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OPIOID USE, HEALTH AND CRIME: INSIGHTS FROM A RAPID REDUCTION IN HEROIN SUPPLY

Timothy J. Moore
Kevin T. Schnepel

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ABSTRACT

In 2001, a large and sustained supply shock halted a heroin epidemic in Australia. We use drug offenses to identify individual opioid users and examine how the shock affected their mortality risks and criminal activity over the next eight years. Initially, gains from fewer overdoses are offset by drug substitution and more crime, including homicides. Most adverse effects dissipate over time, while persistent mortality reductions save the lives of around one in 48 individuals in our sample. Our results demonstrate that reducing the supply of illicit opioids can lead to meaningful longer-term improvements, even when the short-term effects are ambiguous.

Timothy J. Moore
Department of Economics
Purdue University
403 W. State Street
West Lafayette, IN 47907
and NBER
moore839@purdue.edu

Kevin T. Schnepel
Department of Economics
8888 University Drive
Simon Fraser University
Burnaby, British Columbia, V5A 1S6
Canada
kevin_schnepel@sfu.ca

Illicit drugs are associated with many adverse outcomes, including premature mortality, violent behavior, child neglect and property crime (Reuter 2010, Maclean et al. 2022). In the midst of a "drug epidemic"—when drug use is intense and widespread—these problems have profound effects on community wellbeing and public safety (Moore and Pacula 2021). Prominent US examples include the crack cocaine epidemic and the current opioid crisis, which resulted in more than 80,000 opioid overdose deaths in 2022 (Ahmad et al. 2023).

In response to drug epidemics, governments often target suppliers by escalating the "war on drugs." However, there is skepticism about the value of doing so, even if it reduces supply. Since the demand for illicit drugs is often inelastic, a drop in supply can increase both sellers' profits and buyers' income-generating crime (Becker et al. 2006, MacCoun et al. 2003). Market disruptions can also destabilize relationships between drug suppliers and increase violence as they fight over market share (Levitt and Venkatesh 2000, Dell 2015, Castillo et al. 2020). Furthermore, consumers may substitute to other drugs, offsetting any benefits from the reduced availability of the drugs targeted by enforcement efforts (Manski et al. 2001). As a result, prominent scholars argue that "the war on drugs may increase addiction rates and even the number of addicts" (Becker and Murphy 2013), and that, in the current opioid crisis, "measures to cut access to opioids offer illusory solutions" (Dasgupta et al. 2018: 182).

Drug supply shocks provide the best opportunity to understand the net effects of reducing supply, but two empirical challenges have limited their usefulness. First, as Dobkin and Nicosia (2009) note, most shocks are too small and temporary to allow researchers to measure the consequences. In their study, a 1995 methamphetamine supply shock in California increased prices for only four months; this is one of the larger effects identified within the literature. The second challenge is that data on individual drug users are rarely available. Aggregate data make it difficult to accurately estimate drug substitution, the impacts on non-drug-defined crime, and the net mortality effects after accounting for competing risks (Cawley and Ruhm 2011, Manski et al. 2001). Consequently, we know little about the effectiveness of supply-side interventions in addressing drug problems (Reuter 2017, Smart et al. 2020).

In this paper, we overcome these limitations by studying the consequences of a large, sustained reduction in Australia's heroin supply using individual-level crime, mortality and drug treatment data. Australia experienced a major heroin epidemic during the 1990s; by 1999, as many people in Sydney and Melbourne were dying from opioid overdoses as from traffic accidents (Lancaster et al. 2011). Then, in late 2000, the availability of heroin declined rapidly and its price soared. As discussed in the next section, this was unique to Australia, most likely because major traffickers halted shipments due to higher enforcement risks, reduced global supplies, and low profit margins. In less than one year, the purity-adjusted price of heroin increased by 400%, due to both higher prices and lower drug purity. Figure 1 shows that prices remained high for many years, and opioid overdose deaths fell by around 60%.

We examine this large supply shock using individual-level administrative data from the state of New South Wales (NSW), Australia's most populous state. We use opioid possession offenses to identify 6,886 individuals using illicit opioids in the six years prior to the supply shock.[1] They had high levels of criminal activity, with average arrest rates of 0.4 charges per year for illicit drug possession, 0.1 charges per year for violent offenses, and 0.6 charges per year for property offences. They also faced high mortality risks: despite an average age of 30, their annual mortality rate was around 1% during the heroin epidemic.

We use event-study approaches to identify the impact of reduced heroin supply on these opioid users. Our primary specifications use comparison groups of non-opioid drug offenders to account for other factors affecting drug use, mortality and criminal activity, and we also assess the effects using alternative specifications and additional controls. In general, the direction and magnitude of our effects are similar across these specifications.

We first examine mortality outcomes. The supply shock led to large and persistent decreases in opioid-related mortality on the order of 36-56%. Given the size of the price shock, these results imply own-price elasticities of demand of around -0.08 in the short term and

[1] Our offense and mortality data allow us to identify opioids, but not specific types like heroin.

-0.12 in the longer term. There is no detectable change in non-opioid causes of death, and a large and sustained drop in overall mortality rates equal to around 90% of the decrease in opioid-related mortality. This implies that direct mortality improvements are—at most—only slightly offset by competing mortality risks.

We next consider drug substitution, finding that the opioid users in our sample substitute to other illicit drugs. In the first year of the heroin supply shock, non-opioid drug possession offenses double, with cocaine offenses increasing by approximately 120% and methamphetamine offenses increasing by 37%. After the first year, cocaine possession offenses return to baseline levels, but methamphetamine offenses remain elevated. Our estimates imply short-term cross-price elasticities of demand of 0.45 for cocaine and 0.08 for methamphetamine, with a long-term cross-price elasticity of demand of 0.10 for methamphetamine.

We then consider the broader impacts on crime. Violent offenses increase by approximately 41% in the first year, driven by large increases in homicides and assaults. One striking result is that there is a 24-fold increase in murder charges in the first year. Most violent offenses return to baseline levels by the third year and are stable thereafter, although homicides and assaults remain slightly elevated throughout our follow-up period. The effects on property offenses are harder to assess, as opioid users committed property crime at much higher rates than non-opioid users prior to the heroin supply shock. Relying primarily on a comparison group of individuals who had committed both non-opioid drug offenses and property crimes before the supply shock, we find increases in robbery, burglary and vehicle theft in the first year of reduced heroin supply. These increases dissipate, while petty/other theft is flat in the first year and then halves over the longer term. These results are consistent with many opioid users initially switching to other drugs and committing more crime, but only a subset maintaining these changes over time.

To understand the net impact of the heroin supply shock on opioid users in our sample, we use information on the value of statistical life and crime costs to aggregate these effects.

Back-of-the-envelope calculations suggest the net present value is uncertain in the short-term, with mortality savings offset by higher crime costs. When evaluated over eight years, the net present value of the savings at the time of the supply shock is around A$169,700 (US$120,500) per opioid user in our sample (in 2022 dollars).[2] Persistent mortality reductions over the eight years—which are estimated to save the lives of around one in every 48 individuals in our sample—are a key driver of these substantial gains.

This research contributes to the literature on illicit drug supply shocks that spans economics, criminology, epidemiology and public health. Most directly, our findings provide a deeper understanding of the consequences of the Australian heroin supply reduction, which has generated substantial interest but has not yet been evaluated using detailed longitudinal data (e.g., Day et al. 2003, Degenhardt et al. 2004, Donnelly et al. 2004, Weatherburn et al. 2003). We also document broader effects than economic studies of other drug supply shocks, primarily because the length and structure of our data delivers greater precision than has previously been possible (e.g., Dobkin and Nicosia 2009, Dobkin et al. 2014, Cunningham and Finlay 2013, 2016, d'Este 2021).[3] For example, Dobkin and Nicosia (2009) find that the 1995 methamphetamine supply shock in California halved methamphetamine hospital admissions and methamphetamine use among arrestees, but do not find any impacts on the demand for other drugs or on property or violent crime. Our ability to precisely identify all of these effects and describe how they differ over time allows us to deliver new insights into what happens when the supply of an illicit drug is reduced.

Two novel insights are especially relevant to addressing problems related to the use of heroin and other illicit opioids. One comes from being the first to use exogenous variation to estimate the degree of substitution from heroin to cocaine and methamphetamines. In the US opioid crisis, policies aimed at reducing the supply of prescription opioids have had perverse effects on the demand for illicit opioids (Alpert et al. 2018, Evans et al. 2019). We show that

[2] We use the average AUD/USD exchange rate for the sample period.

[3] For reviews of the broader literature on drug supply shocks outside of economics, see Caulkins and Reuter (2010), Werb et al. (2011), and Pollack and Reuter (2014).

when the opioids driving the epidemic only have more expensive substitutes, as was the case for heroin in our setting and is currently the case for fentanyl in the US, substitution to other hard drugs may be temporary.[4] A second key insight is the degree to which reductions in fatal opioid overdoses are offset by competing mortality risks (Honoré and Lleras-Muney 2006, Swensen 2015). Studies across Australia, Europe, and the US find that opioid users have elevated mortality risks across many causes of death (Bargagli et al. 2006, Degenhardt et al. 2011). We find competing risks to be a relevant but minor concern, with approximately 90% of the gains from fewer opioid-related deaths translated into lives saved. This can help policy-makers understand the potential of policies that target opioid overdoses.

We also advance the understanding of the relationship between drugs and crime (MacCoun et al. 2003, Reuter 2017).[5] One finding is that market disruptions can increase violence, even in a developed country with low levels of violent crime.[6] Opioid users in our sample committed one-fifth of all murders in NSW in the first year of the supply shock, validating concerns that market disruptions can increase violence among drug users. We also shed light on the link between drugs and property crime. The lack of sustained increases—and the longer-term decline in petty theft—refutes a common view among policy analysts that illicit drug users "will 'do anything' and 'pay anything' to consume drugs" (Manski et al. 2001: 142).

Finally, we show that the impact of a heroin supply reduction can look very different over time. After one year, there were fewer overdoses but more homicides and other serious types of crime, including assault, robbery and vehicle theft. Beyond that, the net benefits changed

[4]See Grossman (2005), Reuter (2010), and Gallet (2014) for reviews of the demand for illicit drugs. Among studies examining drug supply reductions, Cunningham and Finlay (2016) find mixed evidence on whether methamphetamine users substitute to cocaine after precursor laws reduce supply, and other studies find no effects. After adjusting for potency, prescription opioids are much more expensive than heroin and fentanyl (Evans et al. 2019). See Moore and Pacula (2021) for a discussion of how cheap illicit drugs drive epidemics.

[5]Among economics studies examining drug supply shocks and crime, we are only aware of d'Este (2021) finding a link (that methamphetaine precursor laws increase county-level crime rates). However, Dobkin et al. (2014) use similar variation and find no drugs-crime relationship. Other studies examining drugs-crime relationships include Corman and Mocan (2000); Grogger and Willis (2000); Kuziemko and Levitt (2004); Adda et al. (2014); Evans et al. (2016); Bondurant et al. (2018); and Dave et al. (2021).

[6]Studies have such a link in Mexico and Colombia as a result of market disruptions (e.g. Dell 2015, Castillo et al. 2020). In 2001, Australia's homicide rate was approximately one third of the size of the US rate and similar to Europe (authors' calculations from `https://dataunodc.un.org/` [Accessed 5/20/2023]).

from ambiguous to sizeable as opioid users reduced their drug consumption and levels of criminal activity. A key takeaway is that studies with a limited follow-up period are unlikely to be informative about supply-side drug policy interventions.

Australia is broadly similar to other developed countries in terms of its drug laws, types of drugs available, and levels of addiction. The price sensitivity of opioid users in our sample is comparable to estimates for heavy illicit drug users in the US.[7] These features suggest our findings can be informative about the potential effects of reducing the supply of illicit drugs elsewhere, especially given the empirical challenges and policy importance of this topic.

There are some important limitations to our analysis. First, we cannot assess the effects on opioid users not in contact with the criminal justice system. That said, when we examine state-level changes in opioid overdoses and crime rates, we find that they reinforce the effects identified for our sample. Second, we unable to precisely attribute how much of the supply reduction is due to law enforcement, and what policies would be needed to generate similar supply reductions. Similar to law enforcement successes with methamphetamine precursor chemicals in the US, Australia's interdiction efforts here likely benefited from the market being supplied by a small number of sophisticated crime syndicates (Degenhardt, Reuter et al. 2005, Dobkin and Nicosia 2009). Few other supply shocks are likely to be as large or persistent, although it is precisely these features that enable us to advance the understanding of how supply reductions affect the adverse consequences associated with illicit drug markets.

The remainder of the paper proceeds as follows. We provide further background in Section 2, outline the empirical approach in Section 3, and describe the data in Section 4. In Section 5, we analyze the impact of the heroin supply reduction on mortality, drug use and crime. In Section 6, we evaluate the net impacts of this reduction, consider how they changed over time, and provide concluding remarks.

[7] In the US, Dave (2006) estimates an own-price elasticity for heroin emergency-room admissions of -0.1, while Dave (2008) estimates heroin participation elasticities for arrestees of -0.1 in the short term and -0.26 in the long term. For methamphetamine users, Cunningham and Finlay (2016) estimate an own-price drug treatment elasticity of -0.13 to -0.17, while the results in Dobkin and Nicosia (2009) imply similar estimates.

2 Background

2.1 Opioids and Other Drugs in Australia

A small heroin market developed in Australia during the Vietnam War, when American soldiers on leave in Sydney created demand for the drug and introduced it to locals. Most heroin initially came from Thailand via body or baggage concealment methods on commercial airlines, or via mail. These methods continued until the 1990s, when South East Asian crime syndicates began to smuggle heroin in shipping containers (Degenhardt, Reuter et al. 2005).

During the 1990s, there was a large increase of heroin coming into Australia. One indication of this was the rapid rise in opioid-related deaths, as shown in Figure 1. States other than NSW showed similar increases. Heroin became cheaper, and injecting drug users reported heroin as the drug they used most frequently (Topp et al. 2002). The most common drug treatment was for opioids, mainly for heroin (Degenhardt et al. 2004).

Other illicit drugs were available around this time, with amphetamines and cocaine the most common 'harder' drugs available. Various forms of amphetamines have long been available in Australia, including methamphetamine (also referred to as 'speed' and 'base'), crystal methamphetamine ('ice' and 'crystal meth') and MDMA ('ecstasy'). Domestically produced speed was a common form of the drug in Australia, although crystal meth became available in Australia in the 1990s and provided a higher-purity form of the drug that was commonly injected. As such, it is a closer substitute to heroin than other forms of amphetamine. Methamphetamines were smuggled from Asia, especially China. There was potentially some overlap between methamphetamine and heroin trafficking, although the source countries and key suppliers were thought to be different (Pieper 2006). The cocaine market was described as operating through tight social networks, perhaps reducing its visibility in terms of use and social problems. Crack cocaine use was rare and has never been a prominent form of cocaine in Australia. Cocaine was sometimes seized at the Australian border during the late 1990s, although typically separately to heroin and other drugs (Shearer et al. 2008).

Drug prices from surveys of injecting drug users suggest that impure per-dose prices of methamphetamine and cocaine were around 25-50% more expensive than for heroin (Topp et al. 2002). To understand how prices changed over time, in Appendix Figure A.1 we show the relative changes in the purity-adjusted prices of heroin, methamphetamine and cocaine from 1998 to 2008 (relative to 2000).[8] The changes in heroin prices in 2001 dwarf other price changes in this period. Cocaine prices are stable throughout, while methamphetamine prices decline over time, with most of the decrease occurring prior to 2001. The decline in the methamphetamine price may reflect a changing mix of substances being used and seized; unfortunately, specific types are not identified in these data. Caulkins et al. (2006) examined Australian forensic data for both heroin and methamphetamines and found no change in methamphetamine purity in response to the heroin supply shock. In our analysis, we will consider whether changes in the prices of other drugs affected opioid users' outcomes.

It is also important to understand the role of prescription opioids as potential substitutes to heroin. Prescription opioids were misused in Australia during the 1990s, especially methadone and morphine. In 1998, opioids other than heroin accounted for 23% of opioid overdose hospitalizations, with a further 12% not attributed to a specific opioid (Blanch et al. 2014). As shown in Appendix Figure A.2, Australia experienced a six-fold increase in opioid prescriptions between 1990 and 2012. Australia and the US had similar prescription opioid rates during most of the 1990s, before the rise in the US outpaced the growth in Australia.

International comparisons of illicit drug markets are difficult, but data from the United Nations Office for Drugs and Crime suggest that Australia's illicit drug prices in the 1990s were broadly similar to other developed countries.[9] Around this time, Reuter (2009) examined illicit drug markets in Australia, the US, Canada and 15 European countries and noted a "marked similarity in drug trends (if not in levels of drug problems)" (p. 21).

[8] As in Figure 1, this is constructed by dividing retail prices from a survey of regular injecting drug users by drug purities from forensic analyses of retail-level quantities. The size and frequency of doses differ across drug types and make "per dose" comparisons difficult, which is why we focus on relative price differences.

[9] For example, their data on the retail price of a gram of heroin in 1999 suggest that Australia's heroin price was the seventh-most expensive of 19 developed countries (see Appendix Figure A.3).

2.2 The Reduction in Heroin Supply

A large and persistent reduction in the supply of heroin appeared across Australia in late 2000. The decline was remarkably quick, occurring in a matter of weeks (Weatherburn et al. 2003). It occurred at a national level, which makes it unlikely that local policing or other state-level factors played an important role (Degenhardt, Reuter et al. 2005). It was also specific to Australia, suggesting that it was not strongly related to general changes in international heroin markets (Caulkins and Reuter 2006).[10]

Degenhardt, Reuter et al. 2005 reviewed and evaluated potential explanations, including changes in opium growing (such as government actions against opium farmers and drought); Australian government actions against international drug traffickers; Australian law enforcement corruption investigations; and the removal of key figures in drug production and trafficking. They concluded that it was likely due to a combination of factors that operated synergistically. Improved drug law enforcement in the late 1990s was likely a key factor. The Australian Federal Police (AFP), the primary agency responsible for drug interdiction, received substantial extra funding in the late 1990s to establish overseas operations near drug trafficking routes in Asia and mobile 'strike teams' aimed at dismantling trafficking groups. As shown in Appendix Figure A.4, AFP funding and its presence in Asia doubled between the 1997 to 2000 Australian financial years. Other agencies also received more funding for customs screening and to detect drug-related money laundering.

These efforts had some success. The AFP seized an average of 127 kilograms of heroin per year over the 1995 to 1997 financial years; over the 1998 to 2000 financial years, the average was 524 kilograms per year.[11] Interestingly, the number of seizures nearly halved, suggesting that the AFP effectively targeted much larger shipments (see Appendix Figure A.4). Caulkins and Reuter (2010) describe three large seizures of heroin bound for Australia in 2000. The first two seizures, each around 100 kilograms, occurred in early 2000. In October 2000, a third

[10] Wood et al. (2006) note a decline in 2001 heroin overdoses in British Columbia, Canada, but Caulkins and Reuter (2006) find it to be quite different to the abrupt reduction experienced in Australia.

[11] For context, in 1997 Australia's heroin consumption was estimated to be 2-3 metric tons (Hall et al. 2000).

seizure of 357 kilograms in Fiji included a large number of arrests and shutting down a front company that had been used to facilitate smuggling. According to the AFP, this operation “rendered ineffective a sophisticated concealment methodology, identified a legitimate cargo stream, and removed some very important [smugglers]” (Hawley 2002: 48).

Degenhardt, Reuter et al. 2005 cite law enforcement sources who believed that two key groups were using shipping containers to smuggle most of the heroin into Australia, and that the withdrawal of these two groups from Australia precipitated the sharp reduction in heroin supply. They likely withdrew from Australia due to the success of more intense drug interdiction, low profit margins, and reduced heroin supplies in the Golden Triangle (resulting from the Taliban in Afghanistan and other global factors) (Gibson et al. 2005). Similar factors likely made Australia unattractive to other large-scale suppliers, which may account for the persistently lower supply. Instead, they were replaced by suppliers using small-scale, inefficient methods like mail packages and drug mules (Degenhardt, Reuter et al. 2005).

This led to a large increase in heroin prices and a large reduction in opioid overdoses, as shown in Figure 1. Heroin users reported longer search times and difficulty obtaining heroin. For example, 88% of Australian injecting drug users surveyed in the middle of 2001 reported that heroin was difficult to obtain; this was in sharp contrast to 12 months before, when 99% of respondents had reported heroin as easy or very easy to obtain (Topp et al. 2002). Large reductions in opioid overdoses were observed across all states (Topp et al. 2002).

As discussed in the introduction, several studies have examined the consequences of the heroin supply reduction. We summarize the approaches and findings of 17 studies in Appendix B, which are all of the published studies we could find examining the effects of the supply shock on opioid use and related outcomes for longer than one year. The key improvement over these studies is our use of individual-level panel data. The primary data in 15 of the studies are monthly/quarterly counts constructed from state or territory administrative data. These data were typically analyzed using time-series methods. The primary data in two other studies are repeated cross-sectional data from the Illicit Drug Reporting System, an annual survey using a convenience-sample of around 900 injecting drug users. Another contribution is the length of our follow-up period. Of the 17 studies, 13 track outcomes for three years or less. Only two go beyond four years after the onset of the supply shock, with Day et al. (2006) examining heroin price, purity and self-reported availability for five years, and Snowball et al. (2008) examining opioid and methamphetamine arrests for 6.25 years.

These studies consistently document a decline in measures of heroin and opioid use after the supply reduction (Day et al. 2006, Donnelly et al. 2004). The results for other outcomes are more mixed. For example, Smithson et al. (2004) finds a short-term decline in robbery, Donnelly et al. (2004) finds a short-term increase in robbery, while Degenhardt, Day, Dietze et al. (2005) finds no change. This is not surprising, given that effects are generally inferred from changes in state-level outcomes or cross-sectional changes in responses from illicit drug user surveys. As a consequence, while the size of the initial supply shock is fairly well understood, little is known about the broader consequences.

2.3 Policy Environment

Australia has a federal structure. State governments are responsible for providing local services, such as police, schools and hospitals. NSW is the most populous state. It had 6.5 million residents in 2000, which represented around one third of Australia's population. Here, we provide background on key policy features in NSW at the time of the heroin supply shock.

Law enforcement. International drug interdiction is the responsibility of the AFP and other agencies that are part of the federal government, such as the Australian Border Force. Each state government has a single, independent police agency that is responsible for all police activities within the state. NSW Police has more than 15,000 police officers and 500 police stations. Federal and state law enforcement budgets are set separately.

The resources devoted to policing in NSW were consistent in the late 1990s and early 2000s. This is evident in Appendix Figure A.5, which shows the number of operational police officers and NSW Police employees per 100,000 residents over the 1996 to 2007 financial years. The figure also shows that, while per-capita prison capacity and the average number of prisoners increased over this period, it did so at a steady rate of approximately 3% each year.

Drug laws and policies were also generally consistent over this period. Needle and syringe programs were first established in NSW in the 1980s, and operated throughout the 1990s and 2000s. The introduction of drug courts in 1999 and a safe-injecting facility in 2001 are both policy changes that could have affected drug taking, although initial participation was low.[12] Another change was that the NSW Police got more search powers in 1998 and 2001, which increased the use of occasional crackdowns in major heroin street markets.[13] Despite these changes, we do not find evidence of major changes in the severity of punishment for various types of offenses, as shown in Appendix Figure A.6.[14]

Drug treatment. In Australia, the main approach to treating opioid addiction has been to provide an opioid replacement like methadone under medical supervision. The primary aim is to stabilize and then slowly reduce opioid use. This process can last years, and often results in a "maintenance" dose that an individual takes indefinitely. Physicians in general practice or addiction treatment clinics prescribe the methadone, and state governments manage central systems that record each authority to dispense methadone. It is then dispensed daily by a pharmacist or nurse, either at a clinic or community pharmacy. The methadone needs to be consumed in front of the person giving it in order to prevent diversion to the black market, although one or two "takeaway" doses may be allowed each week. Counselling or other support services may be offered, but are not required. Many drop out of treatment early, and multiple

[12] The NSW Drug Court had only around 150 entrants each year through to 2008 (State of NSW 2009). Around 800 individuals visited the safe-injecting facility each month between May 2001 and April 2007 (National Centre in HIV Epidemiology and Clinical Research 2007).

[13] *The Crimes Legislation Amendment (Police and Public Safety) Act 1998* allowed the police to ask people in public places to move on and search those suspected of having weapons. In 2001, it was expanded to allow searches of people suspected of selling drugs. In the late 1990s, there were occasional crackdowns in street markets in Cabramatta and Central Sydney/Kings Cross (Maher and Dixon 2001, Degenhardt et al. 2004).

[14] We plot the trends for different types of sanctions and punishment for opioid offenses, non-opioid drug offenses, violent and property offenses. We find no large shifts around the supply shock in the probability of incarceration, average length of incarceration, probability of a fine and the average amount of fine.

treatment episodes are common (Burns et al. 2009). NSW introduced buprenorphine in 2001, although methadone still accounts for the majority of treatment episodes.[15]

Participants pay around A$30 (US$21) per week for methadone, with some clinics providing it for free.[16] Treatment capacity can be limited, as new or returning participants typically receive methadone at specialist clinics until they are judged to be stable, which can take several months. They are then transferred to a community pharmacy. There were 424 physicians in NSW who could prescribe opioid pharmacotherapies in 2001; the number grew steadily in the 2000s, to approximately 800 by 2008. Medication-dispensing locations numbered around 700 in the mid-2000s, with about 35 specialist treatment clinics.[17]

Other policies. A broader set of policies are also relevant. Universal health insurance provides free access to health services. These services were funded in a consistent way before and after the heroin supply reduction. Given the importance of mental health treatment for those with drug addictions, Appendix Figure A.5 shows the number of bed and employees devoted to public specialist mental services in NSW. The number of available beds, nearly all of which are in psychiatric hospitals or specialist wards of public hospitals, is flat over this period, while the number of full-time-equivalent employees increased at a steady rate.

Social insurance payments for unemployment and disability are made by the federal government. Payment levels change smoothly around the time of the heroin supply reduction, as shown in Appendix Figure A.5. We also plot the unemployment rate, employment-to-population ratio, and average weekly earnings over time in NSW. The economy steadily improved over this period, with the unemployment rate declining from around 7-8% in 1996 to around 5% by 2008. There were no recessions in Australia over this period.

[15] Buprenorphine has a lower risk of overdose and longer-lasting effects than methadone, but takes longer to administer and is associated with lower treatment retention (Burns et al. 2009).

[16] Participants may pay a pharmacy dispensing fee; governments pay for doctor visits and the methadone.

[17] Data on NSW pharmacotherapy providers is limited before the 2000s, but the policies and funding arrangements were stable throughout the late 1990s and early 2000s. NSW pharmacotherapy participants numbered 12,107 in 1998, 15,069 in 2001, 15,719 in 2004, and then climbed to 17,168 by 2008 (Australian Institute of Health and Welfare 2013, Jones et al. 2021).

In summary, the policy environment was relatively stable over our sample period. We assess the potential for policy changes to influence our findings in Section 5.5, by separately interacting time-varying policy measures with identifiers for the treatment and comparison groups. Our primary results are robust to these additional controls.

3 Empirical Approach

We use an event study framework to evaluate the dynamic effects of the heroin supply reduction on various outcomes. We define our treatment group (in terms of the supply reduction) as individuals arrested for using or possessing illicit opioids prior to the onset of the supply reduction in the fourth quarter of 2000. Across our specifications, we use indicator variables relative to the supply-reduction onset to flexibly assess changes in outcomes.

Our main approach is to use a differences-in-differences event study specification that includes a comparison group of individuals arrested for using or possessing a non-opioid illicit drug before the fourth quarter of 2000 (but not arrested or treated for using opioids). This specification takes the following form:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{\substack{s=-5 \\ s\neq-1}}^{S=8} \beta_s Opioid_i \times \mathbb{1}[s_t = t] + X_{it}\lambda + \epsilon_{it} \tag{1}$$

For a given outcome Y_{it}, i indexes individuals and t indexes calendar time in quarters. We let s denote years relative to the onset of the heroin reduction in the fourth quarter of 2000. Our primary coefficients of interest are given by β_s. They come from the interaction of $Opioid_i$, which identifies individuals in the treatment group, with indicator variables for each event-time year. Since the onset of the heroin shortage occurred partway through the fourth quarter of 2000, we define event-time years before the event from the fourth quarter of one calendar year to the third quarter of the next (e.g., $s = -1$ from the fourth quarter of 1999 through the third quarter of 2000). After the event, event-time years follow calendar years (e.g. $s = 0$ for 2001, $s = 1$ for 2002, etc.). The reference period is $s = -1$, the 12 months prior to the onset of the supply reduction. We estimate coefficients for each of the four event-time years before the event and eight afterwards.

We include individual fixed effects α_i and a vector of time controls γ_t consisting of a full set of event-year fixed effects; quarter-of-year fixed effects; and a dummy variable for the fourth quarter of 2000 that is interacted with the treatment group indicator. We also include a vector of individual-level time-varying controls given by X_{it}, which consists of a complete set of age fixed effects interacted with sex and Indigenous status indicator variables.[18] These control nonparametrically for lifecycle trends related to drug use, drug treatment, criminal activity and health that may vary by demographic characteristics. The error term is ϵ_{it}, and we allow for an arbitrary correlation in errors over time at the individual level.

This approach relies on several key identifying assumptions. First, that the supply reduction was unanticipated and exogenous to the opioid users in our sample. The available evidence described in Section 2 suggests that the heroin reduction was unexpected and unrelated to local drug demand. Second, that the time-varying unobserved determinants of our outcomes of interest affect opioid and non-opioid illicit drug users in similar ways (e.g., local policing or healthcare policies). These assumptions can be partially tested by using the pre-event time dummy variables to assess whether the trends in outcomes are similar across the treatment and comparison groups. We also relax the second assumption by allowing time-varying economic and policy measures to affect the treatment and comparison groups differently.

A third assumption is that the comparison group of non-opioid illicit drug users are not affected by the heroin supply reduction. This could be violated if the comparison group includes opioid users not arrested for using/possessing opioids, or who never participate in medication-assisted treatment. It could also be violated if there are spillover effects of the heroin supply reduction that affect non-opioid drug users. We consider the potential for drug-price-related spillover effects by allowing non-opioid illicit drug prices to affect individuals differently based on their pre-reduction drug offenses. We further assess these concerns by using alternative comparison groups and also a specification without a comparison group. In general, these exercises suggest that ourresults are not strongly affected by spillovers.

[18]For mortality outcomes, we use permanent demographic controls instead of individual fixed effects.

4 Data

We create a new longitudinal dataset for this paper that links, at the individual level, administrative data held by the NSW Government from criminal justice, drug treatment and mortality databases. NSW is unique among Australian states in having consistent individual-level criminal justice and treatment records since the early 1990s. This enables us to follow individuals several years either side of the onset of the heroin supply reduction.

The criminal offense data comes from the NSW Bureau of Crime Statistics and Research's Reoffending Database (Offence Level Data). It includes all individuals charged with a criminal offense in NSW since 1994, and includes information on offense types; date of occurrence; court outcomes; and demographic information, including sex, age and Indigenous status. Drug offenses include information on the class of drug involved (e.g., opioids, cocaine, methamphetamines), but not subtypes of these drugs.

The drug treatment data comes from the NSW Department of Health's Pharmaceutical Drugs of Addiction System (PHDAS). The PHDAS records all individuals who have been prescribed methadone or buprenorphine in NSW since 1985. An entry to the database occurs when a medical physician obtains an "authority to dispense" these drugs, which is mandatory as they are classified as drugs of addiction.[19] Information in the PHDAS database includes opioid treatment start and end dates, drug type, and maximum dose authorized.

To create our sample, the Centre for Health Record Linkage extracted information on individuals with arrests between 1994 and 2001 for a drug, property, or violent offenses (i.e., nearly all offenses except for traffic and bail offenses). They then linked these individuals to more recent versions of these data sets, along with two mortality data sets.[20] One is the death registration data from the NSW Registry of Births Deaths and Marriages, which includes

[19] There are limited exceptions, such as hospital inpatients who receive schedule drugs for less than 14 days. See: `https://www.health.nsw.gov.au/pharmaceutical/doctors/Pages/Prescribe-S8-opioid.aspx`.

[20] The Centre for Health Record Linkage is part of the NSW Department of Health that match individuals across data sets using names and other available personal information. They expect that false negatives and false positives each occur at rates equal to around 0.5%. See: `https://www.cherel.org.au`.

information on individuals' date of death, age at death, sex and Indigenous status. The other is the Cause of Death Unit Record File, which includes causes of death classified according to International Classification of Disease systems. Drug poisoning information allows us to identify deaths related to opioids, but not particular types of opioids. Both use information from death certificates, which are signed by physicians. We also have information from three other Department of Health data sets; however, they are not available before 2001 so are only used to check and fill in demographic information for our sample.[21]

Our combined data includes information on criminal offenses, opioid treatment, and mortality. We organize the data by classifying criminal offenses as drug, violent and property offenses;[22] cleaning the opioid treatment episodes in line with previous research using these data (Burns et al. 2009);[23] and using cause-of-death codes to identify deaths that are related to opioids, the use of other drugs and alcohol, and other causes of death.[24] The panel starts in 1994, which is when offending data are first available, and ends in 2008.

[21] The Admitted Patient Data Collection records information on inpatient admissions to public hospitals; the Emergency Department Data Collection records information on public hospital ER visits; and the Ambulatory Mental Health Data Collection records information on participants in mental health programs. The first two data sets begin in 2005, and the third begins in 2001. For more information, see: https://www.cherel.org.au/data-dictionaries#section1.

[22] Offenses are defined using the Australian and New Zealand Standard Offence Classification (ANZSOC) (Australian Bureau of Statistics, 2011). We define offense groups (ANZSOC codes) as: (i) illicit drug offenses (1011-1042): possession/use (1022,1041,1042); supply/distribution (1011,1012,1031,1032); (ii) violent offenses (100-699 except 610-619): homicide (111); manslaughter/attempted homicide (121,131); driving causing death (132); assault and other acts to cause injury (211-299), including serious assault with injury (211), serious assault with no injury (212), and common assault (213); sexual assault and related offenses (311-329); driving under the influence of alcohol and other drugs (411); harassment and threatening behavior (531-532); (iii) property offenses (611-612 plus 700-999): robbery (611-612); burglary/break and enter (711); motor vehicle theft (811- 812); other theft (821-841); and fraud, deception and related offenses (911-999).

[23] We remove episodes related to a buprenorphine trial (90 episodes) and data cleanup (520 episodes).

[24] Deaths after 1997 are coded using the International Classification of Diseases, Tenth Revision (ICD-10). The Australian Bureau of Statistics (2002) define drug-induced deaths if they have an ICD-10 underlying cause of death related to drug overdose (X40-X44, X60-64, X85, Y10-Y14) or drug abuse (F11 for opioids and F19 for multiple drugs). We define opioid-related deaths as deaths with one of those codes and a drug identification code specifying that opioids were found in the decedent's system (T40.-T40.4). The underlying cause R99 (Ill-defined and unknown cause of mortality) was sometimes paired with the opioid drug identification codes, so we include those deaths as opioid-related. This is similar to how the US CDC define opioid drug overdoses. Deaths prior to 1997 are coded according to ICD-9. We classify opioid-related deaths using crosswalk codes included in the Australian Bureau of Statistics (2002), and choose the sub-codes focused on opioids or polydrug use: 304.0, 304.6, 304.9, 305.5, E850.0, E850.2, E853.2, E854.0, E854.0, E854.1, E858.9, and E980.0. We define drug- and alcohol-related deaths using ICD-9 and ICD-10 codes in Evans and Moore (2011), who create a list from studies of substance abuse in Australia and the US.

We use a longitudinal panel that tracks individuals on a quarterly basis. To estimate the response to the heroin supply reduction among opioid users, we identify 6,886 individuals arrested for opioid use or possession between January 1994 and September 2000. They enter the panel after the quarter of their first opioid offense, and remain through to their death or the end of 2008. Our primary comparison group consists of 25,814 individuals who have a charge for non-opioid illicit drug use or possession between 1994 and September 2000, but do not have any opioid-related charge prior to the onset of the supply reduction or opioid treatment admission during the sample period. They enter the panel in the quarter of their first non-opioid illicit drug offense and remain through to their death or the end of 2008. We also use a second comparison group that consists of 2,216 members of the primary comparison group who had at least one "serious" property offense (burglary, motor vehicle theft or robbery) prior to the onset of the heroin supply reduction. Our treatment group commits a lot of property crime prior to the heroin supply reduction, and this second comparison group is more similar on that dimension. Later in the paper, we assess the robustness of our estimates to different sample selection criteria, including the use of additional comparison groups.

Summary statistics are presented in Table I. These are calculated over the five years prior to the heroin supply reduction (i.e., from the fourth quarter of 1995 through the third quarter of 2000), and incidence rates are scaled to an annual basis and per 1,000 individuals. The means for our treatment group of opioid users are shown in column (1). The average age is 30.2 years, and 21% are female. Mortality averages 9.94 deaths each year per 1,000 individuals, and nearly 70% of these deaths are directly related to opioids. This mortality rate is around seven times higher than the mortality rate at similar ages in the general population (Australian Bureau of Statistics 2001). Among those who died, the mean time between first opioid offense and death is 4.4 years.

By definition, our treatment group has high rates of opioid use/possession offenses, but they also are charged with other harder illicit drug use/possession offenses at an average of 18 times each year per 1,000 individuals. About half of this is for methamphetamine, and about

35% is for cocaine. Charges for supplying or dealing illicit opioids are rare; the annual average is 1.1 per 1,000 individuals. There is an average of 109 violent offenses each year per 1,000 individuals. Nearly two thirds are for assault, which are fairly evenly split between serious and common assault.[25] The annual average for property offenses is 591 per 1,000 individuals. Petty and other minor theft accounts for approximately 70% of these offenses, while burglary accounts for 12% and motor vehicle theft accounts for 7.2%.

The equivalent pre-event means for the primary comparison sample are shown in column (2) of Table I. Their average age is the same as for the treatment sample (30.2 years) and 13% are female, which is lower than the 21% for the opioid-user sample. Their mortality rate is approximately one third the size of treatment sample, at 3.35 deaths each year per 1,000 individuals, with nearly all of the gap explained by differences in opioid-related causes of death. Relative to the treatment group, the comparison group has a lower rate of non-opioid drug use/possession offences (9.3 annual offenses per 1,000 individuals), although this does not include the initial offenses that led to their inclusion in the comparison group. They have a similar rate of violent offending (95 offenses) and much lower rates of property offending (80 offenses). The second comparison group of individuals with both pre-reduction non-opioid drug use/possession offenses and pre-reduction serious property offenses is more similar to treatment group in several dimensions, especially in terms the rate at which they were charged with robbery, burglary, and motor vehicle theft. Our specifications will allow us to examine the pre-event trends across the treatment and comparison samples, which we discuss in Section 5.

To understand how the "first stage" response of opioid users in our sample compares to the other opioid users in NSW, in Appendix Figure A.7 we plot their opioid-related mortality rates and the state-level per-capita rates for all other opioid-related deaths in NSW. The absolute rates are naturally very different, given that few NSW residents are opioid users.

[25]Serious assault generally involves an injury or particularly concerning circumstances (e.g., a weapon is used, includes a threat to kill, the victim is pregnant or a criminal justice official). For full definitions, see: `https://www.abs.gov.au/statistics/classifications/australian-and-new-zealand-standard-offence-classification-anzsoc/2011/02/021`.

However, in relative terms, the reductions in opioid-related mortality rates are remarkably similar. Compared to the opioid-mortality rates in 1998 and 1999, the 2001 to 2008 rates are on average 67% lower in both our sample and at the state level.[26] This is our best measure of opioid consumption, and suggests that the relative impact of reduced heroin supply on the opioid users in our sample – who generally have intense drug problems and high rates of criminal activity – was similar to other opioid users in NSW.

5 Results

Our data allow us to estimate the impact of the heroin supply reduction on many different outcomes. We organize our results as follows. We examine opioid-related mortality and other mortality outcomes in Section 5.1; substitution to other drugs in Section 5.2; the impact on violent crime in Section 5.3; and the impact on property crime in Section 5.4. We examine robustness to specification choices and sample-inclusion criteria in Section 5.5, effect heterogeneity in Section 5.6, and the broader impacts of the supply reduction in Section 5.7.

5.1 Mortality

The left column of Figure 2 shows mortality rates from 1995 to 2008 for the treatment sample of opioid users and the primary comparison group of non-opioid illicit drug users. Figure 2a focuses on opioid-related mortality. In our treatment sample, there are around seven of these deaths per 1,000 opioid users each year prior to the heroin supply reduction, a rate approximately 100 times larger than the population-level rates in Figure 1. A visually clear reduction in opioid-related deaths occurs late in 2000, when the opioid-related mortality rate approximately halves and remains low thereafter. Not surprisingly, the comparison sample has few opioid-related mortality deaths before or after the onset of the heroin supply reduction.

The right column of Figure 2 displays the estimates from our event study specification, described by equation (1). Each coefficient represents the difference between treatment and

[26] The state-level data is available annually, so we exclude the year 2000 – which includes the onset of the supply reduction – from this calculation, although Appendix Figure A.7 shows that the relative trends are also similar in that year.

control outcomes relative to the difference in our reference period (the 12 months before the onset of the supply reduction). Overall, these estimates mirror the patterns in the raw data. Morover, the pre-onset estimates do not differ from the reference period at statistically significant levels, while the post-onset estimates for opioid-related mortality (Figure 2b) and total mortality (Figure 2f) are different from zero at the 5% level of statistical significance.

We summarize these difference-in-difference estimates in Table II by averaging the coefficients over four time periods: (i) the four pre-reduction years (years -5 to -2); (ii) the first post-reduction year (year 0); (iii) the second post-reduction year (year 1); and (iv) longer-term effects over the third to eighth post-reduction years (years 2 to 7).[27] The first row shows these estimates for opioid-related mortality. The average pre-onset change is small and not statistically different from the reference period, whic alleviates concerns about pre-existing trends affecting inference. After the onset of the heroin supply reduction, the change in the first year is -2.56 opioid-related deaths annually per 1,000 individuals, -3.95 in the second year, and an average of -2.95 in the last six years. These estimates are between 36% and 56% of the reference-period mean. The first-year estimate has a p-value of 0.06, while the other two estimates have p-values less than 0.05.

We next consider the effect of the heroin supply reduction on other causes of death. Figure 2c shows the quarterly non-opioid mortality rates for the treatment and comparison groups. They have similar rates of non-opioid deaths before and after the onset of the supply reduction, at around three deaths annually per 1,000 individuals. The regression estimates, which are plotted in Figure 2d and summarized in Table II, are close to zero and not statistically significant either before or after the onset of the heroin supply reduction. We also do not find any changes to more specific non-opioid causes of death.[28] However, these

[27] While the magnitude of the estimates for opioid-related mortality are similar after the heroin supply shock, the short-term changes for some other outcomes differ to the longer-term changes, so this grouping of estimates provides a consistent way to summarize our findings.

[28] In Appendix Table A.1, we present separate estimates for (i) deaths related non-opioid drugs; (ii) a slightly broader category of deaths related to alcohol or non-opioid drugs; (iii) deaths not involving any alcohol or drugs; and (iv) suicide. The post-reduction coefficients for the two drug-related mortality outcomes are generally positive and small, although are sometimes several times higher than the reference-period means.

results provide little information about whether there was a change in the use of other drugs, as opioids have much larger and more direct mortality risks than other drugs.

We next evaluate the impact on overall mortality rates. The raw rates are presented in Figure 2e. The patterns are similar to those for opioid-related deaths in Figure 2a, which is not surprising given they account for the majority of deaths in the treatment group and non-opioid mortality does not change throughout the sample period. The regression estimates for overall mortality are presented in Figure 2f and summarized in Table II. The average change in pre-reduction mortality is small, at 0.20 deaths per 1,000 individuals. In the first two years after the heroin supply reduction, mortality changes by -2.33 and -2.90 deaths per 1,000 individuals, respectively. For the subsequent six years, we find an average change of -2.76 deaths per 1,000 individuals, relative to the reference period. The longer-term estimates are statistically significant at the 5% level, while the first two post-reduction estimates are noisier (with p-values of 0.16 and 0.09, respectively). The point estimates imply relative declines in overall mortality of around 25% in the short term and 30% in the longer term.

Importantly, the post-reduction estimates for overall mortality are similar to those for opioid-related mortality. The post-reduction point estimates for overall mortality are, on average, 91% the size of the equivalent opioid-related estimates. The reduction in opioid overdoses leads to a similarly sized reduction in overall deaths. For our sample of relatively young opioid users, this suggests that the effect of competing mortality risks is mild.

5.2 Other drug and alcohol outcomes

We next consider more direct and precise measures of non-opioid drug use. Our main outcomes are from charges for using or possessing non-opioid illicit drugs, which are mostly for methamphetamines and cocaine.[29] For each outcome, we again present figures of the quarterly rates for the treatment and primary comparison groups; figures of the annual regression

However, only the estimate for alcohol and other non-opioid drugs in the second post-reduction year is statistically significant at the 5% level.

[29] They account for 90% of these offenses; others include ecstasy, barbiturates, and hallucinogens. We do not include marijuana, as from 2000 police could issue cautions that are not in our data. See: https://www.police.nsw.gov.au/crime/drugs_and_alcohol/drugs/drug_pages/drug_programs_and_initiatives.

estimates from our primary differences-in-differences event-study specification described by equation (1); and a tabular summary of these regression estimates.

Figure 3a shows that opioid users' quarterly rates of non-opioid illicit drug use/possession offenses are flat prior to the heroin supply reduction. Immediately after its onset, there is a striking increase in these offenses that diminishes after around five quarters. The equivalent offending rates of the comparison group is fairly stable throughout the sample period. Figure 3b shows the annual regression estimates, which are summarized in the fourth row of Table II. The average pre-reduction estimates are small and not statistically different to the reference period. In the first post-onset year, opioid users' rate of non-opioid illicit drug offenses increases by 16.5 offenses per 1,000 individuals, which is nearly a doubling relative to the reference-period mean and statistically significant at the 1% level. The subsequent estimates are much smaller, at around two to three offenses annually per 1,000 individuals, and not statistically different from the pre-reduction reference period.

In order to understand if these patterns differ by drug type, we show the same results for cocaine and methamphetamine offenses in Figure 3 and Table II. Neither outcome displays trends prior to the onset of the heroin supply reduction. Cocaine offenses drive the increase in non-opioid drug offenses in the first post-reduction year, with the statistically significant change of 12.0 cocaine offenses per 1,000 individuals accounting for around three-quarters of the overall estimate. These effects dissipate in the second year and are followed by a statistically significant longer-term annual decline, with cocaine changing by an average of -3.13 offenses per year over the last six years. In contrast, there is a persistent increase in methamphetamine offenses after the supply reduction of around four offenses annually per 1,000 individuals, and the longer-term estimate is statistically significant at the 5% level.

We also examine some related outcomes in Appendix Figure A.8 and Appendix Table A.1. To assess whether the alcohol use of opioid users was affected by the heroin supply reduction, we examine offense rates for driving impaired by alcohol or other drugs. In practice, this represents driving impaired by alcohol, as roadside testing was only for alcohol at the time. This is the only consistent measure of alcohol use in our data. The raw rates and event-study estimates for this outcome show no change after the onset of the heroin supply reduction. As a more general measure of changes in alcohol or drug-affected driving, we consider whether there is a change in offense rates for driving causing death, and find no change in this outcome.

We also examine whether there are changes in offenses for selling illicit drugs. This is a rare outcome: in our treatment group, there were only 0.64 of these offenses per 1,000 individuals in the reference year. The raw rates in the treatment and comparison groups are similar except during the reference period, and both pre-reduction and post-reduction estimates are higher than the reference period at statistically significant levels. This, along with the results for opioid and non-opioid drug sales presented in Table II, provides at best weak evidence of an increase in drug selling in response to the heroin supply reduction.

Our results suggest that the rapid reduction in heroin supply led some opioid users to substitute to other illicit drugs: temporarily for cocaine, and more permanently for methamphetamines. This substitution does not show up in the causes of death associated with non-opioid drugs, although such deaths are not common and thus the confidence intervals are wide.[30] There are no meaningful changes in alcohol-impaired driving or drug dealing, although selling drugs does not seem to be a meaningful source of income at any stage.

5.3 Violent crime

We next consider the impact on more general types of criminal offending, focusing first on violent crime. The estimated changes for overall violent offenses, based on equation (1), are summarized in Table II. Prior to the supply reduction, violent crime rates are flat. In the first year after its onset, opioid users' violent offenses increased by a statistically significant 47 offenses per 1,000 individuals, or 41% of their reference-period mean. The effect more than

[30] For example, the annual estimate for post-onset methamphetamine deaths (i.e., any deaths with ICD-9 code T43.6) averages 0.13 deaths per 1,000 individuals, and the 95% confidence interval spans -0.38 to 0.64 deaths per 1,000 individuals. Given the reference-period mean of 0.32 deaths per 1,000 individuals, these estimates are not inconsistent with meaningful changes in methamphetamine offenses.

halves in the second year, to 19 offenses per 1,000 individuals, although remains statistically significant at the 5% level. In the longer term, it settles at a statistically insignificant increase of 14 offenses per 1,000 individuals.

We present results for specific types of violent crime in Figure 4 and Table II. None have pre-trends ahead of the heroin supply reduction. There is a striking increase in homicide/manslaughter in the first post-reduction year of nearly four offenses per 1,000 individuals, a 24-fold increase relative to the reference period. To put this increase in context, individuals in the treatment group were charged with one murder in the year prior to the supply reduction and 23 in 2001, accounting for 20% of the total murders in NSW that year. The magnitude of the longer-term estimate is 75-80% lower, although it is still different from the reference period at statistically significant levels.

Assaults are the most common violent offenses in our treatment group, accounting for around two thirds of the violent offenses in the reference year. Serious assaults, which represent nearly half of all assaults, increase immediately after the onset of the heroin supply reduction and remain above pre-reduction levels in the longer term. There are statistically significant increases of 19 offenses per 1,000 individuals in the first year, 12 in the second year, and an annual average of 13 in the final six years of the sample period. Injuries occur in around half of serious assaults, and serious assaults causing injury drive the increase in serious assaults.[31] Common assaults, which account for slightly more than half of all assaults in our treatment sample, also increase after the supply reduction, with statistically significant increases of 38% in the first year and 24% in the longer term. There are no detectable changes in sexual assaults, which account for less than 2% of all assaults. There are also no detectable changes in harassment charges.[32]

[31] Estimates are in Appendix Figure A.8 and Appendix Table A.1. All of the post-reduction estimates for serious assaults causing injury are large (9-15 annual offenses per 1,000 individuals) and statistically significant, while the estimates for serious assaults not causing injury are around one third the size and not statistically significant at conventional levels.

[32] Sexual assault charges include those for rape, attempted rape, indecent assault and threats to commit sexual assault. Harassment charges encompass actions that harass or are intended to harass, threaten or invade the privacy of an individual, but not amounting to an assault or other more serious offense.

In summary, there is a clear increase in violent offenses that dissipates over time. However, some of the most serious violent offenses – homicides and serious assaults causing injury – drive the initial increase and display more persistence than other types of violent offenses. These results may, at least partially, be because opioid users substitute towards drugs more closely connected to violent or impulsive behavior, like methamphetamines and cocaine.[33]

5.4 Property crime

We next consider the impact on property crime, which appears to be a key source of income for individuals in our sample. While robberies are often considered a violent offense, we examine them here given the pecuniary incentives involved and their importance to many drug users who obtain cash to buy drugs. For different property offense outcomes, we present quarterly rates for the treatment and comparison groups, together with regression estimates.

Thus far, with the exception of opioid-related mortality, the treatment and comparison groups have had broadly similar mortality, drug and crime outcomes in the years before the heroin supply reduction. However, as discussed in Section 3, opioid offenders' pre-reduction property offending is substantially higher than that of non-opioid offenders. We address this by using a comparison group of non-opioid users that had at least one "serious" property offense (burglary, motor vehicle theft or robbery) prior to the onset of the heroin supply reduction. We show that the treatment group and this second comparison group have similar rates of serious property offenses before the supply reduction. We therefore mainly rely on that comparison, although for completeness we also present rates and results using the original comparison group. Petty theft, which is higher in the treatment sample than both comparison groups, is considered separately to the more serious types of property crime.

We first present results for all of types of serious property crime offenses in Table II, based on equation (1) and using the property-crime comparison group. Pre-reduction offending rates are flat. In the first year after the onset of the heroin supply reduction, there is a visually

[33] These substances are relevant to the psychopharmacological channel connecting drugs and violent crime discussed by Goldstein (1985).

clear and statistically significant increase in serious property crime of 61 offenses per 1,000 individuals, or 54% of the reference-period mean. The effects then decline and return to baseline levels in the third year and beyond.

We estimate the changes for specific types of property offenses. The quarterly rates for robbery, burglary and motor vehicle theft are presented for the treatment and comparison groups in Figure 5. These highlight the comparability of the treatment group to the property crime comparison group more than the original comparison sample. Regression estimates based on the first comparison are plotted in Figure 5 and summarized in Table II . Pre-reduction trends are flat for each outcome. Robbery increases by a statistically significant 15 offenses per 1,000 individuals in the first year, a 325% increase relative to the reference period. The effect approximately halves in the second year and then halves again over the longer term, with the latter estimate not statistically significant at conventional levels. Burglaries increase initially, but not at statistically significant levels. There is a statistically significant increase in motor vehicle thefts in the first year that is approximately 60% of the reference-period mean; the second-year and longer-term estimates are small and not statistically significant.[34]

Finally, petty/other theft, which is the least serious but most common type of property crime, declines after the supply reduction. While neither comparison sample matches the treatment sample in terms of pre-reduction offending, estimates based on both samples have no pre-trends and produce similar results. In the first year there is a small, statistically insignificant decline in petty/other theft, which is followed by statistically significant longer-term declines where the estimated change of -220 offenses per 1,000 individuals implies that property offending rates approximately halved over time. There are qualitatively similar results for petty/other theft using counterfactuals based on other specifications and using different comparison groups, as shown in the next section. Furthermore, as discussed in Section 5.7, NSW experienced a striking decline in property offenses starting in 2001.

[34] If we use the broader comparison group, the estimates are similar except for longer-term declines in burglary and motor-vehicle theft. These results are shown in Section 5.6.

In summary, we find substantial short-term increases in criminal charges for serious property crimes after the reduction in heroin supply. Most of these effects dissipate over the longer term, while there is a long-term decline in less serious types of theft.

5.5 Robustness of results to alternative specifications and sample definitions

We assess the sensitivity of our results to specification choices and sample restrictions, showing results across several tables in the appendix. We focus on opioid-related mortality as the most direct measure of the change in opioid use, and other key outcomes: all-cause mortality; non-opioid drug use/possession offenses; violent offenses; and serious property offenses.

We first assess the robustness of our results to the specification used, including the role of the covariates used in equation (1). The results are presented in Appendix Table A.2. In column (1), we start with results from a parsimonious version of our equation (1) that has no individual fixed effects or other covariates. In column (2), we add non-time-varying demographic controls (sex, Indigenous status) when using mortality outcomes and individual fixed effects for other outcomes. In column (3), we present our baseline results using equation (1), which involves adding sex- and Indigenous- specific age controls. The results are almost identical across these specifications, suggesting that permanent individual differences and lifecycle effects do not strongly affect our inference. The largest differences are for serious property offenses, with the controls reducing the longer-term post-reduction estimates while leaving the initial spike in serious property crime little changed. In column (4), we replace pre-event year indicators with group-specific linear trends to allow for pre-existing trends in outcomes that differ across the treatment and comparison groups. The results are similar to those in column (3), which is not surprising given there are relatively constant pre-event differences in outcomes across the treatment and comparison groups. Finally, to assess whether time-varying economic conditions have a differential impact on the treatment and comparison groups, we present results in column (5) that include controls for quarterly unemployment rates interacted with treatment group status. Overall, these results are consistent with the results from our baseline specification in column (3).

We next incorporate time-varying policy and non-opioid drug market variables into our specification, and present these results in Table A.3 for the same five key outcomes. We take measures of government services presented in Figure A.5 and interact them separately with the treatment and comparison groups to allow the policy environment to separately affect opioid and non-opioid drug users. We present our baseline results in column (1) of Table A.3, and then present results accounting for the time-varying effects of the number of police officers in column (2); the number of prisoners in column (3); the number of specialized mental health staff in column (4); and the average payment amounts for disability insurance in column (5).[35] Except for slightly larger point estimates for the mortality reductions after the drop in heroin supply, these results do not differ from our baseline estimates.

We also present results with time-varying controls for cocaine and methamphetamine prices. We identify individuals arrested for cocaine use/possession prior to the supply reduction; some are in the treatment sample and some are in the comparison group. We separately interact the cocaine price series presented in Figure A.1 with an identifier for these individuals, and another for other individuals not arrested for cocaine use/possession prior to the supply reduction. We do the same for methamphetamines, allowing the prices of these non-opioids to have distinct effects on individuals based on their pre-reduction use of these drugs. These results are presented in column (6) of Table A.3. The results are largely similar to the baseline results, although the all-cause mortality point estimates are slightly smaller and the changes in violent offenses beyond the first year are slightly larger. In line with Caulkins et al. (2006) finding no change in methamphetamine purity after the heroin supply shock, these results suggest that drug market spillovers played a minor role in its impact.

We also consider the robustness of the results to using different comparison groups. We present these results in Appendix Table A.4, using the same five key outcomes of interest. Columns (1) and (2) show, respectively, the results using the main comparison group of

[35] Of the policy measures presented in Figure A.5, we focus on the ones with relatively large changes over our sample period. Adding the other policy variables in similar ways do not change our estimates.

non-opioid offenders and the subset of non-opioid with a serious property offense prior to the heroin supply reduction. These represent a combination of the main results presented thus far, and therefore it is not surprising that the estimates are similar across these columns, except for the lack of persistent serious property crime effects once we focus on the more comparable set of non-opioid offenders (who also had committed a serious property crime). Columns (3) and (4) present results when there is no comparison group; the latter results allow for a time trend in the outcome of interest. These estimates are based on an event-study specification where the estimates come from indictor variables for each event-time year preceding and following the onset of the heroin supply reduction.[36] They are similar to the main ones, except that the mortality reductions are slightly larger and there are persistent increases in serious property crime. These estimates suggest that are main results are not strongly influenced by the use of a comparison group. Finally, in the last column, we present estimates based on equation (1) when using a broader set of pre-reduction offenders in addition to non-opioid drug users. These results are again qualitatively similar to the main results, apart from larger mortality reductions and more persistent serious property crime estimates. Overall, the patterns in Appendix Table A.4 ameliorate concerns that our results may be attenuated by having opioid users in the comparison group, or by spillovers affecting non-opioid illicit drug markets.

Finally, we present results in Appendix Table A.5 for petty/other theft using the same comparison-group variations, given that opioid users look different to other individuals in our data in terms of this outcome. Across these results, there is no pre-trend in petty/other theft. The estimates for the long-term decline are also statistically significant and generally of a similar magnitude, with the exception that adding a time trend inflates the magnitude of the estimate by around 40%. In general, these results suggest that there was a meaningful decline in petty/other theft the supply reduction.

[36] The individual fixed effects are dropped, as event time and the age controls are now collinear at the individual level (Borusyak and Jaravel 2017). The additional time controls now consist of quarter-of-year fixed effects and a dummy for the fourth quarter of 2000, while we also include permanent measures of sex and Indigenous status interacted with a complete set of age fixed effects. We allow for an arbitrary correlation in errors over time at the individual level.

5.6 Heterogeneous effects by different types of opioid users

We next consider how our results differ by opioid users' demographic and other characteristics. To do this, we interact an indicator for a specific subgroup to those for each event-time year from equation (1) to measure heterogeneity. We focus on the same key outcomes: opioid-related mortality; all-cause mortality; non-opioid drug use/possession offenses; violent offenses; and serious property offenses. For the serious property offense outcome, we continue to use the comparison group of non-opioid drug offenders a pre-shock serious property offense. We focus on effects in the first post-onset year to understand the short-term changes, and the avaerage annual estimate for the last six years to understand the longer-term changes.

We first examine differences by sex and age. There is mixed evidence on how opioid use and its risks varies by sex and age.[37] Columns (1) and (2) of Table III show the results for females and males, respectively. They have similar mortality responses. For non-opioid drug use/possession, they differ in their short-term response: males and females have short-term increases of 19 and 5.7 offenses per 1,000 individuals, respectively, with a p-value of 0.08 on the hypothesis test that the coefficients are equal. Their longer-term estimates are similar for this outcome. The sex-specific results for violent and serious property offenses are qualitatively similar to those for non-opioid drug/use possession. The short-term increase in violent offenses per 1,000 individuals is 53 for males and 24 for females (p-value on equality: 0.09). For serious property offences, the short-term increase for males is much larger than for females (62 versus 11 offenses; p-value on equality: 0.01). Pre-existing differences in offending explain little of these results.[38] For both violent and serious property offenses, there are no meaningful longer-term differences between females and males.

[37]For example, while Degenhardt, Conroy, Gilmour and Hall (2005) found that drug treatment patterns after the heroin supply shock were similar for males and females, Stoové et al. (2009) tracked the fatal overdose risks for Australians who had a non-fatal opioid overdose and found males had higher conditional mortality risks. Likewise, these studies find that the differences by age are mixed, with larger changes in treatment after the supply shock for the young versus the old, and higher conditional mortality risks among older opioid users (Degenhardt, Conroy, Gilmour and Hall 2005, Stoové et al. 2009).

[38]For violent and serious property crime, males commit offenses at higher rates than females in the reference period (45% more for violent and 51% for serious property offenses). However, the differences in the first-year coefficients are much larger than these differences.

We then use close to the median age of individuals at the time of the supply shock to split the sample into those who were younger (29 years or younger) and older (30 years or older). The results are presented in columns (3) and (4) of Table III. There are no meaningful differences by age in terms of opioid-related mortality, all-cause mortality, or non-opioid drug use/possession. However, younger opioid users have larger short-term increases in violent offenses than older ones (62 versus 26 offenses; p-value on equality: 0.02). Younger opioid users also account for nearly all of the short-term increase in serious property offenses (86 versus 3.5 offenses; p-value on equality: <0.01). There are no meaningful differences in these outcomes in the longer term.

We next consider the time between first opioid offense and the onset of the heroin supply reduction. Mortality risk has been found to increase with the time since first using opioids, while some studies find that criminal activity decreases over the course of an opioid addiction (Reuter 2010, Degenhardt et al. 2011). We split the treatment sample into those first arrested for opioids in 1995-1997 and those arrested 1998-2000, reporting their respective estimates in columns (5) and (6) of Table III. Individuals first arrested for opioid use/possession in 1995-1997, several years before the onset of the heroin supply reduction, have larger first-year opioid-related mortality reductions than more recent first-time opioid offenders (-4.2 versus -1.2 deaths per 1,000 individuals, p-value on equality: 0.04). This difference is also present for all-cause mortality in the first year (-5.0 versus -0.1 deaths per 1,000 individuals, p-value on equality: 0.01). These mortality differences disappear in the longer term, with both groups experiencing decreases in opioid-related and all-cause mortality. Both those first arrested for opioids in 1995-1997 and 1998-2000 have broadly similar responses in terms of non-opioid drug use/possession, violent and serious property offenses. The absolute and relative increases in these three outcomes after the supply shock are slightly higher for the 1995-1997 group than the 1998-2000 group, although the differences are not statistically significant.

We therefore have crime increases driven by those aged below 30 at the start of the heroin reduction, but not by more-recent opioid arrestees (who are generally younger than earlier opioid arrestees). We make sense of these results by splitting the sample based on the median age of first opioid offense, which is 26 years. In Table III, we present results for younger first-time opioid arrestees (aged 26 or younger when first arrested) in column (7) and older first-time opioid arrestees (aged 27 or older when first arrested) in column (8). While they have similar mortality reductions, our point estimates suggest that younger arrestees have larger short-term increases in non-opioid drug use/possession and violent offenses, although these differences are not statistically significant (p-values on equality: 0.29 and 0.13). For these outcomes, they are similar in the longer term. The differences in serious property offenses are starker and more persistent: younger arrestees account for nearly all of the short-term increase (88 versus 10 offenses for older arrestees, p-value on equality: <0.01) and any persistence in the longer term (9.4 versus -26 offenses, p-value on equality: 0.02). Thus, individuals arrested for using or possessing opioids at younger ages drive the increases in offending after heroin supply is reduced. Even if first arrested several years before its onset, many of these individuals are still relatively young at the time it occurred and account for opioid users in their 20s driving the post-reduction increases in offending.

We also consider whether outcomes differ by measures of the intensity of opioid use before the heroin supply reduction, presenting these results in Appendix Table A.6. We consider (i) the number of times an individual was arrested for using or possessing opioids; (ii) whether the individual participated in medication-assisted treatment (i.e., methadone); and (iii) for those that participated in drug treatment, the maximum dose of methadone prescribed.[39]

The number of opioid arrests should be a function of drug use, although we can only split our treatment group into those arrested for opioid use/possession once and those arrested for it multiple times. Across these two groups, there are no meaningful differences in our key

[39]The first measure closely aligns with our sample selection criteria, although many individuals only have one such offense. Methadone participation, which is the second measure, typically occurs after some time using opioids; drug treatment may also have some protective effects in terms of reducing overdose risk and criminal activity (Reuter 2010, Degenhardt et al. 2011, Swensen 2015, Bondurant et al. 2018). The third measure, based on methadone dosing, aligns with drug treatment guidelines around dosing increasing with opioid use (Burns et al. 2009).

outcomes. Next, we consider participation in drug treatment. Opioid users typically participate in methadone treatment some years after first becoming dependent on the drug; for example, in a longitudinal study of heroin users recruited from medication-assisted treatment in NSW, the average difference between the age of first opioid use and age of first drug treatment was six years (Larance et al. 2018). In our setting, we find that those who participated in drug treatment experience lower mortality reductions in the short term. They also have persistently higher violent offending than those who did not, with statistically significant differences in the long-term effects. The mortality results are consistent with methadone treatment being protective, lessening the impact of reduced heroin availability (Degenhardt et al. 2011, Manski et al. 2001). For our third split, we find that—conditional on drug treatment participation—higher maximum methadone doses are associated with larger mortality reductions in the first year, but are otherwise not associated with meaningful differences in outcomes.

In summary, there is some important heterogeneity in responses to the heroin supply shock. The mortality reductions are reasonably uniform, although the first-year mortality reductions were larger among earlier opioid arrestees; those who did not participate in treatment; and those whose methadone treatment dosing suggested high levels of opioid addiction. Increases in criminal activity were concentrated among opioid users who were male; young when first arrested for opioid use/possession; and younger at the time of the supply reduction. This helps to understand who may be most sensitive to heroin supply changes, and how treatment and law enforcement activity can affect mortality risks and criminal activity.

5.7 Evidence of broader changes in drug use and crime

Our focus is on individuals identified as opioid users by their criminal charges. There is a strong public policy interest in understanding how such individuals are affected by the supply of illicit drugs; in our context, they had high mortality rates and substantial levels of criminal activity. However, it is also helpful to understand how the effects we estimate relate to other opioid users in NSW at the time of the supply reduction, and especially whether any changes in our sample were offset by broader changes among individuals outside of our sample.

We first consider mortality. As already discussed in Section 3 and shown in Appendix Figure A.7, the relative changes in the NSW opioid-related mortality rates are similar to those found for our treatment group. This suggests that opioid users not arrested for using or possessing opioids experienced a broadly similar change in overdose risks to our treatment group, although it is not possible to make any inference about their overall mortality.

We can also compare our estimates to the full set of criminal charges in NSW. In Appendix Figure A.9a, we first show the opioid use/possession charges per 10,000 individuals in NSW, and then the counterfactual rates without charges for individuals in our sample. By definition, our sample accounts for all of these offenses before the heroin supply reduction. The post-reduction counterfactual rates are flat, with the actual drop fully driven by our sample. This suggests that there was no offsetting increase in opioid charges outside of our sample.

We also present the same actual and counterfactual offense rates for violent and property offenses in Appendix Figure A.9. The actual and counterfactual aggregate violent crime rates are shown in Figure A.9b. Our estimates imply only a small increase in aggregate violent offenses in 2001, and no measurable longer-term impacts. Moreover, there are no discernible changes in the underlying trend in violent offenses, suggesting that responses to the heroin supply reduction beyond our sample had little impact on aggregate levels of violent crime. The actual and counterfactual aggregate property crime rates are shown in Figure A.9c. Aggregate property crimes peak in 2001, the first year of the supply reduction. This is consistent with the patterns we see in our sample. Beyond 2001, there is a drop in aggregate property crime rates, with our sample of opioid users having a non-trivial impact on this long-term decline. The roughly 25% decrease in the NSW property crime rate between 2001 and 2005 makes a halving of petty/other theft in our sample plausible, given we have individuals who are were frequently arrested for property crimes ahead of the reduction in heroin supply. Furthermore, while we have a limited ability to determine exactly what state-levels changes result from the reduction in heroin supply, the aggregate patterns certainly do not suggest any offsetting property-crime effects by individuals outside of our sample.

6 Conclusion

We provide new insights as to how opioid users respond to a long-term reduction in heroin supply. We estimate meaningful changes in their mortality risks, drug use and criminal offending. A few features help us to develop more precise estimates of the impact of an illicit drug supply shock than have been possible in other settings. First, the reduction in supply is large and long-lasting relative to other supply shocks. Second, individual-level data allow us to account for individual factors, such as lifecycle effects. Third, we follow opioid users for several years. This allows us to understand both the immediate and longer-term effects.

There are several key findings. We find large and persistent reductions in opioid-related and all-cause mortality. There is substitution to cocaine and methamphetamines, and methamphetamine remains elevated. This substitution, however, does not result in detectable increases of non-opioid drug deaths. As a result, all-cause mortality rates fall by around 30% in the longer term. To put this in perspective, we estimate that the the supply shock saved the lives of around one in every 48 individuals in our sample over the subsequent eight years.

However, most criminal activity increases in response to the heroin supply shock. Some of the most serious violent offenses—homicides and serious assaults causing injury—drive an initial 41% increase in violent crime and display some persistence. Serious property crimes—robberies, burglaries and motor vehicle thefts—increase by 46% in the first year, then return to baseline levels in the longer term. There is a long-term decline in petty/other theft.

The effects vary across outcomes and over time, so we value them and calculate a net present value at the start of the supply reduction. We use Australian estimates of the value of statistical life (Viscusi 2018), and value crime using Australian Institute of Criminology reports that include estimates of the extent of under-reporting to police (Mayhew 2003, Smith et al. 2014). No value is attributed to drug-defined crimes, such as drug use offenses. We combine this information with annual point estimates from our main results, use a 2% discount rate, and scale the estimates to 2022 dollars. More details are provided in the appendix.

On this basis, our back-of-the-envelope estimates are that the heroin supply shock results in net savings of around A$169,700 (US$120,500) per individual opioid user in our sample. The net value in the first year is a cost of A$4,890 (US$3,470) per user, with crime costs outweighing mortality reductions. However, from the second year, the mortality reductions dominate changes in crime, resulting in average discounted savings over the remaining seven years of A$24,240 (US$17,210) per year.[40] This exercise highlights that the heroin supply reduction had mixed effects on mortality and crime immediately after its onset. It further emphasizes that, in the longer term, the heroin supply reduction produced substantial benefits in terms of increasing life expectancy, without large persistent increases in serious crime.

Given that the state-level changes in opioid-related mortality are similar in relative terms to what we observe for our sample, and the fact that there are not offsetting aggregate changes in crime rates, reduced heroin supply may have delivered tens of billions of dollars in savings through lower mortality.[41] More generally, our findings demonstrate that is it important to assess supply-side interventions over several years, as the immediate effects may not reflect the direction or magnitude of longer-term responses.

There are several limitations to the analysis. First, the administrative data do not allow us to gain a full understanding of the opioid users in our sample. We have only coarse measures of their drug use, addiction and health, and cannot assess other outcomes. As a result, we cannot evaluate the full welfare implications for these individuals. Nor can we determine the fraction actively using opioids at the time of the supply shock. Some individuals may have ceased using opioids before the supply shock, although the opioid-related mortality rates in the years before it suggest opioid use was fairly consistent.[42] Second, we are missing opioid

[40] In Appendix C, we consider the effects of using different valuations: a higher value of statistical life more in line with US estimates from Viscusi and Masterman (2017), and incorporating recent estimates of violent-crime victimization costs in Australia from Johnston et al. (2018). The estimates do not depend on how we treat petty/other theft, as a much lower valuation is placed on it than other outcomes in this study.

[41] For context, over the 1996-2000 period, our sample of accounts for around 8.5% of opioid-related deaths in NSW and around 2% of opioid-related deaths in Australia.

[42] That is, we expect that "treatment-on-the-treated" estimates would be similar or only slightly higher than our "intent-to-treat" estimates, where inclusion in the sample is based on an opioid offense and the being treated by the supply shock requires active (or potential) opioid use at the time it occurred.

users not charged with opioid-related criminal offenses, which makes it difficult to assess the overall impact on mortality risks and criminal activity. Third, the heroin market disruption in Australia could have affected other countries, such as by increasing heroin supply to other markets. Such changes have not been detected, but would affect its net impact.

There are also limitations in terms of external validity. Heroin was the main opioid consumed in Australia in the early 2000s, and fewer opioids were available than are generally available in most countries now. It is also the case that the "harder" illicit drugs were more expensive than heroin prior to the supply reduction, implying that the substitution possibilities may have been more limited than if cheaper options were available. That said, many illicit drug epidemics appear to occur because a particular drug delivers a much cheaper "high" than has previously been available (Moore and Pacula 2021). An additional caveat is that we study a supply reduction associated with heightened drug-interdiction activities aimed at international trafficking networks. Supply reductions resulting from local retail-market crackdowns or incarcerating dealers may have different effects.

Reflecting on his experience chairing a National Research Council report on drug policy data and evidence (Manski et al. 2001), Charles Manski (2003, pp. 543-4) wrote:

> I regret that... the nation is in no better position to evaluate the effectiveness of enforcement now than it was 20 years ago... I worry that the necessary investments still not having been made, the nation will continue to be engaged 20 years from now in uninformed debate about the effectiveness of its drug control policy.

Recent reviews suggest that this concern was valid (Smart et al. 2020, Maclean et al. 2022). The large reduction in heroin supply in Australia and the data available for this paper enable us to generate new insights into how opioid users respond to reduced supply, and more general insights into the relationships between drugs, health and crime. Hopefully, such findings can aid in developing policies that mitigate the terrible effects of the current opioid crisis, as well as dealing with other drug epidemics.

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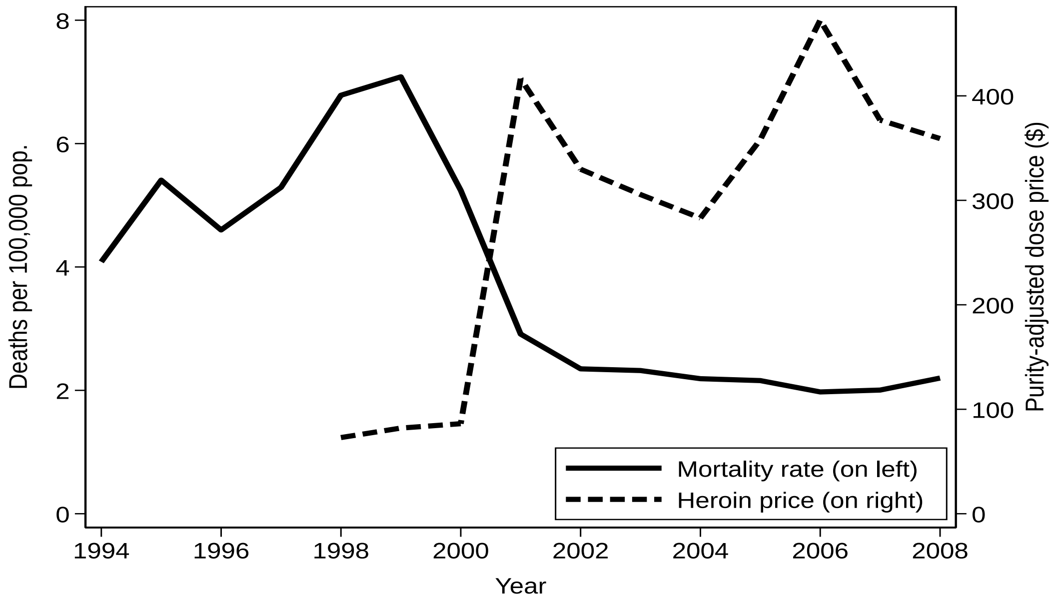
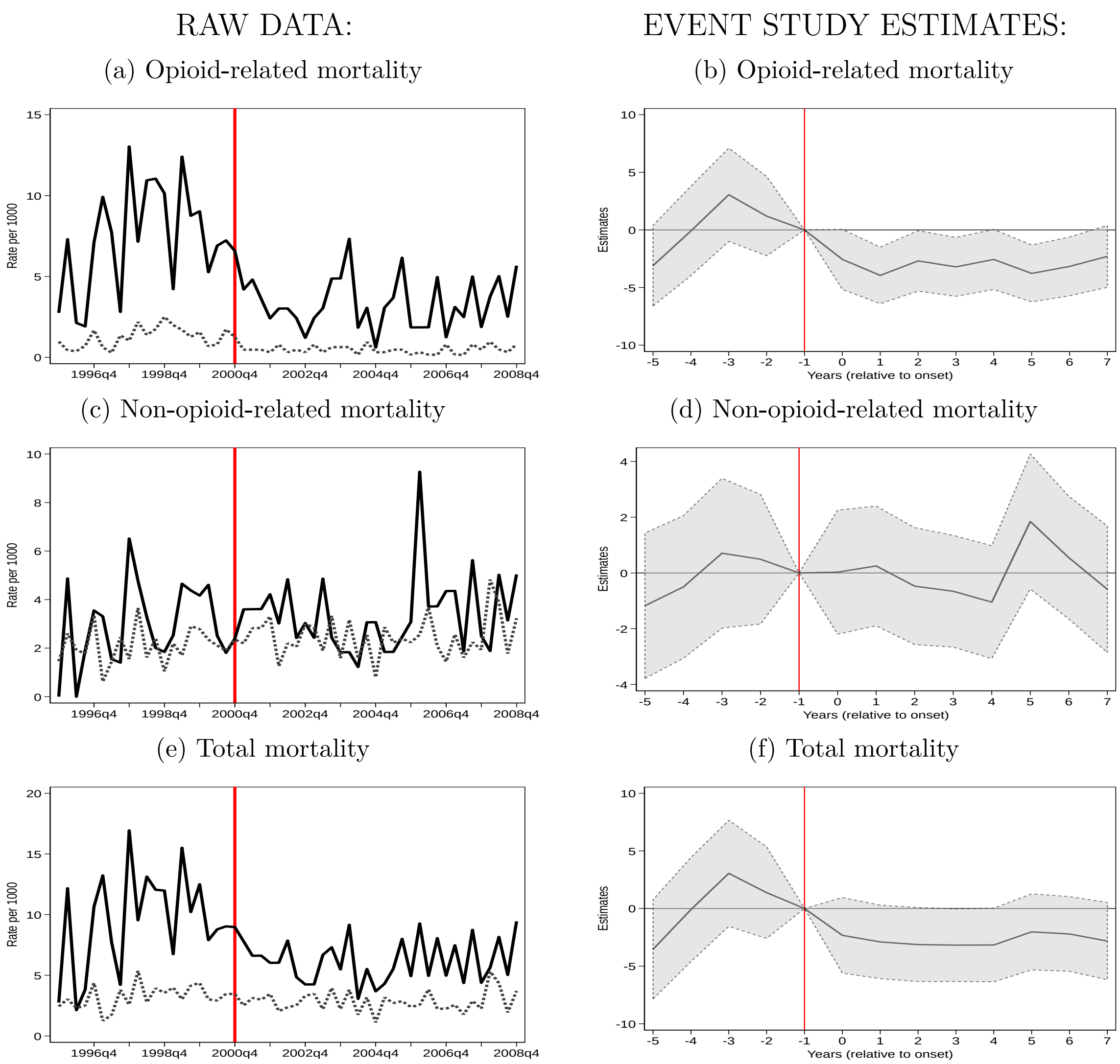
Figure 1: Opioids-related deaths and the heroin price in New South Wales, Australia

(a)



Notes: Pure heroin prices come from reported prices paid divided by average heroin purity. Data on the prices paid for a dose ("cap") of heroin are from the Illicit Drug Reporting System and purity data are from the Victoria Police Forensic Science Centre. Data are not available prior to 1998. The methodology is similar to other studies examining illicit drug prices (e.g., Moore 2006, Arkes et al. 2008). The NSW opioid-related mortality rate uses data from the Australian Institute for Health and Welfare. Opioid-related deaths are defined in accordance with the Australian Bureau of Statistics (2002). Prior to 1997, opioid-related deaths are those with ICD-9 underlying-cause-of-death codes E850.0, E850.8, E850.9, 304.0, 304.7, 304.9, 305.5 or 305.9. From 1998, opioid-related deaths are those with an ICD-10 underlying-cause-of-death code of X42, X44, X62, X64, X85, Y12, or Y14 together with an opioid drug identification code of T40.0-T40.4. These ICD codes cover 91% of the opioid overdoses defined by the CDC (Ahmad et al. 2023). US mortality rates data are calculated using the Centers for Disease Control and Prevention's WONDER database.

Figure 2: Mortality outcomes

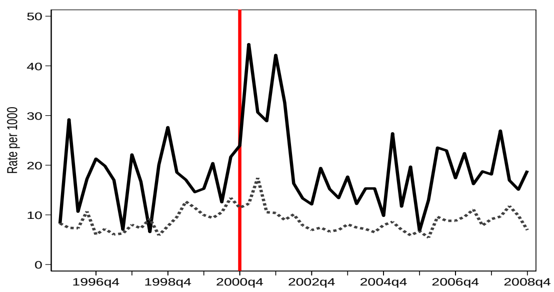


Notes: The left column shows unadjusted quarterly rates for our sample of opioid users and control group of non-opioid drug offenders. A vertical red line marks the onset of the heroin supply reduction in 2000-Q4. The right column shows the estimated coefficients and 95% confidence intervals for the event-year indicator variables from Eq (1) in Section 3. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, which is identified with a vertical red line. The mortality outcomes are estimated without individual fixed effects and extra demographic controls. Rates in all panels are scaled to represent annual outcomes per 1,000 individuals.

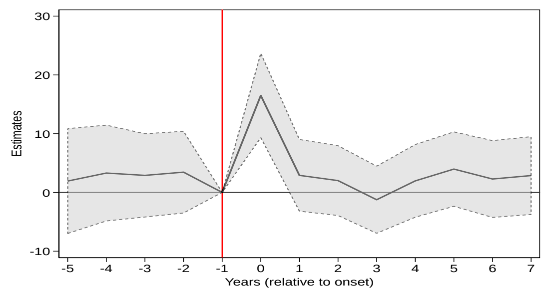
Figure 3: Non-opioid hard drug outcomes

RAW DATA: | EVENT STUDY ESTIMATES:

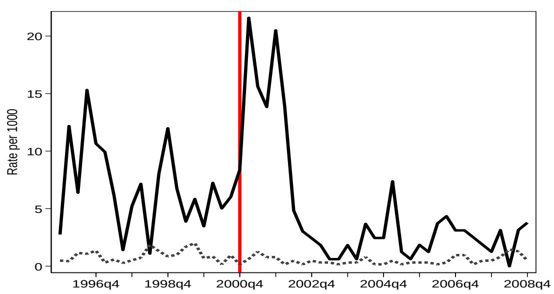
(a) Non-opioid hard drug use/possession



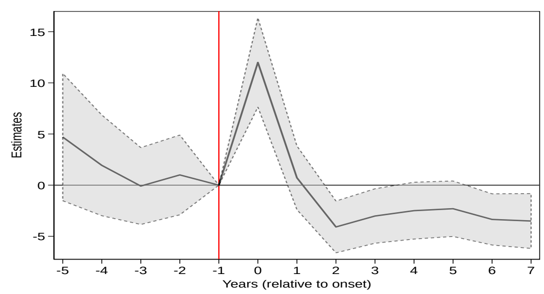
(b) Non-opioid hard drug use/possession



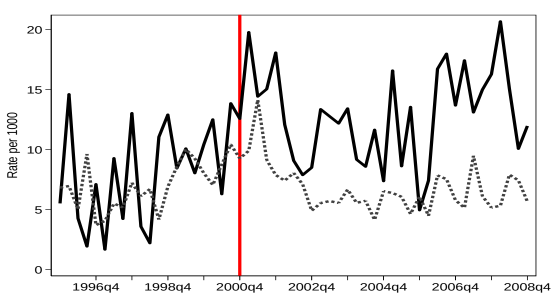
(c) Cocaine use/possession



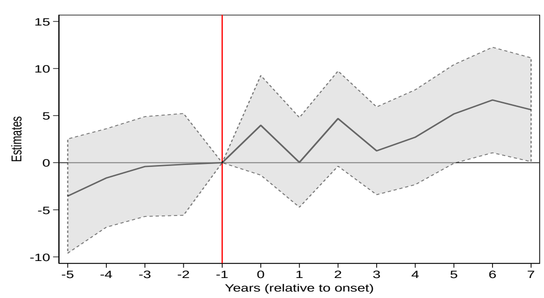
(d) Cocaine use/possession



(e) Methamphetamine use/possession



(f) Methamphetamine use/possession



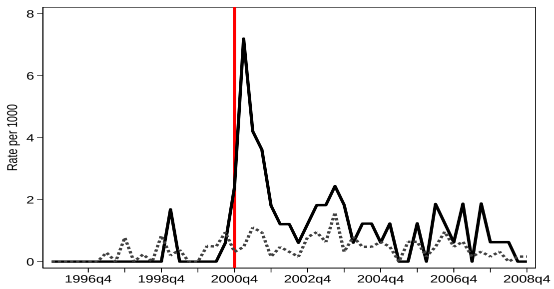
Notes: The left column shows unadjusted quarterly rates for our sample of opioid users and control group of non-opioid drug offenders. A vertical red line marks the onset of the heroin supply reduction in 2000-Q4. The right column shows the estimated coefficients and 95% confidence intervals for the event-year indicator variables from Eq (1) in Section 3. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, which is identified with a vertical red line. Rates in all panels are scaled to represent annual outcomes per 1,000 individuals.

Figure 4: Violent crime outcomes

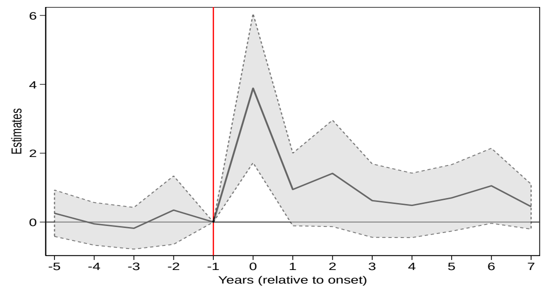
RAW DATA:

EVENT STUDY ESTIMATES:

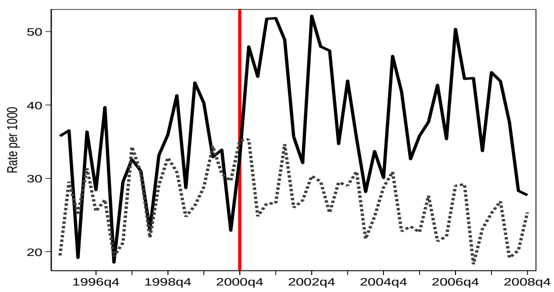
(a) Homicide and manslaughter



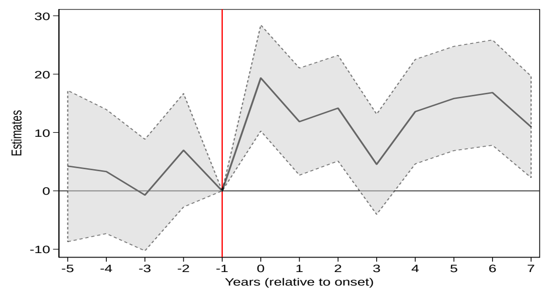
(b) Homicide and manslaughter



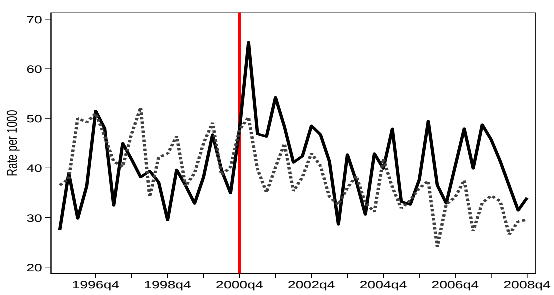
(c) Serious assaults



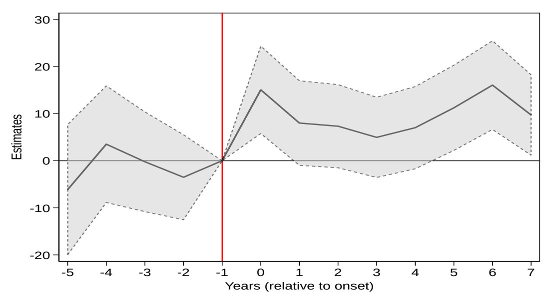
(d) Serious assaults



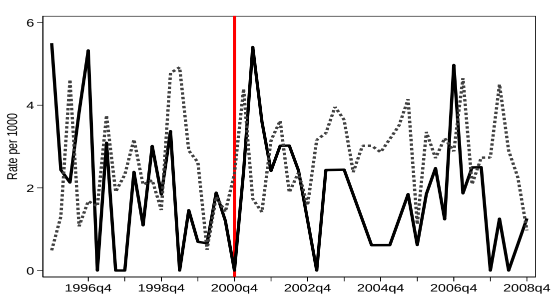
(e) Common assault



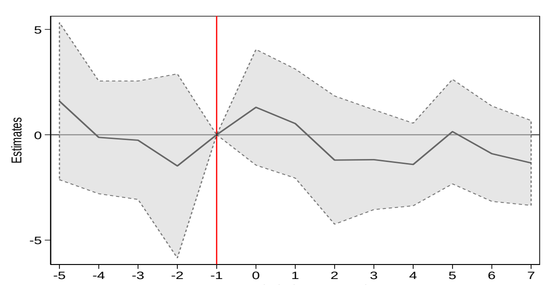
(f) Common assaultt



(g) Sexual assaults

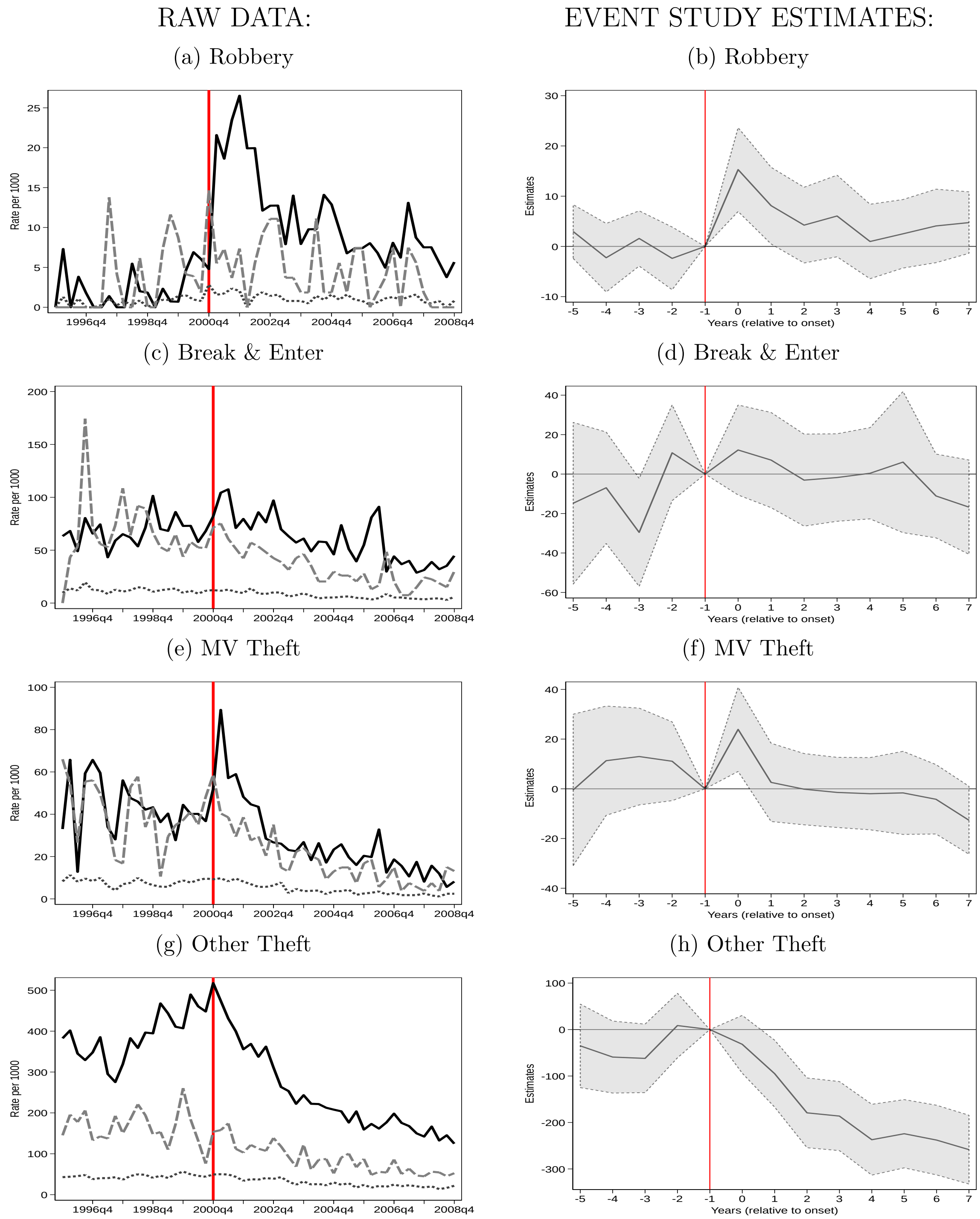


(h) Sexual assaults



Notes: The left column shows unadjusted quarterly rates for our sample of opioid users and control group of non-opioid drug offenders. A vertical red line marks the onset of the heroin supply reduction in 2000-Q4. The right column shows the estimated coefficients and 95% confidence intervals for the event-year indicator variables from Eq (1) in Section 3. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, which is identified with a vertical red line. Rates in all panels are scaled to represent annual outcomes per 1,000 individuals.

Figure 5: Property crime outcomes



Notes: The left column shows unadjusted quarterly rates for our sample of opioid users and two separate control groups of non-opioid drug offenders. The dotted line depicts rates among the non-opioid drug user control group. The dashed line represents rates among a subsample of our non-opioid drug offender group who were charged with a serious property offense (robbery, break & enter, motor vehicle theft) prior to the onset of the heroin supply reduction. A vertical red line marks the onset of the heroin supply reduction in 2000-Q4. The right column shows the estimated coefficients and 95% confidence intervals for the event-year indicator variables from Eq (1) in Section 3. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, which is identified with a vertical red line. Rates in all panels are scaled to represent annual outcomes per 1,000 individuals.

Table I: Mean sample characteristics prior to heroin supply reduction

	Opioid drug offenders	Non-opioid drug offenders	Non-opioid drug & property offenders
Demographic characteristics			
Age	30.2	30.2	25.2
Female	0.21	0.13	0.056
Annualized incidence rates per 1000 individuals			
Mortality	9.94	3.35	5.68
Opioid-related mortality	6.84	1.21	2.84
Non-opoid drug or alchol related mortality	0.097	0.077	0.16
Mortality not related to drugs or alcohol	3.01	2.06	2.68
Illicit drug offending:			
Opioid use/possession	353.5	0	0
Non-opioid illicit drug use/possession	17.9	9.29	14.8
Cocaine use/possession	6.55	0.89	0.79
Methamphetamine use/possession	9.07	7.46	12.8
Opioid supply/distribution	1.12	0	0
Non-opioid illicit drug sale/distribution	0.29	0.22	0.47
Violent offending	108.8	94.7	219.5
Homicide and manslaughter	0.34	0.29	0.63
Serious assault	32.6	28.7	75.2
Sexual assault	1.55	2.37	3.31
Common assault	39.0	43.3	97.1
Harassment	2.18	2.98	7.41
Property offending	591.1	79.6	309.0
Robbery	2.76	0.87	4.89
Burglary/break & enter	70.2	12.1	64.5
Motor vehicle theft	42.4	7.93	39.7
Petty/other theft	413.7	45.7	161.9
No. of unique individuals	6,886	25,814	2,216

Notes: This table presents mean characteristics for the five years prior to the reduction in heroin supply (1995:Q4-2000:Q3) for our sample of opioid users and the comparison group of non-opioid drug offenders. Our sample of opioid users comprise everyone with an opioid possession charge from 1994 (earliest available data) to the third quarter of 2000. Our comparison sample consists of all individuals with a non-opioid illicit drug charge in the criminal charge database during the same time period who never entered opioid treatment and did not have an opioid-related criminal offense. Key outcome variables are presented in terms of annualized incidence rates per 1,000 individuals.

Table II: Estimated changes in outcomes (per 1,000 individuals)

		Coefficients (standard errors)							
Outcome	Avg. for treatment group year -1	Years -5 to -2		Year 0		Year 1		Years 2 to 7	
		A. Mortality							
Opioid-related	7.08	0.27	(1.34)	-2.56	(1.33)	-3.95**	(1.25)	-2.95*	(1.13)
Non-opioid	3.22	-0.12	(0.94)	0.03	(1.13)	0.25	(1.10)	-0.06	(0.85)
Any cause	9.50	0.20	(1.58)	-2.33	(1.67)	-2.90	(1.63)	-2.76*	(1.36)
		B. Non-opioid drug use/possession							
All hard drugs (excl. opioids)	17.54	2.89	(2.88)	16.50**	(3.66)	2.91	(3.11)	1.98	(2.50)
Cocaine	5.47	1.89	(1.66)	12.00**	(2.23)	0.72	(1.59)	-3.13*	(1.20)
Methamphetamine	10.78	-1.44	(2.16)	3.97	(2.70)	0.03	(2.43)	4.35*	(2.07)
		C. Violent offenses							
Violent (excl. robbery)	113.14	-3.57	(7.86)	46.83**	(9.56)	18.51*	(9.00)	13.96	(6.97)
Homicide & manslaughter	0.16	0.09	(0.30)	3.89**	(1.10)	0.95	(0.54)	0.79*	(0.32)
Serious assault	32.19	3.45	(3.87)	19.34**	(4.64)	11.86*	(4.68)	12.65**	(3.40)
Common assault	39.75	-1.59	(4.18)	15.06**	(4.74)	7.99	(4.58)	9.38*	(3.54)
Sexual assault	1.13	-0.07	(1.17)	1.30	(1.40)	0.53	(1.32)	-0.98	(0.85)
Harrassment	2.58	0.67	(1.33)	1.25	(1.89)	1.59	(1.60)	1.48	(1.39)
		D. Property offenses (*with alterntative control group*)							
Serious property offenses (incl. robbery)	112.66	-1.40	(14.14)	51.41**	(16.22)	17.79	(16.15)	-4.28	(13.10)
Robbery	4.67	-0.02	(2.50)	15.29**	(4.26)	8.11*	(3.89)	3.77	(2.88)
Burglary	67.76	-10.12	(10.50)	12.21	(11.64)	7.10	(12.31)	-4.37	(10.10)
Vehicle theft	40.23	8.74	(7.99)	23.91*	(8.64)	2.58	(8.03)	-3.68	(6.52)
Petty/other theft	451.92	-36.93	(32.83)	-31.73	(31.75)	-94.89*	(36.76)	-220.56**	(36.06)

* $p < 0.05$, ** $p < 0.01$.

Notes: This table summarizes the estimated effects of the heroin reduction. We present coefficients and standard errors for the year indicator variables in the event study specification described by Eq (1). For the mortality outcome in column (1), we replace individual fixed effects with demographic controls. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, denoted as Year -1. All estimates are scaled to represent annual outcomes per 1,000 individuals. Standard errors are robust to arbitrary correlation within individuals over time; standard errors are calculated using the delta method for multi-year estimates. Each regression uses 1,459,907 observations in Panels A, B, and C, and 385,185 observations with the alternative control group in Panel D. To help understand the relative size of the effects, we show the treatment group mean for each outcome variable in the reference period (Year -1).

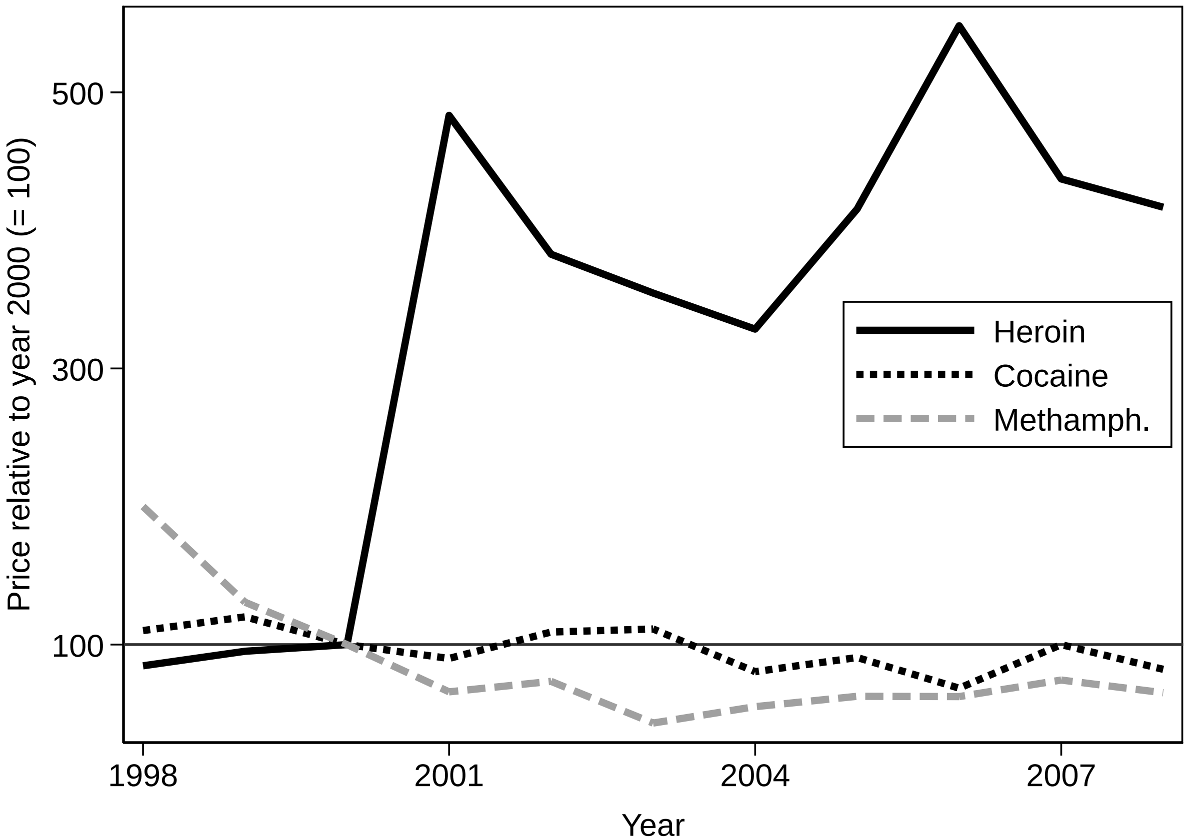
Table III: Heterogeneity of effects by gender, age and recency of opioid offenses

	Gender		Age at time of supply shock		Year of first opioid offense		Age at first opioid offense	
	Female	Male	< 30	>= 30	1995 - 1997	1998 - 2000	Below med. (≤ 26)	Above med. (> 26)
					A. Opioid-related mortality			
Short term (yr 0)	-1.72	-2.78*	-2.60	-2.50	-4.19**	-1.18	-2.68	-2.42
	(2.07)	(1.38)	(1.46)	(1.62)	(1.36)	(1.63)	(1.49)	(1.58)
Longer term (yrs 2-7)	-3.19*	-2.88*	-3.18*	-2.62*	-3.13*	-2.79*	-3.14*	-2.73*
	(1.25)	(1.14)	(1.15)	(1.20)	(1.17)	(1.17)	(1.16)	(1.18)
Avg in yr -1	7.58	6.95	6.49	7.86	7.16	7.00	6.53	7.68
					B. All-cause mortality			
Short term (yr 0)	-2.36	-2.32	-2.52	-2.07	-5.01**	-0.07	-3.15	-1.44
	(2.44)	(1.77)	(1.83)	(2.15)	(1.76)	(2.08)	(1.82)	(2.11)
Longer term (yrs 2-7)	-3.12*	-2.65	-3.18*	-2.15	-2.51	-2.97*	-3.08*	-2.39
	(1.53)	(1.38)	(1.38)	(1.49)	(1.43)	(1.41)	(1.39)	(1.46)
Avg in yr -1	10.62	9.19	8.47	10.85	9.44	9.55	8.08	11.02
					C. Non-opioid drug use/possession			
Short term (yr 0)	5.65	19.38**	19.39**	12.63*	15.69**	16.74**	19.88**	12.86*
	(7.20)	(4.04)	(4.76)	(5.11)	(4.72)	(5.09)	(4.96)	(4.90)
Longer term (yrs 2-7)	-3.97	3.49	4.48	-1.34	5.82	-1.74	4.92	-1.19
	(5.39)	(2.70)	(3.13)	(3.64)	(3.23)	(3.43)	(3.20)	(3.52)
Avg in yr -1	20.47	16.75	17.23	17.96	14.00	21.00	15.54	19.70
					D. Violent offenses (excl. robbery)			
Short term (yr 0)	24.02	52.85**	62.03**	26.07*	55.94**	38.55**	58.85**	33.89**
	(14.76)	(10.96)	(13.33)	(11.05)	(13.42)	(12.25)	(14.21)	(10.75)
Longer term (yrs 2-7)	10.74	14.62	15.99	11.38	18.64*	9.42	13.84	14.12
	(10.99)	(7.86)	(9.28)	(8.63)	(8.70)	(9.38)	(9.81)	(8.30)
Avg in yr -1	83.41	121.15	130.18	90.56	102.26	123.78	134.87	89.80
					E. Serious property offenses (robbery, burglary, vehicle theft)			
Short term (yr 0)	11.39	62.19**	86.07**	3.47	52.33**	49.39*	88.48**	9.85
	(20.46)	(17.51)	(20.21)	(16.65)	(18.30)	(19.76)	(20.86)	(17.04)
Longer term (yrs 2-7)	-24.60	1.66	3.53	-16.52	18.31	-22.42	9.41	-26.16
	(19.42)	(13.96)	(15.19)	(14.79)	(15.62)	(14.32)	(15.42)	(14.92)
Avg in yr -1	80.38	121.35	139.78	76.71	84.67	140.00	139.22	84.13

* $p < 0.05$, ** $p < 0.01$. *Notes*: This table summarizes the heterogeneous effects of the heroin reduction. We present estimated coefficients for selected treatment group by year indicator variables in the event study specification described by Eq (2) in Section 3, which are further interacted with indicator variables identifying our subgroups of interest (e.g., males and females). Specification and sample notes from Table II apply.

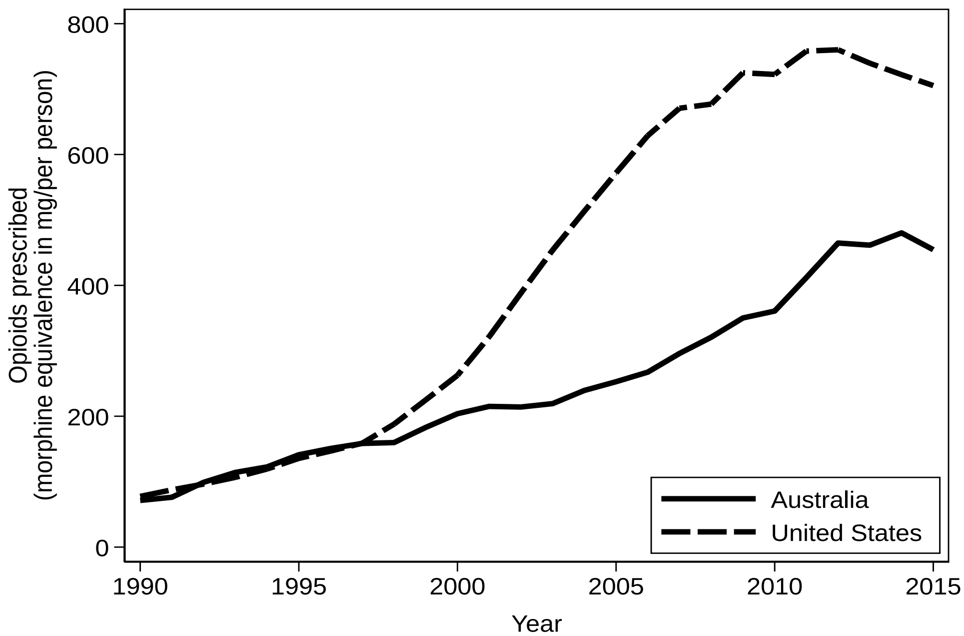
Appendix A: Additional Figures and Tables

Figure A.1: Prices of heroin, cocaine and methamphetamine, relative to year 2000



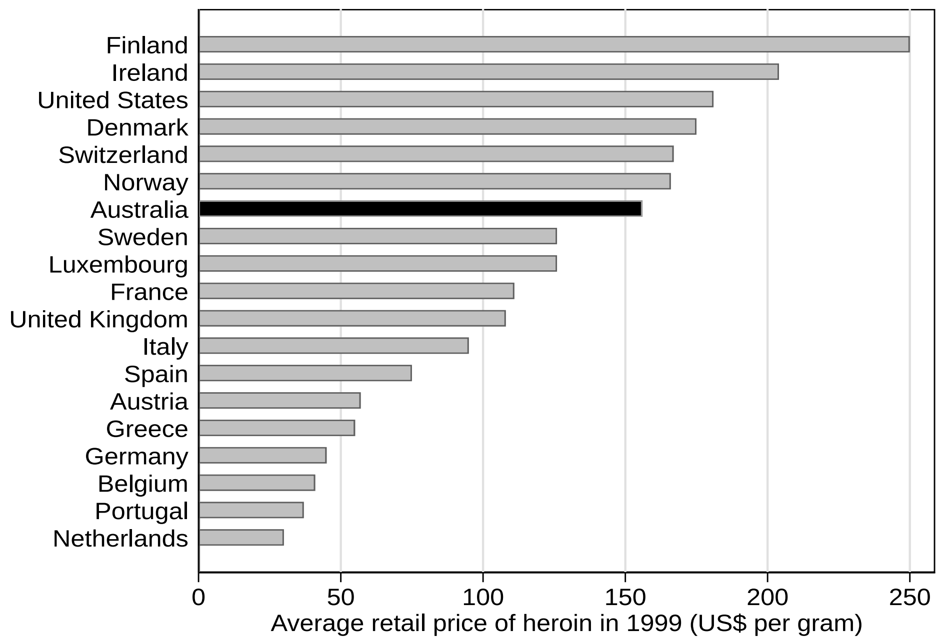
Notes: These are purity-adjusted prices for doses of heroin, cocaine and methamphetamine relative to the year 2000. They are constructed by dividing retail prices (from a survey of regular injecting drug users) by the average purities (from police forensic analyses of retail-level quantities). We use annual survey data on the prices paid for impure doses of each drug from the Illicit Drug Reporting System, which surveys 1,000 regular injecting drug users across Australia in the middle of each year (see `https://ndarc.med.unsw.edu.au/project/illicit-drug-reporting-system-idrs`). We use NSW respondents. Average purities come from forensic analyses of all packages weighing less than one gram from the Victoria Police Forensic Science Centre.

Figure A.2: Trends in prescription opioids in Australia and the United States



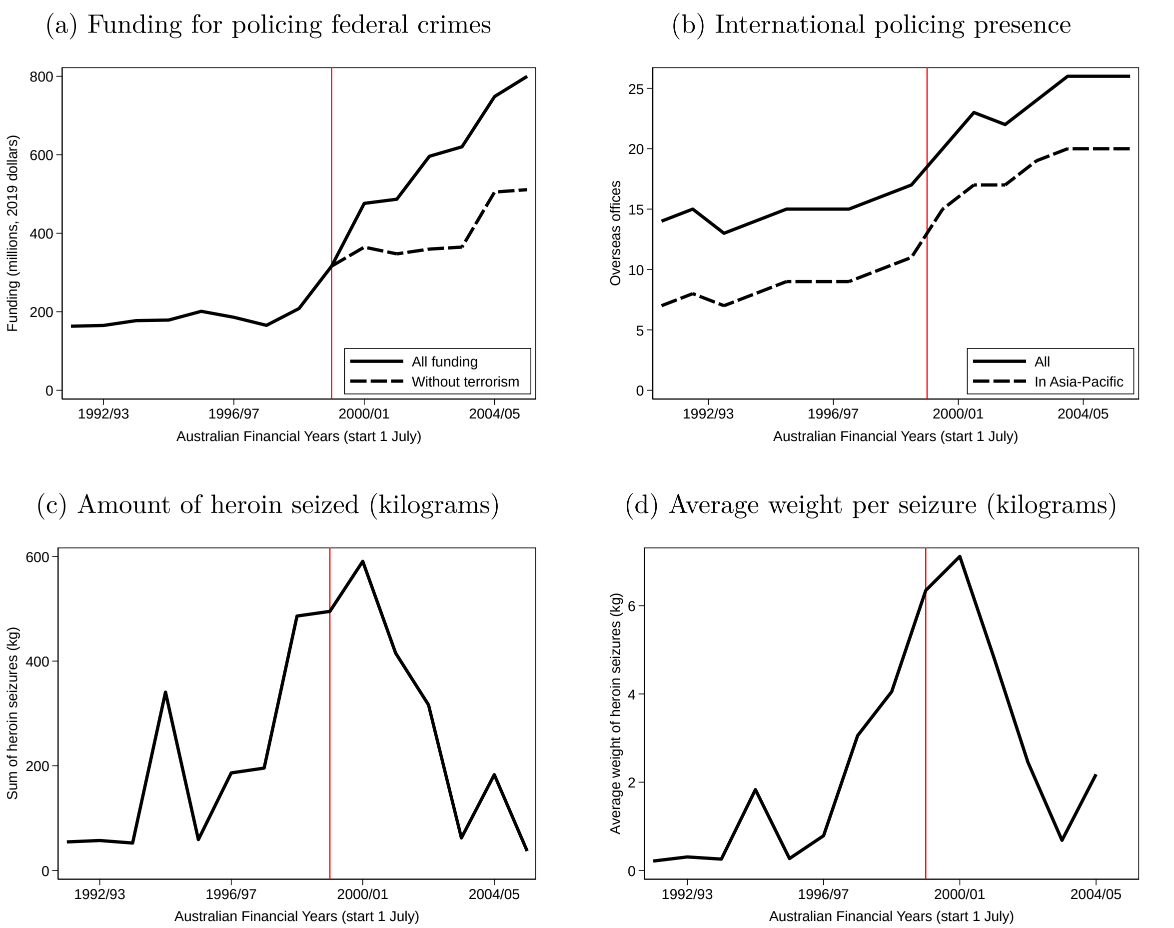
Notes: This figure plots estimates of the annual milligrams per person of opioids prescribed in both Australia and the United States from 1990 through 2015. Data was originally compiled from International Narcotics Control Board reports by the Pain & Policy Studies Group at the University of Wisconsin. We obtained figures for Australia from a 2019 report, "Pharmaceutical opioids in Australia: A double-edged sword" available at `http://nceta.flinders.edu.au/files/2415/4960/5275/Pharmaceutical_opioids_in_Australia_A_double-edged_sword.pdf`[Accessed 2 June 2020] and figures for the United States from `https://www.hrw.org/report/2018/12/18/not-allowed-be-compassionate/chronic-pain-overdose-crisis-and-unintended-harms-us`[Accessed 2 June 2020]. All prescription amounts are adjusted to morphine-equivalent units based on the notion that different doses of different opioids may give a similar analgesic effect.

Figure A.3: Heroin prices in Australia and other countries, 1999



Notes: This compares heroin prices in Australia in 1999, the final year before the onset of the supply reduction there, to heroin prices in Europe and the US. Prices are in 1999 US dollars and are not adjusted for heroin purity. Australian data are the average prices paid for a gram of heroin from the Illicit Drug Reporting System (see `https://ndarc.med.unsw.edu.au/project/illicit-drug-reporting-system-idrs`). The data for the other countries are from the United Nations Office of Drugs and Crime (see `https://www.unodc.org/wdr2018/prelaunch/7.5_Standardized_prices_of_cocaine_and_heroin_in_the_United_States_and_Western_Europe.xlsx`; accessed 15 Nov 2020).

Figure A.4: Australian Federal Police activities and outcomes related to drug interdiction



(a) Funding for policing federal crimes

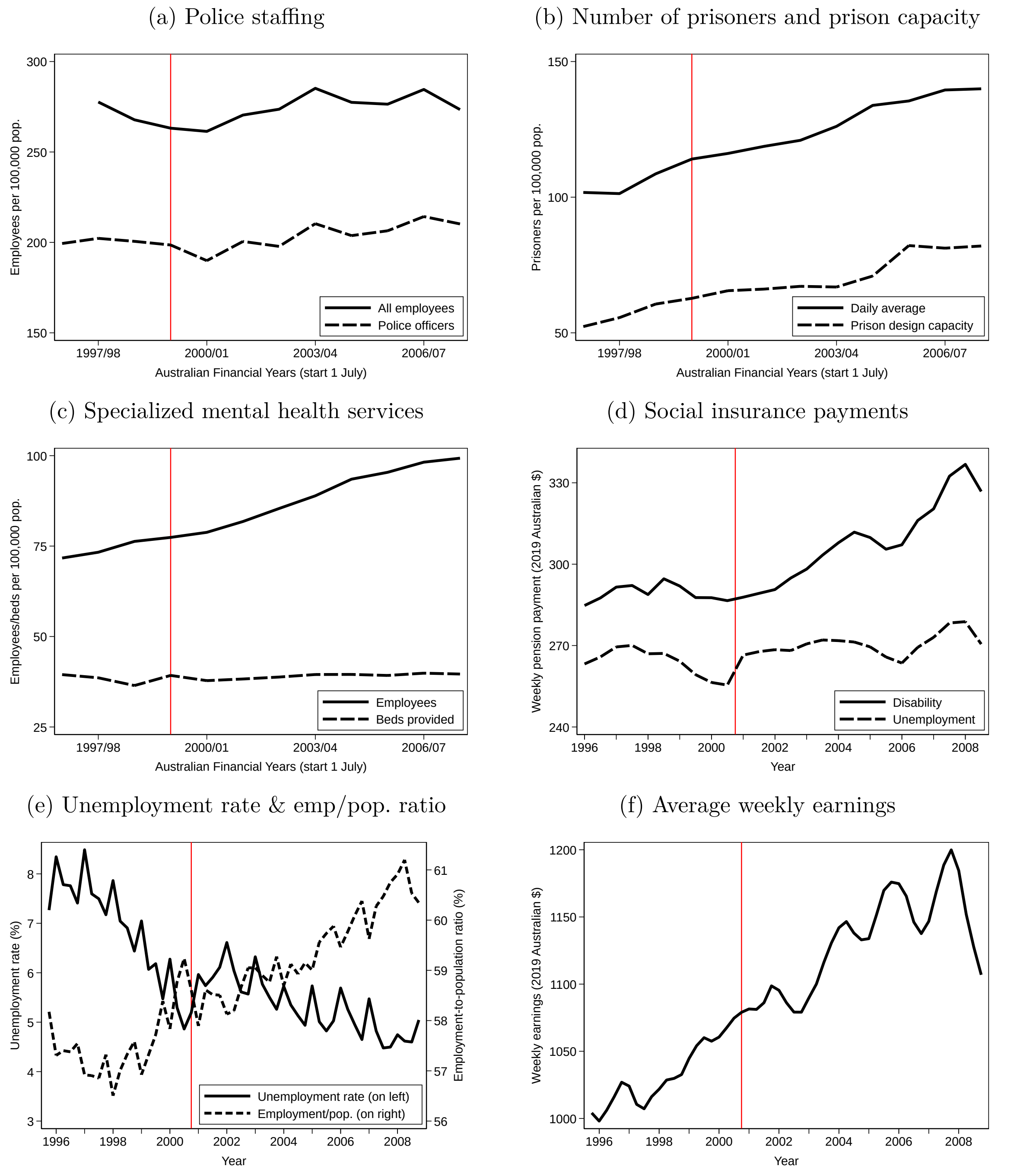
(b) International policing presence

(c) Amount of heroin seized (kilograms)

(d) Average weight per seizure (kilograms)

Notes: These figures are constructed from annual reports from the Australia Federal Police, the agency responsible for international drug interdiction. They are based on Australian financial years, which run from July 1 to June 30 of the following year. The vertical red lines show the 1999/2000 Financial Year, which is the last Australian Financial Year before the onset of the heroin supply reduction. Panel A shows the funding for policing federal crimes, which was primarily related to drug trafficking and organized crime until 2000/01, when the Australian Federal Police took on extra responsibilities related to terrorism. The amount of heroin seized and average weight of seizures are not adjusted for heroin purity, as that information is not provided in the annual reports.

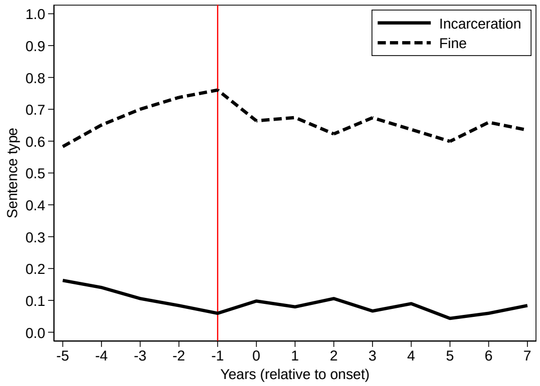
Figure A.5: Measures of government services and labor market outcomes in New South Wales



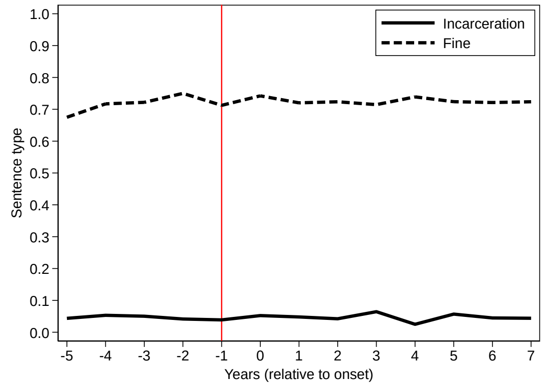
Notes: This figure plots various measures of government services and labor market activity in New South Wales from 1996 to 2008. Panels (a) to (c) use data from the Productivity Commission's Report on Government Services, which is an annual data collection on key service outcomes for the federal and state governments in Australia. Employee numbers are fulltime equivalent. Reports are available at: `https://www.pc.gov.au/research/ongoing/report-on-government-services`[Accessed 10 May 2020]. Panel (d) reports the weekly payment rates for disability and unemployment benefits provided by the federal government. Rates are for single individuals aged 21+, and are converted to 2019 Australian dollars using the Consumer Price Index. They are available at: `https://guides.dss.gov.au/guide-social-security-law/5/2/1`[Accessed 10 May 2020]. Panel (e) shows the state's quarterly trend unemployment rate and employment/population ratio, and comes from Table 4 of the Australian Bureau of Statistics' *6202.0 Labour Force, Australia* publication. Panel (f) shows trend quarterly average weekly earnings, and are converted to 2019 Australian dollars using the Consumer Price Index. The data come from Table 11A of Australian Bureau of Statistics' *6302.0 Average Weekly Earnings, Australia* publication. The Australian Bureau of Statistics publications are available online at `https://www.abs.gov.au/`[Accessed 10 May 2020].

Figure A.6: Severity of punishment for drug, violent and property offenses

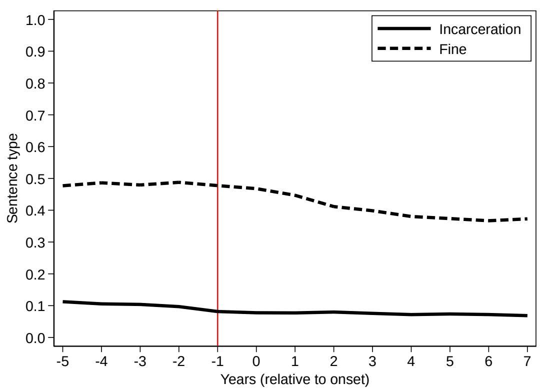
(a) Opioid offenses



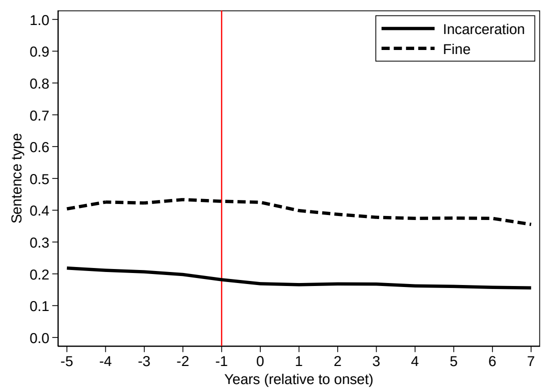
(b) Cocaine or methamphetamine offenses



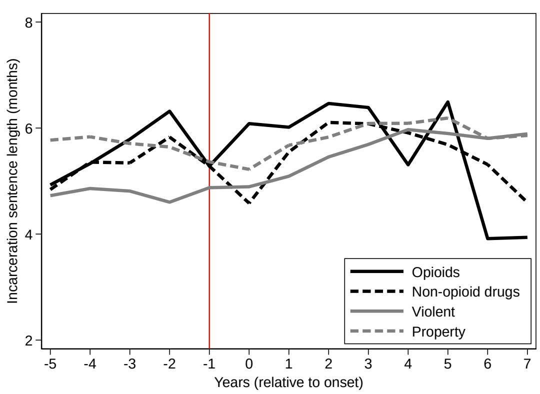
(c) Violent offenses



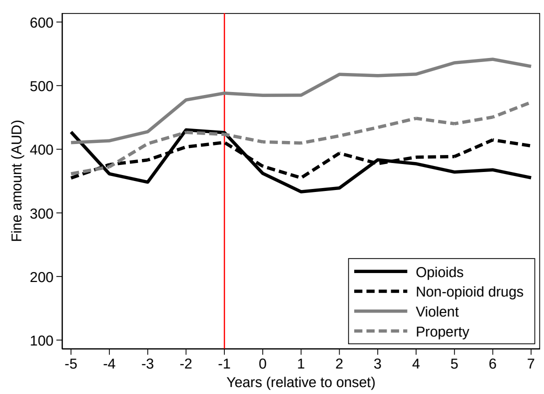
(d) Property offenses



(e) Average length of incarceration sentences



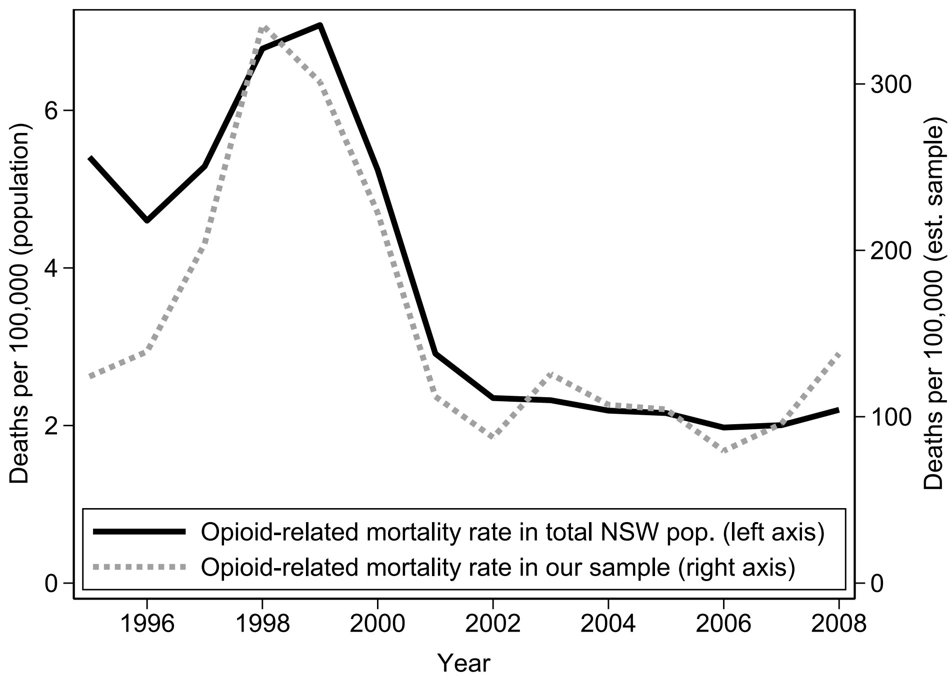
(f) Average amount of fine



Notes: This figure plots trends in the type and severity of punishments imposed in NSW. Regression-adjusted average rates for three types of sanctions (incarceration or fine) are displayed in figures (a) through (d). Other sanctions not depicted include sanctions of home detention, suspended sentences, bonds, or community service. Regression-adjusted average sentence lengths or average fine amounts are shown in figures (e) and (f) for opioid drug offenses, non-opioid drug offenses (methamphatmine, cocaine, and other non-cannibis illicit drugs), violent offenses, and property offenses. To account for changes in offender characteristics during our analysis period that influence penalties, we adjust rates as follows: We estimate a model that includes an indicator variable for the sentence type (or incarceration sentence length) as the dependent variable using the court outcome data from the NSW Bureau of Crime Statistics and Research. As regressors we include indicators for years since the heron supply reduction; the number of prior charges for each type of drug offenses as well as property and violent offenses; the number and type of concurrent charges; and indicators for age (in years), gender and indigenous status. We add the coefficients associated with the year's since reduction indicator variables to the baseline period (1999-Q4 through 2000-Q3) average outcome to obtain regression-adjusted values that are not influenced by changes in defendant characteristics.

Figure A.7: Opioid-related mortality

(a)

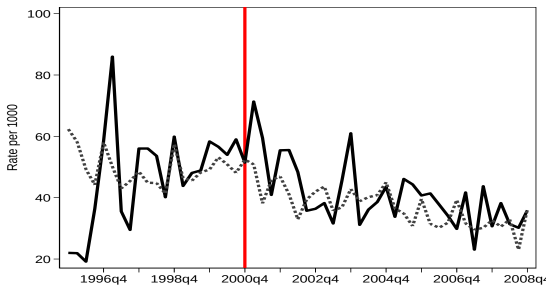


Notes: This figure shows the opioid-related mortality rate in NSW by year from 1995 through 2008 (solid black lines) along with the opioid-related mortality rate our sample of 6,886 opioid users (dashed gray lines).

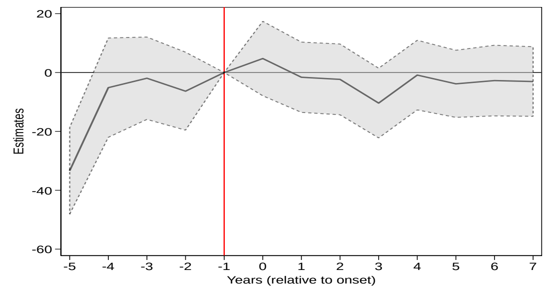
Figure A.8: Other outcomes

RAW DATA: | EVENT STUDY ESTIMATES:

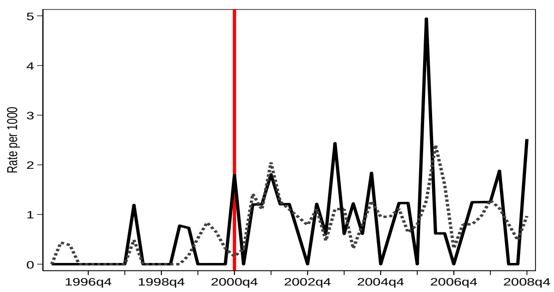
(a) Impaired driving



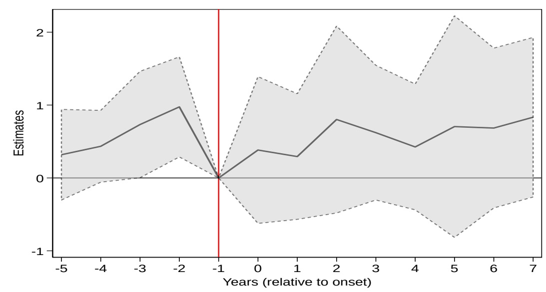
(b) Impaired driving



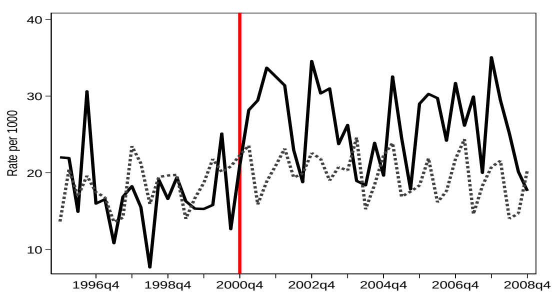
(c) Hard drug sale/dist.



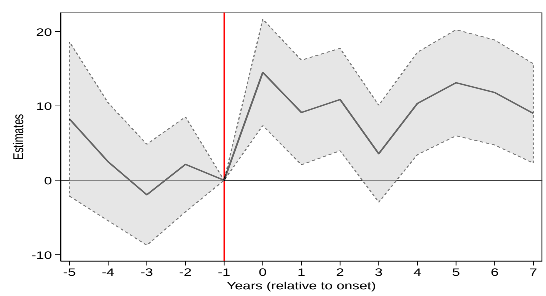
(d) Hard drug sale/dist.



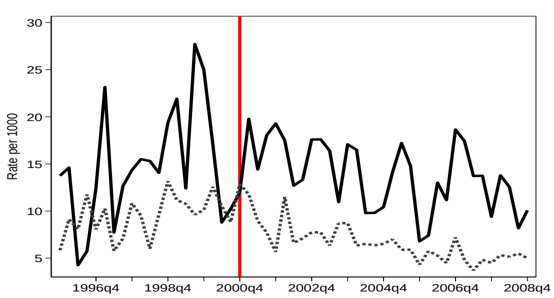
(e) Assaults serious injury



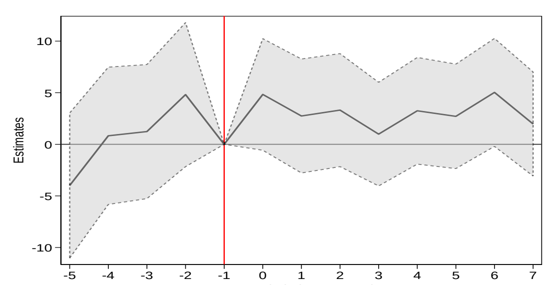
(f) Assaults serious injury



(g) Assaults serious non-injury



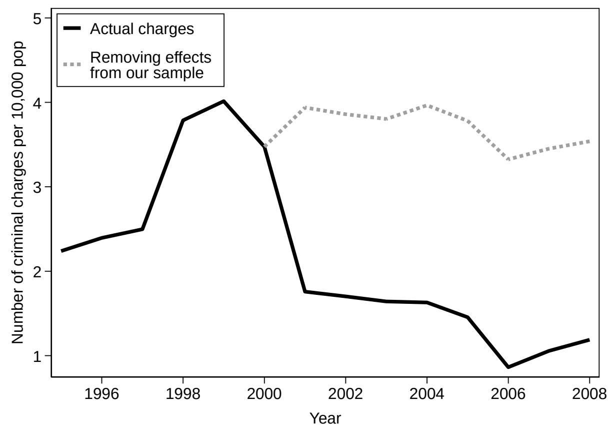
(h) Assaults serious non-injurry



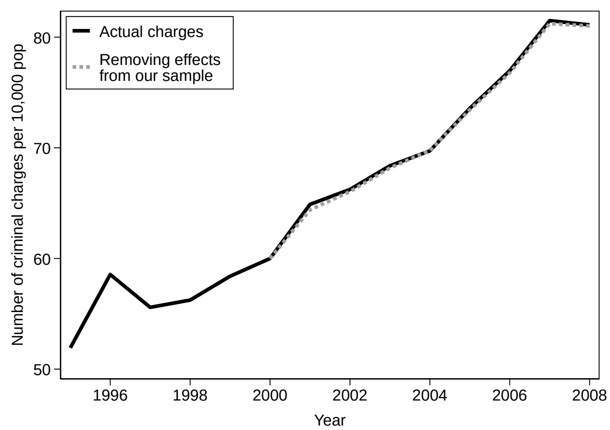
Notes: The left column shows unadjusted quarterly rates for our sample of opioid users and control group of non-opioid drug offenders. A vertical red line marks the onset of the heroin supply reduction in 2000-Q4. The right column shows the estimated coefficients and 95% confidence intervals for the event-year indicator variables from Eq (1) in Section 3. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, which is identified with a vertical red line. Rates in all panels are scaled to represent annual outcomes per 1,000 individuals.

Figure A.9: Estimates applied to overall NSW criminal charges

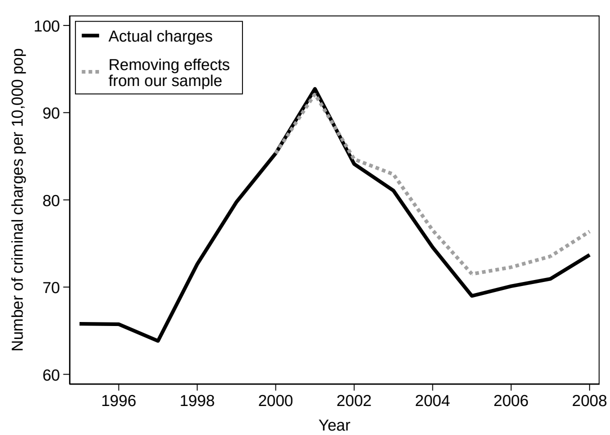
(a) Opioid use/poss. offenses



(b) Violent offenses



(c) Property offenses



Notes: This figure shows the aggregate criminal charges per year per 10,000 population in NSW by year from 1995 through 2008 (solid black lines) along with a counterfactual path implied by removing the estimated effects of the heroin supply reduction on our sample of 6,886 opioid users (dashed gray lines) for violent offenses (a) and property offenses (b). The cumulative difference in the number of offenses between the actual and counterfactual scenarios imply 1,534 more violent offenses from 2001 through 2008 (1.5 per 10,000 population), 12,290 less total property offenses from 2001 through 2008 (18 per 10,000 population), and 12,313 less total opioid use/possession offenses (18 per 10,000 population).

Table A.1: Estimated changes in other outcomes (per 1,000 individuals)

Outcome	Avg. for treatment group year -1	Coefficients (standard errors) Years -5 to -2		Year 0		Year 1		Years 2 to 7	
		A. Other Mortality outcomes							
Non-opioid drug related	0.16	-0.04	(0.19)	0.60	(0.38)	0.41	(0.35)	0.05	(0.19)
Non-opioid drug or alcohol related	0.32	0.03	(0.29)	0.37	(0.44)	1.04*	(0.51)	0.24	(0.29)
Not drug nor alcohol related	2.09	-0.10	(0.78)	-0.14	(0.92)	0.01	(0.90)	-0.05	(0.70)
Suicide	1.13	-0.37	(0.50)	-0.39	(0.58)	-0.41	(0.61)	-0.27	(0.48)
		B. Other drug outcomes							
Impaired driving alcohol-related	42.01	-7.25	(4.18)	9.64	(4.92)	5.24	(4.66)	3.44	(3.73)
Driving causing death	0.16	0.00	(0.20)	0.30	(0.45)	-0.04	(0.21)	0.12	(0.22)
Hard drug sale	0.64	1.47*	(0.72)	1.59	(0.97)	2.00*	(0.91)	1.78*	(0.64)
Opioid drug sale	0.64	0.85	(0.66)	1.21	(0.79)	1.71*	(0.80)	1.11	(0.58)
Non-opioid drug sale	0.00	0.62*	(0.25)	0.38	(0.51)	0.29	(0.44)	0.68*	(0.29)
		C. Other violent							
Serious assault (with injury)	17.22	2.73	(2.86)	14.51**	(3.65)	9.11*	(3.59)	9.78**	(2.60)
Serious assault (with no injury)	14.97	0.72	(2.44)	4.83	(2.76)	2.74	(2.82)	2.88	(2.09)

* $p < 0.05$, ** $p < 0.01$.

Notes: This table summarizes the estimated effects of the heroin reduction. We present coefficients and standard errors for the year indicator variables in the event study specification described by Eq (1). For the mortality outcome in column (1), we replace individual fixed effects with demographic controls. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, denoted as Year -1. All estimates are scaled to represent annual outcomes per 1,000 individuals. Standard errors are robust to arbitrary correlation within individuals over time; standard errors are calculated using the delta method for multi-year estimates. Each regression uses 1,459,907 observations. To help understand the relative size of the effects, we show the treatment group mean for each outcome variable in the reference period (Year -1).

Table A.2: Robustness to alternative specifications

	No controls	Add dem. controls or indiv. FE	Add age controls	Include pre-trend	Add controls for unemp. rate
	(1)	(2)	(3)	(4)	(5)
A. Opioid-related mortality:					
Years -5 to -2	0.25	0.30	0.27		0.99
	(1.34)	(1.34)	(1.34)		(2.77)
Year 0	-2.56	-2.56	-2.56	-3.13**	-2.36
	(1.33)	(1.33)	(1.33)	(1.00)	(1.50)
Year 1	-3.94**	-3.94**	-3.95**	-4.52**	-3.75**
	(1.25)	(1.25)	(1.25)	(0.90)	(1.40)
Years 2 to 7	-2.94**	-2.94**	-2.95**	-3.53**	-3.07*
	(1.13)	(1.13)	(1.13)	(0.72)	(1.21)
B. All-cause mortality:					
Years -5 to -2	0.20	0.30	0.20		-1.18
	(1.58)	(1.58)	(1.58)		(3.57)
Year 0	-2.33	-2.34	-2.33	-2.91*	-2.70
	(1.67)	(1.67)	(1.67)	(1.30)	(1.90)
Year 1	-2.88	-2.89	-2.90	-3.48**	-3.27
	(1.62)	(1.62)	(1.63)	(1.24)	(1.83)
Years 2 to 7	-2.74*	-2.75*	-2.76*	-3.34**	-2.52
	(1.36)	(1.36)	(1.36)	(0.86)	(1.46)
C. Non-opioid drug use/possession:					
Years -5 to -2	1.98	2.93	2.89		-6.34
	(2.78)	(2.89)	(2.88)		(7.11)
Year 0	17.2**	16.5**	16.5**	14.5**	14.0**
	(3.61)	(3.66)	(3.66)	(3.20)	(4.05)
Year 1	3.45	2.82	2.91	0.87	0.40
	(3.08)	(3.11)	(3.11)	(2.54)	(3.53)
Years 2 to 7	2.42	1.90	1.98	-0.057	3.56
	(2.48)	(2.50)	(2.50)	(1.76)	(2.76)
D. Violent offenses (excl. robbery):					
Years -5 to -2	-4.34	-3.02	-3.57		4.36
	(7.60)	(7.88)	(7.86)		(19.7)
Year 0	46.7**	46.6**	46.8**	47.6**	49.0**
	(9.46)	(9.56)	(9.56)	(8.46)	(10.8)
Year 1	18.1*	18.4*	18.5*	19.3*	20.7*
	(8.88)	(8.99)	(9.00)	(7.52)	(10.3)
Years 2 to 7	12.9	13.9*	14.0*	14.7**	12.6
	(6.88)	(6.95)	(6.97)	(4.96)	(7.49)
E. Serious property offenses (robbery, burglary, vehicle theft):					
Years -5 to -2	-18.1	-17.5	-1.40		-27.4
	(14.3)	(14.2)	(14.1)		(33.8)
Year 0	60.6**	62.0**	51.4**	49.4**	44.4*
	(16.0)	(16.2)	(16.2)	(14.3)	(18.4)
Year 1	33.4*	34.6*	17.8	15.7	10.7
	(15.9)	(16.1)	(16.1)	(13.7)	(17.7)
Years 2 to 7	20.6	21.5	-4.28	-6.43	0.080
	(11.9)	(12.1)	(13.1)	(10.4)	(14.9)
Dem. controls / ind. FE		✓	✓	✓	✓
Age controls			✓	✓	✓
Pre-reduction trend				✓	
Unemp. rate controls					✓

* $p < 0.05$, ** $p < 0.01$. See table notes on page A13

Table A.3: Robustness to trends in government services

	Baseline results	Control for police staffing trends	Control for incarceration trends	Control for mental health staff trends	Control for disability trends	Control for meth & cocaine prices
	(1)	(2)	(3)	(4)	(5)	(6)
A. Opioid-related mortality:						
Years -5 to -2	0.27	0.098	0.82	0.64	0.23	2.67
	(1.34)	(1.36)	(1.26)	(1.29)	(1.35)	(1.37)
Year 0	-2.56	-2.62*	-2.67*	-2.71*	-2.59	-2.01
	(1.33)	(1.34)	(1.35)	(1.36)	(1.34)	(1.10)
Year 1	-3.95**	-4.08**	-4.18**	-4.35**	-4.10**	-3.40**
	(1.25)	(1.27)	(1.30)	(1.33)	(1.28)	(1.00)
Years 2 to 7	-2.95**	-3.36**	-3.94**	-4.17**	-3.51**	-2.40**
	(1.13)	(1.20)	(1.32)	(1.36)	(1.24)	(0.85)
B. All-cause mortality:						
Years -5 to -2	0.20	0.050	0.79	0.61	0.22	2.82
	(1.58)	(1.60)	(1.48)	(1.52)	(1.59)	(1.57)
Year 0	-2.33	-2.37	-2.46	-2.49	-2.30	-1.69
	(1.67)	(1.68)	(1.70)	(1.71)	(1.68)	(1.42)
Year 1	-2.90	-3.01	-3.15	-3.33	-3.00	-2.27
	(1.63)	(1.64)	(1.67)	(1.71)	(1.66)	(1.36)
Years 2 to 7	-2.76*	-3.16*	-3.82*	-4.05*	-3.29*	-2.12*
	(1.36)	(1.44)	(1.58)	(1.63)	(1.48)	(1.03)
C. Non-opioid drug use/possession:						
Years -5 to -2	1.81	1.67	2.42	2.28	1.76	0.97
	(2.77)	(2.82)	(2.61)	(2.67)	(2.79)	(2.56)
Year 0	17.1**	17.0**	17.1**	17.1**	17.0**	17.4**
	(3.61)	(3.62)	(3.64)	(3.66)	(3.62)	(3.31)
Year 1	3.40	3.28	3.34	3.21	3.25	3.39
	(3.07)	(3.10)	(3.15)	(3.21)	(3.12)	(2.69)
Years 2 to 7	2.32	1.96	1.63	1.44	1.81	2.56
	(2.48)	(2.62)	(2.84)	(2.94)	(2.69)	(1.99)
D. Violent offenses (excl. robbery):						
Years -5 to -2	-7.18	-7.23	-6.71	-6.81	-7.35	4.09
	(7.58)	(7.71)	(7.13)	(7.29)	(7.63)	(6.66)
Year 0	46.5**	46.6**	46.5**	46.5**	46.4**	50.9**
	(9.46)	(9.50)	(9.55)	(9.59)	(9.50)	(8.70)
Year 1	17.8*	17.8*	17.7	17.6	17.6	22.3**
	(8.89)	(8.97)	(9.08)	(9.23)	(9.02)	(7.83)
Years 2 to 7	12.7	12.5	12.0	11.9	12.1	17.1**
	(6.88)	(7.26)	(7.88)	(8.13)	(7.44)	(5.55)
E. Serious property offenses (robbery, burglary, vehicle theft):						
Years -5 to -2	-8.04	-9.33	-0.35	-2.92	-8.28	4.46
	(14.2)	(14.3)	(13.5)	(13.7)	(14.2)	(12.7)
Year 0	51.7**	51.9**	50.7**	50.1**	51.6**	57.8**
	(16.0)	(16.1)	(16.2)	(16.3)	(16.1)	(15.2)
Year 1	20.2	19.5	17.6	15.3	18.5	25.8
	(16.0)	(16.1)	(16.3)	(16.5)	(16.2)	(14.6)
Years 2 to 7	3.93	-0.41	-8.53	-11.7	-3.18	9.84
	(12.4)	(13.1)	(14.2)	(14.7)	(13.4)	(10.8)

* $p < 0.05$, ** $p < 0.01$. *Notes*: See table notes on page A13

Notes to Table A.2: This table summarizes the estimated impacts of the heroin reduction across different specifications using data from our treatment and comparison groups. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, denoted as Year -1. All estimates are scaled to represent annual outcomes per 1,000 individuals. Standard errors are robust to arbitrary correlation within individuals over time; standard errors are calculated using the delta method for multi-year estimates. Each regression uses 1,459,907 observations in Panels A through D, and 385,185 observations with the alternative control group in Panel E. Column (1) only includes indicator variables for time since the heroin supply reduction; Column (2) adds individual fixed effects (or controls for gender and indigenous status when mortality is the outcome); Column (3) adds a complete set of age fixed (making it equivalent to Eq (1)); Column (4) replaces the pre-reduction year indicator variables with a linear trend; and Column (5) adds controls for unemployment rates in NSW interacted with treatment group status to allow for differential labor market effects for the treatment and comparison groups.

Notes to Table A.3: This table summarizes the estimated impacts of the heroin reduction across different specifications using data from our treatment and comparison groups. The reference period is the 12 months before the onset of the heroin reduction in 2000 Q4, denoted as Year -1. All estimates are scaled to represent annual outcomes per 1,000 individuals. Standard errors are robust to arbitrary correlation within individuals over time; standard errors are calculated using the delta method for multi-year estimates. Each regression uses 1,459,907 observations. Column (1) reports results from our baseline regressions (without individual fixed effects); Column (2) adds controls for police officer trends in NSW (from part (a) of Figure A.5) interacted with treatment group status to allow for differential effects for the treatment and comparison groups; Column (3) adds controls for prison population trends in NSW (from part (b) of Figure A.5) interacted with treatment group status to allow for differential effects for the treatment and comparison groups; Column (4) adds controls for mental health staff in NSW (from part (c) of Figure A.5) interacted with treatment group status to allow for differential effects for the treatment and comparison groups; Column (5) adds controls for disability insurance payments in NSW (from part (d) of Figure A.5) interacted with treatment group status to allow for differential effects for the treatment and comparison groups; Column (5) adds controls for methamphetamine and cocaine retail prices in NSW (from Figure A.1) interacted with whether an individual has a methamphatamine or cocaine use/possession charge prior to the onset of the heroin supply reduction to allow for differential effects for different types of drug users.

Table A.4: Robustness to alternative sample definitions for comparison group

Comparison group:	Non-opioid drug offenders	Non-opioid drug & prop. offenders	No comparison group	No comparison group w/ pre trend	Comparison group: Non-opioid drug property and violent offenders
	(1)	(2)	(3)	(4)	(5)
A. Opioid-related mortality:					
Years -5 to -2	0.27	-2.46	0.92		0.29
	(1.34)	(1.74)	(1.34)		(1.32)
Year 0	-2.56	-2.95	-3.50**	-4.44**	-2.88*
	(1.33)	(1.63)	(1.31)	(1.51)	(1.31)
Year 1	-3.95**	-3.69*	-5.01**	-5.90**	-4.16**
	(1.25)	(1.51)	(1.24)	(1.79)	(1.23)
Years 2 to 7	-2.95**	-2.84*	-4.14**	-4.85	-3.17**
	(1.13)	(1.35)	(1.14)	(3.12)	(1.11)
Treatment group avg. in Year -1	7.08	7.08	7.08	7.08	7.08
B. All-cause mortality:					
Years -5 to -2	0.20	-5.69*	0.69		-0.16
	(1.58)	(2.34)	(1.56)		(1.53)
Year 0	-2.33	-4.64*	-2.98	-3.86*	-2.70
	(1.67)	(2.34)	(1.60)	(1.81)	(1.61)
Year 1	-2.90	-4.31*	-4.32**	-5.16*	-3.72*
	(1.63)	(2.18)	(1.56)	(2.14)	(1.57)
Years 2 to 7	-2.76*	-3.82*	-4.67**	-5.40	-3.11*
	(1.36)	(1.74)	(1.34)	(3.58)	(1.31)
Treatment group avg. in Year -1	9.50	9.50	9.50	9.50	9.50
C. Non-opioid drug use/possession:					
Years -5 to -2	2.89	-3.24	-4.59		2.72
	(2.88)	(5.13)	(5.26)		(2.72)
Year 0	16.5**	13.4*	20.9**	18.2**	16.9**
	(3.66)	(5.92)	(4.08)	(3.71)	(3.45)
Year 1	2.91	4.16	5.37	0.88	1.42
	(3.11)	(4.82)	(5.14)	(3.93)	(2.92)
Years 2 to 7	1.98	1.99	12.5	1.54	0.39
	(2.50)	(4.38)	(10.9)	(6.55)	(2.34)
Treatment group avg. in Year -1	17.54	17.54	17.54	17.54	17.54
D. Violent offenses:					
Years -5 to -2	-3.57	-11.7	-2.29		-12.6
	(7.86)	(19.6)	(14.1)		(7.28)
Year 0	46.8**	52.1**	41.2**	38.8**	44.7**
	(9.56)	(20.1)	(10.6)	(9.78)	(8.95)
Year 1	18.5*	5.41	9.97	6.40	17.7*
	(9.00)	(21.2)	(13.9)	(11.2)	(8.31)
Years 2 to 7	14.0*	31.5	-0.12	-7.61	16.5**
	(6.97)	(16.3)	(29.4)	(17.8)	(6.38)
Treatment group avg. in Year -1	113.14	113.14	113.14	113.14	113.14
E. Serious Property offenses:					
Years -5 to -2	17.1*	-1.40	-15.2		15.4*
	(7.63)	(14.1)	(21.3)		(7.49)
Year 0	61.2**	51.4**	81.7**	76.8**	59.7**
	(10.6)	(16.2)	(15.1)	(11.5)	(10.5)
Year 1	22.6*	17.8	53.5*	43.7**	22.3*
	(9.50)	(16.1)	(21.8)	(12.5)	(9.36)
Years 2 to 7	-25.9**	-4.28	45.3	18.0	-26.6**
	(7.34)	(13.1)	(50.6)	(19.3)	(7.18)
Treatment group avg. in Year -1	112.66	112.66	112.66	112.66	112.66
No. of opioid users:	6,886	6,886	6,886	6,886	6,886
No. in control group:	25,814	2,216	0	0	134,219

* $p < 0.05$, ** $p < 0.01$. *Notes*: This table summarizes estimated impacts of the heroin reduction across several alternative definitions of our comparison group. Each estimate is based on Eq (1); see Table II notes for details. Column (1) reproduces estimates from Table II; Column (2) presents results using the high-property offending subset of the main comparison group; Column (3) presents results for a specification without a comparison group; Column (4) adds a pre-shortage linear trend to the specification in Column (3); and, Column (5) broadens the control group to include non-drug offenders charged with a property or violent crime prior to the supply reduction.

Table A.5: Robustness to alternative sample definitions for comparison group, petty property crime

Comparison group:	Non-opioid drug offenders	Non-opioid drug & prop. offenders	No comparison group	No comparison group w/ pre trend	Comparison group: Non-opioid drug property and violent offenders
	(1)	(2)	(3)	(4)	(5)
Years -5 to -2	-25.1	-36.9	-46.7		-33.8
	(18.5)	(32.8)	(27.7)		(18.3)
Year 0	-35.7	-31.7	-25.8	-43.7	-38.6
	(23.9)	(31.7)	(27.1)	(25.7)	(23.8)
Year 1	-100.4**	-94.9**	-86.0**	-121.2**	-101.7**
	(21.8)	(36.8)	(31.2)	(28.5)	(21.5)
Years 2 to 7	-240.3**	-220.6**	-218.9**	-314.9**	-244.7**
	(17.9)	(36.1)	(56.7)	(45.4)	(17.7)
Treatment group avg. in Year -1	451.92	451.92	451.92	451.92	451.92

* $p < 0.05$, ** $p < 0.01$. *Notes*: This table summarizes estimated impacts of the heroin reduction across several alternative definitions of our comparison group. Each estimate is based on Eq (1); see Table II notes for details. Column (1) reproduces estimates from Table II; Column (2) presents results using the high-property offending subset of the main comparison group; Column (3) presents results for a specification without a comparison group; Column (4) adds a pre-shortage linear trend to the specification in Column (3); and, Column (5) broadens the control group to include non-drug offenders charged with a property or violent crime prior to the supply reduction.

Table A.6: Heterogeneity of effects by intensity of opioid use

	No. of opioid offenses at supply shock		Any methadone treatment pre supply shock?		Max dose of treatment pre supply shock	
	$= 1$	> 1	No	Yes	≤ 150 mg	> 150 mg
			A. Opioid-related mortality			
Short term (yr 0)	-2.10	-3.37*	-3.62*	-1.88	-1.91	-4.85*
	(1.48)	(1.55)	(1.50)	(1.51)	(1.61)	(1.80)
Longer term (yrs 2-7)	-2.86*	-3.10*	-4.21**	-2.14	-2.23	-2.10
	(1.16)	(1.19)	(1.16)	(1.17)	(1.19)	(1.49)
Avg in yr -1	5.70	9.34	6.41	7.49	7.91	4.45
			B. All-cause mortality			
Short term (yr 0)	-2.07	-2.79	-2.89	-1.97	-1.56	-6.36*
	(1.86)	(2.09)	(2.05)	(1.88)	(2.04)	(2.43)
Longer term (yrs 2-7)	-2.60	-3.02*	-4.56**	-1.60	-1.87	-1.01
	(1.40)	(1.45)	(1.41)	(1.42)	(1.44)	(1.92)
Avg in yr -1	7.00	13.59	8.55	10.07	10.88	5.93
			C. Non-opioid drug use/possession			
Short term (yr 0)	14.54**	20.17**	15.25*	17.19**	16.46**	12.27
	(4.18)	(6.26)	(5.60)	(4.48)	(5.01)	(9.88)
Longer term (yrs 2-7)	4.49	-2.22	-1.61	4.16	4.37	5.07
	(2.89)	(4.13)	(3.60)	(3.11)	(3.52)	(6.19)
Avg in yr -1	14.51	22.51	17.52	17.56	17.15	17.78
			D. Violent offenses (excl. robbery)			
Short term (yr 0)	46.48**	47.66**	29.41*	57.45**	52.40**	71.00*
	(11.40)	(14.96)	(13.64)	(12.00)	(13.03)	(29.40)
Longer term (yrs 2-7)	15.37	11.70	-0.60	22.87*	23.74*	21.06
	(8.51)	(9.69)	(9.42)	(8.72)	(9.44)	(20.08)
Avg in yr -1	111.96	115.07	97.85	122.38	121.03	134.86
			E. Serious property offenses (robbery, burglary, vehicle theft)			
Short term (yr 0)	54.18**	47.37*	58.40*	46.91*	39.53*	41.63
	(17.99)	(21.04)	(21.47)	(17.76)	(18.60)	(31.89)
Longer term (yrs 2-7)	5.83	-21.42	-3.55	-5.11	-1.02	-11.13
	(14.73)	(14.48)	(14.10)	(15.25)	(16.51)	(22.50)
Avg in yr -1	108.85	118.90	100.42	120.05	119.71	120.04

* $p < 0.05$, ** $p < 0.01$. *Notes*: This table summarizes the heterogeneous effects of the heroin reduction. We present estimated coefficients for selected treatment group by year indicator variables in the event study specification described by Eq (2) in Section 3, which are further interacted with indicator variables identifying our subgroups of interest (e.g., males and females). All specification and sample notes from Table II apply except that our treatment group for the final comparison based on methadone dose restricts our treatment group to those who participated in opioid substitution therapy prior to the onset of the heroin supply reduction (4,208 or 68% of our treatment group) .

Appendix B: Studies of the effects of the Australian heroin supply reduction

Table B1: Studies of the effects of the Australian heroin supply reduction

Focus	Study	Outcomes	Follow up	Data	Methods	Key findings
A. Studies that primarily focus on heroin (or opioid) outcomes						
Heroin & opioid outcomes	Smithson M., McFadden M., and Mwesigye, S.-E. 2005. "Impact of Federal drug law enforcement on the supply of heroin in Australia. *Addiction*, 100: 1110-20.	- Street-level heroin purity - Federal heroin seizures	2.8 years	- Territory- and national-level daily administrative data	Time-series analysis	- Large decrease in heroin purity in 2001 - Increase in heroin seizures one year before a decrease in heroin purity
Heroin & opioid outcomes	Day, C., Degenhardt, L., and Hall, W. 2006. "Documenting the heroin shortage in New South Wales." *Drug and Alcohol Review*, 25: 297–305.	- Self-reported heroin accessibility - Heroin price and purity	3.5-5 years	- Self-reports from injecting drug users (annual cross-sections, convenience sampling) - State-level quarterly data on heroin seizures - Interviews with health/law enforcement professionals	Descriptive	- Clear reduction in heroin supply, increase in heroin price and decrease in heroin purity in NSW in early 2001 - Slight rebound in measures after 2001 but lasting reductions in heroin supply
Heroin & opioid outcomes	Day, C., Degenhardt, L., and Hall, W. 2006. "Changes in the initiation of heroin use after a reduction in heroin supply." *Drug and Alcohol Review*, 25: 307–13.	- Age of survey respondent - Age of initial heroin use	4 years	- Self-reports from injecting drug users (annual cross-sections, convenience sampling)	Time-series analysis	- Estimate 2,745-10,560 young people avoided initiation into heroin use during 2001 due to reduced heroin supply - Increase in amphetamine injecting but "unclear to what extent any reduction in heroin injecting has been offset by increased amphetamine injecting"
B. Studies that include outcomes for other non-opioid illicit drugs						
Heroin & opioid outcomes / Other drug use	Day C., et. al. 2004. "Effects of reduction in heroin supply on injecting drug use: Analysis of data from needle and syringe programmes." *British Medical Journal*. 329(7463): 428-29.	- Needles/ syringes distributed as a measure of injecting drug use	2.25 years	- State-level quarterly data	Descriptive	- 28% decrease in needles/syringes distributed from fourth quarter of 2000 to first quarter 2002
Heroin & opioid outcomes / Other drug use	Degenhardt, L., et al. 2005. "The impact of the Australian heroin shortage on demand for and compliance with treatment for drug dependence." *Drug and Alcohol Dependence*, 79: 129–135.	- Number of persons entering treatment for drug addiction	2.25 years	- State-level monthly administrative data	Time-series analyses	- Decrease in number entering opioid treatment - Improvements in treatment adherence and retention - Small increases in treatment for cocaine dependence
Heroin & opioid outcomes / Other drug use	Degenhardt L., et al. 2005. "Age differentials in the impacts of reduced heroin: Effects of a "heroin shortage" in NSW, Australia." *Drug and Alcohol dependence*, 2005 79(3): 397-404.	- Arrests for heroin possession - Number of drug-related deaths - Number of persons entering treatment for heroin, amphetamine dependence	2.5 years	- State-level monthly administrative data by age	Time-series analysis	- 41% reduction in new registrations for opioid pharmacotherapy for ages 25-34 years, a 26% reduction for ages 15-24 years, and no change for ages 35 and older - 49% reduction in heroin possession offenses for ages 15-24 years and 31-40% decrease for those 25 and older - Large reduction in opioid-related deaths - No change in non-opioid drug-related deaths

Table B1 [Continued]: Studies of the effects of the Australian heroin supply reduction

Heroin & opioid outcomes / Other drug use	Roxburgh A., Degenhardt L., and Breen C. 2004. "Changes in patterns of drug use among injecting drug users following a reduction in the availability of heroin in New South Wales, Australia." *Drug and Alcohol Review*, 23: 287-94.	- Self-reported heroin use, prices, availability - Self-reported cocaine use, prices, availability	3 years	- Self-reports from injecting drug users (annual cross-sections, convenience sampling)	Descriptive	- Marked decrease in the frequency of heroin use in 2001, stayed low in 2002 and 2003 - injecting drug users shifted from heroin to cocaine during 2001, however, patterns of cocaine use were not maintained - cocaine price has remained stable
Heroin & opioid outcomes / Other drug use	Degenhardt L., et al. 2005. "Patterns of illicit drug use in NSW, Australia following a reduction in heroin supply." *International Journal of Drug Policy*, 16(5):300-07.	- Helpline calls for heroin and other drugs - - Visits to inner Sydney needle syringe program where users reported the last drug injected	3.5 years	- Drug helpline calls about different drug types - Data from an inner Sydney needle and syringe program	Time-series analyses	- 39% decline in calls of concern related to heroin from late 2000 to early 2001 - Short-term increases in call of 75% for methamphetamine, 191% for cocaine, no change for benzodiazepines - 40% reduction in the total number of needles and syringes distributed
Other drug use	Snowball L, et al. 2008. "Did the heroin shortage increase amphetamine use? A time series analysis." *Crime and Justice Bulletin*, 114: 1-8.	Arrests for use/possession of heroin or amphetamines	6.25 years	- State-level monthly administrative data	Time-series analysis	- No evidence of a temporal relationship between heroin and amphetamine use
C. Studies that focus on health and mortality outcomes						
Health & mortality	Degenhardt L., et al. 2005. The effect of a reduction in heroin supply on fatal and non-fatal drug overdoses in New South Wales, Australia. *Medical Journal of Australia*. 182(1): 20-23.	- Drug-related deaths - Ambulance calls to opioid overdoses - Hospital admissions for heroin and other drug overdoses	2.33 years;	- State-level monthly administrative data	Time-series analyses	Reduction in heroin-related overdose events and deaths, greatest among younger age groups - Increases in. non-fatal overdoses with cocaine, methamphetamines or benzodiazepines
Health & mortality	Day C., et al. 2005. "The impact of changes to heroin supply on blood-borne virus notifications and injecting related harms in New South Wales, Australia." *BMC Public Health*. 5(1):1-8.	- HIV, hepatitis B, hepatitis C notifications – Hospital and emergency department admissions for injection-related problems	2.5 years	- State-level monthly administrative data	Time-series analysis	- No changes detected in hospital visits for injection-related problems - No significant change detected for HIV and hepatitis B notifications - Potentially a delayed reduction in hepatitis C notifications, restricted to patients aged 15–19 years
D. Studies that include non-drug criminal offending outcomes						
Heroin & opioid outcomes / Other drug use / Crime	Gilmour S., et al. 2006. "Identification and quantification of change in Australian illicit drug markets." *BMC Public Health*, 6(200).	- Heroin and other drug possession offenses - Ambulance attendances for drug overdoses - Drug helpline calls - Property, robbery, prostitution offenses	1.5 years	- State-level monthly administrative data	- Times series and principal component analysis	- Used the data series to assess whether the changes to the heroin market in 2001 could be attributed to "normal" drug epidemic processes - Found change at start of the heroin supply reduction is three times larger than variation found in other epidemic processes within the data

Table B1 [Continued]: Studies of the effects of the Australian heroin supply reduction

Heroin & opioid outcomes / Crime	Degenhardt, L., et al. 2005. "The effect of a reduction in heroin supply in Australia upon drug distribution and acquisitive crime." *British Journal of Criminology,* 45: 2–24.	- Police incidents for drug possession (by drug type), robbery, break and enter, and theft	2 years	- State-level monthly administrative data by sex - Police investigation reports - Interviews with health/ law enforcement professionals, heroin users	Time-series analyses	- Significant decreases in police incidents of heroin possession - Increase observed for cocaine possession - Decrease in theft and temporary increase in robbery
Other drug use / Crime	Degenhardt L., et al. 2005. "Was an increase in cocaine use in New South Wales, Australia, accompanied by an increase in violent crime?" *BMC Public Health* 5(40).	- Self-reported cocaine use - Robberies with weapons	2-3 years	- Self-reports from injecting drug users (annual cross-sections, convenience sampling) - State-level monthly data on drug, property and robbery offenses - Interviews with health/law enforcement professionals	Time-series analysis	- There was a significant increase in cocaine use and cocaine possession offences in first few months - There was also an increase in incidents of robbery where weapons were involved - There were no increases in offences involving firearms, homicides or reported assaults
Heroin & opioid outcomes / Other drug use / Crime	Smithson M., et al. 2004. "The impact of illicit drug supply reduction on health and social outcomes: The heroin shortage in the Australian Capital Territory." *Addiction* 99(3): 340-8.	- Ambulance callouts for heroin and other drugs overdoses - Numbers in medication-assisted treatment - Property crime	2.3 years	- Territory-level monthly administrative data	Time-series analysis	- Reduction in heroin purity predicted decline in heroin-related ambulance callouts and an increase in treatment program enrolments - No evidence of increase in callouts for other drugs - Declines in robbery and burglary, but not theft
Heroin & opioid outcomes / Other drug use / Crime	Degenhardt, L., et al. 2005. "Effects of a sustained heroin shortage in three Australian States." *Addiction,* 100: 908–20.	- Drug overdoses - Police incidents for drug possession and property crime - Drug treatment episodes - Needles distributed to injecting drug users	3 years	- State-level monthly administrative data from three states (NSW, VIC, SA)	Time-series analyses	- Fatal and non-fatal heroin overdoses decreased by 40-85% - No significant increases in cocaine, methamphetamine or benzodiazepine overdose events - Drop in number of needles and syringes distributed in VIC - Short-term increase in property crime followed by a reduction in NSW
Heroin & opioid outcomes / Other drug use / Crime	Donnelly, N., Weatherburn, D., and Chilvers, M. 2004. "The Impact of the Australian Heroin Shortage on robbery in NSW." *Crime and Justice Statistics Bureau Brief*. Sydney: NSW Bureau of Crime Statistics and Research.	- Arrests for drug possession - Non-fatal opioid overdoses - Reported robberies	3 years	- District- and state-level monthly administrative data	Time-series analysis	- Decrease in heroin overdoses - Decreases in the majority of property crime categories - Short-term increase in robbery followed by a decrease

Basis for estimates

We convert the mortality and crime estimates to dollar values and calculate their net present value at the start of the heroin supply reduction. Here, we provide more information about the basis for the valuations, including the sources used and how the calculations are made. It is important to note that no value is attributed to drug-defined crimes, which includes drug use/possession offenses and drug supply offenses.

Mortality costs. We value mortality changes using value of a statistical life (VSL) estimates. US estimates are currently on the order of US$10 million, or around A$15 million at current exchange rates (Kniesner and Viscusi 2019). Viscusi (2018) considers Australian and international evidence on the likely VSL for Australia, and estimates that it is around A$10 million in 2015 dollars. This is approximately A$11.7 million in 2022 dollars when updated using Australia's average wage index for all industries (Australian Bureau of Statistics 2023).

Crime costs. There are many different types of costs associated with crimes. These include tangible costs to victims (e.g., property losses, medical bills), victims' pain and suffering, and costs to the community such as those associated with crime prevention, insurance and precautionary behavior. Studies have estimated the costs of crime for several countries, including a series of studies from the Australian Institute of Criminology that have estimated the crime costs for Australia (Mayhew 2003; Rollings 2008; Smith et al. 2014). The methodology applied has been consistent and similar to that developed for Britain (Brand and Price 2000) and the United States (Cohen and Bowles 2010). For different types of crime, the authors estimate medical costs, lost earnings and intangible costs to arrive at a per-crime cost estimate. They then use nationally representative victimization surveys to estimate the degree to which crimes are under-reported, and use this multiplier to scale up the number of crimes reported to police.

We take the per-crime estimates from Smith et al. (2014), as this provides the most recent cost information. These are for 2011, so we adjust them to 2022 dollars using the Australian wage index. We use their estimates for the costs of assault (A$5,307 per crime); sexual assault (A$5,506 per crime); robbery (A$4,937 per crime); burglary/break and enter (for breaking into residences - A$3,860 per crime); motor vehicle theft (A$8,613 per crime); fraud (A$17,856 per crime); and other theft (A$786 per crime); and value other types of violent crime based on assaults without injury (A$591 per crime). For homicides, we use the same 2022 value of a life based on Viscusi (2018) that is used for the other mortality effects. Multipliers for the under-reporting of the different crime categories are taken from Mayhew (2003). These are for 2001, so are better than later estimates if the degree of under-reporting changes over time. The only relevant crime category not included in Mayhew (2003) is fraud, so we take the underreporting estimate for fraud from Smith et al. (2014).

Estimating the net present value. We calculate the change in the number of deaths and each type of crime by multiplying the number of individuals in our treatment sample by the annual point estimates from our equation (1) specification. We then multiply those quantities by the per-death and per-crime costs outlined above, and aggregate the costs for each year after the onset of the heroin supply reduction.

Back-of-the-envelope estimates

The discounted net values per opioid user in our sample are presented for each of the eight years in Figure C.1 as a bold black line. The net value in the first year is an increase of A$4,891 per individual. The estimates for the other seven years are net savings that are larger in magnitude, averaging A$24,246 per individual per year and ranging from A$16,201 to A$31,530. Overall, the discounted net savings resulting from changes in mortality risks and criminal activity amount to A$169,705 (US$120,491) per individual opioid user in our sample. Across all 6,886 individuals, this amounts to around A$1.2 billion (US$830 million).

It is worth noting that petty/other theft has no material effect on these estimates. The longer-term estimate for this outcome, which imply it halved over time, contributes less than A$1,000 to the overall NPV calcuation. This is because it is valued so much lower than most of the other outcomes we examine.

We consider two variations to our approach to assess the sensitivity of these valuations. The first is to what the numbers look like using higher VSL estimates from Viscusi and Masterman (2017), who propose scaling estimates from the US to other countries using income elasticities. On that basis, they estimate that Australia's VSL should have been A$13 million in 2015 dollars, or approximately A$15.6 million when updated using the wage index. The crime costs are valued as before.

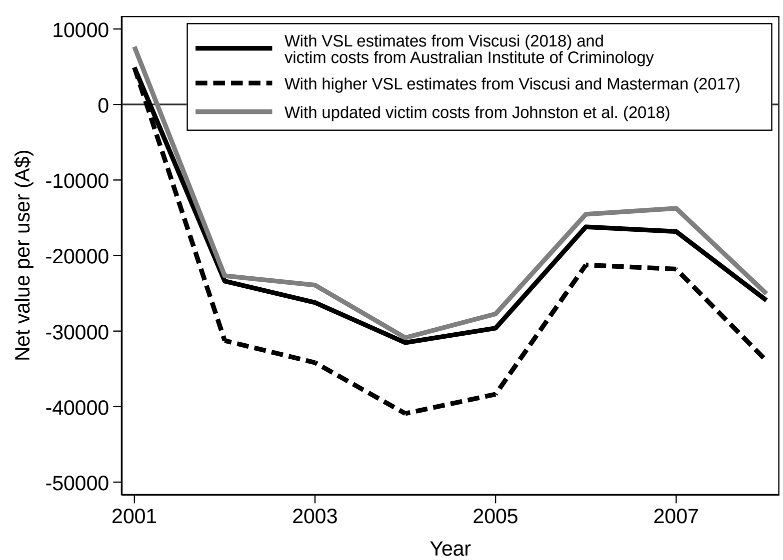
The discounted net values per opioid user when using this approach are shown in Figure C.1 as a black dashed line. The net value in the first year is remarkably similar, with a net cost per individual of A$4,909 (compared to A$4,891 above). This highlights that the mortality increases from increased homicides entirely offset the mortality reductions from reduced opioid overdoses. The estimates for the other seven years remain net savings that are slightly larger in magnitude, as the mortality reductions from opioid overdoses are now valued more highly than before. Overall, the discounted net savings resulting from changes in mortality risks and criminal activity amount to A$221,681 (US$157,393) per individual opioid user in our sample, which is around 31% larger than our baseline valuation.

The second variation is to replace the intangible victim costs with higher estimates from Johnston, Shields and Suziedelyte (2018). The authors use nationally representative panel data from Australia to estimate that income of A$88,000 (in 2012 dollars) is required to return victims of violent crime to their prior level of wellbeing. We again convert this value to 2022 dollars using the Australian wage index. The question used to identify victims of violent crime is quite general, asking respondents if they were a "victim of physical violence (e.g., assault)." We therefore apply this valuation to all violent crimes except for homicides, by replacing the intangible victim costs used in Smith et al. (2014) (and keeping all of the other elements the same). We use the baseline VSL estimate from Viscusi (2018).

The discounted net values per opioid user when using this second variation are shown in Figure C.1 as a gray line. The net value in the first year is a larger cost of A$7,650 per individual. The estimates for the other seven years remain net savings, although they are now slightly smaller in absolute magnitude than in the baseline case due to the persistence of some of the violent-crime effects. Overall, the discounted net savings resulting from changes in mortality risks and criminal activity amount to A$158,517 (US$112,547) per individual opioid user in our sample, which is around 7% smaller than our baseline valuation.

These illustrative exercises highlight that there are substantial net gains for opioid users in our sample after the supply of heroin was reduced, at least in terms of the outcomes in our data. It also highlights that there is a small net cost in the first year, that is robust to the VSL used but likely sensitive to statistical uncertainty. In contrast, the net effects in the longer term represent clean net gains to the individuals in our sample, primarily because the mortality reductions are persistent while most of the initial crime increases are not.

Figure C.1: Annual net value per user of mortality and crime effects



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