The Effect of Workfare Policy on Crime.

Peter Fallesen, Rockwool Foundation and University of Copenhagen

Lars Pico Geerdsen, The Danish National Centre for Social Research

Susumu Imai, Queen's University

and Torben Tranaes, Rockwool Foundation

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Abstract

In this paper, we estimate the effect of Danish workfare policy on crime by exploiting two exogenous welfare policy changes.

First, we use a unique policy experiment that began in 1987 by an innovative mayor of the Danish city of Farum, where he imposed a 100 % work or training requirement for all welfare recipients immediately from the date of enrollment. By comparing the changes in crime rates among the unemployment uninsured workers, who are potential welfare recipients, in Farum before and after 1987 with that of the rest of Denmark, we identify the effect of workfare on the crime rate.

Second, we examine the effect of a series of national welfare reforms introduced during the 1990s. Those reforms strengthened the work requirement for the welfare recipients younger than 30 and were introduced gradually, starting with younger people first. We exploit the differential introduction of workfare across different age groups and the difference in municipality level enforcement as the exogenous variation.

Our results show a dramatic decline in the arrest rate among unemployment uninsured

after the introduction of the stronger workfare requirements, both in Farum and at the national level. But we find no policy effect on the unemployment insured, who do not receive welfare when unemployed. Those results imply a strong and significant crime reducing effect of the workfare policy.

1 Introduction

In many countries there has been a high level of interest on active labor market policies, i.e., mandatory work requirement on jobless individuals who receive unemployment insurance or welfare payment[1], as a way of helping them into employment. The employment effect, i.e., the "treatment effect" of active labor market policies (in shorthand, ALMPs) in Denmark and elsewhere is mixed at best (see Rosholm and Svarer (2008)). This is especially true for the welfare recipients. Both Bolvig et al. (2003) and Graversen (2004) find that most training programs have large lock in effect, which reduces the transition out of unemployment during the program period, but only have modest treatment effect after the program period. Bolvig et al. (2003) finds a negligible lock in effect and strong treatment effect for the private and public employment programs, whereas Graversen (2004) finds the treatment effect only for the private employment programs. But he also finds that private employment programs deal with workers that have characteristics that makes them more employable than the other welfare recipients.

One of the reasons for the ineffectiveness of ALMP in reducing the welfare dependency is due to the characteristics of the welfare recipients. Graversen (2004) argues that welfare recipients in Denmark have weaker attachments to labor market than the other workers, and are more likely to have other problems such as antisocial behavior, drinking and drug abuse. Indeed, two thirds of the welfare recipients are not included in the official unemployment statistics because they are not considered to be employable. This is why many argue that ALMPs for welfare recipients are not worth the cost, except perhaps, the private employment programs applied to the more employable welfare recipients.

However, ALMPs may not only be good for employment. Participation in the programs may also help individuals abstain from criminal activity. This is especially the case for the programs

[1]From now on, we will use the terms: mandatory work requirement, workfare, activation policy, active labor market policy, and active labor market programs to have the same meaning.

for welfare recipients because their crime rates are much higher than the rest of the population. In fact, the social benefit from crime reduction can be stronger than the benefit due to the reduction in welfare dependency. This is because crimes impose strong negative externality to the community, and the conventional methods for reducing crimes, such as incarceration may be much more costly than the workfare policies. The cost of incarceration not only includes the direct cost of prisons and other facilities and the criminal justice system, but also dynamic costs, which are the stigma of an arrest record and criminal human capital accumulation in prison. Bayer et al. (2009) forcefully argue that prison environment greatly facilitates criminal human capital accumulation through learning from the peers.

The issue is relevant not just for European countries where ALMPs cover many unemployed workers and workers on welfare, but also for countries like the U.S. that have experienced high crime rates. According to Freeman (1996), the percentage of men incarcerated in the U.S. is roughly the same as the percentage of men in long term unemployment in Europe. Much research has been done on what government and local communities can do to reduce crime rate in the U.S. Donohue and Siegelman (1998) survey evaluation studies of U.S. social programs on whether they reduce crimes [2]. They discuss the Job Corps program in length because that is the program they argue has the most promise in terms of reduction in crime. Job Corps is a residential program where economically disadvantaged youths aged 16-21 voluntarily participate in educational and training programs for 7 months. In order to stay in the program, one must not be arrested for felonies, pass drug tests, avoid fighting, robbery, or sexual assault. One must also abide by other rules, such as rulse on dress and appearance, as well as dormitory inspection rules. The participants are randomly assigned into treatment group and control group. The program is estimated to reduce overall crime by 12 %. Notice that in most of those programs,

[2] Lochner (2010) also extensively surveys the studies on the crime reduction effects of education and job training programs.

such as Job Corps, participants not only self select in to the program but often are also carefully screened. In contrast, the Danish ALMPs apply to anybody whose stay on welfare has exceeded the passive period. They also have been around for more than 20 years, in large scale in many of the European countries, and their employment effects have been extensively studied. In this pape, we highlight the role of ALMPs as an effective policy tool against crime, which so far has been mostly overlooked. Our results would not only help communities that already have adopted the ALMPs in taking their crime reduction effect into account, but also suggest to other communities a ready to implement and well understood program as a promising option for reducing crimes.

Participation in an ALMP may influence individuals' risk of committing crime in various ways. First, there could be an indirect effect of activation, through an increase in employment of the welfare recipients. It is well known in the crime literature that employment reduces crime. Welfare recipients enrolled in the activation program or under the threat of imminent enrollment would be more likely to find work. There may exist a direct effect: work, training or education may simply leave less time for crime. Jacob and Lefgren (2003) measure the short run effects of a schooling day on crime. According to their results, when students are given days off from school exogenously, they commit more property crimes and less violent crimes. What they measure is the intensive margin of the effect of schooling on crime of students who attend classes regularly. Those who would be the most criminally at risk may rarely come to school, thus may only be weakly affected by the policy. The third effect would be a direct effect of workfare that is not due to the extensive margin, i.e. due to the workfare programs, people may have changed their lifestyle from a criminal to a noncriminal one.

In this paper, we separately identify those three effects. Since we have data on whether they are working on regular private sector jobs, or workfare training and government provided jobs, we can separately identify the indirect effect through changes in private sector employment and the direct effect of activation. To separately identify the two direct effects, i.e. the intensive margin effect and the extensive margin, we use an unconventional source of information. Since from 1991 we have information on the exact date of crimes committed, we separate the crimes between those committed on the weekdays, when unemployed individuals on welfare are engated in training, job search or publicly provided jobs, and the crimes committed on weekends, when those programs are closed. Our results show that the crime reduction effects are not only due to the indirect effects of an increase in employment, but also due to direct effect of activation, and since we see large and signficant reduction in weekend crimes, those direct effects come from changes in the lifesyle. Furthermore, we show that the major part of the policy effect comes from those who depend on welfare the most, i.e. who are on welfare more than 75 % of the time. Those are the individuals who are on average the most criminally active.

The existence of a strong positive relationship between unemployment and crime has been hypothesized for almost a hundred years in the social sciences literature (see Cantor and Land (1985) for details). Reviews of the literature can be found in Wilson (1983), Long and Witte (1981), and Chiricos (1987). According to Chiricos (1987) and Levitt (2001) there is a predominance of estimates with a positive correlation between unemployment and property crime. For unemployment and violent crimes, however, the connection does not seem to be equally clear.

Estimating the causal effect of unemployment to crime remains a challenge. This is because many unobserved characteristics or events that make individuals more likely to become unemployed also make them more likely to commit crime. Therefore, researchers try to find exogenous variations that affect unemployment but not crime directly. Raphael and Winter-Ebmer (2001) use closing of military base and Gould et. al. (2002) and Fougere et. al. (2009) use changes in industry structure is used as the exogenous variation. Notice that changes such as plant closing or changes in industry structure could affect local communities directly, which could change the community level crime policies, and thus the crime rate. Nilsson and Agell (2003) estimate the effect of unemployment and labor market program participation on crime using Swedish municipality level data, where they use lagged unemployment and lagged labour market program participation as instruments.

In this paper, we explicitly address the endogeneity issue of program participation. We do this by exploiting two types of policy changes. First, we analyze the effect of a radical workfare policy in a municipality. In 1987, Farum, a Danish municipality, introduced an immediate ALMP participation requirement for all individuals who received social benefits. In the rest of Denmark ALMP participation would normally not occur until individuals had received benefits continuously for at least 3 months. We use the introduction of imminent activation in Farum as treatment and examine its causal effect on crime in Farum compared to the rest of Denmark.

Second, we examine the effect of a series of national reforms on activation policy for young people introduced during the 1990s, and the municipality level variation of its enforcement. Those reforms strengthened the work requirement for the welfare recipients and were introduced gradually, starting with younger welfare participants first. Hence, we exploit the differential introduction of workfare reform across different ages as well as the municipality level differences in actual enforcement of those rules. As reported in Graverson (2004), the actual implementation of the reforms were left to the local authorities, and there is a lot of municipality level variation in the actual length of welfare spell until the start of activation.

The difference between our identification strategy and the ones by studies such as Raphael and Winter-Ebmer (2001), Gould et. al. (2002) and Fougere et. al. (2009) is that the ALMPs we use are only for welfare recipients, and should not affect the crime rates of unemployment insured, which is what we find. Hence, we can rule out the possibilities of policies or changes in socioeconomic conditions in municipalities that occur in parallel to the welfare reforms that may impact the arrest rates, because those should affect the crime rate of unemployment insured as well.

We use Danish register data on individuals supplied by Statistics Denmark. We have access to information on labor market status and demographics of the entire Danish population from 1981 to 2005. Furthermore, from the central crime register of the Danish Police, we obtained detailed records on arrest, verdict and sentencing outcomes as well.

In both estimation exercises using the reforms in Farum and at the national level, ALMPs have a statistically and economically significant negative effect on crime, which leads us to conclude that the crime reduction effect of ALMPs is robust to the environment that is implemented. It comes from the reduction of crimes by unemployment uninsured, who were the target of the ALMP reforms. We also find that an important source of the policy effect comes from the direct effect of activation on those who stay on welfare, and the effects are due to changes in their lifestyle, and not only due to the reduction in leisure hours. That means, ALMPs are beneficial to the society even if they do not lead to any transition to regular employment. And ALMPs not only reduced crimes by welfare recipients by restrict criminal opportunities of individuals but also by transforming them into better citizens.

Even though the two policy variations can be considered exogenous, there still remain some sources of bias. First, if we estimate the policy effect only on individuals who are unemployment uninsured, i.e. potential welfare recipients, we would miss the policy effect on individuals who may avoid stricter work requirement by getting unemployment insurance. This would be a source of downward bias because those who obtain unemployment insurance are working in higher paid and more stable jobs, thus are likely to be less criminally active[3]. In the specification where we also estimate the policy effect on the sample of individuals that includes both unemployment insured and uninsured young men, the estimated overall policy effect would not be subject to the above bias. However, in Farum there is another selection effect: individuals can avoid

[3]Since we estimate the model on unemployment uninsured or insured, and do not estimate it on the welfare participants, our results are not subject to the bias due to the endogeneity of welfare participation.

tough work requirement by leaving Farum. This is a more serious issue because we cannot a priori determine the likely direction of bias. Therefore, in one specification we also estimate the location choice of Farum versus other municipalities jointly with the crime equation. But that requires some additional exclusion restrictions. The estimation exercise using national data using national level reforms does not suffer from such bias. However, the results where we also include municipality level variation in enforcement as policy, the results are subject to the above bias as well. Second, since the national reforms were introduced for younger welfare recipients first, in later reforms for older individuals, the control group may have been treated already. Indeed, most cohorts were either not treated at all, or treated from the age 18. Thus, when we use the fixed effects estimation strategy, the treatment effect of later reforms would only measure the marginal effect on top of the earlier treatment, and thus have a downward bias. Furthermore, in its implementation, the municipalities were given a wide discretion, and thus those reforms were only gradually enforced. Therefore, we only see the strong and significant policy effects from the national reforms after we incorporated the information on the local level enforcement in the estimation.

In Section 2, we explain the institutional details of the welfare and workfare policies in Denmark, then the national level workfare reforms during the 1990's and the unique welfare policy experiment in the Danish municipality Farum. In Section 3, we discuss the details of the panel data we assembled from the Danish register. In Section 4, we present the empirical model and the estimation strategy. In Section 5 we report the estimation results, and in Section 6, we conclude.

2 Unemployment Benefits, Social Assistance and Labor Market Programs in Denmark and Farum

2.1 Unemployment Benefits, Social Assistance and Labor Market Programs in Denmark

In Denmark unemployed individuals fall into two categories: members of an unemployment insurance fund who are entitled to unemployment benefits and those who are not. The latter individuals are entitled to social assistance (welfare).

In the beginning of the 90's, to be able to become a member of the unemployment insurance fund (UI fund), one had to either work for an employer, be self-employed, or participate in a training course or higher education for at least 18 months. Furthermore, the UI fund member is eligible to receive unemployment insurance payments only if he/she has been a member for more than one year and has worked full time for 26 weeks (6 months) during the last 3 years. However, individuals who had just finished education or apprenticeship could become a member of UI fund and eligible for benefits after only one month of membership and without the past employment requirement. Once eligible, the unemployed UI fund member can receive UI benefit for two and a half years of "passive period", as long as they can claim they are searching for a job. After that, individuals have to participate in activation programs, which provide training or government supported employment for half a year, which is called "active period". Before 1994, it was possible to continue receiving UI benefits after the expiry of passive periods of unemployment insurance as long as the individual participated in activation program, up to 9 years.

After 1994, individuals who were unemployed for four years had to participate in the activation programs up to three years, and after that, he/she would not be eligible for another rounds of UI benefit. The first passive period of unemployment has been gradually shortened as well.

In 1996, it was reduced to two years, (I did not understand this: except for workers who already had UI benefits or had lost the benefit and had not regained it.) For them, the passive period was reduced to three years, and from 1998, it was further reduced to two years.

The unemployed individuals who cannot receive unemployment insurance benefits or unemployable individuals receive social assistance (welfare) from the government. Individuals with a personal fortune or an employed spouse may not be entitled to any assistance or subject to some reduction.

The recipients of social assistance are younger, less educated, have less work experience and have longer unemployment periods. It is also the case that they tend to be less integrated to the society, tend to suffer alcohol or drug abuse, and are more likely to be subject to physical and mental health problems. Furthermore, a relatively large fraction of the welfare benefit recipients are immigrants and refugees. More than two thirds of the welfare benefit recipients are not included in the official unemployment statistics since they are not considered to be immediately available for work (Graversen 2004).

Both unemployment insurance benefits and social assistance are administered at the local municipalities. For the unemployment insurance benefits, the local municipalities have to strictly follow the national policy. On activation policy, municipalities are allowed to deviate substantially from the national policy.

There has been a number of changes to the social assistance system, both during the late 1980s and the 1990s. In particlar, more and more emphasis was put on workfare (activation), especially for the young.

In July 1990 the so called youth-benefit law ("Ungdomsydelse") was introduced for the youth below 20, and in October 1991 it was expanded to the 20 year olds. According to the law, in order to receive the welfare benefit, the young unemployed has to register within 2 weeks of unemployment, and then, from day one of registration, be activated. That is, he/she would either be given a government subsidized private employment or public relief work, or participation in a training program. The workfare offer was for a spell of 5 month, which was extended to 8 months in 1992.

In 1994, the law was amended so that for individuals who are below age 25 and on social assistance, the mandatory activation was to start after 13 weeks of unemployment, and for those older than 25 it started after 12 months of unemployment. From 1995, the activation requirements for welfare recipients have been gradually strengthened. That is, in 1995 the mandatory weekly hours of activation have increased from 20 hours to 30 hours. In 1996 the period of mandatory activation has been extended from 6 months to 18 months, and in January, 1998 all individuals below the age 30 had to be activated after 13 weeks of unemployment.

However, the actual implementation of the ALMPs for the unemployment uninsured were left to the local municipalities, and many of them could delay the activation or reduce them due to lack of resources. On the other hand, some municipalities, such as Farum, implemented more ambitious activation schemes that started earlier and lasted longer than the national guidelines.

2.2 The case of Farum

From 1987, shortly after the appointment of Lars Bjerregård as an employment consultant in 1986, the municipality of Farum made a series of radical changes to its activation policy for recipients of social assistance. [4]

The practice in Denmark until then had been to send individuals on social assistance into activation only after a very long period of unemployment, and only if the municipality believed that these individuals were not capable of finding work by themselves. The activation programs in Farum until the end of 1986 was of similar nature and with a focus on employment/activation in service jobs inside the municipality, shoveling of snow for the elderly, cleaning of local nature

[4] He would later in 1991 be appointed head of employment administration.

areas etc. (Birkbak, 1997:13)

After the appointment of Bjerregård, two drastic changes were made. First, the unemployed welfare recipients were activated the very first day they applied for social assistance at the municipality and had to report to the firm where they would work or to local activation facility "Produktionshuset" (the Production House) every workday from then on – at 7.00 am. Second, the activated individuals mainly went to work in private firms at reduced wages instead of working for the municipality itself. If it was not possible to find a suitable position in a private firm because a position was not available or the individual did not have the necessary skills, linguistically or otherwise, from May 1987 he/she was assigned to work at the Production House.

These policy changes were introduced over the period of late 1986 through out 1987. From 1988 individuals with physical or mental disabilities who received social assistance were also subject to lighter forms of activation. Alcoholics, drug addicts e.g. were subjected to mandatory treatment. It is also important to note that Farum made no distinctions based on age-groups, gender, education or any other demographic characteristics. (Birkbak, 1997)

In the late 1990s and early 2000s Farum relaxed their activation policies in response to severe criticisms that resulted in a series of lawsuits from Danish labour unions and complaints from the ministry of employment alleging that activation policies violate the laws protecting the workers and give unfair competitive advantages to some firms. In particular, there were allegations put forward by labor unions in 2000-2001 that some firms were given favorable access to activated workers as cheap labor and contracts with the Production House. In 2000, 2001, Danish parliament started to discuss the matter. In 2002, after a series of newspaper reports on the allegations, Danish minister asked mayor Brixtofte to adjust the workfare programs to address those concerns. Furthermore, Anti-Trust Board of Denmark formally launched an investigation into anticompetitive nature of the program. Because of all those events, we consider 2002 to be the year when Farum's activation experiment started to unravel. Several years later, the activation operation at the Production House was found to be illegal and the Production House was closed in 2006. It is interesting to see that even though activation experiment in Farum has been under heavy criticism, Danish national government effectively followed it so that by 1998, welfare recipients below the age of 30 were under a mandated activation scheme very similar to that of Farum. Later reforms in 2002 further increased the similarities. The difference is that the implementation of the national policy was weaker than that of Farum.

3 Data

3.1 Danish Register Data

In Denmark, every person is from birth or immigration given a unique personal code called a CPR-number (Central Personal Register). This code is used every time a person is in contact with a public authority. Information on the person obtained by the government is saved at Statistics Denmark. All individuals are followed until they either die or emigrate. The result is an extremely detailed panel data sets with, sometimes weekly observations of the entire Danish population for more than 20 years. The dataset made available to us is from the year 1981 to 2005 and covers the entire Danish population in that time span. It has detailed information on demographic, educational, income and labor market variables. From the police departments we also have information on each individual's criminal record. All the information is registered with very high reliability and no attrition.

Our focus will be men between ages 18 and 30, who have by far the highest crime rate compared to any other demographic groups. Approximately 25 % of all Danish males is arrested before the age of 30, but very few first-time offenders are rearrested after the age of 30. At the same time, this specific age group has been the target of numerous labor market programs since the late 1980's.

3.2 Crime measures

Our measure of criminal activity is the arrests, which led to a verdict in court. From now we will call them simply arrests. We also call the average number of arrests for a group simply the arrest rate for the group.

We obtain information on criminal activity from the Central Crime Register. It provides data on all arrests recorded by the Danish police. The data consists of all cases filed against individuals in the entire sample, both primary as well as secondary ones. The information includes whether the case went to court and the subsequent verdict, including whether the charges were withdrawn or not, and whether the case was dismissed in court or not. It also has information on incarcerations: type and place of prison and the actual time spent in jail.

The register covers the period 1981 to 2005. Information in the register can be merged with all the other information that we have access to through the perpetrators CPR-number. In this paper we focus on convictions. We divide various crimes into property crimes, violent crimes and other crimes. Other crimes include drug related crimes as well criminal activity which cannot be classified in any of the above mentioned categories. In the following we focus on total crimes, property crimes and violent crimes. [5]

3.3 Descriptive statistics for Farum and the rest of Denmark

In Table 1 we show some sample statistics of the variables used in our analysis. They are shown for men between the ages 18 to 30 for the municipality of Farum, and 5 % random sample of the rest of Denmark. We find that Farum has higher rate of unemployment uninsured than the rest of Denmark. We also find that the young men in Farum have somewhat higher arrest rate than

[5] Danish Criminal Register does not have information on date of crime committed before 1990 and crime dates recorded in 1990 may not be very accurate. To deal with this issue, we impute the crime date for the crimes committed before 1990 or before 1991, based on the difference in crime date and arrest rate observed after 1991.

the rest of Denmark, and slightly lower level of education. The more pronounced differences are in marriage rate. Young men in Farum are 30 % more likely to be married, and are more likely to have children. Furthermore, as we can see from the ratio of Danish population and that of the Danish or immigrants from western countries [6], higher proportion of the young men in Farum are immigrants from nonwestern countries. Also, young men in Farum are 50 % more likely than those of the rest of Denmark to live with parents, and slightly more likely to live in the same municipality as their parents.

Next, we compare the sample statistics of unemployment uninsured and insured. We first note that the arrest rate of unemployment uninsured is more than twice as much as that of the unemployment insured young men, both in Farum and in the rest of Denmark. Also the arrest rate of insured individuals are higher in Farum than for the rest of Denmark, but for unemployment uninsured, the arrest rate is lower in Farum. This difference could be mainly due to the fact that the sample period includes those where the unemployment uninsured in Farum was under a very strict activation policy, which we argue reduced criminal activities. Furthermore, the unemployment insured individuals are on average older, higher educated than the unemployment uninsured. In Farum, the unemployment insured are twice as likely to be married than the unemployment uninsured, and in the rest of Denmark, they are 50 % more likely to be married than the uninsured. In Farum, the unemployment insured are also about two and a half times as likely to have children as the uninsured and in the rest of Denmark, the insured are twice as likely to have children as the uninsured. It is interesting to note that in the rest of Denmark, relative size of Danish and developed (change to western) country immigrants are higher for the unemployment insured than the uninsured, but in Farum it is the opposite.

[6] We decided to use immigrants from western countries instead of developed countries because during the period of 1981 to 2003, many countries grew out of the developing country status. In any case, the difference between the two classifications are very minor.

Finally, in Farum the unemployment uninsured are twice as likely to stay with parents than the insured. In the rest of Denmark the unemployment uninsured are more likely to stay with parents, but the difference is not as large as in Farum. Those differences could also explain the difference in arrest rates between Farum and the rest of Denmark for unemployed uninsured and insured.

Figure 1 plots the Danish national unemployment rate of men at different ages. (for unemployment uninsured?) We can see that in Denmark young men from 18 to 29 have experienced a surge in unemployment rate from 1986 to 2001. The unemployment rate of 18, 19 years old men peak around January 1990, and then gradually decline thereafter. The peak unemployment rate of men between ages 20 to 24 is around January 1992, and that of men between ages 25 to 29 is around January 1994. Notice that the peak year of unemployment for 18 to 19 years old men coincides with the year when mandated activation policy was introduced for them. Furthermore, the peak unemployment year for 20-24 year olds coincides with the year when the mandate was applied to them as well, and the same for 1994 for the 25-29 year olds when they started to face mandated activation. That is, for each age group, the period when unemployment rate starts to go down roughly coincides with the period when the mandated activation was introduced for them. We can see from this that mandated activation policy looks to be very effective in reducing the unemployment rate.

Figure 2 plots the ratio of men in schooling or regular jobs for different age groups. We can see that regardless of the age group, they all hit the bottom at year 1995. Furthermore, even though we see a decrease in trend level of unemployment from 1987 to 2002: a 2 % decrease for the 18-19 age group, 3 % decrease for 20-24 age group, and 2 % decrease for 25-29 age group, we do not necessarily see a corresponding increase in the employment and schooling ratio. That is, from 1987 to 2002, it only increased by 1% for 18-19 age group, and for 25-30 (change to 25-29) age group it actually decreased. These two figures illustrate the recent literature that evaluates the active labor market policies in Denmark, such as Bolvig, et al. (2003) and Graversen (2004), where they argue that the effect of activation policy in moving the welfare recipients off the welfare dependency is small at best. Thus, most of the reduction in unemployment rate due to the activation policy seems to come from jobs that are part of the training program, such as the ones that hires trainees temporarily with government subsidy in wages and public sector employment.

On the other hand, if we look at Figure 3, where we plot the monthly property crime arrest rates of different age groups of Danish and western immigrant men who were not unemployment insured, we clearly see the effect of the three main activation reforms implemented over the 1990s.[7] We can see that from 1991 until 1993, the arrest rate of 18-19 years old men have decreased more than the ones of other age groups. Then, from 1994 to 1998, the arrest rates of 20-24 age group went down, whereas those of the other age groups increased. Notice that the timing of the relative decrease of the 20-24 age group, which is 1994, does not coincide with the peak year of unemployment, 1992. This is because the 1992 reform for the 21-24 age group was not implemented very strictly. The subsequent decrease in unemployment is known to be rather cosmetic, mostly due to generous application of leave schemes and early retirement. In contrast, the 1994 reform had more teeth, and resulted in real reduction in unemployment. Thereafter, from 1998, during the years when the 25-29 age group were subjected to the reforms in activation, their arrest rates dramatically decreased relative to those of other age groups.

In Figure 4, we plot the monthly violent crime arrest rates for unemployment uninsured men who are either Danish citizens or Western immigrants. There, we see a rapid increase of

[7] Before 1990 the crime dates were not recorded. Instead, only verdict dates are available, and verdict dates are heavily clustered around the early months of the year. In later analysis, we used the relationship between crime date and verdict date for different crime categories using data after 1990 to impute the crime dates before 1990.

violent crimes for all age groups from 1991 until 1995. It is interesting to notice that the year the violent crime arrest rates start to increase coincides with the year when about half of the hospitals for mental illness were closed. From around 2000, the arrest rates dramatically increase again, except for that of the 25-29 year olds. We believe that this is likely due to the general trend of increase in the rate of reporting of violent crimes. Thus, it is reasonable to conclude that most of the time series variation in violent crime arrest rates are not due to the changes in activation policy.

In Figures 5 and 6, we plot both the property and violent crime arrest rates for unemployment insured individuals. From both figures, we can see that the arrest rates for different age groups are not related to the reform dates for the unemployment uninsured of the corresponding age groups. The decrease in arrest rate from 1992 does not seem to occur only for the 18-19 age group, and the decreasing trend seems to be common for all age groups, until the year 2000, when the arrest rate of 20-24 age group starts to increase. Furthermore, we also do not see a radical decrease in the arrest rates after 1994, 1996 or 1998, which are the years when the activation policy for unemployment insured has changed. This is why in this paper, we mainly focus on the arrest rates of the unemployment uninsured.

Next, we show the time series plots of various statistics for Farum and the rest of Denmark before, during and after the Farum policy period. In Figure 7, we plot the average jobless rates of uninsured men in Farum and the rest of Denmark. Notice that until 1986, both of them are very close. However, after 1987, the jobless rate of Farum continues to decline, whereas that of the rest of Denmark sharply increases over time until 1995. From 1995, both rates decline over time. This is mainly due to the effect of the implementation of the active labor market policies nationwide. After around 2000, the gap of jobless rates between them is much smaller than before, which we believe is mainly due to the convergence of the activation policies of Farum and the rest of Denmark. In 2002, the national activation policy became almost the same as the one in Farum, except for the stricter implementation in Farum. Notice also that the unemployment rate of unemployment uninsured in Farum was still exceptionally low even after 2002, the years when the Farum activation policy started to unravel. This is because the unemployment statistics does not include those on welfare who are deemed not employable. During the periods of very low aggregate unemployment rate for unemployment uninsured, the unemployment rate could vary primarily due of the differences in who among the welfare recipients the municipality would classify as employable. The national activation reforms targeting the unemployable only started much later, around 2004.

In Figure 8 we plot the unemployment rate of unemployment insured men in both Farum and the rest of Denmark. We can see that they resemble each other very closely. Hence, it is unlikely that Farum had a large labor market shock that affected the unemployment insured workers differently than those of the rest of Denmark, especially that the unemployment insurance policy is administered at the national level uniformly to all municipalities. One possibility that labor market shock could affect differently for unemployment insured and uninsured would be that during the policy period the composition of the unemployment uninsured in Farum has dramatically diverged from the rest of Denmark, thus their labor supply behavior may have changed, or labor demand shock could have affected the unemployment uninsured in Farum differently from the rest of Denmark. For example, the ratio of nonwestern immigrants has become higher in Farum than the rest of Denmark during the policy period, and because they are more likely to be unemployment uninsured, that could partially explain the divergence in jobless rate in Farum. To take that into account, we carefully control for the observed characteristics, and also use fixed effects estimation when we econometrically evaluate the policy effect.

In Figure 9, we plot the arrest rates for unemployment uninsured men. As we can see, the arrest rates for uninsured young men do not differ between Farum and the rest of Denmark until 1987. Thereafter, we see the arrest rates of the rest of Denmark starting to increase, whereas the ones for Farum staying constant. The gap between Farum and the rest of Denmark lasts until around 1998, when the arrest rate of the rest of Denmark starts to drop to the level of Farum. We can see the same pattern in Figure 10, where we plot the arrest rates of Farum and the rest of Denmark for property crimes. If we look at Figure 11, where we plot the arrest rates of violent crimes, although the time series patterns are similar for Farum and the rest of Denmark, we still perceive a slight tendency for the arrest rate of Farum to decrease relative to the rest of Denmark from 1989 to 1997, indicating some policy effect.

In Figures 12, we plot the arrest rate of unemployment insured men in Farum and the rest of Denmark, and in Figures 13 and 14, we plot the arrest rates for the same people for property and violent crimes, respectively. Here, we do not see any large discrepancies between the arrest rates of Farum and the rest of Denmark until 1998, and the increase in the arrest rate in Farum is only due to the dramatic increase in the violent crime arrest rates in Farum after 1998, where property crime arrest rates are close to those of the rest of Denmark.

The divergence of jobless rate and the arrest rate between Farum and the rest of Denmark is only occurring for the uninsured. It is important to remember that in Farum the aggressive activation policy was only instituted for the uninsured. The policies against insured were very similar to the ones of the rest of Denmark. This leads us to suspect that the relative decline in verdict rates of the uninsured men in Farum from around 1987 is primarily caused by the decline in jobless rates induced by the aggressive activation policies.

The other reasons could be due to the differences in observed characteristics of the unemployment uninsured in Farum and the rest of Denmark. In Figures 15 and 16, we plot what we believe would be the most likely candidates among the observed characteristics for the source of the reduction of the relative arrest rates in Farum during the policy period, i.e. the ratio of nonwestern immigrants who are unemployment uninsured, and the average years of schooling of the unemployment uninsured, for both Farum and the rest of Denmark. As we can see, during the policy period in Farum, the ratio of nonwestern immigrants have declined somewhat relative to that of the rest of Denmark. On the other hand, the average years of schooling has been higher relative to the rest of Demark. Hence, in order to assess the policy effect, those variables need to be controlled for in the econometric analysis we conduct later.

4 Empirical model

We use the following linear Difference-in-Differences model to evaluate the policy experiment in Farum.

$$C_{it} = X_{it}\beta + \sum_{a=19}^{30} I_a(a_{it})\gamma_a + \sum_{y=1982}^{2005} I_y(t)\gamma_y + \sum_{m=2}^{12} I_m(t)\gamma_m + \sum_{k=2}^{274} I_k(k_{it})\gamma_k + I_F(k_{it}) \times I_P(t)\delta + \varepsilon_{it} \quad (1)$$

where I_a is the age dummy for age a, I_y and I_m the year and month dummies, respectively and I_k is a municipality dummy, which equals 1 if indivdiual i lives in municipality k at period t, i.e. $k_{it} = k$ and 0 otherwise. I_F is the Farum dummy, which equals 1 if individual i lives in month t in Farum municipality, i.e. $k_{it} = F$ and 0 otherwise, I_P is the policy dummy which equals to 1 if the time period t belongs to the policy period in Farum, and 0 otherwise. The policy periods are either from 1987 to 2001 or from 1987 to 1997. The policy effect is identified by the parameter δ. We estimate the equation 1 for the unemployment uninsured and insured separately. The OLS estimator of δ then will be unbiased if ε_{it} is orthogonal to $I_F \times I_P$.

Bertrand et. al. (200?) have argued that in the Difference in Differences estimation if the serial correlation was not properly taken into account in determining the standard error, one tends to overreject the hypothesis of no policy effect. Furthermore, they show that in short panels, unless one has many local governments with differential timing of policy variation, the problem persists even if the serial correlation is taken into account by using the robust procedures in deriving the standard errors, because of the short time series dimension. Conley and Taber

(2005) propose a method to conduct inference even in the above situation. In our data, we only have one single municipality that adopted an activation policy that was radically different from the other municipalities. But we still are able to consistently estimate the standard errors using robust procedures due the large time series sample size of $(2005-1981)\times 12 = 288$, which is divided between 179 or 131 policy policy periods and the remaining non-policy periods.

There could be two sources of bias. First, the policy effect for the unemployment uninsured could be entirely due to the fact that those who are unemployment uninsured change their status from uninsured to insured during the policy period to avoid the possibility of activation. Indeed, later we present evidence that shows that during the policy period more people in Farum were unemployment insured relative to the rest of Denmark. However, this endogeneity bias would reduce the estimated policy effect for the unemployment uninsured workers in Farum since the individuals who would switch from unemployment uninsured to insured status during the policy period would be the uninsured workers who have better labor market prospects and thus are less criminally inclined. Thus, the OLS policy effect would be a conservative estimate. Second, it could be that individuals who are more criminally inclined left Farum during the policy period, which could have been the reason of the reduction in arrests in Farum during the policy period. This is a more serious issue because a priori we cannot be sure of the direction of the bias. To deal with these issues, we first estimate the Difference-in-Differences model with fixed effects. That is, we add fixed effects to the above equation as follows.

$$C_{it} = X_{it}\beta + \sum_{a=19}^{30} I_a(a_{it})\gamma_a + \sum_{y=1982}^{2005} I_y(t)\gamma_y + \sum_{m=2}^{12} I_m(t)\gamma_m + \sum_{k=2}^{274} I_k(k_{it})\gamma_k + I_F(k_{it})\times I_P(t)\delta + \alpha_i + \varepsilon_{it} \tag{2}$$

The Fixed Effects estimator then will be unbiased if

$$E\left[\varepsilon_{it}|I_{Pt}=1, I_{F,it}=1, X\right] - E\left[\varepsilon_{it}|I_{Pt}=0, I_{F,it}=1, X\right]$$
$$-\left\{E\left[\varepsilon_{kt}|I_{Pt}=1, I_{F,kt}=0, X\right] - E\left[\varepsilon_{kt}|I_{Pt}=0, I_{F,kt}=0, X\right]\right\} = 0$$

and biased downwards if LHS is negative, which could occur if people who left Farum during the policy period commit more crimes even after controlling for Farum and time dummies.

We use Heckman Sample Selection procedure to formally deal with the selection issue. That is, for the treatment sample, we run the following first stage probit.

$$\Pr\left(I_{Fit}=1|Z_{it}\right)=\Phi\left(\theta Z_{it}\right) \tag{3}$$

where Z_{it} includes constant term, X_{it}, age and time dummies and a dummy indicating whether both parents live in Farum or not. Then, we estimate the following second stage regression model.

$$\begin{aligned} C_{it} &= X_{it}\beta+\sum_{a=19}^{30} I_a(a_{it})\gamma_a+\sum_{y=1982}^{2005} I_y(t)\gamma_y+\sum_{m=2}^{12} I_m(t)\gamma_m+\sum_{k=2}^{274} I_k(k_{it})\gamma_k \\ &\quad +\lambda_1\left(Z_{it}\right)I_F(k_{it})\gamma_{F1}+\lambda_2\left(Z_{it}\right)\left(1-I_F(k_{it})\right)\gamma_{F2}+I_F(k_{it})\times I_P(t)\delta+\alpha_i+\varepsilon_{it} \end{aligned} \tag{4}$$

where

$$\lambda_1\left(Z_{it}\right)=\frac{\phi\left(\theta Z_{it}\right)}{\Phi\left(\theta Z_{it}\right)},\ \lambda_2\left(Z_{it}\right)=\frac{\phi\left(\theta Z_{it}\right)}{1-\Phi\left(\theta Z_{it}\right)}$$

are the inverse Mill's ratios used to correct for the endogeneity bias due to selection. The exclusion restriction is that whether both parents live in Farum or not affects the decision of individuals to live or not to live in Farum but not his decision of committing a crime, i.e., is not included in X_{it}. This could be violated if children with a positive utility shock of committing a crime leave Farum to avoid activation and parents follow. Another possibility would be when parents themselves are on social assistance and leave Farum to avoid activation. To minimize bias due to those possibilities, we only choose children who have at least one parent who is unemployment insured. We believe that those parents have been working in a regular job most of the time, which makes it more likely that their location choices primarily depend on the job requirement and not based on the location of their children. Another possibility that parental location could affect criminal behavior of children is that children may reduce crime when they are living with parents. To control for this, we include in the RHS of the second stage regression equation dummies indicating whether children are living with parents, and whether they are living in the same municipality as parents.

We then ask whether the policy effect on crime is due to the indirecty effect through reduction in welfare dependency, or due to the direct effect of the reduction of crime of the individuals staying on welfare. That is, we run the following regression.

$$C_{it} = X_{it}\beta + \sum_{a=19}^{30} I_a(a_{it})\gamma_a + \sum_{y=1982}^{2005} I_y(t)\gamma_y + \sum_{m=2}^{12} I_m(t)\gamma_m + \sum_{k=2}^{274} I_k\gamma_k + I_{F,it} \times I_{Pt}\delta + \gamma_W W_{it} + \alpha_i + \varepsilon_{it} \tag{5}$$

where W_{it} is the fraction of welfare payment of period t relative to the full monthly pay. Next, we discuss the empirical specification we use to estimate the policy effect at the national level. In Denmark strict activation policy was introduced gradually during the 90's, with different starting dates for different age groups. In principle, this would enable us to estimate the policy effect separately from the time trend.

We use the following linear Difference-in-Differences model.

$$\begin{aligned} C_{it} &= X_{it}\beta + \sum_{a=19}^{35} I_{a,it}\gamma_a + \sum_{t=1991}^{2005} I_t\gamma_t \\ &\quad + \left[I_{a\in\{21,24\},it} \times I_{t\geq 1994:7}\right]\delta_1 + \left[I_{a\in\{25,29\},it} \times I_{t\geq 1998}\right]\delta_2 + \varepsilon_{it} \end{aligned} \tag{6}$$

$I_{a,it}$ is the age dummy. That is, $I_{a,it} = 1$ if the individual i at month t is a years old and 0 otherwise. Similarly, I_t is the time dummy, which equals 1 in year t and 0 otherwise. Furthermore, $I_{a\in\{21,24\},it}$ equals 1 if individual i in month t is aged from 21 to 24, and 0 otherwise, and $I_{t\geq 1994:7}$ equals 1 if month t is greater than or equal to July 1994, and 0 otherwise. The parameter δ_1 estimates the policy effect of 1994 reform for age group 21-24, and δ_2 the policy effect of 1998 reform for age group 25-29. We also estimate the above equation using fixed effects regression[8].

There are several caveats to the estimation strategy. First, since the actual implementation of welfare and its activation policies were left to municipalities, it is well known that there were lags in implementation of the reforms and also large differences in the level of strictness of activation rules across municipalities. While many municipalities would be less strict than the national rules, others, such as Farum would implement much tougher activation policies.

Second, since the reforms were implemented first to younger individuals and then the older ones, most of the cohorts were either never subjected to the tougher national rules or subjected to them at all ages until the age 29. Hence, for most cohorts, fixed effects would only capture the difference in policy effect across age groups.

To address those issues, we also estimate the model by using the municipality level activation policy as the policy variation. From the AMFORA, which collects detailed data on activation for each person living in Denmark, we extract spells of welfare which ended up in activation and use the following regression to predict the length of spells until activation, and use them as the strictness of the activation policy. We do this separately for each year from 1994 - the start of the AMFOR data, until 2002.

$$S_{it} = \sum_{a=1}^{4} I_a \alpha_{at} + \sum_{m} I_m \alpha_{mt} + \varepsilon_{it} \tag{7}$$

S_{it} is months of welfare spells until the start of activation. To derive the spells, we look at the welfare data from year t until year $t+2$ and choose welfare spells that started in year t and was activated until year $t+3$. The passive welfare spells that neither resulted in regular employment nor activation was in almost all municipalities less than 2 % of the passive spells that did not

[8]Before 1991, we only have verdict date for each crime, and not crime date. Since the estimation of the model depends on the knowledge of the month of the crime committed, we cannot use the data before 1991. Because we only use data from 1991, we cannot evaluate the reforms in 1990 and 1991, which were targeted for age groups 18-19, and 20.

result in regular employment. Therefore, we conclude that bias due to right censoring can be ignored. I_a is a dummy for the age group a. We have four age groups, those below or equal to 20, 21 to 24 age group, 25 to 29, and 30 to 35. The predicted municipality level enforcement is

$$\hat{S}_{mt} = \sum_{a=1}^{4} I_a \hat{\alpha}_{at} + \sum_{m} I_m \hat{\alpha}_{mt} \tag{8}$$

Then, we use the predicted spell as the policy variable indicating municipality level enforcement. That is, we estimate the following regression.

$$C_{it} = X_{it}\beta_X + \sum_{a=19}^{35} I_{a,it}\beta_a + \sum_{t=1994}^{2002} I_t\beta_t + \sum_{m} I_m\beta_m + \hat{S}_{mt}\gamma + \varepsilon_{it} \tag{9}$$

Notice that there are two sources of identification. The first source is the over time change in age coefficients of the predicted municipality level enforcement, and the second one is the over time change in the coeffients of the municipality dummies in the enforcement equation. The implicit assumption is similar to the Difference in Differences assumption that the time series variation in age effects and the municipality specific effects come from the changes in enforcement.

This has the additional benefit that we also can estimate the effect of tighter activation policy on crime for the individuals below 20 years, because it also relies on municipality level variation on the activation policies for them as well.

5 Estimation Results

5.1 Results Based on the Reform in Farum

We present the estimation results where we use the radical activation reform introduced in Farum in 1987 as the exogenous variation.

We first divide the policy periods into three periods, with each period being 4 or 5 years of length. That is, the first period is from 1987 to 1990, the second from 1991 to 1997, and the third from 1998 to 2001. We know from the statment by Lars Bjerregard that not all the activation policies were immidiately implemented. We therefore classify periods from 1987 to 1990 as the introductory phase of the policy, and from 1991 to 1997 the fully implemented policy period, and For example, the requirement that all welfare recipients who did not get a job had to report to the production house was only implemented from 1990. In Table 2, we report the policy effects for the unemployment uninsured estimated by OLS. In column 2 (OLS1), we report the OLS result where we separately estimate the policy effect of the $87-90$ introductory period, the full implementation period of $91-97$ and $98-01$, the period where the Farum activation policy and the national level policy start to converge. We also consider two policy periods: one that starts in 1987 and ends in 2001, i.e. before the start of the formal investigation of Farum and, the other that starts in 1987 and ends in 1997, i.e. before 1998 when, at the national level all unemployment uninsured individuals below the age 30 had to be activated after 13 weeks of unemployment. We can see that men with more years of schooling, , married men and men with children get arrested less. Higher education dummy, which equals one if the individual has a degree higher than highschool and zero otherwise, has a positive coefficient estimate, but since its value is similar to that of the years of schooling, years of schooing effect dominates. It is also interesting to see that Danish and Western origin immigrant dummy is insignificant. That is, nonwestern immigrants do not significantly get arrested more than the others, controling for other characteristics. [9] The coefficients for the interaction terms between Farum and the policy dummies are negative for both $87-90$ and $91-97$ periods, but the former is insignificant. Furthermore, the one for the $98-01$ period is insignificantly positive. That is, the policy is estimated to have a significant crime reduction effect only for the full implementation period of $91-97$. It is estimated to be quite large, i.e. to reduce the arrest rate by 0.031 in annual terms, a 33% reduction relative to the mean arrest rate of unemployment uninsured in Table 1.

[9] Even though the RHS variables include age, year, month and the municipality dummies, we neither present their coefficient estimates nor discuss them due to space limitations.

In column 3 and 4, we present similar results where the policy period is set to be $87-01$ and $87-97$, subsequently. In both cases, policy effects are estimated to be negative and significant, and large as well, with annual reduction of 0.017, i.e. 17 % reduction in annual arrest rate for the $87-01$ policy period and and 0.027 (28 %) reduction for the $87-97$ policy period. In all three specifications, the autocorrelation of the error term (ρ) is estimated to be small, around 0.055. Hence, the issues of autocorrelation in the error term raised by Bertrand et. al. (200?) do not seem to matter in our results.

In Table 3, we report the Fixed Effects results. Notice that the coefficients on Years of schooling, higher education, married are positive and significant. Only the coefficient on having children dummy is negative and insignificant. Imai and Krishna (2004) estimated the dynamic model of criminal decision on the life cycle data on arrest rates. They conclude that it is the high crime types that reduces crimes more after the age 18. Since in the fixed effect, level of crime is differenced out, and only the change of crime is left as the dependent variable, the results is consistent with Imai and Krisha (2004) when the noncriminal types are those who have higher education, and are married. The policy effects are estimated to be similar to that of the OLS in Table 2. That is, if we divide the policy periods into three subperiods, then, the policy effects of $1987-90$ and $1998-01$ periods are estimated to be negative but insignificant, and the policy effect of full implementation period of $1991-97$ is estimated to be negative and significant, resulting in annual reduction of arrests by 0.045 a 47 % reduction. If we set the policy period to be $1987-2001$, then the policy effect is significant and reduces annual arrest rate by 0.0264 (27 %), and for $1987-97$ policy period, the annual reduction is estimated to be 0.030 (31 %). Again, the autocorrelation is estimated to be small in all three specifications.

The reduction of arrests for the young men during the policy period could be due to the changes in Farum that is unrelated to the activation policy. Those could be, for example, an increase in police spending in Farum, or an increase in municipal spending on youth activities.

To consider the possibility, we next run the regressions for the unemployment insured. If, during the policy period, crime decreased for the unemployment uninsured but not for the unemployment insured, then we can rule out the effect of policies that affect both unemployment insured and uninsured.

In Table 4, we report the results of the same OLS estimation exercise for the unemployment insured. The individual observables have coefficient estimates similar to those of the unemployment uninsured. Except for the higher education dummy, all the other coefficient estimates are negative, and significant at 5 % significance level except for the Danish and Western immigrant dummy. Even though the coefficient on the higher education dummy is positive, it is insignificant and it is so small that the overall schooling effect is negative. These results are the same for all specifications with different policy periods. On the other hand, the policy effects are very different from those of the unemployment uninsured. In column 2, we can see that the policy effect of all the subperiods, the introductory period of $87-90$, the full implementation period of $91-97$, and the convergent period $98-01$ are positive, and significant for the $91-97$ and $98-01$ periods. The policy effect estimate for the $1987-01$ policy period is also positive and significant, and even though that of the $1987-97$ policy period is negative, it is insignificant and the effect is small. In sum, in contrast to the unemployment uninsured, we do not see any evidence for the reduction in arrests for the unemployment insured during the policy period.

Similar results are confirmed in Table 5, where we report the Fixed Effects estimation results for the unemployment insured, using the same model specification as the Fixed Effects estimates of the unemployment uninsured.

As we mentioned earlier, policies on the unemployment insured individuals are administered at the central government and do not have much local variation. The fact that OLS results and the fixed effects results show no policy effects on the insured is reassuring for the validity of our empirical analysis, since it excludes any possibility of exogenous changes in Farum that had a strong effect on criminal behavior for both unemployment insured and uninsured during the policy period.

Now, there could be two types of endogeneities that could bias the fixed effects estimation of the policy effect. First, in order to avoid activation when unemployed, individuals could seek jobs that provide unemployment insurance. In Figure 17 we plot the ratio of men aged 18-30 who are insured in Farum and in the rest of Denmark. We can see that the young men are less unemployment insured in Farum than in the rest of Denmark, and the difference has been slowly increasing over time until 1990, from 0.130 in 1986 to 0.172 in 1990. After that, it has decreased until 1995 to 0.075, whereafter it has been increasing. Hence, if we just look at Figure 17, the sign of the effect of workfare policy on insurance choice of workers in Farum relative to the rest of Denmark is ambiguous. In Table 9, we report the results of the probit analysis, which estimates the probability of unemployment insurance choice. There, after controlling for the observables (did we include age dummies), during policy period the unemployment insurance probability is estimated to be higher, and significant at 5% level. Since the individuals who can find insured jobs are those who are less criminally active, the selection due to the relative decrease in uninsured in Farum during the policy period should increase the crime rates of the uninsured. Therefore, the selection would bias the policy parameters upwards. Hence the negative policy effect we obtain is likely to be a conservative estimate. On the other hand, the individuals who, during the policy period switched their status from uninsurance to insurance may be more likely to commit crimes, thus increasing the arrest rate for the unemployment insured during the policy period, which could be the reason for the slightly positive and significant policy effects estimated by OLS and the slightly positive and insignificant Fixed Effects policy effects for the unemployment insured.

One potential source of bias would arise, when during the policy periods young men would leave Farum or stay out of Farum for fear of strict activation. Here, we cannot a priori assess whether individuals who leave or stay out of Farum would be more criminally active or not. Hence, we use Heckman 2 step approach to deal with the above endogeneity.

In Tables 7, 8 and 9, we report results of the Heckman's two step estimation. In the second column of Table 7 (Step 1), we present the parameter estimates of the first stage Probit model.[10] Both the dummies indicating whether both parents lived in Farum or whether both parents lived outside Farum are highly significant in explaining the individual's choice of whether to live in Farum or not, with parents living outside Farum having a negative effect on residence in Farum and vice versa. That is, instruments are highly significant in explaining the Farum dummy. In Tables 8 and 9, we present the coefficient estimates of the second step fixed effects regression. Dummies for living with parents in the same home and in the same municipality are included in the RHS. In the second step fixed effects regression, the inverse Mill's ratio term, representing the selection bias for the arrest rate in Farum is negative, and that for the arrest rate in rest of Denmark is positive. That is, the error term of the probit equation and the second stage fixed effect equation for Farum is negatively correlated, implying downward bias of the fixed effects estimation without selection bias correction. If we compare the estimated policy effects of Table 8 and that of Table 3, we can see that all the coefficients are similar in magnitude exept for those of the policy periods $87-90$ and $98-01$, where the Heckman sample selection corrected policy effect estimates are smaller in magnitude than those of the uncorrected Fixed Effects estimates.

Note that the effect of living together with parents is negative and significant at 10% for all specifications, whereas the effect of living in the same municipality with parents is positive but insignificant.

In Table 9, we present the policy effect for the unemployment insured estimated by Heckman 2 Step procedure. As we mentioned earlier, policies on the unemployment insured individuals

[10] We included year and age dummies on the RHS, but we do not report the coefficients on those dummies to save space.

are administered at the central government and do not have much local variation. The fact that OLS results and the fixed effects results show no policy effects on the insured is reassuring for the validity of our empirical analysis, since it excludes any possibility of exogenous changes in Farum that had a strong effect on criminal behavior for both unemployment insured and uninsured during the policy period.

5.2 Direct and Indirect Effects of Activation

So far, we have obtained results that indicate that ALMP for unemployment uninsured young men in Farum is effective in reducing their crime rate. The next issue we try to address is why it does so. We consider two potential reasons for it. First, activation could induce unemployment uninsured welfare recipients into regular employment, and the transition to regular employment reduces crime. This would be an indirect effect of activation. There is a sizeable literature documenting that employment decreases crime. Examples are Raphael and Winter-Ebmer (2001) and others. Another effect would be when activation reduces crimes of individuals who stay on welfare. That would be a direct effect, which, has not been investigated much in the literature. In Table 10, we present OLS results of the effect of activation policy reform in Farm where we control for the fraction of days of Social Assistance participation in a month. We have data on the monthly social assistance payment from 1984 until 2005, and we use them to compute the fraction of Days on Social Assistance in a month. Due to space limitations, we only report coefficients on Social Assitance and the policy effect. We can see that the Social Assitance participation increases crime significantly, both for uninsured and insured, and the policy effect is negative ans significant for the fully implementation policy period of $1991-97$ and for $1987-97$ period for the uninsured, but positive for the insured. It is important to notice that there are individuals who declare themselves unemployment insured in the data that receive welfare payments. Since the registered data only has information on unemployment insurance status at the end of the year, it could be due to the transition of the UI status within a year. This is especially true since individuals need to be a UI member for at least one year before being eligible for the UI payment. Those who are a member of the UI Fund but are not yet eligible for the UI benefit, but lose their job receive Social Assitance. However, they were not subject to the tough activation policies implemented in Farum. Therefore, the results in Table 10 compares the arrest rates of non-UI and UI recipients, where the same social assistance payment is controlled for, when they are unemployed and not eligible for UI, but the tough activation rule only applies for the non-UI. The fact that the policy is only effective and statistically significant for the non-UI recipients is in support of our claim that it is the tough activation policy, that is also effective in reducing crime. In Table 11, we show the same results for the Fixed Effects Estimation. Then, the polcy effects for both the non-UI and UI workers have the same signs as those estimated by OLS. However, the policy effect for the Non-UI workers for $1991-97$ and $1987-97$ policy periods are only significant at 10 % significance level. In Table 12, we present the Fixed Effects Difference-in-Difference-in-Differences estimator of the policy effect. There, the policy effect is estimated by the coefficient on the interaction term of policy period, Farum dummy and the Non-UI dummy. All the OLS policy effect estimates are negative and significant, and the policy effect of $1991-97$ policy period is now negative and significant at 5% significance level.

In Table 13, column 3 (FE1) we report fixed effects results, separately for uninsured young men who are on welfare more than 25 % of the days in a year, and less than or equal to 25 % of the days in a year. Similarly, column 4 (FE2) shows the results for 50 % and column 5 (FE3) for 75 %. As we can see, the policy effects for the welfare dependents are estimated to be extremely large, and it increases with the magnitude of the dependency: -0.23 for 87-01 policy period and -0.18 for 87-97 policy period for the men who were on welfare more than 25 % of the time, and -0.37, -0.36 subsequently for those who were on welfare more than 75 % of the time. Notice that the decrease in arrest rate is even higher than the overall annual arrest rate of the unemployment uninsured, which is around 0.10 for Farum and 0.11 for the rest of Denmark. This comes from the fact that the welfare dependents have average annual arrest rates that are much higher than the rest of the unemployment uninsured men to begin with. On the other hand, if we look at the results for men who have been on welfare less than or equal to 25 % of the days in a year, the policy effects are estimated to be insignificant, and similarly for the men with less than or equal to 50 %, and 75 % of the days on welfare. There is a slight tendency for the policy effect to become more negative from 25 % to 75 %. From those exercises, we can conclude that the activation policy has strong negative impact on crime for those individuals who mostly depend on welfare. We also know from pervious literature that those are the ones for whom ALMP has the least employment effect. And it seems that those are the individuals who are most active criminally. By reaching out to individuals with very low chances of regular employment, ALMP is improving the local community by reducing the criminal activities of those who are the most at risk of committing crimes.

5.3 Results Based on National Reforms

In Table 17, we present the estimation results of equation 5. That is, we estimate the policy effect of the reform targeting 18-19 year olds, 20 year olds, 21-24 year olds and 25-29 year olds. In columns 2 and 3, we report the OLS and Fixed Effects estimates of the policy effect for the unemployment uninsured, and in columns 4 and 5, we have those for the unemployment insured. As we can see, policy effects are estimated to be negative and significant for the OLS for unemployment uninsured, and negative for the unemployment insured but only significant for the $21-24$ year olds, but their magnitude is much smaller than that of the uninsured. On the other hand, none of the Fixed Effects policy effecs are significant, and some for the uninsured have the wrong sign. As we discussed earlier, the reason for the discrepancy between the OLS and FE results could be due to the fact that there are lags in enforcement of the policy reform, and given the timing of the reforms, some cohorts are facing tough activation policy at all ages, and other cohorts do not face any changes in the welfare policy at any age. This could be why the FE estimates do not seem to properly identify the policy effect.

In Table 15, we present the estimation results of equation 8, where we use the predicted municipality level welfare spell until activation as policy variation. The OLS estimates for the unemployment uninsured, are positive and significant for the uninsured. The positive coefficient estimate is expected because shorter spells means stronger enforcement. On the other hand, for the insured, the coefficient is positive but insignificant. The fixed effects estimator is positive but significant only at the 10% level for the unemployment uninsured and negative and insignificant for the insured. The DDD estimates of the policy effect is positive and significant for both OLS and FE.

Next, we present the DDD coefficient estimates where the dependent variable is the weekend crime. There, both OLS and FE coefficients have the expected positive sign and are significant. This shows that the activation policy does not only reduce crime through the reduction in hours available for crime during weekdays. Activation also changes the lifestyle of the individuals which also results in reduction of criminal activities during the weekends.

6 Concluding Remarks

We have estimated the effect of ALMPs on the criminal behavior of the young male workers, both unemployment uninsured and insured. We exploited two policy changes. First, we use a unique policy experiment that began in 1987 in Farum, where a 100 % work or training requirement was imposed for all welfare recipients immediately from the date of enrollment. By comparing the changes in crime rates among the welfare recipients in Farum before and after 1987 with that of the rest of Denmark, we identify the effect of workfare on the crime rate. Second, we examine the effect of a series of national welfare reforms introduced during the 1990s. Those reforms strengthened the work requirement for the young welfare recipients and were introduced gradually, starting with younger welfare participants first. The differential introduction of workfare reform across different age groups work as the exogenous policy variation. We estimate the policy effect by including the municipality level variation in enforcement of the national policy. We find the crime reduction effect to be both statistically and economically significant. We also find that the effect not only comes from the reduction in welfare participation but also from the reduced criminal activities of the individuals who are activated. Furthermore, the strong and significant policy effect on weekend crime indicates that the reducing in crime is also a result of positive change in lifestyle, not just reduction in hours that can be allocated to criminal activities.

Nowadays in Denmark and many other countries in Europe, ALMPs cover most workers in the labor force when they are unemployed. Hence, it is fair to say that they affect a large fraction of the population. However, research on those policies have been almost exclusively focused on their effect on employment. We believe that it is important that we also take a careful look at other aspects of activation policies that could be of importance for the general public. An important issue that is left for future research is to investigate which programs work best in reducing the crime rate of the young unemployment uninsured workers. Our results imply that activation programs that carry less sticks which induce "threat effect" (see Black et. al. (2003)) and are less targeted towards immediate employment could be more effective in reducing crimes of the welfare recipients, and given sufficiently strong crime reduction effects, those programs could be a better choice for the general public.

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7 Tables and Figures

Table 1: Sample Statistics

	Total				Unemployment Uninsured				Unemployment Insured			
	Farum		Rest of Denmark		Farum		Rest of Denmark		Farum		Rest of Denmark	
variable	mean	std. dev	mean	std. dev	mean	std. dev	mean	std. dev	mean	std. dev	mean	std. dev
Unemployment Insured	0.5308	0.4991	0.6438	0.4789								
Arrest Rate	0.0679	0.3422	0.0618	0.3271	0.0967	0.4100	0.1063	0.4436	0.0424	0.2659	0.0372	0.2359
Age	24.80	3.576	25.20	3.417	23.57	3.688	23.99	3.754	25.89	3.088	25.87	3.013
Years of Schooling	10.75	2.495	10.90	2.273	10.30	2.274	10.29	2.257	11.16	2.611	11.23	2.212
Higher Education	0.4163	0.4929	0.4763	0.4994	0.2427	0.4287	0.2607	0.4390	0.5697	0.4951	0.5956	0.4908
Married	0.2286	0.4199	0.1729	0.3782	0.1364	0.3432	0.1214	0.3266	0.3101	0.4625	0.2014	0.4011
Having children	0.2370	0.4252	0.2111	0.4081	0.1663	0.3723	0.1559	0.3628	0.2995	0.4581	0.2416	0.4281
Western or Danish	0.8380	0.3685	0.9463	0.2254	0.8714	0.3348	0.9240	0.2650	0.8084	0.3936	0.9586	0.1991
Parents in the same home	0.3112	0.4630	0.2004	0.4003	0.4233	0.4941	0.2568	0.4368	0.2222	0.4158	0.1712	0.3767
Parents in the same municipality	0.5113	0.4999	0.4827	0.4997	0.6006	0.4898	0.4616	0.4985	0.4403	0.4964	0.4937	0.5000
Sample Size	273007		1884703		128100		671301		144907		1213402	

Table 2: OLS for Unemployment Uninsured Workers

	OLS1		OLS2		OLS3	
Dependent Var	No. of Arrests		No. of Arrests		No. of Arrests	
Years of Schooling	-0.00294	(0.00021^{**})	-0.00294	(0.00021^{**})	-0.00294	(0.00021^{**})
Higher Education	0.00294	(0.00057^{**})	0.00293	(0.00058^{**})	0.00293	(0.00057^{**})
Married	-0.00168	(0.00091^{*})	-0.00167	(0.00090^{*})	-0.00167	(0.00091^{*})
Children	-0.00230	(0.00060^{**})	-0.00231	(0.00060^{**})	-0.00231	(0.00060^{**})
Danish or Western	0.00054	(0.00099)	0.00053	(0.00099)	0.00054	(0.00099)
$[87-90] \times Far$	-0.00116	(0.00091)				
annual	-0.01392					
$[91-97] \times Far$	-0.00262	(0.00044^{**})				
annual	-0.03149					
$[98-01] \times Far$	0.00072	(0.00067)				
annual	0.00864					
$[87-01] \times Far$			-0.00140	(0.00039^{**})		
annual			-0.01678			
$[87-97] \times Far$					-0.00223	(0.00061^{**})
annual					-0.02679	
R Squares	0.0054		0.0054		0.0054	
ρ	0.0552		0.0548		0.0551	
Sample Size	799401		799401		799401	

Table 3: Fixed Effect for Unemployment Uninsured

	FE1		FE2		FE3	
Dependent Var	No. of Arrests		No. of Arrests		No. of Arrests	
Years of Schooling	0.00048	(0.00018**)	0.00048	(0.00018**)	0.00047	(0.00018**)
Higher Education	0.00179	(0.00073**)	0.00179	(0.00073**)	0.00180	(0.00073**)
Married	0.00185	(0.00077**)	0.00187	(0.00077**)	0.00187	(0.00077**)
Children	-0.00089	(0.00058)	-0.00090	(0.00058)	-0.00089	(0.00058)
$[87-90] \times Far$	-0.00156	(0.00126)				
annual	-0.01869					
$[91-97] \times Far$	-0.00376	(0.00142**)				
annual	-0.04512					
$[98-01] \times Far$	-0.00102	(0.00147)				
annual	-0.01220					
$[87-01] \times Far$			-0.00220	(0.00106**)		
annual			-0.02636			
$[87-97] \times Far$					-0.00252	(0.00111**)
annual					-0.03022	
ρ	-0.0073		-0.0074		-0.0073	
Sample Size	799401		799401		799401	

Table 4: OLS for Unemployment Insured

	OLS1		OLS2		OLS3	
Dependent Var	No. of Arrests		No. of Arrests		No. of Arrests	
Yrs. of School	-0.00092	(0.00009^{**})	-0.00092	(0.00009^{**})	-0.00092	(0.00009^{**})
Higher Educ	0.00009	(0.00024)	0.00009	(0.00024)	0.00009	(0.00024)
Married	-0.00062	(0.00017^{**})	-0.00061	(0.00017^{**})	-0.00061	(0.00017^{**})
Children	-0.00105	(0.00017^{**})	-0.00105	(0.00017^{**})	-0.00105	(0.00017^{**})
Western	-0.00058	(0.00056)	-0.00060	(0.00056)	-0.00060	(0.00056)
$[87-90] \times Far$	0.00017	(0.00024)				
annual	0.00208					
$[91-97] \times Far$	0.00043	(0.00018^{**})				
annual	0.00516					
$[98-01] \times Far$	0.00140	(0.00027^{**})				
annual	0.01675					
$[87-01] \times Far$			0.00055	(0.00015^{**})		
annual			0.00659			
$[87-97] \times Far$					-0.00003	(0.00017)
annual					-0.00035	
R Squares	0.0030		0.0030		0.0030	
ρ	0.0190		0.0190		0.0190	
Sample Size	1358309		799401		799401	

Table 5: Fixed Effect for Unemployment Insured

	FE1		FE2		FE3	
Dependent Var	No. of Arrests		No. of Arrests		No. of Arrests	
Yrs. of School	0.00004	(0.00016)	0.00004	(0.00016)	0.00004	(0.00016)
Higher Educ	0.00088	(0.00064)	0.00088	(0.00064)	0.00088	(0.00064)
Married	0.00074	(0.00022**)	0.00074	(0.00022**)	0.00074	(0.00022**)
Children	−0.00070	(0.00021**)	−0.00070	(0.00021**)	−0.00070	(0.00021**)
$[87-90] \times Far$	0.00065	(0.00078)				
annual	0.00779					
$[91-97] \times Far$	0.00042	(0.00064)				
annual	0.00502					
$[98-01] \times Far$	0.00050	(0.00082)				
annual	0.00595					
$[87-01] \times Far$			0.00053	(0.00058)		
annual			0.00636			
$[87-97] \times Far$					0.00035	(0.00054)
annual					0.00421	
ρ	−0.0192		−0.0192		−0.0192	
Sample Size	1358309		1358309		1358309	

Table 6: Probit Estimation for Insurance Choice

	Probit1		Probit2	
Dependent Variable	UI		UI	
Yrs. of Schooling	-0.0526	(0.0041**)	-0.0526	(0.0041**)
Higher Education	0.8365	(0.0202**)	0.8366	(0.0202**)
Married	0.0236	(0.0182)	0.0239	(0.0182)
Children	0.2554	(0.0158**)	0.2556	(0.0158**)
Western or Danish	0.0834	(0.0296**)	0.0829	(0.0296**)
Farum	-0.2986	(0.0243**)	-0.2818	(0.0218**)
$[87-01] \times Farum$	0.0657	(0.0285**)		
$[87-97] \times Farum$			0.0493	(0.0276*)
Sample Size	2157710		2157710	

Table 7: Heckman 2 Step Estimation, Non-UI, 1st Step

Dependent Variable	Farum	
Yrs. of Schooling	-0.03427	(0.00980**)
Higher Education	0.11014	(0.04904**)
Married	0.16269	(0.06083**)
Children	0.29229	(0.04724**)
Parents outside Farum	-1.93635	(0.04305**)
Parents in Farum	0.09409	(0.04851*)
Sample Size	499410	

Note: Standard errors are in parentheses.

Table 8: Heckman 2 Step Estimation, 2nd Step, Non-UI

	FE1		FE2		FE3	
Dependent Variable	No. of Arrests		No. of Arrests		No. of Arrests	
Yrs. of Schooling	0.00074	(0.00027**)	0.00075	(0.00027**)	0.00075	(0.00027**)
Higher Education	0.00189	(0.00092**)	0.00188	(0.00092**)	0.00191	(0.00092**)
Married	−0.00023	(0.00095)	−0.00021	(0.00096)	−0.00022	(0.00096)
Children	−0.00166	(0.00085**)	−0.00168	(0.00085**)	−0.00168	(0.00085**)
Parents same Home	−0.00139	(0.00081*)	−0.00138	(0.00081*)	−0.00138	(0.00081*)
Parents same Muni.	0.00149	(0.00092)	0.00148	(0.00092)	0.00147	(0.00092)
Inverse Mill's Ratio λ_1	−0.00861	(0.00642)	−0.00871	(0.00642)	−0.00881	(0.00642)
Inverse Mill's Ratio λ_2	0.00930	(0.00489*)	0.00954	(0.00485**)	0.00948	(0.00490*)
$[87-90]\times$Farum	−0.00125	(0.00168)				
annual	−0.01499					
$[91-97]\times$Farum	−0.00427	(0.00166**)				
annual	−0.05124					
$[98-01]\times$Farum	−0.00037	(0.00152)				
annual	−0.00445					
$[87-01]\times$Farum			−0.00206	(0.00128)		
annual			−0.02477			
$[87-97]\times$Farum					−0.00297	(0.00134**)
annual					−0.03568	

Note: Standard errors are in parentheses.

Sample size is 499410.

Table 9: Heckman 2 Step Estimation, 2nd Step, UI

Dependent Variable	No. of Arrests	
$[87-90]\times$Farum	0.00152	(0.00108)
$[91-97]\times$Farum	0.00022	(0.00074)
$[98-01]\times$Farum	0.00062	(0.00088)
$[87-01]\times$Farum	0.00082	(0.00072)
$[87-97]\times$Farum	0.00049	(0.00068)

Note: Standard errors are in parentheses.

Sample size is 947038.

Table 10: OLS: Controling for Social Assistance

Dependent Variable: No. of Arrests				
	Non-UI		UI	
Social Assistance	0.02481	(0.00206**)	0.01474	(0.00087**)
$[87-90]\times$Farum	-0.00180	(0.00117)	0.00054	(0.00026**)
$[91-97]\times$Farum	-0.00176	(0.00076**)	0.00082	(0.00018**)
$[98-01]\times$Farum	0.00204	(0.00060**)	0.00152	(0.00025**)
R Squares	0.0132		0.0048	
ρ	0.0443		0.0258	
Social Assistance	0.02480	(0.00206**)	0.01474	(0.00087**)
$[87-01]\times$Farum	-0.00089	(0.00067)	0.00088	(0.00016**)
R Squares	0.0132		0.0048	
ρ	0.0439		0.0258	
Social Assistance	0.02480	(0.00206**)	0.01474	(0.00087**)
$[87-97]\times$Farum	-0.00248	(0.00096**)	0.00017	(0.00016)
R Squares	0.0132		0.0048	
ρ	0.0442		0.0258	
Sample Size	693403		1151852	

Note: Standard errors are in parentheses.

Table 11: FE: Controling for Social Assistance

Dependent Variable: No. of Arrests				
	Non-UI		UI	
Social Assistance	0.00782	(0.00080**)	0.00339	(0.00086**)
[87 − 90]×Farum	−0.00107	(0.00139)	0.00102	(0.00074)
[91 − 97]×Farum	−0.00261	(0.00155*)	0.00097	(0.00069)
[98 − 01]×Farum	0.00019	(0.00152)	0.00095	(0.00086)
ρ	-0.0062		-0.0138	
Social Assistance	0.00782	(0.00080**)	0.00339	(0.00086**)
[87 − 01]×Farum	−0.0012	(0.0012)	0.00099	(0.00060*)
ρ	-0.0064		-0.0138	
Social Assistance	0.00782	(0.00080**)	0.00339	(0.00086**)
[87 − 97]×Farum	−0.00205	(0.00120*)	0.00061	(0.00054)
ρ	-0.0062		-0.0138	
Sample Size	693403		1151852	

Note: Standard errors are in parentheses.

Table 12: Controling for Social Assistance, DDD

	OLS		FE	
Social Assistance	0.02257	(0.00168**)	0.00645	(0.00058**)
Non-UI	0.00104	(0.00026**)	−0.00080	(0.00029**)
Farum × Non-UI	0.00158	(0.00040**)	0.00253	(0.00106**)
[87 − 90]×Farum	0.00043	(0.00051)	0.00032	(0.00082)
[87 − 90]×Farum × Non-UI	−0.00176	(0.00009**)	−0.00068	(0.00140)
[91 − 97]×Farum	0.00092	(0.00027**)	0.00098	(0.00069)
[91 − 97]×Farum × Non-UI	−0.00262	(0.00013**)	−0.00349	(0.00137**)
[98 − 01]×Farum	0.00220	(0.00033**)	0.00150	(0.00083*)
[98 − 01]×Farum × Non-UI	−0.00115	(0.00008**)	−0.00241	(0.00149)
R-Squares	0.0107			
ρ	0.0204		-0.0164	
Social Assistance	0.02256	(0.00168**)	0.00645	(0.00058**)
Non-UI	0.00104	(0.00025**)	−0.00081	(0.00029**)
Non-UI × Farum	0.00158	(0.00040**)	0.00236	(0.00105**)
[87 − 01]×Farum	0.00104	(0.00026**)	0.00086	(0.00060*)
[87 − 01]×Farum × Non-UI	−0.00199	(0.00008**)	−0.00218	(0.00114*)
R-Squares	0.0107		-0.0138	
ρ	0.0203		-0.0138	
Social Assistance	0.02256	(0.00168**)	0.00339	(0.00086**)
Non-UI	0.00104	(0.00025**)	−0.00081	(0.00029**)
Non-UI × Farum	0.00115	(0.00039**)	0.0058	(0.00089*)
[87 − 97]×Farum	−0.00005	(0.00038)	0.00011	(0.00055)
[87 − 97]×Farum × Non-UI	−0.00186	(0.00012**)	−0.00141	(0.00110)
R-Squares	0.0107			
ρ	0.0204		-0.0164	
Sample Size	1845255		1845255	

Note: Standard errors are in parentheses.

Table 13: Fixed Effects for Unemployment Uninsured Workers

		25 %	50 %	75 %
		FE1	FE2	FE3
Dependent Variable		No. of Arrests	No. of Arrests	No. of Arrests
$> x\%$	$[87-01] \times Farum$	-0.2255 (0.0853**)	-0.3741 (0.1277**)	-0.3723 (0.1601**)
	$[87-97] \times Farum$	-0.1776 (0.0778**)	-0.2645 (0.1195**)	-0.3614 (0.1524**)
	Sample Size	11423	8286	5634
$\leq x\%$	$[87-01] \times Farum$	0.0098 (0.0096)	0.0095 (0.0108)	-0.0067 (0.0122)
	$[87-97] \times Farum$	-0.0071 (0.0091)	-0.0082 (0.0102)	-0.0200 (0.0122*)
	Sample Size	35685	38822	41474

Note: Standard errors are in parentheses.

Table 14: Regression with National Data

	Unemployment Uninsured		Unemployment Insured	
	OLS	FE	OLS	FE
$\text{Policy}_{18,19}$	-0.00263 (0.00118^{**})	-0.00051 (0.00165)	-0.00087 (0.00187)	0.00029 (0.00179)
Policy_{20}	-0.00472 (0.00109^{**})	-0.00146 (0.00148)	-0.00145 (0.00144)	-0.00027 (0.00116)
$\text{Policy}_{21\sim24}$	-0.00313 (0.00070^{**})	0.00037 (0.00107)	-0.00087 (0.00034^{**})	-0.00041 (0.00039)
$\text{Policy}_{25\sim29}$	-0.00390 (0.00115^{**})	0.00037 (0.00098)	-0.00036 (0.00018^{*})	0.00018 (0.00025)
R squares	0.0058		0.0028	
ρ	0.0559	-0.0024	0.0197	-0.0143
Sample Size	719202	719202	1663254	1663254

Note:

$\text{Policy}_{20\sim24} \equiv I\{age \in \{21, ..., 24\}\} \times \{t \geq 1994\}$, $\text{Policy}_{25\sim29} \equiv I\{age \in \{25, ..., 29\}\} \times \{t \geq 1998\}$

Standard errors are in parentheses.

Table 15: Regression with National Data, Local Level Variation in Enforcement.

Non-UI	OLS	FE
Policy	0.00535 (0.00273**)	0.00429 (0.00239*)
R-Squares	0.0073	
ρ	0.1229	0.0124
Sample Size	244176	244176
UI	OLS	FE
Policy	0.00096 (0.00065)	-0.00008 (0.00065)
R-Squares	0.0024	
ρ	0.0086	-0.0240
Sample Size	596548	596518
DDD	OLS	FE
Policy	0.00440 (0.00106**)	0.00211 (0.00082**)
R-Squares	0.0055	
ρ	-0.0086	-0.0302
Sample Size	840724	840724
	Weekend Crime	
DDD	OLS	FE
Policy	0.00059 (0.00023**)	0.00075 (0.00037**)
R-Squares	0.0017	
ρ	-0.0113	-0.0357
Sample Size	818319	818319

Note: Standard errors are in parentheses.

Figure 1: Unemployment Rate of Young Men

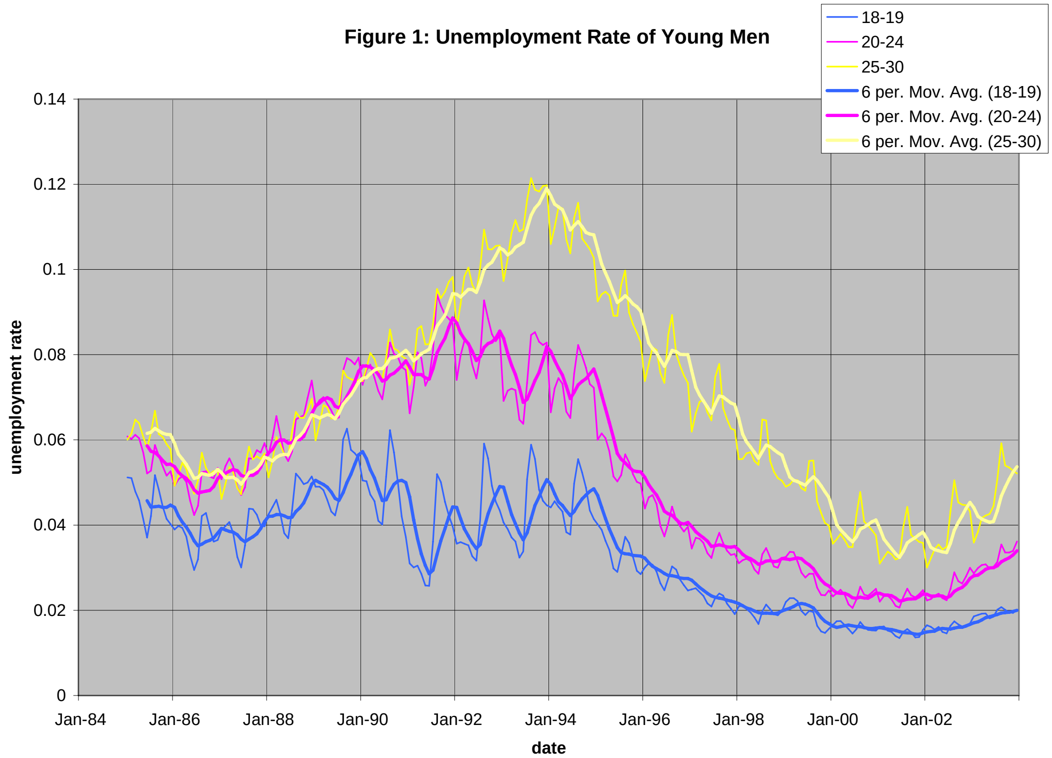

Figure 2: Ratio of Young Men Employed in Regular Job or at School

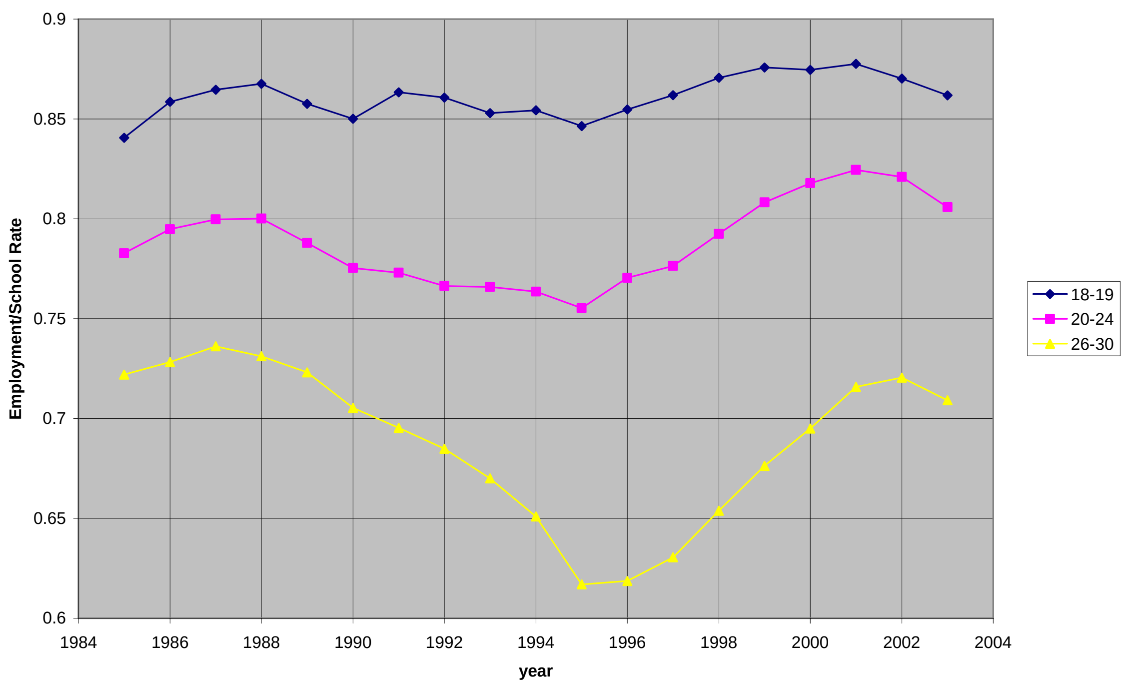

Figure 3: Property Crime Arrest Rate of Unemployment Uninsured Danish Citizens and Western Immigrangs, 12 months moving average

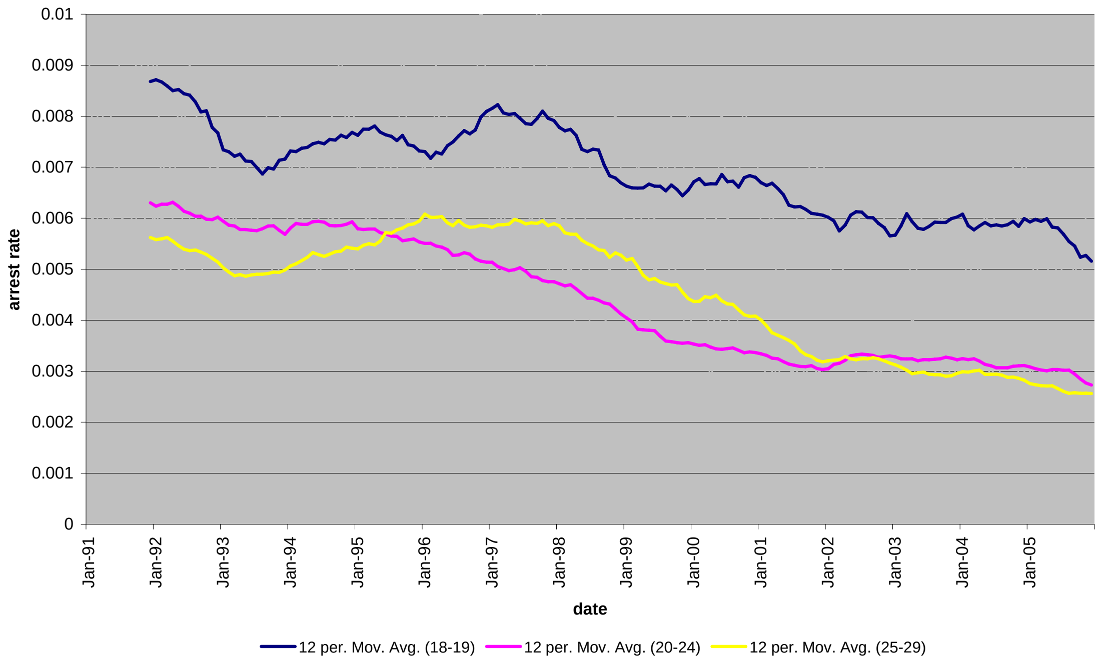

Figure 4: Violent Crime Arrest Rate of Unemployment Uninsured Danish Citizen and Western Immigrants, 12 months moving average

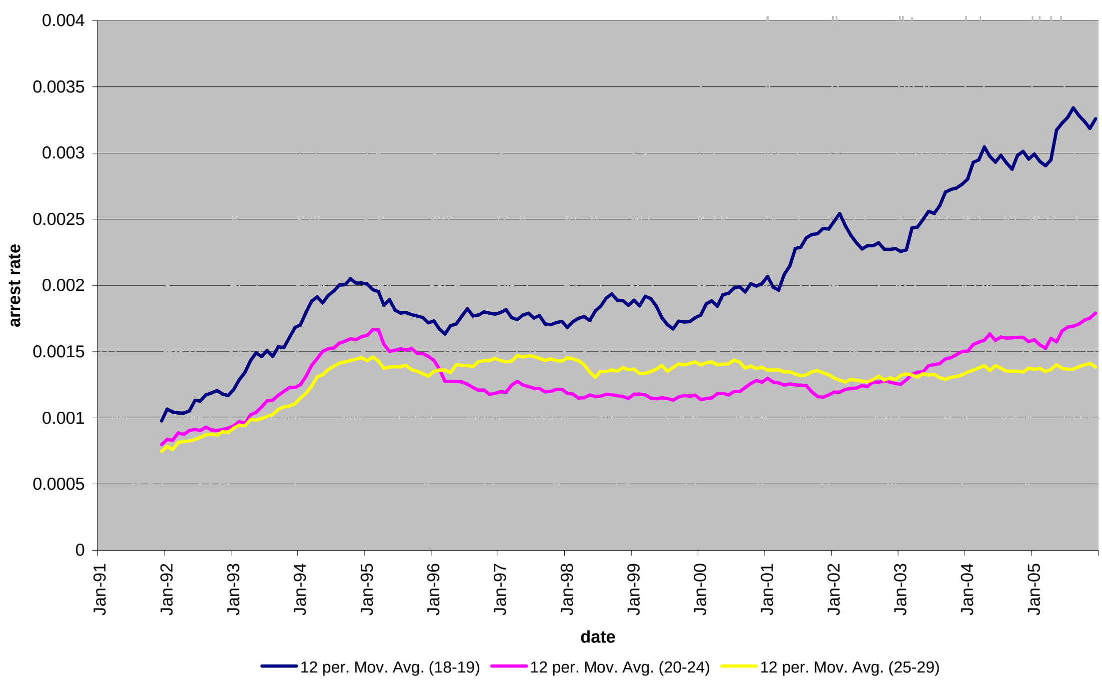

Figure 5: Property Crime Arrest Rate of Unemployment Insured Danish Citizens and Western Immigrants 12 months moving average

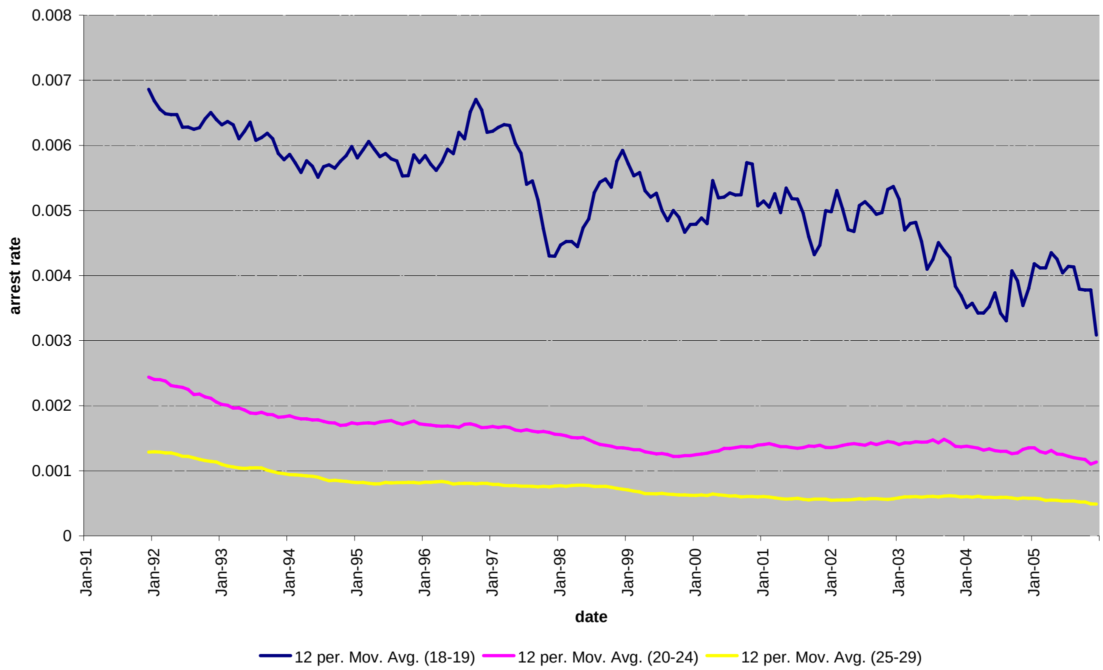

Figure 6: Violence Crime Arrest Rate of Unemployment Insured Danish Citizens and Western Immigrants,12 months moving average

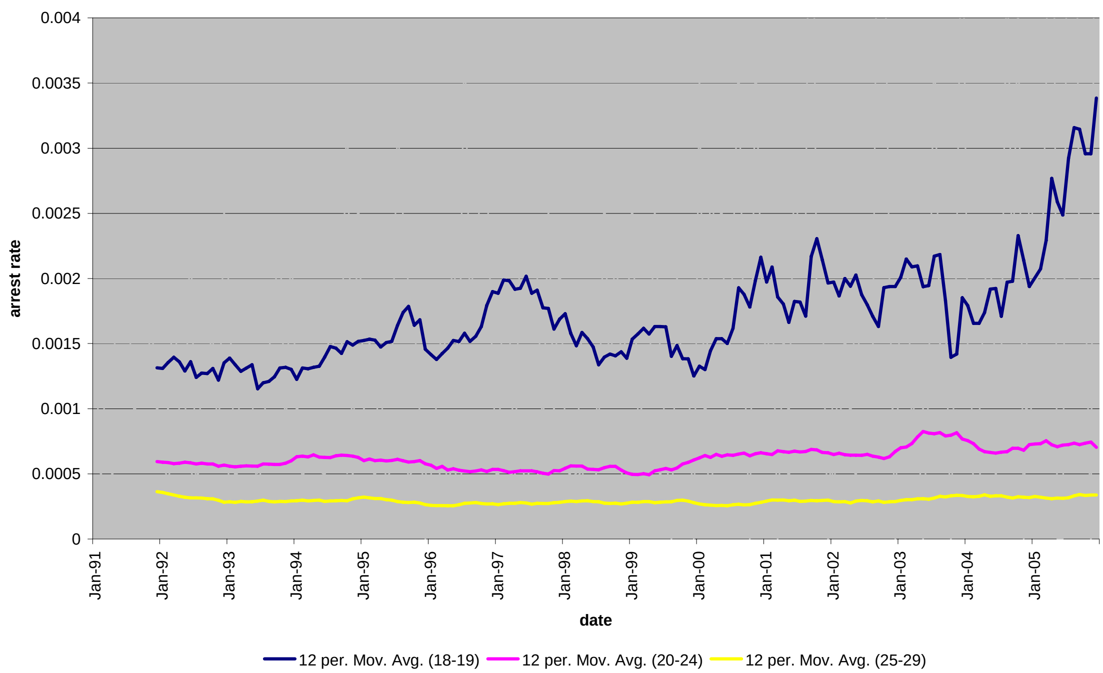

Figure 7: Jobless Rates of Unemployment Uninsured Males, between Ages 18 to 30

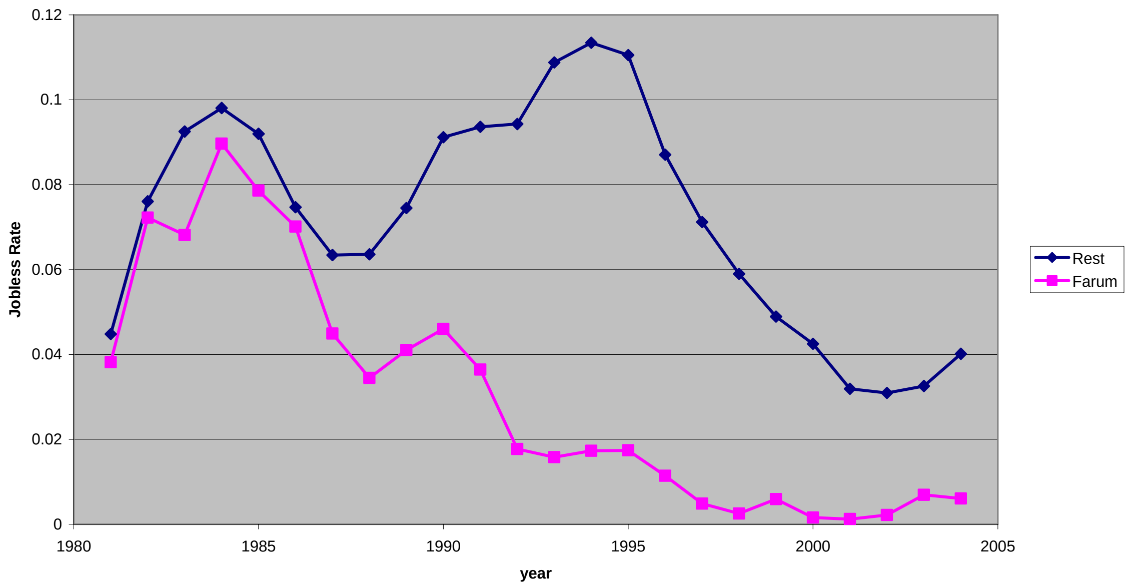

Figure 8: Jobles Rates of Unemployment Insured Men between Ages 18-30

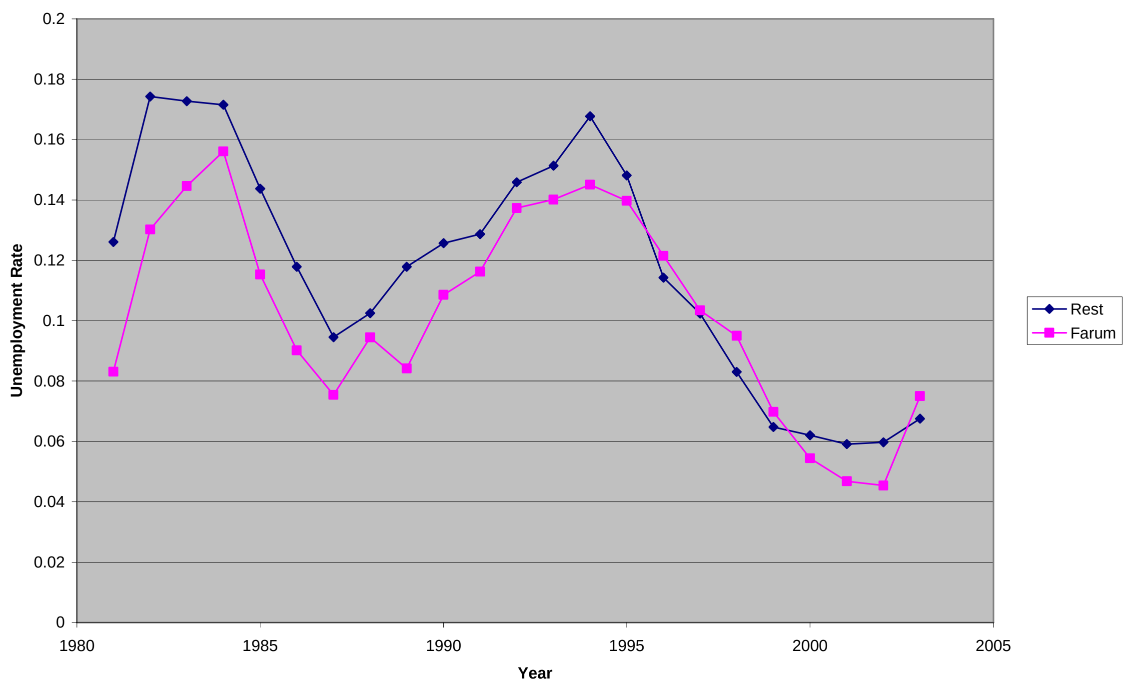

Figure 9: Arrest Rates of Unemployment Uninsured Men between Ages 18-30

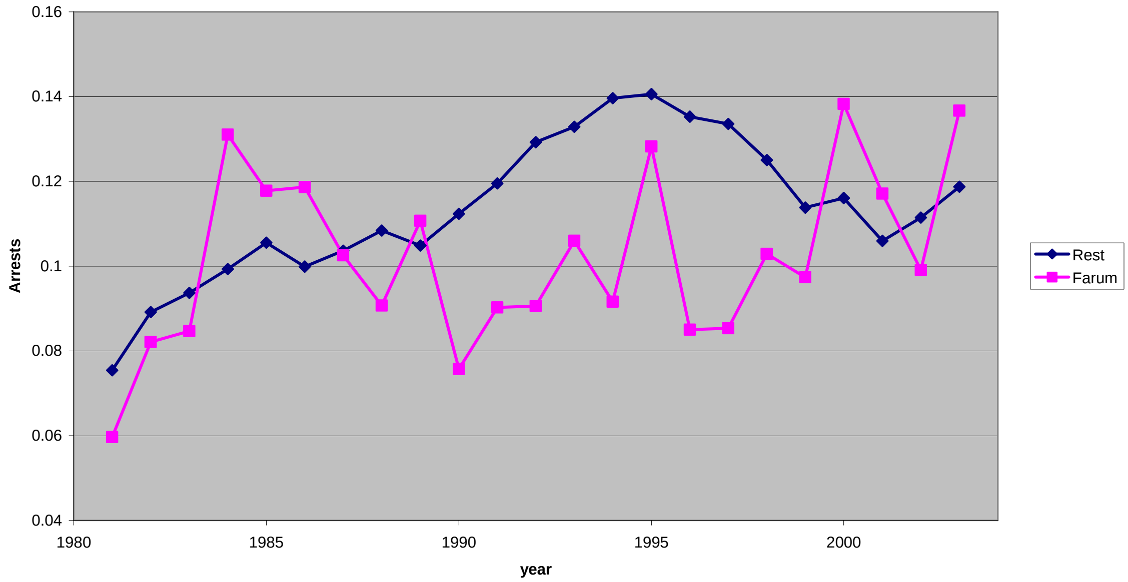

Figure 10: Property Crime Arrest Rates of Unemployment Uninsured Men between Ages 18-30

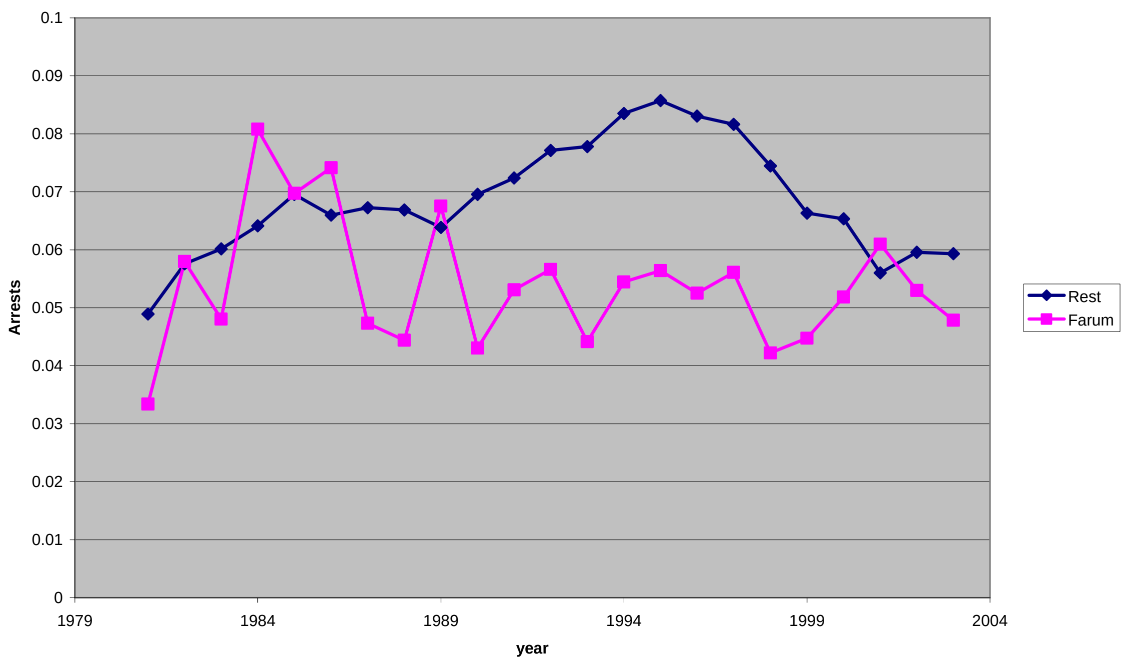

Figure 11: Violent Crime Arrest Rates of
Unemployment Uninsured Men between ages 18-30.

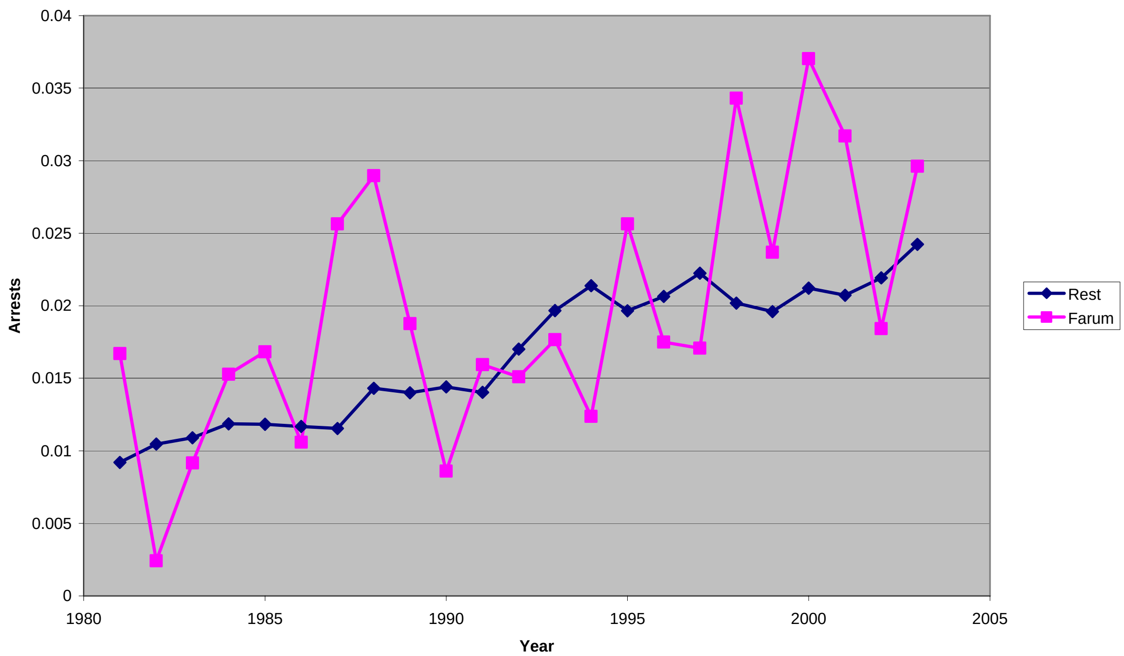

Figure 12: Arrest Rates
of Unemployment Insured Men between Ages 18-30

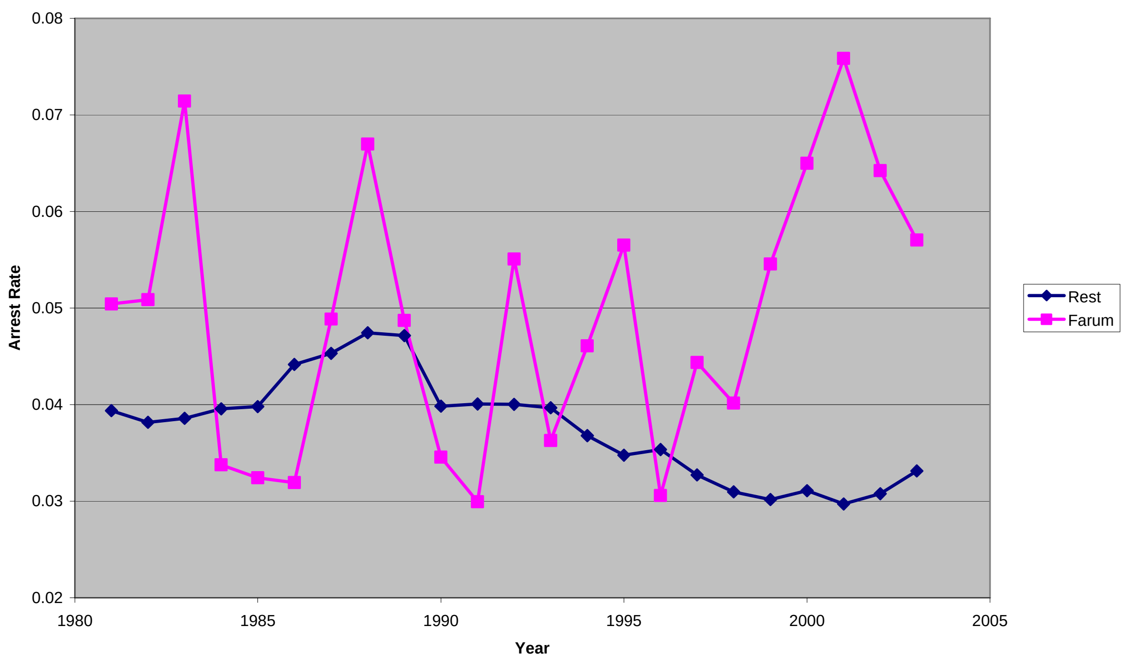

Figure 13: Property Crime Arrest Rates of Unemployment Insured Men between Ages 18-30

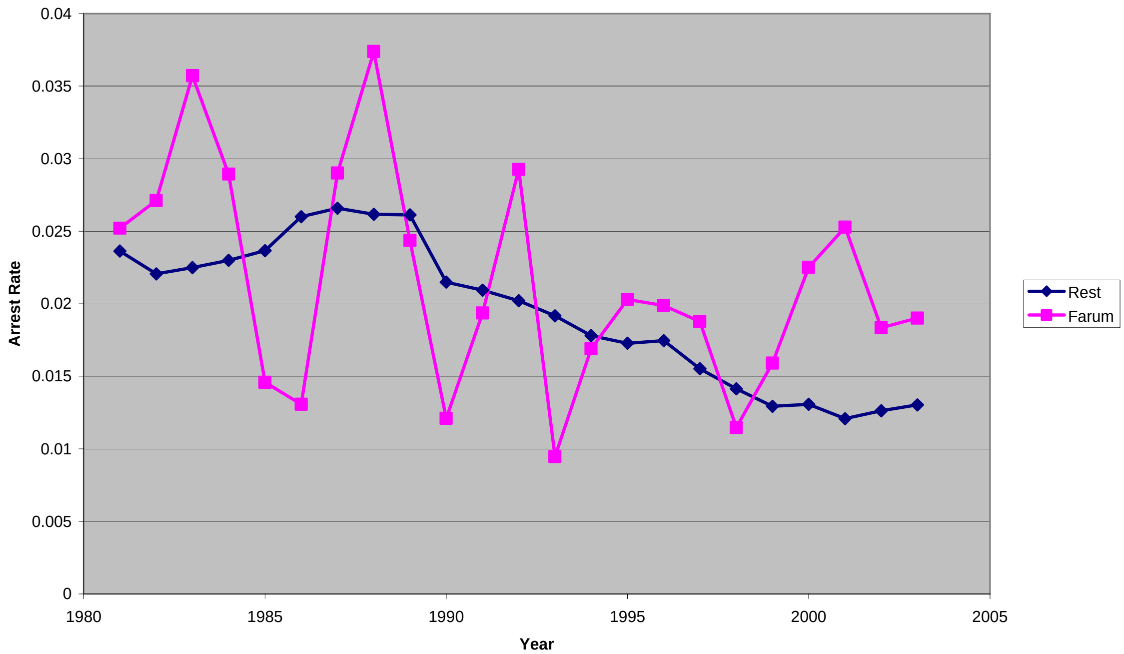

Figure 14: Violent Crime Arrest Rates of Unemployment Insured Men between Ages 18-30

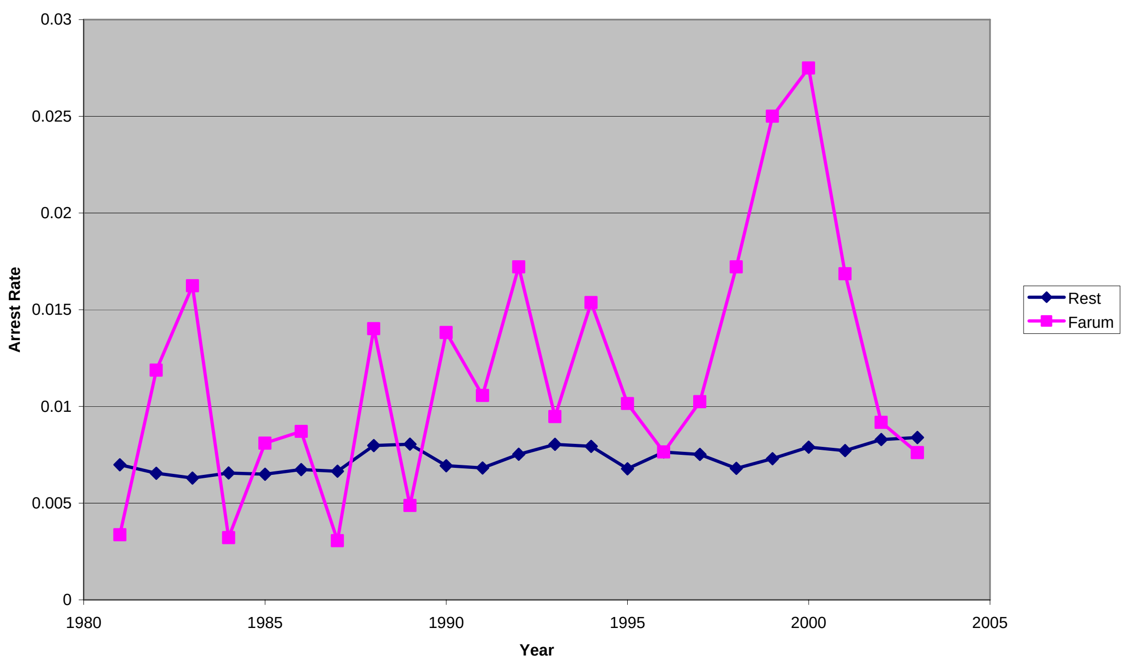

Figure 15: Ratios of Nonwestern Origin Immigrants of Unemployment Uninsured Men between Ages 18-30

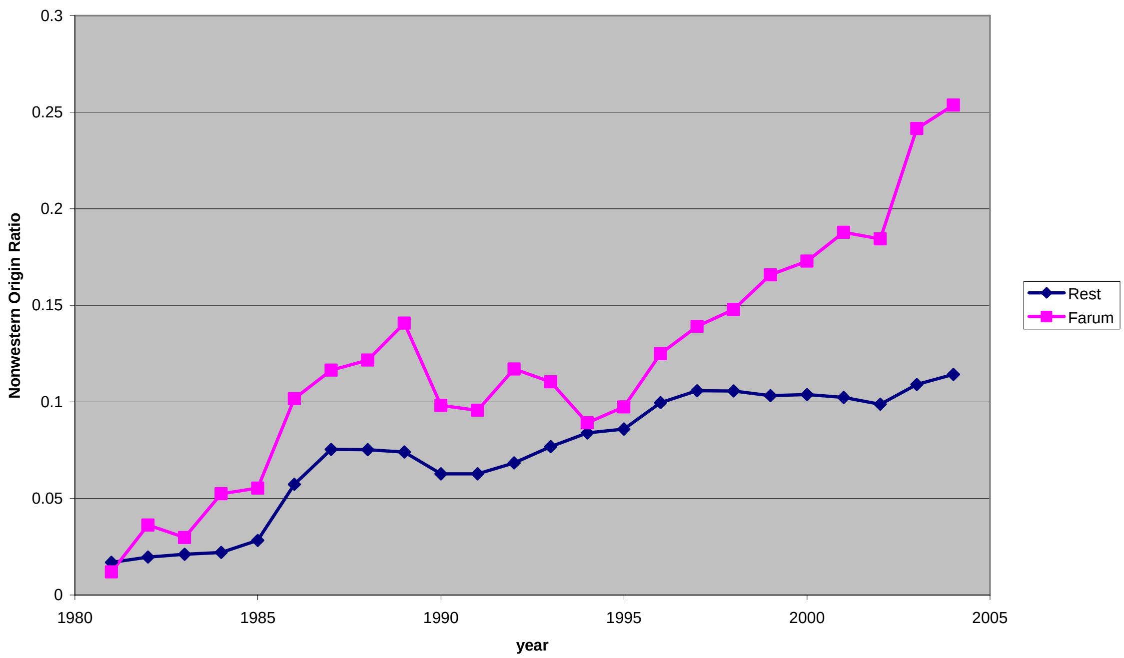

Figure 16: Average Years of Schooling of Unemployment Uninsured Men between Ages 18-30

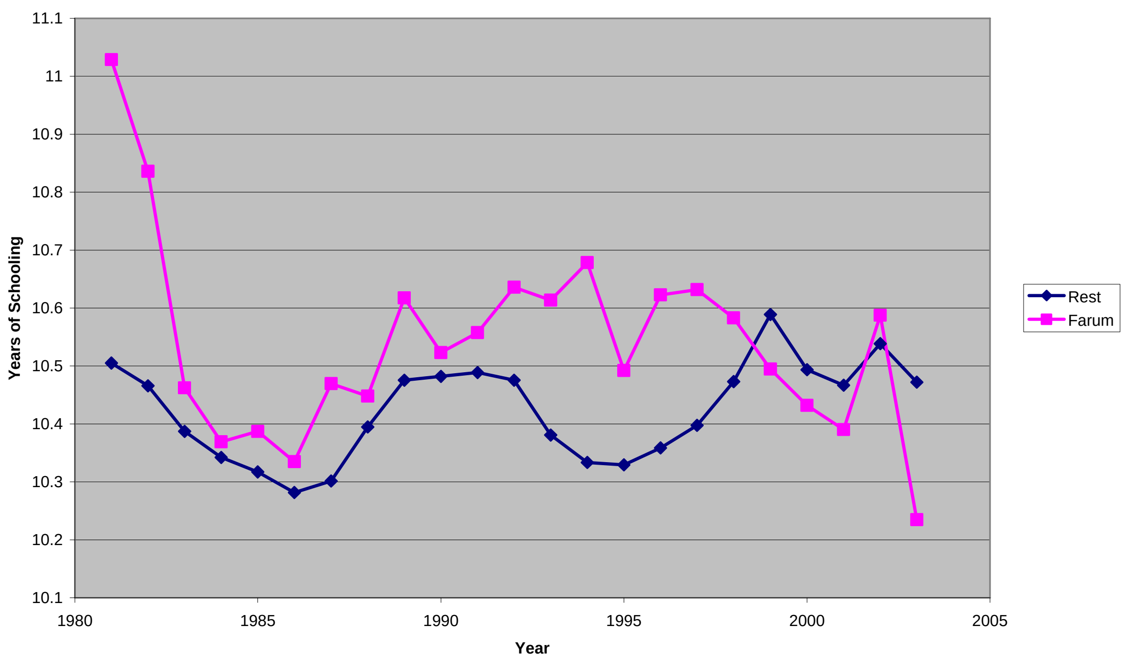

Figure 17: Ratio of Unemployment Insured Men between Ages 18-30