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Empirical Evidence from Panel Data of the German States

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Empirical Evidence from Panel Data of the German States

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Abstract

The rising trends both in drug addiction and crime rates are of major public concern in Germany. Surprisingly, the economic theory of crime seems to ignore the drugs–crime nexus, whereas the criminological literature considers illicit drug use a main reason of criminal activities. This paper provides an econometric assessment of the drugs–crime channel within a Becker–Ehrlich model of crime supply. Estimation with panel data from the German states allows us to take into account further factors that might influence both drug abuse and crime. The results indicate that drug offences have a significant impact, in particular on property crimes. We attribute this to a strong economic–related channel of drug abuse on crime.

*We are indebted to two referees for valuable comments on a preliminary version of this paper. Nevertheless, we take full responsibility for any remaining shortcomings.

1 Introduction

The rising trends both in drug addiction and crime rates are of major public concern in Germany. Public concern about narcotics and crime covers both moral and economic aspects. The analysis mainly concentrates on the costs of drug addiction and related crimes which include expenditures of the public health system for drug addicts, negative productivity effects of drug use, tax evasion of black market activities, costs of prosecution among others. Furthermore, drug abuse may contribute to an increase in overall crime rates.

In fact, the evidence of a strong connection between illicid drug use and a wide range of criminal activities seems to be overwhelming. For instance, Beck et al. (1993) report that 49% of U.S. State Prison Inmates committed their offence under the influence of drugs or alcohol, and 17% indicated committing their offence to get money to buy drugs. Harrison and Gfroerer (1992) found that 26.1% of persons booked for any violent crime, and 24.7% of persons booked for any property crime used alcohol, cannabis and cocaine. A recent research study of the British Home Office analysed the results of urine tests carried out on 839 people arrested in five areas of England (London, Cambridge, Manchester, Nottingham and Sunderland). The outcome was that nearly two thirds tested positive for at least one illegal drug. Moreover, the Home Office estimates that one third of acquisitive crime is drug–related (see NACRO, 1999). German official crime statistics reveal that among the total number of offences cleared by the police 8.2% were under the influence of hard[1] illegal drugs and 8.7% were under the influence of alcohol in 2003 (Bundeskriminalamt, 2004). For robbery, the shares were 16.7% and 14.6%, for rape, the shares were 4.6% and 27.6%, and for homicides 7.4% and 25.2%, respectively.

Following Goldstein (1985) and Goldstein, Brownstein and Ryan (1992), criminologists provide three different explanations of drug–related crime (see also Corman and Mocan, 2000). First, system–related crimes include those that are related to the system of drug production and drug trafficking, in particular concomitant crimes such as corruption, intimidation, extortion and crimes of violence. Competition for drug markets and customers cause

[1]This refers mostly to heroine and cocain. The quoted source of German crime statistics (Bundeskriminalamt, 2004, Table 12, Appendix) does not include abuse of cannabis products.

disputes and rip–offs among individuals involved in the illegal drug–market, murders as means of enforcing systemic codes, killing of informants, injury or death resulting from disputes over drug possession, territory, etc. (Goldstein, Brownstein and Ryan, 1992). On the one hand, these crimes originate from interdicting the production and traffic of drugs and on the other hand from the high rents, which can be obtained by violating the law. Concomitant crimes raise market entry barriers for potential concurrent suppliers in order to protect monopolistic rents from drug traffic.

Second, economic–related crimes are committed as a result of the drug users' compulsion to obtain drugs (Goldstein, Brownstein and Ryan, 1992). Seen from an economist's viewpoint, economic–related crimes are more related to the demand side of the illegal drug market, whereas systemic effects can be attributed to the supply side. The high costs of narcotics[2] combined with a possibly low price elasticity resulting from addiction require high income. If the drug addicts can afford their consumption out of current income or wealth, an increase of criminal offences is not to be expected from the demand side. However, a large fraction of drug addicts are younger people without finished education and other groups of the population with low income from legal activities. These addicts might finance their consumption through prostitution or illegal activities like theft and robbery.

Finally, victim/offender use–related crimes include those that are consequential to the consumption of drugs by the victim or offender, since the ingestion of a drug may cause irrational or violent behaviour. This effect of drug abuse is also called the pharmacological effect.

The major part of the empirical evidence in the literature on drug–related crime is based on correlative evidence between drug addiction or drug offences and criminal activity. However, third factors might be responsible for both drug consuming and crime, e.g. poverty and unemployment may be claimed to be the true underlying causes of both use of narcotics and acquisitive crime. In a rare exception to the criminological literature, Otero–Lopez et al. (1994) have found support of this view. Based on a survey of over two thousand male students between 14 and 18 years of age, the authors claimed

[2]Despite a falling trend in prices since the mid eighties due to increased supply, the price per gram of cocaine or heroine is still of the order of magnitude of €50 (Landeskriminalamt Baden–Württemberg 2001, p. 55).

support for the notion that peer, family, and individual factors were more predictive of both risk behaviours than either risk behaviour was on the other.

Economists who follow the general approach of "The Economics of Crime" (Becker 1968, Ehrlich 1973) for testing the deterrence hypothesis are used to control for "third factors". Changing economic incentives can influence crimes as was shown in numerous articles. Recent important examples focus on the impact of unemployment (Raphael and Winter–Ebmer 2001) or show that the introduction of minimum wages has led to a fall in crime rates in the U.K. (Hansen and Machin 2001). However, economists usually ignore the importance of illicit drug use in economic crime studies. Corman and Mocan (2000) and Kuziemko and Levitt (2001) provide remarkable exceptions. Based on monthly time series from New York City Corman and Mocan (2000) compare the relative magnitudes of the effects of local law enforcement activities on crime with the magnitude of variations in drug use on crime. They find that law enforcement effects on crime are stronger and more significant than drug usage, which only has a small effect on property crimes. Kuziemko and Levitt (2001) analyse the effect of imprisoning drug offenders and find that the reduction in violent and property crime associated with adding one additional drug prisoner was almost as great as the reduction in crime when a violent or property offender was sentenced to prison.

The purpose of this study is to provide an econometric assessment of the economics–of–crime model with a special focus on the impact of illicit drugs on crime. Unlike Corman and Mocan (2000), who were restricted to consider deterrence, poverty and drug usage as explanatory variables in their high–frequency data set coming from a big metropolis, we take a broader view to the crime problem, inspired by recent social and economic problems like unemployment, migration, inequality, and demographic changes in Europe. The focus of our contribution has changed from the traditional testing of the deterrence hypothesis to the analysis of socio–economic, drug–driven and demographic factors.

Evidence from a panel of the German Laender (the German "states") allows us to exploit the very heterogeneous experiences in densely populated urban areas such as Bremen and Hamburg (which are also states, so–called "Stadtstaaten", i.e. "city–states") and sparsely populated areas such as Lower Saxony. Some considerable heterogeneity is also given due to the German federal

system, according to which state governments are responsible for their police and the fight against crime within the borders of the corresponding Laender. Given the short time span of data for East Germany, our econometric analysis is restricted to West Germany.

The theoretical modelling of dynamic adjustment through partial adjustment of expected deterrence combined with a statistical model of measurement error in crime rates results in a dynamic specification of our econometric model. In addition, the model might include regional and time specific effects resulting from possible measurement errors due to different shares of underreporting in crime rates and other sources of unobserved heterogeneity.

Our results indicate that ignoring the effect of drug use in empirical models of the economics of crime would lead to an omitted variable bias. Drug offences have a relatively strong effect on property crimes and and on rape although conditional estimates controlling for factors known from the economics of crime literature turn out to be much smaller than correlative evidence would suggest. The deterrence hypothesis of the economics of crime model is confirmed for property crimes, whereas no convincing evidence has been found for violent crime.

The remainder of this article is organised as follows. Section 2 describes general tendencies of drug abuse and crime rates and provides stylized facts on the direct crime–drugs nexus and potential third factors. The econometric modelling framework, the choice of relevant variables and the available data are introduced in section 3. The estimation results are summarized in section 4. They allow for conclusions on the extent to which illegal drug use contributes to the development of overall crime rates. In section 5, we present results of our sensitivity analysis of the estimates. The findings are linked to the public issue of drug related crimes in section 6.

2 Crime, drug offences and potential third factors of crime

2.1 General tendencies

Due to the illegal nature of drug abuse and drug related crimes, it is difficult to obtain reliable data on the extent of these phenomena. Corman and Mocan (2000) use the number of deaths which are due to drug poisoning as a proxy for drug use. Figure 1 shows these numbers for Germany[3] in the lower panel, while the upper panel depicts the number of first time users of drugs, which became noticed by the police.[4]

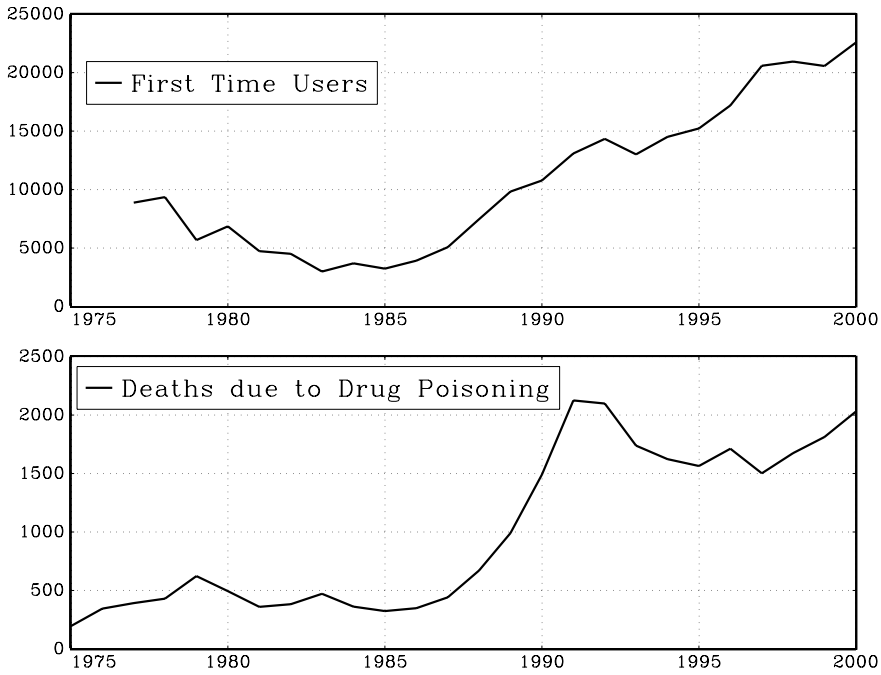
The number of deaths due to drug use are not a very close proxy for the development of drug use for several reasons. First, there exists a time lag between drug use and death caused by drug abuse, since a large fraction of these deaths is caused by physical decay due to persisting drug abuse.[5] Second, the number of deaths is influenced by changing concentration of narcotics within the drugs supplied (Bundeskriminalamt 2000). Furthermore, drug–addicted may move from rural areas to cities during their "drug career". Then, the use of death rates may overstate drug abuse in the German city states compared to the other states. Consequently, estimates based on a panel of German states might be biased using this proxy. Using the number of first time users is not a satisfying proxy either. First, the numbers depend on the awareness of the police officers who get into contact with the drug user. Thus, these numbers might be biased downward for areas where drug abuse is less common. Second, these numbers measure flows into the pool of drug users. Since no estimates of the outflow are available, it seems difficult

[3]The data are for West Germany until 1990, for West Germany and Berlin in 1991 and for Germany since 1992.

[4]By definition of the German Federal Criminal Police Office, first time users of drugs are persons coming in contact to police or customs officers for the first time in relation to the use of illicit drugs. This definition does not imply that all first time users are or become drug addicts.

[5]For example, no relevant number of deaths which are due to drug poisoning were yet reported for East Germany in 1999. In contrast, the number of reported first time drug users, increased by 23.4% in East Germany from 1998 to 1999, while a slight decrease of 1.8% in West Germany for the same time period corresponds to an increase in the number of deaths by 8.2%.

Figure 1: First time users and deaths due to drug posisoning



Source: Bundeskriminalamt (1999, 2000, 2001); own calculations.

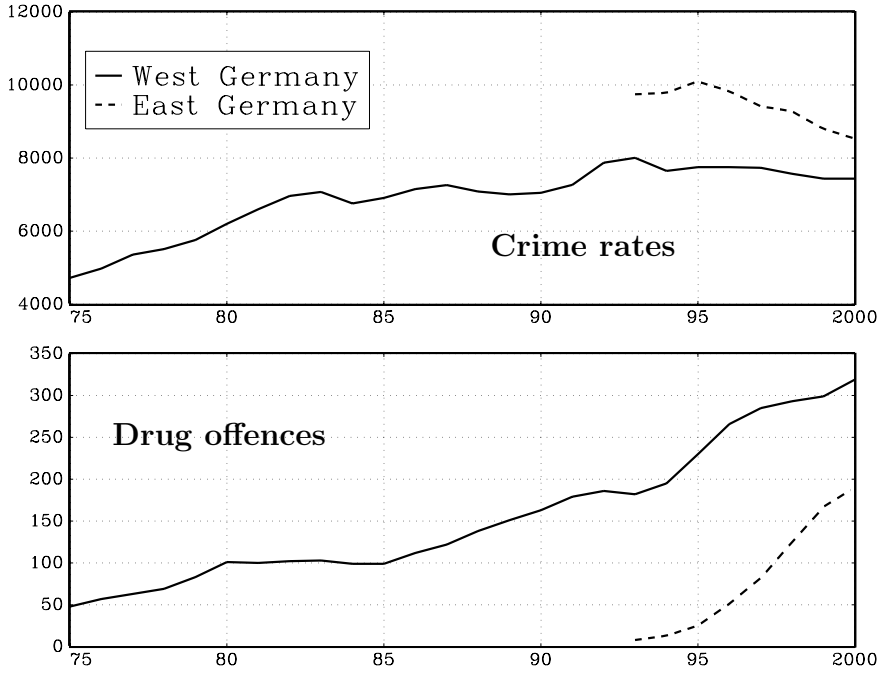
to obtain a proxy of the stock of drug addicts which is more relevant for our analysis of the drug–crime nexus.

Given the limits of these proxies for drug abuse, the numbers on direct drug offences reported by the German Federal Criminal Police Office (Bundeskriminalamt) appear to be a more sensible proxy for the overall development of drug abuse. These numbers include reported cases of illegal drug trafficking, possession and consumption of drugs, but do not include procuring crimes like theft from pharmacies. While this measure shares the drawback to depend on the effort of the police spent on persecuting these crimes,[6] it appears to be the most suitable proxy for monitoring the impact of drug abuse on overall crime rates.

[6]We will control for this effect in the econometric specification (see Section 3).

The relevance of an analysis of drug abuse and its impact on overall crime rates is substantiated by the increase of drug abuse as measured by these numbers from the German Federal Criminal Office. Figure 2 and Table 1 show the development of overall crime rates and offences against drug related laws.

Figure 2: Crime rates and drug offences (cases per 100 000 inhabitants)



Source: Bundeskriminalamt (1999, 2000, 2001); own calculations.

The upper panel of Figure 2 plots the development of overall crime rates in West and East Germany. While West German data exhibit no clear tendency during the 1990s, East German crime rates start at a higher level in 1993 and slowly adjust downwards. The development of drug offences shown in the lower panel shows a rising trend for West Germany and a fast adjustment of East German rates. From the early seventies to the end of the nineties, the numbers increase from less than 50 to almost 300 cases per 100 000 inhabitants for West Germany. However, the increase is not uniform.

In recessionary periods like the early eighties and again in 1992/93, the numbers remain almost constant. In East Germany after unification almost no offences against drug related laws are reported. Reliable numbers are available only from 1993 on. They show a tremendous increase over the seven year period to 2000, when East German figures amounted already to more than 50% of West German figures.

Like all crime related data, these numbers have to be interpreted with some care since an increase might result from a real increase in offences, a stiffening of relevant laws, or an increase of the share of known cases among all cases. However, a stiffening of relevant laws cannot be observed during the 1990s. Thus, although underreporting might have been more pronounced in East Germany immediately after unification, the sharp increase in cases from 1993 to 2000 indicates that the overall trend might be mainly determined by an increase in offences.

In Table 1, a disaggregation is undertaken with regard to the characteristics of the federal states. The first two entries show that the rate of offences is much higher in the city states, which might correspond to a supply side effect on the one side and higher income opportunities on the other. The numbers for the federal states in West Germany with highest and lowest case numbers exhibit some dispersion, which, at least partially, might be attributed to the higher share of urban population in Nordrhein–Westfalen as compared to the rather rural state Schleswig–Holstein. Finally, the last two entries show the enormous increase in cases for the East German federal states with highest (Brandenburg) and lowest (Sachsen) case numbers in 1993.

The statistics of the German Federal Criminal Police Office also provides some further information on the link between (illegal) drug use and other crime categories. Unfortunately, these data are not very comprehensive and do not allow for a causal interpretation. Nevertheless, they may provide some further stylized facts. For example, Bundeskriminalamt (2004), p. 131, indicates that in 2003 16% of robbery cases, which have been solved, were committed by drug addicts. However, a large share of unsolved cases makes it difficult to draw inferences from these findings. Comparable figures for crimes of violence are not provided for drug addicts, but Bundeskriminalamt (2004), p. 132, reports that 62% of violent bill dodging and 41% of solved cases of manslaughter were committed under influence of alcohol. Consequently, at

Table 1: Drug offences (cases per 100 000 inhabitants)

federal state	1993	2003	Change
Federal state corresponding to cities			
Hamburg	437	729	66.8%
Bremen	450	567	26.0%
Other federal states, West Germany			
Max.: Nordrhein–Westf.	213	328	54.0%
Min.: Schleswig–Holstein	85	288	238.8%
Other federal states, East Germany			
Max.: Brandenburg	11	222	1918.2%
Min.: Sachsen	6	221	3583.3%

Source: Bundeskriminalamt (1994, 2004); own calculations.

least the pharmacological effect of drug abuse might have an even stronger impact on overall crime rates, in particular for crimes of violence, when legal drugs like alcohol are also taken into consideration.[7]

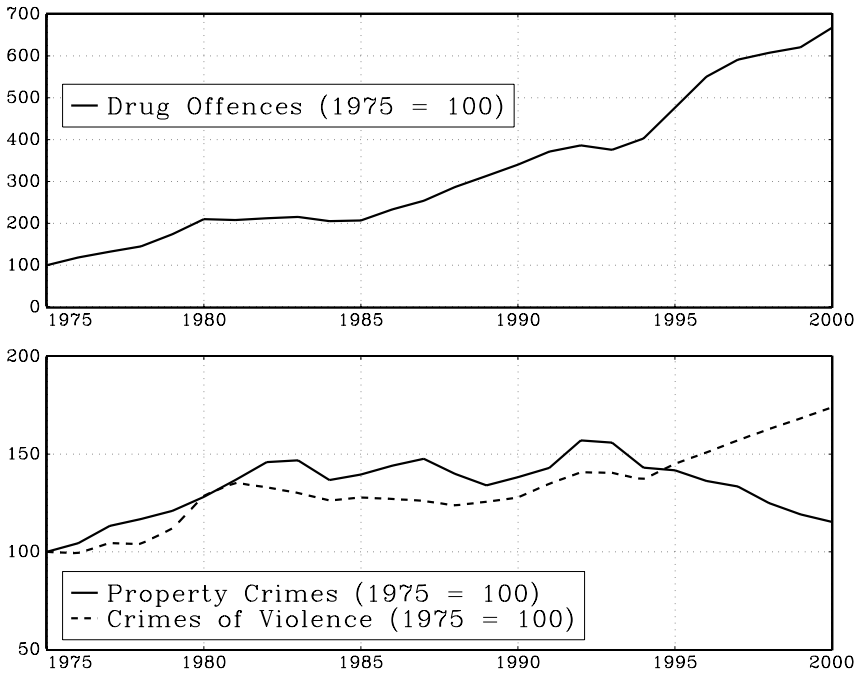
2.2 General crime trends and the direct crime–drugs nexus

Obviously, the reported figures on drug offences do not cover all crimes, which might be related to drug–addiction and the market for illegal drugs. Neither do they include direct procurement crimes such as theft from pharmacies[8] nor further economic or system–related crimes. In order to obtain a first hint on a potential direct crime–drugs nexus, Figure 3 plots the development of drug offences in the upper panel against the developments of crimes of violence and property crimes in the lower panel. All data are for West Germany and indexed to the base year 1975.

[7]For example, Edberg (2004) reports a highly significant effect of alcohol consumption on violent crime.

[8]Surprisingly, the statistics indicate, that only about 60% of these directly drug related crimes are committed by drug–addicted. In Bundeskriminalamt (1998), p. 68, this observation is explained by a non complete assessment of drug–addiction among criminals.

Figure 3: Trends in crime rates (West Germany)



Source: Bundeskriminalamt (1999, 2000, 2001); own calculations.

The crime rates of all considered crime categories exhibit a positive trend. However, the trend in drug offences is much more pronounced than in the other crime categories. The correlation between drug offences and the other two crime categories is substantial. In fact, the correlation with crimes of violence amounts to 0.80, and the correlation with property crimes to 0.61.[9] However, given that the trends in all crime categories might be determined by further (common) factors, it is not appropriate to draw far reaching causal conclusion from these findings. In particular, it is not adequate to deduce a stronger effect on system–related crimes, which are more likely to be crimes of violence, relative to the effect on economic–related crimes, which fall into the category of property crimes. An assessment of the impact of drug offences on

[9]The highest correlation of drug offences with a more disaggregated set of crime categories is found with theft. It amounts to 0.93.

overall crime rates requires a comprehensive model which controls for third factors.

To a certain extent, higher crime rates might be the result of a more intensive work of the police itself. This surprising conclusion might be drawn from Figure 4, which shows growth rates of expenditures on police per capita and (total) crime rates in Western Germany. The high positive correlation of 0.47 indicates that a better financial endowment enables the police to light up the share of "dark figures" of official crime statistics, since more criminal acts can be processed and registered.[10] Since the share of unreported crimes sheds doubts on the official crime statistic as a basis upon researchers wish to make inferences, we need to consider this helpful information in our econometric model (see Section 3).

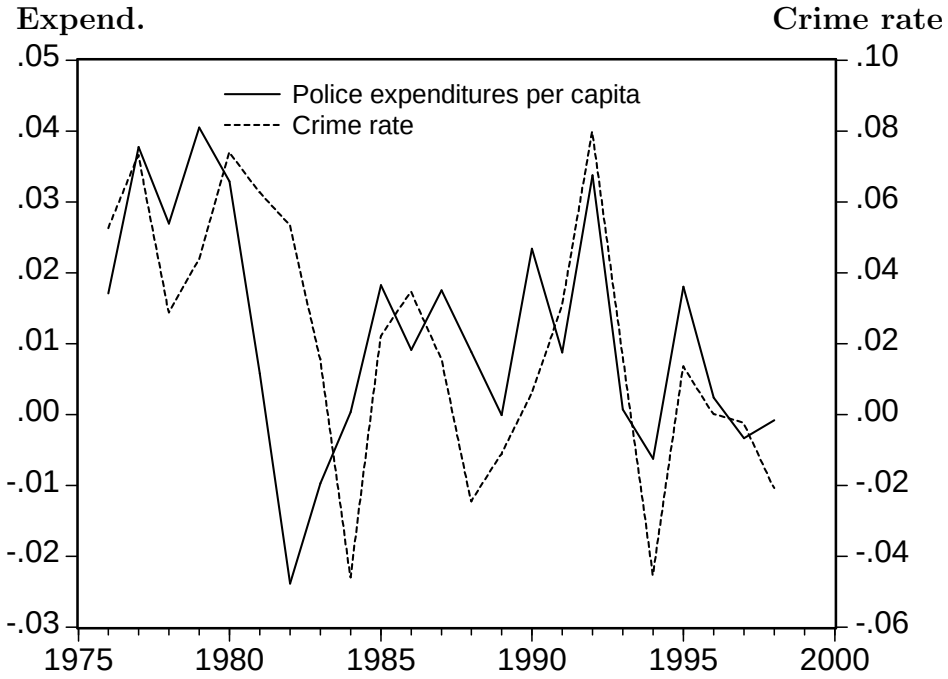
2.3 Potential third factors

Third factors, which might influence the crime–drugs nexus, are all those, which determine drug consumption, overall crime rates or both. Consequently, all the socio–economic variables, found to be important in the econometric modelling of crime rates, have to be considered, e.g. measures of income and income distribution (GDP, unemployment, share of young men), a distinction between rural areas and cities, or political factors such as ruling party or coalition. Since these factors might also be linked to drug consumption, Table 2 shows some correlations between the number of drug offences per inhabitant and these factors.

Again, the bivariate correlations do not allow for a causal interpretation. For example, the positive correlation between a conservative government and the number of drug offences at the federal level might be due solely to the fact that a conservative government was in place for the whole period 1981 – 1997, i.e. during the period with highest increase in the number of drug offences. Nevertheless, the results indicate the potential importance of neglecting third factors, which exhibit some statistical correlation both with drug offences and

[10] As can be seen from the calculation of cross–correlation coefficients, police expenditures are rather leading than lagging with respect to crime rates: The coefficient is 0.46 if it is estimated using a one–year lag of crime, and it is -0.10 if crime would lead by one year.

Figure 4: Growth rates of police expenditures and crime rates



Source: Bundeskriminalamt (1999, 2000, 2001), Öffentliche Finanzen, (various years); own calculations.

overall crime rates.

Our econometric model will include two–way fixed effects, i.e. for the German Laender and each year in order to control for further unobserved factors, e.g. the development of prices on the market for illegal drugs.[11]

3 Econometric specification and data

The econometric model to test for the influence of drug offences on other crime rates is embedded in the standard theoretical framework of "The Eco-

[11] Unfortunately, the price data are not yet available on a Laender base.

Table 2: Drug offences and third factors

Correlation between drug offences and	Cross section of West German Laender in 1990	Time series for West Germany 1975–1997
Unemployment rate	0.71	0.61
Share of young men	-0.50	-0.76
GDP (1991 = 100) per capita	0.83	0.93
City states	0.89	—
Ruling party is conservative	-0.47	0.68

Source: Bundeskriminalamt (1994, 2000); own calculations.

nomics of Crime" provided in Becker's (1968) seminal article. Becker's theory of deterrence is an application of the general theory of rational behaviour under uncertainty. Optimising individuals engage in criminal activities when expected payoffs of the criminal activity exceed the costs of criminal activity, mainly given by the probability and severity of sanctions. Ehrlich (1973) extended Becker's model by considering a time allocation model. Since time can be allocated to legal and illegal activities, besides deterrence "third" variables of legal and illegal income opportunities start to play a central role in empirical tests of the Becker–Ehrlich model, approximated by abilities, family income, human capital, and other socio–economic variables.

These considerations have led to the basic Becker–Ehrlich specification of the so-called "supply of offences" (see Grogger 1991, Ehrlich 1996, Levitt 1996, Corman and Mocan 2000, Edberg 2004, and Entorf and Spengler 2000, for recent applications). From this theoretical background, we derive the base model in logarithmic notation as

$$\ln O_{it} = \beta \ln P_{it}^{e} + \gamma' X_{it} \,, \tag{1}$$

where O_{it} is the crime rate in region i and period t (number of offences per 100,000 inhabitants), P_{it}^{e} is the expected probability of detection and X_{it}

represent other explanatory variables like income opportunities, demographics and illicit drug use. Given that laws applying to offenders are identical in all German states and did not change substantially during the sample period, the expected probability of detection is an adequate measure of deterrence. The significance of deterrence seems to be well documented for the US, where recent contributions by Corman and Mocan (2000) and in particular by Levitt (1996, 1997, 1998) confirm early results of, for instance, Ehrlich (1973). In our German case study, deterrence is measured by the expected percentage of (registered) offences cleared by the police. Unfortunately, such a variable might cause some problems because the denominator of this variable (the number of crimes) is the dependent variable and some reverse causation effect might be present. However, in the dynamic model (3) used for our empirical analysis this ratio bias is avoided since only lagged values of the deterrence variable have to be included (see Levitt, 1998).

The expectations about the probability of detection are modelled in a partial adjustment framework, which assumes that expectations are persistent and partially adjusted to expectation errors, i.e.

$$\ln P_{it}^{e} = \ln P_{i,t-1}^{e} + \lambda \left(\ln P_{i,t-1} - \ln P_{i,t-1}^{e} \right) , \tag{2}$$

where λ measures the speed of adjustment. It ranges between 0 (no adjustment of expectations to observed detection rates at all) and 1 (full adjustment of expected detection rates).

Solving this partial adjustment model and inserting the solution into the base model of economics of crime (1), the following dynamic model results, where the restrictions imposed on some of the coefficients result from the partial adjustment model (2):

$$\ln O_{it} = \lambda\beta \ln P_{i,t-1} + \gamma' X_{it} + (1-\lambda)\gamma' \ln X_{i,t-1} + (1-\lambda) \ln O_{i,t-1} . \tag{3}$$

Empirical investigations on the causes of crime suffer from the fact that a substantial share of crimes is not registered by the police. Furthermore, the variable might be subject to measurement error. Consequently, the number of crimes reported to the police in region i during time period t can be modelled as

$$O_{it}^{*} = \theta_i^{*} \delta_t^{*} O_{it} \nu_{it}^{*} , \tag{4}$$

where θ_i^* is the share of crimes in region i reported to the police, δ_i^* the share of crimes during time period t reported to the police (normalized with regard to the regional effects), O_{it} the actual number of crimes committed in region i during time period t, and ν_{it}^* a positive measurement error. Taking logarithms gives

$$\ln O_{it}^* = \ln O_{it} + \theta_i + \delta_t + \nu_{it}\,, \tag{5}$$

where θ_i, δ_t and ν_{it} denote the logarithms of θ_i^*, δ_t^* and ν_{it}^*, respectively. Thus, different shares of crimes are reported to the policy due to regional and/or time–specific influences on reporting behaviour.

Solving (5) for O_{it} and inserting in (3) provides the econometric specification for the observable variables

$$\begin{aligned} \ln O_{it}^* \;=\; & \lambda\beta \ln P_{i,t-1} + \gamma' X_{it} + (1-\lambda)\gamma' \ln X_{i,t-1} + (1-\lambda)\ln O_{i,t-1}^* \\ & + \lambda\theta_i + \delta_t - (1-\lambda)\delta_{t-1} + \nu_{it} - (1-\lambda)\nu_{i,t-1}\,. \end{aligned} \tag{6}$$

Resulting from the theoretical modelling of dynamic adjustment through a partial adjustment of expected deterrence in combination with a statistical model of measurement error in crime rates, we obtain a dynamic specification and the suggestion to test for state and time specific effects which might result from unobserved heterogeneity, in particular, differing shares of crimes reported to the police.[12] Finally, since no assumption about independent distribution of the terms in (4) has to be made, the error terms of (6) do not have to be autocorrelated. Nevertheless, it is indicated to test for this eventuality.

Including both state– and time–specific effects in the estimated model, i.e. applying a two–way fixed effects model, we also take into account institutional changes as, for instance, changes of the federal law, or other unobserved heterogeneity that applies to all cross–sectional units simultaneously. Statistical inference in applied fixed–effects panel econometrics is often based on (asymptotic) standard errors of the ordinary least squares estimator without consideration of any potential serial correlation of estimated residuals which would render standard t–values and F–statistics of only descriptive

[12]It should be noted, however, that this conclusion requires the assumption of a time invariant structure of unreported crimes between states. Experience with similar data (Entorf 1996, Entorf and Spengler 2000) has shown that the inclusion of state effects also covers the explanatory power of population density.

use. Also for the reason mentioned above, we inform about potential serial correlation by calculating a statistic provided by Bhargava, Franzini and Narendranathan (1982), who modified the classical Durbin–Watson statistic for the use with panel data, which we call BFN–DW.[13]

In order to cover the whole spectrum of the potential drugs–crime nexus, we estimate the model (6) for different crime categories. In particular, we are interested in the explanatory power of drug offences for murder, rape, assault, i.e. violent crimes that might be motivated as system–related crimes or result from pharmacological factors, and for robbery and theft (with and without aggravating circumstances), i.e. property crimes that are possibly drug–driven in the sense of economic–related crimes.[14]

The data for the estimation of model (6) consist of a balanced West German panel containing annual data for the years 1976 – 1995 from all 10 states (Laender) that formed the Federal Republic of Germany prior to the German unification in 1990 with the sole exception of Berlin. Berlin, which contained a West and East German part, is removed from our empirical analysis.[15] During the years following the unification there were difficulties in the registration of crimes and clear–ups in the five new states (Brandenburg, Mecklenburg–Vorpommern, Saxony, Saxony–Anhalt, Thuringia).[16] In order to rely on relatively long time series, and since drug related crimes would need a different approach in East Germany, we refrained from includ-

[13]The DW–statistic might be biased towards two in the presence of a lagged dependent variable. As there is a strong cross–sectional dimension, this issue is of minor importance in the case of panel data. Moreover, note that both DW as well as BFN–DW are used as descriptive statistics indicating problems of misspecification.

[14]The exact econometric identification of system–related crimes, economic–related crimes and of pharmacological effects is not subject of this paper. Tackling this aim would require information on prices of illicit drugs, which however, are not available for Germany at disaggregate levels over a long period of time. In our paper we try to find out whether there is any — perhaps confounded — influence stemming from illicit drugs in economics of crime models.

[15]In fact, the Land Berlin consisted of former West–Berlin and East–Berlin after 1990. This property and the fact, that Berlin became the capital of Germany suggests a special situation for Berlin as pointed out by a referee.

[16]According to notes provided in our data source (Bundeskriminalamt (1996)) East German police statistics of the years 1990 to 1992 are biased downwards due to administrative adjustment problems. Thus, 1993 is the first year after unification which allows for a reasonable comparison between East and West German crime figures.

ing data from the five East German states.

Crime and clear–up rates are taken from the German Federal Criminal Police Office. We use the rate of drug related crimes per inhabitant (DRUGS) for the German Laender as proxy for the use of illicit drugs. This variable comprises illegal drug consumption and trade with narcotics, but does not include drug related crimes, such as theft of drugs from pharmacies or theft in order to obtain money for drug consumption. Therefore, our proxy variable does not simply coincide with "economic–related crimes" of drug offenders in the sense of Goldstein (1985). In most studies, the effect of deterrence variables (clear–up or conviction rates, length of arrest, fines) are found to be more or less negative, i.e. in line with predictions from theory (see, for instance, Eide, 1994, for a survey). In our specification, as in most applications of the Becker–Ehrlich model, we measure deterrence by clear–up rates. We refrain from testing deterrence by use of an indicator of the severity of punishment, because the identification of state–specific law interpretations is difficult to obtain and is left to future research.

Legal income opportunities are represented by the unemployment rate (UR), i.e. the probability to find a legal job. The data for this variable is taken from annual reports of the Federal Employment Service (Bundesanstalt für Arbeit). Unemployment rates measure absence of legal income opportunities, and are integral part of most empirical models of the Becker–Ehrlich type, although the expected positive sign cannot be observed in many econometric studies (Chiricos 1987, surveying 68 studies, shows that fewer than half find positive significant effects). Raphael and Winter–Ebmer (2001) argue that the failure to control for variables that exert pro–cyclical pressure may downwardly–bias estimates of the unemployment–crime effect. Given our measure of income, in line with Raphael and Winter–Ebmer (2001), Cook and Zarkin (1985) and related literature, we are able to adjust for any omitted variable bias of this kind. Furthermore, we use data from the Federal Statistical Office of Germany (Statistisches Bundesamt) to calculate state specific real GDP per capita (GDP_H) as an additional income measure. The expected sign of this variable is not evident a priori. Persons who choose between legal and illegal income opportunities, and who are looking for a legal job and/or certain reservation wage levels, would be more successful in prospering regions, suggesting that better legal income opportunities would lead to a negative sign. On the other hand, states that do better than the

average provide lucrative targets and attract potential criminals who, moreover, might leave degrading regions. Such "crime migration" would result in a crime enhancing effect, i.e. a positive sign.

Demographic factors are strongly correlated with crime, at least in a bivariate framework. For instance, of 100 suspects in 1999, more than 75 are male, and more than 40 are younger than 25 years. Young men aged 15–24 years are suspected to have committed 27% of all registered crimes, whereas the population share of this group is only 6% (Bundeskriminalamt, 2000). These facts have led us to consider the share of young men under 25 years of age (`M15-24`) in the crime–supply equation. The data for this and other demographic factors are all calculated based on data supplied by the Federal Statistical Office of Germany. Besides gender and age, the share of migrants (foreign citizens) exhibits a positive correlation with some crime rates. For example, in 1999, more than 25% of all suspects were migrants, whereas the population share is about 9% (Bundeskriminalamt, 2000). It should be noted, however, that there are many reasons why foreigners might be over–represented in the group of suspects.[17] However, in order to avoid potential omitted–variable biases, we have included the share of migrants in the population (`MIGRANT` as a further demographic factor in our set of explanatory variables. It might also serve as a proxy for overall mobility of the population and ethnic heterogeneity.

As demonstrated in Section 2 of our paper, expenditures on German police are positively correlated with registered crime, most probably because additional financial means could be used by the police to reduce the share of unreported crimes. Thus, since the dependent variable is registered crime, available from official police statistics, and not (unobservable) actual crime, it is important to control for such "crime producing" factors in order to achieve

[17]First, they may be more often wrongly suspected than the native population. Second, there are some laws – like the foreigner and asylum laws – which can, by definition, only be broken by foreigners. Third, migrants residing in Europe are to a higher percentage young men. Fourth, some migrants may be in European countries after fleeing their homeland, because they were offenders there. Finally, most migrants enter European countries, because they had no economic success in their home country. The latter may be due to factors that foster crime, for example, lack of education. These points should be kept in mind when judging the coefficients of the migrant variable in our empirical results (see also Entorf and Larsen (2004) for a detailed description of the link between immigration and documented crime).

crime rates adjusted for distortions arising as a consequence of varying government expenditures. It has been argued before how our econometric model covers some of these aspects by introducing a partial adjustment framework and allowing for two–way fixed effects. Nevertheless, we also include the police expenditures per capita (`POL_H`) as an additional control variable. Data on police expenditures can be found in statistics on public finance (Öffentliche Finanzstatistiken) published by Federal Statistical Office of Germany.

Finally, the description of our data seems to suggest that conservative governments are more successful in fighting or preventing crimes. In addition to correlative evidence presented in Section 2, it might be interesting to note that in our sample (1976 – 1995) conservative party participation in state governments is associated with crime rates being 33.7% lower in conservatively governed states and government periods. Of course, there are other ways of interpretation. For instance, voters of Christian–democrat parties may be more law–abiding than voters of other parties. Another explanation may be that conservative parties are more successful in rural or wealthy regions, whereas crime is an urban phenomenon, located in cities with social problems like unemployment and illicit drug use, rendering bivariate correlation coefficients potentially spurious. To judge suppositions of this kind would require the consideration of the conservative government effect in a multivariate context. In our framework, we use a dummy variable `CONSERV` indicating whether conservative parties (CDU or CSU) belong to the ruling coalition of the respective state at time t.

Table 3 provides some descriptive statistics for all the variables used in our estimations.

4 Estimation results

Panel regression results are summarised in Table 4 and Table 5. The results in Table 4 are based on the dynamic specification (6) with fixed effects for the Laender, while the results for two–way fixed effects are reported in Table 5. For a discussion of alternative specifications see the discussion of our sensitivity analysis reported in Section 5 below. The BFN–DW–statistics are also reported. They do not indicate any remaining serial correlation which

Table 3: Descriptive statistics of pooled data

Variable			Mean	Std.Dev.
O1 - O6	=	Crime rates calculated as number of crimes known to the police per 100,000 inhabitants		
O1	=	Murder and manslaughter	4.94	1.86
O2	=	Serious assault	112.01	40.94
O3	=	Rape	11.03	4.73
O4	=	Robbery	65.45	63.18
O5	=	Theft under aggravating circumstances	2885.86	1833.21
O6	=	Theft without aggravating circumstances	2047.30	754.62
P1 - P6	=	Clear–up rates calculated as ratio of cases cleared up to cases known to the police		
P1	=	Murder and manslaughter	94.01	4.18
P2	=	Serious assault	84.17	5.05
P3	=	Rape	71.30	5.80
P4	=	Robbery	49.41	7.50
P5	=	Theft under aggravating circumstances	16.36	4.95
P6	=	Theft without aggravating circumstances	46.70	5.88
DRUGS	=	Number of drug offences per 100,000 inhabitants	149.55	116.67
MIGRANT	=	Percentage of foreign citizens in the population	7.21	2.93
GDP_H	=	Real gross domestic product per capita in prices from 1991 (in Euro)	19247.55	5197.07
UR	=	Unemployment rate	7.82	3.16
M15-24	=	Percentage of males aged 15–24 in the population	7.45	1.06
POL_H	=	Police expenditures (in Euro) per capita in prices from 1991	108.40	35.50
CONSERV	=	Dummy variable that takes the value 1 if the state is ruled by a Christian party (CDU or CSU), or if a Christian party belongs to the ruling coalition of political parties	0.46	-

could point at a potential misspecification of the dynamic model.

Table 4: Estimation results, dynamic model, state effects

Independent Variables	Parameter	Theft u.a.c.	Theft w.a.c.	Robbery	Murder	Assault	Rape
Partial Adjustment	λ	0.43**	0.46**	0.34**	0.57**	0.23**	0.46**
		(0.07)	(0.05)	(0.05)	(0.07)	(0.04)	(0.07)
log ($\mathtt{P}_{t-1}$)	β	-0.30**	-0.33*	-1.05**	0.03	1.35**	0.32
		(0.09)	(0.14)	(0.22)	(0.54)	(0.62)	(0.26)
log (DRUGS)	γ_1	0.07*	0.08**	0.08*	0.03	0.08**	0.16**
		(0.03)	(0.02)	(0.04)	(0.07)	(0.03)	(0.05)
log (GDP_H)	γ_2	-0.07	-0.37**	0.06	-0.82*	0.06	-1.22**
		(0.20)	(0.14)	(0.26)	(0.36)	(0.22)	(0.29)
log (UR)	γ_3	0.17**	-0.05	0.07	0.10	-0.00	-0.14**
		(0.03)	(0.03)	(0.04)	(0.06)	(0.03)	(0.04)
log (M15-24)	γ_4	0.83**	0.39**	0.74**	0.59**	0.37**	0.76**
		(0.12)	(0.09)	(0.16)	(0.21)	(0.14)	(0.18)
log (MIGRANT)	γ_5	0.34**	0.45**	1.00**	0.52**	0.51**	0.25
		(0.10)	(0.07)	(0.13)	(0.19)	(0.10)	(0.15)
log (POL_H)	γ_6	0.47*	0.24	0.16	0.42	0.44*	0.09
		(0.20)	(0.14)	(0.24)	(0.40)	(0.18)	(0.31)
BFN–DW–statistics		1.77	1.97	2.25	2.06	2.14	2.05
Adjusted R–squared		0.986	0.978	0.985	0.790	0.966	0.929

Notes: Standard errors in parantheses, **, * denote significance at the 1% and 5% level, respectively; fixed state effects included; sample: West German states without Berlin, 1976–1996 (200 observations). See equation (6) for the econometric specification and the role of parameters therein.

The economics of crime literature presumes variables of deterrence among the most important third factors. Our estimation results in Table 4 show the expected negative sign of the clear–up rate for the first three crime categories covering property crimes. In fact, for these crimes, a significant negative impact of the deterrence variable P is found. The magnitude of the estimated elasticities for property crimes is in line with results reported in the literature. For instance, Eide (1994, p. 156) reports a median of -0.34 based on the evaluation of a sample of several studies. In contrast, for violent crimes, the coefficient of the deterrence variable is positive and for assault even significant. Overall, we might conclude that there is evidence in favour of the deterrence hypothesis of the economics of crime model for property crimes, but not for violent crimes.

The drugs–crime nexus appears significant for all crime categories except murder. The estimates of the drug impact range between insignificant and 0.19 for rape in the two–way fixed model (see Table 5), while most values are between 0.06 and 0.10. The estimated parameter on rape appears surprisingly high compared to other crime categories, but is robust across different specifications. This result might be at least partly explained by the pharmacological effect of illegal narcotics, but deserves a closer analysis. Given the moderate values of the estimated elasticities for the other crime categories, suppositions about the dominating influence of drug addiction on crime need to be put in perspective. The inclusion of third factors has shown the spurious nature of high bivariate correlation coefficients.[18]

Estimated results are similar to those of Corman and Mocan (2000), who report significant effects for burglary and robbery, but who did not detect any significance of drug use on both murder and assault (there was no analysis for rape). The latter outcome is somewhat surprising for the U.S., given the high death rate among the members of a drug selling gang reported in Levitt and Venkatesh (2000).

Higher wealth as mirrored by the variable `GDP_H` exhibits the expected negative impact on all crime categories where significant. Higher income means both reduced incentives to commit property crimes and more safety with regard to violent crimes. Unemployment `UR`, however, has the ambiguous result usually found in the literature. Positive (crime enhancing) significant effects of higher unemployment can be found for theft u.a.c., whereas estimated coefficients are insignificant for other categories and even significantly negative for rape. The reason for the existence of ambiguous results is often discussed in the literature, but we suggest to keep in mind that many studies (like ours) are dealing with regional data. Evidence using information on the mobility and origin of offenders in the German state Baden-Württemberg (Büttner and Spengler, 2003) reveals that criminals prefer regions with low unemployment over regions with high unemployment, a result that also holds for violent crimes, and that might be explained by the presence of more lucrative targets in advantaged regions. Thus, in exactly the same way as GDP p.c., also unemployment rates can be interpreted in the sense of both legal

[18]For instance, regressing "log (theft under aggravating circumstances)" (theft u.a.c.) on "log (drug offences)" using pooled OLS (without any further regressor) would lead to a highly significant regression coefficient of 0.47 (see Tables 6 and 8).

and illegal income opportunities, such that also negative signs are plausible when offender mobility is taken into account.

The effect of demographic variables does not seem to differ substantially between property and violent crimes. Higher shares of young men are always linked to significantly higher crime rates, while a higher share of migrants as a measure of ethnic heterogeneity and mobility has a significant positive impact for all crime categories except rape.

Contrary to expectations based on simple correlation analysis, police expenditures do show up slightly significantly only for two crime categories. This lack of significance might be explained by considering a regression of police expenditures per capita on fixed time and region effects. After adjusting for serial correlation, the adjusted R–squared of this unreported regression is 0.994, hence almost all variation can be explained by cross sectional differences, aggregate cyclical variation of the Federal budget and by serial correlation, all of which are controlled for in our specification of considered crime categories.

Also contrary to the results of a simple correlation analysis, government participation of conservative parties does not show any significant impact on (recorded) crime. After controlling for third factors given by demographic movements, unemployment, income possibilities etc., "conservative governments" do not perform any better than non–conservative governments. Therefore, this variable has been omitted from the final specifications.

For Table 5, the estimated model comprises both state and time specific effects. Possibly due to the high collinearity between some explanatory variables, their lagged values and time effects, the significance of some estimated parameters decreases when time effects are included. For example, the strong evidence on a significant effect of the share of young men (`M15-24`) disappears for theft under aggravating circumstances and assault. Furthermore, the estimated effects of deterrence become smaller and several variables loose significance.

Summing up, property crimes fit the economic model of crime much better than violent crimes, a result which is not surprising given that property crimes are supposed to be closer to the idea of the "rational" offender, whereas violent crimes often seem subject to irrational behaviour. The phar-

Table 5: Estimation results, dynamic model, two–way fixed effects

Independent Variables	Parameter	Theft u.a.c.	Theft w.a.c.	Robbery	Murder	Assault	Rape
Partial Adjustment	λ	0.44**	0.39**	0.31**	0.60**	0.22**	0.44**
		(0.07)	(0.06)	(0.06)	(0.07)	(0.05)	(0.08)
log (P_{t-1})	β	-0.32**	-0.29	-1.15**	0.26	0.50	0.27
		(0.07)	(0.20)	(0.26)	(0.57)	(0.74)	(0.31)
log (DRUGS)	γ_1	0.06*	0.10**	0.07	-0.08	0.08**	0.19**
		(0.03)	(0.02)	(0.04)	(0.08)	(0.03)	(0.06)
log (GDP_H)	γ_2	-0.43	0.04	-0.25	-1.41*	0.26	-0.76
		(0.26)	(0.22)	(0.39)	(0.60)	(0.35)	(0.51)
log (UR)	γ_3	0.18*	0.00	0.00	0.10	-0.13	-0.03
		(0.08)	(0.06)	(0.11)	(0.17)	(0.09)	(0.14)
log (M15-24)	γ_4	0.42	0.77**	1.27*	1.78**	-0.37	1.52**
		(0.30)	(0.24)	(0.54)	(0.62)	(0.52)	(0.54)
log (MIGRANT)	γ_5	0.27	0.10	0.42	0.34	-0.11	0.11
		(0.17)	(0.13)	(0.27)	(0.35)	(0.25)	(0.32)
log (POL_H)	γ_6	0.03	0.20	0.07	0.04	0.12	0.52
		(0.20)	(0.15)	(0.26)	(0.49)	(0.22)	(0.37)
Significance of time effects		Yes	Yes	Yes	No	Yes	Yes
BFN–DW–statistics		2.14	2.17	2.33	2.05	2.10	2.14
Adjusted R–squared		0.992	0.985	0.989	0.789	0.970	0.936

Notes: Standard errors in parantheses, **, * denote significance at the 1% and 5% level, respectively; fixed state and time effects included; sample: West German states without Berlin, 1976–1996 (200 observations). See equation (6) for the econometric specification and the role of parameters therein.

macological effect of drugs and the systemic effects related to the climate of the notoriously violent drug scene seem to be responsible for the high coefficient on rape. The economics of crime model performs best for theft u.a.c., both with respect to fitting the data and to accordance with effects predicted by theory.

Furthermore, the impact of drugs appears stable and robust. Nevertheless, omission of drugs does not result in severe signs of misspecification, but imply somewhat higher parameter estimates for ethnic heterogeneity and a small positive bias on police expenditures (see our sensitivity analysis below).

5 Sensitivity analysis and comparison of results

Some additional estimation results presented in the Appendix reveal the robustness of our results. They are presented in Tables 7 – 9, where we first consider the extent of the omitted variable bias stemming from the neglect of illicit drug use in economics of crime models, we then present the robustness of drug-use elasticities of crime in response to various econometric specifications, and finally test the performance of "drug deaths" as alternative measure of illicit use of narcotics.

We have performed estimations without consideration of the drugs–crime channel (see Appendix, Table 7). This is interesting for two reasons. First, we are able to quantify the degree of bias caused by omitting the influence of drugs in econometric models of crime. Second, there might be some simultaneity bias stemming from endogenous drug use, when illicit use of narcotics arises as a consequence of crime, for instance when within growing criminal milieus drug abuse belongs to the everyday pattern of criminal social interaction. However, results reveal that omitting drugs from the model has only a minor impact on the estimation of other effects. Comparing Table 7 to Table 4, we observe that the main difference lies in somewhat larger parameter estimates for both `MIGRANT` and `POL_H`. As an important consequence though, the influence of ethnic heterogeneity would be erroneously significant for rape when drug offences were omitted. Since high shares of foreigners, police expenditures and drug offences all are phenomena of urban and densely populated areas, these variables are highly correlated in a regional setting like ours, but not necessarily in any causal way. Leaving out one of these variables bears the danger that the other variables spuriously cover the effect of the neglected factor, which is a property of the classical omitted variable bias that seems to take place here, albeit in a moderate way.

Estimates of drug–use elasticities have shown parameter estimates ranging between 0.03 and 0.16 in Table 4 and -0.08 (which is the insignificant parameter estimate on murder) and 0.19 (for rape) in Table 5, with the medians being 0.08 (Table 4) and 0.075 (Table 5), respectively. Results appear rather stable within a small range except for the rape equation, which produces the maximal estimated drug elasticity. Table 6 summarizes results from Ta-

bles 4 and 5 and from further estimates listed in the appendix (Table 8) and puts them into perspective. Table 8 considers unconditional estimates, i.e. log–log specifications with log(DRUGS) as sole explanatory variable, but with different kinds of control for unobserved heterogeneity. Here, three different specifications are tested for each crime category: a) pooled OLS, b) two–way fixed effects (state and year effects), c) two–way fixed effects plus correction for serial correlation of residuals (which are treated as autoregressive process of order one). Looking at the pooled OLS estimate, it becomes obvious that the absence of any other control variable would produce spuriously high estimates. For instance, theft u.a.c. and robbery would be highly sensitive to illicit drug use with estimated elasticities being 0.47 and even 0.69, respectively (see also the first row in Table 6). Estimated parameters change substantially when state and time effects are included in order to cover time series and cross–sectional variance, estimates in equations for murder and rape even change their sign (Table 8, and second row in Table 6). However, applying the BFN–DW residual diagnostic test for identification of general model misspecification in the time series dimension,[19] there are severe signs of misspecification also after including state and time effects (BFN–DW ranges between 0.34 for robbery and 0.94 for murder, i.e. both deviate significantly from two). In order to alleviate bias from misspecification, each category is re–estimated under consideration of autoregressive state–specific residuals, i.e. each regression is based on two–way fixed effects and additionally corrected for serial correlation such that BFN–DW no longer indicates problems of misspecification. Inspecting Table 6 (3rd row), we observe that these results from a pure time–series approach almost coincide with those of our full (dynamic) models in Table 4 and 5 (Table 6, rows 4 and 5), and we confirm rape being an outlier from an otherwise rather homogenous set of drug elasticities.

When we compare our results to estimates reported by Corman and Mocan (2000) (henceforth CM), it should be noted that we use a different measure of illicit drug use (we use drug offences and not deaths due to drug poisoning; see Section 2 for an explanation of the failure with regional data sets, and attempts at applying drug deaths below), different econometric methods

[19]The Durbin–Watson statistic is designed to detect errors that follow a first–order autoregressive process. As is well known from the applied econometrics literature, this statistic also fills an important role as a general test of model misspecification. See, for example, the discussion in Gujarati (1995, pp. 462–464).

Table 6: Summary of drug–use elasticities of different types of crime dependent on model specification

Specification	Theft u.a.c.	Theft w.a.c.	Robbery	Murder	Assault	Rape
DRUGS only, pooled OLS	0.47	0.23	0.69	0.16	0.30	0.12
	(0.05)	(0.03)	(0.04)	(0.03)	(0.02)	(0.04)
DRUGS only, two–way fixed effects	0.18	0.13	0.42	-0.02	0.16	-0.11
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.03)
DRUGS only, two–way fixed effects	0.08	0.08	0.09	0.03	0.08	0.19
correction for serial correlation	(0.03)	(0.02)	(0.04)	(0.06)	(0.03)	(0.06)
Full model: state effects (Table 4)	0.07	0.08	0.08	0.03	0.08	0.16
	(0.03)	(0.02)	(0.04)	(0.07)	(0.03)	(0.05)
Full model: state and time effects	0.06	0.10	0.07	-0.08	0.08	0.19
(Table 5)	(0.03)	(0.02)	(0.04)	(0.08)	(0.03)	(0.06)

Notes: See Tables 4, 5 and 8 for estimation details; standard errors in parantheses

(panel econometrics versus time series analysis) and different observational units (German states versus New York City). In spite of these differences, there are similarities between our results and those presented by Corman and Mocan (2000). With respect to violent crimes, in both articles no significant relationship is found between drug-use and murder (rape is not considered by CM). In both articles, property crimes were found to be affected by illicit use of narcotics. As far as coinciding property crime categories are considered (CM estimate robbery, burglary and motor-vehicle theft), estimated drug–use elasticities of robbery are higher in New York than in Germany (0.18 to 0.28 versus 0.07 to 0.08). Different results are found for assault, which is insignificant in New York, but significant (0.08) in Germany. Hence, CM can draw the conclusion that combating drug–use is less important for violent crimes which is not readily supported by German evidence, in particular when the rather clear and stable effect on rape is taken into consideration. To put these German and US results into perspective, it is interesting to compare them with alcohol consumption elasticities found for Sweden. Edberg (2004) provides evidence for both violent and property crimes. In her preferred (so called 'baseline') specifications she found an insignificant elasticity for aggregate property crime where she estimated alcohol elasticity to be 0.09, and a significant alcohol elasticity of aggregate violent crime of about 0.28. In her category–specific estimation, the largest alcohol consump-

tion effect is reported for rape, although the very high parameter estimate of 0.730 (which is the highest among all reported categories) is insignificant (and significant only at the 18%-level, respectively). Contrary to CM, she found significant results for murder and assault. By way of summing up this international evidence on pharmacological and systemic effects, we observe that most addiction/intoxication related elasticities of crime range between 0.05 and 0.30, and that no clear distinction can be made between violent and property crime.

The variable "drug offences" is replaced by the "number of deaths due to drug poisoning" in Table 9 (see Appendix). The sample, too, has changed since the latter variable has only been available since 1984 at the disaggregate level. Drug deaths, the indicator of drug problems used by Corman and Mocan (2000), works in neither case for our kind of regional data: estimated drug-use elasticities range between 0.00 and 0.08 and remain insignificant in all specifications. One might suspect this outcome to be an artefact of the different sample period, but repeating regressions of Table 9 using "log (drug offences)" instead of "log (drug deaths)" results in coefficients which are almost the same as for the longer time period in the corresponding Table 4, as can be seen from the coefficients for theft u.a.c.: 0.09 (Table 4: 0.07), theft w.a.c.: 0.10 (Table 4: 0.08), robbery: 0.11 (Table 4: 0.08), murder: n.s. (Table 4: n.s.), assault: 0.08 (Table 4: 0.08) and rape: 0.23 (Table 4: 0.16) (estimates not listed elsewhere). Similar results are obtained when we re–estimate Table 9 using both state and time effects and compare resulting estimates to those of the corresponding Table 5 (results not reported). Thus, we may conclude that the variable "drug offences" instead of "drug deaths" is more responsive to the drugs–crime channel in econometric models of crime based on disaggregate regional data.

6 Conclusions and discussion

Drug addiction is a topic of major public concern. This is partially due to the assumption that there might exist a causal relationship between drug abuse and increasing crime rates. In fact, a cursory inspection of the problem might lead to this conclusion. Many surveys indicate that drug users are more likely to have a connection with the criminal justice system (through arrests

and incarcerations) compared to non–drug users, and criminal justice system data indicates that a large percentage of arrestees test positive for illicit drug use at the time of their arrest (see French et al., 2000, for a survey of the related criminological literature). However, drug use may be, as French et al. (2000) put it, the "catalyst" for criminal activity, but the interrelationships between drug use and crime are more complex and require more than a two–dimensional view of the drugs–crime nexus. In this paper, this link is analysed within the Becker–Ehrlich model of crime supply, augmented by the consideration of currently discussed factors such as unemployment and demographic changes.

Estimation with panel data for West German states allows us to assess the importance of the drug abuse crime link, which comprises systemic, economic and pharmacological effects. According to our estimation results, which appear to be quite robust with regard to different specifications, a significant drugs–crime nexus appears to be relevant for several crime categories. This link has been ignored in most previous applications of the Becker–Ehrlich framework. Our analysis of the variable `MIGRANT` (i.e. the share of foreign citizens in the German resident population) demonstrates the potential importance of the resulting omitted variable bias. Nevertheless, illicit drug use is far from being the only or exclusive driving force behind the evolvement of crime rates. For example, the median of estimated deterrence elasticities of property crimes is -0.33, whereas drug–use elasticities of property crimes are relatively small (but significant) and range between 0.06 and 0.10 with a median of 0.08. Thus, the rational–choice hypothesis is confirmed for property crimes.

Significant effects found on property crimes such as robbery and theft can be attributed to a significant economic crime effect of drug abuse in Germany. The lack of significance of the drug measure for murder and assault might be taken as weak evidence against a pronounced systemic effect of the market of illicit drugs on these crime categories, although it has to be taken into account that the overall statistical performance of the economics of violent crime model is less convincing than the evidence in favour of the economics of property crimes. Pharmacological and systemic effects might be the driving force behind the relatively high and surprisingly robust estimate for the drug–use elasticity of rape.[20]

[20]Results of the effect of illicit drug use on rape are rarely found in the criminological

In spite of a different approach to the drug problem (different drug measurement, methods and observational units), our results are similar to those presented by Corman and Mocan (2000), underlining the robustness of the results found. We are sceptical, however, with respect to the far–reaching policy implications made by Corman and Mocan (2000). Based on the comparison of estimated elasticities of robberies, for instance, which are, 0.18 to 0.28 for drug use and -0.71 to -0.94 for robbery arrests, they conclude that "increased law enforcement is a more effective methods of crime prevention in comparison to efforts targeted at drug use". Without consideration of any cost–benefit analysis, such suggestions are difficult to justify. They would require cost estimates of a one percent change in drug offences and arrests or clear–up rates on the one hand, and estimation of benefits in terms of reduced costs of crime, more particularly of murder, assault, theft, robbery, vandalism etc. on the other. Corman and Mocan (2000) do not use such figures, although costs of crime are available for the U.S. (see, for instance, Anderson 1999; unfortunately, no comparable cost of crime estimates can be obtained for Germany). Furthermore, insights from the literature on the economic costs–of–illness need to be included in a comparative evaluation of the costs and benefits of combating illicit drug use. According to a survey by Cartwright (2000), in one of the latest cost–of–illness estimates for drug abuse in the U.S. (Harwood et al. 1998), the aggregate burden was 98 billion dollars in 1992, of which 59% was attributed to the related costs of criminal behaviours. Thus, there is only fragmentary evidence that needs to be integrated into a homogenous picture in future work.

Chronic drug users are also victims of crime. This is a neglected aspect when quantifying the benefits of crime reduction. French et al. (2000) find that, relative to non–drug users, chronic drug users are 16 percent more likely to become a victim of crime, 23 percent more likely to be a perpetrator of crime, and 25 percent more likely to be either a victim or a perpetrator. The latter aspect might also be relevant for the strong impact of drug use on rape indicated by our estimates.

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literature. Entorf and Spengler (2002, p. 159, p. 201) report a strong crime–enhancing effect of drug offences for rape and theft using 2946 panel observations from 440 counties ("Kreise"). Edberg (2004) has shown that violent crimes in general might be significantly affected by alcohol consumption, hence intoxication and addiction are confirmed as important factors. Corman and Mocan (2000) do not include the crime category rape in their analysis.

The most difficult task, however, remains the evaluation of the benefits of drug prevention programs or rehabilitation programs. There is a variety of programs in use, and not all of them seem to be ineffective. A substantial body of research in the US, mainly performed by the Department of Health, has found that treatment programs can produce marked reductions in illegal drug use and drug–related crime. For instance, the United States' 1996 National Treatment Improvement Evaluation Study found that clients reported a decrease of about 50% in the year following treatment and that arrests had declined from 48.2% to 17.2% (NACRO 1999). Of course, there are good reasons to be sceptical of such great successes, and econometricians might suspect some selectivity problems. However, it is for precisely this reason that much more evaluative work needs to be done before far–reaching conclusions like the one given by Corman and Mocan (2000) can be drawn.

Given the lack of unambiguous cost–benefit results, besides conventional measures which try to increase the clear–up rate, programs aiming at reducing economic–related and pharmacologically induced crimes as a consequence of drug abuse remain important tools of public policy. However, in order to obtain sustainable effects, such programs should not contribute to an increase in rents on the illegal drug market, but rather aim at reducing these rents. The development of actual proposals to this end remains very much on our research agenda.

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A Appendix: Results of Sensitivity Analysis

Table 7: Sensitivity analysis, omission of illicit drugs (dynamic model, state effects)

Independent Variables	Parameter	Theft u.a.c.	Theft w.a.c.	Robbery	Murder	Assault	Rape
Partial Adjustment	λ	0.41**	0.46**	0.34**	0.57**	0.27**	0.52**
		(0.06)	(0.05)	(0.05)	(0.07)	(0.05)	(0.08)
log (P)	β	-0.29**	-0.24	-1.12**	0.03	1.21*	0.19
		(0.09)	(0.14)	(0.22)	(0.54)	(0.56)	(0.24)
log (DRUGS)	γ_1	–	–	–	–	–	–
log (GDP_H)	γ_2	0.04	-0.23	0.20	-0.76*	0.18	-0.92**
		(0.20)	(0.14)	(0.26)	(0.34)	(0.21)	(0.26)
log (UR)	γ_3	0.18**	-0.04	0.07	0.10	0.00	-0.12**
		(0.03)	(0.03)	(0.04)	(0.06)	(0.03)	(0.04)
log (M15-24)	γ_4	0.82**	0.40**	0.72**	0.59**	0.33*	0.75**
		(0.13)	(0.10)	(0.17)	(0.21)	(0.14)	(0.17)
log (MIGRANT)	γ_5	0.38**	0.50**	1.04**	0.53**	0.54**	0.37**
		(0.08)	(0.07)	(0.13)	(0.18)	(0.10)	(0.15)
log (POL_H)	γ_6	0.54**	0.29*	0.20	0.45	0.49**	0.12
		(0.21)	(0.15)	(0.24)	(0.40)	(0.19)	(0.31)
BFN–DW–statistics		1.78	1.97	2.22	2.06	2.12	2.00
Adjusted R–squared		0.986	0.977	0.984	0.790	0.965	0.926

Notes: Standard errors in parantheses, **, * denote significance at the 1% and 5% level, respectively; fixed state effects included; sample: West German states without Berlin, 1976–1996 (200 observations)

Table 8: Sensitivity analysis of estimated drug elasticities

Explanatory factors	Theft u.a.c.			Theft w.a.c.			Robbery		
log(DRUGS)	0.47	0.18	0.08	0.23	0.13	0.08	0.69	0.42	0.09
	(0.05)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.04)	(0.03)	(0.04)
Two–way fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Correction for serial correlation of residuals	no	no	yes	no	no	yes	no	no	yes
BFN–DW–statistics	0.06	0.36	2.29	0.05	0.67	2.15	0.10	0.34	2.37
Adjusted R–squared	0.324	0.966	0.991	0.234	0.972	0.985	0.561	0.936	0.987
	Murder			Assault			Rape		
log(DRUGS)	0.16	-0.02	0.03	0.30	0.16	0.08	0.12	-0.11	0.19
	(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.06)
Two–way fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Correction for serial correlation of residuals	no	no	yes	no	no	yes	no	no	yes
BFN–DW–statistics	0.35	0.94	2.24	0.08	0.37	2.14	0.10	0.80	2.51
Adjusted R–squared	0.122	0.694	0.784	0.393	0.887	0.967	0.042	0.866	0.926

Notes: Standard errors in parantheses; sample: West German states without Berlin, 1976–1996 (200 observations)

Table 9: Sensitivity analysis, use of "drug deaths" (dynamic model, state effects)

Independent Variables	Parameter	Theft u.a.c.	Theft w.a.c.	Robbery	Murder	Assault	Rape
Partial Adjustment	λ	0.51**	0.63**	0.40**	0.63**	0.53**	0.52**
		(0.11)	(0.05)	(0.07)	(0.10)	(0.08)	(0.10)
log (P)	β	-0.31*	0.05	-0.28	0.09	1.40**	0.55
		(0.03)	(0.18)	(0.35)	(0.64)	(0.46)	(0.37)
log (DEATHS)	γ_1	0.03	0.02	0.02	0.08	0.00	0.01
		(0.02)	(0.01)	(0.02)	(0.05)	(0.02)	(0.03)
log (GDP_H)	γ_2	-0.97*	-0.11	-0.03	-1.29	0.11	-0.67
		(0.42)	(0.23)	(0.44)	(0.74)	(0.27)	(0.60)
log (UR)	γ_3	-0.05	-0.02	-0.13	0.10	-0.03	-0.07
		(0.09)	(0.06)	(0.10)	(0.17)	(0.06)	(0.14)
log (M15-24)	γ_4	0.76*	0.80**	0.50	0.20	0.10	1.26**
		(0.32)	(0.17)	(0.33)	(0.53)	(0.20)	(0.41)
log (MIGRANT)	γ_5	0.42*	0.64**	1.06**	0.11	0.39**	0.57*
		(0.20)	(0.11)	(0.20)	(0.34)	(0.13)	(0.27)
log (POL_H)	γ_6	0.79**	0.70**	0.63*	0.22	0.45*	1.24**
		(0.30)	(0.17)	(0.31)	(0.57)	(0.21)	(0.44)
BFN–DW–statistics		1.56	1.87	2.25	1.88	2.24	2.18
Adjusted R–squared		0.985	0.982	0.989	0.834	0.972	0.933

Notes: Standard errors in parantheses, **, * denote significance at the 1% and 5% level, respectively; fixed state effects included; sample: West German states without Berlin, 1983–1996 (130 observations)

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