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Shedding Light on the Dark:

The Impact of Legal Enforcement on Darknet Transactions¹

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ABSTRACT

Darknet markets have been increasingly used for the transaction of illegal products and services in the last decade. In particular, it is estimated that drugs make up two-thirds of darknet market transactions. The growth of illicit transactions on darknet markets have led enforcement agencies to invest greater proportion of time and efforts to monitor and crack down on criminal activities on the darknet websites. Despite the successes in convicting perpetrators, it is unknown whether these policing efforts are truly effective in deterring future darknet transactions, given that the identities of the transacting parties are very well protected by the markets' features. To this end, this study attempts to empirically evaluate the susceptibility of darknet markets breaking down upon police arrests of dealers and buyers. Using drug review data from a few major darknet markets, we discovered a deterrence impact – the transactions in the targeted country would decrease on the market where arrests occurred. More interestingly, we find that disclosure of the arrests will influence the transactions of vendors not only from the same country but also those

¹ we gratefully acknowledge financial help from the NET Institute www.NETinst.org.

from other countries that did not experience any arrest incidents. As the darkweb techniques have led to a considerable proliferation of online drug trading, we believe that study findings that reveal the nature of these illicit markets will have key policy and theoretical implications to law makers, enforcement agencies, and academicians.

INTRODUCTION

Globally, darknet markets have been increasingly used for the transaction of illegal products and services over the last decade. A combination of factors make darknet markets ideal avenues for trading illicit goods: 1) these sites are run as “hidden services” via Tor (Dingledine et al., 2004, Benjamin, et al., 2019), which conceals information such as the location, IP address, or identity of its users, making these transactions anonymous, 2) the use of crypto-currencies (e.g., Bitcoin) for these transactions enhances user-anonymization (Nakamoto, 2008), 3) advanced privacy-enhancing techniques (e.g., Invisible Internet Project) are deployed to further mask the identity of the transaction parties and hide the existence of the transactions (Moore and Rid, 2016), and 4) communications between vendors and consumers on the darknet are encrypted using Pretty Good Privacy to avoid surveillance and monitoring by third parties. Jointly, these multiple techniques offer ironclad protection for illicit traders, which facilitates the steady growth of these markets (Soska and Christin 2015).

Of importance, the majority of darknet transactions involves drug trading, which is estimated to account for approximately two thirds of this market (European Monitoring Centre for Drugs and Drug Addiction²). Compared to traditional drug sales on the street, transacting on darknet markets are perceived to hold a lower risk of detection and violence, providing motivation for more buyers

² <https://www.europol.europa.eu/publications-documents/drugs-and-darknet-perspectives-for-enforcement-research-and-policy>

and sellers of illicit products/services to transact online (Chan et al. 2019). The digital nature of the Internet breaks down geographical frictions (Overby and Forman 2015), facilitating successful transaction of drug sales at global level. Moreover, with access to a larger set of drug dealers online, buyers on the darknet markets have access to a wider range of drugs (Brynjolfsson et al. 2003). These characteristics of darknet platforms increase the willingness of sellers and buyers to participate, thereby fueling the increase in the market size and the transaction volume of drugs in this market. The expansion of this illicit market requires the attention of enforcement authorities. Yet, the policing of these illegal transactions is met with difficulties in two main ways. First, the concealment of the presence of transactions and the protection of the identities of market participants via the darknet techniques have made it hard to detect and prosecute the market participants. Second, the cross-national nature of darknet transactions poses challenges to legal enforcement, given that the prosecutorial reach of authorities is constrained by geographic boundaries.

Despite these challenges, the enforcement agencies have attempted to monitor and prosecute darknet crimes, through a series of local arrests and multi-national crackdowns. A handful of policing efforts have seen some success, by which some marketplaces were successfully infiltrated, allowing for the revelation and arrest of the perpetrators and the closure of these sites. Although initial efforts have paid off, critics argue that policing efforts are ineffective in deterring buyers and sellers in making future transactions (Popper 2019). Following the arrest of a few people and the closure of a few sites, new sites and transactions might show up elsewhere in the darknet network. Instead of deterring future participation, enforcement may simply be pushing participants to move deeper into darknet. Empirically, it is unclear how these policing efforts affecting future darknet transactions. On the one hand, enforcement efforts may set off the alarms of the transacting

parties, causing them to reduce their transaction intensity and/or move to alternative transaction sites to avoid detection. Yet, on the other hand, it is possible that the market participants may continue in their existing level of transactions, given that they have faith in the security and anonymization techniques that the darknet platform offer. Here, it is important to understand how localized arrests on darknet sites might change the behavior of market participants on the same platform within and across countries.

To provide answers to the above research questions, we collected data on darknet transactions and arrest information to investigate the impacts of legal enforcement on these markets. In particular, we examine the impacts of police arrests targeted toward offenders of the same country as the enforcing agency and the impacts on transactions of vendors in countries different from targeted areas. Specifically, we evaluate the change in transaction levels of a) participants who reside in the same prosecutorial jurisdiction by which the enforcement agency that made the arrests, and b) participants who reside in locations outside the legal jurisdiction by which the agency made arrests.

Our analyses on the domestic arrests show that there is a downward pressure on the number of vendors, variety of drugs for sales, and total transactions on the affected site for participants who reside in the same country as that of the enforcement agency. At a vendor level analysis, we found evidence that the average transaction level for these local vendors has dropped following news of arrests made on vendors residing in the same country, suggesting that each seller is selling at a lower intensity than before. Interestingly, we find that news of arrests can also have a dampening effect on vendors who reside in countries where policing efforts did not take place. At the same time, we find the arrests can also reduce the number of vendors operating in the market. The decrease in vendor count appears to be long term in nature and we do not find clear evidence of vendors migrating to alternative darknet markets. We interpret this finding as arrests causing

vendors to permanently exit the market. Furthermore, we find that a presence of heterogeneity effects, wherein small vendors are more affected by the arrest shock compared to large vendors. This finding suggests that the deterrent effects of policing tend to affect the risk-adverse sellers who are not engaged in large scale illicit trading on the darknet markets. These results provide insights on the nature of darknet markets and their participants and have meaningful policy and theoretical implications to law makers, enforcement agencies, and academicians.

LITERATURE REVIEW

With the rise of the darknet markets, there is an increasing research interest on the topic. The majority of extant work have attempted two main approaches to study a variety of questions on the darknet markets. The first approach analyzes the data available on the darknet sites, and makes inferences about the various variables found on the platforms. For instance, Dolliver and Kenney (2016) examines the characteristics of drug vendors and contrast them across different darknet sites. Norgaard et al. (2018) studied the network structure of darknet markets, and find that their interaction networks structure is less hierarchical and slightly more monopolistic than that of the traditional black market. In a related vein of work, Broséus et al. (2017) examined trafficking flow of illicit products across countries transacted on darknet markets. Finally, Hardy and Norgaard (2016) explores whether sellers' reputation determine the prices of the goods sold in these illicit online markets. The second approach attempts to draw a relationship between the activity on darknet sites and offline observations and trends. Rhumorbarbe et al. (2016) examined the accuracy of the information posted on darknet sites by contrasting it with the physical characteristics of the transactions. Through actual drug purchases made on a darknet site, they found that digital information (e.g., concealment methods and shipping country) are accurate, but the illicit drugs purity is found to be different from the information indicated on their respective

listings. In another large-scale study, Dittus et al. (2018) determine the darknet trading geography of three plant-based drugs across four of the largest darknet markets, and compare it to the global footprint of production and consumption for these drugs. They found that cannabis and cocaine vendors are primarily located in a small set of consumer countries, rather than producer countries, suggesting that darknet trading happens at the last mile. Van Buskirk et al. (2016) recruited drug users and surveyed them to understand the factors associated with drug purchase from darknet markets. They found that participants who purchased from dark net marketplaces tended to be younger, more likely to be involved in recent property crime and to have used more classes of drugs in the preceding six months, relative to other users of psychostimulant drugs. Given the difficulty in linking offline observations with the obscured online data from darknet sites, there is understandably fewer studies of the second type. Consequently, our knowledge of the economic interactions of darknet transactions with real world events are limited, representing a gap in the literature of darknet markets.

Another related stream of work pertains to the literature on the deterrence effect of policing efforts. Early work on the topic is centered on identifying the presence of a deterrence effect. Erlich (1975) has found an effect of capital punishment to exist, while Hoenack et al. (1980) and Layson (1985) have raised doubts about this finding. Subsequent works have continued to empirically validated the presence of a deterrence effect with stronger identification strategies (e.g., Cover and Thistle 1988; Kessler and Levitt 1999). Apart from the economic estimation of the deterrence effects, scholars have also attempted to theorize the possible factors that could influence the potency of policing efforts on deterring criminal behavior. In particular, Silberman (1976) found that the perceived certainty of punishment, level of conformity to legal norm, and the offense type are crucial factors that can influence the likelihood of a deterrence effect materializing. Under the

novel context of drug-based transactions on the darknet markets, it is unclear if a deterrence effect would ensue given that these factors are distinct from those in past settings. As elucidated previously, technological affordances of darknet markets can drastically reduce the likelihood of detection and identification of transacting parties; differences in legislative punishment across countries can make it for homogenous criminal persecution to take place. These aspects can drastically lower the perceived certainty of punishment of transacting parties on darknet markets. Moreover, the sales and use of certain drugs may not be deemed as risky or as a criminal activity (Kilmer et al. 2007; Babcock and Byrne 2010). The inconsistencies in such perceptions would mean that transaction parties find it ethically acceptable to buy and sell drugs on the darknet. As such, the question of whether arrests would deter subsequent transactions on the darknet platforms is largely empirical in nature.

BACKGROUND AND DATA

Darknet markets

Darknet markets function in a similar way as regular commercial websites like Amazon and eBay, where buyers and sellers trade for various goods/services. A key difference is that transactions on darknet marketplaces are mostly illicit, featuring drugs, weapons, and stolen cards et al. The crutch for such illegal conducts to prosper is the anonymous protection provided by darknet markets. For the risk of detection and ensuing sanction, participants of illegal transactions must first seek for good concealment to hide themselves far from the police. Darknet markets, with the integration of different technologies, right confer advantages to ensure the anonymization and security needed in illegal activities. First, darknet markets use Tor's hidden service model (Dingledine et al., 2004), which encrypts and transmits information through a series of random selected nodes in a network. Known as the onion routing, this transmission mechanism makes it immensely difficult to trace

back users' IP addresses and location. Good concealment on identities hence makes darknet markets a trustworthy and reliable medium for illicit exchange. Second, communications between vendors and consumers on the darknet can be encrypted using PGP (Pretty Good Service) to avoid surveillance and monitoring by third parties. Advanced privacy-enhancing techniques (e.g., Invisible Internet Project) are also deployed to further ensure the privacy and anonymity of darknet transactions, hence reducing the risk of being intercepted. Third, on the financial side, cryptocurrencies are commonly adopted on darknet markets to achieve payment anonymization. Because cryptocurrencies like bitcoin are designed to be free from control and surveillances from outside institutions (Nakamoto, 2009). Any two people can complete peer-to-peer payments in a decentralized system without revealing their identities. There are also tumbling/mixing services which further obscure traces and sources of bitcoin coming from a darknet market. All these features together provide considerable anonymization and privacy protection for users on darknet markets, which are no surprise to become the very active place for illicit trading.

Among all the transactions on darknet markets, drug trading is the major offering and estimated to account for approximately two thirds. We hence use drug sale data as the major analyses in our empirical section. The prosperity of drug trading on darknet markets should be first owed to the anonymization services supported by aforementioned various technologies. Compared to traditional on-street drug exchange, darknet markets reduce exposure to the risk of detection and violence, constituting further motives for online trading. The flatten nature of the Internet also makes drug distribution much more readily accessible to almost every corner of the worldwide. The unprecedented connections elevate illicit drug sales to an astonishing wide range and increase the availability of drugs to a greater degree. What people need are just a few clicks to have their drug parcels delivered directly to the front of their door. It's also suggested that purchasing from

darknet markets enable consumers to get a wider range of different categories of drugs. And such online traded drugs are perceived to have higher quality than alternative offline sources (Barratt et al., 2014). This is because online markets adopt escrow service to make fraud costly, incorporate forums and feedbacks to allow buyers value sellers' reputation (Armona 2018). Multilayer protection on privacy and all such advantages make darknet markets much attractive for drug trading, which has been recognized as a growing threat to the society security and also creates significant challenges for law enforcement authorities. It's hence very necessary and important to understand of the functioning of darknet markets and their adaptive feature, so that appropriate actions can be taken to respond to this new drug trading paradigm.

Data Description and Processing

To fulfill the research goal of assessing the impacts of police enforcement on darknet transactions, we capture news of police arrests from published online reports and discussions for subsequent analyses³. To make sure the reliability and soundness of the arrest data, we also recruited two research assistants to verify the original news source of each arrest event. The success of arrest enforcement and its timing are plausibly exogenous for a few reasons. First, these darknet market crackdowns are conducted in secrecy, to which sellers and buyers do not have foreknowledge of, hence are unable to react preemptively. Second, it takes time and effort for crackdown team to infiltrate the darknet site, gather evidence on the identities of the transacting parties, and arrest these perpetrators. On top of the variation in the success of making arrests across perpetrators, the time elapsed leading to each arrest (if succeeded) can vary, as the ease of getting evidence and gaining cooperation from local authorities for making the arrest are different. In addition, the public announcement of these arrests on news are subjected to another layer of variation, making

³ <https://www.gwern.net/DNM-arrests#data>

it very hard for transacting parties to learn of the exact timing and location of each crackdown to evade them. We hence believe that arrest events are a helpful resource to investigate the impacts of police enforcement on combating darknet tradings. In particular, we focused on one famous arrest event that appeared on Silk Road 2 in June 2014⁴. It's reported that one SR2 seller with the nickname "Xanax King" and his associates were arrested. The police later controlled this account and arrested around 60 buyers related with this enterprise. We therefore adopt the press-release date (i.e., June 2nd, 2014) for the arrest of the final group as our exogenous shock time point.

In our analysis, we focus on the arrests of drug-related perpetrators and trading activities of drug products as this is the largest class of transactions on darknet markets. In particular, through a collection of webpage scrapes on darkweb sites, we parsed out the vendor information (country of origin is given by "shipped from" field), product information and buyer feedback information for transactions that took place on Silk Road 2. In addition, we also collected the webpages of Agora and Evolution, which were two other leading markets during the operation period of Silk Road 2, and will be as comparison in our analyses. We constrain our study period from April, 2014 to October, 2014, during which no more additional arrests took place on Silk Road 2 while Agora and Evolution were also free from any arrest enforcement. This time window enables us to avoid contaminations from other arrests and more precisely quantify the impacts of enforcement efforts.

Darknet markets are unique in that the platforms would transfer crypto payments to the vendor only after the consumer verified that he/she has received the ordered product and leaves a review for the vendor (Christin 2013, Armona 2018, Dittus et al. 2018). This is done to make fraud costly and incentivize vendors to be honest. Thus, past works associate the number of feedbacks with the number of transactions on darknet markets. As expected of all scraping exercises, the scrapes are

⁴ <https://www.gwern.net/DNM-arrests#usa-5>

subjected to intermittent crawling blocks. To reduce the impact on such blocks on the data integrity, the scrapes are conducted on a daily basis such that duplicated data is available for recovery of the complete set of feedbacks for each vendor. We further validate the integrity of the scraped data by comparing it with the search results of a widely used darknet search engine, GRAMS. The comparison revealed that the number of products harvested from the two sources are not significantly different, suggesting the integrity of scraped data.

EMPIRICAL ANALYSES

Treatment effect on the focal website

We investigate the impact of arrest disclosure on the focal website's transactions. The data we have include review data from major DNMs from 2013 to 2015. Specifically, we choose the data from three DNMs Silkroad2, Agora, and Evolution because the following reasons: 1). They have hit the top rank of product offerings among all DNMs based on the most famous dark web search engine Gram's statistics during the analysis period, as can be seen in Figure 1. Hence, we think the results derived from these three websites should be able to generalize to customers' behaviors in other websites. 2). There was one arrest news report on Silkroad2 at June 2nd, 2014. It was reported that one major U.S. drug supply and about 60 buyers were arrested. The report can be viewed as an exogenous shock to other vendors on the DNMs. It is hard for DNM sellers and buyers to estimate when policemen from different countries would initiate an action to arrest illegal users or staff on these DNMs. 3). Around Silkroad2's arrest on June 2nd, 2014, there were no arrests on Agora or Evolution so that we could exclude potential confounding effects.

We rely on a difference-in-differences (DID) model to estimate the impact of arrests news on subsequent transaction activities on the focal darknet markets. The treatment group in our analysis

is the U.S. vendors on Silkroad2 and the control group is non-U.S. vendors on Agora and Evolution. This makes sense because generally the police will only consider criminals in their own country. And to our best knowledge, there wasn't any other international cooperated operation during the time period except the November operation. For the “*after*” dummy, we define it to be 1 for observations after June 2nd, 2014. To abstract from the influences from unobserved factors in the early periods of these three websites, we base our analysis from April 2014, after the sites have matured and stabilized. To have a complete view on the short term and long term effects of arrest publicity, our analysis period ends on Oct 2014 when is about one month before the Silkroad2's shutdown. The main dependent variables are number of total reviews, average reviews per vendor, and number of vendors with positive review of each drug type within every week. We focus on drug reviews as the arrest news in our data set is about drugs and a large portion of reviews on these two websites are also drug reviews. The summary statistics of main variables are displayed in Table 1.

The main regression models are as follows:

$$y_{it} = \alpha_0 + \alpha_1 * treat_i + \alpha_2 * after_t + \alpha_3 * treat_i * after_t + week_t + drug_i + \varepsilon_{it},$$

where y_{it} is the logarithm transformed dependent variables as we can see the percentage change of total reviews, average reviews and number of vendors after the arrests were publicized. As all dependent variables are count variables, we also report the results using fixed effects negative binomial models as robustness checks.

Columns (1)-(3) of Table 2 summarizes the main results of the DID analysis. We can see that compared with the platforms without any arrest events, the number of product listings, total reviews, and vendors of the focal website in the same country decrease significantly after its first

arrest news released, as the coefficients of the after dummy are consistently negative and significant in different specifications.

Treatment effects on vendors from other countries

One common belief in the existing literature is that the effects of police's operations are restricted to the criminal activities within the same country, as different countries are subject to varying legislative regulations. We would like to test if the arrest on U.S. vendors would negatively influence other vendors from different countries. As the transactions on DNMs are highly dependent on the anonymity and security of the website, a public arrest from Silkroad2 would undermine the reliability of the website. Therefore, we compare non U.S. vendors' review information on Silkroad2 with that of those non U.S. vendors in our control group to see if the negative effect would spread to non-focal countries. The results are reported in the columns (1)-(3) of Table 3. We have observed qualitatively equivalent treatment effects on non U.S. vendors on Silkroad2. This contradicts to the findings in existing literature.

So far, we use drug level aggregated number of reviews or vendors as our empirical analysis unit to see the arrest's overall impact on the platform. On the other hand, we can also implement the DID model on vendor level review data to explore how the arrest news would influence vendor level review information. The results are summarized in Table 4 below. The results are quite consistent with our main results.

Synthetic control method

One concern on the DID analysis is that vendors on Agora or Evolution may not be a comparable group for the vendors on Silkroad2. Therefore, we implement the synthetical control method to construct a comparable control unit by weighting all available control vendors' information

(Abadie et al. 2010). As our data set has multiple treatment units and an unbalanced panel data set, we employ a generalized synthetic control method (Xu 2017) with both time and drug level fixed effects. Please find the corresponding estimated treatment effects in Columns (4)-(6) of Tables 2-3 and Column (3)-(4) of Table 4. The results are quite consistent with those derived from the DID analysis. This provides us more confidence in our empirical results. In addition, we have visually presented how the treated group and the control group's total reviews, average review per vendor, and number of vendors with at least one review during the weekly period evolve over time in Figures 2-3. It is clear to us that before the arrest on Silkroad2, the treatment group and the constructed "control group" are quite similar to each other in terms of all the dependent variables. However, after the arrest was publicly available, the measures of the treatment group are significantly lower than those of the control group. Therefore, we can conclude that the observed treatment effects are not due to any pre-existing trend.

Parallel trend test

One fundamental assumption for the DID analysis is the parallel trend assumption (Abadie 2005; Angrist and Pischke 2008), which means in the absence of treatment, the difference between the control and treatment groups is constant over time. Violation of this assumption can lead to biased estimates. This assumption is generally not testable because we cannot observe the counterfactual post-treatment outcome of the treatment group. One common approach to this is to include the leads and lags of the treatment effect (Autor 2003). If the parallel trend assumption is valid, the interaction with the leads should be insignificant. We follow this procedure and add interactions between the treatment dummy and the leads and lags of the after dummy. Please find the estimated coefficients of those interactions displayed on Figure 4. Generally, we can see that the majority of

the coefficients before the arrests happened are not statistically different from zero, while those after the arrests are negative and many of them are significant.

Underlying reasons for the reduced sales

So far, we have found consistent evidence that sales on the focal website will significantly decrease when there is major arrest news disclosure. A natural follow-up question is what the underlying reason of the decreased sales is. There are two potential explanations of the observed permanent reduction in sales on Silkroad2: 1. Vendors and buyers have migrated to other websites due to the security and privacy concerns on Silkroad2; 2. Participants who are more risk-averse left the market. To have a better understanding of underlying mechanism, we have done additional analysis on vendors' platform choice behaviors.

Effects on existing and leaving vendors

First, we evaluate how the number of vendors on Silkroad2 changes before and after the arrest news. We define the time a vendor leaves a platform based on the last review it receives on the focal website. If the last review of a vendor was posted within one month of the platform's closure time, then we regard it to be on the platform until the end. Based on this definition, we count the number of U.S. and non-U.S. vendors existed on each website each week and use the logarithm transformed value as the dependent variable. The results are reported in Table 5, where columns (1)-(2) are based on the linear model and columns (3)-(4) are based on the negative binomial model. It is clear to us that the Silkroad2 has received a negative shock from the arrest news as there were less vendors selling drugs both from the U.S. and other countries after the arrest news disclosed. The visual representations of the parallel trend tests are displayed in Figure 5. It shows that the vendors on Silkroad2 dropped significantly right after when the arrest news was published and stayed at a relatively constant level after that.

Vendors' migrating behaviors

So far we have found that some vendors would stop selling drugs on Silkroad2 when there was focal news arrest disclosed. These vendors might choose to leave the darknet market or they might migrate to other websites. Therefore, we explored whether the number of new vendors settling on Agora and Evolution increases significantly after Silkroad2' arrest. Using the number of new vendors at time $t-1$ as the reference group, we drew weekly number of new vendors in Figure 6. Overall, we did not observe any significant increase in number of new vendors on the two control websites, though it seems that the number of new vendors decrease within one month after the arrest news disclosure. More importantly, because of the reputation concern, some vendors may use the same or quite similar user name on a different darknet website. Therefore, among all new vendors we manually counted the number of which have shown up on Silkroad2. Repeatedly, we did not observe a larger number of Silkroad2 vendors moving to Agora or Evolution. Last but not least, some vendors might use the same name on multiple DNMs because of the reputation concern. So we also checked the number of new vendors, which share the same names with Silkroad2 vendors, appearing on Agora and evolution before and after the arrest news reported. The visual representations are shown in Figure 7. Repeatedly, we did not find new similar name vendors arrived at the control websites. To conclude, the reviews' reduction cannot be explained by the vendors' migration behaviors.

Heterogeneous treatment effect

When the arrest news publicized, there will be more security and privacy concerns for darknet website's participants, especially for smaller vendors. Therefore, we divide all treated and control vendors into two groups based on their pre-treatment number of reviews. If a vendor's pre-treatment total reviews are lower than the median value of all vendors, then we regard it as a small

vendor. Then we counted number of existing small and large vendors for each drug type on every website. We reported the results using data only from small vendors, only from large vendors, and from all vendors separately. Compared with the results using data from large vendors, we have observed a larger reduction in the size of small vendors available, as we have expected.

Robustness checks

Placebo test

To provide stronger evidences that the estimated significant treatment effects are not due to randomness, we will do a placebo test by randomly selecting part of the Silkroad2, Agora, and Evolution vendors into the treatment group. In the placebo test, we randomly select 50% vendors as treated ones and replicate the DID analysis above for 10000 times. Please find the summary of our simulation results in Figure 8 and Table 6. It is clear to us the average of the estimated hypothetical “treatment effects” are not statistically significant from zero. Therefore, the observed negative treatment effect are not driven by randomness.

Matched samples

Another potential caveat of our main analysis is vendors in the control group may be different from those in the treated group, leading to bias in the estimators. To alleviate this potential issue, we will try to match vendors in the control group and treatment group based on their characteristics before the arrest disclosure on June 2, 2014. Specifically, we are considering the following covariates in the matching process using coarsened exact matching (CEM):⁵ total number of reviews, total sales, total number of unique products, total number of drug types, days since the first review. After we have successfully matched the treated vendors with control vendors, we will

⁵ We adopt the k2k matching option for the CEM matching.

apply t-tests and KL tests to verify the vendors in the control and treated groups are equivalent in all matching covariates, as reported in Table 7. Using these matched vendors, we are able to replicate the main analysis based on aggregated measures and vendor-level measures. The results in Tables 8 and 9 show that our main results still hold using matched samples.

Negative Binomial Model

As our three dependent variables are all count variables with over dispersed distribution, we add a robustness check here using negative binomial model using raw data as the dependent variable. Please find the estimated results in Table 10. The results show that the treatment effects are relatively weaker for non-U.S. vendors on Silkroad2 compared with U.S. vendors. Overall, the results are quietly consistent with our main results.

CONCLUSION

In this study, we assess and quantify the impact of legal enforcement on darknet transactions using a unique dataset that combined darknet transactions with arrest information. In particular, we conduct a series of empirical analyses to understand the potential impacts of police enforcement activities on darknet transactions. Our findings show that arrests conducted in a localized geographical reduced the transaction activities of both vendors residing in the same geographical location and those living in different countries. As news of arrests spread relatively fast on the internet channel, vendors that are participating on the platform react to it quickly by either exiting the market or reducing their transaction volume.

To our best knowledge, this study represents the first empirical effort to assess the impacts of police enforcement on darknet transactions. The results provide important insights to the IS community, policy makers with respect to cybercrime issues. Given that market participants are highly responsive to enforcement activity, it might also suggest that vendors that choose to transact

on darknet markets have a risk adverse nature. The police hence can utilize this nature to design effective enforcement strategy to combat darknet crime. But at the same time, darkweb provides good protection for users' privacy and identifies, it's still challenging for the police to generate a thorough deterrence through the whole darknet system. Thus, enforcement agencies would need to consider novel techniques of curbing illicit transactions on darknet.

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Figure 1 Comparison of vendor shares

Figure 2 Treatment effects on U.S. vendors on Silkroad2 using the Synthetic Control Method

Figure 3 Treatment effects on non U.S. vendors on Silkroad2 using the Synthetic Control Method

(a) Total review

(b) Avg review per vendor

(c) Number of vendors

(d) Review of each vendor

Figure 4 Parallel trend test of treatment effects on Silkroad2

Figure 5 Parallel trend tests with number of existing vendors as the DV

Figure 6 Number of new vendors and total reviews on Agora and Evolution over time

Figure 7 Number of new vendors on Agora and Evolution sharing the same name with Silkroad2 vendors

Figure 8 Estimated treatment effects based on the 10000 simulations

Table 1 Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
U.S. vendors on Silkroad2					
Total review	338	415.216	468.616	2	3850
Avg review	338	12.990	13.974	0.667	141.429
Unique vendor	338	35.683	28.336	2	109
After	338	.731	.444	0	1
Week	338	81.962	8.214	68	95
Drug type	338	7	3.747	1	13
Non U.S. vendors on Silkroad2					
Total review	338	1463.586	1417.861	46	11117
Avg review	338	15.267	7.619	2.190	48.335
Unique vendor	338	88.601	51.676	20	230
After	338	.731	.444	0	1
Week	338	81.962	8.214	68	95
Drug type	338	7	3.747	1	13
Non U.S. vendors on Agora and Evolution (treat=0)					
Total review	598	247.326	314.023	0	2205
Avg review	598	4.472	3.025	0	20.065
Unique vendor	598	46.828	37.602	1	191
After	598	.731	.444	0	1
Week	598	81.962	8.209	68	95
Drug type	598	7.130	3.874	1	14

Table 2 The impacts of arrest news disclosures on U.S. vendors' transactions on Silkroad2

VARIABLES	Linear			Synthetic control		
	Log(Total review)	Log(Avg review)	Log(Unique vendor)	Total review	Avg review	Unique vendor
Treat _i *After _t	-1.194*** (0.248)	-0.416** (0.179)	-0.690*** (0.0824)	-1.472*** (0.132)	-0.599** (0.0909)	-0.691*** (0.00295)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes	Yes	Yes
Website FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.806*** (0.129)	2.483*** (0.0988)	3.504*** (0.0291)			
Observations	936	936	936			
R-squared	0.373	0.295	0.567			
Number of website and drug	36	36	36	36	36	36

Table 3 The impacts of arrest news disclosures on non U.S. vendors' transactions on Silkroad2

VARIABLES	Linear			Synthetic control		
	Log(Total review)	Log(Avg review)	Log(Unique vendor)	Total review	Avg review	Unique vendor
Treat _i *After _t	-0.946*** (0.234)	-0.277* (0.151)	-0.620*** (0.0803)	-0.965*** (0.105)	-0.319*** (0.0813)	-0.610*** (0.0126)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes	Yes	Yes
Website FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	6.270*** (0.126)	2.574*** (0.0920)	3.858*** (0.0288)			
Observations	936	936	936			
R-squared	0.371	0.317	0.589			
Number of website and drug	36	36	36	36	36	36

Table 4 The impacts of arrest news on vendor level review data

	Linear		Synthetic control	
VARIABLES	U.S. vendors	Non U.S. vendors	U.S. vendors	Non U.S. vendors
Treati*Aftert	-0.871***	-0.748***	-0.945***	-0.788***
	(0.0940)	(0.0750)	(0.0484)	(0.0350)
Week FE	Yes	Yes	Yes	Yes
Vendor FE	Yes	Yes	Yes	Yes
Constant	2.302***	2.618***		
	(0.0361)	(0.0324)		
Observations	29,184	35,761		
R-squared	0.111	0.129		
Number of vendors	2,151	2,539	2069	2351

Table 5 The impacts of arrest news disclosures on the number of existing vendors

	Linear		Nbreg	
VARIABLES	US vendors	Non-US vendors	US vendors	Non-US vendors
Treati*Aftert	-0.927***	-0.621***	-1.107***	-0.732***
	(0.237)	(0.164)	(0.206)	(0.117)
Weekly FE	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes
Constant	2.023***	2.465***	1.960***	2.064***
	(0.112)	(0.104)	(0.0482)	(0.0368)
Observations	793	878	793	878
R-squared	0.306	0.362		
Number of drug types	36	36	36	36

Table 6 The heterogeneous treatment effects on large and small vendors

	US vendors			Non-US vendors		
VARIABLES	Small vendors	Large vendors	All	Small vendors	Large vendors	All
Treat _i *After _t	-1.044***	-0.225***	-0.227***	-1.000***	-0.154***	-0.157***
	(0.117)	(0.0625)	(0.0616)	(0.122)	(0.0553)	(0.0544)
Treat _i *After _t *Small			-0.796***			-0.839***
			(0.120)			(0.123)
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	2.406***	2.975***	3.143***	2.815***	3.315***	3.492***
	(0.0496)	(0.0360)	(0.0541)	(0.0502)	(0.0343)	(0.0509)
Observations	868	926	1,794	897	926	1,823
R-squared	0.615	0.397	0.716	0.628	0.351	0.725
Number of drug types	34	36	36	36	36	36

Table 7 Robustness check with randomly selected treated vendors

	Log(Total review)	Log(Avg review)	Log(vendors with review)
Number of iterations	10000	10000	10000
Mean	0.000911	0.000207	0.000461
S.D.	0.0953	0.0656	0.0510
H0: Mean = 0	0.956	0.316	0.904
P value	0.339	0.752	0.366

Table 8 Comparisons between treated and control vendors using T-test and Kolmogorov–Smirnov test

VARIABLES	Difference	t-test	Kolmogorov–Smirnov test
Log_review	0.0264	0.909	0.996
Log_price	-0.0992	0.687	0.409
Unique products	0.187	0.790	0.738
Drug types	0	1.000	1.000
Tenure	-0.619	0.927	0.996
# weeks with reviews	-1.009	0.892	0.844
# pairs	107	107	107

Table 9 Robustness check using CEM matched vendors

	linear			nbreg		
VARIABLES	Log(Total review)	Log(Avg review)	Log(vendors with review)	Total review	Avg review	vendors with review
$Treat_i$ * $After_t$	-1.027***	-0.627*	-0.690***	-1.007**	-0.649	-0.725***
	(0.361)	(0.321)	(0.119)	(0.460)	(0.417)	(0.115)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes	Yes	Yes
Website FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.696***	2.023***	1.738***	3.588***	1.633***	1.288***
	(0.161)	(0.133)	(0.0602)	(0.190)	(0.165)	(0.0644)
Observations	895	895	895	895	895	895
R-squared	0.239	0.157	0.397			
Number of drugs	36	36	36	36	36	36

Table 10 Robustness check using CEM matched vendors using vendor level data

	linear		nbreg	logit
VARIABLES	Log(vendor review)	If has sales	vendor review	If has sales
$Treat_i * After_t$	-0.688***	-0.292***	-0.730***	-2.550***
	(0.238)	(0.0843)	(0.273)	(0.656)
Week FE	Yes	Yes	Yes	Yes
Vendor FE	Yes	Yes	Yes	Yes
Constant	2.334***	0.912***	-0.212	0.489***
	(0.0921)	(0.0294)	(0.195)	(0.159)
Observations	3,157	3,157	3,145	3,145
R-squared	0.141	0.084		
Number of vendors	197	197	191	191

Table 11 The impacts of arrest news disclosures on Silkroad2 using Negative Binomial model

VARIABLES	U.S. vendors			Non U.S. vendors		
	Total review	Avg review	Unique vendor	Total review	Avg review	Unique vendor
Treat _i *After _t	-1.049*** (0.222)	-0.354* (0.202)	-0.701*** (0.0673)	-0.723*** (0.204)	-0.150 (0.182)	-0.597*** (0.0633)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Drug type FE	Yes	Yes	Yes	Yes	Yes	Yes
Website FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.991*** (0.186)	2.264*** (0.178)	3.705*** (0.157)	6.004*** (0.171)	2.302*** (0.150)	3.631*** (0.111)
Observations	936	936	936	936	936	936
Number of website and drug	36	36	36	36	36	36