Coming Out of the Woods. Do local support services influence the propensity to report sexual violence?

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Abstract. Sexual crimes against women are severely underreported to the police, allowing for impunity of perpetrators. Observers suggest that a stimulus towards reporting the crime comes from nearby support services for victims of sexual offences -like refuges and advisors. Still, the evidence about the effects of nearby support on the reporting of sexual crimes remains scattered and mainly qualitative. This paper provides quantitative evidence on this effect, by exploiting the uneven geography of local support services which resulted in the UK after the introduction of the austerity program. Findings highlight a positive net effect of the provision of local support services on the victims' propensity to report. The positive effect holds also in the aftermath of a space-neutral high-impact media campaign empowering women to report sexual violence. This evidence relates to relevant policy implication, given that in some countries the austerity-driven cuts to public budgets have reduced and dispersed the local availability of support services, making digital support and/or helpline the only available options in many places.

1. Introduction

Sexual violence, labelled as a "*global public health problem of epidemic proportions*" by the World Health Organization (WHO, 2013), has extremely high rates of underreporting (Bureau of Justice Statistics, 2018), which are associated to a high likelihood of repeated victimization, to growing numbers of victims of the unreported offenders and to high level impunity of perpetrators (Felson et al., 2005; González and Rodríguez-Planas, 2020). The impunity granted by under reporting makes the socioeconomic cost of sexual violence persistently high: at the European level, the annual cost of sexual violence amounts to 228 billions of euro, corresponding to 1.5% of EU GDP (Bonnewit and DeSantis, 2016). Hence, understanding which initiatives are effective in countering the under reporting of sexual crimes is an open and highly policy-relevant question needing sound answers (Kelly, 2018; Uggen et al., 2021), but not

yet supported by established evidence on the impact of the different initiatives adopted by governments (Miller and Segal, 2019).

This paper provides a contribution on the estimated impact of local support services on the victims' propensity to report, comparing changes in reporting in places where local support services are available to changes in reporting in places where local support is not provided. We target local Violence Against Women and Girls (VAWG) support services since praticioners and institutions consider the provision of evenly geographically distributed and 24/7 available VAWG support services as valuable policy to stimulate reporting by victims (Council of Europe, 2011; Council of the European Union, 2012; European Parliament, 2017; United Nations and European Commission, 2018). Qualitative investigations on European countries support this perspective, showing that survivors feel more empowered after referring to a specialized support service (British Medical Association, 2014), and that specialized service relates to an improved willingness of framing the violence as a crime (Bettio and Ticci, 2017). Similar seminal evidence on the US relates the provision of nearby services for victims to an increase in reporting (McVeigh et al., 2003; Zweig et al., 2003) However, thse contributions do not convey any measure of the impact of nearby VAWG support services on the victims' propensity to report. Impact measures are important for the policymakers who are working to prioritize local VAWG support services, since these services are disappearing from many places as a consequence of the combination of austerity-driven reduction in the government spending on VAWG initiatives and of the devolution of these initiatives to the local level (GREVIO, 2019; Sanders-McDonagh et al., 2016). Evidence on the impact of VAWG support services on crime reporting is also needed to contribute to the assessment of their overall effectiveness given that their even spatial distribution is specified as mandatory by the Istanbul Convention, the first regional legally binding instrument covering all forms of violence against women in Europe (Council of Europe, 2011). Finally, evaluating their impact also contribute to increase the information-base on the effect of different types of policy in promoting reporting by female victims of sexual violence, allowing to detect policy complementarities as well as potential redundancies. Recent contributions have analyised the role of female police officers in US, Brazil and India (Amaral et al., 2019; Miller and Segal, 2019; Perova and Reynolds, 2017) and the role of female officers in the law and enforcement stages of sexual crime reporting in Peru (E. Kavanaugh et al., 2017). This paper contributes to the build-up of this information-base by estimating the effects of locally provided VAWG support services on sexual crime reporting in England and Wales. Due to the fine-grained spatial perspective used in the analysis, the paper cannot consider individual features, which are increasingly investigated in their role in sexual violence (González and Rodríguez-Planas, 2020) but that are available at higher spatial scales.

We target England and Wales for several reasons, starting from their high level of sexual offences which contributes to the UK ranking third among European countries for the share of women having experienced sexual violence (European Union Agency for Fundamental Rights, 2014). Second, following the 2010 austerity programme, the UK experienced a structural shift in the policy framework addressing VAWG, appointing local authorities as the public body in charge for the discretionary choice on the provision of VAWG support service and the consequent funding. Before 2011, VAWG support service, although already non mandatory, were financed through government grants; after 2011, only Scotland decided to shield national public grants benefitting VAWG support services whereas England and Wales devolved the funding to the local level, at the same time slashing the flow of government grants that was overall benefitting the local budgets (Hirst and Rinne, 2012). The introduction of this local policy choice implied a relevant trade-off for local authorities, since it meant choosing whether to prioritize VAWG support services against other policies under tight budget constraints (Gray and Barford, 2018; Simmonds, 2016). The majority of local authorities did not devote local budget spending to VAWG support services, resulting in many services shutting down and in a relevant increase in their spatial dispersion, which rose by 24.67% in 7 years as outlined by Figure 1.

Figure 1. The reduction of the spatial distribution of VAWG support services in England and Wales after the austerity.

2010

Coefficient of variation: 58.32

2017

Coefficient of variation: 82.99

Spatial distribution of VAWG Specialised Services before and after the Austerity Programme

▉ VAWG service available   ▉ VAWG service non available

England experienced the most significant decrease in the number of women's shelter places among European countries after 2011 (Lesur et al., 2014). The decommisioning choices pursued

by the majority of English local authorities caused more than 12 000 referred victims turned away from required support in 2016, amounting to the 60% of referrals (Miles and Smith, 2019; Women's Budget Group, 2019). The resulting spatially fragmented outlook of VAWG support services does not comply with the Istanbul Convention, making the UK the last Western European country to still ratify the legally binding agreement (Council of Europe, 2020).

Our analysis takes advantage of the shift in the policy framework for the provision of VAWG support service, and of the resulting different policy choices made at the local level. The spatial heterogeneity in nearby VAWG support services resulting from the distinctive choices made by local councils in 2011 gives room for a comparative case study to assess the impact of the local policy for VAWG support services on crime reporting. This impact can be inferred by estimating the changes in the propensity to report sexual crimes to the police in a place that provided local support services and by comparing these changes with the changes in the propensity to report sexual crimes in places that are similar to this place, except for the local provision of support services. To identify the treated place and the places used for comparison, we mapped the local authorities of England and Wales along two dimensions. First, we recorded the places that implemented a local policy for VAWG support services after 2011, collecting spatially fine-grained data about the changing landscape of VAWG service provision from different sources (Coy et al., 2011; Grierson, 2018; The Bureau of Investigative Journalism, 2017; Towers and Walby, 2012). Second, given that our chosen proxy for the victims' propensity to report is given by police records as done in the related literature (Card and Dahl, 2011; Hagan et al., 2018; Lindo et al., 2018), we also mapped the places where sexual crime records represent an accurate measure for reporting to the police before and after austerity, since they do not suffer from misrecording/unrecording and other biases that may affect police activity (Long, 2019). By designing a novel database containing evidence on these two dimensions, we are capable of identifying Brigthon and Hove as the appropriate place to scrutinize.

After 2011, the Brighton and Hove Council devoted a share of local budget spending for the provision of VAWG support services notwithstanding the sizeable austerity-driven cuts in the Government grants flowing to the local budget (National Audit Office, 2019). Figures show that the trend in police records on sexual crimes against women aged 16 and above in Brighton and Hove after 2011 is steadily above the same trend for England and Wales and the local police force displays a good quality in the management of the reports of sexual offences, alleviating concerns about measurement bias on our proxy for the propensity to report. Through the Synthetic Controls Method (Abadie et al., 2010), we estimate the evolution of reporting in Brighton and Hove against the evolution in reporting in a synthetic counterfactual designed from a weighted average of places that were similar to Brighton and Hove before the introduction of

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the local policy. We further assess the robustness of the results through the Difference-in-Difference estimation, the Generalized Difference-in-Difference approach and the Trajectory Balancing approach, which are competing statistical approaches to assess the impact of the policy intervention (Hazlett and Xu, 2018; Varian, 2016).

Our estimates highlight that the introduction of the policy resulted in increased reporting for each year after the introduction of the policy. The results hold to the inclusion of local socioeconomic features which may contribute to influencing the propensity to report sexual assaults, as well as to the inclusion of the potentially competing policy initiatives. Findings show that the positive effect of the local provision of support services is not substituted for by the high-profile media campaign supporting victims of sexual offences that happened in the UK between the end of 2012 and the beginning of 2013. This campaign, labelled the "*Yewtree effect*", is considered as a trigger in women's propensity to report sexual offences (Office for National Statistics - ONS, 2018) and it relates to the analogous "*Weinstein effect*" (Maddaus, 2018), introduced after the global media reach of the #*Metoo* movement. Our results show that although the "*Yewtree effect*" actually boosted the propensity to report, the local provision of VAWG support services still determined a higher propensity to report compared to places which experienced only the "*Yewtree effect*".

This paper provides evidence supporting a positive impact of the local provision of specialized VAWG support services on crime reporting, and it expands on previous research along four dimensions. First, it adds quantitative evidence on the beneficial effect of VAWG support services on reporting to the existing qualitative evidence (Bettio and Ticci, 2017). Second, it adds to the existing evidence within the economics of crime literature on institutions and violence against women by analysing the role of the local provision of support services (*i.a.* Amaral et al., 2019; Miller and Segal, 2019). Notably, our results hold when we include the already identified positive effect exerted by other polices. Third, the paper contributes to the quantitative investigation of crime reporting as an outcome in the social sciences, a topic which is acknowledged as relevant but still under investigated (Aizer and Dal Bó, 2009; Hagan et al., 2018; Small, 2018). Forth, it contributes to the evidence on the geographies of territorial injustice resulting from the austerity (Gray and Barford, 2018). Fifth, it also refers to the literature analysing the role of local policies and local conditions in influencing population behaviours (Wright et al., 2020).

The remaining of the paper is organized as follows. First, we provide a background description of the effect of VAWG support services in supporting survivors, together with the recent history of the provision of VAWG support services in England and Wales and the description on the selected case for the case study analysis. Then, we explain the proposed

empirical approach and we detail the characteristics of the database. Afterwards, results are presented and discussed and, finally, conclusions are detailed.

2. Relevant Literature and Case Selection

2.1 Effective policy for VAWG support services and policy approach in England and Wales

VAWG support services provide confidential and dedicated support including safe locations to stay, legal advice and psychological help. Although VAWG support services do not require the survivors to report offences to the police, the survivors' actual access to justice depends largely on their availability (Boba and Lilley, 2009; Fahmy et al., 2016). At the European level, specialised services relate to an improved willingness of framing the violence as a crime (Bettio and Ticci, 2017) and in the UK the 80% of survivors say that they felt more empowered after referring to a specialised support service (British Medical Association, 2014). Scholars and institutions argue that the effectiveness of VAWG support services depends on them being evenly geographically distributed and 24/7 available, so that survivors can reach out for help with no additional costs of searching, travelling and waiting (Christofi et al., 2017; Kelly, 2018; Macy et al., 2010). The absence of nearby support services increases the survivors' sense of isolation, reducing their willingness to seek for justice (Miles and Smith, 2019; Xie and Lynch, 2017). The local availability of VAWG support services also signals that there is a local commitment to a culture of respect of women, which can contribute to reducing the perception of socio-cultural prejudices (Ellsberg et al., 2015; Iyer, Mani, Mishra, & Topalova, 2012). Nonetheless, the geography of VAWG support services has been negatively affected by the cuts to the public budgets driven by austerity (GREVIO, 2019; United Nations and European Commission, 2018), increasing territorial injustice for survivors and the high cost paid by women due to austerity measures (Holly, 2017).

In England and Wales, the provision of VAWG support service is insufficient both in terms of overall capacity and of geographic evenness. Both nations fail to meet the Istanbul Convention requirements. As a consequence of the austerity cuts, in 2015 England was short of 1 646 out of 5 233 refuge places for women having experienced a sexual crime, corresponding to a 31.51% shortage (House of Lords House of Commons, 2015); Wales displayed a shortage of around 25% (Lesur, Stelmaszek, & Iris, 2014). In 2016, figures worsened: nearly 12 000 referred female survivors were turned away from refuges, amounting to the 60% of referrals (Miles and Smith, 2019). Alongside the overall lack of capacity with respect to actual needs, also

the uneven spatial distribution of VAWG specialised services in England and Wales is a source for concern (Taylor, 2013), and the situation of survivors of social violence was labelled as a "*a postcode lottery*", due to the critical impact of the place of residency on the availability of support (Simmonds, 2016).

The outlook of VAWG support service in England and Wales follows from the 2011 shift in the policy framework for the provision of VAWG support service due to the austerity program. Before 2011 the funding of VAWG support services pivoted on state-level ring-fenced grant flowing to local authorities on the basis of the local assessment of need (Heady et al., 2011; Hirst and Rinne, 2012), although with no mandatory commitment for local authorities on the provision. Figures show that the provision of VAWG support services grew across places (Towers and Walby, 2012). In 2011, the state level austerity programme introduced a structural shift from state-level funding to local commissioning, overall devolving to the local authorities the scope, the scale, the nature and the funding of the local VAWG services endowment (Hirst and Rinne, 2012). While stressing the local commissioning, the austerity programme also imposed relevant cuts to local public budget spending through the reduction of the overall flow of state-level grants supporting the local budgets (Gray and Barford, 2018; Hastings et al., 2015). Hence, local authorities had to decide whether or not to realize and finance VAWG support services considering their tight budget decisions about which services to either prioritise or cut (Hirst and Rinne, 2012).

The sizeable impact of the severe budget cuts on the local availability of VAWG support services has prompted several investigations to collect spatial evidence about the effect of austerity on the supply of specialized services for female survivors of sexual violence (Coy et al., 2009; Grierson, 2018; Holly, 2017; The Bureau of Investigative Journalism, 2017; Towers and Walby, 2012). By collecting and analysing this evidence, it emerges: *(i)* that more than 75% of England's local authorities slashed their spending on VAWG services between 2011 and 2017 (Women's Budget Group, 2019); *(ii)* 18% net reduction in the number of local VAWG support services between 2009 and 2017; *(iii)* an increase in the spatial dispersion of services around 25%. Figure 1 shows the spatial distribution of VAWG support services before and after austerity. This "*postcode lottery*" of VAWG support services has also been recently acknowledged by the 2019 First Report of the Joint Committee on the Draft of the Domestic Abuse Bill (House of Commons Education Committee, 2017), suggesting that the 2011-2015 implementation of the cross-governmental strategy to tackle violence against women and girls had not been effective in countering the spatial thinning of specialised services fuelled by austerity due to the strong

reduction on public funding devoted to VAWG support services[1] and to the lack of mandatory duty for local authorities (UK All-Party Parliamentary Group on Domestic and Sexual Violence Inquiry, 2015).

Given that the provision of VAWG support services was not mandatory, local authorities were allowed to choose whether to prioritise them or not in lights of reduced flows of government grants previously benefitting the local budgets. This local policy choice was not only a measure through which the local level public spending substitutes for the state-level spending, but also a measure that prioritises VAWG services against other services within the local budget. In fact, alongside the reduction to state-level public spending for VAWG specialised services, the austerity also implied a reduction in the state-level grants which were previously contributing to the total budget of local governments (Gray and Barford, 2018). Local authorities councils across England and Wales made different decisions, with the majority of them deciding not to prioritise the implementation of local policies for VAWG support services (Women's Budget Group, 2019). Notably, all types of VAWG services providers - public and private - were depending on the implementation of a financed local policy[2] (Heady et al., 2011; Women's Aid, 2015).

After the different choices on the implementation of the local policy resulted in a strongly uneven geography of support services, a relevant space neutral event which is anecdotally related to an increase in the propensity to report sexual crimes occurred, namely the high-impact media campaign on sexual violence labelled "*Yewtree effect*" (Office for National Statistics - ONS, 2018). Starting from an ITV programme broadcasted at the end of 2012 which featured five women recounting being abused by the late television presenter Jimmy Savile during the 1970s, a police investigation - labelled "*Operation Yewtree*" – started to subsequently broaden to include other perpetrators due to hundreds of survivors coming forward to report sexual assaults perpetrated by other people. High-profile media coverage of sexual offences is conjectured to exert a positive influence on women's willingness to report sexual offences by reducing the feeling of isolation (Mendes et al., 2018). This effect is not spatially limited, due to the national and/or global reach. Our analysis allows to gauge some evidence on whether this effect acts as a substitute for the

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[1] The 2003-2009 state-level dedicated grants for VAWG specialised support in the supporting People Programme amounted to £68.8 million (Ashton and Hempenstall, 2009). The 2011-2015 cross-governmental strategy to counter VAWG had a total endowment of almost £40 million mainly devoted towards state-level programmes and towards services that activate only after a survivor enters the criminal justice system (UK All-Party Parliamentary Group on Domestic and Sexual Violence Inquiry, 2015). The financial support to local commissioning had to share the remaining state resources with other purposes, such as state-level campaigns and international schemes, children abuse and youth gang (Home Office, 2014).

[2] On average, more than 50% of funding for non-statutory organisations providing VAWG support services comes from the local authority (Women's Aid, 2015).

provision of nearby VAWG support services in influencing survivors' propensity to come forward.

2.2 Case selection: Brighton and Hove

The spatial heterogeneity in the discretionary choice of implementing a local policy for the provision of VAWG support services across England and Wales gives room for the analysis of the impact of the actual policy adoption on the propensity to report to the police by female survivors. We build our comparative case study approach starting from the identification stage to detect a place that implemented the local policy (treated unit) and the places against which we compare the treated unit. We begin with the design of the database mapping England and Wales according to places that adopted the policy against places that decommissioned, collecting data on the local authorities' decisions on whether or not to implement the local policy for VAWG support services after 2011 through various sources (Coy et al., 2011; Grierson, 2018; Holly, 2017; The Bureau of Investigative Journalism, 2017; Towers and Walby, 2012). These decisions were adopted at the Upper-Tier Local Authority (UTLA) level (for their geography, see Appendix Figure S1.a). According to the collected evidence, 12 UTLAs decided to adopt a local policy for specialized support for victims of sexual offences after 2011 (Appendix Figure S2.a). In the second step of the database construction, we identified our proposed measure for the propensity to report sexual offences in England and Wales following the established literature in using police records to measure the propensity to report (Card and Dahl, 2011; Lindo et al., 2018). Thus, we collected data from 2004 to 2016 on police records referring to sexual crimes (rape and sexual assaults) agaist women aged 16 and above. These data are available from the UK Home Office and they are collected at the Community Safety Partnerships (CSP) level. The 315 CSPs are statutory bodies for England and Wales and the smallest spatial units on which data on records of sexual crimes against women are available (Appendix Figure S1.b). We discarded data relating to 2003 backwards since they were classified differently and cannot be compared with subsequent years. Being the propensity to report the focus of our investigation, we consider the CSPs as the spatial units for our comparative case approach. Hence, we map the policy choices adopted by each UTLA on the corresponding CSP. Out of the 12 UTLAs which adopted a local policy for specialized support for victims of sexual offences after 2011, 11 UTLAs correspond to a single CSP each. The remaining UTLA -Hertfordshire- corresponds to 6 CSPs, thus we mapped the policy choice on each one of the 6 CPSs. These 17 CSPs constitute the potential group of treated units for our comparative case study, letting the remaining 298 CSPs as potential contributors to the donor pool.

As third stage, we checked for the accuracy of police record data as proxy for the propensity to report, in order to avoid measurement bias. Exploiting the independent annual audit reports on the police force areas by Her Majesty's Inspectorate of Constabulary (Her Majesty's Inspectorate of Constabulary, 2014), we discarded the 176 CSPs in which the local police force suffered from inaccuracy in the handling of the records referring to sexual offences (Appendix Figure S2.b). In the 139 remaining CSPs characterized by suitable proxies for measuring the propensity to report sexual crimes, only 2 out of the 17 CSPs which adopted the local policy remained, namely Brighton and Hove and Waltham Forest. The latter was discarded, since it may be affected by spatial spillovers due to its belonging to the London area, where VAWG support services provided by the different London Boroughs are reachable with limited travelling and searching costs. Thus, through stages 1-3 we identified Brighton and Hove as our treated place. Figures also show that Brighton and Hove belongs to the share of UTLAs suffering from a higher reduction in government funding compared to the remaining UTLAs (NAO, 2019). Hence, the local policy choice of providing VAWG support services was not done benefitting from a milder impact of the austerity programme with respect to the UTLAs that decided to reduce public support to VAWG services.

Stage 4 allowed to identify the comparison group made by suitable untreated places to be exploited in the design of the couterfactual. We assessed the suitability of the comparison group according to our chosen estimation methodology, *i.e.* the Synthetic Control Method approach (Abadie, 2019). Hence, we dropped places: *(i)* affected by large idiosyncratic shocks that may affect the outcome of interest; *(ii)* benefitting from similar types of interventions with respect to the one that we are evaluating; *(iii)* different from the treated place with respect to the pre-2011 endowement of VAWG supporrt services. We applied these criteria to our potential comparison group of 137 CSPs which provide suitable proxies for measuring the propensity to report sexual crimes and which did not implement any local policy for nearby support services after 2011. We dropped Westiminster for its relevant and idiosyncratic night economy with consequences also on the size of sexual crimes (Office for National Statistics - ONS, 2018). We dropped 3 other CPSs (Preston, Lincoln and Ceredigion), because they benefitted from new rape centres financed directly by the UK Government between 2010 and 2015 (The UK Ministry of Justice, 2015). Finally, we restricted the donor pool to those CSPs with the same pre-2011 endowment of support services for victims of sexual offences as Brighton and Hove. This restriction aligns with the requirement of restricting the donor pool to places with characteristics that are similar to the treated place and it follows from the assumption that the opportunity cost of starting a local provision of specialised VAWG support services is different between places which did not have
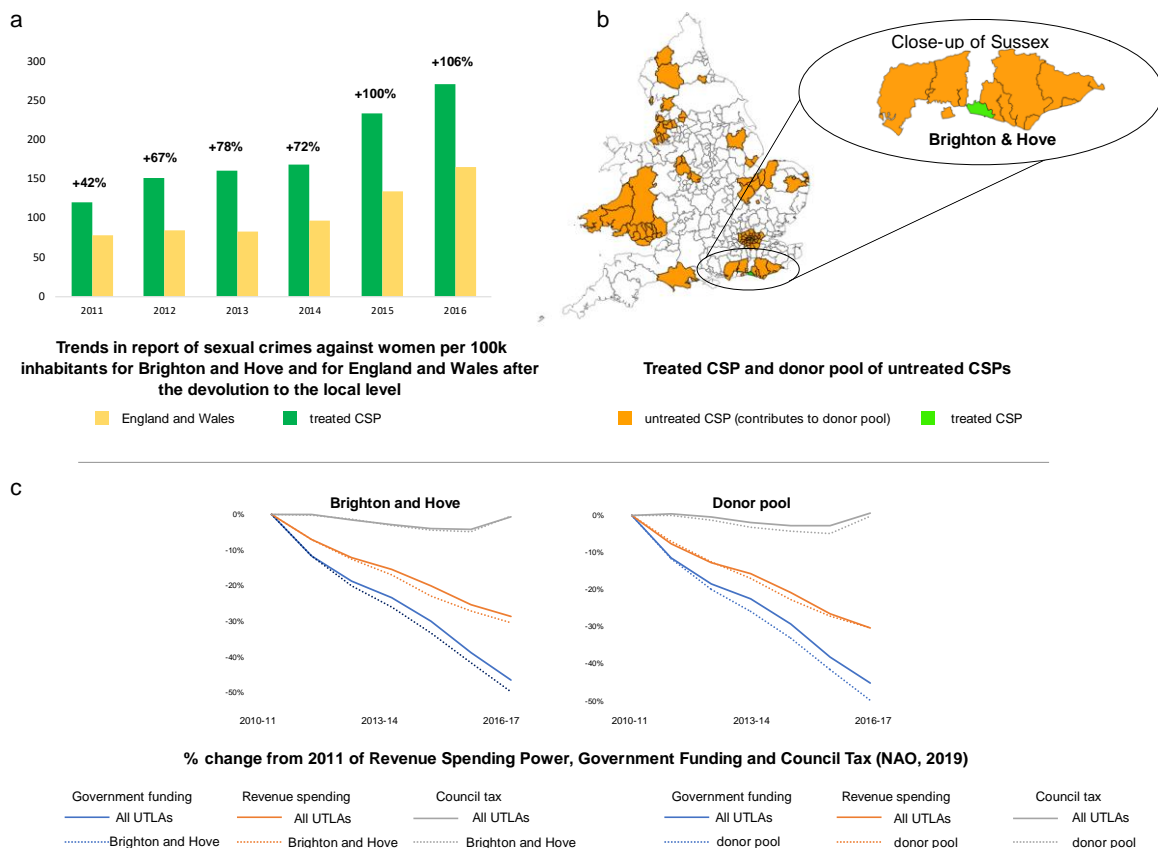
to bear the start-up costs for the build-up of new services and places which had to evaluate also to bear the cost of services build-up. Table 1 summarises the four stages.

Table 1. Selection of CSPs considered in the donor pool

CPSs	Donor pool
Inaccurate police records	dropped
Same local policy	dropped*
Close-but-different policy	dropped°
Idiosyncratic shock	dropped^
No VAWG support services before 2011	dropped

*after dropping the CSPs with inaccurate police records, there is only one CSP -Waltham Forest- to be dropped due to the implementation of the same local policy.
° 3 CSPs (Preston, Lincoln, Ceredigion) are dropped because they benefitted from new rape centers financed directly by the UK Government between 2010-2015.
^ Westminster is dropped for its idiosyncratic night economy and the consequences on the volume of sexual assaults.

Figure 2. Outline of the selected case study. Figure 2.a: Trends in the reporting of sexual crimes against women above 16 in Brighton and Hove (in green) and in England and Wales (in yellow). Figure 2.b: Map of the sample of places considered for the Synthetic Control Method. Brighton and Hove is coloured in green, the 84 places coloured in orange contribute to the design of the counterfactual unit which mimic Brighton and Hove in the absence of the intervention. Figure 2.c: 2010-2017 trends in the budget for Brighton and Hove and for the donor pool compared with the rest of local authorities.

a

Trends in report of sexual crimes against women per 100k inhabitants for Brighton and Hove and for England and Wales after the devolution to the local level

England and Wales   treated CSP

b

Treated CSP and donor pool of untreated CSPs

untreated CSP (contributes to donor pool)   treated CSP

c

Brighton and Hove   Donor pool

% change from 2011 of Revenue Spending Power, Government Funding and Council Tax (NAO, 2019)

Government funding	Revenue spending	Council tax	Government funding	Revenue spending	Council tax
All UTLAs	All UTLAs	All UTLAs	All UTLAs	All UTLAs	All UTLAs
Brighton and Hove	Brighton and Hove	Brighton and Hove	donor pool	donor pool	donor pool

The resulting donor pool consists of 84 places (See Figure S2.c), which will be exploited to design the counterfactual on which to assess the impact of the policy adopted by Brighton and Hove. Figure 2.b summarizes the geography of the places that contribute to our comparative case study. Figure 2.c shows the effect of austerity in the local authority budget for Brighton and Hove and for the donor pool. It is straightforward to notice that the reduction in resuources is extremely similar in both cases, alleviating from endogenous placement of the policy concerns.

3. Methodology and Data

We measure the impact of the local policy under investigation through a comparative study approach, by comparing the trend of the outcome variable of interest between the unit experiencing the policy and a set of units that are similar to the treated unit but were not experiencing the policy. We consider the Synthetic Control Method (SCM) that compares the treated unit to a synthetic control unit designed through a combination of untreated units belonging to a donor pool (Absher et al., 2020). With respect to traditional regression analysis techniques that require large samples and many observed instances of intervention of interest, the SCM represents a suitable approach to assess the impact of a sporadic event such as policy intervention resulting from a new policy framework (Abadie, 2019). Moreover, the SCM exploits a data-driven methodology to design the counterfactual, by selecting through a weighting process the combination of untreated the units from the donor pool which provide the best pre-treatment match with the treated unit. Through this, the SCM accounts for a drawback of comparative case studies of this type given by the lack of formalization of the selection process for the comparison units (Abadie, 2019). Finally, compared to competing methodologies within the comparative study approach, the SCM accounts for both observed and unobserved time-varying confounding that may influence the outcome. Formally, let D_{it} be the indicator for the local policy implemented in the CSP_i at time t and y^N_{it} be the counterfactual, that is the propensity to report that would have been observed for the CSP_i at time t in the absence of the policy. Then, we express the propensity to report sexual offences to the police in the CSP_i at time t as follows (Abadie et al., 2010)

$$y_{it} = \alpha_{it}D_{it} + y_{it}^N = \alpha_{it}D_{it} + (\delta_t + \theta_t X_i + \lambda_t \phi_i + \xi_{it}) \qquad (1)$$

where y^N_{it} is given by the sum of a time effect, δ_t, vectors of observed and unobserved predictors of y^N_{it} not affected by the policy, X_i and ϕ_i, and the error terms, ξ_{it}. Now let the only CSP exposed to the policy be the first one, then the treatment effect of interest, $\hat{\alpha}_{1t}$ is estimated by approximating y^N_{it} with a weighted average of the untreated CSPs in the donor pool,

$$\hat{\alpha}_{1t} = y_{1t} - \sum_{i \geq 2}^{I+1} w_i y_{it} \qquad (2)$$

for $t \in \{T_0 + 1, ..., T\}$. Weights, w_i, are such that $0 \leq w_i \leq 1$ for $i = 2,...,I+1$ and $w_2 + \cdots + w_{I+1} = 1$. $T_0 + 1$ is the year of the policy introduction, T is the total number of considered years and $I+1$ is the total number of CSPs in the sample. Therefore, the synthetic counterfactual is the time-invariant weighted average of the available control units within the donor pool, which prior to the intervention had similar pre-intervention characteristics and outcome trajectory to the treated unit (Kreif et al., 2016). For the SCM to be effective, the synthetic counterfactual must be extremely similar to the treated unit, a feature that is assessed estimating X_i and ϕ_i in the pre-intervention periods, at the same time accounting for potential specification issues and their potential estimation bias. To this regard, there is a trade-off about the efficiency of the SCM depending on the number of lagged values for the outcome variable to consider. By increasing the considered lags, the weights assigned to the control variables get reduced and this can reduce efficiency. At the same time, increasing the t is possible that the efficiency of the SCM gets reduced. But at the same time, increasing the considered lags allow to take more care of unobserved confounders (Athey and Imbens, 2017; Kaul et al., 2015). We asses this issue by estimating several SCM specifications, that differ with respect to the predictors included in X_i and ϕ_i. More into details, in each considered SCM specification we vary X_i and ϕ_i with respect to: *(i)* M linear combinations of y in the pre-treatment periods; *(ii)* r other covariates with explanatory power for y, again in the pre-treatment period. These variables are used by the SCM to compute the combination of the donor units capable of minimize the difference of the predictors' values of the treated and the counterfactual units. Hence, by varying these variables, the SCM can attribute different weights to the CSPs in the donor pool and determining different impact for the policy (Kaul et al., 2015). To account for these specification issues, we evaluate the goodness-of-fit of our chosen SCM specification against other considered SCM specifications to check whether results and inference change (Ferman et al., 2020).

The analysis estimates also a Difference-in-Difference (DID) model, a Generalized Difference-in-Difference (GDID) model and a Trajectory Balance (TJBAL) approach, representing competing specifications to the SCM, to check whether results are robust to the distinctive assumptions associated to the different methodologies. The DID and the GDID construct the counterfactual without assigning weights to the units belonging to the control group. Formally, we start estimating the baseline DID in which we collapse the pre-intervention years on a single pre-intervention period by averaging the 2004-2010 data about each variable for each CSP and we repeat the same process for the post-intervention years. Formally, we estimate

$$y_{it} = \beta_1 treat_i + \beta_2 Post_t + \beta_3(treat_i \times Post_t) + \beta_4 X_{it} + \epsilon_{it} \tag{3}$$

where $treat_i$ is a dummy variable thar is equal to 1 for the treated unit , $Post_t$ is another dummy variable that equals 1 in the period after the intervention, X_{it} is a matrix of the observed characteristics that have been considered as potential confounders in the SCM. The policy impact, conveyed though an Average Treatment Effect on the Treated (ATT) is captured by the estimate of β_3. Then, to exploit the longitudinal dimension of our data, we consider the following GDID model expressed through a two-way fixed effect regression model (Wing et al., 2018)

$$y_{it} = \gamma_i + \mu_t + \delta_1 D_{it} + \delta_2 X_{it} + \epsilon_{it} \tag{4}$$

where γ_i are CSP fixed effects, μ_t are year fixed effects, D_{it} is the treatment variable which is equal to one if a CSP i is treated at time t and 0 otherwise, and X_{it} is a matrix of the observed characteristics that have been considered as potential confounders in the SCM. In the GDID specification, the impact of the local policy is captured by the estimate of δ_1.

Finally, we also consider an alternative re-weighting strategy for the units in the control group with respect to SCM, given by the Trajectory Balancing (TJBAL) approach. We consider two balancing approaches: *(i)* the mean balancing, which reweights control units such that the averages of the pre-treatment outcomes and covariates are approximately equal at each point in time between the treatment and (reweighted) control groups; and *(ii)* the kernel balancing with intercept shift which goes beyond the mean balancing on the average trajectory by allowing for volatility and variance in the pre-treatment trajectories of the outcome and the covariates through a kernel-based feature expansion (Hazlett and Xu, 2018). These balancing approaches works properly also with short pre-treatment time periods that can reduce the SCM efficiency, rely on less stringent assumptions than the SCM on the structure of the data (Hazlett and Xu, 2018).

We apply these estimation strategies to a database containing the year observations for the 85 spatial units of observations in the 2004-2016 time span, providing 7 years of pre-intervention data. To define the synthetic control model specification, the pre-intervention periods is set between 2004 and 2010, since the devolution to local bodies became operative in 2011. We measure the propensity to report sexual crimes to the police through data for the police records on sexual offences on women above 16 following established literature (Card and Dahl, 2011; Lindo et al., 2018). We collected analogous time-series for potential confounders for women's vulnerability and for their propensity to report, as well as proxies for the local level propensity to report violent crimes that we include on the estimation as control variables (Aizer, 2010; Anderberg et al., 2016; González and Rodríguez-Planas, 2020; Miller and Segal, 2019). To this regard we consider local level data from administrative sources on: the share of unemployment, the real wage of female workers (2001 CPI), the share of female population aged 16-44, the share

of female officers, the homicide rate, the drug possession rate and the share of foreign population (See Appendix Tables S1-S2 for Data Sources and Descriptive Statistics).

4. Results

The SCM method evaluates the impact of the introduction of the local policy in Brighton and Hove by measuring the estimated difference in the propensity to report between Brighton and Hove and its synthetic counterpart. The reliability of the SCM results hinges on having a good level of affinity for the propensity to report sexual offences between Brighton and Hove and its synthetic counterpart in the absence of the local policy, so that it is reasonable to assume that the synthetic counterfactual can effectively reproduce Brighton and Hove. This is the case if the synthetic counterfactual mimics the actual Brighton and Hove before the policy intervention (Abadie, 2019). To gauge this affinity, we estimate the pre-intervention values for a set of predictors, namely several pre-intervention values for the outcomes and the other covariates which accounts for *(i)* demographics, *(ii)* economic features influencing women's vulnerability, *(iii)* crime features, and *(iv)* other public policies (Aizer, 2010; Anderberg et al., 2016; Miller and Segal, 2019), and we compare them between Brighton and Hove and its synthetic counterpart. Table 2[3] provides these estimates, showing that the values of the predictors in the pre-intervention period are quite the same between the actual Brighton and Hove and its synthetic counterpart. The control variables that we have included behave accordingly to extant literature (Aizer, 2010; Miller and Segal, 2019)**,** and postestimation shows that they contribute in a balanced way to the good level of pre-intervention affinity between Brighton and Hove and its synthetic counterpart (Appendix Table S4 - column 2). Table 2 also shows that the SCM specification is characterized by a high level of goodness of fit for the pre-intervention period between the two units (Pre-intervention RMSPE = 0.0659). The affinity between the synthetic control and Brighton and Hove is robust to alternative specifications for the arrays of considered pre-intervention variables, supporting the fact that our results are not driven by a particular specification for the SCM (Ferman et al., 2020; Kaul et al., 2015) (Appendix Tables S3-S4, Figures S3 for the comparison between the alternative SCM specifications). The synthetic control is designed assigning positive weights to 66 places in the donor pool, with 10 places receiving the highest weights (Appendix Table S5, Figure S4).

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[3] SCM, DID and GDID estimations are performed using STATA 14. For the SCM we used **synth** (Abadie, Alberto et al., 2014) and **synth_runner** (Galiani and Quistorff, 2017) for the inference. For the DID we used **diff** (Villa, 2016) and for the GDID **xtreg, fe.** TJBAL estimation is performed using **tjbal** (Hazlett and Xu, 2018) in R.

Table 2. Estimates of the pre-treatment fit between Brighton and Hove and its synthetic counterpart for the main SCM specification.

Variable	Brighton and Hove	
	Real	Synthetic
Reporting sexual crimes on women in 2009	4.98830	4.9999
Reporting sexual crimes on women in 2007	4.82755	4.8377
Reporting sexual crimes on women in 2005	4.97569	4.9862
Unemployment	6.73178	6.7506
Real wage of female workers	6.28467	6.2979
Female population between the ages of 16 and 44	0.4848	0.48774
Drug possession	-0.1437	-0.14462
Female police force	0.26208	0.2616
Pre-intervention RMSPE	0.0659	

Notes: all variables except lagged reporting of sexual crimes are averaged for the 2004-2010 period. Reporting of sexual crimes is measured for the count of sexual crimes records on women per 100 000 women in the CSP (logs). Unemployment is in share. Real wage of female workers is measured in 2001 CPI (logs). Share of female population is in logs. Drug possession are measured per 100 000 inhabitants (logs). The female police force is measured in share of total police force.
The synthetic Brighton and Hove is reproduced by a combination of 84 CSPs

Figure 3. Effects of the local policy for the provision of VAWG support services over time. Trends in the propensity to report sexual crimes to the police (in logs) for Brighton and Hove (black solid line) and the synthetic counterfactual (green dashed line). The vertical black dashed line marks the beginning of the local policy implementation (year 2011). The vertical red solid line marks the beginning of the country-wide high-impact media campaign referring to the "Operation Yewtree" (year 2013).
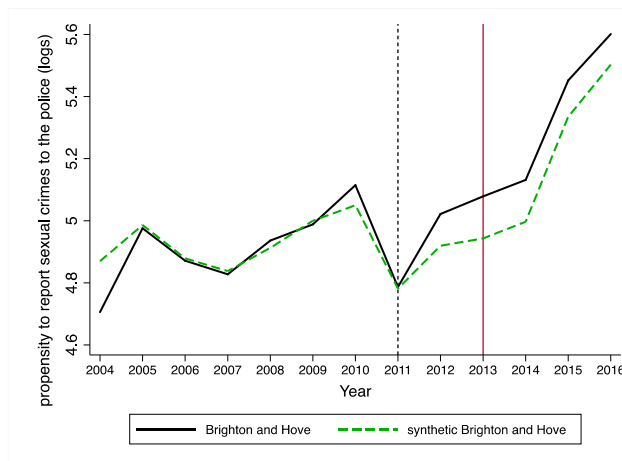

Figure 3 displays the propensity to report sexual offences for Brighton and Hove and its synthetic counterpart before and after the introduction of the local policy. The trend in the propensity to report for the synthetic control closely matches the corresponding trend for Brighton and Hove before the introduction of the local policy, as already showed by Table 2. The upward trend between 2008 and 2010 is concurrent with a commitment by the UK

Government to improve VAWG support services, which was pursued through national-funded programs that were later reduced due to the economic recession (Coy et al., 2011; Hirst and Rinne, 2012). The intervention effect is given by the difference between the line representing the rate of reporting to the police in Brighton and Hove and the line representing its synthetic counterpart after the implementation of the local policy in 2011. The discrepancy between the two lines is always positive, meaning that the local policy exerted a continuous positive impact after its introduction.

Another interesting finding shows that the positive impact of local support services is not substituted for by the potential competing effect of a country-wide campaign supporting victims to come forward. Figure 3 shows a relevant increase in the propensity to report for the synthetic counterfactual between the end of 2012 and the beginning of 2013, when the high-impact media coverage of the "*Operation Yewtree*" occurred. The media coverage of "*Operation Yewtree*" is acknowledged to be related to a country-wide increase in the propensity to report sexual offences to the police (Office for National Statistics - ONS, 2018). Notwithstanding this effect, the propensity to report in Brighton and Hove remains higher than the one in the counterfactual place, suggesting that the influence of the space-neutral campaign did not replace the role of nearby support services in helping victims to seek justice.

4.1. Inference

Table 3 shows that the positive impact of the local provision of support services is statistically significant for all the years after the introduction of the policy. Findings from Table 3 result from postestimation diagnostics and placebo tests (Abadie et al., 2010) in which we check how often results of the magnitude estimated for Brighton and Hove could be obtained by choosing a place at random for the study instead of Brighton and Hove (Appendix Figure S5 and Table S6 columns 1-2). The estimation results summarized in Table 3 supports that the gap between the propensity to report in Brighton and Hove and its counterfactual is relevant, since it is unusually large relative to the gaps for the places that did not implement the local policy (pseudo p-value$_{2011}$ = 0.0357; pseudo p-value$_{2012}$ = 0.0172; pseudo p-value$_{2013}$ = 0.0178; pseudo p-value$_{2014}$ = 0.000; pseudo p-value$_{2015}$ = 0.0170; pseudo p-value$_{2016}$ = 0.0357). Starting from 2012, the propensity to report in Brighton and Hove is 17.8% higher than in its synthetic counterpart. The gap widens in the following years: 29.6% in 2013; 29.3% in 2014; 27.8% in 2015; 22.2 % in 2016 (α_{2011} = 0.0607; α_{2012} = 0.1644; α_{2013} = 0.2592; α_{2014} = 0.2576 α_{2015} = 0.2459; α_{2016} = 0.2012). The positive and significant impact holds notwithstanding a potentially competing policy included within predictors, given by the share of female police officers (Miller and Segal, 2019). The findings summarized in Figure 3 and Table 3 highlight that controlling for the potential

competing policy measure given by female police officers does not offset the positive and significant impact of the provision of nearby support services. Moreover, being that the estimated treatment effect remains both positive and significant from 2013 onwards, we can further support that nearby support services are not substituted for by the "*Yewtree effect*". The leave-one-out falsification tests (Abadie, 2019) show that the results are not driven by the CSPs receiving the highest weights in the donor pool (Appendix Figure S6) and placebo tests (Abadie et al., 2015) show that the findings hold also when outliers are removed from the donor pool (Appendix Table S6 columns 1-2, Figure S7.a).

Table 3. Yearly estimated effects of the introduction of the local policy for the provision of VAWG support services on the survivors' propensity to report in Brighton and Hove (in logs) together with their statistical significance for the main SCM specification.

Brighton and Hove

Post-treatment period	Estimates of the treatment effect	Pseudo P-value
2011	0.0607**	0.0357
2012	0.1644**	0.0172
2013	0.2592**	0.0178
2014	0.2576***	0.000
2015	0.2459**	0.0170
2016	0.2012**	0.0357

***, ** and * denote statistical significance at the 1, 5 and 10 percent levels, respectively.
Estimates convey a measure of semi-elasticity since the outcome is in logs.

Findings remain consistent also when we restrict the donor pool of the places which are used by the SCM to design the counterfactual to places with a high level of detention rate related to sexual offences (Appendix Table S6 columns 3-4, Figure S7.b). This feature is acknowledged to stimulate the propensity to report by signaling a high commitment of the police force to proceed with an investigation after recording a sexual assault (Aizer and Dal Bó, 2009; Mazerolle et al., 2013; Spohn and Tellis, 2012). We also account for the potential existence of spillover effects such that the possibility that the local provision of nearby services in Brighton and Hove has a substantial positive effect on the propensity to report in the neighboring CSPs. The SCM estimates allows to evaluate the existence and the direction of the potential biases created by these spillovers (Abadie et al., 2015). The positive spillover effect of the local policy would provide an overestimate of the synthetic control trajectory, thus underestimating the positive impact of the local policy (Appendix Figure S6).

Table 4 outlines that the positive impact of the introduction of the local policy holds also when we estimate the competing specifications represented by the DID and the GDID models.

In the DID model with covariates, the estimated effect of the introduction of the local policy equals 0.257, (p-value = 0.000); in the GDID without covariates the estimated average effect of the introduction of the local policy between 2011 and 2016 equals 0.0504 (p-value = 0.001), to slightly increase to 0.059 (p-value = 0.003) adding the control variables. The overall post-treatment estimated impact in the SCM equals 0.1154. The GDID estimates hold considering several specifications. (Appendix Table S7). Results hold also estimating a Trajectory Balancing (TJBAL) approach (Hazlett and Xu, 2018), which provides robust estimates with few pre-treatment periods and it relaxes some assumptions inherent to the SCM (Appendix Figure S9 and Table S8).

Table 4. Estimates of the DID and the GDID models evaluating the impact of the introduction of the local policy for the provision of nearby support services on the propensity to report sexual offences to the police in Brighton and Hove.

Variable	Dependent variable: Report of sexual crimes on women to the police (for 100,000 inhabitants in logs)			
	DID		GDID	
	(1)	(2)	(3)	(4)
Brighton and Hove × (2011-2016) (Treatment Effect)	0.050 ***	0.257 ***	0.0504**	0.0590**
	(0.015)	(0.065)	(0.0153)	(0.0192)
CSP FE	NO	NO	YES	YES
Year FE	NO	NO	YES	YES
Control variables	NO	YES	NO	YES
No. of clusters	85	85	85	85
No. of observations	1 105	1 105	1 105	1 105
R-Squared	0.07	0.72	-	-
Within R-Squared	-	-	0.5003	0.4673

The period of observation is 2004-2016. Outcome variable is the propensity to report sexual crimes for 100 000 women (logs) Control variables are unemployment, real wage of female workers (logs), female population between the age of 16 and 44 (logs), drug possession (logs) and female police force. Standard errors are clustered at CSP level and they are in parenthesis. ***, ** and * denote statistical significance at the 1, 5 and 10 percent levels, respectively. DID estimation considers two time periods: pre-intervention period and post-intervention period. For each CSP, a pre-intervention period predictor is given by the 2004-2010 average of the same predictor. Similarly, a post-intervention period predictor is given by the 2011-2016 average of the same predictor

Table 5 summarizes the estimated treatment effect under the different model specifications. Notably, the GDID and the DID appear to provide a lower and upper bound to the estimates of the treatment effect, whereas the estimates from the TJBAL are close the SCM one. The difference in the magnitude of the GDID and the DID estimates compared to the TJBAL and the SCM can be understood recalling that the formers do not apply a weighting strategy to the

untreated units, whereas both the TJBAL and the SCM do, therefore designing different counterfactuals. We can conclude that the DID, the GDID and the TJBAL results supports our evidence as robust in spite of the various caveats associated to the different methodologies (Kreif et al., 2016).

Table 5. Comparison of results from the SCM, the GDID and the DID estimations

Method	Estimated ATT
SCM	0.1154
GDID	0.0590
GDID no outliers	0.0958
DID	0.257
TJBAL mean balancing	0.1822
TJBAL kernel balancing	0.0839

Results refers to model specifications including the control variables. SCM refers to the main specification summarized in Figure 3. GDID refers to Table 4, column 4. GDID with no outliers refers to Table S7, column 1 and DID to Table 4, column 2. The TJBALs refer to the models summarized in Figure S9.
The SCM estimate measures the overall before-after treatment gap between Brighton and Hove and "Synthetic Brighton and Hove". It is derived as the difference in before-after treatment differences of Brighton and Hove and "Synthetic Brighton and Hove" (Roesel, 2017)

5. Conclusions

Extant evidence details that policy initiatives can increase the propensity to report sexual offences by female victims. Yet, existing empirical works have not considered the impact of the provision of local support services, although the latter are widely acknowledged by institutions and practitioners as fundamental features in helping victims to seek justice. Results from this paper provide causal evidence supporting a positive impact of nearby support services on the propensity to report. The findings are consistent to the fact that nearby support services lower the victims' sense of isolation (Miles and Smith, 2019; Xie and Lynch, 2017) alongside providing necessary practical support such as a safe place to stay. The local availability of VAWG support services also signals to victims that there is a local commitment to a culture of respect of women, which can contribute to reducing the perception of socio-cultural prejudices (Ellsberg et al., 2015; Iyer et al., 2012). Evidence from the paper also shows that the relevance of nearby support services is not offset by national high-impact communication and media campaign and by other polices such as the enrollment of female police officers.

Overall, the analysis entails non-negligible policy implication by providing sound evidence supporting the policy initiatives for the provision of spatially even support services for the victims of sexual violence. Notably, the results from this paper support the aim of the Istanbul Convention, the first legally binding instrument covering all forms of violence against women,

which contains specific requirements for the spatial distribution of VAWG support services (Council of Europe, 2011; Kelly, 2018).

Obviously, this study considers only England and Wales, therefore there are questions about generalization of results to other contexts. Also, this analysis targets a behavioral outcome observed at the spatial level and not individuals. In other terms, results refer to the impact of a local policy on the local-level reporting rate and they do not convey any information about the individual probability of reporting.

Our analysis does not account for non-public source of funding for the local provision of VAWG support services. However, figures show that public funding accounts for the majority of the financial support for these services and that collecting money from alternative source of funding such as donations is particularly difficult for charities dealing with violence against women (Heady et al., 2011).

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Appendix

Data. The data on the local authorities' budget choices on VAWG support services are available at the Upper-Tier Local Authorities (UTLAs) level, being UTLAs the administrative authorities in England and Wales that were devolved the discretionary choice on the provision and the funding of local support services for the victims of sexual violence. In March 2019 (Office for National Statistics - ONS, 2019), the 152 English UTLAs consisted of: 27 Counties (Government Statistical System (GSS) code E10), 33 London Boroughs (GSS code E9), 36 Metropolitan Districts (GSS code E8), 56 Unitary Authorities (GSS code E06). The Welsh UTLAs consisted of 22 Unitary Authorities (GSS code W06).

The outcome variable is proxied by the police records on rape (UK Home Office crime code: 19C) and sexual assault on women aged 16 and above (UK Home Office crime code: 20A) (The UK Home Office, 2020). The Community Safety Partnerships (CSPs) are the smallest statistical geographies on which data on police records about sexual offences on women aged 16 and above are collected. There are 315 CSPs in England and Wales (GSS codes: E22, W14). The CSP coincides with the UTLA for Unitary Authorities, Metropolitan Boroughs and London Boroughs. Only for 26 Counties, a single UTLA (which is the County) includes several CSPs. In these cases, the policy decision taken at the UTLA level is applied to every CSPs belonging to the UTLA.

Figure S1 describes the geographies of UTLAs (Figure S1.a) and CSPs (Figure S1.b) for England and Wales.

Figure S1. Statistical Geographies of England and Wales considered in the investigation.

a

b

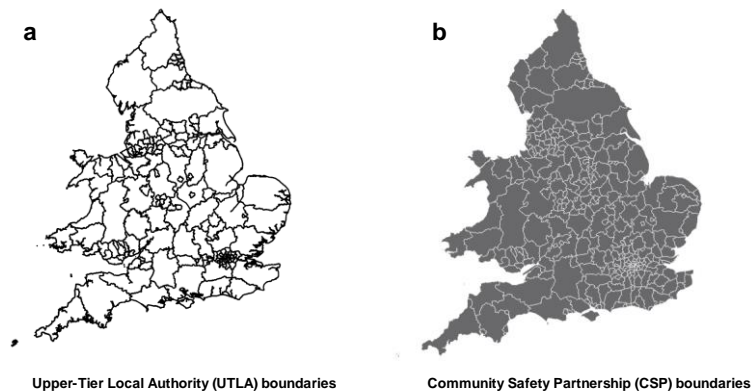Upper-Tier Local Authority (UTLA) boundaries   Community Safety Partnership (CSP) boundaries

Figure S2 outlines stages 1,3 and 4 of the design of the database to perform the SCM approach. Stage 1 refers to the mapping of the local policy choice about the provision of VAWG support services (Figure S2.a). Stage 3 is about the classification of CSPs with respect to the good management of sexual offences reports (Figure S2.b). Stage 4 is about discarding the CSPs which did not have any VAWG support service before the beginning of the austerity, the CSPs

benefitting from close-but different policies (Preston, Lincoln and Ceredigion benefitted from the startup of VAWG support services financed directly by the UK Government) and those CSPs characterized by idiosyncratic shock (Westminster) (Figure S2.c).

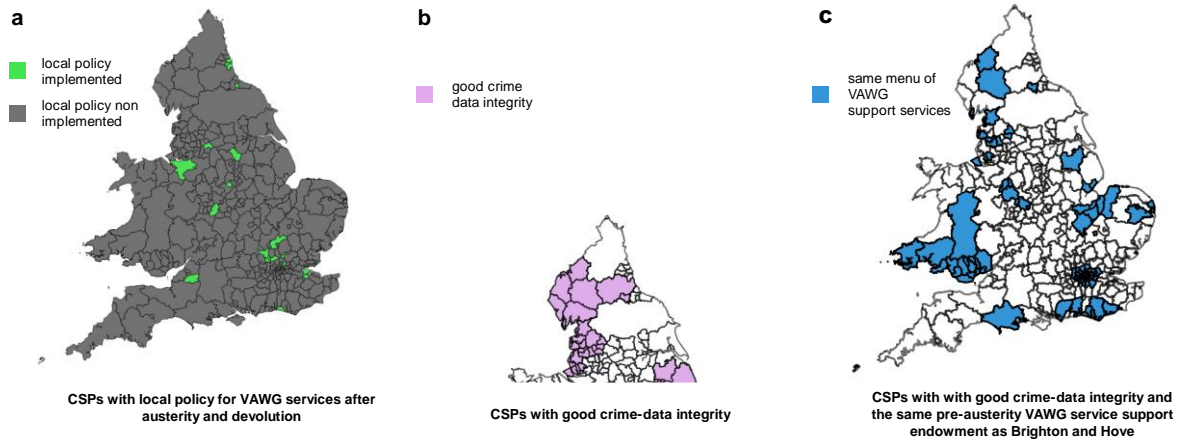*Figure S2. Selection of the suitable CSPs for the comparative case study.*

a

local policy implemented

local policy non implemented

CSPs with local policy for VAWG services after austerity and devolution

b

good crime data integrity

CSPs with good crime-data integrity

c

same menu of VAWG support services

CSPs with with good crime-data integrity and the same pre-austerity VAWG service support endowment as Brighton and Hove

Table S1. Data Sources

Variable	Source
Quality of police records on sexual offences	Her Majesty's Inspectorate of Constabulary and Fire & Rescue Services
Local policy for VAWG support services after austerity and devolution	Coy et al., 2011; Grierson, 2018; Holly, 2017; Towers and Walby, 2012; The Bureau of Investigative Journalism 2017; The UK Ministry of Justice 2015
Local endowment of VAWG support service before austerity and devolution	Coy et al., 2011
Police records of sexual offences to a female older than 16	The UK Home Office
Female population	ONS
Female population aged between 16 and 44	ONS
Unemployment	The UK Labour Force Survey
Wage differential	The UK Labour Force Survey
Drug crimes	The UK Home Office
Homicides	The UK Home Office
Crimes	The UK Home Office
Share of female police force	The UK Home Office
Foreign population	ONS

Table S2. Descriptive Statistics

Variable	Obs	Mean	Std Dev	Min	Max
Police records of sexual crimes on women per 100k women (logs)	1 105	4.5016	0.52106	2.0364	5.8358
Share of female population aged 16-44 years (logs)	1 105	0.4053	0.0728	0.2536	0.6077
Share of unemployment (sh)	1 025	7.0701	2.8092	1.0121	22.1797
Female real wage (logs)	1 095	6.2230	0.1878	5.7347	6.7678
Homicide per 100k inhabitants (logs)	1 105	- 0.1222	1.4068	-3.5572	4.0795
Drug possession per 100k inhabitants (logs)	1 103	- 0.0265	0.5477	-2.5661	2.7085
Share foreign population (logs)	1 095	2.2681	1.0287	- 0.0488	4.0726
Share of female police ranks	1 105	0.24959	0.02999	0.180907	0.34549

Results from SCM specifications and robustness checks

Figure S3 outlines the estimation results from the SC considering alternative model specifications to account for the concern that the results may be driven by specific set of predictors (Botosaru and Ferman, 2019). Following the ongoing debate in extant literature (Athey and Imbens, 2017; Kaul et al., 2015), we have considered several specifications characterized by different combination of predictors varying both the number of considered lags for the outcome variable and the set of covariates. The results summarized by Figure S3 suggest that our estimation does not suffer from "*specification-searching*" opportunities (Ferman et al., 2020), since findings remain consistent under every considered specification.

Table S3 details the pre-intervention values for the actual Brighton and Hove and its synthetic counterfactual under the different SCM specifications, showing the good level of pre-intervention affinity between the treated unit and the counterfactual does not depend on a particular set of predictors.

Table S4 shows the weights assigned by the SCM to each considered predictors in each model specification. Overall weights appear balanced across specifications, supporting that results are not driven by a specific predictor, such as a specific lag for the outcome variable and/or a specific combination of lags for the outcome variable. Table S4 also reports the values for the pre-intervention Root-Mean-Squared-Prediction-Error (RMSPE) for each SCM specification, which gives a measure for the goodness-of-fit of the synthetic counterfactual. All specifications appear as not suffering from poor goodness-of-fit, given the small values for the RMSPE.

Table S5 shows the weights assigned to every CSP in the donor pool by the main SCM specification summarized in Table 2 in the paper.

Figure S4 shows the CSPs receiving the highest weights pool by the main SCM specification summarized in Table 2 in the paper.

Figure S3. Effects of the local policy for the provision of VAWG support services over time. Trends in the propensity to report sexual crimes to the police (in logs) for Brighton and Hove (black solid line) and the synthetic counterfactual (green dashed line) for the considered SCM specifications. Brighton and Hove and its counterfactual behave similarly before 2011 in all the SCM specifications. After the adoption of the local policy, the propensity to report in Brighton and Hove is always higher than in the counterfactual in all the SCM specifications. (subset of covariates: share of female population aged 16-44, real wage of female workers, drug possession, unemployment, share of female police officers; full set of covariates: share of female population aged 16-44, real wage of female workers, drug possession, unemployment, share of female police officers; homicide rate, share of foreign population)
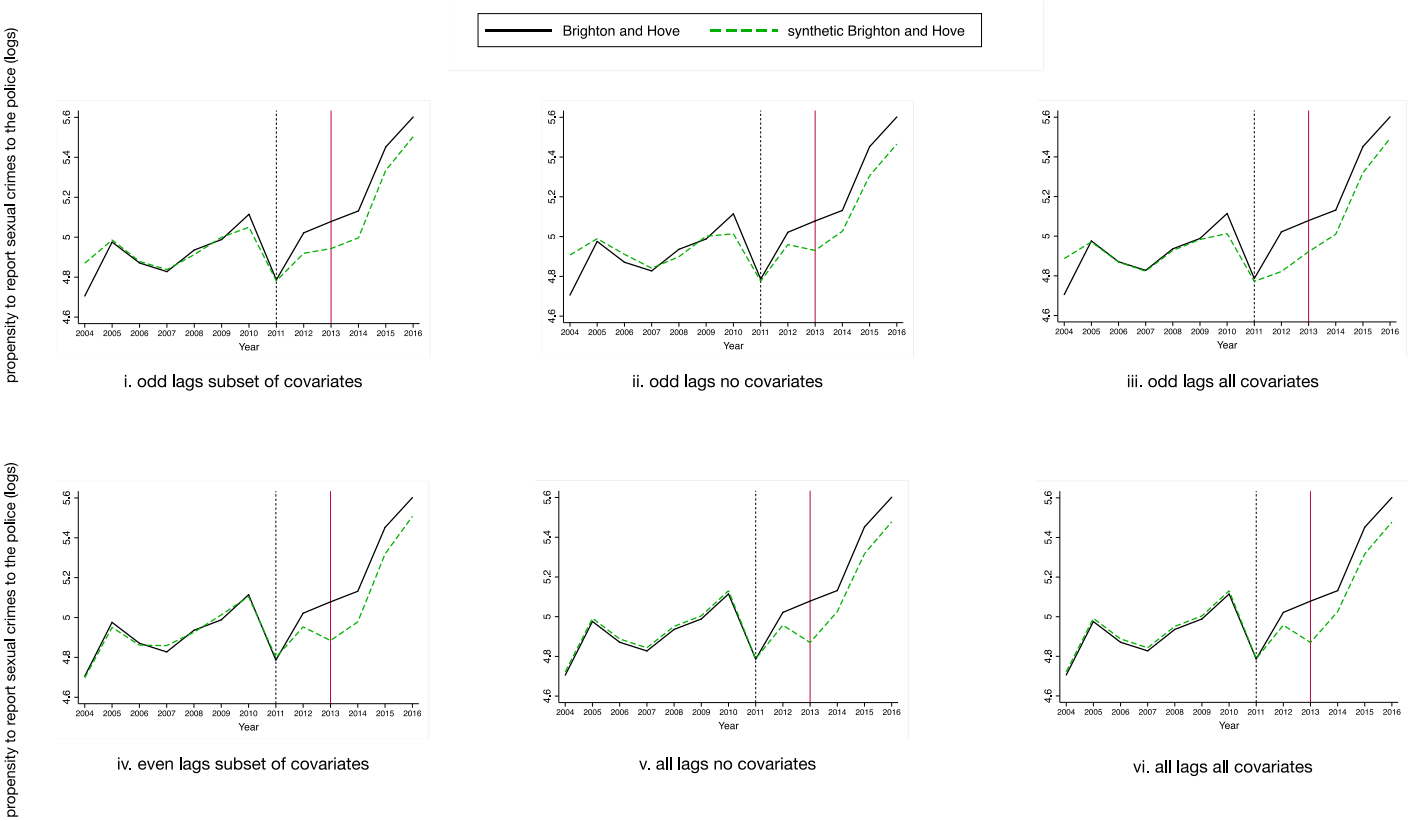
Table S3. Pre-treatment affinity measured through pre-treatment predictors means between Brighton and Hove and its synthetic counterpart for all considered SCM specifications.

	(1)		(2)§		(3)		(4)		(5)		(6)	
	Brighton and Hove		**Brighton and Hove**		**Brighton and Hove**		**Brighton and Hove**		**Brighton and Hove**		**Brighton and Hove**	
Variable	**Real**	**Synthetic**	**Real**	**Synthetic**	**Real**	**Synthetic**	**Real**	**Synthetic**	**Real**	**Synthetic**	**Real**	**Synthetic**
Reporting sexual crimes on women 2010	-	-	-	-	-	-	5.11452	5.1048	5.11452	5.130736	5.11452	5.1307
Reporting sexual crimes on women 2009	4.98830	4.9834	4.98830	4.9999	4.98830	5.0019	-	-	4.98830	5.00406	4.98830	5.0041
Reporting sexual crimes on women 2008	-	-	-	-	-	-	4.93593	4.9262	4.93593	4.951149	4.93593	4.9511
Reporting sexual crimes on women 2007	4.82755	4.8235	4.82755	4.8377	4.82755	4.84115	-	-	4.82755	4.843352	4.82755	4.8877
Reporting sexual crimes on women 2006	-	-	-	-	-	-	4.87113	4.8617	4.87113	4.887707	4.87113	4.8877
Reporting sexual crimes on women 2005	4.97569	4.9709	4.97569	4.9862	4.97569	4.98951	-	-	4.97569	4.991772	4.97569	4.9917
Reporting sexual crimes on women 2004	-	-	-	-	-	-	4.70581	4.6908	4.70581	4.722115	4.70581	4.7221
Unemployment	6.7318	6.9181	6.7318	6.7506	-	-	6.73178	6.7661	-	-	6.7318	7.8589
Real wage of female workers	6.28467	6.2657	6.28467	6.2979	-	-	6.28467	6.2691	-	-	6.2847	6.2752
Female population aged 16-44	0.4848	0.48453	0.4848	0.48574	-	-	0.4848	0.48524	-	-	0.4848	0.4807
Drug possession	-0.1437	-0.14237	-0.1437	-0.14462	-	-	-0.1437	-0.14246	-	-	-0.1437	-0.1033
Homicide	-0.2581	-0.24667	-	-	-	-	-	-	-	-	-0.2581	-0.3213
Foreign population	2.6346	2.653	-	-	-	-	-	-	-	-	2.6346	2.8091
Female police force	0.26208	0.2616	0.26208	0.2626	-	-	0.26208	0.2609	-	-	0.26208	0.25623

Notes: all variables except lagged reporting of sexual crimes are averaged for the 2004-2010 period. Reporting of sexual crimes is measured for the count of sexual crimes records on women per 100000 women in the CSP (logs). Unemployment is measured in share. Real wage of female workers is measured in 2001 CPI (logs). Homicide and drug possession are measured per 100000 inhabitants (logs). The foreign population is measured in share of total resident population (logs). The female police force is measured in share of total police force.
§ SCM main specification.

Table S4. Synthetic Brighton and Hove Predictor Weights for all considered SCM specifications

Variable	Weights					
	(1)	(2)	(3)	(4)	(5)	(6)
	odd outcome lags			even outcome lags	all outcome lags	
	all confounders	subset of confounders§	no confounders	subset of confounders	all confounders	no confounders
Reporting sexual crimes on women 2010	-	-	-	0.2207	0.1482	0.1482
Reporting sexual crimes on women 2009	0.3213	0.3326	0.3439	-	0.1469	0.1469
Reporting sexual crimes on women 2008	-	-	-	0.2232	0.1312	0.1312
Reporting sexual crimes on women 2007	0.2504	0.2510	0.2712	-	0.1142	0.1142
Reporting sexual crimes on women 2006	-	-	-	0.2287	0.1026	0.1026
Reporting sexual crimes on women 2005	0.3158	0.3842	0.3359	-	0.1310	0.1310
Reporting sexual crimes on women 2004		-	-	0.3093	0.2257	0.2257
Share of Unemployment	0.0056	0.0019	-	0.0008	1.03e-27	-
Real wage of female workers	0.00723	0.0014	-	0.0044	8.57e-26	-
Share of female population aged 16-44	0.00415	0.0369	-	0.0030	2.62e-27	-
Drug possession rate	0.00832	0.0038	-	0.0079	1.53e-26	-
Homicide rate	0.01066	-	-		8.75e-28	-
Share of female police force	0.03503	0.0365	-	0.0020	6.01e-27	-
Share of foreign population	0.02477	-	-		1.40e-26	-
Pre-intervention RMSPE	0.0791	0.0659	0.0864	0.02100	6.83e-11	5.89e-11

§ SCM main specification. It displays the highest goodness of fit among the SCM specifications with few lags for the outcome variables (Pre-intervention RMSPE = 0.0659). SCM specifications 1-4 do not display remarkable changes in the weights assigned to the considered predictors, which appear balanced across specifications.

Table S5. Weights given by the SCM estimation to the 84 CSPs in the donor pool to design the synthetic Brighton and Hove for the main SCM specification (Tables 2-3 and Figure 3; Appendix Table S3 column 2, Table S4 column 2 and Figure S3.i)

CSP	Weight	CSP	Weight	CSP	Weight
Barking&Dagenham	0.001	Fenland	0	Newham	0.002
Barnet	0.001	Fylde	0	Newport	0.001
Barrow-in-Furness	0	Great Yarmouth	0.001	Norwich	0.002
Bexley	0.001	Greenwich	0.001	Pembrokeshire	0.001
Blackburn&Darwen	0.001	Hackney	0.003	Pendle	0
Blackpool	0.001	Hammersmith&Fulham	0.001	Peterborough	0.002
Blaenau Gwent	0	Haringey	0.002	Powys	0.001
Boston	0	Harrow	0.001	Redbridge	0.001
Bournemouth	0.306	Hastings	0.006	Rhondda Cynon Taf	0.001
Brent	0.001	Havering	0.001	Richmond up Thames	0.001
Bridgend	0.001	Hillingdon	0.001	Rother	0.001
Bromley	0.001	Horsham	0	South Norfolk	0
Burnley	0	Hounslow	0.001	South Ribble	0
Caerphilly	0.001	Huntingdonshire	0.001	Southwark	0.002
Cambridge	0.001	Islington	0.306	St. Helens	0
Cannock Chase	0.001	Kensington&Chelsea	0.042	Stafford	0.001
Cardiff	0.074	King's Lynn W Norfolk	0	Stoke-on-Trent	0.056
Carlisle	0.013	Kingston up Thames	0.001	Sutton	0.001
Carmarthenshire	0.001	Lambeth	0.067	Swansea	0.013
Chichester	0.001	Lancaster	0.001	Tamworth	0.001
Chorley	0	Lewes	0.001	Torfaen	0.001
Croydon	0.001	Lewisham	0.001	Tower Hamlets	0.027
Darlington	0.001	Lichfield	0.001	Vale of Glamorgan	0.027
Dorset	0	Merthyr Tydfil	0	Wandsworth	0.003
Ealing	0.001	Merton	0.001	Wealden	0
Eastbourne	0.001	Monmouthshire	0.001	West Lindsey	0
Eden	0.001	Neath Port Talbot	0.001	Wirral	0
Enfield	0.001	Newcastle un. Lyme	0.001	Worthing	0.001

Inference and Robustness

Outliers are detected measuring the value for the preintervention mean squared prediction error (MPSE) for each CSP. Those CSPs with a preintervention mean squared prediction error (MPSE) five times larger than the MPSE of the treated CSP (Abadie et al., 2010) are discarded since they are outliers. Figure S5 outlines the estimated pseudo p-value referring to the significance of the policy effect for 2011-2016, removing the outliers (Galiani and Quistorff, 2017).

Figure S5: p-values per period for posttreatment periods for the standardized effects of the local policy in the placebo tests.
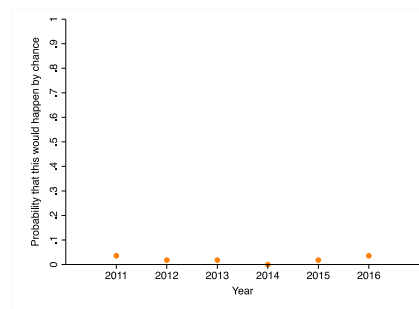
Figure S6 shows the results from the leave-one-out falsification tests and from the accounting for potential spillovers (SUTVA violation).

Figure S6: Leave-one-out falsification tests for the SCM model and accounting for potential spillover of the local policy in Brighton and Hove on neighbouring CSPs.

| leave-one-out: Bournemouth | leave-one-out: Islington | removing neighbouring CSPs |
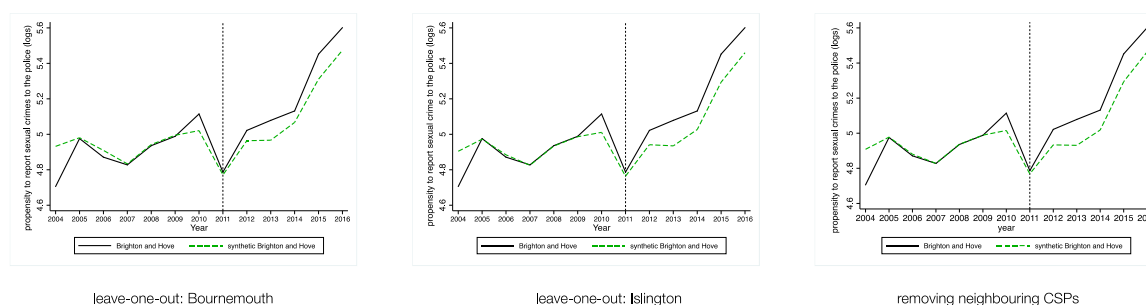
Table S6 summarizes the pre-intervention affinity designed by the SCM when we *(i)* remove the outliers, *(ii)* impose a further restriction on the donor pool discarding the CSPs with a low detection rate for sexual crimes.

Table S6. Pre-treatment affinity measured through pre-treatment predictors means between Brighton and Hove and its synthetic counterpart without outliers (columns 1-2) and with a further restriction on the donor pool (columns 3-4)

	No outliers		High sanction/ detection rate for sexual crimes	
	(1)§		(2)°	
	Brighton and Hove		**Brighton and Hove**	
Variable	**Real**	**Synthetic**	**Real**	**Synthetic**
Reporting sexual crimes on women 2009	4.98830	4.9430	4.98830	4.98167
Reporting sexual crimes on women 2007	4.82755	4.7831	4.82755	4.82623
Reporting sexual crimes on women 2005	4.97569	4.9299	4.97569	4.98023
Unemployment	6.7318	6.6664	6.7318	6.97829
Real wage of female workers	6.28467	6.2225	6.28467	6.30203
Female population aged 16-44	0.4848	0.4807	0.4848	0.47946
Drug possession	-	-	-0.1437	-0.12290
Female police force	0.26208	0.25961	0.26208	0.25102
Pre-intervention RSMPE	0.06101		0.08297	

§ The donor pool used in the SCM summarized by (1) consists of 56 CSPs. The 28 CSPs that have been removed are outliers entailed with a bad pre-intervention fit (pre-intervention MSPE 5 times higher than the one of Brighton and Hove(Abadie et al., 2010)). Inference after removing the outliers delivers robust findings on the positive effect of the policy intervention. The yearly impact of the policy is positive and significant (Table 2 in the main paper, Figure S5). The model has good fit (Galiani and Quistorff, 2017) as summarized by the non-significance of the proportion of placebos that have a pre-treatment RMSPE at least as large as the average of Brighton and Hove (avg_pre_rmspe_p = 0.982)

°The donor pool used in the SCM summarized by (2) consists of 59 CSPs. The 25 CSPs that have been removed display low rate of detention/sanction rate for sexual crimes (RMP Rape Monitoring Group, 2016) and they refers to the following Police Force Areas: Cumbria, Dorset, Dyfed-Powys, Lincolnshire, Merseyside, South Wales and Staffordshire.

All variables except lagged reporting of sexual crimes are averaged for the 2004-2010 period. Reporting of sexual crimes is measured for the count of sexual crimes records on women per 100000 women in the CSP (logs). Unemployment is measured in share. Real wage of female workers is measured in 2001 CPI (logs). Drug possession is measured per 100000 inhabitants (logs). The foreign population is measured in share of total resident population (logs). The female police force is measured in share of total police force.

Figure S7 outlines the estimation results from the SC when *(i)* outliers are removed and *(ii)* when CSPs with a low detention/sanction rate for sexual crimes are discarded.
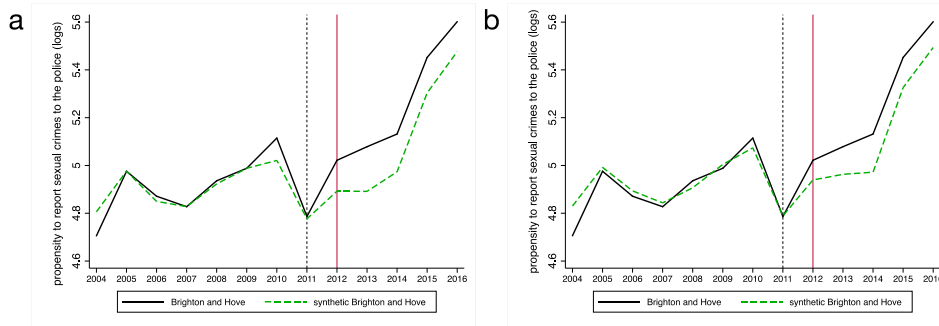
Figure S8 outlines the visual investigation for the parallel trends assumption for the DID and the GDID. Table S7 summarizes several robustness checks for the GDID model.
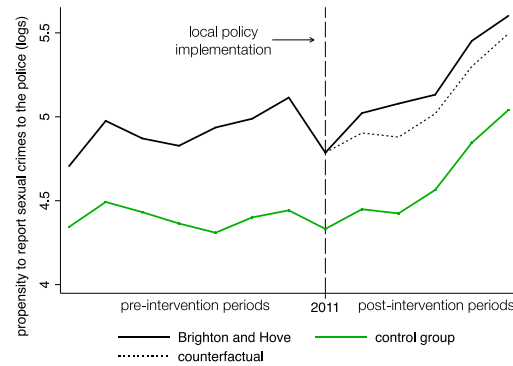
Table S7. GDID estimates for the local policy impact under several robustness checks: (i) without the outliers detected through the SCM estimation (column 1); (ii) dropping CSPs with a low detention rate for sexual crimes (column 2)

Variable	Dependent variable: Report of sexual crimes on women to the police (for 100,000 inhabitants in logs)		
	No SCM outliers	No low detention rate CSPs	All CSPs bootstrap se
	(1)	(2)	(3)
Brighton and Hove ✕ (2011-2016) (Treatment Effect)	0.0958 ***	0.0776***	0.0590***
	(0.0219)	(0.019)	(0.0145)
CSP FE	YES	YES	YES
Year FE	YES	YES	YES
Control variables	YES	YES	NO
No. of clusters	57	73	85
No. of observations	687	870	1 105
Within R-Squared	0.5123	0.4486	0.4673

The period of observation is 2004-2016. Outcome variable is the propensity to report sexual crimes for 100 000 women (logs) Control variables are unemployment, real wage of female workers (logs), female population between the age of 16 and 44 (logs), drug possession (logs) and female police force. Standard errors are clustered at CSP level and they are in parenthesis. ***, ** and * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Figure S9 outlines the balance between the outcome and the control variables in the pre-treatment periods alongside the pre and post treatment trend for the outcome variable for the TJBAL approach with mean balancing and kernel balancing. The post balancing differences are given by $(\text{mean}_{\text{treated}} - \text{mean}_{\text{control}}) / |\text{mean}_{\text{treated}}|$. All the periods of the pre-treatment outcome as well as the five covariates are included because TJBAL does not require to set aside a validation period and it allows for minimizing user discretion when choosing pre-treatment variables (Hazlett and Xu, 2018). The mean balancing estimation is performed without taking out the average of pre-treatment outcomes for each unit (demeaning), given the high number of units in the control groups (Hazlett and Xu, 2018). The kernel balancing estimation is done with demeaning because from the GDID estimation we can assume plausibility of parallel trends (Hazlett and Xu, 2018). The smaller value for the bias ratio in the mean balancing shows that this estimator provides a higher improvement than the kernel balancing. Table S8 outlines the TJBAL estimated impact of the local policy for each year after the policy intervention.

Figure S9. TJBAL estimates for the pre-post treatment trends of the propensity to report sexual crimes to the police and for the mean balance between the outcome and the covariate in the pre-treatment periods under the two considered approaches (mean balancing and kernel balancing with demeaning).
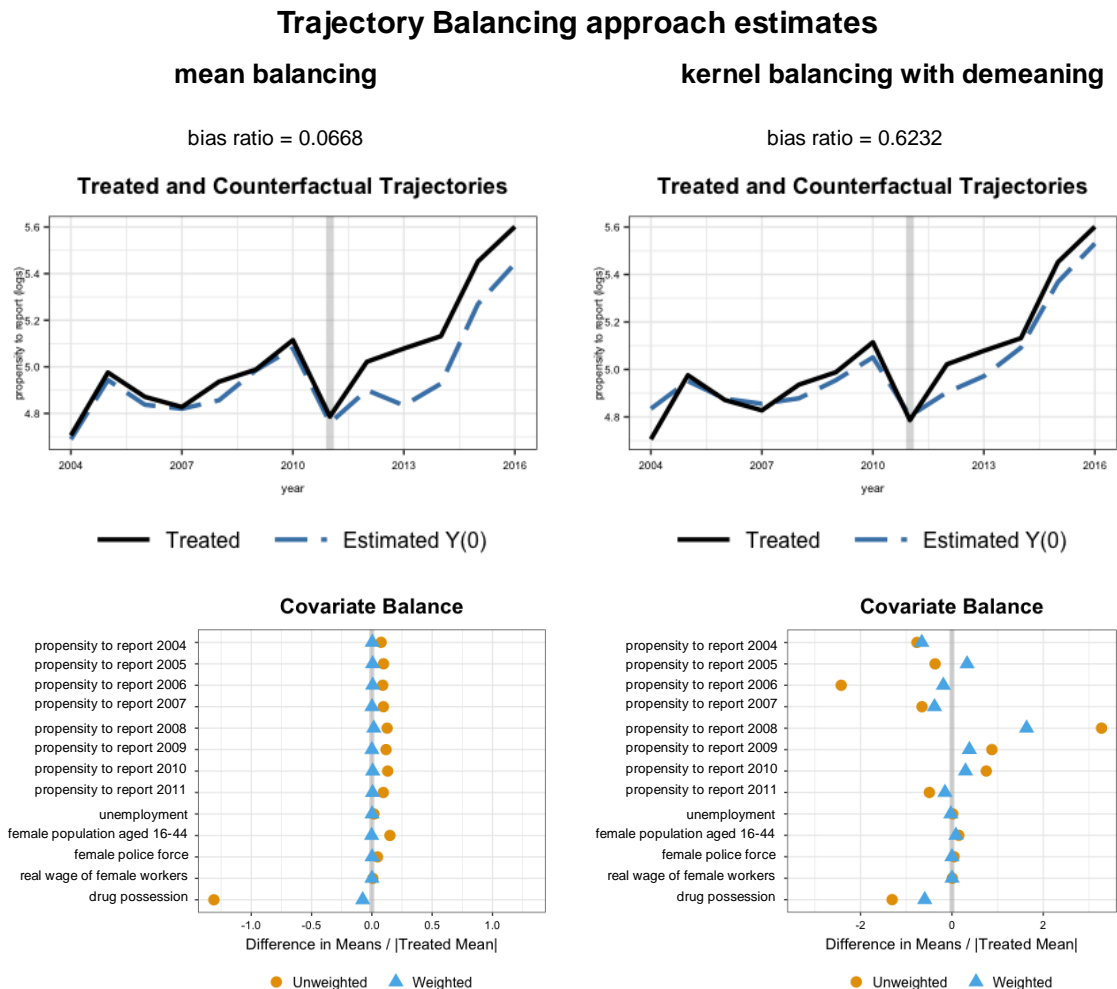

Trajectory Balancing approach estimates

Table S8. TJBAL yearly estimated effect for the local policy impact under mean balancing and under kernel balancing with demeaning

Brighton and Hove

	Estimates of the treatment effect (mean balancing)	Estimates of the treatment effect (kernel balancing)
2011	0.0275	0.0813
2012	0.1218	0.1180
2013	0.2442	0.1070
2014	0.2038	0.0401
2015	0.1828	0.0837
2016	0.1583	0.0708