Research report

Estimating the size of the fentanyl market in British Columbia

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Executive summary

Background

This report presents estimates of the size of the market for fentanyl in British Columbia (BC) by calculating what people who use drugs (PWUDs) spend on fentanyl or fentanyl-contaminated opioids or stimulants. Two emerging trends speak to the potential size of the fentanyl market: trends in overdose from fentanyl exposure and contamination of the local drug supply. Among PWUDs, high exposure to fentanyl through opioid use has been responsible for the high rates of fatal and non-fatal overdose throughout the current opioid crisis. And testing of heroin, other opioids, and stimulants bought on the street for contaminants confirms high levels of fentanyl exposure. Fentanyl therefore seems to represent one important source of revenue for money laundering in BC.

To calculate what PWUDs spend on fentanyl or fentanyl-contaminated substances, we first estimate fentanyl prevalence of use and/or exposure for the City of Vancouver. To so, we use interview schedules from three concurrent surveys of PWUDs living within Downtown Vancouver to estimate total survey coverage, or the eligible pool of participants who weren't recruited or otherwise did not participate. After we improve the city-level estimates by correcting for probable gaps in survey coverage, we then make similar inferences for the entire province. We then use street prices of opioids provided by the Vancouver Police Department and reported spending patterns on heroin use (Midgette et al., 2019) to project total expenditures on fentanyl and fentanyl-contaminants.

To be clear, our estimates represent plausible, yet imperfect calculations of total expenditures on fentanyl, "down" (i.e., heroin contaminated with fentanyl), and stimulants (i.e., crack/cocaine, methamphetamine, etc.) laced with fentanyl. Along the way, we make various assumptions that may be substituted with others. In the end, we settled on estimates of total expenditures which ought to reflect significant shares of the revenues from fentanyl, but that we nonetheless deem to be conservative.

Data

To calculate fentanyl prevalence of use or exposure, we combine surveys on substance use from three groups of people who use drugs in downtown Vancouver for two years (c. 2017–2018). Each of the three groups represent one the three cohort studies run by the British Columbia Centre on Substance Use (BCCSU): the Vancouver Injection Drug Users Study (VIDUS) consists of ~1,500 people who inject drugs (PWID); the AIDS Care Cohort to Evaluate Exposure to Survival Services (ACCESS) consists of ~1,000 HIV-positive people who use drugs; and the At-Risk Youth Study (ARYS) consists of ~1,000 street youth. For more complete city-wide survey coverage, we pool the survey records from the three cohort studies. Each study recruits eligible people through various points-of-contact between service providers and PWUDs (i.e., harm-reduction services, health and social services, etc.). A standardized questionnaire collects information on participant demographics, lifetime and past six-month substance use, housing, participation in treatment for

opioid use, contact with the criminal justice system, and their health and well-being. After recruitment and the initial in-take interview, follow-up interviews occur in six-month intervals.

For this report, we use cohort interviews from the two most recent years available to us (c. 2017–2018). Across the three cohort studies, we first group interviews occurring over the entire observation period into four survey periods—each lasting six months in length. For each year of the observation period, the first six-month survey period runs from January 1st through June 30th, while the second six-month survey period occurs from July 1st through December 31st.

As our sampling criteria, we include participants who self-reported using fentanyl or heroin and/or had screened positive for fentanyl exposure through urine testing. Because few cohort participants self-reported using fentanyl over the observation period ($n < 20\%$), screening for fentanyl exposure represented our most important sampling criterion. Although recognizing the limitations of screening for fentanyl use (e.g., the short detection window, not 100% reliable, etc.), we include participants who self-reported heroin use to increase our sample coverage of people exposed to fentanyl (because of fentanyl contaminants in heroin). Our final sample of cohort participants consists of 1,213 people who use or were exposed to fentanyl over the three cohort studies (N = 1,213). By sampling on both self-reported fentanyl/heroin use and screening for fentanyl exposure, we estimate people who never use or were otherwise exposed to fentanyl (PWUEF).

To supplement the survey records from BCCSU's three cohort studies, we use three other sources of data that help us calculate total expenditures on fentanyl: counts of fatal overdoses from fentanyl use/exposure reported by BC Coroners Service; BCCSU drug screening studies, and street prices for fentanyl provided by the Vancouver Police Department.

Methods

To estimate the prevalence of PWUEF, we use well-established methods for evaluating the completeness of census reporting and monitoring trends in the life cycles of wildlife species. Both methods use observed frequencies and patterns of "captures" and "recaptures" from our sample (i.e., interviewing patterns of cohort participants) to find gaps in survey coverage and calculate rates of survey recapture. For each cohort participant, their first interview following recruitment represents their first "capture". All subsequent interviews represent "recaptures". Although because of gaps in survey coverage (i.e., from recruiting, non-participation, etc.), our estimates of fentanyl prevalence of use/exposure reflects PWUEF eligible to participate in one of the three cohort studies.

We estimated two types of capture-recapture models: one requiring the eligible pool of cohort participants to remain 'stable' (i.e., no massive drops in participation resulting from trends in fatal overdose; no period of mass recruitment; etc.) throughout the observation period (c. 2017–2018); the other does not require the eligible pool of cohort participants to remain stable over the observation period (i.e., changes in recruitment and/or lower participation with time don't pose problems for estimation). Assumptions for both types of models come with trade-offs. Although the first model isn't realistic over long observation periods or changing local conditions (e.g.,

mounting fatalities from fentanyl exposure), this model nonetheless provides robust estimates for typical prevalence of use over the entire observation period. By comparison, the second model lets us evaluate the effects of trends occurring over time and project trends of overall prevalence for each specific survey period; however, compared to the first model, this second model isn't so robust to issues in sampling error. We found no major differences between the results from the two models. Because the first model provided one estimate of the typical or average prevalence of fentanyl use/exposure for the entire period, we used it for the inferences we make in the report.

To correct for unequal probabilities of survey recapture for cohort participants, our models control for observed characteristics and self-reported behavior of interviewed participants (i.e., factors predicting participation and retention over the observation period). Apart from controlling for variations in the characteristics and behaviour of individual participants, we further control for the effects of time and previous participation (i.e., trap effects) on the likelihood of future survey participation. Assigning temporal order to the interview schedules lets us factor in the effects of time on probabilities of survey recapture. And controlling for trap effects lets us offset the high observed rates of retention in survey participation. Both time and trap effects offset potential transience and high risk of fatal overdose within the recruitment pool of eligible survey participants.

Findings

Fentanyl prevalence of use/exposure

From overall frequencies and patterns of survey recapture of the 1,213 cohort participants, we estimate 2,561 PWUEF (95% confidence interval = 2,484 – 2,638) were eligible to participate in one the three cohort studies, on average, over the observation period (c. 2017–2018).

Because of the exclusion/inclusion criteria for each of the cohort studies, gaps in recruitment, and non-participation, we do not have total sample coverage of everyone using or exposed to fentanyl for city of Vancouver. First, PWUD, who don't use safe injection sites or other health services, might be under-recruited because harm-reduction services represent important mechanisms for recruitment. Also, people who do not inject drugs might be under-recruited for similar reasons—though more than 95% of our sample report injection drug use. Second, the study design itself limits total survey coverage in terms of target enrollments for potential recruits and participants. All three cohort studies have target enrollments limiting the number of participants they can recruit, which leads to conservative estimates. Third, recruitment mechanisms lead to over-sampling within Vancouver's DTES and under-sampling throughout Vancouver's other neighbourhoods. With the exception of the ARYS cohort surveying street youths in Vancouver's Downtown South neighbourhood, most participants report living in Vancouver's DTES—reflecting the concentration of substance use problems and possible bias in recruiting within this neighbourhood. And fourth, we exclude participants who have been exposed to fentanyl outside the detection window for screening (i.e., ≤ 96 hours). As most of our sample tested for exposure

to fentanyl rather than self-reporting its use, we under-report the most infrequent drug use occurring outside the detection window for screening.

To get closer to city-wide estimates of prevalence of fentanyl use/exposure, we correct for the over-sampling of DTES residents. We believe this source of sampling bias to spillover or effect multiple other sources of sampling bias (i.e., recruitment), so it's perhaps the most pertinent source of sampling error. We make corrections for over-sampling of DTES residents using methods established in previous work.

We make inferences to city-wide estimates for two possible scenarios. In the first scenario, the estimate from the cohort studies represents residents from Vancouver's DTES and other neighbourhoods. As the cohort studies over-represent DTES residents, we keep the proportion of DTES residents from both our sample and non-surveyed group (i.e., eligible, but didn't participate), from our prevalence of use estimates (i.e., 2,484 – 2,561 – 2,638). We further group DTES residents by their typical frequencies of self-reported use over the observation period. Next, we correct for gaps in survey coverage of non-residents by inflating prevalence of fentanyl use/exposure by the ratio of fatal overdoses reported to occur within Vancouver's DTES (i.e., Vancouver Centre North (VCN)) versus the city totals reported by the Local Health Areas (LHAs), combined (i.e., 45%). This yields an estimate of 3,754 – 3,987 PWUEF in the City of Vancouver. We believe this range to be conservative, because cohort studies do not provide full coverage of DTES residents exposed to fentanyl.

For the second scenario, we take our estimated range to represent DTES residents and/or Vancouver residents frequenting the DTES. Although one-third of participants report not residing within Vancouver's DTES, the strong focus of recruiting from DTES harm-reduction services (e.g., safe injection sites, needle exchanges, etc.) suggests non-residents frequent the DTES to score opioids or other substances, and be closer to local health and social services. Again, VCN reported 45% of fatal overdoses reported by Vancouver LHAs (c. 2017). Applying the 45% inflation factor, we estimate 5,520 – 5,862 PWUEF for the City of Vancouver—much higher than the range calculated from the first scenario.

After making corrections for the city-level, we then go on to make inferences of provincial-level fentanyl prevalence of use/exposure. After weighing our options, we found fatal overdoses to represent the most reliable metric of the size of Vancouver's fentanyl market relative to the rest of the province. Applying the regional fatal overdose multiplier (i.e., 0.25) to our city-level estimates, we calculate 15,014 – 15,948 PWUEF throughout the entire province for the first scenario (i.e., 3,754/0.25 – 3,987/0.25) and 22,080 – 23,448 for the second scenario (i.e., 5,520/0.25 – 5,862/0.25); therefore, we can say, with some confidence, ~15,000 – 23,000 PWUEF – minimum – were living in BC throughout the observation period (c. 2017–2018). We deem this range to be conservative, representing the floor estimate for fentanyl prevalence of use/exposure within the province.

As our estimates represent fentanyl use or exposure, specifically, rather than poly-drug use or exposure, generally, we expect our estimates to be lower than the cited city- and provincial-level estimates (i.e., Jacka et al., 2020; Janjua et al., 2018; McInnes et al., 2009), which lends face validity to our estimates.

Total expenditures on fentanyl use

After having estimated prevalence of fentanyl use and/or exposure for Vancouver and BC, two steps were involved for calculating total expenditures on fentanyl and fentanyl-contaminated opioids or stimulants. Beginning from our provincial-level prevalence estimates by frequencies of use for the first and second scenarios:

1. Assign expenditures (per day of use) by frequencies of use;
2. Multiply by number of days of use per month to calculate monthly expenditures; and
3. Multiply by twelve to calculate annual expenditures

As cohort surveys did not include questions pertaining to participants spending on opioid use, specifically, we take daily expenditures on heroin use from studies reporting similar frequencies of heroin use to BCCSU's cohort studies (Midgette et al., 2019). Based on reporting for frequencies of use, daily use equates to $23,747 spent on fentanyl and/or fentanyl-contaminated opioids or stimulants per year; frequent use costs $10,700 per year; and infrequent use costs $5,192 each year.

And by multiplying expenditures for daily, frequent, and infrequent use by the prevalence of fentanyl use/exposure estimates from our first and second scenarios yields retail expenditures ranging $200M – $300M for fentanyl and fentanyl-contaminated opioids and stimulants.

At this time, no prior estimates exist to help situate this range of retail expenditures. It is worth noting that 1) the starting point for these estimates – the prevalence of PWUEF – appears to err on the conservative side; 2) the estimate is relatively sensitive to the proportion of daily users included in the calculations: increasing the proportion of daily users from 34–50%, for example (with a similar decrease in infrequent users), would increase retail expenditures by more than 22%; 3) Not every dollar spent translates into profits for dealers and, by extension, into illicit revenue for money laundering. A supply-side look into the fentanyl market – in terms of estimating the revenues of street-level dealers and upper-level traffickers – would be necessary to estimate the proportion of profits laundered versus spent.

Conclusions

Fentanyl and fentanyl-adulterated substances have taken over 90% of the opioid market in BC—the hardest hit province in the opioid crisis (Centre on Substance Use, 2020; Pardo et al., 2019). This shift in BC's opioid market supply has undoubtedly changed drug-related revenue flows. Any discourse pertaining to potential revenue flows from fentanyl, however, will remain speculative without credible estimates of the size of the fentanyl market. Although revenues do not equal profits, by estimating potential revenues from fentanyl, heroin, and synthetic opioids in BC, our

report provides the potential contribution of this revenue source to money laundering within the province.

From our prevalence of use/exposure estimates (c. 2017–2018), we calculated total expenditures busing reported spending patterns for daily, frequent, and infrequent heroin use (Midgette et al., 2019). All total, we calculated retail expenditures ranging $200M – $300M.Although BCCSU's cohort studies were better suited to providing estimates for prevalence of fentanyl use/exposure than expenditures, we nonetheless provide plausible estimates of total expenditures ranging in the low hundreds of millions. And, by doing so, we provide some sense of the size of this potential revenue source for money laundering. After correcting for inflation, our estimates would represent 2–3% of the world's opium/heroin market shares (UNODC, 2010). Further study of trends in local seizures – both in terms of size and frequency – and prices will provide more clarity to the potential of this revenue source for money laundering within the province.

Introduction

In large parts of the United States and Canada, fentanyl contamination of opioids and stimulants has led to the current and unprecedented overdose crisis (Bardwell, Boyd, Arredondo, McNeil, & Kerr, 2019; Bardwell, Boyd, Tupper, & Kerr, 2019; Caulkins, Gould, Pardo, Reuter, & Stein, 2021; Pardo, 2019; Pardo et al., 2019; Tupper, McCrae, Garber, Lysyshyn, & Wood, 2018). Between 2013 and 2018, fatal overdoses involving opioid use increased tenfold in the United States, from 1.0 fatal overdose per 100,000 to 9.9 fatalities per 100,000 (Caulkins et al., 2021). By 2016, fatal overdoses from fentanyl exposure had increased to 8.4 per 100,000 in Canada (Pardo et al., 2019), and more Canadians died from fentanyl-contaminated opioid use than were killed in motor vehicle accidents (Belzak & Halverson, 2018). And by 2018, the rate of fatal overdose from opioid use reached 12 per 100,000 (Pardo et al., 2019).

High mortality from fentanyl exposure stems from its potency—reported to be nearly 25 times more potent than heroin (Pardo et al., 2019). Fentanyl is cheaper than heroin too, which means its emergence has been motivated by traffickers desire to cut costs and increase profits (Caulkins et al., 2021). Fentanyl's high potency means traffickers can make considerable profits by smuggling very small quantities (Caulkins et al., 2021). And its production chain is shorter compared to heroin, which reduces overall manufacturing costs. Fentanyl is manufactured from chemical precursors, so traffickers bypass the first part of the heroin distribution chain (i.e., farmers cultivating opium from poppy fields). Although bought and sold itself, fentanyl contaminates large quantities of heroin, opioids, and stimulants sold on the street (Bardwell, Boyd, Arredondo, et al., 2019).

In Canada's overdose crisis, British Columbia (BC) has been the hardest-hit province. At the current rate of 30.6 per 100,000, BC reports the highest number of fatal overdoses from opioid use of Canada's ten provinces (Pardo et al., 2019).[1] In 2012, fatal overdose from fentanyl exposure represented 5% of BC's fatal overdoses (Office of the Provincial Health Officer BC, 2019). At this time, reports emerged fentanyl was being sold as counterfeit oxycodone to people seeking diverted narcotics in the wake of new restrictions on medical prescriptions (Pardo et al., 2019; Vancouver Police Department, 2017). By 2018, fatal overdose from fentanyl exposure represented 85% of the province's fatal overdoses.

Each region of BC has been hit by the opioid crisis. Annual reporting of fatal overdoses from fentanyl exposure by region reveals the emergence of fentanyl market over time (see Figure 1). By 2018, fatal overdoses from fentanyl exposure peaks for every region expect the Interior. Although each Health Authority reports sharp increases of fatal overdoses (c. 2012–2018), Vancouver Coastal Health Authority and the Fraser Health Authority report more cases relative to other Health Authorities.

[1] Apparent overdose deaths reported from BC represented 60% of fatal overdose deaths (c. 2017) in Canada (Statistics Canada, 2018).

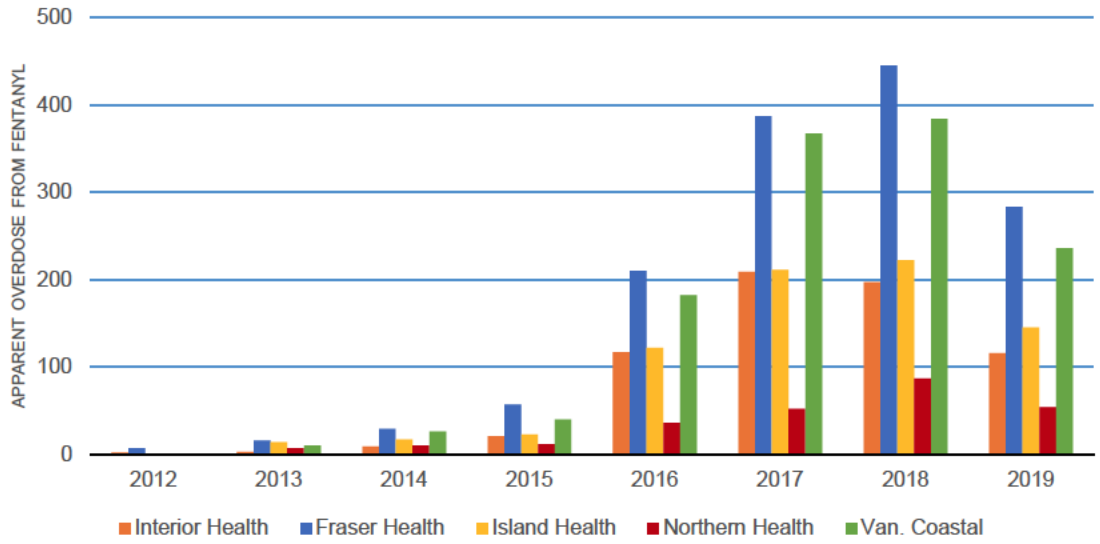


Figure 1 Fatal overdoses (f. fentanyl) by Health Authority, 2012-2019
Source: BC Coroners Service, reported fentanyl-related toxicity deaths (see https://www2.gov.bc.ca/assets/gov/birth-adoption-death-marriage-and-divorce/deaths/coroners-service/statistical/fentanyl-detected-overdose.pdf)

Between 2012 and 2020, Fraser Health and Vancouver Coastal Health Authorities reported ~60% of fatal overdoses from fentanyl exposure in the province. And, between 2015 and 2020, incidents reported for the city of Vancouver Coastal Health Authority, specifically, reported 25% of the provincial total (BC Coroners Service, 2020a).[2] For comparison, we plot trends in fatal overdose reported by Vancouver Coastal Health Authority and BC's other Health Authorities (see Figure 2). Both trends reflect similarities in reporting throughout the province. For each of the two peak years of reporting (c. 2017–2018), Vancouver Coastal Health Authority reported 350–400 fatal overdoses, while BC's other health authorities reported 850–950 fatal overdoses, combined. Following the peak (c. 2019), incidents fell nearly 40%, but have yet to return to the levels before the public health declaration of the opioid crisis (c. April 14th, 2016); however, fatal overdoses from fentanyl exposure occurring over the first nine months of this year (963) have already surpassed last year's totals (833) (BC Coroners Service, 2020a).

[2] According to the reporting from Vancouver's Local Health Areas, most incidents of fatal overdose occur within Vancouver's DTES—reflecting the high concentration of opioid use by local residents.

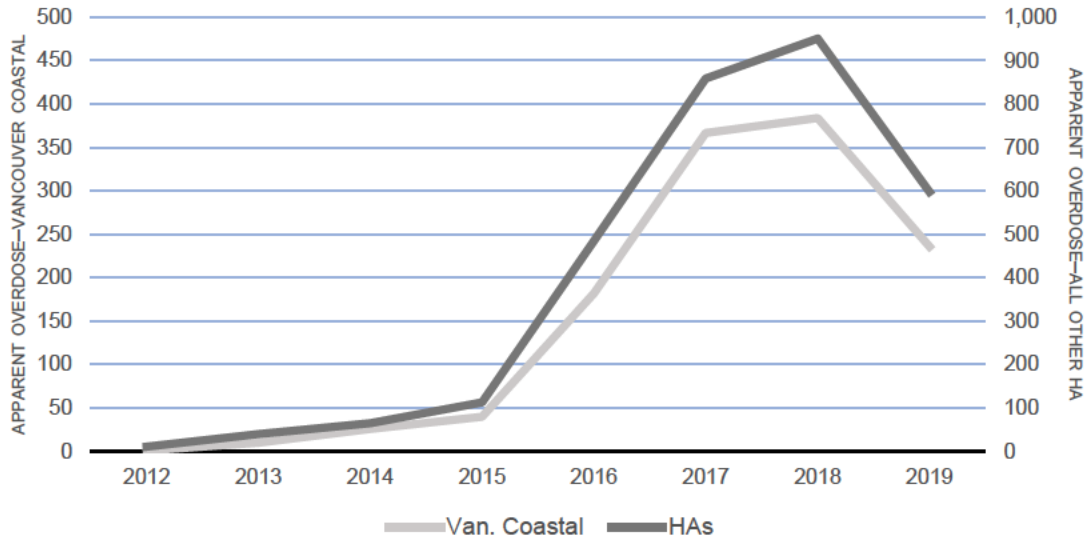
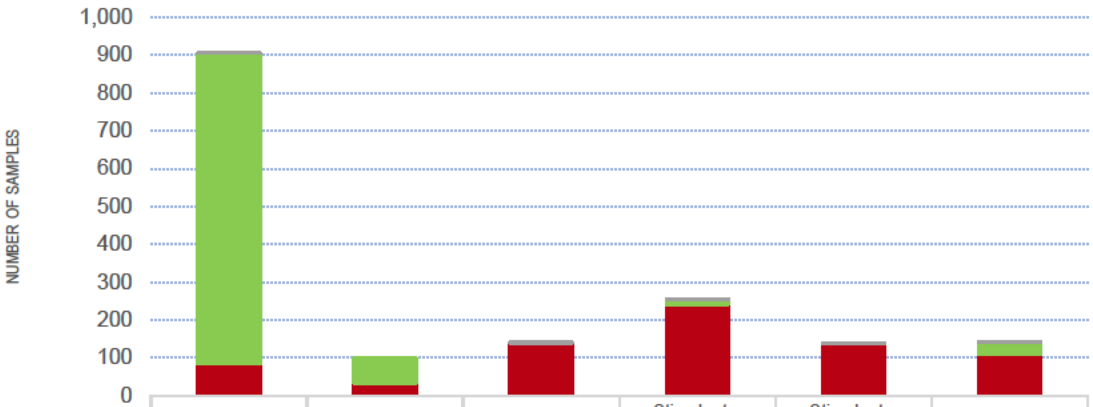


Figure 2 Apparent fatal overdose from fentanyl exposure reported by Vancouver Coastal versus BC Health Authorities 2012–2019

Source: BC Coroner's data on fentanyl-related toxicity deaths https://www2.gov.bc.ca/assets/gov/birth-adoption-death-marriage-and-divorce/deaths/coroners-service/statistical/fentanyl-detected-overdose.pdf

At the beginning of the opioid crisis (c. 2016), high levels of exposure to fentanyl was tied to high levels of contaminated heroin or other opioid use (Amlani et al., 2015; Hayashi et al., 2017; Karamouzian et al., 2020; Tupper et al., 2018). As fentanyl has come to saturate the local heroin and opioid supply, "down" – heroin laced with fentanyl – has been used on the streets in reference to both fentanyl and heroin (Pardo et al., 2019). Fentanyl – either from intended or unintended use – now represents the most common opioid consumed by PWUD throughout Vancouver and BC, generally. Testing of local opioid samples for fentanyl contaminants reflects high fentanyl exposure, with 90% of samples tested containing fentanyl (see Figure 3).[3] According to the Canadian Centre on Substance Use and Addiction (2020), opioid samples in Vancouver have higher rates of fentanyl contamination than the national average (69%).

[3] A total of 1,714 local samples of opioids and stimulants were tested for fentanyl contamination between November 2017 and April 2018 (see https://drugcheckingbc.ca/results/).



	Opioid – Heroin	Opioid – Other	Psychedelic	Stimulant – Amphetamines	Stimulant – Cocaine	Unknown
N/A (declined)	2	0	2	1	1	3
Positive	822	66	0	15	3	30
Negative	83	33	139	240	136	110

Figure 3 Drug check test results for fentanyl contamination, reported by Tupper et al. (2018)
Note: Opioid-Other includes carfentanil, fentanyl, hydromorphone, morphine, methadone, oxycodone, oxycodone/paracetamol, U-44770. Pyschedelic includes DiPT, DMT, GHB, ketamine, LSD, MDA, MDMA, MDA/MDMA, mushroom extract, 2C-B, 4-MMC, 5-MeO-DiPT. Stimulant-Amphetamines includes crystal meth, amphetamine, speed.

An implication of the emergence of fentanyl for the Cullen Commission on money laundering is that BC drug dealers and traffickers recently started to reap their profits from fentanyl use or cutting opioids and stimulants with fentanyl. A shift from heroin to fentanyl consumption suggests a parallel shift in the revenue flows of drug suppliers in BC. After 2015, increasing shares of potential revenues for money laundering in BC became tied to fentanyl production and trafficking.

Although the emergence of fentanyl has caused serious public health problems and changed the revenue flows of traffickers, no estimates of the size of the fentanyl market in BC, or Vancouver, exist. All previous city- or province-level estimates cover persons who inject drugs (PWID), generally, not people who use or were otherwise exposed to fentanyl (PWUEF), specifically (e.g. Jacka et al., 2020; Janjua et al., 2018). And the most recent estimates of PWIDs we could find were already four years old (c. 2016, see Jacka et al., 2020), two years before fentanyl overdoses peaked (c. 2018).

As we do not have records on revenue flows from drug trafficking, we cannot speak to the contribution or potential of the fentanyl market for money laundering in BC; however, estimating volumes of money laundering first requires estimating the size of potential revenues sources (e.g., opioid trafficking). Below we outline our methods for 1) estimating the number of PWUEF in BC; and 2) what they spend on fentanyl or other drugs contaminated with fentanyl. To project total expenditures, we combine survey records on fentanyl consumption and exposure patterns from three cohort studies of PWUDs in Downtown Vancouver, reports of fatal overdose from BC Coroners Service, test results for fentanyl contamination of opioid samples (Tupper et al., 2018),

street prices of opioids provided by the Vancouver Police Department, and reported spending patterns on heroin use (Midgette et al., 2019).

To estimate the prevalence of PWUEF, we use well-established methods for evaluating the completeness of census reporting and monitoring trends in the life cycles of wildlife species. Both methods use observed frequencies and patterns of "captures" and "recaptures" from our sample (i.e., interviewing patterns of cohort participants) to find gaps in survey coverage and calculate rates of survey recapture. For each cohort participant, their first interview following recruitment represents their first "capture". All subsequent interviews represent "recaptures". By sampling on both self-reported fentanyl or heroin use (i.e., from participant interviews) and screening for fentanyl exposure (i.e., urine testing), we estimate the number of people who use or were exposed to fentanyl, eligible to participate in one of the three cohort studies.

We estimated two types of capture-recapture models: one requiring the eligible pool of cohort participants to remain 'stable' (i.e., no massive drops in participation resulting from trends in fatal overdose; no period of mass recruitment; etc.) throughout the observation period (c. 2017–2018); the other does not require the eligible pool of cohort participants to remain stable over the observation period (i.e., changes in recruitment and/or lower participation with time don't pose problems for estimation). Assumptions for both types of models come with trade-offs. Although the first model isn't realistic over long observation periods or changing local conditions (e.g., mounting fatalities from fentanyl exposure), this model nonetheless provides robust estimates for typical prevalence of use over the entire observation period. By comparison, the second model lets us evaluate the effects of trends occurring over time and project trends of overall prevalence for each specific survey period; however, compared to the first model, this second model isn't so robust to issues in sampling error.

To correct for unequal probabilities of survey recapture for cohort participants, our models control for observed characteristics and self-reported behavior of interviewed participants (i.e., factors predicting participation and retention over the observation period). Apart from controlling for variations in the characteristics and behaviour of individual participants, we further control for the effects of time and previous participation (i.e., trap effects) on the likelihood of future survey participation. Assigning temporal order to the interview schedules lets us factor in the effects of time on probabilities of survey recapture. And controlling for trap effects lets us offset the high observed rates of retention in survey participation. Both time and trap effects offset potential transience and high risk of fatal overdose within the recruitment pool of eligible survey participants.

From the perspective of total survey coverage (i.e., total representative sampling), our methods correct for certain gaps in sampling, but not others. Because of gaps in survey coverage (i.e., from recruiting, non-participation, etc.), we make further corrections to improve city-wide coverage using established multiplier methods. As cohort studies over-recruit DTES residents, we correct for over-sampling within the DTES. Assuming fatal overdoses represent out-of-sample PWUEF

who would otherwise be eligible to participate, we use reporting on fatal overdoses to correct for this type of sampling bias. For the purposes of official reporting, DTES is part of Vancouver Centre North (VCN) Local Health Area (LHA); therefore, we take VCN's proportion of fatal overdoses for Vancouver LHAs (i.e., 45%) to be our multiplier. Assumptions for the multiplier method hold if there is equal risk of fatal overdoses between LHAs. Although we can't test for possible violations without reliable reporting on participant frequencies of use and overdose incidents, we believe the results from the multiplier correction to be closer to the overall fentanyl prevalence of use/exposure for the City of Vancouver than our estimate without further correction.[4] We nonetheless still believe this range to be conservative, because of other probable gaps in survey coverage.

Fentanyl use by PWUD

The city of Vancouver has been particularly hit by the overdose crisis. From 2015 to 2016, the combined number of overdose calls to the Vancouver Fire and Rescue Services (VFRS) and the BC Ambulance Service (BCAS) rose from 14,863 to 23,987 (Vancouver Police Department, 2017), an increase of 61%. The number of fatal opioid overdoses reported in the Vancouver – City North Local Health Area (LHA), which covers the DTES, was much higher compared to the rest of the city. In 2017, opioid overdose deaths in Vancouver – City North represented 45% of fatal overdoses in the Vancouver Health Service Delivery Area.

Both in the United States and Canada, fentanyl was first introduced covertly, as an adulterant or sold as other substances, such as oxycodone (Caulkins et al., 2021; Midgette, Davenport, Caulkins, & Kilmer, 2019; Pardo, 2019; Tupper et al., 2018). PWUD have progressively become aware of this practice. Amlani et al. (2012) found that 73% of the PWUD in Vancouver who tested positive for fentanyl use did not self-report using it in the previous three days. More recent studies indicate high proportions of PWUDs suspect they have been exposed to fentanyl. McCrae, Hayashi, et al. (2020) report 76.3% of their sample suspect exposure to fentanyl, while Beaulieu et al. (2020) report 51.6% thought they have been exposed to it.

Although fentanyl comes with high risk of overdose compared to other drugs, some PWUD prefer fentanyl's intense effects (Bardwell et al., 2019; Kenney, Anderson, Conti, Bailey, & Stein, 2018; Miller, Stogner, Miller, & Blough, 2018; Morales et al., 2019).[5] Among PWUDs, fentanyl use is most popular for people in their thirties and forties, (non-Hispanic) whites, people who have overdosed, and non-prescription opioid users (e.g., heroin, etc.) (Morales et al., 2019; Statistics Canada, 2018).

[4] For example, supervised injection sites and emergency medical services brought on by the opioid crisis ought to lower risks of fatal overdose within VCN. At the same time, other factors may increase risks of fatal overdose for DTES residents, including higher frequencies of use (i.e., exposure to risk).

[5] In some cases, PWUD combine fentanyl with heroin for the intense rush produced by fentanyl and experience the longer effect of heroin (Ciccarone, Ondocsin, & Mars, 2017), while others use fentanyl to overcome opioid tolerance caused by opioid agonist therapy (Karamouzian et al., 2020) or excessive opioid use (Mars, Rosenblum, & Ciccarone, 2019). To minimize their risk of overdose, PWUD often inject smaller doses in test shots and buy from one regular dealer (Bardwell et al., 2019; McKnight & Des Jarlais, 2018; Mars et al., 2018).

Prior studies estimating the size of opioid markets

A review of studies estimating PWUD in BC

For this report, we calculate total provincial-level expenditures by tallying up what PWUD spend on fentanyl and fentanyl-contaminated substances. To do so, we first estimate the number of PWUEF living within Vancouver, and, from there, we then make projections for the number of PWUEF throughout the province. At that point, we can then tally their spending by their self-reported frequencies of use for fentanyl and "down".

As with studies estimating spending on heroin use (i.e., Midgette et al., 2019), our estimates of total expenditures on fentanyl depend on first estimating how many PWUD and PWID use fentanyl or other drugs laced with fentanyl. To estimate the city-level population of PWUEF, we use well-established methods for evaluating the completeness of census reporting and monitoring trends in the life cycles of wildlife species. A classic example of capture-recapture field experiments involves trapping members of some wildlife species of interest, who then get marked or tagged, before being released back into the wild. This process is repeated over regular time intervals. The ratio of the species tagged once versus the species tagged twice, three times, and so on, provides the basis for calculating the capture rate for the local species susceptible to capture (which excludes members of the species not susceptible to capture).

A rich public health literature exists of modified capture-recapture experiments for the purpose estimating local, regional, and national prevalence of injection and non-injection drug use, to inform on-the-ground policy (e.g., resources required for effective harm-reduction services) and, more recent, the probable effects of over-prescribing opioids on prevalence of use, treatment for opioid use, and overdose. In place of field experiments, public health researchers often use longitudinal surveys and/or link multiple healthcare and/or treatment records together to mimic the capture-recapture process (i.e., the ratio of PWUD surveyed once versus twice, etc.). To keep our review short, we focus on recent studies within Canada, and BC, more specifically, that we can use to make plausible comparisons to our own estimates later in this report.

From cross-sectional survey records collected between 2003 and 2005, Xu et al. (2014) use multiple capture-recapture methods to estimate PWID in Greater Victoria, BC. Across three types of models, prevalence estimates were similar (~3,330; 95% confidence interval = ~2,245 – 5,100), projecting ~0.90% of Greater Victoria's population to have intravenous drug use over this timeframe. As a point of comparison, Leclerc et al. (2014) estimate 3,910 (95% confidence interval = 3,180 – 4,900) PWID in Montreal (c. July 1, 2009 – June 30, 2010). Based on census of population estimates, this prevalence of use estimate represents 0.28% (95% confidence interval = 0.23 – 0.35) of Montreal's population 14–65 years old—showing the high overall concentration of injection drug use in Victoria, BC, relative to Montreal.

A more recent effort to estimate BC's population of PWID did not use capture-recapture methods per se, but links together multiple record systems – medical visits, hospitalizations, prescription

records, and treatment for opioid use – to calculate the ratios of people with probable intravenous drug use over more and less restrictive sampling criteria. Between 2013 and 2015, Janjua et al. (2018) estimate 41,358 (95% confidence interval = 40,944 – 41,771) PWID throughout BC, by testing variations in their sampling criteria—representing 1.2% of BC's population 11–65 years old.[6] To correct for probable gaps in sampling (i.e., PWID with no medical visits, hospitalizations, or receiving treatment for opioid use), Janjua et al. (2018) inflate their estimates by 10%. According to their model, Fraser (15,016), Vancouver Coastal (10,969), and Vancouver Island (6,634) Health Authorities report ~36%, ~27%, and ~16% of the provincial total (i.e., 41,358), respectively.

Jacka et al. (2020) used established multiplier methods to estimate provincial-level prevalence of injection drug use throughout Canada. A multiplier represents the inflation factor used to correct for gaps in sampling occurring from non-contact between service providers and PWUD. For example, if half of survey respondents (e.g., 1,500 PWID), report receiving treatment for opioid use, than multiplying the number survey respondents by two corrects for gaps in survey coverage to include PWID who did not participate in the survey and/or receive treatment for opioid use (i.e., 3,000 PWID). Having received counts of known PWID from provincial custodians, Jacka et al. (2020) multiply each count by multipliers taken from provincial-level statistics for methadone treatment. For BC, specifically, Jacka et al. (2020) report a notable increase in the PWID from 2011 and 2016. In 2011, they estimate 36,000 PWID (probable range = 31,900 – 40,100), or 1.15% (probable range = 1.02–1.28%) of BC's population 15–64 years old. By 2016, they estimate 47,600 PWID (probable range = 42,100 – 53,000), or 1.48 (probable range = 1.31–1.65%) of BC's general population 15–64 years old.[7] And, consistent with other modes of reporting (i.e., Statistics Canada, 2018, Jacka et al. (2020) report BC had the highest prevalence of injection drug use compared to other provinces over this timeframe.

The RAND studies on the size of heroin market

To calculate expenditures on fentanyl and fentanyl-contaminants in BC, we draw from established methods for estimating the size of US heroin markets (Kilmer & Pacula, 2009; Kilmer et al., 2014; Midgette et al., 2019).

Midgette et al. (2019) estimate the size of the US heroin market by combining survey data from Arrestee Drug Abuse Monitoring program (ADAM) with Uniform Crime Reports (UCR), and

[6] Algorithm performance, evaluation, and consequent selection was based on the calculation of sensitivity, specificity, positive predictive, and negative predictive values. The best performing model consisted of sampling on two medical visits or one hospitalization *and* participation in opioid treatment therapy.

[7] To be clear, the change in size over the two reporting periods (i.e., +32%) reflects the change in the ratio of provincial-reporting of PWID and treatment for opioid use. Although this change likely reflects increasing numbers of PWID throughout the province, the change, or some part of it, might reflect improvements in reporting over this five-year span, too.

various other data sources.[8] [9] Across US counties, Midgette et al. (2019) first estimated the proportions of male arrests with positive heroin tests reported by ADAM, controlling for county-level prevalence of use, participation in drug treatment, mortality rates from overdose, percentage of the population 18–24 years old, poverty rate, county population, and percentage of the population who graduated from high school or equivalent. Next, to calculate counts of arrested male heroin users by county, their estimates were multiplied by police-reported arrests involving males from county-level UCR. Arrestees with current heroin use were then grouped into three categories, consistent with ADAM's self-reported frequencies of use: 21 or more days in the past month (i.e., daily use), 11–20 days in the past month (i.e., frequent use), and 4–10 days in the past month (i.e., infrequent use). As the counts represented male arrestees within counties, Midgette et al. (2019) used several inflation factors to include other groups of heroin users, including criminally active adult men who were not arrested, adult men not criminally active, women, and juveniles.[10] All groups were further inflated by 1.03 to correct for more sporadic heroin use (i.e., less than 4–10 days per month, on average).

Average monthly expenditures were then calculated on a per month basis for counties by year, where ADAM survey coverage made it possible to do so. For each of the three groups of daily, frequent, and infrequent heroin users, Midgette et al. (2019) multiply their counts of heroin users by the typical price paid for heroin and typical number of use days per month.[11] For 2016, monthly expenditures ran USD$1,880 for daily heroin use, USD$847 USD for frequent heroin use, and USD$411 for infrequent heroin use. By year, spending on heroin cost USD$22,560 for daily use, USD$10,164 for frequent use, and USD$4,932 for infrequent use. All estimates were inflated by 1.125 to factor in non-cash transfers (e.g., gifts, trade, etc.), as not all heroin is purchased with money.

After calculating consumption from total expenditures (per year) and street prices for heroin reported by System to Retrieve Information from Drug Evidence (STRIDE), the nation-wide estimate for heroin expenditures was $USD 43 Billion (c. 2016).

This methodology was adapted to the context of PWUEF in BC. After having estimated prevalence of fentanyl use and/or exposure for Vancouver and BC, three steps were involved for calculating

[8] ADAM was run by the United States Department of Justice (DOJ) from 1997–2003 (i.e., ADAM I) and the Office of National Drug Control Policy from 2007–2014 (i.e., ADAM II). Although now defunct, its purpose was to monitor prevalence and patterns of drug use by arrestees.

[9] As ADAM has been defunct since 2014, well before the start of the current opioid crisis, Midgette et al. (2019) could not estimate the size of the US fentanyl market.

[10] To calculate criminally active heroin users who evaded arrest, counts of daily, frequent, and infrequent were divided by risk of arrest. Adjustments were made for heroin users who were not criminally active using inflation factors from previous research. A sex/gender ratio taken from various sources was used to calculate heroin use for women. And counts of youths were calculated from ratios of prevalence of use for adults and juveniles.

[11] Average prices for heroin reflect ADAM participant self-reporting of their most recent purchase. All participants were grouped by their self-reported frequency of heroin use and their responses for the price paid for heroin were then averaged to obtain mean prices paid by daily, frequent, and infrequent heroin users.

total expenditures on fentanyl and fentanyl-contaminated opioids or stimulants. Beginning from our provincial-level prevalence estimates by frequencies of use for the first and second scenarios:

1. Assign expenditures (per day of use) by frequencies of use;
2. Multiply by number of days of use per month to calculate monthly expenditures; and
3. Multiply by twelve to calculate annual expenditures

As cohort surveys did not include questions pertaining to participants spending on opioid use, specifically, we take daily expenditures on heroin use from studies reporting similar frequencies of heroin use to BCCSU's cohort studies (Midgette et al., 2019). Based on reporting for frequencies of use, daily use equates to $23,747 spent on fentanyl and/or fentanyl-contaminated opioids or stimulants per year; frequent use costs $10,700 per year; and infrequent use costs $5,192 each year.

Data and Methods

BCCSU cohort studies of injection and non-injection drug use

For this report, we combined interview data from three concurrent prospective cohort studies of people who use drugs (not including cannabis and via either injection and non-injection routes) living within Vancouver: the Vancouver Injection Drug User Study (VIDUS);[12] the AIDS Care Cohort to Evaluate Exposure to Survival Services (ACCESS);[13] and the At-Risk Youth Study (ARYS).[14] Figure 4 presents the timeline for the start of the three cohort studies in relation to the current opioid crisis and our observation period (c. 2017–2018).

Figure 4 A timeline of the running cohort studies in relation to the onset of opioid crisis and the observation period for this report

[12] For more information, see: https://www.bccsu.ca/vidus/
[13] For more information, see: https://www.bccsu.ca/access/
[14] For more information, see: https://www.bccsu.ca/arys/

All three cohort studies have been running simultaneous since 2005, through the efforts of the British Columbia Centre on Substance Use (BCCSU).[15] In 1996, VIDUS was started in response to the high HIV prevalence for people who inject drugs living in Vancouver's Downtown Eastside (DTES) (see McInnes et al., 2009).[16] Today, VIDUS represents one of the world's longest-running prospective cohort studies of people who inject drugs. From 1996 to 2005, approximately 1,500 PWID—both HIV-positive and HIV-negative—were recruited through local harm reduction services (e.g., needle exchanges), local treatment centres for drug dependence (e.g., methadone clinics), and from the open drug market and other community settings in the DTES. In 2005, ACCESS was established with HIV-positive PWID participating in the VIDUS study; from then on, HIV-negative participants were followed in VIDUS, and HIV-positive participants were followed in ACCESS. At the same time, eligibility requirements for ACCESS where changed to include people who use drugs (i.e., non-injectors, though more than 90% report injection drug use). Also in 2005, investigators now at the BCCSU established their third cohort (ARYS) for the purpose of surveying HIV-negative street-involved youth (16-28 years old) in Vancouver's Downtown South neighborhood. Table 1 summarizes the criterion to be eligible for participation for each cohort.

Table 1 A summary of the criterion to be eligible for participation for each cohort

BCCSU cohort	All criterion for eligible participation	Enrollment
VIDUS	– At least one instance of injection drug use in the 30 days prior to recruitment – HIV negative (2005-)	~ 1,500
ACCESS	– Injection drug use in the 30 days prior to recruitment or illicit drug use (except for cannabis) in the 30 days prior to recruitment – HIV-positive	< 1,000
ARYS	– Injection or non-injection drug use in the 30 days prior to recruitment (except for cannabis) – Between 16-28 years old at recruitment – Homelessness, marginal housing, or use of services for homeless youth (e.g., shelters, etc.)	< 1,000

Notes: All VIDUS or ARYS participants testing HIV-positive are transferred into ACCESS cohort. Across our observation period, only one VIDUS participant acquired HIV and changed cohorts.

Across the three cohort studies, more than 3,000 participants are surveyed on an ongoing basis. As open cohorts, the studies periodically recruit new participants to replace those lost-to-follow-up; enrollment for VIDUS tops out near 1,500 participants, while enrollment for ACCESS and ARYS tops out close to 1,000 participants each. After recruitment and the first interview, follow-up

[15] BCCSU's mandate concerns translating research into improvements and evidence-based treatments for substance use and addiction.

[16] After the 1980s AIDs epidemic, Vancouver continued to report high rates of HIV prevalence throughout the 1990s – 2000s. At this time, needle sharing between IDUs in Vancouver's DTES posed the highest risk of contracting and transmitting HIV. By 2006, Vancouver's HIV prevalence for people 15 years and older was six times higher than the national rate (McInnes et al. 2009).

interviews occur every six months thereafter (i.e., participants can be interviewed twice each calendar year). Follow-up interviews concern participant behaviour (i.e., drug use; risk behaviour; harm-reduction, health, and social services; etc.) from the previous six months. Every six months, trained interviews survey close to 1,500 participants in total—500 from each cohort, or one-third of VIDUS participants and half the current enrollments for ACCESS and ARYS. A typical participant takes part in interviews for less than two years.

As the three cohort studies use standardized semi-structured questionnaires, we can pool or combine survey records from each of the three cohort studies.[17] By pooling survey records in this fashion, we get more complete reporting from PWUD of various backgrounds than we would otherwise if we studied each cohort in isolation—consistent with the principles of sample coverage (Good, 1953).

All survey participants complete their questionnaire with the help of trained interviewers. Interviews revisit participant drug use, risk behaviour for acquiring blood-borne pathogens and their use of harm-reduction, health, and social services over the previous six months. After interviews, clinic nurses test participants for HIV (VIDUS & ARYS) and Hepatitis C virus antibodies (VIDUS, ACCESS, & ARYS), while ACCESS participants provide blood samples for HIV clinical monitoring. VIDUS or ARYS participants who test HIV-positive transfer to the ACCESS cohort.[18] And, since June 2016, participants urine screen for nine substances (fentanyl, morphine [i.e., heroin], methadone, buprenorphine, oxycodone, cocaine, amphetamine, benzodiazepines, and tetrohydrocannabinol [THC, the primary component of cannabis.]).

A description of cohort surveying

For this report, we use cohort interviews from the two most recent years available to us (c. 2017–2018). Across the three cohort studies, we first group interviews occurring over the entire observation period into four survey periods—each lasting six months in length. For each year of the observation period, the first six-month survey period runs from January 1st through June 30th, while the second six-month survey period occurs from July 1st through December 31st.[19]

[17] From conception, the original survey design and instruments for the three cohort studies have remained unchanged for the most part. Between the three cohorts, ACCESS includes questions specific to HIV (health, treatment, risks of transmission to others, etc.) not posed to VIDUS or ARYS participants.

[18] Across the observation period (c. 2017 – 2018), one VIDUS participant was transferred to ACCESS. To be clear, cohort participants can be transferred between cohorts. Throughout the observation period, just one participant transferred from VIDUS into the ACCESS cohort because they contracted HIV. For the purpose of statistical modelling, we group this person in with the ACCESS cohort for the entire two-year observation period.

[19] BCCSU's survey design operates on six-month cycles ranging from December 1st through May 31st and June 1st through the end of November. For this report, we shift the beginning of the survey period to coincide with the six month cycle of the calendar year, to be consistent with other mechanisms of reporting used to derive total expenditures (e.g., fatal overdoses reported by local Health Authorities, street prices of fentanyl provided by Vancouver Police Department, etc.). Aligning our survey periods with the calendar year has little to no effect on composition of this sample, or the frequencies and patterns of survey participation because 1) we maintain the six month cycle and 2) the high rates of survey retention from one survey period to the next. For example, someone interviewing in December (c. 2016), which we omit from our observation period, would be scheduled for their follow-up interview in June (c.

Together, the full slate of interviews occurring within each six-month period represents the surveying of eligible cohort participants (i.e., akin to census reporting); therefore, we use the term survey period when referring to interviews occurring within one of the four six-month interview cycles and we use the term survey or survey participation to refer to interviewing in general.

A variety of factors influenced the starting point for our observation period (i.e., January 1st, 2017). First, our observation period needed to begin sometime following the emergence of substantial amounts of fentanyl in the unregulated market. To this end, our observation period covers the two full calendar years (January 1st–December 31st) following the official public health declaration of BC's overdose crisis (c. April 14th, 2016).[20] A lag between then and our stating point (c. January 1st, 2017) lets us begin observing once both cohort participants and investigators had more awareness of fentanyl—in terms of its prevalence, price, potency, and so on. And second, each of urine screening for drug use didn't begin until June 2016; therefore, our observation period provides us with two full calendar years of valid and reliable reporting of exposure to fentanyl.

Analysis of trends in prevalence of use from interview responses and urine screening further supports our decision to begin our observation period c. 2017. Fentanyl prevalence of use – be it either self-reported or detected through screening – was much lower throughout 2016—especially for the first six months (i.e., January 1st–June 30th) before urine screening began (not shown in tabular format). Even once urine screening began, the lower overall prevalence of use detected over the next six months (i.e., July 1st–December 31st, 2016) compared to the next 24 months (i.e., 2017– 2018) suggests most opioids on the street were not yet contaminated with fentanyl (Tupper et al., 2018). If we began our observation period earlier (i.e., January 1st, 2016), the overall lower prevalence of fentanyl use in the first year of reporting – which may be influenced by timing of participant interview schedules – might impact our prevalence estimates.[21]

Apart from timing, the other criterion for sampling concerned participant fentanyl use. We condition sampling on cohort participants who either self-report fentanyl and/or heroin use or screen for fentanyl exposure sometime in the two-year observation period (N = 1,213). As upwards of 90% of heroin contained fentanyl over much of our study period (Tupper et al., 2018), our sampling criteria includes self-reported heroin use.[22] In fact, the low overall levels of self-reported

2017); therefore, they could still be surveyed within the first six months of our observation period, if they continue to participate—which most do.

[20] In 2016, close to 1,000 overdose deaths were reported throughout BC, with more than 200 overdose deaths reported in Vancouver—increasing 80% over the previous year's deaths from overdose. According to BC's coroners report (2017), fentanyl was detected in 60% of overdose deaths, compared to 30% of overdose deaths in the year prior.

[21] An increase in reporting and detection of fentanyl use over might upward bias our estimates from one model (i.e., the 'closed' model), while downward biasing our estimates from the other model (i.e., the 'open' model). For 2016, the gaps in reporting and testing result in few participants self-reporting and/or screening for fentanyl use (n < 50). But with two more survey periods (f. 2016), we'd invite more 'transience' into the overall patterns of survey participations for cohort participants observed over the three years (recall typical participation in the three cohort studies lasts less than two years, so more survey periods means more transient participation).

[22] In response to the opioid crisis, BC established drug checking programs to help keep PWUD keep informed about what's in their drugs to mitigate risks (Laing, Tupper, & Fairbairn, 2018). Testing shows ~90% of heroin samples

fentanyl use for our sample participants (n = 217) suggests most fentanyl use identified through screening was tied to using "down".[23]

To be certain, sampling on heroin/fentanyl prevalence of use within the observation period means we observe cohort participants in retrospect, over each of the survey periods—no matter if they interviewed or not, or if they used fentanyl or not. In the first survey period, for example, most participants have yet to participate (i.e., interview) and/or use fentanyl or otherwise be exposed to it. But everyone participating throughout our two-year observation period interviews one or more times and self-reports or screens for fentanyl use one or more times.

Analysis of survey recapture over the observation period (2017–2018)

Table 2 presents frequencies of interview participation for participants in terms of their total number of interviews over the observation period. As we observe four six-month interview periods over the length of the observation period, participants can take part in four interviews over this timeframe. For each participant, each interview they complete equates to one "capture". The first interview for each participant that we observe represents their initial capture—regardless of whether it occurs in the first, second, third, or fourth survey period. Each follow-up interview (i.e., second, third, or fourth interview) equates to one survey "recapture".

Table 2 Frequencies of survey recapture for PWUEF, 2017–2018

	N SURVEY RECAPTURES			
	1	2	3	4
Across cohort studies[a]	215 (17.73%)	204 (16.82%)	389 (32.07%)	405 (33.39%)
VIDUS[b]	97 (16.67%)	93 (15.98%)	190 (32.65%)	202 (34.71%)
ACCESS[c]	42 (13.38%)	39 (12.42%)	105 (33.44%)	128 (40.76%)
ARYS[d]	76 (23.98%)	72 (22.71%)	94 (29.65%)	75 (23.66%)

[a] VIDUS + ACCESS + ARYS (N = 1,213)
[b] VIDUS = Vancouver Injection Drug User Study (n = 582)
[c] ACCESS = AIDS Care Cohort to Evaluate Exposure to Survival Services (n = 314)
[d] ARYS = At-Risk Youth Study (n = 317)

For each cohort, our sample coverage represents close to one-third the enrollment over the two-year observation period. Almost half of our participants reporting or screening for fentanyl use belong to the VIDUS cohort (n = 582). The remaining other half of participants were split between ACCESS (n = 314) and ARYS cohorts (n = 317). In total, more than two-thirds of participants take part in three or four interviews. And more than one-third took part in each of the four interviews occurring over the two-year observation period. A total of 215 cohort participants (17.73%) had one interview or "capture".

contained fentanyl. As odds of exposure to fentanyl through heroin use were very high over the observation period (c. 2017 – 2018), by including self-reported heroin use in our sampling criteria, we include participants who were at high-risk of exposure, but who's last exposure was outside the detection window from screening (i.e., ≤ 96 hours).

[23] According to previous research, "down" refers to both heroin and fentanyl sold on the streets of Vancouver and BC, generally (Pardo et al., 2019).

Figure 5 further decomposes frequencies of recaptures (i.e., Table 2) by cohorts and for other subgroups. Across cohorts, the largest groups of VIDUS and ACCESS cohort participants participate in three or four interviews over the two-year observation period. By comparison, interviewing patterns of ARYS cohort participants were less predictable—no clear pattern in participation emerges. Frequencies of recaptures by age groups further reinforce trends in cohort survey participation. Trends in interview participation for the youngest cohort participants (i.e., 10-29 years old) mirror the trends in interview participation for ARYS cohort participants. For older cohort participants (i.e., 40-69 years old), their trends of participation seem more persistent than the younger cohort participants—most participants older than 40 years old take part in three or four interviews. And the higher frequencies of older cohort participants reflect the trends in overall interview participation and for VIDUS and ACCESS cohorts in particular. Across sex/gender and race/ethnic groups, within-group frequencies of participation tend to follow the overall trends within and between cohorts.[24]

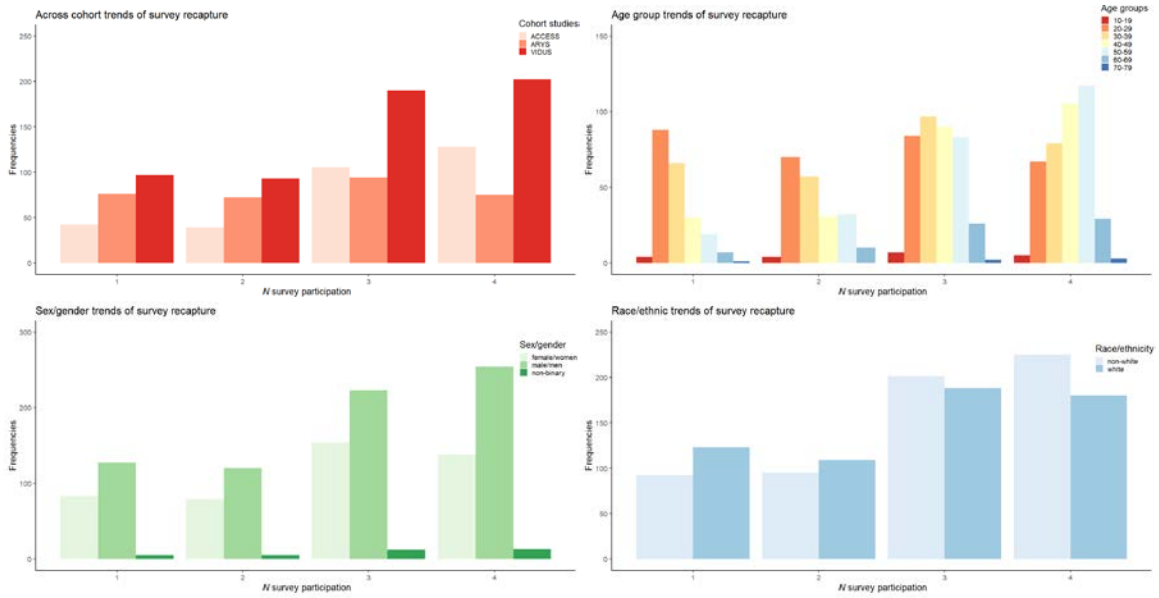


Figure 5 Frequencies of survey recapture for fentanyl prevalence of use, 2017-2018

Besides frequencies, temporal patterns of interview participation help us understand trends in overall prevalence of fentanyl use between cohorts. Table 3 presents frequencies of participant in-flow (i.e., their first observed interview) and out-flow (i.e., last known survey participation) over the observation period. A large in-flow of participants taking their 'first' interview and/or using fentanyl occurs in the first survey period (i.e., January 1st–June 30th, 2017) compared to the other survey periods because of high survey 'retention' throughout the observation period—most participants take part in three or four interviews.[25] According to participant self-reporting and screening, fentanyl prevalence of use somewhat wavers over the observation period. In the first

[24] Across race/ethnic groups, non-white participants had higher frequencies of recapture than white participants.
[25] To be sure, no survey retention occurs for the first survey period because it's the beginning of our observation period. As explained earlier, prior survey periods occur outside the observation period for this report.

survey period, 51% of participants surveyed reported or screened for fentanyl use. Although over the next three survey periods, fentanyl prevalence of use within and between cohorts stabilizes (≅ 58–63%).

Table 3 Across cohort trends in survey participation for PWUEF, 2017–2018

	SURVEY PERIOD			
	(1) Jan-Jun '17	(2) Jul-Dec '17	(3) Jan-Jun '18	(4) Jul-Dec '18
Survey participation				
In-flow for survey t[a]	881	156	115	61
Retention from survey t_{-1}[b]	—	679	692	616
Out-flow for survey t[c]	62	73	319	759
N	881	835	935	759
Fentanyl prevalence of use				
In-flow for survey t[a]	450	237	194	90
Retention from survey t_{-1}[b]	—	255	303	284
Out-flow for survey t[c]	85	142	302	442
N	450	492	586	442
% Fentanyl prevalence of use[d]	51.08	58.92	62.67	58.24

[a] participants interviewed for the first time at survey period t (i.e., their first observed interview)
[b] participants interviewed at survey period t who were interviewed in the previous survey period t_{-1}
[c] participants interviewed for the last time at survey period t
[d] % participants self-reporting fentanyl use or exposed to fentanyl for survey period t

As for patterns of interview out-flow for cohort participants (i.e., participants seen for the last time), it is important for us to view interview out-flows in light of the high numbers of deaths from overdose occurring over the observation period (c. 2017–2018). In total, low outflows from the first (n = 62) and second (n = 73) survey periods does not suggest high death rates for cohort participants; however, outflow spikes in the third interview period (n = 319). Although part of this increase reflects the fact participants have just one more chance of recapture beyond the third interview period (i.e., the fourth interview period), the higher than expected outflow suggests deaths from overdose might influence trends in interview participation.

To provide more insight to the potential effects of deaths from overdose on recapture, Figure 6 presents capture correlations between survey periods. Both within- and between-cohorts, correlation coefficients for recapture weaken over the observation period (i.e., reflecting lower levels of participation and retention): recaptures most often occur over consecutive interview periods, but occur less often over three or more survey periods. As our interview periods coincide with the opioid crisis, cumulative fatalities from reported overdose might affect interview recapture through lower interview retention. Indeed, fewer participants self-report overdosing with each passing survey period (not shown in tabular format), so low recapture in later interview periods might reflect high reported incidence of overdose earlier in the observation period—if those participants later died from overdose.

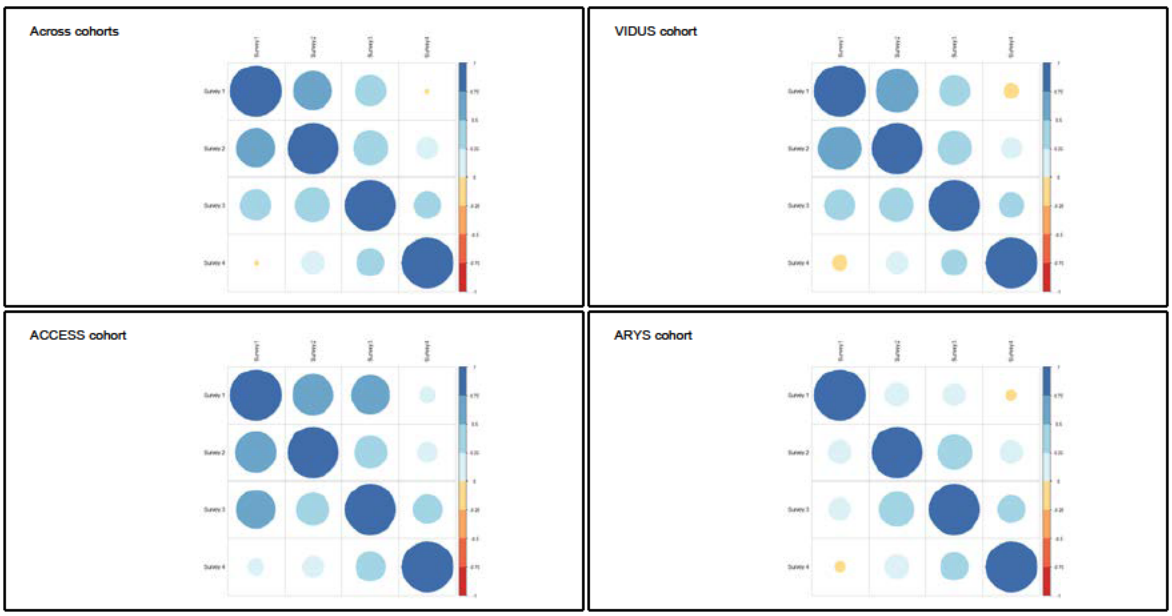


Figure 6 Between- and within-cohort patterns of survey recapture, 2017–2018

Note: Bubbles represent Yule's *Q* correlation coefficients for patterns of survey recapture, where the size of the bubbles within each cell reflects the (±) strength of the correlation coefficient and the colour scaling represents either positive (blue) or negative (red) correlations.

Along with falling rates of recapture over the observation period, we observe variations in the overall strength of capture correlations between consecutive interview periods. Across cohorts, we observe the highest participant retention between the first and second interview periods. For individual cohorts, this overall pattern holds for VIDUS and ACCESS cohorts participants, but not ARYS cohort participants. For ARYS participants, retention peaks between the second and third interview periods, but still remains strong between the third and fourth interview periods—perhaps reflecting retainment efforts; nonetheless, between-cohort variations in recapture reinforce our prior observations of the potential effects of overdose on recapture over the observation period. For instance, higher risks of death for VIDUS and ACCESS cohort participants – tied to more frequent injection drug use and worse overall health – might help us rationalize their rates of recapture compared to ARYS cohort participants.

A note on probable gaps in survey recruitment

All three cohort studies represent samples of marginalized people who use drugs (i.e., people who inject or use via other routes) living in Downtown Vancouver. Each cohort on its own is not representative of Vancouver's population of people who use drugs (see Table 1): VIDUS surveys people who inject, most of whom live in Vancouver's DTES; ACCESS consists of HIV-positive people who use drugs; ARYS surveys street-involved youth less than 30 years old. As each cohort consists of marginalized people who use drugs from various backgrounds, combining interview records from the three cohort studies provides us with more representative sampling than we would have otherwise from each separate cohort. Although sampling from multiple cohort studies still has its limits when it comes to 'total coverage' of PWUD in Vancouver. Irrespective of the reach of three cohort studies with respect to recruitment, mechanisms of recruiting either miss or outright

exclude eligible participants from participating in the cohort studies in the first place. Table 4 summarizes multiple sources of probable sampling error that we believe might exclude potential eligible participants from one or more of the three cohort studies.[26]

First, each of the three cohort studies might be less likely to include eligible participants who *never* use Vancouver's safe injection facilities (SIFs), harm-reduction services, or drug dependence treatment sites (e.g., methadone clinics). However, study staff recruit extensively from individuals in the open drug market. Also, cohort staff can leave automatic messages for participants who use Insite, Vancouver's first SIF, reminding them of upcoming/missed interview opportunities. As for the effects of this kind of behavior for estimating prevalence, those not using SIFs or other harm-reduction or treatment services would have to be less likely to be recruited. This gap in sampling affects eligible participants, regardless of their frequencies of use. As we describe in more detail later, we control for participant self-reported SIF use and participation in methadone treatment to offset some of the issues of non-sampling.

Second, the current enrollment targets (i.e., cohort size) pose constraints for estimating prevalence of use. As noted, each of the three cohort studies 'cap' survey participation (≅ 1,000–1,500 participants). For the purpose of survey enumeration, enrollment targets for participation impose sampling bias. Enrollment targets lead to the over-recruitment and retention of 'stayers' – eligible participants most susceptible to recruitment and continued participation – compared to 'transients'—people less susceptible to recruitment and/or continued participation. As for the resulting prevalence of use estimates, enrollment targets may lead to 'lowball' estimates, or estimates much too conservative. To evaluate the effects of capped enrollment on our prevalence of use estimates, we re-estimated our preferred models using random samples of various sizes (i.e., 10%, 25%, 50%, 75%, 90%). We didn't get close to replicating our estimates until our random sample was sufficiently large (i.e., 90%); therefore, it's more probable than not that the surveying of eligible participants hasn't yet reached saturation—the point where more recruitment or capping enrollment wouldn't change our estimates.

Third, we encounter problems from the effects of boundaries on sampling—in terms of both recruitment and retention. Throughout the observation period (c. 2017–2018), most cohort participants report living in Vancouver's DTES (i.e., two-thirds of cohort participants). Yet, one-third of participants still report living outside Vancouver's DTES. By focusing surveying efforts in the DTES, surveying misses or excludes eligible participants residing outside Vancouver's DTES. Boundary problems therefore result in lower overall prevalence estimates of eligible participants living outside of Vancouver's DTES than what would otherwise occur with city-wide coverage in terms of recruiting and surveying.

[26] To be clear, potential gaps in cohort recruitment have no effect on recapture for participants. Attrition in cohort participation (i.e., participants 'dropping out' of the studies) occur for various reasons. Deaths from overdose represents another mechanism of study non-participation, especially when considering the mounting death tolls from reported overdose over the observation period (Tupper et al., 2018).

Fourth, recall that urine screening for fentanyl exposure represents one of our sampling criteria. For cohort participants to be included in our sample over the two-year observation period, they either need to self-report fentanyl/heroin use or screen for fentanyl exposure. In comparison to self-reporting, screening represents the more reliable criterion for sampling. Furthermore, most of our sample participants identified through screening (*n* >60%); however, the observation period for fentanyl screening (i.e., ≤ 96 hours—though different metabolites have different window periods) might exclude participants who use less than once per week (i.e., the time of their most recent use occurred outside the detection window). By comparison, frequent users (i.e., those using 1-3 days per week) patterns of use ought to be sporadic or random enough not to slip past screening. All said, non-detection from urine screening should have marginal effects on prevalence of use estimates and/or resulting estimates of expenditures—more frequent drug use matters more for calculating total expenditures (Midgette et al., 2019).

Fifth, ARYS recruiting through Peer Research Associates (PRAs) might impose recruiting effects influencing survey participation. For example, recruiting efforts might focus on prospective participants within PRA social networks rather than others outside their network of contacts. If PRA contacts do not represent the social network of street-involved youth, then we would estimate lower overall prevalence for participants with certain characteristics (e.g., age) or those with no contact with service providers (which might mean less contact with PRAs).

A capture-recapture of cohort surveying

Because the study design splits our observation period into four six-month interview periods, we're able to replicate the methods used for census enumeration. Adjustments to the general census get made through post-enumeration surveys (PES), which occur in the months following the census. PES help census takers evaluate the completeness of the census (i.e., its population coverage) through re-sampling: the ratio of people recaptured from the original census to the people who were missed. As with census enumeration, frequencies and patterns of survey participation over the observation period (c. 2017–2018) lets us fill the gaps in survey coverage to estimate total fentanyl prevalence of use (i.e., the numbers of PWUEF, consisting of both survey participants and non-survey participants). Admissions for treatment for substance use disorders have been used for similar purposes in past work (Brecht & Wickens, 1993; Hser, 1993; Leclerc et al., 2014).

To estimate prevalence of fentanyl use, we use two separate capture-recapture methods. Assumptions between the two methods differ with respect to closure (i.e., the effects of births, deaths, and/or migration on trends in overall prevalence). The first type of model we use belongs to the set of 'closed' capture-recapture models. All closed models make strong assumptions with respect to closure. In effect, closure means the recruitment pool of eligible survey participants for each of the three cohort studies remains stable over the entire two-year observation period. From closed models, we calculate the average or typical prevalence over the length of the observation period. By comparison, the second type of model we use belongs to the set of 'open' capture-recapture models which don't require closure; rather, open models project trends in overall prevalence for each specific survey period—controlling for the effects of birth, deaths, and/or

migration. Across cohort studies, non-closure means the recruitment pool of eligible survey participants might be unstable (or is not required to be stable) because of factors like participant emigration from Vancouver's DTES or the city itself and high risk of fatal overdose.

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For both types of models, our main variable of interest concerns self-reported frequencies of heroin use by cohort participants. By conditioning probabilities of survey recaptures on frequencies of heroin use, we can then calculate overall prevalence of use for each type of user—important for computing total expenditures. We report on frequencies of heroin use over fentanyl frequencies of use for two reasons. First, participants provided better reporting on their heroin use. And second, most heroin on the street over our observation period contained fentanyl—meaning there's high concordance between fentanyl and heroin use (Tupper et al. 2018). For those reasons, self-reported frequencies of heroin use ought to represent fentanyl use patterns.

Table 5 reports self-reported frequencies of fentanyl and heroin use by cohort participants over the observation period. Across the four interview periods, self-reported frequencies of use for fentanyl and heroin remain stable but isn't well reported in comparison to heroin use. A large group of daily heroin users report over the observation period (≅ 36–40%). Another large, stable group reports infrequent heroin use (≅ 44–46%), consisting of cohort participants who report using less than once per week and often less than once per month. A third, smaller group reports frequent heroin use (≅ 15–18%), consisting of one to three use days per week. Here we use group in reference to each specific reporting period. Participants who report frequent use in one survey period might report daily use or infrequent use in the next survey period, or vice versa. In fact, frequencies of self-reported use do often change over the two years; so much so that frequent use represents the most typical group over the entire two years (see Table 6 below).

Table 4 A summary of types of sampling error and probable effects for estimating fentanyl prevalence of use

Issue of sampling error	Description of non-sampling	Affect on sampling inclusion	Affected cohort studies
Never SIF users, or other non-users of social and health services for drug use	Non-sampling of PWID who *never* use SIFs (i.e., Insite, VANDU). As Insite provides a mechanism for retention efforts for multiple cohort studies, our sampling frame (i.e., cohort studies) would be less likely to include PWIDs who never use SIFs and PWIDs. A similar logic holds for those who don't use harm-reduction services or never receiving treatment for substance use disorders.	PWID not using SIFs, or other harm-reduction or treatment services would have less chance to be recruited in the first place. This gap in sampling effects every 'type' of drug user (in terms of frequencies of use). We control for participant self-reported SIF use and participation in methadone treatment to offset some of the issues of non-sampling.	VIDUS; ACCESS; ARYS
'Enrollment targets'	Each of the three cohort studies target survey participation (≅ 1,000–1,500 participants). For the purposes of survey enumeration, enrollment targets for participation impose sampling bias by over-recruiting and retaining 'stayers' – participants susceptible to recruitment and continued participation – compared to 'transients'—people less susceptible to recruitment or continued participation.	For estimating prevalence, enrollment targets may result in 'lowballing' (i.e., too conservative). After re-estimating our models using random samples of various sizes (i.e., 10%, 25%, 50%, 75%, 90%), we don't replicate our estimates until our random sample was sufficiently large (i.e., 90%); therefore, it's more probable than not the surveying of eligible participants hasn't yet reached saturation—the point where more recruitment (i.e., increasing sample size) wouldn't change our estimates.	VIDUS; ACCESS; ARYS
Boundary or 'edge' problem	Across the length of the observation period (c. 2017–2018), most cohort participants report living in Vancouver's DTES; however, one-third of participants report living outside the DTES. It's probable we are under-sampling groups of PWUDs who would be eligible to participate in each of the three surveys, but reside outside Vancouver's DTES.	Boundary or 'edge' problems would result in lower overall prevalence estimates for PWUDs living outside of Vancouver's DTES than what would otherwise occur with better city-wide sample coverage (i.e., recruiting).	VIDUS; ACCESS; ARYS
Urine screening – 'window' period	Time of most recent use occurs outside the ≤ 96 hours hour fentanyl detection window for urine screening. Assuming sporadic use for frequent users (i.e., 1-3 days/week), urine tests might undercount infrequent users.	As infrequent fentanyl use would be missed through screening more often then more frequent use, high non-detection should have marginal effects on prevalence of use estimates and/or resulting estimates of drug expenditures—more frequent drug use matters more for calculating total expenditures.	VIDUS; ACCESS; ARYS
ARYS recruiting through Peer Research Associates (PRAs)	A recruiting effect might occur, where some eligible participants within PRA social networks of contacts could be more likely to be recruited than others outside their social network of contacts.	If PRA social networks do not represent the social networks of street-involved youth, than we would estimate lower overall prevalence for street youths with certain characteristics (i.e , age) or those with no contact with service providers.	ARYS

Table 5 Frequencies of fentanyl and heroin use by cohort participants, 2017–2018

	SURVEY PERIOD			
	(1)	(2)	(3)	(4)
Frequencies of use – self-report	Jan-Jun '17	Jul-Dec '17	Jan-Jun '18	Jul-Dec '18
Fentanyl use (%)				
1) Daily	35 (3.97%)	28 (3.35%)	38 (4.06%)	45 (5.93%)
2) Frequent	21 (2.38%)	18 (2.16%)	12 (1.28%)	12 (1.58%)
3) Infrequent	825 (93.64%)	789 (94.49%)	885 (94.65%)	702 (92.49%)
Heroin use (%)				
1) Daily	323 (36.66%)	317 (37.96%)	360 (38.50%)	305 (40.18%)
2) Frequent	159 (18.05%)	137 (16.41%)	162 (17.33%)	110 (14.49%)
3) Infrequent	399 (45.29%)	381 (45.63%)	413 (44.17%)	344 (45.32%)
N	881	835	935	759

Notes: Frequencies of use were self-reported by cohort participants. For heroin, daily use was self-reported in high frequencies (≅ 36–40%). Frequent use (i.e., 1-3 use days per week) was the least common reported within survey periods (≅ 14–18%), yet represents the most common profile when taking 'typical' response over the entire observation period. Infrequent use (i.e., often less than 1 use day per month) represents the largest profile of drug use over the observation period (≅ 44–46%).

For closed models, we take the typical response for frequencies of heroin use for each participant. For open models, we use participant self-reporting on their frequency of heroin for each survey period to estimate its effect on prevalence of use over the two-year observation period.

A closed model of survey recapture

To explain the workings of 'closed' models of survey recapture, we begin with most simple example of survey recapture possible, where both the rate of survey recapture and overall prevalence can be calculated from two surveys:

$$\frac{n}{N} = \frac{M}{R}$$

Equation 1 An example of survey recapture with two surveys

where,

- *n* equals the cohort participants surveyed in survey period t_1;
- *N* equals overall prevalence of surveyed and non-surveyed participants;
- *M* equals the cohort participants surveyed in survey period t_2; and
- *R* equals the recaptured survey participants (i.e., participants retained from t_1–t_2)

If we knew the value of *N* then we could calculate the rate of survey capture (i.e., n/N). Although we don't know *N*, so we need to estimate it. To do so, we rearrange Equation 1 into:

$$\widehat{N} = \frac{Mn}{R}$$

Equation 2 Estimating *N* from two surveys, with known recaptures[27]

where, $\widehat{N}$ represents the estimate of *N*. To make things more concrete, let's take the participants interviewed in last two survey periods for our observation period (i.e., 2018) and participants retained from one survey period to the next (see Table 3) to calculate the non-surveyed group of eligible (or potential) cohort participants (c. 2018):

$$1{,}152.05 = \frac{935 * 759}{616}$$

Across both periods, we get the estimate of the non-surveyed, eligible group of potential cohort participants (≅ 1,152). Adding the total sample of cohort participants for this year of the observation period (= 1,078) to the estimate of the non-surveyed yields the overall prevalence estimate (≅ 2,230); therefore, from this demonstration, the non-surveyed eligible group of potential cohort participants represents more than half of the overall prevalence estimate.[28]

Assuming *p* represents the probabilities of survey participation for survey periods t_1 and t_2, we can instead calculate survey recapture using multinomial logit regression (Darroch, 1958; Feinberg, 1972):

$$p = \log\left(\frac{p}{1-p}\right) = \beta + \beta\left(\text{survey}_{t_1}\right) + \beta(\text{survey}_{t_2})$$

Equation 3 A multinomial regression for estimating survey recapture from two surveys

where β represents the intercept—the predicted value of *p* when other model parameters equal 0 (i.e., equivalent to the estimate for survey non-participation), $\beta\left(\text{survey}_{t_1}\right)$ represents the mean predicted value of survey participation for survey t_1, and $\beta\left(\text{survey}_{t_2}\right)$ represents the mean predicted value of survey participation for survey t_2.[29]

After estimating probabilities of survey recapture, we then transform those estimated probabilities from Equation 3 into prevalence estimates of non-surveyed PWID, eligible to participate in one of the three cohort studies. To do so, we use the formula:

$$\widehat{N} = {}^{N}/_{\hat{p}}$$

[27] Equation 2's called the Lincoln-Petersen (Lincoln, 1930; Petersen, 1896) method, though it's been used to correct census registrars for hundreds of years (i.e., 250–400 years). Earliest known use of this method date to 1700s France to evaluate the completeness of the census and 1600s England to evaluate the effects of the plague on population size.
[28] To keep things simple, we do not walk through calculation of the confidence intervals for this demonstration.
[29] As the intercept (β) represents the predicted value of *p* when the mean predicted values of survey participation for t_1 and t_2 = 0, exponentiating it provides the prevalence estimate for non-surveyed potential cohort participants.

Equation 4 Estimator for non-surveyed eligible potential cohort participants

where the prevalence of those not surveyed ($\widehat{N}$) equals the sample size (N) divided by the mean probability of survey recapture for the sample ($\hat{p}$). Adding together the sample (N) and the prevalence estimate ($\widehat{N}$) yields the overall prevalence estimate for those eligible to be surveyed (based on the sampling criteria).

A closed model of survey recapture (c. 2017–2018)

At its core, the observation period represents four consecutive recapture surveys of cohort participants occurring over two years. Applying methods of enumeration surveys to fill in the gaps of cohort recruitment and interviewing, we estimate probabilities for participants *i* interviewing in survey period *t* from the observed frequencies and patterns of survey recapture (see Table 3):

$$\log\left(\frac{p_i}{1-p_i}\right) = \beta + \beta_i(\text{survey}_{t_1}) + \beta_i(\text{survey}_{t_2}) + \beta_i(\text{survey}_{t_3}) + \beta_i(\text{survey}_{t_4}),$$

Equation 5 A multinomial regression for estimating survey recapture from four surveys

where, p_i represents the predicted probabilities of recapture for cohort participants *i*, β represents the intercept, and β_i represent probabilities of survey recapture for each specific interview period *t*.

Next, we correct for unequal probabilities of survey recapture for cohort participants (i.e., characteristics of interviewed participants predicting participation and retention over the observation period). Here we use systems of equations to estimate predicted values for $\hat{\beta}_i(\text{survey}_{t_1})$, $\hat{\beta}_i(\text{survey}_{t_2})$, $\hat{\beta}_i(\text{survey}_{t_3})$, and $\hat{\beta}_i(\text{survey}_{t_4})$ from participant response items from interview records:

$$\begin{aligned}\hat{\beta}_i(\text{survey}_{t_1}) = {} & \beta_i(\text{age}) + \beta_i(\text{age}^2) + \beta_i(\text{sex}) + \beta_i(\text{race}) + (\text{education}) + \beta_i(\text{LGBTQ}_+) \\ & + \beta_i(\text{age, first use}) + \beta_i(\text{overdose}) + \beta_i(\text{frequencies}) \\ & + \beta_i(\text{heroin availability})\end{aligned}$$

$$\begin{aligned}\hat{\beta}_i(\text{survey}_{t_2}) = {} & \beta_i(\text{age}) + \beta_i(\text{age}^2) + \beta_i(\text{sex}) + \beta_i(\text{race}) + (\text{education}) + \beta_i(\text{LGBTQ}_+) \\ & + \beta_i(\text{age, first use}) + \beta_i(\text{overdose}) + \beta_i(\text{frequencies}) \\ & + \beta_i(\text{heroin availability})\end{aligned}$$

$$\begin{aligned}\hat{\beta}_i(\text{survey}_{t_3}) = {} & \beta_i(\text{age}) + \beta_i(\text{age}^2) + \beta_i(\text{sex}) + \beta_i(\text{race}) + (\text{education}) + \beta_i(\text{LGBTQ}_+) \\ & + \beta_i(\text{age, first use}) + \beta_i(\text{overdose}) + \beta_i(\text{frequencies}) \\ & + \beta_i(\text{heroin availability})\end{aligned}$$

$$\begin{aligned}\hat{\beta}_i(\text{survey}_{t_4}) = {} & \beta_i(\text{age}) + \beta_i(\text{age}^2) + \beta_i(\text{sex}) + \beta_i(\text{race}) + (\text{education}) + \beta_i(\text{LGBTQ}_+) \\ & + \beta_i(\text{age, first use}) + \beta_i(\text{overdose}) + \beta_i(\text{frequencies}) \\ & + \beta_i(\text{heroin availability})\end{aligned}$$

Equation 6 A systems of equations to correct unequal probabilities in survey recapture

where the coefficients for each control variable (i.e., for each β_i) get jointly estimated from our system of equations. As part of the systems of equations, we first control for unequal probabilities of recapture for cohort participants with respect to their age, sex, race, education (i.e., high school equivalent?), and sexual orientation (LGBTQ+?). Age quadratic terms let us evaluate whether cohort participants age in and/or age out of survey participation. And from retrospect (i.e., knowing what we know at the end of the observation period), we control for possible effects of participant drug use behaviour. Age of first drug use (not including cannabis), one or more self-reported overdose events (c. 2017–2018), typical frequencies of drug use (i.e., daily; frequent; infrequent use), and typical self-reported heroin availability (i.e., score ≤ 10 minutes; score ≤ 90 minutes; score ≥ 24 hours) provide us with possible sources of variation in recapture.

Table 6 presents descriptive statistics (sample means) for cohort participants self-reporting fentanyl/heroin use and/or screening for fentanyl exposure throughout the observation period. All descriptive statistics represent pooled sample means—the mean values reported by participants taking part in one or more surveys over the entire observation period.

Across cohorts, most participants were men (≅ 60%), non-white (> 50%), have not completed high school or equivalent (> 53%), and heterosexual (> 82%). Across interview periods, cohort participants get younger over time—reinforcing our prior claims regarding the weaker patterns of recapture for older VIDUS and ACCESS participants (c. 2017) versus the stronger patterns of recapture for younger ARYS participants throughout the observation period (see Figure 6). Age of first drug use (i.e., for non-cannabis), in comparison, remains stable over each of the four survey periods, with most participants reporting having first used before turning 25 years old. For prevalence of injection drug use, most participants report (in retrospect) having injected (> 95%). And in terms of other risk behaviour, the proportion of participants who report (in retrospect) having overdosed over the observation period remains stable (≅ 38–41%).

Besides controlling for variations in the characteristics and behaviour of individual participants, we further control for time effects and trap effects influencing trends in survey participation (see Otis, Burnham, White, & Anderson, 1978). Assigning temporal order to the patterns of survey participation lets us factor in the effects of time (i.e., with respect to timing of capture and/or recapture(s)) on probabilities of survey recapture. And trap effects let us offset the high observed rates of retention in survey participation over the observation period (Bishop, Fienberg, & Holland, 1975; Fienberg, 1972). Adding both time and trap effects to our models should control for violations of closure in the recruitment pool of eligible survey participants stemming from transience and high risk of fatal overdose.

Table 6 Mean sample statistics for PWUEF in Vancouver, 2017–2018

	Across cohorts	SURVEY PERIOD (1) Jan-Jun '17	(2) Jul-Dec '17	(3) Jan-Jun '18	(4) Jul-Dec '18
Cohort studies (%)					
VIDUS	47.98	48.81	48.62	48.13	49.41
ACCESS	25.89	28.94	28.14	26.95	27.01
ARYS	26.13	22.25	23.23	24.92	23.58
Age groups (%)					
10-19	1.65	1.59	1.32	1.39	1.98
20-29	25.47	20.20	21.56	22.78	23.32
30-39	24.65	22.13	22.40	23.32	24.64
40-49	21.10	23.16	23.83	22.57	22.13
50-59	20.69	25.43	23.83	23.21	21.08
60-69	5.94	6.92	6.59	6.20	6.19
70-79	0.49	0.57	0.48	0.53	0.66
Sex/gender (%)					
Female/women	37.43	35.98	35.09	38.61	37.42
Male/man	59.69	61.29	61.68	58.40	59.42
Trans/non-binary	2.89	2.72	3.23	2.99	3.16
White (%)	49.46	48.24	47.07	47.91	47.30
High school completion? (%)	46.66	44.27	45.87	47.17	46.38
LGBTQ+ (%)	17.64	16.00	17.13	18.18	18.18
Age, first drug use					
>12	3.38	3.97	3.83	3.10	3.56
12-17	44.93	44.27	43.23	44.60	44.40
18-24	28.52	27.70	30.18	28.66	26.88
25-29	10.22	10.44	9.94	10.05	11.33
30-34	6.68	6.70	5.63	7.27	6.98
35-39	3.63	4.20	4.43	3.85	3.82
40-44	0.99	1.02	1.20	1.07	1.32
45-49	0.66	0.68	0.84	0.43	0.66
50+	0.99	1.02	0.72	0.96	1.05
Has overdosed? (%)	38.42	38.59	41.08	39.14	40.18
Frequency of use[a] (%)					
Daily use	32.07	26.45	25.87	27.70	27.40
Frequent use	37.26	39.73	40.24	39.36	40.84
Infrequent use	30.67	33.83	33.89	32.94	31.75
Availability heroin[b] (%)					
score ≤ 10 minutes	73.29	74.69	72.81	71.55	71.28
score ≤ 90 minutes	22.67	22.59	24.67	25.24	25.30
score ≥ 24 hours	4.04	2.72	2.51	3.21	3.43
Ever inject? (%)	95.47	95.91	95.33	94.76	95.92
N	1,213	881	835	935	759

Notes: 'Across cohorts' column presents pooled sample means for participants over the entire observation period and irrespective of cohort.

[a] Frequencies of use represent 'typical' participant response over the observation period.

[b] Availability of heroin represent 'typical' participant response over the observation period.

Fentanyl prevalence of use estimates by frequencies of use

And to calculate specific estimates by frequencies of use, we first transform the estimated probabilities of survey recapture (Equation 5) into estimates of non-surveyed daily, frequent, and infrequent users, eligible to participate in one of the three cohort studies:

$$\hat{n}_k = {}^{n_k}/_{\hat{p}_k}$$

Equation 7 Estimator for non-surveyed eligible potential cohort participants, by their frequencies of use

let *k* denote participant 'typical' frequencies of use, where *k* can represent daily users, frequent users (i.e., 1-3 use days per week), or infrequent users (i.e., 1 or fewer use days per month). And, once more, n_k represents the specific counts of participants by their 'typical' frequencies of use *k*, $\hat{p}_k$ represents the mean probability of survey recapture for participants of typical frequencies of use *k* (estimated from Equation 5), and $\hat{n}_k$'s the estimate for the non-surveyed, with typical frequencies of use for group *k*.

Adding the prevalence estimates for the non-surveyed groups ($\hat{n}_k$) to our samples (n_k) yields the overall prevalence estimates for each group or typical frequency of use. As for interpreting the size of the prevalence estimate, we recommend thinking of the estimates in terms of the typical prevalence of use over the length of the two-year observation period (c. 2017–2018). As we include controls for both time and trap effects, we ought to control for trends in survey participation that would otherwise limit our confidence in stating the estimates to represent the typical prevalence of use for the observation period.

An 'open' model of survey recapture

Estimates of trends in prevalence of fentanyl use over the two-year observation period (c. 2017–2018) were projected by predicting the probabilities of survival (ϕ_{it}) and survey recapture (p_{it}) for cohort participants *i* over each survey period *t*. To do so, we use the Cormack-Jolly-Seber (CJS) Model—one of the most tried and tested models for monitoring trends – through prediction – in wildlife populations (Cormack, 1972, 1989; Jolly, 1965; Seber, 1982):[30]

[30] A primary interest when monitoring the health of wildlife species concerns estimating mortality rates within the sampled population, by calculating probabilities of survival from one survey period to the next. "Apparent survival" describes the life cycles of wildlife in terms of their survival, though out-of-sample migration of wildlife often factors into projected rates of non-survival. Apparent survival for cohort participants, by comparison, concerns their survey retention over the observation period (see Table 3). A second consideration concerns estimating the prevalence of species. "Abundance" often refers to the total "head count" for a species or its population density within set physical boundaries (e.g., the number of white-tailed deer in the Kootenay-region woodlands). Abundance of cohort participants equates to city- and provincial-level prevalence of fentanyl use/exposure.

$$t_0 \xrightarrow{\phi_{it_0}} \underbrace{t_1}_{p_{it_1}} \xrightarrow{\phi_{it_1}} \underbrace{t_2}_{p_{it_2}} \xrightarrow{\phi_{it_2}} \underbrace{t_3}_{p_{it_3}} \xrightarrow{\phi_{it_3}} \underbrace{t_4}_{p_{it_4}}$$

where,

- t_0 represents the "precapture period" (i.e., the six months leading up to the first of our four survey periods) and t_1, t_2, t_3, and t_4 each represent the first, second, third, and fourth six-month survey periods of our observation period (c. 2017–2018);[31]
- ϕ_{it} represents the probabilities of participants *i* surviving the six months between survey *t* and survey t_{+1}; and
- p_{it} represents probabilities of survey recapture for participants *i* for each survey period *t* through t_{+1}

At the beginning of each survey period (i.e., post pre-capture), we estimate probabilities of survival and survey recapture from patterns and trends in surveying of individual cohort participants. For each survey period *t*, we have record of everyone who interviewed. And over the length observation period, we know the patterns of participation for cohort participants in terms of both timing and overall interviewing frequencies. Analyzing patterns of survey participation for cohort participants lets us predict probabilities of survival and survey recapture through the trends or patterns emerging through sampling (see the Appendix for more details on the model description—Table A1 presents time-varying survey response items predicting trends in survival and survey recapture).

Findings

Estimates of fentanyl prevalence of use/exposure

Table 7 reports model summaries for our estimates of fentanyl prevalence of use/exposure from the closed model (Table A2 reports odds ratios for the effect of each specific parameter predicting survey recapture). For comparison, we estimate two prediction models—one without trap effects and one with trap effects. By including trap effects, we control for the high survey retention observed over the observation period (c. 2017–2018) and potential behavioural change on the part of cohort participants (i.e., in terms of non-participation) and/or recruiters (i.e., variations in recruitment over the observation period).

[31] For us to estimate prevalence of use over the length of the survey period (c. 2017–2018), we needed to walk backwards six-months from our previous start point to include the six-month survey period before the observation period. As CJS models estimate recapture probabilities for specific survey periods, prevalence cannot be estimated for the first time point in the series—it serves the purpose of the "precapture" period, from which estimates for subsequent survey periods can be calculated. And, important for our research design, the precapture period shares similarities with the four survey periods in the observation period: it comes following the official public health declaration of the opioid crisis (c. April 14th, 2016) and urine screening for drug use began in this six-month survey period.

Table 7 A summary of estimated prevalence of fentanyl use/exposure, 2017-2018

Panel (1): Estimated prevalence of use	(1)	(2)
All survey non-participants	1,239.45	1,348.10
95% confidence intervals	1,227.16 – 1,251.74	1,271.45 – 1,424.74
All survey participants and non-participants (i.e., total)	2,452.45	2,561.10
% survey recapture	49.46	47.36
Panel (2): Estimated prevalence by frequencies of use		
Daily use (% total)	797.83 (32%)	868.70 (34%)
Frequent use (% total)	747.13 (31%)	764.42 (30%)
Infrequent use (% total)	907.49 (37%)	927.98 (36%)
Panel (3): A summary of model parameters		
Cohort studies?[a]	Yes	Yes
Individual effects?[b]	Yes	Yes
Time effects?[b]	Yes	Yes
'Trap effects'?[d]	No	Yes
Panel (4): Goodness-of-fit		
-log likelihood	-2,748.27	-2,714.42
AIC	5,534.53	5,468.84
Hauck-Donner effects?	No	No
% injection drug users	95.47%	95.47%
N	1,213	1,213

Notes: Estimated prevalence of use from closed models.

[a] Reference group = VIDUS

[b] Individual-level effects factored into the prediction equation include participant age, sex/gender, race (white vs. non-white), education (i.e., high school equivalent or not), sexual orientation (i.e., LGBTQ+), age of first drug use, and their drug use behaviour (i.e., self-reported overdose, 'typical' frequency of heroin use, and 'typical' self-reported availability of heroin use over the observation period).

[c] Time effects control for possible trends emerging from the temporal ordering of the survey periods.

[d] Trap effects control for high frequencies of survey recapture observed through patterns in survey participation over the observation period (c. 2017–2018).

For the first model (i.e., Model 1), we estimate probabilities of survey recapture for participants from their cohort membership (i.e., VIDUS, ACCESS, ARYS); socio-demographic characteristics (age, sex/gender, race, education, sexual orientation); age of first drug use (i.e., 'hard' drug use); their use behavior (i.e., self-reported overdose, 'typical' frequency of heroin use, and 'typical' self-reported availability of heroin use over the observation period); and time effects controlling for possible trends emerging from the temporal ordering of the survey periods (e.g., effects of high rates of fatal overdose on survey participation). A second model (i.e., Model 2) includes trap effects on top of the parameters included in the first model.

For both models, the 1,213 cohort participants in the sample represent close to half of the total (estimated) number of cohort participants and non-participants. From the improvements in goodness-of-fit between the first and second models (i.e., lower -log likelihood, lower AIC, etc.),

we take the estimated 2,561 PWUEF (95% confidence intervals = 2,484 – 2,638) for our second model to be the preferred estimate. To be clear, this estimate represents PWEUF who were eligible to participate in one of the three cohort studies during the observation period (c. 2017–2018).

From the overall frequencies and patterns of survey participation for the known cohort participants, this model estimates that cohort recruiting and surveying misses 1,348 PWUEF who would be eligible to participate in one the three cohorts studies. Apart from the goodness-of-fit statistics, the tight confidence interval for this point estimate (1,271 – 1,425) reflects the overall precision of the estimate—provided we don't have gaps in recruiting or survey coverage (see below).

A strong trap effect on survey recapture reinforces our prior descriptive analysis of the patterns of survey participation over the observation (see Table A2). By including the trap effect, we control for unequal patterns in survey participation—placing more relative weight on participants who participate in one or two interviews opposed to those who participate in three or four. Not controlling for the trap effect (i.e., Model 1) results in lower overall estimates (1,239.45; 95% confidence intervals = 1,227.16 – 1,251.74).

Both models control for observed characteristics and self-reported drug use behaviour of cohort participants to help predict variabilities in survey recapture (see Table A2). Belonging to the ACCESS or ARYS cohorts compared to the VIDUS cohort (reference group) doesn't result in significant differences in the probabilities of survey recapture. Age of participants, by comparison, predicts survey recapture. By and large, older participants return for multiple interviews. Although the oldest cohort participants (i.e., 60–69 years old, 70–79 years old, etc.) have lower probabilities of survey recapture, relative to participants younger than 60 years old. All other socio-demographic characteristics, except for race, were non-significant predictors of survey recapture. In comparison to whites, non-whites had lower odds of survey recapture over the observation period.

Although compared to socio-demographic characteristics, participant drug use has strong effects on overall survey participation. Across the reported frequencies of use, participants reporting frequent and infrequent heroin use were more likely to participate in multiple interviews than participants who self-report daily use. Because we condition survey participation on frequencies of use, we can calculate prevalence of use for each of the three frequency of use categories. Each group represents close to one-third of cohort participants and the non-surveyed group. As participants reporting daily use were less likely to interview multiple times over the observation period, the model places more weight on this group compared to the others for computing the overall estimate and confidence intervals.

Finally, participants who reported having overdosed (i.e., non-fatal) over the observation period participated in more interviews compared to participants who had not reported overdosing. This result might reflect the high recruiting and retention of DTES residents and frequenters, who may have higher risks of overdose than residents of Vancouver's other neighbourhoods—based on overall patterns of reporting for LHAs. An implication of this result for our purposes concerns our need to correct for the lower levels of recruiting and overall participation for participants living

and frequenting outside of Vancouver's DTES before making inferences from the city-level to the province-level.

A check on the robustness of the closed model

To test the robustness of our estimates (i.e., whether they replicate given alternate conditions), we first re-estimate our models when changing our sampling criterion. Factoring the survey period occurring six-months before the start of our observation period (i.e., July 1st–December 31st, 2016) into our prediction model doesn't change our overall prevalence of use/exposure estimates. Both our point estimate and confidence intervals (i.e., lower and upper bound estimates) remain similar. We believe the robustness in our estimates when including this survey period reflects two things. First, the overall high retention in survey participation throughout the observation period, irrespective of the spiking numbers of fatal overdose occurring over this timeframe. And second, by controlling for time trends and trap effects, we control for the observed trends and patterns in survey participation between the three cohorts (see Figure 6).

A robustness check from open model

Another means to test the robustness of our estimates involves comparing our prevalence of fentanyl use/exposure estimates from the closed model to other sets of estimates taken from the open model, where we calculate time-specific estimates for each survey period—controlling for similar time and trap effects, while factoring participant survival into the equation.

Table A3 summarizes specifications for each prediction model we estimated from the survey records, with goodness-of-fit statistics (i.e., AIC, ΔAIC).[32] We began by estimating the simplest prediction model possible—setting each survey participant *i* to have equal probabilities of survival and survey recapture over the four waves of the observation period. Although neither equal probabilities of survival nor survey recapture provide us with the realistic conditions required for calculating reliable estimates, it provides the baseline model from which to evaluate other nested models.[33] Adding parameters to the prediction model results in overall improvement (i.e., lower values for AIC). From the baseline model, we observe big changes in goodness-of-fit come when we control for time effects and other effects influencing survival and/or survey capture. For instance, letting probabilities of survival (ϕ) vary with participant response patterns of self-reported overdose in the past-six months and probabilities of survey recapture (p) vary over the observation periods, while controlling for frequencies of heroin use over the past six months, results in big

[32] Akaike information criterion (AIC) provides one means to compare goodness-of-fit between nested models estimated from the same data (i.e., Are the predictions 'better' when including or removing parameters, changing assumptions, etc.?). For interpreting 'better' predictive models, lower values for AIC indicates improvement over previous models. A 'corrected' AIC (AICc) corrects for the number of parameters in the model, which corrects for over-fitting (i.e., changes in AIC between nested models resulting from including more or, in some cases, 'too many' parameters into the model).

[33] Assuming equal chances of survival, we estimate more than 95% of participants survive the length of the observation period ($\hat{\phi}$ = 0.96; 95% confidence intervals = 0.95 – 0.97). And by treating everyone participating with equal chance of recapture over each of the four surveys, we estimate high survey recapture too ($\hat{p}$ = 0.82; 95% confidence intervals = 0.80 – 0.83).

improvements in overall model fit (i.e., CJS Model (3)). And including participant characteristics, self-reported drug use behavior over the past six months, and trap effects from previous survey participation results in further improvements to overall goodness-of-fit (i.e., CJS Model (4)).

From each of the four open models (Table A3), we project the estimated trends in prevalence of fentanyl use/exposure over the observation period (see Figure A1). Here, we observe variation in the trends that seem to reflect the effects we either include and/or don't included in the model. For instance, the model with the constant effects for survival and survey recapture (i.e., CJS Model (1)), does not waver too much over the observation period. By contrast, we project volatile trends when predicting survival from self-reported overdoses and survey recapture from frequencies of heroin use and the effects of time (i.e., CJS Model (3)). And the model controlling for time effects (i.e., CJS Model (2)) and the best fitting model with time-varying controls for both survival and survey recapture (i.e., CJS Model (4)) both project gradual rising trends over the observation period; however, the best fitting model factors in more uncertainties given the long set of controls that we include in this model (see Table A1), which result in wider confidence intervals.

For the two best fitting models (i.e., those models with the lowest AIC scores), we provide further breakdowns of the projected trends by frequencies of use (see Figure A2). By conditioning participant survival on self-reported overdose and their survey recapture on their frequencies of use (i.e., CJS Model (3)), we observe the source of the volatility in the trend—participants reporting frequent heroin use (i.e., 1–3 times per week) fall in number over the observation period, while participants reporting daily or infrequent use (i.e., 1 or fewer days per month). For the best fitting model (i.e., CJS Model (4)), the trends in prevalence by frequencies of use reflect the overall trends—none of the three groups deviate from the gradual upward trend in overall prevalence throughout the observation period.

We further breakdown our projected trends by frequencies of use in tabular format (see Tables A4 and A5). A comparison of the breakdowns from the open models versus the closed models (see Table 7) shows gaps between the two sets of estimates, with lower prevalence estimates from the CJS Models; however, we except lower overall estimates from the CJS model, which factors in probable non-survival and transience from observed patterns of survey participation over the observation period.

As we nonetheless estimate high rates of survival over the observation period from the closed models (> 90%), we prefer the closed model estimates. In our opinion, with the high rates of participant survival, we shouldn't run into major violations with respect to closure (i.e., no drop off in participation, no large number of deaths within the sample, no emigration, etc.).

Are survey participants representative of non-participants?

Are survey participants representative of those not surveyed? To evaluate gaps in recruiting and survey coverage, we plot the population pyramid of the surveyed cohort participants and the non-surveyed group of eligible participants (see Figure 7), who we estimate from the 'closed' trap effects model (see Table 7). Age-group-size for cohort participants shows probable gaps in

reporting for men and women. Bar charts represent observed (i.e., surveyed) and unobserved (i.e., non-surveyed) frequencies for cohort participants and the non-surveyed group of the same age and sex. Higher unobserved frequencies for the non-surveyed compared to the observed frequencies of the known cohort participants reflects potential gaps in recruitment and sampling.

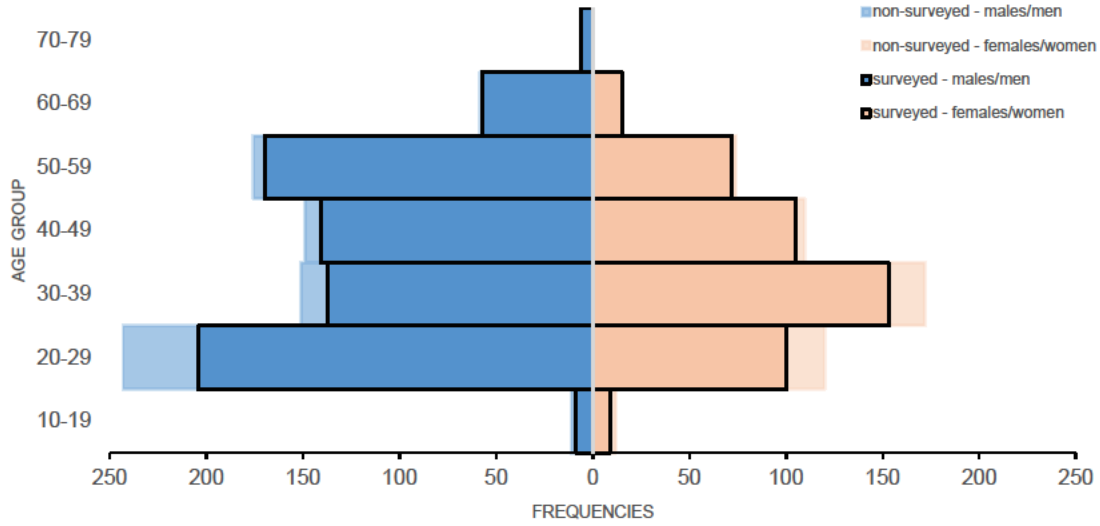

Figure 7 Frequencies of survey respondents and non-respondents, by age group and sex/gender

Notes: Estimated prevalence of use stratified over the population pyramid of cohort participants for men and women. Estimates taken from the 'closed' trap effects model.

For men, cohort participants in their twenties were the most under-recruited when comparing their observed and unobserved frequencies. By comparison, the unobserved frequencies do not correct for the low observed frequencies for cohort participants in their thirties or forties. And for women, we observe more of what we expected to see in terms of observed frequencies. Age peaks in the thirties, with smaller observed frequencies into the forties, fifties, and sixties. From the unobserved frequencies, we see gaps in recruiting and survey coverage for women in their twenties and thirties—perhaps reflecting gaps in recruiting from high risk groups like sex workers.

Overall, we find potential gaps in surveying between cohort participants and non-surveyed groups of men and women who would be eligible to participate in one of the three cohort studies. After correcting for the observed sampling bias, we still observe probable gaps in coverage for men in their thirties and forties that weren't corrected for by the capture-recapture model. Here, the observed gaps in sampling may reflect the high cumulative fatalities for men in their thirties through to their fifties over the observation period (BC Coroners Service, 2020b; Office of the Provincial Health Officer BC, 2019).[34]

[34] According to official reporting, men 30–39 years old represent the highest-risk group for fatal overdose from opioid use in BC (Statistics Canada, 2018).

Making inferences from the cohort studies to the rest of BC

Are our prevalence estimates representative of overall prevalence of use for the city of Vancouver? Are the estimates too high or too low? Is the total sample coverage of the three cohort studies reflective of fentanyl prevalence of use/exposure, in terms of population demographics? Answering questions of this sort help to evaluate whether estimates seem plausible or not. And, furthermore, evaluating the validity and representativeness of prevalence of fentanyl use/exposure estimates helps us make provincial-level inferences from our city-level estimates. Before proceeding further with our results, we briefly describe other sources of data we used to help make calculate provincial-level expenditures.

Auxiliary data

To supplement the surveys from the three cohort studies, we use counts of fatal overdoses from fentanyl exposure reported by BC Coroners Service and the University of Victoria's Canadian Institute of Substance Use Research (CISUR). Annually, CISUR and BC Coroners Service report counts of fatal overdose for each region of the city (i.e., LHAs) and the province (i.e., health authorities), respectively.[35] Apparent deaths from overdose were required to correct for gaps in city-level survey coverage and for making inferences from the city of Vancouver (i.e., BCCSU's cohort studies) to the rest of the province (for trends in fatal overdose from fentanyl, refer back to Figure 1); however, counts of fatal overdose from fentanyl exposure were not reported for LHAs, so we use counts of fatal overdose from opioid use instead. We downloaded counts of fatal overdoses for LHAs in the Vancouver Health Delivery Area (HDA) from CISUR's online database (see http://aodtool.cisur.uvic.ca/aod/tool.php). For 2017, the most recent year of reporting, Vancouver – Centre North LHA reports 45% of fatal overdoses of opioid use in the Vancouver HDA.

The size of the fentanyl market in Vancouver

From our preferred model, we estimate 2,561 PWUEF (95% confidence interval = 2,484–2,638), who were eligible to participate in one the three cohort studies over the observation period (c. 2017–2018). Because of the exclusion/inclusion criteria for the studies, gaps in recruitment, and non-participation, we do not have total sample coverage of everyone using or exposed to fentanyl for city of Vancouver. Although we identified multiple ways in which the cohort studies under- or over-recruit PWUDs (see Table 4), we correct for the over-sampling of residents from the DTES in efforts to improve our city-wide coverage. We believe this source of sampling bias to spillover or effect multiple other sources of sampling bias (i.e., recruitment), so it's perhaps the most pertinent source of sampling error.

We make corrections for over-sampling using multiplier methods established in previous work estimating the prevalence of opioid use in BC and Canada, generally (e.g., see Jacka et al., 2020).

[35] BC Coroners Service reports can be downloaded from: https://www2.gov.bc.ca/gov/content/life-events/death/coroners-service/news-and-updates/reports

We correct for over-recruiting of DTES residents by inflating prevalence of fentanyl use/exposure by the ratio of fatal overdoses reported by VCN, which services Vancouver's DTES, versus the city totals reported by Vancouver LHAs, combined. Table 8 lists counts of fatal overdose from opioid use for Vancouver's LHA. For 2017, 45% of fatal overdoses from opioid use in the City of Vancouver were reported by VCN. Assuming fatal overdoses represent out-of-sample PWUEF who would otherwise be eligible to participate in one of BCCSU's three cohort studies, we take proportion of fatal overdoses reported by VCN to be our city-level multiplier (i.e., 0.45). Assumptions for the multiplier method hold if there is equal risk of fatal overdoses between LHAs. Although we can't test for possible violations without reliable reporting on participant frequencies of use and overdose incidents, we believe the results from the multiplier correction to be closer to the overall fentanyl prevalence of use/exposure for the City of Vancouver than our estimate without further correction.[36]

Table 8 Fatal overdoses from opioid use, reported by Local Health Area (Vancouver), 2017

Local Health Area	Fatal overdose count	% Local Health Areas
Vancouver – City Centre	26	17
Vancouver – Centre North	68	45
Vancouver – Northeast	17	11
Vancouver – Westside	6	4
Vancouver – Midtown	11	7
Vancouver – South	23	15

Note: Fatal overdose counts from Canadian Institute of Substance Use Research, University of Victoria.

We make corrections for two possible scenarios. In the first scenario, we keep the proportion of DTES residents from both our sample and non-surveyed group (i.e., eligible, but didn't participate), from our prevalence of use estimates (i.e., 2,484 – 2,561 – 2,638). Table 9 shows the proportions of cohort participants from our sample reporting DTES residence. Across cohorts, two-thirds of our sample report living in Vancouver's DTES (68%), though we report variations by cohort: more VIDUS (76%) and ACCESS (70%) participants report living in the DTES, compared to ARYS participants (43%). Applying the multiplier to counts of DTES residents yields 3,754 – 3,987 PWUEF for the City of Vancouver.

Table 9 DTES residence by cohort, for survey participants and non-survey participants

	COHORT		
	VIDUS	ACCESS	ARYS
DTES residents (%)	1,069 (76)	556 (70)	266 (43)
non-residents (%)	338 (24)	243 (30)	353 (57)
$\hat{n}$	1,407	799	619

Note: All means calculated from pooled survey responses, therefore reflecting the 'typical' response over the observation period. For the non-surveyed group, we estimate their residence from the 'closed' model with trap effects.

[36] For example, supervised injection sites and emergency medical services brought on by the opioid crisis ought to lower risks of fatal overdose within VCN. At the same time, other factors may increase risks of fatal overdose for DTES residents, including higher frequencies of use (i.e., exposure to risk).

For the second scenario, we keep our prevalence of fentanyl use/exposure estimates intact, irrespective of DTES residence. Here, we take our estimated range to represent DTES residents and/or participants frequenting the DTES. Although one-third of participants report not residing within Vancouver's DTES, the strong focus of recruiting from DTES harm-reduction services (e.g., safe injection sites, needle exchanges, etc.) suggests non-residents frequent the DTES to score opioids or other substances and be closer to local health and social services. Applying the 45% fatal overdose multiplier once more, we calculate 5,520 – 5,862 PWUEF for the City of Vancouver—much higher than the range calculated from the first scenario.

How many PWUEF in BC?

As for the question of how many PWUEF for the province, we use a similar multiplier strategy using fatal overdose data to guide us in making the proper inference. After weighing our options, we found fatal overdoses to represent the most reliable metric of the size of Vancouver's fentanyl market relative to the rest of the province. The City of Vancouver captures 14% of the total population of BC, though fentanyl prevalence of use/exposure might be higher in Vancouver than BC's other cities or regions—the concentration of opioid use in the DTES itself might result in higher per capita prevalence of use for Vancouver, compared to other cities or regions. Table 10 shows that fatal overdoses reported by Vancouver Coastal Health Authority represent 25% of the provincial total in 2017–2018.

Table 8 Fentanyl overdoses reported by Health Service Delivery Areas, 2017–2018

Health Authority	Health Service Delivery Area	Fatal overdose count	% Health Authority Total	% provincial total overdose
Interior	East Kootenay	9	0	2
	Kootenay Boundary	27	1	7
	Okanagan	244	8	60
	Thompson Cariboo Shuswap	126	5	31
Fraser	Fraser East	162	6	19
	Fraser North	240	10	29
	Fraser South	430	17	52
Vancouver Coastal	Richmond	36	1	5
	Vancouver	**646**	**25**	**86**
	North Shore/Coast Garibaldi	69	3	9
Island Health	South Vancouver Island	209	9	48
	Central Vancouver Island	166	6	38
	North Vancouver Island	58	2	13
Northern	Northwest	18	1	13
	Northern Interior	85	4	61
	Northeast	36	2	26

Note: Data from British Columbia Coroners Service. (2019). Fentanyl-detected illicit drug overdose deaths in BC, January 1, 2009—March 31, 2019. Retrieved from https://www2.gov.bc.ca/assets/gov/birth-adoption-death-marriage-and-divorce/deaths/coroners-service/statistical/fentanyl-detected-overdose.pdf

Applying the regional fatal overdose multiplier (i.e., 0.25) to our city-level estimates (see Table 11), we calculate 15,014 – 15,948 PWUEF throughout the entire province for the first scenario (i.e., 3,754/0.25 – 3,987/0.25) and 22,080 – 23,448 for the second scenario (i.e., 5,520/0.25 – 5,862/0.25); therefore, we can say, with some confidence, ~15,000 – 23,500 PWUEF – minimum – were living in BC throughout the observation period (c. 2017–2018). We deem this range to be conservative, representing the floor estimate for fentanyl prevalence of use/exposure within the province.

Table 9 Two scenarios for inferences from survey estimates to the city of Vancouver and BC, 2017–2018

	LOWER BOUND	MIDDLE	UPPER BOUND
SCENARIO 1			
Capture-Recapture Estimate	2,484	2,561	2,638
68% DTES	1,689	1,742	1,794
Vancouver Estimate (DTES = 45% of YVR)	3,754	3,870	3,987
BC Estimate (YVR = 25% of BC)	15,014	15,480	15,948
SCENARIO 2			
100% DTES	2,484	2,561	2,638
Vancouver Estimate (DTES = 45% of YVR)	5,520	5,691	5,862
BC Estimate (YVR = 25% of BC)	22,080	22,764	23,448

Note:

Its hard to compare our estimates with previous estimates of PWUD or PWID opioids, stimulants, or other drugs, generally, rather than for fentanyl prevalence of use, specifically. Janjua et al. (2018) and Jacka et al. (2020) estimate ~40,000 PWID living in BC. McInnes et al. (2009) estimate 13,500 PWIDs for Vancouver. And Xu et al. (2014) estimate ~2,000 – 3,000 PWIDs for Vancouver Island. All of these estimates exceed our estimates of PWUEF (c. 2017-2018). As our estimate is specific to fentanyl, we expect it to be lower compared to previous estimates.

The retail expenditures of PWUEF in BC

We now estimate total spending by PWUEF.[37] As much as 90% of opioids sold in BC are estimated to contain fentanyl. In the DTES, most people buy "down" which is a dull white powder that used to be mostly diacetylmorphine (with a lot of filler), but now contains fentanyl (with more filler); therefore, weuse the self-reporting on heroin use to provide estimates of expenditures in the fentanyl markets.

A summary of our method for estimating expenditures on fentanyl

To calculate total expenditures on fentanyl, we use similar methods established in prior research on opioid use (Midgette et al., 2019) and other drugs (Wilkins & Sweetsur, 2007). To do so, we take the following steps:

[37] Initially we had foreseen estimating this quantity by estimating the fentanyl consumption; however, "down" represents much of the fentanyl consumed over the past couple years (c. 2017–2018) and we didn't have systematic testing of heroin or opioids for purity over the observation period to help us make expenditure calculations.

1. We start from our provincial-level estimates of PWUEF (c. 2017–2018). To simplify, we use the point estimates for each scenario: 15,480 PWUEF for the first scenario and 22,764 PWUEF for the second scenario;
2. We breakdown PWUEF by frequencies of use: 1) daily; 2) frequent (1–3 times per week); 3) infrequent users (less than once per week). We take proportions of daily use (34%), frequent use (30%), and infrequent use (36%) estimated from our capture-recapture model (see summary Table 11); and
3. Assign expenditures (per day of use) by frequencies of use, multiply by days of use per month for monthly expenditures, and then multiply by twelve (months) to project total expenditures per year.

As we can only determine the proportion of participants who spent more or less than $50 per day on drug use, generally (i.e., spending wasn't specific to heroin or fentanyl), from the survey records, we can't estimate total expenditures from participant self-reporting; therefore, we use the monthly expenditures on heroin use reported by Midgette et al. (2019). Average spending per month by frequencies of use (Table 12).

Table 10 Average monthly and yearly heroin expenditures and consumption

	HEROIN USE, PAST-MONTH		
Averages, estimated	Daily (i.e., 21+ days)	Frequent (i.e., 11–20 days)	Infrequent (i.e., 4–10 days)
Expenditures ($USD), by month[a]	$1,880.00	$847.00	$411.00
Expenditures ($CAD), by month[b]	$1,979.20	$891.20	$432.00
Expenditures ($USD), by year	$22,560.00	$10,164.00	$4,932.00
Expenditures ($CAD), by year	$23,747.20	$10,699.20	$5,192.00
Consumption (grams), by year[c]	12.37g	5.57g	2.70g
Consumption (grams), by year[c]	148.42g	66.87g	32.45g

Note: Heroin prices weren't adjusted by purity.
[a] Average monthly expenditures taken from Midgette et al. (2019), representing heroin expenditures by price per gram (c. 2016)
[b] Average monthly expenditures estimated from price per gram for fentanyl and heroin reported by Vancouver Police Department (c. 2017–2019)
[c] Estimated from the street price of USD$152 per gram, reported by the United Nations Office on Drugs and Crime for the United States (c. 2016): https://dataunodc.un.org/drugs/heroin_and_cocaine_prices_in_eu_and_usa

Although reporting on heroin use from Midgette et al. (2019) comes from ADAM participants, who might not be representative of BCCSU cohort participants, monthly spending on heroin use reported by ADAM participants is similar to our own back-of-the-envelope estimates—calculated by multiplying the numbers of daily, frequent, and infrequent fentanyl or "down" users (see Table 11) by street prices of fentanyl and heroin from VPD. Across the US, heroin cost CAD$152 per gram (c. 2016) (Midgette et al., 2019). By comparison, VPD reports CAD$160 per gram for fentanyl and heroin (c. 2017–2019) (see Figure 8). Furthermore, the estimated twelve grams of monthly consumption for daily users equate to the 0.4 grams of heroin used per day for daily users in Stockwell et al.'s (2010) "The price of getting drunk or high in BC" study. Also, the daily expenditures of USD$60 – USD$65 used by Midgette et al. (2019) match what 0.4 grams of

"down" costs on the streets of Vancouver (0.1g = CAD$20 vs. 0.5g = CAD$70 – CAD$80) (see Table A6).

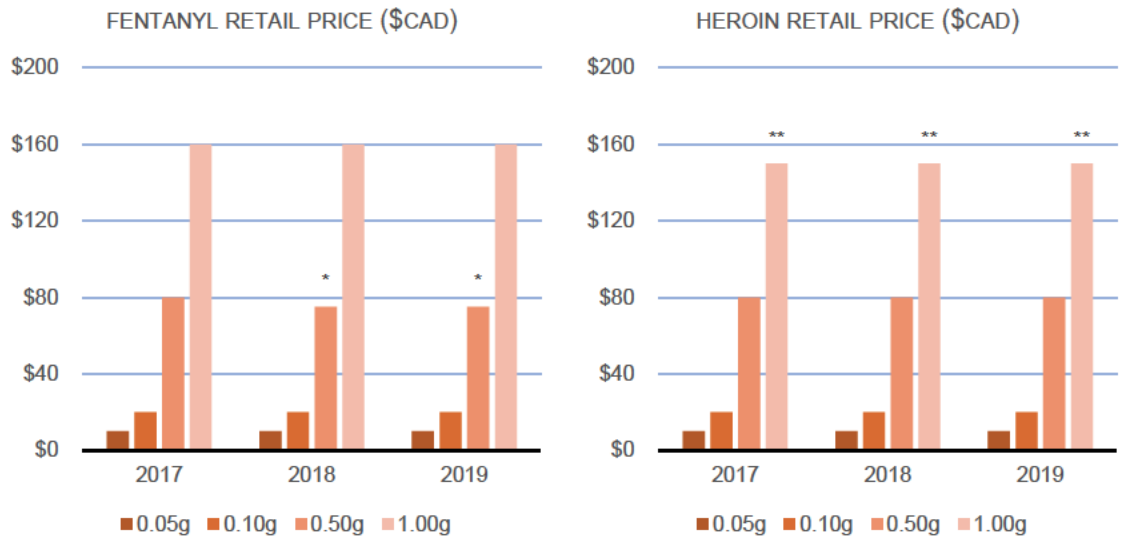

Figure 8 Fentanyl and heroin retail prices ($CAD) by unit (g), reported by Vancouver Police Department, 2017–2019

Note. Fentanyl and heroin street prices provided by Vancouver Police Department (VPD). Because of variation in street-level purity, the lethal dose range for fentanyl use/exposure ≅ 0.05–2.00mg (variation in street-level purities).

* Average of the listed price range ($70–$80)

** Average of the listed price range ($140–$160)

As for fentanyl, there is only a very small amount in each dose. Pardo et al. (2019) note that each half point dose in British Columbia contains approximately 1 mg of pure fentanyl. Consequently, eight half point doses result in an average daily consumption of 4 mg of pure fentanyl for daily users. This is consistent with a calculation in Pardo et al. (2019). The authors estimate that the users in Estonia inject an average of four times per day a dose of 2 mg of pure fentanyl to arrive at an annual consumption of 15 kg of pure fentanyl.

We used these purchasing patterns to estimate the monthly and yearly retail expenditures of PWUEF in BC. Table 13 presents our prevalence of fentanyl use/exposure estimated earlier from the first and second scenarios, broken down by self-reported frequencies of use.

Using the street price per gram for fentanyl and heroin reported by the Vancouver Police Department (CAD$160 per gram), monthly consumption for daily use costs ~CAD$2,000, while frequent use (i.e., 1–3 days per week) costs ~CAD$900 (purchasing six grams per month) and infrequent use (i.e., less than once per week) costs ~CAD$400 (equivalent to purchasing three grams per month). We multiplied by twelve to project monthly expenditures into annal expenditures.

Table 11 Average monthly and yearly fentanyl expenditures, BC

		HEROIN USE, PAST-MONTH			
		Daily	Frequent	Infrequent	All total
SCENARIO 1	PWUEF, BC (%)	5,263	4,644	5,573	15,480
		(34%)	(30%)	(36%)	
	Expenditures (%)	$124,980,461	$49,686,156	$28,935,016	$203,601,633
		(61%)	(24%)	(14%)	
SCENARIO 2	PWUEF, BC (%)	7,740	6,829	8,195	22,764
		(34%)	(30%)	(36%)s	
	Expenditures (%)	$183,801,780	$73,063,471	$42,548,440	$299,413,691
		(61%)	(24%)	(14%)	

Note: Average monthly and yearly fentanyl expenditures estimated using the street price of CAD$160 per gram of fentanyl/heroin, reported by the Vancouver Police Department.

Because we already had estimated the annual prevalence of PWUEF by frequencies of use, we could then estimate the retail expenditures for each daily, frequent, and infrequent use. Multiplying expenditures by the prevalence estimates from the first and second scenarios gives us a range of retail expenditures of CAD$200 – CAD$300 Million for PWUEF in BC. We estimate that the 34% of daily users we found in the cohort studies account for 61% of the total amount of expenditures. Infrequent users, despite being the largest group of PWUEF (36%), only account for 14% of expenditures.

No prior estimates exist to help situate this range of retail expenditures. It is worth noting that 1) the starting point for these estimates – the prevalence of PWUEF – errs on the conservative side; and 2) not every dollar translates into profits for dealers and, by extension, into dollars that need to be laundered before being spent. Analysis of supply-side economics – with respect to the costs incurred from fentanyl production through trafficking – would be required to grasp the profit margins from sales revenue, which would more so reflect the potential contribution of fentanyl to money laundering within the province.

An important assumption of these estimates is that the proportions for the type of PWUEF from the cohorts (i.e., daily, frequent, infrequent) are a good proxy for what they are for the full population. Should the proportion of daily users in the cohorts be too low, for instance, compared to the proportion of daily users in the province as a whole, the total retail expenditures would be under-estimated (and vice-versa).

We did a quick sensitivity analysis to understand the impact of having different proportions of each type of PWUEF. Instead of the cohort breakdown of 34% (daily), 30% (frequent), and 36% (infrequent) we had, we used one scenario where daily users take more importance (50%) and infrequent users less (20%), with the middle category of frequent users staying at 30%. Using prevalence numbers from the second scenario, this would bring the retail expenditure estimates to close to CAD$367 Million – an additional CAD$67 Million from our current estimates of CAD$300 Million. If, instead, we reverse this scenario and the proportion of daily users is decreased to 20%, and infrequent users to 50%, then the scenario two estimates are decreased from CAD$300 to CAD$240 Million.

Conclusions

The unprecedented death toll of the current opioids crisis has been driven by the potency of fentanyl and its contamination of heroin and other opioids in BC. Fentanyl and fentanyl-adulterated substances have taken over 90% of the opioid market in BC, the hardest hit Canadian province in the opioid crisis (Canadian Centre on Substance Use and Addiction, 2020; Pardo et al., 2019). Apart from the public health consequences of high fentanyl exposure, this change in the opioid market must have shifted revenue flows from heroin and opioid trafficking. Although without plausible estimates of total expenditures, the potential revenue flows remain speculative.

This report represents the first provincial-level estimates of the fentanyl drug market, in terms of total expenditures on fentanyl itself and other drugs laced with fentanyl. From 1,213 participants over three cohort studies who has used fentanyl and/or were exposed to it, we estimated 2,561 PWUEF were eligible to participate in one of the three studies (c. 2017–2018); however, because of gaps in survey coverage, we deemed our estimates to be much too conservative. To correct for probable gaps in sampling, we inflate our original estimates from the closed capture-recapture model using local and provincial reporting on fatal overdose from fentanyl exposure. By first inflating our estimates from fatal overdoses reported by Vancouver – Centre North, which reports fatal overdoses occurring in Vancouver's DTES, we improve city-wide coverage over the three cohort studies. To project our city-level estimates to provincial-level prevalence of use/exposure, we further inflate our estimates by fatal overdose reporting from Vancouver Coastal Health Authority relative to other regional health authorities throughout the province. A conservative scenario – where we place the weight on DTES residents – projects 15,014–15,948 PWUEF throughout the province, while the other scenario projects 22,080–23,448 PWUEF.

After calculating expenses by frequencies of use, we estimated total provincial retail expenditures to range CAD$200 – CAD$300 Million. Although BCCSU's cohort studies were much better suited to providing estimates for prevalence of fentanyl use/exposure than expenditures, we nonetheless provide plausible estimates of total expenditures ranging in the low hundreds of millions. After correcting for inflation, our estimates would represent 2–3% of the world's opium/heroin market shares (UNODC, 2010). And while revenues do not equal profits, our report provides some sense of the potential contribution of this revenue source to money laundering within the province. Further study of trends in local seizures – both in terms of size and frequency – and prices will provide more clarity to the proceeds of crime from fentanyl and opioid consumption.

Recommendations

In our opinion, our estimates of total expenditures and the potential of revenues for money laundering can be improved in various ways:

1. We spent considerable efforts trying to understand our sample with respect to the sampling criteria of BCCSU's cohort studies. In particular, we identified potential sources of sampling bias, which helped us make proper inferences. Although the cohort studies

provide rich survey data, other sampling techniques like referrals (i.e., between friends) or needle sharing facilitate the use of respondent-driven sampling, providing other means for evaluating the representativeness of cohort studies with respect to prevalence of fentanyl use and/exposure (e.g., see Caulkins et al., 2015).

2. Questions pertaining to consumption and spending on drug use need to be integrated into cohort studies and general population surveys on drug use, specifically. Asking questions specific to consumption and spending on opioid use are required for calculating total revenues, and for evaluating the internal and external validity of total expenditures self-reported by PWUD.
3. This report contributes insight to the size of the demand-side of the fentanyl market, both in terms of overall prevalence and total expenditures; however, the supply-side remains obscure. Interviews with traffickers and suppliers in the upper-levels of the opioid market could provide insights into the stage in the production chain when fentanyl is mixed with herion, the profit share from total sales revenues, and money laundering strategies. Interviews might have to take place with inmates (see e.g., Bouchard & Ouellet, 2011; Reuter et al., 1990), but interviews have taken place with traffickers who were recruited outside of prison settings (e.g., see Sandberg & Copes, 2013).
4. Estimates of money laundering from fentanyl revenues implicates traffickers, not persons who strictly use fentanyl. As mentioned, each dollar spent doesn't translate into profits. We need estimates of profits of traffickers and their spending patterns, generally—not simply transactions facilitating money laundering, specifically.
5. We need to better understand geographic variations in opioid consumption and risks of overdose, specifically variations in urban and rural regions of the province. A significant portion of the fatal and non-fatal overdoses reported within in the province occur within Vancouver's DTES. For many of the inferences we make in this report, we assume equal risk of fatal overdoses for DTES residents and PWUD throughout province, generally. Yet, opioid consumption and risks of overdose within the DTES may not be consistent with consumption and risks within other cities or regions of the province. Access to harm-reduction services and emergency health care in urban settings, might lower risks of opioid use, while opioid consumption – in terms of frequencies of use and method of consumption – might be quite different between urban and rural settings.
6. Analysis of fentanyl and heroin seizures reported by the Canadian Drug Analysis Services ought to inform what's being sold at the retail-level and how that may differ from what's sold at the wholesale-level (Government of Canada, 2020). We did not have test samples of fentanyl or "down" sold in Vancouver's DTES or elsewhere in BC, required for testing purity and estimating quantities of fentanyl flowing onto the streets.
7. As previous studies show, personal interaction and the consequent flow of information help PWUD identify and avoid potentially toxic or contaminated sources of opioids and stimulants. Future study should focus on better understanding social networks in places like the DTES. A network perspective would help map high-risk clusters or groups with high occurrence of overdose (possibly from consuming contaminated opioids), which would inform targeted overdose prevention efforts (Bouchard et al., 2018).

APPENDIX

An 'open' model of survey recapture—detailed model summary

Estimates of trends in prevalence of fentanyl use over the two-year observation period (c. 2017–2018) were projected by predicting the probabilities of survival (ϕ_{it}) and survey recapture (p_{it}) for cohort participants *i* over each survey period *t*. To do so, we use the Cormack-Jolly-Seber (CJS) Model—one of the most tried and tested models for monitoring trends – through prediction – in wildlife populations (Cormack, 1972, 1989; Jolly, 1965; Seber, 1982):

$$t_0 \xrightarrow{\phi_{it_0}} \underbrace{t_1}_{\mathrm{p}_{it_1}} \xrightarrow{\phi_{it_1}} \underbrace{t_2}_{\mathrm{p}_{it_2}} \xrightarrow{\phi_{it_2}} \underbrace{t_3}_{\mathrm{p}_{it_3}} \xrightarrow{\phi_{it_3}} \underbrace{t_4}_{\mathrm{p}_{it_4}}$$

where,

- t_0 represents the "precapture period" (i.e., the six months leading up to the first of our four survey periods) and t_1, t_2, t_3, and t_4 each represent the first, second, third, and fourth six-month survey periods of our observation period (c. 2017–2018);
- ϕ_{it} represents the probabilities of participants *i* surviving the six months between survey *t* and survey t_{+1}; and
- p_{it} represents probabilities of survey recapture for participants *i* for each survey period *t* through t_{+1}

At the beginning of each survey period (i.e., post pre-capture), we estimate probabilities of survival and survey recapture from observed patterns of survey participation for cohort participants *i*. Apparent survival for cohort participants concerns their survey retention over the observation period (see Table 3). We predict survival throughout the four survey periods using the standard survival function:

$$\phi_{it} = \log\left(\frac{\phi_{it}}{1-\phi_{it}}\right) = \alpha_i + \alpha_{it} + \alpha_t$$

Equation A1 A survival function for cohorts participants

where,

- α_i represents the unobserved effects influencing survival for participants *i*;[38]
- α_{it} represents the effects of self-reported overdose for participants *i* in survey period *t* on their survival from *t* through t_{+1}; and

[38] To be clear, unobserved effects includes factors influencing participant survival, but not observed through surveying the three cohorts or perhaps other not-so-obvious factors influencing survival that we do not know of.

- α_t represents time effects for each survey period that control for trends in survival probabilities over the observation period (i.e., survey non-retention, trends in fatal overdose, etc.)

Apparent deaths from overdose, first and foremost, would result in unequal rates of survival and recapture for cohort participants.[39] Because we do not know if cohort participant *i* dies or not over the observation period, we need to estimate their chances of survival from their self-reported overdose histories, while controlling for trends in survey retention (time effects). Although conditioning survival on self-reported overdose might seem problematic from the standpoint that we condition on the reporting of survivors (i.e., those who died from overdose couldn't later report it), recall periodic survival's estimated from survey period *t* through survey period t_{+1}; therefore, having overdosed in the previous six months helps predicts survival over the next six months.

And, to estimate survey recapture, we once more use multinomial logistic regression:

$$p_{it} = \log\left(\frac{p_{it}}{1 - p_{it}}\right) = \beta_i + \beta_{it} + \beta_t$$

Equation A2 A model of survey recapture for cohort participants

where,

- β_i represents the observed characteristics of participants *i* (e.g., sex/gender, race, etc.);
- β_{it} represents survey responses of participants *i* in survey period *t*, with respect to their past six-month housing situation, residence within or outside the DTES, participation in treatment for opioid use, official sanctions from the criminal justice system, spending on drug use, their frequency of heroin use; and the typical time it takes them to score heroin on the street; and
- β_t represents time effects for each survey period that control for trends in survey recapture over the observation period (i.e., survey non-retention)

As with the closed model, controlling for observed characteristics of participants and their self-reporting on interview response items corrects for unequal probabilities in of survey recapture. Although in contrast to the closed model, many of the effects included in the CJS models represent participant self-reporting on response items for each specific survey period (i.e., time-varying effects). For instance, we're able to evaluate the effects of jail spells or community supervision on survey recapture and whether concurrent participation in treatments for opioid use increases or reduces overall levels of participation.

[39] We tried estimating more elaborate survival functions, by including the observed characteristics of participants and other response items for each six month survey period; however, the estimated parameters from the more elaborate survival function were too correlated with the parameters estimated for predicting survey recapture. We therefore opted to keep the survival function simple, since our main interest concerns estimating survey recapture.

Table A1 presents descriptive statistics (sample means) for cohort participants self-reporting fentanyl/heroin use and/or screening for fentanyl exposure for each survey period of the observation period. All time-invariant effects (i.e., age group, sex/gender, race, etc.) represent pooled sample means (i.e., mean values reported by participants taking part in one or more surveys over the entire observation period). As we use the exact same sample, the demographics of our sample throughout the survey period haven't changed—consistent with previous reporting (see Table 6). In comparison, time-variant effects (i.e., survey response items) can change over time depending on participant response. Although with the exception of self-reported overdose (≅ 16–24%), other survey response items that might effect survival or survey recapture remain stable over the observation period (c. 2017–2018). For each survey period, most cohort participants report living in Vancouver's DTES (≅ 61–64%), having housing (i.e., homelessness ≅ 24–25%), attending treatment for opioid use (≅ 50–54%), and no jail spells or community supervision (≅ 14–17%).

As for prevalence of use, most report using or screen for fentanyl (≅ 51–63%) and the overwhelming majority self-report heroin use or screen for morphine (≅ 79–82%). High prevalence of use reflects the two large groups of daily users (≅ 36–40%) and infrequent users (≅ 44–45%) reporting throughout the observation period; however, reporting on frequencies of heroin use indicates most cohort participants report periods of higher or lower frequencies of use over the observation period (e.g., participants may report using daily in survey period t, but report using less often in survey period t_{+1}). Availability of heroin – the time it takes for participants to score heroin on the street – reinforces the high prevalence and frequencies of use—most of the sample report being able to buy heroin within 10 minutes (≅ 78–85%). And the majority of cohort participants report spending less than $50 each day on drug use (≅ 54–58%), which remains stable over the two-year observation period.

Table A1 Time-varying effects (self-report) for PWUEF, 2017–2018

	SURVEY PERIOD			
	(1) Jan-Jun '17	(2) Jul-Dec '17	(3) Jan-Jun '18	(4) Jul-Dec '18
Cohort studies[a] (%)				
VIDUS	48.81	48.62	48.13	49.41
ACCESS	28.94	28.14	26.95	27.01
ARYS	22.25	23.23	24.92	23.58
Age groups[a] (%)				
10-19	1.59	1.32	1.39	1.98
20-29	20.20	21.56	22.78	23.32
30-39	22.13	22.40	23.32	24.64
40-49	23.16	23.83	22.57	22.13
50-59	25.43	23.83	23.21	21.08
60-69	6.92	6.59	6.20	6.19
70-79	0.57	0.48	0.53	0.66
Sex/gender[a] (%)				
Female/women	35.98	35.09	38.61	37.42
Male/man	61.29	61.68	58.40	59.42
Trans/non-binary	2.72	3.23	2.99	3.16
White[a] (%)	48.24	47.07	47.91	47.30
High school completion?[a] (%)	44.27	45.87	47.17	46.38
LGBTQ+[a] (%)	16.00	17.13	18.18	18.18
Age, first drug use[a]				
>12	3.97	3.83	3.10	3.56
12-17	44.27	43.23	44.60	44.40
18-24	27.70	30.18	28.66	26.88
25-29	10.44	9.94	10.05	11.33
30-34	6.70	5.63	7.27	6.98
35-39	4.20	4.43	3.85	3.82
40-44	1.02	1.20	1.07	1.32
45-49	0.68	0.84	0.43	0.66
50+	1.02	0.72	0.96	1.05
Has overdosed?[b] (%)	24.29	24.31	18.82	16.34
DTES residence?[b] (%)	64.47	63.23	61.50	60.74
Homelessness?[b] (%)	24.75	25.03	24.28	25.17
Treatment for opioid use?[b] (%)	54.37	52.70	51.34	49.54
Jail or other sanctions?[b] (%)	16.91	16.41	15.72	14.36
Fentanyl prevalence of use[b] (%)	51.08	58.92	62.67	58.24
Heroin prevalence of use[b] (%)	81.73	80.72	80.54	78.92
Frequencies of heroin use[b] (%)				
Daily	36.66	37.96	38.50	40.18
Frequent	18.05	16.41	17.33	14.49
Infrequent	45.29	45.63	44.17	45.32
< $50/day spent on drugs (%)	58.23	55.33	54.33	56.39
Availability of heroin[b] (%)				
score ≤10 minutes	84.56	85.03	79.89	77.73
score ≤90 minutes	8.51	6.59	9.84	8.70
score ≥24 hours	6.92	8.38	10.27	13.57
N	881	835	935	759

[a] All observed characteristics for cohort participants don't change over the observation period (i.e., time-invariant).
[b] All survey self-response items might change over the observation period given changes in participant behaviour (i.e., time-variant).

Table A2 Odds ratios for survey re-capture of PWUEF in Vancouver, 2017–2018

	Model (1)		Model (2)	
	OR	95% CIs	OR	95% CIs
Surveys				
(1) Jan 2017–Jun 2017	0.30**	0.14 – 0.63	0.24***	0.10 – 0.55
(2) Jul 2017–Dec 2017	0.25***	0.12 – 0.52	0.09***	0.04 – 0.23
(3) Jan 2018–Jun 2018	0.38*	0.18 – 0.80	0.12***	0.05 – 0.30
(4) Jul 2018–Dec 2018	0.18***	0.09 – 0.39	0.05***	0.02 – 0.13
Cohort studies[a]				
ACCESS	1.06	0.89 – 1.26	1.04	0.86 – 1.25
ARYS	1.12	0.88 – 1.43	1.06	0.81 – 1.38
Controls				
Age	2.12***	1.47 – 3.06	2.10***	1.41 – 3.13
Age squared	0.94**	0.90 – 0.98	0.94**	0.89 – 0.99
Gender[b]				
Woman	1.00	0.86 – 1.16	1.03	0.72 – 1.79
Trans/non-binary	1.09	0.72 – 1.66	1.03	0.87 – 1.20
White?	0.82**	0.72 – 0.95	0.80**	0.69 – 0.93
High school completion?	0.93	0.81 – 1.07	0.98	0.84 – 1.14
LGBTQ+?	1.01	0.84 – 1.21	1.10	0.90 – 1.35
Drug use behaviour				
Age of first drug use	0.95*	0.90 – 1.00	0.95	0.90 – 1.01
Has overdosed?	1.39***	1.21 – 1.61	1.39***	1.19 – 1.62
Heroin frequency of use[c]				
Frequent use	2.08***	1.77 – 2.45	2.08***	1.74 – 2.49
Infrequent use	2.06***	1.72 – 2.46	2.04***	1.68 – 2.49
Availability of heroin[d]				
Score ≤ 90 minutes	1.44**	1.20 – 1.72	1.48***	1.22 – 1.80
Score ≥ 24 hours	0.45***	0.32 – 0.65	0.45***	0.31 – 0.67
Trap effect	—		3.58***	2.39 – 5.37
AIC	5,534.53		5,468.84	
% injection drug users	95.47		95.47	
$\hat{\rho}$	49.46%		47.36%	
$\widehat{N}$	1,239.45		1,348.10	
95% confidence intervals	1,227.16 – 1,251.74		1,271.45 – 1,424.74	
N	1,213		1,213	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a] reference group = VIDUS

[b] reference group = Men

[c] reference group = Daily use

[d] reference group = Score ≤ 10 minutes.

Table A3 All nested open model comparisons, 2017–2018

Model	Assumptions	AIC[a]	ΔAIC[b]
$\phi\ (.)\ p(.)$ - CJS Model (1)	All participants have equal probabilities of survival (*ø*) and survey recapture (*p*) over the four survey periods.	4,257.57	—
$\phi\ (t)\ p(t)$ - CJS Model (2)	All probabilities of survival (*ø*) and survey recapture (*p*) may change over the four survey periods.	4,165.10	92.47
$\phi\ (OD_{it})\ p(t + FQ_{it})$ - CJS Model (3)	All probabilities of survival (*ø*) may vary with participants response patterns of self-reported overdose in the past-six months; probabilities of survey recapture (*p*) may change over the four survey periods and with survey responses to frequencies of heroin use over the past-six months.	3,717.14	540.43
$\phi\ (t + OD_{it})\ p(t + TD_{it_{-1}})$ - CJS Model (4)	All probabilities of survival (*ø*) and survey recapture (*p*) may change over the four survey periods; survival probabilities (*ø*) may vary with participants response patterns of self-reported overdose in the past-six months; probabilities of survey recapture (*p*) may vary with individual effects and survey responses to drug use behavior over the past-six months; probabilities of survey recapture (*p*) subject to "trap effects" from previous survey participation.	3,469.91	787.66

[a] Akaike information criterion (AIC) used to compare goodness-of-fit between nested models, where <AIC equates to better mode fit.
[b] ΔAIC represents the change in AIC from the null model of equal survival and/or survey recapture probabilities (i.e., $\phi\ (t)\ p(t)$) to nested models.

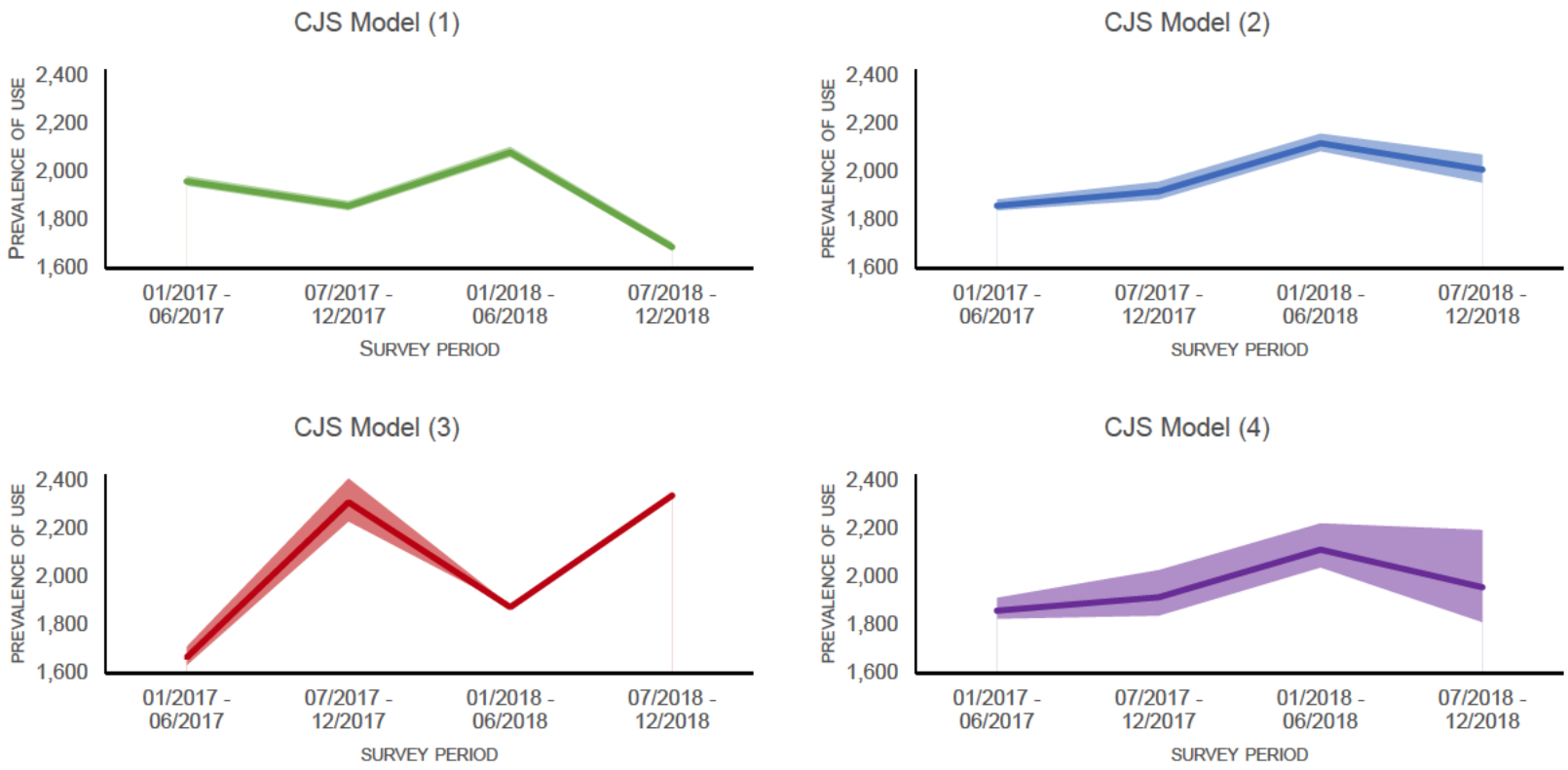

Figure A1 Estimated trends for fentanyl prevalence of use/exposure, 2017–2018

Note: Fentanyl prevalence of use estimated from self-reporting and detection from urine testing. Bands represent 95% confidence intervals. Table A3 contains model descriptions and goodness-of-fit statistics for each projection.

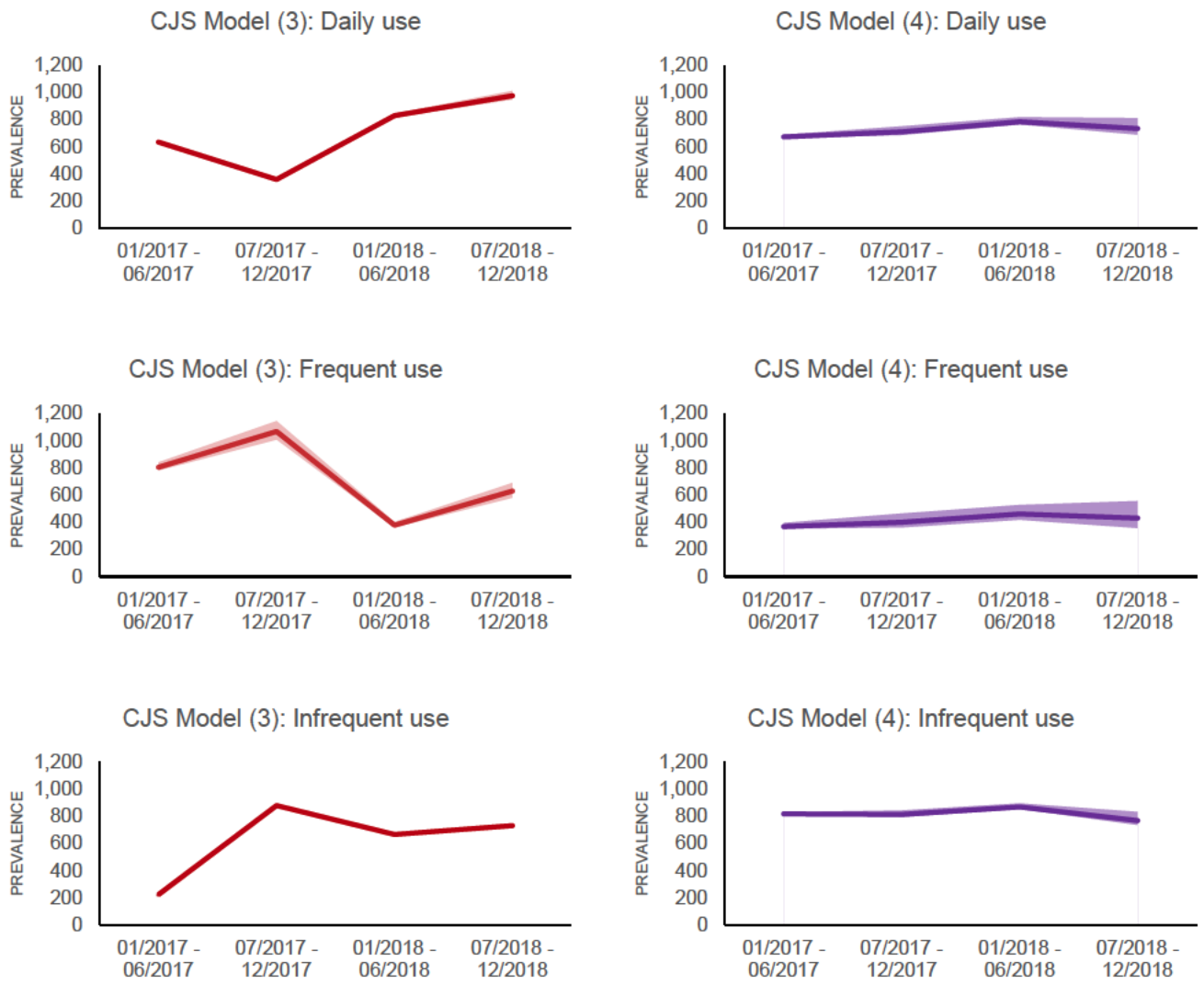

Figure A2 Trends for prevalence of fentanyl use/exposure, by frequency of use, 2017–2018

Notes: Trends on the left-hand side (red) projected from CJS Model 3. Trends on the right-hand side (purple) projected from CJS Model 4 (see Table A3).

Table A4 Trends of fentanyl prevalence of use/exposure (open model), 2017–2018

	Heroin frequency of use[a]	Estimate	95% CIs lower bound	95% CIs upper bound
Jan – Jun ‘17	Daily	633.78	627.94	641.53
	Frequent	806.29	778.94	841.87
	Infrequent	225.08	223.81	226.78
Jul – Dec ‘17	Daily	359.57	352.86	367.85
	Frequent	1068.66	1005.57	1145.68
	Infrequent	879.73	869.13	892.94
Jan – Jun ‘18	Daily	828.08	814.59	845.04
	Frequent	378.80	359.55	402.38
	Infrequent	666.58	659.66	675.35
Jul – Dec ‘18	Daily	975.28	944.82	1012.06
	Frequent	629.57	579.11	689.79
	Infrequent	731.03	715.75	749.65

Note: Estimates calculated from CJS Model (3) (see Figure A1).

[a] Frequencies of heroin use for the past-six months were self-reported by participants for each wave of the survey. We refer to frequencies of heroin use over fentanyl frequencies of use for two reasons. First, participants provided better reporting on their heroin use (i.e., VIDUS, ACCESS, and ARYS cohort studies). And second, most heroin on the street over our observation period contained fentanyl—meaning there’s high concordance between fentanyl and heroin use (Tupper et al. 2018). For those reasons, self-reported frequencies of heroin use ought to represent fentanyl use patterns.

Table A5 Trends of fentanyl prevalence of use/exposure (open model), 2017–2018

	Heroin frequency of use[a]	Estimate	95% CIs lower bound	95% CIs upper bound
Jan – Jun ‘17	Daily	672.91	662.89	688.84
	Frequent	369.31	351.05	397.58
	Infrequent	817.54	809.62	830.91
Jul – Dec ‘17	Daily	711.26	685.15	750.44
	Frequent	401.14	358.85	465.61
	Infrequent	813.33	794.17	843.98
Jan – Jun ‘18	Daily	784.13	761.73	818.26
	Frequent	461.74	416.51	529.05
	Infrequent	869.22	852.54	896.44
Jul – Dec ‘18	Daily	733.77	686.33	812.75
	Frequent	430.42	354.76	557.37
	Infrequent	769.07	733.99	832.81

Note: Estimates calculated from CJS Model (4) (see Table Figure A1).

[a] Frequencies of heroin use for the past-six months were self-reported by participants for each wave of the survey. We refer to frequencies of heroin use over fentanyl frequencies of use for two reasons. First, participants provided better reporting on their heroin use (i.e., VIDUS, ACCESS, and ARYS cohort studies). And second, most heroin on the street over our observation period contained fentanyl—meaning there’s high concordance between fentanyl and heroin use (Tupper et al. 2018). For those reasons, self-reported frequencies of heroin use ought to represent fentanyl use patterns.

Table A6 A detailed breakdown of fentanyl and heroin retail and wholesale prices by unit ($CAD), reported by Vancouver Police Department, 2017–2019

		Fentanyl prices $CAD			Heroin prices $CAD		
	Unit (g)	2017	2018	2019	2017	2018	2019
Powder							
½ point	0.05g	$10	$10	$10	$10	$10	$10
1 point	0.10g	$20	$20	$20	$20	$20	$20
½ gram	0.50g	$80	$70–$80	$70–$80	$80	$80	$80
1 gram	1.00g	$160	$160	$160	$140–$160	$140–$160	$140–$160
½ eight ball	1.75g	$225	$225	$225	—	—	—
1 eight ball	3.50g	$400	$400	$400	—	—	—
½ ounce	14.18g	—	$1,500	$1,500	—	—	—
1 ounce	28.35g	$2,000–$2,300	$2,800–$3,600	$2,800–$3,600	$3,000–$5,000	$3,000–$5,000	$3,000–$5,000
1 kilogram[a]	1,000.00g	$70,000–$80,000	$70,000–$80,000	$70,000–$80,000	$68,000–$72,000	$68,000–$72,000	$68,000–$72,000
Counterfeit OxyContin							
1 pill[b]	0.01g	$15–$40	$15–$40	$15–$40	—	—	—
1,000 pills[b,c]	10.00g	$11,000–$13,000	$11,000–$13,000	$11,000–$13,000	—	—	—

Note. Fentanyl and heroin street prices provided by Vancouver Police Department (VPD). At the retail-level (i.e., street), VPD reports no differences between fentanyl and heroin prices. The lethal dose range for fentanyl use/exposure ≅ 0.05–2.00mg (from variation in street-level purities).

[a] Adulterated kilogram price; for fentanyl manufactured domestically, price for one kilogram = CAD$280,000.

[b] Assuming 10mg tablets (0.01g) being sold on the street (based on unit price).

[c] $11–$13/pill when purchasing 1,000 pill quantities.

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