services, STI rates and on sexual violence. To do so we construct three new data sets, discussed in detail below. We first discuss the data and then outline the estimation methods we use.

Data

We build three new data sets. First, using information collected from online marketplaces, we construct a data set of reviewed sexual service transactions per six-month period.

Second, we combine publicly, readily available data with data acquired via a number of Freedom of Information (FoI) requests from the Ministry of Health in England and Wales and Public Health England, from the Northern Irish Department of Health, the Irish Health Protection Surveillance Centre, and Health Protection Scotland on the STI rates.

Third, we combine publicly, readily available data with data obtained via a number of FoI requests for Police Force Area (PFA) statistics on the numbers of recorded crimes, and sexual violence in particular, from the Office of National Statistics and the Home Office for England and Wales, Police Services of Northern Ireland, and the Scottish Government.

Full discussion of these data sets follow below. Table 1 is a summary of the data we use.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Mean	Standard Deviation	Min.	Max.
Panel A: NUTS2*-level				
Transactions per 6 months	66.9	78.81	0	547
Unique sex workers per 6 months	34.5	37.5	0	266
Unique clients per 6 months	42.7	51.0	0	409
Observations	684			
Panel B: Transaction-level				
Duration (minutes)	97.4	97.2	40.0	720.0
Total fee (£)	213.0	183.5	10.3	10,691.4
Observations	31650			
Panel C: Local Authority/District-level				
Female gonorrhea (per 100k)	11.1	10.9	0.0	81.2
Male gonorrhea (per 100k)	31.6	58.8	0.0	762.7
Female chlamydia (per 100k)	89.2	55.8	0.0	613.4
Male chlamydia (per 100k)	89.3	64.3	0.0	733.0
Observations	3498			
Panel D: PFA-level				
Recorded sexual assault on a female	428.2	692.6	0.0	7,271.0
Recorded sexual assault on a male	77.0	103.8	0.0	1,085.0
Recorded rape of a female	371.6	622.7	1.0	7,463.0
Recorded rape of a male	40.9	70.3	0.0	857.0
Recorded non-sexual-violence crime	70,649.0	106,822.4	4,127.0	977,262.0
Total charged crime	17,643.6	21,206.2	931.0	171,248.0
FTE police officers	2,220.3	3,926.7	157.0	33,366.0
Observations	938			
Panel E: Local Authority-level				
Population ('000)	168.4	112.0	2.2	1,155.3
Unemployment rate	5.6	2.8	0.4	21.6
Economically active rate	56.0	7.0	21.9	81.1
Per household income	18.4	9.7	9.9	213.3
Observations	4858			

In Panel A we report summary statistics on reviewed transactions, unique sex workers and unique clients that we observe per six-month period (December to May and June to November) in 38 NUTS2* regions[15] for June 2010 through 2018.

These data are aggregates based on data collected from sex market websites. While advertising sexual services in magazines and on fliers is illegal in the United Kingdom, advertising online is not. In fact, sex work has become almost exclusively an online market.[16] In their survey of UK sex workers, Sanders et al. (2018) found that as of 2015, 2% of sex workers engaged in street work with 98% using online platforms to find clients. As such, there is a large online marketplace for sexual services and a number of websites designed to facilitate transactions.

We collect and combine data on participants in the sex market and on transactions between those participants from two online sex work platforms which we will call Platform 1 and Plat-

[15] NUTS2* are equivalent to NUTS2 statistical areas save for those in London. Our data do not allow us to locate transactions in one of London's 5 NUTS2 areas, so we use only 2 areas for London (Inner and Outer).

[16] From Sanders et al. (2016): "Research is beginning to demonstrate the everyday realisation that the majority of sex work is now mediated by or provided through digital technologies and that internet-based sex markets are pervasive."

form 2. These platforms operate similarly to other online labor markets such as fiverr.com or peopleperhour.com. Platform 1 was at the time of collection (early 2019) the largest sex work platform in Great Britain in terms of participation and Platform 2 was at the time of collection the largest sex work platform in Ireland.[17] The two platforms overlap in Northern Ireland. Both Platforms are freely accessible and do not require registration to access profiles or reviews. All the information we collect from the websites is available to anyone with an Internet connection.

The two platforms operate in similar ways. In general, the sex workers create a profile where they introduce themselves to potential clients. Profiles provide us with information about each sex worker including demographics, geographical areas in which they work, activities offered, often a menu of prices for meetings of different durations, and whether the sex worker has been verified on the platform.[18]

An important feature of these platforms for our study is the system of reviews each uses. Clients can leave reviews of their transaction with a given sex worker and these are visible to anyone viewing that sex worker's profile. The characteristics of the reviews are particular to the platform.

On Platform 1 reviews take two forms. First, clients can write a short-form review which generally includes information about which client and sex worker was involved with the transaction, the date of the transaction, a categorical rating and a short free text review usually just a sentence or two. Both clients and sex workers can leave reviews of this type for each other. We have about 900,000 of these short-form reviews in our data. Unfortunately, they do not contain information on the location of the transaction. We do not use these reviews in the analysis that follows.

Second, clients on Platform 1 can write long-form reviews. These are detailed, client-written reviews of the meeting with the sex worker(s). Information from the reviews generally includes who was present, when and, importantly, where the meeting took place, the fee paid, the time spent together, a quantitative assessment (marks out of 10) of various features of the transaction. There are also free-text portions of these long-form reviews where clients provide an account and assessment of the transaction. On Platform 1 the reviews are only available if the sex workers or clients associated with the review had an active profile at the time the data were collected. We

[17] See report from the Northern Ireland Department of Justice, 2019.

[18] This verification is used as a signal to the clients that their advertisements are genuine. The sex workers are required to send a photo of themselves holding a piece of paper with the name of the platform on it to identify the person represented in the profile is indeed them.

therefore do not observe a complete history of reviewed transactions on Platform 1. Note that the sex worker or client need not be an active participant in the market, just that they have an active profile. It seems many active accounts are associated with people who are no longer market participants. A quarter of the active profiles do not have a single review in the two years before we collected the data. We have 39,246 of these reviews for the United Kingdom, though only 159 reviews from Platform 1 in Northern Ireland for 2010 through 2018.

The reviews on Platform 2 are similar to the long-form reviews of Platform 1 in that they include information on where and when the transaction took place, the fee paid and who was involved, a quantitative assessment of the transaction (albeit different from that used in Platform 1). The free text section tends to be much shorter, including less detail than the long-form reviews in Platform 1. There is no short-form review analogous to Platform 1. Unlike Platform 1, we have a much fuller history of reviews on Platform 2 as reviews are available even if the reviewer and reviewee are no longer active on the site. We have 6,394 of these reviews from Platform 2 in Northern Ireland for 2010 through 2018. There are no Platform 2 reviews in Great Britain. In late 2016, Platform 2 began allowing reviewers to hide the location of the transaction in their review, having previously required it to be declared. In 2017, 12% of reviews on Platform 2 had hidden locations, rising to 15% in 2018.

It is important to note that observing reviews is not the same as observing transactions as not every transaction is reviewed. Moreover, any change we see in the number of reviewed transactions may be the result of a change in the actual number of transaction, the propensity to review transactions or both. We address this concern when we discuss our results below.

The combined data collected from these platforms include 32,307 reviews for 4,534 uniquely identifiable sex workers and 12,053 clients from June 2010 to June 2018. We aggregate the number of transactions to create a panel defined in the six-month-NUTS2* space. These aggregated data are summarized in Panel A of Table 1.

In Panel B we report summary statistics for transactions. The data on duration and fees are taken from the reviews written by clients. Fees are reported in 2019 prices. Not every review reports fee and/or duration. We have data on both fees and duration for 31,650 transactions from June 2010 to June 2018.

Measuring the effect of the SBL on the fees is complicated by a number of things. First, we do not observe supply in our data. Instead we rely on data in Ellison et al. (2019) on the

number of advertisements posted in Northern Ireland as discussed below. Second, the SBL may have opposing effects on the total fee per transaction. This is because demand may fall at the intensive margin (i.e. the duration of the transactions may be shortened) as well as at the extensive margin (i.e. the number of transactions a client pays for).

The total fee for a transaction is, among other things, a non-linear, increasing function of the duration of that transaction with the total fee increasing but the fee per minute falling with the duration.[19] If the SBL reduced demand at the extensive margin only we would expect a decrease in fees per minute. But if demand falls at the intensive margin, as clients may opt for shorter transactions to reduce the probability of detection by law enforcement, then one effect would be an increase in fee per minute, all else being equal. As such the SBL might exert both a positive and negative effect on the observed total fee. Duration, being an outcome in our setting, is a 'bad control' (Angrist and Pischke 2009, p. 64) and so cannot simply be included on the right-hand side of a regression. Estimating the effect of the SBL on fee per minute conflates the potential effects of the SBL on the duration and on the fee. We do not, however, find any evidence in our data that the SBL lead to reduction in the duration of transactions. We proceed by analyzing the effect of the SBL on fee per minute.

In Panel C we report summary statistics for the number of gonorrhea and chlamydia infections annually. The data set was constructed by combining publicly available data and data from a number of FoI requests.[20] We measure the number of STIs at a local level, although 'local' means slightly different things in different areas. In England and Wales, we observe the number of infections by local/unitary authority of residence, i.e. where the infected person lives. We have these data for 310 local/unitary authorities in England and 20 in Wales. In Scotland and Northern Ireland we observe the number of infections by the clinic recording the diagnosis, i.e. the where the person was diagnosed. There are five such clinics in Northern Ireland and 15 in Scotland, each responsible for a particular geographic area. We have these data for 350 local areas for 2009 to 2018.[21]

In Panel D we report summary statistics on recorded crime and police force numbers. The

[19] We estimate a duration elasticity of fee per minute of about -0.1 in our data.

[20] Data for Wales were obtained via a FoI request submitted to Public Health Wales. Publicly available data for Scotland are from Health Protection Scotland. Publicly available data for English region for 2014-2018 are from Public Health England and were obtained for 2009-2013 via a FoI request. Data for Northern Ireland were obtained via a FoI request submitted to the Northern Irish Public Health Agency.

[21] Data for England were provided by Public Health England. Data for Scotland are from Health Protection Scotland. Data for Northern Ireland were obtained via a series of FoI requests to each of the five regional health trusts. Welsh data was obtained from Public Health Wales.

administration of law enforcement and collection of crime statistics differs in the details between England, Wales, Scotland, and Northern Ireland. In England and Wales policing is administered, and crime statistics reported, primarily in the Police Force Areas (PFAs) of which there are forty-three. In Scotland there are thirteen Police Divisions. Law enforcement in Northern Ireland is administered by a single police force, the Police Service of Northern Ireland (PSNI), though this is divided into eleven local policing districts where data on recorded crimes are collected. Data on recorded crimes, including sexual violence, are published by the Office for National Statistics (ONS) for England and Wales. In Northern Ireland we use data on recorded crimes published by the Northern Irish statistical office OpenDataNI. Additional data on sexual violence were obtained via a FoI request. In Scotland, data on the number of recorded crimes, including sexual violence, are published by the Scottish Government. Data on the number of police officers at the PFA level for England and Wales are taken from Allen and Zayed (2019). Data on the number of police officers at the local policing district level were obtained for Northern Ireland via a FoI request submitted to the PSNI. Data on the number of police officers in each division in Scotland are published by The Scottish Government. We have annual data from sixty-seven PFA/policing districts for 2005 to 2018. We discuss some important particularities of these crime data in greater detail below.

In Panel E we present descriptive statistics of the variables we have concerning local economic conditions obtained from the Office of National Statistics, Northern Ireland Statistics and Research Agency, and the Scottish Government. We have these data annually for 389 local authorities/districts across the UK for 2005 to 2018. To include these as controls in the analysis that follows, we aggregate up to the appropriate geographic level, i.e. NUTS2*, NUTS2 or PFA, as needed.

Lastly, the change to the policy on Platform 2 at the end of 2016 which allowed reviewers to hide the location of the meeting coupled with the introduction for the SBL in the Republic of Ireland in March 2017 means that we have doubts about usefulness of the data from the end of 2016 onward. As noted above, a small share of reviews written after 2016 actually have their locations hidden. While there is no direct, legal effect of the introduction of the SBL in the Republic of Ireland on Northern Ireland, there may be indirect effects on the sex market as there is movement of sex workers and clients across the open border between the Republic and Northern Ireland. We carry out our primary analysis in all that follows using the full data set

up to 2018, but we include checks for the sensitivity of our results by excluding the post-March 17 period.

Evaluating the Sex Buyer Law in Northern Ireland

We use these data to estimate the effect of the SBL on the size of the sex market, sexual health, and sexual violence. Our objective of estimating the effect of the SBL in Northern Ireland is an example of a regional policy evaluation. The econometric methods brought to bear on such studies have often mirrored those used in applied microeconomic settings (Imbens and Wooldridge 2009). Difference-in-differences (DiD) is an approach to obtaining causal estimates of a treatment effect that is widely used in such endeavours.[22] In recent years there have been a number of developments of the DiD estimator (e.g. Cengiz et al. 2019; Ferman and Pinto 2019; de Chaisemartin and d'Haultfoeuille 2020; Callaway and Sant'Anna 2020; Goodman-Bacon 2021; Arkhangelsky et al. 2020) addressing problems like heterogeneous effects, differential treatment timing, and violations of the identifying parallel trends assumption.

In our setting we have a binary treatment and no differential treatment timing. As such recent advancements in the DiD estimator relevant to the case of differential treatment timing (e.g. Goodman-Bacon 2021; Callaway and Sant'Anna 2020) are not necessary. Under our conditions the difference-in-difference estimate of an average treatment effect on the treated (ATT) can be obtained via OLS estimation of:

$$y_{it} = \beta D_{it} + X'_{it}\theta + f'_t\lambda_i + \epsilon_{it} \tag{1}$$

where $i = 1, ..., N$ cross-sectional units. These units are NUTS2* areas, PFAs, sex workers, and NUTS3/clinic areas at various points in the analysis that follows. $t = T_S, ..., T$ time periods where the treatment is introduced to the treated units in $T_S < T_0 < T$ where t is years or six-monthly periods at various points in the analysis that follows and T_0 is either 2015 or the second half of 2015 at different points. y_{it} is the outcome of interest in cross-sectional unit i at time t which can be reviewed transactions, fee per minute, STI rates or sexual violence. NI_i is a dummy equal to 1 indicating that the cross-sectional unit is in, or is, Northern Irleand, $Post_t$ is a dummy equal to 1 if the observation $t > T_0$ and $D_{it} = NI_i \times Post_t$. X_{it} is a vector of

[22] Angrist and Pischke (2010) call difference-in-differences 'probably the most widely applicable design-based estimator' (p. 14).

exogenous controls including mean income, unemployment rate, economically active rate and population. The error term is constructed of two parts. First, we start with a conventional two-way fixed-effects specification so that $f_t'\lambda_i + \epsilon_{it}$ where $\lambda_i = \left[\begin{smallmatrix}\alpha_i \\ 1\end{smallmatrix}\right]$ and $f_t = \left[\begin{smallmatrix}1 \\ \psi_t\end{smallmatrix}\right]$ and so $f_t'\lambda_i = \alpha_i + \psi_t$. Second, ϵ_{it} is a random error term.

The OLS estimator of β can be causally interpreted under the identifying assumptions that: 1) the treatment is exogenous, 2) that in the absence of the treatment y_{it} would have evolved the same for all units, 3) there is no variation in treatment timing.

As discussed in Gabillon and Magnac (2016) and Abadie and Cattaneo (2018), there are challenges particular to a regional policy evaluation that mean the simple application of established microeconometric tools, such as the estimation of (1) by OLS, might bias the OLS estimator. We face a number of these challenges for different parts of our analysis. The main challenges we face are that we have a single treated geographic unit, that we see evidence that the effect of the SBL on our outcomes varied over time, that, in the case of sexual violence, there is unobserved time-varying heterogeneity and, lastly, that our data are not a random sample drawn from the population of interest. In the analysis that follows we estimate equation (1) by OLS, but we also pursue three alternative, but related, approaches to estimating the effect of the SBL in Northern Ireland in an effort to address these challenges. First we present event studies of the effects of the SBL to take into account the changing effect of the SBL over time. Second, we use the synthetic control method which relaxes the parallel trends assumption and offers an approach to obtaining a more credible control given our single treated unit in some cases. In an effort to address the unobserved time varying heterogeneity we use factor models and triple difference-in-differences in our analysis of sexual violence, discussed in Section 6. Lastly, we use randomization inference, discussed below, to measuring the degree of uncertainty around our estimates.

Synthetic Control Method

The Synthetic Control Method (SCM) (Abadie and Gardeazabal 2003; Abadie et al. 2010; Abadie et al. 2015) relaxes the parallel trends assumption required for identification in DiD by creating a synthetic counterfactual treatment unit as the weighted average of untreated units in the pre-intervention period. The weights are chosen to create the synthetic counterfactual that best approximates the real treated unit, minimizing the residual mean squared prediction error (RMSPE), i.e the error between the treated unit and the synthetic. In our setting this means

we construct a synthetic Northern Ireland as the weighted average of other areas in the Great Britain.

While the original SCM is an conceptually fairly simple, there is a growing literature developing extensions of the SCM (e.g. Chernozhukov et al. 2017; Arkhangelsky et al. 2019; Ferman and Pinto forthcoming; Ferman 2019; Botosaru and Ferman 2019; Abadie and L'Hour 2019; Doudchenko and Imbens 2016 and an overview in Abadie 2021).

One practical issue with the estimator is the choice of the variables and the pre-treatment periods that are used to obtain the weights used to construct the synthetic control. Although the weights are chosen algorithmically to minimize the residual mean squared error, the decision of which periods and which variables are included in that minimization problem is left to the researcher. In simulations, Ferman et al. (2020) show that the set of predictor variables can be varied in such a way as to produce statistically significant or insignificant results by choosing which variables for which pre-treatment periods are matched on. This cherry picking of pre-treatment predictors means the SCM is effectively vulnerable to an opaque form of p-hacking. Ferman et al. (2020) propose the minimization of the post-treatment RMSPE as a criterion for specification selection. However, even given an optimization problem such as this, the specification of predictors is not trivial. Attempts to choose the specification which minimizes the RMSPE are subject to a curse of dimensionality of sorts with the number of potential combinations of predictors being $\sum_{w=1}^{W}\sum_{s=1}^{S}\frac{w!}{s!(w-s)!}$ where W is the number of variables that can be matched on including the outcome and S is the number of pre-treatment periods. Some (e.g. Cavallo et al. 2013) match on all pre-treatment outcomes. Kaul et al. (2015) shows that additional covariates add no information to the SCM when all each pre-treatment outcomes are used to match. In every SCM reported below we use the outcome, or a transformation thereof, in all pre-treatment periods to obtain the weighting matrix. The transformation depends on the particular characteristics of the data which we discuss in the respective results sections below.

Inference

Inference is complicated by three features of our data and setting. First, the calculation of standard errors for the DiD estimator is complicated by the presence of serial correlation. As Bertrand et al. (2004) show, conventional DiD standard errors that fail to account for this correlation will severely underestimate standard errors and result in over-rejection. They propose a bootstrap as a potential solution, though this requires a large number of units and MacKinnon

and Webb (2020) show that different variants of the bootstrap can lead to over- or under-rejection as well. This brings us to the second challenging feature: we have only a small number of treated units. There have been a number of approaches developed recently to estimate standard errors in such a setting (Cameron and Miller, 2017; MacKinnon and Webb, 2019; Ferman and Pinto, 2019). However, no single approach has emerged as the single right one since correct performance of these methods has been shown to depend in different ways on the absolute number of treated units, the number of treated units relative to the number of untreated units, and the nature of the effect being estimated. As Mackinnon and Webb (2020) note:

> The performance of all the procedures we study depends in a complicated way on the numbers and sizes of the treated and control clusters, the cluster-level covariance matrices of the error terms, and the numbers of treated observations within the treated clusters. This suggests that the best procedure to use will depend on the specific dataset under analysis. (p. 450)

The third feature is the source of uncertainty in our data. In the case of STI rates and sexual violence, our analysis is not based on a sample, random or otherwise, but on the population of areas (NUTS2$*$, NUTS2, PFAs, local authorities). The uncertainty about our estimates is not from sampling but rather from the fact that we observe some units under the treatment (i.e. the SBL) but we do not observe those treated units in the absence of the treatment. The uncertainty comes from the unobserved potential outcomes, from our ability to observe each unit in only one of the relevant states (treated or untreated) rather than random sampling error. Hirschauer et al. (2020) argue that in the absence of random sampling, conventional p-values are effectively meaningless.

These three features of our data and setting challenge the use of conventional calculated standard errors as a useful measure of uncertainty. We therefore use randomization inference to measure the degree of uncertainty around our estimates (Fisher 1935; Rosenbaum 2002; Dinov et al. 2018; Athey and Imbens 2017).

In instances where we have multiple treated units, the empirical p-values for the test statistic θ are generated as follows. First, $\hat{\theta}$ is obtained from equation (1). Second, based on the timing of the actual treatment, we randomly re-assign units to a control or treatment group, in our setting $NI_i = 0$ and $NI_i = 1$, respectively, such that $\overline{NI}$ is constant. Third, we re-estimate equation (1) using the same data but with the now randomly re-assigned 'treatment.' The estimated

'treatment effect' from permutation k, θ_k^*, is obtained and stored. This process is repeated K times producing a vector of $\hat{\theta_k^*}$ which is then sorted from largest to smallest. Lastly, $\hat{\theta}$ is located among the sorted θ_k^* vector and the empirical p-value is calculated according to:

$$p^{\theta} = \frac{1 + \sum_{k=1}^{K} \mathbf{1}(|\theta_{\mathbf{k}}^*| > |\hat{\theta}|)}{1 + K}, \tag{2}$$

where p^* is the adjusted proportion of $\hat{\theta_k^*}$s which exceed $\hat{\theta}$ in absolute value. We follow Davison and Hinkley (1997) in adding 1 to both the numerator and denominator. The reasoning is that under the null the test statistic from the actual data, i.e. $\hat{\theta}$, and those from the replications, i.e. θ_k^*, are drawn from the same distribution. These draws are ranked and the probability that the $\hat{\theta}$ has the observed rank or higher is given by equation (2). Failing to add 1 to both numerator and denominator will underestimate p^* under the null. It also has the nice feature of avoiding $p^* = 0$. We set $J = 1,000$.

We perform randomization inference based on two test statistics $\theta = \beta\,, \tau$ from equation (1). First, we use the treatment effect β, which we will call RI-β. Second, we use the t-statistics for $\hat{\beta}$ based on standard errors clustered at the level of geographic aggregation (i.e. Police Force Areas), which we will call RI-τ. This latter approach is found to perform better, in terms of accurate rejection rates, than RI-β in the case of smaller numbers of treated units (MacKinnon and Webb, 2020).

When we have just one treated unit we use a special case of this approach where we re-estimate equation (1) assigning the treatment to each untreated unit in turn in order to obtain a placebo $\hat{\theta^*}$. We then compare the estimated effect for Northern Ireland to this distribution of placebo effects. As this procedure differs slightly from the randomization inference procedure outlined above, we refer to this as Placebo-θ. Note that the smallest empirical p-value we can obtain for the NUTS2*-level data is $1/(38+1) = 0.03$; for the NUTS2-level data is $1/(40+1) = 0.02$, and for the PFA-level data is $1/(67+1) = 0.01$.

Intuitively, the qualitative interpretation of p-values obtained via randomization inference is the same as more traditional inference. If $\hat{\beta}$ is in one of the tails of the empirical distribution β_j^*, the p-value is small and we are more confident that the estimated effect is not the result of noise. Rejection of the null hypothesis is based on whether this actual value of the chosen test statistic is in the tails of the distribution. An empirical, or exact, p-value is calculated as the proportion of produced placebo test statistics that exceed, in absolute value for a two-tailed test, the actual

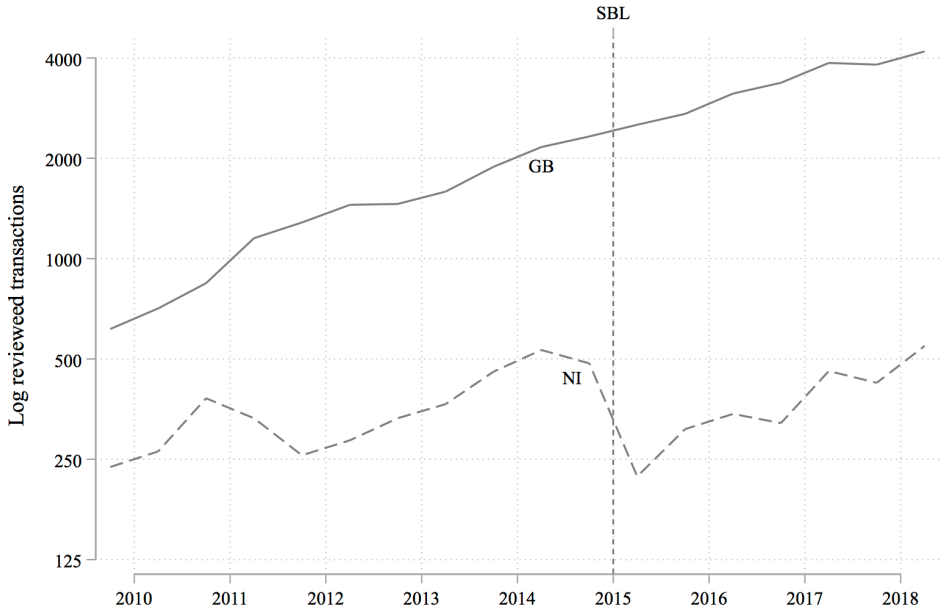
value. These tests are not contingent on distributional assumptions about the error term, the specification of the model being correct or sample size. Because if its independence from such assumptions, this approach has become increasingly popular among applied researchers (Heß, 2017) and was used in Cunningham and Shah (2018).

4 Results for Reviewed Transactions and Fees

The objective of the SBL is to reduce demand for sexual service and thus reduce the size of the market. We cannot observe transactions directly, but we do observe the number of reviewed transactions that take place in a particular place over time. Using the data collected from the sex market platforms, we estimate the effect of the SBL on the number of reviewed transactions. This will provide a measure of the size of the sex market to the degree that the number of reviewed transactions correlates to the number of actual transactions. We first present the results and then give them context below.

The identification strategy outlined above proposes using Great Britain as a counterfactual for Northern Ireland. This approach is complicated in the case of reviewed transactions because the observability of those reviews differs across the two platforms and all the reviews in Great Britain come from Platform 1 while the vast majority of reviews we have in Northern Ireland come from Platform 2. Figure 1 makes the challenge clearer. Here we plot the number of reviewed transactions in Great Britain and in Northern Ireland per six-month period.

Figure 1: Reviewed transaction per six-month in Great Britain and in Northern Irleand



Note: This figure plots the log number of reviewed transactions in Great Britain and Northern Ireland in each six-month period.

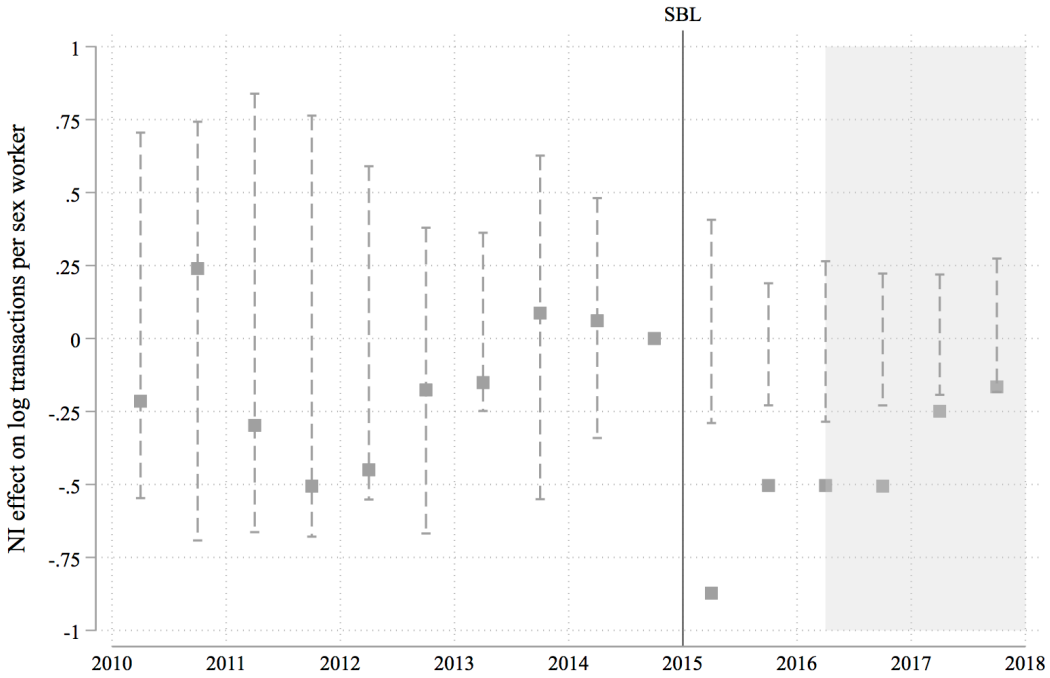
Three issues of comparability are evident. First, the levels are not comparable at any time. Based on these data alone, in the second half of 2018 there were about 66 reviewed transactions per million people in Great Britain and 277 reviewed transaction per million people in Northern Ireland. Such a difference in the size of the sex market is not supported in any other study and a claim that this represents a real difference would not be credible. The difference in the levels can be driven by the fact that Platform 1 might have a smaller share of the market in Great Britain than Platform 2 has in Northern Ireland. It might also be driven by the fact that Platform 1 has a short-form review, meaning clients that might otherwise have left a long-form review and thus be counted in our data leave a short-form review instead.

Second, there is a clear upward trend in the number of transactions in Great Britain which may reflect real increases in the number of transactions but is at least in part due to the observation rule, i.e. we observe reviews in Great Britain only if the client or sex worker associated with the review has an active account at the time the data were collected. This is not the case in Northern Ireland as we observe a much fuller history of reviews from Platform 2.

We are interested in comparing changes within Northern Ireland with changes in Great Britain. To facilitate such a comparison in light of the different observation rules we de-mean and de-trend the data presented in Figure 1 using unit-specific pre-treatment trends (Ferman and Pinto forthcoming; Liu et al. 2020). We also re-do all the following estimation but applying the Platform 1 observation rule to the data from Platform 2 and present those results in the appendix.

Figure 2 presents estimates of the Northern Ireland effect based on the transformed data.

Figure 2: Northern Ireland effect on log reviewed transactions



Note: This figure plots the estimated Northern Ireland effect on the log number of reviewed transactions using data from Platform 1 and 2 and using Great Britain as the control group. The reference period is the six months preceding the SBL. The dashed lines represent the third smallest and third largest (of 38 NUTS2* areas in Great Britain), approximately the 10th and 90th percentiles of the distribution of effects obtain via randomization of the treatment over these NUTS2* areas.

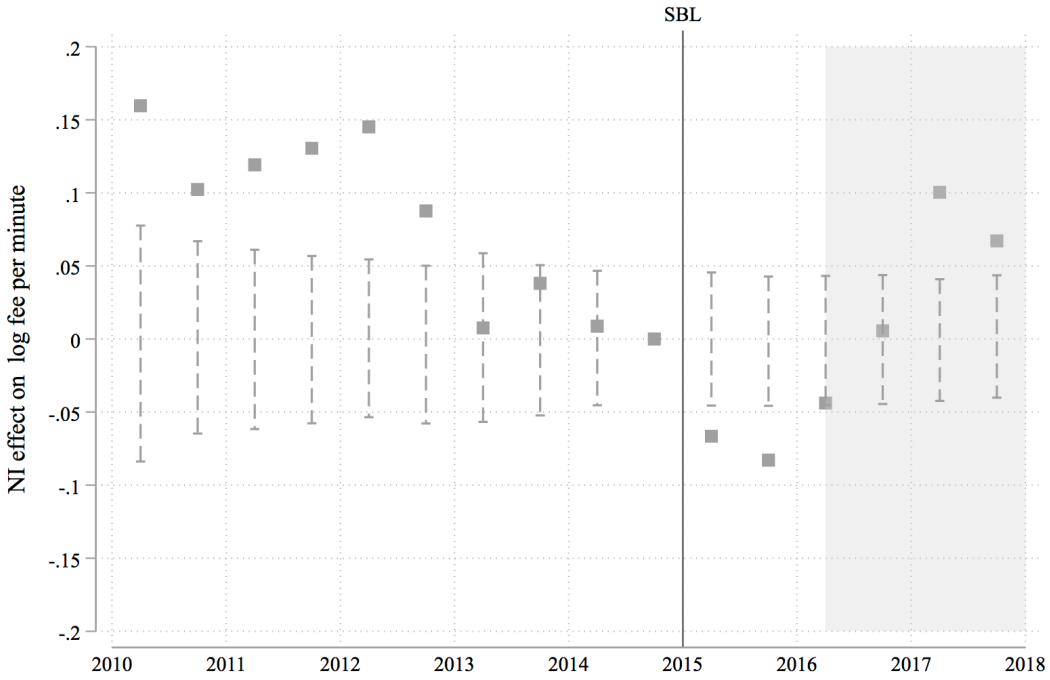
We estimate the annual effects for each untreated unit, i.e. placebo effects, and the confidence interval marks the third smallest and third largest (of 38 NUTS2* areas in Great Britain), approximately the 10th and 90th percentiles of the distribution of effects obtain via randomization of the treatment over these NUTS2* areas.

There is a marked drop in the Northern Ireland effect following the introduction of the

SBL. The shaded area represents the period over which we have concerns about the data from Platform 2 from late 2016 and when the SBL was introduced in Ireland. The picture is very much the same if we re-estimate the Northern Ireland effects excluding this period. In the appendix we present the same event study using Republic of Ireland as the control group.

Figure 3 presents the Northern Ireland effects on the log fee per minute estimated using sex-worker-level data. Until the first half of 2013 the fee per minute in Northern Ireland was approximately 12% higher than in the rest of the UK, followed by a relative decrease in the fee per minute and for the two years prior to the introduction of the SBL the average fee per minute in Northern Ireland was effectively the same as in the rest of the UK. The reason for this relative decrease in fee in 2013 is not immediately clear, though Ellison et al. (2019) find that there was an increase in the number of sex worker profiles in Ireland from 2013 to 2014 and then a retrace from 2016 and 2017 (see their Figure 1 on p. 87). In the 18 months following the introduction of the SBL the fee per minute in Northern Ireland fell by 6% relative to Great Britain but had returned to the pre-SBL equality with Great Britain by the first half of 2017. This pattern is similar to what we see for the number of reviewed transactions, consistent with a decrease in demand for sexual services in Northern Ireland in those initial 18 months following the introduction of the SBL when the number of active profiles in Ireland was stable (Ellison et al. 2019).

Figure 3: Northern Ireland effect on log fee per minute



Note: This figure plots the estimated Northern Ireland effect on the log fee per minute of a transaction using data from Platform 1 and 2 and using Great Britain as the control group. The reference period is the six months preceding the SBL. The dashed lines represent the 10th and 90th percentiles of the distribution of effects obtain via 1,000 randomization of the treatment over sex workers in the United Kingdom.

The effect of the SBL on fees per minute is less pronounced than for transactions but present. Visually it seems the results here might be explained by a downward trend in fees in Northern Ireland relative to Great Britain from 2010 through 2016. However, given the relative stability of the Northern Ireland-Great Britain fee differential in the two years preceding the SBL, the 7% drop following the introduction of the SBL is arguably not part of such a trend.

Taking Figures 2 and 3 together offers evidence of a decrease in demand following the introduction fo the SBL. We see both reviewed transactions and fees per minute decline, though only for 18 to 24 months.

We next estimate equation (1) and present the resulting difference-in-differences estimate in Table 2 allowing for country-specific linear trends.

Table 2: Difference-in-differences estimates of the effect of the SBL on log transactions and log fee per minute

	(1)	(2)	(3)	(4)
	Reviewed transactions		Fee per minute	
ATT of the SBL	−0.60	−0.60	−0.07	−0.08
Placebo-β: $Pr(\lvert\hat{\beta}^j\rvert > \hat{\beta})$	0.05	0.05		
Placebo-τ: $Pr(\lvert\hat{t^j}\rvert > t)$	0.05	0.03		
RI-β: $Pr(\lvert\hat{\beta}^j\rvert > \hat{\beta})$			0.00	0.00
RI-τ: $Pr(\lvert\hat{t^j}\rvert > t)$			0.00	0.00
Observations	684	684	31,433	31,433
Controls	No	Yes	No	Yes
NUTS2* and Year FE	Yes	Yes	Yes	Yes

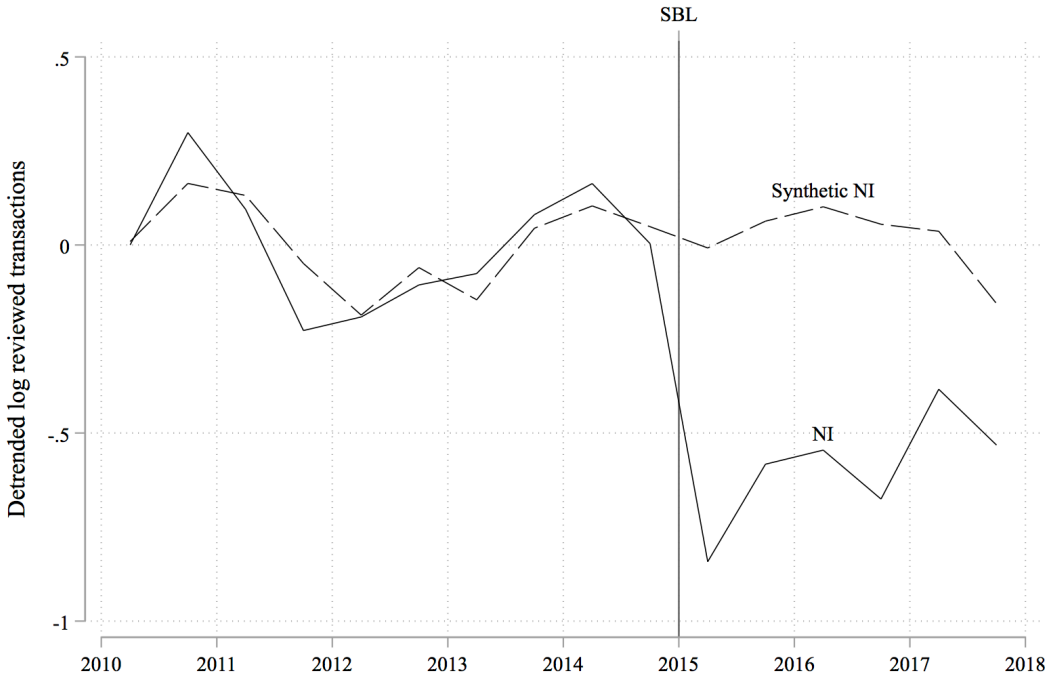
Notes: This table presents results from estimating Equation (1) for where the dependent variable is the log number of reviewed transactions (columns (1) and (2)) obtained using the number of transactions aggregated to the NUTS2*-level over six-month periods, the log fee per minute (columns (3) and (4)) obtained using transaction-level data over six-month periods.

In columns (1) and (2) we present the results to reviewed transactions, in (3) and (4) for the log fee per minute using Great Britain as the counterfactual. Inference is via Placebo-β and Placebo-τ and the empirical p-values obtained via equation (2) are reported beneath the estimated effects. Recall that the smallest possible Placebo-θ p-value when we use NUTS2*-level data is 0.03. The results here are consistent with those in Figures 2 and 3, though do not allow for the changing effect of the SBL over time.

In our analysis of the number of transaction we have a single treated unit and so, as discussed above, we next estimate the effect of the SBL via the Synthetic Control Method, matching on all pre-SBL semi-annual deviations from unit-specific linear trends. We use the NUTS2* regions from Great Britain as our donor pool.

Figure 4 plots the deviations from linear trends of the real Northern Ireland and the synthetic Northern Ireland constructed of 26.1% Cheshire, 21.8% East Anglia, 12.4% East Wales, 21.2% Kent and 18.6% Leicester.

Figure 4: Real and Synthetic Northern Ireland



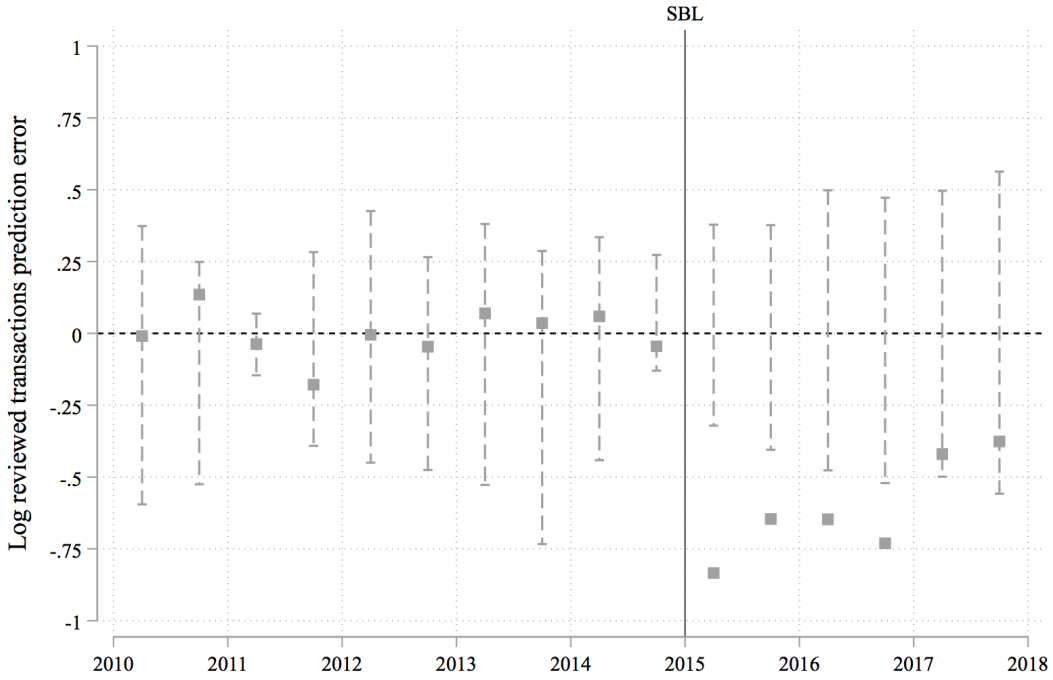
Note: This figure plots the detrended log number of reviewed transactions in Northern Ireland and in the synthetic Northern Ireland constructed from NUTS2* regions in Great Britain over six-month periods.

We assess the performance of the SCM by calculating exact p-values under Fisher's sharp null using a test statistic equal to the ratio of the post-treatment residual mean squared prediction error (RMSPE) to pre-treatment RMSPE. The ratio of the post-treatment RMSPE to the pre-treatment RMSPE is a measure used to assess the meaningfulness of post-treatment deviations between the real Northern Ireland and the synthetic; a large post-treatment deviation is less indicative of a treatment effect if the pre-treatment deviation is similarly large. The exact p-value is the probability that the ratios constructed for the untreated units exceeds that for Northern Ireland. The pre-treatment RMSPE for Northern Ireland is 0.01, the post-SBL RMSPE is 0.52 giving a ratio of 86.32 and an exact p-value of 0.03.

Figure 5 plots the prediction gap, i.e. difference between the synthetic and real Northern Ireland, from Figure 4. The confidence bands are based on the placebo prediction gaps for the 38 other NUTS2* areas, excluding the largest two and smallest two in each period. This is analogous to what was done in Figure 2 above. Constructing these placebos allows us to

measure the degree to which the large gap between the real and synthetic Northern Ireland occurring at the introduction of the SBL is just noise.

Figure 5: Prediction gaps between real and synthetic Northern Ireland and between real and synthetic untreated units for log reviewed transactions



Note: This figure plots the period-specific gap between the real and synthetic Northern Ireland and confidence bands based on the placebo prediction gaps for the 38 other NUTS2* areas.

Comparing this figure to Figure 2 the estimated effects of the SBL are very similar to those obtained from the naive event study in Figure 2 in terms of size and pattern.

Reviewed Transactions Discussion

Our results suggest that there was a pronounced fall in the number of reviewed sex market transactions in Northern Ireland following the introduction of the SBL relative to the counterfactual of Great Britain. The effects we estimate are large. From Figure 5 we have an approximately $100 \times (e^{-0.75} - 11)$ =53% decrease in the number of reviewed transactions in the six months following the introduction of the SBL and an approximately $100 \times (e^{-0.50} - 1)$ =39% decrease in the next 18 months relative to the synthetic. Initially large, the effect seems to be temporary, lasting a couple of years, but decaying over time. Were the effects on the number of actual

transactions, as opposed to the number of reviewed transaction as is the case here, it would be compelling evidence of the SBL being effective in substantially, though temporarily, reducing the size of the sex market.

The outcome here is the number of reviewed transactions, not the number of transactions per se. One explanation for our result is that the introduction of the SBL did not discourage clients from participating in the market but just discouraged clients from leaving reviews, perhaps out of fear of leaving a digital record of the crime. Unfortunately, we cannot test this directly. However, there are reasons to think that the decrease in reviewed transactions does represent, at least in part, a fall in the actual number of transactions.

First, clients and sex workers are incentivized to write reviews following a transaction as the reviews allow them to build a good reputation. Writing reviews, and having reviews written about oneself, builds up a market participant's reputation (Cunningham and Kendall 2016).

Second, clients said they would reduce market participation. Ellison et al. (2019) find that nearly two-fifths of clients surveyed in Northern Ireland reported that the SBL would either lead them to reduce their participation in the market or leave the market entirely. Our results here are consistent with these stated preferences of clients. However, Ellison et al. (2019) find no evidence of a reduction in the number of advertisements placed on websites similar to Platform 2 in Northern Ireland over the period the SBL was introduced. Cameron and Collins (2003) and Della Giusta et al. (2017) both find evidence from surveys of male sex market clients that participation in the market is affected by perceived risks particularly with respect to the social stigma, reputation, and established partnerships.

If the sex market did in fact contract as clients became less likely to participate in the market, then we might expect to see this in crime statistics. As noted, although the exchange of sex for money is legal in the UK, many activities associated with sex work are illegal. If the sex market contracts, then we would expect the instances of such crimes recorded by police to contract as well. To test this we estimate the effect of the SBL on the number of police-recorded instances of exploiting/controlling a prostitute, i.e. 'pimping'). We have data for such crimes for 56 PFAs in Great Britain and and an annual observation for Northern Ireland for 2011 to 2018. Via a simple difference-in-differences estimator with year and PFA fixed effects, as well as the controls in X, we obtain a point estimate of -0.27 (Placebo-β p-value=0.03, Placebo-τ p-value=0.02).

Lastly, the observed fall in fees at the time of the SBL's introduction also lends credence to

the decrease in reviewed transactions reflecting, at least in part, a decrease in actual transactions. The fall in fees per minute and subsequent return to pre-SBL levels corresponds to what we see in the changes in reviewed transactions.

The market may have drifted back to pre-SBL levels as clients learned that the law was difficult to enforce and the real chances of them being caught were smaller than perhaps they had initially thought. Catching and prosecuting sex buyers is difficult. In evidence given to the Home Affairs Committee in their inquiry into prostitution, an Assistant Chief Constable noted that it would be "really, really difficult to police the internet" and that although police could in principle use platform profiles to gather intelligence about sex workers, information on sex clients would require more intrusive and resource-intensive techniques (House of Commons Home Affairs Committee 2016). That same report cites given evidence that the SBL might make sex workers less likely to provide information to police. If both parties deny that money was involved in a sexual encounter, then arrest and prosecution is extremely unlikely. Effectively, the only way to secure a conviction is to elicit an admission of guilt. To date only one person has been successfully prosecuted for purchasing sex in Northern Ireland. Just over a year after the introduction of the law, only 10 men had been investigated by police despite the police-estimated 800 men per day who pay for sex in Northern Ireland (McClafferty 2016). The first conviction didn't come until June 2018 (Newsroom 2018). But, given the changes to Platform 2 in late 2016 discussed above and the introduction of the SBL in the Republic of Ireland in March 2017, this decay is difficult to interpret as strictly a reduction in the effectiveness of the SBL in Northern Ireland.

5 Results for Sexual Health and the Sex Buyer Law

In theory, the direction of the effect of criminalization on STIs is ambiguous and is related to both the effect the new regulatory regime has on the size of the market and the relative bargaining power of participants. If the SBL does reduce the size of the market, then we would expect a fall in the STI rate, all else being equal. However, if the SBL changes the risk profile of clients and/or the riskiness of the activities sex workers engage in, then we would expect a rise in STI rates.

On average, sex workers are more likely to be infected with gonorrhea than the average non-sex worker. Using data on the population of women attending Genitourinary Medicine Clinics

(GUMs) in 2011, McGrath-Lone, Marsh, Hughes and Ward (2014*a*) show that 1.2% of gonorrhea diagnoses are for women who have indicated to the clinic that they have worked as sex workers. There are varying estimates of the total number of sex workers in the UK but we calculate this rate of infection to be between 6 and 26 times what would be expected if sex workers were no more likely to be infected than the population at large.[23] Note that sex workers have a higher infection rate for all sexually transmitted infections. Lower bound estimates for the relative infection rates of sex workers to women in general for other STIs are lower but always greater than 1: 2.9 for chlamydia, 1.73 for herpes and 1.17 for genital warts. For syphilis it is 5.6 but the disease is exceedingly rare with only 3 cases of syphilis among those people acknowledging that they had engaged in sex work in 2011.

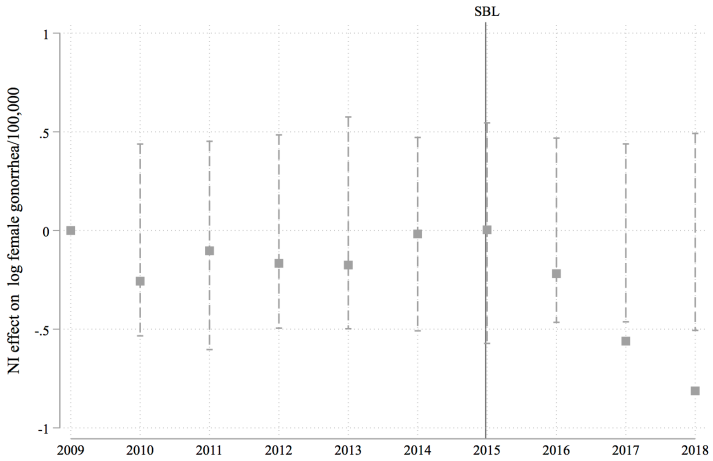
Cunningham and Shah (2018) find no evidence of an association between the the probability of being diagnosed with chlamydia and having ever engaged in sex work in the US for either men or women. Using data from sexual health clinics in England and Wales, McGrath-Lone et al. (2014*a*) find an association between having worked as a female sex worker and gonorrhea infections with weaker associations for chlamydia infections. Similar results are found for male sex workers in McGrath-Lone et al. (2014*b*).

In Figure 6 we present the Northern Ireland effect on the number of gonorrhea infections per 100,000 people and in Figure 7 we present the same for chlamydia infections. These effects are estimated using local authority/local clinic-level data and include both year and local-level fixed effects. The point estimates represent the difference between the log number of infections per 100,000 in Northern Ireland and the mean number of such infections in Great Britain. The dashed lines represent the 95% randomization inference confidence bands based on K=1,000 permutations.

[23] Using figures from McGrath-Lone et al. (2014*a*), estimates of the number of active sex workers ranging from 15,000 to 60,000 (Spiegelhalter, 2015) and estimates from the 2011 census of the number of 32.2 million women in the UK we calculate that between 0.11% and 0.45% of sex workers are infected versus 0.017% of women in general were infected as of 2011. No comparable data are available for Northern Ireland.

Figure 6: Northern Ireland effect on log gonorrhea infections per 100,000

Infections among women



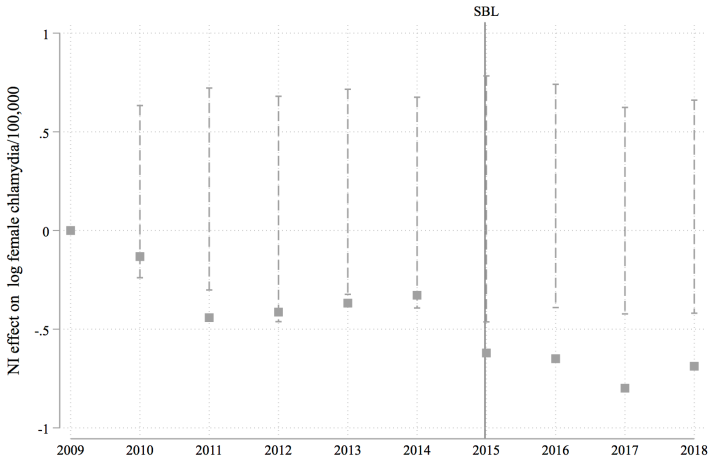
Infections among men

Note: These figures plot the estimated Northern Ireland effect on the log number of gonorrhea infections in women (top) and men (bottom) per 100,000 people. The vertical line marks when the SBL came into effect. The dashed lines represent the 5th and 95th percentiles of the distribution of effects obtained via randomization of the treatment 1,000 times.

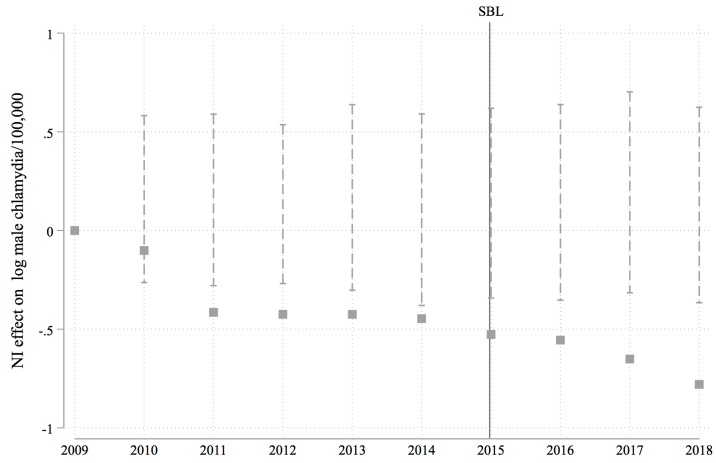
For women, the pre-SBL Northern Ireland did not differ systematically from Great Britain. Following the introduction of the SBL, however, the female infection rate in Northern Ireland fell steadily over time relative to Great Britain. For male infections, we do not see much of a difference over time.

Figure 7: Northern Ireland effect on log chlamydia infections per 100,000

Infections among women



Infections among men



Note: These figures plot the estimated Northern Ireland effect on the log number of chlamydia infections in women (top) and men (bottom) per 100,000 people. The vertical line marks when the SBL came into effect. The dashed lines represent the 5th and 95th percentiles of the distribution of effects obtained via randomization of the treatment 1,000 times.

Chlamydia infections among women are systematically lower than they are in Great Britain in the four years preceding the SBL. Following the introduction of the SBL, we see the chlamydia infection rate among women fall even lower. A similar pattern can be seen for men.

In Table 3 we present results from the estimation of equation (1) for log gonorrhea infections

and log chlamydia infections per 100,000 people.

Table 3: Difference-in-differences estimates of the effect of the SBL on log gonorrhea infection rates of women and men

	(1)	(2)	(3)	(4)
	Panel A: Gonorrhea			
	Females		Males	
ATT of the SBL	−0.25	−0.27	−0.14	−0.13
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.14	0.13	0.26	0.29
RI-τ: $Pr(\lvert\hat{t^j}\rvert > t)$	0.00	0.00	0.00	0.00
	Panel B: Chlamydia			
	Females		Males	
ATT of the SBL	−0.36	−0.36	−0.27	−0.26
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.03	0.03	0.02	0.29
RI-τ: $Pr(\lvert\hat{t^j}\rvert > t)$	0.00	0.00	0.00	0.00
Observations	3,498	3,498	3,498	3,498
Controls	No	Yes	No	Yes
Local and Year FE	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates of the effect of the SBL in Northern Ireland on the log of the number of cases of gonorrhea per 100,000 people. The first number in the first row, −0.25, means that we estimate the SBL lead to a 25% ($1 - e^{-0.27} = -0.22$) decrease in gonorrhea cases in females per 100,000 people in Northern Ireland. Randomization inference is performed on β and τ with 1,000 replications.

Results for gonorrhea are presented in Panel A and for chlamydia in Panel B. In columns (1) and (2) we present the results for log female infections per 100,000 with area and year fixed effects in column (1) and we add the vector of controls, X_{it}, in column (2). We repeat this pattern in columns (3) and (4) for men.

The results suggest that the SBL lead to a reduction in both gonorrhea and chlamydia infection rates among women and men. The estimated effects are large, indicating reductions of $100 \times (e^{-0.27} - 1) = 24\%$ in female gonorrhea infection rates and about $100 \times (e^{-0.13} - 1) = 12\%$ for men. Reductions for chlamydia are estimated as $100 \times (e^{-0.36} - 1) = 30\%$ fall for women and $100 \times (e^{-0.26} - 1) = 23\%$ for men. In general, the randomization inference based on τ suggests these effects are unlikely to be the result of random variation in infection rates, though RI-β offers less clarity on the presence of effects, particularly for men. This means that while the effects estimated for the placeboes are often larger in absolute value than the effect estimated for Northern Ireland, the ratio of effect size-to-precision is largest for Northern Ireland.

The results in Table 3 use the local authorities in Great Britain as the counterfactual to local area in Northern Ireland. We next re-estimate the effect of the SBL using the SCM. We again match on transformed data, but here rather than de-meaning and de-trending the data,

we follow Powell (2018) and use predicted values of the outcome to construct the counterfactual weights. The predictions are obtained by regressing the outcome on a polynomial of time[24] for the pre-treatment period only. Powell notes that this is equivalent 'to an instrumental variable method where the state-specific time trends are used as 'instruments' to predict changes in the outcome variable' (p. 17). This first stage requires the assumption that the time trends, however specified, are predictive of the outcome. The synthetic control is then constructed using weights that minimize the RMSPE using the predicted outcomes in all pre-treatment periods as predictors. While this approach might not globally minimize the RMSPE, i.e. the smallest RMSPE that we could get with these data, in the pre-treatment period, its simplicity and transparency makes it less vulnerable to cherry picking.

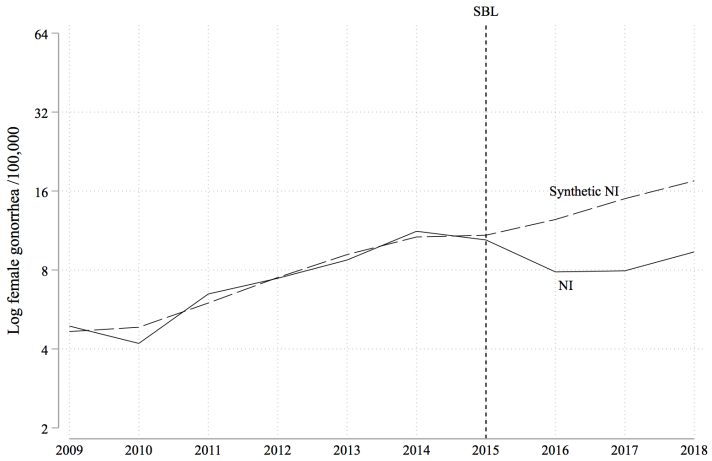
To do so we aggregate our local authority/local clinic-level data to NUTS2 areas, creating untreated units that are comparable to our single treated Northern Ireland unit.

In Figure 8 and 9 we present the annual number of infections per 100,000 people for female and male gonorrhea and chlamydia, respectively. The solid line shows Northern Ireland and the dashed line synthetic Northern Ireland.

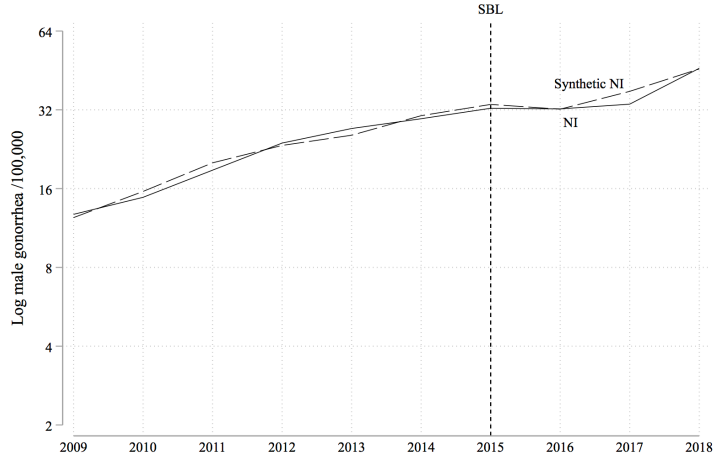
[24] As Ferman and Pinto (forthcoming) notes, there are many possible approaches that could be used to obtain these predicted outcomes, for which the polynomial is only one option. Powell suggests that with longer pretreatment periods, splines might be more appropriate.

Figure 8: Gonorrhea infections per 100,000 in Northern Ireland and Imperfect Synthetic Northern Ireland

Infections among women



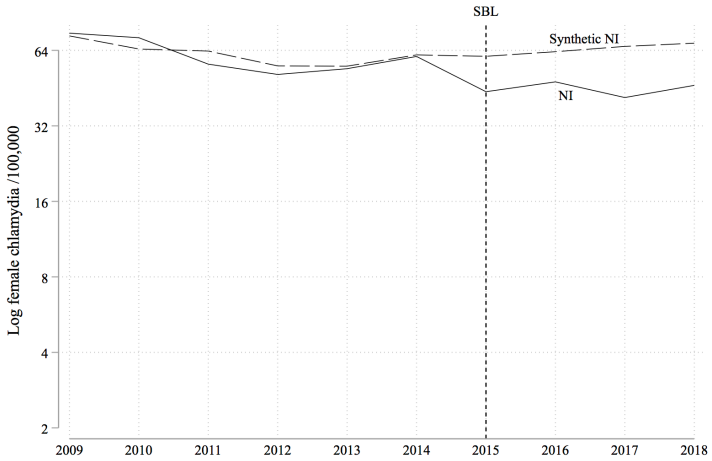
Infections among men



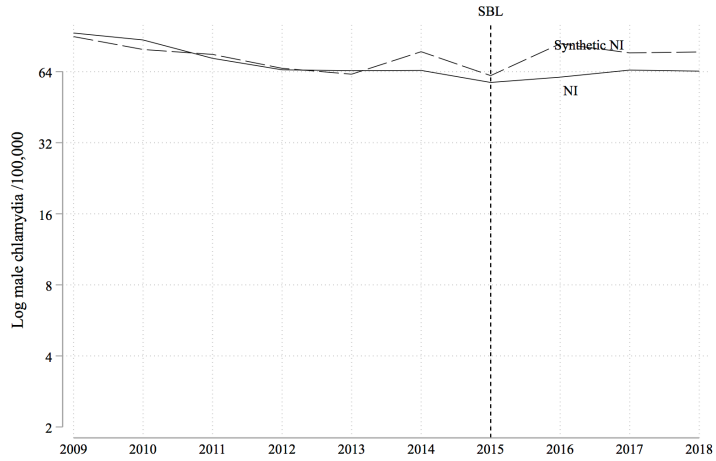
Note: These figures plot the number of gonorrhea infections in women and men per 100,000 people in Northern Ireland and in the synthetic Northern Ireland using the ISCM from Powell (2018). The vertical line marks when the SBL came into effect. The synthetic Northern Ireland for gonorrhea infections in women consists of 28.1% East Yorkshire and Northern Lincolnshire, 9.4% Devon, 5.2% Tees Valley and Durham, and smaller percentages (all <5%) of all the other NUTS2 areas except North Yorkshire. The synthetic Northern Ireland for gonorrhea infections in men consists of 14.7% Greater Manchester, 14.6% West Midlands, 6.1% East Anglia, and smaller percentages (all <5%) of all the other NUTS2 areas.

Figure 9: Chlamydia infections per 100,000 in Northern Ireland and Imperfect Synthetic Northern Ireland

Infections among women



Infections among men



Note: These figures plot the number of chlamydia infections in women and men per 100,000 people in Northern Ireland and in the synthetic Northern Ireland using the ISCM from Powell (2018). The vertical line marks when the SBL came into effect. The synthetic Northern Ireland for chlamydia infections in women consists of of 74.8% Dorset and Somerset, 19.7% South Yorkshire and 5.5% of North Yorkshire. The synthetic Northern Ireland for chlamydia infections in men consists of 67.8% Cumbria and 32.2% South Yorkshire.

We assess the performance of the SCM, by calculating exact p-values under Fisher's sharp null using a test statistic equal to the ratio of the post-treatment RMSPE to pre-treatment RMSPE, summarized in Table 4. The ratio of the post-treatment RMSPE to the pre-treatment

RMSPE is a measure used to assess the meaningfulness of post-treatment deviations between the real Northern Ireland and the synthetic; a large post-treatment deviation is less indicative of a treatment effect if the pre-treatment deviation is similarly large. The exact p-value is the probability that the ratios constructed for the untreated units exceed that for Northern Ireland.

Table 4: Ratio of post-treatment RMSPE to pre-treatment RMSPE

	(1)	(2)	(3)	(4)
	Pre-RMSPE	post-RMSPE	Ratio	p-value
Female gonorrhea	0.01	0.25	44.72	0.07
Male gonorrhea	0.00	0.00	1.69	0.51
Female chlamydia	0.01	0.14	25.35	0.08
Male chlamydia	0.01	0.04	5.66	0.22

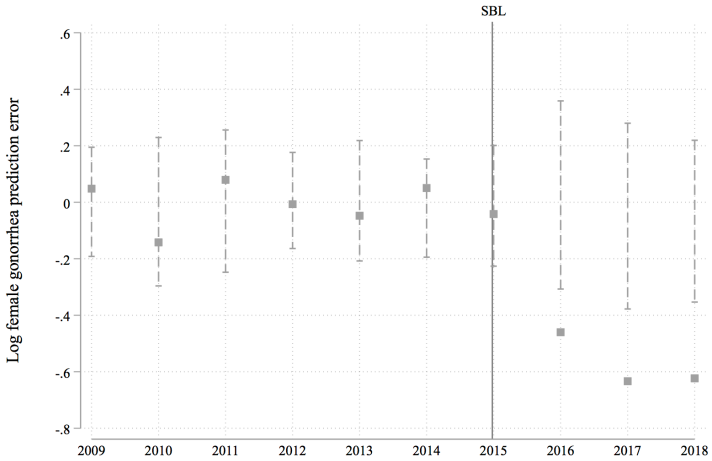
Notes: This table presents diagnostics for the ISCM analysis in the form of the pre- and post-treatment RMSPE, the ratio of the two, and the exact p-value giving the probability that the ratios constructed for the untreated units exceed that for Northern Ireland.

For female gonorrhea, the RMSPE ratio for Northern Ireland is the second largest giving a p-value of 0.07. For female chlamydia, the ratio is the third largest. For males, the pre-treatment fits are as good or better than for females, though there is little post-treatment deviation between Northern Ireland and the synthetic. The ratios for male gonorrhea and chlamydia are smaller and closer to the middle of the distribution of the ratios for the placeboes.

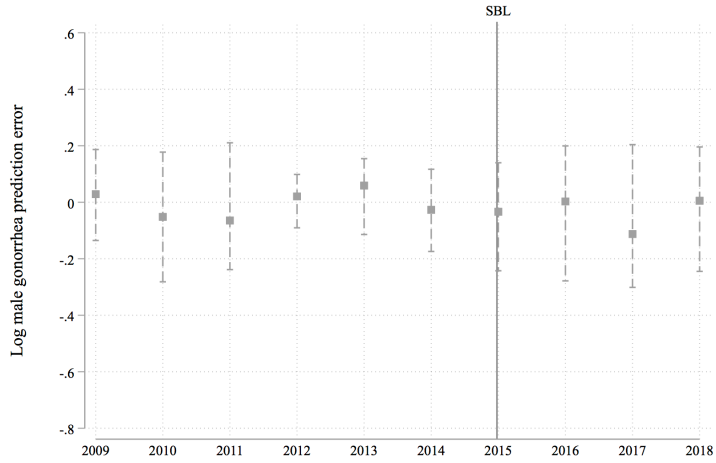
Figures 10 and 11 plot the prediction gap, i.e. difference between the synthetic and real Northern Ireland, from Figures 8 and 9. The confidence bands are based on the placebo prediction gaps for the 38 other NUTS2* areas, excluding the largest two and smallest two prediction gaps in each period. Constructing these placebos allows us to measure the degree to which any gap between the real and synthetic Northern Ireland is just noise.

Figure 10: ISCM prediction gap for gonorrhea infections per 100,000

Women



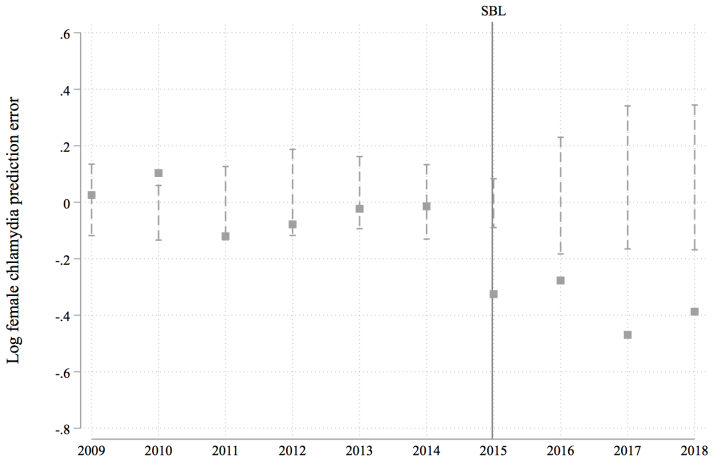
Men



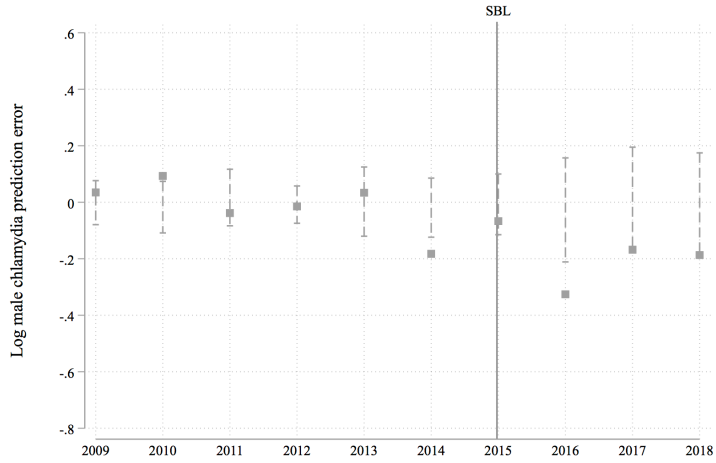
Note: These figures plot the prediction gaps for each NUTS2 region and its synthetic counterfactual for the number of gonorrhea infections in women and men per 100,000 people. The vertical line marks when the SBL came into effect.

Figure 11: ISCM prediction gap for chlamydia infections per 100,000

Women



Men



Note: These figures plot the prediction gaps for each NUTS2 region and its synthetic counterfactual for the number of chlamydia infections in women and men per 100,000 people. The vertical line marks when the SBL came into effect.

A comparison with Republic of Ireland

Our results suggest that the SBL lead to a large decrease in the female gonorrhea infection rate with weaker evidence of an effect on female chlamydia infections or male STI rates in Northern Ireland. The counterfactual we use is areas in Great Britain, in the DiD, or some weighted combination thereof, in the SCM. We are interested in what happened to gonorrhea

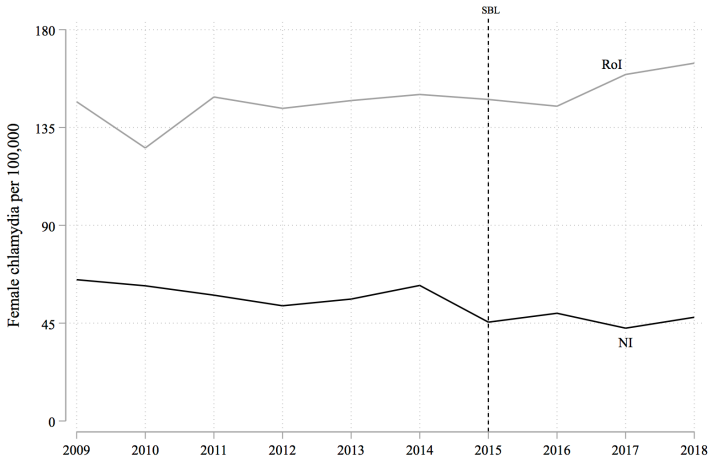
and chlamydia infection rates in the Republic of Ireland. Our results could plausibly be explained by an outbreak of STIs in Great Britain the spread of which was limited in Northern Ireland by the physical separation of the to regions by the Irish Sea. In Figures 12 and 13 we plot the gonorrhea and chlamydia infection rates for women and men, respectively, in Northern and Republic of Ireland.

Figure 12: Gonorrhea infection rate in Northern and Republic of Ireland

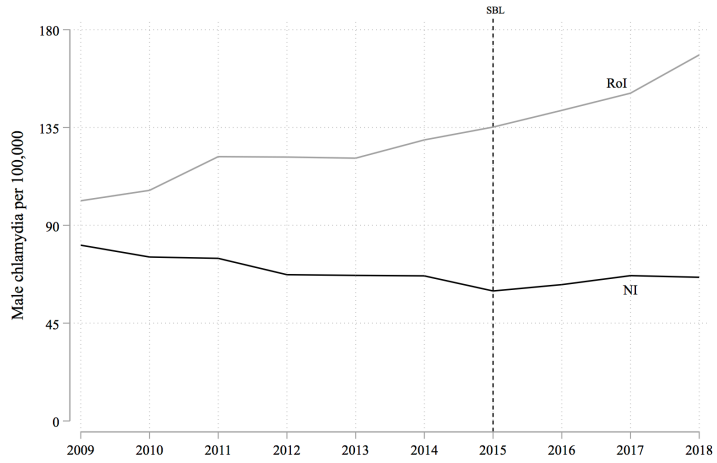
Note: This figure plots the gonorrhea infection rates for women and men in Northern and Republic of Ireland between 2009 and 2018. Data from the Republic of Ireland is taken from HSE Health Protection Surveillance Centre report *Gonorrhoea in Ireland* for 2017 and 2018.

Figure 13: Chlamydia infection rate in Northern and Republic of Ireland

Women



Men



Note: This figure plots the chlamydia infection rates for women and men in Northern and Republic of Ireland between 2009 and 2018. Data from the Republic of Ireland is taken from HSE Health Protection Surveillance Centre report *Gonorrhoea in Ireland* for 2017 and 2018.

The evolution of infections among both men and women in the Republic of Ireland followed a pattern more similar to that in Great Britain. This suggests that the estimated effect above is indeed a Northern Irish, as opposed to an island of Ireland, effect.

STI Discussion

The introduction of an SBL might affect STIs via two main channels. First, the SBL may cause the market to contract, an effect for which we provide evidence above. A smaller market for sexual services will lead to a decrease in STIs, all else being equal. Second, the SBL might lead to sex workers engaging in riskier activities, e.g. unprotected sex with clients, perhaps due to the reduction in demand caused by the law. An increase in the riskiness of the activities engaged in will lead to an increase in STIs, all else being equal. The net effect of the SBL on STIs is an empirical question.

The above results suggest the SBL in Northern Ireland led to a decrease in the rate of sexually transmitted infections among women with evidence of the effect for men being much weaker. Note that among men, a large proportion (72% in Northern Ireland in 2018) of the infected are men who have sex with men (MSM), a group that is less engaged with the market for sexual services. We have data on the number of infections for MSM, though only at the regional (NUTS1) level and only for a subset of years for English regions. In Northern Ireland, 60% of the gonorrhea infection of men between 2009 and 2018 are for MSM. Between 2015 and 2018, the gonorrhea infections among MSM increased by 65% but remained stable for other men there.

Though the SBL came into effect in June 2015, we do not see any evidence of a reduction in the gonorrhea infection rate among women in that year. A delayed effect was also found in Cunningham and Shah (2018), where a reduction in the infection rate was not realized until a year and a half after the decriminalization of indoor prostitution in Rhode Island (see: Figure 2, p. 1692).

Looking at Figure 10 we see that the female gonorrhea infection rate is ultimately about $e^{-0.50} - 1 = 39\%$ lower than it would have been in the absence of the SBL and female chlamydia rates about $e^{-0.40} - 1 = 33\%$. Cunningham and Shah (2018) find that decriminalization led to the rate of gonorrhea infections to fall by about 75% for females and 35% for males. Here we find a similar decrease in infections but following criminalization. However, in their evaluation of the effects of the recriminalization of the sexual services market, Cunningham and Shah (2018) find negative point estimates for the effect on female gonorrhea infections, though statistical evidence of the effect is lacking; their point estimates for males are effectively 0. These results are very similar to our own. Cameron et al. (2020) also find evidence that criminalization of

the sex market reduced STI rates in the general population, though rates increased among sex workers.

Cunningham and Shah (2018) argue that their result is driven by an decrease in risky sexual practices, for which they produce evidence, which itself might be the result of the decriminalization moving sex work indoors. By 2015, the Northern Irish sex market was already almost entirely indoors and therefore an increase in risky practices due to the move away from permissiveness might not be as strong as it would be were it to move the sex market back outdoors. Unfortunately, we are unable to estimate the effect of the SBL on the activities undertaken during a transaction. Ellison et al. (2019) present some descriptive evidence that instances of clients removing condoms without consent (a crime) increased in the years following the introduction of the SBL. They also present anecdotal evidence that clients became more likely to request unsafe sexual practices or sexual practices that sex workers were not willing to perform. Such changes would likely result in higher rates of STIs.

The size of the effect we estimate is large. Huschke et al. (2014) estimate that there were approximately 325 sex workers working on a given day in Northern Ireland in 2014. If we assume the rate of infection was equal to that in England, on average 0.27%, there would be only one sex worker with gonorrhea. We might instead use an infection rate of 2.5 per cent, more in line with other studies for example 2.6% in the Netherlands (Verscheijden et al. 2015), 6.4% in USA border towns (Loza et al. 2010), 3.7% in Mexico City (Uribe-Salas et al., 1997), 8 per% in Iran (Nasirian et al. n.d.), 2.3 to 4.5% in Australia (Seib et al. 2009; Callander et al. 2018), 2.2% in the UK (Platt et al. 2011) and 2.7% of female sex workers attending genitourinary medicine (GUM) clinics (McGrath-Lone et al. 2014*a*). In the UK, Groom and Nandwani (2006) find a 7% infection rate among male clients and Ward et al. (2004) find a rate of 4.5% for 1985-1992 and 1.1% for 1996-2002. Bremer et al. (2016) find a 3.2% infection rate among female sex worker in Germany. With an infection rate of 2.5%, there would have been approximately 8 infected female sex workers in Northern Ireland. Say the SBL reduced female gonorrhea infections by 30% or about 3 per 100,000 or about 21 total female infections per annum.[25] If all infected sex workers become uninfected as a result of the SBL, it explains only about 40% of our result. This is similar to the results in Cunningham and Shah (2018) who also find rather large effects. For the entire effect to be explained by changes to sex workers' health, the pre-treatment infection

[25] Using a population of 1.5 million adults in Northern Ireland.

rate among sex workers would need to have been about 8% given an estimated 325 sex workers. Alternatively, there would need to be 855 female sex workers in Northern Ireland, given an estimated rate of infection of 0.25% (based on data from England (McGrath-Lone et al., 2014*a*). Both numbers are in excess of existing estimates.

There may be other unobserved time-varying effects that might explain our results. In communications we have had with the Northern Irish Public Health Agency to discuss alternative explanations, they mentioned that in 2013 a new sexual health campaign was approved and money appropriated for it. The campaign launched a year later. Such a campaign might also negatively effect infection rates in Northern Ireland relative to Great Britain. However, it is not clear why such a campaign would only affect infection rates among females.

6 Results for Sexual Violence and the Sex Buyer Law

Our objective here is to measure the effect of the SBL in Northern Ireland on sexual violence. The prevalence of sexual violence is difficult to measure because the actual number of crimes committed is unobservable. As such, a great deal of research on crime, including studies of the market for sex, use data based on police-recorded crimes. Cunningham and Shah (2018) and Ciacci (2020) use the number of rapes recorded, i.e. submitted in official statistics. Bisschop et al. (2017) consider both rape and sexual abuse crimes recorded by police. But variation in sexual violence crimes recorded by police may or may not reflect variation in the actual number of such offenses occurring because some crimes go unreported to police and some reported crimes go unrecorded by police.

For a sexual violence to be recorded by police, a sexual violence crime must first occur. Let N_{it} be the number of sexual violence crimes committed. The crime must then be reported to police, but many committed crimes go unreported. Less than 20% of victims of rape or sexual assault by penetration report the offense to police (Office of National Statistics 2018). Let π^{Report} be the probability that a committed crime is reported to the police.[26] Lastly, even if reported to the police the crime may or may not be recorded by the police. There are guidelines for when and how police should record a reported crime, codified in the Home Office Accounting Rules, that leave room for interpretation on the part of the officer charged with recording a

[26] Kelly et al. (2005) finds that the proportion of reported cases of sexual violence that are false is likely about 3%.

reported crime.[27] Let π^{Record} by the probability that a reported crime is recorded by police. The number of police recorded sexual violence crimes, R_{it} is then given by:

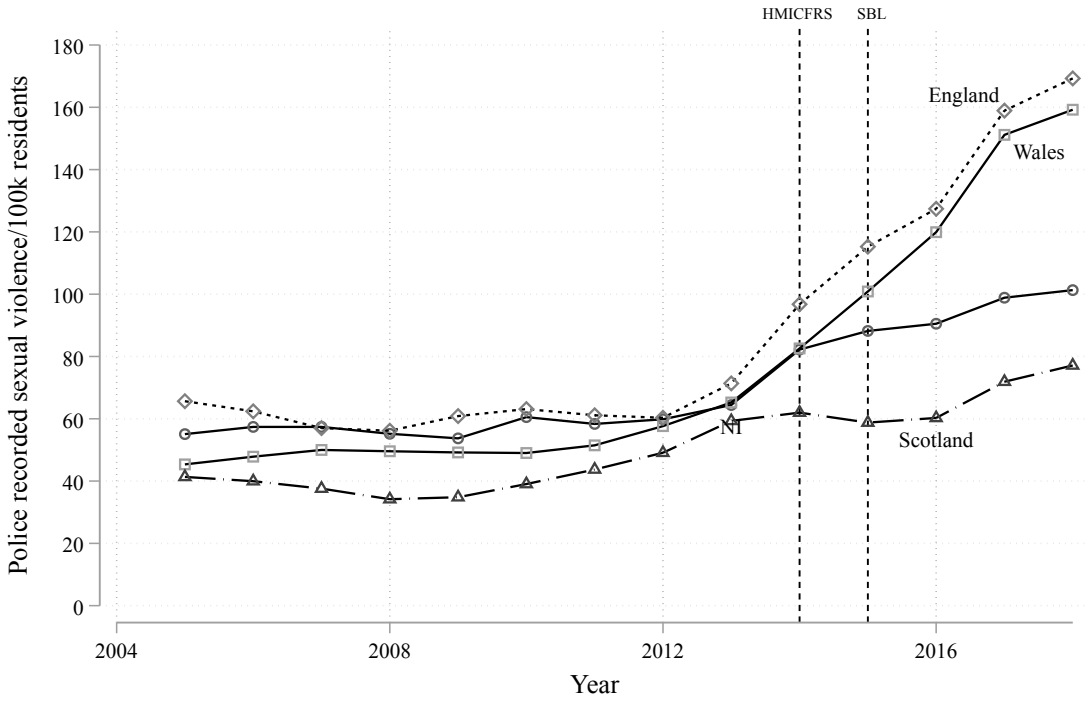
$$R_{it} = N_{it} \times \pi^{Report} \times \pi^{Record} \tag{3}$$

Our interest is in the effect of the SBL on the number of sexual violence crimes committed. What we are able to observe, however, is the number of sexual violence crimes recorded by police. To identify the former from the latter, we must use police-recorded crime and make assumptions about the sub-scripts on π^{Report} and π^{Record}. If π^{Report} and π^{Record} are assumed to be fixed, then changes in R_{it} reflect changes in N_{it}. However, if π^{Report} and π^{Record} vary over PFAs and time, i.e. should they have i and/or t subscripts, and are unobserved, as is generally the case, then inference about N_{it} based on analysis on R_{it} is more complicated.

In Figure 14 we plot police-recorded sexual violence, the sum of sexual assaults and rapes perpetrated against women, per 100,000 residents in England, Wales, and in Northern Ireland between 2005 and 2018.

[27] The National Crime Recording Standard flowchart has seven questions that must be answered in the affirmative about a reported crime before police are recommended to record it including 'On the balance of probabilities has a notifiable crime been committed?'

Figure 14: Sexual violence perpetrated against women recorded by police, 2005-2018



Note: This figure plots the number of police-recorded instances of sexual violence per 100,000 residents in England, Wales, Scotland, and Northern Ireland. The vertical line in 2014 marks the year the HMIFCRS report published and the vertical line in 2015 marks the year the SBL was introduced in Northern Ireland.

The number of police-recorded instances of sexual violence, R_{it}, is relatively stable in the three countries until 2013. At that point police-recorded sexual violence increases annually in each country. After 2015 both England and Wales outpace Northern Ireland. The question is, to what degree does this observed increase in police-recorded sexual violence reflect an increase in actual sexual violence, N_{it}, an increase in the propensity to report sexual violence, π_{it}^{Report}, and the propensity for the police to record reported sexual violence, π_{it}^{Record}.

There is reason to think that the propensity for police in the United Kingdom to record reported crimes has changed in recent years. In 2014, Her Majesty's Inspectorate of Constabulary and Fire and Rescue Services (HMICFRS) published a report titled *Crime Recording: Making the Victim Count* that found that crimes were being systematically under-recorded by the police to varying degrees across PFAs. The report found that only 81% of reported crimes that should have been recorded are actually recorded by the police. There was variation in the recording rate across PFAs and also across crime type with sexual offenses being one of the most under-

recorded crimes with recording rates closer to 70%. While the HMICFRS formally reports on the work of police services in England and Wales, the HMIC (but not the FRS) review and report on the PSNI as well on a non-statutory basis at the invitation of the Secretary of State for Northern Ireland. In preparing the 2014 report, HMICFRS completed inspections of the recording practices of the PSNI, though the PSNI does not feature in the 'Making the Victim Count' itself (see p. 111), a report was published separately (HMIC 2015). This report found that about 90% of crimes that should have been recorded were indeed recorded by the PSNI and a similar rate for sexual offenses, though the sample size was very small.

The report made several practical recommendations for how the Home Office and police forces can improve the recording of crimes. In their annual review of crime statistics, the ONS notes that the report 'led to police forces reviewing and improving their recording processes' (Office for National Statistics 2018). The ONS also notes that increases in the number of police-recorded crimes are 'largely because of recording improvements' and that 'the effect has been particularly pronounced for some types of violence such as sexual offenses' (ONS, 2019).

The timing of the HMICFRS reports complicates our analysis by introducing a unobserved, time-varying confounder via a t subscript on π_{it}^{Record}. Given the lower recording rates in England and Wales as of 2014, it is possible that the number of recorded instances of sexual violence in England and Wales rose faster than in Northern Ireland, all else being equal. As a result we expect a negative bias in our difference-in-differences estimator of the effect of the SBL.

An alternative source of data on sexual violence in the UK comes from annual crime surveys. The Crime Survey of England and Wales (CSEW) is an annual survey of approximately 40,000 people.[28] The Northern Ireland Safe Community Survey, called the Northern Ireland Crime Survey until 2017, is an annual survey of about 2,500 people in Northern Ireland.[29] In Scotland, the Scottish Crime and Justice Survey (SCJS) serves the same purpose though there have not been surveys every year.[30] These surveys ask respondents about their experiences of crime, the justice system, and perceptions of public safety. The surveys are generally comparable, but unfortunately we were not granted access to data on sexual violence from the NISCS due to

[28] The CSEW was called the British Crime Survey until 2011. The sample size varies between 33,299 in 2014 and 47,796 in 2005. The most recent wave had 34,163 respondents.

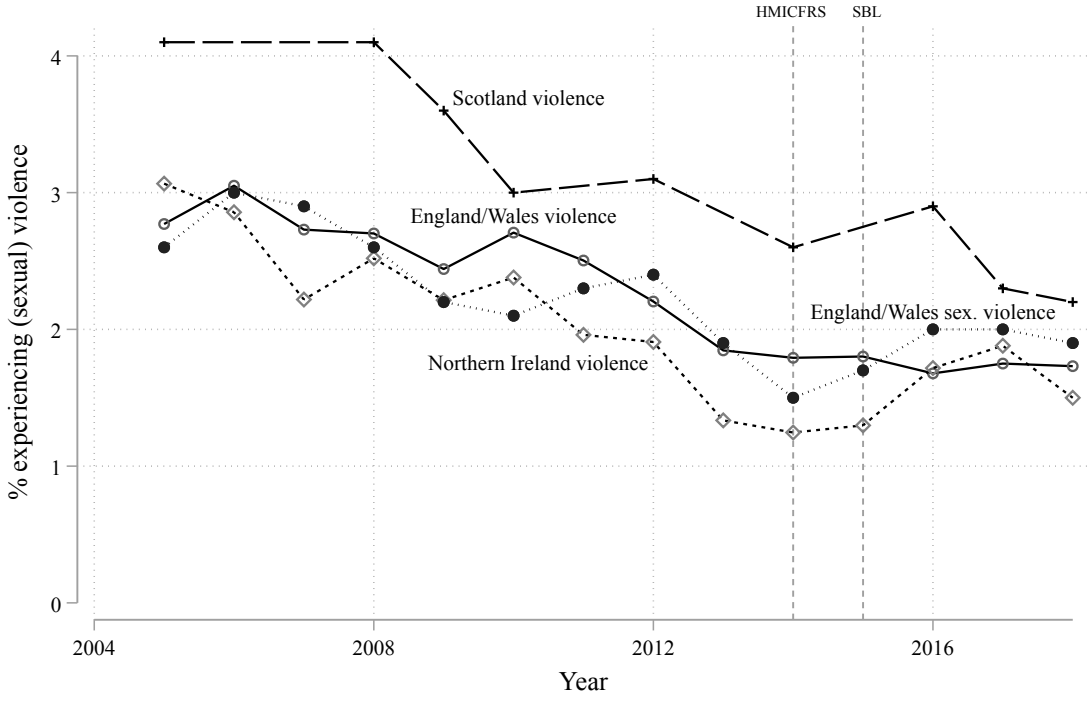
[29] The sample size for the NISCS varies between 1,582 in 2017 and 4,182 in 2011. The 2018 wave had 3,429 respondents.

[30] The SCJS took place in 1993, 1996, 2000 and 2003 (when it was called the Scottish Crime Survey), 2004, 2005 (though this report has since been mostly withdrawn due to errors), 2008-2010, 2012, 2014, 2016-2018.

small samples.[31] These surveys can help us understand the extent to which the rise in police recorded sexual violence is driven by changes in N_{it}, π_{it}^{Report}, and π_{it}^{Record}.

In Figure 15, we plot the proportion of all respondents in the CSEW, SCJS and NISCS indicating that they had been victims of violence. We also plot the percentage of respondents in the CSEW who were victims of sexual violence in England/Wales.

Figure 15: Percentage of respondents to the CSEW, SCJS and NISCS indicating that they were victims of violence or sexual violence, 2005-2018



Note: This figure plots the annual percentage of respondents to the CSEW (formerly BCS), SCJS and NISCS (formerly NICS) indicating that they were victims of violence and sexual violence (for England/Wales only). The vertical line in 2014 marks the year the HMICFRS report was published and the vertical line in 2015 marks the year the SBL was introduced in Northern Ireland.

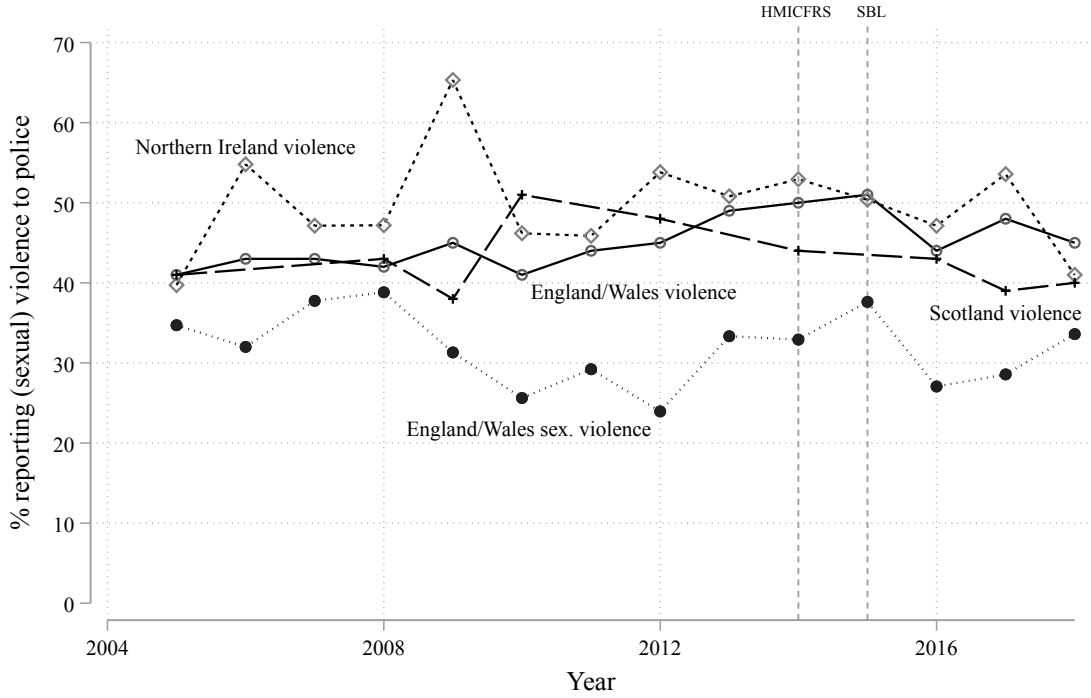
There is a general downward trend in violence in England/Wales and in Northern Ireland with a leveling off from about 2013 as well as a very similar pattern in sexual violence in England and

[31] A FoI request (Reference number: FoI 19 64) submitted to the Northern Irish Department of Justice (NIDOJ) for the annual number of respondents in NISCS that reported being raped in the last year was unsuccessful. The reason cited for rejecting the FoI request was the very small number of respondents. The NIDOJ in noted that 'we can advise that for the survey years 2011/12 - 2017/18 the total number of respondents who indicated the force or violence used on them was rape, attempted rape or sexual assault was in the range of 1 - 15'. Even if we did have access, the data would be of limited value since the NISCS currently only asks about sexual offenses in a domestic context.

Wales. There is no increase after 2014 that is comparable to the rise in police-recorded crimes in Figure 14.

In 2013, the Metropolitan Police Service published *Giving Victims a Voice*, a summary of their investigation into sexual abuse allegations against Jimmy Saville and others. It has been hypothesized that the 'high-profile coverage of this operation and the police response to it are likely to have increased the willingness of victims to come forward to report such offences, both current and non-recent' (ONS 2016, p. 14). In Figure 16 we plot the percentage of respondents in the CSEW, SCJS and NISCS that were victims of violence or, for CSEW only, sexual violence and who ultimately reported the crime to the police. This is a measure of π_{it}^{Report}.

Figure 16: Percentage of respondents to the CSEW, SCJS and NISCS indicating that they reported violence or sexual violence to the police, 2005-2018



Note: This figure plots the annual proportion of respondents to the CSEW (formerly BCS), SCJS and NISCS (formerly NICS) indicating that they were victims of violence and sexual violence (for England/Wales only) and that the crime was reported to to police. The vertical line in 2014 marks the year the HMICFRS report was published and the vertical line in 2015 marks the year the SBL was introduced in Northern Ireland.

The propensity to report violence appears fairly stable over time in both England/Wales and Northern Ireland with no steady trend up or down. The propensity to report any crime (not

shown) is also flat in both England/Wales and Northern Ireland over the period. The propensity to report sexual violence is also stable over time in England/Wales. There is again no increase after 2014 that would explain the rise in police-recorded crimes in Figure 14.

The surveys offer some evidence as to changes in π^{Report} and N. First, sexual violence was flat or falling in England/Wales over the observed period. Non-sexual violence followed a very similar pattern in both England/Wales and Northern Ireland. Second, the propensity for victims of such crimes to report them to the police is markedly stable over the same period, and perhaps even falling over more recent years, despite events that may plausibly have increased this propensity. Taken together these patterns are consistent with changes to π^{Record} that lead to the increase in recorded sexual violence. In the case of Northern Ireland, because we do not have survey data on sexual violence we can only conclude that it is either the number of actual sexual violence crimes being committed, the propensity for police to record crimes, or that both have increased in Northern Ireland since the HMICFRS report was published.

Were we to estimate equation (1) with police-recorded instances of sexual violence on the left-hand side, the estimated effect of the SBL, β^R, would conflate changes in N, π^{Record}, and π^{Report}. In order to interpret β^R as an approximation of β^N we must, as any researcher using police-recorded crime data, make identifying assumptions about π^{Report} and π^{Record}. Namely, to ensure counterfactual trends are parallel and shocks are common either π^{Record} and π^{Report} are stable over time or that changes in π^{Report} and π^{Record} are common to treated and untreated units.

In our case, the evidence presented above suggests such assumptions about π^{Report} likely hold, but not about π^{Record}. If π^{Record} changes over time, which the evidence suggests it does, and it may not change in parallel in Great Britain and Northern Ireland, which the evidence presented suggests it does not, then $\beta^R \neq \beta^N$.

We use two estimators to control for this unobserved, time-varying heterogeneity, allowing us to interpret β^R as a change in N. First, we estimate a factor model using the interactive fixed-effects estimator of Bai (2009) to control for common factors and unit-specific factor loadings. The additional identifying assumption, beyond the standard ones for difference-in-differences, is that the unobserved heterogeneity is captured by $\lambda_i' f_t$ and there is no temporal variation in the factor loadings.

Factor models allow for a flexible error structure which nests other commonly-used structures

like unit-specific time trends and unit cluster-time fixed effects. Factor models allow us to control for unobservable heterogeneity, beyond what can be controlled for with two-way fixed effects, without making specific assumptions as to the nature of that heterogeneity, e.g. unit-specific linear trends. Factor models flexibly allows us to control for unit-specific responses to common temporal shocks.

For example, in 2014 a government report was published criticizing British police forces for the manner in which they recorded reported crimes, i.e. a shock. But how PFAs responded likely varied across the country, i.e. unit-specific responses to that shock. We discuss this in much greater detail below. In the case of STIs, there has been a large increase in the rate of gonorrhea infection in Europe and the USA over the past decade or so. Between 2013 and 2017, the number of confirmed gonorrhea cases in Europe increased by 51% (ECDC, 2017). In the USA, the number of cases increased 75.2% between 2009 and 2017 with an 18.6% spike in the last year (CDC, 2018). The increasing prevalence of gonorrhea across the USA and Europe may have differential effects on infection rates in different regions of the UK due to variation in, say, the number of people traveling internationally to and from each region or the number of men having sex with men since almost half of the rise in Europe was in this community (ECDC 2019).

More formally, we redefine f_t as $1 \times r$ vector of time-specific factors and λ_i as a $1 \times r$ vector of unit-specific factor loadings so that to include additive time and unit fixed effects $f_t = (1,\ f_{t1},\ f_{t2}, ...,\ f_{tr})'$ and $\lambda_i = (\lambda_{i1},\ 1,\ \lambda_{i2}, ...,\ \lambda_{ir})'$.

These models can be estimated in large N and T settings via an augmented equation (1) with cross-sectional means of y_{it} and X_{it} (Pesaran 2006) or by estimating the common factors and loadings directly (Bai 2009) via an iterative process in which both the regression coefficients and $\lambda_i' f_t$ are estimated. The advantage of this approach is that we can allow for potential omitted variables to be correlated with ϵ_{it} provided they are captured by $\lambda_i' f_t$ and there is no temporal variation in the factor loadings. We use Bai (2009) in the analysis that follows.[32] The disadvantage of Bai's estimator is that we need to know the number of factors, r, to include. Bai and Ng (2002) and Moon and Weidner (2015) propose tests for the number of factors but the tests are 'fragile' (Gabillon and Magnac 2016) and in practice they have proven to be uninformative (Bailey et al. 2016; Kim and Oka 2014; Totty 2017). Results in Moon and

[32] Bai's (2009) estimator is $\sqrt{NT}$-consistent for the regression coefficients ($\sqrt{N}$-consistent for the factors). Pesaran's (2006) estimator is $\sqrt{N}$ consistent for the regression coefficients.

Weidner (2015) suggest that including too many factors will not result in any bias or a loss of precision, consistent with results in Bai (2009), Kim and Oka (2014) and Pesaran and Tosetti (2011). They show that factor estimators perform well even if there are no common factors (i.e. 'too many' factors are included) and is consistent with simulation results in Gabillon and Magnac (2016). Including too few factors means there will be unobserved heterogeneity remaining in the error term and the estimator will be biased. We estimate a factor model with a treatment dummy, which is the approach that performs best in terms of bias in Gabillon and Magnac (2016)'s simulations, with the number of factors $r \leq 8$.

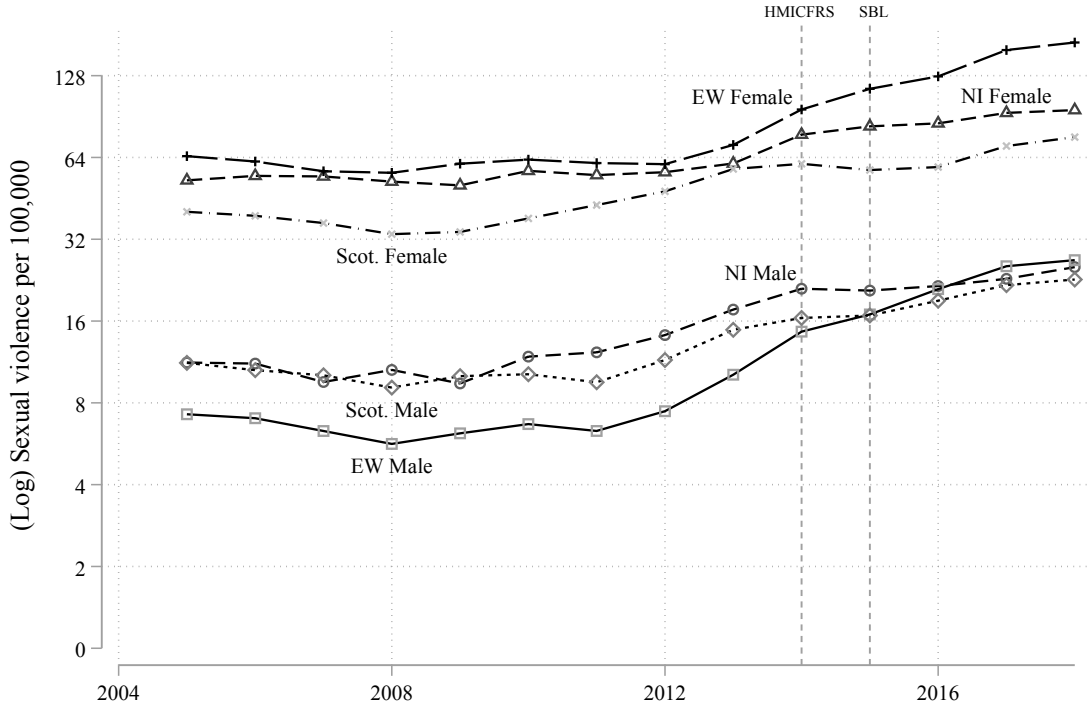
Second, we use a triple difference-in-differences (TDiD) approach which identifies the effect of the SBL under a different set of identifying assumptions. To do so we use data on police recorded sexual violence committed against males. The TDiD estimator controls for two types of potentially confounding time-varying unobservables: first, common changes in recorded sexual violence across PFAs in Great Britain and Northern Ireland; second, changes in recorded sexual violence that are common across genders within Great Britain and within Northern Ireland. The additional identifying assumptions are: first that sexual violence perpetrated against males was not affect by the SBL in Northern Ireland, i.e. $P(N^{Male}|do(SBL) = 0$; second, that $\Delta\pi^{Report}_{Male,i} = \Delta\pi^{Report}_{Female,i}$ and $\Delta\pi^{Record}_{Male,i} = \Delta\pi^{Record}_{Female,i}$. In words, this second assumption means that the change in the probability that sexual violence perpetrated against a male in PFA i is reported is equal to the change in the probability that sexual violence perpetrated against a female in PFA i is reported, and that the change in the probability that reported sexual violence perpetrated against a male in PFA i is recorded is equal to the change in the probability that reported sexual violence perpetrated against a female in PFA i is recorded.

There are reasons to think the SBL is unlikely to affect sexual violence perpetrated against men. The vast majority of the sex market is male clients paying for the services of female sex workers. Moreover, the male clients of male sex workers seem to differ from male clients of female sex workers. Jamel (2011) finds little evidence of sexual violence committed against male sex workers, whereas such violence among female sex workers is much more common (Farley and Barkan 1998). Della Giusta et al. (2018) find that 'clients of [female] sex workers tend to be risk-takers' (p. 22), Jamel also found that male clients of male sex workers tended to be 'non-confrontational.' They may therefore be less likely to substitute sexual violence for purchasing sexual services than male clients of female sex workers, the hypothesized mechanism

through which decriminalization lead to decreases in sexual violence in Bisschop et al. (2017) and Cunningham and Shah (2018), and through which an SBL lead to increased sexual violence in Ciacci (2018). We estimate the effect of the SBL on sexual violence perpetrated against men using the interactive fixed-effects model (results reported in the Appendix) and do not find evidence of an effect.

In Figure 17 we plot the number of recorded instances of sexual violence committed against men and women in England/Wales, Scotland, and Northern Ireland.

Figure 17: Police-recorded sexual violence by victim gender in England/Wales and Northern Ireland



Note: This figure plots the number of police-recorded instances of sexual violence per 100,000 residents in England/Wales and Northern Ireland. The vertical line in 2014 marks the year the HMICFRS report was published and the vertical line in 2015 marks the year the SBL was introduced in Northern Ireland.

To estimate the TDiD model we restructure our data, stacking it so that each row is a PFA-Year-Gender of victim so we now have two observations for each PFA-Year, one for sexual violence perpetrated against men and a second for sexual violence perpetrated against women. We can then estimate the effect of the SBL on sexual violence perpetrated against women using triple difference-in-differences by augmenting equation (1) as:

$$y_{git} = \beta D_{git}^{TDiD} + X_{it}'\theta_1 + \phi_1 NI_i \times Female_g + \phi_2 Post_t \times Female_g + \phi_3 NI_i + \phi_4 Post_t + Female_g + f_t'\lambda_i + + \nu_{git} \quad (4)$$

where y_{git} is the number of instances of sexual assault or rape perpetrated against gender $g = Female, Male$ in PFA i in year t. $Female_g$ equals 1 if the observation pertains to crimes perpetrated against women and $D_{git}^{TDiD} = NI_i \times Post_t \times Female_g$ and the effect of the SBL on sexual violence perpetrated against women is β. The rest of the variables and parameters are as defined above including $\lambda_i = \left[\begin{smallmatrix} \alpha_i \\ 1 \end{smallmatrix}\right]$ and $f_t = \left[\begin{smallmatrix} 1 \\ \psi_t \end{smallmatrix}\right]$. The TDiD estimate of the ATT is given by β

We present our results in Table 5.

In column (1) to (4) we estimate the effect of the SBL on sexual assaults (columns (1) and (2)) and rape (columns (3) and (4)) perpetrated against women via difference-in-differences (equation (1)). In columns (1) and (3) we include only time and PFA or district fixed effects and in columns (2) and (4) we add controls: PFA/district-level population, unemployment rate, economically active rate and log mean household income plus the log number of full-time equivalent police officers in each PFA or district. The coefficients suggest a large negative effect of the SBL on the order of a 26% decrease in sexual assaults and a 37% decrease in rapes.

However, as discussed above, the difference-in-difference estimator conflates changes in the propensity for victims to report crimes and the propensity for police to record crimes with changes in actual sexual violence. Given the evidence presented above, the negative effects presented in columns (1)-(4) are likely due to the propensity for police to record crimes increasing more quickly in Great Britain than in Northern Ireland, i.e. the results are driven by an unobserved time-varying confounder in π_{it}^{record}.

In columns (5)-(8) (odd columns without controls, even columns with controls) we present results from the estimation of equation (1) via Bai's (2009) interactive fixed-effects estimator with 8 common factors. We show results for other numbers of factors below in Table 6. Once we account for common shocks and unit-specific responses the effect of the SBL on sexual assault, columns (5) and (6), becomes positive suggesting the SBL increased sexual assaults by 15 to 20% with empirical p-values of about 0.02. We do not find compelling evidence of an effect on rape (columns (7) and (8)).

Table 5: Difference-in-differences estimates of the effect of the SBL on log police-recorded sexual violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	DiD OLS				DiD Interactive Fixed Effects				Triple DiD OLS			
	Sexual assault		Rape		Sexual assault		Rape		Sexual assault		Rape	
ATT of the SBL	−0.26	−0.26	−0.37	−0.37	0.15	0.20	−0.04	−0.08	0.14	0.11	0.04	0.05
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.00	0.00	0.00	0.00	0.02	0.00	0.59	0.25	0.09	0.18	0.72	0.63
RI-τ: RI-β: $Pr(\lvert t^j\rvert > t)$	0.00	0.00	0.00	0.01	0.01	0.02	0.35	0.03	0.01	0.28	0.16	0.65
Observations	938	938	938	938	938	938	938	938	1,836	1,836	1,836	1,836
No Scotland												
NI × Post	−0.3	−0.30	−0.53	−0.53	0.42	0.26	−0.03	−0.11	0.18	0.18	0.05	0.14
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.00	0.00	0.00	0.00	0.00	0.00	0.73	0.11	0.01	0.01	0.47	0.04
RI-τ: $Pr(\lvert t^j\rvert > t)$	0.00	0.00	0.00	0.00	0.09	0.11	0.81	0.23	0.00	0.05	0.01	0.00
Observations	756	756	756	756	756	756	756	756	1,512	1,512	1,512	1,512
PFA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Factors					8	8	8	8				

Notes: This table presents the difference-in-differences estimates of the effect of the SBL on police-recorded sexual assaults or rapes perpetrated against women obtained via OLS (columns (1)-(4)) and the interactive fixed-effects estimator of Bai (2009) (columns (5)-(8)). Results from the triple difference-in-differences are reported in columns (9)-(12). The dependent variable is the log number of police-recorded sexual assaults or rapes in each PFA or district in each year. In Panel A we include all areas of Great Britain in the control group. In Panel B we exclude Scotland for reasons discussed in the text. Randomization inference is performed on β and on τ-statistics with J=1,000 and calculated according to equation (2). The t-statistics used for $RI-\tau$ are based on standard errors that are clustered at the level the HMICFRS report was issued, i.e. one cluster for Northern Ireland and 56 for Great Britain. The estimated effect in column (7), (0.22), means that the Sex Buyer Law increased the number of recorded instances of sexual violence perpetrated against women in Northern Ireland by about ($e^{0.21}$ - 1) × 100 ≈ 23.4% relative to how many there would have been in the absence of the SBL.

Again, as discussed above, the factor model approach assumes the unobserved heterogeneity is of the form $\lambda_i' f_t$. Under different identifying assumptions we present triple difference-in-differences results from the estimation of equation (4) via OLS in the columns (5)-(8) (odd columns without controls, even columns with controls). The results for sexual assaults are very similar to those obtained via interactive fixed effects though the point estimates for the effect of the SBL on rape becomes positive.

Note that there is an argument for excluding Scotland from this analysis. Scotland was not subject to the HMICFRS report. The HMICFRS did not review practices in Scotland nor does the HMICFRS have any authority in Scotland, for which there is a separate inspectorate called Her Majesty's Inspectorate of Constabulary in Scotland (HMICS). Also, Scotland uses a different crime reporting standard than England, Wales, and Northern Ireland.[33] Recorded crime statistics for Scotland are not directly comparable with those from the rest of the United Kingdom. In Panel B we exclude Scotland and re-estimate everything. Results are qualitatively unchanged.

To check the sensitivity of our results in columns (6) and (8) and Table 5 to the choice of r, we estimate equation (1) via Bai (2009) for different numbers of factors and present results in Table 6.

For sexual assault the estimated effect is negative for $r \leq 3$ but becomes positive and stable around 0.22 for $r \geq 3$ with empirical p-values$\leq$0.03. For rape, the estimated effect stabilizes around -0.07 for $r \geq 5$ generally greater than 0.20.

In Table 7, we present results for estimates of the effect of the SBL on police-recorded, non-sexual violent crime by estimating equation (1) via Bai (2009) with different values of r.

[33] See Section 18 of 'User Guide to Recorded Crime Statistics in Scotland,' June 2016.

Table 6: Difference-in-differences estimates of the effect of the SBL on police-recorded sexual violence for different number of common factors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Sexual assault						Rape					
All areas												
ATT of the SBL	−0.16	−0.13	0.25	0.22	0.25	0.24	−0.02	0.01	−0.02	−0.07	−0.07	−0.08
RI: $Pr(\lvert\hat{\beta}^j\rvert > \hat{\beta})$	0.16	0.19	0.00	0.00	0.00	0.00	0.82	0.86	0.74	0.26	0.34	0.28
RI: $Pr(\lvert t^j\rvert > t)$	0.05	0.09	0.00	0.03	0.01	0.01	0.67	0.81	0.6	0.22	0.18	0.01
Observations	938	938	938	938	938	938	938	938	938	938	938	938
PFA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Factors	2	3	4	5	6	7	2	3	4	5	6	7

Notes: This table presents the difference-in-differences estimates of the effect of the SBL on police-recorded sexual assault and rape perpetrated against women obtained via the interactive fixed-effects estimator of Bai (2009) using 2 (column (1)) to 8 (column (7)) common factors. The dependent variable is the log number of police-recorded sexual assaults or rapes perpetrated against women in each PFA or district in each year. The estimated effect in column (6) (0.24) means that the Sex Buyer Law increased the number of recorded instances of sexual assault perpetrated against women in Northern Ireland by about ($e^{0.24}$ - 1) × 100 ≈ 27.1%. Randomization inference is performed on β and on t-statistics with J=1,000 and calculated according to equation (2). The t-statistics used for $RI - \tau$ are based on standard errors that are clustered at the level the HMICFRS report was issued, i.e. one cluster for Northern Ireland and 56 for Great Britain.

Table 7: Difference-in-differences estimates of the effect of the SBL on police-recorded non-sexual violence crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DiD OLS	DiD Interactive Fixed Effects						
	Non-sexual violence crime							
ATT of the SBL	0.00	−0.05	0.04	0.00	0.05	0.02	0.02	0.01
RI-β: $Pr(\lvert\hat{\beta}^j\rvert > \hat{\beta})$	0.98	0.12	0.26	0.92	0.11	0.50	0.40	0.62
RI-τ: $Pr(\lvert t^j\rvert > t)$	0.96	0.12	0.11	0.93	0.08	0.34	0.39	0.63
Observations	938	938	938	938	938	938	938	938
PFA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Factors		2	3	4	5	6	7	8

Notes: This table presents the difference-in-differences estimates of the effect of the SBL on police-recorded non-sexual violence crimes obtained via the interactive fixed-effects estimator of Bai (2009) using 2 (column (1)) to 8 (column (7)) common factors. The dependent variable is the log number of police-recorded non-sexual violence crime in each PFA or district in each year. Randomization inference is performed on β and on τ-statistics with J=1,000 and calculated according to equation (2). The t-statistics used for $RI-\tau$ are based on standard errors that are clustered at the level the HMICFRS report was issued, i.e. one cluster for Northern Ireland and 56 for Great Britain.

In column (1) we use OLS to estimate equation (1) with the log of non-sexual violence crime as the outcome. In columns (2)-(8) we use Bai (2009) with 2 to 8 factors. In each case the point estimate of the effect of the SBL is close to 0. We find no evidence that the SBL affected the recording of non-sexual violence crime.

Sexual violence discussion

These results are broadly consistent with findings in Ciacci (2020), Cunningham and Shah (2018) and Bisschop et al. (2017) insofar as the direction of the relationship between criminalization of the sex market and sexual violence, i.e. increased criminalization increases sexual violence. Other studies have found larger effects. Cunningham and Shah (2018) find that decriminalization reduced rapes in Rhode Island by 30%. Bisschop et al. (2017) find that localized decriminalization reduced sexual assaults by about 35% and rapes by about 3%. It is, however, difficult to directly compare the magnitudes of these effects since each study considers a different change to the regulation of the sex market. But Ciacci (2020) also finds a larger effect than we do despite studying the same regulatory change in the introduction of the SBL in Sweden. He finds that the SBL reduced rapes by 47% there. One possible explanation for the smaller effect here is the displacement of the sex market in Northern Ireland and ease of access to the Irish sex market which remained legal until 2017. Given the relatively small size of the country and the

lack of any border between Northern Ireland and the Republic of Ireland, the cost of traveling to the Republic of Ireland to purchase sex is low enough that it might mean that the marginal rate of transformation between buying such gratification and obtaining it via violence is higher in Northern Ireland than in Sweden.

There are multiple channels through which the introduction of the SBL might affect sexual violence. First, the change in regulation may affect the risk of sexual violence faced by sex workers themselves. Reducing the size of the sex market may reduce instances of sexual violence simply because there are fewer opportunities for sex workers to be victimized. As such the SBL would have a ceteris paribus negative effect on sexual violence. However, this decrease in demand may induce sex workers to engage in riskier behaviors in an effort to compete for clients including more frequent outcalls or taking on riskier clients who they may have turned down before the SBL. This would lead to a ceteris paribus increase in sexual violence committed against sex workers.

The SBL may have changed the composition of the Northern Irish client base. By increasing the risk of purchasing sex, more risk-averse clients will exit the market leaving a riskier client base for sex workers to access. This would also lead to a ceteris paribus increase in sexual violence committed against sex workers. Della Giusta et al. (2019) find evidence that a strengthening of the regulatory regime around the English sex market, though short of criminalization, correlates to an increase in the riskiness of clients (though they do not find any evidence that the policy changes shrank the market).

Sex work can be a dangerous occupation. In their survey of more than 600 sex workers in the United Kingdom, Sanders et al. (2018) find that 22.2% of sex workers had experienced a sexual assault in the last 5 years and 7.6% had experienced a sexual assault in the last year. Compare this to the 3.4% of women who indicated that they had ever experienced sexual assault of any kind in the Crime Survey of England and Wales.[34] Potterat et al. (2004) find that active female sex workers are 6 times more likely to be victims of homicide than similar women who are not sex workers and Ward et al. (2004) find that the mortality rate of sex workers is 12 times that of similarly aged women. In a survey of the literature related to the victimization of sex workers, Deering et al. (2014) find that 45-75% of sex workers report being the victims of criminal violence. In Norway, 59% of surveyed sex workers reported experiencing violence in

[34] Based on combined data for women aged 16-59 from the year ending March 2016 to year ending March 2018.

their work since the introduction of the SBL there in 2009 (Bjørndah 2012). Given the relative risk of criminal victimization faced by sex workers, a reduction in the size of the sex market should have a negative effect on crime and sexual violence in particular.

A number of researchers have hypothesized that the criminalization of clients makes sex workers less safe (Levy 2014, Wallace 2019). This is consistent with the views of sex workers themselves. Mai (2016) found that 98% of surveyed sex workers opposed the criminalization of clients 'which they see as increasing their vulnerability to violence and poverty by pushing the industry underground and discouraging safer clients.' (p. 3). However, the relationship between sex work, and the regulation thereof, and crime is complicated and extends beyond the sex workers themselves.

There is some descriptive evidence that sex work in Northern Ireland became more dangerous after the SBL was introduced. In a survey carried out by NUMS and submitted to Parliament, 80% of sex workers said that the SBL would make them feel 'less safe' (NUMS, 2017). Recent data released by NUMS (2019) suggest that crime against sex workers has doubled in the two years since the SBL was introduced in the Republic of Ireland. However, in Northern Ireland, 9.3% of sex workers interviewed reported being raped or sexually assaulted in the previous 12 months (Ellison et al. 2019) which is only slightly higher than the proportion reported in Sanders et al. (2018) for sex workers in England.

The effects of the SBL on sexual violence may extend beyond such crimes committed against sex workers themselves. A potential mechanism through which the effects identified in Bisschop et al. (2017), Cunningham and Shah (2018), and Ciacci and Sviatschi (2021) are operating is the substitution of the services of sex workers for sexual violence. As participation in the sex market becomes riskier for clients, marginal would-be clients may substitute towards sexual violence.

The net effect of the SBL on sexual violence in Northern Ireland is an empirical question. The existing evidence suggests that criminalization of the sex market leads to an increase in sexual violence. Bisschop et al. (2017) find that legal street prostitution zones are associated with a 35% decrease in sexual violence recorded by police. Nguyen (2016) shows that the effective decriminalization of sexual services frequently provided by massage parlors leads to a 28% decrease in rape cases.[35] Cunningham and Shah (2018) find that the decriminalization of

[35] Nguyen simulates the effects of decriminalization by reducing the licensing fee paid by massage parlors to 0. The 28% figure is obtained by using the elasticity she estimates at the mean fee and a 100% reduction in the licensing fee.

indoor sex work lead to a decrease in rapes recorded by police but do not find evidence that the recriminalization had any effect. These studies consider the effects of forms of decriminalization of the market for sex. However, as noted above, the general trend in terms of policy seems to be towards the criminalization of clients (The Economist, 2019).

Following the introduction of the SBL in Norway, sex workers reported that they were more exposed to violence as many of their reliable customers had left the market leaving behind fewer, but more violent, clients (Bjørndah 2012). Sex workers in Northern Ireland also reported that their work had become 'more subjectively risky than was the case before the law' (Ellison et al. 2019, p. 14), 57% of sex workers surveyed (N=133) said they felt the criminalization of buyers made sex work more dangerous (Ellison et al. 2019, p. 148), and sex workers reported that anti-social behavior from clients increased after the SBL in Northern Ireland. Similar sentiments were reported in Rasmussen et al. (2014) though they also report that the police had no indication of increased violence following criminalization. Ciacci (2020) finds that the introduction of the SBL in Sweden increased instances of police-recorded rape there. Berlin et al. (2020) find that the observed rise in violence, also in Sweden, was mostly due to an increase in domestic violence as opposed to increased violence perpetrated against sex workers themselves. However, Statens Offentliga Utredningar (2010) came to the conclusion that there had not been any increase in violence faced by sex workers or others following the introduction of the law in Sweden.

7 Conclusions

We study the effect of the criminalization of the purchasing of sex, the Sex Buyer Law, in Northern Ireland in 2015 on the size of the sex market, sexual health, and sexual violence. Our results suggest that it reduced the number of reviewed transactions by 25 to 50% in the year following the introduction of the law. However, our results also suggest that the market rebounded after that. We also find evidence of a short-term fall in fees for sexual services.

Our findings for the effect of the SBL on STIs in Northern Ireland are mixed. The new law reduced the rate of gonorrhea infections among women by about 30% and chlamydia by a similar amount but the fall in female chlamydia infections came a year after the introduction of the SBL. This result is in contrast to the results in Cunningham and Shah (2018) and Cameron et al. (2020) who find that (de)criminalization leads to (lower) higher STI rates. The effect of criminalization is likely context-dependent and also may depend on whether the change in the

regulation is in the direction of criminalization or decriminalization relative to the status quo, as the net effect on STI rates of changes to the size of the market and changes in the relative bargaining position of sex workers will vary. We do not find compelling evidence for an effect on male infection rates.

We also find evidence that the SBL increased sexual assaults perpetrated against women in Northern Ireland. This result is obtained using multiple sets of identifying assumptions and are broadly consistent with findings in other studies of the effects of criminalizing the sex market on sexual violence which suggests that moving regulation of the sex market away from permissiveness leads to higher rates of sexual violence.

Our results suggest that the SBL had real effects in Northern Ireland in terms of shrinking the sex market, though perhaps only temporarily, reducing STI transmission, and increasing sexual assaults. These results are in contrast to recent conclusions about the SBL by policymakers in Northern Ireland that the new law had no effect.

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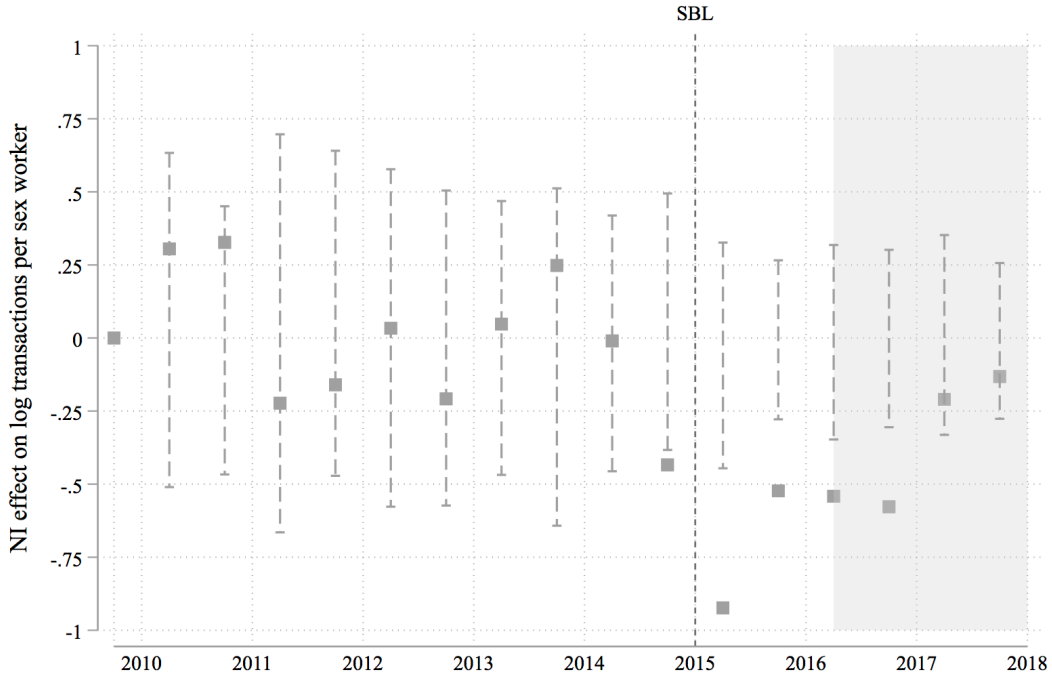
Appendix

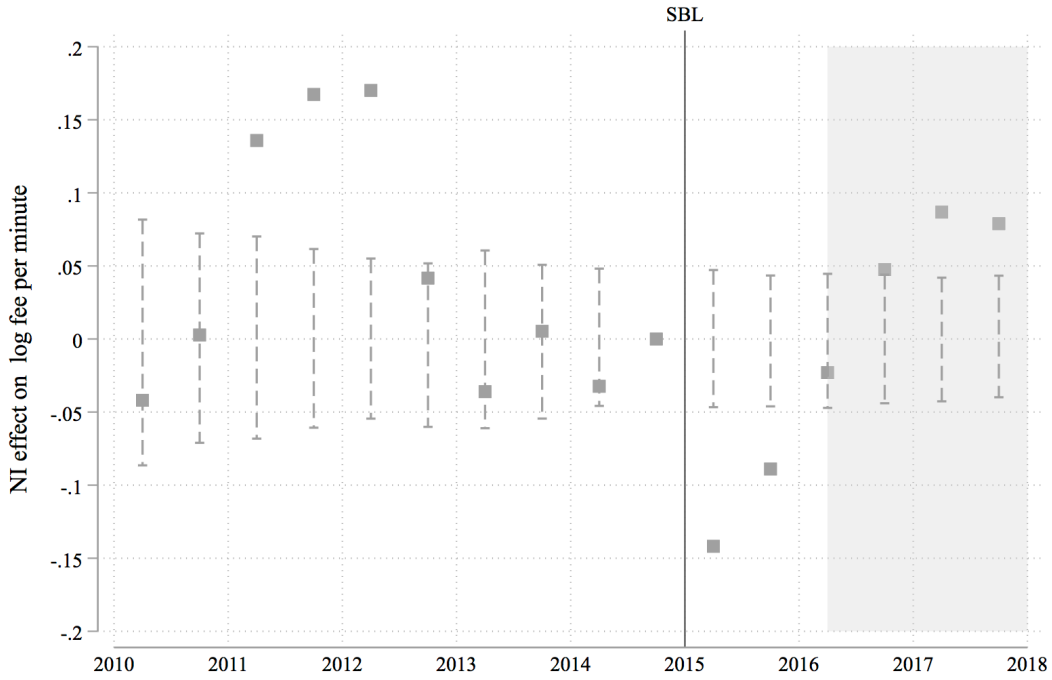
In Figure A1 and A2 we present the event study based on the transformed reviewed transaction and fee per minute data but apply the observation rule from Platform 1 to the data from Platform 2, i.e. we include only sex workers with active profiles at the time of our data collection.

Figure A1: Northern Ireland effect on log reviewed transactions using Platform 1 observation rule



Note: This figure plots the estimated Northern Ireland effect on the log number of reviewed transactions using data from from Platform 1 and 2 but applying the observation rule from Platform 1 to the data from Platform 2 and using Great Britain as the control group. The dashed lines represent the third smallest and third largest (of 38 NUTS2* areas in Great Britain), approximately the 10th and 90th percentiles of the distribution of effects obtained via randomization of the treatment over these NUTS2* areas.

Figure A2: Northern Ireland effect on log fee per minute using Platform 1 observation rule



Note: This figure plots the estimated Northern Ireland effect on the log fee per minute of a transaction using data from from Platform 1 and 2 but applying the observation rule from Platform 1 to the data from Platform 2 and using Great Britain as the control group. The reference period is the six months preceding the SBL. The dashed lines represent the 10th and 90th percentiles of the distribution of effects obtain via 1,000 randomization of the treatment over sex workers in the United Kingdom.

The patterns in both cases are similar to those in Figures 2 and 3.

We next estimate equation (1) applying the observation rule from Platform 1 to the data from Platform 2 and present the resulting difference-in-differences estimate in Table A1 allowing for country-specific linear trends in columns (1) and (2). Results are presented in an analogous fashion to this in Table 2.

Table A1: Difference-in-differences estimates of the effect of the SBL on log transactions and log fee per minute

	(1)	(2)	(3)	(4)
	Reviewed transactions		Fee per minute	
ATT of the SBL	−0.45	−0.47	0.01	0.01
Placebo-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.11	0.13		
Placebo-τ: $Pr(\lvert\hat{t^j}\rvert > t)$	0.16	0.13		
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$			0.58	0.31
RI-τ: $Pr(\lvert\hat{t^j}\rvert > t)$			0.77	0.54
Observations	684	684	30,285	30,285
Controls	No	Yes	No	Yes
NUTS2* and Year FE	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation (1) for where the dependent variable is the log number of reviewed transactions (columns (1) and (2)) obtained using transaction aggregated to the NUTS2*-level over six-month periods, the log fee per minute (columns (3) and (4)) obtained using transaction-level data over six-month periods. We apply the observation rule for Platform 1 to the data from Platform 2 before estimating.

The estimates for reviewed transactions are consistent with what we see above, but the results for fee per minute are sensitive to the application of the Platform 1 observation rule to Platform 2.

Table A2: Difference-in-differences estimates of the effect of the SBL on police-recorded sexual violence perpetrated against men for different number of common factors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DiD OLS	DiD Interactive Fixed Effects						
All areas								
ATT of the SBL	−0.30	−0.25	−0.09	0.07	0.08	0.02	0.10	0.03
RI-β: $Pr(\lvert\hat{\beta^j}\rvert > \hat{\beta})$	0.00	0.04	0.41	0.53	0.44	0.87	0.35	0.74
RI-τ: $Pr(\lvert t^j\rvert > t)$	0.01	0.00	0.29	0.58	0.53	0.8	0.51	0.76
Observations	938	938	938	938	938	938	938	938
PFA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Factors		2	3	4	5	6	7	8

Notes: This table presents the difference-in-differences estimates of the effect of the SBL on police-recorded sexual violence (assault and rape) perpetrated against men obtained via the interactive fixed-effects estimator of Bai (2009) using 2 (column (1)) to 8 (column (7)) common factors. The dependent variable is the log number of police-recorded sexual assaults or rapes perpetrated against women in each PFA or district in each year. Randomization inference is performed on β and on t-statistics with J=1,000 and calculated according to equation (2). The t-statistics used for $RI - \tau$ are based on standard errors that are clustered at the level the HMICFRS report was issued, i.e. one cluster for Northern Ireland and 56 for Great Britain.